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Three Essays on Social Learning

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Three Essays on Social Learning

Abstract

Social learning broadly refers to learning through the acquisition of information from social sources. In the three essays of my dissertation, I investigate the various underlying drivers of social learning and how such learning can impact purchase decisions.

In Essay 1, I investigate the link between social learning and

sales of experiential products. In particular, I focus on how social capital (i.e., the propensity for people to trust and communicate with each other) moderates the level of social learning for experiential products and thus impacts aggregate sales.

In Essay 2, I study how social learning operates differently across the various stages of physician prescription - trial and repeat of a new prescription drug. Given that the mechanisms of social influence varies across trial and repeat stages, the second essay further assesses who is most influential and who is most influenceable across stages.

In Essay 3, I examine how consumers make purchases of experiential products and link it to their active search for information from interdependent social sources. Essay 3 assesses the impact of the pattern of similarity of preferences in individual-level social networks (homophily, i.e., the tendency of individuals to associate with similar others, and structural balance, i.e., the congruency of preference in a social network) on consumer search, learning, and purchase.

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Raghuram Iyengar

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THREE ESSAYS ON SOCIAL LEARNING

Jae Young Lee

A DISSERTATION

in

Marketing

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

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ABSTRACT

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Social learning broadly refers to learning through the acquisition of information from social sources. In the three essays of my dissertation, I investigate the various underlying drivers of social learning and how such learning can impact purchase decisions.

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and *structural balance*, i.e., the congruency of preference in a social network) on

consumer search, learning, and purchase.

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INTRODUCTION

Social learning broadly refers to learning through the acquisition of information from social contacts such as friends, neighbors, colleagues, etc. Researchers have long recognized the theoretical importance of social learning, and documented that social learning is a significant driver of consumer decisions. In an influential study, Roberts and Urban (1988) formally modeled consumers' decision to choose a brand based on, among other factors, word-of-mouth from their friends, and showed that the social learning is a significant driver of consumers' decision. Duflo and Saez (2003) analyzed a randomized experiment and showed the role of social learning in employees' decisions to enroll in a Tax Deferred Account (TDA) retirement plan within a large university. Conley and Udry (2010) investigated the role of social learning in the diffusion of a new agricultural technology in Ghana.

Beyond the significant effect of social learning in consumer decisions, recent studies have investigated different drivers of social learning and provided insights on the factors which make social learning more efficient. For instance, Iyengar, Van den Bulte, and Valente (2011) consider the adoption of a new drug and find that physicians' self-reported opinion leadership moderate the weight they put on others prescription behavior. Godes and Mayzlin (2009) show that, for products with low awareness (a brewery chain in their study), word-of-mouth information from less loyal customers is more effective than more loyal customers at driving sales.

My dissertation adds to this stream of literature by investigating different moderators of social learning. In Essay 1, I investigate how the neighborhood *social capital*, i.e., the aggregate propensity for neighbors to trust and communicate with each other, moderates social learning and the evolution of new trials at the aggregate level. Essay 2 focuses on social learning at an individual level, and broadens the scope of interest by including not only the new product *trial* but also *repeat* stage, and examines how social learning operates differently across the different stages. In Essay 3, I extend Essay 1 by focusing on a characteristic of an individual's immediate social network which can potentially drive aggregate-level social capital. To be more specific, I examine how the pattern of the *similarity of preferences* in an individual's immediate social networks affects consumer search, learning from social contacts, and purchase behavior. As the similarity of preferences is often confounded with other network characteristics (Aral and Walker 2012), I assess the role of the similarity of preferences in an experimental setting.

In each essay, I further provide managerially actionable insights on how practitioners can effectively leverage social learning based on their understanding about the drivers of social learning.

Essay 1 investigates the link between social learning and aggregate sales of products with experience attributes, i.e., attributes of products that cannot be fully verified prior to the first purchase. Experience attributes are prevalent and salient when consumers shop through catalogs, home shopping networks, and over the Internet. Using data from Bonobos.com, a leading US online fashion retailer, I find that local social learning not only facilitates customer trial, but also that the effect is economically important as about half of all trials were partially attributable to it. Furthermore, merging data from the Social

Capital Community Benchmark Survey, I find that "neighborhood *social capital*" (See Putnam 1995 for details), enhances the social learning process, and makes it more efficient. Social capital *does not* operate on trials directly; rather, it improves the learning process and therefore indirectly drives sales when what is communicated is favorable. These findings suggest that online retailers may want to use geographic targeting based on the embeddedness of social relationships, and I propose a proxy of neighborhood social capital that practitioners could act on.

Essay 2 proposes that social influence may affect not only *trial* but also *repeat* behavior, though the process and source of influence are likely to differ between trial and repeat. The analysis of the acceptance of a risky prescription drug by individual physicians suggests that social learning drives social influence at the adoption stage, but social conformity drives social influence at the repeat stage. Given that the mechanisms of social influence vary across trial and repeat stages, who is most influential varies across stages. Physicians with high centrality in the discussion and referral network and with high prescription volume are influential in trial but not repeat. In contrast, immediate colleagues—few of whom are nominated as a discussion or referral partner— are influential in both trial and repeat. Furthermore, who is most influenceable also varies across stages. For trial, it is physicians who do not consider themselves to be opinion leaders, whereas for repeat, it is those located towards the middle of the status distribution as measured by network centrality. The pattern of results is consistent with informational social influence reducing risk in trial and normative social influence increasing conformity in repeat. The findings suggest that practitioners should consider adapting their messaging so that considerations of perceived risk, status, and normative

conformity receive different weights when trying to get prospects to adopt versus trying to get adopters to repeat.

In essay 3, I examine how the *similarity of preference* in individual social network impacts information search, learning, and purchase of products. To rule out potential confounds with the similarity of preferences, I conduct an incentive compatible stated choice experiment where each participant searches for information about a product from their contacts, learns about the product, and makes a purchase or not. I build a formal model for search and purchase decisions, that flexibly accommodates the behavioral aspects of the similarity of preference. There are two key insights. First, the reason consumers prefer to gather information from similar others (i.e., homophily) is the greater informational benefit rather than the greater convenience of collecting information from them. Second, structural balance, which captures the consistency in the pattern of the similarity of preference among individuals' immediate social networks, is another key driver of consumer search and learning from social contacts. Analogous to Heider (1946), we term a social system as balanced if the valence of preferences (i.e., positive for similar preferences, and negative for dissimilar preferences) in the system multiples out to be positive. While people understand that informational benefits are greater under an imbalanced relationship, their cost of information seeking is also higher. As a result, consumers search less under an imbalanced than a balanced system, yet the lower amount of search still leads to a higher rate of purchase. The findings have implications for companies that facilitate social search for products. For instance, companies that facilitate social search may be able to increase their search traffic by making consumers perceive that the search results are from others whose preferences are similar to theirs and to each other. Also, companies that provide consumers with reviews for

experiential products to increase the purchase rate may be able to increase their purchase rate

by making consumers perceive that the search results are from those who have similar

preferences, and that they are also being exposed to others with diverse preferences.

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ESSAY 1: NEIGHBORHOOD SOCIAL CAPITAL AND SOCIAL LEARNING FOR EXPERIENCE ATTRIBUTES OF PRODUCTS

1.1. Introduction

Information about new products passed from existing to potential customers is an influential and widely studied driver of sales (e.g., Iyengar, Van den Bulte, and Valente 2011; Manchanda, Xie, and Youn 2008). Information regarding *experience* attributes, i.e., attributes which cannot be fully observable and verifiable pre-purchase, plays a key role in reducing the uncertainty faced by potential customers in their first-time purchases. The "experience attribute problem" is a general one; it is, however, particularly acute for consumers who buy products through catalogs, home shopping networks, and over the Internet.[1] Firms selling through these channels face a ubiquitous issue: How to help consumers overcome initial apprehension about buying what they sell.

By any measure, online retailing is by far the fastest growing retail sector around the world. According to Forrester research, the United States will see growth from $231b in 2013 to $370b in 2017 (*CAGR* of 10%); projected rates are almost identical in Europe where the total market should reach $247b by 2017.[2] This phenomenon is not confined to developed markets; in China, year-on-year growth through March 2012 exceeded 50% and *The Economist* predicts that China will quickly become largest market by value.[3]

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[1] Complementary terms have been introduced to the literature for use in particular contexts, e.g., Degeratu, Rangaswamy, and Wu (2000) refer to "searchable sensory attributes" for goods sold online, whereas Lal and Sarvary (1999) use the term "non-digital attributes" to describe product attributes which cannot be fully conveyed when items are sold over the Internet.
[2] http://techcrunch.com/2013/03/13/forrester-2012-2017-ecommerce-forecast/.
[3] http://www.economist.com/news/briefing/21573980-alibaba-trailblazing-chinese-internet-giant-will-soon-go-public-worlds-greatest-bazaar.

Thus, the global consumer economy is one in which information about experience attributes plays an increasingly larger and more important role in buying decisions.

In this dissertation essay, we document how social learning reduces consumer uncertainty for experience attributes in this context; more specifically, we explain why and how neighborhood social capital (defined shortly) makes the social learning process more efficient. Critically, it is not simply the case that social capital stimulates trial and adoption of new products per se—it does not—rather, it works through a specific mechanism to improve the quality of information transmitted in the social learning process.

The institutional setting for our empirical work is best understood by example. Premier and rapid-growth US Internet retailers like Bonobos.com, Trunkclub.com and WarbyParker.com employ methods that include "totally free" return policies, "home-try on", and "pop-up stores" in large part to combat consumer uncertainty about the experience attributes of the products they sell. In September 2012, leading industry observer GigaOm.com reported on a $40m fundraising round by WarbyParker.com and noted: "That (home try-on) has helped Warby Parker overcome one of the *biggest hurdles* (italics added) for online fashion brands, getting people to feel comfortable about their online purchase."[4]

Naturally, these firm-initiated methods can be costly. We document a complementary customer-initiated process for the resolution of pre-trial uncertainty that occurs naturally offline: Social learning and information transmission between existing and potential

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[4] See http://gigaom.com/2012/09/10/warby-parker-raises-36-8m-to-expand-fashion-eyewear-brand/ for details.

customers. Neighborhood social learning is observed in numerous settings including diffusion of information about agricultural, healthcare, and retirement practices (e.g., Conley and Udry 2010; Sorensen 2006; Duflo and Saez 2003); we add to this body of literature by demonstrating why social learning is so important for the growing consumer Internet sector. Furthermore, we show why social capital, i.e., "the information, trust, and norms of reciprocity inhering in one's social networks" (Woolcock 1998, p. 153) moderates local social learning, and makes the learning process about experience attributes more efficient.

We model social learning and the proposed moderating effect of social capital using data from Bonobos.com, a leading pure-play US fashion retailer, and neighborhood social capital data from the Social Capital Community Benchmark Survey (SCCBS).[5] Identification of social influence from secondary data is challenging (Manski 2000) and the identification of a specific mechanism of social influence requires additional model assumptions that are based on the institutional setting.

In this study, we identify the social learning process under the widely-employed Bayesian Learning approach for modeling learning through direct experience (Erdem and Keane 1996) or from advertisements (Narayanan, Manchanda, and Chintagunta 2005). The Bayesian Learning assumption behind social learning is justified conceptually in Section 1.4.1 and validated empirically in Section 1.5.2. Specifically, we develop a model

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[5] For an interesting introduction to social capital concepts data by one of the foremost authorities on the SCCBS, see Robert D. Putnam (2000), *Bowling Alone: The Collapse and Revival of American Community*. These data are housed at the Kennedy School of Government at Harvard University and have been widely used in social science research; we are the first researchers, to our knowledge, to utilize them in marketing. We provide more details on applications and the data themselves in Sections 1.2 and 1.3, respectively. For information on access, visit http://www.hks.harvard.edu/saguaro/communitysurvey/index.html.

of individual learning and from there derive a neighborhood level (zip-level) model of new trials arising in each time period.

Our model identifies social learning process as a process that is distinct from alternative forms of social influence such as awareness dispersion (Van den Bulte and Lilien 2001), social conformity (Amaldoss and Jain 2005), and network externality (Manchanda, Xie, and Youn 2008). Moreover, we control for possible confounding effects from correlated unobservables (Section 1.4.2), and capture the efficiency of social learning in a single parameter.

We make three new substantive contributions. First, we show that social learning about experience attributes is a key phenomenon in the rapidly growing consumer Internet sector. In our empirical application, more than fifty percent of all trials in the first three and a half years of operations at Bonobos.com are partially attributable to social learning. Second, we explain and document a novel and critical role of local social capital in this process. Again, it is important to note that local social capital does *not* per se stimulate trial and diffusion; rather, it operates only on the learning process itself. It reduces inefficiency in information transmission; in our empirical application the moderating effect impacts about 8% of all trials. This effect is roughly constant throughout the data period, suggesting that a fixed increment in social capital results in a fixed improvement in information transmission, independent of the total number of customers at any time period, or when they arrive.

Third, we highlight an important theme from recent related work; namely, that "real world" factors influence consumer decisions to buy online (see, for example, Anderson et al 2010; Brynjolfsson, Raman and Hu 2009; Choi and Bell 2011; Forman, Ghose and

Goldfarb 2009) and that insights from geographic variation in online buying are actionable. SCCBS data are not available commercially so we identify and justify a readily accessible measure, the "number local bars and liquor stores per capita per zip code" as a proxy for neighborhood social capital in the target group. We show that this variable moderates learning (of course it is *not* significant in a model that also contains the "true" measure of social capital).

The remainder of the paper is organized as follows. Section 1.2 summarizes relevant prior research and develops the conjectures for social learning and social capital. Section 1.3 describes the research setting, data, and measures. The empirical model is developed in Section 1.4. Section 1.5 reports the findings and Section 1.6 concludes the paper.

1.2. Background and Prior Research

1.2.1. Consumer Uncertainty about Experience Attributes of Products Sold Online

Prior to their first purchase, consumers buying via catalogs, home shopping networks, and the Internet lack complete knowledge about experience attributes of products (e.g., "fit, feel, touch, and taste"); for example, " … fit is not fully observed by the customer prior to purchase … [in] retail settings where customers select from a catalog or Internet site without being able to fully inspect the product." (Anderson, Hansen, and Simester 2009, p. 408).

For a consumer who is considering buying a pair of pants in a store, the texture of the pants is a search attribute, i.e., an attribute that is directly verifiable pre-purchase. As implied by Anderson et al. (2009), when the consumer considers buying the same item online or through a catalog, this same attribute—the texture of the pants—becomes an

experience attribute, i.e., not fully observable and verifiable pre-purchase. The consequences are well known. Uncertainty about experience attributes decreases purchase frequency (Cox and Rich 1964) and dollars spent (Jasper and Ouelette 1994) for catalog and home shopping purchases.[6]

In some instances, offline distribution that allows customers physical access to products is imperative, at least for some segments, as: "There are still people who want to *touch and feel* (italics added) clothing before they purchase." (Andy Dunn, CEO of Bonobos.com).[7] Moreover, when a product is available online *and* offline, consumers might visit the offline store to inspect it and then order it online, perhaps from a competing retailer.[8] Thus, in general, the experience attribute issue is particularly acute for consumers when they consider buying from vertically integrated brands without offline distribution. Consequently, Bonobos.com (fashion apparel) has "insanely easy returns", Zappos.com (shoes) offers "totally free" returns and WarbyParker.com (eyewear) has a "home try-on" option where potential customers are shipped five frames (without lenses) to try for free.

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[6] According to the National Mail Order Association, the first cataloger in the United States is believed to be Richard Sears in late 1880s (http://www.ehow.com/facts_4925839_history-mail-order-shopping.html). TV home shopping emerged in 1977 and Amazon.com first opened an online bookstore in 1994. About 7-8% of all US retail sales are now online.
[7] See http://www.forbes.com/sites/kellyclay/2012/04/12/nordstrom-invests-16-4-million-in-bonobos-now-available-in-stores/ for details.
[8] This phenomenon of "show-rooming" (see http://moneyland.time.com/2012/01/24/target-doesnt-want-to-be-a-showroom-for-the-stuff-you-buy-for-less-at-amazon/) where consumers scout out and examine products at giant offline retailers such as Best Buy or Target, and then purchase (at a lower price) at online alternatives like Amazon.com is problematic for offline stores. "Show-rooming" is a major reason why Circuit City went bankrupt (see http://business.time.com/2012/04/10/best-buy-ceo-brian-dunn-resigns-amid-shift-to-online-shopping/).

These efforts are costly, and absent an understanding of how information about experience attributes spreads naturally and organically for free, e.g., through social learning, firms may be relying too much on efforts that undermine margins.

1.2.2. Local Social Learning in Local Neighborhoods and Internet Retailing

Consumers often learn from their peers before making purchase decisions, i.e., through social learning. When consumers shop online, we expect, *ex ante*, that social learning is a plausible source of information about experience attributes for new customers and thereby helps trial at Bonobos.com (our empirical application) and at other online retailers as well.

Conceptually, this social learning process operates as follows. A potential consumer updates her belief via signals on experience attributes that are received from previous purchasers. Signals relate to the typical quality, "texture", and "style" of products sold on the website. There are various kinds of signals—including those from observations of use, direct conversations, and online reviews—all of which can drive social learning for a focal customer. We focus on *local social learning*; that is, learning that operates through signals from *physically close* others who have made *a prior purchase*, all else held constant.

Social scientists have a longstanding interest in how physically proximate neighbors influence each other, i.e., so-called "neighborhood effect" and how it drives consumption, investment and purchase decisions. In addition, recent studies pinpoint social learning as a key mechanism underlying the observed neighborhood effects in categories where agents face risk or uncertainty (Conley and Udry 2010; Duflo and Saez 2003; Sorensen 2006).

In the substantive domain of online retailing, contagion phenomena have been documented (e.g., Bell and Song 2007; Choi, Hui, and Bell 2010) but the underlying mechanisms largely unexplored. Local social learning is interactive (information senders and recipients know each other) and visceral (McShane, Bradlow, and Berger 2012), so it is potentially more powerful than learning via other sources such as online reviews and Internet-mediated interaction (Choi, Bell, and Lodish 2012). Thus, a more detailed elaboration of social learning as it relates to this important domain is needed.

1.2.3. Local Social Capital as a Moderator of Local Social Learning

In general terms, social capital is the ability of focal actors to secure collective, economic, or informational benefits by virtue of social networks, trust, and other norms in a community (Adler and Kwon 2002; Putnam 1995). In a review article, Nahapiet and Ghoshal (1998) provide a conceptual summary and describe relational and structural dimensions of social capital.[9] In this study, we operationalize the relational dimension as social trust and the structural dimension as frequency of interaction and provide illustrative examples in Table 1.1. In Section 1.3, we develop our operational measure of local social capital from the SCCBS and note its consistency with extant approaches in the literature.

Prior work implies that a higher level of social capital leads to more efficient information transfer (Reagans and McEvily 2003; Uzzi 1997). In our context, we

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[9] In an influential paper Adler and Kwon (2002) note that, for substantive and ideological reasons, there is no "commonly agreed upon" definition of social capital that will suit all contexts. Thus, particular operational definitions may vary by discipline and level of investigation (Robison, Schmid, and Siles 2002). Our study therefore focuses on the relational and structural dimensions of social capital (Nahapiet and Ghoshal 1998) as they are a good conceptual fit to the mechanism, have operational variables available in the SCCBS, and as explained in Section 1.3, have precedent in the extant literature.

conjecture that local social capital enhances local social learning by affecting the *proportion* of signals arising from previous purchases and the *noise* associated with these signals. Specifically, we test whether higher levels of social capital reduce inefficiencies in the social learning process. The theoretical prediction is very specific—social capital operates on the information transformation process and there is no reason to expect that it will have a direct effect on the rate of diffusion. Our empirical specification mirrors this as we model the moderating effect on social learning while at the same time controlling for a potential direct effect on diffusion (and we find it to be insignificant).

There are three interesting aspects to this empirical test. First, as discussed in the Introduction, geographic variation in the propensity of consumers to buy online is explained by geographic variation in various neighborhood characteristics, e.g., offline tax rates, presence of stores, and so on. We examine whether variation in this propensity is related to the quality of interaction among members of a local community as well. Note too, that the effect of neighborhood social capital is qualitatively different from these other factors as it arises from the "multiplier" produced by previous purchases.

Second, previous studies relate social learning and individual characteristics such as opinion leadership (Iyengar, Van den Bulte, and Valente 2011; Nair, Manchanda, and Bhatia 2010). In contrast, we connect the efficiency of social learning to relational characteristics between individuals. Third, most studies focus on benefits from social capital accruing to community members; we show that Internet retailers (who are outside the local community) can benefit as well.

Table 1.1. Dimensions of Social Capital and Effects on Local Social Learning

Dimensions	Definition	Effect on Local Social Learning
Relational Dimension	Social assets in a relationship. This involves factors such as trust and intimacy (e.g., Coleman 1988; Granovetter 1985; Putnam 1995).	Social cohesion arises from the *relational dimension* of social capital because it motivates actors to devote time and effort to communicating and should enable potential customers to get a better sense of experience attributes (e.g., Aral and Van Alstyne 2011). *Hence, a higher relational dimension will lead to higher quality signals.*
Structural Dimension	The pattern of connections and interactions between actors. This involves strength of ties, interaction frequency (e.g., Granovetter 1985), and network closure and density (e.g., Coleman 1988).	Social cohesion arises from the *structural dimension* of social capital because actors connected by stronger and denser networks are more likely to interact. *Hence, a higher structural dimension will make it more likely that signals are observed.*

1.2.4. Summary and Testable Conjectures

We examine two new conjectures. First, that incomplete consumer knowledge about experience attributes prior to trial is partially resolved through local social learning from past local purchases made by others. Second, that local social capital reduces inefficiencies in the local social learning process by improving the likelihood that signals are: (1) observed by potential customers, and (2) less noisy. Finally, as noted previously, it is important to recall that social capital does not, per se, make purchases more likely. Rather, it improves the efficiency of the learning process itself. In instances where the social learning process results in favorable updating, i.e., potential customers come to learn that the product is better than they might have initially imagined, sales will be positively impacted.

1.3. Research Setting, Data, and Measures

1.3.1. General Condition and Research Setting

Our data for the empirical application need to satisfy two conditions. First, the products

need to have experience attributes, and second, consumers should have incomplete

consumer knowledge about experience attributes *ex ante*. Our data from Bonobos.com,

an iconic Internet-based fashion retailer, satisfies these conditions. (More details about

Bonobos' origins are provided shortly.)

In the apparel category fit, feel, and style are very important to consumers (Kwon,

Paek, and Arzeni 1991) and these attributes are by definition experience attributes and

non-verifiable pre-purchase when consumers buy online for the first time (Park and Stoel

2002). Since Bonobos.com targets trendy and fashion-forward males, the importance of

these attributes is amplified. (Industry observer TechCrunch.com refers to the target

customer as a "hip, semi-athletic, 25-to-40 year old guy."—See

http://techcrunch.com/2010/12/16/bonobos-raises-18-5-million-metrosexuals-unite/ for

details.)

By way of additional background, Bonobos.com has manufactured and sold

fashionable men's apparel under their own brand online since October 2007. Unique

pants are their signature product—even several years after launch—the site leads with

"Pantsformation—Fit changes everything" (see http://www.bonobos.com/welcome/n). As

Bonobos grew, they established offline "guide shop" stores in Boston, Chicago,

Georgetown and San Francisco and in April 2012 Bonobos also partnered with

Nordstrom. Nordstrom contributed $16m in capital and agreed to carry Bonobos products;

this accomplished two things—Bonobos could not only to reach new segments of

consumers but also provide consumers with an opportunity to "touch and feel" the products before purchase.[10] (As noted below, our data precede these moves into offline retail.)

1.3.2. Data

The data come from three sources: (1) monthly observations on the number of purchases at Bonobos.com from October 2007 (when the site opened) to March 2011, (2) social capital data from SCCBS, and (3) zip-level demographic information and information on spending at offline retailers from the 2010 ESRI Business Database. Summary statistics for the key variables (all described subsequently) are given in Table 1.2.

Purchase data at Bonobos.com. Our dependent variable is the number of new trials in a zip code for each period since the site opened, i.e., an aggregate count of individual customer trials from inception of the site. As such, the data do not suffer from "left-censoring". We focus on trials, because pre-trial customers have no direct experience, i.e., we deliberately model decisions of consumers who have incomplete knowledge about experience attributes *ex ante*. (The data we use pre-date the period where Bonobos products were made available at either "guide shops" or local Nordstrom stores, so there is no alternative channel where consumers can "touch and feel" the products prior to purchase; see also, Section 1.3.2). Specifically, we analyze data for 42 months from launch (October 2007 through March 2011), during which time more than 40,000 customers tried Bonobos.com.

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[10] For popular press stories on the Bonobos.com "guide shop" store concept and the Nordstrom deal, see http://bostinno.com/2012/05/01/bonobos-launches-first-offline-store-on-newbury-st-bringing-color-fit-discounts-to-bostinno-readers/ and http://www.forbes.com/sites/kellyclay/2012/04/12/nordstrom-invests-16-4-million-in-bonobos-now-available-in-stores/, respectively.

The lagged number of total transactions in a zip code (the sum of trial and repeat transactions) is a key independent variable that serves two control roles. First, it is the source of local signals on experience attributes in the local social learning process (see Section 1.4.1). Second, it controls the potential confounding effects of temporal, spatial, and time-varying spatial influences on the social learning process as well as social influence through mechanisms other than social learning (see Section 1.4.2).

Table 1.2. Descriptive Statistics for Model Variables

		Mean	SD	1	2	3	4	5	6	7	8	9	10	11	12	13
1	New Trials	.28	.83													
2	Lagged Transactions	.62	1.94	.61												
3	Social Capital	.00	1.00	.08	.09											
4	Target Population	5.30K	3.13K	.18	.18	.22										
5	Density	1.04K	1.56K	.24	.24	.30	.40									
6	Local Stores	541.87	412.54	.11	.12	.18	.40	.52								
7	Total Spending	5.14M	2.86M	.21	.21	-.11	.65	.26	.28							
8	Average Income	87.2K	37.5K	.21	.20	-.38	-.05	-.02	.01	.57						
9	Gini Coefficient	.62	.06	-.15	-.16	-.35	-.03	-.34	-.18	.17	.33					
10	Age25	.19	.06	.14	.16	.42	.24	.39	.16	-.09	-.40	-.44				
11	Age40	.30	.07	-.17	-.18	-.46	-.41	-.45	-.23	.03	.45	.39	-.87			
12	Education	.42	.17	.27	.26	-.24	-.13	.12	.02	.39	.69	-.01	.07	.08		
13	Race Diversity	51.91	22.18	.05	.05	.31	.50	.50	.41	.19	-.11	-.22	.32	-.44	-.23	
14	Internet Score	.00	1.00	.10	.10	.01	.06	.11	-.01	.23	.19	.00	.12	-.11	.29	.05

Note: In the analysis we standardize all non-dummy variables aside from Lagged Transactions.

Social capital data from the SCCBS. The SCCBS was undertaken by the John F. Kennedy School of Government, Harvard University between July 2000 and February 2001 and the data are widely used by social science researchers. Published articles report effects of local social capital on local behaviors such as home ownership (Hilber 2010), labor force choices (Aguilera 2002), social vulnerability (Cutter, Boruff, and Shirley 2003), and public health (Harpham, Grant, and Thomas 2002). Documentation for the SCCBS describes it as the "first attempt at systematic and widespread measurement of social capital in the United States, particularly as it occurs within local communities."

Our key zip-level social capital measures for the main model and falsification tests were extracted from the SCCBS. Specifically, we utilized questions relating to the two dimensions of social capital described in Table 1.1: (1) trust among local neighbors (relational dimension), and (2) the frequency of interaction between neighbors (structural dimension). The local trust and interaction scores are simple averages of the relevant survey questions (e.g., "How much do you trust neighbors?") in the SCCBS. Section 1.8.1 (Appendix) provides the details. The neighborhood social capital measure is, in turn, a simple average of trust and interaction frequency, consistent with the standard concepts in the literature (Burt 1992; Marsden and Campbell 1984) and with empirical studies that utilize the SCCBS (e.g., Hilber 2010).

Data on neighborhood characteristics. Zip code characteristics and the aggregated individual demographics of zip residents serve as controls in the empirical analysis (Brynjolfsson, Hu, and Rahman 2009; Forman, Ghose, and Goldfarb 2009). Our control variables are constructed from data purchased from ESRI in Redlands, CA and are available through the 2010 ESRI Demographics and Business Database (see http://www.esri.com/data/esri_data/demographic-overview for details). Specific variables describing zip code characteristics are: *Target Population* (total number of 25-45 year-old males in the zip code), *Population Density* (target density per square mile), *Local Stores* (number of offline clothing stores in the 3-digit zip code area). *Non-metro Area*, *Near-suburb Area*, and *Far-suburb Area* dummies control for the geographic proximity of the focal zip to city centers.

Variables aggregated from individual demographics of zip residents are: *Total Spending* (total annual offline retail spending on the men's clothing category in a zip

code as estimated by ESRI), *Average Income* (average annual income among the target

population), *Gini Coefficient* (income inequality), *Age25* (proportion of males aged less

than 25), *Age40* (proportion of males over the age of 40, i.e., those somewhat outside the

target demographic), *Education* (proportion of people who are "highly educated", i.e.,

have a graduate degree), *Race Diversity* (the diversity measure defined by ESRI), and

Internet Score (a proxy for Internet use and reliance on online information).[11]

Figure 1.1. The Number of New Trials in High Versus Low Social Capital Zip Codes
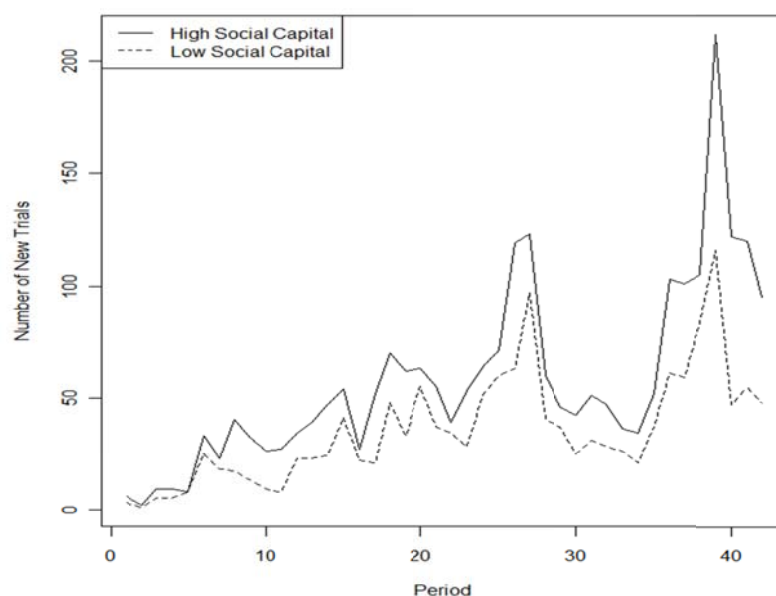
Note: The peaks at month 27 and 39 are December of 2009 and 2010, respectively.

1.3.3. Combined Data for Analysis and Descriptive Patterns

We study how a *previous* trial influences potential subsequent trials by local neighbors,

so we focus on 495 zip codes where the SCCBS is conducted and *at least one* customer

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[11] See http://www.esri.com/library/whitepapers/pdfs/diversity-index-methodology.pdf for the information on *Race Diversity*. *Internet Score* is operationalized as the average of the zip-level average frequency of Internet usage and the zip-level average participation in online discussions as recorded in the SCCBS.

within the 42-month period after the site was launched. Thus, the data consist of 20,790 zip-month observations on the number of new customers. The SCCBS covers 1,104 zip codes so it is possible that the 609 (1,104 – 495) zip codes with no trials at all are somehow different from the 495 zip codes used in estimation, with respect to social capital status. To check that this is not the case we estimate a binary choice model of having at least one trial, using data from all 1,104 zips (see Section 1.8.2). There is no effect of neighborhood social capital in this model, confirming that there is no "selection" of zips with buyers versus no buyers, on the basis of neighborhood social capital. We also fitted a model with the entire 1,104 zip codes in Section 1.8.3 (Appendix).

These data are not geographically condensed as the 495 zip codes span 23 different states and 201 different cities. By virtue of where the SCCBS was conducted, the data exclude New York City and Los Angeles—two locations where Bonobo.com has high sales. This strengthens our study because it means that the findings will not be skewed by particularly "high growth" locations where sales are potentially driven by other mechanisms (such as the fashion orientation of the community and so on). Furthermore, it removes Manhattan zip codes and makes it extremely unlikely that potential customers in our sample are visiting Bonobos.com headquarters on 25th Street and evaluating products in person.[12]

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[12] Potential customers have always had the option of visiting Bonobos.com headquarters in Manhattan and examining products there in a showroom that is part of the head office. (As noted in Section 1.3.1, in 2012, *after* the period of our data, Bonobos.com opened additional "guide shops" in Boston and Palo Alto and obtained distribution via Nordstrom.) It is approximately 200 miles from Bonobos.com headquarters on 25th Street in Manhattan to the nearest zip code in our data, 02215 in Boston, MA. This makes it very unlikely that potential customers in our data were resolving their pre-purchase uncertainty about experience attributes by physically inspecting products in Manhattan.

Figure 1.1 is a model-free view of trial evolution based on the final dataset. It compares the number of new trials in each time period in zips that are in the top one-third based on their social capital scores (165 zips) with the number in the bottom one-third (165 zips). In both groups, the number of new trials increases over time ($p < .001$). Furthermore, in every period, the number of new trials in zips with higher social capital tends to be greater than the number of new trials in zips with lower social capital ($p < .001$). Absent a formal model (see Sections 1.4 and 1.5) this is not conclusive evidence of our proposed effects, but it is nevertheless interesting to observe such a clear pattern in the raw data.

1.3.4. Steps Taken to Mitigate Threats to Validity

Our research setting and data provide us with an opportunity to identify social learning while at the same time offering protection from the four standard threats to validity in social contagion studies. First, we avoid truncation bias (see Van den Bulte and Iyengar 2011) by estimating the trial model on *all* potential consumers in the risk set of 495 zip codes, not just those who ultimately made a purchase in the 42-month data window.

Second, we avoid simultaneity bias by using the lagged rather than contemporaneous number of total transactions in a neighborhood. Third, endogenous group formation is not a credible threat to validity because individuals do not decide on where to live based on a neighbor's trial of a specific website. Of course, we also control for observed and unobserved factors that vary by location. Fourth, by using the lagged number of total transactions in a neighborhood as a control on correlated unobservables between neighbors, we mitigate potential bias arising from the Bayesian learning mechanism (see Sections 1.4 and 1.5 for details).

1.4. Model

Individual consumers make a binary decision every period—to try Bonobos.com or not—on the basis of the expected utility from trial. The overall utility that consumer j in zip code i obtains by trying Bonobos.com at period t is

$$\tilde{U}_{ijt} = \tilde{U}^E_{ijt} + U^D_{ijt} + \varepsilon_{ijt}, \text{ where } \varepsilon_{ijt} \sim \text{IID Standard Gumbel Distribution.} \qquad (1.1)$$

$\tilde{U}^E_{ijt}$ denotes random utility under incomplete knowledge about experience attributes. This utility component evolves through social learning and information acquisition on experience attributes. U^D_{ijt} denotes deterministic utility and is unrelated to the social learning process. As explained shortly, deterministic utility serves as a control to help identify social learning and establish its significance. Finally, ε_{ijt} represents the individual- and time-specific random errors that are not observed

1.4.1. Experience Attributes and the Social Learning Process

Random utility on experience attributes. We assume that there is general agreement about the objective quality of Bonobos.com products (in texture, style, color, etc.) among consumers who have tried them. We denote this by Q. For potential consumers, knowledge of Q (how good the texture is, how fashionable the color is, etc.) is a key input to the trial decision. However, when shopping online, potential consumers are not fully informed of Q because they cannot physically verify experience attributes. Thus, they form beliefs about Q.

Let $\tilde{Q}_{ijt}$ denote the belief about experience attributes for consumer j in zip code i at period t who has yet to try Bonobos.com. Beliefs relate to *products* only not

Bonobos.com "service". This is reasonable because in the period 2007-2011 in the United States there should be no uncertainly about the legitimacy of the site, e.g., Bonobos.com is not going to take orders and then not fill them. In addition, the "Fast and free shipping. Insanely easy returns"[13] promise eliminates uncertainty about service-dependent experience attributes.

Random utility on experience attributes for consumer j in zip i at time t is[14]:

$$\tilde{U}_{ijt}^E = \tilde{Q}_{ijt}.$$ (1.2)

Social learning as Bayesian learning. Uncertain beliefs about experience attributes ($\tilde{Q}_{ijt}$) are represented by a distribution:

$$\tilde{Q}_{ij1} \equiv \tilde{Q}_0 \sim N(Q_0, 1),$$ (1.3)

where Q_0 is the mean of initial belief distribution before trial. Initial uncertainty is set to 1 for identification. The prior belief comes from local signals emanating from previous purchases by local neighbors. Of course social learning alone cannot fully resolve uncertainty, which is resolved only when the product is tried on.

Because they are based on actual purchases, local signals convey information about average objective quality of experience attributes, but these observed signals do not

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[13] See http://www.bonobos.com/welcome/n (top left) and especially http://www.bonobos.com/about/ where it states: "Free both ways. Always." under "Free Shipping" and "Return anything, any time, any reason." under "Painless Returns".

[14] We can also define $\tilde{U}_{ijt}^E$ as a quadratic function of the uncertain belief rather than a linear function to allow for a flexible specification with respect to risk (Erdem and Kean 1996; Narayanan, Manchanda, and Chintagunta 2005). Here, the risk aversion parameter is theoretically estimable, but with a single category data it is hard to know how meaningful this is. We estimated the quadratic model and found that the risk aversion parameter was not significant ($p = .45$), and that the substantive findings were unchanged. Details are available from the authors upon request.

perfectly represent Q. This is because: (1) previous buyers who are sources of signals might differ in their assessments of the average quality of Bonobos.com products depending on their experience, and (2) some information could be "lost in translation" in the sense that a prior buyer may not be able to fully express their assessments of the products to recipients. Given this, the kth local signal in zip code i at time t, S_{ikt}, is:

$$S_{ikt} = Q + u_{ikt} + v_{ikt}, \text{ where } u_{ikt} \sim N(0, \theta_{ui}^2) \text{ and } v_{ikt} \sim N(0, \theta_{vi}^2). \tag{1.4}$$

u_{ikt} allows assessments of quality in zip code i at time t to vary by different purchases (k); similarly, v_{ikt} allows for individual-level variability in signal transmission. Spatial variation in the random components of signals is captured by θ_{ui}^2 and θ_{vi}^2 which vary over zip codes. Assuming independence between the two errors, we write Equation (1.4) as:

$$S_{ikt} \sim N(Q, \theta_{ui}^2 + \theta_{vi}^2). \tag{1.5}$$

As analysts, we cannot observe signals directly, so we assume that the number of signals sent in a location is *proportional* to the number of transactions there in the previous period.[15]

Now, let N_{it-1} denote the lagged number of local transactions in zip code i at period *t-1*. Our assumption implies that the number of observed signals is $\omega_i N_{it-1}$ where ω_i denotes the proportion of signals arising from the lagged local purchases (N_{it-1}). Spatial variation in the observability of signals, (perhaps stemming from spatial variation in local relationships), is captured by ω_i which varies over zip codes. Potential consumers update

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[15] Narayanan, Manchanda, and Chintagunta (2005) assume that the number of signals that a physician observes on the quality of a prescription drug is proportional to the dollars spent on marketing efforts.

their prior beliefs in a Bayesian fashion so that the uncertain belief about Q in zip code i

at time t ($\tilde{Q}_{ijt}$) is:

$$\tilde{Q}_{ijt} \sim N(\bar{Q}_{ijt}, \sigma^2_{ijt}), \tag{1.6}$$

where the variance (σ^2_{ijt}) and the mean ($\bar{Q}_{ijt}$) of the posterior belief are as follows:

$$\sigma^2_{ijt} = \left(1 + \frac{1}{\tau^2_i} \sum_{\ell=1}^{t-1} N_{i\ell}\right)^{-1}, \quad \bar{Q}_{ijt} = \sigma^2_{ijt} \times \left(\frac{\bar{Q}_{ijt-1}}{\sigma^2_{ijt-1}} + \frac{1}{\tau^2_i} \sum_{k=1}^{N_{it-1}} S_{kit}\right), \text{ and } \tau^2_i \equiv \frac{\theta^2_{ui} + \theta^2_{vi}}{\omega_i}.$$

We write the posterior mean and variance in terms of τ^2_i because θ^2_{ui}, θ^2_{vi}, and ω_i are not

separately identified, but identified only up to τ^2_i. (The over-parameterized model, while

not directly estimable, is helpful for exposition.) Most straightforwardly, τ^2_i represents

the "inefficiency of social learning" because as it increases, potential consumers place

less weight on local information. Thus, the smaller the value of τ^2_i the more quickly $\tilde{Q}_{ijt}$

converges to the true Q, or, alternatively, the more efficient the social learning process.

The over-parameterized model also helps in showing that the effect of local social

capital on information transfer is unambiguous. Specifically, in Section 1.2 we

conjectured that social capital boosts the "observability" of signals and reduces noise in

information transmission, i.e., that it increases ω_i and decreases θ^2_{vi}, respectively. (The

nature of social relationships has no effect on variation in the assessment of Q, i.e., no

effect on θ^2_{ui}). Thus, by increasing ω_i and decreasing θ^2_{ui}, an increase in social capital

must lead to a smaller value of τ_i as $\tau^2_i \equiv \frac{\theta^2_{ui} + \theta^2_{vi}}{\omega_i}$. We test this empirically by specifying:

$$\log(\tau_i) = \alpha_0 + \alpha_1 SC_i, \text{ where } SC_i \text{ is social capital in zip code } i. \tag{1.7}$$

Zip-level variables are mean-centered so $\log(\tau_i) = \alpha_0$ when zip i has an average amount

of social capital, i.e., $SC_i = 0$. Since social capital reduces inefficiency in social learning,

we expect that $\alpha_1 < 0$.

1.4.2. Deterministic Utility and Means of Identifying Social Learning

Deterministic utility (Equation 1.1) is unrelated to social learning. While not of central

interest, it nevertheless serves to control for confounds that might affect our ability to

measure the social learning process, and the moderating role of social capital as well.

To control for correlated unobservables, we specify temporal, spatial and time-

varying spatial effects that are separate from the effects of social learning, and the

moderating role of social capital on social learning. It could be the case, for example, that

consumers in cities with more opportunities for socializing prefer Bonobos.com. If this

were true, an observed correlation between the propensity to try and the number of

previous trials in the local community could simply reflect local preferences and not a

causal effect of prior trials on current behavior. Since we focus on social learning as a

specific mechanism of social influence we need to control for awareness dispersion,

social conformity, network externality, as they are competing mechanisms. Thus, we

specify:

$$U_{ijt}^E = \beta_{0t} + X_i\beta_1 + \beta_2 SC_i + \left(\gamma_0 + \gamma_1 SC_i\right)N_{it-1} + \mu_{it}. \tag{1.8}$$

β_{0t} is the period-specific intercept and controls global period effects unrelated to social

learning, e.g., an increase in customer trials from (locally untargeted) marketing activities

such as press coverage, via a flexible semi-parametric approach. X_i is a vector of

observed zip-level characteristics (see Table 1.2) as well as two-digit zip fixed effects[16]

and β_1 are the corresponding parameters. SC_i is zip-level neighborhood social capital,

and its direct impact on the utility is captured by β_2 and our theory of the mechanism

predicts $\beta_2 = 0$.

Lagged local transactions (N_{it-1}) control for types of social influence other than

social influence through social learning (e.g. awareness diffusion, social conformity,

network externality, etc.), and their effects are captured by $\gamma_0 + \gamma_1 SC_i$. We allow them to

vary with social capital to prevent the effect of social capital on social learning (α_1) from

being confounded by its potential moderating effect on the other social contagion

mechanisms (γ_1).

Finally, μ_{it} represents *unobserved* spatial and time-varying spatial effects. Here too

we use N_{it-1} to control μ_{it} because time-varying spatial effects are typically auto-

regressive trends so factors affecting μ_{it} will also be correlated with lagged local

transactions. For instance, suppose a zip code is revitalizing and over time residents have

come to desire more fashionable apparel. This would increase μ_{it} over time, so N_{it-1} is a

reasonable control for μ_{it}; hence, Equation 1.8 becomes:

$$U_{ijt}^D = \beta_{0t} + X_i\beta_1 + SC_i\beta_2 + (\gamma_0 + \gamma_1 SC_i)N_{it-1} + (\delta_0 + \delta_1 SC_i)N_{it-1}, \tag{1.9}$$

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[16] Ideally, we could include five digit zip code-period specific fixed effects to control for potential correlated unobservables (Narayanan and Nair 2012); however, given the non-linearity of our model this will yield an inconsistent estimator with unconditional estimation methods (Arellano and Honore 2001).

$$= \beta_{0t} + X_i\beta_1 + SC_i\beta_2 + \left((\gamma_0 + \delta_0) + (\gamma_1 + \delta_1)SC_i\right)N_{it-1},$$

$$= \beta_{0t} + X_i\beta_1 + \beta_2 SC_i + \beta_3 N_{it-1} + \beta_4 SC_i \times N_{it-1}.$$

In Equation 1.9, γ_0 and δ_0 (γ_1 and δ_1) are not separately identified, but identified only up to β_3 and β_4. The equation clearly shows how lagged local transactions help with correlated unobservables and time-varying spatial trends in error term, μ_{it}.[17]

1.4.3. Expected Utility Function and Aggregate Model of Trial

Since $\tilde{U}_{ijt}$ is a random variable from a consumer's prospective, the consumer makes trial decisions so as to maximize expected utility, $E\left(\tilde{U}_{ijt}\right)$, where:

$$E\left(\tilde{U}_{ijt}\right) = E\left(\tilde{U}_{ijt}^E\right) + U_{ijt}^D + \varepsilon_{ijt} = E\left(\tilde{Q}_{it}\right) + U_{ijt}^D + \varepsilon_{ijt} \tag{1.10}$$

$$= \bar{Q}_{it} + \beta_{0t} + X_i\beta_1 + \beta_2 SC_i + \beta_3 N_{it-1} + \beta_4 SC_i \times N_{it-1} + \varepsilon_{ijt}.$$

From Equation 1.1, the probability that consumer j in zip i tries Bonobos.com at period t is:

$$\Pr_{ijt} = \frac{\exp(\bar{Q}_{it} + \beta_{0t} + X_i\beta_1 + \beta_2 SC_i + \beta_3 N_{it-1} + \beta_4 SC_i \times N_{it-1})}{1 + \exp(\bar{Q}_{it} + \beta_{0t} + X_i\beta_1 + \beta_2 SC_i + \beta_3 N_{it-1} + \beta_4 SC_i \times N_{it-1})}. \tag{1.11}$$

Our dependent variable is Y_{it}, the number of trials in a neighborhood (zip code) and is the aggregate of individual trial behavior. It follows a Poisson distribution as an approximation of a Binomial distribution. This is because given a large population size and a small event probability a Binomial distribution with parameters (n, p) can be

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[17] As with Equation 1.8, the interaction effect of social capital on the time-varying spatial pattern is included to prevent the effect of social capital on social learning (α_1) from being confounded by any potential interaction effect between N_{it-1} and social capital on μ_{it} i.e., via δ_1.

expressed as a Poisson distribution with the parameter np.[18] The likelihood of observing y_{it} is:

$$\Pr(Y_{it} = y_{it}) = \frac{\exp(-\lambda_{it}) \times \lambda_{it}^{y_{it}}}{y_{it}!}, \text{ where } \lambda_{ij} = M_{it} \times Pr_{it}. \tag{1.12}$$

where M_{it} denotes the observed number of *non-triers* in zip code i at time t.

To estimate the model we simulate 50 draws for signals, and compute the entire belief vector on the quality of experience attributes for these draws. Next, we compute the conditional likelihood of observing y_{it} for all observations under different combinations between 50 different strings of $\tilde{Q}_{it}$. The unconditional zip-level likelihood of observing $y_i = [y_{i1} \quad y_{i2} \quad ... \quad y_{iT}]$ is obtained by sequentially integrating conditional y_{it} over conditional y_i over signal samples through Monte Carlo simulation. We estimate the parameters by maximizing the integrated likelihood.

1.4.4. Identification of Parameters

Observations with no local signals (i.e., before the first trial in the zip), identify Q_0. Similarly, Q is identified with the observations under steady state, i.e., when there are sufficiently large numbers of signals such that there is little updating; in our data the cumulative number of signals reaches 525 so we can assume that steady state is achieved.[19] α_0 (Equation 1.7), the average inefficiency in information transferred is identified from the pattern of increase in trials. α_1 (Equation 1.7), the effect of social

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[18] These two conditions are met in our data: The range of the observed number of subjects at risk, i.e., target customers, in a zip code is [451, 19321] and the range of the empirical hazard rate is [0,0.009].
[19] Figure 1.3a is additional evidence that the steady state is achieved in our data set. It shows that there is little change in utility when cumulative number of signals reaches around 100.

capital on the inefficiency of information transferred, is identified from the differences in the cross-sectional variability of the pattern of increase in trials under different levels of social capital.

In the deterministic utility component, the average effect of lagged local transactions (β_3), is separately identified from the social learning process from the observations in the steady state. The interaction effect between social capital and lagged local transactions (β_4) is identified from the differences in sales evolution patterns by social capital under steady state.

1.5. Empirical Findings

Table 1.3 shows the parameter estimates. They suggest that: (1) local social learning is at work, and (2) neighborhood social capital moderates the social learning process by reducing inefficiency in information transfer. The effects are statistically and economically significant and in Sections 1.5.2 and 1.5.3, we report falsification tests and robustness checks, respectively.

Statistical significance of the social learning process is established when the model indicates that consumers enjoy significantly better expected utility from trial as a result of social learning, and it is based on the interplay of several parameters (Q_0, Q, α_0, and α_1). This is identical to saying that the local social learning process is statistically significant when an additional local transaction significantly increases pre-trial expected utility; thus, we use a Bootstrap method to quantify the marginal utility increase from an additional local transaction. In Figure 1.2a the solid line is the marginal utility increase from an additional local transaction under the average level of social capital (mean-

centered $SC_i = 0$). The 95% bootstrap confidence interval (indicated by dotted lines) is always positive; hence, there is significant evidence of local social learning.

Table 1.3. Social Learning and Local Social Capital: Estimates from Bonobos.com

	Model Estimates	Standard Errors
Parameters of the Social Learning Process		
Q_0, Initial Prior Mean of the Quality of Experience Attributes	-12.517	(.153)**
Q, True Quality of Experience Attributes	-11.107	(.082)**
α_0, log (Signal SD|SC=0)	1.092	(.089)**
α_1, ∂ log (Signal SD|SC)/ ∂ SC	-.204	(.065)**
Control Variables		
Lagged Local Transactions (N_{it-1})	.013	(.005)**
Social Capital (SC_i)	-.019	(.033)
Lagged Local Transactions $\times$ Social Capital ($N_{it-1} \times SC_i$)	-.002	(.003)
Race Diversity	.071	(.040)
Gini Coefficient	-.318	(.029)**
Average Income	.329	(.062)**
Education	.502	(.050)**
Target Population Density	.168	(.040)**
Local Offline Stores in Three-Digits Zip	-.232	(.092)*
Offline Spending on the Men's Clothing Category	.035	(.031)
Observations and Model Fits		
Number of Observations	20,790	
Log Likelihood	-9,846.2	
BIC	20,607.04	

Notes: * indicates that $p < .05$ and ** indicates that $p < .01$. The models include 41 period fixed effects and 29 two-digit zip fixed effects and all variables listed in Table 1.2. Estimates for the dummies and non-central control variables are not reported for ease of exposition but are available from the authors upon request.

Figure 1.2. The Estimated Significance of Social Learning

(a) Trial—Marginal Utility Increase
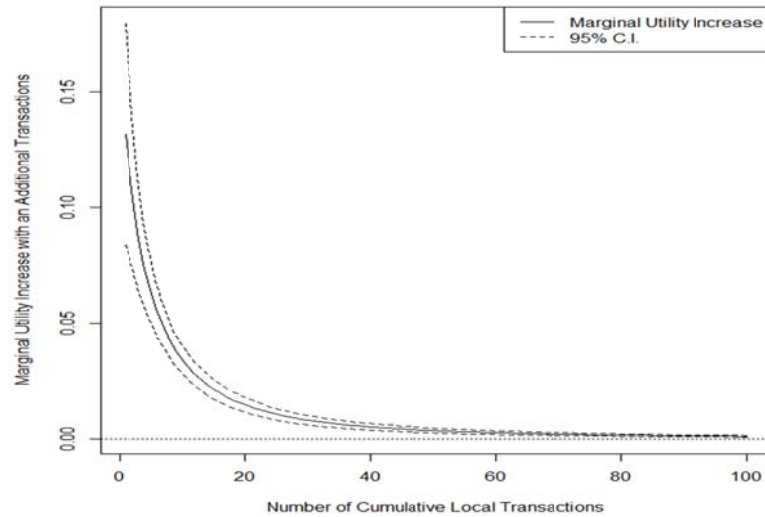
(b) Repeat—Marginal Utility Increase
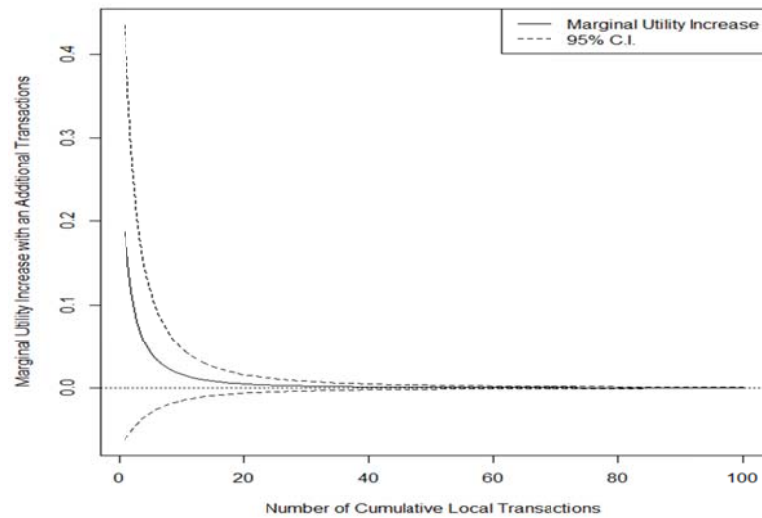
Notes: For Bonobos.com, the range of the cumulative number of transactions over all 20,790 observations (495 zips * 42 periods) is [0,525]. In Figure1.2, the range of x-axis is [0,100] for better visualization. The result in the rest of range is also consistent with what is shown here—a diminishing but *significantly* positive marginal utility gain for Figure 1.2a and a diminishing and *insignificantly* positive marginal utility gain for Figure 1.2b. Given the underestimation of initial quality (see Section 1.5.1), the observed diminishing marginal return to local transactions (N) is an assumption of the Bayesian learning model. It is consistent with the notion that a consumer observes "overlap" in each new piece of information, as s/he collects more information.

We quantify the economic value of social learning as the number of trials partly attributable to social learning on experience attributes, i.e., the number of actual triers minus the number who would have tried *without* the benefits of local social learning. This benchmark is computed as the number of new trials when the quality belief distribution does not update from the initial belief, all other parameters and variables held constant. We find that about 50% of trials (2,987 out of 5,745) are affected. This is consistent with a common practitioner belief; namely, that incomplete knowledge about experience attributes in general, and underestimation of product quality in particular, is a major barrier to trial. We demonstrate an important antidote: Information transferred locally from existing customers to potential customers helps to mitigate this problem.

Social capital as a moderator of social learning. The estimate of α_1 in Table 1.3 shows that social capital reduces the inefficiency in social learning ($\alpha_1 = -.20$; $p < .001$). In terms of magnitude, this implies that when social capital is *increased* by one standard deviation from the average, the inefficiency inherent in social learning (τ_i^2) will be brought down to about two-thirds of its original value (an approximately 50% increase in $1/\tau_i^2$). In Section 1.5.1 we reported that for an "average community" nine local transactions are required to accomplish this reduction; in neighborhoods that are one standard deviation above average in social capital, only six local transactions are required.

We quantify the economic value of social capital as the number of trials attributable to the efficiency of social learning, i.e., the number of actual triers minus the number who would have tried if the level of social capital were lowered by one standard deviation in all zip codes. (Alternatively, we can interpret economic value as the difference in new

trials between two zips that are exactly the same in all regards except one—they differ in

the extent of social capital by one standard deviation.) Our simulations show that about 8%

(438 out of 5,745) of the new trials were affected by the efficiency of social learning

process.

Control variables. Effects of the control variables are not of interest per se; we

document them to illustrate consistency with prior findings and provide additional face

validity for our main findings. The number of lagged local transactions (N_{it-1}) is

positively related to local demand ($\beta_3 = .01$, $p < .05$), perhaps a result of other contagion

mechanisms, time-varying spatial effects, or both. As expected, there is no main effect of

local social capital on local demand ($\beta_2 = -.19$, $p = .55$); social capital does not, per se,

increase trial, but operates only through the learning mechanism, which identifies the

effect.[20] New trials are higher in more densely populated areas ($p < .001$) perhaps due to

greater use of the Internet in such locations (Katona, Zubcsek, and Sarvary 2011), and in

locations where residents have more education and higher average incomes ($p < .001$ in

both cases). More offline stores reduces new trials at the online retailer ($p < .05$),

consistent with online-offline demand substitution (Brynjolffson, Hu, and Rahman 2009).

1.5.2. Falsification Tests

Falsification tests for the local social learning finding. The controls in Equation 1.9

notwithstanding, additional evidence that the learning process for experience attributes is

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[20] Moreover, as noted earlier and reported in Table 1.4 there is no evidence that the 1,014 zip codes "select" into those with buyers (495 zips) and those without (609 zips) on the basis of social capital stock. The absence of a main effect in Table 1.3 further affirms that social capital works not directly on sales, but indirectly through the specific mechanism of reducing inefficiency of information transfer among local residents.

not contaminated by other contagion mechanisms (e.g., awareness dispersion, normative pressure, etc.), or by temporal, spatial, and spatio-temporal effects, is helpful.

For that purpose, we perform a falsification test for social learning. The test relies on the premise that the Bayesian updating process on learning about experience attributes should *not* be significant when estimated on *repeat* transaction data where Bonobos.com consumers have been able to resolve their uncertainty about product quality in general via their first purchase.

To analyze repeat purchases we use the same model as before (Equation 111), but this time the dependent variable is the count of repeat customers. Since the number of consumers who can make repeat purchases are limited to those who have tried the website previously, the aggregate number of repeat transactions follows a Binomial rather than a Poisson distribution.

The pictures in Figures 1.2a (trial) and Figure 1.2b (repeat) are very different even though they represent an identical test for social learning about experience attributes. For trial (Figure 1.2a), the 95% confidence interval never contains zero, whereas for repeat (Figure 1.2b) it always does. In Figure 1.2a this is because the estimated difference between the initial belief (*pre-trial Q_0*) and the updated belief (*trial Q*) is highly significant as noted previously (see Table 1.3). Consumers have a positive update after trying the product. In Figure 1.2b, as expected, the estimated difference between the initial belief (*trial Q_0*) and the updated belief (*repeat purchase Q*) is not significant (*p* = .41). The finding is additional evidence that our model of social learning for experience attributes performs as it should—it does not find evidence of social learning when individual customers already direct experience with the product.

Falsification test for the moderating role of social capital. This falsification test is a subtle test of social capital measure itself.[21] The SCCBS asks respondents not only about trust and communication with neighbors but also about trust and communication with workplace colleagues (see Section 1.8.1 (Appendix)). Our proposed measure of social capital is defined using the questions about *neighbors* (see Section 1.3.2). Neighbors, by definition, live in the same zip code, whereas work colleagues need not. In fact, commute times and related data strongly suggest that they often do not.[22]

Hence, we define a new variable "workplace social capital" and re-estimate the model with this variable as a replacement for "neighborhood social capital". If the moderating effect of social capital really is about local information transfer, there should be no moderating effect of workplace social capital. As with the counterpart, neighborhood social capital, workplace social capital is a simple average of local scores on: (1) workplace trust (the average among related SCCBS survey questions such as "How much do you trust colleagues?"), and (2) workplace interaction frequency (the average among related SCCBS survey questions such as "How much do you socialize with your colleagues outside work?"). This measure captures the embedded-ness of relationships with colleagues among those who "live" in a specific zip code, not "work" in a specific zip code. Details are in Section 1.8.1 (Appendix).

We fit two models to demonstrate the test. First, we replace neighborhood social capital with workplace social capital and re-estimate the main model. When workplace

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[21] We are extremely grateful to an anonymous reviewer for suggesting this analysis.

[22] According to a 2011 OECD survey, the average commuting time per day in the U.S. is around 50 minutes. (http://www.economist.com/blogs/freeexchange/2011/10/surveys). It is therefore very unlikely that many US residents live and work in the same zip code.

social capital enters the model alone it does not enhance the efficiency of the local

neighborhood social learning process ($p = .06$). The corresponding effect for

neighborhood social capital reported in Table 1.3 is, on the other hand, highly significant

($\alpha_1 = -.20$, $p < .001$). Second, we include *both* variables in equation 1.7 and find that

neighborhood social capital moderates the local social learning process ($\alpha_1 = -.26$, p

$< .001$) whereas workplace social capital does not ($p = .38$).

1.5.3. Robustness Checks

Unobserved time-varying spatial effects. In Equation 1.9, we used lagged local

transactions (N_{it-1}) to control unobserved time-varying spatial effect (μ_{it}). While this is

in some respects a reasonable control, it is potentially incomplete in that we cannot be

fully assured that there is no *concurrent* demand shock in a specific zip code that is not

explained by past local transactions. To alleviate this, we would ideally find a proxy to

control concurrent demand shocks, but it is challenging to find such a variable for each

zip code every period. As an alternative we introduce a random component for the

unobserved time-varying spatial effect unexplained by lagged local transaction (η_{it}) and

specify Equation 1.9 as:

$$U_{ijt}^D = \beta_{0t} + X_i\beta_1 + SC_i\beta_2 + (\gamma_0 + \gamma_1 SC_i)N_{it-1} + (\delta_0 + \delta_1 SC_i)N_{it-1} + \eta_{it}. \qquad (1.13)$$

Note that Equation 1.9 is a special case of Equation 1.13 where there is no unobserved

time-varying spatial effect that is unexplained by past local transaction (i.e., $\eta_{it} = 0$).

We fit models with two different distributional assumptions for η_{it}. First, we assume

that:

$$\eta_{it} \sim IID \ N(0,\phi_\eta^2).$$ (1.14)

Under this assumption, we estimate a model with zip-period specific random effect. To estimate ϕ_η, we simulated 50 draws of η_{it} for each observation and integrated numerically when computing the likelihood. Under this relatively straightforward model of *IID* shocks, we found no significant effect of time-varying spatial elements that are unexplained by lagged local transactions ($p = .06$). Moreover, the substantive findings from our focal model are preserved.

In Equation 1.14, the *IID* assumption implies that a random shock has no influence on demand in a subsequent period, and all those carry-over effects are captured by lagged local transaction. To relax this assumption, we specify η_{it} as:

$$\eta_{it} = \rho\eta_{it-1} + \xi_{it}, \text{ where } \xi_{it} \sim IID \ N(0,\phi_\xi^2).$$ (1.15)

To estimate ρ and ϕ_ξ, we simulated 50 draws of ξ_{it} for each observation, computed entire vectors of η_{it}, and numerically integrated as before. There is evidence of significant concurrent effects of ξ_{it} if ϕ_ξ is significantly greater than 0, and carry over effects if ρ is significantly different from 0. In this more general specification, neither the concurrent ($p = .21$) nor carry-over effects ($p = .72$) of random shocks were significant. Again, the substantive findings while our key findings remain robust.

Spatially varying Q. In the main specification, we assume that previous triers agree on the quality of Bonobos.com products. If, however, there is any systematic difference in evaluation of Q, the assumption that signals are *IID* breaks down.[23] We relaxed this

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[23] Unbiasedness of signals is standard assumption. We assume that: (1) signals represent agreement about

assumption and fit two models where Q (now Q_i) is a function of observed

demographics. In the first model, *both* Q_i and τ_i are defined as functions of neighborhood

social capital, SC_i. The purpose of the specification is to show that the estimate of α_1 in

Table 1.3 is not confounded by spatially-varying Q_i over SC_i. We found that social

capital still significantly reduces signal variance ($\alpha_1 = -.21$, $p < .001$), but does not affect

Q_i ($p = .14$).

Next, we define Q_i as a function of three variables most likely to be related to the

evaluation of fashion items—population density, average income among target customers,

and offline spending in the category. Again, we found that social capital still significantly

reduces signal variance ($\alpha_1 = -.21$, $p < .01$) *even when* Q_i varies over density, average

income, and spending on the category ($p < .001$ in all three cases).

Our earlier findings in Table 1.3 are robust under spatially varying Q_i. In addition,

the BIC of main model reported earlier (20,607) is better than either of the alternative

models that allow Q_i to vary by location. (The respective values are $BIC_1 = 20,614$ and

$BIC_2 = 20,632$.)

Alternative specification of moderation. Equation 1.7 specifies inefficiency of

information transfer as a function of social capital only. The falsification tests in 1.5.2

notwithstanding, it is helpful to examine alternative specifications. From a conceptual

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quality with no systematic deviation, and (2) potential consumers believe that signals are unbiased. It is hard to test whether both assumptions hold or not. Conceptually, our findings are valid as far as (2) holds where Q becomes "perceived agreement" rather than "objective agreement" about quality. When (2) breaks down, our finding will be valid only when consumers know the direction and extent of systematic deviation, and Q becomes objective agreement after cancelling out systematic deviation.

perspective, previous purchases by local neighbors with demographics similar to those of potential customers, but with whom potential customers do not interact, should boost *neither* the observability of signals (ω_i) nor the richness of signals (θ_{vi}). What matters is the "embedded-ness" (Granovetter 1985) of relationships. When we allow signal variance to depend on racial diversity, income inequality, and social capital we find that social capital reduces inefficiency as before, ($\alpha_1 = -.20, p < .01$), but that diversity ($p = .41$) and income ($p = .12$) have no effect.

1.6. Summary

1.6.1. Key Findings

We began with the observations that information passed from existing to potential customers is a key driver of sales, and that information about *experience* attributes (which cannot be fully observable and verifiable pre-purchase) is important in reducing the uncertainty faced by potential customers. Moreover, the global consumer economy is driven increasingly by online commerce, such that information about experience attributes plays a critical and ever larger role in buying decisions. The top-line message from our research is that while firms can expend considerable resources to reduce consumer uncertainty about experience attributes, naturally occurring *customer-driven* processes, specifically interactions between existing and potential customers, could perform a similar role.

Drawing on existing conceptual frameworks and empirical studies, we proposed that: (1) local social learning is a specific mechanism for reducing uncertainty about experience attributes, and (2) the local social learning process is enhanced by

neighborhood social capital such that higher levels of social capital reduce inefficiency in the learning process. Both conjectures are supported from models estimated on data from Bonobos.com, a leading and iconic US online apparel retailer.

To our knowledge, our paper is the first in marketing to identify the proposed mechanism of social learning in this important context, and in addition, to demonstrate the novel moderating role of social capital. It is crucial to note that social capital does not, per se, influence trial of new products. It operates directly on the learning process itself, by reducing inefficiency in information transfer. In instances where consumers update favorably, e.g., in the case of Bonobos.com where initial beliefs underestimated true quality, more efficient information transfer will naturally help trials indirectly.

1.6.2. Actionable Insights, Limitations, and Future Research

Managers are of course well aware that existing customers are important sources of information and uncertainty resolution for potential customers, i.e., that "social learning" is a mechanism for information transmission about experience attributes in particular, even if they don't phrase it in exactly those terms. Nevertheless, the magnitude of this effect might be cause for surprise—we estimate that up to half of all Bonobos.com trials were affected by it.

Furthermore, the fact that neighborhood social capital reduces inefficiency is potentially actionable as well. While the SCCBS is extensive (over 30,000 respondents), it covers only just over 1,000 zip codes (there are more than 30,000 residential zip codes in the US; moreover, it may not be possible for managers to obtain the SCCBS from the Kennedy School.) To demonstrate the practical value of the social capital finding, we first conceived and obtained data on a proxy variable that is widely available.

As noted earlier, the Bonobos.com target customer is a "hip, semi-athletic, 25-to-40 year old guy". We sought a neighborhood-level proxy for the "potential for interaction" among such individuals and this led us to collect data on the number of bars and liquor shops per capita per zip code, for all 495 zip codes in our data (these data can be obtained manually via the Internet, or, as we did, from a professional supplier such as ESRI). This proxy is suitable because individuals are not usually alone (or, at least, not exclusively!) when they drink liquor. Most likely, they are with friends or neighbors watching sports, celebrating birthdays, having parties, and so on. Likewise, local bars are places where people, especially males, socialize with neighborhood residents.

Therefore, we expect that the number of bars and liquor shops is a reasonable proxy for embedded-ness of local relationships and interaction frequency among local neighbors. Consistent with this expectation, the correlation between the neighborhood social capital measure from the SCCBS and the number of bars and liquor shops per capita is significantly positive ($\rho = .32, p < .001$). Of course, as we found with our falsification test using workplace social capital, we would not expect the bars and liquor store variable to be significant in a model that also included the true neighborhood social capital measure.

First, we fit a model where neighborhood social capital is replaced with the "local bars and liquor shops" variable. Like neighborhood social capital, this variable does enhance the efficiency of social learning process ($p < .05$). Next, we included *both* the neighborhood social capital variable *and* the local bars and liquor shop variable into the model. In this case, the local bars and liquor shop variable loses its significance ($p = .80$)

while neighborhood social capital remains significant (α_l = -.20, $p < .05$) as before. These findings imply that the local bars and liquor shop variable, which is conceptually related to embedded-ness of relationships—especially among males in the target segment—is a proxy for neighborhood social capital in our context. More generally, managers could act on the "social capital finding" by looking for observed local characteristics that suit their own product context (e.g., number of churches, gyms, or cooking clubs, etc.), and use it as a proxy for the extent of offline social relationships that are product-relevant. In locations with better and more frequent interaction among constituents, information transfer will be more efficient, which is of course desirable when firms have valued products.

The limitations of our study suggest future research directions. First, we focus on social learning on vertical quality only, but social learning on horizontal fit is important too—especially for experiential goods. Second, we controlled time-varying spatial effects using both the trend captured by past purchases and alternative error structures for concurrent demand shocks. Alternative methods (perhaps natural experiments) with other exogenous controls on time-varying spatial effects would be helpful in further establishing the implied casual relationships in our work. Third, we focus exclusively on the identification of social learning only; one could of course explicitly separate other social contagion mechanisms such as awareness dispersion, and attempt to determine the relative importance of each.

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1.8. Appendix

1.8.1. Measures from the SCCBS

Neighborhood social capital. The following survey question is used to construct the neighborhood social trust score.

- How much can you trust neighbors?
  1. Trust not at all.
  2. Trust only a little.
  3. Trust some.
  4. Trust a lot.

The following survey questions are used to construct the local interaction frequency score.

- How often did you interact with your neighbor within last twelve months?
- How often did you have friends over to your home within last twelve months?
- How often did you hang out with friends in a public place within last twelve months?
  1. Never did this
  2. Once
  3. A few times
  4. 2-4 times
  5. 5-9 times
  6. About once a month on average
  7. Twice a month
  8. About once a week average
  9. More than once a week.

SCCBS data include two versions of variables for each question, the raw score and standardized score in the local community (zip code). For each question, we use the local average of standardized scores to construct social trust and interaction frequency scores. We operationalize neighborhood social capital as the average between neighborhood trust and interaction frequency scores.

Workplace social capital. The following survey question is used to construct the workplace social trust score.

- How much can you trust co-workers?
  1. Trust not at all.
  2. Trust only a little.
  3. Trust some.
  4. Trust a lot.

The following are survey questions to construct local interaction frequency score.

- How often did you socialize with co-workers outside of work within last twelve months?
  1. Never did this
  2. Once
  3. A few times
  4. 2-4 times
  5. 5-9 times
  6. About once a month on average
  7. Twice a month
  8. About once a week average
  9. More than once a week.

For each question, we operationalize workplace social capital as the average between workplace trust and interaction frequency scores.

1.8.2. Zip Codes With and Without Customers

The SCCBS data cover 1,104 zip codes and since the purpose of our research is to understand how information from a *previous* trial influences potential subsequent *first* trials by local neighbors, we focus on 495 zips with at least one customer within the 42 month period after the site launched. Since the observation period is quite long—three and a half years—it's possible that the 609 (1,104 – 495) zips with *no* trials at all could be different from the 495 zips used in estimation. To check and document these differences, we estimate a binary probit of the probability of at least one trial, using data from all 1,104 zips. The results are in Table 1.4.

Significant effects for some control variables are to be expected; indeed, there is higher probability of least one Bonobos.com customer in zip codes with a more educated population and in those where residents spend more on men's clothing. Most important however, is that zip codes *do not* sort on our key independent variable, neighborhood social capital. The estimate is not significantly different from zero ($p = .29$). We thank an anonymous reviewer for suggesting this analysis.

Table 1.4. Probit Estimates - The probability of at least one customer in a zip code

	Model Estimates	Standard Error
Estimated Parameters		
Intercept	-.219	(.468)
Social Capital (SC_i)	.063	(.060)
Race Diversity	.609	(.372)
Gini Coefficient	-7.216	(1.382)**
Average Income	-.355	(.297)
Education	.794	(.180)**
Target Population Density	.102	(.112)
Local Offline Stores in Three-Digits Zip	.033	(.109)
Offline Spending on the Men's Clothing	1.388	(.554)*
Observations and Model Fits		
Number of Observations	1,055	
Log Likelihood	-428.5	
BIC	1,198.1	

1.8.3. Analyses with 1,104 zip codes

To check the robustness of our finding, we also fitted a main model with the entire 1,104 zip codes. As we found in our main model (where we focus on 495 zips with at least one customer within the 42 month period), local social learning is a significant driver of sales. We also found that social capital reduces signal variance ($\alpha_1 = -.11$), but

not significantly ($p = .22$). The impact of social capital on learning process might have lost its significance because 609 zips with no trial for three and a half year have systematically different preferences from 495 zips with at least one trial. People in 609 zips may have preferences strongly against Bonobos.com no matter how much information they have, and such preferences cannot be fully captured by observable zip characteristics, 2-digit level fixed effects, or lagged number of transaction. Therefore, it is likely that the learning model is confounded with other unobservable factors when we fitted a model with 1,104 zips. In our main model, we rule out the zip codes which potentially have extreme preferences, so we can better understand the impact of social learning and the role of social capital.

ESSAY 2: SOCIAL CONTAGION IN NEW PRODUCT TRIAL AND REPEAT

2.1. Introduction

How new products gain market acceptance is of key interest to marketers. The notion that adoption or trial can be affected by peer influence or social contagion is well accepted. Having customers try a new product, however, does not mean that they will keep using it and that the product will gain market acceptance. Marketers seek not only trial but also sustained use or repeat purchases. Research on how social contagion helps new products gain market traction, however, focuses almost exclusively on adoption or trial.

So, several important questions remain unanswered. Can social contagion affect not only trial but also repeat behavior? If so, are those who influence others to adopt the same as those who influence others to repeat? i.e., are the same customers influential in both trial and repeat, or should marketers seek to leverage different customers to support trial versus repeat? And what about differences in susceptibility to social influence? i.e., are those who are the most influenceable at trial also the most influenceable at the repeat stage? Finally, if contagion operates differently at each stage, can we gain some insights about why this happens?

The presence of social contagion in repeat may appear a bit puzzling. Why would adopters' subsequent behavior be affected by peers, since adoption provides the opportunity to learn directly about the product's advantages and disadvantages? Theory and empirical evidence suggest four reasons. The first is that social contagion can result from both informational and normative peer influence (e.g., Deutsch and Gerard 1955).

Whereas one expects informational influence to decline as customers proceed from trial to repeat, theory and empirical research provide no basis for normative influence to decline—some work even implies the opposite (e.g., Tolbert and Zucker 1983). The second reason is that informational influence need not be limited to trial but may affect repeat as well. When learning about product quality from personal experience is slow, customers may rely on peers as a source of information not only for trial but also repeat decisions (e.g., Dulleck and Kershbamer 2006). The third reason is that for products and services where interconnectivity or standardization is important, the utility of use increases with the number of other users, such that contagion affects not only adoption but also repeat or churn (e.g., Haenlein 2013; Nitzan and Libai 2011). The fourth reason is that environmental shocks can raise new doubts about an accepted product, making repeat users again susceptible to informational influence from peers, as suggested by Nair et al. (2010).

Investigating social contagion in trial versus repeat can provide new insights that are both theoretically and managerially valuable. Three benefits stand out. First, who the influentials are, who the influenceables are, and how that varies across trial and repeat matters to marketers keen on leveraging social contagion to help their products gain market acceptance. Who should they seek for leverage at trial versus repeat? Who can they afford not to target with costly resources, and does that change from trial to repeat?

Second, research focusing exclusively on trial provides only limited insights into what drives new product acceptance. This is especially so for three types of products. For consumables and services where trial purchases account for only a small fraction of customer lifetime value and overall product profitability, managers need to know what

drives trial as well as repeat (Gielens and Steenkamp 2007; Shih and Venkatesh 2004). For credence goods and complex innovations that generate uncertainty or ambiguity for their users even after trial, managers need to understand how these post-adoption sentiments operate so they can prevent them from becoming hurdles to repeat (Wood and Moreau 2006). For products and technologies targeted towards professionals and business users, managers need to understand how intra-organizational factors affect the sustained implementation of innovations (Downs and Mohr 1976).

Third, similarities or differences in who is most influential and influenceable at trial versus repeat may provide insights into the nature of the contagion mechanism(s) at work—a key research priority (e.g., Aral 2011; Godes 2011; Iyengar et al. 2011b; Lewis et al. 2012; Libai et al. 2010). Recent work has documented systematic variations across customers in influence and susceptibility, but has done so only in the realm of new product adoption (e.g., Aral and Walker 2012; Goldenberg et al. 2009; Hu and Van den Bulte 2014; Iyengar et al. 2011a; Katona et al. 2011) or outside the realm of new products altogether (e.g., Godes and Mayzlin 2009; Trusov et al. 2010). Studying social contagion in both trial and repeat provides the opportunity to assess the effect of peer behavior on two different dependent variables. This in turn enables one to more sharply identify the nature of the contagion mechanism(s) at work (Oster and Thornton 2012).

We investigate the presence and nature of contagion in trial versus repeat by studying the acceptance of a new prescription drug by physicians. Our study combines individual-level trial and repeat data, social network data, survey data, and individual-level sales call data.

There are three novel findings. First, we find evidence of contagion in both trial and repeat. Second, who is most influential varies across stages. Physicians who are central in the network of discussion and referral and who prescribe the new drug heavily drive the contagion at the trial stage (as found in an earlier analysis of the same drug), but they do not drive contagion at the repeat stage. Instead, repeat prescriptions are affected by the behavior of immediate colleagues, only some of whom are also discussion/referral partners. Third, who is most influenceable also varies across stages. For trial, it is physicians who do not see themselves as opinion leaders (consistent with prior analysis). For repeat, in contrast, it is physicians in the middle of the status distribution as measured by network centrality.

Observing contagion operates in very different ways across trial and repeat suggests that different mechanisms are at work at each stage. Specifically, the moderator effects in each stage as well as the contrast across stages is consistent with informational influence reducing risk in trial and normative influence increasing conformity in repeat. Hence, this study answers recent calls to move research from whether contagion is at work to how and why it is at work (Aral 2011; Godes 2011). In addition, our evidence of a non-monotonic status effect extends recent insights into how status considerations affect customer behavior (Hu and Van den Bulte 2014).

Our findings are also relevant to managers, as they suggest that marketers should consider leveraging peer influence not only to trigger adoption, but also to support subsequent repeat—at least for risky products like the one studied here. Also, marketing policies to leverage contagion should be designed and targeted differently, since who is most influential and who is most influenceable varies across stages. Finally, the results

suggest that marketers of products like the one we study may want to emphasize different motivations—perceived risk versus conformity to local norms—in their sales calls and other marketing communications targeted towards prospects vs. adopters.

We proceed by first further developing the research questions, building on theories and findings from psychology and sociology. We next describe the research setting, data, and modeling approach. We then present the findings and discuss their implications for theory, research, and practice.

2.2. Research Questions

Though social contagion and trial-repeat behavior have both long been the object of active research, and though studying them jointly would provide three important benefits, there is virtually no research of this kind to build on. So, we rely mostly on theoretical arguments to develop our research questions.

We first very briefly describe marketing research on trial vs. repeat. We then discuss informational and normative influence as two distinct contagion mechanisms. This provides the basis for refutable hypotheses on how and why contagion operates differently in trial vs. repeat.

2.2.1. Prior Research on Trial vs. Repeat

Prior research on social contagion focuses only on adoption or does not discriminate between trial and repeat. Similarly, to the extent that new product research has studied repeat behavior, it has done so without considering contagion.

Modeling trial-repeat behavior has a long history in marketing (e.g., Parfitt and Collins 1968). However, such work is typically conducted in packaged goods categories

for which social contagion was until recently believed not to matter much even at the trial stage because of low functional, financial, and social risk (Du and Kamakura 2011). As a result, empirical studies of this kind have not provided insights into contagion dynamics.

Aggregate-level diffusion modeling also has a long history. Several studies of this kind distinguish between trial and repeat sales, but do not investigate contagion in each stage either (e.g., Hahn et al. 1994).

Diffusion researchers have also investigated whether initial deployment or trial of new technologies by organizations is driven by different factors than subsequent deployment within those organizations. However, studies contrasting "inter" and "intra" firm diffusion do not investigate social contagion dynamics (e.g., Levin et al. 1992).

2.2.2. Informational versus Normative Influence

Peer influence leading up to social contagion in customer behavior can be both informational and normative (e.g., Bearden et al. 1989; Deutsch and Gerard 1955, Turner 1991, pp. 34-39). Informational influence occurs when information obtained from peers serves as evidence about reality and so changes one's beliefs about the true state of the world. Normative influence arises from the desire to conform to the expectations of others about what is the right and proper thing to do.

The notion of social contagion through informational influence, affecting awareness or beliefs about products' risks and benefits, is quite familiar to marketing scientists. The notion of contagion through normative influence is less so, and two important characteristics need to be borne in mind.

First, normative influence is fundamentally a group phenomenon (Deutsch and Gerard 1955; Hogg 2010; Turner 1991, p. 37). Social norms are rules and standards that

are understood, endorsed and expected by members of a group (Cialdini and Trost 1988) and, consequently, conformity to norms is fundamentally a group rather than interpersonal process.

Second, normative influence can be of two types (Bearden et al. 1989; Kelman 1958, 2006; Scott 1996, p. 96; Turner 1991, pp. 39, 117-118): compliance based on others' power to mediate rewards and costs, and identification based on the concern to live up to others' expectations of one's role. Whereas compliance requires public observability and monitoring so persons can be rewarded or punished depending on whether they act in accordance to the norm, identification requires that the persons care about maintaining a positive relationship with other members of their group (Kelman 1958; Turner 1991, p. 117). Whereas compliance operates mostly through reward and coercive power, identification operates mostly through referent power (Warren 1968).[24] Whereas compliance is about adhering to rules, identification is about enacting roles based on others' expectations (Kelman 2006).

2.2.3. Informational Influence in Trial versus Repeat

Trial of new products, especially those presenting substantial risk, can be subject to social contagion through informational peer influence. Evidence that contagion increases with

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[24] Consequently, both theory (e.g., Bicchieri 2006, pp. 11 and 42-44) and empirical research (e.g., Cialdini et al. 1990) imply that the actions of any *specific* people need not be observed for norms to operate. Actions of specific influencers need not be observed because people can form normative expectations, i.e., beliefs about what others expect them to do, without directly observing the actions of any specific person. E.g., people can infer from the presence of litter on the ground that littering is socially acceptable even if they do not see any specific person littering (Cialdini et al. 1990). Actions of influencees need not be observed either. Though normative influence through compliance involving rewards and punishment requires that others can observe one's actions, public observability is not required for normative influence through identification, as the latter involves only one's own assessment of how well one meets others' expectations.

the sources' credibility, experience or expertise and that it decreases with the decision makers' self-confidence in their judgment indicates that contagion stems from informational influence (e.g., Deutsch and Gerard 1955; Kelman 1958; Iyengar et al. 2011a).

Informational influence is less likely to affect repeat decisions, as personal consumption experience substitutes for input from peers. Hence, a contagion effect that is larger in trial than repeat would be quite consistent with informational influence. Yet, some peer influence may be at work in repeat when learning from experience is slow. For instance, whereas physicians can quickly learn about the effectiveness of drugs used to treat acute conditions with easy to observe symptoms, this is not so for drugs used for chronic conditions that are hard to monitor. Learning from personal experience can be slow even for such simple products and services as laundry detergents and mobile phone service (e.g., Iyengar et al. 2007). Hence, for risky products with slow experiential learning, some customers may rely on the judgment of peers even when making repeat decisions (Dulleck and Kershbamer 2006).

In short, for risky new products, informational influence considerations lead one to expect that social contagion (i) is at work in trial, (ii) originates from trusted peers, (iii) is lower for people confident in their judgments, and (iv) operates with greater strength in trial than repeat. As a corollary, a contagion effect with characteristics (i)-(iv) is more likely to stem from informational influence than one without these characteristics.

2.2.4. Normative Influence in Trial versus Repeat

The acceptance of innovations can be subject to social contagion through normative influence (e.g., DiMaggio and Powell 1983; Van den Bulte and Stremersch 2004). Since

norms are endorsed and expected by members of a group, customers are more likely to

experience normative influence from group members than from outsiders, even those

with experience or expertise (Deutsch and Gerard 1955; Turner 1991, pp. 117-118). This

suggests that informational and normative influence may very well stem from different

sources (e.g., experts versus family members or colleagues).

The extent to which customers conform to social norms is likely to vary by status, i.e.,

their social rank in terms of esteem and respect. Customers with very low status have

little to lose from not conforming and little to gain from conforming. Whether they

conform or not simply does not affect them very much (Dittes and Kelley 1956; Harvey

and Consalvi 1960). The same holds for customers with the highest status. They gain

little additional esteem from adhering to group norms and are given greater latitude than

others to deviate from group norms (Hollander 1958). Consequently, it is customers in

the middle of the status distribution who have the greatest tendency to conform to norms,

a pattern referred to as middle-status conformity and documented in adoption studies by

Phillips and Zuckerman (2001) and Hu and Van den Bulte (2014). Along similar lines,

Bosk (2003, p. 75) describes how physicians of middle status experience the most

pressure to adhere to their surgical ward's local norms.

In contrast to informational influence, there is little theory or empirical research

suggesting that the susceptibility to normative influence declines as customers proceed

from trial to repeat. Rather, the opposite is likely. The first reason is that adopters' desire

to appear legitimate by conforming to normative expectations increases over the diffusion

process, as several studies suggest. Whereas early adoptions are affected mostly by

technical and performance considerations, the evidence suggests, later behavior is

increasingly affected by the concern to appear legitimate (Kennedy and Fiss 2009; Tolbert and Zucker 1983; Westphal et al. 1997). The mechanism posited to be at work is that, as time progresses, products and practices are increasingly evaluated using a "logic of social appropriateness" rather than a "logic of instrumentality" (Westphal et al. 1997, p. 374). This shift is similar to that in Maslow's hierarchy of needs: As one feels that basic functional requirements are met, social acceptability and integration become more important considerations. To the extent that customers similarly shift some emphasis from functional performance to social acceptability after adoption, repeat use and sustained implementation should be more affected by normative concerns than initial use. The second reason to expect susceptibility to normative influence to increase as customers proceed from trial to repeat is that social disapproval based on deviations from the norm are easier to condone for trial than for repeat. Normative disapproval of a trial decision can easily be deflected by claiming exigent circumstances (when proven right) or by showing contrition and desisting (when proven wrong). These tactics, however, are not available to someone who violates norms of proper behavior repeatedly (Bosk 2003, pp. 35-70).

Note that the two reasons to expect the susceptibility to normative influence to increase as customers proceed from trial to repeat are of a different nature. The first does not pertain to a genuine difference between trial and repeat but to a change over time in how much people care about conforming to social norms. So, the difference across stage is merely a corollary of a temporal effect. The second reason pertains to a genuine difference between trial and repeat, regardless of time since launch.

In short, normative influence considerations lead one to expect that social contagion (i) is at work in repeat, (ii) originates from group members, (iii) varies in an inverse-U fashion with the decision maker's status, and (iv) operates with greater strength in repeat than trial. As a corollary, a contagion effect with characteristics (i)-(iv) is more likely to stem from normative influence than one without these characteristics.

2.2.5. Hypotheses

The theoretical arguments lead to four predictions for risky products:

H1. New product adoption is affected by social contagion that originates from trusted peers, and people with low confidence in their judgments are more susceptible to it.

H2. Social contagion that originates from trusted peers and that is negatively moderated by the recipients' self-confidence is more pronounced in trial than in repeat.

H3. New product repeat behavior is affected by social contagion that originates from group members, and people with middle-status are more susceptible to it.

H4. Social contagion that originates from group members and that is non-monotonically moderated by the recipients' status is more pronounced in repeat than in trial.

Two observations are in order. First, the hypotheses are based on the assumption that contagion in adoption is driven mostly by informational considerations whereas contagion in repeat is driven mostly by normative considerations. Support for the hypotheses would provide credence to this underlying assumption, but does not provide direct evidence of the informational or normative nature of contagion. This is not a major limitation, as theoretical mechanisms are typically inferred from their observable consequences rather than observed directly even in experimental research. Second, the

hypotheses go far beyond basic main effects. This makes it hard to find credible alternative explanations for the data in case the hypotheses are supported.

2.3. Strengthening Internal Validity in Contagion Studies

Obtaining good estimates of an effect is rarely straightforward in non-experimental studies. Whereas observational designs do not offer the same level of internal validity as randomized field experiments (e.g., Aral and Walker 2012; Hinz et al. 2011), researchers have found many ways to strengthen the internal validity of observational contagion studies.

2.3.1. Temporal precedence

One way is simply to be mindful that causes precede effects, and to plan one's study accordingly. For instance, one can avoid simultaneity bias by using panel data with sufficiently fine temporal resolution and by modeling contagion in terms of lagged rather than contemporaneous peer behavior. As another example, one can avoid endogenous tie formation and truncation biases by not operationalizing contagion in terms of social ties that can come into existence only after the adoptions that one seeks to explain have occurred.

2.3.2. Technical Fixes

The second way to boost the internal validity of contagion research consists of using one or more of the standard approaches to strengthen causal inference in observational designs. These include studying acyclic networks to avoid simultaneity bias (e.g., Iyengar et al. 2011a), using covariates or fixed effects to control for common contextual effects and attributes (e.g., Nair et al. 2010; Van den Bulte and Lilien 2001), using matching

techniques to do the same (e.g., McShane et al. 2012), using instrumental variables to capture exogenous variations in contagion (e.g., Land and Deane 1992), and jointly modeling ties and behavior to account for endogenous tie formation (e.g., Lewis et al. 2012).

2.3.3. Theoretical Elaboration

The third way to more confidently identify contagion is theoretical elaboration. The idea is conveyed in an anecdote involving two eminent statisticians, R.A. Fisher and W.G. Cochran.

> "About 20 years ago, when asked in a meeting what can be done in observational studies to clarify the step from association to causation, Sir Ronald Fisher replied: 'Make your theories elaborate.' The reply puzzled me at first, since by Occam's razor, the advice usually given is to make theories as simple as is consistent with known data. What Sir Ronald meant, as subsequent discussion showed, was that when constructing a causal hypothesis one should envisage as many *different* consequences of its truth as possible, and plan observational studies to discover whether each of these consequences is found to hold." (Cochran 1965, p. 252, emphasis in original)

The idea, in essence, is that more elaborate predictions cannot be accounted for as easily by threats to internal validity. As Shadish, Cook and Campbell (2002, p. 105) note, "The more complex the pattern that is successfully predicted, the less likely it is that alternative explanations could generate the same pattern, and so the more likely it is that the treatment had a real effect." Shadish et al. call this method of strengthening internal validity "coherent pattern matching" whereas Rosenbaum (2002, pp. 209-214) calls it "increasing the specificity of predictions."

Such theoretical elaboration often entails putting forward boundary conditions and moderator effects (e.g., Cochran 1965; Shadish et al. 2002, p. 105). Consumer psychologists and other laboratory researchers have made this notion central to their

research strategy. Even when using randomized experiments, they put greater confidence in results supporting moderator predictions than basic main effects. For instance, a moderator effect limits the set of possible confounds to only those that would generate the same pattern, e.g., only omitted variables that are similarly moderated.

Theoretical elaboration may also increase causal confidence by positing non-monotonic effects, Cochran (1965) notes. For instance, a predicted non-monotonic effect rules out monotonic confounds as threats to validity.

Theoretical elaboration may also involve positing that a specific cause has an effect on one outcome variable but not another. Threats to internal validity in such "nonequivalent dependent variables" designs are less plausible when purported confounds are expected to affect all dependent variables but one observes only responses on those outcomes consistent with one's theory (Rosenbaum 2002, pp. 209-213; Shadish et al. 2002, pp. 110-111). Though specificity of outcome does not guarantee the causal nature of associations in observational designs, it makes potential confounds common across outcomes less likely and so strengthens the evidence of a causal connection (Hill 1965; Holland 1986).

Our hypotheses follow R.A. Fisher's dictum, as they involve different dependent variables, different sources of contagion, different moderators, and a non-monotonic pattern. This allows us to be more confident that the analysis detects genuine effects. Before we proceed with the empirics, a brief clarification about the role of informational vs. normative influence in our application may be in order. We use the experimentally documented theoretical distinction between informational and normative influence to motivate non-obvious hypotheses involving (i) different dependent variables, (ii)

different sources of contagion, (iii) different moderators, and (iv) a non-monotonic

pattern. The distinction between informational and normative influence is *a means* and

not the end in our application of R.A. Fisher's insight. Accordingly, the hypotheses are

stated in terms of observables rather than informational vs. normative influence, and

support for the hypotheses provides indirect credence but not direct evidence of the

informational vs. normative nature of contagion.

2.4. Research Setting

We analyze the acceptance of a risky new prescription drug over a 17-month period,

studied earlier by Iyengar, Van den Bulte and Valente (2011a), hereafter referred to as

IVV. We extend that earlier work by investigating (i) both trial and repeat[25] and (ii)

contagion from both trusted expert peers and immediate colleagues.[26]

The drug is used to treat a chronic viral infection that can cause severe damage to

internal organs and—if left untreated—sometimes even lead to patients' death. Physicians

cannot observe drug efficacy quickly and adjust a patient's therapy if necessary. Also,

there is uncertainty in the medical community regarding the best treatment because there

is no compelling evidence about the new drug's long-term efficacy compared to that of

two older drugs. In such situations characterized by high risk, high complexity and low

observability of results, potential adopters are likely to turn to opinion leaders for

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[25] We exclude refill prescriptions from the repeat data. So, the repeat events we study involve the physicians writing a new prescription.

[26] Hypothesis H1 was already documented by IVV using the same data but omitting immediate colleagues as a distinct source of contagion. Though our evidence in support of H1 is hence a robustness check of IVV's earlier finding rather than truly new evidence, H1 is part of our broader aim to document differences in social contagion between trial and repeat posited in H2.

guidance (Hahn et al. 1994).[27]

Social contagion may also be at work after trial. The first reason is the physicians cannot quickly assess the drug's efficacy even after having prescribed it. The drug treats a chronic rather than acute condition which is mostly asymptomatic until the patient is gravely ill. Not only do patients not feel whether the treatment is working or not, but even physicians have difficulty assessing improvements in patient health. They can only do so using indirect indicators, such as viral loads. Moreover, even if the treatment is effective, progress occurs only very slowly. All this makes the product's effectiveness with one's patients difficult to assess. The effectiveness of the focal drug compared to its two established competitors is ambiguous as well. Even large-scale clinical trials with strict test/control conditions provide far from definitive evidence of long-term superiority. Considering how difficult it is for physicians to gain much conclusive information from experience, it is possible that they rely on their peers' judgment even after trial.

The second reason that contagion may affect repeat behavior is that physicians want to act in a way that their peers deem proper and legitimate. Physicians look to their peers for information as well as normative guidance (Bosk 2003, pp. 35-70; Prosser and Walley 2006). Normative influence is likely to be stronger in repeat than in trial decisions and to vary as a function of status, something which is quite salient among physicians (Bosk 2003, pp. 36-67, 111-146; Menchik and Meltzer 2010) and can affect their prescription behavior (Burt 1987; Menzel 1957).

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[27] The severity of the medical condition and the limited observability of effectiveness also make willful experimentation on patients by forward-looking physician quite unlikely (Chintagunta et al. 2012, pp. 807-808).

Physicians who are influenced by the normative expectations of their colleagues do not necessarily make medically suboptimal choices that jeopardize the lives of their patients in order to look good. Such cynicism would be misguided in our research setting where which treatment option was medically optimal was far from clear-cut. When faced with such ambiguity, acting in ways that fellow medical professionals deem proper and legitimate is medically reasonable.

2.5. Data

The data cover the adoption and repeat prescriptions of the new drug by physicians in Los Angeles (LA), New York City (NYC) and San Francisco (SF) over a period of 17 months from the time of launch time. As the drug was the third entry in its category, the relevant population within each city was defined by the firm as every physician who had prescribed at least one of the other two drugs in the two years prior to the focal drug's launch.

The data consists of (i) monthly physician prescription data (excluding refills), (ii) answers to a survey by physicians providing information on discussion and patient referral ties, self-reported opinion leadership, and several other physician characteristics, (iii) the address where each physician practiced, and (iv) company records on sales calls to each physician.

2.5.1. Prescription Data

For each physician within the network boundary (not only survey respondents), the time of adoption is measured using monthly individual-level prescription data from IMS Health. Of the 193 doctors who responded to the survey, 68 or 35% adopted within 17

months. The average prescription incidence rate after adoption, or monthly repeat rate for short, is around 75%.

2.5.2 Discussion and Referral Ties

A mail and Internet survey was administered to all physicians in the network boundary. The survey asked the respondents to name up to eight physicians with whom they felt comfortable discussing the clinical management and treatment of the disease for which the drug was developed (discussion ties) and up to eight physicians to whom they typically refer patients with the disease (referral ties). Both lists could but did not need to overlap. The highest number of discussion partners nominated by any physician was 6 and that of referral partners was 5. Both these values are below the maximum number of nominations allowed. The survey was administered in SF several months before the product launch, but in LA and NYC 10 months after the launch. This exogenous variation helps us address threats to internal validity.

67 of the 150 physicians in the population of interest in SF responded. 57 out of 197 did in LA, and 69 out of 284 in NYC. As discussed in detail by IVV (see also Christakis and Fowler 2011), there is no evidence of non-response bias and the 24%-45% response rates avoid sizable error in the network-based covariates introduced below.

The study restricts the relevant networks to physicians practicing in the same city. The importance of local as opposed to national opinion leaders is well documented in the medical literature and the pharmaceutical industry is keenly aware of the importance of such social dynamics at the local level (e.g., IVV 2011a; Liu and Gupta 2012). So, physicians who were nominated by survey respondents but were not part of the population of interest were excluded from the study. Physicians who were part of the

population of interest but did not respond to the survey, in contrast, were included in the set of potential discussion or referral partners. A physician who is mentioned as both a discussion and a referral partner is deemed twice as influential as another who is mentioned as only one or the other. Contagion over this total network describes the pattern of adoption better than contagion over only discussion or referral ties (IVV 2011a).

2.5.3. Immediate Colleagues

Normative influence is more pronounced among individuals forming a group, and norms often operate locally (Bosk 2003, pp. 51-67; Cialdini and Trost 1998; Deutsch and Gerard 1955; Hogg 2010; Turner 1991). Consequently, immediate colleagues one interacts with daily are likely to exert normative influence through identification. They help define the local norm of what is legitimate practice, and the desire to maintain a satisfactory relationship with one's colleagues motivates people to conform to their expectations.[28]

We use the group practice or hospital where each physician works to identify his or her immediate colleagues. Physicians do not consider each and every of their colleagues a trusted expert on the medical condition treated by the new drug. As shown in the top row of Table 2.1, physicians in SF report on average only 9% of their colleagues for discussion and only 5% for referral regarding this specific medical ailment. The numbers for New York and Los Angeles are even lower. However, controlling for the fact that

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[28] Given our research setting of U.S. physicians making treatment decisions for a potentially lethal medical condition, we expect normative influence to operate through identification and referent power, not through compliance and coercive/reward power. Though the experiments of Deutsch and Gerard (1955) focused on the latter process, the importance of the former is now well documented and accepted (e.g., Kelman 1958, 2006; Turner 1991, p. 37).

there are many more non-colleagues than colleagues available, physicians are

significantly more likely to turn to colleagues than to non-colleagues for discussion or

referral ($p < .01$).[29]

Table 2.2 reports what fraction of referral and discussion ties involves colleagues.

Once again, the evidence is clear that the peers one turns to for discussion or referral

regarding the ailment treated by the drug are rarely one's immediate colleagues.

Table 2.1. Fraction of all Colleague-Dyads that Involve a Discussion or Referral Tie

	San Francisco (SF)	Los Angeles (LA)	New York (NYC)
Discussion	.086	.038	.067
Referral	.049	.026	.017

Table 2.2. Fraction of all Discussion and Referral Ties that Involve Colleagues

	San Francisco (SF)	Los Angeles (LA)	New York (NYC)
Discussion	.170	.042	.176
Referral	.139	.046	.058

2.5.4. Contagion Variables

We model social contagion as the effect of exposure to others' use of the drug, and do so

using lagged endogenous autoregressive terms. The extent to which physician i is

exposed at time t to *influence from discussion and referral partners* is captured through

the term $\sum_j w_{ij1} q_{jt-1}$ where w_{ij1} captures how relevant each physician j is to i for

discussion or referral (0, 1, 2), and q_{jt-1} is the number of prescriptions written by j at time

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[29] Standard test procedures like a chi-square test on a 2-by-2 matrix (presence or absence of tie vs. colleague or not) do not properly handle the lack of independence among the dyadic observations. We resolve that problem by regressing the sociomatrix of discussion/referral ties on the sociomatrix of collegial ties (OLS is unbiased even when errors are not independent) and using the permutation-based quadratic assignment procedure for assessing statistical significance (Krackhardt 1988).

t-1. The volume-weighted contagion from discussion and referral partners captures exposure to risk-reducing information. The more a physician's network contacts have prescribed the drug recently, especially in high volumes, the more credible their input is and hence the more confident the physician feels that using the drug may help her own patients (IVV 2011a).

The extent to which physician *i* is exposed at time *t* to *influence from immediate colleagues* is captured through the term $w_{ij2}s_{jt-1}$ where w_{ij2} equals 1 if *i* and *j* are colleagues and zero otherwise, and s_{jt-1} is the share at time *t*-1 of the new drug in *j*'s total number of prescriptions in the category. Though we use volume-weighted contagion from immediate colleagues in our robustness checks, we prefer using the share-weighting based on theoretical grounds. As Turner (1991, p. 87) notes, intrapersonal consistency is a sign of commitment—an insight that underlies the popularity of share-of-wallet or share-of-category-requirements as a measure of affective brand loyalty (Fader and Schmittlein 1993). This implies that share-weighted contagion may capture exposure to colleagues strongly committed to the new drug better than volume-weighted contagion. A colleague treating 5 patients for the medical condition and prescribing the new drug for all of them is more committed to it than a colleague prescribing it for only half of his 10 patients. Hence, share-weighting may better reflect how strongly each colleague feels that using the new drug is the proper thing to do.

2.5.5. Confidence: Self-Reported Opinion Leadership

Self-reported opinion leadership (*SRL*) captures the extent to which a physician feels he or she can learn from others. *SRL* is measured using a six item scale (for details, see IVV

2011a). We construct the *SRL* variable by taking the average of the six items. The first two scale items pertain to frequency of interaction, whereas the last four are an assessment of oneself versus others as a valuable source of information about treatment options, so high *SRL* is likely associated with high self-confidence.[30] Perceiving others to be less knowledgeable than oneself is distinct from being accorded high status by others (IVV 2011a) and from disregarding social norms, so there is no reason to expect *SRL* to moderate contagion through normative influence (Deutsch and Gerard 1955).[31]

2.5.6. Status: Indegree Centrality

Status is one's social rank in terms of esteem and respect bestowed by others (e.g., Phillips and Zuckerman 2001) and is measured here as the logarithm of the number of discussion and referral nominations received from other physicians.[32] Such "indegree centrality" is the most basic measure of status in networks, especially those involving deferential ties like advice-seeking or favor-seeking (Hu and Van den Bulte 2014; Knoke

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[30] Several studies have shown that *SRL* is rather weakly correlated with sociometric status as opinion leader (IVV 2011a; Jacoby 1974; Lee et al. 2010; Molitor et al. 2011; Rogers and Svenning 1969, pp. 224-227) or other-reported opinion leadership (Gnambs and Batinic 2013), suggesting that *SRL* need not capture opinion leadership. Based on its low correlation with sociometric status and their finding that *SRL* is negatively correlated with susceptibility to contagion, IVV propose that *SRL* captures self-confidence rather than opinion leadership. Subsequent research by Martin and Lueg (2013) finds that the link between word of mouth usage and attitude is stronger for people with low vs. high self-perceived knowledge. Along similar lines, Szymanowski and Gijsbrechts (2013) find that self-reported market mavens (people reporting acting as an opinion leader and sharing their information and experiences with others) learn less from their experience, which those authors interpret as possibly stemming from overconfidence.

[31] Also, the middle-status conformity hypothesis does not make any prediction about a change in self-perceived status. Instead, our application of the hypothesis implies that physicians expect that their prescription behavior will affect their *true* status, which we measure as degree centrality rather than *SRL*.

[32] Self-reported measures of status like *SRL* are dubious in general because status by definition involves esteem bestowed by others. They are especially useless when testing for middle-status conformity which requires a common metric across all actors (Hu and Van den Bulte 2014; Phillips and Zuckerman 2001), a requirement obviously violated when using self-reported status measures subject to the well documented Lake Wobegon or above-average effect.

and Burt 1983; Lu et al. 2013; Menchik and Meltzer 2010; Menzel 1957; Prell 2012, p. 9 9; Sauder et al. 2012; Sgourev 2011; Wasserman and Faust 1994, p. 202). As discussed by IVV, many studies show that indegree is robust to random node sampling as long as the sampling rate is 20% or higher (e.g., Costenbader and Valente 2003). We use the log-transformation (after adding 1 to avoid the log(0) problem) because indegree has a highly right-skewed distribution which creates numerical problems when testing for middle-status conformity by interacting colleagues contagion with indegree and its square. The log transformation stabilizes the estimation.

2.5.7. Control Variables

We control for several other physician characteristics which might be associated with trial or repeat. *Past Drug 1* and *Past Drug 2* are the number of prescriptions written by each physician for each of the other two drugs in the market during the twelve months prior to the launch of the focal drug. *University/Teaching Hospital* is a dummy variable indicating whether the physician works in or is affiliated with a university or teaching hospital. *Solo Practice* is a dummy variable capturing whether the doctor is in solo practice. *Early Referral* is a dummy variable taking the value 1 if the physician reports sometimes referring patients to other doctors before initiating any treatment, and 0 otherwise. *Primary Care* is a dummy variable capturing whether the doctor is a primary care physician rather than a specialist more likely to focus on the relevant medical condition (internal medicine, gastroenterologists, and infectious diseases).

Sales calls is the monthly physician-level amount of detailing for the focal drug. There was only very limited medical journal advertising and no direct-to-consumer advertising. There was no sampling either, because of major concerns about patients

developing resistance after taking a sample but not continuing on the drug. *City dummies* for LA and NYC control for city-specific differences. SF is the baseline.

Time dummies for each month capture the effect of any system-wide time-varying factor, such as aggregate diffusion, changes in disease prevalence, or the emergence of new clinical evidence. The dummies capture all cross-temporal variation in the mean tendency to adopt or repeat, leaving only variance across physicians within particular months to be explained by contagion.

Lagged prescription volume. Including lagged behavior as a covariate often helps controlling for both state dependency and unobserved heterogeneity. It also controls for endogeneity of sales calls when managers or salespeople allocate their effort based on prior prescription volume. In addition, it can capture variation across both time and physicians of (i) the number of patients seen by the physician for whom the drug could be part of a treatment plan and (ii) the physician's "enthusiasm" for the new drug (Bell and Song 2007). Of course, lagged prescription volume is zero until after adoption, so it can be a covariate only when modeling repeat behavior.

2.5.8. Final Data Set

Data on past prescription of the two incumbent drugs are missing for 8 doctors, 3 of whom adopted the focal drug. After deleting these 8 physicians, there are 185 adoption spells of which 65 end with adoption, and 570 opportunities for repeat of which 424 indeed show repeat behavior. Descriptive statistics for physician-months up to adoption (2575), physician-months with adoption (65), physician-months after adoption (570), and physician-months with repeat (424) are reported in the Table 2.3.

The plots in Figure 2.1 show how the average hazard of adoption, sales calls, and the

two contagion variables evolved over time among physicians who had not adopted yet. Though the hazard is rather flat with only 3 of the 17 values outside the narrow 2%-3.5% range, this does not imply the absence of contagion because neither heterogeneity in physician characteristics which creates spurious negative duration dependence or sales calls which trend downwards are accounted for (see IVV 2011a for details). The amount of volume-weighted influence from network ties operating before adoption increases steadily, whereas the volume of share-weighted influence from immediate colleagues increases more slowly after month 6. Not only do the two kinds of ties exhibit a different pattern as discussed above, but so do the contagion variables.

The plots in Figure 2.2 show how the average repeat rate, sales calls, and the two contagion variables evolve over time among physicians who had already adopted. The repeat rate in the second month is 100%, as all 6 physicians who adopted in the first month also prescribed in the next month. The average repeat rate decreases over time, which is consistent with evidence that heavy users adopted the drug early (IVV 2011a). Average sales calls decrease after month 5, which is consistent with a "hard launch" strategy (Liu and Gupta 2012; Sinha and Zoltners 2000), but may also result from the firm's allocating more sales calls to heavy prescribers while light prescribers, who tend to adopt late, make up an increasing proportion of the repeat-prescriber base. The amount of volume-weighted influence from network ties increases rather steadily, whereas the amount of share-weighted influence from immediate colleagues does so only after 4 months. The high value in month 2 is not a fluke and stems from the fact that 4 of the 6 adopters in month 1 were colleagues in a prominent research/teaching hospital.

Table 2.3. Descriptive Statistics and Correlations among Covariates

(a) Pre-Adoption Physician-months (Trial)

		Mean	SD	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	Adoption (Binary indicator)	0.03	0.16	0	1	1.00													
2	Adoption Volume	0.03	0.25	0	5	0.87	1.00												
3	Detailing (Sales Calls)	0.29	0.83	0	9	0.23	0.22	1.00											
4	Indegree	0.36	1.31	0	36	0.25	0.26	0.26	1.00										
5	Self-Reported Leadership	4.26	1.29	1	7	0.11	0.10	0.18	0.23	1.00									
6	LA Dummy	0.31	0.46	0	1	-0.01	-0.03	-0.02	-0.02	0.15	1.00								
7	NYC Dummy	0.37	0.48	0	1	-0.02	0.01	0.04	-0.02	0.02	-0.52	1.00							
8	Solo Practice	0.39	0.49	0	1	0.00	-0.01	0.13	-0.11	-0.12	-0.01	0.10	1.00						
9	Univ. Hospital	0.21	0.41	0	1	0.00	0.04	-0.10	-0.05	0.07	-0.12	0.03	-0.41	1.00					
10	Primary Care	0.13	0.34	0	1	-0.05	-0.04	-0.10	-0.08	-0.24	0.13	-0.14	-0.06	-0.01	1.00				
11	Early Referral	0.35	0.48	0	1	-0.07	-0.06	-0.17	-0.10	-0.44	-0.17	0.00	-0.05	0.13	0.15	1.00			
12	Past Drug 1	10.90	25.76	0	265	0.25	0.26	0.50	0.50	0.24	-0.07	0.15	0.02	-0.07	-0.10	-0.16	1.00		
13	Past Drug 2	10.46	24.87	0	510	0.25	0.21	0.32	0.40	0.05	-0.11	0.09	0.09	-0.06	-0.05	-0.04	0.53	1.00	
14	Contagion from Ref/Dis Ties	7.09	18.39	0	178	0.06	0.04	0.01	0.02	-0.07	-0.23	-0.11	-0.07	-0.10	0.00	0.13	-0.05	-0.02	1.00
15	Contagion from Colleagues	0.09	0.35	0	4.4	0.07	0.04	-0.05	-0.04	0.03	-0.07	-0.08	-0.08	-0.05	0.02	0.12	-0.05	-0.04	0.31

Note: Values computed on all physician-month observations for which the physician is at risk of adopting, $N = 2575$. Correlations with an absolute value equal or larger than 0.04 are significant at $p \leq .05$.

(b) Post-Adoption Physician-months (Repeat)

		Mean	SD	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	Repeat (Binary indicator)	0.74	0.44	0	1	1.00														
2	Repeat Volume	3.36	5.81	0	43	0.34	1.00													
3	Lagged Prescription Volume	3.08	5.43	0	43	0.27	0.93	1.00												
4	Detailing (Sales Calls)	1.57	1.64	0	8	0.18	0.01	0.01	1.00											
5	Indegree	3.63	6.92	0	36	0.23	0.71	0.69	0.11	1.00										
6	Self-Reported Leadership	5.38	1.14	2	7	0.18	0.28	0.28	0.12	0.43	1.00									
7	LA Dummy	0.30	0.46	0	1	0.02	-0.18	-0.18	0.05	-0.27	-0.11	1.00								
8	NYC Dummy	0.29	0.45	0	1	-0.10	-0.07	-0.07	-0.13	-0.14	0.20	-0.42	1.00							
9	Solo Practice	0.37	0.48	0	1	0.08	-0.15	-0.15	0.22	-0.21	-0.43	0.06	-0.24	1.00						
10	Univ. Hospital	0.24	0.43	0	1	0.12	0.04	0.04	-0.03	-0.09	0.34	0.00	0.16	-0.44	1.00					
11	Primary Care	0.02	0.14	0	1	0.08	0.00	0.00	-0.08	-0.07	-0.37	0.19	-0.09	0.16	-0.08	1.00				
12	Early Referral	0.07	0.25	0	1	0.00	-0.06	-0.06	-0.12	-0.14	-0.44	-0.17	-0.14	0.30	-0.15	-0.04	1.00			
13	Past Drug 1	68.63	80.39	0	265	0.30	0.52	0.50	0.11	0.52	0.41	-0.24	0.21	-0.08	0.06	-0.11	-0.19	1.00		
14	Past Drug 2	71.04	108.50	0	510	0.20	0.54	0.51	0.14	0.56	0.19	-0.23	-0.15	0.02	-0.13	-0.07	-0.15	0.68	1.00	
15	Contagion from Ref/Dis Ties	16.99	34.35	0	193	-0.13	-0.04	-0.03	-0.08	-0.06	-0.28	-0.32	-0.24	0.15	-0.23	-0.04	0.31	-0.23	-0.06	1.00
16	Contagion from Colleagues	0.55	1.19	0	5.7	-0.02	0.52	0.52	-0.07	0.51	0.07	-0.27	-0.11	-0.19	-0.10	-0.07	0.21	0.16	0.35	0.35

Note: Values computed on all physician-month observations for which the physician has already adopted, $N = 570$. Correlations with an absolute value equal or larger than 0.08 are significant at $p \leq .05$.

(c) Prescription Volume and Covariates at Time of Adoption

		Mean	SD	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13
1	Adoption Volume	1.37	0.76	1	5	1.00												
2	Detailing (Sales Calls)	1.49	1.43	0	5	0.12	1.00											
3	Indegree	2.40	5.45	0	36	0.12	0.01	1.00										
4	Self-Reported Leadership	5.10	1.25	2	7	0.15	0.14	0.40	1.00									
5	LA Dummy	0.28	0.45	0	1	-0.30	0.29	-0.17	0.08	1.00								
5	NYC Dummy	0.31	0.47	0	1	0.34	-0.23	-0.12	0.11	-0.41	1.00							
7	Solo Practice	0.38	0.49	0	1	-0.18	0.19	-0.18	-0.36	0.01	-0.18	1.00						
8	Univ. Hospital	0.22	0.41	0	1	0.44	0.08	-0.06	0.24	-0.07	0.22	-0.41	1.00					
9	Primary Care	0.03	0.17	0	1	0.03	0.00	-0.06	-0.37	0.09	-0.12	0.04	-0.09	1.00				
10	Early Referral	0.14	0.35	0	1	-0.02	-0.23	-0.18	-0.47	-0.25	-0.07	0.32	-0.10	-0.07	1.00			
11	Past Drug 1	50.62	69.51	0	265	0.19	0.14	0.54	0.40	-0.13	0.17	-0.10	0.06	-0.10	-0.21	1.00		
12	Past Drug 2	48.58	87.49	0	510	-0.03	-0.06	0.57	0.21	-0.17	-0.09	0.00	-0.06	-0.06	-0.18	0.68	1.00	
13	Contagion from Ref/Dis Ties	13.55	30.42	0	142	-0.05	-0.20	-0.15	-0.36	-0.27	-0.24	0.10	-0.21	0.14	0.29	-0.21	-0.12	1.00
14	Contagion from Colleagues	0.25	0.75	0	4.4	-0.14	-0.29	-0.13	-0.16	-0.16	-0.06	-0.03	-0.08	-0.06	0.40	-0.19	-0.13	0.51

Note: Values computed on all physician-month observations for which the physician adopts, $N = 65$. Correlations with an absolute value equal or larger than 0.25 are significant at $p \leq .05$.

(d) Prescription Volume and Covariates at Time of Repeat

|		Mean	SD	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	Repeat Volume	4.51	6.34	1	43	1.00													
2	Lagged Prescription Volume	3.95	6.04	0	43	0.93	1.00												
3	Detailing (Sales Calls)	1.74	1.70	0	8	-0.06	-0.04	1.00											
4	Indegree	4.56	7.75	0	36	0.70	0.68	0.05	1.00										
5	Self-Reported Leadership	5.50	1.13	2	7	0.28	0.28	0.10	0.44	1.00									
5	LA Dummy	0.30	0.46	0	1	-0.23	-0.22	0.01	-0.33	-0.24	1.00								
7	NYC Dummy	0.26	0.44	0	1	-0.05	-0.05	-0.06	-0.14	0.24	-0.39	1.00							
8	Solo Practice	0.39	0.49	0	1	-0.22	-0.21	0.19	-0.27	-0.41	0.11	-0.22	1.00						
9	Univ. Hospital	0.27	0.45	0	1	0.00	0.00	-0.05	-0.12	0.34	0.04	0.15	-0.49	1.00					
10	Primary Care	0.03	0.16	0	1	-0.03	-0.03	-0.11	-0.09	-0.45	0.21	-0.10	0.17	-0.10	1.00				
11	Early Referral	0.07	0.25	0	1	-0.07	-0.07	-0.10	-0.16	-0.45	-0.18	-0.12	0.29	-0.16	-0.04	1.00			
12	Past Drug 1	82.72	85.49	0	265	0.49	0.47	0.06	0.49	0.40	-0.31	0.26	-0.10	0.04	-0.15	-0.21	1.00		
13	Past Drug 2	83.79	120.02	0	510	0.52	0.49	0.09	0.54	0.18	-0.28	-0.16	-0.01	-0.19	-0.10	-0.15	0.69	1.00	
14	Contagion from Ref/Dis Ties	14.44	30.13	0	193	0.01	0.01	-0.03	-0.04	-0.20	-0.31	-0.20	0.23	-0.24	-0.04	0.37	-0.22	-0.01	1.00
15	Contagion from Colleagues	0.54	1.13	0	5.5	0.69	0.68	0.01	0.67	0.27	-0.28	-0.06	-0.30	-0.10	-0.08	0.00	0.27	0.49	0.19

Note: Values computed on all physician-month observations for which the physician repeats, $N = 424$. Correlations with an absolute value equal or larger than 0.10 are significant at $p \leq .05$.

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Figure 2.1. Descriptive Plots for Trial
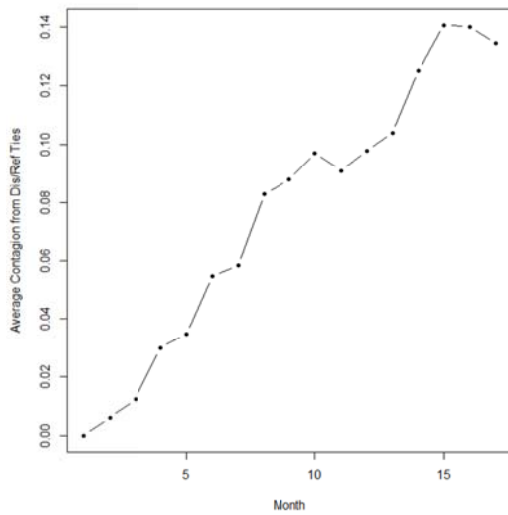**(Using all physician-months in which physicians are at risk of adopting)**

(a) Empirical Hazard Rate
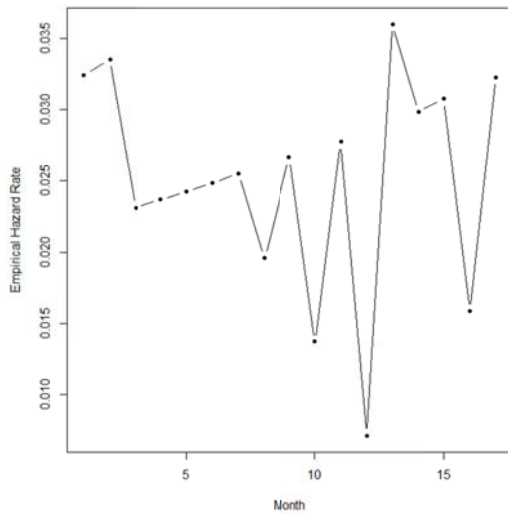
(b) Average Sales Calls per Physician
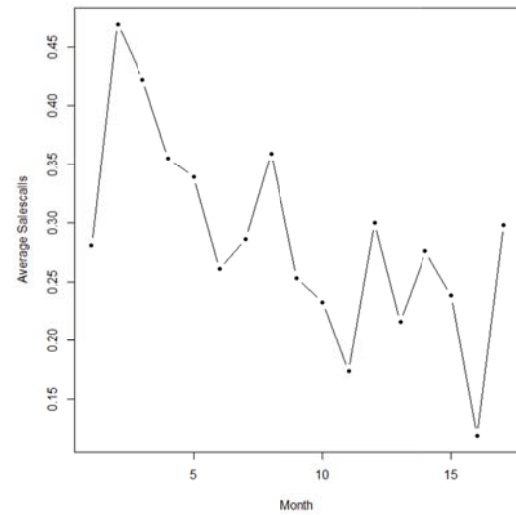
(c) Average Volume-weighted Contagion
(00s) from Discussion/Referral Ties

(d) Average Share-weighted Contagion
from Colleagues

Figure 2.2. Descriptive Plots for Repeat
(Using all physician-months in which physicians have already adopted)

(a) Empirical Repeat Rate

(b) Average Sales Calls per Physician

(c) Average Volume-weighted Contagion
(00s) from Discussion/Referral Ties
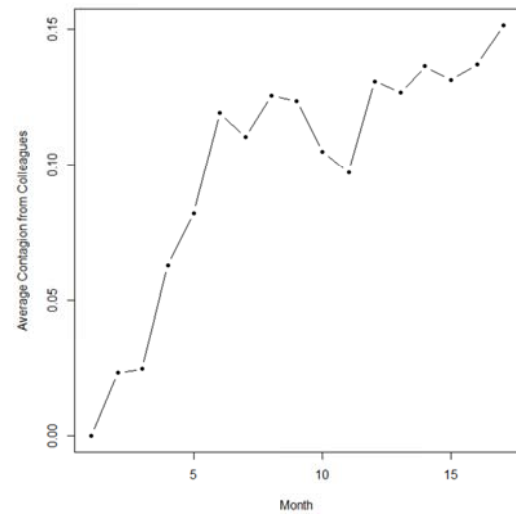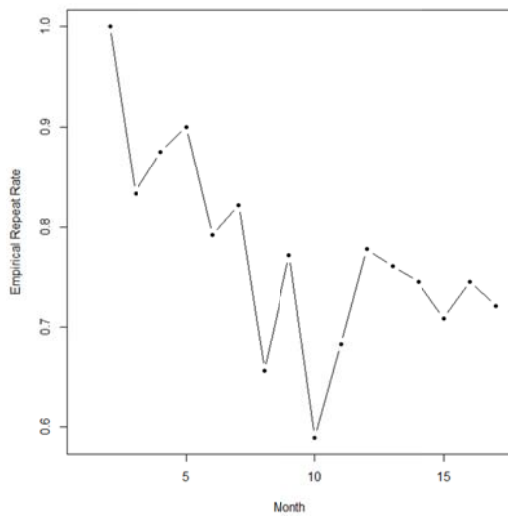
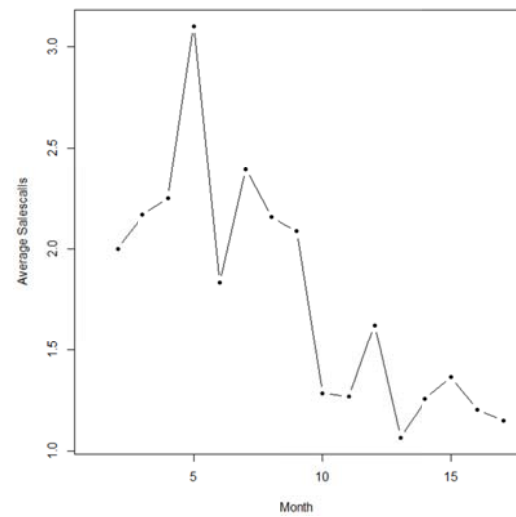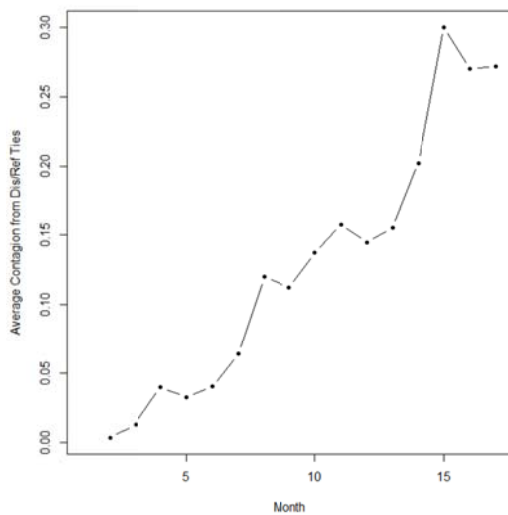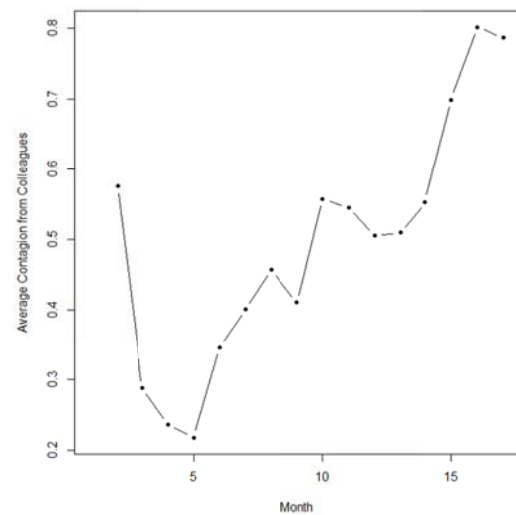(d) Average Share-weighted Contagion
from Colleagues

2.6. Model

We model adoption and repeat prescription of the focal drug in discrete time. We model

repeat conditional on adoption, rendering selectivity moot (Poirier and Ruud 1981). We

account for possible endogeneity in sales calls using a control function approach.

2.6.1. Adoption Model

We specify the appeal or utility that physician i sees in trying the drug at period t (U_{it}^a) as:

$$U_{it}^a = \beta_{0i}^a + X_{it}^a \beta_1^a + \varepsilon_{it}^a, \text{ where } \varepsilon_{it}^a \sim N(0,1) \text{ and } \beta_{0i}^a \sim N(\bar{\beta}_0^a, \sigma_a^2). \tag{2.1}$$

The row vector X_{it}^a contains covariates *up to adoption* or month 17, whichever happens

first, and β_1^a is a column vector of corresponding parameters. The parameter β_{0i}^a is a

physician-specific baseline utility and controls for unobserved characteristics related to

adoption. We assume that β_{0i}^a follows a normal distribution. We express the discrete-time

hazard of adoption or trial as:

$$P(Y_{it}^a = 1 \mid Y_{it-1}^a = 0) = P(U_{it}^a > 0) = \Phi(\beta_{0i}^a + X_{it}^a \beta_1^a), \tag{2.2}$$

where Y_{it}^a is an indicator variable that equals 0 before adoption and 1 at the time of

adoption and later, and Φ is the normal cumulative distribution function. Therefore, the

likelihood of observing $Y_{it}^a = y_{it}^a$, where $y_{it}^a \in \{0,1\}$, can be expressed as:

$$P(Y_{it}^a = y_{it}^a \mid Y_{it-1}^a = 0) = \Phi\left(\beta_{0i}^a + X_{it}^a \beta_1^a\right)^{y_{it}^a} \cdot \left(1 - \Phi\left(\beta_{0i}^a + X_{it}^a \beta_1^a\right)\right)^{1-y_{it}^a} \tag{2.3}$$

Two observations are in order. First, since adoption is a non-recurrent event, the

lagged dependent variables are always zero and including them as covariates is pointless.

Second, we do not include person-specific fixed effects as those generate truncation biases in the adoption equation (Van den Bulte and Iyengar 2011).

2.6.2. Repeat Model

Whereas trial can occur only once, repeat can occur several times. We specify the utility that physician i sees in repeat prescribing the drug at time t given adoption at a prior time (U_{it}^r) as:

$$U_{it}^r = \beta_{0i}^r + X_{it}^r \beta_1^r + \varepsilon_{it}^r, \text{ where } \varepsilon_{it}^r \sim N(0,1) \text{ and } \beta_{0i}^r \sim N(\bar{\beta}_0^r, \sigma_r^2). \qquad (2.4)$$

The row vector X_{it}^r contains covariates *after adoption*, and β_1^r is a column vector of corresponding parameters. The parameter β_{0i}^r is a physician-specific baseline of repeat utility, which is normally distributed. The probability of repeat prescription, conditional on having adopted earlier, is then given by:

$$P(Y_{it}^r = 1 \mid Y_{it-1}^a = 1) = P(U_{it}^r > 0) = \Phi(\beta_{0i}^r + X_{it}^r \beta_1^r), \qquad (2.5)$$

where Y_{it}^r is an indicator variable that takes a value of 1 if i prescribes at a time t and is 0 otherwise. Therefore, the likelihood of observing $Y_{it}^r = y_{it}^r$, where $y_{it}^r \in \{0,1\}$, is:

$$P(Y_{it}^r = y_{it}^r \mid Y_{it-1}^a = 1) = \Phi\left(\beta_{0i}^r + X_{it}^r \beta_1^r\right)^{y_{it}^r} \cdot \left(1 - \Phi\left(\beta_{0i}^r + X_{it}^r \beta_1^r\right)\right)^{1-y_{it}^r}. \qquad (2.6)$$

Several points are worth noting. First, since repeat can be a recurrent event, one can include lagged dependent variables among the covariates as well as random or fixed effects. We use random effects because fixed effects result in inconsistent estimates in probit models (e.g., Wooldridge 2002, p. 484). Second, repeat is by definition conditional on trial, each physician's adoption and repeat events occur in non-overlapping time

periods, and we assume the absence of forward-looking experimentation by physicians in this category consistently with Chintagunta et al. (2012). Consequently, our repeat model is conditional rather than unconditional on trial, the random shocks between trial and repeat can be treated as uncorrelated, and exclusion restrictions are unnecessary (e.g., Poirier and Ruud 1981). However, the time-invariant physician-specific effects may be correlated across stages. Third, including both random effects and lagged dependent variables is appropriate if the initial value of the lagged dependent variable can be assumed to be independent of the random effect (e.g., Wooldridge 2002, p. 494). In our setting, this requires the random effects in trial and repeat to be uncorrelated.

2.6.3. Correlated Random Effects

We allow the physician-specific random effects of trial and repeat to be correlated as:

$$\begin{bmatrix} \beta_{0i}^a \\ \beta_{0i}^r \end{bmatrix} \sim N\left(\begin{bmatrix} \bar{\beta}_0^a \\ \bar{\beta}_0^r \end{bmatrix}, \begin{bmatrix} \sigma_a^2 & \sigma_{ar} \\ \sigma_{ar} & \sigma_r^2 \end{bmatrix} \right). \tag{2.7}$$

Let Y_{it} indicate whether i prescribes at time t or not, let T_i^a denote the period in which physician i adopts the focal drug or is right-censored, and let T denote the length of data window (i.e., T=17).[33] The likelihood is then:

$$P(Y_{it} = y_{it} \mid \beta_1^a, \beta_1^r) = \int_{\beta_{0i}^a, \beta_{0i}^r} \prod_{t=1}^{T_i^a} P(Y_{it} = y_{it} \mid Y_{it-1}^a = 0, \beta_1^a, \beta_{0i}^a) \cdot$$

$$\prod_{t=T_i^a+1}^{T} P(Y_{it} = y_{it} \mid Y_{it-1}^a = 1, \beta_1^r, \beta_{0i}^r) \cdot f\left(\beta_{0i}^a, \beta_{0i}^r \right) d\beta_{0i}^a \, d\beta_{0i}^r \tag{2.8}$$

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[33] Right-censored physicians who do not adopt within the 17-month data window have $T_i^a = T$.

We estimate the model using simulated maximum likelihood.

2.6.4 Control Function Approach for Endogeneity in Sales Calls

Marketers and sales people may have set the amount of detailing effort towards a

physician in a particular month based on demand shocks that are not accounted for by the

covariates in the model. The resulting correlation between sales calls and the error terms,

if not properly addressed, would bias the model estimates. We handle this possible

endogeneity using a control function approach that quantifies its severity by directly

estimating the correlation between the random shocks in physician behavior and sales

calls, as detailed in the Section 2.11.1 (Appendix).

2.7. Results

Our covariates include terms for contagion from expert peers and from colleagues, terms

for the interactions hypothesized in H1 and H3, and the control variables described in

Section 2.5.7. We first estimated the model with correlated random effects but without

lagged volume. Consistent with prior evidence that a non-parametric baseline absorbs

much of the effects of unobserved heterogeneity in hazard models for non-repeated

events (e.g., Lin and Wei 1989; Struthers and Kalbfleisch 1986), the model is over-

parameterized. Specifically, the variance in random effects in trial is quite small ($\hat{\sigma}_a^2 =$

0.014, $p = 0.533$). A second model without that random effect and its associated

covariance performs better in BIC terms (ΔBIC = 9.99).[34] Given the absence of random

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[34] The difference in deviance (-2LL) between the two models is only 3.26. This would not be significant at even 10% under a likelihood ratio test with 2 df. However, an LR test is not appropriate here because it involves restricting a variance to zero which lies on the boundary of the parameter space. Since we observe 185 adoption spells and 570 opportunities for repeat, we use $N = 755$ when computing BIC values.

effects in the trial equation, adding lagged volume as a covariate to control for state dependency in the repeat equation does not create an initial condition problem. Since this third model fits markedly better than the first (ΔBIC = 24.10) and the second model (ΔBIC = 14.11; Δ-2LL = 20.74, $p < .001$), we use it as the main specification.

Table 2.4 reports the parameter estimates of substantive interest and of several control variables. SRL and Indegree (log-transformed) are mean-centered for estimation, so the coefficient of non-moderated contagion is the effect for the "average" physician. To avoid reporting very small coefficients, volume-weighted contagion is expressed in hundreds of units. Though our model includes many control variables and several non-linear effects, collinearity is not a concern since the condition index of the data matrix is only 15.47 in trial and 15.30 in repeat—well below 30 which is commonly considered a necessary condition for harmful collinearity.

Table 2.4 shows the presence of contagion in not only the trial hazard (Δ-2LL = 25.36, df = 5, $p < .001$) but also in repeat incidence (Δ-2LL = 13.10, df = 5, $p < .05$). Unlike the earlier analysis by IVV, we do not find a significant linear effect of sociometric status on the adoption hazard. That the lower-order degree effects are different is hardly surprising because the higher-order interaction covariates differ between the two analyses designed with different objectives in mind (compare Table 2.4 here with Table 4 in IVV 2011a). We next turn to the findings of key interest: the contrasts between advice/discussion ties vs. colleagues as sources of influence, and the contrast between trial and repeat as stages in new product acceptance behavior.

Table 2.4. Model Estimates

Variables	Trial Hazard	Repeat Probability
Intercept	-2.069 ***	-0.333
	(0.312)	(0.467)
SRL	0.133	-0.088
	(0.069)	(0.157)
Ln(Indegree + 1)	0.106	0.073
	(0.228)	(0.425)
Ln(Indegree + 1)^2	0.020	0.126
	(0.132)	(0.299)
Contagion from Dis / Ref Ties (00s)	0.056	-0.067
	(0.344)	(0.423)
Contagion from Dis / Ref Ties (00s) × SRL	-0.677 **	0.390
	(0.250)	(0.260)
Contagion from Colleagues	0.759 *	0.479
	(0.377)	(0.257)
Contagion from Colleagues × Ln(Indegree + 1)	0.625	2.533 ***
	(0.917)	(0.686)
Contagion from Colleagues × Ln(Indegree + 1)^2	-0.787	-0.840 *
	(1.213)	(0.305)
Solo Practice	-0.044	0.487
	(0.180)	(0.306)
University / Teaching Hospital	0.226	0.975**
	(0.186)	(0.344)
Primary Care	-0.223	10 †
	(0.307)	
Early Referral	-0.286	0.900
	(0.197)	(0.616)
Past Drug 1	0.000	0.010 ***
	(0.002)	(0.003)
Past Drug 2	0.006**	-0.003
	(0.002)	(0.003)
Sales Calls	0.556 **	-0.201
	(0.195)	(0.385)
Endogeneity Correlation	-0.288	0.269
	(0.201)	(0.342)
Ln(q_{it-1} + 1)	-	0.892 ***
		(0.183)
Random Effect Stand. Dev.	0 ††	0.473 ***
		(0.166)
Random Effects Covariance	0 ††	

* $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$. Standard errors in parentheses. LL= -406.79, BIC = 1,270.82.
The model includes several additional covariates: Monthly time dummies (16 for trial, 14 for repeat) and city dummies for LA and NYC in both equations. These estimates are not reported to avoid clutter.
† Dummies for Primary Care and Month 2 are perfect predictors for repeat incidence. We set their coefficients to a very large number (10) so the predicted repeat probability for these physician-months is essentially 1 and the observations do not affect the likelihood estimation.
†† Set to zero based on BIC. See first paragraph of Section 2.7.

2.7.1. Contagion from Discussion/Referral Ties versus Colleagues

Peers one turns to for discussion or referral exert contagion in trial, and the strength of that influence varies across potential adopters. In contrast, those same peers exert no influence in repeat. As reported in the first column in Table 2.4, the main effect of contagion from discussion/referral ties on the "average" physician is not significant, but physicians with a low SRL are significantly more susceptible to such contagion in the trial stage ($p < 0.01$). In contrast, there is no main or moderator effect at the repeat stage. Figures 2.3(a) and 2.3(b) convey the relationship between contagion and self-reported leadership visually. Figure 2.3(a) shows that contagion from discussion/referral ties is positive at trial for physicians with SRL less than 4.57, which corresponds to 55% of the physicians. It is significantly positive at 95% confidence for physicians with SRL lower than 3.56 (27% of physicians) and never turns significantly negative. Figure 2.3(b) shows a very different pattern for repeat: there is no significant contagion effect from discussion/referral ties at any level of SRL.

The coefficients for contagion from colleagues in Table 2.4 and the bottom two panels in Figure 2.3 show that this type of contagion operates quite differently. In trial, colleagues exert significant contagion on the "average" physician ($p < .05$), and the effect is not significantly moderated by the potential adopter's status. In repeat, the effect varies in a pronounced inverse-U fashion with the physician's status (Δ-2LL $= 10.64$, df $= 2$, $p < .01$). The latter is conveyed more compellingly by the plot in Figure 2.3(d). The expected contagion effect from colleagues is the largest for a physician with Indegree of about 5, which is well within the observed range. The effect is significantly positive at

95% confidence for physicians with Indegree between 1 and 10 (21% of physicians,

between the 77th and 98th percentiles of the Indegree distribution).[35] The confidence band

in Figure 2.3(c) is extremely wide because of the insignificant moderator effects of status

in trial. Though not obvious from the plot, the 76% of physicians with Indegree less than

1 exhibit positive contagion from colleagues at trial significant at 95% confidence.

So, discussion and referral ties have a pronounced effect in trial but not repeat,

colleagues have an effect on both trial and repeat, and an inverse-U relation with status is

present only for colleagues contagion at the repeat stage. These findings support

hypotheses H1 and H3.

2.7.2. Trial versus Repeat

We now turn to whether contagion operates differently across trial and repeat, as posited

in hypotheses H2 and H4. Our model structure makes formal testing easy, because the

discrete-time hazard of trial and the probability of repeat are both modeled using a probit

specification. We use a likelihood ratio test comparing the full model in Table 2.4 (where

all coefficients are allowed to vary freely across stages) against a restricted model where

the two discussion/referral contagion coefficients and the three colleagues contagion

coefficients are constrained to be equal across trial and repeat. To account for the

arbitrary scaling in probit models, we specify a model where the five contagion effects

are restricted to be equal across stages up to a common scaling constant, as proposed by

Train (2003, p. 26), while all other coefficients vary freely. This model fits significantly

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[35] The critical Indegree value at the lower end is 0.38. Since Indegree is a count variable we round it up to
1. Re-estimating the model without mean-centering such that the linear contagion effect pertains to a
physician with zero Indegree confirms that that colleagues contagion effect is not significant at 95%
confidence at Indegree = 0.

Figure 2.3. Social Contagion in Adoption and Repeat Incidence

(a) Contagion from Discussion/Referral Ties in Adoption
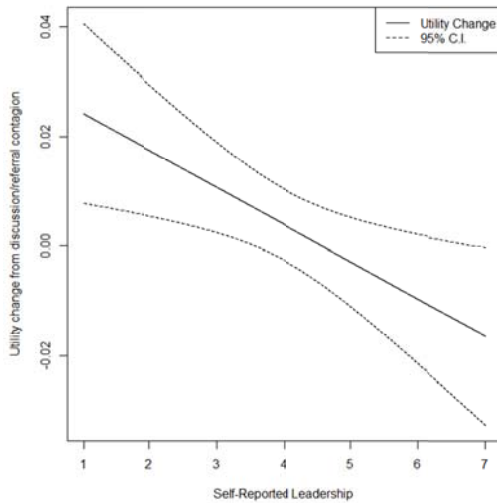
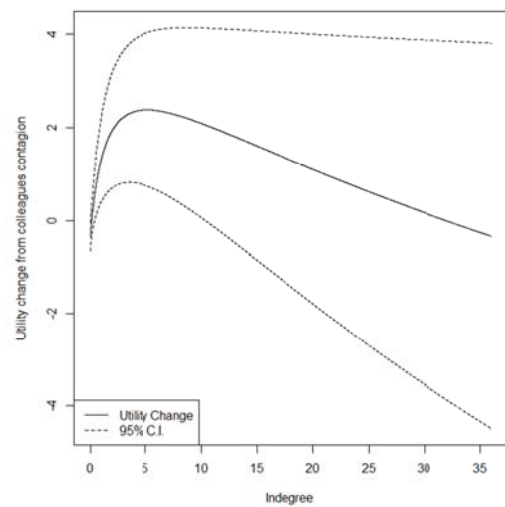(b) Contagion from Discussion/Referral Ties in Repeat
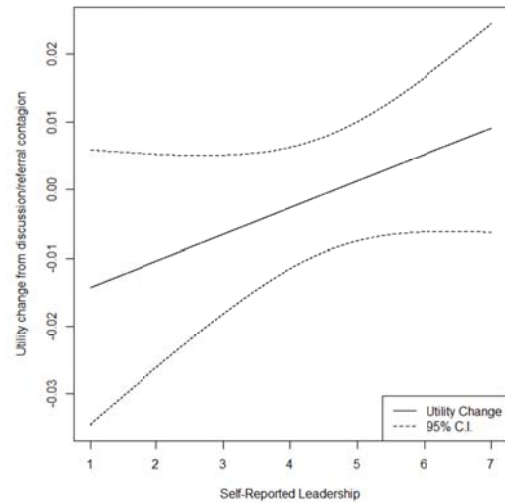
(c) Contagion from Colleagues in Adoption
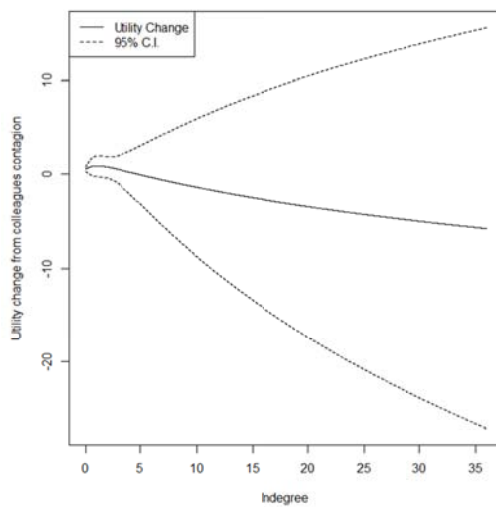
(d) Contagion from Colleagues in Repeat

worse than the unconstrained model (Δ-2LL = 14.21, df = 4, p < .01), indicating that contagion operates differently across trial and repeat. Additional nested tests indicate that this holds also for contagion from immediate colleagues considered separately (H4, p

< .01) but not for contagion from discussion/referral ties considered separately (H2, *p*

> .10). The latter is consistent with the wide confidence bounds in Figure 2.3(b).

Our discussion in Section 2.5 proposed two reasons to expect the susceptibility to normative influence to increase as customers proceed from trial to repeat. One pertained to a genuine difference between trial and repeat, regardless of time since launch, whereas the other pertained to a change over time in how much people conform to social norms, with the difference between trial and repeat only being a corollary of this temporal effect. This raises the question to what extent the cross-stage difference in interactions with SRL and indegree reported in Table 2.4 represent mere cross-time effects rather than true cross-stage effects.

Extending the model with interactions between time since launch and the two contagion variables in the two stages allows one to answer that question. (There is no need to add linear time trends since the time dummies already capture any main effect of time.) Adding those four interaction terms does not significantly improve model fit (Δ-2LL = 6.35, p = .17) and the BIC strongly favors the original model (ΔBIC = 20.15), though the influence from colleagues in the repeat stage increases over time ($p < .05$). More importantly, the interactions of substantive interest remain significant. So, even after controlling for systematic changes over time in the strength of contagion from advice/discussion ties and from colleagues, people who fancy themselves to be opinion leaders are less susceptible to contagion from their advice/discussion ties in trial but not repeat, and people of middle-status are more susceptible to contagion from colleagues in

repeat but not trial (Table 2.5).[36]

In short, the results are consistent with both reasons to expect the susceptibility to normative influence to increase as customers proceed from trial to repeat: (i) over time, people become increasingly susceptible to normative considerations and hence to colleagues enacting and enforcing norms, and (ii) as they progress from trial to repeat, people find it more difficult to defend their deviations from colleagues' behavior, especially if they are middle-status as one would expect if those deviations are seen as normative transgressions.

2.7.3. Other Variables

Physician characteristics included as control variables do not show consistent coefficients across the adoption and repeat columns in Table 2.4. Sales calls accelerate adoption but not repeat behavior. Assuming that sales calls and expert peer influence are both informative, this contrast is consistent with the presence of expert peer contagion in trial only. The contrast is also consistent with evidence that pharmaceutical detailing is effective mostly as an acquisition tool rather than a retention tool (Montoya et al. 2010) and with the empirical generalization that marketing efforts like personal selling and advertising are more effective early in the product life cycle (Albers et al. 2010; Lodish et al. 1995; Sethuraman et al. 2011). More generally, the lack of consistency in the estimates

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[36] Table 2.5 reports a significant interaction in trial of contagion from colleagues with status squared, $Ln(Degree+1)^2$, which is not present in the main model reported in Table 2.4. However, the extended model reported in Table 2.5 shows no significant interaction with status itself, and a plot like Figure 2.3(c) for the extended model shows no inverse-U pattern. Also, deleting the interactions of contagion from colleagues with status and status squared in trial from the extended model does not generate a significantly worse fit to the data (Δ-2LL = 0.602, 2 df, p = .740). Hence, the extended model in Table 2.5 does not provide evidence of middle-status conformity to colleagues' behavior in trial.

across trial and repeat supports the notion that research conclusions can vary across facets of product acceptance (Bell and Song 2007; Chandrashekaran and Sinha 1995).

2.7.4. Robustness Checks

IVV already reported quite a few robustness checks, but their analysis did not include contagion among co-located colleagues. As reported in the Section 2.11.2 (Appendix), our results are robust to (i) alternative specifications of contagion among colleagues, (ii) alternative specification of moderators, (iii) controlling for differences in demographics among the ZIP codes in which the physicians practice, (iv) controlling for lagged sales calls and (v) changing the centering of the status variable to minimize the correlation between status and its squared value.

2.8. Threats to Internal Validity

Our findings likely reflect genuine behavioral contagion patterns rather than confounds. Though some alternative explanations are *conceivable*, they are not *credible* given our data and analysis. Of course, this assessment is a matter of judgment and depends on the set of rival explanations one is aware of (Dawid 2013; Stanford 2006).

2.8.1. Instrumentation Bias

It is conceivable that the sociometric survey may have sensitized the physicians to the new drug or to their peers, and so may have increased the baseline prescription behavior or the susceptibility to peer influence. If that were the case, then one should see an uptick in the baseline (intercept) or network contagion after the survey was administered. Extending the model with a shift after month 10 in the baselines in LA and NYC

Table 2.5. Model Estimates Allowing for Cross-temporal Changes in Contagion

Variables	Trial Hazard	Repeat Probability
Intercept	-2.081 ***	-0.311
	(0.317)	(0.472)
SRL	0.127	-0.103
	(0.068)	(0.157)
Ln(Indegree + 1)	0.109	0.023
	(0.223)	(0.394)
Ln(Indegree + 1)^2	0.006	0.155
	(0.132)	(0.297)
Contagion from Dis / Ref Ties (00s)	-0.014	0.008
	(0.013)	(0.020)
Contagion from Dis / Ref Ties (00s) × SRL	-0.610 *	0.404
	(0.259)	(0.260)
Contagion from Dis / Ref Ties (00s) × Time	0.112	-0.066
	(0.094)	(0.124)
Contagion from Colleagues	1.057 **	-1.297
	(0.402)	(0.688)
Contagion from Colleagues × Ln(Indegree + 1)	0.548	2.663 ***
	(0.373)	(0.400)
Contagion from Colleagues × Ln(Indegree + 1)^2	-0.666 *	-0.739 **
	(0.273)	(0.248)
Contagion from Colleagues × Time	-0.026	0.121 *
	(0.037)	(0.050)
Solo Practice	-0.038	0.510
	(0.176)	(0.302)
University / Teaching Hospital	0.217	0.997**
	(0.186)	(0.349)
Primary Care	-0.234	10 †
	(0.308)	
Early Referral	-0.293	0.887
	(0.197)	(0.629)
Past Drug 1	0.000	0.010 ***
	(0.002)	(0.003)
Past Drug 2	0.006**	-0.003
	(0.002)	(0.003)
Sales Calls	0.567 **	-0.239
	(0.193)	(0.377)
Endogeneity Correlation	-0.292	0.291
	(0.198)	(0.334)
Ln(q_{it-1} + 1)	-	0.837 ***
		(0.186)
Random Effect Stand. Dev.	0 ††	0.486 ***
		(0.160)
Random Effects Covariance	0 ††	

* $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$. Standard errors in parentheses. LL= -403.61, BIC = 1,290.97.
The model includes several additional covariates: Monthly time dummies (16 for trial, 14 for repeat) and city dummies for LA and NYC in both equations. These estimates are not reported to avoid clutter.
† Dummies for Primary Care and Month 2 are perfect predictors for repeat incidence. We set their coefficients to a very large number (10) so the predicted repeat probability for these physician-months is essentially 1 and the observations do not affect the likelihood estimation.
†† Set to zero, as in the model in Table 2.4.

indicates that they are not systematically higher after the survey was administered (month

10) than they are before—they are actually all lower, though not significantly so with p

= .12 or worse. Extending the model with a shift after month 10 in the contagion effects

in LA and NYC shows that contagion from discussion/referral ties is insignificantly

lower after month 10 in NYC (p = .23 or worse) and insignificantly higher after month 10

in LA (p = .64 or worse). The data do not support the presence of instrumentation bias.

2.8.2. Endogenous Tie Formation: Network Peers

Another concern is that contagion coefficients do not capture the effect of ties on

behavior but that of behavior on tie formation. For instance, if physicians with low

confidence are more likely to build connections with prior adopters of the drug, then the

finding that self-reported followers are more sensitive to peer influence might reflect

selective tie formation rather than higher susceptibility to social contagion.

Several features of the data indicate that such endogenous tie formation is not a

credible threat to internal validity. The first is the wording in the sociometric survey. The

questions measuring discussion and referral ties pertained to the medical condition in

general rather than the new drug specifically (IVV). The second is the correlation

between SRL and the number of connections made to peers for discussion or referral,

referred to by IVV as "outdegree centrality". That correlation is -0.04 (IVV, p. 205),

indicating that the number of peers one reaches out to is uncorrelated with one's self-

reported opinion leadership. The third feature is that the network was measured before

launch in SF but after launch in LA and NYC. Whereas endogenous tie formation, in

which non-adopters selectively build ties to others they know have adopted, might

conceivably have affected the measured network in LA and NYC, it cannot have affected

it in SF. So, endogenous tie formation implies that network contagion effects are smaller in SF than in LA and NYC (ceteris paribus). Extending the model with such contrasts does not support this notion: network contagion effects are actually larger in SF, though not significantly so in either trial ($p = .20$) or repeat ($p = .21$). Also, there is no evidence consistent with the notion that the new product's launch prompted physicians to form additional ties. There is no significant difference in the mean or distribution of the number of peer nominations made by physicians in SF versus LA and NYC jointly (t-test: $p = 0.52$; Wilcoxon rank sum test: $p = 0.39$, Two-sample Kolmogorov-Smirnow test: $p = 0.91$)., in SF versus LA only (Tukey test: $p = 0.99$; Wilcoxon rank sum test: $p = 0.69$; Two-sample Kolmogorov-Smirnow test: $p = 0.70$) or in SF versus NYC only (Tukey test: $p = 0.60$; Wilcoxon rank sum test: $p = 0.29$; Two-sample Kolmogorov-Smirnow test: $p = 0.57$). In short, the data are inconsistent with the endogenous formation of discussion or referral ties acting as a confound to our contagion findings.

2.8.3. Endogenous Tie Formation: Colleagues

Endogenous tie formation is not a credible threat for contagion from colleagues either. First, the argument does not apply to our research setting. The threat requires that the decisions not to practice solo and to join a specific hospital or group practice rather than another are affected by the extent to which prospective colleagues (are expected to) prescribe the focal drug. The threat also requires that hospitals and group practices are more likely to invite or accept physicians who they (fore-)see adopting the specific new drug. Both notions are too risibly farfetched to be credible. Second, the specific pattern in colleagues contagion further detracts from endogenous tie formation's credibility as a threat to internal validity (e.g., Rosenbaum 2002, pp. 209-214). Endogenous formation of

collegial ties, if it were present, would operate equally across trial and repeat, but we observe different collegial contagion effects across stages ($p < .01$). Furthermore, endogenous tie formation cannot account for the non-monotonic interaction we observe.

2.8.4. Reflection

Reflection arises when the peer behavior used to explain the behavior of a focal physician is actually caused by that very same physician. This is not a credible threat, since we operationalize contagion in terms of lagged rather than current peer behavior, all physicians at risk of adoption have by definition not adopted before, and we control for lagged behavior of the focal physician in the repeat equations. Moreover, the network data are almost perfectly acyclic: of the 204 discussion ties and 138 referral ties, only 3 are symmetric and these three ties form the only triad (IVV 2011a, p. 200).

2.8.5. Correlated Unobservables

Unobserved shocks that vary over time but are common across all physicians are controlled through time fixed effects. This leaves variance across physicians within particular months to be explained by contagion. Time-invariant unobserved differences across cities are also captured through city fixed effects. This leaves only factors that are specific to physicians and their network peers or colleagues as possible sources of bias from correlated unobservables. The latter often are cause concern about the validity of main effects in contagion studies, and justifiably so. However, they cannot explain our findings involving multiple dependent variables, multiple contagion variables, multiple moderators, and a non-monotonic effect. What omitted variable(s) could account for peer contagion affecting trial but not repeat, peer contagion being significant only for those who do not consider themselves opinion leaders, and middle-status conformity in

colleague contagion? Our contagion interpretation provides a coherent account for this complex pattern of findings, whereas correlated unobservables do not. Consequently, the latter are not a credible threat to validity (Cochran 1965; Hill 1965; Rosenbaum 2002, pp. 209-211; Shadish et al. 2002, p. 105).

For instance, it is likely that unobserved preferences for particular treatment options are correlated among network peers (Landon et al. 2012), but this cannot explain why network contagion is detected in trial but not repeat or why network contagion varies systematically with self-reported opinion leadership. Similarly, unobserved preferences for treatments, unobserved similarities in patient mix, or unobserved constraints (e.g., the absence of the drug from a list of approved drugs) may conceivably have been correlated among colleagues. Yet, that cannot account for the presence of a moderator effect by status.

2.8.6. Truncation Bias

Our hazard analysis of adoption timing includes all the physicians at risk rather than only those who adopted. So, our contagion estimates do not suffer from upward truncation bias (Van den Bulte and Iyengar 2011).

2.8.7. Mere Duration Dependence in Usage

Yet another concern might be that repeat incidence increases not just over time (a "period effect" already controlled for by monthly dummies) but also with the time since the physician adopted (an "age effect" not yet controlled for). If positive, such duration dependence might inflate the estimates of contagion at the repeat stage. However, controlling for how long it has been since a physician adopted does not improve model fit (Δ-2LL $= 0.16$) and does not affect the estimated contagion patterns in the repeat stage.

2.9. Discussion

We investigated the presence and nature of contagion in the acceptance of a risky prescription drug by physicians. There are three novel findings. First, there is evidence of contagion not only in trial but also in repeat. Second, who is most influential varies across stages. Physicians with high network centrality and high prescription volume are influential in trial but not repeat. In contrast, immediate colleagues—few of whom are nominated as discussion or referral partner—are influential in both trial and repeat. Third, who is most influenceable also varies across stages. For trial, it is physicians who do not consider themselves to be opinion leaders, whereas for repeat, it is those located in the middle of the status distribution as measured by network centrality.

These findings help move the research frontier from documenting whether contagion is at work to understanding how and why it is at work (Aral 2011; Godes 2011). The pattern of findings is consistent with informational social influence reducing risk in trial and normative social influence increasing conformity in repeat. Marketing scientists have emphasized the former and ignored the latter, yet our findings indicate that contagion in new product acceptance can operate in richer ways than hitherto documented.

Our work provides fresh evidence about the role of status in social contagion and new product acceptance (Van den Bulte and Joshi 2007). Specifically, our findings add to recent evidence that social status affects new product acceptance separately from self-confidence or social class (Hu and Van den Bulte 2014).

Our findings about the presence and nature of social contagion in new product repeat behavior complement and enhance recent work on the role of social contagion and social enrichment in customer retention and churn (Haenlein 2013; Nitzan and Libai 2011;

Schmitt et al. 2011). Specifically, new insights into customer management may come from investigating under what conditions social status and normative considerations affect usage intensity and customer churn.

Our study will also be of interest to researchers concerned about the identification of contagion effects in non-experimental studies. We apply R.A. Fisher's advice on how to move from association to causation in observational studies—"Make your theories elaborate". The theoretically informed associations we observe involve multiple dependent variables, multiple contagion variables, multiple moderators, and a non-monotonic effect. Those specific patterns cannot be accounted for by the standards threats to validity in contagion studies. Going beyond mere linear associations in a single facet of contagion provides empirical insights that are not only substantively richer but also methodologically stronger (e.g., Hodas and Lerman 2014).

A brief discussion of the scope conditions of our theoretical claims and empirical application seems warranted. Contagion in repeat, we contend, may occur when the product poses some significant functional, financial or normative risk even after adoption. This is likely for (i) "credence goods" for which people seek informational guidance even after personal use experience, and (ii) products, services or practices the use of which is subject to normative influence. Contagion can also exist in repeat for (iii) products and services with installed-base effects where the utility of use increases with the number of relevant other users, as shown by recent findings on contagious churn among customers of telephone providers (Haenlein 2013; Nitzan and Libai 2011). Contagion can also occur in repeat when (iv) environmental shocks raise new doubts about an accepted product (Nair et al. 2010). In short, even though our study focused on only a single drug and even

though our evidence of post-adoption contagion is consistent only with normative influence, post-adoption contagion is likely to affect many more product categories than risky drugs.

Because our study was limited to a single product, corroboration in other settings would be quite useful. Studies covering multiple products with different risk and status characteristics and studies with a longer window extending beyond *early* repeat would be especially valuable as they could further sharpen insight into the nature of the mechanisms at work. Also, research of social learning or contagion in new product acceptance that uses a more direct measure of self-confidence than self-reported opinion leadership or self-reported market mavenship would be useful additions to this study and that by Szymanowski and Gijsbrechts (2013). Further research on the nature of colleagues contagion would also be welcome. Intra-organizational diffusion is a topic of great importance to both users and marketers that we know too little about.

Our findings are also of interest to practitioners. Marketers should consider leveraging peer influence not only to trigger adoption, but also to support subsequent repeat—at least for risky products like the one studied here. As Christakis and Fowler (2011) note, aptly targeting word-of-mouth (WOM) marketing campaigns requires knowing not only who is especially influential but also who is especially influenceable. Our findings suggest that the answer to both questions may vary between trial and repeat. In-depth assessments of such differentiated targeting at trial vs. repeat, using experimental (e.g., Hinz et al. 2013) or simulation designs (e.g.; Aral et al. 2013; Haenlein and Libai 2013), would be of clear managerial value.

Practitioners willing to go beyond the mere operational definition of our variables and seeing value in the theoretical lens we used, should also consider adapting their messaging so that considerations of perceived risk, status, and normative conformity receive different weights when trying to get prospects to adopt versus trying to get adopters to repeat.

Over the last several years, managers have come to embrace the notion that not only attracting new customers but also retaining them has a large impact on the corporation's profits and long-term value. Managers also have become increasingly keen on leveraging contagion among customers. Our results suggest that these two major endeavors in current marketing practice are related: Not only trial but also repeat can be subject to social contagion.

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2.11. Appendix

2.11.1. Control Function Approach for Endogeneity in Sales Calls

Marketers and sales people may set the amount of detailing towards a physician in a particular month based on demand shocks that are not accounted for by the covariates in the model. The resulting correlation between sales calls and the error terms in the incidence or volume equations, if not properly addressed, would bias the model estimates. We handle this endogeneity concern using a control function (CF) approach (e.g., Papke and Wooldridge 2008; Park and Gupta 2009; Petrin and Train 2010) in a manner that provides a direct estimate of the severity of endogeneity. For clarity of exposition, we focus on the adoption hazard equation.

Adoption Model with CF. We start by rewriting Equation (2.1) as:

$$U_{it}^a = \beta_{0i}^a + W_{it}^a \delta_1^a + D_{it}^a \delta_2^a + \varepsilon_{it}^a, \text{ where } X_{it}^a = \begin{bmatrix} W_{it}^a & D_{it}^a \end{bmatrix} \text{ and } \beta_1^a = \begin{bmatrix} \delta_1^a & \delta_2^a \end{bmatrix}, \qquad (2.9)$$

Where D_{it}^a denotes the number of sales calls that physician i receives in period t (up to adoption) and the row vector W_{it}^a contains all other, exogenous covariates. We next express sales calls (D_{it}^a) as a function of exogenous variables (W_{it}^a and Z_{it}^a) in the following manner:

$$D_{it}^a = \gamma_0 + W_{it}^a \gamma_1 + Z_{it}^a \gamma_2 + \eta_{it}^a, \text{ where } \eta_{it}^a \sim N(0, \tau_a^2). \qquad (2.10)$$

Vector Z_{it}^a contains exogenous variables that are related to sales calls, but not to the prescription behavior of physician i at time t. We use two such instruments: the average number of lagged sales calls (i.e., in month t-1) to physicians who are located in the *other* two cities but who are similar to i in (i) status measured by indegree and (ii) prelaunch

prescription volume of the other two drugs. Physicians in other cities are considered to be similar to physician i on a variable if the percentile they occupy within their city is within 10% points of physician i's percentile within his or her own city.

The parameter γ_0 captures baseline for sales calls, γ_1 and γ_2 are column vectors of parameters to be estimated, and η_{it}^a is a random error which is assumed to be identically and independently distributed. Endogeneity arises when there is a non-zero correlation between sales calls (D_{it}^a) and the demand shocks (ε_{it}^a). Given the exogeneity of W_{it}^a and Z_{it}^a, the endogeneity problem stems from the correlation between ε_{it}^a and η_{it}^a. We assume that the two error terms are jointly normally distributed. Thus,

$$\begin{bmatrix} \varepsilon_{it}^a \\ \eta_{it}^a \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho_a \tau_a \\ \rho_a \tau_a & \tau_a^2 \end{bmatrix} \right), \tag{2.11}$$

where ρ_a denotes the correlation between the error terms and its estimate provides a testable measure for the severity of endogeneity in sales calls.

By using the conditional property of a bivariate normal distribution, we rewrite Equation (2.9) as a function of mutually independent random components, η_{it}^a and ξ_{it}^a, in the following manner (Smith and Blundell 1986):

$$U_{it}^a = \beta_{0i}^a + W_{it}^a \delta_1^a + D_{it}^a \delta_2^a + \frac{\rho_a}{\tau_a} \eta_{it}^a + \xi_{it}^a, \text{ where } \xi_{it}^a \sim N\left(0, 1 - \rho_a^2\right). \tag{2.12}$$

The error term ξ_{it}^a is independent of any other term on the right-hand side. Therefore, the discrete-time hazard of adoption after controlling for the endogeneity of sales calls can be expressed as:

$$P(Y_{it}^a = 1 \mid Y_{it-1}^a = 0) = \Phi \left(\frac{\beta_{0i}^a + W_{it}^a \delta_1^a + D_{it}^a \delta_2^a + \dfrac{\rho_a}{\tau_a} \eta_{it}^a}{\sqrt{1 - \rho_a^2}} \right). \tag{2.13}$$

The overall likelihood of observing y_{it}^a can be obtained by entering this expression rather than that in Equation (2.2) into Equations (2.3) and (2.8).

Because the values of η_{it}^a and τ_a are not directly observed, we estimate the model in two stages. First, we estimate Equation (2.10) with OLS, and obtain estimates, $\hat{\eta}_{it}^a$ and $\hat{\tau}_a$. Next, we estimate the parameters, $\begin{bmatrix} \bar{\beta}_0^a & \delta_1^a & \delta_2^a & \rho_a & \sigma_a^2 \end{bmatrix}$ by plugging in the first-stage estimates ($\hat{\eta}_{it}^a$ and $\hat{\tau}_a$) into Equation (2.13). As $\hat{\eta}_{it}^a$ and $\hat{\tau}_a$ are only estimates rather than actual values of $\hat{\eta}_{it}^a$ and τ_a, we use a bootstrap procedure to avoid underestimating the standard errors (Petrin and Train 2010).

The control function approach is a general methodology that can be applied regardless of any distributional assumptions for the error terms (e.g., Petrin and Train 2010). It can be implemented by simply "plugging" $\hat{\eta}_{it}^a$ into the utility function and estimating a standard hazard model. However, the corresponding coefficient of $\hat{\eta}_{it}^a$ does not really measure the strength of endogeneity ρ_a but only the ratio ρ_a / τ_a. The approach outlined here assuming normality is more informative.

We apply the same approach for repeat incidence, using the same γ parameters in (2.10). i.e., we use a common control function for detailing across all $185 \times 17 = 3,145$ physician-month observations, but use the relevant first-stage estimates $\hat{\eta}_{it}$ to match the physician-months in each equation. Note, the control function includes all covariates in

the adoption and repeat equations, including the monthly dummies and lagged prescription volume, as well as the two time-varying instruments.

Invariance to scaling. The parameter of key interest for assessing endogeneity is identified regardless of the error scaling in the utility equation. We briefly show this, omitting superscripts for adoption or repeat. Let the utility shocks $\varepsilon_{it} \sim N(0, \sigma^2)$. As a result, the covariance term in (2.11) equals $\rho \sigma \tau$ and Equation (2.12) becomes (Smith and Blundell 1986):

$$U_{it} = \beta_{0i} + W_{it}\delta_1 + D_{it}\delta_2 + \frac{\rho \sigma \tau}{\tau^2}\eta_{it} + \xi_{it}, \text{ where } \xi_{it} \sim N(0, \sigma^2(1-\rho^2)). \tag{2.14}$$

The corresponding probit model (2.13) then becomes:

$$\Phi\left(\frac{\beta_{0i} + W_{it}\delta_1 + D_{it}\delta_2 + \dfrac{\rho \sigma}{\tau}\eta_{it}}{\sigma\sqrt{1-\rho^2}}\right). \tag{2.15}$$

In this model, the estimated ρ coefficient of the ratio of first-stage estimates $\hat{\eta}_{it}/\hat{\tau}$ is invariant to the scaling of σ.

2.11.2. Robustness Checks

Alternative Operationalizations of Contagion. In the main analysis, we assume contagion from discussion and referral ties to be driven by prescription *volume* of the focal drug and that from colleagues to be driven by prescription *share*. This choice is based on theoretical considerations under the assumption that influence from discussion/referral ties is mostly informational whereas that from colleagues is mostly normative. Using a difference in BIC of at least 2 to indicate positive evidence of a

difference in descriptive fit (Raftery 1995), this a priori preferred specification fits the data about as well as weighting contagion from both discussion/referral ties and colleagues by prescription volume (ΔBIC = -1.00) and better than weighting both by prescription share (ΔBIC = 7.07). The key results are robust, except that SRL does not moderate share-weighted contagion from discussion/referral ties in trial. So, both the model fit and the absence of an interaction consistent with theory speak against share-weighting the contagion from discussion/referral ties.

We also operationalized colleagues contagion as stemming from the new drug's share of category requirements at the practice level, qnew(t-1) / [qnew(t-1) + qDrug1(t-1) + qDrug2(t-1)]. This alternative metric differs from that in the main model in two ways. (i) It is affected by the focal physician's own lagged prescriptions, not just those of his or her peers. (ii) It corresponds to the sum of the new drug's share of the category requirements of each physician weighted by the physician's share in the practice total, whereas the metric in the main model assumes that each colleague contributes equally to the local norm. Neither assumption (i) or (ii) is appealing a priori if colleagues contagion is meant to capture local normative influence. Replacing the colleagues contagion variable in the main model by this alternative indeed leads to a markedly worse fit (ΔBIC = 16.65). The moderator effects of indegree and squared indegree on colleagues contagion in repeat turns non-significant, which is inconsistent with the middle-status conformity hypothesis. So, both the model fit and the absence of interactions consistent with theory speak against this a priori unappealing alternative metric of colleagues contagion.

Additional Interactions. The main analysis indicates that SRL moderates contagion from discussion/referral ties in adoption, while indegree moderates contagion from colleagues in repeat. To strengthen these findings, we extend the main model with the interaction between SRL and contagion from colleagues in the adoption equation, and the interactions between log indegree (linear and squared) and contagion from discussion/referral ties in the repeat equation. Adding those variables does not improve model fit significantly (Δ-2LL = 3.40, df = 3, $p > .05$). None of the additional interactions is significant individually either ($p > .05$), and all the results from the original model remain valid. So, (a) SRL moderates contagion from discussion/referral ties but not from colleagues in adoption, and (b) indegree moderates contagion from colleagues but not from discussion/referral ties in repeat.

Spatial Variation in Demand within Cities. It is conceivable that the main effect of contagion from colleagues captures not only true contagion, but also spatial variation in demand for the new drug. Because the medical condition is more prevalent among Asians, we add a control for the percentage of Asians in the zip code where the physician practices (2000 US Census). We also add a control for the percentage of households below the poverty level in the zip code. Its effect is not clear *a priori* because one of the main causes of contracting the medical condition is more prevalent among poor people, yet they are less likely to seek and obtain treatment. There is no clinical evidence that any other patient characteristic interacts with drug efficacy. Adding these controls to the model does not significantly improve model fit (Δ-2LL = 5.76, df = 4, $p > .05$) and does not affect the substantive conclusions about contagion in either stage.

Carry-over Effects of Sales Calls. Prior research has reported the presence of 50%-70% carry-over in the effect of monthly detailing (IVV 2011a; Liu and Gupta 2012; Manchanda et al. 2008). We therefore also extend the main model with lagged sales calls. We do so without controlling for endogeneity because the control function approach becomes unwieldy with multiple lagged values of the suspected endogenous variable. Extending the main model with sales calls lagged either by one period or two periods does not significantly improve model fit (both $p > .05$). None of the lagged sales calls effects is significant individually either (all $p > .05$). More importantly, adding lagged sales calls does not affect the research conclusions about social contagion in trial and repeat.

Correlation between Status and Status-Squared. As noted in the second paragraph of Section 2.7, SRL and Ln(Indegree+1) are mean-centered for estimation, so the coefficient of non-moderated contagion is the effect for the "average" physician. The Pearson correlation between mean-centered Ln(Indegree+1) and its square, both of which enter the trial and repeat equations to test for middle-status conformity, is 0.86. As this may cause concerns about collinearity artifacts, we changed the centering point from the mean indegree (0.35) to 2. At this level of centering, the correlation between Ln(Indegree+1) and its square decreases from 0.86 to 0.00. Re-estimating the main model in Table 2.4 with these newly centered covariates produces the same pattern of significant and non-significant coefficients (Table 2.6) and almost exactly the same pattern of overall contagion effects (Figure 2.4). In short, our substantive conclusions do not stem from a high correlation between status and its square.

Figure 2.4. Social Contagion in Adoption and Repeat Incidence, after Centering Ln(Indegree+1) such that It Is Uncorrelated with its Squared Value
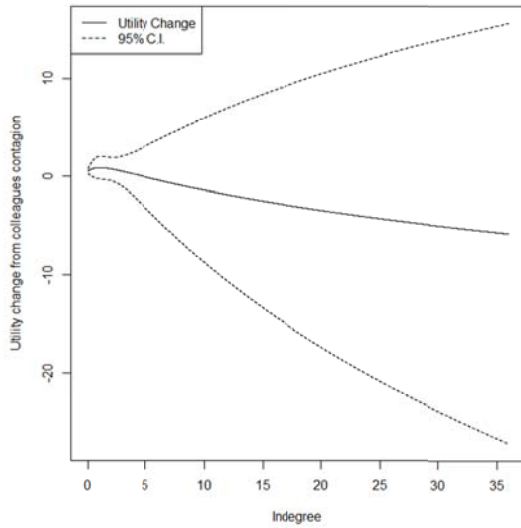
(a) Contagion from Discussion/Referral Ties in Adoption
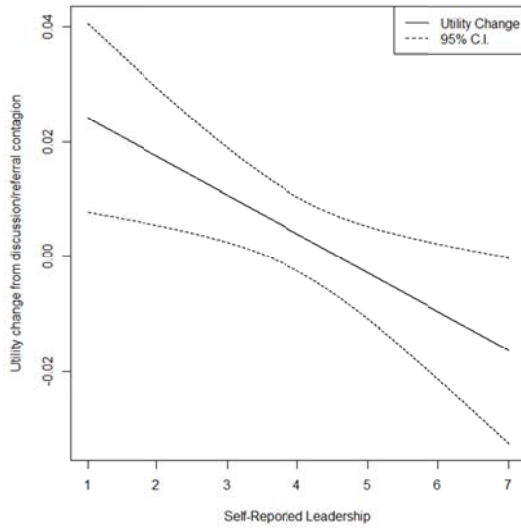
(b) Contagion from Discussion/Referral Ties in Repeat
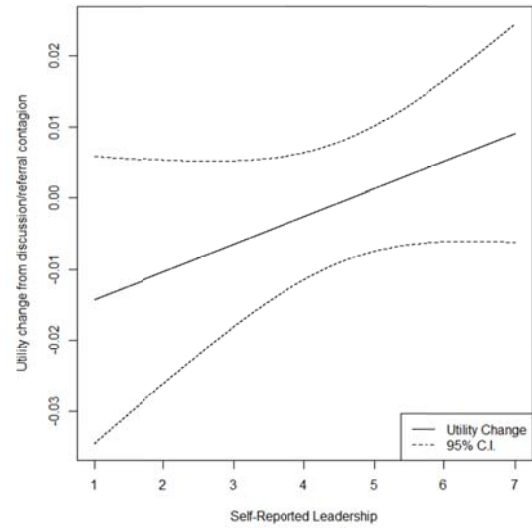
(c) Contagion from Colleagues in Adoption

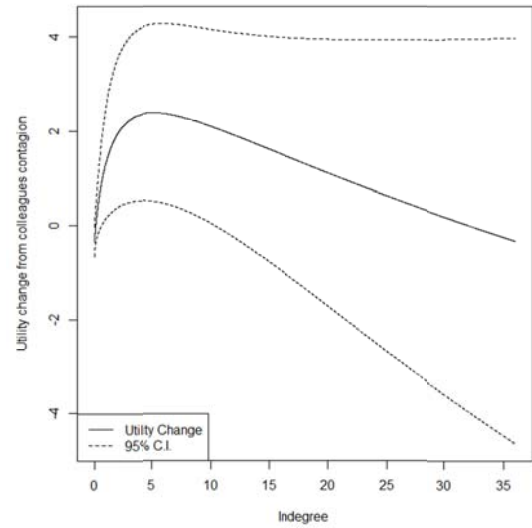(d) Contagion from Colleagues in Repeat

Table 2.6. Model Estimates, after Centering Ln(Indegree+1) such that It Is Uncorrelated with its Square

Variables	Trial Hazard	Repeat Probability
Intercept	-1.972 **	-0.194
	(0.322)	(0.531)
SRL	0.133	-0.088
	(0.069)	(0.157)
Ln(Indegree + 1)	0.137	0.275
	(0.173)	(0.319)
Ln(Indegree + 1)^2	0.020	0.126
	(0.132)	(0.309)
Contagion from Dis / Ref Ties (00s)	0.056	-0.067
	(0.344)	(0.423)
Contagion from Dis / Ref Ties (00s) × SRL	-0.677 **	0.390
	(0.250)	(0.260)
Contagion from Colleagues	0.755	1.968 *
	(0.598)	(0.795)
Contagion from Colleagues × Ln(Indegree + 1)	-0.635	1.189 **
	(1.310)	(0.458)
Contagion from Colleagues × Ln(Indegree + 1)^2	-0.787	-0.840 *
	(1.213)	(0.402)
Solo Practice	-0.044	0.487
	(0.180)	(0.306)
University / Teaching Hospital	0.226	0.975**
	(0.186)	(0.344)
Primary Care	-0.223	10 †
	(0.307)	
Early Referral	-0.286	0.900
	(0.197)	(0.615)
Past Drug 1	0.000	0.010 ***
	(0.002)	(0.003)
Past Drug 2	0.006**	-0.003
	(0.002)	(0.003)
Sales Calls	0.556 **	-0.201
	(0.195)	(0.385)
Endogeneity Correlation	-0.288	0.269
	(0.201)	(0.341)
Ln(q_{it-1} + 1)	-	0.892 ***
		(0.183)
Random Effect Stand. Dev.	0 ††	0.473 ***
		(0.166)
Random Effects Covariance	0 ††	

* $p \le .05$, ** $p \le .01$, *** $p \le .001$. Standard errors in parentheses. LL= -406.79, BIC = 1,270.82.
The model includes several additional covariates: Monthly time dummies (16 for trial, 14 for repeat) and city dummies for LA and NYC in both equations. These estimates are not reported to avoid clutter.
† Dummies for Primary Care and Month 2 are perfect predictors for repeat incidence. We set their coefficients to a very large number (10) so the predicted repeat probability for these physician-months is essentially 1 and the observations do not affect the likelihood estimation.
†† Set to zero based on BIC.

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ESSAY 3. THE IMPACT OF HOMOPHILY AND BALANCE IN CONSUMER SEARCH FROM SOCIAL CONTACTS

3.1. Introduction

Consumers often turn to social contacts for information or advice before making decisions as mundane as which restaurant to go to on a Friday night or as critical as where to have a wedding. They may turn to particular friends with preferences similar to theirs because recommendations from similar others are more diagnostic, or because they are easier to collect and process. On occasion, they may wish to gather recommendations from multiple friends to obtain different points of view. While these different points of view help to make a more informed decision, they can also be cognitively taxing. In this paper, we model and empirically examine the impact of the similarity of preferences among information seekers and their social sources on the benefit and cost of gathering social information, and on ultimate purchasing behavior.

Marketing researchers have long recognized the importance of social learning, i.e., of updating one's beliefs by gathering information from other consumers (Erdem et al. 2005; Roberts and Urban 1988; Zhao et al. 2013). From a managerial stand point, several Internet retailers such as opentable.com (restaurants) and netflix.com (movie / DVD) have begun providing customers with their friends' reviews and are now seeking to use this feature more effectively. Should customers be given the opportunity to gather reviews from others similar to them? Should they be exposed to different points of view? The answer to these questions, we show, depends on how the similarity in preferences

among consumers and their social contacts impacts the way they *search* for information, *learn* from the acquired information, and make a *purchase* decision.

How may similarity of product preferences between consumers and their social contacts impact the way they seek information? A prevalent feature of social settings is that contacts tend to be more frequent among similar people than dissimilar people. This is the principle of homophily, which is captured by the proverbial expression "birds of a feather flock together" (Lazarsfeld and Merton 1954; McPherson et al. 2001).[37] Consumer search has long been documented to be more prevalent among similar consumers in various contexts such as physician selection (Feldman and Spencer 1965), selection of teachers for private tutoring (Brown and Reingen 1987), customer referral programs (Schmitt et al. 2011) and clicking behavior for online advertisements (Goel and Goldstein 2014).

While the vast evidence of homophily indicates that it plays an important role in consumer decisions, it does not directly shed much light on the specific mechanisms at work (Currarini et al. 2009; Kossinets and Watts 2009; Wimmer and Lewis 2010 and Zeng and Xie 2008). For instance, it is possible that the observed effects are due to *opportunity-induced* homophily (which reflects the fact that people have a greater opportunity to meet with similar others than with dissimilar others) rather than *preference-induced* homophily (which reflects the purposive contact with similar others). Even when preference-induced homophily is largely at work, it may be driven because people perceive information gathered from similar others as more diagnostic (Brown and Reingen 1987; Feldman and Spencer

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[37] Lazarsfeld and Merton (1954) distinguish between two types of homophily, status and value. Homophily due to similarity in socio-demographics such as age, gender, race is termed as status homophily. In this paper, we focus on similarity in attitudes or beliefs, which is termed as value homophily. We use similarity among consumers to refer to the similarity in their preference for a product or service.

1965) or due to the ease of interacting with them (McPherson et al. 2001; Price and Feick 1984). While the origins of homophily are important to understand from a theoretical perspective, it is managerially beneficial as well: If homophily is driven largely by diagnosticity of information, Internet retailers offering social evaluations for products may need to consider which information sources to present to stimulate sales. If, however, homophily is driven by the ease of interaction, these retailers may need to focus on how to facilitate the collection of reviews from others.

Going beyond dyads, social systems typically have a mix of homophilous and non-homophilous relationships, and we turn our interest to the consistency of the similarity of preference in a social system. To do so, we build on Heider's Balance theory that conceptualizes the consistency of liking or sentiment in relationships in a social system (Heider 1946). According to Heider's definition, a social triad is balanced if the affect valence (i.e., positive for a liking relationship between two individuals, and negative for dislike) in the system multiples out to be positive and the pattern of relationships is termed as consistent in the system. For instance, the typical phrase "an enemy of my enemy is my friend" applies to a balanced system. We apply the concept of balance to denote the consistency of *preferences* (as opposed to affect) in a social system. The following example shows our use of balance. Consider John with two friends Jim and Mary and suppose John's preferences for food are reasonably similar to Jim's and Mary's. If Jim and Mary are similar (dissimilar) then the patterns of the similarity of preference are consistent (inconsistent), and we denote the social system as balanced (imbalanced).

We shed light on how balance in a social system moderates the reliability of information and cost of search. Statistical theory suggests that information gathered in an

imbalanced social system is more reliable than that gathered in a balanced social system. The intuition is that differing viewpoints will reduce the bias in information gathered from any one source. Consistent with statistical theory, prior studies have shown that the agreement on an issue between individuals who have different viewpoints can increase others' confidence about the consensus (Burt 2001; Goethals and Nelson 1973; Orive 1988). While there is benefit from higher reliability, empirical studies have also shown that people incur more cognitive effort to process inconsistent information (Harkins and Petty 1981; Mandler 1982), which is likely under an imbalanced system. Thus, people may experience greater discomfort of collecting and processing the information from an imbalanced system than a balanced system. The net impact of balance on consumers' search and purchase depends on how consumers resolve the tension between informational benefit and cost.

We investigate the tradeoffs in informational benefit and cost through which the similarity of preferences between consumers and their sources impact their search, learning, and purchase. We do so using a novel incentive compatible stated choice experiment where consumers make purchase decisions for individual music tracks while having access to others' evaluations. Such an experimental approach has several advantages over data from field settings for addressing our research questions (e.g., Centola 2011; Narayan et al. 2011; Shalizi and Thomas 2011; Aral and Walker 2012). First, we manipulate the similarity of preference, which is usually confounded with opportunity of meeting similar others, interpersonal affect, or frequency of interaction in observational data. Second, the experimental design controls for unobserved confounds such as endogenous group formation when identifying social influence. Third, there is no

possibility of passive social learning (from exogenous social information), awareness diffusion, or normative pressure in our study. Finally, because we manipulate the content and availability of social information, which is difficult to observe in secondary data, we can quantify the effect of the similarity of preferences on consumers' decisions.

We analyze our experimental data using a utility-based model of consumer search and purchase. Our approach is based on the cost-benefit framework for assessing the amount of information that people gather from multiple sources to make a more informed purchase decision (e.g., Erdem et al. 2005; Hauser et al. 1993; Ratchford et al. 2003; Seiler 2013). The novel component of our framework is how we capture the features of social learning. We extend the standard multivariate Bayesian learning (SMBL) model which allows for a correlation of information among different sources (Erdem 1998; Winkler 1981). We do so in two ways. First, a consumer may purposely gather information from others who may have systematically different tastes compared to his own. This is unlike learning from own experience (e.g., Erdem and Keane 1996) or from targeted marketing activities such as detailing (e.g., Narayanan and Manchanda 2009) where the information provides unbiased signals for consumers' evaluation. This is also unlike prior models of social learning (Roberts and Urban 1988; Erdem et al. 2005; Zhao et al. 2013) that assume social reviews provide unbiased signals. Second, social search and learning is allowed to be affected by behavioral aspects related to the similarity of preferences among information receivers and providers. For instance, people may perceive information from similar others to be more diagnostic than from dissimilar others. SMBL model cannot accommodate such aspects (as discussed in detail later). Our proposed extended multivariate Bayesian learning (EMBL) model flexibly

accommodates behavioral aspects related to the similarity of preferences. While prior research has accommodated such behavioral aspects of learning as forgetting (Mehta et al. 2004), salience of recent signals (Camacho et al. 2011), and valence of signals (Zhao et al. 2011), a model that accommodates behavioral aspects related to the similarity of preferences has not been well developed.

Our results provide two key insights regarding social learning. First, we present evidence of preference-induced homophily in consumer search. Consumers prefer to gather information from similar others, and since opportunity-induced homophily is ruled out with our experimental design, this must stem from preference-induced homophily. Our modeling framework pinpoints the key driver behind this phenomenon. In our context, preference-induced homophily is driven by the higher diagnosticity of information gathered from similar others (Brown and Reingen 1987; Feldman and Spencer 1965) rather than the comfort of collecting such information (McPherson et al. 2001; Price and Feick 1984). Second, balance of a social system has a nuanced effect on social learning. On the one hand, people understand that information from an imbalanced social system is more reliable (Burt 2001; Goethals and Nelson 1973; Orive 1988) than a balanced one. On the other hand, people experience greater discomfort under an imbalanced system (Heider 1946) and must expend greater cognitive effort to process the information under imbalanced system (Harkins and Petty 1981; Mandler 1982), so the cost of information-seeking is higher. Our results suggest that people tend to search less under an imbalanced system, as compared to a balanced one. However, the lower amount of search under imbalance has greater informational benefit. This is consistent with both higher informational benefit and higher cost in imbalanced versus balanced social system.

The remainder of the essay is organized as follows. We begin with describing our theoretical framework and develop hypotheses. This is followed by a description of our research setting, experimental design and description of data. Next, we build a formal model for consumers' decisions in our setting, and specify the model for the empirical application. We conclude with our results and its implications for theory and practice.

3.2. Theoretical Framework and Hypothesis

In this section, we develop a theoretical framework and propose specific hypotheses for the information benefit and search cost through which the similarity of preference between consumers and their sources may impact their search decisions and social learning.

3.2.1. Preference Similarity with Sources

The diagnosticity of information from a source depends on the strength of association between an individual's preference and that of the source. If a source with tastes similar to those of a focal consumer gives positive feedback about a product, then that consumer may infer that he will like it. If the source has dissimilar tastes to those of the focal consumer, then he may infer that he will not like it. According to a normative model of consumer learning (SMBL), positive feedback from similar others has equivalent information as negative feedback from dissimilar others. In other words, the feedback from similar and dissimilar others with the same strength of association should be equally diagnostic. Likewise, prior studies of consumer search have not considered the preference similarity of the information seeker with their sources and hence implicitly assume that there is no difference in the cost of collecting information from similar or dissimilar

sources as long as a consumer searches for the same amount (e.g., Hauser et al. 1993; Ratchford et al. 2003).

This prediction is at odds with prior evidence of homophily in social search (Feldman and Spencer 1965; Brown and Reingen 1987; Schmitt et al. 2011). However, studies using an observational design have difficulty in identifying whether this tendency to seek information from similar others than dissimilar others is driven by a greater opportunity to meet with similar others (i.e., opportunity-induced homophily) or a purposive contact with similar others (i.e., preference-induced homophily). If the evidence of homophily in the previous studies is driven *only* by opportunity-induced homophily, then there is little reason to expect that people may find the information from similar others as more diagnostic or may have less discomfort of search from similar others. In such cases, an SMBL model would hold. However, clear evidence of preference-induced homophily in social search would be inconsistent with SMBL. In this study, we test the diagnosticity of information and cost of search as two key drivers of preference-induced homophily where we rule out opportunity-induced homophily in our experimental setting.

Several prior studies using an experimental design provide deeper insights into drivers at work. One stream of research shows that consumers find information from similar others to be more relevant. For instance, Gilly et al. (1998) suggest that people may pay more attention to the information from similar others than that from dissimilar others. Yaniv et al. (2011) note that consumers find more personally relevant information from similar others, who tend to share similar product needs, than from dissimilar others. We build on this past work and propose that the greater diagnosticity of information gathered from similar others is actually a driver of preference-induced homophily.

Another stream of research has investigated the cost of associating with dissimilar others (with similarity based on socio-demographics). For example, Stephan and Stephan (1985) show that people have greater anxiety and discomfort when interacting with dissimilar others. Please see Pettigrew and Tropp (2006) for a summary of findings from intergroup contact theory. Given these findings, we expect that such discomfort may also stem from gathering information from others with dissimilar preferences. In sum, we propose the following hypotheses.[38]

H1a: For the same amount of search, information collected from similar others will be more diagnostic than information collected from dissimilar others.

H1b: People incur lower (mental) cost of collecting and processing information from similar than from dissimilar others.

3.2.2. Structural Balance

Heider (1946) defined balance in a social triad based on the consistency of liking or sentimental relationships in a social system. He proposed that a triad is balanced (imbalanced) if the valence of liking relationship (i.e., positive for like, and negative for dislike) in the triad multiples out to be positive (negative). Cartwright and Harary (1956) expanded the definition on balance for social systems larger than a triad. Such expansion made the concept applicable to a wider range of situations such as the development of intelligence (Piaget 1972), the creation of commonly shared norms and values (Sternberg

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[38] Little empirical research has jointly tested the impact of the two drivers on preference-based homophily. This is likely as without a formal model it would be difficult to disentangle whether the factors that are important for search are due to their impact on informational benefit or the cost. The argument is similar in spirit to the issue of disentangling persuasive and informative effects of advertising without a formal model (Ackerberg 2001).

1998), and dynamics of social network formation (Hummon and Doreian 2003). We apply the concept of balance to denote the consistency of *preferences* (as opposed to affect) in a social system. Thus, we term a social system as balanced if the valence of preferences (i.e., positive for similar preferences, and negative for dissimilar preferences) in a system multiples out to be positive.

Normative model of consumer learning (i.e., SMBL) indicates that the reliability of information is greater from an imbalanced than balanced social system. According to the statistics of correlation, the variance of a sum of two random variables is smaller when the correlation between the two random variables is negative than positive. This simple intuition applies to SMBL:[39] Any bias in information gathered from sources in an imbalanced system is expected to be in opposing directions, so the overall bias will be reduced when one integrates the information. In contrast, the information from sources in a balanced system share the bias in the same direction, so bias will be amplified upon integration. We explain this notion in detail in Section 3.10.3 (Appendix). This normative prediction has related empirical findings that the exposure to the different vantage points increases the reliability of information (Burt 2001; Goethals and Nelson 1973; Orive 1988). As there is greater opportunity of acquiring information from different viewpoints from an imbalanced system, we propose the following hypothesis for the manner in which balance will affect the reliability of information.

H2a: For the same amount of search, information collected under an imbalanced system will be more reliable than information collected under a balanced system.

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[39] The intuition also applies to modern portfolio theory (Markowitz 1952). For a given level of return, the overall risk (variance) of a portfolio can be reduced by investing in assets with negative correlation because the poor performance of one asset can be offset with the good performance of another.

Prior models on consumer search (e.g., Hauser et al. 1993; Ratchford et al. 2003) implicitly assume that the pattern of consistency of relationships among sources does not affect the cost of collecting information and processing it. Past research suggests that this may be a strong assumption. According to Heider (1946), people tend to feel greater discomfort and tension under an imbalanced system when there is inconsistency of liking or affect with others. We expect that people will have experience greater discomfort even when there is inconsistency in preferences with others. This is likely as people may have greater cost of processing information gathered from an imbalanced system where inconsistent information is expected and requires more cognitive effort to digest (Harkins and Petty 1981; Mandler 1982). Thus, we propose the following hypothesis:

H2b: People incur greater (mental) cost of collecting and processing information under an imbalanced system than a balanced system.

3.3. Research Setting and Experimental Design

3.3.1. Research Setting

To test how similarity in preferences that consumers have with their sources may impact their decision to search and how much they learn, we characterize the research setting based on the following features - (*i*) the number of available information sources, (*ii*) the relationship between consumers and their contacts, (*iii*) the decision framework, and (*iv*) the type of search decision.

Number of sources. Suppose a consumer has N friends who have evaluated the product. In this case, the similarity of preference with N friends (his direct connections) and the similarity of preference between $N(N-1)/2$ pairs will impact the way that he

collects and processes the information. Even with *N*=4 (a relatively small number) there are 10 different similarity parameters. The general problem is clearly challenging. Thus, to maintain the essence of the problem and make it tractable for testing theory, we assume that a consumer's social sources can be categorized into two exogenous groups (groups A and B). As an example, a consumer may have several friends who can be categorized into those he knows from school or from work. Thus, the consumer and his two social groups form a social triad.

Type of relationship between the consumer and his contacts. We assume that a consumer has a mature relationship with both social groups and so knows the similarity of preferences in the social triad.[40] Figure 3.1 shows an example of a triad where the focal consumer and two social groups form the nodes and the link among any two nodes denotes the similarity of preference between them. In the figure, *a* (*b*) denotes the similarity of preference between a consumer and group A (group B), and *c* denotes the similarity of preference between the two social groups. We operationalize the similarity measure as correlation: the value gets closer to 1 (-1) as the positive (negative) association of preference between two nodes gets stronger. There is no association of preference between two nodes when the similarity measure between them is 0.

We denote that a social system is balanced if the valence of preference similarity multiplies out to be positive; the triad is balanced when *abc > 0,* and imbalanced when *abc < 0*. Under balance (imbalance), the pattern of the similarity of preference is consistent (inconsistent) with each other. For instance, if a focal consumer has a similar

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[40] There are contexts where consumers may be uncertain about their similarity of preference with others and learn about them over time. In addition, consumers' social contacts may be categorized into multiple (more than two) groups and these may not be exogenous.

preference with the two social groups ($a>0$ and $b>0$) and both groups also have similar preferences ($c>0$), a triad has a balanced preference ($abc > 0$). If, however, a focal consumer has a similar preference with similar preference with the two social groups ($a>0$ and $b>0$) but both groups have dissimilar preferences ($c<0$), a triad has an imbalanced preference ($abc < 0$).

Figure 3.1. The Similarity of Preference in a Social Triad
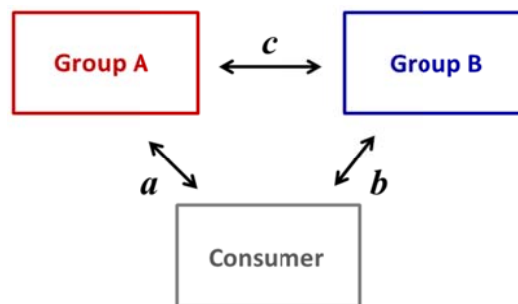
In field data, similarity of preferences may be associated with contacts' willingness to provide feedback and their honesty. In our experiment, we isolate the similarity of preference from these confounds as all information providers are willing to share their feedback and are honest in their evaluations regardless of the dyadic similarity of preference and the structural balance.

Decision framework. A consumer has to make a *purchase* decision, which is whether to purchase a specific product. To make a more informed decision, he collects the evaluations from his contacts. Each individual evaluation is a "signal." A consumer decides on the number of signals to collect from each group. We term this as his *search* decision. As summarized in Figure 3.2, we focus on the scenario where a consumer

makes a search decision, processes the information acquired from search, and finally

makes a purchase decision.

Figure 3.2. Decision Framework
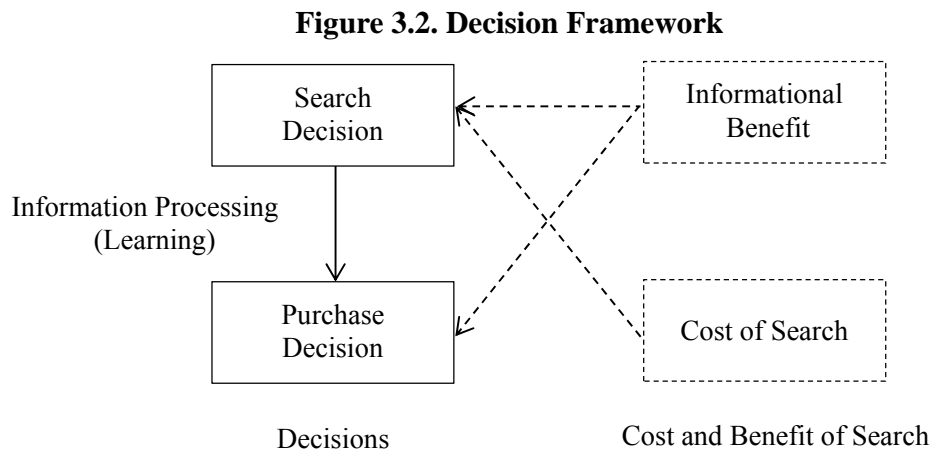
We posit that respondents make their search decision based on their belief about how

informative the signals would be and how effortful it would be to collect and process

them. Respondents make their purchase decision after gathering signals. The purchase

decision is affected by how informative the collected signals are. However, the cost of

search (which respondents have already experienced) does not directly affect the

purchase decision – it only does so indirectly through the search amount.

Type of search decision. A consumer makes the search decision before he observes

any signals, so a search decision in this study denotes *simultaneous search* (or fixed

sample search) about a specific product.[41]

While our research setting is appropriate for theory testing, it also captures real world

contexts in which consumers are time constrained and cannot sequentially decide to

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[41] Morgan and Manning (1985) have found that either sequential or simultaneous search (or a combination of both) can be optimal for a consumer. More recently, De los Santos et al. (2012) estimated both simultaneous and sequential search models in the context of online search for experiential products, which is also the setting in our experiment, and found that a simultaneous search model fit the data better.

collect information. For instance, suppose Ryan is deciding whether or not to dine in a particular restaurant on a Friday evening. As he shares a lot of common dining experiences with his two groups of friends, for instance neighbors and colleagues, Ryan knows how similar his taste for restaurants is to those of the two groups of friends and how similar the two groups' tastes are to each other. Ryan sends out multiple messages (e.g., SMS messages) to his friends from each group at the same time, and waits for their evaluations. After collecting feedback, Ryan updates his belief about how much he will like the restaurant, and then decides whether to visit the restaurant or not.

3.3.2. Experimental Design

Our design is a novel incentive compatible stated choice experiment in which we control for potential confounds typically found in contexts with social influence (e.g., endogenous group formation). Our experiment has two phases: Phase 1 (calibration task) and Phase 2 (incentive compatible choice task). Each phase is described below.

In Phase 1, participants listen to 10 songs of different genres and rated each song on a 0-10 scale.[42] Participants are told to rate each song carefully as their ratings would be used in matching them with other participants and that such matching would be useful in the second phase of the experiment.

In Phase 2, respondents make a purchase decision for 18 unidentified songs (no artist or genre was specified) without listening to them.[43] All songs are worth $1.25 on iTunes, and participants know it. For each song, a purchase decision for participants means

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[42] We generated two different lists of 10 songs, and each participant was randomly assigned to one of the two lists. All songs have similar average evaluation on iTunes and the order was randomized across participants

[43] We did not identify the songs to isolate the causal impact of preference similarity on search.

deciding between receiving the mp3 file of the unidentified song or $1 cash (this cash was in addition to participation fee). For instance, participants can decide to purchase each one of those 18 songs or not purchase any one of them. Participants' decisions are incentive aligned as they are told that one out of all 18 unidentified songs will be randomly picked at the end of the survey, and they will be compensated with either the actual song (if they had chosen to purchase that song) or $1 additional cash otherwise.

Each song is described by six attributes. The first two attributes gives a summary of aggregate evaluations for the song - (1) the average of the song's rating (R_j^{0M}) from iTunes on a 0-10 scale and (2) the standard deviation (τ_j^M) which captures the population heterogeneity in song evaluations. The superscript M denotes manipulated attributes. If the aggregate average evaluation is high, respondents should expect to like the song. The average of aggregate evaluation has three levels: low (0.5-3.0), medium (3.0-7.0) and high (7.0-9.5). Note that while each level has a range, a respondent sees a randomly chosen value in the range corresponding to a level. If the aggregate evaluations are more dispersed, respondents will be more uncertain about how much they would like the song. The standard deviation of aggregate evaluations has three levels (0.5-1.5, 1.5-3.5, 3.5-3.5), and the respondent sees an actual value randomly chosen within a range corresponding to a given level.

In each profile, participants also have access to social information before they make a purchase decision. Respondents are told that 200 Undergraduates and 200 MBAs have previously listened to the same 10 songs as they did in Phase 1 and also the 18 unidentified songs that they will be making purchase decisions for. They are then told

that genre-specific similarity of preference measures in the triad have been computed

based on their evaluations of the 10 songs in Phase 1. We explain that the similarity

measures are constructed by comparing the respondents' ratings and the *average* rating

within each group. Respondents are provided with (3) similarity in preference between

the participant and undergraduates (a_j^M), (4) between the participant and MBAs (b_j^M),

and (5) between undergraduates and MBAs (c_j^M). We manipulate all three measures for

each song (as the superscript indicates). The absolute similarity between the participant

and Undergraduates as well as the participant and MBAs has three levels each (0.1-0.3,

0.3-0.7, 0.7- 0.9). A respondent sees a similarity measure for each source which has an

absolute value randomly chosen within a range corresponding to a given level, and a sign

randomly chosen to be positive or negative (representing similar or dissimilar preference).

The similarity in preference between MBAs and Undergraduates (c_j^M) is randomly

chosen within a range where the covariance of triadic similarity satisfies regularity

conditions (Section 3.10.2 in Appendix).

Finally, (6) the standard deviation of evaluations within the two social groups (σ_j^M)

captures within-group heterogeneity in evaluations. This is set to be equal between the

two groups, and fixed to be one-half of the standard deviation of the aggregate

evaluations. We do not manipulate it independently primarily to reduce the complexity of

the problem for respondents.[44] We generate two orthogonal designs of 18 profiles

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[44] In a pilot study with a convenience sample of 10 students, we found that it was difficult for them to understand all the information in a profile where we had different standard deviations for each of the two social groups. As our primary goal is to understand how homophily and balance impact consumers' search and purchase behavior, we believe that the lack of orthogonal manipulation of the within-group

(unidentified songs) from a full factorial design using Proc Optex in SAS. Each participant in the study is assigned to one of the two designs.

For each unidentified song, respondents first make the search decision. Based on the aggregate evaluations of the song and preference similarity measures, they decide on how many individual evaluations (i.e., signals) to acquire from each social group. As each group consists of 200 participants, the maximum number of signals that one can acquire from each group is 200. Acquisition of each signal is not costless for respondents – they have to wait for half a second to retrieve each signal. Figure 3.3a shows an example of the search decision interface.

After completing the search decision (and having waited for the designated amount of time), respondents move to the purchase decision (Figure 3.3b for the interface). Respondents are provided with the average rating of randomly sampled individuals from each social group with the sample size based on their search decision. In Figure 3.3a, for example, a respondent decides to collect 2 (3) signals from undergraduates (MBAs). He is told that the average rating among the 2 undergraduates (3 MBAs) is 6.0 (5.4). We manipulate the signals that respondents see. After observing the signals from each group, respondents make their purchase decision. This completes the task for one song, and respondents go through the same task for 18 songs.

Figure 3.3. Screenshot of Survey Interface

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heterogeneity should have little impact.

(a) Search decision interface

(b) Purchase decision interface

3.3.3. Summary Statistics

Our data contains 2,736 (=152 subjects × 18 profiles) pairs of search (how many signals

to acquire) and purchase decisions (whether or not to purchase a song). Table 3.1

provides the summary statistics of search and purchase decisions. The average total

search amount is around 20 signals per song. As a check for our manipulation of the

similarity of preferences of respondents with those of the two groups, there is no

difference in the search amount between Undergrads and MBAs ($p=0.26$).[45] In 33% of

observations, respondents choose to purchase a song, and 88% of them purchase at least

one song.

Table 3.1. Summary Statistics of Search and Purchase Decision

	Mean	SD	2.5%	25%	50%	75%	97.5%
Search from Undergrads	9.9	17.2	0	2	5	10	50
Search from MBAs	9.3	17.7	0	1	5	10	50
Total Amount of Search	19.2	32.2	0	5	10	20	100
Purchase Decision (0 is no purchase; 1 is purchase)	0.33	0.47					

3.3.4. Descriptive Results

Prior to developing a formal model, we investigate the drivers for the two decisions using

simple regressions. The results of the regressions broadly support our hypotheses and

show that the preference similarity with social sources and the overall balance in the

social system play a significant role in consumers' decisions. Please refer to Section

3.10.1 in Appendix for details. In the regression models, however, the endogenous

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[45] In our study, there is roughly equal number of contacts to undergrads and MBA although 80% of respondents are undergrads. Our finding is consistent with past studies which show that the feedback from those who share similar preferences impact consumer decisions, but the feedback from those who share similar demographics does not (Yaniv et al. 2011).

relationship between search and purchase decisions is not accounted for. In addition, without a formal model, it is not possible to disentangle whether the attributes that are significant for the amount of search are due to their impact on either the informational benefit from search or the cost of accessing information (or both).

3.4. Model

In this section, we develop a formal model for decisions that consumers make in the stated-choice experiment. For each song, a consumer makes two interconnected, but temporally separated decisions: In stage 1 (t_1), a consumer decides how many signals to acquire about song evaluation from other consumers ("search decision") and in stage 2 (t_2), makes a binary decision of whether or not to purchase the song ("purchase decision"). Between the two stages, a consumer processes any collected information. We will make a distinction between a normative consumer learning model (SMBL) and our proposed specification (EMBL) in the empirical section.

3.4.1. Utility Specification

We assume that consumers are utility maximizers and the search and purchase decisions are driven by the same utility function. Let $I_i(t)$ denote the information set of consumer i at time t. For notational simplicity, we omit the song subscript j in the information set. The information set at a particular time characterizes the state of the consumer and includes all known factors that affect current utility at time t and any future utilities. In our setting, there are two time points, t_1 and t_2, and a consumer has a different information set at these two time points.

We define a consumer i's indirect utility from purchasing song j at time t using a constant absolute risk aversion (CARA) specification (Narayanan and Manchanda 2009; Zhao et al. 2013):

$$U_{ij}\left(I_i(t)\right) = \alpha_i - \exp\left(-\beta_i R_{ij}^E\left(I_i(t)\right)\right) + \varepsilon_{ij}\left(I_i(t)\right), \qquad t = t_1 \text{ or } t_2. \tag{3.1}$$

The term R_{ij}^E refers to consumer i's rating (or evaluation) of song j and is realized only *after* product experience. We use the term $R_{ij}^E\left(I_i(t)\right)$ to denote explicitly that consumer i's knowledge about R_{ij}^E at time t depends on his information set $I_i(t)$. The parameter α_i captures the baseline utility of purchasing a song,[46] and β_i captures the effect of song evaluation on purchase utility. The error term $\varepsilon_{ij}\left(I_i(t)\right)$ is also dependent on the information set. We assume that at the time of search (Stage 1), the error term is stochastic to consumers while at the time of purchase (Stage 2), it is observable. The assumption implies that there is a temporal separation between the search and purchase decisions. Finally, the utility from not purchasing the song is set to 0.

3.4.2. Stage 1: Search Decision

To ease the exposition of the search model, we first explain how a consumer makes the purchase decision after conducting a specific amount of search. This discussion illustrates the link between search and its impact on purchase. Next, we specify the beliefs that consumers hold for stochastic variables at the time of search. Finally, we describe how consumers determine their optimal level of search given these beliefs.

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[46] In the experiment, we did not provide search attributes of the songs (e.g., genre, artist, etc) to isolate the impact of aggregate evaluations and preference similarity on consumer search decisions. We can easily generalize our model to incorporate search attributes by including the attribute-based utility that a consumer associates with a song.

Link between search and purchase. Suppose consumer i collects $n_{ij} = \left(n_{ij}^A, n_{ij}^B \right)$

signals from the two groups and the average of these collected signals is $\overline{s}_{ij} = \left(\overline{s}_{ij}^A, \overline{s}_{ij}^B \right)$.

After this search, consumer i's information set at time $t_2, I_i(t_2)$, includes n_{ij} and $\overline{s}_{ij}$. Let

$f_i^{R^E}(R_{ij}^E \mid I_i(t_2))$ denote consumer i's belief about his own evaluation given this

information set.

As consumer i is uncertain about his own song evaluation, he will determine the

expected utility of purchasing song j with respect to his beliefs about the song. Consumer

i will purchase song j if and only if it provides higher expected utility than not purchasing

it. Equivalently, consumer i will purchase song j if:

$$E_{R_{ij}^E(I(t_2))}\left[U_{ij}\left(I_i(t_2) \right) \right] > 0, \tag{3.2}$$

where E[.] is the expectation operator. Using the expression in Equation 3.1, the term

$E_{R_{ij}^E(I(t_2))}\left[U_{ij}\left(I_i(t_2) \right) \right]$ can be expressed as:

$$E_{R_{ij}^E(I(t_2))}\left[U_{ij}\left(I_i(t_2) \right) \right] = E_{R_{ij}^E(I(t_2))}\left[\alpha_i - \exp\left(-\beta_i R_{ij}^E(I(t_2)) \right) \right] + \varepsilon_{ij}\left(I(t_2) \right), \tag{3.3}$$

$$= u_{ij}\left(I_i(t_2) \right) + \varepsilon_{ij}\left(I(t_2) \right),$$

where $u_{ij}\left(I_i(t_2) \right)$ denotes the systematic component of the expected utility of purchase.

Note that the stochastic component of utility, ε_{ij}, is observable to consumers at Stage 2.

The above description emphasizes that a consumer's decision of whether or not to

purchase a song depends on his earlier search decision as the number and the content of

signals (n_{ij} and $\overline{s}_{ij}$) alter his information set at the time of purchase.

Consumer beliefs. In this section, we elaborate on consumers' beliefs about the relevant stochastic variables given the search decision, n_{ij}, and the information set at the search stage, $I(t_1)$. We specify beliefs that consumers hold about (1) own evaluation prior to search, $R_{ij}^E\left(I(t_1)\right)$, (2) signals to be observed, $\bar{s}_{ij}\left(I(t_1), n_{ij}\right)$, and (3) random component of utility, $\varepsilon_{ij}\left(I(t_1)\right)$.

Consumer i is uncertain about his own evaluation $\left(R_{ij}^E\right)$ for a song j. Likewise, we assume that he is also uncertain about the average evaluations in the two social groups $\left(\bar{R}_{ij}^A, \bar{R}_{ij}^B\right)$. Uncertain beliefs that a consumer has about vector $R_{ij} = \left(R_{ij}^E, \bar{R}_{ij}^A, \bar{R}_{ij}^B\right)$ is represented by the distribution, $f_i^R\left(R_{ij} \mid I_i(t_1)\right)$.

As explained earlier, a consumer knows the aggregate distribution of song evaluation when he makes a search decision (t_1). As a respondent's evaluation is a sample from the population distribution of song evaluation, his prior belief about his evaluation $\left(R_{ij}^E\right)$ is represented by the aggregate distribution which is normally distributed with mean R_{ij}^0 and variance τ_{ij}^2. A consumer also knows the size of the two social groups $\left(N_{ij} = \left(N_{ij}^A, N_{ij}^B\right)\right)$, so the average of any signals gathered from the two social groups $\left(\bar{R}_{ij}^A, \bar{R}_{ij}^B\right)$ are drawn from the distribution with mean R_{ij}^0 and variance $\left(\tau_{ij}^2/N_{ij}^A, \tau_{ij}^2/N_{ij}^B\right)$ respectively.

We assume that the consumer knows the similarity of preference in the social triad, $\rho_{ij} = \left(a_{ij}, b_{ij}, c_{ij}\right)$ – how similar (or dissimilar) his preferences are to those of each social

group $\left(a_{ij}, b_{ij} \right)$, and how similar (or dissimilar) the preferences of the two social groups

are $\left(c_{ij} \right)$. Therefore, a consumer believes that the vector R_{ij} is a multivariate sample from

the population distribution. With the normality assumption, we can express

$f_i^R \left(R_{ij} \mid I_i(t_1) \right)$ as:

$$f_i^R \left(R_{ij} \mid I_i(t_1) \right) = N \left(\begin{bmatrix} R_{ij}^0 \\ R_{ij}^0 \\ R_{ij}^0 \end{bmatrix}, \tau_{ij}^2 \times \begin{bmatrix} 1 & a_{ij}/\sqrt{N_{ij}^A} & b_{ij}/\sqrt{N_{ij}^A} \\ a_{ij}/\sqrt{N_{ij}^A} & 1/N_{ij}^A & c_{ij}/\sqrt{N_{ij}^A N_{ij}^B} \\ b_{ij}/\sqrt{N_{ij}^A} & c_{ij}/\sqrt{N_{ij}^A N_{ij}^B} & 1/N_{ij}^B \end{bmatrix} \right). \quad (3.4)$$

As a covariance matrix should be positive definite, there are restrictions on the values

for the similarity of preference $\left(\rho_{ij} \right)$ (see Section 3.10.2 in Appendix). Given Equation

3.4, a consumer i's initial belief about his own rating, $f_i^{R^E} \left(R_{ij}^E \mid I_i(t_1) \right)$, is obtained from

the marginal distribution:

$$f_i^{R^E} \left(R_{ij}^E \mid I_i(t_1) \right) = N \left(R_{ij}^0, \tau_{ij}^2 \right). \tag{3.5}$$

A consumer believes that a signal from group A or B is *i.i.d.* normal with an unknown

average evaluation ($R_{ij}^A \left(I_i(t_1) \right)$ or $R_{ij}^B \left(I_i(t_1) \right)$) and standard deviation (σ_{ij}^A or σ_{ij}^B). With

these assumptions, the belief about the sample average of $n_{ij} = \left(n_{ij}^A, n_{ij}^B \right)$ signals,

$f_i^{\bar{s}} \left(\bar{s}_{ij} \mid I_i(t_1), n_{ij} \right)$, is

$$f_i^{\bar{s}} \left(\bar{s}_{ij} \mid I_i(t_1), n_{ij} \right) = N \left(\begin{bmatrix} R_{ij}^0 \\ R_{ij}^0 \end{bmatrix}, \begin{bmatrix} \tau_{ij}^2/N_{ij}^A + \left(\sigma_{ij}^A\right)^2/n_{ij}^A & c_{ij}/\sqrt{N_{ij}^A N_{ij}^B} \\ c_{ij}/\sqrt{N_{ij}^A N_{ij}^B} & \tau_{ij}^2/N_{ij}^B + \left(\sigma_{ij}^B\right)^2/n_{ij}^B \end{bmatrix} \right), \quad (3.6)$$

where the expression is obtained by combining the uncertainty about the average among collected signals from each group and the uncertainty about average evaluation in each group.

The utility error is stochastic from consumers' perspective in the search stage. A consumer believes that utility error is normally distributed with mean 0 and unit variance, i.e., $f_i^\varepsilon(\varepsilon_{ij} \mid I_i(t_1)) = N(0,1)$.

Optimal search. For consumer i, let k_{ij}^A and k_{ij}^B denote the search cost for obtaining a signal from the social groups A and B, respectively. Such cost could be due to the hassle of collecting information or cost of processing information. The cost can differ by group, and the consumer knows this cost.

Given the information set of consumer i at t_1, $I_i(t_1)$, the utility from search $U_{ij}^S(.)$ for a specific amount of search n_{ij} is as follows.

$$U_{ij}^S\left(I_i(t_1),\ n_{ij}\right) = \begin{cases} U_{ij}\left(I_i(t_2) \mid I_i(t_1), n_{ij}\right) - k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B + \xi_{n_{ij}}, & \text{when song } j \text{ is purchased,} \\ -k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B + \xi_{n_{ij}}, & \text{when song } j \text{ is not purchased.} \end{cases}$$

(3.7)

Here the term $\xi_{n_{ij}}$ is known to the consumer. The term can be interpreted as a fixed cost of gathering n_{ij} signals (De los Santos et al. 2013). We use the term $U_{ij}\left(I_i(t_2) \mid I_i(t_1), n_{ij}\right)$ to denote explicitly that the consumer i's utility is based on how his information set at t_2 will change due to his information at t_1 and the amount of search he decides to engage in. The term contains two key components that are uncertain to consumers at time t_1. First, the utility error is stochastic, and consumer believes that it

follows $f_i^\varepsilon(\varepsilon_{ij} \mid I_i(t_1))$. Second, the consumer is yet to observe any signals, and his

uncertain belief follows $f_i^{\bar{s}}(\bar{s}_{ij} \mid I_i(t_1), n_{ij})$. The latter is important as it indicates that a

consumer does not know what beliefs he will hold about his own song evaluation,

$f_i^{R^E}(R_{ij}^E \mid I(t_2))$, at the time of purchase. Thus, a consumer i's expected utility from search

for song j's evaluations is:

$$E\left[U_{ij}^S\left(I_i(t_1),\ n_{ij}\right)\right] = E_{I_i(t_2)\mid I_i(t_1)}\left[E_{R_{ij}^E(I(t_2))}\left[U_{ij}\left(I_i(t_2)\mid I_i(t_1), n_{ij}\right)\right]\right] - k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B + \xi_{n_{ij}}. \quad (3.8)$$

The above equation implies that expected search utility for a consumer equals the

expected purchase utility (with respect to uncertain belief about own evaluation after

search) after integrating over all possible $I(t_2)$ he may have given $I(t_1)$. Given Equations

3.2 and 3.3, we can rewrite Equation 3.8 as:

$$E\left[U_{ij}^S\left(I_i(t_1),\ n_{ij}\right)\right] = E_{I_i(t_2)\mid I_i(t_1)}\left[1_{I_i(t_2)} \times \left(u_{ij}\left(I_i(t_2)\right) + \varepsilon_{ij}\left(I(t_2)\right)\right)\right] - k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B + \xi_{n_{ij}}, \quad (3.9)$$

where $1_{I_i(t_2)}$ denotes an indicator which is 1 if a consumer i purchases a song j, and 0

otherwise. A purchase decision is made based on Equation 3.2, so it is a function of

$u_{ij}\left(I_i(t_2)\right)$ and $\varepsilon_{ij}\left(I(t_2)\right)$.

The expectation operator $E_{I_i(t_2)\mid I_i(t_1)}[\cdot]$, can be decomposed into the expectation over

signals that a consumer may receive, $E_{\bar{s}_{ij}(I_i(t_2)\mid I_i(t_1))}[\cdot]$, and the expectation over the utility

errors, $E_{\varepsilon_{ij}(I_i(t_2)\mid I_i(t_1))}[\cdot]$. Equation 3.9 can be rewritten by using the properties of conditional

expectation and normality of utility error as:

$$E\left[U_{ij}^S\left(I_i(t_1),\ n_{ij}\right)\right] = E_{\bar{s}_{ij}(I_i(t_2)\mid I_i(t_1))}\left[E_{\varepsilon_{ij}(I_i(t_2)\mid I_i(t_1))}\left[1_{I_i(t_2)} \times \left(u_{ij}\left(I_i(t_2)\right) + \varepsilon_{ij}\left(I(t_2)\right)\right)\right]\right] \quad (3.10)$$

$$-k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B + \xi_{n_{ij}},$$

$$= E_{\bar{s}_{ij}(I_i(t_2)|I_i(t_1))} \Big[\Pr\big[u_{ij}\big(I_i(t_2)\big) + \varepsilon_{ij}\big(I(t_1)\big) > 0 \big]$$
$$\times E\big[u_{ij}\big(I_i(t_2)\big) + \varepsilon_{ij}\big(I(t_1)\big) \,|\, u_{ij}\big(I_i(t_2)\big) + \varepsilon_{ij}\big(I(t_1)\big) > 0 \big] \Big] - k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B + \xi_{n_{ij}},$$

$$= \underbrace{ E_{\bar{s}_{ij}(I_i(t_2)|I_i(t_1))} \Big[\Phi\big[u_{ij}\big(I_i(t_2)\big) \big] u_{ij}\big(I_i(t_2)\big) + \phi\big[u_{ij}\big(I_i(t_2)\big) \big] \Big] }_{\text{Informational Benefit from Search}} - \underbrace{ k_{ij}^A n_{ij}^A - k_{ij}^B n_{ij}^B }_{\text{Cost of Search}} + \xi_{n_{ij}},$$

$$= v^b\big(n_{ij} \,|\, I_i(t_1)\big) - v^c\big(n_{ij}\big) + \xi_{n_{ij}},$$

where $v^b\big(n_{ij} \,|\, I_i(t_1)\big)$ denotes the expected informational benefit from search, and $v^c\big(n_{ij}\big)$

denotes the cost of search.

We assume that the consumer evaluates the expected utility associated with each level

of search. He then chooses the level (n_{ij}^*) that maximizes the expected utility from search.

Given the size of each group (N_{ij}), the consumer cannot contact more than $N_{ij}^A\big(N_{ij}^B\big)$

number of people from each group. Thus,

$$n_{ij}^* = \arg\max_{n_{ij}} E\Big[U_{ij}^S\big(I_i(t_1),\, n_{ij}\big) \Big], \text{ where } 0 \le n_{ij}^{A*} \le N_{ij}^A \text{ and } 0 \le n_{ij}^{B*} \le N_{ij}^B. \quad (3.11)$$

3.4.2. Learning Process (between Stage 1 and Stage 2)

After collecting signals from each group, consumers update their belief about not only

their own song evaluation (R_{ij}^E) but also the average evaluations in the two social groups

$(\bar{R}_{ij}^A, \bar{R}_{ij}^B)$. We assume that consumers update their beliefs about all evaluations

according to Bayes rule. The update mechanism outlined in this section is common to

both SMBL and EMBL models; the difference is in whether update is based on objective

and manipulated attributes (SMBL) or subjective attributes (EMBL), not in the update

mechanism per se. We explain the difference between SMBL and EMBL in detail in the empirical section. Given the prior and the signal distribution specified in the previous section, the learning process follows multivariate Bayesian learning. As a result, we obtain the posterior belief about all three evaluations, $f_i^R\left(R_{ij} \mid I_i(t_2)\right)$, which follows a multivariate normal distribution. Thus, we can obtain a consumer's posterior beliefs about their own song evaluation, $f_i^{R^E}\left(R_{ij}^E \mid I_i(t_2)\right)$, as a marginal distribution of $f_i^R\left(R_{ij} \mid I_i(t_2)\right)$:

$$f_i^{R^E}\left(R_{ij}^E \mid I_i(t_2)\right) = N\left(PM_{ij}\left(n_{ij}, \overline{s}_{ij}\right), PV_{ij}\left(n_{ij}\right)\right), \tag{3.12}$$

where $PM_{ij}\left(n_{ij}, \overline{s}_{ij}\right) = \omega_{ij}^0 R_{ij}^0 + \omega_{ij}^A \overline{s}_{ij}^A + \omega_{ij}^B \overline{s}_{ij}^B$,

$$\omega_{ij}^A = \frac{\tau_{ij}^2 N_{ij}^A n_{ij}^A \left(a_{ij}\left(N_{ij}^B \left(\sigma_{ij}^B\right)^2 + \tau_{ij}^2 n_{ij}^B \right) - b_{ij} c_{ij} \tau_{ij}^2 n_{ij}^B \right)}{\left(N_{ij}^A \left(\sigma_{ij}^A\right)^2 + \tau_{ij}^2 n_{ij}^A \right)\left(N_{ij}^B \left(\sigma_{ij}^B\right)^2 + \tau_{ij}^2 n_{ij}^B \right) - c_{ij}^2 \tau_{ij}^4 n_{ij}^A n_{ij}^B},$$

$$\omega_{ij}^B = \frac{\tau_{ij}^2 N_{ij}^B n_{ij}^B \left(b_{ij}\left(N_{ij}^A \left(\sigma_{ij}^A\right)^2 + \tau_{ij}^2 n_{ij}^A \right) - a_{ij} c_{ij} \tau_{ij}^2 n_{ij}^A \right)}{\left(N_{ij}^A \left(\sigma_{ij}^A\right)^2 + \tau_{ij}^2 n_{ij}^A \right)\left(N_{ij}^B \left(\sigma_{ij}^B\right)^2 + \tau_{ij}^2 n_{ij}^B \right) - c_{ij}^2 \tau_{ij}^4 n_{ij}^A n_{ij}^B},$$

$$\omega_{ij}^0 = 1 - \omega_{ij}^A - \omega_{ij}^B,$$

and $PV_{ij}\left(n_{ij}\right) = \dfrac{\tau_{ij}^2}{\left(N_{ij}^A \left(\sigma_{ij}^A\right)^2 + \tau_{ij}^2 n_{ij}^A \right)\left(N_{ij}^B \left(\sigma_{ij}^B\right)^2 + \tau_{ij}^2 n_{ij}^B \right) - c_{ij}^2 \tau_{ij}^4 n_{ij}^A n_{ij}^B}$

$$\times \left(\left(\tau_{ij}^2 n_{ij}^A \left(1 - a_{ij}^2\right) + N_{ij}^A \left(\sigma_{ij}^A\right)^2 \right)\left(\tau_{ij}^2 n_{ij}^B \left(1 - b_{ij}^2\right) + N_{ij}^B \left(\sigma_{ij}^B\right)^2 \right) - \tau_{ij}^4 n_{ij}^A n_{ij}^B \left(a_{ij} b_{ij} - c_{ij} \right)^2 \right).$$

3.4.3. Stage 2: Purchase Decision

In the purchase stage, a consumer determines whether or not to purchase the song. Given

consumer's beliefs about their own evaluation at the time of purchase, the expected utility

from purchasing song j is:

$$E_{R_{ij}^{E}(I(t_2))}\left[U_{ij}\left(I_i(t_2)\right)\right] = \alpha_i - \exp\left(-\beta_i PM_{ij}\left(n_{ij}, \overline{s}_{ij}\right) + \frac{\beta_i^2}{2} PV\left(n_{ij}\right)\right) + \varepsilon_{ij}\left(I(t_2)\right),$$

$$= u_{ij}\left(I_i(t_2)\right) + \varepsilon_{ij}\left(I_i(t_2)\right) \tag{3.13}$$

Note that the utility error is known to a consumer when a purchase decision is made.

A consumer purchases the song when its expected utility is higher than not purchasing it.

3.5. Empirical Specification of the Learning Model– SMBL vs. EMBL

We propose two different specifications of the consumer learning model – SMBL and

EMBL. Table 3.2 summarizes both model specifications.

SMBL model is the baseline model where people update the belief about their own

product evaluation according to Bayes rule applied to the *objective* (manipulated) values

of all six attributes (R_j^{0M}, τ_j^M, a_j^M, b_j^M, c_j^M, and σ_j^M). As described in the theory section,

the SMBL model has two key limitations. First, the informational benefit from similar

and dissimilar sources is forced to be identical. Thus, positive feedback from similar

others and negative feedback from dissimilar others are equivalent. Clearly, we cannot

test whether the information from similar others is more diagnostic or not (*H1a*) by

employing the SMBL model. Second, the informational benefit is always greater in an

imbalanced system than a balanced one. The absolute similarity between sources cancels

the noise of signals under imbalanced condition but amplifies it under a balanced

condition, so informational benefit is always greater under imbalance. Therefore, we

cannot test whether people find the information under imbalance is more reliable or not (*H2a*) by using SMBL model, as it has that property built-in. Please see Section 3.10.3 in Appendix for discussion of these two limitations.

Table 3.2. SMBL vs. EMBL Specification

(a) Related to Informational Benefit (Θ_i)

Attributes	SMBL Specification	EMBL Specification
Similarity with Groups $\left(a_{ij}, b_{ij}\right)$	$a_{ij} = a_j^M$ and $b_{ij} = b_j^M$	$a_{ij} = a_j^M \times \exp\left(\theta_{i0}^{with} + \theta_{i1}^{with} neg_j^A\right),$ $b_{ij} = b_j^M \times \exp\left(\theta_{i0}^{with} + \theta_{i1}^{with} neg_j^B\right).$
Similarity b/w Groups $\left(c_{ij}\right)$	$c_{ij} = c_j^M$	$c_{ij} = c_j^M \times \left(\theta_{i0}^{bw} + \theta_{i1}^{bw} imb_j\right)$
Aggregate Mean $\left(R_{ij}^0\right)$	$R_{ij}^0 = R_j^{0M}$	$R_{ij}^0 = R_j^{0M}$
Aggregate Variation $\left(\tau_{ij}\right)$	$\tau_{ij} = \tau_j^M$	$\tau_{ij} = \tau_j^M \times \exp\left(\theta_{i0}^{pri}\right)$
Within-Group Variation $\left(\sigma_{ij}^A, \sigma_{ij}^B\right)$	$\sigma_{ij}^A = \sigma_{ij}^B = \sigma_j^M$	$\sigma_{ij}^A = \sigma_{ij}^B = \sigma_j^M \times \exp\left(\theta_{i0}^{sig}\right)$

(b) Related to Cost of Search (Δ_i)

Attributes	SMBL Specification	EMBL Specification
Cost of Search $\left(k_{ij}^A, k_{ij}^B\right)$	$k_{ij}^A = k_{ij}^B = \exp\left(\delta_{i0} + \delta_{i1} order_j\right)$	$k_{ij}^A = \exp\left(\delta_{i0} + \delta_{i1} order_j + \delta_{i2} neg_j^A + \delta_{i3} imb_j\right),$ $k_{ij}^B = \exp\left(\delta_{i0} + \delta_{i1} order_j + \delta_{i2} neg_j^B + \delta_{i3} imb_j\right)$

To test our hypotheses, we propose the EMBL model. It maintains the assumption that consumers update in a Bayesian manner but adds that they rely on a *subjective* value of attributes for doing so (Camacho et al. 2011). Allowing for subjective interpretation by consumers provides substantial model flexibility. As summarized in Table 3.2a, the subjective values of attributes depend on the two characteristics of central interest. First, to assess the impact of homophily (*H1a* and *H1b*), the type of relationship that recipients have with their sources is moderated by the sign of similarity of each group (neg_j^A and

neg_j^B, which is 1 when a respondent has negative similarity with Group A or Group B,

respectively, and is 0 otherwise) Second, to test consumers' attitude towards balance

(*H2a* and *H2b*), the type of relationship between the sources is moderated by the balance

status in the social system (imb_j is 1 if the relationship is imbalanced and is 0 for balance).

Note that $\left(\theta_{i0}^{with}, \theta_{i1}^{with}\right)$ denotes parameters related to the subjective similarity with each

source, and $\left(\theta_{i0}^{bw}, \theta_{i1}^{bw}\right)$ denotes parameters related to the subjective similarity between the

two sources.

In the EMBL specification, let Θ_i denote the vector of all related individual-level

parameters related to informational benefit from others' evaluations. Subjective similarity

with each information source (a_{ij}, b_{ij}) is proportional to the manipulated similarity

$\left(a_j^M, b_j^M\right)$. If consumer *i* perceives the information from dissimilar others to be *less*

diagnostic (*H1a*), then the parameter θ_{i1}^{with} will be significantly negative. Subjective

similarity between the groups (c_{ij}) is specified as a function of balance status. Unlike a_{ij}

and b_{ij}, we allow c_{ij} to have a different sign from the manipulated c_j^M.[47] This flexible

specification allows us to test whether the informational benefit from search is different

between balance and imbalance (*H2a*). For instance, if both θ_{i0}^{bw} and θ_{i1}^{bw} are positive,

people find information under imbalance more reliable than balance as the normative

model suggests. If, however, both θ_{i0}^{bw} and θ_{i1}^{bw} are negative, people find information

under balance more reliable than imbalance. The reliability of information under other

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[47] We fix the sign of subjective similarity with each source (a_{ij} and b_{ij}) as our manipulation (a_j^M and b_j^M).
Otherwise, we cannot identify the subjective correlations. The details of identification are outlined in
Section 3.10.4 in Appendix.

possible combinations of θ_{i0}^{bw} and θ_{i1}^{bw} depend on the magnitude of each parameter. For instance, people find information under imbalance more reliable than balance when θ_{i0}^{bw} is not different from 0 but θ_{i1}^{bw} is positive.

We allow for subjectivity on prior and signal distributions as well. We cannot identify both subjective prior (i.e., aggregate) mean and variance jointly. See Section 3.10.4 for a discussion of model identification. We assume the subjective prior mean is the same as the manipulated prior mean while the subjective prior standard deviation $\left(\tau_{ij}\right)$ is proportional to the manipulated prior standard deviation $\left(\tau_j^M\right)$.[48] Subjective signal standard deviation $\left(\sigma_{ij}^A, \sigma_{ij}^B\right)$ is also proportional to the manipulated signal variance $\left(\sigma_j^M\right)$.

In Table 3.2b, we summarize the specification of attributes related to the cost of search. For consumer i, let Δ_i denote a vector of all related individual-level parameters. Recall that we manipulated the cost of search as the time that respondents have to wait to acquire a single signal. Therefore, the parameter δ_{i0} captures consumer i's (baseline) unit cost of search (wait time) on the utility scale. If the consumer has greater cost of search from dissimilar others (*H1b*), the parameter δ_{i2} will be significantly positive. If a consumer has either greater cost of collecting and processing the information under an imbalanced social system (*H2b*), the parameter δ_{i3} will be significantly positive. Finally,

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[48] As outlined in Section 3.10.4 in Appendix, we cannot identify subjectivity in prior mean and variance at the same time. For consistency of specification, we incorporate subjectivity in prior and signal variance.

we allow cost to be a function of the order of songs to control for respondents' fatigue.

3.6. Estimation

We had 152 respondents ($i = 1 \dots N$), and each made search and purchase decisions for 18

songs ($j = 1 \dots J$). For respondent i and song j, let n_{ij}^* denote the actual search decision and

let y_{ij} be an indicator variable that takes a value of 1 if he decides to purchase the song

and is 0 otherwise.

We make the following distributional assumptions on the two errors in our model.

First, the search utility error ($\xi_{n_{ij}}$) follows IID Type I Extreme value distribution with a

scale parameter λ_i. Second, purchase utility error (ε_{ij}) follows a standard Normal

distribution. Then, the conditional likelihood that a consumer i makes a search decision

of n_{ij}^* and purchase decision of y_{ij} for a song j can be expressed as:

$$\Pr\left(n_{ij}^*, y_{ij} \mid \Gamma_i, \Delta_i, \Theta_i, \lambda_i\right) = \Pr\left(n_{ij}^* \mid \Gamma_i, \Delta_i, \Theta_i, \lambda_i\right) \times \Pr\left(y_{ij} \mid n_{ij}^*, \Gamma_i, \Theta_i\right), \qquad (3.14)$$

where Γ_i denote the vector of utility parameters $\left(\alpha_i, \ln(\beta_i)\right)$ defined in the Equation 3.1.

The first term on the right-hand side is the search likelihood which follows multinomial

logit and the second term is the purchase likelihood which follows binary Probit.

In the experiment, search amount can be any combination of two integers between [0,

200]. As there are 40,401 possible options of search for each song, it is not feasible to

estimate the model as is. We used a subset of options to estimate the model by the

positive conditioning property (McFadden 1978; Train et al. 1987). For consumer i and

song j, let W_{ij} denote a consideration set that includes the actual search option (n_{ij}^*) and 5

other possible options of n_{ij}, which are randomly selected from the empirical distribution

of search decisions in our dataset.[49] Then, the search likelihood can be written as:

$$\Pr\left(n_{ij}^{*}\mid\Gamma_{i},\Delta_{i},\Theta_{i},\lambda_{i}\right)=\frac{\exp\left(v_{ij}^{b}\left(n_{ij}^{*}\mid I_{i}(t_{1}),\Gamma_{i},\Theta_{i}\right)\big/\lambda_{i}-v_{ij}^{c}\left(n_{ij}^{*}\mid\Delta_{i}\right)\big/\lambda_{i}\right)\times h\left(W_{ij}\mid n_{ij}^{*}\right)}{\displaystyle\sum_{n_{ij}\in W_{ij}}\exp\left(v_{ij}^{b}\left(n_{ij}\mid I_{i}(t_{1}),\Gamma_{i},\Theta_{i}\right)\big/\lambda_{i}-v_{ij}^{c}\left(n_{ij}\mid\Delta_{i}\right)\big/\lambda_{i}\right)\times h\left(W_{ij}\mid n_{ij}\right)},$$

$$(3.15)$$

where $h\left(W_{ij}\mid n_{ij}\right)$ denotes a bias adjustment factor to account for using a subset of options.

Specifically, $h\left(W_{ij}\mid n_{ij}\right)$ is the probability that consumer i formed a consideration set of

W_{ij} given that he made a search decision of n_{ij}. We used importance sampling and

computed the bias adjustment factors from the empirical distribution of search decisions.

Lastly, note that $v_{ij}^{b}(\cdot)$ does not have a closed form expression (Equation 3.15) and is

computed using a Monte-Carlo simulation.

The conditional purchase likelihood is a binary Probit likelihood specified as:

$$\Pr\left(y_{ij}\mid n_{ij}^{*},\Gamma_{i},\Theta_{i}\right)=\Phi\left(u_{ij}\left(I_{i}(t_{2})\mid\Gamma_{i},\Theta_{i}\right)\right)^{y_{ij}}\left(1-\Phi\left(u_{ij}\left(I_{i}(t_{2})\mid\Gamma_{i},\Theta_{i}\right)\right)\right)^{1-y_{ij}},\qquad(3.16)$$

where the information set in the second stage $\left(I_{i}(t_{2})\right)$ includes the search decision made

in the first stage $\left(n_{ij}^{*}\right)$.

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[49] We also estimated a model where W_{ij} consists of 10 alternatives including the observed search decision. All substantive findings remained unchanged. Research on case-control modeling indicates that little precision is gained by going beyond a 1-5 ratio of other alternatives (e.g., Donkers et al. 2003; Hu and Van den Bulte 2014).

Given that there are common parameters $\left(\Gamma_i, \Theta_i\right)$ in both stages, the two decisions are estimated jointly. Therefore, the conditional likelihood of observing the decisions for consumer i for all J songs is:

$$L_i \mid (\Gamma_i, \Delta_i, \Theta_i, \lambda_i) = \prod_{j=1\ldots J} \Phi\left(u_{ij}\left(I_i(t_2) \mid \Gamma_i, \Theta_i\right)\right)^{y_{ij}} \left(1 - \Phi\left(u_{ij}\left(I_i(t_2) \mid \Gamma_i, \Theta_i\right)\right)\right)^{1-y_{ij}} \qquad (3.17)$$

$$\times \frac{\exp\left(v_{ij}^b\left(n_{ij}^* \mid I_i(t_1), \Gamma_i, \Theta_i\right)\Big/\lambda_i - v_{ij}^c\left(n_{ij}^* \mid \Delta_i\right)\Big/\lambda_i\right) \times h\left(W_{ij} \mid n_{ij}^*\right)}{\displaystyle\sum_{n_{ij} \in W_{ij}} \exp\left(v_{ij}^b\left(n_{ij} \mid I_i(t_1), \Gamma_i, \Theta_i\right)\Big/\lambda_i - v_{ij}^c\left(n_{ij} \mid \Delta_i\right)\Big/\lambda_i\right) \times h\left(W_{ij} \mid n_{ij}\right)}.$$

To capture consumer heterogeneity, individual-level parameters $(\Gamma_i, \Delta_i, \Theta_i, \ln(\lambda_i))$ are assumed to be distributed multivariate normal with mean vector $\left(\Gamma, \Delta, \Theta, \ln(\lambda)\right)$ and covariance matrix Σ. The unconditional likelihood L for a sample of N customers is:

$$L = \prod_{i=1\ldots N} \int L_i \mid (\Gamma_i, \Delta_i, \Theta_i, \lambda_i)\ dF\left(\Gamma_i, \Delta_i, \Theta_i, \lambda_i \mid \Gamma, \Delta, \Theta, \lambda, \Sigma\right). \qquad (3.18)$$

where $F\left(\Gamma_i, \Delta_i, \Theta_i, \lambda_i \mid \Gamma, \Delta, \Theta, \lambda, \Sigma\right)$ denotes the multivariate normal density function.

As summarized in Figure 3.2, informational benefit and cost of search have asymmetric effects on search and purchase decisions: Both informational benefit and cost drives search decision, but only informational benefit drives purchase decision given the search decision. Therefore, we can identify the parameters related to informational benefit with purchase observations given the search decisions, and identify the parameters related to cost of search with search observations. Section 3.10.4 in Appendix outlines the identification of parameters in more detail.

The model parameters are estimated using standard hierarchical Bayesian Markov chain Monte Carlo (MCMC) methods. We use the following set of priors for all

population level parameters. Let $(\Gamma, \Delta, \Theta, \ln(\lambda))$ be a $p \times 1$ vector and that Σ^{-1} is a $p \times p$ matrix. Then, the prior for $(\Gamma, \Delta, \Theta, \ln(\lambda))$ is a multivariate normal with mean of 0 and covariance of $0.1 \times I_{p \times p}$ matrix. The prior for Σ is a Wishart distribution where the scale matrix is $0.1 \times I_{p \times p}$ matrix, and $p+4$ degrees of freedom. The details of the full conditional distributions are available from the authors upon request.

We ran sampling chains for 200,000 iterations, and convergence was assessed by monitoring the time series of the draws. We report the results based on 100,000 draws retained after discarding the initial 100,000 draws as burn-in iterations. For each participant, we randomly select 15 of the 18 song profiles for model estimation and use the remaining 3 for out-of-sample prediction.

3.7. Results

3.7.1. Model Comparison: SMBL vs. EMBL

Table 3.3 reports the model fit of SMBL and EMBL models. We compared the two models on several measures of model fit. First, we report deviance information criterion (DIC) to evaluate within-sample fit and complexity of each model. Smaller numbers denote a better model (Spiegelhalter et al. 2002). Second, we computed within and out-of-sample hit rates. To assess the prediction for the purchase decision, we used purchase hit-rate where cut-off was fixed at 0.5. To evaluate the model prediction for search decision, we used search hit-rate. For computing the search hit rate, we *discretized* the observed $n_{ij}^{A^*}$ and $n_{ij}^{B^*}$ into 3 levels (5 levels) each based on their quartiles and thus the overall search decision, which is a combination of $n_{ij}^{A^*}$ and $n_{ij}^{B^*}$, is classified into 9 options (25 options). The search hit rate is the proportion of observations where the

observed search option matches the option with the highest search utility based on our model estimates. Third, we computed validation log-likelihood (VLL) in the holdout sample to assess predictive validity (Montoya et al. 2010; Iyengar and Jedidi 2012). A comparison of models on the several criteria shows that the EMBL model generally outperforms the SMBL as well as other intermediate model variants. Note though the search hit rate for the EMBL model is significantly higher than that from SMBL (a 10% difference) while being marginally lower for the purchase hit rate (a 2-3% difference).

Table 3.3. Model Comparisons

		(a) Within Sample Fit				(b) Holdout Sample Fit		
	DIC	Search Hit Rate		Purchase Hit Rate	VLL	Search Hit Rate		Purchase Hit Rate
		9 groups	25 groups			9 groups	25 groups	
SMBL	12,828.5	32.4%	20.1%	77.5%	-1,234.1	30.2%	16.9%	75.2%
EMBL	10,819.2	46.9%	29.9%	75.3%	-1,172.9	39.7%	28.3%	72.1%

3.7.2. Estimation Results

In Table 3.4, we present the estimation results of SMBL and EMBL models. We present SMBL model results as a baseline, and use EMBL results for hypotheses testing and further discussion. As is common in Bayesian analysis, we summarize the posterior distribution of the parameters by reporting their posterior means and 95% posterior confidence intervals.

Impact of homophily. Our EMBL model provides several insights on what causes homophily in search behavior. A significantly negative estimate of θ_1^{with} in Table 3.4 indicates that people perceive information from dissimilar others to be less diagnostic than that from similar others (*H1a*). To be more specific, positive similarity is discounted by 13% (=1-exp(-0.14)) and negative similarity is discounted by 47% (=1-exp(-0.14-

0.50)) compared to what is implied by normative SMBL model. Thus, people discount the diagnosticity of information from both types of sources but more so from dissimilar others than similar others.

Table 3.4. Model Estimates for SMBL and EMBL models

| Population Parameter Estimates	(a) SMBL (Baseline Model)	(b) EMBL (Proposed Model)
Utility Parameters: Intercept (α_0)	-0.10 (-0.24, 0.04)	**0.25**** (0.12, 0.38)
Utility Parameters: Rating ($Ln(\beta)$)	**1.01**** (0.81, 1.22)	**0.21*** (0.03, 0.35)
Similarity with sources: Base (θ_0^{with})		**-0.14**** (-0.20, -0.09)
Similarity with sources: Dissimilar (θ_1^{with})		**-0.50**** (-0.75, -0.36)
Similarity between sources: Base (θ_0^{bw})		-0.10 (-0.27, 0.09)
Similarity between sources: Imbalance (θ_1^{bw})		**0.70**** (0.45, 0.92)
Prior Standard Deviation: Base (θ_0^{pri})		**1.15**** (1.01, 1.31)
Signal Standard Deviation: Base (θ_0^{sig})		**-0.40**** (-0.67, 0.11)
Cost: Base (δ_0)	**-0.80**** (-1.19, -0.41)	-0.36 (-0.77, 0.02)
Cost: Order of songs (δ_1)	**0.61**** (0.46, 0.76)	**0.65**** (0.50, 0.80)
Cost: Negative (δ_2)		-0.07 (-0.38, 0.15)
Cost: Imbalance (δ_3)		**0.40**** (0.27, 0.54)
Scale Parameter ($Ln(\lambda \times 10^3)$)	**1.83**** (1.57, 2.12)	0.42 (-0.03, 0.86)

Note: * denotes significance in 95% confidence level, and the corrsponding intervals are in parentheses. ** denotes significance in 99%.

In contrast, as the insignificant estimate of δ_2 shows, the sign of similarity does not have a significant effect on the cost of collecting and processing the information (*H2b*). In other words, there is no evidence that people have a greater cost of collecting and processing the information from dissimilar than similar others. Our finding may appear inconsistent with past studies (McPherson et al. 2001; Price and Feick 1984), but it is worth noting that there was no face-to-face social interaction in our setting. Thus, our

results suggest that the discomfort people feel from getting information from dissimilar others in real life need not be from processing the information (which was part of our experiment), but may stem from having to interact with dissimilar others (which was not part of our experiment).

Figure 3.4 depicts the impact of the sign of similarity on consumer decisions. For these plots, we computed the search amount and purchase likelihood for each respondent under both SMBL and EMBL models. Given the attribute values and the estimates of individual parameters, the plots show the average across all respondents. In the SMBL specification, where the diagnosticity of information is forced to be identical between similar and dissimilar others, the sign of similarity does not have an impact on either search or purchase decision (Figure 3.4a and 3.4b). In the proposed EMBL specification, where the informational benefit is greater from similar others than dissimilar others, people tend to search more from similar others than dissimilar others (Figure 3.4c). Search amount is up to 50% greater when both sources have similar preference than when one of them has dissimilar preference. Also, people purchase more when the information is collected from similar others than dissimilar others (Figure 3.4d). Purchase likelihood is up to 8% greater when both sources have similar preference than when one of them has dissimilar preference.

Figure 3.4. Comparative Statics of Search /Purchase Decisions:
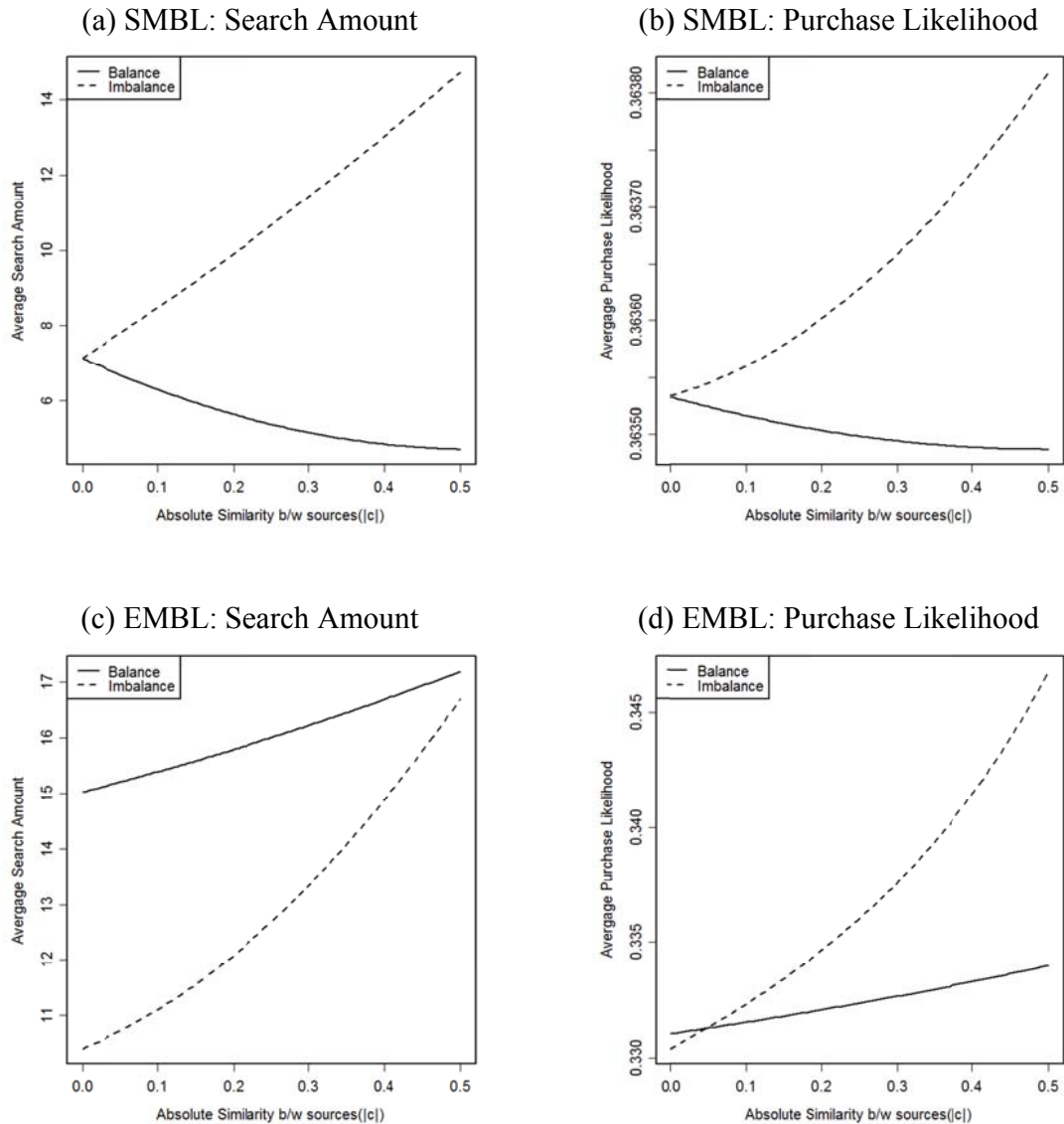**Similar vs. Dissimilar**

(a) SMBL: Search Amount

(b) SMBL: Purchase Likelihood

(c) EMBL: Search Amount

(d) EMBL: Purchase Likelihood
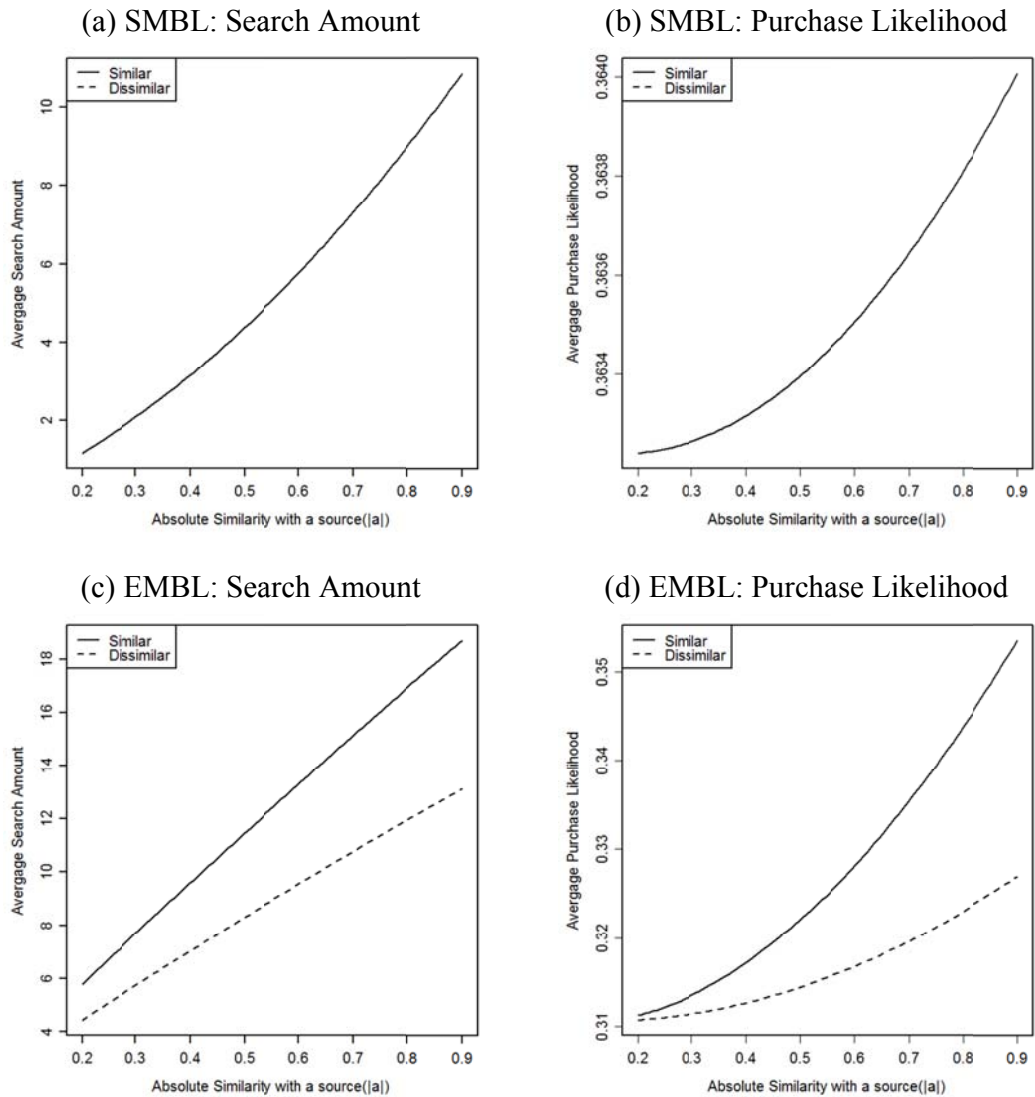
Note: For generating the figures, we only varied the relevance of one source $\left(a_j^M \right)$ and fixed all other attributes ($R_j^{0M} = 5$, $\tau_j^M = 2.5$, $\sigma_j^M = 1.25$, $b_j^M = 0.2$, $c_j^M = 0.0$, and $order_j = 10$). The figures show the average search amount and average purchase likelihood averaged across all respondents.

Impact of balance. Imbalance has a nuanced effect on social learning. On the one hand, consumers find the informational under imbalance more reliable than balance (*H2a*). To be specific, the results in Table 3.4 suggest that people ignore the similarity

between information sources under balance and consider the sources to be uncorrelated (insignificant θ_0^{bw}). Thus, people find the information under balance to be more reliable than what the normative model (SMBL) will imply. This finding is consistent with the illusion of validity (Tversky and Kahneman 1974), which suggests that people falsely believe information from two highly redundant sources to be more reliable than what is implied by the statistics of correlation. In contrast, people do not ignore the similarity between information sources under imbalance (significantly positive θ_1^{bw}). In other words, the overall bias will be reduced when people integrate the information from an imbalanced system. In sum, people reduce their uncertainty more when the information is collected in imbalanced than balanced social systems even when people may tend to suffer from the illusion of validity under the latter.

On the other hand, as the positive estimate of δ_3 shows, people have significantly greater subjective cost of gathering and processing information under imbalance than balance (*H2b*). The subjective cost of search is almost 50% greater (= exp(-0.36+0.40)/exp(-0.36)) under imbalance as compared to balance. Thus, our finding suggests that imbalanced relationships have a higher search cost due to difficulty in processing the collected information even when there is no real interaction with others (which is the case in our experimental setting) .

**Figure 3.5. Comparative Statics of Search and Purchase Decisions:
Balance vs. Imbalance**

(a) SMBL: Search Amount

(b) SMBL: Purchase Likelihood

(c) EMBL: Search Amount

(d) EMBL: Purchase Likelihood

Note: For generating the figure, we only varied the relevance of one source $\left(c_j^M \right)$ and fixed all other attributes ($R_j^{0M} = 5$, $\tau_j^M = 5$, $\sigma_j^M = 2.5$, $a_j^M = 0.6$, $b_j^M = 0.3$, and $order_j = 10$). The figures show the search amount and purchase likelihood averaged across all respondents.

In Figure 3.5, we show how structural balance impacts search and purchase decisions.

As before, given the attribute values and the estimates of individual parameters, we

computed the purchase likelihood and total search amount for each respondent under both

model specifications. The plots show the averages across all respondents. In the SMBL specification, where the reliability of information is forced to be greater under imbalance, the search amount and the purchase likelihood is always greater under imbalance (Figure 3.5a and 3.5b). In the proposed EMBL model, however, people search less under imbalance because of greater subjective cost of search (Figure 3.5c). People search up to 45% more under balance than imbalance. However, as the absolute similarity between sources increases, so does the informational benefit under imbalance thus shrinking the difference in the search amount between the two conditions. Notably, the lower amount of search under imbalance still leads to greater purchase likelihood (Figure 3.5d) as compared to the balance condition because people can reduce their uncertainty to a greater extent under the former. Purchase likelihood is around 4% greater under imbalance than balance.

3.8. Conclusions

We investigate how the similarity in preferences of consumers with their contacts impacts how they collect product information from social contacts, learn, and purchase experiential products. We address these questions through an incentive compatible stated choice experiment where consumers make purchase decisions for individual music tracks while having access to others' evaluations. We build a structural model of consumers' decisions in which consumer learning is purposive and accommodates information search from consumers for a planned product purchase.

There are three important aspects of our modeling framework. First, consumers' search and purchase decisions are modeled as inter-related, but temporally separated,

decisions thus allowing us to assess the impact of social relationships on each of the two drivers – informational benefit and cost of search. Second, consumers can gather information from their contacts who may have preferences that are systematically different from theirs and the various contacts themselves may have systematically different preference among each other. Finally, the model is grounded in the widely-accepted framework of Bayesian consumer learning but extends it by including the impact of behavioral aspects related to the similarity of preferences, more specifically homophily and structural balance, on consumer learning. Our model thus adds to a stream of research that incorporates the behavioral aspects into Bayesian learning model (Camacho et al. 2011; Mehta et al. 2004; Zhao et al. 2011).

Our results provide insights into the drivers that impact how consumers collect social information. First, social learning exhibits significant homophily as consumers prefer to collect information from similar others. A key contribution is that we disentangle whether this is due to either the greater diagnosticity of information from similar others or a reduction in the cost of seeking information from similar others. The results suggest that the main driver is the former - consumers find reviews from similar others to be more diagnostic than those from dissimilar others. Second, the impact of balance on social learning is nuanced: people prefer imbalanced systems for their higher reliability of information, but balanced systems for their lower cost most likely due to the cognitive and affective burden of dissonance. Thus, people appear to understand that informational benefit is greater under an imbalanced social system but that it can be burdensome to process the gathered information. In sum, the impact of the similarity of preference on consumer search and learning is over and above what is captured by the standard

Bayesian learning model. As we manipulated the similarity of preference among consumers, our results do not suffer from confounds such as interpersonal affect, higher frequency of interactions with similar others typically present in observational data.

Our study will be interest to researchers who study different moderators of social learning. For instance, Godes and Mayzlin (2009) show that, for products with low awareness (e.g., a brewery chain), word-of-mouth from less loyal customers is more effective than more loyal customers at driving sales. Iyengar et al. (2011) consider the adoption of a new drug and find that physicians' self-perceived opinion leadership moderates the weight they put on other physicians' prescription behavior. There is evidence for social learning in online contexts as well. For example, in a study that investigated the spatial adoption of a new online retailer, Lee and Bell (2013) show how much neighbors trust and communicate with each other makes the social learning process more efficient. We add to this stream of literature by specifically considering how similarity of preference among information seekers and providers can moderate the level of social learning.

Our results provide a novel view on how the characteristics of ties in a social system can drive informational benefit. Our finding of greater reliability of information under imbalance may look analogous to the theory of the strength of weak ties. For instance, Granovetter (1973) found that weak ties (e.g., acquaintances) provide new information more so than strong ties. Similarly, Burt (1980) noted that individuals that span the structural holes in a network (i.e., have ties across different subgroups) have an advantage in that they can broker the flow of information. In contrast, we focus on the informational benefit from the validation of one's belief, not from the inflow of novel information.

Therefore, our study complements prior work, and broadens the understanding about how the characteristics of social ties impact the informational benefit from a social system.

Our study is in a context in which consumers gather product evaluations from their peers. Prior work, however, suggests that people are often persuaded more by experts than non-experts (e.g., Petty et al. 1981). How may our results change when there is an expert source. It is possible that people may gather a large number of (if not all) evaluations from the expert and not rely on similarity of preferences. Recent research suggests otherwise. For instance, in pharmaceutical contexts where expertise should clearly matter and key opinion leaders play a critical role, modern medical literature has actually shown that local opinion leaders are more important that national leaders (e.g., Flodgren et al. 2011, Keating et al. 2007, Kuo et al. 1998). This is because nationally reputed "expert opinion leaders" are much less representative than local "peer opinion leaders" who are members of their own community and face similar patients and working conditions (Locock et al. 2001). Thus, even when expertise matters, similarity of preferences may continue to play an important role.

In the last few years, companies are actively embracing the notion of providing their customers with access to their friends' evaluations. For instance, companies that facilitate social search (e.g., Google plus your world, Facebook graph search, Bing social search) allow information seekers to search for content from their social contacts. With advertising being the major source of revenue for these websites, our results suggest that they may be able to increase their search traffic by making consumers perceive that the search results are from others whose preferences are similar to theirs and to each other. This strategy will increase the search clicks as people perceive that the results are

informative (as they come from similar others), and less effortful to process (since they come from people who have balanced preference).

Our findings are also relevant for companies that provide consumers with reviews for experiential products (e.g., Open Table for restaurants, Goodreads for books). Many of these websites wish to increase the purchase rate of products and are trying to do so by providing consumers with their friends' reviews. For instance, Open Table, a restaurant review portal, receives commission from restaurants when a consumer reserves through the website. Our results suggest that Open Table (and other such websites) may be able to increase their purchase rate by making consumers perceive that the search results are from those who have similar preferences, and that they are also being exposed to others with diverse preferences. In sum, the top-line message to practitioners is that effective use of social recommendation systems involves paying careful attention to which social contacts consumers should access.

3.9. References

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3.10. Appendix

3.10.1. Descriptive Results

We investigate the drivers for the two decisions of total amount of search and purchase

using simple regressions. We estimate a regression model of (logarithm of) the total

amount of search (after adding 1 to avoid the log(0) problem) using covariates such as the

number of groups with dissimilar preferences in a profile (which takes a value of 0, 1, or

2), an indicator for whether the social system is imbalanced, the mean and standard

deviation of aggregate evaluations, and (logarithm of) the order in which the song is

presented (which takes a value of 1, 2, … 18). The latter is included to control for

respondents' fatigue as they go through the study. The unit of analysis is a subject-song

observation, with 2,736 (=152 subjects × 18 song profiles) observations in total. Table

3.5 shows the results.

Table 3.5. Drivers for Search Decision

	Est (SE)
Intercept	3.01 (0.08)**
Number of Dissimilar Sources	-1.20 (0.01)**
Imbalance	0.03 (0.05)
Mean of Aggregate Evaluation	0.05 (0.01)**
SD of Aggregate Evaluation	0.06 (0.14)*
Log(Order of song)	-0.29 (0.03)**

Note: * $p \leq 0.05$, ** $p \leq 0.01$. Standard errors in parentheses

The total search amount significantly decreases with the number of sources with

dissimilar preference ($p<0.001$). While this finding provides evidence for the impact of

homophily on the amount of search, it is not possible to disentangle if this is due to the

impact of homophily on the diagnosticity of information (*H1a*) or on the cost of search

(*H1b*). The search amount is not affected by imbalance ($p = 0.57$). Interestingly, this null

effect may be consistent with both hypotheses regarding imbalance – if people find the

information more reliable under imbalance (*H2a*) but have greater cost of search as well (*H2b*), the two effects may cancel each other out. Effects of other control variables were not of interest per se, but provide face validity for the experiment: the amount of search increased with the average and variance of aggregate evaluations (*p<0.001, p<0.05* respectively). The results remain unchanged when we introduce the subject-specific random intercept.

To understand how the purchase decision is driven by the factors of interest, we estimate a binary probit model of purchase incidence with covariates such as the number of sources with dissimilar preference, imbalance indicator, mean and standard deviation of aggregate evaluations, (logarithm of) search amount and social information content. We operationalize social information content as the weighted sum of the observed average rating from each group where the weights are (logarithm of) the number of contacts from each group.[50] We also include the interactions of social information content with the number of dissimilar sources and the indictor of structural imbalance. As the unit of analysis is subject-song observation, we have 2,736 (=152 subjects × 18 song profiles) observations in total. Table 3.6 shows the results.

The results show that the probability of purchase increased with the search amount (*p<0.001*) and with favorable social information (*p<0.001*). The number of dissimilar sources (*p=0.61*) and imbalance (*p=0.83*) do not directly impact the purchase rate, but both variables moderate the impact of social information content. Consistent with *H1a*,

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[50] We also fit a model where social information content was operationalized as the average of the observed average rating from each group (i.e., weighted sum of the observed average rating from each group where the weight is 1/2 each). The findings in Table 3.6 remain robust.

people tend to be less affected by social information when there are a greater number of dissimilar others (*p<0.001*) and discount the diagnosticity of information from dissimilar others. Note that we cannot directly test whether people have greater cost of search from dissimilar others (*H1b*). Consistent with *H2a*, people are more affected by social information collected under an imbalanced system (*p<0.001*). This result sheds further light on the null effect of structural imbalance on the amount of search (See Table 3.5). If people do not have greater cost of search under an imbalanced system, they should have searched more under it due to the informational benefit. Therefore, it is likely that people experience a greater cost of search under imbalance (*H2b*). Effects of other control variables provide face validity for our findings. People are more likely to purchase a song when they collect more signals (*p<0.001*), the average aggregate rating is greater (*p<0.001*), and the variation of aggregate rating is greater (*p<0.001*).[51] The results remain unchanged when we introduce a subject-specific random intercept.

Table 3.6. Drivers of Purchase Decision

	Est (SE)
Intercept	-1.28 (0.13)**
Number of Dissimilar Sources	-0.04 (0.08)
Imbalance	0.02 (0.11)
Mean of Aggregate Evaluation	0.37 (0.02)**
SD of Aggregate Evaluation	0.18 (0.05)**
Log(Search Amount+1)	0.11 (0.04)**
Social Information Content	0.09 (0.01)**
Social Information Content × Number of Dissimilar Sources	-0.08 (0.01)**
Social Information Content × Imbalance	0.12 (0.02)**

Note: * $p \leq 0.05$, ** $p \leq 0.01$. Standard errors in parentheses

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[51] The latter result is a bit surprising but the model is ad-hoc where potential confounders (e.g., search endogeneity) are not properly controlled for. We build a formal utility-based model where we control for the confounders.

In sum, the results from simple regressions show that the preference similarity with social sources and the overall balance in the social system are associated with consumers' decisions. The above regressions, however, we did not account for the endogenous relationship between search and purchase decisions. In addition, without a formal model, it is not possible to disentangle whether the attributes that are significant for the amount of search are due to either their impact on the informational benefit from search or the cost of accessing information (or both).

3.10.2. Regularity Conditions on Similarity Structure

We have two regularity conditions on the covariance matrix of triadic similarity – positive definiteness condition and inference condition.

Positive-Definiteness Condition. The covariance matrix for the triadic similarity structure should be a proper covariance matrix. Given the expression of similarity in Equation 3.4 and $N^A = N^B$, we can write the positive-definiteness condition in the following way. (For notation simplicity, we omit subscripts for respondent and product)

$$1 - a^2 - b^2 - c^2 + 2abc > 0. \tag{3.19}$$

Inference Condition. Intuitively, the information from sources with greater absolute correlation (e.g., greater $|a|$ or $|b|$) should reduce the uncertainty more. As an extreme example, the information from those who have exactly same preference will completely resolve any uncertainty. The same is the case for information from those who have exactly the opposite preference. However, some similarity structures violate this intuition of learning process and uncertainty actually increases by acquiring information from more relevant sources. Suppose a similarity structure is represented by Equation 3.4 in

the main text, and the respondent is informed of the values of $\bar{R}^A$ and $\bar{R}^B$. Then, the

conditional distribution for his own evaluation, R^E, given $\bar{R}^A$ and $\bar{R}^B$ is:

$$f^{R^E}\left(R^E \mid \bar{R}^A, \bar{R}^B\right) = \tag{3.20}$$
$$\left(R^0 + \sqrt{N^A}\,\frac{a-bc}{1-c^2}\left(\bar{R}^A - R^0\right) + \sqrt{N^B}\,\frac{b-ac}{1-c^2}\left(\bar{R}^B - R^0\right), \frac{\tau^2\left(1-a^2-b^2-c^2+2abc\right)}{1-c^2}\right)$$

A similarity structure violates our intuition when the conditional variance, which

denotes the uncertainty after having complete knowledge of $\bar{R}^A$ and $\bar{R}^B$, increases as

absolute similarity ($|a|$ or $|b|$) increases. As a result of comparative statics, we obtain the

following condition for the conditional variance to not increase with the absolute

similarity:

$$|a| > |bc| \text{ and } |b| > |ac|. \tag{3.21}$$

We exclude similarity structures where the statistical axiom does not fit our intuition.

3.10.3. Informational Benefit under SMBL

The informational benefit refers to the amount of the uncertainty reduction after update.

We operationalize the informational benefit (*IB*; for notation simplicity, we omit the

subscripts for respondent and product) as prior variance (i.e., how uncertain people are

before the update) subtracted by the posterior variance (i.e., how uncertain people are

after the update). For ease of exposition, we can rewrite *IB* as a function of absolute

similarity with each source ($|a|$ and $|b|$) and absolute similarity between sources ($|c|$) in

the following way.

$$IB = \tau^2 - \frac{\tau^2\left(\tau^2 n^A\left(1-|a|^2\right)+N^A\sigma^{A^2}\right)\left(\tau^2 n^B\left(1-|b|^2\right)+N^B\sigma^{B^2}\right)+\tau^6 n^A n^B\left(2D|abc|-|ab|^2-|c|^2\right)}{\left(N^A\sigma^{A^2}+\tau^2 n^A\right)\left(N^B\sigma^{B^2}+\tau^2 n^B\right)-|c|^2\tau^4 n^A n^B}$$

,

(3.22)

where D denotes an indicator of balance status which takes a value of 1 when $abc>0$

(balance), -1 when $abc<0$ (imbalance), and is 0 when $abc=0$.

   Sign of similarity. When the balance status (D) is held fixed, the sign of similarity per

se does not affect the informational benefit. Suppose the similarity of a source (a)

switches the sign, but balance status remains unchanged. We can think about a scenario

where either $D=0$, or one of other similarity measures (b or c) also switches the sign

when $D \neq 0$. Given that a enters Equation 3.22 only in absolute value, the sign of

similarity per se will not affect informational benefit. The sign of similarity will change

the informational benefit only through a change in the balance status of the system.

Figure 3.6a depicts the property of SMBL that the sign of similarity does not have a

direct impact on the informational benefit.

   Balance status and similarity between sources. Given all the other values fixed, we

can immediately see that IB is always greater when D=-1 (imbalance) than when D=1

(balance). That is, the informational benefit is always greater under imbalance than

balance.

   Next, the result of comparative statics shows that the effect of similarity between

sources (c) on the posterior variance is contingent on the balance status (D).

$$\frac{\partial IB}{\partial |c|} = -\frac{2D\tau^6 n^A n^B \left(|a| N^B \sigma^{B^2} + \left(|a|-|bc|\right)\tau^2 n^A n^B\right)\left(|b| N^A \sigma^{A^2} + \left(|b|-|ac|\right)\tau^2 n^A n^B\right)}{\left(\left(N^A \sigma^{A^2} + \tau^2 n^A\right)\left(N^B \sigma^{B^2} + \tau^2 n^B\right) - |c|^2 \tau^4 n^A n^B\right)^2}, \quad (3.23)$$

Given the second regularity condition (A3), the sign of the first derivative is determined by balance status; it is positive when $D=-1$, and negative when $D=1$. In other words, the increase in the absolute similarity between sources ($|c|$) decreases (increases) the informational benefit under balanced (imbalanced) system. Figure 3.6b depicts the property of SMBL: The absolute similarity between sources cancels the noise of signals under imbalanced condition but amplifies it under a balanced condition, so informational benefit is always greater under imbalance.

Figure 3.6. Informational Benefit under SMBL

(a) Informational Benefit:
Similar vs. Dissimilar

(b) Informational Benefit:
Balance vs. Imbalance
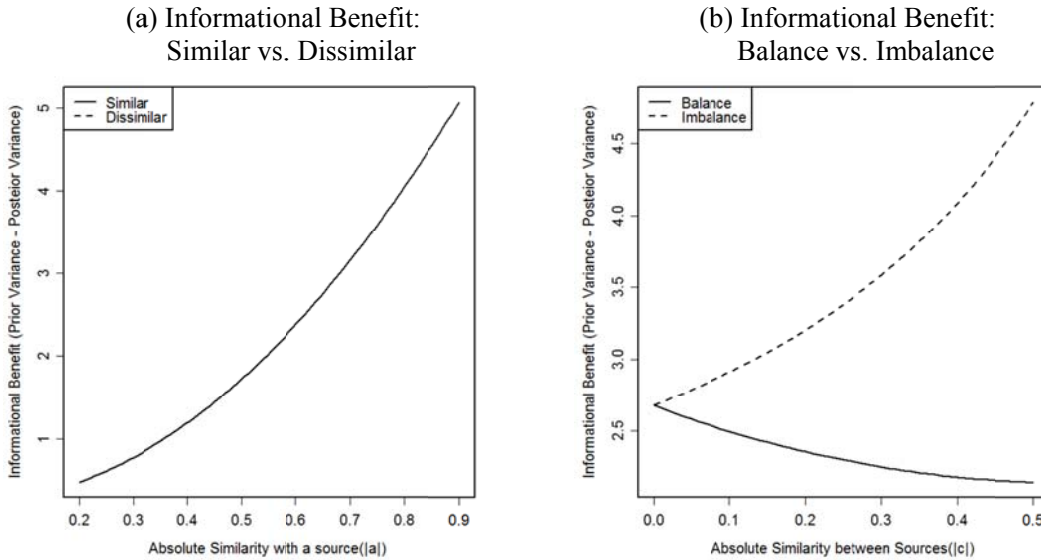
Note: For generating Figure 3.6a, we varied the relevance of one source $\left(a_j^M\right)$ and fixed all other attributes ($R_j^{0M} = 5$, $\tau_j^M = 2.5$, $\sigma_j^M = 1.25$, $b_j^M = 0.2$, and $c_j^M = 0.0$). For generating Figure 3.6b, we varied the relevance of one source $\left(c_j^M\right)$ and fixed all other attributes ($R_j^{0M} = 5$, $\tau_j^M = 2.5$, $\sigma_j^M = 1.25$, $a_j^M = 0.6$, and $b_j^M = 0.3$). The figures show the search amount and purchase likelihood averaged across all respondents.

3.10.4. Identification of Population Parameters

In this section, we outline the identification of population parameters. Given the

identification of population parameters $(\Gamma, \Delta, \Theta, \lambda)$, one can easily identify individual-

level parameters $(\Gamma_i, \Delta_i, \Theta_i, \lambda_i)$ with distributional assumptions on individual parameters

(i.e., normally distributed around population parameters).

First, we can identify Γ and Θ from observed purchase decisions given the search

decisions. Observed purchase decisions with $n_{ij}^* = (0,0)$ can identify utility parameters

$(\Gamma = [\alpha, \beta])$ and perceived prior variance (θ_0^{pri}).[52] Given that there was no search at all,

the general tendency of purchase is captured by α, the effect of manipulated prior mean

(R_j^{0M}) on purchase is captured by β, and the effect of manipulated prior standard

deviation (τ_j^M) on purchase is captured by β and θ_0^{pri}. Thus, subjectivity in both prior

mean and variance cannot be identified simultaneously.

The parameters related to perceived similarity with each sources $(\theta_0^{with}, \theta_1^{with})$ are

identified from observed purchase decisions given (1) no search from one source and (2)

a sufficiently large amount of search from the other source (i.e., steady state where an

additional signal hardly increase the search utility).[53] For these observations, purchase

utility depends only on parameters identified above (i.e., utility parameters and perceived

---

[52] In our data, we have 307 observations with no search at all, and 63 of them converged to purchase.

[53] Our model estimates suggest that perceived prior SD is around 10 times of the perceived signal SD. In this case, steady state is quickly achieved – after collecting 20 signals, an additional signal will decrease the posterior variance by less than 5%. In our data, 110 observations reached steady state (more than 20 signals collected) for one source, but did no search from the other source. Among those observations, 90 observations collected signals from positively relevant source only (42/90 converged to purchase), and 20 observations collected signals from negatively relevant source only (7/20 converged to purchase). Therefore, we have sufficient information to identify $(\theta_0^{with}, \theta_1^{with})$.

prior variance) and perceived similarity with the source where the steady state is achieved (a_j or b_j). Therefore, general tendency of purchase among these observations identifies parameter θ_0^{with}, and the difference in purchase driven by the sign of similarity identifies θ_1^{with}.

The parameters related to perceived similarity between the two sources $\left(\theta_0^{bw}, \theta_1^{bw}\right)$ are identified from the observed purchase decisions given a sufficiently large amount of search from both sources.[54] For these observations, purchase utility depends only on parameters identified so far (i.e., utility parameters, perceived prior variance, and perceived similarity with each source) and perceived similarity between the sources (c_j). Therefore, general tendency of purchase among these observations identifies parameter θ_0^{red}, and the difference in purchase driven by balance status identifies the parameter θ_1^{red}.

A general pattern of increase in purchase likelihood with respect to the amount of observed search (n_{ij}^*) will identify the parameters of the perceived signal variance θ_0^{sig}.

Finally, we can identify cost-related parameters (Δ) and scale parameter (λ) with observed search decisions. Given the identification of Γ and Θ, the expected informational benefit, $v_j^b(\cdot)$ in Equation 3.17, is identified. The effect of the expected informational benefit on search decision will identify λ. The effect of $\left(n_{ij}^A, n_{ij}^B\right)$ on the search decision captured through $v_j^c(\cdot)$ will identify δ_0. The parameter δ_1 is identified

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[54] In our data, around 265 observations reached steady state (i.e., more than 20 signals collected) for both sources. Among those observations, 126 observations were under balance (48/126 converged to purchase), and 139 were under imbalance (53/139 converged to purchase). Therefore, we have sufficient information to identify (θ_0^{bw}, θ_1^{bw}).

from a systematic difference in $v_j^c(\cdot)$ when signals are collected from a source with a negative similarity as opposed to a source with a positive similarity. Similarly, the parameter δ_2 is identified from any systematic difference in $v_j^c(\cdot)$ when signals are collected under an imbalanced system as opposed to a balanced system.

CONCLUSION

I have not only provided novel evidence that social learning is a significant driver of consumer decisions but also provided a richer, more nuanced understanding of how social learning operates. Essay 1 shows that the neighborhood *social capital* drives social learning and the evolution of new trials in aggregate level. In Essay 2, I show that social learning operate differently across *trial* and *repeat* stages, so who is most influential and who is most influenceable varies across the stages. Essay 3 documented the evidence that the pattern of *similarity of preferences* such as homophily and structural balance drives consumer search, learning from social contacts, and purchase decision.

The findings can help marketing researchers deepen the understanding of social learning, and further stimulate future study. They are of interest to marketing practitioners as well. The findings suggest that social learning, which occurs naturally among consumers, can partially resolve consumer uncertainty, so help firms achieve their goals. Moreover, the understanding about the drivers of social learning hints what practitioners can do to effectively leverage social learning.