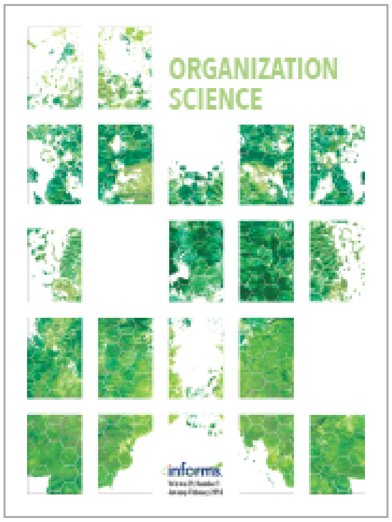

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Social Network *Positions*, Peer Effects, and Evaluation Updating: An Experimental Test in the Entrepreneurial Context

Jason Greenberg[a,b,c]

[a] Wharton School, University of Pennsylvania, Philadelphia, Pennsylvania 19104; [b] Sloan School of Management, Massachusetts Institute of Technology, Cambridge, Massachusetts 02142; [c] Center for the Study of Economy and Society, Cornell University, Ithaca, New York 14853

Contact: jason06g@wharton.upenn.edu, https://orcid.org/0000-0001-9276-6035 (JG)

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Abstract. In many facets of life, individuals make evaluations that they may update after consulting with others in their networks. But not all individuals have the same positional opportunities for social interaction in a given network or the ability and desire to make use of those opportunities that are available to them. The configuration of a person's network can also alter how information is spread or interpreted. To complicate matters further, scant research has considered how positions in social networks and the valence of network content interact because of the difficulty of (a) separating the "player" from the position in networks and (b) measuring all germane content in a particular network. This research develops a novel experimental platform that addresses these issues. Participants viewed and evaluated an entrepreneurial video pitch and were then randomly assigned to different networks, and positions within networks, and thus various opportunities for peer influence that were orthogonal to their network history, inclinations, attributes, or capabilities. Furthermore, all the content of social interaction, including its valence, was recorded to test underlying assumptions. Results reveal that those assigned to a position with brokerage opportunities in a network updated their evaluations of the entrepreneurial video considerably more negatively.

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Keywords: social evaluation • peer effects • networks • experiment • entrepreneurial pitch • agency/structure

Introduction

From the trivial to the profound, individuals constantly make evaluations, which they often update after discussing the evaluated stimulus or subject with peers. Juries epitomize this process, which also occurs in organizational, scientific, cultural, and investment situations—indeed, any situation in which individuals are "at risk" of changing their evaluations as a result of what others may think (e.g., Merton 1957, Harary 1959, DeGroot 1974, Gartrell 1987, Lamont 2012, Zuckerman 2012). Job candidates, for example, are often interviewed separately by a series of interviewers who implicitly or explicitly evaluate candidates and then compare and update their evaluations (e.g., Rivera 2015, Umoh 2018).

Many evaluative processes result in consequential individual, organizational, or societal outcomes, including the adjudication of guilt in trials[1] or merit and funding in scientific or business contexts, such as entrepreneurial pitches or proposals (e.g., Couzin-Frankel 2013, Lee et al. 2013, Huang and Pearce 2015, Wu 2016, Botelho and Abraham 2017, Li 2017, Greenberg 2019a, Bian et al. 2021). Consequently, understanding the mechanisms that influence how individuals update their evaluations after exposure to peers is important. Doing so, however, is a difficult theoretical and empirical matter.

Organizations making hiring, performance, or investment (e.g., venture capital) evaluations vary in how they structure their evaluative processes in terms of the opportunity for, and nature of, social influence (e.g., Suster 2018, Gompers et al. 2019). Notwithstanding a similar purpose, evaluative systems vary because they are designed based on different institutional assumptions about what information and influence are transmitted in social interaction (Wu 2016). These assumptions invoke questions about what kinds of information are shared with respect to content and valence given contextual norms and incentives (Merton 1942), and how the social network structures connecting evaluators shape how shared information is interpreted (Bavelas 1950, Coleman 1988, Burt 1992).

To further complicate matters, evaluation often does not begin or end in formal meetings where interactional structures and decision rules can be designed and enforced, to some extent. Rather, social interaction and influence ("peer effects") begin and continue with informal interaction around the "coffee machine" or "water cooler." Some of these interactions occur by chance or organizational design to increase the odds of certain types of interaction; others occur because some individuals have common interests or characteristics that draw them to similar places (Feld 1981; Liu 2010; Reagans 2011; Bernstein 2012, 2014; Liu and Srivastava 2015; Chown and Liu 2015).[2]

For these reasons, there are limits to what organizations can achieve when trying to shape information flow and social influence that reflect underlying empirical and theoretical issues: First is the challenge of identifying all the important factors that lead individuals to interact, a list that can be intractably long and includes characteristics and behaviors that are difficult to measure (Lazersfeld and Merton 1954, McPherson, Smith-Lovin, and Cook 2001, Greenberg and Mollick 2017 [esp. the online appendix], Greenberg et al. 2020b). Second, within any social setting, individuals differ considerably in their desire and ability to engage with others (Mehra et al. 2001, Kearns et al. 2009, Burt 2012, Smith et al. 2012, Burt et al. 2013, Gulati and Srivastava 2014). Third, theoretical work on social networks suggests different ways in which occupying particular network *positions* leads to different predictions concerning susceptibility to peer effects: Occupying a position that affords the opportunity to mediate other parties (brokerage) allows for more information and the cognitive freedom to resist peer effects (e.g., Burt 2010). On the other hand, receiving similarly valenced information from different and disconnected network sources could increase its effect on updating by reinforcing a common interpretation of a stimulus (e.g., Centola 2010). Fourth, and related to the prior point, the *opportunities* available in different social network positions are not randomly distributed. Rather, the characteristics of "players" and positions are correlated (Giddens 1979, Burt 1982, Sewell 1992, Emirbayer and Goodwin 1994). Those players with superior capital often occupy preferential positions that, in turn, help facilitate greater accumulation of capital.

These issues reflect deep theoretical and associated empirical challenges for evaluative systems with peer effects, including endogenous, correlated, and contextual effects (Manski 1993, Angrist 2014). These challenges often arise, as alluded to above, because of the following: (a) Social interaction, especially informal interaction, is not random. Rather, it is shaped, in part, by a host of supraindividual forces, including history, institutions, culture, and networks ("structure"), that conspire to bring similar individuals together in common settings. (b) Within a social context, there is considerable variation in individuals' desires and abilities to engage with others ("agency" or "choice"). (c) These forces are reciprocal and heterogeneous, making it difficult to separate them (Giddens 1979, Burt 1982, Sewell 1992, Emirbayer and Goodwin 1994, Rider 2009, Kleinbaum et al. 2013). Indeed, as Burt (2012, p. 545) put it, "The agency question is often raised: How much does the network association with achievement depend on the person at the center of the network? Though often raised, the question has received too little attention to allow a general response." Finally, (d), whereas we know about how information flows in various network structures, measuring and discerning differences in the valence of network *content* within distinct structural positions is not well understood (Burt 2004, 2008; Greenberg 2014, 2019b), nor is it well understood how content and network position can interact to amplify or dampen effects.

In this research, I develop a novel experimental approach to contend with these empirical issues to answer the question: How does an individual's (experimentally assigned) *position* in a network of peers cause the individual to update the individual's evaluation in light of the individual's (experimental) peers' evaluations?

To do so, I created an experimental platform that allows for the random assignment of individuals to networks of possible peers, and positions (e.g., brokerage) within those networks that have distinct *opportunities* for information acquisition and influence. What is particularly valuable about this approach is that it makes no assumptions about how actors "should" behave in particular positions; it does not "assume away" that position and player are collinear given the presumed benefits of a position and actors' maximizing, homogeneous interests (Burt 2012)—that is, that the "prince" occupies a favorable position because of his inherent qualities the "pauper" does not possess. Furthermore, it does not assume a baseline in which interaction and information opportunities are randomly distributed—an implausible assumption that is contravened by a substantial body of prior research (e.g., Lazersfeld and Merton 1954, McPherson et al. 2001, Kossinets and Watts 2009, Greenberg and Mollick 2017 [esp. the online appendix]).

Rather, the design developed here randomly assigns participants to positions that reflect different possible communication and thus influence opportunities. Hence, the social network opportunities randomly available to each participant are knowable and measurable by the researcher ex ante and uncorrelated with each participant's history, personality, social or human capital, or any other characteristic or condition. The platform then enables the observation of how study participants chose to make use of the randomly assigned opportunities made available to them, and descriptively what information content resulted from those choices. Finally, the platform enables the researcher to measure the implications of those random opportunities for peer effects as they cause individuals to update their evaluations.

This research platform thus allows the researcher to observe choices and the resulting content circulating within random social network positions with distinct informational opportunities that are exceedingly difficult, if not impossible, to observe "in the wild." Further, given the correlation observed between the random opportunities and observed choices that will be discussed in greater depth below, this research can employ an "intent to treat" (ITT) analytical approach (Fisher et al. 1990). Hence, experimental positions are used to predict observed outcomes without including "posttreatment" choices or correlates in empirical models that might introduce bias (Montgomery et al. 2018).

Results reveal that those assigned to a brokerage opportunity *position* in the network update their evaluations of the stimulus considerably more negatively as a main effect, and this effect is statistically moderated (exacerbated) based on the (random) peer effects to which these participants are exposed, particularly negative peer effects.

The evaluation updating model tested in this paper is a general model that potentially applies in many consequential settings, including scientific review processes and organizational, hiring, and investment decisions (see, generally, Shapira 2002). Furthermore, the experimental design has several advantageous features: (i) First, it enables the assessment of temporal evaluation updating, not only differences in adoption between those assigned to treatment or control. (ii) Although the opportunities for social interaction were randomized within network positions, and individuals were randomly assigned to networks, choices within these randomly assigned opportunities could be observed as a basis of testing assumptions about agency rather than "assume them away." (iii) Social interaction in the experiment is dynamic and mimics real-world messaging, which adds to its verisimilitude. (iv) The experimental platform is scalable and modular, which might be valuable for future research.

To develop intuition about the issues at stake, the following section discusses network theory concerning structures and positions. I then integrate these ideas with those pertaining to information valence effects from psychology. After motivating the theoretical arguments, I discuss the empirical challenges along with my experimental approach to contend with them. Findings are then presented, followed with a discussion of limitations, organizational implications, and future research opportunities.

Theory Concerning: Network Positions, Opportunity, Choice, and Content Valence

Network theory and experimental and observational research have established quite convincingly that networks are an important conduit of information (e.g., Granovetter 1973, 1983; Burt 1992; Greenberg and Fernandez 2016; Greenberg 2019b). Furthermore, empirical work, beginning with the classic MIT communication studies, substantiated the idea that the structure of a network has a bearing on its communication flow and pattern, which predicts performance outcomes (Bavelas 1950, Leavitt 1951). Research has also demonstrated that individuals who occupy (or are assigned to) axial positions in a network structure have specific advantages that include a greater volume of information (Bavelas 1948, Shimbel 1953, Shaw 1954, Freeman 1978, Koka and Prescott 2002). Theoretical models of network structural effects progressed significantly from the 1970s to the 1990s, particularly with the introduction of Burt's (1992) conception of structural holes and brokerage.

According to Burt, brokers have benefits in access, timing, or referrals, or vision advantages (Burt 1992, 2012). The sites for the realization of these benefits are structural holes, which reflect a specific type of position in a social structure in which (clusters of) actors are disconnected from each other yet connected to a common third party (the broker). Burt (1992, p. 65) used the analogy of the circuit-breaking function of structural holes insofar as they potentially limit and allow for the control or capture of information like "a buffer, like an insulator in an electric circuit." Hence, the occupant of this position is given the opportunity to access more—and a greater variance of—information. More recently, Burt (2010, p. 10) suggested that being a broker has cognitive implications as well, insofar as it enables the broker to be more comfortable handling divergent information, opinions, or ways of operating (Burt 2004). This exposure to varying information, in turn, also increases the odds of exposure to negative views about a stimulus or subject in probabilistic terms. There are also significant social-psychological and sociological reasons that this is likely.

Most pointedly, a substantial body of research has documented a reliable, pronounced, and pervasive negativity bias in information integration, recall, and judgment (see, generally, Taylor 1991, Rozin and Royzman 2001, Lane et al. 2020). This negativity bias implies that in information integration and judgment, information that is negatively valenced is weighted more heavily, and recalled more easily, than positive information that is similarly scaled (Cusumano and Richey 1970, Birnbaum 1972, Hodges 1974, Ito et al. 1998). For example, Lutz (1975) found that balanced presentation of relevant product attribute information led study participants in the negative condition to shift their assessments further in the negative direction relative to the positive shift initiated by those in the positive condition.

Underlying this pervasive negativity bias are several information-processing mechanisms. First, in studies of trait adjectives, negative adjectives are rated as both more influential and less ambiguous (Wyer 1973). Second, negative information is often easier to recall (Carlston 1980) and is construed as more distinctive, which also increases the weight associated with it in evaluation (Fiske 1980, Soroka 2006). Finally, research also indicates that negative information is deemed to be more truthful (Hilbig 2009).

Sociological and social-psychological contextual factors pertaining to evaluation also increase the odds of negative information transmission and weighting in evaluation updating in many settings. Most pointedly is the underlying system of evaluation and its method of defining what is considered to be a contribution, as well as to whom credit for discernment, taste, intelligence, or good judgment is due.

In settings where intelligence or creativity is assessed, systems often incentivize criticality, skepticism, and negativity in evaluation on the part of the evaluator (see, generally, Merton 1942). Consider, for example, a venture capitalist interrogating an entrepreneur seeking funding about his business model in a meeting with the firm's partners. If the venture capitalist extols the virtues of the business model, then he acknowledges the cleverness of the entrepreneur. If, on the other hand, he criticizes the model on principled albeit negative terms, then he evidences his acuity to his peers. Similar systems are often evident in academic training and discourse, where graduate students are trained in the science and art of finding fault with reviewed research in a system of organized skepticism (Merton 1942). Hence, in many evaluative settings, there are contextual factors, incentive systems, and self-enhancement motives that favor negativity (Amabile and Glazebrook 1982, Gibson and Oberlander 2008).

Hypothesis 1. *Individuals in brokerage opportunity positions update their evaluations more negatively.*

To summarize the discussion above and the linkage of network structure with expectations about how information with different valences is interpreted and weighted, it should follow that participants assigned to the brokerage position vis-à-vis those who are not should have (a) greater volume of information, which should, in turn, (b) lead to a higher probability of hearing (more) negative information, which implies a negative main effect for brokers.

Hearing information from multiple sources, as a broker does, has two additional implications. First, in probabilistic terms, brokers should receive a greater variance in information concerning the subject of evaluation, as diversity in information is a central facet of brokerage (Burt 1992, 2004, 2012). However, as noted by Burt (2010), brokers are more comfortable handling or ignoring information that deviates across sources along with differences in opinion, which implies that they should be less reactive to their peers' opinions.[3]

Having access to additional information sources, in general, increases the probability of a greater deviation in information valence. That said, it is certainly possible that one's peers have similar evaluations of a subject or stimulus. For example, all members of a faculty or organizational hiring committee may have similarly valenced evaluations of a candidate and thus information to share (negative or positive) in their hiring meeting to discuss the candidate. This information, in turn, is used by the other hiring committee members in the meeting to update their evaluations (on similar reasoning pertaining to opinion aggregation, see, Harary 1959, DeGroot 1974, Golub and Jackson 2010, Jadbabaie et al. 2012). In this case, the individual with more disparate contacts, and thus information, can receive reinforcing signals of a particular interpretation from varied network sources. In this sense, peer effects in the evaluation of a subject or stimulus, particularly an important one, can be construed as a "complex contagion" issue (Centola and Macy 2007, Centola 2010). A complex contagion is one that requires reinforcing affirmations from multiple sources to help lend credibility and legitimacy to a particular interpretation while also reducing uncertainty about whether an interpretation is correct. Given the pronounced negativity bias, the next hypothesis should follow.

Hypothesis 2. *Individuals in brokerage opportunity positions are more reactive to their peers' mean evaluations, particularly if they are negative.*

Experimental Design Issues and Considerations

Endogeneity concerns undermine social network studies concerned with identifying peer effects (VanderWeele and An 2013; Mouw 2006; for exceptions to some of the issues, see Marmaros and Sacerdote 2002, 2006; Falk and Ichino 2006; Mas and Moretti 2009; Hasan and Bagde 2013; Srivistava 2015; Eesley and Wang 2017). The key challenge is that realized social relationships are a function of the nonrandom opportunities available to different individuals, as well as their choices to make use of their opportunities coupled with the choices of those they seek to interact with (Sewell 1992; Emirbayer and Goodwin 1994; Gulati 1995; Smith 2005; Rosenkopf and Padula 2008; Burt 2010, 2012).

Recent experimental network research has made progress identifying causal peer effects by focusing on these different mechanisms in isolation by randomizing opportunity for peer effects broadly defined (Salganik et al. 2006, Aral and Walker 2011), or by experimentally imposing specific network structures (Kearns et al. 2006; Centola 2010, 2011; Shore et al. 2015). This research has thus either identified causal effects at the network structural level or the social conditions assigned to individuals, but not both simultaneously, because of a vexing identification challenge: if one randomizes opportunity for interaction, but (implicitly) allows agentic choice, then the structure is not strictly exogenous with respect to actors' or players' characteristics, preferences, and choices. Conversely, if one imposes a network structure on individuals (e.g., requiring a specific communication network), then individuals' opportunities for interaction are fixed, thereby removing choice.

However, social interaction "in the wild" entails structural, positional, dyadic, and individual-specific factors, including personality and networking style (Giddens 1979, Burt 1982, Sewell 1992, Emirbayer and Goodwin 1994, Mehra et al. 2001, Burt 2012, Smith et al. 2012, Burt et al. 2013, Gulati and Srivastava 2014). Simply put, from an empirical perspective, the "wild" is a complicated endogenous mess. Yet, empirical and theoretical work is based on strong primitives about the drivers or constraints on action as they causally shape outcomes of interest in a given context. Figure 1 depicts these priors in a stylized "game." In the real world, the game board can be construed as a matrix of positions underlying a social network or the formal positions in organizational "games." These games can include those pertaining to staffing on favorable projects, status, or promotion. Figure 1(a) presumes that causal force is derived from the characteristics of individual actors or players such as their human capital. Figure 1(b) presumes that positions are the primary determinants of outcomes. Figure 1(c) depicts the endogeneity in these outcomes in real-world settings.

Experimental Approach Developed Here

To address these issues, I developed an experimental platform that allows for the estimation of randomized opportunity effects derived from network *positions.* (Figure A.1 in the appendix provides a graphic of the design; Table 1 articulates the steps.) I utilize these randomized positions using an ITT analytical approach to predict longitudinal evaluation updating.[4] The advantage of an ITT approach is that it maintains the virtues of the original experimental randomization. ITT also results in conservative estimates, insofar as it includes those who do not fully utilize the "treatment" (i.e., those assigned to a brokerage position but who do not exploit it) in the estimation of the treatment effect (Fisher et al. 1990, Lachin 2000, Shrier et al. 2014). The platform also allows one to observe and measure the dynamic choices that individuals make given their assigned positions and the implications of randomized positions on evaluation updating at a later time point. Observing these choices given experimentally assigned positions allows for the direct assessment of assumptions concerning the uptake of the treatment, as well as the mechanisms presumed to underlie observed networks. This includes the content that is conveyed across, and the action enabled by, particular positions (Burt 2008, 2012).

Experimental Particulars: Place, Process, and Subject of Evaluation

The experiment was administered in a behavioral laboratory. After check-in, participants were brought into the laboratory and instructed to sit in front of private computer terminals that ensured that no participant could view the screens of other participants. Once seated, participants read and were then asked to execute an electronic consent form. After offering consent, participants were directed to view the subject of evaluation—an entrepreneurial promo video for a real start-up seeking funding.[5] An entrepreneurial promo video is a short depiction and description of a core business problem, its extent, and the proposed solution. The quality of a promo video appreciably increases the odds of a successful crowdfunding campaign (Mollick 2014; Greenberg and Mollick 2017; Younkin and Kuppuswamy 2017, 2019; Greenberg et al. 2020a).

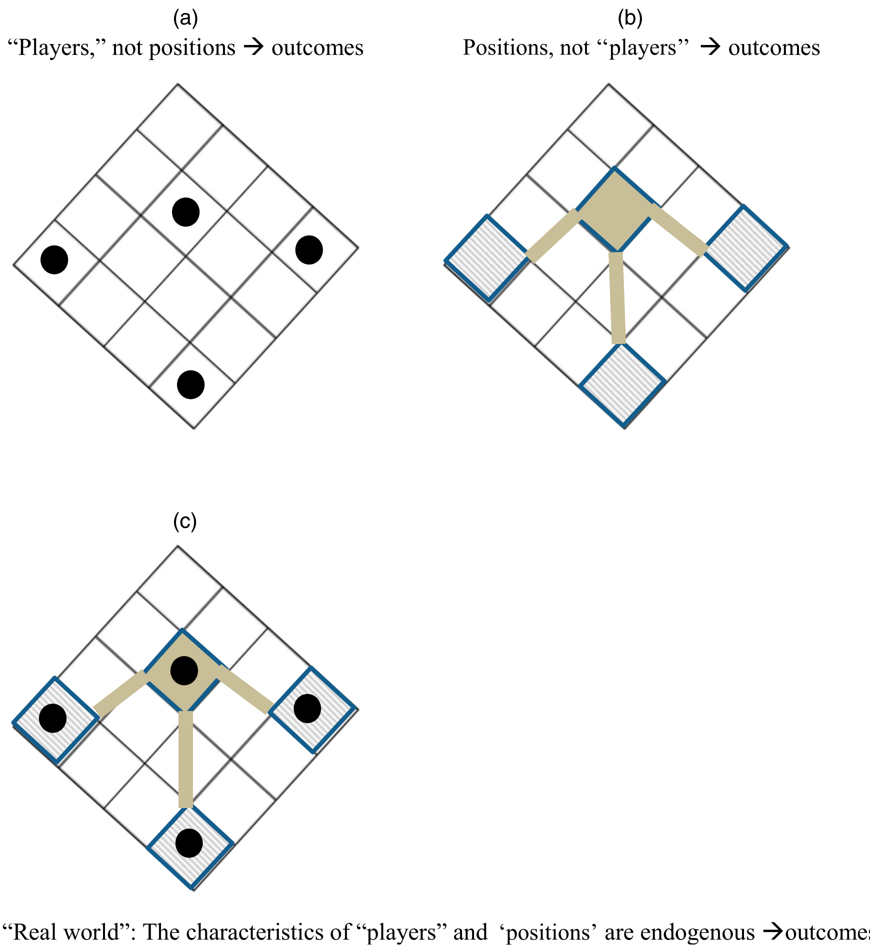
Evaluation Measurement

Once the video was viewed in its entirety, participants were linked to a survey within the experimental platform in which they were asked to evaluate the start-up promo video. Participants first evaluated the video using a seven-point evaluative scale (1 = poor, 7 = excellent) concerning their overall summary assessments of the quality of the entrepreneurial concept. Participants completed their evaluations of the video without the possibility of any contact or collusion with others.

Network Assignment

After evaluations were recorded, participants were randomly assigned to networks in which they had the *opportunity*, but not obligation, to discuss the video with another participant or participants in networks that ranged in potential size from three to six

Figure 1. (Color online) Thought Experiment: Stylized Representation of Social Structure as a Game with Positions and "Players"



Notes. (a) "Players," not positions → outcomes. (b) Positions, not "players" → outcomes. (c) The "real world": the characteristics of "players" and positions are endogenous → outcomes. (1) "→" represents a causal linkage. (2) Stylized social structures in which (i) black dots represent players; (ii) squares denote positions that offer certain rights, responsibilities, obligations, and opportunities. (3) The solid square denotes the brokerage opportunity position. Solid lines connecting squares represent linkages connecting positions (e.g., reporting relationships in an organization).

(mean [M] = 3.27, standard deviation [SD] = 0.68). Approximately 10% of participants were assigned to an "isolate condition" with no possibility of social interaction to help calibrate findings, as these cases are not informative in and of themselves, given the lack of peer effects or network content.[6] Within these randomly assigned networks, participants were randomly assigned one of two positions: One network member was assigned the brokerage opportunity position that entailed the exclusive opportunity but not the obligation to mediate or connect the other network members to whom she had the opportunity to connect (i.e., the two to five other participants). Other network members (alters) were given the opportunity but not the obligation to connect only with the individual assigned to the brokerage opportunity position. Participants in either position (brokerage or alter) could send the initial chat invitation. The recipient of the invitation had no obligation to accept. Critically, chat invitations were made in isolation. Participants could view only their own list of randomly assigned potential peers. No collusion was possible. All opportunities, invitations, and acceptances or rejections were recorded unobtrusively.[7]

Social Interaction in the Experiment

Once assigned to networks, participants had five minutes to discuss the proposed business concept via a user interface that was designed to function and look like real-world messenger applications to ensure ease of use. (A sample conversation is included in Figure A.2 in the appendix.)

The user interface was embedded within the experimental platform. There was no alternative way

Table 1. Summary of Research Process and Logic

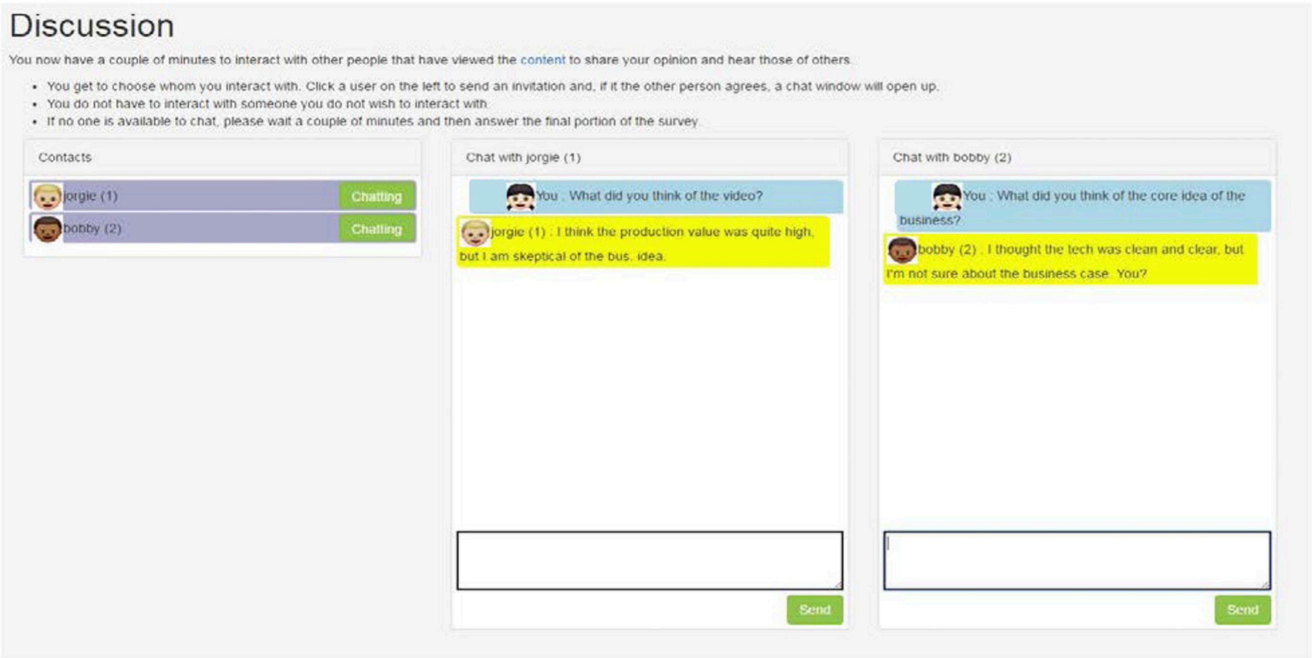
Steps
1. View video individually
2. *Evaluate video*$_{t1}$ *(no social contact)*
3. Conditions (time 2; see also Figure A.1)
a. Some participants randomly assigned to "isolate condition" for calibration
b. Other participants randomly assigned to different networks that vary in size (3–6)
c. Within randomly assigned networks, individuals assigned at random to various *network positions* and thus communication opportunities
i. Brokerage opportunity position (more potential contacts by construction; opportunity to connect alter players|chat invites being extended and accepted by and to alter)
ii. Alter (can send/accept invite only to or from broker)
4. *Evaluate video*$_{t3}$
5. Differencing "4" – "2" provides a measure of each participant's change or updating in her evaluation
6. 3a provides a "no social" condition for calibration
7. 3b allows for the estimation of experimentally controlled opportunity for social interaction
8. 3 provides the opportunity to descriptively measure one's willingness and ability to convert (random) opportunities into social connections
9. 3 also provides an opportunity to measure emergent structural effects

for participants to communicate. After the five minutes elapsed, participants were instructed to evaluate the video again using the same evaluative scale used at time one. Table 1 provides a summary of the logic and flow of the experiment, and Figure A.1 in the appendix provides a graphical representation.

Participant Recruitment and Characteristics

Participants were recruited from a private university in the northeastern region of the United States and were paid $10. All members of the university community at least 18 years old were eligible to participate. A study announcement was posted on the behavioral laboratory's website. Participants were unaware in all respects of the design of the experiment. Study invitation material indicated a scholarly interest in understanding evaluation. The user interface was inspired by current instant messaging and chat applications to aid ease of use. The design process was iterative, and each step entailed user testing (on an unrelated population) to ensure that it was simple, streamlined, and intuitive, as evidenced in Figures 2 and A.2. Two hundred and fifty-six individuals signed up for the experiment. Twenty-six participants did

Figure 2. (Color online) Screenshot of Discussions Reflecting (Reciprocated) Choice to Engage with Randomly Assigned Contacts



Note. Left panel represents the random opportunity set, s (= 2 potential contacts); the middle and right panels represent example discussions that resulted from reciprocated choices.

not show or experienced technical issues at the onset, which precluded their participation in the study. Because these latter individuals were randomly assigned to networks, and their inability to participate may have had a bearing on the network structures of those assigned to the same experimental networks as them, I excluded those randomly assigned to these individuals' experimental networks as well, leaving a total of 197 observations for analysis, including isolates.[8] Those who experienced technical issues were compensated for their time and not allowed to participate again in the experiment.

Table 2 provides demographic information concerning the participants, along with pairwise correlations between key variables discussed in detail in the next section. As is evident, the majority of participants were female, and the modal racial category was Asian, the excluded baseline category. These demographics are generally in line with the racial background of the university subject pool, which tends to attract students from majors that skew female.

Data Analysis

Description of Participant Chat Behavior to Substantiate Model Assumptions

Participants were not directly incentivized to communicate with one another. Rather, I sought to mimic real-world online communication in which some individuals, as a result of personal or social inclinations (Mehra et al. 2001, Burt et al. 2013), seek to discuss a common stimulus for a variety of reasons (e.g., calibrate one's evaluation, socialization, avoid boredom). See Figure A.2 in the appendix for an example chat session.

(Randomly Assigned) Positional Opportunities Strongly Predict Action. Of those randomly assigned an opportunity for social interaction, I calculate an overall attempt to converse percentage of nearly 90.5% (SD = 0. 2368) (i.e., number of chat invitations sent/total number of possible contacts)*100, with the mean number of invitations sent, contingent on the (random) number of opportunities to do so, equal to 1.2022 (SD = 0.5953). The ratio of realized conversations of two or more messages exchanged (which required an invitation and acceptance) was approximately 0.7056 per participant (SD = 0.4083).[9] Participants randomly assigned to the brokerage opportunity position (a) had larger communication opportunity sets by construction (an average of 2.1964 vs. 1.0, t-ratio = 22.6556, p = 0.0000); (b) sent more invitations (an average of 1.732 vs. 0.959, t-ratio = 10.075, p = 0.0000); (c) and engaged in more conversations (an average of 1.4643 vs. 0.7311, t-ratio = 8.6507, p = 0.0000). In regressions predicting the number of invitations sent or meaningful conversations actually engaged in, the experimental positions explain 36.58% and 30% of the variance, respectively.

Collectively, these estimates suggest that most experimental opportunities available to participants were pursued and attained, and the social interactions realized (posttreatment) were strongly predicted by the experimental conditions. Hence, there is empirical justification for the ITT approach used here (Fisher et al. 1990, Lachin 2000, Shrier et al. 2014).[10] This ITT approach, in turn, is consistent with the assumption underlying Burt's (1992, p. 80) initial conceptualization of structural holes in which he proposed to "leap over the motivation issue by taking the network as simultaneously an indicator of entrepreneurial opportunity and motivation." Here, the experimental set-up addresses the motivation issue via randomization and direct and exceptionally granular observation rather than "assume it away."

Evaluation Process and Measurement. For simplicity, it is useful to consider the study as entailing three

Table 2. Descriptive Statistics and Pairwise Correlations

	Variable	M	SD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	Time-three evaluation	4.9213	1.0547	1								
2	Time-one evaluation	5.2360	0.8960	0.6893	1							
3	"Raw" change in evaluation	−0.3146	0.7825	0.5586	−0.2159	1						
4	Proportional change in evaluation	−0.0546	0.1720	0.5578	−0.1865	0.9653	1					
5	Brokerage opp. position	0.3146	0.4657	−0.1794	−0.03	−0.2075	−0.1801	1				
6	Mean centered peer evaluation	−0.0069	0.5808	−0.0521	−0.1452	0.0961	0.0919	0.0342	1			
7	Male	0.3258	0.4700	0.0976	0.0981	0.0192	0.0144	−0.0838	−0.0525	1		
8	African American	0.0449	0.2078	−0.0869	−0.0573	−0.0515	−0.0732	−0.0886	0.0521	0.0806	1	
9	Caucasian	0.3090	0.4634	0.0384	0.0139	0.0359	0.0207	−0.0079	0.0491	−0.0239	−0.0277	1
10	Other race	0.0337	0.1810	−0.0748	−0.119	0.0354	0.0141	0.0746	−0.0492	−0.0634	0.1097	0.0098

Source. Unique experiment administered at a university in the northeast United States.
Notes. N = 178. Omitted category for race/ethnicity is Asian. Pairwise correlations equal to or greater than |0.1452| statistically significant at p < 0.05 or lower.

time points, as depicted in Table 1: time one (*t1*) represents the period in which participants viewed the video content and evaluated it without any social contact, and were also asked to provide background information (pretreatment); and time two (*t2*) reflects the period in which participants were assigned at random to network opportunity positions and experimental networks, and were then given the opportunity to initiate social contact to discuss the video. Communication via the platform messenger ensued. Time three (*t3*) is when participants were given the chance to update their evaluations of the video using the same evaluative scale employed at time one.

Measures

Outcome. The primary outcome measure employed in this study is the time-three evaluation (E_{t3}) after the potential for social influence (M = 4.921; SD = 1.055; range = 2–7).[11] I also calculate (a) a "raw change" metrics as $E_{t3} - E_{t1} = -0.3146$ (SD = 0.7825), with a range of −3 to 2, so that I need not include a lagged version of the predictor on the right-hand side of some equations; and (b) a proportional change measure, $\left(\frac{E_{t3} - E_{t1}}{E_{t1}}\right)_i$, that quantifies for each participant, *i*, her proportional degree of updating relative to her initial, independent, evaluation. The mean is −0.0546 (SD = 0.172; range = −0.5–0.67).

Predictors. Participants were assigned at random to *positions* in distinct networks. These positions, in turn, entailed different *opportunities* for social interaction, as depicted in Figures 1 and 2. Approximately 10% of participants were assigned to a control condition that did not offer any social interaction opportunities as a check on social influence. A dummy variable denotes those assigned to the brokerage opportunity position (M = 0.3146; SD = 0.4657) (Burt 1992). The omitted baseline in peer-effect regression models represents those who, based on their own randomized opportunities, can extend or accept an invitation only to the broker ("alter" players viewed from a "broker-as-ego" perspective).

This measure was interacted with a measure denoting the mean of each participant's experimental network's peers' time-one evaluations of the video, netting out each participant's evaluation from this calculation (i.e., "one's peers' average evaluations") $\bar{\boldsymbol{u}}_{(i)j}\frac{N_j\bar{u}_j - u_{ij}}{N_j - 1}{}_{t1}(\beta_3)$. This measure captures, for each individual, her distinct (potential) peer effects. I then interact the peer mean measure with the dummy variable denoting the brokerage opportunity position. This interaction term is used to test the aforementioned moderation hypothesis.

Analytical Model

A series of regressions is specified below to calculate network opportunity (positional) and structural effects. Because predetermined variables such as race or gender do not add to model fit in the assessment of time-one evaluation (model-F = 0.99, $p = 0.4540$), time-three evaluation (model-F = 0.79, $p = 0.6361$), or the "raw" change in evaluation from time one to time three (model-F = 0.42, $p = 0.9333$), they are not included in the models and do not alter results when included (all available upon request).

I estimate Linear Probability Models (LPM) for consistency because of the need to test interaction terms for Hypothesis 2 and the attendant technical challenges in doing so in nonlinear models (Ai and Norton 2003, Forman et al. 2009, Karaca-Mandic et al. 2012, Greenwood et al. 2019, Greenberg et al. 2020a). Given the categorical measurement of the outcome measure, I also estimate ordered logit models as a demonstration of robustness. A general representation of the model to test hypothesis one is:

$$
\begin{aligned}
Evaluation_{t3} = \beta_0 + \sum_{p=1}^{p} \beta_1 (Evaluation)_{t1} \\
+ \beta_2 (Brokerage\ opportunity\ position)_{t2} \\
+ \varepsilon.
\end{aligned}
$$

In testing Hypothesis 2, which requires an interaction term between the peer evaluation effects at time one as a moderator, it follows that networks of peers create emergent social environments that, in turn, can result in heterogeneous treatment effects. Hence, in these models, I include fixed effects to account for any unobservable network-level environmental effects that are not fully captured by the measured peer evaluations from time one (Abadie et al. 2017). Standard errors (SEs) are robust and cluster-adjusted (Hessian SEs yield similar conclusions). To test the second hypothesis, the following model is estimated:

$$
\begin{aligned}
Evaluation_{t3} = \beta_0 + \sum_{p=1}^{p} \beta_1 (Evaluation)_{t1} \\
+ \beta_2 (Brokerage\ opportunity\ position)_{t2} \\
+ \beta_3 (Peer\ effect)_{t1} \\
+ \beta_4 (Peer\ effect)_{t1} {*} (Brokerage \\
opportunity\ position)_{t2} \\
+ \textbf{Network}\ \mathrm{FE} + \varepsilon.
\end{aligned}
$$

Tests of Experimental Integrity

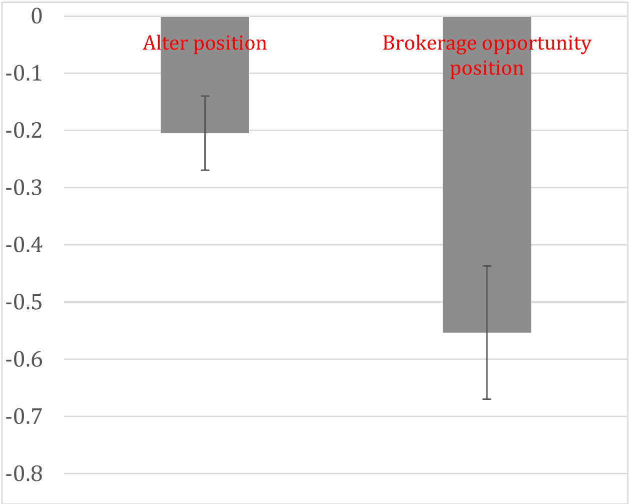
Before moving to the regressions employed to test the hypotheses, I conducted several tests to substantiate the integrity of the experimental assignment mechanism. In particular, I tested for an association between experimental network assignment, pretreatment peer evaluations, and the network structure that *emerged* (i.e., fully connected in which all participants extended/accepted all available invitations) and the predetermined characteristics of participants to detect deviations from randomness using correlations and regressions. Regression results are presented in Table A.1 in the appendix. These models offer no suggestion of any deviation from randomness. More specifically, there is no association between any predetermined variable, such as race or gender and assignment to network position (e.g., brokerage), the magnitude of the mean peer effect, or being in a fully connected network, which required that each randomly assigned participant in a network extend/accept every invitation made available to him or her as a function of her random position and its attendant opportunities. Further, there is no association between any of the variables derived in the experiment—random assignment to network positions, emergent structures in random networks, or pretreatment peers' evaluations.

Presentation of Findings

Baseline Results

To build intuition, Figure 3 graphically depicts how participants in the different conditions updated their evaluations given the opportunity for social interaction relative to their initial evaluations conducted in isolation. Estimates are mean values representing the "raw" change in participants' evaluations from time one (individual evaluation) to time three (postexperimental conditions). The mean change from time one to time three for those assigned to the brokerage opportunity position

Figure 3. (Color online) Mean Change in Evaluation Updating as a Function of Randomly Assigned Network Positions



Source. Unique experiment administered at a university in the northeast United States.
Note. The y-axis represents "raw" mean point change in evaluation from time one (pretreatment) to time three. T-ratio of difference = 2.8138, p = 0.0055.

was an updating *down* of more than half a point (M= −0.5536; SD = 0.1166). This compares with a reduction of −0.2049 points (SD = 0.0647) for alters. These values are statistically significantly different (t-ratio = 2.8138, p = 0.0055).[12]

In Table 3, I present regression models to provide a variety of tests of the hypotheses. The first model, an unconditional LPM model, demonstrates that those randomly assigned to the brokerage opportunity position evaluated the video at time three −0.4063 points (SE = 0.1548; p = 0.011) lower than those assigned to the alter position, ignoring participants' initial (time-one) evaluations.

The second model includes the time-one evaluation. As one would expect, it explains a substantial amount of the variance as it captures each participant's presocial interaction evaluative priors, and has a large, statistically significant effect (b = 0.8059; SE = 0.0741; p = 0.0000). The estimate for the brokerage opportunity position, in turn, now implies that those assigned to this position adjust their evaluations *down* −0.36 points (SE = 0.1232; p = 0.005) relative to those assigned to the alter position. This finding lends support for Hypothesis 1.[13]

Further, to ensure that the effect is robust to a host of assumptions, I estimated several additional models. First, in Table 3, models 3 and 4, I estimate models similar to those just discussed, but which employ ordered logistic regression given the ordinal measurement of the outcome measure. Results remain robust. In Table 4, I estimate several additional models to assess robustness. Model 7, for example, controls for the number of other contacts who participants could theoretically interact with given their random network and positional assignment. The estimate is positive (b = 0.1119) but not statistically significant. Further, the estimate for the experimental brokerage position remains stable in magnitude. Hence, it does not appear that the simple number of potential contacts can explain away the positional argument made here.[14]

In models 8 and 9 in Table 4, I estimate models conditional on participants' time-one evaluations being below or above average, respectively. These estimates suggest that, irrespective of one's initial evaluation and (partially observable) inclination to be positive or negative, being assigned to the brokerage opportunity position leads to a more negative time-three evaluation. The effect is particularly large for those who were more negative before social interaction. Model 10 uses "raw change" in evaluation (i.e., $E_{t3} - E_{t1}$) as the outcome rather than include a lagged evaluation measure as a predictor. The results are consistent with those presented in Table 2. Finally, model 12 in Table 4 employs the proportional change in evaluation measure as the outcome. The results suggest that those assigned at random to the brokerage

Table 3. Regression Models Predicting Evaluation Updating

	(1)	(2)	(3)	(4)	(5)	(6)
Model	LPM	LPM	Ordered logit	Ordered logit	LPM	LPM
Variables	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Brokerage opp. position	−0.4063**	−0.3598***	−0.6684**	−0.9679***	−0.3427***	−0.3505***
	(0.1548)	(0.1232)	(0.2734)	(0.3356)	(0.1212)	(0.1190)
Time-one evaluation		0.8059***		2.3352***	1.5865***	1.5214***
		(0.0741)		(0.3210)	(0.3957)	(0.4181)
Mean-centered peers' time-one evaluation					1.7379**	1.5125*
					(0.8531)	(0.8954)
Brokerage opp. position* Mean-centered peers' time-one evaluation						0.4211**
						(0.1736)
Constant	5.0492***	0.8150*			–	–
	(0.0828)	(0.4101)			–	–
Model fit/diagnostics						
N (networks)	178	178	178	178	178 (56)	178 (56)
F/Wald-χ^2 (df)	6.89	66.45	5.98	55.25	43.34	34
Model exact-*p*	0.0112	0.0000	0.0145	0.0000	0.0000	0.0000
(Pseudo) R^2	0.0322	0.5004	0.01	0.2768	0.5549	0.5659

Source. Unique experiment administered at a university in the northeast United States.
Note. Robust, clustered SEs in parentheses (Hessian SEs yield similar conclusions).
$*p < 0.1$; $**p < 0.05$; $***p < 0.01$ (two-tailed tests).

opportunity position update their evaluations *down* by approximately 6.64% (SE = 0.0291; p = 0.026). Considered collectively, these results provide strong support for Hypothesis 1.

As argued in the motivation for Hypothesis 2, it should follow that a brokerage position increases the odds that its occupants will receive more information, and thus a stronger signal to facilitate

Table 4. Robustness Checks: Regression Models Predicting Evaluation Updating

	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Model	LPM	LPM	LPM	LPM	LPM	LPM	LPM
Variables	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Brokerage opp. position	−0.4935**	−0.6895*	−0.3033**	−0.3487**	−0.3628***	−0.0664**	−0.0704**
	(0.2391)	(0.3451)	(0.1456)	(0.1311)	(0.1200)	(0.0291)	(0.0268)
Time one evaluation	0.8099***						
	(0.0743)						
# of possible connections	0.1119						
	(0.1751)						
Mean-centered peers' time-one evaluation					0.4183**		0.0818
					(0.1606)		(0.0510)
Brokerage opp. position* Mean-centered peers' time-one evaluation					0.4493**		0.0950**
					(0.1703)		(0.0366)
Constant	0.6817	3.7895***	5.2816***	−0.2049***	−0.2007***	−0.0337***	−0.0326***
	(0.4700)	(0.2430)	(0.0712)	(0.0697)	(0.0377)	(0.0092)	(0.0084)
Model fit/diagnostics							
N (networks)	178	29	149	178	178 (56)	178 (56)	178 (56)
F	44.32	3.99	4.34	7.07	8.56	5.21	5.69
Model exact-*p*	0.0000	0.0582	0.0419	0.0102	0.0001	0.0264	0.0018
R^2	0.5016	0.1146	0.0279	0.043	0.1773	0.0481	0.1396

Source. Unique experiment administered at a university in the northeast United States.
Note. Robust, clustered SEs in parentheses (Hessian SEs yield similar conclusions).
$*p < 0.1$; $**p < 0.05$; $***p < 0.01$ (two-tailed tests).

interpretation, from peers concerning a negative evaluation of the stimulus. This is consistent with evaluation updating as an individual-level facet of a complex contagion (Centola 2010) in which confirmation from multiple, disconnected sources is required to accept or internalize a particular interpretation. Given the pronounced negativity bias, this should be particularly true when confirming evaluations are negative in valence.

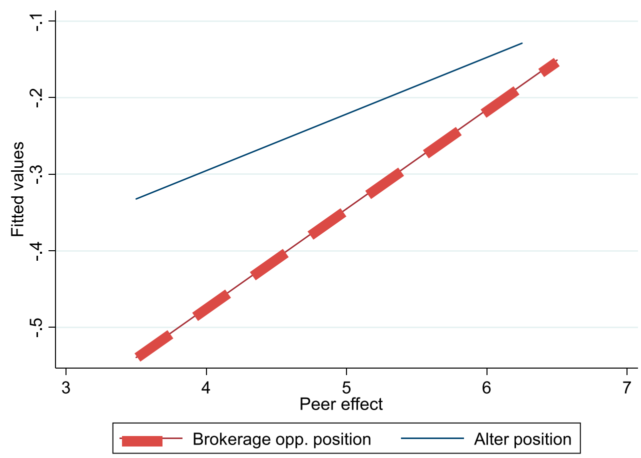
To assess this argument formally, recall that I calculate a measure of one's peers' mean evaluation prior to social interaction, netting out each focal individual's evaluation. I then interacted this measure with the measure denoting a randomly assigned brokerage opportunity position. As argued above, we should observe a moderating effect of the network structure on peer effects, with a larger negative versus positive effect given the negativity bias.

Model 6 in Table 4 provides strong support for Hypothesis 2, that is, that those assigned to a brokerage position update their evaluations as a function of their peers' influence. In model 6, for example, the interaction effect is 0.4211 (SE = 0.1736; p = 0.019). Consistent results hold in model 11 with the raw evaluation updating measure, as well as model 13 with the proportional change outcome measure.[15]

Figure 4 graphically depicts fitted values from a fixed-effects regression model predicting the raw change in evaluation from time one to time three as a function of the brokerage opportunity position, peer effects, and their interaction. (I use the raw change outcome measure because it results in a more intuitive updating interpretation as a visualization. Using the categorical measure results in the same conclusions.)

Consistent with the logic outlined earlier to motivate Hypothesis 2, the brokerage opportunity position and peer effect interaction is large and statistically significant. I then calculated marginal effects at various levels of peer effects. A noticeable trend is evident, as illustrated in Figure 4: The effects are considerably larger at lower (i.e., more negative) levels of the peer effect. In particular, $\frac{dy}{dx}$ = at a mean peer effect of 3 = −1.37 (z-ratio= −3.4); at a mean peer effect of 4 = −0.92 (z-ratio= −3.74); 5 = −0.472 (z-ratio = −3.69); 6 = −0.023 (z-ratio = −0.13); 7 = 0.427 (z-ratio = 1.34). I then compared the slopes for the brokerage opportunity position and alter network conditions at various levels of peer effects using Wald-χ^2 tests. Slopes are statistically significantly different at p < 0.001 or greater for all peer effects below 6. Taken together, these results provide strong support for Hypothesis 2.

Figure 4. (Color online) Evaluation Updating as a Function of Randomly Assigned Peers' Evaluations and Randomized Network Positions



Source. Unique experiment administered at a university in the northeast United States.
Notes. $\frac{dy}{dx}$ = at 3 = −1.37 (z-ratio= −3.4); 4 = −0.92 (z-ratio= −3.74); 5 = −0.472 (z-ratio = −3.69); 6 = −0.023 (z-ratio = −0.13); 7 = 0.427 (z-ratio = 1.34). Wald-χ^2 tests comparing peer effects across the slopes are statistically significant different at p < 0.001 or greater for peer effects below 6.

Measuring Network Content to Substantiate Model Interpretation

Recall that the theoretical mechanism argued to underlie the brokerage opportunity positional effects pertain to the volume of information and its valence. Generally, in the literature, these mechanisms are presumed rather than tested. An appealing feature of the experimental platform used here is that all of the network content is captured and coded in a relational database. Hence, I can test these assumptions. To that end, I analyzed the communication text. After tokenizing and removing stop words, I used SentiWordNet 3.0 to obtain a tuple for each word in a sentence (positive, negative), in addition to the total volume of words and the number of unique and duplicative words (see Figure A.2 in the appendix for sample text).

I find that the volume of words conveyed to a focal individual (i.e., this figure only includes text written *to*, not from, each participant) is considerably higher for those randomly assigned to a brokerage opportunity position (see Table A.2). If I specify a negative binomial model treating network size as the exposure variable (model Wald-χ^2 = 34.53, p < 0.0000), the estimate for the brokerage position is 0.6695 (z-ratio = 5.88). Similarly, those in the brokerage position hear a greater volume of distinct words b = 0.625 (z-ratio = 5.55; model Wald-χ^2 = 30.84, p < 0.0000). Finally, in a QMLE fractional logit model, I predict normalized negative sentiment (b = 0.3387; SE = 0.1344; p = 0.012) as a function of network opportunity position (brokerage) controlling for network size (model Pearson = 117.361). Note that because these measures are posttreatment and endogenous to evaluation updating, they cannot be included in the regressions above without compromising causal inference. However, these analyses describe what content was conveyed in the experimental interactions.

Summary and Conclusion

In virtually every facet of our personal and work lives, we make evaluations. In turn, we often discuss the topic, issue, or entity evaluated with others, and often weigh their views as we update our evaluations. This dynamic may end with a decision, but may also continue thereafter as we gather new information and continue to reassess (and possibly regret) initial evaluations, ultimately leading to learning about how we process and weigh information, and render evaluations in the first place.

How we evaluate—or are *evaluated*—can have significant consequences for ourselves, as well as the organizations and institutions for which evaluations are rendered. Hence, it is important to understand the factors that shape how we update our evaluations, as well as how evaluative systems are designed to allow for, or restrain, social influence. Doing so, however, is difficult because there are numerous forces that shape our opportunities for social influence that tend to co-occur and are reciprocal. Social life is complicated by a host of endogenous processes. In particular, the choices we make (where to live, work, socialize, when and where to eat, whom to speak with and confide in) partially shape the social structure of opportunity for social influence that, in turn, delimits the options available to different people in ways that are generally hard to observe and even harder to tease apart (Giddens 1979, Burt 1982, Sewell 1992, Emirbayer and Goodwin 1994). This is a classic conundrum in the social sciences—the "agency structure" problem.

As Burt (2012, p. 545) put it, "The agency question is often raised: How much does the network association with achievement depend on the person at the center of the network? Though often raised, the question has received too little attention to allow a general response." Yet, if we cannot make progress on this considerable empirical challenge, then we cannot fully understand how the places we occupy in social networks shape what social influence we are exposed to that can change how we update our evaluations. The integrity of evaluative systems, in turn, is undermined accordingly.

This research offered an experimental approach to do so and thus distinguish how (random) opportunity associated with particular *positions* in network structures impact evaluation updating, and how the latter effect is statistically magnified by negatively valenced information from peers to which one is randomly assigned. The experimental design used here reflects a thought experiment. I began with the idea that, rather than "assume away" agency, this research can randomize and record the *potential* positions available to actors, and, therefore, can have people or players with different networking histories, inclinations, and abilities—great networkers and weak networkers—"trade places." By doing so, the most previously advantaged and capable networkers may be assigned to network positions with limited opportunities. On the flip side, the real-world introverts and wallflowers may be randomly placed in advantageous opportunity positions that they may never occupy in the real world.

A second issue concerns content. Networks are (potential) plumbing. However, what flows across networks can vary and have distinct implications. However, content is very difficult to measure (Burt 2008, Greenberg 2019b). This research uses an approach explicitly designed to capture this content. Finally, this research also provides insights concerning how network structure and content interact to shape evaluation updating, thereby addressing two important limitations in prior work.

The data and analyses produced in this research substantiate, build on, and extend important strands of network and organizational research. First, this research provided, in an experimental setting, a direct test of the assumptions that have long undergirded observational research on structural holes and brokerage. In particular, this research shows that (random) *positional* opportunities are highly predictive of actualized behavior (Burt 1992). This finding should be underscored, as it suggests—consistent with Burt's (2012) observational study with individual fixed effects—that "position" is an important driver of action.

These results beg questions concerning how institutional design features and incentives might alter results. This is particularly true as evaluators in a host of contexts, including human resources, fret over the extent to which their evaluative systems are imbued with certain biases, but might be unaware or desensitized to others. It is possible, and indeed probable, that these (latent) biases can be shaped by the social influences exerted on each evaluator in the manner suggested here. In particular, this research demonstrates that placing (at random) individuals in network positions that facilitate access to disconnected contacts is likely to result in more negative evaluation updating.

Hence, matters of system design and institutional context can be considered as future avenues of research. For example, it is possible that both institutional factors and incentive systems will have moderating effects on various parameters in the model that should be experimentally altered and tested. Recall that, in the experiment reported here, participants were all members of the same university community and generally relatively young. This plausibly limits external generalizability, as any study of a particular population does, but was necessary for the use and testing of the novel platform developed here that provides insights concerning the internal validity of mechanisms that have heretofore been exceedingly difficult to estimate and separate in a cohesive model. That said, future research employing the method proposed here can be used on other populations to determine the extent to which the observed findings apply to other settings and evaluations as well. Additionally, all participants were paid the same participation fee in a noncompetitive environment. A conjecture worth further analysis thus concerns whether varying incentive systems (e.g., pay for individual or network performance) and competitive dynamics, including competition for status, might alter agentic behavior to seek additional network contacts at the individual level, as well as whether these individual incentives result in different network-level structures and outcomes (Schilling and Fang 2014, Zhang, et al. 2016). Investigating how individual evaluation updating and N-actor consensus formation converge and diverge could also prove fruitful. As speculated above, it is possible and indeed probable that a consensus can be reached as a function of power and status dynamics, but individual members may still hold divergent evaluations. This distinction is important because the divergence may sow seeds of discontent.

Future work that disentangles the heterogeneous effects of *different* peers by race, gender, religion, *class*, relational style (e.g., Canales and Greenberg 2015), and status on decision making, with experimental control, would add useful insights into which peers influence others, as well as why and to what extent. Finally, the stimulus that individuals evaluate may be referred or suggested by a source (e.g., friend) that may bias one's inclinations in advance of his exposure and initial evaluation. In the empirical model used here, this is incorporated into the initial evaluation and cannot bias the estimate for updating. That said, it might be useful for various reasons to vary preexposure (biasing) information as well. I conclude by noting the potential that the method and platform proposed here offer in adjudicating between subtly different yet important mechanisms that underlie social evaluation more generally.

Acknowledgments

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Appendix

Figure A.1. Schematic of the Experimental Design

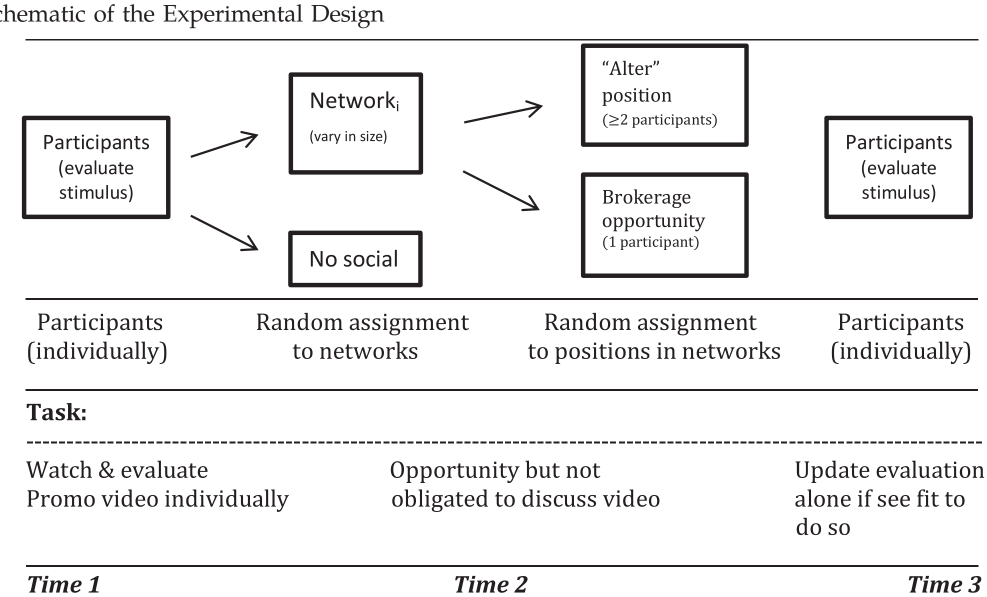

Figure A.2. Example (Unedited) Chat Session with Three Participants

[P1] Hi
[P3] hello lol
[P3] what'd you think
[P2] What did you think of the video?
[P1] Nice visuals. no purpose
[P3] It was good quality
[P3] It may have been able to organize the information better.
[P3] What do you think
[P2] Do you think the product is useful?
[P3] hahah true
[P2] I agree, I thought the music was too annoying.
[P3] seems like a weird idea potentially
[P3] but could work
[P3] Would work I think
[P1] the video has every attribute of the standard ad that appeals to emotion. No appeal to reason was made tho
[P2] People are lazy. Want to skip lines
[P1] I agree
[P3] It was visually appealing and exciting but presented the info in a not so organized way. I was distracted by visuals and the phones used.
[P3] It could be useful. I think people would use it.
[P3] People like to skip lines.
[P2] I don't think it would be very useful, to many problems with the design.
[P1] yeah. cheesy music and the 'feel good' vibe: check. But no explanation of why I need this.

Note. Each line represents a separate text made by each participant.

Table A.1. Logit and OLS Models Testing Exogeneity Assumption

Model	(1)	(2)	(3)
Outcome	Logit: Brokerage position	OLS: Peer effect	Logit: Fully connected network
Variables			
African American	–1.1829	0.1749	—
	(1.0886)	(0.2146)	—
Caucasian	–0.0664	0.0626	0.0996
	(0.3546)	(0.0954)	(0.5762)
Other race	0.5467	–0.0401	0.1594
	(0.4792)	(0.1374)	(0.8053)
Female	0.3381	0.0677	–0.0978
	(0.3607)	(0.0948)	(0.5795)
Fully connected network	0.0424	0.0407	—
	(0.5787)	(0.1590)	—
Peer effect	0.136	—	0.1199
	(0.2847)	—	(0.4748)
Brokerage opp. position		0.0465	0.0432
		(0.0959)	(0.5800)
Model fit/diagnostics			
N (networks)	178 (56)	178 (56)	170 (56)
Log likelihood	–108.7271		–50.6511
F/LR-χ^2 (df)	4.24	0.3	0.17

Table A.1. (Continued)

Model	(1)	(2)	(3)
Outcome	Logit: Brokerage position	OLS: Peer effect	Logit: Fully connected network
Variables			
Model exact-*p*	0.6442	0.9352	0.9994
(Pseudo) R^2	0.02	0.01	0.0016

Source. Unique experiment administered at a university in the northeast United States.
Notes. Models 1 and 3 are logit models. Model 2 is a linear model. "—" denotes unidentifiable. Hessian SEs in parentheses (robust SEs yield similar conclusions).
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed tests).

Table A.2. Random Position and Communication Text

Model	Neg. binomial	Neg. binomial	QMLE Frac logit
Variables	Volume of words	Volume unique words	Net negative score
	b/(SE)	b/(SE)	b/(SE)
Brokerage opp. position	0.6695***	0.6250***	0.3387**
	(0.1139)	(0.1125)	(0.1344)
Network size			–0.2621***
			(0.0927)
Constant	1.5513***	1.5477***	–2.6969***
	(0.0899)	(0.0900)	(0.3155)
N	175	175	175
Pearson			117.361
Wald-χ^2 (df)	34.53	30.84	
Model exact-*p*	0.0000	0.0000	

Source. Unique experiment administered at a university in the northeast United States.
Notes. Robust, clustered SEs in parentheses (Hessian SEs yield similar conclusions). Exposure in negative binomial models set to *Ln*(network size).
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed tests).

Endnotes

[1] One might presuppose that in trials heard by a single judge there is no opportunity for social influence. There is reason to doubt this view, however, as judges often consult with their peers to discuss thorny case-specific issues (Weiser 2015).

[2] For practical applications of these issues see https://executive.mit.edu/blog/space-exploration-mit-experiments-in-open-floor-unassigned-seating#.XYEBbG5FzSE and http://media-publications.bcg.com/ebooks/BCG_Changing_the_Game_Dec_2013.pdf.

[3] I treat this point concerning variability as a testable assumption in the analyses that follow, rather than as a formal hypothesis.

[4] This design was inspired by sociological work on social influence and opinion change, and models of social learning more generally (e.g., French 1956; Harary 1959; DeGroot 1974; Friedkin and Johnson 1990, 1999; Friedkin 2006; Schilling and Fang 2014). However, the design used here focuses attention on how an individual updates her evaluation as a result of both random network opportunity (rather than forced communication structures) for peer effects, whereas the earlier work focused on social influence and group-level opinion consensus or "N-actor agreement." The mechanisms that facilitate consensus or agreement are of considerable importance. However, untangling these mechanisms is analytically complicated because consensus formation entails social influence, individual opinion change, and status, power, and politics. Hence, it is possible that a consensus can emerge for reasons that are only partially correlated with opinion change. Many of these experiments also impose network structures, require a minimum number of conversations (thereby conflating opportunity and agency), and provide instructions indicating that the end goal is consensus. It is also possible that because of status, power, or political reasons, one member might accede to a consensus (e.g., hiring decision) without changing her opinion or evaluation.

[5] The author received consent to use said video from the start-up's founder.

[6] The intraclass correlation in the time-one evaluation and experimental network position is small (0.02) and insignificant (F = 1.67, p = 0.1995).

[7] Participants assigned to one condition had no ability to move to another condition, and could not communicate or coordinate in any way outside of the experimental environment that could lead to possible contamination or interference. Furthermore, to participate in any experiment in the behavioral laboratory, participants required a unique ID that they use whenever participating in an experiment in the university behavioral laboratory. These IDs were required for remuneration, which provided an additional probity check. In terms of assignment conditions, the platform unobtrusively recorded, for each participant, every opportunity for social interaction made available to them, with whom, and what was made of said opportunities.

[8] Including these participants in the analyses results in similar conclusions. To be conservative, however, I prefer this exclusion criterion.

[9] Requiring at least one message and at least one response implies that this estimate is conservative with respect to social influence, as one message could conceivably have an impact on one's evaluation updating.

[10] Hence, "noncompliance" in this case refers to those assigned brokerage positions who do not avail themselves of the opportunities made available to them (akin to someone in a clinical trial assigned to take the treatment "medicine" failing to do so properly). This, in turn, implies that observed effects are conservative as they average in these participants into the estimate. Instrumental variable analyses are discussed below to focus on complier effects.

[11] As an additional check on the reasonableness of this estimate, I conducted a survey on Amazon Mechanical Turk using the same video and scale. The mean evaluation in that study is consistent with the figure reported here: 5.59 (SD = 1.12; N = 565).

[12] Those in the no possible social-influence condition did not update their evaluations (results available upon request).

[13] To calculate the effect for treatment compliers, I estimated an instrumental variable (2SLS) model similar to model 2, but with the number of actual conversations included as an instrumented variable with the randomly assigned *potential* brokerage position the instrument. This model results in an estimate of −0.4788 (SE = 0.1831, p = 0.009). The model first-stage partial R^2 = 0.3024; robust-F = 64.61, p = 0.0000; test of endogeneity of instrument (Ho = exogenous): robust-F = 2.4144, p = 0.1221.

[14] In models not presented here but available upon request, I descriptively analyzed the network structures that emerged as a function of the individual choices that participants made to connect with each other. Of empirical interest here is that, in such situations, I can describe how participants assigned to an alter position changed their evaluations when they had more than one interlocutor. This was possible when participants in a network invited/accepted chat invitations, and the broker chose to "join" chats such that all of those with whom the broker was connected could speak with each other. In a network Fixed Effect Linear Probability Model restricted to those assigned to an alter position with time-three evaluation as the outcome, time-one evaluation as a covariate, and a measure indicating that the participant engaged in two or more chats (model-F = 30.24, p = 0.0000; R^2 (within) = 0.6234), the estimate for the number of interlocutors coefficient is −0.2177 (SE = 0.1454, p = 0.14).

[15] In note 4, I discussed the matter of variability in peer effects and brokerage. In models not presented here but available upon request, I calculated a peer leave-out measure of variability (standard deviation). I then mirrored the approach used here for mean peer effects using the standard deviation measure. The estimates did not approach statistical significance. Additionally, if this model is estimated with random rather than fixed effects, then the results are substantively similar with an interaction effect of 0.3533, SE = 0.1793, p = 0.049. The main effect for the brokerage opportunity position in the model is −0.3695, SE = 0.1201, p = 0.002.

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Jason Greenberg is a visiting assistant professor of management at the Wharton School, a research affiliate at the Massachusetts Institute of Technology, and a research fellow at the Center for the Study of Economy & Society at Cornell University. Jason earned his PhD in economic sociology and organization studies from the Massachusetts Institute of Technology and then served as a postdoctoral research fellow at the Institute for Quantitative Social Science (IQSS) at Harvard. Prior to enrolling at MIT, Jason earned a bachelor's degree in sociology (SUNY Binghamton), a master's degree in sociology (Florida), and a master's degree in public policy (University of Chicago). His research concerns network content and structure, including work on "relational styles" in microfinance; how various dimensions of social structure have a bearing on the shape, functioning, and performance of founding teams; and how networks influence interpersonal evaluation.