Convergence Versus Divergence: How Exposure to Unfamiliar Colleagues Within and Across Network Communities Affects Social Belonging and Network Change

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Abstract

Social belonging is a fundamental human need, which people experience to varying degrees in the workplace. Interventions to boost belonging have typically focused on changing individuals' mindsets. We instead develop a structural intervention that seeks to foster belonging by exposing people to *unfamiliar colleagues*—ones they are not in regular contact with. We consider two forms of such exposure: *convergent*, to colleagues from the same network community as the focal actor; and *divergent*, to colleagues from different network communities. Participants in a non-profit organization (N=213) engaged in a facilitated professional development program with unfamiliar colleagues and were randomly assigned to either convergent or divergent groups. Consistent with pre-registered expectations, convergent-condition participants reported more group solidarity and—three months post-intervention—more persistent ties to fellow group members and greater social belonging. Using computational linguistics and machine learning techniques to impute survey responses, we further show that convergent-condition participants experienced greater belonging than did a synthetic control group. Yet, pointing to the tradeoffs of the two forms of exposure, divergent-exposure participants experienced steeper declines in network constraint and greater increases in betweenness and closeness centrality, *independent of fellow-group-member ties.* We discuss implications for research on social networks, workplace belonging, and organizational interventions.

Social belonging is a fundamental human need, which—when present—is associated with positive emotions, better health, and enhanced self-efficacy, and—when absent—is linked to anxiety, depression, and other negative consequences (e.g., Baumeister and Leary, 1995). Although the sense of belonging can be felt relative to any social group, one of the most important groups to which people can experience belonging is their work organization. In an era when employees developed long-term attachments to work organizations, the experience of belonging was stable and, in many workplaces, generally high—though not necessarily for historically marginalized groups. Yet recent years have seen a fraying of the bonds between employers and employees throughout the employment landscape and a corresponding erosion in the extent to which people experience belonging at work (Hall, 2002; Kalleberg, 2009; Bidwell et al., 2013).

Given the widespread belief that social belonging at work can promote individual productivity and well-being, as well as organizational success, recent years have seen employers seeking to promote social belonging as part of their investments in diversity, equity, and inclusion (DEI). The vast majority of these investments—for example, training programs designed to raise awareness of various forms of bias in interpersonal judgments—have been targeted at changing participants' mindsets and have achieved, at best, mixed success (Kalev et al., 2006; Chang et al., 2019; Dobbin and Kalev, 2022). For example, so-called "wise" interventions that seek to boost newcomers' sense of belonging by exposing them to narratives of past entrants who have successfully overcome obstacles have proven effective in educational contexts (e.g., Walton and Cohen, 2011; Walton, 2014); however, they do not seem to port over successfully to work organizations (Mobasseri et al., 2021). Thus, even in organizations that have seen increases in the representation of historically marginalized groups, many employees do not feel that they truly belong (e.g., Georgeac and Rattan, 2023). Moreover, given the current backlash against DEI programs (e.g., DiTomaso, 2024; Prasad and Śliwa, 2024), there is a pressing need to understand what practices can boost the experience of social belonging in the workplace.

As an alternative to interventions targeted at changing individual mindsets, we propose a structural pathway to boosting the sense of belonging: helping individuals extend their workplace social network connections. This approach is rooted in the core sociological insight that social networks arise not only from individual preferences (e.g., Kossinets and Watts, 2009; Greenberg and Mollick, 2017) but also from opportunity structures for interaction (e.g., Blau, 1977; Feld, 1981; Hofstra et al., 2017)—including the many facets of structure that shape network formation and change in organizational settings (e.g., Dahlander and McFarland, 2013; Kleinbaum et al., 2013; Srivastava, 2015a). It follows that one way to support positive network change within organizations is by exposing people to colleagues they are not in regular contact with, whom we label *unfamiliar colleagues*. Moreover, if the ties formed or reactivated through such exposure persist, they could potentially provide access to such valuable social resources as strategic intelligence, mentorship, and identity affirmation (e.g., Podolny and Baron, 1997; Srivastava, 2015b; Piezunka and Grohsjean, 2023). Although some prior work has examined how access to such resources can shape an individual's attitudes and beliefs—for example, how strongly a person identifies with an organization (Walker, 1985; Jones and Volpe, 2011; Yang et al., 2025)—much of the existing evidence is correlational rather than causal, and the link between positions in network structure and the specific outcome of social belonging has yet to be explored.

Indeed, only a handful of prior studies have attempted to introduce workplace interventions that aim to experimentally "rewire" individuals' social networks or otherwise draw causal inferences of the implications of doing so (Burt and Ronchi, 2007; Srivastava, 2015b; De Vaan and Wang, 2020; Kneeland and Kleinbaum, 2023; Carnabuci and Quintane, 2023). Although these studies have surfaced certain practices that can positively shift workplace networks, they are largely silent on the question of *what kind* of peer exposure is most likely to stimulate network change and what the downstream consequences of such exposure might be. Recognizing that social networks exhibit varying degrees of clustering and can therefore be partitioned into distinct network communities (e.g., Clement et al., 2018; Traag et al., 2019), we consider two kinds of exposure: (a) to peers who come from the same network community as the focal individual and are therefore likely to have similar underlying attitudes, beliefs, and preferences and thus many shared ties; or (b) to peers who inhabit different network communities and are therefore likely to differ along these same dimensions and have few shared ties. We label the former form of exposure as *convergent* and the latter as *divergent*.[1] We then ask: (a) which form of exposure, convergent or divergent, will lead to greater network change? and (b) insofar as these network shifts persist, which form of exposure is more likely to boost an individual's downstream sense of social belonging?

The first question encapsulates a core tension that individuals face when seeking to form new connections. On one hand, exposure to unfamiliar others who hail from different network communities can bring people into contact with valuable new perspectives and ideas (e.g., Page, 2019; Soda et al., 2021; Guilbeault et al., 2023), which might stimulate them to invest in deepening the relationship. Yet prior work has also shown that, when exposed to unfamiliar others, people tend to revert to interacting with similar or familiar others with whom they have more comfort (e.g., Ingram and Morris, 2007; McFarland et al., 2013). On balance, we therefore hypothesize that convergent exposure will produce more group solidarity and thus support the formation and persistence of more ties than will divergent exposure.

With respect to the second question, we acknowledge that divergent exposure can help people tap into information about the "goings on" in an organization, which can help them better integrate and communicate with a wider range of colleagues and thereby boost the sense of belonging (Choi et al., 2023; Yang et al., 2025). Yet we anticipate that convergent exposure, which introduces people to peers with whom they have many shared contacts, is more likely to lead to the closing of triads than is divergent exposure. Closed triads facilitate the flow

[1] Note that convergent exposure need not imply homophilous exposure on such sociodemographic dimensions as gender, racial identity, or age. People may inhabit the same network community based on other underlying similarities such as shared attitudes, beliefs, or occupational backgrounds. Similarly, divergent exposure need not imply heterophilous exposure on sociodemographic traits but could instead reflect other underlying differences.

of expressive social resources such as social support and identity affirmation (e.g., Coleman, 1988; Krackhardt, 1999). Through a psychological process referred to as "affect transfer," the positive attitudes and sentiments that the flow of these resources produces with respect to one's local network tend to be projected onto the organization as a whole (Sluss and Ashforth, 2008; Sluss et al., 2012). Thus, we further hypothesize that convergent exposure will produce higher levels of social belonging than will divergent exposure.

We tested these ideas in the context of a pre-registered field experiment at a non-profit organization.[2] A feature of this setting is that all employees work remotely, thus enabling us to "engineer" different forms of network exposure through a videoconferencing platform without the potentially confounding effects of incidental exposure in an office setting (e.g., at the proverbial water cooler). The study unfolded in two phases. First, we implemented a traditional social network survey and collected metadata of internal employee communications on Slack (an internal communications platform that was used widely by employees in the organization) and email. These data allowed us to identify the mode of internal communications metadata that was most predictive of self-reported social network ties. This turned out to be Slack direct messages. Armed with this insight, we then designed the study's second phase, which involved a novel, network-based field experiment (described in greater detail below). All full-time employees in the organization (approximately 1,000)[3] were invited to participate in our two-arm experiment, with 213 agreeing to do so and completing all facets of it.

Participants were randomly assigned to one of two conditions and invited to participate in

[2]The blinded pre-registration can be found here: `https://osf.io/nkd36/?view_only=4924b5b7ebe24586b7c011010add218f`. Hypotheses 1 and 2 in the paper correspond to H2 in the pre-registration, while Hypothesis 3 in the paper corresponds to H6 in the pre-registration. Given the number of participants we were able to recruit, we did not have a large enough sample to test the hypotheses involving interaction effects (Hypotheses H1a, H1b, H2a, H2b, H5a, and H5b). We did not find consistent support for the other hypotheses (e.g., related to creativity or energy), which were also peripheral to our primary endpoints of interest: social belonging and positions in network structure.

[3]At the organization's request, we do not provide the precise number of employees so as to avoid inadvertently disclosing the organization's identity.

a professional development experience in peer learning circles. This intervention had two components: (a) exposure to a set of unfamiliar colleagues; and (b) a collective learning experience with those colleagues. The latter was consistent between conditions, while the former varied. In one condition, learning circles were constructed to maximize the chances of convergent peer exposure, while, in the second condition, they were formed to maximize the chances of divergent peer exposure. We collected survey data before, immediately after, and three months after the intervention. We used de-identified Slack metadata to delineate distinct network communities in the organization, which then enabled us to assign people to the two experimental conditions and track how participants' networks changed following the intervention. We also used Slack metadata for a supplementary analysis of how the overall *organizational* network changed following the intervention. Finally, we used the content of Slack messages, which were transformed into linguistic categories using the Linguistic Inquiry and Word Count (LIWC) lexicon (Tausczik and Pennebaker, 2010), for a supplementary analysis that sought to compare our two treatment groups to a synthetic control group. Specifically, we adapted an existing machine learning technique (Lu et al., 2024) to impute the social belonging survey responses of the synthetic control group based on LIWC features of their Slack messages.

Consistent with our pre-registered expectations, convergent-condition participants reported more group solidarity immediately after the intervention than did divergent-condition participants. Three months after the intervention, the former also had more persistent ties to fellow group members and reported greater social belonging than did the latter. Moreover, post hoc (i.e., not pre-registered) analyses revealed that convergent-condition participants experienced greater social belonging relative to the imputed social belonging scores of the synthetic control group. Yet we also found that, relative to those in the convergent condition, divergent-condition participants experienced steeper declines in network constraint and greater increases in both betweenness and closeness centrality. Importantly, these shifts can be detected even when we exclude peer learning circle ties from participants' network when computing these measures. In other words, even the relatively brief exposure to divergent versus convergent participants induced by our intervention appears to have caused a lasting shift in the networking behavior of the two groups *independent of the individuals they were directly exposed to.* At the organizational, rather than individual level, the intervention's implementation (across both the convergent and divergent conditions) was followed by an overall decline in network constraint, no change in local clustering, and an increase in global reach (i.e., the extent to which ties span network communities), suggesting that the organizational network as a whole became more interwoven.

Together, these results suggest a potential tradeoff in convergent versus divergent exposure. The former may provide psychological benefits in the form of a positive social experience with peers with whom one experiences group solidarity and network expansion within an individual's comfort zone of like-minded colleagues. Over time, these positive, familiar experiences can improve the psychological sense of belonging in the organization. Yet, at the same time, divergent exposure may enable one to move to advantaged structural positions in the workplace social network. A long line of work has, for example, documented the positive career consequences of bridging structural holes in an organization (the inverse of network constraint) (e.g., Burt, 1992, 2004). Similarly, betweenness centrality often indexes the extent to which others are dependent on a focal actor, while closeness centrality measures the actor's ability to efficiently gain access to resources from others. In other words, convergent exposure may *feel* better to the individual, whereas divergent exposure may enable the person to, over time, *perform* better. At the organizational level, the combination of convergent and divergent exposure may have helped to better integrate the organizational network. We discuss implications of these findings for research on social networks, workplace belonging, and organizational interventions.

SOCIAL BELONGING IN THE WORKPLACE

The need to belong—that is, to form strong, stable, and mutually supportive interpersonal relationships—is a fundamental human motivation that, when realized, is associated with positive emotions, improved health, and enhanced self-efficacy. Conversely, the absence of a sense of attachment to a group has been linked to such outcomes as anxiety, depression, and even suicidal ideation (Baumeister and Leary, 1995; Waller, 2021). Although the need to belong transcends social groups, it is especially salient in the workplace, where the failure to experience it can also have adverse consequences for an individual's job engagement and mental health (Cockshaw et al., 2013; Follmer and Follmer, 2021; Becker et al., 2022).[4]

Although social belonging has been associated with a variety of consequential outcomes, much less is known about how to systematically enhance an individual's experience of it. The best available evidence comes not from workplace settings but rather from the context of higher education. A prominent line of work in this tradition has emphasized so-called "wise" interventions, which trace adverse outcomes such as poor achievement in a new social setting to the process of subjective meaning-making—that is, the working hypotheses people have about a new or unfamiliar social world and what it takes to successfully navigate their way through it (Walton and Wilson, 2018). In line with this approach, a brief social-belonging intervention targeting first-year college students significantly reduced the academic achievement gap for historically marginalized student groups (Walton and Cohen, 2011). Specifically, the intervention exposed new college entrants to narratives from older students who framed the adversity they experienced in their transition to college as commonplace and transient (Walton and Cohen, 2011). Students exposed to this treatment were less apt to assume that hardships they faced in their own college transition were a reflection of

[4]Although belonging is conceptually linked to various other constructs such as inclusion, organizational attachment, and organizational identification, these other constructs all capture an individual's sentiments toward a specific target (e.g., attachment to an organizational subunit or to the organization as a whole). In contrast, social belonging reflects a more generalized and diffuse psychological experience that need not center on a particular target.

their poor fit with the world of higher education and thus more likely to persist through setbacks. Although this particular study was based on a small sample drawn from one university, a subsequent randomized controlled experiment with over 26,000 students across 22 institutions reported that a similar intervention increased the rate at which participants completed their first year of college. This was especially the case for individuals from groups with historically low academic progression rates (Walton et al., 2023).

To our knowledge, only one prior study has sought to experimentally manipulate the experience of social belonging in the workplace. Adapting the brief social-belonging intervention described above (Walton and Cohen, 2011), Mobasseri et al. (2021) implemented a quasi-random field experiment that spanned twelve months and included over 500 newly hired engineers (24% female) who joined a US-based technology firm. Paralleling the design of the social-belonging intervention in higher education, participants in the treatment arm: (a) watched a video of experienced engineers describing the challenges they faced in their transition to the organization and how they successfully overcame them; (b) wrote a self-reflection about the likely challenges they might encounter in their own transition and how they would seek to overcome them; and (c) filmed a video that they were told was intended for the next batch of new entrants with advice about how to successfully integrate into the organization. Participants in the control condition completed the same three steps but with a focus on identifying and overcoming customer service challenges rather than ones related to social belonging. Unlike the comparable social-belonging intervention in higher education, this one produced null results across the range of outcomes examined: annual performance bonus (relative to base salary); time-to-promotion; and network centrality in the firm's internal communications network. Moreover, the intervention did not provide any discernible boost to female engineers, who have been historically disadvantaged in the technology sector.

In sum, attempts to systematically shift the experience of social belonging have taken the same broad tack of many diversity, equity, and inclusion programs (Dobbin and Kalev, 2022;

Cervantez and Milkman, 2024): focusing on changing individuals' mindsets. Yet, at least in the workplace context, the evidence on the effectiveness of such approaches has to date been mixed. As an alternative, we start with the premise that the experience of social belonging may be influenced by the nature and quality of one's social connections in the workplace.

THE STRUCTURAL UNDERPINNINGS OF BELONGING

Research in the social capital tradition has documented the myriad ways in which individuals can gain access to valuable social resources through the ties they form in the workplace (Burt, 1992; Podolny and Baron, 1997; Lin, 2002). Such resources are typically thought to encompass two broad categories: "instrumental" resources that enable one to achieve broader career objectives and "expressive" resources that one acquires for their own sake. Examples of the former include task advice, strategic intelligence, and mentorship or sponsorship (Podolny and Baron, 1997; Srivastava, 2015b; Soda et al., 2021; Iorio, 2022). Instrumental resources can foster social belonging by helping individuals improve their relative performance, enhance their social standing, and ultimately increase their longevity in the organization (Goldberg et al., 2016; Srivastava et al., 2018). Examples of the latter include social support and identity affirmation (Jones and Volpe, 2011; Ingram, 2023; Yang et al., 2025). Expressive resources can amplify social belonging by helping individuals perceive themselves as being more similar and emotionally attached to their colleagues (Paxton and Moody, 2003), including to the point that they even see their self as being intertwined with the organization as a whole (Jones and Volpe, 2011; Yang et al., 2025).

We further build on a core sociological insight that realized patterns of social networks arise not only from individual preferences and choices but also from the opportunities for, and constraints, on interpersonal contact (Lazarsfeld and Merton, 1954; Blau, 1977; Feld, 1981; Kleinbaum et al., 2013). This is especially the case in organizational settings, wherein network ties tend to hew to the boundaries defined by both formal (e.g., departments, functions, hierarchical levels) and semiformal structure (e.g., governance bodies, task forces, and committees that are overlaid onto the formal structure) (Lazega and Van Duijn, 1997; McFarland et al., 2013; McEvily et al., 2014; Srivastava, 2015a). Occupying the same structural position creates opportunities for people to form ties with one another. For this reason, even a transitory structure such as a corporate offsite (Kneeland and Kleinbaum, 2023) or a social mixer (Ingram and Morris, 2007) can serve as vehicles for new tie formation.

Following this logic, we propose that one way to help people forge or new workplace connections, or fortify ones that are nascent or lapsed, is by exposing them to *unfamiliar colleagues*—colleagues with whom they are not in regular contact.[5] This in turn could provide access to social resources that promote social belonging. We acknowledge that the term "unfamiliar colleagues" may not be the best label for this definition given that people may be familiar with or even have had a past connection to colleagues they are not in regular contact with at present. Indeed, there is a growing literature on the activation of so-called "dormant ties" (Levin et al., 2011; Walter et al., 2015; Rondi et al., 2024). From a theoretical standpoint, we are agnostic about whether network exposure leads to the formation of de novo ties, the activation of dormant ties, or the strengthening of weak or nascent ties since all of these network shifts can facilitate the flow of social resources to the focal individual. Hence, our choice to prefer the simpler term, "unfamiliar colleagues," over more complicated alternatives. We turn next to considering the forms of exposure to unfamiliar colleagues that are more or less likely to fuel network change and social belonging.

[5]In differentiated organizations, people may not be in regular contact with one another because they occupy non-overlapping structural positions and thus have limited opportunities for interaction or because they are exposed to one another but have simply chosen not to form a tie. In the former case, exposure may lead them to discover that they have complementary goals or shared personal interests. In the latter case, increased relative exposure can lead people to develop an affinity for one another or learn about shared interests that were previously obscured (Mrkva and Van Boven, 2020). That said, we assume that exposure to unfamiliar colleagues does not guarantee but simply increases the likelihood that a tie will form between them.

NETWORK COMMUNITIES: CONVERGENT / DIVERGENT EXPOSURE

Social networks, including those found in organizational settings, tend to exhibit varying degrees of clustering such that they can be described as encompassing a set of "neighborhoods" or "communities." Prior research has, for example, identified network communities across such diverse settings as alliance networks across firms (Schilling and Phelps, 2007), artist networks across Broadway productions (Uzzi and Spiro, 2005), underwriting syndicates among investment banks (Baum et al., 2003), interorganizational partnerships in the computer industry (Gulati et al., 2012; Sytch and Tatarynowicz, 2014), and professional networks in the French television game show industry (Clement et al., 2018). A characteristic feature of network community structures is that individuals tend to be ensconced in a dense web of ties with others who inhabit the same community and to have only sparse and often indirect connections to peers in other communities.

Depending on the circumstances under which it arises, exposure to unfamiliar colleagues could occur within or across network communities. It is more natural to imagine contact with an unfamiliar colleague occurring across network communities given the relative scarcity of pre-existing, cross-community ties. There are, as a result, fewer opportunities for contact to occur naturally through referrals by shared contacts. Yet, even within the same network community, two individuals may not be in regular touch with one another—even if they have many shared ties—because of time constraints or the nature of the tasks they are each engaged in and the extent to which these tasks are interdependent. We refer to these two forms of exposure—to colleagues who are primarily from the *same* network community as the focal actor and to colleagues who are mostly from *different* network communities, as *convergent* and *divergent*, respectively. In both cases, we assume that the exposure is happening in the context of a meaningful interaction—for example, to discuss an important work-related issue, explore mutual outside-work interests, or engage in a shared professional development opportunity—that motivates the two parties to seek to deepen their connection.

Although both forms of exposure could, in principle, facilitate network change that boosts an individual's sense of belonging, there are tradeoffs associated with each (cf. Reagans and McEvily, 2003; Reagans et al., 2004; Tortoriello et al., 2012). On one hand, individuals who belong to the same network community tend to have a shared identity and develop emotional attachments to one another (Jones and Volpe, 2011; Paxton and Moody, 2003). On the other, individuals who come from different network communities tend to have access to non-redundant or novel information and perspectives, which could have instrumental career benefits or simply help the individual feel better integrated into the organization as a whole (Granovetter, 1973; Burt, 1992; Yang et al., 2025).

In comparing the consequences of these two different forms of exposure, we first begin with the straightforward premise that convergent exposure is more likely to produce feelings of group solidarity than will divergent exposure. This is because people are not only drawn to but also have more positive experiences when they interact with others who have common goals and interests, as well as many shared contacts (Hechter, 1978; Byrne, 1997; Willer et al., 2012; Doreian and Fararo, 2012).

We further propose that convergent exposure will lead to the formation of more persistent ties than will divergent exposure. Although divergent exposure can bring people into contact with colleagues who offer valuable new perspectives and ideas (cf. Page, 2019; Soda et al., 2021; Guilbeault et al., 2023) and might therefore motivate them to invest in deepening these ties, there are also powerful countervailing forces when people come into contact with dissimilar others. Indeed, prior work has demonstrated that, when people are actually exposed to unfamiliar others, they tend to retreat to their social comfort zone of similar or familiar contacts (Ingram and Morris, 2007; McFarland et al., 2013; Reschke et al., 2024). On balance, we therefore expect that the enhanced group solidarity produced by convergent exposure will also translate into greater tie persistence.

Although divergent exposure can help people tap into information about the "goings on" in an organization, thereby helping them better integrate and communicate with colleagues throughout the organization (Choi et al., 2023; Yang et al., 2025), we anticipate that convergent exposure, which introduces people to colleagues with whom they have many shared contacts, is more likely to boost social belonging. This is because convergent exposure is more likely to lead to the closing of open triads. This is a natural consequence of the expectation of greater tie persistence; however, it could also arise over the course of an exposure event as people discover that they have shared contacts. Closed triads, in turn, facilitate the flow of expressive social resources such as social support, emotional connection, and identity affirmation (Coleman, 1988; Lin, 2002; Paxton and Moody, 2003) that can boost attachment to one's local network. Moreover, the presence of mutual ties in a group can: (a) erode downplay one's individuality given the difficulty of asserting one's personal interests over those of the group; (b) diminish one's bargaining power by making the threat of withdrawal from the group less credible; and (c) ease interpersonal conflicts (Krackhardt, 1999). Finally, through a psychological process referred to as "affect transfer," the positive attitudes and sense of "oneness" with the group that social resource exchange produces with respect to one's local network are projected onto the organization as a whole (Sluss and Ashforth, 2008; Sluss et al., 2012). Over time, we therefore anticipate that convergent exposure will lead people to experience heightened levels of social belonging. Integrating these arguments, we therefore propose the following three pre-registered hypotheses:

Hypothesis 1: Having a meaningful social interaction with unfamiliar colleagues from the *same* network community as a focal actor (i.e., convergent exposure) will promote more *group solidarity* relative to having a comparable interaction with unfamiliar colleagues from *different* network communities (i.e., divergent exposure).

Hypothesis 2: Having a meaningful social interaction with unfamiliar colleagues from the *same* network community as a focal actor (i.e., convergent exposure)

will lead to more *persistent ties* relative to having a comparable interaction with unfamiliar colleagues from *different* network communities (i.e., divergent exposure)

Hypothesis 3: Having a meaningful social interaction with unfamiliar colleagues from the *same* network community as a focal actor (i.e., convergent exposure) will promote greater *social belonging* relative to having a comparable interaction with unfamiliar colleagues from *different* network communities (i.e., divergent exposure).

In addition to testing these pre-registered hypotheses, we also conducted post hoc (i.e., not pre-registered) analyses to compare the two treatment groups to a synthetic control group (whose social belonging scores we imputed using a machine learning technique described below). We further assessed how the two forms of exposure, convergent and divergent, might have affected other facets of network change—for example, network constraint, betweenness centrality, and closeness centrality. We did so in two separate analyses: in one case including, and in the other excluding, ties to the individuals a person was directly exposed to through the intervention. The latter allowed us to examine whether mere exposure to colleagues in the context of convergent or divergent groups produced a shift in an individual's networking behavior independent of the ties they might have formed to those group members. Finally, we conducted a pre- / post-analysis of the organizational network as a whole to understand how it changed following the intervention.

Before reporting results from both the pre-registered and post hoc analyses, we first highlight key findings from a baseline analysis that was conducted prior to the intervention and that helped inform its design. This first phase of the study helped us overcome a methodological challenge of assessing network change using traditional network surveys: They can be cumbersome to implement and often yield low response rates—particularly when implemented at multiple points in time. To address this problem, we administered a single network survey before the intervention and collected metadata from various forms of internal employee communications (e.g., emails, Slack direct messaging, Slack public channel communication). This allowed us to identify the form of communication metadata that best predicted the existence of self-reported network ties. By identifying this relationship, we could then use communication metadata as a proxy for the existence of ties in subsequent analyses that assessed network change and could do so without having to rely on longitudinal network surveys.

PHASE 1: BASELINE ANALYSIS

Empirical Setting and Data

Our empirical setting is a large non-profit organization based in the United States. The organization had approximately 1,500 full-time employees at the start of Phase 1[6]. Given that the organization has an equity-focused mission and a mandate that encompasses rural and low-income communities, it has an especially diverse workforce. Employees were distributed across the United States and worked remotely, with nearly all interactions taking place via Slack, email, or Zoom. This feature of the setting rendered communications metadata a more reliable window into internal communications than it would be in an organization where people interacted both face-to-face and electronically.

Social Network Survey

In September of 2022, we invited all full-time employees to participate in a roster-method social network survey. Before we sent participants links to the survey, the organization sent a communication explaining the goals of the research study and clarifying that survey responses would be kept confidential. Of the approximately 1,500 employees employed at the time, 607 employees completed this survey (75 percent female; 36 years mean age; 6.07

[6]We do not provide the precise number of employees so as to avoid inadvertently disclosing the organization's identity.

years mean tenure; 16.7 percent at the manager level; 54 percent minority group members). This sample was fairly representative of the overall population (75 percent female; 36 years mean age; 5.82 years mean tenure; 18 percent at the manager level; 56 percent minority group members). None of these differences between the sample and overall population was statistically significant.

Following standard practice, the survey presented each participant with a list of all other employees in the organization, first those who worked in the same department that they did and then those who worked in other departments. Participants were asked to check the names of colleagues with whom they had meaningful contact in the past three months. They were then presented with a random subset of up to ten of these contacts and asked to indicate what kinds of social resources—for example, task advice or mentorship support—they exchanged with each.

Archived Slack, Email, HR Data

We obtained internal email and Slack metadata for the period from March 2019 through March 2022. Slack data included a total of over 29 million messages, including in 3,176 public channels, 4,049 private channels, and 269,166 direct messages. Email data included a total of over 4.2 million sent emails, 1.9 million of which were meeting invites. Separately, we obtained monthly records, including employee gender, race, tenure, department affiliation, and seniority, from the organization's human resource information system. To protect employee privacy and company confidentiality, all data were de-identified. We also did not obtain access to raw email or Slack message content. We were, however, able to get access to message content that was transformed into linguistic categories using the Linguistic Inquiry and Word Count (LIWC) lexicon (Tausczik and Pennebaker, 2010).

Identifying the Best Metadata Proxy for Self-Reported Social Network Ties

To bridge the gap between the network survey data and the observational digital trace data, we used an amplified-asking approach (Salganik, 2019). Amplified asking allows us to gain many of the benefits of both "observing-" and "asking-based" methods by using predictive modeling to combine survey data from a subset of employees with observational data from the entire organization. We treat the survey data as the "ground truth" of individual networks at a snapshot in time for a sample of employees. The observational digital trace data can then be paired with the one-time survey data to infer granular and longitudinal records for all employees in the organization (e.g., Lu et al., 2024).

Although we obtained both email and Slack data, we learned that email usage varied considerably across employee segments (e.g., older versus younger employees), whereas Slack was the dominant mode of communication for all employees. Thus, we focused on identifying which form of Slack metadata best predicted the existence of self-reported social network ties (based on the social network survey).

Specifically, we constructed 30 different Slack communication networks that varied along three dimensions: (a) type of Slack communication (e.g., direct messages versus messages sent to Slack channels consisting of three of fewer members versus messages sent to all Slack channels, and combinations thereof); (b) threshold number of messages required to infer the existence of a tie (e.g., one or more messages exchanged versus six or more messages exchanged); and (c) time horizon examined (ranging from the two-week span preceding the administration of the network survey to the three-month span).

After constructing these 30 candidate networks, we used the existence (or absence) of a binary tie in a given candidate network to predict the existence (or absence) of binary self-reported ties. We used F_1 scores to evaluate the performance of the metadata networks as predictors of self-reported ties. F_1 scores are a standard method of predictive performance of binary classifiers and are defined as the harmonic mean of the classifier's precision and recall[7]:

$$F_1 = \frac{2}{\frac{1}{\text{precision}} + \frac{1}{\text{recall}}}$$

From this analysis, we concluded that the best predictor of an individual reporting a tie to a colleague was whether or not they had exchanged at least one direct message in any 24-hour period in the 90 days preceding the network survey. We therefore used this measure as a proxy for the existence of a tie in the subsequent analyses that followed in Phase 2 of the study.

PHASE 2: FIELD EXPERIMENT

Sample

In the months following Phase 1, the organization underwent a significant restructuring, which included considerable layoffs. In consultation with the leadership team, we deferred the timing of Phase 2 until the layoffs were complete and remaining employees were able to settle into a new set of routines. In July 2023, we invited the remaining approximately 1,000 full-time employees to participate in our study's Phase 2. Our recruitment email indicated that participants would have an opportunity to opt into the study and that, if they did so, they would gain access to online learning and development content and would then have the opportunity to engage in a peer learning experience that would be facilitated by a professional external coach. They would also be asked to complete short surveys before, immediately after, and three months after the professional development experience.

Given the challenges of scheduling dozens of small group meetings with busy professionals, we implemented the study in two waves—one that took place in August 2023, and the other in November 2023.[8] Of the roughly 1,000 employees who were invited to participate,

[7] $\text{precision} = \frac{\text{true positives}}{\text{true positives}+\text{false positives}}$, $\text{recall} = \frac{\text{true positives}}{\text{true positives}+\text{false negatives}}$

[8] The analyses we report below include wave fixed effects to account for any temporal heterogeneity.

we received initial consent from 318. Of these, 213 (175 in Wave 1; 38 in Wave 2) followed through in attending their learning circle meeting and completing the survey we administered immediately thereafter. This sample represents our treated population. Of those 213, 152 (118 in Wave 1; 34 in Wave 2) completed the three-month survey, with 118 of those (101 in Wave 1; 17 in Wave 2) completing the relevant dependent variable items. Figure 1 depicts the two phases of activity, the steps involved in phase (which are also detailed below), and how the sample size varied across the phases and steps.

———————— INSERT FIGURE 1 ABOUT HERE ————————

Table 1 provides a comparison between the treated sample of 213 employees and the population of about 1,000 full-time employees. We report p-values for two-tailed tests of differences in proportion or means for each demographic variable. The differences between the two groups are generally not statistically significant, although there is a marginally significant difference in tenure.

In supplemental analyses (described in greater detail below), we applied a second amplified-asking approach that allowed us to expand our sample in two different ways. Specifically, we developed a model to predict individuals' responses to the social belonging survey based on the LIWC content features of their Slack communications. Unlike prior work that has sought to use machine learning techniques to predict survey responses based on communication data (Lu et al., 2024), here we had access to three different waves of surveys on employees' social belonging and corresponding Slack data for all three time periods. Thus, we could develop a model that was more robust to temporal variation in the relationship between language use in electronic communications and attitudes expressed in a survey. Armed with this model, we could then impute social belonging scores for the 93 individuals who completed the pre-intervention survey but not the post-intervention one. This imputation technique also enabled us to construct and impute social belonging scores for a synthetic control group of individuals who were observationally very similar to our two treated groups but did not experience the intervention. The sample size for the analysis including all treated participants, as well as those in the synthetic control group, was 485.

——————— INSERT TABLE 1 ABOUT HERE ———————

Experiment Design

The experiment unfolded in six steps, which are detailed below.

Step 1: Identify Network Communities. In the first step, we used Slack metadata to identify employee network communities. To detect an employee's community, we implemented the Leiden community detection algorithm (Traag et al., 2019) to partition the network into distinct communities and infer to which community each employee belonged. The Leiden algorithm is an unsupervised method for partitioning networks into disjoint communities by optimizing the modularity of the partition. The resulting partition identifies communities with relatively dense ties for individuals in the same community and relatively sparse ties for individuals in different communities.

This procedure identified 18 distinct communities within the organization. As the communities formed this way are disjoint, community assignment was exclusive (each individual belonged to only one community) and exhaustive (each individual belonged to a community). While the communities were inferred exclusively from the Slack communication metadata, we found that the communities primarily correlated with organizational—rather than demographic—features (further details are provided below in Results: Descriptive Statistics, Correlations, and Balance Checks and in Table 5). Two individuals from the same community were treated as convergent, while two individuals from different communities were treated as divergent.

Step 2: Develop and Test Learning Circle Assignment Algorithm. Next, we developed a group assignment algorithm that sought to construct peer learning circles consisting of up to four employees who: (a) were unfamiliar with one another (i.e., not in regular contact with each other, as indicated by their Slack direct messaging activity); and (b) either came (mostly) from the same network community (convergent condition) or (mostly) from different communities (divergent condition). Given differences in the sizes of the different network communities, it was often not possible to construct "perfect" learning circles in which *all* participants hailed from the same community or different communities. Instead, our algorithm sought to maximize the likelihood that groups assigned to a given condition (convergent or divergent) included members with the desired network community memberships (all the same or all different). Given uncertainty about how many people would eventually sign up and then complete participate in the intervention, we tested the algorithm's robustness and refined it by running simulations of random subsets of different numbers of employees opting into the study. Figure 2 provides a schematic representation of the two experimental conditions.

——————— INSERT FIGURE 2 ABOUT HERE ———————

Step 3: Intervention Design. The goal of our intervention was to expose participants to unfamiliar colleagues (either convergent or divergent) in a manner that would encourage the formation or activation of social network ties. To accomplish this, we needed for the exposure to take place in the context of a meaningful experience—ideally one in which participants came to appreciate each other's qualities and felt motivated to deepen the connection even after the initial exposure. The approach we selected included several components. First, participants received access to four modules of content from an online learning and development platform: Harvard ManageMentor.[9] Each individual in a learning circle was instructed to complete one of the four modules (to which they were randomly assigned) and prepare a summary of the key insights and how to apply them in the specific context of this non-

[9]Further details about this platform can be found here: https://hbr.org/harvardmanagementor. For our study, participants were given access to one of four of the ManageMentor courses: Global Collaboration; Change Management; Feedback Essentials; and Innovation and Creativity.

profit organization. They were told that other participants would be preparing summaries of the other modules and that they would have an opportunity to learn this content from their peers. We contracted with a service provider, New Level Work, to engage professional facilitators for each learning circle.

We instructed the facilitators to follow a consistent approach in each learning circle meeting. Participants first introduced themselves and shared some personal information about their goals and aspirations in the organization. Next participants provided a summary of the learning and development module they had individually prepared. The facilitator then helped participants integrate the insights across the four modules and think about how they could apply the learning in their job roles and how doing so might support their career goals and aspirations. The facilitator then encouraged participants to stay in touch with each other even after the session concluded and highlighted how they could be an ongoing resource for each other. Given that this was a complicated procedure, we designed and delivered a "train-the-trainers" mini-course to the professional facilitators before the actual intervention to ensure that participants had a comparable experience across learning circles.

Neither participants nor facilitators knew which experimental condition they were assigned to or what specific hypotheses were being tested. Importantly, the experience of the intervention was identical across convergent and divergent groups. Given that both groups included unfamiliar colleagues, it is unlikely that participants would have drawn indirect inferences about the type of group (convergent or divergent) they were assigned to. At the end of the learning circle, participants were asked to complete a short survey and were told that, three months after the session, they would be asked to complete a final survey.

Step 4: Intervention Implementation. After sending out invitations and obtaining employee commitments to participate in the study, we implemented the group assignment algorithm that had previously been tested on simulated data for the actual sample of participants. As noted above, we received confirmation and informed consent from a total of 318 participants.

To simplify the logistics for participants and facilitators, we obtained everyone's availability and sent out calendar invitations and Zoom links to each group.

Step 5: Immediately-Post-Intervention Survey. Immediately after the intervention, participants were asked to complete a short survey that focused on their experience with their learning circle. Included among these questions was one related to the feeling of group solidarity (Hypothesis 1).

Step 6: Three-Month-Post-Intervention Survey and Additional Network Data Collection. Three months after each implementation wave, we sent participants the final survey, which included questions related to their sense of social belonging in the organization (Hypothesis 3). Our final three-month survey sample of 118 participants (101 from Wave 1 and 17 from Wave 2) included all participants who completed the relevant sections of the three-month survey, and who participated in a learning circle. We also obtained additional Slack data for the three month period following the intervention to assess the persistence of ties (Hypothesis 2) and to conduct our post hoc network analyses. Because we had access to Slack data for all employees who opted into the study and not just those who completed the three-month-post-intervention survey, the sample for our network analyses includes 211 participants.

Slack Communication Networks

As noted above, to calculate network outcome measures for each employee, we began by imputing the existence or non-existence of a tie using Slack direct messaging data as a proxy for self-reported ties. Nodes in the network represent users, and edges represent the existence of a direct message or multi-user direct message exchange between users.

We construct networks for the 90 days before and after each treatment period, with one exception. Because of the organizational restructuring alluded to above, we began the window of analysis of the Slack communication networks on June 9, 2023. The first rolling network window is therefore shorter than 90 days, running from June 9, 2020 to August 1, 2023 (the start of the treatment window). This is the same window used to generate the network communities that informed the treatment condition assignments. From then on, we used 90 day windows in order to harmonize the timing with the follow-up surveys. While this induces lack of time balance between the pre- and post-treatment periods, it is nonetheless a common duration between both conditions for all individuals, and both periods reflect long, conservative windows for the persistence of network interventions compared to the prior literature. Moreover, our network measures are based on synchronous communication ties: the existence of a direct message exchange between employees in any 24 hour period in the 90 day window, which should not be significantly affected by the difference in the (already long and conservative) window durations. Of the 213 treated individuals who participated in the learning circle and took the learning circle survey, two had no Slack activity during the pre- or post-treatment periods, yielding an ultimate sample of 211.

Variables

Our analyses estimate a number of outcomes of interest, including ones specified in our pre-registration, as well as network outcomes used in post hoc analyses.

Dependent Variables

We had three pre-registered dependent variables, which correspond to our three main hypotheses: group solidarity immediately after the intervention, learning circle tie persistence, and belonging.

Group Solidarity. We measured perceived group solidarity, immediately after the learning circle completed, using three items (alpha = .93) on a scale from 1 = "Not at all" to 7 = "A great deal", adapted from Willer (2009). The items included "How much solidarity do you feel your learning circle had?"; "How much did your learning circle feel like a team?"; and "How much do you feel your learning circle stuck together?"

Learning Circle Tie Persistence. As described in our pre-registration, we measured the persistence of a learning circle tie by determining whether there was a bilateral communication tie between two individuals assigned to the same circle at any time in the 90-day post-treatment period. As described above, a tie represents the existence of a direct message or multi-user direct message exchange between employees (i.e., each sending at least one message) in any 24-hour period. No users had prior ties with circle peers, following the intervention design. This sum of such ties produces our measure of the persistence of a tie with a circle peer.

Δ*Belonging.* As specified in our pre-registration, we measured belonging using four items (alpha = .93) on a scale of 1 = "Strongly Disagree" to 7 = "Strongly Agree", adapted from Good, Rattan, and Dweck 2012. Items included "I feel that I belong at (organization name);" "I consider myself a member of (organization name);" "I feel like I am a part of (organization name);" and "I feel a connection with (organization name)." Then, we take the first-difference of an individual's belonging in the pre-treatment and post-treatment periods to create the measure of ΔBelonging.

Additionally, for several robustness tests related to addressing survey attrition and for assessing the effects of the intervention relative to a synthetic control group, we employed computational linguistics and machine learning techniques to impute survey-based belonging from participants' Slack direct message communications, following an amplified-asking framework (Salganik, 2019; Lu et al., 2024). This approach, detailed in Appendix A and summarized conceptually in Figure A.1, allows us to extend measurement of belonging to individuals who did not complete surveys, while preserving coherence with our observed survey responses. Our implementation represents a methodological advance over prior work (Lu et al., 2024) by leveraging multiple waves of survey data (rather than a single wave), which is aligned with corresponding periods of Slack communications. This allows us to construct and validate a measure of social belonging that is more robust to potential changes in the underlying relationship between the language expressed in everyday communications and the attitudes reported in a survey. By training a gradient boosting model on linguistic features extracted from Slack messages and evaluating its predictive performance across survey waves, we ensure that the resulting imputed measure captures the stable dimensions of belonging rather than transient fluctuations. We use this approach to impute a measure of ΔBelonging for all members of the organization who sent direct messages using Slack.

We also conducted post hoc analyses that included additional network outcome measures.

ΔConstraint. We measured the change in network constraint for each employee from the pre- to post-intervention period (Burt, 1992). Network constraint can be thought of as a measure of redundancy that is related to an individual's propensity to serve as a bridge across otherwise disconnected communities. Individuals with low network constraint tend to have ties that span "structural holes" in the organizational social network, whereas those with high constraint tend to have overlapping ties with others. Prior work has documented a generally negative relationship between network constraint and various indicators of career success (e.g., Burt, 2004).

We first constructed binary bilateral synchronous communication networks. A tie represents the existence of a direct message or multi-user direct message exchange between employees in any 24-hour period across the the 90 day pre-treatment and post-treatment periods. Constraint for individual i is defined as

$$c_i = \sum_{j \in N(i)} \left(p_{ij} + \sum_{q \in N(i) \setminus j} p_{iq} p_{qj} \right)^2$$

where $N(i)$ is the network neighborhood of individual i and p_{ij} represents the tie between individuals i and j. Then, we take the first-difference of an individual's constraint in the pre-treatment and post-treatment periods to create the measure of ΔConstraint.

Δ*Betweenness.* We measured the change in betweenness centrality for each employee from pre- to post-intervention. Betweenness centrality is typically viewed as a measure of how dependent others are on the focal actor. We first constructed weighted bilateral synchronous communication networks. A weighted tie represents the number of direct messages or multi-user direct messages exchanged between employees in any 24-hour period across the the 90 day pre-treatment and post-treatment periods. Betweenness centrality for an individual measures the number of shortest paths that pass through that individual. Then, we take the first-difference of an individual's betweenness in the pre-treatment and post-treatment periods to create the measure of ΔBetweenness.

Δ*Closeness.* We measured the change in closeness centrality for each employee from pre- to post-intervention. Closeness centrality is typically considered to be a measure of an actor's ability to efficiently gain resource access from others. We first constructed binary bilateral synchronous communication networks. A tie represents the existence of a direct message or multi-user direct message exchange between employees in any 24-hour period across the the 90 day pre-treatment and post-treatment periods. A focal individual's closeness measures how many steps are required to reach every other individual from that focal individual. Specifically, we take the inverse of the average length of the shortest paths to all the other individuals in the network, so that individuals with larger values of closeness can reach every other individual in the network in fewer steps. Then, we take the first-difference of an individual's closeness in the pre-treatment and post-treatment periods to create the measure of ΔCloseness.

Independent Variable

Convergent Condition. We experimentally manipulated whether participants in one's learning circle were mostly convergent (i.e., from the same network community as the focal actor) or mostly divergent (i.e., from different network communities). We include a "Convergent Condition" indicator variable that takes the value of 1 for participants assigned to this condition and of 0 for those in the divergent condition (or in the synthetic control group for analyses that included this comparison). Thus, the coefficient of this variable can be interpreted as the difference in outcomes between the two experimental conditions. In models that include the synthetic control group and an indicator for the other treatment condition, the coefficient represents the difference in outcomes between participants in the convergent condition and those in the synthetic control group.

Divergent Condition. For the analyses involving the synthetic control group, we were able to include indicator variables not only for "Convergent Condition" but also for the other treatment arm, "Divergent Condition." In models that include both treatment indicators, this coefficient represents the difference in outcomes between participants in the divergent condition and those in the synthetic control group.

Control Variable and Additional Variables for Balance Checks

As noted above, to account for differences in the timing of implementation, we include wave fixed effects in all models. Separately, to assess balance across conditions and to construct a synthetic control sample, we consider several other variables. Demographic variables include minority-group membership, female, age, and current tenure. Organizational variables include compensation and salary grades, management level, and department. There were 16 compensation grades. Management level included five categories: entry level, senior level, management level, director level, and senior managing director level. And there were seven broad departments (e.g., recruitment; marketing and communications; finance and legal).

Estimation: Main Analyses

Our main analyses involve regressions of survey-based and network outcomes on *Convergent Condition* with controls and fixed effects. Specifically, we estimate OLS models of the following form:

$$Outcome_i = \alpha W_i + \beta_1 Convergent\ Condition_i + \varepsilon_i \tag{1}$$

where i refers to an individual, and W_i are Wave fixed effects. These fixed effects account for unobserved heterogeneity across treatment implementation waves. To account for the non-independence of observations, we use heteroskedasticity-robust and clustered standard errors. We cluster at the level of treatment: the learning circle. Additionally, as a robustness test, we estimate negative binomial regression models for the dependent variable that is a count variable.

For our pre-registered hypotheses, which are all directional (meaning we made a prediction about the sign of the coefficient, not just its statistical significance), we use one-tailed tests. For all post hoc analyses, we use two-tailed tests.

Results

Descriptive Statistics, Correlations, and Balance Checks

Table 2 shows descriptive statistics for each variable included in our analyses across all treated employees. To verify the efficacy of our random assignment procedure and condition assignment, we show the results of balance checks by condition. These checks affirm that there were no statistically significant differences between experimental condition on key demographic measures. Further, self-reported levels of belonging captured at the beginning of the intervention were not significantly different between the two conditions.

As a manipulation check of our group assignment algorithm, we also tested the proportion of each participant's fellow learning circle participants who were from the same network community as the focal individual. As shown in Table 3, in the convergent condition, 77.89% of participants' learning circle peers were from their own community. In the divergent condition, that proportion was 30.91%, a statistically significant difference. This suggests that our group assignment procedure was successful.

——————— INSERT TABLE 2 ABOUT HERE ———————

——————— INSERT TABLE 3 ABOUT HERE ———————

Table 4 reports descriptive statistics for the sample of Slack messaging data used in the network analyses. Two participants had no activity on Slack during the sample period and have consequently been dropped from all analyses based on the Slack networks. Figure 3 presents the raw mean changes in network outcomes by convergent and divergent treatment condition, along with t-tests of the difference in means between conditions for each outcome.

——————— INSERT TABLE 4 ABOUT HERE ———————

——————— INSERT FIGURE 3 ABOUT HERE ———————

Table 5 shows the result of χ^2 contingency tests of the independence of the network communities with various organizational and demographic characteristics. The network communities correlate primarily on organizational, rather than demographic, features. We observe that we only reject the null hypothesis of independence in the case of management level, compensation grade, department, and age, suggesting that the communities inferred from the organizational Slack networks are related to organizational hierarchy and to department but are unlikely to be related to any other demographic characteristics. As such, our network communities likely reflect characteristics beyond simple demographic homophily. One way to think about the two experimental conditions is that people's baseline networks largely hewed to the contours of formal organizational structure. This is reflected in the correlations between various facets of formal structure and network community membership. The convergent exposure condition afforded the opportunity to interact with unfamiliar peers who are still within these contours, while the divergent exposure condition provided the chance to interact with unfamiliar peers outside of these contours. The former group of peers were likely similar to the focal actor on such dimensions as occupational background, interests, and perspectives (e.g., a marketing versus finance orientation), whereas the latter were likely different on these dimensions.

——————— INSERT TABLE 5 ABOUT HERE ———————

Pre-Registered Analyses

Table 6 reports the results of our two main survey-based pre-registered analyses: regression models of the *convergent* treatment condition on *solidarity* and Δ*belonging* in the presence of implementation wave fixed effects. A positive coefficient implies that a given outcome is higher among the convergent condition, while a negative coefficient implies that a given outcome is higher among the divergent condition.

——————— INSERT TABLE 6 ABOUT HERE ———————

Model (1) shows that, on average, those in the convergent condition reported a .327 standard deviation higher level of group solidarity relative to those in the divergent condition, consistent with Hypothesis 1. Similarly, Model (2) shows that those in the convergent condition reported a .268 standard deviation larger increase in social belonging ($p = 0.043$) 90 days after the intervention relative to those in the divergent condition, consistent with the predictions of Hypothesis 3.

Model (3) leverages the imputed belonging measure to estimate the same model on the full sample of those who participated in the intervention but did not complete the follow-up survey. One concern may be that these attrited individuals differ systematically from those who remained in the study, potentially introducing bias if attrition is correlated with unobserved factors related to belonging. For instance, participants who felt less connected to the group may have been more likely to disengage from both the intervention and the follow-up survey. By using the linguistically derived imputed belonging measure, we can partially address this concern by capturing behavioral indicators of belonging for these individuals based on their communication patterns—even though we lack survey responses for them. This allows us to test whether the estimated relationships observed in the respondent sample generalize to the broader participant population. While this approach cannot fully eliminate the possibility of attrition-related selection bias, it provides a means of recovering information from otherwise missing observations. We observe that those in the *convergent* condition reported a .278 standard deviation larger increase in social belonging 90 days after the intervention relative to those in the *divergent* condition. This estimate is similar in magnitude to the main result in Model (2), suggesting that attrition has not unduly biased the result.

Similarly, Model (4) leverages the imputed belonging measure to estimate the effect of each of the treatment conditions relative to a synthetic control. We use coarsened exact matching on demographic characteristics to construct a synthetic control group that is observationally similar to participants in each treatment group on such key pre-treatment covariates as age, gender, compensation grade, management level, minority group membership and baseline belonging. The matching procedure is described upon in Appendix B, and Figure B.1 illustrates the standardized mean differences in these covariates in the matched sample compared to the overall organizational population. Using this approach, we are able to estimate the effect of each treatment condition relative to the baseline of the synthetic control group. We observe that those in the *convergent* condition reported a .498 standard deviation larger increase in social belonging 90 days after the intervention relative to those in the synthetic control. Taken together, Models (2)-(4) present evidence consistent with the predictions of Hypothesis 3. They go beyond Hypothesis 3 by providing suggestive evidence that participants in the convergent condition experienced a boost in social belonging relative to a counterfactual group that received no treatment at all.

Table 7 reports the results of the main network-based analyses: regression models of the *convergent* treatment condition on various network outcomes in the presence of wave fixed effects. Model (1) provides support for the pre-registered Hypothesis 2, showing that those in the convergent condition formed more persistent ties with those in their learning circle relative to those in the divergent condition. Being in the convergent condition was associated with a .567 standard deviations greater number of learning circle ties relative to being in the divergent condition. Given that learning circle tie persistence is a count variable, Model (5) presents a robustness check that uses a negative binomial regression of the convergent condition on learning circle tie persistence. Hypothesis 2 is again supported, suggesting that those in the convergent condition had more persistent ties with those in their learning circle relative to those in the divergent condition. We note that the intervention leads to significant changes in both social belonging and tie persistence *90 days* after the intervention, suggesting that the effects of the intervention are lasting rather than ephemeral.

Post Hoc Analyses

The remaining models in Table 7 report the results of the post hoc network-based analyses: regression models of the *convergent* treatment condition on various network outcomes in the presence of implementation wave fixed effects. Model (2) shows that those in the convergent condition experienced a less steep decrease in network constraint compared to those in the divergent condition. As Figure 3 illustrates, constraint fell for both conditions, yet those in the convergent condition experienced .286 standard deviations less of a decrease in constraint than those in the divergent condition. Similarly, Model (3) shows that while the intervention seems to have induced greater closeness for both conditions, it led to much greater increases for those in the divergent condition. Those in the convergent condition experienced .244 standard deviations less of an increase than those in the divergent condition. Finally, Model (4) shows that those in the divergent condition experienced much larger increases in betweenness than those in the convergent condition (who experienced a decrease in betweenness). Specifically the change in betweeness for those in the convergent condition was .249 standard deviations smaller than the change for those in the divergent condition.

—————————— INSERT TABLE 7 ABOUT HERE ——————————

Next, we demonstrate that the observed changes in network outcomes are not solely driven by ties among fellow group members but remain robust when these ties are excluded. Specifically, we recompute measures of constraint, closeness, and betweenness using networks in which all potential connections between individuals within the same peer learning circle have been removed. Table 8 reports the results from regression models estimating the effect of the *convergent* treatment condition on these network outcomes, controlling for implementation wave fixed effects. The results indicate that our findings are robust to this alternative specification. The estimated coefficients for the convergent condition remain stable in both magnitude and direction (Δ*constraint*: .286 vs. .284; Δ*closeness*: $-.244$ vs. $-.249$; Δ*betweenness*: $-.249$ vs. $-.252$). This consistency suggests that the treatment effects are not an artifact of within-circle network density but reflect broader structural changes in participants' organizational networks.

——————— INSERT TABLE 8 ABOUT HERE ———————

Together, these results suggest that the convergent condition (relative to the divergent condition) appears to afford greater access to affective and expressive resources that promote solidarity with the intervention group and more persistent intervention-group ties. It also appears to differentially boost participants' sense of social belonging to the organization as a whole. In contrast, the divergent condition (relative to the convergent condition) may enable movement to more advantageous network positions in the form of increased opportunities for brokerage (i.e., decreases in network constraint), fewer steps needed to reach key key colleagues or critical know-how (i.e., increases in closeness centrality), and having more of the organizational knowledge flowing through the focal individual (i.e., increases in betweenness centrality).

Finally, we turn to the question of how the overall organizational network changed following the intervention (across both treatment arms). Table 9 shows a comparison of the organizational network, based on Slack communication, in the pre- and post-intervention periods. The upper block of the table shows means of individual-level network measures for the pre- and post-intervention periods (and associated t-statistics and p-values for a test of the difference in means between the two periods). The lower block of the table shows network-level measures for the pre- and post-intervention periods. We observe significant differences in the organizational network in the period after the intervention relative to the period before—even though only roughly a quarter of organizational members were (directly) treated.

Overall, the decline in the mean individual constraint and the increase in mean individual closeness and global reach suggest that the intervention served to better integrate the organization, leading to a more well-knit network. At the network level, the decrease in diameter (i.e., the shortest distance between the two most distant individuals in the network) also points to a better integrated organization after the intervention. Finally, we observe an intriguing pair of findings with respect to triadic closure. First, we observe that there is no significant effect of the intervention on mean *local* clustering, although it is directionally negative. At the same time, we observe that *global* clustering of the overall network increases post-intervention. This suggests that while the intervention may have increased the total number of complete triplets in the organizational network (as evidenced by the increase in global clustering), individuals have *also* formed new ties that cannot simply be ties formed due to triadic closure (as evidenced by the slight decline in mean local clustering). Of course, these patterns should be interpreted with caution given that they are just based on pre- versus post-intervention comparisons and are not causal.

——————— INSERT TABLE 9 ABOUT HERE ———————

DISCUSSION

The goals of this article have been to identify the structural antecedents of social belonging in organizations and surface the tradeoffs associated with two different forms of network exposure: convergent (i.e., to colleagues from the same network community as the focal actor) and divergent (i.e., to colleagues from different network communities). We did so through an in-depth study at a non-profit organization that unfolded in two phases and tapped into a broad array of data sources—including archived metadata on internal employee communications, a social network survey, repeated administrations of attitudinal surveys, an amplified-asking approach to infer network ties based on communications metadata, and a separate amplified-asking approach to infer survey responses based on linguistic features of the communications content. We designed and implemented a pre-registered field experiment in which participants were randomly assigned to engage in a facilitated professional development experience in either convergent or divergent peer "learning circles." We also used imputed social belonging scores to fill in missing data for individuals who did not complete the study and to construct a synthetic control group of employees who did not participate in the experiment.

Consistent with pre-registered expectations, convergent-condition participants reported more group solidarity immediately after the intervention than did divergent-condition participants. Three months later—highlighting the intervention's long-lasting effects—they also had more persistent peer group ties and reported greater social belonging than did their divergent-condition counterparts. Using computational linguistics and machine learning techniques to impute survey responses, we further showed that convergent-condition participants experienced greater belonging than did a synthetic control group. Yet, pointing to the tradeoffs of the two forms of exposure, it was divergent-condition participants who experienced network changes that are typically associated with career success: steeper declines in network constraint and greater increases in betweenness and closeness centrality. Importantly, these shifts could be detected even when we calculated a focal participant's network statistics when removing ties to their peer learning circle. Intriguingly, this suggests that a relatively brief, one-time exposure to a divergent group of unfamiliar others prompted people to make lasting shifts in their networking behavior and that these changes were not a mechanical byproduct of the ties they formed during the intervention. Moving beyond the individual unit of analysis, we also observed a change in the overall organizational network, which became more integrated in the period after the intervention was implemented.

Contributions

Findings from this study contribute to research on social networks, workplace belonging, and organizational interventions. First, we offer fresh insights on the dynamic interplay between social networks and individual attributes, attitudes, and beliefs (Burt et al., 1998; Totterdell et al., 2008; Srivastava and Banaji, 2011; Fang et al., 2015; Tasselli et al., 2015; Kuwabara et al., 2020; Ingram, 2023). Whereas much of this work focuses on how such individual differences as self-monitoring orientation (Mehra et al., 2001; Sasovova et al., 2010; Kleinbaum et al., 2015), lay theories of networking ability (Kuwabara et al., 2020), and the implicit collaborative self-concept (Srivastava and Banaji, 2011) relate to network outcomes, the present study joins a small but growing set that considers the reverse causal pathway: how positions in network structure influence individual attitudes, beliefs, and values (e.g., Walker, 1985). For example, those who occupy closed network positions tend to engage in more temporal discounting, exhibit limited strategic foresight, and are less likely to cooperate beyond their network or with strangers (Burt, 2017; Opper and Burt, 2021; Burt et al., 2022). More closely related to the present study, individuals who are embedded in dense network clusters or whose ties span multiple network communities tend to identify more strongly with their organization (Yang et al., 2025). Yet the evidence presented in these prior studies has been correlational. In contrast, we provide rare causal evidence of how the experience of interacting with unfamiliar others who are structurally similar (i.e., come from the same network community) or dissimilar (i.e., come from different network communities) can not only propel subsequent movement within structure but also shape the experience of social belonging in the organization.

A second contribution to social networks research speaks to the tradeoffs of occupying network positions characterized by cohesion versus range (Reagans and McEvily, 2003; Reagans et al., 2004; Tortoriello et al., 2012). Network cohesion, or the degree to which a focal actor's contacts are highly interconnected, supports the emergence of shared norms and trust that can facilitate flows of information and social support (Coleman, 1988, 1994; Jan Piskorski and Gorbatâi, 2017). Meanwhile, network range, or the breadth of a focal actor's contacts, provides access to novel or at least non-redundant information to task-relevant knowledge and mentorship (Reagans and McEvily, 2003; Reagans et al., 2004; Lutter, 2015). Moreover, range may be particularly useful in organizational settings like ours that allow individuals to span standard organizational boundaries like departments or levels of managerial hierarchy. Prior studies on network closure and range have examined the consequences of occupying such positions for extended time periods (typically on the order of months or even years). In contrast, we demonstrate that even brief, one-time exposure to unfamiliar colleagues whose network positions can be characterized as closed (i.e., they come from the same network community) or open (i.e., they come from different network communities) relative to the focal actor can influence the flows of social resources and movement within social structure. We also present evidence that this movement in social structure is not a mechanical byproduct of the ties formed through direct contact with peers; rather, it came about through other mechanisms that have yet to be explored (e.g., referrals made by learning circle peers or the focal actor's motivation to break out of her social comfort zone following a positive experience with a divergent group). This finding opens the door to identifying other brief network-exposure interventions that matter for performance or inequality outcomes.

Next, this study contributes to research on social belonging in the workplace. Although the need to belong has long been recognized as fundamental and pervasive (Baum and Singh, 1994; Baumeister and Leary, 1995; Walton and Cohen, 2011), and organizational leaders have spoken extensively about and invested heavily in programs to promote diversity, equity, inclusion, and belonging (Kalev et al., 2006; Dobbin and Kalev, 2022), remarkably little is known about how to systematically influence the experience of belonging in the workplace. The interventions that have been tried to date have largely focused on changing individuals' mindsets (Mobasseri et al., 2021) and have had, to our knowledge, limited success. We offer an alternative, network-based approach to fostering the sense of belonging. In a sense, one can think about our approach through the lens of a construct that is a close cousin of social belonging: inclusion in the workplace. Prior research has conceptualized inclusion as a set of *perceptions* about the workplace—for example, the degree to which it has fair employment practices, appreciates and integrates diverse employee perspectives and viewpoints, and involves a broad range of actors in organizational decision making (Nishii, 2013). In contrast, our intervention can be thought of as a form of *structural inclusion*—that is, helping people "re-wire" their networks in ways that enable them to more deeply tap familiar and comfortable social resources (through convergent exposure), as well as novel but perhaps uncomfortable ones (though divergent exposure). In other words, one way for organizational leaders to promote the subjective sense of social belonging in their workplace is by tackling the objective barriers to structural, rather than just perceptual, inclusion.

In addition, this study has important implications for research on network-based interventions in organizations (Burt and Ronchi, 2007; Srivastava, 2015b; De Vaan and Wang, 2020; Kuwabara et al., 2020; Kneeland and Kleinbaum, 2023; Carnabuci and Quintane, 2023). Insofar as these studies have used random or quasi-random assignment procedures, they have done so at the individual level, without regard for the pre-existing network structure in which individuals are embedded. So, for example, the treatment group might consist of individuals who are selected to participate in a training program (Burt and Ronchi, 2007) or in a mentorship program (Srivastava, 2015b) or in an offsite event (Kneeland and Kleinbaum, 2023). Our intervention introduces three such innovations to such an approach. First, we use an amplified-asking approach (Salganik, 2019) to link ground-truth network tie surveys to longitudinal digital trace data for the entire organization. Second, we mine an organization's communication archives to locate people within the pre-existing network structure. Third, we use this information to construct groups that vary systematically on a network-structural dimension. Indeed, the group assignment algorithm we developed could be readily applied to the context of creativity and innovation—for example, constructing divergent groups when the task at hand involves exploration or idea generation and convergent groups when it instead involves exploitation and efficient execution (March, 1991; Lazer and Friedman, 2007; Lix et al., 2022). Given the ubiquity of digital trace data in modern organizations and the ease with which such data can be analyzed using computational methods (Lazer et al., 2009; Goldberg and Srivastava, 2017; Edelmann et al., 2020), we see great potential in future research that takes a similar approach but extends it to other types of network-based group interventions (e.g., connecting people on the periphery of a network to those who are more central as a means to reducing network-based inequality).

Although we are not the first to apply an amplified-asking approach to integrating survey and digital trace data (Salganik, 2019; Lu et al., 2024), the approach we implement here represents a methodological advancement because we take advantage of multiple waves of survey data. Prior work assumed but was unable to verify that the relationship between linguistic characteristics and attitudes expressed on a survey is stable over time. In contrast, our design explicitly tests this assumption by training and validating the model across three waves of survey data that correspond with contemporaneous digital trace observations. This temporal structure allows us to train on earlier data and evaluate predictive performance on subsequent data. The resulting model is, by construction, attuned and robust to the facets of the relationship that are relatively stable over time.

Finally, although the evidence is not causal, it is noteworthy that the overall structure of the organizational network shifted following a brief, one-time intervention that involved only about a quarter the employee population. If future research designs can pin down a causal relationship between interventions such as the one we implemented and greater integration across different parts of an organizational network, it might point to a new set of levers that organizational leaders can pull to break down the structural boundaries that inhibit effective cross-unit coordination and collaboration.

Limitations and Directions for Future Research

We acknowledge that this study has certain limitations, which also point to avenues for future research. First, although the study was made available to all employees, only a subset chose to participate. Although these participants were randomly assigned to experimental conditions, it is unclear the extent to which the intervention might generalize to the organization as a whole or to other organizations. For example, it is possible that those who self-selected into the study were already favorably inclined to engage with new colleagues and broaden their networks. They may also have been on an upward trajectory with respect to feelings of social belonging. The construction of a synthetic peer group whose social belonging survey responses are imputed based on linguistic features of their electronic communications helps to alleviate these concerns. However, we acknowledge that our coarsened exact matching technique is based just on the attributes of individuals that we can observe in our data. It would be useful in future research to replicate this design in a larger sample such that a true control group (to which participants are randomly assigned) could also be included.

Second, replications of this study in larger employee populations, including ones in which people work in hybrid rather than all remote mode, would also allow for the exploration of heterogeneous treatment effects and surface potential scope conditions of the intervention. For example, we anticipated in our pre-registration that employees from historically marginalized groups might benefit more from divergent exposure, while employees from majority groups might benefit more from convergent exposure. Unfortunately, the present study's sample size was not large enough to investigate these possibilities. Similarly, it is unclear how effective the intervention would be in settings where colleagues have the opportunity to meet each other in a physical office location—including the proverbial water cooler. We conjecture that the effects we document here might attenuate somewhat in such settings because of cross-contamination between the two experimental groups.

Third, although we assessed one mechanism—feelings of group solidarity—by which convergent exposure may fuel network change and social belonging, we generally do not observe the full range of mechanisms through which the two forms of exposure produced their respective effects. It would be useful in subsequent research to administer additional network surveys to, for example, track network referrals that may have taken place between learning circle peers and to conduct supplemental qualitative interviews with participants.

Finally, we acknowledge that the intervention we designed is both logistically complicated (e.g., requiring the collection of communications metadata to identify network communities and construct peer learning circles) and costly (e.g., requiring the licensing of learning and development content and the hiring of professional facilitators). We suspect that there are ways of designing convergent and divergent exposure experiences that are meaningful but less costly (e.g., scheduling regular group lunches with random subsets of employees from the same versus different network communities). Identifying *de minimus* forms of convergent and divergent exposure that still produce comparable effects may be the key to scaling such an intervention to larger organizational settings.

Conclusion

The need to belong, including to one's work organization, is fundamental to the human experience. Yet the sense of social belonging is rooted not only in psychological dispositions and mindsets but also in the social-structural position one occupies and the resources to which it affords access. Moreover, even brief but meaningful exposure to unfamiliar others can help people shift their social-structural positions. Two distinct forms of exposure appear to provide complementary benefits: Convergent exposure promotes feelings of solidarity, enmeshes people in the comfort of similar others, and promotes feelings of belonging; meanwhile, divergent exposure may produce some initial unease but ultimately allows people to move to positions of greater network advantage. Building organizations in which people not only feel that they belong but also thrive in their careers may ultimately require finding creative ways to provide them with access to both the experience of social convergence and of divergence.

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Tables and Figures

Table 1: Comparison of Treated Population to Overall Employee Population on Key Demographic Variables

	Treated Sample	Population	P-value
Minority-Group Membership	0.519	0.569	0.137
Female Proportion	0.757	0.748	0.977
Age	37.07	36.52	0.263
Current Tenure (days)	2,719	2,518	0.081
Management Level	0.085	0.093	0.623

Table 2: Descriptive Statistics and Correlations

Variables	Mean	SD	1	2	3	4	5	6	7
1. Solidarity	5.61	1.13	1.00						
2. Belonging	5.93	0.92	0.14	1.00					
			(0.15)						
3. Learning Circle Tie Persistence	0.04	0.20	-0.15	0.07	1.00				
			(0.03)	(0.47)					
4. Constraint	-0.03	0.03	-0.05	-0.10	-0.01	1.00			
			(0.50)	(0.31)	(0.89)				
5. Betweenness	57.04	2489.87	0.11	0.02	-0.12	-0.21	1.00		
			(0.11)	(0.84)	(0.08)	(0.00)			
6. Closeness	0.03	0.03	-0.05	0.09	0.01	-0.60	0.16	1.00	
			(0.45)	(0.37)	(0.90)	(0.00)	(0.02)		
7. Convergent Condition	0.48	0.50	0.17	0.23	0.22	0.16	-0.12	-0.15	1.00
			(0.01)	(0.01)	(0.00)	(0.02)	(0.07)	(0.03)	

Note: P-values in parentheses.

Table 3: Balance Checks of Random Assignment Procedure and Manipulation Check of Experimental Condition Assignment

	Convergent Condition	Divergent Condition	P-value
Minority-Group Membership	0.516	0.516	1.000
Female Proportion	0.774	0.780	0.893
Age	37.42	36.77	0.439
Current Tenure (days)	2,264	2,223	0.828
Baseline Belonging	5.92	5.79	0.256
Manipulation Check			
Percent in-community	77.89	30.91	< 0.001

Table 4: Descriptive Statistics of Slack Direct Message Data Used in Phase 2 Network Analyses.

	Wave 1	Wave 2	Full Period
No. of messages	1,997,939	2,157,191	3,323,446
No. of distinct authors	1,232	1,229	1,289
Average length of messages (words)	20.7	21.0	20.8

Table 5: χ^2 Contingency Tests of the Independence of Slack Network Communities and Demographic and Organizational Characteristics.

Attribute	χ^2 Stat.	p-value	DoF
Minority Group Membership	18.5	0.19	14
Female	15.7	0.33	14
Age	75.8	< 0.01	42
Compensation Grade	254.8	0.02	210
Management Level	110.5	< 0.01	56
Department	507.8	< 0.01	98

Table 6: Standardized OLS Regressions of Group Solidarity and Social Belonging on Treatment Condition. Model (3) uses imputed belonging to account for attrition in the sample of treated individuals. Model (4) uses coarsened exact matching to compare each treatment condition to a synthetic control.

		ΔBelonging		
Dependent Variables:	Solidarity	Survey	Attrited	CEM
Model:	(1)	(2)	(3)	(4)
Variables				
Convergent Condition	0.327*	0.268*	0.278*	0.498*
	(0.180)	(0.154)	(0.135)	(0.245)
Divergent Condition				-0.234
				(0.230)
Fixed-effects				
Wave	Yes	Yes	Yes	Yes
Strata				Yes
Fit statistics				
Observations	213	118	211	485
R^2	0.028	0.021	0.030	0.290

Clustered standard errors are shown in parentheses
Circle clusters in (1)-(3); Strata clusters in (4)
(1), (2): One-tailed tests (pre-registered); (3), (4): Two-tailed tests
*Note: ***: 0.001, **: 0.01, *: 0.05*

Of the 213 treated individuals who participated in a learning circle and completed the learning circle survey, two had no Slack direct message activity during their pre- or post-treatment periods, yielding a final sample of $N = 211$.

Table 7: Standardized OLS Regressions of Network Outcomes on Treatment Condition.

	OLS				Neg. Bin.
Dependent Variables:	Learning Circle Tie Persistence	ΔConstraint	ΔCloseness	ΔBetweenness	Learning Circle Tie Persistence
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Convergent Condition	0.567***	0.286*	-0.244**	-0.249*	1.874**
	(0.164)	(0.130)	(0.091)	(0.114)	(0.727)
Fixed-effects					
Wave	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	211	211	211	211	211
R^2	0.091	0.250	0.570	0.033	
Pseudo R^2					0.118

Clustered (Circle) standard-errors in parentheses
*Note: ***: 0.001, **: 0.01, *: 0.05*

Of the 213 treated individuals who participated in a learning circle and completed the learning circle survey, two had no Slack direct message activity during their pre- or post-treatment periods, yielding a final sample of $N = 211$.

Table 8: Standardized OLS Regressions of Network Outcomes on Treatment Condition—Omitting Learning Circle Ties.

Dependent Variables:	ΔConstraint	ΔCloseness	ΔBetweenness
Model:	(1)	(2)	(3)
Variables			
Convergent Condition	0.284*	-0.249**	-0.252*
	(0.129)	(0.090)	(0.114)
Fixed-effects			
Wave	Yes	Yes	Yes
Fit statistics			
Observations	211	211	211
R^2	0.250	0.572	0.033

Clustered (Circle) standard-errors in parentheses
*Note: ***: 0.001, **: 0.01, *: 0.05*

Of the 213 treated individuals who participated in a learning circle and completed the learning circle survey, two had no Slack direct message activity during their pre- or post-treatment periods, yielding a final sample of $N = 211$.

Table 9: Comparison of Slack Network Communities for the Full Organization Pre- and Post-Intervention.

	Pre-Intervention (Mean)	Post-Intervention (Mean)	T-Stat.	p-value
Constraint	0.16	0.13	3.79	< 0.01
Betweenness	2348.62	2094.76	1.47	0.14
Closeness	0.34	0.37	-19.07	< 0.01
Eigenvector Centr.	0.11	0.14	-4.89	< 0.01
Local Clustering	0.40	0.38	1.55	0.12
Global Reach	0.09	0.14	-7.58	< 0.01
High Belongingness (> 6)	0.87	0.90	-2.27	0.02
	Pre-Intervention	Post-Intervention		
Density	0.015	0.019		
Diameter	7.00	6.00		
Global Clustering	0.23	0.24		

The top block of the table presents means of individual-level network measures for the pre- and post-intervention periods, along with t-statistics and associated p-values of a test of the difference in means between the two periods. The bottom block of the table presents network-level measures for the pre- and post-intervention periods.

Figure 1: Study Timeline and Response Rates

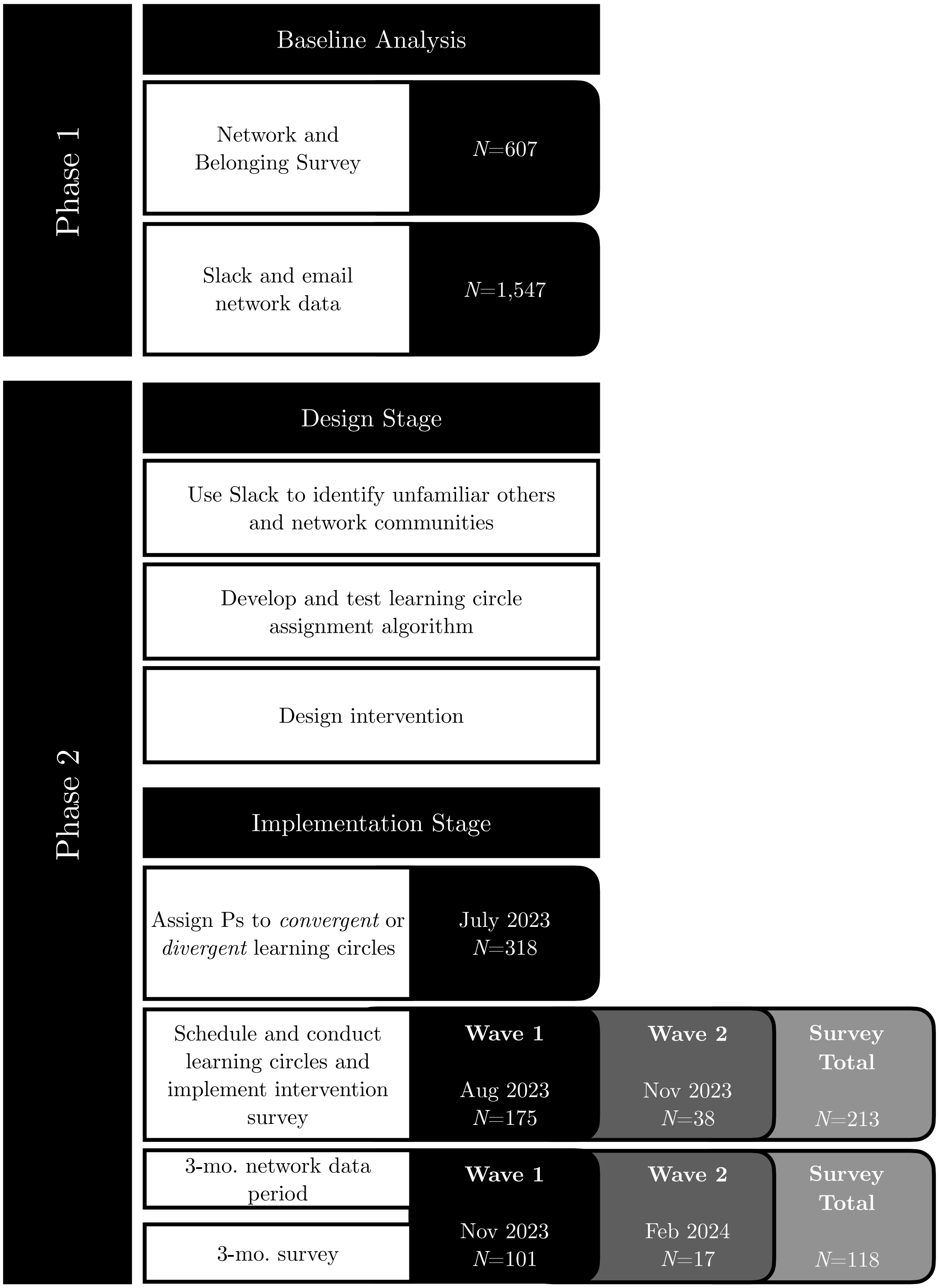
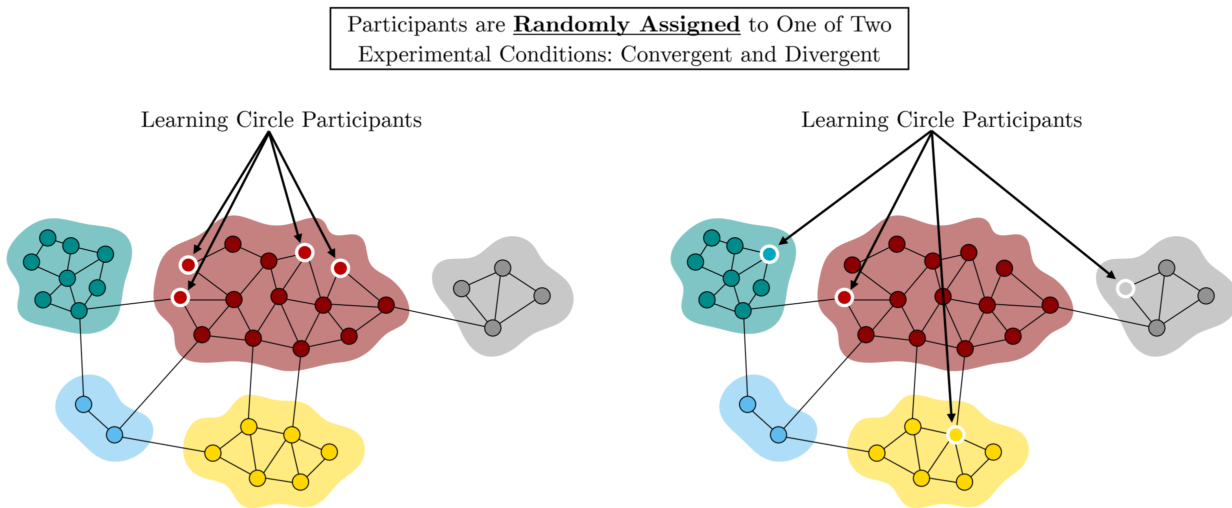


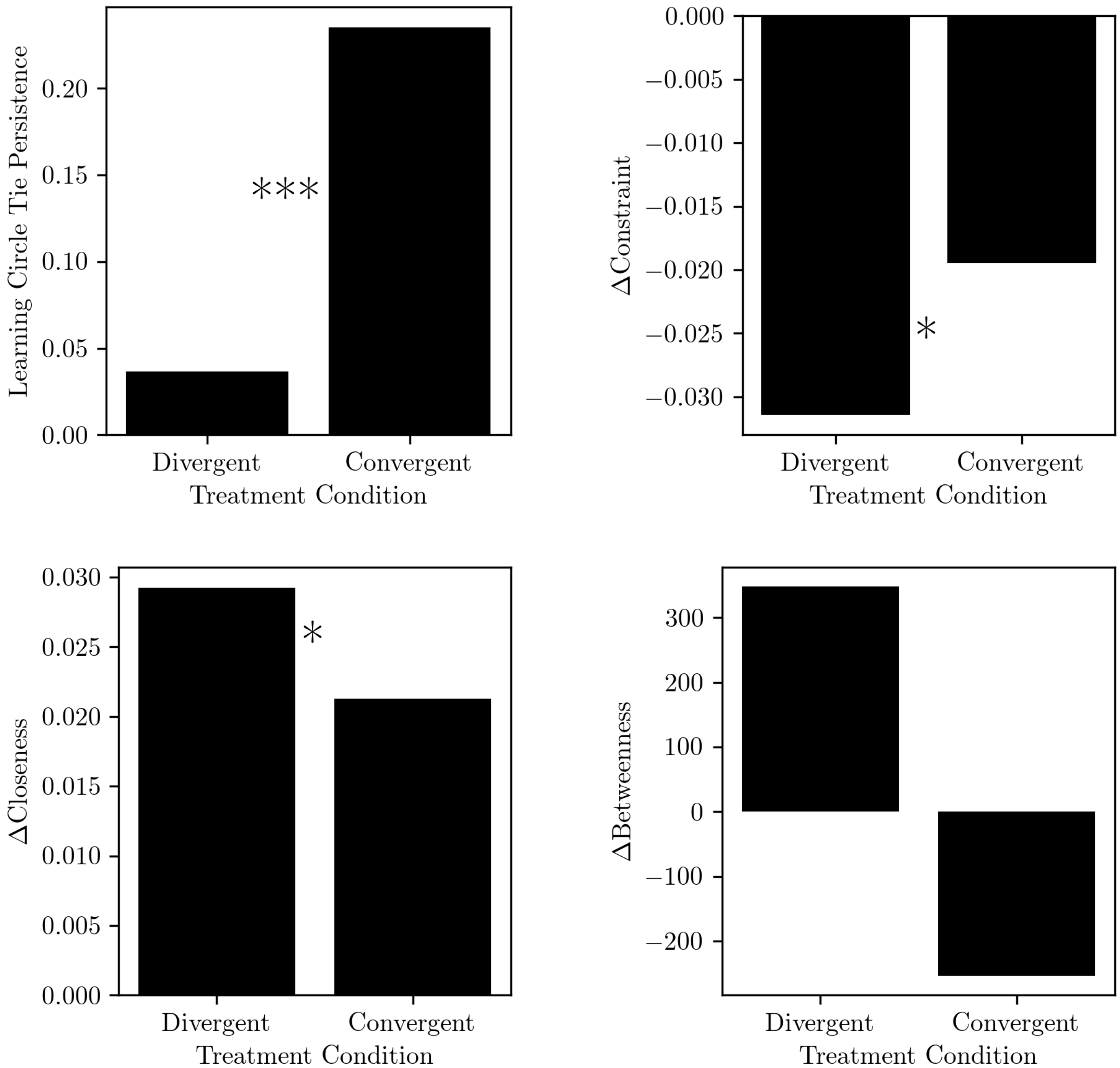
Figure 2: Schematic Representation of the Two Experimental Conditions



Convergent Condition: Learning Circle Participants (Mostly) Belong to the **Same** Community

Divergent Condition: Learning Circle Participants (Mostly) Belong to **Different** Communities

Figure 3: Changes in Network Outcomes by Convergent and Divergent Treatment Conditions. Each plot shows means by treatment condition and t-tests of the difference in means between treatment conditions.



*Note: ***: 0.001, **: 0.01, *: 0.05*

Appendices

A Imputation of Belonging Responses Based on LIWC Features

This appendix details our approach for imputing individuals' social belonging survey responses based on linguistic features of their Slack communications. This approach is intended to help us deal with two empirical challenges common in many experimental studies, including ours: attrition and the absence of a clear counterfactual when sample size restrictions render it infeasible to construct a random control group. By extracting signals of belongingness from naturally occurring communication, we generate a measure that is less sensitive to potentially non-random attrition and can thus be estimated even for individuals who do not complete follow-up surveys. Moreover, language-based measures allow us to benchmark our sample against a pseudo-control of organizational members who did not opt in to the experiment. In doing so, we not only hedge against attrition which may be biased by the experimental conditions but also enhance the generalizability of our findings beyond the experimental cohort.

Additionally, our approach leverages the fact that we have three waves of belongingness surveys. As a result, we can not only account for variation within a given period but also forecast belongingness in a future period. In other words, we can develop a model that identifies how the linguistic features of Slack communications relate to belongingness in a manner that is robust to temporal variation in this relationship.

Our imputation approach consists of five major steps, illustrated conceptually in Figure A.1. At a high level, we (1) transform Slack messages into usable features; (2) dichotomize the Likert-scale measures of belongingness into high/low belongingness; (3) fit a grandient boosting classification model based on the Slack message features and survey belongingness; (4) evaluate the out-of-sample performance of the model based on future period Slack messages and survey belongingness' and finally (5) impute measures of belongingness for those without a completed survey in a given period.

——————— INSERT FIGURE A.1 ABOUT HERE ———————

First, we transform the Slack direct message data into usable features for input into the gradient boosting classification model. Specifically, we convert each of the 14,815,272 messages into their corresponding Linguistic Inquiry and Word Count (LIWC) feature representations. LIWC is a psycholinguistic dictionary that maps words to psychologically meaningful categories—such as affect, social orientation, cognition, and identity—that have been validated across a wide range of behavioral and organizational studies. In addition to these measures, we also create cluster-based features using k-means clustering, grouping commonly occurring similar linguistic features within each period. Each message's distance to the cluster centroids and cluster membership probabilities are then used as additional comment-level features, enabling us to capture higher-order similarities in language use that may reflect shared patterns of belonging or disengagement. Next, we apply a series of transformations and aggregations to the LIWC-derived features to move from the message level to the individual level of analysis within each time period. This is the level that corresponds to the level of our social belonging measure. For each user and period, we compute summary statistics such as the mean, median, variance, and skew of each LIWC category, capturing both the central tendency and variability of linguistic expressions over time. These aggregated linguistic profiles provide a temporally bounded yet comprehensive representation of each participant's communication style in given period. This in turn serves as the input for the gradient boosting model that we use to predict belongingness across survey waves.

The second step is to map our Likert-scale survey measure of social belonging to a dichotomous measure. The original belonging measure is ordinal rather than continuous, so the scale includes too many discrete categories for an efficient categorical prediction task. This, in turn, leads to sparsity problems and unstable estimates across response levels. Similarly, the data are not sufficiently continuous to accommodate continuous model evaluation metrics. By recoding the measure into a binary outcome (high versus low belonging), we simplify the prediction problem, increase model interpretability, and align the empirical task with our theoretical interest in distinguishing those who feel integrated from those who do not. We set the cutoff at the median (6 on the Likert scale).

We chose this cutoff for several reasons. First, as shown in Figure A.2, the empirical distribution of social belonging exhibits a discernible gap between the values of 5 and 6, suggesting a natural dividing line in the data. The value 6 also corresponds to the mode of the distribution, while responses below 6 form a smoother, more continuous decline. Moreover, since the median value is 6, this threshold produces a relatively balanced split between low and high social belonging across the sample. However, because many respondents cluster at the median value, the resulting classes are not perfectly balanced: There is still a modest over-representation of one class. Consequently, in the evaluation section below, we rely on performance metrics that are robust to uneven class distributions to ensure that our model assessment remains reliable.

——————— INSERT FIGURE A.2 ABOUT HERE ———————

Third, we fit a gradient boosting classifier using Slack message features and the dichotomized, survey-based social belonging measures. Gradient boosting is a tree-based ensemble method that iteratively combines weak learners to minimize prediction error, making it particularly effective at capturing complex, nonlinear relationships between linguistic features and social belonging scores. We train the model on data from two survey waves and evaluate its predictive performance on the held-out wave to assess its temporal generalizability. To prevent overfitting, we tune key hyperparameters such as learning rate, maximum tree depth, and the number of estimators using grid search to avoid overfitting to the training data. The resulting model yields a probabilistic estimate of social belonging for each individual based on her communication patterns. We interpret this measure as a linguistic proxy for self-reported social belonging.

To estimate the performance of the gradient boosting classification model, we evaluate its predictive accuracy on a held-out test set from the final survey wave using performance metrics such as F1-score, precision, and recall. We adopt this temporal holdout approach to assess the model's ability to generalize across time rather than merely fit patterns specific to a single period. This design provides a more stringent test of predictive validity, as linguistic patterns and communication volumes can shift over the course of the study. To account for class imbalance in the dichotomized social belonging measure, we focus on the F-1 score, recall, and precision rather than simple accuracy (which we also report), ensuring that the model's performance reflects its performance across both high- and low-belonging cases. We consider the F1-score to be the central model evaluation measure in our case because it remains robust to uneven class distributions. Accuracy alone would overstate performance in our data, as the model could trivially achieve high accuracy by predicting the more frequent class. In contrast, the F1-score emphasizes the model's ability to correctly identify both high- and low-belonging cases. We report all of these temporal out-of-sample measures in Table A.1, highlighting that we are able to achieve an out-of-sample F1-score of 0.85. Taken together, these evaluation procedures allow us to assess not only how well the model predicts social belonging at a single point in time but also whether the underlying linguistic signals of social belonging remain stable across survey periods.

——————— INSERT TABLE A.1 ABOUT HERE ———————

Additionally, we also present the area under the precision–recall curve (AUPRC) in Figure A.3 and the area under the receiver operating characteristic curve (ROC AUC) in Figure A.4. These metrics provide complementary views of model performance beyond threshold-dependent measures such as accuracy or F1-score. The ROC AUC summarizes the trade-off between the true positive rate and the false positive rate across all possible classification thresholds, offering a measure of how well the model distinguishes between high- and low-social belonging individuals—independent of a specific cutoff we might apply. In contrast, the AUPRC focuses on the trade-off between precision and recall and is particularly informative in the presence of class imbalance (which is important in our case), where high ROC AUC values may overstate performance. By examining both curves, we can assess not only whether the model ranks cases correctly but also how well it identifies true cases of high social belonging under varying decision thresholds. Together, these metrics provide a more complete and robust evaluation of the classifier's discriminative ability, similarly suggesting strong out-of-sample predictive performance.

——————— INSERT FIGURE A.3 ABOUT HERE ———————

——————— INSERT FIGURE A.4 ABOUT HERE ———————

In our fifth and final step, we impute measures of predicted social belonging for those without a completed survey in a given period. Using the fitted gradient boosting classifier, we generate predicted probabilities of high social belonging based on each individual's linguistic features from Slack messages. These predictions serve as imputed social belonging scores for participants who did not respond to the survey, allowing us to recover data that would otherwise be lost due to attrition. By leveraging the model's learned relationship between language use and self-reported social belonging, we are able to extend our measure to the full set of active users, including those who disengaged from survey participation but remained active on the organization's communication platform. This approach mitigates potential bias introduced by selective non-response and provides a more complete, temporally robust window into social belonging across the population of study participants.

Figure A.1: Concpetual Diagram Illustrating the Imputation Procedure

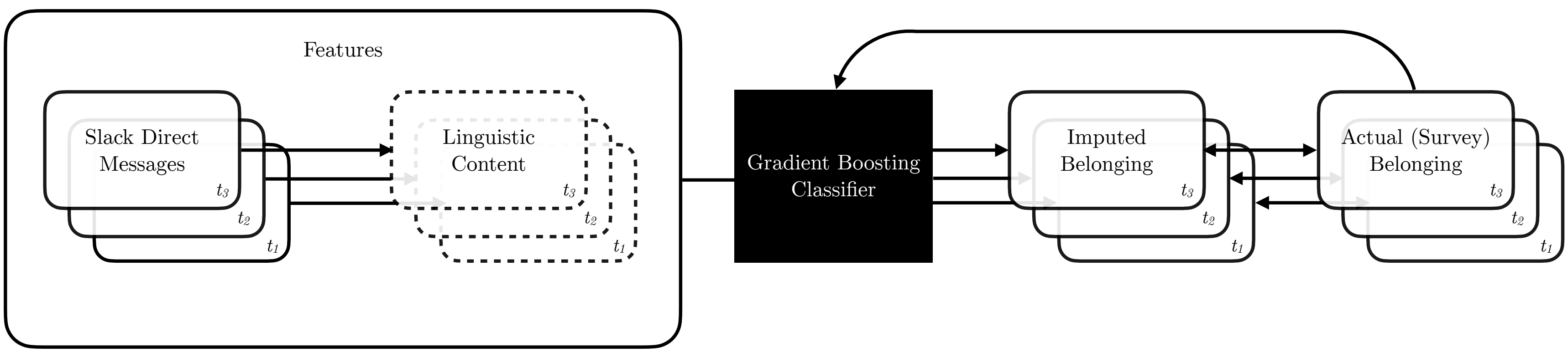


Figure A.2: Social Belonging Score Histogram, Which Motivates the Choice to Set the High-Belonging Cutoff at 6 (out of 7)

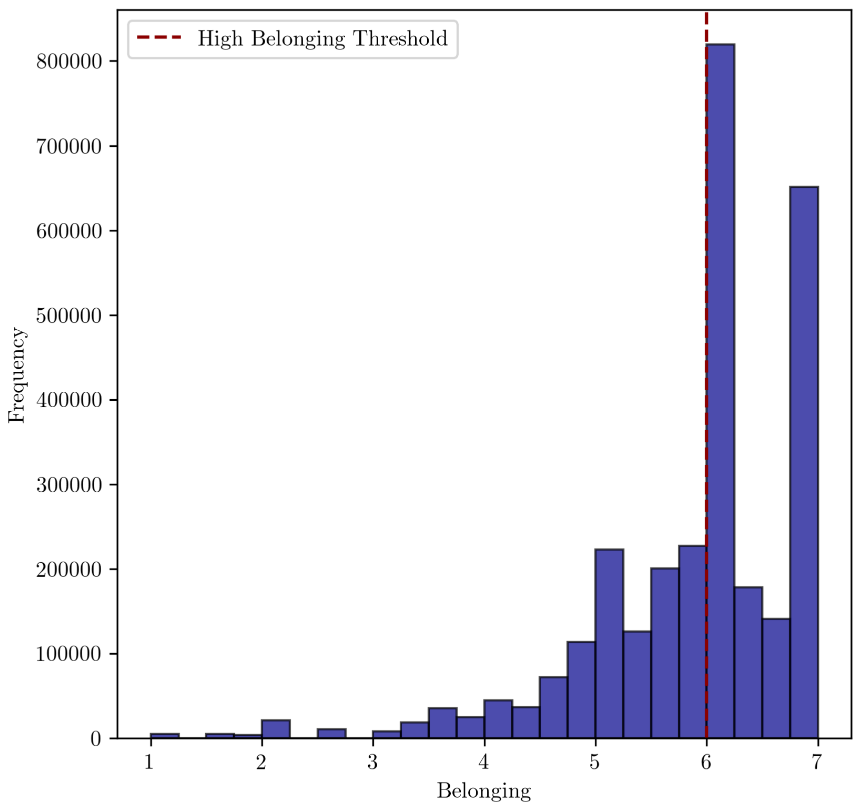


Table A.1: Temporal Out-of-Sample (Future) Model Evaluation Metrics

Metric	Score
F-1 Score	0.85
Recall	0.92
Precision	0.79
Accuracy	0.78
AUPRC	0.75

Figure A.3: Temporal Out-of-Sample Precision-Recall Curve

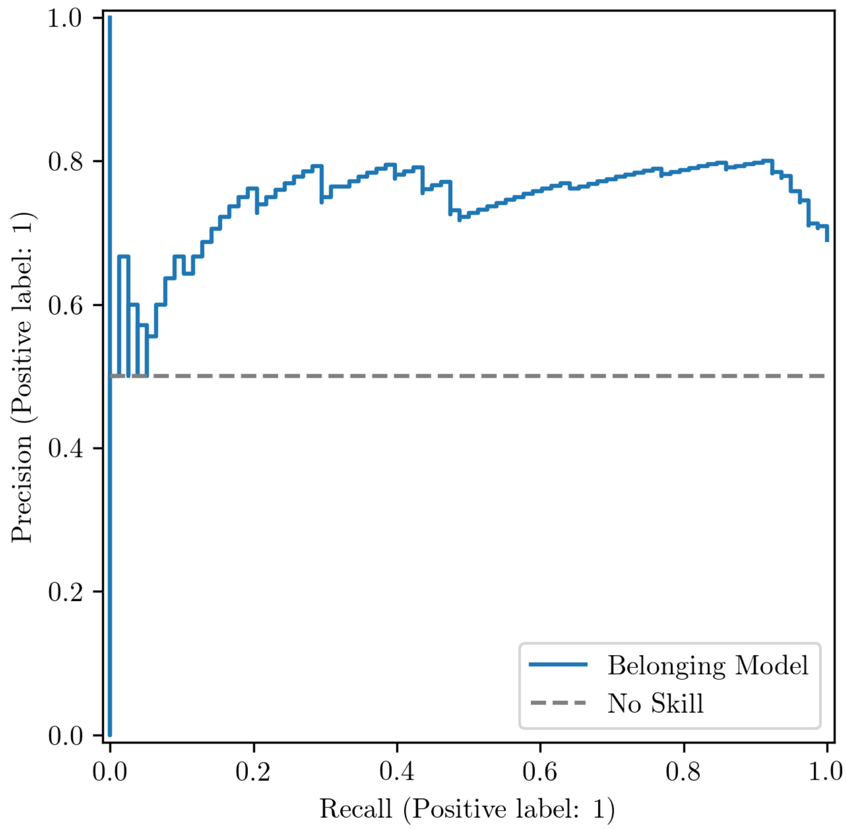
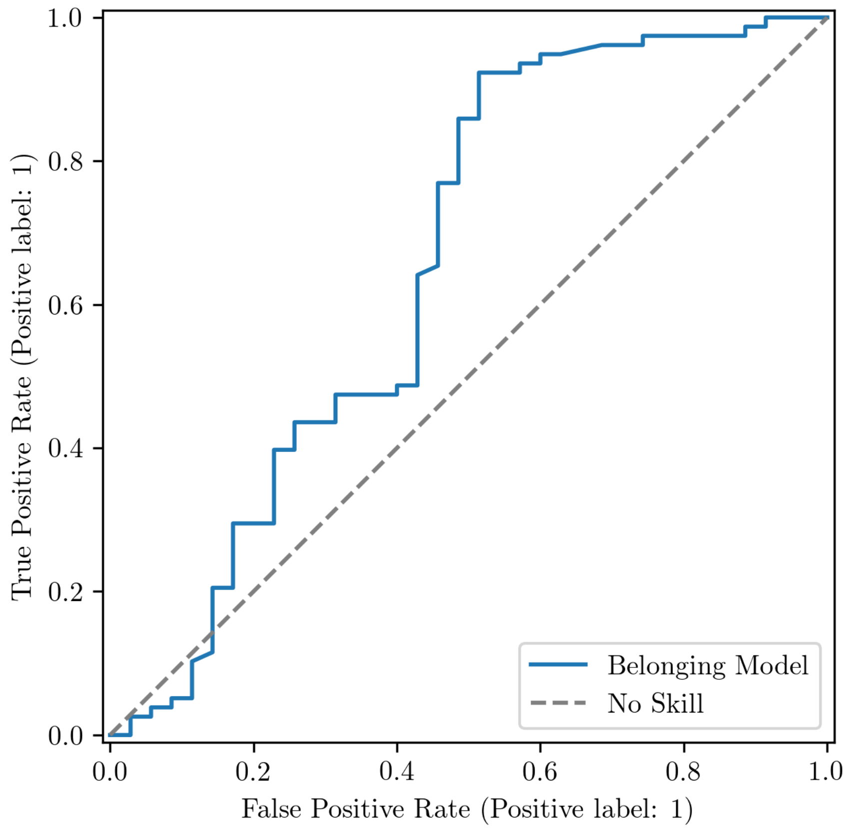


Figure A.4: Temporal Out-of-Sample ROC Curve



B Coarsened Exact Matching Analysis

To construct a credible comparison group for estimating the effects of the intervention, we employ coarsened exact matching (CEM) to create a synthetic control sample that is observationally similar to the treatment group on key pre-intervention covariates. CEM is a nonparametric matching procedure that coarsens continuous variables into substantively meaningful bins, performs exact matching within those coarsened strata, and then discards unmatched observations (Iacus et al., 2012). This approach improves covariate balance between treated and control units while minimizing model dependence in subsequent estimation. Unlike propensity score matching, which seeks balance through probabilistic weighting, CEM achieves deterministic balance by design, ensuring that any remaining differences between the treatment and control groups are minimized before outcome estimation.

In our application, we match participants in each treatment condition to non-treated individuals using demographic and pre-treatment covariates measured prior to the intervention, including age, gender, compensation grade, management level, minority group membership, and baseline belonging. Each variable is coarsened into bins that preserve sufficient variation while ensuring a reasonable number of matched observations. Individuals from the control pool who fall outside the coarsened strata of any treatment participant are excluded from the matched comparison. The resulting matched sample serves as a synthetic control group—a set of non-treated participants who are statistically similar to treated participants on all observed pre-intervention characteristics.

After matching, we assess the quality of covariate balance using standardized mean differences and visual diagnostics to verify that the treated and synthetic control groups are comparable across all matching variables. Figure B.1 displays the standardized mean differences for each covariate in the matched sample relative to the overall organizational population. The results indicate that the matching procedure substantially reduced pre-treatment differences between the treated participants and the broader population, yielding a more comparable control group. We then re-estimate the treatment effects using this matched sample, weighting observations by their CEM-derived strata weights. This approach enables us to estimate the causal effect of the intervention on belonging relative to a synthetic control group while mitigating bias arising from pre-existing differences in observable characteristics.

Figure B.1: CEM: Standardized Mean Differences between the Unadjusted and Adjusted Samples. An asterisk next to a variable denotes no difference in means between the treated and the adjusted matched sample.