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Can Brief Interventions Improve Functioning in Engineering Student Dyads?

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Poor interpersonal dynamics can hinder collaboration, but engineering educators have failed to address this problem. Short interactive exercises may ameliorate such problems. We introduced an introductory lecture and interactive exercises into engineering classes to evaluate their effects on interpersonal outcomes in student dyads. Two large-sample quantitative studies and one small qualitative study were conducted ($N = 227$) to evaluate the exercises. Although the qualitative results (Study 2) indicated mixed effects, we found no evidence in the large-sample studies that the intervention improved any outcomes. The results suggest that cohesion and similar factors are enhanced through collaboration, and short exercises do not cause any further enhancement. Intensive long-term interventions may be necessary to produce stronger effects than acquaintance.

Keywords: teamwork, problem-based learning, collaborative learning, cohesion, perceived partner responsiveness, psychological safety, educational psychology

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Introduction

Engineering instructors have made little progress in educating students to be responsive and respectful toward one another. Engineering classes are system-centered rather than human-centered: instructors mainly educate students on how to apply principles from physics and chemistry to problems of device design and improvement (Beder, 1999). Although real engineers are often placed into teams, there is only sporadic attention to the interpersonal domain in engineering classes (Joyner et al., 2012), and engineering professionals in industry perceive college graduates to be weak in job-relevant communication skills (Donnell et al., 2011). Because of this deficit, engineering educators have called for programmatic research on the development of personal and social skills (Martinez-Mediano & Lord, 2012; Palero Aleman et al., 2021).

Though contentious, research shows that engineering also attracts people who are more talented at systemizing and less talented at empathizing (Baron-Cohen, 1998). It also attracts students who have a profile of strong quantitative but weak verbal ability, as opposed to both strong quantitative and strong verbal ability (Wang et al., 2013); and students who place greater value on mathematical and scientific tasks than altruistic tasks (Wang et al., 2015, 2017).

Furthermore, social prejudices can hinder engineering teams (Cech & Waidzunas, 2011; but see Loes et al., 2018). Team members can feel excluded due to visible characteristics like race and gender and hidden characteristics like political identity, religious affiliation, and sexual orientation (Cooper & Brownell, 2016; Eddy et al., 2015; Henning et al., 2019; Sullivan et al., 2018). Although several interventions have been tested to address belonging in the traditional classroom (e.g., Walton & Cohen, 2011), researchers have not studied interventions that improve team skills like listening and responsiveness, which can enhance belonging in small groups. Such skill training has typically been discussed in theoretical terms without empirical data (Leydens & Lucena, 2009; Seat & Lord, 1999) or without a control condition (Norkunas, 2011). Although evaluations have been conducted in other fields (Boesen et al., 2009; Krueger et al., 2019), it is unclear whether these results are generalizable.

For these reasons, it is relevant to develop interventions that improve the interpersonal skills of engineering students. The goal of this article is to test an embedded intervention. By embedded, we mean the intervention is inserted into a course that does not focus on interpersonal skills. The reason for using an embedded intervention is that the engineering curriculum is already so extensive that it is not feasible to add a new course on interpersonal skills. To assess the effectiveness of the intervention, we focus on three outcomes that are related to interpersonal engagement, which is relevant because loss of engagement in teamwork causes numerous pedagogical problems (Marion & Thorley, 2016; Nokes-Malach et al., 2015; Pauli et al., 2008).

The first construct, perceived partner responsiveness, is the sense that one's partner is attentive and concerned with one's personhood and welfare (Reis et al., 2011; Reis & Carmichael, 2006). Perceived partner responsiveness can increase intellectual humility and openness to novel ideas (Reis et al., 2018). Intellectual humility enables team members to learn from others (Porter & Schumann, 2018; Reis et al., 2018), acquire knowledge (Krumrei-Mancuso et al., 2020), and react non-defensively to failure (Caprariello & Reis, 2011).

The second construct, psychological safety, is the sense that teammates can take interpersonal risks such as suggesting unconventional ideas and criticizing others' ideas (Edmondson, 1999). In psychologically safe teams, people feel like they can take these risks without consequences like status loss, demotion, and removal. Field studies of firms suggest that psychological safe teams engage in learning behavior, which encompasses help seeking, collaborative error fixing, and experimenting (Edmondson, 1999). Without such behavior, people tend to discuss common knowledge (Lu et al., 2012). When teams resist that tendency and instead engage in learning behavior, they improve their collective understanding of a situation and assess risks and errors more effectively (Edmondson, 1999). They also become better at learning from failure (Carmeli & Gittell, 2009). Some field research indicates that psychological safety has particular benefits for minority groups (Singh et al., 2013).

The third construct, relational cohesion, is a form of closeness that is relevant to collaborative dyads. Introduced in Study 3 here, relational cohesion (or perceived cohesion) is a sociological construct that describes the perception by members of a group that there is a force that pulls them together into a meaningful integrated team or "a distinct, unifying social object" (Lawler & Yoon, 1996, p. 94). Relational cohesion is thus distinct from the other factors. As with perceived partner responsiveness, it is definitionally subjective and must be measured through self-reports. All three constructs—responsiveness, psychological safety, and cohesion—have robust associations with positive outcomes, adduced through studies from different researchers and samples (Burlingame et al., 2018; Evans & Dion, 2012; Gable et al., 2012; Gadassi et al., 2016; Kim, 2018; Reis et al., 2014; Thye et al., 2002).

Overview of the Current Studies

For the current studies we modified the curriculum of a class where students work in dyads. In a normal 15-week semester, the course uses a combination of short lectures and long problem-solving studio sessions, which are periods where students sit at shared desks and collaborate to solve problems that are too difficult for one individual (Le Doux & Waller, 2016). In the studio sessions, students sit at four-person tables; students on each side of the table are paired in a dyad. Dyad members are bound together—they are assigned only one large paper pad and one marker for both to share, which engenders turn taking and mutual interdependence. Students are instructed to begin work in dyads but the dyads at any table can merge and start a four-person discussion if they wish. Many dyads choose not to start such larger discussions. Teaching assistants and instructors circulate around the classroom and advise groups who need help. Professors usually keep dyads

together for the entire semester, but they can make discretionary changes to dyad membership.

In Study 1, we added listening activities to the curriculum, adopted from an organizational behavior class taught by Avraham Kluger, a social psychologist who conducts research on listening. Dr. Kluger drew these exercises from improvisational theater (Spolin, 1986), the psychology literature, and other sources. For the remaining studies, we used an abbreviated set of these exercises combined with an introductory lecture or video. Our initial research questions were:

1. Will these interventions increase perceived partner responsiveness?
2. Will these interventions increase psychological safety?

In Study 2, we conducted a qualitative evaluation of a shorter set of exercises with a small sample. In Study 3, we returned to the original methodology and implemented a revised version of the intervention. We also added a measure of dyadic cohesion. Thus, our third research question was:

3. Will these interventions increase dyadic cohesion?

After Study 3, we conducted a replication, which showed no effect of the intervention. However, during this replication study, normal class activities were interrupted due to the COVID-19 pandemic and students only participated in remote classes thereafter. These results are hence unlikely to be generalizable and are not reported.

Study 1

Introduction

Responsiveness and interpersonal functioning can manifest in several ways. To set a manageable scope for our first intervention, we focused on listening as a trainable skill. Students who are indifferent to each other typically avoid listening to one another, whereas students who care about each other typically pay attention.

For Study 1, we adopted a set of ten listening exercises (see Appendix A) from a graduate-level course on listening (A. Kluger, personal communication, November 19, 2018). The exercises are focused on listening, one enactment of responsiveness, conducted under the assumption that learners adopt a growth mindset—listening is like a muscle that gets stronger with exercise and practice. These exercises were partially chosen based on convenience, given the set of possibilities, but the deployment of these activities in an existing listening class also suggested that they might cohere as an intervention.

The ten exercises were inserted into ten consecutive class sessions. Each exercise took approximately ten minutes of class time and typically occurred at either the beginning or the end of the class. After each activity, students wrote down one thing they had learned from the exercise. Unpublished results from the MBA class suggest that the exercises have a measurable effect.

Study Design

A waitlist-control design was use for the study: two sections of the course received the intervention in the first half of the semester (experimental) and two sections received it in the second half of the semester (waitlist control). We predicted both groups would benefit from it, but the growth trajectories would differ based on timing of intervention: the experimental group (early treatment) would show an early rise followed by a high plateau, and the control group (delayed treatment) would show a low plateau followed by a late rise.

Data were collected from participants in multiple waves, separated by fixed intervals. In the statistical results, the coefficient for each wave—coded as a set of dummy variables from Wave 2 onwards—indicates change relative to the first wave. There should be a positive interaction term between condition and wave in the early waves, indicating a faster rate of improvement in the experimental group (condition = 1) than the control group (condition = 0). There should be negative interaction term between condition and wave in the later waves, indicating a faster rate of improvement in the control group than the experimental group.

Method

Participants

The participants were 119 undergraduate students from 68 dyads. These undergraduates were students at Georgia Institute of Technology, enrolled in a first-year class on conservation principles in biomedical engineering. At the beginning of the semester, the instructors informed students they would receive extra credit if they filled, at minimum, a certain number of panel surveys. Students could alternatively write a short essay on the psychology of listening to receive credit.

Every student who participated was part of a dyad, but because participation was voluntary, some dyads only contributed one member to the study. We do not know if students who opted out of the study differed from students who opted in but only a small number opted out; we received data from 128 out of 143 students. We excluded data from nine participants who switched from one partner to another during the semester or whose group membership was unknown.

The gender composition of the sample was 32.8% male, 58.8% female, and 8.4% unknown gender. The racial and ethnic composition was 38.7% White, 4.2% Black, 35.3% Asian, 3.4% Asian–White, 1.7% Middle Eastern, 7.6% other races, and 9.2% unknown race or ethnicity. In all, 79.8% were domestic students, 11.8% were international students, and 8.4% had missing data on this question.

Materials

Demographic questions were modeled after items in the Healthy Minds 2015-16 Study, a survey of 34,217 students at 23 higher education institutions in the U.S. (Healthy Minds Network,

2016). The race and gender recoding follow Martin (2019). Gender was measured with a single item with six options: "male," "female," "trans male/trans man," "trans female/trans woman," "genderqueer/gender non-conforming," and "other." Answers were recoded into a three-level variable: 1 (male), 2 (female) and 3 (other). The "other" category included all options from "trans male/trans man" through "other" because only a small number of individuals (often nil) chose one of these options.

Race and ethnicity (henceforth race) were measured with a single question where students could select multiple options simultaneously. We coded a participant's race if they selected at least one option. A participant was coded as (a) White, Black, or Asian if they exclusively selected the corresponding option (b) Hispanic if they selected "Hispanic" and up to two other options, (c) Asian–White biracial if they exclusively selected "White" and "Asian," (d) Middle Eastern if they exclusively selected "Middle Eastern, Arab, or Arab American," (e) Other if they selected any other single option, e.g., "American Indian or Alaskan Native," "Native Hawaiian or Pacific Islander, "Other" or a set of options not specified above, and (f) Unknown if they selected none of the options. Race was entered into regressions as a categorical variable with White as the reference category.

We used multi-item scales to measure psychological constructs in each survey wave. In anchors, we used terms such as "true" and "accurate" rather than "agree" to mitigate acquiescence bias.

Perceived Partner Responsiveness. We created a scale using items from the perceived partner responsiveness scale, which has 18 items grouped under three categories: general items, understanding items, and validation items (Reis et al., 2011). Respondents answered using a five-point scale from 1 (*not at all true*) to 5 (*completely true*). To create the four-item scale, we selected both general items and one concise item from each other category, avoiding items that suggested friendship or intimacy. Respondents rated to what degree, in the past 7 days, "my partner has really listened to me," "been responsive to my needs," "understood me, and "valued my abilities and opinions." The internal consistency coefficients (Cronbach's α) from wave 1 through 6 were .90, .85, .93, .96, .96, and .95.

Psychological Safety. Psychological safety was measured using Edmondson's (1999) 7-item psychological safety. Items were modified for dyads as follows:

1. If one of us makes a mistake on this team, it is often held against that person.
2. Both my partner and I are able to bring up problems and tough issues.
3. My partner sometimes rejects me for being different.
4. It is safe to take risks on this two-person team.
5. It is difficult to ask my partner for help.
6. My partner would not deliberately act in a way that undermines my efforts.
7. Working with my partner, my unique skills and talents are valued and utilized.

Respondents answered on a 7-point scale from 1 (*very inaccurate*) to 7 (*very accurate*).

The internal consistency coefficients (Cronbach's α) from wave 1 through 6 were .70, .75, .80, .83, .81, and .82.

Procedure

The procedures for all studies were deemed exempt from review by an Institutional Review Board. We obtained informed consent from all participants. Data were collected using Qualtrics.

A researcher visited a course session to explain the study's purpose, distribute a consent form, and notify students about upcoming email invitations to surveys. Survey invitations for the initial long survey, which included demographic items, were sent shortly after the beginning of the semester. If a participant did not respond within two days, we sent them a reminder. Invitations for survey waves were sent out by email on Thursdays at 11 am to coincide with the completion of the week, a subjectively meaningful unit for participants. Initially, we asked students respond within 24 hours. Following a low response rate to the first two waves of Study 1, the deadline to complete the questionnaire was updated by six hours to 5pm on Friday. There was two-week gap between survey waves, except where a break or other contingency necessitated a three-week gap.

The study was conducted in a semester that began on January 7, 2019. Survey invitations for the initial long survey, which included demographic items, were sent on January 18. People who did not respond received two invitation reminders. The survey closed on January 21. In late April, we reopened the survey for a brief period so that five participants who had only filled out short survey waves could have another opportunity to fill out the long survey. The biweekly survey dates were January 24, February 7, February 21, March 7, March 28 (delay for spring break), and April 11. In the experimental group, the listening sessions were conducted from January 23 to March 1 (weeks 3-8). In the control group, the listening activities were conducted from February 27 to April 10 (weeks 8-14).

Data Analysis

Hierarchical linear models were used to initially analyze the data, with dyads at Level 3, persons at level 2, and waves at level 1. Although dyads were nested in sections, a fourth level was not added because there were just four sections (Nezlek, 2011). We verified that results did not change significantly after controlling for section. The initial model results indicated there was trivial variance at the dyad level. We therefore switched to a two-level model with persons at level 2 and waves at level 1:

Level 1 (Wave):

$$y_{jk} = \beta_{0k} + \beta_{1k} * Wave2 + \beta_{2k} * Wave3 + e_{jk} + (\rho * e_{j-1,k})$$

Level 2 (Person):

$$\beta_{0k} = \gamma_{00} + \gamma_{01} * Condition + \gamma_{02} * Asian + \gamma_{02} * Black + \cdots \gamma_{08} * Female + \gamma_{08} * OtherGender + u_{0k}$$

$$\beta_{1k} = \gamma_{10} + \gamma_{11} * Condition + u_{1k}$$

$$\beta_{2k} = \gamma_{20} + \gamma_{21} * Condition + u_{2k}$$

where *y* refers to the outcome at wave j of person k, *Wave2* and *Wave3* are dummy variables for the second and third waves, *Asian*, *Black*, etc. are dummy codes for race, *Female* and *OtherGender* are dummy codes for gender, *Condition* is a dummy code for experimental condition (0 = control; 1 = experimental); and ρ is the correlation between residuals from consecutive waves—the model has this autoregressive residual structure with lag 1 because consecutive waves were adjacent in time (see Hung & Wang, 2012). If the initial model failed to converge, slopes in later waves were fixed from the last wave forwards (i.e., Wave *n*, then Wave *n-1*, etc.) until convergence was obtained. Wave was treated as a categorical variable because we did not assume a linear functional form.

Even though we focused on within-person change, we entered two between-person covariates—race and gender—because these variables may correlate with non-responding. We verified that models that ignored race and gender produced somewhat different results.

Results

Table 1 contains descriptive statistics and correlations between the three dependent variables. Both means were above the midpoint of 3.5 and close to the scale maximum. The high mean for psychological safety could indicate that there was little adversarial conflict, such as undermining of the partner's work, which is measured only by the psychological safety scale—three of its items pertain to the perception that one's partner was engaging in such adversarial behavior. The standard deviations indicate there was moderate variance between persons, suggesting that individuals had varying experiences, and moderate variance within persons, suggesting that for many participants, there was change across time.

The strong correlation of .83 between perceived partner responsiveness and psychological safety suggests that the two scales may fail to measure distinct constructs in dyads when the perceived partner responsiveness scale is abbreviated. The correlation could also mean that the two constructs are themselves indistinct. However, the correlation could also indicate that psychological safety depends on responsiveness, or that the two factors have a reciprocal relationship.

The results of the models are in Table 2; the hypothesized interaction effects were not detected. The means, plotted in Figure 1, also show that the pattern of change did not align with our predicted pattern. The only discernible effect was that psychological safety was higher in wave 6 than wave 1 across both groups. Although this effect was only statistically significant in the control group, the trend suggests that improvement would have continued in both groups had the class lasted longer.

Discussion

The goal of Study 1 was to evaluate a ten-session intervention in which each session lasted approximately ten minutes. To have a manageable scope, the intervention focused on listening rather than diverse types of interpersonal functioning. The results suggest that the interventions had no discernible effect. In fact, contrary

Table 1. Descriptive Statistics and Correlation between Perceived Partner Responsiveness and Psychological Safety (Study 1)

	M	SD_o	SD_b	SD_w	PPR
PPR (1-5)	4.1	0.8	0.7	0.5	
Psych. Safety (1-7)	5.9	0.9	0.8	0.5	0.83***

Note. PPR = perceived partner responsiveness. o = overall. b = between. w = within. *** $p < .001$.

Table 2. Multilevel Models with Perceived Partner Responsiveness and Psychological Safety as Outcomes (Study 1)

	PPR		PS	
	B	*SE*	*B*	*SE*
Intercept	4.051***	(0.142)	6.030***	(0.168)
White				
Black	-0.714**	(0.266)	-0.576	(0.310)
Asian	-0.009	(0.122)	-0.382**	(0.143)
Asian-White	-0.302	(0.299)	-0.405	(0.348)
M.East.	-1.592***	(0.410)	-1.862***	(0.474)
Other	-0.026	(0.210)	-0.212	(0.245)
Unknown	-1.249*	(0.577)	-1.430*	(0.669)
Male				
Female	0.064	(0.117)	0.083	(0.136)
Wave 1				
Wave 2	0.043	(0.097)	0.033	(0.105)
Wave 3	0.080	(0.112)	0.218	(0.128)
Wave 4	0.023	(0.122)	0.067	(0.136)
Wave 5	0.128	(0.129)	0.111	(0.138)
Wave 6	0.285*	(0.125)	0.234	(0.138)
Control				
Experimental	0.046	(0.138)	-0.068	(0.164)
Wave 1 x Exp				
Wave 2 x Exp	-0.192	(0.125)	-0.064	(0.135)
Wave 3 x Exp	-0.274	(0.146)	-0.118	(0.166)
Wave 4 x Exp	-0.146	(0.158)	0.045	(0.178)
Wave 5 x Exp	-0.101	(0.168)	-0.051	(0.181)
Wave 6 x Exp	-0.159	(0.163)	-0.052	(0.180)
SD_{Person}	0.639	(0.591)	0.497	
$SD_{Residual}$	0.240	(1.571)	0.644	
Random Slopes				
SD_{Wave2}	0.499	(1.487)	0.057	
SD_{Wave3}	0.628	(1.200)	0.196	
SD_{Wave4}	0.704	(1.072)	0.220	
SD_{Wave5}	0.744	(1.014)		
SD_{Wave6}	0.733	(1.029)		
ρ	0.015		0.510	
N	585		584	

Note. PPR = perceived partner responsiveness. PS = psychological safety. ρ = correlations between residuals of consecutive waves. * $p < .05$, ** $p < .01$, *** $p < .001$. Blank coefficient cells indicate reference categories. Blank SE cells indicate Stata was unable to estimate the SE. Correlation between random slopes are not displayed.

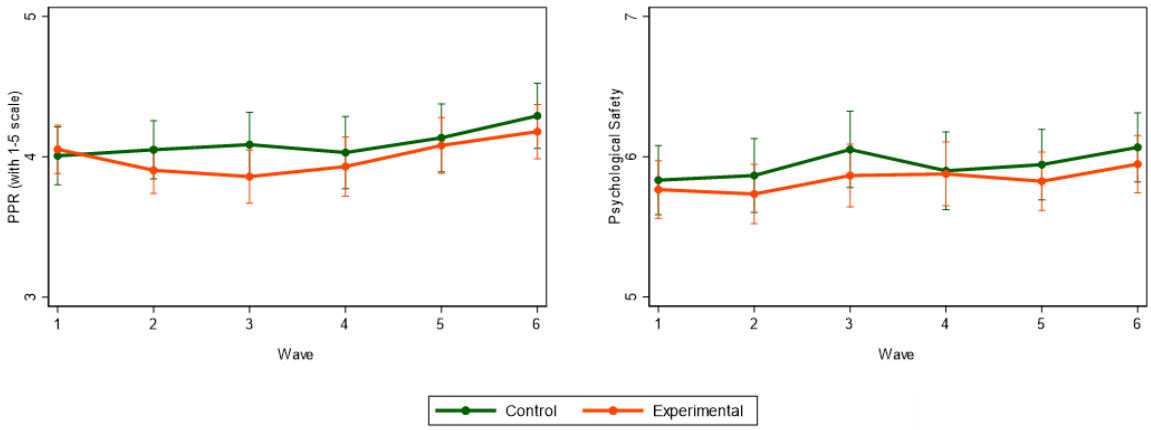

Figure 1. Changes in Perceived Partner Responsiveness and Psychological Safety by Treatment Group in Study 1
Note. Marginal means are shown. Error bars represent 95% confidence intervals.

to our hypothesis, there was some sign of delayed improvement in the experimental group, possibly driven by unmeasured factors.

The reason for inefficacy could be that these ten-minute sessions were short and disruptive, and subjectively did not make sense to the participants due to the lack of a meaningful introduction. They were inserted into class sessions where the main topic was unrelated to interpersonal relationships and they were led by a researcher who was otherwise absent. Another problem was the introduction to the interventions was brief. This process may have inadvertently conveyed that the interventions were unimportant.

Study 2

Study 2 was conducted in a compressed 10-week summer semester. We modified the intervention to address the limitations of Study 1. First, we added a lecture on the rationale for collaborative learning and listening exercises at the outset, which reviewed the advantages and disadvantages of collaborative learning (Nokes-Malach et al., 2015). The lecture emphasized that disadvantages, such as social loafing, were less likely to occur in responsive teams. Students would therefore maximize benefits by becoming better listeners. This lecture was later converted to an online animated video (https://youtu.be/aA4ym2vuqn4). We allotted longer blocks of time for the student exercises to convey their importance and allocated more time to the Fast Friends exercise, because there is consistent evidence of its efficacy (Aron et al., 1997).

We collected open-ended qualitative data from this sample because the class size was too small for an adequately powered quantitative analysis, and because we wanted to find out if participants mentioned factors that we had failed to consider. Using open-ended questions enabled us to discover such factors.

Methods

Participants

The participants were 24 students in a compressed summer-semester version of the course described earlier. Because a short survey form was used here, we did not gather demographic data and could not cluster data by dyad. There was no incentive for participating in the survey, but class activities were part of the regular course curriculum. Participation in the study was voluntary.

Procedure

The interventions were added to a summer semester that began on May 13, 2020. The lead author visited the course on June 15, at which point each student had one month of acquaintance through interaction with their dyadic partner. In this visit, he delivered a 30-minute lecture with PowerPoint slides on teamwork and listening, and then allocated 25 minutes of class time to the Fast Friends exercise. He asked students to use at least two questions from each of the three sets of the questions and then choose questions based on their discretion. The sets are ordered such that each subsequent set requires greater disclosure than the previous one. After this 25-minute period, he then allocated 10 minutes for random students to ask him questions from the Fast Friends list.

On July 10, the course instructor allotted 20 minutes of class time for student dyads to continue the Fast Friends exercise. Some students had a different partner in this session for unknown reasons. In the following week, students were asked to take the BFI-2 (Soto & John, 2017) as homework and bring a printout or photograph of their results to class. On July 16, the lead author visited the class and explained that Big Five traits do not have desirable and undesirable poles such that a high or low level of a trait is desirable. The goal was to avoid having students feel stigmatized by their personality profile. To be transparent, he noted that the exception is emotional instability. He then asked students to form groups of eight, based on proximity. Each person in the group took a turn to talk about how they feel toward people with contrasting traits.

The goal of both the closeness and trait sharing exercise was to make students more comfortable about sharing their core self with their partner. Both exercises were variations of exercises used in

Study 1. We hypothesized that this sharing would have some effect on interpersonal dynamics.

At the end of the semester, students received an email invitation to take a Qualtrics survey which included a consent form. This form contained four questions about the impact of the intervention (see Appendix B).

Results

Because of the small sample—short answers from 24 students with a cumulative word count of 2,036—we decided to use simple coding by one rater to analyze participants' answers. This rater—the first author—made a single pass through all the answers and noted the presence of absence of positive and negative claims in each participant's answer. The coding was done by participant rather than by answer, limiting the maximum count of a given code to 24. The rater made one pass through the data spreadsheet to create and apply thematic tags based on answers and then made a second pass for thoroughness. The Fast Friends has been shown to increase closeness so it was not surprising that the most common theme was *closeness and collaboration* (Aron et al., 1997). It was followed by *openness and clarity*, a related theme pertaining to disclosure, e.g., "I felt some of the questions (and) statements allowed myself to open up before starting a project with a new person." Although one out of four participants considered the exercises as potentially awkward in future teams, only one out of 12 reported actual awkwardness in this class. Three students also appreciated that the exercises changed their routine and created variety, e.g., "It was very engaging and having a team building exercise outside of engineering problems is a nice break."

An observed drawback was potential or real inconsistency across teams, mentioned by three students. In the potential case, one student noted, "They helped a little with my first partner but with my second partner they didn't have much impact because we didn't try too hard to go deep into them." Another student wrote, "In this class we did those exercises in two moments, with two different groups. The first time I did it, the group was very open and we were able to connect in a great personal level. Until today, we have a great relationship and it was mainly because everyone was open and the questions helped us showing what we had in common." These answers indicate that effectiveness may be conditional on factors that instructors and students cannot control.

Discussion

Although statistically underpowered, this study suggested that a listening intervention may increase closeness and cohesion rather than have no effect when instructors explain its purpose and assign a few long exercises rather than a few short exercises. Cohesion was not measured in Study 1, which could explain its null findings. The finding about inconsistency across partners revealed why these interventions may produce weak results. Through the simultaneous triggering of positive processes in some teams and negative processes in other teams, the net effect can be weak or nil. This type of inconsistent mediation (rather than absence of causation) can be the source of weak correlations (Thrash et al., 2012).

Study 3

Our aim in this study was to measure the efficacy of the lecture-and-exercise intervention in a normal full-length semester. Given the positive outcomes mentioned by Study 2 participants, we anticipated it would have measurable results. In addition, we added a measure of relational cohesion because data from Study 2 and other tests of Fast Friends (Aron et al., 1997) indicated that cohesion and closeness may be enhanced by the intervention. Relational cohesion (perceived cohesion) is a sociological construct that describes the perception by members of a group that they are yoked together creating a meaningful integrated team or "a distinct, unifying social object" (Lawler & Yoon, 1996, p. 94).

Cohesion has both bright and dark sides. By definition, cohesive teams are not split along psychological fault lines, so there is less antagonism (Gal, 1986). As people cohere into a team, however, they are also likely to notice similarities and become friends, forming affective ties alongside their original instrumental ties (Reis et al., 2011; Thye et al., 2019). They may spend more time socializing than on task (Carter & Phillips, 2017) and succumb to groupthink by reaching conclusions too soon and failing to share information fully (Janis, 1982; but see Leana, 1985). However, when the task is nonroutine—no algorithm exists for solving it—cohesion seems to improve performance (Jehn, 1995). Moreover, cohesion is also unlikely to trigger too much socializing in a class where students have constraints on their time. Furthermore, cohesion should bolster social support for students who feel that they are on the margins, leading them to perceive technically challenging problems as growth opportunities where risk taking and failure are informative (Carmeli & Gittell, 2009; Feeney & Collins, 2015). Consequently, an intervention that increases cohesion may be useful in classrooms, even if doesn't improve psychological safety or perceived partner responsiveness.

We therefore added a third research question:

3. Can a simple intervention increase cohesion in student dyads?

Our construal of cohesion derives from the sociological study of interactions and exchanges in dyads (Lawler, 2006; Lawler et al., 2014; Molm et al., 2007): continued exchanges between members of a group cause the group itself to perceived as a meaningful entity, engendering cohesion and identification with the group. This conception includes elements of both social and task cohesion: two people feel relational cohesion when they are unified (social) and working together (task). Because the sample was smaller due to lower course enrollment, we used a pre-post design to maximize statistical power. An added advantage—relative to Study 1—was that all participants were equally acquainted when the intervention commenced.

Table 3. Descriptive Statistics and Correlations Between Perceived Partner Responsiveness, Psychological Safety, and Cohesion (Study 3)

Variable	*M*	SD_o	SD_b	SD_w	PPR	PS
PPR (1-7)	6.3	1.0	0.9	0.7		
Psych. Safety (1-7)	6.2	0.8	0.6	0.5	0.67***	
Cohesion (1-9)	7.7	1.4	1.1	0.9	0.41***	0.58***

Note. PPR = perceived partner responsiveness. o = overall. b = between. w = within. *** $p < .001$.

Methods

Sample

The participants of this study were 84 undergraduate engineering students from 44 dyads. Partial course credit was an incentive as in Study 1. We excluded four participants who switched from one partner to another in the middle of the semester. The gender composition of the sample was 30% male, 64% female, and 5% unknown gender. The racial and ethnic composition was 42.5% White, 2.5% Black, 25% Asian, 11.3% Asian–White, 6.3% Middle Eastern, 6.3% other races, and 6.3% unknown race or ethnicity. In all, 91.3% were domestic students, 3.8% were international students, and 5.0% had missing data on this question.

Materials

We used the scales described earlier. However, the PPR items were answered on a seven-point scale instead of a five-point scale (Study 1), making the PS and PPR scales comparable. To minimize the negative wording in the sixth item, we changed it from "My partner would not deliberately act in a way that undermines my efforts" to "Never would my partner deliberately act in a way that undermines my efforts." For perceived partner responsiveness, the internal consistency coefficients (Cronbach's α) from wave 1 through 6 respectively were .66, .73, .65, .75, .78, and .82. For psychological safety, they were .66, .73, .65, .75, .78, and .81.

Relational cohesion was measured with six items from a nine-item relational cohesion scale for dyads (Lawler & Yoon, 1996, 1998). Each item has a bipolar scale with adjective and phrase pairs on the anchors, such as "diverging" and "converging;" "coming apart" and "coming together;" and "working separately" and "working together." Numerical labels from 1 to 9 and the label "neutral" above the midpoint are also displayed. Cronbach's αs from wave 1 through 6 respectively were .94, .94, .95, .96., .97, and .99.

Procedure

As in the previous study, students were introduced to the purpose of collaborative learning and listening exercises. At the beginning of the semester, they watched an animated video, created by the first author, that followed the lecture used in the first phase of Study 2.

The basic survey protocol followed Study 1. A long initial survey and six short surveys separated by two-week intervals were used. The study was conducted in a fall semester that began on August 20, 2019. The long survey was conducted on September 2, and the short survey waves began on September 12 and ended on November 21.

On October 5, which was between Wave 2 and 3, we administered the intervention in a 50-minute block in a longer class session. The intervention had five parts: (1) introduction to explain purpose (2 minutes); (2) Fast Friends exercise (16 minutes); (3) Fast Friends questions asked to researcher (3 minutes; see Gehlbach et al., 2016); (4) traits and experience sharing (8 minutes); and (5) verification of goals and values (8 minutes). In the fourth segment, students were presented with a list of the ten facets of the Big 5 personality traits (DeYoung et al., 2007) and asked to independently estimate whether they were high or low in these traits. They then discussed their traits with their dyadic partner as in Study 2. In the fifth segment, students were asked to tell their partner about at least two goals and values that were personally significant; students were asked to verify that they fully understood their partner's answers.

Data Analysis

In the model, the dyad level was added because the intercept had substantial variance at that level. Slopes at the dyad level were fixed to enable convergence.

Level 1 (Wave):

$$y_{ijk} = \pi_{0jk} + \pi_{1jk} * Wave2 + \pi_{2jk} * Wave3 + \pi_{3jk} * Wave4 + \pi_{4jk} * Wave5 + \pi_{5jk} * Wave6 + e_{ijk} + (\rho * e_{i-1,jk})$$

Level 2 (Person):

$$\pi_{0jk} = \beta_{00k} + \beta_{01k} * Asian + \beta_{01k} * Black + \cdots \beta_{08k} * Female + r_{0jk}$$

$$\pi_{1jk} = \beta_{10k} + r_{1jk}$$

$$\pi_{2jk} = \beta_{20k} + r_{2jk}$$

…

$$\pi_{5jk} = \beta_{50k} + r_{5jk}$$

Level 3 (Dyad):

$$\beta_{00k} = \gamma_{000} + u_{00k}$$

$$\beta_{10k} = \gamma_{100}$$

$$\beta_{20k} = \gamma_{200}$$

…

$$\beta_{50k} = \gamma_{500}$$

Results

Table 3 contains descriptive statistics and correlations between the three outcome variables. As in the earlier study, all means were above the scale midpoint; the mean of 5.9 in psychological safety

was close to the maximum of 7. The standard deviations indicated there was moderate variance between and within persons.

The results of the three multilevel models are in Table 4. Graphs showing the marginal means are in Figure 2. For all outcomes, there was no significant difference between Waves 3 and 4 and we therefore did not proceed to secondary techniques such as piecewise regression.

Discussion

In this study, the intervention included a prefatory lecture and a set of intensive activities, intended to convey to students that the intervention was important. It also used a within-subject design, which improves statistical power (Maxwell & Delaney, 1990). The results of Study 3 therefore provide stronger evidence than Study 1 that these interventions have negligible effects on perceived partner responsiveness, psychological safety, and cohesion.

These findings do not rule out the possibility that a larger intervention, including additional lectures and exercises, could be effective. However, an intervention of that magnitude would nearly constitute a small course. Another possibility is that partners were already well acquainted at the time of the intervention, so there was little room for improvement.

General Discussion

The aggregate results of these studies suggest that the range of brief interventions that we tested have little effect in a problem-solving environment. Class time is mainly occupied by conversations between students, which can engender responsiveness and caring on their own, making additional exercises redundant. Given the mere exposure effect, even proximal non-interacting students should like one another (Moreland & Beach, 1992). Repeated acts of exchange, such as those required in this context, should increase cohesion and liking further (Lawler et al., 2014). Our interventions may have worked better in classes where dyads convene infrequently.

The timing of the intervention could have mattered too. None of these studies implemented the intervention in the first week of the semester and then kept partners together for a full regular semester. In that situation, the intervention may have accelerated the natural effect of acquaintance. The timing of measurements

Table 4. Multilevel Models with Measurement Wave as Predictor and Perceived Partner Responsiveness, Psychological Safety, and Cohesion as Outcomes (Study 3)

	PPR		PS		Cohesion	
	B	*SE*	*B*	*SE*	*B*	*SE*
Intercept	6.316***	(0.210)	6.185***	(0.160)	7.497***	(0.280)
White						
Black	-1.029	(0.547)	-0.463	(0.415)	-1.217	(0.717)
Asian	0.133	(0.214)	-0.177	(0.160)	-0.287	(0.269)
Asian-White	0.366	(0.280)	0.121	(0.208)	0.061	(0.345)
M.East.	0.057	(0.365)	-0.118	(0.268)	0.135	(0.436)
Other	0.390	(0.361)	0.106	(0.271)	0.077	(0.447)
Unknown	0.249	(0.755)	0.127	(0.574)	0.240	(0.985)
Male						
Female	-0.150	(0.198)	0.058	(0.150)	-0.148	(0.261)
Wave 1						
Wave 2	-0.038	(0.105)	0.009	(0.081)	0.354*	(0.164)
Wave 3	-0.005	(0.132)	0.016	(0.083)	0.434*	(0.172)
Wave 4	0.041	(0.131)	0.018	(0.088)	0.518**	(0.166)
Wave 5	0.074	(0.159)	0.103	(0.088)	0.709***	(0.171)
Wave 6	0.001	(0.142)	0.085	(0.091)	0.622***	(0.171)
SD_{Dyad}	0.227		0.300		0.733	
SD_{Person}	0.394		0.386		0.599	
$SD_{Residual}$	0.911		0.542		1.051	
Random Slopes						
SD_{Wave2}	0.447		0.305		0.968	
SD_{Wave3}	0.594		0.099		0.839	
SD_{Wave4}	0.279		0.115		0.477	
SD_{Wave5}	0.741				0.469	
ρ	0.667		0.411		0.594	
N	418		418		417	

Note. PPR = perceived partner responsiveness. PS = psychological safety. ρ = correlations between residuals of consecutive waves. * $p < .05$, ** $p < .01$, *** $p < .001$. Blank coefficient cells indicate reference categories. Blank SE cells indicate Stata was unable to estimate the SE. Correlation between random slopes are not displayed.

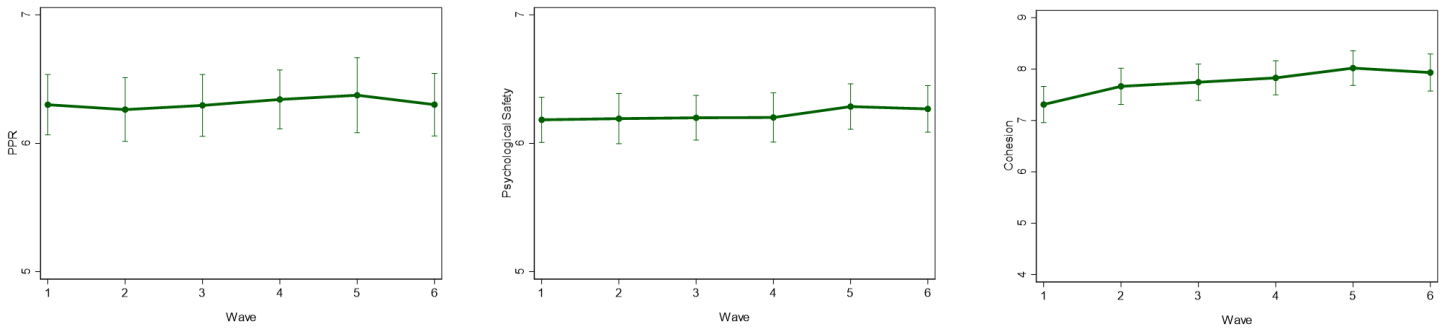

Figure 2. Changes in Perceived Partner Responsiveness. Psychological Safety, and Cohesion in Study 3
Note. Marginal means are shown. Error bars represent 95% confidence intervals.

and the use of repeated measures can also affect results (Longwell & Truax, 2005; Shrout et al., 2018). A study with a single wave of post-intervention measures and propensity score analysis may be an alternative in future studies.

There may also be a dose-response association such that one portion of the exercises could be improved by increasing its intensity and duration. A longer introductory lecture, spread across multiple sessions, may have motivated students to be more engaged. We cannot claim that the current dose had no effects because some unmeasured skills may have been learned. Nevertheless, these studies suggest that the current interventions may be unfruitful.

Prior work on adults suggests that listening is trainable. For instance, a study by Itzchakov and Kluger (2017a) showed that a business workshop on listening was effective: participants were either enrolled in a listening-circle workshop or a confidence-enhancement (control group) workshop. A pre-post comparison showed that the listening workshop induced participants to experience better listening and less social anxiety overall (although there were no observers acting as evaluators). Studies with weaker methodologies also suggest an effect of training. A study on communication skills in a pharmacy education program is one example (Boesen et al., 2009; see also Krueger et al., 2019). The researchers did a pre-post comparison between the three cohorts before and after the introduction of the new training module. Results indicated that improvisational exercises from theater improved communication skills during standardized patient examinations where an instructor plays the patient. Among other things, students began to recognize subtle cues about the patient's emotions. However, these interventions were clearly framed as interpersonal workshops—they were not embedded into another course as a seemingly peripheral topic. Moreover, in the patient-examination study, the participants could immediately transfer their skills to workplace settings, whereas in our case they could not.

Mandatory dyadic work may cause students to feel connected to other students because they can elicit support and concern from at least one other student, unlike in lecture-based courses. Such affective and social ties can contribute more to group cohesion than instrumental and work-based ties, due to the sense of social identity that emerges (Thye et al., 2019). Artificial interventions may have no additional effect given this already high level of cohesion. However, this point is speculative—the current studies cannot be used to discern whether lecture-based courses are conductive to weaker ties because there no comparison of lecture vs. non-lecture courses.

The current work may also serve as a model for cohesion studies in online workgroups. As online learning grows, it will be fruitful to explore whether early exercises can mitigate problems that arise with online collaboration, particularly in short-term teams that have less time to build natural cohesion. Another implication, also deserving further inquiry, is that brief interventions involving small amounts of time and instructor expertise may have little efficacy in this domain. Research in other domains also suggests that short-term interventions have little efficacy (Hayes et al., 2019; Sin & Lyubomirsky, 2009). If students in professional fields are to learn and apply principles of interpersonal functioning, longer interventions may be worthwhile. Alternatively, the interventions should be administered when students are working as interns or trainees, and the form of the intervention should closely correspond to the nature of the work at hand (see Boesen et al., 2009). Under these conditions, deep transfer of skills and knowledge may occur.

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Appendix A

In the first batch of exercises, the roles of listener and speaker are unambiguous, which enhances listening quality (Itzchakov & Kluger, 2017b). The first exercise was the *weekly review* exercise (#1). Person A is instructed to talk about three things that happened in their life in the last week and Person B's only goal is to listen closely. Then the roles are switched.

Participants then did the *verification* exercise (#2 and #3), which requires listening and retrieval (Rogers & Roethlisberger, 1952). For two minutes, Student A talks about any topic. For the next two minutes, B summarizes what they heard and concludes with the question, "Did I hear you correctly?" Then person A describes what B repeated correctly and incorrectly; the dyad then cycles

through feedback and response until A is satisfied.

In the *free conversation* exercise (#4), both partners talk for one minute of free conversation. This is followed by Person A solely talking for 3 minutes, followed by Person B solely talking. This exercise allowed participants to consider what it was like to simply listen.

Then, we used the *mirror* exercise (#5) where partners in dyads had to mirror each other's actions respectively for two minutes each.

The secondary theme in these exercises was unconditional acceptance of what one says and hears, which stands in contrast to critical evaluation. In the "*Yes and…"* exercise (#6), drawn from improvisational theater (Halpern et al., 1994), one partner proposes a story idea and each partner then continues the story, starting with "Yes and" each time, which expresses acceptance (cf. Rogers, 1951). Some formative research suggests this exercise is effective in professional training (Boesen et al., 2009).

The three final exercises also focused on acceptance. In the *choir of complaints* exercise (#7), the whole class stands in a circle (Tatsuki & Houck, 2010). Each person takes a turn complaining about some disappointment. Next, that person repeats the complaint by chanting it rather than stating it; the whole group repeats that chant in unison.

Each person's complaint is thus unconditionally accepted. In the *trait reflection* exercise (#8), each person estimates their Myers-Briggs trait profile (without an inventory), and then describes how they experience people on the extremes of each trait.

In the *group identity* exercise (#9), participants share unique stories about an identity with each other.

The *personal story* exercise (#10) blends these two themes. Students ask each other questions that build interpersonal closeness (Aron et al., 1997). Person A spends two minutes answering, and then Person B spends two minutes answering. The goal is to listen well and understand one's partner's life perspective.

Appendix B

1. Please describe any positive impact that these exercises may have had on you. If you felt there was a positive impact on your partner, please describe that as well.
2. Please describe any negative impact that these exercises may have had on you. If you perceived a negative impact on your partner, please describe that as well.
3. Please describe whether you felt there was an absence of impact, despite the intended goals of the exercises.
4. If you feel like the listening exercises made this class better or worse than similar classes at this university, please describe why.

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