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Supplementary Materials for

A psychological intervention strengthens students' peer social networks and promotes persistence in STEM

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Supplementary Text

I. Additional Details on Study Procedure

Participants were recruited by researchers both online and in person. Researchers emailed all students in the class with information about the study and a personalized link to the online consent form and baseline questionnaire during the first week of the semester. The next day (the second day of the course), researchers visited the biology course to make an announcement about the study at the beginning of class, distribute paper consent forms to all students, and answer any questions students had about participating at a table outside the classroom after class. During the following week, researchers again set up a table outside the classroom to recruit participants and answer any questions about the study before and after class, and also sent an additional recruitment email containing a personalized link to the online consent form and baseline questionnaire to all students who had not yet signed up to participate. Any students who consented in person using paper consent materials were emailed a link to complete the baseline questionnaire by the end of the day they consented. All students who consented to participate in the study during the first two weeks of the semester were included as participants in the study.

Participants were randomly assigned to the values affirmation or control condition before the third week of the course using blocked randomization through the blockTools package in R (random greedy algorithm). The blocking variables were gender, race (a binary variable for racial/ethnic groups marginalized in STEM—Black, Latinx and American Indian—or not marginalized in STEM—White and Asian), parental income, weekly course section, year in school, and two variables indicating participants' willingness to participate in other elements of the overarching study. Following the random assignment procedure, participants' assigned writing exercises were placed into envelopes labeled with their names and delivered to course teaching assistants.

The week prior to the intervention, the class professors announced in lecture that students would be completing a writing exercise in their course sections the following week. The exercise was presented as part of the course, as an activity to practice thinking and writing (it was not linked to the ongoing research study in the class). Then, during students' weekly sections in the third week of the course, teaching assistants introduced the exercise and handed out these envelopes to the students in their sections (see project OSF page for teaching assistant script and intervention materials). The envelopes were designed to look identical so that students would not know that there were different conditions. Students who did not consent to participate in the study received the

control writing exercise inside their envelope, which was discarded immediately upon transfer to the research team.

After opening their envelopes, students in both conditions first ranked a list of 11 values (e.g., creativity, independence, relationships with friends or family, religious values) in order of personal importance. Next, participants were asked to write a short essay for fifteen minutes. In the affirmation condition, participants wrote this essay about the value they had ranked as most important. In the control condition, participants wrote about why the value they ranked as ninth most important might be important to someone else. To reinforce the manipulation, participants in both conditions then summarized the top two reasons the value they selected was important to them (affirmation) or someone else (control) and indicated their agreement with two statements about the value's importance (affirmation: "This value has influenced my life," "This value is an important part of who I am"; control: "This value has influenced some people," "This value is important to some people").

Finally, participants were emailed an individualized link to the end-of-semester measures in the last week of the course (Week 14). Participants completed these measures by the end of finals period (by Week 16).

During the subsequent semester, we obtained the enrollment status of each participant in the second semester of the biology course to determine next-semester biology track retention.

The social network study described in this paper was administered as one component of a larger study, which aimed to understand students' experiences in gateway STEM classes and the types of processes that underlie affirmation effects. Accordingly, the baseline and end-of-semester questionnaires included questions about psychological wellbeing, perceptions and experiences related to STEM and identity, and other individual difference measures that were not part of the social network study. These measures are not described in detail here; however, we did examine whether participants who completed all aspects of the social network study differed on any collected measures from participants who were lost to attrition (including non-network measures), and those measures and results of the attrition analyses are reported in the attrition analyses section below.

II. Additional Details on Course

The course selected for this research was Introduction to Molecular and Cellular Biology I, the first semester of a two-semester yearlong introductory biology course. Both semesters of the course (I and II) are required for premedical and other prehealth (e.g., dentistry, veterinary medicine) tracks, bioscience majors (biology, biochemistry, biophysics, and neuroscience), and the biology concentration at this university. Students cannot substitute AP Biology from high school or biology from community college for this course. Students typically take this course in their second year, given that one year of college chemistry (or a demonstrably strong high school chemistry background) is a prerequisite. In turn, both semesters of the course are prerequisites for advanced courses offered by the Biology Department, such that students majoring in a bioscience field typically need to take both semesters sequentially to complete their major requirements. Students taking the course include undergraduates and postbaccalaureate students (in a postbac premedical program or the continuing education school).

The course consists of a large lecture twice per week as well as smaller weekly recitation sections. Each lecture was delivered twice on class days, once in the morning and once in the evening, and students could choose which lecture to attend based on their schedules. Two professors cotaught the lectures (one, a White man, taught the lectures for approximately the first third of the course, while the second, a White woman, taught the lectures for the remainder of the semester).

Attendance at recitation sections was required for undergraduate students and optional for postbaccalaureate students. In the semester studied, there were 15 undergraduate recitation sections with a mean of 26 students (the smallest contained 22 students; the largest contained 30 students). Postbaccalaureate students could voluntarily attend any one of five additional recitations each week; these students were categorized as a 16th section for analysis. The recitation sections were taught by student teaching assistants, usually undergraduate or postbaccalaureate students who previously took the course and received high grades. (Given that postbaccalaureate students could attend different recitations each week, we assembled a file box of all of the postbaccalaureate students' intervention envelopes and circulated this file box to the teaching assistants of all five postbaccalaureate recitations during the week of the intervention.)

Students took four exams for the course, the first three of which were administered during lecture periods. The final exam was administered during finals period. Undergraduate students also took weekly quizzes in their recitation sections, which were graded essentially as pass/fail (students had to reach a certain threshold of total points across quizzes, after which additional quiz points did not affect course grades).

III. Additional Details on Intervention Content

Of the 290 participants who were present in their weekly course section the day of the intervention and therefore completed the writing exercise, 287 were coded as unambiguously following instructions in their essays. Of the remaining three (2 affirmation, 1 control), the two students in the affirmation condition did not fully engage with the prompt in their essay, but did follow instructions for the rest of the exercise (i.e., wrote two reasons the top-ranked value was important to them; indicated their agreement that the value had influenced their life and was an important part of who they were). The student in the control condition engaged with the prompt but began their essay by discussing why the value they ranked ninth was not important to them (instead of why it could be important to someone else), and wrote that they ran out of time to finish answering the question ("Didn't have time to finish I realize I haven't answered the prompt"). Given that all three of these students engaged in some form with the exercise—and, more importantly, that we had decided a priori that we would not exclude any participants who completed the study—we retained them in the analyses.

On average, students wrote 129.04 words in their essay (135.84 and 122.23 words in the affirmation and control conditions, respectively). The top three values ranked as most important were the same for students in both conditions: *Relationships with friends or family* (n_{affirm} = 102, $n_{control}$ = 93), *Independence* (n_{affirm} = 20, $n_{control}$ = 24), and *Creativity* (n_{affirm} = 9, $n_{control}$ = 11). The top three values ranked as ninth most important were *Being good at art* ($n_{control}$ = 31), *Religion* ($n_{control}$ = 22), and *Politics* ($n_{control}$ = 21) in the control condition and *Being good at art* (n_{affirm} = 28), *Politics* (n_{affirm} = 27), and *Physical attractiveness* (n_{affirm} = 22) in the affirmation condition.

At the suggestion of a reviewer, we examined the possibility that the positive social effects of the affirmation intervention may have only emerged for participants who wrote about relationships with friends or family as their top-ranked value. As is typical in values affirmation work, very few affirmed participants did not select relationships with friends or family as their top value: 43 affirmed participants total wrote about a different value. This group was further reduced to 34 affirmed participants in the sample of 226 participants who completed all aspects of the study including network measures. We thus urge caution in interpreting the results of these analyses, as underpowered analyses produce more uncertain and less reliable estimates.

To examine this question, we recoded intervention condition as having three categories: control, affirmation-relationships, and affirmation-other. These were entered into models as two dummy variables with control as the reference group. We first examined whether the intervention had any positive social effects for affirmed participants who wrote about other values compared to

those in the control condition. We found that the 34 affirmed participants who wrote about other values had significantly higher total degree centrality (b = 1.21, SE = 0.44, p_{perm} = .039) and marginally higher out-degree centrality (b = 0.76, SE = 0.33, p_{perm} = .078) and in-degree centrality (b = 0.48, SE = 0.22, p_{perm} = .098) than those in the control condition. These 34 affirmed participants did not significantly differ from control participants in closeness, betweenness centrality, or strength of ties, although all effects were positive. Thus, consistent with analyses reported in the main text, we found that these 34 affirmed participants also had increased social integration in the class compared to unaffirmed participants; affirmed participants had more friends in the class than unaffirmed participants regardless of the value they wrote about.

We next tested whether the effects of writing about relationships with friends and family on these network variables differed from the effects of writing about a different value (i.e., affirmation-relationships vs. affirmation-other). The effects of the two categories of affirmation did not differ significantly on any variable (closeness: b = 0.01, SE = 0.005, p_{perm} = .302; betweenness: b = 0.0003, SE = 0.0008, p_{perm} = .728; total degree: b = -0.31, SE = 0.46, p_{perm} = .605; out-degree: b = 0.04, SE = 0.35, p_{perm} = .938; in-degree: b = -0.34, SE = 0.22, p_{perm} = .259; total strength: b = -0.05, SE = 0.28, p_{perm} = .878; out-strength: b = 0.13, SE = 0.32, p_{perm} = .730; in-strength: b = 0.02, SE = 0.29, p_{perm} = .955).

Overall, we found little evidence that writing about relationships with friends and family versus other values had a meaningful difference on effects. It appears that (1) writing about relationships with friends and family and writing about other top-ranked values both had positive social effects relative to completing the control exercise, and (2) the effects of writing about friends and family versus a different value did not differ significantly from one another. These analyses may suggest that the social effects of affirmation do not hinge on selecting relationships with friends and family as one's top value, although we are hesitant to overinterpret these results due to the small sample size of participants who selected a different top value. However, these findings are consistent with prior work on the social effects of affirmation that found boosts in positive social feelings (e.g., love) regardless of the value participants wrote about (*27*).

IV. Attrition Analyses

Analyses were conducted to assess whether the sample of participants who completed all aspects of the study and are therefore included in the main analyses, "completers" (n = 226), differed from those who enrolled in the study but did not complete all parts, "non-completers" (n = 102). Completers and non-completers differed in age, whereby completers were significantly younger (t(138.13) = -4.17, $p < 0.001$; completers: M = 20.60, SE = 0.22 vs. non-completers: M = 22.79, SE = 0.47). This age difference is likely explained by the decreased proportion of postbaccalaureate students (i.e., "postbacs"), who are on average older (t(90.49) = -13.32, $p < 0.001$; postbacs: M = 26.14, SE = 0.48 vs. undergraduate students: M = 19.60, SE = 0.12), in the final sample of completers ($\chi^2(1) = 22.93$, $p < 0.001$; postbacs comprised 16.8% of completers but 42.2% of non-completers). This decrease in the proportion of postbac students in the final sample of completers resulted from the fact that attendance at weekly recitation sections—where the intervention was administered—was not required for postbac students. As such, relatively fewer postbacs were present in class the day that the intervention was administered, and therefore fewer of these students completed the intervention. In addition, completers had significantly more friends at the start of the semester than non-completers (t(212.91) = 2.30, p = 0.02; completers: M = 4.67, SE = 0.24 vs. non-completers: M = 3.75, SE = 0.32) and were higher in collective threat (*52*) than non-completers (t(174.96) = 2.12, p = 0.04; completers: M = 2.77, SE = 0.05 vs. non-completers: M = 2.59, SE = 0.07). The final sample of completers did not differ significantly from non-completers on any of the other demographic or baseline measures collected at the beginning of the semester, including intervention condition, gender, race, parents' income, first vs. continuing college

generation status, relationship status, strength of friendships, number and strength of study and support ties, medical school motivation, perceptions that the course was diagnostic of ability to get into and succeed in medical school, theories of intelligence, grit, Big 5 personality facets, belonging at the university, gender and race rejection sensitivity, everyday discrimination, psychological distress, and self-esteem (citations for these measures available on OSF page for the project).

We also assessed differences between completers, who completed all parts of the study, and those who completed the in-class intervention, but did not complete both of the questionnaires ("intervention-only"; $n = 64$). Because the in-class intervention writing exercise was presented as part of the course but the questionnaires were an optional online study, we were concerned that the 226 completers included in our analyses would not be representative of all participants who completed the intervention. Analyses comparing the completers to the intervention-only participants suggested that the two groups were fairly similar: Compared to the 64 participants who completed the intervention but did not complete the post-semester questionnaires, the final sample of 226 participants who completed all three parts were significantly higher in grit ($t(92.69) = 2.27$, $p = 0.03$; completers: $M = 3.49$, $SE = 0.04$ vs. intervention-only: $M = 3.29$, $SE = 0.08$; this difference is perhaps unsurprising considering that grit is a measure of persistence and follow-through), but did not differ significantly on any of the other demographic or baseline measures collected.

V. Network Properties

Friendship Network

Full network. At the beginning of the semester, through the baseline questionnaire, a total of 239 participants nominated one or more friends. The remaining 89 participants who filled out the baseline questionnaire reported having no friends in the course. These responses produced a valued, directed start-of-semester friendship network of 460 students (all participants who completed the baseline network survey and the friends in the course whom they nominated), with 855 friendship ties between them. (Many students began the course with friends because they decided to take the class at the same time as friends—in focus groups before the study, students revealed that this was a common strategy for coping with the difficulty of course—and/or because they had met classmates in previous science and pre-medical courses.) At the end of the semester, 181 of the 257 participants who completed the end-of-semester questionnaire nominated one or more friends, while the remaining 76 reported no friends in the course. These responses produced a valued, directed, end-of-semester friendship network of 394 students, with 629 ties between them. See Fig. 1 in main text and Table S1 for additional information about the start- and end-of-semester friendship networks.

Completers only. Among the participants who completed all parts of the study and were thus included in the analyses ($n = 226$ "completers"), 172 nominated one or more friends at the beginning of the semester. The remaining 54 reported having no friends in the course. At the end of the semester, 167 of the 226 completers reporting having one or more friends, while the remaining 59 reported no friends in the course.

Study Partnership Network

Full network. At the beginning of the semester, through the baseline questionnaire, a total of 202 participants nominated one or more study partners. The remaining 126 participants who filled out the baseline questionnaire reported having no study partners in the course. These responses produced a valued, directed start-of-semester study network of 419 students (all participants who completed the baseline network survey and the study partners in the course whom they nominated), with 475 study partnership ties between them. At the end of the semester, 107 of the 257 participants who completed the end-of-semester questionnaire nominated one or more study partners, while the remaining 150 reported no study partners in the course. These responses produced a valued, directed, end-of-semester study network of 326 students, with 226 ties between

them. See Fig. S2 for study network visualizations and Table S1 for additional information about the start- and end-of-semester study partnership networks.

Completers only. Among the 226 participants who completed all parts of the study, 145 nominated one or more study partners at the beginning of the semester. The remaining 81 reported having no study partners in the course. At the end of the semester, 100 participants reported having one or more study partners, while the remaining 126 reported no study partners in the course.

Support Network

Full network. At the beginning of the semester, through the baseline questionnaire, a total of 176 participants nominated one or more support-providing peers in the course. The remaining 152 participants who filled out the baseline questionnaire reported having no support providers in the course. These responses produced a valued, directed start-of-semester support network of 392 students (all participants who completed the baseline network survey and the support providers in the course whom they nominated), with 350 support ties between them. At the end of the semester, 127 of the 257 participants who completed the end-of-semester questionnaire nominated one or more support providers, while the remaining 130 reported no support providers in the course. These responses produced a valued, directed, end-of-semester support network of 326 students, with 225 ties between them. See Fig. S2 for support network visualizations and Table S1 for additional information about the start- and end-of-semester support networks.

Completers only. Among the 226 participants who completed all parts of the study, 130 nominated one or more support-providing peers at the beginning of the semester. The remaining 96 reported having no support providers in the course. At the end of the semester, 120 participants reported having one or more support providers, while the remaining 106 reported no support peers in the course.

Relationships Between Networks

As noted in the text, the study and support networks substantially overlapped with the friendship network. At the start of the semester (Time 1), 96.5% of study partners were also named as friends and 99.2% of support providers were also named as friends. At the end of the semester (Time 2), 95.3% of study partners were also named as friends and 96.7% of support providers were also named as friends. Additionally, the networks were significantly more correlated than one would expect by chance, as measured via Quadradic Assignment Procedure (QAP) correlation tests, a method of calculating association between two matrices. Correlations decrease from Time 1 to Time 2, but the networks remain highly and significantly correlated (see Table S2). These results indicate that students who study together or who seek out one another for support also tend to be friends and, at least in this course, that students' study and support networks are essentially subsets of their friendship network.

VI. Study and Support Network Results

Study network. At the end of the semester, there were no significant differences between conditions in closeness or betweenness centrality in the study network (closeness: $b = .001$, $SE = .001$, $p = .430$; betweenness: $b = -1.74 \times 10^{-6}$, $SE = 3.99 \times 10^{-6}$, $p = .664$). Affirmed students had marginally more study partners than unaffirmed students at the end of the term (degree centrality: $b = .43$, $SE = .23$, $p = .061$) and studied significantly more often with their study partners than unaffirmed students (total tie strength: $b = .25$, $SE = .12$, $p = .047$).

Support network. There were no significant differences by condition in closeness or betweenness centrality in the support network (closeness: $b = .001$, $SE = .001$, $p = .100$; betweenness: $b = -3.60 \times 10^{-6}$, $SE = 8.27 \times 10^{-6}$, $p = .664$). Affirmed students had significantly more support-provision relationships in the course (degree centrality: $b = .37$, $SE = .18$, $p = .040$) and

were significantly more likely to go to their support providers for support and vice versa than unaffirmed students (total tie strength: $b = .42$, $SE = .18$, $p = .019$).

VII. Alternative Explanations of Network Findings

Here we investigate three potential alternative explanations for the observed difference between conditions in friendships. We do not find evidence in support of these alternative explanations for the observed effects of intervention condition on students' social networks.

A. Perception of Friendships

One possibility is that affirmed participants did not actually have more and closer friendships than unaffirmed participants by the end of the semester, but rather that they simply had a rosier perception of their classmates and relationships in the course. In other words, perhaps the observed findings do not reflect a difference between conditions in actual social relationships, but instead a mere difference in *perception* of others. After all, past research has found that affirmed individuals report more positive other-directed feelings following the intervention than unaffirmed individuals (*27*).

Regarding this possibility, perceiving a friendship may in and of itself be beneficial, regardless of whether an actual friendship exists. For example, research on social support shows that perceived availability of support is as or more important for wellbeing as actually receiving support (*53*). Perceiving a friendship may correspond with perceiving that support and other social resources are available, potentially yielding benefits for the perceiver. Moreover, the perception that an individual is one's friend is not necessarily theoretically distinguishable from "actual" friendship (see, for example, research on long-distance friendship, which highlights the importance of subjective assessments of and attitudes toward friendship (*54*)).

That said, a strength of the social network methodological approach is that all participants provide information on social relationships in the course, which allows us to corroborate any one participants' friendship nomination with the nominations of others. For example, for any Participant A who names Participant B as a friend, we know whether Participant B named them back (i.e., whether the tie was reciprocal). This allows for the possibility of distinguishing between a friendship that is perceived by only one person in the pair and a mutual friendship reported by both individuals. If the observed difference between conditions were simply a matter of rosy perception, we would expect that affirmed (vs. unaffirmed) participants would name more classmates as friends at the end of the semester, but fewer of these ties would be reciprocal (as in Fig. S4, Panel A). We thus compared affirmed and unaffirmed students' proportion of reciprocal friendship ties at the end of the semester.

There was no significant difference between conditions in proportion of reciprocal ties among participants who named at least one friend at the end of the semester ($b = 0.005$, $SE = 0.05$, $t(150) = 0.09$, $p = 0.93$; see Fig. S4, Panel B). Although this analysis does not definitively rule out that the difference in reported ties between affirmed and unaffirmed students was simply a difference in perception of others, it does not support that conclusion.

B. Attention to survey

Because participants nominated friends in the class through an online questionnaire, it is possible that a difference in attention paid to the survey could have affected the social ties students reported. For example, a participant who paid more attention to carefully recalling their friends in the course could list more friends than a participant who was more carelessly speeding through the survey without paying attention. If the affirmation intervention caused participants to attend more carefully to the questionnaire than those in the control condition, this could explain why affirmed participants reported more social ties at the end of the semester than unaffirmed participants.

To investigate this potential alternative explanation, we examined participants' level of attention paid to the survey through two attention check questions embedded within each questionnaire (e.g., "If you are paying attention, please select 'Strongly Agree'"). Intervention condition did not predict responses to these questions on the end-of-semester questionnaire (b = -0.009, SE = 0.02, $t(209)$ = -0.48, p = 0.63), suggesting that intervention condition did not affect the level of attention participants devoted to the questionnaires. This analysis does not support the conclusion that the difference in reported ties between affirmed and unaffirmed students was simply a difference in attention paid to the survey.

C. Course attrition

Finally, we examined whether course attrition explained condition differences in social network integration. The majority of students who dropped the course did so during the first two weeks of the semester during "Add/Drop period" (hence why we scheduled the intervention to occur during the third week of the term, when course enrollment was more stable). Even so, 21 (8 affirmed, 13 control) of the 226 participants included in the present network analyses dropped the course after receiving the intervention. It is possible that differential patterns in course attrition, especially given that fewer affirmed students dropped the course than unaffirmed students, explained the observed differences by condition in social network integration.

To test this alternative explanation, we removed all individuals who dropped the course from the class friendship network, recalculated network centrality measures, and then examined the effects of intervention condition on network metrics only among the individuals who completed the course. Excluding students who dropped the course did not change the pattern of results. Consistent with effects reported in the main text, affirmed students who completed the course had significantly higher closeness centrality (b = 0.008, SE = 0.003, p_{perm} = .027) and degree centrality (b = 0.886, SE = 0.284, p_{perm} = .035) than their unaffirmed counterparts, and effects on betweenness centrality and friendship tie strength were positive but not significant. These results indicate that affirmation led to social gains over the control condition even when excluding those who dropped the course; course attrition did not account for the observed differences in social network integration.

VIII. Mediation Analyses

For analyses (binomial regressions) determining whether the effects of affirmation on students' social networks resulted in positive downstream effects on students' retention in the biology track, we measured whether students enrolled in the second half of the biology course in the subsequent spring semester using the subsequent semester's course roster. This retention data was available for every student in the study sample. Thus, we had retention data from all 290 participants who completed the intervention. Intervention condition significantly predicted whether students took the next biology class in this sample (b = 0.69, SE = 0.29, z = 2.37, p = .02). However, end-of-semester social network data was only available for the 226 "completers." The effect of intervention condition on next-semester biology retention dropped below the threshold for significance when the sample was limited to these 226 participants (b = 0.50, SE = 0.34, z = 1.49, p = .14). We chose to do Bayesian mediation analyses because this estimation procedure allowed us to retain the data from all 290 participants by allowing for missing mediator data. However, frequentist analyses in the sample of 226 yield a similar pattern. Tables S3 and S4 report the Bayesian and frequentist mediation results, respectively. Overall, the analyses show that closeness centrality, total degree centrality, and number and strength of the friendships participants nominated (outdegree and out-tie strength) mediate the relationship between intervention condition and next-semester biology retention.

IX. Alternative explanations for persistence

We examined three alternative explanations for affirmed students' greater likelihood of persisting in the bioscience track aside from their strengthened social network positions.

First, we examined the possibility that course performance, rather than or in addition to social networks, mediated the effect of affirmation on bioscience persistence. If affirmed students performed better in the biology course, this may have explained their greater likelihood of taking the next course in the bioscience sequence. Results showed, however, that intervention condition did not significantly predict course performance, neither among all participants who completed the intervention (b = -0.04, SE = 6.45, $t(271)$ = -0.006, p = .995; 288 students were included in this analysis, as two students were missing data for their total number of points in the course) nor among completers (b = 0.13, SE = 7.16, t(208) = 0.02, p = .985; 225 students were included in this analysis, as one student was missing data for their total number of points in the course). Course performance did, as to be expected, strongly predict taking the next course in the bioscience sequence, but intervention condition continued to predict taking the next course in the bioscience sequence to the same degree, or even a slightly greater degree, when course performance and intervention condition were entered as simultaneous predictors of taking the next bioscience course (among all participants: course performance: b = 0.005, SE = 0.0003, $t(270)$ = 15.50, $p < .001$; intervention condition: b = 0.11, SE = 0.04, $t(270)$ = 2.91, p = .004; among the completers: course performance: b = 0.005, SE = 0.0004, $t(207)$ = 13.65, $p < .001$; intervention condition: b = 0.07, SE = 0.04, $t(207)$ = 1.82, p = .07).

Next, we examined the possibility that affirmation may have boosted either grit or growth mindset, and that perhaps these variables, rather than or in addition to social networks, mediated the effect of affirmation on bioscience persistence. However, intervention condition did not significantly predict grit (b = 0.05, SE = 0.09, $t(207)$ = 0.57, p = .57; 224 students were included in this analysis, as two students were missing data for grit) or growth mindset (b = -0.07, SE = 0.18, $t(209)$ = -0.41, p = .68). Additionally, grit did not significantly predict bioscience persistence (b = 0.004, SE = 0.04, $t(207)$ = 0.09, p = .93), nor did growth mindset (b = 0.02, SE = .02, $t(209)$ = 1.01, p = .32).

X. Gender and Race Subgroup Effects

Affirmation theory suggests that values affirmation interventions should be most effective for those individuals facing the greatest psychological threat in a given environment (*26*). The STEM context may be particularly threatening for students from social groups currently and/or historically marginalized in STEM, such as women, students from underrepresented racial/ethnic minority groups, and first-generation students. Indeed, values affirmation studies in academic settings have often found that students from these groups experience a boost in academic outcomes following affirmation, whereas students from groups not marginalized in academic settings are unaffected (*45*, *55*).

It was unclear whether we should expect similar subgroup patterns in social outcomes in the current study, such as students' friendship networks. Some prior work has found main effects whereby people who were affirmed showed more prosocial or positive relational outcomes (*27*, *33*). In contrast, one study examined the social effects of values affirmation among marginalized versus non-marginalized group members, finding that affirmation specifically insulated African American students' sense of belonging during middle school (*29*). However, most findings related to the social effects of affirmation have focused on groups facing other types of threat that are not academically driven, such as relational threat in individuals with low self-esteem (*30*, *32*) or threat from being confronted with information about others' suffering (*31*) or about the perils of smoking (*27*).

Thus, previously reported findings of the effects of affirmation on academic outcomes suggested that affirmation might similarly have stronger effects on social networks for groups marginalized in STEM, whereas prior research on the social effects of affirmation did not suggest a clear prediction. Nevertheless, we tested whether any effects reported in the main manuscript were moderated by gender and/or by race/ethnicity. We did not run subgroup analyses for first versus continuing generation students because there were few first-generation students in the class (n = 39 total across both conditions).

No significant interactions between intervention condition and gender (see Table S5) or between intervention condition and race (see Table S6) emerged in the current study, neither for social outcomes nor for persistence. There was one marginal interaction in the gender analyses suggesting that the values affirmation intervention may have led to slightly but not significantly bigger gains in incoming tie strength for women compared to men in the class.

Why affirmation benefits were not larger among members of groups marginalized in STEM, as in prior research, is an open question. One possibility is that the social effects of affirmation operate in a different manner from its academic effects; a main effect of affirmation on social outcomes would be consistent with some prior research on the social effects of affirmation mentioned above. However, in that case, we would still have expected larger affirmation benefits for women and members of underrepresented racial groups in persistence and performance.

Another possibility is that the particular STEM context we studied was not uniquely threatening for women and students from underrepresented racial groups, compared to other college STEM settings that have been a subject of affirmation research (e.g., physics (*56*)). Research suggests that there may be variation in stereotype prevalence across STEM fields (*57*). Specifically, stereotypes of women and people from underrepresented racial groups may be more prevalent in fields that believe that raw, innate talent is especially important for success—such as physics and math—than in fields where these ability beliefs are not as prevalent, such as molecular biology (the subject of the course in this study). If this is the case, future research may find greater social benefits of affirmation for women and members of underrepresented minority groups in other STEM contexts.

However, potentially countering this explanation, other research in undergraduate introductory biology classes has found significant moderating effects of race on the effects of values affirmation (*58*). In that work, both White students and students from underrepresented minority groups experienced a boost in biology exam performance from the affirmation intervention, but affirmed students from underrepresented minority groups experienced a larger boost. Thus, it does not appear that affirmation effects should be expected to be uniformly unmoderated by group membership in biology contexts (see also (*55*) which found moderation of affirmation effects in undergraduate biology by first versus continuing-generation status).

Ingroup representation may be another potential explanation, at least with respect to gender. Ingroup representation has been suggested as a moderator of the effects of values affirmation and other social psychological interventions. For example, prior research in a college engineering program has observed benefits of these interventions for women only in male-dominated (< 20% women) engineering fields (*41*). This prior work suggested that male-dominated engineering fields may give rise to a particularly "chilly climate" for women, including a greater likelihood that women will encounter more environmental and social cues that they do not belong, stereotypes that they lack STEM ability, and sexism. In contrast, the majority of students in the biology course examined in this research were women (women comprised 66.8% of the study sample) and one of the instructors of the course was a woman. It is possible that women in this course may have experienced less gender-based threat than women in prior research due to relatively greater ingroup representation and, as a result, did not experience relatively larger benefits of affirmation than men. However, this explanation would not account for the lack of moderation of effects by race, as Black, Latinx, and American Indian students were underrepresented in the class.

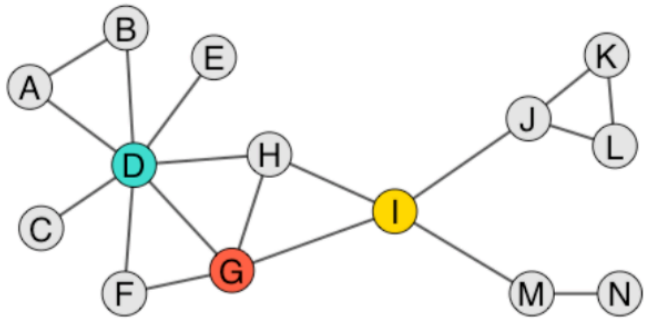
Alternatively, perhaps unique characteristics of this particular course made it especially threatening for all students regardless of race or gender. The course had four very difficult exams with low average scores; for example, the raw mean score on the first exam among all participants was 59.76% (SD: 13.09). Given that this course is the gateway to many students' desired majors and career paths (e.g., bioscience majors, pre-medical track), the difficulty of these exams and prevalence of low scores may have raised the stakes of the class for all students. As a result, everyone could have faced generally high levels of threat, and thus been responsive to the values affirmation intervention.

The central idea of this explanation is illuminated by the Zone Model of Threat (*59*). This model extends the Yerkes-Dodson parabolic relationship between arousal and performance to the context of psychological threat and social psychological interventions in academic settings. The model suggests that threat-reducing interventions (like values affirmation) will have a positive effect on students who are facing higher-than-optimal levels of psychological threat—in essence, that there is a threshold of psychological threat at which people to benefit from threat-reducing interventions (as well as a level of very extreme threat at which people no longer meaningfully benefit from social psychological interventions).

This model could explain why researchers would sometimes expect to see benefits of affirmation only among members of groups marginalized in STEM and sometimes benefits for everyone. In classroom contexts where the primary or only sources of threat are identity-based (e.g., stereotype threat, contingent belonging, group-based exclusion), the Zone Model of Threat would predict that threat-reducing interventions will yield benefits only for members of groups contending with these threats (e.g., women, members of underrepresented racial groups, first-generation students). In a setting where all students experience higher-than-optimal psychological threat—perhaps due to high difficulty and low exam scores challenging their perceived competence, self-regard, and future aspirations, and/or the sense that the demands of the course were far exceeding their personal resources—all students may be expected to benefit. This does not mean that all students experience the same level of threat. In this class, women and/or members of underrepresented groups may have experienced even higher levels of threat than men and/or White and Asian students, contending with identity threat on top of the baseline level of threat all students experienced. However, it is possible that this baseline level of threat was high enough for members of marginalized and non-marginalized groups alike to benefit from threat reduction.

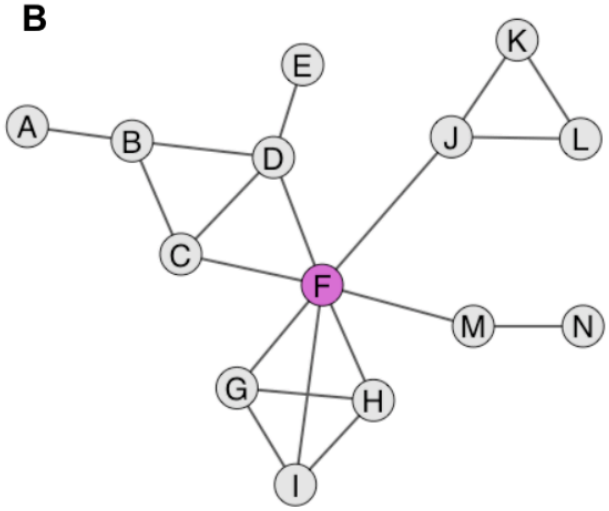
Overall, more research is needed to illuminate the boundary conditions of values affirmation interventions, who experiences the greatest benefits, and when. Future research replicating this experiment in other contexts will shed light on the conditions under which the social and persistence effects of affirmation are experienced broadly by affirmed individuals, and when these effects are moderated by group membership.

A



Student	Closeness	Betweenness	Degree
A	0.342	0.000	2
B	0.342	0.000	2
C	0.333	0.000	1
D	0.481	0.532	**7**
E	0.333	0.000	1
F	0.406	0.000	2
G	**0.520**	0.276	4
H	0.500	0.192	3
I	0.500	**0.590**	4
J	0.382	0.282	3
K	0.289	0.000	2
L	0.289	0.000	2
M	0.361	0.154	2
N	0.271	0.000	1

B



Student	Closeness	Betweenness	Degree
A	0.283	0.000	1
B	0.382	0.154	3
C	0.500	0.115	3
D	0.520	0.269	4
E	0.351	0.000	1
F	**0.650**	**0.782**	**7**
G	0.433	0.000	3
H	0.433	0.000	3
I	0.433	0.000	3
J	0.464	0.282	3
K	0.333	0.000	2
L	0.333	0.000	2
M	0.433	0.154	2
N	0.310	0.000	1

Fig. S1. Illustration of centrality measures. Above are two simple hypothetical friendship networks of 14 students. Circles represent students and lines represent friendships between two students. The OSF repository for this paper includes R code to reproduce these networks and centrality computations.

(**A**) Here, three different students have the highest closeness, betweenness, and degree centralities, respectively. **G** has the highest closeness centrality because she requires the fewest intermediaries on average to reach each of the other students in the network (i.e., she is the *closest* on average to all others in the network). **I** has the highest betweenness centrality because he is positioned on the most direct path *between* the greatest number of his peers. **D** has the highest degree centrality because she has the most direct friendships out of any student in the network.

(**B**) Here, one student, **F**, has the highest closeness, betweenness, and degree centrality. **F** requires the fewest intermediaries on average to reach each of the other students in the network, is positioned on the most direct path between the most other students, and has the most direct friendships out of everyone else in the network.

Note: These example networks are simplified for illustrative purposes, compared to the networks analyzed in this paper. The networks analyzed in this paper take into account the direction of ties (who nominated who) and the strength of ties (the interpersonal closeness ratings participants gave to each friend they nominated). For a more straightforward demonstration, these example networks and associated calculations do not model direction or strength of ties (e.g., total degree

centrality here is simply the number of ties, whereas in a network with directed ties, total degree is equal to the number of ties a participant nominates plus the number of times the participant is nominated by others). Additionally, because these example networks do not contain disconnected components, Freeman's classic form of closeness centrality (*36*) is reported; however, harmonic closeness centrality calculations are also included in the posted R code.

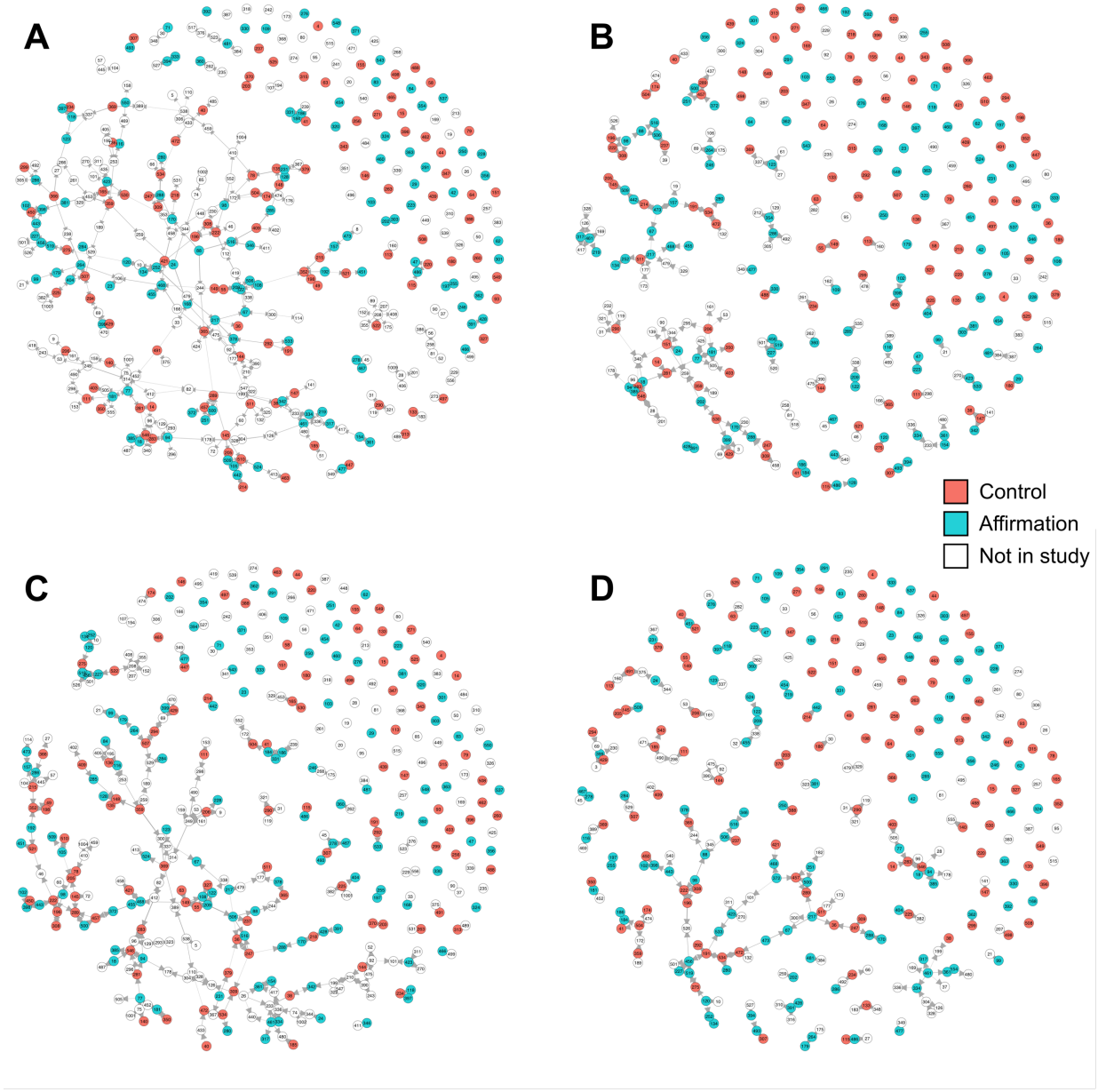


Fig. S2. Study partnership and support networks. Weighted, directed study partnership network at (**A**) the start of the semester (Time 1) and (**B**) the end of the semester (Time 2); weighted, directed support network at (**C**) the start of the semester (Time 1) and (**D**) the end of the semester (Time 2).

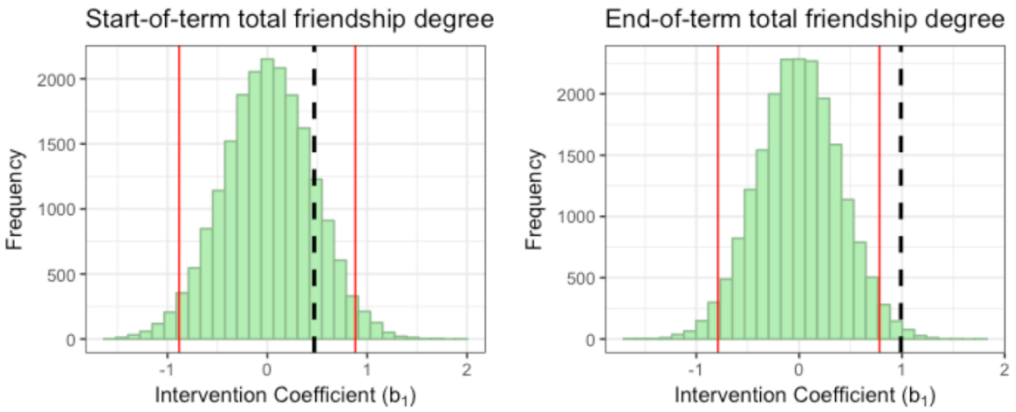


Fig. S3. Permutation tests of significance. The bars of the two histograms display the 20,000 calculated b_1 intervention coefficients from the linear models fitted to the random networks. The red vertical lines correspond to the 2.5 and 97.5 percentiles of the distribution of b_1 coefficients. The black dotted line corresponds to the intervention coefficient from the linear model fitted to the observed network (b_{1obs}). b_{1obs} were 0.47 (start-of-term, left) and 0.99 (end-of-term, right) and p_{perm} values ($|b_1| \geq |b_{1obs}|$) were .30 and .01 respectively.

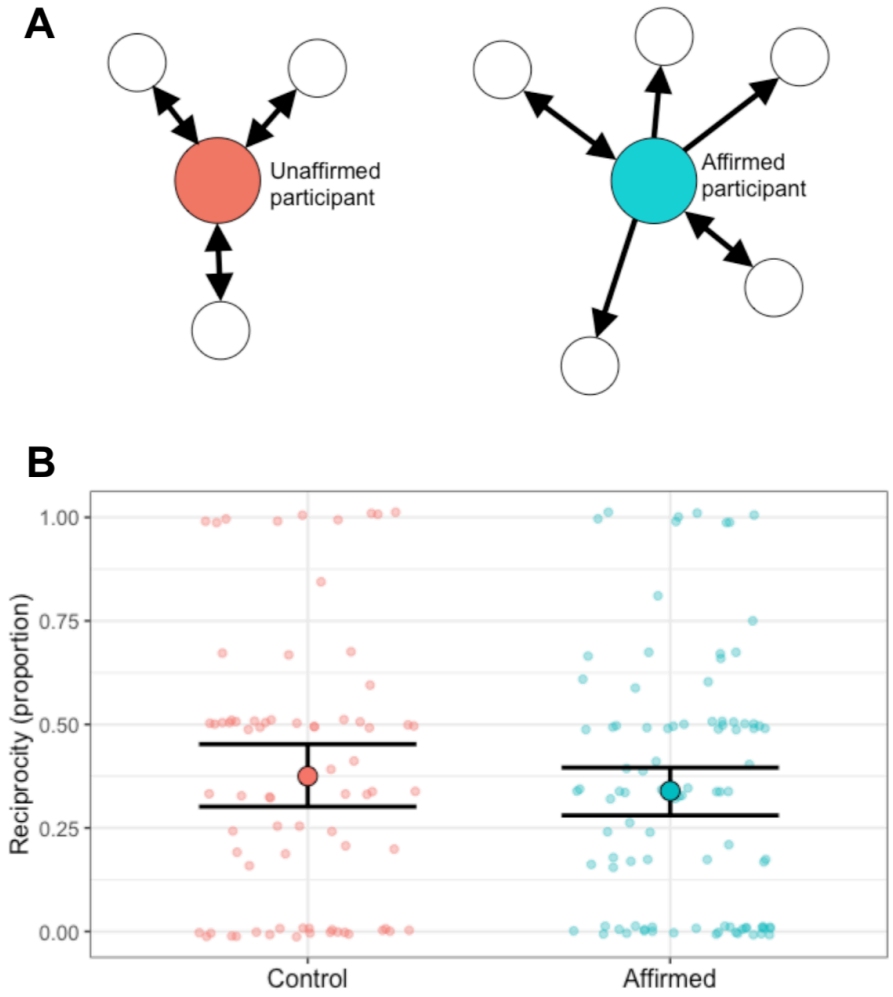


Fig. S4. Test of the "rosy perception" potential explanation. (**A**) Conceptual demonstration of expected results if difference between conditions reflected perception of friends, rather than actual friendships. In this demonstration, the average affirmed participant (right) names five friends, but only two (40%) are reciprocal. In contrast, the average unaffirmed participant (left) names only three friends, but all three (100%) are reciprocal. In this case, these results would suggest that affirmed participants would have more friends than unaffirmed participants, but in reality, unaffirmed participants have a similar number or more reciprocal friendships (which some might consider to be more "true" friendships). (**B**) Actual difference between conditions in friendship reciprocity. There was no significant difference between conditions in friendship reciprocity at the end of the semester. Error bars show bootstrapped 95% confidence intervals. Mean proportion of reciprocal ties at the end of the semester for the control condition was .37; mean for affirmation condition was .34. Past research has found that approximately .35-.65 of reported friendship ties were reciprocal (see (*60*) for a discussion). The reciprocity values in this study are on the low end because participants were allowed to nominate classmates who were not participating in the study (and thus could not nominate the participants back). Calculating reciprocity of ties only within nominations of friends who were participants in the study yields a mean proportion of reciprocal ties of .65 for the control condition and .56 for the affirmation condition, and still no significant differences between conditions (b = -.08, SE = .07, $t(128)$ = -1.25, p = .21).

Table S1. Network statistics by network type and time point.

Network	*N*	*E*	*Iso*	*Deg*	*Weight*	*Den*	*Clust*	*Recip*	*Jaccard*
Friendship									
Time 1	460	855	60	3.72	.25	.004	.22	.40	.32
Time 2	394	629	53	3.19	.21	.004	.23	.35	
Study Partnership									
Time 1	419	475	90	2.27	.20	.003	.26	.38	.22
Time 2	326	226	122	1.39	.10	.002	.36	.32	
Support									
Time 1	392	350	112	1.79	.17	.002	.23	.33	.28
Time 2	326	225	103	1.38	.15	.002	.24	.25	

Note. Time 1 = start-of-semester (baseline/pretreatment); Time 2 = end-of-semester (posttreatment); *N* = number of nodes in network; *E* = number of edges; *Iso* = number of isolates (nodes with no ingoing or outgoing ties); Incl = inclusiveness; *Deg* = average degree (number of others nominated + number of times nominated by others); *Weight* = average weight of ties (strength of relationships), scaled from 0 to 1 for comparison between network types; *Den* = graph density; *Clust* = global clustering coefficient (i.e., transitivity); *Recip* = reciprocity; *Jaccard* = Jaccard index of similarity between networks of same type at times 1 and 2.

Table S2. QAP correlations between networks.

Network	Friendship	Study	Support
Time 1			
Friendship	1		
Study	.76	1	
Support	.72	.74	1
Time 2			
Friendship	1		
Study	.58	1	
Support	.63	.59	1

Table S3. Bayesian mediation results. Table of mediation analysis results using Bayesian estimation (on the full sample of 290 participants who completed the intervention) with 30,000 iterations and uninformative (default) priors *(calculated using Mplus version 7.11 path analysis with a categorical dependent variable and continuous mediating variable with missing data).*

Mediator	Estimate	*SE*	95% CI	$p_{positive\text{-}effect}$	Percent of total effect mediated
Closeness					
Average indirect effect	**0.133**	**0.063**	**[0.035, 0.277]**	**.003**	**31%**
Average direct effect	**0.292**	**0.175**	**[-0.054, 0.637]**	**.048**	
Betweenness					
Average indirect effect	0.000	0.002	[-0.004, 0.004]	.475	0%
Average direct effect	0.396	0.166	[0.074, 0.725]	.008	
Total degree					
Average indirect effect	**0.159**	**0.069**	**[0.051, 0.318]**	**.001**	**40%**
Average direct effect	**0.233**	**0.178**	**[-0.119, 0.583]**	**.095**	
Outdegree					
Average indirect effect	**0.140**	**0.064**	**[0.041, 0.289]**	**.001**	**35%**
Average direct effect	**0.253**	**0.177**	**[-0.095, 0.601]**	**.076**	
Indegree					
Average indirect effect	0.060	0.050	[-0.015, 0.179]	.058	15%
Average direct effect	0.332	0.173	[-0.007, 0.674]	.028	
Total tie strength					
Average indirect effect	0.070	0.051	[-0.009, 0.188]	.043	17%
Average direct effect	0.340	0.172	[0.003, 0.680]	.024	
Out-tie strength					
Average indirect effect	**0.175**	**0.076**	**[0.050, 0.347]**	**.002**	**41%**
Average direct effect	**0.244**	**0.180**	**[-0.111, 0.599]**	**.087**	
In-tie strength					
Average indirect effect	0.026	0.034	[-0.021, 0.111]	.134	6%
Average direct effect	0.386	0.169	[0.055, 0.719]	.010	

Note. Estimate = mean of posterior distribution; *SE* = standard deviation of the posterior distribution; $p_{positive\text{-}effect}$ = posterior distribution density below zero (i.e., the probability that the effect is not above zero, similar to a one-tailed *p*-value). Average estimated total effect = 0.411, *SE* = 0.176, 95% CI [0.069, 0.760], $p_{positive\text{-}effect}$ = .010. (Due to the Bayesian estimation approach, there were slight variations in the estimated total effect across models; thus, the total effect reported here is averaged across the total effect estimates of the eight models reported in Table S3.) Variables bolded in table "significantly" mediated the relationship between intervention condition and next-semester biology retention (i.e., $p_{positive\text{-}effect}$ of indirect effect < 0.05 and 95% CI does not include 0). Analyses presented in table controlled for the baseline measure of the network variable, but results do not differ meaningfully in analyses that do not include this covariate (except that, without including the baseline measure as a control, in-degree could be interpreted as "significantly" mediating the relationship between intervention condition and next-semester biology retention, with $p_{positive\text{-}effect}$ < .05 and a credible interval that does not include 0; results from both sets of analyses are included on the OSF page for this project).

Table S4. Frequentist mediation results. Table of mediation analysis results using frequentist estimation (on the 226 "completers" only) with confidence intervals computed through nonparametric bootstrapping with 10,000 simulations *(calculated using 'mediation' R package version 4.5.0)*.

Mediator	Estimate[a]	95% CI	*p*	Percent of total effect mediated
Closeness				
Average indirect effect	0.033	[0.01, 0.07]	.01	41%
Average direct effect	0.050	[-0.06, 0.15]	.37	
Betweenness				
Average indirect effect	0.000	[-0.01, 0.02]	.96	0%
Average direct effect	0.084	[-0.02, 0.19]	.12	
Total degree				
Average indirect effect	0.044	[0.01, 0.09]	.003	57%
Average direct effect	0.033	[-0.07, 0.13]	.547	
Outdegree				
Average indirect effect	0.041	[0.01, 0.08]	.002	54%
Average direct effect	0.035	[-0.07, 0.14]	.505	
Indegree				
Average indirect effect	0.013	[-0.01, 0.04]	.27	17%
Average direct effect	0.063	[-0.04, 0.16]	.23	
Total tie strength				
Average indirect effect	0.013	[-0.01, 0.04]	.28	17%
Average direct effect	0.062	[-0.04, 0.17]	.24	
Out-tie strength				
Average indirect effect	0.036	[0.003, 0.08]	.03	51%
Average direct effect	0.035	[-0.06, 0.14]	.47	
In-tie strength				
Average indirect effect	0.004	[-0.01, 0.02]	.54	6%
Average direct effect	0.075	[-0.03, 0.18]	.15	

Note. Estimated total effect (standardized estimate) = 0.079, 95% CI [-0.02, 0.19], $p = .13$.
As above, analyses presented in table controlled for the baseline measure of the network variable; results did not differ meaningfully in analyses that did not include this covariate (results from both sets of analyses are included on the OSF page for this project).
[a]Standardized estimates

Table S5. Gender x intervention interactions. Means and standard errors for women and men in the affirmation and control conditions, plus the *p*-value derived from permutation tests indicating significance of the gender x condition interaction.

Dependent variable	Affirmed		Control		Gender x Condition
	Women (N=79)	Men (N=37)	Women (N=72)	Men (N=35)	p_{perm}
Closeness	0.05 (0.003)	0.04 (0.005)	0.04 (0.003)	0.04 (0.004)	0.16
Betweenness	0.0018 (0.0005)	0.0022 (0.0009)	0.0014 (0.0005)	0.0019 (0.0007)	0.63
Total degree	4.85 (0.35)	4.35 (0.62)	3.64 (0.36)	3.63 (0.54)	0.33
Outdegree	3.08 (0.24)	2.70 (0.39)	2.35 (0.25)	2.14 (0.36)	0.50
Indegree	1.77 (0.18)	1.65 (0.31)	1.29 (0.16)	1.49 (0.24)	0.42
Total tie strength	2.95 (0.14)	2.52 (0.28)	2.62 (0.19)	2.80 (0.26)	0.19
Out-tie strength	2.78 (0.17)	2.35 (0.32)	2.32 (0.20)	2.18 (0.30)	0.49
In-tie strength	2.47 (0.20)	1.91 (0.30)	2.18 (0.23)	2.40 (0.30)	0.07
Next-semester persistence	0.84 (0.04)	0.86 (0.06)	0.76 (0.05)	0.74 (0.07)	0.63[a]

[a]*p*-value from standard binomial regression model as next-semester persistence does not violate assumption of independence of observations

Table S6. Race x intervention interactions. Means and standard errors for students belonging to racial/ethnic groups marginalized in STEM (Black, Latinx, and American Indian students) and students belonging to racial/ethnic groups not marginalized in STEM (White, Asian) in the affirmation and control conditions, plus the *p*-value derived from permutation tests indicating significance of the race group (marginalized/non-marginalized) x condition interaction.

Dependent variable	Affirmed		Control		Race group x Condition p_{perm}
	Marginalized (N=33)	Non-marginalized (N=85)	Marginalized (N=32)	Non-marginalized (N=76)	
Closeness	0.04 (0.005)	0.05 (0.003)	0.04 (0.005)	0.04 (0.003)	0.58
Betweenness	0.0018 (0.0008)	0.0020 (0.0005)	0.0009 (0.0003)	0.0018 (0.0005)	0.79
Total degree	3.79 (0.52)	4.99 (0.37)	3.25 (0.43)	3.75 (0.38)	0.48
Outdegree	2.58 (0.36)	3.08 (0.24)	2.06 (0.33)	2.34 (0.26)	0.81
Indegree	1.21 (0.23)	1.91 (0.19)	1.19 (0.20)	1.41 (0.17)	0.27
Total tie strength	2.58 (0.26)	2.93 (0.15)	2.67 (0.29)	2.65 (0.19)	0.86
Out-tie strength	2.34 (0.27)	2.79 (0.18)	2.32 (0.30)	2.23 (0.20)	0.63
In-tie strength	2.05 (0.32)	2.37 (0.19)	2.15 (0.33)	2.26 (0.22)	0.76
Next-semester persistence	0.67 (0.08)	0.91 (0.03)	0.66 (0.09)	0.80 (0.05)	0.27[a]

Note. Students who identified as multiracial were categorized as belonging to racial/ethnic groups marginalized in STEM if they indicated that at least one of their racial/ethnic groups was Black, Latinx, or American Indian; multiracial students were categorized as belonging to racial/ethnic groups not marginalized in STEM if they were exclusively White and/or Asian. The six students who reported other races or declined to report their race were categorized as belonging to marginalized groups based on the racial/ethnic description they provided if available (e.g., students who reported that they were Persian or Middle Eastern were categorized as non-marginalized in STEM), or otherwise by coding facial appearance on the course roster.

[a]*p*-value from standard binomial regression model as next-semester persistence does not violate assumption of independence of observations

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