

Bridging the empathy perception gap fosters social connection

Received: 5 October 2024

Accepted: 13 August 2025

Published online: 16 October 2025

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Young adults face a rising tide of mental illness and loneliness. We propose that an overlooked barrier for social connection is how people perceive each other's empathy. Here, our longitudinal study of an undergraduate student community ($N = 5,192$) reveals that undergraduates who perceive their peers as empathic report better current and future well-being. Yet we document an 'empathy perception gap': people systematically see others as less empathic than others see themselves. Students who perceived their peers as less empathic were less willing to take social risks and grew more isolated over time. To disrupt this cycle, we conducted two field experiments that presented students with data on their peers' self-reported empathy and behavioural nudges to encourage social risk taking. These interventions reduced the empathy perception gap, increased social behaviours and expanded social networks months later. This work offers a promising, scalable strategy to cultivate social well-being, simply by presenting people with data about each other.

Adolescents and young adults face a mental health crisis. Historically, young adulthood marked one of the happiest stages in life¹, but this trend has reversed, as young people in the USA, UK and Canada now report the lowest happiness among all age groups^{2,3}. Young adults also suffer a rising tide of depression, anxiety and even suicide³⁻⁵.

Social connection can help address these issues—when people feel supported, they are less likely to suffer mental health challenges^{6,7}, are more resilient in the face of stress⁸⁻¹⁰, enjoy better physical health¹¹ and live longer lives^{12,13}. Yet, as mental health has deteriorated among young adults, their levels of loneliness have surged¹⁴. On the basis of a meta-analysis including 345 studies conducted worldwide, feelings of loneliness were estimated to have increased by 9.19% in the past four decades, representing an average of 0.22% per year¹⁴. At this rate, an estimated 3 million additional young adults will report feeling lonely in 2026. In a sad irony, young people experience increasing distress while also feeling more disconnected from the support networks that could help them.

This rise in loneliness is probably the result of many factors at different levels, including urbanization¹⁵, changes in family structures¹⁶,

shifts in social norms^{17,18}, an increase in individualism¹⁹ and the impacts of technology²⁰ (for a more detailed discussion, see Supplementary Information). To address these concerning trends, policy makers, educators and researchers have attempted to enhance social connection among young adults (for reviews, see refs. 21,22). These interventions promote positive social interaction using strategies such as smartphone delivered behavioural nudges²³, social support groups²⁴ and social skills training²⁵. Meta-analyses find that these strategies are generally effective at reducing young adult loneliness²¹. However, many also involve in-person components (for example, social support groups and social skill courses) that are expensive to scale. As such, there is an urgent need for scalable approaches to foster connection among young adults.

Here, we propose to complement existing strategies by targeting social behaviour indirectly, via social perceptions. People may avoid social interactions because they hold miscalibrated perceptions of themselves and others. For instance, people systematically underestimate how fulfilled and positive they will feel after talking with strangers²⁶ and deep conversations with friends²⁷. They also underestimate

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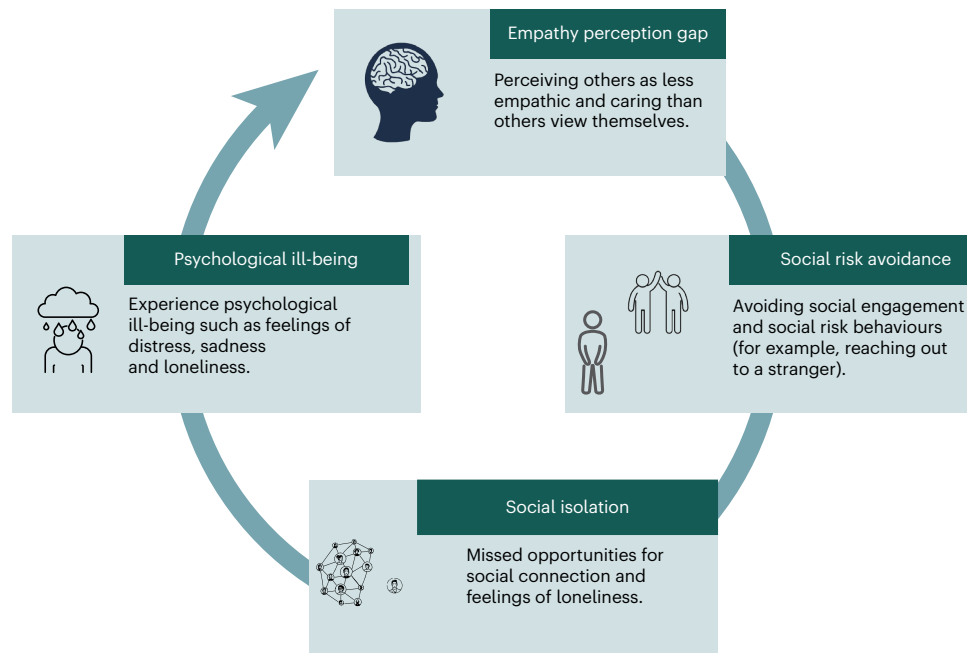


Fig. 1 | The vicious cycle of empathy misperception. A conceptual figure illustrating the associations among empathy perception gap, social risk avoidance, social isolation and psychological ill-being.

how positively others will react when they express gratitude²⁸ or ask for help²⁹. Although these pessimistic perceptions are observed in the general population, they may be exacerbated among individuals experiencing mental illness, social anxiety or loneliness, who often perceive the social world as more hostile and expect more negative social interactions¹⁸.

These misperceptions do not remain confined to thoughts; they affect people's choices to engage—and especially avoid—others^{30,31}. Consider social risk behaviours, acts with uncertain outcomes in the social domain, such as striking up a conversation with a stranger or sharing vulnerabilities with a friend³². Any risky decision requires people to assess the likelihood that their choice will pay off. In the social domain, misperceptions skew that calculus, leading people to be unnecessarily risk averse. Individuals who underestimate how pleasant conversations with strangers can be are less likely to initiate them²⁶ and people who incorrectly assume others do not want to help them are less likely to reach out for help²⁹.

Misperceptions thus interfere with social life. Here, we theorize that these previously observed misperceptions across different situations may, at least in part, stem from people's lay theories about each other. In particular, people may hold low expectations about others' social qualities. We propose that individuals might imagine their peers are less empathic than peers perceive themselves, and that this empathy perception gap might leave individuals socially risk averse and ultimately more isolated.

Empathy—the ability to share, understand and care about others' experiences—is socially magnetic³³. Failing to recognize others' empathy (for example, believing that no one would offer help during difficult moments) can create obstacles to engaging in social risk behaviours, leading to missed opportunities to benefit from the support and care available to us. People who withdraw from social interaction might in turn deepen their negative views of others³⁰, creating a vicious cycle of empathy misperception (Fig. 1).

This perspective sheds light on why young people feel increasingly isolated and offers testable approaches to address this problem. When individuals perceive those around them as empathic, they are more likely to connect with strangers³⁴ and seek social support^{35,36}. These social risk behaviours can help build meaningful social connections,

expand social networks and benefit mental health³⁷. Moreover, engagement in social interactions might reinforce positive perceptions of others' empathy, creating a virtuous cycle of empathy perception and social connection. While our model highlights one direction of this process, we view it as part of a broader, dynamic system in which social perceptions and mental health mutually influence one another.

Here, we test a perception-based model of social connection through three studies within the Stanford Communities Project (SCP). Since 2018, the SCP has continually assessed thousands of undergraduate students multiple times a year, gathering data on personality traits, psychological well-being, social networks and ecological momentary assessments including measurement of social activity. The SCP provides means through which to examine perceptions, social behaviour and psychological well-being in a young adult population.

Study 1 draws on longitudinal survey and social network data from SCP between 2022 and 2024, revealing that students who initially perceived their peers as empathic also reported better psychological well-being both immediately and months later ($N = 5,084$ unique participants and $N = 14,849$ total observations).

Study 2 explores whether individuals' perceptions of their peers' empathy mapped onto their peers' self-reports. We approached this research question by quantifying people's evaluations of others' empathy and supportiveness, and compared these evaluations with the aggregated, self-reported ratings of community members. Our results revealed an 'empathy perception gap', where people perceived others as less empathic and caring than those others perceived themselves. At an individual level, students who hold wider empathy perception gaps are more likely to avoid social risks and become more lonely over time.

Study 3 consists of two field experiments in which we test whether we can 'disrupt' the cycle of empathy misperception through a combination of a social norms campaign and smartphone-delivered behavioural nudges (total $N = 731$ participants). The social norms campaign exposed students to accurate information about their peers' self-reported empathy (for example, "95% of undergraduate students are likely to help others who are feeling down") and the behavioural nudges encouraged students to take small social risks to help build social connections (for example, reach out to a classmate they have not talked to before). The results showed that these intervention

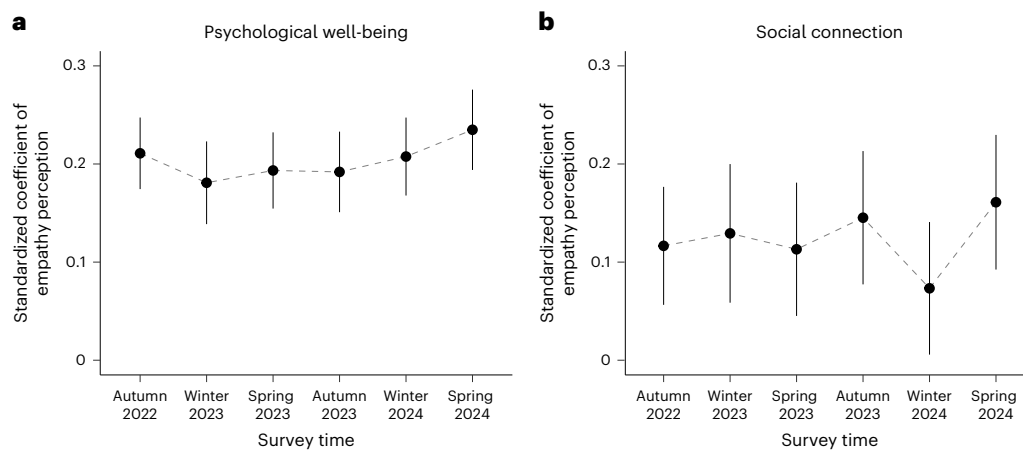


Fig. 2 | Empathy perception is associated with well-being and social connectedness. a, b, Empathy perception predicting psychological well-being (a) and social connectedness (b) across all sample waves ($N = 5,084$). Associations were evaluated using OLS regression at each sample wave. All statistical tests

were two sided, and no adjustments were made for multiple comparisons. Error bars represent 95% CIs around the mean coefficient estimates. All coefficients were significant at $P < 0.001$.

methods significantly reduced the empathy perception gap, increased social risk taking and led to more friendships in the months following the intervention.

Results

Study 1: the role of empathy perception in social networks and well-being

Study 1 tests the relationship between empathy perceptions and well-being using longitudinal survey and social network data from the SCP collected between October 2022 and May 2024 (5,084 total participants, 14,849 total observations). This includes six sample time-points, three per academic year (for details, see Methods). At each sample, all undergraduate students were invited to complete the study. Thus, some participants contributed repeated observations over time. Study 1 analyses were first conducted using data from the 2022–2023 academic year. We then preregistered and successfully replicated these findings using data from the 2023–2024 academic year (preregistration at <https://osf.io/ez8gw>).

Our primary independent variable of interest is students' empathy perception, measured through self-report questions such as "If someone else is feeling bad, how likely are Stanford students to try and help them feel better by listening to them and acting kindly?". Our main dependent variables are psychological well-being and social connectedness. Consistent with a prior study using data from the SCP³⁷, psychological well-being was extracted from a factor analysis and combines several measures from an in-depth survey, including satisfaction with life, subjective happiness, depression (negative loadings), anxiety (negative loadings) and loneliness (negative loadings). Social connectedness was operationalized as the number of people participants reported in their network of close friends. Well-being and social connectedness were weakly but significantly correlated across the waves of our data (Extended Data Table 1).

Overall, students who viewed their peers as more empathic also reported significantly higher psychological well-being across six sample waves (autumn 2022 to spring 2024; $r = 0.18$ – 0.23 , all $P < 0.001$; Fig. 2a and Extended Data Table 1). The results from a multilevel mixed-effect model across all samples show that a 1 s.d. increase in empathy perception maps onto an average of 0.20 s.d. increase in reported well-being. Regarding social connectedness, students who reported higher empathy perception also nominated significantly more people as close friends across all sample waves (autumn 2022 to spring 2024; $r = 0.08$ – 0.13 , all $P < 0.001$; Fig. 2b and Extended Data Table 1). On average, participants nominated 4.60

students as their close friends (range (1, 6), s.d. 1.73). A 1 s.d. increase in empathy perception is estimated to map onto 0.21 additional people that participants nominated as close friends.

We conducted preregistered follow-up analyses to test the associations between empathy perception and the outcome variables (psychological well-being and social connectedness) after controlling for a wide range of variables including individual traits, social behaviours, social network characteristics and academic performance. The effects of empathy perception on well-being as well as social connectedness remained significant after controlling for these factors (Extended Data Fig. 1).

We next examined the longitudinal associations between empathy perception in the previous academic term and well-being and social connectedness in the subsequent term. Consistent with our preregistered hypothesis, after controlling for baseline well-being and demographics, students who viewed others as more empathic generally reported greater well-being several months later (Extended Data Table 2 and Extended Data Fig. 2a). Baseline empathy perception was significantly associated with later well-being in all sample waves except for data collected in autumn 2023. On the other hand, baseline empathy perception did not robustly predict social connectedness in the next academic term across sample waves (Extended Data Table 2 and Extended Data Fig. 2b). For parallel results using a cross-lagged panel analysis, see Supplementary Information.

Together, these findings highlight an association between empathy perceptions on the one hand, and psychological well-being and social connectedness on the other hand. Although this study is not designed for causal inference, empathy perceptions prospectively predict psychological well-being months later, offering intriguing suggestive evidence that people who view their community as empathic feel better over time.

Study 2: the empathy perception gap

Study 2 examined whether students' perceptions of empathy among their peers matched their peers' self-report at an aggregate level. This set of analyses utilized survey data collected as part of the SCP from the winter term of 2023 to spring term of 2024 (five sample points, $N = 4,678$ unique participants, $N = 12,083$ total observations). At each sample point, undergraduate students reported how they and "the average Stanford student" would respond under specific circumstances. As we recruited a substantial proportion of the entire undergraduate student body at each sample point (mean recruitment 32.97%, range (29.66%, 40.40%)), the aggregated self-reported responses of participants'



Fig. 3 | Gaps in empathy perception between perceived empathy of others and aggregated self-reported ratings from community members. a, Gaps in empathy perception at each sample point ($N_{\text{winter 2023}} = 2,336$, $N_{\text{spring 2023}} = 2,628$, $N_{\text{autumn 2023}} = 2,451$, $N_{\text{winter 2024}} = 2,384$, $N_{\text{spring 2024}} = 2,284$). Violin plots depict the distribution of data. Box plots within the violins indicate the median (centre line), the interquartile range (bounds of the box) and whiskers extending to 1.5 times

the interquartile range; data points beyond the whiskers are shown as outliers. **b,** The mean perceived empathy of others and mean self-reported empathy across different scenarios assessed in the scale ($N = 4,678$). Error bars represent 95% CIs. Raw data for **b** were binary and are not shown. Statistical comparisons between perceived and self-reported empathy were conducted using two-sided paired *t*-tests. All tests in **a** and **b** were statistically significant at $P < 0.001$.

own tendencies served as a proxy measure of the actual norms of the student body.

Across all sample waves, students perceived their peers as an average of 23.9% less empathic than peers perceived themselves (all $P < 0.001$; Fig. 3a). For instance, participants estimated that 86.8% of Stanford students would “act kindly by helping others who are feeling bad”, whereas 95.5% of Stanford students responded positively to the same question (Fig. 3b), indicating a 8.7% empathy perception gap on this measure. Empathy perceptions were robust across gender, race and class year. Individuals with higher anxiety, depression and loneliness reported larger empathy perception gaps, but these perception gaps were significantly observed among individuals low in these traits as well (Supplementary Fig. 2). Together, these results reveal a systematic empathy perception gap, such that students tend to view other students as less empathic and caring than others view themselves.

This gap can be explained by several factors. First, students might not realize how empathic their peers truly are. Second, students might self-enhance when they respond to our questions, overestimating how empathic they themselves are³⁸. Study 3 further explores this question. Through a field intervention, we found that people increased their empathy perception after given the opportunity to learn about the empathy in others, demonstrating that this observed gap may at least be partially explained by underestimates rather than self-enhancement.

We also examined the cross-sectional and longitudinal associations between the empathy perception gap and social outcomes including social risk behaviours and loneliness at the individual level. As this set of analyses was exploratory, we controlled for multiple comparisons using false discovery rate correction³⁹. The social risk-taking measure assessed people’s tendency to engage in social behaviours with uncertain outcomes, such as socializing with strangers, sharing vulnerabilities and disagreeing with others (Methods). At an individual level,

the empathy perception gap was negatively associated with social risk taking both concurrently ($\beta = -0.16$, 95% confidence interval (CI) -0.18 to -0.14 , $t(14,721) = -17.61$, $P_{\text{adjusted}} < 0.001$) and longitudinally ($\beta = -0.03$, 95% CI -0.06 to -0.01 , $t(3,938) = -2.95$, $P_{\text{adjusted}} = 0.03$). Similarly, the empathy perception gap was positively associated with loneliness both concurrently ($\beta = 0.08$, 95% CI 0.07 to 0.09 , $t(14,455) = 15.68$, $P_{\text{adjusted}} < 0.001$) and longitudinally ($\beta = 0.04$, 95% CI 0.03 to 0.05 , $t(8,308) = 7.38$, $P_{\text{adjusted}} < 0.001$). When students perceive their peers as less empathic and caring, they are less likely to engage in social risk behaviours (for example, talking to strangers or sharing vulnerabilities) and report feeling slightly lonelier. These results indicate that empathy perceptions are not only passive reflections of the social world, they are also significant predictors of students’ social behaviours.

Study 3: promoting social connection through field experiments

Empathy perception reliably tracks well-being (Study 1), yet people systematically view others as less empathic than others view themselves (Study 2). In Study 3, we report two preregistered field experiments that test (1) whether a simple intervention can shift empathy perceptions and (2) whether this change could promote social connectedness and well-being among students. Both experiments involved first-year undergraduates at Stanford University. We chose to focus on first-year students because they are in a transitional period in which new social bonds are crucial⁴⁰. Additionally, they are still learning about their community, possibly making their community perceptions relatively more malleable⁴¹.

Study 3a. Study 3a (conducted between October 2022 and February 2023, preregistered at <https://osf.io/uzr6n>) explored whether we could shift students’ empathy perceptions through a combination of a social

norms campaign and an in-person educational workshop, both of which were carried out at the dorm level. All incoming first-year students were randomized into an experimental or control condition based on the dorm they live in (Methods). Eight weeks into the autumn term, we displayed a series of posters in experimental dorms. These posters (Fig. 4a) featured real statistics about students' self-reported social qualities, based on data we gathered from the previous term. For instance, one poster read "95% of Stanford students are likely to help others who are feeling down" another read "85% of Stanford students enjoy meeting and becoming friends with students they did not know". The control condition dorms were not exposed to these posters.

The in-person educational workshop was administered as part of a course named 'Frosh 101' at Stanford University. Offered only to first-year students, Frosh 101 is a course designed to help new students adjust to college life. About 50% of first-year students enroll in this course each year. Frosh 101 is split into small sections, and students who reside in the same dorm attend the same section, allowing us to deliver different course materials to students at the dorm level. Students from experimental dorms who enrolled in this course received a 1-h workshop that educated them about their peers' empathy and social goals through a series of interactive exercises and reflections (for course details, see Supplementary Information). Those enrolled in the Frosh 101 who resided in dorms in the control condition attended a 1-h workshop on handling difficult times during college.

We evaluated the outcomes of these interventions at two timepoints: at the end of the autumn term (T1) and at the start of the winter term (T2). At both timepoints, all first-year students were invited to respond to a survey that assessed their empathy perception, social risk behaviours and time spent socializing. Recruitment numbers at T2 ($N_{\text{experimental}} = 74$, $N_{\text{control}} = 161$) differed significantly between the two conditions due to project design considerations not relevant to the current study; however, the groups did not differ on demographics, baseline empathy perception or baseline social risk taking (Methods and Supplementary Information).

At T1, which happened approximately 2 weeks after the in-person workshops, we observed no significant differences in the outcome variables between the experimental and control conditions (all $P > 0.05$; $N_{\text{experimental}} = 165$, $N_{\text{control}} = 175$). However, by T2 (about 3 months after the in-person workshop), participants in the experimental condition perceived their peers as significantly more empathic ($\beta = 0.28$, 95% CI 0.06 to 0.50, $t(218) = 2.46$, $P = 0.01$; Fig. 4b). On average, students from the experimental condition underestimated their peers' empathy by 0.11 points on a 7-point scale, whereas control group participants underestimated empathy by 0.40. Compared with participants in the control condition, perception ratings from experimental group participants were 72.79% closer to their peers' self-reports. Students from experimental dorms also reported engaging in social risk behaviours significantly more frequently (standardized $\beta = 0.39$, 95% CI 0.07 to 0.70, $t(219) = 2.44$, $P = 0.02$; Fig. 4b). On the other hand, we did not observe a significant difference in the amount of time spent socializing between the two conditions ($\beta = 0.15$, 95% CI -0.08 to 0.38, $t(219) = 1.32$, $P = 0.19$). While our intervention did not increase the amount of time students spent socializing, it shifted how students chose to socialize, with participants in the experimental condition engaging in significantly more social risk behaviours. For a comparison between these two measures, see Supplementary Information.

Study 3b. The results from Study 3a showed that we could effectively shift students' empathy perceptions by presenting students with real data about their peers' self-reports. In addition to directly informing students about population statistics, people are also likely to shift their perceptions through engaging in social risk behaviours (that is, activities with uncertain social outcomes) and receiving feedback directly from their peers. As such, Study 3b explored whether our intervention could be enhanced by adding smartphone-delivered

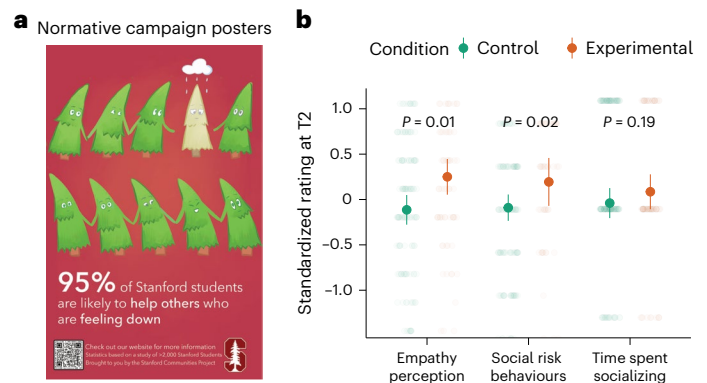


Fig. 4 | Using normative campaign posters and in-person workshops to shift students' empathy perception and social risk behaviours. **a**, An example poster used in the intervention. **b**, The effects of the intervention on outcome variables at T2 ($N = 235$). Intervention effects in **b** were estimated using OLS regressions, with experimental condition specified as a predictor and adjusting for age, gender and race. The error bars in **b** represent 95% CIs.

behavioural nudges alongside the social norms campaign to encourage behaviour change. We theorize that these behavioural nudges can provide opportunities to help participants better 'calibrate' their empathy perceptions through directly engaging in social risk behaviours. We also expanded our outcome measures to include not only self-reported empathy, but also momentary changes in social risk behaviour, momentary positive affect, social network metrics and behavioural sign-ups for a social event.

Study 3b (preregistered at <https://osf.io/sqwy4>) was conducted between October 2023 and May 2024. We used a 2×2 between-subject experimental design where all first-year students at Stanford University were randomized into one of the following four conditions: control (no treatment, $N = 150$), social norms ($N = 82$), behavioural nudge ($N = 63$) and combined (both social norms and behavioural nudges, $N = 96$). As in Study 3a, randomization occurred at the dorm level.

We originally adopted this design as it allowed us to compare the relative strength of social norms intervention and behavioural nudges. However, given the limited sample size in each condition, we were underpowered to detect effects of each treatment condition. For example, a post hoc power analysis indicates that we need 129 participants per condition to have sufficient power (80%) to detect a moderate effect size of $d = 0.35$ at $\alpha = 0.05$ (for a power sensitivity analysis, see Supplementary Information). As such, we collapsed the three intervention conditions into one overall experimental condition for analyses in the main manuscript (consistent with the procedures outlined in the preregistration). That said, exploratory regressions do suggest that social norms and behavioural nudges contribute unique variances to our outcomes of interest. For the results for each condition separately, see Supplementary Information.

In the autumn of 2023, all first-year students were invited to take part in an experience sampling study, in which they received 'pings' on their smartphone four times a day for 3 weeks. These pings served as assessments of momentary affect and social risk behaviour. The first 2 weeks of the experience sampling period also served as an intervention period. During this time, participants in the experimental condition also received daily intervention messages along with the assessment questions. Specifically, students in the social norms condition received questions assessing their empathy perception of other university students (Fig. 5a), and received feedback about actual community norms observed in our studies. For instance, one of the questions asked: "Do you agree with the following statement: more than 95% of Stanford students are likely to help others who are feeling down." For students who selected 'yes', they received a text affirming that their perception mapped onto the real statistics we collected from the Stanford

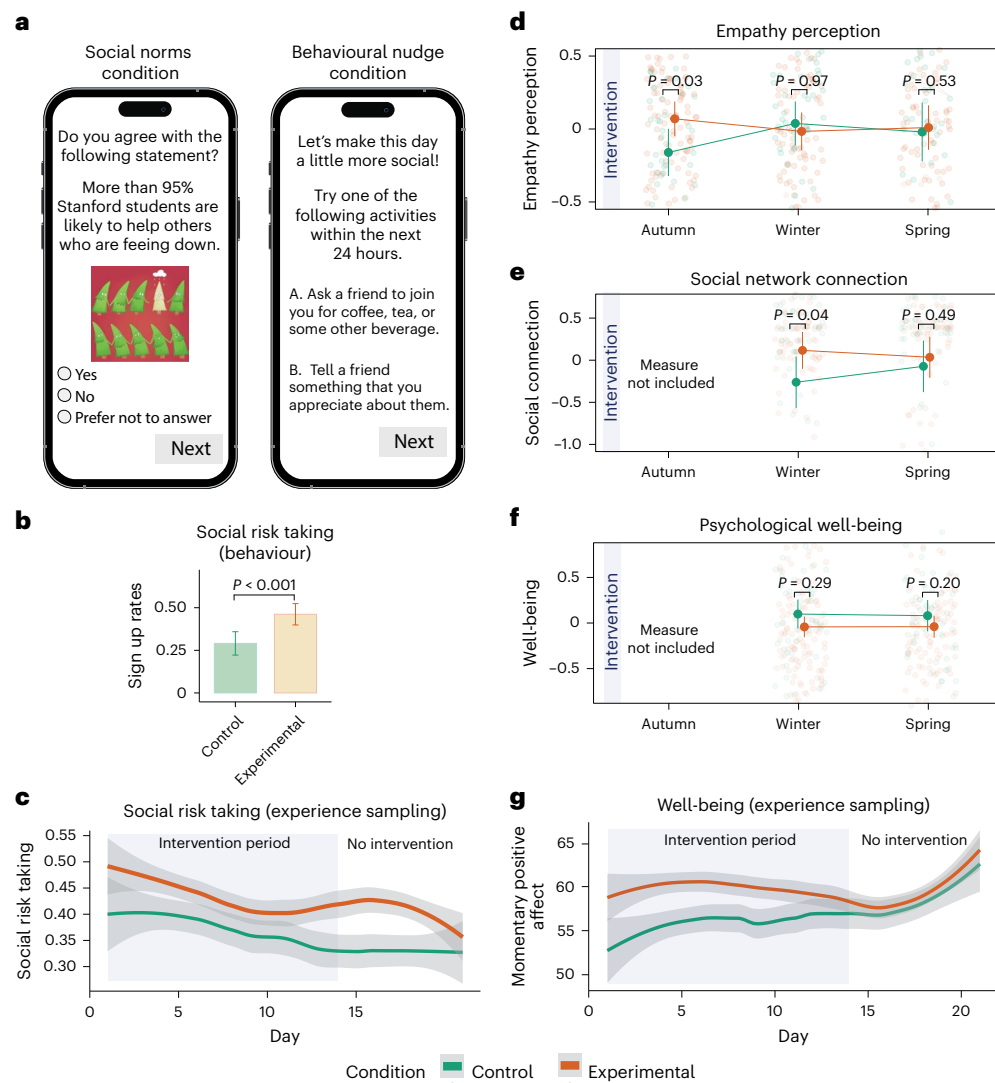


Fig. 5 | Intervention effects on social risk behaviours, empathy perception, social connectedness and well-being. **a–g**, The effects of the intervention in Study 3b (**a**) ($N = 391$) on social risk behaviours (sign-up rates for a social event (**b**) and momentary social risk behaviour (**c**), empathy perception (**d**), social connectedness (**e**) and psychological well-being (long-term well-being (**f**) and momentary well-being (**g**)). The P value in **b** corresponds to a two-proportion

z -test comparing intervention and control groups. Raw data in **b** were binary and are not shown. Intervention effects in **d–g** were estimated using OLS regression models, with the experimental condition specified as a predictor and adjusting for age, gender and race. Social network and well-being measures were not assessed during the autumn term. All tests were two sided and no adjustments were made for multiple comparisons.

community. For students who selected ‘no’, they received the following text: “Not quite right. But good news, students are more empathic than you thought! In fact, in our past research with Stanford undergraduates, we find that 95% of students report being willing to help others who are feeling down. We also find that the majority of students actually underestimate the kindness of their peers!” Similar to Study 3a, we displayed a series of posters in dorms for both the social norms and combined conditions. These posters showed real statistics about students’ self-reported social qualities. These posters were put up before the experience sampling study and remained throughout the school year.

Participants in the behavioural nudge and combined conditions received behavioural nudges that encourage them to engage in social risk taking. For instance, one nudge read “Compliment a person you don’t know yet” (Fig. 5a). Given the importance of setting implementation intentions⁴², each behavioural nudge was followed by interactive questions that prompted participants to plan out when and where they would carry out this behaviour. Participants in the combined condition received both treatments and control condition participants did not receive any normative messages or behavioural nudges.

We evaluated the outcomes of the intervention in four aspects: empathy perception, social risk taking, social network metrics and psychological well-being (see Table 1 for definitions of these variables). First, empathy perception was assessed at four timepoints throughout the school year: baseline (in the beginning of the fall term before the intervention), autumn term, winter term and spring term. Consistent with our preregistered hypothesis, we observed a small shift in empathy perception in the autumn term (2–3 weeks after the intervention; $\beta = 0.25$, 95% CI 0.02 to 0.47, $t(193) = 2.19$, $P = 0.03$; Fig. 5d). On average, those in the experimental condition perceived their peers to be about 5% more empathic compared with participants in the control condition. We did not observe significant differences between the two conditions in the winter term (4 months after the intervention) or the spring term (6 months after the intervention).

We next examined whether our intervention shifted students’ social risk behaviour. Social risk taking was measured in two ways: (1) we measured participants’ momentary social risk behaviours during the experience sampling study and (2) to complement this self-report measure, we assessed participants’ likelihood to sign up for a social

Table 1 | Measures included in Study 3b

EMA	
Social risk behaviours	In the past hour, have you done any of the following? Select all that apply. –Open up to other students when you are struggling. –Initiate an activity/conversation with a friend (for example, ask a friend to grab lunch). –Go to a social gathering. –Initiate a conversation with another student that you didn't already know. –Share bad news with friends. –Voice an honest opinion that you believed in. –None of the above This measure is represented as a binary variable, where it is coded as 1 if at least one social risk behaviour is reported and 0 if none are reported.
Momentary happiness	Please use the slider bar to indicate how you feel right now (0, not at all happy; 100, extremely happy).
Self-reported measures	
Empathy perception	Same as Study 1.
Social risk behaviours	Same as Study 2.
Psychological well-being	Same as Study 1.
Social network measures	
Social connectedness	Social connectedness is defined as the number of people that participants nominate in response to the following question: "Who are your closest friends?". Participants could nominate up to six peers. Note that this measure was named 'social connection' in the preregistration.

event aimed at meeting new students. First, we examined the effect of intervention on social risk taking in the experience sampling data. As predicted, the intervention significantly increased social risk taking during the 2-week intervention period: on average, participants in the experimental condition had 39% higher odds to report engaging in social risk taking compared with control condition participants (odds ratio (OR) 1.39, 95% CI 1.05 to 1.83, $P = 0.02$; Fig. 5c). Importantly, this difference widened over time. During week 3, even though participants in the experimental conditions no longer received intervention messages, they had 63% higher odds to report engaging in social risk taking compared with control condition participants in week 3 (OR 1.63, 95% CI 1.06 to 2.52, $P = 0.03$; Fig. 5c).

To complement the experience sampling social risk assessment, we also included an additional behavioural measure of willingness to take social risks. This was assessed using the sign-up rate for a social activity hosted by our research team. In the winter term (3 months after the intervention), the research team invited all participants across conditions for an in-person activity aimed at connecting students who do not previously know each other. Consistent with our hypothesis, students in the experimental group had 1.91 times the odds of signing up for a social activity that involved talking with strangers compared with control group participants (OR 1.91, 95% CI 1.25 to 2.94, $N = 391$, $P = 0.003$; Fig. 5b). On average, 46.2% of experimental group participants signed up for the in-person activity, compared with 29.1% of the control group participants.

We also tested whether there were differences in social connectedness and well-being following the intervention. As the timing of the autumn survey was only a week apart from the 3-week experience sampling study, we administered a shorter survey in the autumn to reduce participant burden. Social network metrics and well-being were not assessed during the autumn sample point. As in Study 1, social connectedness was operationalized as the number of people that a

participant nominates as close friends. Because this outcome measure was not included in the preregistration, we used a false discovery rate correction to account for multiple hypothesis testing³⁹. Our results indicated that participants in the experimental condition reported significantly more close friends in the winter term, but not in the spring term (Fig. 5e). On average, after controlling for baseline social connectedness, participants in the experimental condition reported 0.44 more close friends compared with participants in the control condition in the winter, 4 months after the intervention ($\beta = 0.45$, 95% CI 0.10 to 0.80, $t(188) = 2.56$, $P_{\text{adjusted}} = 0.04$). By spring term (6 months after the intervention), there were no longer significant differences in social connectedness between the two experimental groups.

Last, we assessed the effect of the interventions on well-being. We assessed both momentary positive affect measured during the experience sampling study and long-term psychological well-being (as measured in Study 1) across the school year. Exploratory analyses showed that participants in the experimental condition reported slightly more positive affect during the 2-week intervention period ($\beta = 3.19$, 95% CI 0.18 to 6.06, $t(384) = 2.08$, $P = 0.04$; Fig. 5g). This effect did not persist into week 3 (no treatment week; $\beta = 0.52$, 95% CI -3.41 to 4.47, $t(303) = 0.26$, $P = 0.79$; Fig. 5g). In week 3, positive affect for participants in both conditions steadily increased, likely because it was the last week before the Thanksgiving break. With regard to long-term well-being, we observed no differences in psychological well-being between the two conditions in surveys measured throughout the school year (Fig. 5f).

Together, these findings indicate empathy perception is malleable, and increasing empathy perception can result in higher social risk taking as well as long-lasting changes in social connectedness.

Discussion

Young adults face a stark decline in mental health at a time when they also feel more disconnected from their peers. Drawing from a longitudinal study of an undergraduate student community (total $N = 5,192$), we found associations between empathy perception and both current and future psychological well-being. These associations held after controlling for a wide range of factors that have been associated with well-being, such as personality traits^{43–45}, emotion regulation tendencies^{46,47}, academic performance^{48,49}, political ideology^{50,51} and social media use⁵². We also documented an 'empathy perception gap': people tend to view others as less empathic and caring than others view themselves. Individuals who hold larger empathy perception gaps are more likely to avoid taking social risks (for example, reaching out to strangers or disclosing vulnerability) and become more socially isolated over time. Finally, we conducted two field experiments in which we presented students with information about their peers' self-reported empathy, along with behavioural nudges to encourage social risk taking. We found that these interventions reduced the empathy perception gap, encouraged social risk taking and increased the size of participants' social networks months afterwards.

From a basic science perspective, our results reveal a critical factor in the well-being of young adults. Existing theories highlight the importance of meaningful, supportive relationships to psychological and physical well-being^{11–13,53–55}. Yet our findings show that social connections alone do not paint a full picture: even when situated in caring communities, people may fail to perceive the others' empathy, creating a disconnect between perception and reality. This 'empathy perception gap' can create a vicious cycle of misperception and social disconnection, representing a key pathway in a larger bidirectional system. Individuals' subjective perceptions of community norms powerfully shape their behaviours⁵⁶. When people perceive those around them as unempathic, they are less likely to take social risks such as reaching out to strangers. This social risk avoidance leads to missed opportunities to connect and receive support from others, thus perpetuating the misguided belief that those around them lack empathy and care.

The vicious cycle of empathy perception connects to a growing line of research on ‘undersociality’, which shows that individuals often shy away from taking social risks because they underestimate how positively others will respond to their social outreach^{26,30}. Prior work in this area has primarily focused on state-level forecasts about specific social scenarios, such as conversation with strangers²⁶, expression of gratitude²⁸ or asking for help²⁹. Our findings build on and extend this work by instead focusing on trait-level perceptions: individuals’ global beliefs about the empathy of those around them.

We find that people tend to underestimate the empathy of their peers and that this generalized perception predicts lower social risk taking across contexts. Much like pessimistic state-level forecasts discourage specific social behaviours, a broader belief that others are unempathic may suppress a wide range of social risk behaviours, thereby limiting opportunities for individuals to benefit from genuine connection.

This observed empathy perception gap may be driven by several mechanisms. First, individuals who avoid social risks will miss out on opportunities to receive feedback based on experiences, sustaining and reinforcing their misperceptions of others’ empathy (also termed ‘asymmetric learning’)^{30,57}. Second, while the majority of social exchanges may be empathic and friendly, isolated negative social events may disproportionately shape one’s perceptions of others due to negativity bias^{58,59}. This effect may be particularly pronounced among individuals with mental health conditions such as anxiety and depression, who exhibit a selective bias towards negative social interactions⁶⁰. Finally, differential construals may cause individuals to attribute their own empathetic behaviours to situational factors while seeing others’ empathetic behaviours as reflecting their personal character, further amplifying the misperception of empathy⁶¹.

At an applied level, our results provide important practical implications for tackling the rising loneliness and social disconnection among young adults. Through two field experiments, we find that a community’s empathy can be a powerful, yet underutilized, resource for psychological well-being. We demonstrated that interventions highlighting peers’ self-reported empathy, as well as behavioural nudges to encourage social risk taking, can effectively shift empathy perceptions, although the intervention effects were not significant across all timepoints. These interventions also led to increased social risk taking as well as more social connections that lasted months after the intervention.

These findings contribute to current research on reducing social disconnection by introducing a perception-based approach^{21,22,62}. Existing interventions have often targeted social interactions directly, for example, through social skills training and offering opportunities for social engagement²². Our approach complements these strategies by targeting social perception as a mechanism for fostering connection. It emphasizes the importance not only to create genuinely supportive social environments²¹, but also to help individuals to better recognize the empathy that surrounds them. Although Study 3a included an in-person workshop that is difficult to implement at scale, Study 3b used smartphone-delivered messages and posters, offering a scalable and low-cost approach to reducing loneliness and social disconnection among young adults. These findings suggest that scalable interventions that target empathy perceptions could serve as a valuable tool to address the rising levels of social disconnection.

In terms of mechanism, social norms messaging (for example, “95% of Stanford students are likely to help others who are feeling down”) may directly shift how people perceive those around them through social influence processes^{63,64}. On the other hand, smartphone-delivered behavioural nudges may encourage students to take social risks, providing more opportunities to experience warmth and support from others. Future research that directly tests these pathways can help better understand how perception shifts translate into lasting improvements in social connection and well-being.

One notable strength of study design is its use of a dataset that includes a relatively large sample size ($N = 5,192$), a longitudinal design, experimental methods and diverse data types (such as surveys, experience sampling, social network metrics and behavioural data). This multimethod approach helps reduce biases that can arise from relying on a single type of measurement.

However, the study also has a few limitations. First, all participants were drawn from first-year students from a single private university in the USA. While this offers important insight into this transitional developmental period marked by heightened vulnerability to loneliness and major shifts in social networks⁴⁰, it also limits the generalizability of our findings to other populations. Future studies that examine effects in more diverse populations from different educational and cultural backgrounds could help understand the generalizability of our findings.

Second, the relatively small sample size in the field experiment in Study 3b limited our ability to infer from each of the three experimental conditions separately. In the main manuscript, we collapsed these three experimental conditions to maximize power, as specified in our preregistration. Future studies are needed to delineate the separate and combined experimental effects of each type of intervention.

Third, while our interventions in Study 3a and Study 3b were successful in shifting empathy perception and improving social connectedness, they did not translate to significant improvements in long-term psychological well-being. This raises important questions about the dosage, timeframe and mechanisms through which empathy perceptions influence well-being. In Study 3b, the observed shifts in empathy perception were relatively small: a 5% increase compared with the control group. It could be that our experimental manipulation was not strong enough to lead to ripple effects in well-being. It is also possible that the benefits to mental health may require more time to manifest as it may depend on a cumulative build-up of positive social experiences over time. Future research should explore these possibilities, perhaps by extending the duration of both intervention and follow-up assessments to increase intervention dosage and capture long-term effects, as well as integrating additional strategies that more directly target psychological well-being.

Last, the empathy perception gap observed in Study 2 may stem not only from an underestimation of others’ empathy, but also from an overestimation of one’s own. Several factors could contribute to this overestimation of one’s own empathy. One possibility is the self-enhancement bias⁶⁵, where individuals tend to perceive themselves more positively than others; another is attribution bias⁶¹, where individuals may tend to attribute their own unempathic behaviours to situational causes (“I could not help my friend because I was too busy preparing for a final exam”), while attributing others’ unempathic behaviours as reflecting personal qualities (“They did not help me because they did not care”). Future research that uses behavioural measures of empathy rather than self-reports could more accurately quantify the extent to which individuals underestimate others’ empathy.

In sum, we identify an ‘empathy perception gap’: people tend to view others as less empathic and caring compared with others’ self-reports. This empathy perception gap fuels a vicious cycle: underestimating others’ empathy leads to social risk avoidance, feelings of isolation and worse mental health, which in turn perpetuates the mismatched empathy perceptions. By leveraging scalable interventions such as social norms campaigns and smartphone-delivered behavioural nudges, we can disrupt this cycle, shifting empathy perception and fostering greater social connection over time. Our work highlights the untapped potential of shifting empathy perceptions as a tool for enhancing social connection and well-being in young adults.

Methods

Studies 1–3 are part of a longitudinal project of the undergraduate student community at Stanford University named SCP^{35,37,66}. All

study procedures were approved by the Institutional Review Board at Stanford University (protocol no. 24593). All participants provided informed consent and received monetary compensation for their participation. All survey data were collected using Qualtrics, and ecological momentary assessment data were collected using an in-house developed software named Well Ping⁶⁷. Data and code used in the analyses are available in a publicly accessible OSF repository at <https://osf.io/u584x/files>. Note that to protect participant privacy, demographic variables are not shared publicly.

Study 1

Study 1 data were collected across 6 sample waves in two academic years (AY 2022–2023 and AY 2023–2024). Data from AY 2022–2023 were used for exploratory analyses, which then guided the preregistered replication conducted with data from AY 2023–2024 (preregistration at <https://osf.io/ez8gw>). All analyses in Studies 1–3 were conducted using R (version 4.3.0).

Participants. At each sample wave, all undergraduate students at Stanford University were invited to complete an online survey that took approximately 30 min to complete, and were compensated US\$30 for their participation. The final dataset contains 14,849 observations from $N = 5,084$ unique participants (mean age of 21.21 ± 1.86 years); 55.84% of the participants identified as women, 38.43% identified as men and 5.73% identified as other or preferred not to report.

Although we reached a large number of students, participation in our studies was voluntary. As a result, our samples in this and the following studies are not representative of the broader Stanford undergraduate community. As participation was voluntary and framed as a study about the Stanford community, our samples may overrepresent students who are interested in community dynamics. This, along with possible social desirability in self-reports, suggests our findings may overestimate levels of connection relative to the broader student population.

Measures. Participants across all samples completed an online survey that assessed a wide range of individual difference measures including well-being, social connectedness, personality traits and demographic variables. In addition, participants gave consent for the research team to access their grade point average (GPA) data from the university. Below we detail the key variables included in the main analysis. Missing survey responses were imputed using mean or mode values.

Psychological well-being. One of the key dependent variables in Study 1 is psychological well-being. Similar to a prior study from the SCP³⁷, this composite measure is defined from trait survey items that load most strongly (positively and negatively) onto a latent well-being factor derived from a factor analysis using data from 2022–2023. This psychological well-being measure loads positively from items from Satisfaction with Life Scale⁶⁸ and Subjective Happiness Scale⁶⁹ and negatively from items from the Center for Epidemiological Studies Depression Scale⁷⁰ and General Anxiety Disorder Scale⁷¹.

Social connectedness. To identify students' connections on campus, participants nominated up to six undergraduate students in response to the following prompts: "Who are your closest friends?". They entered their peer's name into a text field, which was auto-completed with names from the entire undergraduate student roster. Social connectedness is defined as the unique number of names that were entered in this question. Note that this measure deviated from the preregistration where we proposed to examine the number of nominations in the support network, instead of the close friend network. The results for both outcome variables were statistically significant. We reported findings on close friend networks to be consistent with Study 3. For results pertaining to the number of nominations in the support network, see Supplementary Fig. 1.

Empathy perception. Empathy perception was evaluated through the following four questions on a seven-point scale (Cronbach's alpha of 0.87): "If someone else is feeling bad, how likely is the average Stanford Student to try and help them feel better by listening to them and acting kindly?", "How likely is the average Stanford student to listen to someone's problems to try to improve how they felt?", "How likely is the average Stanford student to do something nice with someone to try to make them feel better?" and "How empathic is the average student at Stanford?". This operationalization of empathy perception included items on general empathy perception as well as on perceptions of empathic behaviours, and was guided by the following considerations: while empathy is a multidimensional construct comprising affective, cognitive and motivational components⁷², when laypeople were asked to define 'empathy', they emphasized empathic behaviour and compassionate actions⁷³. In addition, others' internal states of empathy, including cognitive and emotional empathy, are difficult to access. We thus prioritized observable, prosocial behaviours as a more reliable and functionally relevant indicator of perceived empathy.

Cross-sectional models. Cross-sectional associations between empathy perception and well-being, as well as empathy perception and social connectedness were evaluated using ordinary least square (OLS) regressions fitted through the `lm` function of the stats package (version 4.3.0) in R (version 4.3.0) at each sample wave. Each of the OLS models included empathy perception as a predictor while controlling for preregistered covariates of gender, race and number of years the participant has been at Stanford. Since there were six sample waves and two outcome variables, this amounted to a total of 12 OLS models. All statistical tests, including those reported in Study 2 and Study 3, were two sided.

Second, although not preregistered, we constructed two separate multilevel models to separately examine the overall effect of empathy perception across sample waves. We included sample wave as a control variable and treated participants as random effects in the models to account for the clustered nature of the data. The multilevel models were fitted through the `lmer` function from the `lme4` package (version 1.1-33).

Finally, following our preregistration, we examined the role of empathy perception in each of the outcome variables (well-being and social connectedness) relative to a large set of variables related to personality traits, empathy, emotion regulation, other social network characteristics and academic performance. The results of these analyses are reported in Extended Data Fig. 1.

Longitudinal models. Longitudinal associations were evaluated using OLS models at each sample wave. Each of the OLS model included empathy perception of the previous term as a predictor while controlling for preregistered covariates of well-being/social connectedness in the previous term, gender, race and number of years the participant has been at Stanford.

Similar to the cross-sectional analyses, we constructed exploratory cross-lagged panel models to examine the overall longitudinal effect of empathy perception across sample waves (Supplementary Information).

Study 2

Study 2 draws on survey data from the SCP from winter term of 2022 to spring term of 2024 as this period included the key variables needed for Study 2 analyses.

Participants. Participants in Study 2 are a subset of Study 1 except it excludes a sample in the autumn term of 2022 because key variables were not yet included in that sample wave. The final dataset contains 12,083 observations from $N = 4,678$ unique participants. All participants were undergraduate students (mean age of 20.85 ± 1.80 years); 56.86% of the participants identified as women, 37.45% identified as men and 5.69% identified as other or preferred not to report.

Measures. *Empathy perception.* Same as Study 1.

Empathy self-ratings. Participants' self-reported levels of empathy were evaluated through the following four questions on a seven-point scale (Cronbach's alpha of 0.86): "If someone else is feeling bad, how likely are you to try and help them feel better by listening to them and acting kindly?", "How likely are you to listen to someone's problems to try to improve how they felt?", "How likely are you to do something nice with someone to try to make them feel better?" and "How empathic are you?".

Social risk taking. Social risk taking was evaluated by asking participants to report on how often they engaged in the following six behaviours on a seven-point scale (Cronbach's alpha of 0.81)³²: disagree with a popular friend in front of a group of people; spend time with someone your friends do not like; open up to other students about a problem I am having; when something bad happens, I am able to share this bad news with friends; go to a party where I do not know anybody; and initiate a conversation with another student that I do not already know.

Loneliness. Loneliness was measured using eight items from the UCLA loneliness scale⁷⁴.

Analyses. Comparisons between participants' empathy perception and self-ratings of empathy perception were evaluated using two-tailed, unpaired *t*-tests. The models used to examine both the cross-sectional and longitudinal relationships between the empathy perception gap and social risk-taking as well as loneliness were specified in the same way as in Study 1.

Study 3

Study 3 includes two field experiments with first-year Stanford students and tested whether enhancing empathy perceptions could promote social connectedness and psychological well-being. Below we outline the experimental techniques that are shared between Study 3a and Study 3b, as well as methodologies that are specific to each study.

Experimental conditions. For both Study 3a and Study 3b, as we were interested in two intervention strategies—norms-based messaging and behavioural nudges—we used a two (norms versus no norms) by two (nudge versus no nudge) between-subject experimental design. Participants were randomized into one of the four conditions at the dorm-level at the beginning of the academic year (see below for the randomization process): (1) the control condition: participants received no norm messaging and no behavioural nudges; (2) the social norms condition: participants in the social norms condition were exposed to posters featuring normative messages in their dorms (Fig. 4a); (3) the behavioural nudge condition: participants in the behavioural nudge condition received behavioural nudges on their phone (Fig. 5a); and (4) the combined condition, where participants received both normative messages and behavioural nudges.

For Study 3a, we failed to recruit a meaningful number of participants for the behavioural nudge component (we were expecting ~200 participants per condition, but ended up recruiting ~50 participants per condition). As such, for Study 3a, we only examined outcome measures that were assessed before the behavioural nudge intervention according to our preregistration. The social norms condition and the combined condition are collapsed into the experimental condition. Similarly, the behavioural nudge and control conditions were collapsed into the control condition in Study 3a.

In both Study 3a and Study 3b, data were collected online using Qualtrics and our in-house-developed software, Well Ping. Participants completed surveys online, without direct interaction with study personnel. Although researchers were not blind to the experimental conditions and study hypotheses, this lack of participant contact minimized the possibility that we influenced participants' responses or behaviour.

Condition assignment. For the norms component of the experiment, the research team put up posters featuring experimental messages in student dorms. As such, randomization for both Study 3a and Study 3b occurred at the dorm level. Given that the number of dorms containing frosh students are relatively small ($n = 22$ dorms for Study 3a and $n = 23$ dorms for Study 3b), and that dorms varied on important factors such as dorm size and student population (whether it is a dorm with only first-year students or students across 4 years of their undergraduate education), we performed a pseudo-random assignment to ensure that the dorms that were assigned in each of the four conditions were balanced on these two factors.

Pseudo-randomization was performed in R using the `randomizr` package 0.22.0. We first created six blocks within which we randomly assigned participants. To do so, we listed every possible combination of (1) the dorm size tercile variable (we transformed the raw count of students in a dorm into a tercile variable that can take on the values 1, 2 or 3) and (2) the dorm population type (frosh only versus four class). We then created a matrix specifying how many dorms should be assigned to each condition within each block. Finally, we used the `block_ra` function from the R package `randomizr` (version 1.0.0) to carry out the assignment, specifying the block each dorm belongs to, the matrix that specifies how many units should be assigned to which condition within a block and that our experiment has four treatment arms: control, combined (social norms + behavioural nudge), social norms and behavioural nudge. As some of the student dorms were ethnic themed, we also verified that in the resulting assignment, the distribution of ethnic-themed dorms was balanced. We verified that there were no significant demographic differences between participants across conditions (for details, see Supplementary Information).

Study 3a

In Study 3a, we launched a social norms intervention that involved (1) a poster campaign and (2) in-person educational workshops. The treatment group was exposed to both campaign components and the control group did not directly receive either. Randomization was done at the dorm level.

Participants. Following the poster campaign and the in-person educational workshops, all first-year students were invited to respond to a follow-up survey aimed at assessing experimental effects of the intervention at the end of the autumn term (T1). A total of 346 participants completed the survey at T1 (mean age of 18.33 ± 0.63 years); 62.14% of the participants identified as women, 34.68% identified as men and 3.18% identified as other or preferred not to report. In the beginning of winter term (T2), all first-year students except participants residing in dorms of the social norms condition were invited to complete an online survey. A total of 240 participants completed the survey at T2 (mean age of 18.31 ± 0.56 years); 62.08% of the participants identified as women, 32.92% identified as men and 5% identified as other or preferred not to report.

Data collection and analyses. We gathered data at two points following the intervention to evaluate its impact on participants' perceptions of empathy and their social behaviours during the autumn and winter terms of the 2022–2023 academic year. The first data collection happened between 7 and 12 December. All first-year students were invited to complete an online survey in which we assessed our key outcome variables: empathy perception, social perception, social risk taking and time spent socializing.

Data collection at T2 occurred between 17 and 20 February 2023 and included the same items that were included in the survey at T1. As this survey was intended to serve as a baseline for the behavioural nudges condition (not included in the current study), only students who resided in the dorms in combined, behavioural nudge and control

conditions were invited. Participants who resided in dorms in the social norms condition were not invited to this survey.

As preregistered, experimental effects of the norms intervention (both dorm posters and in-person workshop) were assessed using OLS regressions where the experimental condition was specified as a predictor, while controlling for age, gender and race. Note that since randomization happened at the dorm level, we originally proposed to test the effect of the experimental manipulation using mixed-effect multilevel models that included a random intercept for dorms. The intraclass correlations for all four dependent variables were relatively small (intraclass correlation of 0.00–0.024), indicating low levels of clustering on our dependent variables. As such, we opted to test these hypotheses using OLS regressions with the same set of control variables.

Poster campaign. To create our poster campaign, we started by collecting statistics from the student body during the spring of 2022 using five items (for example, “If someone else is feeling bad, how likely are you to try and help them feel better by listening to them and acting kindly?”). To translate these data into a legible percentage, we marked anyone as ‘likely’ to engage in an action if they reported a value above the midpoint of the scale. Each statistic was unique and required a different poster design.

We then created these five poster designs in collaboration with a graphic designer. We solicited feedback from Stanford University personnel and students throughout the design process to ensure our messages were legible, the design was aesthetically pleasing and, most importantly, that our posters resonated with students.

We also created a custom website that a QR code on each poster would direct students to if they wanted to learn more about their peers. The webpage included a brief description of the aim of the posters and showcased all five designs and statistics.

We hung five to ten posters in each dorm, depending on the dorm size. Posters were hung on community cork boards, in staircases and in restrooms. Posters were hung in late November 2022. We checked on posters periodically to ensure they were still hanging in dorms. We left the posters up through the end of the academic year.

In-person educational workshops. In addition to our social norm posters, we also recognize the importance of in-person workshops to help students learn and internalize information about their peers. As such, we designed a 1-h workshop that involved a combination of lecturing, activities and reflections.

We integrated our workshop into an existing course (‘Frosh 101’) at the target university. Students can opt to take this class in their first term of college to help them acclimate to their new environment. We saw our intervention component as aligned with the goals of the course.

Our workshop curriculum followed this format: (1) guiding questions, (2) overview, (3) objectives, (4) activity 1: expectations versus reality activity, (5) correcting social inaccuracies lesson, (6) activity 2: action planning and (7) wrap up. Our curriculum structure was modeled after examples provided by the university.

We administered the workshop through course facilitators, who were all second-year students at the same university who had taken this course the year prior. We met with all facilitators in person across multiple 1-h sessions to review our materials. We provided each facilitator with a script to follow and a slide deck to present. We assessed facilitator compliance and attitudes in a post-session survey that they completed in the week following the delivery of the materials to their students (Supplementary Information).

Workshops occurred during the last week of November 2022. For full facilitator script and workshop outline, see Supplementary Information.

Measures. *Empathy perception.* Empathy perception was measured in the same way as in Study 1. Note that the originally preregistered

outcome was normative perceptions, which combined questions on empathy perception and sociality perception. We focused on empathy perception as the outcome variable in the main manuscript to be consistent with all other studies. For the combined perception measure that yielded consistent results, see Supplementary Materials.

Social risk taking. Social risk taking was measured in the same way as in Study 2.

Time spent socializing. We assessed overall time spent on socializing through the following question: “In the past two weeks, how much time per day did you spend on socializing (both offline and online) on average?”. Participants answered on a four-point scale: 1: under 30 min; 2: 30 min to 1 h; 3: 1–3 h; 4: over 3 h.

Study 3b

Study 3b was conducted in the 2023–2024 academic year. We used a two (norms campaign versus no norms campaign) by two (behavioural nudge versus no behavioural nudge) between-subject experimental design. The randomization and dorm poster procedures were identical to Study 3a.

Participants. In the autumn of 2023, all first-year students were invited to take part in a 3-week ecological momentary assessment (EMA) study. A total of 419 participants completed the survey at T1 (mean age of 18.57 ± 0.74 years); 57.53% of the participants identified as women, 37.44% identified as men and 5.03% identified as other or preferred not to report.

Study procedures. *Condition assignment.* For condition assignment, the same as Study 3a, before data collection, all first-year students were randomized into one of four conditions. Randomization happened at the dorm level and we used pseudo-randomization procedures to ensure that the four resulting conditions were balanced on dorm size and study type (all-frosh dorm versus four class dorm).

Baseline survey. In the fourth week of the 2023 autumn academic term, before the start of any interventions, all first-year undergraduate students (as well as undergraduate students from other years that are not included in Study 3b) were invited to complete a 30-min online survey that assessed their baseline ratings on key outcome measures including empathy perceptions and social behaviours (Table 1).

Poster campaign. After concluding the baseline survey, on 29–30 November 2023, the research team put up posters in dorms that were part of the social norms and combined conditions. The posters procedures were identical to Study 3a.

EMA and EMI. The week after concluding the baseline survey, all first-year students (as well as undergraduate students from other years that are not included in Study 3b) were invited to take part in a 3-week EMA study in which they received four momentary surveys per day for a compensation of up to US\$84 (25% of the compensation for the EMA study were prorated based on completion to incentivize high completion rates). A ‘ping’ refers to one EMA that occurs when students are pinged at a random time within fixed time intervals. Each of these surveys took approximately 2 min to complete on average and contained items on momentary affect, social risk behaviours, locations, activities and other contextual factors. The EMA surveys were divided into different streams that focused on topics such as social interactions, well-being goals or a general well-being assessment. The different streams had varying lengths, but overlapping items, and the EMA items used in the final dataset were available for most streams and surveys. See ‘Measures’ section for EMA measures that are included in the current study. The EMA was delivered using an in-house developed application, Well Ping⁷⁵.

Importantly, the first 2 weeks of EMA also served as an intervention period for participants in the experimental (social norms, behavioural nudge and combined) conditions. This design allowed us to deliver in-the-moment messages through ecological momentary interventions (EMI) while simultaneously assessing momentary outcomes of these EMIs. During the intervention weeks, the second ping of the day included an additional message in addition to the regular questions. See below for details on the EMI messages.

Follow-up surveys. To assess intervention outcomes, all students who have participated in EMI (as well as other undergraduate students who are not included in Study 3b) were invited to complete three online surveys throughout the academic year: at the end of the autumn term (approximately 3 weeks after EMI), in the winter term (approximately 2 months after EMI) and in the spring term (approximately 6 months after EMI). See Table 1 for items included in these surveys. Given the temporal proximity between the 3-week experience sampling study and the survey in the autumn term, the autumn survey was shorter to reduce participant burden and did not include measures on social connectedness and well-being.

EMI. At the beginning of the school year, all first-year students were randomized into four conditions based on the dorm they live in: control, norms, behavioural nudge or combined. For participants who took part in the EMA study, their experiences differed depending on the experimental condition.

Social norms condition. Participants in the social norms condition received norms-based messages three times per week for the first 2 weeks in addition to regular EMA. These messages were presented as survey questions where we asked participants to provide estimates of the normative statistics of their peers' empathy. Unlike other survey items, we provided feedback based on participants' responses. Examples of this feedback are (when participants answered correctly) "You got it right! In fact, in our past research with Stanford undergraduates, we find that 95% of students report being open to meeting other students. We also find that the majority of students actually underestimate the kindness of their peers!" or (when participants answered incorrectly) "Not quite right. But good news, students are more social than you thought! In fact, in our past research with Stanford undergraduates, we find that 95% of students report being open to meeting other students. We also find that the majority of students actually underestimate the kindness of their peers!"

Behavioural nudge condition. For the first 2 weeks of EMA, participants in the behavioural nudge condition received six experimental messages per week, three messages encouraging them to engage in a socially risky activity, and three messages that served as reminders. Examples of these activities are "Initiate a conversation with a classmate you don't know", "Ask a friend to introduce you to one of their friends that you don't know personally yet" and "Catch up with a friend that you haven't talked to in a while".

Combined condition. Participants in the combined condition received both messages in the social norms condition and messages in the behavioural nudge condition.

Analyses. EMA. Consistent with the preregistration, the experimental effect of the intervention during EMA was assessed through multilevel models in which gender and race were included as control variables and participant was treated as a random effect to account for the clustered nature of the data. The multilevel models were constructed using lmer function from the lme4 package (version 1.1-33).

Note that in our preregistration, we planned to include age as a control variable. However, since age was not assessed during the EMA,

we were unable to do so. We believe this omission is unlikely to affect our results as all participants were first-year college students with little age variance.

Self-report surveys. As preregistered, the effects of intervention on empathy perception and well-being were evaluated using OLS models while controlling for age, gender and race.

Social network analyses. Although not preregistered as primary hypotheses, we conducted exploratory analyses to examine the effect of intervention on social network outcomes (that is, social connectedness and support network in degree). These effects were evaluated using OLS models after controlling for age, gender and race.

Behavioural outcomes. In our exploratory analyses, we used a logistic regression model to examine the effect of the intervention on signing up for a social activity, controlling for age, gender and race.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

Data used in the analyses are available in a publicly accessible OSF repository at <https://osf.io/u584x/files>. Note that to protect participant privacy, demographic variables are not shared publicly.

Code availability

All code was written using available R packages and has been provided at <https://osf.io/u584x/files>.

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Acknowledgements

This research was funded by the National Institute of Mental Health (no. 1R01MH125974-01 awarded to J.Z.). The funders had no role in study design, data collection and analysis, decision to publish or preparation of the manuscript.

We thank D. Ogunbamowo, G. Yeung and A. Ferreira da Motta Costa for their help collecting data; V. Khandelwal and X. Zhao for their feedback on study design and writing; H. Echo Huang for designing the posters used in Study 3a and Study 3b; Y. He, B. Xu and C. Brennan for developing the Well Ping application; and S. Doğa Karaca for checking the replication materials. We are also grateful to the staff at Stanford University for their institutional support, particularly C. Wong Mineta, J. Calvert, L. Lambeth and the Frosh 101 program team: P. Hanlon-Baker, N. Wilson and S. Quoc Doan.

Author contributions

R.P., S.J.G., R.E.A., S.S., S.B.G., M.O.J., G.M.H. and J.Z. designed the studies and experiments. R.P., S.J.G., A.B. and E.H. implemented the study and collected the data. R.P., S.J.G., R.E.A. and S.S. analysed the data. R.P., S.J.G., R.E.A. and J.Z. drafted the paper. All authors provided critical feedback and approved the final paper for submission.

Competing interests

All authors declare no competing interests.

Additional information

Extended data is available for this paper at <https://doi.org/10.1038/s41562-025-02307-1>.

Supplementary information The online version contains supplementary material available at <https://doi.org/10.1038/s41562-025-02307-1>.

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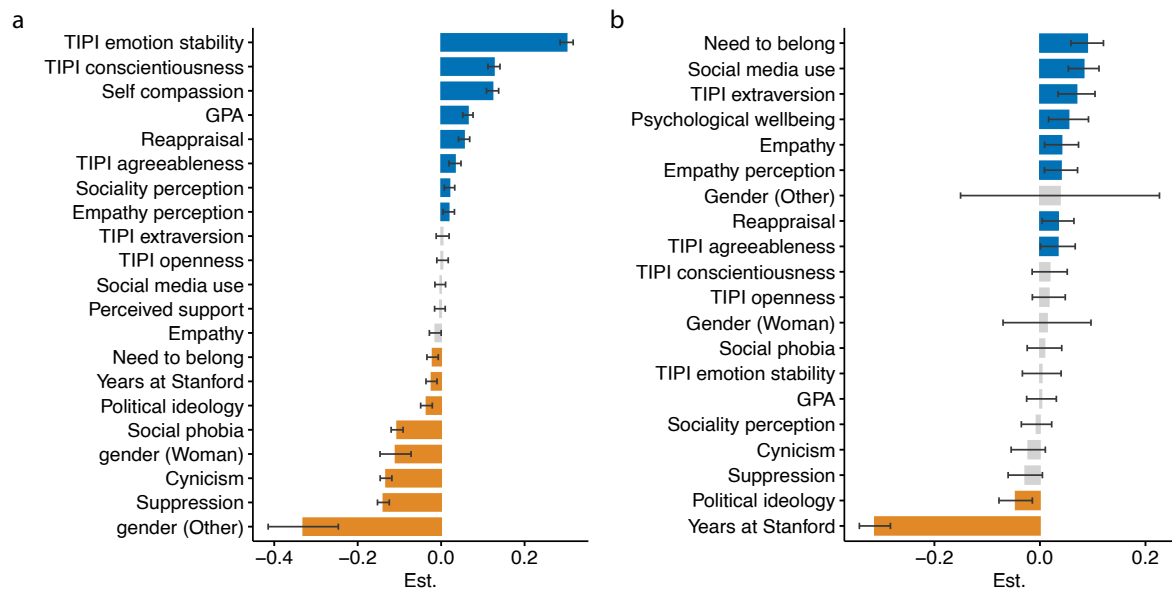
Peer review information *Nature Human Behaviour* thanks Nicholas Epley, Leah Sharman and Simone Shamay-Tsoory for their contribution to the peer review of this work.

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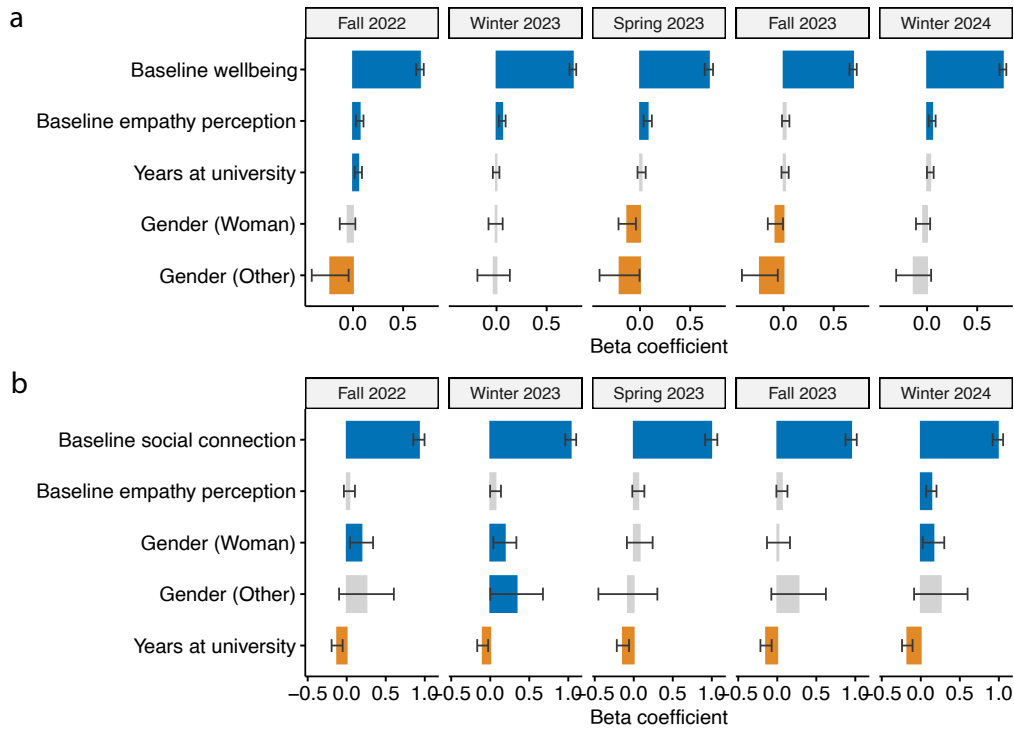
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Extended Data Fig. 1 | Empathy perception is associated with psychological well-being and social connectedness. a, b. Standardized regression coefficients from mixed-effect multilevel models examining the association between empathy perception and (a) psychological wellbeing as well as (b) social connectedness ($N_{participants} = 4970$; $N_{observations} = 14437$). Both models include covariates for individual traits, social behaviors, social network characteristics, and academic performance. The standardized regression coefficient for

empathy perception was statistically significant in panel **a** ($\beta = 0.02$, 95% CI: [0.00, 0.03], $P = 0.01$), and **b** ($\beta = 0.08$, 95% CI: [0.03, 0.12], $P = 0.0004$). P-values are two-sided, and no adjustments were made for multiple comparisons. Error bars represent 95% confidence intervals. Blue bars indicate significant positive coefficients, orange bars indicate significant negative coefficients, and grey bars indicate non-significant coefficients.



Extended Data Fig. 2 | Standardized regression coefficients for baseline empathy perception predicting. (a) psychological wellbeing as well as. **(b)** social connectedness in the next quarter after controlling for baseline

wellbeing and social connectedness, respectively ($N_{participants} = 4970$; $N_{observations} = 14437$). Blue = significant positive coefficient ($p < 0.05$); orange = significant negative coefficient ($p < 0.05$); gray = not significant coefficient.

Extended Data Table 1 | Cross-sectional correlation between empathy perception, well-being, and social connectedness across all samples in Study 1

Sample point	Empathy perception and well-being				Empathy perception and social connectedness				Well-being and social connectedness			
	r	df	p	95% CI	r	df	p	95% CI	r	df	p	95% CI
Fall 2022	0.22	2764	< 0.001	[0.18, 0.26]	0.10	2764	< 0.001	[0.06, 0.13]	0.04	2764	0.03	[0.001, 0.08]
Winter 2023	0.18	2334	< 0.001	[0.14, 0.21]	0.10	2334	< 0.001	[0.06, 0.14]	0.07	2334	0.0008	[0.03, 0.11]
Spring 2023	0.19	2626	< 0.001	[0.15, 0.23]	0.09	2626	< 0.001	[0.05, 0.13]	0.06	2626	0.003	[0.02, 0.1]
Fall 2023	0.19	2449	< 0.001	[0.16, 0.23]	0.13	2449	< 0.001	[0.09, 0.17]	0.08	2449	0.0001	[0.04, 0.11]
Winter 2024	0.20	2382	< 0.001	[0.17, 0.24]	0.08	2382	< 0.001	[0.04, 0.12]	0.06	2382	0.002	[0.02, 0.10]
Spring 2024	0.23	2282	< 0.001	[0.19, 0.27]	0.13	2282	< 0.001	[0.08, 0.17]	0.07	2282	0.0006	[0.03, 0.11]

Extended Data Table 2 | Longitudinal associations between empathy perception and well-being as well as empathy perception

Time point	Empathy perception and well-being				Empathy perception and social connectedness			
	Standardized β	df	p	95% CI	Standardized β	df	p	95% CI
Fall 2022 – Winter 2023	0.07	1780	0.0005	[0.03, 0.10]	0.05	1780	0.23	[-0.03, 0.14]
Winter 2023 – Spring 2023	0.06	1725	0.0008	[0.02, 0.09]	0.07	1725	0.12	[-0.02, 0.16]
Spring 2023 – Fall 2023	0.08	1419	0.0003	[0.04, 0.12]	0.10	1419	0.05	[0.00, 0.21]
Fall 2023 – Winter 2024	0.02	1653	0.257	[-0.02, 0.06]	0.08	1653	0.13	[-0.02, 0.17]
Winter 2024 – Spring 2024	0.05	1647	0.0003	[0.02, 0.09]	0.17	1647	0.0002	[0.08, 0.26]

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Software and code

Policy information about [availability of computer code](#)

Data collection

Well Ping Application (Custom Code):

Purpose: This is a custom-built mobile app designed for ecological momentary assessment (EMA) and intervention (EMI). It was used to send "pings" (notifications) to participants, prompting them to complete momentary surveys about their social behaviors and affect. It also delivered intervention messages in the experimental conditions.

Version: Version 1.1.3

Repository: The app was developed in-house and is available on GitHub at Well Ping GitHub Repository (<https://github.com/welling/welling>).

Survey Software:

The longitudinal surveys were administered using Qualtrics(e.g., Qualtrics or REDCap).

Version: Versions September 2022 - May 2024.

Data analysis

We used a combination of open-source software and custom code to analyze the data. The primary tool we used was R, an open-source programming language, which helped us conduct various statistical analyses, including ordinary least squares (OLS) regressions, multilevel models, and cross-lagged panel models. We used R version 4.3.0 for our analysis.

Additionally, we wrote custom R scripts to clean the data, run the analyses, and generate the visualizations. We've made all of our custom code available to the public on the OSF repository at <https://osf.io/u584x/files>.

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Research involving human participants, their data, or biological material

Policy information about studies with [human participants or human data](#). See also policy information about [sex, gender \(identity/presentation\), and sexual orientation](#) and [race, ethnicity and racism](#).

Reporting on sex and gender

We determined the gender of participants based on self-reporting methods. Participants could answer to which gender they identified the most, being given three different options (i.e., man, woman, and other with an open answer in which they could write their gender).

Based on the literature we have reviewed, there may be gender differences in empathy perception and wellbeing, we thus included gender as a control variable in our analyses.

Reporting on race, ethnicity, or other socially relevant groupings

Participants' race/ethnicity was self-reported by selected one or multiple from the following categories: Native American, Pacific Islander, East Asian, South Asian, Southeast Asian, Black or African American, White or Caucasian, Hispanic or Latino/a, Middle Eastern, Other (with an open answer in which they could specify), Prefer not to answer.

Based on the literature we have reviewed, there may be racial differences in mental health and wellbeing, we thus included race as a control variable in our analyses.

Population characteristics

See details in "Behavioural & social sciences study design" section.

Recruitment

For Study 1 and Study 2, all undergraduate students at Stanford University were invited to participate in our study. For Study 3a and Study 3b, all first-year undergraduate students at Stanford University were invited to participate in our Study. For all studies, participants had to be over 18 years to be eligible. Since all participants were drawn from a single private university in the United States, this limits the generalizability of our findings to other populations.

Ethics oversight

The study protocol was approved by Stanford University's Institutional Review Board (protocol #24593), and all participants provided informed consent.

Note that full information on the approval of the study protocol must also be provided in the manuscript.

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Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description

Study 1: quantitative cross-sectional, quantitative longitudinal; Study 2: quantitative cross-sectional; Study 3: quantitative experimental.

Research sample

The research sample consists of undergraduate students at Stanford University. In Study 1, we recruited N = 5,084 participants (38.43% man, 55.84% woman, 5.73% other or preferred not to answer. Mean age = 21.21 years old, SD = 1.86). Study 2 participants

were a subset of Study 1, $N = 4,678$ participants (37.45% man, 56.86% woman, 5.69% other or preferred not to answer. Mean age = 20.85 years old, $SD = 1.80$). In Study 3a, at T1, we recruited $N = 346$ participants (34.68% man, 62.14% woman, 3.18% other or preferred not to answer. Mean age = 18.33 years old, $SD = 0.63$). In Study 3a, at T2, we recruited $N = 240$ participants (32.92% man, 62.08% woman, 5% other or preferred not to answer. Mean age = 18.31 years old, $SD = 0.56$). In Study 3b, we recruited $N = 419$ participants (37.44% man, 57.53% woman, 5.03% other. Mean age = 18.57 years old, $SD = 0.74$). Our sample may not be a representative sample.

Sampling strategy

We used a convenience sample in this work.

Study 1 & Study 2

The maximum number of eligible participants is approximately 7000, which is the expected undergraduate student population. All undergraduates students will be invited to complete surveys as we aim to have as much data as possible at each data collection point. Examining past data, approximately 44% of the students complete our survey at each sample point. Therefore, based on the past survey participation rates, we expect to have approximately 2800 students at each sample point, and 1680 students to participate in both surveys.

Power analysis:

1. To detect the linear cross-sectional effect of empathy perception on wellbeing, we based our power analysis on effect sizes detected in data from 2022-2023 (Cohen's $f^2 = 0.03$) and a desired power of 0.80, we estimate the appropriate sample size to be 669.
2. To detect the linear cross-sectional effect of empathy perception on support network size, we based our power analysis on effect sizes detected in data from 2022-2023 (Cohen's $f^2 = 0.01$) and a desired power of 0.80, we estimate the appropriate sample size to be 1878.
3. To detect the linear longitudinal effect of empathy perception on wellbeing, we based our power analysis on effect sizes detected in data from 2022-2023 (Cohen's $f^2 = 0.01$) and a desired power of 0.80, we estimate the appropriate sample size to be 1878.
4. To detect the linear longitudinal effect of empathy perception on support network size, we based our power analysis on effect sizes detected in data from 2022-2023 (Cohen's $f^2 = 0.009$) and a desired power of 0.80, we estimate the appropriate sample size to be 2034.

Given these considerations, we expect to be sufficiently powered for most of our hypothesized analyses.

Study 3a & 3b

A power analysis indicates that we need 175 participants per condition in order to have sufficient power (80%) to detect a moderate effect size of $d = 0.30$ at $\alpha = 0.05$. We therefore aimed to recruit $N = 200$ for each condition.

Data collection

Study 1, 2 & 3a

We collected data using a 30-min online survey which participant completed on their own device.

Study 3b

Study 3b data were collected through (1) online surveys, which participant completed on their own device; (2) Well Ping mobile phone app.

Timing

Study 1: October 2022 - May 2024

Study 2: March 2023 - May 2024

Study 3a: Nov 2022 - March 2023

Study 3b: Oct 2023 - May 2024

Data exclusions

No data were excluded in Study 1, Study 2, and Study 3a. For Study 3b, consistent with our preregistered exclusion criteria, 36 participants were excluded because they were not exposed to any experimental messages.

Non-participation

No participants dropped out or declined participation.

Randomization

Pseudo-randomization was performed in R version 4.1.1 using the randomizr package 0.22.0. We first created 6 blocks within which we randomly assigned dorms. To do so, we listed every possible combination of (a) the dorm size tercile variable (we transformed the raw count of students in a dorm into a tercile variable that can take on the values 1, 2, or 3) and (b) the dorm population type (fresh only vs. 4-class). We then created a matrix specifying how many dorms should be assigned to each condition within each block. Finally, we used the block_ra function from the R package randomizr to carry out the assignment, specifying the block each dorm belongs to, the matrix that specifies how many units should be assigned to which condition within a block, and that our experiment has 4 treatment arms: Control, Combined (Social Norms + Behavioral Nudge), Social Norms, Behavioral Nudge. As some of the student dorms were ethnic themed, we also verified that in the resulting assignment, the distribution of ethnic-themed dorms was balanced. We verified that there were no significant demographic differences between participants across conditions (see Supplemental Information for details).

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involvement in the study
<input checked="" type="checkbox"/>	<input type="checkbox"/> Antibodies
<input checked="" type="checkbox"/>	<input type="checkbox"/> Eukaryotic cell lines
<input checked="" type="checkbox"/>	<input type="checkbox"/> Palaeontology and archaeology
<input checked="" type="checkbox"/>	<input type="checkbox"/> Animals and other organisms
<input checked="" type="checkbox"/>	<input type="checkbox"/> Clinical data
<input checked="" type="checkbox"/>	<input type="checkbox"/> Dual use research of concern
<input checked="" type="checkbox"/>	<input type="checkbox"/> Plants

Methods

n/a	Involvement in the study
<input checked="" type="checkbox"/>	<input type="checkbox"/> ChIP-seq
<input checked="" type="checkbox"/>	<input type="checkbox"/> Flow cytometry
<input checked="" type="checkbox"/>	<input type="checkbox"/> MRI-based neuroimaging

Plants

Seed stocks

Report on the source of all seed stocks or other plant material used. If applicable, state the seed stock centre and catalogue number. If plant specimens were collected from the field, describe the collection location, date and sampling procedures.

Novel plant genotypes

Describe the methods by which all novel plant genotypes were produced. This includes those generated by transgenic approaches, gene editing, chemical/radiation-based mutagenesis and hybridization. For transgenic lines, describe the transformation method, the number of independent lines analyzed and the generation upon which experiments were performed. For gene-edited lines, describe the editor used, the endogenous sequence targeted for editing, the targeting guide RNA sequence (if applicable) and how the editor was applied.

Authentication

Describe any authentication procedures for each seed stock used or novel genotype generated. Describe any experiments used to assess the effect of a mutation and, where applicable, how potential secondary effects (e.g. second site T-DNA insertions, mosaicism, off-target gene editing) were examined.