Correction

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Correction for "Changing climates of conflict: A social network experiment in 56 schools," by Elizabeth Levy Paluck, Hana Shepherd, and Peter M. Aronow, which was first published January 4, 2016; 10.1073/pnas.1514483113 (*Proc Natl Acad Sci USA* 113:566–571).

The authors wish to note the following: "We reported an estimated 30% decrease in administrative reports of conflict and would like to correct this estimate to 25%. The original paper reported this estimate as the rounded covariate-adjusted estimated average treatment effect (−0.06) divided by the rounded unadjusted control group mean (0.20). Without the rounding error, the original estimate is 29%. Using both covariate adjusted estimates, we have an estimate of 25%. Using both unadjusted estimates, we have an estimate of 23%. The original estimate of 30% was reported in the Abstract (line 11), the Significance Statement (line 8), in the results on page 569 (left column, first paragraph, line 9), and in the discussion on page 571 (left column, first paragraph, line 6). In each of these places, we would like to replace the number 30 with 25. The estimated overall effects on administrative reports of conflict remain statistically insignificant at the α=0.05 level."

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Changing climates of conflict: A social network experiment in 56 schools

Elizabeth Levy Paluck[a,1], Hana Shepherd[b], and Peter M. Aronow[c,d]

[a]Department of Psychology and Public and International Affairs, Princeton University, Princeton, NJ 08540; [b]Department of Sociology, Rutgers University, New Brunswick, NJ 08901; [c]Department of Political Science, Yale University, New Haven, CT 06520; and [d]Department of Biostatistics, Yale University, New Haven, CT 06520

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Theories of human behavior suggest that individuals attend to the behavior of certain people in their community to understand what is socially normative and adjust their own behavior in response. An experiment tested these theories by randomizing an anticonflict intervention across 56 schools with 24,191 students. After comprehensively measuring every school's social network, randomly selected seed groups of 20–32 students from randomly selected schools were assigned to an intervention that encouraged their public stance against conflict at school. Compared with control schools, disciplinary reports of student conflict at treatment schools were reduced by 30% over 1 year. The effect was stronger when the seed group contained more "social referent" students who, as network measures reveal, attract more student attention. Network analyses of peer-to-peer influence show that social referents spread perceptions of conflict as less socially normative.

social influence | social norms | bullying | adolescents | social psychology

One of the most elusive and important goals in the behavioral sciences is to understand how community-wide patterns of behavior can be changed (1–8). In some cases, social scientists seek to reduce widespread and persistent patterns of negative behavior like corruption or conflict; in others, to promote positive behavior like healthy eating or environmental conservation. Research on changing individual behavior provides many intervention strategies targeted to the psychology of the individual, such as attitudinal persuasion, situational cues, and peer influence (9–12). Another body of research focuses on scaling up behavior change interventions to the community level, studying attempts to reach every individual in a population with mass education or persuasion messaging (13), or with institutional regulation or defaults (14). A third strategy has been to seed a social network with individuals who demonstrate new behaviors, and to rely on processes of social influence to spread the behavior through the channel of structural features of the network (15–18).

The present paper incorporates all three approaches. We implemented a social influence strategy designed to change individual behavior, and we tested whether, as a result, new behaviors and norms are transmitted through a social network and also whether they scale up to shift overall levels of behavior within a community. Specifically, we randomized the selection of students within a comprehensively measured social network to determine the relative power of certain individuals to influence the behavior of others. We randomly assigned the presence of this treatment to some community networks and not others. This approach allowed us to determine whether influence from a small group of influential people is enough to shift a community's behavioral climate, which we define as a widespread and persistent behavioral pattern across the community.

Our experimental design is motivated by theoretical debates about how social norms emerge and are transmitted within communities (1, 19–23). At the community level, it is believed that social norms, or perceptions of typical or desirable behavior, emerge when they support the survival of the group (24) or because of arbitrary historical precedent (23). Once formed, these informal rules for behavior are transmitted by the survival of those who follow them, or through the punishment of deviants and the social success of followers. For these reasons, theory suggests that most individual community members strive to understand the social norms of a group and adjust their own behavior accordingly (21, 25). When many individuals in a community perceive a similar norm and adjust their behavior, then a community-wide behavioral pattern may emerge.

Social norms may be explained directly to community members through storytelling or advice, but small-scale experiments and theory suggest that individuals often infer which behaviors are typical and desirable through observation of other community members' behavior (1, 21, 22). A large literature attempts to identify which community members are effective at transmitting social information across a community (16, 18, 26–28). Theories of norm perception predict that individuals infer community social norms by observing the behavior of community members who have many connections within the community's social network (29). Sometimes called "social referents" (20), individuals may view these community members as important sources of normative information, in part because their many connections imply a comparatively greater knowledge of typical or desirable behavioral patterns in the community. In fact, social referents may have many connections for numerous reasons: they may have a higher status, they may be more popular, or they may have a greater capacity for socialization. Social referents may be different on many dimensions, but what they share is a

Significance

Despite a surge in policy and research attention to conflict and bullying among adolescents, there is little evidence to suggest that current interventions reduce school conflict. Using a large-scale field experiment, we show that it is possible to reduce conflict with a student-driven intervention. By encouraging a small set of students to take a public stance against typical forms of conflict at their school, our intervention reduced overall levels of conflict by an estimated 30%. Network analyses reveal that certain kinds of students (called "social referents") have an outsized influence over social norms and behavior at the school. The study demonstrates the power of peer influence for changing climates of conflict, and suggests which students to involve in those efforts.

Author contributions: E.L.P., H.S., and P.M.A. designed research; E.L.P. and H.S. performed research; P.M.A. contributed new reagents/analytic tools; E.L.P., H.S., and P.M.A. analyzed data; and E.L.P., H.S., and P.M.A. wrote the paper.

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[1]To whom correspondence should be addressed. Email: epaluck@princeton.edu.

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comparatively greater amount of attention from their peers. Theory and evidence point to the prediction, supported by recent experimental evidence (20, 30), that social referents are particularly influential over perceptions of community norms and behavior in their network.

However, despite the large theoretical and empirical literature devoted to ideas about how social norms and behavioral patterns emerge and persist, the central question of which individual level interventions can shift a community's behavioral climate remains open. We pose this question in the context of adolescent school conflict, such as verbal and physical aggression, rumor mongering, and social exclusion. Although the term "conflict" lacks a consensus definition (31), we follow other social scientists (32, 33) who define conflict broadly, as characterized by antagonistic relations or interactions, or behavioral opposition, respectively, between two or more social entities. This broad definition includes harassment or antagonism from a high-power or high-status person aimed at a person with lower power or status (i.e., bullying), but also conflict between or among people with relatively balanced levels of social power and status.

Within many middle and secondary schools in the United States, student conflict is part of the schools' behavioral climate; that is, conflict is widespread and persistent (34, 35). In contrast to claims that conflict is driven by a minority group of student "bullies" (36), evidence suggests a majority of students contribute to conflicts at their school (37), and these conflicts persist over time because of cyclical patterns of offense and retaliation (38).

Student conflict, and in particular bullying, has recently attracted research and policy attention as online social media have brought face-to-face student conflicts into adult view (34, 39). New laws and school policies have been introduced to improve school climate, along with many school programs targeting students' character and empathy. However, basic research illustrates that students perceive social constraints on reporting or intervening in peer conflict (40). That is, students may perpetuate and tolerate conflict not because of their personal character or level of empathy, but because they perceive conflict behaviors to be typical or desirable: that is, normative within their school's social network. In such a context, reporting or intervening in peer conflict could be perceived by peers as deviant.

Methods and Materials

We designed an experimental intervention to encourage anticonflict norms and behavior among a group of students in United States middle schools. We tested whether these students' behavior, particularly the behavior of social referents, could influence other students' perceptions of social norms of conflict and shift overall levels of conflict behavior at the school. In contrast with previous work that randomizes at either the student or the school level (41–43), the present experiment combined these two approaches. In an experiment conducted over the school year from September 2012 to June 2013, we randomly assigned half of 56 public middle schools (which include students ages 11–15 y old) scattered throughout the state of New Jersey to receive the intervention. Within these treated schools, we randomly selected students to participate in the intervention as seed students. After schools consented to participate, all parents were informed of the survey at each school and provided opt-out consent; parents of seed students provided written informed consent, and students provided informed consent for the survey, in a protocol approved by the Princeton University Institutional Review Board.

Our multilevel experiment allows us to evaluate the spread of the seed students' anticonflict stance to their peers within the treatment schools, measured subjectively by student-reported norms and administratively by school-reported disciplinary events. On a school-wide level, we tested whether the influence from this group, and particularly from the social referent seeds, was strong enough to shift perceived social norms and disciplinary events in treatment schools compared with control schools after 1 y.

In the 28 of 56 schools randomly assigned to receive the intervention, we selected a group of students (the seed-eligibles) using a deterministic algorithm (see *SI Appendix* for materials and methods used) designed to represent 15% of the school population, blocked by gender and grade and capped at 64 students (grades ranged from 5 to 8) (*SI Appendix*, Figs. S1 and S2, and Table S1). We randomly assigned 50% of that group (the seeds) to be invited to participate in the anticonflict intervention, which our team implemented over the course of the school year. Within each seed group at each school (on average 26 seeds at each school, for a total of 728 seeds across 28 schools), a random proportion were social referent seeds, meaning they were in the top 10% of their school in the number of connections reported by other students (i.e., indegree).

We measured social connections at the school, in which we asked students to report which students they chose to spend time with in the last few weeks. This question is specifically designed to uncover the structure of attention in a social network, and identifies social referents as people who are drawing the most attention. Specifically, we conducted a survey to map the complete social network for all 56 schools before randomization, ∼3 wk following the start of school. Each school's entire school body took a survey at the same time on a given day ($n = 24{,}191$ students). The social network question, accompanied by a full student roster for the school, asked students to nominate up to 10 students at their school whom they chose to spend time with in the last few weeks, either in school, out of school, or online. When the network was remeasured nearly 9 mo later at the end of the school year, 42.2% of reported nominations persisted, a degree of stability that is typical of longitudinal social networks (e.g., ref. 44) (see *SI Appendix* for more explanation).

Our approach to social network measurement, which we designed and piloted in a previous study (20), differs from typical questions used to map adolescent and adult social networks. Typically, social network researchers ask respondents to nominate popular people or to list their friends. We designed this new measurement approach because our theory predicts that individuals form and reshape their perceptions of norms by observing their peers' behavior. Our network question (i.e., "whom did you choose to spend time with, face to face or online") directly measures who is observing whose behavior. Asking about popularity or friendship measures behavioral observation less directly; for example, individuals may know who is popular in their network but they may not observe their behavior regularly. Our question, based on the theoretical construct of attention in a social network, provides a behavioral measure of that attention.

We also prefer our network question to other common measurement strategies based on popularity and friendship because the definition of popularity and friendship is subjective and can differ from student to student (45). Moreover, according to our understanding of social referents, whom we wish to identify with our network measurement, social referents may be popular and have many friends, but they may also be unpopular except among a select group of students or not defined as a true "friend" by many individuals. The key feature of a social referent is that their behavior is observed by many other individuals.

Because of the way we measure the social network of each school, we do not separate the structural position of social referents (the top 10% in nominations at their school) from their personal characteristics. As an empirical matter, we note that the students who are social referents tend to be different from nonsocial referents in their observable covariates: for example, they are more likely to self-report dating people at school, receiving compliments on their house from peers, and to have older siblings (*SI Appendix*, Table S2). These differences suggest that theoretical and simulated network analyses, including "key player" models (46), may fail to account for important heterogeneity in the traits of social referents. Our approach allows us to directly assess how referents differ from nonreferents, not only in their traits but also in their capacity for peer-to-peer influence and in their ability to induce climate-wide change.

The survey that all students took at the beginning of the year also included a battery of social norms questions regarding conflict behaviors, specifically students' estimates of descriptive norms (how many students at the school participate in various forms of conflict), and prescriptive norms (how many students at the school think it is desirable to participate in various forms of conflict). Norms questions were on a scale of 0–5, with answers ranging from "almost nobody" to "almost everybody." We repeated the survey at the end of the school year. In addition to surveys, we tracked behavior using schools' administrative records on peer conflict-related disciplinary events across the entire year (see *SI Appendix* for materials and methods used). (Administrative data were available for 49 of the 56 schools, and we restricted our attention to these schools for analyses of conflict-related events.) Our use of disciplinary events to measure conflict could pose a threat to our conclusions about the causal impact of the intervention if teachers' reporting of conflicts were affected by the treatment. However, we find this possibility to be unlikely given how the intervention was implemented. Namely, to prevent demand effects, no teachers were informed that we were tracking disciplinary reports. In addition, schools' conflict reporting practices were

determined before the research team was in contact with the school, and before treatment was assigned.

During the anticonflict intervention, a trained research assistant met with the seed group every other week to help seed students identify common conflict behaviors at their school, so that the intervention could address the conflicts specific to each school. Seed students were then encouraged to become the public face of opposition to these types of conflict. For example, seed groups at each school compiled a list of conflict behaviors they could address, created hashtag slogans about those behaviors, and turned the slogans into online and physical posters. The seed students' photos were posted next to the slogan to create an association between the anticonflict statement and each seed student's identity. In another activity, seed students gave an orange wristband with the intervention logo (a tree) as a reward to students who were observed engaging in friendly or conflict-mitigating behaviors (over 2,500 wristbands were distributed and tracked). This intervention model can be likened to a grassroots campaign in which the seed students took the lead and customized the intervention to address the problems they noted at their school. Notably, it lacked an educational or persuasive unit regarding adult-defined problems at their school. To maintain standardized procedures, trained facilitators followed the same semistructured scripts and activity guides (see *SI Appendix* for materials and methods used).

Across all treatment schools, attendance at each intervention meeting was on average over 55% of the invited students, which we consider strong given that meetings were entirely optional and that students did not self-select into the group. To motivate this participation, we made it easy to attend the meetings, by holding them during school to avoid the need for after school transportation arrangements, by providing passes in advance to leave class (meetings were at different times each week, to avoid absenteeism from any one class in particular), and by holding the meeting during one class period. We also strove to make the meetings as enjoyable as possible, by providing snacks, ensuring that activities were always hands-on, participatory, and student-driven rather than lecture-based, and finally by involving as much technology as possible, including electronic tablets, video and animation generation software, and well-designed aesthetically pleasing materials.

The randomization of schools and seeds facilitates the design-based evaluation of the causal effects of our experimental intervention. Climate effects are based on the between-school randomization to treatment and control, with linear regression of school-level outcomes on school-level assignment and covariates producing consistent estimates of average school-level causal effects. To characterize effect heterogeneity with respect to the seed group composition (i.e., the proportion of seeds who are social referents, which varies from school to school), we use linear regression interacting school-level treatment with seed group composition and controlling for the proportion of students in the seed group who were social referents. Although there was heterogeneity across schools in the proportion of seed-eligible students who were social referents, any differences between the proportion of treated seeds who were referents and the proportion of seed-eligibles who were referents are attributable to randomization. As a result, our regression strategy allowed us to estimate the causal effect of the proportion of social referents in the seed group on school-wide conflict and other outcomes. Accordingly, we can make comparisons between average potential outcomes in, for example, treated schools where seed groups had 20% social referents to control schools, or even to treated schools where seed groups had 10% social referents. We computed confidence intervals and *P* values using robust SEs under a normal approximation.

The second type of effect we observed, peer-to-peer social influence effects, are based on the random assignment of the treatment to seed-eligibles, and were assessed by how seed students causally affect other students in their social network. In estimating these effects, we must address problems of confounding because of the network setting. We cannot simply compare students who were exposed to seeds to the students who were not exposed to seeds, because the presence of a seed in a student's social network is not directly randomized. The probability of being exposed to a seed depends in part on how many seed-eligible peers a student has. In a naive analysis, the number of seed-eligible peers and any other correlated factor could confound the analysis.

By virtue of prerandomization measurement of each school's network and randomized assignment of seeds and schools, we can know the exact probability that each student in a school network will be exposed to a seed or not, and furthermore, whether or not they will be exposed to a social referent seed or to a nonreferent seed. We may condition on these known probabilities, thereby ensuring that exposure to seed students is statistically independent of all pretreatment variables, both observed and unobserved (47). Using inverse probability weighting, a well-known nonparametric correction (48, 49), we used these probabilities to predict population means of potential outcomes (50) for students under different levels of exposure. In practice, this implies weighting each observation by the inverse of its probability of falling into its observed exposure condition in a weighted least-squares regression, and using fitted values from the regression to compute the average predicted value in the population. Thus, average causal effects are differences between these population means of potential outcomes, allowing for comparisons between average potential outcomes of, for example, students in treated schools with a treated social referent peer to students in control schools, or even to students in treated schools with no treated peers.

We considered four conditions of exposure to the seed students in each school: (*i*) students in control schools, (*ii*) students in treated schools for whom no peers are seeds, (*iii*) students in treated schools for whom at least one peer is a seed but no seed is a social referent, and (*iv*) students in treated schools for whom at least one peer is a social referent seed. In this particular analysis, we restricted our network-based analyses to the subpopulation of 2,451 students who had a positive probability of falling into all four levels of exposure.

Twenty-four percent of seed students did not accept our invitation to join the anticonflict intervention group. To preserve the integrity of the experimental design given such noncompliance, we used a conservative intention-to-treat approach in our analysis that counts noncompliers as directly treated seeds (see *SI Appendix* for materials and methods used).

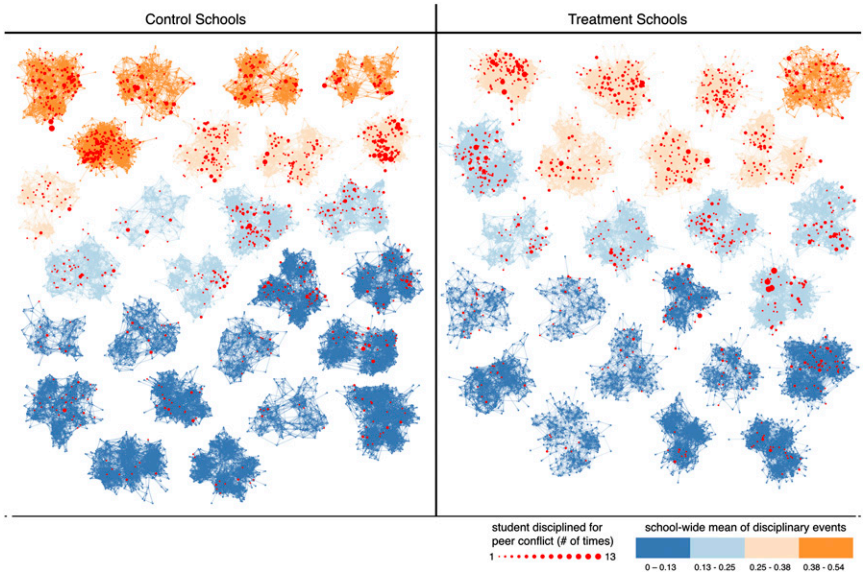

Fig. 1. Overall school climate results: distribution of disciplinary events throughout school networks, comparing treatment, and control schools. Visualization of the effect of treatment on disciplinary reports of peer conflict among the 49 schools that provided administrative data (26 in control, 23 in treatment). Color coding reveals the average number of times each student in the school was disciplined for peer conflict, from dark blue (little conflict) to dark orange (many disciplinary events; higher concentration of dark oranges among control schools). Student nodes are colored red when the student was disciplined for conflict, and their node is scaled to the number of times they were disciplined during the year.

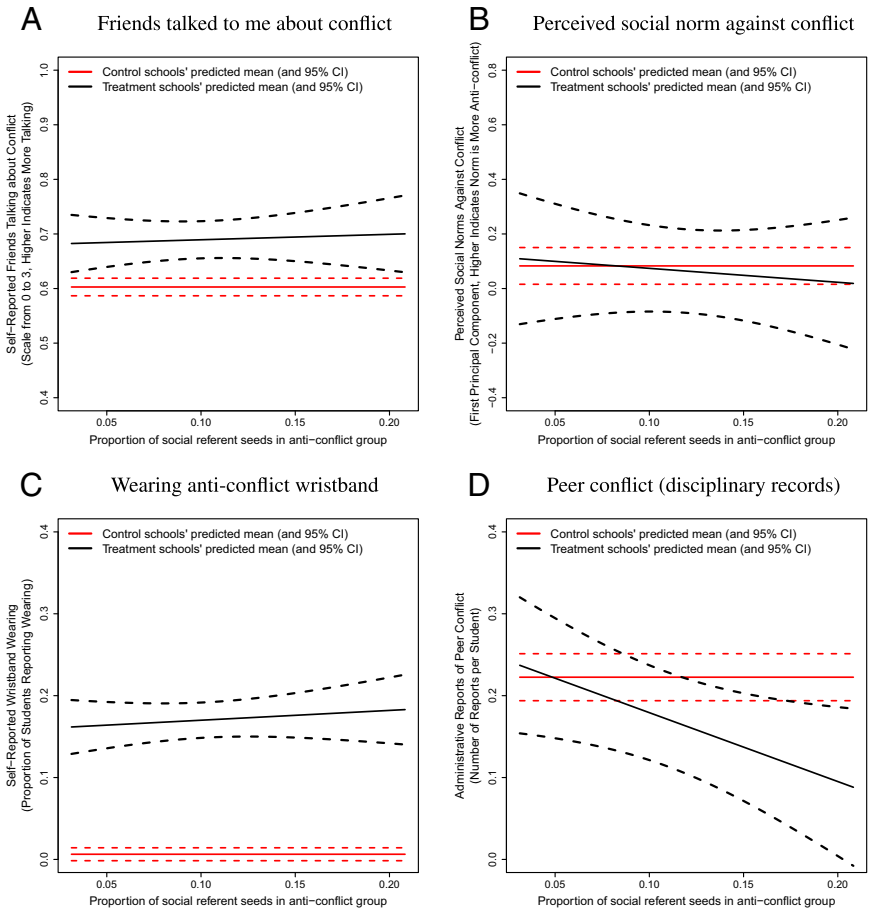

Fig. 2. (*A–D*) Average school-level climate differences between treatment and control schools caused by anticonflict groups; differences shown to vary with the proportion of social referent seeds in the group. Lines represent estimates of the conditional average potential outcome with respect to the proportion of social referent seeds, and are surrounded by 95% confidence intervals estimated using heteroskedasticity-robust SEs for the regression fit. In examples (*A*) talking about conflict with friends, (*C*) wearing anticonflict wristbands, and (*D*) disciplinary reports of peer conflict, we find evidence that the intervention caused average overall school differences in talking about conflict, wearing anticonflict wristbands, and in actual peer conflict as reported by the school. We find that a higher proportion of social referent seeds assigned to the anticonflict group increases the size of the overall school effect on disciplinary reports of peer conflict ($P = 0.07$, heteroskedasticity-robust Wald test). See *SI Appendix*, Table S7 for regression tables.

Results

We first turn to the question of school climate, as measured by schools' overall levels of norms and conflict behavior. Figs. 1 and 2 reveal that the treatment significantly reduced average levels of disciplinary reports of peer conflict in treatment compared with control schools ($P < 0.05$, heteroskedasticity-robust Wald test). In a control school, we estimated that each student in the school was officially disciplined for peer conflict on average 0.20 times per year. We estimated an average decrease of 0.06 disciplinary events per student in treatment schools, a 30% reduction in peer conflict reports. Fig. 1 visualizes this contrast, showing more control networks colored in orange and red (representing conflict) and with greater intensity of those colors, compared with treatment networks. Supporting this result, we found that, on average, students in treatment schools report higher levels of talking with friends about how to reduce conflict and of wearing anticonflict wristbands. We found no average differences in social norms between treatment and control schools (Fig. 2*B* and *SI Appendix*, Fig. S6).

Social referent seeds were more influential at shifting schoolwide conflict compared with other seed students. As illustrated in Fig. 2*D*, schools with the highest proportion of social referent seeds assigned to their anticonflict groups had the greatest declines in disciplinary reports of peer conflict. When 20% of a treatment school's anticonflict group is composed of social referent seeds (proportions ranged from 0 to 37%), we estimate that each student was disciplined on average 0.08 times during the year, a reduction of 60% compared with control schools and twice the average effect of the anticonflict groups.

We next turn to peer-to-peer social influence effects. As depicted in Fig. 3, we found a significant social influence effect attributable to seed students, and particularly as a result of social referent seeds. By the end of the year, relative to all other levels of exposure to the treatment, students exposed to social referent seeds were more likely to report in the survey that a friend discussed how to reduce conflict with them (Fig. 3*A*), with the effect relative to students in control schools reaching statistical significance ($P < 0.05$, Wald test with randomization-based variance estimate). As predicted, on average students exposed to social referent students also reported shifted perceptions of whether conflict was normative among their peers; they reported that more students in their school disapproved of conflict, relative to students in treatment schools who were not exposed to social referent seeds ($P < 0.01$, in both of two Wald tests with randomization-based variance estimates). This effect size depicted in Fig. 3*B* is equivalent to, for example, moving from a statement that only "a few" students disapprove of racial and ethnic jokes (when students were not exposed to seeds in a treatment school) to stating that "about 75%" of students disapprove (when students were exposed to a social referent seed student). These statements about the undesirability of conflict behaviors, or prescriptive norms, moved in the same manner as those describing the typicality of conflict behaviors, or descriptive norms (*SI Appendix*, Figs. S3 and S6 and Tables S3 and S4).

Students exposed to social referent seeds were more likely to be wearing an orange wristband that seed students had distributed as an award for conflict-mitigating behavior during the year, relative to all other exposure conditions (Fig. 3*C*) ($P < 0.00001$, in each of three Wald tests with randomization-based variance estimates). However, in contrast to our strong climate-level effects, we did not find a statistically significant pattern of peer-to-peer social influence on discipline resulting from peer conflict (Fig. 3*D*).

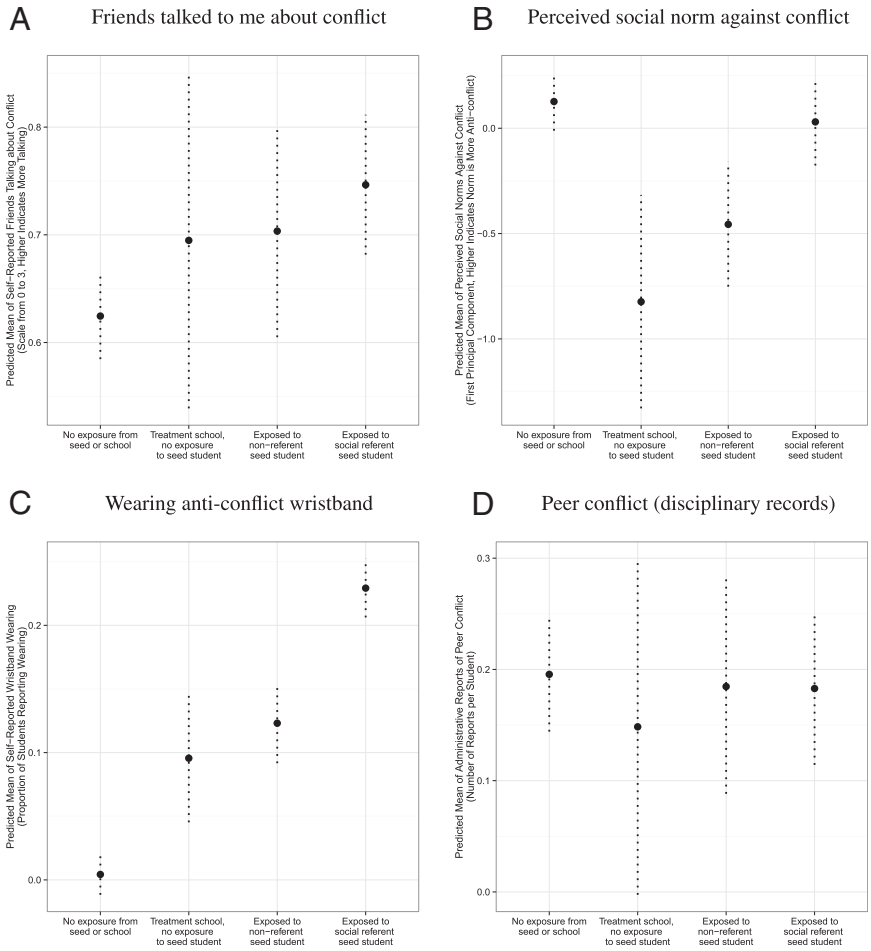

Fig. 3. Causal social influence effects from seed students. The figures illustrate estimates of predicted population means under different levels of exposure and 95% confidence intervals generated via randomly permuting treatment assignment under a maintained hypothesis of constant effects. In examples *A–C*, social referent seed students are most effective at influencing peers. (*D*) We find no evidence of peer social influence on discipline for peer conflict.

Our results for perceived norms underscore the importance of examining differential effects of a community intervention by differential exposure to social influence within a social network. School-wide averages of perceived norms of conflict were indistinguishable between treatment and control schools (Fig. 2*B*). However, within treatment schools, there is clear evidence of anticonflict norm transmission, such that students exposed to treated social referent seeds perceived more anticonflict norms than students exposed to treated nonreferent seeds, who perceived more anticonflict norms than students who were not exposed to any treated seed students (Fig. 3*B*). One explanation may be that the introduction of a community anticonflict intervention increases community attention to conflict. Community members may view the intervention as a signal that their community suffers from worse conflict than they previously thought; this realization and increased discussion of conflict (as shown in Figs. 2*A* and 3*A*) may lead community members to use revised standards for evaluating norms of conflict. Such a phenomenon would account for the fact that control school students' norms are indistinguishable from students exposed to social referent seeds in treatment schools, and are slightly better off than treatment school students who were exposed to nonreferent seeds or to no seeds at all.

Taken together, our results on norms unify two bodies of theoretical predictions regarding norms. First, the lack of differences between school-wide average norms in treatment and control schools supports theories suggesting that interventions can serve as a signal of a problem in the community, which may produce new concerns or standards for evaluating the problem, changes that are not reflected by a commensurate overall shift in reported social norms (19, 51, 52). Second, the norm transmission we identify as coming from social referents supports predictions that exposure to social referents' behavior influences other individuals' normative perceptions and behavior (20, 23). By virtue of randomization both within and across schools, we find that both phenomena may be operative here, and that attention to social referent behavior plays a critical role in shaping perceptions of social norms.

Our results are robust to several alternative explanations and to alternative statistical specifications. We conducted four placebo tests: as expected, our methodology shows no social influence effects on pretreatment norms, or on student attributes like height and weight. Our results are also robust to alternative specifications, including the method used by Bond, et al. in their analysis of social transmission in an online network (15) (*SI Appendix*, Fig. S5). These results parallel those discussed above, demonstrating that the anticonflict treatment spread through the social network, and that the social referent seeds have the greatest influence on their peers' behavior and perceived norms.

Discussion

Despite an enormous surge in policy and research attention, there is little evidence to suggest that anticonflict and antibullying interventions have reduced student conflict or improved school climate. The prevalence of unevaluated school programs, most of which assume that conflict is driven by students' personal characteristics, triggers concern that the programs may be wasting resources or even creating a backlash.

The current intervention was designed from the idea that community members pay particular attention to the behavior of

certain individuals in their community, as they infer which behaviors are socially normative and adjust their own behaviors accordingly. By seeding the network with students who were encouraged to take a public stance against typical forms of conflict at their school, our intervention reduced overall levels of disciplinary reports of peer conflict by an estimated 30% over 1 y. To put this in perspective, our estimates imply that the intervention reduced the total number of disciplinary events from 2,695 events to 2,012 events across the 11,938 students in treatment schools. The intervention successfully spread new anticonflict norms and behaviors through a student network using a small number of seed students encouraged to publicly oppose conflict.

Highly connected students, the social referent seeds, were the most effective at influencing social norms and behavior among their network connections and at the school climate level. Our social influence analyses show that social referent seeds' influence was stronger per student than the influence of nonreferent seeds: a connection with one social referent seed produced greater change in perceived norms of conflict than a connection with a nonreferent seed. Social referents are unusual in terms of their traits, their experiences, and in their capacity for peer-to-peer social influence, which goes beyond the mere structural advantage of having a relatively greater number of connections in the network. Our empirical findings further demonstrate that the social referent's role in affecting change at the climate level is outsized, compared with other students in the network. Our empirical results suggest that future interventions would do well to use as many social referents in their intervention group as possible.

Experiments with social networks of real-world communities can help social scientists to understand the spread of social influence through the sustained behavioral patterns of everyday life. Studying this kind of influence allows for a better understanding of how behavioral climates are produced and changed.

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