

The Propagation of Everyday Prosociality in the Workplace

Joseph Chancellor (josephchancellor@gmail.com)

Seth Margolis^a (seth.margolis@email.ucr.edu)

Sonja Lyubomirsky^a (sonja.lyubomirsky@ucr.edu)

^aUniversity of California, Riverside

900 University Ave.

Riverside, CA 92521

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Correspondence should be addressed to Seth Margolis (seth.margolis@email.ucr.edu).

Abstract

Prosocial behaviors typically benefit those who perform them but can create mixed emotions in recipients. Yet, how does prosociality affect the well-being of those who merely observe it? The current study aimed to answer this question by experimentally prompting employees to perform prosocial acts at work (Givers), be the recipient of such acts (Receivers), or to do neither (Observers). Our focus was on everyday prosociality, which involves kind acts directed at those in one's social circle, rather than at individuals in need. Social proximity to Givers, but not Receivers, positively predicted boosts in well-being. Indeed, social proximity to Receivers was associated with a nonsignificant trend towards decreased well-being. However, both social proximity to Givers and social proximity to Receivers predicted increases in prosocial behavior among Observers. These results suggest that prosocial behavior and its emotional benefits propagate through social networks, particularly for those in close social proximity to prosocial actors.

Keywords: everyday prosociality, well-being, propagation, social proximity, workplace

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Remember there's no such thing as a small act of kindness.
Every act creates a ripple with no logical end.
– Scott Adams

People performing prosocial acts appear to benefit more than the recipients of such acts (Schwartz, Meisenhelder, Ma, & Reed, 2003). Consistent with this finding, research has found that social support is often ineffective (Rafaeli & Gleason, 2009), possibly because recipients (i.e., Receivers) commonly experience negative feelings from being assisted (Fisher, Nadler, & Whitcher-Alagna, 1982). Specifically, support that is apparent to the Receiver is more likely to have adverse effects than invisible support (Bolger, Zuckerman, & Kessler, 2000) because being the beneficiary of a prosocial act can foster feelings of inadequacy and indebtedness (McClure et al., 2014; Rafaeli & Gleason, 2009).

By contrast, those who perform prosocial behavior (i.e., Givers) typically experience decreased negative affect (Rietschlin, 1998). Furthermore, Givers are often rewarded with increases in positive emotion, life satisfaction, and flourishing (e.g., Dunn, Aknin, & Norton, 2008; Layous, Lee, Choi, & Lyubomirsky, 2013; Nelson, Layous, Cole, & Lyubomirsky, in press). Self determination theory (Deci & Ryan, 2000) provides an explanation for why prosociality might foster well-being. Deci and Ryan emphasize three basic needs—connectedness, autonomy, and competence—and performing prosocial behavior appears to fulfill each of these needs. Prosocial acts might satisfy one's need for connectedness by strengthening social ties; they might foster a sense of autonomy by highlighting one's freedom from acting in a purely self-directed manner; and they might lead to meaningful outcomes in others, which forges a sense of competence.

The vast majority of research on prosocial behavior has focused on its effects on Givers and Receivers (see Crocker, Canevello, & Brown, in press, for a review). Yet, prosocial behavior

may also cause collateral effects in the emotions and behaviors of those who witness the behavior (i.e., Observers). Other people in an individual's social network may profit emotionally from the charitable spirit (Hypothesis 1) and even be inspired to act in kind (Hypothesis 2). Alternatively, Observers could feel rejected because they were not chosen as a recipient of a prosocial act, rendering them less likely to be generous themselves. Observers might also feel guilty for not being generous themselves, which would increase their negative affect but could motivate them to behave more prosocially. Furthermore, prosocial behavior may have positive effects for some Observers and negative effects for others. Because Givers typically benefit and Receivers frequently experience mixed outcomes, one possibility is that Observers in closer social proximity to recipients are more negatively affected than Observers in closer social proximity to Givers (Hypothesis 3). In the current study, we examined the spillover effects of prosociality in one's social network—by assessing both additional prosocial behavior and increased well-being—to understand if and how prosociality is differentially contagious from those performing prosocial acts versus from those receiving them.

Everyday Prosociality

Prosocial acts are those intended to benefit others (see Penner, Dovidio, Piliavin, & Schroeder, 2005, for a review). Most work on prosocial behavior has examined behaviors that benefit individuals who are in need. However, some research has examined everyday prosociality, which focuses on acts of kindness performed for others in one's social network (Choi & Chou, 2010), revealing a variety of benefits for Givers' families and communities (Morrow-Howell, Hong, & Tang, 2009). A large proportion of this research, however, has used correlational longitudinal studies. To examine the causal relationships between prosociality and well-being, experimental designs are needed. Research using random assignment has

demonstrated that performing acts of kindness increases happiness and provides social benefits (e.g., peer acceptance) and that these effects can last for weeks (Layous, Nelson, Oberle, Schonert-Reichl, & Lyubomirsky, 2012; Lyubomirsky, Sheldon, & Schkade, 2005; Nelson et al., 2015; Sheldon, Boehm, & Lyubomirsky, 2012, Study 2).

Prosociality in the Workplace

Frequent prosocial behavior and high well-being have each been found to produce a range of benefits in the workplace. Prosocial acts reduce burnout and emotional exhaustion (Grant & Sonnentag, 2010) and cultivate empathy and creativity by focusing attention on others (Grant & Berry, 2011). Prosocial acts can even boost productivity (Anik, Aknin, Norton, Dunn, & Quoidbach, 2013). Similarly, well-being is associated with stronger work performance along multiple metrics (Boehm & Lyubomirsky, 2008). Although such previous studies demonstrate the benefits of prosocial behavior in the workplace, none have examined everyday prosociality. In the current study, we investigate whether simple daily acts of kindness are also beneficial and whether their benefits can multiply due to the propagation of such kind acts.

Modeling Prosociality

According to social learning theory (Bandura, 1977), people learn which acts they should perform after observing a behavior and its consequences (i.e., vicarious reinforcement). Indeed, social learning may be one mechanism by which children learn to behave prosocially (Eisenberg & Mussen, 1989). One experiment found that children experimentally assigned to observe prosocial behaviors acted prosocially themselves, whereas children who did not observe prosocial behaviors did not act prosocially (Rosenhan & White, 1967). We hypothesized that adults would also exhibit more prosocial behavior after observing others do acts of kindness (Hypothesis 2).

In line with social learning theory, research suggests that feelings (e.g., happiness) and behaviors (e.g., smoking) can spread from one adult to another (Christakis & Fowler, 2007, 2008; Fowler & Christakis, 2009; Cacioppo, Fowler, & Christakis, 2009). If prosocial behaviors can also spread indirectly through social learning (that is, to an individual who is not the intended recipient), then who is more contagious—the benefactor or the beneficiary? Theories of leadership, for example, hold that individuals are most influential when they become an example for other people to follow (e.g., idealized influence and inspiration in transformational leadership; Bass, 1999), suggesting that Givers may be a particularly compelling vector for spreading prosociality because they become an exemplar for others. However, witnessing the effects of prosociality (e.g., watching others receive prizes; Silvers & Haidt, 2008) can also be uplifting, and thus may encourage observers to act in kind. For example, fundraisers who had personally met beneficiaries worked harder to raise money than those who had only read a recipient's letter (Grant et al., 2007). Identifying whether contact with beneficiaries or benefactors best propagates prosociality has clear implications for understanding how networks spread or inhibit kindness.

Finally, what emotional consequences, such as increased well-being, elevation, or even envy, can arise indirectly from prosociality as it propagates through a social network? Watching a Giver sacrifice for others is likely to be particularly elevating (Algoe & Haidt, 2009), yet some might feel a little guilty from not having been as generous. Identifying with Receivers, by contrast, may prompt both positive feelings (e.g., feeling happy for the recipient) and upward social comparisons or envy (e.g., wishing one would have also benefited from the act). Despite the potentially nuanced experience produced from observing both Givers and Receivers, we believe the weight of the research suggests that observing Givers boosts well-being more than

observing Receivers. For example, studies show that beneficiaries experience negative emotions more often than do benefactors, because receiving help can threaten an individual's autonomy and self-efficacy and cause feelings of indebtedness (Fisher, Nadler, & Whitcher-Alagna, 1982; see also McClure et al., 2014). Due to emotional contagion (Barsade, 2002), we believe people closer to Givers are likely to benefit more than those closer to Receivers (Hypothesis 3). The emotional consequences that others derive from interacting with benefactors and beneficiaries may further explain why and when prosociality is more or less contagious, and once undertaken by others, what sustains its continued practice.

In the present research, we experimentally assigned the role of Givers, Receivers, and Observers, a design that allows us to capture Givers' and Receivers' differential impact on their social networks and to observe emotional changes associated with one's proximity to these key groups of individuals.

Hypotheses

To explore the potential ripple effects of prosociality, we conducted a longitudinal study with a 4-week intervention and 1-month follow-up (see Figure 1) in a naturalistic environment (a corporate workplace in Madrid, Spain). Additional findings from this study are described in Chancellor, Margolis, Bao, and Lyubomirsky (2016).

Defining Prosociality

For the purposes of this study, prosociality was defined as performing acts of kindness for those who do not necessarily need them (i.e., *everyday prosociality*). Participants decided which specific acts constituted kindnesses. Those assigned to be Givers were instructed to act with altruistic motivation (i.e., only for the recipient's benefit); however, their prosocial behaviors could potentially have been motivated by any basis.

One group (Givers) was instructed to perform acts of kindness for others (Receivers). A third group (Observers) was not administered an intervention, but participants in this condition responded to questionnaires about what they observed and how they felt. None of the participants were informed about the research questions driving the study.

We tested the following three hypotheses:

1. **Spillover.** We anticipated that the hedonic benefits of Givers' prosociality would spill over to those they had not directly targeted (i.e., Observers). We expected this contagion effect to be moderated by social interaction; those with more social proximity to Givers and Receivers were predicted to show bigger benefits compared with those with less social proximity. In particular, we expected that participants who are proximate to Givers would report relatively more feelings of elevation—that is, being moved by witnessing others' morally excellent acts.
2. **Inspire Others.** Similarly, Givers and Receivers were expected to inspire acts of kindness in Observers who are socially proximate. Givers may model prosocial behavior for others, whereas Receivers may move others to act prosocially as they embody the positive effects of prosociality done on their behalf.
3. **Givers Most Contagious.** Finally, we anticipated the social propagation effects to be more pronounced and positive for Observers in close proximity to Givers rather than Receivers because Givers are active and inspiring prosocial role models, whereas observing Receivers could be a mixed experience.

Method

Participants

Participants were employees recruited from Coca-Cola Iberia in Madrid, Spain. Out of a total of approximately 1,200 employees, 88 (72.7% female; $M_{age} = 35.60$, $SD = 8.99$; range = 22-

55) participated in the study. Because none of these participants were excluded in our analyses, the sample size was 88 for all of our analyses. They worked in a variety of departments, including Marketing, Accounting, Information Technology, and Customer Care. All instructions and measures were administered in Spanish. If a Spanish translation was not already available, instructions and measures were translated and back-translated following conventional procedures (Brislin, 1970). (See Chancellor et al., 2016, for similar methods and procedures.)

Procedure

Recruitment and cover story. Participants were recruited in their workplace and offered both a prize of university merchandise and a donation to a charitable organization based on their enrollment in the study. All participants were informed that they would be practicing a potentially happiness-increasing activity over several weeks, which might include performing acts of kindness, expressing gratitude, counting blessings, using one's signature strengths, or practicing optimism. We told participants that a computer program would randomly assign them to an activity, that it might change from week to week, and that some would not be assigned any activity for the duration of the study. We instructed everyone to keep their activities confidential and focus only on completing their assignments to the best of their abilities.

Group assignment. We randomly assigned participants to one of three groups: Givers ($n = 19$), Receivers ($n = 35$), and Observers ($n = 34$). We aimed for Receivers and Controls to comprise 40% of the sample each (i.e., 80% total) to provide Givers an opportunity to choose from a list of Receivers and to ensure a sufficient distribution of participants in the Control group with high and low social proximity to Givers and Receivers. None of the participants were aware of their group assignment or that examining prosociality was the true purpose of the study. They

were only informed of their activity instructions each week. Thus, Receivers were not aware that Givers had been assigned to do acts of kindness on their behalf.

Measurement occasions. Participants logged into the study website every week for 4 weeks to complete surveys and perform their assigned activity. Participants also completed weekly outcome measures throughout the intervention. Monthly outcome measures were completed at baseline, the end of the intervention, and at a 1-month and 3-month follow-up. (See Figure 1 for an overview of study procedures and timeline.)

Materials

Prosocial acts intervention. We asked Givers to do five prosocial acts (i.e., acts of kindness) in one day for recipients on a specific list (see Appendix for complete instructions). We emphasized that Givers could choose the specific types of activities they performed, when they performed them, and whom they chose from their randomized lists of potential recipients. To assist Givers in selecting prosocial acts, we offered ideas such as ‘bringing someone a beverage,’ ‘cheering up a coworker who seems to be having a bad day,’ and ‘emailing a thank you note.’ Our examples varied from week to week and included sacrifices of time, resources, and money. Although the specific acts of kindness that Givers did were likely to be known or directly observed by others, we instructed Givers to keep the actual details of their positive activity assignment secret.

Givers performed their acts of kindness for Receivers each week. At the beginning of the study, we created a customized, randomized Receiver list for each Giver. Each week’s list included 10 coworkers’ names (from the Receivers group) and these lists differed for each of the 4 weeks of the intervention. Each Receiver appeared on an average of 2.5 Givers’ lists per week.

We emailed this weekly list to Givers with instructions to refer to it for their assigned activity but keep it confidential.

Like Receivers, Observers did not perform any activity assignments throughout the study. However, unlike Receivers, Controls were never direct recipients of assigned acts of kindness. Rather, they could only witness acts of kindness performed by Givers, as well as the potential behavioral and emotional effects of such acts on both Givers and Receivers.

Social network. In the baseline questionnaire, participants nominated up to 15 individuals with whom they normally socially interact at work. They listed other coworkers in the office, whether or not they were also participating in the research study. We used mutual ties (i.e., instances in which two coworkers were both participating in the study and nominated one another) to analyze the spread of well-being outcomes.

Weekly outcomes.

Prosocial behaviors. All participants were presented a list of positive and negative workplace behaviors and asked to recall the frequency with which they observed specific instances of each of these behaviors performed by others and themselves over the previous week. Positive behaviors comprised acts like ‘expressing sincere gratitude for a coworker’ and ‘performing an unexpected act of kindness.’ Negative behaviors included ‘repeating gossip or rumors about a coworker’ and ‘insulting a coworker.’ We summed positive behaviors and subtracted negative behaviors to compute a total count. Due to participants’ tendencies to report more positive than negative behaviors, this total was almost always positive.¹ We have two primary prosocial behavior variables: others’ prosocial behaviors and own prosocial behaviors. After subtracting negative behaviors from positive behaviors, the average number of others’

¹In the rare case that summation led to a negative value, this value was changed to zero.

prosocial behaviors was 4.97 per week and the average number of own prosocial behaviors was 6.32 per week. The average number of others' negative behaviors was 1.40 per week and the average number of own negative behaviors was 0.43 per week.

Need satisfaction. Participants reported on three types of need satisfaction (i.e., feelings of connectedness with others, feelings of autonomy, and feelings of competence; Deci & Ryan, 2000; Sheldon, Elliot, Kim, & Kasser, 2001). These constructs were assessed with three sets of 3-item measures. Example items include 'I felt a sense of contact with people who care for me, and whom I care for' (connectedness), 'I was free to do things my own way' (autonomy), and 'I was successfully completing difficult tasks and projects' (competence). Participants rated their level of agreement with each item on 7-point Likert-type scales (1 = *not at all*, 4 = *somewhat*, 7 = *very much*). Across all time points, Cronbach α s ranged from .74 to .91 for autonomy, .76 to .83 for competence, and .71 to .88 for connectedness.

Weekly affect and satisfaction. The brief Weekly Affect and Satisfaction Measure (Jacobs Bao, 2012) is designed for repeated measurements over short time periods and asks, 'How have you been feeling in the last week?' (-10 = *extremely negatively*, 10 = *extremely positively*) and 'How satisfied with your life have you been in the last week?' (-10 = *extremely satisfied*, 10 = *extremely dissatisfied*).

Elevation. Participants reported their feelings of elevation on a 7-item questionnaire (Algoe & Haidt, 2009). Examples items include feeling 'moved or uplifted' and 'optimistic about humanity' (1 = *strongly disagree*, 7 = *strongly agree*). Across all time points, α s ranged from .82 to .87.

Monthly outcomes.

Happiness and life satisfaction. The Subjective Happiness Scale (Lyubomirsky & Lepper, 1999) is a 4-item measure that asks respondents to rate on 7-point Likert-type scales how generally happy they are (1 = *not a very happy person*, 7 = *a very happy person*), how happy they are relative to their peers (1 = *less happy*, 7 = *more happy*), and the extent to which a description of either a ‘very happy’ or ‘very unhappy’ person (two separate items) characterizes them (1 = *not at all*, 7 = *a great deal*). Across all time points, α s ranged from .69 to .83.

The Satisfaction With Life Scale (Diener, Emmons, Larsen, & S. Griffin, 1985) is a 5-item measure of global life satisfaction. Sample items include ‘In most ways my life is close to ideal’ and ‘I am satisfied with my life’ (1 = *strongly disagree*, 7 = *strongly agree*). Across all time points, α s ranged from .78 to .91.

Job satisfaction. The Overall Job Satisfaction Scale (Cammann, Fichman, Jenkins, & Klesh, 1983) is a 3-item measure that assesses employees’ liking and satisfaction with their job. Participants rated their level of agreement with statements such as ‘I like working here’ on 7-point scales (1 = *strongly disagree*, 7 = *strongly agree*). Across all time points, α s ranged from .79 to .83.

Depression. The Quick Inventory of Depressive Symptomatology Self-Report (QIDS-SR; Rush et al., 2003) is a measure of depressive symptom severity. The 16 items address sleep problems, appetite/weight issues, sadness, lethargy, and restlessness.

Timeline. As illustrated in Figure 1, participants completed weekly outcomes at baseline (Week 0), during each week of the 4-week intervention (Weeks 1-3), and immediately post-intervention (Week 4). They completed monthly outcomes at baseline (Week 0), post-intervention (Week 4), the 1-month follow-up (8 Weeks), and the 3-month follow-up (16 Weeks).

Analytic Approach

Behavioral Outcomes. Because discrete data violates assumptions inherent in OLS regression, for reports of others' and own prosocial behaviors, we used the lme4 package in R to run mixed-effects models with the Poisson family using a log link. Given that Poisson regression makes strict assumptions about the means and variance of the data, we estimated an additional random effect to control for over-dispersion. With a log link, coefficient estimates indicate that every 1-unit increase in the predictor results in a $e^{\text{coefficient}}$ multiplicative change in the dependent variable.

To examine proximity effects (i.e., being influenced by a Giver or Receiver), we analyzed all participants according to their proximity to Givers and Receivers using the following model:

Level 1 model:

$$\log(Y_{ij}) = \beta_{0j} + r_{ij}$$

Level 2 model:

$$\beta_{0j} = \gamma_{00} + \gamma_{01}Y_{\text{baseline}} + \gamma_{02}\text{PROXIMITY}_{\text{Giver}} + \gamma_{03}\text{PROXIMITY}_{\text{Receiver}} + u_{0j} + u_{1j}\text{OD}$$

Full model:

$$\log(Y_{ij}) = \gamma_{00} + \gamma_{01}Y_{\text{baseline}} + \gamma_{02}\text{PROXIMITY}_{\text{Giver}} + \gamma_{03}\text{PROXIMITY}_{\text{Receiver}} + (u_{0j} + u_{1j}\text{OD} + r_{ij})$$

Weekly and Monthly Outcomes. Using the lavaan library in R, we conducted latent growth curve modeling to investigate changes in weekly and monthly outcomes. We employed full information maximum likelihood estimation (Enders & Bandalos, 2001) with robust standard errors. The growth models for weekly and monthly outcomes are displayed in Figures 2 and 3.

Results

Baseline Analyses

No significant differences for any of the weekly or monthly outcome variables (all $ps > .27$) emerged among our three groups at baseline, indicating that random assignment was successful. However, in analyses involving reports of others' and own prosocial behaviors, we control for baseline levels.²

Completion Rates

The percentages of participants completing each measurement occasion were as follows: Baseline: 100%; Week 1: 94.3%; Week 2: 84.1%; Week 3: 68.2%; Week 4: 55.7%; 1-Month Follow-Up: 34.1%; 3-Month Follow-Up: 26.1%. Our sample in this naturalistic field study was relatively small and suffered from attrition. Thus, cell sizes at the end of the experiment are quite small (see Table 1). In light of this, findings involving follow-up data should be interpreted cautiously. However, the use of repeated measures boosted our power in two ways. First, our use of repeated measures expanded our total number of observations to 407 (see Table 1). Second, our use of repeated measures allowed for within-subject analyses, which eliminates error due to individual differences present in between-subject analyses.

Fortunately, with one exception, we found no differences in baseline levels of any outcome measure between those who did and did not complete later time points in the study (all

²Although we randomly assigned Givers and Receivers to two different groups, we could not randomize social relationships to the individuals in those groups, and thus needed to control for any baseline differences that might arise from preexisting differences in personality or other factors.

$ps > .18$): Participants who completed the 1-month follow-up were marginally higher in feelings of competence at baseline than those who did not, $t(86) = 1.73, p = .087$. Attrition did not vary based on group assignment (all $ps > .55$).

Workplace Social Network

The workplace network contained 451 ties (i.e., nominations from one employee to another), of which 325 (72.1%) were made to other participants in the study. Of all ties, 122 (35%) were mutual (i.e., employees nominated each other). On average, the workplace network contained 2.83 ties per employee, and overall, the network density (i.e., proportion of actual ties to possible ties) was relatively sparse ($\delta = .002$ out of 1.000). Figure 4 displays all participants and their nominations to each other in a social network map using a Fruchterman-Reingold (1991) layout.

Giver and Receiver Proximity

Others' and own behaviors. Were Givers' acts of kindness visible to others in the office environment and did their acts of kindness inspire these observers to also act prosocially (i.e., the Inspire Others hypothesis)? We used participants' direct proximity to Givers and Receivers to predict differences in reports of others' and own prosocial behaviors (see Table 2 and Figure 5). We found that controlling for baseline levels of reports of others' and own prosocial behaviors, each direct connection to a Giver was marginally associated with 94% more observed prosocial behaviors throughout the intervention and follow-up period, $b = 0.66, SE = 0.40, t = 1.65, p = .099$. However, connections to Receivers were not associated with reports of others' prosocial behaviors.

We also found that controlling for baseline levels, each additional direct connection to Givers was associated with 89% more prosocial behaviors of one's own, $b = 0.63, SE = 0.32, t =$

1.99, $p = .047$. Furthermore, each additional connection to Receivers was associated with 63% more prosocial behaviors, $b = 0.49$, $SE = 0.19$, $t = 2.51$, $p = .012$. Thus, consistent with the Givers Most Contagious hypothesis, Givers appeared to propagate prosociality marginally more than did Receivers.

Weekly and monthly well-being outcomes. Did colleagues who interacted with Givers and Receivers also demonstrate spill-over benefits for their well-being (i.e., the Spillover hypothesis)? We found that one's social proximity to Givers (see Table 3 and Figure 6) predicted increases in weekly satisfaction, $b = 0.257$, $SE = 0.12$, $\beta = .320$, $p = .033$, weekly affect, $b = 0.27$, $SE = 0.127$, $\beta = .239$, $p = .034$, elevation, $b = 0.069$, $SE = 0.031$, $\beta = .269$, $p = .023$, and connectedness, $b = 0.048$, $SE = 0.023$, $\beta = .192$, $p = .038$. However, we found no significant Giver-proximity effects for competence, autonomy (both $ps > .24$), or any of our monthly outcomes (all $ps > .10$, see notes on Table 3 for estimates).

Interestingly, we found no significant influence of Receivers on others in their social circles (see Table 3). Whereas estimates of the effects of Givers on their social networks were always positive (but not always significant), estimates of the effect of Receivers on their social network were negative (albeit nonsignificantly so).

Discussion

Prosociality's Rewards Spill Over

Supporting our Spillover and Givers Most Contagious hypotheses, Givers benefited other coworkers in their social networks, even though these individuals were not the explicit targets of Givers' prosocial behavior: As they practiced prosociality, Givers made Observers feel happier, more connected to one another, more elevated, and more satisfied with their lives. However, no social proximity effects were evident at the 1-month follow-up (i.e., in monthly outcomes),

which could be the result of low-powered tests (i.e., cell sizes were very small at the 1-month follow-up) or evidence that social influences are relatively weak and time-limited. Givers benefited the well-being of those in their social networks more than did Receivers, whose influence was either neutral or even slightly negative, which supports our Givers Most Contagious hypothesis—namely, watching someone *exhibit* kindness produces more of a positive impact than watching someone *receive* kindness. Such results are consistent with recollection experiments, which have shown more prosociality after reflecting on a memory of giving rather than receiving (Grant & Dutton, 2012). Givers' positive influence on their social networks may partially explain why their generosity is relatively contagious to others.

Interestingly, Receivers had a nonsignificant negative effect on the well-being of those around them. The potential negative influence of Receivers on their social circles could be due to social comparison effects: Throughout the entire intervention, Receivers were always recipients of others' prosociality, even though the Givers who performed acts of kindness for them varied each week, and those close to Receivers were bound to notice the upward comparison.

Why did Observers benefit when others in their social network performed prosocial acts? At least three explanations are possible. First, directly witnessing prosocial acts, which promotes feelings of elevation and warmth, could lead people to feel happy. Second, observing Givers' heightened well-being might drive increases in well-being via emotional contagion. Third, observing prosocial acts triggers one's own prosocial behavior, and this might foster increases in well-being among Observers. This last option, however, is unlikely, considering that workers who were socially proximate to Receivers performed more prosocial acts but did not experience increases in well-being themselves. We prefer the first two explanations, but more data are needed to test them.

Prosociality Inspires Others to Act

Supporting our Inspire Others hypothesis, we found that an individual's social proximity to both Givers and Receivers predicted more prosocial behaviors. Our results suggest that Givers' acts of kindness were visible to others with whom they interacted, leading those others to report that they observed marginally more positive and less negative behaviors around the office. And although Receiver proximity did not predict more observed prosocial behaviors, direct connections to Receivers were associated with more reported acts of kindness throughout the study. Thus, although these results support our hypothesis that social connections to Receivers would also predict more prosocial behaviors, the reasons that Receivers were also socially propagating prosociality are not entirely clear. Those in Receivers' social networks may be noticing Receivers' own emotional benefits, even if they are unaware of the acts of kindness that led to these benefits.

Each connection to a Giver was associated with reporting 89% more prosocial behaviors and each connection to a Receiver was associated with reporting 63% more prosocial behaviors—differences that are also consistent with our hypothesis of greater Giver influence. Our findings show that prosociality may arise spontaneously from being around a prosocial coworker or observing the impact that another's prosociality has on a coworker—with the former producing emotional benefits, as well as potentially more behavioral change than the latter. Social learning theory suggests that observing both prosocial behavior and its consequences leads to social learning, and vicarious reinforcement in particular, which explains why prosociality propagated throughout the social network.

Limitations and Future Questions

Because we conducted a field study in a workplace setting, we were not able to obtain a large sample size. Our sample of 88 participants was split among three conditions and suffered from attrition. Although this led to small cell sizes (see Table 1), our study did employ repeated measurements, boosting our power. Future investigators may wish to replicate our findings with larger and more diverse samples.

Because our study was characterized by considerable attrition beginning at 4 weeks, and escalating at the 1-month and 3-month follow-ups, the participants who opted to continue with the study could have differed in substantive ways from those who dropped out. However, we only found one such difference (involving self-reports of competence). Although those who persisted with the intervention did not differ in their baseline levels of other well-being measures from those who dropped out, we cannot fully eliminate the possibility that participants experienced different reactions to the interventions that affected whether they continued or withdrew.

Because we randomly assigned participants to groups but could not randomly assign social relationships, our proximity results are only somewhat suggestive of causality. Because of this limitation, we focused on how Givers and Receivers influenced those in their social networks after controlling for preexisting baseline differences. To provide even stronger causal evidence, researchers would need to experimentally manipulate both the practice of prosociality and participants' social proximity to Givers and Receivers. Although, for ethical reasons, experimenters may not wish to reduce naturally occurring social interactions (e.g., 'Don't talk to Dave this week'), they could increase them (e.g., 'Make sure you talk to Julia this week').

Relatedly, we do not know how often Receivers were the targets of prosocial acts. Hence, receiver proximity effects may have been nonsignificant because some Receivers may have been infrequent targets. However, this possibility is relatively unlikely, because each Receiver appeared on an average of 2.5 Givers' lists per week.

Because the incentives we offered for participation included charitable rewards, this procedure may have generated a sample composed of a higher proportion of prosocially inclined individuals than was natural in the population. However, we offered both personal and prosocial rewards—a prize of university merchandise and a donation to a charitable organization, respectively. Thus, our sample likely included participants with a variety of motivations. Furthermore, given that approximately one-third of employees participated, the effects that we observed should still generalize to a sizable percentage of the entire organization. A second selection effect could have stemmed from Givers picking Receivers based on nonrandom characteristics (such as extraversion); notably, however, their choices were limited to a list of randomly selected employees. Lastly, all participants were informed they would be engaging in a potentially happiness-increasing activity—a design that may have oversampled happiness seekers. Although these factors may have biased our sample selection, our use of random assignment to conditions ensures that our analyses were not impacted by these selection effects.

Of course, typically, people do not choose the beneficiary of their prosocial acts in daily life from a preprinted list, as Givers did in our study. Although this design limits our external validity, it is likely that Givers chose the Receivers on their list whom they knew best and we expect that many of these Receivers would have been their beneficiaries under more natural circumstances.

We instructed Givers to keep their activity assignment secret, and to the best of our knowledge, they followed our instructions. Indeed, because we encouraged everyday prosociality, it is likely that the behaviors of Givers did not stand out as particularly unusual. Thus, Receivers likely did not suspect that the kind acts done to them were the result of the experiment. However, even if “contamination” had occurred, we would argue that our findings are important. Contamination (i.e., social propagation) is a fundamental element of our hypotheses, and we instructed Givers to keep their activity assignment (but not necessarily their acts) confidential to minimize artificiality (i.e., experimenter-prompted changes) in favor of naturalistic contamination (i.e., individuals acting kindly under their own volition).

Concluding Words

The benefits of prosocial behavior extend not only to those who perform it and those who receive it: Our results demonstrate that people who simply observe kind acts benefit as well. In particular, we predicted that those closer (versus farther) in social proximity to Givers and Receivers would report relatively larger boosts in well-being. However, this prediction was only supported with respect to Givers.

Notably, prosocial behavior also inspired others to act in similar, positive ways. Taken together, our findings demonstrate that kind acts and their emotional consequences are contagious, particularly from prosocial actors. Hence, prosociality operates not only on an individual level. Prosocial behavior propagates, potentially influencing not just benefactors and beneficiaries, but observers, neighbors, and communities.

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Appendix
Acts of Kindness Instructions

In our daily lives, we all perform acts of kindness for others. Within the next week (7 days from today), you are to perform five acts of kindness for the assigned coworkers on your list – all five in one day.

These acts may be:

- Large or small
- Anonymous or identified
- Sacrifices of time, energy, or money

Some ideas for acts of kindness include:

- Helping a coworker complete a task above and beyond your normal job duties.
- Bringing someone a beverage, such as a soda, energy drink, hot coffee, or tea, without them asking.
- Writing or emailing a thank you note.
- Giving someone a gift card to a favorite store or restaurant.
- Making a special attempt to recognize someone who often gets overlooked.
- Helping someone carry their stuff.
- Leaving a flower on coworker's desk.
- Spending time learning more about someone else's life.
- Telling a coworker something that you noticed they do well.
- Cheering up someone who seems to be having a bad day.
- Your own ideas!

We encourage you to select recipients with a singular focus on benefiting another person. Do not anticipate receiving any particular response, such as being thanked, appreciated, engendering favor, or benefiting from any reciprocal prosociality . In short, act prosocially and expect nothing in return.

As much as you can, please keep the details of this assignment confidential! Of course, you may attract attention from your acts of kindness. Keep in mind that while this activity was assigned to you, you are completely free to choose what you want to do and for whom you want to do it. If people ask about your motivation, you could say:

- I thought you/he/she would like it.
- It seemed like a nice thing to do.

Also, do not perform any acts that may place yourself or others in danger.

Next week you will report what acts of kindness you chose to perform, what day you did them, and for whom you did them.

q I have read these instructions

Table 1

Sample Size by Condition and Time Point

Condition	Baseline	Week 1	Week 2	Week 3	Week 4	Week 8	Week 16
Givers	19	16	13	10	8	4	4
Receivers	35	34	32	26	24	14	12
Controls	34	33	29	24	17	12	7
Total	88	83	74	60	49	30	23

Table 2

Changes in Others' and Own Behaviors by Giver and Receiver Proximity

Variable	<i>Initial Value</i>			<i>Giver Proximity</i>				<i>Receiver Proximity</i>			
	<i>b</i>	<i>SE</i>	<i>t</i>	<i>b</i>	<i>RR</i>	<i>SE</i>	<i>t</i>	<i>b</i>	<i>RR</i>	<i>SE</i>	<i>t</i>
Others' Prosocial Behaviors	0.09	0.02	3.97***	0.66	94%	0.40	1.65†	0.20	22%	0.24	0.83
Own Prosocial Behaviors	0.13	0.02	6.34***	0.63	89%	0.32	1.99*	0.49	63%	0.19	2.51*

Note. RR = risk ratio. † $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

Table 3

Changes in Weekly Outcomes by Giver and Receiver Proximity

Outcome	N (df)	Time Points (Time Span)	Giver Proximity			
			β	SRMR	RMSEA	PCLOSE
Connectedness	88 (11)	5 (4 Weeks)	.192 *	.061	.000	.765
Competence	88 (11)	5 (4 Weeks)	.142	.053	.000	.597
Autonomy	88 (11)	5 (4 Weeks)	.047	.069	.030	.579
Elevation	88 (11)	5 (4 Weeks)	.269 *	.047	.000	.862
Weekly Affect	88 (11)	5 (4 Weeks)	.239 *	.074	.030	.578
Weekly Satisfaction	88 (11)	5 (4 Weeks)	.320 *	.055	.000	.722

Outcome	N (df)	Time Points (Time Span)	Receiver Proximity			
			β	SRMR	RMSEA	PCLOSE
Connectedness	88 (11)	5 (4 Weeks)	.057	.069	.080	.228
Competence	88 (11)	5 (4 Weeks)	-.202	.051	.080	.233
Autonomy	88 (11)	5 (4 Weeks)	-.103	.069	.010	.660
Elevation	88 (11)	5 (4 Weeks)	-.176	.061	.040	.551
Weekly Affect	88 (11)	5 (4 Weeks)	-.032	.056	.000	.914
Weekly Satisfaction	88 (11)	5 (4 Weeks)	-.044	.047	.000	.847

Note. * $p < .05$. Models using proximity to predict Monthly outcomes did not fit well, and were not included above. The following estimates were from models of Giver proximity—happiness: $\beta = -.032, p = .836$; life satisfaction: $\beta = -.168, p = .254$; job satisfaction: $\beta = -.197, p = .338$; and depressive symptoms: $\beta = .144, p = .544$. For Receiver proximity—happiness: $\beta = -.177, p = .281$; life satisfaction: $\beta = -.242, p = .142$; depressive symptoms: $\beta = .204, p = .416$; and job satisfaction: $\beta = .205, p = .106$.

Figure 1. Study timeline and order of materials.

Figure 2. Latent growth curve model of weekly outcomes by social proximity.

Figure 3. Latent growth curve model of monthly outcomes by social proximity.

Figure 4. Social network map of all participants and their nominations to each other in a Fruchterman-Reingold (1991) layout.

Figure 5. Scatterplots of observed and performed behaviors (square rooted) reported by Giver and Receiver Proximity. Due to the nature of count data, distributions are not Gaussian and appear highly positively skewed; thus, they were transformed for graphing.

Figure 6. Weekly outcomes by giver proximity over the intervention period (4 weeks). Higher numbers indicate that the participant was more socially proximate to Givers.