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Ripples of Generosity in the Workplace: The Benefits of Giving, Getting, and Glimpsing

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by

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ABSTRACT OF THE DISSERTATION

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Does generosity spur collateral benefits as well as direct ones? I hypothesized that others in one's social network may profit from the charitable spirit and even be inspired to act in kind. In a longitudinal study, participants were randomly assigned to be Givers, Receivers, or Controls. Givers performed acts of kindness over 4 weeks for customized, randomized groups of Receivers who were unaware of the Givers' assignment. Results showed that both Givers and Receivers mutually benefit in well-being from the Givers' practice of generosity, that receiving generosity is an unequivocally positive experience, and that the rewards of practicing generosity spill over to others in the Givers' social networks. Furthermore, Receivers spontaneously chose to practice their own acts of kindness—demonstrating a pay-it-forward effect—and both Givers and Receivers inspired others in their social networks to act kindly towards others. This study advances scientific understanding of how generosity operates in the real world—how individuals practice it, profit from it, and propagate it to others.

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"The great acts of love are done by those who are habitually performing small acts of kindness."
– Victor Hugo, *Les Misérables*

"It is one of the most beautiful compensations of this life that no man can sincerely try to help another without helping himself."
– Ralph Waldo Emerson

By the time most people learned that Chuck Feeney, founder of Duty Free Stores, had become a billionaire, he wasn't: Most of his wealth had already gone to charity (O'Clery, 2008). Feeney later wrote, "I cannot think of a more personally rewarding and appropriate use of wealth than to give while one is living." Although strict reciprocity is perhaps one of the oldest human norms (Apicella, Marlowe, Fowler, & Christakis, 2012), the fact that most cultures hold generosity as a virtue suggests that even when unrequited, taking a voluntary loss for another's gain may be mutually beneficial. Although a number of correlational and experimental studies have examined the consequences of generosity for either the actor or the recipient, very few have captured how such charitable overtures affect both parties' well-being—especially when practiced over months instead of minutes.

Another unanswered question is whether the rewards of generosity are limited only to those who "pay" and those who "profit." After all, generosity may spur collateral benefits: Others in an individual's social network may profit from the charitable spirit and even be inspired to act in kind. Our study aims to investigate the hedonic and behavioral consequences of the everyday practice of generosity not only for givers and receivers, but also for observers.

Generosity as a Solitary Decision

A number of experiments on generosity have measured the impact of solitary decisions that experimenters have predetermined to be manifestations of generosity (e.g., prosocial spending; Dunn, Aknin, & Norton, 2008). However, what participants themselves consider generous may vary among individuals, age groups, and cultures (Eisenberg & Mussen, 1989). Thus, single-choice research designs may not capture the diversity of participants' conceptions of generosity or the impact of the sustained practice of generosity over time—both of which become important when results are generalized to everyday life. For example, a single act of generosity is likely not to confer the same benefits when repeated, because individuals acclimate emotionally to changes over time (i.e., due to hedonic adaptation; Sheldon, Boehm, & Lyubomirsky, 2012).

Other experimental paradigms that have manipulated generosity operationalize it as a fair or generous division of a monetary windfall within the context of economic games (e.g., DeSteno, Bartlett, Baumann, Williams, & Dickens, 2010). Generous behavior, however, encompasses much more than the transfer of money: Sacrifices of time (e.g., offering a shoulder to cry on), talent (e.g., fixing a friend's car), or even tissue (e.g., donating blood or bone marrow) can all be generous, yet none of these acts involve the exchange of treasure.

Generosity as a Sustained Practice

Correlational longitudinal studies can more naturalistically examine generous behavior over sustained periods of time (e.g., charitable giving, volunteering, etc.; Choi & Chou, 2010). However, without the benefit of random assignment, such studies can

only implicate, but not fully disentangle generosity's complex causes and consequences. Fortunately, experimental studies in which participants are prompted to practice generosity in their daily life over a period of time combine the advantages of longitudinal studies and laboratory experiments. In fact, in controlled experiments, the practice of kindness indeed boosts happiness and produces social benefits, sometimes even weeks later (Della Porta, Jacobs Bao, Lee, Choi, & Lyubomirsky, 2012; Layous, Nelson, Oberle, Schonert-Reichl, & Lyubomirsky, 2012; Lyubomirsky, Sheldon, & Schkade, 2005; Sheldon et al., 2012; Study 2). In the current study, we aim to extend the previous work on the benefits of practicing kindness by measuring the differential impact on both receivers and givers over longer time periods, as well as examining the "spillover" effect of generosity on third parties.

The Spread of Generosity

A key question is how generosity develops naturalistically (without having been directly manipulated by the experimenter). Although humans are probably bestowed with innate prosocial tendencies (Warneken & Tomasello, 2009), a good deal of generosity is likely spread socially—inspired and reinforced by strangers, friends, parents, or role models. In fact, a growing number of observational studies suggest that many human states and behaviors can propagate from person to person, including obesity (Christakis & Fowler, 2007), smoking (Christakis & Fowler, 2008), happiness (Fowler & Christakis, 2009), and loneliness (Cacioppo, Fowler, & Christakis, 2009).

Researchers have examined the social contagion effects of generosity in experimental economic games. They found that generous allocations of resources could

indeed spread from person to person (DeSteno, Bartlett, Baumann, Williams, & Dickens, 2010; Gray, Ward, & Norton, in press; Fowler & Christakis, 2010). Indeed, controlled, experimental studies that include both givers and receivers can be difficult to design without the use of economic games. For this reason, to our knowledge, only a few non-economic experiments have included both givers and receivers in the same study, and few of these studies included participants who were actually recipients of other participants' generosity (e.g., Weinstein & Ryan, 2010). In the present research, we experimentally assigned the role of givers, receivers, and observers (i.e., others who may collaterally benefit from givers' generosity or propagate it to others). Our aim was to employ a broader conceptualization of generosity in a naturalistic environment to complement prior studies and advance researchers' understanding of how generosity operates in the real world.

Good to Receive?

Benefiting others is the most straightforward motivation that underlies generous behavior (Dovidio, Piliavin, Schroeder, & Penner, 2006). However, recipients' actual responses to being "helped" can be mixed (Fisher, Nadler, & Whitcher-Alagna, 1982). For example, receiving assistance from another can threaten the recipient's self-esteem (e.g., if it implies that one must be having difficulties) or incur an emotional cost (Bolger, Kessler, & Zuckerman, 2000). In marriage, spouses' reports of having made major sacrifices are associated with decreased marital quality, more marital conflict, and higher likelihood of divorce (Dew & Wilcox, 2009). Likewise, giving (but not receiving) social support predicts longevity in older married adults, after controlling for the other type of

support (Brown, Nesse, Vinokur, & Smith, 2003). In sum, although generous people may intend to benefit their recipients, we aim to validate that such overtures actually have their intended positive impact.

Generosity at Work

Managers and CEOs have a special interest in cultivating generosity in workplaces: Organization citizenship behaviors (OCBs; an operationalization of generous employee behavior) relate to a number of benefits for employees (e.g., manager ratings), managers (e.g., employee turnover, absenteeism), and organizations (e.g., customer satisfaction; Podsakoff, Whiting, Podsakoff, & Blume, 2009). Although researchers have examined a number of antecedents of OCBs, such as personality, attitudes, task characteristics, perceptions of fairness, and the influence of leadership, to our knowledge, no previous studies have used the practice of generosity in a workplace as a way of potentially boosting OCBs. By contrast, we conducted the present study entirely in a work setting at a multi-national company to determine whether practicing generosity could spur beneficial work-related outcomes.

Primary Aims and Hypotheses

To explore the benefits of practicing generosity, the benefits of receiving generosity, and the potential ripple effects to others, we designed a longitudinal study (which included baseline measures, a 4-week active intervention, and two monthly follow-ups) and conducted it in a naturalistic environment (a corporate workplace in

Madrid, Spain). Notably, we used experimental methods—the random assignment of Givers and Receivers to groups—that allow for causal inferences.[1]

In this study, we used a strictly behavioral (rather than motivational) definition of generosity, defining it as the practice of performing acts of kindness for others. Also, rather than track the consequences of a single individual decision, we asked participants to practice performing acts of kindness for others over several weeks. We tested the following five hypotheses:

1. **Mutual-benefit (Givers and Receivers)**. We hypothesized that both Givers and Receivers would benefit in well-being (e.g., happiness and need satisfaction) from being assigned to perform and receive acts of kindness, respectively.
2. **Spill-over (Observers)**. We anticipated that the well-being benefits of Givers' generosity would spill over to those they had not directly targeted. Because this contagion effect should be mediated by social interaction, we hypothesized that those with the highest social proximity to Givers and Receivers would show bigger benefits compared with those who have little to no social proximity to Givers and Receivers.
3. **Pay-it-forward (Receivers)**. Givers' generous behavior was also expected to be "contagious." We hypothesized that Receivers would spontaneously perform their own acts of kindness for others (i.e., exhibit a pay-it-forward effect), even though no one had instructed them to do so.

[1]However, because we could not randomly assign social relationships, our findings regard participants' proximity to Givers and Receivers are suggestive, but not conclusive, of causality.

4. **Inspire-others (Observers)**. Similarly, Givers and Receivers were expected to inspire acts of kindness in observers who are socially proximate to them. Givers may model generous behavior for others, whereas Receivers may move others to act generously as they enjoy and appreciate the acts of kindness done on their behalf.
5. **Greater-Giver-influence (Observers)**. Finally, we anticipated the social propagation effects to be more pronounced and positive for observers close in social proximity to Givers than to Receivers. Watching a Giver sacrifice for others is likely to be particularly elevating (Algoe & Haidt, 2009), as those who identify with the Giver imagine themselves also acting generously. However, observing a Receiver could be a more nuanced experience: Identifying with Receivers may prompt positive feelings (e.g., feeling happy for the recipient), as well as upward social comparisons or envy (e.g., wishing one would have also benefited from the act).

Method

Participants

Employees of Coca-Cola Iberia in Madrid, Spain (N = 94, 72% female) participated in the study. Their ages ranged from 22 to 61 (M = 35.60, SD = 8.99), and they worked in a variety of departments, including Marketing, Accounting, Information Technology, and Customer Care. All instructions and measures were completed in Spanish. If a Spanish translation was not already available, instructions and measures were translated and back-translated following conventional procedures (Brislin, 1970).

Procedure

Recruitment and cover story. We recruited participants in their workplace and offered them a small prize of university merchandise, as well as a donation to a charitable organization based on enrollment in the study. We told participants that they would be practicing a potentially happiness-boosting activity over a number of weeks, which might include performing acts of kindness, expressing gratitude, counting blessings, using one's signature strengths, or practicing optimism. Our instructions informed participants that the computer 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 all participants to keep their activities confidential and focus only on completing their assignments to the best of their ability.

Group assignment. We randomly assigned participants to one of three groups: Givers (n = 21), Receivers (n = 37), and Controls (n = 36). We planned for Receivers and Controls to comprise 40% of the sample each (i.e., 80% total) to allow Givers 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.

Measurement occasions. Participants logged into the study website every week for 4 weeks to complete surveys and perform their assigned activity. After the 4-week intervention, participants completed 1-month and 3-month follow-up surveys.

Materials

Acts of kindness intervention. We instructed Givers to perform five acts of kindness in one day for recipients on a specific list (see Appendix for complete

instructions). We highlighted Givers' autonomy in choosing the specific kinds of activities they did, when they performed them, and whom they choose from their randomized lists of recipients. To help Givers select acts of kindness, 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 both large and small sacrifices of time, resources, and money. Although the specific acts of kindness that Givers performed could be public, 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 outset of the study, we created a customized, randomized Receiver list for each Giver. Each week's list had 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 sent these lists to Givers via email with instructions to refer to it for their assigned activity while keeping it confidential.

Neither Receivers nor Controls performed any other activity assignments throughout the study.

Behavioral self-reports. Participants were asked to recall specific instances of positive and negative workplace behaviors performed by others and themselves. Positive behaviors included "expressing sincere gratitude for a coworker" and "performing an unexpected act of kindness." Negative behaviors, which we included only as filler items (as our hypotheses concern only positive workplace behaviors), contained items like "repeating gossip or rumors about a coworker" and "insulting a coworker." Also, we

excluded four participants from analyses of behavioral self-reports, because they provided answers that were outliers both when compared to others and when compared to their own reports from week to week. In Tables 1 and 3, we indicate where the use of the untrimmed sample substantially changes estimates. Unless noted, all other estimates between the two samples were not substantially different.

Social network. 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 nominated one another) to analyze the spread of both well-being and behavioral outcomes.

Well-being outcomes.

Happiness, life satisfaction, positive affect, and negative affect. The Subjective Happiness Scale (Lyubomirsky & Lepper, 1999) is a 4-item measure that asks respondents first to rate on 7-point Likert-type scales how generally happy they are (1 = *not a very happy person*, 7 = *a very happy person*) and how happy they are relative to their peers (1 = *less happy*, 7 = *more happy*). Across all time points, αs 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, αs ranged from .78 to .90.

The Weekly Affect and Satisfaction Measure (Jacobs Bao, 2012) is a brief 2-item measure—designed for repeated measures over short time periods—that asks, "How have you felt this week?" (-10 = *extremely negative*, 10 = *extremely positive*) and "How satisfied are you with your life this week?" (-10 = *extremely satisfied*, 10 = *extremely dissatisfied*).

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

Elevation. Participants reported their feelings of elevation on a 7-item questionnaire (Algoe & Haidt, 2009). Examples items include "I felt 'lifted up' or 'nobler' myself" and "I felt more open and loving toward people in general." Participants rated their level of agreement with each item on 7-point Likert-type scales (1= *strongly disagree*, 7 = *strongly agree*). Across all time points, αs ranged from .83 to .88.

Need satisfaction, intrinsic motivation, and flow. Participants reported their intrinsic motivation, 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), and experience of flow (Csikszentmihalyi, 1990). The questionnaire contains five sets of 3-item measures. Example items include "I felt a strong sense of enjoyment" (intrinsic motivation), "I felt a sense of contact with people who care for me, and whom I care for" (relatedness), "I was free to do things my own way" (autonomy), "I was successfully completing difficult tasks and projects"

(competence), and "I was only aware of the task-at-hand" (flow). 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, αs ranged from .74 to .91 for autonomy, .76 to .83 for competence, .57 to .77 for flow, .78 to .91 for intrinsic motivation, and .71 to .88 for relatedness. As our measure of flow showed relatively poor reliability on some occasions, findings related to flow should be interpreted cautiously.

Occupational measures. Job performance was measured with the Job Performance Scale (M. A. Griffin, Neal, & Parker, 2007)—an 18-item measure requiring participants to assess their own individual and team proficiency, adaptability, and initiative (1 = *very little*, 5 = *a great deal*). Across all time points, αs ranged from .84 to .92.

The Organizational Citizenship Behavior Scale (Lee & Allen, 2002) is a 4-item measure that indicates the degree to which individuals exhibit commitment and dedication to their work duties, coworkers, and employer over and above what is strictly required by their job. The frequency of performing such behaviors is rated on 7-point scales (1 = *never*, 7 = *always*). Across all time points, αs ranged from .80 to .93.

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, αs ranged from .79 to .83.

Timeline. Participants completed all measures at every major time point (i.e., baseline [0 weeks], immediate post-intervention [4 weeks], and both follow-ups [8 weeks and 16 weeks]). During the middle of the intervention period (i.e., Weeks 1-3), participants reported only their weekly affect and satisfaction, need satisfaction, intrinsic motivation, flow, and workplace behaviors.

Analytic Approach

Behavioral outcomes. Because discrete data violates assumptions inherent in OLS regression, for all behavioral outcomes, we defined mixed-effects models with the negative binomial family using a log link (glmmADMB package in R). A negative binomial distribution allows the mean and variance of discrete data to differ (i.e., under- and over-dispersion), and thus carries fewer assumptions than a Poisson distribution, which assumes that means and variances are equal. Coefficient estimates using a log link indicate that every 1-unit increase in the predictor results in a $\log^{coefficient}$ multiplicative change in the dependent variable.

In analyses of group effects, group was always dummy-coded with Controls serving as the reference group. In all mixed-models, we coded time so that the end point of each time span was 0 in order to make our y-intercepts interpretable as differences between groups at the end of the specific time span being analyzed.

We determined that only letting intercepts vary provided the best fitting model, and fit was not significantly improved by adding other random effects (e.g., letting time vary). The composite equation for group effects (excluding the Givers group) is the following:

$$\log(Y_{ij}) = \gamma_{00} + \gamma_{01}RECEIVER + \gamma_{10}TIME + \gamma_{11}RECEIVER \times TIME + (\varepsilon_{ij} + \zeta_{0i})$$

When examining proximity effects (i.e., being influenced by a Giver or Receiver), we only analyzed the Control group. The composite equation for proximity effects is the following:

$$\log(Y_{ij}) = \gamma_{00} + \gamma_{01}PROXIMITY_{GIVER} + \gamma_{02}PROXIMITY_{RECEIVER} + \gamma_{10}TIME + \gamma_{11}PROXIMITY_{GIVER} \times TIME + \gamma_{12}PROXIMITY_{RECEIVER} \times TIME + (\varepsilon_{ij} + \zeta_{0i})$$

Well-being outcomes. We used mixed-effects modeling with a normal distribution (using the lme4 library in R) for all well-being-relevant outcome variables. To estimate *p*-values for each coefficient in the model (which are not provided in the lme4 library), we performed a likelihood ratio test between the full model and a reduced model excluding the term (Fitzmaurice, Laird, & Ware, 2011). We coded group and time the same as for behavioral outcomes (i.e., Control group as the comparison group, all end points as 0).

We determined that letting only intercepts vary provided the best fitting and most parsimonious model, compared to also letting time vary. We also tested non-linear models (e.g., adding a quadratic term for time), but the term did not consistently improve the fit of the model. The composite equation of the model used for group effects is the following:

$$Y_{ij} = \gamma_{00} + \gamma_{01}GIVER + \gamma_{02}RECEIVER + \gamma_{10}TIME + \gamma_{11}GIVER \times TIME + \gamma_{12}RECEIVER \times TIME + (\varepsilon_{ij} + \zeta_{0i})$$

The composite equation for all proximity analyses (using only the Control group) was the following:

$$Y_{ij} = \gamma_{00} + \gamma_{01} PROXIMITY_{GIVER} + \gamma_{02} PROXIMITY_{RECEIVER} + \gamma_{10} TIME + \gamma_{11} PROXIMITY_{GIVER} \times TIME + \gamma_{12} PROXIMITY_{RECEIVER} \times TIME + (\varepsilon_{ij} + \zeta_{0i})$$

Results and Discussion

Random Assignment

No significant differences for any of the well-being outcome variables (all *p*s > .27) or behavioral measures (all *p*s > .13) emerged among our three groups at baseline. However, several differences were observed at baseline for those with greater proximity to Givers and Receivers,[2] which we are mindful of when interpreting proximity findings.

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%. We found no differences in baseline levels of any outcome measure (except for flow and competence; see below) for those who did not complete later time points in the study (all *p*s > .18). Participants who completed the final time point were lower in flow at baseline than those who did not, $t(86) = 2.815$, $p = 0.006$, and participants who completed the final 2 time

[2]At baseline, each additional connection to a Giver was associated with 64% more observations of others' positive behaviors, $\gamma = 0.497$, $SE = 0.140$, $z = 3.54$, $p = .0004$, and each additional connection to a Receiver was associated with 18% more observations of others' positive behaviors, $\gamma = 0.168$, $SE = 0.0751$, $z = 2.23$, $p = .026$. Also, at baseline, each additional connection to a Giver meant reporting 75% more of one's own positive behaviors relative to Controls, $\gamma = 0.559$, $SE = 0.147$, $z = 3.80$, $p < .001$. Baseline differences in proximity analyses are not surprising, considering that although the Givers, Receivers, and Controls themselves were randomly assigned to their group, we could not randomly assign those who socially interact with them in the office. A participant who is relatively social, for example, is more likely to interact with Givers and Receivers. Accordingly, these baseline differences may be interpreted as pre-existing personality differences.

points were marginally lower in feelings of competence than those who did not (t[86] = 1.73, p = 0.087; t[86] = 1.69, p = 0.095).

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 (δ = .002 out of 1.000). Figure 1 displays all participants and their nominations to each other in a social network map using a Fruchterman-Reingold (1991) layout.

Givers and Receivers

Behavioral outcomes.

We asked Givers to list the acts of kindness they performed for Receivers. Altogether, they reported 98 specific acts of kindness[3], including such behaviors as "I brought him a coffee", "I gave her a sweatshirt," and "I simply called him over, gave him a [friendly] kiss, and encouraged him."

Even though they were unaware of being assigned to be recipients of generosity, Receivers noticed the relative increase[4] in positive behaviors in the office (see Positive Behaviors [Other], Mid-Intervention and Immediate Post-Intervention rows in Table 1, as well as left panel of Figure 2). Compared to Controls, they observed an increase per week

[3]This total implies that we did not capture all the acts of kindness that our participants performed. Givers may have chosen not to report their acts because they had already forgotten what they had done, were too modest to list their acts, or did not consistently complete the activity as assigned.
[4]See Limitations for a discussion of the interpretation of relative and absolute increases in our behavioral self-reports.

of 43.9% more of others' positive behaviors in the first 2 weeks. Over the entire intervention, Receivers observed an increase of 19.7% more positive behaviors. Receivers' observations of positive behaviors provide further validation that they noticed that Givers were practicing their acts of kindness as instructed.

Notably, according to Receivers' observations, the positive behaviors in the workplace persisted long after our study had ended (see Follow-Up [16 Weeks] row in Table 1). Three months after the intervention, Receivers reported 296.3% more positive behaviors in the office relative to controls and a weekly increase of 9.4% more positive behaviors each week of the study. Thus, relative to Controls, Receivers noticed more positive behaviors in the workplace even after Givers had finished practicing their assigned acts of kindness.

Were Receivers inspired to practice generosity themselves? Supporting our pay-it-forward hypothesis, Receivers paid their generosity forward (to others) or backward (to Givers) by performing more of their own acts of kindness, even though they had not been specifically asked to do so (see Positive Behaviors [self], Follow-Up [16 Weeks] row in Table 1 and right panel of Figure 2). Three months after the intervention, relative to Controls, Receivers reported performing 325.9% more positive behaviors at an increase of 9.1% each week. In sum, Receivers spontaneously performed their own acts of kindness for others as a direct consequence of Givers acting generously toward them, though this presumed pay-it-forward effect took a number of weeks to manifest itself.

Well-being outcomes.

Mid-intervention (2 weeks). Was Givers' generosity mutually beneficial in well-being for both Givers and Receivers? Results over time from the mixed-effects models are reported in Table 2. Examining only what happened during the first 2 weeks (see Mid-Intervention rows for each outcome), one could conclude that "'Tis better to receive than it is to give," as we found that Receivers (but not Givers) showed multiple benefits to their well-being and need satisfaction (see Receiver and Receiver × Time columns in Table 2 and Figure 3). Relative to Controls, Receivers showed significant increases in autonomy, competence, intrinsic motivation, relatedness, and weekly affect. Two weeks into the intervention, Receivers also ended up with higher relatedness, weekly affect, and weekly satisfaction, as well as marginally more elevation, autonomy, and intrinsic motivation, than Controls. Although prior research suggests that receiving specific kinds of kindness from others may produce mixed results, our initial findings suggest that being a Receiver of unexpected generosity in the workplace tends to be an unambiguously positive experience.

Likewise, although previous studies have shown that practicing kindness boosts happiness, Givers in the present study did not show significant benefits right away (even though their labor clearly "paid off" for recipients). Givers may have not yet have profited because their kindness assignments were new, challenging, and perhaps even burdensome or awkward at times (given their "Secret Santa" nature). Alternatively, the rewards of practicing kindness may take time to kick in.

Immediate post-intervention (4 weeks). Only at the end of the intervention period did evidence of improved well-being for both Givers and Receivers emerge, supporting our mutual-benefit hypothesis. Although they had not demonstrated any significant well-being benefits earlier, 4 weeks after they began practicing generosity, Givers reported significantly fewer depressive symptoms relative to Controls (see Depressive Symptoms, Immediate Post-Intervention row in Table 2, as well as Figure 4). Depressive symptoms include sleep problems, appetite/weight issues, restlessness, and irritability, all of which may indicate the presence of clinical depression when extreme or frequent. At baseline, Givers' depressive symptoms were moderate to low (M = 0.38 on a scale from 0 to 3, SD = 0.09) and not significantly different from Receivers or Controls (M = 0.34, SD = 0.09); yet 4 weeks later, Givers' depressive symptoms had dropped even further (M = 0.21, SD = 0.03), while those of Receivers and Controls increased slightly (M = 0.38, SD = 0.10). Indeed, Givers moved from endorsing around 5 or 6 symptoms to endorsing 1 or 2 symptoms 1 month after the intervention (as this downward trajectory continued over time). Thus, although Givers' depressive symptoms were not severe, relative to Controls, the practice of generosity led to even fewer of these troubling and problematic feelings and behaviors.

Givers also became more positively engaged in their work as indicated by significant increases in flow (see Flow, Immediate Post-Intervention row in Table 2), a finding that might seem counter-intuitive, given that Givers had more "work" to do because of their acts of kindness assignment. However, a key aspect of the experience of flow is that individuals forget themselves as they become drawn into challenging yet

engrossing tasks. Excessive self-focus may impede this process, or alternatively, less self-focus may facilitate it. Because Givers' assignments helped them focus on others and feel better (by reducing bothersome depressive symptoms), they may have more easily lost themselves in engrossing tasks at work.

Interestingly, 4 weeks after being the recipients of kind acts, Receivers maintained most of the benefits evident at mid-intervention. At the end of the intervention period, Receivers continued to report more autonomy, competence, and intrinsic motivation. Like Givers, they also reported more experiences of flow. However, their previous boosts in relatedness, weekly affect, and weekly satisfaction appeared to wear off, reverting them closer to their baseline levels.

1 month and 3 months follow-up. Interestingly, a full month after Givers finished practicing acts of generosity, they overtook Receivers in reaping benefits from their activity (see Follow-Up rows in Table 2). Givers maintained their significant losses in depressive symptoms—even 3 months after the kindness intervention had stopped. They also ended the 1-month follow-up with marginally more flow and intrinsic motivation, and marginally increased in flow, competence, happiness, and life satisfaction.

Although Receivers clearly benefited more quickly from the intervention (just 2 weeks after Givers began being especially kind to them), by the 1-month follow-up, they had already returned to baseline. However, Receivers did exhibit a few long-term benefits. Three months after the study ended, Receivers reported significant increases in flow. Receivers also showed marginal increases in OCB and self-reported job performance. Receivers' increases in flow, OCB, and job performance might suggest that over time

they channeled their appreciation of others' generosity into their work: feeling more positively engaged, performing their work responsibilities better, and going above and beyond their duties.

Giver and Receiver Proximity

Behavioral outcomes. Directly testing our inspire-others hypothesis, were coworkers (i.e., Controls) who socially interacted with Givers and Receivers prompted to perform more positive behaviors? Indeed, Controls with social proximity to both Givers and Receivers reported performing more of their own positive behaviors at different times during the intervention period (see Positive Behaviors [Self] rows of Table 3 and bottom panel of Figure 5). Notably, we found a significant positive trajectory (i.e., Receiver Proximity × Time) immediately after the intervention for those who socially interacted with Receivers and a significant positive trajectory (i.e., Giver Proximity × Time) 3 months after the intervention for those who socially interacted with Givers. However, most of the significant differences in intercepts we observed for those in proximity to Givers and Receivers (i.e., Giver Proximity and Receiver Proximity) are likely to reflect pre-existing differences in groups. In sum, our inspire-others hypothesis was supported with respect to both Givers and Receivers, but at different time points in the study. While Receivers' influence on their social network emerged earlier, Givers' influence appeared later and was more pronounced.

We also examined whether Controls who socially interacted with Givers and Receivers noticed any increased generosity in the office. The results showed that those in proximity to Givers and Receivers noted increases in positive behaviors 3 months after

the study, as reflected in differences in intercepts (see Positive Behaviors [Others] rows and Giver Proximity and Receiver Proximity columns of Table 3). However, only those in proximity to Givers demonstrated changes in trajectory (i.e., Giver Proximity × Time, see top panel of Figure 5). Taken together, these estimates suggest that those who interacted with Givers observed more positive behaviors long after the intervention period was over (i.e., 3 months later).

Well-being outcomes.

Did colleagues who interacted with Givers and Receivers demonstrate spill-over benefits for their well-being as well as for their behavior? Results from the mixed-effects models are reported in Table 4. Supporting our spill-over hypothesis, during the first 2 weeks of the intervention, Givers' acts of kindness produced well-being benefits for others in their social circle (see Figure 6). Controls with reciprocal (mutual) ties to Givers reported significant increases in relatedness, weekly affect, and weekly satisfaction even though they themselves were not recipients of Givers' generosity (see Mid-Intervention rows, Giver Proximity × Time column in Table 4). Although Figure 6 suggests that Givers were different at baseline and endpoint, the intercepts for those with reciprocal ties to Givers were not significantly different from other Controls at the endpoint (i.e., see Giver Proximity column in Table 4). Furthermore, we interpret baseline differences for those with ties to Givers or Receivers as representing pre-existing differences in personality. Thus, we believe that findings of significant changes in trajectory for those with reciprocal ties to Givers suggest that benefits from Givers' practice of generosity indeed spill-over to those in their social circles.

Furthermore, Givers continued to benefit those in their social networks over the entire course of the 4-week intervention. Controls with mutual ties to Givers maintained their increases in relatedness, positive affect, and satisfaction relative to Controls with no ties to Givers (see Immediate Post-Intervention rows).

However, contrary to our hypothesis, Receivers exerted a mildly negative influence on their social circles. Controls with reciprocal ties to Receivers (compared to Controls with none) showed significant decreases in elevation and relatedness (see Receiver Proximity × Time column). This surprising 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' generosity, 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. It is worth noting, however, that most of the negative effects of social proximity to Receivers appeared to wear off after 4 weeks.

In sum, our results suggest that being around a generous person is a positive experience, even when one is not the recipient. Givers' acts of kindness rendered those around them happier, more satisfied with life, more satisfied with their jobs, and feeling more connected with others. However, the initial impact of those who were recipients of generosity on their social networks was negative, although these detriments dissipated over time. Thus, supporting our greater-Giver-influence hypothesis, the well-being benefits from Givers' practice of generosity spilled over for those in proximity to Givers, but not Receivers.

General Discussion

Our 4-week experimental intervention involved assigning Givers to perform acts of kindness for randomly selected Receivers at their workplace, who were unaware that they had been chosen as targets. Our hypotheses were generally well-supported, with some interesting twists. Receivers experienced well-being benefits after a mere 2 weeks, including increased feelings of need satisfaction (i.e., autonomy, competence, and relatedness), intrinsic motivation, and weekly affect. Over time, however, these benefits wore off, as Receivers appeared to adapt to being recipients of kindness. However, 3 months after our intervention, Receivers reported additional benefits, exhibiting significant increases in flow, and marginal increases in organizational citizenship behaviors and self-reported job performance. Compared to Controls, Receivers also became inspired to perform their own acts of kindness, paying back or forward the generosity that had been shown to them 3 months earlier.

Givers did not benefit from their assignments right away. After 1 month of trying to be more generous to coworkers, however, they reported fewer depressive symptoms, more flow, more happiness, and higher life satisfaction. Notably, Givers' drop in depressive symptoms lasted as long as 3 months after we stopped instructing them to do acts of kindness.

Supporting our spill-over and greater-Giver-influence hypotheses, Givers also benefited other coworkers who happened to be in their social networks, even though these individuals were not the explicit targets of Givers' generosity: Those with mutual social ties to Givers increased in feelings of relatedness, weekly affect, and weekly

satisfaction. Thus, Givers' generosity benefitted themselves, their targets, and others in the office with whom they interacted. However, contrary to our expectations, Receivers exerted a mildly negative influence on their social circles, although this effect dissipated over time.

Finally, supporting our inspire-others hypothesis, both Givers and Receivers moved others in their social network to practice generosity. Receivers inspired others to act generously immediately after the intervention stopped. In contrast, Givers' impact on those in their social circle emerged 3 months after the intervention. Furthermore, Givers' influence was relatively more pronounced, consistent with our greater-Giver-influence hypothesis.

Receiving Generosity Feels Good

In contrast to prior research on receiving social support, we found no negative impact of receiving interpersonal acts of kindness in the workplace. The first 2 weeks of our intervention were most powerful for Receivers, probably because the benefits of generosity were most unexpected and meaningful during that time. Interestingly, although the benefits of receiving kindness were almost immediate, they also evaporated relatively quickly. However, a few occupational benefits of receiving kindness emerged only after time: Three months after the intervention, Receivers seemed to channel their appreciation of generosity into their work.

Practicing Generosity Takes Time to Pay Off.

The benefits of generosity for Givers took time to emerge but persisted until at least 3 months after our study ended. Givers became happier, more positively engaged,

felt more competent, more satisfied with their lives, and reported fewer depressive symptoms. Although they labored for the benefit of others, Givers earned well-being rewards for themselves, with their efforts to be kinder possibly lowering their likelihood of future depression.

Depression can negatively affect work performance and productivity, costing companies a great deal of money. For example, according to the Milken Institute (2007), mental health conditions (mostly depression and anxiety) have been found to account for one-third of sick days (1.3 billion days total) and are projected to cost the US $116 billion by 2023. Our results suggest the possibility that simply practicing kindness might protect against depression, while (mostly) elevating the entire office environment.

"Assigned" Generosity Works

Practicing generosity did not appear to require purely altruistic motivation. Participants likely enrolled in our study for a combination of self-focused or altruistic reasons—for example, to boost their own happiness, please the experimenter, obtain a prize for themselves, or earn money for charity. Although those whom we randomly assigned to be Givers did not literally "choose" to do acts of kindness, they benefited from doing them nonetheless. Individuals can thus reap rewards from being generous regardless of their initial motivation to do so.

Generosity's Rewards Spill Over

As they practiced generosity, Givers made their colleagues feel more connected to the office and more satisfied with their lives. That Givers benefited their social networks more than did Receivers (whose social network influences at times were negative)

suggests that watching someone *be* kind produces more of a positive impact than watching someone *receive* kindness.

The negative impact of interacting with recipients of generosity that we observed (i.e., short-term decreases in elevation and relatedness) could be an unanticipated artifact of our research design—namely, that the Receivers in our study benefited from the generosity of different Givers' consistently over 4 weeks. In the real world, however, generous people are kind to a variety of others, not just pre-assigned recipients on a list. In future studies, we expect that varying the recipients of generosity will mitigate the negative social impacts observed in the present study.

Generosity is Contagious, Especially from Givers

Both Receivers and observers of workplace generosity spontaneously performed more of their own acts of kindness. Givers engendered generosity in those they helped (i.e., pay-it-forward effect) and from those in their social networks (i.e., inspire-others effect). Although Receivers also inspirited generosity in others who socially interacted with them, Givers' generosity was more contagious. Givers may be particularly influential because watching another act kindly can be inspirational, yet not in a comparative way that can make others feel "less than."

Generosity also "Works" at Work

The workplace is an often overlooked context in which to conduct psychological research, yet it is highly relevant to everyday life: On a typical day, employed adults spend over half of their waking hours at work (Bureau of Labor Statistics, 2011). Our

results show that workplace environments are also conducive to self-improvement, and that positive outcomes for companies and employees need not be in conflict.

In fact, the practice of generosity appears to produce tangible benefits for companies over and above the individual: Givers' practice of generosity boosted others' positive feelings, life satisfaction, and need satisfaction, as well as occupationally-relevant constructs such as job satisfaction, job performance, and positive engagement. Our results suggest that employees can "catch" the benefits of a generous spirit in the office from those with whom they work.

Limitations

Instead of having our Control group perform a neutral activity, we used a no-treatment approach. Neutral activity controls ensure that differences that arise from performing an activity are not due to placebo effects. However, this criticism of our research design is only valid for hypotheses related to Givers; all hypotheses that concern Receivers, Controls, and social proximity to Givers and Receivers cannot be subject to placebo or demand effects, because none of these participants practiced an assigned activity. Furthermore, demonstrating that Givers would benefit from performing acts of kindness was our least risky hypothesis (given that the benefits of practicing generosity have been documented in other studies; see Layous & Lyubomirsky, in press, for a review).

Although we intentionally assigned a higher proportion of participants to be Receivers and Controls, the Givers' smaller numbers may have meant we lacked the statistical power to detect positive changes. In fact, many coefficients that we estimated

for Givers were positive but not statistically significant. Thus, we may have underestimated the degree to which Givers actually benefitted from generosity. Given the small number of Givers in our study, however, the effects of their generosity on the workplace are especially noteworthy, as well as their ability to influence their social networks more than that of Receivers (a relative advantage that we may have also underestimated).

Our measures of positive workplace behaviors were unusual in that they involved participants counting observed and performed behaviors. As is evident in the time estimates in our mixed models (see Time columns of Tables 1 and 3), participants reported fewer and fewer workplace behaviors as the study progressed. Two potential criticisms of our behavioral self-reports are that 1) we are misinterpreting relative increases (relative to Controls) as absolute increases and that 2) our measure may not be capturing actual behavioral changes, but merely participants' recollection or awareness of behavior. We believe that we observed an overall decrease in behavioral reports over time because of survey fatigue. These behavioral reports were more cognitively taxing than our other measures, because they involved the recollection of past events and adding up of discrete episodes; therefore, they were particularly attractive to skip or report a zero value. To the extent that the overall downward trend is an artifact of our measure, baseline-adjusted changes in behaviors could represent actual changes. Furthermore, our acts of kindness intervention produced results over 4 weeks that exactly matched our predictions: Givers reported performing more acts of kindness and Receivers noticed

them. This manipulation check lends credibility to our behavioral measures' capturing what we intended—that is, changes in actual performance of generous behavior.

Because major attrition in our study began at 4 weeks, and escalated at the 1-month and 3-month follow-ups, the participants who elected to continue the intervention could have substantively differed from those who dropped out. To be sure, we noted baseline differences in flow and competence for those who completed the final time points. Thus, our findings regarding Givers' competence and Receivers' flow at follow-up may be exaggerated by the attrition of those who began high, but may have remained the same or decreased. Although those who continued the study did not differ in their baseline levels of other well-being measures from those who left, we cannot fully eliminate the possibility that our participants experienced different reactions to their assigned activity that affected their participation in the study.

Were Givers successful in keeping their activity assignment secret? We instructed them to do so, and to the best of our knowledge and results, they followed our instructions. However, we would also argue that our findings are important even if "contamination" had occurred. In fact, contamination (i.e., social propagation) is a key component of our hypotheses. Furthermore, if "assigned" generosity—let alone publicly assigned generosity—produces these benefits, the impact of autonomously practiced generosity in the real world should be even more impactful.

Concluding Words and Future Directions

Although our Spanish sample is more diverse in background and age than many published psychological studies (which primarily rely on U.S. undergraduates; Jones,

2010), cultural psychologists may feel disappointed that our study's single-nation sample makes it impossible to uncover any cross-cultural differences. For now, our findings do suggest broadly that positive activities such as practicing kindness can be effective in cultures other than the U.S., although they may need to be applied to specific environments (as we tailored our acts of kindness intervention to a Spanish workplace). Future research should examine the degree to which deliberate acts of generosity produce the same results in different cultural contexts.

As we have already mentioned, because we could not randomly assign social relationships, our proximity results are less suggestive of causality than group results. Because of this limitation, we focused on how Givers and Receivers changed those in their social networks over time rather than how such participants were consistently different from others. To provide even stronger causal evidence, researchers would need to experimentally manipulate both the practice of generosity and participants' social proximity to Givers and Receivers. Although, for ethical reasons, experimenters might not want to reduce naturally occurring social interaction (e.g., "Don't talk to John this week"), they could increase it (e.g., "Make sure you talk to Christina this week").

Finally, we believe our results have direct applications to both workplaces and clinical settings. In clinical settings, as the benefits of acting generously may take time to emerge, our results support the idea of "acting one's way into a feeling" more than "feeling one's way into acting." For clinicians who assign "homework" to clients on a weekly basis, practicing acts of kindness could be a valuable activity for boosting

happiness and potentially alleviating symptoms of mental health conditions associated with excessive self-focus (e.g., major depression and dysphoria).

In the workplace, we envision office-based programs that encourage generosity, but of course, are voluntary and free from stigma or coercion. Even though Givers did not choose their positive activity, all participants elected to cooperate in our research. Forcing or compelling employees to participate in a workplace program—however well-meaning the intention—is not only potentially unethical, but would likely backfire. Most important, our results suggest that CEOs and managers could best foster generosity in their workplaces through their own examples—by funding and modeling the kind of generosity that they aspire to cultivate in others.

In sum, our study suggests that although acts of kindness may be small, they are not insignificant. The benefits of generosity do multiply, blessing both the giver and receiver and rippling through one's interpersonal relationships. Moreover, our results show that interpersonal acts of generosity have an inspiring and transforming power—as today's getters become tomorrow's givers.

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Table 1
Behaviors by Receivers and Controls Over Time

		Fixed Effects						*Random Effects*	
DV	Time Period	Intercept γ_{00}	Time γ_{10}	Receiver γ_{01}	Receiver Rate Ratio $e^{\gamma_{01}}$	Receiver × Time γ_{11}	Receiver × Time Rate Ratio $e^{\gamma_{11}}$	Residual $\sigma^2\varepsilon$	Intercept Variance σ^2_0
Positive Behaviors (Other)	Mid-Intervention (2 Weeks)	0.451† (0.254)	-0.487*** (0.107)	0.484 (0.35)	162.3%	0.364* (0.147)	143.9%	0.612	1.627
	Immediate Post-Intervention (4 Weeks)	-0.418 (0.290)	-0.460*** (0.067)	0.603 (0.398)	182.8%	0.180† (0.092)	119.7%	0.657	1.988
	Follow-Up (8 Weeks)	-1.084** (0.337)	-0.283*** (0.039)	0.408 (0.465)	150.4%	0.044 (0.054)	104.5%	0.701	2.167
	Follow-Up (16 Weeks)	-2.174*** (0.406)	-0.201*** (0.024)	1.377** (0.523)	396.3%	0.090** (0.031)	109.4%	0.736	2.165
Positive Behaviors (Self)	Mid-Intervention (2 Weeks)	0.629* (0.256)	-0.281* (0.118)	0.271 (0.353)	131.1%	0.084 (0.163)	108.8%	0.570	1.516
	Immediate Post-Intervention (4 Weeks)	-0.243 (0.289)	-0.364*** (0.067)	0.442 (0.397)	155.6%	0.082 (0.092)	108.6%	0.649	1.992
	Follow-Up (8 Weeks)	-0.732* (0.328)	-0.215*** (0.037)	0.054 (0.459)	105.6%	-0.027 (0.053)	97.3%	0.695	2.174
	Follow-Up (16 Weeks)	-1.882*** (0.395)	-0.177*** (0.024)	1.449** (0.51)	425.9%	0.087** (0.030)	109.1%	0.733	2.082

Note. †$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$. Using the untrimmed sample, estimates of γ_{11} for Others' Positive Behaviors at 2 and 4 weeks were 0.304† (0.165) and 0.200* (0.097), respectively.

Table 2

Well-Being Outcomes by Group Over Time

		Fixed Effects						*Random Effects*	
		Intercept	Time	Giver	Receiver	Giver × Time	Receiver × Time	Residual	Intercept Variance
DV	Time Period	γ_{00}	γ_{10}	γ_{01}	γ_{02}	γ_{11}	γ_{12}	$\sigma^2\varepsilon$	σ^2_0
Elevation	Mid-Intervention (2 Weeks)	3.814*** (0.194)	-0.05 (0.076)	-0.099 (0.332)	0.455† (0.269)	-0.022 (0.14)	0.166 (0.106)	0.338	0.955
	Immediate Post-Intervention (4 Weeks)	3.817*** (0.197)	-0.023 (0.037)	-0.044 (0.336)	0.368 (0.272)	0.004 (0.067)	0.041 (0.050)	0.295	0.994
	Follow-up (8 Weeks)	3.756*** (0.223)	-0.019 (0.023)	0.122 (0.402)	0.400 (0.309)	0.027 (0.044)	0.022 (0.031)	0.306	1.001
	Follow-up (16 Weeks)	3.646*** (0.259)	-0.016 (0.014)	-0.116 (0.443)	0.502 (0.343)	-0.004 (0.025)	0.019 (0.018)	0.333	0.945
Flow	Mid-Intervention (2 Weeks)	2.535*** (0.129)	-0.031 (0.062)	0.229 (0.224)	0.027 (0.179)	0.150 (0.110)	0.046 (0.086)	0.224	0.341
	Immediate Post-Intervention (4 Weeks)	2.393*** (0.137)	-0.058† (0.032)	0.416† (0.236)	0.313† (0.187)	0.119* (0.057)	0.106* (0.043)	0.224	0.375
	Follow-up (8 Weeks)	2.355*** (0.162)	-0.029 (0.02)	0.554† (0.299)	0.220 (0.221)	0.069† (0.037)	0.029 (0.027)	0.228	0.366
	Follow-up (16 Weeks)	2.327*** (0.196)	-0.015 (0.012)	0.156 (0.337)	0.481† (0.255)	0.003 (0.021)	0.03* (0.015)	0.237	0.352
Autonomy	Mid-Intervention (2 Weeks)	2.512*** (0.132)	-0.177** (0.058)	0.244 (0.227)	0.335† (0.182)	0.066 (0.104)	0.184* (0.08)	0.196	0.397
	Immediate Post-Intervention (4 Weeks)	2.511*** (0.138)	-0.075* (0.031)	0.288 (0.239)	0.363† (0.189)	0.044 (0.055)	0.085* (0.042)	0.211	0.411
	Follow-up (8 Weeks)	2.608*** (0.164)	-0.016 (0.019)	0.277 (0.302)	0.305 (0.223)	0.016 (0.037)	0.026 (0.026)	0.221	0.412
	Follow-up (16 Weeks)	2.757*** (0.194)	0.003 (0.012)	0.057 (0.334)	0.287 (0.253)	-0.008 (0.02)	0.01 (0.015)	0.220	0.395
Competence	Mid-Intervention (2 Weeks)	2.703*** (0.13)	-0.118† (0.063)	0.026 (0.227)	0.169 (0.181)	0.046 (0.113)	0.175* (0.088)	0.238	0.341
	Immediate Post-Intervention (4 Weeks)	2.665*** (0.135)	-0.06† (0.033)	0.254 (0.234)	0.260 (0.184)	0.087 (0.058)	0.098* (0.044)	0.238	0.343
	Follow-up (8Weeks)	2.651*** (0.161)	-0.027 (0.02)	0.449 (0.299)	0.218 (0.219)	0.064† (0.038)	0.033 (0.027)	0.237	0.347
	Follow-up (16 Weeks)	2.758*** (0.193)	-0.004 (0.012)	0.277 (0.332)	0.285 (0.251)	0.017 (0.02)	0.019 (0.015)	0.232	0.337

Intrinsic Motivation	Mid-Intervention (2 Weeks)	2.462*** (0.146)	-0.178* (0.07)	0.24 (0.254)	0.354† (0.203)	0.102 (0.125)	0.216* (0.097)	0.290	0.44
	Immediate Post-Intervention (4 Weeks)	2.408*** (0.151)	-0.090** (0.034)	0.359 (0.261)	0.490* (0.207)	0.077 (0.061)	0.129** (0.047)	0.262	0.481
	Follow-up (8Weeks)	2.529*** (0.18)	-0.018 (0.022)	0.551† (0.334)	0.268 (0.245)	0.059 (0.042)	0.019 (0.029)	0.283	0.464
	Follow-up (16 Weeks)	2.658*** (0.214)	0.001 (0.013)	0.262 (0.368)	0.300 (0.279)	0.007 (0.022)	0.010 (0.017)	0.275	0.455
Relatedness	Mid-Intervention (2 Weeks)	2.663*** (0.124)	-0.193** (0.061)	0.244 (0.215)	0.36* (0.171)	0.113 (0.108)	0.187* (0.084)	0.218	0.303
	Immediate Post-Intervention (4 Weeks)	2.689*** (0.129)	-0.071* (0.031)	0.219 (0.224)	0.318† (0.177)	0.037 (0.055)	0.063 (0.042)	0.210	0.329
	Follow-up (8 Weeks)	2.661*** (0.155)	-0.033† (0.019)	0.313 (0.289)	0.296 (0.211)	0.029 (0.037)	0.023 (0.026)	0.221	0.321
	Follow-up (16 Weeks)	2.863*** (0.188)	-0.001 (0.012)	0.003 (0.324)	0.148 (0.245)	-0.009 (0.02)	0000 (0.015)	0.221	0.317
Depressive Symptoms	Immediate Post-Intervention (4 Weeks)	0.444*** (0.065)	0.023 (0.015)	-0.22* (0.114)	-0.075 (0.087)	-0.064* (0.027)	-0.016 (0.02)	0.035	0.054
	Follow-up (8 Weeks)	0.428*** (0.065)	0.009 (0.008)	-0.285* (0.126)	-0.108 (0.092)	-0.039* (0.016)	-0.013 (0.011)	0.032	0.051
	Follow-up (16 Weeks)	0.4*** (0.078)	0.002 (0.005)	-0.301* (0.136)	-0.148 (0.102)	-0.019* (0.009)	-0.009 (0.007)	0.034	0.045
Life Satisfaction	Immediate Post-Intervention (4 Weeks)	5.345*** (0.186)	-0.032 (0.037)	0.032 (0.323)	0.119 (0.249)	0.067 (0.068)	0.038 (0.048)	0.203	0.629
	Follow-up (8 Weeks)	5.245*** (0.184)	-0.031† (0.019)	0.303 (0.345)	0.164 (0.254)	0.070† (0.037)	0.026 (0.025)	0.167	0.687
	Follow-up 16 Weeks)	5.323*** (0.207)	-0.008 (0.011)	0.133 (0.358)	0.109 (0.274)	0.020 (0.019)	0.006 (0.014)	0.154	0.687
Happiness	Immediate Post-Intervention (4 Weeks)	5.372*** (0.186)	-0.008 (0.04)	-0.076 (0.331)	-0.089 (0.25)	0.063 (0.074)	0.012 (0.053)	0.254	0.567
	Follow-up (8 Weeks)	5.252*** (0.183)	-0.022 (0.02)	0.257 (0.356)	0.154 (0.255)	0.077† (0.04)	0.042 (0.027)	0.209	0.631
	Follow-up (16 Weeks)	5.419*** (0.219)	0.002 (0.013)	0.219 (0.387)	0.124 (0.29)	0.032 (0.023)	0.017 (0.017)	0.229	0.583

Weekly Affect	Mid-Intervention (2 Weeks)	2.903*** (0.608)	-0.664* (0.317)	1.045 (1.063)	2.04* (0.845)	0.368 (0.567)	1.107* (0.443)	6.114	6.631
	Immediate Post-Intervention (4 Weeks)	3.468*** (0.646)	-0.09 (0.175)	0.796 (1.134)	1.487† (0.879)	0.089 (0.311)	0.269 (0.236)	6.952	6.48
	Follow-up (8 Weeks)	3.568*** (0.804)	-0.024 (0.107)	1.061 (1.518)	0.879 (1.098)	0.077 (0.205)	0.018 (0.146)	7.043	6.426
	Follow-up (16 Weeks)	5.142*** (0.992)	0.100 (0.064)	-0.585 (1.718)	0.091 (1.286)	-0.081 (0.111)	-0.049 (0.083)	6.961	6.233
Weekly Satisfaction	Mid-Intervention (2 Weeks)	3.726*** (0.579)	-0.414 (0.286)	-0.045 (1.008)	1.725* (0.804)	-0.310 (0.513)	0.580 (0.399)	4.954	6.614
	Immediate Post-Intervention (4 Weeks)	4.347*** (0.609)	0.023 (0.154)	0.001 (1.064)	1.075 (0.832)	-0.103 (0.275)	0.028 (0.209)	5.377	6.656
	Follow-up (8 Weeks)	3.658*** (0.746)	-0.098 (0.096)	1.068 (1.397)	1.347 (1.019)	0.12 (0.183)	0.055 (0.13)	5.535	6.587
	Follow-up (16 Weeks)	4.513*** (0.91)	0.017 (0.057)	0.246 (1.573)	0.963 (1.184)	-0.005 (0.099)	-0.002 (0.074)	5.491	6.472
OCB	Follow-up (8 Weeks)	5.474*** (0.195)	-0.052* (0.025)	0.584 (0.377)	0.095 (0.275)	0.049 (0.048)	0.038 (0.035)	0.296	0.441
	Follow-up (16 Weeks)	5.501*** (0.236)	-0.021 (0.015)	0.343 (0.434)	0.361 (0.309)	0.005 (0.027)	0.034† (0.019)	0.294	0.465
Job Performance	Follow-up (8 Weeks)	5.716*** (0.181)	-0.006 (0.022)	-0.367 (0.348)	-0.052 (0.255)	-0.007 (0.043)	0.016 (0.031)	0.240	0.414
	Follow-up (16 Weeks)	5.591*** (0.216)	-0.011 (0.013)	0.013 (0.396)	0.325 (0.283)	0.022 (0.025)	0.031† (0.017)	0.238	0.415

Note. †$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$. OCB = Organizational Citizenship Behaviors.

Table 3

Behaviors by Proximity Over Time

		Fixed Effects										*Random Effects*	
DV	Time Period	Intercept γ_{00}	Time γ_{10}	Giver Proximity γ_{01}	Giver Proximity Rate Ratio $e^{\gamma_{01}}$	Receiver Proximity γ_{02}	Receiver Proximity Rate Ratio $e^{\gamma_{02}}$	Giver Proximity × Time γ_{11}	Giver Proximity × Time Rate Ratio $e^{\gamma_{11}}$	Receiver Proximity × Time γ_{12}	Receiver Proximity × Time Rate Ratio $e^{\gamma_{12}}$	Residual $\sigma^2\varepsilon$	Intercept Variance σ^2_0
Positive Behaviors (Other)	Mid-Intervention (2 Weeks)	0.306 (0.325)	-0.444*** (0.131)	-0.032 (0.485)	96.9%	0.239 (0.286)	127.0%	-0.090 (0.195)	91.4%	-0.023 (0.118)	97.7%	0.623	1.652
	Immediate Post-Intervention (4 Weeks)	-0.863* (0.383)	-0.529*** (0.087)	0.432 0.523)	154.0%	0.480 (0.322)	161.6%	0.117 (0.111)	112.4%	0.047 (0.072)	104.8%	0.679	2.005
	Follow-Up (8 Weeks)	-1.481*** (0.444)	-0.305*** (0.051)	0.871 (0.637)	238.9%	0.216 (0.369)	124.1%	0.114 (0.075)	112.1%	-0.023 (0.043)	97.7%	0.729	2.106
	Follow-Up (16 Weeks)	-3.561*** (0.612)	-0.281*** (0.038)	1.797* (0.739)	603.2%	0.814* (0.411)	225.7%	0.117** (0.045)	112.4%	0.032 (0.024)	103.3%	0.757	2.041
Positive Behaviors (Self)	Mid-Intervention (2 Weeks)	0.342 (0.321)	-0.328* (0.143)	-0.170 (0.477)	84.4%	0.497† (0.277)	164.4%	-0.196 (0.205)	82.2%	0.149 (0.125)	116.1%	0.606	1.455
	Immediate Post-Intervention (4 Weeks)	-0.852* (0.372)	-0.474*** (0.086)	0.109 (0.518)	111.5%	0.838** (0.311)	231.2%	0.021 (0.115)	102.1%	0.141* (0.070)	115.1%	0.614	1.855
	Follow-Up (8 Weeks)	-1.394** (0.429)	-0.270*** (0.05)	0.682 (0.618)	197.8%	0.681† (0.351)	197.6%	0.099 (0.073)	110.4%	0.028 (0.040)	102.8%	0.684	1.949
	Follow-Up (16 Weeks)	-3.504*** (0.596)	-0.266*** (0.037)	1.805* (0.725)	608.0%	1.019** (0.393)	277.0%	0.124** (0.044)	113.2%	0.036 (0.022)	103.7%	0.674	2.013

Note. †$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$. Using the untrimmed sample, only coefficients relevant to receiver proximity were substantially different. Specifically, γ_{12} for Positive Behaviors (Self) for 4 weeks was 0.147† (0.079). The remaining differences were in γ_{02}, which we interpret as pre-existing differences and are thus not relevant to our hypotheses of change over time.

Table 4

Well-Being Outcomes by Proximity Over Time

		Fixed Effects						*Random Effects*	
		Intercept	Time	Giver Proximity	Receiver Proximity	Giver Proximity × Time	Receiver Proximity × Time	Residual	Intercept Variance
DV	Time Period	γ_{00}	γ_{10}	γ_{01}	γ_{02}	γ_{11}	γ_{12}	$\sigma^2\varepsilon$	σ^2_0
Elevation	Mid-Intervention (2 Weeks)	3.949*** (0.289)	0.13 (0.108)	-0.014 (0.397)	-0.125 (0.238)	0.061 (0.147)	-0.241** (0.085)	0.360	1.256
	Immediate Post-Intervention (4 Weeks)	3.844*** (0.297)	0.017 (0.056)	-0.075 (0.395)	0.035 (0.237)	0.007 (0.069)	-0.046 (0.04)	0.319	1.263
Relatedness	Mid-Intervention (2 Weeks)	2.859*** (0.169)	-0.151† (0.089)	0.078 (0.231)	-0.263* (0.135)	0.307* (0.121)	-0.155* (0.07)	0.248	0.271
	Immediate Post-Intervention (4 Weeks)	2.68*** (0.182)	-0.118** (0.046)	0.221 (0.235)	-0.092 (0.138)	0.169** (0.057)	-0.007 (0.033)	0.221	0.312
Weekly Affect	Mid-Intervention (2 Weeks)	2.557** (0.922)	-1.29** (0.454)	0.707 (1.171)	0.064 (0.733)	1.442** (0.552)	0.212 (0.349)	6.439	9.280
	Immediate Post-Intervention (4 Weeks)	3.215*** (1.013)	-0.333 (0.278)	1.632 (1.261)	-0.399 (0.757)	0.859* (0.341)	-0.035 (0.202)	8.491	7.869
Weekly Satisfaction	Mid-Intervention (2 Weeks)	3.771*** (0.9)	-0.602 (0.411)	0.416 (1.156)	-0.279 (0.722)	1.24* (0.498)	-0.265 (0.315)	5.238	9.961
	Immediate Post-Intervention (4 Weeks)	4.017*** (0.935)	-0.175 (0.231)	1.461 (1.184)	-0.23 (0.716)	0.819** (0.282)	-0.083 (0.167)	5.751	8.783

Note. †$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

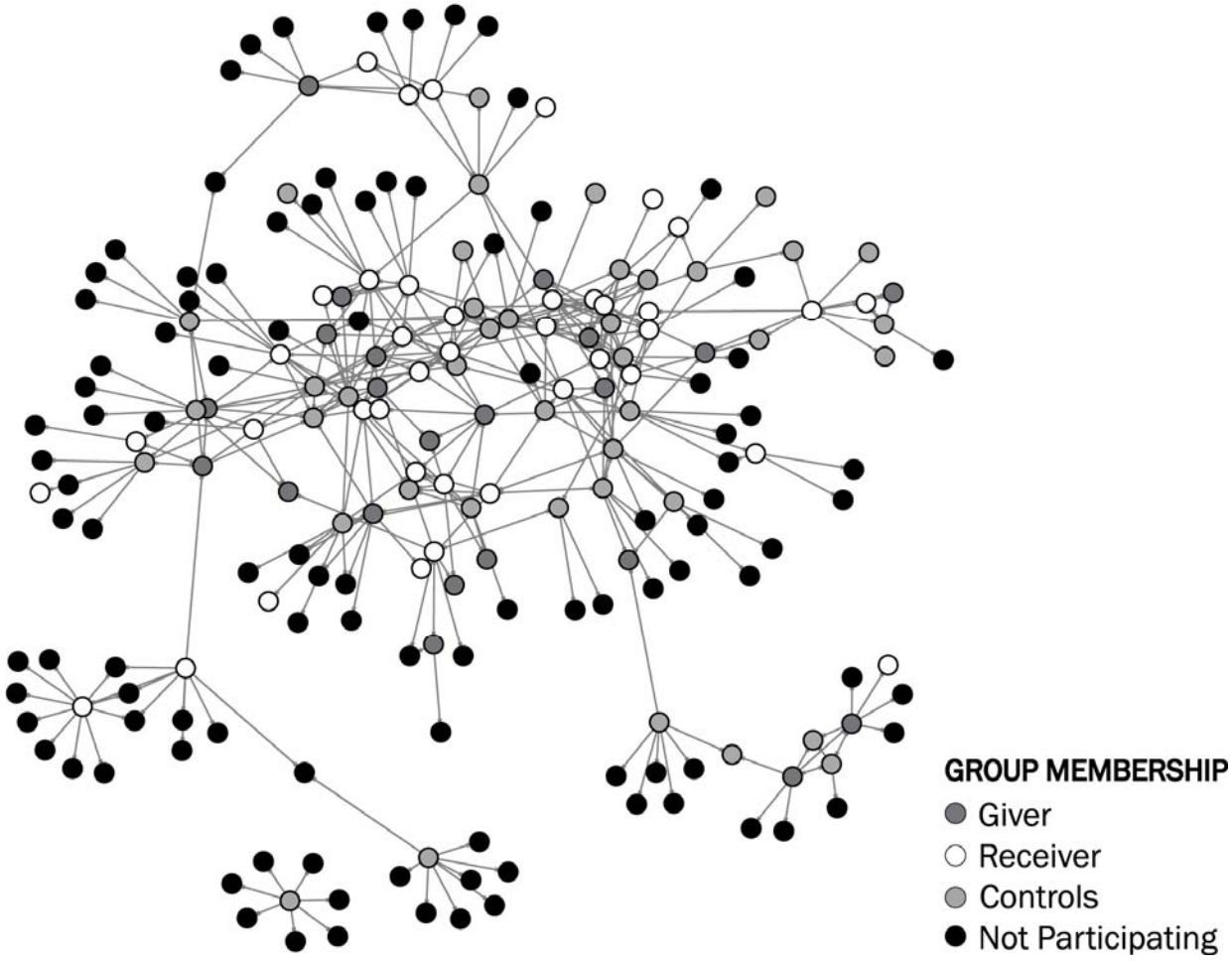

Figure 1. The workplace social network map.

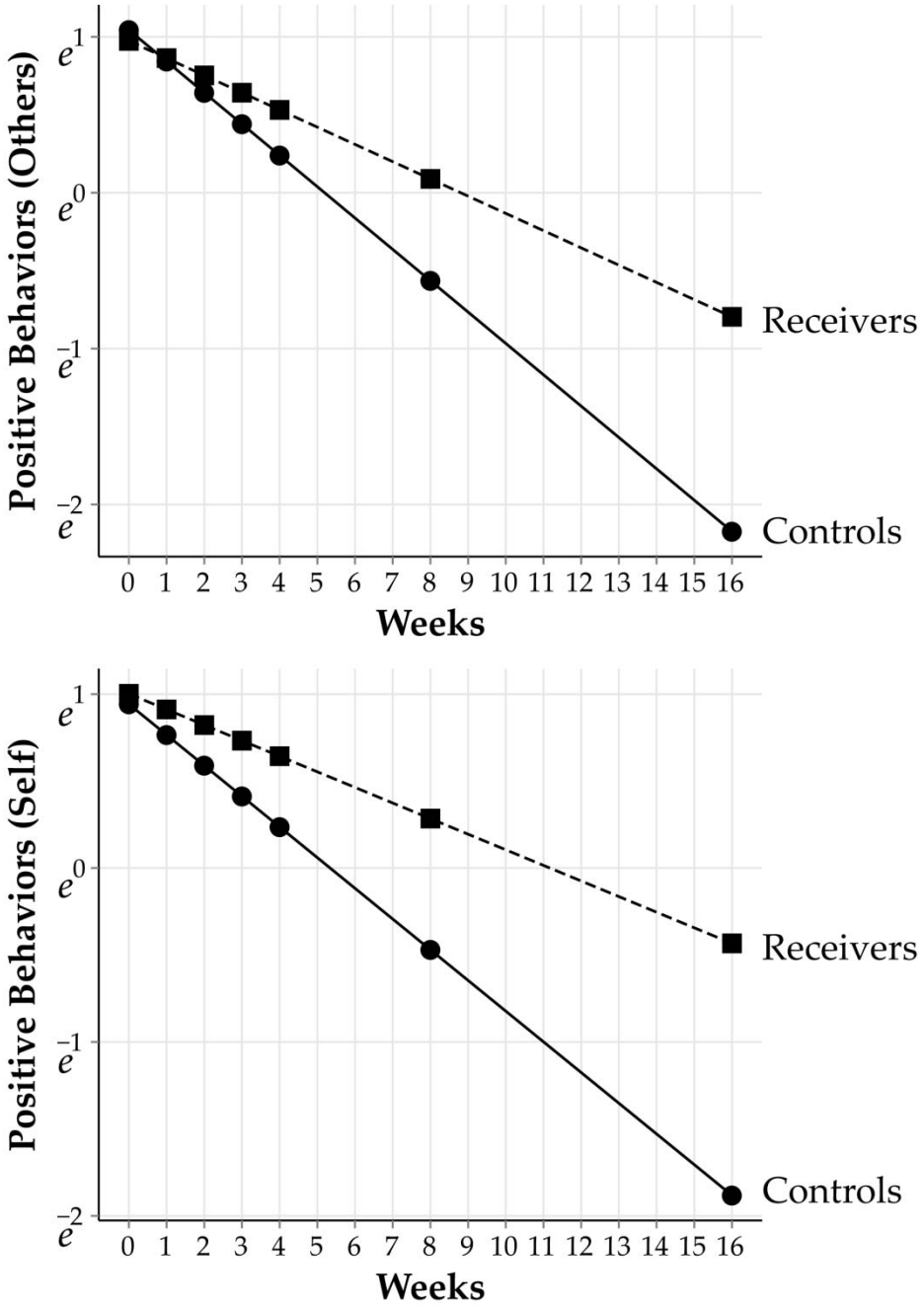

Figure 2. Observed (top panel) and performed (bottom panel) positive behaviors by Receivers and Controls.

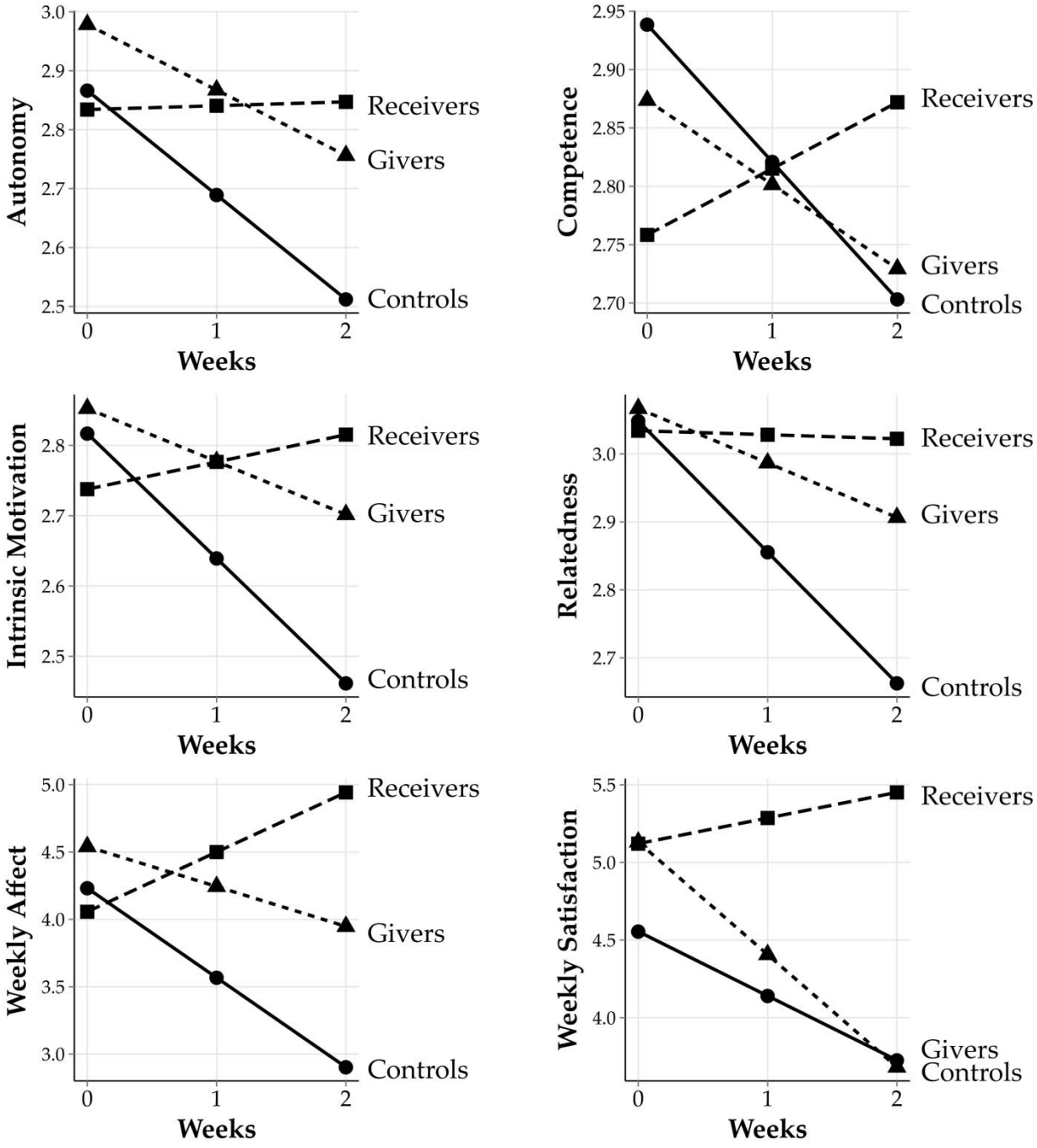

Figure 3. Autonomy, competence, intrinsic motivation, relatedness, weekly affect, and weekly satisfaction by group over 2 weeks.

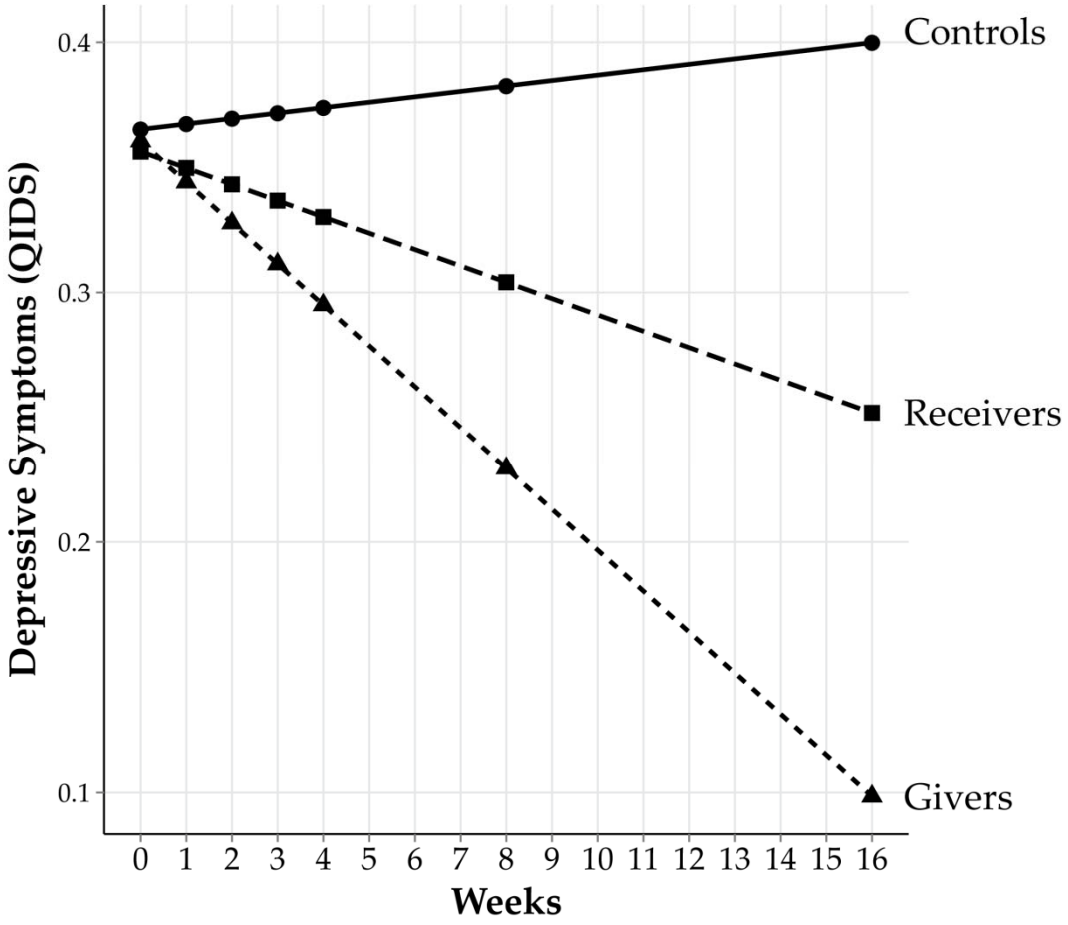

Figure 4. Depressive symptoms by group over 16 weeks.

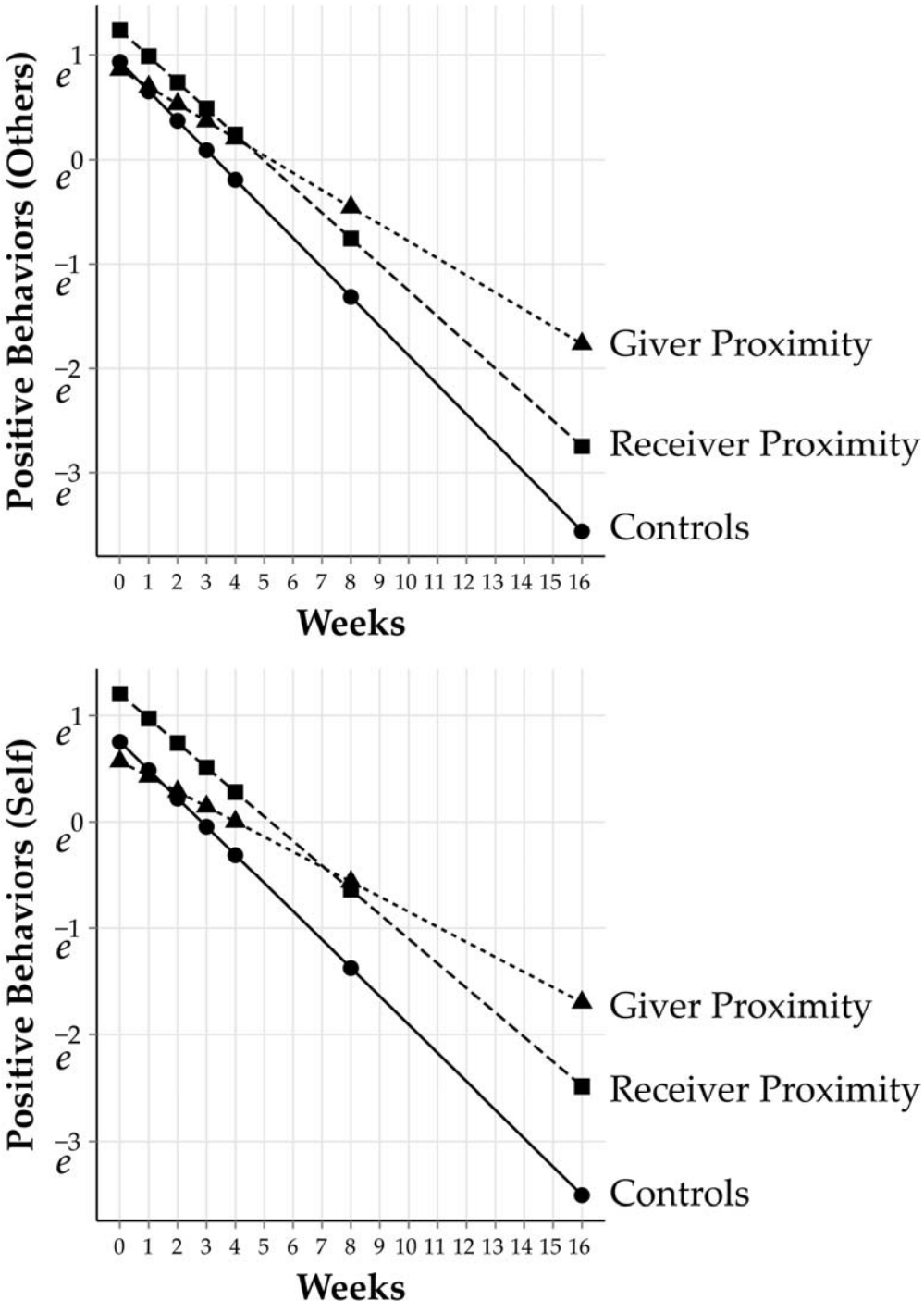

Figure 5. Observed (top panel) and performed (bottom panel) positive behaviors by proximity over 16 weeks.

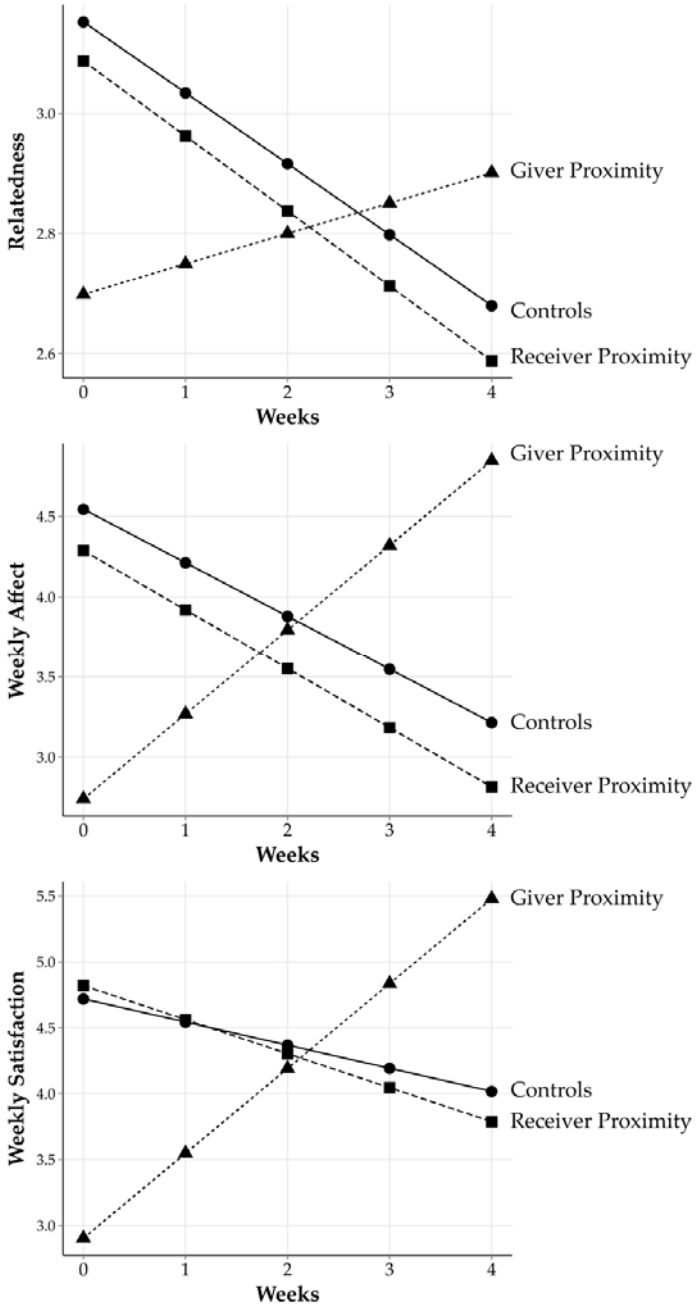

Figure 6. Relatedness, weekly affect, and weekly satisfaction by proximity across 4 weeks.

Appendix

Acts of Kindness

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 generosity. In short, act generously 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.