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Everyday Prosociality in the Workplace:

The Reinforcing Benefits of Giving, Getting, and Glimpsing

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Abstract

A functional analysis of prosociality considers how predispositions for prosocial behavior prompt, reinforce, and propagate kind behaviors in the real world. To examine the effects of practicing, receiving, and observing everyday prosociality—as well as the mechanisms underlying these effects—we randomly assigned employees in a Spanish corporate workplace (*N*=111) to be Givers, Receivers, and Controls. Givers practiced five acts of kindness for a personalized list of Receivers over 4 weeks. We found that Givers and Receivers mutually benefited in well-being in both the short-term (e.g., on weekly measures of competence and autonomy) and the long-term (e.g., Receivers became happier after 2 months, and Givers became less depressed and more satisfied with their lives and jobs). In addition, Givers' prosocial acts inspired others to act: Receivers paid their acts of kindness forward with 278% more prosocial behaviors than Controls. Our results reveal that practicing everyday prosociality is both emotionally reinforcing and contagious (inspiring kindness and generating hedonic rewards in others) and that receiving everyday prosociality is an unequivocally positive experience (which may further reinforce Givers' actions). Prosociality's benefits shed light on its surprising ubiquity in humanity compared with our closest evolutionary cousins.

Keywords: generosity, prosociality, well-being, pay-it-forward

Everyday Prosociality in the Workplace:

The Reinforcing Benefits of Giving, Getting, and Glimpsing

"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

The goal of promoting both well-being and prosocial behavior (i.e., doing kind acts for others) has important and unique implications for workplaces. Well-being predicts a number of positive occupationally relevant outcomes, such as persisting longer, performing better, working reliably (i.e., fewer absences), and earning higher supervisor evaluations (Boehm & Lyubomirsky, 2008; Fisher, 2010). Acting prosocially buffers against burnout and emotional exhaustion (Grant & Sonnentag, 2010), and by inducing a focus on others, promotes perspective-taking, empathy, and creativity (Grant & Berry, 2011). For example, sales teams awarded prosocial salary bonuses (i.e., bonuses granted to a salesperson on behalf of a sales teammate) are more productive than sales teams awarded personal salary bonuses (Anik, Aknin, Norton, Dunn, & Quoidbach, 2013). We conducted the present study in a work setting at a multi-national corporation to determine whether practicing prosocial behavior could spur beneficial work-related outcomes and increase well-being.

Consequences of Everyday Prosociality

Well-being. Correlational longitudinal studies can naturalistically examine prosocial behavior (e.g., charitable giving, volunteering, etc.) over sustained periods of time (Choi & Chou, 2010); their results suggest that practicing prosociality is associated with greater health and well-being (Office of Research and Policy Development, 2007), and its rewards can even extend to givers' families and communities (Morrow-Howell, Hong, & Tang 2009). However, without the benefit of random assignment, such studies may strongly implicate, but not fully

disentangle, prosociality's complex causes and consequences. Experimental studies in which participants are prompted to practice prosociality 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 (Dunn, Aknin, & Norton, 2008; Layous, Nelson, Oberle, Schonert-Reichl, & Lyubomirsky, 2012; Lyubomirsky, Sheldon, & Schkade, 2005; Nelson et al. 2014; Sheldon, Boehm, & Lyubomirsky, 2012; Study 2).

But beneficiaries' responses to prosocial overtures are also crucial, inextricably linked to helpers' responses, and a frequently overlooked part of the story: Receivers' reactions—whether positive, neutral, or negative—may reinforce or inhibit future prosociality. Prior research shows that receiving help can be a mixed experience because it can threaten self-efficacy, curtail autonomy, and cultivate indebtedness (Fisher, Nadler, & Whitcher-Alagna, 1982). However, we argue that mixed results are most likely when help implies *helplessness*—e.g., requesting, needing, coping, distressing, or struggling. As a contrast, everyday prosocial behavior—that is, small acts of kindness performed for partners, parents, friends, and coworkers—is both ubiquitous in normal life and far less likely to cause unintended side effects. Although a number of correlational and experimental studies have examined the consequences of prosociality for either the actor or the recipient, very few have captured how everyday prosocial acts affect both parties' well-being simultaneously and over extended periods of time.

Need Satisfaction. Self-determination theory (Deci & Ryan, 2000) provides insight into how prosocial behavior can meet individuals' core psychological needs—namely, connectedness (meaningful relationships), autonomy (sense of choice), and competence (self-efficacy). Engaging in various kinds of positive activities, such as expressing gratitude and practicing

optimism, has already been shown to satisfy two of the three basic needs—autonomy and connectedness (Boehm, Lyubomirsky, & Sheldon, 2011)—and doing kindness may have parallel effects.

Given that prosociality is usually performed for another person—and intended to benefit or make that person happier—it is not surprising that it would promote a greater sense of connectedness and closeness with others, perhaps the most crucial component of human flourishing (Berscheid, 2003). Competence could also be a key outcome because committing kind acts may lead people to feel more competent in their abilities to enact change and improve their relationships (Lyubomirsky, King, & Diener, 2005; Otake et al., 2006).

Although prosociality is often equated with reciprocity (i.e., tit-for-tat), feeling indebted to another, being "forced" to give, or feeling obligated to return a favor should impinge on one's core need for autonomy—that is, the feeling that one's behavior is freely chosen and "owned" at the highest level. We hypothesize, however, that carefully nudging individuals to practice their own prosocial acts can actually cultivate feelings of autonomy as individuals implement their own unique brand of kindness and are poised to take ownership of the fruit of their overtures (see also Nelson et al., 2014).

Receiving everyday prosociality means experiencing another person demonstrate care, support, sensitivity, or thoughtfulness. As a result, a key outcome of prosociality is likely to be increased feelings of connectedness to others. Likewise, because recipients may interpret kind acts as validation of themselves or their behavior, they may feel more competent and autonomous. For example, as her act of kindness, Karen might choose to praise Tim for his contribution on a group project, which he interprets as an explicit endorsement of his abilities—

fostering his competence (i.e., having acted properly in the past) and autonomy (i.e., feeling freer when making subsequent decisions).

In the current study, we aimed 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. Although our study necessarily contained some artificial elements, its aim was to employ a broader conceptualization of prosociality in a naturalistic environment to advance researchers' understanding of the functional benefit of prosociality in the real world.

Who Spreads Prosociality?

A key question is how prosociality develops naturalistically (without having been directly prompted by an experimenter). Although humans seem to be endowed with innate prosocial tendencies (Warneken & Tomasello, 2009), a good deal of prosociality 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 prosociality in experimental economic games, and found that generous allocations of resources could indeed spread from person to person (DeSteno, Bartlett, Baumann, Williams, & Dickens, 2010; Fowler & Christakis, 2010; Gray, Ward, & Norton, 2014). In an economic exchange game, for example, participants who had been helped by another gave more money to a stranger than those who had not been helped—a pay-it-forward effect (DeSteno et al., 2010). In a multi-round economic game in which participants were constantly changing partners, giving more money to a partner instead of

keeping it increased the partner's voluntary donations to others in subsequent rounds (Fowler & Christakis, 2010). 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' prosociality (for an exception, see Weinstein & Ryan, 2010).

Primary Aims and Hypotheses

Our study aimed to investigate the hedonic consequences (e.g., increased happiness and need satisfaction) and behavioral consequences (e.g., increased prosocial behaviors) of everyday prosocial behaviors for givers and receivers. Accordingly, we designed a longitudinal study (which included baseline measures, a 4-week active intervention, and one monthly follow-up; see Figure 1) and conducted it in a naturalistic environment (a corporate workplace in Madrid, Spain). Notably, we used experimental methods—the random assignment of employees to be Givers, Receivers, or Controls—that allow for causal inferences.

Defining Prosociality

In this study, we used a subjective and behavioral definition of prosociality, operationalizing it as performing acts of kindness for others (i.e., *everyday prosociality*). These acts are entirely participant-defined; we asked one group (Givers) to plan and perform "acts of kindness" for others, without knowing the purpose of our study. Under the guise of measuring workplace morale, we also prompted others (Receivers and Controls) to count prosocial actions they both observed and performed from a list of positive and negative work-relevant behaviors.

We use the terms *everyday prosociality, acts of kindness*, *prosociality*, or *prosocial behaviors* to refer to all of these personally-defined prosocial behaviors. Although we instructed

Givers to act with altruistic motivation (i.e., acting solely for the benefit of others, without expecting a payback), our definitions do not otherwise depend on any particular motivation, but only on the prosocial behaviors that participants told us they performed.

Hypotheses

We tested the following hypotheses:

1. **Mutual-benefit (Givers and Receivers)**. We hypothesized that both Givers and Receivers would report benefits in well-being and need satisfaction over the short- and long-term from being assigned to perform and receive acts of kindness, respectively.
2. **Pay-it-forward (Receivers)**. Givers' prosocial 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.

Method

Participants

Employees were recruited from Coca-Cola Iberia in Madrid, Spain. Of the approximately 1,200 employees, 88 (72.7% female) participated in the study. Sample size was solely determined by availability; we used the largest possible sample given the constraints of this type of field experiment. There were no data exclusions. Participants' ages ranged from 22 to 55 (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, who were given both a prize of university merchandise and a donation to a charitable organization based on enrollment in the study. We told all 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 abilities.

Group assignment. We randomly assigned participants to one of three groups: Givers (n = 19), Receivers (n = 35), and Controls (n = 34). There were no other conditions. 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. No 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 for the 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 completed weekly

outcomes throughout the intervention.[1] Monthly outcomes were completed at baseline, the end of the intervention, and at a 1- and 3-month follow-up. (See Figure 1 for an overview of study procedures and timeline.)

Materials

Acts of kindness intervention. We instructed Givers to perform five acts of kindness in one day for recipients on a specific list (see our Online Supplemental Material for complete instructions). We highlighted that Givers could choose 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 sacrifices of time, resources, and money. Although the specific acts of kindness that Givers performed were likely to be known 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 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.

[1]Several measures are not reported in this paper. Participants wore RFID badges that tracked their social interactions and reported on their social ties; these measures are described elsewhere (Chancellor, Margolis, Layous, & Lyubomirsky, 2016). Other measures (of personality, work performance, health symptoms, social relations, and flow) employed in the study were not analyzed because they either lacked relevance to the primary aims of this paper (thus, their inclusion did not justify the loss in brevity) or suffered from technical and power issues.

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

Behavioral self-reports. All 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 contained items like "repeating gossip or rumors about a coworker" and "insulting a coworker." We summed positive behaviors and subtracted negative behaviors to arrive at a final count. Due to participants' tendency to report more positive than negative behaviors, this total was almost always positive.[2] Thus, there are two main prosocial behavior variables: Others' prosocial behaviors and own prosocial behaviors.

Weekly outcomes.

***Need satisfaction*.** Participants reported 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) with three sets of 3-item measures. Across all time points, αs ranged from .74 to .91 for autonomy, .76 to .83 for competence, and .71 to .88 for connectedness.

Affect and life satisfaction. The brief Weekly 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*).

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

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 .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 their general happiness on 7-point Likert scales. 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. Across all time points, αs ranged from .78 to .91.

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.

Occupational measures. The Overall Job Satisfaction Scale (Cammann, Fichman, Jenkins, & Klesh, 1983) is a 3-item measure that assesses employees' liking and satisfaction with their job. Across all time points, αs ranged from .79 to .83.

Timeline. 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; see Figure 1).

Analytic Approach

Behavioral Outcomes. Because discrete data violates assumptions inherent in OLS regression, for all behavioral outcomes, we employed mixed-effects models with the Poisson family using a log link (using the lme4 package in R). As 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 $\log^{coefficient}$ multiplicative change in the dependent variable.

As our manipulation involved instructing Givers to change their behaviors, we excluded them from analyses of prosocial behavioral changes by group. The combined equation for prosocial behavior changes by group (i.e., Controls and Receivers only) is the following:

$$\log(Y_{ij}) = \gamma_{00} + \gamma_{01}RECEIVER + \gamma_{10}TIME + \gamma_{11}RECEIVER \times TIME + (u_{oj} + u_{1j}OD + r_{ij})$$

Weekly and Monthly Outcomes. We conducted latent growth curve modeling (using the lavaan library in R) to examine changes in weekly and monthly outcomes. We used full information maximum likelihood estimation (Enders & Bandalos, 2001) with robust standard errors. The growth models for weekly and monthly outcomes are displayed in our Online Supplemental Material A correlation matrix of weekly and monthly outcomes are presented in our Online Supplemental Material.

Results and Discussion

Baseline Analyses

No significant differences for any of the weekly or monthly outcome variables (all *p*s > .27) emerged among our three groups at baseline, indicating that random assignment was successful.

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%. Because our sample was relatively small and suffered from considerable attrition, cell sizes became quite small by the end of the experiment (see Table 1). Thus, caution should be used in interpreting our results, particularly those involving data gathered at follow-ups.

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 *p*s > .18). Participants who completed the 1-month follow-up were marginally higher in feelings of competence at baseline than those who did not, $t(86) = 1.73$, $p = 0.087$. Attrition did not vary based on group assignment (all *p*s > .55).

Givers and Receivers

Givers' kind acts. We asked Givers to list the acts of kindness they performed for Receivers. Altogether, they reported behaviors such as "I brought him a coffee," "I gave her a sweatshirt," "I encouraged him," and "I showed him how to make a PO."

Others' and own behaviors. Even though they were unaware of their special status as recipients of prosociality, Receivers noticed the relative increase in prosocial behaviors in the

office[3] (see *Receiver* and *Time* × *Receiver* estimates for *Others' Prosocial Behaviors* in Table 2 and top panel of Figure 4). Receivers finished the intervention reporting observing 1,035% more prosocial behaviors than controls, $b = 2.43$, $SE = 0.76$, $z = 3.20$, $p = .0014$, which translated to a linear increase of 13% per week, $b = 0.12$, $SE = 0.05$, $z = 2.61$, $p = .0091$. Receivers' observations of prosocial behaviors serve as a manipulation check that Givers were practicing their acts of kindness as instructed. Notably, according to Receivers' observations, this linear increase persisted after the intervention had ended—3 full months after Givers' prosociality assignment had actually concluded.

Were Receivers inspired to practice prosociality themselves? Supporting our pay-it-forward hypothesis, Receivers reported performing more of their own acts of kindness, even though they had not been specifically asked to do so (see *Own Prosocial Behaviors* in Table 2 and lower panel of Figure 4). Receivers reported performing 278% more prosocial behaviors, $b = 1.33$, $SE = 0.61$, $z = 2.18$, $p = .029$, an increase of 7% each week, $b = 0.07$, $SE = 0.04$, $z = 1.97$, $p = .049$. In sum, Receivers reported performing their own prosocial acts for others as a direct consequence of Givers acting prosocially toward them.

Were Receivers paying back acts of kindness to Givers or forward to others? We analyzed Givers' report of others' prosocial behaviors, but found no increase in final intercept, $b = 0.137$, $SE = .972$, $z = 0.141$, $p = .88$, or in change over time, $b = 0.015$, $SE = 0.0677$, $z = 0.22$, $p = .82$. Thus, Givers' reports of others' behaviors do not suggest that Receivers are simply reciprocating prosociality back to Givers.

[3]See Limitations for a discussion of the interpretation of relative and absolute increases in our behavioral self-reports.

Weekly well-being outcomes. Were Givers' prosocial acts mutually beneficial in well-being for both Givers and Receivers (i.e., our mutual-benefit hypothesis)? Over the 4 weeks of the assigned prosociality intervention (see top half of Table 3 and Figure 2), Givers significantly increased in feelings of competence, $b = 0.146$, $SE = 0.07$, $\beta = .729$, $p = .036$, and autonomy, $b = 0.085$, $SE = 0.043$, $\beta = .405$, $p = .048$, compared to Controls. Receivers significantly increased in feelings of autonomy, $b = 0.087$, $SE = 0.04$, $\beta = .484$, $p = .032$, but not competence, compared to Controls. Although Receivers' estimate of increases in competence was also moderate in terms of its effect size, it was not significantly different from zero, $b = 0.102$, $SE = 0.066$, $\beta = .595$, $p = .119$.

Brief discussion. Supporting our mutual-benefit hypothesis, both Givers and Receivers benefited in the short-term from the Givers' practice of prosociality by feeling more autonomous. Givers additionally reported feeling more competent. Surprisingly, neither Givers nor Receivers reported increases in feelings of connectedness with others. Thus, Givers' practice of prosociality did not lead to more satisfaction of core relational needs, but instead resulted in a greater sense of self-efficacy in interacting with the world (i.e., competence) and acting in alignment with their core values (i.e., autonomy).

Likewise, acts of kindness done on Receivers' behalf did not boost satisfaction of core relational needs, but instead resulted in more autonomy. Receivers may have perceived Givers' prosocial acts (which Givers reported as including recognition of Receivers' work accomplishments [e.g., "I praised him in front of everyone for being efficient with what I needed from him"]) as work-relevant, but not socially-relevant, feedback from co-workers. Positive feedback could have increased Receivers' sense that their choices were more meaningful (i.e., eudaimonic; see King & Hicks, 2012) and thus more reflective of their "true selves" and core

values (i.e., autonomy). Finally, Receivers' results suggest that benefiting from everyday prosociality tends to be an unambiguously positive experience.

Monthly well-being outcomes. Further supporting our mutual-benefit hypothesis, over two months (8 weeks; see bottom half of Table 3 and Figure 3), compared to Controls, Givers showed significant increases in life satisfaction, $b = 0.079$, $SE = 0.023$, $\beta = .467$, $p = .001$, and job satisfaction, $b = 0.056$, $SE = 0.023$, $\beta = .381$, $p = .017$, and significant decreases in depressive symptomatology, $b = -0.03$, $SE = 0.008$, $\beta = -.659$, $p < .001$, whereas Receivers showed increases in happiness, $b = 0.052$, $SE = 0.026$, $\beta = .324$, $p = .048$. Giver's changes in happiness were also positive and of similar magnitude as those of Receivers, but not significant, $b = 0.060$, $SE = 0.042$, $\beta = .320$, $p = .154$.

Brief discussion. Thus, a full month after Givers finished practicing their prosociality assignment, the long-term benefits of prosociality remained primarily with Givers—namely, more life satisfaction, more job satisfaction, and fewer symptoms of depression.[4] Receivers' reported gains in happiness, but not in life satisfaction, depressive symptoms, or job satisfaction. Thus, our results suggest that windfalls of prosociality (i.e., being a Receiver) produce relatively

[4]At baseline, Givers' depressive symptoms were relatively low ($M = 0.38$ on a scale from 0 to 3, $SD = 0.30$) and not significantly different from Receivers or Controls ($M = 0.34$, $SD = 0.30$); yet 4 weeks later, Givers' depressive symptoms had dropped even further ($M = 0.21$, $SD = 0.18$), while those of Receivers and Controls increased slightly ($M = 0.38$, $SD = 0.31$). 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.

short-term benefits, whereas efforts involved in creating windfalls for others (i.e., being a Giver) leads to more numerous long-term benefits.

General Discussion

Our 4-week experimental intervention involved assigning Givers to perform everyday prosocial acts for randomly selected Receivers at their workplace, who were unaware that they had been chosen as targets. Givers successfully carried out their assignment, as was reflected in Receivers' ten-fold mean difference in observations of prosocial behaviors (e.g., perform an act of kindness, speak up on behalf of another, make a coworker feel appreciated) around the office compared with Controls.

Givers and Receivers Mutually Benefit

Supporting the hypothesis of mutual benefit, over the 4-week intervention period, Receivers and Givers both reported increases in autonomy compared with Controls, and Givers also reported more competence. However, as Figure 2 illustrates, these results were primarily driven by decreases in the control group. Thus, it may be that giving and receiving prosocial behavior buffer against decreases in these constructs. Over the long-term (i.e., 2 months), the benefits of kindness remained primarily with Givers, which were manifested in higher life satisfaction, fewer symptoms of depression, and higher job satisfaction. Receivers did report more happiness (although Givers' estimates were similarly large, but not significant; see Limitations for a discussion of group size and power). We found no negative short- or long-term impact of receiving interpersonal acts of kindness in the workplace, consistent with prior findings on the effects of autonomously-motivated helpers on recipients (Weinstein & Ryan, 2010).

Altogether, the benefits that Givers earned for themselves were more numerous and lasted longer than the benefits they imparted to others. They became more satisfied with their lives and their jobs, and reported fewer depressive symptoms. Although they labored for the benefit of others, Givers earned positively reinforcing well-being rewards for themselves, with their efforts to be kinder possibly boosting their chances of success across a variety of life domains (Lyubomirsky et al., 2005).

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 elevating the entire office environment.

Even though doing acts of kindness for others is fundamentally a relational activity (and we are arguing that prosociality is a positive "social signal"), we found little evidence that increased connectedness was an outcome of practicing/receiving prosociality. Our results instead suggest that practicing or receiving acts of kindness is distinctly different from everyday social interactions, with prosociality primarily meeting personal psychological needs for mastery and control. An alternative explanation for the inability of prosociality to change participants' feelings of connection with others is that the present study was conducted in an office environment, where the explicit focus is usually on tasks, not people, and thus, Givers may have infused their acts with a high degree of work relevance. To the degree that this is true, a change in contexts (e.g., on a college campus) could produce different results. Cultural context or participant demographics could also play a role; because Spanish culture highly values

relationships (Inglehart & Baker, 2000) or because most of our participants were women, the benefits that participants earned from prosociality could have manifested themselves in other areas of need satisfaction (i.e., a cultural or gender ceiling effect). Lastly, we may have failed to observe changes in connectedness due to our limited sample size. However, despite our relatively small sample size and the plausibility of both contextual and cultural influences, our results are still notable in showing that prosociality can increase happiness by meeting non-relational core needs.

Receivers Pay-It-Forward

Supporting our hypothesis that Receivers would spontaneously report more of their own acts of kindness, taken together, they reported almost three times more prosocial behaviors than Controls—an approximate increase of 7% per week. Our results show that benefitting from a number of "prosocial encounters" over multiple weeks leads directly and spontaneously to reports of greater prosocial behavior towards others in a highly naturalistic environment. Furthermore, because Givers' reports of others' behaviors did not systematically shift over the study, Receivers are likely to be paying-it-forward to others—not simply paying-it-back.

Potential Limitations and Future Questions

Our study faced sample size constraints common in field studies, particularly those in workplace settings. Our sample of 88 participants was divided among three conditions and suffered from substantial attrition. Although this led to small cell sizes (see Table 1), our study did benefit from the power of repeated measurements. Yet, findings from this study should be interpreted with caution due to our small sample size. We hope that future studies attempt to replicate our findings.

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. In our study, all participants were told that they would be assigned activities, and thus those in the control condition (which involved no activity) may have realized that they were in the control condition. However, this criticism of our research design is only valid for hypotheses related to Givers, because neither Receivers nor Controls were assigned to practice an activity. Furthermore, demonstrating that Givers would benefit from performing acts of kindness was our least risky hypothesis (given that the benefits of practicing prosociality have been documented in other studies; see Layous & Lyubomirsky, 2014, for a review).

Did demand or placebo effects play a role in our study? We attempted to reduce this possibility by informing participants in *all* conditions that they would be practicing a potentially happiness-boosting activity and asking all participants about both well-being and positive behaviors. However, because Givers were the only group instructed to perform specific behaviors, they may have had stronger suspicions than other groups that their assigned activity should boost happiness, and, as a result, Givers may have responded more positively to the well-being measures. In addition, our emphasis on Givers' autonomy to choose the who, what, and where of their prosocial acts might have created demand, which could explain Givers' reported increases in autonomy. However, this does not explain why we found increases in (and mediation via) autonomy among Receivers. More broadly, the act of observing others and reporting the number of prosocial acts could have elicited reporting biases. In addition, we may have primed prosociality by offering charitable incentives for participating.

Two potential limitations relate to the conclusions that can be drawn from our results. First, Receivers reported that they engaged in more acts of kindness as the experiment unfolded,

which we interpret as evidence for a pay-it-forward effect. However, Receivers may have simply been more attentive to their own kind acts because they were receiving more kindnesses than usual. Alternatively, Receivers could have been motivated to believe they performed more acts of kindness to justify the kindnesses they themselves received.

Second, our results illuminate the benefits that follow from giving or receiving everyday prosocial acts. However, these benefits may be even broader than we anticipated. For example, given that many everyday prosocial behaviors (e.g., bringing a colleague coffee in a public space) are observable by others, Givers may have experienced praise from others for their kind acts. In turn, this boost in reputation or esteem may have helped drive the effects we observed on Givers. Similarly, Receivers may have benefitted because receiving kindnesses in public may signal one's value to the group.

A set of critiques of our design relate to selection effects. Participation incentives offered included charitable rewards that might have led to oversampling prosocially inclined individuals. Yet, because we offered two kinds of rewards (both personal and prosocial prizes), we likely attracted participants with a variety of motivations. Further, the effects that we observed should still hold true for a sizable percentage of the entire organization (i.e., as approximately one-third of employees participated). In addition, all participants were told they would be engaging in a potentially happiness-boosting activity, which may have led to an oversampling of happiness seekers. Although our sample selection may not have been completely random, assignment to conditions was completely random, suggesting that our between-group comparisons were not impacted by this selection effect. However, it is possible that particularly prosocial individuals were impacted relatively more by being a Giver or Receiver, thus bolstering our effects. Another selection effect could have resulted from Givers picking Receivers based on nonrandom

characteristics (such as attractiveness); notably, however, their choices were constrained to a list of randomly selected employees.

Because considerable attrition in our study began at 4 weeks, and escalated at the 1-and 3-month follow-up, the participants who elected to continue the intervention could have substantively differed from those who dropped out. To be sure, we noted only a marginal baseline difference in competence for those who completed the final time points. 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, and our instructions to Givers to keep their activity assignment secret (but not necessarily the acts themselves) were designed to minimize artificiality (i.e., experimenter-prompted changes) in favor of naturalistic contamination (i.e., individuals acting kindly out of their own volition).

As anticipated by the pay-it-forward hypothesis, Receivers performed prosocial acts at an increasing rate throughout the experiment, thus becoming like Givers in this way. Similarly, Controls may have had experiences paralleling Receivers as Receivers paid their acts forward. Although this sequence of events potentially blurs the distinctions between our conditions, it likely led to more conservative tests of our hypotheses.

Lastly, the type of relationship between Givers and Receivers (e.g., close friends vs. workplace acquaintances) may moderate some of the effects we observed. Future investigators may wish to collect such information to address this question.

Concluding Words and Future Directions

Although our Spanish sample is more diverse in background and age than those used in 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 prosociality intervention to a Spanish workplace). Future research should examine the degree to which deliberate prosocial acts produce the same results in different cultural contexts.

In the workplace, we envision office-based programs that encourage prosociality, 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 and chose how to express or show kindness. 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 prosociality in their workplaces through their own examples—by funding and modeling the kind of prosociality that they aspire to cultivate in others, much like Bill Gates of Microsoft has done for more than 15 years and Mark Zuckerberg of Facebook did in 2015.

In sum, our study suggests that although everyday prosocial acts may be small, they are not insignificant. The benefits of prosociality do multiply, favoring not only those who give but also those who receive and observe.

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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 in Receivers and Controls

	Time			*Receiver*				*Time × Receiver*			
Variable	*b*	*SE*	*t*	*b*	*RR*	*SE*	*t*	*b*	*RR*	*SE*	*t*
Others' Prosocial Behaviors	-0.27	0.04	-6.88***	2.43	1,035%	0.76	3.20**	0.12	13%	0.05	2.61**
Own Prosocial Behaviors	-0.21	0.03	-7.15***	1.33	278%	0.61	2.18*	0.07	7%	0.04	1.97*

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

Table 3

Changes in Weekly and Monthly Outcomes by Experimental Group

Weekly Outcome	N (df)	Time Points (Time Span)	Giver (A) β	Receiver (B) β	SRMR	RMSEA	PCLOSE
Connectedness	88 (15)	5 (4 Weeks)	.131	.192	.059	.050	.472
Competence[1]	88 (9)	4 (3 Weeks)	.729 *	.595	.051	.000	.848
Autonomy	88 (15)	5 (4 Weeks)	.405 *	.484 *	.078	.000	.832
Elevation	88 (15)	5 (4 Weeks)	.032	.161	.054	.000	.759
Weekly Affect	88 (15)	5 (4 Weeks)	.199	.192	.066	.000	.810
Weekly Sat	88 (15)	5 (4 Weeks)	.048	.209	.073	.050	.489
Monthly Outcome	**N (df)**	**Time Points (Time Span)**	**Giver (A) β**	**Receiver (B) β**	**SRMR**	**RMSEA**	**PCLOSE**
SHS	88 (5)	3 (8 Weeks)	.320	.324 *	.051	.000	.696
SWL	88 (5)	3 (8 Weeks)	.467 ***	.211	.049	.000	.861
QIDS	88 (5)	3 (8 Weeks)	-.659 ***	-.244	.064	.040	.457
OJS	88 (5)	3 (8 Weeks)	.381 *	.005	.057	.050	.434

Note. †$p < .10$. *$p < .05$. **$p < .01$. Weekly Aff = weekly affect. Weekly Sat = weekly satisfaction. SHS = subjective happiness scale. SWL = satisfaction with life. QIDS = Quick Inventory of Depressive Symptomatology. OJS = overall job satisfaction. PCLOSE = Probability that the model is not a perfectly fitting model. The model for weekly outcomes (5 time points over 4 weeks) is displayed in our Online Supplemental Material. The model for monthly outcomes (3 time points over 8 weeks) is displayed in in our Online Supplement Material. [1] To improve the overall fit of the model, we included one fewer time point (i.e., only $T_2 – T_6$).

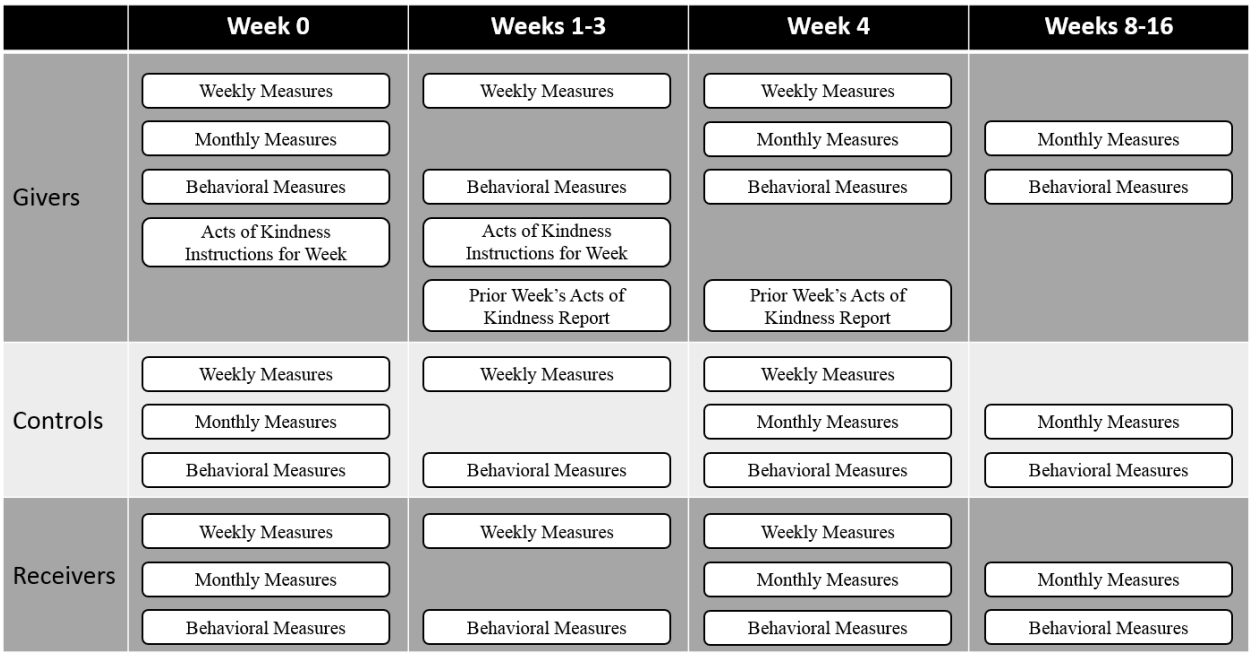

Figure 1. Study timeline and order of materials.

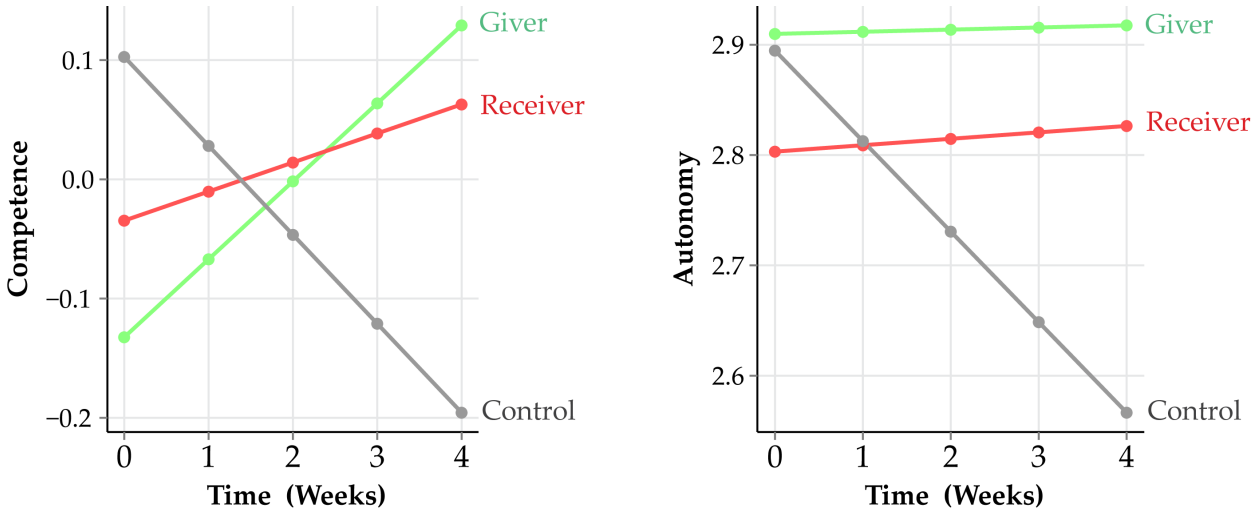

Figure 2. Weekly outcomes by group over the intervention period (4 weeks).

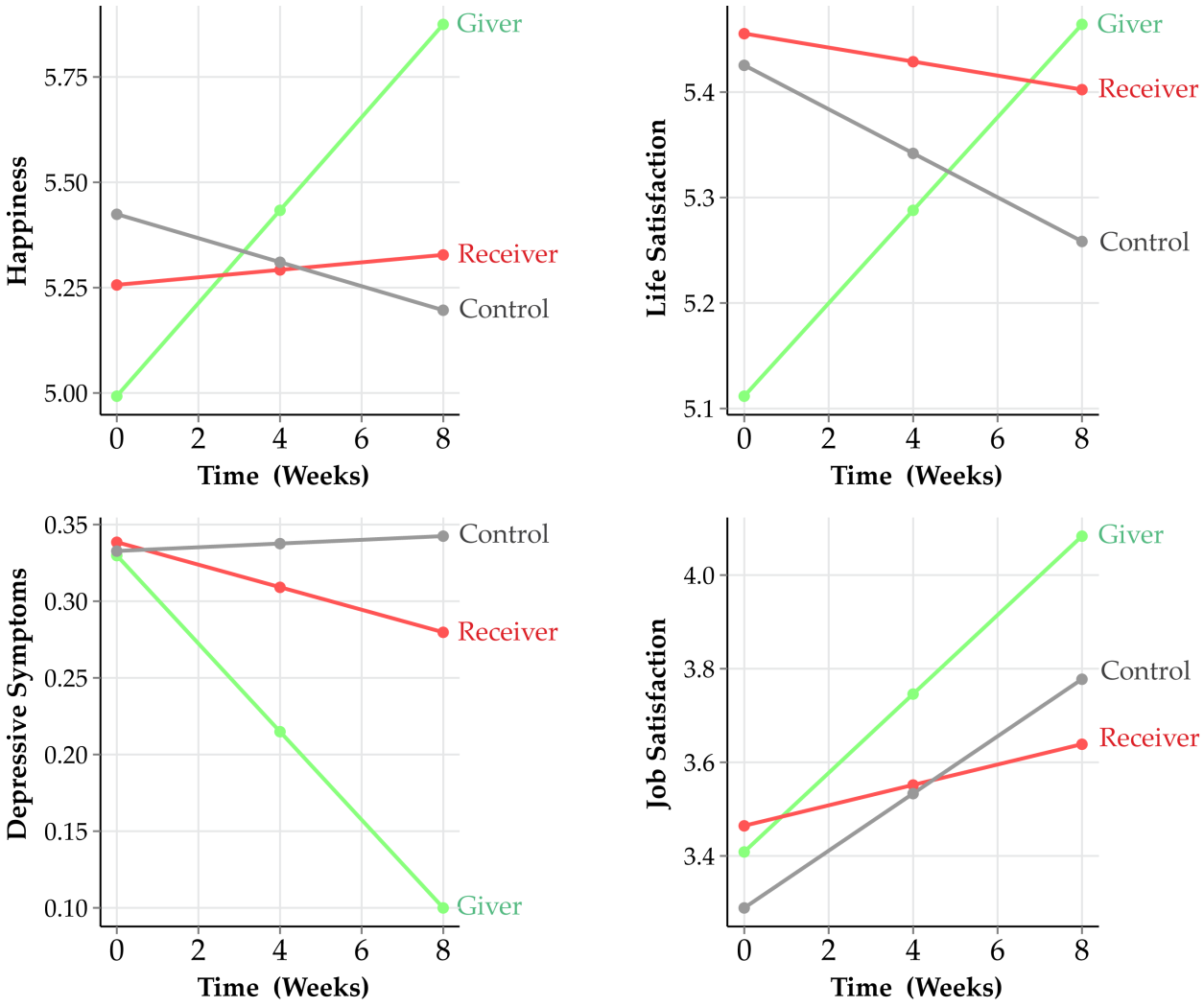

Figure 3. Monthly outcomes by group over the intervention period (4 weeks) and 1-month follow-up.

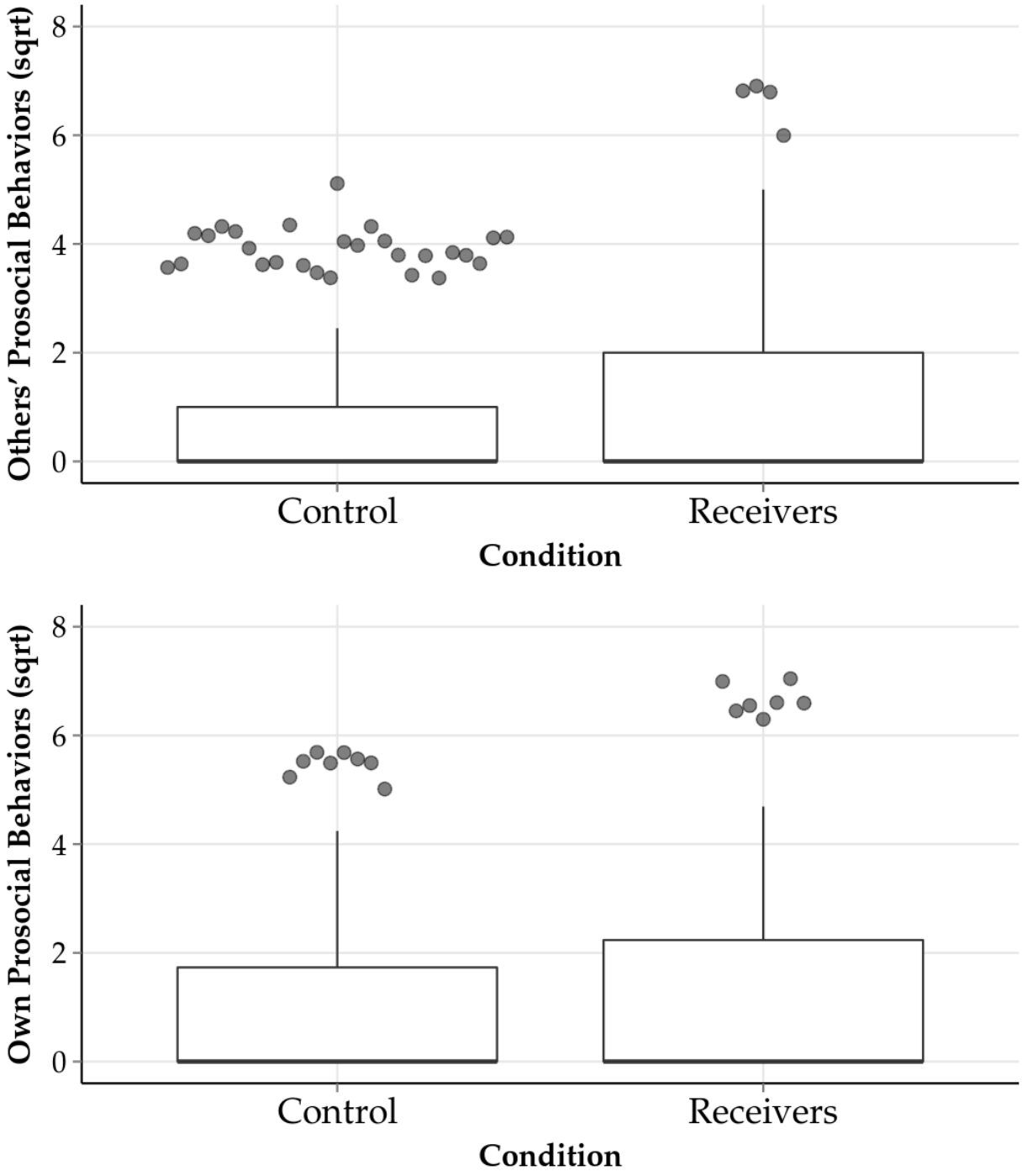

Figure 4. Box-plots of counts of observed and performed behaviors reported by Controls and Receivers (square-rooted). *Note:* Controls are more numerous than Receivers (n = 43 vs. n = 25). Also, due to the nature of count data, distributions are not Gaussian, appear highly positively skewed, and were thus transformed for graphing.