Having Less, Giving Less: The Effects of Unfavourable Social Comparisons of Affluence on People's Willingness to Act for the Benefit of Others

Ana I. Gheorghiu[1], Mitchell J. Callan[2], William J. Skylark[3]

[1] Department of Psychology, University of Portsmouth, Portsmouth, United Kingdom

[2] Department of Psychology, University of Bath, Bath, United Kingdom

[3] Department of Psychology, University of Cambridge, Cambridge, United Kingdom

Corresponding author:

Ana Gheorghiu

ana.gheorghiu@port.ac.uk

Department of Psychology,

University of Portsmouth,

King Henry Building,

King Henry I Street,

Portsmouth,

PO1 2DY

UK

Declarations

Conflicts of interest

The authors declare that they have no conflict of interests.

Availability of data and materials

The data and materials for all studies are available at osf.io/5gky8/

Authors' contributions

All authors contributed to the study conception and design. Material preparation and data collection were performed by Ana I Gheorghiu and Mitchell J Callan. Data analysis was conducted by all authors. All authors contributed to writing the manuscript.

Ethical approval

All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Declaration of Helsinki and its later amendments or comparable ethical standards.

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Abstract

Previous research has found a negative relationship between individual differences in
personal relative deprivation (i.e., resentment stemming from the belief that one is worse off
than similar others) and prosociality. Whether personal relative deprivation causes reductions
in people's willingness to act for the benefit of others, however, is yet to be established.
Across six studies, we experimentally examined whether experiences of personal relative
deprivation via unfavourable (vs. favourable or lateral) social comparisons of affluence
reduced prosociality towards known others and strangers. We found that making hypothetical
(Study 1) or real (Study 2) unfavourable social comparisons of affluence in workplace
contexts reduced participants' organisational citizenship behavioural intentions. Furthermore,
adverse social comparisons of affluence reduced generosity towards the targets of those
comparisons during a Dictator Game (Studies 3 to 6). Across studies we also measured
participants' subjective and objective socioeconomic status and found, contrary to previous
theory and research, no consistent relationship between status and prosociality, and no
modulation of this relationship by either local or macro-level inequality. These results
suggest that local, specific interpersonal comparisons of affluence play a more dominant role
in people's willingness to act for the benefit of a comparison target than do their subjective or
objective class rank or the prevailing income inequality of the state in which they reside.

Keywords: personal relative deprivation, prosocial behaviour, social comparison, social
class, income inequality.

Personal relative deprivation refers to resentment and dissatisfaction stemming from the perception that one is deprived of desired and deserved outcomes relative to other people who are similar to oneself (e.g., co-workers or neighbours). A growing body of evidence suggests that, over and above indicators of objective socioeconomic status, personal relative deprivation is a potent predictor of a multitude of beliefs, behaviours, and social outcomes (Crosby, 1976; Smith et al., 2012). For example, people higher in personal relative deprivation tend to have worse mental and physical health (Beshai et al., 2017; Callan, Kim, & Matthews, 2015), be more interpersonally aggressive (Greitemeyer & Sagioglou, 2018, 2019; Jiang & Chen, 2020), be more materialistic (Aruguete et al., 2020; Kim et al., 2018), have greater problem gambling tendencies (Callan, Shead, & Olson, 2015; Tabri, Wohl, et al., 2017), have lower life satisfaction (Erdogan, et al., 2018; Smith et al., 2018), and prefer smaller-sooner over larger-later monetary rewards (Mishra & Novakowski, 2016; Tabri, Shead, & Wohl, 2017).

Recent research has also documented that personal relative deprivation is negatively associated with people's willingness to act for the benefit of others. For example, Zhang et al. (2016) found that people higher in personal relative deprivation reported lower intentions to engage in volunteering behaviours, and Kim et al. (2017) found that people higher in personal relative deprivation gave less of a hypothetical financial windfall to charity. Callan et al. (2017) similarly observed that higher personal relative deprivation was associated with lower prosociality across a range measures (e.g., generosity during an incentivised Dictator Game, self-reported desire to help others) over and above its covariation with measures of subjective and objective socioeconomic status.

Why are people higher in personal relative deprivation typically less prosocial? Research into the psychology of justice suggests that experiences of personal relative deprivation may lead to a focus on one's own immediate self-advancement and benefit (Callan et al., 2011; Mishra & Novakowski, 2016), rather than on helping and cooperating with others, as a means of redressing the sense of unfairness than can arise from unfavourable social comparisons of affluence. Thus, acting for the benefit of others may lose relevance compared to other goal pursuits (e.g., self-advancement) if a person believes that they are getting less than they deserve compared to what others have (Kim et al., 2017; Kim et al., 2018). Indeed, Callan et al. (2017) found that the negative relationship between personal relative deprivation and the desire to help others was mediated by perceived unfairness.

The research reviewed above points to a robust negative relationship between personal relative deprivation and prosociality, and researchers have tended to assume that experiences of personal relative deprivation cause reduced prosociality; most of this research has been correlational, such that researchers have measured individual differences in personal relative deprivation (e.g., using the Personal Relative Deprivation Scale; Callan et al., 2011). It is therefore not clear whether personal relative deprivation influences prosociality, whether the converse is true, or whether potential third variables are contributing to both higher personal relative deprivation and lower prosociality (e.g., social desirability, traits associated with honesty-humility, Kim et al., 2017; Hilbig et al., 2014; Mishra & Novakowski, 2016).

By manipulating the social comparison contexts that give rise to feelings of resentment and unfairness, the first aim of the current research was to investigate the direction of the relationship between personal relative deprivation and prosociality. In six

studies, we adopted previously-validated manipulations of personal relative deprivation, which involved asking participants to make upward (vs. downward and/or lateral) social comparisons of affluence (Kim et al., 2018), to test the effects of personal relative deprivation on prosocial intentions and behaviours. Studies 1-3 focus on situations in which the target of social comparison and the recipient of prosocial behaviour are the same, known individual (a real or hypothetical co-worker); Studies 4-6 examine whether the effects of PRD generalize to unfamiliar others (strangers) in a one-off interaction. Theoretical accounts of PRD suggest that it entails a widespread change in behaviour such that the individual is focused on increasing their material status and prioritizing their needs over those of others (e.g., Mishra & Novakowski, 2016; Tabri et al., 2015; Zhang et al., 2016). Correspondingly, correlational research has found that self-reported PRD is associated with reduced willingness to help strangers – for example, a reduced willingness to donate to charity (Kim et al., 2017) and less generosity towards an unfamiliar, anonymous co-participant in a Dictator Game (Callan et al., 2017). However, there are reasons for thinking that the causal effect of PRD may be more limited, partly because of the general observation that levels and drivers of prosociality often differ between strangers and acquaintances (e.g., Balliet et al., 2014; Ben-Ner & Kramer, 2011; Jung et al., 2014; Padilla-Walker et al., 2018), but also because, to the extent that the behavioural consequences of PRD reflect a desire to redress perceived injustice (e.g., Callan et al., 2017), such behaviours may primarily be focussed on individuals who are responsible for the feeling that one is relatively deprived. Taken together, Studies 1-6 help establish the causal role of PRD on prosociality, and the generality of any such effect.

As well as probing the relationship between prosociality and the social comparisons that underlie personal relative deprivation, our research had a second aim: to contribute to ongoing debates about the interplay between these variables and socioeconomic status. The correlation between personal relative deprivation and prosociality remains robust after controlling for objective indicators of status (income and education) and subjective status (Callan et al., 2017), but the association between status and prosociality itself remains contentious, with studies variously reporting a negative (e.g., Piff et al., 2010), positive (e.g., Korndörfer et al., 2015), or no relationship (e.g., Callan et al., 2017). Côté et al. (2015) suggest that these divergent findings arise because this relationship between status and prosociality is moderated by macro-level income inequality. They contend that higher income inequality leads upper-class people to engage in more downward social comparisons, producing a sense of entitlement and less helping behaviour, whereas a more equal society reduces this social comparison tendency. Consistent with this, Côté et al. (2015) found that the association between household income and generosity during a dictator game was negative in US states with high levels of income inequality (as gauged by the Gini coefficient) and positive in states that were low in income inequality. However, recent large-scale studies have found no such moderating effect in other populations (Schmukle et al., 2019; von Hermanni & Tutić, 2019). In our studies, we collected measures of objective and subjective socioeconomic status, as well geographical data, allowing us to examine the overall relationship between social rank and prosociality and whether this is moderated by the inequality in the participant's home state. In addition, our manipulations afford a new test of Côté et al.'s theoretical proposal: if the association between social class and prosociality depends on the extent to which upper-class individuals engage in downward social

comparisons, then our manipulation of local comparisons (that is, of inter-personal inequality) would be expected to produce the same kind of modulation as macro-level inequality. We therefore test whether prosociality depends on the interaction between social class rank and our experimental conditions.

To summarize, our goals were (1) to explore further the relationship between personal relative deprivation and prosociality, by investigating whether local comparisons with similar others affect generosity towards those comparison targets and other individuals, and (2) to explore further the association between social class and prosociality, including the overall relationship, its moderation by macroscopic income inequality, and its moderation by local social comparisons that call to mind interpersonal inequalities in affluence. Our studies proceeded as follows: In Studies 1 and 2, participants made hypothetical (Study 1) or real (Study 2) favourable or unfavourable social comparisons in workplace contexts and indicated the degree to which they would engage in prosocial behaviours towards the target of the social comparison and more generally within the organization. In Study 3 participants thought of people who were similar to them but better off, equally well off, or worse off, and played a Dictator Game where they had to decide how much of a monetary endowment to give to their targets for social comparison; Study 4 elicited the same social comparisons but the Dictator Game was played with a stranger (the next person in the survey); and Study 5 asked participants to judge how their own financial status compares with that of the next participant prior to playing the dictator game with that participant as recipient. Finally, Study 6 had participants compare themselves with a similar other who was better or worse off than them and then play a Dictator game in which they allocated funds to themselves, the target of social comparison, and the next participant in the study. In all studies, we measured various indicators of social class rank (household income, educational attainment, and subjective socioeconomic status), and asked our (American) participants to report their home state (Studies 2 to 6).

Participant Sampling

We recruited participants from the U.S.A. through Amazon's Mechanical Turk for all studies except for Study 1 (see details below). Sample characteristics for each study are shown in Table 1. The minimum required sample sizes across studies were determined ahead of data collection; the final sample sizes were not completely predetermined due to the unpredictable nature of online recruitment (e.g., because of over-recruitment, removing participants for having duplicate IP addresses within studies, failing attention/comprehension checks; see Table 1). We based our sample sizes on achieving 80% power to detect at least small-to-medium effects. The data and materials for all studies are available at osf.io/5gky8/. We report all measures, manipulations, and exclusions in these experiments.

[INSERT TABLE 1 HERE]

Study 1

Study 1 asked participants to engage in a hypothetical upward or downward comparison with an imaginary co-worker, and measured hypothetical organisational citizenship behaviours (OCBs; for a review, see LePine et al., 2002) towards both the co-

worker and organisation more generally. OCBs are a form of cooperation and measure of prosociality, which refer to positive workplace behaviours which support co-workers and/or the work environment, but are different from the work tasks an employee is expected to perform as part of their job (Carpenter et al., 2014). These prosocial behaviours can be targeted towards either *individuals* in the organisations (OCB-*I*s; e.g., helping a colleague by switching shifts with them) or towards the *organisation* itself (OCB-*O*s), such as volunteering to promote the company in a presentation towards sponsors (Carpenter et al., 2014). Study 1 investigated whether manipulating feelings of PRD in relation to the workplace (e.g., salary inequality) reduced people's self-reported likelihood of acting prosocially towards the comparison target (the co-worker) and more general prosocial behaviour (towards the organisation).

Method

Participants

Participants were volunteers recruited through social media as part of a class project by students on a postgraduate statistics course in the United Kingdom. Based on IP geolocation, participants were located in 21 different countries, with a large majority residing in the United Kingdom (82.5%; 10.7% were in the United States).

Procedure and measures

As part of a "workplace context" survey, participants were asked to imagine themselves in the following situation (cf. Kim et al., 2018):

> Imagine that you are a **Senior Sales Executive** with a competitive salary at a large management consultancy firm. You earned your **Master's degree in Economics**. Soon after completing your master's degree, you received the offer for your current position. You have now been **working in the company for the last 3 years**. You plan to stay with company for a while, and you have developed a good working relationship with the clients you have brought to the firm since you started with the company.

After being reminded of the key details of the situation on the following page, we asked participants to imagine that they had accidentally received an email from Human Resources with a list of salaries for many of the employees at their company. The salary for one of their departmental co-workers, Sam, was listed in the email. Participants were informed that Sam had the same position, level of experience, and qualifications as them. Varied between-subjects, participants learned either that Sam made £15,000 more than them per year, £15,000 less than them per year, or had the same salary as them.

Next, as a manipulation check, participants rated how satisfied they were with their salary compared to Sam's salary using a 9-point slider scale ("In this situation, how dissatisfied or satisfied are you with your salary compared to Sam's salary?"; -4 = *completely dissatisfied* to +4 = *completely satisfied*). We then asked participants to rate how likely they were to engage in behaviours related to helping Sam (adapted from Moorman & Blakely, 1995; two items for OCB-I: "Sam is experiencing a work-related problem and needs assistance. How likely are you to give up your time to help Sam with a work-related problem?" and "Sam has asked for some time off work. How likely are you adjust your work schedule to accommodate Sam's request for time off?") and helping the organization (two

items for OCB-O: "It is required that at least two people from your department hold a presentation to students at a careers fair at a local college, describing your job roles and what the company does. How likely are you to volunteer to give this presentation at the local college?" and "There are many opportunities outside of work to tell others about your company. How likely are you to show pride when representing your company outside of work?"). Response scales ranged from -4 (*Extremely unlikely*) to +4 (*Extremely likely*) for each of the four items. The two OCB-I items and the two OCB-O items were significantly correlated (r_{OCB-I} = .70, $p < .001$ and r_{OCB-O} = .48, $p < .001$, respectively) and averaged to form composite variables. Higher scores indicated a higher likelihood of engaging in citizenship behaviours.

Two multiple choice attention check items gauged participants' comprehension of the scenario ("Based on the scenario you read, what is your position in the company?" and "Based on the scenario we provided, what degree did you earn?"). Following Wolff et al. (2010; see also Callan et al., 2015), we measured participants' subjective social status (SSS) by asking them to rate their current standing in life in terms of their income, education, and job prestige compared with people in the country that they currently resided in (1 = *I'm very much worse off* to 9 = *I'm very much better off*). Participants also provided their age and gender.

Results

Manipulation check

A one-way between-subjects ANOVA revealed a significant effect of social comparison direction on income satisfaction, Welch's $F(2, 450.23) = 490.64$, $p < .001$. As shown in Table 1, Bonferroni-corrected Welch t-tests revealed that participants who learned that Sam earned more than them felt less satisfied with their income than participants who learned that Sam earned the same as them, $t(451.50) = -30.79$, $p < .001$, $d = 2.89$, and less than them, $t(425.46) = -18.50$, $p < .001$, $d = 1.73$. Participants who learned that Sam earned the same as them felt more satisfied than participants who learned that Sam earned less than them, $t(433.73) = 7.94$, $p < .001$, $d = 0.74$.

[INSERT TABLE 2 HERE]

Personal relative deprivation and citizenship behaviours

A one-way ANOVA indicated there was a statistically significant effect of social comparison direction on participants' willingness to act prosocially towards the target (i.e., Sam), Welch's $F(2, 425.44) = 66.77$, $p < .001$. As shown in Table 2, participants who learned that the target earned more than them were less likely to offer help than participants in either of the other two conditions: more vs. same income, $t(326.17) = -11.06$, $p < .001$, $d = 1.06$, and more vs. less income, $t(323.78) = -10.84$, $p < .001$, $d = 1.04$. Participants were just as willing to help the target when they read they earned the same as vs. less than them: $t(464.99) = 0.383$, $p = .70$, $d = 0.04$ (all comparisons were Bonferroni-corrected).

There was also an effect of the PRD manipulation on people's willingness to engage in organization-level citizenship behaviours, Welch's $F(2, 442.31) = 35.39$, $p < .001$. As shown in Table 2, participants who learned that Sam earned more than them were less inclined to help the organisation than participants who learned that Sam earned the same as them, $t(397.57) = -8.40$, $p < .001$, $d = 0.80$, and earned less than them, $t(435.93) = -4.75$, $p <$

.001, $d = 0.45$. Participants who learned that Sam earned the same as them were also more likely to engage in organization-level citizenship behaviours than participants who learned that Sam earned less than them, $t(446.00) = 3.58$, $p < .001$, $d = 0.33$ (all comparisons were Bonferroni-corrected). Taken together, our results suggest that participants who felt more relatively deprived were less willing to act for the benefit of the comparison target and for the company. OCB-I and OCB-O were significantly correlated, $r = .37$, $p < .001$.

Subjective social status and citizenship behaviours

There was no statistically significant effect of the social comparison direction manipulation on SSS, Welch's $F(2, 454.78) = 0.54$, $p = .58$. Table 3 shows the correlations between SSS and prosocial intentions within each of the experimental conditions and collapsed across conditions. By and large, SSS did not correlate meaningfully with prosocial intentions. The exceptions were within the "target earns less" condition where participants higher in SSS were *more* prosocial. However, as shown in Table 3, moderated regression analyses revealed that the relationships between SSS and OCB-I and SSS and OCB-O were not significantly modulated by comparison target direction (these analyses have 93% power to detect small-to-medium partial-R^2 of 0.02; here and throughout, power calculations were computed using GPower 3, Faul et al., 2007) and overall there was no significant association between SSS and helping intentions (see "Overall Helping" column in Table 3; the tests for correlation between "Overall Helping" and social status indicators have more than 99% power to detect a small-to-medium r of 0.2).

[INSERT TABLE 3 HERE]

Study 2

Consistent with Callan et al.'s (2017) correlational findings involving individual differences in PRD, Study 1 provided initial evidence that upward (vs. downward) social comparisons of affluence reduce people's intentions to engage in interpersonal helping and organization-level citizenship. We also found no evidence for a negative relationship between SSS and prosociality, nor was this relationship modulated by interpersonal inequalities. One limitation of Study 1 was the hypothetical nature of the social comparison context. In Study 2, we asked participants to think of real co-workers who varied in their relative affluence before soliciting their interpersonal helping intentions. We also gauged a broader range of social class indicators (SSS, size adjusted household income, and educational attainment) to further examine the potential relationship between social class and prosociality.

Method

Design and Procedure

All participants retained in the final sample were employed at the time of completing the study (2 additional participants were excluded for not being employed).

Study 2 used a within-subject design. We asked participants to think of three work colleagues one at a time who were similar to them in terms of their background qualifications and attributes (e.g., roughly the same educational or vocational qualifications, years of experience, skill set, motivation) but who differed in their relative affluence: one who was worse off financially than them, one just as well off financially, and one better off financially than them. Participants were given a text box to provide the initials of their comparison target (e.g., AG), which were "piped" through to the questions that followed. Using this

manipulation, Kim et al. (2018) found that participants who made upward vs. downward social comparisons of affluence reported higher resentment and a greater sense of unfairness. After identifying each comparison target, participants were asked how likely they were to help the target with two work-related problems: "How likely would you be to give up your own time to help [target's initials] with a work related problem?" and "How likely are you to adjust your work schedule to accommodate [target's initials]'s request for time off?". These items were identical to those used in Study 1; the response scales ranged from -4 (*Extremely unlikely*) to +4 (*Extremely likely*). The order in which participants were presented with the 3 scenarios (worse off/just as well off/better off) was randomized. The two prosocial intentions items within in each condition were averaged into a single measure (all rs > .65), resulting in three responses per participant.

Finally, participants reported their annual household income ("What is your annual household income (before taxes)?", with response options of less than $15,000; $15,001 to $25,000; $25,001 to $35,000; $35,001 to $50,000; $50,001 to $75,000; $75,001 to $100,000; $100,001 to $150,000; and greater than $150,000), number of adults and children living in the household, education level (did not finish high school, high school graduation, college graduation, postgraduate degree), gender, age, SSS (as per Study 1), employment status, and state of residence. For each study, the annual household income measure was re-coded from 8 categories to a scale using category mid-points, with the value for the unbounded top category being Parker and Fenwick's (1983) median-based Pareto-curve estimator (see Callan et al., 2017). Annual household income was then adjusted by the number of adults and children living in the household (household income divided by the number of adults in the household plus 0.5 times the number children in the household; cf. Skylark & Baron-Cohen, 2017).

Results

Personal relative deprivation and workplace helping

A one-way repeated-measures ANOVA revealed that the manipulation of social comparison direction significantly affected people's likelihood of helping a co-worker, $F(1.92, 591.91) = 19.65$, $p < .001$ (Greenhouse-Geisser corrected). Paired-samples t-tests (Bonferroni corrected for three comparisons, $\alpha = .0167$) revealed that participants who thought of a co-worker who was better off than them ($M_{better} = 1.39$, $SD_{better} = 1.80$) were less willing to help than participants who thought of a target just as well off ($M_{same} = 1.93$, $SD_{same} = 1.57$), $t(308) = -6.30$, $p <. 001$, $dz = 0.36$, and those who thought of a target worse off than them ($M_{worse} = 1.83$, $SD_{worse} = 1.74$), $t(308) = -4.36$, $p < .001$, $dz = 0.25$. There was no significant difference in helping intentions between the "target worse off" and "target similar" conditions, $t(308) = 1.17$, $p = .24$, $dz = 0.07$. These results suggest that engaging in a social comparison with a real co-worker who is similar in background characteristics but financially better off reduces the likelihood of acting for their benefit.

Social class and workplace helping

As shown in Table 3, there were no statistically significant correlations between size-adjusted household income, educational attainment and SSS and the workplace helping measure either within conditions or when helping was averaged across conditions ($\alpha = .79$); the tests for an association between average helping and each social class indicator had 94% power to detected a small-to-medium effect ($r = 0.2$). To test the differences among the

relationships between the social class indicators and helping between conditions, we performed multilevel modelling using the lme4 package (Bates et al., 2015, version 1.1-26) in R (R Core Team, 2021, version 4.0.5). For each social class indicator, we compared two models: a main effects model, which included fixed effects of social class indicator and social comparison direction condition with random intercepts for participants, and an interaction model, which added the cross-product interaction between class indicator and condition. As shown in Table 3, likelihood ratio tests showed that the interaction terms did not significantly improve the fit, suggesting that the relationships between the social class indicators and helping are not modulated by inequality at the level of interpersonal social comparisons of affluence. Because of the complexity of computing power for multilevel models, we also ran a simpler, fixed-effects analysis for which we could more readily report the power and which also provides a robustness check that our inferences are not the consequence of particular analytic decisions (e.g., Skylark et al., 2020). To this end, for each participant we subtracted the helping measure in the downward comparison condition from the value in the upward comparison condition an index of the effect of social comparison; we then tested whether this index was correlated with any of the social class indicators. In each case, the correlation was close to zero: $r_{SSS} = .10$, 95% CI [-.01, .21], $p = .079$; $r_{Income} = .07$, 95% CI [-.04, .18], $p = .215$; $r_{educ} = -.01$, 95% CI [-.13, .10], $p = .794$. These analyses had 94% power to detected a small-to-medium effect ($r = .2$).

Taken together, these analyses suggest that social status does little to moderate the effect of social comparison. Although we had collected data about each participant's home state, there data were too sparse to permit meaningful analysis of the interaction between macro-level inequality and social class. Rather, we postpone such analysis until the end of the experimental series, when we collate data across multiple studies.

Study 3

Study 2 replicated our Study 1 findings using a real social comparison target: experimentally-manipulated personal relative deprivation affected willingness to help the target individual, and there were no meaningful associations between any of the social class indicators and interpersonal helping. In our next two studies, we aimed to generalize the findings from our first two studies to a different measure of prosociality, specifically, the Dictator Game (DG). In the DG, a participant is asked to assume the role of "Dictator" and decide whether to give any portion of an endowment to a recipient. The DG is widely viewed as measure of prosociality insofar as giving any amount of money to a recipient departs from selfishness on the part of the dictator. In Study 3, we asked participants to think of real social comparison targets who varied in their relative affluence before they played hypothetical DGs with these identified targets as recipients. We expected that participants would be less generous during the DG when they were induced to experience PRD (i.e., when making upward vs. lateral or downward social comparisons of affluence).

Method

Design and Procedure

We told participants the study concerned "social information and decision making". Participants first reported their annual household income, number of adults and children living in their household, education level, gender, age, SSS, and state of residence as per Study 2. We then asked participants to think of three comparison targets across different

levels of relative affluence (better off, worse off, and just as well off financially as them). For example, in the "better off" condition, participants read: "In this question, we'd like you to think of one individual that you know who is **similar to you** in many respects (e.g., roughly the same educational or vocational qualifications, years of experience, skill set, motivation) and **who is better off financially than you are.**" Participants identified and responded to their chosen targets one at a time. Like Study 2, participants identified their comparison target by typing their initials into a text box which was "piped" to a dictator game. After each comparison, we asked participants to imagine they were unexpectedly given $1,000 and to divide the money between themselves and the comparison target ("Imagine you have been unexpectedly given $1,000 and you have the opportunity to give some or all of the money to [piped initials]. How would you divide the full $1,000 between yourself and [piped initials]"). Participants could only enter numerical values that summed to $1,000. The order in which the conditions were presented was randomised.

Results

Financial generosity

The social comparison manipulation had a significant effect on the financial generosity of the participants towards the targets, $F(1.89, 760.76) = 172.47$, $p < .001$ (Greenhouse-Geisser corrected). Follow-up paired-samples t-tests (Bonferroni corrected for three comparisons, $\alpha = .0167$) illustrated that participants gave less money to targets who were financially better off than them ($M_{better} = 130.60$, $SD_{better} = 196.55$) than to targets who were just as well off financially as them ($M_{same} = 205.14$, $SD_{same} = 211.48$), $t(402) = -7.44$, $p <.001$, $dz = 0.37$, or targets who were financially worse off than them ($M_{worse} = 325.61$, $SD_{worse} = 253.75$), $t(402) = -16.54$, $p < .001$, $dz = 0.82$. Participants gave more to the worse off than similarly off target, $t(402) = 12.21$, $p < .001$, $dz = 0.61$.

Social class and financial generosity

As shown in Table 3, there were no significant correlations between participants' education level, size-adjusted household income, or SSS and their financial generosity within any of the three conditions or averaged across conditions ($\alpha = .78$; for the tests of the association with overall helping, the power to detect r of .20 is 98%). Following the same multilevel modelling approach as Study 2 produced the same result: the relationships between the social class indicators and generosity did not differ significantly between conditions (see Table 3). Like for Study 2, we supplemented this multilevel modelling with a simpler analysis that examined the correlations between the effect of social comparison (upward minus downward) and the social class indicators. As before, there was little indication of a meaningful effect: $r_{SSS} = .02$, 95% CI [-.08, .12], $p = .683$; $r_{Income} = .02$, 95% CI [-.08, .11], $p = .736$; $r_{educ} = .03$, 95% CI [-.07, .12], $p = .603$; these analyses have 98% power to detect a small-to-medium effect of $r = .20$. Taken together, these results again suggest that inequality at the level of interpersonal social comparisons does not meaningfully modulate the relationship between social class and prosociality.

Study 4

Our results so far indicate that manipulating the direction of social comparisons of affluence affects prosociality towards real or imagined acquaintances. Study 4 probed the generality of this effect by examining whether favourable/unfavourable comparisons with acquaintances affected generosity towards an anonymous stranger. The approach was based

on that of Callan et al. (2017), who had participants play a Dictator Game in which they split a $10 windfall with the (unnamed and unknown) next participant in the study; responses were incentivized by (truthfully) informing participants that 10 people would be randomly selected to receive the amount they chose to keep for themselves, and another 10 would be randomly selected to receive the amounts that the first group donated. In Callan et al.'s study, participants' generosity correlated with their self-reported background levels of PRD; the present study employed the same approach to assessing prosociality, but experimentally manipulated PRD between-subjects by having participants call to mind acquaintances who were better or worse off than themselves.

Method

Design and Procedure

Participants first reported their annual household income, number of adults and children living in their household, education level, gender, age, SSS, and state of residence as per Study 2. Next, we told participants that later in the survey they would be playing a decision-making game where they would have to imagine themselves in a situation in which they are given $10, which they could keep or give any portion thereof to the next MTurk worker completing the survey. They were provided with examples of the game and were asked to complete a multiple-choice comprehension check item: "If Player A (you) transfers $1 to Player B (the next worker), how much will Player A (you) get?".

Using a between-subjects design, we then asked participants to think of and identify the initials for "three people who are like you (e.g., a friend from high school) but who are materially and/or socially better off [worse off] than you." Participants were provided with three open text boxes to provide the targets' initials along with brief descriptions of how they know the targets and in what ways they are better off (or worse off) than them.

Finally, participants played an incentivized dictator game where they indicated how they would distribute $10 between themselves and the next participant. At the end of the study, we paid bonuses to 10 randomly selected "dictators" according to how much of the hypothetical windfall they said they would keep for themselves. We also paid another 10 randomly selected participants according to how much these participants said they would give to the next worker. Participants were informed of this procedure prior to making their decision.

Results

Relative deprivation and financial generosity

An independent samples t-test indicated no significant difference in the amount of money participants gave to the next worker between the better off ($M_{better} = 3.35$, $SD_{better} = 2.28$) and worse off target ($M_{worse} = 3.37$, $SD_{worse} = 2.37$) conditions, $t(612.95) = .130$, $p = .896$, $d = 0.01$ (this analysis had 99% power to detect a small-to-medium $d = 0.35$), suggesting that unfavourable social comparisons with known others do not elicit a generalized reduction in prosociality that extends to strangers.

Social class and financial generosity

As shown in Table 3, there were no significant correlations between the social class indicators and generosity during the dictator game within the two conditions (these analyses had 94% power to detect $r = .2$). SSS correlated significantly *positively* with generosity collapsed across conditions. Moderated regression analyses showed that the relationships

between the social class indicators and generosity did not differ significantly between the social comparison conditions (see Table 3; these analyses had 94% power to detect a small-to-medium partial R^2 of .02).

Study 5

Unlike Study 3, Study 4 found no difference in generosity during a DG played with the next MTurk worker when participants first thought of social comparison targets who were better (vs. worse off) than them. What might explain this null effect? One possibility is that our social comparison manipulation simply did not influence people's experiences of PRD. To test this possibility, we conducted a manipulation validation study (285 Amazon MTurk workers; 140 men, 145 women; M_{age} = 35.05, SD_{age} = 11.9) where, like the Study 4 manipulation, participants thought of three individuals who were similar to them (e.g., a friend from high school), but who were materially and/or socially worse off than them (downward comparison), similar to them (lateral comparison) or better off than them (upward comparison). To assess PRD, we used Callan et al.'s (2011) 5-item Personal Relative Deprivation Scale (e.g., "I feel deprived when I think about what I have compared to what other people like me have"; 1 = *strongly disagree* to 6 = *strongly agree*; α = .83). A one-way between-subjects ANOVA revealed a significant effect of social comparison direction on PRD, Welch's $F(2, 186.60)$ = 10.03, $p < .001$, ηp^2 = .07. Bonferroni-corrected follow-up tests revealed that participants who made upward comparisons felt more relatively deprived (M_{better} = 3.41, SD_{better} = 1.04) than those who made lateral (M_{same} = 2.94, SD_{same} = .96), $t(190.7)$ = 3.31, p = .001, d = .47, or downward comparisons (M_{worse} = 2.76, SD_{worse} = 1.02), $t(183.5)$ = 4.31, $p < .001$, d = .63. Thus, our manipulation of social comparison of affluence affects PRD in the expected directions yet it did not seem to "spillover" to influence generosity during a DG in Study 4.

Another possible explanation for why PRD did not affect giving behaviour in Study 4 is that, unlike in Study 3, the relative affluence of the recipient during the DG was ambiguous (i.e., we did not specify whether the recipient was better or worse off financially than the participant). Our participants probably varied in their beliefs about the financial resources that the "next worker" had available to them, and Matthews et al. (2016) found that people tend to believe that others are more affluent than they are. If our participants in Study 4 tended to believe, on average, that the "next worker" was financially better off than them, then it was unlikely that any level of momentary felt deprivation was going to affect generosity during the DG, presumably because participants are less prosocial towards a "better off" recipient (cf. Studies 1 to 3). We addressed this possibility in our next two studies. In Study 5, we asked participants to rate how their discretionary income compares with that of the next MTurk participant to complete the survey. Participants then played a DG with the next participant as the recipient. Following Matthews et al. (2016), we expected that on average participants would believe that the next worker's discretionary income is higher than their own, and that these latent beliefs about others' relative affluence would correlate negatively with giving during the DG game.

Method

Design and Procedure

Participants first reported how their discretionary income ["the amount you have to spend as you wish after paying taxes and unavoidable outgoings (e.g., bills/mortgage/rent)"]

compares with that of "the next MTurk participant to complete this HIT [Human Intelligence Task]" using a 9-point scale ranging from "Mine is very much lower" to "Mine is very much higher" (Matthews et al., 2016). We coded responses from -4 to +4 such that zero corresponded to equal affluence ("They are exactly the same") and more positive numbers corresponded to a belief that the other person is relatively more affluent. Next, participants completed a DG game where they had to imagine that they were unexpectedly given $1,000 and to divide the money between themselves and the next MTurk participant (cf. Study 4). Finally, participants reported their annual household income, number of adults and children living in their household, education level, gender, age, SSS, and state of residence as per Study 2.

Results

Consistent with Matthews et al. (2016), participants on average rated the next person's discretionary income to be higher than their own (i.e., compared against the midpoint of 0; $M_{income} = 0.51$, $SD_{income} = 2.06$), $t(277) = 4.16$, $p < .001$ (54% of the sample judged the next worker as better off, 35% worse off, and 12% exactly the same). These subjective beliefs about the next worker's affluence correlated significantly with generosity during the DG, $r = -0.17$, $p = .005$, such that the more participants believed the next worker was better off than them, the less money they gave. This relationship was largely unchanged in a multiple regression analysis controlling for SSS, educational attainment, and size adjusted household income, $\beta = -0.16$, $sr = -0.13$, $t(273) = 2.28$, $p = .024$. As shown in Table 3, none of the social class indicators correlated significantly with generosity during the DG (these correlation analyses had 92% power to detect a small-to-medium r of .2).

These results are consistent with the idea that the null effect of social comparison with acquaintances on generosity towards the next participant in Study 4 was due to the perception that the next participant was better off than them – in other words, the next participant constitutes an upward social comparison (that is, relative deprivation) and correspondingly reduced prosociality toward that target.

Study 6

Study 6 directly compared the effects of social comparisons with acquaintances on prosociality towards both those acquaintances and strangers. Participants completed DG games where they had to distribute monetary windfalls to themselves, the next worker, and identified targets who were better and worse off than themselves. Following our Study 3 and 4 findings, we expected that the effects of upward vs. downward social comparisons of affluence on generosity would be greater for the social comparison targets (who are known others with known relative affluence) than for the next worker (a stranger, whose affluence is ambiguous).

Method

Design and Procedure

Study 6 had a 2 (comparison direction: target better off vs. worse off) X 2 (windfall target: identified target vs. next worker) within-subjects design. Following Study 3, participants were asked to think of and provide the initials for two people, one at a time, who are similar to them but either financially better off or worse off than them. After each comparison, participants were told to imagine they were unexpectedly given $1,000, and

asked to divide the money between themselves, their comparison target, and next MTurk worker. Thus, participants completed two DGs in total involving two other recipients. The order in which participants completed the two DGs was randomized.

Results

Relative deprivation and financial generosity

A 2 (Comparison direction: target better off vs. worse off) x 2 (Windfall target: identified target vs. next worker) repeated-measures ANOVA revealed significant main effects of comparison direction, $F(1, 240) = 73.17$, $p < .001$, $\eta_p^2 = .23$, and the windfall target, $F(1, 240) = 59.61$, $p < .001$, $\eta_p^2 = .20$, on the amount of money given during the DGs. More importantly, there was a significant interaction, $F(1, 240) = 84.63$, $p < .001$, $\eta_p^2 = .26$ (see Figure 1). Two paired-samples t-tests indicated that for the identified targets, participants gave less money when they made upward comparisons ($M_{better} = 134.99$, $SD_{better} = 154.24$) than downward comparisons ($M_{worse} = 254.31$, $SD_{worse} = 199.22$), $t(240) = -10.18$, $p < .001$, $dz = -0.66$. When it came to their generosity toward the next worker, however, participants gave more money when they had first made an upward comparison ($M_{better} = 129.11$, $SD_{better} = 155.06$) than a downward comparison ($M_{worse} = 107.34$, $SD_{worse} = 129.67$), $t(240) = 3.24$, $p = .001$, $dz = 0.21$. Additionally, there was no significant difference between how much participants gave to the known, social comparison target and the amount they gave to the next worker within the "better off" condition, $t(240) = -0.52$, $p = .61$, $dz = -0.03$ – that is, participants were as generous towards a better off target as they were towards the next worker.

[INSERT FIGURE 1 HERE]

Social class and financial generosity

As shown in Table 3, although the majority of the correlations between education, SSS, household income and financial generosity within conditions were not statistically significant, SSS did *positively* correlate with the amount of money given to the "better off" identified target, and with overall giving averaged across the 4 responses ($\alpha = .74$; for the overall giving measure, the analyses had 88% power to detect small-to-medium correlations of $r = .20$). All in all, there were no consistent patterns of associations between the social class indicators and financial generosity.

Following Studies 2 and 3, we used multilevel modelling to test whether social class moderates the effect of social comparison. We compared two models for each social class indicator: a main effects model (i.e., fixed effects of social class indicator, social comparison direction condition and comparison target, plus the two-way interactions between them, with random intercepts for participants), and an interaction model (i.e., main effects model plus the three-way interaction between class indicator, social comparison condition and comparison target). Likelihood ratio tests showed that the three-way interaction terms did not significantly improve the fit (see Table 3), again suggesting that inequality at the level of interpersonal social comparisons does not modulate the relationship between social class and generosity. Like for Studies 2 and 3, we also explored simplified models in which we computed, for each participant, the difference between the total amount given to other people (the next worker and the target of social comparison) when the social comparison was upwards and the total amount given when the comparison was downwards, and tested

whether this overall index of the effect of local social comparison correlated with SSS, Income, and Education; none of the effects differed significantly from zero: $r_{SSS} = .11$, 95% CI [-.02, .23], $p = .085$; $r_{Income} = -.04$, 95% CI [-.17, .09], $p = .520$; $r_{educ} = -.02$, 95% CI [-.14, .11], $p = .776$; these tests had 88% power to detect a small-to-medium effect of $r = .20$.

Does state-level income inequality modulate the relationship between social class and generosity?

We examined the potential cross-level interaction effects of state-level income inequality and indicators of social class on generosity by collating the data across Studies 3 to 6 ($N = 1,536$; data from one participant was not included because they reported not living in the US). Because there were no significant differences in the relationships between the social class indicators and generosity within social comparison conditions across studies (See Table 3), we used each participant's mean proportion given to the recipient(s) during the DG(s) as our dependent variable for the combined, multi-level analyses (e.g., had a participant given the recipients $100 for each of their three DGs in Study 3, their average proportion given for these aggregated analyses would be 0.10). Following Côté et al. (2015), we obtained Gini coefficients for each US state from the United States Census Bureau; namely, we used the 2016, 1-year estimates. Higher Gini coefficients indicate greater income inequality within states, where 0 represents perfect equality (everyone has the same income) and 1 represents perfect inequality (one individual has all the income). For household income, we re-estimated the value for the unbounded top category of the household income scale using the entire distribution of values (i.e., rather than using the within-study income estimations as above). Household income was again adjusted for household size as above. Following Côté et al. (2015), we divided income values by $10,000, the social class indicators were centred at the grand mean across individuals and Gini coefficients were centred at the grand mean across states prior to analyses (we share the view of Schmukle et al., 2019, that it might be better to mean-centre the social class indicators within states; however, for the sake of comparability with the work that motivated our own we elected to adopt the same analytic approach as that work).

Separately for each indicator of social class, the proportion given during the DG(s) was fit with linear mixed effects models using the lme4 package in R (version 1.1-26) via maximum likelihood estimation. The models included fixed effects for the social class indicator, state-level income inequality (Gini coefficients) and the Social Class Indicator X Income Inequality interaction. We included random intercepts for studies and US state of residence, random slopes by study for the effects of Social Class, state-level Income Inequality and the Social Class X Income Inequality interaction, and random slopes by United States for the effect of Social Class; random effects were allowed to correlate (i.e., the models were "maximal"; Barr et al., 2013; Schmukle et al., 2019). We used Satterthwaite approximations to calculate p-values and confidence intervals using the parameters package (Lüdecke et al., 2020, version 0.13.0) in R.

As shown in Table 4 (Model A), none of the Social Class X State-Level Income Inequality interactions were statistically significant (if anything, the interactions were in the opposite direction from Côté et al.'s, 2015, findings). Inspection of the random-effect variance estimates across models suggested potential overparameterization. As robustness checks, we refit the models with a simplified random effects structure that included only

random intercepts for Study and State. Likelihood ratio tests showed that the simpler, random-intercepts only models were preferable to the models including random slopes and intercepts for all three indicators of social class (for SSS, $\chi2(11) = 2.24$, $p = .997$; for size-adjusted household income, $\chi2(11) = 1.57$, $p = .999$; for education, $\chi2(11) = 5.03$, $p = .930$). As shown in Table 4 (Model B), the fixed effect estimates from these simpler models were very similar to those from the models including random slopes and intercepts.

[INSERT TABLE 4 HERE]

Like for Studies 2, 3 and 6, we also conducted supplementary fixed-effects analyses to further check the robustness of the results to alternative specifications and to facilitate computation of the power of our analyses to detect modest effects. First, we fit models that treated study and state as fixed effects (these models were rank-deficient so the software did not estimate a parameter for the state of Wyoming); the key interaction terms (between social class indicator and Gini coefficient) were $b_{SSS.Gini} = 0.347$, 95% CI [-0.009, 0.703], $p = .056$; $b_{Income.Gini} = 0.002$, 95% CI [-0.001, 0.005], $p = .189$; $b_{educ.Gini} = 0.869$, 95% CI [-0.086, 1.824], $p = .075$. Second, we fit models that collapsed over Study and State; the key interaction terms were then $b_{SSS.Gini} = 0.188$, 95% CI [-0.172, 0.548], $p = .307.$; $b_{Income.Gini} = 0.002$, 95% CI [-0.001, 0.005], $p = .222$; $b_{educ.Gini} = 0.611$, 95% CI [-0.355, 1.577], $p = .215$. All of these tests had more than 99% power to detect a small effect, partial-$R^2 = .02$ (in fact, the power is over 99% even when the putative interaction accounts for only 1% additional variance). Notably, in those cases where the interaction term has confidence intervals that come close to excluding zero (i.e, where $p < .1$), the effect is the opposite of that reported by Côté et al. (2015).

General discussion

Our research aimed to clarify the relationship between prosociality and (1) personal relative deprivation, which arises from local comparisons with people who are similar to oneself, and (2) socioeconomic status, which arises from one's overall position in a society. We consider these in turn, and then propose directions for future work.

Prosociality and personal relative deprivation

Our results support and clarify the negative association between personal relative deprivation and prosociality reported in previous work (Callan et al., 2017; Zhang et al., 2016). In Studies 1 and 2, participants reported less willingness to help a colleague who was paid more than them than to help a similar colleague who was paid less than them; in Studies 3 and 6 participants allocated more funds in a dictator game to peers who were less affluent than them than to peers who were more affluent; and in Study 5, participants' generosity towards a stranger was negatively correlated with the extent to which they believed that the stranger was more affluent than them. These results fit with a large body of behavioural economics and social psychology research indicating that people often have a preference for fairness and seek to reduce iniquities between individuals who are ostensibly similar to one another (Bechtel et al., 2018; Macro & Weesie, 2016). A preference for fairness can also be seen in Study 1, where participants were more willing to engage in organizational citizenship behaviours when they were paid the same as a similar co-worker than when they were paid either less or more -- that is, when the employer was treating its workers justly.

In contrast to these robust results, we found little indication that consideration of more affluent (vs less affluent) peers led to a generalized reduction in generosity towards strangers (Studies 4 and 6). There are several possible reasons for this, beyond the general observation that the levels and moderators of prosociality will depend on social distance (e.g., Jung et al., 2014). While the peer-listing procedure is sufficient to elicit changes in self-reported deprivation (Study 5; Kim et al., 2018), it might be too weak to elicit substantial behavioural effects; a stronger manipulation of perceived deprivation might produce more widespread effects on prosociality (e.g., Callan et al., 2011, Zhang et al., 2016). Alternatively, it might be that people are primarily concerned about their status relative to their acquaintances and peers: allocating less funds to an anonymous survey respondent does less to materially or emotionally redress the imbalance in affluence with a named peer than does directly allocating funds between oneself and that peer. Finally, the "strangers" in our study were typically perceived to be more affluent than the participants themselves (Study 5; Matthews et al., 2016), which might create a ceiling effect on participants' generosity towards them. Notably, in Study 5 the target of (latent) social comparison and the prospective recipient of generosity were the same anonymous stranger. The finding that prosociality towards that individual correlated with the perception that they were better/worse off than oneself suggests that the key determinant of whether PRD affects prosociality is perhaps not whether the recipient of help is an acquaintance or stranger, but whether the recipient is a source of relative deprivation. In any case, the overall picture to emerge from our studies is one in which relative affluence substantially affects people's willingness to help the targets of social comparison – that is, the individuals whose affluence determines their own relative status -- but is less important in shaping generosity towards other individuals.

Prosociality and social class

Our second aim was to contribute to ongoing debates about the links between prosociality and social class. Piff and colleagues (2010; see also Kraus et al., 2012; Piff et al., 2018) have argued that the abundant material resources and higher social rank enjoyed by upper-class individuals frees them to focus on internal, self-focused goals – and therefore to be less prosocial – than lower-class individuals, who adopt a more communalistic, other-oriented focus in respond to the more uncertain and hostile environments that they face. In keeping with a growing body of research, our data argue against this characterization: we found no indication of the predicted negative association between social status and prosocial intentions or generosity (see also Balakrishnan et al., 2017; Callan et al., 2017; Greitemeyer & Sagioglou, 2018; Korndörfer et al., 2015; Smeets et al., 2015; Stamos et al., 2020; Van Doesum et al., 2017).

We also found no indication for the interaction between social class and state-level inequality predicted by Côté et al. (2015), mirroring recent large-scale analyses of representative samples from the US and Europe (Schmukle et al., 2019; von Hermanni & Tutić, 2019). Our results contribute to the recent discussions between Coté and colleagues and Schmukle and colleagues, providing further evidence the state-level inequality does not moderate the effect of household income on generosity, particularly when measured through a dictator game (in line with Schmukle & Egloff, 2020), rather than pointing towards mixed evidence of this relationship (Coté & Willer, 2020). One possible reason for these results is that people are often unaware of, or misperceive, the macrolevel wealth inequalities under

which they live (for a review, see Hauser & Norton, 2017), while they are more acutely aware of the wealth inequalities within their local environments—that is, of those individuals who are better off or worse off financially than they are (Callan et al., 2015; Kim et al., 2018). Côté et al.'s proposal that macroscopic inequality leads affluent individuals to engage in more pronounced downward social comparisons that foster a sense of entitlement (and hence less prosociality) might therefore be expected to receive stronger support when we consider the interaction between social class and the social comparisons elicited by our experimental tasks. However, we again found no evidence for such an effect.

There is always the possibility that null results reflect lack of power to detect a small effect. Nonetheless, our results suggest that a person's overall social class or subjective socioeconomic status has a relatively weak (or perhaps rather complex) relationship with their willingness to help others. Rather, our data imply that helping behaviour is shaped by local, specific comparisons more than by overall social class -- as has been found for other outcomes and behaviours (e.g., Callan et al., 2015).

Conclusions and Future Directions

Taken together, the present results coupled with those of other recent studies suggest that the primary determinant of a person's sense of status is the comparisons they make with similar others, and the primary effect on prosociality is a greater willingness to help peers who are worse off than oneself more than those who are better off (e.g., Kim et al., 2018). One limitation of our work is that our manipulations involved explicitly telling people about, or asking them to think of, others who are better/worse off than them. In future, it would be useful to foster such comparisons more naturalistically, for example by initiating interactions between participants who differ in their material wealth but who are otherwise similar. Likewise, our measures of helping were somewhat indirect: we used self-reported intentions (Studies 1 and 2) and hypothetical dictator games (Studies 3 and 6). Study 4 did use incentivized dictator games, but the outcomes were only realized for a randomly-selected subset of participants. Reassuringly, there is evidence that participants transfer a similar amount of money during actual or hypothetical dictator games (Ben-Ner et al.,2008), and respond similarly in studies run online versus in the physical laboratory (Rand et al., 2016). Nonetheless, it would be beneficial for future research to measure more naturalistic prosocial behaviours. For example, participants may be given the opportunity to donate to charity on the way out of the building, a test that is seemingly unrelated to the study they participated in (c.f., Korndörfer et al., 2015).

Finally, our research may have practical value. Our results suggest that, if the goal is to encourage people to act more prosocially, then the focus should be not on changing their actual or perceived social class or rank, or on macroscopic inequalities, but rather on reducing unfavourable social comparisons at a local level – i.e., comparisons with known, similar others (e.g., Kim et al., 2018). For example, if an employer wishes their employees to be more helpful towards their colleagues and the organisation was a whole (Carpenter et al. 2014), they should seek to discourage unfavourable comparisons between co-workers, which are likely to lead to reduced prosociality. Ensuring equal pay for people who do similar jobs and who have similar skills are paid equally is one obvious way to achieve this.

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Tables

Table 1

Sample Characteristics

	Study 1 (N = 686)	Study 2 (N = 309)	Study 3 (N = 403)	Study 4 (N = 615)	Study 5 (N = 278)	Study 6 (N = 241)
M age (*SD*)	42.59 (12.40)	34.90 (10.90)	35.50 (12.07)	34.98 (11.05)	35.26 (11.92)	35.85 (10.94)
Gender (%)						
Male	19.20	61.5	44.4	47.0	45.7	51.0
Female	79.20	38.5	55.3	52.7	53.2	49.0
Unreported	1.60	0	0.2	0.3	1.1	0
M Annual household income (*SD*)	--	59.22k (40.22k)	61.73k (39.45k)	58.25k (39.13k)	55.17k (43.96k)	62.65k (45.26k)
M size adjusted annual household income (*SD*)	--	29.05k (20.29k)	28.05k (19.74k)	26.14k (18.46)	25.79k (21.95k)	29.47k (25.79k)
Education (%)						
Did not finish high	--	0.3	0.2	0.7	1.1	0.4
High school	--	30.7	38.2	35.6	32	34.9
College graduation	--	56.3	48.9	52.8	56.5	53.5
Postgraduate degree	--	12.6	12.7	10.9	10.4	11.2
Participant Exclusions						
Duplicate IPs	0	23	4	26	3	4
Comprehension	96	--	--	32	--	--

Table 2

Mean income satisfaction and organizational citizenship behaviours by experimental condition

	Comparison Direction Manipulation		
	Target earns more	Target earns the same	Target earns less
Income Satisfaction	-2.64$_a$ (1.73)	2.44$_b$ (1.79)	0.91$_c$ (2.33)
OCB-I	1.10$_a$ (1.93)	2.72$_b$ (1.02)	2.68$_b$ (1.01)
OCB-O	1.14$_a$ (1.83)	2.41$_b$ (1.34)	1.92$_c$ (1.63)

Note. Means across rows that do not share a common subscript are significantly different ($p <$ .0167; Bonferroni corrected for 3 comparisons). Standard deviations are shown in parentheses. OCB-I = individual organizational citizenship behaviours. OCB-O = organizational citizenship behaviours. PRD = personal relative deprivation.

Table 3

Correlations between indicators of social class and prosociality across studies and within

conditions

Measures	Social Comparison Direction			Tests of differences among relationships	Overall Helping
	Target better off	Target just as well off	Target worse off		
Study 1 ($N = 686$)					
SSS and OCB-I	.028	.014	.142*	$F(2,680) = 0.50,$ $p = .605$	.036
SSS and OCB-O	.133	-.055	.148*	$F(2,680) = 2.97,$ $p = .052$	.070
Study 2 ($N = 309$)					
SSS and OCB-I	.084	.041	-.014	$\chi^2(2) = 3.74, p = .154$	.044
Income and OCB-I	.098	.094	.031	$\chi^2(2) = 2.03, p = .362$	.088
Educ. and OCB-I	-.027	-.015	-.014	$\chi^2(2) = 0.10, p = .947$	-.022
Study 3 ($N = 403$)					
SSS and giving	.026	-.050	.001	$\chi^2(2) = 2.26, p = .323$	-.014
Income and giving	-.036	-.076	-.043	$\chi^2(2) = 0.73, p = .693$	-.064
Educ. and giving	.043	.068	.009	$\chi^2(2) = 1.30, p = .522$	.042
Study 4 ($N = 615$)					
SSS and giving	.054	--	.102	$F(1,611) = 0.30,$ $p = .586$	.080*
Income and giving	-.065	--	.019	$F(1,611) = 1.13,$ $p = .288$	-.017

	Identified Target	Next Worker		Identified Target	Next Worker		
Educ. and giving	-.007		--	-.088		$F(1,611) = 1.04$, $p = .308$	-.048
Study 5 ($N = 278$)							
SSS and giving	--		--	--		--	.113
Income and giving	--		--	--		--	-.043
Educ. and giving	--		--	--		--	.090
Study 6 ($N = 241$)							
SSS and giving	.242*	.065	--	.098	.063	$\chi^2(1) = 1.00$, $p = .318$	.155*
Income and giving	-.014	-.085	--	.064	-.160*	$\chi^2(1) = 2.05$, $p = .153$	-.048
Educ. and giving	.049	-.024	--	.055	-.029	$\chi^2(1) = 0.04$, $p = .833$	.022

Note. $*p < .05$. SSS = Subjective socioeconomic status. OCB-I = individual organizational citizenship behaviours. OCB-O = organizational citizenship behaviours. Educ. = Educational attainment. Income = household size-adjusted household income (i.e., household income/(number of adults in household + 0.5*number of children in household). Overall helping = helping either collapsed across between-subjects conditions (Studies 1 and 4) or averaged across within-subjects conditions (Studies 2, 3, 5, and 6).

Table 4

Linear mixed effects models predicting generosity during the dictator games

| | Model A | | | | Model B | | | |
| | (random slopes and intercepts) | | | | (random intercepts only) | | | |
Variable	*b (se)*	*t (df)*	*p*	*95% CI*	*b (se)*	*t (df)*	*p*	*95% CI*
SSS								
Intercept	0.233 (0.034)	6.78 (4.10)	.002	[0.166, 0.301]	0.234 (0.033)	7.04 (4.15)	.002	[0.169, 0.299]
SSS	0.006 (0.004)	1.57 (6.92)	.162	[-0.002, 0.014]	0.007 (0.003)	2.00 (1,533)	.046	[0.000, 0.013]
Inequal	-0.292 (0.444)	-0.66 (5.21)	.539	[-1.162, 0.578]	-0.382 (0.346)	-1.10 (43.4)	.276	[-1.059, 0.296]
SSS X Inequal	0.254 (0.180)	1.41 (38.73)	.166	[-0.099, 0.606]	0.225 (0.173)	1.30 (1,531)	.194	[-0.115, 0.565]
Income								
Intercept	0.233 (0.034)	6.80 (4.09)	.002	[0.166, 0.300]	0.234 (0.033)	7.13 (4.15)	.002	[0.170, 0.298]
Income	-0.005 (0.003)	-1.68 (159)	.096	[-0.010, 0.001]	-0.005 (0.003)	-1.62 (1,524)	.106	[-0.010, 0.001]
Inequal	-0.232 (0.427)	-0.54 (4.51)	.613	[-1.070, 0.605]	-0.369 (0.342)	-1.08 (42.01)	.287	[-1.039, 0.301]
Income X Inequal	0.101 (0.141)	0.72 (40.47)	.477	[-0.175, 0.377]	0.097 (0.138)	0.71 (1,516)	.480	[-0.172, 0.367]
Education								
Intercept	0.233 (0.034)	6.79 (4.11)	.002	[0.165, 0.300]	0.234 (0.033)	7.08 (4.15)	.002	[0.169, 0.299]
Educ	0.002 (0.012)	0.13 (5.93)	.901	[-0.022, 0.025]	-0.003 (0.009)	-0.39 (1,532)	.700	[-0.021, 0.014]
Inequal	-0.276 (0.432)	-0.64 (5.27)	.550	[-1.122, 0.571]	-0.417 (0.348)	-1.20 (43.19)	.237	[-1.099, 0.265]
Educ X Inequal	0.643 (0.516)	1.25 (9.66)	.242	[-0.368, 1.655]	0.734 (0.467)	1.57 (1,523)	.116	[-0.181, 1.649]

Note. Inequal = Income inequality; Educ = Education.

Figure legends

Figure 1

Participants' generosity toward either their identified target or the next MTurk worker to do the survey as a function of making upward and downward comparisons
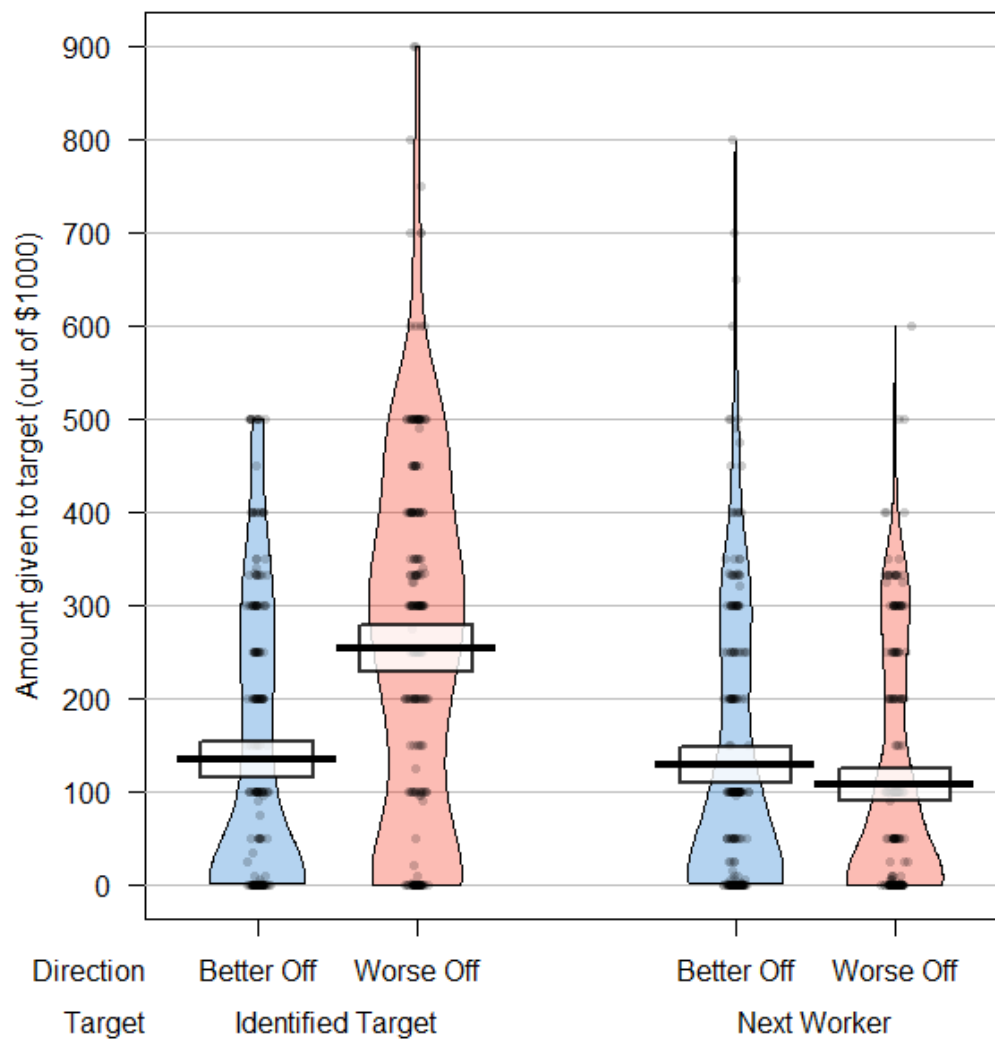
Note. Raw data, descriptive and Inferential statistics plot (using the "yarrr" package in R; Phillips, 2017) of participants' generosity toward either their identified target or the next MTurk worker to do the survey as a function of making upward (identified target better off)

and downward (identified target worse off) comparisons. The black horizontal lines show

mean generosity within conditions and the rectangles show 95% confidence intervals.