

ORIGINAL RESEARCH REPORT

Do Perceiver Effects in Interpersonal Perception Predict Cooperation in Social Dilemmas?

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People's general tendencies to view others as cold-hearted and manipulative (rather than affectionate and trustworthy) may explain defection in social dilemma situations. To capture idiosyncratic tendencies in other-perceptions, we collected mutual judgments in groups of unacquainted individuals in two studies ($N_1 = 83$, $N_2 = 413$) and extracted perceiver effect scores using the Social Relations Model. In both studies, participants later played a public goods game. In Study 1, perceiver effects predicted cooperation beyond self-reported and group-related control variables. However, results were not replicated in a preregistered second study with higher power and a more diverse sample. We discuss implicit group norms as a likely explanation for the inconsistent findings and suggest future directions for addressing generalized expectations in social dilemmas.

Keywords: social dilemma; personality; cooperation; person perception; perceiver effect; generalized other-perception

Many societal goals such as environmental protection or political engagement are characterized by a conflict of interests: Cooperative behaviors (e.g., separating waste, voting) have short-term costs for the individual but promise a profitable long-term payoff for everyone. Thus, there exists a *social dilemma* in which the group's interest is maximized by cooperation, whereas individual interests are maximized by defection (Dawes, 1980). A strategy that individuals commonly apply in such situations is to base their choices on the expected choices of others (e.g., separate waste only if others are expected to do the same). In search of the dispositional underpinnings of such choices, we introduce perceiver effects—people's individual judgment tendencies when forming impressions of strangers—as a novel way to measure generalized expectations about others.

Cooperation and Defection in Public Goods Environments

A widespread approach for creating a social dilemma in experimental research is via the *public goods game* (PGG): Players are part of a group of n members who are endowed with an initial asset a and asked to simultaneously decide whether to contribute (parts of) their asset to a public

good. The public good is then multiplied by a factor x (with x being larger than 1 but smaller than n) and redistributed equally among all players regardless of their individual contribution.¹ Since all players, even those choosing to defect by withholding their entire asset, profit from the public good, the PGG is often referred to as a “free rider problem”. Yet, when all players chose to defect, no public good exists whatsoever and no profit is gained by a multiplication thereof. Thus, a *better* outcome for everyone is achieved if the group collectively cooperates (i.e., all players contributing their entire asset). From the perspective of an individual player, however, the *best* possible outcome is achieved by defection whilst all co-players cooperate.

Importantly, defection can result from two distinct psychological motives. In particular, some players may withhold contributions because they are unconcerned with everyone else's outcome and are outrightly looking for a freeride. Others, however, may defect because they do not want to risk “being a sucker”. Thus, there can be a *temptation* to exploit others but also a *fear* of being exploited by others (Poppe & Utens, 1986; Rapoport, 1988). Here, we are interested in trait-like individual differences that underlie the fear of being exploited.

Individual Differences in Social Dilemma Choices

A myriad of traits have been shown to be related to social dilemma choices including rather narrow constructs such as altruism (e.g., Haesevoets et al., 2018; McAuliffe et al., 2019), social value orientation (e.g., Balliet, Parks, &

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Joireman, 2009), and the dark triad (e.g., Bereczkei & Czibor, 2014; Campbell, Bush, Brunell, & Shelton, 2005; Mokros et al., 2008). The list also includes broader constructs such as agreeableness (e.g., Volk, Thöni, & Ruigrok, 2011) from the Big Five model (Goldberg, 1990) and honesty-humility (e.g., Hilbig, Zettler, & Heydasch, 2012) from the HEXACO model (Lee & Ashton, 2004). Importantly, these constructs seem to share a common core (Hodson et al., 2018) that taps into individual differences in being concerned vs. unconcerned with the welfare of others. We refer to this core as *dispositional communion* (Abele & Wojciszke, 2007).

Whereas dispositional communion readily accounts for variation in the temptation to exploit others, it might not sufficiently account for variation in the fear of being exploited by others. One option for addressing the latter is to have players guess their co-players' contributions and to test whether the players' own contributions depend on those guesses. Indeed, meta-analytic evidence shows that cooperation in social dilemmas is influenced by how much players think their co-players cooperate (Balliet & van Lange, 2013). However, understanding social dilemma behavior from a personality perspective requires a less context-specific and more trait-like conceptualization of people's expectations about others' cooperativeness.

In this context, previous research has mostly considered *generalized trust* (also referred to as trust propensity; meta-analytic $r = .15$; Thielmann, Spadaro, & Balliet, 2020). However, this construct has often been studied in ways that make it difficult to interpret it as trait-like for at least two reasons.

First, most existing research has assessed generalized trust via self-report scales directly before or after an economic game (Balliet & van Lange, 2013), but there is cause for concern that placing such a measure in an economic gaming context will hamper the ability to capture a broad disposition. That is, participants might answer items such as "most people are trustworthy" (Yamagishi & Yamagishi, 1994) with their social dilemma choices in mind and, as such, their responses might serve as justifications for their own behavior (Dawes, McTavish, & Shaklee, 1977). In fact, this concern led the authors of one of the most commonly used generalized trust scales to apply their measure several weeks prior to having participants play a PGG in a seminal study (Yamagishi & Sato, 1986).

Related to this point, think-aloud techniques suggest that, when participants respond to generalized trust scales, many tend to refer to people they personally know, even when items use generic terms such as "most people" (Sturgis & Smith, 2010). Further, the same scales have been criticized for measuring people's levels of *caution* rather than *trust* (Miller & Mitamura, 2003). Taken together, effects of generalized trust in the existing literature may to some extent reflect rather narrow, contextualized constructs that pertain to the specific economic game situations in which they were measured.

Second, it is unclear whether generalized expectations shape cooperation independently of dispositional communion. Evidence suggests that people high in dispositional communion expect more prosociality from others (Pletzer

et al., 2018; Thielmann, Hilbig, & Zettler, 2020), and it is possible that this association overlaps with effects of generalized expectations. This calls into question whether generalized expectations can indeed be considered a unique explanatory variable for social dilemma choices beyond established measures of dispositional communion or whether they are better understood in terms of a mediator explaining how dispositional communion translates into cooperative behavior.

Overall, existing findings show that expectations about others affect individuals' choices in social dilemmas. However, it remains unclear whether (a) it is warranted to interpret such expectations in terms of a broad and decontextualized disposition and (b) such generalized expectations are incrementally valid above dispositional communion. To address these limitations, the present work draws on the concept of perceiver effects in interpersonal perception and investigates their predictive validity in PGGs.

Perceiver Effects as Generalized Expectations

According to the Social Relations Model (SRM; Kenny, 1994), a perceiver's judgment of a target person can be decomposed into the *perceiver effect*, the *target effect*, and the *relationship effect*. If Peter rates Tina's trustworthiness as above average, this can be due to (a) Peter generally seeing others as more trustworthy than most people (his perceiver effect), (b) Tina generally being seen as more trustworthy than most people (her target effect), or (c) some dyad-specific phenomenon (their relationship effect). In a setup where all members of a group judge each other (i.e., a *round-robin* design), subjects are both perceivers and targets, and the SRM can estimate how strongly perceiver, target, and relationship effects contribute to the overall variation in judgments. Further, the model estimates participants' perceiver (and target) effect scores.

Perceiver effects (PEs) track idiosyncrasies that people bring to their first impressions of others, and thus, we conceptualize them as *generalized expectations*. Interpersonal perception research has shown that PEs are moderately related to self-reported dispositional communion and that they have a temporal stability comparable to personality traits (Rau, Nestler, Dufner, & Nestler, in press; Srivastava, Guglielmo, & Beer, 2010). Further, PEs are not susceptible to differences in socially desirable responding (Rau, Nestler et al., in press). It is important to note that conceptualizing PEs as generalized expectations assumes that perceptions are captured at a minimum level of acquaintance and outside of specific contexts (e.g., economic games). Otherwise, PEs may tap into perceptual tendencies that are tied to a particular social environment rather than referring to a broad, trait-like tendency of the perceiver (Rau, Nestler et al., in press).

Regarding the structure of PEs, it has been shown that, across different trait frameworks, PEs reflect not only trait-specific tendencies (e.g., Peter seeing others as particularly trustworthy) but also globally evaluative tendencies (e.g., Peter seeing others positively across

traits) and acquiescence bias (Rau, Carlson et al., in press). Here, we employed a common trait framework for initial social judgments by considering perceptions of agency and communion. Agentic attributes pertain to traits that are thought to benefit the self and include *assertiveness* and *competence* whereas communal attributes serve to benefit others and include *warmth* and *morality* (Abele et al., 2016; Abele & Wojciszke, 2007).

Theoretically, the fear of being exploited by others in a social dilemma should pertain to PEs in the communal domain: Seeing others as cold-hearted and manipulative (vs. affectionate and trustworthy) should lead to defection, but high or low perceptions of others' assertiveness or competence should not matter. Practically, however, it is unclear whether PEs for agency should simply be disregarded or whether they should be included as a control variable when using PEs for communion to predict social dilemma behavior. The reason is that controlling for PEs for agency (or any other theoretically irrelevant domain) frees PEs for communion both from acquiescence variance and from positivity variance.

To illustrate this, let us reiterate that PEs for agency and communion tend to be substantially correlated because some people use higher numbers on Likert-type scales than others irrespective of what is being judged (i.e., both on desirable and undesirable traits; acquiescence) and because some people make more lenient judgments than others irrespective of the trait content being judged (i.e., they provide high ratings on all desirable and low ratings on all undesirable traits; positivity). Whereas removing acquiescence variance is clearly indicated (given its lack of psychological substance), removing positivity variance may be an instance of throwing out the baby with the bathwater: Perhaps, seeing others in globally positive ways is an integral part of seeing them as affectionate and trustworthy. Yet, it is also conceivable that the predictive utility of PEs is specific to communal trait content and that controlling for positivity is expedient. Given this uncertainty, a side goal of the present research was to explore the consequences of controlling for PEs for agency when predicting social dilemma choices by PEs for communion.

Research Objective

We introduce PEs as a novel operationalization of generalized expectations about others to test their predictive validity for cooperation in social dilemmas. Specifically, we hypothesized that PEs for communion measured at zero-acquaintance would predict PGG contributions. To learn about the effects of removing acquiescence and positivity variance from PEs for communion, we explored the role of PEs for agency as a control variable. Further, we conceptualized PEs as a stable trait and therefore predicted that their effect would be independent of influences by the particular group of co-players in the PGG. Finally, we explored whether PEs predicted cooperation incrementally over and above the effect of dispositional communion. We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in both studies.

Study 1

Method

Study 1 data were collected in the context of a comprehensive study on personality and social relationships in a cohort of university students (the CONNECT study). Geukes et al. (2019) provided a detailed description of CONNECT, and a list of previous publications using the CONNECT data can be retrieved from <https://osf.io/2pmcr/>. None of these publications assessed the relation between PEs and PGG choices. We used data from two stages of the study, which we refer to as the *zero-acquaintance stage* and the *social dilemma stage*. The zero-acquaintance stage took place in a lecture hall on the welcoming day at the university one week before the semester began. This stage involved the assessment of PEs. The social dilemma stage took place in a laboratory 14–15 months later and involved the PGG. Further, participants completed three waves of an online questionnaire between these stages (approximately 2 weeks, 3 months, and 10 months after the zero-acquaintance stage), which included the assessment of dispositional communion and demographic information.

Sample

The sample consisted of 83 participants who provided complete data across all stages. The sample size was determined by the size of the cohort and was thus not the result of an a priori power analysis. The sample size was sufficient to detect effects of $r \geq .27$ at an alpha-rate of 5% and a beta-rate of 20% in a one-sided test. Participants ranged in age from 18 to 42 years ($M = 20.45$, $SD = 3.29$) and were mostly female (78%) and native German speakers (95%).

Procedure and measures

At the zero-acquaintance stage, participants were welcomed, randomly seated, and asked not to talk to each other until instructed otherwise. They were then asked to step forward one after the other and to briefly introduce themselves while the others made first-impression judgments concerning agency (from 0 = *This person is submissive/insecure* to 5 = *This person is dominant/self-confident*) and communion (from 0 = *This person is cold-hearted/manipulative* to 5 = *This person is loving/trustworthy*) using a rating sheet. They also indicated how well they knew each person (from 0 = *not at all* to 5 = *good friends*). As intended, the overall level of familiarity was very low ($M = 0.16$, $SD = 0.59$).

Midway through their third semester, participants completed the social dilemma stage in which self-assigned mixed-sex groups of three or four individuals played the PGG. They were given envelopes containing 5€ in cash and asked to contribute any amount they liked (in 50ct increments) to a group pot. It was common knowledge that the group pot would be doubled and equally redistributed among all group members (Game 1). Next, they played another round of the PGG, the only difference being that this time, the reference group was the entire cohort of students participating in the CONNECT study (Game 2). To warrant anonymity, participants were alone when they placed their contribution in a box. Verbatim instructions

can be retrieved from the online supplemental material (OSM1).

After each game, participants answered several questions about their co-players (see **Table 1**). We used these control variables to account for the possibility that players might make larger contributions the better they knew their co-players and/or the more they felt attracted to them. We further controlled for gender given that females tend to have more positive PEs than males (Winqvist, Mohr, & Kenny, 1998).

Finally, in the online questionnaire assessing dispositional communion, participants completed an extended German version of the Self-Attributes Questionnaire (Pelham & Swann, 1989) that incorporated items from the Interpersonal Adjective List (Jacobs & Scholl, 2005). Items were rated on percentile ranks with 10 response options (i.e., the lower or upper 50%, 30%, 20%, 10%, or 5% of the distribution), which were coded from 1 (*lowest rank*) to 10 (*highest rank*). The items “helpful,” “sensitive,” “trustworthy,” “affectionate,” and “cold-hearted” (reverse-keyed) were aggregated first within waves, $\alpha_{w1} = .78$, $\alpha_{w2} = .79$, $\alpha_{w3} = .80$ (for the same procedure, see Rau, Nestler, Geukes, Back, & Dufner, 2019) and then across waves to index dispositional communion ($\alpha = .87$). When waves were missing, participants’ responses were averaged across the remaining waves. Correlations between all assessed variables are provided in Appendix B.

Statistical analyses

We used the *R* (R Development Core Team, 2008) package *TripleR* (version 1.5.3; Schönbrodt, Back, & Schmukle, 2012) to analyze the round-robin ratings from the zero-acquaintance stage to obtain PE scores. In a series of regression models, we then predicted PGG contributions from the PE for communion and added control variables in a stepwise manner. Dummy variables representing group ID did not substantially explain differences in the outcome, $F(23, 59) \leq 1.00, p \geq .476$, in either game, indicating that the nested data structure could be ignored. To account for the high skewness in outcomes, we computed white-corrected standard errors using the packages *lmerTest* (version 0.9–36; Hothorn et al., 2019)

and *sandwich* (version 2.5-0; Zeileis, 2006). In addition, the online supplement (OSM2 & OSM3) contains results based on bootstrapped confidence intervals using the *lavaan* package (version 0.6-3; Rosseel, 2012) and results using logarithmized PGG contributions.

Results and Discussion

Table 2 shows the SRM decomposition of the zero-acquaintance judgments. Crucially, PEs consistently accounted for over 10% of the variance in judgments, a threshold commonly considered indicative of a substantial contribution (Kenny, 1994, 2020) and an important prerequisite for using PEs as predictors in subsequent analyses. Moreover, PEs were captured with excellent reliability, which resulted from the large round-robin group size. Note that a relatively high level of consensus (i.e., target variance) and low level of assimilation (i.e., perceiver variance) for agency compared to communion is a common finding in interpersonal perception research and explicable by the fact that agentic attributes are typically more easily observable and less evaluative than communal attributes (Kenny, 1994, 2020). PEs for agency and communion were substantially correlated ($r = .38$) as is expected if PEs reflect unspecific response tendencies, that is, acquiescence and positivity, in addition to trait-specific judgment tendencies.

Figure 1 shows the relative frequencies of the PGG contributions. Across games, the most common choice was the maximum possible contribution of 5€ (65% and 48%

Table 2: Decomposition of Initial Mutual Perceptions in Study 1.

	Agency	Communion
Perceiver variance	.12	.18
Target variance	.22	.12
Relationship + error variance	.66	.71
Reliability of perceiver effect scores	.95	.96

Note: All estimates are standardized. Variances can be interpreted as proportions of the total variance in dyadic ratings.

Table 1: Descriptive Statistics for Group-Related Control Variables in Study 1.

Game	Variable	Scale	M (SD)
Game 1	Familiarity with other players	1 (<i>not at all</i>) to 6 (<i>very high</i>)	3.53 (0.89)
	Interactions with other players	1 (<i>never</i>) to 6 (<i>almost always</i>)	2.44 (1.19)
	Liking other players	–5 (<i>unlikable</i>) to 5 (<i>likable</i>)	2.50 (1.26)
	Friendships with other players	Binary; 0 = no; 1 = yes	0.26 (0.28)
Game 2	Familiarity with other players	1 (<i>not at all</i>) to 6 (<i>very high</i>)	3.49 (0.88)
	Interactions with other players	1 (<i>never</i>) to 6 (<i>almost always</i>)	4.80 (1.50)
	Liking other players	–5 (<i>unlikable</i>) to 5 (<i>likable</i>)	2.28 (1.16)
	Number of friendships with other players	Open response	9.15 (5.56)

Note: For Game 1, questions were answered separately for each group member and averaged within respondents. For Game 2, questions were answered only once, referring to “the Connecties” as a whole.

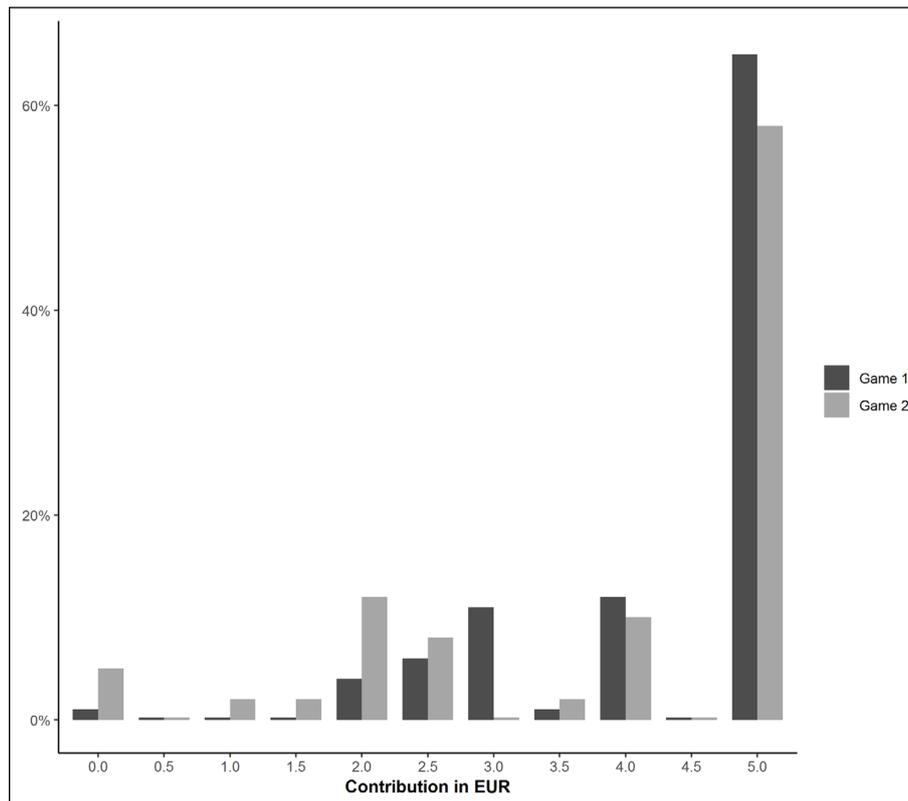


Figure 1: Relative frequencies of contributions in the PGGs in Study 1.

in Games 1 and 2, respectively), which reflects a remarkably high mean level of cooperation, $M(SD) = 4.33(1.07)$ and $3.87(1.55)$ € for Games 1 and 2, respectively. Importantly, however, there was also interindividual variation. In line with our trait perspective, participants' contributions were quite consistent across games, $r = .67$, 95% CI [.53, .78].

Table 3 shows the results of the regression analyses. As expected, participants who perceived others as more communal at the zero-acquaintance stage contributed more money in the social dilemma stage. Whereas the zero-order associations were not statistically significant, the predictive power of PEs for communion was enhanced when PEs for agency were included to account for acquiescence and positivity (see Rau et al., 2019, for a similar suppression pattern). Crucially, PEs for communion significantly predicted PGG contributions across games in the final models that included group-related control variables, gender, and dispositional communion. This effect was sizable with an increase of 1 *SD* in perceived communion leading to an expected increase of 29ct, 46ct, and 35ct in PGG contributions for Game 1, Game 2, and their combination, respectively. Importantly, results remained virtually unchanged when bootstrapped confidence intervals (OSM2) or logarithmized PGG contributions (OSM3) were used.

In sum, Study 1 provided preliminary evidence that generalized expectations in terms of communion may explain cooperation in social dilemmas above and beyond group-related variables, gender, and dispositional communion. This was true no matter whether the dilemma involved a rather small or large group. However, there are also caveats. First, the sample was relatively small

and homogenous with respect to demographic variables. Second, a somewhat narrow operationalization of PEs for agency and communion was used in the zero-acquaintance stage because mutual perceptions were assessed on only one item per domain. Third, given that PEs were measured in a face-to-face setting, it cannot be ruled out that they partly reflected perceptions of actual behavior directed at the perceiver in addition to generalized expectations. For instance, participants who gave a very charming self-introduction might have been smiled at more and might have thus perceived others as more warm-hearted than they would have without evoking as many smiles. Finally, dispositional communion did not predict PGG contributions, which may hint that the used measure did not sufficiently tap into motivational differences underlying social dilemma choices. Overall, Study 1 appeared promising but called for replication.

Study 2

Study 2 was designed to address the limitations of Study 1. Specifically, Study 2 used a larger and more heterogeneous sample, operationalized PEs for agency and communion more broadly, was supplemented by a PE measure that rules out the possibility of PEs reflecting evoked social realities, and used a more established measure of dispositional communion.

Method

Data were collected in the context of a multiwave study on interpersonal attraction and group performance. Participants first completed an online questionnaire and were then invited to the laboratory to take part in

Table 3: Prediction of Public Goods Game Contributions in Study 1.

	Predictor	Model 1		Model 2		Model 3	
		<i>b</i>	<i>p</i>	<i>b</i>	<i>p</i>	<i>b</i>	<i>p</i>
Game 1	PE communion	0.20	.125	0.28	.042	0.29	.028
	PE agency			-0.21	.099	-0.12	.315
	Familiarity with others					-0.28	.112
	Liking others					0.34	.031
	Interactions with others					-0.03	.887
	Friendships with others					0.14	.590
	Gender					0.79	.003
	Dispositional communion					-0.10	.395
	Model		$R^2 = .03$		$R^2 = .07$		$R^2 = .21$
Game 2	PE communion	0.23	.172	0.33	.079	0.46	.008
	PE agency			-0.25	.190	-0.22	.291
	Familiarity with others					-0.54	.010
	Liking others					0.12	.499
	Interactions with others					0.25	.277
	Friendships with others					0.14	.459
	Gender					1.24	.014
	Dispositional communion					0.26	.224
	Model		$R^2 = .02$		$R^2 = .04$		$R^2 = .22$
Combined	PE communion	0.22	.129	0.30	.050	0.35	.007
	PE agency			-0.23	.132	-0.13	.437
	Familiarity with others					-0.59	.004
	Liking others					0.25	.151
	Interactions with others					0.28	.157
	Friendships with others					0.20	.324
	Gender					1.05	.004
	Dispositional communion					0.10	.512
	Model		$R^2 = .03$		$R^2 = .06$		$R^2 = .27$

Note: All predictors except for gender are z-standardized. Gender is coded 0 = female, 1 = male. P-values are two-sided. Contributions are scaled from 0 to 5 EUR. Combined results refer to PGG contributions and group-related control variables that are averaged across games.

small-group meetings once a week for 3 weeks. At these meetings, participants judged each other repeatedly on several dimensions and engaged in a diverse set of tasks. Paralleling Study 1, we focus on round-robin ratings of agency and communion at the first time point (zero-acquaintance stage) and the PGG played at the end of the final group meeting (social dilemma stage). Further, we used data from the online questionnaire to assess dispositional communion and an additional operationalization of PEs (see below).

Sample

The sample consisted of 413 participants nested in 106 same-sex groups of three or four previously unacquainted individuals.² Participants ranged in age from 18 to 62 years ($M = 26.43$, $SD = 5.79$), were balanced in terms

of gender (51% female), and were mostly native German speakers (98%). The sample size was determined based on a priori power computations regarding the study's main purpose, but not regarding the effect of PEs on PGG choices. Nonetheless, the sample size was sufficient to detect effects of $r \geq .12$ at an alpha-rate of 5% and a beta-rate of 20%, which we considered satisfactory.

Procedure and measures

At the zero-acquaintance stage, participants were welcomed, randomly seated, and asked not to talk to each other until instructed otherwise. They were then asked to briefly introduce themselves to their group members and completed round-robin judgments on two agency items (*assertive* and *intelligent*) and two communion items (*affectionate* and *honest*) on a scale from 1 (*does not apply*

at all) to 6 (*applies perfectly*) using personal computers. As judgments were provided in relatively small groups, we expected limited reliability for PE scores. Further, as in Study 1, there was a possibility that perceiver effects would to some degree reflect evoked social realities (e.g., perceptions of non-verbal reactions by one's interaction partners). Thus, we also included an alternative measure of generalized expectations in the online questionnaire, the Online-Tool for Assessing Perceiver Effects (O-TAPE; Rau, Nestler et al., in press).

The O-TAPE is specifically designed to reliably measure PEs without drawing on ratings in face-to-face groups. Instead, it presents participants with screenshots of Facebook pages of 10 standard targets and thereby rules out any social feedback influences. For each target, participants are asked about their first impressions on several items using an 8-point scale. Item-level PEs are then calculated by averaging responses across targets. We used the same four agency and communion items as in the round robin.

In the social dilemma stage, we used the same procedure as in Game 1 of Study 1, with the exception that assets were 4€. Instructions were virtually identical but were supplemented with an example to increase comprehensibility (see OSM1). As a control variable, we assessed the level of liking on a scale from 1 (*not likable at all*) to 6 (*absolutely likable*) among group members shortly before the PGG ($M = 4.26$, $SD = 0.75$).

Finally, we assessed self-reported honesty-humility (HH) as a measure of dispositional communion. By definition, HH describes the "the tendency to be fair and genuine in dealing with others, in the sense of cooperating with others even when one might exploit them without suffering retaliation" (Ashton & Lee, 2007, p. 156). Correspondingly, HH has been shown to be one of the strongest trait predictors of prosocial behavior in economic games in general, and the PGG in particular (Thielmann, Spadaro et al., 2020). We thus considered HH a viable measure of dispositional communion that should be particularly suited as a personality-based predictor in the current context. As operationalized via the HEXACO Personality Inventory-Revised (HEXACO-PI-R, Lee & Ashton, 2004), HH has four facets, and we captured the *modesty* facet with the items "Being a very special person gives me a lot of strength" and "I deserve to be seen as a great personality" (both reverse-keyed) from the Narcissistic Admiration and Rivalry Questionnaire (Back et al., 2013) and the *sincerity*, *fairness*, and *greed avoidance* facets with eight items from the 60-item HEXACO-PI-R (Ashton & Lee, 2009). The choice to assess the modesty facet via narcissism items resulted from our desire to reduce participants' response burden. Due to technical problems, however, items of the latter three facets were only completed by a subsample of $n = 247$ participants, and responses were hardly correlated (average interitem correlation $r = .14$), which led to unsatisfactory internal consistency for the HH scale ($\alpha = .62$; see OSM4 for details). The modesty facet, however, was completed by the full sample and was more consistent (interitem correlation $r = \alpha = .34$). We therefore present results based on modesty (full sample) along with results based on HH (reduced sample). Correlations between all variables are provided in Appendix B.

Preregistration

We preregistered our predictions before we finished collecting the data (<https://osf.io/6mkf3/>) and strictly adhered to the preregistered analysis plan. However, in addition to the issue related to the HH scale, two minor deviations were necessary: First, the level of liking at the third group meeting was computed as the average liking for all fellow group members instead of running TripleR on these ratings because there were too many missing values (i.e., many groups of three rather than four). Second, dummy variables for groups did not substantially explain variance in the outcome, $F(105, 307) = 0.90$, $p \geq .476$, which is why we did not include them in the analyses presented here. However, we verified that including them would not lead to substantively different results.

Results and Discussion

Table 4 shows the SRM decomposition of the zero-acquaintance ratings. As expected, PEs substantially contributed to the overall variance in judgments. Other than in Study 1, however, no substantial differences in the relative contributions of perceiver and target variance were found between agency and communion. This is likely because one of the agency items (i.e., "intelligent") is more evaluative than common agency items (e.g., "dominant"). PE reliabilities were lower than in Study 1 given that they referred to ratings of only three targets. In such designs, it is common for SRM parameters to reach reliabilities of around .50 (Bonito & Kenny, 2010). As in Study 1, PEs for agency and communion were substantially correlated ($r = .48$).

As intended, reliabilities were higher for the O-TAPE. Consistency across targets was high for all four items ($.68 < \alpha < .76$) as well as across agentic item-level PEs ($r = .63$, 95% CI [.57, .69]) and across communal item-level PEs ($r = .73$, 95% CI [.68, .77]). Moreover, PEs from the O-TAPE converged moderately with PEs from the round-robin ratings ($r_{agy} = .32$, 95% CI [.23, .41]; $r_{com} = .32$, 95% CI [.23, .40]). Previous work on the structure of PEs has found that the relative prominence of positivity over trait-specificity is particularly strong when perceiver's exposure to targets is low (Rau, Carlson et al., in press). Given the minimal exposure to targets in the O-TAPE, PEs for agency and communion were highly correlated ($r = .75$). Contributions in the PGG were similar to Study 1

Table 4: Decomposition of Initial Mutual Perceptions in Study 2.

	Study 2	
	Agency	Communion
Perceiver variance	.17	.15
Target variance	.05	.05
Relationship + error variance	.12 + .66	.25 + .55
Reliability of perceiver effect scores	.54	.45

Note: All estimates are standardized. Variances can be interpreted as proportions of the total variance in dyadic ratings. Other than in Study 1, relationship and error variance were separable due to the use of two indicators per construct.

(Figure 2; $M(SD) = 3.56(1.04)\text{€}$), with the most common choice being the maximum contribution of 4€ (73%).

As shown in Table 5, the regression analyses did not support our hypothesis. The PE for communion did not predict PGG contributions at the zero-order level (Model 1) or when the PE for agency or the remaining potential confounders were accounted for (Models 2 and 3). The only variables that significantly predicted contributions were the level of liking and modesty. In fact, the regression weights of the PE for communion, which we predicted would be positive, were close to zero in all models. The results remained virtually unchanged with bootstrapped confidence intervals (OSM5) or logarithmized PGG contributions (OSM6).

When O-TAPE PEs were used instead of the round-robin PEs, the PE for communion did once again not emerge as a significant predictor. In fact, the regression weight was negative once the covariates were included in the model (Model 3a: $b = -0.08, p = .372$, Model 3b: $b = -0.10, p = .257$; see OSM7 for detailed results). Going beyond the preregistered analysis plan, we also checked whether differences would be observed when we used an overall positivity score from the O-TAPE (i.e., the average PE across agentic and communal items) instead of focusing on the PE for communion. Indeed, the prominence of a general positivity factors warrants a unidimensional conceptualization of the O-TAPE (Rau, Nestler et al., in press). Not surprisingly, however, the positivity score

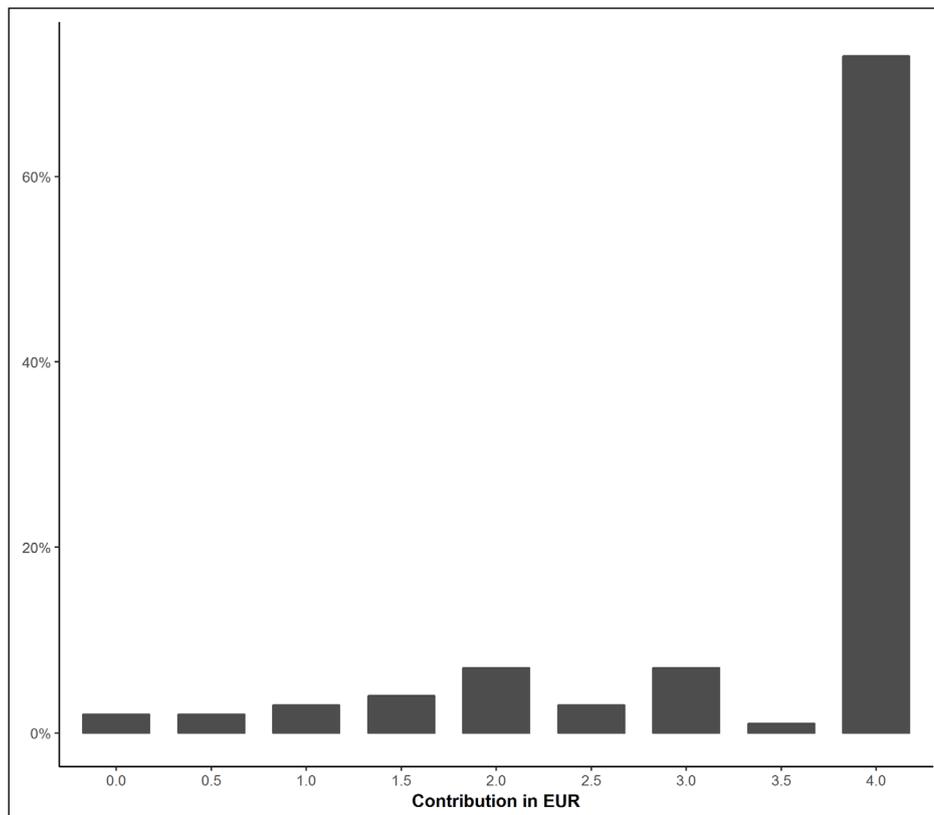


Figure 2: Relative frequencies of contributions in the PGG in Study 2.

Table 5: Prediction of Public Goods Game Contributions in Study 2.

	Model 1		Model 2		Model 3a		Model 3b	
	<i>b</i>	<i>p</i>	<i>b</i>	<i>p</i>	<i>b</i>	<i>p</i>	<i>b</i>	<i>p</i>
PE communion	0.01	.798	0.01	.933	-0.03	.668	0.07	.508
PE agency			0.02	.762	-0.02	.775	-0.07	.364
Liking others					0.15	.024	0.13	.108
Gender					0.17	.100	0.26	.066
a: Dispositional communion: Modesty					0.16	.003	-	
b: Dispositional communion: Honesty-Humility					-		0.07	.302
Model					$R^2 < .01$		$R^2 < .01$	
							$R^2 = .04$	$R^2 = .03$

Note: All predictors except for gender are z-standardized. Gender is coded 0 = male, 1 = female. Contributions are scaled from 0 to 4 EUR. Model 3b is based on a reduced sample of $n = 247$ who completed the full Honesty-Humility scale instead of just the modesty facet.

did not predict PGG contributions in any of the models ($bs < .07$; $ps > .31$).

Theoretically, the absence of evidence for our hypothesis might be explained in terms of measurement issues with respect to the PGG. However, this seems unlikely given that the game was implemented in a straightforward manner and given that liking and modesty predicted people's choices in a sensible direction. Another possibility are measurement issues with round-robin PEs. However, even when we used the O-TAPE measure, which produces highly reliable PEs and rules out social feedback influences on ratings, there was no support for our prediction.

General Discussion

When people defect in a social dilemma, they might do so not only out of a desire to promote their self-interest but also because they are concerned about being exploited by others. To test the dispositional underpinnings of this concern, we introduced PEs in interpersonal perceptions as a novel and subtle operationalization of generalized expectations about others. Specifically, in two studies, participants provided ratings of unknown others' communion and later played a PGG to assess cooperation. Study 1 pointed toward the unique predictive validity of PEs, but Study 2—using a larger and more heterogeneous sample—did not replicate this finding. In fact, the focal estimates in Study 2 were close to zero, suggesting that the significant finding in Study 1 may be a false positive or may have been produced by peculiarities that did not generalize to the design of Study 2.

On the one hand, these results could be interpreted as indicating that social dilemma choices actually depend less on generalized expectations than could be expected from the literature (Balliet & van Lange, 2013; Thielmann, Spadaro, & Balliet, 2020). Instead, how much players liked *their particular co-players* emerged quite consistently as a predictor in our studies, which may hint that PGG choices are primarily shaped by expectations generated ad hoc and thus not indicative of any stable trait. On the other hand, however, it appears plausible that stable interindividual variation exists not only for the temptation to exploit others but also for the fear of being exploited by others. Thus, even though PEs were not robustly related to social dilemma behavior in the present work, it would seem premature to discard them as a measure of generalized expectations that may explain fear-driven defection. Instead, the specific implementation of the PGG used here might explain why PEs did not play out as much as they potentially could.

We implemented the PGG in face-to-face groups using cash money, which had the advantage of high ecological validity but also the disadvantage of lower experimental control. Specifically, although group members made their choices independently and anonymously, they did interact with each other before playing the PGG. Therefore, groups might have established a social identity and implicit norms to some degree. These factors have been suggested to boost cooperation (Bicchieri, 2002), and consistent with this, the contributions observed here were generally quite high. As such, participants might

have inferred that cooperation was demanded not only from them but also from their co-players. Thus, given the presence of a strong group norm, cooperation might have appeared as a safe option, even to participants with general skepticism toward strangers. This would explain why high PGG contributions were observed even among participants with low PEs. Similarly, another recent study failed to replicate well-established effects of personality on cooperation when group members had previously met (Columbus, Thielmann, & Balliet, 2019). Also related to the high level of cooperation observed here, variability in PGG contributions was likely somewhat restricted, which may have made it difficult to detect associations for purely statistical reasons. Importantly, this very fact can arguably not be attributed to the seemingly small incentives at stake in the PGG (i.e., 5€ in Study 1 and 4€ in Study 2). According to recent reviews and meta-analyses, effects of incentive size on behavior in games are small at best (Karagözoğlu & Urhan, 2017; Larney, Rotella, & Barclay, 2019). This also holds for the PGG in particular (Kocher, Cherry, Kroll, Netzer, & Sutter, 2008) as well as when incentives become very large (i.e., equivalent to over 1,000€, Johansson-Stenman, Mahmud, & Martinsson, 2005). In sum, we consider the fact that players were somewhat acquainted and possibly adhering to implicit group norms but not the size of the PGG assets a likely account of the present null results.

Where to go from here?

To guard against influences of strong group norms and restricted variability in future research, we suggest that PGGs should be implemented at earlier stages of the getting-acquainted process and/or in more abstract and anonymous environments (e.g., drawing on computer-mediated interactions). Such setups are known to decrease the likelihood of cooperation (Ledyard, 1995) and might cause concerns of becoming exploited by others to play out in one's behavior more strongly. At the same time, however, future research is needed to systematically address the effects of acquaintance among participants on the usefulness of economic games to study prosocial behavior. In this regard, a fruitful direction for future research might also be to adjust the dynamic of the PGG such that the possibility of being exploited becomes more salient. This could be achieved by multiplying only the lowest individual contribution to the public good (rather than the sum) before the good is redistributed. In such a setup, a single defector suffices to undermine the group's interest, and the fear of being exploited has been shown to be particularly decisive for corresponding behavior (Yamagishi & Sato, 1986).

Finally, future attempts to capture generalized expectations should draw on PE measures that are unconfounded with feedback influences from social interactions such as the O-TAPE (Rau, Nestler et al., in press). In light of the prominence of a global evaluation factor in PEs captured that way, PEs should then be conceptualized along a single negative-positive dimension rather than distinguishing between agentic and communal trait content. Although PEs for particular trait content (e.g., agency) may exert

psychological effects on a state-level (Rau et al., 2019), PEs in terms of global positivity may plausibly account for any consequences that arise from trait-like generalized expectations.

Conclusion

In the present research, we set out to explain individual differences in social dilemma choices resulting from the fear of being exploited. Contending that tendencies to view others as more or less communal in initial encounters reflect consequential generalized expectations, we introduced PEs for communion as a potential predictor of PGG contributions. However, evidence from two face-to-face group studies did not robustly support our claim, suggesting the limited explanatory value of PEs in these contexts. Nevertheless, PEs might be a promising candidate for predicting social dilemma behavior when they are thoroughly measured and when the possibility of being exploited is more likely and made more salient.

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Appendix A

Overview of Online Supplemental Materials (retrievable from <http://bit.ly/2Lorez0>).

- OSM1: Verbatim Instructions for Public Goods Games in Studies 1 and 2
- OSM2: Study 1 Results using Bootstrapped Confidence Intervals
- OSM3: Study 1 Results using Logarithmized Outcomes
- OSM4: Background Information on the Honesty-Humility Measure in Study 2
- OSM5: Study 2 Results using Bootstrapped Confidence Intervals
- OSM6: Study 2 Results using Logarithmized Outcomes
- OSM7: Study 2 Results using O-TAPE Scores

Correlation Matrix of all Variables from Study 1.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1: PE communion	1													
2: PE agency	.38*	1												
3: Contribution in PGG 1	.18	-.10	1											
4: Knowing group members	.02	.18	-.07	1										
5: Liking group members	.15	-.05	.20	.54*	1									
6: Interactions with group members	-.02	.00	-.03	.78*	.47*	1								
7: Friendships with group members	.07	.11	.07	.70*	.48*	.77*	1							
8: Contribution in PGG 2	.15	-.08	.67*	-.19	.01	-.09	-.03	1						
9: Knowing study participants	.09	.09	-.07	.31*	.04	.18	.14	-.15	1					
10: Liking study participants	.11	.05	.16	.11	.25*	-.01	-.01	.11	.28*	1				
11: Interactions with Connecties	.10	-.08	.07	-.03	-.18	-.09	-.21	.06	.37*	.15	1			
12: Friendships with Connecties	.06	.14	.15	.14	.04	.10	.22	.07	.29*	.22*	.21	1		
13: Gender (0 = female, 1 = male)	-.31*	-.09	.18	-.14	-.23*	-.18	-.08	.21	.19	.05	-.03	.11	1	
14: Dispositional communion	.02	.11	-.03	-.01	.14	-.07	-.06	.11	.17	.21	-.06	-.02	.07	1

Note: * $p < .05$.

Correlation Matrix of all Variables from Study 2.

	1	2	3	4	5	6	7	8	9
1: PE communion	1								
2: PE agency	.48*	1							
3: PE communion (O-TAPE)	.32*	.25*	1						
4: PE agency (O-TAPE)	.25*	.32*	.75*	1					
5: Contribution in PGG	.01	.02	.03	.09	1				
6: Liking group members	.34*	.33*	.22*	.24*	.11*	1			
7: Gender (0 = female, 1 = male)	.01	.01	-.18*	-.24*	.03	-.23*	1		
8: Honesty-humility	.00	.10	.13*	.21*	.06	.16*	-.16*	1	
9: Modesty	.01	.01	-.05	-.03	.15*	.01	-.10*	.42*	1

Note: PE = perceiver effect; O-TAPE = Online Tool for Assessing Perceiver Effects; PGG = public goods game; * $p < .05$.

OSM8: Data and R-Code to Reproduce all Reported Analyses

Appendix B

Data Accessibility Statement

Online supplemental materials including the original data and R-code to reproduce all analyses can be retrieved from <http://bit.ly/2Lorez0>. An overview of all additional materials is presented in Appendix A.

Notes

- ¹ There exist numerous variants and implementations of the PGG. For instance, instructions may frame the rules of the game in terms of a societal problem, the game may be repeated several times, the public good may only be distributed across group members if the sum of contributions reaches a certain threshold, etc. Here, we focus on the PGG in its most basic form (e.g., no substantive framing, no repetitions, no threshold).
- ² Initially, all groups were composed of four individuals, and the first meeting was postponed if any members were missing. However, the later meetings were held even when one member was absent.

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Competing Interests

The authors have no competing interests to declare.

Author Contributions

- Contributed to conception and design: RR, SN
- Contributed to acquisition of data: SMB, KG, LN, SK
- Contributed to analysis and interpretation of data: RR, SN
- Drafted and/or revised the article: RR, IT, MDB
- Approved the submitted version for publication: RR, IT, SMB, KG, LN, SK, MDB, SN

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