ADVANCING THE PSYCHOLOGY OF PRO-SOCIAL BEHAVIOR—ALTRUISM, COOPERATION, RECIPROCITY, AND BEHAVIORAL ETHICS

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PUBLISHED IN: Frontiers in Psychology

frontiers Research Topics

frontiers

ISSN 1664-8714
ISBN 978-2-83250-325-6
DOI 10.3389/978-2-83250-325-6

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ADVANCING THE PSYCHOLOGY OF PRO-SOCIAL BEHAVIOR—ALTRUISM, COOPERATION, RECIPROCITY, AND BEHAVIORAL ETHICS

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Citation: Myrseth, K., Wollbrant, C. E.-P., Tinghög, G., eds. (2022). Advancing the Psychology of Pro-social Behavior—Altruism, Cooperation, Reciprocity, and Behavioral Ethics. Lausanne: Frontiers Media SA.
doi: 10.3389/978-2-83250-325-6

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frontiers in Psychology

ORIGINAL RESEARCH
published: 30 August 2021
doi: 10.3389/fpsyg.2021.712333

Reaching Out for Inaccessible Food Is a Potential Begging Signal in Cooperating Wild-Type Norway Rats, *Rattus norvegicus*

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Begging is widespread in juvenile animals. It typically induces helpful behaviours in parents and brood care helpers. However, begging is sometimes also shown by adults towards unrelated social partners. Adult Norway rats (*Rattus norvegicus*) display a sequence of different behaviours in a reciprocal food provisioning task that have been interpreted as such signals of need. The first behaviour in this sequence represents reaching out for a food item the animal cannot obtain independently. This may reflect either an attempt to grasp the food object by itself, or a signal to the social partner communicating the need for help. To distinguish between these two possibilities, we tested in female wild-type Norway rats if the amount of reaching performed by a food-deprived rat changes with the presence/absence of food and a social partner. Focal rats displayed significantly more reaching behaviour, both in terms of number and total duration of events, when food and a potentially helpful partner were present compared to when either was missing. Our findings hence support the hypothesis that rats use reaching behaviour to signal need to social partners that can help them to obtain food.

Keywords: helping, prosocial behaviour, food provisioning, honest signalling, communication, iterated prisoner's dilemma, cooperation, reciprocity

OPEN ACCESS

Edited by:
Conny Ernst-Peter Wollbrant, University of Stirling, United Kingdom

Reviewed by:
Marlen Fröhlich, University of Zurich, Switzerland
Jerome Cohen, University of Windsor, Canada

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *20 May 2021*
Accepted: *30 July 2021*
Published: *30 August 2021*

Citation:
Paulsson NI and Taborsky M (2021) Reaching Out for Inaccessible Food Is a Potential Begging Signal in Cooperating Wild-Type Norway Rats, Rattus norvegicus. Front. Psychol. 12:712333. doi: 10.3389/fpsyg.2021.712333

INTRODUCTION

The ability to comprehend the need of others is widespread in the context of brood care, where variation in offspring begging allows parents to adaptively modulate food provisioning (Grodzinski and Lotem, 2007). Begging signals are frequently used by offspring towards their brood caring parents in mammals (e.g., Kunc et al., 2007; Fröhlich et al., 2020), birds (e.g., Leech and Leonard, 1996) and insects (e.g., Mattey et al., 2018). In contrast it is currently unclear to which extent begging is employed in reciprocal cooperation (cf. de Waal, 2008). If animals respond to the need of prospective receivers of help by increasing their generosity (Schneeberger et al., 2020), this would select for the evolution of signals of demand (Kilner and Johnstone, 1997; Grodzinski and Lotem, 2007), also among unrelated adults (Carter and Wilkinson, 2016; Schweinfurth and Taborsky, 2018a). In fact, great apes have been shown to adjust visual signals depending on how well they seem to understand the intentions of the signaller (Leavens et al., 2005; Cartmill and Byrne, 2007), even if not all studies find support for a response to such signals (Liebal and Rossano, 2017). During reciprocal exchange of goods and services begging can increase the propensity of a previous receiver

of help to return the service (Schweinfurth and Taborsky, 2018a). Even without previous helping experience begging signals can provide an incentive to generously donate goods to a social partner in need, which may serve as a first step to establish reciprocal cooperation (Trivers, 1971; Axelrod and Hamilton, 1981; Barta et al., 2011; Roberts, 2020).

Reciprocal altruism or "reciprocity," where a cost is accepted by an individual to provide a service to a social partner for a delayed benefit, is a mechanism generating evolutionarily stable levels of cooperation between unrelated individuals (Trivers, 1971; Lehmann and Keller, 2006). In the recent past, evidence for enhanced cooperative tendencies of individuals after having received aid from social partners has accumulated in both humans and non-human animals (rats: Rutte and Taborsky, 2007, 2008; Schweinfurth and Taborsky, 2018b,c; bats: Carter and Wilkinson, 2013, 2015; dogs, Gfrerer and Taborsky, 2017, 2018; primates, including humans: Schino, 2007; Jaeggi and Gurven, 2013; Schweinfurth and Call, 2019; birds: St-Pierre et al., 2009; Krama et al., 2012; fish: Croft et al., 2006; Brandl and Bellwood, 2015; for review, see Taborsky et al., 2016, 2021). The propensity to return received favours to social partners may also be modified by the value of previously received service (Dolivo and Taborsky, 2015b; Kettler et al., 2021), the need of prospective receivers (Schneeberger et al., 2012, 2020), and by relatedness among social partners, with kinship affecting reciprocal donations rather negatively (Carter and Wilkinson, 2015; Schweinfurth and Taborsky, 2018c). A question of particular interest in this context is how animals determine the need of prospective receivers, and in turn whether the latter communicate requests to prospective donors (Schweinfurth and Taborsky, 2018a).

A recent study showed that adult Norway rats communicate need to a potentially helpful partner in a reciprocal food-provisioning task (Schweinfurth and Taborsky, 2018a). In 41 out of 50 observed instances involving potential signalling for help in that study, rats in need expressed at least two of three specific behaviours, which appeared in a particular, non-random sequence. These behaviours included reaching out towards the food, emitting ultrasonic calls, and noisy attention grabbing; the behaviours accelerated with increasing need of the recipient (hunger), and they were shown to decrease the latency to food donations provided by the partner. Moreover, prospective receivers displayed the respective next behaviour in the sequence sooner if food donation was delayed, suggesting a sense of urgency communicated to the partner. However, hitherto these alleged signals of need have not been manipulated experimentally in order to test the implied intention of the signaller. This is a serious gap particularly for the first of these three behaviours, "reaching," which is not directed towards the recipient but to the desired food. It is hence unclear whether it is a signal sent to the potential donor, or merely an inadvertent cue used by the latter. A "signal" implies a behaviour that has been selected for the purpose of communication, i.e., to transmit information, whereas a "cue" corresponds to any feature or trait that can be used by others as a guide to future action (Maynard Smith and Harper, 2003).

Here we aimed to clarify whether the reaching behaviour of Norway rats corresponds to a signal or a cue. We studied female wild-type Norway rats in a reciprocal food-exchange task that was modified from a design used by Rutte and Taborsky (2008). Norway rats are highly social animals (Barnett, 1963; Schweinfurth, 2020) that apply the decision rules of both generalised and direct reciprocity (Rutte and Taborsky, 2007, 2008; Schneeberger et al., 2012; Dolivo and Taborsky, 2015a; Wood et al., 2016; Schweinfurth and Taborsky, 2017, 2018a,b; Delmas et al., 2019; Kettler et al., 2021). Rats have been shown to communicate using vocal (Brudzynski, 2013 for review) and olfactory signals (Gheusi et al., 1997; Moyaho et al., 2015). In the context of reciprocal cooperation, recent studies revealed that rats transfer olfactory information about both their helping behaviour (Gerber et al., 2020) and their current need for help (Schneeberger et al., 2020). Rats were also shown to use visual cues to evaluate challenging tasks (Schneeberger et al., 2012), but the use of visual communication among social partners is currently unclear (Prusky et al., 2000; Dolivo and Taborsky, 2015b).

To distinguish whether reaching out for a food item that cannot be obtained without help from a conspecific is a signal to this social partner, or merely a cue that the latter can use, we experimentally manipulated both the presence of food and the presence of a partner. We measured the number, timing and duration of reaching behaviours of food deprived Norway rats in a setup where either a desired food item that could not be obtained alone, a social partner (potential helper), or both were present in a familiar reciprocal food exchange task (Rutte and Taborsky, 2008), in which one rat can provide food for another, but not for itself. We predicted that if the main purpose of reaching behaviour is to signal a desire for help to a partner, it would be displayed more often or sooner when both food and a partner are present compared to when one of those factors were missing. If the purpose of the behaviour is primarily to acquire the food without assistance, which could also be used as a cue by a partner, we would expect the reaching behaviour to be correlated with the presence of food, regardless of the presence of a potential helper. Finally, if reaching behaviour were a general appeal for support, and not for a particular item, it should be more common in the presence of a partner regardless of the presence of a desirable food item.

MATERIALS AND METHODS

Subjects

Forty-four female Norway rats (source: Animal Physiology Department, University of Groningen, Netherlands) were kept in nine sister groups of five rats each (one of four). Home cages ($80 \times 50 \times 40$ cm) contained a wooden house, platform and stick as well as a plastic tunnel, an empty toilet roll, hay, and wood shavings for nesting material. In addition to *ad libitum* access to water and food in the form of conventional rat pellets (except when temporary fasting was required for the experiment, see below), the rats in each cage received fresh food (fruits and vegetables) twice a week and a seed mix four times a week. As rats are nocturnal we employed an inversed 12:12 h light:dark cycle with lights off at 08:00 h to allow us to work during their active period. Artificial red lights were used to enable the observation

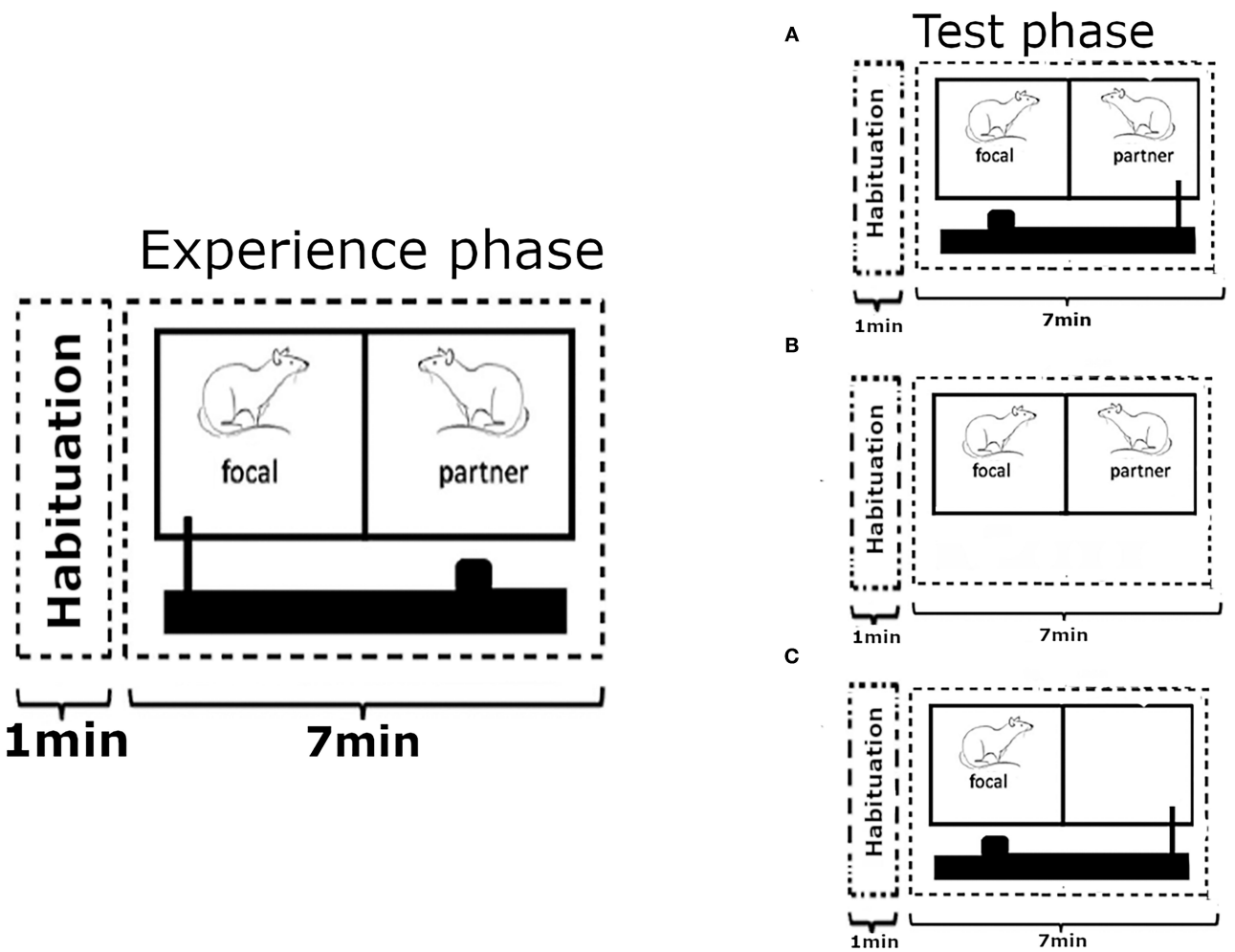

FIGURE 1 | Outline of the experimental design. Each test phase was preceded by an experience phase where the focal animal could provide food to its partner an undetermined number of times, over 7 mins, by pulling a baited tray with a wooden stick towards the cage. On the following day, after 18 h of food deprivation, the focal animal was returned into the experimental cage where the roles were reversed and **(A)** the partner rat could now provide food to the focal subject. In the first control condition **(B)** the partner from the previous day was present in the neighbouring cage compartment, but no moveable tray with food was present. In the second control condition **(C)** the focal rat was on the receiving side of the food tray like in condition **(A)**, but no partner was present to move it. Each focal animal went through all three experimental treatments in a random order, each time preceded by the experience phase with a new partner on the previous day. Figure modelled on comparable depiction in Schweinfurth and Taborsky (2018a).

of the rats during dark hours as they possess a low sensitivity towards red light (Jacobs et al., 2001).

Pre-experimental Training

All rats were taught to pull a stick for receiving a food reward via a moveable tray following an established protocol (Dolivo and Taborsky, 2015b). As the stick was pulled, a tray containing an oat moved into the cage of the pulling rat. After eight training sessions each lasting 7 mins, 43 rats had learned to perform this task successfully. We used eight successful pulls in one training session as the learning criterion.

In the next training phase, each rat was assigned a partner from their home cage for dyadic training. In this training period no rat ever acquired food for itself by pulling the stick, but it was instead providing food to its partner placed in a neighbouring cage compartment. Over the course of 7 mins the rats took turns first pulling once before the stick was switched to the partner that could then reciprocate the donation, after which the stick was moved back to the first rat. Gradually the number of pulls required before a rat experienced reciprocation was increased. Subsequently a time delay was introduced between reciprocation periods, which was stepwise increased to 24 h. In between training sessions the test rats were returned to their home cages. After 18 training sessions 40 rats had learned to perform this task successfully.

The Moveable Tray

The tray consisted of a PVC sheet mounted to rails with ball bearings allowing it to be moved with minimal resistance. On

opposite ends of the front of the tray, two wells were placed to hold food items, which prevented the food from sliding when the tray was pulled. On the outer side of each well, a small plastic tube was attached to act as an anchor point for a stick that could be pulled to move the tray (see sketch in **Figure 1**). A Raspberry Pi 3B computer in combination with a small limit switch attached to the base of the platform was used to record the exact time at which the tray had been pulled to the maximum extended position where the food could be reached by the receiver. Following a 10 s delay, a servo arm controlled via a remotely powered 16-channel, 12-bit PWM Fm+ I2C-bus LED controller (PCA9685) moved the tray back and held it in a locked position for 2 s, to allow a new food item to be placed on the tray by the experimenter. Then the tray could be moved again by the experimental subjects. At the end of each trial, the servo arm moved the tray back to the locked position to mark the end of the observation and prevent further pulling.

Test Procedure

The experimental design followed the procedure of Schweinfurth and Taborsky (2018a), where rats were enabled to reciprocate food donations to a previously helpful partner that was now food-deprived. Each experimental treatment started with an experience phase during which a focal animal ($N = 25$) was paired up with an unrelated and unfamiliar individual, to avoid confounding effects of relatedness and previous social interactions. The focal rat could then provide the partner with food by pulling a stick connected to the moveable tray, over the course of 7 mins (**Figure 1**). Thereafter, both rats were returned to their respective home cages and the food was removed from the cage of the focal rat to increase the likelihood of reaching behaviour in the subsequent test phase (Schweinfurth and Taborsky, 2018a). The order at which rats from different cages were tested was randomised, as was the order of focal animals from within each cage. At no time was a partner rat food-deprived as part of the test procedure, and partner rats that shared their home cage with a focal rat were always given a minimum of 36 h of free access to food prior to being used.

Eighteen hours after the removal of the food from the home cage of the focal animal it was returned to the test cage to undergo one of three treatments for 7 mins. In the "food present" treatment the focal animal was put on the receiving side of the moveable tray, unlike in the experience phase where it had played the part of the provider, and no partner was present to move the tray to fetch the food for the focal rat. In the "partner present" treatment, the partner from the previous experience phase was present in the adjacent cage compartment, but no moveable tray with food was presented. In the "food and partner present" treatment, both a moveable tray with food and the partner from the previous experience phase to operate it were present (**Figure 1**). The same partner rat was never used for more than one treatment to retain the unfamiliarity status, and the position of the focal rat within the test cage was randomised to avoid potential side bias. Each focal rat was tested once for each treatment in a random order. The experiment extended over 3 weeks, and each focal animal was used for testing only once per week. Experience phases took place always on a Tuesday or Thursday, leaving 4–6 days between a test phase and the experience phase of the next treatment for each focal rat.

Behavioural Data

Each trial was video-recorded using a handycam with night vision-mode (Sony HDR-CX550). From these recordings the numbers, beginnings and ends of all reaching behaviours were scored at a 0.2 s resolution. The total number of food items donated in both experience and test phases were recorded automatically by the Raspberry Pi 3B computer. The rats would pull the stick either with their teeth (more often), or with their forelimbs (rarer), and we considered a rat to be reaching when either the mouth or forelimbs were being held outside the cage through the gap in the cage bars where the food tray would enter (estimated maximum distance reached: 1 cm for mouth, 4 cm for forelimbs). These behaviours were easily identifiable with recordings taking place from a mostly top-down view, allowing the bars of the cage to act as a line that, if crossed by forelimbs or nose, was interpreted as reaching. Any pause in reaching longer than 0.5 s was considered to mark the end of that reaching bout. All video recordings were analysed by the same person (NP). Ten videos were re-analysed to assess intra-observer consistency, and found no difference in the number of reaching behaviours observed, and agreement in the duration of 92% (44/48) observed reaching behaviours. Additionally, a bat detector (Pettersson 1000X) was used to record all ultrasonic vocalisations by the focal rats during testing to be used in a concomitant study.

Statistical Analyses

All statistical analyses were performed using R (Version 4.0.2. R Core Team, 2020; packages "lme4," "lmtest," "MASS," "survival," "outliers"), applying a significance criterion of $p < 0.05$ as standard.

To test whether reaching behaviour differed between treatments with or without food and/or a partner, we analysed (i) number of reaching events using general linear mixed models (GLMM) assuming a negative binomial distribution, and (ii) total duration of reaching using a GLMM assuming a gamma distribution with a log-link function. Our initial models included the following fixed effects: treatment (levels: Food present, Food and Partner present, Partner present; using Food present as the control treatment), number of stick pullings performed by the focal rat in the experience phase (range: 0–15), and number of stick pullings performed by the partner rat in the test phase (confined to the "food and partner present" treatment; range: 0–17). As each focal rat was used multiple times, the ID of the focal animals grouped by housing cage was included as random factor. Partner ID was included as a random factor in the analysis of reaching duration, but not of the number of reaching events due to low variance (variance: 1.8×10^{-14}, SE: 1.039×10^{-7}). The full models were tested against null models using only intercept and random effects with a log-likelihood test to validate that key factors improved model fit. Using the drop1 function from the "lme4" package the number of pulls in the test phase was dropped from both models as this improved the AIC by at

least two. A Grubbs-test from the "outliers" package was used to test for statistical outliers.

To test for treatment effects on the latency until the first reaching behaviour was shown by the focal rats we utilised a Cox proportional hazard model (CPHM; function "coxph"). We estimated the model-predicted survival probability using focal animal ID as frailty random effect assuming a gamma distribution following, (Landes et al., 2020).

As rats with access to the pulling stick would occasionally move the tray only part of the way required for the food item to be reached by their partner, the latter were sometimes able to complete the movement of the tray by reaching out and grabbing it. In 6 out of 25 test trials of the "food and partner present" treatment this occurred before the first reaching behaviour had been shown. These six observations were not considered for the analysis of latencies to first reaching behaviour ($N = 19$), because the response of the receiver could not be unequivocally interpreted.

RESULTS

Number of Reaching Behaviours

Rats ($N = 25$) showed more reaching behaviours when a partner capable of providing food was present than when none was present (i.e., Food treatment; GLMM: ß = 0.887 ± 0.164 SE, $p \leq 0.001$), but not in the presence of only a partner without food that it could have fetched for the focal subject (GLMM: ß = 0.083 ± 0.170 SE, $p = 0.624$; **Figure 2A**; **Table 1**). Additionally, the number of reaching events was significantly higher in focal rats that had pulled more often for their partner in the experience phase (ß = 0.064 ± 0.026 SE, $p = 0.016$; **Table 1**; **Figure 3**).

Total Duration of Reaching

One observation from the food-only treatment was considered an outlier by the Grubbs test ($p < 0.001$) and was not considered for the observation ($N = 24$). As with the number of reaching behaviours, the total duration of reaching by focal rats in the presence of a partner and food was significantly longer than when no partner was present (i.e., Food treatment; GLMM: ß = 0.729 ± 0.207 SE, $p < 0.001$; **Figure 2B**; **Table 1B**), but this was not the case when only a partner was present without food it could have fetched for the focal subject (GLMM: ß = −0.068 ± 0.210 SE, $p = 0.746$). The number of food donations performed by the focal animal in the experience phase showed a non-significant trend to correlate positively with the total amount of reaching in the test (GLMM: ß = 0.070 ± 0.038, $p = 0.062$).

Latency to First Reaching Behaviour

Kaplan-Meier conditional probabilities estimated the mean time of all first reaching behaviours ($N = 19$) at 27 s from start of the experiment. The distribution of censored and uncensored data was deemed acceptable for continued analysis, which revealed that the latency to the first reaching behaviour changed significantly with the presence of both food and a partner (CPHM: ß = −0.898 ± 0.356 SE, $p = 0.012$; HR = 0.2.454; 95% CI of HR = 1.222–4.925), but not with partner alone (CPHM: ß = −0.114 ± 0.297 SE, $p = 0.701$; HR = 0.892; 95% CI of HR = 0.499–1.597) when compared to the control treatment with only food present.

DISCUSSION

In this study we investigated whether reaching out for inaccessible food corresponds to a signal of need for help in wild-type Norway rats. In accordance with the hypothesis that reaching is an intentional signal serving to elicit help by a social partner, we found a significant increase in both the number of reaching behaviours and the total duration for which this behaviour was shown, as well as a decrease in the latency to its first occurrence, when both food and a partner were present compared with a situation where either was missing. Our data do not support the two alternative hypotheses we tested, namely that reaching corresponds to a general signal for help, or that it reflects merely a self-serving attempt to reach the inaccessible desideratum. This is all the more remarkable because in this experimental test, which followed a phase in which the rats had supplied a partner with food, they experienced a situation for the first time in which either a partner to pull food for them, or food to be fetched, were missing. Regardless, the latency to start reaching was shorter when both food and partner were present compared to when there was no partner available to provide food, further substantiating that rats alter their reaching behaviours depending on whether or not a partner is present to provide help.

In addition to the clear effect of the presence of food and a partner, reaching was also shown significantly more often by rats that had performed a higher number of food donations to their partner in the preceding experience phase. This suggests some contingency regarding the propensity of a rat to help a partner and its expectancy of a restitution. Norway rats have indeed been shown to return more help to previously more helpful individuals in a similar food provisioning task (Kettler et al., 2021), and to modify their help also in response to the quality of help they received (Dolivo and Taborsky, 2015b). Rats were shown to switch between alternative roles also in other turn-taking games (Reinhold et al., 2019), and neurological evidence suggests that rats possess rudimentary capabilities to predict forthcoming events (Seamans et al., 2008). It seems possible, therefore, that rats providing more help to a partner in a turn-taking game have a higher expectation of a socially mediated return benefit in the subsequent phase, similar to anticipation effects as known from conditioned tasks (Bolles and Moot, 1973).

Norway rats are nocturnal animals that obviously rely less on visual stimuli than diurnal species, and previous studies have shown that rats make use particularly of auditory (Blanchard et al., 1991; Brudzynski and Ociepa, 1992; Brudzynski, 2005; Pereira et al., 2012) and olfactory (Brown, 1979; Gheusi et al., 1997; Moyaho et al., 2015; Schneeberger et al., 2020) information from conspecifics. In an experimental setup similar to ours, visual cues have indeed turned out to be of little importance for successfully performing reciprocal food exchanges (Dolivo

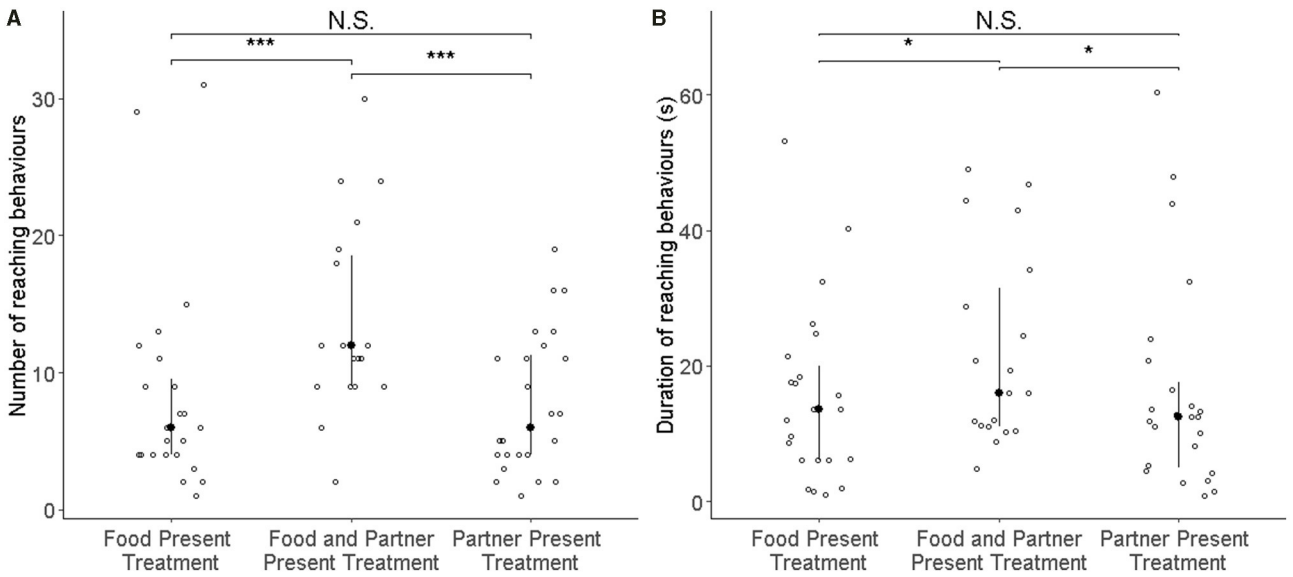

FIGURE 2 | Number of reaching events **(A)** and the total duration of all reaching behaviours **(B)** of all focal rats ($N = 25$) in the test phase. Open circles represent individual data points, filled circles the median value, and whiskers the interquartile ranges. In the three treatments, either food or the partner were absent (left and right columns), or both were present (middle). Both the number of reaching events and their total duration increased significantly in the presence of both food and partner relative to the two control conditions as denoted by asterisks (***$p < 0.001$, *$p < 0.05$).

TABLE 1 | Effects of experimental treatment and previous helpful acts on (A) the number and (B) duration of reaching events, using GLMMs with a negative binomial or gamma distribution (log-link), respectively.

(A)	Number of reaching events	Estimate	SE	Z	*p*
	Intercept	1.587	0.207	7.666	**<0.001**
	Food and partner treatment	0.887	0.164	5.391	**<0.001**
	Partner treatment	0.083	0.170	0.490	0.624
	Pulls in experience phase	0.064	0.026	2.419	**0.016**
(B)	**Total duration of reaching**	**Estimate**	**SE**	***t***	***p***
	Intercept	2.206	0.282	7.832	**<0.001**
	Food and partner treatment	0.729	0.207	3.513	**<0.001**
	Partner treatment	−0.068	0.210	−0.324	0.746
	Pulls in experience phase	0.070	0.038	1.867	0.062

Both models used Focal ID [model (A) N = 25; model (B) N = 24] as a random factor, with (B) also using Partner ID as a second random factor. All treatments are in comparison to the "food present" treatment. Significant p-values are marked in bold, non-significant trends are underlined.

and Taborsky, 2015b). So it seems puzzling that reaching out towards something, which appears to be primarily a visual signal, elicits a helpful response in a receiver of such signal, as has been demonstrated in a previous study (Schweinfurth and Taborsky, 2018a). In general, signals are considered to be mechanically ineffective behaviours, unable to accomplish the desired goal (e.g., Pika and Bugnyar, 2011), but this does not mean that mechanically effective behaviours cannot be used as signals in a different context. In our case, a food fetching behaviour is shown by Norway rats in a situation where only a social partner can provide food to them, i.e., where the behaviour is mechanically ineffective, and apparently they use this behaviour mainly when both food and partner are available. Whether the visual component of this behaviour is indeed recognised by the signal receivers, or its inevitable or intended correlates in another sensory modality, poses an interesting question for future studies. In the context of food provisioning to social partners, Norway rats have been shown to respond to the odour produced by a cooperating conspecific (Gerber et al., 2020), and they adjust their helpfulness to the hunger state of social partners merely based on olfactory information (Schneeberger et al., 2020). Therefore, it seems possible that the reaching behaviour shown in this study may also coincide with the emission of odour that can be more easily detected by signal receivers than the visual feature. In fact, a combination of cues of need by a partner could be used to pinpoint who is signalling for help in a large colony where movement of air and individuals may make it difficult to locate the exact origin of a particular scent. The production of acoustical signals concurrently with the reaching behaviour might be another

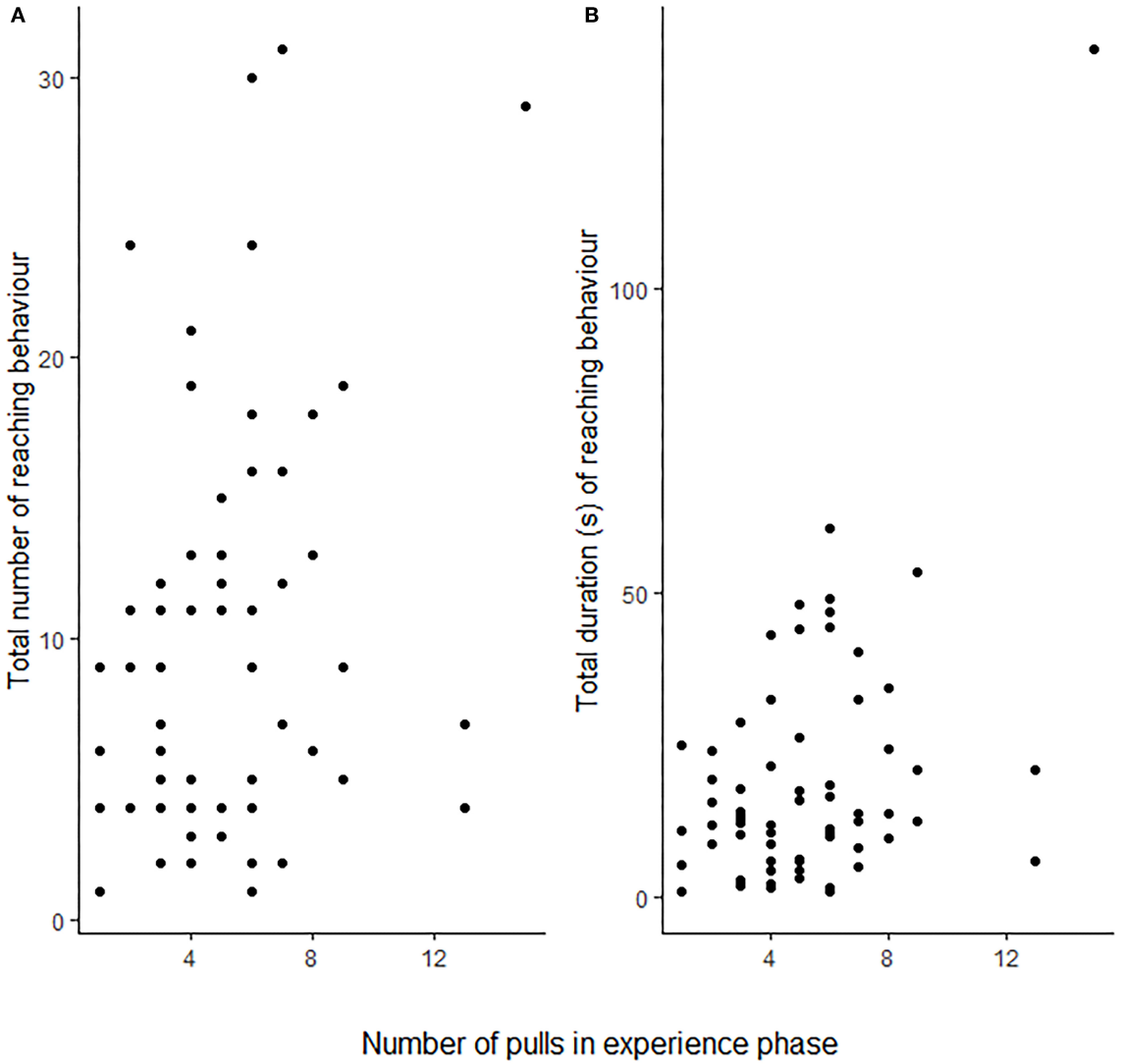

FIGURE 3 | The focal rats' ($N = 25$) total number of reaching events **(A)** and total amount of time spent reaching **(B)** in the test phase in relation to the number of food provisionings the same focal animal performed during the test phase. Each point corresponds to a single individual. For statistical results see **Table 1**.

possibility, and the reaching behaviour itself may be detectable also by auditory means, which would provide alternative ways of signal transmission.

In conclusion, our study provides evidence that Norway rats enhance reaching behaviour in the presence of a partner and food the latter can deliver to them, as expected if it is used as a signal a need for help to social partners. In connection with a previous study showing that this behaviour indeed triggers help in a receiver of the signal (Schweinfurth and Taborsky, 2018a), reaching out for an inaccessible item seems to be part of the communication system of these highly social animals. Future studies should unveil which sensory modality involved in this signal conveys the most critical information. Furthermore, our data revealed that there is a quantitative contingency between the helpfulness of a rat and the number of reaching behaviours shown, which might suggest an expectation of return benefits. This is in accord with previous results showing the inverse relation: that rats accredit more to social partners that have provided more or better service to them before (Dolivo and Taborsky, 2015b; Kettler et al., 2021).

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article are available in the **Supplementary Material**.

ETHICS STATEMENT

The animal study was reviewed and approved by Veterinary Office of the Canton of Bern (Licence Number BE55/18).

AUTHOR CONTRIBUTIONS

NIP and MT designed the study. NIP performed the experiments and analysed the data. NIP and MT both contributed to the design of the data analysis and jointly wrote the manuscript. All authors contributed to the article and approved the submitted version.

FUNDING

This study was financed by the Swiss National Science Foundation, Project Grant Number 31003A 176174, awarded to MT.

ACKNOWLEDGMENTS

We thank Sacha Engelhardt for statistical advice and general feedback throughout the entire project, Evi Zwygart for her care of the rats, and two reviewers for constructive feedback that helped to improve the manuscript.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2021.712333/full#supplementary-material

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frontiers in Psychology

BRIEF RESEARCH REPORT
published: 16 December 2021
doi: 10.3389/fpsyg.2021.794422

The Effect of the Number and Identification of Recipients on Organ-Donation Decisions

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We examined how presentations of organ donation cases in the media may affect people's decisions about organ donation issues. Specifically, we focused on the combined effect of the information about the number of recipients saved by the organs of one deceased person (one vs. four) and the identifiability of the donor and the recipient(s) in organ donation descriptions, on people's willingness to donate the organs of a deceased relative. Results suggest that reading about more people who were saved by the organs of a deceased donor does not increase willingness to donate. Replicating earlier research, we found that reading about a case of organ donation involving an identified deceased donor, deceased willingness to donate. However, this effect was attenuated when participants read about more recipients who were saved by the donation. Importantly, the presentation that prompted the greatest willingness to donate a deceased relative's organs was the one that featured an unidentified donor and only one identified recipient. Finally, an explorative investigation into participants' subconscious thoughts of death following the organ donation story revealed that identifying a deceased organ donor prompts more thoughts of death in the perceiver (regardless of the number of recipients).

Keywords: organ donation, willingness to donate, prosocial behavior, identifiable victim effect, scope neglect

OPEN ACCESS

Edited by:
Gustav Tinghög, Linköping University, Sweden

Reviewed by:
Arvid Erlandsson, Linköping University, Sweden
Elmar Nass, Philosophisch-Theologische Hochschule SVD St. Augustin, Germany

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *13 October 2021*
Accepted: *18 November 2021*
Published: *16 December 2021*

Citation:
Harel I and Kogut T (2021) The Effect of the Number and Identification of Recipients on Organ-Donation Decisions. Front. Psychol. 12:794422. doi: 10.3389/fpsyg.2021.794422

INTRODUCTION

"One donor can save eight lives!" This phrase is often used in appeals to members of the public to sign a commitment to donate their organs after death, or to donate the organs of a deceased relative. Moreover, we often encounter—in the printed press, online, or in television reports—of cases of organ donations with information about a deceased donor and about one or several recipients whose lives were saved by that donation.

How might these ads and stories affect readers? In a previous study (Harel et al., 2017), we demonstrated that when participants read about organ donation cases that include identifying information (a name and a photo) about the recipient whose life was saved as a result, it increased their willingness to commit to organ donation themselves, and their willingness to donate (WTD) the organs of a deceased relative. Conversely, identifying the deceased donor was found to induce

thoughts of death rather than about saving lives—resulting in fewer participants willing to donate organs (Harel et al., 2017). A study of online news found that in the coverage of organ donation cases in real life, identification of the donor is significantly more common than identification of the recipient—with possibly adverse effects on the incidence of organ donations (Harel et al., 2017).

In the present research, we take one step further in investigating the impact of the presentation of organ donation cases in the media on people's WTD organs, by examining the role played by the number of recipients saved by the organs of one deceased person, and whether learning about more recipients who were saved as a result reduces thoughts of death, thereby increasing support for organ donation. In addition, we sought to examine the combined effect of the number of recipients saved by the organs of one deceased person and the identifiability of the donor and the recipient[s] in people's decisions about organ donation issues. Answering these two questions has the potential to make both a theoretical and a practical contribution. First, this investigation will help in understanding the role played by the number of people saved by organ donation, in organ donation decisions (specifically, whether or not *scope neglect* occurs in this context), and to learn about the underlying mechanism (namely, thoughts about death). From the practical standpoint, it will help in identifying the best way to present the issue of organ donations in the media, in a manner that encourages people's willingness to donate organs.

Research of charitable giving indicates that donation-giving is more likely to be triggered when recipients are identified by name, photograph, or story, than when they are anonymous or merely statistical individuals, even when the identification conveys no meaningful information (Jenni and Loewenstein, 1997; Small and Loewenstein, 2003; Small et al., 2007). When the needs of an identifiable individual are presented, emotional responses (e.g., empathy and compassion) immediately come into play, which increase the incidence of helping. However, when needy individuals are perceived in a negative light—such as when they are perceived responsible for their plight (Kogut, 2011)—identifying information about them may actually increase feelings of anger and blame toward them, reducing willingness to help.

Research on the identified victim effect suggests, however, that identifiability of the recipient increases donations mainly when it involves a single identified individual (Kogut and Ritov, 2005a,b)—and less so when a group of several individuals is presented. As a result, a single identified victim elicits more donations than a group of several victims (whether they are identified or not). Indeed, such is the impact of the number of victims on the willingness to help that it drops dramatically when the number of victims increases even from one to two (Slovic, 2007; Dickert et al., 2015). This *singularity effect*—the preference to help a single identified victim over a group of victims (Kogut and Ritov, 2005a,b) is in line with research of recent decades that consistently shows that people are insensitive to the magnitude of the impact of their support of public causes and of moral decisions (e.g., Desvousges et al., 1993; Kahneman and Ritov, 1994; Baron, 1997; Frederick and Fischhoff, 1998). Hsee and Rottenstreich's (2004) research suggest that peoples' subjective values are highly sensitive to the presence or absence of a stimulus (i.e., a change from zero to some number), but they are largely insensitive to further variations in scope, especially when affect-rich stimuli (such as identified victims) are involved. Furthermore, large numbers of victims become dry statistics that fail to spark emotion and feelings, and thus fail to motivate actions (Slovic, 2007). However, it is important to note that some studies have failed to replicate the effect (e.g., Lesner and Rasmussen, 2014; Hart et al., 2018). Moreover, the effect may be restricted to individualistic cultures and societies, and may even reverse in collectivist ones (Kogut et al., 2015; Wang et al., 2015). Furthermore, the effect occurs only in a separate evaluation mode, when prospective donors contemplate helping a single identified recipient or a group of recipients, and are unaware of the alternative condition. In a joint evaluation mode—i.e., when people directly compare the needs of the single individual with those of the group, or when they are asked to choose between them—the decision becomes more rational, and the effect tends to reverse (Kogut and Ritov, 2005b; Wiss et al., 2015; Erlandsson, 2021). Finally, manipulations to increase rational thinking (versus intuitive or emotional thinking modes) and to enhance self-efficacy, attenuated the effect (Small et al., 2007; Sharma and Morwitz, 2016), highlighting the emotional origins of the preference to help single identified individuals.

As previously noted, the presentation of a victim in need of help may be fundamentally different from the presentation of prospective donors and recipients of organ donations. When people donate money to help an identified victim, they believe that their donation will directly help that specific individual—whereas, with organ donations, the commitment to help is directed at an unknown future recipient, and in the unfortunate event of their own death (or that of a close family member). Thus, when a specific case of a prospective organ donation recipient is presented, it can only be by way of illustration, rather than as an actual request for help (Harel et al., 2017).

Moreover, when people consider the issue of organ donations, they are confronted with the disturbing thought of their own demise, or that of a relative. According to terror management theory (e.g., Greenberg et al., 1997), prosocial action helps to suppress anxiety-inducing thoughts of death. Thus, people may act prosocially to shield themselves from the looming prospect of their own mortality—inasmuch that, by helping others, they feel more valuable, and the world seems more meaningful (Jonas et al., 2002). However, Hirschberger et al. (2008) found that, when an appeal for help makes the prospect of one's own death all the more salient, people may react by setting it aside, and avoiding appeals to help altogether. For example, in one of their studies, mortality-salience manipulation increased charitable donations, but decreased organ donor card registrations (compared with a control condition).

To the best of our knowledge, Harel et al.'s (2017) study is the first to use identified prospective recipients to illustrate an issue (i.e., as individuals who have been saved by organ transplants that had already taken place), rather than as the actual beneficiaries of the decision to donate. In addition, to date, this is the only study that has examined the *identifiability*

effect in the context of organ-donation decisions. However, in that study, the recipient was always a single individual, and the donated organ was always a kidney. The research on scope insensitivity and on the singularity effect of identified victims, as reviewed above, raises the question of whether presenting more than one individual who has been saved by organ donations would boost support for organ donations among the public.

This question is important from a theoretical perspective, since while stories about several individuals being saved by the donation of organs of a deceased person may boost organ donations—by prompting thoughts about the lives being saved (rather than about death) (Harel et al., 2017)—they may also reduce WTD due to the natural human tendency to scope insensitivity and the difficulty to adopt the perspective of several other individuals (as opposed to one individual—Slovic, 2007).

In light of recent appeals for organ donations that highlight the fact that one dead person can save the lives of nine people, it is also important to examine this strategy from a practical perspective.

In the present research, we sought to examine the combined impact of the identifiability of the donor and the recipient, and their number (one vs. four recipients) on organ-donation decisions. To this end, we chose to focus on the decision to donate the organs of a deceased close relative (rather than one's own), since it covers all prospective donors, including those who are willing or have already committed to donate their own organs after death.

In light of the findings of Harel et al. (2017), we expected vivid identifying information about the donor (a deceased individual who has donated his or her organs) to reduce participants' WTD organs, since such details about deceased donors has been found to prompt thoughts of death (rather than saving lives), decreasing WTD. However, we expected that telling participants that four (rather than one) organ recipients were saved by the donation of organs of a deceased person would attenuate this effect, as it may prompt thoughts about saving lives.

When the deceased donor is left unidentified, we expected identifying information about only one prospective recipient to prompt greater support for organ donations, especially when only one such recipient is presented—in line with the research on the singularity effect, which states that people are more likely to sympathize with, and tend to take the perspective of, a single identified victim, than when a group of such victims with the same need are involved.

To examine these predictions, we used the study design used by Harel et al. (2017), whereby participants read about a recent case of a young man who had been killed in a car accident and whose organs saved the life of another young man. In Study 1, we included eight between-subject conditions in a 2 × 2 × 2 design, varying the identifiability of the donor (identified vs. unidentified), the identifiability of the recipient (identified vs. unidentified), and the number of recipients saved by the organ donation (one vs. four). After reading the story, participants were asked if they were WTD the organs of a deceased family member. In Study 2, we used the same basic description to examine whether reading about more recipients whose lives had been saved by the donation of organs of one deceased donor prompted thoughts of saving lives rather than of death, by examined participants' subconscious thoughts of death, using a word-completion task.

STUDY 1

Method

To determine the number of participants to recruit for the study, we conducted a power analysis by means of the *G*Power* computer application (Erdfelder et al., 1996). This indicated that a sample of approximately 300 people would be sufficient to detect a small-to-medium effect size (f = 0.15), with a power of 80%. Accordingly, we recruited 304 undergraduate students at Ben-Gurion University (72% female, mean age = 24.39 y, SD = 3.30), through an online subject pool in exchange for monetary prizes—to complete a short survey online. Participants were randomly assigned to one of eight experimental conditions, in a 2 × 2 × 2 design of Donor's Identification (identified vs. unidentified), Recipient's Identification (identified vs. unidentified), and the Number of Recipients (*1* vs. *4*), as explained below.

Participants first read a story (adopted from Harel et al., 2017) about a young man who had been killed in a car accident the previous week. He was a registered organ donor, so his parents decided to donate his organs. His kidney [heart, pancreas, two kidneys] was [were] transplanted into the body of another young man [four young men], whose life was [lives were] saved as a result. In the Identified Donor condition, the name and picture of the deceased donor were presented; in the Identified Recipient[s] condition, the same name[s] and picture[s] were attributed to the organ recipient[s]. We used five different typical photos of young men in their twenties to identify the donor and the recipients, while randomly varying the photos in the Identified Donor and the Single Recipient conditions, such that each photo was equally used to identify a single deceased donor and a recipient. In the Four Recipients condition, participants were told that four different organs (from the same deceased donor) were donated to four different recipients: two kidneys, a heart and a pancreas. In the One Recipient condition, we varied the donated organ between-subjects accordingly, such that 1/4 of the participants read about a heart donation, 1/4 about a pancreas donation, and 2/4 about a kidney donation. To enhance involvement, subjects were also asked to indicate whether they had heard about this case (*Yes*/*No*).

Next, participants were asked to imagine that a close relative of theirs had just died, and that the hospital's medical staff were asking their family to consider donating his organs to save the life of someone waiting for transplantation. Participants were then asked to rate their WTD their deceased relative's organs on a seven-category scale ranging from *1 (Strongly disagree)* to *7 (Definitely agree)*.

Finally, they were asked to provide demographic information about themselves, including ratings of their degree of religiosity, a variable found in previous studies to be related to willingness to donate organs (1-secualr; 2- traditional; 3- religious; and 4-

ultraorthodox) and to indicate whether they themselves were registered organ donors (*Yes/No*).

Results

Willingness to donate the relative's organs did not significantly differ under the different organ conditions used in the Single Recipient condition (kidney, heart and pancreas; $p = 0.80$), nor under the different photos used to identify the donor and the recipient ($p = 0.85$). We therefore analyzed the Single Recipient condition beyond the different organs and photos.

One hundred and ninety-seven participants reported being registered donors, while 107 were not. Since the participant's own commitment to organ donations (i.e., whether he/she is a registered donor, or not) was found to play a significant role in the decision about donating the organs of a deceased relative, and may interact with the different presentations (Harel et al., 2017), we used the participant's consent status (as registered donor or not) as a covariant in the analysis. A 2 × 2 × 2 ANOVA on the WTD the organs of a deceased relative (hereafter, WTD) was conducted, with Donor's Identification, Recipient's Identification, and Number of Recipients as predictors.

Results revealed a significant main effect for consent status—such that, as expected, registered donors expressed greater WTD the organs of a deceased relative ($M = 6.29$, SD = 1.05) than unregistered people ($M = 4.54$, SD = 1.62), $F(1, 295) = 135.93$, $p < 0.001$, $\eta_p^2 = 0.31$. No other significant main effects were found.

The interaction between Donor's Identification and Number of Recipients was significant $F(1, 295) = 5.77$, $p = 0.017$, $\eta_p^2 = 0.02$. As illustrated in **Figure 1**, replicating the results of Harel et al. (2017), simple effect analysis shows that when only one recipient was presented, participants who were told about an identified deceased donor ($M = 5.53$, $SD = 1.67$) were less willing to donate the organs of a deceased relative than those whose account talked about an unidentified donor ($M = 5.94$, $SD = 1.44$); $F(1, 295) = 6.09$, $p = 0.014$, $\eta_p^2 = 0.02$. However, when four recipients were saved by the organs of the one deceased donor, identifiability of the donor had no significant effect on willingness to donate—$F(1, 295) = 0.85$, $p = 0.36$, $\eta_p^2 = 0.003$. This suggests that knowing about several people who were saved by the organs of a single dead donor attenuates the effect of Donor's Identification in reducing support for organ donations. However, reading about four people who were saved by the organ donation did not have a significant effect in increasing WTD.

The interaction between Recipient's Identification and Number of Recipients was also significant $F(1, 295) = 5.99$, $p = 0.015$, $\eta_p^2 = 0.02$. As evident in **Figure 2**, in the Identified condition, one recipient encouraged greater WTD (M = 5.86, SD = 1.37) than four recipients (M = 5.38, SD = 1.67), $F(1, 295) = 5.93$, $p = 0.015$, $\eta_p^2 = 0.02$; while in the Unidentified condition no significant difference was found between one recipient and four recipients, $F(1, 295) = 0.96$, $p = 0.33$, $\eta_p^2 = 0.003$. This result is in line with previous research on the singularity effect in charitable giving, which suggests that a single identified recipient prompts a greater WTD than a group of recipients. Another way to look at the interaction is to examine the effect of identifiability of a single target and that of a group of four on WTD. A simple effect analysis reveals that identifying four recipients, actually decreased WTD ($M = 5.38$, SD = 1.67), compared to four unidentified recipients (5.89, SD = 1.72),

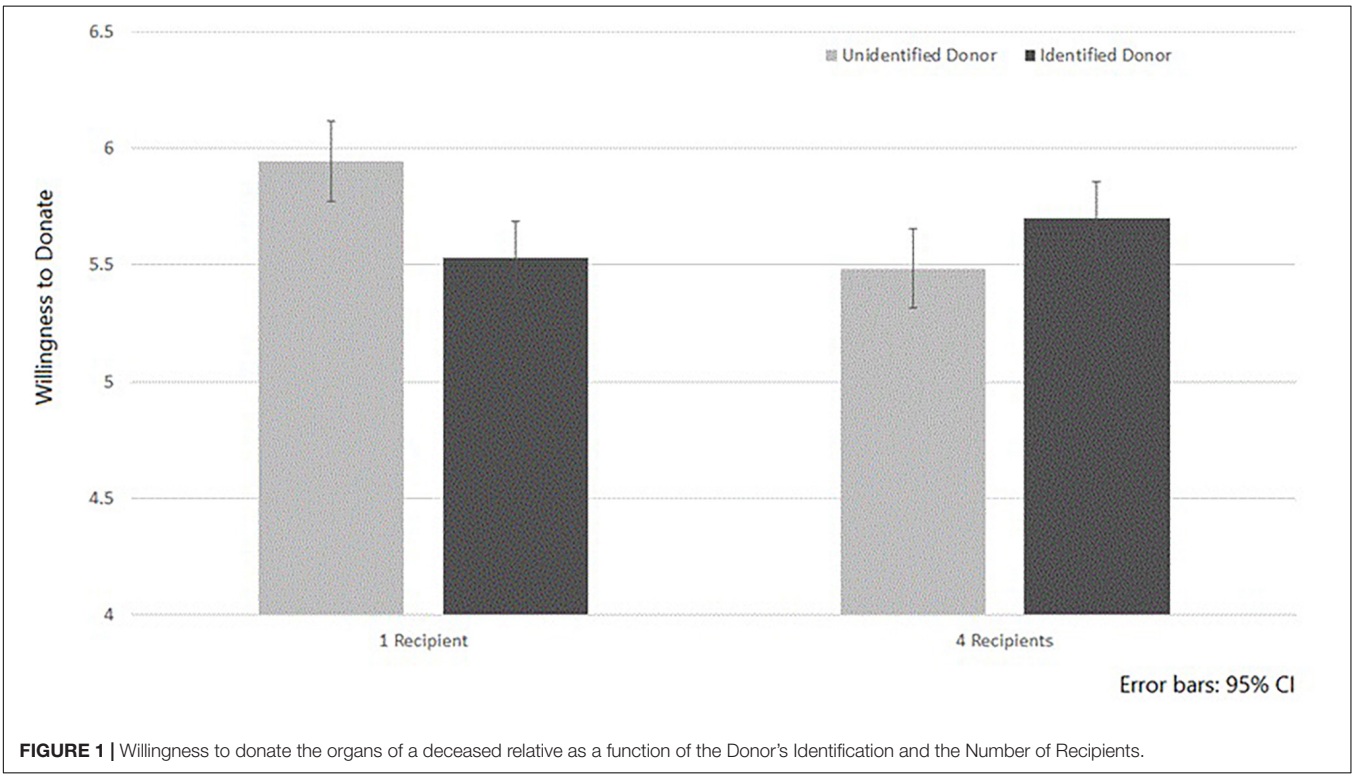

FIGURE 1 | Willingness to donate the organs of a deceased relative as a function of the Donor's Identification and the Number of Recipients.

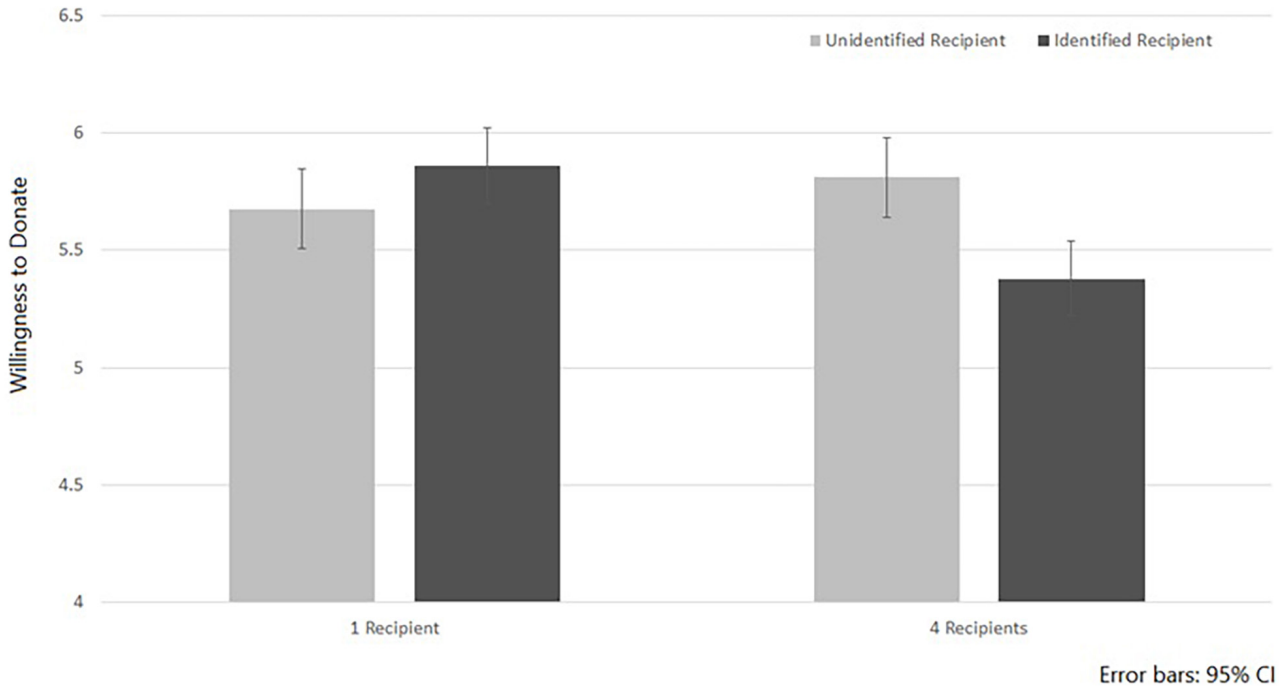

FIGURE 2 | Willingness to donate the organs of a deceased relative as a function of the Recipient's Identification and the Number of Recipients.

$F(1, 295) = 7.23$, $p = 0.008$, $\eta_p^2 = 0.024$. However, the role of the recipient's identifiability was far from significance when only one recipient was presented ($F(1, 295) = 0.47$, $p = 0.49$, $\eta_p^2 = 0.002$). This finding is interesting, since it highlights the notion that identifiability may have a negative effect on WTD when several targets are presented (rather than only one). It is possible that providing too much information about several people and various transplanted organs increases stress among the perceivers, distancing them from the situation (e.g., Cameron and Payne, 2011). Alternatively, it might be that thinking about four recipients (rather than one), increased a "calculative mode of thinking" among the participants, which increased their sensitivity to scope (Small et al., 2007; Erlandsson et al., 2016).

Finally, the three-way interaction between donor's identifiability, recipients' identifiability and the number of recipients approached significance $F(1, 295) = 3.23$, $p = 0.069$, $\eta_p^2 = 0.011$. As illustrated in **Figure 3**, this interaction suggests that when only one recipient is presented, Donor's Identification is the only significant predictor for WTD. As found in the study by Harel et al. (2017), when the donor is identified, people are overall less willing to donate the organs of a deceased relative ($M = 5.54$, SD = 1.67) than when the donor is unidentified ($M = 5.95$, SD = 1.44); $F(1,167) = 4.17$, $p = 0.043$, $\eta_p^2 = 0.024$. When four recipients are presented, no significant effects were found, and the main effect of Recipient's Identification approached significance, suggesting that four unidentified recipients encouraged greater WTD ($M = 5.80$, SD = 1.24) than four identified ones ($M = 5.38$, SD = 1.67) $F(1, 132) = 2.75$, $p = 0.099$, $\eta_p^2 = 0.02$. Previous research on the role of the identifiability of a group of recipients in promoting monetary donations found mixed results: in some studies, it had no effect on donations, while in others it decreased them (Kogut and Ritov, 2005a,b). Replicating the ANOVA with participants' ratings of their level of religiosity as a covariate revealed similar results. Specifically, both two-way interactions remained significant, while religiosity ratings were not significant ($F(1, 289) = 2.15$, $p = 0.14$).

Judging by **Figure 3**, the condition that appears to increase WTD (among all eight conditions) is the one in which the deceased donor is not identified, and only one identified recipient is presented. Results of a one-way ANOVA on WTD—with Condition as the independent variable (eight levels), while holding consent-status as a covariant—reveals a significant difference between the eight conditions ($F(1, 295) = 2.46$, $p = 0.018$, $\eta_p^2 = 0.055$. *Post hoc* analysis suggests that participants who were told about an unidentified donor and one identified recipient were significantly more WTD than participants in most of the other conditions, as reported in **Table 1**. No other significant differences in WTD were found between any other two conditions.

One key finding of Study 1 is that being told about four recipients who were saved by the organs of a single deceased donor attenuates the effect of Donor's Identification in reducing the willingness to donate. Since previous research (Harel et al., 2017) suggests that the identifiability of the donor is more likely to prompt thoughts of death in people's minds (as opposed to thoughts about saving lives), resulting in diminished WTD, in Study 2 we sought to explore the degree to which this occurred, and whether being told about more recipients who were saved by the organs of the deceased reduces this tendency.

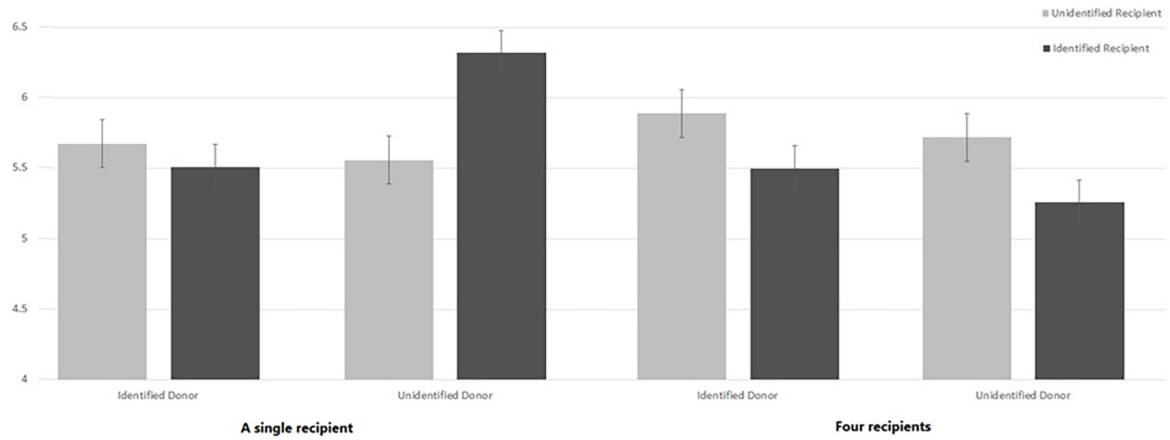

FIGURE 3 | Willingness to donate the organs of a deceased relative (WTD) under the one vs. the four-recipients-conditions, as a function of the Donor's Identification and recipient(s)' identifiability.

TABLE 1 | A comparison between WTD under the unidentified donor and an identified recipient condition, and all other conditions.

		Mean difference	SE	Sig
Unidentified donor and 1 identified recipient	Unidentified donor and 1 unidentified recipient	0.57	0.34	0.090
	Identified donor and 4 unidentified recipients	0.43	0.37	0.245
	Identified donor and 1 unidentified recipient	0.76*	0.36	0.039
	Unidentified donor and 4 unidentified recipients	0.60	0.38	0.116
	Identified donor and 1 identified recipient	0.81*	0.36	0.026
	Unidentified donor and 4 identified recipients	1.06*	0.38	0.005
	Identified donor and 4 identified recipients	0.82*	0.38	0.030

**The mean difference is significant at the 0.05 level.*

STUDY 2

Study 2 was an exploratory attempt to examine the psychological mechanism that may explain the interaction between identification of the donor and the number of recipients, in terms of the participants' WTD, as found in Study 1. As noted, previous research suggests that the identifiability of the donor prompts thoughts of death, rather than about saving lives, resulting in diminished WTD. In Study 1, we found that donor identifiability reduced WTD when only one recipient was saved by the organ donation—but when participants were told that *four* recipients were saved by the organs of the deceased, this effect was attenuated, such that their WTD was not significantly different from that of participants who had been told about an unidentified donor.

In Study 2, we examined the salience of death-related thoughts in people's minds after reading about a case of organ donation. As in Study 1, participants were given a written account about a recent case of a young man who had been killed in a car accident, whose organs were donated to save the lives of others. The study included a 2 × 2 design, manipulating the Donor's Identification (identified vs. unidentified) and the Number of Recipients (one vs. four) in the story. However, in this case, to keep the design simple, the recipients in all conditions were unidentified. We then examined the participants' subconscious thoughts of death after the various descriptions, by means of a word-completion task.

Method

Four hundred and forty undergraduate students at Ben-Gurion University (from a computerized pool of subjects) took part in the study: 63% females, Mage = 26.56, SD = 13.32. Participants were randomly assigned to one of four groups in a 2 × 2 design manipulating the identifiability of the donor (identified vs. unidentified) and the number of organ recipients (one versus four). As in Study 1, participants first read about a young man who had been killed in a car accident, with or without identifying information. They next read that the organ[s] of this man saved the lives of one [four] young men who urgently required them. To examine participants' subconscious death thoughts, we used a word-completion task involving words that could be completed with either neutral or death-related words. This procedure has been used successfully in previous research to examine people's accessibility of various subconscious contents (e.g., Greenberg et al., 1994; Arndt et al., 1997; Mikulincer and Florian, 2000; Kogut and Kogut, 2013). The word-completion task included 13 Hebrew word fragments which participants were instructed to complete with the first word that came to their mind by filling in one or two missing letters. Six of the 13 Hebrew word fragments could be completed with neutral or death-related Hebrew words. The death-related words were the Hebrew words for *death*; *funeral*; *grave*; *body*; *deceased*; *mourning*; and "*Shivah*" (a week-long mourning period in *Judaism)*. The dependent measure was the number of death-related words with which a participant

completed the fragments. Finally, participants provided their demographics including information about whether they are registered donors (yes/no), and religiosity rating (as in Study 1).

In this study we examined accessibility to death-related words after reading about the case of organ donation without assessing WTD, building upon the relationship between thoughts of death and WTD after reading about an identified versus unidentified donor found in previous research (Harel et al., 2017), since several pilot studies (with small samples) revealed that being employed in one of the tasks (completing the connectedness words or making a decision regarding the donation of a deceased relative organs) may distance the participants form the identifiability manipulation, hence weakening its effect on the second task (i.e., only the task that follows the story manipulation is affected by it).

Results

The number of death-related words completed by the participants in condition is presented in **Table 2**. Overall, this number ranged between 0–5, $M = 1.37$, $SD = 1.07$. A two-way ANOVA on the number of death-related words by the two independent variables (identifiability and number of recipients) was conducted. Results reveal a significant main effect for Donor's Identification—$F(1, 436) = 4.17$, $p = 0.04$, $\eta_p^2 = 0.01$—such that reading about an identified deceased donor prompted more thoughts of death among participants ($M = 1.50$, SD = 1.15) than reading the same story with an unidentified donor ($M = 1.25$, SD = 0.95). The Number of Recipients fell far short significance $F(1, 436) = 0.40$, $p = 0.85$, $\eta_p^2 < 0.001$. Although the interaction between identifiability and the Number of Recipients was not significant $F(1, 436) = 0.82$, $p = 0.37$, $\eta_p^2 = 0.002$, in light of the results of Study 1, we looked at the effect of Donor's Identification in each of the Recipient Number conditions separately. Simple-effect analysis revealed that Donor's Identification increased thoughts of death in the One Recipient condition only $F(1, 436) = 4.32$, $p = 0.038$, $\eta_p^2 = 0.01$, while in the Four Recipients condition Donor's Identification had no significant impact on thoughts of death, $F(1, 436) = 0.65$, $p = 0.42$, $\eta_p^2 = 0.001$. Holding Consent Status and level of religiosity constant in the analysis revealed similar results. Specifically, the main effect of Donor's Identification remained significant ($p = 0.049$) while Consent Status ($p = 0.56$) and Religiosity ($p = 0.31$) did not reveal significant results.

TABLE 2 | The number of death-related words completed by the participants in condition (Study 2).

Recipients	Identifiability	Mean	SD
One	Unidentified	1.19	0.95
	Identified	1.49	1.21
	Total	1.36	1.11
Four	Unidentified	1.31	0.96
	Identified	1.42	1.09
	Total	1.37	1.03
Total	Unidentified	1.25	0.95
	Identified	1.46	1.15
	Total	1.37	1.07

DISCUSSION

The results of our investigation of the effect of the presentation of organ donation cases on people's WTD the organs of a deceased relative, replicated those of previous research by showing that when the participants read about a case of organ donation involving an identified deceased donor, their WTD diminished. However, it also yielded innovative findings about the effect of the number of recipients saved by a single deceased donor on people's WTD the organs of a deceased relative. As with monetary donation decisions (e.g., Kogut and Ritov, 2005a,b; Slovic, 2007), we found that in the context of organ-donation decisions people are also insensitive to number of victims saved—insofar as reading about more people who were saved by the organs of a deceased donor does not increase WTD. Moreover, when the organ recipients were identified, reading about one person who was saved by organ donation prompted greater WTD than reading about four such individuals. This finding is in line with research that found that people are insensitive to the scope of the problem, especially when emotional triggers are involved (e.g., Hsee and Rottenstreich, 2004; Slovic, 2007). Interestingly, the presentation that prompted the greatest WTD a deceased relative's organs was the one that featured an unidentified donor and only one identified recipient. This condition combines that of an unidentified donor (which has been found to boost support for organ donation—Harel et al., 2017), and a single identified recipient, which according to research on the identifiable victim effect sparks greater emotions and willingness to help than a group of victims (be they identified or otherwise—Kogut and Ritov, 2005a,b).

Our explorative investigation into participants' subconscious thoughts of death following the organ donation story replicated previous findings that identifying a deceased organ donor prompts more thoughts of death in the perceiver (Harel et al., 2017). While previous research examined explicit, self-reported thoughts of death, in the present research we used an implicit measure of subconscious death thoughts, as elicited by a word-completion task. In keeping with the pattern found for WTD the organs of a deceased relative in Study 1, we found that identification of the donor significantly increased thoughts of death when only one recipient was saved by the donation, and less so when the participant was told that four people were saved by the donation. Thus, it appears that being told about more people being saved by the organs of a deceased donor actually somewhat weakens the impact of Donor's Identification on the tendency to think thoughts of death.

In the present research, thoughts of death and WTD were not examined in the same study, since several prior pilot studies (with small samples) showed that only the task that is closely linked to the story (and to the identification manipulation) was influenced by the manipulation—subsequent tasks were not. Future study is therefore needed to further examine the possible role played by thoughts of death in mediating the link between Donor's Identification and support for organ donation, perhaps by using physiological measures.

Our research has a number of limitations that should be considered when drawing conclusions from it, or when planning

related research. First, the experiments were not pre-registered. Specifically, Study 2 was of an explorative nature, and included pilot studies to explore the effect of the order of the two tasks (WTD, and thoughts of death) on the participants' responses. Thus, future research is needed to replicate these findings, and to examine the mechanisms underpinning the pattern we observed, by means of other methods of gaging thoughts about death. Second, the participants in our experiments are from relatively individualistic societies and cultures. Since the identifiable victim effect has been found mainly in Western cultures (Kogut et al., 2015; Wang et al., 2015), future research is needed to examine how the presentation of organ donations may affect people of more collectivist cultures. Besides its theoretical contributions, our research offers practical implications for efforts to promote organ donations. As suggested by Harel et al. (2017), recruiting people whose lives have been saved by organ donation, identifying them by name, and telling their story may increase media coverage about such individuals, and spur members of the public to think about saving lives when reading about organ donations, and generally to view organ donations in a favorable light. Telling about more people who were saved by the organs of one deceased donor does not seem to be the best strategy to increase support for organ donations. The manipulation we propose to increase willingness to donate organs may be perceived as a way of "programing" people to behave in a certain way. However, the present situation—where only families who have donated the organs of their loved one are telling their story (due to the incentive of commemorating the dead)—appears to be unconsciously affecting the public. Encouraging organ recipients to publish their story may create a more balanced picture of the subject, and increase willingness to donate organs. The greatest positive impact on people's decisions regarding organ donation, according to the results of our research, appears to be when organ donation reports involve an unidentified deceased donor, and a single identified recipient.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Ben-Gurion University of the Negev. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

IH and TK designed the research, and analyzed the data and wrote the manuscript. IH performed the research. Both authors contributed to the article and approved the submitted version.

FUNDING

This research was supported by the National Science Foundation (NSF), Grant 1559546, and the Israel Science Foundation Grant 1426/20.

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Psychology

OPINION
published: 27 January 2022
doi: 10.3389/fpsyg.2022.800752

Empathy at the Gates: Reassessing Its Role in Moral Decision Making

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Keywords: empathy, compassion, affective empathy, norms, value-based choice, rationality

INTRODUCTION

A plethora of research conducted in the past decades has shown that empathy can be essential in guiding and motivating prosocial behavior (e.g., Batson et al., 1981; Eisenberg and Miller, 1987; Batson, 2011; Dickert et al., 2011; Erlandsson et al., 2015). However, there is still considerable debate around whether empathy-driven altruism does more harm than good. For example, several parochial biases have been linked to empathy (Bloom, 2016), such as a preference for helping in-group over out-group members (Cikara and Fiske, 2012) and identifiable over statistical victims (Small and Loewenstein, 2003; Kogut and Ritov, 2005). In response to the limited and scope-insensitive empathic responses to large numbers of victims (Slovic, 2007; Västfjäll et al., 2014), suggestions have been put forward that moral decisions should be guided by rational compassion rather than empathy (Bloom, 2016).

In this article, we attempt to clarify some main points of confusion in the discourse surrounding the merits and pitfalls of empathy. In doing so, we also lay out several possible directions for future research.

DEFINING EMPATHY

Much of the disagreement surrounding the utility of empathy can be attributed to the non-overlapping definitions of empathy used in the field. The fuzzy definitions of empathy and compassion have also been pointed out by several different researchers (Neumann et al., 2015; Cuff et al., 2016; Västfjäll et al., 2017; Eklund and Meranius, 2021; Scheffer et al., 2021). Indeed, the lack of a consistent definition of empathy and compassion is a crucial issue which holds back progress in this area of research.

Most commonly, empathy is described as a multi-factorial construct, with (1) an affective component (experience-sharing), which involves feeling the emotions of another person, (2) a cognitive component (perspective-taking/mentalizing), which involves perceiving another person's thoughts or feelings, and (3) a motivational component (compassion/empathic concern), which involves an emotional response that creates the urge to foster the well-being of others (Batson et al., 1997; Decety and Cowell, 2014; Zaki, 2014; Marsh, 2018).

While empathy is described as having several distinct components, critics of empathy (Prinz, 2011; Singer and Klimecki, 2014; DeSteno, 2015; Bloom, 2017) often equate it *exclusively* to its affective component and consider compassion a distinct process with the capacity to motivate prosocial behavior in more effective ways than empathy (Scheffer et al., 2021). On the other hand, compassion is considered a sub-facet of empathy by many researchers (Decety and Cowell, 2014). Whether compassion should be classified as a sub-component of empathy or if it is a separate process is still an open question which future research should examine. Recent research has already begun investigating the different components of empathy and whether they are separate or co-occur in

OPEN ACCESS

Edited by:
Conny Ernst-Peter Wollbrant, University of Stirling, United Kingdom

Reviewed by:
Daniel Vastfjall, Linköping University, Sweden
Andrea Martinangeli, Burgundy School of Business, France

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *24 October 2021*
Accepted: *04 January 2022*
Published: *27 January 2022*

Citation:
Khalid AS and Dickert S (2022) Empathy at the Gates: Reassessing Its Role in Moral Decision Making. Front. Psychol. 13:800752. doi: 10.3389/fpsyg.2022.800752

daily life experiences (for review see Weisz and Cikara, 2020; Depow et al., 2021). One possibility laid out by Decety (2021), based on an understanding of the evolutionary roots of empathy, is that empathy's core constituents are (1) emotion sharing, which evolved to facilitate cooperation, and (2) concern for others' well-being, which is an adaptive mechanism that evolved to facilitate the care of offspring. According to this multidisciplinary perspective, these core components are influenced by elements such as perspective-taking and theory of mind. While this is a promising theoretical framework, in light of the definitional challenge of empathy, more research is needed to examine what components constitute empathy, to what degree and in which contexts these components co-activate, and how these components differentially (or collectively) facilitate prosocial behavior.

EMPATHY AS A VALUE-BASED CHOICE

Empathy is often described both as an automatic, intractable reaction and as a limited resource that can be depleted with overuse (Decety and Cowell, 2014; Bloom, 2016). These limitations of empathy have been cited as a reason behind its parochialism and insensitivity to statistical victims. However, alternative accounts of empathy have been put forward (Zaki, 2014; Cameron et al., 2017) which argue that, instead of being only intuitive and uncontrollable, empathy can be a motivated phenomenon which individuals choose to approach or avoid based on perceived costs and benefits. The malleability of empathy implies that suggestions to discard empathy as a poor moral compass may be premature because empathy can be shaped toward more positive outcomes.

In support of this view, research that looks at the *cognitive effort costs* of empathizing suggests that empathy's biases may not be inherent in empathy itself but caused by shifts in incentives. When people view empathy as hard work, they might choose to avoid it if given the opportunity (Cameron et al., 2019). People are also more willing to bear the costs of empathy when they have more incentive to do so, such as when it involves members of their kin. This is in line with previous research showing that people avoid empathy in situations where the costs of helping would be too high (Shaw et al., 1994).

One limitation of these studies is that most of them either used a broad definition of empathy or just investigated affective empathy. Whether different components of empathy are affected differentially by motivational cues is still an important question for future research (Ferguson et al., 2020). Another crucial point to note is that while critics of empathy usually recognize that empathy is subject to motivational biases, they fail to make the same judgment for compassion. However, results of a recent study support the view that compassion suffers from some of the same biases as empathy, perhaps even to a greater degree (Scheffer et al., 2021). This study indicated that participants perceived compassion as more cognitively costly than empathy, especially when applied to strangers, which is at odds with the view of compassion as less exhausting than empathy and more likely to lead to sustained helping (Bloom, 2017). Indeed, scope insensitivity has been observed for a range of emotions, including empathy, sympathy, and compassion (Kogut and Ritov, 2005; Dickert and Slovic, 2009; Cameron and Payne, 2011; Västfjäll et al., 2014).

Some of the above research conceptualizes empathy as a value-based choice. From a value-based perspective, individuals make a choice based on its relative subjective value. Thus, under this framework, people might *choose* to feel empathy by (un)consciously considering its costs (effort, time, money) and benefits (monetary rewards, norm conformity, status). Indeed, research in moral decision-making has begun to make the case for a value-based framework which can help reconcile conflicting findings in the literature (Cameron et al., 2017; Pärnamets et al., 2020).

While more work is needed to understand whether (or when) empathy is best conceptualized as a value-based choice or if it is, as often suggested, an automatic, intuitive response unaffected by value processes, empathy can be a dynamic system that shifts with changing values. Consequently, if the aim is to expand the circle of individuals for whom we feel empathy (Singer, 1981), then changing underlying motives or incentives might be a useful direction toward this.

AFFECTIVE EMPATHY: NOT ALL BAD

Affective empathy is subject to a number of biases. We can be insensitive to the number of those suffering (Slovic, 2007; Västfjäll et al., 2014; Dickert et al., 2015), biased toward in-group members (Harris and Fiske, 2006), and prefer helping identifiable victims over faceless masses (Small et al., 2007; Lee and Feeley, 2016). Our internal "empathy meters" often don't scale up with the magnitude of the problem.

However, this aspect does not necessitate the dismissal of empathy as a whole. For one, empathy is an evolved mechanism that serves adaptive functions when it comes to caring for the young and coordinating toward achieving a shared goal (Preston and de Waal, 2002; Cohen et al., 2006; Preston, 2013; de Waal and Preston, 2017). It is possible that intuitive, affective empathy constitutes a primary mechanism for helping in the first place (Decety, 2021). Thus, it could very well serve as an activation process by which further cost and benefit calculations are triggered.

Moreover, some evidence suggests that empathy has expanded to a wider circle of individuals over the past few decades (Pinker, 2011). The increasing efforts toward globalization in several domains of life such as economics, culture, politics and communication as well as technological innovations may create the initial sparks of empathy that allow people to consider the perspective of those outside their immediate in-group (Bhagwati and MacMillan, 2004; Pinker, 2011). These, in-turn, may pave the ways for policies and norms protecting the rights of minority groups, which have already been embedded into the moral psychology of some cultures. Ultimately, these policies and norms may affect our experience of empathy. Indeed, this is consistent with research on how incentives and culture affect the empathic experience (Atkins

et al., 2016; Nook et al., 2016). This suggests that increasing opportunities for intuitive empathy combined with changing existing norms and incentives will be a key aspect of expanding our circle of empathy. In summary, affective empathy is an instinctive, evolved phenomenon which is important for social functioning. While it would appear that it is often intuitive and automatic, the history of moral progress and evidence from contemporary research in psychology suggests that empathy may reflect values which are, at least in part, changeable.

CONCLUSION: MOTIVATED BY THE HEART, GUIDED BY THE HEAD

The research summarized here suggests several ways for empathy research to move forward. First, it is important to figure out exactly which sub-components fall under the empathy umbrella. How these sub-components operate independently and in concert is another crucial question for future research. Future research should also test whether interventions influence all components of empathy, or whether they are more effective for certain components over others. The efficacy of different motivational cues (such as financial rewards, social rewards or psychological benefits like warm glow) should also be tested on each empathic sub-component.

Further, while claims have been made by researchers on how our circle of empathy has expanded over the centuries, causal research on how our morals change is limited (Bloom, 2010; Andreoni et al., 2021). There is some research which suggests that decision framing matters for aiming to expand our circle of moral regard (Laham, 2009). However, our understanding of this process would benefit from longitudinal research on what processes trigger changes in norms and emotions of individuals and what the causal chain of this process is.

On a related note, some scholars have also argued that initiators of norm abandonment (i.e., trendsetters) are a crucial part of the norm change process (Bicchieri and Funcke, 2018). Effective Altruism (EA)–a movement based on using evidence and reason to do the most good–attempts to initiate such a change (Caviola et al., 2021). The central message espoused by its proponents is that individuals in affluent countries are morally obligated to donate to socially distant individuals living in extreme poverty (Singer, 1972, 2015). Focusing on aspects such as effectiveness and efficacy also allows comparisons and makes help more quantifiable. Recent research suggests that donors do not instinctively consider the efficacy of their donations (Burum et al., 2020). Making efficacy salient could shift moral norms and hence make people more sensitive to it.

While the emphasis on effectiveness, efficacy, and rationality may seem like a blow for empathy, this need not be the case. Affective processes might be necessary to create the initial spark that lights the fire of moral progress, as affect-rich stimuli often motivate prosocial behavior (Small and Loewenstein, 2003; Kogut and Ritov, 2005; Erlandsson et al., 2015; Dickert et al., 2016). While empathy and/or compassion are the fuel that kick starts our morality, tools such as logic, critical thinking, utilitarian cost-benefit analyses, argumentation with others and reasoning based on empiricism can serve as the steering wheel–allowing us to recognize and reach our preferred destination (Decety, 2021; Pinker, 2021) and perhaps shift our very experience of empathy.

AUTHOR CONTRIBUTIONS

AK and SD contributed to writing and editing the manuscript. Both authors contributed to the article and approved the submitted version.

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Psychology

ORIGINAL RESEARCH
published: 24 February 2022
doi: 10.3389/fpsyg.2022.800742

Stress and Emotional Intelligence Shape Giving Behavior: Are There Different Effects of Social, Cognitive, and Emotional Stress?

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Acute stress has been linked with prosocial behavior, yet it is entirely unexplored how different types of stressors may affect individuals' willingness to help: This is particularly relevant while people is experiencing multiple sources of stress due to the COVID-19 pandemic. Here we explore whether different types of stress influence peoples' giving behavior and the moderating role of emotional intelligence (EI). Undergraduate students were exposed to experimentally induced social, cognitive, or emotional stress and were asked to self-report on their willingness to help and donate to a charity raising funds for COVID-19 and flu patients. Results showed that when compared to a control condition, after being exposed to a social stress, participants were more willing to help a person in need. Our results also provide evidence that, after experiencing a social stress, participants with high (vs low) trait EI were more willing to help, and, as a result, donated more. Findings indicate that moderate levels of distress are associated with increased donations. Interestingly, when stress is not too threatening, high EI can regulate it and promote prosocial behaviors.

Keywords: acute stress, prosocial behavior, willingness to help, donation behavior, trait emotional intelligence

OPEN ACCESS

Edited by:
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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *24 October 2021*
Accepted: *14 January 2022*
Published: *24 February 2022*

Citation:
Hovnanyan A, Mastromatteo LY, Rubaltelli E and Scrimin S (2022) Stress and Emotional Intelligence Shape Giving Behavior: Are There Different Effects of Social, Cognitive, and Emotional Stress? Front. Psychol. 13:800742. doi: 10.3389/fpsyg.2022.800742

INTRODUCTION

Since ancient times, philosophers have considered the act of one person helping another as the greatest of human values. Indeed, prosocial acts are fundamental features of a healthy and well-functioning society (Nelson et al., 2016; Van Tongeren et al., 2016). It is unquestionable that humans are prosocial species willing to help others. Prosocial behavior is defined as individuals' voluntary intention to serve others at a temporary cost to the self (Eisenberg and Miller, 1987). However, such behaviors are influenced by situational factors (Berger and Rodkin, 2012) as well as how we emotionally respond to them (Lerner and Keltner, 2000). Individuals are constantly exposed to internal demands and environmental sources of stress, that is events that are perceived to be threatening to the self and well-being, which may influence how willing they are to help others.

The possible relation between stress and prosocial behavior has been poorly studied (Von Dawans et al., 2012), for example data are lacking on the effects of different types of stress on willingness to help and donate. Yet, this information is particularly important while humanity is facing a major worldwide health emergency.

Several types of stress fill our daily life and significantly differ one another in terms of how individuals perceive and respond to them. This in turn shapes our behaviors

(Starcke and Brand, 2012) including whether we are willing or not to help others (Youssef et al., 2012). A large body of work has investigated the factors that influence willingness to help others (Agnoli et al., 2015), however, the role played by different types of stress has not been studied systematically. Thus, one of the aims of this study is to assess how willingness to help changes depending on the types of stress individuals encounter and the affective reactions experienced in response to them.

Furthermore, stress responses vary significantly among individuals in relation to how effectively they regulate their emotions. The impact of stress on willingness to help has also been found to be moderated by emotional intelligence (Agnoli et al., 2015).

The goal of the present work is to clarify the relationship between different types of stress and emotional intelligence in shaping willingness to help, while considering the exceptional condition imposed by the COVID-19 pandemic.[1]

Stress can be defined as an adaptive way to mobilize energy and motivate behavior when facing danger (Sapolsky et al., 2000; McEwen and Akil, 2020) as such we here conceptualize responses to stress in terms of a set of mechanisms (biological and behavioral) that enhance survival and that are mediated by dispositional factors within the individual (Kim and Diamond, 2002). When an individual faces a source of stress a complex set of neurohormonal response will take place together with a general unspecific physiological response that can also be linked with a specific subjective emotional experience (Del Giudice et al., 2018). This complex set of responses are thought to fluctuate based upon the intensity, nature, and duration of the stressor, as well as several internal factors of the individual experiencing it (Joëls and Baram, 2009). The way individuals respond to stressful events is determined by one's perception of the event that can be both unconscious (Porges, 2007) and conscious (Lazarus and Folkman, 1984). In addition, the effects of stress on the functioning of the individual follows a non-linear trend: moderate stress and arousal are often adaptive and can bolster performance, whereas high levels of stress sometimes impair behavioral performance (Yerkes and Dodson, 1908; Sandi, 2013). Some studies have shown that, several factors moderate this relationship. One of these factors is thought to be the type of task, for example tasks that are more cognitively demanding require grater arousal for a better performance (Sandi, 2013). Last, different types of stress may trigger different affective, cognitive, and behavioral responses. Affective response to different stressors influences individuals' appraisal of the environment or situation, which can lead to different choices or decisions, for example oriented toward or away from others (Lerner and Keltner, 2000).

Recent work in the field of prosocial behavior and charitable giving has shown the central role played by affect heuristic (Slovic et al., 2007). This heuristic affirms that when people make decisions they rely on their affective state (Slovic et al., 2007). So, decisions to help are significantly influenced by contextual factors (e.g., the charity people are asked to support) and people's affective state (e.g., whether they are in a positive or negative mood; Loewenstein and Lerner, 2003). In everyday life, decisions to help others or not are frequently made under stress and this is particularly true during a worldwide sanitary emergency (Mazza et al., 2020). Given the affective response to different stressors may vary it is expected that the prosocial decisions and actions may be partly influenced by the specific affective state induced by each type of stressors. Despite the attempts to study the link between stress exposure and prosocial behavior, in terms of decision to help and donate, data are often conflicting, and several questions remain unanswered. A growing body of literature reports a positive link between exposure to stressful events and prosocial behaviors (Taylor et al., 2000; Wolf et al., 2015); however, there are also data showing a reduction in helping when people are under stress (Vinkers et al., 2013), and the effect of stress, provoked by time pressure or cognitive load, on altruistic behavior was reported to be barely significant (Tinghög et al., 2016; Fromell et al., 2020). In addition, the types of stress (e.g., social, cognitive, and emotional) and the degree experienced (from low to high) can vary significantly and, consequently, may plausibly influence prosocial behavior in specific ways.

Different situations or events may induce stress. For example, social evaluation and social exclusion (Kogler et al., 2015) or cognitive stress derived from workload and demanding tasks (Roesch et al., 2002) as well as exposure to emotional cues or situations that evoke negative and stressful emotions (van Stegeren et al., 2008). Each of these types of stress influences on one's affective state at different levels and challenges the individual in a different way that implies the need to actively respond to restore homeostasis. In the case of a social stress, we may respond through an increased arousal and anxiety when the interaction with others seams to threaten us (Dickerson et al., 2008). Cognitive types of stress can occur when environmental demands are perceived as taxing or potentially exceeding one's own capacity or resources to manage them, such as in complex arithmetic task when a great amount of cognitive effort needs to be used to solve the problem (Van Bockstaele et al., 2020). Emotional stress is linked with the exposure to highly negative events, cues or even thoughts that cause strong emotional distress and the mobilization of a significant amount of energy to deal with the triggered negative emotions (Mendelson, 2013).

Social, cognitive, and emotional types of stress generate the mobilization of resources that are needed to restore homeostasis; such resources might be linked to different behaviors aimed toward or away from others partly depending on the level of stress experienced (Wolf et al., 2015). In other words, the way an individual respond to a specific source of stress, and how this stress is processed by the mind and body of the individual (see the concept of neuroception proposed by Porges, 2007) may require different amount of energy in order to restore the pre-stressor balance and the selection of different behavioral responses based on a more or less conscious appraisal of the situation. Previous work on the effect of acute stress on willingness to help and donate partially backs our reasoning since, for example some evidence exists about the effect of social and cognitive stress on prosocial behavior (Sollberger et al., 2016; Tomova et al., 2017). For instance, social stress

[1]The present data have been collected during the 2020 pandemic of COVID-19, therefore this variable was accounted for in the manuscript both controlling for fear of COVID-19 and including families of COVID-19 patients as potential targets for donations.

increases the frequency of donation to environmental causes (Sollberger et al., 2016) and Wolf et al. (2015) found that being exposed to social stress (TSST) enhanced emotional empathy. Additionally, there is work showing that cognitive stress increases empathy toward others in pain (Tomova et al., 2017). However, there are scant data on the effect of a purely emotional type of stress on prosocial behavior and to our knowledge there is no data simultaneously exploring the effect of different types of stress on willingness to help and donation behaviors. As a result, one of the goals of the present work is to provide evidence for the effect of emotional stress on willingness to help and donate, while, at the same time, comparing this type of stressor with those that have already been linked to prosocial behavior. Addressing this issue might give practitioners valuable information to select the best contexts in which to maximize people's contributions.

Large variability exists in how an individual reacts to stressors as well as how the same person reacts to different stressors since the response depends on one's appraisal of the specific situation (Lazarus and Folkman, 1984). Extensive recent work has focused on how individual differences impact people's response to challenging or even stressful events.

One of the constructs used to assess these individual differences is trait emotional intelligence. This construct is defined as "perceived emotional self-efficacy" and measures people's tendency to perceive and manage their emotions (Sevdalis et al., 2007). Trait EI includes a series of emotion-related personality traits and is considered as a broad and general dimension of personality (Petrides et al., 2007). Critically, Peña-Sarrionandia et al. (2015) suggested that, compared to the study of specific regulatory strategies, trait EI is a better measure of individual differences in emotion regulation. This is a key insight for our work, since the high variability in people's responses to stress means that targeting specific regulation strategies may expose us to the risk of not capturing it. Instead, measuring trait EI we can focus on the flexibility and adaptability of people's regulation. Consistently, Peña-Sarrionarndia and colleagues showed that people with high (vs. low) trait EI are more likely to downregulate intense emotions (such as fear, anger, or sadness) in stressful situations, and are more prone to perceive events as less negative. In line with this conclusion, Mikolajczak and Luminet (2008) have found that individuals with high trait EI appraise a stressful situation as a challenge, rather than a threat. Additionally, EI has been associated to the efficient processing of positive and negative emotions (Fernández-Berrocal and Extremera, 2006). So, it is possible that individuals with high (vs low) EI have faster mood recovery after being exposed to negative or stressful events (Salovey et al., 2002). Finally, existing data report that people with high (vs. low) trait EI tend to be more effective at stress management and to have superior levels of trait happiness, trait optimism, and self-esteem (Petrides, 2009). For instance, people with high trait EI report lower levels of occupational or life stress than their low EI counterparts (Mikolajczak et al., 2006). To our knowledge, the moderating effect of trait EI on the relationship between stress and prosocial behavior has seldom be tested, especially when looking at different types of stressors. There is a lack of understanding on how EI may affect prosocial behavior in terms of individuals' willingness to help and donate when experiencing stress.

The goal of the present study is to assess the relationship between different types of acute stress and willingness to help and donating behaviors also considering the role of emotional intelligence. In addition, given that data were collected during the 2020 COVID-19 pandemic, we also considered whether willingness to help and donations change as a function of the target of the donation. We assessed whether participants were more willing to give to a charity collecting funds for either COVID-19 or flu patients and their families. To achieve this goal, we designed a 4×2 experiment in which participants were randomly exposed to one of the four stress/control conditions (e.g., cognitive, social, emotional stress or control condition), while all were presented with the two charity scenarios.

Specifically, we aimed at answering the following research questions (RQ).

RQ1a) Does willingness to help change as a function of the type of stress experienced by participants (i.e., cognitive, emotional, and social stress vs. control)?

RQ1b) Furthermore, does willingness to help change as a function of the target of the donation (i.e., COVID-19 vs. flu)?

Given the previously reported relationship between social and cognitive stress and willingness to help (Sollberger et al., 2016; Tomova et al., 2017), it is expected a positive change in willingness to help after the exposure to those types of stress. While for the effect of emotional stress on willingness to help remains to be explored. It is hypothesized that people will be more willing to help COVID-19 (vs flu) patients considering their potential sensitivity to current pandemic related situation (Jones et al., 2020).

RQ2) Does people's trait emotional intelligence moderate their willingness to help as a function of type of stress and target of the donation?

It is hypothesized that individuals with higher (vs lower) trait EI scores will be more willing to help others when exposed to stress (Agnoli et al., 2015), and that trait EI can have a moderating role on the stress and willingness to help link. In relation to whether this moderating role changes as a function of the type of stress and target of the donation, given the lack of data, no specific hypothesis can be advanced, hence this question remains exploratory.

RQ3) Does willingness to help mediate the effect of the independent variables on the actual donation behavior displayed by participants?

This research question is consistent with existing work in the domain of charitable giving showing that people's willingness to help has an impact on their actual decision to donate (Caserotti et al., 2019). Since we expect to find that specific types of stress should have different impact on both willingness of help and donations, we should be able to find the mentioned mediation effect. Furthermore, we will also assess whether the trait EI will have a moderating role in the mediation model. As we reported above, no specific hypothesis can be advanced, and we assess the role of trait EI in an exploratory way.

MATERIALS AND METHODS

Participants

The sample was composed of 400 undergraduate students, 200 male participants (50%) with a mean age of 24.2 (SD = 4.72). Each of four conditions comprised 100 students balanced for gender (50/50). Students of developmental psychology course, at the University of Padova, were invited to participate in the study in exchange for course credits.

Procedure

Data were collected on-line between October and December 2020. As shown in **Figure 1**, after obtaining informed consent from participants, an initial survey allowed to collect demographic information together with data on fear of COVID-19, trait emotional intelligence and empathy. Subsequently, participants were invited to take part in an online video-interview with two experimenters to investigate the effect of stress on willingness to help and donate. Participants filled in the initial questionnaires at the beginning of the data collection and scheduled their call in within one week after they provided the first information. We did approximately 5–7 interviews per day. The rational for the video-call was to assure that the participants remained focused on the task and did not avoid the stress exposure. Overall, five experimenters were involved in the study while two experimenters for each interview were randomly assigned among conditions. During the interviews, participants were not requested to talk, but type or chose preferred answers. Participants were randomly assigned to one of the four conditions: three included the exposure to different types of stress (social; cognitive; and emotional) while one was a no-stress control condition. Before and after the stress or control task exposure, participants were asked to self-report on their negative affect. After the stress exposure, to measure willingness to help and donation behavior, all participants were presented with the description of a charity raising funds for a very ill COVID-19 or flu patient.[2] Participants were later asked to self-report on their willingness to help the patient and the amount of money they were willing to donate to the organization. Subsequently, participants were exposed to a short reminder of the stressful/control task they had experienced before and were then asked to read the other patient scenario and self-report on willingness to help and donate. The scenarios were randomized within condition, so that 50% of the participants were exposed to COVID-19 case after the task and the flu case after the reminder and vice versa. Lastly, they were asked to self-report on perceived danger of COVID-19 and flu, and the probability of getting the viruses.

[2]To select the most adequate condition to be compared with a severe COVID-19 illness a pretest was conducted during the summer (July–August 2020). During the pretest, COVID-19 was compared with flu, pneumonia, and melanoma (given the relatively comparable prevalence and mortality rates of these illnesses in Italy) in terms of affective response to the illness, perceived danger, and probability of getting the illness and reported importance to help patients suffering because of the illness. Results showed that flu was the most comparable illness with the COVID-19.

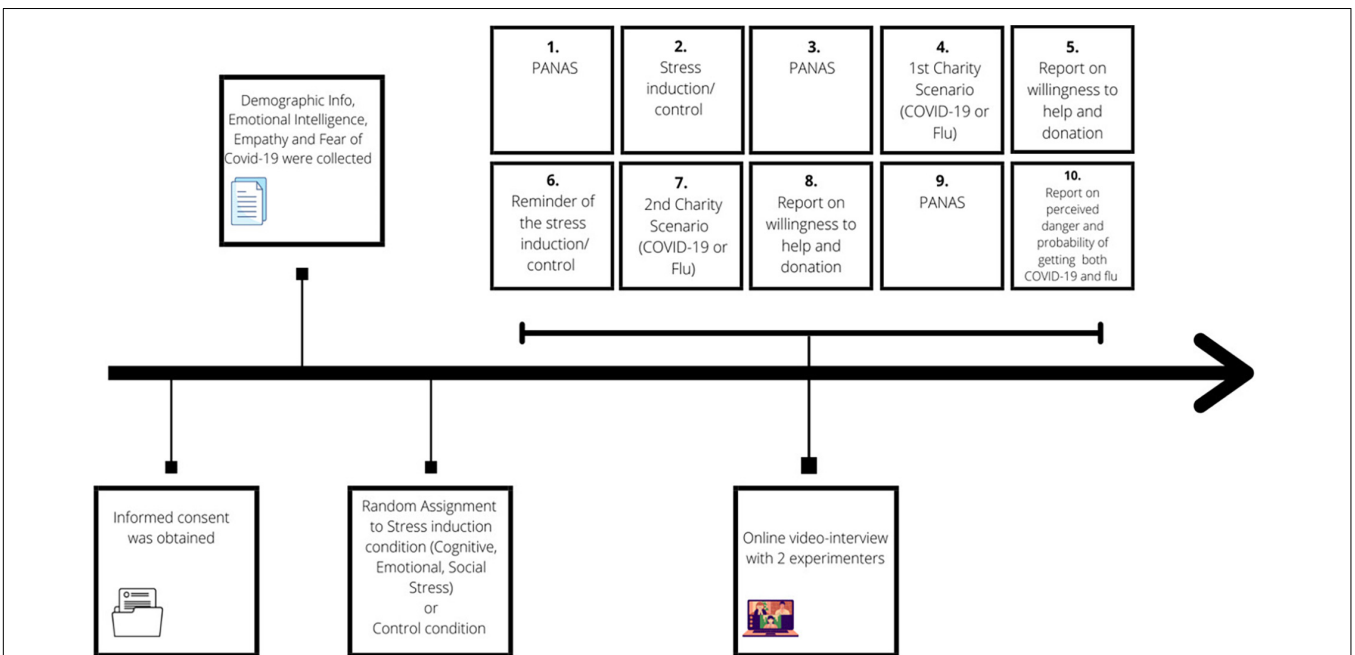

FIGURE 1 | Graphical representation of study procedure. The overall data collection lasted approximately 3 months, containing three different sessions and the distance between one and the other session was kept similar per each participant. The first session included the study procedure description when the informed consent was obtained as well. From 2 to 3 days later, they were sent an online questionnaire on demographic data and individual variables lasting approximately 10 min. One week later, after being randomly assigned to one of the conditions, participants were invited to take part in the online video-interview lasting about 15 min. In average 5–7 interviews have been done per day for 3 months.

Measures

Stress Induction Conditions

The Opensesame software (Mathôt et al., 2012) was used to develop online manipulations, and the duration of each condition was assured to be approximately the same (around 13 min).

Social Stress

To induce social stress, participants were exposed to an online version of the Sing-a-Song Stress Test (SSST; Brouwer and Hogervorst, 2014). In the present study the SSST was administered in an internet-based version (e-SSST), but the stimuli and overall duration of the task were comparable with the original task. Participants were requested to sit comfortably and read the phrases appearing on the monitor one of which contained a task (task essence was not specified). Nine neutral phrases with the same length were selected from Italian Wikipedia (e.g., "The body of the average human adult male is about 60–63% water and the average adult female is about 52–55%"), and were presented for 8,000 ms. The 10-th phrase contained the task: "Please, choose a random song and start singing in a loud voice. We are registering your performance so that our colleagues can watch and judge it later. Once you are ready, please, press the button and keep singing till the 'Rec' disappears." The recording simulation was done with the "Rec" icon being active at the right top of the screen for 3 min (duration was not previously specified). In the end, they got a message that the registered performance will be sent for evaluation. During the reminder, they were asked to sing a short piece of song ("Rec" lasting for 1 min); this second part of the task was justified by saying that we need to make sure the recording went well.

Cognitive Stress

To induce cognitive stress, a mental arithmetic task was adopted, following previously used protocol (Qi et al., 2017). Six blocks ($2 \times 2 \times 2$) of addition, subtraction and multiplication expressions were presented respectively with one (e.g., $3.4 + 6.3$) and two decimal numbers (e.g., 2.06×4.72) so that each block contained seven arithmetic expressions of the same type while the expressions containing one or two decimals presented randomly. Under time pressure, participants were asked to estimate whether the result of each calculation would be above 10 or not by pressing "z" or "m" keys. At each block, following the fixation point of 100 ms, participants were given 3,000 ms to see the calculation and to provide their response. As soon as the response was submitted (or 3,000 ms passed) the formula disappeared. After each block, participants got a feedback on their reaction time and accuracy, and in 80% of cases an automatically generated negative feedback (e.g., "Oh no, you failed, you could be faster.") appeared despite the performance. The reminder of the task was composed by only three blocks that followed the same design.

Emotional Stress

To induce emotional stress, participants were exposed to 36 pictures[3] selected from the International Affected Picture System (IAPS; Lang et al., 2008). Based on IAPS norms, all the pictures had a negative valence (2.0 or less) and with high arousal (at least 6.0) which have been reported to correspond to the ranges of pictures inducing negative stress (van Stegeren et al., 2008). Participants were asked to sit comfortably and watch the pictures, each lasting 8,000 ms and following one after another. The sequence of the stimuli was the same for all participants. The reminder of the task was composed by 16 distress inducing pictures following the same procedure.

Control Task

The no-stress control task was developed based on a standardized low-cognitive-demand task (Plain Vanilla; Jennings et al., 1992). Participants were asked to watch images containing gray balls of different shapes and positions at each stimulus, and to count the cases when a green rectangle appears. Thirty-six images of 8,000ms each were presented among which nine images contained a rectangle. The reminder task consisted of 12 images with three rectangle cases.

Charity Scenarios

To measure willingness to help and donation behavior, all participants were presented with two scenarios describing a case of a very ill COVID-19 or flu patient for whom a charitable organization was collecting funds (see **Supplementary Figure 1**). Specifically, participants were instructed to read an article on a serious case of a COVID-19 or flu patient. Both the COVID-19 and flu articles had the same length (one page), and structure and the patients' pictures were balanced for participants' gender. After reading the article, they were asked to self-report on willingness to help him/her (i.e., "If you were given a chance how much would you be willing to help him/her?") on a scale from 0 (not at all) to 6 (very much) (adapted from CLS, Sprecher and Fehr, 2005). Lastly, participants were asked whether they were willing to donate to the charitable organization in support of the COVID-19 and flu patients and if yes how much they were willing to donate on a scale ranging from 0 to 10 euros (e.g., "Imagine having 10 euros in your wallet, would you like to donate money for this patient? If yes, how much would you donate (0–10)?").

Trait Emotional Intelligence

The TEIQue-SF (Petrides, 2009) is a 30-item self-report scale that measures trait EI using a 7-point scale ranging from 1 (completely disagree) to 7 (completely agree). Items ask participants about their tendency to perceive, regulate, and express their emotions (e.g., "I usually find it difficult to regulate my emotions; I often pause and think about my feelings"). The internal reliability of the scale was high in this study (Cronbach's $\alpha = 0.86$).

Control Variables

Changes in Negative Affect

To assess the changes in negative affect before and after the stress induction procedure the Negative affect subscale of the

[3]The IAPS stimuli manipulated for the emotional stress were labeled with the following slide numbers; and the sequence was kept similar: 2141, 2095, 3030, 3530, 2703, 3053, 2800, 3080, 9940, 3170, 6300, 3140, 2799, 3160, 6230, 3213, 2683, 3215, 3059, 3185, 6212, 3101, 2780, 3102, 6210, 3195, 3230, 3005.1, 3103, 3001, 3261, 6250, 3216, 2205, 9910, 9911. While for the reminder the following stimuli were used: 6840, 3266, 9050, 9007, 9183, 9040, 9413, 3350, 9414, 6623, 9491, 9432, 9430, 3300, 9000, 9810.

Positive and Negative Affect Schedule (PANAS; Watson et al., 1988) was used. Specifically, we asked participants to indicate the extent to which they feel in a specific way at that moment from 0 (not at all) to 4 (extremely). The list of all the negative affective states is presented in **Supplementary Table 1**. The scale showed a high internal reliability in this study (Cronbach's $\alpha = 0.90$). For the analysis, we have computed the delta PANAS which is the difference between the PANAS 2 (after stress) and the PANAS 1 (before stress). It is worth noting that, having a self-report measure of how participants perceive their affective response after being exposed to different stressors might offer important information on the conscious subjective component of the specific response activated after each type of stressor.

Fear Related to COVID-19

The fear of COVID-19 scale (Ahorsu et al., 2020; Soraci et al., 2020) was used to measure participants' fear of the virus. It is a 7-item self-report scale asking the participants to report on the extent to which they agree or disagree with the presented statements using a 5-point scale ranging from 1 (strongly disagree) to 5 (strongly agree) (e.g., "I am afraid of losing my life because of Corona"). The scale's internal reliability was high in this study (Cronbach's $\alpha = 0.87$).

Empathy

Toronto Empathy Questionnaire (TEQ; Spreng et al., 2009) is a 16-item scale that was used to measure empathy. The internal reliability of the TEQ was high in this study (Cronbach's $\alpha = 0.83$).

RESULTS

Preliminary Analyses

Descriptive statistics are presented in **Table 1** (**Supplementary Table 2** shows group comparisons).

TABLE 1 | Descriptive statistics of main study variables.

		Stress type condition			
		Control *M*(SD)	**Cognitive *M*(SD)**	**Emotional *M*(SD)**	**Social *M*(SD)**
	Scenario				
Help	COVID-19	3.48(1.49)	3.60(1.60)	3.71(1.38)	3.91(1.67)
	Flu	3.30(1.42)[a]	3.16(1.61)[b]	3.54(1.34)	3.73(1.55)[a,b]
	COVID-19 and Flu	3.39(1.45)[c]	3.38(1.62)[d]	3.63(1.36)	3.82(1.61)[c,d]
Donation	COVID-19	7.55(3.23)	7.09(3.13)[e]	8.12(2.76)[e]	7.58(3.26)
	Flu	7.36(3.25)	6.63(3.33)[f]	7.70(2.87)[f]	7.26(3.18)
	COVID-19 and Flu	7.46(3.23)	6.86(3.23)[g]	7.91(2.82)[g]	7.42(3.22)
Delta PANAS		−3.64(5.63)	2.13(6.01)	9.12(9.19)	3.63(7.65)
Trait EI		4.92(0.84)	5.02(0.65)	5.02(0.70)	5.00(0.65)
Empathy		65.00(7.22)	62.02(15.38)	64.00(9.54)	62.06(13.17)
Fear of COVID-19		25.64(9.88)	24.82(8.80)	25.40(8.58)	25.74(8.31)

Letters indicate group comparisons. [a]*t = −2.001, p = 0.046.* [b]*t = −2.53, p = 0.012.* [c]*t = −2.11, p = 0.035.* [d]*t = −2.04, p = 0.042.* [e]*t = 2.41, p = 0.016.* [f]*t = 2.37, p = 0.018.* [g]*t = 2.45, p = 0.014.*

Results of the correlations between main variables (**Table 2**) showed that willingness to help both COVID-19 and flu patients were correlated with each other and with donation behavior (both COVID-19 and flu), emotional intelligence, empathy, fear of COVID-19, age, and gender. Donation behaviors for both illnesses were correlated with each other, and with gender, while donating for COVID-19 patients was also correlated with empathy and fear of COVID-19. There was a correlation between emotional intelligence and affective state, empathy, and fear of COVID-19. And empathy was correlated with fear of COVID-19. Finally, there was a correlation between gender and almost all the variables (except the affective state); and between the age and fear of COVID-19.

Affective State

To assess whether the stress induction had an effect on participants' affective states, we computed a delta PANAS, that is the difference between the PANAS score immediately after the stress induction and at baseline, in this way we were able to obtain an index for the change in the negative affect. Then, a multilevel linear regression was performed with type of stress (control, cognitive stress, emotional stress, and social stress) and time (baseline and after the stressor) controlling for gender. Specifically, there were significant difference between the control condition and each other type of stress across time: respectively, $B = 1.32$, $SE = 0.57$, $t = 2.32$, $p = 0.02$ for the cognitive stress, $B = 3.94$, $SE = 0.58$, $t = 6.83$, $p < 0.001$ for the emotional stress, and $B = 1.11$, $SE = 0.57$, $t = 1.94$, $p = 0.05$ for the social stress. A slope analysis showed that while in the control condition there was a significant decrease in stress over time (mean at baseline = 11.63, SD = 11.65 vs. mean at t2 = 7.99, SD = 7.90; $t = -2.71$, $p < 0.001$), a significant increase emerged after the emotional stress inductions (mean at baseline = 8.65, SD = 8.35 vs. mean at t2 = 17.77, SD = 11.19; $t = 6.89$, $p < 0.001$). No significant effect on the PANAS was found after the cognitive stress induction (mean at baseline = 11.01, SD = 10.81 vs. mean at t2 = 13.13, SD = 10.10; $t = 0.56$, $p = 0.57$) and after the social stress induction (mean at baseline = 10.32, SD = 8.46 vs. mean at t2 = 13.95, SD = 8.27; $t = 0.04$, $p = 0.97$). Hence, the change in negative affect, was included as a covariate in the following analyses. Given the important changes in the overall negative affect score, in **Supplementary Table 1** we report also the changes in the single affective states composing the total score. As reported in the table, participants reported to experience high levels of "Alert," "Ashamed" and "Nervous" states when exposed to the social stress condition (M = 0.99, SD = 1.36; M = 2.19, SD = 1.37; M = 0.68, SD = 1.32, respectively); while "Embarrassed" state was high in the cognitive stress condition (M = 0.93, SD = 1.18); and "Afraid," "Miserable," "Disgusted," "Sad," and "Shocked" were reported as high in the emotional stress condition (M = 0.63, SD = 1.13; M = 0.86, SD = 1.19; M = 2.09, SD = 1.35; M = 1.22, SD = 1.29; M = 1.55, SD = 1.36, respectively).

TABLE 2 | Correlation matrix between main variables.

	1	2	3	4	5	6	7	8
1. Help COVID-19								
2. Help Flu	0.72***							
3. Donation COVID-19	0.44***	0.38***						
4. Donation Flu	0.36***	0.43***	0.89***					
5. Delta PANAS	0.04	0.00	0.01	0.01				
6. Trait EI	0.11*	0.12*	0.01	0.03	0.14**			
7. Empathy	0.23***	0.17***	0.12*	0.10	0.10	0.13*		
8. Fear of COVID-19	0.24***	0.17***	0.10*	0.08	−0.05	−0.15**	0.13**	
9. Age	−0.15**	−0.11*	−0.01	0.00	−0.01	0.01	−0.04	−0.15**

*$p < 0.05$. **$p < 0.01$. ***$p < 0.001$.

Main Results

Stress and Willingness to Help

To assess if willingness to help changed as a function of the type of stress experienced by participants (i.e., cognitive, emotional and social stress vs. control) and in response to the target of the donation (i.e., COVID-19 vs. flu) we run a multilevel linear regression model with willingness to help as the dependent variable and type of stress and target of the donation as factors as well as a second model in which we included the interaction between type of stress and target of donation. In addition, in both models, we included as covariates: empathy, fear of COVID-19, gender, and change in negative affect. A model comparison showed that the addition of the interaction did not improve the fit to the data ($X^2 = 3.54$, $p = 0.32$). As a result, here we discuss only the model with the main effects of type of stress and target of the donation (**Table 3**). We found a significant effect of target of the donation ($B = -0.26$, $SE = 0.06$, $t = -4.48$, $p < 0.001$), indicating that participants were more willing to help when the target was suffering from COVID-19 rather than flu. Furthermore, for the type of stress a significant difference emerged for the comparison between control condition and social stress ($B = 0.53$, $SE = 0.20$, $t = 2.61$, $p = 0.01$), indicating that participants were more willing to help when experiencing social stress. The differences between the control condition and the other two stress conditions were not significant (ps = 0.09 or higher, see also **Table 1** for mean and group comparisons). Given the social stress condition was the only one different from the control, we decided to run a second analysis to assess whether any difference emerged among the three types of stress manipulations. Once we changed the reference level to the social stress condition, the results showed that it was different from the control ($B = -0.53$, $SE = 0.20$, $t = -2.61$, $p = 0.01$) and the cognitive stress conditions ($B = -0.43$, $SE = 0.19$, $t = -2.26$, $p = 0.02$), whereas the difference with the emotional stress condition was not significant ($B = -0.14$, $SE = 0.20$, $t = -0.71$, $p = 0.48$).

Finally, there was a significant positive effect on willingness to help for both empathy and fear of COVID-19, while females were more willing to help than males. In addition, the theoretical relevance of the covariates was also statistically supported as the model was stronger when the covariates were included ($R^2 = 0.12$) compared to when they were not ($R^2 = 0.02$). However, the same difference among stress manipulation on the willingness to help remained significant also when covariates were removed from the model.

TABLE 3 | Multilevel linear regression model with willingness to help as the dependent variable, type of stress and target of the donation as factors.

	B(SE)	*df*	*t*	*p*
(Intercept)	2.54(0.50)	402.84	5.13	0.000
Condition				
Cognitive	0.11(0.20)	378.24	0.55	0.581
Emotional	0.39(0.23)	378.12	1.71	0.088
Social	0.53(0.20)	378.18	2.61	0.009
Charity Scenarios (flu = 0; COVID-19 = 1)	0.26(0.06)	376.72	−4.48	0.000
Delta PANAS	−0.01(0.01)	389.18	−0.99	0.323
Gender	−0.00(0.00)	378.12	−3.93	0.000
Empathy	0.02(0.01)	378.46	2.69	0.007
Fear of COVID-19	0.02(0.01)	377.87	2.05	0.041

Moderating Role of Emotional Intelligence

To assess the moderating role of peoples' trait emotional intelligence we performed a multilevel linear regression model with willingness to help as the dependent variable, type of stress and target of the donation as well as the interaction between type of stress and trait EI. In addition, we included as covariates empathy, fear of COVID-19, gender, and PANAS (**Table 4**). Results revealed a significant interaction between the trait EI and the contrast comparing the control condition with the social stress induction. The two contrasts including the cognitive stress induction and the emotional stress induction were not significant. All covariates that were significant in the previous analysis remained significant. A slope analysis showed that the effect of trait EI was only significant in the social stress induction condition ($t = 2.52$, $p = 0.001$) but not in all other conditions (ts = 1.50 or lower, ps = 0.14 or higher). See also **Figure 2**.

Donation Behavior

We then assessed participants' donation decisions by way of a multilevel linear regression model with type of stress, target of the donation, trait EI, willingness to help, and the interaction between condition and trait EI as predictors. In addition, we included in the model the same covariates as in previous analyses.

TABLE 4 | Multilevel linear regression model with willingness to help as the dependent variable, type of stress and target of the donation as well as the interaction between type of stress and trait EI.

	B(SE)	*df*	*t*	*p*
(Intercept)	2.89(0.95)	377.07	3.04	0.002
Condition				
Control vs. Cognitive	−1.79(1.32)	367.63	−1.36	0.174
Control vs. Emotional	−1.07(1.31)	367.34	−0.82	0.411
Control vs. Social	−2.41(1.33)	367.38	−1.81	0.07
Trait EI	−0.07(0.17)	369.96	−0.43	0.667
Charity Scenarios (COVID-19, flu)	−0.26(0.06)	375.82	−4.47	0.000
Fear of COVID-19	0.02(0.01)	366.00	2.29	0.022
Gender	−0.01(0.00)	366.26	−3.67	0.000
Empathy	0.02(0.01)	366.33	2.44	0.015
Delta PANAS	−0.01(0.01)	367.91	−1.16	0.245
Condition Control vs. Cognitive × Trait EI	0.38(0.26)	367.72	1.46	0.144
Condition Control vs. Emotional × Trait EI	0.30(0.26)	367.64	1.15	0.250
Condition Control vs. Social × Trait EI	0.59(0.27)	367.51	2.24	0.025

Baseline category for Condition was Control Condition.

Results showed a significant effect of willingness to help ($B = 0.93$, $SE = 0.07$, $t = 12.90$, $p < 0.001$). All other effects were not significant (ps = 0.07 or higher).

Mediation Analysis

Lastly, we assessed whether willingness to help mediated the effect of the independent variables on the actual donation behavior displayed by participants. The tested model is presented in **Figure 3**. As it can be seen it included the main effects of condition (control vs. social stress induction) and trait EI as well as their interaction as predictors of both willingness to help (mediator) and donation behavior (dependent variable). We also included the same covariates as in previous analyses. Although neither the condition nor the interaction had a direct effect on donation behavior (respectively, $B = 0.98$, $SE = 0.69$, $t = 1.43$, $p = 0.16$ for condition and $B = -0.21$, $SE = 0.14$, $t = -1.54$, $p = 0.12$ for the interaction, see also **Table 1** for mean and group comparisons), there was a significant indirect effect of willingness to help ($B = 0.50$, $SE = 0.19$, $t = 2.60$, $p = 0.009$). In other words, in the social stress condition compared to the control, emerged an effect of trait EI whereby an increasing score on this dimension led to an increase in willingness to help and, as a result, to higher donations.

DISCUSSION

This is the first study comparing the effects of social, cognitive, and emotional stressors on willingness to help and donation behavior. The aim of this study was to investigate how acute stress affects individual's willingness to help and donation behavior when considering the potential moderating role of emotional

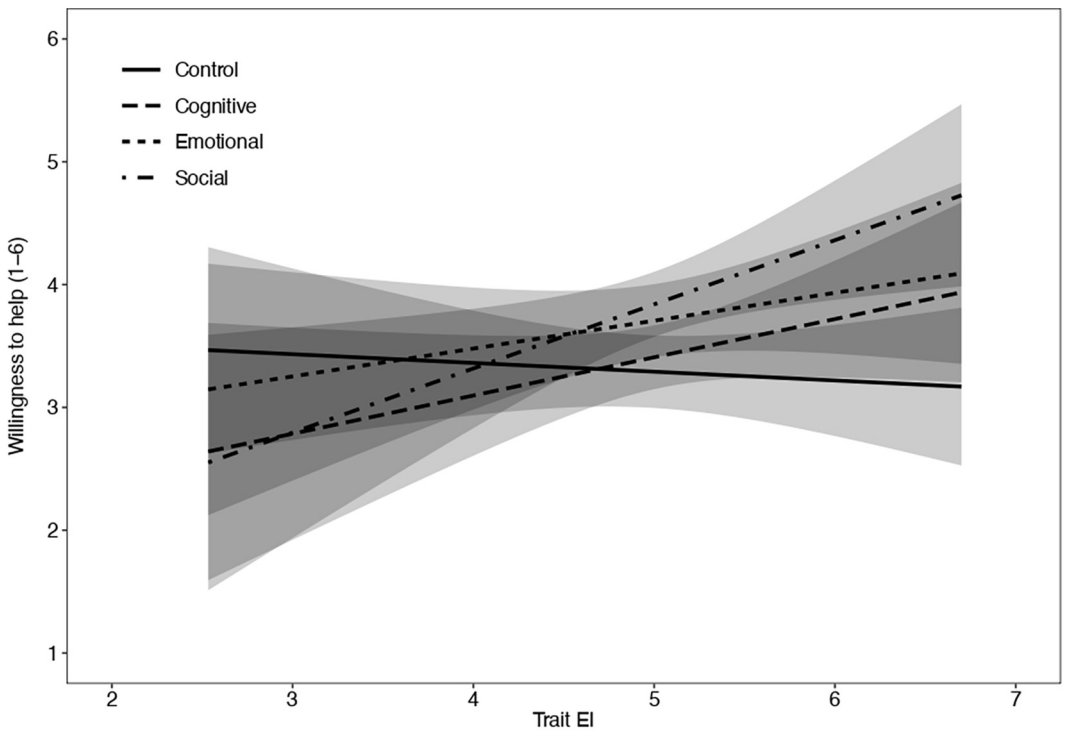

FIGURE 2 | Simple slope of trait emotional intelligence predicting willingness to help for control, cognitive, emotional, and social conditions. The x-axis represents the score of trait EI, and the y-axis represents the degree of willingness to help. Conditions are represented by the types of lines.

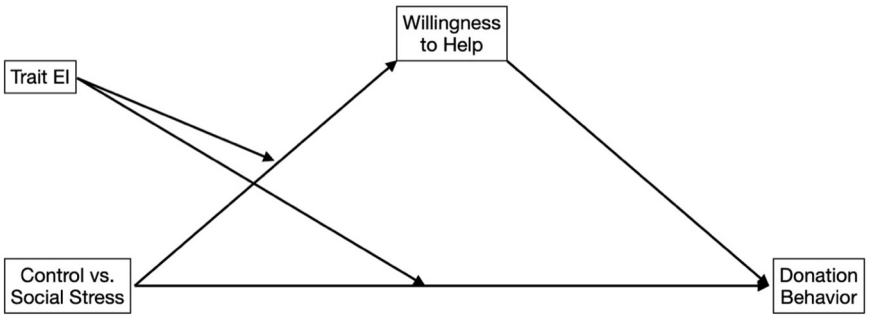

FIGURE 3 | Path of the mediation analysis. The model tests whether willingness to help mediates the effect of the independent variables on donation behavior.

intelligence. Given the current situation related to the COVID-19 pandemic, we assessed how willingness to help changes as a function of donation target, that is COVID-19 or flu patient.

The results are consistent with previously reported data on the negative impact of social, cognitive, and emotional stressors on people's affective state (van Stegeren et al., 2008; Qi et al., 2017). Namely, when comparing with the control condition, all three stressors were associated with a change in negative affective state.

One of the key findings of our study is related to the comparison of the effects that different types of stress have on willingness to help. We found that participants were more willing to help after being exposed to social stress compared with the control condition. The change in negative affect participants reported in the social condition was intermediate compared to the emotional (highest) and cognitive (lowest) conditions. Based on the impact that each type of stress had on the participants, a plausible explanation of the effect of stress on willingness to help is that people are more likely to act when experiencing medium negative affect. That is, when exposed to social stress, participants were more willing to help than when exposed to the control condition. At a broader conceptual level, this data may be explained by the tend-and-befriend hypothesis proposed by Taylor et al. (2000), which assumes that at situations of stress an adaptive way to respond to stress may be the tendency to help others with the potential to have collaborative relations at future challenging conditions. While when participants were exposed to high distressing emotional images or the low distressing cognitive task no difference from the control condition was found. These findings are in line with Decety's empathy model (Decety and Lamm, 2006) stating that other-oriented feelings and prosocial behaviors may not occur under high levels of personal distress, since it can challenge resources and activate an adaptive stress response. It is possible that for the participants who had an increased stress level after watching emotionally negative pictures, helping others would have been too demanding as they had to use their resources to manage their own reactions. Even from an evolutionary point of view, focusing on self needs at times of highly stressful situation may potentially increase one's chances to survive. It should, however, be noted that when comparing willingness to help in the social stress condition to the other two stress conditions (i.e., emotional, and cognitive stress), no significant difference was found in participants' willingness to help after a social and an emotional stressor. Yet, participants were more willing to help after the social stressor compared with the cognitive one. Not different effects of social and emotional stressors on willingness to help can be explained by the fact that specific elements of stress manipulations in both cases could somewhat promote prosociality, unlike at cognitive stress condition. Namely, participants knew their song would be watched and they might think their behavior would be evaluated as well, so perhaps they tried to perform "well" by their willingness to help. In the same way, stress inducing images could potentially promote helping behavior through visual cues, such as an image of a person in negative mood or in danger who might need support.

In contrast to highly stressful emotional condition, the cognitive stressor was associated with stress level not too different from those of the control condition and, thus, it did not affect participants' willingness to help. Hence, when thinking about the relationship between stress and willingness to help we might refer to a non-linear, inverted U-shaped function as proposed by Wolf et al. (2015). That is, possibly, under low and high levels of stress individuals may be less willing to help others in need, while a medium level of stress can be associated with seeking and providing support and may lead individuals to orient toward others. Yet, previous studies linking social stress and willingness to help or more in general prosocial behavior have found that social stress exposure increased participants' trust, trustworthiness and sharing behavior in social interaction (Von Dawans et al., 2012); as well as altruistic responses (Buchanan and Preston, 2014).

Additionally, the affective response to each stressor could have influenced the prosocial behavior as well. As such, an alternative possible explanation of why participants in the social stress condition were more willing to help could be that the affective states like alert, ashamed, and nervous that were experienced high in the social stress condition, may have potentially led to pro-social actions (compared to afraid, miserable, disgusted, sad, and shocked states that were high in emotional stress condition). More specifically, the effect of social stress induction on prosocial behavior could be due to the negative affect experienced, for instance, the participants who felt ashamed during the manipulation might want to help others to "recover"

their reputation and to give a second chance to the "evaluators" to reconsider their performance.

This first analyses also showed that the participants were more willing to help when the patient was suffering because of COVID-19 (vs flu). Even though flu when data were collected was comparable with COVID-19 in terms of prevalence and mortality rates, participants demonstrated more helping intentions for COVID-19 patients. This finding may be due to the fact, that during the pandemic people are more sensitive to this specific topic and, so, give more importance to helping for COVID-19 related reasons (Jones et al., 2020). However, it is important to point out that whereas participants were more willing to help for the COVID-19 patients, the type of stress did not interact with the patient case, thus indicating that findings were not influenced by the pandemic[4].

The second research question aimed at exploring the possible moderating effect of trait EI on the relationship between social stress and willingness to help. Results showed that participant's willingness to help under social stress was moderated by trait EI, namely participants' having high (vs. low) levels of EI were more willing to help under social stress. Since people with high trait EI are more effective at regulating their emotions, a possible explanation of this finding is that they were more able to regulate the negative affect elicited by the social stressor, thus being more willing to exert an effort to help others in need. This is in line with the notion that high trait EI scores may lead to efficient stress management and high trait happiness, trait optimism, self-esteem (Petrides, 2009).

Interestingly, this finding strengthens the hypothesis that a moderate level of distress can increase participants' willingness to help, that is better self-regulatory abilities (higher trait EI) can tune down distress and promote prosocial behaviors. It should be noted here that trait EI interacts only with social stress and not with the response elicited by the emotional stress, even though exposure to this type of stressor caused a greater negative affect compared to all the other types of stress. The rational here might be that EI moderates the link between stress exposure and willingness to help when the response elicited by the stressor is associated with high arousal. We might expect that the social stress task, while eliciting less negative affect compared to the emotional stressor, caused greater arousal. This is supported by a wide literature using social types of stress such as the Trier social stress test (McRae et al., 2006) or the sing a song test (Brouwer and Hogervorst, 2014) to elicit a stress response and an increase in arousal (Eagle et al., 2021). This explanation however should be address by future studies registering the elicited response to different types of stress in particular addressing arousal, for example through registration of peripheral physiological indexes such as heart rate or skin conductance response.

Our third research question investigated whether willingness to help mediated the effect of the independent variables on the actual donation behavior displayed by participants. The mediation analysis showed that the effect of the type of stress on willingness to help led to differences in donation behavior as well. As a result, by increasing willingness to help, the social stress manipulation had the indirect effect (compared to other types of stress) of increasing how much people were willing to donate. Furthermore, participants with greater trait EI were more willing to help and, as a result, also donated more. These results could be explained by the Theory of reasoned action (TRA; Ajzen and Fishbein, 1980) and the Theory of Planned Behavior (TPB; Ajzen, 1991): both theories assume that human behavior is affected by behavioral intention. Indeed, the intention to help, expressed by participants as willingness to help, had a direct positive effect on the actual donation behavior. A possible explanation might be that when participants felt middle levels of stress, that is in the social stress condition, acting in a prosocial fashion my reduce the stress people experienced (Taylor et al., 2000; Buchanan and Preston, 2014), whereas when stress is too high or too low participants may not be able to use giving as a regulation strategy or do not need it.

Moreover, trait EI moderated the effect of social stress on willingness to help, which in turn influenced donations. Therefore, we must conclude that the indirect effect of stress on donations is not equal for all participants but depends on individual differences in emotion regulation. Indeed, on the one hand, it has been shown that there is ample variability in how specific individuals deal with stress and, on the other hand, previous work on trait EI has found it to be a good proxy of the use of more adaptive regulation strategies. As we stated in the introduction, our analysis of the role of trait EI was explorative and, as such, it should be further investigated in the future.

The present study has several limitations. Specifically, we believe that it would have been very interesting to assess the physiological correlates of stress response. We were not able to collect this data due to the pandemic, but this work would benefit from a replication study comparing the peripheral physiological responses to different types of stress and studying how it might be linked to willingness to help and donating behaviors. Moreover, the potential confounds related to the different effects of all three manipulations on prosocial behavior need to be considered. As mentioned above, the reason why participants decided to help could partly be the specific affect induced by the stressor (e.g., ashamed) and/or their belief that the experimenters may continue to evaluate their "helping performance" after singing at social stress condition. Similarly, the pictures that they were exposed to during the emotional stress condition could potentially contain visual cues (e.g., images of someone in need/danger) promoting helping behaviors. It should also be noted that the different stress manipulation tasks required acts of different nature (e.g., signing, doing arithmetic tasks, or watching images) which might somehow influence the elicited response. However, when comparing different sources of stress, it is very difficult to have the same actions involved. Once more the inclusion of physiological indexes might help to better control this issue (e.g., checking for the effect of movement, degree of sympathetic response). Overall, further investigations are needed to address

[4]In support to the conclusion that stress induction was not influenced by the pandemic, the same pattern of results was found when controlling for the fear of COVID-19 scale.

all these critical aspects. Finally, the data collection was conducted online, and together with its benefits (e.g., faster communication, less financial resources) the online data collection may potentially be a source of several issues. One of the limitations of online experiments is that the experimental conditions cannot be identical for each participant, and some external factors may be uncontrollable. We made a great effort to reduce this variability to the minimum, for instance asking participants to sit alone in a quiet room, yet we may expect the stress manipulations to work better in experimental rooms specifically designed for the task rather than within home environments (it should be noted that very recent and preliminary data have shown the efficacy of on-line stress exposure in terms of emotional response (Eagle et al., 2021).

Despite the limitations, this study significantly contributes to both the literature on stress and that on willingness to help. Here we emphasize the importance of studying how specific types of stress, which potentially can be experienced at different levels, may be associated with people's willingness to help, and donating behaviors. Findings reveal that after being exposed to social stress, which causes an intermediate (i.e., not too high, or too low) negative emotional response, people are prone to act pro-socially and help others particularly when they have high trait EI. Overall, from the present study, we can expect people to engage more in giving behaviors when they are experiencing an average degree of negative affectivity in response to a social stress compared to when they are too negatively affected by an emotional stress or even compared to when in an emotionally neutral state as when in the cognitive stress and control conditions. Moreover, after experiencing social stress the fact of being good emotion regulators promotes even more helping behaviors. In other words, it would make sense to expect grater donations, for example to charities, when people are either experiencing some distress but not too much or when they are very good at regulating their distress. At the same time, when individuals are in an extremely negative affective state due to emotional distress, they are much less willing to donate. This data should give a heads up to organizations relaying on charity donations in times when the population is experiencing major distress, just like is happening now during the worldwide COVID-19 pandemic.

In a time when people of all socio-economic backgrounds struggle due to either emotional distress due to COVID-19 and restrictive measures, cognitive challenges related to on line working while juggling family and house cores and social stress due to lack of social contacts for long periods of time and subsequently the return to social gatherings and interactions this study gives important indication on whether giving behaviors should be expected in relation to different distressing situations. Moreover, the slow reopening after the immunization following vaccine administration and gradual return to normality might be associated to the experience of social stress. Indeed, people might be overwhelmed by going back to daily and possibly judging social interactions. This source of stress, however, especially among better self-regulators may promote willingness to help and might be a significant period to ask for donations.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found at: https://osf.io/pc5jt/?view_only=55392060b26d4d4cac9bddbe4202422f.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Ethical Committee for the Psychological Research of the University of Padova (Ethic Approval Number: 90898E5831387486DC98DB0C57111546). The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

AH was responsible for the conceptualization and design, data collection and analysis, and original draft preparation and editing. LM contributed to designing, data collection, and writing. ER and SS supervised the conceptualization, data analysis, and writing. All authors contributed to the writing and approved the submitted version.

FUNDING

This study was supported in part by the CARIPARO (Cassa di Risparmio di Padova e Rovigo) Foundation research fellowship.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2022.800742/full#supplementary-material

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frontiers in Psychology

ORIGINAL RESEARCH
published: 10 March 2022
doi: 10.3389/fpsyg.2022.800701

Incidental Emotions and Cooperation in a Public Goods Game

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The study reported here considers the relationship between emotional state and cooperation. An experiment is conducted in which the emotions of fear, happiness, and disgust are induced using 360-degree videos, shown in virtual reality. There is also a control condition in which a neutral state is induced. Under the Fear, Happiness, and Disgust conditions, the cooperation level is lower than under the Neutral condition. Furthermore, cooperation declines over time in the three emotion conditions, while it does not under Neutral. The findings suggest that emotions are associated with the dynamic pattern of declining cooperation over time.

Keywords: emotion, cooperation, virtual reality, experiment, free-riding

OPEN ACCESS

Edited by:
Gustav Tinghög, Linköping University, Sweden

Reviewed by:
Johannes Lohse, University of Birmingham, United Kingdom
Jordi Brandts, Spanish National Research Council (CSIC), Spain

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *23 October 2021*
Accepted: *10 February 2022*
Published: *10 March 2022*

Citation:
Nguyen Y and Noussair CN (2022) Incidental Emotions and Cooperation in a Public Goods Game. Front. Psychol. 13:800701. doi: 10.3389/fpsyg.2022.800701

INTRODUCTION

Cooperation is the sacrifice of one's individual interest to increase social welfare. Cataloging the determinants of cooperative behavior has attracted a great deal of interest from economists and other social scientists. Experimental research has established that the level of cooperation follows predictable patterns, and numerous correlates of cooperative behavior have been identified. Nonetheless, among individuals, there is considerable heterogeneity in the propensity to cooperate. Indeed, the same individual may cooperate in one instance, and then shortly thereafter, in a similar situation, behave totally selfishly. One potential source of this variability is the decision maker's emotional state, which differs across individuals and changes over time, sometimes rapidly. In traditional theories of economic decision-making, the role of emotions has typically been neglected. The link between emotional state and the tendency to cooperate is the topic of the study reported here*.

One of the most widely used experimental paradigms to investigate the circumstances under which individuals cooperate is the Voluntary Contributions Mechanism (VCM). Originally studied in a somewhat different form by Marwell and Ames (1979) and Dawes (1980), this paradigm is also often referred to as the Public Good game. In this game, a number of agents in a group each have an endowment, which each agent can allocate, in any proportion, between a private and a group account. The amount that an individual puts into her private account is hers to keep. The amount placed into the group account is multiplied by a factor greater than 1 (though lower than the number of players, N) by the experimenter, and the resulting total is divided equally among all group members. These incentives mean that each individual has a dominant strategy to place the entirety of her endowment into the private account, while the strategy profile that maximizes the group's total payoff is for all players to place their whole endowment into the group account. The amount placed into the group account is referred to as a contribution, and the percentage of endowment contributed is taken as a measure of cooperation. Thus, the VCM paradigm permits measurement and comparison, both between individuals and groups, of the extent of self- versus group-interested behavior.

It was established early on that cooperation is not uncommon but also not universal (Dawes, 1980). With repetition of the game, cooperation declines (Andreoni, 1988; Isaac and Walker, 1988a). There are a number of correlates of cooperation, most prominently the marginal-per-capita return (Isaac and Walker, 1988a), the amount that each unit contributed to the group account yields to each group member (the higher the marginal-per-capita-return, the more that is contributed to the group account). Changes to the institutional structure, such as permitting communication (Isaac and Walker, 1988b), as well as allowing for peer-to-peer punishment (Yamagishi, 1986; Ostrom et al., 1992; Fehr and Gächter, 2000) also can increase cooperation.

The characteristics of participants can also influence the level of cooperation that a group exhibits. Some correlates include program of study (Marwell and Ames, 1981), risk attitude (Teyssier, 2012; Kocher et al., 2015), and level of cognitive sophistication (Lohse, 2016)[1]. The level of cooperation is also influenced by extent to which players have preferences to reciprocate kind or unkind actions. There is strong empirical evidence of a correlation between cooperation in the public goods game and expectations about the cooperation of others (Fischbacher et al., 2001; Fischbacher and Gaechter, 2010; Bechtel and Scheve, 2017).

Some work has considered the correlation between personality and cooperation. Balliet et al. (2009) conduct a meta-analysis of studies relating Social Value Orientation (SVO, Messick and McClintock, 1968) to cooperativeness. They find that the SVO measure correlates with cooperation level, with less competitive individuals cooperating more. Hilbig and Zettler (2009) show that those exhibiting greater values of the personality dimension of honesty-humility are more cooperative. Thielmann et al. (2020) find, among other results, that agreeableness and environmentalism correlate positively with cooperation in a social dilemma.

Though less explored, it is quite plausible that transitory forces affecting participants at the time their decisions are made could matter as well. Here, we consider whether the emotional state of participants is a determinant of behavior. We conduct an experiment in which we induce, in different treatments, three emotional states: happiness, fear, and disgust, as well as a neutral state that serves as a control treatment. We then compare the resulting level and dynamics of cooperation under the different emotional states. We induce, rather than track, emotional state, in order to be able to establish causal relationships between emotional state and cooperation.

Indeed, many psychologists view emotions as a key determinant of human cooperation, and assert that cooperative behavior is affected differently by different emotions (Fessler et al., 2015). Several different mechanisms have been proposed. The *Affect Infusion Model* (Forgas, 1995, 2001) argues that emotional state colors one's decision-making process, so that, for example, a positive emotional state might affect beliefs about the likelihood that outcomes will be positive or negative. The *Affect as Information* framework (Schwarz and Clore, 1988, 2003) posits that one's emotional state is used as an informative input into the decision process, e.g., if one is in a fearful state, it is interpreted as a sign that there is adverse risk possible in the decision one is making, and that one should avoid the risk.

In experimental economics, the connection between emotions and cooperation has been explored by a number of authors. Drouvelis and Grosskopf (2016) show that cooperation is sensitive to subjects' current emotional state. Specifically, a happy emotional state leads to higher contributions, and an angry state leads to lower contributions, to a public good. In a similar vein, Joffily et al. (2014) report that a more positive emotional state is associated with greater cooperation. Boyce et al. (2016) find that sadness or happiness does not affect the willingness-to-pay for environmental goods. Capra (2004) observes that a positive emotional state increases giving in dictator games. These studies build on a long tradition in management and psychology studying emotions and cooperation. For example, Hertel and Fiedler (1994) consider the effect of positive and negative emotional states, induced using film clips, on cooperation framed as an airplane maintenance task. They find that their mood induction did not affect the average level of cooperation, but they did observe that positive mood increased the variability of cooperation. Other studies investigate cooperative behavior in response to shame and guilt (de Hooge et al., 2007), gratitude (DeSteno et al., 2010), and anger (Motro et al., 2016)[2].

Clarifying the relationship between emotional state and cooperation can shed light on the ongoing debate about whether cooperation is intuitive. Rand et al. (2012) report that cooperative behavior is intuitive. They use time pressure to elicit spontaneous behavior, and they observe that decisions taken under time pressure tend to be more cooperative[3]. However, Kvarven et al. (2020), in a meta-analysis of 82 studies on intuition and cooperation, show that the relation between intuition and cooperation is driven by six studies in which the use of emotional processing was manipulated. For example, Levine et al. (2018) report that, when informed that players in a prisoners' dilemma used emotion to determine their action, observers thought that the players were more likely to have cooperated. Players who reported using emotion rather than reason in their own decisions, as well as those who thought their partner employed emotion, were also more likely to cooperate. Participants instructed to use emotion in their decisions were more likely to cooperate. Gärtner et al. (2022) find that inducing an affective decision mode increased pro-social behavior in five of the six paradigms

[1] Marwell and Ames (1981) find that students of economics tend to free-ride more than those in other programs of study. Kocher et al. (2015) find no relationship between risk aversion and contributions, while Teyssier (2012) observes that risk aversion and contributions are negatively correlated. Lohse (2016) documents a positive correlation between a cognitive ability measure, the Cognitive Reflection Test, and contribution level.

[2] de Hooge et al. (2007) observe that guilt is associated with greater cooperation while shame is not. DeSteno et al. (2010) report that inducing gratitude on the part of players increases cooperation. Motro et al. (2016) find that under some conditions, anger reduces cooperation levels.

[3] Tinghög et al. (2013) have called this result into question by showing that the relationship between time pressure and cooperativeness is only present if the data from those participants who did not make a decision within the allowable time frame are excluded.

they studied, including the prisoners' dilemma, but the exception was the Public Good Game. These results indicate that a deeper understanding of the relation between emotional state and cooperation is needed. In particular, isolating the effect of specific emotions such as happiness, fear and disgust on cooperation, may clarify the precise manner whereby emotional processing and cooperation are associated.

In our experiment, we induce three different emotional states and a neutral state, and then observe behavior in a repeated Public Good game. The conditions are Fear, Happiness, Disgust, and a Neutral treatment. A positive relationship between happiness and cooperation has been documented by Drouvelis and Grosskopf (2016) and thus our evaluation of the effect of happiness represents a conceptual replication of this earlier result. To our knowledge, the effect of fear and disgust on cooperation have not been studied. Each of the three emotions are among the six basic universal emotions as cataloged by Ekman and Friesen (1975). We find that Fear, Happiness, and Disgust all result in lower contributions compared to the Neutral treatment. In other words, the incidental emotions we study, whether positive or negative in valence, result in less cooperation than occurs under the Neutral treatment.

Our approach is novel in terms of method. In particular, to induce emotional states, we employ a new research tool, the use of immersive 360-degree videos shown in virtual reality. One commonly used traditional means of emotion induction is the use of film clips shown on a computer screen. It has been argued that the use of film clips as emotion-inducing stimuli is advantageous compared to showing still pictures, since the dynamic nature of films creates more realism (Dhaka and Kashyap, 2017). Film clips are typically regarded as an effective mood induction method (Westermann et al., 1996). A major advantage of film clips is that they can be used without explicit instructions that can tip participants off about the fact that the experimenter intends to induce a certain emotional state (Kuijsters et al., 2016).

Gomez et al. (2009) assess the persistence of different moods induced by film clips during a computerized task. They find that emotion induction via film clips still lasted after nine minutes. After that time interval, participants who had a negative emotional state induction report more negative emotional valence than those who had a positive induction. The results also suggest that induced changes in positive and negative emotional states are maintained throughout an intervening task. Murray et al. (1990), also found that neutral and positive moods induced with film clips were sustained after an intervening cognitive task on categorization of about 9 min. The effects of audio-visual emotion induction are presumably further reinforced when using 360-degree videos shown in virtual reality. Hence, we posit that the emotion induction via VR would last considerably longer than 9 min.

The use of virtual reality is potentially particularly valuable in inducing negative withdrawal emotions such as fear or disgust. This is because it is difficult to guarantee that individuals' attention is on aversive videos when they are shown in a conventional manner on computer screens, since it is possible to avert one's gaze. Looking away from the stimulus is not possible in a 360-degree video, in which the video appears in every direction[4]. The videos are shown with individually head-mounted Oculus Rift^{TM} gear to display 360-degree videos to subjects. Such videos create a fully immersive environment while simultaneously giving users full control of their angle of view in the pre-recorded footage[5]. Subjects are completely and inescapably surrounded by the audio-visual stimuli, minimizing their awareness of being in a physical laboratory environment. The video is filmed from the point of view of a participant in the video, rather than that of an observer. As a result, virtual reality presumably creates more powerful emotion induction than conventional techniques. The procedures were approved by the Institutional Review Board of the University of Arizona, and a representative of the IRB viewed each video prior to its use in any study.

The balance of the prior evidence is that positive emotional states are associated with more cooperation and negative emotions with more self-interested behavior. One possible mechanism for this effect is a preference for conditional cooperation (Fischbacher et al., 2001) coupled with the Affective Generalization Hypothesis proposed by Johnson and Tversky (1983). Under the Affective Generalization Hypothesis, positive emotional states lead to more optimistic beliefs, while negative states lead to pessimism. Thus, if one would like to cooperate only if others cooperate as well, a positive mood might make one have stronger beliefs that others will cooperate. This makes one more likely to cooperate as well. Similarly, one of the negative emotional states would make an individual less likely to cooperate than under a Neutral condition, by inducing more pessimistic beliefs. This hypothesized effect of happiness is line with the study of Drouvelis and Grosskopf (2016), who find that happiness leads to more cooperation, and the effect of negative emotions is consistent with Motro et al. (2016), who find that anger reduces cooperation. This account is plausible to us, and thus we posit that an emotion with positive valence, happiness, will result in higher contributions than the Neutral condition. We also hypothesize that the emotions with negative valence, fear and disgust, will result in lower contributions than the Neutral condition. Because the hypothesis is consistent with prior work, it can be viewed as a replication hypothesis, with the replication conceptual since we depart considerably from the procedures of the earlier studies.

> **Hypothesis 1:** Happiness will result in higher contributions than the Neutral condition, while Fear and Disgust will result in lower contributions than the Neutral condition.

Prior studies typically find that contributions decay over time (Andreoni, 1988; Isaac and Walker, 1988a; for a review see Chaudhuri, 2011). However, this prior work has not controlled for or induced emotional states. Thus, while it is not evident that the decline would be observed in each of our conditions, in the absence of any contradictory evidence, we hypothesize that:

[4]Fear and disgust are among the emotions that have proven to be reliably induced using movies (Kreibig et al., 2007; Rottenberg et al., 2007).

[5]Virtual reality has been previously employed in experimental economics to study trust (Kugler et al., 2020), the effect of peers on worker effort (Boensch et al., 2017), and the effect of being observed on honesty (Mol et al., 2020).

Hypothesis 2: Contributions decrease over time in all treatments.

This paper is structured as follows. Section 2 describes the experiment and presents the hypotheses. Section 3 reports the results and Section 4 contains a brief discussion.

EXPERIMENTAL DESIGN

All sessions of the experiment were conducted at the Economic Science Laboratory, located at Eller College of Management, University of Arizona, Tucson, Arizona, United States, in early 2018. All 141 participants in the study were University of Arizona undergraduate students, who self-enrolled for the experiment through the recruitment system of the laboratory. All participants were between 18 and 25 years old. The experiment was computerized using the Z-tree software package (Fischbacher, 2007) and conducted in English. The groups playing the game always consisted of either three or four participants. There was only one group participating in each session, due to the fact that the laboratory only had 4 VR headsets available[6]. There were 17, 19, 21, and 18 women in the Neutral, Happiness, Fear and Disgust treatments, respectively. There were 18, 17, 14, and 17 male participants in the four treatments.

The sample size was chosen based on calculations of statistical power. Our sample sizes in each treatment allow us to detect a medium-sized effect of d = 0.5 (Cohen, 1988) with a probability of at least of.665 if the hypothesis test of a treatment difference is one-sided. We conducted a sensitivity analysis using G-Power^{TM} to calculate the power to detect an effect size of 0.5 between each pair of treatments given the sample size in each treatment, using a *t*-test for independent sample means and applying $\alpha < 0.05$ as a standard of statistical significance. The power to detect an effect of d = 0.5 is 0.665 −0.670 depending on the pair of treatments being compared (the sample sizes in each treatment have slight differences). We have a power of 0.8 of detecting an effect of d = 0.596 −0.600, depending on the treatments being compared.

Virtual Reality technology (Oculus Rift headsets) was used to play the immersive 360-degree videos that were used for the emotion induction. The Economic Science Laboratory had previously conducted a validation study on the effectiveness of these particular videos in increasing the intended emotion without producing unintended emotions. The Neutral video was selected because it did not significantly increase the reported level of any emotion when it was viewed. See Medai and Noussair (2021) for the results for the happiness, fear and neutral videos and Kugler et al. (2020) for the disgust video[7].On the bases of these earlier manipulation checks, the videos were chosen for emotion induction in this experiment. Neutrality was induced with a video of a field of flowers. Fear was induced with a video in which the subject is walking on a tightrope across a steep canyon. The happiness video was one in which the subject was surfing in the tropics, and disgust was created with a video of disgusting things found in food. Each video was played for 5 - 6 min. The experimental design was between-subject. Each individual had only one emotion induced and all individuals in a session knew that they are watching the same video. They viewed the video simultaneously.

After the experimenter read the instructions for the game aloud, subjects played ten periods of the Voluntary Contributions Mechanism[8]. The four members of each group interacted repeatedly and anonymously for 10 periods. The specific parameters were the following. In each period, each participant received an initial endowment of 20 tokens referred to as "Experimental Currency Units" (ECU; with a conversion rate of 17 ECU = 1 $US). Players then simultaneously decided how to allocate the 20 tokens. A participant could contribute any number of tokens to a "project," which benefited all players equally and keep the remaining tokens for herself. The marginal per-capita return to the project equaled 0.5. In other words, each token contributed to the project yielded a payoff of 0.5 tokens to each of the four group members. Thus, if all players contributed their entire endowment to the project, each player would receive double the earnings that she would if they all contributed zero.

Specifically, the payoff function in each period was:

$$\pi_i = \left(20 - c_i + 0.5 * \sum_{j=1}^{n} c_j\right),$$

where π_i is individual *i*'s payoff and *i*'s contribution to the project is denoted by c_i. At the end of each period, participants were shown a summary screen that informed them of the sum of all contributions c_j to the project and their earnings for the period.

In the experiment, the game is finitely repeated. If the game is played once, the only Nash equilibrium is for all players to contribute zero. Thus, the only subgame perfect equilibrium of the 10-period finitely repeated game of our experiment is for all players to contribute zero in each of the ten periods, regardless of the history of play. As a result, each group member would earn 20 ECU (Experimental Currency Units) in each period. If each player would contribute her full endowment to the group project,

[6]Most sessions had four participants, and our intention was to have exactly four participants in each session. On three occasions, only three individuals appeared at the sessions, and we proceeded to conduct the sessions with the three participants present with the same MPCR in effect. These data are included in the analysis. Previous studies report mixed results on whether larger groups are more or less cooperative given the same MPCR. Isaac et al. (1994), Carpenter (2007), Diederich et al. (2016) and Pereda et al. (2019) report that cooperation is greater for larger groups. On the other hand, Isaac and Isaac and Walker (1988a), Capraro and Barcelo (2015), Feltovich and Grossman (2015), and Nosenzo et al. (2015) report ambiguous results regarding the effect of group size on cooperation. Excluding the three person groups does not affect the results with regard to statistical significance, with the exception that the difference in contributions in period 10 between the Neutral and Disgust treatments is borderline significant at $p = 0.054$ rather than at $p < 0.05$.

[7]In addition, we have recently (in late 2021) conducted two new manipulation checks of the Neutral, Fear and Happiness videos. These are reported in **Appendix B**, along with the results of a manipulation check for the Disgust video.

[8]The experimenter carefully read the instructions to the participants. After the instructions, subjects answered control questions to test their understanding of the rules of the experiment. See **Appendix A** for the instructions and the control questions. After all subjects finished the control questions, the experimenter checked their answers and to ensure correct understanding, explained the correct answers to any questions answered incorrectly privately to the individual. Then, the computerized experiment was initiated through launching of the Z-tree program.

the maximum feasible group payoff would be attained. In this case, each group member would earn 40 ECU each period. As indicated earlier, strong empirical evidence exists that individuals cooperate more than in the subgame perfect equilibrium, but also exhibit less than full cooperation. The level of cooperation declines over time.

At the end of each period, participants are informed of the group's total contribution and their own earnings, and are reminded of their own contribution. They are not informed about the individual contributions or the earnings of other group members. No communication between participants was possible. All periods counted towards participants' monetary payment. Earnings averaged $US15 per subject. The duration of the instructions was approximately 10 min followed by 5 minutes of play of the game. The data and all materials are available from the authors.

RESULTS

In this section, we present the results of our empirical investigation into whether emotional states influence an individual's contributions in a repeated linear public goods game[9]. Hypothesis 1 asserted that the positive emotional state of Happiness would enhance cooperation relative to Neutrality, while the two negative states, Fear and Disgust, would have the effect of reducing contributions. Our first finding, however, is the existence of quite a different pattern.

Result 1. There is no difference among treatments in the initial period. Inducing emotions has no statistically detectable effect on early game behavior. In the final rounds of the game, however, subjects in the Neutral condition contribute significantly more on average than subjects assigned to the Happy, Fear and Disgust conditions.

Figure 1 depicts the average per period contribution by treatment (Neutral, Happiness, Fear, Disgust). The data shown in the figure are the average individual single-period contribution in ECU in each emotion treatment. The figure reveals the following patterns. The overall results indicate that the emotion treatments, Fear (9.7 tokens), Happiness (10 tokens), and Disgust (10.3 tokens), all exhibit lower contributions in comparison to the Neutral condition (12.7 tokens). Subjects in the Neutral treatment contribute an average of 27.5% more than under the three emotion treatments.

[9]No treatments or observations are excluded from the analysis that we report here in this paper. All of the statistical tests that we have conducted are reported.

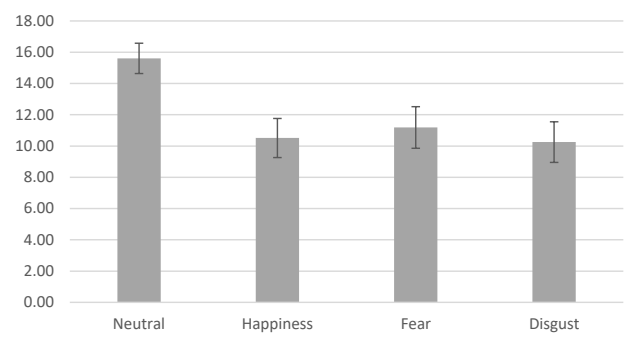

FIGURE 1 | Average contribution, by treatment. The figure shows the average per-period contribution by treatment (Neutral, Happiness, Fear, Disgust), for the pooled data from all participants. The error bars are 95% confidence intervals for the means. The range of possible contributions is from 0 to 20. All periods and all participants are included.

Table 1 considers whether the differences between treatments, in terms of average contribution, are significant. It reports the results from *t*-tests, conducted to determine whether the emotion treatments exhibit average contributions that are significantly different from each other. The tests are performed for the data from the first period, the last period, and for the ten periods overall.

Table 1 reveals a number of interesting patterns. Hypothesis 1 asserted that the emotions with negative valence, fear and disgust, would lead to lower contributions compared to the Neutral condition. Conversely, the emotion with a positive valence, happiness, would lead to higher contributions compared to neutrality. The tests reported in the table indicate no treatment differences at the outset of play or for the ten periods considered as a whole. However, by period 10, there is significantly lower cooperation in the three emotion treatments than in the Neutral condition, while the three emotion treatments do not differ from each other.

We next consider whether the decay of contributions with repetition of the game appears under each of our emotion conditions, as proposed in Hypothesis 2. Our findings are stated as Result 2.

Result 2. Contributions decline over time in the three emotion treatments, but not in the Neutral treatment.

Figure 2 below shows the average contribution made in each period in each of the four treatments. The data are averaged over all participants, separately for each treatment in which a given induced emotion was in effect. The average contribution in ECU, by period, is given on the vertical axis, and the period number is indicated on the horizontal axis for each treatment separately. The data in the three emotion treatments exhibit the following patterns. The average initial contributions are substantial, starting with a contribution of between 10 to 13 ECU out of a maximum of 20 in the first period, but decline as the game is repeated. A second pattern is that subjects contribute more

TABLE 1 | Results of *t*-tests of pairwise differences between treatments, for Period 1, Period 10, and all Periods 1 - 10.

Emotion treatments	Period 1	Average Period 1 – Period 10	Period 10
Neutral and Disgust	0.789	0.249	0.032**
Neutral and Fear	0.289	0.111	0.009***
Neutral and Happiness	0.829	0.381	0.045**
Happiness and Disgust	0.954	0.856	0.850
Happiness and Fear	0.420	0.569	0.593
Fear and Disgust	0.379	0.649	0.748

*This table shows the results of t- tests that were conducted to evaluate the impact of the treatment on average contributions. The entries in the table are t-statistics. ***p < 0.01; **p < 0.05; *p < 0.1. Significance levels are Bonferroni uncorrected.*

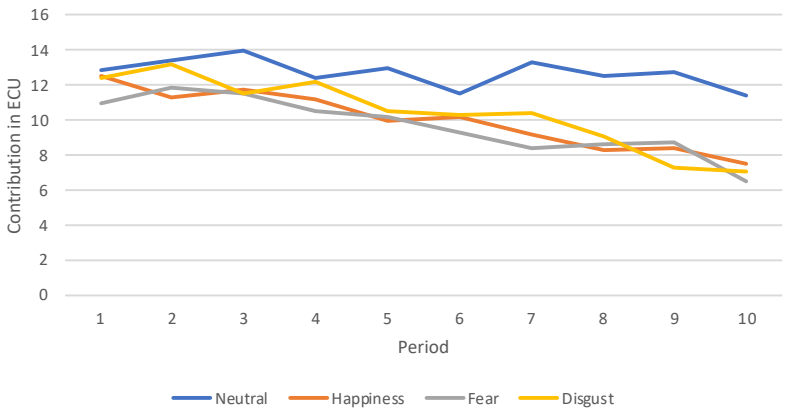

FIGURE 2 | Average contribution in each period, by treatment. The figure shows the average contribution made in each period in each of the four treatments (Neutral, Happiness, Fear, Disgust).

in the Neutral treatment than in all three emotion treatments throughout the ten-period horizon, with the gap increasing over time. Contributions in the three emotion treatments converge downward and remain similar to each other over time.

We now consider whether the declining time trend in contributions over time is significant by conducting signed rank tests. **Table 2** contains the results of signed-rank tests that were also conducted to evaluate the impact of the treatment on the change in average contribution between periods 1 and 10. The tests examine whether the distributions in periods 1 and 10 are significantly different from each other, using the sign and the ranking of the absolute magnitude of the change in the average contribution of a group between periods 1 and 10. Each group is treated as an observation, and there are 9 groups per treatment.

The data reveal several interesting findings. Hypothesis 2 asserted that contributions would decrease over time in all treatments. The results from the signed rank tests report some ambiguity in this regard. We find compelling evidence that cooperation declines over time for all three emotion treatments, but not for the Neutral treatment. For the Neutral treatment, we fail to reject the null hypothesis of no change. For the Happiness and Disgust treatments, we can reject the null hypothesis of no change over time at $p < 0.05$. For the Fear treatment, we reject the null hypothesis at $p < 0.1$. As we discuss later in Section 4, one possible, though speculative, explanation for why the change over time under Neutral is not significant is that the Neutral emotion induction suppresses subjects' integral emotions. In other words, when inducing neutrality, we are suppressing the emotions that would occur naturally in response to activity and outcomes in the game.

The regression estimates displayed in **Tables 3A,B** confirm the patterns that we have discussed above. The estimates are from the estimation of models assuming a random effect for each individual and robust standard errors. The dependent variable is individual *i*'s contribution in period *t*. The independent variables are treatment dummies and a time trend. One of the

TABLE 2 | Results of signed-rank tests of the change in average contribution between Periods 1 and 10.

Emotion treatment	Z	Prob > | z|
Neutral	− 0.713	0.476
Happiness	− 2.255	0.024
Fear	− 1.779	0.075
Disgust	− 2.196	0.028

This table shows the results of signed-rank tests that were conducted to evaluate whether the change in average contribution between periods 1 and 10 is significant in each treatment. P-values are not Bonferroni corrected.

TABLE 3A | The effect of treatment and period on the contribution of individual i.

Effect	Estimate	SE	95% CI		P
			LL	*UL*	
Random effects					
Constant	17.233	1.094	15.088	19.377	< 0.01
Happiness	−5.456	1.479	−8.354	−2.557	< 0.01
Fear	−6.314	1.540	−9.332	−3.296	< 0.01
Disgust	−3.895	1.559	−6.951	−0.839	< 0.05
Period	−0.527	0.067	−0.658	−0.396	< 0.01

Number of observations = 1,410, R^2 = 0.139.

TABLE 3B | The effect of treatment, period, and gender on the contribution of individual i.

Effect	Estimate	SE	95% CI		p
			LL	*UL*	
Random effects					
Constant	17.734	1.068	15.641	19.827	< 0.01
Happiness	−5.145	1.537	−8.158	−2.132	< 0.01
Fear	−5.924	1.598	−9.056	−2.792	< 0.01
Disgust	−3.823	1.524	−6.810	−0.836	< 0.05
Gender	−1.337	1.164	−3.618	0.944	> 0.1
Period	−0.527	0.067	−0.658	−0.396	< 0.01

Number of observations = 1,410, R^2 = 0.146.

two specifications includes gender as a regressor. The estimates show that relative to the Neutral treatment, which is the baseline category, the three emotion treatments yield lower contributions. The negative coefficient on the period variable indicates that contributions decline over time. The lack of significance on the gender variable indicates that neither men or women contributed systematically more or less than the other. The similarity of the significance levels and estimates under the two specifications shows that the inclusion of gender as a variable does not alter the effects of treatment and time period.

A number of authors have noted (see for example Fischbacher et al., 2001) that there are distinct types of players in the Voluntary Contributions Mechanism. To investigate the possible effects of emotions on the incidence of different behavioral types, we classify subjects into three types of players: free-riders, conditional cooperators, and altruists, according to how they respond to the prior contributions of other group members. Consider an estimated regression with the functional form:

$$c_i^t = \alpha + \beta c_{avg}^{t-1} + \varepsilon_i^t$$

where c_i^t is the contribution of Individual i in period t, and c_{avg}^{t-1} is the average contribution in the group in period t - 1.

Subjects are classified as 'free-riders' when their estimated $\alpha = 0$ and $\beta = 0$. This means that they contribute zero regardless of the past behavior of others. Subjects are considered 'conditional cooperators' when their $\beta > 0$, since they contribute more, the more cooperative the rest of their group was in the immediately preceding period. They are considered "altruists" when their estimated $\alpha > 0$ and $\beta = 0$. Altruists[10] contribute a positive amount that does not depend on the past decisions of others. Subjects who meet none of these criteria are grouped under a category called 'Other'. **Table 4** below reports the distribution of the behavioral types as a percentage of all participants in each treatment.[11]

In Public Good Games, it is commonly found that a plurality of participants behave as "conditional cooperators," i.e., people who are willing to contribute more if others contribute more as well (Fischbacher et al., 2001; Chaudhuri and Paichayontvijit, 2006). Our results from **Table 4** confirm these findings. Furthermore, our results indicate that compared to the emotion treatments, the Neutral treatment has a greater proportion of altruists, and this appears to be associated with the absence of a decline of contributions in that treatment. There are more altruists in the Happiness than in the Fear and Disgust conditions. The Fear condition has the most conditional cooperators. Remarkably, free riders are completely absent in the Neutral treatment.

DISCUSSION

In this study, we applied a new emotion induction methodology, 360-degree videos shown in Virtual Reality, to study a fundamental question in the social sciences. Does an individual's emotional state, specifically happiness, disgust or fear, have an effect on the individual's tendency to cooperate? This study is an example of how emerging technologies can create new ways of conducting research in experimental economics. Technologies such as Virtual Reality can serve as useful complementary tools to existing emotion analysis and induction methods. While no one study can be definitive, and our sample is relatively small, we draw two conclusions from our findings.

The first conclusion is that *incidental emotions, whether positive or negative in valence, result in lower contributions compared to a Neutral state*. Our results indicate that on average, subjects contributed 27.5% less in the three emotion treatments, Fear, Happiness and Disgust, than they did in the Neutral condition and the differences are significant in later periods. More than one third of the subjects were classified as altruistic in the Neutral condition, which was 2 to 12 times the number of altruists in the three emotion treatments. The part of Hypothesis 1 that is supported is that negative emotions, Fear and Disgust, decrease contributions. The other part of Hypothesis 1, that positive emotions increase contributions, is not supported. Of course, this is only one study and future studies will allow for refinements of the results. In particular, they may establish whether some of the effects of emotions on behavior are too small to be detected with the number of participants we have employed.

[10]The definition of altruism that we employ here is behavior that reduces an individual's own payoff but raises the group's overall payoff, which is the effect of a contribution in the Public Good game. This behavior is often described as cooperativeness rather than altruism. In the Social Value Orientation literature (see for example Murphy et al., 2011) there is a clear distinction drawn between Altruism (maximizing the payoff of other individuals) and Cooperativeness (maximizing the income of the group). Our notion of altruism corresponds to the latter.

[11]We conducted Chi-squared tests comparing the distribution of the four categories: Free-riders, Altruists, Conditional cooperators, and Others in each pair of treatments. The distributions are significantly different at $p < 0.01$ for all pairs of treatments except for Fear vs. Disgust, which is not significant at $p < 0.05$.

TABLE 4 | Classification of participants into behavioral types.

Behavioral type	Neutral	Happiness	Fear	Disgust
Free-riders	0%	5.6%	2.9%	5.7%
Conditional Cooperators	51.4%	58.3%	74.3%	65.7%
Altruists	34.3%	16.7%	2.9%	5.7%
Other	14.3%	19.4%	20%	22.9%
Total observations	35	36	35	35

The table reports the distribution of behavioral types by treatment as percentages of the participants in the treatment. Subjects are classified as free-riders when $\alpha = 0$ and $\beta = 0$. Subjects are considered conditional cooperators when $\beta > 0$, and altruists when $\alpha > 0$ and $\beta = 0$.

The second conclusion is that we confirm that contributions decrease over time in all of the induced emotion treatments, but that they do not do so in the Neutral treatment. Thus, while Hypothesis 2, that contributions would decrease over time, is mostly supported, there is an important exception. The fact that our Neutral treatment does not exhibit the typical empirical pattern observed in prior studies is interesting. This suggests that inducing a Neutral emotion is not the same thing as not inducing an emotion at all. Hence, we propose the following conjecture*: Emotions are linked to a decrease in contributions in the Public Good game, perhaps because they lead to reciprocation of the behavior of other players. The Neutral treatment attenuates the decrease in cooperation by suppressing these emotions.* This last statement is certainly speculative, and further work focused directly on this mechanism would be required to evaluate the validity of this conjecture.

The differences that we observe among treatments do not appear immediately but open up after several periods of play. Thus, the emotions do not affect initial behavior, but interact with the dynamics of play to produce different outcomes in the Neutral treatment. In a standard Public Good game with no emotion induction, cooperation begins at an intermediate level and then declines over time. This dynamic pattern is also present in our Fear, Disgust, and Happiness treatments. In the Neutral treatment, the dynamics are affected by the Neutrality induction. We have seen that in the Neutral treatment, we do not observe pure free-riding. If the decline in cooperation over time that is typically observed is due to conditional cooperators responding to free-riding by lowering their own contributions, the lack of free-riders in the Neutral treatment eliminates this dynamic that generates the declining time trend.

There have been many studies studying the effects of emotions by means of emotion induction and this work has produced numerous valuable findings to aid our understanding of the relation between emotions and economic behavior. See for example the surveys by Baumeister et al. (2009), Izard (2009) and Lerner et al. (2016). In our opinion, a line of research using *emotion suppression* would also be beneficial in uncovering the role of emotions in behavior. Prior research on emotions and decision making has not considered, to our knowledge, whether a neutral emotion induction has a different effect from no emotion induction at all. It is not clear to us after conducting this study that Neutrality is in any sense a default emotion. A future avenue for study would therefore be to further investigate the particular effects of Neutrality. What does Neutrality really do? Does it cause people to behave differently in different tasks than they would behave otherwise? When does it do so? In our view, such a line of inquiry promises to yield very valuable insights.

There are several limitations to our study. The session size was limited by the number of VR headsets that we had available. The level of anonymity, while lower than it would be in a larger session, was the same among the treatments. It was also similar to the level that would exist in some workplace settings, where individuals might know who the other group members are, but cannot observe their specific actions. We recognize, however, that there may be an interaction effect on behavior between a lack of anonymity and emotional state. However, this would be an equal concern for any level of anonymity, and it is possible that the relationship between emotions and cooperation could differ at other levels of anonymity. The size of the sample was modest, our study was not preregistered, and we do not correct for multiple hypothesis testing, so our study can be considered as an initial exploration. Another limitation is that, although the individuals who participated in the manipulation checks for the videos were drawn from the same subject pool, the checks were conducted at different times and on different individuals than those who participated in the main experiment. Future research is needed to confirm our results.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

This study involving human participants were reviewed and approved by University of Arizona Institutional Review Board. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

Both authors listed have made a substantial, direct, and intellectual contribution to the work, and approved it for publication.

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APPENDIX A | EXPERIMENTAL INSTRUCTIONS

In this part of the experiment, your earnings will be calculated in ECU (Experimental Currency Units). At the end of the experiment the total amount of ECU you have earned will be converted to Dollars at the following rate:

$$17 ECU = \$1$$

This part of the experiment is divided into 10 periods. All rounds will count for payment. You will be in a group with the 3 other participants.

Detailed Instructions

At the beginning of each period each participant receives 20 ECU. In the following we call this his or her endowment. Your task is to decide how to use your endowment.

You have to decide how many of the 20 ECU you want to:

- Contribute to a project and;
- How many of them to keep for yourself.

We will play this game on the computer. After choosing your contribution you must press the OK button. Once you have done this, your decision can no longer be revised.

Once all members of your group have made their decision, your screen will show you the total amount of ECU contributed to the project by each of the four group members (including your contribution). This screen shows you how many ECU you have earned.

Your income consists of two parts:

Part (1) The ECU you kept for yourself
Part (2) The income from the project = 50 percent of the total contribution of all 4 group members to the project (including your own contribution).

Your income in ECU in each period:

= Part 1 + Part 2
= (20 - your contribution to the project) + 0.5*(total contributions to the project)

The income of each group member is calculated in the same way, this means that each group member receives the same income from the project.

Income part (1) The ECU you kept for yourself

For each ECU that you keep for yourself you earn an income of 1 ECU.

Income part (2) The income from the project

For every ECU you contribute to the project instead, the total contribution rises by one ECU. Your income from the project would rise by 0.5*1 = 0.5 ECU. However, the income of the other group members would also rise by 0.5 ECU each, so that the total income of the group from the project would rise by 2 ECU.

Your contribution to the project therefore also raises the income of the other group members. On the other hand you earn an income for each ECU contributed by the other members to the project. For each ECU contributed by any member of the group you earn 0.5*1 = 0.5 ECU.

For example, suppose the total of the contributions of all group members is 60 ECU. In this case each member of the group receives an income from the project of 0.5*60 = 30 ECU.

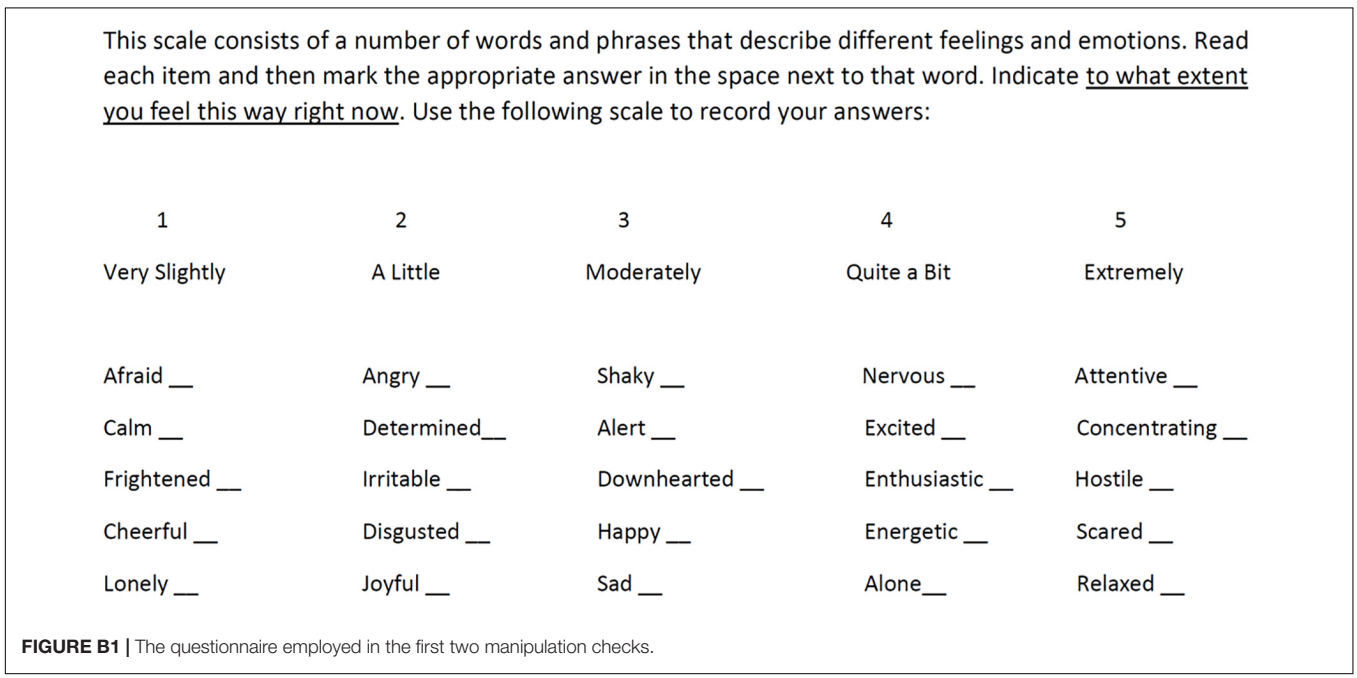
To check your understanding of the experiment, please answer the following questions:

(1) Suppose each group member has an endowment of 20 ECU. Nobody (including yourself) contributes any ECU to the project. How high is:
   (a) Your income for the period? _____
   (b) The income for each of the other group members for the period? _____
(2) Suppose each group member has an endowment of 20 ECU. You contribute 20 ECU to the project. All other group members contribute 20 ECU to the project.
   (a) What is your income for the period? _____
   (b) The income for each of the other group members for the period? _____

(3) Suppose each group member has an endowment of 20 ECU. The other three group members contribute a total of 30 ECU to the project.
   (a) What is your income if you contribute 0 ECU to the project? _____
   (b) What is your income if you contribute 15 ECU to the project? _____
(4) Suppose each group member has an endowment of 20 ECU. You contribute 8 ECU to the project.
   (a) What is your income if the other group members together contribute a total of 7 ECU to the project? _____
   (b) What is your income if the other group members together contribute a total of 22 ECU to the project? _____

APPENDIX B | MANIPULATION CHECK

In this appendix we report the results from three different manipulation checks of the videos we used to induce emotions. In separate sessions from those of the main study described above, subjects viewed one of the four videos used in this study. In the first two manipulation checks, they subsequently reported the strength, on a scale of 1 - 5, that they felt each of the following emotions indicated on the form shown in **Figure B1**. The questionnaire items are drawn from the PANAS-X survey (Watson and Clark, 1994). The subjects were undergraduate students at the University of Arizona, the same pool of participants that did the main study reported in the paper. The study was conducted in October and November, 2021.


This scale consists of a number of words and phrases that describe different feelings and emotions. Read each item and then mark the appropriate answer in the space next to that word. Indicate to what extent you feel this way right now. Use the following scale to record your answers:

1	2	3	4	5
Very Slightly	A Little	Moderately	Quite a Bit	Extremely
Afraid __	Angry __	Shaky __	Nervous __	Attentive __
Calm __	Determined__	Alert __	Excited __	Concentrating __
Frightened __	Irritable __	Downhearted __	Enthusiastic __	Hostile __
Cheerful __	Disgusted __	Happy __	Energetic __	Scared __
Lonely __	Joyful __	Sad __	Alone__	Relaxed __

FIGURE B1 | The questionnaire employed in the first two manipulation checks.

From the items in the above questionnaire, the following indices were constructed:
Joviality = Average (Cheerful, Joyful, Happy, Excited, Enthusiastic, Energetic)
Fear = Average (Afraid, Frightened, Nervous, Scared)
Hostility = Average (Angry, Irritable, Disgusted, Hostile)
Sadness = Average (Lonely, Downhearted, Sad, Alone)
Attentiveness = Average (Determined, Alert, Attentive, Concentrating).

In the first manipulation check study, there were 48 participants, and 16 viewed each video. They completed the questionnaire above both before and after viewing the video. The average value and standard deviation of each index after viewing each video is reported in **Table B1**. The table shows that the Happy video significantly increases the level of joviality, while lowering the amount of fear and hostility the average person reports. The Fear video increases the reported level of fear without significantly affecting any of the other four indices. The Neutral video does not increase any of the emotions, though it lowers both hostility and attentiveness.

The second manipulation check study had 108 participants and was conducted in September – November 2021. Participants each viewed one of the three videos and completed the questionnaire shown in **Figure B1** afterwards. The average responses are shown in **Table B2**. The superscript *a* indicates that the average of the emotional index was significantly different after viewing the indicated

TABLE B1 | Average value of emotional indices before and after Happy, Neutral, and Fear videos, manipulation check #1.

Video		Index				
		Joviality	**Fear**	**Hostility**	**Sadness**	**Attentiveness**
Neutral	Before	2.84	1.45	1.47	1.47	3.59
(*N* = 16)	Video	(0.95)	(0.66)	(0.59)	(0.54)	(1.02)
	After	2.80	1.36	1.17*	1.42	3.14**
	Video	(1.08)	(0.56)	(0.24)	(0.60)	(1.23)
Happy	Before	2.75	1.66	1.39	1.52	3.88
(*N* = 16)	Video	(0.97)	(0.74)	(0.54)	(0.41)	(0.81)
	After	3.35**	1.28**	1.02**	1.34	3.48*
	Video	(0.84)	(0.40)	(0.06)	(0.43)	(0.71)
Fear	Before	2.58	1.63	1.38	1.44	3.34
(*N* = 16)	Video	(0.89)	(0.60)	(0.47)	(0.66)	(0.86)
	After	2.54	2.15*	1.41	1.42	2.98
	Video	(1.07)	(1.08)	(0.60)	(0.66)	(1.16)

**: value of index significantly different before and after viewing the video at $p < 0.05$ according to t-test. **: before and after significantly different at $p < 0.01$.*

TABLE B2 | Average value of emotional indices after Neutral, Happy, and Fear videos, manipulation check #2.

Video		Index				
		Joviality	**Fear**	**Hostility**	**Sadness**	**Attentiveness**
Neutral	After	2.92[a]	1.40[d]	1.21	1.39	2.95
N = 40	Video	(0.95)	(0.81)	(0.53)	(0.50)	(0.92)
Happy	After	3.27[a]	1.23[d]	1.16	1.43	3.12
N = 35	Video	(1.06)	(0.31)	(0.30)	(0.56)	(1.05)
Fear	After	2.47[a]	2.63[a]	1.48[c]	1.85[a]	3.29
N = 33	Video	(1.07)	(1.11)	(0.59)	(1.07)	(0.83)

a: significantly different from both other videos. b: significantly different from Neutral video only. c: significantly different from Happy video only. d: significantly different from Fear video only. All significance thresholds are $p < 0.05$.

TABLE B3 | Average value of emotional indices before and after Neutral and Disgust videos, manipulation check #3.

Video	Index				
	Happiness	**Fear**	**Anger**	**Disgust**	**Sadness**
Before any video	2.52[d]	1.62[a]	1.37[a]	1.08[d]	1.65[a]
N = 25	(0.59)	(0.69)	(0.46)	(0.12)	(0.72)
Neutral	2.75[d]	1.28[a]	1.13[a]	1.09[d]	1.23[b]
N = 14	(0.54)	(0.34)	(.21)	(0.15)	(0.25)
Disgust	1.59[a]	1.82[a]	1.84[a]	3.27[a]	1.32[b]
N = 11	(0.78)	(0.77)	(0.91)	(1.25)	(0.39)

a: significantly different from both other conditions. b: significantly different from the level before video is shown only. c: significantly different from Neutral video only. d: significantly different from Disgust video only. All significance thresholds are $p < 0.05$.

video than the other two videos at $p < 0.05$. The superscripts *b, c, and d* indicates that the average value of the index after the video is significantly different from after exactly one of the other two videos.

The data in **Table B2** shows that the Happy video generates a higher degree of Joviality than the Neutral treatment or the Fear treatment, but there are no significant differences in the other indices other than leading to lower fear than the Fear video. The Fear video has significantly higher fear than the other two treatments, though it also leads to greater sadness than the other two videos. The only two effects that are consistent over both manipulation checks 1 and 2 are that the Happy video increases Happiness and the Fear video leads to greater fear.

The third manipulation check covers the Disgust and the Neutral videos. In an earlier study, Kugler et al. (2020), reported the results of a manipulation check of the same videos that we used to induce neutrality and disgust with 25 members of the same subject pool that was employed in our study, undergraduate students at the University of Arizona. The PANAS-X protocol was used to measure

emotional states both before and after the Neutrality and the Disgust videos. **Table B3** below reports the average values of the indices given above for Joviality, Fear, Hostility and Sadness, as well as for Disgust (which was not measured in the data provided above).

Comparison of the data before any video is shown and the after the neutral video is viewed reveals the following pattern. The neutral video yields an emotional state that is similar to that present before the video with regard to Disgust and Happiness, but it lowers Fear, Anger, and Sadness. The Disgust video has a significantly higher level of disgust than before any video is shown, but does not change any other emotion significantly. The Neutral and Disgust videos yield different levels of Happiness, Fear, Anger and Disgust from each other, with the largest being the difference in Disgust.

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frontiers in Psychology

ORIGINAL RESEARCH
published: 18 March 2022
doi: 10.3389/fpsyg.2022.800528

Set Size and Donation Behavior

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Choice overload is the phenomenon that increasing the number of options in an assortment makes choosing between options more difficult, sometimes leading to avoidance of making a choice. In this pre-registered online experiment ($N = 501$), choice overload was tested in a charitable behavior context, where participants faced a monetary donation choice. Charity organization assortment size was varied between groups, ranging between 2 and 80 options. The results indicate that there were no meaningful differences in donation likelihood between the 16 organization assortment sizes, neither for individuals with high preference certainty nor for individuals with uncertain preferences among charitable causes. Having more charitable organizations to choose from did not affect donation behavior.

Keywords: charitable giving, donation behavior, choice overload, choice architecture, deferral

OPEN ACCESS

Edited by:
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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *23 October 2021*
Accepted: *28 February 2022*
Published: *18 March 2022*

Citation:
Lindkvist AM and Luke TJ (2022) Set Size and Donation Behavior. Front. Psychol. 13:800528. doi: 10.3389/fpsyg.2022.800528

INTRODUCTION

Information about philanthropic efforts have become increasingly easy to find. With just a quick search you can find websites of a multitude of charitable organizations focusing on important causes. In addition, there are numerous charity evaluation sites which list and rank organizations (such as animalcharityevaluators.org, charitynavigator.org, and givewell.org). A donation to a preferred organization can be made from the comfort of one's screen in a matter of minutes. In theory, this increased accessibility ought to increase the number of people who contribute to philanthropic causes. However, the abundance of available organizations may be experienced as overwhelming. Reaching a decision about where to donate may have become increasingly difficult—resulting in inertia instead of increased philanthropic action. We tested this proposition experimentally in the present study.

If humans were able to weigh all the relevant information to make the best possible choice, an increase in the number of options ought to help decision makers (Chernev et al., 2015). Having a large assortment to choose from should increase the possibility of finding an option in line with what one is looking for. However, humans make decisions within the boundaries set by their available cognitive resources (Gigerenzer and Selten, 2002). These cognitive boundaries can lead to a larger assortment impeding, as opposed to facilitating, the choice process. The result of this *choice overload* is that decision makers are worse off in a number of ways when they have to decide between many options in an assortment (i.e., a large set size) compared to when choosing from a small assortment. Commonly measured outcomes of choice overload can be divided to outcomes capturing the subjective state of the decision maker (e.g., choice satisfaction, decision regret, and decision confidence) and behavioral outcomes (e.g., likelihood of deferring the choice, likelihood of switching to another option, and which option or assortment is selected from the available alternatives; see Chernev et al., 2015 for a conceptual model). Choice overload has gained a lot of recognition over the past two decades and has been studied extensively. Meta-analytic estimates of choice overload have

resulted in mixed conclusions about how robust the effect is. Scheibehenne et al. (2010) found a meta-analytic effect size of virtually zero across studies, with a substantial degree of heterogeneity between studies. They concluded that choice overload effect is not very robust and likely relies on study-specific preconditions (such as whether participants have prior preferences or expertise) as well as whether and when studies were published. Simonsohn et al. (2014) performed p-curve analyses on the same dataset to separately evaluate the evidential value of results in support for the choice overload effect and results in support of the opposite effect (i.e., having more options facilitates choosing). Based on the distribution of significant *p*-values, Simonsohn et al. (2014) concluded that the results showing a choice overload effect lack collective evidential value. In a subsequent review and meta-analysis, Chernev et al. (2015) outlined a conceptual model of choice overload, describing four potential moderators (choice set complexity, decision task difficulty, preference uncertainty, and decision goal). Chernev et al. (2015) found support for the moderating role of these four factors across studies, leading to the conclusion that choice overload effect reliably occurs when any of these factors are at high levels. McShane and Böckenholt (2018) reexamined the same dataset as Chernev et al. (2015), to assess whether and how the effect of the four moderators differs depending on the outcome measure. They found that for some outcome measures, the choice overload effect is reversed at low levels of the moderators, while for other outcome measures there is no discernable effect (i.e., the effect is attenuated) at low levels of the moderators. Additionally, McShane and Böckenholt (2018) comment on how future studies estimating choice overload ought to carefully measure choice deferral (i.e., choosing not to choose or postponing the choice) in relation to the relevant moderating factors, as there was much larger variation in this outcome than the other measured outcomes. As such, their meta-analysis of the current body of literature likely does not give a clear estimate of under what conditions choice overload in the form of deferring the choice occurs.

Most research on choice overload has been focused on the effect of set size on choices between consumer goods, such as foods (Iyengar and Lepper, 2000; Chernev, 2003a,b; Sela et al., 2009; Townsend and Kahn, 2014) or electric appliances (Gourville and Soman, 2005; Sela et al., 2009; Diehl and Poynor, 2010; Greifeneder et al., 2010; Inbar et al., 2011). However, the effect has also been tested in the prosocial context of choosing between different charity organizations (non-governmental organizations, NGOs). These studies have reached mixed conclusions regarding the relationship between NGO set size and charitable behavior. The results from one study, focusing on volunteering behavior, suggest that the choice overload effect is generalizable to prosocial choice scenarios (Carroll et al., 2011). However, there are contradictory findings, suggesting that larger set sizes lead to increased donations (Soyer and Hogarth, 2011) and that set size does not have a robust effect on donation behavior, but might have an effect when individuals are required to justify their choice (Scheibehenne et al., 2009).

Directly comparing these apparently contradictory results is difficult since the studies vary in what set sizes are operationalized as large and small. There does not appear to be clear guidance for what set sizes to use when testing the effect of assortment size on charitable decisions. The cut-offs used for consumer goods may not be transferable to donation choices. For instance, there may be important differences between choosing a product vs. choosing an experience as well as between spending money on oneself vs. spending money on others (Polman, 2012; Shaddy et al., 2021). The lack of clear theoretical guidance for what qualifies as too large a set size in specific decision contexts is especially important to consider given previous results suggesting that the relationship between set size and choice outcomes is non-linear, following the shape of an inverted U (Shah and Wolford, 2007; Reutskaja and Hogarth, 2009; Park and Jang, 2013). These results suggest that an increased set size facilitates choosing up to a certain point, after which increases in set size have the opposite effect. Given these prior results, comparing only a few set sizes risks only partially capturing the relationship between set size and choice, making it difficult to draw accurate inferences about the full nature of the relationship. This is an especially large problem for choice domains, where there is a lack of guidance from prior studies regarding what assortment set sizes should be considered large and small. To better understand the boundary conditions for choice overload, it is necessary to test whether this inverted U-shaped relationship extends to other choice domains than consumption choices (such as prosocial choices). Herzenstein et al. (2020) set out to estimate the shape of the relationship between set size and prosocial choice and consistently found results indicating a U-shaped relationship (i.e., a pattern that is opposite to the results mentioned above). These surprising results, indicating that set size might have the opposite effect on prosocial choices than on consumption choices, ought to be confirmed with further research. Establishing the shape of the choice overload effect in different choice domains is useful for two reasons. First, this may provide practically useful estimates of which set sizes are beneficial for the decision maker and which are overwhelming, for different option categories. Second, this will allow for a more nuanced interpretation of results found in previous studies reporting a failure to detect a choice overload effect.

Relatedly, a choice from a large assortment might be perceived differently between individuals, depending on whether they have strong prior conceptions about the options or the category of options. Preference uncertainty, one of the suggested moderating factors of choice overload, has been defined either as a lack of expertise about the option category or as the lack of an available articulated ideal point from which to evaluate the options (Chernev et al., 2015). Expertise has been suggested to allow for a narrower, more detailed processing of stimuli (Rota and Zellner, 2007). Experts, in contrast to novices, make comparisons within a smaller selection and therefore need to make fewer trade-offs. In the context of charitable donations, an individual with substantial expertise about NGOs may be able to easily categorize organizations along certain attributes (e.g., NGOs with low overhead costs), without being explicitly

given this information. The individual with high expertise can then make their selection from within these smaller categories based on their preferences. Having an articulated ideal point means that the decision maker has clear preferences for how to prioritize between attributes when making trade-offs between options within a specific category (Chernev et al., 2015). This allows the decision maker to quickly sort out options that do not have preferred levels on different attributes. When choosing where to donate money, an individual with an articulated preference for NGOs focusing on mitigating climate change may try to decide between the available options that fulfil this criterion and not consider organizations focused on other causes. Due to comparing within a smaller selection, individuals with more certain preferences are less susceptible to the cognitive strain of facing a large assortment of options. Scheibehenne et al. (2009) as well as Soyer and Hogarth (2011) either measured or manipulated prior knowledge of NGOs (which can be viewed as a form of expertise). Their results suggest that people may be more likely to donate to well-known NGOs (Scheibehenne et al., 2009) and that these organizations received a larger proportion of the allocated donations than unknown NGOs did (Soyer and Hogarth, 2011). However, neither Scheibehenne et al. (2009) nor Soyer and Hogarth (2011) address potential interaction effects between set size and prior knowledge on donation behavior. As such, further research is needed to determine the potential moderating role of preference uncertainty on the choice overload effect in donation contexts.

With the present study, we aimed to provide further insights into the relationship between charity organization set size and charitable behavior. The aim of the study was to provide a more complete model of the relationship between set size and donation choice as well as to examine boundary conditions based on individual differences in preference certainty. The following pre-registered hypotheses[1] were tested experimentally:

> *Hypothesis 1*: Increasing the set size will lead to a lower donation proportion.
>
> *Hypothesis 2*: This relationship will follow a quadratic function, with donation proportion increasing until a certain set size and then decreasing as set size increases from that point.
>
> *Hypothesis 3*: The relationship between set size and donation proportion will be moderated by preference uncertainty, so that the negative effect of set size on donation proportion will be weaker or non-existent for individuals with higher preference certainty.[2]

[1]https://osf.io/mk245

[2]In our pre-registration, Hypothesis 3 was phrased as "The relationship between set size and donation proportion will be moderated by preference uncertainty, so that people with certain preferences will be more likely to donate when faced with a larger set size than people with uncertain preferences." The phrasing has been altered here to better illustrate the expected pattern of the hypothesized moderation.

MATERIALS AND METHODS

Participants

The sample ($N = 501$) was recruited through the online survey panel Prolific. A requested sample size of 500 participants was set based on a power analysis using a Monte Carlo simulation approach and inference criteria of $\alpha = 0.05$. The requested sample size of 500 participants gives over 80% power to detect interaction effects between set size and preference certainty ranging between log(OR) = 0.10 and 0.20, given plausible combinations of individual coefficients for set size and preference certainty. More details on the power analysis are available in the pre-registration document (https://osf.io/6fr8d/). Participant recruitment was set to be automatically stopped by Prolific when the requested number of participants was reached. One additional participant was miss-specified by Prolific as unfinished and manually approved prior to data extraction, resulting in the final sample of 501. The following pre-screening criteria were set up in Prolific: participants had to be fluent in English, have completed at least 50 prior submissions on Prolific, and have an acceptance rate of at least 95% on their total previous submissions. In addition, Prolific users who entered the survey on other devices than a desktop/laptop computer or who had participated in our pilot study were not able to participate. Only completed survey submissions were included into the sample.

The sample had a mean age of 28.2 ($SD = 9.6$, median = 25, range = 18–80) and a gender distribution with 63.5% males, 36.1% females, and <0.5% non-binary or unwilling to specify. The sample was predominantly European, with 92.2% reporting a European country as their current country of residence, while 88.2% reported a European country as their nationality. The most frequently reported countries of residence were Poland (21.0%), Portugal (17.8%), the United Kingdom (11.4%), and Italy (11.2%). As for current occupation, 49.5% reported being employed or self-employed, while 36.5% reported studying as their main occupation.

Survey Procedure

The study recruitment page specified that the study would be about helping and that a payment of £0.38 would be given for completed responses. After receiving instructions and providing informed consent, participants were asked to answer how often they donate to charity. The next page of the survey asked participants to rate different charity causes (these ratings make up the preference certainty score). On the following page of the survey, participants faced a monetary donation choice. After making their choice, participants answered two questions about the choice on the subsequent page. On the last page of the survey, participants were asked to fill in demographic information about themselves as well as potential comments on the survey.

Survey Materials

The study was set up in the online survey platform Qualtrics. Below follows a description of the set size manipulation and

all measures included in the survey. The full Qualtrics setup is available at the OSF project page.[3]

Set Size Manipulation

Set size was manipulated as a continuous between-groups factor, with 16 levels (2, 3, 4, 5, 6, 7, 8, 9, 10, 20, 30, 40, 50, 60, 70, and 80). This continuous manipulation was set up to allow for modeling a non-linear relationship between set size and donation behavior. The upper limit for the set size factor was set at 80 as this was the largest set size found in prior studies looking at the relationship between NGO set size and donation choice (Scheibehenne et al., 2009). Participants were randomly allocated to one of the 16 set size conditions and were blind to this manipulation.

Preference Certainty Measure

Participants were asked to rate how important five different charitable cause areas were to them on a slider scale ranging from 0 (=Not at all important) to 100 (=Extremely important). The five cause areas were (1) Medical prevention and treatment, (2) Access to food, nutrients, and clean water, (3) Alleviating poverty, economic empowerment, (4) Animal welfare and rights, and (5) Environmental protection and conservation. The causes were presented in a randomized order to eliminate order-effects. These ratings were then used to compute an individual preference certainty score.[4]

The preference certainty score was based on the ideal point availability measurement used by Chernev (2003b) (Experiment 3). Chernev (2003b) asked participants to rate the importance of a set of product attributes. These ratings were then used to form a difference score, based on the difference in rating between the highest rated (i.e., the most attractive) attribute and the second highest rated attribute. Participants with high difference scores were labeled as having an articulated ideal point. The preference certainty score used in the present study differs in two central ways from the measurement used by Chernev (2003b). Firstly, participants only rated the importance of one attribute (the organization cause area) instead of multiple attributes. Secondly, the ratings were computed into the preference certainty score based on the variability (SD) in rating between all the attribute levels (the five cause areas). We made this change based on results from pilot testing the scale and finding that a score based on the variability between ratings was a better representation of the pattern of scores suggesting articulated preferences between charity causes than a score based on differences in rating between the two highest rated causes.

The preference certainty score was used as a measure of the extent to which each participant perceived the causes as varying in importance. Higher scores were interpreted as the participant having more certain preferences between the five charity causes. The indexed score was analyzed as a continuous measure, with a possible range from 0 to approximately 55.

[3]https://osf.io/jbprn/

[4]The term preference certainty is used throughout the rest of this paper, instead of the commonly used negative phrasing of preference uncertainty (used by, e.g., Chernev et al., 2015; McShane and Böckenholt, 2018).

Donation Choice Measure

After finishing the preference certainty rating, participants were told that they were eligible to receive a bonus payment of £0.25 for their participation. They were told that they could either keep the bonus or donate it to one of the charity organizations presented to them. Participants saw a number of charitable organizations corresponding to the set size condition they had been randomized to. The organizations were presented in a grid format, with five columns. The cause category of each organization was displayed below the NGO's name. To better illustrate the choice setup that participants faced, we have uploaded animations showing what the survey page could look like for participants presented with the smallest set size (two NGOs)[5] and the largest set size (80 NGOs).[6]

The charity organizations were drawn randomly from a pool of 80 real NGOs, containing 16 organizations for each of the five cause areas described above. A full list of the organizations, their cause area, and what source they were gathered from is available at https://osf.io/2gxhc/. By mistake, one organization (Conservation Strategy Fund) was entered twice into the pool. Due to this mistake, some participants in the conditions with larger set sizes (seeing 20 or more NGOs) were presented with the set size they were assigned to but containing one less unique option than intended. This issue will be further addressed in the results section.

Only organizations which we judged to be relatively unknown were included in the organization pool. In addition, we took care to exclude organizations with connotations to specific parts of the world or well-known individuals.[7] This was done in order to minimize the risk of including a clearly dominant option into the assortment, as the inclusion of a dominant option is a suggested moderator of the choice overload effect (Chernev et al., 2015; McShane and Böckenholt, 2018).

To donate their bonus, participants were told to select one of the presented organizations before continuing to the next page. A selection of either of the presented NGOs was coded as 1 = donated. To keep the bonus for themselves, participants were told to either select the option "Keep the bonus" (which was always presented as the last option) or to not select any option and simply move to the next page. Both responses were coded as 0 = did not donate. Regardless of what choice they made, all participants had to scroll to the bottom of the donation choice page to continue to the next page of the study. As such, all participants saw the full set size they were presented with before they left the page.

Additional Measures

In the beginning of the survey, participants were asked how often they donate to charity, with five response options ("Never," "Sporadically," "Every year," "Several times per year," and

[5]https://osf.io/m6k92/

[6]https://osf.io/h2dum/

[7]In the pilot study, where 30 participants faced a random assortment of 30 NGOs drawn from this pool of 80 NGOs, 73.3% stated either that they had not heard about any of the organizations before or that they were not sure whether they had heard about them.

"Every month"). After choosing whether or not to donate, participants were asked two questions about the choice they made. They were asked whether they searched for more information about any of the listed organizations while making their choice, with three response options ("No," "Yes, one organization," and "Yes, several organizations"). They were also asked whether they had heard about any of the listed organizations before, with four response options ("No," "I'm not sure," "Yes, one organization," and "Yes, several organizations"). These three items, as well as a measure of time duration for the donation choice, were included to provide a better understanding of the choice through exploratory analyses.

In addition, participants were asked to answer demographic questions regarding their age, gender, occupational status, and the number of surveys they complete on Prolific per day. Additional demographic data for the sample was provided by Prolific.

Data Analysis

All analyses were performed using R (version 4.0.4). The analysis procedure for the three hypothesis tests followed the planned strategy specified in the pre-registration, without any alterations nor unreported data exclusions. Predictors were mean centered before entered into any of the logistic regression models. Code and data are available at https://osf.io/jbprn/.

Ethics

Participants were informed that any publication of results or data would not be linkable to identifying information about them, before giving their consent to participating. Participants were compensated for their time in line with the fair payment recommendations provided by Prolific. The donated bonuses were transferred to the chosen charity organizations, except for those donated to one organization (Al Majmoua) that had a malfunctioning donation page. We followed applicable laws and regulations concerning the ethical conduct of research with human participants. Regulations did not require formal review for the present study.

RESULTS

Descriptive Results

Of the sample, 63.1% ($n = 316$) chose to donate their bonus while 36.9% ($n = 185$) chose to keep their bonus. Of those who kept the bonus, nine participants made their choice through not selecting any option while the remaining 176 selected the "Keep the bonus" option. **Figure 1** illustrates the proportion and frequency of participants who chose to donate their bonus for each set size condition. The group size of the 16 set size conditions ranged between 30 and 32 participants.

The sample had a mean preference certainty score of 20.3 ($SD = 11.3$, median = 19.4). **Table 1** shows the distribution of preference certainty scores for each donation choice.

Hypothesis Testing

Hypothesis 1

To test Hypothesis 1, a logistic regression predicting donation choice by set size and preference certainty was fitted to the data. Set size was not a significant predictor of donation choice, $b = 0.0006$, 95% CI [−0.007, 0.008], $z = 0.16$, $p = 0.873$, which means that Hypothesis 1 was not supported. This estimate suggests that the probability of donating neither significantly increased nor decreased between the 16 set sizes. **Figure 2** illustrates predicted donation probabilities across the range of set sizes, based on this model.

Hypothesis 2

To test whether including set size as a predictor with a quadratic effect would improve the predictive power of the model, this effect was added to the model specified above. This model thus predicted donation behavior by set size, set size^2, and preference certainty. Set size^2 was not a significant predictor of donation likelihood, $b = 0.0003$, 95% CI [−0.0001, 0.0007], $z = 1.32$, $p = 0.185$. A Likelihood Ratio test, comparing the two models, showed no significant improvement in predictive power by including the quadratic effect, χ^2 (1) = 1.77, $p = 0.184$. This means that we found no support for Hypothesis 2.

Hypothesis 3

To test whether there was a moderating effect of preference certainty on the relationship between set size and donation behavior, an interaction term between these predictors was added to the first model. The results showed no significant interaction effect (see **Table 2** for model coefficients). This means that Hypothesis 3 was not supported. See **Supplementary Figure 1** for a simple slopes plot showing predicted probabilities of donating for low, medium, and high preference certainty scores.

Robustness Check

To control for the fact that one organization mistakenly was entered twice into the organization pool (as described in section "Donation Choice Measure"), the models used to test Hypothesis 1–3 were re-run with 1 subtracted from the set size variable for participants who were presented with the same organization twice ($n = 98$). Thus, this transformed set size variable reflected the number of unique options presented, for all participants. Re-running the models with this transformed set size variable did not alter the result in any meaningful way, neither model had an AIC change of more than 0.02.

Exploratory Analyses

Post-choice Questions

Approximately 35.7% of the sample stated that they had not heard about any of the listed organizations before, while 9.2% stated that they had heard of one organization and 15.6% of several. The remaining sample (39.5%) were not sure whether they had heard about the organizations before. As such, it appears likely that the majority of participants did not perceive any of the presented organizations as a dominant option based

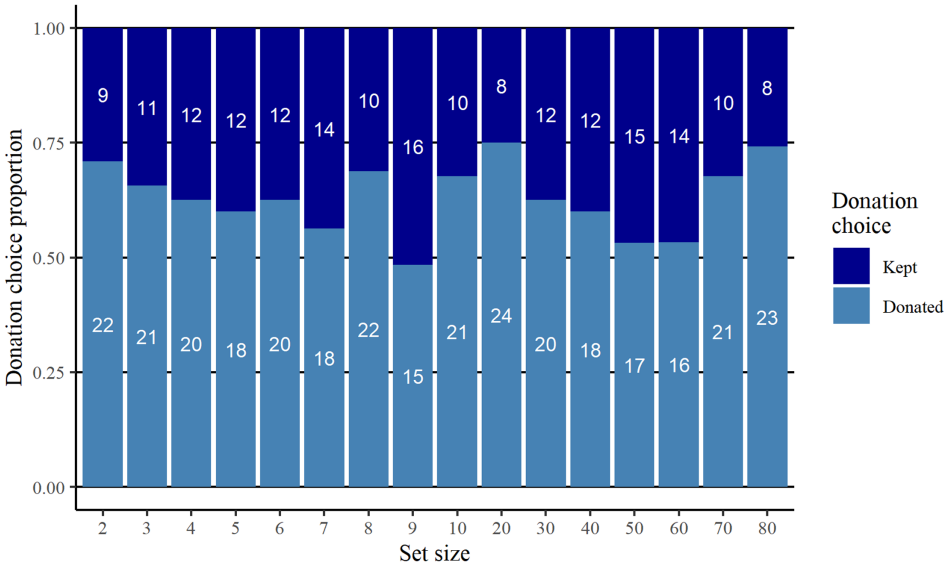

FIGURE 1 | Proportion and frequency who donated respectively kept the bonus, for each set size condition. Number of participants who made each donation choice is depicted in white font.

TABLE 1 | Distributions of preference certainty scores, grouped by donation choice.

Donation choice	Preference certainty			
	Mean	*SD*	Median	*n*
Kept bonus	21.2	11.7	20.7	185
Donated bonus	19.7	11.1	19.1	316

on prior conceptions about the NGOs. The distribution of answers for whether participants had previous knowledge about the presented organizations did not significantly differ depending on their donation choice, χ^2 (3, $N = 501$) = 5.16, $p = 0.161$. Likewise, the distributions of answers for whether participants searched for information about the organizations did not significantly differ depending on their donation choice, χ^2 (2, $N = 501$) = 3.34, $p = 0.189$. Approximately 19.8% of the sample reported that they searched for information about one or several of the listed organizations while making their choice, while the remaining 80.2% did not search for information.

Duration of Choice

As can be seen in **Supplementary Figure 2**, participants who chose to donate generally spent longer time on the donation page than those who kept the bonus, especially when faced with a larger number of organizations.

Preference Certainty and NGO Choice

Among the participants who chose to donate their bonus ($n = 316$), approximately 62.3% donated to an organization which matched the cause area or areas, which the participant had rated as most important during the preference certainty rating. If the preference certainty ratings were unrelated to the NGO choices, and participants chose a cause area at random, around 20% would be expected to match (given that there were five cause areas to choose from).

There was, however, large variation (between 36.4 and 83.3%) in this proportion between the different set size conditions. This variation will be further discussed in the limitations section (under the heading *NGO Randomization*).

Presentation Order and NGO Choice

To see whether participants were more likely to choose from a specific section of the assortment, we checked what position the chosen organization was displayed at for participants who donated their bonus. We chose to focus on participants who saw 20 or more options, as participants in the smaller set size conditions only saw one or two rows and therefore could easily get a quick overview of all the options in the assortment. Among the participants who saw 20 or more options and chose to donate their bonus ($n = 139$), 45 participants chose an NGO presented in the top two rows of the assortment, while 30 participants chose an NGO from the bottom two rows of the assortment. The remaining participants chose an NGO presented somewhere in the middle of the assortment. **Supplementary Table 1** shows the number of participants who chose from the top two rows and bottom two rows for set size conditions 20 through 80. Participants generally chose from the top two rows more frequently than from the bottom two rows. However, there does not appear to be any overwhelming presentation order effect, as there were participants choosing from each section (top, middle, and bottom) of the assortment in each of these seven conditions.

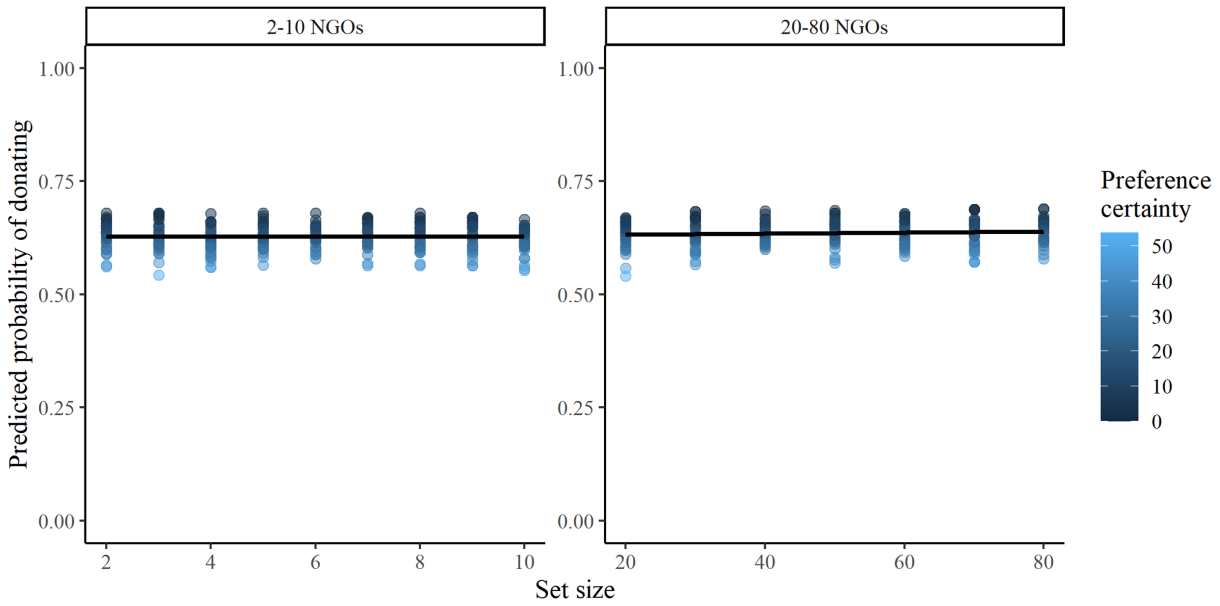

FIGURE 2 | Predicted probability of donating by organization set size. Individual points are colored by preference certainty score, with higher scores depicted as lighter points. Points illustrate predicted values for each participant based on the fitted model.

TABLE 2 | Model coefficients from logistic regression predicting donation behavior.

Predictors	*b* (*SE*)	*z* (*p*)	95% CI for odds ratio		
			Lower	OR	Upper
Intercept	0.538 (0.093)	5.80 (<0.001)			
Set size (Ss)	0.0005 (0.004)	0.15 (0.881)	0.993	1.001	1.008
Preference certainty (Pc)	−0.011 (0.008)	−1.36 (0.173)	0.973	0.989	1.005
Interaction Ss*Pc	0.0001 (0.0003)	0.39 (0.699)	0.999	1.000	1.001

Both predictors were mean centered before entered into the model.

DISCUSSION

Summary and Strength of Evidence

With this study, we aimed to determine whether the choice overload effect occurs for donation choices and to provide further insights into the relationship between organization set size and charitable behavior. The results clearly indicate that set size did not have any meaningful effect on donation likelihood. This was true when the relationship between set size and donation likelihood was modeled as a linear relationship, when modeled as a quadratic relationship, and when preference certainty was included as a moderator. These estimates of the effect between set size and donation likelihood all had narrow CIs, suggesting that the effects are likely close to zero and therefore negligible. Thus, choice overload does not appear to affect choices about whether or not to donate money to charity, at least not when the donation choice is set up as in the present study.

In relation to previous studies looking at set size and charitable behavior, the results found here are in line with studies finding no robust effect of set size (Scheibehenne et al., 2009), and in contrast to findings suggesting that larger NGO set sizes have negative effects on charitable decision making (Carroll et al., 2011) as well as findings suggesting that larger NGO set sizes lead to an increased donation proportion (Soyer and Hogarth, 2011).

How much confidence should we have in the confirmatory results presented here? The bonus sum which participants could choose to keep or donate was relatively small compared to the mean amount for online charitable donations (Nonprofits Source, 2018). In addition, the bonus amount was smaller than amounts used in prior studies with a similar donation measure (e.g., Scheibehenne et al., 2009; Schulz et al., 2018). Given this, donating the bonus might not have been experienced as a substantial loss. However, the choice still appears to have been consequential for participants. This is reflected in that participants who chose to donate spent more time on the donation page, which might indicate that they spent time making sure their donation went to the right cause. In addition, a high percentage (about 62%) of donating participants donated to an organization focusing on a cause that they had ranked as the most important. This indicates that participants gave thought to their donation choice and used their preferences to guide how they made trade-offs between organizations. Given these results, it seems unlikely that the bonus amount was perceived as inconsequential.

Furthermore, the present study setup (where a wide range of NGO set sizes were included) allowed us to model potential

behavioral differences with a high degree of precision, reflected in the narrow CIs around the effect estimates. Due to the high degree of precision, we can confidently conclude that there was no meaningful difference in donation proportion between set size conditions. Of course, the same level of precision could have been achieved by comparing only two set size conditions (large vs. small), with a large enough sample size. The continuous manipulation used here would however have allowed us to detect a non-linear relationship between set size and choice. The absence of a non-linear effect in the present sample contrasts with previous results indicating a U-shaped effect of set size in prosocial choice contexts (Herzenstein et al., 2020) and previous results indicating an *inverted* U-shaped effect of set size for different consumer choices (Shah and Wolford, 2007; Reutskaja and Hogarth, 2009; Park and Jang, 2013). However, the absence of a non-linear relationship may be explained by the absence of a choice overload effect in the present sample. Given this, we suggest that future studies should be set up to increase the likelihood of a choice overload effect (see suggestions below), while including a continuous manipulation of NGO set size.

Limitations and Future Directions

NGO Randomization

Above we mentioned that a high proportion of donors made their donation to a cause which they had rated as the most important. However, there was a large variation in this proportion between the different set size conditions (between 36.4% in set size 2 and 83.3% in set size 20). This is not surprising, as the chance of *not* seeing at least one organization from the cause one rated as most important was 63.8% for a participant seeing two options, while it was only 0.5% for a participant seeing 20 options. In other words, for the smaller set sizes there was a relatively high probability of not being able to choose from the cause category which one had rated as most important. For the larger set sizes this was unlikely.

The difference in how likely participants were to see at least one organization from their preferred cause area might have had important implications for the results. There is a possibility that fewer participants in the smaller set size conditions donated with the current study set-up than they would have if more of them had been presented with an assortment containing options that matched their preferences. The current set-up was used to make all 80 organizations in the NGO pool equally likely to be presented. However, given the potential limitations that come with the currently used randomized set-up, future studies could instead present participants with assortments, which are matched to their individual stated preferences. An assortment, that is, matched to individual preferences would level the playing field, giving participants in all set sizes equal opportunity of finding an option they prefer. Future studies should explore whether the choice overload effect is more likely to occur when participants in large and small set size conditions all are presented with options which match their stated preferences.

Alternative Presentation Formats

In the present study, we chose to focus on preference uncertainty as a potential moderator of choice overload. Chernev et al. (2015) referred to preference uncertainty as an intrinsic moderator, meaning that the decision maker enters the decision situation with a certain degree of preference uncertainty. Chernev et al. (2015) also suggested two extrinsic moderators (decision task difficulty and choice set complexity) which are determined by the choice situation and how information about the choice is presented. Below follows a discussion on how manipulating these extrinsic factors might have altered how participants interacted with the donation choice.

Decision task difficulty is described as the extent to which the choice task has features that increase cognitive demands. Higher decision task difficulty is suggested to increase the risk of choice overload occurring (Chernev et al., 2015). The choice task used in the present study likely had relatively low decision task difficulty, as relatively little information was presented for each option and participants had unlimited time to take in this information. A few ways to increase decision task difficulty would be to provide more details about each NGO (Chernev et al., 2015); setting time constraints for the donation choice (Dhar and Nowlis, 1999; Chernev et al., 2015); requiring participants to justify their choice (Scheibehenne et al., 2009); and presenting visual instead of verbal (i.e., text-based) information about the NGOs (Townsend and Kahn, 2014).

The complexity of the choice set is higher when options within a set are overall more attractive, when the options share common attributes, or when attributes are complementary in how well they fulfill the needs of the decision maker. More complex choice sets are suggested to increase the risk of choice overload occurring (Chernev et al., 2015). Choice set complexity was likely relatively high in the present study, as only relatively unknown organizations were included in the NGO pool and given that the majority of participants saw an assortment, where options shared common attributes (focusing on the same charitable cause). Personalized assortments (discussed in the previous section) would include options which are overall more attractive to the participant and therefore further increase choice set complexity (Bollen et al., 2010; Chernev et al., 2015).

Alternative Outcome Measures

In the present study, the outcome of interest was donation behavior. While actual donation behavior may be the outcome that is of most practical relevance, inclusion of other outcome measures could give further insights into the choice process underlying the decision of whether or not to donate. To better understand how the choice setup will affect future donation behavior, it might be relevant for future studies to measure how participants felt and reasoned, while choosing between the available options. Including a measure of satisfaction with choice could have given insights into whether there were differences in how participants felt about the choice, depending on the set size, even though there were no meaningful differences in actual choice behavior. A measure of satisfaction with choice might also reflect *warm glow* (i.e., positive feelings resulting from helping other people), an emotion that is suggested to

motivate donation behavior (Andreoni, 1990; Crumpler and Grossman, 2008). To get more insight into why participants who chose not to donate made that choice, follow-up questions alternatively a second donation opportunity could be included. This would make it possible to separate individuals who did not want to donate in this study nor in the future from individuals who chose not to donate because they wanted the bonus sum to go to an organization of their own free choice (not presented within the assortment).

Real-Life Application

Donation behavior in an experimental setting may not perfectly align with donation behavior in a natural setting (Benz and Meier, 2008). In the present study, donating the bonus is presumably perceived as the socially desirable choice (Lee and Woodliffe, 2010) as well as the choice that would enforce one's self-image as a good person (Batson, 2008). While the anonymous answer format used in the present study may reduce the influence of social desirability (Joinson, 1999), participants' choices were likely still influenced to some degree by a desire to maintain a positive self-image. In real world scenarios, individual interests might be more conflicting than in an experimental setup. To exemplify, a person may have to make a trade-off between donating to unknown individuals and saving one's money to put one's children through college. Both options could be viewed as socially desirable and self-image enhancing. As such, these forms of real-life trade-offs might be harder to make than the trade-off set up in the choice scenario used in the present study. In addition, it may be unlikely for individuals to face an assortment only including relatively unknown charity organizations in real-life donation choice situations. Given these potential differences from real-life trade-offs, it might not be advisable to use the conclusions drawn here to motivate design choices for charity rating sites or field studies of donation behavior.

CONCLUSION

The results from this pre-registered online experiment suggest that an increased charitable organization set size did not have any meaningful effect on donation behavior. These results call into question whether the choice overload effect is applicable to donation choices. Future studies should explore additional moderating conditions, measure additional outcomes, and test whether these results extend to natural choice settings to fully answer this question. In addition, we suggest that researchers interested in choice overload should manipulate set size in a continuous way, unless there is clear theoretical guidance for what qualifies as a too-large set size in the choice domain of interest.

DATA AVAILABILITY STATEMENT

The dataset generated for this study can be found in the OSF repository https://osf.io/jbprn/.

ETHICS STATEMENT

Ethical review and approval was not required for the study on human participants in accordance with the local legislation and institutional requirements. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

AL conceptualized the study, while AL and TL designed the study methodology. AL organized the study material and data collection and wrote the first draft of the manuscript. AL and TL contributed to coding and interpreting the statistical analyzes as well as data visualization. All authors contributed to the article and approved the submitted version.

ACKNOWLEDGMENTS

The authors would like to thank Ylva Andersson and Arvid Erlandsson for helpful comments on an earlier draft of this manuscript.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2022.800528/full#supplementary-material

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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BRIEF RESEARCH REPORT
published: 06 April 2022
doi: 10.3389/fpsyg.2022.800199

Look Behind Me! Highly Informative Picture Backgrounds Increase Stated Generosity Through Perceived Tangibility, Impact, and Warm Glow

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In this study, we investigated whether background information of a visual charity appeal can influence people's motivation to donate and the hypothetical amount donated. Specifically, participants were presented with a charity appeal to help a local hospital respond to the Coronavirus Disease-19 (COVID-19) emergency depicting a man sitting on a bed in a hospital room. The number of visual details (i.e., medical equipment) depicted in the background was manipulated according to three conditions: (1) "High information" condition (i.e., a room full of medical equipment), (2) "low information" condition (i.e., room with few pieces of medical equipment), and (3) "no information" condition (i.e., non-contextual background). We investigated whether the number of visual background details would have increased the tangibility of the cause measured as the hospital's adequate preparedness to deal with the COVID-19 emergency and severity of the patient's medical conditions. We also investigated whether increased tangibility, elicited by a higher amount of background information, would heighten participants' perceived impact of their donation and warm glow, which in turn would have led to increased motivation to donate and the amount donated. We found no significant direct effect of condition on the donated amount. However, path models revealed that more background information positively influenced participants' motivation to donate and the amount donated indirectly through increased tangibility, impact, and warm glow. Finally, we showed that a higher risk perception of COVID-19 was associated with higher donations. Results are discussed in line with relevant literature.

Keywords: prosocial behavior, donation, tangibility, visual information, background, impact, warm glow, COVID-19

OPEN ACCESS

Edited by:
Conny Ernst-Peter Wollbrant, University of Stirling, United Kingdom

Reviewed by:
Mikael Knutsson, Linköping University, Sweden
Natalie Struwe, University of Innsbruck, Austria

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *22 October 2021*
Accepted: *22 February 2022*
Published: *06 April 2022*

Citation:
Caserotti M, Vacondio M, Maze M and Priolo G (2022) Look Behind Me! Highly Informative Picture Backgrounds Increase Stated Generosity Through Perceived Tangibility, Impact, and Warm Glow. Front. Psychol. 13:800199. doi: 10.3389/fpsyg.2022.800199

INTRODUCTION

The recent Coronavirus Disease-19 (COVID-19) pandemic brought to the fore the fragilities of several health systems, undermining the stability of health agencies and governments around the world. To support medical facilities burdened by the emergency, the governments themselves, as well as charities, and non-profit organizations, increased their effort to raise funds to address the health and social emergency that was and still is pervasive. In 2020, Americans alone have

responded to such calls by donating more than 42 billion to health-related causes (Giving USA, 2021), funds that were critical in supporting important health and social projects. Hence, this situation shed light on the pivotal role of private donations in supporting distressed communities, increasing the necessity and urgency to better understand how to encourage and increase donations to deal with both sudden and chronic emergencies.

Although a variety of studies have investigated the factors that may contribute to a successful donation appeal, most have focused on the role of the donation recipient characteristics (in both visual and textual appeals), leaving out, to the best of our knowledge, information related to a contextual background. Therefore, this study is aimed at filling this gap by investigating the role played by visual background information depicted in a charity appeal in shaping donation behaviors.

Previous literature on facial expressions of donation recipients, has demonstrated how both distressed (e.g., Small and Verrochi, 2009; Cao and Jia, 2017; Jang et al., 2019) and happy (Zemack-Rugar and Klucarova-Travani, 2018) expressions, can elicit empathy in donors and thus increase their donations. Further research demonstrated that people tend to donate more to identifiable victims, i.e., presented through personal details that identify them (Small et al., 2007), rather than to a greater or equal number of unidentified or statistical victims (Schelling, 1968; Jenni and Loewenstein, 1997; Small and Loewenstein, 2003). This effect also holds when a single identified victim is compared to a group of equally identified victims (Kogut and Ritov, 2005; Small et al., 2007; Kogut, 2011).

The positive effect of a single identified victim on pro-social behavior has been explained in terms of increased tangibility (Cryder and Loewenstein, 2010). Tangibility refers to the degree of specificity and concreteness of the mental representation of a situation. It depends on the richness of details used to describe the situation or the way those details are processed. Tangibility positively impacts generosity through three interrelated causal mechanisms (Cryder and Loewenstein, 2010). First, providing tangible information about the charity and the project that will benefit from the donation increases perceived impact (i.e., donors' perception of how much their contribution can concretely help the beneficiary; Erlandsson et al., 2014, 2015), which in turn leads to greater prosocial behavior (Cryder and Loewenstein, 2010; Cryder et al., 2013). Second, vivid and tangible information with high imaginability boost generosity through increased emotional responses toward the recipients (Slovic, 2007; Cryder and Loewenstein, 2010; Cryder et al., 2013). Finally, a higher perceived impact elicited by giving to a tangible cause can also increase donors' "warm glow" (i.e., anticipated and experienced good feelings associated with doing something good for others; Andreoni, 1990; Cryder and Loewenstein, 2010; Dickert et al., 2016).

In general, perceived impact of a donation and warm glow have been both identified as core motivations of prosocial behaviors and charitable giving (Andreoni, 1990; Duncan, 2004; Dunn et al., 2008; Cryder et al., 2013; Erlandsson et al., 2014, 2015; Västfjäll et al., 2015). For instance, when overhead costs (i.e., administrative expenses of charitable organizations) are perceived as high (Sargeant and Woodliffe, 2007; Caviola et al., 2014), perceived donation impact drops thus consequently reducing motivation to donate. Similarly, a warm glow has been found to motivate people to act prosocially by positively impacting donors' short-term affective reactions (Konow, 2010). Specifically, self-focused feelings (i.e., warm glow) have been found to directly influence the motivation to donate, but not always the amount donated (Dickert et al., 2011).

Since tangible and vivid information about the cause or the recipient can increase prosocial behaviors, it is plausible that visual information depicted in the background of a visual charity appeal can influence people's willingness to donate through increased tangibility too. Nevertheless, studies focusing on background information in the prosocial domain are relatively scarce. A recent study by Choi et al. (2020) tested the influence of background on charitable giving. However, this study focused on the concordance between the positive or negative emotions generated by charity appeal messages (i.e., text and images) and the background color used (i.e., blue and orange), showing that the contrast between the two increases donations. Notably, this study examined a solid (or context-free) background, namely, a color background that lacks any kind of pattern or specific contextual information.

On the contrary, the role of the contextual background has been widely studied in marketing but results are mixed. In e-commerce, websites' products can be presented with a white, context-free background or with a background related to the context of the use of the target product. Some studies suggested that context-free images are preferred to contextual ones because image details derived from the background increase its complexity while decreasing liking (Winkielman et al., 2003). More recent studies, however, indicate that despite their greater complexity, contextual images can be perceived more fluently and enjoyed more, since they facilitate product recognition (Maier and Dost, 2018). Notably, the contextual background has a positive effect on product evaluation, especially, for more ambiguous products, since the greater amount of information helps to reduce the number of possible interpretations (Maier and Dost, 2018), thus eliciting more favorable attitudes toward the product (Wang et al., 2020; Wu and Li, 2021).

Hence, drawing on the abovementioned literature, the present study aims at investigating whether the number of visual details (i.e., medical equipment) depicted in the picture's background of a charity appeal can influence the motivation to donate and the amount donated. We investigated whether a higher amount of information (vs. no or low information) depicted in the background of a visual charity appeal should increase people's perceived tangibility of the cause and in turn their motivation to donate and the amount donated. In addition, we inquired whether this relationship could be mediated by higher perceived donation impact and warm glow.

METHODS

Participants

We recruited 474 American respondents *via* MTurk (Paolacci et al., 2010) with human intelligence task Approval Rate

greater than 95% and paid them 0.10$. TurkGate (Goldin and Darlow, 2013) was used to avoid multiple responses from the same participant. Participants (women = 46%; M_{age} = 38.75; *SD* = 11.35) were randomly assigned to one of three between-subject conditions ("high information" n = 157; "low information" n = 156; and "no information" n = 161). No significant differences in the demographics (e.g., age, gender, education, political orientation, and type of health insurance) have been found among conditions (see **Supplementary Table 1**). The study has been conducted under the Declaration of Helsinki and informed consent was obtained for all participants before the completion of the questionnaire.

Design and Procedure

Data collection took place on August 18, 2020. On that day, the recorded number of COVID-19 cases in the United States was 5,377,178, while 31,678 new hospitalizations were recorded in that week only. At that point in the pandemic, the fatality rate was 3.13% (Ritchie et al., 2020).

Participants were presented with a written donation appeal for a "COVID-19 Relief Fund" to help their local hospitals best respond to the pandemic. Together with the text, the picture of a patient, with his back turned, sitting on a hospital bed was presented. The amount of medical equipment in the picture's background was manipulated to vary the quantity of information provided according to three experimental conditions: (1) "High information": The patient was depicted in a hospital room filled with a high amount of medical equipment; (2) "low information": The patient was depicted in a hospital room with a low amount of medical equipment; and (3) "no information": The patient was depicted with a white background (for more details see **Supplementary Method 1**).

Participants were asked to report their motivation to hypothetically donate on a 7-point scale ranging from 1 ("Not at all") to 7 ("Very much") and whether they wanted to make a donation (Yes/No). Those who responded "Yes" were then asked the amount they would like to donate (amount; 10$, 25$, 50$, 75$, 100$, and others). Then, they were asked to what extent they thought that their donation could make a difference (impact) and how good donating to the Relief Fund made them feel (warm glow). Responses were given on a 7-point scale from 1 ("Not at all") to 7 ("Very much").

Perceived tangibility was then assessed with two *ad hoc* items. Specifically, participants had to rate on a slider from − 10 ("not prepared at all") to + 10 ("absolutely prepared") to what extent did the local hospital depicted in the picture seem adequately prepared for the medical emergency (adequacy), and from − 10 ("not severe at all") to + 10 ("extremely severe") to what extent did the medical situation of the man in the picture seemed severe (severity of the patient).

Finally, the risk perception of COVID-19 was assessed by adapting two items from Caserotti et al. (2021, 2022) and Vacondio et al. (2021). Participants were asked to rate their likelihood and their family and friends' likelihood (Likelihood) to contract COVID-19 in the next months from 1 ("extremely low") to 7 ("extremely high") and to what extent they perceived the virus as dangerous (seriousness) to themselves and their close ones from 1 ("not dangerous at all") to 7 ("extremely dangerous"). Given the high internal consistency (Cronbach's alpha = 0.86), these variables were then collapsed into a single factor called "risk." The questionnaire ended with demographic questions.

A detailed description of the conditions and supplementary analysis is displayed in **Supplementary Material**.

RESULTS

To investigate whether a higher amount of background information (i.e., condition) would lead to increased tangibility (i.e., adequacy and severity of the patient), we ran a bivariate correlation. Next, we ran path models to test the effect of the condition on our main dependent variables (i.e., motivation and amount) mediated by tangibility and the precursors of the donation (i.e., impact and warm glow).

To conduct our analyses, we recorded our variable condition and created two dummy variables using Helmert contrasts. Dummy 1 was created to contrast the presence of information (i.e., high and low information) against none information (i.e., high information = − 1, low information = − 1, no information = 2). Dummy 2 was created to contrast high information against low information condition (i.e., high information = 1, low information = − 1, and no information = 0).

Correlations Between the Amount of Background Information and Main Dependent Variables

We conducted a Spearman correlation between our dummy variables and continuous one, whereas a Pearson correlation was run between the continuous variables. Our findings showed that higher number of information in the background (vs. low; Dummy 2) was associated, out of the two tangibility variables, only with higher perceived adequacy of the hospital, while Dummy 1 (i.e., presence of information vs. no information) did not correlate with any of the main variables in our study (see **Table 1**). To confirm the effect of condition on our tangibility variables, we also ran an ANOVA. Results confirmed findings from the correlation matrix (see **Supplementary Method 2**). These results show that high background (vs. low) information makes participants perceive the cause as more tangible, which in our study is represented by higher perceived adequacy of the hospital to face the emergency. Moreover, we found that high adequacy was associated with higher impact, warm glow, motivation, and amount.

Being in high information (vs. low information) condition did not have a significant direct association with motivation, the precursor of donations (i.e., impact and warm glow), or our main dependent variables, i.e., amount. We also ran an ANOVA to specifically test our Average Treatment Effect (ATE) and we found no difference in the donated amount between conditions [Dummy 1, $F(2,474) = 0.03$, $p = 0.868$; and Dummy 2, $F(2,474) = 0.03$, $p = 0.968$]. However, the main goal of our paper was to test the psychological mechanisms that mediated the effect of background information on prosocial decisions. Therefore, we found it essential to test these mechanisms through a mediation

model (path model). Indeed, extant literature demonstrated how investigating indirect effects in the absence of a total effect (i.e., ATE) is important when the goal of an experiment is to test the psychological mechanisms behind a simple effect (Zhao et al., 2010; Hayes, 2012, 2017).

Effect of Amount of Background Information on Motivation and Amount

To investigate potential direct and indirect effects of the amount of background information on our main dependent variables, we used Stata 14 (StataCorp, 2015) to conduct a path analysis using structural equation modeling (SEM). Due to the results of the correlation analysis, we used only Dummy 2 (i.e., high vs. low information) out of the two dummy variables created.

We first examined Path Model 1 to investigate the indirect effect of Dummy 2 on motivation and amount mediated by the adequacy and the two precursors. Due to the results observed in the correlation matrix, the severity of the patients was excluded from the model, and Dummy 2 was associated directly only with adequacy. Next, we tested the direct effect of adequacy on the precursors and the main dependent variables. Finally, we investigated the direct path of the precursors on the main dependent variables and the direct effect of motivation on the amount. Further, in line with Cryder and Loewenstein (2010) and Dickert et al. (2016), we investigated the effect of impact on a warm glow. The resulting model was not significantly worse than the fully specified model, [$X^2(4, N = 474) = 1.85$, $p = 0.763$] and showed good fit indices [root-mean mean-square error of approximation (RMSEA) < 0.001, $p = 0.959$, comparative fit index (CFI) = 1.000, and Bayesian information criterion (BIC) = 10,437.7] according to Kline (2011). Results showed that participants in the high information condition perceived a higher tangibility of the cause, i.e., adequacy ($z = 0.11$, $p = 0.013$). A higher adequacy was associated with a higher warm glow (indirect effect: $z = 0.038$, $p = 0.018$) and impact (indirect effect: $z = 0.039$, $p = 0.018$), and in turn led indirectly to a higher motivation [overall indirect effect: $z = 0.05$, $p = 0.015$, 95% CI (0.02, 0.20)] and a higher amount [overall indirect effect: $z = 0.04$, $p = 0.017$, 95% CI (0.02, 0.18)]. Moreover, we showed that warm glow ($z = -0.03$, $p = 0.495$) and impact ($z = 0.085$, $p = 0.081$) had no direct effect on the amount. However, participants who reported higher warm glow and impact reported a higher motivation that led to a higher amount (indirect effect warm glow: $z = 0.21$, $p < 0.001$; indirect effect impact: $z = 0.32$, $p < 0.001$).

We then removed the paths that did not show a significant effect to create a second, more parsimonious model (**Figure 1**). The second model tested the indirect effect of Dummy 2 (i.e., high vs. low information) on the two main dependent variables (i.e., Path Model 2). The model showed a good fit, [$X^2(6, N = 474) = 4.89$, $p = 0.558$, RMSEA < 0.001, $p = 0.934$, the CFI = 1.000, and BIC = 10,428.4], and was not significantly worse than Path Model 1, [$X^2(2) = 3.04$, $p = 0.219$]. For our main model, we also tested a path model (i.e., Path Model 2.1) where we inverted the direction of the path between impact and warm glow and the model did not differ from Path Model 2 (Goodness of fit: [$X^2(6, N = 474) = 4.89$, $p = 0.558$, RMSEA < 0.001, $p = 0.934$, the CFI = 1.000, and BIC = 10,428.4)]. The results of Path Model 2 were consistent with the results of Path Model 1.

Finally, we tested a third model (i.e., Path Model 3) to control for the effect of the risk perception of COVID-19 (i.e., risk) as a covariate on our main variables. The model showed a good fit, [$X^2(6, N = 474) = 4.34$, $p = 0.631$, RMSEA < 0.001, $p = 0.953$, the CFI = 1.000, and BIC = 11,920.63]. The results showed that people who perceived a higher risk also perceive a higher adequacy ($z = 0.34$, $p < 0.001$), a higher impact ($z = 0.26$, $p < 0.001$), and warm glow ($z = 0.11$, $p = 0.006$). Finally, higher risk was also associated with higher motivation (z = 0.19, $p < 0.001$) and amount ($z = 0.11$, $p = 0.005$).

TABLE 1 | Correlation between amount of background information and main dependent variables.

	1	2	3	4	5	6	7
1. Dummy 1							
2. Dummy 2	–0.003						
3. Adequacy	–0.048	0.101*					
4. Severity of the patient	0.049	0.009	0.482**				
5. Impact	0.023	0.038	0.340**	0.520**			
6. Warm glow	–0.019	0.047	0.337**	0.458**	0.628**		
7. Motivation	–0.018	0.011	0.455**	0.608**	0.669**	0.663**	
8. Amount	0.011	–0.003	0.383**	0.514**	0.485**	0.438**	0.659**

*$p < 0.05$, **$p < 0.01$.

DISCUSSION

In this study, we investigated the effect of background information depicted in a visual charity appeal on prosocial behaviors. While previous studies inquired the role of contextual background mainly in e-commerce advertisements (Maier and Dost, 2018; Wu and Li, 2021), this is among the first articles that address this issue on donation behavior.

Results show that a higher amount of information (vs. low information) depicted in the background of a visual charity appeal increased participants' perceived tangibility of the cause. This result is in line with previous studies showing that higher amounts of textual details in written charity appeals increase the perceived tangibility of the cause (Cryder and Loewenstein, 2010; Cryder et al., 2013). Further, we show that the presence vs. the absence of contextual information does not produce *per se* a difference in perceived tangibility. This result stands between mixed findings that, in the e-commerce literature, show advantages of presenting products both with and without background (Winkielman et al., 2003; Larsen et al., 2004; Reber et al., 2004; Maier and Dost, 2018). We thus extend the literature by showing how the effect of tangibility holds also for pictorial details in the background of visual appeals. Further, in the present study, we used two *ad hoc* items to assess tangibility. Among those, only the one related to how the hospital seemed to be adequately prepared to deal with the COVID-19 emergency was found to increase along with the number of background details,

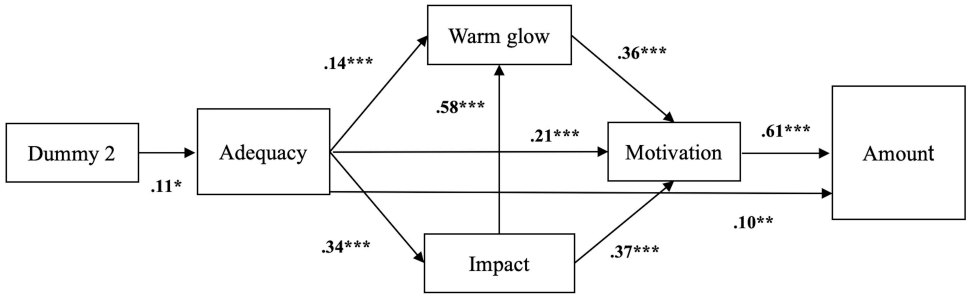

FIGURE 1 | Path model testing the indirect effect of the Dummy 2 on motivation and amount, through tangibility (i.e., adequacy), warm glow and impact. Coefficients presented are standardized. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

while no effect was found for the perceived severity of the medical conditions of the patient depicted in the appeal. We can speculate, therefore, that our manipulation of the contextual information effectively influenced the perception of the environment in which the scene took place, but did not affect the perception of the victim since no information directly associated with him has been instead manipulated.

Results also demonstrated that the higher tangibility perceived in the high information condition made participants perceive a higher impact of their donations and higher warm glow. This finding is in line with previous studies associating greater perceived donation impact (Cryder et al., 2008; Cryder and Loewenstein, 2010) and positive feelings associated with a contribution to the cause (Cryder et al., 2008; Cryder and Loewenstein, 2010) with greater tangibility derived from detailed textual information. Furthermore, even though we found no significant direct effect of the condition on the amount donated (ATE), our results showed that higher tangibility increased participants' motivation to donate and consequently the amount they would hypothetically donate, through the mediating effect of higher perceived donation impact and warm glow. These findings are in line with previous studies showing that perceived donation impact (Cryder et al., 2013; Erlandsson et al., 2014, 2015) and higher positive feelings (Andreoni, 1990; Dickert et al., 2011) mediate the motivation to donate.

As suggested by the two-stage model (Dickert et al., 2011), our results showed that the warm glow had a direct effect on the motivation to donate but not on the amount they were willing to donate. According to this model, the donation process is organized in two stages: Stage 1, i.e., the initial motivation and decision to donate, and Stage 2, i.e., the choice of how much to donate. Each stage is driven by different mechanisms: while the first is driven by emotions directed to the self (e.g., warm glow), the second is driven by emotions directed to others (e.g., empathy). In the present study, however, the perceived impact was found to affect only Stage 1 as warm glow did since no direct effect was detected on the amount donated. Considering that the perceived donation impact is the result of a trade-off between the expected benefits for the recipients and the costs for the donors (Caserotti et al., 2019) and that the latter is weighted more (Rubaltelli and Agnoli, 2012; De Bruyn and Prokopec, 2013; Sussman et al., 2015; Rubaltelli et al., 2020). We can speculate that perceived impact affected only the first stage of the model since it entails more self-oriented emotions similarly to warm glow. Indeed, in line with this speculation, our results showed that participants who perceived higher impact showed also a higher warm glow.

Our findings showed that the indirect effect, and not the direct effect, of the background information of charity appeal can have an effect on charitable donations. Therefore, taking into consideration the pivotal role of tangibility and the precursors of donation, our results can also have potential practical implications. For instance, including high background details in the pictures used for online or printed appeals could be a low-cost expedient that charities can use to boost the effectiveness of their fundraising campaigns. Background information could represent an ethical alternative to the debatable exposure and exploitation of inappropriate and shocking personal images of the victims' emotional, facial, and physical characteristics to increase appeal's pervasiveness. Unlike regular businesses, non-profit organizations are generally held to higher ethical standards (Lawry, 1995) and should consider avoiding using the victim's sorrow in a demeaning way. However, the fact that people are more likely to donate to a hospital that looks already adequately prepared is somewhat disheartening, especially considering the conditions of many realities around the World. Nevertheless, the suggested applications should be taken with caution since further studies (e.g., within-subjects design or field studies) are required to corroborate our results.

Further, the data of the present study were collected in the emergency context of the COVID-19 pandemic. We thus also controlled for the role of COVID-19 risk perception in shaping perceived tangibility and donation behaviors. Participants with a high perception of risk associated with COVID-19 perceived the hospital as more adequately prepared to deal with the medical emergency, their donation as more impactful, and felt a higher warm glow. Consequently, people with higher COVID-19 risk perception showed higher motivation to donate to a COVID-19 relief found and higher stated donation amounts. This result is in line with previous literature suggesting that perceiving COVID-19 as highly risky increases donation for causes related to the ongoing pandemic (Abel et al., 2021; Adena and Harke, 2021). Nevertheless, considering the peculiarity of the COVID-19 emergency (e.g., highly dreadful, very close, and

world-spread), it is possible that other elements related to the pandemic (e.g., personal knowledge about the situation of the hospitals, familiarity with the disease) might have affected our results. Thus, future studies should try to replicate and generalize the effect of visual background information also with different types of scenarios and in non-emergency contexts. It is indeed possible that the role of visual background information might be particularly important in increasing tangibility, and in turn generosity, for more distant, both strictly and figuratively, causes (e.g., a medical emergency, unknown in the Western World, in a far country on the other side of the globe). Further, it was recently shown that although COVID-19-related risk perception correlates positively with pandemic-related donations, when people can choose among multiple causes to support, and thus other aspects take over in the assessment, COVID-19 risk perception is no longer significant (Blanco et al., 2021). Future studies should therefore investigate the role of visual background information when different causes are compared jointly.

Moreover, in the present study, we used a picture of a man with his back turned. This choice was made to avoid confounding effects of personal characteristics and facial expressions. Nevertheless, it could be interesting to investigate how detailed information related to the victim and those related to the context might interact and which of the two is effectively more powerful. Furthermore, we could not control how much attention participants actually paid to the visual details manipulated in the pictures' background. Future studies should thus consider implementing process measures, e.g., eye-tracking tools, that can track attention allocation in specific areas of interest to better understand the effect of similar manipulations.

In addition, we investigated hypothetical rather than actual donation decisions. Although this choice might limit our findings' generalizability, extant literature showed similarities in the psychological mechanisms behind hypothetical and real contributions (Kogut and Ritov, 2005; Dickert et al., 2016). Nevertheless, future studies should test our model with actual donations to increase its ecological validity. Besides, although donations from a single individual may not be repeated over time, the effect multiplied by the number of people who may be exposed to the charity advertisement makes the result relevant for policy aiming (Funder and Ozer, 2019). However, since this is the first study on this topic, we encourage future studies with bigger samples to corroborate our results.

In conclusion, the present study shows that high background information in charity appeal's pictures can increase people's stated generosity through perceived tangibility and the precursors of donation (e.g., perceived impact and warm glow). We think that these first results hold potentially interesting insights from both a theoretical and practical perspective that is worth investigating further.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: OSF https://osf.io/yw3uz/?view_only=015933180278497998bb1289551a7e12.

ETHICS STATEMENT

Ethical review and approval were not required for the study on human participants in accordance with the local legislation and institutional requirements. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

MC and MV: conceptualization, formal analysis, visualization, writing—original draft, and writing—review and editing. MM: conceptualization, and writing—review and editing. GP: conceptualization, visualization, writing—original draft, and writing—review and editing. All authors contributed to the article and approved the submitted version.

ACKNOWLEDGMENTS

We would like to thank Enrico Rubaltelli (University of Padova) and Stephan Dickert (University of Klagenfurt and Queen Mary University of London) for their suggestions on the development of the project and the first draft of the present article. We would also like to thank the Judgment and Decision Making Lab (JDMLab, University of Padova) for having been a source of inspiration for this project.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2022.800199/full#supplementary-material

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frontiers | Frontiers in Psychology

ORIGINAL RESEARCH
published: 27 April 2022
doi: 10.3389/fpsyg.2022.748298

To Steal or Not to Steal: Self-Discrepancies as a Way to Promote Pro-social Behavior: The Moderating Role of Self-Interest

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Previous research showed that acting immorally on one occasion can determine a greater availability for pro-social behavior on a subsequent occasion. Nevertheless, moderating factors for this effect, such as financial interest remained largely unexplored. The present field experiment ($N = 587$) was organized in an urban setting, in a post-communist society (Romania), in a context of public anonymity and examined passersby's pro-social behavior on two consecutive occasions. The procedure involved a confederate "losing" a banknote of different values (1, 10, 50, 100, or 500 RON), which invited passersby's pro-social behavior to return it (or not). Participants who decided to steal the banknote were approached by a second confederate and asked politely to return the banknote. Our research was articulated mainly as a quantitative approach by measuring participants' pro-social behavior toward the person who lost the banknote, their subsequent pro-social behavior toward the confederate who exposed their behavior and the number of words they produced during a post-experimental interview in which they could justify their behavior. At the same time, we also performed a qualitative approach, through which we explored the themes evoked in their justifications and their relation with their previous behavior. Results indicate a moderating effect of economic interest on pro-social behavior toward the confederate who lost the banknote, as well as on their subsequent pro-social behavior toward the second confederate. Participants who stole the banknote also used significantly more words to justify their behavior, and this tendency could be observed especially in the case for higher values of the banknote. Results are critically discussed in a context dominated by an inherited pattern of distrust and social cynicism.

Keywords: pro-social behavior, stealing, field experiment, post-communism, Romania

OPEN ACCESS

Edited by:
Kristian Myrseth,
University of York, United Kingdom

Reviewed by:
Alexander Vostroknutov,
Maastricht University, Netherlands
Christiane Schwieren,
Heidelberg University, Germany

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *27 July 2021*
Accepted: *04 April 2022*
Published: *27 April 2022*

Citation:
Gavreliuc A, Gavreliuc D and Semenescu A (2022) To Steal or Not to Steal: Self-Discrepancies as a Way to Promote Pro-social Behavior: The Moderating Role of Self-Interest. Front. Psychol. 13:748298. doi: 10.3389/fpsyg.2022.748298

INTRODUCTION

Pro-social behavior has been considered an essential contributor to social welfare (Piliavin et al., 1981; Batson and Powell, 2003; Weinstein and Ryan, 2010; Wittek and Bekkers, 2015; Smith, 2019). Its role in generating interpersonal and societal wellbeing has been shown at the individual (Henrich et al., 2001; Weinstein and Ryan, 2010), group (Busching and Krahé, 2020), and societal levels

(Levine et al., 2001; Knafo et al., 2009; Smith, 2019). The scientific literature focused on pro-social behavior describes it as an interpersonal act between a benefactor and a receiver of the action, which a particular society defines as beneficial to other people or the social order (Dovidio et al., 2017, p. 17). Thus, pro-social behavior is like the "social glue," which emerges in interpersonal interactions and encourages living together peacefully and productively (Lay and Hoppmann, 2015).

The literature distinguishes between altruistic and egoistic motives of pro-social behavior (Frazier et al., 2013; Feigin et al., 2014). Egoistic motives are centered around the own interest of the person involved in the social interaction, such as the need for self-esteem and a positive self-image, for increasing his/her social status, or for managing negative emotions associated with the situation (e.g., anxiety, fear, sadness, or guilt). On the other hand, altruistic motives are generated by a genuine desire to support others without seeking any benefits for oneself (Penner et al., 2005). Thus, altruism is an entirely other-oriented and generous way of thinking and acting that proves to be beneficial both for the recipients and for society (Snyder and Dwyer, 2013).

One way of studying pro-social behavior is by involving individuals in situations in which they have the opportunity to act honestly or to cheat. The literature on this topic is extensive, yet studies that go beyond the strict confines of the laboratory space and investigate real-life situations, while still maintaining a high methodological quality, are surprisingly rare (see Gomes et al., 2021). Moreover, most of the experimental literature is based on procedures in which participants are aware that they are being observed, with low financial stakes involved and conducted mostly on Western, educated, rich and democratic populations (Cohn et al., 2019). Consequently, in the present study, we addressed these shortcomings by conducting a field experiment, in which we created a scenario of interaction with unaware participants, through which we tested pro-social behaviors in the context of everyday life context with ostensibly high financial stakes, in a highly underrepresented culture (Romania). Our scenario is similar to others applied in cross-cultural studies (e.g., Falk et al., 2018) and involves a situation in which participants unexpectedly find some money on the street and have to decide whether they act pro-socially and return it to the person who lost it or appropriate it. This decision is soon followed by another situation in which participants who initially stole the money are made aware of their unethical behavior and have to decide again whether they return or keep the money for themselves. Unlike other approaches however, the present study used both a quantitative as well as a qualitative methodology, to better understand the particularity of pro-social behaviors when a spontaneous need for help is activated in the public space.

LITERATURE AND THEORETICAL BACKGROUND

Several factors have been shown to be associated with pro-social behavior, among which the characteristics of the help-provider (e.g., Wollman et al., 1980; Gire and Williams, 2007), the characteristics of the help-seeker (e.g., Milgram et al., 1965; Simmons and Zumpf, 1983), situational factors (e.g., Newman, 1979; Keizer et al., 2008), or cultural variables (e.g., Vives and FeldmanHall, 2018; Cohn et al., 2019). Concerning the impact of situational factors, which is the main focus of the present study, one relevant theoretical framework that was used to explain differences in pro-social behavior when financial stakes are involved is the *subjective expected utility theory* (SEUT; Savage, 1954, Farrington, 1979, Farrington et al., 2020). SEUT describes how in a risk situation (like a specific context in which a person is confronted with a potential gain that can be obtained dishonestly), the person involved activates a behavioral decision based on: (a) utility (the subjective benefit or attractiveness of the potential gain), (b) subjective costs (the threat or sense of apprehension of being "discovered"), and (c) the probabilities related to them. By pondering all these factors, the decision-maker selects the behavioral alternative with the highest subjective expected utility and acts accordingly.

The results of several empirical studies support the validity of this theoretical framework. For example, a classical study conducted by Merritt and Fowler (1948) tested whether letters containing visible money were returned or not by pedestrians from East and Midwest cities in the US, by comparing the rate of return with that of "normal" letters containing only a simple visible message on a sheet of paper. Results showed that 85% of the letters were returned in the "normal" condition and only 54% in the money condition. More than that, in the money condition, 11 out of 54 letters were returned opened. Thus, when ordinary people are confronted with an invitation to help an unknown person, the majority of Americans acted pro-socially, yet only about half did so when their own immediate gain was also involved. Gabor and Barker (1989) also used the "lost letter" technique in Canada and observed that almost a quarter of all participants failed to return an envelope containing $150 (measured as stealing behavior).

Using a similar paradigm, Penner et al. (1976) tested the dispute between situational and personal factors in predicting reactions to "lost" money (returning, ignoring or taking) by an identifiable person (because of his wallet), by an unknown person belonging to a certain group (a person from the psychology department) or by an unidentifiable owner, in three different contexts: a psychology laboratory, a testing room from campus where evaluation services were provided and an impersonal place, like a campus washroom. Results showed that people's decision to return the money was influenced by the characteristics of the person who lost the money and by the context, whereas personality had almost no influence on behavior. Based on a cost analysis associated with the bystander effect (Piliavin et al., 1975), the authors argued that the harder it was to identify the real owner of the money and the more impersonal the contexts were (and thus the lower the probability of being sanctioned was), the less pro-social was participants' behavior. Similarly, Newman (1979) tested the role of familiarity with the context and the value of money and observed their influence on the rate of returning "lost"

money. Results showed that non-familiar and impersonal places (like a central shopping area) were more likely to induce stealing and that in these places stealing increased proportionally with the value of money. In another field experiment, Keizer et al. (2008) showed that cues of norm violation (i.e., garbage bags in the vicinity of a mailbox) impacted passersby's pro-social behavior. They evidenced an increase of stealing an envelope visibly containing money in "disordered" settings, in which other norms were previously violated and thus in which the perceived probability of being caught was lower.

Despite the reviewed examples, field experiments for testing pro-social behavior are relatively rare, even though studying people's pro-social behavior in everyday life is frequently suggested as a way to go beyond the controlled environment of laboratories (Lay and Hoppmann, 2015). For instance, Gomes et al. (2021) conducted a systematic review on stealing and monetary dishonesty incorporating 40 years of research (between 1979 and 2019) and found only 14 field experiments conducted in the area of Psychology/Social Sciences, of which only one was carried out also in ex-communist countries (i.e., Cohn et al., 2019). Even though the majority of these studies showed that higher levels of financial benefits and lower probabilities of being caught anticipated lower levels of pro-social behavior, there was also an opposite tendency that appeared in some studies, in which higher potential benefits determined higher levels of pro-social behavior in some circumstances (see also Mazar et al., 2008). For example, Cohn et al. (2019) tested the influence of self-interest on pro-social behavior in 355 different cities across 40 national cultures, in large field experiments involving more than 17,000 participants. The procedure involved a "lost" wallet in different public places (e.g., museums and post offices), containing a business card, in two conditions: with 13.45$ inside (money condition) or without money inside (no money condition), which was "found" by a confederate. The confederate then asked an employee of these public spaces to return it, because he/she was in a hurry, and left it on the counter. In contrast to the self-interest evidenced in most studies, in 38 out of the 40 national cultures the presence of money inside the wallet increased the rate of return. Furthermore, in another study organized in the United Kingdom, United States, and Poland, the authors manipulated the sum of money inside the wallet (introducing a "big money" condition—94.14$) and observed a further increase of this tendency, with the rate of return being the highest in the "big money" condition. A similar result was obtained by Azar et al. (2013), who found that customers of a restaurant were more likely to return a higher amount of excessive change (about 12$) than a smaller amount (about 3$). The inconsistency between such results and those initially reviewed suggests an interaction between costs and benefits that needs to be investigated further (Gomes et al., 2021). Thus, one of the aims of the present study is to contribute to the body of existing literature by investigating, through a field experiment, the influence of the costs–benefits mechanism on pro-social behavior, in anonymity conditions, when high financial stakes are involved and in a new cultural context (post-communist Romania).

Self-Discrepancies and Pro-social Behavior

While previous studies investigated the influence of self-interest, another direction of research focused on the role of moral inconsistencies in pro-social behavior. Moral consistency is defined by Campbell (2017) as "responding morally in a similar way to cases that are morally alike." Consequently, if one decides to follow one's own interest in a particular situation, to be morally consistent, then one should do the same in all similar situations. Nevertheless, research shows that this is frequently not the case. For instance, Otto and Bolle (2020) found in their experiment that, for those participants that decided to engage in stealing, half continued to follow their own interest while the other half engaged in pro-social behavior in their next immediate similar decision. Furthermore, several studies found that immoral behavior in one situation can even encourage a higher degree of moral behavior on a subsequent occasion (e.g., Jordan et al., 2011; Mulder and Aquino, 2013) as an act of moral cleansing (Zhong and Liljenquist, 2006) and this may be because of the negative feelings, such as shame, guilt, anger, or a threat to self-image that one may experience (Mazar et al., 2008; Bonner et al., 2017). A relevant theoretical framework that can account for this relationship is *self-discrepancy theory* (Higgins, 1987; see also Barnett et al., 2017) which, like other "inconsistency" theories, such as cognitive dissonance (Festinger, 1957), self-consistency (Lecky, 1945), or incongruity (Osgood and Tannenbaum, 1955), proposes that people are motivated to avoid inconsistencies. The theory distinguishes between one's *self-concept* (how the self is currently represented) and one's *self-guides*, which are standards that are yet to be achieved. Self-discrepancy theory postulates that people are motivated to find themselves in a condition in which their self-concept is congruent with their self-guides, because discrepancies generate discomfort.

In the moral domain, self-discrepancy theory posits that people maintain a state of equilibrium by behaving in ways that adhere to internalized moral standards or the standards of important others (Higgins, 1997). Therefore, instances of immoral behavior that violate such standards should generate discomforting thoughts and emotions, as a result of discrepancies between the actual self-concept and self-guides. However, discomfort is felt only when discrepancies are made accessible (Higgins, 1987; see also Duval and Wicklund, 1972), which highlights the role of situational factors as particularly important. For instance, Higgins et al. (1986) showed that, for people who were highly discrepant, priming their discrepancies lead to the experience of negative emotions, such as dejection and agitation. Therefore, activating discrepancies in people who acted immorally on one occasion can trigger negative emotions that can motivate them to restore congruence by behaving morally on a subsequent occasion. For example, in a classic study, Carlsmith and Gross (1969) showed that participants who delivered painful electric shocks to a confederate were more likely to comply with a pro-social request to help prevent

the construction of a freeway through redwood trees in Northern California than participants in the control condition. Similarly, Sachdeva et al. (2009) showed that people who activated an image of themselves as immoral persons donated five times more money to charity than those who activated the image of a moral person. Jordan et al. (2011) evidenced an increase in pro-social intentions and a decrease in cheating behavior for those individuals who recalled an instance of immoral behavior, compared to those who recalled moral or neutral events, while Dai et al. (2018) showed that people who had just paid a fine for riding the public transport without a ticket acted more honestly than other fare-dodgers who were not caught.

Even though this effect is well-researched, it goes without saying that not everyone will have the same degree of motivation to reduce discrepancies. Not all people who behave immorally will have the drive to restore moral congruence and not all that do will actually engage in compensatory behaviors on all occasions. Thus, the question that arises is for whom and in what circumstances this effect takes place? While individual differences, such as moral identity, have been shown to moderate the relationship between immoral acts and subsequent moral behavior by determining a stronger compensatory reaction (Mulder and Aquino, 2013; Ding et al., 2016), the role of situational factors in this relationship, such as self-interest, in not yet clear. Based on SEUT, it is likely that in low-gain conditions people may prioritize congruence restoration after a moral transgression, while they may be more ready to incur the cost of self-discrepancies when their reward for persisting in dishonesty is higher. Therefore, a second goal of the present study is to investigate self-interest (in the form of financial gain) as a moderator of engagement in subsequent moral behavior after a moral discrepancy is activated.

Further, when self-discrepancies are publicly revealed, people may also be motivated to engage in a process of self-image negotiation called *facework* (Ting-Toomey, 1988; Zhang et al., 2014), which refers to the communicative strategies that one uses to maintain her/his positive image in social interactions (Ting-Toomey, 1988; Oetzel et al., 2001). For this purpose, they may try to explain, rationalize or excuse their behavior or may engage in deceptive strategies that allow them to maintain a positive "face" in the eyes of others. Whatever the strategy, we argue that engaging in such facework is more socially and cognitively demanding compared with the situation when no self-justification is needed (i.e., no self-discrepancy is activated) and that this extra effort is reflected verbally in the volume of explanations that people produce when they are required to offer an explanation for their inconsistencies. Thus, a third goal of the present study is to investigate how self-discrepancies affect people's verbal behavior.

The Present Study

The present study aims to present new evidence on the moderating role of self-interest on pro-social behavior in an everyday life context, to explore how it influences individual behavior after self-discrepancies are activated and to find out how people respond verbally when inconsistencies in their behavior are made salient. For this purpose, we used both a quantitative approach through which we manipulated the potential gain of the participants and measured their concrete pro-social behavior on two consecutive occasions and the number of words they used to justify it, as well as a qualitative analysis of participants' interviews, in which they explained their behavior. More concretely, we created a scenario in which we investigated passersby's pro-social behavior by arranging that a banknote of different values is "lost" in front of them, in a public space, and monitored their behavior. For those who initially stole the banknote, self-discrepancy was activated by a confederate who revealed their behavior and offered them a second chance to return the banknote. Soon after, participants were requested to explain their behavior, in their own words, in a short interview. Based on SEUT, self-discrepancy theory and previously reviewed studies, we expect that:

> *Hypothesis* 1: Participants' pro-social behavior toward the person losing the banknote decreases when their potential gain increases.
> *Hypothesis* 2: For those participants who decide to steal the banknote, subsequent pro-social behavior decreases when their potential gain increases. In other words, participants' self-interest will moderate the relationship between self-discrepancies and subsequent moral behavior.
> *Hypothesis* 3: The volume of explanations provided by self-discrepant participants (i.e., those who steal the banknote) will be higher than for those who return it.

In an explorative manner, we will also analyze participants' verbal explanations and how these relate to their previous behavior (i.e., returning or stealing the banknote).

MATERIALS AND METHODS

Procedure

The present study ("the fast-handed passerby") involved an interaction between a confederate, an aged man (around 65 years old) appearing to come from a poor background and naïve pedestrians. The place of interaction was in a supermarket's vicinity, in the city of Timisoara (approximately 350,000 inhabitants), on a relatively crowded street, placed at least 50 m from the entrance of the supermarket. In each new trial, when a pedestrian spontaneously passed by him, the confederate passed his coat over his shoulder and "unexpectedly lost," by "mistake," a 1, 10, 50, 100, or 500 RON banknote (Romanian currency, 1 RON $\approx$ 0.20 euros, photocopied from https://www.allnumis.ro/catalog-bancnote/romania, with a short mention added on it: "*This is a photocopied paper, used only for the experimental purpose in a Social Psychology field experiment study*." The photocopied banknotes were identical to real ones and could not be recognized as fake at first sight, as evidenced in our pilot study—see an example in **Supplementary Material**). Participants' pro-social behavior was monitored by a collaborator placed relatively close (around 5 m) to this spontaneous interaction, who appeared to be checking his mobile phone. It was agreed with the experimenter that the collaborator who

monitored the interaction should count participants' behavior only if: (a) the pedestrian non-ambiguously observed the whole incident and (b) she/he raised the banknote from the ground. We were interested only in the participants who did this explicit gesture and picked up the banknote, because in this way they became the holders of a resource that could be returned to the real owner, or appropriated. Moreover, their way of acting could more adequately and non-ambiguously measure pro-social behavior, without the need to speculate on their reasons if they did not intervene at all (see Lin et al., 2016). If the pedestrian just watched the incident and continued, after around 5 s, our collaborator returned the banknote to the "old man" and the new trial was prepared. Whatever the pedestrian's behavior (to return or steal the money), after around 10 s, a second confederate politely stopped the pedestrian and explained the whole scenario, the stake of the research and asked for participant's verbal consent. He/she also politely requested the participant to explain in a post-experimental interview (fixed at a maximum of 120 s) her/his previous behavior and to return the money, if the participant stole the banknote. At this step, two other dependent variables were measured, namely, *pro-social behavior toward the second confederate* and *face-saving behavior*.

The experiment took place during weekdays business afternoon hours, in similar locations (described before) and only in stable weather contexts, avoiding any unpleasant atmospheric conditions (like rainy or windy moments), in the same season (in spring, between March and May 2019). After each interaction, a new trial could start only after at least 10 min from the previous one, in a similar area, but not closer than 200 m from the previous place of interaction. Thus, we did not organize more than 15 trials on each day during the data collecting process.

Variables

The independent variable (IV) was the *value of the lost banknote* (five conditions: 1, 10, 50, 100, and 500 RON). The dependent variables were: *pro-social behavior toward the first confederate* (DV1), operationalized by measuring the return rate of the banknote to the person who lost it, *pro-social behavior toward the second confederate* (DV2), operationalized by measuring the return rate of the banknote to the confederate who requested participants to return the money and *face-saving behavior* (DV3), operationalized by measuring the number of words produced by the participants in the post-experimental interviews.

Selection Procedure

To ensure a roughly random selection, in each new trial, the 10th pedestrian was selected. If the 10th pedestrian did not fit the selection criteria, the confederate was instructed to select the next appropriate person. Criteria for selection were based on exclusion: participants were excluded if they were in a hurry, expressed any explicit distress, were accompanied by someone else (i.e., were not alone) or were involved in another task (like reading or talking on their mobile phone, etc.). Thus, each pedestrian who was not characterized by these features could become a potential participant in our study. We did not precisely count the number of participants who were rejected using these exclusion criteria, but the approximate number of them was around 1/3 of the pedestrians integrated in this field experiment.

Calibration of the Sample

For an adequate calibration of our sample size, we performed a power analysis (PA), using G*Power, version 3.1.9.7 (Faul et al., 2007). We calculated the required sample size in order to detect small effects. Because our analyses implied chi-square tests and one-way ANOVA, we performed PA for both. Thus, for a small effect of Cohen's $w = 0.15$ for a chi-square test with $df = 4$, $\alpha = 0.05$ and a power of 0.80, the required sample size was $N = 531$. The PA analysis for the ANOVA test to detect a small effect size $f = 0.15$, with five groups, $\alpha = 0.05$ and a desired power of 0.80, revealed a required sample size of $N = 540$. Based on this rationale, our global sample was established at $N > 540$. In our concrete design, the sample size was $N = 587$.

Approaching the Qualitative Data

Methods, such as oral history or non-structured interviews, are useful in "giving a voice" and "making sense" of the genuine communication of participants (Larkin et al., 2006). We performed a thematic analysis on participants' interviews, using the Interpretative Phenomenological Analysis Method (IPA; Smith and Shinebourne, 2012). IPA is suitable for integrating an insider's perspective in explaining participants' meanings associated with their behavior. Because it is a phenomenological interview type (Goffman, 2017), IPA can significantly enrich the understanding of the meanings generated during the interaction between the confederate and individual participants, by producing a coherent narrative as close as possible to the participant's view (Larkin et al., 2006). After collecting all interviews, we followed the methodological recommendations for interpreting such data (see Smith et al., 2009). Thus, in the first step, we randomly selected around 15% of the interviews ($N = 93$), and a group of two experts trained in the IPA analyzed all the emerged themes and the associated subthemes. After that, the experts confronted the themes and finally agreed by consensus on seven of them, each focused on a specific semantic area. IPA is less preoccupied with the quantitative accuracy of measuring all the categories included in an interpersonal discourse, like a classical content analysis (Vaismoradi et al., 2013), and focuses more on the thematic salience of the major categories that guide the argumentative speech. In the last stage of the qualitative analysis, all interviews were analyzed ($N = 587$) based on the emerged themes. The major themes were the following: implicitly normative, explicitly/ostentatiously normative, interpersonal functional cynicism, absurd/incoherent explanations, mercy/support, recognition and assuming the mistake and non-informative message. Each participant was assigned to one of these themes, while disagreements were resolved through consensus.

Ethics and Pilot Study

The present research was ethically approved by the Scientific Committee of the Center for Social Diagnosis from the Faculty of Sociology and Psychology of the West University of Timisoara.

In requesting approval, the title, procedure, ethical implications for human participants, methods and expected results were described. Even if the pedestrians were not aware of their initial participation in the experiment, their privacy was respected during and after the experimental scenario. All naïve individuals who accepted the interaction with the confederate were debriefed at the end and asked for their consent. Before starting the actual experiment, we tested in a pilot study ($N = 12$) whether similar participants (naïve pedestrians) are likely to be distressed by the proposed scenario. None of them reported any explicit distress once they discovered the true nature of the research at the debriefing step. Also, none of the participants involved in the actual field experiment reported any explicit distress caused by their participation in the experiment. Through the pilot study we also tested the realism of the proposed scenario: Of the 12 participants involved in the pilot study, none could tell that the money used was fake.

Statistical Analyses

Because pro-social behavior and value of the lost banknote were measured as discrete variables, we use chi-square tests to check their association, hypothesized in H1 and H2. To test H3, an independent samples t-test is conducted, to check the difference in the volume of words produced in the post-experimental interviews, between the participants who stole the banknote and those who returned it.

RESULTS

SPSS v.21.0 was used to conduct all analyses. Of all passersby involved in the experiment, 65.29% (587 from the total of 899) saw the lost banknote and reached down for it. To test our first hypothesis, we first performed a chi-square test to verify whether pro-social behavior toward the first confederate depended on the value of the lost banknote. Test results evidenced significant differences in pro-social behavior, depending on the value of the banknote, $\chi^2(4) = 24.848$, $p < 0.001$ (see **Table 1**). The influence of value on pro-social behavior toward the first confederate had an effect $V = 0.206$ which, according to Cohen (1988, p. 222) guidelines, represents a large effect.

The highest rate of return was for the lowest value banknote (74.4% for 1 RON), while the lowest rate of return was for the highest value banknote (45.4% for 500 RON). The rate of pro-social behavior in the 1 RON condition was significantly higher than in the 100 RON ($Z = 2.717$, $p = 0.003$) and 500 RON ($Z = 4.482$, $p < 0.001$) conditions, but not significantly higher compared to 10 and 50 RON conditions; the rate of pro-social behavior in the 10 RON condition was significantly higher than in the 100 RON ($Z = 1.853$, $p = 0.032$) and 500 RON ($Z = 3.674$, $p < 0.001$) conditions, but not significantly higher than in the 50 RON condition; the rate of pro-social behavior in the 50 RON condition was significantly higher than in the 500 RON ($Z = 3.107$, $p < 0.001$) condition, but not significantly higher than in the 100 RON condition, while that in the 100 RON condition was significantly higher than in the 500 RON ($Z = 1.851$, $p = 0.032$) condition. Thus, when the potential gain is small (1, 10, and 50 RON conditions), the decrease in pro-social behavior is rather small and non-significant, but as soon as it becomes substantial (100 or 500 RON), results illustrate a progressive decrease in pro-social behavior. Therefore, the data supports our first hypothesis (H1).

To test our second hypothesis, we conducted the analysis only on the subsample of participants who initially stole the banknote ($N = 219$). In the sequence called "the moment of truth," when participants were approached by the second confederate and their previous behavior was revealed, participants could decide either to return the stolen money or to definitively appropriate them. When self-discrepancies were activated, 198 of the 219 participants (90.4%) that initially stole the banknote, decided to return it. However, this rate was not equally distributed across conditions. In low-gain conditions (1, 10, and 50 RON), almost all participants returned the banknote, indicating that they were more preoccupied with restoring self-congruence than their personal gain, while in high-gain conditions (100 and 500 RON) only 88% and 80.3% did do (see **Table 2**). Therefore, we conducted a chi-square test to investigate whether the value of the banknote moderated participants' subsequent pro-social behavior toward the second confederate. Results indicated that the rate of return was significantly associated with the value of the lost banknote, $\chi^2(4) = 13.283$, $p = 0.01$, $V = 0.246$ (large effect). The rates of return were significantly higher in the 1 RON condition than in the 500 RON condition ($Z = 2.101$, $p = 0.017$); in the 10 RON condition than in the 500 RON condition ($Z = 2.449$, $p = 0.007$); and in the 50 RON condition than in the 100 RON ($Z = 1.672$, $p = 0.047$) and 500 RON ($Z = 2.523$, $p = 0.005$) conditions. Moreover, the rate of return in the 10 RON condition was higher than in the 100 RON condition, though this was just above the threshold of statistical significance ($Z = 1.615$, $p = 0.052$). There were again no significant differences between 1, 10 and 50 RON conditions.

TABLE 1 | Cross-tabulation of values of the "lost" banknote and pro-social behavior toward the first confederate ($N = 587$).

Pro-social behavior (DV1)	Value of banknote						χ^2	*df*	*V*
	1 RON	10 RON	50 RON	100 RON	500 RON	Total			
Returned the banknote	87 (74.4%)	85 (69.1%)	77 (65.8%)	68 (57.6%)	51 (45.4%)	368 (62.7%)	24.848***	4	0.206
Appropriated the banknote	30 (25.6%)	38 (30.9%)	40 (34.2%)	50 (42.4%)	61 (54.5%)	219 (37.3%)			

V = effect size (Cramer's V coefficient) and RON, Romanian currency (Leu).
****p < 0.001.*

TABLE 2 | Cross-tabulation of values of the "lost" banknote and pro-social behavior toward the second confederate ($N = 219$).

Pro-social behavior (DV2)	Value of banknote						χ^2	*df*	*V*
	1 RON	10 RON	50 RON	100 RON	500 RON	Total			
Returned the banknote	29 (96.7%)	37 (97.4%)	39 (97.5%)	44 (88.0%)	49 (80.3%)	198 (90.4%)	13.283**	4	0.246
Appropriated the banknote	1 (3.3%)	1 (2.6%)	1 (2.5%)	6 (12.0%)	12 (19.7%)	21 (9.6%)			

V = effect size (Cramer's V coefficient) and RON, Romanian currency (Leu).
***p = 0.01.*

TABLE 3 | Means and SD of the number of words in the post-experimental interviews.

Value of banknote	Participants who returned the banknote ($N_1 = 368$)			Participants who appropriated the banknote ($N_2 = 219$)			Global sample ($N = 587$)		
	N	*M*	*SD*	*N*	*M*	*SD*	*N*	*M*	*SD*
1 RON	87	13.57	12.49	30	21.17	9.58	117	15.51	1.54
10 RON	85	12.40	9.99	38	23.82	15.56	123	15.93	1.51
50 RON	77	13.65	15.55	40	19.85	11.43	117	15.77	1.51
100 RON	68	14.22	12.05	50	24.34	17.26	118	18.51	1.47
500 RON	51	15.80	14.51	61	38.66	26.11	112	28.26	1.51
Total	368	13.75	12.78	219	26.98	19.67	587	18.68	16.99

Therefore, the pattern of results is similar to the previous one and offers support for our second hypothesis (H2).

Regarding the volume of explanations produced by the participants in relation to the value of the banknote, **Table 3** indicates the means and SD for this variable for the participants who returned the banknote, for the ones who did not and for the global sample.

To test our third hypothesis, we conducted an independent samples t-test by which we compared the mean number of words produced by the participants who initially stole the banknote ($M = 26.980$, $SD = 19.666$) with that of the participants who returned it to its rightful owner ($M = 13.750$, $SD = 12.872$). A check of normality was conducted by inspecting skewness (0.952 and 2.110, respectively) and kurtosis (0.896 and 6.643, respectively) values for both groups, which revealed no serious violations, as all values were between the limits of −3 to 3 for skewness and −7 to 7 for kurtosis (see Hair et al., 2010). The difference between groups was significant, $t(585) = 9.846$, $p < 0.001$, $d = 0.796$ (large effect), indicating that self-discrepant participants used significantly more words to explain their behavior than those who were not self-discrepant. Our third hypothesis (H3) was therefore supported by the data. Exploratively, we investigated the impact of value of money on the number of words produced. For this, we used a factorial ANOVA with pro-social behavior toward the first confederate and value of money as predictors. The main effect of pro-social behavior $F(1) = 76.854$, $p < 0.001$, $\eta_p^2 = 0.118$ was significant, indicating that, as in the previous test, participants who stole the money produced significantly more words than those who did not. This difference in pro-social behavior explained 11.8% of the variance in the number of words produced by the participants. The analysis revealed also a main effect for value, $F(4) = 8.665$, $p < 0.001$, $\eta_p^2 = 0.057$, indicating that the number of words produced was dependent on the value of the banknote. *Post-hoc* tests with Tukey correction indicated significant differences between the condition of the most valuable banknote (500 RON) and all other conditions [mean difference (500 RON—100 RON) = 9.74, $t = 4.513$, $p < 0.001$; mean difference (500 RON—50 RON) = 12.48, $t = 5.769$, $p < 0.001$; mean difference (500 RON—10 RON) = 12.32, $t = 5.765$, $p < 0.001$; mean difference (500 RON—1 RON) = 12.73, $t = 5.885$, $p < 0.001$], while there were no significant differences between the other conditions. The value of the banknote explained 5.7% of the variance in the number of words. There was also a significant interaction between pro-social behavior toward the confederate and the value of the banknote, $F(4) = 5.109$, $p < 0.001$, $\eta_p^2 = 0.034$, indicating that the increase in the volume of explanations with the value of the banknote depends on the type of behavior participants engaged in (returned vs. stole the banknote). There was no change in the number of words for different values of the banknote for those who returned the money ($F(4) = 0.584$, $p = 0.675$), while for self-discrepant participants the number of words increased with the value of the banknote, $F(4) = 8.958$, $p < 0.001$.There were significant differences in the number of words between the 500 RON condition and all the other conditions [mean difference (500 RON—100 RON) = 14.32, $t = 4.085$, $p = 0.001$; mean difference (500 RON—50 RON) = 18.806, $t = 5.032$, $p < 0.001$; mean difference (500 RON—10 RON) = 14.84, $t = 3.909$, $p = 0.001$; mean difference (500 RON—1 RON) = 17.49, $t = 4.27$, $p < 0.001$] and no significant differences between the other conditions (see **Figure 1**).

Regarding the results obtained from the thematic analysis, our study does not claim to be representative; it is more concerned with the in-depth process of meaning creation by ordinary people in real-life interactions. The referential themes were grouped in a portfolio of seven categories (see **Table 4** for the English version, and **Supplementary Material**, for the

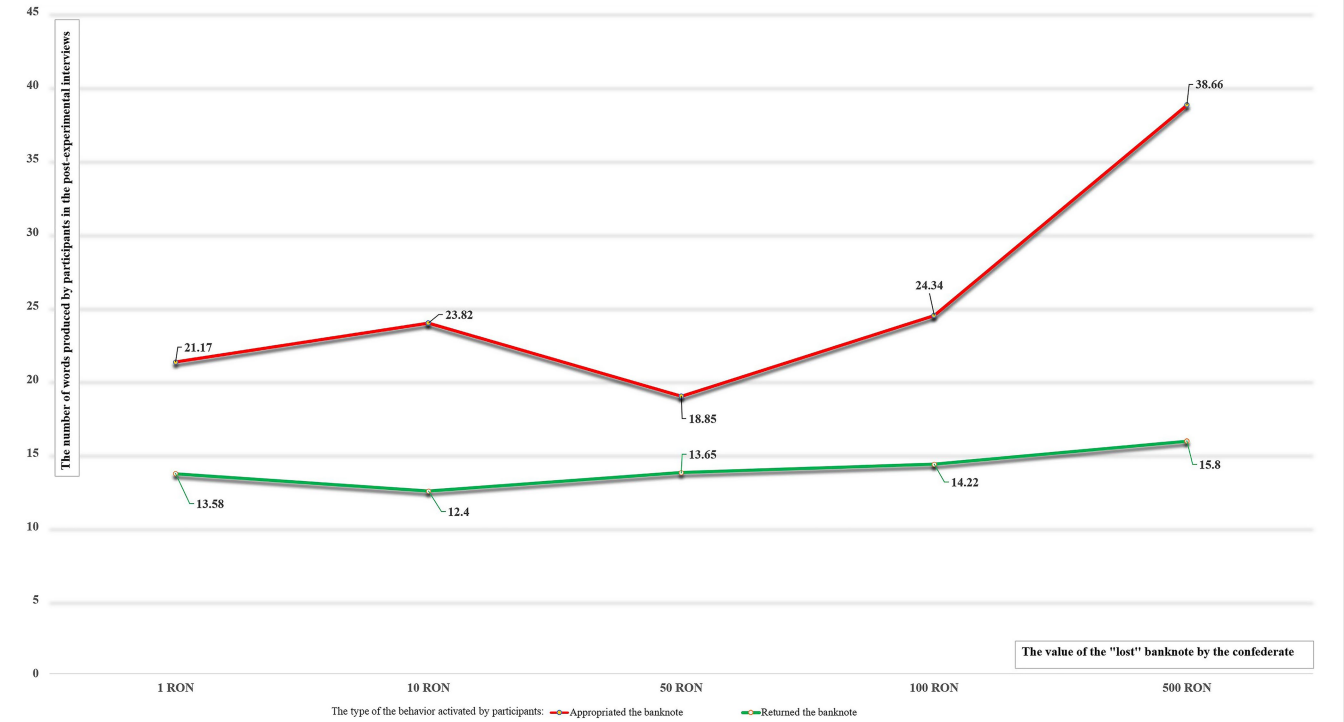

FIGURE 1 | Number of words produced in the post-experimental interviews as a function of returning behavior and value of the lost banknote.

TABLE 4 | IPA matrix of referential themes and participants' statements (global sample, *N* = 587).

Theme	*N* (%)	Relevant examples
1. Non-informative messages	157 (26.74%)	- "I'm in a hurry, goodbye"/"That's it"/"I'm sorry, I am late"/"Goodbye, I'm in a hurry, sorry"/"Hello"/"I cannot"/"Yes"/"No"/"Give me a break"/"I cannot now"/"Good day" etc.
2. Interpersonal functional cynicism	101 (17.21%)	- "And, what's the problem?"/"Obviously I took the money, because I also get cheated in life, not rewarded. Was I supposed to be the loser when for once I have the occasion to be the winner?"/"After all, most would have done as I did!"/" I do not earn 500 lei in half a month, so what was I supposed to do?!"/"I do not give a damn about your research, the only things that matters is to win here and now! Do you think someone is doing charity to me?"/"If you receive such mana from heaven you must be a loser to blow it away!"/"Look, I did something that others would have done to me, so I do not see the problem?!"/"I bended the rules gracefully, because I just wasn't going to leave it to another hunger-bitten to take it. Am I the one to feed a hunger-bitten?"/"In short, if everyone steals from me, I am not going to play generous!"/"The thief goes hand in hand with the lord"/"Giving others a bum deal earns somebody a living" etc.
3. Explicitly/Ostentatiously normative	92 (15.67%)	- "To be honest is a golden rule in life"/"I've always done the right thing and I want to go to sleep at peace every night"/"I've never stolen in my life"/"Well, if we all stole from each other, what would be left of this country?… Not that there's much left…"/"That's what we all should do! I hope that's also what happened!"/"Honesty is something that should never be given away, for nothing!"/"We must always help each other, because that's what my parents taught me. Otherwise, it will be very bad for all of us"/"Mister, whoever steals others steals himself!" etc.
4. Mercy and support	73 (12.43%)	- "I wonder how others could have stolen from a penniless?"/"A poor old man… he should have been helped"/"Look at him, he's close to dropping dead. If he saw that he was really left without 100 lei, he would have died on the spot. How was I supposed to seal from such a guy?"/"Well, look how needy he is!"/"How was I supposed not to give him his money back when you clearly see he needs it?"/"That poor old guy… I'd lose my right arm if I'd steal from this guy. It was a must to help him" etc.
5. Implicitly normative	71 (12.09%)	- "This is what you should do"/"I could not have done it otherwise"/"It's natural"/"But what would you suggest me to do?"/"To be such a jerk to steal a poor old man, is hard to imagine"/"Well, that's the order of things, to give back what is not yours" etc.
6. Absurd/Incoherent explanations	59 (10.05%)	- "I knew it was a worthless piece of paper"/"I knew that if he looked at me and looked after his money, I would have returned it to him"/"I did not realize it"/"I thought he was a dirty peasant, what do you want from such a guy?"/"I went ahead, I just wasn't going to go back …" etc.
7. Recognition and assuming the mistake	38 (6.47%)	- "I'm truly sorry"/"I really do not know what happened to me"/"I'm sorry"/"Sorry, that's it"/"I was a lame brain, but I'm sorry. Look, mister, your money back (n. ns.—it is pointed out that it is not "real" money)… Uff, I'm sorry mister…"

original version, in Romanian). The narrative that appeared with the highest incidence was *non-informative messages* ($N = 157$ from a total of 587 interaction), which covered routinely expressed messages, like a salute or a brief refuse of the dialogue, regularly formulated in a few words. Therefore, more than a quarter of participants decided not to communicate any significant informative message in their final interaction with the second confederate. The next most mentioned themes were *interpersonal and functional cynicism* ($N = 101$), *explicitly/ostentatiously normative* ($N = 92$), *mercy and support* ($N = 73$), *implicitly normative* ($N = 71$), *absurd/incoherent explanations* ($N = 59$), while *recognizing/assuming the mistake* ($N = 38$) was the least mentioned theme.

We performed a chi-square test to investigate whether pro-social behavior toward the first confederate influenced participants' propensity for specific themes in their narratives. Test results evidenced significant differences in selecting specific themes, depending on participants' behavior toward the first confederate: $\chi^2(6) = 451.276$, $p < 0.001$, generating a very large effect, $V = 0.877$ (Cohen, 1988, p. 222; see **Table 5**). While those who returned the banknote had narratives mostly dominated by normative considerations (44.3%), non-informative messages (33.4%) and mercy and support for the victims (19.8%), those who decided to steal the banknote evoked cynicism (46.1%) or offered absurd or incoherent explanations for their behavior (22.8%) and only in 17.4% of the cases they recognized their mistake.

Similarly, to test whether participants' subsequent pro-social behavior toward the second confederate influenced their tendency to produce specific themes, we performed a chi-square test only on the sample of participant who initially stole the banknote. Our outcomes indicated that there were significant differences in selecting specific themes, depending on the activated behavior in relation to the second confederate: $\chi^2(3) = 91.581$, $p < 0.001$. The effect, $V = 0.647$, was again a very large one (see **Table 6**). Thus, more than 2/3 of the participants who stole the banknote (68.9%) produced in the interaction with the second confederate either a cynical or an absurd/incoherent explanation regarding their previous behavior. In the same time, only less than 1/5 of participants (17.4%) from this category decided to assume the mistake and express remorse in their spontaneous narrative provided to the second confederate.

DISCUSSION

The present study investigated pedestrians' pro-social behavior toward an unknown person, who supposedly lost a banknote of different values, through a field experiment organized in a

TABLE 5 | Cross-tabulation of pro-social behavior toward the first confederate and the themes generated during the post-experimental interview ($N = 587$).

Themes	Pro-social behavior toward the first confederate					
	Returned the banknote	Appropriated the banknote	Total	χ^2	*df*	*V*
Non-informative messages	123 (33.4%)	30 (13.7%)	153 (26.1%)	451.276***	6	0.877
Implicitly normative	71 (19.3%)	0 (0%)	71 (12.1%)			
Explicitly/Ostentatiously normative	92 (25.0%)	0 (0%)	92 (15.7%)			
Functional interpersonal cynicism	0 (0%)	101 (46.1%)	101 (17.2%)			
Absurd/Incoherent explanations	9 (2.4%)	50 (22.8%)	59 (10.1%)			
Mercy and support	73 (19.8%)	0 (0%)	73 (12.4%)			
Recognizing/Assuming the mistake	0 (0%)	38 (17.4%)	38 (6.5%)			
Total	368	219	587			

V = effect size (Cramer's V coefficient).
****p < 0.001.*

TABLE 6 | Cross-tabulation of pro-social behavior toward the second confederate and the themes generated during the post-experimental interview ($N = 219$).

Themes	Pro-social behavior toward the second confederate					
	Returned the banknote	Refused to return the banknote	Total	χ^2	*df*	*V*
Non-informative messages	13 (6.6%)	17 (81%)	30 (13.7%)	91.581***	3	0.647
Implicitly normative	0 (0%)	0 (0%)	0 (0%)			
Explicitly/Ostentatiously normative	0 (0%)	0 (0%)	0 (0%)			
Functional interpersonal cynicism	101 (51%)	0 (0%)	101 (46.1%)			
Absurd/Incoherent explanations	46 (23.2%)	4 (19%)	50 (22.8)			
Mercy and support	0 (0%)	0 (0%)	0 (0%)			
Recognizing/Assuming the mistake	38 (19.2%)	0 (0%)	38 (17.4%)			
Total	198	21	219			

V = effect size (Cramer's V coefficient).
****p < 0.001.*

public space. Our study used both a quantitative approach, through which we measured participants' pro-social behavior toward the confederate who lost the banknote, their subsequent pro-social behavior toward a second confederate who exposed their immoral behavior (for those that initially stole the banknote) and the number of words they produced in their explanations, as well as a qualitative one, through which we explored the themes emerging from their interviews and their relation with participants' previous behavior. Based on SEUT, we expected to see a progressive reduction in participants' pro-social behavior toward the confederate who lost the money and in their subsequent behavior toward the second confederate, as their the potential economic gain increased, while based on self-discrepancy and face-negotiation theory we expected to see a higher volume of explanations for those participants who initially appropriated the money than for those who returned it to its rightful owner.

Firstly, regarding global pro-social behavior, 63% of the total number of participants in our sample acted pro-socially and returned the banknote to the person who lost it. This proportion is remarkably similar to the one in Cohn et al. (2019), who found in a sample of 400 Romanians from seven cities (including Timisoara), that the rate of returning "lost" wallets was 63% in the money condition (when they contained 28 RON) and 50% in the no money condition. Compared to Cohn et al. (2019) study, however, in which the amount of money was fixed, the value of the lost money in our study was manipulated, to test its impact on returning rates. Consistent with SEUT, our results show that when the economic gain was experimentally increased, participants' propensity to act in a pro-social manner decreased significantly; their behavior was the least pro-social in the maximum gain condition (500 RON) and the most pro-social in the minimum gain condition (1 RON). Therefore, the majority of participants (almost 75%) behaved pro-socially toward an unknown person needing help in a public space when there was almost no economic gain, yet this percentage dropped to less than half (45%) when their own immediate gain became substantial (approximately 100 euro). Even though no significant differences were detected between the 1, 10, and 50 RON conditions, possibly due to a lack of statistical power, the proportion of those who were willing to help another person in need decreased progressively with the increase in the value of the lost money. These results reconfirm the findings of previous studies (e.g., Newman, 1979; Armantier and Boly, 2011; Castillo et al., 2014), which found that increased benefits lead to less pro-social behavior. However, other studies in the literature (e.g., Azar et al., 2013; Cohn et al., 2019) found that greater rewards yielded more pro-social behavior, which might be partly explained by the non-anonymous nature of participants in these studies. While in the present study participants' anonymity was guaranteed by the place of interaction (a busy public space) and by the fact that no interaction with participants took place before their behavior was measured, in Cohn et al. (2019) study participants were entrusted lost wallets in a highly personal setting (i.e., at their workplace), while in Azar et al. (2013) study they were (in some cases even regular) customers of a restaurant who had previously established some rapport with the waiter on whom they were offered the opportunity to cheat.

Regarding participants' subsequent pro-social behavior toward the second confederate who politely asked them to return the banknote, results show that activating self-discrepancies motivated almost all of those who initially appropriated the banknote to return it in the 1 RON (96.7%), 10 RON (97.4%) and 50 RON (97.5%) conditions, while 88% and 80.3% returned it in the 100 RON and 500 RON conditions, respectively. Such results indicate that, when self-discrepancies were activated, the majority of participants significantly improved their behavior, a result that contradicts the moral consistency evidenced in some studies (e.g., Martens et al., 2010). Thus, becoming aware of discrepancies from personal or societal standards caused most of the tempted individuals to behave inconsistently and revert their previous anti-social behavior. This helped them to restore their sense of morality in two different ways: participants engaging in self-deceiving strategies after their initial moral transgression (i.e., those avoiding the recognition of the discrepancy between their initial behavior and their moral standards, Batson et al., 1997; see also Rustichini and Villeval, 2014) restored moral congruence by acting in line with their own values, whereas the reverting behavior of those participants using other-deceiving strategies can be understood as a form of social signaling and a desire to appear moral in the eyes of others rather than an authentic desire to be moral. This moral hypocrisy (Batson et al., 1997, 1999) through which people are concerned with appearing moral while also benefiting from dishonesty, was also evidenced in the narratives of the participants who acted dishonestly, which indicated that almost 83% of them did not recognize their immoral behavior publicly but tried to justify it instead. Overall, the morally inconsistent behavior evidenced in the present study is in line with other studies that identified a propensity to engage in compensatory behaviors as a response to previous moral transgressions (e.g., Jordan et al., 2011; Mulder and Aquino, 2013). However, our results show that this process does not happen equally for everybody, but it is moderated by personal benefit. While almost all participants reverted their dishonest behavior and returned the previously appropriated banknote in the 1, 10, and 50 RON conditions, almost 20% refused to do so in the 500 RON condition, when their personal gain was significant. When economic gain becomes subjectively significant, people's desire to profit from dishonest behavior increases, yet so does the threat to one's self-image (Cohn et al., 2019). Therefore, it seems that in such high-gain circumstances, people may be more willing to incur the discomfort of self-discrepancy and the cost to their self or public image in exchange for economic benefit, while in low-gain situations their main motivation is to restore moral congruence by engaging in compensatory moral behavior. This malleability in moral behavior due to situational influences attests to the opportunistic, self-serving use of morality, through which individuals balance moral considerations with their self-interested motivations (see also Rustichini and Villeval, 2014).

Making participants aware of their moral transgression (i.e., activating self-discrepancies) not only improved their subsequent

behavior, but also motivated them to engage in a cluster of communicative behaviors to cover up their immoral behavior and negotiate their own self-image. Those who initially stole the banknote used on average two times more words to explain their behavior than those who returned it, and this difference was more pronounced for higher values of the banknote. This may indicate that participants' image in the 500 RON condition was the most threatened by their self-discrepant behavior, which motivated them to engage in face-restoring strategies more than in other conditions. In collectivistic cultures, such as the Romanian one (Hofstede et al., 2010; Gavreliuc, 2011), where an interdependent self-construal pattern is prevalent (Gavreliuc and Ciobotă, 2013; Moza et al., 2021), individuals tend to use more avoidance strategies, less aggressive conflict styles, more obliging and compromising strategies and show more mutual face concern compared to individualistic cultures (Ting-Toomey, 2010). However, the results of our qualitative analysis are in many respects in contradiction with these expectations because, even though they eventually returned the money, the majority to those who initially stole the banknote adopted a cynical (46.1% of them) or absurd or incoherent description of their previous behavior (22.8%) and only in a relatively reduced number of cases they evoked something that could suggest remorse by recognizing and assuming their mistake (in 17.4% of cases), evidencing therefore a desire to appear moral even though they failed to admit their dishonest behavior. Thus, many of them refused to adopt an obliging or compromising strategy and were guided more by an egoistic face concern, without much consideration for the other. Most of the narratives produced by these participants stressed their interpretation in terms of moral hypocrisy, spontaneously activated in few memorable statements (e.g., "Obviously I took the money, because I also get cheated in life, not rewarded. Was I supposed to be the loser when for once I have the occasion to be the winner?," or "I do not give a damn about your research, the only things that matter is to win here and now! Do you think someone is doing charity to me?," or "The thief goes hand in hand with the lord"). Turning to the issue of cross-cultural consistency, participants who behaved pro-socially by returning the money to the person who lost it had narratives that were more dominated by morality (mercy and support) or were explicitly or implicitly normative.

Pro-social behavior was shown to vary considerably across different cultures. For instance, in Cohn et al. (2019) study, the incidence of returning the lost wallets in the money condition was the highest in countries like Sweden (82%), Denmark (82%), Norway (80%) or Switzerland (80%), while in Mexico and Peru it was only 16% and 14%, respectively. The relatively low rate of pro-social behavior in our study could be explained by the persistence of a social background characterized by a high level of social cynicism (Dincă and Iliescu, 2008; Gavreliuc and Gavreliuc, 2018), generalized interpersonal and institutional distrust (Gavreliuc, 2011; Friedlmeier and Gavreliuc, 2013; Voicu, 2020), a prevalent pattern of negative interactional experiences with others (Mihăilescu, 2017; Gavreliuc et al., 2021) and the prevalence of traditionalist and conservative values (Voicu and Voicu, 2007; Gavreliuc, 2011), associated with a visible decline of solidarity toward the "(ordinary) people from Romania" (Rusu, 2020, p. 66). Therefore, this egoistic concern could be interpreted as a functional way of thinking and acting (Gavreliuc et al., 2009) in a society characterized by mistrust and low normative climate, by routinely activating a mechanism of tolerated deviance (Stebbins, 2012). In a social context characterized by these features, the propensity to act pro-socially in spontaneous interpersonal interactions with strangers can prove to be too costly for a lot of individuals.

LIMITATIONS AND FUTURE STUDIES

The present study has a few limitations worth mentioning. A first possible methodological limitation lies in the way facework was operationalized. We considered the higher number of words expressed by the participants that stole the banknote as a sign of their engagement in facework, yet there is a possibility that this represents a proxy for other type of behavior. One way to approach this dilemma is by inspecting the themes emerging from participants' interviews, which show that at least three of the four evoked themes (i.e., interpersonal functional cynicism, absurd explanations, and recognizing/assuming the mistake) could be related to facework. Participants adopting a cynical attitude generally gave a "lesson about life's unfairness" to the confederate interviewing them, possibly as a way of emphasizing their "normal" behavior, while the behavior of those using more words to offer absurd explanations could be understood as a symbolic act to exculpate oneself.

A second limitation that could have impacted the results of the present study is related to the fake banknotes used. Even though they were almost identical to the real ones and differences could not be identified at first sight (as revealed also in the pilot study), there is the possibility that some participants identified them as fake money, which could have affected their behavior. However, it is likely that this realization (if it happened) actually increased the global rate of pro-social behavior, as participants could tell whether the banknotes were real or not only after they picked them up from the ground. In this case, participants had no reason to retain the money and most probably returned them to their rightful owner. It is also worth adding that participants had a chance to justify (to the second confederate) keeping the note on the grounds that it was worthless, but it appears that they did not.

Another limitation resides in the fact that we assumed that confronting participants with their own immoral behavior will generate self-discrepant states and their associated discomfort, yet we did not measure self-discrepancies or participants' emotional states. Also, the theoretical support used to understand and interpret the findings (i.e., self-discrepancy theory) is just one of the possible theoretical lenses through which such findings can be viewed. The behavior of participants can also be understood as form of cognitive dissonance (Festinger, 1957), incongruity (Osgood and Tannenbaum, 1955), or other forms of inconsistent behavior.

Because pro-social behavior is also determined by cultural factors and can vary considerably across different cultures (see Cohn et al., 2019), it is not yet clear whether the same

moderating effect of economic gain on subsequent pro-social behavior can be observed in cultures with a high vs. a low pro-social orientation. In order to understand whether the impact of economic interest in anonymity conditions on pro-social behavior is universally manifested and to the same degree, more research on cross-cultural samples is needed.

Finally, it is clear that pro-social behavior is a complex phenomenon that cannot be explained solely by self-interest motivations (Gibson et al., 2013; Abeler et al., 2014). Future studies should attempt to manipulate further the interplay between costs and benefits by varying different situational (e.g., anonymity and presence of peers) and individual factors (e.g., the salience of moral identity or religiosity) and investigate their interaction with economic interest, to be able to delineate the boundary conditions of such influences on pro-social behavior. A more complex design could also vary the identity of the confederate and its associated stereotypes (e.g., a business person and an exponent of a sexual or religious minority), the type of residence (rural vs. urban), the nature of the place of interaction (private vs. public one), or the type of task required (volunteer vs. non-volunteer one), in order to extend and deepen the analysis. At the same time, we have to caution about generalizing the present results to all types of pro-social behavior. As the task in our study involved low engagement, it is unclear whether in circumstances of higher personal involvement the same effect of economic benefit on behavior can be observed. Future studies will have to investigate this possibility.

CONCLUSION

The present field experiment identified a moderating effect of economic interest on pro-social behavior toward a stranger losing money on the street. Thus, when their potential gain was larger, participants were less likely to return a lost banknote to its rightful owner than when their potential gain was smaller.

Activating a self-discrepant state in those who initially appropriated the banknote, by recognizing their immoral behavior, led them to improve their subsequent behavior and return the stolen banknote in most cases.

However, this effect was again moderated by economic interest such that participants were less likely to return the money in high-gain than in low-gain situations. Moreover, to cover their behavior and restore their threatened image, those who initially stole the money were more likely to engage in a face-negotiation process, during which they used significantly more words to explain their behavior compared to those individuals who acted pro-socially and returned the money. A qualitative analysis of their interviews also revealed completely different themes in their narratives than in the narratives of those who decided to return the money. The present study provides new evidence on the moderating effect of financial interest on pro-social behavior, in a context of public anonymity, with ostensibly high financial stakes involved and in an under-studied culture.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found at: OSF (Open Science Foundation): https://doi.org/10.17605/OSF.IO/H2U5W.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Scientific Committee of the Center for Social Diagnosis from the Faculty of Sociology and Psychology from the West University of Timisoara. Written informed consent for participation was not required for this study in accordance with the national legislation and the institutional requirements.

AUTHOR CONTRIBUTIONS

AG and DG contributed to the conception and design of the study, coordinated the field experiment, organized the database, and wrote the first draft of the manuscript. AG, DG, and AS performed the statistical analysis. All authors contributed to the article and approved the submitted version.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2022.748298/full#supplementary-material

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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ORIGINAL RESEARCH
published: 27 May 2022
doi: 10.3389/fpsyg.2022.854747

The Effect of Ingroup vs. Outgroup Members' Behavior on Charity Preference: A Drift-Diffusion Model Approach

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Providing potential donors with information about the behavior of others (i.e., social information) is an increasingly used strategy to nudge prosocial decision-making. In the present study, we investigated the effect of ingroup vs. outgroup information on participants' charity preferences by applying a Drift Diffusion Model (DDM) approach. In a joint evaluation scenario, we manipulated different levels of ingroup/outgroup preference ratios for two charities within subjects. Every subject was presented with three stimulus types (i.e., high, medium, and low ingroup ratio) randomized in 294 trials divided into six blocks. We expected that for stimuli with a high ingroup/outgroup ratio, participants should more often and faster decide for the ingroup's most favored charity. We expected that the speed of evidence accumulation will be higher the larger the ingroup/outgroup ratio. Additionally, we investigated whether variations in model parameters can explain individual differences in participants' behaviors. Our results showed that people generally followed ingroup members' preferences when deciding for a charity. However, on finding an unexpected pattern in our results, we conducted *post-hoc* analyses which revealed two different behavioral strategies used by participants. Based on participants' decisions, we classified them into "equality driven" individuals who preferred stimuli with the least difference between ingroup and outgroup percentages or "ingroup driven" individuals who favored stimuli with the highest ingroup/outgroup ratio. Results are discussed in line with relevant literature, and implications for practitioners are given.

Keywords: charitable donations, ingroup, outgroup, DDM, social information, conformity

OPEN ACCESS

Edited by:
Gustav Tinghög,
Linköping University, Sweden

Reviewed by:
Milica Mormann,
Southern Methodist University, United States
Andre Luzardo,
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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *14 January 2022*
Accepted: *25 April 2022*
Published: *27 May 2022*

Citation:
Reich LM, Mahr LAM, Vacondio M and Khalid AS (2022) The Effect of Ingroup vs. Outgroup Members' Behavior on Charity Preference: A Drift-Diffusion Model Approach. Front. Psychol. 13:854747. doi: 10.3389/fpsyg.2022.854747

1. INTRODUCTION

Donations made by private persons make up a large part of charitable giving. In the UK alone, there are more than 200,000 registered charity organizations, making it important to answer not only what motivates people to donate in general but also what motivates them to select a specific cause, organization, or program to donate to.

Research has identified various driving factors in charitable giving decisions, for example, the neediness of the recipient (Kogut and Ritov, 2005), identifiability of the donor (Small et al., 2007; Lee and Feeley, 2016), or personality characteristics of the donor such as social value orientation (Van Lange et al., 2007).

An increasingly used and promising strategy of donor acquisition is to provide potential donors with information about the behavior of others (e.g., amount given by previous donors), that is, to implement social information (van Teunenbroek et al., 2020). Learning about others' behavior establishes a social norm to which people are generally inclined to adapt (e.g., Festinger, 1954; Bernheim, 1994). Conformity can foster one's social acceptance, and others can serve as a source of information on what is more effective to do in a given situation (Cialdini and Goldstein, 2004), especially if a situation is new, ambiguous, or uncertain (Goldstein et al., 2008). Based on this knowledge, various programs and campaigns have implemented a social norm approach to promote desirable behaviors (Schultz et al., 2007), including charitable giving (Minguez and Sese, 2021). However, evidence on the effect of social information on donations is not as consistent as one might assume, with several studies showing a positive effect while others find no or even negative effects (van Teunenbroek et al., 2020). Therefore, it is crucial to identify contextual factors under which social information is particularly effective in promoting charitable giving.

A relevant contextual factor is the source of social information, i.e., whose previous behavior is provided to potential donors (van Teunenbroek et al., 2020). According to social identity theory, people base their self-concept in part on the social groups they belong to, accompanied by a differentiation of the social world into in- and outgroups (Tajfel et al., 1979; Turner, 1999). Besides a general tendency to favor ingroups over outgroups (Aberson et al., 2000), research has demonstrated that people are generally more receptive to social influence from ingroup rather than outgroup members (e.g., Abrams and Hogg, 1990; Knippenberg and Wilke, 1992). Moreover, studies on the influence of norms on prosocial behavior show the superiority of ingroup-specific over general norms (e.g., Lede et al., 2019).

Although research in the donation domain has frequently investigated the role of group membership in victims or recipients (e.g., favoring ingroup victims; James and Zagefka, 2017), surprisingly, the group membership of other donors has received little attention. One study examined the effect of ingroup vs. outgroup average donations' anchors on the decision to donate and the amount donated (Hysenbelli et al., 2013). They demonstrated that people tend to donate more when high anchors are attributed to ingroup donors than outgroup donors in a separate evaluation setting. However, to the best of our knowledge, no studies to date have examined the effect of ingroup vs. outgroup information in a joint evaluation scenario, leaving it unclear how group membership affects donation decisions when in- and outgroup information is presented simultaneously. Former research has emphasized that both the decision-making process and its outcomes may differ if attributes and alternatives are evaluated relative to rather than isolated from each other (e.g., Payne et al., 1993; Caviola et al., 2014). Moreover, when the information provided refers to in- and outgroup members' behavior, a joint evaluation scenario creates an intergroup context, increasing the salience of social identity and social intergroup comparison (e.g., Turner, 1999). Therefore, the first aim of our study was to test the effect of ingroup vs. outgroup information on charity preference in a joint evaluation scenario. Further, by applying the Drift-Diffusion Model (DDM, Ratcliff, 1978; Ratcliff and McKoon, 2008; Ratcliff et al., 2016), our second goal was to understand the process of how ingroup vs. outgroup information influences charity preferences while accounting for interindividual differences.

The DDM is a computational model and describes decision-making. Applying the model assumes that evidence is accumulated over time until an evidence threshold is reached that triggers the decision. We assume that decision-makers extract evidence from provided information and receive it from memory. The accumulation process, called drift rate (v), tends in a stochastic manner to either the in- or outgroup response, depending on the evidence. The larger the value of the drift rate, the higher the accuracy and the faster the response (Lerche and Voss, 2019). Three additional main parameters are threshold (a), non-decision time (t_0), and starting point (z). The threshold a defines the relative distance between the thresholds for both choice options. The larger the a, the more information needs to be accumulated. In a speed-accuracy manipulation, it has been shown that by deciding as accurately as possible, the threshold a increases, as does the caution and the accuracy of the decision-maker (Ratcliff and Rouder, 1998). The non-decision time summarizes processes that are not directly involved (e.g., motor responses) in the decision process. When forcing participants to press a key three times in a row (instead of just once) to respond, the non-decision time t_0 increases (Lerche and Voss, 2019). The starting point bias z indicates whether participants are biased toward a response before seeing the task. By randomizing the trial order, the relative bias z should be at 0.50, which means participants are unbiased. Three variability parameter (i.e., sz, st_0, sv) ensure the intertrial variability (for more information; e.g., Voss et al., 2004, 2013; Ratcliff and McKoon, 2008; Wagenmakers, 2009).

Specifically, by applying the DDM, we investigated how varying proportions of in- vs. outgroup members' decisions influence individual decision-making. We expected that for stimuli with a high ingroup ratio, participants should more often and faster decide for the ingroup's most favored charity and that we will find this phenomenon in the drift rate of the model. Furthermore, we assumed that individuals with stronger ingroup identification show even stronger effects on the drift rate parameter. Thus, individuals with a high sense of ingroup-identification have higher drift rate values for charities favored by the ingroup. Nevertheless, it is common in behavioral science that the observed effects do not affect participants homogeneously. Therefore, we also investigated whether variations in model parameters can explain individual differences in participants' behaviors which tend to remain undetected in the analysis of aggregated data (e.g., negligible or even reverse effects).

2. MATERIALS AND METHODS

2.1. Participants and Design

Based on the literature that used a DDM approach similar to our study, we planned for a sample size of at least $N = 30$ participants (see, e.g., Krajbich et al., 2012). To compensate for potential

dropouts, we increased the study's sample size to N = 39 (16 women, 22 men, 1 diverse, M_{age} = 30.44, SD = 9.90). Participants needed to be native English speakers and at least 18 years old to qualify for the study. We recruited UK citizens *via* the online subjects' pool Prolific. Prolific holds good recruitment standards and explicitly informs participants that they are recruited for participation in research (Palan and Schitter, 2018). For the duration of our study (i.e., 1 h), participants received £7.50. No participants were excluded from the analysis. To ensure data quality, we applied the same outlier handling (Lerche and Voss, 2019) did (see Section 2.4).

To examine the effects of other donors' group membership on individual donation decisions, we created an online experiment with lab.js (Henninger et al., 2021). We manipulated participants' group membership experimentally in the first step, manipulated different levels of ingroup/outgroup preference for the two charities within subjects in the second step, and measured participants' decisions as well as decision times. We further measured ingroup identification to evaluate whether ingroup compliance might be elevated for those showing stronger ingroup identification. The study was reviewed and approved by the University's internal ethics committee before data collection.

2.2. Procedure

After obtaining informed consent, the minimal group priming was applied to manipulate participants' group membership (Tajfel, 1970; Bornstein et al., 1983). Across eight trials, participants had to choose which of two presented paintings (either painted by Klee or Kandinsky) they liked the most. Subsequently, all participants received false feedback that Kandinsky painted most of the pictures they preferred and that they would thus be assigned to the "Kandinsky group". Next, they completed a manipulation check, indicating their feelings (sympathetic, warm, soft-hearted, compassionate, tender, moved; Batson et al., 1997) toward the Kandinsky- and Klee group on a 7-point Likert scale (1 = "not at all" to 7 = "very much"). A paired sample t-test indicated our manipulation was successful: participants had significantly more empathy toward their ingroup (Kandinsky; M = 3.47, SD = 1.45) than the outgroup (Klee; M = 2.91, SD = 1.40), $t_{(38)}$ = 4.50, $p < 0.001$, d = 0.72, 95% CI = [0.31, 0.81]. After this, participants answered four items on the strength of identification with their ingroup (e.g., "I see myself as a Kandinsky member"; Doosje et al., 1995) on a 7-point-Likert-scale (1 = "not at all" to 7 = "very much").

In the main experiment, participants were asked to make several decisions on which of two charities they preferred to donate to. For every decision, participants were provided with information on the in- and outgroup members' ostensible behavior (i.e., the number of people from the in- and outgroup that decided to donate to the two charities), resulting in a 2x2 table. Across trials, different levels of in- vs. outgroup preferences were manipulated within subjects by implementing different stimulus types (see stimuli description). The trials' values in columns (A, B) were summed to 100 to ensure comparable choice options regarding the number of donors. They indicated how many donors had already donated to that charity option. The rows (Kandinsky, Klee) are independent and provided information on how many group members donated to one of the charity options.

After eight practice trials, every subject was presented with 294 trials divided into six blocks, with 49 trials each. Every trial started randomly after 200, 400, or 600 ms, after responding by either pressing the key "x" for option A or "m" for option B (see **Figure 1**). Participants had a response window of 5 s, which, when reached, automatically triggered the next trial. After each block, participants had the opportunity to take a break. The blocks were randomized across participants and differed in the instruction that the following 49 decision tasks were charities for either: "cancer charities", "disabled charities", "poverty charities", "medical charities", "elderly charities", or "children's health charities". The general design of this experiment was similar to experimental procedures from multi-attribute decision experiments (e.g., Trueblood, 2012).

In the last part of the study, participants answered demographic questions (age, gender, weekly income) and were given the possibility to describe their decision strategy within an open-response format before being fully debriefed.

2.3. The Stimuli

We created three stimulus types (see **Figure 2**), reflecting three different difficulty levels to follow the ingroup: low vs. medium vs. high. For each stimulus type, we constructed 49 different stimuli by varying the ingroup preference, e.g., stimulus type 1 for option A between [92, 98] and for option B between [12, 18], in a way that each preference for option A is combined with each preference of option B. Each of the 49 stimuli was presented twice by presenting option A, either left or right (and vice versa for option B). The same procedure was applied for stimulus type 2 (option A [92,98], option B [52,58]), and stimulus type 3 (option A [52,58], option B [12,18]) resulting in 294 different stimuli that were randomized for each subject in the experiment. The respective outgroup preference filled the choice options to 100.

Stimulus type 1 represented stimuli where the ingroup preference for one of the two charities was the most obvious. For example, 95 ingroup members chose charity A while five outgroup members chose this option A. Option B was chosen by 13 ingroup members and 87 outgroup members. The ratio of ingroup/outgroup for option A (95/5) dominates the ingroup/outgroup ratio for option B (13/87); thus, the ingroup response is reflected in option A. The difference between the ingroup/outgroup ratio of charity A and charity B was then lower for stimulus type 2 (vs. stimulus type 1; e.g., 54/46 for A, 92/8 for B; B is the ingroup response here) and the lowest for stimulus type 3 (e.g., 54/46 for A, 13/87 B; A is the ingroup response here).

Thus, ingroup preference for one of the two charities was most evident in stimulus type 1, then 2 and then 3. Assuming that participants follow the ingroup, we assumed that it is more difficult to follow the ingroup when the ingroup/outgroup ratio (ingroup preference) between choices (A and B) is lower. Therefore, we expected ingroup conforming responses to be more frequent and faster in stimulus type 1 and consequently less in stimulus type 2. We expected the lowest proportion and the greatest difficulty in choosing the ingroup-compliant option in stimulus type 3.

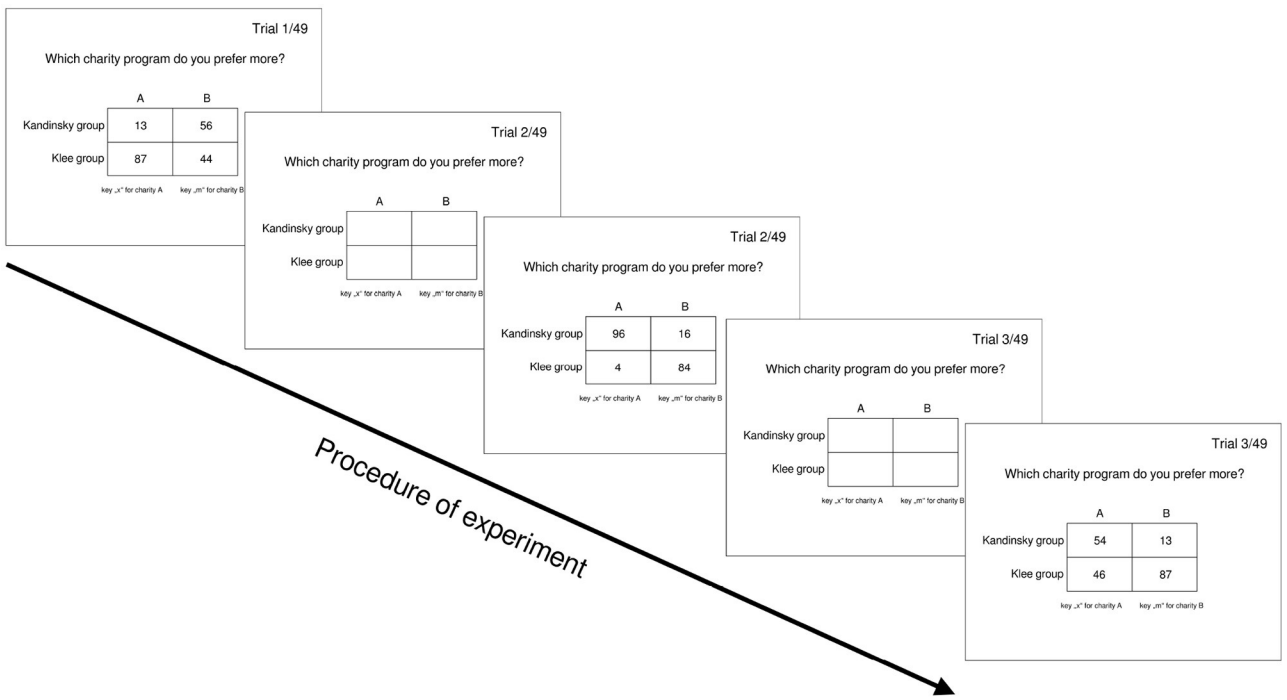

FIGURE 1 | Exemplary representation of the experimental procedure. Between trials, a blank screen was displayed for 200, 400, or 600 ms.

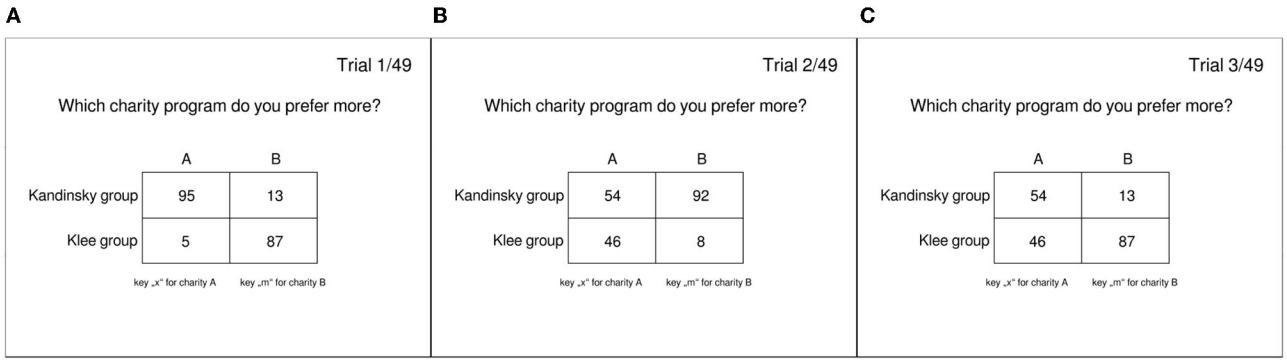

FIGURE 2 | Example of the three stimulus types. Left **(A)** stimulus type 1 (low difficulty), middle **(B)** stimulus type 2 (medium difficulty), right **(C)** stimulus type 3 (high difficulty).

2.4. Data Analysis

The data analysis was done in R (version 4.1.1; R Core Team, 2021). To estimate the DDM, we used the "rtdists" (version 0.11-2; Singmann et al., 2022) and "fddm" (version 0.4-0; Foster, 2022) package. We removed outlier trials for each participant separately, lying more than three interquartile ranges outside of the first and third quartiles of the log-transformed reaction time distribution (see Lerche and Voss, 2019). Furthermore, we removed trials with response times shorter than 200 ms (Schmitz and Voss, 2012). Overall, less than 1.26% of trials were removed. We used maximum likelihood (ML) with nlminb for parameter estimation. This algorithm provides stable parameter estimates for non-contaminated data, such as data without many outlier trials (Lerche et al., 2017).

2.4.1. Drift Diffusion Model (DDM)

For the modeling approach, responses in line with the ingroup were linked to the lower boundary, and responses not in line with the ingroup were linked to the upper boundary. Within our analyses, we defined following the ingroup as choosing the charity that was most preferred by the ingroup (e.g., option A for stimulus type 1 and 3; option B for stimulus type 2; see **Figure 2**)

We tested our research questions by applying two different DDM groups. First, we expected the DDM variant, which allows

for varying the drift parameter over the three different stimulus types, to provide a better Goodness of Fit than other DDM variants that do not allow for varying the drift. By allowing the drift to vary across the stimulus types, the model should capture hypothesized differences in the difficulty of the stimuli. The harder a task gets, the lower the drift should be. Therefore, it seemed reasonable to assume that one drift for all three stimulus types cannot represent this difference in difficulty appropriately. Second, we tested DDM variants to explain how subjects combine the ingroup/outgroup information in a joint evaluation scenario. Unlike the first variants, these variants explicitly consider the ingroup and outgroup information to model the decision by reformulating the drift (v) in a linear decomposition. A similar approach is known from Hierarchical Diffusion Models (e.g., Vandekerckhove et al., 2011; Wiecki et al., 2013). The decomposition of the drift has also been applied to non-hierarchical models, for example, when analyzing eye-tracking data (Krajbich et al., 2012). We expected the DDM variants that consider the ingroup and outgroup information (see Equation 1b) to outperform the DDM variants that only take into account the ingroup information (see Equation 1a).

$$v = \beta_0 + \beta_1 * ingrInfo \quad (1a)$$

$$v = \beta_0 + \beta_1 * ingrInfo + \beta_2 * outgrInfo \quad (1b)$$

One way to use the trial information to inform the drift is to extract and weigh (β_1) only the ingroup information (ingrInfo). However, in a joint evaluation scenario, both the ingroup and outgroup information could be extracted by decision-makers. So we implemented an additional weighting parameter β_2 for the outgroup information (outgrInfo). In both cases, β_0 represents the intercept, i.e., baseline level of evidence accumulation (compare with Trueblood et al., 2014).

2.4.2. Trial Information

We tested five possible ways of extracting trial information for the in- and outgroup. First, the ratio of group members that chose option A and option B; For example, if 95 ingroup members chose A and 13 chose B, then the ratio A/B = 95/13 = 7.31 indicates that option A is 7.31 times more likely for ingroup members than option B. Second, the percentage of group members that chose option A; The percentage of previous ingroup donors for option A is A/(A+B) = 95/(95+13) = 0.88 indicates that 88% of the group members' choice was option A. Third, a more rudimentary abstraction is to set the extracted information into dichotomous information. In our case, we defined the item information to be 1 if the highest number (most of the donors) in the trial table belongs to the ingroup. We also tested models that use alternative-wise comparisons. Here we utilized direct (A-B) and relative A/(A-B) difference (see Dai and Busemeyer, 2014).

2.4.3. Model Selection

The evaluation of Goodness-of-Fit was based on the Bayesian Information Criteria (BIC). We selected one model that had the lowest BIC value from each model group, indicating that this model fits the data, compared to all tested models, most accurately.

3. RESULTS

Response times and choices were analyzed to test the effects of in- vs. outgroup information on individual donation decisions. Secondly, we used the Drift-Diffusion Model to understand how ingroup vs. outgroup information influenced charity preferences. For the analysis, we coded choices into "ingroup compliant" (Kandinsky) or "ingroup non-compliant". Responses that point to the same choice option as the highest ingroup/outgroup ratio were classified as ingroup responses. Answers that do not refer to the highest ingroup/outgroup ratio were declared as ingroup non-compliant responses.

3.1. Behavioral Data

The behavioral data shows that 78% of the overall responses of our sample were ingroup compliant. The median response time for these responses was 697 ms (M = 863 ms; SD = 543 ms), while the median response time for ingroup non-compliant responses was 894 ms (M = 1,081 ms; SD = 667 ms). Comparing the three different stimulus types (see column *all participants* in **Table 1**), an increasing trend in ingroup compliant response times could be identified from stimulus type 1 to stimulus type 3. The choice frequency of ingroup-compliant responses showed a different pattern in contrast to the response time. Ingroup-compliant responses were least frequent on stimulus type 2, followed by stimulus type 3, while these responses were most frequent on stimulus type 1.

To test whether the response times of the ingroup compliant responses differed between the three stimulus types, we conducted a repeated-measures ANOVA. Two participants were excluded from the analysis because they only gave ingroup non-compliant responses to one of the three stimulus types. The sphericity assumption was violated; thus, we applied a Greenhouse-Geisser correction. The stimulus types differed significantly from each other, $F_{(1.78,64.24)} = 4.91$, $\eta^2_G = 0.020$, $p = 0.01$. A *Post-hoc*-Tukey test showed that stimulus type 1 and 3 differed significantly from each other, $t_{(36)} = 2.9$, $p = 0.02$, while the others did not, all $ps \geq 0.17$.

Pearson's product-moment correlation analyses were performed to investigate whether ingroup identification was related to (speed of) ingroup compliant responses. However, for stimuli 1, 2, and 3, the median response time, separated and aggregated over stimuli and responses, did not correlate significantly with group identity, all $ps \geq 0.07$. Only the ingroup choices for stimulus type 3 correlated with ingroup identification, $r_{(37)} = 0.44$, $p = 0.005$, with higher ingroup identification being related to more ingroup compliant choices.

3.2. Modeling Approach of Prosocial Behavior

We created two large model groups in the diffusion approach (see **Table 2**). The first model group (30 models) was tested to determine which item information is used to form participants' decisions. We applied models only informed by the ingroup (β_1) or by the ingroup and outgroup (β_1 and β_2) for all five different item information. We fixed for each combination the starting

TABLE 1 | Behavioral data—*all participants* vs. *ingroup driven* vs. *equality driven*.

Stimulus		All participants		Ingroup driven		Equality driven	
1	% Ingroup choices	0.86	(0.19)	0.95	(0.15)	0.63	(0.3)
	Ingroup RT	0.889	(0.39)	0.823	(0.361)	1.057	(0.53)
	Outgroup RT	1.008	(0.394)	1.01	(0.379)	1.005	(0.416)
2	% Ingroup choices	0.71	(0.27)	0.9	(0.24)	0.24	(0.37)
	Ingroup RT	0.893	(0.369)	0.869	(0.345)	0.96	(0.432)
	Outgroup RT	1.067	(0.559)	1.126	(0.627)	0.937	(0.424)
3	% Ingroup choices	0.76	(0.2)	0.74	(0.18)	0.83	(0.27)
	Ingroup RT	0.976	(0.474)	0.981	(0.468)	0.964	(0.492)
	Outgroup RT	1.123	(0.551)	1.161	(0.525)	1.03	(0.603)

Mean responses and response times per stimuli type. RT in seconds. Standard Error in parentheses.

TABLE 2 | DDM group one.

Model	Par	Par fixed	Par vary	Item information
		Model group 1		
1 (2)	$a, t_0, \beta_0, \beta_1, (\beta_2), sv$	z		$A - B$
3 (4)	$z, a, t_0, \beta_0, \beta_1, (\beta_2)$	sv		$A - B$
5 (6)	$a, t_0, \beta_0, \beta_1, (\beta_2)$	$z + sv$		$A - B$
7 (8)	$a, t_0, \beta_0, \beta_1, (\beta_2), sv$	z		$A/(A - B)$
9 (10)	$z, a, t_0, \beta_0, \beta_1, (\beta_2)$	sv		$A/(A - B)$
11 (12)	$a, t_0, \beta_0, \beta_1, (\beta_2)$	$z + sv$		$A/(A - B)$
13 (14)	$a, t_0, \beta_0, \beta_1, (\beta_2), sv$	z		A/B
15 (16)	$z, a, t_0, \beta_0, \beta_1, (\beta_2)$	sv		A/B
17 (18)	$a, t_0, \beta_0, \beta_1, (\beta_2)$	$z + sv$		A/B
19 (20)	$a, t_0, \beta_0, \beta_1, (\beta_2), sv$	z		$A/(A + B)$
21 (22)	$z, a, t_0, \beta_0, \beta_1, (\beta_2)$	sv		$A/(A + B)$
23 (24)	$a, t_0, \beta_0, \beta_1, (\beta_2)$	$z + sv$		$A/(A + B)$
25 (26)	$a, t_0, \beta_0, \beta_1, (\beta_2), sv$	z		1 or 0
27 (28)	$z, a, t_0, \beta_0, \beta_1, (\beta_2)$	sv		1 or 0
29 (30)	$a, t_0, \beta_0, \beta_1, (\beta_2)$	$z + sv$		1 or 0
		Model group 2		
31, (32), [33]	a_i, t_0, v	z, (sv), $[z + sv]$	a	–
34, (35), [36]	a, t_0, v_i	z, (sv), $[z + sv]$	v	–
37, (38), [39]	a, t_{0i}, v	z, (sv), $[z + sv]$	t_0	–
40, (41), [42]	a_i, t_0, v_i	z, (sv), $[z + sv]$	$a + v$	–
43, (44), [45]	a_i, t_{0i}, v	z, (sv), $[z + sv]$	$a + t_0$	–
46, (47), [48]	a, t_{0i}, v_i	z, (sv), $[z + sv]$	$t_0 + v$	–
49, (50), [51]	a_i, t_{0i}, v_i	z, (sv), $[z + sv]$	$a + t_0 + v$	–

Linear composition of drift v; parameter range for optimization: s = 1, β components: [−15;15], t_0: [0;median(RT_{ID})], z [0;1]; sv [0;15], a [0;15], st0 = 0, sz = 0; fixed pars z =.5, sv = 0.

point $z = 0.5$, the drift variation $sv = 0$ or both parameter which resulted in 30 models.

We used a standard DDM with parameters varying across the three stimulus types in the second model group. For each specific model, we let either threshold a, drift rate v, non-decision time t_0, or combinations of these parameters vary across stimulus conditions. For each combination we fixed either the starting point $z = 0.5$ (note that z is relative to the threshold), the drift variation $sv = 0$ or both of the parameters which resulted in 21 models. By utilizing this model group, we aimed to investigate which parameters can capture the different difficulties for the three types of stimuli.

All models were fitted separately for each subject, and parameters were optimized by using the maximum likelihood algorithm. The best three mean model results for both model groups and aggregated across our sample can be found in **Table 3**.

TABLE 3 | Mean model results of three best-fitting models for each model group and sample (*all participants* vs. *ingroup driven* vs. *equality driven*); TI, trial information used to inform β_1 and β_2; *z* is the relative bias; an empty cell in *z* or *sv* means, that the parameter is fixed to 0.5 and 0, respectively; an empty cell for *a*, or t_0 means, that this parameter was fixed for all three stimuli types—otherwise the parameter was allowed to vary between the stimuli types.

Mod	*z*		*a*			t_0			*v*			*sv*	BIC	TI
								β_0	β_1	β_2			
Aggregated over all participants														
22	0.48		1.99			0.32		1.92	−2.82	−1.11		271.86	A/(A+B)
10	0.49		1.99			0.32		−2.63	0.55	0.40		271.95	A/(A-B)
8			2.25			0.30		−3.44	0.67	0.52	0.8	272.75	A/(A-B)
		*a*1	*a*2	*a*3	$t_0$1	$t_0$2	$t_0$3	*v*1	*v*2	*v*3			
35	0.48		2.01			0.32		−1.58	−0.88	−1.00		268.48	-
46			2.19		0.31	0.31	0.33	−2.08	−1.24	−1.41	0.81	268.95	-
34			2.27			0.30		−2.14	−1.26	−1.39	0.80	269.18	-
Ingroup driven													
22	0.47		1.99			0.32		2.22	−1.32	−3.27		188.29	A/(A+B)
10	0.47		1.99			0.32		−3.37	0.31	0.93		188.57	A/(A-B)
8			2.30			0.30		−4.47	0.43	1.12	0.87	189.27	A/(A-B)
		*a*1	*a*2	*a*3	$t_0$1	$t_0$2	$t_0$3	*v*1	*v*2	*v*3			
35	0.47		2.01			0.32		−2.04	−1.60	−0.93		185.34	-
34			2.32			0.30		−2.80	−2.20	−1.44	0.87	185.99	-
46			2.24		0.30	0.31	0.34	−2.73	−2.16	−1.46	0.88	187.54	-
Equality driven													
12			1.98			0.30		−0.71	1.15	−0.94		484.12	A/(A-B)
10	0.52		1.99			0.31		−0.74	1.16	−0.95		484.19	A/(A-B)
24			1.98			0.30		1.24	−6.6	4.43		484.42	A/(A+B)
		*a*1	*a*2	*a*3	$t_0$1	$t_0$2	$t_0$3	*v*1	*v*2	*v*3			
47	0.50		1.96			0.36		−0.38	0.98	−1.16		473.87	-
48			1.94			0.35		−0.37	0.97	−1.15	0.87	474.79	-
50	0.50	2.20	1.86	2.03	0.34	0.32	0.33	−0.44	0.94	−1.18	0.88	474.85	-

BIC values are highly similar for each group, indicating that each fits the data almost equally well. However, based on the best BIC, we chose model 22 and model 35. Further, both models showed a satisfying fit the 0.10th, 0.30th, 0.50th, and 0.70th quantiles (predicted/observed) overlap for ingroup responses (see **Figure 3**). However, the best fitting models underestimated observed response durations at the higher quantiles. For the 0.90ths quantile, a misfit produced. This is common in Diffusion Model approaches, especially for response times greater than 1 second. Extreme quantiles (i.e., 0.90 quantile) show a less satisfactory fit due to higher variability in response times (Aschenbrenner et al., 2016). The high variability in response times is visualized using the red error bar, which represents 1 for the respective quantiles. We would like to add that we did not exclude any participants and applied a conservative outlier handling.

The basic parameter structure (see **Table 3**) is overlapping, indicating that the hypothesized item difficulty is mapped on the drift *v* parameter. For example, the best fitting models had *z* fixed to 0.5 or estimated the relative starting bias close to 0.5. The evidence threshold *a* did not vary across the stimuli types. Further, we found that the non-decision time component t_0 was not varying over stimuli types for all but one model (model 46). For model group 2, the drift parameter varies across the three stimuli types. Altogether, it seems plausible that the item difficulty is mainly mapped on the drift *v* parameter of the DDM. Negative drift (*v*) and drift-components (β) for trial information A/(A+B) indicate evidence sampling toward the ingroup boundary (ingroup compliant response belongs to the lower boundary). The trial information A/(A-B) is vice versa, meaning that the negative drift and drift components indicate evidence sampling toward the non-ingroup boundary.

In model 22, the positive value for β_0 is relevant to capture the evidence accumulation toward the non-ingroup compliant response since both weighting parameter β_1 and β_2 of the trial

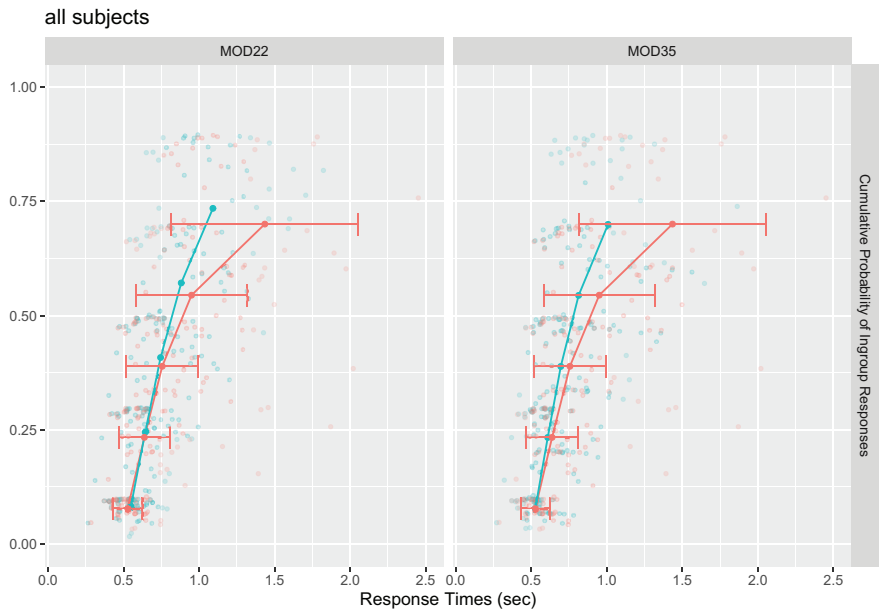

FIGURE 3 | Cumulative Distribution Function Plot—predicted (red) vs. observed (blue) responses for model 22 and 35; quantiles per subject aggregated; only ingroup responses; lines represents 0.10th, 0.30th, 0.50th, 0.70th, 0.90th aggregated quantiles; dots are individual quantiles; standard deviation as error bar; red: observed data; blue: predicted data.

information A/(A+B) strongly tend to the ingroup compliant response (negative sign). Note that weighting parameters for models utilizing A/(A-B) trial information are interpreted in a way that negative values indicate evidence accumulation toward the non-ingroup compliant response.

In model 35, only the drift parameter v varied between the three stimulus types. The drift value was the largest for stimulus type 1, where the ingroup dominates the most. Contrary to our expectations, the smallest ingroup effect occurred for stimulus type 2 and not, as initially predicted, for stimulus type 3.

3.3. Group Identification and DDM Parameter

We computed Pearson's product-moment correlations to test whether individuals with stronger ingroup identification show stronger effects on the drift rate parameter. For model 35, the drift of stimulus type 3 correlated significantly with ingroup identification, $r_{(37)}$ = -0.41, p = 0.009, whereas all other correlations were insignificant (ps > 0.09). Only the third stimulus type correlated with the cognitive process mainly responsible for responding in an ingroup compliant manner, such that stronger ingroup identification was related to faster and more ingroup compliant responses.

3.4. Classifying Participants as "Ingroup Driven" and "Equality Driven"

We performed further *post-hoc* analyses to investigate whether variations in model parameters can explain individual differences in participants' behaviors. Both DDM models showed unexpected results. Parameter estimates for model 22 (which uses in- and outgroup information) show that a median drift component β_2 for the outgroup information of 0.02 (M = −1.11; SD = 5.91). However, the large standard deviation indicates that not all people were affected equally by the information provided.

The drifts for each stimuli type in model 35 further indicated that not all people were equally affected by other donors' group membership. We found the ingroup compliance effect across all stimulus types and in the individual stimuli. However, we were able to show through the modeling approach that the ingroup effect was stronger in stimulus type 1 than in stimulus type 3 and stimulus type 2 but weaker in stimulus type 2 than in stimulus type 3. We, therefore, concluded that some participants used a different decision strategy for stimulus type 2.

Participants' self-reported decision strategy suggested the possibility that some might have built their decisions on equality considerations, i.e., they chose the option that showed the smallest discrepancy between ingroup and outgroup. A reinspection of the experimental stimulus types revealed that, without this being initially intended, within stimulus type 2, participants could follow one of two strategies: follow the ingroup or choose equality. If they followed the ingroup, they chose the charity that presented the highest share from the ingroup independently of the preference of the outgroup. If they chose equality, participants chose the charity that minimized the difference in the share of the in- and outgroup (e.g., charity A: 92 share ingroup, 8 share outgroup; charity B: 54 share ingroup, 46 share outgroup). When being presented with stimulus type 3, participants could follow the ingroup and decide based on equality by choosing the same charity. In this stimulus type,

one of the two options represents the charity most shared by the ingroup and the lowest difference in share between the ingroup and outgroup choices (e.g., charity A: 13 share ingroup, 87 share outgroup; charity B: 54 share ingroup, 46 share outgroup). Finally, stimulus type 1 presented a clear ingroup preference of the ingroup toward one of the two charities but did not present the possibility to choose equality since the difference between the share of the ingroup and outgroup in the two charities was large (e.g., charity A: 95 share ingroup, 5 share outgroup; charity B: 17 share ingroup, 83 share outgroup).

To investigate whether this possible alternative strategy would be reflected in the data, we split our sample into two strategy groups and reran our main analyses on an exploratory basis. Group assignment of participants was based on ingroup compliant behavior in stimulus type 2, as this stimulus forced participants to decide either to follow the ingroup or choose equality. Participants with 50% or less ingroup compliant decisions were assigned to the "equality driven" group (11 subjects). In contrast, those with more than 50% of ingroup compliant decisions were assigned to the "ingroup driven" group (28 subjects).

3.5. Rerunning Analyses

To rule out the possibility that the difference in behavior in the two subgroups merely resulted from a discrepancy of the group membership manipulation's effectiveness, we ran our manipulation check separately for both groups. A paired sample *t*-test indicated that our manipulation was successful for "ingroup driven" participants, as they showed significantly higher levels of empathy toward their ingroup (Kandinsky; $M = 3.58$, $SD = 1.42$) compared to the outgroup (Klee; $M = 2.98$, $SD = 1.47$), $t_{(27)} = 3.90$, $p < 0.001$, $d = 0.73$, 95% CI = [0.28, 0.92]. For the "equality driven" subjects, participants reported higher levels of empathy toward their ingroup (Kandinsky; $M = 3.20$, $SD = 1.56$) compared to the outgroup (Klee; $M = 2.74$, $SD = 1.27$). However, this difference was not significant, $t_{(10)} = 2.18$, $p = 0.05$, 95% CI = [−0.01, 0.92]. A sensitivity power analysis for the paired sample *t*-test suggested our sample size of 11 participants provided 80% power to detect a minimum effect size of $d = 0.94$, indicating that the sample size of the "equality driven" subgroup provided reasonable power only for detecting a considerably large effect.

3.5.1. Ingroup Response Times for Ingroup and Equality Driven Participants

We reran the repeated-measures ANOVA for both participant groups to test whether "ingroup driven" and "equality driven" participants respond faster for ingroup compliant choices. One "ingroup driven" participant was excluded from the analysis because they only gave ingroup non-compliant responses to one of the three stimulus types. The sphericity assumption was violated; thus, we applied a Greenhouse-Geisser correction. The stimulus types differed significantly from each other, $F_{(1.21,31.59)} = 11.87$, $\eta^2_G = 0.048$, $p < 0.001$. A *Post-hoc*-Tukey test showed that stimulus type 1 and 3, $t_{(26)} = 4.8$, $p < 0.001$, and stimulus type 1 and 2, $t_{(26)} = 4.4$, $p < 0.001$, differed significantly, while stimulus type 2 and 3 did not, $p = 0.15$. For the equality "driven participants", we did not find any significant differences for ingroup compliant response times for the stimulus types, $p = 0.55$ (see **Table 1** for mean responses and response times aggregated across participants and aggregated for both groups).

3.5.2. Modeling Results for Ingroup and Equality Driven Participants

By rerunning the analysis separately for both subgroups, the fit for ingroup-driven participants improved, while the fit for the equality-driven subjects became worse compared to the aggregated data (see **Table 3**). The best fitting model in the first model group, model 22, showed that outgroup information (mean [median] $\beta_2 = -3.27[-1.45]$) and the ingroup information ($\beta_1 = -1.32$ [−1.49]) for "ingroup driven" subjects accumulate toward a ingroup response. For the "equality driven" subjects, the best fitting model in group 1 shifted from model 22 to model 12. Responding ingroup compliant for "ingroup driven" subjects, model 35 showed that stimulus type 1 was easier than stimulus type 2, which in turn was easier than stimulus type 3.

Concerning ingroup identification, it showed that for "ingroup driven" participants, ingroup identification is negatively correlated with the drift for the third stimulus, $r_{(26)} = -0.54$, $p = 0.003$, indicating that participants were more ingroup compliant on this stimulus when their ingroup identification is higher.

4. DISCUSSION

In this experiment, we aimed to investigate the effect of ingroup vs. outgroup information in a joint evaluation scenario by applying a DDM approach. Specifically, we argued that people would be more likely to follow the ingroup, i.e., charity options with a high ingroup ratio would be chosen faster and more often than charity options with a low ingroup ratio.

We tested our research questions by applying two different DDM groups. The first group investigated how participants used the presented ingroup/outgroup ratio in a joint evaluation scenario. The second model group investigated whether drift rate differences emerge when varying the ingroup/outgroup ratio. Model 22 for model group one and model 35 for model group two showed the best fit.

Results from model 22 confirmed our intuitions. We showed that it is easier for participants to follow the ingroup and that they were faster when they do so. As a result, participants were more likely to choose the charity most preferred by their ingroup. We also demonstrated on aggregated data that they were using the information provided about the ingroup preferences to follow the ingroup (i.e., a negative β_1).

Within model 35, it showed that, unlike initially predicted, people most often and fastest decided ingroup compliant in stimulus type 1, followed by stimulus type 3, followed by stimulus type 2. This partially aligned with the initial assumption: people were fastest and most often decided according to their ingroup when the ingroup preference was most explicit and obvious. However, we observed a reverse pattern for stimuli 2 and 3, i.e.,

participants showed more and faster ingroup compliant decisions for stimuli with the least evident ingroup preference compared to stimuli where ingroup preference was less obvious but still clearly evident.

When examining the stimulus types more closely, it became apparent that participants might have used two different strategies in their decision process, as some decision options reflected following the ingroup. In contrast, other options allowed decisions based on fairness and equality considerations. While stimulus type 1 lacked an option for equality, the nature of stimulus type 2 forced participants to choose between an option that reflected following the ingroup or following equality. For stimulus type 3, following the ingroup and following equality were reflected in the same decision option. This might explain why more and faster ingroup compliant decisions were observed in stimulus type 3 compared to stimulus type 2, as here ingroup compliant decisions did not conflict with the alternative decision strategy of equality that participants might have used.

Based on participants' self-reported strategy and participants' decisions for ingroup compliance or equality in stimulus type 2, we sorted participants by type of strategy that might be reflected in their decisions into an "ingroup driven", and "equality driven" group and reran our analyses separately for these groups. When we reran the model group 1 for the "ingroup driven", the same models as for the aggregated data showed a superior fit. Further, an increase in the model fit could be found. The "ingroup driven" also used the outgroup and ingroup preferences to follow the ingroup (i.e., negative drift component for trial information A/(A+B)). The higher the outgroup preference for one charity, the more likely they chose the other, ingroup preferred charity. We can speculate that people who blindly conform to their ingroup use the ingroup and outgroup information to maximize their conformity. For the "equality driven" participants, slightly different best-fitting models were found, although these models used the same information type as the best fitting models for the "ingroup driven". "Equality driven" participants used the ingroup preferences to decide in favor of the ingroup (i.e., negative drift component see model 24) and the outgroup preferences to follow the outgroup (i.e., positive drift component see model 24). In this case, we can speculate that they were less biased toward the ingroup, therefore using more equally the information provided.

When we reran model group 2, we showed that for "ingroup driven" participants, the best-fitting models were also the best fitting models for the aggregated data. "Ingroup driven" participants displayed highly and particularly fast ingroup compliant decisions. Moreover, "ingroup driven" participants behaved in a way that matched our initial prediction: they showed the most and fastest ingroup compliant decisions for stimulus type 1, followed by stimulus type 2, followed by stimulus type 3. The "equality driven" participants, in contrast, did not show the same level of ingroup compliance in their decisions. For this group, the highest and fastest ingroup compliant decisions emerged for stimulus type 3, where following the ingroup and following equality was reflected within the same decision option, followed by stimulus type 1. In stimulus type 2, where participants were forced to choose between following the ingroup or equality, "equality driven" participants more often and faster decided on the better-fitted equality option rather than an ingroup compliant approach. As for the ingroup orientated participants, the stimulus difficulty seemed to be mapped on the drift rate.

Based on these results, we might speculate that participants indeed used two different strategies when deciding which charity they should donate to: While most people followed the ingroup in all of the decision scenarios, there was also a smaller group of participants that seemingly strived for equality when circumstances allowed it. However, none of the tested models in the present study captured the presumed equality-oriented behavior well for these equality-oriented participants. Future research could address such equality-based models systematically.

Overall, our results align with research showing that people tend to favor their ingroups and orient toward other ingroup members. Information on ingroup members has been found to trigger greater in-depth processing reflected in neuronal activity (Bavel et al., 2008). People also show better performance in remembering information somehow associated with an ingroup, even if this association is incidental rather than substantial (Jeon et al., 2021). On a cognitive level, this suggests that ingroup information automatically attracts attention as it is considered as more relevant to the self. Besides conformity effects arising from the desire to be socially accepted by other group members, in our context of minimal groups, it seems reasonable that people used ingroup members' most favored decision as a heuristic for how to behave, for what is "the right thing to do." Indeed, research on morality judgments shows that group membership plays an essential role when people use a "what is common is good" heuristic. Whereas commonality of behavior among ingroup members is used as an indicator for the behavior's morality, its commonality among outgroups is rather irrelevant or weakly related to morality judgments (Goldring and Heiphetz, 2020). While former research has already established such an ingroup sensitivity effect in donation decisions when either information on in- or outgroup members' behavior is presented (Hysenbelli et al., 2013), we were able to extend these findings to situations where people were simultaneously confronted with in- and outgroup behavior.

At the same time, our results are also consistent with literature pointing out that the extent of intergroup bias might be dependent on interindividual differences. Specifically, value and social orientations have been found to moderate the strength of intergroup bias (Hewstone et al., 2002). For example, humanitarian and egalitarian values are related to lower prejudice and more positive intergroup attitudes across different types of outgroups (Biernat et al., 1996; Biernat and Vescio, 2005). For individuals personally motivated to avoid prejudice, automatic activation of egalitarian goals even alleviates implicit forms of negative outgroup bias (Johns et al., 2008). In fact, it has been argued that individuals' endorsement of anti-egalitarianism or situations where some social groups dominate others is a stable trait called Social Dominance Orientation (SDO) that predicts negative intergroup attitudes (Sidanius and

Pratto, 1999). Preferential allocation to the ingroup has also been found to be correlated with SDO in a minimal group setting (Amiot and Bourhis, 2005). Similarly, social value orientation (SVO) has been shown to moderate ingroup favoritism in a conflict setting (De Dreu, 2010). Furthermore, individuals with a prosocial value orientation (vs. a pro-self-value orientation) invest more effort and spend more time on information search in an outgroup decision setting (Rahal et al., 2020). This suggests that individuals with fairness concerns are more likely to pay attention to outgroup information when making a prosocial decision – a conclusion also supported by the present research.

These findings, however, typically refer to intergroup bias reflected in attitudes toward or treatment of outgroup compared to ingroup members. Based on the results of our study, one might speculate that traits such as SVO also influence intergroup bias when it comes to decision formation with outcomes unrelated to the in- and outgroup (i.e., the target of donation was never an in- or outgroup member but rather a third party with no group membership stated). To be able to validate this presumption, future research might thus examine whether explicit measures of social value orientations are related to participants' ingroup conformity when information on in- and outgroup behavior is presented.

In the aggregated data analysis for model 51, we found a correlation between the drift parameter and group identity for stimulus type 3, indicating that the stronger people identified with their ingroup, the more likely they decided to choose it. However, we did not find a significant correlation between group identification and the drift parameters of stimulus types 1 and 2. One potential reason we find this pattern of results could be that, in stimulus type 3, compared to the other stimuli, the ingroup has only a slight preference for one charity over the other [e.g., Charity A: 55 (ingroup)/45 (outgroup), Charity B: 13 (ingroup)/87 (outgroup)]. In other words, out of all the stimuli, stimulus type 3 is the most ambiguous with regard to ingroup preference. Therefore, we can speculate that people with higher ingroup identification were more likely to follow the ingroup when making a decision based on highly ambiguous group preference information.

We also investigated the correlation between group identification and drift parameters for each of the two groups (i.e., "ingroup driven" and "equality driven"). For participants classified as "ingroup driven", we find the exact same pattern that we found for the aggregated data, i.e., a significant correlation between drift parameter and group identification for stimulus type 3, but none for stimulus type 1 and 2. However, for "equality driven" participants, we found no significant correlation between group identification and drift parameters (participants' likelihood to follow the ingroup). Thus, the pattern we found for the aggregated data was mainly due to the behavior of the "ingroup driven" participants and not the "equality driven" participants. "Ingroup driven" participants with higher group identification were more likely to follow the ingroup even in an ambiguous context. This lends support to our behavioral classification of participants as "ingroup driven" and "equality driven".

In general, higher ingroup identification can be but is not necessarily connected to greater intergroup bias (Hewstone et al., 2002; Dovidio and Gaertner, 2010). In our study on the effect of intergroup social influence on individual decision making, ingroup identification only played a role for stimulus type 3, which was the only stimulus type where the number of outgroup donors outweighed the number of ingroup donors. Although being speculative at this point, this dominance of the outgroup might have been perceived as an implicit threat to the ingroup's power and sovereignty. For natural groups (i.e., immigrants), it has been found that outgroup size is positively related to higher levels of perceived intergroup threat and discriminatory attitudes (Schlueter and Scheepers, 2010). Additionally, intergroup bias is known to increase under perceptions of group threat, and such effects tend to be stronger for those with higher levels of ingroup identification (e.g., Smurda et al., 2006; Rios et al., 2018). Highly identified individuals also express enhanced conformity to ingroup specific values when they consider their ingroup as being threatened (Jetten et al., 2002; Morrison and Ybarra, 2009), and they show more loyalty to a low-status ingroup when being given the possibility to move to a higher status outgroup (Ellemers et al., 1997). Thus, if participants in our experiment indeed regarded the outgroup dominance as a kind of threat, it would seem reasonable that the high identifiers among "ingroup driven" participants were even more likely to stand in line with the ingroup when making their decision for this stimulus type.

There are limitations we must take into account when interpreting our results. One limitation is that this study was done in an artificial, laboratory online experiment, making it unclear how these findings would translate to real-world scenarios. While this allows us to identify basic psychological processes under high internal validity, future studies might benefit from building on our findings in more naturalistic settings. For example, we gave participants a contrived choice with limited information, which is not typical in most donation settings, and used minimal instead of natural groups. Although minimal groups have several advantages, such as a lack of confounding factors that arise from known stereotypes, the external validity of this paradigm is low. Thus, it is unclear whether the current findings regarding the classification of participants' into "equality driven" and "ingroup driven" individuals would hold for real-world groups. "Equality driven" individuals might not be motivated by fairness if the outgroup charity supported a cause that was particularly abhorrent to them (e.g., a racist organization). Future research that attempts to replicate our *post-hoc* analysis should also investigate under what group contexts these two different strategies emerge. Additionally, the focus should shift from models that describe aggregated data to models that best fit single strategies, particularly if these strategies can be replicated successfully in future studies. Further, our experimental design did not allow us to capture all possible kinds of ingroup vs. outgroup proportions (e.g., cases where options were weakly preferred by more outgroup than ingroup members). Thus, future research might benefit from adding more variety within the stimulus types.

Our analytic strategy also has a few limitations. The DDM approach assumes a single-stage process. Although our results

suggest that participants maintain their strategy across all stimuli (indicating a single-stage process), we cannot rule out the possibility that the initially chosen strategy is replaced by another strategy in the same decision-making process (multi-stage process). Furthermore, the best-fitting parameter (point estimate) is determined using maximum likelihood estimation. By applying Bayesian estimation, one can use the highest density interval (HDI) for each parameter (e.g., the drift *v*) within participants to test for intra-individual differences between stimulus types. A plausible assumption might be that the HDI should be small for easy choices, as opposed to difficult choices.

Future research should focus on these parameter deviations as an indicator of the strength of commitment to a strategy. For example, lower drift parameter deviation may indicate greater certainty in participants' strategy choices. While keeping these limitations in mind, the present research provides first valuable insights into the cognitive process underlying donation decisions when information on ingroup and outgroup members' behavior is presented simultaneously. Specifically, the DDM approach revealed two types of donors that process in vs. outgroup information differently. Although people generally tended to follow the ingroup through their decisions, there was also a group trying to minimize differences between the in- and outgroup, therefore trying to be as unbiased as possible toward their ingroup when being presented with the behavior of both groups at the same time. Recognizing this may have important implications when using a social norm and social identity approach for donor acquisition and can help charities and other fund raising organizations in designing tailored and effective campaigns for their causes and target groups.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/6kv3b/?view_only=dfe7e6a6801f410a84daa2f21b9fbd9c.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Ethics at the University of Klagenfurt (ER-AAU). The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

LR: conceptualization, formal analysis, visualization, writing, reviewing, and editing. LM, MV, and AK: conceptualization, writing, reviewing, and editing. All authors contributed to the article and approved the submitted version.

ACKNOWLEDGMENTS

Acknowledgments are due to Mario Graf (University of Klagenfurt) for implementing the experiment and exporting the data. We also thank Bartosz Gula (University of Klagenfurt) for general feedback and computational modeling advice, and Stephan Dickert (University of Klagenfurt, Queen Mary University of London) and Enrico Rubaltelli (University of Padova) for their suggestions during the development of the project.

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frontiers | Frontiers in Psychology

ORIGINAL RESEARCH
published: 02 June 2022
doi: 10.3389/fpsyg.2022.800867

The Influence of Proportion Dominance and Global Need Perception on Donations

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Many donation-raising platforms request that first-time donors choose the charitable causes they most care about so that future campaign recommendations can best match donors' charitable preferences. While matching charitable campaigns to donors' reported preferences has its benefits, little is known about other effects that choosing charitable causes may evoke. We focus on how choosing charitable causes influences charitable behavior. We find two effects of the number of charitable causes donors choose on their subsequent charitable behavior. In studies 1 and 2, we show that a reference number of the maximum charitable causes donors can choose has a negative effect on charitable behavior. A small (versus large) reference number yields a greater likelihood to donate and a higher donation amount. This effect is aligned with the proportion dominance rationalization. In studies 3 and 4, we show that the number of charitable causes donors voluntarily choose as important to them is positively associated with subsequent charitable behavior. This association is mediated by global need perception. As the number of causes donors choose increases, donors experience an escalation in their perception of global neediness, which in turn motivates their willingness to donate and the donation amount. In Study 5, we show how the two effects together shape charitable behavior. These effects are observed while controlling the donors' inherent prosocial attitudes toward help giving. With more than 1.5 million registered non-profit organizations operating in the United States (National Center for Charitable Statistics, 2019), it has become almost impossible for donors to easily choose which charitable campaigns to support. Online charitable fundraising platforms (e.g., One Today by Google, Round Up, and Charity Miles), websites (e.g., AmazonSmile) and crowdfunding platforms (e.g., Fundly, JustGiving, and GoFundMe) try to ease donors' search and decision processes by offering them personalized charitable options. First-time donors are asked to indicate the charitable causes they care most about, and then asked to donate to charitable campaigns that best match their preferences. Interestingly, little is known about how this initial stage of choosing charitable causes influences subsequent donation behavior. In this research, we ask how choosing the charitable causes one cares most about influences subsequent response to a charitable appeal. Obviously, the mere selection of preferred causes enables charities to offer personalized campaigns and create a better fit between non-profits and donors, which has a generally positive effect on charitable giving. However, in this research we focus on an overlooked aspect of these practices. We examine how the number of charitable causes donors indicate as important to

OPEN ACCESS

Edited by:
Conny Ernst-Peter Wollbrant, University of Stirling, United Kingdom

Reviewed by:
Uma R. Karmarkar, University of California, San Diego, United States
Erik Løhre, BI Norwegian Business School, Norway
Lewend Mayiwar, BI Norwegian Business School, Norway in collaboration with reviewer EL

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *24 October 2021*
Accepted: *27 April 2022*
Published: *02 June 2022*

Citation:
Ein-Gar D and Give'on A (2022) The Influence of Proportion Dominance and Global Need Perception on Donations. Front. Psychol. 13:800867. doi: 10.3389/fpsyg.2022.800867

them influences their donation giving. We test two opposite effects: the *proportion dominance effect*, an effect driven by prior research, and *the global need perception effect*, a new effect identified in this article. Both effects are driven by the number of causes donors choose.

Keywords: charity, donations, prosocial, help-giving, proportion dominance, choice, reference point, need perception

INTRODUCTION

Donors constantly make choices; they choose charitable causes (e.g., world hunger, human rights), charitable organizations (e.g., savethechildren.org, childfund.org), charity-raising platforms (e.g., gofundme.com, fundly.com), as well as specific donation recipients (e.g., a needy child in Africa, a needy immigrant in the United States).

Research on the choice of donation recipients shows that when donors need to choose between helping a group or helping a single person, they prefer to donate to the group (Kogut and Ritov, 2005; Ein-Gar et al., 2018), suggesting that helping many is valued more than helping one. However, this size-valuing effect may be sensitive to group size. For minimal groups, such as a group of three needy individuals, the effect was not replicated (Erlandsson, 2021). When the choice is between two single people in need, donors may face a moral dilemma between the wish to help and the wish to do so in a fair manner. When fairness concerns arise, 35–50% of prospective donors decide to avoid choosing altogether (Ein-Gar et al., 2021). However, if the two people in need differ on some attribute such as gender or physical attractiveness, then donors are more likely to choose one over the other, basing their decision on peripheral attributes such as beauty (Cryder et al., 2017) or gender (Bareket et al., 2022; Ein-Gar et al., 2022).

When donors need to choose one of several charitable organizations or campaigns, familiarity becomes a key factor. Familiar charities and campaigns are chosen more often than non-familiar ones (Soyer and Hogarth, 2011). Offering donors choice sets that vary across many options (8 vs. 16; 7 vs. 13) does not increase donation likelihood (Soyer and Hogarth, 2011). It may even reduce the likelihood of helping because of decision difficulty (Carroll et al., 2011).

The above studies demonstrate that when donors need to choose one donation recipient (a specific individual, a campaign, or an organization) certain underlying processes come into play. However, different underlying processes take place for different types of choices. For example, donors may feel that it is unfair to choose one needy individual over another similarly needy individual, but may not feel it is unfair to choose one charitable cause (e.g., immigrants) over another similar charitable cause (e.g., minorities). Furthermore, these processes are unique to choosing a specific recipient that will directly benefit from the decision. However, donors sometimes make sequential decisions, where the first decision involves a choice that does not directly influence a recipient. For example, donors may begin by choosing a general charitable cause (e.g., helping children from underprivileged backgrounds), then choose an organization (e.g., worldvision.org), and only then decide to help a specific recipient (e.g., donating to a specific child who can't afford school supplies). In these cases, other processes may drive charitable behavior. Furthermore, in most of the aforementioned studies, the underlying process was relevant to cases where donors were obligated to choose a single option. It is, therefore, essential to broaden the research investigation of how choices influence donation behavior to choices that do not directly influence a specific recipient and to decisions that involve choosing more than one option (as in the case of choosing charitable causes). These broader choice settings may reveal new underlying processes that influence charitable behavior.

The current research focuses on how an initial stage of choosing several charitable causes (rather than choosing a single donation recipient) influences subsequent donation-giving behavior. Specifically, we ask participants to choose several charitable causes they care most about and test whether the number of charitable causes they choose influences their donation giving. We explore two effects driven by the number of charitable causes chosen. We first test how giving participants a number referencing the maximum number of causes they need to choose influences their subsequent donation giving (studies 1 and 2). We find that a large reference number (being asked to choose 8 or 10 causes) yields fewer donations than a small reference number (being asked to choose 4 or 5 causes). These findings are in line with prior research on the effect of proportion dominance. We then test whether the actual number of causes that donors can voluntarily choose from (being asked to choose up to 7 causes, without manipulating a fixed reference number) is related to their donation behavior (studies 3 and 4). We find that the more causes donors select as important to them, the more they are likely to donate. Our reason is that the voluntary process of choosing more causes activates an escalation of global need perception, which in turn increases donation giving. Finally, we show how both effects together influence donation giving (Study 5).

Reference Number and the Proportion Dominance Effect

Proportion responding occurs when individuals make decisions based on a proportion inference rather than absolute quantity (Mata, 2016). Thus, for example, individuals consume more quantities of food when they are offered food from a large bowl versus a small bowl. The quantity consumed is assessed as a portion of the entire bowl. As a result, the same quantity is perceived as proportionally smaller when the bowl is large compared to when it is small. A similar phenomenon, termed the proportion dominance effect, has been found in risk assessment

and help giving (Baron, 1997; Slovic et al., 2002). Individuals are more likely to help victims that are part of a small group than to help the same number of victims that are part of a large group (Bartels, 2006; Erlandsson et al., 2014, 2015). This effect is attributed to an assessment of the utility of the help given. Individuals perceive higher utility when they help 10 out of 11 people in need than when they help 10 out of 1,000 people in need, despite the fact that in both cases the absolute number of people being helped is the same (Erlandsson et al., 2014). The effect was even observed when the proportion was higher yet the absolute quantity was lower, such that individuals prefer saving 10 out of 10 lives over saving 11 out of 100 lives (Mata, 2016). The smaller the reference group, the greater the perceived impact of the help giving (Friedrich and Dood, 2009). Some studies suggest that this effect is influenced by mental representations. When individuals think about their decision in terms of a group rather than in terms of many individuals, the effect strengthens because helping a large proportion of a whole unit (i.e., a group) is more satisfying than helping a small proportion of many units (Bartels and Burnett, 2011). This effect was found for decisions involving helping not only humans but also non-humans such as animals (Bartels and Burnett, 2011).

Following this logic, we propose that when individuals first choose the charitable causes they care about and then consider helping a campaign related to one of the causes, they evaluate the impact of their donation in reference to the number of causes they care about. The smaller the reference number of causes, the more impactful the donation feels. Therefore, helping one cause (by supporting its campaign) out of three important causes would be valued more than helping one cause out of seven important causes.

According to this reasoning, we hypothesize that the smaller the reference number of charitable causes donors consider as important to them, the greater their willingness to donate and their donation amount.

Number of Causes Selected and the Global Need Perception Effect

Informing prospective donors about causes, non-profits, and groups or individuals in need of help is a prerequisite for donation giving. According to the literature review by Bekkers and Wiepking (2011), need awareness is the first mechanism that drives donation giving. Once aware of a need, donors assess its extremeness before deciding whether to reach out and help. Need assessment can be based on such aspects as the helplessness of the victim, as in the case of children (Lee and Feeley, 2016); the severity or urgency of the cause, as with organ donations (Tsai et al., 2000); or the magnitude of the need, as in the case of humanitarian crises (Bennett and Kottasz, 2000; Huber et al., 2011). The more individuals perceive the intensity of the need, the more likely they are to provide help (Wagner and Wheeler, 1969; Schwartz, 1974).

Most, if not all, research on need perception has focused on a specific need related to a specific cause, event, group, or individual. Thus, for example, the well-established effect of the identifiable victim (Kogut and Ritov, 2005) suggests that a specific individual with a specific, vivid need raises more charitable responsiveness than statistical victims or charitable organizations (Kogut and Ritov, 2011; Ein-Gar and Levontin, 2013; Lee and Feeley, 2016). This effect, which is driven by the salience of a single person in need, diminishes when donors become aware of others who have a similar need yet are not given help (Västfjäll et al., 2015; Ein-Gar et al., 2021).

In this research, we propose that in addition to awareness of a specific need, there also may be an awareness of the overall neediness in the world. We define "global neediness perception" (hereafter GNP) as a reflection of donors' perception regarding the extent to which there are few or many causes in the world that need charitable support. For example, some individuals may feel that geopolitical and environmental changes (e.g., polarized societies, global warming, industrialized pollution, and global pandemics) have increased the number of social and environmental causes that need charitable support. Others might feel that social and technological advances such as social and environmental movements and advancements in agrotechnology, biotechnology, and medicine offer solutions to many social and environmental problems and that overall need in the world is declining.

Global neediness perception may reflect a relatively stable individual difference. Thus, for example, in an online survey among US participants (Prolific, $n = 501$, $M_{age} = 40$, November 2021), we examined the relationship of GNP with other individual differences. Specifically, participants read: "Some people feel that there are many important social issues in need of charitable support, while others feel that everything narrows down to a few general important issues in need of support." Participants then reported their estimation of the number of social issues in need of support in the world on a 5-point scale ranging from 1- very few causes to 5- numerous causes. Participants also answered the Helping Attitudes Scale (HAS; 20 items, $\alpha = 0.89$; Nickell, 1998) and the Fear of COVID-19 scale (FCV-19; 7 items, $\alpha = 0.91$; Ahorsu et al., 2020). The order of all measures was randomized. GNP was positively correlated with differences in individuals' concerns regarding the COVID-19 pandemic ($r = 0.195$, $p < 0.001$) and with a prosocial personality reflected by general attitudes toward help giving ($r = 0.33$, $p < 0.001$).

However, as with many other individual differences, this perception can be temporally altered. For example, in two unrelated studies asking US participants to donate in different contexts, participants also reported their GNP at the end of the studies (same introduction to GNP as in the previously described study; single item, scale 1–7). One study (Prolific, $N = 440$, $M_{age} = 33$) was conducted during October 2019, prior to the COVID-19 outbreak, which started in November 2019. The other study (Prolific, $N = 395$, $M_{age} = 31$), was conducted during September 2021 while the global pandemic was ongoing and induced participants to think about the pandemic and its implications. We found that participants who reflected on the pandemic and its implications reported significantly higher GNP ($M = 6.03$, $SD = 1.04$) than participants whose mindsets were not focused on the pandemic ($M = 3.79$, $SD = 1.07$); these scores are significantly different [$t(833) = 30.62$, $p < 0.001$].

In the current research, we test whether choosing several charitable causes is related to the prospective donors' GNP. We suggest that as donors consider the different charitable causes they can support and decide which ones they care about most, they also consider the needs of each cause. Regardless of which causes they choose, as the number of causes chosen as important and in need of support rises, donors reflect on different needs worldwide and experience an escalation in their GNP. We test whether the higher the GNP results in a greater willingness to reach out and help as expressed by the willingness to support a charitable campaign and donate greater amounts.

Overview of the Present Research

We conducted five studies. The first two studies test how a fixed reference number of small vs. large important charitable causes influences donation giving and find a negative effect, such that a small number of charitable causes increases donation likelihood more than a large number of causes. The second two studies test how the actual rise in the number of causes selected (without manipulating a fixed reference number) relates to GNP and donation giving and find a positive relationship, such that the more charitable causes donors choose, the higher their GNP and the more likely they are to donate. In the last study, participants are given a reference number of charitable causes yet can still make a varied choice of causes, and we test both effects together.

We report in our studies how we determined our sample size, all manipulations, and all measures. No data were excluded from analyses.

STUDY 1

The goal of Study 1 was to test how asking participants to select either a small or large number of charitable causes important to them influences their GNP and their subsequent donation giving. In this study, participants were asked to choose either a fixed small number (4) or a fixed larger number (8) of charitable causes from a list of causes and afterward indicate their willingness to donate to a campaign related to one of their chosen causes. According to the proportion dominance effect, we hypothesize that participants referenced with a small number of causes (4) will be more likely to donate than participants referenced with a large number of causes (8). However, we did not have a prediction on whether obligating participants to choose a small or large number of causes will influence GNP.

Considering the different charitable causes is likely to elicit general thoughts about help giving. Such thoughts may activate heightened perceptions about the importance of helping and the positive implications of help giving, which in turn may influence more charitable behavior. To account for such an effect, Study 1 measured participants' attitudes toward help giving. Some participants reported their attitudes toward helping before the reference number manipulation and the decision to donate, while others reported their attitudes after the decision to donate. This was done to test whether this measure reflects a stable individual difference in attitudes toward helping. We incorporate this measure into the model as a covariate to test the effect of the reference number of charitable causes on donation giving, above and beyond dispositional attitudes toward helping.

Materials and Methods

Two hundred and seven students (M_{age} = 24.67, 60.3% female) recruited from a university online pool participated in this study in exchange for course credit and entered a raffle with 20 prizes of 50ILS each (equivalent to $15).

In this and all subsequent studies, we strived to achieve sample sizes over n = 90, which are sufficient to detect medium-sized effects of r = 0.30 with a power of 0.90 and 0.05 Type I error probability.

Participants were introduced to a description of a charity app named "CausePick." They read that the app sends its users monthly personalized recommendations of charitable campaigns. Each month users choose a campaign to which "CausePick" automatically transfers their donation. Participants were asked to assume they are entering the app for the first time and needed to indicate their charitable preferences, according to which the app will generate personalized recommendations for charitable campaigns. All participants received the same list of 17 causes (hunger, education, minorities, environment, etc.) and were asked to choose the causes they most care about. Participants were randomly assigned to two experimental conditions. In the small reference number condition, participants were instructed to choose 4 causes (small condition), while in the large reference number condition participants were instructed to choose 8 causes (large condition). On the following page, participants indicated their intention to support a charitable campaign, assuming it fits their charitable preferences. Participants responded on a 7-point scale (1 = no chance I will donate, 7 = I will definitely donate). Next, participants indicated the amount they would be willing to donate to the campaign if they won the prize raffle, with answers recorded on a 50-point scale (0 = I will not donate any amount, 50 = I will donate all). On a subsequent page, participants indicated their GNP. This measure is similar to the one reported in the introduction. Specifically, they read: "*Some people feel that there are many important social issues in the world that need the support of donations, while others feel that everything converges into a small number of important social issues that need charitable support. What, in your opinion, is the scope of all the social issues that need the support of donations in the world?*" Answers were given on an 11-point Likert scale (1 = very few, 11 = plentiful).

In this study participants also completed the Helping Attitude Scale (HAS; 20 item, α = 0.77; Nickell, 1998) measuring beliefs, feelings, and behaviors related to helping (same measure as mentioned in the "Introduction" section). At random, half of the participants completed the scale at the beginning of the study before reading about the charity app and making their donation decision; the other half completed the scale after reading about the app, choosing their causes, and making the donation decision. Finally, participants indicated demographics such as age, gender, and mother tongue. (For more details see **Supplementary Appendix A**). The data from studies 1, 2, 4, and 5 and the studies

TABLE 1 | Mean, std. deviation, and intercorrelations of Study 1 variables (N = 207).

Variable	Mean	SD	1	2	3	4	5
Reference number of charitable causes (0 = Four; 1 = Eight)	1.48	0.50	–				
WTD (Single item; 1–7)	4.52	1.88	−0.16*	–			
Donation amount (0–$50)	24.18	16.59	−0.15*	0.63**	–		
GNP (Single item; 1–11)	7.33	2.51	0.03	0.30**	0.32**	–	
HAS	3.33	0.25	02	0.24**	0.19**	0.22**	–

*$p < 0.05$; **$p < 0.001$.

reported in the introduction section can be accessed at: https://osf.io/8gr6n/?view_only=b0703de9c9de4710ac3b48a85bbd54ec.

Results

As a preliminary step, we tested the effect of HAS presentation order and found it to be non-significant [$t(205) = 0.08$, $p = 0.94$]. We tested the interaction effect of HAS presentation order and the reference number of charitable causes condition on the two independent variables. We found no significant effects (see results in **Supplementary Material**). This suggests that thoughts about charitable causes or the number of charitable causes people think about do not influence the helping attitude or interact with helping attitudes, presumably because helping attitudes reflect a stable personality attribute.

Descriptive statistics and intercorrelations of the study variables appear in **Table 1**. As seen in the table, HAS was significantly correlated with both GNP and the dependent measures and, therefore, was controlled for in the subsequent analysis.

The manipulated reference number of charitable causes did not have an effect on GNP, but did affect the dependent variables: Participants presented with a smaller reference number of causes (4) were more willing to donate (M = 4.81, SD = 1.73) and indicated donations of greater amounts (M = 26.56, SD = 16.31) than participants with a larger reference number of causes (8; willingness to donate: M = 4.20, SD = 2.0; donation amount: M = 21.52, SD = 16.57). These differences are significant [willingness to donate: $t(205) = 2.34$; $p = 0.02$; donation amount: $t(205) = 2.19$; $p = 0.03$]. GNP was significantly correlated with both of the dependent variables: $r = 0.30$ with willingness to donate and $r = 0.32$ with donation amount (both r's $p < 0.001$).

The hypothesized relations between research variables were tested as a path model using Mplus Version 8.6 (in this and all subsequent studies; Muthén and Muthén, 2017). This saturated model fitted the data perfectly, with $\chi^2(0) = 0.00$.

The results (**Figure 1**) show that the reference number of charitable causes had negative effects on both dependent variables: willingness to donate ($p = 0.007$) and donation sum ($p = 0.013$); however, these effects were not mediated by GNP (both p's = 0.775). Controlling for HAS, GNP was significantly and positively associated with both willingness to donate and the donation amount (both p's < 0.001).

Discussion

The results of Study 1 show that when donors' reference number of charitable causes is small, they are more likely to donate and to donate greater amounts than when their reference number of charitable causes is large. These findings are in line with proportion dominance reasoning, according to which donors who help one cause out of four feel that their help is more meaningful and as a result have a stronger motivation to donate than donors who help one cause out of eight.

The small versus large reference numbers of charitable causes did not change participants' perception of neediness in the world or their attitudes toward helping. One of the reasons could be that participants were obligated to choose a fixed number of causes (either 4 or 8) rather than having the liberty to choose their own number of causes. In the next study, we aim to test the robustness of these findings by replicating the results with a different sample and slightly different reference numbers.

STUDY 2

The main goal of Study 2 was to replicate the results of Study 1 with a non-student sample and with different reference numbers of charitable causes. To that end, the design of this study was similar to that of Study 1, with participants instructed to select a fixed small or large number of charitable causes. We tested how these reference numbers influence subsequent donation giving. According to the proportion dominance effect, we hypothesize that participants referenced with a small number of causes will be more likely to donate than participants referenced with a large number of causes. Following the results of Study 1, we do not expect that the number of charitable causes that participants are obligated to select will influence their GNP or HAS.

An additional goal of this study was to provide further validity for the GNP measure. Therefore, in this study, GNP was measured with four items (rather than 1), and we test its relation to two other individual differences constructs. Specifically, we test whether a general high or low optimistic nature (Scheier et al., 1994) changes how one experiences GNP and whether concerns with the COVID-19 pandemic and its implications (Ahorsu et al., 2020) are positively related to GNP. These results will replicate initial findings that were reported in the Introduction (see page 9), but with a 4-item measure of GNP.

Materials and Methods

Five hundred and one adults (M_{age} = 38.8, 49.5% female) recruited through Prolific participated in this study in exchange for $0.8 payment and entered a raffle with a $20 prize.

The procedure was the same as in Study 1 with a few changes: First, the reference numbers were 5 (small condition) and 10 (large condition). Second, we added two individual-

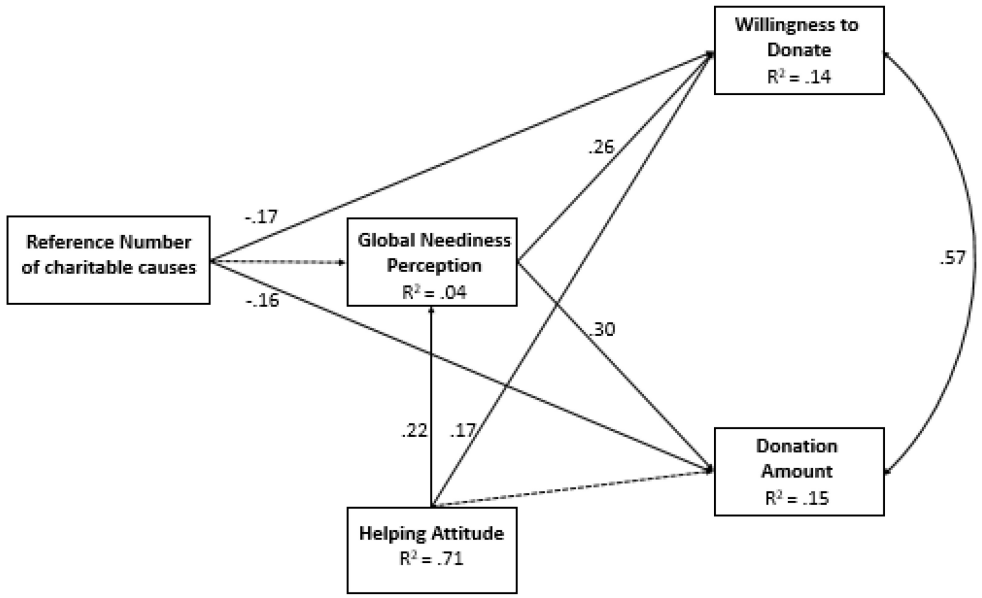

FIGURE 1 | Path model from reference number of charitable causes to willingness to donate and donation amount, mediated by GNP and controlled for HAS (Study 1). The values along the paths are standardized regression coefficients (betas), and correlation is shown along the double-arrow curve. The broken lines indicate statistically non-significant paths ($p \geq 0.05$).

differences measures at the end of the study: dispositional optimism assessed by the Life Orientation Test (LOT; 10 items, α = 0.73; Scheier et al., 1994) and the Fear of COVID-19 scale (FCV-19; 7 items, α = 0.90; Ahorsu et al., 2020). Based on initial results (see "Introduction" section) we expected FCV-19 to positively correlate with GNP. However, we did not expect GNP to correlate with LOT. Third, we measured GNP with more items. In addition to the original item in Study 1 (measured on a 5-point scale), we added three items: *Please state how you perceive the global neediness in the world today* (1 = Almost no global neediness, 7 = Excessive global neediness); *In your opinion, what is the scope of social and environmental issues worldwide which require charitable support* (1 = Few issues, 7 = Many issues); *What is your opinion about the social and environmental issues worldwide which require charitable support* (1 = Insignificant issue/s, 7 = Significant issue/s). All four items were averaged into a single GNP score (α = 0.85). As in Study 1, we counterbalanced HAS, such that half of the participants completed it at the beginning of the study and half at the end (α = 0.88). Finally, participants indicated demographics such as age, gender, and mother tongue (For more details see **Supplementary Appendix B**).

Results

As in Study 1, we first tested for an order effect for HAS. Unlike in Study 1, we found that participants who completed HAS at the end of the study reported higher scores (M = 3.98, SD = 0.50) compared to participants who completed the scale at the beginning of the study [M = 3.83, SD = 0.53, $t(499) = -3.34$; $p < 0.001$], suggesting that in the present study, the mere thought of charitable giving increased attitudes toward helping. To test if the reference number of charitable causes influenced HAS, we conducted a t-test only among participants who completed HAS at the end of the study (N = 254). We did not find significant differences between participants in the small condition (M = 3.97, SD = 0.46) and participants in the large condition [M = 3.99, SD = 0.53, $t(252) = -0.25$, p = 0.80]. Thus, attitudes toward helping were not influenced by the reference number manipulation. Finally, we tested the interaction effect of HAS presentation order and the reference number of charitable causes condition on the two independent variables. We found no significant effects (see results in **Supplementary Material**).

For descriptive statistics and intercorrelations between the study variables, see **Table 2**. As **Table 2** shows, GNP is positively correlated with HAS (r = 0.43, $p < 0.001$); GNP is also positively correlated with FCV-19, although to a lesser extent than with HAS (r = 0.11, p = 0.02), and does not correlate with LOT (r = 0.07, p = 0.11). These results suggest that individuals who feel there are many issues in the world that need charitable support also hold positive attitudes toward helping and are concerned with the negative implications of COVID-19, However, they are not highly pessimistic or optimistic in nature.

The reference number of charitable causes was not a significant predictor of GNP (p = 0.83), but it was significantly related to the dependent measures: Participants with a smaller reference number of charitable causes (5) were more willing to donate (M = 4.51, SD = 1.59) and indicated donations of greater amounts (M = 9.35, SD = 6.14) than participants with a larger reference number of charitable causes (10; willingness to donate: M = 4.20, SD = 1.58; donation amount: M = 8.0, SD = 6.21). These differences are significant [willingness to donate: $t(499) = 2.15$; p = 0.032; donation amount: $t(499) = 2.45$; p = 0.015].

TABLE 2 | Mean, std. deviation, and intercorrelations of study 2 variables (N = 501).

Variable	Mean	SD	1	2	3	4	5	6	7
Reference number of charitable causes (0 = Five; 1 = 10)	0.49	0.50	–						
WTD (Single item;1–7)	4.36	1.59	−0.10*	–					
Donation amount (0–$20)	8.68	6.21	−0.11*	0.44**	–				
GNP (4 items; 1–7)	5.19	0.99	0.01	0.25**	0.24**	–			
HAS	3.91	0.52	0.02	0.42**	0.39**	0.43**	–		
Optimism	2.59	0.62	−0.02	0.14**	0.13**	0.07	0.27**	–	
Fear of COVID-19	1.08	0.83	−0.01	0.14**	0.07	0.11*	0.12**	−0.14**	–

*$p < 0.05$; **$p < 0.001$.

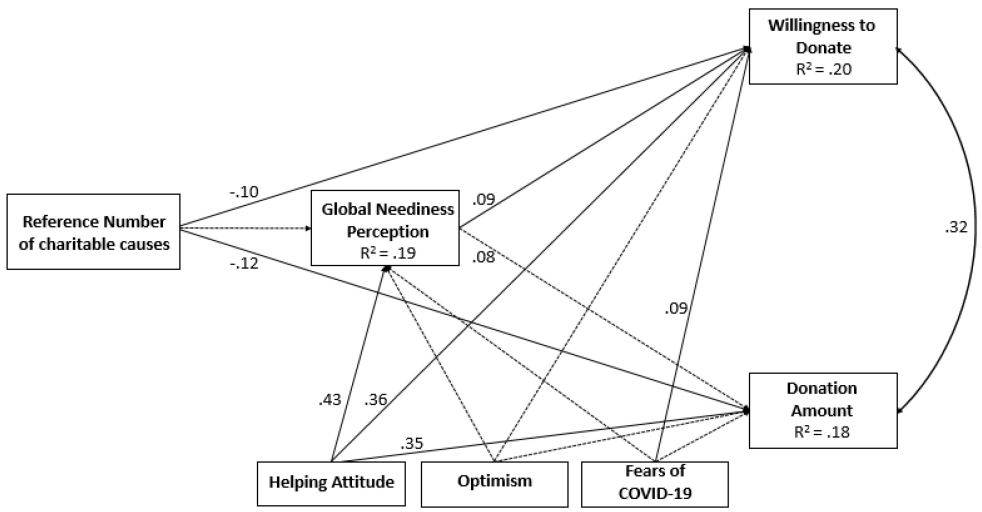

FIGURE 2 | Path model from reference number of charitable causes to willingness to donate and donation amount, mediated by GNP and controlled for HAS, optimism and fear of COVID-19 (Study 2). The values along the paths are standardized regression coefficients (betas), and correlation is shown along the double-arrow curve. The broken lines indicate statistically non-significant paths ($p \geq 0.05$).

The path model of the theoretical relations between research variables fit the data well, with $\chi^2(3) = 0.56$, $p = 0.90$; its results appear in **Figure 2**.

The reference number manipulation has a direct negative effect on willingness to donate ($p = 0.011$) and the donation amount ($p = 0.004$); however, these effects are not mediated by GNP (both p's = 0.965). Controlling for HAS, LOT, and FCV-19, GNP is related to willingness to donate ($p = 0.046$) and is related in the expected direction to donation amount yet does not reach significance ($p = 0.060$).

We conducted another path model without the covariates of optimism and fear of COVID-19 (similar to the model in Study 1); the results are almost identical (for the full model description see **Supplementary Material**).

Discussion

The results of Study 2 echo the results of Study 1 in showing that the reference of a small number of charitable causes induces a higher likelihood to donate as well as higher donation amounts compared with the reference of a larger number of charitable causes. According to the proportion dominance rationalization, this effect can be explained by donors' sense of contribution impact. Donating to one cause out of five could be experienced as more impactful than donating to one cause out of ten. Therefore, when referenced with a smaller rather than larger number of charitable causes, donors perceive their help as more meaningful and, as a result, the motivation to donate increases. Furthermore, as in Study 1, results show that the smaller vs. larger reference number did not influence other antecedents of donation giving such as GNP, attitudes toward helping, or concerns regarding the COVID-19 pandemic.

Taken together, the results of studies 1 and 2 suggest that asking donors to choose a fixed small number rather than a larger number of charitable causes they care most about increases their overall prosocial behavior, as expressed by greater willingness to donate and higher donation amounts. However, in both studies, donors were compelled to choose an exact number of charitable causes (either small or large). In the next studies, we ask donors to choose the charitable causes they most care about, but without forcing them to choose an exact number. This "freedom of choice" allows other underlying motivations to kick in. We aim to test whether the number of causes donors voluntarily choose influences their charitable behavior.

Although we allow variation in the number of causes donors choose, we provide a maximum number in order to minimize task depletion or choice overload effects.

STUDY 3

The goal of Study 3 was to test whether and how the number of causes chosen influences charitable behavior in a natural setting. We used data from an online fundraising platform that offers its users monthly personalized recommendations for charitable campaigns. The web-based platform designated for raising donations was established in November 2016 and was active until the end of 2018.[1]

Users registering on the platform for the first time chose the charitable causes they care about most and were presented with three personalized charitable campaigns matching their charitable preferences. Users picked one campaign to which they wished to donate and then were transferred to a payment page where they indicated the amount they wished to donate.

In this study, we focused on users' charitable behavior the first time they entered the platform and in reference to the first donation decision they made. We tested whether the number of charitable causes users chose predicted their willingness to support a charitable campaign and affected their donation amount.

Materials and Methods

When users entered the platform for the first time (i.e., the home page), they read a description about the platform and how it works. On the next page, users viewed 24 charitable causes (presented by title and icon) and chose up to 7 causes they care about the most. The number of causes they chose served as our independent measure. After making their choice, users were transferred to a new page on which they were offered three personalized charitable campaigns. Users could decide to donate to one of the three campaigns or exit the platform. Whether or not users clicked on one of the campaigns, thereby indicating an initial willingness to donate, served as our first dependent measure. Users who clicked on the campaign were transferred to the payment page on which they indicated the amount of donation they wished to make for their chosen charity. The amount users donated served as our second dependent measure. On the final page, users provided their personal payment details, and their donation was transferred to their preferred charitable campaign (For more details see **Supplementary Appendix C**).

All our analyses are based on data provided by 480 users who entered the platform and indicated the charitable causes important to them. The data for each user included the number of causes chosen (up to 7), willingness to donate as indicated by whether the user clicked on a campaign they wish to support, and the donation amount users indicated on the payment page (ranging from 0 to $108).

Results

Descriptive statistics and intercorrelations between the study variables are shown in **Table 3**.

[1]https://web.archive.org/web/20180309140128/https:/www.causeisrael.org/

TABLE 3 | Mean, std. deviation, and intercorrelations of study 3 variables (N = 480).

Variable	Mean	SD	1	2	3
Number of charitable causes chosen (up to 7)	3.80	2.18	–		
WTD (1 = Choose a campaign; 0 = Did not choose a campaign)	0.36	0.48	0.14**	–	
Donation amount (0–$108)	2.21	12.61	0.08	0.23**	–

**$p < 0.001$.

Willingness to Donate

Users who clicked on a charitable campaign, thus indicating their willingness to donate, were coded as 1, while users who did not select a campaign were coded as 0. We conducted a logistic regression to test if the number of causes chosen served as the predictor of whether donors clicked on one of the campaigns or not. Results show that the number of causes chosen serves as a positive and significant predictor of willingness to donate—that is, initial intention to support a specific charitable campaign (B = 0.135, $S.E.$ = 0.044; Wald = 9.38, p = 0.002).

Donation Amounts

We conducted a regression on the entire sample to test whether the number of causes chosen predicted the amount of money donors decided to donate. Results are in the expected positive direction; however, they did not reach significance (B = 0.46, $S.E.$ = 0.26, β = 0.08, t = 1.75, p = 0.08).

Discussion

Results of Study 3 show that the more charitable causes users voluntarily chose as important to them (without manipulating a reference number of causes), the greater the likelihood to support a campaign. However, in this study, an increase in the number of causes chosen did not yield a significant increase in the amount of money users donated.

This study, although based on actual behavior in a natural field setting, is not without limitations. First, users entering the app were not randomly selected; most users *a priori* indicated some interest in giving to charity. Hence, there may be a selection bias. Second, the results, although in the same direction for both donation-behavior measures, were found to be significant only for willingness to donate but not for the donation amount. One possible explanation could be attributed to restricted variance. The app recommended certain donation amounts, which could have reduced the variance and suppressed a significant relationship from emerging. Third, the data provided evidence for the main effect without providing any insight as to why these relations emerge and whether they are related to an escalation in GNP. Finally, it is possible that the number of charitable causes chosen is merely an expression of individuals' attitudes toward helping. The more users demonstrate positive helping attitudes, the more charitable causes they choose and the more willing they are to donate. Therefore, it is important to control for individual differences in helping attitudes when exploring the link between number of charitable causes chosen

and donation behavior, as was done in studies 1 and 2. The next studies aim to expand our understanding of the relationship between the number of charitable causes chosen and charitable behavior.

STUDY 4

The goal of Study 4 was to test the relationship between the number of causes voluntarily chosen (without manipulating a reference number of causes), and donation giving, along with GNP, while controlling for donors' dispositional attitudes toward help giving (i.e., HAS).

Materials and Methods

Participants were 95 students (M_{age} = 25.35, 61.7% female) recruited from a university online pool who voluntarily enrolled in this study in exchange for course credit and entered a raffle with three prizes of 50ILS each (equivalent to $15).

Participants were introduced to a description of the "CausePick" app, similar to studies 1 through 3. Participants were asked to assume they are entering the app for the first time and needed to indicate their charitable preferences from a list of 17 causes. Unlike studies 1 and 2 but similar to Study 3, participants were given the option to choose up to 7 causes they most care about. On the following page, participants indicated their intention to support a charitable campaign, assuming it fits their charitable preferences. Participants responded on a 7-point scale (1 = no chance I will donate, 7 = I will definitely donate). Next, participants indicated the amount they would be willing to donate to the campaign if they won prize money on a 50-point scale (0 = I will not donate any amount, 50 = I will donate all). On the page that followed, participants indicated their GNP with a single item as in Study 1, and completed the HAS scale (α = 0.77). Finally, participants indicated demographics such as age, gender, and mother tongue (For more details see **Supplementary Appendix D**).

Results

Descriptive statistics and intercorrelations between the research variables are presented in **Table 4**. As seen in the table, the number of causes chosen by the participants was positively and significantly correlated with GNP (r = 0.34, p = 0.001) but not with willingness to donate (r = 0.18, p = 0.085) and donation amount (r = 0.05, p = 0.60). HAS was correlated with willingness to donate (r = 0.30, p = 0.003) but not with donation amount (r = 0.15, p = 0.13).

In a path model [$\chi^2(0) = 0.00$, see **Figure 3**], the indirect effect of the number of causes chosen, mediated by GNP, was in the expected direction yet did not reach significance for willingness to donate (p = 0.054) but did reach significance for the donation amount (p = 0.010).

Given that in this study causality cannot be inferred, we tested an alternative model in which the number of causes affected both willingness to donate and donation amount, with these two donation behavior variables in turn affecting GNP. In this path model, indirect effects on GNP of the number of causes chosen, mediated by the willingness to donate and donation amount, were not significant (willingness to donate: p = 0.857 and donation amount: p = 0.692). This provides additional evidence for the notion that GNP mediates the relationship between the number of causes chosen and donation giving.

Discussion

The results of Study 4 replicate and add to the findings of Study 3 by showing that the relationship between the number of causes selected and willingness to support a campaign is driven by donors' escalating sense of neediness in the world and not by their disposition toward helping. Although this disposition is related to help-giving to some extent, it is not influenced by the number of causes chosen and, hence, does not mediate the relationship.

In this study, the number of charitable causes did not relate directly to charitable behavior but only indirectly via GNP. This is somewhat inconsistent with the results found in our field data (Study 3), in which the number of charitable causes did relate to donors' choice of a specific campaign—that is, to the willingness to donate (but not to the donation amount). One explanation could be that Study 4 employed a hypothetical general question to measure charitable behavior, which attenuated the potential relationship. In Study 3, donors viewed actual campaigns, while in Study 4 they were asked to consider donating to a campaign assuming it fit their charitable preferences but without viewing any information about an actual campaign. Another potential explanation is that Study 4 is underpowered to fully detect both the direct and indirect effects of the tested model. In the next study, we test these relations again with a larger sample.

In the final study, we sought to explore together the two forces that impact donation behavior. To that end, we manipulate the reference number of charitable causes donors can choose as important to them. Thus, some participants considered a small reference number (up to 3), while others considered a large reference number (up to 7), similar to studies 1 and 2. However, we did not confine participants to choosing an exact number, which allowed variations in the number of causes chosen as in studies 3 and 4. In line with our findings from previous studies, we expect that manipulation of the reference number will be negatively linked to charitable behavior; however, the actual number of causes participants voluntarily choose will be positively linked to charitable behavior, and the latter will be mediated by GNP. These effects will occur while controlling for participants' attitudes toward helping.

STUDY 5

In this study, we manipulate the reference number of causes donors can choose as important to them, but without forcing donors to choose a maximum number (i.e., participants can choose either up to 3 or up to 7 causes). In doing so, we allow for two opposing forces to shape donors' behavior. The reference of a small or a large number of charitable causes is expected to drive behavior that follows the proportion dominance rationale. Thus, we hypothesize that participants with the smaller reference number (up to 3) will express more charitable behavior than

TABLE 4 | Mean, std. deviation, and intercorrelations of study 4 variables (N = 95).

Variable	Mean	SD	1	2	3	4	5
Number of charitable causes chosen (up to 7)	5.88	1.52	–				
WTD (Single item; 1–7)	4.17	1.69	0.18	–			
Donation amount (0–$50)	24.33	15.33	0.05	0.61**	–		
GNP (Single item; 1–11)	7.40	2.54	0.34**	0.30**	0.37**	–	
HAS	3.95	0.38	0.09	0.30**	0.16	0.16	–

**$p < 0.001$.

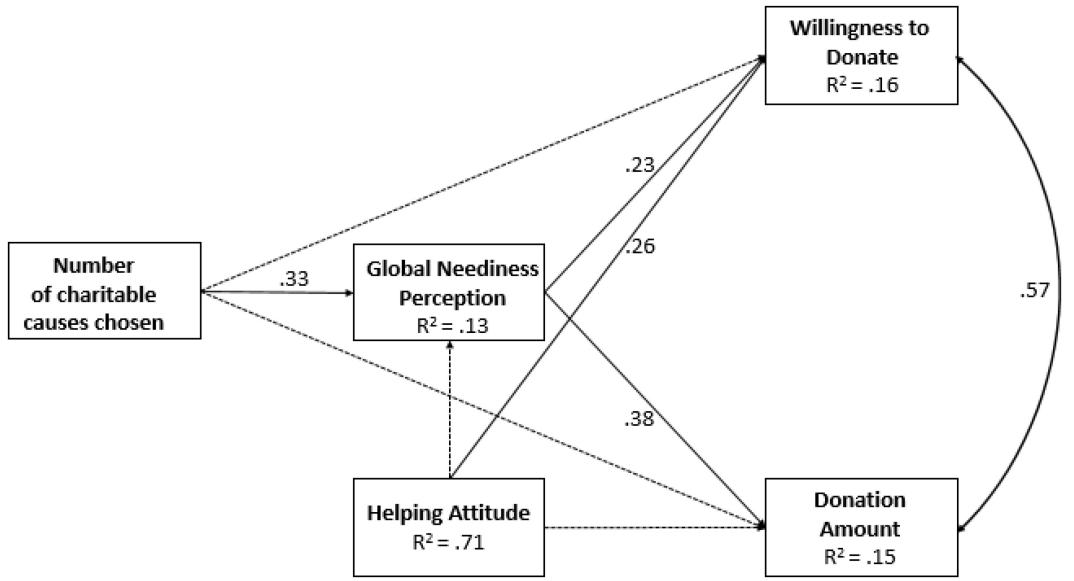

FIGURE 3 | Path model from number of charitable causes chosen to willingness to donate and donation amount, mediated by GNP and controlled for HAS (Study 4). The values along the paths are standardized regression coefficients (betas), and correlation is shown along the double-arrow curve. The broken lines indicate statistically non-significant paths ($p \geq 0.05$).

participants with the larger reference number (up to 7). By enabling donors to freely choose up to the maximum number of causes presented, we allow for another process to take place. As the number of causes donors choose increases, the perception of global neediness escalates, prompting more charitable behavior. Thus, we hypothesize that the greater the number of causes participants choose (out of the maximum causes they can choose from), the greater their charitable behavior (i.e., willingness to donate and donation amount). We further hypothesize that this relationship will be mediated by GNP. We expect the hypothesized relationships to emerge while controlling for HAS.

Materials and Methods

Five hundred participants (M_{age} = 36.67, 50.2% female) recruited from Prolific participated in this study in exchange for 0.5£ payment and the chance to win a $50 raffle prize.

As in our previous studies, participants read about the "CausePick" app. All participants were instructed to choose from a list of 17 causes the ones they most care about. Participants were randomly assigned to a different reference number of maximum causes from which they could choose. Participants were either instructed to choose up to 3 causes (small condition) or to choose up to 7 causes (large condition). This reference number served as our first independent measure. Unlike in studies 1 and 2, where participants were obligated to choose a fixed number, in this study they could "freely" choose causes (up to 3 or 7, depending on the condition). The actual number of charitable causes participants chose served as our second independent measure. On the following pages, participants indicated their willingness to donate, the amount they would be willing to donate if they won a $50 prize raffle, and their GNP (single item, as in studies 1 and 4). HAS was measured either at the beginning or the end of the survey as in studies 1 and 2 (α = 0.77). Finally, participants indicated demographics such as age, gender, and mother tongue (For more details, see **Supplementary Appendix E**).

Results

We first conducted a t-test to compare the number of charitable causes chosen between the two reference number conditions. As expected, participants in the large condition chose more causes (M = 5.75, SD = 1.69) than those in the small condition [M = 2.96, SD = 0.24; $t(498) = 25.55$, $p < 0.001$], suggesting that the reference number manipulation was successful. We also tested whether the

TABLE 5 | Mean, std. deviation, and intercorrelations of study 5 variables (N = 500).

Variable	Mean	SD	1	2	3	4	5	6
Reference number of charitable causes (0 = Three; 1 = Seven)	0.51	0.50	–					
Number of charitable causes chosen	4.37	1.85	0.75**	–				
WTD (Single item; 1–7)	4.46	1.67	−0.09*	0.04	–			
Donation amount (0–$50)	14.70	13.27	−0.06	0.04	0.36**	–		
GNP (Single item; 1–11)	3.83	1.09	−0.07	0.13**	0.26**	0.16**	–	
HAS	3.33	0.32	−0.10*	0.06	0.34**	0.29**	0.24**	–

*$p < 0.05$; **$p < 0.001$.

presentation order of HAS had an effect on the results, and, as in Study 1, it was non-significant, $p = 0.48$.

Descriptive statistics and intercorrelations between the research variables are presented in **Table 5**. As seen in the table, the reference number of charitable causes (the experimental manipulation) was negatively and significantly ($p = 0.045$) related to willingness to donate, and non-significantly related to the amount donated ($p = 0.20$) or to GNP ($p = 0.13$). The number of causes chosen was not related to the two dependent measures; willingness to donate ($p = 0.33$) and donation amount ($p = 33$) but was positively and significantly related to GNP ($r = 0.13$, $p < 0.001$).

The hypothesized path model was tested and found to fit the data well, with $\chi^2(2) = 2.71$, $p = 0.26$ (see results in **Figure 4**).

The experimental manipulation of reference number of charitable causes was positively related to the actual number of causes chosen and negatively related to the GNP. The number of causes chosen was positively related to GNP, which, in turn, was positively related to the dependent variables: the willingness to donate and donation amount.

As part of the model, indirect (mediated) effects of experimental manipulation on the dependent variables were tested. In predicting willingness to donate, we found that reference number manipulation had a negative indirect effect mediated by GNP ($\beta = -0.06$, $p = 0.001$), and a positive effect mediated by number of causes chosen and then by GNP ($\beta = 0.05$, $p = 0.001$). The same pattern of mediation paths was found for the prediction of donation amount: a negative indirect effect of reference number manipulation mediated by GNP ($\beta = -0.03$, $p = 0.049$), and a positive effect mediated by number of causes chosen and then by GNP ($\beta = 0.03$, $p = 0.046$).

Discussion

The results of Study 5 provide support for two opposing forces that shape charitable behavior. Results show that when the reference number of the maximum causes participants could choose was small, they expressed more charitable behavior than when the reference number was large. This result replicates the findings of studies 1 and 2 and is in line with proportion dominance reasoning, which suggests that contributing to one cause out of a small number of cases feels more meaningful than contribution to one cause out of a large number of causes. Results further show that the more charitable causes donors chose as important, the more global neediness they felt and the more charitable behavior they expressed, as indicated by greater willingness to donate and higher donation amounts. This result replicated the findings of studies 3 and 4.

GENERAL DISCUSSION

The prevalence of personalized charitable campaigns in the donation-raising arena has accustomed donors to pre-select their charitable preferences before making donation decisions.

Past research that has studied choice-driven effects in the donation setting has focused mainly on the choice of a donation recipient, either an individual person in need (e.g., Kogut and Ritov, 2005; Cryder et al., 2017; Ein-Gar et al., 2018; Bareket et al., 2022; Ein-Gar et al., 2022) or a charitable organization (e.g., Carroll et al., 2011; Soyer and Hogarth, 2011), thereby neglecting a pre-stage in which donors first choose the general charitable causes they care about. In this study, we focus on this unexplored stage of choosing charitable causes prior to deciding on a specific donation recipient. We demonstrate two opposing effects, showing how the process of choosing charitable causes donors care about influences their subsequent willingness to support a charitable campaign and their donation amount. We find that referencing donors to a larger rather than smaller number of causes reduces donation likelihood and donation amount (studies 1 and 2). We explain this effect based on the proportion dominance rationale, suggesting that helping one cause out of a small number of causes (Study 1: 4 causes; Study 2: 5 causes) feels more meaningful than helping one cause out of a large number of causes (Study 1: 8 causes; Study 2: 10 causes). Thus, the reference number of charitable causes has a negative effect on charitable behavior. We also find that as the actual number of charitable causes donors choose increases, so does their willingness to donate and to donate a larger amount (studies 3 and 4). This association is mediated by donors' GNP (studies 4 and 5). Thus, the overall number of causes chosen has a positive relationship with charitable behavior. Finally, we find that the two effects can simultaneously influence charitable behavior (Study 5).[2] In all studies testing path models (studies 1, 2, 4, and 5), HAS

[2]It should be noted that while one of the independent variables in our studies was experimentally manipulated (i.e., small vs. large reference number of charitable causes) and hence was causally related to the mediator (i.e., GNP) and dependent variables (i.e., willingness to donate and donation amount) in the tested models, the paths from the mediator to the dependent variables are only (partial) correlations, as suggested by Pieters (2017). Our measures may not fully comply with Pieters' conditions for "meaningful" mediation analysis; nevertheless, we believe that our findings are at least not contradictory to interpretation in terms of mediation processes.

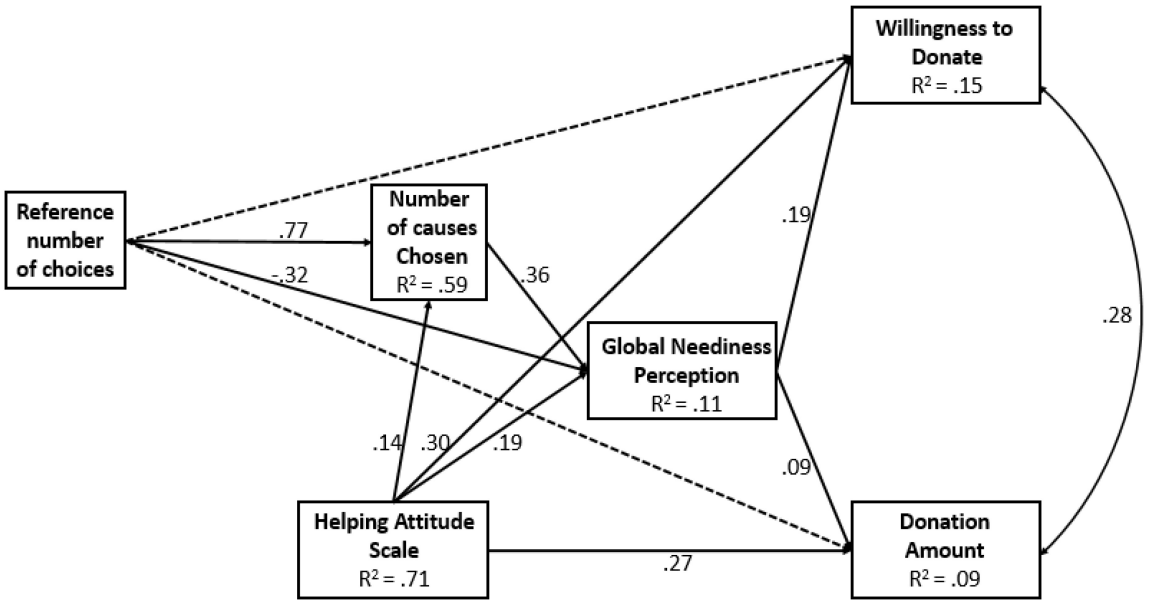

FIGURE 4 | Path model from manipulation of reference number of charitable and number of charitable causes chosen to the willingness to donate and the donation amount, mediated by GNP, and controlled for HAS (Study 5). The values along the paths are standardized regression coefficients (betas), and correlation is shown along the double-arrow curve. The broken lines indicate statistically non-significant paths ($p \geq 0.05$).

was a covariate; therefore, we test the direct and indirect paths between the model variables while accounting for individuals' different attitudes toward help giving.

In this article, we introduce a new underlying mechanism that drives charitable behavior—namely, global need perception. We find that a small or large reference number of charitable causes does not influence GNP and that GNP does not mediate the effect of the reference number of charitable causes on charitable giving. However, we do find that GNP mediates the relationship between the number of charitable causes chosen and charitable giving. Furthermore, we find that GNP consistently correlated significantly positively with both willingness to donate and donation amount, regardless of whether GNP was measured with a single item (studies 1, 3, and 5) or 4 items (Study 5). In three studies (studies 1, 2, and 5), GNP correlated significantly positively with helping attitudes, while only in one study (Study 4) the relationship was not significant. In Study 2, we find that GNP correlated positively with fear of COVID-19, but to a lesser extent than its correlation with HAS. This pattern of results (and their magnitude) replicates previous findings we report in the Introduction. Finally, GNP was not found to relate to optimism. These results shed light on the convergent and discriminant validity of GNP.

Limitations and Future Research

This article opens important new research avenues in the study of charitable behavior; however, it is not without limitations. From a methodological perspective, the studies were not preregistered, and some yielded relatively small effects. Future research should attempt to replicate the findings with more powered and pre-registered studies. Another limitation of this research was that, apart from Study 3, participants considered donating to a general hypothetical campaign. Future studies should test these predictions in situations when donors consider making an actual donation to a specific campaign. From a theoretical perspective, this research shows that different choice settings of charitable causes influence donations in opposing directions and may be driven by different underlying processes. However, we cannot infer causality between the number of charitable causes selected and donation behavior. Future research should provide causal evidence by varying the number of charitable causes selected and by manipulating the underlying driver of GNP. Furthermore, the choice-setting of the charitable causes was similar across studies. This means that in all studies participants saw a similar variety of causes, in a similar format. Future research should expand the scope of this investigation to other choice settings. For example, in this research, we did not explore the effect of choice-set size, or variety. Future research could test whether choosing a cause out of a choice set of 10 causes/charities or 50 causes/charities, all from similar or different domains changes charitable behavior. Furthermore, in our studies participants were asked to consider donating to a single campaign, which is a limitation, given that in reality they can donate to more than one campaign. Another interesting research direction would be to test whether the number of causes donors choose influences donors' willingness to donate to several campaigns (as opposed to a single campaign).

Our research offers a new psychological driver for prosocial behavior—namely, GNP. However, we did not find that the reference number of charitable causes influences GNP. Future research can advance the exploration of this mechanism and its relation to the selection of charitable causes, identifying when

and why GNP is heightened due to choosing charitable causes. Furthermore, given that this is a new individual difference, it is important that future research distinguish it from other individual differences and donation-related mechanisms. Our investigation was limited to attitudes toward helping, optimism, and fear of COVID-19. Future research could explore GNP's relation to other individual differences such as perception of donation efficacy, self-signaling, and moral self. It also could identify other antecedents that impact the magnitude of GNP, such as mortality salience and types of charitable causes. Future research could also explore additional prosocial behaviors that may be driven by this mechanism—including volunteerism, advocacy of charitable campaigns, and even pro-environmental behaviors. Moreover, past research has shown that drivers enhancing a self-focused mindset reduce help giving (Levontin et al., 2015; Roux et al., 2015). This research has shown that drivers enhancing an other-giving mindset through GNP increase help giving. Future research can explore whether heightened GNP reduces self-focused behavior such as indulgent consumption and self-gifting.

In this research, we explore choice that does not directly influence the donation recipient (i.e., choice of charitable causes). Future research could explore how such initial choices have a downstream effect on choices that directly influence donation recipients. For example, past research has shown that choosing between two similar donation recipients leads to choice aversion (Ein-Gar et al., 2021). Future research can explore whether initially choosing a charitable cause reduces a donor's tendency to opt-out of choosing a donation recipient. Intuitively, we assume that when individuals reflect on the charitable causes that are important to them, this reflection will in turn increase their willingness to help. Yet in this manuscript, we show that the effect of choosing charitable causes on donation giving is more complex than assumed. While the reference number of overall charitable causes may have a negative impact on donation giving, the actual number of charitable causes chosen has a positive impact on donation giving. These findings suggest that choosing charitable causes activates different motivational processes such as perceptions of proportion dominance and perceptions about the magnitude of neediness in the world.

Practical Implications

This research is the first to show the important role that the pre-stage of selecting charitable causes has on donors' subsequent behavior. One implication is that donation-raising agencies should consider the reference number they activate in donors' minds when they ask them to choose a small or large number of causes. Our results suggest that smaller numbers would be more effective than larger numbers. Another implication is that asking donors to choose an exact number (e.g., choose 7 causes) or giving donors the option to choose causes with some variance (e.g, choose up to 7 causes) activates different mental processes and changes their donation decisions. Our results suggest that when donors are given choice variance, the more causes they choose, the greater their perception of global neediness in the world, and the more likely they are to donate. By designing the pre-registration stage in different ways, donation-raising agencies can decide whether they influence their prospect donors' decisions through proportion dominance rationalizations or through neediness perception rationalizations.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/8gr6n/?view_only=b0703de9c9de4710ac3b48a85bbd54ec.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Tel-Aviv University Ethics Committee. The participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

DE-G designed studies 1, 2, 4, and 5 and collected the data. The studies were analyzed with the assistance of a statistical consultant (Dr. Ilan Roziner). The data of Study 3 was collected by AG and analyzed by DE-G. DE-G wrote the manuscript. AG read and approved the manuscript. Both authors contributed to the article and approved the submitted version.

FUNDING

This research was supported by Israel Science Foundation Grant #471/19 and by the Henry Crown Institute grant given to DE-G.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2022.800867/full#supplementary-material

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers | Frontiers in Psychology

ORIGINAL RESEARCH
published: 13 July 2022
doi: 10.3389/fpsyg.2022.801150

Motivated Down-Regulation of Emotion and Compassion Collapse Revisited

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Compassion collapse is a phenomenon where feelings and helping behavior decrease as the number of needy increases. But what are the underlying mechanisms for compassion collapse? Previous research has attempted to pit two explanations: Limitations of the feeling system vs. motivated down-regulation of emotion, against each other. In this article, we critically reexamine a previous study comparing these two accounts published in 2011 and present new data that contest motivated down-regulation of emotion as the primary explanation for compassion collapse.

Keywords: compassion, emotion, emotion regulation, down-regulation, charitable giving, prosocial behavior

OPEN ACCESS

Edited by:
Michael B. Steinborn, Julius Maximilian University of Würzburg, Germany

Reviewed by:
James Kirby, The University of Queensland, Australia
Chenggang Wu, Shanghai International Studies University, China

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Specialty section:
This article was submitted to Cognition, a section of the journal Frontiers in Psychology

Received: *24 October 2021*
Accepted: *20 June 2022*
Published: *13 July 2022*

Citation:
Hagman W, Tinghög G, Dickert S, Slovic P and Västfjäll D (2022) Motivated Down-Regulation of Emotion and Compassion Collapse Revisited. Front. Psychol. 13:801150. doi: 10.3389/fpsyg.2022.801150

INTRODUCTION

A central question in research on charitable giving and prosocial giving is how we value human lives (Slovic, 2007). Donors are often scoped insensitive and do not increase donations as the need increases (Dickert et al., 2015; Slovic et al., 2017). Donors may even be inversely scoped sensitive—a form of "compassion collapse" or "compassion fade" has been documented, where feelings and helping behavior decrease as the number of needy increases (Kogut and Ritov, 2005a,b; Slovic, 2007; Västfjäll et al., 2014). Although this finding has been replicated in different contexts and countries (Kogut and Ritov, 2005a,b; Markowitz et al., 2013; Västfjäll et al., 2014; Kogut et al., 2015), the psychological mechanisms underlying this effect is not well understood. In an attempt to remedy this, Cameron and Payne (2011; henceforth C&P) tested an affect trigger or capacity explanation vs. a motivated emotion regulation account for compassion collapse. According to C&P, the affect trigger account suggests that emotions are more strongly elicited for individuals than groups and that as the scale increases, individuals start to lose feelings (Slovic, 2007; Genevsky et al., 2013; Västfjäll et al., 2014, 2015; Lindauer et al., 2020; Moche et al., 2020). In a sense, the affect trigger account suggests that compassion collapse occurs because of capacity limitations of the affective system and is an inherent property of compassion (and other emotions; see the extensive work on psychophysical numbing; Fetherstonhaugh et al., 1997, as well as the general decreased marginal utility in descriptive models of decision making such as prospect theory; Slovic, 2007).

In C&P's view, the motivated emotion regulation account, on the other hand, suggests that groups indeed elicit strong emotion, but that people may engage in motivated behaviors and goal-focused emotion regulation to prevent those feelings to occur. Specifically, C&P suggested that costly helping will be avoided. They reasoned that both financial and emotional costs will be downregulated and further that these costs would be perceived as greater as the scale of helping

increases. Thus, they hypothesized that compassion collapse would emerge strongly when there is a clear motivation to avoid feeling compassion for multiple victims. Previous research on compassion collapse has primarily explored individuals' willingness to donate money toward victims (Kogut and Ritov, 2005a), and why C&P reasoned that the expectation of being asked to help may serve as a financial motivation to avoid emotions toward many victims.

As a critical test of this hypothesis, C&P conducted an experiment (Experiment 1 in C&P, 2011 published in the prestigious outlet *Journal of Personality and Social Psychology*) where the motivation to regulate emotion was experimentally manipulated. In this study, 120 participants first read about one or eight children in Darfur (a common between-subjects manipulation in compassion collapse studies; Kogut and Ritov, 2005a). Second, C&P introduced the critical between-subject manipulation where participants either (a) rate their feelings toward the children or (b) first rate their feelings toward the children *and* then respond to how much money they would be willing to donate. We will refer to "a" as the no-help request condition and "b" as the help request condition. Critical to the current article, the no-help request condition (a) did not explicitly state that subjects would not be asked to make a donation decision. Since C&P argued that for the no-help request condition, they did not explicitly state that the participants would not be asked to donate because *such instructions* could have inadvertently focused participants on the idea of donating. Following this manipulation, participants in both conditions rated their feelings on a 9-item compassion scale that included statements such as "how sympathetic do you feel toward the child (children)?" and "how compassionate do you feel toward the child (children)?"

C&P predicted and found that there was no main effect on help request (donation-no donation) or the number of children (1 vs. 8) on rated compassion, but a significant interaction where the compassion collapse (greater compassion for one over eight) only occurred in the donation condition. In the no-donation condition, the pattern reversed so that compassion was instead greater for the eight children.

C&P interprets these findings as suggesting that the driving mechanism behind compassion collapse is active down-regulation of emotion that only occurs *when people themselves expected to help*—a finding that supports the motivated emotion regulation account over the affect trigger explanation. While this is only studying one of three studies in the C&P 2011 paper, this particular finding has been cited several times in support of the notion of "empathy/compassion as a choice" (Zaki, 2014; Cameron, 2017; Cameron et al., 2019, 2022; Scheffer et al., 2021) and is an important contribution for a critical experiment demonstrating a boundary condition for compassion collapse.

In this article, we revisit this experiment and present new data that suggest that the experimental manipulation of motivated emotion regulation used by C&P is problematic and consequently is limited as an explanation for compassion collapse. We identified several critical methodological problems in the original study and we present new data from a large-powered study (using the original materials) that experimentally manipulates or measures the methodological concerns identified with the original study.

PROBLEMS WITH C&P 2011 EXPERIMENT 1

1. First, even though the compassion collapse effect has been replicated across more than 20 studies (as cited in the C&P paper), the published effects range from medium effect sizes in the expected direction (Kogut and Ritov, 2005a,b; Moche et al., 2022) over null effects (Dickert et al., 2016; Hart et al., 2018) to small effects in the opposite direction (more giving to the many: Wiss et al., 2015). Thus, a minor, but still noteworthy aspect of Experiment 1 in C&P (2011) is the relatively small sample size (roughly 30 participants per condition: a total of 120). This n is just on the border of the minimal sample adequate to detect medium effect size in an interaction (f = 0.25; Cohen, 1988; n = 128 required at 0.80 power), but would not be able to detect a small effect (f = 0.10), where an n = 787 would be required (computed using G*Power 3.1).

2. Second, and much more critical, is the item in the main dependent variable—the compassion scale. While most of the items are standard sympathy and distress items, one item (henceforth called the "Give money" item) of the scale is formulated: "*To what extent do you feel that it is appropriate to **give money** to aid the child (children)?*" (Emphasis added by current authors). Given that this item was included in the main dependent variable that was used in both the help request and no-help request conditions, it could be argued that the inclusion of this item may effectively wipe out any expected difference between conditions. Participants in the no-help request condition would arguably come to expect that they would be asked to donate. This is particularly interesting, as C&P told participants in no-donation condition explicitly that they would not be asked to donate *"because such an instruction might have seemed like a violation of conversational norms and might have inadvertently focused participants on the idea of a donation even as we assured them of its absence."* (p. 4). We argue that it is an equally big risk to focus participants on donation by including that very question as an item in the scale, and further, to use this scale as the main dependent variable. This methodological problem casts doubt on the effectiveness of the donation/no-donation manipulation, even though the original study found a condition difference in the expected direction.

Arguably, if donation requests have an effect, then the order of the donation item in the compassion scale could make a difference so that if appearing first, it would have a larger influence than if appearing last. We contacted C&P (personal communication) to clarify if there were any order effects and learned that the order was fixed so that the Give money item was always randomized to occur in order 5, 6, 7, or 8 position of the 9-item scale. In the current experiment, we systematically manipulate the serial position of the give money item in the compassion scale so that either the Give money item question appears first or last. If the Give money item indeed cues thinking

on donations, we should see a larger effect when it appears first than when it appears last.

3. Third is related to the second concern and was also raised by C&P: Even when not asked for a donation, participants may have expected a donation request because the materials depicting victims are often based on actual charity advertisements with the purpose to solicit donations. Given that C&P modeled their stimuli on those in studies conducted by Kogut and Ritov (2005a,b) and that the information given about the victims (west Darfur civil war victims suffering from deadly diseases such as malaria, dysentery, and cholera) is very typical of charity requests (Erlandsson et al., 2016, 2018), it is likely that even participants in the no-donation condition may have been cued to think about donations. Simply asking participants to state their expectations about if they would be asked to donate or if they thought about donating during the stimuli presentation would have given information about this, but C&P does not present any such data. We believe that participants in both conditions may have thought about donating at some point and an important piece of information that is missing is, if so, what was the prevalence, and was it different between conditions? Perhaps fewer participants in the no-donation condition thought about donating and thus the manipulation was (relatively) successful. On the other hand, if there was a roughly equal proportion of participants in both conditions that report thinking about donating, the effectiveness of the manipulation must be seriously questioned. We cannot give estimates of the prevalence of thoughts about donating with the existing data from the C&P article, and together with the problems identified with the compassion scale, this is a central point to evaluate the validity of the manipulation and findings. In the present article, we therefore also measured people's self-reported expectations about donating as well as thoughts about donating in both the help request and no-help request conditions to assess if the manipulation worked as intended.

4. Fourth, given the potential problem that the Give money item in the compassion scale resembles the help request manipulation, it is problematic that it is used as the only dependent variable to measure compassion collapse as C&P did. In the present article, we include measures of donation (both a yes-no decision as well as amount; a standard way of probing helping intentions: Dickert et al., 2011) in both conditions (but presented last in the session and without participant's prior knowledge). Furthermore, given that a central feature of C&P's account for explaining compassion collapse is emotion regulation, we measure self-reported mood at several points (baseline, after picture, and after donation) to directly estimate the hedonic consequences of any emotion regulation attempts. Thus, we can independently, and using a measure that is not potentially contaminated by eliciting thoughts about donation, assess if emotion regulation is more effective in the donation than in the no-donation conditions as predicted by C&P.

We conducted a high-powered replication (sample size sufficient to detect a small effect) of Experiment 1 from C&P (2011) with additional measures and manipulations to help clarify the issues stated above. As C&P argued, evidence of the motivated emotion regulation account would be found if compassion collapse (more giving to the one than the eight) occurred only in the help request conditions but not in the no-help request condition. Assuming that the between-subjects manipulation would replicate and taking into account our concerns with the main dependent variable, we expected a three-way interaction so that the compassion collapse effect should be stronger when the critical Give money item is presented first (as opposed to last) in the help request condition and induce compassion collapse in the no help request condition. Further, the motivated emotion regulation account suggests that our additional measures (donation and repeated assessment of mood) should show similar effects (e.g., less giving and better mood in the no-donation conditions). Thus, we seek to test this prediction to see if additional support for the emotion regulation account can be obtained. Finally, we assessed the prevalence of thoughts of donating and expectations to donate in all conditions. If the manipulation of expecting to donate is successful, significantly fewer participants should report thinking about donating or expecting to donate in the no-donation conditions.

MATERIALS AND METHODS

Design

A total of 1,177 participants (53.1% female, ages 18–82 years, mean 39.55) were recruited from a US sample by the Decision Research, Eugene, OR, to complete this study online.

We followed the 2 × 2 design used by C&P so that participants were randomly assigned to read about one or eight children from Darfur (number of victims) or was given the expectation that they would have to report a donation amount later in the experiment or that they would just be asked to rate their emotions toward the child (children) (help request). The sample sizes in each of these four cells ranged from a minimum of 290 to a maximum of 307.

In addition, a novel design feature was introduced to examine the potential attenuating effect of the Give money item (*To what extent do you feel that it is appropriate to* ***give money*** *to aid the child (children)* on the help request manipulation. Roughly half of the participants in each group received either the Give money item in the compassion scale as the first or the last item in their rating of compassion. The resulting design was a 2 (number of victims) × 2 (help request) × 2 (Give money item placement) between-subjects design (with a range of 135–155 participants per cell). The critical dependent variable was self-reported compassion toward the child (children) measured with the same compassion scale used by C&P.

Procedure

All critical stimuli and instructions are identical to C&P. We included an additional mood measure so that after viewing an introductory page, the participants answered demographic questions and the mood rating question "Overall how do you feel right now?" on a Likert scale ranging from −10 (Very Negative) to 10 (Very Positive). Participants then saw the same information about Darfur as presented in C&P where they either saw one or eight child images (with names and ages), depending upon the victim condition. Like the original study, the images and text were displayed on the screen for 1 min.

Participants were then given the donation manipulation. In the *help request condition*, they were told the following:

"Later in the experiment, you will be asked to rate your emotions toward this child [these children] and report how much money you would be willing to donate." Before viewing the images, participants were reminded, "Remember that later in the experiment, you will be asked to rate how you feel toward the child [children] you saw and how much you would be willing to donate." In the *no-help request condition*, participants were told the following: "Later in the experiment, you will be asked to rate your emotions toward this child [these children]. Remember that later in the experiment, you will be asked to rate how you feel toward the child [children] you saw." Participants in both conditions then saw the same Darfur information and images for min. They completed the 9-item scale from C&P measuring compassion-related feelings and attitudes toward the target or targets of aid and also answered the mood-rating question "Overall how do you feel right now?" on a Likert scale ranging from –10 (Very Negative) to 10 (Very Positive). To be as close as possible to the original study, we included the same series of scales measuring alternative explanations for the collapse of compassion as used by C&P (in all conditions). These measures did, however, not yield any additional information and are therefore not presented here. Following this, we asked participants to rate their thoughts about donating as well as their expectations about being asked to donate (not included in the original C&P study).

Thoughts of Donation and Expecting to Donate

Participants were asked "When you were rating your emotions toward this child (children), did you reflect on whether or n not to donate anything to the child (children)? (reflect item)" and "Did you expect that you were going to be asked to donate money to the child (children)" (expect item). Both measures had the answer alternatives "yes" or "no."

Next, participants responded to a hypothetical donation question (Dickert et al., 2011) that first contained the "yes" or "no" question: "Imagine you had $25 dollars in your wallet right now. Would you be willing to donate money to help the children shown earlier in the survey?" Participants that answered "yes" could indicate with a slider the amount they would donate (0–25 dollars). Finally, participants answered the mood-rating question again followed by a short version of the difficulties in emotion regulation scale[1] (DERS).

RESULTS

Following the analysis strategy of C&P, we averaged the nine items in the compassion scale (Cronbachs $\alpha = 0.96$) and two-way between-subject analysis of variance (ANOVA) was conducted to examine the effect of the number of victims and help requests on compassion. As shown in **Figure 1A** and contrast to the C&P's results, we find a significant main effect of the number of victims on rated compassion (i.e., participants felt more compassion for the many), $F(1, 1{,}173) = 23.549$, $p = 0.000$, $\eta_p^2 = 0.020$. Compassion was higher for the eight than for the one child, both in the help request and no-help request conditions. There was no significant effect of help request on rated compassion, $F(1, 1{,}173) = 3.328$, $p = 0.068$, $\eta_p^2 = 0.003$. Importantly, the interaction term between help request and the number of victims, which was used as the critical outcome in C&P, was not significant, $F(1, 1{,}173) = 0.012$, $p = 0.913$, $\eta_p^2 = 0.000$.

To further test if the interaction between help requests and the number of victims could be detected with other outcome measures, we first conducted an analysis of the donation data. For the binary donation decision, 613 out of the 1,177 participants chose to donate. When split by conditions, the donation patterns resemble the compassion ratings, where slightly more participants donated when presented with eight children (in both the help request conditions). However, this effect failed to reach significance difference, $X^2(1, 613) = 0.890$, $p = 0.372$ (see **Figure 1B**).

A two-way between-subject analysis of variance (ANOVA) was conducted to examine the effect of number of victims and help request on donation amount (including zero). Here, neither the main effect of number of victims, $F(1, 1{,}173) = 3{,}335$, $p = 0.068$, $\eta p^2 = 0.003$, nor the main effect for help request $F(1, 1{,}173) = 2{,}722$, $p = 0.099$, $\eta p^2 = 0.002$, were significant. Similar to the results for the compassion scale, the expected interaction between help request and number of victims was not significant, $F(1, 1{,}173) = 0.099$, $p = 0.754$, $\eta_p^2 = 0.000$ (**Figure 1C**).

Next, we examined the mood ratings (pre-post difference) with a two-way between-subject analysis of variance (ANOVA). As shown in **Figure 1D**, we find a significant main effect of number of victims on difference in mood (i.e., participants felt worse after seeing many children), $F(1, 1{,}173) = 15.473$, $p = 0.000$, $\eta_p^2 = 0.013$. There was no significant effect of help request on difference in mood, $F(1, 1{,}173) = 0.297$, $p = 0.856$, $\eta_p^2 = 0.000$. Moreover, the interaction term between help request and number of victims was not significant, $F(1, 1{,}173) = 3.194$, $p = 0.074$, $\eta_p^2 = 0.003$.

Taken together, the compassion scale, donation data, and mood change ratings failed to show the expected interaction between the number of victims and help requests. The main dependent variable from C&P, the compassion scale, showed that participants experienced higher levels of compassion for eight (over one) children in both the help and no-help request conditions. This finding is consistent with some previous research on compassion collapse using different materials and contexts (Wiss et al., 2015) but was not expected since we used the same materials as C&P. Thus, we fail to directly replicate the effect of Experiment 1 in C&P.

The Potential Problem With the Compassion Scale

The failure to replicate the compassion collapse effect may be related to the issue of the serial position of the Give money item [To what extent do you feel that it is appropriate to give

[1]DERS had a significant main effect $F(1, 436) = 15.223$, $p = 0.000$, $\eta_p^2 = 0.034$, where participants with lower score on DERS felt more compassion compared to participants with higher scores, but no significant interaction effects for the critical comparison number of victims $F(1, 440) = 0.476$, $p = 0.491$ $\eta_p^2 = 0.001$ or with help request $F(1, 440) = ,794$, $p = 0.373$, $\eta_p^2 = 0.002$.

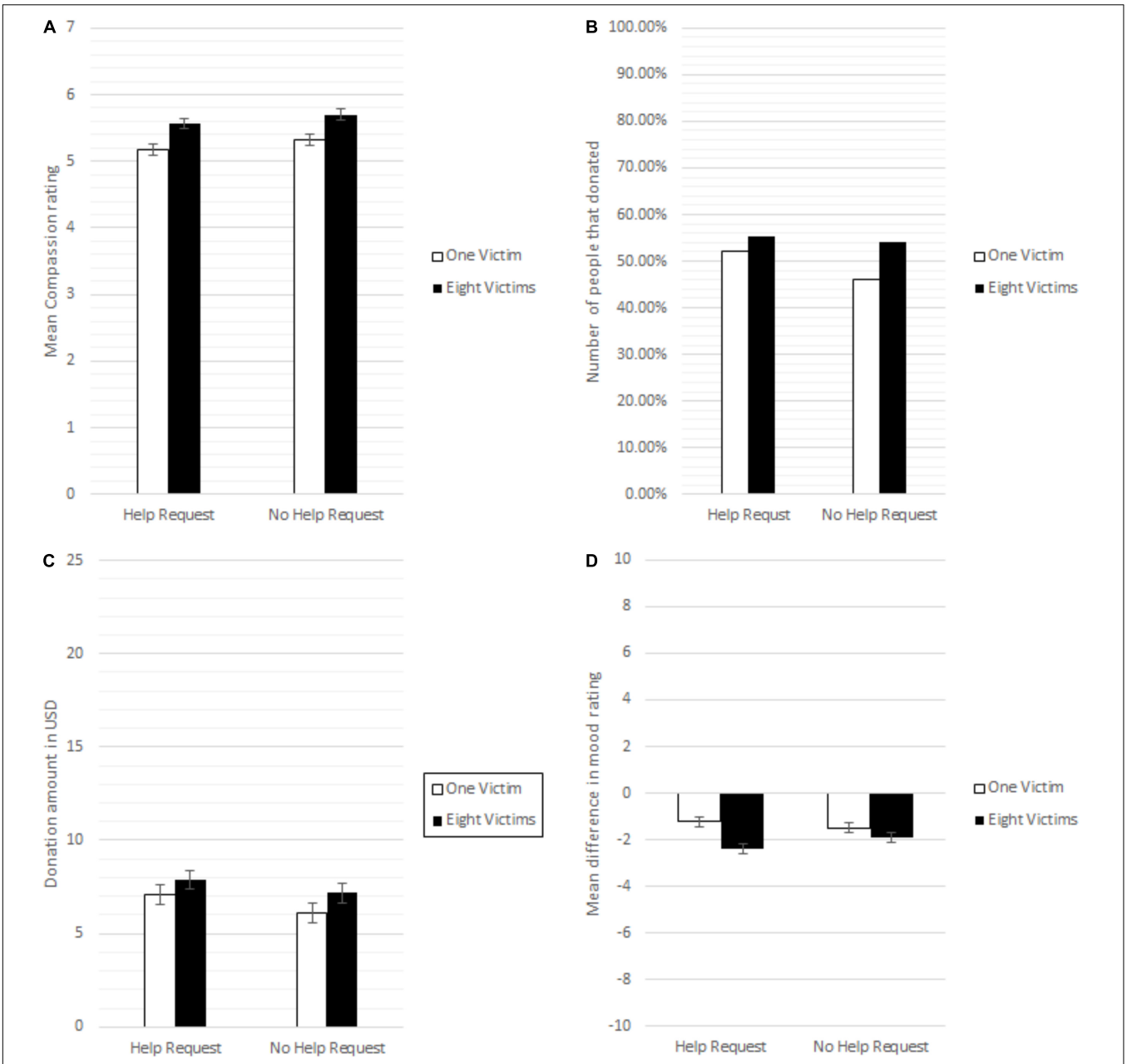

FIGURE 1 | (A) Mean compassion toward the child (children) across conditions (higher number indicates more compassion). **(B)** Number of participants that choose to donate across conditions. **(C)** Mean donation amount in USD split by help request and number of victims' condition. **(D)** Mean change in mood ratings between baseline and after viewing the child/children (lower number indicates a more negative mood). Error bars are SD.

money to aid the child [children)] in the compassion scale as outlined above. We thus conducted a 2(number of children) × 2(help request) × 2 (placement of Give money item; first/last) ANOVAs on the compassion ratings. As shown in **Figure 2**, there was no significant effect of order of the Give money item, $F(1, 1{,}169) = 3.231$, $p = 0.073$, $\eta_p^2 = 0.003$. Further, there was no interaction between the number of victims and order of the Give money item, $F(1, 1{,}169) = 0.095$, $p = 0.758$, $\eta_p^2 = 0.000$. However, there was a significant interaction between the order of the Give money item and help request, $F(1, 1{,}169) = 6.527$, $p = 0.011$, $\eta_p^2 = 0.006$, implying that, if anything, the participants rated higher compassion when prompted early with the "giving money" item. The three-way interaction between number of victims, help request, and the placement of the Give money item did not reach significance; $F(1, 1{,}169) = 0.016$, $p = 0.900$, $\eta_p^2 = 0.000$.

We conclude that the serial position of the Give money item did not have a substantial effect on the compassion ratings

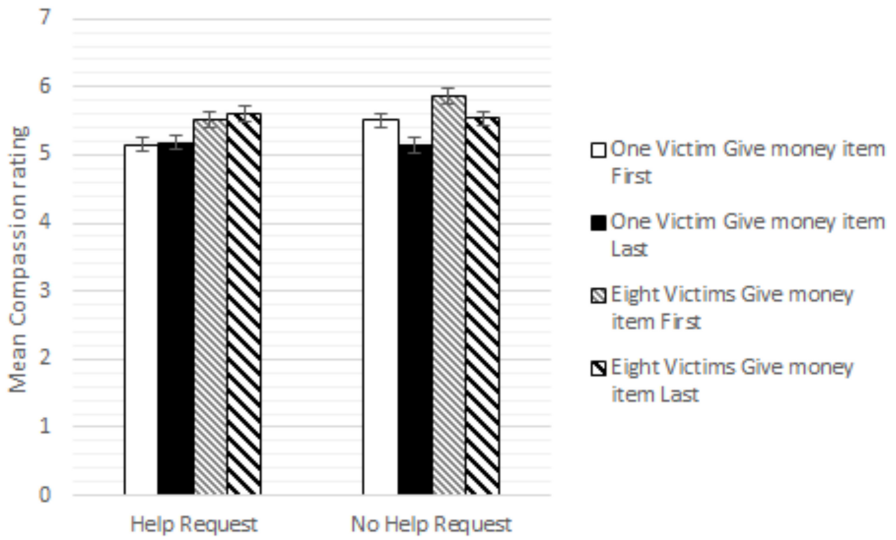

FIGURE 2 | Mean compassion toward the child (children) across conditions. Error bars are SD.

measured with the original scale used by C&P. *A priori*, we argued that the inclusion of the item could have minimized the effect of the critical help request manipulation and that if placed earlier, it may have had a larger attenuating effect (compared to if it occurred last). We do not find support for this but given that we also could not replicate the critical two-way interaction between the number of children and help requests found in C&P, it is inconclusive what role the serial position of this item played in the original C&P study.

A related concern about the help request manipulation was that participants in both conditions may be either thinking about donations (because of the nature of the stimulus material which is very similar to charitable ads; Erlandsson et al., 2016) or expecting to donate (because showing suffering child victims typically is associated with help requests; Erlandsson et al., 2016).

In the help request conditions, which explicitly stated that participants would: "Report how much money you would be willing to donate," 33.4% of the participant **did not** expect to be asked to donate money (measured using the "expect item"), suggesting that even explicitly instructing participants is not a guarantee that they will believe that they will be asked to donate. More central to the manipulation and the interpretation of our failure to replicate the original findings, in the no-help requested condition, only 41.3% reported that they **did not** expect that they would be asked to donate. Furthermore, a majority of the participants in both the help requested (53.4%) and no-help requested (59.2%) conditions stated that they did think about donating to the child (children) when rating their emotions (as measured by the "reflect item"). Combined, these results cast serious doubt on the validity of the manipulation and may partly account for the fact that we could not replicate C&P findings.

The "expect" and "reflect" items provide another quasi-experimental approach for studying compassion collapse. A central prediction from C&P is that compassion collapse should occur for those that expect to donate. Thus, we used the expect and reflect items as categorical variables substituting the help request variable. A two-way between-subjects ANOVA was conducted to examine the effect of expecting to be asked to donate (expect/not expect) and the number of victims on compassion. A significant main effect was found for number of victims, $F(1, 1{,}173) = 26{,}679$, $p = 0.000$, $\eta_p^2 = 0.022$, where again compassion was higher for the many, while the main effect of expecting to donate, $F(1, 1{,}173) = 5{,}631$, $p = 0.018$, $\eta_p^2 = 0.005$, was significant, but with higher compassion for the participants expecting to donate (**Figure 3**). A two-way ANOVA of the reflect item (think/did not think) and the number of victims was conducted on the compassion scale, where a similar pattern emerged: A significant main effect for the number of victims, $F(1, 1{,}173) = 23{,}438$, $p = 0.000$, $\eta_p^2 = 0.020$, was found where, again, compassion was higher for the many. Further, a main effect of reflecting, $F(1, 1{,}173) = 95{,}160$, $p = 0.000$, $\eta_p^2 = 0.075$, was found, with higher compassion for the participants reflecting on donation. All in all, even using these items, we again fail to replicate the expected two-way interaction from C&P.

Given our earlier concerns about the validity of the manipulation, this analysis allowed us to capitalize on participants' reports to categorize the entire sample, independent of the experimental help request manipulation, as expecting vs. not expecting to donate. Using this quasi-experimental approach, which arguably should have maximized the possibility to replicate the original number of victims × help request interaction, we still fail to find an effect.

GENERAL DISCUSSION AND CONCLUSION

Compassion collapse is a central concept in the psychology of giving and has received much attention in the literature (Kogut and Ritov, 2005a; Slovic, 2007; Västfjäll et al., 2014, 2015;

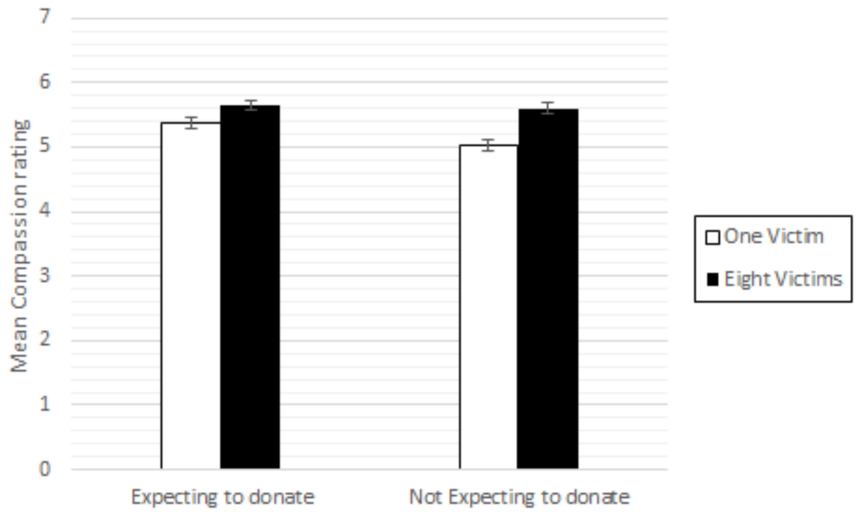

FIGURE 3 | Mean compassion toward the child (children) across conditions (higher number indicates more compassion). Error bars are SD.

Erlandsson et al., 2016; Västfjäll and Slovic, 2020). What is lacking still is a comprehensive account of the mechanisms underlying compassion collapse. The motivated emotion regulation account suggested by C&P (2011) was a much-needed attempt to shed further light on the underlying driving forces and boundary conditions of this effect. While we feel that the original account of compassion collapse (the by C&P so-called affect trigger account: Slovic, 2007), suggesting that our affect system has limitations in dealing with large numbers, is in no way incompatible with the emotion regulation account suggested by C&P, we do see great merit in the notion of anticipated financial and emotional cost as a motivator of feelings and behavior. In fact, in previous work, it has been suggested that "psychic numbing"—the automatic or motivated down-regulation of emotion—is one of the key mechanisms behind emotion collapse (Slovic, 2007; Slovic and Västfjäll, 2010; Dickert et al., 2012, 2015). We do, however, disagree with the view that anticipated financial and emotional cost is the **single** driver of compassion collapse. More specifically we take issue with how Experiment 1 of C&P can be taken as evidence for motivated down-regulation. *A priori*, our main concerns with this study by C&P centered around four key issues: (1) The relative small n in the original study, (2) the inclusion of a donation item in the main dependent variable, (3) the possibility that the critical help request manipulation was ineffective, and (4) the sole reliance on a single dependent variable that may attenuate the difference between conditions. Of these four original concerns, three main issues deserve special attention.

The Compassion Scale

The main dependent variable in C&P's experiment 1 contained an item asking for donations—although the critical manipulation of help request was explicitly instructing participants that they would be asked to donate vs. explicitly only mention that participants would rate their feelings. We argue that the inclusion of this item might effectively erase or minimize the expected effect of the help request condition. Thus, in the present study, we systematically varied the serial position of the give money question (first vs. last) based on the prediction that if the item occurred early, it would more strongly attenuate the difference than if it did occur last. We did not find this pattern. Instead, participants rated higher compassion when prompted with giving money in the no-help request condition.

Failure to Replicate the Critical Interaction

Most central to the motivated emotion regulation account, we were unable not find any evidence across four measures (the original compassion scale used by C&P, donation decision, donation amount, and mood ratings) of the critical number of children × help request interaction. C&P's main finding was that compassion collapse (giving more to 1 over 8 children) only occurred in the donation request condition and not in the no-request condition—a finding that is interpreted as evidence of motivated down-regulation of emotion when the anticipated cost is high (i.e., when participants expected to donate). Given that we were not able to find this, even though we used a large sample enough to detect a small effect sheds some doubt on the validity of the original finding. Admittingly, even though we used the same materials and presentation of stimuli as the original study, there were some differences between our study and C&P (2011): (a) C&P used a student sample, whereas we used two more heterogeneous and representative samples and (b) participants in the original C&P study was tested individually, whereas our samples responded online. We believe the sample issue to be a relatively small difference, whereas it is possible that the procedural difference (laboratory vs. online) between our studies may have played a role. For instance, participants in our study may have not engaged emotionally in the same way as participants in the original study. However, it appears from both the compassion ratings and the mood measure

that our manipulations emotionally affected the participants. Importantly, other research shows that compassion collapse can be observed using online samples (Moche et al., 2022), and research in other related domains of decision-making has shown that results from online samples are very similar to those obtained in the lab (Birnbaum, 2000; Ruggeri et al., 2020). Thus, it is unlikely that differences in sample and procedure between the studies would account for the differences in results. If motivated down-regulation occurs only in a tightly controlled laboratory setting, then this would be a severe limitation of this account of compassion collapse.

The Help Request Manipulation

Drawing on the reasoning used by C&P themselves (see section "Introduction"), we argue that it is very likely that participants in both the help request and no-help request conditions spontaneously thought about donation and maybe even have expected to donate. This is especially likely since the conversational norm activated by showing starving African child victims combined with a story about need most likely is an expectation to be asked to help. However, this may be less of a problem if the proportion of participants thinking about and expecting to donate is substantially lower in the no-help request condition than in the help request condition. When we asked participants to what extent they thought about donations, more than 50% in both conditions reported "yes" with no significant difference between conditions. Even more critical is that when asked if they expected to be asked to donate, over 50% of participants in the no-help request answered "yes" and less than 70% in the help request (that were explicitly instructed that they would be asked to donate) answered "yes." This finding is unlikely solely driven by the failure of comprehension since recent studies have shown that online samples typically perform much better on comprehension tests of instruction typically used in psychology studies than do undergraduate samples (Hauser and Schwarz, 2016). Instead, this finding likely reflects the fact that when showing experimental stimuli resembling what participants typically see in charitable ads, they spontaneously think about how to help and thus expect to be asked this question at some point during the experiment. These findings undermine the validity of the help request manipulation but also presented a possibility for us to perform another test of the motivated emotion regulation account. Following the same logic as used by C&P, we reasoned that participants reporting that they expected to donate would show compassion collapse, whereas participants not expecting to donate would not show this effect. Thus, we substituted the help request variable (i.e., an intention to treat analysis) with the quasi-experimental variable self-reported expectation (expected vs. did not expect; a per protocol analysis). This analysis should have maximized the possibility to find the expected interaction, but here the effect was significant in the opposite direction, namely that people that expected to donate gave higher ratings of compassion. Taken together, these findings suggest both that help request manipulation may not be very effective and that even when relying on participants' reports about expecting to donate, it is difficult to find evidence for the motivated emotion regulation account.

In summary, this high-powered replication of Experiment 1 in C&P (2011) failed across multiple measures (including controlling for a potentially confounding item in the original main dependent variable) and ways of categorizing/testing the help request manipulation. These results naturally do not invalidate the entire emotion regulation account of compassion collapse, but they suggest that the often-cited findings from experiment 1 of C&P may be difficult to replicate. Therefore, the implications of this particular study should be interpreted with caution. Recent work by Cameron et al. (2019, 2022) on the role of empathy and compassion (Scheffer et al., 2021) as a choice relies heavily on the results of experiment 1; for example, Cameron (2017) argues that compassion collapse should **only** emerge when people are motivated to avoid compassion for multiple victims and when they engage in emotion regulation processes to reduce compassion for multiple victims. In pitting the two views against each other, Cameron (2017) further argues that manipulating motivation and emotion regulation should not influence compassion collapse according to the "capacity" account. The evidence of the motivational account is then summarized with an explicit focus on study 1 in C&P and Cameron (2017) later concludes: "*One take-home message is that change is possible: unlike the claims of capacity accounts, the motivational account suggests that people can choose to feel more compassion for mass suffering*" (p. 265).

While it is possible that compassion may be subject to active choice, the results of the current study suggest that role of active down-regulation of compassion when expecting financial and emotional costs are salient is still open for interpretation and discussion. Given the current results, it appears difficult to fully refute the affect trigger/capacity account of compassion and empathy based on the original C&P Experiment 1 alone. Therefore, a continued active research program and discussion on the boundary limits and driving forces behind compassion collapse are much needed.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Decision Research IRB. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

WH collected data and data analysis. DV contributed to project management and drafted manuscript. GT, SD, and PS revised writing. All authors contributed to the article and approved the submitted version.

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