'Sharing in need': How allocator and recipient's hunger shape food distributions in a dictator game☆

Lotte van Dillen [a,*], Gert-Jan Lelieveld [a], Wilhelm Hofmann [b], Erik W. de Kwaadsteniet [a]

[a] *Leiden University, the Netherlands*
[b] *Ruhr University Bochum, Germany*

ABSTRACT

Do people in need share less with others? And what if the recipient is in need too? In two experiments, we addressed these questions by testing whether fasting-induced and self-rated hunger influence allocations in a dictator game in which allocators distribute food (cookies) between themselves and a recipient. In line with rational choice theory, which posits that a deprived good should increase in value (Smith, 1759; Von Neumann & Morgenstern, 1944), findings from the current studies consistently showed that participants shared fewer cookies when they were, and/or perceived themselves to be, in a food-deprived state. Across studies, participants moreover seemed to project their own hunger onto the recipient's state, as emotional perspective taking accounts propose, but this did not vary between fasting and control conditions, and did not translate into actual sharing differences, suggesting that these formed no basis for other-regarding decisions. Whether or not participants accounted for the recipient's hunger when redistributing foods, depended on whether they possessed actual knowledge of the recipient's deprivation state, such that participants engaged in greater sharing when they knew the recipient had been fasting (Study 2), but not in the absence of such knowledge (Study 1). Taken together, the results involving the need state of the recipient seem to provide most support for the need principle, that suggests that people share out of distributive justice considerations, where they take into account the recipient's need (Deutsch, 1975).

When facing the decision to share scarce resources, people often have a tendency to self-protect by keeping resources to themselves. At the same time, humans prefer to share goods fairly and behave according to distributive justice norms. In the present research, we investigate how being in need influences the sharing of scarce goods. Do people in need share less with others? And what if the recipient is in need too? In two experiments, we address these questions by both experimentally manipulating and measuring participants' hunger states, and by testing whether this influences allocations in a dictator game in which allocators have to distribute food (cookies) between themselves and a recipient. Importantly, we do not only investigate how the allocator's own hunger influences the distribution, but also how the distribution is influenced by their perceptions of the recipient's hunger state, either when they are unaware of whether the recipient has fasted (Study 1) or have been made aware of the recipient's fasting status (Study 2) prior to the dictator game.

Based on classical economic theory (Smith, 1759; Von Neumann & Morgenstern, 1944) – which assumes that people are self-interested, rational individuals – one might reason that allocators' decisions are primarily influenced by their own individual interests. This would

suggest that people are mostly influenced by their own needs (and not by the need states of the recipient), because being in need (i.e., being hungry) should increase the subjective value of commodities that satisfy that need (i.e., high-calorie foods, Siep et al., 2009; Skrynka and Vincent, 2019). However, in recent decades, behavioral economists have come to acknowledge that people tend to not only take their own interests into account, but also those of others. Most notably, Fehr and Schmidt (1999, 2006) argued that people have other-regarding preferences, and are often averse to unequal distributions of resources (termed "inequity aversion"). Likewise, the idea that people have other-regarding preferences has long been acknowledged in the social-psychological literature on social value orientations (see e.g., Messick & McClintock, 1968; Van Lange, 1999). However, such theories have not incorporated the notion that others' need states may influence one's other-regarding behavior. As such, the present paper aims to fill this gap in the literature by measuring perceptions of, and experimentally manipulating the others' actual need states in an economic game setting, namely a dictator game.

Based on theories of distributive justice (e.g., Deutsch, 1975; Lind, 2019), one might reason that allocators will – besides their own need –

also take the recipient's needs into account (i.e., the so-called need principle of distributive justice; Deutsch, 1975, 1985). Specifically, participants should keep more cookies for themselves when they are hungry, but also allocate more cookies to the other when the other is (perceived to be) hungry, as it is acknowledged that the instrumental value of the cookies for the recipient increases as a function of needs. This prediction would lead to both a main effect of own hunger and a main effect of recipient's hunger, that depending on the strength of each motive, may, or may not balance each other out.

A more specific possibility, finally, is that the influence of other-regarding motives in distributive decisions depends on people's own need states. Based on research on affective forecasting and the empathy gap (e.g., Loewenstein, 1996; Nordgren, Van der Pligt, & Van Harreveld, 2007), one might reason that sharing the same visceral state with a recipient (e.g., both being hungry) might increase allocations to this recipient. That is, when the allocator is perceived to be in the same hunger state as the recipient, this facilitates perspective-taking, which in turn could increase the number of cookies allocated to the recipient. In light of the need principle discussed in the before this could imply that need-based distributive justice becomes more salient when there is the perception that there is a "collective" need context, i.e., when the allocator is in need too. Relatedly, perspective-taking may have its most pronounced effects with regard to the means seen as instrumental in helping the other person. When both allocator and recipient are satiated, food is not as instrumental in benefitting the other person and so in that case a shared need context would not increase actual sharing. This prediction would lead to an interaction effect of the allocator and recipient's hunger with the highest number of cookies given in the condition where both are (perceived to be) hungry.

Several studies before us have addressed the question to what extent need states like hunger affect prosocial tendencies and the allocation of goods, but with a primary focus on the allocator's need state. For instance, Briers and colleagues (Briers, Pandelaere, Dewitte, & Warlop, 2006) found that participants who had fasted for 4 h donated less money to charity than satiated participants. Aarøe and Petersen (2013) varied blood glucose levels as a physiological indicator of hunger through a similar four-hour fasting manipulation and examined their effects on prosocial attitudes and behaviors. They found that participants with low compared to high induced blood glucose levels showed greater support for social welfare, but this support did not materialize into greater actual sharing during an incentivized monetary dictator game. To explain this attitude-behavior inconsistency, the authors proposed that attitudinal support mainly serves to encourage the sharing of others, rather than reflecting the individual's own sharing tendencies, because giving up actual resources is costly for hungry individuals (Blurton Jones, 1984). Thus, they considered attitudinal support to reflect 'cheap talk', as both attitude expression and actual behavior served the purpose of maximizing the individual's own benefits, in line with what rational choice theory would predict.

A recent series of four studies (Häusser et al., 2019), however, contested this line of reasoning. Using various (quasi)experimental hunger inductions such as the previously implemented four-hour fast as well as comparing pre- to post-meal decisions, the authors observed no effects of acute hunger on prosociality across non-interdependent tasks (e.g. the dictator game) and interdependent tasks (e.g. public goods games) or any correlations between subjective hunger or blood glucose levels and prosocial decisions incentivized with money or food. Thus, the current state of the literature still appears inconclusive when it comes to whether and how individuals' hunger states affect their redistributive decisions.

Even less is known about how the *recipient's* (perceived) need state is incorporated in such decisions. With regard to this question, the psychological literature provides only indirect clues. Fehr and Schmidt (1999), for example, propose that people dislike inequality as it relates to others' payoffs relative to their own payoff but they do not explicitly link this to perceived differences in need states. Based on the assumption

that need states should heighten the desire for and valuation of the deprived good (Cameron, Goldfield, Finlayson, Blundell, & Doucet, 2014; Goldstone et al., 2009; Hofmann & Van Dillen, 2012), both for the allocator and the recipient, beliefs about their relative deprivation should thus factor into the distributive decisions by the allocator.

A large number of studies however demonstrated that people often fail to appreciate the influence of visceral states on both their own and other people's behavior (e.g., Van Boven & Loewenstein, 2005; Van Boven, Loewenstein, Dunning, & Nordgren, 2013). Especially when they are in a neutral, cold state, people chronically underestimate the impact of hunger, fatigue, and other deprived needs on their decisions (Loewenstein, 1996). When in a visceral, hot state, however, people do appreciate such influences, and show a greater empathic understanding for others' impulsive behavior believed to originate from such visceral states (Nordgren et al., 2007). For example, in one experiment, participants who had previously fasted rated individuals who displayed excessive food consumption as less negative than participants who had not fasted (Nordgren et al., 2007). Notably, this effect was restricted to participants in a similar need state, as fatigued participants did not show greater understanding for other people's excessive food consumption, whereas hungry participants did not empathize more with fatigue-based impulsive actions. Thus, the authors reasoned, facilitated understanding has to be the result of emotional perspective taking based on seeing the similarity between one's own and the other person's need state. This approach thus suggests that people are more likely to share with others when they also share their needs.

1. Current research

The current research involved two behavioral experiments that served to examine the extent to which allocators' (perceptions of) own and recipient hunger uniquely or interactively affect allocators' distributive decisions during a food dictator game. Across two studies, allocator hunger was manipulated through an overnight fast. By additionally measuring (in Study 1) and experimentally manipulating (Study 2) the recipient's needs, we provide an empirical test of two competing models: (a) a model based on other-regarding preferences and equity theory (which would predict a main effect of the allocator's own and a main effect of the recipient's hunger), and (b) a model based on the state similarity (or empathy gap) literature (which would predict an interaction between the allocator's and recipient's hunger).

Upon entry to the lab, participants were all informed to be allocator. Hunger was manipulated through random assignment to overnight fasting or not fasting (Studies 1 and 2) and was measured through self-reports prior to and after the dictator game. We chose for overnight fasting rather than the four hours of fasting used in previous research on blood glucose and sharing (Aarøe & Petersen, 2013; Briers et al., 2006; Häusser et al., 2019), because overnight fasting is considered an effective induction of hunger in research on the physiology of food cravings (e.g., Van der Laan, De Ridder, Viergever, & Smeets, 2011). In addition, the previously used four-hour fasting interval yielded mixed results, sometimes demonstrating an effect (Aarøe & Petersen, 2013; Briers et al., 2006) and sometimes failing to do so (Häusser et al., 2019)." In addition, we added a food picture viewing task in Study 1 to further enhance subjective feelings of hunger (e.g., Van Dillen & Andrade, 2016) and to mask the focus on our primary task, the dictator game.

Besides their own hunger, as allocators, participants estimated the recipient's hunger right after the dictator game either without knowing the recipient's actual fasting status (Study 1) or after having been informed about their fasting status upon entry to the lab (Study 2). In other words, whereas Study 1 was designed as correlational in nature regarding perceived recipient hunger (when allocator hunger was experimentally manipulated), Study 2 was designed to independently manipulate both allocator and recipient hunger. This way, we could test the role of emotional perspective taking in participants' redistributive decisions, by establishing to what extent their uninformed versus

informed estimates of recipient hunger affected these choices, and to what extent these were based on their own hunger state.

We chose to employ a dictator game (Forsythe, Horowitz, Savin, & Sefton, 1994) because it is a well-established (Engel, 2011), simple, and externally valid (Franzen & Pointner, 2013) method for measuring sharing behavior. The dictator game is related to the ultimatum game, another commonly used task to study motivated sharing behavior (Güth, Schmittberger, & Schwarze, 1982), where an allocator makes an offer to a recipient to distribute a certain commodity (e.g., money, food). In the ultimatum game, the recipient can then accept the offer, after which the commodity will be distributed accordingly, or reject the offer, after which both allocator and recipient receive nothing. In the dictator game, however, recipients cannot reject the offer, but have to accept any offer they receive. An advantage of using this game is that it allowed us to investigate the effects of both allocator and recipient need states, without interference of strategic motivations (e.g., participants did not need to consider whether a low offer would be rejected) or expectations of reciprocity. We pilot tested the food-commodity to be shared as to maximize its subjective value. As it turned out, Oreo cookies were most liked by most participants. In these studies, we report all measures, manipulations and exclusions and sample sizes were determined before any data analysis.

2. Study 1

2.1. Method

2.1.1. Participants and design

Study 1 consisted of two between-participants conditions (fasting vs. non- fasting) in which 50[1] Leiden University students ended up participating. Individuals who were diabetic or had any food allergies could not participate. After the first five participants, we made a slight adjustment in the experimental procedure[2]. Therefore, these five participants were excluded from the study, leaving a total sample of $N = 45$ participants for the analysis (28 women, 17 men, $M_{age} = 21.36$ years, $SD_{age} = 2.14$) who were randomly assigned to the fasting ($n = 23$) and non-fasting ($n = 22$) conditions. A sensitivity analysis (calculated in GPower 3.1; Faul, Erdfelder, Buchner, & Lang, 2009) was conducted for the primary hypothesized decrease in the number of cookies shared for the fasting compared to the non-fasting group, based on an independent *t*-test. This analysis indicated that with $p = .05$, and a power of $\beta = 0.80$, the obtained sample size provided sufficient power to detect a main effect of fasting versus non-fasting of $d = 0.75$ (one-tailed).

The experiment took about 15 min and participants were paid €2.50 or 1 course credit for their participation plus the number of cookies earned in the dictator game (the ones they kept plus the ones they received). All experimental procedures were approved by the Leiden Psychology Institute's Ethics Committee (protocol number: CEP16-1221/387) on December 21, 2016 and are documented on https://osf.io/ue23b/?view_only=d99da04b69034cbb84bc861a74916c91, along with all other study materials, raw datafiles, and analysis code.

2.1.2. Procedure

Following recruitment, participants were randomly assigned to a fasting or non-fasting condition. In the fasting condition, participants were instructed to not eat or drink (other than plain water) overnight and for a minimum of 8 h prior to the experimental session, which was always scheduled in the mornings between 9:00 h and 12:30 h. In the non-fasting condition participants did not receive any fasting instructions. To mask the true purpose of the study, all participants were informed that it involved a pilot study of ghrelin hormone fluctuations in relation to decision-making, and that for that reason, at various moments during the experiment, saliva samples would be taken and they would be asked to self-report their hunger at that moment. Only after the experiment, participants were informed that saliva samples would not actually be analyzed.

Upon entry to the lab, the experimenter briefly introduced the study outline[1] and then took participants' first saliva sample with a Q-tip from the inside of their cheek and placed the sample in a test tube on a desk. Consecutively, participants were given a transparent box with five Oreo cookies and two plastic plates and were guided to one of eight individual cubicles. The uneven number of five cookies was chosen such that participants could not distribute them evenly but had to choose between at least one more for the recipient or at least one more for themselves. There, on a personal computer using Qualtrics software (Qualtrics [Computer Software], 2019), they first filled in some demographic information (sex, age) and control questions on whether they had received any fasting instructions (yes/no), how long ago they had eaten (in hours), and how hungry they were on a ten-point Likert scale (1 = *not at all*, 10 = *very*). Next, to make hunger sensations more salient, participants indicated for seven series of four high-calorie food pictures (e.g., brownies, pizzas), which they would like to consume most at this moment, after which another saliva sample was taken and they again self-reported their hunger.

Thereafter, participants were informed that they would play a dictator game with another participant who was present in one of the other cubicles. Participants did not know who this was and were agnostic to whether this recipient had fasted or not. All participants were informed to be allocator. Unbeknownst to them, they were at the same time recipient, but this was only explained to them after the game. Participants were instructed to allocate the five Oreo cookies[3] by taking them out of the transparent box and by distributing them over the two plastic plates, one plate for themselves, and the other plate for the recipient. After participants had made their decision and notified the experimenter to bring the plate to the recipient, another saliva sample

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[1] The study was originally conducted to validate the experimental set-up and to establish the behavioral effects for a larger project involving neuroendocrine assessments of ghrelin, a hormone thought to play a central role in appetite regulation. Because of very slow recruitment in January (after the Christmas break), and limited lab availability, the sample size was lower than expected. The sensitivity analysis for Study 1 yielded a critical effect size of $d = 0.75$, one-tailed, suggesting that the study was somewhat underpowered to detect the smallest reported effect size of the experimental fasting versus non-fasting difference ($d = 0.71$). Note though, that in the absence of adequate procedures, effect sizes were computed based on normal linear models of variance and that the negative skew in the data was not accounted for.

[2] The first five participants were presented with nine Oreo cookies to distribute. However, during the oral debriefing of these first participants, they indicated that nine cookies were too much for two participants. Moreover, the mean number of cookies these participants kept for themselves was much lower than half of the cookies ($M = 1.60$, $SD = 2.19$). Because of this, we decided to change the task slightly by letting all subsequent participants distribute five cookies instead. The first five participants – who had distributed nine cookies – were excluded from the data analyses, leaving 45 participants in the dataset.

[3] Oreo chocolate cookies were selected for the dictator game after a pilot test ($N = 15$) confirmed that of several small sweet snacks (e.g., biscuits, chocolate peanuts, mini stroop waffles) these were rated as most attractive on taste, smell, and appearance, ($M = 7.17$, $SD = 1.67$, on ten-point scales ranging from 1 = *very bad* to 10 = *very good*) and only two of the participants rated Oreo cookies below the midpoint of the scale ($M = 3.5$). A linear mixed effects analysis of cookie liking (1 = *Not at all*, 10 = *Very much*) with a random participant effect and liking item (look, smell, taste) as within-participants and allocator and recipient fasting as between-participants fixed factors revealed that liking did not vary across conditions ($ps > 0.073$). Participants indicated to generally like the look ($M = 6.87$, $SD = 2.28$), smell ($M = 7.78$, $SD = 2.29$), and taste ($M = 8.11$, $SD = 2.21$) of Oreo cookies. Participants moreover indicated that they would consume on average 6 Oreo cookies ($SD = 5$) but because the distribution was skewed and was influenced by one participant indicating to want to eat at least twenty, we later based the number of cookies to be distributed on the median instead, which was five.

was taken after which they again self-reported their hunger. In addition, they were now asked to indicate on the same 10-point Likert scale how hungry they thought the recipient in the dictator game was. Because all participants were at the same time allocator and recipient, in addition to the cookies they had kept for themselves, everyone then also received the additional cookies provided to them by the participant they had been paired with. Finally, all participants were thanked, debriefed, and compensated with credits or money, and were provided with additional fruits and crackers to still their hunger.

2.2. Results

2.2.1. Manipulation checks and hunger ratings

A series of comparisons were done to establish that our fasting manipulation worked as intended. As effect size, Cohen's d is reported, i. e., the mean difference between groups, divided by the pooled standard deviation. As expected, pairwise comparisons showed that participants who had fasted indicated to have eaten longer ago ($M = 11.14$ h, $SD = 1.91$, range 8–16 h) than participants who had not ($M = 1.61$ h, $SD = 1.99$, range 0–3 h), $t(43) = 16.37$, $p < .001$, $d = 4.89$. Likewise, a linear mixed effects analysis using the lmerTest function in R (version 3.6.1) with a random participant effect and measurement time (1–3) as within-participants and conditions as between-participants fixed factors revealed that across the three time points, fasting participants reported to be hungrier ($M = 6.55$, $SD = 1.63$, $d = 1.24$) than non-fasting participants ($M = 3.59$, $SD = 2.96$; $B = 1.56$, $SE = 0.56$, $F(1, 121.61) = 9.28$, $p = .003$). In addition, there was a main effect of time across the two conditions, $B = 0.33$, $SE = 0.19$, $F(2, 88.32) = 3.18$, $p = .046$; self-reported hunger increased slightly from the intake measurement ($M = 4.78$, $SD = 2.91$) to the second measurement right after the food choice task ($M = 5.22$, $SD = 2.81$, $p = .052$, $d = 0.15$), and then stabilized from measurement 2 to measurement 3 following the dictator game ($M = 5.11$, $SD = 2.85$, $p = .827$, $d = 0.038$). The complete overview of means and standard deviations is depicted in Table 1 (columns 1 to 3). Together, these results indicate that the chosen food commodity was valued and that the fasting manipulation had worked as intended and resulted in significant hunger increases.

2.2.2. Food sharing

To test whether participants in the fasting condition shared fewer cookies than participants in the non-fasting condition, we ran a Tobit regression model (or censored regression model; Tobin, 1958) with condition as (dummy-coded) predictor and cookies given to the recipient as outcome variable (using the R package censReg; Henningsen, 2011).

We decided to use a Tobit model (widely implemented in behavioral economics; e.g. Engel, 2011) instead of an OLS regression because the number of cookies participants could distribute was limited (lower limit $= 0$, upper limit $= 5$) – in other words, the outcome variable was censored – which led to a violation of homoscedasticity. The Tobit model deals with censoring and the resulting heteroscedasticity by (instead of y) assuming a latent variable y* (e.g., what participants would have reported if the number of cookies had not been restricted between 0 and 5) that linearly depends on the predictors. Note that Tobit

Table 1
Means and standard deviations (between brackets) of ratings (1 = Not at all, 10 = Very) of allocator hunger at measurement time (T1–3) and perceived recipient hunger at T3 as a function of condition (Fasting; Not Fasting), Study 1.

	T1 (intake)	T2 (after food choice task)	T3 (after dictator game)	Recipient Hunger (T3)
Fasting	6.27 (1.98)	6.73 (1.49)	6.64 (1.65)	7.23 (1.23)
Not Fasting	3.35 (3.11)	3.78 (3.04)	3.65 (3.02)	4.57 (1.88)

regression coefficients are interpreted in a similar manner to OLS unstandardized regression coefficients; however, the linear effect is on the uncensored latent variable, not the observed outcome (McDonald & Moffitt, 1980). Because the relationship between the outcome and predictor variables is non-linear, a simple effect size in the form of a standardized regression coefficient is unavailable. For illustration purposes, we do report standard deviations and effect sizes (i.e., Cohen's ds) for group differences, but note that these should be interpreted with some caution. The means and standard errors of the number of cookies allocated per condition are depicted in Fig. 1, along with the individual 'jittered' datapoints.

The Tobit model of fasting versus non-fasting on sharing yielded a significant effect of condition, $B = -0.89$, $SE = 0.39$, $t = -2.31$, $p = .021$. As expected, participants who had fasted allocated fewer cookies to the recipient ($M = 2.82$, $SD = 1.10$) than participants who had not fasted ($M = 3.57$, $SD = 1.04$, $d = 0.70$).

When added to the above model to examine more fine-grained effects of individual variation in hunger, self-reported hunger at T2 was significantly negatively associated with the number of cookies allocated, $B = -0.18$, $SE = 0.08$, $t = -2.32$, $p = .021$, whereas fasting was no longer significant, $B = -0.37$, $SE = 0.43$, $t = -0.86$, $p = .392$, suggesting that the negative effect of fasting on cookies shared could be explained by the allocator's self-rated need state.

2.2.3. Perceived hunger of the recipient

Following the dictator game, but before they could consume their cookies, participants also indicated on a 10-point Likert scale how hungry they thought the recipient in the dictator game was. Like self-reported hunger, fasting influenced these perceptions, with higher estimations for the fasting ($M = 7.23$, $SD = 1.23$) than for the non-fasting condition ($M = 4.57$, $SD = 1.88$); $t(43) = -5.59$, $p < .001$, $d = -1.65$.

When included as predictor in addition to fasting conditions and self-reported (allocator) hunger in the above-described models of the number of cookies allocated to the recipient, perceived recipient's hunger however did not add incremental validity; $Bs < 0.156$, $ts < -1.12$, $ps > 0.264$. This suggests that participants' estimations of the recipient's hunger, were based, at least in part, on the allocator's own hunger at
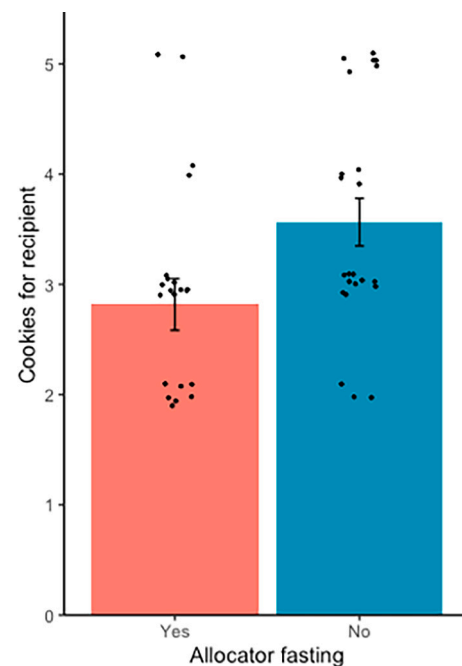

Fig. 1. Average number and individual 'jittered' datapoints of cookies allocated to the recipient as a function of allocator fasting. Error bars reflect standard errors.

that moment. This was corroborated by an ANOVA of participants' perceptions of the recipient's hunger with self-reported hunger following the dictator game (T3), and while controlling for fasting condition, that yielded a significant effect of self-reported hunger on perceived recipient hunger, $F(1,42) = 49.26, p < .001$, Cohen's $F = 1.08$. A similar highly significant association between allocator and perceived recipient's hunger was obtained when the allocator's hunger ratings prior to the dictator game were included instead ($F(1,42) = 44.85, p < .001$, Cohen's $F = 1.03$).

2.3. Discussion

Study 1 provided a first test of whether both allocator and perceived recipient hunger, as a result of overnight fasting, influence food allocations in a dictator game. Results showed that whereas cookies were generously shared in both fasting and control conditions, allocators' own objective and subjective need states did influence their redistributive decisions, such that they allocated fewer cookies to a recipient when they themselves had fasted than when they had not, and when they reported higher compared to lower hunger levels.

In line with literature on emotional perspective taking (Van Boven & Loewenstein, 2005), as allocators, participants seemed to project their own need states onto the recipient, as their perceptions of the recipient's hunger were likewise affected by the fasting manipulation, such that allocators estimated the recipient's hunger to be greater when they themselves had fasted than when they had not. Perceptions of the recipient's hunger moreover correlated positively with allocators' own hunger ratings. However, these perceptions of the recipient's hunger did not correlate with actual sharing when allocator hunger was controlled for. In sum, the findings of this first examination suggest that only the allocators' own need states, and not the need states of the recipient, appeared to have affected their decisions, leading hungry allocators to share fewer cookies with the recipient, not more.

Note, though, that because we were interested in allocators' spontaneous inferences about the recipient's need state, these assessments were post-hoc and correlational in nature. In addition, they were tested in a relatively small sample. As such, it remains unclear whether these assessments were the result or the driver of allocators' decisions in the dictator game. A more stringent test of whether allocators account for recipients' need states when sharing valued food commodities, would be to manipulate the objective need state (through fasting) of both allocator and recipient independently, and to make this salient to participants prior to the food dictator game. To this end, a second, preregistered, study was performed. In addition, a larger sample was recruited to enhance power and to allow for more sensitive moderator analyses of hunger ratings and the exploration of individual differences in perspective taking.

3. Study 2

3.1. Method

3.1.1. Participants and design

Study 2 used a 2 (allocator fasting: yes vs. no) × 2 (recipient fasting: yes vs. no) between-subjects design. We recruited 225 nondiabetic, non-allergic students at Leiden University (182 women, 41 men, 2 participants indicated other, $M_{age} = 20.99$ years, $SD_{age} = 3.09$) who were randomly assigned to the four conditions. Five participants were found to have broken cookies in half[4], despite instructions not to do so, and were removed from the analyses, resulting in a final sample of $N = 220$

participants (allocator fasting $n = 55$; recipient fasting $n = 55$; both fasting $n = 54$; both not fasting, $n = 56$).

A sensitivity analysis was conducted (in GPower 3.1; Faul et al., 2009) based on an analysis of variance for main and interaction effects of allocator fasting (yes, no) and recipient fasting (yes, no) on our primary outcome variable of number of cookies shared. This analysis indicated that with $p = .05$, and a power of $\beta = 0.80$, the obtained sample size provided sufficient power to detect a main effect of $f = 0.22$, or $d = 0.44$. Note though, that, in the absence of adequate power calculation procedures for censored data, we based our sensitivity analyses on linear models of variance, which is why the exact values should be interpreted with some caution.

The experiment took about 15 min and each participant was paid €2.50 or 1 course credit for his/her participation plus the number of cookies earned in the dictator games. All experimental procedures were approved by Leiden University's Psychology Institute's Ethics Committee (protocol number CEP18-0529/266; approved on May 30, 2018) and the study procedure and its main predictions were preregistered at the project's OSF page, see: https://osf.io/8kuge/?view_only=db12811 e7ce249bea438903ef522b94b

3.1.2. Procedure

Using a similar procedure to Study 1, following recruitment pairs of participants were randomly assigned to a fasting condition or a non-fasting condition, so that allocators and recipients were either both fasting, both non-fasting, or one fasting and one non-fasting. Participants were scheduled to come to the lab in pairs at the same time. To make participants aware of their own and the recipient's hunger state, upon arrival at the laboratory, they were explicitly asked by the experimenter whether or not they had fasted prior to the experiment. The experimenter stated the following: "have you been given special instructions about fasting?". This verbalized manipulation check was intended to make salient who of the participants pair had fasted and who had not. Participants thus received direct information about whether the other participant had fasted or not, from which they could then infer their hunger state. As in Study 1, the experimenter next took participants' first saliva samples after which they were assigned to individual cubicles and filled in some control questions on how long ago, they had eaten and how hungry they were. Contrary to Study 1, Study 2 did not include an additional picture viewing task as further hunger induction, because Study 1's analysis of participants' hunger ratings already showed a strong effect prior to the task (and relatively minor further increases). Instead, participants indicated how much they liked the look, smell and taste of the Oreo cookies as a further validation of their attractiveness. Next, as in Study 1, participants were told they were assigned the role of allocator (and that the paired participant was the recipient) and played the dictator game, in which they were instructed to distribute the five Oreo cookies. After participants had made their allocation decision and notified the experimenter, another saliva sample was taken after which they again reported how hungry they were and to what extent they believed the recipient was hungry. Next, to assess their perspective taking tendencies, they reported on the extent to which they accounted for the recipient's need state when making the decision, by indicating how much they 1) had focused on their own needs, 2) had focused on the recipient's needs, and 3) had empathized with the recipient. Finally, participants filled in demographic questions (sex, age, height, and weight) and were debriefed, compensated, received the cookies the other participant had allocated to them, and were provided with additional fruits and crackers to still their hunger.

3.2. Results

3.2.1. Manipulation checks and hunger ratings

An ANOVA of time since last eaten with allocator fasting (Yes vs. No), recipient fasting (Yes vs. No) and the interaction term as predictors confirmed the effectiveness of the fasting manipulation; participants

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[4] Four of the participants who broke cookies in half, distributed the cookies equally (2.5 cookies each). They were all assigned to the condition in which both the allocator and recipient had fasted. A fifth participant who was not instructed to fast, gave a fasting recipient 3.5 cookies out of the five.

instructed to fast indicated that they last ate longer ago ($M = 12.62$ h, $SD = 2.05$) than participants not instructed to fast ($M = 1.86$ h, $SD = 1.93$), $F(1,216) = 1572.02$, $p < .001$, $d = 5.42$.

Similar to Study 1, we analyzed the effects of our fasting manipulation and measurement time on subjective hunger ratings with a mixed ANOVA using the lmer package. Means and standard deviations for each condition are depicted in Table 2. Cohen's ds are again reported as effect sizes, derived from the comparison's means and standard deviations.

This analysis revealed an effect of allocator fasting, $F(1, 430) = 86.46$, $p < .001$, such that allocators reported greater hunger when they had fasted ($M = 3.72$, $SD = 0.94$) than when they had not ($M = 2.71$, $SD = 1.04$, $d = 1.03$), as well as of time, $F(1, 430) = 45.92$, $p < .001$, such that hunger increased slightly from the first measurement ($M = 3.10$, $SD = 1.11$) to the second ($M = 3.33$, $SD = 1.11$, $d = 0.21$). No other effects on self-rated hunger, main or interactions, were significant.

We also again asked participants about their perception of the recipient's hunger ($1 = $ *Totally not hungry*, $5 = $ *Very hungry*). As expected, a similar ANOVA showed that participants who had been coupled with a fasting recipient estimated this person to be hungrier ($M = 4.27$, $SD = 0.71$) than participants who had been coupled with a non-fasting recipient ($M = 3.05$, $SD = 0.79$), $F(1,216) = 160.87$, $p < .001$, $d = 1.69$. Additionally, whether or not participants had fasted themselves also had a significant effect on their estimates of the recipient's hunger, $F(1,216) = 8.03$, $p = .02$, $d = 0.29$. But contrary to Study 1, participants who had fasted themselves estimated the recipient to be *less* hungry ($M = 3.53$, $SD = 0.97$) than participants who had not fasted ($M = 3.80$, $SD = 0.95$; for means and SDs per condition, see Table 2).

Overall, analysis of the hunger ratings indicated that our fasting manipulations had worked as intended. Fasting participants were (estimated to be) hungrier than non-fasting participants. Additionally, the manipulation checks showed participants' perceptions of the recipient's hunger were also affected by whether or not they themselves had fasted.

3.2.2. Food sharing

The (jittered) individual datapoints, means and standard errors of the number of cookies allocated per condition are depicted in Fig. 2. As in Study 1, to account for the maximized number of cookies to distribute, we ran Tobit regression models to examine the additive and interactive effects of allocator and recipient fasting and hunger ratings on the number of cookies participants gave to the recipient. Nonsignificant interaction effects were removed before reporting main effects.

In a first Tobit model, we included allocator fasting (Yes vs. No), recipient fasting (Yes vs. No), and the interaction term as (dummy-coded) predictors and cookies shared with the recipient as the outcome variable. No significant interaction effect between allocator and recipient fasting condition was observed, $t < 1$, $p = .60$. Whereas participants again shared generously in all conditions, allocator fasting was a significant predictor of the number of cookies shared, $B = 0.83$, $SE = 0.15$, $t = 5.57$, $p < .001$, indicating that participants who had fasted gave on average significantly fewer cookies to the recipient ($M = 2.94$, $SD = 1.02$) than participants who had not fasted ($M = 3.68$, $SD = 0.90$, $d = $
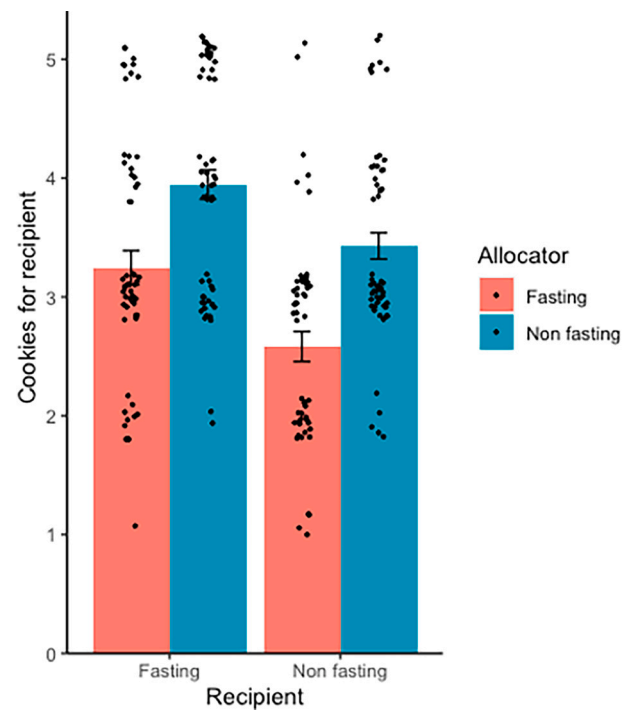

Fig. 2. Average number and (jittered) individual datapoints of cookies allocated to the recipient as a function of allocator and recipient fasting. Error bars reflect standard errors.

0.77). Additionally, recipient fasting condition also was a significant predictor of the number of cookies allocated, $-B = 0.75$, $SE = 0.15$, $t = -5.04$, $p < .001$, indicating that participants who were coupled with a fasting recipient gave significantly *more* cookies ($M = 3.60$, $SD = 1.01$) than participants who were coupled with a recipient who had not fasted ($M = 3.01$, $SD = 0.97$, $d = 0.60$).

Furthermore, we ran a Tobit model of the number of cookies allocated to the recipient, with self-reported hunger (at T2), and the perceived recipient's hunger and their interaction, next to the allocator fasting and recipient fasting condition variables. No significant interaction effect was found between self-rated allocator and perceived recipient hunger, $t < 1$, $p = .90$. This model showed that, in addition to a still significant main effect of allocator fasting condition, $B = 0.32$, $SE = 0.16$, $t = 1.98$, $p = .048$, self-reported hunger had a significant negative effect on the number of cookies allocated, $B = -0.41$, $SE = 0.08$, $t = -5.37$, $p < .001$. Additionally, perceived recipient's hunger had a significant positive effect on the number of cookies shared, $B = 0.32$, $SE = 0.10$, $t = 3.15$, $p = .002$, whereas the effect of recipient fasting was no longer significant, $B = 0.36$, $SE = 0.19$, $t = 1.72$, $p = .085$.

Thus, based on convergent results of two models including the fasting conditions or the more proximal hunger ratings, the allocators and recipients' need states seemed to affect the distribution of cookies in an additive rather than interactive manner. This pattern of results suggests that when this information is available, allocators do factor in the recipient's need state in line with the need principle of distributive justice, and contrary to the greed hypothesis; however, it does not support our more specific hot-cold empathy gap prediction.

3.2.3. Perspective taking

To explore the effects of the conditions on the three perspective taking items, we ran separate full factorial analyses of variance with focus on own needs, focus on recipient's needs (measured on a scale from $1 = $ not at all to $7 = $ very much), and empathy for the recipient (measured on a scale from $1 = $ not at all to $10 = $ very much) as the outcome variables and with allocator fasting (Yes vs. No), recipient fasting (Yes vs. No) and the interaction term as (dummy-coded)

Table 2

Means and standard deviations (between brackets) of ratings ($1 = $ Not at all, $5 = $ Very) of allocator hunger at measurement time (T1–2) and perceived recipient hunger at T2 as a function of conditions (Allocator Fasting versus Not Fasting and Recipient Fasting versus Not Fasting), Study 2.

	Recipient fasting			Recipient not fasting		
	T1	T2	Recipient	T1	T2	Recipient
Allocator Fasting	3.48 (0.95)	3.65 (0.89)	4.15 (0.71)	3.76 (0.96)	4.00 (0.92)	2.91 (0.77)
Allocator Not Fasting	2.76 (0.98)	2.98 (1.03)	4.44 (0.63)	2.39 (1.00)	2.70 (1.09)	3.18 (0.79)

predictors.

For means and standard deviations per condition, see Table 3.

These analyses revealed a significant main effect of recipient fasting on focus on self $F(1,216) = 11.70$, $p < .001$, $f = 0.23$, and empathy for the recipient $F(1,212) = 6.58$, $p = .011$, $f = 0.18$. Participants reported to be less focused on themselves ($M = 3.47$, $SD = 1.59$) and to experience more empathy ($M = 6.90$, $SD = 1.70$) for a fasting recipient than for a non-fasting recipient ($M = 4.23$, $SD = 1.62$ and $M = 6.23$, $SD = 2.04$, respectively). Other effects were nonsignificant ($Fs < 2.28$, $ps > 0.132$, $fs < 0.10$).

For focus on the recipient's need, there was a similar main effect of recipient fasting, $F(1,216) = 13.06$, $p < .001$, $f = 0.25$, indicating that participants focused more on the recipient's needs when the recipient had fasted ($M = 4.85$, $SD = 1.45$) than when the recipient had not ($M = 4.14$, $SD = 1.52$). There was also an interaction effect between allocator and recipient fasting, $F(1,216) = 3.96$, $p = .048$, $f = 0.14$, indicating that participants focused more on the needs of fasting than of non-fasting recipients, specifically when they themselves had not fasted ($B = 1.11$, $SE = 0.28$, $t = 3.97$, $p < .001$, $d = 0.71$), but not when they themselves had fasted ($B = 0.31$, $SE = 0.28$, $t = 1.13$, $p = .672$, $d = 0.23$).

All other effects were nonsignificant ($Fs < 2.28$, $ps > 0.132$, $fs < 0.10$).

Altogether these findings provide most converging evidence that allocators' perspective taking was mainly affected by the recipients' fasting state, and that allocators focused especially on fasting recipients' needs when they, themselves, were not in need.

3.3. Discussion

As a more elaborate test of the question whether allocator and recipient hunger, as a result of fasting, influences food allocations in a dictator game, a second study was performed in which the objective need states (through overnight fasting) of both allocator and recipient were manipulated independently.

Similar to the first study, the findings of this second study revealed an effect of the allocators' need state: whereas, overall, participants again shared generously, they shared fewer cookies when they had fasted than when they had not, and when they reported high compared to low levels of hunger. In contrast to Study 1, the recipient's need state now also affected allocators' redistributive decisions, such that participants allocated more cookies to a fasting recipient compared to a non-fasting recipient, and when they estimated the recipient to be hungrier compared to less hungry. Thus, when allocators were informed about whether or not the recipient had fasted prior to the dictator game, allocators did consider this while making distributive decisions. This result is in line with the need principle of distributive justice.

Further analysis revealed that the influence of the (perceived) need state of the allocator and recipient on participants' sharing behavior were not conditional on one another. When sharing foods, allocators accounted for both their own need state and that of the recipient, and this was done in an additive (or in fact countervailing) manner. Because increases in allocator hunger decreased sharing, and (perceived) recipient hunger resulted in an increase, the net effect was comparable for matched deprived and non-deprived pairs, resulting in average sharing rates in these conditions. And, as a result of the two main effects, sharing rates were highest, overall, when satiated allocators were teamed up with hungry recipients, and lowest when hungry allocators met satiated recipients.

Analysis of allocators' own hunger ratings moreover showed that, as in Study 1, these were again positively correlated with estimates of the recipient's hunger, suggesting that participants projected their own need states on the recipient. However, contrary to the findings from Study 1, allocators estimated the recipient to be *less* hungry when they themselves had fasted than when they had not fasted, irrespective of the recipient's actual fasting status. This finding runs counter to theories of emotional perspective taking, which would suggest that people are more receptive to another person's need state when they themselves are similarly deprived (Nordgren et al., 2007).

Exploratory analyses of participants' self-rated perspective taking showed mainly effects of the recipient's fasting status, such that participants reported lesser self-focus and greater empathy for a fasting compared to a non-fasting recipient. The allocator and recipient fasting manipulations influenced participants' focus on the recipient in an interactive manner, such that participants reported to have focused more on the needs of fasting than non-fasting recipients, specifically when they themselves had not fasted, which mimics the pattern of their distributive decisions. Again, this pattern of findings contradicts emotional perspective taking theory on the hot-cold empathy gap (Nordgren et al., 2007; Van Boven et al., 2013), which would predict that perspective taking would be facilitated when the perceiver and the target are in a similar 'hot' visceral state. Rather, the current findings suggest that participants were more concerned with fasting than non-fasting recipients' needs, irrespective of their own needs, or when they themselves were in a neutral 'cold' state.

4. General discussion

In two studies, we investigated how the sharing of scarce goods is influenced by participants' needs states, by testing whether allocator and recipient hunger influence allocations in a dictator game in which allocators distribute food (cookies) between themselves and a recipient. The question we asked here is: do allocators take into account the recipient's need state when distributing cookies between themselves and the recipient, and to what extent is this dependent on their own need state? Hunger was experimentally manipulated through overnight fasting and was moreover measured through self-reports. In addition to the allocators' own hunger, it was examined how allocators' perceptions of the recipient's hunger state influenced their distributive decision, either when they were unaware of the recipient's fasting status (Study 1) or when they were made aware of whether the recipient had fasted or not prior to the dictator game (Study 2).

Together, the findings from two studies consistently showed that the allocator's need state influences sharing. Participants displayed more self-interested behavior (less sharing of cookies) when they were in a food-deprived state. This is in line with the logic of rational choice theory, as deprivation should increase the value, or utility, of the deprived good (Smith, 1759; Skrynka & Vincent, 2019; Von Neumann & Morgenstern, 1944).

Less conclusive were the findings for other-regarding decision-making. Whether or not participants accounted for the recipient's hunger when redistributing foods, depended on whether they possessed actual knowledge of whether or not the recipient fasted, such that participants engaged in greater sharing when they knew the recipient had been fasting (Study 2), but not when they were uncertain about whether

Table 3

Means and standard deviations (between brackets) of focus on self and recipient (1 = Not at all; 7 = Very), and empathy for the recipient (1 = Not at all, 10 = Very), as a function of conditions (Allocator Fasting versus Not Fasting and Recipient Fasting versus Not Fasting).

	Recipient fasting			Recipient not fasting		
	Focus on self	Focus on recipient	Empathy for recipient	Focus on self	Focus on recipient	Empathy for recipient
Allocator fasting	3.74 (1.56)	4.54 (1.34)	6.67 (1.76)	4.33 (1.62)	4.22 (1.41)	6.11 (2.06)
Allocator not fasting	3.24 (1.61)	5.16 (1.50)	7.15 (1.56)	4.13 (1.62)	4.05 (1.62)	6.36 (2.03)

the other had fasted (Study 1). Whereas across studies, participants seemed to project their own hunger onto the recipient's state, as emotional perspective taking accounts propose, this did not vary between fasting and control conditions, and did not translate into actual sharing differences, suggesting that these formed no basis for other-regarding decisions. Contrary to the hot-cold empathy gap (Nordgren et al., 2007), deprived allocators (who fasted) did not empathize more with the recipient, or were more sensitive to their needs, and when the recipient was in a similar (hungry) state, this did not result in more sharing than when allocators believed the recipient was not hungry (did not fast). Thus, the results involving the need state of the recipient seem to provide most support for the need principle, that suggests that people share out of distributive justice considerations, where they also take into account the recipient's need (Deutsch, 1975).

4.1. Limitations and Future research

Whereas we observed clear effects of allocator hunger on sharing in a food dictator game, earlier work has reported null findings with a similar task (Häusser et al., 2019). As already alluded to in the introduction, the way hunger is manipulated seems to matter. Whereas earlier work tested the effects of hunger on distributions in a food dictator game using a quasi-experimental set-up (testing participants before versus after lunch) or using a four-hour fasting period, we induced hunger through overnight fasting. Whereas substantial differences in self-reported hunger were observed across all these methods, it is conceivable that the extent of food deprivation makes a difference for sharing behavior and that only more extreme hunger yields more selfish choices. The fact that we also observed a negative relationship between self-reported hunger and sharing further corroborates this view. To what extent hunger and sharing involves a linear relationship or whether behavior shifts from prosocial to selfish beyond a certain (subjective) threshold is something that could be addressed in future work, for example by varying fasting duration, and perhaps by extending it further (e.g., to 24 h, see Goldstone et al., 2009).

It would moreover be interesting to see if specific deprivation of for example salt or sugar would selectively affect the sharing of food commodities that would satisfy that particular need but not of food commodities that would not satisfy that need. So far, evidence is inconclusive on this note, with some findings supporting selectivity (Orquin & Kurzban, 2016; Skrynka and Vincent, 2019; Williams, Pizarro, Ariely, & Weinberg, 2016), and other findings pointing to more generic effects across various domains and commodities (Briers et al., 2006; Skrynka and Vincent, 2019; Xu, Schwarz, & Wyer, 2015). A recent meta-analysis (Orquin & Kurzban, 2016) suggests that food deprivation (operationalized as blood glucose level) influences the willingness to work and the willingness to pay, in particular, in the context of food. However, a recent study (Sorokowski et al., 2017) comparing sharing of money, food, and daily life objects in Poles and Tsimane' people found no difference between sharing tendencies across these commodities.

Although we observed fasting to facilitate self-interested behavior, participants were generous overall. Participants on average allocated more cookies to the recipient than they kept for themselves. Note though that participants were instructed to distribute an uneven number of cookies, making it impossible to strictly apply the equality norm by forcing an uneven distribution between self- versus other-serving outcomes. It is unclear how this element of the experimental design affected participants' need-based decisions, and whether differences would have been more or less pronounced if they would have had the opportunity for equal sharing. For example, participants in the food dictator game implemented by Häusser et al. (2019) could make an even split, and showed no differential behavior as a function of hunger. Having the option to split equally often decreases variation in participants' decisions, as the norm of equality makes it a "strong" situation (see De Kwaadsteniet, Van Dijk, Wit, & De Cremer, 2006). Future research could directly compare situations with and without the possibility for an even

split to investigate this further.

Whereas others have examined the effects of hunger on various forms of prosocial behavior (Briers et al., 2006; Häusser et al., 2019), we focused specifically on the dictator game. The dictator game has the advantage that it is a simple, well-established (Engel, 2011; Forsythe et al., 1994), externally valid (Franzen & Pointner, 2013) method to study sharing behavior, without interference of strategic motivations. This however also implies that the dictator game does not consider the full dynamic of giving and taking. For instance, in real-life situations, expectancies about whether or not the other will reciprocate or accept their prosocial or selfish behavior may influence people's sharing decisions. Future work could therefore extend our findings to more dynamic interactive settings and by using different economic games that include more interdependent decisions, such as a trust game, an ultimatum bargaining game and/or a public goods dilemma.

Across two studies we showed that when examining the effects of hunger on food distributions, and perhaps also prosocial behavior in general, it is important to not only consider the allocator's hunger, but also the recipient's (perceived) hunger. Our findings demonstrate that allocators do not make these decisions independently and based on their own needs only, but that they take the perception of the recipient's hunger into account as well. Viewing the sharing of resources as an interactive process where recipients' perceived needs influence allocators and vice versa, allows researchers to study the social processes involved in the effects of need states like hunger on prosocial and selfish behavior.

Open practices

Materials, data and analysis scripts for the two studies are available at: https://osf.io/ue23b/?view_only=d99da04b69034cbb84bc861a74916c91.

The preregistration of Study 2 is available at: https://osf.io/8kuge/?view_only=db12811e7ce249bea438903ef522b94b.

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