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Hunger affects social decisions in a Public Goods Game but not an Ultimatum Game

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Abstract

People sometimes cooperate successfully, and sometimes fail to do so. Although there is a large literature on human cooperation, we still lack evidence on the individual-level factors that influence its success or failure. Here, we investigate the impact of experimentally manipulating hunger, via asking participants to skip breakfast, in two economic games involving cooperation and punishment. In a multi-round public goods game (n = 264), participants who had skipped breakfast were more generous in the first round of the game without punishment, and made their decisions more quickly, compared to those who had had breakfast. However, no-breakfast participants were also less likely to punish their group-mates for low contributions. As a consequence, the possibility of punishment was less effective in increasing group cooperation levels in no-breakfast groups than in breakfast groups. In a one-shot ultimatum game (n = 106), skipping breakfast had no significant effects on either amounts proposed or minimum acceptable offers. We discuss the mixed and heterogeneous findings that characterise the study of hunger and social behaviour.

General introduction

It is commonplace in evolutionary psychology to state that humans are a remarkably cooperative species. The statement is, however, misleading. Humans are capable of remarkable cooperation, but also of its opposite. Failure to cooperate even when it would be mutually advantageous, and damaging anti-social behaviour, are recurrent themes of human social life. The extent of cooperative behaviour differs markedly across societies (Cohn et al., 2019; Henrich et al., 2010; Herrmann et al., 2008), across cities in the same country (Levine et al., 1994), and between neighbourhoods of the same city (Holland et al., 2012; Nettle et al., 2011; Sampson et al., 1997). Cooperative patterns of behaviour that have been quite stable can abruptly and drastically break down. Thus, a key question for the human sciences is: what factors make cooperation more likely to come and go?

Behaving cooperatively requires one set of motivations, for example the motivation to achieve long-term reciprocal, social or reputational benefits, to weigh more heavily than others, for example immediate personal resource payoff. Thus, we should expect cooperation to be affected by the state of the actors. A number of state variables may affect cooperative behaviours, such as lack of sleep (Anderson and Dickinson, 2010; Barnes et al., 2011). Here, we consider the state of hunger. Hunger, in both mild and more chronic forms, produces a suite of cognitive and motivational changes consistent with its function in securing personal food resources, quickly (Nettle, 2017). These changes may extend to monetary payoffs, which appear to be treated by the mind as a kind of food (Briers et al., 2006; Wang and Dvorak, 2010).

There is a longstanding observational literature on hunger in relation to social cooperation or its breakdown. A puzzling feature of this literature is that hunger is argued to make people both more cooperative, and less (see Dirks, 1980 for review). On the islands of Yap and Tikopia, food shortages increased cooperation as families shared more, at least initially (Firth, 1959; Schneider, 1957). In the Dutch hunger winter of 1944-5, mounting famine initially produced ‘communal kitchens’ where individuals minimised their risk though social cooperation (Burger et al., 1948). On the other hand, the ‘hungry months’ in Zambia were argued to curtail all generosity (Richards, 1932). Persistent hunger has been argued to make individuals ‘atomistic’ or ‘egotistical’: they withdraw initially from broader networks of cooperation to narrow families, and even from narrow families into complete asociality (Dirks, 1980; Turnbull, 1972). Thus, the sphere of social cooperation progressively contracts when people are hungry. Hunger has also been cited as the cause of waves of ‘famine crime’: looting, banditry, and general anti-social behaviour (Arnold, 1993). The contradictory nature of these observations lay behind Sorokin’s (1942) ‘law of diversification and polarization’: hunger brings out both the best and the worst in people. A restatement of this principle would be that hunger might increase some aspects of cooperation whilst decreasing others.

There have also been experimental studies of the effect of hunger on cooperation. Here, the hunger manipulations are milder and briefer than the severe or chronic hunger studied in the observational literature, but again, patterns have been found consistent with effects in both directions. Briers et al. (2006) found food-deprived individuals to be less generous in giving both to charity and to another participant. Yam et al. (2014) found that hungry participants were more likely to cheat on a task when a food-related reward could be gained. On the other hand, classic experiments showed that brief food deprivation made people gregarious and more motivated to affiliate with one another (Schachter, 1959). More recently, Rantapuska et al. (2017) administered a series of one-shot economic games and cooperative decisions to overnight-fasted participants who had been given either a standardized meal (control) or water (hungry) prior to the session. Hungry participants were significantly more cooperative in a prisoner’s dilemma, and tended to be so in the sender role of a trust game, whilst donations to charity, contributions in a one-shot public goods game, and behaviour in the sender role of the trust game were not significantly different. In none of the tasks was the mean level of cooperation substantially lower for the hungry than the control participants: the means were either the same or higher for the hunger condition. Rantapuska et al. (2017) link these trends to the social heuristics hypothesis (Rand, 2016). This is the idea that, in many of the artificial cooperation tasks commonly studied in the laboratory, people who reason more extensively about the specific task structure they have been assigned will tend to cooperate less, whilst those who rely on simple derived-from-life heuristics will cooperate more. Hunger has been shown to make people more reliant on quick, intuitive modes of decision making (Orquin and Kurzban, 2016).

Thus, hunger may have either positive or negative effects on cooperation, or possibly a mixture of the two, depending which aspect of which cooperative task is studied. Fully understanding the effects of hunger on cooperation would be valuable for several reasons. Different societies, social groups, and historical periods, appear to vary substantially in their ability to maintain cooperative equilibria (Henrich et al., 2010; Herrmann et al., 2008; Nettle et al., 2011; Sampson et al., 1997). Though these patterns have been correlated with various historical or ecological factors, we lack understanding of the individual-level mechanisms linking the ecological factors to the behavioural outcomes. The beauty of hunger is that it can be easily and unambiguously experimentally manipulated. Moreover, the experience of hunger does vary substantially between societies and between social groups. Even within affluent societies such as the USA and UK, deprived socioeconomic groups experience surprisingly high levels of food insecurity, poor diet, and temporally irregular eating (Gundersen et al., 2011; Nettle, 2017; Nettle and Bateson, 2019). These same groups or areas have often been found to exhibit reduced cooperation and higher levels of antisocial behaviour (Holland et al., 2012; Nettle et al., 2011; Sampson et al., 1997). Since hunger and food insecurity can be mitigated through public policy, the study of hunger and cooperation offers a potential arena not just to document variation in cooperation, but to point to routes by which real-world well-being could be improved.

In this paper, we report the results of two experiments in which we manipulated hunger and studied the effects on cooperative behaviour in economic games. Experiment 1 used a multi-round public goods game (PGG), both with and without punishment. In the PGG, participants in groups of four are given an endowment which they can either keep for themselves (non-cooperation) or contribute to a central pot (cooperation). The central pot is then augmented by the experimenter and distributed across the group. In the punishment version of the game, they can also use their game funds to sanction other group members for their behaviour. The PGG is attractive because it measures both willingness to contribute (the decision about how much to put in the fund), and also, willingness to uphold a prosocial outcome at cost to oneself (the decision about whether to sanction other group members, also known as punishment).

Experiment 2 used an Ultimatum Game (UG). In the UG, one participant proposes a split of a monetary endowment between herself and another participant. The second participant either accepts the split, in which case the money is paid, or rejects it, in which case both participants receive nothing. Our rationale for choosing the UG, and our predictions, were based on the findings of experiment 1 (see Experiment 2: Introduction).

Experiment 1: Introduction

Experiment 1 replicated the classic PGG study of Fehr and Gachter (2000), with an additional experimental manipulation of hunger. Participants, in groups of four, played a sequence of ten rounds of a PGG with no possibility of punishment, and ten rounds where they have the option to sanction one another on the basis of contributions to the central pot. The canonical finding in PGG experiments of this type is that, in the no-punishment game, contributions to the central pot are in the vicinity of 50% of endowment in the first round, but successively reduce over the rounds, approaching zero in many groups by ten rounds. By contrast, in the punishment game, contributions are stable and may even increase over the course of the rounds.

Experimental sessions were conducted in the morning, and the experimental manipulation was the instruction to either breakfast as usual (breakfast condition), or skip breakfast (no breakfast condition). We have used this manipulation before, and shown it to be efficacious in producing differences in impulsivity as well as self-reported hunger (Allen and Nettle, 2019). We ran experiment 1 in two samples of participants, sample 1 and sample 2. Our initial aims were exploratory, given the prior evidence of hunger effects on cooperation in both directions, and thus we went into sample 1 without strong directional predictions. After sample 1, we analysed the data, and ran sample 2 as a confirmatory sample using exactly the same protocol.

Methods

Participants

A total of 264 participants were recruited from registers held at Newcastle University (132 in sample 1, 132 in sample 2; 147 female and 117 male). Participants were mostly students. The two samples did not overlap. Participants were paid a £2 participation fee, and in addition, credits accumulated in the PGG were converted to cash at the rate of £1 per 50 credits. Game earnings averaged 394 credits (i.e. £7.88). All participants gave written informed consent to participate. The experiment was approved by the Faculty of Medical Sciences ethics committee, Newcastle University.

Public Goods Game

A four-player multi-round PGG, with both a punishment and no-punishment version, was implemented in oTree (Chen et al., 2016). Participants completed the game on networked computers in a room with 36 individual desks with separation screens. At least 8 participants were present per session and participants did not know which others were members of their game group. Within each round, each player was given an initial endowment of 20 credits, and had to decide how much, if any, to allocate to a central pot. Allocations to the central pot were multiplied by 1.4, and the pot was divided equally between the four group members. In the punishment version of the game, following decisions about how much to contribute to the central pot, which were known to all players, participants could elect to use their credits to reduce the earnings of other group members. Each credit used for this purpose decreased the income of the selected individual by 10% for the round, to a minimum of zero. Credits were accumulated across rounds for final conversion into cash. Participants completed 10 rounds of the no-punishment game, and 10 rounds of the punishment game, with the order of the two game versions counterbalanced. Group composition remained the same for all 20 rounds.

All game information was explicitly presented in initial on-screen instructions. Prior to the first round, participants completed three multiple-choice questions to demonstrate understanding of game rules for the payoffs from the common fund, and an additional two in the punishment game to demonstrate an understanding of how punishment worked. Comprehension questions used a multiple-choice response format. On completion of the final round, participants remained at their desks and were brought a sealed envelope containing the conversion of their accumulated credits to cash.

No-breakfast manipulation

Participants were invited to take part in the study via email, one to three days before the session. This email instructed them to either eat as normal, or refrain from eating, breakfast on the morning of the session. Self-reported compliance was collected in an initial questionnaire, as was self-reported hunger on a ten-point scale. The experimental treatment was randomised on a session level: that is, all participants on a given day, and therefore all members of each game group, were in either the breakfast or no-breakfast condition.

Data analysis

The primary outcome measures were contribution to the central pot (0-20), and punishment allocated (punishment game only). Latency to make each decision (seconds, from the decision screen appearing to the participant entering their choice) was also recorded, and we used this as an ancillary outcome measure. Due to a server problem, latency data were only available for 220 of the 264 participants (88 of sample 1, all 132 of sample 2). Experimental effects were analysed on an intention-to-treat basis. That is, the independent variable was the treatment the participant had been assigned to, rather than whether they had in fact eaten breakfast or not.

Data were analysed using general linear and, where appropriate, linear mixed and generalized linear mixed models to take account of the structure of the data (repeated rounds within participant, and participants clustered in groups). For contributions, our a priori model featured as fixed predictors: game (punishment vs. no punishment), number of previous rounds (0 – 9), experimental condition (breakfast vs. no breakfast), and all possible interactions of these predictors. We expected, based on previous literature, an interaction between game and number of previous rounds, with contributions declining over the rounds in the no punishment game, but not the punishment game. The decision to punish was treated as binary (punish at least one group member vs. not punish), using a generalized linear mixed model with binomial error structure. Our a priori model for the decision to punish involved the contribution levels of other group members, experimental condition, and their interactions.

Data were analysed in R (R Core Development Team, 2018). Linear mixed models were fitted using maximum likelihood estimation. Significance tests for parameters in mixed models used Satterthwaite's method via R package 'lmerTest'. Latencies to decide were logged for analysis.

Results

Manipulation checks

Of the participants assigned to the breakfast condition, 84.6% reported at the experimental session that they had in fact eaten breakfast (76.6% in sample 1, 91.7% in sample 2). Of those assigned to the no-breakfast condition, 99.2% reported that they had in fact skipped breakfast (100% in sample 1, 98.3% in sample 2). Participants in the no-breakfast condition reported being significantly hungrier (mean 5.59, s.d. 2.18) than those in the breakfast condition (mean 3.12, s.d. 2.18; $t = 9.16$, $p < 0.001$). The condition difference in hunger was present and similar in each of the two samples separately (sample 1: 5.78, s.d. 1.98 vs. 3.45, s.d. 2.39, $t = 6.10$, $p < 0.001$; sample 2: 5.37, s.d. 2.39 vs. 2.83, s.d. 1.45, $t = 6.71$, $p < 0.001$).

Game comprehension questions

Comprehension questions were not all answered correctly (mean central pot questions: 4.21 out of 6, s.d. 1.67; mean punishment questions: 1.32 out of 2, s.d. 0.79). The number of central pot questions correct predicted contributions: those who answered more questions correct contributed less (linear mixed model with game, number of previous rounds and number of central pot questions correct as predictors: B = -0.31, s.e. 0.11, t = -2.97, p = 0.003). Likewise, those who answered more punishment questions correct were less likely to punish (generalized linear mixed model with mean contribution of others plus punishment questions correct: B = -0.44, s.e. 0.11, z = -3.80, p < 0.001).

However, comprehension questions correct did not differ significantly by condition (central pot: breakfast: 4.18, s.d. 1.72; no breakfast: 4.24, s.d. 1.62; t = -0.32, p =0.749; punishment: breakfast: 1.27, s.d. 0.76; no breakfast: 1.37, s.d. 0.81; t = -0.98, p =0.329). Nor did they differ by sample (central pot: sample 1: 4.36, s.d. 1.59; sample 2: 4.05, s.d. 1.73; t = 1.52, p =0.130; punishment: sample 1: 1.31, s.d. 0.80; sample 2: 1.33, s.d. 0.78; t = -0.16, p =0.876). None of the results reported below for experimental condition is affected by the inclusion of number of comprehension questions correct as an additional predictor variable.

Analyses of samples 1 and 2 separately

We completed a preliminary analysis of the sample 1 data after running the sample 1 sessions. We fitted a model predicting individual contribution from number of previous rounds, game, condition and their interactions, plus random effects of group and individual (model 1; for full output of all models, see Appendix, table A1). This model substantially improved the AIC compared to a model without condition (AICs: 16160 vs. 16178). There was a significant interaction between number of previous rounds and game, with the decline in contributions over the rounds eliminated in the punishment game (B = 0.63, s.e. 0.10, t = 6.64, p<0.001). The main effect of condition (which here represents the effect of condition on contributions in the first round of the no-punishment game) was not significant (B = 1.15, s.e. 1.10, t = 1.04, p = 0.304), but there was a marginally significant interaction between condition and number of previous rounds (B = -0.19, s.e. 0.09, t = -2.00, p = 0.046), with the decline in contributions over rounds slightly greater in the no-breakfast groups than the breakfast groups. There was also a significant interaction between game and condition (B =-2.25, s.e. 0.71, t = -3.19, p = 0.001).

We fitted the same model to the sample 2 data (model 2). Again, including condition in the model substantially improved AIC (16489 vs. 16494). However, the significant experimental effects were not the same ones as in sample 1. In sample 2, there was a significant main effect of condition (B = 3.23, s.e. 1.15, t = 2.82, p = 0.009). The interaction between condition and number of previous rounds was not significant (B = -0.06, s.e. 0.10, t = -0.61, p = 0.539), and neither was the interaction between condition and game (B = -0.83, s.e. 0.76, t = -1.10, p = 0.270).

To clarify the comparability of the effects in the two samples, we created a forest plot of the parameter estimates and their confidence intervals from each sample (figure 1A). As the figure shows, although the samples differed in which effects were significantly different from 0, the parameter estimates were very similar in the two cases: the main effect of condition was positive in both; and the interaction between condition and game was negative in both. The interaction between condition and round was close to zero in both cases. For the remaining analyses, therefore, we pooled the sample 1 and sample 2 data into a single dataset.

Experimental effects in the full data set

In the pooled dataset, the main effect of condition was marginally significant (model 3; B = 1.96, s.e. 0.90, t = 2.17, p = 0.033), indicating that participants in the no-breakfast groups contributed more in the first round of the no-punishment game than those in the breakfast group (no-breakfast: mean 10.20, s.d. 5.79; breakfast: mean 8.26, s.d. 6.06). The interaction between number of previous rounds and condition was non-significant (B = -0.12, s.e. 0.07, t = -1.76, p =0.079). However, there was a significant interaction between game and condition (B = -1.42, s.e. 0.52, t = -2.75, p = 0.006). Thus, analysis of the whole dataset shows that: in the no-breakfast condition, participants started out by contributing more in the first round; and that the possibility of punishment had a different effect in the no-breakfast than the breakfast groups. The pattern of mean contributions across rounds and games by condition is shown in figure 1B.

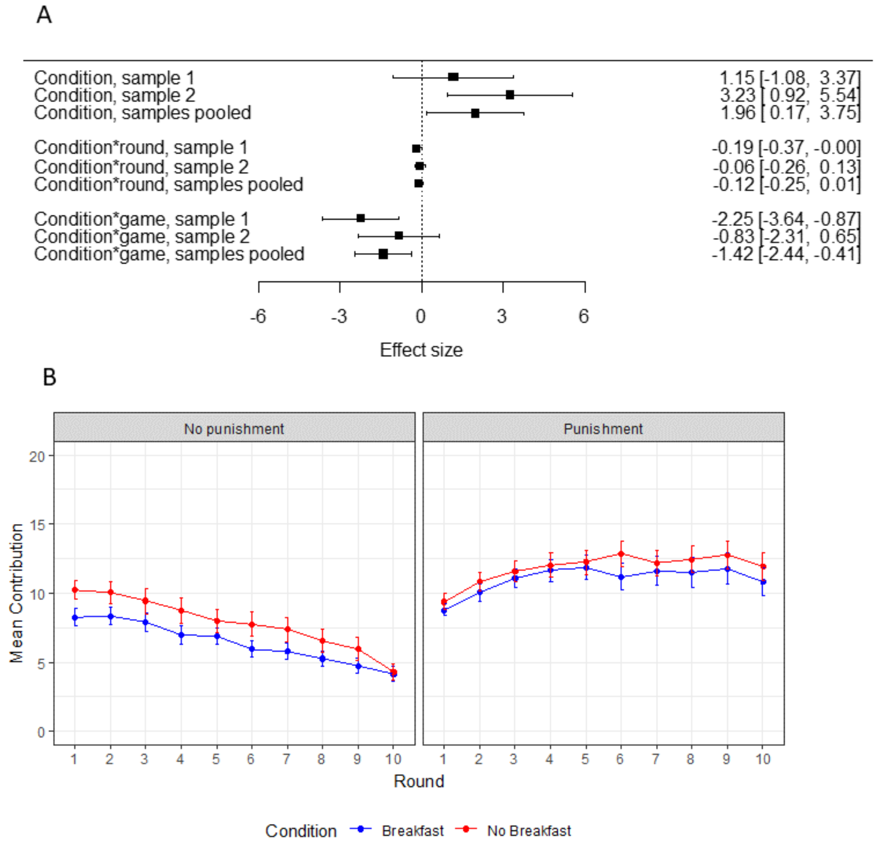

Figure 1. Experimental effects in experiment 1. A. Forest plot of the experimental effects (main effect of condition, interaction between condition and number of previous rounds, interaction between condition and game) for samples 1 and 2 separately, and the two samples pooled. The squares show parameter estimates and the whiskers their 95% confidence intervals. B. Mean contribution by round, game (punishment or no punishment) and condition (breakfast or no breakfast), both samples pooled. Error bars represent between-group standard errors of the group mean.

Effects of condition on punishment and response to punishment

The interaction between condition and game was due to the availability of punishment making less of a difference to contributions in the no-breakfast groups than in the breakfast groups, especially in the early and middle rounds (figure 2A). We explored two possibilities for why this might be the case: in the no-breakfast groups, participants changed their behaviour less in response to being punished; or else less punishment occurred in the punishment game in the no-breakfast groups. To test the first possibility, we predicted an individual's contribution, in the punishment game after round 1, from their contribution in the previous round, whether they were punished or not, condition, and their interactions (model 4). There was a significant main effect of previous-round contribution ($B = 0.45$, s.e. 0.05, $t = 9.93$, $p < 0.001$). The main effect of being punished was significant and positive ($B = 2.18$, s.e. 0.68, $t = 3.20$, $p = 0.001$; this represents the effect of being punished at a previous-round contribution of zero). There was also a significant interaction between previous-round contribution and being punished ($B = -0.17$, s.e. 0.05, $t = -3.35$, $p < 0.001$). This interaction represents the fact that whilst punishment received for a low contribution increased the subsequent contribution, punishment received for a high contribution reduced the subsequent one (figure 2B). However, neither the main effect of condition ($B = 0.57$, s.e. 1.18, $t=0.48$, $p = 0.629$), nor the interaction between condition and being punished ($B = -0.06$, s.e. 0.96, $t=-0.06$, $p = 0.954$) were significant. Thus, condition had no systematic effect on the response to punishment (figure 2B).

We computed the number of decisions to punish in the punishment game in the breakfast and no-breakfast groups. In the breakfast groups, participants chose to punish at least one other group member on 717 of 1360 possible occasions (52.7%). In the no-breakfast group, they chose to punish on 575 of 1280 possible occasions (44.9%). This difference was statistically significant ($\chi^2 = 15.74$, $p < 0.001$). Moreover, it was not due simply to differences in the amount that the other players contributed. We modelled the odds of punishing at least one other player, as predicted by the mean contribution of other group members, condition, and their interaction (model 5). There was both a significant main effect of mean contribution of group members ($B = -0.31$, s.e. 0.11, $z = -2.89$, $p = 0.004$), and a significant interaction between mean contribution of other group members and condition ($B = -0.51$, s.e. 0.16, $z = -3.14$, $p = 0.002$). At all levels of contribution from other group members, participants in the no-breakfast groups were less likely to punish, but this difference was strongest at higher levels of contribution (figure 2C).

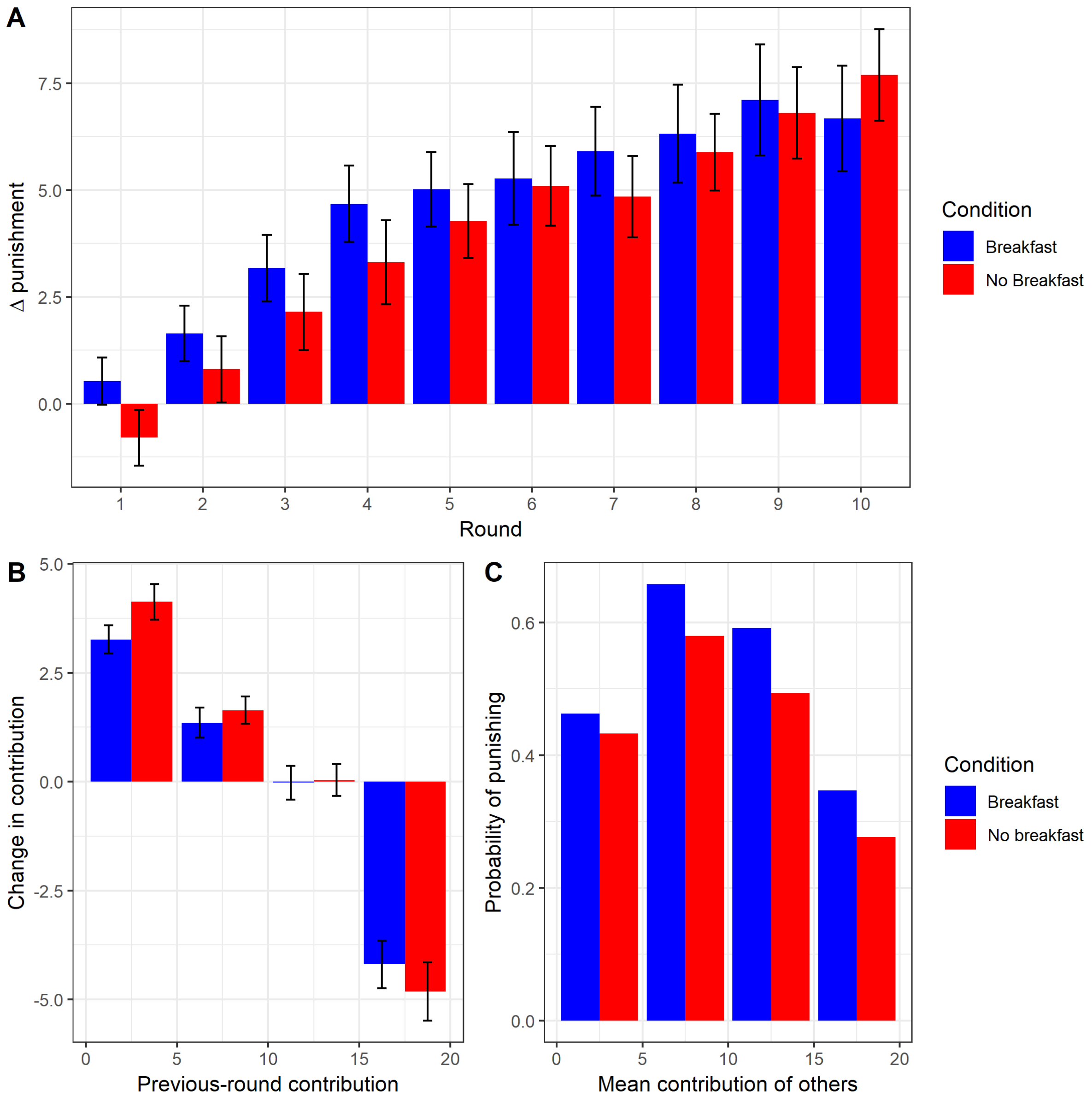

Figure 2. Punishment results in relation to condition, experiment 1. A. The difference in group mean contribution in the punishment game as compared to the no-punishment game, by round and condition. A positive value indicates that contributions were higher in the punishment than the no-punishment game. Error bars represent between-group standard errors. B. Individual participants' change in contribution from one round to the next as a result of being punished, by previous-round contribution and condition. C. The probability of individual participants deciding to send punishment to at least one other player, by the mean contribution offered by the other group members, and condition.

Effects of condition on decision latency

We examined, for the subset of participants where latency data were available, the latency to decide how much to contribute, in relation to number of previous rounds, game and condition (model 6; figure 3). There was a significant effect of number of previous rounds, with decision latency decreasing as the rounds progressed (B = -0.13, s.e. 0.006, t = -21.43, p < 0.001); and a significant interaction between number of previous rounds and game, with the decline over rounds less steep in the punishment than the no-punishment game (B = 0.02, s.e. 0.009, t =2.27, p = 0.023). There was a main effect of condition (B = -0.21, s.e. 0.06, t = -3.62, p < 0.001). This represents the difference between breakfast and no-breakfast participants in the first round of the no-punishment game; no-breakfast participants made their decisions more quickly (breakfast: mean 3.05 log seconds, s.d. 0.90; no-breakfast: mean 2.69, s.d. 0.74). There was also a significant interaction between game and condition (B = 0.30, s.e. 0.07, t = 4.52, p < 0.001); the shorter decision latency of the no-breakfast participants was not seen in the punishment game (breakfast: mean 2.92 log seconds, s.d. 0.74; no-breakfast: mean 3.05, s.d. 0.74).

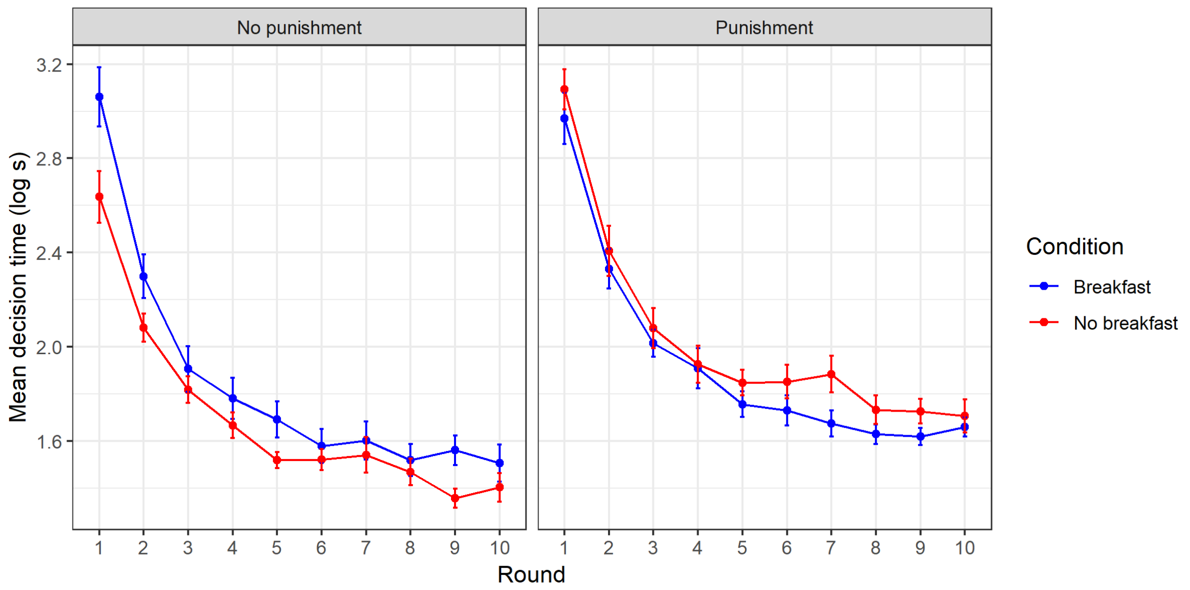

Figure 3. Latency to decide on how much to contribute by round, game and condition, experiment 1. Shown are means and between-group standard errors of the group mean.

The effect of condition on decision latency in the first round of the no-punishment game did not mediate its effect on contributions, since there was only a negligible correlation between decision latency and amount contributed (r = -0.06, p = 0.379).

We also examined, for the punishment game, the latency to decide whether to punish other group members or not (model 7). The only significant predictor was number of previous rounds (B = -0.13, s.e. 0.005, t = -23.98, p < 0.001), with decisions becoming faster as the rounds progressed. Neither the main effect of condition (B = -0.03, s.e. 0.08, t = -0.42, p = 0.674), nor its interaction with number of previous rounds (B = 0.01, s.e. 0.01, t =0.97, p = 0.335), was a significant predictor.

Discussion

In experiment 1, using a multi-round PGG, we found clear evidence that a breakfast-skipping manipulation affected patterns of cooperative behaviour. There were significant effects of experimental condition in both of the two samples considered separately. Though the two samples differed in which effects (the main effect, or the interaction with game) were significantly different from zero by the conventional criteria, the qualitative pattern of results was similar. We considered this justification for pooling the data from the two samples. In the pooled dataset, the experimental manipulation had two significant, somewhat contradictory, effects. First, participants in the no-breakfast condition contributed more to the central pot at the beginning of the no-punishment game. Second, the possibility of punishment was less effective at increasing contributions in the no breakfast as compared to the breakfast groups. We briefly consider each of these effects in turn.

The increased contributions at the beginning of the no-punishment game in the no-breakfast groups could be interpreted in terms of the hungry participants thinking less thoroughly and deliberately about the strategic ramifications of their decision (Orquin and Kurzban, 2016). This would be an example of a 'social bright side' to limited cognitive processing, similar to what Halali, Bereby-Meyer and Meiran (2014) observed using cognitive depletion paradigms in trust games. In line with this, the effect of experimental condition on initial contributions was in the same direction as the effect of getting more of the comprehension questions wrong (although no-breakfast participants did not get significantly more comprehension questions wrong). Moreover, no-breakfast participants made their contribution decisions faster than breakfast participants in the no-punishment game, although faster decisions did not lead to higher levels of cooperation in this game. Thus, no-breakfast participants were both more cooperative at the beginning of the no-punishment game, and quicker to decide, but the latter did not explain the former.

The second finding was the lessened effectiveness of the availability of punishment in increasing contribution levels in the no-breakfast groups. Further investigation showed that this was not because no-breakfast participants responded differently to breakfast participants when they were punished. Rather, it was because participants in the no-breakfast groups were significantly less likely to take up the option of punishing their group-mates. This could be seen as a kind of apathy; more focussed on their own resource acquisition, hungry participants devoted less energy to protecting the group's social dynamics. Equally, it could be seen as a kind of future discounting, a hallmark of hungry people (Allen and Nettle, 2019), since the payoff for punishing others is realised in the changes to their future behaviour. However, both of these interpretations are post-hoc. Previous studies of state manipulations have found that time pressure, lack of sleep or ego depletion if anything *increase* punitiveness (Anderson and Dickinson, 2010; Halali et al., 2014; Sutter et al., 2003), though these studies typically use the Ultimatum Game rather than the PGG. Thus, we might have expected our breakfast manipulation to increase the amount of punishment, not decrease it.

Given the findings of experiment 1, we decided to perform a second experiment with a different game, the UG. Though the structure of the UG is different from the PGG, it too allows for generosity in contribution, and for a kind of punishment. Thus, if the findings of experiment 1 were robust and generalized beyond the PGG, we had clear expectations about what we would find in a UG: greater generosity of hungry participants, coupled with lessened tendency to punish.

Experiment 2 Introduction

Experiment 2 used a single-shot UG as the cooperation experiment. In the UG, one participant (the proposer) is given an endowment and proposes a division between herself and another, unseen, participant. The second participant, the responder, either accepts the proposal, in which case the division is made, or rejects it, in which case both participants leave with nothing. The income-maximising strategy for the responder is to accept any non-zero offer. The income-maximising strategy for the proposer is to propose the lowest offer likely to be acceptable to the responder. In practice, low but non-zero offers are typically rejected, and responders offer more than they would need to have their offers accepted (Oosterbeek et al., 2004; Tisserand et al., 2015). The proposer's offer level can be interpreted as reflecting the degree of prosocial motivation (though not purely so; see Experiment 2 discussion), and the responder's minimum acceptable offer can be interpreted as an operationalization of punitiveness: to reject a low offer is to punish the proposer, at cost to oneself. Based on the findings of experiment 1, we made the following main predictions. First, no-breakfast participants should offer more than breakfast participants in the proposer role of the UG. This was by analogy to the higher contributions made by no-breakfast participants in the first-round of the PGG in experiment 1. We also predicted no-breakfast participants to state lower minimum acceptable offers than breakfast participants in the responder role of the UG. This was by analogy to the reduced propensity to punish amongst the no-breakfast participants in experiment 1.

In addition to the main measures, we again collected decision latency data, and participants also completed a cognitive reflection test. This is a widely used measure of deliberate versus intuitive decision-making. These ancillary measures were included to allow us to examine whether any differences in cooperation by condition were mediated by hungry people deciding more quickly, or with less deliberation.

Methods

Participants

We recruited 106 participants from the same sources as experiment 1 (66 male, 40 female). Due to use of genuine randomization to assign participants to conditions, our experimental groups were not equal in size (breakfast, 60; no-breakfast 46). A compromise power analysis using GPower (Faul et al., 2007) indicated that these group sizes would give us 77% power to detect a medium-sized (d = 0.5) simple experimental effect. All participants gave written informed consent to participate. Participants attended singly and made their decisions at a computer with the experimenter withdrawing. They received a £5 gift voucher as a show-up fee, in addition to whatever money they took away from the UG. The experiment was approved by the Faculty of Medical Sciences ethics committee, Newcastle University.

Ultimatum game

Each participant completed both the proposer and responder role of the UG, with the order counterbalanced. Game rules were fully explained prior to the first role, and the participant was given a chance to ask questions. The decisions of the previous participant were used to decide game outcome. Information on the other player's decisions was not provided until both proposer and responder decisions had been made. At the end of the session, money from one of the two roles was paid out according to game decisions, a visible coin toss deciding which one.

For the proposer role, the participant chose an amount to offer by moving a bar on a computer screen anywhere from £0 to £10 in increments of £0.50. The responder role used the strategy method: participants were provided with a list of all the 21 possible offers the proposer might make, and indicated which ones they would accept. From this, we calculated the participant's minimum acceptable offer.

Cognitive reflection test

Participants completed a version of the cognitive reflection test after completing the UG. We used an 8-item test, using the original three items introduced by Frederick (2005), four further items from the expanded version of the test (Toplak et al., 2014), and an additional item featured in Trouche, Sander and Mercier (2014). Each item consists of a question (e.g. 'If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?') with intuitively obvious but wrong answers (e.g. 100 minutes), and a correct answer (5 minutes) that requires more deliberation. The score is the number of correct items, which should reflect the degree of deliberation an individual is using.

No-breakfast manipulation

The no-breakfast manipulation was the same as experiment 1.

Data analysis

Data were analysed using general linear models in R (R Core Development Team, 2018). Experimental effects were primarily analysed on an intention to treat basis. However, since compliance with the manipulation was poorer in experiment 2 than experiment 1 (see Results), we also reran all analyses using self-rated hunger; none of the results reported below was affected. Decision latencies were log-transformed.

Results

Manipulation checks

Of the participants assigned to the breakfast condition, 75% reported that they had in fact eaten breakfast. Of those assigned to the no-breakfast condition, 95.7% reported that they had in fact skipped breakfast. Participants in the no-breakfast condition reported being significantly hungrier (mean 6.13, s.d. 2.06) than those in the breakfast condition (mean 3.23, s.d. 2.29, $t = -6.73$, $p < 0.001$). There were no significant differences in amount offered or minimum acceptable offer according to the order the two UG roles were played in (amount offered: first, 4.82, s.d. 1.87; second, 4.84, s.d. 1.29; $t = 0.06$, $p = 0.95$; minimum acceptable offer: first, 3.50, s.d. 1.57; second, 3.32, s.d. 1.61; $t = 0.57$, $p = 0.57$). There was a marginally non-significant, weak negative correlation between participants' proposed amounts and their minimum acceptable offers ($r = -0.18$. $p = 0.06$).

Experimental effects: Proposer role

There was no significant effect of condition on amount offered in the proposer role ($B = 0.11$, s.e. 0.32, $t = 0.33$, $p = 0.740$), though the mean difference was in the predicted direction (breakfast: 4.78, s.d. 1.33; no breakfast: 4.89, s.d. 2.01). The distribution of offers was dominated by a mode of £5 (i.e. 50% of the stake; figure 4a). This constituted 42% of all offers. Dividing offers dichotomously into those of 50% of the stake or more, and those less than 50% of the stake, the association of offer type with condition was not significant ($\chi^2 = 0.85$, $p = 0.36$). Amongst breakfast participants, 57% offered 50% of the stake or more; amongst no-breakfast participants, 67% did.

Experimental effects: Responder role

There was no significant difference in minimum acceptable offer by condition ($B = 0.32$, s.e. 0.32, $t = 1.02$, $p = 0.31$). In fact, the difference in means was in the contrary direction to prediction (breakfast: 3.26, s.d. 1.53; no breakfast: 3.58, s.d. 1.66). This was driven by a non-significant trend for more responders to demand fully 50% of the stake in the no breakfast condition (35% of responders in no breakfast condition; 20% in breakfast condition; $\chi^2 = 2.22$, $p = 0.14$; figure 4b).

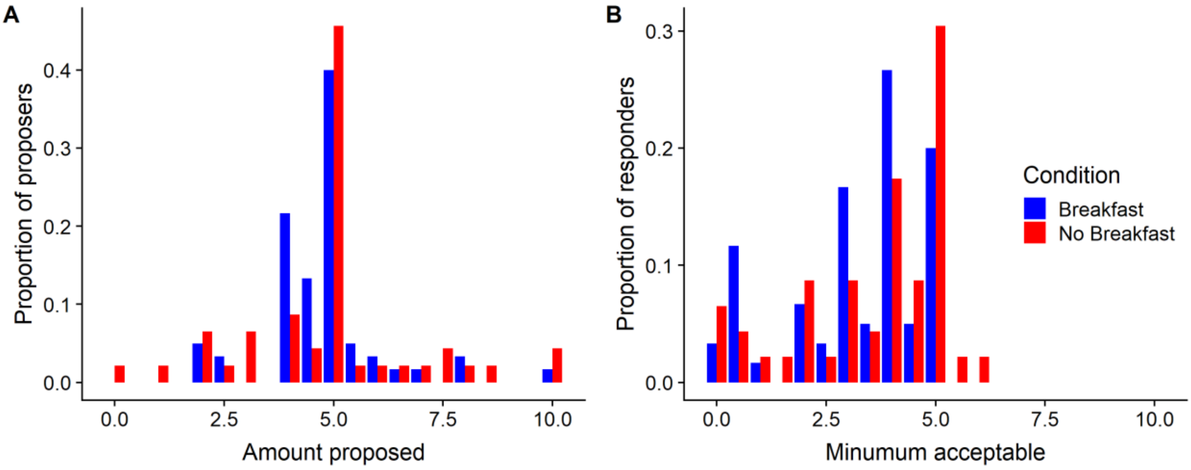

Figure 4. Experiment 2 results. A. Distribution of amounts proposed in the UG, by experimental condition. B. Distribution of minimum acceptable offers, by experimental condition.

Decision latency

There was no effect of condition on decision latency in either the proposer role (breakfast: 3.26 log seconds, s.d. 0.57; no breakfast: 3.24, s.d. 0.44; $t = -0.18$, $p = 0.86$), or the responder role (breakfast: 4.21, s.d. 0.45; no breakfast: 4.28, s.d. 0.47; $t = -0.75$, $p = 0.45$). In the proposer role, there was a significant though weak positive correlation between decision latency and amount proposed ($r = 0.21$, $p = 0.03$). In the responder role, the association between decision latency and minimum acceptable amount was not significantly different from zero ($r = 0.02$, $p = 0.85$).

Cognitive reflection test

The effect of condition on number of CRT questions correct was not significant (breakfast: 3.73, s.d. 2.07; no breakfast: 3.26, s.d. 2.00; $t = -1.18$, $p = 0.24$). Moreover, the no-breakfast participants did not spend any less time on the CRT questions than the breakfast participants did (breakfast: 6.05, s.d. 0.46; no breakfast: 6.13, s.d. 0.45; $t = 0.97$, $p = 0.33$). Number of CRT questions correct was not significantly associated with amount offered in the proposer role ($r = 0.05$, $p = 0.65$), or minimum acceptable offer in the responder role ($r = - 0.15$, $p = 0.13$).

Experiment 2 discussion

Using a one-shot UG as our cooperation and punishment task, we found no clear evidence in support of either of the main predictions we made as a result of the findings of experiment 1. Participants who had skipped breakfast did not make significantly more generous offers in the proposer role; nor did they have significantly lower minimum acceptable offers in the responder role. It is unclear how to interpret these null findings (see also General Discussion). One possibility is that the one-shot UG is too cognitively simple for any effects of hunger on social cognition to affect it. Set against this, there was considerable variation in behaviour in both roles (figure 4). Although the modal amount offered was £5.00, only a minority of participants chose this offer. There was also substantial variation in the time taken to decide. Thus, there was variability in the outcome, and we should therefore assume there was potential for the individual’s state to influence it. Another possibility concerns the structural ambiguity of the UG itself, at least for the proposer role. Proposer generosity is typically interpreted as a measure of prosocial motivation, but it is not purely this, given that low offers may be rejected (see Oosterbeek et al., 2004; Tisserand et al., 2015). This means it is also be affected by strategic considerations including risk aversion, since a rejection leads to leaving with nothing. Thus, if hunger, or any other manipulation of state, affected social motivations and risk aversion in different ways, then overall results could be null.

As well as the non-significant experimental effects on the main outcomes, we did not find a number of other relationships that we might have expected. If the no-breakfast manipulation is in effect a cognitive processing manipulation, causing people to adopt a more intuitive, heuristic approach to social decision making, then we expected it to make decision latencies faster, and scores on the CRT lower. The experimental treatment had neither of these effects. Moreover, there were no relationships between CRT score and UG behaviour. The amount offered as proposer actually increased slightly with increasing time taken to decide. This is the opposite to the prediction stated in Rand (2016), namely that in this one-shot game, more deliberation should be associated with lower offers. (Although see the point about the inherent ambiguity of the UG, above: more deliberation might lead proposers to appreciate the greater riskiness of making a low offer. Rand's (2016) prediction appears better applicable to the simpler Dictator Game, where there is no question that making a zero-offer is the income-maximising strategy within the game.)

General discussion

The results of our experimental explorations of the effects of hunger on cooperation, and punishment for non-cooperation, were mixed. In experiment 1, we found clear evidence that participants who skipped breakfast began the PGG by contributing more to the common pot; but were also less prone to punish their group members for non-contribution when they had the opportunity to do so. The consequence of this was that the possibility of punishment was less successful at increasing contributions in no-breakfast groups than in breakfast groups. We also found clear evidence that participants who had skipped breakfast made their decisions more quickly in the no-punishment game, providing a possible cognitive basis for their different behaviour.

In experiment 2, we used a different, simpler economic game, but one that also provided a measure of cooperation and a measure of punishment for non-contribution. The expectations from the findings of experiment 1 were straightforward: participants who skipped breakfast should contribute more, but have lower minimum acceptable offers (which within the UG paradigm represents a lower inclination to punish). There was no evidence for either of these patterns, nor for the pattern seen in experiment 1 whereby participants who skipped breakfast made their decisions faster. Indeed, in contrast to experiment 1 where there were treatment effects on multiple variables, we could find no clear evidence that the breakfast-skipping manipulation in experiment 2 had any systematic effect other than making participants feel hungrier.

Faced with this mixed picture, there are a number of possible reflections. First, the true effects of hunger on cooperative behaviour in economic games, at least within the mild range studied here, may be null. In this case, the experiment 1 findings would represent a false positive. This is always possible, though there are two arguments against it being the most likely explanation. First, treatment effects were found across several different variables and were not marginally significant by conventional criteria (the experimental effect *p*-values were in the <0.01 range rather than $0.01<p<0.05$). Second, experiment 1 was conducted in two separate samples of 132 participants each. In each sample separately, including condition in the analysis substantially improved explanatory power, and the effects were very similar, though not identical, across the two samples (figure 1A).

Another possibility is that there are general and systematic effects of hunger on behaviour in economic games, and experiment 2 represents a false negative. The power of experiment 2 to detect medium effects (d = 0.5) was reasonably high (77%). Its power to detect a small effect of the manipulation (d = 0.2) was much more modest at 40%; and indeed, for amount proposed, the observed non-significant difference was d = 0.07 in the direction of the prediction. Thus, the null results of experiment 2 allow us, at best, to say that if there are effects of skipping breakfast on the behaviour in the UG in this population, they seem likely to be quite small. We cannot strongly conclude that they would be zero in a large enough sample.

A third possible reflection is that there are reliable effects of skipping breakfast on behaviour in the multi-round PGG, but not the UG. This is plausible for several reasons. The proposer behaviour in the UG is ambiguous as measure of prosocial motivation (see Experiment 2 discussion), being potentially affected by risk aversion too. However, this does not apply to the responder behaviour as a measure of punishment. The PGG is more cognitively complex than the UG. The UG experiment was very brief (around 90 seconds); the PGG experiment was long, with sessions typically lasting around 20 minutes. If hunger leads to limitations in sustained attention or in resources available to devote to difficult ongoing cognitive challenges, the long PGG sessions and complex task were much more likely to reach this limit than the brief UG. Even though one of the main results of experiment 1 concerns the amount contributed in the first round of the no-punishment game, by the time participants reached this, they had already had to attend to complex instructions, complete multiple comprehension questions involving mental arithmetic, and possibly (depending on game order) completed 10 rounds of the no-punishment game as well. Thus, the more elaborate PGG may simply push participants further in terms of the cognitive resources they are prepared to expend in the service of a social task. If this is correct, the PGG is probably a better model of many real-world processes of social cooperation, which are typically temporally extended and interactionally complex, than the one-shot UG is. The fact that we found evidence for effects of hunger in the PGG is thus noteworthy.

Given the discrepancy between experiments 1 and 2, strong claims regarding the nature and strength of hunger effects on cooperation in laboratory games would be premature. We found two features in our research that resonate with previous reports. First, there are experimental effects more often than would be expected by chance, but these appear in some tasks and not others (compare Rantapuska et al., 2017). Whether this represents systematic differences between tasks or just sampling variability in the presence of small effects remains to be determined. Second, hunger effects, where found, appear quite paradoxical: increasing initial contributions but decreasing punishment in our experiment 1; increasing cooperation in some experiments (Rantapuska et al., 2017), but decreasing cooperation or increasing antisocial behaviour in others (Briers et al., 2006; Yam et al., 2014). This implies that 'cooperation' is not psychologically unitary. Different cooperative tasks have different psychological components, as well as payoffs over different timescales. The fact that cooperativeness tends to be consistent across different experimental dilemmas in survey studies (Peysakhovich et al., 2014) does not mean that state-changing experimental manipulations could not affect some dilemmas but not others (or affect different dilemmas in different directions). Different elements of cooperation may be more or less motivationally salient to hungry people, who are concerned to obtain at least some resources soon. For example, simple forms of social pooling can reduce the risk of being left with nothing, a salient concern for a hungry animal (Stephens, 1981). Other elements of cooperation involve extensive negotiation and future-oriented investment to establish and uphold norms. These elements may be beyond the motivation of hungry individuals, who have more immediate priorities (Allen and Nettle, 2019; Wang and Dvorak, 2010).

What about applications of our results to real-world variation in social cooperation and anti-social behaviour? The ecological validity of experimental economic games like the PGG and UG is certainly questionable (see e.g. Burton-Chellew and West, 2013), and this is a further reason for caution in extrapolating to natural social processes. Hence, there is a need for experimental studies of hunger and cooperation that use more naturalistic tasks. Nonetheless, if the account we sketched above correctly captures the reasons for the mixed and heterogeneous findings in the laboratory, it may also help explain the complex picture depicted in real-world histories of hunger too. In the literature, it is easy to find descriptions of enhanced generosity and sociality in times of hunger, akin to the greater contributions at the beginning of experiment 1 (see Dirks, 1980). However, it is also easy to find descriptions of the gradual breakdown in the upholding of any norms not related to immediate food distribution, like a more dramatic version of the reduced prosocial punishment in hungry groups in our experiment 1 (Keys et al., 1950; Turnbull, 1972). Hunger may both provide a greater motivation for an initial coming together of different individuals; but also make it more likely that these cooperative endeavours will flounder in the long run. This duality—heightened desire for sociality, coupled with greater turbulence in its dynamics—is not just a feature of social groups experiencing hunger, but perhaps of social groups experiencing economic adversity more generally. The literature on poverty and social behaviour contains evidence for poverty and deprivation increasing social solidarity, and also undermining it (see Nettle, 2015; Nettle et al., 2011; Piff et al., 2010). We end, in other words, reiterating and extending Sorokin's (1942) 'law of diversification and polarization': hunger can bring out both the best and the worst in people, and sometimes does not seem to make any difference at all.

Data availability

Raw data and code for this study are freely available via the Zenodo repository at: https://zenodo.org/record/3469364

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Appendix

Table A1. Full model output for experiment 1.

Model	Data	Outcome	Predictor	B (s.e.)	t or z	p-value
1	Sample 1 only	Contribution	Previous rounds	-0.42 (0.07)	-6.29	<0.001
			Game	2.97 (0.51)	5.85	<0.001
			Condition	1.15 (1.10)	1.04	0.304
			Previous rounds * Game	0.63 (0.10)	6.64	<0.001
			Previous rounds * Condition	-0.19 (0.09)	-2.00	0.046
			Game * Condition	-2.25 (0.71)	-3.19	0.001
			Previous rounds * Game * Condition	0.11 (0.13)	0.83	0.405
2	Sample 2 only	Contribution	Previous rounds	-0.55 (0.07)	-8.14	<0.001
			Game	0.28 (0.51)	0.54	0.587
			Condition	3.23 (1.15)	2.82	0.007
			Previous rounds * Game	0.73 (0.10)	7.71	<0.001
			Previous rounds * Condition	-0.06 (0.10)	-0.61	0.539
			Game * Condition	-0.83 (0.75)	-1.10	0.270
			Previous rounds * Game * Condition	0.27 (0.14)	1.90	0.058
3	All	Contribution	Previous rounds	-0.49 (0.05)	-10.25	<0.001
			Game	1.54 (0.36)	4.28	<0.001
			Condition	1.96 (0.90)	2.17	0.033
			Previous rounds * Game	0.69 (0.07)	10.15	<0.001
			Previous rounds * Condition	-0.12 (0.07)	-1.76	0.079
			Game * Condition	-1.42 (0.52)	-2.75	0.006
			Previous rounds * Game * Condition	0.18 (0.10)	1.83	0.067
4	Punishment game	Response to punishment	Lagged contribution	0.45 (0.05)	9.93	<0.001
			Condition	0.57 (1.18)	0.48	0.630
			Lagged punishment	2.17 (0.68)	3.20	0.001
			Lagged contribution * Condition	-0.01 (0.06)	-0.09	0.929

			Lagged contribution * Lagged punishment	-0.17 (0.05)	-3.36	<0.001
			Condition * Lagged punishment	-0.06 (0.96)	-0.06	0.954
			Lagged contribution * Condition * Lagged punishment	0.02 (0.07)	0.23	0.820
5	Punishment game	Decision to punish	Group contribution	-0.31 (0.11)	-2.88	0.004
			Condition	-0.41 (0.35)	-1.17	0.243
			Group contribution * Condition	-0.52 (0.16)	-3.14	0.002
6	All	Log latency to decide contribution	Previous rounds	-0.13 (0.01)	-21.43	<0.001
			Game	-0.03 (0.05)	-0.55	0.583
			Condition	-0.21 (0.06)	-3.62	<0.001
			Previous rounds * Game	0.02 (0.009)	2.27	0.023
			Previous rounds * Condition	0.01 (0.01)	1.53	0.127
			Game * Condition	0.30 (0.07)	4.52	<0.001
			Previous rounds * Game * Condition	-0.02 (0.01)	-1.25	0.21
7	Punishment game	Log latency to decide punishment	Previous rounds	-0.13 (0.01)	-23.98	<0.001
			Condition	-0.03 (0.08)	-0.42	0.67
			Previous rounds * Condition	0.01 (0.01)	0.97	0.34

Note. All models contain random effects of participant nested within group. Model 5 is a generalized linear mixed model with a binomial error structure. All other models are general linear mixed models. The reference categories are no punishment for Game and breakfast for Condition.