**Title:**

Coordinated Decision-Making Boosts Altruism – But Not Trust

Classification: Social Sciences

**Authors:**

1. Matthew Chennells

Department of Philosophy, University of Warwick

Email: [m.chennnells@warwick.ac.uk](mailto:m.chennnells@warwick.ac.uk)

1. …
2. Stephen Butterfill

Department of Philosophy, University of Warwick

Email: [s.butterfill@warwick.ac.uk](mailto:s.butterfill@warwick.ac.uk)

1. John Michael

Affiliation: Department of Cognitive Science, Central European University

Email: [Johnmichael.cogsci@gmail.com](mailto:Johnmichael.cogsci@gmail.com)

**Abstract**

Previous research has shown that coordinated decision-making increases people’s willingness to cooperate in subsequent social dilemmas. In the current study, we conducted two pre-registered experiments to investigate the cognitive and motivational mechanisms underpinning these effects. Specifically, we separately tested whether coordinated decision-making increases altruism and whether it increases trust. To this end, we designed and implemented a paradigm in which participants repeatedly perform a coordinated decision-making task either with the same partner on every trial (Fixed Partner Condition), or with a different partner on each trial (Variable Partners Condition). When both players coordinate on the same option, both are rewarded. In Experiment 1, participants were sometimes presented with tempting opportunities to defect from the coordination. In Experiment 2, participants sometimes had to decide whether or not to trust that their partners had resisted such tempting opportunities. The results show that repeatedly coordinating with the same partner increased participants’ resistance to temptation (Experiment 1) but did not increase trust (Experiment 2). These findings support the hypothesis that repeatedly coordinating with a partner stabilizes cooperation by increases altruistic motivation towards one's partner, not by increasing trust.

*Keywords*: coordination, decision-making, altruism, trust, cooperation

**Significance**

We humans are highly adept at coordinating our decisions and actions with others. This enables us to achieve our goals more efficiently than we could do alone, and also to attain outcomes that we could not otherwise attain. In addition to these directbenefits of coordination, the current study shows that coordination may have important indirect benefits as well. Specifically, our results show that repeatedly coordinating with a stable partner can foster a willingness to make sacrifices to benefit that partner – although, surprisingly, it does not foster a willingness to trust one’s partner to make such sacrifices.

**Introduction**

The prevalence and flexibility of human cooperation is unparalleled by any other species. We routinely work together to achieve ends that we could not achieve alone, even setting aside short-term interests to maximize the benefits to our interaction partners and larger social groups. In recent decades, a substantial body of research in evolutionary theory, experimental economics and psychology has been devoted to investigating the evolutionary origins of human cooperation (Henrich & Henrich, 2007; Nowak, 2012; Tomasello, 2009; Skyrms, 2004; West, Griffin, & Gardner, 2007).

This has led to significant progress in specifying evolutionary mechanisms that are likely to have supported the evolution of cooperation in humans. Some of the key ideas that have been explored in this context are kin selection (Hamilton, 1963; Smith, 1964), direct (Trivers, 1971) and indirect (Nowak & Sigmund, 2005) reciprocity, competitive altruism (Roberts, 1998), cultural group selection (Boyd & Richerson, 2008), and the interdependence hypothesis (Roberts, 2005).

This research on the evolution of cooperation has also informed and constrained research into the cognitive and motivational mechanisms that *proximally* support cooperation. For example, theoretical work on indirect reciprocity and on competitive altruism has inspired research devoted to illuminating the mechanisms by which people manage their reputations (Nowak & Sigmund, 2005; Fehr & Gächter, 2002; Andreoni & Bernheim, 2009; Rege & Telle, 2004). This research has provided evidence that reputation management may be subserved by prosocial preferences, such as a preference for fairness (Andreoni, 1990), an aversion to inequity (Fehr & Schmidt, 1999) and an aversion to disappointing others’ expectations (Dana et al., 2007; Heintz et al., 2015).

Moreover, research on the evolution of cooperation also provides us with reason to expect that people’s willingness to cooperate with another agent should be strengthened by any cue that one is likely to interact with that agent in the future (direct reciprocity), that the interaction is relevant for one’s reputation (indirect reciprocity; competitive altruism) or that one has a stake in that agent’s well-being (interdependence hypothesis). For example, when two agents coordinate with each other – i.e. when they mutually adapt their decisions or actions to bring about a shared goal or compatible but distinct individual goals (Michael & Pacherie, 2015; Skyrms, 2004) – each may come to perceive the other as an in-group member and/or a valuable partner. If so, then coordination with a partner may boost people’s willingness to cooperate with that partner – i.e., to coordinate with them despite the availability of alternative options which may be individually preferable (Michael & Pacherie, 2015). And indeed, previous research has shown that people’s willingness to cooperate in social dilemmas may be fostered by repeated coordination in decision-making (Guala & Mittone, 2010; Rusch & Luetge, 2016) or in action (Wiltermuth & Heath, 2009; Van Baaren et al., 2004).

This raises the further question as to what the specific cognitive and motivational mechanisms are by which coordination increases cooperation. One possibility is that coordination may enhance trust in one’s partner. This is the hypothesis put forward by Rusch & Luetge (2016). They reasoned that coordination with a partner may lead people to perceive their partner as being reliable in general, and therefore also as someone who is likely to resist the temptation to behave selfishly. As a result, people should be more likely to cooperate with a partner with whom they share a history of coordination. And indeed, this rationale is consistent with the results of an earlier study in which Knez & Camerer (2000) showed that cooperation rates in a prisoners’ dilemma were higher if participants had previously performed a coordination game together than if they had not. Building on this, Rusch & Luetge (2016) found that cooperation rates in a prisoners’ dilemma were boosted when rounds of the prisoners’ dilemma were interspersed among rounds of a coordination game (i.e. the stag hunt) played together with a fixed partner.

But while this hypothesis may explain why people would be more likely to expect their partners to cooperate in a prisoners’ dilemma when they have repeatedly been coordinating with the same partner than when they have been coordinating with different partners, it does not directly explain why people would then themselves be motivated to cooperate. Indeed, if one expects the other player in a prisoners’ dilemma to cooperate, then one can expect to attain the highest possible reward by defecting. This implies that an increase in trust could only explain why people who want to cooperate do not defect in order to avoid being exploited, but it does not explain why they would desire to cooperate. Moreover, it is worth noting that a track record of coordination with a partner who coordinated when it was in her interest does not directly provide evidence that that partner would resist tempting alternatives if they were to arise. ~~In this case, it means that a history of coordination is not in fact a strong indicator of trustworthiness.~~

A distinct but compatible hypothesis is that coordination increases cooperation by eliciting altruistic motivation towards one’s partner (i.e. a willingness to pay a cost to benefit the partner; Fehr & Fischbacher, 2003). Theoretically, this hypothesis is directly motivated by the interdependence hypothesis, which states that human cooperation arose in a period in which our ancestors lived in small groups of individuals whose interests were largely interdependent, and for whom it was therefore not typically beneficial to act selfishly to the detriment of other group members (Roberts, 2005). Insofar as repeated coordination with a partner may provide a cue that one has a stake in their welfare, interdependence supports the hypothesis that coordination boosts cooperation by boosting altruistic motivation towards one’s partner. ~~This hypothesis is also consistent with direct and/or indirect reciprocity, as well as competitive altruism insofar as repeated coordination may indicate that one is likely to interact with the same partner in the future and/or that the interaction is relevant for one’s reputation.~~

Importantly, this hypothesis is consistent with previous findings from studies using prisoners’ dilemmas to measure agents’ willingness to cooperate (Knez & Camerer, 2000; Rusch and Luetge, 2016): as altruistic motivation is both necessary and sufficient for cooperation in a standard prisoners’ dilemma, it is difficult to determine whether, and to what extent, people’s trust that their partner will cooperate mediates cooperation rates in prisoners’ dilemmas. Moreover, this hypothesis is also supported by research showing that a history of collaboration with a partner creates a sense of debt, boosting altruistic sharing with that partner (McGrath & Gerber, 2019). Furthermore, it is motivated by a wealth of research on the effects of coordinated *action* upon altruistic behaviour. For example, it has been shown that sensorimotor coordination increases helping behaviour in adults (Kokal et al., 2011; Michael, Sebanz & Knoblich, 2016; Valdesolo & Steno, 2011) and children (Barragan & Dweck, 2014; Cirelli, Einarson & Trainor, 2014; Hamann et al., 2012).

*The Current Research*

We conducted two pre-registered experiments to investigate the cognitive and motivational mechanisms by which repeated coordination with the same partner increases people’s willingness to cooperate with that partner. More specifically, the two experiments were designed to separately test two distinct (albeit compatible) hypotheses:

*Hypothesis 1:* Repeated coordination boosts cooperation by eliciting altruistic motivation towards one’s partner (Experiment 1).

*Hypothesis 2:* Repeated coordination boosts cooperation by eliciting trust in one’s partner (Experiment 2).

**Results**

*Experiment 1*

To test hypothesis 1, Experiment 1 probed the effects of repeated coordination upon cooperation *independently of trust.* To this end, we implemented a sequential joint decision-making task in which participants could choose whether or not to coordinate with a partner. We varied whether and to what degree the option not to coordinate constituted a temptation, and measured the frequency with which participants chose to coordinate despite this temptation (cooperation rates). In a within-subjects design, we manipulated the partner’s relationship: in one experimental block, participants played with the same partner on every trial (Fixed Partner Condition), whereas in a separate experimental block they played with different partner on each trial (Variable Partners Condition). Crucially, the choices made by their partners could not affect them negatively, and they were informed that their partners would receive no feedback about their choices. This ensured that participants’ willingness to cooperate could only be explained by altruistic motivation, not by trust or by any expectation of reciprocity.

Participants were told that they would be participating in a sequential joint decision-making task (see **Figure 1**), in which they would ostensibly be matched with partners in an online program. In reality, partners were virtual pre-programmed agents. In both conditions, participants performed an induction phase in which the cooperative option always had a higher point value than the alternative option. This was to establish a history of coordination with either the same partner or with varying partners.

A close up of a logo

Description automatically generated

**Figure 1A:** Experiment 1 Trial Structure.At the beginning of each trial, an image of the partner’s player number was displayed, which was either the same (Fixed Partner Condition) or different (Variable Partners Condition) on every trial. Then, the partner first chose one of two values. The participant did not see what these two values were but was then herself presented with two values to choose between. One of these, indicated in green or blue, was the same value that the partner had chosen (Cooperative Option); the other, indicated in orange, was an alternative value (Alternative Option). We varied whether and to what extent the alternative option constituted a temptation.

A screenshot of a cell phone

Description automatically generated A screenshot of a cell phone

Description automatically generated

**Figure 1B:** Experiment 1 Payoff Structure. Payoffs for each trial were determined as follows. If the participant chose the Cooperative Option, then each received the corresponding amount (left box above). If the participant chose the Alternative Option, then the participant received the amount corresponding to the alternative option and their partner received no reward (right box above). Participants thus were thus aware that both their and their partner’s payoffs depended on the choice they made. The amounts associated with the payoffs for participant’s cooperative and alternative options varied, over pre-specified intervals and ranges, unpredictably across trials.

Hypothesis 1 generates the prediction that cooperation rates should be higher in the Fixed Partner Condition than in the Variable Partners Condition. To test this, we employ a mixed-effects logistic regression model on our dependent variable (DV), Cooperation Rate, an indicator of participants’ trial-by-trial cooperation choice (0: alternative option; 1: cooperative option). Our two independent variables (IVs) of interest in the model are Partner Condition, a dummy equal to 1 if the subject’s trial in question was in the fixed partner block and 0 if in the variable partners block, and Temptation Level, a numerical variable ranging from -2 to 5 indicating the attractiveness of the subject’s alternative option. We include a random effect to allow the intercept to vary by subject and a by-subject random coefficient for trial number to control for a time effect.

One concern when using multiple-regression models to analyse data for confirmatory hypothesis testing is the possibility of inflated Type I error rates when specifying models with random effects – i.e. when incorrect random-effect structures are specified in the model, which do not reflect random effects present in the underlying population or fail to control for random measurement error in the data (Barr et al., 2013; Schielzeth & Forstmeier, 2009; Shear & Zumbo, 2013). Barr et al. (2013) show that this is of particular concern with data generated by experimental designs which involve within-subject manipulations and multiple observations per treatment level per unit and where fixed effects of interest are estimated and tested for significance – as in the case of our paradigm. Not including random coefficients for fixed variables in the sample risks boosting the rate of Type 1 error (i.e. false positives) by giving overconfident estimates that fail to account for weaker conditional independence between multiple within-subject observations (Schielzeth & Forstmeier, 2009).

To control for this, we include in our analysis by-subject random coefficients for both of our fixed independent variables of interest. This allows for subjects to differ in the slopes of their responses, thus accounting for the nonindependence of data points. Given the models’ failure to converge when both random coefficients are present, we follow Barr et al. (2013) in pursuing separate analyses for each. **Table 1** shows the results of two regression models, each including the by-subject random coefficient corresponding to one IV of interest. As a robustness check, we also ran a model excluding both of these random coefficients, but which did not alter the interpretation or significance of our findings and is therefore not shown here. **Figure 2** presents the data from Experiment 1, showing mean cooperation rates by Temptation Level and by Partner Condition.

Results shown in the column for Model 1 corroborate our prediction that cooperation rates were significantly higher (*b* = 0.426, *p* = .038) when participants coordinated with the same partner (Partner Condition = 0) on every trial than when coordinating with different partners on each trial (Partner Condition = 1). The odds of subjects choosing the cooperative option change by 1.53 (95% conf. int.: 1.02, 2.37) when they coordinate with a fixed partner relative to coordinating with variable partners.

As a manipulation check, we also predicted a negative main effect of Temptation Level, with participants more likely to choose the alternative option as the payoff for doing so relative to the cooperative option increased. The results confirm this prediction: Temptation Level is significantly associated with reduced levels of cooperation (*b* = -1.277, *p* = .000), all else constant. From Model 2, an increase in temptation level by one unit is associated with a 0.28 (95% conf. int.: 0.19, 0.40) change in the odds of cooperation.

The results from Experiment 1 provide evidence that repeated coordination with the same partner may stabilize cooperation by boosting altruistic motivation towards that partner, and thereby increasing resistance to tempting alternative options. Crucially, the design of Experiment 1 enabled us to rule out trust as a mediating factor between coordination and the willingness to cooperate.

**Table 1**. Analysis results from Experiment 1 using mixed-effects logistic regressions of Partner Condition and Temptation Level on subject’s cooperative choices.

|  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Dependent variable = Cooperative choice** | **Model 1** | | | | | **Model 2** | | | | |
|  |  | 95% CI for odds ratio | | |  |  | 95% CI for odds ratio | | |
| B (SE) | *p* = | Lower | Odds Ratio | Upper | B (SE) | *p* = | Lower | Odds Ratio | Upper |
|  |  |  |  |  |  |  |  |  |  |  |
| Partner Condition | 0.426 | .038 | 1.023 | 1.532 | 2.371 | 0.301 | .029 | 1.025 | 1.351 | 1.782 |
|  | (0.205) |  |  |  |  | (0.138) |  |  |  |  |
| Temptation Level | -0.898 | .000 | 0.390 | 0.407 | 0.425 | -1.277 | .000 | 0.190 | 0.279 | 0.397 |
|  | (0.022) |  |  |  |  | (0.181) |  |  |  |  |
| Constant | 1.162 | .001 | 1.594 | 3.195 | 6.546 | 0.252 | .582 | 0.510 | 1.288 | 3.255 |
|  | (0.352) |  |  |  |  | (0.460) |  |  |  |  |
|  |  |  |  |  |  |  |  |  |  |  |
| By-subject random coefficient | Partner Condition | | | | | Temptation Level | | | | |
| Observations | 8,320 | | | | | 8,320 | | | | |
| Log Likelihood | -2,813.80 | | | | | -2,394.80 | | | | |
| Akaike Inf. Crit. | 5,641.60 | | | | | 4,803.70 | | | | |
| Cooperative choice dummy equal to 1 if subject chose cooperative option and 0 if alternative option. Partner Condition dummy equal to 1 in fixed partner condition and 0 in variable partners condition. Both regressions include as covariates a by-subject random intercept and a by-subject random coefficient for trial number. | | | | | | | | | | |
|

**A picture containing text, white

Description automatically generated**

**Figure 2**. Experiment 1 data. Graphs show mean cooperation rates by partner condition for the corresponding temptation level of participants’ alternative option. Regression results (see Table 1) show that cooperation rates were significantly higher when participants coordinated with the same partner (Fixed Partner) on every trial than when coordinating with different partners (Variable Partners) on each trial. In addition, cooperation rates are significantly decreasing with increases in the level of temptation of the alternative option.

*Experiment 2*

In Experiment 2, we adapted the paradigm used in Experiment 1 to investigate whether repeated coordination also stabilizes cooperation by enhancing trust (Hypothesis 2). Whereas in Experiment 1, participants unilaterally chose whether to cooperate and thereby benefit or harm their partners, in Experiment 2 it was the partner who unilaterally chose whether to cooperate and participants, who were not informed about which choice the partner had made, had to decide whether to trust that the partner had cooperated (see Table 2).

**Table 2**. Overview of differences in how participant and partner are impacted by each other’s choices in Experiments 1 and 2.

|  |  |  |  |
| --- | --- | --- | --- |
|  | Who chooses whether to cooperate | Partner impacted by Participant’s choice | Participant impacted by Partner’s choice |
| Experiment 1 (altruism, not trust) | Participant only | yes | no |
| Experiment 2 (trust, not altruism) | Partner only | no | yes |

To this end, in Experiment 2 we introduced a new game that differed in two ways from the previous game. First, participants could choose whether or not to trust their partners, where choosing to trust would potentially gain participants a higher reward but also implied a risk of receiving a lower reward if the trust was misplaced. Second, participants’ choices had no effect on their partner’s payoffs, thereby excluding altruistic motivation as a possible mediating factor between coordination and the willingness to cooperate. Moreover, participants were informed that they would not receive any feedback about the choices made by their partner, and vice versa, until the end of the experiment. As in Experiment 1, participants were informed that they were coordinating with their partners via an online program (partners were again pre-programmed virtual agents) in a sequential joint decision-making task (see **Figure 3**).

[FIGURE 3 HERE]

To establish a presumed history of coordination with the partner(s), each experimental block began with an induction phase in which the partner’s tempting alternative option was always lower than the cooperative option. This enabled us to assess the effects of a presumed history of coordination upon participants’ trust in the test phase of each experimental block. We used an identical modelling approach in Experiment 2 as in Experiment 1, with the same DV (cooperation rate) and covariates.

**Table 3** shows the results of two regression models, each including, as in Experiment 1, the respective IV of interest’s corresponding by-subject random coefficient. Again, as a robustness check, we ran but do not present the results of a model containing neither random coefficients given no impact on our findings. **Figure 4** presents the data from Experiment 2, showing mean cooperation rates by Partner Temptation Level and by Partner Condition.

Hypothesis 2 predicts higher cooperation rates in the Fixed Partner Condition than in the Variable Partners Condition. The results of mixed-effects logistic regressions do not corroborate this prediction. In fact, from Model 1, predicted cooperation rates were significantly lower (*b* =-0.337, *p* = .000) when participants coordinated with the same partner across trials than when coordinating with different partners on every trial. The odds of choosing the cooperative option change by 0.714 (95% conf. int.: 0.589, 0.860) when subjects coordinate with a fixed partner relative to variable partners. This finding is difficult to reconcile with the hypothesis that repeated coordination boosts the willingness to cooperate by enhancing trust.

Like Experiment 1, as a manipulation check we predicted a negative main effect of Partner Temptation Level. This was because participants should be less likely to expect their partner to cooperate when their partner’s alternative option was more tempting. The results in Model 2 confirm this prediction: at higher Partner Temptation Levels, participants were significantly less likely (*b* = -2.03, *p* = .000) to trust their partner had chosen the cooperative option.

**Table 3**. Analysis results from Experiment 2 using mixed-effects logistic regressions of Partner Condition and Partner Temptation Level on subject’s cooperative choices. Regression models are identical to those used in Experiment 1.

|  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Dependent variable = Cooperative choice** | **Model 1** | | | | | **Model 2** | | | | |
|  |  | 95% CI for odds ratio | | |  |  | 95% CI for odds ratio | | |
| B (SE) | *p* = | Lower | Odds Ratio | Upper | B (SE) | *p* = | Lower | Odds Ratio | Upper |
|  |  |  |  |  |  |  |  |  |  |  |
| Partner Condition | -0.337 | .000 | 0.589 | 0.714 | 0.860 | -0.493 | .000 | 0.508 | 0.611 | 0.725 |
|  | (0.096) |  |  |  |  | (0.089) |  |  |  |  |
| Partner Temptation Level | -0.927 | .000 | 0.382 | 0.396 | 0.409 | -2.030 | .000 | 0.082 | 0.131 | 0.198 |
| (0.017) |  |  |  |  | (0.215) |  |  |  |  |
| Constant | -0.958 | .052 | 1.678 | 2.607 | 4.064 | 1.240 | .000 | 2.239 | 3.457 | 5.405 |
|  | (0.223) |  |  |  |  | (0.221) |  |  |  |  |
|  |  |  |  |  |  |  |  |  |  |  |
| By-subject random coefficient | Partner Condition | | | | | Partner Temptation Level | | | | |
| Observations | 12,339 | | | | | 12,339 | | | | |
| Log Likelihood | -4,635.10 | | | | | -3,724.10 | | | | |
| Akaike Inf. Crit. | 9284.20 | | | | | 7462.30 | | | | |
| Cooperative choice dummy equal to 1 if subject chose cooperative option and 0 if alternative option. Partner Condition dummy equal to 1 in fixed partner condition and 0 in variable partners condition. Both regressions include as covariates a by-subject random intercept and a by-subject random coefficient for trial number. | | | | | | | | | | |
|

A screenshot of a social media post

Description automatically generated

**Figure 3A.** Experiment 2 Trial Structure. At the beginning of each trial an image of the partner’s player number was displayed, which was either the same (Fixed Partner Condition) or different (Variable Partners Condition) on every trial. Next, two boxes (coloured in green and blue respectively) containing payoff options appeared on the screen, and the participant waited for a fixed duration of 4000 ms while the partner ostensibly selected one of these two boxes. Crucially, participants were led to believe that one of the options presented to the partner was a more or less tempting alternative option. Participants then chose whether to trust (i.e. to select the blue or green box) if they expected that their partner had previously chosen this same option, or to exit (i.e. to select the alternative option, in orange) if they did not trust their partner to have chosen the mutually beneficial option. Participants’ alternative options always entailed a lower reward.

*Payoffs if participant chooses alternative option*

A screenshot of a cell phone

Description automatically generated

*Payoffs if participant chooses cooperative option*

A screenshot of a cell phone

Description automatically generated

**Figure 3B**. Experiment 2 Payoff Structure. If the participant chose the alternative option (orange), then payoffs were determined as in the top half of the figure above: the participant received the guaranteed amount (300), and the partner received the amount corresponding to the option s/he had chosen: 600 if s/he had chosen the alternative option (blue) or 500 if she had chosen the cooperative option (green). Conversely, if the participant chose the cooperative option, the payoffs were determined as in the bottom half of the figure above: If the partner had chosen the cooperative option (green), then each player received the corresponding amount (500); if the partner had chosen the then both chosen the alternative option (blue), the partner received the corresponding amount and the participant received 0. Participants should thus only choose the cooperative option if they believed their partner had likewise done so. While payoffs for the cooperative options and for participants’ alternative options were fixed, we varied the amount associated with the partner’s alternative option unpredictably across trials.

A close up of text on a white background

Description automatically generated

**Figure 4**. Experiment 2 data. Graphs show mean cooperation rates by partner condition for the corresponding temptation level of partners’ alternative option. Regression results (see Table 3) show that cooperation rates were significantly lower when participants coordinated with the same partner (Fixed Partner) on every trial than when coordinating with different partners (Variable Partners) on each trial. In addition, cooperation rates are significantly decreasing with increases in the level of partners’ temptation of the alternative option.

**Discussion**

The findings from the two experiments presented here build upon previous research showing that repeated coordination over time with the same partner can increase people’s willingness to cooperate with that partner – i.e. to coordinate even when doing so is not in their own short-term interest (Guala & Mittone, 2010; Knez & Cammerer, 2000; Rusch & Luetge, 2016) – relative to their willingness to cooperate with changing partners. Our findings extend this previous research by illuminating the underlying cognitive and motivational mechanisms underpinning the effects of coordination upon the willingness to cooperate. Specifically, they provide evidence that repeated coordination with the same partner increases the willingness to cooperate by boosting altruistic motivation towards that partner, and not by increasing trust.

Our design also permits us to exclude the possibility that the effects of repeated coordination upon participants’ willingness to cooperate were driven by adherence to specific conventions arising during the experiment, as in one earlier study (Guala & Mittone, 2010). This is because the values, colours and positions of the choices varied stochastically from one trial to the next, such that the cooperative option could not take on the character of a convention.

The absence of any positive effect of repeated coordination upon trust is consistent with rational decision-making insofar as a partner’s willingness to coordinate when it is in her interest does not directly provide evidence that she would resist tempting alternatives offers. The finding that participants in fact exhibited less trust when playing with the same partner on every trial than when playing with a different partner on every trial is more surprising. We may speculate that participants may have been more risk-averse when paired with the same partner across trials than when spreading the risk across multiple partners, or that playing with a fixed partner may have led them to consider the overall amount that one single partner would need to forego over the whole experimental block if she were to resist tempting outside offers. The need to explore these possibilities provides an important avenue for further research.

Further research on the cognitive and motivational foundations of human cooperativity may also draw upon the novel experimental designs developed here. One key innovation is the technique, employed in both experiments, of seamlessly alternating between coordination problems with aligned interests and social dilemmas without having to change the task structure. Moreover, the task designed for Experiment 2 constitutes an important innovation insofar as it precisely isolates trust as a factor in decision-making, ruling out any influence of altruistic motivations or of expectations of reciprocity that may be confounded with trust in standard trust games economic trust games (Berg, Dickhaut, & McCabe, 1995).

In sum, we found that coordination’s effect on cooperation is a consequence not of trust but of altruistic motivation. These findings, together with the paradigms introduced in our two experiments, provide new insights and directions for further investigation into the cognitive and motivational underpinnings of human cooperation.

**Methods**

﻿

For both experiments, all participants were recruited through the University of Warwick SONA System, a voluntary sign-up system available to anyone interested in taking part in paid research conducted by Warwick researchers. All participants reported speaking and understanding English. Participants provided their informed written consent prior to the testing. Ethics clearance for the experiment was obtained from the University of Warwick’s Humanities and Social Sciences Research Ethics Committee (HSSREC) and all methods were performed in accordance with the relevant guidelines and regulations. Links to the pre-registrations of each experiment are provided below.

**Experiment 1**

The pre-registration for this experiment can be accessed at: <https://osf.io/fnj6r/>.

*Participants.* Using G\*Power 3.1 (Faul et al., 2009) we determined that a sample size of 52 in a within-subjects design would provide 80% statistical power for detecting a medium-sized effect (Cohen’s d = 0.4) equivalent to what we observed in a pilot study. We therefore recruited 52 participants (33 females, 18 males, 1 other; age range: 18-40, *M* = 21.9, *SD* = 4.4), who performed the experiment in group sessions of 12-16 participants. Participants were instructed that both their own and their partner’s monetary payments at the end of the experiment would depend on the points accumulated during their task, which would vary between £5 and £10. Approximately half the participants experienced one chronology of rounds and the other half a mirror version.

*Apparatus and Stimuli.* The experiment was displayed on a 24-inch wide screen (16:9) computer monitor (resolution: 1920 x 937 pixels, framerate = 60Hz.), consistent across participants and experiment sessions. The program for the experiment was written in Open Sesame (Mathôt, Schreij, & Theeuwes, 2012). The two choice options were presented as 7 cm x 5 cm rectangular fields, separated by 36 cm. The mouse start field was also 7 cm x 5 cm, and was positioned 18 cm lower at the midpoint between the two choice options. During trials, participants used the mouse to select by clicking on one of their choice options. The computer screen provided participants with real-time visual feedback on their inputs.

*Procedure.* Experiment 1 has a within-subjects, factorial design - structured according to a 2 (partner condition) x 8 (participant reward temptation condition) treatment design - with repeated measures - in which participants complete multiple trials of the same 8 trusting condition levels within each interaction partner block. Analysis is conducted within-subjects, between interaction partner block and across trusting conditions.

After participants had given their informed written consent, they were told that they would be paired with various partners during the experiment. They were informed that in addition to the £5 show-up fee, they would also be paid a bonus based on the number of points they earned during one randomly selected trial of the experiment, and that the same was true for their partner(s). Instructions were provided on the screen and participants read these on their own. They were encouraged to take as much time as they needed and had the opportunity to ask the experimenter clarification questions during the instructions phase; questions and their answers were repeated in public for all participants to hear. The task began only when all participants confirmed they had finished with the instructions and the experimenter announced that they could begin the experiment. At the beginning of the experiment participants were assigned a player number between 0 – 20. They were told that their partner(s) would see this number when interacting together and that they would, in turn, also see the partner number of their partner(s). These numbers remained consistent throughout the experiment. Participants were thus led to believe they were interacting with real partners; in reality, player numbers were pre-set and participants interacted with pre-programmed virtual partners.

At the beginning of each trial, an image of the partner’s player number was displayed for 4000ms, which was either the same (Fixed Partner Condition) or different (Variable Partners Condition) on each trial. Then, the partner first chose one of two values. The participant did not see what these two values were, but was then herself presented with two values to choose between. One of these, indicated in green or blue, was the same value that the partner had chosen (Cooperative Option); the other, indicated in orange, was an alternative value (Alternative Option). The values for the cooperative options ranged from 200-500 points. The value of the alternative option – the Temptation Level – ranged unpredictably, over intervals of 50, from 100 points less to 250 points more than the value for the cooperative option. Thus, the lowest value for the alternative option was 100 and the highest 750. If the participant chose the cooperative option, the participant and the partner would each receive the amount of points corresponding to the selected value. If the participant chose the alternative option, she received that many points, while her partner received 0. The alternative option was therefore only tempting when its value was greater than that of the cooperative option, and the degree of temptation which it presented was a function of the value of the alternative option. Each trial ended when the participant clicked on one of the two options or when 2999 milliseconds had elapsed. After the participant had made her or his selection, there was a 500 ms delay, and then the payoffs were displayed for 3000ms. The payoff display showed the following: the participant’s choice; their partner’s ostensible choice; and the payoffs for each player.

The experiment lasted approximately 40 minutes, during which participants performed two experimental blocks, one in each condition, in counterbalanced order. In each experimental block, participants first underwent an induction phase consisting of 10 trials: 4 trials for which the alternative option was 100 less than the value for the cooperative options; 4 for which it was 50 less; and 2 for which it was the same. There was no perceptible gap between the induction and test phases. In the test phase of each experimental block, there were 80 test trials, giving a total 160 trials across the two blocks. For each of the 8 different levels of temptation there were 20 trials, drawn in random order. The position of the two options (cooperative and alternative) varied equally between the left- and right-hand sides of the screen, and the colour of the cooperative option varied equally between blue and green.

Participants were told that their partner would receive no feedback on the choices the participant made until one trial was selected for payment at the end of the experiment. Payment was automatically calculated and participants were paid at the end of the experiment when all participants had finished.

**Experiment 2**

The pre-registration for this experiment can be accessed at: <https://osf.io/kepj8>.

*Participants.* Using G\*Power 3.1 (Faul et al., 2009) we determined that for an increased statistical power of 95% a sample size of 88 would detect a medium-sized effect (Cohen’s d = 0.4) equivalent to what we observed in our previous study. Sessions were structured to include between 16-20 participants and our stopping rule was such that we included data from all participants up to and including those participating in the final session in which we crossed the participant threshold. We therefore recruited 97 participants (53 females, 44 males; age range: 18-35, *M* = 20.8, *SD* = 2.4), seated in the same computer lab under identical conditions as Experiment 1. As a pre-condition for participation, those who participated in Experiment 1 were unable to participate in Experiment 2. Participants were instructed that both their own and their partner’s monetary payments at the end of the experiment would depend on the points accumulated during their task, which would vary between £5 and £10 and take maximum 1 hour. Approximately half the participants experienced one chronology of rounds and the other half a mirror version.

*Apparatus and Stimuli.* The experiment was displayed on the same computer (size, resolution, keyboard and mouse input) as in Experiment 1. The program for the experiment is written in Javascript using the jsPsych toolbox (de Leeuw, J. R. (2015)). Boxes are presented in a horizontal line on the screen, with partners’ choices separated from participants’ by a dark vertical line, while choice buttons are shown on a line below the boxes. During trials, participants used the mouse to select by clicking on one of their choice options. The computer screen provided participants with real-time visual feedback on their inputs.

*Procedure.* Experiment 2 has a within-subjects, factorial design - structured according to a 2 (partner condition) x 8 (partner reward temptation condition) treatment design - with repeated measures - in which participants complete multiple trials of the same 8 trusting condition levels within each interaction partner block. Analysis is conducted within-subjects, between interaction partner block and across trusting conditions.

Experiment 2 was designed visually and procedurally to be as similar as possible to Experiment 1. Again, after participants had given their informed written consent, they were told that they would be paired with various partners during the experiment. They were informed that in addition to the £5 show-up fee, they would also be paid a bonus based on the number of points they earned during one randomly selected trial of the experiment, and that the same was true for their partner(s). Instructions were provided on the screen and participants read these on their own. They were encouraged to take as much time as they needed and had the opportunity to ask the experimenter clarification questions during the instructions phase; questions and their answers were repeated in public for all participants to hear. The task began only when all participants confirmed they had finished with the instructions and the experimenter announced that they could begin the experiment. At the beginning of the experiment participants were assigned a player number between 0 – 20. They were told that their partner(s) would see this number when interacting together and that they would, in turn, also see the partner number of their partner(s). These numbers remained consistent throughout the experiment. As in Experiment 1, participants were thus led to believe they were interacting with real partners; in reality, player numbers were pre-set and participants interacted with pre-programmed virtual partners.

At the beginning of each trial, an image of the partner’s player number was displayed for 4000ms, which was either the same (Fixed Partner Condition) or different (Variable Partners Condition) on each trial. Next, two boxes (colours green and blue) containing payoff options appeared horizontally on the screen (purportedly visible to both participant and partner), one box positioned in the middle of the screen (cooperative option) and the other against either the right or left hand side of the screen (partner alternative option). Each box contained two numbers representing points: a value of top for the partner’s points and a value on the bottom for the participant’s points. The box in the middle always contained a value of 500 for both partner and participant. The partner-alternative box always contained some positive value for the partner value ‘A’ and a value of zero for the participant. Value ‘A’ ranged, unpredictably, over intervals of 50, from the lowest of 400 points to the highest of 750 points. Thus the differential between the partner’s value in the middle box (500) and the side-aligned box (value A) ranged from negative 100 to positive 250. Participants waited while their partner supposedly selected between one of these two green and blue boxes. The duration the partner had to choose was always 4000 ms, which matched the time given to the participant when making their own choice later.

Then, an additional box of a different colour (orange) appeared on the empty side of the screen (participant alternative option). The partner’s value in this box was unknown while the participant’s value was either 200 or 400. Thus, participant’s alternative option value was always lower than the central cooperative option value. Two buttons simultaneously also appeared below the boxes, one to the left and one to the right-hand side of the screen. One contained the text “Enter” and the other “Exit”; participants clicked the former if they wanted to select the cooperative option and the latter if they wanted to select the guaranteed alternative option. Participants thus used these buttons to make their choices, based on how they believed their partner had previously behaved, with payoffs for each trial determined as follows.

If both participant and partner chose their alternative options, each received the corresponding amount for certain. Conversely, if both chose the cooperative option, each received the corresponding amount. However, crucially, if the participant chose the cooperative option and the partner did not, then the partner received the amount corresponding to their tempting alternative option, while the participant received nothing. If the partner chose the cooperative option but the participant did not, the partner would nevertheless receive the amount corresponding to the cooperative option, and the participant would receive the (lower) guaranteed amount.

Participants therefore have the option of joining their partner (cooperative option) if they trust that their partner has previously chosen this option; or they can exit (alternative option) if they do not trust their partner has chosen the mutually beneficial option, though their exit option systematically has a lower reward. Participants would thus only choose the cooperative option if they believed that their partner had likewise done so. Their partner’s rewards were also thus unaffected by the participant’s decisions. While the payoffs for the cooperative options and for participants’ alternative options were fixed (as described above), we varied the amount associated with partner’s alternative option unpredictably across trials. This allowed us to measure participants’ trust in their partner at varying levels of reward temptation for their partner; e.g. in some trials, the partner faced a high temptation to not cooperate while in other trials the reward for cooperation and non-cooperation were identical. Each trial ended when the participant clicked on one of the buttons to make a choice, or when 3999 ms has elapsed (i.e. they automatically progressed to the next trial, receiving a bonus of zero for the missed attempt). After the participant makes their selection, there is a 500 ms delay, and then possible payoffs are displayed for 3000ms. The reward display showed the following: the participant’s choice; their partner’s ostensible choice; and the rewards for each player. The participant’s reward was equal to the guaranteed reward if the participant chose the alternative option, or both of the possible rewards if the participant chose the cooperative option.

The experiment lasted approximately 40 minutes, during which participants performed two experimental blocks, one in each condition, in counterbalanced order. In each experimental block, participants first underwent an induction phase consisting of 10 trials: 4 trials for which the alternative option was 100 less than the value for the cooperative options; 4 for which it was 50 less; and 2 for which it was the same. There was no perceptible gap between the induction and test phases. In the test phase of each experimental block, there were 64 test trials, giving a total 124 trials across the two blocks. Within each block, for each of the 8 different levels of temptation there were 8 trials, drawn in random order. The position of the two options (cooperative and alternative) varied equally between the left- and right-hand sides of the screen, and the colour of the cooperative option varied equally between blue and green.

Participants were told that their partner would receive no feedback on the choices the participant made until one trial was selected for payment at the end of the experiment. Payment was automatically calculated and participants were paid at the end of the experiment when all participants had finished.

**References**

Andreoni, J. (1990). Impure altruism and donations to public goods: A theory of warm-glow giving. *The Economic Journal*, *100*(401), 464-477.

Andreoni, J., & Bernheim, B. D. (2009). Social image and the 50–50 norm: A theoretical and

experimental analysis of audience effects. *Econometrica, 77*(5), 1607-1636.

Barr, D. J., Levy, R., Scheepers, C. & Tily, H. J. (2013) Random effects structure for confirmatory hypothesis testing: keep it maximal. *Journal of Memory and Language 68*, 255 – 278.

Barragan, R. C., & Dweck, C. S. (2014). Rethinking natural altruism: Simple reciprocal interactions trigger children’s benevolence. *Proceedings of the National Academy of Sciences*, *111*(48), 17071–17074.

Berg, J., Dickhaut, J., & McCabe, K. (1995). Trust, reciprocity, and social history. *Games and economic behavior*, *10*(1), 122-142.

Boyd, R., & Richerson, P. J. (1992). Punishment allows the evolution of cooperation (or anything else) in sizable groups. *Ethology & Sociobiology, 13,* 171–195. http://dx.doi.org/10.1016/0162- 3095(92)90032-Y

Cirelli, L. K., Einarson, K. M., & Trainor, L. J. (2014). Interpersonal synchrony increases prosocial behavior in infants. *Developmental Science*, *17*(6), 1003–1011.

Dana, J., Weber, R. A., & Kuang, J. X. (2007). Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness. *Economic Theory*, *33*(1), 67-80.

Faul, F., Erdfelder, E., Buchner, A., & Lang, A. G. (2009). Statistical power analyses using G\* Power 3.1: Tests for correlation and regression analyses. *Behavior research methods*, *41*(4), 1149-1160.

Fehr, E., & Fischbacher, U. (2003). The nature of human altruism. *Nature*, 425 (6960), 785–791.

Fehr, E., & Gächter, S. (2002). Altruistic punishment in humans. *Nature, 415* (6868), 137-140.

Fehr, E., & Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *The Quarterly Journal of Economics*, *114*(3), 817-868.

Guala, F & Mittone, L (2010). How history and convention *create* norms, *Journal of Economic Psychology*, 31 (4): 749-756.

Hamann, K., Warneken, F., & Tomasello, M. (2012). Children’s developing commitments to joint goals. *Child Development*, *83*(1), 137–145.

Hamilton, W. D. (1963). The evolution of altruistic behavior. *The American Naturalist*, *97*(896), 354-356.

Heintz, C., Celse, J., Giardini, F., & Data, S. M. (2015). Facing expectations: Those that we prefer to fulfil and those that we disregard. *Judgment and Decision Making*, *10*(5), 442.

Henrich, J., & Henrich, N. (2007). *Why humans cooperate: A cultural and evolutionary explana- tion*. Oxford, UK: Oxford University Press.

Knez, M., Camerer, C. (2000) Increasing cooperation in Prisoner’s Dilemmas by establishing a precedent of efficiency in coordination games. *Organisational Behaviour and Human Decision Processes, Vol. 82, Issue 2*, 194-216.

Kokal, I., Engel, A., Kirschner, S., & Keysers, C. (2011). Synchronized drumming enhances activity in the caudate and facilitates prosocial commitment-if the rhythm comes easily. *PLoS One*, *6*(11), e27272.

Mathôt, S., Schreij, D., & Theeuwes, J. (2012). OpenSesame: An open-source, graphical experiment builder for the social sciences. Behavior Research Methods, 44(2), 314-324. [doi:10.3758/s13428-011-0168-7](http://dx.doi.org/10.3758/s13428-011-0168-7)

McGrath, M. C., & Gerber, A. S. (2019). Experimental evidence for a pure collaboration effect. *Nature human behaviour*, *3*(4), 354-360.

Michael, J. & Pacherie, E. (2015) On commitments and other uncertainty reduction tools in joint action. *Journal of Social Ontology, 1(1)*, 89-120.

Michael, J., Sebanz, N., & Knoblich, G. (2016). The sense of commitment: a minimal approach. *Frontiers in Psychology*, *6*, 1968. doi: 10.3389/fpsyg.2015.01968

Nowak, M. A. (2012). Evolving cooperation. *Journal of Theoretical Biology, 299,* 1– 8. http://dx.doi.org/ 10.1016/j.jtbi.2012.01.014

Nowak, M. A., & Sigmund, K. (2005). Evolution of indirect reciprocity. *Nature: Reviews Vol. 437*,1– 8.

Rege, M., & Telle, K. (2004). The impact of social approval and framing on cooperation in public good situations. *Journal of public Economics, 88*(7), 1625-1644.

Roberts, G. (1998). Competitive altruism: from reciprocity to the handicap principle. *Proceedings of the Royal Society of London. Series B: Biological Sciences, Vol. 265, Issue 1394*, 427-431.

---(2005). Cooperation through interdependence. *Animal Behaviour*, *70*(4), 901-908.

Rusch, H. & Luetge, C. (2016). Spillovers from coordination to cooperation: Evidence for the interdependence hypothesis? *Evolutionary Behavioral Sciences,* 10(4): 284

﻿Schielzeth, H., & Forstmeier, W. (2009). Conclusions beyond support: Overconfident estimates in mixed models. *Behavioral Ecology, 20*, 416–420.

Shear, B. R. & Zumbo, B. D. (2013) False positives in multiple regression: unanticipated consequences of measurement error in the predictor variables. *Educational and Psychological Measurement 73(5)*, 733-756.

Skyrms, B. (2004). *The stag hunt and the evolution of social structure*. Cambridge, UK: Cambridge University Press.

Smith, J. M. (1964). Group selection and kin selection. *Nature*, *201*(4924), 1145-1147.

Tomasello, M. (2009). *Why we cooperate*. Cambridge, MA: MIT Press.

Trivers, R. L. (1971). The evolution of reciprocal altruism. *The Quarterly review of biology*, *46*(1), 35-57.

Valdesolo, P., & DeSteno, D. (2011). Synchrony and the social tuning of compassion. *Emotion*, *11*(2), 262.

Van Baaren, R. B., Holland, R. W., Kawakami, K., & Van Knippenberg, A. (2004). Mimicry and prosocial behavior. *Psychological Science*, *15*(1), 71–74.

West, S. A., Griffin, A. S., & Gardner, A. (2007). Evolutionary explanations for cooperation. *Current Biology, 17,* R661–R672. http://dx.doi.org/10 .1016/j.cub.2007.06.004

Wiltermuth, S. S. & Heath, C. (2009) Synchrony and cooperation. *Psychological Science Vol. 20 (1)*, 1 -5.