# Sensorimotor communication fosters trust and generosity: The role of effort and signal utility

Luke McEllin[1,2], John Michael [2]

[1]University of Warwick, Department of Psychology, Coventry CV4 7AL UK

[2]Central European University, Department of Cognitive Science, Oktober 6 utca 7, 1051 Hungary

* Luke McEllin

**Email:** lukemcellin1990@gmail.com

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# Abstract

Previous research has shown that a co-actor’s willingness to bear the monetary costs of prior cooperative activities influences our cooperative behaviour towards them. But what about when such information is lacking? In addition to monetary costs, people routinely engage in joint actions in which they incur effort costs in order to help each other achieve their goals, for example by adapting their goal-directed actions in order to send informative signals. We aimed to investigate whether people act more cooperatively towards those who are willing to bear the effort costs of an interaction by adapting their movements to send informative signals. We find that the effort that a co-actor invests in order to produce informative movement adaptations increases a participant's trust towards that co-actor, and that both the effort and utility of these informative movement adaptations increase generosity toward that co-actor. This demonstrates that we may base decisions about cooperation with a person on their willingness to bear the effort costs of interaction. These findings are discussed with respect to the role that effort investment plays in sustained cooperation.

**Keywords**: Cooperation, Coordination, Joint Action, Effort, Signaling

# Introduction

Humans are remarkable in their ability to cooperate and their willingness to do so (Nowak & Sigmund 2005; Warneken & Tomasello, 2009). For both small- and large-scale societies, effective cooperation is pivotal for the success of the group and the well-being of its members. Within everyday social interactions, we therefore need to make prudent decisions in order to ensure that we maintain stable relationships with valued co-actors and also avoid being exploited by less cooperative co-actors.

There has been much research investigating the cues that inform these decisions in everyday social interactions. Although trait-related cues, such as facial features (Bonnefon, Hopfensitz & De Neys, 2017), voice (O'Connor & Barclay, 2017), group membership (Everett, Faber & Crockett, 2015) or moral and political ideologies (Everett, Pizarro, Crockett, 2016; Tognetti, Durand, Barkat-Defradas & Hopfensitz, 2020) may influence cooperation, many of the decisions we make in social interactions are driven by our experience with our co-actor as an interaction partner. Specifically, we act more cooperatively towards those who themselves exhibit a willingness to bear the costs of cooperation (Baumard, Andre & Sperber, 2013). We are more cooperative towards those who bear the costs of generosity for the social good (Hardy & Van Vugt, 2006; Barclay & Willer, 2007; Gintis, Smith & Bowles, 2001); those who bear the costs of norm enforcement by punishing transgressors (Barclay & Raihani, 2016; Kurzban, DeScioli & O'Brian, 2007), and those who are willing to share the risks and rewards of cooperation (Nowak & Sigmund, 1993; Hoffman, Yoeli & Nowak, 2015). Importantly, the fact that actors are more trusting and generous towards those who intend to cooperate regardless of the material outcome of such actions highlights the importance of the prosocial attitudes driving cooperation (Eisenbruch & Roney, 2017; Raihani & Barclay, 2016).

With regard to the research around human cooperation, there has been a significant focus on *economic transactions* - interactions in which monetary resources are directly exchanged. Yet many of our day-to-day cooperative interactions do not involve the exchange of monetary costs at all. Rather, from carrying a table together to playing in a band, a great many of our everyday cooperative interactions can be defined as *joint actions* - interactions that require us to coordinate our actions in space and time with no direct monetary implications (Sebanz, Bekkering & Knoblich, 2006). Such joint actions do however require us to incur effort costs, as we adapt our actions both to benefit our partner and to ensure success of the interaction (Szekely & Michael, 2018). For example, we may modulate the timing of our movement in order to help accommodate a partner's actions or plan our actions in such a way as to make our partner's task easier (Konvalinka, Vuust, Roepstorff & Frith, 2010; Török, Pomiechowska, Csibra, & Sebanz, 2019).

One of the most striking instances of incurring effort costs in a joint action is *sensorimotor communication*. Here, an actor effortfully adapts kinematic parameters of their movements in order to provide a co-actor with a task-relevant informative signal, thus enabling the co-actor to more easily disambiguate the action and predict its outcome (Pezzulo, Donnarumma & Dindo, 2013). For example, an actor may exaggerate spatial parameters of their movements in order to help their co-actor predict the end location of their movements or modulate the size of their grip to transmit information about the size of the object that they are about to grasp (Vesper & Richardson, 2014; Sacheli et al. 2013). Sending such informative signals requires an actor to bear physical effort costs, as they involve a deviation from the most optimal way of executing an action, as well as cognitive effort costs, as they require an actor to be mindful of their co-actor’s perspective, in order to tailor these signals to the specific knowledge state of their co-actor (Pezzulo et al. 2013; McEllin, Knoblich & Sebanz, 2018).

Just as generosity, costly punishment or trust require an actor to be willing to bear monetary costs to support cooperation (Baumard et al., 2013), sensorimotor communication requires an actor to bear effort costs to support coordination. This raises the following question: *Are we more trusting and/or more generous towards those who are willing to bear the effort costs of cooperation just as we are more trusting and/or more generous towards those who are willing to bear the monetary costs of cooperation?* This is an important question insofar as it may shed light on the extent to which trust and generosity, both in our evolutionary past and today, may have been supported by evidence gleaned from these everyday joint actions rather than from explicitly economic interactions (Ibbotson, Hauert & Walker, 2019). Moreover, demonstrating such indirect benefits associated with everyday joint actions may help us understand our strong penchant for cooperative interactions that do not lead to any explicit monetary gain.

To address this question, this study aimed to investigate the extent to which effort costs in the form of sensorimotor communication incurred by a co-actor in a joint action may influence trust and generosity towards that co-actor. The rationale for focusing on generosity and trust is that they are two distinct but complementary prosocial attitudes that play a role in sustaining cooperation: cooperation requires actors to be generous (to be willing to incur costs to sustain cooperation) and also to trust that their co-actor will also be generous. In Experiment 1, we utilised the dictator game to probe the effects of sensorimotor communication upon generosity; in Experiment 2 and 3, we utilised the trust game to probe the effects of sensorimotor communication upon trust. Finally in Experiment 4, we aimed to investigate the role that the co-actor's intentions play in the effect of sensorimotor communication upon trust.

In the first three experiments, participants first completed a coordination task (see figure 1 for schematic) with a partner who either sent useful signals, redundant signals, or no signals; next, we measured participants' generosity (Experiment 1) and trust (Experiment 2) towards that partner.

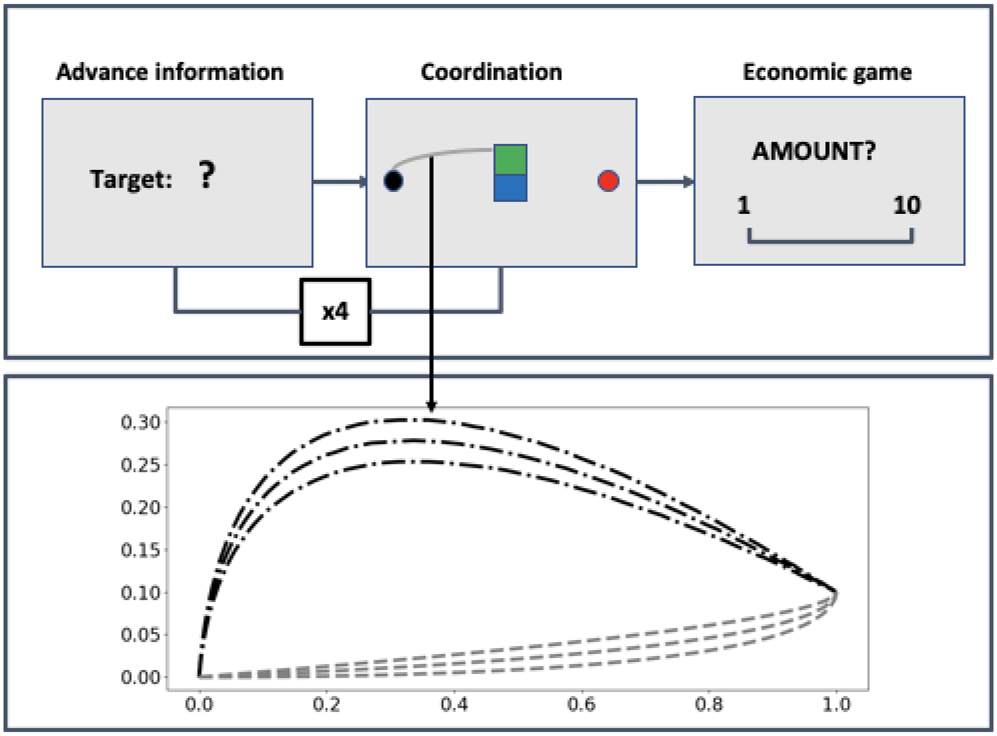


*Figure 1: Schematic overview of the study. The top panel illustrates the procedure, including the coordination task and the economic game. The top left frame represents the advance information phase. The top middle frame represents the coordination phase. Here, the black dot is the dragger's (virtual partner) marker, the red dot is the presser's (participant) marker, and the coloured boxes are the two targets. The top right frame represents the donation/investment*

*phase. The bottom panel illustrates the base trajectories of partners' movements on individual trials of the coordination task (normalized from 0-1). Black lines show base trajectories from signal trials (both useful and redundant); grey lines show base trajectories from no-signal trials.*

Our coordination task required participants and their partners to move their respective markers that were located at opposite sides of the screen (e.g., the participant's marker was on the centre-left and their partner's marker was on the centre-right) to one of two targets stacked horizontally in the centre of the screen Both participants and partners would get points (which participants believed translated to a bonus at the end of the experiment) depending on how quickly they reached the correct target. The participant was instructed to respond to the correct target with a button press, whilst their partner would respond by dragging their marker to the correct location with their mouse, with participants being informed that their partner would always know the correct location of the target. Participants completed this task with a number of virtual partners, who were programmed to appear as if they were picking up the target with their mouse and dragging it to the correct target, with the kinematic trajectory of the partner's movement depending on the trial type. For *no-signal trials*, the partner moved in such a manner that their kinematics did not carry a signal with respect to the location of the correct target. For *useful-signal* trials the partner's movements displayed a strong curve towards the correct target (e.g., Pezzulo & Dindo, 2011), thus allowing the participant to quickly disambiguate the target that they were moving to. In Experiments 1-3, we also had *redundant-signal* trials in which the partner's movements were the same as in the useful-signal trials, but with the participant being informed about the location of the correct target (participants were led to believe that their partners did not know they were informed). This condition allowed us to probe whether in our task sensorimotor communication facilitated trust and generosity because of the utility of the signal itself or because the signal reflected a willingness to incur effort costs for the good of the interaction (cf. a preference for those who incur costs regardless of the outcome in an economic game, Eisenbruch & Roney, 2017; Raihani & Barclay, 2016). In Experiment 4 we compared the difference in trust between no-signal trials and useful-signal trials in an Intentional-signal condition where the useful signals were sent intentionally, with the difference in trust between no-signal trials and useful-signal trials in an Unintentional-signal condition where the useful signals were sent unintentionally, as a byproduct of fulfilling an instrumental goal.

## Experiment 1

Generosity helps ensure a fair distribution of resources between individuals and is driven by strong norms for fairness, both in general and with respect to what the recipient is perceived to deserve (Rand & Nowak, 2013; Bolton, Katok & Zwick, 1998). This is important insofar it ensures that members of a group do not free-ride and are compensated adequately for their investments into an interaction (Bowles & Gintis, 2002). How do we decide who is more or less deserving on the basis of the effort costs they invest into a coordinated interaction? Do we decide to reward those whose efforts prove to be useful for our task, or do we simply reward the effort, regardless of the outcome? Experiment 1 used a Dictator game in order to examine the effects of a co-actor’s effortful sensorimotor communication upon an actor’s generosity towards them. We predicted that if participants only rewarded efforts that yielded useful outcomes for them, they would be more generous (i.e., donate more points) only to those who sent useful signals. In contrast, if participants rewarded the effort itself rather than the outcome, then they would be more generous both to those who sent useful signals and to those who sent redundant signals.

## Experiment 1: Method

The hypotheses, sample sizes, methods, and initial analyses were all pre-registered before data collection. The pre-registration can be accessed at: https://aspredicted.org/blind.php?x=xq3xb2

### *Participants*

Because comparisons between all three levels of our design was necessary for discriminating between our hypotheses, we carried out a G*Power analysis for 95% power with a t-test with an alpha level of .017 (Bonferroni corrected for three comparisons) with an effect size of .51 (Cohen's *d* based on an effect size from piloting, see SM1 for pilot), which determined we collect 66 participants. We also decided to add 10% to this value due to the likelihood that an online study would result in noisy data. This resulted in a final sample size of 72 participants (43 women, 29 men) with a mean age of 35.41 (SD = 13.51) who we recruited using the Prolific recruitment platform (www.prolific.co), were required to be fluent English speakers, and were paid 2 GBP for their time, plus a 1 GBP bonus payment. All participants gave informed consent and were fully debriefed at the end of the study. The experiment was conducted in accordance with the Declaration of Helsinki and was approved by the Humanities & Social Sciences Research Ethics Sub-committee (HSSREC) at the University of Warwick (approval number: 01/16-17), as part of the ERC-funded project ‘[679092: Sense of Commitment]’.

### *Apparatus and stimuli*

All experimental stimuli and instructions were presented in JavaScript using PsychoJS (all stimuli were defined relative to the height and width of the participant's screen) and were hosted on Pavlovia.com.

Virtual waiting room: In order to create the impression that participants would be playing a multiplayer game with other participants, we had them wait in a 'virtual waiting' room before the start of the experiment. They were informed that they were the eighth person out of thirteen to sign up and that they would have to wait up to two minutes whilst other participants joined. We animated dots (height = 0.1) which moved in the same manner as a typical loading screen, and at random intervals updated the screen to say that another person had joined the experiment (e.g., the display went from 8/13 participants signed up to 9/13 participants signed up; height = 0.1). This went on for around 60 seconds, until the display informed participants that all thirteen participants had signed up.

Advance information: In rounds in which participants would receive advance information about the target, the colour of the correct target was displayed with a box (height = 0.2, width = 0.2) in the centre of the screen. In rounds where the participant would not receive advance information about the target, they were presented with a question mark (height = 0.1) in the middle of the screen (see top left of figure 1).

Coordination task: Our coordination task required participants to decide to move a cursor at the edge of the screen (either far left on the x-axis and center on the y-axis or far right on the x-axis and center on the y-axis) to one of two targets in the middle of the screen. The two targets were presented stacked vertically in the centre of the screen. One of the targets was coloured green and the other was coloured blue. Importantly, the colour of the top and bottom boxes were fixed for all trials (but randomized across participants) in order to keep the informativeness of the partner’s signal in the redundant-signal condition to a minimum. This was because any ambiguity with respect to the location of the correct target within this condition (e.g., if participants did not know whether the blue target was the top or bottom box) would render the signal in the redundant-signal condition useful (therefore not redundant) as it could help resolve this ambiguity (i.e., the participant could still gain task-relevant information by following the partner’s movements). Each of the actors (i.e., the presser and the dragger) were represented by a marker (diameter = 0.05), with one positioned at the far left on the x-axis and center on the y-axis of the screen and another positioned at far right on the x-axis and center on the y-axis of the screen (whether the participant was on the left of right was fixed across trials but randomized across participants). The presser selected either the top or bottom target by pressing z or m on the keyboard (this was fixed across trials but randomized across participants) at which point their marker would move in a straight line to the selected target. The dragger was programmed to appear to select one of the targets by using their mouse to drag their cursor to one of the targets using either an informative or uninformative trajectory (see below for a detailed description). See the top middle of figure 1. Also see SM4 for example videos.

Movement trajectories: Non-signalling movements were made minimally informative by programming the trajectory (using a Bézier curve function, see OSF link for raw trajectories and SM4 for example videos) to move horizontally along one axis until curving towards the correct target in the later stages of the movement (see grey trajectories in bottom panel of figure 1), meaning that the participant could not use the trajectory to anticipate the correct target until later on in the movement. This represented an actor moving towards the target in such a manner that was inconsiderate to the fact that the presser is relying on their movements to select the correct target, as they would in a non-social task (assuming that humans have a strong preference for straight rather than diagonal mouse movements, Slijper et al. 2009). Signalling movements were made informative by programming the trajectory (using a Bézier curve function, see SM2 for raw trajectory) in such a way that it curved towards the correct target clearly and at the initial stage of the movement (see black trajectories in bottom panel of figure 1) allowing the participant to use the trajectory to anticipate the correct target quickly, at an early stage of the movement. This represented an actor engaging in sensorimotor communication -- i.e., modulating the kinematics of their movement in order to send an informative signal that allowed the participant to quickly and effectively disambiguate the correct target based on their movements. See SM3 for a manipulation check demonstrating that signalling trajectories are rated as more effortful than non-signalling movements. For both movement types, we programmed three 'base' trajectories, which differed slightly (see OSF for raw data and SM4 for example videos) and were randomly presented to participants.

In this study we decided to use artificial kinematics rather than real kinematic recordings in order to have complete control over the kinematic parameters in our stimuli, both in order to precisely modulate our kinematic parameters, and also to ensure that participants were not exposed to any unwanted movement features associated with real movement (McEllin, Knoblich & Sebanz, 2020). Although it is well established that artificially generated motion (even if it does not have properties of biological motion) is processed similarly to biological motion (Johansson, 1973; Kilner, Hamilton & Blakemore, 2007), we took additional steps to make our stimuli more realistic, thus minimizing the risk that participants believed they were interacting with a virtual partner. We added noise to the dragger's movement onset time (300-500ms) and to the x-axis of the trajectory, i.e., the end position of their movements (height = 0-0.05, width = 0-0.1 from the centre of the target), also ensuring that after adding noise, our trajectories never curved towards the incorrect option, as participants may have attributed a deceptive intention towards their co-actor (i.e., signalling the incorrect trajectory to deceive the participant). Moreover, there was also noise on the y-axis of the movement, with respect to time and magnitude of the trajectory's maximum curvature. We also programmed all trajectories to have a minimally jerky, bell-shaped velocity profile (by resampling the data on the basis of a bell-shaped distribution; see OSF link for raw trajectories and SM4 for example videos), a common property of biological motion (Flash & Hogan, 1985).

Dictator game: Participants were presented with a scale from 1-10 (height=0.1, width=0.4). The trajectory was clickable, meaning participants could click on the number that represented the number of points they wanted to donate. See the top right of figure 1 for an illustration.

### *Procedure*

Participants who signed up for our study were first fully briefed and instructed before being asked a battery of catch questions to check their understanding of the task (participants who answered more than one question incorrectly were informed that they could not continue with the study). After passing the catch questions, participants were informed that they would be participating with twelve other participants and placed in a virtual waiting room (they were informed that they were the eighth person to enter the room). They were informed that they were waiting for more participants to sign up to the study and work through the instructions. This was done to lead participants to believe that they were interacting with other participants in real time. After waiting for around one minute, they were informed that a sufficient number of participants had signed up and that they would now be randomly assigned to either the role of ‘dragger’ or of ‘presser’ for the duration of the experiment - the participant was always ‘randomly assigned’ to the role of presser, whilst the other twelve participants were assigned to the role of dragger. Participants were then informed that they would be participating in a task in which they were first required to coordinate several times on a task with a partner, before playing an investment game with that same partner. They were informed that they would complete twelve rounds in total, one with each partner, and for each of these rounds playing the role of ‘presser’ whilst their participant played the role of ‘dragger’. Each participant was also assigned a player number.

Each round was structured as follows. Firstly, participants were randomly paired with one of the other players and informed that they may have to wait until the other player was ready to begin, for example if they needed to finish up with their previous partner (randomly between 30-45 seconds). This was done in order to lead participants to believe that their partners were in fact interacting with others throughout the experiment. Participants would then start the coordination phase. There was first a knowledge assignment, in which they were either given no information about the upcoming target (this was the case for no-signal and useful-signal rounds) or were informed about the colour of the upcoming target (this was the case for the redundant-signal rounds). Importantly, they were informed that the ‘dragger’ always believed that the ‘presser’ would never get any information about the upcoming target throughout the experiment, ensuring that participants believed that from the partner’s perspective the intention to send a signal was always rational, as they believed they were sending useful information. Participants would then start the coordination task with their partner, in which they would both get points depending on how quickly they reached the correct target, getting no points if they reached the incorrect target. Participants believed that, as the ‘dragger’, their partner had been informed about the colour of the correct target and was required to click on their marker and drag it to the correct target with their mouse, whilst they as the ‘presser’ would select the correct targets with their keyboard by pressing z to select the top target or m to select the bottom target (this was randomized across participants). Importantly, the dragger would move with either a signalling trajectory (in the useful-signal and redundant-signal conditions) or a non-signalling trajectory (in the no signal condition; see apparatus and stimuli for specific information about the partners’ behaviour). Once participants had made their choice, they were given feedback with respect to whether they selected the correct or incorrect target. Participants would complete four coordination trials per round (i.e., four times knowledge assignment-coordination task).

Finally, we measured the participants’ generosity towards their partner with a donation game, which was a standard dictator game (Forsythe, Horowitz, Savin & Sefton, 1994). Participants were informed that they would play a donation game in which they would receive an endowment of 10 points which they could distribute between themselves and their partner as they please.

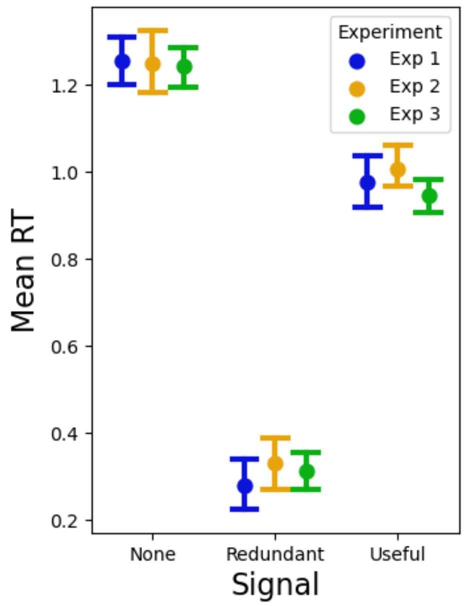
With respect to their performance, participants were informed that their bonus payment of 1GBP was dependent on how quickly and accurately they responded in the coordination task, and how many points in total they kept during each of the donation games (thus they were aware that making a donation to their partner would be costly for themselves with respect to their bonus).

### *Design*

Our study consisted of Signal Type (no-signal, redundant-signal, and useful-signal) as a within-subjects factor, and points donated as a dependent variable.

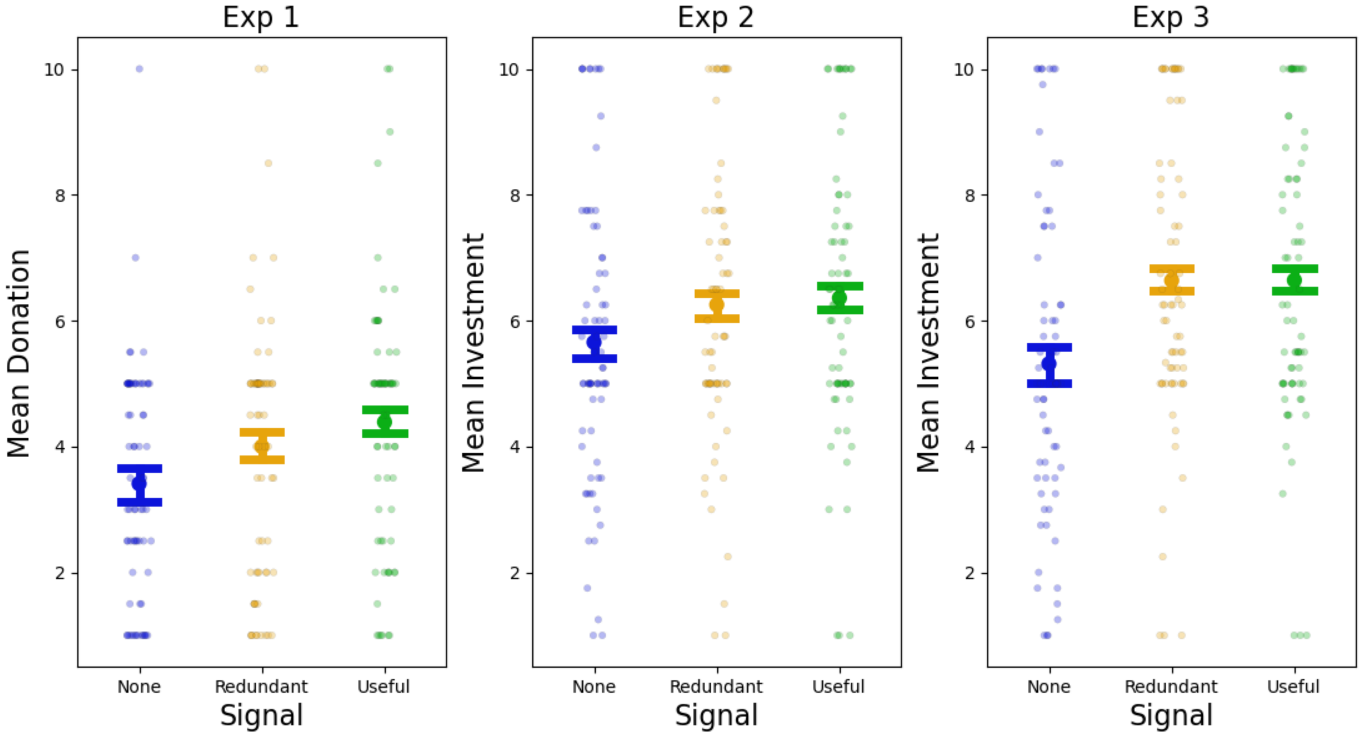
## Experiment 1: Results

As a manipulation check, we first compared participant's mean reaction times between the three conditions in order to verify that the partner's signal (at least in the useful-signal condition) disambiguated the correct target. We first removed any coordination trials with RTs longer than ten seconds from the analysis; this was because participants taking longer than ten seconds on a trial are highly likely to have been inattentive on that trial (this was not pre-registered, as we had not anticipated this issue). This applied to all studies in the manuscript. A one way ANOVA (see left panel of graph 1) revealed a significant main effect of signal type, $F(2,142) = 246.43$, $p < .001$, $\eta p2 = .78$. As expected, post hoc t-tests (Bonferroni corrected for three comparisons) showed that participants were faster in redundant-signal trials, compared to useful-signal trials $t(71) = -20.36$, $p < .001$, $d = -2.4$, and no-signal trials, $t(71) = -14.54$, $p < .001$, $\eta p2 = -1.71$. Participants were faster in the useful-signal trials than in the no-signal trials, $t(71) = 6.46$, $p < .001$, $\eta p2 = .76$, demonstrating that the signal did indeed provide the participant with an advantage with respect to responding to the correct target.



*Graph 1: Mean RTs for each of the three trial types, for each of the experiments. Error bars depict 95% within-subject confidence intervals.*

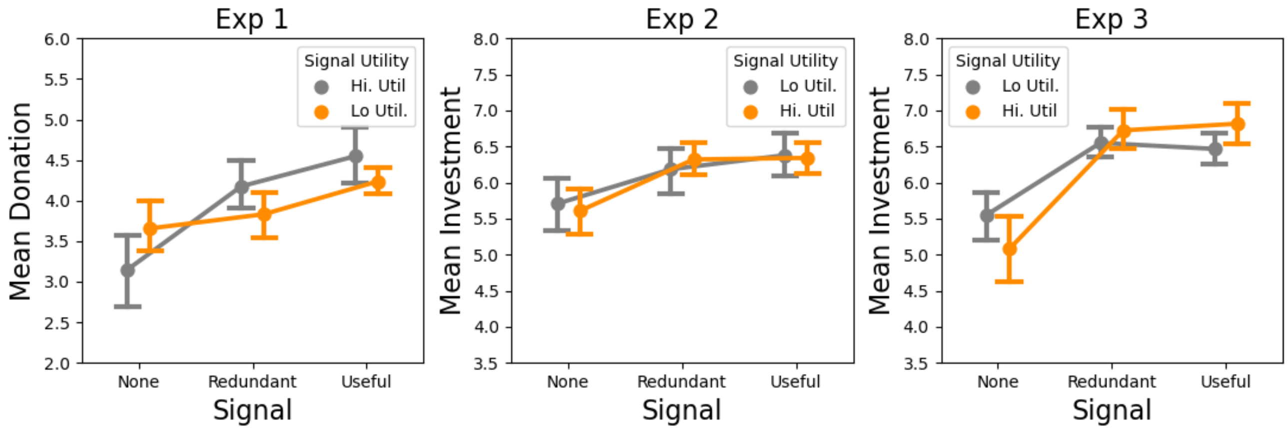
In order to investigate the effect that our manipulation had on generosity, we compared participant's mean donation between the three trial types. Firstly, we removed any donation trials in which participants took more than 1 minute (we chose this duration in order to accommodate for participants' deliberation regarding their decision) to respond, as it was likely that participants taking more than 1 minute were inattentive during that trial. This applied to all studies in the manuscript. A one-way ANOVA (see left panel of graph 2) revealed a significant main effect of signal type, $F(2,142) = 13.04$, $p < .001$ $\eta p2 = .16$. Bonferroni corrected t-tests (corrected for three comparisons) demonstrated that compared to no-signal trials, participants donated significantly more in both redundant-signal trials $t(71) = -2.68$, $p=.028$, $d = -.32$ and useful-signal trials $t(71) = -4.46$, $p < .001$, $d = -.53$, demonstrating that sensorimotor communication made participants more generous to their partner, regardless of the utility of the signal. Moreover, participants donated significantly more in useful-signal trials compared to redundant-signal trials *t*(71) = -3.21, *p* = .006, *d* = -.38, demonstrating that receiving useful information made participants more generous to their partners.



*Graph 2: Mean Donation/Investment for each of the three trial types, for Experiment 1 (Left), Experiment 2 (Middle) and Experiment 3 (Right). Error bars depict 95% within-subject confidence intervals (Cousineau, 2005) and dots represent individual participants.*

## Experiment 1: Exploratory analyses

As a further exploratory test of the extent to which the effect of sensorimotor communication on generosity depended on the utility of the signal, we calculated a ‘utility score’ for each participant by subtracting their mean RT in no-signal trials from their mean RT in useful-signal trials. This allowed us to directly quantify the advantage that the signal provided to the participant with respect to responding to the correct target as quickly and accurately as possible. We then carried out a median-split using the utility score in order to sort participants into a high-utility group and low-utility group, before conducting a 2x3 mixed ANOVA (see graph left panel of graph 3) with group (low-utility, high-utility) as a between-subjects factor and trial type (no-signal, redundant-signal, useful signal) as a within-subjects factor. This analysis revealed a main effect of signal type, *F*(2,140) = 14.35, *p* < .001, *ηp2* = .17, but no main effect of group, *F*(1,70) = .04, *p* = .85, *ηp2* = .003. There was a significant interaction between signal and group *F*(2,140) = 8.14, *p* < .001, *ηp2* = .1. To unpack the interaction, we carried out two separate one way ANOVAs in order to examine how the utility of the cues affected each of the trial types specifically. The one-way ANOVA for the low utility group was not significant, *F*(2,70) = .97, *p* = .39, *ηp2* = .03. However, the one-way ANOVA for the high utility group revealed a significant main effect of signal, *F*(2,70) = 17.02, *p* < .001, *ηp2* = .33. Post hoc t-tests (corrected for three comparisons) demonstrated that compared to no-signal trials, participants donated significantly more in both redundant-signal trials *t*(71) = -3.58, *p*=.003, *d* = -.6 and useful-signal trials *t*(71) = -4.82, *p* < .001, *d* = -.8, and participants donated significantly more in useful-signal trials compared to redundant-signal trials, *t*(71) = -2.8, *p* = .025, *d* = -.47. The fact that the signal affected participant’s donation behaviour in the high-utility group but not in the low-utility group further underlines the importance of the utility of the signal with respect to its effect on generosity. This effect was not present when we treated utility score as a continuous dependent variable in a linear mixed model, which revealed a significant main effect of trial type, $t = 4.12$, $p < .001$, but no effect of utility score, $t = .99$, $p = .32$, and no interaction between trial type and utility score, $t = .31$, $p = .76$, suggesting that perhaps the interaction between our utility group factor and signal type in Experiment 1 is better explained by subgroups with regard to the utility of the signal, rather than a continuous relationship between these factors.



*Graph 3: Interaction between Trial type and Utility group for Mean Donation/Investment, for Experiment 1 (Left), Experiment 2 (Middle) and Experiment 3 (Right). Error bars depict 95% within-subject confidence intervals.*

## Experiment 2

In a cooperative setting, the willingness to invest significant resources into an interaction depends upon our belief that our partner has prosocial motives and will give us a fair return on this investment (Berg, Dickhaut & Mccabe, 1995). Rather than using cues to a co-actor's willingness to bear the costs of an interaction in order to decide how to distribute resources with that co-actor (as in the case of generosity), trust requires us to use these cues in order to weigh up the relative risks and rewards of cooperating with the co-actor. Can we use cues to a co-actor investment of effort costs into an interaction in order to decide to what extent we are willing to trust that co-actor with our resources? Experiment 2 used a Trust game in order to examine the effects of a co-actor's effort costs (in the form of sensorimotor communication) upon an actor's willingness to trust that co-actor. We predicted that if participants only trusted those whose efforts yielded useful outcomes for them, they would invest more points only with those who sent useful signals. However, if participants trusted based on the effort itself rather than the outcome, then participants would invest more both with those who sent useful signals and with those who sent redundant signals.

## Experiment 2: Method

The hypotheses, sample sizes, methods, and analyses were all pre-registered before data collection. The pre-registration can be accessed at: https://aspredicted.org/blind.php?x=ju3mz7

### *Participants*

As in Experiment 1, we collected 72 participants (39 women, 33 men) with a mean age of 34.69 (SD = 12.16) using the Prolific recruitment platform (www.prolific.co).

### *Apparatus and stimuli*

The apparatus and stimuli were identical to that of Experiment 1. For the trust game, the slider used to make donations was identical to that of Experiment 1 for the dictator game.

### *Procedure*

The procedure was identical to that of Experiment 1. However, rather than a dictator game, participants played an investment game, i.e., a standard trust game (Berg, Dickhaut & McCabe, 1995). Participants received an endowment of 10 points and were told that they could choose how many points to keep for themselves and how many points to invest with their partner. They chose how many points to invest by clicking a slider. They were informed that their investment would be tripled by the experimenter before being passed to their partner, who could choose how much of the tripled investment to give back to the participant and how much to keep for themselves. With respect to their performance, participants were informed that their bonus payment of 1GBP was dependent on how quickly and accurately they responded in the coordination task, and how many points in total they accrued during each of the investment games.

### *Design*

Our study consisted of Signal Type (no-signal, redundant-signal, and useful-signal) as a within-subjects factor, and points invested as a dependent variable.

## **Experiment 2: Results**

A one-way ANOVA (see middle panel of graph 1) on the RTs from the three trial types revealed a significant main effect of signal, $F(2,142) = 171.52$, $p < .001$, $\eta p2 = .71$. Post hoc t-tests (Bonferroni corrected for three comparisons) demonstrated that participants were faster in redundant-signal trials, compared to useful-signal trials $t(71) = -15.59$, $p <.001$, $d = -1.84$, and no-signal trials $t(71) = -.15.18$, $p <.001$, $d = -1.79$. Moreover, participants were faster in the useful-signal trials than in the no-signal trials *t*(71) = 4.89, *p* < .001, *d* = .58, demonstrating that the signal helped the participant more quickly respond to the correct target.

In order to investigate the effect that our manipulation had on trust, we compared participant's mean investment between the three trial types. A one way ANOVA (see middle panel of graph 2) revealed a significant main effect of signal type *F*(2,142) = 8.41, *p* < .001, *ηp2* = .11. Bonferroni corrected t-tests (corrected for three comparisons) demonstrated, that compared to no-signal trials, participants invested significantly more in both redundant-signal trials, *t*(71) = -2.93, *p* = .014, *d* = -.35, and useful-signal trials, *t*(71) = -3.68, *p* = .001, *d* = -.43 demonstrating that sensorimotor communication led participants to trust their partner more, regardless of the utility of the signal. However, investment did not differ between useful-signal trials and redundant-signal trials *t*(71) = -.69, *p* = 1, *d* =-.08, demonstrating that participants were more trusting of partners who incurred effort costs to send information, regardless of the utility of the information.

**Experiment 2: Exploratory analyses**

As in Experiment 1, we carried out a median-split using the utility score (useful-signal RTs - no-signal RTs) to sort participants into a high-utility group and low-utility group, before conducting a 2x3 mixed ANOVA with group (low-utility, high-utility) as a between-subjects factor and trial type (no-signal, redundant-signal, useful signal) as a within-subjects factor (see middle panel of graph 3). This analysis revealed a main effect of signal, *F*(2,140) = 8.32, *p* <.001, *ηp2* = .11, but no main effect of group, *F*(1,70) = 1.95, *p* = .17, *ηp2* = .03, and no interaction between signal and group, *F*(2,140) = .2, *p* = .81, *ηp2* = .003, further demonstrating that participants investment behaviour was affected only by the fact their co-actor sent a signal, regardless of its utility. A linear mixed model treating utility score as a continuous factor also revealed no effect of utility score, demonstrating a significant main effect of trial type, $t$=2.49, $p$ = .01, but no effect utility score, $t$ = -1.33, $p$ = .18, and no interaction between trial type and utility score, $t$ = .79, $p$ = .43.

## Experiment 3

Our first two Experiments demonstrate that people are more cooperative towards those who are willing to bear the effort costs of cooperation, just as they are more cooperative towards those who are willing to bear the monetary costs of cooperation. So far, participants were not given any specific information about the relative payoffs of the coordination task and the economic interaction (i.e., the dictator game or the trust game); we assume that they believed that the stakes were relatively similar between these two tasks. However, in reality, many day-to-day joint actions are relatively low stakes, being performed for no clear monetary gain, or even just for fun. This raises the following question: If a co-actor’s willingness to bear effort costs informs an actor’s decision about how much trust to exhibit in an economic interaction with that co-actor when the stakes of the joint action and economic interaction are equivalent, can the same be said about cooperative interactions with relatively low-stakes compared to the economic interaction? Specifically, will an actor use effort cues from relatively low-stakes cooperative interactions in order to make decisions pertaining to relatively high-stakes economic interactions? Or does the relative unimportance of these low-stakes interactions lead people to disregard these effort cues when deciding whether to engage with their co-actor in economic interactions with higher stakes? Thus, our third experiment aimed to investigate whether explicitly decreasing the stakes of the coordination task relative to the trust game would lead to the same or a different pattern of results as in Experiment 2.

## Experiment 3: Method

The hypotheses, sample sizes, methods, and initial analyses were all pre-registered before data collection. The pre-registration can be accessed at: https://aspredicted.org/blind.php?x=wh54w3

### *Participants*

As in Experiment 1 and 2, we collected 72 participants (34 women and 38 men) with a mean age of 34.49 (SD = 12.72) using the Prolific recruitment platform (www.prolific.co).

### *Apparatus and stimuli*

The apparatus and stimuli were identical to that of Experiment 2.

### *Procedure*

The procedure was identical to that of Experiment 2 with the following exception: during the instructions, participants were informed that 10% of their bonus would come from their performance on the coordination task, and 90% of their bonus would come from the amount of points collected from each investment game.

### *Design*

Our study consisted of Signal Type (no-signal, redundant-signal, and useful-signal) as a within-subjects factor, and points invested as a dependent variable.

## Experiment 3: Results

A one-way ANOVA (see right panel of graph 1) on the RTs from the three trial types revealed a significant main effect of signal, $F(2,142) = 343.31$, $p < .001$, $\eta p2 = .83$. Post hoc t-tests (Bonferroni corrected for three comparisons) demonstrated that participants were faster in redundant-signal trials, compared to useful-signal trials $t(71) = -23.8$, $p <.001$, $d = -2.81$, and no-signal trials $t(71) = -.16.55$, $p <.001$, $d = -1.95$. Moreover, participants were faster in the useful-signal trials than in the no-signal trials $t(71) = 9.7$, $p < .001$, $d = 1.14$, demonstrating that the signal helped the participant more quickly respond to the correct target.

A one way ANOVA (see right panel of graph 2) on mean investment between the three trial types revealed a significant main effect of signal, $F(2,142) = 30.8$, $p < .001$, $\eta p2 = .3$. Bonferroni t-tests (corrected for three comparisons) demonstrated that compared to no-signal trials, participants invested significantly more in both redundant-signal trials $t(71) = -5.72$, $p <.001$, $d = -.67$ and useful-signal trials $t(71) = -5.8$, $p < .001$, $d =-.68$, demonstrating that sensorimotor communication led participants to trust their partner more, regardless of the utility of the signal. However, investment did not differ between useful-signal trials and redundant-signal trials, $t(71) = .2$, $p = 1$, $d = .02$, demonstrating that participants were more trusting of partners who incurred effort costs to send information, regardless of the utility of the information.

## Experiment 3: Exploratory analyses

As in Experiment 1, we carried out a median-split using the utility score (useful-signal RTs - no-signal RTs) to sort participants into a high-utility group and low-utility group, before conducting a 2x3 mixed ANOVA with group (low-utility, high-utility) as a between-subjects factor and trial type (no-signal, redundant-signal, useful signal) as a within-subjects factor (see right panel of figure 3). This analysis revealed a significant main effect of signal, $F(2,140) = 31.4$, $p < .001$, $\eta p2 = .31$, but no main effect of group, $F(1,70) = 1.47$, $p = .23$, $\eta p2 = .02$ and no main interaction between signal and group, $F(2,140) = 2.38$, $p = .1$, $p = .03$, underlining the fact that investment behaviour was affected only by the fact their co-actor sent a signal, regardless of its utility. A linear mixed model treating utility score as a continuous factor also revealed no effect of utility score, demonstrating a significant main effect of trial type, $t=5.12$, $p <.001$, but no effect utility score, $t = -1.17$, $p = .24$, and no interaction between trial type and utility score, $t = 1,19$, $p = .23$.

Finally, in order to investigate whether changing the stakes of the coordination task relative to investment game affected participants' investment behaviour across the three trial types, we carried out an exploratory analysis (i.e., not pre-registered), directly comparing investment behaviour from Experiment 2 (equal stakes) with investment behaviour from Experiment 3 (unequal stakes). A 2x3 ANOVA with Experiment (equal, unequal) as a between-subjects factor, and trial type (no-signal, redundant-signal, useful-signal) as a within-subjects factor revealed a significant main effect of signal (2,284) = 35.87, $p < .001$, $\eta p2 = .2$, but no main effect of Experiment, $F(1,142) = .08$, $p = .78$, $\eta p2 = .005$. However, there was a significant interaction between signal and Experiment, $F(2,284) = 4.08$, $p = .02$, $\eta p2 = .03$, demonstrating that increasing the stakes of the investment game compared to the coordination game did indeed influence how participants use effort costs to inform their investment behaviour. In order to unpack the interaction, we first computed an index of how much more each participant invested with partners who signalled (in both useful-signal trials and redundant-signal trials) compared to partners who did not signal (no-signal trials). We created the variable ‘advantage’ by subtracting mean investments in no-signal trials from their mean investments in redundant-signal trials and useful-signal trials. We then carried out a 2x2 ANOVA with ‘advantage’ (redundant, useful) as a within-subjects factor and Experiment (equal, unequal) as a between-subjects factor. This analysis revealed no main effect of advantage, $F(1,142) = .25$, $p = .62$, $\eta p2 = .002$, a significant effect of Experiment, $F(1,142) = 5.14$, $p = .03$, $\eta p2 = .04$, and no interaction between advantage and Experiment, $F(1,142) = .48$, $p = .49$, $\eta p2 = .003$. A post-hoc independent samples t-test for Experiment was significant $t(142) = -2.27$, $p = .025$, $d = -.19$, demonstrating that participants donated significantly more to those who signalled compared to those who did not signal when the stakes were unequal, compared to when the stakes were equal.

## Experiment 4

Experiments 1-3 demonstrated that a co-actor’s communicative movement adaptations foster prosocial attitudes such as trust and generosity towards that co-actor, regardless of the utility of the signal. However, what is not yet clear is whether or not a co-actor’s signal needs to carry a communicative intention in order to foster prosocial attitudes such as trust and generosity (e.g., Sperber & Wilson, 1986). Specifically, is it the case that any useful signal produced effortfully, whether intentionally or incidentally, leads to an increase in prosocial attitudes such as trust and generosity? Or does the useful signal need to be produced intentionally, with the co-actor incurring an effort cost *with the intention to communicate*? Thus, Experiment 4 aimed to investigate whether the communicative intention behind a useful signal influences the effect of that signals upon trust. To test this, we administered a similar task to that in Experiment 2 (coordination task and then trust game) in either an intentional-signal group or an unintentional-signal group. In the intentional-signal group, participants completed no-signal trials with partners who did not signal and useful-signal trials with partners who produced signals by curving their movements towards the target at an early point in time, thus disambiguating the correct target (identical to the no-signal and useful-signal trials in the previous experiments). In the unintentional-signal group, participants completed no-signal trials with partners who did not signal, and useful-signal trials with partners who produced signals by curving their movements towards the target at an early point in time in order avoid an obstacle, thus disambiguating the correct target. Specifically, participants were informed that an obstacle would sometimes appear in the dragger's path, and that the dragger would lose points if their marker touched the obstacle. This allowed us to directly compare the increase in trust towards those who appeared to produce a signal with a communicative intention (i.e., the dragger appearing to voluntarily move along a curved trajectory) with the increase in trust towards those who appeared to produce the signal unintentionally (i.e., the dragger appearing to move along a curved trajectory to fulfil an instrumental goal). If a communicative intention does not influence the effect that useful signals have on prosocial attitudes such as trust, then we should expect that participants would invest more with those who send useful signals than with those who do not signal, irrespective of whether the signal was produced intentionally or unintentionally (i.e., a main effect of Trial Type). However, if a communicative intention does influence the effect that useful signals have on prosocial attitudes such as trust, then we should expect that participants would invest more with those who produce useful signals than with those who do not signal, to a greater extent (or only) when the useful signal is produced intentionally (i.e., an interaction between Trial Type and Signal Type).

## Experiment 4: Method

The hypotheses, sample sizes, methods, and analyses were all pre-registered before data collection. The pre-registration can be accessed at: https://aspredicted.org/t8iy6.pdf

### *Participants*

We collected 64 participants (28 women, 36 men) with a mean age of 36.15 (SD = 12.72) using the Prolific recruitment platform (www.prolific.co).

### *Apparatus and stimuli*

The apparatus and stimuli were identical to that of Experiment 2. However, for useful-signal trials in the unintentional signal group, a gray obstacle (height = 0.15, width = 0.15) was placed in the path of the dragger. All movement trajectories used were the same as those in Experiment 1-3.

### *Procedure*

The procedure was similar to that in Experiment 2, except there were no redundant-signal trials, meaning that participants were never given advance information about the upcoming target. Participants were randomly assigned to either the intentional-signal group or unintentional-signal group. In the intentional-signal group, participants completed six no-signal trials in which the dragger moved along a straight trajectory (thereby providing no information about the target) and six useful-signal trials in which the dragger moved along a curved trajectory (thereby providing the participant with information about the upcoming target).

In the unintentional-signal group, participants were informed that an obstacle would sometimes appear in the dragger's path, and that the dragger would lose points if their marker touched the obstacle. Participants completed six no-signal trials in which there was no obstacle, with the dragger moving along a straight trajectory (thereby providing no information about the target). Participants also completed six useful-signal trials in which the dragger moved along a curved trajectory around the obstacle (thereby providing the participant with information about the upcoming target). This allowed us to examine participants' relative trust of useful-signalers compared to no-signalers in cases in which the signal was produced intentionally (i.e., the dragger appearing to voluntarily move along a curved trajectory) and unintentionally (i.e., the dragger appearing to move along a curved trajectory in order to fulfil an instrumental goal).

### *Design*

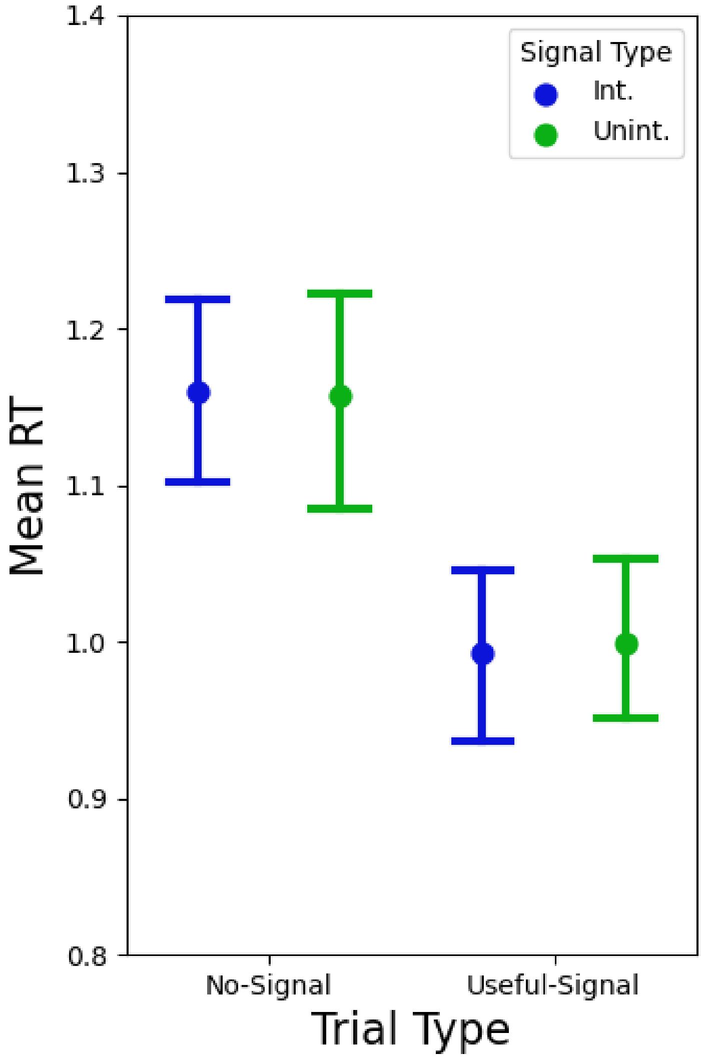
This experiment comprised a 2 x 2 mixed design, with Trial Type (no-signal, useful-signal) as a within-subjects factor, Signal Type (intentional-signal, unintentional-signal) as a between-subjects factor, and points invested as a dependent variable.

## **Experiment 4: Results**

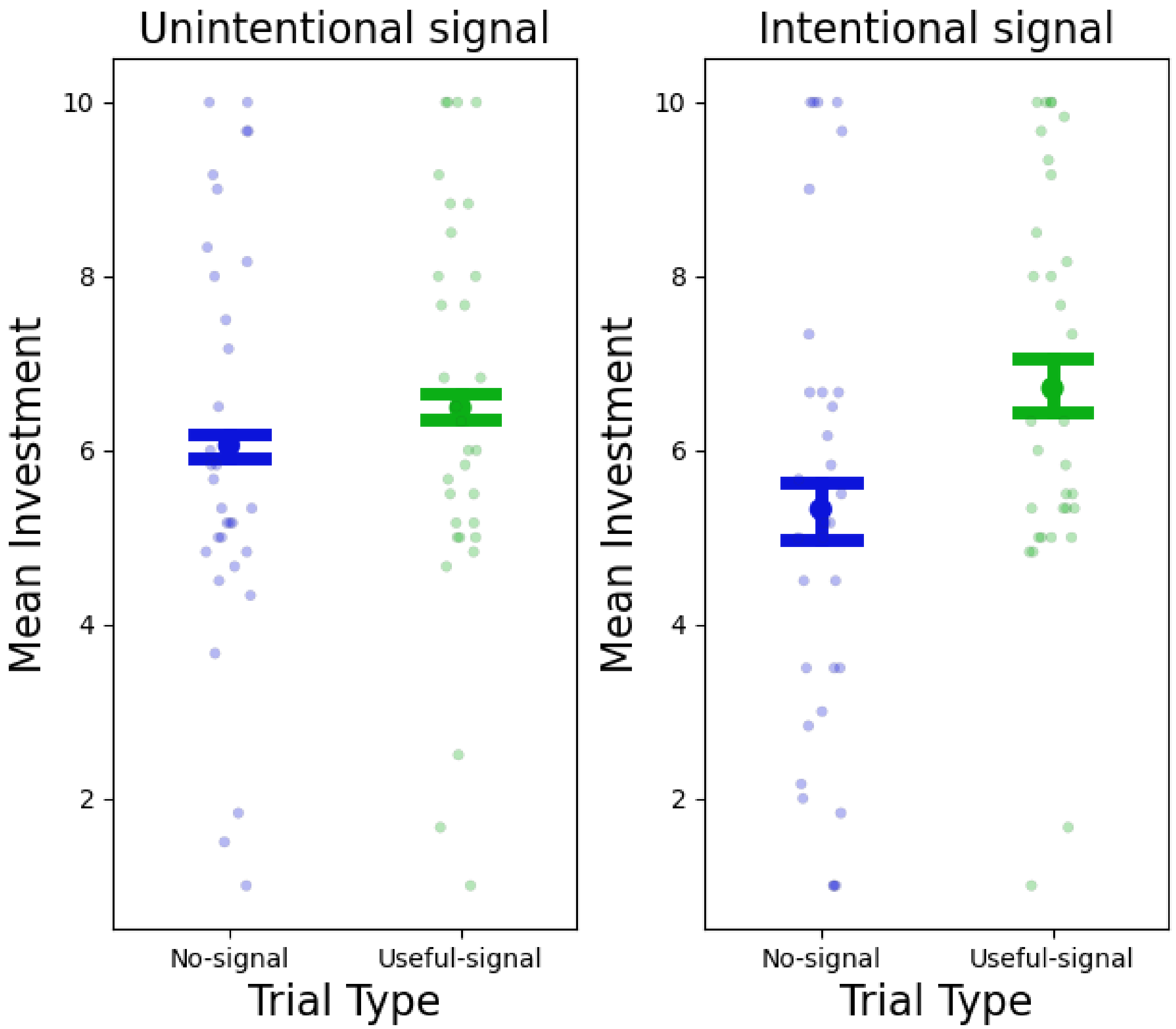
A 2x2 mixed ANOVA on the RTs revealed a main effect of Trial Type, $F(1,61) = 16.54$, $p < .001$, $\eta p2 = .21$, but no main effect of Signal Type, $F(1,61) = .17$, $p =$ ,67, $\eta p2 < .001$, or no interaction between Trial Type and Signal Type, $F(1,61) = .01$, $p = .9$, $\eta p2 < .001$.

In order to investigate the effect that our manipulation had on trust, we carried out a 2x2 mixed ANOVA with Trial Type (no-signal, useful-signal) as a within-subject factor, Signal Type (intentional-signal, unintentional-signal) as a between-subjects factor, and points invested as a dependent variable. This analysis revealed a significant main effect of Trial Type, $F(1,62) = 23.21$, $p < .001$, $\eta p2 = .27$, and no main effect of Signal Type, $F(1,62) = .19$, $p = .67$, $\eta p2 = .003$. Crucially, there was a significant interaction between Trial Type and Signal Type, $F(1,62) = 6.41$, $p = .01$, $\eta p2 = .09$, demonstrating that participants' investment behaviour in the useful-signal trials relative to the no-signal trials differed depending on whether or not the signal was performed with a communicative intention.

Post-hoc t-tests (Bonferroni corrected for six comparisons) revealed a significant difference between no-signal and useful signal trials, both for the Intentional-signal group, $t(31) = -4.012$, $p < .001$, $d = -.54$, and the Unintentional-signal group, $t(31) = -2.84$, $p = .048$, $d = -.18$ No other comparisons were statistically significant ($p>.05$). This demonstrates that participants based their investment behaviour on their partner's signaling behaviour, both when the signal was produced with a communicative intention, but also when the signal was produced unintentionally (albeit to a lesser extent as highlighted by the effect sizes). Thus, although our interaction demonstrates investment behaviour in the useful-signal trials relative to the no-signal trials did depend on the presence or absence of a communicative intention, the signal itself did also influence investment behaviour.



*Graph 5: Mean RTs for the two trial types in Experiment 4, for each condition. Error bars depict 95% within-subject confidence intervals (Cousineau, 2005).*



*Graph 6: Mean Investment for the two trial types in Experiment 4, for each condition (unintentional-signal condition: left panel, intentional-signal condition: right panel). Error bars depict 95% within-subject confidence intervals (Cousineau, 2005) and dots represent individual participants.*

## General discussion

Across four experiments, we provide new insight into the kinds of cues that serve to stabilize cooperation – i.e., we demonstrate that a co-actor's willingness to bear the effort costs of an interaction provides a powerful cue that helps to foster trust and generosity towards them. Specifically, a co-actor's efforts to adapt their actions in order to provide task-relevant information led participants to be more generous towards them, and more willing to trust them with an investment. Moreover, the findings from our Experiment 4 highlight the fact that the communicative intent behind a signal is a significant driving factor with respect to the relationship between effortful adaptation and trust, although the signal itself was also, albeit to a lesser extent, a factor contributing to this relationship.

What is the mechanism underpinning the relationship between sensorimotor communication and trust and generosity that we observed in our experiments? We believe that it is unlikely that an increase in trust and generosity were simply down to the fact that the participants' superior performance (faster RTs) in the signal trials gave them the feeling that they had more resources to share. Specifically, such an account would predict that participants would be most cooperative in redundant-signal trials, where they were able to perform much better at the task in virtue of having advance information about the target. It is also possible that participants acted more cooperatively due to the positive affect associated with being helped (Wood, Froh & Geraghty, 2010). However, this would not explain differences between the useful-signal and redundant-signal condition with respect to fostering generosity (Experiment 1), nor would it explain the fact that increasing the relative stakes of the trust game also increases the extent to which sensorimotor communication affects trust. More research is needed to investigate how the effects of sensorimotor communication upon trust and generosity are moderated by affect.

What about the informational value of the signal itself? The finding that in Experiment 4, participants invested significantly more with those who sent unintentional signals than with those who sent no signals (although this effect was small relative to the comparison of those who sent intentional signals and those who sent no signals) demonstrates that the informational value of a signal, regardless of whether it is produced intentionally, may play a role in the effects of sensorimotor communication upon trust. This is also corroborated by our finding in Experiment 1 that participants were more generous to those who sent useful signals than to those who sent redundant signals, also highlighting the value that an actor may place on the signal itself. Thus, it is likely that in addition to the intentions behind a signal, the informational value of a signal is also important with regard to its effects upon generosity and trust.

However, in Experiment 4, the finding that investment behaviour in the useful-signal trials relative to the no-signal trials did depend on whether the signal was produced intentionally or unintentionally (as indexed by an interaction effect) highlights the role that a communicative intention plays in the relationship between sensorimotor communication and trust. Moreover, the fact that Experiments 1-3 consistently demonstrate that redundant signals yield prosocial outcomes highlights the role that the intention to inform plays in the relationship between sensorimotor communication and generosity and trust. Thus, we believe that sensorimotor communication may act as a cue that the co-actor was willing to invest effort into an interaction for the good of those involved (see SM3 for evidence that participants perceived sensorimotor communication as effortful relative to no communicative signal). And indeed, this interpretation is supported by the moderating influence that intentionality has on the effect of sensorimotor communication upon trust (Experiment 4). With respect to investment behaviour in the trust game, sensorimotor communication may have been used as an index of trustworthiness, with those who voluntarily produced communicative signals being deemed more likely to make prosocial decisions (i.e., make a fair return) and those who chose not to produce communicative signals being deemed more likely to make selfish decisions (i.e., make an unfair return).

It is also likely that participants had reciprocal motivations (Axelrod & Hamilton, 1981) paying back co-actors who sent communicative signals with points in the economic game. This is particularly plausible for generosity, where donations to a co-actor depended on the utility of their signal whereas trust towards a co-actor did not. For instance, the effortful signal may have been deemed irrelevant with respect to deciding whether or not to trust the co-actor but may have elicited a ‘sense of debt’ towards the co-actor, thereby motivating participants to generously repay this debt (McGrath & Gerber, 2019). This is also evidenced by the finding in Experiment 4 that participants were more willing to invest with those who produced signals unintentionally than with those who did not produce signals at all. Thus, in addition to positively evaluating their co-actors on the basis of their efforts to communicate, participants may have also reciprocated signals on the basis of their utility with respect to the task. This is consistent with findings demonstrating that in addition to an actor’s efforts to act prosocially, the outcomes of these acts can also, to a lesser extent, influence evaluations of the actor (Eisenbruch & Roney, 2017).

Overall, our study demonstrates that the willingness to bear effort costs increases trust and generosity towards that actor, highlighting the value that actors place on their partners’ effort. Specifically, these effort cues may be taken as indicative of that actor’s social preferences. Interestingly, even when the stakes of the coordinated joint action were low relative to the economic interaction, participants still considered the co-actor’s willingness to incur effort costs in making their decision. In fact, participants seemed to rely more on these cues in this scenario. We may speculate that, because the stakes of the economic task in Experiment 3 were relatively high compared to Experiment 2, participants were more careful to search for cues to cooperation. These findings show that in addition to high-stakes economic interactions, people may use cues from smaller day-to-day joint actions in order to inform their decisions in a cooperative context, even when there are more substantial resources at stake. Future research could investigate the extent to which other effortful behaviours that support joint actions, such as temporal adaptation, perspective taking, or action planning (Konvalinka et al. 2010; Kampis & Southgate, 2020; Török et al. 2019), may also boost trust and generosity. Likewise, future research could also investigate the extent to which trust and generosity scales up with increasing effort costs, testing the prediction that larger effort costs lead to larger increases in trust and generosity.

These findings have several implications for our understanding of how people decide to direct their trust and generosity. By now, it has been well established that actors' trust and generosity is informed by how their co-actors' behave in economic interactions in which there are often relatively high financial stakes (Camerer & Loewenstein, 2011). The current study illustrates that how our trust and generosity towards our co-actors' is influenced by how they perform in the smaller joint actions that make up the vast majority of our everyday interactions and do not involve any significant financial costs. This finding taken together with the fact that humans can accurately estimate the effort expended by others (Ibbotson et al. 2019), may help to explain why we humans are such effective cooperators -- i.e., by virtue of our ability to use relatively unimportant day-to-day interactions to prudently choose when to act cooperatively and with whom.

Moreover, our findings open up a new perspective on why humans are so motivated to engage in coordinated activities, even in the absence of anticipated material benefits, or indeed if it comes at a cost to themselves (Reddish, Fischer & Bulbulia, 2013; Curioni et al. 2020). Previous research has proposed that our motivation to coordinate comes from the fact that coordination increases prosocial attitudes towards each other (Hove & Risen, 2009; Reddish et al., 2016), and that engaging in coordinated interactions provides the opportunity to signal the strength of a group's bond (Michael, McEllin & Felber, 2019; Fessler & Holbrook, 2016). Our findings raise the possibility that humans are attracted to coordination in part because it provides the opportunity to appraise those we interact with and to access important cues that inform our decisions about how much trust and generosity to exhibit towards specific individuals.

The current findings also raise new questions for further research. For example: What are the cognitive mechanisms that may underpin our ability to appraise our partners on the basis of their effort? One possibility is that humans may rely on evolved general mechanisms for effort perception, which serve the function of keeping track of group members' relative contributions to an interaction (Ibboston et al. 2019). Alternatively, the same coupling between perception and action that allows us to predict others' actions and to coordinate with them (Wolpert, Doya & Kawato, 2003, Sebanz et al. 2006) may also allow us to keep track of others' effort contributions using motor simulation. Future research could investigate the role of motor simulation in effort perception, perhaps by comparing the performance of non-experts and experts interacting in their domain of expertise (e.g., Aglioti, Cesari, Romani & Urgesi, 2008).

Moreover, our study employed one-shot economic interactions in which participants interacted with bots rather than human partners.  Although this was necessary to ensure experimental control over the partners' movement kinematics and to ensure any effects associated with reciprocity did not confound our results, it also precluded us from investigating the dynamics that may unfold if participants perform the task with real partners over multiple rounds. By employing a similar procedure (i.e., a joint action task preceding an economic game) with real pairs of human participants interacting with each other over multiple rounds of the task, future research could further probe the role that effort costs incurred in everyday joint actions play as a currency in cooperative interactions. Specifically, this could also allow us to examine the extent to which actors may exchange effort and resources in a reciprocal manner. Such a study could allow us to examine whether participants may use punishment in the economic game in order to incentivize each other to invest effort in the coordination task. Moreover, it would also allow us to investigate whether people may willingly incur significant effort costs, for example in the form of sensorimotor communication, in order to demonstrate their value as an interaction partner -- i.e., just as they use generosity to demonstrate their value as a cooperation partner (e.g., Hardy & van Vugt, 2006).

It may also be fruitful for future research to probe the boundary conditions of the effect observed here. In light of previous research showing that unrequired help can be perceived as patronizing and can elicit negative feelings (Chasteen, Horhota, Crumley-Branyon, & Haseley, 2021), we would speculate that sensorimotor communication may be perceived as pedantic or patronizing, and may elicit negative feelings, when the recipient takes it to indicate that the co-actor producing the signal judges the recipient to be incompetent and therefore in need of extra assistance. In our experiments, this is unlikely to have been the case, since the co-actor producing the signal did not know that the signal was redundant (i.e. did not know that the participant had advance knowledge of the target).

Finally, due to our largely Western sample (see SM2 for demographic information), there may be limits to the generalizability of our findings, particularly to those living in non-industrialized societies (Henrich, Heine & Norenzayan 2010). Thus, particularly considering the significant variation in performance on economic games among different cultures (Henrich et al. 2001), and the fact that inferring intentions from motion may be universal (Barrett, Todd, Miller & Blythe 2005), further research should investigate cultural differences in the effect that effort cues have on trust and generosity.

## Conclusion

We demonstrate that the willingness to invest effort costs into an interaction (in addition to the material outcome of these actions) is used as a cue to inform our decisions about how much trust and generosity to exhibit towards that partner. Not only does this suggest that effort cues may reflect an actor's social preferences, but it also suggests that these cues can be used to cooperate prudently. This raises the possibility that humans can use information from even the most casual of interactions in order to appraise partnerships, which may also help us understand why we are so motivated to engage in joint actions together.

## Data Availability

Data deposition: The data that support the findings of this study are available in the Open Science Framework (https://osf.io/mfs3q/).

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