Playing a different game:

Situation perception mediates framing effects on cooperative behaviour

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We thank Daniel Balliet, Erik de Kwaadsteniet, Tore Ellingsen, Christoph Engel, Simon Gächter, Philipp Gerlach, Adam Stivers, audience members at the Max Planck Institute for Research on Collective Goods, and two anonymous reviewers for helpful comments and discussion, as well as Joachim Prinz for help with data collection.

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Abstract

Context frames such as describing a Prisoner's Dilemma as a "community" or a "stock exchange" game cause significant variation in cooperative behaviour. Here, we draw on recent advances in research on situation construal to propose that perceived conflict of interests is a mechanism underlying framing effects on cooperation in experimental games. Specifically, we argue that people readily think about situations in terms of conflicts of interests, and how people perceive the conflict of interests in different games predicts differences in cooperation across these games. In a formal model, we show that variation in perceived conflict of interests can affect cooperative decisions even when first-order beliefs and social preferences are static. We test this model across two studies (total $N = 1,615$). In each study, we elicited situation perceptions and beliefs about others' behaviour in a framed game. The basic framing effect replicated in a dichotomous-choice Prisoner's Dilemma (study 1), but not in a continuous-choice Prisoner's Dilemma (study 2). Perceptions of conflict of interests mediated between framing and cooperative behaviour, whereas beliefs and preferences did not. Exploratorily, perceptions of conflict of interests had a causal effect on cooperative behaviour when induced orthogonally to the frame. This study provides evidence for perceptions of conflict of interests as a mechanism underlying framing effects and integrates framing effects with the wider literature on situation perception. Moreover, it supports a causal role of situation perception in social decision-making. However, the results also call into question the generalisability of framing effects.

Keywords: cooperation; framing; interdependence; situation perception; social dilemmas.

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There have been considerable efforts in psychology and behavioural game theory to study how “the choice an individual makes is sometimes sensitive to the way it is presented, or ‘framed’, in the sense that even theoretically equivalent choices may elicit different responses when presented differently” (Roth, 1995; as cited in Gerlach, Jaeger, & Hertwig, 2017). Recently, a class of frames that evoke certain motivations or settings—so-called “context framing” effects (Gerlach & Jaeger, 2016; Levin, Schneider, & Gaeth, 1998)—have received renewed attention. An example is Liberman, Samuels, and Ross’s (2004) seminal study showing that describing the Prisoner’s Dilemma as a “community” versus a “stock exchange” game induced stark differences in cooperation rates.

The dominant explanation of context framing effects is that such frames affect beliefs about the behaviour of other players (*first-order beliefs*), which in turn induces conditionally cooperative players to change their behaviour (cf. Fischbacher & Gächter, 2010). Alternatively, frames may alter player’s preferences about others’ outcomes (*social preferences*; Dreber, Ellingsen, Johannesson, & Rand, 2013; Ellingsen, Johannesson, Mollerstrom, & Munkhammar, 2012). Here, we draw on recent advances in research on how people construe situations to put forward an alternative mechanism. Specifically, players may use seemingly superfluous information provided to them explicitly or implicitly through the description of the task to infer aspects of their interdependence with the other player, particularly in terms of their perceived conflict of interests (i.e., the degree to which the best outcomes for each interaction partner are conflicting). Players then act on their perception of the game (i.e., the *psychological situation*, Balliet, Tybur, & van Lange, 2017; Gerpott, Balliet, Columbus, Molho, & de

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Vries, 2018; Rauthmann, Sherman, & Funder, 2015). We propose an experimental test of this hypothesised mechanism and a comparison against a mechanism via frame-dependent first-order beliefs. Overall, we integrate research on situation perception with studies of framing effects, and propose a plausible mechanism underlying context framing effects across a wide range of frames and games.

Theoretical Background

Mixed-Motive Games

Mixed-motive games are situations that involve both an opportunity for two (or more) players to cooperate for mutual benefit and to defect for personal gain.¹ The Prisoner's Dilemma is a commonly studied mixed-motive game. Fig. 1A shows the outcomes for two players for each combination of their individual choices to cooperate or defect. The Prisoner's Dilemma is defined by the rank order of the four outcomes such that Temptation (T) > Reward (R) > Punishment (P) > Sucker's payoff (S). In this game, mutual cooperation maximises joint outcomes (as $2R > T+S$), but there is an individual incentive to defect for personal gain (as $T > R$). This gives rise to a conflict of interest stemming both from greed (as $T > R$) and from fear of being exploited (as $P > S$). Other mixed-motive games are defined by a different rank order of the outcomes. For example, the Stag Hunt game is defined by the rank order $R > T > P > S$. In this game, the reward for mutual cooperation trumps the temptation of unilateral defection (as $R > T$), hence conflict stemming from greed is eliminated from the game (but conflict stemming from fear remains as $P > S$). To summarize, the rank order of

¹ When originally introduced by Schelling (1958), 'mixed-motive games' referred to games involving both conflict of interests and mutual dependence, like the game of Chicken; however, usage has widened to include games that involve a conflict between mutual benefit and individual gain.

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outcomes defines different types of games.

Mixed-motive games differ in the degree of conflict of interests, both between and within types of games. The most prominent index of conflict of interests is the K index (Rapoport & Chammah, 1965; see Stivers, 2018, for an overview of different indices). The cooperation index $K = \frac{(R-P)}{(T-S)}$ formalises two intuitions: Higher benefits from cooperation (i.e., higher R and S) increase the index, and higher benefits from defection (i.e., higher T and P) decrease it. It can be shown formally that a lower degree of conflict of interests provides a greater *area of cooperation*—the area defined by the combination of social preferences and first-order beliefs for which cooperation is justifiable (Murphy & Ackermann, 2015). Consistent with this, empirical results show that cooperation rates are strongly positively correlated with indices of conflict of interests (Hilbig, Kieslich, Henninger, Thielmann, & Zettler, 2018; Rapoport & Chammah, 1965; Vlaev & Chater, 2006). In other words, games with different indices result in different behaviour. This suggests that an index of conflict of interests can help to advance the broader conceptual understanding of how people think about games.

Framing

The concept of framing encompasses a vast range of manipulations loosely held together by the shared notion that only the “surface structure” (Wagenaar, Keren, & Lichtenstein, 1988; cf. Hagen & Hammerstein’s, 2006, notion of “implicit features”) is manipulated while the “deep structure” (or “explicit features”; especially the outcome matrix) remains the same. To taxonomise the vast range of manipulations, Gerlach and Jaeger (2016; Gerlach et al., 2017) distinguish between three broad classes of frames: focal points, valence frames, and context frames. First, in games in which game theory is indeterminate because it leaves open multiple solutions, frames can provide focal points which players use to coordinate their choices (Bacharach & Bernasconi, 1997).

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Second, framing games in terms of gains and losses—so-called valence framing (Levin et al., 1998)—induces systematic variation in choice behaviour (e.g. Andreoni, 1990, 1995; for further conceptual clarification see Levin et al., 1998; for a meta-analysis of Public Goods games see Zelmer, 2003).

Finally, Gerlach et al. (2017) describe a class they call context frames. This includes manipulations that aim to induce certain motivations, such as Deutsch's (1957, 1958) exhortation to consider only one's own or, conversely, others' welfare, as well as frames that evoke certain social settings, such as Liberman et al.'s (2004) "community" and "stock exchange" descriptors. Other context frames highlight certain aspects of the player's identity, such as religion primes (e.g. Shariff & Norenzayan, 2007). Our central proposition in this manuscript is that these manipulations induce subjects to think differently about their interdependence with the other player(s), especially in terms of conflict of interests. We propose such perceptions of interdependence as a mechanism that underlies at least some, and potentially the vast majority of, context framing effects described in the literature.

Mechanistic explanations of context framing effects. Three major theoretical accounts of framing effects have arisen in the literature (Bernold, Gsottbauer, Ackermann, & Murphy, 2014; Ellingsen et al., 2012; Gerlach et al., 2017), namely the *coordination hypothesis*, the *variable sociability hypothesis*, as well as a less formalised *social norms* perspective.

First, the *coordination hypothesis* suggests that frames affect behaviour by altering beliefs about other players' behaviour (i.e., *first-order beliefs*, Ellingsen et al., 2012; Gerlach et al., 2017). Even players whose preferences are not altered by a frame may behave differently if they have certain social preferences and they come to believe that other players will react to the frame. In this case, frames serve as coordination

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devices for players with relevant social preferences (Dreber et al., 2013; Ellingsen et al., 2012; Gerlach & Jaeger, 2016). Such players condition their own behaviour on the behaviour of others, for example by cooperating only to the degree that they believe other players will cooperate (Fischbacher & Gächter, 2010; Fischbacher, Gächter, & Fehr, 2001). The effect is that the Prisoner's Dilemma is transformed into a Stag Hunt with pure strategy equilibria at mutual cooperation and mutual defection (Ellingsen et al., 2012). These players may draw on social context to infer others' likely behaviour, especially in the absence of better information. For example, a conditionally cooperative player (Fischbacher et al., 2001) may believe that their partner is more likely to cooperate in a "community game", and may consequently be willing to cooperate themselves. In this way, framing could lead to cooperation via first-order beliefs, as long as players have social preferences.

The second perspective proposes that frames affect behaviour by modulating players' social preferences (with Ellingsen et al., 2012, the *variable sociality hypothesis*; cf. McCusker & Carnevale, 1995; van Dijk & Wilke, 2000; Weber, Kopelman, & Messick, 2004). According to this view, preferences for cooperation are unstable across situations (see Levitt & List, 2007), and can be changed by the framing of a game. For example, it is a common claim that humans readily classify others as in- and out-group members, and are intrinsically motivated to behave altruistically towards in-group members (i.e., *parochial altruism*, Balliet, Wu, & de Dreu, 2014; Bernhard, Fehr, & Fischbacher, 2006; de Dreu, Balliet, & Halevy, 2014; Yamagishi & Mifune, 2016). If a frame induces such in-group favouring *parochial altruism* in a player, their preferences may thus change to positively or negatively value benefits obtained by the other, depending on their classification of the other player (Gerlach et al., 2017). In this way, framing may affect cooperative behaviour via frame-dependent variable social

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preferences.

Formalising both the coordination and variable sociality hypothesis, Ellingsen et al. (2012) show that for various values of a social preference parameter, a Prisoner's Dilemma may be transformed into another game, such as the Stag Hunt, in utilities. Alternatively, for players with (stable) social preferences, a Prisoner's Dilemma may be transformed into a Stag Hunt in utilities given a belief that the other player will cooperate (Ellingsen et al., 2012).

Norm theories form a third class of theories that aim to explain framing effects (Bicchieri, 2005; Biel & Thøgersen, 2007; Gerlach et al., 2017). According to these accounts, individuals first identify the given situation as an exemplar of some class of situations. Classification makes accessible a script—beliefs about actions that are appropriate in this class of situations. Once a script is activated, people follow the norm embedded in the script when they believe others to do so (first-order beliefs, in descriptive norms) and/or when following the rule is what they believe others expect them to do (second-order beliefs, in social norms). Context frames may lead people to identify a game as an exemplar of different classes of social situations (Bicchieri, 2005). For example, a “community game” may be identified as an instance of communal sharing, whereas a “stock market game” may be identified as a case of market pricing (Fiske, 1992). These different classes of situations may then come with different decision rules, leading to frame-induced changes in behaviour. In this way, norms accounts can capture all framing effects; at the same time, they do not specify distinct mechanisms (Gerlach & Jaeger, 2016; note also that Gerlach et al., 2017 do not identify outcomes that would falsify a social norms account). Indeed, insofar as people follow descriptive norms—i.e., conditioning only on their first-order beliefs—the account makes the same predictions as the coordination hypothesis.

Prior empirical tests of mechanistic accounts. Sequential games have been exploited to test the *coordination hypothesis*, as changes in first-order beliefs are a plausible mechanism only for simultaneous games. In games in which players move sequentially (i.e., the first player's choice is known to the second player, as in the Trust Game) or in which the player's outcomes are independent of the other player's choices (such as the Dictator Game), first-order beliefs seemingly cannot explain framing effects. Some studies have used this to experimentally compare the variable sociality and coordination hypotheses (Dreber et al., 2013; Ellingsen et al., 2012). Evidence from sequential games is mixed, but Gerlach et al. (2017) provide meta-analytic evidence of framing effects on Dictator Game giving, which speaks against first-order beliefs as the sole mechanism underlying context framing effects. However, framing effects on elicited beliefs are also well-documented (Gerlach et al., 2017). This opens up the possibility that beliefs may play a role even in non-strategic games where they should have no bearing on behaviour (cf. Rabbie, Schot, & Visser, 1989). We take this finding to suggest that first-order beliefs may explain some, but likely not all variation induced by context frames.

Most evidence favouring the *variable sociality hypothesis* comes from the observation of framing effects on behaviour in Dictator and sequential games, in which beliefs ought to be irrelevant (Gerlach et al., 2017). Frame-induced changes in Dictator Game giving, in particular, have been interpreted as evidence that frames affect social preferences (Dreber et al., 2013; Ellingsen et al., 2012; Gerlach et al., 2017). However, while such studies can show that frames do not (or at least not entirely) enter behaviour through beliefs, they only provide indirect support for the variable sociality hypothesis. Indeed, they do not exclude other explanations, such as social norms accounts (Gerlach et al., 2017) or a mechanism via perceptions of interdependence, as proposed here.

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To test the variable sociality hypothesis directly, Bernold et al. (2014) elicited social preferences using the social value orientation slider measure (Murphy, Ackermann, & Handgraaf, 2011). Social preferences were not affected by framing. However, this may be a matter of procedure: framing was implemented by varying the name of the game (between “Community” and “Wall Street”, as well as two other conditions, “Environment” and a neutral treatment), but social preferences were elicited before the social dilemma task was explained to the participants. Overall, there is little empirical work directly studying how framing affects social preferences, so this pathway remains understudied.

Framing Effects via Situation Perceptions

There has been surprisingly little work on how people think about the games they play (but see Butler, Burbank, & Chisholm, 2011). Experimentalists put significant effort into making sure that players correctly understand the structure of the game before making a decision (e.g., Guala, 2005), and consequently the common assumption has been that players directly interact with the deep structure of the experiment (Hagen & Hammerstein, 2006). Indeed, both the *variable sociality* and the *coordination hypothesis* tacitly assume that players accurately perceive their own material payoffs as well as those of the other player. Players are merely thought to transform the utility matrix under the influence of frames—such that a Prisoner’s Dilemma becomes, for example, a Stag Hunt game in utilities. In contrast, we will argue that framing affects players’ perception of conflict of interests—i.e. that framing leads players to think differently about the material payoff structure of the game. In this case, a player may perceive a Prisoner’s Dilemma described as a “stock exchange” game as containing a greater degree of conflict of interests with the other player than the same matrix

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described as a “community” game. The player then plays this transformed game rather than the game described by the explicit features communicated to them.

Recently developed models of how people think about situations (such as an economic game) posit that people draw on many features of their environment as well as internal states to construct a psychological situation on which they act (Balliet et al., 2017; Rauthmann et al., 2015). This psychological situation is not a matrix, but rather thought to be a set of dimensions on which situations—including games in the laboratory—differ. A number of different sets of dimensions have been proposed in recent years (Balliet et al., 2017; Gerpott et al., 2018; Parrigon, Woo, Tay, & Wang, 2017; Rauthmann et al., 2014). Functional Interdependence Theory—which draws on Interdependence Theory (Kelley et al., 2003; Kelley & Thibaut, 1978)—proposes a dimension of conflicting (vs. corresponding) interests that is explicitly thought to track the K index (Balliet et al., 2017). Empirical results support the importance of this dimension. For example, people can easily describe situations high or low in conflict of interest and readily rate daily life and laboratory situations—including experimental games—on a corresponding scale (Gerpott et al., 2018). Hence, we have good reasons to assume that perceptions of conflict of interests may indeed mediate the effects of explicit features of games on choice behaviour. For example, one study found that perceptions of conflict of interests fully explained the difference in cooperation rates between a Dictator Game and a Prisoner’s Dilemma (Gerpott et al., 2018). This supports the claim that (typical) players base their decision not directly on the payoff matrix of a game, but rather integrate the information in this matrix into variables

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representing the dimensions of interdependence. It is this *psychological situation* on which players base their decision.²

Our argument also builds on previous research on the role of illusions in people's decision to cooperate. Baron (1997) observed that when presented with scenarios of social dilemmas in which cooperation is not in one's (monetary) self-interest, participants would nevertheless insist that it was. This *self-interest illusion* is moderated by deep but irrelevant features of the game: When participants played a public goods game either with a single group or with two groups where one group's gain is the other group's loss (cf. Bornstein & Ben-Yossef, 1994), individuals were more likely to (erroneously) believe that cooperation with their in-group was in their self-interest and that they would gain more money if they contributed to the group's public good in the 'conflict' condition (Baron, 2001). In an extension of this work, we provide a perspective that integrates this single bias into a comprehensive account of situation perception and bring this perspective to bear on framing effects.

Formalising the situation perception mechanism. Previous work has shown that subjective perceptions of conflict of interests reflect differences in objective conflict of interest (i.e., deep structure) between different games, and may mediate the effect of such differences on behaviour. We propose that differences in the description of games (i.e., surface structure) are similarly integrated into the mental representation of the games, which becomes the basis for behaviour. For example, describing a Prisoner's Dilemma as a 'community game' may lead players to perceive the game as containing less conflict of interests, compared to a description of a 'stock exchange

² It is of course likely that players trained in game theory or otherwise not naive to these types of experiments may draw on this knowledge to make their decisions. Here, we speak only to the behaviour of naive participants.

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game'. This changes the area of cooperation—i.e., the degree of positive expectations and prosocial preferences required for a players to choose to behave cooperatively (Murphy & Ackermann, 2015).

		Player B	
		C	D
Player A	C	R R	T S
	D	S T	P P

		Player B	
		C	D
Player A	C	8 8	10 2
	D	2 10	4 4

Figure 1. The Prisoner's Dilemma. **A** The outcomes given both players' individual choices to cooperate or to defect are labelled R(eward), T(emptation), S(ucker's payoff), and P(unishment). **B** The Prisoner's Dilemma is a game such that $T > R > P > S$.

We first establish a simple model of cooperation under different degrees of conflict of interests. Consider the Prisoner's Dilemma given in Figure 1B. This matrix has parameters T (temptation) = 10, R (reward) = 8, P (punishment) = 4, and S (sucker's payoff) = 2; therefore, $K = 0.5$. This reflects the size of payoffs resulting from potential cooperation (i.e., R, S) relative to the size of payoffs resulting from potential defection (i.e., T, P), or in other words the severity of the conflict of interests. The degree of conflict of interests of a PD can affect a player's behaviour if (and only if) the player has positive social preferences (Murphy & Ackermann, 2015). Further, given a particular K index and a player's social preferences, the player's decision will also depend on their belief about whether the other player will cooperate (Murphy & Ackermann, 2015).

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There exist various models of social preferences. For the sake of simplicity, we adopt the model used in Murphy and Ackermann (2015). The joint utility function for a player attaches a single parameter α to the other player's payoff:

$$u(\pi_s, \pi_o) = \pi_s + \alpha \times \pi_o. \quad (1)$$

Here, π_s is the player's own payoff, π_o is the other player's payoff, and α is an index of social preferences.

Cooperation in the PD further requires a certain degree of trust; i.e., a belief β that the other player will also choose to cooperate. This belief must be sufficient so that the risk of unilateral defection is outweighed by the expected utility from mutual cooperation. Thus, Murphy and Ackermann (2015) propose a model of cooperation under social preferences and positive expectations. In this model, the expected utility from choosing to cooperate is

$$u(C) = [\beta \times (R + \alpha \times R)] + [(1 - \beta) \times (S + \alpha \times T)]. \quad (2)$$

The expected utility from choosing to defect is, in turn,

$$u(D) = [\beta \times (T + \alpha \times S)] + [(1 - \beta) \times (P + \alpha \times P)]. \quad (3)$$

These formulas allow the computation of values of α and β for which cooperation becomes favoured [i.e., $u(C) > u(D)$] given a specific PD matrix. The *area of cooperation* is the set of values (α, β) for which cooperation is favoured on any given matrix. Murphy and Ackermann (2015) observe that “as the K-index increases, the size of the cooperate area [...] generally increases as well” (see Fig. 2). In other words, games with a higher K index require weaker social preferences and/or less positive expectations to justify cooperation.

Figure 2. Correlation between K index and the area of cooperation. K indices were computed for games with $T = 1$, $S = 0$, and $0 \leq P < R \leq 1$ and players with $0 \leq \alpha \leq 1$ and $0 \leq \beta \leq 1$. The parameters P , R , α , and β were varied in steps of 0.1. Correlation $r = .976$. For methodological details, see supplementary materials.

Previous accounts of framing effects on cooperative behaviour have considered that either social preferences (i.e., α) or first-order beliefs (i.e., β) may be frame-dependent (Ellingsen et al., 2012). Here, we propose that conflict of interests also enters behaviour via a psychological mechanism and that perceptions of conflict of interests may equally be frame-dependent. Specifically, we suggest that people do not respond to the matrix directly, but rather integrate cues from the deep and surface structure of the game into an internal index of conflict of interests, which in turn informs behaviour (Balliet et al., 2017; Holmes, 2002; Rauthmann et al., 2015). We suggest that this internal index depends on both the degree of correspondence of the matrix as well as the surface structure of the game. One way to formalise this is by adding what we may call a “framing parameter” δ to the Murphy-Ackermann model which modulates the values

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in the K index formula such that the *perceived* K index is $K_\delta = \frac{(R+\delta_R)-(P-\delta_P)}{(T-\delta_T)-(S+\delta_S)}$ and the

expected utilities of cooperation and defection are

$$u(C) = \{\beta \times [(R + \delta_R) + \alpha \times (R + \delta_R)]\} + \{(1 - \beta) \times [(S + \delta_S) + \alpha \times (T - \delta_T)]\}; \quad (4)$$

$$u(D) = \{\beta \times [(T - \delta_T) + \alpha \times (S + \delta_S)]\} + \{(1 - \beta) \times [(P - \delta_P) + \alpha \times (P - \delta_P)]\}. \quad (5)$$

Values of $0 < \delta < \min(T - R, R - P, P - S)$ thus make the conflict of interests less severe (and cooperation more appealing), whereas values $\max(S - P, P - R, R - T) < \delta < 0$ make the conflict of interests more severe (and cooperation less appealing) while preserving the Prisoner's Dilemma's rank order $T > R > P > S$. For simplicity, we assume that $\delta_T = \delta_R = \delta_P = \delta_S$; i.e., the frame equally affects different motivational factors of the game. However, asymmetries can easily be incorporated in the model. Replacing the K index in Murphy and Ackermann's (2015) simulation with K_δ , which responds to changes in K as well as to changes in the surface structure δ , we can see that frames may affect the rate of cooperative choices via perceptions of conflict of interests, even when (positive) social preferences and first-order beliefs are stable (see supplementary materials). In the following studies, we draw on a measure of perceived conflict of interests, the conflict subscale of the Situational Interdependence Scale (Gerpott et al., 2018), to measure K_δ , test the degree to which it is influenced by framing effects, and probe perceptions of interdependence as a mechanism underlying framing effects on behaviour.

Current Studies

In the following, we present two studies. Study 1 was independently preregistered on the Open Science Framework (OSF) before submitting this

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manuscript.³ The study involves preregistered sequential analyses for two waves of data ($n_1 = 323$; $n_2 = 205$). Data for study 1 were collected and analysed according to the previously preregistered protocol. Study 2 is the proposed registered report. It is similar in design to study 1, but implements some changes. Data for study 2 will be collected and analysed according to the protocol proposed in this manuscript.

Study 1

Hypotheses

We propose an underlying general mechanism for a broad class of framing effects which have been classified as “context frames” (Gerlach & Jaeger, 2016; Levin et al., 1998). We test this by framing a Prisoner’s Dilemma as either a ‘community’ or ‘stock exchange’ game, expecting to replicate the effect of this frame on the rate of cooperation in a simultaneous one-shot game (Lieberman et al., 2004; Ross & Ward, 1996).

H1: Framing the Prisoner’s Dilemma as a Community rather than as a Stock Exchange Game causes a higher rate of cooperation.

We posit perceptions of conflict of interests as a mechanism underlying the effect of framing on cooperative behaviour. This means that framing will alter perceptions of conflict of interests, which in turn will lead to variation in cooperative behaviour.

H2: Perceptions of conflict of interests are lower when a Prisoner’s Dilemma is framed as a Community Game rather than as a Stock Exchange Game, and this mediates the effect of the framing treatment on cooperative choices in the Prisoner’s Dilemma.

³ The preregistration, materials, and analyses can be found under <https://osf.io/fm562/>.

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An alternative account is the *coordination hypothesis*, according to which framing affects behaviour via first-order beliefs (Ellingsen et al., 2012). We therefore elicit beliefs about the other player's behaviour and test for mediation by conflict of interests while controlling for beliefs as an alternative mechanism.

H3: Conflict of interests mediate the effect of framing on cooperative choices after controlling for mediation by beliefs.

We test for causal mediation of framing by perceived conflict of interests without (model 1) and with alternative mediation by first-order beliefs (model 2). Hypothesis 1 is tested by the average total effect in model 1. A failure to reject the null hypothesis of no total effect of framing treatment on cooperative choices will be interpreted as evidence against framing effects on cooperative behaviour in Prisoner's Dilemma games. Hypothesis 2 is tested by the average causal mediation effect in model 1. It is possible that there is a framing effect, but that perceptions of conflict of interests are not the underlying mechanism (i.e., no causal mediation effect in model 1). Therefore, in case we observe a significant average total effect but no significant average causal mediation effect, we will test the effect of framing treatment on perceptions of conflict of interests, including all covariates included in model 1. A failure to reject the null hypothesis of no effect of framing treatment on perceptions of conflict will be treated as evidence against effects of changes to the surface structure of games—such as framing—on perceptions of interdependence. Hypothesis 3 is the central test to evaluate the proposed mechanism and is tested by the average causal mediation effect in model 2.

Methods

Sample.

Sample size determination. The method for determining the final sample size is reported below. Techniques to estimate the power of tests using the proposed causal mediation analysis are not readily available. We therefore estimated the effect size for the total effect from the meta-analysis by Gerlach et al. (2017), which indicates an odds ratio of 1.467. The power to detect an indirect effect is generally larger than that to detect the total effect (Kenny & Judd, 2014; Loeys, Moerkerke, & Vansteelandt, 2015; Rucker, Preacher, Tormala, & Petty, 2011), so we base our sample size planning on this estimate.

We calculated the required sample size for a power of 80% and $\alpha = .05$, in line with common practices in the field, using the method described by Demidenko (2007), assuming equal assignment to treatments and a cooperation rate of 41.6% in the “stock exchange” treatment (based on Gerlach et al., 2017). This indicated a required sample size of $n = 859$, more than twice the size of the largest previous study on such framing effects.

Sequential analyses allow cost-efficient realisation of high-powered studies (Lakens, 2014). In such a procedure, interim analyses are specified a priori, and α thresholds are adjusted for multiple testing to ensure consistent type I error control. This means that despite multiple testing, the total type I error rate remains fixed at five percent under this procedure. We therefore preregistered sequential analyses on up to two waves of data. We computed two-sided symmetric Pocock-type bounds (Pocock, 1977) for $k = 2$ equally spaced time points and total $\alpha = .05$ using the R package ‘GroupSeq’ (Pahl, 2017; Pahl, Ziegler, & König, 2006). This results in $\alpha_{k=1} = .031$ and $\alpha_{k=2} = .019$. We adhered to these thresholds in significance testing for all analyses.

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Because the key analyses on wave 1 yielded a non-significant result, we collected and analysed two waves of data and report results using $\alpha = .019$ and corresponding 98.1% confidence intervals.

Sample characteristics. Because we relied on one-off classroom experiments for recruitment, we aimed to obtain data from 400-500 participants with each wave.

Unfortunately, participation fell short of this goal on both waves ($n_1 = 323$; $n_2 = 205$). Therefore, we only had 57% power to detect the hypothesised effect, and 80% power to detect an effect size of $OR = 1.668$.⁴ Because our preregistration specified the number of waves of data collection rather than the sample size as the decision rule for sequential analyses, we continue the analyses on this sample.

Participants were students in two cohorts of a first-semester course on business administration for non-majors. We specifically chose a first-semester class to assure that most participants are naive to the task. The samples showed no major differences in demographics (Wave 1: 70.59% female; $\bar{x}_{age} = 20.63$, $sd_{age} = 2.54$; Wave 2: 66.34% female, $\bar{x}_{age} = 20.46$, $sd_{age} = 2.28$). They were students in cognitive and media studies (56% and 53%), cultural industries (21% and 20%), Asian studies (10% and 11%), and various other social sciences. Most participants (77% and 66%) indicated that they did not know and had never participated in a Prisoner's Dilemma experiment; only 4% and 10%, respectively, indicated having participated in such an experiment before.

Materials. We aimed to replicate the effect of framing a Prisoner's Dilemma as either a "stock exchange" or a "community" game. This framing was first used by

⁴ Post-hoc power and sensitivity analyses following Demidenko (2007), based on 51.1% assigned to the community treatment and an observed cooperation rate of $\hat{p} = .577$ in the stock market treatment.

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Lieberman et al. (2004)⁵ and has been reproduced several times in recent years (Bernold et al., 2014; Brandts & Schwieler, 2009; de Haan & van Veldhuizen, 2015; Dreber et al., 2013; Ellingsen et al., 2012). For our purposes, we adapted instructions for the game from study 1 in Ellingsen et al. (2012), with additional instructions for the added measures. Participants received a sheet providing detailed instructions to the game, titled either “Community Game” (German: Gemeinschaftsspiel) or “Stock Exchange Game” (German: Börsenspiel). The Prisoner’s Dilemma uses a version of the payoff matrix used in Ellingsen et al. (2012, study 1) with payoffs given in Euro (Fig. 3). The choices were labelled ‘A’ and ‘B’, and players are labelled ‘You’ and ‘The other person’.

		You	
		A	B
The other person	A	€20 €20	€32 €2
	B	€2 €32	€8 €8

Figure 3. The payoff matrix in study 1.

Perceived interdependence was measured using the German version of the Situational Interdependence Scale (SIS; Gerpott et al., 2018). The scale contains five items each for the dimensions mutual dependence, conflict of interests, power, future interdependence, and information certainty, each measured on a five-point Likert scale (1 = completely disagree, 7 = completely agree). The conflict subscale contains items such as “Our preferred outcomes in this situation are conflicting.”

⁵ This study and this particular framing were previously reported in Ross and Ward (1996), but not published until much later.

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Internal reliability of the SIS was assessed using factor analysis of the non-imputed raw data set (function `fa()` with maximum likelihood estimation in the R package `psych`; Revelle, 2018). Exploratory factor analysis with five factors found that all but two items on the Information Certainty subscale loaded on the correct factor ($\Omega_t = .77$). Confirmatory factor analysis indicated acceptable model fit ($\chi^2(395) = 810.072$, $p < .001$; $CFI = 0.855$; $TLI = 0.840$; $RMSEA = 0.049$, $p = .652$).

Beliefs about the other player's behaviour were assessed using a single-item measure asking whether the other player was more likely to choose 'A' or 'B'. We did not incentivise belief elicitation. Although incentivised beliefs appear to be better predictors of subjects' own choices than mere introspection (Armantier & Treich, 2013; Gächter & Renner, 2010; Schlag, Tremewan, & van der Weele, 2015; Trautmann & van de Kuilen, 2014), they also engender biases, including those following from risk aversion, stakes, and hedging (Blanco, Engelmann, Koch, & Normann, 2010; Hossain & Okui, 2013; Offerman, Sonnemans, van de Kuilen, & Wakker, 2009). Here we followed the suggestion of Trautmann and van de Kuilen (2014) that in light of these biases incentivised elicitation does not show a clear advantage over unincentivised introspection.

Finally, we asked two questions about participants' prior knowledge of and experience with the economic game as well as their gender, age, and programme of study. All measures are disclosed above and the original materials are made available on the OSF. All manipulations are described in the following section.

Procedure. The procedure is designed to avoid, as much as possible, problems with causal interpretations of mediation analyses (e.g. Kline, 2015). The causal paths $X \rightarrow Y$ and $X \rightarrow M$ are assured by random assignment to treatments. The procedure is designed to preclude reverse causality between M and Y (i.e., $Y \rightarrow M$) by establishing

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temporal precedence. Confounding of this path is addressed in the analysis. Although temporal precedence does not assure that a causal interpretation is warranted, it supports it (Kline, 2015). Therefore, we aim to ascertain to the degree possible that reported situation perceptions precede the decision to cooperate.

Treatment								Proportion
1A	Community	Hypothetical	SIS	Reinstruction	Choice	Belief	Demographics	19.8%
1B	Stock market	Instruction						19.2%
2A	Community	Hypothetical	SIS	Reinstruction	Belief	Choice	Demographics	20.4%
2B	Stock market	Instruction						20.1%
3A	Community	Full	SIS		Belief	Choice	Demographics	9.6%
3B	Stock market	Instruction						10.8%

Figure 4. Order of treatments and measures.

The procedure and different order treatments are summarised in Fig. 4. In treatments 1A/B and 2A/B, participants were first instructed to imagine a hypothetical framed Prisoner's Dilemma game. This game is otherwise equivalent to the actual game. Subsequently, they reported their situation perceptions on the SIS. Then, participants were informed that they would actually play an incentivised game before providing their belief and choice. The order of the belief elicitation task and decision task were randomised.⁶ At the end, participants were asked about their prior knowledge of the task, age, gender, and the study programme in which they are enrolled.

⁶ Eliciting beliefs before actions are taken may focus the subject's attention, make them expend greater cognitive effort, and deepen their understanding of the situation, leading them to act in a more sophisticated fashion (Gächter & Renner, 2010; Schlag et al., 2014). Conversely, when beliefs are elicited after actions are taken, subjects may be swayed by consensus bias, the salience of their own choice, or a need to justify their choice towards the experimenter or themselves (Schlag et al., 2014). Unfortunately, empirical findings on this question so far are

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To be able to test whether the hypothetical scenario altered SIS responses, we also ran a control treatment in which participants were informed of the opportunity to play an incentivised game in the initial instructions (treatment 3A/B in Fig. 3). The column ‘proportion’ in Fig. 4 indicates the proportion of responses in each treatment.

The study was conducted as an in-class experiment before the start of a lecture on an unrelated topic. Participants received a brochure of eight sheets with their participant number, an informed consent form, the hypothetical instructions, SIS, reinstruction, belief elicitation task, decision task, and naivety check and demographics. Participants were instructed to be silent, to not communicate with other participants or to look at others’ instructions, and to not look at subsequent sheets before having provided their answer.

Each sheet was identified using a unique participant number. Participants were instructed to remove the cover page of the brochure and retain it as a coupon with their participant number. To make paying participants directly after the session feasible, 20 participants were chosen at random from all participants, their answers paired up with each other to compute their payoffs, and their participant numbers announced at the end of the class. Chosen participants received the sum in Euros indicated in the payoff matrix. Participants received their payment in envelopes with their participant number upon presenting their participant coupon. Due to tax regulations, participants were then asked to fill in a form with personal details confirming receipt of payments, fold it, and return it to the experimenter.

Analysis. Preregistered analyses on the first wave of data yielded a non-significant target effect (average causal mediation effect of conflict of interests when

scarce and equivocal (Schlag et al., 2014), such that counter-balancing of elicitation before and after action seems warranted.

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controlling for beliefs; see model 2). In the following, we report analyses on the full two-wave dataset. For analyses of the first wave only, please see the supplementary materials on the OSF. All analyses were performed in R (R Core Team, 2017).

Multiple imputation of missing data. As planned, we did not exclude subjects from the analyses. All returned response forms on which subjects completed at least one task or survey were included in the analysis ($n = 528$). Multiple imputation was performed to handle missingness at the item level.⁷ Specifically, we used the package 'mice' (van Buuren & Groothuis-Oudshoorn, 2011) in R to perform predictive mean matching to impute numeric variables (SIS ratings) and logistic regression imputation to impute binary variables (beliefs, choice, naivety). We imputed five datasets using a maximum of five iterations.

Testing for order effects. Contrasts between order conditions were used to test for (a) an order effect on beliefs and choices and (b) effects of hypothetical rather than actual instruction on situation perceptions. To test (a), we ran contrasts between conditions 1A/B versus 2A/B and evaluate the effect of the contrast on choice and belief as well as the interaction between contrast and treatment. To test (b), we ran contrasts between conditions 1A/B and 2A/B versus 3A/B and evaluated the effect of the contrast on SIS Conflict ratings as well as the interaction between contrast and treatment. These tests indicated a negative effect of hypotheticality on choice as well as an interaction between hypotheticality and the framing treatment (Table 1), but neither an order effect of choice and beliefs nor a hypotheticality effect on perceptions of conflict. Thus, we continued with the planned analyses.

⁷ Missingness ranged from 0.2% to 2.3% per item, with exception of age (8.3% missing).

Table 1. *Effects of treatment order and hypotheticality on a) decisions, b) beliefs, and c) perceptions of conflict of interests.*

Decision					
Predictor	<i>B</i>	<i>SE</i>	<i>t</i>	<i>df</i>	<i>p</i>
(Intercept)	0.568	0.090	6.320	518.768	<.001
Treatment	-0.379	0.123	-3.093	503.924	0.002
Hypothetical	-0.562	0.223	-2.520	519.614	0.012
Order	0.168	0.175	0.962	516.266	0.337
Treatment:Hypothetical	0.609	0.297	2.051	509.466	0.026
Treatment:Order	-0.396	0.250	-1.587	511.141	0.113
Belief					
(Intercept)	0.416	0.084	4.941	511.287	<.001
Treatment	-0.083	0.121	-0.685	503.318	0.494
Hypothetical	-0.131	0.202	-0.644	519.151	0.520
Order	0.113	0.174	0.649	512.442	0.517
Treatment:Hypothetical	-0.369	0.291	-1.269	499.355	0.205
Treatment:Order	-0.346	0.249	-1.391	510.817	0.165
Perceived Conflict					
(Intercept)	4.133	0.050	83.303	519.299	<.001
Treatment	0.212	0.071	3.007	518.301	0.003
Hypothetical	0.083	0.119	0.704	518.273	0.482
Order	0.148	0.104	1.420	518.786	0.156
Treatment:Hypothetical	-0.147	0.168	-0.877	511.408	0.381
Treatment:Order	-0.115	0.150	-0.767	517.636	0.444

Results

Descriptive results. Based on the non-imputed data, participants cooperated more when the game was framed was a community game (cooperation rate $\hat{p} = .684$) than when it was framed as a stock exchange game ($\hat{p} = .577$; OR = 1.586). They had more positive expectations (rate of cooperative expectations, $\hat{p} = .653$ vs. $\hat{p} = .610$; OR = 1.202) and perceived the game as containing less conflict of interests ($\bar{x} = 4.156$, $SD = .757$ vs. $\bar{x} = 4.320$, $SD = .777$; Cohen's $d_s = .214$). Zero-order correlations between all treatments and measured variables are shown in Table 2.

Mediation analyses. To test the total treatment and the indirect effect, we used a mediation approach within the potential outcomes framework, which allows for a causal interpretation under the assumption of sequential ignorability (Imai, Keele, & Tingley, 2010; Kline, 2015). Although this assumption cannot be tested, we report sensitivity

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analyses which indicate the robustness of the presented model to confounding by unobserved variables. The key sensitivity parameter is ρ , the correlation of the residuals of the mediator and outcome regressions; the analysis indicates the level of ρ for which the average causal mediation effect is zero. Sensitivity analyses were performed for the mediation model fitted to each imputed dataset separately and the estimated sensitivity parameters for each model were averaged.

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Table 2. Zero-order correlations (lower triangle) and corresponding 95% confidence intervals for treatment and measured variables.

	Treatment	Beliefs	Decision	Conflict	Gender	Age	Know.	Part.	Wave
Treatment		[-.13, .04]	[-.19, -.03]	[.02, .19]	[-.09, .08]	[-.12, .06]	[<.01, .17]	[-.01, 0.16]	[-.12, .05]
Beliefs	-.04		[.26, .41]	[-.24, -.07]	[-.06, .11]	[-.02, .16]	[-.11, .06]	[-.19, -0.02]	[-.02, .15]
Decision	-.11	.34		[-.31, -.14]	[-.06, .11]	[-.03, .14]	[-.06, .11]	[-.19, -0.02]	[-.06, .11]
Conflict	.11	-.15	-.22		[-.11, .07]	[-.18, <.01]	[-.11, .07]	[-.07, 0.11]	[-.07, .11]
Gender	-.01	.02	.03	-.02		[-.16, .02]	[-.04, .13]	[.01, 0.19]	[-.10, .08]
Age	-.03	.07	.06	-.09	-.07		[-.03, .14]	[-.07, 0.11]	[-.12, .06]
Knowledge	.08	-.02	.02	-.02	.04	.05		[.24, 0.39]	[-.20, -.04]
Participation	.08	-.10	-.10	.02	.10	.02	.32		[-.19, -.02]
Wave	-.03	.07	.03	.02	-.01	-.03	-.12	-.11	

Note: Knowledge indicates whether participants stated that they had prior knowledge about the Prisoner's Dilemma; Participation indicates whether participants stated that they had previously participated in an economic games such as the Prisoner's Dilemma.

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The key hypothesis was that perceptions of conflict of interests mediate the effect of framing on cooperative behaviour. This hypothesis was tested by causal mediation analysis using the package ‘mediation’ for R (Imai et al., 2010; Tingley, Yamamoto, Hirose, Keele, & Imai, 2014). In model 1, we entered frame as a dichotomous treatment variable, perceived conflict of interest as a continuous mediator, and the participants’ choices as a dichotomous outcome variable. To predict choices we used probit regression to allow for sensitivity analysis. We also control for naivety and wave. This model shows a significant total ($B = -.099$, 98.1% CI = $[-.144, -.057]$, $p < .001$) and average causal mediation effect ($B = -.025$, 98.1% CI = $[-.036, -.016]$, $p < .001$) as well as a significant average direct effect ($B = -.074$, 98.1% CI: $-.118, -.032$; $p < .001$; see Table 3). Perceptions of conflict account for 25.6% of the total framing effect. Sensitivity analysis indicated that the average causal mediation effect would be 0 when $\rho = -0.2$; i.e., for a confound inducing a correlation of -0.2 between the residuals of the mediator and the outcome regression. We also tested for heterogeneity of variances using plots of residuals versus fitted values using fitted regression models for the mediator and outcome variables; these plots were generated for each of the imputed datasets separately. Plots did not indicate heteroscedasticity for either model 1 or model 2.

Table 3. *Estimated average causal mediation, direct, and total effects and 98.1% confidence intervals. Model 1 tests mediation of the framing effect on cooperation by conflict of interests. Model 2 controls for mediation by first-order beliefs.*

	Model 1			Model 2		
	Estimate	CI Lower	CI Upper	Estimate	CI Lower	CI Upper
ACME	-.025	-.036	-.016	-.035	-.104	.033
ADE	-.074	-.118	-.032	-.092	-.202	.019
Total	-.099	-.144	-.057	-.100	-.195	-.003

Note: ACME = average causal mediation effect; ADE = average direct effect.

In model 2, we tested whether this mediation holds even after accounting for beliefs about the other players’ behaviour as an alternative mediator. Beliefs were

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entered as an alternative mediator into a multiple mediation model based on the method developed by Imai and Yamamoto (2013; Tingley et al., 2014). This method allows us to model beliefs as a second mediator that is potentially causally related to the main mediator of interest. Because pooling of estimates obtained on multiply imputed data sets is not supported by available software, we averaged the estimates and confidence intervals for each effect. Controlling for an alternative mediating path, the indirect effect via perceived conflict was non-significant ($B = -.035$, 98.1% CI = $[-.104, .033]$), as was the direct effect ($B = -.092$, 98.1% CI = $[-.202, .019]$), though the total effect remained significant ($B = -.100$, 98.1% CI = $[-.195, -.003]$; Table 3). Since no significant average causal mediation effect was discovered, we did not further test for robustness of the mediation effect.

Exploratory analyses

Predictors of cooperation. The preregistered analyses showed an indirect effect of frames on cooperative behaviour via perceived conflict of interests; however, this effect was not robust to inclusion of beliefs as an alternative mediator. We therefore explored the effects of beliefs and perceptions of conflict on cooperative behaviour. For the exploratory analyses, we report 95% confidence intervals. We tested whether beliefs mediated the effect of framing on cooperative decisions, using a model analogous to model 1 with beliefs as the mediator. This yielded a significant average causal mediation effect (Table 4) which accounts for 14.3% of the total effect.⁸ Furthermore, we tested the individual paths of model 1 separately. This analyses showed that the framing affected perceptions of conflict of interests ($B = 0.192$, $SE = 0.067$, $t(520.522) = 2.869$, $p = 0.004$). Moreover, individual differences in the perception of conflict of

⁸ Note that sensitivity analyses are not available for this type of model.

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interest predicted cooperative decisions (Table 5, model 1). After also entering beliefs, this effect remained significant. Beliefs, in addition, strongly predicted the rate of cooperation (Table 5, model 2).

Table 4. *Results of a mediation model with mediation by beliefs only.*

	Estimate	CI Lower	CI Upper	<i>p</i>
ACME	-.015	-.030	-.001	.034
ADE	-.088	-.123	-.053	< .001
Total Effect	-.103	-.142	-.064	< .001

Note: ACME = average causal mediation effect; ADE = average direct effect.

Further dimensions of interdependence. Interdependence Theory (Kelley et al., 2003) describes people's interdependence in a given situation along multiple dimensions, of which conflict of interests is just one. We measured perceptions of conflict of interests using the Situational Interdependence Scale, which also contains subscales for four other dimensions of interdependence. Mutual dependence, in particular, is the degree to which interaction partners can control each other's outcomes through their choices. This dimension of interdependence may also be evoked by the reference to community in the frames we used. However, mutual dependence showed a non-significant average causal mediation effect ($B = -.004$, 95% CI = $[-.009, <.001]$, $p = .052$), with a significant average direct effect ($B = -.097$, 95% CI = $[-.135, -.061]$, $p < .001$) and total effect ($B = -.101$, 95% CI = $[-.140, -.065]$, $p < .001$). Controlling for beliefs led to a non-significant average causal mediation effect ($B = .015$, 98.1% CI = $[-.021, .051]$) while the average direct effect ($B = -.107$, 98.1% CI = $[-.195, -.018]$) and total effect ($B = -.101$, 98.1% CI = $[-.179, -.019]$) remained significant.

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Table 5. *Results of regression models predicting the rate of cooperation from covariates and only perceptions of conflict (model 1) as well as also beliefs (model 2).*

	Model 1					Model 2				
	<i>B</i>	<i>SE</i>	<i>t</i>	<i>df</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>t</i>	<i>df</i>	<i>p</i>
Intercept	1.857	.369	5.028	459.85	<.001	1.247	.390	3.199	452.24	.001
Treatment	-.201	.115	-1.743	495.38	.082	-.196	.119	-1.643	502.02	.101
Conflict	-.372	.078	-4.772	451.22	<.001	-.326	.081	-4.035	421.75	<.001
Naivety	.237	.132	1.794	428.37	.074	.185	.138	1.342	407.24	.180
Wave	.076	.119	.643	516.12	.521	.032	.123	.264	516.96	.792
Belief						.819	.122	6.735	479.33	<.001

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Discussion

We replicate the effect of framing a Prisoner's Dilemma as either a 'stock market' or 'community' game on rates of cooperation. Framing the game as a community game lead to an 11% point increase in cooperation relative to describing it as a stock exchange game. This effect is in line with the effect size reported in the meta-analysis of Gerlach et al. (2017), although the overall rate of cooperation was somewhat higher in our study.

As predicted, perceptions of conflict of interests mediated the effect of framing the Prisoner's Dilemma on the rate of cooperative decisions. Framing significantly swayed participants' perception of the game as involving a conflict of interests, and these perceptions translated into lower rates of cooperation under the 'stock exchange' frame. When we controlled for participants' beliefs, however, this indirect effect became statistically non-significant.⁹ This may mean that perceptions of conflict are a spurious side-effect, while frames enter behaviour via first-order beliefs. However, it is also possible that perceptions of conflict of interests and beliefs are chained causal mechanisms. We further test this possibility in study 2.

Study 2

Our results from study 1 raise the question how perceived conflict of interests relates to the belief that one's interaction partner will behave cooperatively. Perceived conflict and beliefs showed a small zero-order correlation ($r = -.15$, 95% CI = $-.24, -.07$). The most plausible explanation is that perceptions of higher conflict of interests lead to more negative expectations. After all, we have argued here that perceiving a conflict of interests should reduce, *ceteris paribus*, people's willingness to behave cooperatively.

⁹ Notably, this was not because the unstandardized effect was reduced in size, but because the standard error of the indirect effect was larger in the second model.

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Thus, perceptions of conflict of interests may affect behaviour via two routes: Once by reducing the willingness to behave cooperatively directly, and once by inducing more negative expectations about others' likely behaviour. Such a chained mechanism cannot be distinguished from confounding based on the data from study 1.

A solution to confounding by post-treatment covariates is to also experimentally manipulate the mediator. Specifically, in the parallel encouragement design, one half of the participants completes the standard experiment with a measured mediator. The other half undergoes a second manipulation, which randomly “encourages” them to take a high (vs. low) value on the mediator (Imai, Tingley, & Yamamoto, 2013). The parallel encouragement design identifies the (unconfounded) causal mediation effect under a set of plausible assumptions (Imai & Yamamoto, 2013). We thus experimentally manipulated perceived conflict of interests in a parallel encouragement design to test the causal mediation effect.

Another limitation of this first study is that beliefs and decisions were both elicited through binary choice tasks. Especially because participants were presented with these tasks directly after each other, it is likely that this introduced common method bias—i.e., the association between beliefs and decisions was inflated because they were measured in similar ways. Therefore, we used a continuous Prisoner's Dilemma game and elicited beliefs using the most likely interval rule (Schlag & van der Weele, 2015) to avoid common method bias.

Methods

Sample

Sample size determination. We computed the sample size necessary to detect the indirect effect due to conflict of interests for $\alpha = .05$ and $\beta = .10$ using the MedPower app (Kenny, 2017). Entering standardised regression coefficients for the a (β

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$= .125$), b ($\beta = -.500$), and c' ($\beta = -.150$) paths obtained from the study 1 data suggested a necessary $N = 666$ for 90% power on the indirect effect. To account for uncertainty in the effect size estimate, we aimed to collect 1.5 times the minimum sample necessary for 90% power, i.e., $N = 999$ participants.

Sample characteristics. Participants were recruited through a German online survey panel. This allowed us to obtain a demographically diverse sample that has not yet been exposed to economic games. We excluded participants who did not complete the entire survey and used forced response to avoid item-level missingness.

1468 participants started the survey. Of these, 1088 participants (54.2% female, 45.4% male, 0.4% other/prefer not to say; $\bar{x}_{\text{age}} = 45.30$, $sd_{\text{age}} = 19.65$) completed the full survey. The difference to the target sample size is due to oversampling on the side of the survey agency. Participants were largely naïve to the task; 86.03% did not know about the game before, and 90.90% had never participated in a game.

Materials. We again aimed to replicate the effect of framing a Prisoner's Dilemma as either a "stock exchange" or a "community" game. In contrast to study 1, we used a continuous Prisoner's Dilemma game in the exchange format (Verhoeff, 1998; Yamagishi & Kiyonari, 2000; Yamagishi, Terai, Kiyonari, Mifune, & Kanazawa, 2007). In this form of the Prisoner's Dilemma, each participant receives an endowment $E_i = 10\text{€}$, of which they can transfer any sum $0 \leq T_i \leq E_i$ to the other player. The transferred sum T_i is doubled. Cooperation is thus a continuous rather than a dichotomous variable. This is convenient because it allowed us to use different measures for decisions and belief elicitation, rather than to elicit both through binary choice tasks. This serves to reduce common method bias that might inflate the association between beliefs and choices.

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Framing was implemented by describing the game as either a “community game” or a “stock market game”, as in study 1. We manipulated perceptions of conflict of interests by adding a line to the description of the game emphasising conflict of interests (“Therefore, you and the other player cannot both obtain your most profitable outcome.”) or correspondence of interests (“Therefore, you and the other player can jointly obtain your most profitable outcome.”).

Perceived conflict of interests was measured using an adaptation of the conflict subscale of the German version of the Situational Interdependence Scale (SIS; Gerpott et al., 2018). The items on the original subscale of the SIS frequently refer to “preferences” or “preferred outcomes”, which might lead to confounding between perceptions of conflict of interests and other-regarding preferences. To address the concern that this might have influenced the results of study 1, we therefore rewrote the items to refer to “best” or “most profitable” outcomes (Appendix 1). The scale showed acceptable reliability ($\Omega_t = .68$).

Beliefs about the other player’s behaviour were assessed using the most likely interval (MLI) elicitation rule (Schlag & van der Weele, 2015). Using this rule, the participant is paid based on the width of the specified interval and on whether or not the eventual outcome falls within the interval. The scoring rule can be varied by a parameter γ , which specifies the degree to which a subject is penalised for providing a wider interval. When subjects state their beliefs about the value of x on the domain $[a, b]$ as the interval $[L, U]$, which has width W , the payment received is denoted as $S_M(L, U, x)$, which depends on W and γ as follows:

$$S_M(L, U, x) = \begin{cases} \left(1 - \frac{W}{b-a}\right)^\gamma & \text{if } x \in [L, U] \\ 0 & \text{if } x \notin [L, U] \end{cases} \quad (6)$$

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where $g = (1-\gamma)/\gamma$. Thus, when $\gamma = .5$, payment is linearly decreasing with the width of the stated interval as a fraction of the domain of x . γ can be interpreted as the minimum confidence level, though the actual degree of confidence may be greater than γ if the subject is risk averse. Because risk-averse subjects can choose to specify a wider interval, the MLI rule is valid for all degrees of risk aversion of the subject.

We used the MLI rule with $\gamma = .5$ and maximum $S_M = 10\text{€}$ to elicit participants' beliefs about the amount transferred by the other player T_j . The midpoint of the elicited interval was used as the location of beliefs (Cettolin & Riedl, 2011; Schlag & van der Weele, 2015), i.e. as the variable to be used in our statistical analyses. Participants were prompted to report an interval by using a slider to indicate the lowest and highest value they expected. They were told that if their interval contained the decision of the matched participant, they could earn a bonus whose size depended on the width of the interval. When moving the sliders, participants were shown the bonus they would receive in case the interval contained the true value.

Finally, we also elicited social preferences using a variant of the strategy method (Fischbacher et al., 2001; Fischbacher, Gächter, & Quercia, 2012). This method is used to distinguish conditional co-operators from free-rider types. Participants decide on their conditional transfer $0 \leq T_{Ci} \leq E_i$ for each integer transfer of the other player $T_j = \{0\text{€}, 1\text{€}, \dots, 10\text{€}\}$. In line with Fischbacher et al. (2012), we define conditional co-operators as having a positive Spearman's rank correlation coefficient between the other player's transfer and their own conditional transfer, significant at the 1% level, or a schedule that increases monotonically with the other player's transfers and shows at least one increase. Free-riders transfer exactly zero for each potential transfer. All other types are undefined.

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Finally, we included 16 items from the Honesty-Humility subscale of the HEXACO-PI-R 100 for use in a different study, two questions about participants' prior knowledge of and experience with the economic game as well as their gender, age, and highest level of education. All measures are disclosed above and the original materials are made available on the OSF. All manipulations are described in the following section.

Procedure. The procedure followed treatment 1a/b of study 1, since we did not observe relevant order and hypotheticality effects in study 1. Participants were first presented with instructions to a hypothetical continuous Prisoner's Dilemma game, which was described as either the "community" or the "stock market" game. Orthogonally, we manipulated perceived conflict of interests (low, high, or no manipulation). Then, participants rated this game on the SIS. Subsequently, they were informed that they would play this game with another participant. They first indicated their beliefs about the other player's transfer in the continuous Prisoner's Dilemma game, and then provided their own decision. After providing their unconditional decision, participants were asked to make their conditional transfer decisions. Subsequently, participants completed an unrelated personality scale and were asked two questions about their knowledge of and prior participation in economic games to assess their naivety.

We used three lotteries to pay participants for their beliefs and decisions. For the decisions, as in study 1, one in 25 participants was randomly selected for payoff (44 total). Half of these participants were paid for the unconditional and half for their conditional transfers (conditional on their matched partner's unconditional decision). Participants were matched to each other within treatments and paid their earnings, which could range from €0 to €30 (average earnings €15.95). For the beliefs, an

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additional 1 in 50 participants was randomly selected for payoff according to the MLI rule, which could range from €0 to €10 (average earnings €1.27).

Analysis. All analyses were performed in R (R Core Team, 2017).

Results

Descriptive results. Participants cooperated slightly more when the game was framed as a community game than when it was framed as a stock exchange game ($\bar{x} = 6.033$, $SD = 3.724$, $n = 584$ vs. $\bar{x} = 5.839$, $SD = 3.634$, $n = 504$; ANOVA: $F(1, 1086) = .745$, $p = .388$, Cohen's $d_s = .055$). They also perceived the game as containing less conflict of interests ($\bar{x} = 4.002$, $SD = .736$ vs. $\bar{x} = 4.068$, $SD = .738$; ANOVA: $F(1, 1086) = 3.564$, $p = .059$, Cohen's $d_s = .117$), had more positive beliefs ($\bar{x} = 4.527$, $SD = 2.609$ vs. $\bar{x} = 4.501$, $SD = 2.649$; ANOVA: $F(1, 1086) = .023$, $p = .880$, Cohen's $d_s = .014$), and were more likely to be classified as both free-riders and conditional co-operators (free-riders: $\hat{p} = .057$ vs. $\hat{p} = .048$, conditional co-operators: $\hat{p} = .606$ vs. $\hat{p} = .593$, rest undefined; $X^2(2) = .853$, $p = .653$). Notably, all effect sizes are non-significant and smaller than in study 1.

The orthogonal manipulation of perceived conflict of interests was successful (ANOVA: $F(2, 1085) = 21.58$, $p < .001$, $\eta_p^2 = .04$). The manipulation had much weaker effects on cooperative behaviour (ANOVA: $F(2, 1085) = 2.533$, $p = .080$, $\eta_p^2 = .01$), beliefs (ANOVA: $F(2, 983) = 3.206$, $p = .041$, $\eta_p^2 = .01$), and social preferences ($X^2(4) = 3.330$, $p = .504$). Zero-order correlations between all treatments and measured variables are shown in Table 6.

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Table 6. Zero-order correlations (lower triangle) and corresponding 95% confidence intervals for treatment and measured variables.

	Frame	Mediator	Decision	Belief	Preference	Conflict	Gender	Age	Naivety
Frame		[-.09, .03]	[-.09, .03]	[-.07, .06]	[-.05, .09]	[.00, .12]	[-.05, .07]	[-.06, .06]	[.00, .12]
Mediator	-.03		[-.12, -.01]	[-.14, -.02]	[-.09, .06]	[.13, .25]	[-.05, .06]	[-.12, .00]	[-.02, .09]
Decision	-.03	-.07		[.28, .39]	[.41, .53]	[-.24, -.12]	[-.11, .01]	[.00, .12]	[-.06, .05]
Belief	.00	-.08	.34		[.21, .35]	[-.17, -.04]	[-.09, .03]	[-.01, .11]	[-.10, .03]
Preference	.02	-.01	.47	.28		[-.16, -.01]	[-.02, .13]	[-.09, .06]	[-.07, .08]
Conflict	.06	.19	-.18	-.10	-.08		[-.07, .05]	[-.07, .05]	[.00, .12]
Gender	.01	.00	-.05	-.03	.06	-.01		[-.13, -.01]	[-.12, .00]
Age	.00	-.06	.06	.05	-.02	-.01	-.07		[-.18, .06]
Naivety	.06	.03	-.01	-.03	.01	.06	-.06	-.12	

Note: Mediator = mediator manipulation treatment; H-H = Honesty-Humility; Conflict = perceived conflict of interests.

Confirmatory analyses. The key hypothesis was that perceptions of conflict of interests mediate the effect of framing on cooperative behaviour. This hypothesis was tested by causal mediation analysis of the parallel encouragement design using two-stage least squares regression (Imai, Keele, Tingley, & Yamamoto, 2011; Kleibler & Zeileis, 2008). In step one, we regressed the observed mediator M_i (i.e., perceived conflict of interests) on the treatment T_i and the instrumental variable Z_i . In step two, we then regressed the transferred amount Y_i on the treatment T_i and the predicted values for M_i :

$$M_i = \alpha_2 + \beta_2 T_i + \lambda Z_i + \varepsilon_{i2} \quad (7)$$

$$Y_i = \alpha_3 + \beta_3 T_i + \gamma M_i + \varepsilon_{i3} \quad (8)$$

The average causal mediation effect and average direct effect are estimated by $\hat{\beta}_2 \hat{\gamma}$ and $\hat{\beta}_3$, respectively. We used ordinary nonparametric bootstrap to obtain the confidence interval of the indirect effect using the adjusted bootstrap percentile method (Canty & Ripley, 2017). Note that with a parallel encouragement design, it is no longer necessary to explicitly account for alternative mediators.

The indirect effect via perceived conflict of interests was significant ($b = -.136$, 95% CI = $[-.467, -.006]$), whereas the direct effect of framing was non-significant ($b = -.067$, $SE = .231$, $t(1085) = -.288$, $p = .773$). The total effect of framing was non-significant ($b = -.193$, $SE = .224$, $t(1086) = -.863$, $p = .388$). We report near-identical results only considering naïve subjects in the supplementary material.

We also tested the indirect effects via beliefs (model 2) and via preferences (model 3). Since we did not manipulate beliefs and preferences, we entered beliefs or preferences as mediators into a causal mediation model using the R package ‘mediation’ (Tingley et al., 2014). We used frame as treatment variable and decisions as the outcome variable. For these analyses, Imai et al. (2010) describe a sensitivity parameter,

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the correlation ρ between the residuals of the mediator and the outcome regression for which the average causal mediation effect becomes zero. There was no significant average causal mediation effect via beliefs ($b = -.012$, 95% CI = $[-.171, .148]$, $p = .888$, $\rho = .3$) nor via preferences ($b = .072$, 95% CI = $[-.242, .376]$, $p = .620$, $\rho = .9$; Table 7). Finally, we tested the indirect effect of beliefs accounting for perceived conflict of interests as an alternative mediator. Again, the average causal mediation effect was non-significant ($b = -.012$, 95% CI = $[-.155, .132]$).

Table 7. *Average causal mediation, average direct, and total effects for mediation by beliefs and preferences.*

	Beliefs			Preferences		
	Est.	CI	<i>p</i>	Est.	CI	<i>p</i>
ACME	-.012	-.171, .148	.888	.072	-.242, .376	.620
ADE	.219	-.676, .207	.350	-.256	-.755, .230	.288
Total	-.231	-.695, .244	.348	-.184	-.771, .395	.520

Exploratory analyses. Because we orthogonally manipulated the mediator, we were able to test whether perceptions of conflict of interest had a causal effect on prosocial behaviour. For this purpose, we entered the mediator manipulation as predictor, perceived conflict of interest as mediator, and cooperative behaviour as outcome into a causal mediation model using the R package ‘mediation’ (Tingley et al., 2014). The results indicated a significant average causal mediation effect ($b = -.295$, 95% CI = $[-.440, -.176]$, $p < .001$, $\rho = -.2$) and non-significant direct effect ($b = -.278$, 95% CI = $[-.805, .238]$, $p = .296$). The average causal mediation effect accounted for 50.1% (95% CI = $[19.3, 286.5\%]$) of the significant total effect ($b = -.574$, 95% CI = $[-1.102, -.053]$, $p = .028$). These results held when controlling for beliefs as an alternative mediator, indicating a significant average causal mediation effect ($b = -.239$, 95% CI = $[-.384, -.094]$), non-significant direct effect ($b = -.359$, 95% CI = $[-.927, .209]$), and significant total effect ($b = -.596$, 95% CI = $[-1.161, -.047]$).

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Both perceived conflict of interests and beliefs are affected by the manipulation of perceived conflict of interest. We therefore tested whether the changes in beliefs are plausibly a downstream consequence of the change in perceptions of conflict of interests. We entered the mediator manipulation as predictor, perceived conflict of interest as mediator, and cooperative behaviour as outcome into a causal mediation model. This model revealed a significant average causal mediation effect ($b = -.107$, 95% CI = $[-.197, -.031]$, $p = .002$, $\rho = .1$) and average direct effect ($b = -.413$, 95% CI = $[-.816, -.011]$, $p = .042$). Perceptions of conflict of interests explained 20.6% (95% CI = $[4.6\%, 87.9\%]$) of the significant total effect ($b = -.520$, 95% CI = $[-.928, -.118]$, $p = .006$).

Discussion

In study 2, we set out to show that framing a Prisoner's Dilemma as either a community game or a stock exchange game affects prosocial behaviour through a causal mediation by perceived conflict of interest, rather than changes in beliefs or preferences. In contrast to study 1, there was only a very small, non-significant framing effect. However, part of this framing effect was accounted for by perceived conflict of interests, and this indirect effect was statistically significant. In contrast, indirect effects via beliefs or preferences were non-significant. Hence, findings support our mechanistic account according to which frames enter behaviour via perceptions of conflict of interests, though we find no evidence to support an overall framing effect on cooperative behaviour.

Incidentally, our design comprised an orthogonal manipulation of perceptions of conflict of interests using a second, more targeted frame. This allowed us to test the causal effect of perceptions of conflict of interests on cooperative behaviour. Indeed, induced perceptions of conflict of interests predicted prosocial behaviour, and did so

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even when controlling for first-order beliefs as an alternative mechanism. As in study 1, beliefs and preferences were correlated, and both were changed by the conflict frame. However, in study 2, it is much more plausible to interpret the changes in first-order beliefs as a downstream consequence of changes in perceptions of conflict of interests. Indeed, we find evidence for such a mechanism, although the results are not very robust to violations of key assumptions needed for a causal interpretation, as indicated by the small sensitivity parameter ($\rho = .1$).

Overall Discussion

Mere changes in the way a choice is presented can affect whether or not a person behaves cooperatively or defects—such context framing effects are well-supported in the economic and psychological literature (Gerlach et al., 2017). In recent years, a number of mechanisms have been proposed to explain how describing a game as a ‘Community’ rather than a ‘Stock Market’ game might lead to more cooperative choices. Most prominent among these are accounts positing framing-induced changes in first-order beliefs and in social preferences (Ellingsen et al., 2012). Here, we sought to test an alternative account: That frames change how people think about games, and specifically, the degree of conflict of interests people perceive in a game.

In study 1, we replicate the ‘Community’ vs. ‘Stock Market’ framing effect on cooperation in the Prisoner’s Dilemma. We also find evidence that perceived conflict of interests, as well as first-order beliefs, can explain at least part of this framing effect. However, our results are inconclusive about the relationship between perceived conflict of interests and first-order beliefs. In study 2, we find no support for an overall framing effect on cooperative behaviour in the continuous-choice Prisoner’s Dilemma. However, the results show that framing affected cooperative behaviour by changing

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perceptions of conflict of interests. In contrast, our data do not support a mechanism via either first-order beliefs or social preferences.

Across both studies, we elicited three possible mechanisms: perceived conflict of interests, first-order beliefs, and—only in study 2—social preferences. We find a non-significant effect of framing on beliefs in both studies, and in study 2 also on social preferences. In contrast, framing the Prisoner's Dilemma as a “stock market game” increased perceived conflict of interests in both studies. These results call into question the *coordination hypothesis*, according to which frames enter behaviour via first-order beliefs, although it is in line with the non-significant effect of conflict-related frames on first-order beliefs in Gerlach et al.'s (2017) meta-analysis. We also find no relationship between framing and social preferences in study 2. To our knowledge, this is the first study to directly elicit social preferences alongside a framed game. However, given the non-significant framing effect on cooperative behaviour, we see a need for further studies investigating the relationship between framing and social preferences.

Our disparate findings from studies 1 and 2 are best explained by a seemingly minor difference in the experimental task. In study 1, we used a Prisoner's Dilemma with a dichotomous choice, whereas in study 2, we used a continuous-choice Prisoner's Dilemma. Importantly, the two games are formally equivalent. Yet, there is some evidence from comparing Prisoner's Dilemmas and Public Goods Games that framing effects might be weaker when prosocial behaviour is not a matter of cooperation and defection, but of degree (Gerlach et al., 2017). Gerlach et al. (2017) estimated the framing effect to be 8.9% points smaller in Public Goods Games. However, this meta-analysis reported very few studies with two-player Public Goods Games, including only a single such study on conflict-related frames (Brandts & Schwieler, 2009). Thus, there is very limited prior direct evidence that framing more strongly affects choices when

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they are dichotomous rather than continuous. Since we do not test this idea directly, further research is needed to substantiate this explanation, and to provide an account of why the size of the choice set matters for the impact of frames.

While the main framing effect (i.e., changing the name of the game) was negligible in study 2, another framing effect, implemented by adding a simple note emphasising either conflicting or corresponding interests, did have an impact on prosocial behaviour. Notably, this manipulation is similar to the earliest conflict frames applied to the Prisoner's Dilemma (Deutsch, 1957, 1958), and fits the definition of conflict framing developed by Gerlach et al. (2017). We find strong evidence that this framing effect works by changing people's perceptions of conflict of interests. Moreover, the effect cannot be accounted for by changes in beliefs. Indeed, insofar as frames change people's beliefs about the choices of others, they might do so because they change their perceptions of conflict of interests in the game. This provides first evidence that perceptions of interdependence have a causal influence on prosocial behaviour.

Conclusion

Across two studies, we tested the proposition that context frames, such as calling the Prisoner's Dilemma a 'Community' or a 'Stock Market' game, enter cooperative behaviour by changing people's perception of the conflict of interests involved in the game. We replicate this framing effect in the dichotomous-choice, but not in the continuous-choice Prisoner's Dilemma. Across both studies, we find evidence that this framing affects perceptions of conflict of interests, and that these changes translate into changes in cooperative behaviour, whereas first-order beliefs and social preferences do not. In addition, we find evidence that perceptions of conflict of interests have a causal

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effect on prosocial behaviour, and that this effect cannot be accounted for by changes in social preferences or beliefs.

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Appendix 1: Revised Situational Interdependence Scale-Conflict Subscale

German and English items of the revised conflict subscale of the SIS. Item numbers refer to numbers in the original SIS (Gerpott et al., 2018); reverse-coded items are marked [R].

German:

- 3. Für die andere Person ist ein anderes Ergebnis aus dieser Situation gut als für mich.
- 7. Wir können beide unser bestes Ergebnis erzielen. [R]
- 11. Es ist schwierig, für uns beide einen guten Ausgang für diese Situation zu finden.
- 15. Wir können beide unsere profitabelsten Ergebnisse in dieser Situation erzielen. [R]
- 19. Unsere besten Ergebnisse in dieser Situation stehen in Konflikt zueinander.
- 23. Was für mich am besten ist, ist auch für die andere Person am besten. [R]

English:

- 3. A different outcome is good for the other person than for me.
- 7. We can both obtain our best outcomes. [R]
- 11. It is difficult to find a good solution for both of us in this situation.
- 15. Both of us can achieve our most profitable outcomes in this situation. [R]
- 19. Our best outcomes in this situation are conflicting.
- 23. What is good for me is also good for the other. [R]