Looking ahead to look out for yourself: A temporal heuristic encourages cooperation among the self-interested

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Short title: Positional order and prosocial choice

Abstract

Cooperation is essential to our success as a species, yet theories of cooperation often focus on how people end up wanting to help others. Here, we present an account of how overtly self-interested individuals decide to act prosocially. It is widely assumed the order in which people act should not matter if they cannot see others' choices. However, the mere knowledge of one's position in a sequence of decision-makers has a powerful effect on self-interested individuals: first-movers cooperate the most, and cooperation declines to the end of the sequence. The effect is removed if future decisions are made by a random process instead of other people, compatible with *as-if* causal reasoning in which one's own cooperation is treated as informative about what others will do. These findings reveal a forward-looking heuristic that shapes human cooperation, and suggest that reminding decision-makers that others will be in their shoes can promote prosocial decisions.

Teaser: Self-interested people act more generously when they know others will be in their shoes.

Introduction

Human cooperation is essential for social welfare, yet it presents a fundamental puzzle for models of rational self-interest [1]. In anonymous one-shot interactions, such as making contributions to public goods, the rational choice is often to defect even though mutual cooperation yields a better collective outcome. This tension is frequently studied using the Public Goods Game (PGG), where cooperation is consistently observed despite theoretical predictions to the contrary [2, 3]. An important variation on this puzzle arises when decisions are made sequentially but without observation of others' prior choices. Standard game theory assumes that if players cannot observe others, the *sequence* of moves shouldn't matter, only the information available at decision time; after all, what should the order of moves matter if no one can see anyone else's move? This follows from the foundational assumption that strategy should be guided by available information (termed "preliminarity" by Von Neumann and Morgenstern), not by the mere chronological order of unobserved moves ("anteriority") [4]. Here, we show that temporal position alone is a surprisingly effective determinant of cooperative behavior in social dilemmas—but only among players maximizing their own direct rewards.

Across studies, participants played real-time sequential public goods games in groups of up to five players. Each player learned their position in the sequence but never observed others' contributions. Figure 1 illustrates the five-player task structure and the key information and agency manipulation used in Study 5.

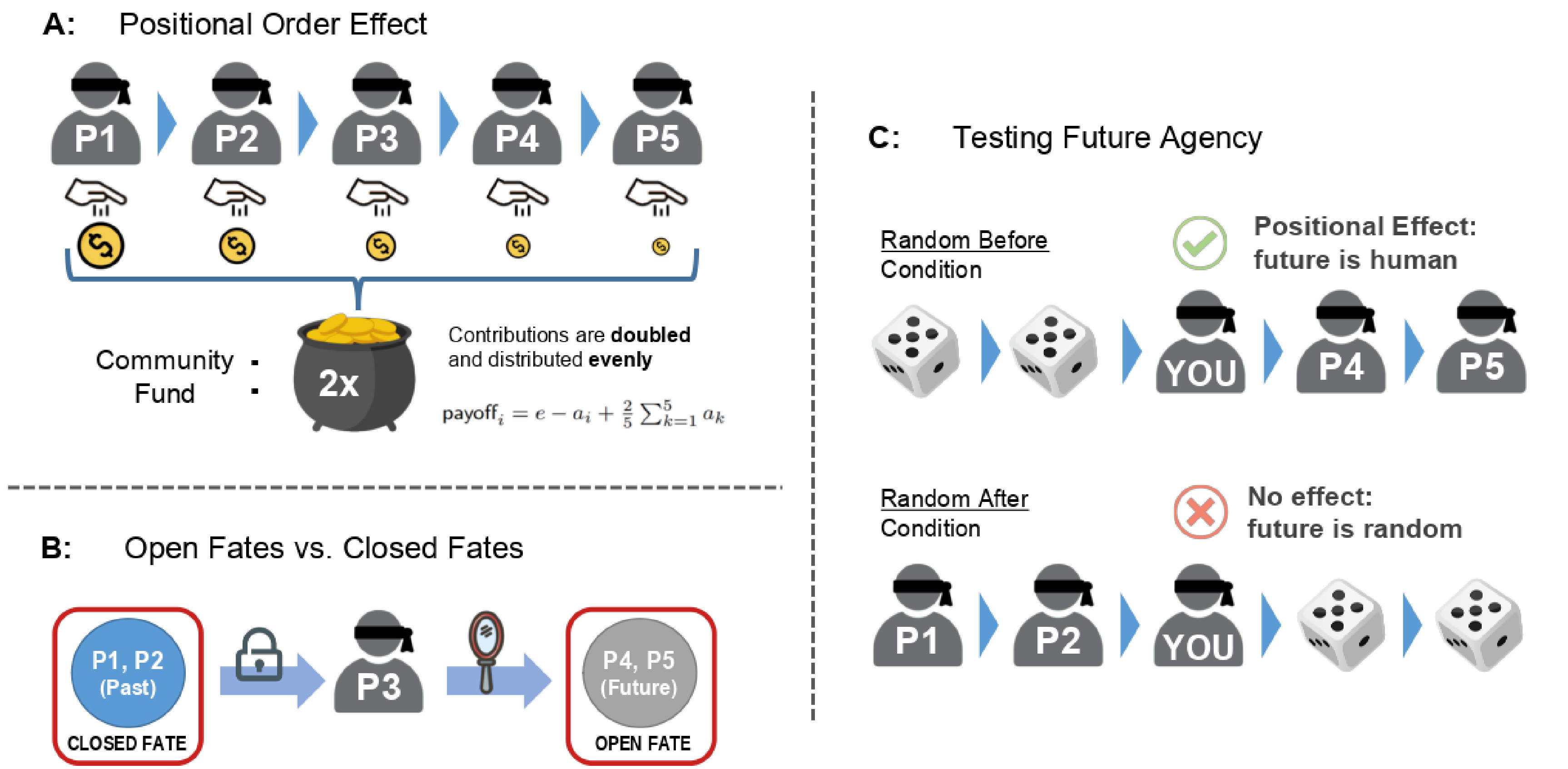


Figure 1: Task structure and information conditions. (**A**) In this Sequential Public Goods Game, players know their position in the sequence but do not observe others' choices. Players contribute to a shared Community Fund and total contributions are multiplied and redistributed evenly. We observe Positional Order Effects: first-movers contribute the most and last-movers the least, with a linear trend in-between. (**B**) We conceptualize uncertainty about players who have already moved as "closed fates" and uncertainty about players who have yet to move as "open fates." Closed fates are unknown but fixed, while open fates are unknown and undecided. Players look to their own decisions to model the *future* decisions of others. (**C**) Study 5 manipulation: players before or after the participant were described as either human decision-makers or random draws (illustrated here for a focal player at position 3). The positional order effect appears only when the future contains human decision-makers, and disappears when the future is determined by a random process.

We uncover a robust "positional order effect": in sequential PGGs without observation of others' moves, cooperation is highest for first-movers and declines progressively with each subsequent position in the sequence. We argue that this pattern reveals a powerful heuristic used to navigate social uncertainty. Past research has investigated several closely-related phenomena; for instance, "diagnostic" actions and "quasi-magical" thinking describe cases in which people treat their own choices as informative about uncertain outcomes, including others' behavior, even absent a causal link [5, 6]. Related ideas include the illusion of control [7, 8], self-signaling and social projection [9–13], the notion of "translucent players" [14], virtual observability [15] and decision-theoretic perspectives that prioritize correlation over causation (e.g., Evidential Decision Theory [16], Subjective Expected Relative Similarity [17], Superrationality [18], and Newcomb's problem [19–21]). Moral universalization provides a complementary lens: people sometimes evaluate actions by asking what would happen if everyone acted similarly Levine et al. [22].

These frameworks are largely agnostic about the temporal ordering of moves. We show a temporal asymmetry: players treat their own move as more diagnostic of *future* unobserved actions than of *past* unobserved actions. This is consistent with reasoning about "open fates" (as-yet unmade decisions, sometimes referred to as aleatory uncertainty) rather than "closed fates" (decisions that have been made, but which are unknown to the participant; epistemic uncertainty) [23]. For early movers, this effectively turns a costly contribution to a public good into a bet that subsequent players will match it, making cooperation appear individually profitable. There is a significant literature about the effects of sequential order when players can see others' moves [24], as well as a smaller literature looking at the effects of uncertainty about position [25, 26]. Prior sequential PD/PGG studies *without* observation generally report no order effects [27–30], but also were generally not designed to detect this pattern. Evidence from social dilemmas under uncertainty is mixed but broadly consistent with the idea that indeterminacy can increase prosociality [23, 31, 32]. For instance, Shafir and Tversky report that there are many people who would prefer to pay for a vacation to

Hawaii in the event that they pass an exam *and* in the event that they fail, but who would also prefer *not* to buy if they do not know how the exam has turned out [33].

Crucially, the positional order effect we report here is not universal; it is driven exclusively by self-interested individuals. For those with prosocial motivations, the decision to contribute often stems from an intrinsic preference for prosociality, or a "warm glow" derived from the act of giving itself, making their behavior less sensitive to the strategic context [34]. Self-interested players, however, face a different tradeoff, weighing a certain personal cost against an uncertain collective benefit. It is precisely for this group that acting as if others will mirror your move can make cooperation seem individually worthwhile. We use this "as if" language descriptively; it is not meant to imply that participants consciously deliberate, endorse, or can verbalize this logic. This finding offers a window into the decision-making of the very individuals least likely to contribute to a public good, opening the door to interventions that target precisely this population.

Across five experiments we provide a multi-stage demonstration of this phenomenon. We first establish the positional order effect, and show that it is moderated by self-interest. We also show a future similarity effect, wherein participants are willing to bet that the moves of future players are more similar to their own relative to those of past players. We then demonstrate that this self-interested strategy can be induced experimentally (Study 4). Finally, we provide direct causal evidence for the future-directed nature of the heuristic by showing that the effect vanishes when the choices of subsequent—but not preceding—players are determined by a random process rather than by other human agents (Study 5). Together, these findings reveal a fundamental and previously undocumented mechanism governing human cooperation.

Results

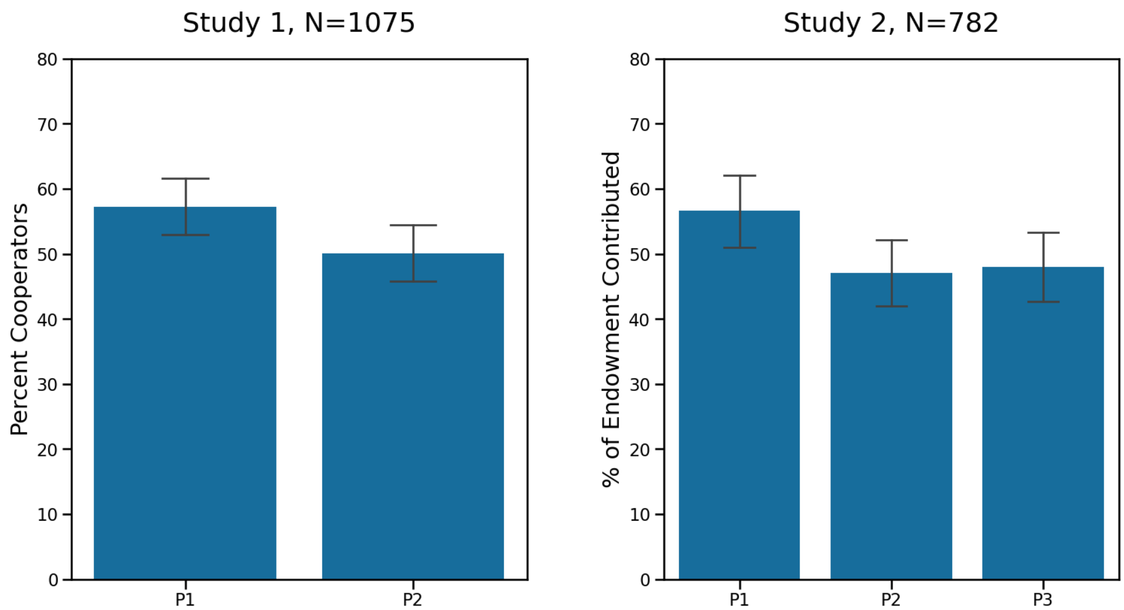
2.1 A positional order effect emerges in sequential games

Across our initial experiments we observe a positional order effect: contributions to the public good were highest for players moving first and declined as their position in the sequence increased. We first observed this serendipitously in a two-player Sequential Prisoner's Dilemma (Study 1), a game structurally equivalent to a two-person PGG, and this effect was preregistered for Studies 2-5. In Study 1, first-movers were significantly more likely to cooperate (i.e., contribute their full endowment) than second-movers (57.2% vs. 50.1%, a 12% decrease; logistic odds ratio = 0.75, 95% CI = [0.59, 0.96], $p = 0.020$). To further investigate this phenomenon, we conducted a three-person SPGG (Study 2). Player 1 contributed on average 56.6% of the endowment, while Player 3 contributed 48.0%, a 15% decrease overall or 7.5% per position on average. Contributions declined from first to second position, though the difference between positions 2 and 3 was not significant. The unexplained lack of difference between positions 2 and 3 led us to design an experiment with a longer sequence and other improvements to replicate the effect. Importantly, this decline is not explained by mere time in the experiment: in simultaneous-move control conditions, there is no evidence that longer delays matched to late-mover waiting times reduce contributions (Supplementary Materials, Section A.6). See Fig. 2.

2.2 The positional order effect is driven by self-interest, not prosocial motivations

Having established the presence of the positional order effect, we next investigated its psychological origins. We used the Social Value Orientation (SVO) scale to distinguish between "Prosocial" players (who value joint outcomes) and "Individualistic" players (who prioritize their own payoffs). SVO is a measure of willingness to give up gains in order to

Figure 2: Contributions to the public good decline with sequential position. Average contribution (as a percentage of initial endowment) is plotted against a player's position in a sequence. Data are from a two-player Prisoner's Dilemma (Study 1) and a three-player Public Goods Game (Study 2), demonstrating a negative trend where earlier movers tend to contribute more. Error bars represent 95% CIs.



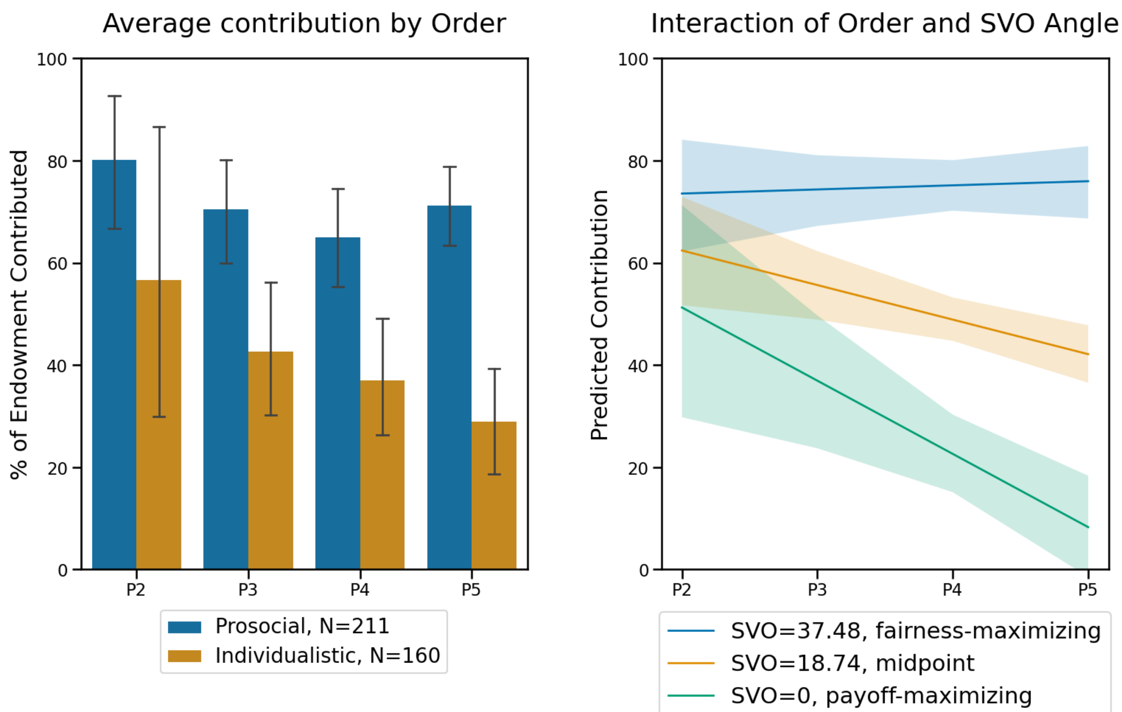
benefit others, and in the SVO battery participants make a series of incentivized decisions similar to Dictator games where they allocate funds between themselves and someone else. Participants can choose to forego gains (or even pay costs) to help or hurt the other player. Congruent with our preregistered hypothesis, we found that the positional order effect is driven almost exclusively by Individualistic players—players who are trying to maximize their own direct payoffs.

In Study 3, Prosocial players contributed generously and at a consistent level regardless of their position. A linear model predicting contribution with order among Prosocial players only shows no effect. In contrast, Individualistic players showed a marked decline in contributions with increasing order. As preregistered, Individualistic players show a strong positional order effect. Player 2 contributed on average 56.7% of the endowment, while Player 5 contributed only 29.0%, a 49% decrease overall or 16% per position (N = 160 Individualistic, $\beta = -7.886$, 95% CI = [-15.106, -0.366], $p = 0.042$). This moderation is most apparent when using the continuous SVO angle measure (the SVO battery yields a continuous "angle" score; we focus on SVO angle because it preserves information relative to coarsening and using categories) instead of categorical Individualistic / Prosocial, where a stronger orientation toward self-interest (a smaller angle) is strongly associated with a steeper decline in contributions with order (Fig. 3; Order $\times$ SVO-angle interaction: $N = 373$, $\beta = 0.405$, 95% CI = [0.106, 0.704], $p = 0.008$).

Tests of the underlying mechanism we planned for Study 5 make use of a sample that is composed entirely of self-interested participants. Because assembling such a sample by filtering the available study populations proved unworkable for a five-person real-time game, we decided to perform a low-cost test whether the mere *instruction* to act selfishly would produce the positional order effect. We confirmed that instructing participants to maximize their personal payoffs could successfully induce the positional order effect in Study 4 and used it in Study 5, demonstrating that this is a readily deployable strategy. In Study 4 we observe a decrease from 70% contribution in Player 1 to 27% in Player 5 among players instructed to maximize earnings, a 61% decline overall or 15% per position. In particular, we still observe very high cooperation rates among first-movers despite the instruction. Among players not instructed to maximize earnings, Player 1 contributes 48% on average compared to Player 5's 77%, a sizable increase. There is substantial noise in these estimates given the small sample, but we observe a strong interaction between order and the instruction to maximize own direct earnings, $N = 130$, $\beta = -15.671$, 95% CI = [-26.052, -4.668], $p = 0.006$; there is a strong order effect in the "instruct" condition, and none at all in the "no instruction" condition. This made us confident that the mere instruction to maximize one's earnings, if deployed in Study 5, would result in a positional order effect.

The presence of a positional order effect moderated by SVO angle in Study 3, combined with the ability to induce the effect with the instruction to maximize their own direct earnings shown in Study 4, suggests that the entire phenomenon—both the action and the belief that supports it—is a strategic heuristic used by those focused on maximizing their personal outcomes. Study 4's result made us confident that deploying the mere instruction to maximize would result in the positional order effect in a larger study designed to test the mechanism directly, Study 5.

Figure 3: The positional order effect is driven by self-interested players. (**A**) In a five-player game (Study 3) from which P1 data was excluded due to technical fault, contributions decline with order only for Individualistic players, not for Prosocial players. Player types were determined by Social Value Orientation (SVO), a measure of preference for own versus joint outcomes. (**B**) The moderating effect is continuous. A smaller SVO angle (indicating greater self-interest) is associated with a steeper negative slope of contribution on order. Shaded areas represent 95% CIs.



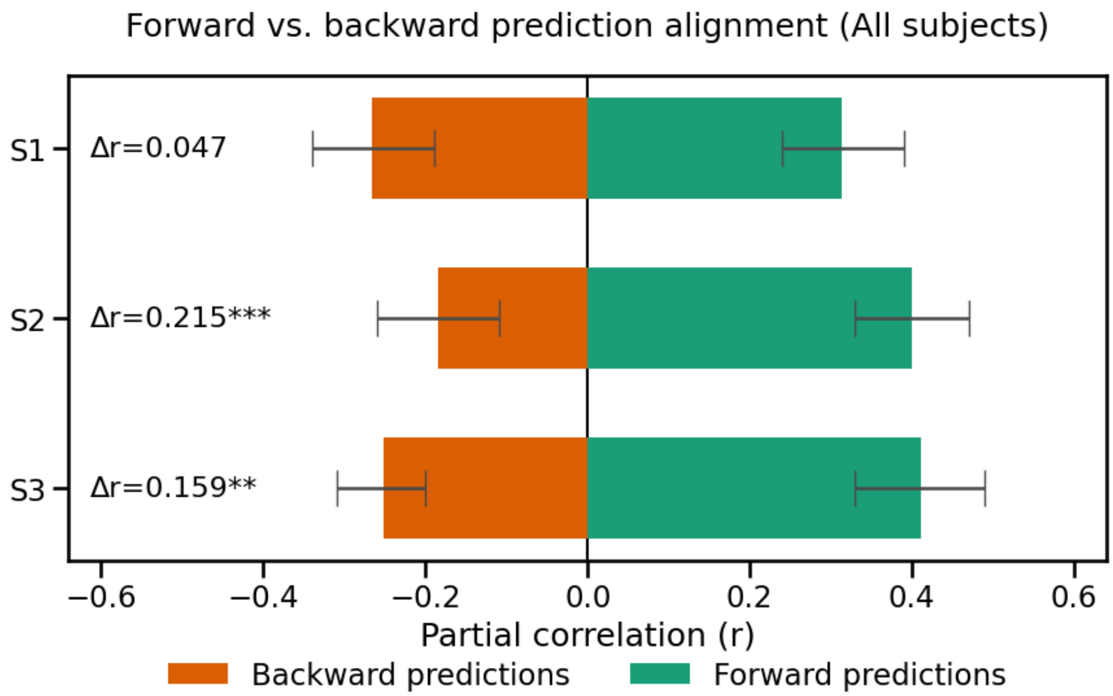
2.3 Players bet that others will do as they have done, especially when those others have yet to move

The pattern of behavior in the positional order effect is reflected in players' beliefs. In Studies 1, 2, and 3, participants were asked to bet on the contribution decisions of other players in their group, and the population as a whole. On average, participants believe that the decisions of future players (open fates) are more similar to their own than those of players who have already moved (closed fates). To measure this future similarity effect, we computed the partial correlation between a player's own contribution and their prediction of others' contributions, controlling for their separate prediction about the population average. By controlling for population-level beliefs, this partial correlation isolates the relationship between one's own choice and beliefs about specific groupmates, effectively asking: conditional on your general optimism or pessimism about people's contributions, how much do you think the people in *your group* will resemble you? We computed correlations separately for predictions about players who had yet to move ("forward" direction, towards open fates) and players who had already moved ("backward" direction, towards closed fates), and compared these correlations using Fisher's z test.

In the three-person SPGG of Study 2, analysis showed that players' own contributions were much more strongly associated with their predictions about later movers than about earlier movers, congruent with our preregistered hypothesis. For forward-looking predictions (toward open fates), we observed a partial correlation of $r = 0.40$, whereas for backward-looking predictions (toward closed fates), the partial correlation was $r = 0.18$. The difference between these correlations was significant ($N = 1198$ predictions, $\Delta r = 0.22$, $z = 4.08$, $p < 0.001$). This pattern was also preregistered for Study 3, which shows the same pattern: predictions about players who had yet to move were more tightly coupled to one's own contribution ($r = 0.41$) than predictions about players who had already moved ($r = 0.25$; $N = 1492$ predictions, $\Delta r = 0.16$, $z = 3.11$, $p = 0.002$ for the difference).

A conceptually parallel exploratory analysis in the two-player Sequential Prisoner's Dilemma (Study 1) shows the same future similarity effect—partial correlations between one's own choice and predictions about the partner's move were larger for forward predictions ($r = 0.31$) than for backward predictions ($r = 0.27$), though the difference between directions does not reach significance. Decisions in 1 were binary (cooperate vs. defect), so there was substantially less variability in responses which reduces statistical power to detect differences. See Fig. 4.

Figure 4: Players will bet their decisions are more diagnostic of others' future decisions relative to past decisions. Partial correlations between a player's own contribution and their prediction of another player's contribution, controlling for predictions of the population average. Players' own choices were more strongly correlated with their predictions about players who had yet to move (forwards, towards open fates) than about players who had already moved (backwards, towards closed fates). Error bars represent 95% CIs. Asterisks on Δr indicate forward–backward differences (Fisher's z test): $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.



2.4 The heuristic is selectively engaged by future human agency

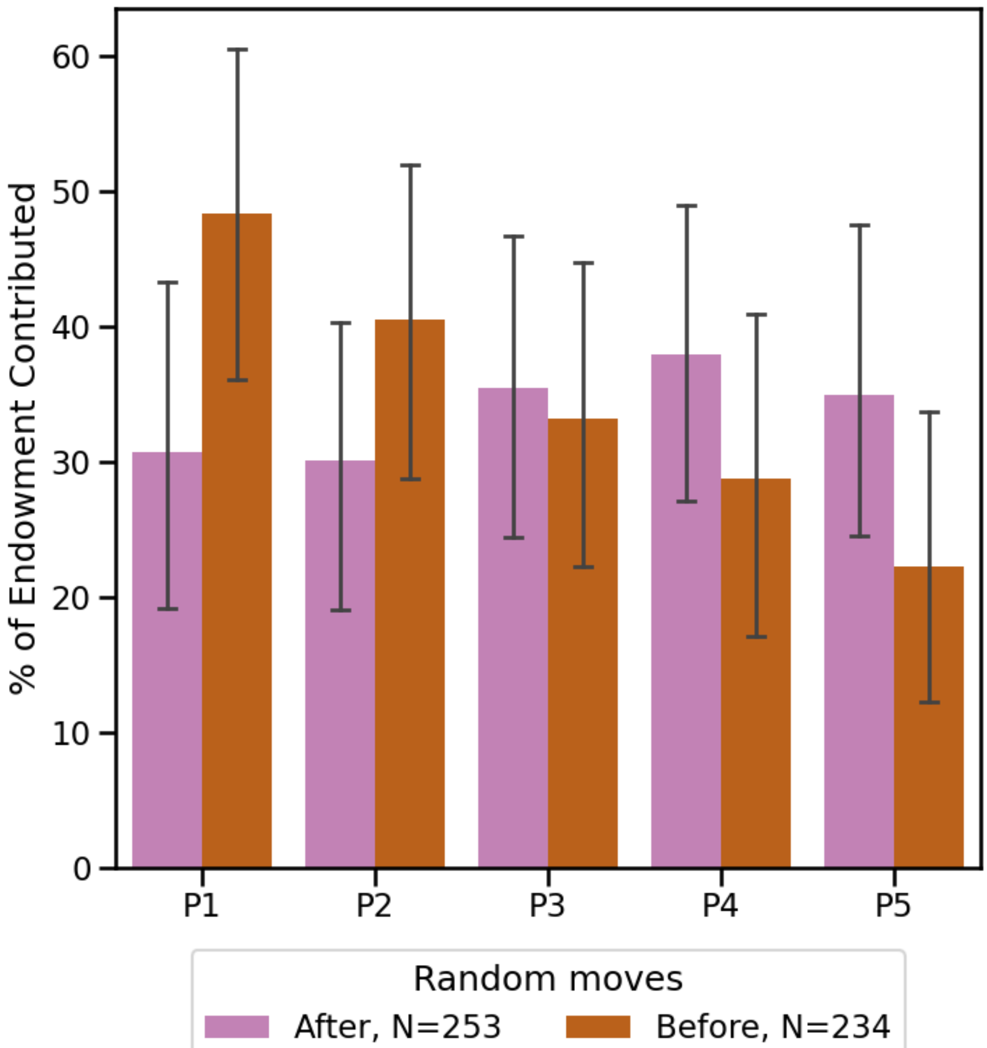
The main experiment (Study 5) tested whether the positional order effect depends on participants facing *future human decision-makers*, as opposed to future outcomes described as non-agentic and fixed by a random process. All participants were instructed to be self-interested as in Study 4, and were randomly assigned to one of two conditions. In the

Random Before condition, players were told that all players moving before them had their moves made by a "random draw", while all players moving after them were described as real people making their own choices. In the *Random After* condition, this was reversed.

The results reveal a sharp boundary condition (Fig. 5). As preregistered, a strong positional order effect emerged in the *Random Before* condition, replicating our previous findings: contributions declined by 54%, from P1 (48% of endowment) to P5 (22% of endowment). In contrast, the order effect was eliminated in the *Random After* condition, where contributions were stable (mean 33%) regardless of position (P1: 31%; P5: 35%). The interaction between order and condition was robust ($N = 487$, $\beta = -8.088$, 95% CI = [-12.822, -3.120], $p = 0.001$), indicating that the contribution gradient appears when the *future* segment of the sequence consists of human partners, and disappears when the *future* is described as determined by a random process.

Players' predictions about others' moves provide convergent evidence from beliefs that is consistent with this interpretation. In the *Random Before* condition—where future moves were described as being made by human partners—participants' own contributions were strongly aligned with their predictions about future human players ($r = 0.60$), but only weakly related to predictions about past randomly determined moves ($r = 0.17$), yielding a large difference ($\Delta r = 0.44$, $z = 8.03$, 95% CI = [0.379, 0.577], $p < 0.001$). In the *Random After* condition, this pattern reversed: own contributions were weakly related to predictions about future random moves ($r = 0.05$) but substantially related to predictions about past human players ($r = 0.47$; $\Delta r = -0.42$, $z = -7.23$, 95% CI = [-0.524, -0.322], $p < 0.001$). In other words, participants' forecasts tracked their own choices *selectively for targets described as human agents*, and this selectivity flipped with the direction of agency in the sequence. This pattern is compatible with the idea that the positional order effect is driven less by position in time per se than by position relative to *future human decision-makers* under uncertainty. See Fig. 6.

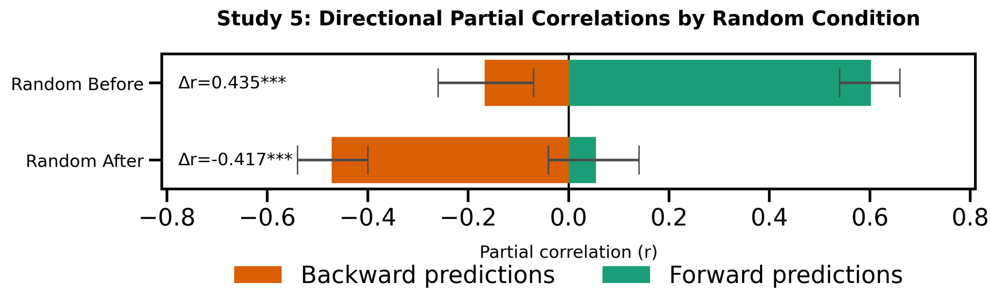
Figure 5: The order effect depends on perceived future human decision-makers. In a five-player game where all participants were instructed to be self-interested (Study 5), the decline in contributions with order appeared only when the players *yet to act* were described as real human decision-makers (*Random Before*). When the players *yet to act* were described as being determined by a random process (*Random After*), contributions no longer declined with position. This experimental dissociation indicates that the positional order effect is selectively engaged by expectations about future human agency. Error bars represent 95% CIs.



Time spent waiting does not explain the effect

Study 5 was designed to explicitly test whether waiting time alone could explain the effect. It does this by including simultaneous-move conditions that mimic first-movers (no delay) and last-movers (80-second delay) in terms of timing. Contributions were statistically

Figure 6: Predictions track participants' own choices selectively for human agents. Study 5: correlation between participants' own contributions and their predictions of other players' contributions, separating predictions about *human* decision-makers from predictions about *randomly determined* contributions. In *Random Before*, own contributions are tightly coupled to predictions about future human players but weakly related to predictions about past random moves; in *Random After*, this pattern reverses. Lines show fitted relationships; shaded areas indicate 95% CIs. Asterisks on Δr indicate forward–backward differences (Fisher's z test): $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.



indistinguishable across these conditions (mean contribution = 38.0% of endowment for no delay, 47.2% for 80-second delay, $t(128) = -1.08$, $p = 0.282$), with the trend actually going up with increased delay time. This result argues that the positional order effect observed is specific to sequential ordering and beliefs about future human decision-makers, and not a function of wait time.

Discussion

Our results reveal a robust forward-looking heuristic that self-interested individuals use in one-shot social dilemmas, causing cooperation to systematically decline with later positions in sequence. This heuristic provides a new explanation for cooperation among those who are least inclined to help others. An individual's willingness to contribute is systematically shaped by their position in a sequence, but this sensitivity only emerges when participants are motivated to maximize their own direct payoffs. Across studies, the effect is not explained by new information about others' choices; instead, it appears to reflect a forward-directed shortcut that links one's own decision to expectations about others. In Study 5, experimentally changing whether the *future* portion of the sequence was described as human decision-making versus a random process selectively turned the contribution gradient on or off. Prediction data mirror this boundary condition: participants' forecasts align with their own contributions for targets described as human agents, and this alignment disappears (or reverses) for targets described as random. Together, these findings identify a principled constraint on when the order effect operates—namely, when the future is construed as containing human agency under "open fates" uncertainty.

This work contributes to theories of diagnostic reasoning and decision-making under uncertainty. Prior research suggests that people sometimes treat their actions as informative about outcomes even when no causal pathway exists [5, 6]. Our data are compatible with a temporally asymmetric version of this broader family of ideas: the self–other coupling observed in beliefs is strongest when reasoning about *future human agents* and attenuates when the future is described as fixed by a non-agentic process. We emphasize, however, that "causal" or "diagnostic" thinking is best understood here as an *as-if* characterization of behavior and belief—people act *as if* they can control others' moves. This *as-if* framing is descriptive; it does not imply that participants consciously endorse a belief in causal control or can articulate the inference. It is one plausible cognitive interpretation among others (e.g., selective projection/anchoring onto human agents, or a false-consensus style inference that is applied more readily to people than to stochastic processes). By directly measuring players' predictions, we show that this diagnostic reasoning is not a post-hoc explanation but an active cognitive process. Our findings introduce a temporal asymmetry: the heuristic we identify is directed exclusively toward the future—the realm of "open fates" that are still mutable—and does not apply to the past, or "closed fates," which are already determined [23]. This suggests that our sense of agency in social contexts, even when illusory, is fundamentally forward-looking. We do not act to change the settled past; we act as if we can influence the unwritten future.

Furthermore, the finding that this heuristic is employed specifically by self-interested individuals adds to our understanding of prosocial motivation. For prosocial individuals, the decision to cooperate appears to be intrinsic and stable, consistent with a "warm glow" utility that is insensitive to the strategic context [34]. For the self-interested, however, the decision is made under profound uncertainty. The positional order heuristic thus serves as a coherent solution for them, making cooperation seem individually sensible when anticipating the future actions of others, even without explicit deliberation. This provides a novel explanation for cooperation among the very people least expected to do so.

The implications of this temporal heuristic may extend to a wide range of real-world collective action problems. In crowdfunding, for example, an early backer may contribute not just to signal support to others, but in part because they act as if their contribution makes it more likely that the hundreds of backers who follow will also contribute. By this action, they turn their small pledge into a catalyst for collective success. In global efforts to address climate change, an individual's costly sustainable choice, though unobserved and of negligible direct impact, may be justified by the belief that it is diagnostic of a broader shift in collective behavior to come. Similarly, the success of nascent social movements may depend on a critical mass of early adopters who act as if their unobserved choice will be mirrored by the many who follow. It is also important to distinguish our setting from goal-based prosocial contexts where progress is visible. In many charitable giving and crowdfunding campaigns, later contributions can become more motivating as the goal nears completion, in part because donors perceive their marginal impact to be higher (a goal-gradient pattern) [35, 36]. Our experiments intentionally remove progress information and keep choices private, isolating a regime in which temporal position operates through beliefs about future human agency rather than observed momentum or goal proximity. We therefore do not view the positional order effect as inconsistent with goal-gradient findings; instead, we predict that making progress salient or contributions observable will attenuate the early-mover advantage we document and can even reverse it.

Several alternative explanations merit consideration. One possibility is that later movers simply have more time to deliberate or better grasp the incentive structure, leading to more self-interested play. However, simultaneous-move control conditions (including variants with long delays matched to late-mover waiting times) show no evidence that longer waits reduce contributions (Supplementary Materials, Section A.6). Moreover, in Studies 4 and 5 the experiment advanced in lock-step with fixed-duration stages to prevent inferences based on others' response times, constraining accounts based on latency signaling. These features make it unlikely that the positional order effect is driven by mere time in the experiment. Other alternatives remain open. For example, the open-fates versus closed-fates distinction could operate through shifts in optimism or pessimism rather than diagnostic reasoning, and future work could measure and test such mediators directly. It also remains unknown whether the relevant belief is a general prior that a third-party observer would share, and to what extent participants hold residual beliefs about hidden observability (e.g., that their choices could be communicated despite the stated privacy of moves).

In addition, this research has several limitations that offer avenues for future work. Our experiments were conducted in an online environment with relatively low stakes, and the generalizability of the effect should be tested in field settings with higher financial or social stakes. While our prediction data provides evidence for the "as if" heuristic, future studies using methods like think-aloud protocols could offer a more granular view of the conscious reasoning (or lack thereof) that accompanies this process. Finally, exploring the boundary conditions of the effect—such as in larger groups, different cultural contexts, or under different payoff structures—will be important for a complete understanding. For instance, if future actions are not truly private (perhaps early movers gain reputation, or progress toward a goal is visible) this heuristic might attenuate or flip (later contributors could become *more* willing, as seen in goal-progress effects [35]). Understanding how feedback or observability interacts with the positional heuristic is another important avenue for future work. Ultimately, this research demonstrates that the temporal architecture of a choice can be a subtle but powerful force in shaping human cooperation. Identifying this forward-looking heuristic opens new avenues for understanding and promoting collective action.

Materials and Methods

4.1 Experimental Design

We conducted five experiments with a total of 3,686 participants, all recruited from U.S.-based online panels. Studies 1-3 used Amazon Mechanical Turk (AMT), while Studies 4-5 used CloudResearch's filtered AMT panel. All studies except Study 4 were preregistered (see Materials and Methods). All decisions were incentivized. To ensure participants understood the incentive structure, we employed stringent comprehension checks; a single incorrect answer resulted in exclusion from analysis. All experiments used real-time, incentive-compatible sequential Public Goods Games (or in the case of Study 1 a structurally equivalent Prisoner's Dilemma) where players moved one at a time without observing prior moves. Full details of each study's design, deviations from preregistration, and supplementary analyses are provided here and in Supplementary Materials.

4.2 Ethics and Preregistration

All studies were approved by MIT's Committee on the Use of Humans as Experimental Subjects (COUHES) and comply with all relevant ethical regulations. We obtained electronic consent from all participants. All experiments we have conducted involving sequential games are reported here. All studies except for Study 4 were preregistered. All data, analysis code, and experimental materials have been made available on the Open Science Framework at https://osf.io/mykzt/overview?view_only=248c1173bf664c78a8ffdcf637dddbd3.

4.3 Common Design Elements

All studies were real-time, one-shot linear PGGs with a multiplier of two (or a structurally equivalent two-person Prisoner's Dilemma in the case of Study 1). Participants contribute three main inputs: comprehension checks, game playing decisions, and predictions of the responses of other players. Apart from a base payment and proceeds from the game, correct answers to comprehension checks are directly incentivized and accurate predictions of others' moves are incentivized based on the mean squared error between the actual and predicted value. In all studies, players participated in a brief text chat with their groupmates before the task to increase engagement and confirm to them that they were interacting with real people in real time. The text chat was prior to learning the game they would play, and there is no interaction once the initial text chat is finished (See Supplementary Materials A.4 for robustness checks against group-level dependencies). All experiments except Study 1 included simultaneous-move control conditions (see Supplementary Materials, Section A.6).

To ensure that participants fully understood the incentive structure we employed comprehension checks. All experiments except Study 1 (which used similar comprehension questions; see Section 4.4) share the following three up-front comprehension and attention check questions (randomized in order):

1. *Do any of the other players* ***know how much YOU decide to contribute?***
2. *Jack and Jill are playing this game together. Jack decided to* ***TRANSFER*** *and Jill decided to* ***KEEP****. Who will make more money, Jack or Jill?*
3. *What year is it?*

Participants are given one chance to get each of these questions right, and a single wrong answer results in their data being excluded from analyses. Responses to the comprehension questions are only relevant to data analysis, however: players continue on whether or not they have answered correctly because it is necessary that they move in order to finish the game. They have no contact with, and so no effect on, others' choices. These factors resulted in the exclusion rates reported for each study. Participant compensation included a base payment, earnings from the game, and incentivized bonuses for correct comprehension answers and accurate predictions of others' moves. All experiment software was written in the open-source oTree framework [37].

4.4 Study 1: Sequential Prisoner's Dilemma

Participants

A total of 2,371 U.S.-based participants were recruited from Amazon Mechanical Turk (AMT) to complete the study. Of these, 1,075 (45%) passed all comprehension checks and were included in the final analysis.

Procedure and Design

This study employed a two-person Sequential Prisoner's Dilemma (PD), which is structurally equivalent to a two-person Public Goods Game. The study was preregistered at `https://osf.io/dbcpv/?view_only=754dbbc2805496380ef89115f7f95c2`.

Upon arrival, participants provided informed consent. Participants were then placed into a real-time chat room for 30 seconds to exchange messages, confirming that their partner was a real person. Following this, they received instructions for the game.

The game was framed as an allocation task. Each player was endowed with a sum of money and chose either to "keep" it or "transfer" it to the other player. If transferred, the amount was doubled before reaching the partner. Player 1 moved first, followed by Player 2. Crucially, Player 2 did not observe Player 1's move before making their own decision. The payoff matrix is provided in Table 1 (Supplementary Materials).

Before playing, participants completed a battery of five comprehension questions to ensure they understood the incentive structure and the sequential (but unobserved) nature of the game. Failure to answer any question correctly resulted in exclusion from the analysis.

After reading the instructions players proceed to 5 comprehension tests:

1. Does the other player know what your move is?

2. If the other person TRANSFERS their money, what earns you the most money?

3. If the other person KEEPS their money, what earns you the most money?

4. If you choose to TRANSFER your money, do you make more money if the other person TRANSFERS or KEEPS?

5. What year is it?

After making their decision, participants predicted the likelihood that their partner had transferred (0–100%) and the likelihood that an average player would transfer.

Treatments

Participants were randomized into one of several conditions. These included the standard sequential PD described above, as well as exploratory conditions involving manipulations of social information and mentalizing. As these exploratory manipulations are not central to the positional order effect reported here, we collapse across conditions for the primary analysis.

Analytic Strategy

We employed a logistic regression to analyze the binary decision to transfer (cooperate) or keep (defect) as a function of the player's order in the sequence (Player 1 vs. Player 2):

$$\text{logit } \Pr(\text{coop\_recode}_i = 1) = \alpha + \beta\, \text{C(order)}_i + \varepsilon_i. \tag{1}$$

Additionally, we analyzed the relationship between a player's own move and their prediction of their partner's move. A secondary, non-preregistered analysis used partial correlations to examine the relationship between a player's decision and the difference in their predictions about their specific partner versus the population average.

To assess whether players treat their own choice as diagnostic of others' choices, we computed partial correlations between a player's own contribution a_i and their prediction of another player's contribution $\hat{a}_k$, controlling for their prediction of the population average $\bar{a}_{\text{pop}}$. The partial correlation isolates the relationship between own choice and predictions about specific group members, above and beyond general optimism or pessimism about the population:

$$r_{a_i,\hat{a}_k\cdot\bar{a}_{\text{pop}}} = \frac{r_{a_i,\hat{a}_k} - r_{a_i,\bar{a}_{\text{pop}}} \cdot r_{\hat{a}_k,\bar{a}_{\text{pop}}}}{\sqrt{1-r^2_{a_i,\bar{a}_{\text{pop}}}} \cdot \sqrt{1-r^2_{\hat{a}_k,\bar{a}_{\text{pop}}}}} \tag{2}$$

We computed these partial correlations separately for predictions about players who had yet to move ("forward" direction) and players who had already moved ("backward" direction), and compared them using Fisher's z-transformation.

4.5 Study 2: 3-Person SPGG

Participants

A total of 1,444 U.S.-based participants were recruited from Amazon Mechanical Turk. Of these, 1,002 (69%) passed all comprehension checks and were included in the final analysis. Among these 1,002 participants, 782 were randomized to the sequential condition and 220 to the simultaneous condition.

Procedure and Design

This study employed a three-person Sequential Public Goods Game (SPGG). The study was preregistered at https://osf.io/3vsxk/?view_only=bf35d2d3d39d48b68869c2cf78bf8e5.

The game was a one-shot linear PGG with a multiplier of two. Each player was endowed with $1 and decided how much to contribute to a "Community Fund." The total amount contributed was doubled and distributed evenly among all three players, regardless of their individual contributions.

The experimental flow proceeded as follows. First, participants transcribed nonsense sentences to filter out bots. Next, they were placed into groups of three and entered a 30-second real-time chat room to establish that they were interacting with real people. The game rules were then explained, emphasizing that choices would be made sequentially but without observing prior moves. Participants subsequently answered the three comprehension questions shared with Studies 2-5; incorrect answers resulted in exclusion from analysis. Players then made their contribution decisions. In the sequential condition, the decision screen highlighted the player's position (e.g., "You are Player 1 of 3"). Finally, participants completed incentivized predictions of others' contributions, a Social Value Orientation (SVO) slider task [38], and demographic questions.

Treatments

Groups were randomly assigned to one of two conditions. In the sequential condition, players moved one after another (Positions 1, 2, or 3). Crucially, players knew their position but could not see the contributions of those who moved before them. In the simultaneous condition, all three players made their decisions at the same time, serving as a control condition where temporal order was removed.

Analytic Strategy

We analyzed contributions using OLS linear regressions. The primary predictor was the player's order in the sequence. The preregistration for Study 2 specified backwards-difference coding, enforcing a monotonic decline in contribution with order and a test for each difference.

$$\text{contribution}_{ig} = \alpha + \beta_1 \, \text{order}_{ig} + \varepsilon_{ig}. \tag{3}$$

We also analyzed the relationship between a player's own contribution and their predictions of others' contributions as in Study 1.

4.6 Study 3: 5-Person SPGG

Participants

A total of 1,298 U.S.-based participants were recruited from Amazon Mechanical Turk (MTurk). Due to a technical error in the experimental software that caused the decision timer for the first-moving player (P1) to expire prematurely, all P1 data and data from groups affected by the cascading timing issue were excluded, leaving 783 unaffected participants (Positions 2–5) who had the full, allotted decision time. Of these, 484 (62%) passed all comprehension checks and were included in the analysis, including 373 randomized to the sequential condition and 111 to the simultaneous condition.

Procedure and Design

This study extended the design to a five-person Sequential Public Goods Game. The study was preregistered at https://osf.io/gw8nc/?view_only=aa0c4825dac4469a82f0156b77390e3c.

Several design refinements were introduced to improve the participant experience and experimental control compared to Study 2. The pre-game chat was extended to 60 seconds to allow for more meaningful interaction. Instead of a chat box while waiting for groups to form, participants played a simple game to maintain engagement. A player's position in the sequence was made explicitly salient on the wait screen, with a diagram of position and the phrase, "You are player [X] out of 5 players to go." Finally, contribution decisions were made using sliders with dynamic anchors displaying the exact consequences for the self and the fund (e.g., "KEEP FOR SELF: \$0.43 ↔ CONTRIBUTE TO FUND: \$0.57"), rather than text boxes.

The game structure remained a linear PGG with a multiplier of two and the flow follows Study 2. Participants subsequently answered the three comprehension questions shared with Studies 2-5; incorrect answers resulted in exclusion from analysis. After the game, participants completed the SVO slider measure [38] and demographic questions.

Treatments

Participants were randomized to either a Sequential condition (Positions 1–5) or a Simultaneous condition.

Analytic Strategy

We analyzed contributions from players in positions 2–5 using linear regressions. To test the hypothesis that the positional order effect is driven by self-interest, we interacted position order with SVO angle (a continuous measure of prosociality):

$$\text{contribution}_{ig} = \alpha + \beta_1\,\text{order}_{ig} + \beta_2\,\text{SVO\_angle}_i + \beta_3\,(\text{order}_{ig} \times \text{SVO\_angle}_i) + \varepsilon_{ig}. \tag{4}$$

We also analyzed the partial correlations between a player's own contribution and their predictions of others' moves, controlling for population-level predictions, as in Studies 1 and 2.

4.7 Study 4: Induced Self-Interest

Participants

A total of 197 U.S.-based participants were recruited from CloudResearch's filtered Amazon Mechanical Turk panel. Of these, 183 (93%) passed all comprehension checks and were included in the analysis. Among these 183 participants, 130 were randomized to the sequential condition and 53 to the simultaneous condition.

Procedure and Design

This study utilized a five-person Sequential Public Goods Game similar to Study 3, but introduced an experimental manipulation of self-interest instructions instead of measuring SVO. This study was not preregistered because it was intended as a quick test of the instruction; the next study, Study 5, is preregistered and depends upon the manipulation.

Several design enhancements were implemented to improve data quality and experimental control. An initial screening involved completing English idioms to ensure high-quality, fluent participants. Participants also completed an incentivized interactive practice round where they calculated payoffs for hypothetical scenarios to ensure understanding of the game mechanics. Participants also experienced additional instruction screens, which reiterated key points. To prevent information leakage through response times (e.g., a late player inferring an early player's hesitation), the experiment advanced in lock-step. Each stage lasted a fixed duration regardless of how quickly a participant made their decision. Participants answered the three comprehension questions shared with Studies 2-5; incorrect answers resulted in exclusion from analysis.

In the "Instruction" condition, participants received the following prompt designed to induce self-interested behavior:

> Please try to play this game however you think will make you the most money. We understand that sometimes you want to help other people, but for the purposes of this experiment we want you to try to make as much money as possible.

Participants in the control condition did not receive this prompt.

Treatments

Participants were randomized into a 2 $\times$ 2 design crossing Instruction (Instruction vs. No Instruction) with Game Type (Sequential vs. Simultaneous). To explicitly control for the effect of waiting time on contributions, the simultaneous condition included two variants: a no delay condition corresponding to the timing of a first-mover, and an 80-second delay condition corresponding to the waiting time experienced by a fifth-mover in the sequential game.

Analytic Strategy

We analyzed contributions using a linear regression to test if the instruction to maximize payoffs induced a positional order effect among sequential-condition subjects. The model included an interaction term between order and the binary instruction variable:

$$\text{contribution}_{ig} = \alpha + \beta_1\,\text{order}_{ig} + \beta_2\,\text{instruct\_or\_no}_i + \beta_3\,(\text{order}_{ig} \times \text{instruct\_or\_no}_i) + \varepsilon_{ig}. \quad (5)$$

4.8 Study 5: Causal Test with Randomized Agents

Participants

A total of 747 U.S.-based participants were recruited from CloudResearch's panel. Of these, 617 (83%) passed all comprehension checks and were included in the analysis. Among these 617 participants, 487 were randomized to the sequential condition and 130 to the simultaneous condition.

Procedure and Design

This study employed a five-person Sequential Public Goods Game to test the causal mechanism of the positional order effect. The study was preregistered at https://osf.io/3kepm/?view_only=614de27fdf4b40a0bad48847f32c879d.

The flow of this study follows that of Study 4. Participants subsequently answered the three comprehension questions shared with Studies 2-5; incorrect answers resulted in exclusion from analysis.

All participants received the instruction to maximize their personal earnings (identical to the "Instruction" condition in Study 4). The key manipulation involved the nature of the other agents in the sequence. To test whether the effect depends on the presence of future decision-makers, we introduced conditions where some players' moves were described as being determined by a random process (a computerized random draw, depicted with dice icons) rather than by a human agent. Visual aids (see Figure 7) reinforced this distinction throughout the game.

We used deception in this study. It was not true that everyone either before or after a given player was making their own decision or having their moves made randomly. Rather, each player in each five-person game made his or her own moves, and was merely told that the others in the game either made their own decisions or had them made randomly. Performing this study without deception would have meant four players who produce no data per five-person game, thus requiring five times as many participants at five times the cost.

We determined this was unworkable, and that the risks of using deception were warranted. All participants were fully debriefed regarding the deception upon completion of the study.

Treatments

Participants were randomized into a 2 $\times$ 2 design crossing the agent type condition (random before vs. random after) with game type (sequential vs. simultaneous). In the random before condition, participants were told that all players moving before them had their contributions determined by a random process, while all players moving after them were human decision-makers. In the random after condition, participants were told that all players moving before them were human decision-makers, while all players moving after them had their contributions determined by a random process.

As in Study 4, the Simultaneous condition included both "No Delay" and "80-second Delay" variants to control for time.

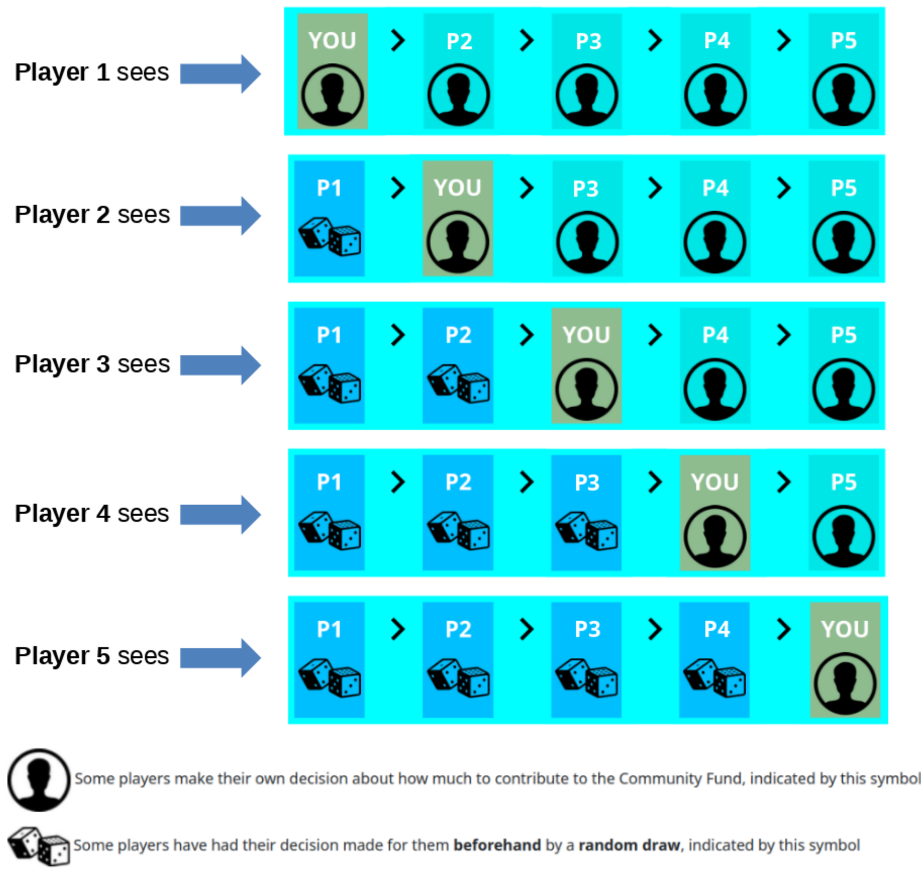
Analytic Strategy

We analyzed contributions using a linear regression model that included an interaction between order and the condition (Random Before vs. Random After) among sequential-condition subjects.

$$\text{contribution}_{ig} = \alpha + \beta_1\,\text{order}_{ig} + \beta_2\,\text{random\_before}_i + \beta_3\,(\text{order}_{ig} \times \text{random\_before}_i) + \varepsilon_{ig}. \quad (6)$$

We also analyzed the relationship between a player's own contribution and their predictions of others' moves, distinguishing between predictions for human versus random players. This was accomplished using the same partial correlations framework used in Studies 1-4. Robustness checks included using a T-test to test for a difference between no-delay and 80-second-delay simultaneous conditions.

Figure 7: Study 5. Stimuli for the Random Before Condition.



Notes: Stimuli for the Random Before condition. Players see a graphical representation of their position relative to other players that clearly conveys which players are having their moves made by a random process. The graphics for the Random After condition have the dice and human figures reversed.

4.9 Statistical Analysis

Primary statistical inference for all linear models relies on a non-parametric bootstrap procedure at the participant level to account for the non-normal distribution of the contribution data (which is heaped at 0, 50, and 100), and potential heteroskedasticity. Though predictions were directional, we report two-tailed tests with $\alpha = .05$ alongside 95% confidence intervals. The primary dependent variable, *contribution*, is treated as a continuous outcome in percentage points on the interval $[0, 100]$. We report 95% confidence intervals and two-tailed p-values derived from 10,000 bootstrap replicates. Figures display point estimates with 95% confidence intervals. We include the results of several robustness tests in the Supplementary Materials A.4. These include standard errors clustered at the group level, to account for any group-level dependence, and corrections to Study 3's preregistered partial correlations analysis (reported in the main text for all studies) to account for multiple measurements per person.

Coding. *Order* is an integer representing a player's position in the sequence. *SVO* is either a categorical factor (Individualistic vs. Prosocial) or a continuous *SVO angle*. In Study 4, *instruct_or_no* is a binary indicator for the instruction to maximize payoffs. In Study 5, *random_before* is a binary indicator for the "Random Before" condition. The subjective *wealth* covariate is mean-centered and standardized.

Software and reproducibility. All analyses were implemented in Python using statsmodels for estimation and custom resampling code for bootstraps. Analysis scripts and preregistrations are available at the OSF link given in the main text. All experiment software was written in the open-source oTree framework [37].

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Competing interests: The authors declare that they have no competing interests.

Data and materials availability: Data, analysis code, and experimental materials are available at https://osf.io/mykzt/overview?view_only=248c1173bf664c78a8ffdcf637dddbd3.

Supplementary Materials

A.1 The Public Goods Game

In a standard PGG, n players are each given an endowment e, and are asked to decide what proportion of their endowments to contribute to the public good, from nothing to all of it. A given player's contribution to the public good is represented by a. The total amount from all the players that is contributed to the public good, c, is then multiplied by a multiplier m (which must be less than the number of players), and this amount is distributed evenly among all the players—even those who chose to contribute nothing. An individual player's payoff function in a standard simultaneous-move PGG is as follows:

$$p = \frac{mc}{n} + e(1 - a) \tag{7}$$

Consequently, whenever the multiplier m is less than the number of players n, the group as a whole does better if everyone contributes their entire endowment (cooperates), but each individual player is better off if he or she contributes nothing (defects). Put another way, the total amount of money in the group is maximized if everyone cooperates, but any individual player always makes more by defecting—independent of anyone else's moves. Since other players do not know your move, they cannot change their own moves in reaction to it. If a group plays the game only once, it is impossible to build reputations, enact retribution, or to reward others for their actions.

A.2 Model

Here we provide a more precise statement of a model that generates the hypothesized interaction between the positional order effect and prosocial motivation.

Prosocial preferences

Consider a sequential PGG with n players endowed with 1 payoff unit each, and multiplier m, with $1 < m < n$. Players are indexed by their order of play in the sequence, $i = 1, ..., n$. Let a_i denote the contribution of player i, $0 \leq a_i \leq 1$, and p_i the payoff to player i.

$$p_i = 1 - a_i + \frac{m}{n} \sum_{k=1}^{n} a_k \tag{8}$$

Prosocial preferences are modeled through a prosocial parameter s_i where $s_i = 0$ indicates pure self-interest and $s_i = 1$ pure prosocial motivation. In keeping with the experimental setup, we assume that players do not learn the specific contributions of other players. The utility of player i is therefore a function of the two variables the player does or will know, namely contribution a_i and payoff p_i:

$$u_i \left(a_1, \ldots, a_n\right) = \left(1 - s_i\right) p_i + s_i m a_i \tag{9}$$

where p_i is determined by the game formula, Equation 8. A purely self-interested player ($s_i = 0$) will aim to maximize own payoff, $u_i = p_i$; a purely prosocial player ($s_i = 1$) will aim to maximize the impact of her contribution to the public good, $u_i = m a_i$. The prosocial motive, captured by the second term, thus reflects the impact of own contribution to the public good; other players' contributions enter the utility model only insofar they determine the first, self-interested utility term. In other words, players can: (a) care how their action affects the payoffs of others, (b) care how other players' contribution affect their own payoff, but (c) do not care how other players' actions affect each others' payoffs.

Decision dependent expectations

We assume that players compare expected utilities conditional on contributing ($a_i = 1$) or not contributing ($a_i = 0$), and choose whichever expected utility is higher (we ignore here fractional contributions). The decision criterion is therefore the difference between the two expected utilities:

$$a_i = 1 \Longleftrightarrow \mathbb{E}\left[u_i \mid a_i = 1, s_i\right] > \mathbb{E}\left[u_i \mid a_i = 0, s_i\right] \tag{10}$$

A player knows the value of their prosocial parameter and hence also knows the utility function in Equation 8. If she were just a spectator, not making a decision, her expectation of the contribution of another, randomly selected player would exhibit projection, along the lines of Bayesian updating. The simplest version of such updating is linear, with an intercept b and slope γ:

$$\mathbb{E}\left[a_k \mid s_i\right] = b + \gamma s_i \tag{11}$$

Prosocial players are more optimistic about the overall contribution level, all other things equal.

The critical assumption we now make is that expectations of future players' contributions are additionally influenced by a player's own action, while expectations of prior players' contributions are not influenced. Let $a_{k<i}$ denote the contribution of any player moving before player i, and $a_{k>i}$ the contribution of any player moving after player i. We assume:

$$\begin{aligned}
\mathbb{E}\left[a_{k<i} \mid a_i, s_i\right] &= b + \gamma s_i \\
\mathbb{E}\left[a_{k>i} \mid a_i, s_i\right] &= b + \gamma s_i + d\left(a_i - \mathbb{E}\left[a_k \mid s_i\right]\right) \\
&= (1-d)\left(b + \gamma s_i\right) + d a_i
\end{aligned}$$

where $\mathbb{E}\left[a_k \mid s_i\right] = b + \gamma s_i$ from Equation 11 is substituted in the final line.

There is no perceived causality with respect to previous players, since expectations are the same irrespective of contribution:

$$\mathbb{E}\left[a_{k<i} \mid 1, s_i\right] - \mathbb{E}\left[a_{k<i} \mid 0, s_i\right] = 0$$

There is perceived causality with respect to future players, proportional to the "as if" influence parameter d, which describes the extent to which the focal player believes other players will mirror his move:

$$\mathbb{E}\left[a_{k>i} \mid 1, s_i\right] - \mathbb{E}\left[a_{k>i} \mid 0, s_i\right] = d$$

The decision criterion in Equation 10 can be expressed as:

$$\begin{aligned}
\mathbb{E}\left[u_i|a_i = 1, s_i\right] - \mathbb{E}\left[u_i|a_i = 0, s_i\right] &= (1 - s_i)\mathbb{E}\left[p_i|a_i = 1, s_i\right] + s_i m - (1 - s_i)\mathbb{E}\left[p_i|a_i = 0, s_i\right] \\
&= (1 - s_i)\left(\mathbb{E}\left[p_i|a_i = 1, s_i\right] - \mathbb{E}\left[p_i|a_i = 0, s_i\right]\right) + s_i m \\
&= (1 - s_i)\left(-1 + \frac{m}{n}\mathbb{E}\left[\sum_{k=1}^{n} a_k|a_i = 1, s_i\right] - \frac{m}{n}\mathbb{E}\left[\sum_{k=1}^{n} a_k|a_i = 0, s_i\right]\right) + s_i m
\end{aligned} \tag{12}$$

where the first line follows from Equation 9 and the third line from Equation 10.

Assuming that expectations about contributions of previous players are not affected by own contribution, the difference in expected total contribution resolves as:

$$\begin{aligned}
\mathbb{E}\left[\sum_{k=1}^{n} a_k \mid a_i = 1, s_i\right] - \mathbb{E}\left[\sum_{k=1}^{n} a_k \mid a_i = 0, s_i\right] &= 1 + \mathbb{E}\left[\sum_{k=i+1}^{n} a_k \mid a_i = 1, s_i\right] - \mathbb{E}\left[\sum_{k=i+1}^{n} a_k \mid a_i = 0, s_i\right] \\
&= 1 + d(n - i)
\end{aligned}$$

Substituting into the criterion,

$$\mathbb{E}\left[u_i|a_i = 1, s_i\right] - \mathbb{E}\left[u_i|a_i = 0, s_i\right] = (1 - s_i)\left(-1 + \frac{m}{n}\left(1 + d(n - i)\right)\right) + s_i m$$

For any particular value of s_i, the minimum *as if* influence parameter $d^*(i)$ that leads to $a_i = 1$, i.e., full contribution to the Public Good, is computed as:

$$\mathbb{E}\left[u_i \mid a_i = 1, s_i\right] - \mathbb{E}\left[u_i \mid a_i = 0, s_i\right] = 0 \Longleftrightarrow d^*(i) = \frac{-m - smn + n}{m(n-i)} \tag{13}$$

Note that $d^*(i)$ is increasing in i (if the expression is positive) and decreasing in s_i. The increase in i is the positional order effect: Players later in the sequence require a higher value of $d^*(i)$ in order to contribute. Assuming that d is an exogenous parameter with some distribution in the participant sample, fewer players will clear the cutoff and contribute if they are later in the sequence. The decrease in s_i simply indicates that prosocial players require less acting *as if* in order to contribute.

The second implication of the model is that the slope of this function with respect to i (the term in the brackets in 13) is steeper if s_i is smaller, that is, if players are more self-interested. To show this, we differentiate:

$$\frac{dd^*(i)}{di} = \frac{1}{(n-i)^2}\left(\frac{n-m}{m} - \frac{s_i}{(1-s_i)}n\right)$$

which is decreasing in s_i. This is the hypothesized interaction of order and prosociality. Less prosocial players will exhibit a stronger effect. Conversely, the positional order effect should disappear if s_i is sufficiently high.

A.3 Sequential PD payoff matrix

A.4 Robustness checks

Here we present robustness checks on analyses from the main paper. Each table presents three specifications. Column (1) shows the main result as reported in the paper. Column (2) presents results with clustered standard errors at the group level (for order effect analyses) or with participant-level aggregation (for directional prediction analyses), accounting for potential dependencies in the data. Column (3) includes all participants, including those who

Table 1: Study 1 Sequential Prisoner's Dilemma payoff matrix

		Player 2	
		Transfer (cooperate)	Keep (defect)
Player 1	Transfer (cooperate)	$(.33, 0.33)$	$(0, 0.50)$
	Keep (defect)	$(.50, 0)$	$(.16, 0.16)$

Notes: All values in dollars.

failed comprehension checks, to test whether our main results depend on sample selection. Across all studies, our key findings remain robust to these alternative specifications.

Study 1 analyses

Table 2: Study 1: Cooperation Order Models (Logistic Regression)

	Dependent variable: coop_recode		
	Base	Clustered SEs	Incl. Failed Comprehension
	(1)	(2)	(3)
C(order)[T.second]	-0.286**	-0.286**	-0.035
	(0.123)	(0.124)	(0.085)
Intercept	0.290***	0.290***	0.532***
	(0.087)	(0.087)	(0.059)
Observations	1075	1075	2371
Pseudo R^2	0.004	0.004	0.000
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3: Study 1: Comparison of Directional Partial Correlations

	(1) Base Case	(2) Participant-level Aggregation	(3) Including Failed Comprehension
Forward Direction			
Pearson's r	0.313	0.313	0.244
95% CI	[0.240, 0.390]	[0.225, 0.393]	[0.190, 0.300]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	542	1075	1127
Backward Direction			
Pearson's r	0.266	0.266	0.207
95% CI	[0.190, 0.340]	[0.174, 0.351]	[0.150, 0.260]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	533	1075	1131
Difference (Forward - Backward)			
Difference	0.047	0.047	0.037
95% CI	[-0.069, 0.169]	[-0.075, 0.171]	[-0.043, 0.121]
p-value	0.404	0.444	0.352
z-score	0.834	–	0.931

Note: Bootstrap estimates with 10,000 replications. CI = confidence interval.

Study 2 analyses

Table 4: Study 2: Contribution Order Models, Sequential Games

	Dependent variable: contribution		
	Base	Clustered SEs	Incl. Failed Comprehension
	(1)	(2)	(3)
Intercept	59.000***	59.000***	61.463***
	(4.261)	(4.344)	(3.498)
order	-4.244**	-4.244**	-3.807**
	(1.957)	(1.998)	(1.624)
Observations	782	782	1116
R^2	0.006	0.006	0.005
Adjusted R^2	0.005	0.005	0.004
Residual Std. Error	44.362 (df=780)	44.362 (df=780)	44.307 (df=1114)
F Statistic	4.700** (df=1; 780)	4.510** (df=1; 780)	5.492** (df=1; 1114)

Note: *p<0.1; **p<0.05; ***p<0.01

Table 5: Comparison of Directional Partial Correlations

	(1) Base Case	(2) Participant-level Aggregation	(3) Including Failed Comprehension
Forward Direction			
Pearson's r	0.400	0.400	0.400
95% CI	[0.330, 0.470]	[0.296, 0.497]	[0.330, 0.470]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	590	599	590
Backward Direction			
Pearson's r	0.185	0.185	0.185
95% CI	[0.110, 0.260]	[0.083, 0.288]	[0.110, 0.260]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	608	599	608
Difference (Forward - Backward)			
Difference	0.215	0.215	0.215
95% CI	[0.122, 0.337]	[0.076, 0.351]	[0.122, 0.337]
p-value	< 0.001	0.003	< 0.001
z-score	4.084	–	4.084

Note: Bootstrap estimates with 10,000 replications. CI = confidence interval.

Study 3 analyses

Table 6: Study 3: Contribution   Order Models, Sequential Games

	Dependent variable: contribution		
	Base	Clustered SEs	Incl. Failed Comprehension
	(1)	(2)	(3)
Intercept	80.200***	80.200***	74.250***
	(17.133)	(18.636)	(13.620)
SVO_angle	-0.218	-0.218	0.104
	(0.605)	(0.618)	(0.479)
order	-14.383***	-14.383***	-11.861***
	(4.280)	(4.327)	(3.394)
order:SVO_angle	0.405***	0.405***	0.301**
	(0.152)	(0.145)	(0.120)
Observations	373	373	575
R^2	0.226	0.226	0.205
Adjusted R^2	0.220	0.220	0.200
Residual Std. Error	37.872 (df=369)	37.872 (df=369)	36.076 (df=571)
F Statistic	35.925*** (df=3; 369)	48.425*** (df=3; 369)	48.952*** (df=3; 571)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 7: Comparison of Directional Partial Correlations

	(1) Base Case	(2) Participant-level Aggregation	(3) Including Failed Comprehension
Forward Direction			
Pearson's r	0.412	0.412	0.406
95% CI	[0.330, 0.490]	[0.269, 0.540]	[0.340, 0.470]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	421	373	667
Backward Direction			
Pearson's r	0.252	0.252	0.329
95% CI	[0.200, 0.310]	[0.153, 0.352]	[0.280, 0.370]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	1071	373	1633
Difference (Forward - Backward)			
Difference	0.159	0.159	0.077
95% CI	[0.067, 0.285]	[0.028, 0.285]	[-0.001, 0.177]
p-value	0.002	0.015	0.053
z-score	3.116	–	1.932

Note: Bootstrap estimates with 10,000 replications. CI = confidence interval.

Study 4 analyses

Table 8: Study 4: Contribution   Order Models, Sequential Games

	Dependent variable: contribution		
	Base	Clustered SEs	Incl. Failed Comprehension
	(1)	(2)	(3)
Intercept	33.037***	33.037**	35.329***
	(11.829)	(14.077)	(11.592)
instruct_or_no[T.True]	27.246	27.246	24.990
	(17.812)	(20.639)	(17.600)
order	8.526**	8.526**	6.739*
	(3.770)	(4.326)	(3.660)
order:instruct_or_no[T.True]	-15.705***	-15.705***	-13.724**
	(5.601)	(5.912)	(5.481)
Observations	130	130	138
R^2	0.096	0.096	0.070
Adjusted R^2	0.074	0.074	0.049
Residual Std. Error	43.795 (df=126)	43.795 (df=126)	44.311 (df=134)
F Statistic	4.449*** (df=3; 126)	5.161*** (df=3; 126)	3.374** (df=3; 134)
Note:			*p<0.1; **p<0.05; ***p<0.01

Study 5 analyses

Table 9: Study 5: Contribution   Order Models, Sequential Games

	Dependent variable: contribution					
	Base	Clustered SEs	Incl. Failed Comprehension	Base	Clustered SEs	Incl. Failed Comprehension
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	37.213***	37.213***	39.416***	28.886***	28.886***	34.666***
	(7.845)	(7.773)	(7.119)	(6.353)	(6.223)	(5.705)
order	1.578	1.578	0.279	1.666	1.666	0.355
	(1.892)	(1.786)	(1.724)	(1.884)	(1.775)	(1.710)
order:random_before	-7.953***	-7.953***	-5.631**	-8.108***	-8.108***	-5.660**
	(2.701)	(2.504)	(2.465)	(2.693)	(2.473)	(2.440)
random_before	24.564***	24.564***	19.350**	25.053***	25.053***	19.084**
	(8.904)	(8.813)	(8.066)	(8.886)	(8.789)	(8.005)
wealth	-3.048*	-3.048*	-1.687			
	(1.628)	(1.656)	(1.436)			
Observations	485	485	593	487	487	597
R^2	0.032	0.032	0.020	0.024	0.024	0.016
Adjusted R^2	0.024	0.024	0.013	0.018	0.018	0.011
Residual Std. Error	41.869 (df=480)	41.869 (df=480)	42.068 (df=588)	41.960 (df=483)	41.960 (df=483)	42.060 (df=593)
F Statistic	4.006*** (df=4; 480)	4.180*** (df=4; 480)	2.933** (df=4; 588)	4.034*** (df=3; 483)	4.491*** (df=3; 483)	3.305** (df=3; 593)

Note: *p<0.1; **p<0.05; ***p<0.01

Table 10: Comparison of Directional Partial Correlations, Random After

	(1) Base Case	(2) Participant-level Aggregation	(3) Including Failed Comprehension
Forward Direction			
Pearson's r	0.054	0.054	0.096
95% CI	[-0.040, 0.140]	[-0.078, 0.192]	[0.020, 0.180]
p-value	0.234	0.425	0.019
n / $N_{clusters}$	484	251	596
Backward Direction			
Pearson's r	0.472	0.472	0.409
95% CI	[0.400, 0.540]	[0.354, 0.584]	[0.340, 0.470]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	520	251	612
Difference (Forward - Backward)			
Difference	-0.417	-0.417	-0.313
95% CI	[-0.524, -0.322]	[-0.574, -0.258]	[-0.422, -0.221]
p-value	< 0.001	< 0.001	< 0.001
z-score	-7.226	–	-5.852

Note: Bootstrap estimates with 10,000 replications. CI = confidence interval.

Table 11: Comparison of Directional Partial Correlations, Random Before

	(1) Base Case	(2) Participant-level Aggregation	(3) Including Failed Comprehension
Forward Direction			
Pearson's r	0.602	0.602	0.560
95% CI	[0.540, 0.660]	[0.483, 0.703]	[0.500, 0.610]
p-value	< 0.001	< 0.001	< 0.001
n / $N_{clusters}$	489	233	608
Backward Direction			
Pearson's r	0.167	0.167	0.172
95% CI	[0.070, 0.260]	[0.017, 0.317]	[0.090, 0.250]
p-value	< 0.001	0.030	< 0.001
n / $N_{clusters}$	443	233	556
Difference (Forward - Backward)			
Difference	0.435	0.435	0.388
95% CI	[0.379, 0.577]	[0.246, 0.612]	[0.331, 0.519]
p-value	< 0.001	< 0.001	< 0.001
z-score	8.029	–	7.814

Note: Bootstrap estimates with 10,000 replications. CI = confidence interval.

Study 5 subset analyses

This section contains exploratory analyses on subsets of the data from Study 5. These analyses condition on post-treatment variables, and so should be taken with a grain of salt. We report them here because we found them interesting and suggestive.

0s and 1s: When considering only participants contributing all or nothing, effect sizes increase The formalization in Supplementary Materials A.2 predicts that any given player who is both trying to maximize her own payoffs and who is acting *as if* in accordance with the model will either give 100% of the endowment or 0% to the public good, with a sharp transition. The point at which the shift from 100% to 0% happens as order increases is a function of d, the *as if* influence parameter, when s_i, the player's prosociality, and m, the game's multiplier, are held constant. Results from players who either give 0% or 100% of their endowment in Study 5 show increased effect sizes.

It may be the case that there is a weaker effect going backwards in time, towards players who have already made their moves. While our formalization only looks forward, our theoretical commitments merely see open fates as more compelling targets for acting *as if*.

Strict comprehension checks: When considering only participants who pass both pre- and post- comprehension checks, effect sizes increase Study 5 implemented several comprehension checks after the main task:

1. Could other players in the game see what choices you made? For instance, did other players know how much you chose to contribute?
   (a) NO, Other players could NOT see the choices I made in the game
   (b) YES, other players could see the choices I made in the game
2. Would you have more money right now if you had decided to contribute less to the Community Fund? (Asked only if the participant contributed something to the public good.)
   (a) NO, I would not have more money right now if I had decided to contribute less
   (b) YES, I would have more money right now if I had decided to contribute less
3. Is there any way the decisions you made while playing the game could have influenced what other players chose to do?
   (a) NO, my decisions could not influence what other players chose to do
   (b) YES, my decisions could influence what other players chose to do

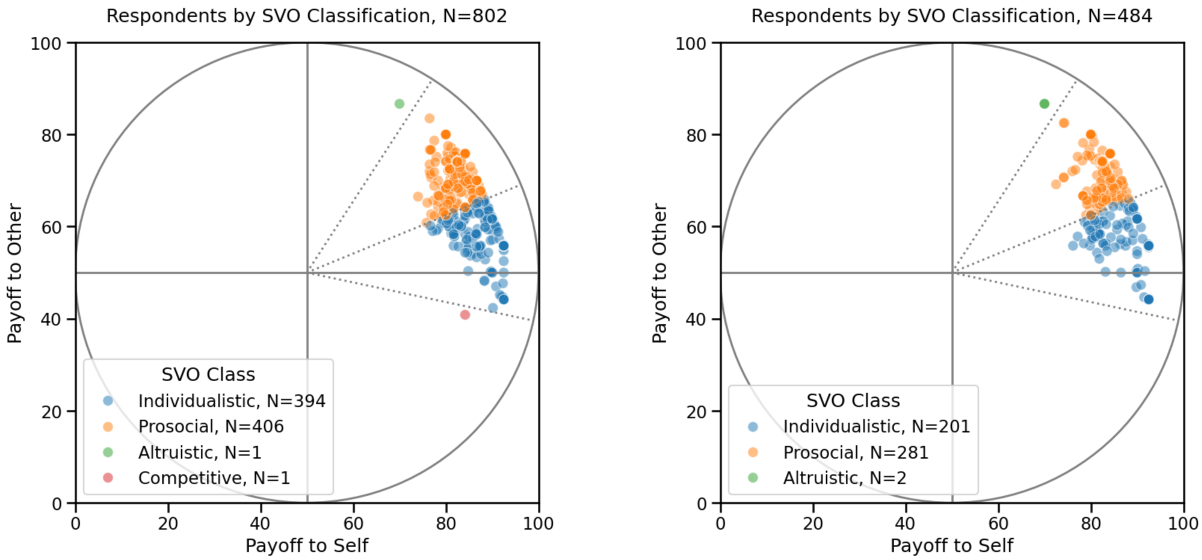
There is some evidence that variation in how people play economic games is a result of variation in understanding, and further that misunderstanding tends to lead to cooperation [39]. We observe an increase in effect size when we restrict analyses to the subset of participants who pass both the pre-game comprehension checks and an additional set of post-game comprehension checks added to Study 5. The fact that effect sizes increase when using a stricter comprehension check regime gives further support to the claim that the positional order effect is generated by people who best understand the game and who are trying to maximize their own personal payoffs, bolstering confidence in the effect.

Table 12: Study 5: Contribution   Order Models, Sequential Games

	Dependent variable: contribution		
	Base	0s and 1s	Pre-Post Comp. Checks
	(1)	(2)	(3)
Intercept	28.886***	24.930***	20.977***
	(6.353)	(7.843)	(6.771)
order	1.666	1.740	2.758
	(1.884)	(2.357)	(2.001)
order:random_before	-8.108***	-9.551***	-9.508***
	(2.693)	(3.344)	(2.869)
random_before	25.053***	32.231***	28.360***
	(8.886)	(10.969)	(9.517)
Observations	487	365	403
R^2	0.024	0.033	0.031
Adjusted R^2	0.018	0.025	0.024
Residual Std. Error	41.960 (df=483)	46.253 (df=361)	40.699 (df=399)
F Statistic	4.034*** (df=3; 483)	4.087*** (df=3; 361)	4.226*** (df=3; 399)
Note:			*p<0.1; **p<0.05; ***p<0.01

A.5 Social Value Orientation Distributional Data

Figure 8: Social Value Orientation distributions for Studies 2 (left) and 3 (right).



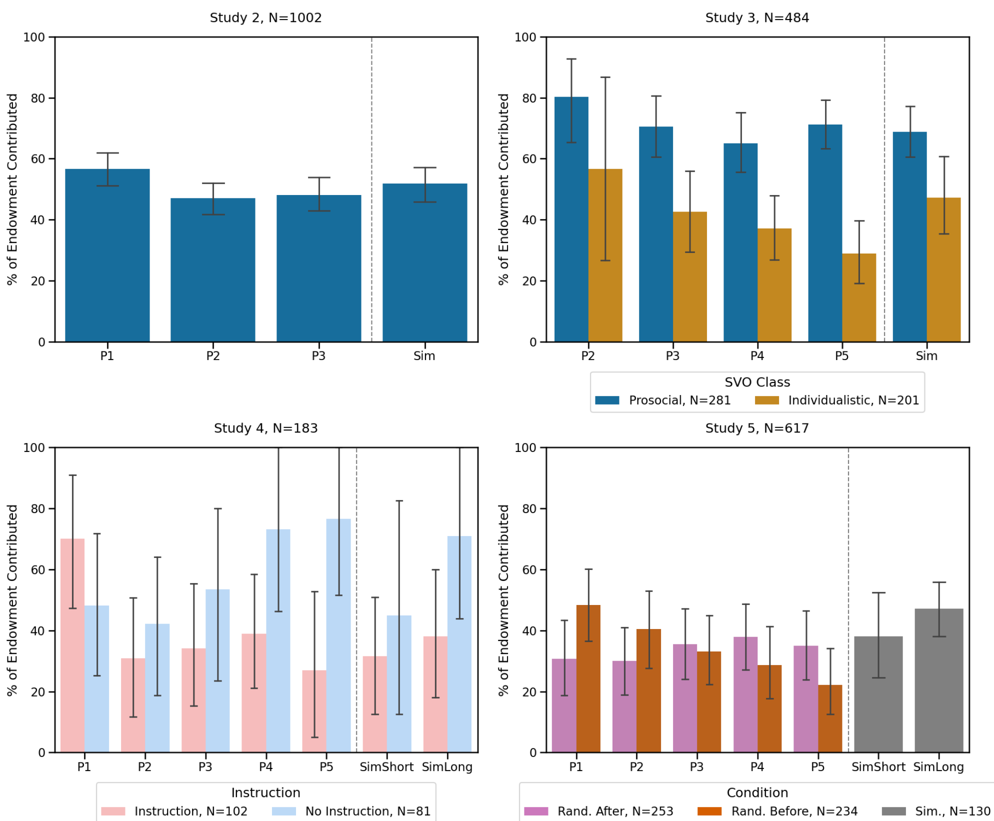
Notes: Participants in the sequential condition who passed comprehension checks.

Social Value Orientation distributional information is reported in Figure 8 for participants from Studies 2 and 3. Participants filled out an SVO dictator game slider task at the end of the experiments.

A.6 Simultaneous-move Data

Studies 2, 3, 4, and 5 included simultaneous-move control conditions where participants moved at the same time rather than sequentially. These controls help rule out explanations based on mere time in the experiment or waiting time. In Studies 4 and 5, we additionally imposed delays (short vs. long) matched to the waiting times experienced by late movers in the sequential condition. Across these simultaneous-move controls, there is no evidence that longer imposed delays are associated with lower contributions, consistent with the interpretation that the positional order effect is specific to sequential ordering rather than temporal factors.

Figure 9: Studies 2-5. Simultaneous-move data.



Notes: Simultaneous-move contribution data for Studies 2-5.

A.7 Preregistrations

- Study 1's preregistration can be found on osf.io. Study 1 deviated from the preregistration in that we stopped data collection half-way through, with 2,400 rather than 4,800 participants. Effect sizes for the manipulations the study was designed to measure were minimal and we did not want to use further resources. The pure positional order effect was a serendipitous finding in this study.

- Study 2's preregistration can be found on osf.io. Study 2 deviated from the preregistration in that the preregistration specifies data from 1,000 participants, while we actually collected data from 775 participants after the preregistration. When pooled with data from before the preregistration we reach 1002 participants. The budget for this study was planned for 1,000 participants total, rather than for 1,000 after the preregistration. The preregistration should have indicated 773 participants, to bring us to the budgeted 1,000 total. Study 2's preregistration also mis-specifies a model for predictions of others' moves, in particular testing the significance of the order x own response interaction, which is not informative. For simplicity, we apply the partial correlations framework used in other studies.

- Study 3's preregistration can be found on osf.io. We preregistered 800 participants who supply usable data and ended up slightly short due to not having perfect control over how many participants finish, with 783. This took 1298 participants total, rather than 1700, due to a higher-than-expected comprehension checks pass rate. This preregistration incorporates an hypothesis about quadratic effects, which are the subject of a separate paper.

- Study 4 was not preregistered.

- Study 5's preregistration can be found on osf.io. The preregistration specifies 500 participants in the sequential conditions, and we collected data from 487 due to not having perfect control over how many participants finish. It also incorporates an hypothesis about quadratic effects, which are the subject of a separate paper. The specified model for the positional order effect includes a control for self-reported wealth, which we report in the supplementary materials. The outcome is not substantially different from the model without the covariate, so we report the simpler model in the main text such that analogous models are used for all studies.

- Analysis and data files can be found on osf.io.