The Positional Order Effect: How Sequential Timing and Self-Interest Drive Prosocial Consumer Choice

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Abstract

Charitable giving campaigns, sustainable consumption, and crowdfunding campaigns often require prosocial consumer choices, where any single person has a tiny influence on the target outcome. Existing literature addresses how consumers may end up *wanting* to help others, but explanations for cooperative behavior among the overtly self-interested are lacking. We document a robust positional order effect that generates cooperation specifically among these self-interested players: In one-shot Public Goods Games where players move sequentially but do not observe others' moves, first-movers contribute the most to the public good and contributions decline progressively with last-movers making the smallest contributions. This is consistent with players maximizing their own payoffs assuming others who are yet to move will make the same choice they have. Three results support this interpretation: (1) the effect is generated by players who are maximizing their own payoffs, (2) instructing players not already self-interested to maximize their own payoff increases the effect, and (3) the effect is eliminated if the moves of future players, but not those of past players, are determined randomly. This phenomenon has direct implications for marketers designing cause-marketing campaigns or encouraging prosocial consumer behavior, suggesting that framing a choice as the first of many can increase participation.

Keywords:
Consumer Psychology, Consumer Choice, Prosocial Marketing, Cooperation, Sequential Games, Public Goods Game, Game Theory, Collective Action

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Introduction

Social cooperation without external monitoring is widely regarded as fundamental to the success of humans, sustaining charitable giving, teamwork, mass political participation, and personal sacrifice for family, tribe, or nation. People often face opportunities to incur an individual cost in exchange for a collective benefit, and there is a rich literature exploring the whys and wherefores (e.g., Henrich and Muthukrishna, 2021; Rand and Nowak, 2013). For example, a pedestrian can choose to throw litter into the gutter, or he can wait until he comes across a trash bin. A CEO might choose to move assets overseas in order to avoid taxes, or she might choose to avoid chicanery, keep assets domestically, and pay more in taxes—in the end, contributing to the public weal. A consumer might choose to pay an extra dollar at checkout as part of a retailer's donation-matching campaign, or he might tap "No" and move on. Each choice involves a tradeoff between what is good for the agent and what is good for the group. This tradeoff is widely studied using Public Goods Games (PGGs, Zelmer, 2003, see Appendix A). The PGG is used as a model of human cooperation because it captures this tension between the benefits accruing to a group via cooperation and the benefits accruing to an individual via defection, and this tension is characteristic of many problems we solve on a daily basis. In standard linear PGGs it is always better for an individual to defect no matter what others do, but it is always better for the group if everyone cooperates.

Most accounts of cooperation in humans are stories about why people end up *wanting* to cooperate. There may, however, be circumstances in which even people who are indifferent to the fates of others end up cooperating. Understanding such phenomena could lead to interventions which increase prosocial choice among the most self-interested consumers, in addition to furthering our understanding of how and why cooperative decisions are made in the first place.

We investigate a subtle variation on the classic one-shot PGG. In this variation, players move sequentially but, critically, *do not observe each others' moves*: a sequential PGG (SPGG) *without* observation. If players are maximizing their own payoffs conditional on those moving after them making the same move, cooperation should be proportional to the number of people *yet* to move. In this setting, we propose and find a "positional order effect": cooperation is highest for those moving first and declines with one's position in the sequence. Such behavior is puzzling under standard game theory, which suggests order should not matter when moves are unobserved. We argue that this behavior can be understood through the lens of an "acting as if" heuristic: players, particularly self-interested ones, behave *as if* their action is diagnostic of the actions of those who have yet to move. On this view, an early-mover cooperates because their action provides a signal that subsequent players will also cooperate, making cooperation seem individually profitable.

Our goal is, first, to robustly establish this positional order effect, and, second, to show that it is most pronounced among the very self-interested players who should be immune to giving up profits to help others. We do this by demonstrating that positional order effects exist *only* among players acting in their own interests, and by eliciting and removing the effect by manipulating the presence of people making their own decisions in the future, after the focal player. If the effect is present when real people are making their own decisions *after* a given focal player, but disappears when people moving after the focal player have their moves made randomly for them, this suggests a connection between the focal player's own decision and her expectations about what others will do in the future. The effect would then rely on the notion that other *people*, specifically, will make a decision similar to one's own. We conclude by elaborating on implications for practitioners and future directions for research.

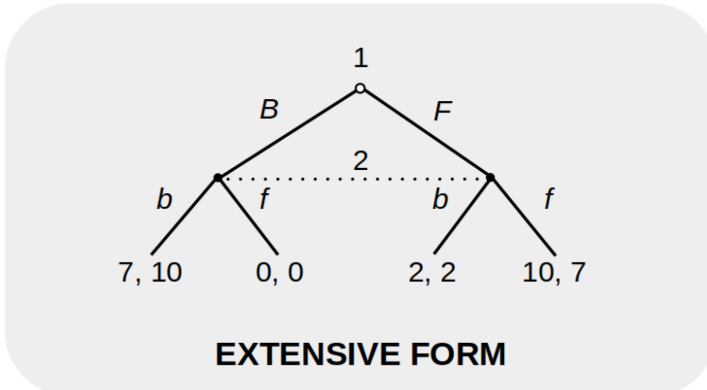
This effect is particularly relevant to consumer behavior in prosocial contexts, such as cause-marketing campaigns that leverage matching donations (Hildebrand et al., 2017). A consumer may be asked to donate $1 to a children's charity on a point-of-sale checkout screen, with the retailer pledging to match every donation. The consumer's single dollar has a vanishingly small impact on the vast societal problem, and their personal share of the societal benefit is even smaller. From a purely rational perspective, a self-interested consumer should keep the dollar. However, the decision changes if the consumer acts as if their choice is diagnostic of what others will do. If they are told they are one of the first customers to be asked, they might reason that their donation will be mirrored by the hundreds of customers who will follow. Their personal $1 contribution, when matched by the company and hypothetically universalized to other shoppers, suddenly feels like part of a large, meaningful collective donation. Conversely, a consumer who knows they are at the end of the campaign has fewer as-yet undetermined decisions, or "open fates" to reason over—and therefore may be less likely to donate. In the absence of clear information about others' behavior, acting as if others will do the same as you have done provides a psychological rationale for contributing, addressing a key challenge in promoting prosocial and sustainable consumption (White, Habib, and Hardisty, 2019). This implies that such campaigns may be more successful than normative decision theory would predict, and that the timing of the ask can be a critical, unexamined variable.

A key question, however, is *which* consumers are most influenced by this positional order effect. Consumer motivations are not homogeneous; at a particular moment, some individuals are genuinely prosocial and willing to incur costs to help others, while others are primarily driven by self-interest. Given the positional order effect is a result of trying to maximize one's own payoffs, we theorize that the effect should be pronounced only among self-interested individuals. For prosocial consumers, the decision to contribute is relatively simple. Their utility is primarily derived from the personal act of contributing—a "warm glow" or sense of duty—rather than from complex calculations about the final collective outcome. Because their motivation to cooperate is intrinsic, it is less sensitive to the strategic uncertainty of the situation and thus their behavior should remain stable regardless of their position in the sequence. For purely self-interested consumers, however, the decision is a difficult knife-edge calculation: they must weigh the certain cost of contributing against a highly uncertain collective outcome. It is precisely this group, focused on maximizing personal payoffs under ambiguity, who should be most receptive to a heuristic that provides a basis for action. The positional order effect, therefore, offers a window into the decision-making of the very consumers who are typically the hardest to persuade to contribute to a public good.

Conceptual Development

Many consumer choices are interdependent, where the value of a decision depends on the actions of others. Straightforwardly, it is possible for an event A to influence an event B only if A precedes B. However, traditional game theory ignores the ordering of moves in time and focuses exclusively on what information is available to a player when making a decision. This is because, on standard causal decision theory, mere antecedence in time does not guarantee a causal influence—it only *permits* it. von Neumann and Morgenstern (1944 / 2004) developed notation to show games graphically based purely on information directly relevant to a move which is available *before* that move is made (which they call "preliminarity"). This formalization ignores the chronological ordering of moves (what they call "anteriority")—though they note that this only holds with perfect information. Because of this, the extensive form representation of a simultaneous game is identical to that of the sequential version of the same game without observation (that is, the version where no player sees other players' moves; see Figure 1). By the mid 1980s, there was a small chorus raising the question of whether ignoring pure timing is a good idea (Kohlberg and Mertens, 1986; Kreps, 1990; Luce, 1992). Luce mentions the lack of a time variable in extensive form games (while real life inevitably involves one), and Kreps asks explicitly: "Can we find a pair of extensive form games that give rise to the same strategic form such that, when played by a reasonable participant population, there is a statistically significant difference in how the games are played?". The question has since been answered with a sure "yes": Rapoport (1997), for instance, reports public goods, resource dilemma, and coordination games that are sensitive to order of play alone.

Figure 1: Battle of the Sexes game in Extensive and Normal Form.



	Prize Fight	Ballet
Prize Fight	10, 7	2, 2
Ballet	0, 0	7, 10

NORMAL FORM

Notes: The Extensive Form representation of the game captures the chronological ordering of moves, with Player 1 choosing B or F before Player 2 chooses b or f. The Normal Form representation of the same game disregards information about chronological ordering of moves, meaning a simultaneous-move version and a sequential-move version without observation appear exactly the same.

Games with an element of coordination have the interesting property that players can coordinate based on pure order of play without any additional information at all, using order as a Schelling point (Schelling, 1960). For instance, people tend to "agree" without speaking to play the first-mover's preferred equilibrium in Battle of the Sexes (Cooper et al., 1993; Güth, Huck, and Rapoport, 1998; Rapoport, 1997; Weber, Camerer, and Knez, 2004), or they will "agree" that the first-mover should get the largest share of the gains in common-pool resource games (Budescu, Suleiman, and Rapoport, 1995; Budescu, Au, and Chen, 1997; Budescu and Au, 2002) and step-level PGGs (Chen, Au, and Komorita, 1996; Rapoport, 1997). This question of whether one's own action matters is also central to consumer participation in crowdfunding, where the belief in one's impact is a key driver of contributions (Kuppuswamy and Bayus, 2017).

Social dilemmas *without* an element of coordination, games like the PGG, pit what is good for you against what is good for everyone else. In these games there is no reason to condition your play on others' decisions, and therefore no obvious reason for order to influence play on standard theory. However, there is evidence that suggests people engage in causal thinking about others even in situations without rewards for coordination. Further, there is reason to think that uncertainty about the state of the world activates this sort of reasoning.

In an early study, Quattrone and Tversky (1984) report evidence for what they term "diagnostic" actions—actions that have no direct causal relationship to desirable outcomes, but which are indicative of them. They report that participants holding an arm in circulating ice water (a painful experience) are able to hold their arms in the water *longer* when they believe this is indicative of having a strong heart, and for shorter amounts of time when long durations are believed to be indicative of having a bad heart. The experience of holding one's arm in water has, of course, no bearing on heart type, but it does appear participants are changing the data they themselves produce in order to receive good news, in apparent disregard of the causal relationship.

In related work, Shafir and Tversky (1992) explore what they call "nonconsequential" reasoning, reasoning about what to do that at least *appears* to either not produce estimates of the consequences of an action or which ignores the consequences of that action. This class of decisions violate the Sure Thing Principle, which states that if X is preferred to Y under all states of the world, then X should still be preferred to Y even if the state of the world is unknown. For instance, there are many consumers 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 (Tversky and

Shafir, 1992). Shafir and Tversky call this pattern of events "accept when win, accept when lose, reject when do not know" and name it the "disjunction effect". In an experiment using the Prisoner's Dilemma, they observe more cooperation in one-shot games when uncertainty about the other player's move is highest: players cooperate more when they *do not know* the other player's move than either when they know it is Defect or when they know it is Cooperate.

Shafir and Tversky introduce the idea of quasi-magical thinking as a possible explanation for the disjunction effect. The idea is reminiscent of the illusion of control (Langer, 1975; Stefan and David, 2013); however, that work focuses on repeated tasks that do not involve other minds as games do. Masel (2007) offers a formalization of quasi-magical thinking where players, upon observing additional information during the game, update their prior distributions in the usual fashion—one's own behavior being just another data point. Daley and Sadowski (2017) develop a similar model of magical thinking that applies to players' preferences over actions rather than outcomes. However, neither formalization incorporates the arrow of time within a single game, whereas moving conditional on the assumption that others will mirror your move reflects reasoning only about others' *future* actions, which are presently undetermined.

There are two flavors of uncertainty at play here: "closed fates" and "open fates". Closed fates uncertainty is uncertainty about a counterpart's move when that move is not known to the player, but has already been made and is therefore fixed, whereas open fates uncertainty is uncertainty about a counterpart's move when that move has yet to be made at all (or, perhaps, is presently being made) (Morris, Sim, and Girotto, 1998). Miller and Gunasegaram (1990) demonstrate that, while events in the past are considered fixed, future events are treated as mutable. Moreover, future actions are perceived as more intentional and blameworthy than otherwise identical past actions (Burns, Caruso, and Bartels, 2012). Harris et al. (2011) document a surprising reversal: when an experimenter initiates a random process, adult participants prefer to guess the outcome *after* it has been determined. However, when the participants themselves start this process, they prefer to guess the outcome *before* it has been determined. This suggests that causal thinking is naturally and sensibly directed towards the future. The distinction between closed fates and open fates is clearly relevant for behavior, but is little-explored in the context of decision-making.

Social dilemmas *without* observation are the specific class of games further investigated here. As mentioned, they are games where players move one after another but do not receive information about others' moves. Players, therefore, know only their position in the sequence of moves ("I moved third"). Existing work on social dilemmas without observation is scant and mixed. However, we can safely conclude that uncertainty matters. Uncertainty about the state of the world seems to push people towards more prosocial actions (Croson, 1999; Hristova and Grinberg, 2010; Morris, Sim, and Girotto, 1998; Shafir and Tversky, 1992), though evidence for positional order effects in sequential PDs or PGGs (games without obvious benefits to coordination) is lacking. When considering quasi-magical thinking, Shafir & Tversky did not distinguish between open fates and closed fates and so could not have measured an order effect. Morris, Sim, and Girotto (1998) report more cooperation in first-movers and larger effects in open fates vs. closed fates cases, but most studies incorporating sequential PDs or PGGs with no observation find no effect of order alone (Abele and Ehrhart, 2005; Figuières, Masclet, and Willinger, 2012; Robinson et al., 2010; Steiger and Zultan, 2014). These studies were generally not designed to investigate the effects of order of play alone, and so tend to be under-powered to identify these effects. They also rematch participants randomly after each round and do so using small pools of students from the same university, not being quite as one-shot as could be hoped. In addition, it is probable that many participants are not even trying to maximize their own direct payoffs in these tasks, as is assumed by the task designs. This limits what can be inferred based on play: Budescu, Au, and Chen (1997) report that 47% of their participants are classified as "cooperative" (maximize joint own + other gains) and 2% as "altruistic" (maximizing others' gains); that is to say, half of their participants are not playing the game to "win" by the usual standards of game theory. While this is fine if the goal is to document participants' behavior as they come to the game, it can be a substantial problem when making the assumption (standard for game theory) that players are trying to maximize profits in incentive-aligned tasks.

It may be reasonable for a player to assume some similarity between herself and others in the absence of better information. Projection from personal decisions to collective behavior can be rational in the sense that it can be consistent with Bayes' rule (Dawes, 1989; Hoch, 1987; Tarantola et al., 2017). This could explain the sensitivity to other players making their own decisions (or not), but would not explain why the arrow of time ("closed fates" vs. "open fates") is important. Self-signaling via social projection could also explain cooperation among these self-interested agents. In a self-signaling account, individuals regard their own decisions as informative about their unknown "deep" characteristics, such as morality, affection, dedication, or willpower, and act to generate good news (a positive self-signal) about these characteristics (Bernheim and Thomadsen, 2005; Bodner and Prelec, 2003; Dhar and Wertenbroch, 2012; Mijovic-Prelec and Prelec, 2010). However, as with social projection, the usual formulation of self-signaling does not naturally provide a direction in time for the effect. It is possible to self-signal about open and closed fates, and so an explanation of why participants only consider open fates would be required. This process of maximizing "news value" is also reminiscent of Evidential Decision Theory (Gibbard and Harper, 1978), Subjective Expected Relative Similarity (Fischer and Savranevski, 2023), Superrationality (Hofstadter, 1983), and Newcomb's problem (Nozick, 1969; Lewis, 1979; Wolpert and Benford, 2013). These theories all, to some degree, try to make sense of doing the thing that *correlates* with success, rather than the thing that *causes* success (causes—on some model of the world).

A related body of work examines universalization as an explanatory model for moral judgment. The basic idea is that, at some level, people ask themselves "What if everyone did this?" in order to determine what is right and wrong. Roemer (2010, 2015) develops the idea of a "Kantian equilibrium", where each player asks: "If I deviate from my action and everyone else were to deviate in the same way, would I prefer the consequences of the new action profile versus not deviating at all?", and Levine et al. (2020) present a computational model of universalization in moral judgment, along with evidence from vignette studies and, significantly, refine the motivating question to, "What if everyone felt free to do that?", which adds a sense of temporal direction—towards open fates. It may be that universalization in the moral domain is a special case of the more general phenomenon we investigate here.

Overview of Studies

Below we present five empirical studies that investigate the positional order effect using sequential games. All studies help establish the effect, where players tend to give less to the public good as their position in the sequence of players nears the end. In addition, we show this effect is driven by "individualistic" players—those who are interested in maximizing their own direct payoffs, as opposed to "prosocial" players whose behavior reflects a desire to help others even at a cost. Prosocial players have reasons to cooperate that are entirely independent of order, so we would expect any order effects that are present to be much reduced in prosocials. Individualistic players who are maximizing their own payoffs by conditioning their own move on others doing the same as them in the future should have a contribution level highly sensitive to the number of "open fates" remaining after them—how many people are yet to move. We initially noticed a positional order effect in Study 1a, which was designed to investigate other hypotheses. Study 1b is the first study designed to investigate the positional order effect specifically, and it does so with a three-person SPGG, categorizing players into prosocial and individualistic categories using a Social Value Orientation (SVO) battery. SVO is a measure of willingness to give up gains in order to 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.[1] Participants can

[1]The standard battery produces a continuous SVO "angle" measure that allows for categorization into individualistic (concerned only with what is best for self), competitive (maximize own outcomes as with individualistic, but also minimize the outcomes for others), prosocial (maximize outcomes for both self and other), and altruistic (eager to give up own gains to help others) categories.

choose to forego gains (or even pay costs) to help or hurt the other player. Study 1c is a refinement and replication of 1b, expanding it to a longer sequence of players.

Studies 1a - 1c establish the positional order effect's presence in individualistic but not prosocial players, and set the scene for Studies 2a and 2b. Studies 2a and 2b work together to narrow the range of possible underlying mechanisms which could drive the positional order effect. In this second set of studies, we wanted to focus only on participants who show the positional order effect: those who are trying to maximize their own direct payoffs. Filtering out participants who arrive at the study not self-interested enough would have been a logistical challenge: self-interested participants would have to arrive at a consistent rate, enough to reliably fill SPGG groups quickly so other players do not wait too long. To get around this problem, we designed Study 2a to test whether the mere *instruction* to maximize one's own payoffs caused otherwise prosocial players to behave like individualistics and produce the effect. Study 2a showed it does, and this made us confident in deploying it in a larger, subsequent study: Study 2b. Study 2b instructs all comers to maximize their own payoffs and, critically, directly tests the causal linkages involved in deciding to cooperate conditional on others making the same move in the future. Study 2b asks whether we still observe a positional order effect in the case where all players after a focal player have their contribution decisions delegated to a random process. If the effect is present when random movers are *before* the focal player, but absent when random movers are *after* the focal player, this would indicate the effect requires having real people who have not yet made a decision, but who will, moving after the focal player—consistent with causal thinking about others' future actions being at play.

All studies are real-time, one-shot linear SPGGs with a multiplier of two (considering the PD as a two-person PGG with contribution of 0 or 100% of endowment). 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 participate in a brief text chat with their groupmates before learning about the task. The purpose of the chat is to assure participants that they are playing in real time with real people and, generally, to give the task more psychological reality than might be felt in an online task with no human interaction. The group they play the game with is the same group from the chat room. All except Study 1a have simultaneous-play PGG control conditions (see Web Appendix B:), and all players pass familiarization tasks and comprehension checks. All experiments except 1a[2] share the following three up-front comprehension and attention check questions:

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. Later studies incorporate additional training and comprehension checks.

In total we tested 3686 participants distributed across five experiments. A convenience sample provided by Amazon Mechanical Turk (MTurk) was selected for Studies 1a, 1b and 1c because it was a reasonable approximation of American adults for our purposes. We selected CloudResearch's filtered MTurk panel for Studies 2a and 2b because it provided among the highest-quality online panel data (Hauser et al., 2023; Douglas, Ewell, and Brauer, 2023) at

[2]Study 1a's comprehension check questions are similar, and are detailed in the relevant methods section, 4.2.

that time. This work makes the point that these effects exist in human populations, and it is left for future work to examine how they vary across ages, sexes, SES, cultures, and other characteristics of interest.

We have made study materials, data, and analysis scripts available on osf.io. All studies except for Study 2a were preregistered on osf.io (see Appendix Web Appendix C: for details), and all experiments we have conducted involving sequential games are reported here. All preregistrations include hypotheses, manipulations of the independent variables, measures for the dependent variables, desired sample size, exclusion criteria, and an analysis plan. All studies were approved by MIT's Committee on the Use of Humans as Experimental Subjects (COUHES) and comply with all relevant privacy and ethical regulations. We obtained electronic consent from all participants. The authors used artificial intelligence as part of an integrated development environment when writing the data analysis scripts (OpenAI's GPT4 and Anthropic's Claude 3.7 Sonnet), as well as with Overleaf's LaTeX environment to perform typesetting (Google Gemini 2.5 Pro), and the authors have no known competing interests to declare. All experiment software was written in the open-source oTree framework (Chen, Schonger, and Wickens, 2016), and data analysis was conducted in python using statsmodels (Seabold and Perktold, 2010) for linear models. We will now detail each study.

Study 1a: Sequential Prisoner's Dilemma

Study 1a is a Sequential Prisoner's Dilemma (PD) study that was not designed to test for a positional order effect, but in which we observed one seredipitously. It is a Prisoner's Dilemma rather than a Public Goods Game like the other studies in this series, but as noted a Prisoner's Dilemma is formally equivalent to a two-player Public Goods Game. It is similar in structure to Studies 1b and 1c. Study 1a contained a number of exploratory conditions not relevant to this line of inquiry, over which we collapse here. These included a manipulation that involved mentalizing about the other player crossed with supplying information about behavior at the population level or not, as well as a standard sequential PD and matched control conditions. As Study 1a was not designed to test for a positional order effect, the effect observed is replicated in subsequent studies.

4.1 Participants

2371 U.S.-based participants from Amazon Mechanical Turk completed the study. Of those, 45% (1075) passed the comprehension check questions. Among participants who passed the comprehension checks, mean total pay per participant (including bonuses for accurate predictions) is \$.90 ($SD = .19$), yielding an hourly rate of \$10.20 at an average 6.0 minutes duration.

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

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

Notes: All values in dollars.

4.2 Method

Players arrive at the experiment web page, are consented, and then engage in a real effort task as attention and activity verification (transcribing nonsense sentences)[3]. The chat room then provides 30 seconds for exchanging a hello or brief message, confirming that their teammate is indeed a real person. After leaving the chat room, the game is described as an allocation task where players choose to "keep" an initial endowment or "transfer" the

[3]Since participants are grouped together for real-time experiments, we must ensure that those who are being grouped are active directly before they are put into groups. If they are not, responsive players may be grouped with non-responsive players.

endowment to the other player, with the transfer doubled before reaching the other player. The payoff matrix is given below in Table 1:

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?

Player 1 moves first, followed by Player 2. After making a move, both players predict "How likely is it that the other person in this game TRANSFERRED?" on a scale from 0-100. Players also answer a population version, "How likely is it that an average person who plays this game would TRANSFER?". Players then exit the experiment and are paid.

4.3 Results

53% of players cooperated (SD = .50) and the average payoff was \$.58 (SD = .18). First-movers cooperate more than second-movers. A logistic regression of Transfer decision on Order reaches significance (Odds Ratio = .751, 95% CI = [.591, .955], $z = -2.333$, $p = .020$). We also observed a strong non-preregistered impact of decision on perception of teammate's behavior relative to the population. Players who keep their endowment think that their teammate is less likely to transfer than the population at large ($M_{teammate-population} = -2.25$), while those who transferred believe their teammate is more likely to transfer ($M_{teammate-population} = +2.76$) A linear regression shows a significant effect, $\beta = 5.01$, 95% CI=[3.211, 6.809], $F(1, 1073) = 29.874$, $p < .001$.

4.4 Discussion

Study 1a provides some evidence for the idea that first-movers would be more likely to cooperate than those moving afterwards despite the only difference between conditions being the knowledge of where one is in the sequence of moves. This sparked the desire to investigate further. In addition, we see strong evidence that players view their own moves as indicative of the moves of others. The positional order effect is the main subject of the next study.

Study 1b: 3-Person Sequential Public Goods Game

Study 1b tests for a positional order effect alone, removing the additional manipulations found in Study 1a. We also sought to investigate whether any such effect is driven by players who are trying to maximize their own payoffs, or by those who are keeping others' interests in mind in addition to their own.

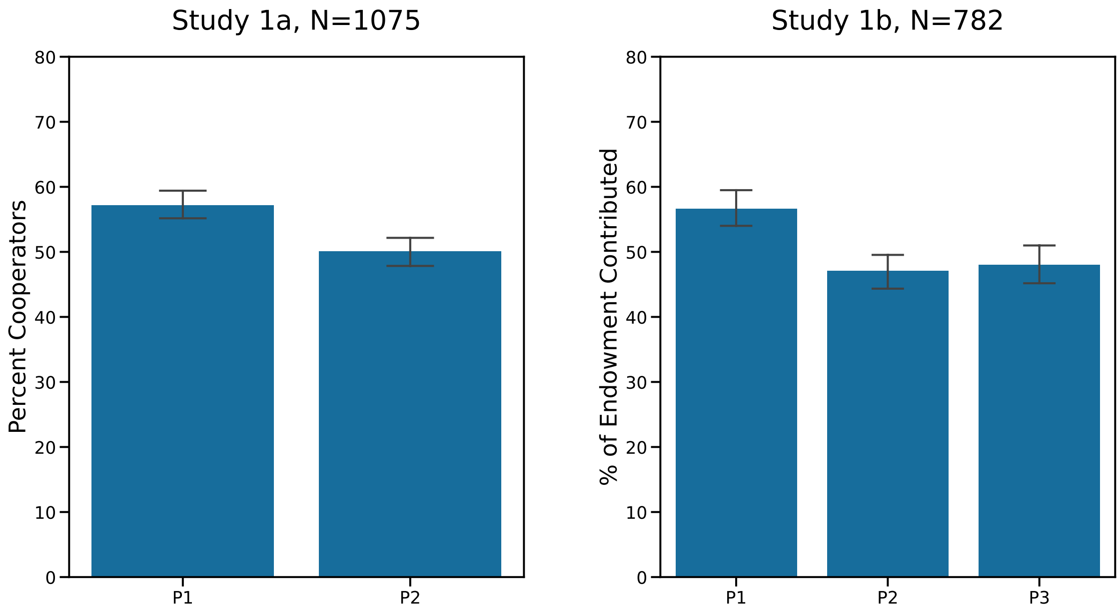
5.1 Participants

1444 U.S.-based participants from Amazon Mechanical Turk completed the study. Of those who passed up-front bot checks and finished the task, 69.0% (1002) passed all of the comprehension check questions. To estimate the sample size required, we performed a power analysis via simulation using pilot data. Among those 1002 participants, 782 were in the Sequential condition and 220 were in the Simultaneous condition. Mean total pay per participant (including bonuses for accurate predictions) was \$3.30 (SD = .74), yielding an hourly rate of \$19.94 (SD = 7.62) at 9.8 minutes average duration.

5.2 Method

Study 1b is a one-shot SPGG with a multiplier of two. Three players can transfer any part of their individual \$1 endowment to the public good. The total amount transferred

Figure 2: Studies 1a and 1b. Contributions to the public good fall with increasing order.



Notes: We observed some evidence for a decline in contribution to the public good with increasing order in Studies 1a and 1b, which led to further investigation. Participants who passed comprehension checks, SEMs.

from all participants is then doubled and distributed evenly among the players, irrespective of individual transfers. Order of play is determined randomly, with no communication among players during the game. The only difference in information among the players is knowledge of their position in the sequence. Each group was randomized to either the sequential game or a simultaneous-move condition. Players arrive at the experiment web page, complete a consent form, and then engage in a real effort task transcribing nonsense sentences in order to filter out bots. After this, they enter a wait room and form groups of three. Then, once groups are formed, they are placed in a chat room for 30 seconds to give participants the (correct) sense that the experiment is, in fact, a real game in real time with real people. After the chat, participants learn the game they will play. They are provided with an explanation of the rules of the game (which appear on every subsequent page for reference). The PGG is framed as a question of how much to contribute to a "Community Fund". A player can "transfer" some or all of her endowment to the Community Fund, and she may "keep" some amount. Instructions include if-then statements about the consequences of certain moves to aid understanding.

Participants are then asked three comprehension and attention check questions, and afterwards make their move. The contribution page includes a graphic at the top highlighting their place in the sequence of moves in red (see the stimuli in supplemental materials). Players in the simultaneous condition do not see any indication of sequence since they are moving simultaneously. Participants then complete incentivized prediction questions, where they are compensated based on the mean squared error between predictions and actual values, and then an SVO-dictator game slider battery (Murphy, Ackermann, and Handgraaf, 2011)[4]. Players complete some demographic questions, exit the experiment, and are paid.

5.3 Results

Study 1b investigated how contribution to the public good varied with order of play, with the additional prediction that any order effects will be driven by in individualistic players but not in prosocial players, as determined by an SVO battery. Almost all[5] participants are classified as either individualistic or prosocial. The mean contribution for individualistic participants was 40.4% (SD = .44) of the $1.00 endowment, and for prosocials 66.1% (SD = .41). Individualistic participants left with slightly more money from the SPGG itself, $1.63 (SD = .44) vs. prosocials $1.56 (SD = .43).

We find some support for the positional order effect in this study. While the preregistered backwards-difference coded model contribution $\sim$ order does not find significance, we do see a decline in contribution without backwards-difference coding, $N = 782$, $\beta = -4.235$, 95% CI = [-8.139, -.322], $p = .034$[6]. Participants classified as prosocial exhibit no significant

[4]SVO is measured post-treatment, but we do not observe an effect of treatment on SVO, N = 601, $\beta = 1.027$, 95% CI = [-.344, 2.414], $p = .145$. See Appendix Web Appendix A: for the distribution.

[5]One participant was classified as Altruistic, and one as Competitive. These participants' data are excluded from SVO analyses.

[6]Contributions to the public good were distributed non-normally in all cases, with modes at 0%, 50%, and 100% of the endowment. When using OLS linear regressions in this and subsequent studies we bootstrap standard errors and p-values to make results robust to non-normal errors and so do not report F statistics.

differences in contribution levels as function of order, N = 309, $\beta = -1.449$, 95% CI = [-11.133, 8.487], p = .774, while we do see a difference among individualistic participants between first-mover data and grouped second- and third-mover contributions, N = 290, $\beta = -14.242$, 95% CI = [-24.729, -3.584], p = .008, see Figure 2. As hypothesized variation in contribution by order is driven by individualistic participants, but interestingly, third-mover contributions among individualistics were on average larger than those of second-movers (31.0%, SD = .38 vs. 41.0%, SD = .46), though the difference is not significant in a linear model, N = 190, β = 9.994, 95% CI = [-2.008, 22.044], p = .101.

In addition, we find support for the prediction that correlations between a player's own move and her predictions of other players' moves are stronger going forward in time vs. backwards. The interaction term in the preregistered regression of predictions of others' moves on the player's own contribution interacted with a binary future/past variable (predicted_value ~ contribution * binary_position) does not find significance, N = 1198, β = .074, 95% CI = [-.004, .151], p = .067 with cluster robust errors, but when applied to only individualistic players a significant equation is found, N = 580, β = .172, 95% CI =[.059, .287], p = .004. There is no effect among prosocial players, N = 618, β = .002, 95% CI = [-.116, .120], p = .970.

5.4 Discussion

Study 1b was designed to investigate pure order effects, and provided evidence that any such effects are driven by the actions of players who tend to maximize their own direct payouts. Those who are most concerned with maximizing their own rewards tend to contribute less to a public good as their order in an SPGG increases, with some variation. Further, self-interested players are willing to bet that other players who have not yet moved will make moves more similar to their own relative to those who have already moved, which is consistent with thinking those moving after you will mirror your move. Study 1b provides evidence for a relationship between positional order effects and self-interest but is not definitive, so investigation with a more sensitive study was warranted. We continue with Study 1c, a refinement of this study.

Study 1c: 4-Person Sequential Public Goods Game

Study 1c is an evolution of Study 1b that is aimed at investigating the role of a player's self-interest in producing positional order effects. It incorporates several refinements to Study 1b's design in an effort to make the experience more intuitive for participants. It also raises the number of players to five from three, though only data from P2 - P5 are reported due to a technical error affecting P1.

6.1 Participants

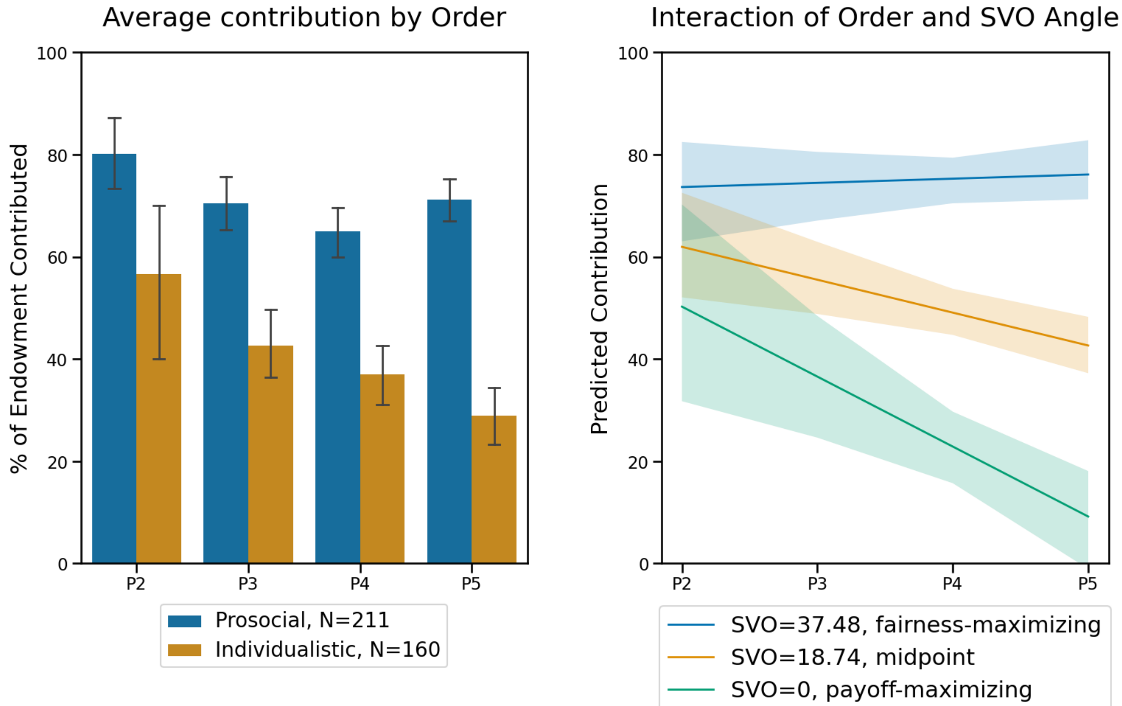
1298 U.S.-based participants from Amazon Mechanical Turk passed up-front bot checks and finished the task. Of those, 788 (62%) passed all of the up-front comprehension check questions and have their data included. Among these, 43.6% were female, 54.5% male, .4% other, and .6% declined to specify. Average age was 37, and mean total pay per participant (including bonuses for accurate predictions) was \$3.52 (SD = .51), yielding an hourly rate of \$11.39 at 18.6 minutes average duration. To estimate the sample size required, we performed a power analysis via simulation using pilot data.

6.2 Method

Study 1c is a one-shot SPGG similar in structure to Study 1b but incorporating a number of improvements. It was designed for five players rather than the three of Study 1b, which allows for better resolution of order effects. The up-front chat was 60 instead of 30 seconds, allowing a little more time for a short conversation (we observed in Study 1b that 30 seconds was usually only enough for greetings). Players also do not have a chat box while waiting for their group to fill; previously, chat time was waiting time + 30 seconds, which led to variable total chat times. Instead, in Study 1c players have a simple game on the wait screen.

In addition, a player's place in the sequence is made much more salient. Players are shown their position in the sequence as they wait for others to move in addition to the simple game. Inputs are also changed to sliders with anchors that display the consequences (e.g., "KEEP FOR SELF: $0.43 ↔ CONTRIBUTE TO FUND: $0.57"), as opposed to simple numerical input boxes. These changes taken together were meant to provide a better experience for the participant.[7]

Figure 3: Study 1c. Positional order effects become stronger as participants become more self-interested.



Notes: [Left] We observe a decline in contributions with increasing order among individualistic, but not prosocial, participants. Participants who passed comprehension checks, SEMs. [Right] Analysis with the more sensitive SVO angle measure, as opposed to categorical SVO classes, yields variation in the effect of order on contribution with SVO angle. Participants who passed comprehension checks, 95% CIs.

[7]As with Study 1b, SVO is measured post-treatment with a battery of slider of dictator games, but we do not observe an effect of order on SVO angle, $N = 373$, $\beta = -.495$, 95% CI = [-1.898, .900], $p = .491$.

6.3 Results

Study 1c provides further evidence that, among specifically self-interested players[8], contribution to the public good declines as position in the order increases. A technical error meant that all first-movers and some later players were forced to make decisions too quickly, often timing out. We have excluded all affected players, which includes all first movers and a decreasing proportion of subsequent players (since response time allotted for a given player was a function of previous players' response times). All included players received the full allotted decision time. We believe the marginal benefit of having 5 vs. 4 players is minimal, and so report results from 783 unaffected participants here, 484 of whom passed comprehension checks (373 in the Sequential treatment, and 111 in the Simultaneous treatment).

The mean contribution for individualistic participants was 38% (SD = .43) of the $1.00 endowment, and for prosocials 70% (SD = .37). Individualistic participants left with slightly more money from the SPGG itself, $1.66 (SD = .40) vs. prosocials $1.48 (SD = .38).

For Players 2-5 the preregistered linear regression (contribution $\sim$ order) among SVO-individualistic players reaches significance: $N = 160$, $\beta = -7.928$, 95% CI = [-15.255, -.331], $p = .040$. We specified a categorical SVO measure in our preregistration, but the continuous SVO angle measure is strictly better in that it avoids throwing away information. When interacting contribution with the SVO angle measure (contribution $\sim$ order*SVO_angle) we see $N = 373$, $\beta = .401$, 95% CI =[.106, .685], $p = .009$ for the interaction. As expected, the effect of order on contribution becomes larger as SVO angle nears 0°(perfect self-interested play), see Figure 3.

The preregistered prediction that the partial correlation between one's own contribution and prediction of others' contributions is stronger going forward in time (towards the open fates of those who have not yet moved) is well-supported. Among all participants, for the forward direction we find $N = 421$, Pearson's $r = .41$ 95% CI =[.33 .49] $p < .001$, and for

[8]Similar to Study 1b, nearly all participants were SVO classified as individualistic or prosocial; two were classified as altruistic, and these participants' data are excluded from categorical SVO analyses. See Appendix Web Appendix A: for the distribution.

backwards in time $N = 1071$, Pearson's $r = .25$ 95% CI =[.2 .31] $p < .001$, z-score for the difference of .159 = 3.11, 95% CI =[.067, .285], $p = .002$.[9]

6.4 Discussion

Study 1c further investigates the positional order effect among self-interested players that was hinted at by Studies 1a and 1b, and does so with a study that is better-suited to answering that specific question. The evidence from Studies 1a-1c, taken together, was sufficient to make us confident in moving on to a second series of studies that probe the mechanism behind the positional order effect among self-interested participants.

Study 2a: 5-Person Sequential Public Goods Game with Induced Self-interest

Having good evidence that positional order effects are driven by players who are trying to maximize their own payouts, we moved on to investigating the mechanism with Studies 2a and 2b. We first attempted to pre-test participants with the SVO measure in order to filter out participants who were not trying to maximize their own profits. Our real-time interaction paradigm meant that it was not possible to fill five-person groups with only self-interested players fast enough, so we explored alternatives. Study 2a tests whether merely *instructing* all comers to maximize personal payoffs generates a positional order effect similar to that observed among participants who arrive at the study already self-interested. If so, it would not be necessary to select for already self-interested participants in a larger study; instead, all comers could be instructed to maximize their own payoffs. This study was not preregistered; It was intended as a quick test of new software and the prompt to be greedy. Its primary purpose was to gauge the effectiveness of this prompt and inform confidence in deploying it at larger scale in Study 2b.

[9]There are more backwards-facing predictions than forwards-facing because data from Player 1 was excluded, and Player 1 only produces forward-facing predictions.

7.1 Participants

197 U.S.-based participants from CloudResearch's filtered MTurk panel passed up-front bot checks and completed the study. Of those, 183 (93.0%) of those passed all of the up-front comprehension check questions and have their data included. The population was 31.1% female, 66.7% male, 1.1% other, and .5% did not specify, with an average age of 40. Mean total pay per participant (including bonuses for accurate predictions) was $3.81 (*SD* = .65), yielding an hourly rate of $14.25 at 16.6 minutes average duration.

7.2 Method

Study 2a served as a testbed for some important improvements to the design from Study 1c. In Study 2a, SVO is not measured. Instead, players are randomized to an "Instruction" and a "No Instruction" condition. In the Instruction condition, players see a prompt:

> 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.

In addition to the prompt, Study 2a incorporates four substantive improvements over Study 1c. First, Study 2a adds an additional simultaneous-play control condition that implements a delay of 80 seconds. These participants will wait about as long as sequential-condition players who are moving last (P5). This condition was incorporated to allow us to rule out effects dependent on time spent waiting. While waiting, participants are shown the task's standard wait screen which incorporates the option to play a simple game to encourage engagement with the task. Second, we incorporate an interactive practice game after the instructions and comprehension questions. This practice game asks participants to calculate the correct answers to questions about payoffs for hypothetical players in a PGG.

Participants are paid for correct answers and they can make multiple attempts at any given question, limited only by time. Third, participants move in lock-step with one another. Each page in the study takes an allotted amount of time no matter the participant's behavior to ensure that information cannot leak to others via response times (e.g., P3 could move quickly relative to P2, and P4 could note the difference). Pages on which players make their contribution or prediction decisions do not force a player to stay for a certain amount of time, but rather let the player move on to a wait page that soaks up any remaining time. Finally, Study 2a incorporates an improved up-front English fluency filter that relies on a native speaker's ability to quickly complete idioms in order to ensure participants are real people who speak English fluently.

7.3 Results

We observe a positional order effect given the instruction to maximize payouts. A linear regression of contribution on order interacted with a binary instructed/not instructed to maximize payoffs variable, contribution ~ order * instruct_or_no, detects the interaction effect, $N = 130$, $\beta = -15.745$, 95% CI = [-25.968, -5.303], $p = .005$.

7.4 Discussion

Participants receiving the instruction to maximize payouts in Study 2a show a decline in contribution with increasing order. The fact that all comers to the experiment can be induced to produce the effect suggests it is a strategy held generally, which can be deployed when appropriate, rather than a stable feature of some subset of the population. Study 2a makes use of a small sample, and consequently there is substantial noise in estimates, but this study gave us enough confidence to deploy this technique in the next, larger experiment.

Study 2b: 5-Person Sequential Public Goods Game with Random Moves

Study 2b incorporates the improvements from Study 2a and extends it in order to test the underlying psychological mechanism. It does this by applying the instruction to act to maximize one's own payouts to all participants and at larger scale, but with two new conditions: all participants are either told that every player *before* them has had their contribution determined randomly ("Random Before"), or that every player moving *after* them has had their contribution determined randomly ("Random After"). This clarifies whether the positional order effect is driven by the fact that other *people*, specifically, will be moving after the focal player—even though she cannot see their moves.

8.1 Participants

747 U.S.-based participants from CloudResearch's filtered MTurk panel completed the study. Of those, 617 (83%) passed the up-front comprehension checks and have their data included in analyses (487 in the Sequential treatment, and 130 in the Simultaneous treatment). Of participants passing the comprehension checks, 45.1% were female, 53.8% male, .3% other, and .5% did not specify. Mean total pay per participant (including bonuses for accurate predictions) is \$4.75 (SD = .67), yielding a mean hourly rate of \$15.16 at 18.4 minutes average duration. To estimate the sample size required, we performed a power analysis via simulation using pilot data and data from previous experiments. Comprehension check pass rates and effect sizes from Cloud Research panels are larger relative to direct Mechanical Turk, meaning smaller sample sizes are required.

8.2 Method

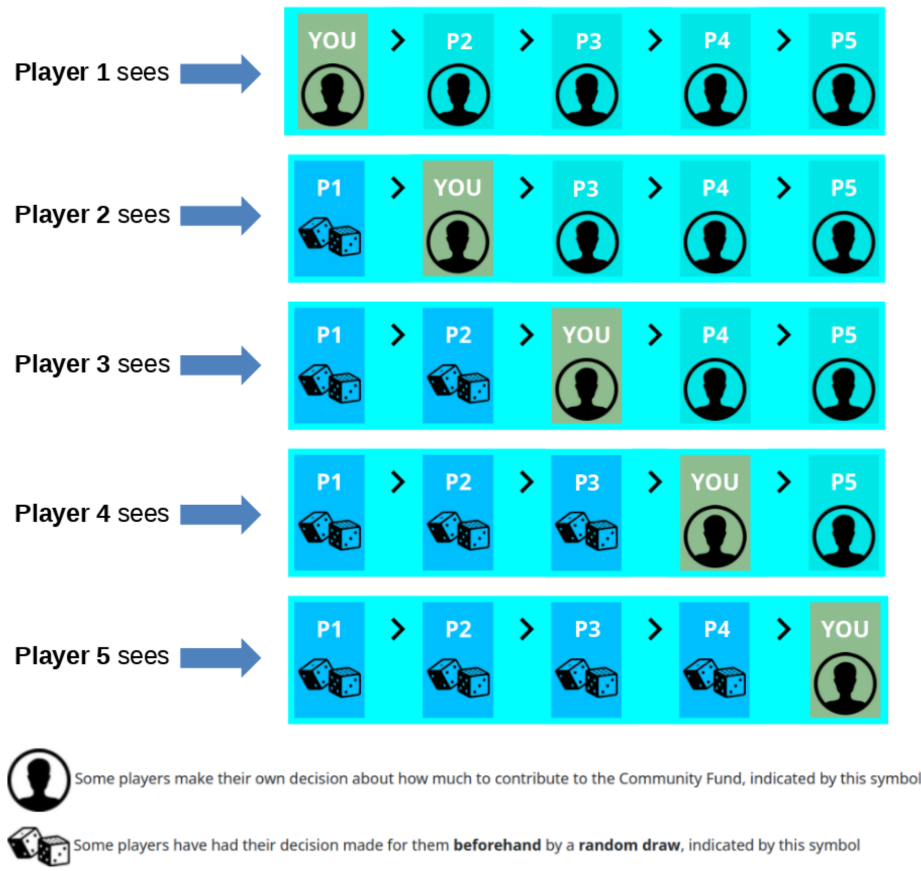
Study 2b is a one-shot sequential PGG identical to Study 2a, with two exceptions: (1) all players receive the instruction to maximize earnings, and (2) players are randomized between the Random Before and Random After conditions, fully crossed with orders 1-5 and the two simultaneous-play treatments. As in Study 2a, one simultaneous condition did not have any wait time, equivalent to moving first in the sequential game, while the other had a delay equivalent to the wait time 5th-movers experience in the sequential game. In the Random Before condition, players are told that everyone *before* them in the sequence has their decision about how much to contribute to the public good made by a random process and everyone moving *after* them makes their own decision as usual. In the Random After condition players are told that everyone *after* them has their decision made by a random process, and those moving *before* make their own decision as usual. Players are presented with a page that explains the setup, and are presented with symbols that make it clear which players' moves were randomly decided. They see the appropriate graphical representation from Figure 4 on all pages from the point at which the concept of random moves is introduced until the end of 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. Participants were debriefed at the end of the experiment.

8.3 Results

Study 2b shows a clear effect of direction in time: we observe the positional order effect among players who are in the Random Before condition, but not among those in the Random After condition. The mean contribution for Random Before participants was 35% of $1.00 endowment (SD = .43), and for Random After 34% (SD = .42). Random Before and Random After participants left with about the same profit from the SPGG on average, $1.40 (SD =

Figure 4: Study 2b. Stimuli for the Random Before Condition.

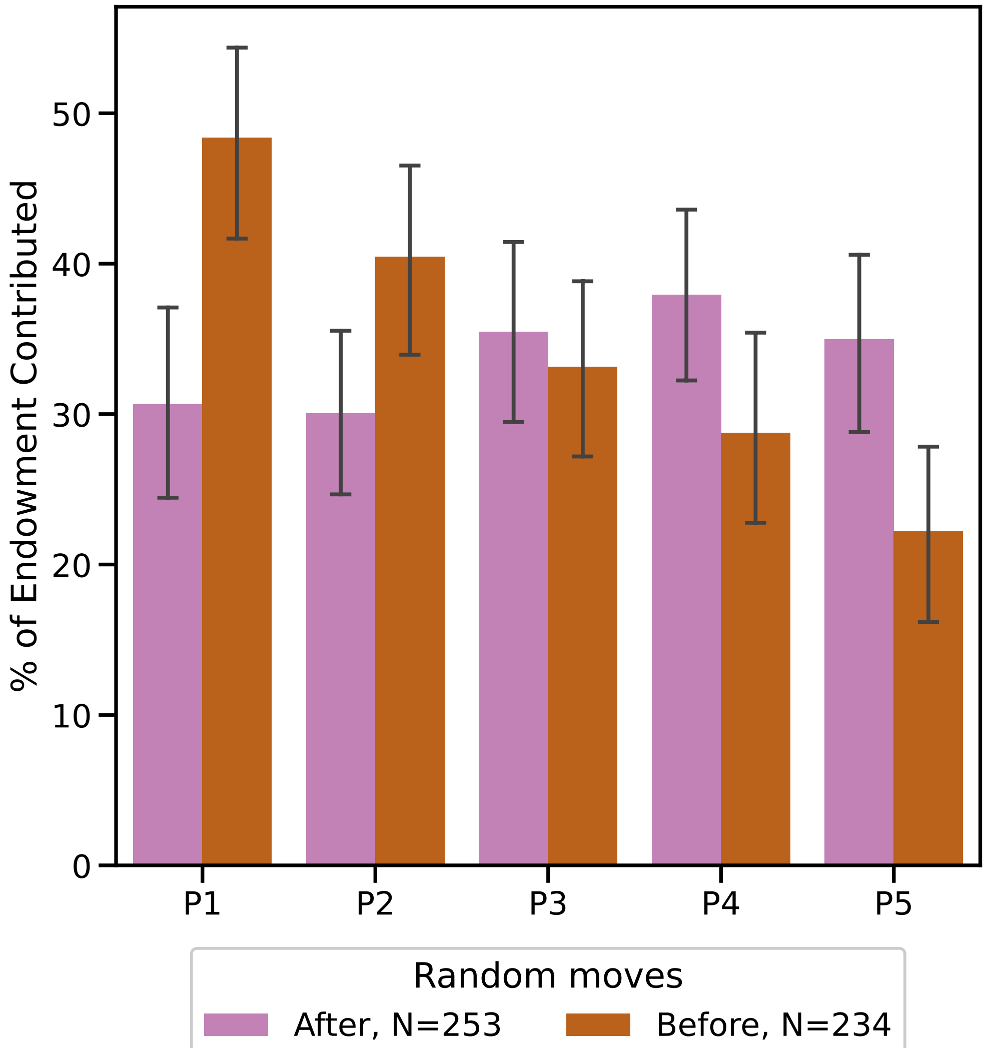


Notes: Stimuli for the Random Before condition. Players see a graphical representation of their position relative to other players that clearly conveys which players are having their moves made by a random process. This is in addition to a previous screen that explains how some players are having their moves made for them by random processes. The graphics for the Random After condition have the dice and human figures reversed.

.41) for Random Before vs. \$1.44 (SD = .39) for Random After.

Players who are told that everyone moving *before* them has their move determined randomly and everyone moving *after* them will decide on what move to make show a positional order effect. The preregistered linear regression contribution ∼ order * random_before + wealth, differing from previous analyses in that it controls for a subjective measure of wealth, finds the effect, $N = 485$, $\beta = -8.005$, 95% CI = [-13.184, -2.769], $p = .004$; $\beta_{std} = -.332$, 95% CI_{std} = [-.547, -.113] for the interaction (see Figure 5). Wealth was added to the re-

Figure 5: Study 2b. The order effect appears when random moves are before, not after, the focal player.



Notes: Study 2b shows a decline in contribution to the public good among players who are told that all players moving *after* them are making their own moves, and all players moving *before* them are having their moves made randomly. No effect is observed among players who are told that everyone moving after them has a move selected at random. Participants who passed comprehension checks. SEMs.

gression given the expectation, common in economics, that players' sensitivity to payoffs is modulated by the marginal change in their wealth. We see a marginally significant effect for wealth, $N = 485$, $\beta = -3.057$, 95% CI = [-6.225, .133], $p = .063$, indicating that as self-reported feelings of wealth increase, contributions to the public good decrease. The regression without wealth also finds the effect, $N = 487$, $\beta = -8.160$, 95% CI = [-13.277, -2.838], $p = .003$. When we restrict the main analysis to only those players who passed a second set of comprehension checks at the end of the experiment (80.1% of players who passed the initial checks), we observe a larger effect size when comparing standardized coefficients ($N = 403$, $\beta = -9.369$, 95% CI = [-14.802, -3.877], $p = .001$; $\beta_{std} = -.401$, 95% CI_{std} = [-.631, -.167] for the preregistered analysis). This gives us reason to believe that the effect is concentrated among participants who understand the rules of the game best. On the formalization included in Appendix B, self-interested participants who are acting as if others will mirror their move should either contribute 0% of their endowment or 100% depending on where they are in the sequence. 75.5% of participants give either 0 or 100% of their endowment, and among these participants effect size increases relative to the overall analysis ($N = 364$, $\beta = -9.391$, 95% CI = [-15.937, -2.931], $p = .004$; $\beta_{std} = -.354$, 95% CI_{std} = [-.601, -.111] for the preregistered analysis). See Appendix C for a comparison of these subsets. We do not observe a difference between the no delay (timing equivalent to P1, mean contribution = $.38) and long delay (80 seconds, timing equivalent to P5, mean contribution = $.47) simultaneous-move control conditions using a T test, t(128) = -1.08, $p = .282$. This shows the effects are not due to mere time in the experiment; indeed, players in the long wait condition contributed slightly more than those in the short wait condition.

In addition, a player's own move is strongly predictive of her expectations of others' moves–but only in the direction people making their own moves are to be found. For the Random Before condition, the OLS model prediction ∼ contribution*future/past yields $N = 932$, $\beta = .366$, 95% CI =[.254, .480], $p < .001$, and for Random After $N = 1004$, $\beta = -.451$, 95% CI = [-.553, -.348], $p < .001$ with cluster robust errors. The coefficient for the interaction is positive in the Random Before condition, and negative in the Random After condition, indicating players are willing to bet their own move is predictive only of moves other people choose themselves.

8.4 Discussion

Players who are trying to maximize their own payoffs adjust their contributions to a public good downwards as their place in a sequence of players approaches the end, but only if there are people making their own decisions moving *after* them. We do not see any effect in players who are told all others moving before them have had their moves made randomly for them. It appears that the positional order effect in SPGGs requires that players believe, first, that there are other people who will freely make a choice involved in the game, and, second, that those people have not yet made their decision. Further, the extent to which a player contributes to the public good is proportional to the number of others yet to freely make their own move. Players in the Random After condition all contribute approximately as much as Player 3 in the Random Before condition as well, implying that acting as if others will mirror your move raises contributions above baseline in the first half of the sequence and lowers them in the second half, with the middle player, Player 3 of 5, making approximately the same decision in both conditions. We also note that the mean contribution in the Random After condition, which shows no positional order effect, is considerably lower than what would be expected from a population that has not been instructed to maximize their own payoffs. Mean contribution in the populations sampled in Studies 1a - 1c was 54% of the endowment across all participants, prosocial and individualistic, whereas mean contribution among Random After participants here is 37%. Player 5 in the Random After condition has no random moves to contemplate at all, since she moves last, and therefore plays a standard 5-person SPGG—and contributes considerably less than non-instructed populations, confirming the efficacy of the manipulation.

General Discussion

Across five studies, we document a robust positional order effect in sequential public goods games without observation: among self-interested players, cooperation is highest for those moving early in a sequence and declines as their position nears the end. The disappearance of this effect when subsequent players' moves are randomized suggests the phenomenon is tied to beliefs about the future actions of other agents. The overall pattern of results is consistent with a player's own choice being treated as diagnostic of the choices of other, like-minded individuals who have yet to decide. Critically, this is not *conditional* cooperation (Thöni and Volk, 2018) since there is nothing for a player to condition on in a one-shot sequential game with no observation, nor can reputation come into play in an anonymous online context. Participants are also willing to bet that people moving after them will make a move that is more similar to theirs relative to those moving before them, which is to be expected in the case that players are acting as if their moves will be mirrored by others.

We speculate that a simple model may capture something of the process generating this behavior specifically in self-interested agents: these agents understand the rules of the game and are trying to maximize their payouts—they just act as if everyone who has not yet moved will make the same move they do, and they choose their own move to maximize their own profits conditional on everyone yet to move doing as they have done. This implies a sharp step between 100% contribution and 0% contribution, which is observed in the data. 75.5% of participants contribute either 0 or 100% of their endowment, and the positional order effect is stronger in this subset. The effect is also stronger in the 80.1% of participants who pass both pre- and post- comprehension checks, implying the effect gets stronger as players better-understand the game (see Appendix C.3 for analyses of these subsets). A formalization of this model is included in Appendix B.

9.1 Theoretical Contributions

This work, firstly, demonstrates a robust positional order effect. Players in an SPGG with no observation tend to give more to a public good the closer they are to the beginning of a sequence. A central contribution of this work is the finding that the positional order effect is not universal, though; it is driven specifically by self-interested individuals. This insight is critical for both theory and practice. While marketers can often rely on appeals to altruism to motivate prosocial consumers, a significant challenge lies in encouraging contributions from those who are more self-interested. Our results identify a novel, non-financial lever that influences this very group. This demonstrates that the architecture of a choice situation can be as important as the direct incentives offered, particularly for this difficult-to-motivate market segment.

One might reasonably ask why this positional order effect is limited to the self-interested. After all, if prosocial consumers care about the collective good, they should also be motivated to act as if others will do as they have done, thereby maximizing the group's total payoff. Our results suggest a more nuanced view of prosocial motivation in this context. The absence of a positional order effect among prosocials implies that their cooperative drive is not based on maximizing the final collective outcome, but rather stems from a more direct, "warm glow" utility associated with the personal act of contributing (Andreoni, 1995). Their decision is guided by a stable preference to do their part, rendering them insensitive to the strategic calculations that self-interested players engage in. This finding contributes to the marketing literature by helping to disentangle different forms of prosocial motivation, showing that interventions based on influencing expectations about others' actions may be uniquely effective for consumers driven by self-interest rather than by a pure sense of duty.

Furthermore, our findings add a temporal dimension to theories of diagnostic reasoning in choice, such as quasi-magical thinking (Shafir and Tversky, 1992) and self-signaling in consumer choice (Dhar and Wertenbroch, 2012). While these theories propose that people act as if their choices are diagnostic of favorable outcomes, they do not distinguish between events that have already occurred ("closed fates") and those yet to occur ("open fates"). Our research shows this distinction is paramount. The positional order effect is sensitive only to the number of future agents, suggesting that this type of reasoning is strongly directed towards the future, a finding that should be incorporated into future models of interdependent choice.

9.2 Limitations and Future Research

The present research has several limitations that offer avenues for future work. While we provide strong evidence for the positional order effect and its moderation by self-interest, the precise psychological mechanism remains a subject for further investigation. Our data are consistent with participants acting as if those moving in the future will do as they have done, but future research could use methods such as think-aloud protocols or further revealed preferences decision-making studies to more definitively establish the mechanism. It may be the case that self-signaling, social projection, or moral universalization could provide a theoretical basis for our empirical results when considered as directed towards the future specifically, and future work should aim to tease these possibilities apart.

As is standard with the PGG and similar games, we do not observe perfectly rational play. Prosocial participants are not at ceiling for contributions to the public good, nor are individualistic participants who are at the beginning of the sequence. This is due in part to noise, which will necessarily have an asymmetric effect (since it is impossible to contribute more than 100% of an endowment), thus lowering the ceiling for contributions. Some participants who do not fully understand the game will pass comprehension checks as well (Andreoni, 1995; Houser and Kurzban, 2002), leading to some noise from misunderstanding in responses. Indeed, Kümmerli et al. (2010) report PGG experiments that explicitly incentivize full cooperation—and which never reach it. Some participants will also evince different levels of prosociality in the SPGG vs. in the SVO measure, leading to inconsistent behavior. Just so, Ackermann and Murphy (2019) report being able to explain more than 50% of variance in contribution decisions using repeated measures of social preferences and beliefs about others—but not much more than 50%. The sources of noise typical for PGGs and similar games suggest that we would see attenuated effects relative to pure strategies, but it is no doubt the case that the paradigms could be further refined to reduce this source of noise.

In addition, our studies were conducted in an online environment with relatively low stakes. Future research should test the generalizability of the positional order effect in field settings and with higher financial or social stakes. It would be valuable to explore the boundary conditions of the effect: for instance, does it persist with consumers experiencing different contribution multipliers, in larger groups, or in different cultural contexts? The extent to which acting as if one's moves will be mirrored generalizes across different contexts, including in the field, remains to be investigated. Finally, this research is limited to one-shot sequential games, and the space of tasks where behavior is consistent with the observations reported here could be further explored.

9.3 Managerial Implications

This research offers actionable insights for marketers, particularly for designing and implementing cause-marketing campaigns, crowdfunding drives, and other initiatives that rely on collective consumer action. The findings are particularly relevant in marketing contexts that rely on prosocial consumer behavior. For example, a consumer facing a checkout donation display might say to herself: many other shoppers will be in my shoes today, so if I donate my dollar, they will donate theirs too; if I choose the sustainable product, so will others, and there will be meaningful environmental benefits. When our contributions are amplified by a corporate match, this feeling can be even stronger. This suggests that my small, costly action can trigger a large, collectively beneficial outcome—making the contribution feel rational even for a self-interested person.

Our results suggest three tactics for practitioners:

1. **Emphasize Early Participation:** In campaigns that unfold over time (e.g., a week-long charity drive, a crowdfunding campaign), frame communications to highlight a consumer's early position in the sequence. Messages like "You will be one of the first to contribute" or "Help us kick off our campaign" may be particularly effective at inducing cooperation.

2. **Highlight Future Participants:** Remind consumers of the many other people who will make the same decision after them. This makes the "open fates" salient and may encourage the diagnostic reasoning that drives the effect. For instance, a sign could read: "500 more shoppers will see this message today. Join them in supporting..."

3. **Segment by Motivation:** While a general intervention is useful, our finding on self-interest suggests that these temporal framing effects are most potent for consumers who are less responsive to simple altruistic appeals. This provides a subtle way to encourage participation from a segment that is often the most challenging to engage in prosocial behavior. Consumers, for example, could be segmented into prosocial and self-interested based on past uptake of matched donation appeals at the point of sale.

Ultimately, this research demonstrates that the temporal structure of a decision can be a powerful and subtle tool for shaping consumer behavior, opening new avenues for both marketing theory and practice.

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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:

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

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. Because 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.

Model

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

B.1 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.

$$p_i = 1 - a_i + \frac{m}{n} \sum_{k=1}^{n} a_k \tag{2}$$

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:

$$u_i(a_1, \ldots, a_n) = (1 - s_i) p_i + s_i m a_i \tag{3}$$

where p_i is determined by the game formula, Equation 2. 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, $u_i = ma_i$. 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.

B.2 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:

$$a_i = 1 \Longleftrightarrow \mathbb{E}\left[u_i \mid a_i = 1, s_i\right] > \mathbb{E}\left[u_i \mid a_i = 0, s_i\right] \tag{4}$$

A player knows the value of their prosocial parameter and hence also knows the utility function in Equation 2. 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 c:

$$\mathbb{E}\left[a_k \mid s_i\right] = b + cs_i \tag{5}$$

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:

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

where $\mathbb{E}\left[a_k \mid s_i\right] = b + cs_i$ from Equation 5 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 4 can be expressed as:

$$
\begin{aligned}
\mathbb{E}\left[u_i|a_i=1,s_i\right]-\mathbb{E}\left[u_i|a_i=0,s_i\right] &= (1-s_i)\mathbb{E}\left[p_i|a_i=1,s_i\right]+s_i m-(1-s_i)\mathbb{E}\left[p_i|a_i=0,s_i\right] \\
&= (1-s_i)\left(\mathbb{E}\left[p_i|a_i=1,s_i\right]-\mathbb{E}\left[p_i|a_i=0,s_i\right]\right)+s_i m \\
&= (1-s_i)\left(-1+\frac{m}{n}\mathbb{E}\left[\sum_{k=1}^{n}a_k|a_i=1,s_i\right]-\frac{m}{n}\mathbb{E}\left[\sum_{k=1}^{n}a_k|a_i=0,s_i\right]\right)+s_i m
\end{aligned}
\tag{6}
$$

where the first line follows from Equation 3 and the third line from Equation 4.

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,

$$
\mathbb{E}\left[u_i|a_i=1,s_i\right]-\mathbb{E}\left[u_i|a_i=0,s_i\right]=(1-s_i)\left(-1+\frac{m}{n}\left(1+d(n-i)\right)\right)+s_i m. \tag{7}
$$

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:

$$
\mathbb{E}\left[u_i \mid a_i=1,s_i\right]-\mathbb{E}\left[u_i \mid a_i=0,s_i\right]=0 \Longleftrightarrow d^*(i)=\frac{-m-smn+n}{m(n-i)} \tag{8}
$$

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 Equation 8) 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.

Study 2b subsets

C.1 Overall data

Study 2b results for the total data set are as follows, for convenience:

Positional order effects

The preregistered linear regression contribution ∼ order * random_before + wealth finds the effect,
$N = 485$, $\beta = -8.005$, 95% CI = [-13.184, -2.769], $p = .004$; $\beta_{std} = -.332$, 95% CI$_{std}$ = [-.547, -.113] for the interaction
We also find a significant equation not controlling for wealth,
$N = 487$, $\beta = -8.160$, 95% CI = [-13.277, -2.838], $p = .003$; $\beta_{std} = -.338$, 95% CI$_{std}$ = [-.552, -.118]

Predictions of others' moves by own move, forwards vs. backwards

The preregistered linear regression predicted_value ∼ contribution * binary_position finds the effect with cluster robust errors,
Participants in the Random Before condition
$N = 932$, $\beta = .366$, 95% CI =[.254, .480], $p < .001$
Participants in the Random After condition
$N = 1004$, $\beta = -.451$, 95% CI = [-.553, -.348], $p < .001$

C.2 0s and 1s: When considering only participants contributing all or nothing, effect sizes increase

The formalization in Appendix B 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. In Study 2b participants were instructed to maximize their own payoffs, and m is constant. Results from players who either give 0% or 100% of their endowment in Study 2b 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*.

Positional order effects

Among the 75.5% of participants give either 0 or 100% of their endowment, the preregistered linear regression contribution ∼ order * random_before + wealth finds the effect,
$N = 364$, $\beta = -9.391$, 95% CI = [-15.937, -2.931], $p = .004$; $\beta_{std} = -.354$, 95% CI$_{std}$ = [-.601, -.111] for the interaction

We also find a significant equation not controlling for wealth,
N = 365, $\beta = -9.588$, 95% CI = [-16.121, -3.087], p = .003; $\beta_{std} = -.361$, 95% CI$_{std}$ = [-.608, -.117]

Predictions of others' moves by own move, forwards vs. backwards

The preregistered linear regression predicted_value ~ contribution * binary_position finds the effect with cluster robust errors,
Participants in the Random Before condition
$N = 704$, $\beta = .351$, 95% CI =[.226, .474], $p < .001$
Participants in the Random After condition
$N = 748$, $\beta = -.491$, 95% CI = [-.608, -.376], $p < .001$

C.3 Strict comprehension checks: When considering only participants who pass both pre- and post- comprehension checks, effect sizes increase

Study 2b 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?[10]
   (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

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.

[10] This question is only applicable to participants who contributed something to the public good.

Positional order effects

Among participants who passed a second set of comprehension checks at the end of the experiment (80.1% of those who passed the initial checks), the preregistered linear regression contribution $\sim$ order * random_before + wealth finds the effect,
$N = 403$, $\beta = -9.369$, 95% CI = [-14.802, -3.877], $p = .001$; $\beta_{std} = -.401$, 95% CI$_{std}$ = [-.631, -.167] for the interaction
We also find a significant equation not controlling for wealth,
$N = 403$, $\beta = -9.504$, 95% CI = [-14.944, -3.988], $p = .001$; $\beta_{std} = -.407$, 95% CI$_{std}$ = [-.638, -.170]

Predictions of others' moves by own move, forwards vs. backwards

The preregistered linear regression predicted_value $\sim$ contribution * binary_position finds the effect with cluster robust errors,
Participants in the Random Before condition
$N = 768$, $\beta = .442$, 95% CI =[.321, .562], $p < .001$
Participants in the Random After condition
$N = 836$, $\beta = -.467$, 95% CI = [-.573, -.359], $p < .001$