Article:

Masiliūnas, A. and Nax, H.H. (2020) Framing and repeated competition. Games and Economic Behavior, 124. pp. 604-619. ISSN 0899-8256

https://doi.org/10.1016/j.geb.2020.10.002

Games and Economic Behavior 124 (2020) 604–619

Contents lists available at ScienceDirect

Games and Economic Behavior

www.elsevier.com/locate/geb

Framing and repeated competition[☆]

Aidas Masiliūnas[a,*], Heinrich H. Nax[b,c]

[a] Global Asia Institute, National University of Singapore, 10 Lower Kent Ridge Road, Singapore 119076, Singapore
[b] Institute of Sociology, University of Zürich, Andreasstrasse 15, 8050 Zürich, Switzerland
[c] Behavioral Game Theory, ETH Zürich, Clausiusstrasse 37, 8092 Zürich, Switzerland

ARTICLE INFO

Article history:
Received 22 June 2019
Available online 23 October 2020

JEL classification:
C72
C91
D43
D74
D83

Keywords:
Framing
Contests
Strategic complements
Strategic substitutes
Beliefs

ABSTRACT

We use a unified framework to model rent-seeking (Tullock) contests and games of strategic complements or substitutes. In each game, we compare an 'abstract' frame with an 'economic' frame. We find more competitive behavior under economic than under abstract framing in the contest and in the game of strategic complements, but not in the game of strategic substitutes. Variation in the strategic nature of the game interacts differently with preferences than with beliefs, allowing us to identify that framing operates primarily through beliefs, and diminishes as beliefs are updated. We model beliefs and preferences using a static and a dynamic framework and show that average choices and adaptation behavior can be explained if both preferences and beliefs are more competitive under economic framing. Our results suggest that some of the commonly observed competitive behavior in contest and oligopoly experiments could be explained by non-abstract framing being used in these studies.

1. Introduction

How a decision is framed has been shown to crucially impact behavior in laboratory experiments.[1] One view is that games should be framed neutrally to 'let the explicit reward structure be the singular source of valuation' (Smith 1976, p. 278). An alternative is to embed the game in meaningful context, enhancing the understanding and external validity but potentially influencing behavior. In economic experiments, it is common to use either abstract or mildly cooperative

☆ We would like to thank participants at seminars and conferences at University of Barcelona (IMEBESS 2017), UC Irvine (Theory, History and Development Series 2017), University of Lund (Department of Economics Seminar Series 2017), Stony Brook University (Workshop on Adaptive Learning 2017, 28th International Conference on Game Theory), University of Rennes (ASFEE 2017), Vilnius University (6th Annual Lithuanian Conference on Economic Research), Tilburg University (TIBER 2017), Vienna University (VfS Jahrestagung) and Kassel University (GfeW Jahrestagung). Amongst them, we would especially like to thank John Duffy, Sergiu Hart, Glenn Harrison, Ozan Isler, Erik Wengström and Vaiva Petrikaitė. We also thank our editor, Gary Charness, an associate editor and three reviewers for their detailed comments and suggestions. Nax acknowledges support from the European Commission through the European Research Council Advanced Investigator Grant 'Momentum' 324247, and from the Swiss National Science Foundation for the 'Eccellenza' Grant 181111.

* Corresponding author.
E-mail addresses: aidas.masiliunas@gmail.com (A. Masiliūnas), heinrich.nax@uzh.ch (H.H. Nax).

1 See, for example, Andreoni (1995) and Liberman et al. (2004) for two seminal studies in economics and psychology. For a survey, see Alekseev et al. (2017) and Gerlach and Jaeger (2016).

https://doi.org/10.1016/j.geb.2020.10.002
0899-8256/

context, in which one of the actions is described as generating a positive externality.[2] An exception to this convention are contest and oligopoly experiments that are almost exclusively run using explicit corporate or competitive framing. The resulting behavioral regularities in these experiments are also somewhat unusual: while choices in most other experimental games (e.g. voluntary contributions games, trust games, dictator games) are typically 'pro-social', that is, between Nash equilibria and social optima,[3] they typically are 'anti-social' in contest and oligopoly games.[4] Moreover, while choices in the other games tend to move toward the Nash equilibrium, oligopolies often drift toward the 'Walrasian' perfectly competitive equilibrium, in which individual payoffs are even lower than Nash.[5] These unusual behavioral patterns in the games of competition raise the question to what extent they are caused by framing.

We address the question by comparing the two framing versions in different types of games. The *economic framing* treatments replicate the framing that is typically used in the corresponding literature: either corporate language from oligopoly experiments (participants are asked to act as a 'manager' who chooses 'how many products to sell') or competitive language from contest experiments (buying 'tokens' to increase the 'share of the prize'). The *abstract framing* treatments do not provide any interpretation for the game and replace potentially loaded terms with generic game-theoretic language ('action', 'other participant').

In each framing condition, we study three games: contest, strategic complements and strategic substitutes, modeled in a unified framework of 'games of competition' and therefore distinguished only by the degree of strategic complementarity and substitutability. The games were designed to have the same payoffs and locations of Nash equilibrium, collusive outcome and competitive equilibrium. In all games, cooperative choices lie below the Nash equilibrium, and competitive choices above it, therefore behavior can be evaluated along a single 'cooperative–competitive' dimension.[6] We compare a hypothesis that economic framing leads participants to expect more competitive behavior from their opponents to a hypothesis that framing triggers competitive preferences. Variation in the degree of strategic complementarity and substitutability across games allows us to identify the relative importance of these two channels. If framing operates primarily through preferences, economic framing should induce more competitive behavior in all games. If framing operates through beliefs, economic framing should induce more competitive behavior in strategic complements, but less competitive behavior in strategic substitutes, while contest should fall between the other two games.

The second feature of our design is repetition, implemented using a repeated single-shot design (Andreoni and Croson, 2008) whereby players are randomly re-matched within their matching group each round. Repetition allows us to test whether the effect of framing lasts for the entire duration of the game, as it would if it affected preferences,[7] or if the effect disappears over time as beliefs are updated in light of feedback. Previous experiments that studied whether framing operates through preferences or beliefs in prisoner's dilemma (Ellingsen et al., 2012), public goods (Dufwenberg et al., 2011) and ultimatum games (Dreber et al., 2013) used one-shot games, thus it is unclear if the observed framing effects would disappear with repetition (similar concerns in social dilemma games were raised in Bernold et al., 2015).

We find that in contest and strategic complements, economic framing makes behavior more competitive, measured either by the average choice or by the frequency of competitive choices.[8] The framing effect in substitutes is not significantly different from zero, but in contest and in complements the difference is significant and the effect size is larger than typically found in other games (12% in prisoner's dilemmas, 6% in public goods games, 11% in trust games, and 10% in dictator games; see Gerlach and Jaeger, 2016). We find that differences between treatments are best explained by a hybrid model in which framing operates mostly through beliefs, and, to a lesser extent, also through preferences. The estimated parameter values suggest that abstract framing triggers slightly pro-social preferences and increased beliefs that others will act pro-socially, while economic framing shifts both preferences (slightly) and beliefs (substantially) in the competitive direction.

These results complement the existing literature on framing effects. It is generally found that abstract framing increases the degree of cooperation compared to economic framing; for instance, there is more cooperation in social dilemmas (Ellingsen et al., 2012; Engel and Rand, 2014; Batson and Moran, 1999; Pillutla and Chen, 1999), and higher transfers in ultimatum games (Hoffman et al., 1994). Our results extend these findings by showing that abstract framing moderates competitive behavior in the games of competition, but the framing effect decreases with repetition. We also find evidence

[2] For example, voluntary contribution experiments participants typically have an option to contribute to a 'group exchange' that benefits everyone else (Cookson, 2000), in trust games participants have an option to place bills in an envelope, which are multiplied and delivered to another participant (Berg et al., 1995).

[3] See, for example, Ledyard (1995); Charness and Rabin (2002); Chaudhuri (2011); Murphy et al. (2011).

[4] The idea that this difference could be caused by framing dates back at least to Andreoni (1995).

[5] See, for example, Holt (1995); Bigoni and Fort (2013); Offerman et al. (2002). Similar patterns are observed in contest games (Sheremeta, 2013). Note that oligopoly experiments have found high levels of collusion in small fixed groups (Potters and Suetens, 2013) and after many repetitions (Friedman et al., 2015). Because we are interested not in collusive behavior but in the origins of competitive behavior, as found in experiments with large groups or random matching, we designed our games so as to limit factors known to promote collusion.

[6] We define a choice as 'cooperative' if it would be chosen in equilibrium by a pair of pro-social players, i.e. those whose utility is increasing in the payoff of the other participant. A choice is defined as 'competitive' if it would be chosen by a pair of anti-social players, i.e. those whose utility is decreasing in the payoff of the other participant. For more details, see Appendix B.3.

[7] Unless non-standard preferences are interactive and conditional (see Nax et al. 2015; Ackermann and Murphy 2019; Ackermann et al. 2016).

[8] Economic framing increases the average choice across all rounds by 16% in contest, by 60% in strategic complements, and by 6% in strategic substitutes. All games are parametrized such that higher choices are more competitive.

that framing operates primarily through beliefs, in line with the results in prisoner's dilemma (Ellingsen et al., 2012), public goods (Dufwenberg et al., 2011) and ultimatum games (Dreber et al., 2013).

The games of strategic complements, substitutes and contests have been widely studied in the experimental literature,[9] but almost exclusively with economic framing. Some experiments studied framing in contests (e.g. Chowdhury et al., 2020), but we are not aware of any studies that would have used abstract framing. Abstract framing has been used in the games of substitutes or complements, but it was not compared to other types of frames (Potters and Suetens, 2009, Abbink and Brandts, 2009, Ozkes and Hanaki, 2019).[10] The only exception is a study by Huck et al. (2004) that compares economic framing to abstract framing in Cournot oligopoly. Huck et al. (2004) find more competitive choices under abstract framing compared with economic framing for duopolies, but no significant framing effect in larger markets. Our duopoly results do not reproduce a significant effect in strategic substitutes, but we confirm and extend their more general point that 'frames interact with the objective structure of a game'.

2. Experimental design

One of the goals of this study is to identify whether framing operates primarily through preferences or through beliefs. We separate these two effects using three games of competition which exhibit different levels of strategic complementarity/substitutability. Variability in the strategic nature of the game results in different predicted framing effects in each game, depending on the mechanism through which framing operates.

Before discussing the details of the experimental design, we will sketch the key identification strategy. First, we assume that economic framing either triggers more competitive preferences than abstract framing or anchors (initial) beliefs regarding opponents' actions/types at more competitive levels. Given this assumption, and given the three games of competition we formulate, we have the following differentiated predicted effects:

Strategic complements: more anti-social preferences and anti-social beliefs both increase the competitiveness of observed behavior.

Strategic substitutes: more anti-social preferences increase competitiveness, but more anti-social beliefs decrease it, resulting in a smaller framing effect than in strategic complements.

Contests: with elements of both substitutes and complements, the predicted framing effect lies between the other two games.

To ensure that the effects pertaining to the variation in the strategic nature of the underlying game are not confounded by other features of the games, we designed all three games to be as similar as possible in all other key aspects.[11]

2.1. Payoff functions

2.1.1. Proportional contest

A prize of value V is divided between two players, proportionally to the size of their investments, which we denote x_1 and x_2. All investments are costly and have to be paid using an endowment E. The payoff of player $i \in \{1, 2\}$ is therefore:

$$\pi_i(x_i, x_j) = \frac{x_i}{x_i + x_j} V + E - x_i \tag{1}$$

Standard rent-seeking (Tullock) contests are usually formulated with payoff risk, that is, the prize is not divided proportionally to investments, but rather allocated to either party according to odds defined by the investments. To make the contest game more comparable with the games of strategic complements and substitutes, we replaced the probabilistic prize allocation rule by the proportional payment rule. With this change, contest is equivalent to a Cournot oligopoly with a unit elastic demand curve (as used, for example, in Friedman et al., 2015).[12] The proportional contest is known to exhibit similar behavioral patterns as the probabilistic contest (see Masiliunas et al., 2014, Fallucchi et al., 2013, Chowdhury et al., 2014).

2.1.2. Games of strategic complements and substitutes

The other two games used in this study are the games of strategic substitutes and strategic complements, which include Cournot and Bertrand oligopoly games with imperfect substitutes as special cases. We represent these games using a payoff

[9] For literature overview, see Dechenaux et al. (2015) and Sheremeta (2013) for contests, and Potters and Suetens (2013) and Engel (2007) for strategic complements and substitutes.

[10] And, indeed, it has been debated whether higher collusion rates observed in some of these studies were caused by abstract framing (Anderson et al., 2015).

[11] This is important because varying game parameters has been shown to alter behavior given the same instantiations of preferences and beliefs (Murphy and Ackermann, 2015).

[12] The payoff function in an n person Cournot game is $\pi_i(x_i, x_{-i}) = a + (P(X) - c)x_i$, and a unit elastic demand function is defined by $P(X) = \frac{b}{\sum_{k=1}^{n} x_k}$. The two are equivalent if $b = V$, $a = E$ and $c = 1$.

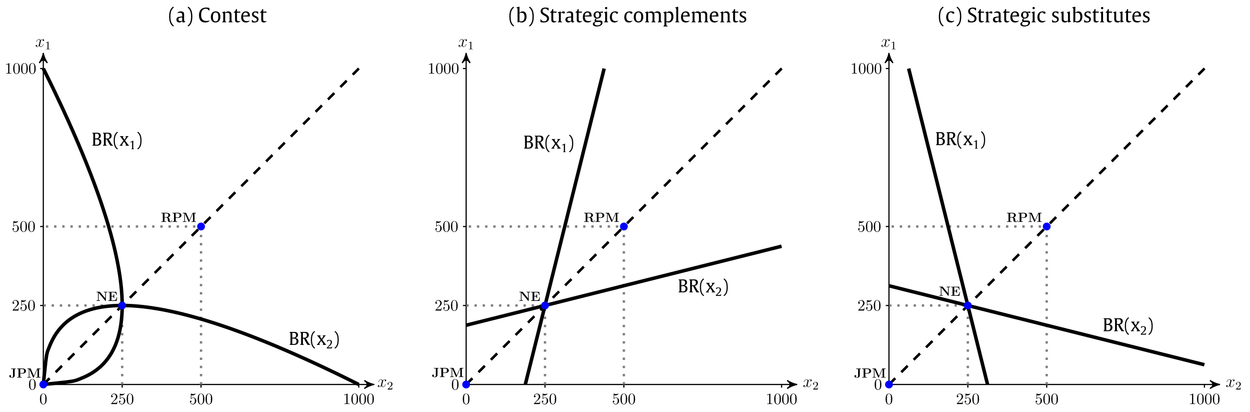

Fig. 1. Best-response curves under pure material self-interest.

Table 1
Comparison of the contest, strategic complements and substitutes games.

	Contest	Complements	Substitutes
Nash action	250	250	250
Nash payoff	1250	1250	1250
Joint payoff max. action	0	0	0
Joint payoff max. payoff	1500	1500	1500
Relative payoff max. action	500	500	500
Relative payoff max. payoff	1000	1000	1000
Percent of dominated strategies	74.93%	74.83%	74.83%
Worst payoff possible	500	407	448
Best payoff possible	1950	1547	1578
Average payoff s.d.	159	195	170
Loss from random choice	184	176	156
Average 'best-minus-worst response' loss	488	638	533

function introduced by Potters and Suetens (2009). This formulation provides the necessary degrees of freedom to make the games comparable to each other and to the rent-seeking contest. The payoff function has six free parameters:

$$\pi_i(x_i, x_j) = a + bx_i + cx_j - dx_i^2 + ex_j^2 + f x_i x_j \tag{2}$$

Crucially, the game is of strategic complements if $\frac{\partial^2 \pi_i}{\partial x_i \partial x_j} = f > 0$, and of strategic substitutes if $\frac{\partial^2 \pi_i}{\partial x_i \partial x_j} = f < 0$.

2.1.3. Comparability and parameter choices

We set the contest parameters to $V = E = 1000$ ECU.[13] To make all three games comparable from a viewpoint of self-interested payoff maximization, the parameters in the games of strategic complements and substitutes were chosen to ensure similar percentages of dominated strategies in all three games and identical locations (and payoffs) of the outcomes that correspond to (*i*) Nash equilibrium, (*ii*) joint profit maximization and (*iii*) relative profit maximization. These conditions are satisfied by the following parameter values: $a = 1500$, $b = 0.5$, $c = -1.5$, $d = \frac{2}{1500}$, $e = \frac{1}{1500}$, $f = \frac{1}{1500}$ in the game of strategic complements and $a = 1500$, $b = 0.5$, $c = -1.5$, $d = \frac{2}{2500}$, $e = \frac{3}{2500}$, $f = -\frac{1}{2500}$ in the game of strategic substitutes.[14]

Fig. 1 compares the best-response curves, which are hump-shaped in the contest game, upward sloping in the game of strategic complements, and downward sloping in the game of strategic substitutes. Table 1 shows that all three games have a similar range and standard deviation of payoffs and an almost identical percentage of dominated strategies. In all games players face similar optimization incentives: a player would lose on average 160-180 ECU by choosing at random and 490-640 ECU by making the worst possible choice, compared to the payoff of the best-response, if the opponent is assumed to choose uniformly.

Our method to identify the relative importance of preferences and beliefs relies on the comparison of behavior in games that differ primarily in terms of their strategic nature.[15] If games differed in other important aspects, variation in treatment

[13] In the experiment, all earnings were expressed in Experimental Currency Units (ECU), converted into cash at the rate 100 ECU = 1 CHF.

[14] A detailed derivation of these parameter values is in Appendix A.

[15] The standard approach of eliciting beliefs or measuring social preferences in a separate task is problematic for several reasons. Preferences could be context-dependent, thus participants who act pro-socially in the post-experimental task (e.g. Murphy et al., 2011) might act anti-socially in the games of competition. For example, Herrmann and Orzen (2008) and Savikhin and Sheremeta (2013) find that the contest environment evokes anti-social behavior

effects could not be attributed to the strategic nature. For this reason, it was important to keep the location and payoff of each outcome of interest the same in all three games. Our approach follows Potters and Suetens (2009), but instead of keeping the payoffs on the best-response curve constant across games, we control the location and payoff of the relative payoff maximization point.[16] This feature is important because we seek to test whether economic framing induces competitive preferences or beliefs, thus we also need to know how competitiveness affects behavior. If the payoffs and the location of the relative payoff maximization point differed across games, it would be difficult to attribute differences in framing effects to the strategic nature of the game. Differences in payoffs would affect the costs of relative payoff maximization, thus the game in which spiteful actions are cheaper would be predicted to have a larger framing effect. Differences in the locations of competitive actions could change their salience and the probability that they will be chosen by mistake, potentially explaining differences in competitiveness and the extent to which competitiveness could be further affected by framing.

Identical locations of the outcomes of interest also ensure that the deviations from the Nash equilibrium towards more cooperative or competitive choices decrease payoffs by a similar amount in all games, and the decrease is sufficiently low to detect slight variations in preferences or beliefs. For example, a player with Nash equilibrium beliefs who deviates from the Nash equilibrium action of 250 by 50 and plays 200 would decrease own earnings by 2 ECU (6 ECU in contests), but would increase competitor's earnings by about 50 ECU. Similarly, a deviation to 300 would decrease the competitor's earnings by about 50 ECU, costing 2 ECU in the game of substitutes, 3 ECU in complements and 5 ECU in contest. Therefore, if framing manipulation has an effect on social preferences, a treatment difference should be observed in the final outcomes.

2.2. Other design details

We designed the other elements of the experiment to minimize the chances that deviations from the Nash equilibrium prediction occur because of reasons other than preferences or beliefs, and also to keep the design similar to the prior literature.

Action Space

In all three games the strategy space was $\{0, 1, \ldots, 1000\}$. A fine strategy space reduces the chance that an outcome of interest will be observed by pure chance; instead, when an outcome is observed, it is likely to be a result of the properties of that outcome.

Instructions and timing

Payoffs were explained using a payoff table (reproduced in Appendix G) and a payoff calculator. The two tools complement each other: a table gives a good overview of the payoff space, but only a limited number of payoff combinations can be displayed. A calculator allows payoffs to be calculated for any action profile.[17] We recorded how much time each subject spent using the table and the calculator, as well as what action combinations were entered into the calculator. To ensure that experiments finished in time, we placed time restrictions on each round.[18] If no decision was made within the time limit, the action from the previous round was implemented. If no decision was made in the first round, a randomly generated action was implemented. Throughout the paper, choices generated at random are excluded from the analysis (i.e. all choices until the first active decision).[19]

Repetition and matching

The game was repeated for 20 rounds. At the end of the experiment, two randomly drawn rounds were chosen for payment. After each round, participants were informed about their own and their opponent's actions and payoffs. We used

in games played subsequently or simultaneously. Direct belief elicitation could influence actions (Hoffmann, 2016) and the quality of elicited beliefs could be low (participants might not be motivated to report accurate beliefs, or reported beliefs could be chosen to justify their behavior to the experimenter; see Schlag et al., 2015, for a detailed discussion).

[16] A change in payoffs on the best-response curve might affect the convergence speed of the best-response dynamics. We favor controlling the payoffs in the competitive outcome than payoffs on the best-response curve since we expect the effect of framing to depend more strongly on the incentives to act competitively than on the incentives to best-respond. In addition, it would not be possible to equalize the best-response payoffs across all three games (since the best-response curve is different in contests), while equalizing RPM payoffs is possible.

[17] For a comparison between the two tools in Cournot oligopoly, see Requate and Waichman (2011).

[18] The limit was 3.5 minutes in each of the first 5 rounds, 2.5 minutes for rounds 5-10, 1.5 minutes for rounds 11-15 and 1 minute for rounds 16-20. We chose this structure of time limits as a pilot study showed that most subjects would not be rushed by such restrictions.

[19] By mistake, in some of the 2019 sessions the laboratory software was started while the participants were still reading the instructions. As a result, some participants did not notice that the experiment had already started and failed to make an active choice within the 3.5 minute time limit given in the first round. This issue affected a third of the participants (compared to less than 3% of participants who did not make an active choice in the first round of 2016 sessions). Only the choices in the first round were affected, as more than 99% of participants from 2019 sessions made an active choice in round 2. Even though we excluded the non-active round 1 choices from the analysis, the remaining number of active choices varies across treatments, and the choices might be of low quality due to potentially being made under severe time pressure. Consequently, we measure the framing effect at the start of the game by comparing decisions in the first 5 rounds, instead of using only the first round data.

two-player games because we expected a small group size to make it easier to understand the game, form beliefs and calculate best-responses, lowering the chances that deviations from the Nash equilibrium will occur because of computational complexity rather than preferences or beliefs. The game is easy to understand because participants can see the payoff landscape using a simple two-dimensional table.[20] Belief learning in two-player games is arguably less cognitively demanding because players do not need to form and update beliefs about several interaction partners. However, a small group size also makes it more likely that participants will collude (Huck et al., 2004) and it would be difficult to identify the framing effect on competitive behavior if the drive to collude crowded out other motives. We therefore made collusion more difficult by randomly re-matching participants each round. Prior studies generally find behavior to be closer to Nash equilibrium predictions when random matching is used.[21]

Framing

We implemented two variations of each game, one with 'abstract' the other with 'economic' framing. Instructions for all treatments are reproduced in full in Appendix F. Abstract instructions were identical in all three games: players were asked to choose an 'action, which is a number between 0 and 1000' and the other party was referred to as the 'other participant'. Behavior in abstract treatments should be driven primarily by the incentive structure, providing a baseline to evaluate behavior in treatments with non-abstract language. Instructions with economic framing followed typical language from previous laboratory experiments,[22] to evaluate whether behavioral patterns in these games are triggered by such language. Framing of the games of strategic complements and substitutes followed Bigoni and Fort (2013), who asked participants to choose the 'quantity produced' and the other party was referred to as a 'competitor'.[23] The economic contest framing was based on Fallucchi et al. (2013) and Masiliunas et al. (2014): players were informed that they will receive a share of the 'prize', depending on how many 'contest tokens' were bought by them and by the 'other participant'.

Sessions

A treatment consists of a game (one out of three), and a frame (one out of two), forming six treatments. The first wave of experiments was run in May and October 2016 and second wave in November and December 2019. In each wave, 24 participants took part in each of the six treatments, with a total of 48 participants per treatment (288 in total). All experiments were run at the Decision Science Laboratory of ETH Zurich. The average duration of the experiment was 1 hour and average earnings were 32.2 Swiss francs (at the time of the experiment the exchange rate was about 1 CHF = 1 USD). Experiments were programmed using z-Tree (Fischbacher, 2007) and participants were recruited using ORSEE (Greiner, 2015).

3. Hypotheses

Full information about the payoff function was available in all treatments, therefore standard Nash equilibrium predictions (assuming selfish preferences) are identical in all treatments.

Hypothesis $\mathbf{H_0}$. Choices are not significantly different between treatments with economic and abstract framing.

Alternative hypotheses are motivated by the psychology literature, which recognizes that behavior is sensitive to how the game is presented (e.g. see Tversky and Kahneman, 1981, Levin et al., 1998). In particular, we will explore the possibilities that either preferences or beliefs are context-dependent.

The first possibility is that *preferences* are context-dependent, and thus depend on framing (group identity theories). In contests, economic framing implies that the other participant represents an opposing team, potentially inducing spiteful preference. In fact, it has been found that contests induce group identity and participants subsequently act less pro-socially towards the members of the opposing team compared to the members of their own team (Zaunbrecher et al., 2017). In the

[20] In fact, we find that players on average spent much more time using a payoff table than a payoff calculator (43% compared to 20%).

[21] Davis et al. (2003) find that the frequency of Nash equilibrium play in a Cournot triopoly is higher with random rather than with partner matching, although only when the marginal cost schedule is 'steep'. Baik et al. (2015) find little difference between random and fixed matching in 2- and 3-player contests. Fallucchi and Renner (2016) find no effect of matching in contests when full information is provided, but random matching shifts choices closer to the Nash equilibrium when information about the choices and payoffs of other participants is suppressed.

[22] In oligopoly games, subjects are often framed to act as 'managers', and market prices depend on their decisions as well as on those from 'rivals' or other 'firms' (Huck et al., 2000, Huck et al., 2001, Huck et al., 2004, Offerman et al., 2002), 'sellers' (Anderson et al., 2015) or 'competitors' (Gürerk and Selten, 2012, Bigoni and Fort, 2013). In typical contest framing, subjects compete with other 'participants' (Fallucchi et al., 2013, Masiliunas et al., 2014, Chowdhury et al., 2014, Sheremeta, 2010, Sheremeta, 2011)/'opponents' (Herrmann and Orzen, 2008, Lim et al., 2014) for a 'prize' (Herrmann and Orzen, 2008, Lim et al., 2014, Fallucchi et al., 2013), 'reward' (Chowdhury et al., 2014, Sheremeta, 2010, Sheremeta, 2011) or for an additional monetary value (Masiliunas et al., 2014) by purchasing 'lottery tickets' (Faravelli and Stanca, 2012), 'contest tokens' (Fallucchi et al., 2013, Abbink et al., 2010) or by making 'bids' (Chowdhury et al., 2014, Sheremeta, 2010, Sheremeta, 2011).

[23] We used such quantity framing in both the games of strategic complements and substitutes, to keep the framing manipulation identical.

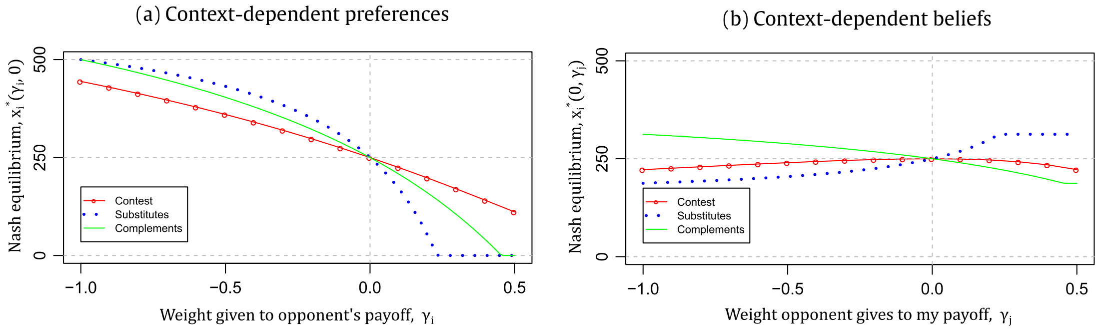

Fig. 2. Nash equilibrium action with context-dependent preferences and beliefs.

games of complements and substitutes, economic framing might suggest that the other party is a member of an outgroup, and thus induce spiteful preference. There is some evidence that the context in which the other participant is embedded affects behavior; for example, both trust and trustworthiness are higher in a trust game when the other participant is referred to as a 'partner' instead of as an 'opponent' (Burnham et al., 2000).

Another possibility is that framing affects not the preferences, but *beliefs* about the type and therefore behavior of the opponent. This is especially plausible at the start of the game, before any feedback has been received. There is prior evidence suggesting that framing primarily affects beliefs rather than preferences (Ellingsen et al., 2012), but there is some disagreement regarding the direction of the effect (Dufwenberg et al., 2011), which we hope to clarify.

We model context-dependent preferences and beliefs using a simple utility function of the form $u_i(\pi_i, \pi_j) = \pi_i + \gamma_i \pi_j$, for $i \in \{1, 2\}$.[24] Player i is altruistic if $\gamma_i > 0$ and spiteful if $\gamma_i < 0$. We assume that $\gamma_i, \gamma_j \in (-1, 1)$, that is players place a lower weight on opponent's payoffs than on own payoffs. Our theoretical benchmark is the action of player i in a Nash equilibrium, $x_i^*(\gamma_i, \gamma_j)$, derived in Appendix B.

We interpret γ_i as the social preference of player i, and γ_j as the belief of player i about the social preference of j. First, we consider these mechanisms separately. To measure purely the effect of preferences, we calculate how $x_i^*(\gamma_i, \gamma_j)$ depends on γ_i, when γ_j is set to 0. To measure purely the effect of beliefs, we calculate how $x_i^*(\gamma_i, \gamma_j)$ depends on γ_j, when γ_i is set to 0.[25]

In Appendix B, we show that $x_i^*(\gamma_i, 0)$ is decreasing in γ_i, in all three games. This result is illustrated in panel (a) of Fig. 2. Compared to the benchmark of purely selfish preferences ($\gamma_i = 0$), spitefulness ($\gamma_i < 0$) induces more competitive (higher) and altruism ($\gamma_i > 0$) induces less competitive (lower) choices. According to the context-dependent preference hypothesis ($\mathbf{H_p}$), economic framing shifts preferences towards spitefulness, predicting that in all three games choices will be more competitive than with abstract framing.

Hypothesis $\mathbf{H_p}$. Choices are significantly less competitive with abstract framing than with economic framing in all three games.

Appendix B shows that the effect of beliefs depends on the nature of the game: $x_i^*(0, \gamma_j)$ is decreasing in γ_j in the game of strategic complements, increasing in strategic substitutes, and non-monotonic in the contest game. This result is illustrated in panel (b) of Fig. 2. The choices of players who expect more altruistic opponents ($\gamma_j > 0$) are less competitive than choices of players who expect more spiteful ($\gamma_j < 0$) opponents in the game of strategic complements, but the pattern is reversed in the game of strategic substitutes. In contests, players who expect more altruistic or more spiteful opponents behave less competitively than players expecting self-regarding opponents ($\gamma_j = 0$). The context-dependent belief hypothesis ($\mathbf{H_b}$) states that economic framing will lead players to expect more spiteful opponents, predicting more competitive choices in the game of strategic complements and less competitive choices in strategic substitutes. The relationship is non-monotonic and smaller in contests. Note that the context-dependent belief hypothesis rests on the assumption that beliefs and preferences are independent; Appendix C shows that it will no longer hold if the interaction between beliefs and preferences is sufficiently high.

[24] This corresponds, for example, to how social preferences are instantiated in the Social Value Orientation (SVO) measure (Murphy and Ackermann, 2014). An alternative assumption would be to allow preference to be interactive (see Nax et al. 2015; Ackermann and Murphy 2019; Ackermann et al. 2016). Appendix C illustrates the predictions of a model with interactive preferences, based on a utility function proposed by Levine (1998). We show that when the interaction between beliefs and preferences is sufficiently high, some of the predictions developed in this section would no longer hold (e.g. the context-dependent belief hypothesis would no longer distinguish strategic complements from substitutes). However, we favor the non-interactive version of preferences and beliefs presented in this section because it has a clear interpretation and distinction between preferences and beliefs.

[25] Of course, framing might operate through both beliefs and preferences at the same time; we will consider such a hybrid model in section 4.4.

Table 2
Average choices across treatments. Standard deviations (in parentheses) are calculated for each matching group and round, and then averaged. MWU is a two-sided Mann-Whitney *U* test.

		Rounds			Independent observations
		1-20	1-5	15-20	
Contest	Economic	249 (76)	221 (124)	264 (47)	12
	Abstract	214 (73)	169 (129)	245 (49)	12
MWU p-value		0.0377	0.0350	0.3263	
Complements	Economic	322 (103)	361 (178)	311 (72)	12
	Abstract	201 (88)	195 (154)	213 (52)	12
MWU p-value		0.0015	0.0001	0.0243	
Substitutes	Economic	251 (128)	243 (190)	269 (104)	12
	Abstract	238 (133)	245 (180)	242 (110)	12
MWU p-value		1.0	0.9081	0.6033	

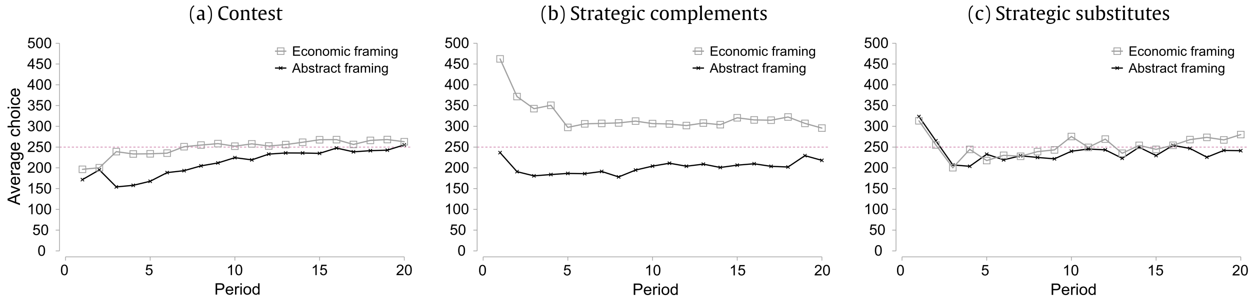

Fig. 3. Average action over time.

Hypothesis $\mathbf{H_b}$. Choices are significantly less competitive with abstract framing in the game of strategic complements, but more competitive in the game of strategic substitutes.

4. Results

4.1. Aggregate results

We find that economic framing induces more competitive behavior, increasing the average choice from 214 to 249 (16%) in contest, from 201 to 322 (60%) in strategic complements and from 238 to 251 (6%) in strategic substitutes. We evaluate the significance of the framing effect by calculating the average action in each matching group (12 matching groups per treatment) and comparing the values using the Mann-Whitney *U*-test. Table 2 shows that across all rounds, the framing effect is significant in contest ($p = 0.0377$) and strategic complements ($p = 0.0015$), but not in strategic substitutes ($p = 1.0$). Both the economic size of the effect and the statistical significance in contest and strategic complements are lower at the end of the game (last 5 rounds) than at the start (first 5 rounds). Further evidence for the diminishing framing effect can be seen by plotting the average choice in each treatment over time (Fig. 3).

We quantify the framing effect and its change over time by regressing the individual choices on a dummy variable indicating the framing condition, round number and their interaction. Table 3 shows the estimated coefficient values. The framing effect is significant in contests and complements, but not in substitutes. On average, economic framing increases first round choices by 56 points in contest and by 165 points in complements. In complements, the treatment difference decreases by 5 points per round. By the end of the experiment, the estimated treatment effect is significant only in complements. The average framing effect in all rounds is significant in contest (35 points difference) and in strategic complements (121 points difference), but not significantly different in strategic substitutes.

None of the three hypotheses listed in the previous section can fully explain the data. The Nash equilibrium hypothesis ($\mathbf{H_0}$) can be rejected because the framing effect is clearly observed in the contest game and in the game of strategic complements. The context-dependent preference hypothesis ($\mathbf{H_p}$) correctly predicts the treatment effect in contest games and in strategic complements, but fails to explain why such pattern is not observed in strategic substitutes. The context-dependent belief hypothesis ($\mathbf{H_b}$) correctly predicts the ranking of the framing effect across the three games, but the effect in strategic substitutes is insignificant instead of being the opposite of strategic complements. These findings suggest that a combination of context-dependent preferences and beliefs is needed to organize the results, as will be discussed in section 4.4.

Table 3
Random-effects regression (GLS). Standard errors are clustered on the matching group level (24 clusters per game).

	(1) All games	(2) Contest	(3) Complements	(4) Substitutes
Economic framing (round 1)	70.794***	55.939***	164.722***	-4.883
	(3.66)	(2.75)	(5.59)	(-0.15)
Round	1.895**	4.988***	1.253	-0.372
	(2.11)	(4.56)	(0.88)	(-0.22)
Round * Economic framing	-1.509	-2.089	-4.596**	1.897
	(-1.22)	(-1.62)	(-2.09)	(0.90)
Economic framing (round 20)	42.127**	16.257	77.403*	31.157
	(2.13)	(0.79)	(1.77)	(0.92)
Economic framing (rounds 1-20)	56.285***	34.907**	120.650***	13.322
	(3.60)	(2.13)	(3.91)	(0.51)
Number of observations	5708	1897	1905	1906

t statistics in parentheses.
Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

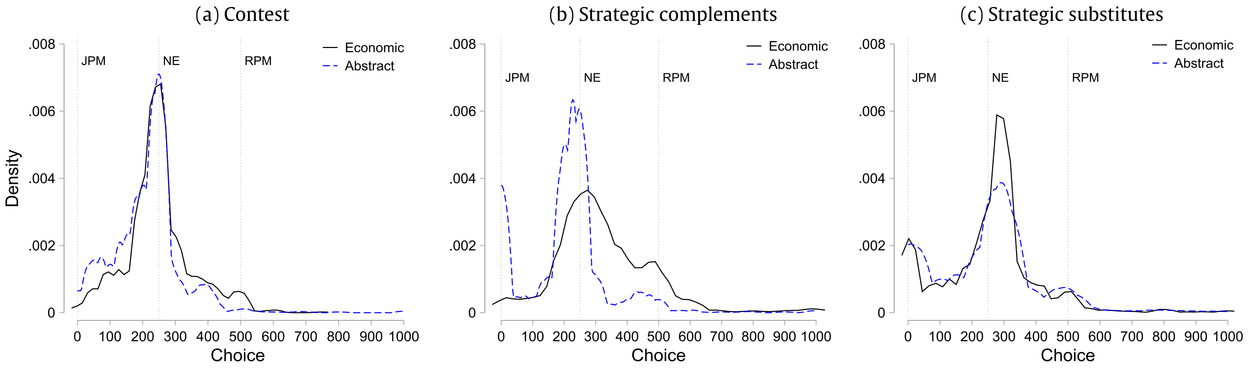

Fig. 4. Kernel density estimation using Epanechnikov kernel function.

4.2. *Classification of individual and matching group behavior*

We have shown that framing has a significant effect on the average action in strategic complements and contest. This section studies how framing affects matching group and individual behavior.

Fig. 4 shows the kernel density estimations of individual choices in each treatment.[26] We classify individual choices into three categories (JPM, NE or RPM), based on which of the three outcomes (joint profit maximization point, Nash equilibrium, relative profit maximization point) is closest to the chosen action (a similar classification procedure is used by Huck et al., 2004). The framing effect is strongest in strategic complements, where abstract framing reduces the fraction of RPM choices from 29% to 8% and increases the fraction of JPM choices from 7% to 24% (NE rates remain similar at 64% and 68% respectively). A similar but smaller effect is found in contest, where abstract framing reduces RPM choices from 11% to 6%, but increases JPM rates from 11% to 19% (NE rates are 78% and 75%). Distributions are similar in strategic substitutes, with slightly higher JPM rates (29% vs 20%) and lower NE rates (58% vs 67%) in abstract treatments (RPM rates are 13% in both).

Classification of individual choices by partitioning the strategy space ignores the data structure. As an alternative, we classify each matching group based on the outcomes most group members chose on most rounds. For example, if a group converges to the Nash equilibrium or is colluding, we would expect most group members to act in line with this prediction. Since there are three main outcomes of interest (collusive outcome is at 0, Nash equilibrium is at 250 and the competitive outcome is at 500), we classify a round as a 'fully collusive round' for matching groups in which most group members (i.e. at least 3 out of 4) choose an action in the 0-50 range, as 'NE round' for groups in which most members choose in the 225-275 range and as 'fully competitive' if the actions are in the 475-525 range. If the action profile does not meet any of these criteria, we classify the round as 'partly collusive' if the choices of most group members are more collusive than the equilibrium prediction (i.e. below 250) and as 'partly competitive' if the majority of choices are more competitive than the equilibrium (i.e. above 250). Otherwise, we leave that round unclassified. We then count the number of rounds a group has been placed into each category, and call the group 'fully collusive' / 'partly collusive' / 'NE' / 'partly competitive' / 'fully

[26] The evolution of choices by each participant is shown in figures E.4 – E.3 in Appendix E.

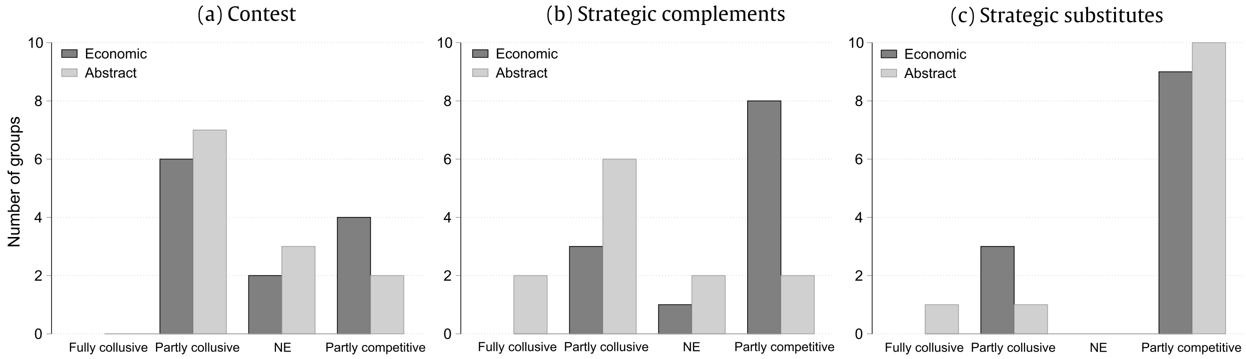

Fig. 5. Classification of matching groups. No groups are classified as fully competitive.

competitive' based on the category to which the group has been assigned in most the rounds (no ties occurred, therefore each group was assigned into one category).

Fig. 5 shows how matching groups were classified in each treatment. No group is classified as fully competitive and only three groups are fully collusive (two in strategic complements and one in substitutes; see figure E.1 in Appendix E for more details). All three colluding groups are found in the abstract framing conditions.[27] In contest and strategic complements, economic framing increases the number of partly competitive groups and decreases the number of NE, fully and partly collusive groups. The large treatment effect in strategic complements is driven in part by higher collusion rates (2 groups fully colluded in abstract framing treatment, none in economic), but mainly by a large difference in partly competitive behavior (2 groups in abstract framing treatment and 8 in economic). In strategic substitutes, most groups are classified as partly competitive and there is little difference between treatments.

4.3. Adaptation and outcomes in the long run

So far we looked only at the effect of framing on aggregate outcomes. Next, we will compare differences in terms of the patterns of adaptation and the long-run outcomes, to test if abstract framing increases the tendency to best-respond and the likelihood of convergence to the Nash equilibrium. In terms of the adaptation process, we compare two rules that could be used our setting: best-responding to the action chosen by the opponent the previous round and imitating the action of the most successful person in the group. Since we study only two-player games, imitate-the-best predicts either choosing the opponent's action (if opponent's payoff was higher than own payoff) or choosing the same action as in the previous round. We evaluate the two theories by estimating the following model from Huck et al. (1999):

$$x_i^t - x_i^{t-1} = \beta_0 + \beta_r(r_i^{t-1} - x_i^{t-1}) + \beta_{ib}(ib_i^{t-1} - x_i^{t-1}) + \varepsilon_i^t$$

where r_i^{t-1} is i's best reply to the opponent's action in round $t-1$ and ib_i^{t-1} is the action of the person in the pair who obtained higher payoffs in $t-1$. The β coefficients measure the importance of each adaptation rule: $\beta_r = 1$ and $\beta_{ib} = 0$ would indicate perfect adjustment to the myopic best-response; $\beta_r = 0$ and $\beta_{ib} = 1$ would indicate perfect imitation.

The first three models in Table 4 show the estimated coefficient values in each game when both framing conditions are pooled. The last column pools all three games. The coefficients of both best-response and imitation are significantly above zero in all three games. The best-response coefficient is higher than the coefficient of imitation, which might suggest that best-response is more important than imitation, although Wald test fails to reject the hypothesis that $\beta_r - \beta_{ib}$ is significantly different from zero ($p = 0.1055$ in contest, $p > 0.4$ in the other two games, $p = 0.1092$ when pooled). The importance of imitation also seems to decrease in abstract treatments, although the difference is not statistically significant (the coefficient for interaction between β_{ib} and abstract framing is not significant in all four models). These conclusions do not change if we additionally control for the game in the pooled model. Table E.1 in Appendix E shows the parameter values estimated separately for each treatment. The results are very similar and there is no significant difference between the coefficients of best-response and imitation (except for the abstract contest, in which Wald test $p = 0.0086$). Overall, even though best-response has a consistently higher coefficient than imitation and there is some indication that the importance of imitation decreases when abstract framing is used, none of these differences are statistically significant. We therefore conclude that both myopic best-response and imitation can explain some patterns of adaptation, but there is no difference between the two and no significant framing effect on the importance of either adaptation rule.

Finally, we test whether framing affects long-run behavior, and specifically the rates of convergence to the Nash equilibrium. If economic framing permanently activates competitive preferences, participants might fail to adapt their choices

[27] Since the games were not designed to study collusion, collusion rates were very low (only 3 out of 72 groups) and we cannot conclude whether colluding is more difficult with economic framing.

Table 4
Random effects GLS regression. Independent variable is the change in chosen action across rounds. Standard errors are clustered on the matching group level.

	(1) Contest	(2) Complements	(3) Substitutes	(4) All games
β_r	0.355***	0.308***	0.357***	0.335***
	(7.76)	(5.85)	(5.58)	(9.65)
β_{ib}	0.202***	0.241***	0.275***	0.241***
	(3.40)	(4.60)	(4.14)	(6.27)
Abstract	-11.29*	-18.49**	-2.639	-11.29**
	(-1.78)	(-1.99)	(-0.24)	(-1.97)
Abstract * β_r	0.172*	0.0550	-0.115	-0.0147
	(1.84)	(0.53)	(-1.39)	(-0.25)
Abstract * β_{ib}	-0.0907	-0.113	-0.0485	-0.0528
	(-0.82)	(-1.24)	(-0.53)	(-0.91)
Constant	-0.821	-2.394	-20.90***	-7.329**
	(-0.24)	(-0.43)	(-3.16)	(-2.12)
Observations	1801	1809	1810	5420

t statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
Fraction of choices in the neighborhood of NE in rounds 11-20. The neighborhood is measured as a percentage of the range between JPM and RPM choices.

	5% neighborhood		10% neighborhood	
	Economic	Abstract	Economic	Abstract
Contest	42%	50%	69%	71%
Strategic complements	21%	44%	56%	69%
Strategic substitutes	18%	16%	57%	51%

in the direction of the best-response and convergence rates would be lower. If framing effects are transitory and subject to updating in light of evidence, they should not prevent convergence to Nash equilibrium. We measure convergence by calculating the fraction of observations in the second half of the experiment that are in the neighborhood of the Nash equilibrium, defined in terms of the percentage of the range between the joint payoff maximization and relative payoff maximization points (see Davis, 2011, for a similar approach). Table 5 shows that economic framing lowers the convergence rates in contest and in strategic complements, but not in strategic substitutes. We evaluate the statistical significance of treatment differences by comparing the relative frequency of choices in the neighborhood of the Nash equilibrium for each matching group. We find a significant difference only in strategic complements when 5% neighborhood is used (MWU $p = 0.0458$). Figure E.1 (Appendix E) illustrates this difference in strategic complements by plotting the dynamics of average choice in each matching group.

4.4. Static model of beliefs and preferences

Our next goal is to uncover what framing differences across games reveal about the origins of the framing effect. First, we develop a static model in which both beliefs and preferences are context-dependent, and estimate the combinations of preferences and beliefs that can best explain our experimental data. As in section 3, we assume the following utility function[28]:

$$u_i(\pi_i, \pi_j) = \pi_i + \gamma_i \pi_j, \text{for } i \in \{1, 2\} \tag{3}$$

We are interested in finding the combination of context-dependent social preferences (γ_i) and beliefs about opponent's social preference (γ_j) that can best explain the experimental data. Our solution concept is the Nash equilibrium action, derived from equation (3) (see equations (15) and (14) in Appendix B). Fig. 6 illustrates the equilibrium action for a range of parameter values. With standard preferences ($\gamma_i = \gamma_j = 0$), Nash equilibrium is equal to 250 in all three games. A movement along the 45 degree line towards the upper right corner is the usual way to model pro-social preferences, with identical preferences for both players ($\gamma_i = \gamma_j > 0$). In all three games, pro-social preferences decrease the equilibrium action of player i (i.e. behavior becomes more cooperative). Similarly, a movement towards the lower left corner is the usual way to model anti-social preferences ($\gamma_i = \gamma_j < 0$), which increases the equilibrium action of player i (i.e. behavior becomes more

[28] Allowing the preferences and beliefs to be interactive (using the utility function from Levine, 1998) would not improve the explanatory power of the model because any utility specification with interactive preferences can be equivalently represented using the utility function specified in (3), with appropriately chosen weights. For more details and examples, see Appendix C.1.

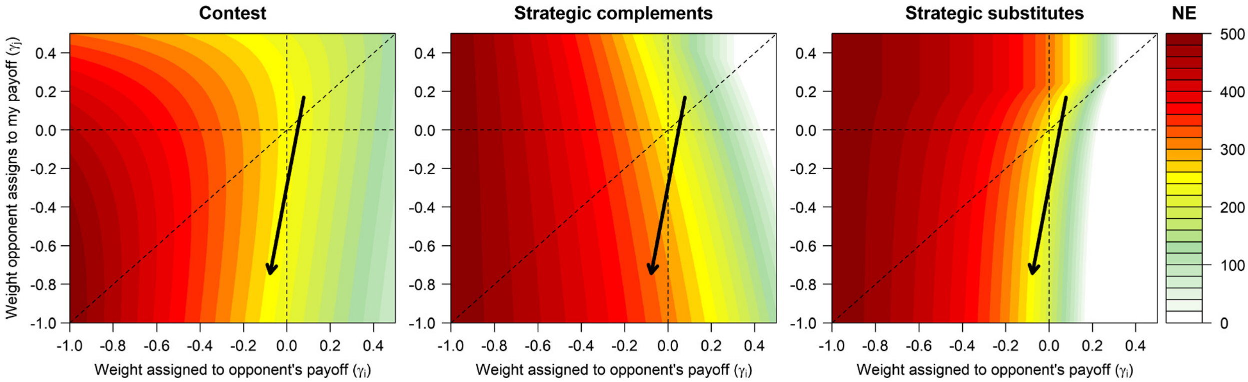

Fig. 6. Nash equilibrium quantity as a function of possible combinations of weight attached by self to other player (γ_i) and weight attached by other player to self (γ_j). The arrows indicate the estimated change in beliefs and preferences caused by economic framing.

competitive). Movement along the vertical or horizontal lines decomposes the effect into a part that is due to beliefs and a part that is due to preferences. Ceteris paribus, a preference change (horizontal movement) has a similar effect as a change in both preferences and beliefs. Effects due to changes in beliefs differ across the three games. In complements, players who expect the other participant to place a positive weight on their own payoff ($\gamma_i = 0$, $\gamma_j > 0$) would lower their actions, and those expecting a negative weight ($\gamma_i = 0$, $\gamma_j < 0$) would increase them. In substitutes, the effect is reversed. In contests, any change in beliefs would decrease chosen actions.

Next, we look for the combination (γ_i, γ_j) that best fits the data. We allow parameters to be context-dependent, but assume no heterogeneity in preferences and beliefs across the population, and no differences between the populations in the three games. We estimate two sets of parameters: (γ_i^a, γ_j^a) for treatments with abstract framing and (γ_i^e, γ_j^e) for treatments with economic framing, by minimizing the mean squared deviation (MSD) between the Nash equilibrium prediction and experimental data. The estimated parameter values indicate anti-social preferences and beliefs with economic framing ($\hat{\gamma}_i^e = -0.08$, $\hat{\gamma}_j^e = -0.74$) and pro-social preferences and beliefs with abstract framing ($\hat{\gamma}_i^a = 0.08$, $\hat{\gamma}_j^a = 0.17$). In Fig. 6, this change from abstract to economic framing is indicated by an arrow. Note that with both abstract and economic framing the estimated effect is much stronger on beliefs than on preferences. When we allow for either only context-dependent beliefs or only context-dependent preferences, the fit of either model is worse than the hybrid model. The estimated root mean squared deviation equals 140.2 in the hybrid model, 144.1 with only context-dependent beliefs and 143.4 with only context-dependent preferences. These results show that the framing effect observed in our data can be explained by a model in which framing operates mostly through beliefs, but also, to a smaller extent, through preferences.

4.5. Dynamic model of beliefs and preferences

Section 4.4 showed that a static model with context-dependent preferences and beliefs can explain the treatment differences in terms of average choices. This section tests whether a dynamic model of context-dependent preferences or beliefs can explain the adaptation behavior. A dynamic setup allows us to implement a game-theoretic view that beliefs are updated based on the observed history, but preferences are time-invariant.

We model beliefs and preferences in a dynamic setting using a belief learning model: observed history is used to form beliefs about opponent's future behavior and players choose a (noisy) best-response to their beliefs. The starting point of our model is weighted fictitious play (Cheung and Friedman, 1997). In the original model, probabilistic beliefs about each action are determined by counting their empirical frequencies. We modify it by allowing point beliefs, calculated as a weighted average of observed past choices (see Offerman et al., 2002, for a similar approach). This approach is necessary because of the large strategy space, with which the empirical counts of each action grow too slowly. The second modification adds context-dependent social preferences and beliefs. Beliefs are modeled through the initial beliefs held in the first round. Social preferences are modeled by assuming that players maximize not the expected payoff, but the expected utility, which includes a term for opponent's payoff. Modeled this way, the importance of initial beliefs diminishes over time, while the effect of preferences remains constant in all rounds.

Overall, the belief of player i in round $t \geqslant 2$ is calculated using the following rule:

$$b_i(t) = \frac{\gamma^{t-1} W b_i(1) + \sum_{u=1}^{t-1} (\gamma^{u-1}) h_{-i}(t-u)}{\gamma^{t-1} W + \sum_{u=1}^{t-1} \gamma^{u-1}} \tag{4}$$

Table 6
Estimated parameter values of a belief learning model with context-dependent preferences and beliefs. Standard errors are in parentheses.

Parameter	Contest		Complements		Substitutes	
	Economic	Abstract	Economic	Abstract	Economic	Abstract
λ (sensitivity)	0.033 (0.0012)	0.033 (0.0012)	0.015 (0.0007)	0.015 (0.0007)	0.014 (0.001)	0.014 (0.001)
γ (discount factor)	0.57 (0.066)	0.57 (0.066)	0.28 (0.10)	0.28 (0.10)	1 (-)	1 (-)
W (weight of initial belief)	62.35 (42.87)	62.35 (42.87)	3.57 (2.81)	3.57 (2.81)	714.29 (4278.60)	714.29 (4278.60)
$b(1)$ (initial belief)	623.47 (31.51)	741.34 (41.52)	999.71 (117.08)	306.62 (90.22)	1000 (-)	84.23 (113.76)
α (other-regarding preference)	0.0044 (0.014)	0.11 (0.015)	-0.087 (0.019)	0.24 (0.02)	-0.16 (0.02)	0.26 (0.055)

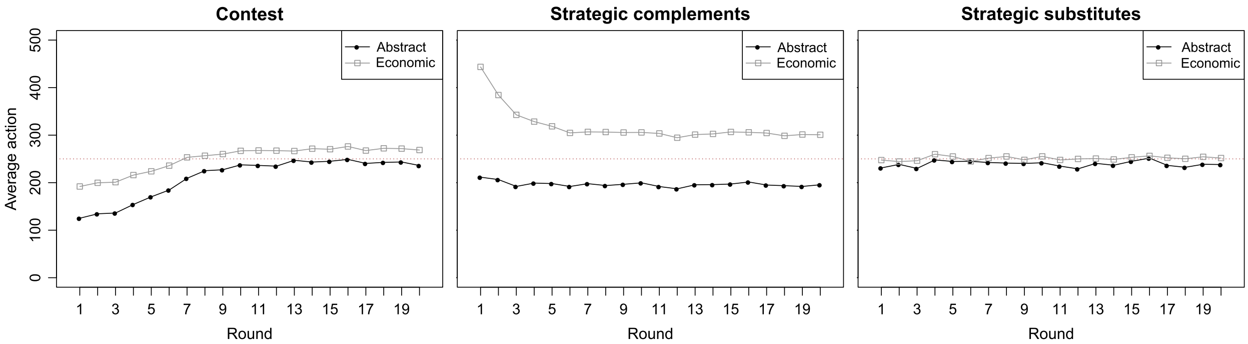

Fig. 7. Choice paths simulated with the parameter values from Table 6.

where $h_{-i}(t)$ is the choice of the other participant in round t, γ is the discount factor,[29] $b_i(1) \in [0, 1000]$ is the initial belief in the first round, and $W \geqslant 1$ is the weight of the initial belief.

The expected utility of action $a \in A \equiv [0, 1000]$ for player i in round t depends on own payoff and on opponent's payoff:

$$Eu_i(a, b_i(t)) = \pi_i(a, b_i(t)) + \alpha \pi_j(b_i(t), a)$$

with payoff functions defined by equations (1) and (2).

The probability to choose action a is calculated using a logistic choice rule, in which parameter λ measures the sensitivity to expected utility differences[30]:

$$Pr_i^t(a) = \frac{e^{\lambda Eu_i(a,b_i(t))}}{\sum_{a' \in A} e^{\lambda Eu_i(a',b_i(t))}}, \quad \text{with } \lambda \in [0, \infty) \tag{5}$$

For each player and each round, we calculate beliefs using equation (4) and the predicted likelihood of the chosen action using equation (5). We round actions to the closest multiple of 5, to reduce the computational complexity. The parameter values of the five free parameters are estimated using log-likelihood maximization, separately for each game and allowing for framing-specific initial beliefs and preferences (as measured by parameters $b(1)$ and α). Estimations were performed using derivative-free optimization routines with various starting values. Standard errors were calculated from the variance-covariance matrix, estimated from the numerical approximation of the Hessian matrix. Standard errors for parameters at the bound are not provided.

Estimated parameter values are displayed in Table 6. In each game, the estimated initial beliefs $b(1)$ are more competitive (higher) with economic compared to abstract framing. Similarly, the estimated preference parameter α is more competitive (lower) with economic compared to abstract framing. Note that in this model almost all the estimated preference parameters are pro-social, in contrast to the anti-social preferences under economic framing estimated in section 4.4. The difference is driven by imperfect sensitivity to expected utility differences added to this model, which biases choices upwards even in the absence of spite.

[29] If $\gamma = 0$, the belief is equal to the action of the other participant in the previous round and the model reduces to Cournot best-response. If $\gamma = 1$, the belief is equal to the average action of the other participant in all past rounds.

[30] Mäs and Nax (2016) have investigated behavioral deviation rates as implied by such a model.

Finally, the estimated parameter values are used to simulate the path of choices in each treatment, testing whether belief learning with context-dependent beliefs and preferences can reproduce the empirical choice patterns. The simulated agents are randomly paired within a 4-agent matching group, just as in the experiment. We simulate the path of choices for 1000 agents, and display the average choices in Fig. 7. Overall, the simulations closely reproduce the dynamics observed in experiments (compare to Fig. 3).

5. Concluding remarks

We find that economic framing as used in typical contest and oligopoly experiments leads to more competitive behavior in contests and in the game of strategic complements. In strategic substitutes, the framing effect is smaller and not significant. The difference between games can be explained with beliefs being more strongly affected by framing than preferences. The finding that the framing effect is increasing in the degree of strategic complementarity can help resolve some puzzles from prior experiments. For example, Huck et al. (2004) find that economic framing has no effect or even reduces the competitiveness of choices in Cournot oligopoly, in contrast to the opposite finding in social dilemma games (Pillutla and Chen, 1999, Engel and Rand, 2014). This difference could be explained by the strategic environment, as Cournot oligopoly is a game of strategic substitutes, while social dilemma games exhibit strategic complementarity (Fischbacher et al., 2001). Our results also complement previous findings about framing in Cournot oligopoly (i.e. strategic substitutes) from Huck et al. (2004), who find that economic framing increases cooperation in duopolies but there is no framing effect in markets with five firms. In our game of strategic substitutes, there is no framing effect despite the use of two-player groups, which may suggest that framing effects in Cournot are relatively weak or design-dependent.[31]

Our study's main focus is on the causes and consequences of the framing effect, therefore we did not discuss the main effect of the strategic complementarity. But since the games were designed to be comparable, we can contribute to the literature by testing whether strategic complements are different from substitutes under either framing condition. Previous literature found that strategic complementarity increases collusion both with standard framing (for an overview, see Suetens and Potters, 2007, or Engel, 2007) and with abstract framing (Potters and Suetens, 2009), although the two effects cannot be compared because of variation in design details across studies. We find more cooperation in strategic substitutes than in complements when economic framing is used, but no significant difference under abstract framing (for more details, see Appendix D). The difference could be attributed to the effect the strategic environment has on beliefs and preferences: competitive preferences and beliefs, activated by economic framing, reinforce each other in strategic complements, but offset each other in substitutes.

Why does strategic complementarity decrease cooperation in our experiment, in contrast to the results in the previous literature (as reviewed in Suetens and Potters, 2007)? The discrepancy is most likely due to variance in design details. First, the previous literature used different framing for Cournot (described as quantity competition) and Bertrand games (price competition), while we used the same framing in both strategic complements and substitutes. Second, there are differences in terms of payoff functions, as we followed Potters and Suetens (2009) instead of the standard Cournot and Bertrand payoff function. Third, we used random rather than fixed matching. If fixed matching is used, some participants might attempt to collude and the presence of strategic complementarity creates incentives for other participants to adjust choices in the same direction, boosting the frequency of partly collusive behavior (see Potters and Suetens, 2009, for a discussion of this hypothesis). Re-matching makes collusion difficult and instead might increase partly competitive choices, especially under economic framing. In this environment, strategic complementarity would operate in the opposite direction, boosting the frequency of partly competitive choices. In future research, it would be interesting to test this hypothesis by comparing the effect of the strategic environment across the two matching protocols.

Our approach of separating preferences and beliefs required some simplifying assumptions, which come with several caveats. First, we assumed that preferences are stable (i.e. not interactive or conditional) over the course of the experiment. To address this issue, we calculate the predictions for interactive preferences (Levine, 1998) and show that our results still hold as long as the interaction is sufficiently low. Second, our static framework follows the conventional assumption that beliefs are always correct. In fact, there is evidence that beliefs are often incorrect or misspecified, and the extent of departures from rational expectations may even depend on the strategic nature of the game (Fehr and Tyran, 2008), which might bias our explanations. Third, beliefs, consistent or not, may be self-confirming which creates a chicken-and-egg problem vis-a-vis our explanation (Fudenberg and Levine, 1993). Some of these issues could be addressed in future research by explicitly eliciting and studying the beliefs. Future research would ideally also investigate other types of frames (e.g. cooperative framing commonly used in social dilemma games) and how frames interact with the nature of the game. It would also be very useful to independently measure preferences and study which participants are more susceptible to the framing effects, especially given the recent evidence about the interaction between types and the strategic environment (Savikhin and Sheremeta, 2013; Prediger et al., 2014; Cárdenas et al., 2015).

In sum, our study indicates that framing choices need to be made carefully, because the impact of framing is not uniform and may depend on the nature of the game. We show that a difference between the games of strategic complements and

[31] Due to different research goals, we made different design choices than Huck et al. (2004): for example, we did not use the standard Cournot payoff function; we used random matching instead of partner matching; our instructions used different language than the corresponding treatments in Huck et al. (2004).

substitutes, found under economic framing, disappears when abstract framing is used. Since these effects are stronger at the start of the game, framing should be of particular importance in short or one-shot experiments. Which type of framing should be used, therefore, depends on the research questions. Economic framing might be more suitable to study specific hypotheses about firm behavior in oligopolies. Abstract framing might be more suitable to evaluate the effect of game-theoretic features. The comparison of both conditions reveals whether the observed behavioral regularities are driven by the economic incentives, or instead by other social or moral factors that are activated by the provided context. Future studies may proceed in this way to identify what kinds of 'non-economic interventions' (i.e. as driven by non-abstract framing such as information design and nudging) might be useful for firms to complement or substitute standard economic interventions aimed at modifying the strategic incentives of the game via mechanism or market design.

Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.geb.2020.10.002.

References

Abbink, K., Brandts, J., 2009. Collusion in growing and shrinking markets: empirical evidence from experimental duopolies. In: Hinloopen, J., Normann, H.-T. (Eds.), Experiments and Competition Policy. Cambridge University Press, Cambridge, pp. 34–60, chapter 3.
Abbink, K., Brandts, J., Herrmann, B., Orzen, H., 2010. Intergroup conflict and intra-group punishment in an experimental contest game. Am. Econ. Rev. 100 (1), 420–447.
Ackermann, K.A., Murphy, R.O., 2019. Explaining cooperative behavior in public goods games: how preferences and beliefs affect contribution levels. Games 10 (1), 15.
Ackermann, K.A., Fleiß, J., Murphy, R.O., 2016. Reciprocity as an individual difference. J. Confl. Resolut. 60 (2), 340–367.
Alekseev, A., Charness, G., Gneezy, U., 2017. Experimental methods: when and why contextual instructions are important. J. Econ. Behav. Organ. 134, 48–59.
Anderson, L.R., Freeborn, B.A., Hulbert, J.P., 2015. Determinants of tacit collusion in a Cournot duopoly experiment. South. Econ. J. 81 (3), 633–652.
Andreoni, J., 1995. Warm-glow versus cold-prickle: the effects of positive and negative framing on cooperation in experiments. Q. J. Econ. 110 (1), 1–21.
Andreoni, J., Croson, R., 2008. Partners versus strangers: random rematching in public goods experiments. In: Plott, C.R., Smith, V.L. (Eds.), Handbook of Experimental Economics Results. Elsevier, pp. 776–783.
Baik, K.H., Chowdhury, S.M., Ramalingam, A., et al., 2015. Group size and matching protocol in contests. University of East Anglia CBESS Discussion paper, (13-11R).
Batson, C.D., Moran, T., 1999. Empathy-induced altruism in a prisoner's dilemma. Eur. J. Soc. Psychol. 29 (7), 909–924.
Berg, J., Dickhaut, J., McCabe, K., 1995. Trust, reciprocity, and social history. Games Econ. Behav. 10 (1), 122–142.
Bernold, E., Gsottbauer, E., Ackermann, K.A., Murphy, R.O., 2015. Social framing and cooperation: the roles and interaction of preferences and beliefs. Working paper, available at SSRN: https://ssrn.com/abstract=2557927.
Bigoni, M., Fort, M., 2013. Information and learning in oligopoly: an experiment. Games Econ. Behav. 81, 192–214.
Burnham, T., McCabe, K., Smith, V.L., 2000. Friend-or-foe intentionality priming in an extensive form trust game. J. Econ. Behav. Organ. 43 (1), 57–73.
Cárdenas, J.C., Dreber, A., von Essen, E., Ranehill, E., 2015. Cooperativeness and competitiveness in children. J. Behav. Exp. Econ. 59, 32–41.
Charness, G., Rabin, M., 2002. Understanding social preferences with simple tests. Q. J. Econ. 117 (3), 817–869.
Chaudhuri, A., 2011. Sustaining cooperation in laboratory public goods experiments: a selective survey of the literature. Exp. Econ. 14 (1), 47–83.
Cheung, Y.-W., Friedman, D., 1997. Individual learning in normal form games: some laboratory results. Games Econ. Behav. 19 (1), 46–76.
Chowdhury, S.M., Sheremeta, R.M., Turocy, T.L., 2014. Overbidding and overspreading in rent-seeking experiments: cost structure and prize allocation rules. Games Econ. Behav. 87, 224–238.
Chowdhury, S.M., Mukherjee, A., Turocy, T.L., 2020. That's the ticket: explicit lottery randomisation and learning in Tullock contests. Theory Decis. 88 (3), 405–429.
Cookson, R., 2000. Framing effects in public goods experiments. Exp. Econ. 3 (1), 55–79.
Davis, D., 2011. Behavioral convergence properties of Cournot and Bertrand markets: an experimental analysis. J. Econ. Behav. Organ. 80 (3), 443–458.
Davis, D.D., Reilly, R.J., Wilson, B.J., 2003. Cost structures and Nash play in repeated Cournot games. Exp. Econ. 6 (2), 209–226.
Dechenaux, E., Kovenock, D., Sheremeta, R.M., 2015. A survey of experimental research on contests, all-pay auctions and tournaments. Exp. Econ. 18 (4), 609–669.
Dreber, A., Ellingsen, T., Johannesson, M., Rand, D.G., 2013. Do people care about social context? Framing effects in dictator games. Exp. Econ. 16 (3), 349–371.
Dufwenberg, M., Gächter, S., Hennig-Schmidt, H., 2011. The framing of games and the psychology of play. Games Econ. Behav. 73 (2), 459–478.
Ellingsen, T., Johannesson, M., Mollerstrom, J., Munkhammar, S., 2012. Social framing effects: preferences or beliefs? Games Econ. Behav. 76 (1), 117–130.
Engel, C., 2007. How much collusion? A meta-analysis of oligopoly experiments. J. Compet. Law Econ. 3 (4), 491–549.
Engel, C., Rand, D.G., 2014. What does "clean" really mean? The implicit framing of decontextualized experiments. Econ. Lett. 122 (3), 386–389.
Fallucchi, F., Renner, E., 2016. Reputational concerns in repeated rent-seeking contests. Technical report, CeDEx Discussion Paper Series 2016-05.
Fallucchi, F., Renner, E., Sefton, M., 2013. Information feedback and contest structure in rent-seeking games. Eur. Econ. Rev. 64, 223–240.
Faravelli, M., Stanca, L., 2012. When less is more: rationing and rent dissipation in stochastic contests. Games Econ. Behav. 74 (1), 170–183.
Fehr, E., Tyran, J.-R., 2008. Limited rationality and strategic interaction: the impact of the strategic environment on nominal inertia. Econometrica 76 (2), 353–394.
Fischbacher, U., 2007. z-Tree: Zurich toolbox for ready-made economic experiments. Exp. Econ. 10 (2), 171–178.
Fischbacher, U., Gächter, S., Fehr, E., 2001. Are people conditionally cooperative? Evidence from a public goods experiment. Econ. Lett. 71 (3), 397–404.
Friedman, D., Huck, S., Oprea, R., Weidenholzer, S., 2015. From imitation to collusion: long-run learning in a low-information environment. J. Econ. Theory 155, 185–205.
Fudenberg, D., Levine, D.K., 1993. Self-confirming equilibrium. Econometrica 61 (3), 523–545.
Gerlach, P., Jaeger, B., 2016. Another frame, another game? Explaining framing effects in economic games. Mimeo.
Greiner, B., 2015. Subject pool recruitment procedures: organizing experiments with ORSEE. J. Econ. Sci. Assoc. 1 (1), 114–125.
Gürerk, Ö., Selten, R., 2012. The effect of payoff tables on experimental oligopoly behavior. Exp. Econ. 15 (3), 499–509.
Herrmann, B., Orzen, H., 2008. The appearance of homo rivalis: social preferences and the nature of rent seeking. Mimeo.
Hoffman, E., McCabe, K., Shachat, K., Smith, V., 1994. Preferences, property rights, and anonymity in bargaining games. Games Econ. Behav. 7 (3), 346–380.
Hoffmann, T., 2016. Does belief elicitation affect game play? The role of game properties. Mimeo.

Holt, C.A., 1995. Industrial organization: a survey of laboratory research. In: The Handbook of Experimental Economics, pp. 349–444.
Huck, S., Normann, H.-T., Oechssler, J., 1999. Learning in Cournot oligopoly–an experiment. Econ. J. 109 (454), 80–95.
Huck, S., Normann, H.-T., Oechssler, J., 2000. Does information about competitors' actions increase or decrease competition in experimental oligopoly markets? Int. J. Ind. Organ. 18 (1), 39–57.
Huck, S., Muller, W., Normann, H.-T., 2001. Stackelberg beats Cournot—on collusion and efficiency in experimental markets. Econ. J. 111 (474), 749–765.
Huck, S., Normann, H.-T., Oechssler, J., 2004. Two are few and four are many: number effects in experimental oligopolies. J. Econ. Behav. Organ. 53 (4), 435–446.
Ledyard, J.O., 1995. Public goods: a survey of experimental research. In: Kagel, J., Roth, A. (Eds.), Handbook of Experimental Economics. Princeton University Press, Princeton, pp. 111–194, chapter 2.
Levin, I.P., Schneider, S.L., Gaeth, G.J., 1998. All frames are not created equal: a typology and critical analysis of framing effects. Organ. Behav. Hum. Decis. Process. 76 (2), 149–188.
Levine, D.K., 1998. Modeling altruism and spitefulness in experiments. Rev. Econ. Dyn. 1 (3), 593–622.
Liberman, V., Samuels, S.M., Ross, L., 2004. The name of the game: predictive power of reputations versus situational labels in determining prisoner's dilemma game moves. Pers. Soc. Psychol. Bull. 30 (9), 1175–1185.
Lim, W., Matros, A., Turocy, T.L., 2014. Bounded rationality and group size in Tullock contests: experimental evidence. J. Econ. Behav. Organ. 99, 155–167.
Mäs, M., Nax, H.H., 2016. A behavioral study of "noise" in coordination games. J. Econ. Theory 162, 195–208.
Masiliunas, A., Mengel, F., Reiss, J.P., 2014. Behavioral variation in Tullock contests. Technical report, Working Paper Series in Economics, Karlsruher. Institut für Technologie (KIT).
Murphy, R.O., Ackermann, K.A., 2014. Social value orientation: theoretical and measurement issues in the study of social preferences. Personal. Soc. Psychol. Rev. 18 (1), 13–41.
Murphy, R.O., Ackermann, K.A., 2015. Social preferences, positive expectations, and trust based cooperation. J. Math. Psychol. 67, 45–50.
Murphy, R.O., Ackermann, K.A., Handgraaf, M.J., 2011. Measuring social value orientation. Judgm. Decis. Mak. 6 (8), 771–781.
Nax, H.H., Murphy, R.O., Ackermann, K.A., 2015. Interactive preferences. Econ. Lett. 135 (C), 133–136.
Offerman, T., Potters, J., Sonnemans, J., 2002. Imitation and belief learning in an oligopoly experiment. Rev. Econ. Stud. 69 (4), 973–997.
Ozkes, A., Hanaki, N., 2019. Talkin'bout cooperation. Working paper, available at SSRN: https://ssrn.com/abstract=3484927.
Pillutla, M.M., Chen, X.-P., 1999. Social norms and cooperation in social dilemmas: the effects of context and feedback. Organ. Behav. Hum. Decis. Process. 78 (2), 81–103.
Potters, J., Suetens, S., 2009. Cooperation in experimental games of strategic complements and substitutes. Rev. Econ. Stud. 76 (3), 1125–1147.
Potters, J., Suetens, S., 2013. Oligopoly experiments in the current millennium. J. Econ. Surv. 27 (3), 439–460.
Prediger, S., Vollan, B., Herrmann, B., 2014. Resource scarcity and antisocial behavior. J. Public Econ. 119, 1–9.
Requate, T., Waichman, I., 2011. "A profit table or a profit calculator?" A note on the design of Cournot oligopoly experiments. Exp. Econ. 14 (1), 36–46.
Savikhin, A.C., Sheremeta, R.M., 2013. Simultaneous decision-making in competitive and cooperative environments. Econ. Inq. 51 (2), 1311–1323.
Schlag, K.H., Tremewan, J., Van der Weele, J.J., 2015. A penny for your thoughts: a survey of methods for eliciting beliefs. Exp. Econ. 18 (3), 457–490.
Sheremeta, R.M., 2010. Experimental comparison of multi-stage and one-stage contests. Games Econ. Behav. 68 (2), 731–747.
Sheremeta, R.M., 2011. Contest design: an experimental investigation. Econ. Inq. 49 (2), 573–590.
Sheremeta, R.M., 2013. Overbidding and heterogeneous behavior in contest experiments. J. Econ. Surv. 27 (3), 491–514.
Smith, V.L., 1976. Experimental economics: induced value theory. Am. Econ. Rev. 66 (2), 274–279.
Suetens, S., Potters, J., 2007. Bertrand colludes more than Cournot. Exp. Econ. 10 (1), 71–77.
Tversky, A., Kahneman, D., 1981. The framing of decisions and the psychology of choice. Science 211 (4481), 453–458.
Zaunbrecher, H.W., Williams, T., Riedl, A., 2017. Social identity in the lab: a horse race between methods. Mimeo, Maastricht University.