Competition, Cooperation, and Social Perceptions

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September 2022

Abstract: Many empirical and experimental studies show that social divisions negatively impact economic outcomes. This experiment reverses the causal arrow and asks if economic settings affect individuals' social perceptions. Subjects receive information about counterparts' preferences and demographics and then work for bonus pay by completing a real-effort task. Subjects who compete for pay against their counterparts report having less in common with their counterparts than subjects in a cooperative setting. This effect emerges despite monetary incentives to report correctly the number of traits in common. The economic setting has little effect on the less precise evaluation of similarity to counterparts.

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1 Introduction

What are the causes of social divisions? How do people come to think of themselves as having more or less in common with others? A large body of research shows that social divisions adversely affect economic outcomes. Ethnically divided communities invest less in public goods; ethnic fractionalization relates directly to deforestation; and ethnic diversity helps explain cross-country patterns of low relative economic growth.[1] At the individual level, much experimental evidence indicates that even mild social distinctions can lead to choices that reduce the payoffs of counterparts from other groups.[2]

This paper reverses the causal arrow and studies possible economic sources of social divisions. The experimental study asks whether the economic context affects how people socially perceive others, as more or less similar to themselves and as having more or less in common. We ask, in particular, whether competitive or cooperative economic settings impact these views. The paper thus hearkens back to early experiments in social psychology which show that competition creates animosities (Sherif et al. (1954)), and our findings are consistent with a recent historical study demonstrating anti-Semitism emerged in Germany more strongly in regions where Christians and Jews could compete in money lending (Becker & Pascali (2019)).

This paper is the first to propose an experiment to test whether economic settings affect social perceptions and to investigate motivated views of social closeness. In the experiment, subjects are given true information about counterparts' preferences and demographics, and subjects perform a real-effort task for pay. We find that subjects randomly assigned to the competitive pay scheme report having less in common with their counterparts than subjects assigned to the cooperative pay scheme, all else equal. This effect emerges despite that commonality is measured objectively, and subjects have monetary incentives to report correctly how many characteristics they have in common with their counterparts.

[1] See, respectively, Alesina, Baqir & Easterly (1999), Miguel & Gugerty (2005), Goldin & Katz (1997), Alesina, Gennaioli & Lovo (2019), Easterly & Levine (1997). For review, see Alesina & Ferrara (2005).

[2] See, for example, Glaeser et al. (2000), Fershtman & Gneezy (2001), Chen & Li (2009), Goette, Huffman & Meier (2012), Klor & Shayo (2010), Bauer et al. (2018), Kranton et al. (2020).

The experimental design isolates the economic setting from other factors that could influence social evaluations, such as relative performance or earnings. The experiment consists of three parts.[3] First, each subject answers a Study Questionnaire which we developed that consists of four questions: gender, political party leaning, preferred season, and marital status. Second, subjects are randomly matched with a counterpart and informed of the real-effort task they will perform to earn bonus pay.[4] Subjects are randomly assigned to one of two settings: (a) *Competition*, where subjects are only paid when they outperform their counterpart or (b) *Cooperation*, where subjects are paid according to the sum of their performance and their counterpart's performance. Subjects then see their counterpart's answers to the Study Questionnaire and work on the real-effort task for three minutes. Third, subjects provide their views of their counterpart by answering two social perception questions. One question asks subjects to assess their similarity to the counterpart. One question asks subjects to report the number of answers to the Study Questionnaire that they have in common with their counterpart, and subjects have monetary incentives to report the correct number. The questions are based on the premise (operative also in the economic experiments reviewed below) that social divisions can derive from differences in individual traits and characteristics. The similarity question asks for a broad assessment of social closeness; the commonality question asks for a precise answer and thus allows incentives for responses. The subjects answer these questions before knowing their relative performance or bonus payments. Therefore, subjects' answers depend only on the treatment, *Competition* or *Cooperation*.

We find that subjects report having significantly fewer Questionnaire items in common with their counterparts in the *Competition* treatment than in the *Cooperation* treatment. The subject pools for each treatment are balanced in terms of demographics and the distributions of the true number of common answers. However, the mean number of common answers

[3]The experiment was approved by Duke University IRB, pre-registered at AsPredicted (#85269), and involved no deception.

[4]The counterparts performed the real-effort task in an earlier experimental session and their performances were recorded.

reported by subjects in the *Competition* treatment is significantly lower than the mean reports of subjects in the *Cooperation* treatment. This treatment effect is robust to controls for the true number of common answers, the order of the social perception questions, and demographic characteristics. The treatment effect is also the same magnitude for male and female subjects. These results demonstrate that subjects report more in common to counterparts who are co-workers rather than competitors. This pattern holds despite that subjects have a monetary incentive to correctly report the number of common answers.

The treatments have no overall significant effect on subjects' assessments of similarity to counterparts. While answers to the commonality and similarity question are highly correlated, we find evidence that subjects attend less to the similarity question. They answer the similarity question about three times faster (a highly significant difference), and no monetary incentives were attached to the similarity question. However, we find that female subjects state slightly but significantly more similarity to counterparts in *Cooperation* than in *Competition*. We discuss this outcome further below.

The investigation in this paper embarks on a new direction in the economic study of social divisions and advances the study of motivated beliefs to the domain of social perceptions. Relative to the experimental studies of social divisions, the present paper is a study of causes rather than consequences. Many previous economic experiments have a structure similar to experiments in social psychology which consider the impact of group divisions on choices such as reward allocations (e.g., Tajfel et al. (1971)) or trait attribution.[5] The experimental treatments invoke group or social divisions, and the outcome variables are subjects' behavior towards others. In one approach, treatments exploit natural groups, with findings that the race, ethnicity, political party, and subjects' fields of study affect play in dictator games and strategic settings such as ultimatum games, prisoner's dilemma, public goods games, trust games, and redistribution (e.g., Fershtman & Gneezy (2001), Glaeser et al. (2000), Goette,

[5]For example, out-group members are often viewed as less than human (Leyens et al. (2001), Paladino et al. (2004)) and described in animalistic terms (Goff et al. (2008)).

Huffman & Meier (2006), Bernhard, Fehr & Fischbacher (2006), Klor & Shayo (2010)). A second approach creates social categories inside the laboratory, as in Chen & Li (2009)'s minimal group experiment on social preferences and other experiments using arbitrary groups as in Charness, Rigotti & Rustichini (2007), Chen & Chen (2011) and Hargreaves Heap & Zizzo (2009)). Research that uses both methods contrasts the effect of minimal groups and real-world groups on cooperative behavior and social preferences (Goette, Huffman & Meier (2012), Kranton et al. (2020)).

The present experiment shares the structure of social psychology studies which seek to uncover the causes of social divisions. The experimental treatments involve different forms of interaction, and the outcomes variables are measures of social relations. The classic (Sherif et al. (1954)) Robbers Cave study shows the effects of competition and then cooperation on social hostility. Two groups of boys developed strong animosity engaging in competitive games, and this animosity was only mitigated by subsequent engagement in a cooperative task. Brewer (1979) reviews the many subsequent lab experiments in social psychology on the effect of inter-group competition or cooperation on outcomes such as the likeability of people in the other group.[6]

This paper also shares the structure of recent economic field studies which place people in situations where they share common goals in order to reduce prejudice, as applications and tests of the contact hypothesis (Allport (1954)).[7] Lowe (2021) studies "adversarial" versus "collaborative" contact by randomly assigning participants in rural India to mixed caste or single caste cricket teams and opponents. Collaborative contact increased subsequent willingness to engage socially and engage in trade across castes.[8] In post-war Iraq, Mousa (2020)

[6]Another social psychology topic is the effect of competition or competition on self-evaluations and evaluations of others' competency and other such traits. See, for example, Ruscher & Fiske (1990) and Stapel & Koomen (2005).

[7]Bertrand & Duflo (2017) reviews economics field experiments on discrimination, including tests of elements of the contact hypothesis. We focus here on the studies of economic competition or cooperation. See Paluck, Green & Green (2019) for review of the social psychology and experimental work on the contact hypothesis.

[8]Lowe (2021) includes two incentive treatments - pay for individual performance vs. pay for team performance. These pay schemes map to the present study's competitive and cooperative pay schemes. However, unlike in the present study, in Lowe (2021) participants learn their pay and relative performance before social

finds, however, that playing together on soccer teams did not lead to more generalized longer-term "tolerance" between Muslims and Christians. In the present paper, the experimental treatments similarly involve cooperative vs. competitive settings. We posit purely economic settings and isolate the settings from confounding variables to identify the effects on social perceptions.

By testing whether people think of each other as having more or less in common or being more or less similar in different economic settings, this experiment expands the economics of motivated beliefs to the social realm. Research on motivated beliefs is rooted in early work in psychology that posits that beliefs, in addition to actions, serve important needs (Kunda (1987)); people derive direct benefits from believing they are able and moral people, their future is bright, and they made the right choices. Experiments in economics have recently advanced our understanding of how individuals form and maintain such comforting beliefs.[9] Only a few studies examine motivated beliefs of subjects involved in strategic or economic interactions with others. These studies typically demonstrate that subjects select information about whether or not their action will hurt others, in a way that make them feel moral while acting immorally.[10] The experiment we propose is quite different: We study whether subjects have a motivated social perception of themselves vis à vis their counterparts and ask if this social perception depends on the economic context. The supposition is that people feel worse when they compete against someone who is socially close and have the reverse sentiments when they cooperate. Thus, people adjust their perceptions of closeness accordingly. We discuss possible mechanisms behind these adjustments in the Conclusion.

outcomes are measured. The results indicate no marginal effect of the incentive schemes.

[9]For example, see Eil & Rao (2011) for the asymmetric use of negative and positive information about oneself, and see Zimmermann (2020) selective memory of feedback on own performance. See Bénabou (2015) for a review of the motivated belief literature.

[10]Work in this area includes Grossman (2014), Grossman & Van der Weele (2017), Serra-Garcia & Szech (2021), Chen & Heese (2021) and Exley & Kessler (2021). In Di Tella et al. (2015), subjects form distorted beliefs about others' altruism in order to justify own giving levels. In Oprea & Yuksel (2021), subjects form beliefs about themselves using their counterparts' beliefs in self-serving ways. See Gino, Norton & Weber (2016) for a survey.

2 Experimental Design

This experiment, which aims to causally identify the impact of economic settings on social perceptions, consists of three parts. At the beginning of the experiment, subjects see summaries of each part, and tasks are described in full as the experiment progresses.[11]

2.1 The Three Parts of the Experiment

Part 1 - Study Questionnaire

Subjects first complete a Study Questionnaire about themselves. The Questionnaire consists of four questions: gender (male, female, or non-binary), political party leanings (Democrat or Republican), married or in a domestic partnership (yes or no), preference for a season (fall or spring). (As described below in Section 2.2, we designed the Questionnaire to elicit as much differentiating information as possible in a few questions). Each subject is presented these questions in one of ten orders, randomly selected.

Part 2 - Work Setting and Counting Task

Subjects are told they will participate in a work setting with another person, *Person A*, to whom they have been randomly matched. Person A is described (truthfully) as a real person who participated in a previous study. Subjects then see a description of the work, which is to count the number of ones in 9 x 9 tables of randomly-ordered zeros and ones. This real-effort task, borrowed from Abeler et al. (2011), requires no prior knowledge, is known to be tedious, and offers little learning possibilities. Subjects' performance is the number of tables for which they report the correct number of ones. Subjects are told (again truthfully) that Person A completed the exact same task previously and that A's performance was recorded.

Subjects are then randomly assigned either to the *Competition* or to the *Cooperation* treatment and informed of the corresponding payment scheme.[12] In *Competition*, subjects

[11] The Online Appendix provides the full instructions.

[12] While we use the words "Competition" and "Cooperation" to describe the settings, the subjects do not see such wording. They just see a verbal description of the pay scheme and an accompanying illustrative gif.

earn bonus money based on their performance only if they perform better than Person A; otherwise, it is Person A who earns money based on Person A's performance. In particular, if the subject outperforms Person A, the subject earns \$0.40 bonus pay for each correctly counted table and Person A earns nothing. In *Cooperation*, the subject and Person A both earn money based on their combined performance. Precisely, both individuals earn \$0.10 times the sum of their correctly counted tables.[13]

Just before starting the real-effort task, subjects see their counterpart's (Person A's) answers to the Study Questionnaire, presented in the same order as the subject answered the questions. Person A's answers are displayed for ten seconds and then the subject advances automatically to the next screen to begin the counting task. This key aspect of our design ensures that when performing the task, subjects know how the task is rewarded and "whom" they are cooperating with or competing against. Competition or cooperation then has the possibility to alter the perception of the counterpart. The subjects have three minutes to do the counting task and when three minutes have elapsed, the screen advances. Subjects are told they will learn their bonus money at the end of the study. Hence, relative performance and earnings cannot affect subjects' responses in Part 3, described next.

Part 3 - Questions about Person A and Yourself

In the third part of the experiment, subjects are asked two social perception questions. To check for order effects, one of the questions is asked first in each of two experimental sessions. We refer to question (i) as the *similarity* question and to question (ii) as the *commonality* question.

(i) How similar are you to Person A?

Possible answers: Not similar at all, Not similar, Neutral, Similar, Very similar.

(ii) You answered the Study Questionnaire at the beginning of the survey. How many

[13]The subject's expected monetary payoff is the same in each treatment. However, the marginal return to a correctly counted table on own bonus pay is \$0.20 in expectation in *Competition* and \$0.10 in *Cooperation*. In *Cooperation*, each of the subject's correctly counted table also gives \$0.10 to Person A. As we discuss below, we find no difference in performance between the two treatments.

answers do you have in common with Person A? You will earn a bonus of \$0.10 if you are exactly correct, \$0.05 if you are within 1 of the correct number and \$0 if you are 2 or more outside the correct number.[14]

Pull-down menu of possible answers: 4, 3, 2, 1, 0.

After answering these questions, subjects advance to a next page and answer some demographic questions about themselves (e.g, state of residence, educational attainment) and questions concerning preferences towards teamwork and competition.[15] On the last screen, subjects are informed of their bonus payments.

Figure 1 provides the timeline of the experiment and summarizes the three successive parts.

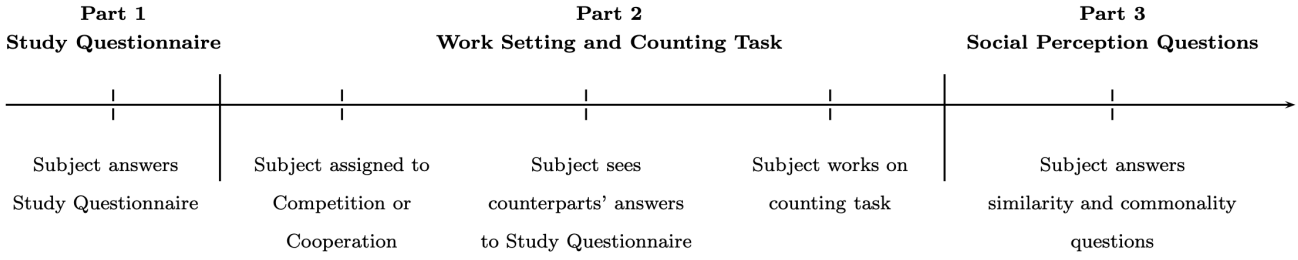


Figure 1: Timeline and Summary of Three Parts of Experiment

Our main hypothesis is that the assigned economic setting in Part 2 affects subjects' answers to the social perception questions in Part 3. Specifically, we hypothesize that subjects assigned to *Competition* report having less in common with and being less similar to their counterparts than do subjects assigned to *Cooperation*, all else equal.[16]

[14]See Charness, Gneezy & Rasocha (2021) for a discussion of the advantages of such simple incentive schemes.

[15]Subjects answer how much they agree with the following statements: "I am a competitive person," "I like to work in teams."

[16]In the pre-registration document, we anticipated that similarity, being a more subjective measure, would be more affected by the treatments. We find the opposite result, as detailed below.

2.2 Design of Study Questionnaire

We designed the Study Questionnaire to be a personal survey which distinguishes individual subjects in terms of demographics and preferences. We sought demographics and preferences which are not highly correlated, so that each answer provides new information about the individual. We also sought a short, four-item questionnaire so that subjects could, in principle, easily remember their own survey answers as well as those or their counterparts.[17]

Building on methods in Lee et al. (2021), we developed the Study Questionnaire by recruiting five hundred subjects on Prolific to answer 50 questions about themselves. The questions concerned demographics such as gender, age above or below 30 years, parental status, martial status, as well as political leanings and preferences about seasons, food, art, computing equipment, and vacation destinations. The answers to all questions were binary except for the question concerning gender. The participants were told there are no right or wrong answers to any question and were paid $1.00 for completing the survey.

We used factor analysis, along with correlations of the answers to the questions, to choose four items for the Study Questionnaire.[18] Principal factor extraction for binary response data yielded eleven ordered factors along with the factor loadings which indicate the extent to which each question is associated with a given factor. Given observed gender differences in preferences for competition and preferences for gender of counterparts in competitive settings (Niederle & Vesterlund (2011), Datta Gupta, Poulsen & Villeval (2013)), we selected gender to be one of the survey questions (gender was the highest loading question of the sixth factor). We then selected marital status, political party leaning, and preferred season by considering the highest loading questions of the top four factors which also had low correlations to each

[17] According to the early memory literature in psychology, seven is the average number of items that individuals can remember in the short run, plus or minus two depending on the individual (Miller (1956)). Research now indicates that this number also depends on the items themselves (length, complexity, etc.), but a consensus has emerged around three or four simple objects that can be visually memorized in the short term (see Luck & Vogel (1997) and Machizawa, Goh & Driver (2012) for discussions).

[18] The Online Appendix provides details. In this analysis and in all analyses of the experimental data, we pool non-binary subjects with males. There is no difference in the results if non-binary subjects are pooled with females.

other.

2.3 Counterparts

Before implementing the main study, we recruited on Prolific two hundred people to serve as subjects' counterparts. These participants answered the Study Questionnaire and completed the same counting task used in the experiment. Participants were paid $1.00 for completion and earned bonus pay of $0.20 per correctly counted table (which yields the same expected bonus pay as for the subjects in the main experiment). Participants were then invited to possibly be passive participants in future studies in which they could earn additional bonus pay; 198 participants agreed and were included as counterparts for the main experiment.

2.4 Implementation

The experiment was run on Prolific with 2000 participants restricted to the United States. The recruitment was filtered to ensure that about half the subjects were male and about half female. Subjects were told they would receive a fixed payment for completion of $1.00 and possible bonus payments. The average payment (fixed and bonus) actually received by subjects was $2.15 (s.d. 0.97), which corresponded to the going rate on the Prolific platform at the time. Randomization into the two treatments, *Competition* and *Cooperation*, occurred at the participant level. The experiment was conducted in two sessions in December 2021 and January 2022 which were identical except for the order of the two social perception questions.

3 Results

Our objective is to test whether the economic treatments affect subjects' social perceptions. To do so, we establish first that the randomizations (of subjects to treatments and of counterparts to subjects) successfully yielded two balanced subject pools in terms of characteristics,

performance on the counting task, and true number of common answers to the Study Questionnaire. We also demonstrate the consistency of subjects' responses to the social perception questions. Unless stated otherwise, reported p-values are obtained from t-tests.

3.1 Balance of Characteristics, Performance, True Commonality

Out of 2000 participants, 996 were assigned to the *Competition* treatment and 1004 to the *Cooperation* treatment. The frequencies of characteristics and preferences are virtually identical:[19] Subjects in each treatment are almost evenly divided between males and females (and about 2.45% non-binary), about three-quarters prefer the Democratic party, about two-thirds prefer the fall to the spring, and marital status is divided almost evenly between yes and no responses. A range of ages (from 18 to 81, with an average at 33.63) and levels of education (considering four levels from less than high school to doctoral degree) were represented with no significant difference between the treatments. Subjects took the same amount of time to complete the study, about 7.36 minutes in both treatments ($p = 0.893$).

As for performance, the average number of correctly counted tables was 5.04 in the *Competition* treatment and 4.97 in the *Cooperation* treatment, which is not different ($p = 0.301$). In both treatments, a large share of subjects counted ones correctly in 4, 5, 6 or 7 tables (69.8% of subjects in *Competition* and 71.3% of subjects in *Cooperation*, which are not different proportions; $p = 0.481$). The Kolomogorov-Smirnov test comparing the distributions of performance in the two treatments gives a p-value of 0.100.[20]

The random matching of subjects and counterparts generated similar distributions of common answers to the Survey Questionnaire in the two treatments. Any subject-counterpart pair could have 0, 1, 2, 3 or 4 answers in common. On average, the true number of common answers is 2.141 (s.e. 0.032) in the *Competition* treatment and 2.163 (s.e. 0.031) in the *Cooperation*

[19] Table A.1. in the Online Appendix provides details on the subjects' characteristics in each pool, showing no significant difference between the pools.

[20] Performance significantly declines in age of subject, by about 0.037 (s.e. 0.004) correct table per year, with no treatment effect on this decline. In both treatments, performance significantly increases with education, by about 0.086 (s.e. 0.048) correctly counted table per level.

treatment, which is not significantly different ($p = 0.610$). There is no significant difference in the frequencies of 1, 2, 3 or 4 common answers between the two treatments ($p > 0.356$ in all cases) but 0 common answers is slightly more frequent in the *Competition* than in the *Cooperation* treatment (5.32% and 3.78%, $p = 0.099$).[21] By Kolmogorov-Smirnov test, we cannot reject that the frequency distributions of the true number of common answers are the same ($p = 1.00$).

3.2 Consistency of Social Perception Responses

We find robust consistency in subjects' answers to the social perception questions. As stated above, the report of common answers ranges from 0 to 4. We code the answers to the similarity question from 1 for *Not similar at all* to 5 for *Very similar*. Conditional on the true number of common answers, the similarity measure and the reported number of common answers exhibit a significant positive correlation ($p < 0.005$ for every possible true number of common answers). Subjects' reports of similarity and commonality are also each significantly increasing in the true number of common answers. Table 1 below presents the estimated linear regressions for the reported number of common answers and stated similarity. Specifications (1) and (6) consider the true number of common answers, (Comm_Ans), as an explanatory variable and show this number positively affects similarity and commonality.[22]

The two outcome variables are, however, different in content and nature, and are treated differently by subjects. The similarity question captures a broad assessment of closeness. The commonality question is more precise and the answer is incentivized in a simple way. In the experiment, subjects answer the similarity question much more quickly than the commonality question, 5.95 seconds on average (s.e. 0.186) versus 20.65 seconds on average (s.e. 0.291).

[21] Across the whole experiment, the frequencies of 0, 1, 2, 3, and 4 common answers are 4.55%, 21.05%, 37.65%, 28.15% and 8.60% respectively.

[22] Decomposing the true number of common answers into four dummy variables taking value 1 if the subjects and their counterparts have the same gender, same marital status, same season preference, or same political party leanings confirms this relationship. Each significantly increases the reported number of common answers and the stated similarity, with common political leanings having the largest effect. Table A.2. in the Online Appendix provides this regression analysis.

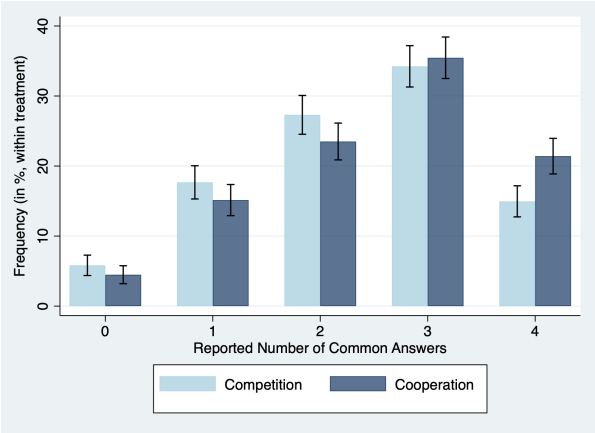
The difference is highly significant ($p < 0.001$) and independent of which question is asked first, suggesting that subjects attend more to reporting the number of common answers than to stating similarity.

3.3 Treatment Effects on Social Perceptions

We find subjects' reports of commonality are significantly lower in the *Competition* treatment than in the *Cooperation* treatment. While the true number of common answers is the same on average in the two treatments (established above in Section 3.1), the average reported number is 2.54 (s.e. 0.035) in *Cooperation* and only 2.35 (s.e. 0.035) in *Competition*. The difference 0.19 is highly significant ($p < 0.001$). Figure 2 shows the effect of the treatment on the frequency distributions of subjects' reports of the number of common answers. The frequency with which subjects report 0, 1, and 2 common answers is higher in *Competition* and the frequency which with subjects report 3 and 4 common answers is lower. The differences in frequencies for particular responses are statistically significant for 2 common answers ($p = 0.051$) and for 4 common answers ($p < 0.001$). By the Kolmogorov–Smirnov test, we can reject that the frequency distributions of reports of commonality question are the same in *Cooperation* and *Competition* ($p = 0.006$).

The gap between the reported and true number of common answers on the Study Questionnaire provides another window on this treatment effect on commonality. Combining the data from both treatments, overall subjects overstate the number of common answers; the average gap is 0.293 (s.e. 0.024) which is positive and significant ($p < 0.001$).[23] However, the overstatement of commonality is significantly lower in the *Competition* treatment than in the *Cooperation* treatment; the average gap is 0.38 (s.e. 0.035) items in the *Cooperation* setting and only 0.21 (s.e. 0.032) items in the *Competition* setting, a significant difference of -0.17

[23]Of the 2000 matches, the mean true number of common answers is 2.152 (s.e. 0.022), while the mean reported number of common answer is 2.445 (s.e. 0.025). Reported numbers are greater than the true number in 30.20% of the matches, correct in 54.25%, and lower in 15.55%. This pattern holds in both treatments; when subjects' reports are not correct, subjects more often overstate than understate commonality.



Note: Solid lines show 95% confidence intervals.

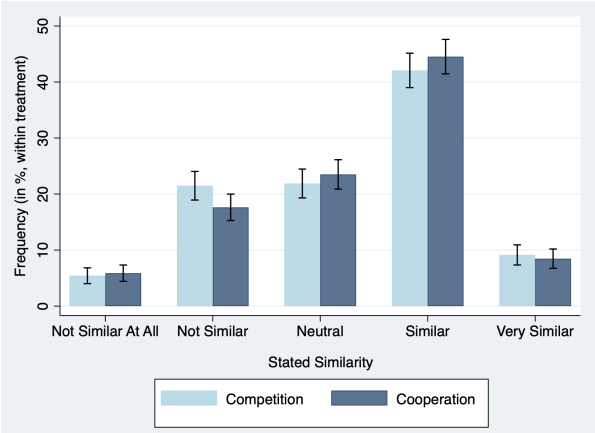
Figure 2: Frequency Distribution of the Reported Number of Common Answers

items ($p < 0.001$). The frequencies of gaps of -2, -1, and 0 common answers are higher in the *Competition* treatment and the frequencies of gaps of 1, 2, 3, and 4 are lower.[24] In sum, the frequency of answers that correspond to overstatements of commonality (gaps of 1, 2, 3 and 4) is significantly lower in *Competition* (26.51%) than in *Competition* (33.86%, $p < 0.001$). By the Kolmogorov–Smirnov test, we can reject that the frequency distributions of gaps in answers are the same in *Cooperation* and *Competition* ($p = 0.009$).

In contrast to commonality, the treatments have no clear effect on the answers to the similarity question. The mean similarity in the *Competition* treatment is 3.280 (s.e. 0.034) and 3.321 (s.e. 0.033) in the *Cooperation* treatment ($p = 0.390$). Figure 3 displays the frequency distributions of similarity answers in the two treatments. While the frequencies of the most common answers, *Similar* and *Neutral*, are not different in the two treatments, the frequency of *Not similar* is higher in *Competition* than in *Cooperation* (21.49% and 17.63%, $p = 0.0297$).[25] However, we cannot reject that the frequency distributions of answers to the

[24]Only 0.55% of subjects reported common answers with a gap of -4 or -3 and 0.8% with a gap of 4. See Figure A.1. in the Online Appendix for details.

[25]The Likert Scale can induce various response biases, including social desirability and acquiescence that could push subjects to the mid-range answers (Kreitchmann et al. (2019)) Over the whole experiment, the most common answers are *Similar*, chosen by 43.30% of the subjects, *Neutral*, chosen by 22.70% of the subjects and *Not similar*, chosen by 19.55% of the subjects. The extreme answers *Not similar at all* and *Very similar* are chosen by only 14.45% of the subjects.



Note: Solid lines show 95% confidence intervals.

Figure 3: Frequency Distribution of the Answers to the Similarity Question

similarity question are the same ($p = 0.609$ for the Kolmogorov-Sirnov test).

Table 1 presents regression analyses which confirm the above results. Controlling for the actual number of common answers (Comm_Ans), the *Cooperation* treatment (Coop) has a significant effect on the reported number of common answers (specification (2)) but not on answers to the similarity question (specification (7)). Specifications (3) and (8) break down the actual number of common answers into controls for common answers to each of the four Study Questionnaire items, showing no change in the treatment effect coefficient.

Specifications (4) and (9) introduce question order and demographics. The treatment effect on commonality, and the lack thereof on similarity, are robust to the inclusion of controls for whether the commonality question was asked first (Com_Before) and subject demographics. Specification (4) shows that the reported number of common answers is significantly higher when the commonality question was asked first. However, interacting the treatment dummy with question order (Coop*Com_Before) shows no effect of order on the treatment effect.[26]

[26]Specification (2) restricted to subjects in the first session – the commonality question was asked first – yields an estimated coefficient of Coop of 0.181 (s.e. 0.063). Specification (2) restricted to subjects in the second session – the similarity question was asked first – yields an estimated coefficient of Coop of 0.177 (s.e. 0.058). Further investigation, reported in Table A.3. of the Online Appendix, shows that Com_Before has a significant positive impact on the reported number of common answers for males only, in both treatments. One possible explanation is that, as discussed below, males have a general tendency to feel less similar to

	Report_Common					Similarity				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Coop		0.181***	0.184***	0.188***	0.201***		0.025	0.037	0.032	0.093
		(0.043)	(0.043)	(0.061)	(0.075)		(0.036)	(0.034)	(0.051)	(0.063)
Common_Ans	0.563***	0.562***		0.554***	0.554***	0.678***	0.678***		0.656***	0.655***
	(0.022)	(0.022)		(0.023)	(0.023)	(0.018)	(0.018)		(0.019)	(0.019)
Same_Gender			0.509***					0.609***		
			(0.043)					(0.035)		
Same_Married			0.528***					0.506***		
			(0.043)					(0.034)		
Same_Season			0.525***					0.505***		
			(0.043)					(0.034)		
Same_PolParty			0.703***					1.157***		
			(0.045)					(0.036)		
Com_Before				0.124**	0.123**				0.004	0.003
				(0.061)	(0.061)				(0.051)	(0.051)
Coop*Com_Before				-0.004	-0.004				-0.009	-0.009
				(0.086)	(0.086)				(0.072)	(0.072)
Male					0.049					0.038
					(0.062)					(0.052)
Coop*Male					-0.026					-0.120*
					(0.086)					(0.072)
N	2000	2000	2000	2000	2000	2000	2000	2000		

Note: The Table reports OLS coefficients (standard errors in parenthesis). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The regressions include a constant. Demographics include age, education (coded using four levels ranging from high school to doctoral degree), answers to the Study Questionnaire (except for gender in (5) and (10)) and subjects' answers to the questions on competitiveness and working in teams (coded as 0 or 1).

Table 1: Regression Analysis for the Social Perception Questions – Specifications (1) to (5) for Commonality and Specifications (6) to (10) for Similarity

3.4 Gender Effects

Given observed differences between women and men's preferences for participating in competition and preferences for gender of counterparts in competitive settings (see, e.g., Niederle & Vesterlund (2011), Datta Gupta, Poulsen & Villeval (2013), Saccardo, Pietrasz & Gneezy (2018)), we test for gender differences in social perceptions in the two economic contexts. First, we note that both males and females overstate the number of common answers compared to the truth and have a tendency towards stating they are similar to counterparts. At the same time, males show greater upward bias in the reported numbers of common answers and women make higher assessments of similarity. In particular, 33.57% of males versus 27.10% of females over-

counterparts than women. The similarity question—when asked first—then depresses males' responses to the commonality question.

state commonality ($p = 0.002$), 10.68% of females versus 6.76% of males answer *Very Similar* ($p = 0.002$), and 24.46% of males versus 20.94% of females answer *Neutral* ($p = 0.064$).

Table 1 tests whether gender interacts with the treatments in subjects' responses to the social perception questions. Specifications (5) and (10) interact the treatment dummy with gender (Coop*Male), controlling for demographics. The regression indicates that gender plays no role in the effect of the treatments on commonality. However, relative to males, female subjects state slightly but significantly more similarity to counterparts in *Cooperation* than in *Competition*.[27] With the weakness of the effect and no effect on commonality, this outcome is possibly more an artifact of how men and women answer the questions than indicative of a gender difference in how the treatments impact social perceptions overall. Finally, we find no difference between men and women's social perceptions for same gender matches within each treatment. In the *Cooperation* treatment, the mean reported number of common answers for females in female-female matches (223 observations) is 2.85 with 2.71 for males in male-male matches (242 observations) ($p = 0.127$); in the *Competition* treatment, the mean reported number of common answers for females in female-female matches (220 observations) is 2.66 with 2.69 for males in male-male matches (229 observations) ($p = 0.779$).

4 Conclusion

This paper tests whether the economic context affects people's social evaluations of others. In the experiment, subjects are given true information about counterparts' preferences and demographics. In a between-subjects design, subjects report how similar they are and how much they have in common with counterparts in (a) a competitive setting where either the subjects or counterparts earn bonus pay depending on who has the highest performance in a counting task or (b) a cooperative setting where both subjects and their counterparts earn

[27] Regressions that split the sample into male and female subjects, reported in Table A.3. of the Online Appendix, confirm that the treatments have no effect on males' stated similarity, but that female's stated similarity is slightly but significantly higher in *Cooperation* than in *Competition*.

bonus pay based on the sum of their performance in the task. Subjects' reports of similarity to their counterparts do not differ between the two settings. Subjects' reports of commonality are significantly lower in the competitive setting despite monetary incentives to accurately report the number of common preferences and demographic traits.

The paper advances and connects two areas of experimental economics—social divisions and motivated beliefs—and speaks to the larger questions of economic interactions and social difference. As elaborated in the Introduction, a growing body of economic experiments demonstrate that dividing people into groups based on preferences and demographics can affect strategic play and allocation of income. The present paper demonstrates the reverse causality: economic settings can lead subjects to view their counterparts as more or less the same or different. An emerging literature on motivated beliefs shows that when constructing views of the world, individuals trade-off the need to be accurate and the need to feel good about themselves and what they do or plan to do. Our study indicates that such a trade-off might be at play when individuals think about their relationships to others. People want to feel socially distant from those with whom they compete and socially close to those with whom they cooperate.

Our study raises several possible mechanisms for such motivated beliefs. People could manipulate their social perceptions for affective and instrumental reasons.[28] The competitive pay scheme in-and-of-itself leads to inequality in payoffs between subjects and their counterparts. To the extent that people are more inequality averse towards people with whom they are socially close (Chen & Li (2009)), reducing social closeness in the competitive setting reduces the utility loss from inequality aversion. The same reasoning would hold for guilt aversion. Increased social distance can also serve to motivate performance; a person who feels socially distant from other people could have greater motivation to compete against them for pay, or if close, to cooperate. Our experiment, where the profile of the counterpart is presented

[28] Adopting the terminology of Bénabou (2015), beliefs have an *affective* value when people feel better and an *instrumental* value when people perform better.

before the subject performs the real-effort task, is consistent with both possibilities. Future research could distinguish between these mechanisms by changing the experimental timeline. Future research could also examine other economic settings, such as private vs. public goods provision and positive vs. negative externalities, which could affect social perceptions.

With its focus on economic settings and motivated social perceptions, the present paper introduces a new dimension to the study of social difference and conflict. Our experiment indicates that people possibly process and report social information about self and others differently, depending on how they interact in the economic realm. This biased processing could contribute to why historical patterns of prejudice and violence have economic roots (e.g. Becker & Pascali (2019)) as well as why returns from the economic success of others can mitigate conflict (e.g. Jha & Shayo (2019)). Further research could range from such societal patterns to the neural foundations of motivated social perceptions, building on findings, for example, that brain regions associated with social rewards are activated while engaging in a cooperative task but not a competitive task (Decety et al. (2004)). These investigations could identify the larger impact of economic settings, which could not just entail pecuniary incentives for people to work for or against each other but could shape their social world.

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