Competition, Cooperation, and Social Perceptions

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Abstract Can competition or cooperation for economic gain affect people's social perceptions of others? This paper experimentally examines this possible link from the economic to the social realm. Subjects engage in a task facing either a tournament or a cooperative pay scheme, after which subjects are asked their social perceptions of their counterparts in the task – how similar they are and how much they have in common. The pay schemes do not affect answers to the subjective similarity measure but significantly influence subjects' reports of commonality. Subjects who compete with counterparts for pay report fewer traits in common with their counterparts than do subjects facing the cooperative scheme. This treatment effect emerges even though our novel measure of commonality provides incentives to report accurately the number of common traits.

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1 Introduction

Can people's social perceptions of others depend on whether they compete or cooperate with them for economic gain? This paper proposes a simple experiment to consider this possible connection between economic and social realms. The experiment exogenously varies how subjects and counterparts earn payoffs and then examines subjects' social perceptions of their counterparts. Specifically, we consider subjects working at a real-effort task who are assigned to either a tournament pay scheme or a cooperative pay scheme. We then ask subjects assigned to each scheme whether they perceive counterparts as more or less similar to themselves and as having more or less in common with them.

The findings provide proof of concept that competition or cooperation for economic gain can, in and of itself, influence interpersonal perceptions. While the pay schemes do not affect subjects' answers to the broad measure of similarity to counterparts, we find a significant effect on the measure of commonality. Precisely, subjects who compete against counterparts for pay report having fewer traits in common with them than subjects who cooperate with counterparts for pay. Our study thus contributes to the growing body of research examining the links between the social and economic relations. Much of this research - theoretical, experimental and empirical - investigates how social differences and divisions shape the choices and outcomes of a wide range of economic settings (see e.g., Akerlof & Kranton, 2010, Alesina & Ferrara, 2005, Charness & Chen, 2020). In contrast, this paper reverses the causal direction by examining whether the nature of the interaction for economic gain can affect social views and finds this direction is indeed operative.

The experimental design insulates the effect of the pay schemes on social perceptions from the possible effects of other outcomes, such as relative performance or earnings. The experiment consists of three parts. First, each subject answers a Study Questionnaire which we developed and 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. The counterparts performed the same real-effort task in an earlier experimental session and their performances were recorded. Subjects are randomly assigned to either (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. After learning the pay scheme, subjects see their counterparts' responses to the Study Questionnaire. The subjects then work on the real-effort task for three minutes. Third, subjects provide their views of their counterpart by answering the two questions detailed below. The subjects answer these questions before knowing their relative performance or bonus payments, so subjects' answers can only depend on the responses to the Study Questionnaire and on the treatment.

The third part of the experiment generates two measures of social perceptions, our main experimental outcomes: (1) *Commonality:* subjects are asked to report the number of answers to the Study Questionnaire that they have in common with their counterpart and receive monetary incentives for accuracy. This technique is a new, incentivized measure of social proximity between subjects and their counterparts; it can be used in any setting in which subjects and counterparts answer the same questionnaire.[1] (2) *Similarity:* subjects are asked to assess their similarity to the counterpart on a 5-item Likert scale ranging from *Very Similar* to *Not Similar at all*.

In the pre-registration we hypothesized that subjects in *Competition* would report having less in common with and say they are less similar to counterparts than would subjects in *Co-operation*. The basic premise behind this hypothesis is that people could feel worse when they compete for pay against someone who is socially close and have the reverse sentiments when they cooperate. People would then adjust their perceptions towards less proximity to competitors and more proximity to co-workers. We also hypothesized a smaller treatment effect on commonality which is a more objective measure of social perceptions. The commonality

[1] Andries et al. (2024) use a analogous measure in a field study and find that people are more empathetic towards others with whom they perceive to share more personality traits.

question asks for a count of the number of traits shared with counterparts. Each trait (e.g. be it preferred season or political leaning) has the same impact on this count and accuracy of subject's responses was incentivized. The similarity question, in contrast, is a subjective measure with no specification of which traits are important to the answer and no incentives can be attached.

We find that subjects' reports of traits in common are significantly lower in the *Competition* treatment than in the *Cooperation* treatment. This treatment effect emerges even though subjects have monetary incentives to report accurately the number of common traits. The effect is robust to controls for the true number of common answers, the order of the social perception questions, and demographic characteristics and is the same magnitude for male and female subjects. We find further that the average gap between subject's reports and the true number of common answers is lower in *Competition* than in *Cooperation*, and gaps in *Cooperation* are biased in the direction of more commonality. Following the criteria of Bénabou & Tirole (2016), this directionality suggests a motivated process, rather than mechanical process such as enhanced attention in *Competition*, behind the treatment effect on commonality.

We find that treatments do not significant impact subjects' subjective assessments of similarity to counterparts. The assessments are dominated by shared political leanings more than by any other factor including cooperation or competition for pay. The similarity question is thus subjective in a way we had not anticipated, and the salience of political identity is a possible explanation for why the treatments do not affect the similarity assessments. Other explanations for the lack of treatment effect for similarity but a significant effect for commonality include inherent differences between the subjective and objective questions, how subjects treat these questions, as well as the possibility that being similar and having common traits are related but different social constructs. We discuss these possibilities further below.

The treatment effect on commonality in *Competition* relative to *Cooperation* raises further questions which we tackle in two supplementary experiments. First, we ask whether *Competition* and *Cooperation* are each operative in this relative difference in social perceptions.

To do so, we implement a benchmark treatment which we call *Piece-Rate*, in which the subjects' pay does not depend on the counterpart's performance. Subjects complete the Study Questionnaire, are introduced to a counterpart and are presented the counterpart's traits, but subjects receive a fixed rate for their work in the real-effort task. As in the main experiment, subjects answer the commonality and similarity questions after they completed the task. We find that the average commonality in the *Piece-Rate* control is significantly higher than in the *Competition* treatment and lower, but not significantly so, than in the *Cooperation* treatment. The average reports of similarity are again dominated by political leanings, with no difference between assessments across *Competition*, *Cooperation*, and *Piece-Rate*.

Second, we ask whether competing or cooperating for pay affects subjects' reports of common features of non-human ensembles and therefore whether cooperation vs. competition affects subjects' comparisons of items unrelated to themselves and their counterparts. For shorthand, we call these reports *non-social perceptions*. To do so, we implement the same design as in the main experiment except that the subjects' and counterparts answers to the Study Questionnaire are replaced by two lists each consisting of four randomly-drawn features describing natural scenes (see Khaw, Kranton & Huettel (2021), for use of natural scenes). Subjects either compete or cooperate for pay with a randomly-selected counterpart, and then subjects report the number of common features of the two scenes with the same incentives for accurate reports as in the main experiment. We find no treatment effect on the reports nor on the gaps between the reports and the true number of common features. This result further supports the interpretation that enhanced attention or other mechanical processes are not driving the treatment effect on commonality in the main study.

The paper's investigation and results embark in a new direction in the study of the links between social and economic realms and advance the study of motivated beliefs to the domain of social perceptions. The economics of identity provides a general framework to study how people's different conceptions of who they are shape economic choices and interactions (Akerlof & Kranton, 2000). Social divisions, such as ethnic or racial cleavages, have been shown to

relate directly to lower public goods provision, deforestation, and slower economic growth (see, respectively, Alesina, Baqir & Easterly, 1999 and Miguel & Gugerty, 2005; Alesina, Gennaioli & Lovo, 2019; Easterly & Levine, 1997). Economic experiments share the structure of classic experiments in social psychology which consider the impact of group divisions on choices such as reward allocations (e.g., Tajfel et al., 1971). The treatments invoke real-world groupings or create distinctions in the lab through answers to survey questions and lead to biased behavior in allocation tasks and strategic play.[2] Relative to this large body of literature, the present paper is a study of causes of social differences rather than consequences.

The present study thus shares the structure of social psychology experiments on the sources of inter-group animosities and more recent work in economics on interventions to mitigate antagonism between groups. The Sherif et al. (1954) classic study investigates competition and cooperation as the cause of social views; sporting competitions between two groups of boys at a summer camp created intense antipathies which were later mitigated by cooperative activities.[3] Recent field experiments consider the contact hypothesis (Allport, 1954), asking whether cooperative settings can effectively reduce discrimination (e.g. Lowe, 2021; Mousa, 2020).[4] The present paper considers economic gains and isolates the effect of competing or cooperating for these gains on social perceptions.

The present experiment expands the economics of motivated beliefs to the social domain.

[2]For treatments which exploit real-world identifiers such as race, ethnicity, and subjects' fields of study, see Fershtman & Gneezy (2001), Glaeser et al. (2000), Goette, Huffman & Meier (2006), Bernhard, Fehr & Fischbacher (2006), Klor & Shayo (2010)). For treatments which employ the minimal group paradigm to create distinctions in the lab, see Chen & Li (2009), Charness, Rigotti & Rustichini (2007), Chen & Chen (2011) and Hargreaves Heap & Zizzo (2009). Studies also contrast subjects' choices when divided into minimal groups versus real-world groups (e.g., Goette, Huffman & Meier, 2012; Kranton et al., 2020).

[3]In social psychology, see Brewer (1979) for review of subsequent lab experiments on the effect of inter-group competition or cooperation on outcomes such as the likeability of people in the other group and Ruscher & Fiske (1990) and Stapel & Koomen (2005) for the effect of competition or competition on self-evaluations and evaluations of others' competency and other such traits.

[4]In rural India, Lowe (2021) studies mixed or single caste cricket matches, with pay for individual vs. team performance, which is learned before social outcomes are measured. The results indicate no marginal effect of the incentive schemes. Mousa (2020) finds no effect of playing together on soccer teams on longer-term 'tolerance" between Muslims and Christians in post-war Iraq. See Bertrand & Duflo (2017) for review of economics field experiments on discrimination including tests of elements of the contact hypothesis. Other studies of the contact hypothesis include Rao (2019) and Bursztyn et al. (2024) which do not consider the effect of cooperative vs. other types of interactions.

Research on motivated beliefs is rooted in work in psychology that posits that beliefs, in addition to actions, serve important needs (Kunda, 1987); people derive direct benefits from believing they are healthy, able and moral and sometimes trade-off these beliefs against more accurate views (see Bénabou & Tirole (2002) for a formalization of such a trade-off and Schwardmann (2019) or Zimmermann (2020) for experimental tests). 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.[5] Incorrect beliefs and motivated reasoning about own and others' capabilities are also a source of discrimination (e.g. Bohren, Imas & Rosenberg, 2019; Bordalo et al., 2019; Eyting, 2024). The experiment we propose is quite different: we study subjects' social perceptions of their counterparts vis à vis themselves and ask whether subjects adapt these perceptions to the way in which they earn economic gains.

2 Experimental Design

This experiment, which aims to causally identify the impact of competition or cooperation for economic gain on social perceptions, consists of three parts. Section 1 of the Online Appendix provides the full instructions for the main experiment. The experiment involves no deception.

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

[5]Work in this area includes Grossman (2014), Di Tella et al. (2015), Grossman & Van der Weele (2017), Serra-Garcia & Szech (2021), Chen & Heese (2021), Exley & Kessler (2021) and Oprea & Yuksel (2021).

or spring).[6] Each subject is presented these questions in one of ten orders, randomly selected.

We designed the Study Questionnaire to be a short, personal survey with each question providing different information about the individual. We sought a four-item survey so that subjects in principle could easily remember the answers to the questions.[7] Using principal factor analysis along with correlations between answers, we selected four questions from a set of 50 questions answered by 500 Prolific participants (details provided in Appendix A).

Part 2 - Competition versus Cooperation

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 this task previously and that A's performance was recorded.

Subjects are then randomly assigned either to the *Competition* or to the *Cooperation* treatment. Subjects see a verbal description of the corresponding pay scheme and an accompanying illustrative gif. In *Competition*, subjects earn bonus money based on their performances only if they perform better than Person A; otherwise, only Person A earns money based on A's performance. Precisely, whomever (the subject or Person A) has the highest number of correctly counted table earns \$0.40 bonus pay per table. In *Cooperation*, the subject and Person A both earn money based on their combined performance. Precisely, the subject and Person A each earn \$0.10 times the sum of their correctly counted tables. The pay schemes are designed

[6] In all analyses of the paper, for simplicity, we pool non-binary subjects with males, with no difference if non-binary subjects are pooled with females.

[7] While the number of items individuals can remember in the short run depends on the items themselves (length, complexity, etc.), a consensus has emerged in the psychology literature around three or four simple objects that can be memorized in the short term (Luck & Vogel, 1997; Machizawa, Goh & Driver, 2012).

so that a subject's expected earnings do not differ by treatment and therefore differences in expected earnings cannot be a confound to any treatment effect on the social perceptions. If the subject and Person A each have the same performance x in expectation (and they do as shown below), a subject in *Competition* earns $\frac{1}{2}(\$0.40) \cdot x$, and a subject in *Cooperation* earns $\$0.10 \cdot (2x)$.[8]

Just before starting the real-effort task, subjects see Person A's answers to the Study Questionnaire, presented in the same order as the subjects answered the questions in Part 1. These answers are displayed for ten seconds before the screen advances automatically to the counting task. Thus, when performing the task, subjects know how the task is rewarded and "whom" they are cooperating with or competing against. Subjects are told they have three minutes to do the counting task and will learn their bonus money at the very end of the study.

Part 3 - Questions about Person A and Yourself

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

(i) *How similar are you to Person A?*

Possible answers (5-items Likert scale): Not similar at all, Not similar, Neutral, Similar, Very similar.

(ii) *You answered the Study Questionnaire at the beginning of the survey. How many 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.* The average pay rate - about $0.30 per second - for the counting task and the report of common answers are similar.

Possible answers (pull-down menu): 4, 3, 2, 1, 0.

[8] By design, the overall expected surplus is also the same across treatments (0.40x$). Given the constraint of equalizing subjects' expected earnings and overall surplus across treatments, the marginal pay to the subject for each table counted is necessarily different ($0.20 per table in expectation in *Competition* and $0.10 in *Cooperation*). Nonetheless, performance is the same in the two treatments.

Finally, subjects answer demographic questions (e.g, age, educational attainment) and questions concerning preferences towards teamwork and competition, and, on the last screen, subjects are informed of their bonus payments. Figure 1 provides the timeline of the experiment and summarizes.

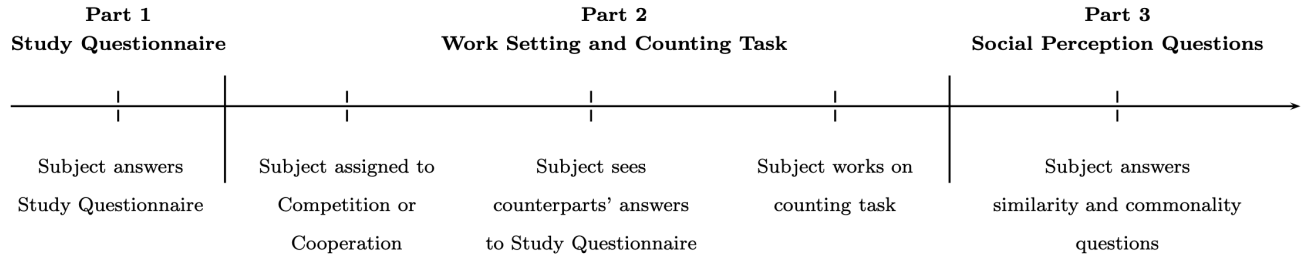


Figure 1: Timeline and Summary of Three Parts of Experiment

2.2 Main Hypotheses

We consider the following two pre-registered hypotheses. Details are given in Appendix B.

Hypothesis 1. Subjects assigned to *Competition* report having less in common with and being less similar to their counterparts than do subjects assigned to *Cooperation*.

Hypothesis 1 follows from the proposition stated in the Introduction: when people compete for pay, they may feel better when their competitor is not socially close, and the reverse when they cooperate for pay. Several theories and findings could justify this proposition. For example, the competitive pay scheme leads to unequal payments, and experimental findings indicate people are less inequality averse towards people in other groups (Chen & Li, 2009). People engaging in competition would then adjust their social perceptions to be less close to counterparts to lower the loss in utility from inequality. Similarly, the cooperative scheme implies providing payments to the counterpart, and research indicates people are more generous to socially closer individuals (Goeree et al., 2010). If individuals benefit more from payments to socially closer counterparts, they would adjust their perceptions to be closer to counterparts

when the interaction is cooperative. The Conclusion further discusses possible reasons why people would make such adjustments and the channels by which people could do so.

Hypothesis 2. The treatment effect is greater for subjects' assessments of similarity than for subjects' reports of commonality.

This hypothesis is based on the possibility that, when giving their views of counterparts, individuals trade-off possible returns from accuracy with the desire for a particular social distance in the given context. This possibility parallels a central idea in the motivated beliefs literature, that individuals form beliefs about themselves weighing confidence and the benefits of accuracy (Zimmermann, 2020). In our setting, we hypothesize that the trade-off between accurate and favored views of others is stronger for the commonality question, which is objective and involves monetary incentives for accuracy, than for the similarity question which is purely subjective.

2.3 Implementation

The experiment was run on Prolific (in December 2021 and January 2022) with 2000 participants restricted to the United States and filtered to ensure gender balance. 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 was $2.13 (s.e. 0.021), 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.

Before implementing the main study, we recruited on Prolific two hundred people to serve as counterparts. These participants answered the Study Questionnaire and completed the same counting task used in the main experiment. Participants were paid $1.00 for completion and earned bonus pay of $0.20 per correctly counted table. Participants were 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. The 2000 subjects of the main

experiment were randomly matched to these 198 passive participants, so different subjects could be matched to the same passive participant. The subjects were not given these details.

3 Main Results: Social Perceptions, Competition vs. Cooperation

Our objective is to test how *Competition* or *Cooperation* affect subjects' social perceptions of their counterparts. 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 and performance on the counting task. We also demonstrate the consistency of subjects' responses to the social perception questions. All p-values reported below are obtained from two-sided t-tests.

3.1 Balance of Characteristics, True Commonality, Performance

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: 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. Age range (from 18 to 81, with an average at 34.63) and education (coded as five 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 7.30 minutes in both treatments ($p = 0.834$). Tables A.1. and A.2. in the Appendix A provide details.

The random matching of subjects and counterparts generated similar distributions of common answers to the Study Questionnaire in the two treatments. 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* treatment ($p = 0.610$). 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%, $p = 0.099$). A Kolomogorov-Smirnov test confirms that the distributions of the number of common answers is not different in the two treatments ($p = 1.00$). There is also no significant difference in the frequencies with which subject-counterpart pairs share the same gender, the same political leaning or the same marital status ($p > 0.283$ in all cases) but slightly more pairs share a preference for the fall in *Cooperation* than in *Competition* (57.37% and 52.51%, $p = 0.029$).[9]

Task performance does not differ by treatment or by the number of common traits in the subject-counterpart pairs. The average number of correctly counted tables was 5.05 (s.e. 0.067) in the *Competition* treatment and 4.95 (s.e. 0.063) in the *Cooperation* treatment ($p = 0.301$). The Kolomogorov-Smirnov test comparing the distributions of performance gives a p-value of 0.111. Regressing subjects' performance on the number of common traits, controlling for the same demographics as in the tables of the main analysis, yields coefficients of 0.020 in the *Competition* and -0.062 in the Cooperation treatment ($p = 0.355$ and $p = 0.756$, resp.). Since by design the subjects do not learn their bonus pay until the end of the experiment, neither performance nor pay can affect subjects' answers to the social perception questions.[10]

3.2 Consistency and Overall Patterns of Social Perceptions

We find robust consistency in subjects' answers to the social perception questions. We code the answers to the similarity question from 1 for *Not similar at all* to 5 for *Very similar*. The reported number of common answers and the similarity assessments are each strongly

[9]Pooling the two treatments, the frequencies with which pairs share the same gender, political leaning, marital status and favorite season are 45.70%, 65.05%, 49.05%, and 54.95% respectively.

[10]While performance does not differ across treatments, there is a mechanical difference in final payments; subjects who are paid for the counting task in *Competition* have to outperform their counterparts. The average payment for that task is \$1.17 (s.e. 0.041) in *Competition* and \$0.96 (s.e. 0.009) ($p < 0.001$) in *Cooperation*.

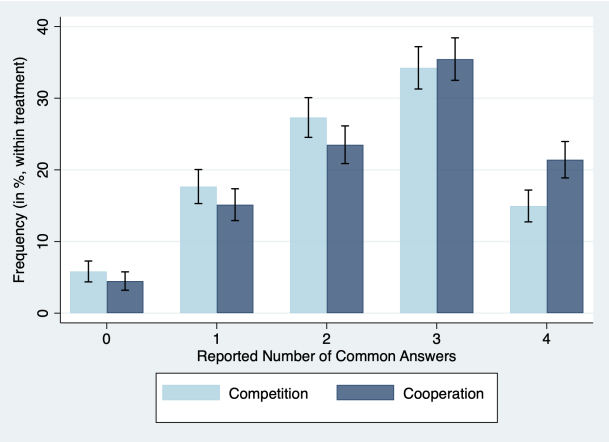
and significantly positively correlated with the true number of common answers (coefficients are 0.503 and 0.640, respectively, $p < 0.001$ for both). Conditional on the true number of common answers, the similarity measure and the reported number of common answers exhibit a significant positive correlation (at least 0.260 and $p < 0.004$ for every possible true number of common answers).

Overall, subjects overstate the number of common answers relative to the true number of common answers. The mean true number of common answers is 2.152 (s.e. 0.022), while the mean report is 2.444 (s.e. 0.025), and the average gap between subjects' reports and the true number of traits 0.291 (s.e. 0.024) which is positive and significant ($p < 0.001$). This tendency to overstate is also present in the *Piece-Rate* treatment and in the experiment on non-social perceptions and thus appears to be a constant artifact of our design and/or implementation.

3.3 Treatment Effect on Social Perceptions

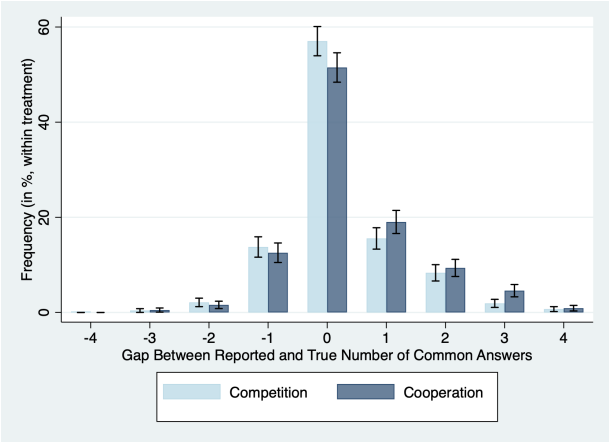
We find subjects' reports of commonality are significantly lower in the *Competition* treatment than in the *Cooperation* treatment. While the true number of common answers is the same on average in the two treatments (see Section 3.1), the average reported number is 2.54 (s.e. 0.035) in *Cooperation* 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: 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 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* ($p = 0.006$).[11]

[11] In addition, we note that within the *Competition* treatment there is no significant difference in the commonality reports between subjects who outperformed counterparts and subjects who did not. Recall that subjects learn they payoffs only after having answered social perception questions. Averages of reports are 2.36 and 2.33 ($p = 0.642$), respectively, and the comparison of distributions (K-S test) gives a p-value of 0.877.



Note: Solid lines show 95% confidence intervals.

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



Note: Solid lines show 95% confidence intervals.

Figure 3: Frequency Distributions of the Gap between Reported and True Number of Common Answers

The gap between the reported and true number of common answers on the Study Questionnaire provides another window on this treatment effect. The average gap is 0.38 (s.e. 0.035) items in *Cooperation* and only 0.21 (s.e. 0.032) items in *Competition*, a significant difference of -0.17 items ($p < 0.001$). On average, subjects give more accurate reports in *Competition* than in *Cooperation*. Figure 3 shows the distributions of the gap by treatment. By the Kolmogorov–Smirnov test, we can reject that the frequency distributions of gaps between reports and the true number of common answers are the same in *Cooperation* and *Competition* ($p = 0.009$). The frequency of reports that correspond to overstatements of commonality (gaps of 1, 2, 3 and 4) is significantly lower in *Competition* (26.41%) than in *Competition* (33.76%, $p < 0.001$), showing the inaccuracy in reports in *Cooperation* is biased upwards.

While the responses to the similarity question are in the same direction as the responses to the commonality question, the treatments do not have a significant effect. The mean similarity assessment in the *Competition* treatment is 3.280 (s.e. 0.034) and 3.320 (s.e. 0.033) in the *Cooperation* treatment ($p = 0.402$). The frequencies of the most common responses, *Similar* and *Neutral*, are not different in the two treatments but the frequency of *Not similar* is higher in *Competition* than in *Cooperation* (21.49% and 17.63%, $p = 0.030$). We cannot reject that

the frequency distributions of responses to the similarity question are the same ($p = 0.609$ for the Kolmogorov-Sirnov test).

As discussed in the section 2.2, in our pre-registration we hypothesized a treatment effect on both the commonality and similarity assessments (Hypothesis 1), with greater magnitude on similarity (Hypothesis 2). Our results validate Hypothesis 1 for the treatment effect on commonality but reject it for the treatment effect on similarity. It follows that Hypothesis 2 is rejected. We discuss below possible explanations for the difference between the effect of treatments on commonality and similarity.

Regression specifications (1)-(3) and (4)-(6) in Table 1 confirm the above results. Controlling for the actual number of common answers (*Comm_Ans*), the *Competition* treatment has a significant, negative effect on the reported number of common answers (specification (1)) but not on answers to the similarity question (specification (4)). Specifications (2) and (5) show no change in the treatment effect coefficient when breaking down the actual number of common answers into four dummy variables: same gender, same marital status, same season preference, same political leanings.

The impacts of each shared trait on the subjects' responses, seen in Specifications (2) and (5), show a fundamental difference between the commonality and similarity measures. The commonality question asks the number of shared traits and each of these traits figures equally into the bonus payment. We find that shared traits have about the same impacts on the reports, with the same political party having a slightly and significantly higher impact. In contrast, the similarity question asks for a general assessment and does not assign weights to the shared traits. In their assessments subjects appear to weigh heavily shared political leaning; the effect is more than two times higher than those of other shared traits.

These results and further comparisons suggest commonality and similarity could be related but not equal measures of a latent variable of social closeness and suggest several, non-exhaustive, reasons why they are affected differently by the treatments. First, the generality and subjectivity of the similarity measure allows subjects to heavily weigh particularly

salient shared traits in their assessments, such as political leanings, which could swamp any treatment effect. Subjects could also consider other factors about their counterparts, such as being registered on the same experimental platform or choosing to participate in the same survey, which could dilute the effect of treatments. Second, with no monetary incentives attached, subjects might pay less attention to the similarity than to the commonality question. While we have no direct measure of attention, subjects responded to the similarity question on average about 3.5 times faster (5.95 seconds, s.e. 0.186, versus 20.65 seconds, s.e. 0.291; $p < 0.001$). Finally, the latent variable of social closeness could have various features - one of which is similarity and another is commonality - raising the question as to which measure captures relevant aspects of social closeness for the setting at hand.[12]

Specifications (3) and (6) show that 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. Interacting the treatment dummy with the order in which commonality and similarity questions were asked (*Comp* Com_Before*) shows no effect of order on the treatment effect. Specification (3), however, shows that the reported number of common answers is significantly lower when the commonality question is asked second. To understand this outcome, we consider the average gap between the reported number of common answers and the true number of common answers, which indicates that subjects are more accurate in their reports when commonality is asked second (0.22 vs. 0.37 answers, $p < 0.01$). One possible explanation is that the similarity question primes the subjects to think about their and Person A's answers to the Study Questionnaire, and this thinking contributes to subjects' responses to the commonality question. Supporting this explanation is the finding that subjects take significantly less time on average to report (more accurately) the number of common answers when commonality is asked second (19.67 vs. 21.63 seconds, $p < 0.001$).

[12]Other measures suggested in the social psychology literature such as the Bogardus Social Distance Scale (Bogardus, 1928) or the Inclusion of Other in the Self Scale (Aron, Aron & Smollan, 1992) aim to capture different features of social closeness.

We find that gender does not significantly affect these results, and we refer interested readers to Appendix B for the analysis.

	Commonality			Similarity		
	(1)	(2)	(3)	(4)	(5)	(6)
Competition	-0.181***	-0.184***	-0.190***	-0.024	-0.036	-0.033
	(0.043)	(0.043)	(0.060)	(0.036)	(0.034)	(0.051)
Common_Ans	0.562***		0.554***	0.678***		0.656***
	(0.021)		(0.023)	(0.018)		(0.019)
Same_Gender		0.513***			0.610***	
		(0.043)			(0.035)	
Same_Married		0.528***			0.505***	
		(0.043)			(0.034)	
Same_Season		0.525***			0.506***	
		(0.043)			(0.035)	
Same_PolParty		0.700***			1.156***	
		(0.045)			(0.036)	
Com_Before			0.123**			-0.005
			(0.060)			(0.051)
Comp*Com_Before			0.008			0.014
			(0.085)			(0.072)
Constant	1.324***	1.295***	0.714***	1.854***	1.759***	1.344***
	(0.056)	(0.056)	(0.149)	(0.047)	(0.045)	(0.124)
Demographics	No	No	Yes	No	No	Yes
Observations	2000	2000	2000	2000	2000	2000
R^2	0.260	0.264	0.272	0.410	0.471	0.428

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

Table 1: Regression Analysis of Treatment Effects on Social Perceptions ((1) to (3) for Commonality and (4) to (6) for Similarity)

4 Controls: Piece-Rate Payment Scheme, Non-Social Perceptions

In this section we present two experiments which control for two different features of our main study. First, to determine whether competition vs. cooperation is key to the relative difference in social perceptions, we consider an experimental treatment in which subjects face a piece-rate payment scheme. Second, to determine whether the effect of competition vs. cooperation

is particular to social perceptions, we consider perceptions of non-social objects. Sections 2 and 3 of the Online Appendix provide the full instructions for the two control experiments.

4.1 Social Perceptions, Piece-Rate Payments

Our first control considers subjects' social perceptions when their pay in the counting task is unrelated to Person A's performance. The experimental design is identical to the main experiment with the following replacement : In Part 2, instead of the competitive or cooperative pay scheme, the subject simply receives piece-rate pay of \$0.20 per table counted correctly. (The subject's expected earnings are the same as in the main study.) Then as in the main study, Person A is introduced to the subject as a randomly selected person who completed the same task previously. Subjects are told their bonus does not depend on the performance of Person A. Person A's answers to the Study Questionnaire are shown to the subject before the counting task, and the social perception questions of similarity and commonality, presented in random order, follow after the task.

We consider the hypothesis (pre-registered) that subjects' reports of common answers to the Study Questionnaire in the *Piece Rate* treatment are between the reports in *Competition* and *Cooperation* of the main study. The premise is that people do not have any particular feelings, positive or negative, about the social distance to a person who has no role in their compensation. Relative to this neutral benchmark, competition would decrease and cooperation would increase perceived social proximity. Given no effect of *Competition* vs. *Cooperation* on similarity in the main study, we pre-registered no treatment effect on similarity in the *Piece-Rate* treatment.

The experimental treatment, referred to as *Piece-Rate*, was run on Prolific in February 2024 with 1000 participants restricted to the United States and filtered to ensure gender balance. The characteristics of the subject population match those of the main study in terms of gender, but do not match on other demographics. Details are given in Appendix A, Tables

A.1 and A.2. and subsequent text. Nonetheless, the average number of answers in common with Person A are not significantly different between *Competition* and *Piece-Rate* (2.14, s.e. 0.032, vs. 2.10, s.e. 0.031, $p = 0.154$), nor between *Cooperation* and *Piece-Rate* (2.16, s.e. 0.031, vs. 2.10, s.e. 0.031, $p = 0.154$). In the subsequent regressions, we control for all demographics, including those that differ between samples.[13]

For the commonality question, we find that the average response is between those of *Competition* and *Cooperation* but closer to *Cooperation*. The mean report is significantly lower in *Competition* than in *Piece-Rate* (2.35, s.e. 0.035, vs. 2.47, s.e. 0.036, $p = 0.016$) and higher in *Cooperation* than in *Piece-Rate* but the latter difference is not significant (2.54, s.e. 0.035, vs. 2.47, s.e. 0.036, $p = 0.157$). As in the main study, the subjects' reports are on average higher than the true number of common answers, with a mean gap of 0.367 (s.e. 0.036, $p < 0.001$). The gap between the report and the true number of common answers is 0.16 lower for *Competition* than for *Piece Rate* (0.206, s.e. 0.032, vs. 0.367, s.e. 0.036, $p < 0.001$) and not significantly different for *Cooperation* and for *Piece Rate* (0.367, s.e. 0.036, vs. 0.376, s.e. 0.347, $p = 0.85$, $p = 0.42$). By Kolomogorov-Smirnov tests, we can reject that the frequency distributions of the reported commonality and of the gap are the same for *Competition* and *Piece Rate* ($p = 0.097$ and $p < 0.001$ respectively), but we cannot reject that these distributions are the same for *Cooperation* and *Piece Rate* ($p = 0.893$ and $p = 0.928$ respectively).

Regressions (1) to (3) in Table 2 confirm the above results for commonality, with and without controlling for demographics and the order of the commonality and similarity questions. In this table, the omitted treatment is *Piece-Rate*, and we estimate the effect of *Competition* and the effect of *Cooperation*. Compared to the piece-rate benchmark, Table 2 demonstrates that the competitive pay scheme decreases reports of commonality while the cooperative pay

[13]We also consider these regressions on sub-populations of subjects, with robust results. Precisely, when splitting subjects along each demographic which differs between samples (marital status, favorite season, political leaning, and age above and below 35 years), the estimated treatment effects are within the confidence intervals of the estimated effects of the main analysis and statistically significant for all but the smallest sample.

scheme increases these reports but insignificantly.[14]

For similarity, we find the same patterns as in the main study. Compared to *Piece-Rate*, *Competition* and *Cooperation* do not robustly affect the assessments of similarity. This is shown by specifications (4) to (6) of Table 2. As in the main experiment, shared political party leaning is the largest determinant of the similarity ratings.

	Commonality			Similarity		
	(1)	(2)	(3)	(4)	(5)	(6)
Competition	-0.143***	-0.135***	-0.147**	0.045	0.023	0.051
	(0.044)	(0.045)	(0.063)	(0.037)	(0.036)	(0.052)
Cooperation	0.038	0.053	0.045	0.069*	0.062*	0.084
	(0.044)	(0.045)	(0.063)	(0.037)	(0.036)	(0.052)
Common_Ans	0.544***		0.535***	0.683***		0.660***
	(0.018)		(0.019)	(0.015)		(0.016)
Same_Gender		0.447***			0.589***	
		(0.036)			(0.029)	
Same_Married		0.519***			0.531***	
		(0.036)			(0.029)	
Same_Season		0.521***			0.497***	
		(0.039)			(0.031)	
Same_PolParty		0.707***			1.166***	
		(0.044)			(0.030)	
Com_Before			0.102			0.037
			(0.062)			(0.052)
Comp*Com_Before			0.027			-0.030
			(0.088)			(0.073)
Coop*Com_Before			0.016			-0.041
			(0.088)			(0.073)
Constant	1.325***	0.764***	0.705***	1.773***	1.305***	1.284***
	(0.049)	(0.127)	(0.131)	(0.041)	(0.102)	(0.109)
Demographics	No	Yes	Yes	No	Yes	Yes
Observations	3000	3000	3000	3000	3000	3000
R^2	0.234	0.249	0.243	0.402	0.466	0.417

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

Table 2: Regression Analysis of Social Perceptions: Piece-Rate vs. Competition vs. Cooperation ((1) to (3) for Commonality and (4) to (6) for Similarity)

[14]In specification (3) of Table 2 (as in specification (3) of Table 1 above), the coefficient of *Com_Before* is positive (but insignificant) indicating that reports of commonality are higher when the commonality question is asked before the similarity question. In the *Piece-Rate* treatment, as it is the case in *Competition* and in *Cooperation*, the gap between the reported and true number of common answer decreases when the similarity question is asked first (0.31 vs. 0.43, $p = 0.109$).

4.2 Non-Social Perceptions, Competition vs. Cooperation

In our second control experiment, we ask if the competitive and cooperative pay schemes affect subjects' report of common features of non-human ensembles. The experimental design is identical to the main experiment – subjects are put either in a competitive or in a cooperative work setting with a randomly-selected Person A – with the following replacements. In Part 1, in the place of the Study Questionnaire, subjects generate four, randomly selected binary features of a natural scene: landscape (Mountains or River), season (Spring or Fall), location (East or West), weather (Rainy or Sunny). In Part 2, in place of Person A's answers to the Study Questionnaire, subjects see a set of four randomly selected features of another natural scene. In Part 3, subjects are asked to report the number of features the first scene has in common with the second scene. Subjects are reminded of the features of the first scene since in our main experiment subjects know their own answers to the Study Questionnaire. Subjects are not asked to assess the similarity of the scenes.

In the pre-registration, we do not specify the direction of a possible effect of *Competition* and *Cooperation* on subjects' reports of common features of natural scenes. The lack of a directional hypothesis reflects the mixed evidence of the effects of competition on cognitive performance; the effects depend on the type of task, the nature of the competition, individual differences in reaction to stress, etc. (See, for instance, Qin, Johnson & Johnson (1995), DiMenichi & Tricomi (2015), or DiMenichi & Tricomi (2017)).

The two treatments, referred to as *Competition_NonSocial* and *Cooperation_NonSocial*, were run on Prolific in May 2023 with 604 participants restricted to the United States and filtered to ensure gender balance. The characteristics of the subject population in the two non-social treatments are not different in terms of age, education and performance in the counting task. Details are given in Appendix A, Table A.3.

We find no treatment effect on the reports of common features of the natural scenes. The mean of the true number of common features is the same – 1.94 (s.e. 0.060) in *Com-*

petition_NonSocial and 1.93 (s.e. 0.059) in *Cooperation_NonSocial* ($p = 0.88$) – and by a Kolmogorov-Smirnov test we cannot reject the frequency distributions are the same ($p = 0.995$). The mean reports of common features are 2.17 (s.e. 0.061) in *Competition* and 2.27 (s.e. 0.061) in *Cooperation* ($p = 0.257$). The average gap between the reported and the true number of common answers is positive and significant in both treatments, 0.23 (s.e. 0.063) in *Competition_NonSocial* and 0.34 (s.e. 0.060) in *Cooperation_NonSocial*; the difference between these average gaps of -0.11 is not significant ($p = 0.204$). By Kolmogorov-Smirnov tests, we cannot reject that the frequency distributions of reports and of gaps are the same in *Cooperation_NonSocial* and *Competition_NonSocial* ($p = 0.708$ and $p = 0.995$). The absence of a treatment effect is confirmed by the regression analyses provided in Table A.4 of Appendix A.

5 Conclusion

This paper tests whether competition versus cooperation for economic gain 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 competitive or cooperative work setting. Subjects' reports of commonality are significantly lower for the competitive pay scheme despite monetary incentives to make accurate reports. The treatments, however, do not affect subjects' subjective assessment of similarity to counterparts, which appears to be dominated by shared political leanings more than any other factor. Follow-up experiments on the effect of competition vs. cooperation for pay on commonality reports indicate that (a) relative to piece-rate pay, competition significantly decreases these reports and cooperation increases them but not significantly so, and (b) no general, mechanical effect of competition vs. cooperation explains the findings.

The paper connects two areas of experimental economics – social groupings and motivated beliefs – and speaks to the larger questions of how economic interactions relate to

social differences. 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: cooperating or competing for pay 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 for accuracy and the need to feel good about themselves and what they do or plan to do. Our study is the first to indicate that such a trade-off might be at play when individuals think about themselves in relation to others.

This study indicates that people can have motivated social perceptions and future research could explore why and how individuals manipulate their perceptions of others. People could manipulate their social views for affective reasons, i.e. to feel better, and instrumental reasons, i.e. to perform better (Bénabou (2015)). As explained in the text, people could decrease social closeness when competing for pay to reduce the utility loss from inequality aversion. Increased social distance to counterparts could also increase motivation to compete against them for pay, or if close, to cooperate. Future experiments could possibly distinguish between these mechanisms by changing the timing at which counterparts' profiles are presented.

Regarding the channels, recent experiments in economics have identified at least three paths to favored views, each of which could be operative in our experiment: Subjects could selectively memorize or selectively forget items on the list of counterparts' traits (see Amelio & Zimmermann (2023) for a review on motivated memory in economics); when reporting the number of common traits, subjects could make counting mistakes (see Exley & Kessler (2024) for a study on motivated errors); subjects could purposefully ignore or pay more attention to some of the traits they see (see Dana, Weber & Kuang (2007) for seminal work on willful ignorance). Adjusting the display of counterparts' traits, giving subjects some choice in reading or accessing information about counterparts, or asking subjects about commonality of each particular trait, could possibly help identify these channels.

With the focus on competition or cooperation for economic gain and social perceptions, the present paper introduces a new dimension to the study of social difference. Our experiment indicates that people possibly process and report social information about self and others differently, depending on how they interact with them in the economic realm. This biased processing could contribute to why historical patterns of prejudice and violence have roots in economic competition (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 an impact of the cooperation and competition beyond the associated economic gains – the competition or cooperation for those gains could also shape the social world.

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Appendix

This Appendix provides (A) Supplementary material including information on the characteristics and performance of subjects, regression analyses for the experiment on non-social perceptions and details about the design of the Study Questionnaire, and (B) Discussion of Pre-Registrations and Hypotheses.

A. Supplementary Material

Subjects' Characteristics and Performance. Table A.1 presents the characteristics of the population of subjects and their performance in the counting task in the treatments *Competition*, *Cooperation* and *Piece-Rate*. Table A.2 then presents the p-values resulting from two-by-two t-tests of differences in these characteristics and performance.

	Competition	*Cooperation*	*Piece-Rate*
Fraction Female (%)	48.49	48.71	48.00
Fraction Democrat (%)	76.10	75.00	67.20
Fraction Prefers Fall (%)	63.96	63.45	53.60
Fraction Married (%)	45.58	45.22	56.80
Average age (years)	34.63	34.64	42.82
Average level of education	3.21	3.19	3.31
Average number of tables counted correctly	5.05	4.95	4.65
Number of Subjects	996	1004	1000

Note: We coded levels of education using 5 levels: level 1 corresponds to less than high school or high school; level 2 corresponds to some college but no degree; level 3 corresponds to Associate's degree (two years of college); level 4 corresponds to Bachelor's degree (four years of college); level 5 corresponds to Master's degree and higher.

Table A.1. Characteristics and Performance of Subjects per Treatment

These tables show no difference in subjects' characteristics or performance in *Competition* and *Cooperation*, but differences between these treatments and *Piece-Rate*. In *Piece-Rate*, a smaller share of subjects lean to the Democratic party, a smaller share of subjects prefer the fall and more subjects declare being married or in a domestic relationship. Theses tables also show that the subject population is significantly younger in the main treatments than

	Comp vs.*Coop*	*Comp* vs.*PR*	*Coop* vs.*PR*
Fraction Female (%)	0.925	0.825	0.752
Fraction Democrat (%)	0.566	<0.001	<0.001
Fraction Prefers Fall (%)	0.813	<0.001	<0.001
Fraction Married (%)	0.870	<0.001	<0.001
Average age (years)	0.978	<0.001	<0.001
Average level of education	0.636	0.106	0.036
Average number of tables counted correctly	0.301	<0.001	<0.001

Table A.2. Two-by-Two Comparisons of Subjects Characteristics and Performance (p-values)

in *Piece-Rate* ($p < 0.001$), which could explain why subjects count significantly more tables correctly in the main treatments ($p < 0.001$) than in *Piece-Rate*.

Table A.3 presents the characteristics and performance of subjects in the two *Non-Social* treatments, as well as the p-value resulting for t-tests of differences in these characteristics and performance. (In the *Non-Social* treatments, subjects did not answer the Study Questionnaire, so we have fewer characteristics.) The table shows no differences in characteristics of performance of subjects in *Competition_NonSocial* and *Cooperation_NonSocial*.

	Comp_NonSocial	*Coop_NonSocial*	*Diff. (p-value)*
Average age (years)	40.67	42.32	0.137
Average level of education	3.13	3.28	0.164
Average number of tables counted correctly	4.53	4.47	0.742
Number of Subjects	301	303	

Note: We coded levels of education using 5 levels: level 1 corresponds to less than high school or high school; level 2 corresponds to some college but no degree; level 3 corresponds to Associate's degree (two years of college); level 4 corresponds to Bachelor's degree (four years of college); level 5 corresponds to Master's degree and higher.

Table A.3. Characteristics and Performance of Subjects in Non-Social Treatments

Commonality in the Experiment on Non-Social Perceptions. Table A.4 presents the regression for the commonality question in the *Non Social* treatments. The dummy variable *Competition* equals one in *Competition_NonSocial* and zero in *Cooperation_NonSocial*.

	Commonality_NonSocial	
	(1)	(2)
Competition	-0.104	-0.102
	(0.076)	(0.076)
Common_Items	0.490***	0.493***
	(0.036)	(0.037)
Constant	1.327***	1.570***
	(0.089)	(0.220)
Demographics	No	Yes
Observations	604	604
R^2	0.231	0.235

Note: The Table reports OLS coefficients (standard errors in parenthesis). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Demographics include age, education, and subjects' answers to the questions on competitiveness and working in teams (coded as 0 or 1).

Table A.4. Regression Analysis for the Reported Number of Common Items in the Study on Non-Social Perceptions

Study Questionnaire Design. We designed the Study Questionnaire to be a short personal survey which distinguishes individual subjects in terms of demographics and preferences. Building on methods in Lee et al. (2021), we collected data from 500 Prolific participants who were asked to answer 50 questions about themselves. The questions concerned demographics such as gender, age, parental status, martial status, as well as political leanings and preferences about seasons, food, art, and vacation destinations. The answers to all questions were binary except for the question concerning gender. The participants were paid $1.00 for completing the survey.

We selected the four items for the Study Questionnaire as follows: We used principal component analysis to determine underlying factors in the data and the questions that load on each factor. Given documented gender differences in preferences in competitive settings (Niederle & Vesterlund, 2011; Datta Gupta, Poulsen & Villeval, 2013), we selected gender as one of the survey items (gender was the primary question among one of the top six factors). We then selected three questions which loaded onto the top three factors and which were not highly correlated with each other: marital status, political party leanings, and preference for

season spring or fall. Table A.5 presents the correlations between the Study Questionnaire items in the main experiment.

	Female	Married	Democrat	Fall
Female	1			
Married	0.015	1		
Democrat	0.113 ***	-0.1285***	1	
Fall	0.010	0.003	0.016	1

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.5. Correlations of Study Questionnaire Items in the Main Study

B. Discussion of Pre-Registered Hypotheses

Here we report and discuss the pre-registered hypotheses. All studies in this paper were pre-registered on Aspredicted.org. For the main study, the pre-registrations references are #80504 (main treatments with the similarity question asked first) and #85269 (main treatments with then commonality question asked first). For the controls, the reference is #161321 for the study on *Piece-Rate* and #132541 for the study on *Non-Social* perceptions.

B.1. Pre-registrations - Main Study

For the main experiment, our two main hypotheses were Hypothesis 1 and 2 stated in section 2.2. That is, we hypothesized that the treatments would affect (more) the most subjective of the two measures of social perception. In the data, we find support for the reverse: the treatments do not affect similarity but do affect the number of common answers reported. As explained in the main text, this pattern could be linked to the subjective nature of the similarity question.

Given observed gender differences in preferences for competition and for the gender of counterparts, the pre-registration also mentioned examining gender differences in the effect of treatments on social perceptions. We generally do not find gender effects.

First, we find on average men and women both report significantly lower commonality in *Competition* than in *Cooperation*, and women have significantly lower average reports of

similarity in *Competition*. In Table B.1, we split the sample into male and female subjects and find a significant treatment effect on commonality of same magnitude. For females, the treatment also has a weakly significant effect on similarity. Given the nature of the similarity question discussed above, we see this latter result as supportive of the general premise of motivated social perceptions rather than indicative of a gender difference.

Second, 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 (227 observations) ($p = 0.766$).

	Commonality				Similarity			
	Female	Female	Male	Male	Female	Female	Male	Male
Competition	-0.197***	-0.199***	-0.173***	-0.180***	-0.087*	-0.092**	0.032	0.012
	(0.060)	(0.060)	(0.062)	(0.061)	(0.048)	(0.047)	(0.052)	(0.050)
Common_Ans	0.589***		0.517***		0.743***		0.567***	
	(0.031)		(0.033)		(0.025)		(0.028)	
Same_Gender		0.539***		0.479***		0.767***		0.460***
		(0.060)		(0.062)		(0.047)		(0.050)
Same_Married		0.602***		0.460***		0.550***		0.456***
		(0.060)		(0.062)		(0.047)		(0.050)
Same_Season		0.555***		0.463***		0.567***		0.404***
		(0.065)		(0.067)		(0.051)		(0.054)
Same_PolParty		0.694***		0.735***		1.217***		1.117***
		(0.076)		(0.073)		(0.059)		(0.059)
Constant	0.807***	0.810***	0.737***	0.722***	1.431***	1.435***	1.319***	1.276***
	(0.200)	(0.200)	(0.203)	(0.203)	(0.162)	(0.155)	(0.173)	(0.165)
Observations	972	972	1028	1028	972	972	1028	1028
R^2	0.315	0.318	0.230	0.238	0.518	0.559	0.352	0.415

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

Table B.1. Regression Analysis for Social Perceptions Questions by Gender

B.2. Pre-Registrations - Controls

Piece-Rate. In the *Piece-Rate* control, subjects' pay does not depend on the performance of counterparts. Following the reasoning presented in Section 4.1, we pre-registered the two following hypotheses concerning commonality: (1) Reports of traits in common are lower in *Competition* than in *Piece-Rate*; (2) Reports of traits in common are higher in *Cooperation* than in *Piece-Rate*. We also pre-registered the following two hypotheses about similarity assessments based on our results in the main study: (3) Similarity is the same in *Competition* and in *Piece-Rate*; (4) Similarity is the same in *Cooperation* and in *Piece-Rate*.

As detailed in section 4.1, hypotheses (1), (3), and (4) are validated in the data. Hypothesis (2) is not fully validated in the sense that commonality is higher in *Cooperation* than in *Piece-Rate* but the difference is insignificant.

Non-Social Perceptions. For the study on non-social perceptions, as explained in Section 4.2, we did not pre-register any specific hypotheses for how the competitive and cooperative pay schemes would affect subjects' reports of features in common in the natural scenes. Studies in psychology examine the effect of competition on cognitive performance, memory and attention (summarized in DiMenichi & Tricomi, 2015 and 2017) and provide mixed conclusions. Effects depend on the nature of the competition, gender, individuals' normative goals, reactions to stress, and already-present cognitive loads.