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Author
Yildirim, Ugur

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By

Ugur Yildirim

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Committee in charge:

Professor David J. Harding, Chair
Associate Professor Daniel J. Schneider
Assistant Professor Dennis M. Feehan
Professor Gabriel S. Lenz

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Abstract

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Ugur Yildirim

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This dissertation studies how inequality perceptions impact Americans' distributional and other-regarding preferences. The first chapter analyzes original data collected from an online networked experiment and demonstrates that the process through which inequality emerges in the network has a significant impact on individuals' fairness perceptions and willingness to invest in their communities. The second chapter analyzes original data from an online survey experiment and shows that the addition of pessimistic information about opportunity does not lead to any more support for redistribution when pessimistic information about inequality is already present.

The third and fourth chapters focus specifically on inequality perceptions during the coronavirus pandemic. The third chapter analyzes original data from another survey experiment and finds that receiving information about class inequalities specifically in relation to the outbreak tends to be much more effective in moving people's opinions compared to receiving that information in a way that does not directly relate it to coronavirus. The fourth and final chapter analyzes data from the same experiment to understand how different framings of the pandemic are influencing public's threat perceptions regarding the outbreak and finds that emphasis on the unequal aspect of the pandemic leads the public to become less concerned about the outbreak and its human toll.

Introduction

This dissertation studies how perceptions of inequality and the related concepts of opportunity and fairness impact Americans' distributional preferences and the amount of concern they show for the economic and social wellbeing of others. More specifically, the goal of this dissertation is to understand how perceptions of inequality, opportunity, and fairness affect outcomes such as willingness to invest in one's community, concern for existing income and wealth gaps in the country, and support for redistribution.

Perceptions, attitudes, and preferences related to inequality is an important topic to study because these have been shown to affect Americans' political and voting behavior (Fisman, Jakiela, and Kariv 2017). In fact, attitudes toward inequality are as important to the study of inequality as are *facts* about inequality: While facts such as the level of unemployment or wage inequality in America are important in shaping one's attitudes toward inequality, it is ultimately one's knowledge of and attitudes toward these facts that determine whether one supports policies to reduce inequality or votes for the candidate who endorses such policies.

Scholars' fascination with America's alleged lack of concern about inequality gave rise to a rich literature trying to understand how American people make sense of ideas such as inequality, opportunity, fairness, and redistribution. In particular, the evidence that indicates that inequality by itself might not be considered as a problem by most Americans (Norton and Ariely 2011, Norton 2014) even in the face of rising inequality in the US (Piketty and Saez 2006, McCall and Percheski 2010) led researchers to hypothesize that the populace might care more about the *source* of inequality (e.g., whether it is fair or not) rather than inequality *per se* (Starmans, Sheskin, and Bloom 2017).

My first chapter on inequality and fairness (pp. 1-15) contributes to this debate by studying how the process through which inequality emerges affects people's fairness perceptions related to the economic arrangements they are operating in and how willing they are to contribute a fraction of their wealth to the community. The chapter is based on a behavioral, networked experiment that is run online on a sample of individuals from the US. A behavioral experiment is chosen over a survey experiment to study this question because it is not possible to manipulate the process through which inequality emerges using the survey-based approach. In other words, the question being asked here requires us to create actual economic arrangements and observe how a given arrangement affects fairness perceptions and contributions.

While the behavioral approach described above is one way to study Americans' perceptions and preferences related to inequality, this approach is limited in the sense that it is based on a very simplified model of society, where many aspects of real society are abstracted away. In particular, we know that in real life individuals form their attitudes not only through what they personally experience but also through what they hear from outside sources, including the media, opinion leaders, and elites. Scholars have shown that these external sources have been influential in moving public opinion (Mutz and Soss 1997, Druckman and Nelson 2003, Bartels and Mutz 2009).

Studies have also shown that information related to certain concepts, in particular inequality, mobility, fairness, and opportunity, are influential in shaping people's level of concern for inequality and how much support they give to redistribution (Alesina and Angeletos 2005, Alesina and Ferrara 2005, Krawczyk 2010, Bjornskov et al 2013, McCall 2013, Kuziemko et al 2015, Shariff, Wiwad, and Aknin 2016, McCall et al 2017, Alesina, Stantcheva, and Teso 2018).

Based on these studies, we expect that pessimistic information related to these concepts should increase concern for inequality as well as support for redistribution.

What we do not know is how information related to these concepts jointly affects the outcomes just mentioned. In particular, while we expect that pessimistic information about inequality (e.g., inequality is very high and rising) should move people's opinions towards increased concern for inequality and the need to do something about it, it is unclear how this information compounded with pessimistic information about opportunity (e.g., children today have worse chances for getting in life compared to parents) would affect the outcomes. This is a serious shortcoming because of the intricate connection between inequality and opportunity, especially in the American context (McCall 2013, McCall et al 2017).

My second chapter on inequality and opportunity (pp. 16-29) enters into the debate on this exact point to study how perceptions of inequality and opportunity separately and jointly affect individuals' attitudes and preferences towards inequality. It is based on a survey experiment that primes respondents' perceptions of inequality and opportunity towards optimism (e.g., inequality actually stopped rising) or pessimism (e.g., inequality is rising at an alarming rate). The treatments are administered in an original way whereby instead of giving the information to the respondents as texts to be read out of the blue, the information is conveyed in a more subtle way as answers to questions asked to respondents. This unique design guards against the known danger of respondents rejecting treatments that are too obvious or intrusive.

The two chapters discussed so far are based on data collected and analyzed before the novel coronavirus pandemic took over the world. Not only did the new virus spread to almost every corner of the world at an incredibly fast pace (Van Bavel 2020) -- with the closest comparable event being the 1918 flu pandemic (Scott and Duncan 2001) -- it also did so in a way that made existing inequalities in society more apparent than usual. In particular, it has been shown that the virus has a much deadly effect on the elderly and those with medical conditions (Zhou et al 2020). Similarly, a lot of evidence has been brought to the fore that shows how the poor, in particular the people of color, have been much more adversely affected by the pandemic than the wealthy (Beyer 2020, Von Braun, Zamagni, and Sorondo 2020).

Given this new background, it is important to study how the coronavirus pandemic has affected Americans' perceptions related to inequality. The first question that needs answering is whether the current context where inequalities have become more apparent than usual has any impact on whether discussion of inequality is able to move people's opinions on related issues, including the government's role in fighting inequality. We know that Americans' opinions on issues related to inequality are generally very much entrenched and hard to change -- even in the context of local crises such as the Hurricane Katrina disaster that struck New Orleans in 2005 and exposed racial and class inequalities in the US (Bobo 2006, Belkhir and Charlemaine 2007). However, a truly national, even global, crisis such as the one we are currently in the midst of might have a different effect by virtue of the fact that the coronavirus crisis has had a direct impact on the lives of almost everyone in the country.

My third chapter on perceptions of inequality during the coronavirus pandemic (pp. 30-46) brings in another set of survey experimental evidence to decide whether it is more effective -- in terms of moving people's opinions on inequality -- to discuss inequality in the context of coronavirus compared to discussing it in general terms without reference to the pandemic. The survey experiment has a clever design whereby there are both general and coronavirus-specific informational treatments related to inequality, which allows us to see

whether the coronavirus-specific treatments are more, less, or equally as effective in moving opinions as the general ones.

While Americans' attitudes towards inequality and preferences for redistribution are an important component of their general concern for the economic and social wellbeing of others, given the enormous human cost of the pandemic -- nearly seven million confirmed cases and more than 200,000 deaths in the US only by September 2020 (ArcGIS 2020) -- it is equally, if not more, important to understand how perceptions of inequality are affecting Americans' concern for the health consequences of the pandemic, including how serious they think the pandemic is and whether they think it is more important to save lives or to save the economy.

My fourth and final chapter (pp. 47-55) studies this question based on the same survey experiment that my third chapter is based on. While the third chapter is interested in studying how general vs. coronavirus-specific framings of inequality impact preferences for redistribution, this chapter instead looks at how emphasis on the disparate impact of the pandemic on different groups -- the elderly, those with medical conditions, the poor, minorities -- affects public concern for health consequences. Given that the pandemic does not seem to be going away anytime soon, the findings presented in this chapter are extremely timely and have the potential to guide future policy and scientific discourse.

To recap, the four chapters of my dissertation bring in original data from different sources -- including both behavioral and survey experimental data -- to answer questions related to what inequality means to Americans, how they make sense of it, and the impact of inequality perceptions on their attitudes towards pertinent issues vis-à-vis other Americans. The chapters that follow flesh out the main ideas discussed in this introduction chapter and present the concrete findings related to each specific question raised here. The conclusion chapter at the end (pp. 56-60) summarizes the main findings in these chapters and discusses their implications.

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Source of Endowment, Inequality, and Fairness: A Networked Experiment

Ugur Yildirim[1]

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Abstract. While many studies have shown that inequality in the US is on the rise, it is unclear whether inequality by itself is considered as a problem by most people. In fact, it seems entirely possible that it is not inequality *per se* that people care about but rather the *source* of inequality, such as whether inequality comes about through everyone getting what they deserve or not. By focusing on the specific mechanism through which inequality comes about we can go beyond the equal vs. unequal dichotomy and start making sense of the reality around us through an alternative lens: fair vs. unfair. The idea of fairness as a separate axis from equality has been shown to be influential in driving preferences. On the other hand, recent behavioral research has not paid attention to the mechanism through which (in)equality in the network is created (that is, source of endowment) as a possible factor to be studied. Accordingly, this study presents results based on a behavioral, networked experiment that manipulates the source of endowment to understand how this factor impacts fairness perceptions and distributive preferences. It finds evidence to support the arguments that (i) people prefer not equal but fair societies, and (ii) feeling entitled to one's wealth makes one less likely to give that wealth away.

source of endowment | inequality | fairness | networked experiment | within-subjects design

Introduction

Many studies have shown that inequality in the US is on the rise (Piketty and Saez 2006, McCall and Percheski 2010). Despite this trend, it is unclear whether inequality by itself is considered as a problem by most people (Norton and Ariely 2011, Norton 2014). That is, it is possible that it is not inequality *per se* that people care about but rather the *source* of inequality. For example, an unequal arrangement where everyone gets what they deserve will likely be interpreted very differently compared to another unequal arrangement where people do not get what they deserve. Understanding the distributional preferences of the populace is important as it affects political and voting behavior (Fisman, Jakiela, and Kariv 2017).

By focusing on the specific mechanism through which inequality comes about we can go beyond the equal vs. unequal dichotomy and start making sense of the reality around us through an alternative lens: fair vs. unfair. Researchers have already proposed making this conceptual leap forward and hypothesized that people prefer not equal or unequal but fair societies (Starmans, Sheskin, and Bloom 2017). This idea of fairness as a separate axis from equality has also been shown to be influential in driving preferences. For example, research (mainly survey-based) has shown that belief in equal opportunity as well as perceptions of fairness make people more likely to be against redistribution (Alesina and Ferrara 2005, Bjornskov et al 2013).

Beyond the two studies cited above, the survey-based strategy has been widely adopted and has provided almost all of our insights into public opinion about concepts such as inequality,

[1] Ugur Yildirim (ugur.yildirim@berkeley.edu) is a Ph.D. candidate in the Department of Sociology, University of California-Berkeley. The author thanks Dennis Feehan, Daniel Schneider, David Harding, Gabriel Lenz, and Xinyi Zhang for all of their helpful comments, Mark McKnight, Carl Mason, and Carl Boe for all of their technical help, without which this project could not have been undertaken, and UC Berkeley XLab, Don Moore in particular, for the generous research grant that funded this study.

opportunity, fairness, and redistribution (Bartels 2005, Kenworthy and McCall 2008, McCall 2013, Engelhardt and Wagener 2014, Niehues 2014, Gimpelson and Triesman 2015, Hauser and Norton 2017). Several survey experiments have also been conducted with the aim of understanding different aspects of how these concepts matter for people (Kuziemko et al 2015, Shariff, Wiwad, and Aknin 2016, McCall et al 2017, Alesina, Stantcheva, and Teso 2018).

The survey-based approach to understanding these concepts has several advantages: survey data may come from a representative, probability sample of the population of interest; repeated surveys can be used to track trends and changes in perceptions over time; and survey experiments are well-suited for answering causal questions related to preferences. However, this approach has an important limitation: it is based on how survey respondents report their attitudes about issues related to inequality, rather than how people actually make decisions *in the context of inequality*. In particular, it is hard to use the survey-based approach to study how the mechanism through which (in)equality in the network is created (that is, source of endowment) impacts preferences.[2] Therefore, in addition to survey-based evidence, we also need evidence from studies whose design enables the identification of the causal effect of different economic arrangements on one's attitudes and behavior.

Luckily, recent technological advances have increased the feasibility of conducting behavioral networked experiments. These experiments require the *simultaneous* participation of a small group of participants who are connected to one another in an online network designed by the researcher. Given this setup, the study involves participants making a series of decisions (attitudinal or behavioral) vis-à-vis themselves, other participants, or the study as a whole. The advantage of this approach is that it enables researchers to precisely identify the causal effects of society-level characteristics—such as inequality or interaction structure—on participants' beliefs *and* behavior. The cost of this approach is that the research design necessarily involves a simplified model of a real society, such as the networked public goods game[3] used in this project. That said, the simplistic nature of these experiments actually allows the researcher to really isolate whatever aspect of society that is being studied.

Researchers from across the social sciences utilized networked experiments to understand the effects of numerous factors on human behavior in groups (Wang, Suri, and Watts 2012, Shirado et al 2013, Rand et al 2014, Nishi et al 2015, Nishi, Shirado, and Christakis 2015). However, none of them looked at the mechanism through which (in)equality in the network is created as a possible factor to be studied. In other words, players' starting scores (i.e., initial endowments) were always assigned to them by the researchers, and so the source of endowment was never a focus of study. However, for all the reasons discussed, there is a lot of reason to believe that source of endowment will have a tremendous impact on human preferences and actions.

Accordingly, this paper achieves two goals. First, it improves upon the behavioral networked experiment literature by studying the effect of source of endowment on human behavior. Second, through its focus on source of endowment, it connects two hitherto unconnected literatures, one based on behavioral data on human actions and the other primarily

[2] Another limitation of the survey-based approaches is that they do not directly demonstrate how such attitudes translate into actions, or how interactions with other people under certain conditions affect such attitudes. See also the related debate on "saying and doing:" Khan and Jerolmack (2013), Jerolmack and Khan (2014), and Lamont and Swidler (2014).

[3] Public goods games capture a variety of concepts including (i) how much participants trust other participants and (ii) how much they are willing to share their wealth with others. Many of these games have a prisoner's dilemma structure with cooperation being a natural outcome of choice.

focused on survey data on preferences. More specifically, the paper connects the recent research on how numerous factors impact human behavior in networks with the separate stream of survey research focused on inequality by studying how the mechanism through which (in)equality in the network is created impacts individuals' perceptions and preferences related to inequality.

Methods

Experimental design

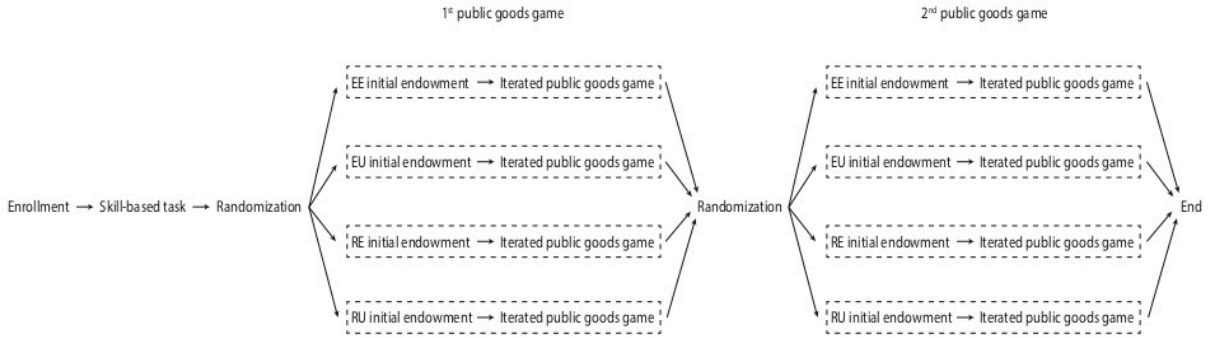
The experiment has the structure of a 2x2 factorial within-subjects design. One factor is the source of endowment (earned, random), and the other factor is the level of inequality (equal, unequal). Together these two factors result in four conditions: (i) earned equal, (ii) earned unequal, (iii) random equal, and (iv) random unequal. See Table 1.1 for a concise representation of these conditions. The earned/random axis determines whether the initial public goods game endowments are to be allocated based on performance in the skill-based task (described below) or randomly. The equal/unequal axis determines whether the initial public goods game endowments are to be allocated equally or unequally.

Table 1.1. The four experimental conditions.

	Equal	Unequal
Earned	Equal initial endowment based on group performance (EE)	Unequal initial endowment based on individual performance (EU)
Random	Equal initial endowment based on random assignment (RE)	Unequal initial endowment based on random assignment (RU)

The within-subjects aspect of the study comes from the fact that players are randomized into these conditions twice, resulting in 4 x 4 = 16 possible combinations. The study's unique within-subjects design -- whereby players participate in not one but two public goods games, played back to back, with randomization before each game -- is a major methodological advantage, because it not only increases the statistical power of the analyses conducted but also allows the researcher to be more confident when making causal claims. See Figure 1.1 for a schematic representation of the experiment.

Figure 1.1. A schematic representation of the experiment.



Each session (i.e., an experimental run) takes place in three phases and enrolls 12 to 18 simultaneous players (i.e., participants in an experimental run). In the first phase of a session, players are asked to perform a skill-based task, which is a fill-in-the-blanks word game. Each player in a group is presented with a list of words that are missing some letters. Players are asked to identify as many incomplete words as possible in a limited amount of time. For example, if a player is shown "t_adit_on_l", then he/she would earn points for responding "traditional", and no points for any other response. The more words a player successfully identifies, the higher his/her score on the task. This first phase is identical for players in all experimental conditions.

The second and third phases of the session involves playing networked public goods games. Players' initial endowments in these games depend on which experimental condition they are in. In each of the two public goods games, participants repeatedly take an action (i.e., make a decision) to either (1) prosocially contribute to a public good (cooperate) or (2) refrain from contributing to a public good (defect, thus free-riding when others do contribute). Players are allowed to unlink/link themselves to other players after each round of a game so as to ensure that they are not necessarily stuck with whomever they happen to be randomly connected at the beginning. At the end of the study, players receive a real payment that is proportional to their final score across both public goods games. See Table 1.2 below for a concise glossary of important terms used in this section. See Appendix 1.1 for the full set of texts and figures used in the experiment.

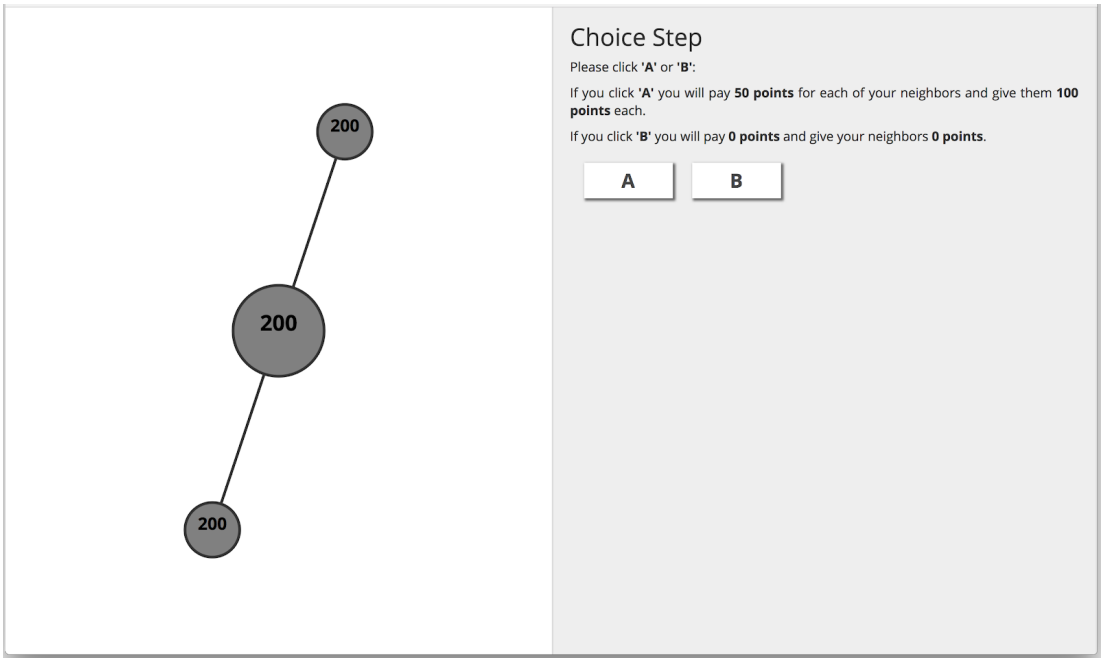
Table 1.2. Glossary of important terms.

Session	Experimental run corresponding to a given network of players
Player	Participant in a given session
Game	Public goods game (first, second) in a given session
Round	Decision round (1-10) in a given public goods game
Action	Decision (e.g., cooperate/defect) made by a player in a given round/game/session

Implementation and subject recruitment

The code needed to run the networked public goods game was implemented using the freely available Breadboard platform which has been designed specifically to allow researchers to conduct networked experiments (McKnight and Christakis 2016). See Figure 1.2 for a screen shot from a standard public goods game on Breadboard. Following several recent studies on cooperation in networked public goods games (e.g., Nishi et al. 2015), subjects were recruited from Amazon's Mechanical Turk (mTurk) platform. Breadboard has functionality to allow it to be linked to an mTurk account. Workers were required that they (i) be located in the US, (ii) have an overall HIT approval rate of 90% or above, and (iii) do not complete the task more than once. Each iteration of the experiment took about 25-30 minutes to complete, and workers were paid an average of $4 for their participation. The research project has IRB approval and is pre-registered on OSF. (All code, materials, and de-identified data will be made public once the study is over.)

Figure 1.2. Screenshot from a standard public goods game from the player's perspective.



Sample size calculations

To determine the target sample size needed for this experiment, a power study was conducted that takes into consideration the multilevel and within-subjects aspects of the study (Moerbeek, Breukelen, and Berger 2000, Snijders 2005, Lakens 2016). Power calculations were done assuming a between-subjects design, based on previous data collected by Nishi et al (2015), adjusted for the fact that the design is actually within-subjects. Based on these calculations, it was determined that 80 sessions needed to be run (160 sessions, if assuming a between-subjects design) of the experiment, each with 12 participants. See Appendix 1.2 for details of the power calculations.

Data structure

The experiment was run in blocks of 16 such that each possible treatment combination was run once before moving on to the next block. 10 such blocks were run, which resulted in 160 different networks. A total of 1,870 mTurk workers participated in these 160 sessions, and 1,759 of them fully completed the task and got paid. Table 1.3 below breaks down these numbers by condition (numbers inside parentheses are complete samples).

Table 1.3. Number of players by condition.

1st public goods game		2nd public goods game		
Earned	Equal	Earned	Equal	Number of players
0	0	0	0	114 (103)
			1	124 (117)
		1	0	127 (119)
			1	105 (99)

	1	0	0	111 (108)
			1	129 (120)
		1	0	123 (118)
			1	122 (113)
1	0	0	0	103 (91)
			1	106 (100)
		1	0	128 (118)
			1	112 (108)
	1	0	0	111 (105)
			1	113 (109)
		1	0	132 (129)
			1	110 (102)
				1870 (1759)

Given that there were 9 rounds in the first public goods game and 10 rounds in the second public goods game, this means that there is complete data on a total of 1,759 * (9+10) = 33,421 player-rounds. The final rectangular dataset used for analyses has one row for each session-player(-game)-round-action (some rounds have more than one action associated with them because players could be asked to make multiple rewiring decisions in a given round).

Variables

The final dataset has a large number of variables. The first set of variables are those that help us precisely identify each unique session, player, public goods game, round, and action as well as the experimental conditions associated with them. These include: (i) session id, (ii) player id, (iii) a dummy variable determining whether an observation comes from the first or the second public goods game, (iv) whether initial endowment was earned, (v) whether initial endowment was equal, and (vi) round. There are also variables for the exact (vii) date and (viii) start time of each session.

The second set of variables relate to the skill-based task and ego's fairness perceptions of the subsequent initial endowment allocation mechanism, including: (i) ego's score in the skill-based task, (ii) the change in ego's score from the skill-based task to the public goods game, (iii) fairness score ego assigned to the endowment allocation mechanism of the first public goods game, (iv) fairness score ego assigned to the endowment allocation mechanism of the second public goods game, (v) which game ego considers to be more fair (asked at the end of the experiment), and (vi) which game ego would prefer to play again if he/she had the chance (asked at the end of the experiment). Players were also asked to answer a survey question regarding (vii) the reason(s) why they chose to cooperate (asked at the end of the experiment).[4]

The third set of variables include those that relate to players' decisions in the two public goods games. These are: (i) ego's cooperation decision in a given round, (ii) ego's score at the time of cooperation decision, (iii) ego's tie formation decision in a given round, (iv) ego's score at the time of tie formation decision, (v) alter's score at the time of tie formation decision, (vi) ego's cooperation decision prior to tie formation decision, (vii) alter's cooperation decision prior to tie formation decision, (viii) ego's tie breakage decision in a given round, (ix) ego's score at

[4] Interested readers can see Appendix 1.3 for a set of qualitative results on "why" people cooperate.

the time of tie breakage decision, (x) alter's score at the time of tie breakage decision, (xi) ego's cooperation decision prior to tie breakage decision, and (xii) alter's cooperation decision prior to tie breakage decision. In addition, there are variables for (xiii) ego's score at the end of the first public goods game, (xiv) ego's score at the end of the second public goods game, (xv) trust score ego assigned regarding other players' behavior at the end of the first public goods game, (xvi) trust score ego assigned regarding other players' behavior at the end of the second public goods game, and (xvii) ego's connections in a given round.

Next is the set of variables related to the overall network. These include: (i) average cooperation in a given round in the whole network, (ii) average cooperation in a given round among ego and his/her immediate connections only, (iii) gini index in a given round in the whole network, (iv) gini index in a given round among ego and his/her immediate connections only, and (v) number of players in a given round.

Demographics

In addition to the variables described above, certain demographic information were also collected from the respondents, including: (i) the number of similar networked tasks they participated in before (i.e., experience), (ii) age, (iii) gender, (iv) race, (v) level of education, (vi) level of income, (vii) political orientation, and (viii) whether they are located in the US. An additional variable for (ix) country based on IP address was also generated. The overall demographic composition of the sample can be described as follows (there is some demographic variation across individual sessions, but the overall patterns do not change much):

(i) Mean experience is 301, while the median is only 2. The huge discrepancy between the mean and the median here is caused by the fact that there are some players who stated that they played a really large number of similar games.

(ii) Mean age is 37, while the median is 34.

(iii) The sample is 47.2% male, 52.2% female, and 0.6% other.

(iv) The sample is 78.7% White, 7.0% Black, 7.1% Hispanic, 5.3% Asian, and 1.9% other.

(v) The level of education in the sample is 0.5% less than high school, 10.8% high school, 30.1% some college, 43.2% college, 15.3% graduate, and 0.1% other.

(vi) The level of income in the sample is 19.2% less than $20K, 26.6% between $20-40K, 23.9% between $40-60K, 14.7% between $60-80K, 7.6% between $80-100K, and 8.0% more than $100K.

(vii) The sample is 17.3% very liberal, 33.8% liberal, 27.6% middle of the road, 17.4% conservative, and 3.9% very conservative.

Statistical models: overview

The models that were fit to data can be categorized, first and foremost, by whether the model is between- or within-subjects. Between-subjects models rely on comparisons between different groups of people (randomly assigned to different experimental conditions), while within-subjects models rely on comparisons within the same people (as their responses change from one experimental condition to another). The within-subjects approach is a significant improvement over the conventional between-subjects approach not only because it gives the study more statistical power but also because, by implicitly controlling for time-invariant confounders (which remain constant from the first game to the second), it is better suited for making causal

inferences. The main text only presents results from within-subjects models; see Appendix 1.5 for between-subjects models.

Beyond the between- vs. within-subjects distinction, it is possible to further categorize the models based on what the outcome variable is. The first set of models that will be presented in the next section are those where the outcome is the fairness score a player assigned to the endowment allocation mechanism for a given public goods game as a function of the experimental conditions. The second set of models are for the behavioral outcomes, most important of which is cooperation. The reason why cooperation is more fundamental than other behavioral outcomes such as tie formation or breakage decision is that these latter outcomes tend to be functions of cooperation in the sense that players tend to form ties with cooperators and break ties with defectors.[5]

Yet another distinction to make is between (i) models with clustered standard errors and (ii) linear mixed-effects models.[6] The most important point to make here is that while point estimates from models with clustered standard errors will be identical to a much simpler model without clustered standard errors, the point estimates from linear mixed-effects models will generally differ from this simpler model. For reference, most of the previous studies of networked experiments used variants of models with clustered standard errors to model their outcomes of interest. However, hierarchical models have certain advantages, at least in theory, over using clustered standard errors, including being better suited to deal with unbalanced data. The main text only presents results with clustered standard errors; see Appendices 1.4 and 1.5 for results from linear mixed-effects models.

Finally, note that most of the models fitted in this paper are at the individual-level and assume the data structure session-player(-game). However, it is also possible to fit session-level models with the data structure session(-game). In an aggregate model, the dependent variable would now be the average outcome (or the change thereof), rather than a specific player's outcome (or the change thereof). The main text only presents results from individual-level models; see Appendices 1.4 and 1.5 for aggregate models.

Results

Fairness perceptions

The within-subjects model fit to estimate the effect of the experimental conditions on fairness perceptions has the following structure, where standard errors are clustered at the session level (see Appendix 1.4 for results based on the within-subjects linear mixed-effects model):

$$\Delta fairness_{ij} = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + e_{ij} \quad (1.1)$$

The Greek letter Δ before the variables denote the fact that what is of interest here is the change in the variable from the first public goods game to the second one. For that reason, while the variables $earned_i$, $equal_i$, and $earned_i \times equal_i$ only take the values 0 and 1, the variables $\Delta earned_i$, $\Delta equal_i$, and $\Delta earned_i \times equal_i$ can take the values -1, 0, or 1. For example, when

[5] See Appendix 1.6 below for results from tie formation/breakage models. Interested readers can also refer to Appendix 1.7 for models with local inequality as the main predictor. These models are included here simply for the sake of completeness since they were listed in the OSF pre-registration document.

[6] Note that the researcher is operating in the frequentist framework here, not Bayesian.

$\Delta earned_i$ is 1, this means that participants in a given session were in a "random" condition in the first game and an "earned" condition in the second game.

Table 1.4 below presents results from this within-subjects model with change in fairness as the outcome variable. The model treats changes in conditions as a continuous variable (see Appendix 1.4 for results based on the within-subjects model that treats changes in conditions as a categorical variable).[7] As can be seen in this table, changing to an earned condition is estimated to have a large positive effect on fairness (≈1.4), while changing to an equal condition has a much smaller effect, about one third of the earned effect, on fairness (≈0.5). On the other hand, the estimated interaction effect is large and negative (≈-1.7), which indicates that the positive earned and equal effects disappear when changing to an earned *and* equal condition.

Table 1.4. Fairness perceptions as a function of the experimental conditions (within-subjects).

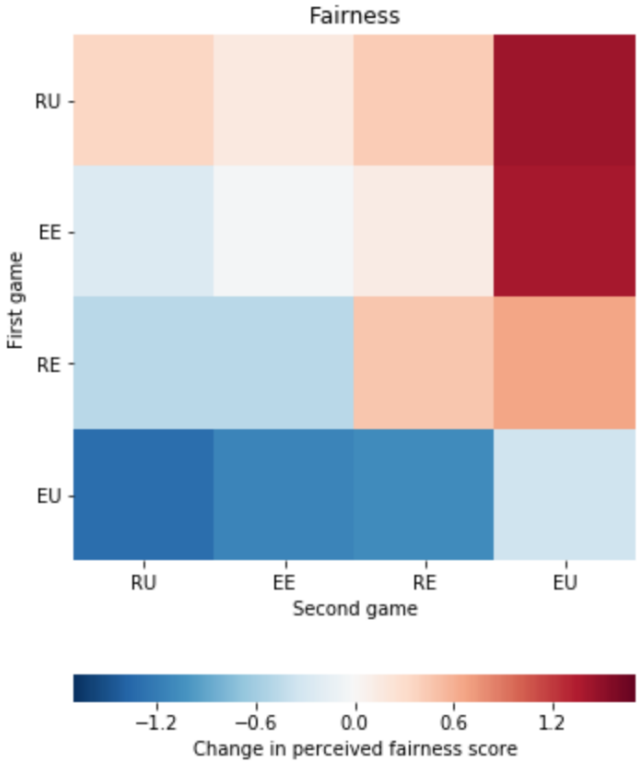
	Coefficient estimate	Standard error	P-value
Intercept	0.071	0.060	0.234
Δ Earned	1.386	0.131	<0.001
Δ Equal	0.482	0.143	0.001
Δ Earned x Equal	-1.706	0.175	<0.001

The number of samples (*n*) for the models fit is 1803, clustered inside 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

In addition to the model above, we can also fit a within-subjects model where the outcome is the change in fairness score from the first to the second game and the predictors are the 16 dummies corresponding to the 16 possible condition pairs (e.g., RU to EU is one possibility) without an intercept and standard errors clustered at the session level. The main advantage of this model is that its results allow for a nice visual presentation (it also allows us to clearly see order effects, such as, RU to EU vs. EU to RU). Accordingly, Figure 1.3 below presents a heatmap produced based on such a model. The columns denote the experimental condition in the first public goods game, while the rows denote the experimental condition in the second public goods game. As can be seen in this table, one prevalent pattern here is that while a change to EU significantly increases the fairness score, a change from EU to any other condition decreases the fairness score. Note that red corresponds to positive coefficient estimates, while blue corresponds to negative coefficient estimates.

Figure 1.3. Heatmap of change in fairness scores between games.

[7] Note that if the effect is symmetric (e.g., "Change to Earned" = -1 * "Change to Random", then the continuous estimator is more efficient, statistically. However, if the effect is not symmetric, then the dummy version will help us reveal that. Formal tests show that it is not possible to reject the hypothesis that the effects are symmetric, which means that the continuous estimator can be trusted. P-value = 0.683 in the earned case, and p-value = 0.898 in the equal case.



Cooperation patterns

The within-subjects model fit to estimate the effect of the experimental conditions on cooperation has the following structure, where standard errors are clustered at the session level (see Appendix 1.4 for results based on the within-subjects linear mixed-effects model):

$$\Delta cooperation_{ijl} = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + \delta_4 round_{il} + e_{ijl} \quad (1.2)$$

Once again, the Greek letter Δ before the variables denote the fact that what is of interest here is the change in the variable from the first public goods game to the second one. In particular, while $cooperation_{ijl}$ can only take the values 0 and 1, $\Delta cooperation_{ijl}$ can take the values -1, 0, and 1. Note also that $\delta_4 round_{il}$ is simply for notational convenience since the actual models fit include dummies for each round. The other predictors have the same structure as above in Equation 1.1.

Table 1.5 below presents results from this within-subjects model with change in cooperation as the outcome variable. The model treats changes in conditions as a continuous variable (see Appendix 1.4 for results based on the within-subjects model that treats changes in conditions as a categorical variable).[8] As can be seen in this table, changing to an earned

[8] Once again, formal tests show that it is not possible to reject the hypothesis that the effects are symmetric, which means that the continuous estimator can be trusted. P-value = 0.160 in the earned case, and p-value = 0.850 in the equal case.

condition has a significant negative impact on cooperation (≈-4 percentage points), while there is no indication of an equal effect nor an interaction between earned and equal.

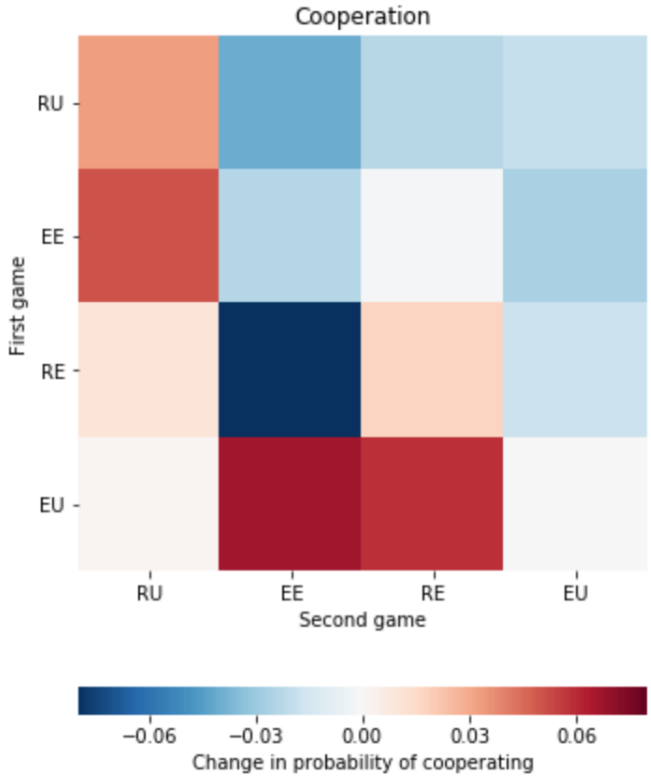
Table 1.5. Cooperation decision as a function of the experimental conditions (within-subjects).

	Coefficient estimate	Standard error	P-value
Intercept	-0.132	0.012	<0.001
Δ Earned	-0.042	0.016	0.010
Δ Equal	-0.002	0.016	0.881
Δ Earned x Equal	0.015	0.023	0.520

The number of samples (*n*) for the models fit is 16098, clustered inside 1803 players and 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Similar to what we did above with fairness perceptions, in addition to the model presented in Table 1.5, we can also fit a within-subjects model where the outcome is the change in cooperation from the first to the second game and the predictors are the 16 dummies corresponding to the 16 possible condition pairs without an intercept and standard errors clustered at the session level. Given the significant drop in cooperation from game 1 to game 2, this model also applies a zero-sum constraint on the estimated coefficients to adjust for this overall drop. The main observation to make here is that the directionality of the estimates are generally the opposite of those observed in Figure 1.3, which suggests that higher perceived fairness is associated with lower cooperation.

Figure 1.4. Heatmap of change in cooperation between games.



Discussion

An influential stream of research suggested that people have egalitarian motives, making them hostile towards inequality itself (Dawes et al 2007, Fehr, Bernhard, and Rockenbach 2008, Johnson et al 2009, Xiao and Bicchieri 2010). However, recent research has suggested that most people may care more about unfairness than inequality per se (Starmans, Sheskin, and Bloom 2017). The results from our fairness perceptions models provide no evidence for any inherent preference towards equality; on the contrary, one of the unequal conditions (earned unequal, EU) emerges as the most fair condition.

Importantly, EU is the condition where all players start the public goods game with whatever score they were able to win in the skill-based task, so this condition simulates a situation where everybody gets what they deserve based on their abilities. While EU is the most fair condition overall, random unequal (RU) appears to be the least fair; this is not surprising because random unequal simulates a situation where inequality is induced with no regard whatsoever to performance in the skill-based task (inequality as a result of pure luck). These results tell us that people do not necessarily consider unequal as unfair; what makes an unequal arrangement fair or unfair is the source of endowment.

Lastly, as far as the behavioral patterns related to cooperation are concerned, findings indicate that earned conditions lead to decreases in cooperation. This finding not only shows that source of endowment has an effect on public good contributions, but also hints at a potential entitlement effect in the sense that people become less willing to give to others if they feel entitled to their scores as in earned unequal. This interpretation is in line with earlier, mainly survey-based, evidence that shows that perceptions of individual effort, equal opportunity, or

simply a general sense of fairness make people less likely to support redistribution (Alesina and Angeletos 2005, Alesina and Ferrara 2005, Bjornskov et al 2013, Krawczyk 2010).

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How do perceptions of inequality and opportunity affect preferences for redistribution?

Ugur Yildirim[9]

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Abstract. Americans' distributional preferences are known to influence their political and voting behavior, but we do not know enough about the determinants of those preferences. How do perceptions of economic inequality and economic opportunity influence redistributive preferences? I answer this question using an innovative survey experiment that jointly manipulates perceptions of economic inequality and economic opportunity. The treatments are administered in the form of videos using a new *ask-then-tell* design, and the sample is gathered from a novel, high-quality source of online data. I find that receiving pessimistic information about inequality makes respondents more pessimistic about the state of inequality and more supportive of government involvement; on the other hand, the addition of pessimistic information about opportunity does not lead to any more concern for inequality or support for redistribution when pessimistic information about inequality is already present. Implications for future research are discussed.

inequality | opportunity | mobility | American Dream | redistribution

Introduction

Economic inequality in the US is high and on the rise (e.g., Piketty and Saez 2006, McCall and Percheski 2010, Alvaredo et al 2018). While Americans are not necessarily unaware or unconcerned about inequality (Bartels 2005, McCall 2013), we know that they generally prefer economic regimes that are at least somewhat unequal (Norton and Ariely 2011, Norton 2014), and that they are not particularly supportive of policies intended to address inequality, at least traditional government redistributive policies (Dallinger 2010, Shaw and Gaffey 2012, McCall 2013, McCall et al 2017).

Understanding the distributional preferences of the populace is important as it affects political and voting behavior (Fisman, Jakiela, and Kariv 2017). In the US context, it is difficult to study the effect of inequality on such preferences separate from opportunity -- and related concepts such as mobility and the American Dream -- due to the crucial role these concepts play in the American culture in shaping how people understand inequality and redistribution (McCall 2013, McCall et al 2017).

Considered separately, we know that perceptions of both inequality and opportunity should have an effect on preferences. However, we do not know enough about their *joint* effects. For example, it might be the case that while people are not bothered by inequality when there is also lots of opportunity, another situation where inequality is compounded with no opportunity could be really worrisome for most people and lead to demands for change. The joint effect could also be in the opposite direction if, for example, receiving too much pessimistic information -- "everything is going wrong in the country" -- makes people think that their own

[9] Ugur Yildirim (ugur.yildirim@berkeley.edu) is a Ph.D. candidate in the Department of Sociology, University of California-Berkeley. The author thanks Gabriel Lenz, David Harding, Daniel Schneider, Dennis Feehan, and Xinyi Zhang for all of their helpful comments, Narae Wadsworth for kindly taking the time to record the experimental texts used in the study, and UC Berkeley XLab, Don Moore in particular, for the generous research grant that funded this study.

situation is actually not that bad compared to others, which might lead them to report more optimistic opinions about the current state of affairs and less support for government action. Alternatively, too much pessimistic information might also lead to a similar response if it triggers some sort of a "throw up your hands" response in people.

Many studies separately looked at the effect of people's perceptions of economic inequality and opportunity and claimed that both factors should have an effect on Americans' policy preferences. The median voter hypothesis stipulated that as inequality rises, support for redistribution rises as well (Meltzer and Richard 1981). While Kenworthy and McCall (2008) found little empirical support for this hypothesis, Engelhardt and Wagener (2014), Niehues (2014), Hauser and Norton (2017), and Gimpelson and Triesman (2018) all make the same point -- and support it with empirical evidence -- that what matters when it comes to policy preferences is not actual but *perceived* inequality.

McCall (2013) too investigates the relationship between perceptions of inequality and support for government action against inequality but finds only weak evidence for this, at least in the US. In a similar vein, Kuziemko et al (2015) finds that while information about inequality has a significant effect on Americans' views on inequality, such information is mostly ineffective in moving policy preferences, with the exception of estate tax. On the other hand, McCall et al (2017) finds that exposure to information about rising inequality actually has a significant effect on respondents' support for policies that reduce economic inequality.

Parallel to this research that studied the effect of inequality perceptions on preferences, and inspired by some of the evidence that found weak to no inequality effect in this regard, other researchers started instead from the premise that people may care more about *something other than* economic inequality, such as economic mobility, opportunity, or simply a general sense of fairness (Starmans, Sheskin, and Bloom 2017). In particular, Alesina and Angeletos (2005) proposed a model that claimed that societies where it is believed that individual effort -- as opposed to luck, family, connections, and so on -- determines income tend to favor low redistribution and low taxes.

In line with this model, Alesina and Ferrara (2005) found that Americans who believe in the existence of equal opportunity are more likely to be against redistribution. Krawczyk (2010) reached a similar conclusion that people are less willing to give to others when the community rules emphasize effort as opposed to luck. Similarly, Bjornskov et al (2013) discovered that there is a negative association between fairness perceptions and demand for equal incomes. Finally, Shariff, Wiwad, and Aknin (2016) found that perceptions of upward mobility make people more tolerant of inequality, while Alesina, Stantcheva, and Teso (2018) found that receiving pessimistic information about mobility makes respondents more supportive of redistribution.

McCall et al (2017) study inequality in conjunction with opportunity, however, their causal story has a clear order effect such that inequality perceptions affect opportunity perceptions, which then affect policy preferences. In other words, in their study, inequality and opportunity perceptions are not *jointly* randomized; rather, they *randomize* inequality perceptions and then *observe* how that affects opportunity perceptions and policy preferences. This study, on the other hand, theorizes inequality and opportunity as independent effects -- both randomized for each respondent -- with the aim of clearly disentangling the main as well as interaction effects related to each.

Accordingly, this study presents results from a survey experiment that manipulates perceptions of both economic inequality and economic opportunity. The treatments are administered using an innovative *ask-then-tell* design that guards against the known danger of

respondents rejecting treatments that are too obvious or intrusive. Furthermore, given the limitations of presenting the informational treatment in the form of a text to be read -- which usually fails to capture respondents' attention and results in null effect estimates -- this study instead presents the information treatment using video as its medium. It also uses a new, high-quality source of online data that is representative of the general population on multiple attributes.

The study has two main findings. First, inequality treatments tend to have larger direct effects -- compared to opportunity treatments -- on respondents' perceptions regarding the state of inequality in the country, general attitudes towards inequality, and concrete policy preferences. Second, and more interesting, while receiving pessimistic information about inequality makes respondents more pessimistic about the state of inequality and more supportive of government involvement, the addition of pessimistic information about opportunity does not lead to any more concern for inequality or support for redistribution when pessimistic information about inequality is already present.

The paper is structured as follows. The next section describes the methodological aspects of the study including experimental design, implementation, subject recruitment, sample characteristics, data structure, and statistical models. The section after that presents the main results of the study related to (i) perceptions, (ii) attitudes, and (iii) policy preferences. Finally, the last section discusses the main findings of the study, touches upon some of its limitations, and mentions ideas for future research.

Methods

Experimental design

The study is designed as a 2x2 factorial between-subjects survey experiment. The first factor is whether a respondent receives pessimistic or optimistic information about the state of economic *inequality* in the country. The second factor is whether the respondent receives pessimistic or optimistic information about the state of economic *opportunity* in the country (economic opportunity is operationalized primarily in reference to inter-generational mobility and the American Dream ideology). These two factors produce four experimental conditions (Table 2.1): (i) inequality optimistic, opportunity optimistic (OO); (ii) inequality optimistic, opportunity pessimistic (OP); (iii) inequality pessimistic, opportunity optimistic (PO); and (iv) inequality pessimistic, opportunity pessimistic (PP).[10]

Table 2.1. Experimental conditions.

	Opportunity optimistic	Opportunity pessimistic
Inequality optimistic	OO	OP
Inequality pessimistic	PO	PP

The experiment flows as follows. First, respondents are recruited into the study and asked to give their consent. (At this stage, respondents are told that the goal of the survey is to "understand the public's knowledge and opinions regarding important societal and economic trends in the US." This general wording is chosen over using specific words such as inequality,

[10] See Appendix 2.1 for the experimental texts, images, videos, and other related content used in the study.

opportunity, or redistribution in an attempt to make sure respondents are not primed to think about these issues from the start.) Second, they are quizzed on two relatively neutral topics (same for all respondents) and asked to watch short video clips for the answers. Third, they are quizzed on their knowledge on the state of income inequality and economic mobility in the country (the order in which the questions are asked is randomized). Upon giving their responses, the respondents are asked to watch short clips for the answers; the content of these clips depend on the experimental condition respondents are in.

Fourth, respondents answer a series of questions designed to capture their attitudes and preferences towards inequality and what to do about it (the order in which these questions are asked is also randomized). Fifth, the respondents answer two questions specifically designed to determine whether the experimental manipulations actually succeeded in changing their opinions regarding the state of economic inequality and mobility in the country (order randomized). Finally, the respondents answer a series of demographic questions, including their income and political orientation. (Most of the questions related to attitudes, preferences, and demographics as well as a large chunk of the consent text are taken directly from the study by Kuziemko et al 2015.)

Implementation and subject recruitment

The survey experiment is implemented using Qualtrics. The videos presented to respondents as part of the experiment are prepared using iMovie and subsequently uploaded to a YouTube channel specifically created for this project (videos are "unlisted", have comments disabled, and show subtitles by default). The texts narrated to respondents in the videos are recorded by a young female in her 20's speaking Standard American English. Female voice is chosen over male voice due to evidence that shows that people tend to find the female voice to be more credible (e.g., Siegel, Breazeal, and Norton 2009). All videos showed an Adobe Stock licensed image in the background related to the content of the narrated text. (The researcher confirmed that most respondents actually watched these videos by checking the number of YouTube "views" of each video.) The experimental texts themselves are written by the researcher after a careful reading of a series of liberal- as well as conservative- leaning news sources and research papers.

Data collection took place on Lucid Theorem. This platform gives researchers access to cheap, fast (thousands of responses within hours), and high quality data that is also nationally representative based on age, gender, ethnicity, and region. A recent scholarly work also validated the quality of Lucid samples (Coppock and McClellan 2019). Finally, while a platform like Amazon Mechanical Turk (mTurk) is highly susceptible to having respondents whose views are extremely entrenched and hard to sway due to having taken too many similar surveys, a relatively new and less used platform such as Lucid is much less susceptible to this phenomenon, and hence more suitable for this survey experiment. The project has IRB approval. (All code, materials, and de-identified data will be made public once the study is over.)

Sample characteristics and data structure

The survey experiment is run on a total of 2,800 respondents with approximately 700 respondents in each condition (Table 2.2). The four conditions appear to be balanced on the demographic covariates (Table 2.3), which gives us confidence that randomization worked as expected. All analyses are conducted on a dataset with the following simple structure: one row

per respondent and as many columns as there are variables. Respondents are required to be US residents and 18 or older.[11]

Table 2.2. Number of respondents by condition.

Condition	Number of respondents
Inequality optimistic, opportunity optimistic (OO)	713
Inequality optimistic, opportunity pessimistic (OP)	685
Inequality pessimistic, opportunity optimistic (PO)	697
Inequality pessimistic, opportunity pessimistic (PP)	705
Total	2,800

Table 2.3. Demographics by condition.

	OO	OP	PO	PP
Age	44.735	44.559	44.188	44.861
Gender				
Male	0.496	0.466	0.481	0.494
Female	0.501	0.531	0.518	0.502
Other	0.003	0.003	0.001	0.004
Marital status				
Single	0.586	0.587	0.594	0.596
Married	0.414	0.413	0.406	0.404
Has children living with them				
No	0.621	0.599	0.626	0.631
Yes	0.379	0.401	0.374	0.369
Ethnicity/race				
European American/White	0.673	0.689	0.679	0.692
African American/Black	0.119	0.139	0.122	0.123
Hispanic/Latino	0.109	0.080	0.103	0.091
Asian/Asian American	0.049	0.034	0.049	0.055
Other	0.049	0.058	0.047	0.038
Highest level of education				
Eighth Grade or Less	0.007	0.006	0.004	0.003
Some High School	0.035	0.035	0.042	0.034
High School Degree/GED	0.258	0.247	0.263	0.251
Some College	0.251	0.266	0.270	0.260
2-year College Degree	0.129	0.127	0.129	0.132
4-year College Degree	0.203	0.210	0.189	0.224

[11] See Appendix 2.2 for information on sample size calculations, a discussion of additional variables included in the dataset, and a distribution of respondents across US states.

Master's Degree	0.086	0.091	0.070	0.077
Doctoral Degree	0.017	0.004	0.014	0.010
Professional Degree (JD, MD, MBA)	0.014	0.015	0.019	0.010
Employment status				
Full-time employee	0.391	0.364	0.397	0.400
Part-time employee	0.095	0.104	0.105	0.087
Self-employed or small business owner	0.074	0.085	0.086	0.078
Unemployed and looking for work	0.102	0.096	0.072	0.098
Student	0.066	0.061	0.052	0.050
Not in labor force (for example: retired, or full-time parent)	0.271	0.291	0.288	0.288
Total household income before taxes				
$0 - $9,999	0.093	0.096	0.083	0.095
$10,000 - $14,999	0.052	0.048	0.067	0.052
$15,000 - $19,999	0.065	0.069	0.080	0.064
$20,000 - $29,999	0.123	0.128	0.122	0.122
$30,000 - $39,999	0.132	0.140	0.112	0.140
$40,000 - $49,999	0.108	0.101	0.099	0.104
$50,000 - $74,999	0.191	0.150	0.202	0.194
$75,000 - $99,999	0.098	0.107	0.086	0.106
$100,000 - $124,999	0.059	0.070	0.066	0.041
$125,000 - $149,999	0.028	0.045	0.036	0.038
$150,000 - $199,999	0.028	0.025	0.029	0.027
$200,000+	0.024	0.020	0.017	0.016
Liberal/conservative spectrum				
Very conservative	0.128	0.112	0.113	0.145
Conservative	0.210	0.207	0.221	0.209
Moderate	0.425	0.429	0.418	0.423
Liberal	0.164	0.171	0.169	0.162
Very liberal	0.073	0.080	0.079	0.062
Party identity				
Republican	0.292	0.280	0.310	0.315
Democrat	0.367	0.406	0.383	0.356
Independent	0.288	0.244	0.242	0.267
None	0.053	0.070	0.065	0.062

Age is in years. All other numbers presented are proportions.

Overview of statistical models used

The outcomes in this study include binary measures (e.g., whether the respondent favors promoting equal opportunity or equal outcomes) as well as ordered ones (e.g., how much government involvement the respondent is in favor of, on a scale of 1 to 5). Despite this, all models discussed in the main text are fitted using ordinary least squares (OLS) regression -- rather than (ordered) logit or probit -- for two reasons: (1) interaction terms in logit and probit models could be wrong (Ai and Norton 2003); and (2) OLS is easier to interpret and discuss. With these having said, in our case, conclusions do not change if non-linear models are used instead, which is reassuring.[12]

[12] See Appendix 2.3 for estimates from these alternative non-linear models.

Two separate models are fitted for each outcome. One model simply presents outcome means in the four experimental conditions (OO, OP, PO, PP); results from this model are very transparent and easy to interpret, and the figures presented in the Results section (Figures 2.1-2.4) are based on this model. The other model presents estimated effects associated with (i) inequality being pessimistic, (ii) opportunity being pessimistic, and (iii) the interaction of i and ii; results from this model are reported in the Results section when discussing inequality and opportunity effects because this is the model that directly allows us to make inferences about the direct and interactions effects (i, ii, iii) associated with these factors. Since the inclusion of demographic covariates does not change our conclusions, the main text only discusses models without these covariates.[13]

Results

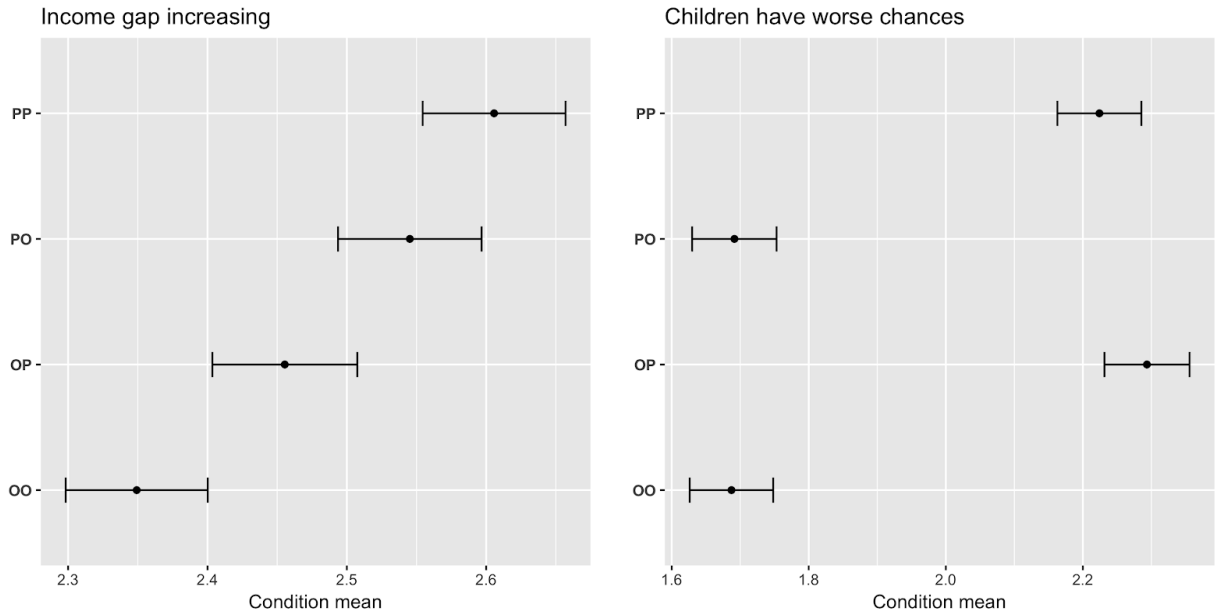
Perceptions regarding inequality, opportunity, and mobility

The first set of models fit to data estimate the effect of the experimental conditions on respondents' perceptions regarding the state of inequality and mobility in the country. The outcomes here are (i) respondents' sense of whether the income gap between richer and poorer Americans are decreasing (1), staying the same (2), or increasing (3); and (ii) respondents' sense of whether American children today have better (1), same (2), or worse (3) chances economically compared to their parents. These two outcomes are deliberately worded to reflect the core ideas in the two informational treatment axes so that they can be used to confirm that the experiment successfully moved respondents' opinions in the expected direction.

Results strongly suggest that the experiment worked as planned. Receiving pessimistic information about inequality makes respondents more likely to say that the gap between the rich and the poor is increasing in the country (coefficient estimate: 0.196, p-value: <0.001); receiving pessimistic information about opportunity has a smaller -- about half as large -- effect on this outcome (coefficient estimate: 0.106, p-value: 0.004). On the other hand, receiving pessimistic information about opportunity makes respondents more likely to say that children today have worse chances economically compared to parents (coefficient estimate: 0.606, p-value: <0.001); there is no significant inequality effect here (coefficient estimate: 0.004, p-value: 0.923). See Figure 2.1 for the outcome means in the four conditions corresponding to these estimated inequality and opportunity effects.

Figure 2.1. Perceptions regarding inequality and opportunity (predicted means).

[13] See Appendix 2.4 for tables with estimated coefficients, standard errors, and p-values; results both with and without demographic covariates are presented for the sake of transparency (Lenz and Sahn 2020). See Appendix 2.5 for additional models that further interact experimental manipulations with political orientation.

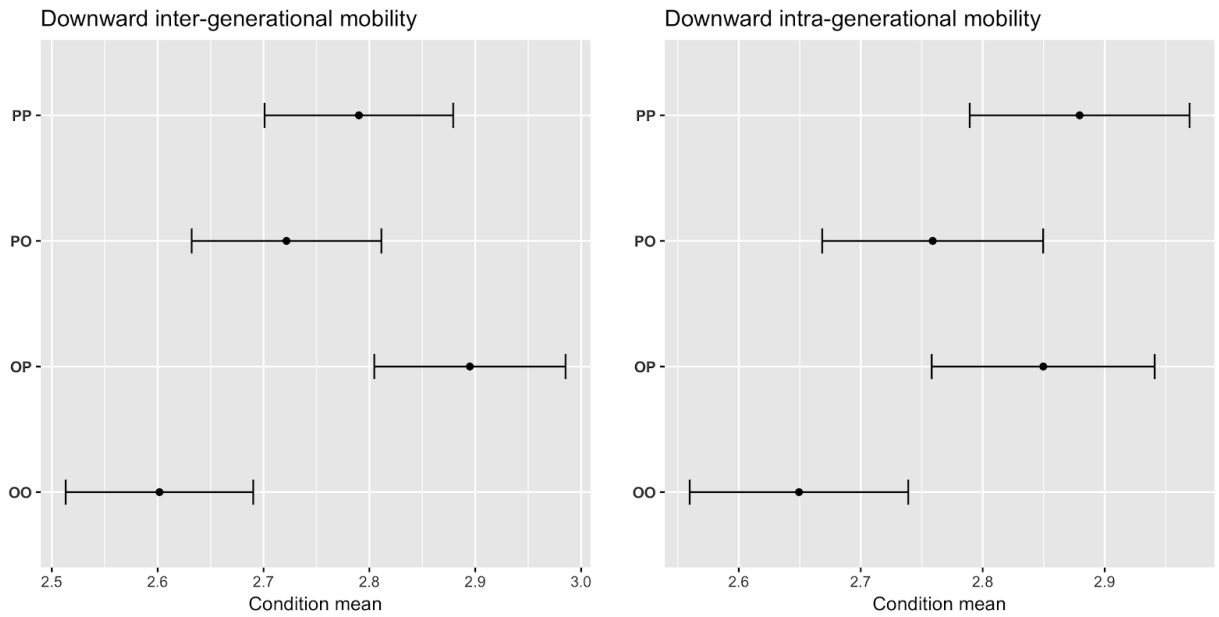


The bars denote 95% confidence intervals. PP: inequality pessimistic, opportunity pessimistic; PO: inequality pessimistic, opportunity optimistic; OP: inequality optimistic, opportunity pessimistic; OO: inequality optimistic, opportunity optimistic.

In addition to these two key outcomes, additional models were fit to data to understand the effect of the experimental conditions on respondents' sense of their *own* mobility experience. The relevant outcomes here are (i) respondents' sense of whether their own standard of living is much better (1), somewhat better (2), about the same (3), somewhat worse (4), or much worse (5) compared to their parents (inter-generational mobility); and (ii) respondents' sense of whether their standard of living is much better (1), somewhat better (2), about the same (3), somewhat worse (4), or much worse (5) compared to 10 years ago (intra-generational mobility). (Note that these variables are coded such that higher values denote reports of *downward* mobility; this coding is chosen to ensure that the direction in which the estimates move are consistent across models.)

Results suggest that the experimental manipulations affected not just respondents' sense of the general state of inter-generational mobility and opportunity in the country (as the outcome related to economic chances of American children compared to parents showed above) but also their sense of their *own* mobility experience. In particular, receiving pessimistic information about opportunity made respondents more likely to report a more *downward* mobility experience (inter-generational mobility, coefficient estimate: 0.293, p-value: <0.001; intra-generational mobility, coefficient estimate: 0.200, p-value: 0.002). See Figure 2.2 for the outcome means in the four conditions corresponding to these estimated inequality and opportunity effects.

Figure 2.2. Perceptions regarding mobility (predicted means).



The bars denote 95% confidence intervals. PP: inequality pessimistic, opportunity pessimistic; PO: inequality pessimistic, opportunity optimistic; OP: inequality optimistic, opportunity pessimistic; OO: inequality optimistic, opportunity optimistic.

One interesting pattern in the case of inter-generational mobility is that the interaction effect associated with receiving pessimistic information about *both* inequality and opportunity is negative and significant (coefficient estimate: -0.225, p-value: 0.014), which indicates that pessimistic information along both axes actually made respondents *less* likely to choose a more *downward* inter-generational mobility category. This interesting pattern can also be seen by observing that the outcome mean is lower under PP (both pessimistic) compared to OP (only opportunity pessimistic) in the left panel of Figure 2.2.

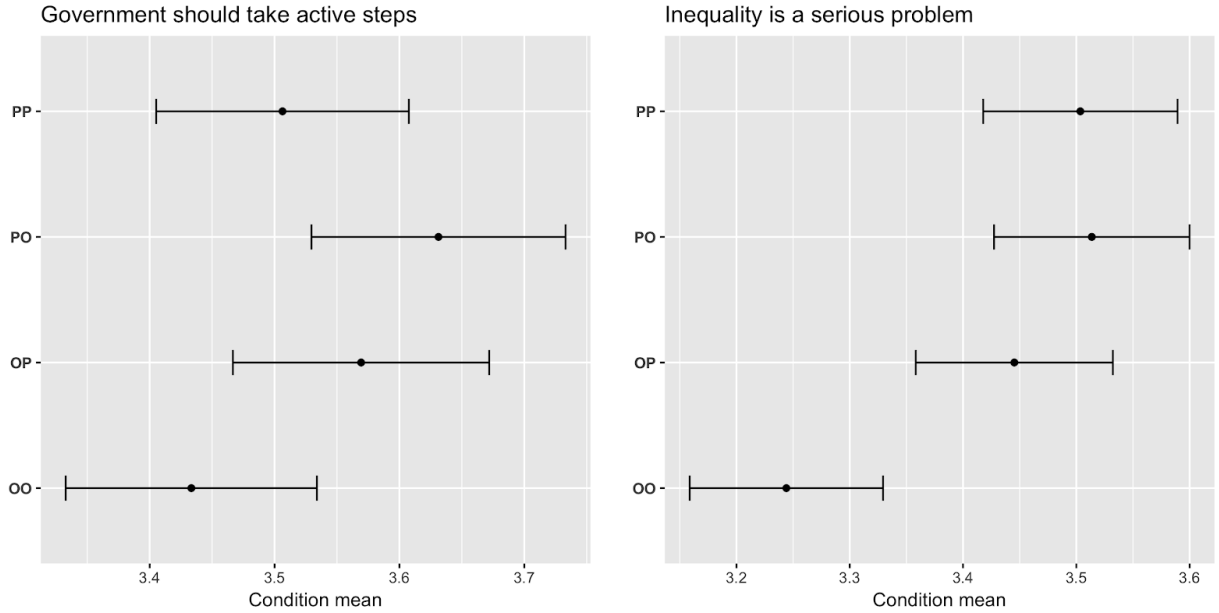
General attitudes towards inequality

The second set of models fit to data estimate the effect of the experimental conditions on respondents' general attitudes towards inequality and related concepts. The relevant outcomes here are (i) whether respondents think one's income and position in society has more to do with individual effort (0) or circumstances (1); (ii) whether respondents favor equal opportunity (0) or equal outcomes (1); (iii) respondents' opinions regarding the role of government vis-à-vis its citizens (1: provide basic functions only, 5: take active steps to improve the lives of citizens); (iv) whether respondents think inequality in America is a problem or not (1: not a problem at all, 5: a very serious problem); and (v) whether respondents think high earners deserve their high incomes most of the time (1), sometimes (2), or rarely (3).

Results show that while the experimental conditions did not have discernible effects on the (i) individual effort vs. circumstances, (ii) equal opportunity vs. equal outcomes, and (iii) high earners deserving or not outcomes (see Figure A2.4.1, Table A2.4.2), we have evidence that shows that the other two outcomes have been significantly affected by our experimental manipulations. More specifically, receiving pessimistic information about inequality leads respondents to (i) support more government involvement (coefficient estimate: 0.198, p-value: 0.007) and (ii) think that inequality is a serious problem (coefficient estimate: 0.270, p-value:

<0.001). The opportunity effect is also significant in the latter case, albeit smaller (coefficient estimate: 0.201, p-value: 0.001). See Figure 2.3 for the outcome means in the four conditions corresponding to these estimated inequality and opportunity effects.

Figure 2.3. General attitudes towards inequality (predicted means).



The bars denote 95% confidence intervals. PP: inequality pessimistic, opportunity pessimistic; PO: inequality pessimistic, opportunity optimistic; OP: inequality optimistic, opportunity pessimistic; OO: inequality optimistic, opportunity optimistic.

In both of these cases, the interaction is significant and negative (government involvement, coefficient estimate: -0.261, p-value: 0.012; inequality serious problem, coefficient estimate: -0.211, p-value: 0.016). Substantively, this means that receiving pessimistic information along both axes does not make respondents any more supportive of government involvement or more likely to think inequality is a serious problem when pessimistic information about inequality is already present. As a matter of fact, as can be seen in the left panel of Figure 2.3, in the case of government involvement, the interaction effect pushes the PP estimate to be statistically significantly lower compared to PO, which means that respondents are actually more supportive of government involvement when only inequality is pessimistic (PO) compared to when both inequality and opportunity is pessimistic (PP).

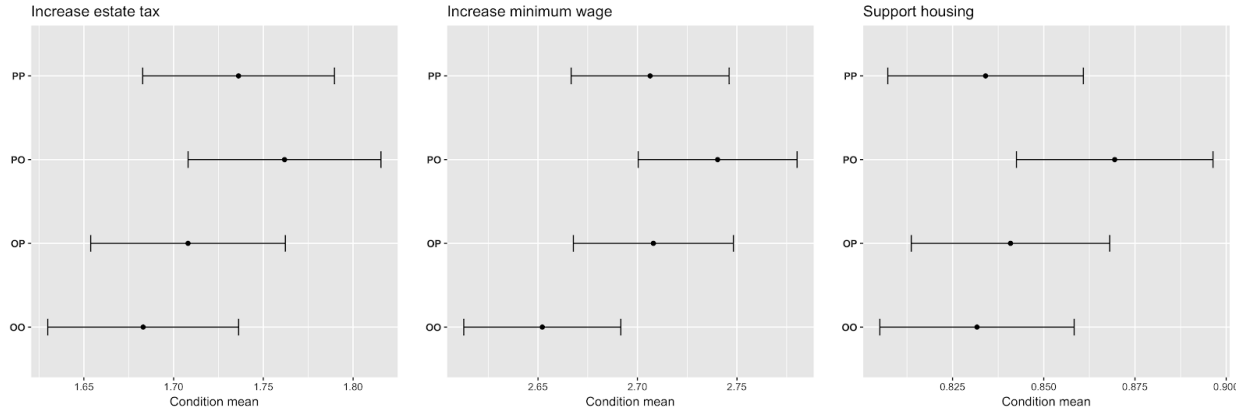
Concrete policy preferences

The third set of models fit to data estimate the effect of the experimental conditions on respondents' concrete preferences towards policies that address inequality. The survey experiment included the following seven policy outcomes: (i) whether respondents think taxes on millionaires should be decreased (1), stay the same (2), or increased (3); (ii) whether respondents think the estate tax should be decreased (1), stay the same (2), or increased (3); (iii) whether respondents think the minimum wage should be decreased (1), stay the same (2), or increased (3); (iv) whether respondents think aid to the poor should be decreased or increased (1: significantly decreased, 5: significantly increased); (v) whether respondents think government

spending on food stamps should be decreased or increased (1: significantly decreased, 5: significantly increased); (vi) whether respondents think government should support entrepreneurs (0: no, 1: yes); and (vii) whether respondents think government should support people in the face of high housing costs (0: no, 1: yes).

Results show that receiving pessimistic information about inequality leads respondents to support (i) increasing the estate tax (coefficient estimate: 0.079, p-value: 0.041), (ii) increasing the minimum wage (coefficient estimate: 0.088, p-value: 0.002), and (iii) government support in the face of high housing costs (coefficient estimate: 0.038, p-value: 0.051, barely insignificant). There is also a nearly significant opportunity effect (coefficient estimate: 0.056, p-value: 0.052) and a significant negative interaction effect (coefficient estimate: -0.090, p-value: 0.027) in the case of minimum wage. See Figure 2.4 for the outcome means in the four conditions corresponding to these estimated inequality and opportunity effects.

Figure 2.4. Concrete policy preferences (predicted means).



The bars denote 95% confidence intervals. PP: inequality pessimistic, opportunity pessimistic; PO: inequality pessimistic, opportunity optimistic; OP: inequality optimistic, opportunity pessimistic; OO: inequality optimistic, opportunity optimistic.

In all three of these outcomes, the PP estimate is lower compared to the PO estimate (statistically significant in the case of support housing), which indicates that receiving pessimistic information along both axes does not make respondents any more supportive of redistribution when pessimistic information about inequality is already present. On the contrary, the PP estimate tends to be lower compared to the PO estimate, though the difference is usually not statistically significant. This general pattern of the PP estimate being lower compared to the PO estimate emerges in five out of the seven policy outcomes (see Figure A2.4.1, Table A2.4.3 for the other four policy outcomes not presented here).

Discussion

The most interesting pattern observed in the results is that while receiving pessimistic information about inequality makes respondents more pessimistic about the state of inequality and more supportive of government involvement, the addition of pessimistic information about opportunity does not lead to any more concern for inequality or support for redistribution when pessimistic information about inequality is already present. One way to interpret these results is that the idea of high and rising inequality might automatically conjure up feelings of lack of opportunity in people regardless of whether opportunity is directly discussed or not. In other

words, even if an individual actually finds the combination of high inequality and no opportunity worrisome, pessimistic information regarding inequality might already imply a pessimistic state of opportunity, which would explain why pessimistic information about opportunity does not have much of an added impact.

Beyond this, some of the results further indicated that not only does pessimism along both axes not make respondents any more pessimistic about the state of inequality and supportive of government involvement, it might actually have the opposite effect. For example, evidence suggested that respondents are more supportive of government involvement when they have a pessimistic sense of inequality and an optimistic sense of opportunity compared to when their senses of both inequality and opportunity are pessimistic. One possible way to make sense of these unexpected results is that when respondents are made to believe that everything is going wrong in the country, this may make them think that their own situation is actually not that bad, which leads them to report more optimistic opinions and less support for government action. Results related to respondents' sense of their own inter-generational mobility experience support this view (left panel of Figure 2.2).

As far as the direct effects of the treatments on respondents' perceptions, attitudes, and policy preferences vis-à-vis inequality are concerned, the inequality axis is more influential compared to the opportunity one. While we have evidence that suggests that receiving pessimistic information about inequality and opportunity both make respondents more likely to think that (i) the income gap is widening, (ii) inequality is a serious problem, and (iii) the government should take a more active role in helping its citizens, the estimated inequality effects are consistently larger compared to the estimated opportunity effects. Furthermore, when it comes to concrete policy preferences, while no significant opportunity effects are detected, receiving pessimistic information about inequality makes respondents more likely to support (i) increasing the estate tax, (ii) increasing the minimum wage, and (iii) government helping people in the face of high housing costs.

There is no indication that the opportunity manipulation was inherently weak -- which could have explained the small and null effect estimates -- as this manipulation had strong effects on outcomes related to mobility (right panel of Figure 2.1, left and right panels of Figure 2.2). While there is no indication that the inequality treatment had any effect on respondents' sense of whether children today have better or worse chances economically compared to their parents, the opportunity treatment -- which explicitly refers to inter-generational mobility -- had a big impact on this, making respondents more likely to think that children have worse chances. Similarly, the inequality treatment had at best a weak effect on respondents' sense of their *own* mobility experience, while receiving pessimistic information about the state of opportunity in the country made respondents more likely to report downward mobility.

As far as the limitations of the study are concerned, the fact that the study does not have *neutral* conditions could be seen as a shortcoming in the sense that none of the conditions is explicitly a "control." Accordingly, future studies might consider also including neutral conditions in addition to the optimistic and pessimistic conditions tried in this study. Of course, the main challenge there is to find informational treatments that are truly neutral such that the estimated neutral vs. optimistic and neutral vs. pessimistic effects could be trusted. Another possible limitation of the study is that it is not based on a probability sample. While the Lucid sample used in this study is representative of the population on a number of attributes, it is still technically a convenience sample. Future researchers might consider trying to replicate the

findings in this study on a true probability sample to make inferences about the US populace easier.

Beyond these two potential shortcomings, it should also be remembered that perceptions of inequality and opportunity are not the only factors that affect a person's attitudes and preferences. As discussed in Kuziemko et al (2015), and before that in Bartels 2005, one possible reason why Americans might not support government action on inequality is that they fail to make the connection between social issues and public policy. Another reason, also discussed in Kuziemko et al (2015), could be related to Americans' low trust in government. It is probably worthwhile to estimate both of these effects -- ability to make connections between social issues and public policy and level trust in government -- in a similar experiment such as the one used in this study.

Finally, I expect my finding regarding the interesting way in which perceptions of inequality and opportunity interact to influence general attitudes towards inequality and concrete policy preferences to open up a wealth of new questions to investigate. For example, when does pessimistic information about society lead an individual to become pessimistic about their *own* situation? And, does pessimism ever become too much so that the individual loses their faith in the possibility of fixing the system?

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Perceptions of Inequality During the Coronavirus Outbreak

Ugur Yildirim[14]

December 1, 2020

Abstract. It is known that Americans' preferences for redistribution are generally not very elastic in relation to their perceptions of inequality. Even localized crises such as Hurricane Katrina that lay bare existing inequalities in society seem to do little to nothing in moving public opinion on this matter. However, the coronavirus pandemic presents a new opportunity for social scientists and policy experts to test whether large-scale national crises can lead to changes in people's opinions. What is the impact of a crisis of this proportion on Americans' attitudes towards inequality? More specifically, is there an "added value" to being informed about class inequalities *in the context of the coronavirus outbreak* compared to being informed about such inequalities in general terms without reference to this extraordinary event? This study answers these questions using an online experiment that manipulates the information respondents receive prior to answering survey questions. I find that receiving information about class inequalities *specifically in relation to the outbreak* tends to be much more effective in moving people's opinions compared to receiving that information in a way that does not directly relate it to coronavirus. This suggests that attitudes can be moved by something as widespread and salient as the pandemic.

coronavirus (COVID-19) | public opinion | inequality | redistribution | survey experiment

Introduction

Attitudes towards inequality is a key component of Americans' distributional preferences, which have been shown to affect their political and voting behavior (Fisman, Jakiela, and Kariv 2017). We know that Americans generally prefer economic regimes that are at least somewhat unequal (Norton and Ariely 2011, Norton 2014), and that they are not particularly supportive of policies intended to address inequality, at least traditional government redistributive policies (Dallinger 2010, McCall 2013, McCall et al 2017).

Evidence suggests that even large events such as the Hurricane Katrina disaster that struck New Orleans in 2005 and exposed racial and class inequalities in the US (Bobo 2006, Belkhir and Charlemaine 2007) might not be enough to change Americans' attitudes towards inequality and redistribution. For example, Grusky and Ryo (2006) found that Hurricane Katrina did not have much of an impact on raising awareness and activism in relation to inequality and poverty in the US. Similarly, Sweeney (2006) found that even after Hurricane Katrina, most Americans remained "color-blind" towards inequality.

Part of the reason why Americans' views about inequality are generally not very elastic to information is that inequality (un)awareness is rarely the main driver of those views. In particular, it has been shown that low levels of support for redistribution are due not so much to unawareness or indifference as to the failure to connect social issues and public policy (Bartels 2005, Kuziemko et al 2015). Furthermore, given America's entrenched political divide, most Americans end up interpreting the information they receive in a way that aligns with their

[14] Ugur Yildirim (ugur.yildirim@berkeley.edu) is a Ph.D. candidate in the Department of Sociology, University of California-Berkeley. The author thanks David Harding, Daniel Schneider, Dennis Feehan, and Gabriel Lenz for all of their helpful comments, Anam Ahmed for kindly taking the time to record the experimental texts used in the study, and UC Berkeley XLab, Don Moore in particular, for the generous research grant that funded this study.

pre-existing views, and so a "liberal" message about inequality is unlikely to change the opinions of conservative Americans (Grusky and Ryo 2006, Sweeney 2006).

Given this background, is there reason to believe that the coronavirus outbreak might have an impact on Americans' views about inequality? The coronavirus pandemic that we are currently in the midst of resembles the natural disasters of the recent past, including Hurricane Katrina in New Orleans and Camp Fire in California, in terms of disproportionately affecting low-income people (Pirtle 2020, Ray and Rojas 2020, Von Braun, Zamagni, and Sorondo 2020). However, the current situation is unique in terms of its scope: as opposed to the localized crises of the past that primarily affected particular sites or states, the current crisis is a truly national, even global, one. This is important because opinions are more likely to move when people are directly affected by a given situation or when the concrete implications of the situation are made clear to the people (Connell et al 2006, Lowe et al 2006), and the coronavirus crisis has had a direct impact on the lives of almost everyone in the country.

What is the impact of a crisis of this proportion on Americans' attitudes towards inequality? More specifically, is there an "added value" to being informed about class inequalities *in the context of the coronavirus outbreak* compared to being informed about such inequalities in general terms without reference to this extraordinary event? This study answers these questions using an online experiment that manipulates the information respondents receive prior to answering survey questions. I find that receiving information about class inequalities *specifically in relation to the outbreak* tends to be much more effective in moving people's opinions compared to receiving that information in a way that does not directly relate it to coronavirus. This suggests that attitudes can be moved by something as widespread and salient as the pandemic.

The paper is organized as follows. The next section sets up the theoretical background underlying the study and fleshes out the hypotheses being tested. The section after that describes the methodological aspects of the study including experimental design, implementation, subject recruitment, measures, and analytical strategy. The following section presents the main results of the study related to inequality perceptions and redistributive preferences. Finally, the last section discusses the main findings of the study and mentions ideas for future research.

Theoretical background

Inequality is high and on the rise in many countries around the globe, including the US (Piketty and Saez 2006, McCall and Percheski 2010, Alvaredo et al 2018). One of the most tangible consequences of inequality is health disparities within and between countries (Murray, Kulkarni, and Ezzati 2005, Adler and Rehkopf 2008, Braveman et al 2010, Marmot 2015). Disease outbreaks such as the new coronavirus pandemic that we are currently in the midst of make health inequalities all the more apparent (Quinn and Kumar 2014).

In the US, while governors around the country issued shelter-in-place and stay-at-home orders in an attempt to "flatten the curve" (Thunstrom et al 2020), only some people are lucky enough to be able to follow these orders; others have to go out everyday due to the nature of their work or their financial status (Valentino-DeVries, Lu, and Dance 2020). Relatedly, minorities such as African Americans and low-income people in general are reported to have higher risks of mortality during the outbreak (Dyer 2020).

How is this situation affecting Americans' attitudes towards inequality? In particular, does becoming sensitized to class disparities "open people's eyes" to inequality and make them more supportive of redistribution? Or, does inequality awareness lead to a sort of "victim

blaming" whereby the disadvantaged groups are blamed for their situation? Preferences for redistribution has been studied extensively in the social science and policy literatures, where scholars have identified numerous factors that could affect these preferences and the demand for more equal incomes.

Perceptions of inequality

The first set of factors is related to actual and perceived levels of inequality in the country, including whether inequality is rising or not. The median voter hypothesis stipulated that as inequality rises, support for redistribution rises as well (Meltzer and Richard 1981). While Kenworthy and McCall (2008) found little empirical support for this hypothesis, Engelhardt and Wagener (2014), Niehues (2014), Hauser and Norton (2017), and Gimpelson and Triesman (2018) all make the same point -- and support it with empirical evidence -- that what matters when it comes to policy preferences is not actual but *perceived* inequality.

McCall (2013) too investigates the relationship between perceptions of inequality and support for government action against inequality but finds only weak evidence for this, at least in the US. In a similar vein, Kuziemko et al (2015) finds that while information about inequality has a significant effect on Americans' views on inequality, such information is mostly ineffective in moving policy preferences, with the exception of estate tax. On the other hand, McCall et al (2017) finds that exposure to information about rising inequality actually has a significant effect on respondents' support for policies that reduce economic inequality.

Perceptions of opportunity and related concepts

Parallel to this research that studied the effect of inequality perceptions on preferences, and inspired by some of the evidence that found weak to no inequality effect in this regard, other researchers started instead from the premise that people may care more about *something other than* economic inequality, such as economic mobility, opportunity, or simply a general sense of fairness (Starmans, Sheskin, and Bloom 2017) -- these constitute the second set of factors to be discussed. In particular, Alesina and Angeletos (2005) proposed a model that claimed that societies where it is believed that individual effort -- as opposed to luck, family, connections, and so on -- determines income tend to favor low redistribution and low taxes.

In line with this model, Alesina and La Ferrara (2005) found that Americans who believe in the existence of equal opportunity are more likely to be against redistribution. Krawczyk (2010) reached a similar conclusion that people are less willing to give to others when the community rules emphasize effort as opposed to luck. Similarly, Bjornskov et al (2013) discovered that there is a negative association between fairness perceptions and demand for equal incomes. Finally, Shariff, Wiwad, and Aknin (2016) found that perceptions of upward mobility[15] make people more tolerant of inequality, while Alesina, Stantcheva, and Teso (2018) found that receiving pessimistic information about mobility makes respondents more supportive of redistribution.[16]

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[15] See Benabou and Ok (2001) and Cojocaru (2014) for more information on the influential "prospect of upward mobility" (POUM) hypothesis.

[16] Beyond perceptions of mobility, the actual mobility experience might also influence preferences for redistribution through mechanisms such as upward mobility making an individual more effort-oriented and more likely to subscribe to the idea that income and position need to be earned, not handed out by others (Piketty 1995). Note that while there are several empirical findings related to mobility effects in the literature (Alesina and La Ferrara 2005, Corneo and Gruner 2002, Clark and D'Angelo 2009, Siedler and Sonnenberg 2012, Steele 2015, Gugushvili 2018, Jaime-Castillo and Marques-Perales 2019), the identification problem inherent to estimating

Inequality and government

The third set of factors relates specifically to Americans' perceptions of government and its role in society. As discussed in Kuziemko et al (2015), and before that in Bartels (2005), one possible reason why Americans might not support government action on inequality is that they fail to make the connection between social issues and public policy. Another reason, also discussed in Kuziemko et al (2015), could be related to Americans' low trust in government. Both of these ideas would explain the apparent discrepancy between Americans' concern for inequality and their unwillingness to do much about it (Bartels 2005, McCall 2013).

Socio-demographic factors

Beyond the three sets of core factors just discussed, many socio-demographic factors have also been claimed to influence preferences for redistribution. In particular, researchers have shown that high income is associated with less support for redistribution, while low income is associated with more support (Corneo and Gruner (2002), Alesina and La Ferrara 2005, Steele 2015). As discussed in Corneo and Gruner (2002), such a negative association between income and support for redistribution can be explained in reference to self-interest -- they call this the "homo oeconomicus effect."

In addition to income, political orientation has also been shown to influence redistributive preferences. The literature is in agreement that conservative people and Republicans are much less likely to support redistribution (Chambers, Swan, and Heesacker 2015, Reynolds and Xian 2014, Alesina, Stantcheva, and Teso 2018). Furthermore, as the debate on whether Americans over- or under-estimate mobility show (Chambers, Swan, and Heesacker 2015, Davidai and Gilovich 2015, Kraus and Tan 2015, Swan et al 2017), perceptions of mobility in the country are divided along political lines -- with conservatives being much more optimistic about the state of mobility -- which means that the effects of political orientation and mobility perceptions on redistribution are very much connected.

Finally, while income and political orientation emerge as the key socio-demographic predictors of redistributive preferences, other factors have also been found to be correlated with this outcome. For example, educated people are generally less supportive of redistribution, and females and minorities seem to be more supportive (Corneo and Gruner (2002), Alesina and La Ferrara 2005, Steele 2015).

Hypotheses

The fact that the coronavirus outbreak made existing inequalities in society all the more apparent provides social scientists with a unique opportunity to understand how awareness of these inequalities shapes Americans' attitudes towards such inequalities and preferences towards doing something about them. Based on earlier findings related to perceptions of inequality discussed above, it is reasonable to predict that:

Hypothesis 1a—Awareness of existing class inequalities in society will make Americans more concerned about inequality and more sympathetic to the situation of the disadvantaged.

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origin-destination-mobility effects, which is practically identical to the age-period-cohort identification problem, makes this exercise very tricky (Fosse and Pfeffer 2019, Fosse and Winship 2019a, Fosse and Winship 2019b).

On the other hand, while a positive outcome such as this is expected based on the literature, it is still possible that awareness of inequality will have the opposite effect through a mechanism such as victim blaming whereby the weak is blamed for their unfortunate situation (Martin 2001, Greenbaum 2015):

Hypothesis 1b—Awareness of existing class inequalities in society will make Americans less concerned about inequality and less sympathetic to the situation of the disadvantaged.

Regardless of which way the effect moves people's attitudes towards inequality, based on the important point from the literature that concern does not necessarily equal willingness to take action (Bartels 2005, McCall 2013), it is crucial to have separate hypotheses for preferences towards redistribution and reducing inequalities:

Hypothesis 2a—Awareness of existing class inequalities in society will make Americans more supportive of redistribution and reducing inequalities.

Hypothesis 2b—Awareness of existing class inequalities in society will make Americans less supportive of redistribution and reducing inequalities.

Once again, while the former hypothesis (*2a*) is more plausible based on the literature, the latter one (*2b*) is nevertheless listed to be explicit about the possibility of the victim blaming mechanism mentioned above. Finally, information about inequality specifically in relation to the coronavirus outbreak -- a potential "rupturing event" -- may lead to a sense of urgency on the side of the receiver (Hedge and Yousif 1992), which could in turn strengthen or amplify the effect of inequality on the outcome. Similar to the previous two sets of hypotheses, we can write down two separate hypotheses to remain agnostic to the possible direction of the effect. The following hypotheses argue that coronavirus will amplify effects related to concern for inequality:

Hypothesis 3a—Awareness of existing class inequalities directly in relation to the outbreak will make Americans even more concerned about inequality and even more sympathetic to the situation of the disadvantaged compared to awareness of these inequalities in general terms.

Hypothesis 3b—Awareness of existing class inequalities directly in relation to the outbreak will make Americans even less concerned about inequality and even less sympathetic to the situation of the disadvantaged compared to awareness of these inequalities in general terms.

And, the last set of hypotheses argue that coronavirus will amplify effects related to support for redistribution and reducing inequalities:

Hypothesis 4a—Awareness of existing class inequalities directly in relation to the outbreak will make Americans even more supportive of redistribution and reducing inequalities compared to awareness of these inequalities in general terms.

Hypothesis 4b—Awareness of existing class inequalities directly in relation to the outbreak will make Americans even less supportive of redistribution and reducing inequalities compared to awareness of these inequalities in general terms.

Methods

Experimental design

The study is designed as a between-subjects survey experiment. Given that (i) any discussion of the outbreak may or may not touch upon inequality, and (ii) any discussion of inequality may or may not relate to the outbreak directly, the experiment randomized each respondent into one of six conditions -- three of them having coronavirus-specific primes and the other three having general primes (Table 3.1).

The three coronavirus conditions correspond to the three possible framings of the pandemic: (1) the "equal pandemic" that is affecting all of us (coronavirus control); (2) the unequal pandemic that is especially hard on poor and low-income communities, minorities in particular ("coronavirus class inequality"); and (3) the unequal pandemic that is especially hard on the elderly and those with medical conditions ("coronavirus natural inequality"). Directly parallel to these are the three general conditions: (4) a brief history of the internet (control); (5) class inequality without any reference to coronavirus; and (6) natural inequality without any reference to coronavirus.[17]

Table 3.1. Experimental conditions.

	General information	Coronavirus information
No information about inequality	Control	Coronavirus control
Information about class inequality	Class inequality	Coronavirus class inequality
Information about natural inequality	Natural inequality	Coronavirus natural inequality

The experiment flows as follows. First, respondents are recruited into the study and asked to give their consent. (At this stage, respondents are told that the goal of the survey is to "understand the public's opinions regarding important societal and economic trends in the US." This general wording is chosen over using specific words such as coronavirus and inequality in an attempt to make sure respondents are not primed to think about these issues from the start.) Second, they are asked to watch a short clip with subtitles and told that the purpose of showing this video is to assess their comprehension skills; the content of the clips depends on the experimental condition respondents are in.

Third, right after watching the video, they are asked to briefly describe the content of the video using their own words. Fourth, they answer a series of questions related to their attitudes towards inequality, opportunity, deservingness, and redistribution. Fifth, the respondents answer a series of demographic questions, including their income and political orientation. Finally, respondents answer questions that are specifically related to the coronavirus outbreak.[18]

[17] The two natural inequality conditions are included here for the sake of completeness since they were part of the experiment that was run; however, results pertaining to them are not directly relevant to the main focus of this paper.

[18] See Appendix 3.1 for the experimental texts, images, videos, manipulation check question, survey questions, and other related project content.

Implementation and subject recruitment

The survey experiment is implemented using Qualtrics. The videos presented to respondents as part of the experiment are prepared using iMovie and subsequently uploaded to a YouTube channel created by the researcher (videos are "unlisted", have comments disabled, and show subtitles by default). All videos showed an Adobe Stock licensed image in the background related to the content of the narrated text. The experimental texts themselves are written by the researcher after a careful reading of relevant news articles and scientific communications.

The texts narrated to respondents in the videos are recorded by a young female in her 20's speaking Standard American English. Female voice is chosen over male voice due to evidence that shows that people tend to find the female voice to be more credible (Siegel, Breazeal, and Norton 2009). The narrated text is also displayed as actual text under the video in case the respondent experiences a problem watching the video or chooses not to watch. (As discussed later under Results, the researcher confirmed that most respondents watched and understood the videos.)

Data collection took place on Lucid Theorem. This platform gives researchers access to cheap, fast (thousands of responses within hours), and high quality data that is also nationally representative based on age, gender, ethnicity, and region. A recent scholarly work validated the quality of Lucid samples (Coppock and McClellan 2019). The experiment is run on a total of 5,249 Lucid respondents with approximately 875 respondents in each condition. The conditions appear to be balanced on the socio-demographic covariates, which gives us confidence that randomization worked as expected.[19] All analyses are conducted on a dataset with the following simple structure: one row per respondent and as many columns as there are variables. Respondents are required to be US residents and 18 or older. The project has IRB approval. (All code, materials, and de-identified data will be made public once the study is over.)

Measures

The study has a large number of outcomes that capture respondents' opinions regarding inequality, redistribution, and related concepts including opportunity, determinants of success, and deservingness. Outcomes related to attitudes towards inequality are (i) whether the respondent thinks income inequality is a serious problem; (ii) whether the respondent thinks poverty is a serious problem; and (iii) whether the respondent thinks unequal access to healthcare is a serious problem. All three of these outcomes are measured on a scale of 1 to 5 with higher values denoting more seriousness.

The study also has outcomes that capture respondents' preferences towards redistribution. One set of outcomes asks respondents to report how effective they think certain tools are to address inequality. The relevant outcomes here are (iv) whether the respondent thinks government regulation is effective; (v) whether the respondent thinks government transfers are effective; (vi) whether the respondent thinks progressive taxes are effective; (vii) whether the respondent thinks education policies are effective; and (viii) whether the respondent thinks private charity is effective. All three of these outcomes are measured on a scale of 1 to 5 with higher values denoting more effectiveness.

Respondents are also asked (ix) whether they think the government should reduce inequalities; (x) whether they think major companies should reduce inequalities; and (xi) who

[19] See Appendix 3.2 for information on sample size calculations, exact sample sizes by condition, and summary socio-demographics by condition.

they think has the greatest responsibility in reducing inequalities. The first two outcomes are measured on a scale of 1 to 7 with higher values denoting more support for reducing income and pay differences. The third variable is a forced choice question, which allows us to create separate binary variables for: income differences do not need to be reduced; low income individuals themselves; high income individuals themselves; private charity; major companies; and government.

In addition to these outcomes directly related inequality and redistribution, the survey has several outcomes that help us capture respondents' opinions towards related concepts. The opportunity outcomes are (xii) whether the respondent thinks Americans today have enough opportunities (on a scale of 1-3) and (xiii) whether they think Americans today have more or less opportunities compared to their parents (on a scale of 1-3). The main outcomes related to determinants of success are (xiv) whether the respondent thinks hard work or luck is more important for success (on a scale of 1-7) and (xv) whether they think people are poor because of laziness or because of an unfair society (on a scale of 1-7). The study also has two deservingness outcomes, which are (xvi) whether the respondent thinks high earners deserve their income incomes (on a scale of 1-5) and (xvii) whether they think low earners deserve their low incomes (on a scale of 1-5).

The main predictor in the study is the informational treatment. It can be thought of as a single variable with six levels, or it can be viewed as the combination of two separate variables, namely (1) whether the treatment relates to coronavirus or not, and (2) whether the treatment provides any information about natural or class inequalities. In addition to the main predictor, the survey includes a large number of socio-demographic covariates, including household income, political orientation, level of education, gender, and race, among others.

Analytical strategy

The main statistical model fit to data is a linear regression of the outcome on the experimental condition treated as a categorical variable with six levels. The general control condition is used as the reference category to be able to get estimates for coronavirus and inequality conditions. In other words, this model allows us to separately estimate effects associated with each of the five non-control conditions. Estimates presented in Tables 3.3 and 3.4 are based on this model. The general form of the model is described in Equation 3.1.

$$y = \beta_0 + \beta_1 cor\_control + \beta_2 class + \beta_3 cor\_class + \beta_4 natural + \beta_5 cor\_natural + \varepsilon \quad (3.1)$$

In addition to this main model, separate models are fit when appropriate to estimate effects associated with (i) receiving any kind of coronavirus information, that is, all three coronavirus conditions viewed as a whole, and (ii) receiving any kind of information about inequalities, that is, the two class inequality conditions and the two natural inequality conditions viewed as wholes. Estimates presented in Table 3.2 are based on these models. The general form of the models are described in Equations 3.2 and 3.3, respectively.

$$y = \gamma_0 + \gamma_1 coronavirus + \varepsilon \quad (3.2)$$

$$y = \delta_0 + \delta_1 class\_ineq + \delta_2 natural\_ineq + \varepsilon \quad (3.3)$$

In the case of all three of these models, results both with and without socio-demographic covariates are presented for the sake of transparency (Lenz and Sahn 2020). In general, given that the independent variable is randomly assigned to respondents, the two results are very similar.

Results

Does the informational treatment sensitize people to inequality?

Results show that respondents who received one of the coronavirus treatments were significantly less likely to say that high earners deserve their high incomes (coefficient estimate = -0.077, p-value = 0.002) (Table 3.2, *high deserve*). On the other hand, no significant treatment effects were observed with the outcome related to whether low earners deserve their low incomes (coefficient estimate = -0.022, p-value = 0.425) (Table 3.2, *low deserve*). While the effect sizes are substantially small, these results suggest that the coronavirus treatments could have slightly increased sensitivity towards inequality among respondents.

The class axis was also influential in moving people's opinions towards inequality. In particular, receiving one of the two class treatments made respondents significantly less likely to say that income differences do not need to be reduced (coefficient estimate = -0.0341, p-value < 0.001) (Table 3.2, *don't reduce*). Once again, the substantive effect here is small (around 3-4%). While the class treatments were able to slightly move attitudes towards income differences, no significant effects are observed regarding support for government or major companies actually reducing these income differences. In this case, the coefficient estimates are 0.018 (p-value = 0.792) for support for government reducing income differences (Table 3.2, *govt reduce*) and 0.071 (p-value = 0.258) for major companies reducing pay differences (Table 3.2, *comps reduce*). These results suggest that while the class treatments were able to make people slightly more concerned about inequality, they might not have been strong enough to move people's opinions towards redistribution.

Table 3.2. Does the informational treatment sensitize people to inequality?

Models w/o any socio-demographic covariates

	high deserve	low deserve	don't reduce	govt reduce	comps reduce
coronavirus	-0.077 (0.025)**	-0.022 (0.027)			
class ineq			-0.034 (0.009)***	0.018 (0.070)	0.071 (0.063)
natural ineq			0.002 (0.009)	-0.118 (0.070)·	-0.075 (0.063)

Models w/ socio-demographic covariates

	high deserve	low deserve	don't reduce	govt reduce	comps reduce
coronavirus	-0.085 (0.024)***	-0.022 (0.026)			
class ineq			-0.037 (0.009)***	0.061 (0.063)	0.102 (0.060)·

natural ineq			0.002 (0.009)	-0.123 (0.063)˙	-0.073 (0.060)

The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001. Socio-demographic covariates include: age, gender, marital status, whether respondent has children living with them, race, religion, education level, employment status, household income, income volatility, political orientation, party preference, frequency of following the news, and degree of belief in the scientific community.

Overall, based on the results presented in this subsection, we have some evidence to support *Hypothesis 1a* over *Hypothesis 1b*. On the other hand, the evidence is not strong enough to support either *Hypothesis 2a* or *Hypothesis 2b*, but the former one seems more likely given the direction in which the treatments moved preferences for reducing inequalities.

Does coronavirus amplify class effects?

Digging deeper into effects associated with each specific experimental condition showed that in most instances the general class treatment was not strong enough to move respondents' opinions, while the coronavirus class treatment led to significant shifts in opinion. First of all, as far as opportunity perceptions are concerned, the general class treatment failed to move either of the two opportunity outcomes. The coefficient estimates here are -0.004 (p-value = 0.887) for the enough opportunities question and -0.012 (p-value = 0.747) for the opportunities compared to the parents question. On the other hand, in both of these instances, the coronavirus class inequality conditions led to larger shifts in opinion, making respondents' opportunity perceptions more pessimistic. The corresponding coefficient estimates are -0.060 (p-value = 0.052) and (-0.089, p-value = 0.024) (Table 3.3, *enough opps*, *better opps*).

Similarly, the coronavirus class inequality condition moved respondents' inequality perceptions towards thinking that inequality is a serious problem, while the general class inequality condition was generally not strong enough. In particular, the coefficient estimates for inequality being a serious problem, poverty being a serious problem, and unequal access to healthcare being a serious problem are 0.155 (p-value = 0.006), 0.127 (p-value = 0.012), and 0.164 (p-value = 0.003) for coronavirus class inequality, while the estimates are 0.136 (p-value = 0.016), 0.069 (p-value = 0.169), and 0.073 (p-value = 0.185) for general class inequality. It is also important to note here that even the coronavirus control condition, which doesn't explicitly refer to inequality in any way, was able to significantly move respondents' inequality perceptions, with the corresponding coefficient estimates being 0.149 (p-value = 0.008), 0.119 (p-value = 0.018), and 0.134 (p-value = 0.016) (Table 3.3, *ineq problem*, *povty problem*, *hcare problem*). These results suggest that coronavirus, with or without a direct reference to inequality, made respondents more conscious of inequality.

Table 3.3. Does coronavirus amplify class effects? Inequality and opportunity perceptions.
Models w/o any socio-demographic covariates

	enough opps	better opps	ineq problem	povty problem	hcare problem
cor control	-0.008 (0.031)	-0.059 (0.040)	0.149 (0.057)**	0.119 (0.050)*	0.134 (0.056)*
class ineq	-0.004 (0.031)	-0.013 (0.039)	0.136 (0.056)*	0.069 (0.050)	0.073 (0.055)

cor class ineq	-0.061 (0.031)˙	-0.089 (0.040)*	0.155 (0.057)**	0.127 (0.050)*	0.164 (0.056)**
nat ineq	-0.041 (0.031)	-0.046 (0.040)	0.030 (0.057)	0.046 (0.050)	-0.004 (0.056)
cor nat ineq	-0.016 (0.031)	-0.003 (0.040)	0.078 (0.056)	0.036 (0.050)	0.075 (0.055)

Models w/ socio-demographic covariates

	enough opps	better opps	ineq problem	povty problem	hcare problem
cor control	-0.007 (0.030)	-0.057 (0.038)	0.113 (0.051)*	0.085 (0.046)˙	0.094 (0.050)˙
class ineq	-0.012 (0.030)	-0.022 (0.038)	0.130 (0.051)*	0.061 (0.046)	0.062 (0.050)
cor class ineq	-0.064 (0.030)*	-0.091 (0.039)*	0.162 (0.051)**	0.136 (0.046)**	0.166 (0.050)**
nat ineq	-0.036 (0.030)	-0.046 (0.038)	0.007 (0.051)	0.023 (0.046)	-0.029 (0.050)
cor nat ineq	-0.021 (0.030)	-0.019 (0.038)	0.063 (0.051)	0.020 (0.046)	0.054 (0.050)

The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001. Socio-demographic covariates include: age, gender, marital status, whether respondent has children living with them, race, religion, education level, employment status, household income, income volatility, political orientation, party preference, frequency of following the news, and degree of belief in the scientific community.

As far as opinions regarding concrete government tools to address inequality are concerned, results show that receiving information about class inequality in the context of the outbreak made respondents more likely to say that government transfers are effective (coefficient estimate = 0.209, p-value < 0.001). There is also some indication of a similar effect for government regulation being effective (coefficient estimate = 0.100, p-value = 0.081), while no significant effects were observed for progressive taxes being effective (0.075, p-value = 0.183). The general class inequality condition is not significant in either of these cases, with coefficient estimates being 0.066 (p-value = 0.247), 0.088 (p-value = 0.125), and 0.011 (p-value = 0.843), respectively (Table 3.4, *transfers eff*, *regulation eff*, *prog taxes eff*).

On the other hand, both class inequality conditions lead to significant increases in thinking that education policies and private charity are effective tools. In fact, the general class inequality condition actually has stronger effects in these areas with coefficient estimates being 0.124 (p-value = 0.018) and 0.132 (p-value = 0.014) for general class inequality and 0.103 (p-value = 0.051) and 0.093 (0.086) for coronavirus class inequality (Table 3.4, *educ pols eff*, *priv charity eff*).

Table 3.4. Does coronavirus amplify class effects? Policy preferences.

Models w/o any socio-demographic covariates

	transfers eff	regulation eff	prog taxes eff	educ pols eff	priv charity eff

cor control	0.085 (0.057)	-0.062 (0.057)	-0.011 (0.056)	0.004 (0.053)	0.052 (0.054)
class ineq	0.066 (0.057)	0.088 (0.057)	0.011 (0.056)	0.124 (0.053)*	0.133 (0.054)*
cor class ineq	0.209 (0.057)***	0.100 (0.057)˙	0.075 (0.056)	0.103 (0.053)˙	0.093 (0.054)˙
nat ineq	0.033 (0.057)	-0.003 (0.057)	-0.053 (0.056)	0.018 (0.053)	0.075 (0.054)
cor nat ineq	0.027 (0.057)	-0.030 (0.057)	-0.012 (0.056)	0.010 (0.053)	-0.0001 (0.054)

Models w/ socio-demographic covariates

	transfers eff	regulation eff	prog taxes eff	educ pols eff	priv charity eff
cor control	0.062 (0.054)	-0.088 (0.055)	-0.038 (0.052)	-0.024 (0.051)	0.038 (0.053)
class ineq	0.062 (0.053)	0.086 (0.054)	0.001 (0.052)	0.111 (0.051)*	0.132 (0.053)*
cor class ineq	0.229 (0.054)***	0.114 (0.055)*	0.079 (0.052)	0.102 (0.051)*	0.099 (0.053)˙
nat ineq	0.051 (0.054)	0.011 (0.054)	-0.052 (0.052)	0.020 (0.051)	0.094 (0.053)˙
cor nat ineq	0.017 (0.054)	-0.040 (0.054)	-0.024 (0.052)	-0.012 (0.051)	-0.012 (0.053)

The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001. Socio-demographic covariates include: age, gender, marital status, whether respondent has children living with them, race, religion, education level, employment status, household income, income volatility, political orientation, party preference, frequency of following the news, and degree of belief in the scientific community.

Results presented in this subsection support *Hypothesis 3a*. Furthermore, if we interpret opinions regarding how effective certain tools are in addressing inequality to reflect preferences for redistribution, *Hypothesis 4a* is supported as well. That said, the latter statement should be interpreted with caution since, as the previous subsection showed, the treatments were generally not strong enough to move preferences for reducing inequalities.

Discussion

It is known that Americans' preferences for redistribution are generally not very elastic in relation to their perceptions of inequality (McCall 2013, Kuziemko et al 2015). Even localized crises such as Hurricane Katrina that lay bare existing inequalities in society seem to do little to nothing in moving public opinion on this matter (Grusky and Ryo 2006, Sweeney 2006). However, the coronavirus pandemic presents a new opportunity for social scientists and policy experts to test whether large-scale national crises can lead to changes in people's opinions. What is the effect of discussing inequality in the context of coronavirus on Americans' attitudes towards inequality and redistribution?

The most interesting pattern observed in the results presented above is that being informed about class inequalities *in the context of the coronavirus outbreak* tends to be much more effective in moving people's opinions compared to being informed about such inequalities in general terms without reference to this extraordinary event. For example, while general information about inequality has no statistically detectable effect on whether respondents think unequal access to healthcare is a problem or not, information that explicitly connects inequality to coronavirus makes respondents significantly more likely to think that unequal access to healthcare is a serious problem.

Similarly, only when inequality is discussed in conjunction with the outbreak were the informational treatment strong enough to move respondents' opinions regarding whether government transfers are an effective tool to address inequality. On the other hand, no matter how the inequality information is presented -- with or without any reference to coronavirus -- the experiment did not have any detectable effects on concrete opinions as to whether the government, or major companies, should actually reduce income differences or not. This is perhaps not surprising given how entrenched Americans' redistributive preferences are.

Together, these findings suggest that the debate revolving around the coronavirus outbreak, especially when connections to inequality were made explicit, could increase concern about inequality among the public, and perhaps even shift opinions regarding specific government tools to address inequality -- government transfers in particular -- even if opinions regarding reducing inequalities remain entrenched. All of this implies that crisis times such as this one could present fruitful opportunities for politicians and opinion leaders to reframe the inequality debate in the country and get the message across to more Americans that inequality is a problem.

This study helps answer important questions about the impact of the coronavirus outbreak on the public's attitudes towards inequality, but it also raises new ones. In particular, it is unclear which of the effects observed in this study will persist over time. For example, it could simply be that the observed effect on preferences towards government transfers is special to the given situation when many people have been directly affected by the coronavirus stimulus checks. Will the coronavirus outbreak lead to a long-term increase in the belief that government transfers are effective? Even more fundamentally, will Americans maintain a higher level of concern for inequality once the coronavirus situation goes back to normal, or will everyone simply revert back to their old opinions? The answers to these questions will no doubt depend on how opinion leaders end up framing the debate.

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Disparate Impact Pandemic Framing Decreases Public Concern for Health Consequences

Ugur Yildirim[20]

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Abstract. It is known that the new coronavirus (COVID-19) is disproportionately affecting the elderly, those with underlying medical conditions, and the poor. What is the effect of informing the public about these inequalities on people's perceptions of threat and their sensitivity to the outbreak's human toll? This study answers this question using a novel survey experiment and finds that emphasis on the unequal aspect of the pandemic, especially as it relates to the elderly and those with medical conditions, could be causing the public to become less concerned about the outbreak and its human toll. Discussion situates this finding in the literature on scientific communication and persuasion and explains why language that emphasizes the impact of the virus on *all of us* -- rather than singling out certain groups -- could be more effective in increasing caution among the general public and make them take the situation more seriously.

coronavirus (COVID-19) | public opinion | inequality | survey experiment

Introduction

Within a few months after its first emergence in Wuhan, China in December 2019, the novel coronavirus (COVID-19) has spread to almost every country on earth, including the US (Van Bavel 2020). As of September 2020, the human toll of the disease worldwide is more than 30 million confirmed cases and nearly one million deaths (ArcGIS 2020). Very few disease outbreaks in history have had such a fast and widespread impact on humanity, with the closest example being the 1918 flu pandemic (Scott and Duncan 2001).

Despite the global nature of the outbreak that has impacted peoples of all sexes, races, and cultural backgrounds, it is known that the disease is not affecting everyone in the same way. In particular, the elderly and those with underlying medical conditions are at higher risk of severe illness due to the virus (Zhou et al 2020). Similarly, more infections and deaths are reported in poor and low-income communities compared to wealthier ones (Von Braun, Zamagni, and Sorondo 2020). Neither of these patterns are surprising given what we know about health disparities (Murray, Kulkarni, and Ezzati 2005, Adler and Rehkopf 2008, Braveman et al 2010, Marmot 2015, Boen, Keister, and Aronson 2020) and the unequal impact of epidemics on certain groups (Luk, Gross, and Thompson 2001, Quinn and Kumar 2014).

While the outbreak is far from having a uniform impact on different groups, the way the media and the scientific community is talking about the outbreak does not always touch upon this unequal aspect of the pandemic. Oftentimes, the account instead emphasizes the *equalizing* aspect of the pandemic, whereby the virus threatens all of us -- all Americans or the entirety of humanity -- regardless of our background (McNeil 2020). Other times, the discussion revolves specifically around how the pandemic has been especially hard on certain groups, such as the elderly and the sick (CDC 2020).

[20] Ugur Yildirim (ugur.yildirim@berkeley.edu) is a Ph.D. candidate in the Department of Sociology, University of California-Berkeley. The author thanks David Harding, Dennis Feehan, Daniel Schneider, Gabriel Lenz, Don Moore, and Xinyi Zhang for all of their helpful comments and Anam Ahmed for kindly taking the time to record the experimental texts used in the study.

How do these different framings of the pandemic affect the public opinion? In particular, is one framing more or less effective than the other in terms of how it influences whether or not the public sees the outbreak as a serious threat or not and whether it is more important to save lives or to save the economy as the outbreak unfolds? This study answers this question using a novel survey experiment and finds that emphasis on the unequal aspect of the pandemic, especially as it relates to the elderly and those with medical conditions, could be causing the public to become less concerned about the outbreak and its human toll. Discussion situates this finding in the literature on scientific communication and persuasion and explains why language that emphasizes the impact of the virus on *all of us* -- rather than singling out certain groups -- could be more effective in increasing caution among the general public and make them take the situation more seriously.

Materials and methods

The project has IRB approval from University of California-Berkeley (protocol type: Soc-Behav-Ed Exempt; protocol number: 2020-04-13247; protocol title: Perceptions of inequality during the coronavirus outbreak). Written consent was obtained from respondents at the start of the survey.

Experimental design

The study is designed as a between-subjects survey experiment. It randomized each respondent into one of three conditions corresponding to three possible framings of the pandemic: (1) the "equal pandemic" that is affecting all of us; (2) the unequal pandemic that is especially hard on the elderly and those with medical conditions ("natural inequality"); and (3) the unequal pandemic that is especially hard on poor and low-income communities, minorities in particular ("class and race inequality"). These conditions are chosen to reflect the ways that the pandemic is discussed in public discourse. Class and race disadvantage is included in the same treatment because this is usually the way the discussion is framed in public discourse -- see, e.g., the recent United States Joint Economic Committee report on coronavirus (Beyer 2020).

The experiment flows as follows. First, respondents are recruited into the study and asked to give their consent. (At this stage, respondents are told that the goal of the survey is to "understand the public's opinions regarding important societal and economic trends in the US." This general wording is chosen over using specific words such as coronavirus and inequality in an attempt to make sure respondents are not primed to think about these issues from the start.) Second, they are asked to watch a short clip with subtitles and told that the purpose of showing this video is to assess their comprehension skills; the content of the clips depends on the experimental condition respondents are in. Third, right after watching the video, they are asked to briefly describe the content of the video using their own words. Fourth, they answer a series of general questions related to their attitudes towards inequality as well as their socio-demographic characteristics such age, gender, race, and income.

Finally, respondents answer questions that are specifically related to the coronavirus outbreak. These questions include: (1) whether the respondent thinks the coronavirus is a serious threat to the American people or not; (2) whether the respondent thinks it is more important to save lives or to save the economy during this outbreak; how satisfied the respondent is with the way (3) their city, (4) their state, and (5) the federal government has been handling the coronavirus situation; (6) how the respondent has been affected by the coronavirus outbreak; and (7) how many times the respondent went outside in the past seven days.

Answers given to questions (1) and (2) constitute the main dependent variables in the study. Both variables take values between 1 and 5 with higher values denoting higher threat perceptions in the case of the first variable (1=not a threat at all, 2=a small threat, 3=a threat, 4=a serious threat, 5=a very serious threat) and attaching more importance to saving the economy over saving lives in the case of the second variable (1=saving lives must be the priority even if it means the economy will suffer, 2, 3, 4, 5=saving the economy must be the priority even if it means lives will be lost). Answers given to questions (3), (4), and (5) are similarly coded to take values between 1 and 5 with higher values denoting more satisfaction.

Multiple binary variables have been generated based on question (6), including whether the respondent or someone in the respondent's family (i) is at risk, (ii) has contracted the virus, (iii) lost their job due to the outbreak, or (iv) experienced a significant decrease in income due to the outbreak. The "at risk" variable is particularly important here because given that the current crisis is caused by a disease outbreak, those who are at risk of severe illness and death will likely view and respond to the crisis very differently compared to those who are not at risk. The variable based on Question (7) takes values between 0 and 7. (See Appendix 4.1 for the experimental texts, images, videos, manipulation check question, survey questions, and other related project content including additional variables and conditions. The study design is pre-registered, while the specific hypotheses tested in this paper are not.)

Implementation and subject recruitment

The survey experiment is implemented using Qualtrics. The videos presented to respondents as part of the experiment are prepared using iMovie and subsequently uploaded to a YouTube channel created by the researcher (videos are "unlisted", have comments disabled, and show subtitles by default). All videos showed an Adobe Stock licensed image in the background related to the content of the narrated text. The experimental texts themselves are written by the researcher after a careful reading of relevant news articles and scientific communications.

The texts narrated to respondents in the videos are recorded by a young female in her 20's speaking Standard American English. Female voice is chosen over male voice due to evidence that shows that people tend to find the female voice to be more credible (Siegel, Breazeal, and Norton 2009). The narrated text is also displayed as actual text under the video in case the respondent experiences a problem watching the video or chooses not to watch. (As discussed later under Results, the researcher confirmed that most respondents watched and understood the videos.)

Data collection took place on Lucid Theorem. This platform gives researchers access to cheap, fast (thousands of responses within hours), and high quality data that is also nationally representative based on age, gender, ethnicity, and region. A recent scholarly work also validated the quality of Lucid samples (Coppock and McClellan 2019). (All code, materials, and de-identified data will be made public once the study is over.)

Sample characteristics and data structure

The survey experiment is run on a total of 2,617 respondents with approximately 870 respondents in each condition. The three conditions appear to be balanced on the demographic covariates, which gives us confidence that randomization worked as expected. All analyses are conducted on a dataset with the following simple structure: one row per respondent and as many columns as there are variables. Respondents are required to be US residents and 18 or older. (See

Appendix 4.2 for information on sample size calculations, exact sample sizes by condition, and summary demographics by condition.)

Overview of statistical models used

Linear regression models are fit to data with the experimental condition as the independent variable. The equal pandemic condition is used as the reference category to be able to get estimates for the natural inequality and class and race inequality conditions. (Note that the choice of reference category is somewhat arbitrary as it can be reasonably argued that equal pandemic is actually the distinct frame here. Accordingly, additional models were fit to data -- see Appendix 4.3 -- that treat the inequality conditions as the reference category to estimate an equal pandemic effect. These additional models do not change our substantive conclusions at all but allow us to see the story from the opposite angle.)

Results from these models are presented in figures in the main text, rather than tables, to make reading easier. All figures include point estimates together with 95% confidence intervals. Since the inclusion of socio-demographic covariates does not change our conclusions -- this is not surprising as the independent variable is randomly assigned to respondents -- the main text only discusses models without these covariates. (See Appendix 4.3 for tables with estimated coefficients, standard errors, and p-values; results both with and without socio-demographic covariates are presented for the sake of transparency in line with recent scholarly work (Lenz and Sahn 2020). Models with additional outcomes as well as results based on ordinal logistic regression models -- which do not change the substantive conclusions discussed in the text -- are also presented.)

Results

Manipulation checks

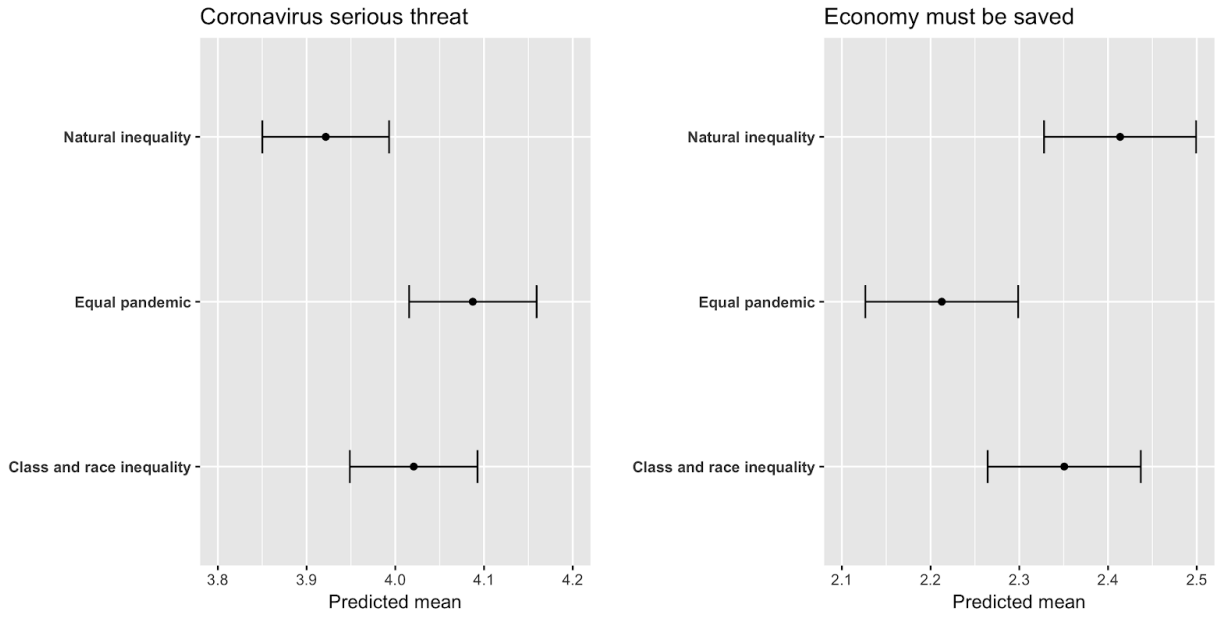
Manipulation checks are used in experimental research to determine whether the subjects actually received the treatments the researcher intended them to receive. The researcher confirmed that most respondents actually watched the videos by checking the number of YouTube "views" of each video. Most respondents also passed the manipulation check question, that is, clearly understood the text being communicated to them. (The researcher used a custom script to look for certain keywords such as "coronavirus" or "elderly" to make sure that respondents' description of the video was correct.) Furthermore, conclusions presented here remain unchanged regardless of whether or not we restrict the sample to only those respondents who passed the manipulation check.

Main findings

The experiment had a significant impact on respondents' opinions regarding whether coronavirus is a serious threat or not and whether the priority should be saving lives or saving the economy. As far as opinions regarding whether coronavirus is a serious threat or not are concerned, respondents who saw the natural inequality condition (which emphasizes how the pandemic has been especially hard on the elderly and those with medical conditions) reported significantly lower levels of threat perception compared to respondents who saw the control condition (coefficient estimate = -0.166, p-value = 0.001, see left panel of Figure 4.1). Regarding opinions as to whether the priority should be saving lives or saving the economy, respondents who saw the natural inequality condition reported significantly more support towards saving the economy

over saving lives compared to control (coefficient estimate = 0.201, p-value = 0.001, see right panel of Figure 4.1).

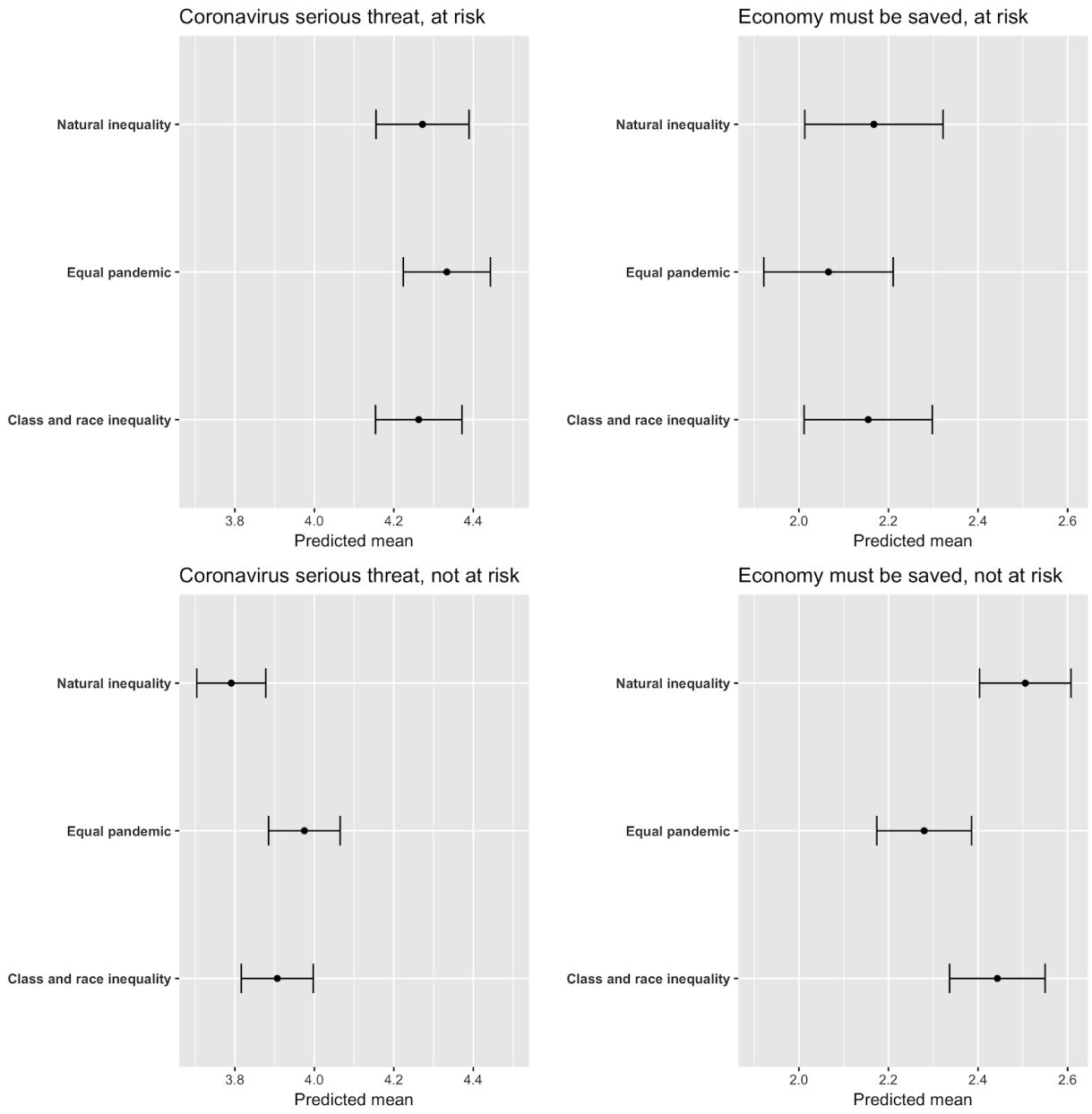
Figure 4.1. The effect of the informational treatment on outcomes.



The point estimates are predicted means. The bars denote 95% confidence intervals. *N*=2,617.

Digging deeper into these patterns revealed an interesting treatment-effect heterogeneity. Both of the effects discussed in the previous paragraph are mainly driven by respondents who are neither at risk themselves nor have family members who are at risk. Significant treatment effects are observed only in this not-at-risk group, while the treatment effect decreases in magnitude by more than half and loses statistical significance among respondents who are at risk or have at risk family members (see Figure 4.2). Effect heterogeneity is demonstrated by fitting separate models for at-risk and not-at-risk sub-groups (i.e., fitting two separate regressions of the outcome on the experimental conditions, one on the at-risk sample and the other on the not-at-risk sample). The natural inequality coefficient estimate for the outcome "coronavirus serious threat" is -0.061 (p-value = 0.454) for respondents at risk and -0.184 (p-value = 0.004) for respondents not at risk. Similarly, the natural inequality coefficient estimate for the outcome "economy must be saved" is 0.101 (p-value = 0.348) for respondents at risk and 0.226 (p-value = 0.003) for respondents not at risk.

Figure 4.2. Effect heterogeneity based on being at risk.



The point estimates are predicted means. The bars denote 95% confidence intervals. *N*=2,617.

In addition to the procedure described here to investigate effect heterogeneity, the researcher fitted additional, pooled models that explicitly modeled the outcome as a function of the experimental conditions, the at-risk variable, and interactions between the two. The interactions from these models are insignificant, which is not surprising because the experiment was not powered to be able to detect interaction effects. That said, the at-risk main effects are significant and both the at-risk main effects and interactions are in the expected direction (i.e., opposite of the treatment effects), which explains why the treatment effects are drastically smaller -- two to three times -- in the at-risk sub-sample. (See Appendix 4.3 for results based on the models with interactions.)

While the natural inequality condition led to significant changes in both outcomes, the class and race inequality condition was weaker in its effects. Despite the effect being in the same direction as natural inequality, class and race inequality led to significant changes only in the "economy must be saved" outcome. The class and race inequality coefficient estimates are -0.067 (p-value = 0.199) for the "coronavirus serious threat" outcome, which is less than half the magnitude of the natural inequality effect, and 0.138 (p-value = 0.027) for the "economy must be saved" outcome, which is about only two-thirds of the natural inequality effect. (The estimated effect gets smaller and statistical significance disappears when we control for the socio-demographic covariates.) On the other hand, data show that the class and race inequality condition had a nearly significant negative effect of -0.111 (p-value = 0.058) on satisfaction with state's handling of the coronavirus situation; no significant effects are observed for natural inequality or for the other two satisfaction outcomes (city and federal government).

Discussion

The information the public receives regarding the coronavirus outbreak influences their threat perceptions and whether they think saving the economy or saving lives should be the priority. Results from this study show that being informed about the disproportionate negative impact of the pandemic on the elderly and those with underlying medical conditions make people less likely to see coronavirus as a threat and more likely to prioritize saving the economy as opposed to saving lives, particularly among those who do not need to worry about themselves or someone in their family being at risk of severe illness.

These findings suggest that the dissemination of scientific information regarding the unequal impact of the pandemic on certain groups could actually be causing the general public to become less concerned about the outbreak and its human toll. The fact that the effect is primarily observed among people not at risk further indicate that when those people are sensitized to the situation of the weak they feel more secure about their own situation as not being at risk, which likely leads to increased optimism bias (Sharot 2011) and underestimation of their risk of infection (Wise et al 2020). These results give more support to mechanisms of deliberation and callousness as opposed to sympathy (Loewenstein and Small 2007, Small, Loewenstein, and Slovic 2007, Martin 2001).

While information regarding the disproportionate negative impact of the pandemic on the elderly and those with underlying medical conditions had a significant impact on coronavirus threat perceptions and preferences regarding whether saving lives or saving the economy should be the priority, information regarding the disproportionate negative impact of the pandemic on the poor and minorities did not have as big of an impact on the outcomes and generally failed to achieve statistical significance. One possible explanation for this null effect is that issues around class and race are highly politicized in the US, and so it is more difficult to move people's opinions on these topics compared to a more neutral and directly health-related topic such as the elderly and those with medical conditions.

The findings also have important policy implications. If the policy goal is to increase caution among the general public and make them take the situation more seriously, then information that emphasizes solidarity -- "we are all in this together" -- is likely to be much more effective, especially when it comes from a credible source (Haslam, Reicher, and Platow 2011, Brinol and Petty 2009). This solidarity framework should be employed even when informing the public about the unequal impact of the pandemic on certain groups, so that the general public is not left with the impression that the outbreak concerns only some -- not all -- of us.

Limitations

One of the limitations of the study is that the 'natural inequality' and 'class and race inequality' conditions are completely separate from one another by design. This is justified because the study is primarily concerned with how people understand the impact of the pandemic, not about the actual facts. That said, it is certainly the case that the poor and minorities are more likely to have medical conditions as a matter of science, and the current study does not look at this issue that concerns how 'natural inequality' and 'class and race inequality' angles intersect. Another, related limitation is that there are no separate conditions for 'class inequality' and 'race inequality' but rather the two forms of disadvantage are included under the same treatment, 'class and race inequality.' Once again, while this choice is justified by virtue of the fact that the framing is in line with the usual way the topic is discussed in public discourse, the literature on group cues (Nelson and Kinder 1996) tells us that whether the information is interpreted primarily in terms of class or race will likely influence the way respondents answer survey questions. A final limitation is that the custom script used for parsing the manipulation check question is developed by the researcher alone and was not independently verified prior to data collection.

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Conclusion

The overarching theme of this dissertation has been how inequality perceptions impact Americans' distributional and other-regarding preferences. The four chapters approached this general theme from a variety of angles, diverse in terms of both substance and methodology. The paragraphs that follow summarize the main findings presented in these chapters and discuss their broader implications.

The first chapter situated itself in the literature on inequality and fairness and sought to understand whether it is inequality *per se* that bothers Americans or whether it is rather the *source* of inequality that matters more in this regard. For this end, the chapter analyzed original data from a behavioral, networked experiment that manipulated the process through which (in)equality emerged in the network and observed how different economic regimes affected participants' fairness perceptions and their willingness to contribute a fraction of their wealth to other participants in the network.

Analyses conducted showed two main results. First, there was no evidence that equal regimes were inherently fair or unequal regimes were inherently unfair. Rather, results clearly showed that what made a certain economic regime fair or unfair was the process through which wealth came to be distributed among the participants. In particular, one of the unequal arrangements, the one where each participant started the experiment with whatever score they were able to *earn* in the skill-based task, emerged as the most fair economic regime. On the other hand, the other unequal arrangement, the one where scores based on the skill-based task were *randomly* shuffled and re-assigned to participants, ended up being the least fair regime.

The second main result was that not only did economic regimes where wealth was earned considered to be more fair by the participants, these regimes also led to lower levels of contributions to other players on average. This result is in line with earlier evidence that showed that a sense of fairness or feelings of entitlement to one's wealth makes it less likely that the individual would share that wealth with others (Alesina and Angeletos 2005, Alesina and Ferrara 2005, Bjornskov et al 2013, Krawczyk 2010).

In terms of their broader implications, these two results suggest that rather than having egalitarian motives (Dawes et al 2007, Fehr, Bernhard, and Rockenbach 2008, Johnson et al 2009, Xiao and Bicchieri 2010), Americans' attitudes towards inequality are more in line with the stream of thinking that it is unfairness, not inequality, that is problematic (Starmans, Sheskin, and Bloom 2017). Taken together with the finding that Americans tend to prefer economic regimes that are at least somewhat unequal (Norton and Ariely 2011, Norton 2014), these results would also help explain why even in the face of rising inequality (Piketty and Saez 2006, McCall and Percheski 2010), many Americans are not particularly supportive of policies intended to address inequality (Dallinger 2010, Shaw and Gaffey 2012, McCall 2013, McCall et al 2017).

The second chapter went beyond the simplified model of society that the first chapter was based on to study -- using a survey experiment -- how information regarding the state of inequality and opportunity in the US affected respondents' perceptions, attitudes, and preferences regarding concrete, real-life outcomes such as opinions on the right amount of government involvement or whether a certain tax should be increased, decreased, or stay the same. This approach is very different from the one taken in the first chapter because (i) the experimental treatments manipulate the information respondents receive from an external source (similar to the information one would receive from the media) rather than directly manipulating the economic regimes themselves, and (ii) the setup, treatments, and outcomes are all

unambiguously based on the US context as opposed to the abstract arrangement that the first chapter was based on.

The results based on this survey experiment indicated, first of all, that receiving pessimistic information related to the state of inequality in the country made respondents report more concern for inequality and a higher willingness to take action against it. However, beyond this expected finding (see, e.g., Kuziemko et al 2015), the analyses also showed that the addition of pessimistic information related to the state of opportunity did not lead to any more concern or willingness to take action when pessimistic information on inequality was already present. The chapter interpreted this result in reference to the intricate relationship between inequality and opportunity in the US context and suggested that pessimistic information about opportunity might not have much of an added impact if pessimistic information about inequality automatically conjures up feelings of no opportunity.

Another interesting result that emerged from the analyses was that not only did pessimism along both axes (inequality and opportunity) did not lead to any more concern or willingness to take action, in a few instances, it actually had the opposite effect. For example, data showed that respondents reported more support for government involvement when only the inequality information was pessimistic compared to when both inequality and opportunity informational treatments were pessimistic. Due to the highly unexpected nature of this finding, the chapter avoided reading too much into these patterns while noting that the results could be made sense of in reference to social comparison and mood (see, e.g., Wills 1981, Gibbons and Gerrard 1989, Johnson and Knobloch-Westerwick 2014): When respondents are made to believe that everything is going wrong in the country, this may make them think that their own situation is actually not that bad, which leads them to report more optimistic opinions and less support for government action. Results related to respondents' perceptions of their own inter-generational mobility experience supported this interpretation.

Regardless of whether the unexpected finding described in the previous paragraph is to be believed or not, the results showed clearly that pessimism along both axes did not have any added positive impact on raising concern for inequality. The broader implication of this result is that conflating the discussion of inequality with opportunity in public discourse might not have the intended effect on the populace, and, if the overall goal is to raise concern for inequality, informing the public about high and rising inequalities appears to be the most effective way to achieve this.

The final two chapters took the ideas discussed so far and applied them to the coronavirus context in the US. In particular, the third chapter studied, using another survey experiment, whether discussing inequality in the context of the coronavirus pandemic is more effective in raising concern for inequality compared to discussing inequality in general terms, without any explicit reference to coronavirus. It achieved this using an experimental design that included both general and coronavirus-specific treatments related to different aspects of inequality.

Results showed that there is indeed an "added value" to discussing inequality in the context of the pandemic. More specifically, compared to being informed about inequality in general terms, being informed about inequality specifically in relation to the pandemic appeared to be more effective in moving people's opinions towards increased concern for inequality. In fact, the coronavirus-specific treatments were even successful in moving opinions related to how effective government interventions, government transfers in particular, are in fighting inequality.

The broader implication of these results should be understood in relation to the known fact that Americans' views on inequality are notoriously entrenched and hard to move even in

the context of local crises such as the Hurricane Katrina disaster that brought existing class and race inequalities to the fore (Bobo 2006, Grusky and Ryo 2006, Sweeney 2006, Belkhir and Charlemaine 2007). While local crises seem to have little to no effect on moving people's opinions, results from my chapter provide evidence that large-scale national, even global, crises such as the coronavirus pandemic that touched the lives of essentially every single person in the country has the potential to shift people's opinions. This means that extraordinary times like these can be opportunities to reshape public opinion if opinion leaders succeed in bringing these issues to the public's attention using the right language.

The fourth and final chapter analyzed data based on the same coronavirus survey experiment to answer a different yet related question on the impact of inequality perceptions on the public's concern for the health consequences of the pandemic. This is achieved by studying how different framings of the pandemic -- related to whether the disparate impact of the pandemic on the elderly, those with medical conditions, the poor, and minorities is emphasized or not, as operationalized by the experimental conditions -- influence respondents' perceptions of threat in relation to coronavirus and whether they think it is more important to save lives or to save the economy. Given the tremendous human toll of the pandemic nationally and globally (ArcGIS 2020), understanding how the discussion of inequality in relation to the pandemic affects these two outcomes is of utmost importance to policymakers and science communicators.

Results presented in the final chapter showed that disparate impact pandemic framing led to decreases in public concern for health consequences of the outbreak. In particular, emphasis on how the pandemic had a disproportionately more negative impact on the elderly and those with medical conditions made respondents less likely to report that coronavirus is a serious threat and more likely to report that saving the economy should be the priority. The chapter further observed that this impact was especially strong among those respondents who are neither at risk themselves nor have any family members at risk. These results were interpreted as being more in line with the mechanisms of deliberation and callousness as opposed to sympathy (Loewenstein and Small 2007, Small, Loewenstein, and Slovic 2007, Martin 2001) and tied to an elevated optimism bias and underestimation of risk of infection among the group that is not at risk (Sharot 2011, Wise et al 2020).

On the other hand, emphasis on the unequal class and race effects associated with the pandemic did not have a large impact on the two outcomes of interest. This null result was interpreted in relation to the highly politicized nature of class and race in the US. In other words, given how politicized these issues are in the country, it is extremely hard to move people's opinions on these topics -- as earlier chapters have also touched upon, especially in relation to class -- compared to more neutral and directly health-related topics such as the elderly and those with medical conditions.

In terms of its policy implications, the final chapter sends a clear message that if we want the public to take the pandemic more seriously, we might be better off emphasizing how the pandemic is a threat to all of us, rather than specific groups such as the elderly or those with medical conditions. This suggestion is also in line with earlier evidence related to the effectiveness of the solidarity framework -- "we are all in this together" (Haslam, Reicher, and Platow 2011, Brinol and Petty 2009).

Taken together, the chapters that constitute this dissertation further our understanding of inequality in the US and show us how Americans' perceptions of inequality impact their distributional and other-regarding preferences. While it answers a number of questions related to this topic, it also raises new ones, and the hope is that future researchers will study and answer

these questions. Some of these questions are: What are the similarities and differences between how Americans view inequality, opportunity, and fairness compared to other nations? Under what circumstances does pessimistic information about societal problems, including inequality, cause an individual to become pessimistic about their *own* situation? Relatedly, when does such information lead to action, and when does it lead to inaction in relation to the problem at hand? What are the factors that determine whether short-term changes in opinion become permanent as opposed to people reverting back to their old beliefs after some time?

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Appendix 1.1: Breadboard content

Screening

Welcome to our task!

Click 'Next' to continue.

Before we begin...

Please answer the following question.

[number 1] plus [number 2] equals…

[ANSWER HERE]

Word game tutorial

Tutorial (1/9)

In this task, you will play **three games**.

Your final payment for this task will be determined **at the end of the third game**.

Note that **only those players who finish the entirety of the task and click 'Submit HIT' at the end will receive any form of payment.**

Note also that once the first game starts, **if you remain idle for more than 20 seconds, you will be dropped from this HIT and hence will be ineligible for any form of payment.**

Click 'Next' to continue.

Tutorial (2/9)

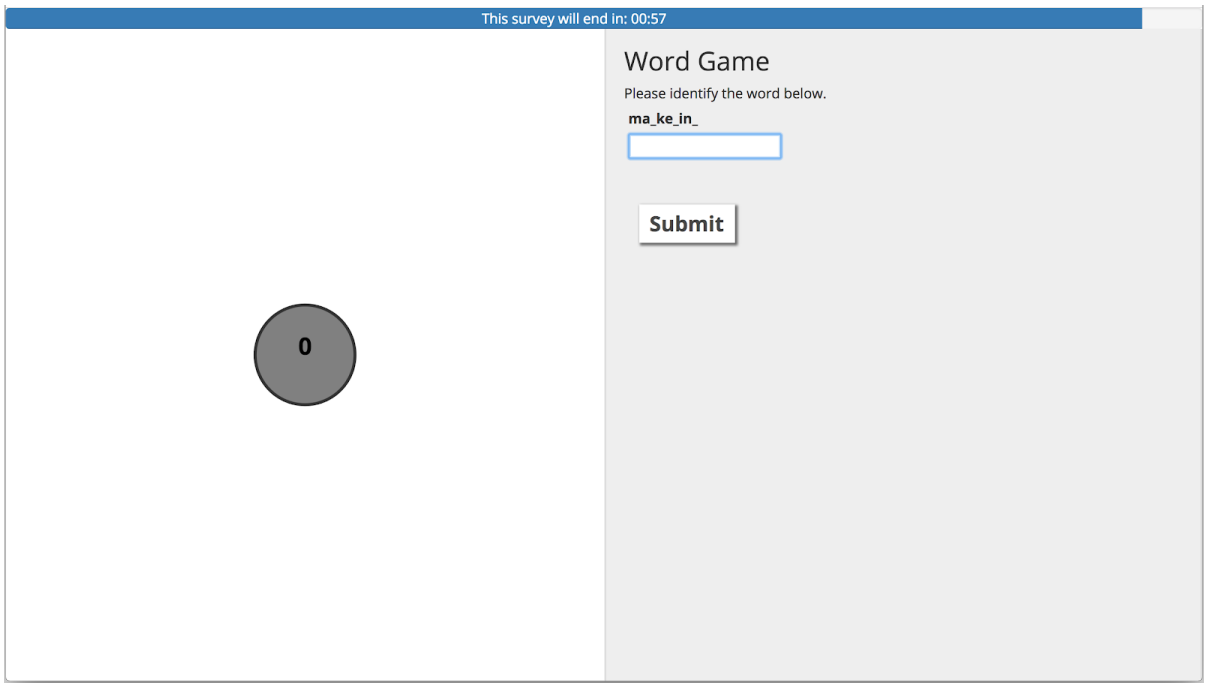
In the **first game**, you will be presented with a series of words with missing letters and asked to identify each word.

You start the **first game** with **0 points**, and each correct answer will add **100 points** to your score.

Click 'Next' to continue.

Tutorial (3/9)

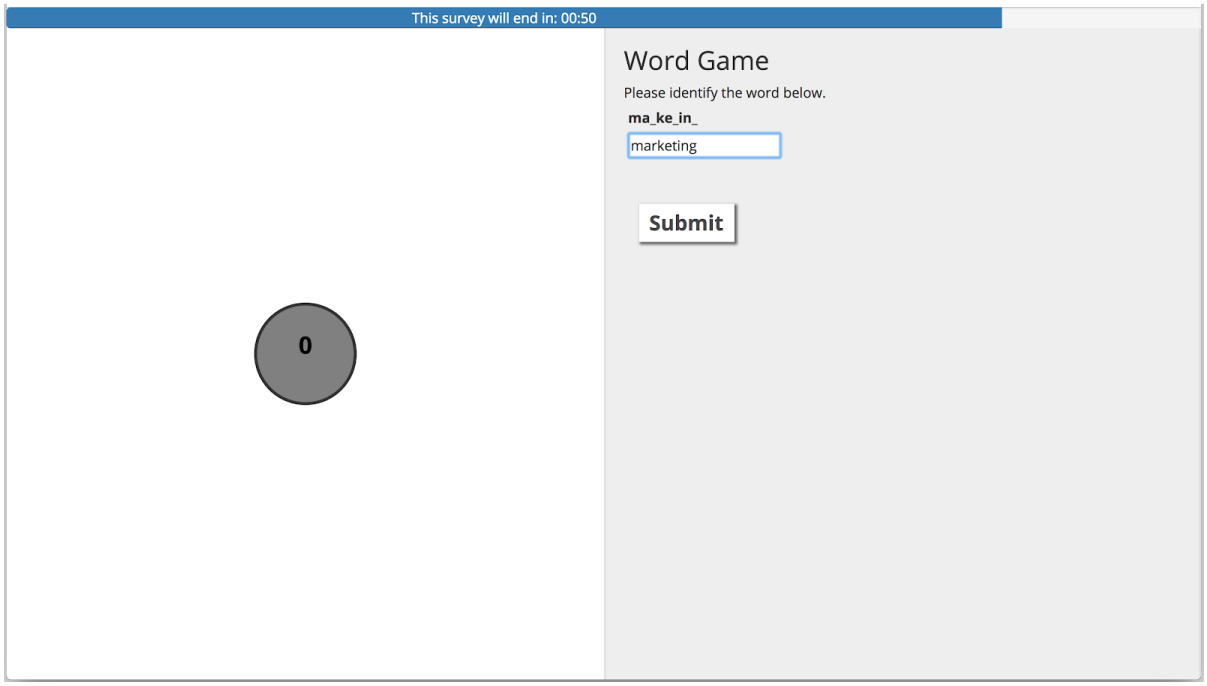
For example, if the first word that you are presented with is **ma_ke_in_**, then you will see the following screen:



Click 'Next' to continue.

Tutorial (4/9)

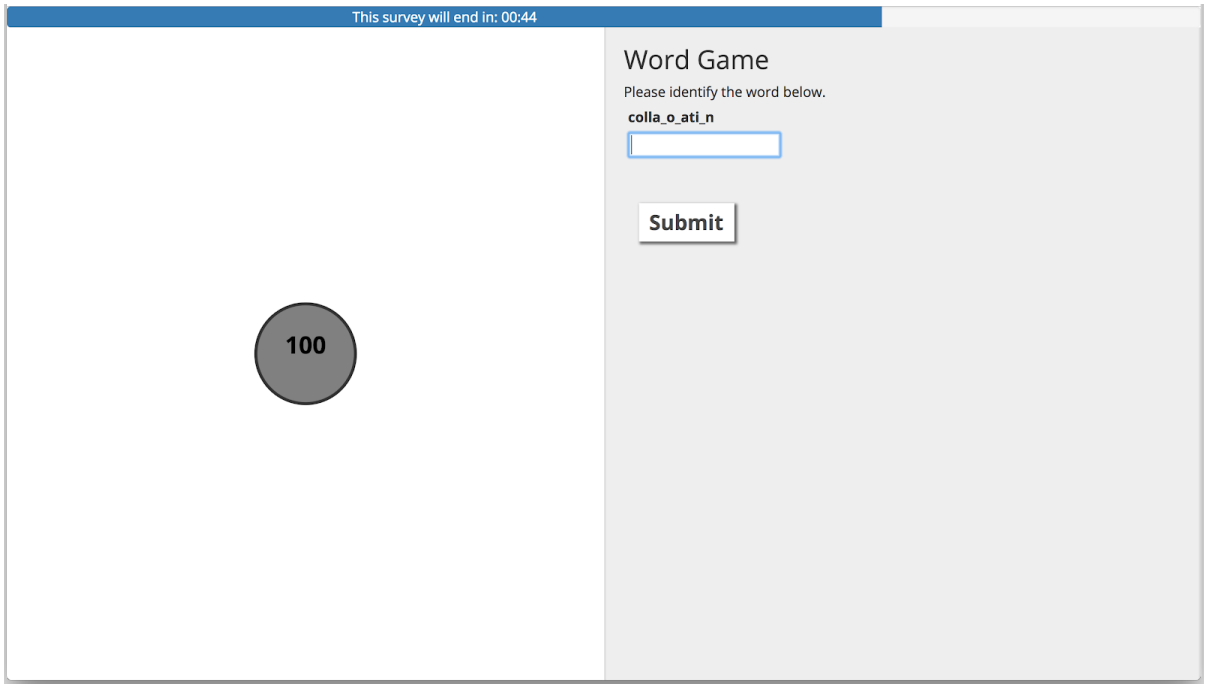
The correct answer, in this case, would be **marketing**, which you can enter in the box provided:



Click 'Next' to continue.

Tutorial (5/9)

Once you click 'Submit' or hit 'Enter' with the correct answer inside the box, **100 points** will be added to you, and the next word will be shown:



Click 'Next' to continue.

Tutorial (6/9)

Please click 'Submit' or hit 'Enter' after you enter each word to see the next word.

If you do not know a word, you can either guess or submit a blank answer.

Click 'Next' to continue.

Tutorial (7/9)

You have **60 seconds** to identify as many words as you can.

You will be able to see how many seconds you have left at the top.

There are a lot more words than you can identify in the given time frame, so you should not feel bad that you will not be able to identify all of them.

Click 'Next' to continue.

Tutorial (8/9)

Please enter all letters lowercase.

There may be some **plurals** among the words (e.g., chairs).

There may be some **proper nouns** among the words (e.g., texas).

Please make sure to enter all letters **lowercase** even if the word is a proper noun.

Click 'Next' to continue.

Tutorial (9/9)

Before you play the real game, we will let you play a **demo run**.

You will now start the demo run.

Note that when the run starts, you may be linked to other players in the game. Regardless of whether or not you are linked to others, remember that you are always the larger circle at the center.

Click 'Next' to continue.

First public goods game tutorial

Tutorial (1/10)

ONE OF THE FOLLOWING TREATMENT TEXTS (EE, EU, RE, RU)

> You will start the second game with a score that is the average of the scores of all players in the word game. This score could be lower than, equal to, or higher than your actual score from the word game depending on how your personal performance compares to how well the other players did in the task. For example, even if you performed well in the word game, if other players did not perform as well as you did, your score will unfortunately go down. Similarly, even if you performed poorly in the word game, if other players performed better than you did on average, then your score will go up.
>
> You will start the second game with your score from the word game. Other players similarly start this game with whatever score they were able to achieve in the word game. In other words, those who performed well in the word game start the second game with a higher score than others who did not perform as well.
>
> We will disregard your score from the word game. Instead, you will start the second game with a score that we randomly assign to you. All participants in your group are assigned the exact same score.
>
> We will disregard your score from the word game. Instead, you will start the second game with a score that we randomly assign to you. This score could be lower than, equal to, or higher than your actual score from the word game. In other words, even if you performed well in the word game, you could unfortunately still get a score that is much lower. Similarly, even if you performed poorly in the word game, you could still get a score that is much higher. It is highly likely that different players will be assigned different random scores.

Your score at the end of the word game was [SCORE IN WORD GAME].

The score you will start the second game with is [DEPENDS ON CONDITION].

The higher your score in the **second game** the higher your bonus payment will be at the end.

Please tell us how fair or unfair you find this rule for allocating scores.

> **Unfair**

1

2

3

4

5

6

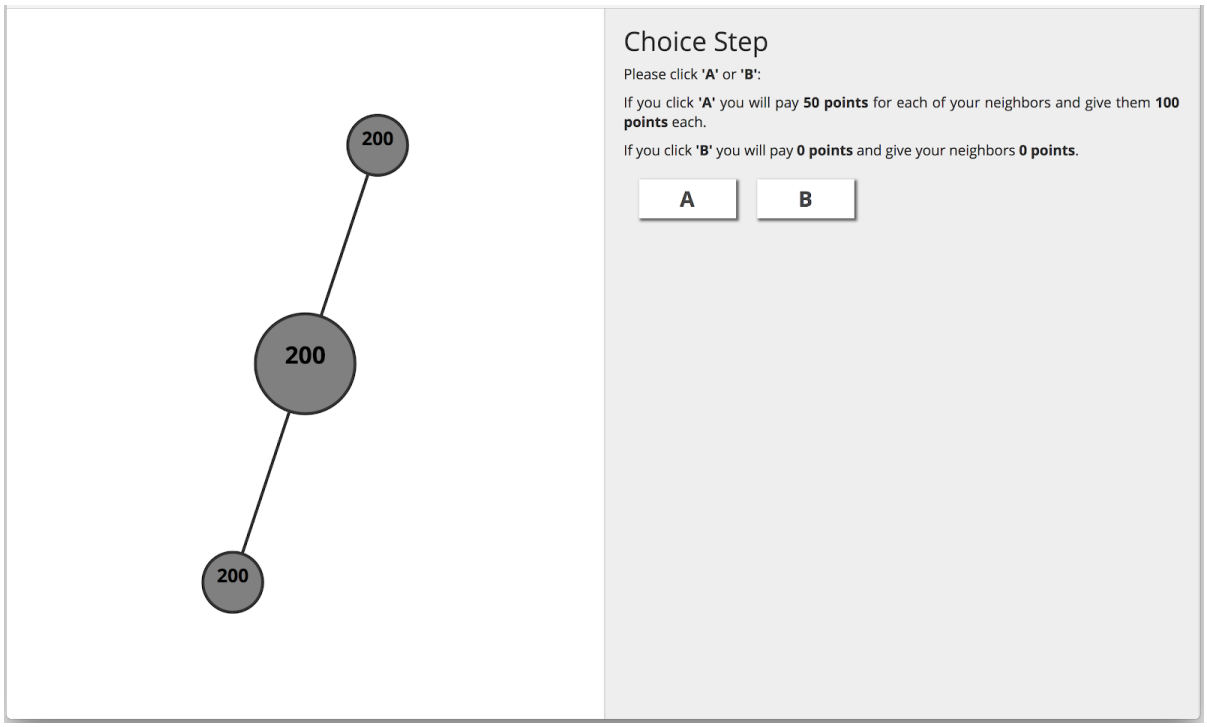
7

Fair

Tutorial (2/10)

In the **second game**, you will be linked together with other players to play a game where you can decide how much to contribute to one another.

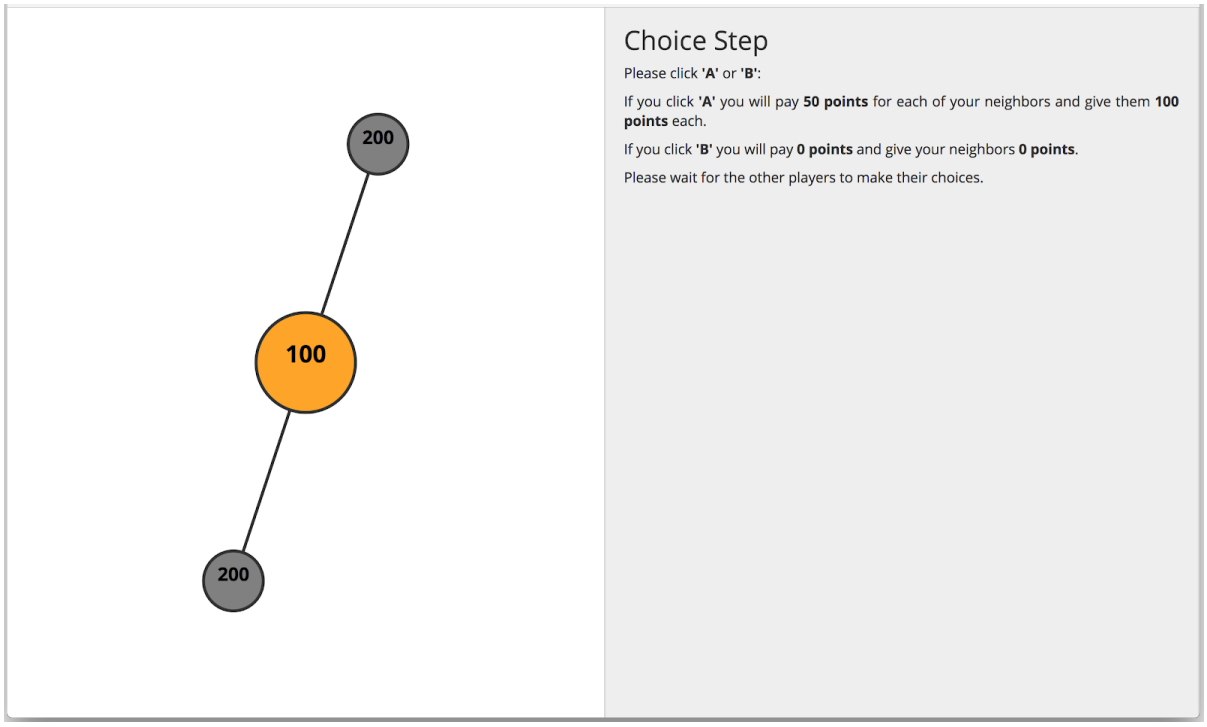
For example, the following screen shows a player with a score of 200 and who is connected to two neighbors also with scores 200 (the larger circle at the center is you):



Click 'Next' to continue.

Tutorial (3/10)

If you click **'A'** you will pay **50 points** for each player you are connected with, and we will give **100 points** to each player you are connected with:



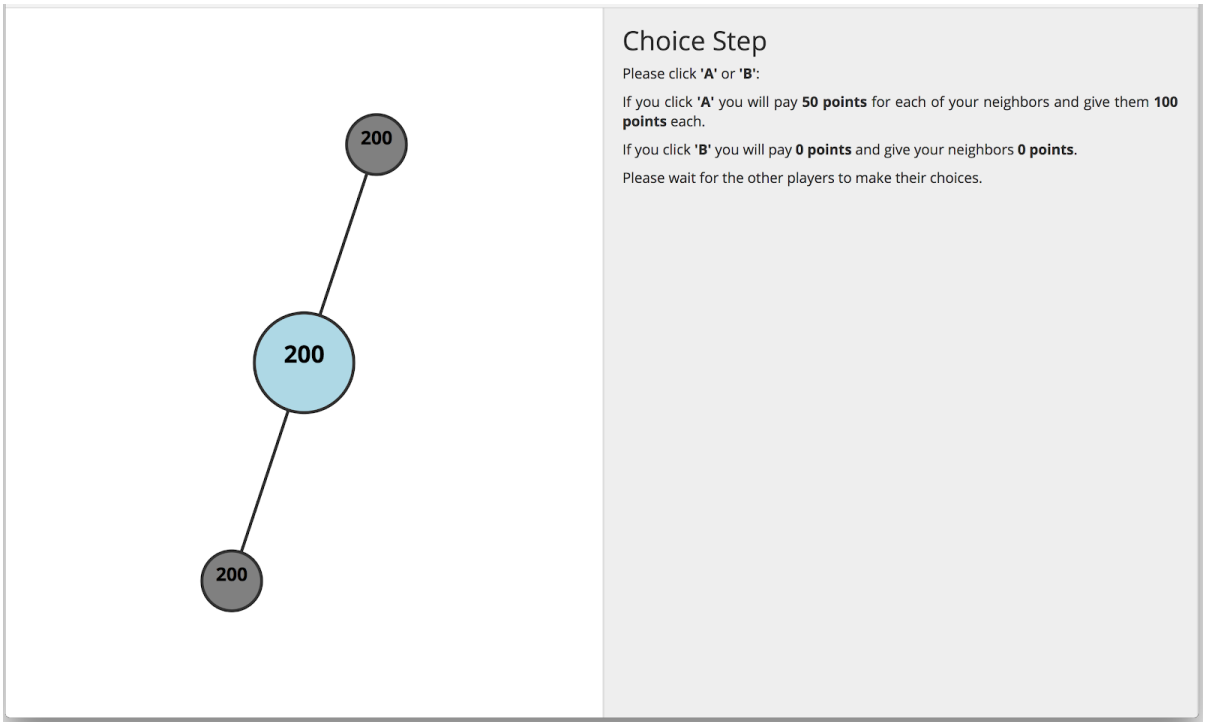
Note that you will not be able to see your neighbors' new scores and choices until after all players made their choices for that round.

Note also that your new score at the end of the round may end up being higher than what you are seeing at this stage (100 in this case) depending on how many of your neighbors also choose **'A'**.

Click 'Next' to continue.

Tutorial (4/10)

If you click **'B'** instead you will pay **0 points** and give **0 points** to each player you are connected with:



Once again, new scores and choices will be shown once all players make their choices.

Click 'Next' to continue.

Tutorial (5/10)

It is important to note that **if everybody chooses 'A'**, then everybody is guaranteed to be better off at the end of the game.

However, **if you choose 'A' and others choose 'B'**, then others will be better off, while you will be worse off.

Similarly, **if you choose 'B' and others 'A'**, then you will be better off, and others will be worse off.

You will be playing **multiple rounds** of this game.

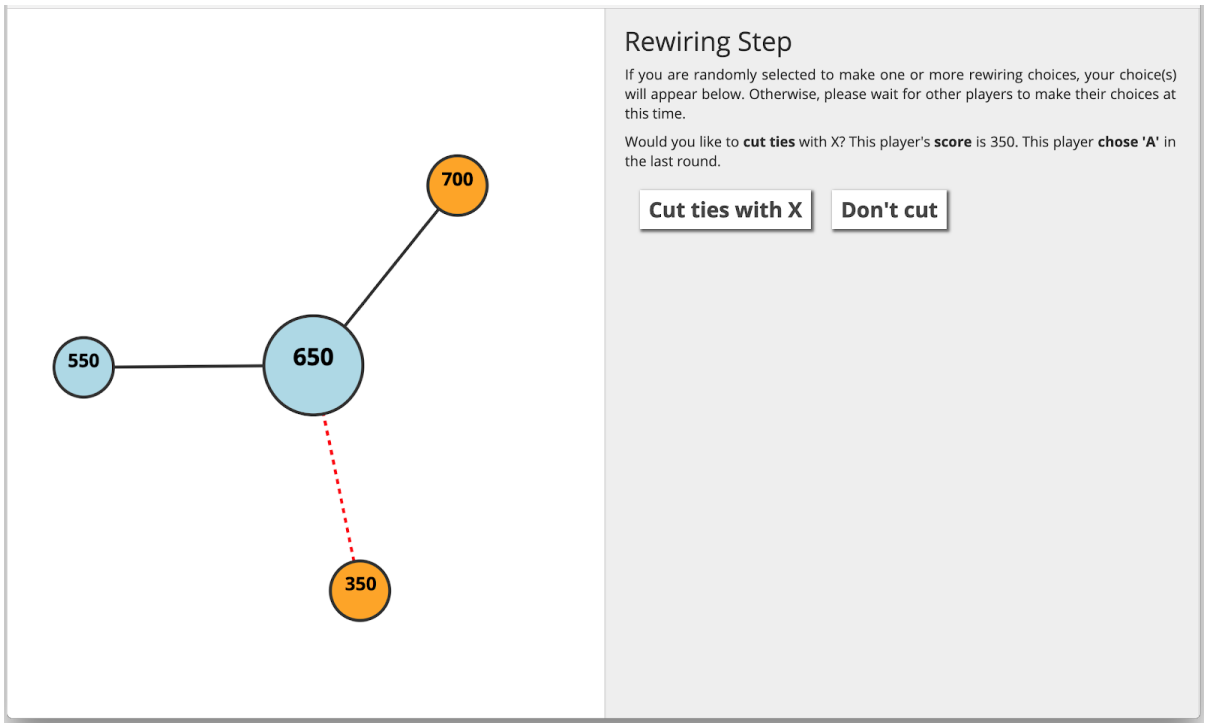
Click 'Next' to continue.

Tutorial (6/10)

After each round, a certain fraction of players will be randomly selected and allowed to cut or add ties with other players.

A tie can be cut from input from a single player.

For example, the following screen shows a player with a score of 650 and who is connected to three neighbors, with scores 350, 550, and 700. The player is asked whether to cut the tie to the player with score 350.

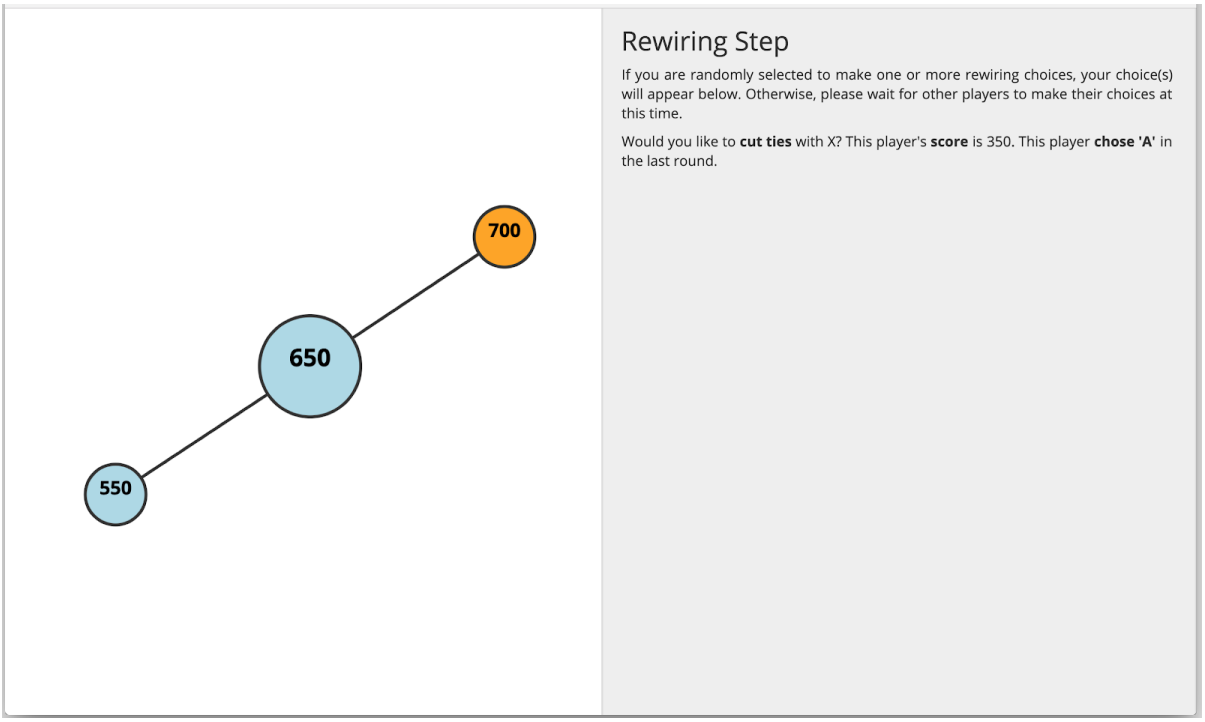


If you click 'Don't cut', then the tie will not be cut.

Click 'Next' to continue.

Tutorial (7/10)

However, if you click 'Cut ties with X', then your tie to player X will be cut at the end of the step:

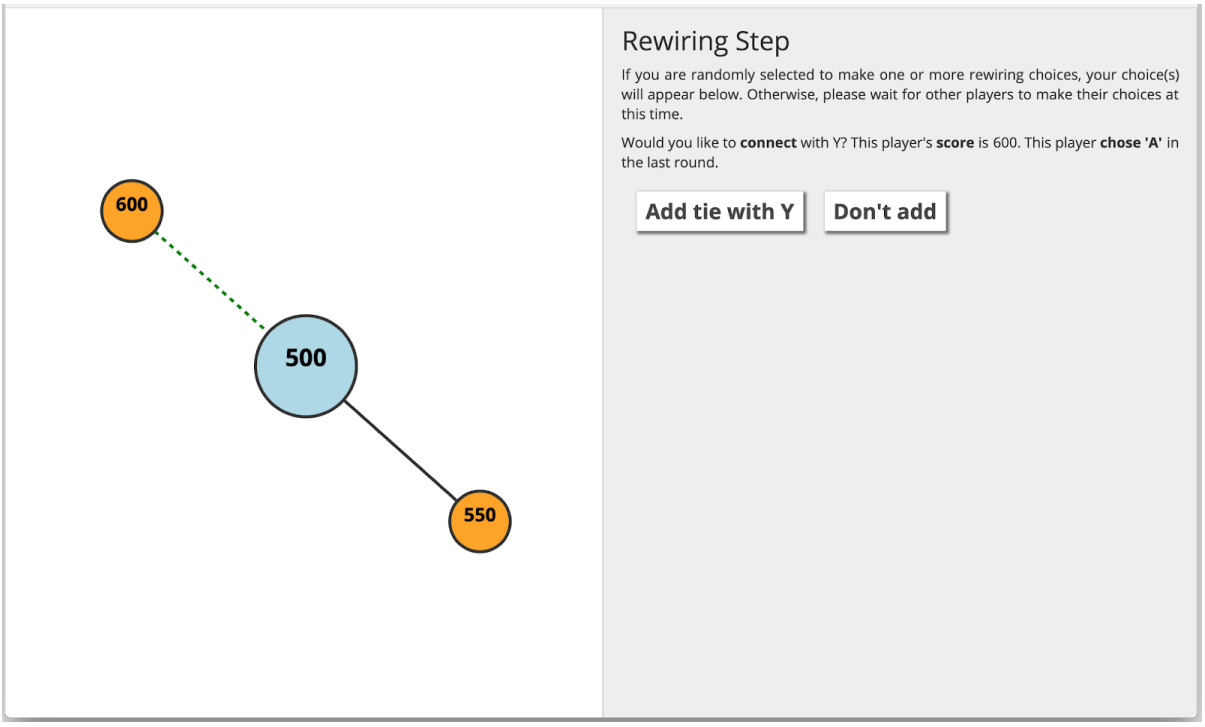


Click 'Next' to continue.

Tutorial (8/10)

A new tie will only be added if both players choose to connect.

For example, the following screen shows a player with a score of 500 and who is connected to one neighbor with a score of 550. The player is asked whether to add a new tie to the player who has a score of 600.



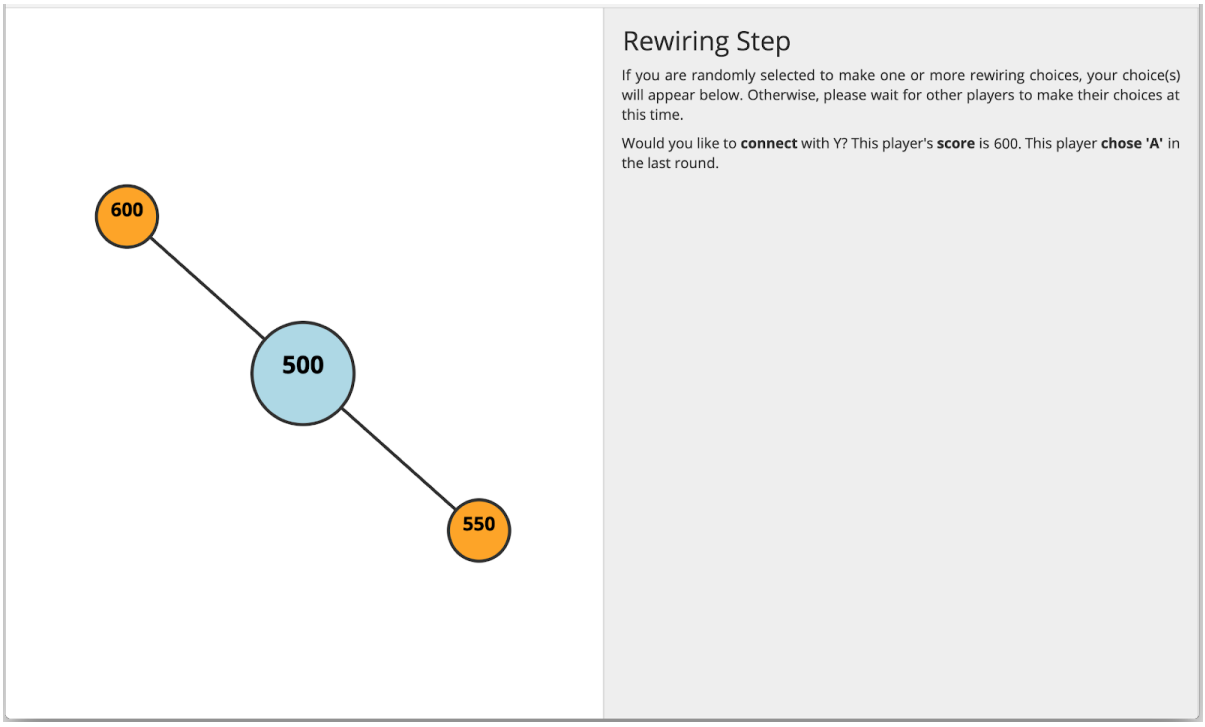
If you click 'Don't add', then the proposed tie will not be added.

Similarly, if you click 'Add tie with Y' but player Y clicks 'Don't add' on their end, then the proposed tie will still not be added.

Click 'Next' to continue.

Tutorial (9/10)

However, if player Y also clicks 'Add tie with X', then you will be connected to player Y:



Click 'Next' to continue.

Tutorial (10/10)

Before you play the real second game, we will let you play a **demo run**.

You will now start the demo run.

Click 'Next' to continue.

Second public goods game tutorial

Tutorial (1/4)

We recorded the score you reached at the end of the second game.

You will now play the third and final game of this task.

Click 'Next' to continue.

Tutorial (2/4)

The rules of this game are identical to the second game you just played.

IF CONDITION DIFFERENT FROM BEFORE

However, two things have changed:

- We will randomly choose a new set of connections to start the game.
- Your starting points will be chosen differently this time.

IF CONDITION SAME AS BEFORE

We will randomly choose a new set of connections to start the game.

Click 'Next' to continue.

Tutorial (3/4)

ONE OF THE FOLLOWING TREATMENT TEXTS (EE, EU, RE, RU)

You will start the third game with a score that is the average of the scores of all players in the word game. This score could be lower than, equal to, or higher than your actual score from the word game depending on how your personal performance compares to how well the other players did in the task. For example, even if you performed well in the word game, if other players did not perform as well as you did, your score will unfortunately go down. Similarly, even if you performed poorly in the word game, if other players performed better than you did on average, then your score will go up.

You will start the third game with your score from the word game. Other players similarly start this game with whatever score they were able to achieve in the word game. In other words, those who performed well in the word game start the third game with a higher score than others who did not perform as well.

We will disregard your score from the word game. Instead, you will start the third game with a score that we randomly assign to you. All participants in your group are assigned the exact same score.

We will disregard your score from the word game. Instead, you will start the third game with a score that we randomly assign to you. This score could be lower than, equal to, or higher than your actual score from the word game. In other words, even if you performed well in the word game, you could unfortunately still get a score that is much lower. Similarly, even if you performed poorly in the word game, you could still get a score that is much higher. It is highly likely that different players will be assigned different random scores.

Your score at the end of the word game was [SCORE IN WORD GAME].

The score you will start the third game with is [DEPENDS ON CONDITION].

The higher your score in the **third game** the higher your bonus payment will be at the end.

Please tell us how fair or unfair you find this rule for allocating scores.

Unfair

1

2

3

4

5

6

7

Fair

Tutorial (4/4)

Your performance bonus will be based on **your final scores at the end of the second and third games**.

You will now start the third game.

Remember that you will be playing MULTIPLE rounds of this game.

Click 'Begin' to join the game.

After you click 'Begin', please stay on this page as you may be dropped for being idle if you don't make your next move within **20 seconds** when it appears.

Trust survey shown at the end of each public goods game

Survey Step

Do you think that most of your neighbors tried to take advantage of you when they got the chance, or did they try to be fair?

Most of them tried to take advantage of me.

1

2

3

4

5

6

7

Most of them tried to be fair.

End survey

IF THE TWO CONDITIONS WERE DIFFERENT

Survey Step

Remember that you played the community game **twice**.

Please compare the **first version** of the game with the **second version** and tell us which version seems **more fair** to you.

Both versions are described below.

The rules for the first version of the game:

[TREATMENT TEXT 1]

The rules for the second version of the game:

[TREATMENT TEXT 2]

Please tell us which version seems MORE FAIR to you.

first version

second version

If you had the chance to play this game one more time, which version would you like to play?

first version

second version

Survey Step

Recall the many choices you had to make whether to **take action A (and contribute points to your neighbors)** or **take action B (and NOT contribute points to your neighbors)** during the game.

Which of the following factors were most influential in making you choose action A (and contribute points to your neighbors)? You can check one or more boxes.

I wanted other players to increase their scores.

I wanted to encourage other players to choose A too.

Most of my neighbors chose A in the previous round.

Most of my neighbors chose B in the previous round.

Most of my neighbors had similar scores compared to me.

Most of my neighbors had higher scores compared to me.

Most of my neighbors had lower scores compared to me.

I found the rule of initial score allocation to be fair.

I didn't find the rule of initial score allocation to be fair.

Other factor not listed here (type your reason below).

Survey Step

How many other HITs have you participated in that required you to interact with other players like this HIT?

[ANSWER HERE]

How old are you?

[ANSWER HERE]

What gender do you identify with?

Male

Female

Other

What race/ethnicity do you identify with?

White

Black

Hispanic

Asian

Other

What is your level of education?

Less than high school diploma

High school diploma or equivalent

Some college

College degree

Graduate degree

Other

What is your yearly income in US dollars?

Less than $20,000

$20,000 to $39,999

$40,000 to $59,999

$60,000 to $79,999

$80,000 to $99,999

More than $100,000

Which of the following best describes your political orientation?

Very liberal

Liberal

Middle of the road

Conservative

Very conservative

Are you located in the U.S.?

Yes

No

Appendix 1.2: Power calculations

To determine the target sample size we need for this experiment, we conducted a power study. Following Snijders (2005), we base our analysis on the approximate relationship for a two-sided test

$$\frac{\gamma}{\text{s.e.}(\widehat{\gamma})} \approx z_{1-(\alpha/2)} + z_{1-\beta} = K_z,$$

where γ is the true multilevel model coefficient on a treatment effect, $\text{s.e.}(\widehat{\gamma})$ is the standard error of the estimated treatment effect, $\alpha = 0.05$ is the significance level, $1 - \beta = 0.8$ is the power, and $K_z = \Phi^{-1}\left(1 - \frac{\alpha}{2}\right) + \Phi^{-1}\left(1 - \beta\right)$ is called the non-centrality parameter. Squaring this relationship, we have

$$\frac{\gamma^2}{\text{var}(\widehat{\gamma})} \approx K_z^2.$$

To estimate sample size, we need an expression for the variance of γ. Following Moerbeek, Breukelen, and Berger (2000), we estimate the expected variance of our multilevel regression coefficient on a treatment variable (assuming 0/1 coding) as:

$$\text{var}(\widehat{\gamma}) = \frac{1}{n_3} K_\sigma,$$

where K_σ is defined as

$$K_\sigma = 4 \times \frac{\left[n_1 n_2 \sigma_3^2 + n_1 \sigma_2^2 + \sigma_1^2\right]}{n_1 n_2} = 4 \times \left[\sigma_3^2 + \frac{\sigma_2^2}{n_2} + \frac{\sigma_1^2}{n_1 n_2}\right].$$

n_i and σ_i^2 are the sample size and the variance of the dependent variable for level i, where $i = 1, 2, 3$ for rounds, participants, and sessions, respectively. To find an expression for the number of sessions, n_3, given all of the other parameter values, we start from the equation above, plugging the expression for $\text{var}(\widehat{\gamma})$ in and rearranging to solve for n_3:

$$\gamma^2 \ \frac{n_3}{K_\sigma} \approx K_z^2 \iff n_3 \approx \frac{K_z^2 K_\sigma}{\gamma^2}.$$

We set $\gamma = 0.075$, $\alpha = 0.05$, and $1 - \beta = 0.8$. We use a multilevel model estimated on the replication data published by Nishi et al (2015) to approximate $\sigma_1^2 = 0.14$, $\sigma_2^2 = 0.08$, and $\sigma_1^2 = 0.02$. Finally, we assume there will be $n_1 = 10$ rounds per game and $n_2 = 12$ participants per session. Solving for n_3, we obtain a preliminary desired sample size of $n_3^b = 160$ sessions for a between-subjects design.

Finally, to account for our within-subjects design, we use the approximate relationship $n_3^w = n_3^b(1-\rho)/2$, where ρ is the within-subjects correlation in the outcome across games, and n_3^w is the within-subjects target sample size (Lakens 2006). Taking $\rho = 0$ to be conservative, we obtain a target within-subjects sample size of $n_3 = 80$ sessions.

Appendix 1.3: Qualitative findings

The experiment also included an end survey that asked players "why" they chose to cooperate. While there is a lot of behavioral research with a particular focus on cooperation, to the best of our knowledge, this study is the first to directly ask people about their "motivations" behind making those decisions (refer to Appendix 1.1 for the text of the motivation question). Table A1.3.1 below lists the counts and percentages of different motivations in the sample (since players were allowed to choose one or more reasons, the percentage here is calculated by dividing the raw count by the number of players who fully completed the task, which is 1,759).

Table A1.3.1. Reasons why players cooperate.

Reason	Count	Percentage
I wanted to encourage other players to choose A too.	1104	63%
Most of my neighbors chose A in the previous round.	647	37%
I wanted other players to increase their scores.	615	35%
Most of my neighbors chose B in the previous round.	388	22%
Most of my neighbors had higher scores compared to me.	328	19%
I found the rule of initial score allocation to be fair.	316	18%
Most of my neighbors had similar scores compared to me.	280	16%
I didn't find the rule of initial score allocation to be fair.	192	11%
Most of my neighbors had lower scores compared to me.	190	11%
Other	76	4%

If a player chose 'Other' as one of their answers, they were asked to write their reason in their own words. One common answer written under 'Other' is that the player actually never cooperated or that the player simply tried to maximize his/her score. See Table A1.3.2 below for the full list of other reasons participants wrote down.

Table A1.3.2. Other answers given to the motivation question.

1	game 2 wasn't as fair as game 1 since people already had their bias of "B" towards the end of game 1 when people didnt trust eachother
2	I never chose action A
3	Selecting a was the right thing to do
4	I never chose A

5	I wanted most money for myself
6	I wanted to reduce the income inequality.
7	i could never cut with the ones who always chose B so i had to switch to B myself
8	I did not choose A as I kept thinking my neighbors would choose B
9	I just tried to not lose my points and hoped others would do the same
10	I never chose A, so none of these factors applied to me.
11	my own morals
12	I would have wanted to choose my own ties in all the games instead of it being random
13	I wanted to play fairly and get the best outcome for us all.
14	I never chose A, because I wanted to maximize my own personal payout.
15	It took awhile to get the gist of the game. When I figured it out, I worried more about winning than being fair.
16	I wanted to determine if the other players were actually humans.
17	I wanted to max my score
18	Trying to maximize my own score
19	I never choose A because I was looking out for only myself
20	I just wanted to be fair in general
21	Ideally I was hoping to disconnect from those that continually chose B.
22	I never chose A
23	I never chose action A because I wanted the most points for myself.
24	I wanted to win
25	General Altruism and Karma
26	felt a sense of connection to them
27	i wanted to increase my score
28	Also I wanted to get as many points as I could.
29	I just wanted to be nice to them

30	It is most beneficial to everyone if I choose A.
31	I didn't want people to disconnect with me
32	my neighbors were greedy
33	everybody eats
34	I wanted to win, so I decided to always choose B and then second round I learned that having more ties meant more money when people choose to do good. I only looked out for me.
35	i just wanted to increase my score
36	I wanted other players to believe I would choose A.
37	I never picked A
38	I wanted to share in others earning more points
39	playing what i thought would be the best odd in each level
40	What is the right thing to do.
41	I didn't choose A
42	I realized there were more points to share if we gave so I did that in the beginning
43	I never chose action A
44	I was always aiming for the highest score
45	Now that I tried both versions, the one where everyone gets a similar score automatically make the gameplay a little more flat, because everyone figures out the strategy to win fairly quickly.
46	I wanted as many points as possible for the biggest bonus.
47	Give and receive
48	I never chose A the entire time
49	if everyone always picks a everyone benefits
50	I went so quickly into the hole it didn't matter anymore.
51	I never chose A
52	I never actually chose A , so this question does not apply to me. i simply did not trust the others, so I went with B.

53	I wanted to maximize my points
54	It adds more money to the game and makes the most sense for everyone to get a higher score overall
55	I wanted to get the highest score.
56	I wanted to maximize my score.
57	maximizing my points
58	greed and strategy
59	I did not choose A. I wanted the points.
60	Wanted to make the most points I could and not be cut by others.
61	peopkle might need the earnings from the game
62	I never once chose A
63	I wanted to maximize my own earnings above anything else.
64	I wanted to be fair to everyone.
65	I didn't want them to cut ties based on me choosing B
66	I wanted to be consistent
67	I didn't choose A to maximize my score
68	I wanted to ensure I kept a majority of points
69	I was trying to be greedy.
70	I wanted to spite those with more by helping those with less
71	i didnt choose A i saw no benefit in it
72	I wanted to "win" a few rounds, but not too many
73	If every one pressed a we would all be better off. assholes pick b
74	I wanted to keep my score
75	Everyone made more money if we worked together by pressing A
76	never got a chance to fully read rules because of timer so never understood fully

Appendix 1.4: Within-subjects models

Fairness perceptions

In addition to the model with clustered standard errors presented in the main text, an additional linear mixed-effects model for fairness perceptions was also fit to data with the following structure:

$$\Delta fairness_{ij} = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + u_i + e_{ij} \quad \text{(A1.4.1)}$$

Results based on this additional model are presented in Table A1.4.1 below side by side with results based on the earlier model with clustered standard errors. This table also presents results based on models that treat changes in conditions as a categorical variable together with the earlier results based on models that treat changes in conditions as a continuous variable. As can be seen in this table, results based on models with clustered standard errors agree with results based on linear mixed-effects models, and the most salient effect appears to be the one corresponding to a change from an earned to a random condition (e.g., EU to RU), which is negative (≈-0.6), based on models that treat changes in conditions as a categorical variable.

Table A1.4.1. Fairness perceptions as a function of endowment regime (within-subjects).

Change in endowment regime treated as a continuous predictor

	Model with clustered standard errors	Linear mixed-effects model
Intercept	0.071 (0.060)	0.073 (0.060)
Δ Earned	1.386 (0.131)***	1.387 (0.120)***
Δ Equal	0.482 (0.143)**	0.481 (0.119)***
Δ Earned x Equal	-1.706 (0.175)***	-1.705 (0.168)***

Change in endowment regime treated as a categorical predictor

	Model with clustered standard errors	Linear mixed-effects model
Intercept	0.183 (0.119)	0.177 (0.151)
Change to Earned	0.393 (0.318)	0.437· (0.260)
Change to Random	-0.627* (0.263)	-0.612* (0.266)
Change to Equal	-0.427 (0.278)	-0.444· (0.261)
Change to Unequal	0.433 (0.280)	0.389 (0.259)
Change to Earned, Equal	0.096 (0.454)	0.058 (0.458)
Change to Earned, Unequal	-0.268 (0.465)	-0.268 (0.447)
Change to Random, Equal	-0.139 (0.418)	-0.147 (0.460)
Change to Random, Unequal	-0.165 (0.438)	-0.135 (0.457)

The number of samples (n) for the models fit is 1803, clustered inside 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Cooperation patterns

In addition to the model with clustered standard errors presented in the main text, an additional linear mixed-effects model for cooperation was also fit to data with the following structure:

$$\Delta cooperation_{ijl} = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + \delta_4 round_{il} + u_i + u_{ij} + e_{ijl} \quad \text{(A1.4.2)}$$

Results based on this additional model are presented in Table A1.4.2 below side by side with results based on the earlier model with clustered standard errors. This table also presents results based on models that treat changes in conditions as a categorical variable together with the earlier results based on models that treat changes in conditions as a continuous variable. As can be seen in this table, results based on models with clustered standard errors agree with results based on linear mixed-effects models, and the most salient effect appears to be the one corresponding to a change from a random to an earned condition (e.g., RU to EU), which is negative (≈-0.6), based on models that treat changes in conditions as a categorical variable.

Table A1.4.2. Cooperation decision as a function of endowment regime (within-subjects).

Change in endowment regime treated as a continuous predictor

	Models with clustered standard errors	Linear mixed-effects models
Intercept	-0.132 (0.012)***	-0.128 (0.013)***
Δ Earned	-0.042 (0.016)*	-0.041 (0.017)*
Δ Equal	-0.002 (0.016)	-0.004 (0.017)
Δ Earned x Equal	0.015 (0.023)	0.016 (0.024)

Change in endowment regime treated as a categorical predictor

	Models with clustered standard errors	Linear mixed-effects models
Intercept	-0.124*** (0.021)	-0.120*** (0.020)
Change to Earned	-0.060* (0.030)	-0.059* (0.029)
Change to Random	-0.009 (0.029)	-0.010 (0.030)
Change to Equal	0.013 (0.031)	0.007 (0.030)
Change to Unequal	-0.016 (0.029)	-0.016 (0.029)
Change to Earned, Equal	-0.003 (0.044)	0.004 (0.052)
Change to Earned, Unequal	0.052 (0.044)	0.048 (0.051)
Change to Random, Equal	0.046 (0.052)	0.055 (0.052)
Change to Random, Unequal	0.068 (0.056)	0.066 (0.052)

The number of samples (n) for the models fit is 16098, clustered inside 1803 players and 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Finally, note that the results are robust to (i) fitting the models on the session- rather than individual-level data (see Equations A1.4.1' and A1.4.2' below), (ii) including additional predictors for changes in player score and experience, and (iii) using an ordinal logistic model instead of OLS, though the p-value for "Changed to Earned" in the models that treat changes in conditions as a categorical predictor is sometimes slightly above 0.05.

$$\Delta avg\_coop_{il} = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + \delta_4 round_{il} + e_{il} \quad \text{(A1.4.1')}$$

$$\Delta avg\_coop_{il} = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + \delta_4 round_{il} + u_i + e_{il} \quad \text{(A1.4.2')}$$

Appendix 1.5: Between-subjects models

Fairness perceptions

Figure A1.5.1 below presents average fairness scores by condition for (i) the first game only, (ii) the second game only, and (iii) both games pooled together. As can be seen in this figure, regardless of which specific plot we focus on, the fairness ordering of the four conditions are always, from least to most fair: random unequal (RU, least fair), earned equal (EE), random equal (RE), and earned unequal (EU, most fair). EU is consistently much more fair compared to the other three conditions, and the fairness scores of the two equal conditions are much closer to one another (EE 4.33, RE 4.45)[21] compared to the fairness scores of the two unequal conditions (EU 5.4, RU 4.05). These results directly speak to the equality vs. fairness debate in the social sciences and suggest that people prefer not equal but fair arrangements.

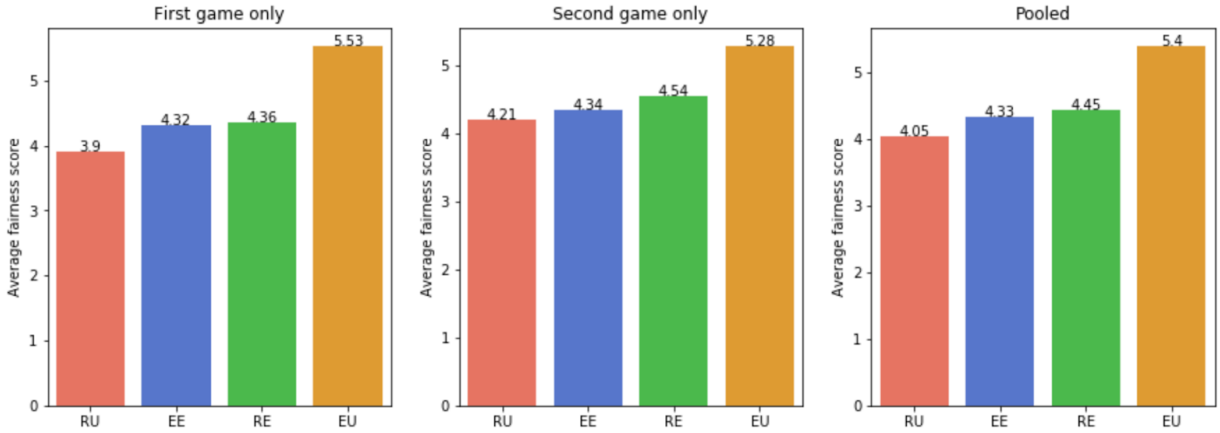
It is also important to note that while fairness scores are overall higher in the second game (4.62) compared to the first game (4.51), the EU condition actually has a lower average fairness score in the second game (5.53 vs. 5.28).[22] As discussed further below, a plausible explanation of this pattern is that lower levels of cooperation under EU lead to lower levels of trust by the end of the first game, which in turn affect fairness perceptions of the second game. (Another, less theoretical explanation would be by resorting to the classic regression to the mean argument: since the fairness score of EU is much higher than the other conditions, it is more likely that it will go down when measurement happens twice.)

The fairness ordering observed in Figure A1.5.1 is also mostly consistent with the pairwise fairness and preference comparisons players made at the end of the experiment. EU is chosen as more fair (more preferred) compared to RU by 69% (62%) of players; RE is chosen as more fair (more preferred) compared to RU by 74% (72%) of players; EE is chosen as more fair (more preferred) compared to RU by 64% (60%) of players; EU is chosen as more fair (more preferred) compared to EE by 62% (53%) of players; RE is chosen as more fair (more preferred) compared to EE by 58% (57%) of players; and EU is chosen as more fair (more preferred) compared to RE by 49% (51%) of players. Perhaps the only surprising result here is that while EU has a much higher average fairness score compared to RE (5.4 vs. 4.45), the two conditions are practically considered to be equally fair (preferable) in the pairwise comparisons.

Figure A1.5.1. Average fairness scores by condition.

[21] A side point to mention here is that earned equal (where players start the public goods game with a score that is the average of all scores from the skill-based task in a given session) seems to be the less fair of the two equal conditions (for reference, random equal assigns an equal score to all players that is independent of the skill-based task). This is perhaps not surprising given that earned equal (EE) somewhat resembles redistribution in the sense that players with higher scores in the skill-based task help raise the scores of the players with lower scores, and most Americans are not particularly favorable towards redistribution (McCall et al 2013).

[22] As can be seen in Table A1.5.1 below, the coefficient estimate on "Earned x Second" is negative and significant.



Going beyond visual inspection, we can fit a series of models to our data. While a number of different model specifications were tried, the general structure of the model with clustered standard errors looks as follows (indices *i* and *j* stand for session and player, respectively), with standard errors clustered at the session (*i*) level.

$$fairness_{ij} = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \varepsilon_{ij} \quad \text{(A1.5.1)}$$

An alternative approach to modeling this outcome would be to use a linear mixed-effects model, which explicitly decomposes the error term into two parts.

$$fairness_{ij} = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \xi_i + \varepsilon_{ij} \quad \text{(A1.5.2)}$$

In both of these models, $fairness_{ij}$ takes discrete values between 1 and 7 (higher values more fair), while $earned_i$ and $equal_i$ each take the values 0 or 1. Note that since players play two public goods games in a given session, it is actually possible to fit models to (i) only the first public goods game, (ii) only the second public goods game, or (iii) both public goods games pooled together. While the models described above assume that the model is fit to a single public goods game only (e.g., the first public goods game), it is possible to extend them into the pooled version by adding an additional index *k* to denote the public goods game (first or second) and a dummy variable denoting whether observations come from the second as opposed to the first game ($second_{ik}$). Such an extension allows us to fit our models to a much larger sample. In this case, standard errors would be clustered at the session (*i*) and player (*j*) levels, while the linear mixed-effects model would include three, instead of two, error terms.

$$fairness_{ijk} = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 second_{ik} + \varepsilon_{ijk} \quad \text{(A1.5.1')}$$

$$fairness_{ijk} = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 second_{ik} + \xi_i + \xi_{ij} + \varepsilon_{ijk} \quad \text{(A1.5.2')}$$

Table A1.5.1 below presents results from these between-subjects models with fairness as the outcome variable. In addition to the pooled Models A1.5.1' and A1.5.2' described above, this table also presents results from a more flexible pooled model that allows for all possible two- and three-way interactions (this model was left out above for the sake of space).

Together, these estimates tell us that being in an earned condition has a large positive direct effect on fairness, whereas being in an equal condition has a still positive but much smaller direct effect. There is also a large negative interaction effect between Earned and Equal, which means that the positive direct effects of Earned and Equal on fairness diminish greatly for the condition EE. Once again, the facts that (i) the Earned coefficient is almost four times as large as the Equal coefficient and (ii) the Earned x Equal coefficient is negative suggest that equal arrangements are not necessarily more fair compared to unequal ones. Rather, what makes an arrangement fair or unfair is the specific mechanism through which equality or inequality comes about, operationalized through the Earned/Random axis. Finally, as can be seen in the flexible pooled model, while the second game is generally higher in terms of fairness (Second is positive), the earned direct effect is actually smaller in the second game (Earned x Second is negative). The models with clustered standard errors and the linear mixed-effects models agree with each other, though the estimates are not identical.

Table A1.5.1. Fairness perceptions as a function of the exp. conditions (between-subjects).

Models with clustered standard errors

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	3.900*** (0.100)	4.208*** (0.095)	4.015*** (0.076)	3.900*** (0.100)
Earned	1.630*** (0.125)	1.072*** (0.135)	1.351*** (0.095)	1.630*** (0.125)
Equal	0.461** (0.153)	0.331* (0.150)	0.402*** (0.105)	0.461** (0.153)
Earned x Equal	-1.673*** (0.192)	-1.267*** (0.199)	-1.468*** (0.143)	-1.673*** (0.192)
Second			0.063 (0.060)	0.308* (0.135)
Earned x Second				-0.558** (0.179)
Equal x Second				-0.130 (0.219)
Earned x Equal x Second				0.406 (0.269)

Linear mixed-effects models

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	3.902*** (0.097)	4.213*** (0.102)	4.022*** (0.073)	3.918*** (0.089)
Earned	1.625*** (0.138)	1.067*** (0.140)	1.360*** (0.090)	1.601*** (0.126)
Equal	0.462** (0.136)	0.328* (0.142)	0.427*** (0.089)	0.481*** (0.123)
Earned x Equal	-1.670*** (0.194)	-1.264*** (0.199)	-1.554*** (0.126)	-1.724*** (0.176)
Second			0.065 (0.054)	0.283* (0.123)
Earned x Second				-0.482** (0.177)
Equal x Second				-0.120 (0.178)

Earned x Equal x Second				0.344 (0.251)

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 1870, 1803, and 3673, respectively, clustered inside 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Lastly, it is important to mention two additional points. The first point is related to the possible effect that the difference between a player's score (in the skill-based task) and his/her endowment (at the start of a public goods game) might have on that player's fairness perceptions. Note that a player's endowment may be higher, lower, or identical compared to his/her score in the skill-based task, depending on the condition. Based on past empirical evidence that shows that winners tend to rationalize their success in moral terms (Ohtsuka and Ohtsuka 2010), there is reason to believe that players who find themselves in a more advantaged position are also more likely to consider their condition as fair, even if that advantage comes about randomly. In line with this argument, both between- and within-subjects models agree that an increase in score from the skill-based task to the public goods game leads to an increase in fairness perceptions (an average increase of ≈0.2 in fairness per every 100 points, significant at the p<0.001 level); the inclusion of this additional predictor do not change the other estimates much, and the previous conclusions stand.[23]

The second point is related to session-level analyses. While all of the analyses above were conducted on a dataset where there is a row for every session-player(-game), as mentioned in the statistical models section in the main text, it is also possible to conduct a more crude analysis using an aggregate version of the data where rows are per session(-game).

$$avg\_fairness_i = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \varepsilon_i \quad \text{(A1.5.1'')}$$

$$avg\_fairness_{ik} = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 second_{ik} + \varepsilon_{ik} \quad \text{(A1.5.1''')}$$

$$avg\_fairness_{ik} = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 second_{ik} + \xi_i + \varepsilon_{ik} \quad \text{(A1.5.2''')}$$

$$\Delta avg\_fairness_i = \delta_0 + \delta_1 \Delta earned_i + \delta_2 \Delta equal_i + \delta_3 \Delta earned_i \times equal_i + e_i \quad \text{(A1.5.3')}$$

Results from such an aggregate analysis do not change the conclusions above. Therefore, separate tables for these models are not included here.

Cooperation patterns

The previous discussion covered how different experimental conditions (as well as changes between conditions) affected players' fairness perceptions. However, we can go even further than that and see how players behave vis-à-vis other players once they form their perceptions of the game. Figure A1.5.2 below visualizes overall patterns of cooperation across rounds. As can be seen in this figure, (i) cooperation decreases in later rounds, and (ii) cooperation levels are lower in the second game compared to the first one. The first pattern is one that is widely observed in similar networked games (Mason, Suri, and Watts 2014); in fact, given that the ratio of benefit of cooperation (*b=100*) to cost of cooperation (*c=50*) is less than the average number of

[23] Results also remain unchanged if an additional predictor for experience is included in the models.

connections ($k=6.8$) in this case (100/50=2 < 6.8), this pattern is both theoretically expected and empirically shown in Rand et al 2014.[24]

The second pattern can be explained in reference to the fact that by the end of the first game, players most likely already lost some amount of trust in other players after observing at least some of them defect. In fact, if we regress average cooperation in the second game on average trust at the end of the first game, controlling for average cooperation in the first game, the coefficient estimate on average trust is 0.073*** (0.017), which can be interpreted to mean that a one unit increase in average trust (on a scale of 1 to 7) at the end of the first game leads to a 7% increase in average cooperation in the second game. Furthermore, given that average trust at the end of the first game is very strongly correlated with average cooperation in the first game ($\varrho = 0.91$), the argument that lower levels of cooperation in the second game is partially due to the generally negative impact of the first game on trust becomes more plausible.

Figure A1.5.2. Overall patterns of cooperation.

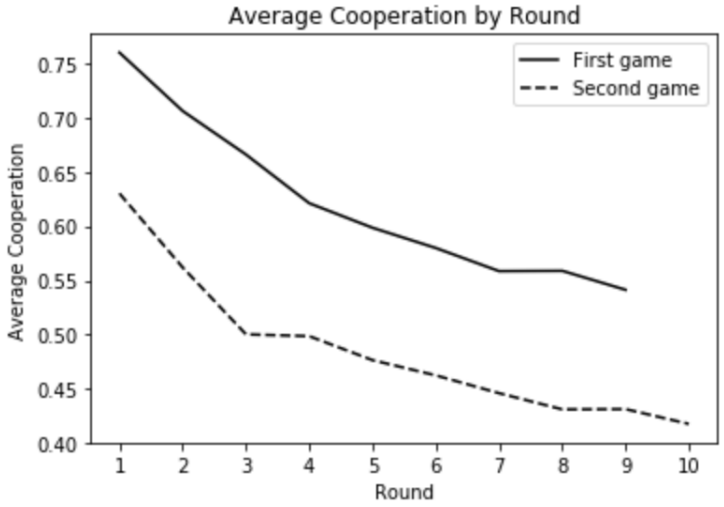


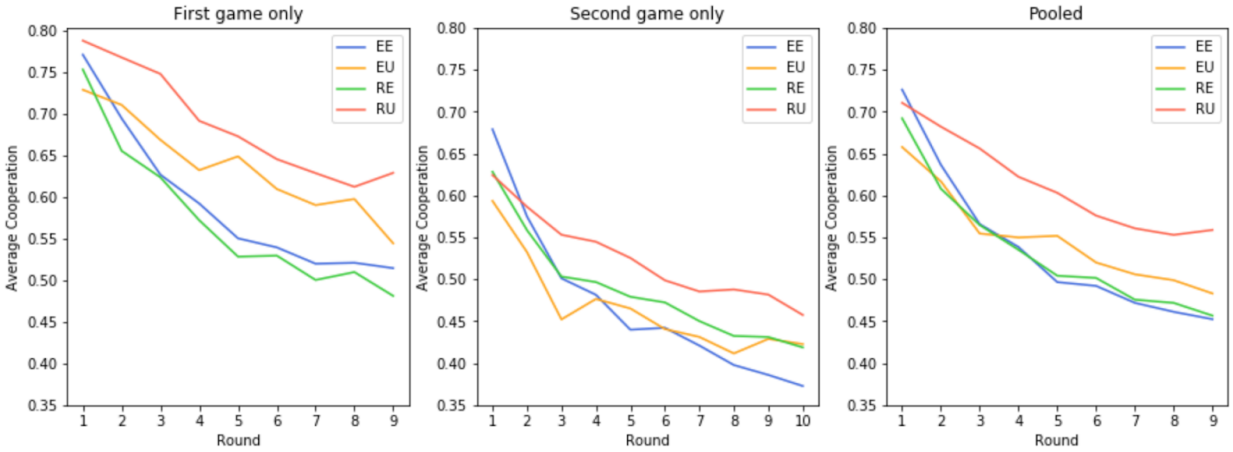
Figure A1.5.3 below visualizes patterns of cooperation across rounds in different experimental conditions. As can be seen in this figure, average cooperation in EU is consistently lower (~5%) compared to RU, while patterns of cooperation in EE and RE are a lot closer to one another (EE is slightly higher compared to RE in the first game, while RE is generally higher compared to EE in the second game). Another observation to make is that the two unequal conditions (EU and RU) are overall higher compared to the two equal conditions (EE and RE), especially in the first game. For reference, average levels of cooperation across conditions in the first game are: RE 57%, EE 59%, EU 64%, and RU 69%; and average levels of cooperation across conditions in the second game are: EU 47%, EE 47%, RE 49%, and RU 52%. These patterns suggest not only a negative "equal" effect,[25] giving support to the argument that people

[24] Note that while Figure A1.5.2 presents aggregate results, a look into the cooperation histories of individual players across rounds are mostly in line with this pattern of gradual collapse of cooperation over time. In fact, around 60% of all cooperation histories are strictly non-increasing, that is, once a player starts defecting, he/she never cooperates again.

[25] It is important to note that while other researchers found a null direct effect of level of inequality in a similar networked study (Nishi et al 2015), none of our conditions (not even random equal) are directly comparable to theirs given that in our case score allocation is always preceded by a skill-based task, which ensures that players in all

prefer unequal societies, but also a negative "earned" effect, whereby players are less willing to share their wealth with others if they believe to have "earned" that wealth, signaling a possible entitlement effect.

Figure A1.5.3. Average cooperation by round by condition.



Similar to the approach we took above when discussing fairness perceptions, we can go beyond visual inspection and fit a series of models to our data with cooperation as the outcome. The indices i, j, k, and l are for session, player, game, and round, respectively.

The between-subjects models with standard errors clustered at the session (i) and player (j) levels can be written down as:

$$g(cooperation_{ijl}) = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \beta_4 round_{il} + \varepsilon_{ijl} \quad \text{(A1.5.4)}$$

$$g(cooperation_{ijkl}) = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 round_{ikl} + \beta_5 second_{ik} + \varepsilon_{ijkl} \quad \text{(A1.5.4')}$$

Similarly, the (generalized) linear mixed-effects between-subjects models are:

$$g(cooperation_{ijl}) = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \beta_4 round_{il} + \xi_i + \xi_{ij} + \varepsilon_{ijl} \quad \text{(A1.5.5)}$$

$$g(cooperation_{ijkl}) = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 round_{ikl} + \beta_5 second_{ik} + \xi_i + \xi_{ij} + \xi_{ijk} + \varepsilon_{ijkl} \quad \text{(A1.5.5')}$$

Note that $\beta_4 round_{il}$ and $\beta_4 round_{ikl}$ are simply for notational convenience since the actual models fit include dummies for each round. Given that there are 9-10 rounds in a public goods game, this simpler notation is chosen here for the sake of avoiding unnecessary clutter. $g(.)$ is the link function used to model the outcome, which is logistic in the case of both $cooperation_{ijl}$ and $cooperation_{ijkl}$.

Table A1.5.2 below presents results from these between-subjects models with cooperation decision as the outcome variable. As can be seen in this table, there is a fair amount of variation between the estimates returned by the models with clustered standard errors on the

conditions must first engage in an activity that requires an effort on their part. (Earlier studies conducted in much smaller groups of three to four players remain largely inconclusive: Chan et al (1999) found a negative equality effect, Cherry, Kroll, and Shogren (2005) found a positive equality effect, and Sadrieh and Verbon (2006) found a null equality effect. For reference, Cherry, Kroll, and Shogren (2005) also found that the source of endowment does not make a difference, though our experimental setup is very different from theirs to allow for a direct comparison.)

one hand, and the generalized linear mixed-effects models on the other, especially in the case of pooled estimates. To begin with, while results from the "1st game only" and "2nd game only" models are similar in terms of sign and significance across both models, estimates from the mixed-effects models are usually more than twice as large in magnitude. Regardless, both models show a larger and significant negative Equal effect and a smaller and insignificant negative Earned effect in the first game, while the Earned effect is larger than Equal in the second game, though neither of them are significant. In other words, between-subjects models indicate that there is less cooperation under equality in the first game, while cooperation across conditions are not significantly different in the second game.

The divergence between the two sets of models becomes more stark in the case of the pooled estimates. While the models with clustered standard errors continue showing a larger and significant negative Equal effect (and a smaller and insignificant negative Earned effect) in the pooled models, estimates from the linear mixed-effects models indicate a larger and significant Earned effect (and a smaller and possibly insignificant negative Equal effect), while also showing a significant positive Equal x Earned interaction. In other words, the mixed-effects models flip the story and attribute the larger effect to Earned, while Equal could still have an effect, though smaller. The results are robust to the inclusion of additional predictors for player score and experience in the models.

Table A1.5.2. Cooperation decision as a function of the exp. conditions (between-subjects).

Models with clustered standard errors

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	1.453*** (0.146)	0.692*** (0.155)	1.331 (0.114)	1.401*** (0.142)
Earned	-0.230 (0.210)	-0.240 (0.225)	-0.247 (0.154)	-0.229 (0.209)
Equal	-0.503* (0.194)	-0.152 (0.229)	-0.327* (0.154)	-0.501* (0.193)
Earned x Equal	0.312 (0.284)	0.170 (0.337)	0.259 (0.209)	0.310 (0.283)
Second			-0.530*** (0.038)	-0.668*** (0.188)
Earned x Second				-0.024 (0.311)
Equal x Second				0.349 (0.296)
Earned x Equal x Second				-0.116 (0.464)

Generalized linear mixed-effects models

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	3.447*** (0.293)	1.841*** (0.333)	2.820*** (0.157)	2.799*** (0.164)
Earned	-0.519 (0.394)	-0.587 (0.447)	-0.426*** (0.071)	-0.443*** (0.102)
Equal	-1.136** (0.391)	-0.485 (0.451)	-0.182** (0.070)	-0.138 (0.098)
Earned x Equal	0.734 (0.556)	0.673 (0.636)	0.331** (0.098)	0.357* (0.139)

Second			-1.166*** (0.036)	-1.124*** (0.095)
Earned x Second				0.033 (0.139)
Equal x Second				-0.092 (0.140)
Earned x Equal x Second				-0.043 (0.197)

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 16579, 17867, and 32677, clustered inside 160 sessions and 1870, 1803, and 1870 players, respectively. All models control for round by including round dummies as predictors. Estimates are left in the original log-odds scale. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

While the above models were fit on an individual-level dataset with the structure session-player(-game)-round per row, once again, we can fit our models on a more aggregate dataset with the structure session(-game)-round, where the outcome would now be average cooperation in a given round (continuous), rather than the cooperation decision for a specific player (0, 1). As discussed in the next paragraph, the main advantage of the session-level models is easier interpretability.

$$avg\_coop_{il} = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \beta_4 round_{il} + \varepsilon_{il} \quad \text{(A1.5.4'')}$$

$$avg\_coop_{ikl} = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 round_{ikl} + \beta_5 second_{ik} + \varepsilon_{ikl} \quad \text{(A1.5.4''')}$$

$$avg\_coop_{il} = \beta_0 + \beta_1 earned_i + \beta_2 equal_i + \beta_3 earned_i \times equal_i + \beta_4 round_{il} + \xi_i + \varepsilon_{il} \quad \text{(A1.5.5'')}$$

$$avg\_coop_{ikl} = \beta_0 + \beta_1 earned_{ik} + \beta_2 equal_{ik} + \beta_3 earned_{ik} \times equal_{ik} + \beta_4 round_{ikl} + \beta_5 second_{ik} + \xi_i + \xi_{ik} + \varepsilon_{ikl} \quad \text{(A1.5.5''')}$$

Table A1.5.3 below presents results from these session-level models. The estimates mostly mirror those in Table A1.5.2 but are much easier to interpret given that the outcome (average cooperation) is now continuous: the models with clustered standard errors (and the linear mixed-effects models for each game separately) indicate a significant large negative Equal effect corresponding up to a 10% decrease in cooperation, while the pooled linear mixed-effects models show a moderately sized (~4%) significant negative Earned effect.

Taken together, these results do not allow us to reach a definitive answer as to whether the main between-subjects effect is due to Equal or Earned, though there is evidence to believe that both of these axes likely have a non-negligible effect on players' behavior: in particular, if we go back to Figure A1.5.3, we can see that while the Equal axis seems to have a clear negative effect on cooperation in the first game, the persistent difference between EU and RU in both games seems to be the main driver of the estimated Earned effect.

Table A1.5.3. Average cooperation as a function of the exp. conditions (between-subjects).
Models with clustered standard errors

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.818*** (0.029)	0.676*** (0.037)	0.808*** (0.025)	0.816*** (0.030)
Earned	-0.051 (0.047)	-0.069 (0.054)	-0.061˙ (0.036)	-0.051 (0.047)

Equal	-0.103* (0.045)	-0.059 (0.058)	-0.081* (0.037)	-0.103* (0.045)
Earned x Equal	0.064 (0.066)	0.074 (0.083)	0.072 (0.052)	0.064 (0.066)
Second			-0.121*** (0.009)	-0.136** (0.042)
Earned x Second				-0.021 (0.072)
Equal x Second				0.044 (0.071)
Earned x Equal x Second				0.017 (0.108)

Linear mixed-effects models

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.818*** (0.034)	0.676*** (0.042)	0.773*** (0.020)	0.767*** (0.021)
Earned	-0.051 (0.046)	-0.069 (0.058)	-0.039*** (0.009)	-0.042** (0.013)
Equal	-0.103* (0.046)	-0.059 (0.058)	-0.005 (0.009)	0.004 (0.013)
Earned x Equal	0.064 (0.065)	0.074 (0.083)	0.015 (0.013)	0.025 (0.018)
Second			-0.121*** (0.004)	-0.111*** (0.012)
Earned x Second				0.007 (0.018)
Equal x Second				-0.018 (0.018)
Earned x Equal x Second				-0.021 (0.025)

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 1440, 1600, and 2880, respectively, clustered inside 160 sessions. All models control for round by including round dummies as predictors. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Appendix 1.6: Tie formation/breakage models

Figures A1.6.1 and A1.6.2 visualize tie formation patterns across games, rounds, and endowment regimes. Table A1.6.1 presents results from the subject-level between-subjects tie formation models. The upper panel presents estimates not controlling for alter's cooperation choice, while the lower panel presents estimates controlling for alter's cooperation choice. Both sets of models use clustered standard errors. The network-level counterparts of these models are presented in Table A1.6.2. Figure A1.6.3 visualizes the effect of changing from one endowment regime to another on tie formation. Table A1.6.3 presents results from the within-subjects tie formation models. The upper panel presents estimates from the subject-level models, while the lower panel presents estimates from the network-level models. Both sets of models use clustered standard errors. Figures A1.6.4, A1.6.5, and A1.6.6 and Tables A1.6.4, A1.6.5, and A1.6.6 repeat the above analyses with outcome as tie breakage.

Table A1.6.1. Add tie choice as a function of endowment regimes (between-subjects).

Models not controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	1.191 (0.116)***	1.012 (0.114)***	1.183 (0.081)***	1.187 (0.098)***
Earned	-0.089 (0.120)	-0.258 (0.109)*	-0.189 (0.075)*	-0.088 (0.120)
Equal	-0.198 (0.102)·	-0.086 (0.112)	-0.136 (0.071)·	-0.197 (0.101)
Earned x Equal	0.188 (0.157)	0.330 (0.148)*	0.276 (0.096)**	0.187 (0.157)
Second			-0.167 (0.041)***	-0.181 (0.124)
Earned x Second				-0.165 (0.188)
Equal x Second				0.127 (0.176)
Earned x Equal x Second				0.139 (0.253)

Models controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	-0.146 (0.121)	-0.161 (0.143)	-0.223 (0.097)*	-0.250 (0.102)*
Earned	0.029 (0.099)	-0.181 (0.114)	-0.078 (0.073)	0.041 (0.100)
Equal	0.008 (0.090)	-0.017 (0.111)	0.015 (0.071)	0.029 (0.092)
Earned x Equal	0.049 (0.130)	0.331 (0.152)*	0.187 (0.102)·	0.036 (0.132)
Alter's coop choice	1.642 (0.082)***	1.932 (0.090)***	1.795 (0.075)***	1.794 (0.075)***
Second			0.075 (0.041)·	0.131 (0.106)
Earned x Second				-0.207 (0.157)
Equal x Second				-0.030 (0.151)

Earned x Equal x Second				0.272 (0.203)

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 12826, 15205, and 26100, clustered inside 160 sessions and 1832, 1785, and 1858 players, respectively. All models control for round by including round dummies as predictors. Estimates are left in the original log-odds scale. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.6.2. Average add choice as a function of endowment regimes (between-subjects).

Models not controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.774 (0.023)	0.742 (0.025)***	0.776 (0.018)***	0.767 (0.021)***
Earned	0.003 (0.029)	-0.076 (0.027)**	-0.038 (0.019)*	0.003 (0.029)
Equal	-0.040 (0.025)	-0.040 (0.028)	-0.039 (0.018)*	-0.040 (0.025)
Earned x Equal	0.028 (0.037)	0.112 (0.038)**	0.071 (0.025)**	0.028 (0.037)
Second			-0.036 (0.010)***	-0.019 (0.027)
Earned x Second				-0.080 (0.044)˙
Equal x Second				0.003 (0.041)
Earned x Equal x Second				0.085 (0.059)

Models controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.457 (0.031)***	0.481 (0.030)***	0.457 (0.025)***	0.450 (0.026)***
Earned	0.018 (0.023)	-0.048 (0.021)*	-0.015 (0.015)	0.018 (0.023)
Equal	0.002 (0.021)	-0.006 (0.022)	0.000 (0.015)	0.002 (0.021)
Earned x Equal	0.003 (0.029)	0.075 (0.030)*	0.038 (0.021)˙	0.003 (0.029)
Alter's coop choice	0.376 (0.028)***	0.370 (0.026)***	0.381 (0.022)***	0.379 (0.022)***
Second			0.012 (0.009)	0.030 (0.022)
Earned x Second				-0.065 (0.033)*
Equal x Second				-0.006 (0.032)
Earned x Equal x Second				0.070 (0.043)

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 1231, 1366, and 2449, respectively, clustered inside 160 sessions. All models control for round. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.6.3. Tie formation as a function of endowment regimes (within-subjects).

Subject-level models

	Not controlling for alter's coop choice	Controlling for alter's coop choice
Intercept	-0.047 (0.033)	0.013 (0.029)
Δ Earned	-0.013 (0.023)	-0.006 (0.021)
Δ Equal	0.027 (0.021)	0.019 (0.018)
Δ Earned x Equal	0.017 (0.030)	0.029 (0.029)
Δ Alter's coop choice		0.377 (0.021)***

Network-level models

	Not controlling for alter's coop choice	Controlling for alter's coop choice
Intercept	-0.050 (0.025)*	0.005 (0.023)
Δ Earned	-0.022 (0.016)	-0.012 (0.015)
Δ Equal	-0.019 (0.017)	-0.012 (0.015)
Δ Earned x Equal	0.035 (0.023)	0.027 (0.023)
Δ Alter's coop choice		0.397 (0.034)***

The number of samples (*n*) for the individual-level models fit is 13171, clustered inside 1558 players and 160 sessions. The number of samples (*n*) for the session-level models fit is 1184, clustered inside 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.6.4. Cut tie choice as a function of endowment regimes (between-subjects).

Models not controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	-1.093 (0.151)***	-0.763 (0.123)***	-1.064 (0.107)***	1.099 (0.143)***
Earned	-0.088 (0.169)	0.177 (0.146)	0.060 (0.102)	-0.087 (0.169)
Equal	0.385 (0.151)*	0.057 (0.153)	0.235 (0.113)*	0.385 (0.150)*
Earned x Equal	-0.079 (0.220)	-0.226 (0.224)	-0.165 (0.135)	-0.079 (0.220)
Second			0.268 (0.042)***	0.343 (0.152)*
Earned x Second				0.237 (0.248)
Equal x Second				-0.323 (0.208)
Earned x Equal x Second				-0.102 (0.355)

Models controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.430 (0.137)**	0.491 (0.119)***	0.476 (0.099)***	0.505 (0.118)***
Earned	-0.248 (0.113)*	0.108 (0.115)	-0.067 (0.079)	-0.261 (0.111)*
Equal	0.132 (0.110)	0.087 (0.129)	0.106 (0.084)	0.118 (0.109)
Earned x Equal	0.159 (0.152)	-0.293 (0.163)˙	-0.042 (0.111)	0.175 (0.150)
Alter's coop choice	-1.993 (0.091)***	-2.192 (0.096)***	-2.108 (0.083)***	-2.106 (0.083)***
Second			0.008 (0.041)	-0.057 (0.112)
Earned x Second				0.347 (0.165)*
Equal x Second				-0.026 (0.162)
Earned x Equal x Second				-0.393 (0.225)˙

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 12348, 12365, and 23482, clustered inside 160 sessions and 1842, 1784, and 1857 players, respectively. All models control for round by including round dummies as predictors. Estimates are left in the original log-odds scale. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.6.5. Average cut choice as a function of endowment regimes (between-subjects).

Models not controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.256 (0.028)***	0.285 (0.026)***	0.239 (0.021)***	0.249 (0.026)***
Earned	-0.021 (0.030)	0.070 (0.033)*	0.026 (0.022)	-0.021 (0.030)
Equal	0.066 (0.029)*	0.055 (0.037)	0.061 (0.025)*	0.066 (0.029)*
Earned x Equal	-0.004 (0.043)	-0.099 (0.051)˙	-0.050 (0.032)	-0.004 (0.043)
Second			0.060 (0.010)***	0.041 (0.028)
Earned x Second				0.094 (0.049)˙
Equal x Second				-0.010 (0.045)
Earned x Equal x Second				-0.091 (0.072)

Models controlling for alter's cooperation choice

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.644 (0.029)***	0.628 (0.026)***	0.641 (0.023)***	0.652 (0.024)***
Earned	-0.047 (0.018)*	0.026 (0.022)	-0.010 (0.015)	-0.048 (0.018)**
Equal	0.010 (0.020)	0.016 (0.024)	0.014 (0.016)	0.009 (0.019)

Earned x Equal	0.039 (0.028)	-0.049 (0.032)	-0.001 (0.024)	0.040 (0.028)
Alter's coop choice	-0.482 (0.024)***	-0.491 (0.023)***	-0.494 (0.019)***	-0.493 (0.019)***
Second			-0.002 (0.009)	-0.025 (0.019)
Earned x Second				0.077 (0.029)**
Equal x Second				0.010 (0.028)
Earned x Equal x Second				-0.081 (0.038)*

The number of samples (*n*) for the models fit on the first game only, the second game only, and both games pooled together are 1279, 1434, and 2554, respectively, clustered inside 160 sessions. All models control for round. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.6.6. Tie breakage as a function of endowment regimes (within-subjects).

Subject-level models

	Not controlling for alter's coop choice	Controlling for alter's coop choice
Intercept	0.053 (0.031)˙	0.008 (0.028)
Δ Earned	0.006 (0.021)	-0.019 (0.019)
Δ Equal	-0.044 (0.024)˙	-0.050 (0.020)*
Δ Earned x Equal	0.022 (0.035)	0.028 (0.031)
Δ Alter's coop choice		-0.433 (0.024)***

Network-level models

	Not controlling for alter's coop choice	Controlling for alter's coop choice
Intercept	0.043 (0.024)˙	-0.005 (0.021)
Δ Earned	0.033 (0.020)˙	-0.0003 (0.016)
Δ Equal	0.014 (0.021)	0.0007 (0.019)
Δ Earned x Equal	-0.020 (0.029)	0.0020 (0.027)
Δ Alter's coop choice		-0.489 (0.031)***

The number of samples (*n*) for the individual-level models fit is 10083, clustered inside 1540 players and 160 sessions. The number of samples (*n*) for the session-level models fit is 1274, clustered inside 160 sessions. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Figure A1.6.1. Overall patterns of tie formation.

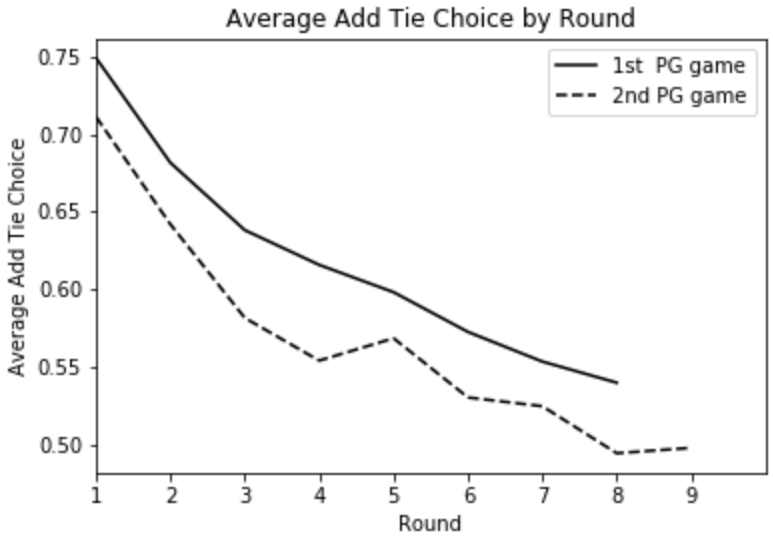


Figure A1.6.2. Average tie formation by round by endowment regime.

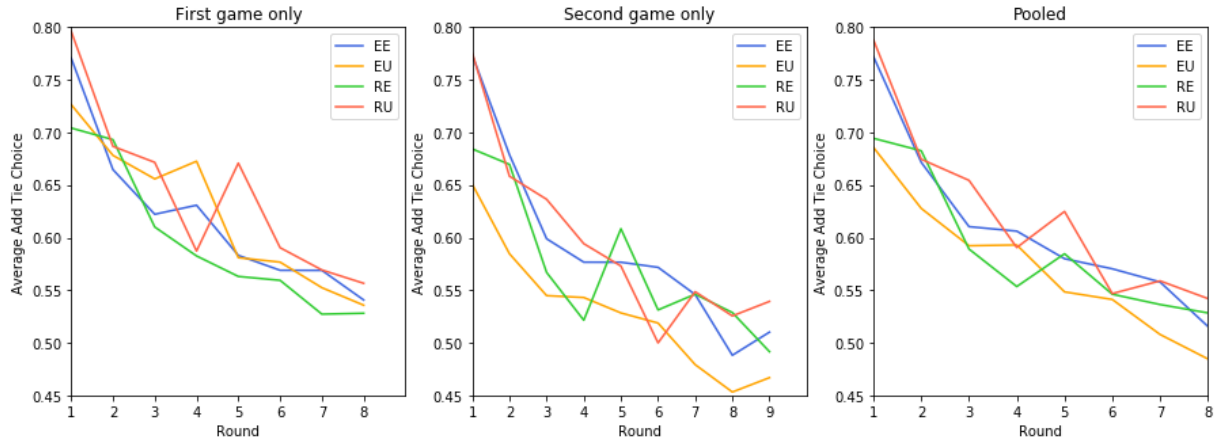


Figure A1.6.3. Heatmap of change in add tie choice between games.

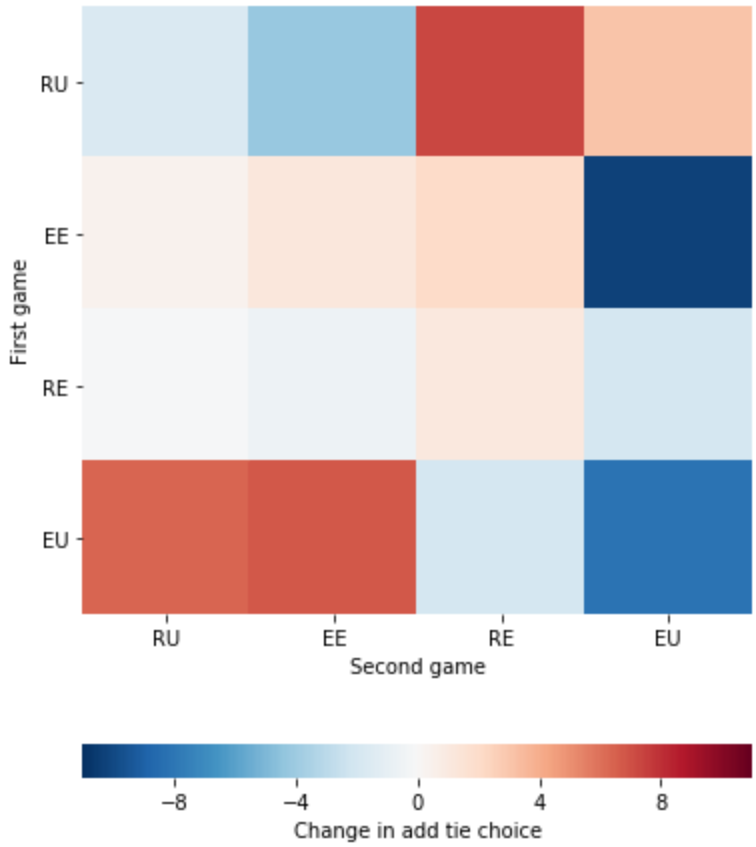


Figure A1.6.4. Overall patterns of tie breakage.

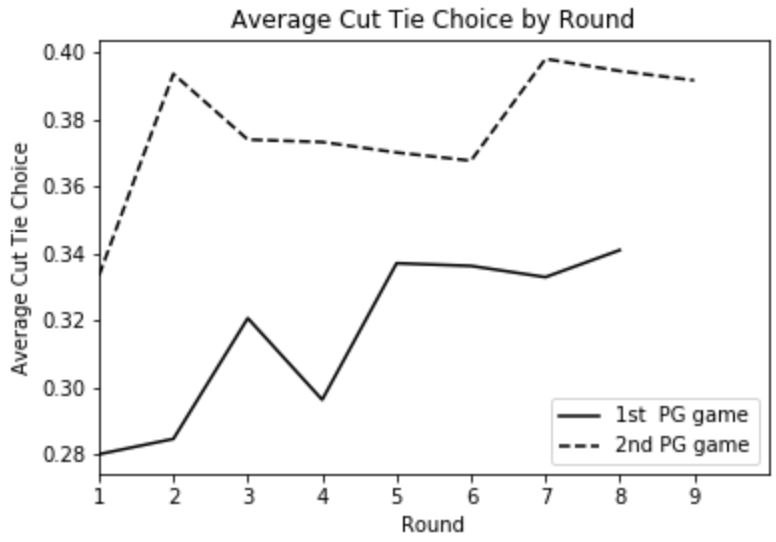


Figure A1.6.5. Average tie breakage by round by endowment regime.

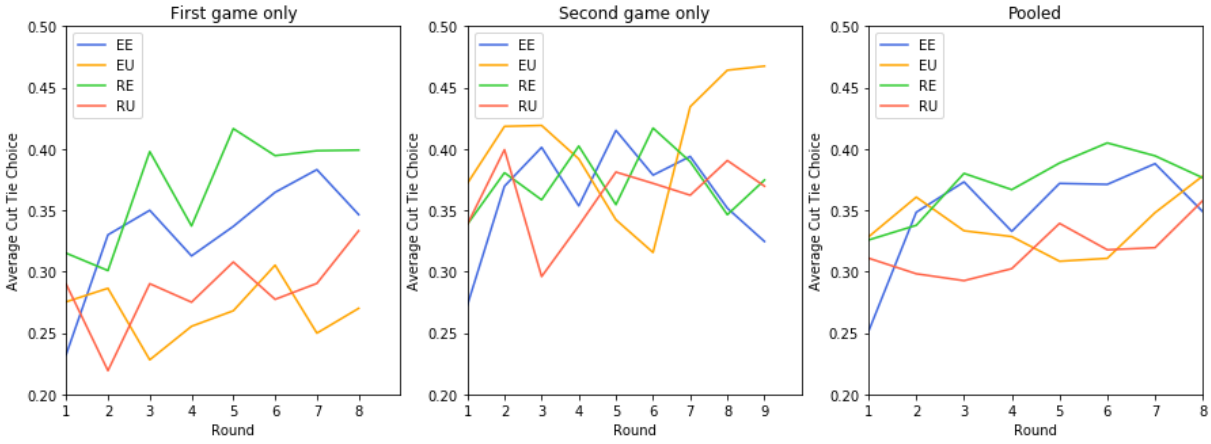
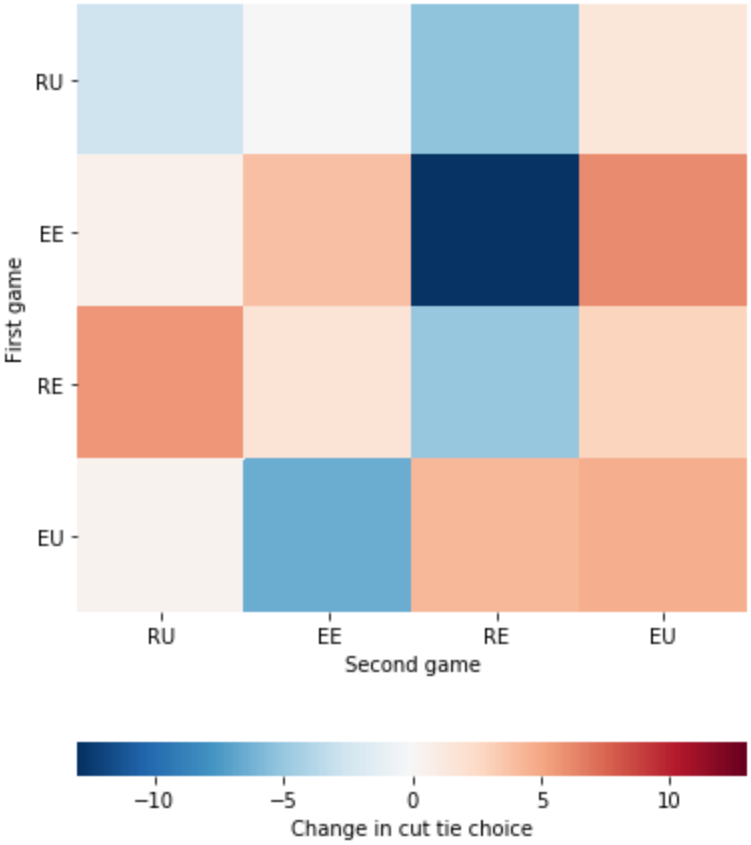


Figure A1.6.6. Heatmap of change in cut tie choice between games.



Appendix 1.7: Local inequality models

Tables A1.7.1, A1.7.2, and A1.7.3 below present behavioral results with local inequality (based on the Gini index) as the predictor. All models use clustered standard errors.

Table A1.7.1. Cooperation ~ local inequality.

Between-subjects, individual-level

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	1.935 (0.145)***	0.752 (0.158)***	1.510 (0.148)***	1.853 (0.118)***
Local inequality	-3.384 (0.285)***	-1.131 (0.568)*	-2.210 (0.502)***	-3.400 (0.280)***
Second			-0.436 (0.040)***	-0.961 (0.182)***
Local ineq. x Second				1.896 (0.581)**

Between-subjects, session-level

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.824 (0.043)***	0.676 (0.030)***	0.806 (0.030)***	0.951 (0.021)***
Local inequality	-0.334 (0.192)˙	-0.233 (0.115)*	-0.303 (0.137)*	-0.910 (0.059)***
Second			-0.102 (0.011)***	-0.281 (0.038)***
Local ineq. x Second				0.685 (0.121)***

Within-subjects

	Individual-level	Session-level
Intercept	-0.132 (0.013)***	-0.126 (0.013)***
Δ Local inequality	-0.006 (0.005)	-0.018 (0.023)

The number of samples (*n*) for the individual-level between-subjects models fit on the first game only, the second game only, and both games pooled together are 16526, 17823, and 32593, clustered inside 160 sessions and 1870, 1803, and 1870 players, respectively. The number of samples (*n*) for the session-level between-subjects models fit on the first game only, the second game only, and both games pooled together are 1437, 1597, and 2888, respectively, clustered inside 160 sessions. The number of samples (*n*) for the individual-level within-subjects models fit is 16016, clustered inside 1803 players and 160 sessions. The number of samples (*n*) for the session-level within-subjects models fit is 1434, clustered inside 160 sessions. All models control for round by including round dummies as predictors. Individual-level between-subjects estimates are left in the original log-odds scale. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.7.2. Add tie choice ~ local inequality.

Between-subjects, individual-level

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	1.355 (0.104)***	0.905 (0.096)***	1.147 (0.101)***	1.355 (0.090)***
Local inequality	-1.066 (0.178)***	-0.046 (0.052)	-0.268 (0.443)	-0.443 (0.098)***

Second			-0.164 (0.051)**	-1.065 (0.176)***
Local ineq. x Second				0.975 (0.274)***

Between-subjects, session-level

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.790 (0.019)***	0.725 (0.022)***	0.774 (0.015)***	0.779 (0.019)***
Local inequality	-0.117 (0.055)*	-0.074 (0.044)˙	-0.100 (0.038)**	-0.118 (0.056)*
Second			-0.032 (0.010)**	-0.040 (0.023)˙
Local ineq. x Second				0.028 (0.072)

Within-subjects

	Individual-level	Session-level
Intercept	-0.073 (0.030)*	-0.059 (0.025)*
Δ Local inequality	-0.008 (0.008)	-0.023 (0.019)

The number of samples (*n*) for the individual-level between-subjects models fit on the first game only, the second game only, and both games pooled together are 12768, 15153, and 26005, clustered inside 160 sessions and 1832, 1785, and 1858 players, respectively. The number of samples (*n*) for the session-level between-subjects models fit on the first game only, the second game only, and both games pooled together are 1228, 1363, and 2443, respectively, clustered inside 160 sessions. The number of samples (*n*) for the individual-level within-subjects models fit is 13061, clustered inside 1557 players and 160 sessions. The number of samples (*n*) for the session-level within-subjects models fit is 1178, clustered inside 160 sessions. All models control for round by including round dummies as predictors. Individual-level between-subjects estimates are left in the original log-odds scale. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A1.7.3. Cut tie choice ~ local inequality.

Between-subjects, individual-level

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	-1.198 (0.220)***	-0.896 (0.131)***	-1.149 (0.120)***	-1.190 (0.224)***
Local inequality	1.257 (0.913)	0.992 (0.393)*	1.132 (0.444)*	1.291 (0.906)
Second			0.203 (0.049)***	0.269 (0.250)
Local ineq. x Second				-0.243 (0.961)

Between-subjects, session-level

	1st game only	2nd game only	Pooled	Pooled (flexible)
Intercept	0.243 (0.030)***	0.293 (0.025)***	0.243 (0.019)***	0.237 (0.030)***
Local inequality	0.170 (0.107)	0.141 (0.070)*	0.148 (0.060)*	0.170 (0.107)
Second			0.053 (0.011)***	0.062 (0.035)˙

Local ineq. x Second				-0.034 (0.127)

Within-subjects

	Individual-level	Session-level
Intercept	0.053 (0.032)˙	0.038 (0.024)
Δ Local inequality	0.124 (0.087)	0.006 (0.033)

The number of samples (*n*) for the individual-level between-subjects models fit on the first game only, the second game only, and both games pooled together are 12294, 12358, and 23421, clustered inside 160 sessions and 1842, 1783, and 1857 players, respectively. The number of samples (*n*) for the session-level between-subjects models fit on the first game only, the second game only, and both games pooled together are 1276, 1431, and 2548, respectively, clustered inside 160 sessions. The number of samples (*n*) for the individual-level within-subjects models fit is 10035, clustered inside 1536 players and 160 sessions. The number of samples (*n*) for the session-level within-subjects models fit is 1268, clustered inside 160 sessions. All models control for round by including round dummies as predictors. Individual-level between-subjects estimates are left in the original log-odds scale. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Appendix 2.1: Experimental texts, images, videos, and other related content
Those interested in seeing how the survey looked like from the respondents' perspective can take the survey for themselves by going to this link. The complete set of texts and figures as well as links to the videos used in the experiment are provided below. (As also mentioned in the main text, most of the questions related to attitudes, preferences, and demographics as well as a large chunk of the consent text are taken directly from the study by Kuziemko et al 2015.)

Welcome to our survey!

The goal of this survey is to understand the public's knowledge and opinions regarding important societal and economic trends in the US.

At no point in the survey should you feel obligated to answer in a particular way; the most important factor for the success of our research is that you **answer honestly**. Anytime you don't know an answer, just give your best guess.

It is also very important for the success of our research project that you **complete the survey until the end**, once you have started. This survey should take (on average) about 10 to 15 minutes to complete.

Notes:
Your participation in this study is purely voluntary, and you may withdraw your participation or your data at any time without any penalty to you. Your name will never be recorded. Results may include summary data, but you will never be identified. If you have any questions about this study, you may contact us at fai_project@demog.berkeley.edu.

YOU MUST BE A **US** RESIDENT TO PARTICIPATE IN THIS SURVEY

- Yes, I would like to take part in this study, and confirm that I AM A US RESIDENT and am 18 or older
- No, I would not like to participate

IF ANSWER = Yes
    CONTINUE
ELSE
    END SURVEY

We'll start by giving you a short quiz related to some societal and economic trends in the US.

Note: the answers to the questions will be presented in the form of a video, so be prepared to watch a short clip with subtitles. Simply press [YouTube play symbol] to hear the answer!

What percentage of Americans do you think rank strengthening the economy as a top priority?

- Less than 50%
- Around 50%
- More than 50%

IF ANSWER = More than 50%

That is correct! Many polls have shown that the overwhelming majority of Americans view strengthening the economy as a top priority. One recent poll conducted by a respectable research center found that about 70% of Americans consider this issue a top priority.

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LINK TO VIDEO

ELSE

Not quite. Many polls have shown that the overwhelming majority of Americans view strengthening the economy as a top priority. One recent poll conducted by a respectable research center found that about 70% of Americans consider this issue a top priority.

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What percentage of American teens do you think work during the summer?

- Less than 50%
- Around 50%
- More than 50%

IF ANSWER = Less than 50%

That is correct! Recent polls have shown that less than 50% of American teens work during the summer. One recent poll conducted by a respectable research center found that 35% of American teens between the ages 16 to 19 have summer jobs.

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EXPERIMENTAL MANIPULATIONS START HERE.

FIRST, RANDOMLY DECIDE WHETHER TO SHOW INEQUALITY OR MOBILITY QUESTION FIRST.

THEN, RANDOMLY DECIDE WHETHER TO SHOW THE PESSIMISTIC OR THE OPTIMISTIC TREATMENT.

Do you think <u>income inequality</u> in the US has increased or decreased in recent decades?

[Income inequality can be defined as the income gap between richer and poorer Americans. High inequality generally means less wealth for most Americans, while low inequality generally means more wealth.]

- Decreased (More wealth for most)
- Stayed the same
- Increased (Less wealth for most)

IF INEQUALITY CONDITION = PESSIMISTIC

IF ANSWER = Increased (Less wealth for most)

That is correct! Income inequality in the US has rapidly increased in recent decades, reaching extreme levels. Based on a recent report published by a group of prominent researchers, while typical earners experienced only a meager 7% increase in their wages between 2000 and 2018, the richest 5% experienced a 25% increase in earnings.

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IF INEQUALITY CONDITION = OPTIMISTIC

IF ANSWER = Decreased (More wealth for most) OR Stayed the same

That is correct! Income inequality in the US has stopped growing. Based on a recent report published by a group of prominent researchers, income inequality today is practically the same as income inequality in 2000. As a matter of fact, economic inequality actually decreased during the period 2007 to 2014.

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Do you think economic mobility in the US has increased or decreased in recent decades?

[Economic mobility can be defined as children doing better than their parents income-wise. High mobility generally means rising incomes for most Americans, while low mobility generally means falling incomes.]

- Decreased (Falling incomes for most)
- Stayed the same
- Increased (Rising incomes for most)

IF MOBILITY CONDITION = PESSIMISTIC

IF ANSWER = Decreased (Falling incomes for most)

That is correct! Economic mobility in the US is decreasing at an alarming rate. Based on a recent report published by a group of prominent researchers, while 90% of children were doing better than their parents in the 1970s, only 51% of children today are doing better. Experts are interpreting this to mean that the American Dream is fading fast, and there is not enough opportunity for people to get ahead in life.

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IF MOBILITY CONDITION = OPTIMISTIC

IF ANSWER = Increased (Rising incomes for most)

That is correct! Economic mobility in the US is on the rise. Based on a recent report published by a group of prominent researchers, 73% of children today are better off financially compared to their parents. Experts are interpreting this to mean that the American Dream is still alive, and there is plenty of opportunity for people to get ahead in life.

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Now, we'll be asking you a few questions regarding your opinions on certain matters.

QUESTIONS WILL PRESENTED IN A RANDOM ORDER.

THE LAST TWO QUESTIONS ALWAYS APPEAR AT THE END.

Which statement do you agree with most?

- One's income and position in society is mostly the result of one's individual effort
- One's income and position in society is to a large extent the outcome of elements outside of one's control (e.g., family background, luck, health issues)

Which do you favor: promoting equal opportunity or promoting equal outcomes?

[Equal opportunity can be defined as allowing everyone to compete for jobs and wealth on a fair and even basis. Equal outcomes can be defined as insuring that everyone has a decent standard of living and that there are only small differences in wealth and income between the top and bottom in society.]

- Equal opportunity
- Equal outcomes

Where would you rate yourself on a scale of 1 to 5, where 1 means you think the government should do only those things necessary to provide the most basic government functions, and 5 means you think the government should take active steps in every area it can to try and improve the lives of its citizens?

- 1 - The government should do only those things necessary to provide the most basic government functions
- 2
- 3
- 4
- 5 - The government should take active steps in every area it can to try and improve the lives of its citizens

Do you think inequality is a serious problem in America?

- Not a problem at all
- A small problem
- A problem
- A serious problem
- A very serious problem

Which of the tools below do you consider the best to address inequality in the United States?

[Please drag and drop the items below to rank them in your preferred order. Your most preferred method for addressing inequality should be at the top (1), your least preferred one at the bottom (5).]

- Education Policies
- Government Regulation (e.g., min wage, caps on top compensation)
- Government Transfers (e.g., food stamps, Medicaid)
- Private Charity
- Progressive Taxes

Describe in your own words what the government can do, if anything, to fix inequality in the US.

TEXT ENTRY HERE

Do you think that the very high earners in our society deserve their high incomes?

- Most of the time
- Sometimes
- Rarely

Do you think income taxes on millionaires should be increased, stay the same or decreased?

- Increased
- Stay the same
- Decreased

Do you think the Federal Estate tax should be decreased, left as is or increased?

[The Federal Estate tax, also known as the Death Tax, is a tax imposed on the transfer of wealth from a deceased person to his or her heirs.]

- Increased
- Left as is
- Decreased

Do you think the minimum wage should be decreased, stay the same or increased?

[The minimum wage is currently $7.25 per hour.]

- Decreased
- Stay the same
- Increased

Should the federal government increase or decrease spending on aid to the poor?

- Significantly increase
- Slightly increase
- Keep at current level
- Slightly decrease
- Significantly decrease

Should the federal government increase or decrease its spending on food stamps?

[Food stamps provide financial assistance for food purchasing to families and individuals with low or no income.]

- Significantly increase
- Slightly increase
- Keep at current level
- Slightly decrease
- Significantly decrease

Should the federal government provide support for entrepreneurs (such as grants to help people start small businesses)?

- Yes
- No

Should the federal government help people in the face of high housing costs (such as creating opportunities for affordable housing)?

- Yes
- No

Do you think the income gap between richer and poorer Americans are decreasing or increasing?

- Decreasing
- Same
- Increasing

Do you think American children today have better or worse chances economically compared to their parents?

- Better
- Same
- Worse

Finally, please answer the following standard demographic questions.

Are you a US resident?

- Yes
- No

In which state do you currently reside?

DROP-DOWN LIST OF STATES HERE

What is your gender?

- Male
- Female
- Other

What is your age? [Enter a number (e.g., 35)]

TEXT ENTRY HERE

What is your marital status?

- Single
- Married

Do you have children living with you?

- Yes
- No

How would you describe your ethnicity/race?

- European American/White
- African American/Black
- Hispanic/Latino
- Asian/Asian American
- Other

Which category best describes your highest level of education?

- Eighth Grade or Less
- Some High School
- High School Degree/GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree
- Professional Degree (JD, MD, MBA)

What is your current employment status?

- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not in labor force (for example: retired, or full-time parent)

What was your TOTAL household income, before taxes, last year?

- $0 - $9,999
- $10,000 - $14,999
- $15,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $74,999
- $75,000 - $99,999
- $100,000 - $124,999
- $125,000 - $149,999
- $150,000 - $199,999
- $200,000+

Compared to your parents when they were the age you are now, do you think your own standard of living now is better or worse than theirs was?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse

Compared to 10 years ago, do you think your standard of living now is better or worse?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse

Which best describes your household's income each month?

- Income is about the same each month
- Income varies somewhat from month to month
- Income varies a lot from month to month

On economic policy matters, where do you see yourself on the liberal/conservative spectrum?

- Very conservative
- Conservative
- Moderate
- Liberal
- Very liberal

Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?

- Republican
- Democrat
- Independent
- None

THE TEXT BELOW IS SHOWN TO RESPONDENTS BECAUSE IRB REQUIRES IT

After submitting your responses, you can protect your privacy by clearing your browser's history, cache, cookies, and other browsing data. (Warning: This will log you out of online services.)

Appendix 2.2: Sample size calculations and variables

Sample size calculations

Sample size calculations are made with the aim of being able to detect a small interaction effect in two-way ANOVA. Since the survey experiment employs a 2x2 factorial design, and since the interaction effect is of particular importance to the study, this approach is the right one to take. Calculations correct for multiple testing. The exact procedure used to calculate power is implemented in `R` in the `powerInteract` library as follows. (Source code can be publicly viewed at https://rdrr.io/cran/powerMediation/src/R/power_interaction.R.)

```
powerInteract=function(nTotal, a, b, effsize, alpha=0.05, nTests=1)
{
  alpha2=alpha/nTests

  nPerCell=floor(nTotal/(a*b))
  df1=(a-1)*(b-1)
  df2=a*b*(nPerCell-1)

  F0=qf(p=1-alpha2, df1=df1, df2=df2, ncp=0)

  ncp=nPerCell*effsize^2
  power=1-pf(q=F0, df1=df1, df2=df2, ncp=ncp)
  return(power)
}
```

where:

```
nTotal = number of observations in total
a = number of levels in factor 1
b = number of levels in factor 2
effsize = effect size
alpha = type I error rate
nTests = number of tests if multiple testing
```

Using this function, and assuming a small effect size (Cohen's $d = 0.15$) and 25 tests, a total of 2,800 respondents gives us more than 80% power to be able to detect the interaction effect. (Note that Cohen's d is defined as `mu/sigma`, where `mu` is the raw effect size in the original scale and `sigma` is the standard deviation of the outcome variable.)

```
powerInteract(nTotal=2800, a=2, b=2, effsize=0.15, alpha=0.05, nTests=25)
```

Given that it is harder to detect a small interaction effect compared to a small main effect, having enough power to detect a small interaction effect guarantees that we also have enough power to be able to detect the main effects.

Additional variables

In addition to the core variables mentioned in the main text, the dataset includes a set of auxiliary variables that can be helpful in later analyses:

- Respondent's answer to the economy quiz question
    - Categories: Less than 50%; Around 50%; More than 50%
- Respondent's answer to the teen summer jobs quiz question
    - Categories: Less than 50%; Around 50%; More than 50%
- Respondent's answer to the inequality quiz question
    - Categories: Decreased (More wealth for most); Stayed the same; Increased (Less wealth for most)
- Respondent's answer to the mobility quiz question
    - Categories: Increased (Rising incomes for most); Stayed the same; Decreased (Falling incomes for most)
- Respondent's ranking of the methods for addressing inequality
    - Categories: Education Policies; Government Regulation (e.g., min wage, caps on top compensation); Government Transfers (e.g., food stamps, Medicaid); Private Charity; Progressive Taxes
- Respondent's open-ended answer to what the government can do, if anything, to fix inequality
- Respondent's sense of their income volatility
    - Categories: Income is about the same each month; Income varies somewhat from month to month; Income varies a lot from month to month
- Whether the inequality or the mobility treatment is shown first to the respondent
- The precise order in which the outcome questions are shown to the respondent
- Standard Qualtrics metadata including date, time, and duration of a survey session

Distribution of respondents across states

Respondents who took the survey come from all over the US. Table A2.2.1 below presents the distribution of respondents across the 50 states and DC.

Table A2.2.1. Total number of respondents in each state.

State	Number of respondents
Alabama	40
Alaska	7
Arizona	66
Arkansas	20
California	273
Colorado	54
Connecticut	37
Delaware	10
District of Columbia	2
Florida	189
Georgia	98
Hawaii	9
Idaho	19
Illinois	90
Indiana	52
Iowa	32
Kansas	23

Kentucky	40
Louisiana	36
Maine	20
Maryland	67
Massachusetts	62
Michigan	67
Minnesota	28
Mississippi	27
Missouri	59
Montana	15
Nebraska	15
Nevada	45
New Hampshire	11
New Jersey	78
New Mexico	20
New York	215
North Carolina	92
North Dakota	2
Ohio	107
Oklahoma	30
Oregon	39
Pennsylvania	142
Rhode Island	14
South Carolina	42
South Dakota	9
Tennessee	73
Texas	185
Utah	19
Vermont	3
Virginia	80
Washington	68
West Virginia	20
Wisconsin	44
Wyoming	5
Total	2,800

Appendix 2.3: (Ordered) logit models

The outcomes "income result of circumstances," "equal outcomes," "support entprens," and "support housing" are modeled using logistic regression. All other outcomes are modeled using ordered logistic regression. The upper side of each table includes as predictors only the two experimental factors and their interaction, while the lower side of the table also controls for the existing demographic covariates: age (mean-centered), gender (reference category: male), marital status (reference category: single), has children living with them (reference category: no), ethnicity/race (reference category: European American/White), highest level of education (reference category: some college), employment status (reference category: full-time employee), total household income before taxes (reference category: $40,000 - $49,999), liberal/conservative spectrum (reference category: moderate), and party identity (reference category: independent).

Table A2.3.1. Perception outcomes.

Models w/o any demographic covariates

	Income gap increasing	Children have worse chances	Inter-gen. mobility down	Intra-gen. mobility down
Ineq. pessimistic	1.809 (0.191)***	1.006 (0.102)	1.177 (0.112)˙	1.160 (0.109)
Opp. pessimistic	1.377 (0.142)**	3.743 (0.385)***	1.517 (0.144)***	1.324 (0.125)**
Ineq. pessimistic x Opp. pessimistic	0.891 (0.136)	0.865 (0.125)	0.733 (0.099)*	0.892 (0.120)

Models w/ demographic covariates

	Income gap increasing	Children have worse chances	Inter-gen. mobility down	Intra-gen. mobility down
Ineq. pessimistic	1.937 (0.212)***	1.010 (0.106)	1.182 (0.115)˙	1.217 (0.117)*
Opp. pessimistic	1.359 (0.145)**	4.001 (0.425)***	1.582 (0.153)***	1.334 (0.129)**
Ineq. pessimistic x Opp. pessimistic	0.914 (0.145)	0.892 (0.133)	0.711 (0.098)*	0.859 (1.118)

Coefficient estimates are in odds ratios. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A2.3.2. Attitudinal outcomes.

Models w/o any demographic covariates

	Income result of circumstances	Equal outcomes	Government should take active steps	Inequality is a serious problem	High earners rarely deserving
Ineq. pessimistic	1.133 (0.121)	1.125 (0.135)	1.292 (0.123)**	1.511 (0.145)***	1.182 (0.121)
Opp. pessimistic	1.057 (0.113)	1.056 (0.128)	1.201 (0.115)˙	1.350 (0.129)**	1.052 (0.108)
Ineq. pessimistic x Opp. pessimistic	1.006 (0.152)	0.887 (0.151)	0.715 (0.097)*	0.732 (0.099)*	0.970 (0.142)

Models w/ demographic covariates

	Income result of	Equal outcomes	Government should	Inequality is a	High earners

	circumstances		take active steps	serious problem	rarely deserving
Ineq. pessimistic	1.149 (0.131)	1.100 (0.138)	1.331 (0.130)**	1.570 (0.153)***	1.182 (0.124)
Opp. pessimistic	1.007 (0.115)	1.025 (0.129)	1.178 (0.116)˙	1.369 (0.134)**	1.010 (0.106)
Ineq. pessimistic x Opp. pessimistic	1.102 (0.178)	0.938 (0.167)	0.736 (0.103)*	0.764 (0.106)˙	1.043 (0.156)

Coefficient estimates are in odds ratios. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A2.3.3. Policy outcomes.

Models w/o any demographic covariates

	Increase taxes on millionaires	Increase estate tax	Increase minimum wage	Increase aid to the poor	Increase spending on food stamps	Support entprens	Support housing
Ineq. pessimistic	1.050 (0.116)	1.213 (0.121)˙	1.400 (0.170)**	1.147 (0.110)	1.058 (0.100)	1.061 (0.156)	1.348 (0.203)*
Opp. pessimistic	1.056 (0.118)	1.045 (0.105)	1.195 (0.142)	1.045 (0.100)	0.989 (0.094)	0.984 (0.143)	1.069 (0.155)
Ineq. pessimistic x Opp. pessimistic	1.030 (0.164)	0.893 (0.127)	0.744 (0.128)˙	0.979 (0.134)	1.045 (0.141)	0.996 (0.207)	0.706 (0.148)˙

Models w/ demographic covariates

	Increase taxes on millionaires	Increase estate tax	Increase minimum wage	Increase aid to the poor	Increase spending on food stamps	Support entprens	Support housing
Ineq. pessimistic	1.048 (0.123)	1.226 (0.126)*	1.435 (0.185)**	1.138 (0.111)	1.057 (0.103)	1.061 (0.159)	1.395 (0.222)*
Opp. pessimistic	1.010 (0.119)	1.052 (0.109)	1.179 (0.149)	1.003 (0.098)	0.976 (0.095)	0.947 (0.141)	1.052 (0.162)
Ineq. pessimistic x Opp. pessimistic	1.087 (0.183)	0.917 (0.134)	0.755 (0.138)	1.061 (0.147)	1.132 (0.156)	1.029 (0.218)	0.703 (0.156)

Coefficient estimates are in odds ratios. The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Appendix 2.4: OLS models

See Figure A2.4.1 for the outcome means in the four conditions corresponding to the outcomes not presented in the main text (first three are related to general attitudes and latter four are policy outcomes). See Tables A2.4.1 through A2.4.3 for the estimated inequality, opportunity, and interaction effects reported in the main text. The upper side of each table includes as predictors only the two experimental factors and their interaction, while the lower side of the table also controls for the existing demographic covariates: age (mean-centered), gender (reference category: male), marital status (reference category: single), has children living with them (reference category: no), ethnicity/race (reference category: European American/White), highest level of education (reference category: some college), employment status (reference category: full-time employee), total household income before taxes (reference category: $40,000 - $49,999), liberal/conservative spectrum (reference category: moderate), and party identity (reference category: independent). These effect estimates are also visualized in Figures A2.4.2 through A2.4.6.

Table A2.4.1. Perception outcomes.

Models w/o any demographic covariates

	Income gap increasing	Children have worse chances	Inter-gen. mobility down	Intra-gen. mobility down
Ineq. pessimistic	0.196 (0.037)***	0.004 (0.044)	0.120 (0.064)˙	0.110 (0.065)˙
Opp. pessimistic	0.106 (0.037)**	0.606 (0.044)***	0.293 (0.065)***	0.200 (0.065)**
Ineq. pessimistic x Opp. pessimistic	-0.046 (0.052)	-0.074 (0.063)	-0.225 (0.091)*	-0.080 (0.092)

Models w/ demographic covariates

	Income gap increasing	Children have worse chances	Inter-gen. mobility down	Intra-gen. mobility down
Ineq. pessimistic	0.201 (0.036)***	0.002 (0.043)	0.118 (0.060)˙	0.125 (0.061)*
Opp. pessimistic	0.095 (0.036)**	0.593 (0.043)***	0.287 (0.061)***	0.186 (0.061)**
Ineq. pessimistic x Opp. pessimistic	-0.034 (0.051)	-0.053 (0.061)	-0.224 (0.086)**	-0.082 (0.086)

The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A2.4.2. Attitudinal outcomes.

Models w/o any demographic covariates

	Income result of circumstances	Equal outcomes	Government should take active steps	Inequality is a serious problem	High earners rarely deserving
Ineq. pessimistic	0.031 (0.027)	0.023 (0.024)	0.198 (0.073)**	0.270 (0.062)***	0.058 (0.035)˙
Opp. pessimistic	0.014 (0.027)	0.010 (0.024)	0.136 (0.073)˙	0.201 (0.062)**	0.019 (0.036)
Ineq. pessimistic x Opp. pessimistic	0.002 (0.038)	-0.024 (0.033)	-0.261 (0.104)*	-0.211 (0.088)*	-0.011 (0.050)

Models w/ demographic covariates

	Income result of circumstances	Equal outcomes	Government should take active steps	Inequality is a serious problem	High earners rarely deserving
Ineq. pessimistic	0.030 (0.025)	0.018 (0.023)	0.190 (0.068)**	0.262 (0.056)***	0.055 (0.034)
Opp. pessimistic	0.001 (0.025)	0.005 (0.023)	0.108 (0.068)	0.177 (0.057)**	0.009 (0.034)
Ineq. pessimistic x Opp. pessimistic	0.022 (0.036)	-0.012 (0.033)	-0.197 (0.096)*	-0.157 (0.080)˙	0.009 (0.048)

The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A2.4.3. Policy outcomes.

Models w/o any demographic covariates

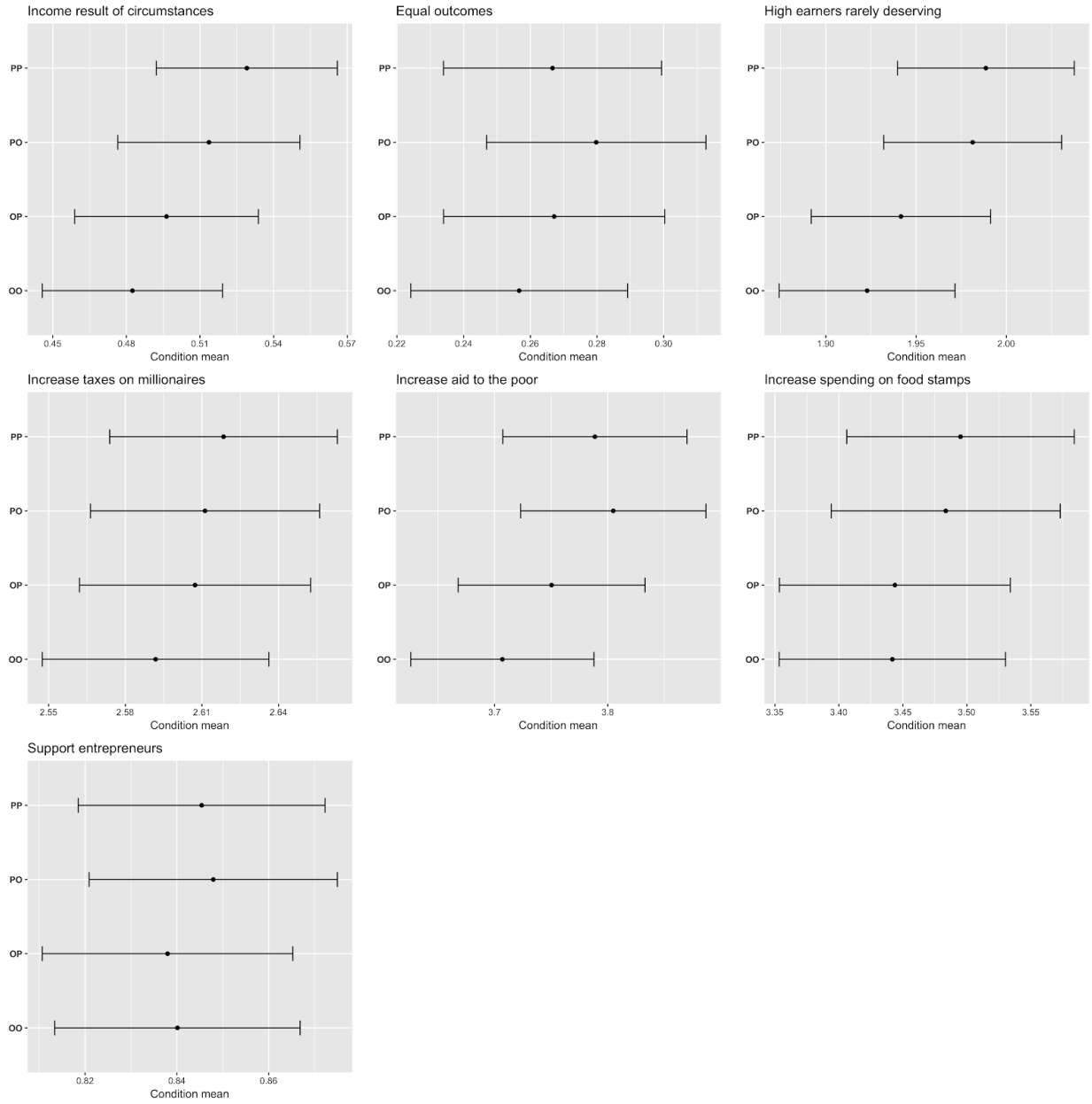
	Increase taxes on millionaires	Increase estate tax	Increase minimum wage	Increase aid to the poor	Increase spending on food stamps	Support entprens	Support housing
Ineq. pessimistic	0.019 (0.032)	0.079 (0.039)*	0.088 (0.029)**	0.098 (0.059)˙	0.042 (0.064)	0.008 (0.019)	0.038 (0.019)˙
Opp. pessimistic	0.015 (0.032)	0.025 (0.039)	0.056 (0.029)˙	0.043 (0.059)	0.002 (0.064)	-0.002 (0.019)	0.009 (0.019)
Ineq. pessimistic x Opp. pessimistic	-0.008 (0.046)	-0.051 (0.055)	-0.090 (0.041)*	-0.060 (0.083)	0.010 (0.091)	-0.0003 (0.028)	-0.045 (0.027)

Models w/ demographic covariates

	Increase taxes on millionaires	Increase estate tax	Increase minimum wage	Increase aid to the poor	Increase spending on food stamps	Support entprens	Support housing
Ineq. pessimistic	0.016 (0.031)	0.078 (0.037)*	0.082 (0.027)**	0.084 (0.056)	0.031 (0.059)	0.007 (0.019)	0.037 (0.019)*
Opp. pessimistic	0.003 (0.031)	0.025 (0.038)	0.044 (0.027)	0.022 (0.056)	-0.020 (0.059)	-0.007 (0.019)	0.007 (0.019)
Ineq. pessimistic x Opp. pessimistic	0.009 (0.044)	-0.037 (0.053)	-0.070 (0.039)˙	-0.011 (0.079)	0.065 (0.084)	0.005 (0.028)	-0.038 (0.027)

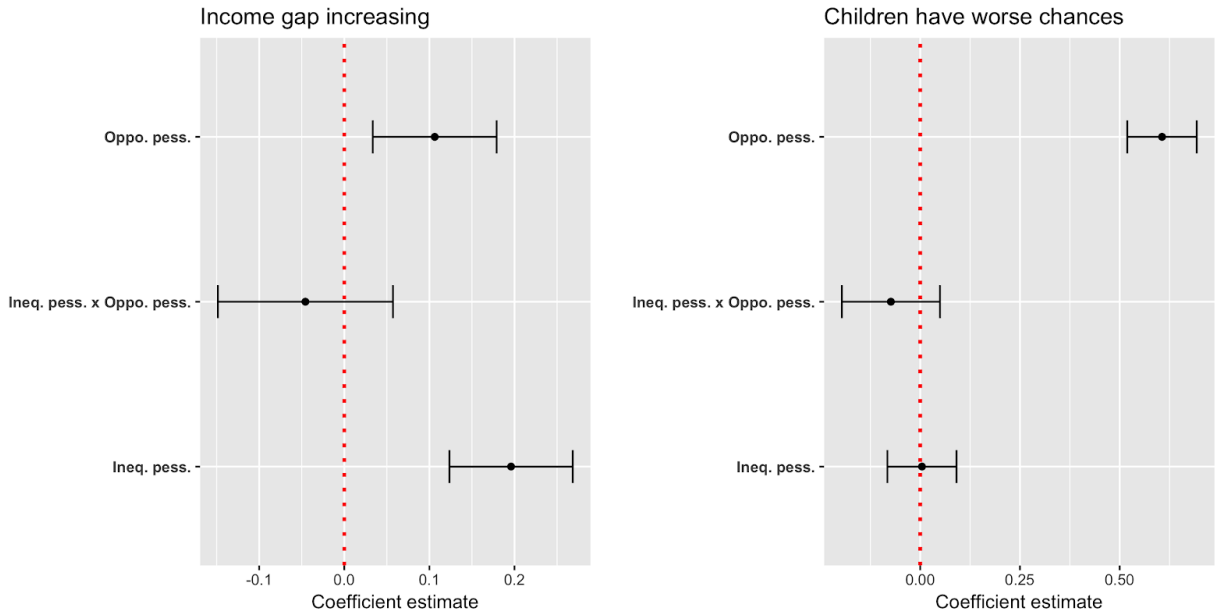
The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Figure A2.4.1. Other outcomes (predicted means).



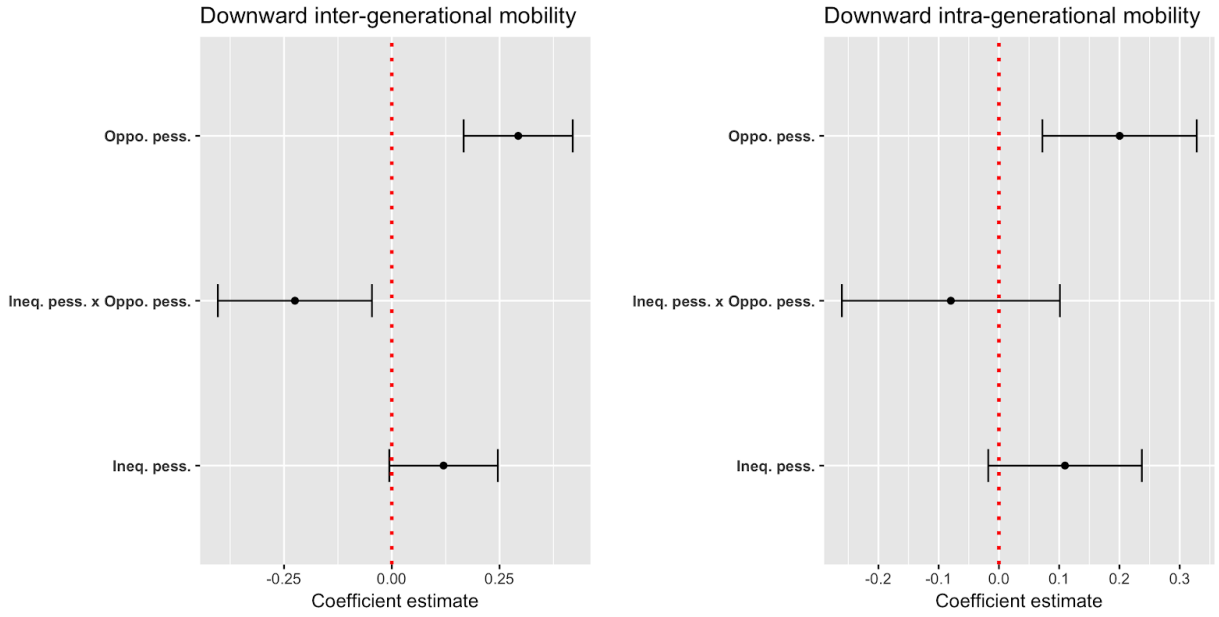
The bars denote 95% confidence intervals. PP: inequality pessimistic, opportunity pessimistic; PO: inequality pessimistic, opportunity optimistic; OP: inequality optimistic, opportunity pessimistic; OO: inequality optimistic, opportunity optimistic.

Figure A2.4.2. Perceptions regarding inequality and opportunity (effect estimates).



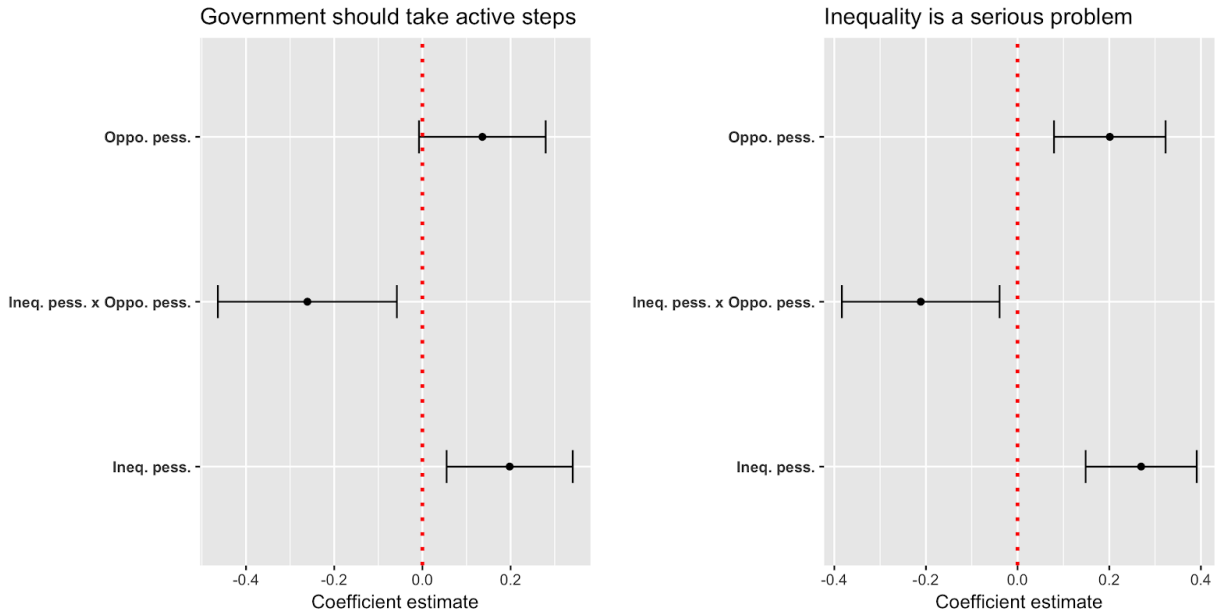
The bars denote 95% confidence intervals. The dashed red line at 0 corresponds to a null effect and is included to show which estimates are statistically significant. Ineq. pess.: inequality pessimistic; Oppo. pess.: opportunity pessimistic; Ineq. pess. x Oppo. pess.: the interaction term between inequality pessimistic and opportunity pessimistic.

Figure A2.4.3. Perceptions regarding mobility (effect estimates).



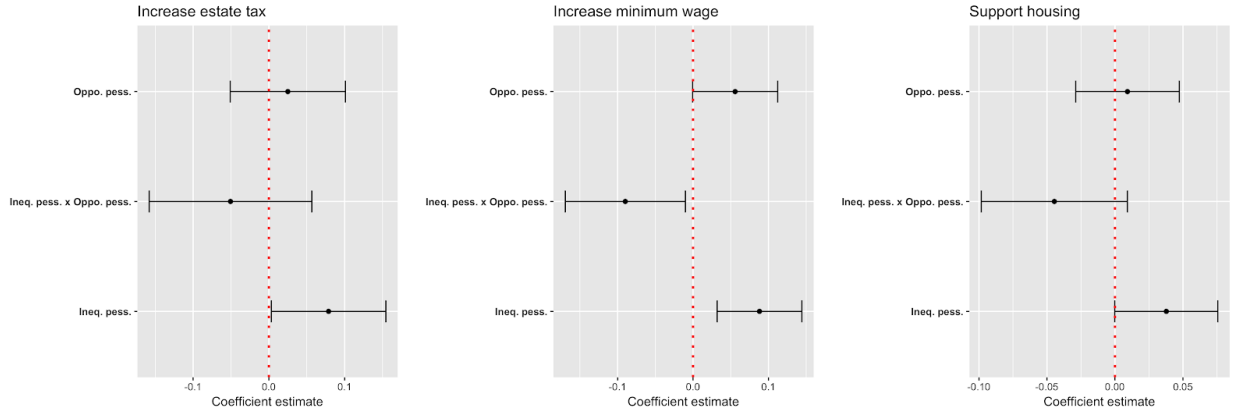
The bars denote 95% confidence intervals. The dashed red line at 0 corresponds to a null effect and is included to show which estimates are statistically significant. Ineq. pess.: inequality pessimistic; Oppo. pess.: opportunity pessimistic; Ineq. pess. x Oppo. pess.: the interaction term between inequality pessimistic and opportunity pessimistic.

Figure A2.4.4. General attitudes towards inequality (effect estimates).



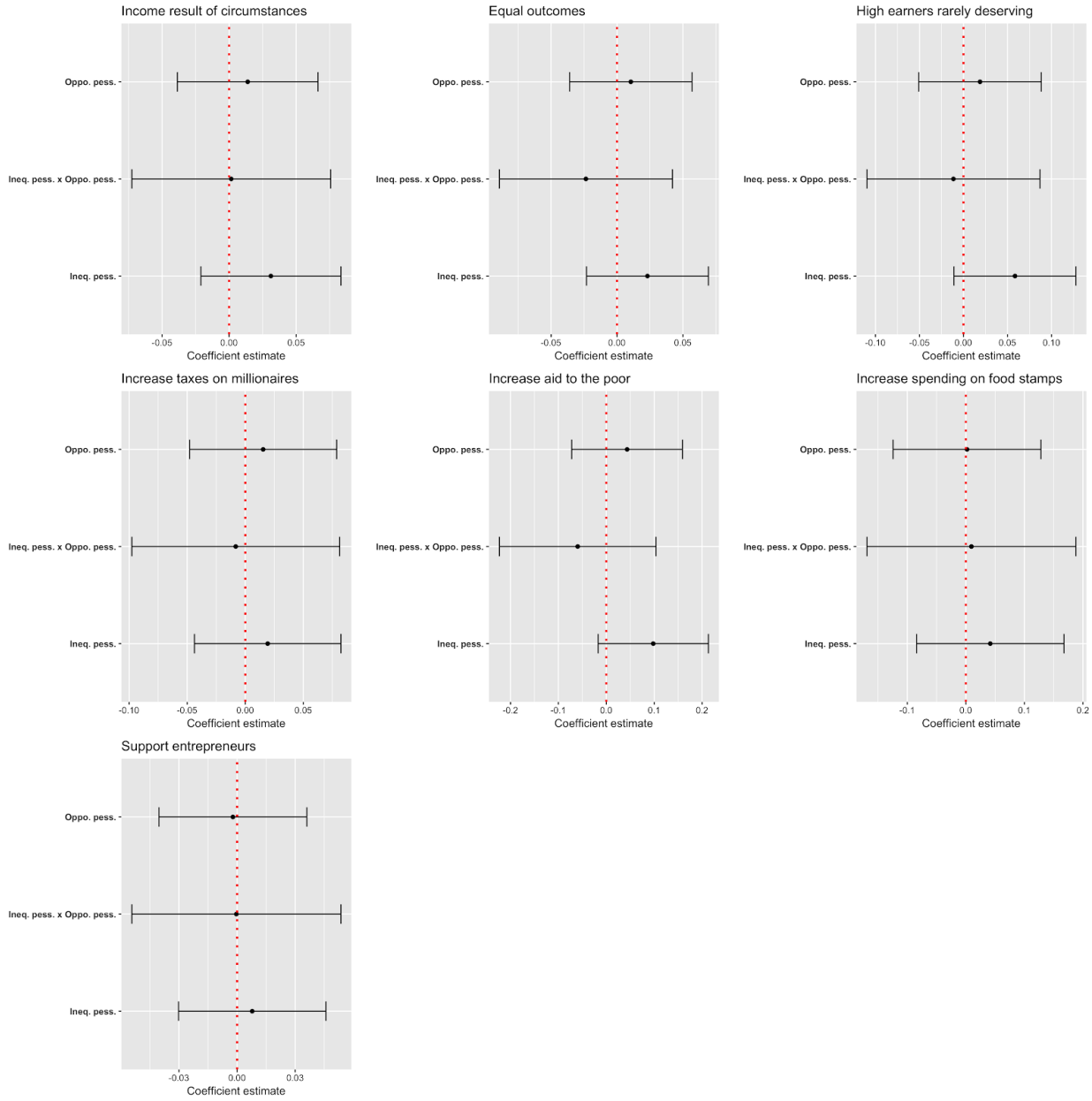
The bars denote 95% confidence intervals. The dashed red line at 0 corresponds to a null effect and is included to show which estimates are statistically significant. Ineq. pess.: inequality pessimistic; Oppo. pess.: opportunity pessimistic; Ineq. pess. x Oppo. pess.: the interaction term between inequality pessimistic and opportunity pessimistic.

Figure A2.4.5. Concrete policy preferences (effect estimates).



The bars denote 95% confidence intervals. The dashed red line at 0 corresponds to a null effect and is included to show which estimates are statistically significant. Ineq. pess.: inequality pessimistic; Oppo. pess.: opportunity pessimistic; Ineq. pess. x Oppo. pess.: the interaction term between inequality pessimistic and opportunity pessimistic.

Figure A2.4.6. Other outcomes (effect estimates).



The bars denote 95% confidence intervals. The dashed red line at 0 corresponds to a null effect and is included to show which estimates are statistically significant. Ineq. pess.: inequality pessimistic; Oppo. pess.: opportunity pessimistic; Ineq. pess. x Oppo. pess.: the interaction term between inequality pessimistic and opportunity pessimistic.

Appendix 2.5: Interactions with political orientation

Beyond the main goal of the study -- which is to estimate inequality and opportunity effects separately and jointly -- we can actually go one step further and see if these factors affect liberals and conservatives differently. In other words, given the known political divide in the country regarding how to address inequality, political orientation could be a significant *moderator* of the inequality and opportunity effects. For example, since liberals are already more inclined to support redistribution compared to conservatives, an informational treatment could be much more effective in swaying their preferences. In fact, Alesina, Stantcheva, and Teso (2018) finds that the pessimistic mobility effect in their study was mainly a result of a significant effect on left-wing respondents, while the treatment had little effect on right-wing respondents.

Accordingly, an additional set of models add interactions between the experimental predictors and political orientation (-2: very conservative, -1: conservative, 0: moderate, 1: liberal, 2: very liberal). Table A2.5.1 below presents results from this last set of models. For the sake of space, only results where at least one of these interactions is significant at the $p<0.1$ level are presented; estimates from the other models are generally very similar to the results presented in earlier tables (Tables A2.4.1-2.4.3) and do not change our conclusions.

The first point to be noted here is that having a more liberal orientation is associated with a higher chance of (i) thinking that the income gap is increasing, (ii) thinking children today have worse chances compared to parents, (iii) thinking income and position in society is a result of circumstances, and (iv) giving support to policies that address inequality. (Liberal orientation also has a similar effect on the other outcomes not presented in this table.) None of these results are surprising given what we know about the liberal/conservative divide in the country.

The more interesting results from this table are those related to the interactions between political orientation and the experimental manipulations. Based on these estimates, we have some evidence to argue that (i) having a more liberal orientation leads to smaller increases in the chances of thinking the income gap is increasing when either of the treatments is pessimistic; (ii) having a more liberal orientation leads to larger increases in the chances of thinking children have worse chances when *both* informational treatments are pessimistic; (iii) having a more liberal orientation leads to larger increases in the chances of thinking income is the result of circumstances when either the inequality or the opportunity treatment is pessimistic, while it leads to smaller increases when *both* treatments are pessimistic (a similar pattern is observed also in the case of advocating for increasing estate tax); (iv) having a more liberal orientation leads to smaller increases in the chances of advocating for increasing minimum wage when the inequality treatment is pessimistic, while it leads to larger increases when both treatments are pessimistic (a similar pattern is observed also in the case of advocating for supporting entrepreneurs); and (v) having a more liberal orientation leads to smaller increases in the chances of advocating for housing support when the inequality treatment is pessimistic.

Table A2.5.1. Experimental manipulations and political orientation.

Models w/o any demographic covariates

	Income gap increasing	Children have worse chances	Income result of circumstances	Increase estate tax	Increase minimum wage	Support entprens	Support housing
Inequality pessimistic	0.183 (0.036)***	-0.001 (0.043)	0.032 (0.026)	0.085 (0.038)*	0.076 (0.028)*	-0.001 (0.019)	0.031 (0.019)
Opportunity pessimistic	0.091 (0.037)*	0.598 (0.043)***	0.012 (0.026)	0.028 (0.038)	0.045 (0.028)	-0.006 (0.020)	0.006 (0.019)

Ineq. x Opp.	-0.023 (0.052)	-0.030 (0.061)	0.010 (0.037)	-0.048 (0.054)	-0.056 (0.039)	0.013 (0.028)	-0.032 (0.027)
Liberal	0.181 (0.024)***	0.150 (0.028)***	0.110 (0.017)***	0.076 (0.025)**	0.153 (0.018)***	0.051 (0.013)***	0.081 (0.012)***
Ineq. x Liberal	-0.056 (0.034)˙	0.003 (0.040)	0.043 (0.024)˙	0.076 (0.035)*	-0.053 (0.026)*	-0.055 (0.018)**	-0.036 (0.018)*
Opp. x Liberal	-0.051 (0.034)	-0.001 (0.040)	0.041 (0.024)˙	0.070 (0.036)˙	-0.027 (0.026)	-0.012 (0.018)	0.008 (0.018)
Ineq. x Opp. x Liberal	0.033 (0.048)	0.102 (0.057)˙	-0.075 (0.034)*	-0.110 (0.050)*	0.090 (0.037)*	0.056 (0.026)*	0.016 (0.025)

Models w/ demographic covariates

	Income gap increasing	Children have worse chances	Income result of circumstances	Increase estate tax	Increase minimum wage	Support entprens	Support housing
Inequality pessimistic	0.193 (0.036)***	0.001 (0.043)	0.036 (0.026)	0.088 (0.038)*	0.075 (0.028)**	-0.001 (0.019)	0.033 (0.019)˙
Opportunity pessimistic	0.086 (0.036)*	0.593 (0.043)***	0.006 (0.026)	0.034 (0.038)	0.041 (0.028)	-0.010 (0.020)	0.007 (0.019)
Ineq. x Opp.	-0.032 (0.051)	-0.033 (0.061)	0.013 (0.036)	-0.048 (0.054)	-0.057 (0.039)	0.015 (0.028)	-0.034 (0.027)
Liberal	0.172 (0.024)***	0.130 (0.029)***	0.084 (0.017)***	0.039 (0.026)	0.140 (0.019)***	0.046 (0.013)**	0.061 (0.013)***
Ineq. x Liberal	-0.061 (0.033)˙	-0.004 (0.040)	0.041 (0.024)˙	0.073 (0.035)*	-0.056 (0.026)*	-0.056 (0.018)**	-0.036 (0.017)*
Opp. x Liberal	-0.065 (0.034)˙	-0.005 (0.040)	0.041 (0.024)˙	0.072 (0.035)*	-0.031 (0.026)	-0.016 (0.018)	0.008 (0.018)
Ineq. x Opp. x Liberal	0.037 (0.047)	0.103 (0.057)˙	-0.072 (0.034)*	-0.097 (0.050)˙	0.090 (0.036)*	0.064 (0.026)*	0.020 (0.025)

The numbers inside the parentheses are standard errors. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Appendix 3.1: Experimental texts, images, videos, and other related content

Those interested in seeing how the survey looked like from the respondents' perspective can take the survey for themselves by going to this link. The complete set of texts and figures as well as links to the videos used in the experiment are provided below.

Many of the questions and text used in this survey are taken directly or adapted from Kuziemko et al (2015) and McCall et al (2017). Other questions are based on existing survey questions from the General Social Survey (GSS), World Values Survey (WVS), Polish Panel Survey (POLPAN), or are written by the researcher.

Welcome to our survey!

The goal of this survey is to understand the public's opinions regarding important societal and economic trends in the US.

At no point in the survey should you feel obligated to answer in a particular way; the most important factor for the success of our research is that you **answer honestly**. Anytime you don't know an answer, just give your best guess.

It is also very important for the success of our research project that you **complete the survey until the end**, once you have started. This survey should take (on average) about 10 to 15 minutes to complete.

Notes:
Your participation in this study is purely voluntary, and you may withdraw your participation or your data at any time without any penalty to you. Your name will never be recorded. Results may include summary data, but you will never be identified. If you have any questions about this study, you may contact us at fai_project@demog.berkeley.edu.

YOU MUST BE A **US** RESIDENT TO PARTICIPATE IN THIS SURVEY

- Yes, I would like to take part in this study, and confirm that I AM A US RESIDENT and am 18 or older
- No, I would not like to participate

IF ANSWER = Yes
    CONTINUE
ELSE
    END SURVEY

Video and Quiz

We'll start by showing you a short video to assess your **comprehension skills**.

A short paragraph will be narrated in the video, so be prepared to watch a **short clip with subtitles**. Simply press [YouTube play symbol] to start the video.

After you watch the video, you will be asked a question about it.

IF CONDITION = Control

The internet that many of us now take for granted came into existence less than forty years ago. In fact, it did not start taking its familiar form as a collection of web pages until thirty years ago. Since then, the internet has become an integral part of many aspects of our lives, including information gathering, banking, and entertainment.

THIS TEXT IS PRESENTED TO RESPONDENTS IN AN UNLISTED YOUTUBE VIDEO WITH SUBTITLES AND THE FOLLOWING LICENSED ADOBE STOCK IMAGE IN THE BACKGROUND.

© jamdesign

IF CONDITION = Natural inequality

Millions of Americans have serious medical conditions, including heart disease, cancer, and diabetes. The risk of having a medical condition increases with age. In addition, elderly people usually have weakened immune systems. While the elderly are at greater risk, millions of younger Americans also suffer from these conditions.

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© alonaphoto

IF CONDITION = Class inequality

Millions of Americans live in poverty. Many more make less than twice the poverty threshold. The fraction of low-income households is higher among minorities such as blacks and Hispanics. Low-income people face difficulties in many areas, including food, housing, and healthcare, that high-income Americans take for granted.

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IF CONDITION = Coronavirus control

We are in the midst of a global disease outbreak. Within a few months after its emergence, the new coronavirus (COVID-19) has spread to almost every country on earth, including the US. Very few events in history have impacted the entirety of humanity in this way, regardless of sex, race, or cultural background.

THIS TEXT IS PRESENTED TO RESPONDENTS IN AN UNLISTED YOUTUBE VIDEO WITH SUBTITLES AND THE FOLLOWING LICENSED ADOBE STOCK IMAGE IN THE BACKGROUND.

© denisismagilov

IF CONDITION = Coronavirus natural inequality

The new coronavirus (COVID-19) is not affecting everyone in the same way. The elderly and those with underlying medical conditions such as heart disease, cancer, and diabetes have been disproportionately affected. The number of infections and deaths are significantly higher among this group compared to the rest of the population.

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IF CONDITION = Coronavirus class inequality

The new coronavirus (COVID-19) is not affecting everyone in the same way. Poor and low-income communities, particularly minorities such as blacks and Hispanics, have been disproportionately affected. The number of infections and deaths are significantly higher among this group compared to the rest of the population.

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Describe in your own words what the text you just listened to was about. [A couple of words or a sentence is enough.]

TEXT ENTRY HERE

Thank you for answering the text comprehension question.

Now, we'll be asking you a few questions regarding your opinions on certain matters.

QUESTIONS ARE PRESENTED IN A RANDOM ORDER.

CHOICES ARE ALSO RANDOMIZED (STANDARD, REVERSED).

Please indicate how important you think each of the following is in terms of **getting ahead in life**.

How important is coming from a wealthy family for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is having well-educated parents for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is having ambition for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is hard work for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is innate ability and talent for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is luck for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is knowing the right people for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is a good education for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

How important is political influence for getting ahead in life?

- Essential
- Very important
- Fairly important
- Not very important
- Not important at all

Do you think Americans today have <u>enough opportunities</u> for getting ahead in life?

- No or very little opportunity
- Some opportunity
- Plenty of opportunity

Do you think Americans today have <u>more or less opportunities</u> for getting ahead in life <u>compared to their parents</u>?

- More
- Same
- Less

Do you think hard work or luck and connections is more important for achieving success in life?

1 - In the long run, hard work usually brings a better life
2
3
4
5
6
7 - Hard work doesn't generally bring success—it's more a matter of luck and connections

Do you think people are poor because of laziness and lack of willpower or because of an unfair society?

1 - People are poor because of laziness and lack of willpower
2
3
4
5
6
7 - People are poor because of an unfair society

Do you think that high earners in our society deserve their high incomes?

- Always
- Most of the time
- Sometimes
- Rarely
- Never

Do you think that low earners in our society deserve their low incomes?

- Always
- Most of the time
- Sometimes
- Rarely
- Never

Do you think income inequality is a serious problem in America?

- Not a problem at all
- A small problem
- A problem
- A serious problem
- A very serious problem

Do you think poverty is a serious problem in America?

- Not a problem at all
- A small problem
- A problem
- A serious problem
- A very serious problem

Do you think unequal access to healthcare is a serious problem in America?

- Not a problem at all
- A small problem
- A problem
- A serious problem
- A very serious problem

Do you think the income gap between richer and poorer Americans is decreasing or increasing?

- Decreasing
- Same
- Increasing

Do you think government regulation (e.g., minimum wage, caps on top compensation) is an effective tool to address economic inequality in the US?

1 - Extremely effective
2
3
4
5 - Not at all effective

Do you think government transfers (food stamps, Medicaid) is an effective tool to address economic inequality in the US?

1 - Extremely effective
2
3
4
5 - Not at all effective

Do you think progressive taxes is an effective tool to address economic inequality in the US?

1 - Extremely effective
2
3
4
5 - Not at all effective

Do you think education policies is an effective tool to address economic inequality in the US?

1 - Extremely effective
2
3
4
5 - Not at all effective

Do you think private charity is an effective tool to address economic inequality in the US?

1 - Extremely effective
2
3

4
5 - Not at all effective

Do you think <u>the government</u> should reduce the income differences between rich and poor?

1 - The government should not concern itself with reducing income differences
2
3
4
5
6
7 - The government ought to reduce the income differences between rich and poor—perhaps by raising the taxes of wealthy families or by giving income assistance to the poor

Do you think <u>major companies</u> should reduce the pay differences between employees with high pay and those with low pay?

1 - Major companies should not concern themselves with reducing pay differences
2
3
4
5
6
7 - Major companies ought to reduce the pay differences between employees with high pay and those with low pay—perhaps by reducing the pay of executives or by increasing the pay of unskilled workers

Which of the following groups do you think has the greatest responsibility for reducing differences in income between those with high incomes and those with low incomes?

- Government
- Major companies
- Private charities
- High income individuals themselves
- Low income individuals themselves
- Income differences do not need to be reduced

Next, please answer the following standard demographic questions.

Are you a US resident?

- Yes
- No

In which state do you currently reside?

DROP-DOWN LIST OF STATES HERE

What is your gender?

- Male
- Female
- Other

What is your age? [Enter a number (e.g., 35)]

TEXT ENTRY HERE

What is your marital status?

- Single
- Married

Do you have children living with you?

- Yes
- No

How would you describe your ethnicity/race?

- European American/White
- African American/Black
- Hispanic/Latino
- Asian/Asian American
- Other

How would you describe your religion?

- Christian (Protestant)
- Christian (Catholic)
- Christian (Mormon)
- Christian (Other)
- Jewish
- Muslim
- Hindu
- Buddhist
- Other religion
- No religion

Which category best describes your highest level of education?

- Eighth Grade or Less
- Some High School
- High School Degree/GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree
- Professional Degree (JD, MD, MBA)

What is your current employment status?

- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not in labor force (for example: retired, or full-time parent)

What is your occupation?

TEXT ENTRY HERE

What was your TOTAL household income, before taxes, last year?

- $0 - $9,999
- $10,000 - $14,999
- $15,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $74,999
- $75,000 - $99,999
- $100,000 - $124,999
- $125,000 - $149,999
- $150,000 - $199,999
- $200,000+

Compared with American families in general today, would you say your family income is above or below average?

- Far below average
- Below average
- Average
- Above average
- Far above average

Which best describes your household's income each month?

- Income is about the same each month
- Income varies somewhat from month to month
- Income varies a lot from month to month

<u>Compared to 10 years ago</u>, do you think your standard of living now is better or worse?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse

<u>10 years into the future</u>, do you think your standard of living will be better or worse?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse

On economic policy matters, where do you see yourself on the liberal/conservative spectrum?

- Very conservative
- Conservative
- Moderate
- Liberal
- Very liberal

Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?

- Republican
- Democrat
- Independent
- None

How often do you follow the news?

- Every day
- A few times a week
- Once a week
- Less than once a week
- Never

How much confidence do you have in the scientific community?

- A great deal of confidence
- Only some confidence
- Hardly any confidence at all

Now, we have a few quick questions about your parents.

What kind of work did your father normally do while you were growing up? That is, what was his job called?

TEXT ENTRY HERE

What kind of work did your mother usually do while you were growing up? That is, what was her job called?

TEXT ENTRY HERE

Which category best describes your father's highest level of education?

- Eighth Grade or Less
- Some High School
- High School Degree/GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree
- Professional Degree (JD, MD, MBA)
- Not Applicable

Which category best describes your mother's highest level of education?

- Eighth Grade or Less
- Some High School
- High School Degree/GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree
- Professional Degree (JD, MD, MBA)
- Not Applicable

Thinking about the time <u>when you were 16 years old</u>, compared with American families in general then, would you say your family income was above or below average?

- Far below average
- Below average
- Average
- Above average
- Far above average
- Not Applicable

<u>Compared to your parents when they were the age you are now</u>, do you think your own standard of living now is better or worse than theirs was?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse
- Not Applicable

Finally, please answer the following questions related to the coronavirus outbreak.

Do you think the coronavirus is a serious threat to the American people?

- Not a threat at all
- A small threat
- A threat
- A serious threat
- A very serious threat

Do you think it is more important to save lives or to save the economy during this outbreak?

- 1 - Saving lives must be the priority even if it means the economy will suffer
- 2
- 3
- 4
- 5 - Saving the economy must be the priority even if it means lives will be lost

On the whole, how satisfied are you with the way <u>your city</u> has been handling the coronavirus situation?

- Very satisfied
- Fairly satisfied
- Neither satisfied nor dissatisfied
- Not very satisfied
- Not satisfied at all

On the whole, how satisfied are you with the way <u>your state</u> has been handling the coronavirus situation?

- Very satisfied
- Fairly satisfied
- Neither satisfied nor dissatisfied
- Not very satisfied
- Not satisfied at all

On the whole, how satisfied are you with the way <u>the federal government</u> has been handling the coronavirus situation?

- Very satisfied
- Fairly satisfied
- Neither satisfied nor dissatisfied
- Not very satisfied

- Not satisfied at all

How have you been affected by the coronavirus? [Select all that apply.]

- I contracted coronavirus and became ill.
- I lost my job because of coronavirus.
- I experienced a significant decrease in income due to coronavirus.
- I have an underlying medical condition that puts me at greater risk for severe illness.
- Someone in my family contracted coronavirus and became ill.
- Someone in my family lost their job because of coronavirus.
- Someone in my family experienced a significant decrease in income due to coronavirus.
- Someone in my family has an underlying medical condition that puts them at greater risk for severe illness.
- I have not been affected by coronavirus in any major way.
- Other (please specify)

How many days have you been outside in the <u>past seven days</u>?

0
1
2
3
4
5
6
7

THE TEXT BELOW IS SHOWN TO RESPONDENTS BECAUSE IRB REQUIRES IT

After submitting your responses, you can protect your privacy by clearing your browser's history, cache, cookies, and other browsing data. (Warning: This will log you out of online services.)

Appendix 3.2: Sample size calculations and sample characteristics

Sample size calculations

Sample size calculations were made with the aim of being able to detect a small effect in one-way ANOVA. The `pwr.anova.test` function in the **R** package `pwr` was used for this end. Using this function, and assuming a small effect size (Cohen's d = 0.05), a total of 5,200 respondents gives us more than 80% power to be able to detect the effect of interest. (Note that Cohen's d is defined as `mu/sigma`, where `mu` is the raw effect size in the original scale and `sigma` is the standard deviation of the outcome variable.) The specific function that was run was this: `pwr.anova.test(k=6,f=0.05,sig.level=0.05,power=0.8)`.

Sample characteristics

Table 3.2.1 presents sample sizes by condition, Table 3.2.2 presents summary socio-demographics by condition, and Table 3.2.3 presents the distribution of respondents across states by condition.

Table 3.2.1. Number of respondents by experimental condition.

Experimental condition	**Number of respondents**
Control	874
Class inequality	885
Natural inequality	873
Coronavirus control	870
Coronavirus class inequality	867
Coronavirus natural inequality	880
Total	5,249

Table 3.2.2. Socio-demographic characteristics by experimental condition.

	control	class	natural	cor_cntrl	cor_class	cor_natural
Age	45.0	45.2	46.5	45.6	45.8	44.9
Gender						
Male	0.471	0.484	0.473	0.452	0.490	0.464
Female	0.523	0.511	0.523	0.539	0.504	0.527
Other	0.006	0.006	0.003	0.009	0.006	0.009
Marital status						
Single	0.519	0.533	0.530	0.509	0.521	0.523
Married	0.481	0.467	0.470	0.491	0.479	0.477
Has children living with them						
No	0.634	0.635	0.648	0.624	0.645	0.649

Yes	0.366	0.365	0.352	0.376	0.355	0.351
Ethnicity/race						
European American/White	0.688	0.681	0.692	0.697	0.691	0.667
African American/Black	0.127	0.128	0.135	0.125	0.119	0.119
Hispanic/Latino	0.085	0.094	0.086	0.091	0.095	0.111
Asian/Asian American	0.070	0.048	0.066	0.053	0.053	0.073
Other	0.031	0.050	0.021	0.035	0.043	0.030
Religion						
Christian (Protestant)	0.285	0.269	0.273	0.268	0.254	0.244
Christian (Catholic)	0.241	0.253	0.227	0.239	0.255	0.243
Christian (Mormon)	0.017	0.017	0.012	0.025	0.015	0.017
Christian (Other)	0.114	0.122	0.143	0.130	0.137	0.157
Jewish	0.028	0.034	0.038	0.032	0.042	0.027
Muslim	0.021	0.019	0.013	0.018	0.019	0.014
Hindu	0.008	0.008	0.003	0.006	0.003	0.015
Buddhist	0.008	0.010	0.009	0.012	0.008	0.013
Other religion	0.057	0.058	0.054	0.048	0.048	0.052
No religion	0.221	0.210	0.229	0.222	0.219	0.218
Highest level of education						
Eighth Grade or Less	0.006	0.003	0.003	0.005	0.006	0.003
Some High School	0.031	0.031	0.024	0.025	0.017	0.036
High School Degree/GED	0.203	0.190	0.183	0.193	0.203	0.207
Some College	0.247	0.232	0.225	0.224	0.209	0.232
2-year College Degree	0.103	0.124	0.118	0.110	0.104	0.112
4-year College Degree	0.244	0.251	0.275	0.264	0.283	0.226
Master's Degree	0.113	0.131	0.124	0.136	0.131	0.134
Doctoral Degree	0.020	0.017	0.015	0.015	0.020	0.014
Professional Degree (JD, MD, MBA)	0.034	0.022	0.033	0.028	0.028	0.035
Employment status						
Full-time employee	0.413	0.407	0.379	0.390	0.403	0.417
Part-time employee	0.098	0.090	0.101	0.106	0.105	0.101
Self-employed or small business owner	0.084	0.066	0.065	0.066	0.070	0.066
Unemployed and looking for work	0.102	0.092	0.110	0.107	0.105	0.097
Student	0.050	0.062	0.058	0.060	0.058	0.057
Not in labor force	0.253	0.284	0.286	0.272	0.260	0.262
Total household income before taxes						
$0 - $9,999	0.050	0.081	0.061	0.069	0.060	0.076
$10,000 - $14,999	0.061	0.054	0.055	0.064	0.039	0.049
$15,000 - $19,999	0.049	0.051	0.050	0.055	0.053	0.047
$20,000 - $29,999	0.098	0.099	0.119	0.087	0.103	0.103
$30,000 - $39,999	0.102	0.106	0.104	0.107	0.125	0.094
$40,000 - $49,999	0.108	0.081	0.094	0.093	0.093	0.089
$50,000 - $74,999	0.182	0.169	0.187	0.184	0.183	0.195
$75,000 - $99,999	0.122	0.142	0.112	0.139	0.128	0.122
$100,000 - $124,999	0.086	0.063	0.071	0.064	0.075	0.077
$125,000 - $149,999	0.062	0.067	0.065	0.055	0.047	0.061
$150,000 - $199,999	0.047	0.052	0.048	0.047	0.044	0.057
$200,000+	0.033	0.033	0.033	0.035	0.050	0.030

Income volatility						
Income is about the same each month	0.619	0.641	0.640	0.634	0.612	0.611
Income varies somewhat from month to month	0.289	0.258	0.257	0.282	0.293	0.280
Income varies a lot from month to month	0.092	0.102	0.103	0.084	0.095	0.109
Liberal/conservative spectrum						
Very conservative	0.106	0.123	0.108	0.113	0.104	0.130
Conservative	0.200	0.184	0.210	0.205	0.204	0.188
Moderate	0.430	0.435	0.419	0.410	0.443	0.432
Liberal	0.177	0.175	0.183	0.186	0.153	0.176
Very liberal	0.086	0.083	0.080	0.086	0.096	0.075
Party identity						
Republican	0.344	0.337	0.310	0.330	0.343	0.318
Democrat	0.383	0.359	0.400	0.393	0.322	0.375
Independent	0.219	0.247	0.246	0.236	0.263	0.234
None	0.054	0.057	0.044	0.041	0.073	0.073
Frequency of following news						
Never	0.028	0.028	0.026	0.025	0.022	0.030
Less than once a week	0.080	0.075	0.071	0.084	0.077	0.073
Once a week	0.116	0.085	0.116	0.095	0.116	0.103
A few times a week	0.269	0.302	0.276	0.240	0.268	0.275
Every day	0.508	0.511	0.511	0.555	0.517	0.519
Confidence in the scientific community						
Hardly any confidence at all	0.090	0.081	0.079	0.070	0.087	0.080
Only some confidence	0.470	0.440	0.468	0.437	0.449	0.436
A great deal of confidence	0.439	0.479	0.452	0.493	0.465	0.484

Age is in years. All other numbers presented are proportions.

Table 3.2.3. Number of respondents in each state by experimental condition.

State	control	class	natural	cor_cntrl	cor_class	cor_natural
Alabama	14	13	12	12	18	14
Alaska	3	3	0	1	2	1
Arizona	23	14	23	14	20	26
Arkansas	3	8	13	10	6	8
California	86	88	100	95	86	87
Colorado	14	12	13	14	12	15
Connecticut	10	9	9	6	12	14
Delaware	2	3	5	4	1	4
District of Columbia	3	3	2	4	3	3
Florida	90	72	74	71	85	76
Georgia	35	30	29	21	33	29
Hawaii	5	4	3	3	2	4
Idaho	2	4	4	7	4	1
Illinois	45	41	37	41	37	41
Indiana	11	24	10	32	7	10
Iowa	8	7	4	5	6	8
Kansas	6	11	6	8	10	3
Kentucky	9	13	11	12	6	13

Louisiana	11	9	10	13	10	7
Maine	5	2	2	5	5	5
Maryland	15	17	8	9	19	22
Massachusetts	16	18	15	22	14	16
Michigan	26	14	23	22	22	35
Minnesota	7	9	13	14	14	8
Mississippi	6	8	11	10	3	8
Missouri	11	18	8	11	9	16
Montana	4	1	1	3	5	1
Nebraska	6	3	3	5	2	4
Nevada	20	11	14	7	13	10
New Hampshire	4	3	4	2	2	3
New Jersey	31	31	25	28	31	33
New Mexico	5	3	1	5	6	6
New York	63	66	73	76	77	54
North Carolina	22	37	33	26	28	23
North Dakota	4	0	3	0	0	2
Ohio	33	37	34	23	27	23
Oklahoma	7	7	9	6	13	4
Oregon	8	16	11	15	9	13
Pennsylvania	35	41	40	49	36	51
Rhode Island	0	1	4	4	1	4
South Carolina	16	13	11	19	10	17
South Dakota	2	2	3	2	0	1
Tennessee	19	21	14	13	15	16
Texas	57	67	72	47	67	75
Utah	4	4	2	5	4	8
Vermont	0	2	0	1	2	1
Virginia	32	21	21	23	31	24
Washington	14	21	19	17	24	18
West Virginia	5	2	5	7	4	0
Wisconsin	14	21	24	19	14	12
Wyoming	1	0	2	2	0	1
Puerto Rico	2	0	0	0	0	0
Respondent does not reside in the United States	0	0	0	0	0	2
Total	874	885	873	870	867	880

Appendix 4.1: Experimental texts, images, videos, and other related content

Experimental videos

Experimental videos can be watched on YouTube (control, natural inequality, class inequality). The images and texts used in the videos are presented below.

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We are in the midst of a global disease outbreak. Within a few months after its emergence, the new coronavirus (COVID-19) has spread to almost every country on earth, including the US. Very few events in history have impacted the entirety of humanity in this way, regardless of sex, race, or cultural background.

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The new coronavirus (COVID-19) is not affecting everyone in the same way. The elderly and those with underlying medical conditions such as heart disease, cancer, and diabetes have been disproportionately affected. The number of infections and deaths are significantly higher among this group compared to the rest of the population.

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The new coronavirus (COVID-19) is not affecting everyone in the same way. Poor and low-income communities, particularly minorities such as blacks and Hispanics, have been disproportionately affected. The number of infections and deaths are significantly higher among this group compared to the rest of the population.

Manipulation check
Respondents are asked to answer the following question after watching the video.

Describe in your own words what the text you just listened to was about. [A couple of words or a sentence is enough.]
TEXT ENTRY HERE

Survey questions related to socio-demographic characteristics of respondents
The following socio-demographic questions are asked to respondents prior to answering coronavirus-specific questions. Most of these questions are taken directly from the study by Kuziemko et al (2015).

Are you a US resident?
- Yes
- No

In which state do you currently reside?
DROP-DOWN LIST OF STATES HERE

What is your gender?
- Male
- Female
- Other

What is your age? [Enter a number (e.g., 35)]
TEXT ENTRY HERE

What is your marital status?

- Single
- Married

Do you have children living with you?

- Yes
- No

How would you describe your ethnicity/race?

- European American/White
- African American/Black
- Hispanic/Latino
- Asian/Asian American
- Other

How would you describe your religion?

- Christian (Protestant)
- Christian (Catholic)
- Christian (Mormon)
- Christian (Other)
- Jewish
- Muslim
- Hindu
- Buddhist
- Other religion
- No religion

Which category best describes your highest level of education?

- Eighth Grade or Less
- Some High School
- High School Degree/GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree
- Professional Degree (JD, MD, MBA)

What is your current employment status?

- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not in labor force (for example: retired, or full-time parent)

What is your occupation?
TEXT ENTRY HERE

What was your TOTAL household income, before taxes, last year?

- $0 - $9,999
- $10,000 - $14,999
- $15,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $74,999
- $75,000 - $99,999
- $100,000 - $124,999
- $125,000 - $149,999
- $150,000 - $199,999
- $200,000+

Compared with American families in general today, would you say your family income is above or below average?

- Far below average
- Below average
- Average
- Above average
- Far above average

Which best describes your household's income each month?

- Income is about the same each month
- Income varies somewhat from month to month
- Income varies a lot from month to month

<u>Compared to 10 years ago</u>, do you think your standard of living now is better or worse?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse

<u>10 years into the future</u>, do you think your standard of living will be better or worse?

- Much better
- Somewhat better
- About the same
- Somewhat worse
- Much worse

On economic policy matters, where do you see yourself on the liberal/conservative spectrum?

- Very conservative
- Conservative
- Moderate
- Liberal
- Very liberal

Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?
- Republican
- Democrat
- Independent
- None

How often do you follow the news?
- Every day
- A few times a week
- Once a week
- Less than once a week
- Never

How much confidence do you have in the <u>scientific community</u>?
- A great deal of confidence
- Only some confidence
- Hardly any confidence at all

Survey questions related to the coronavirus outbreak
After watching the video, respondents were asked to answer the following questions related to coronavirus. Choice ordering was reversed for a random half of respondents in the first five questions.

Do you think the coronavirus is a serious threat to the American people?
- Not a threat at all
- A small threat
- A threat
- A serious threat
- A very serious threat

Do you think it is more important to save lives or to save the economy during this outbreak?
- 1 - Saving lives must be the priority even if it means the economy will suffer
- 2
- 3
- 4
- 5 - Saving the economy must be the priority even if it means lives will be lost

On the whole, how satisfied are you with the way <u>your city</u> has been handling the coronavirus situation?

- Very satisfied
- Fairly satisfied
- Neither satisfied nor dissatisfied
- Not very satisfied
- Not satisfied at all

On the whole, how satisfied are you with the way <u>your state</u> has been handling the coronavirus situation?

- Very satisfied
- Fairly satisfied
- Neither satisfied nor dissatisfied
- Not very satisfied
- Not satisfied at all

On the whole, how satisfied are you with the way <u>the federal government</u> has been handling the coronavirus situation?

- Very satisfied
- Fairly satisfied
- Neither satisfied nor dissatisfied
- Not very satisfied
- Not satisfied at all

How have you been affected by the coronavirus? [Select all that apply.]

- I contracted coronavirus and became ill.
- I lost my job because of coronavirus.
- I experienced a significant decrease in income due to coronavirus.
- I have an underlying medical condition that puts me at greater risk for severe illness.
- Someone in my family contracted coronavirus and became ill.
- Someone in my family lost their job because of coronavirus.
- Someone in my family experienced a significant decrease in income due to coronavirus.
- Someone in my family has an underlying medical condition that puts them at greater risk for severe illness.
- I have not been affected by coronavirus in any major way.
- Other (please specify)

How many days have you been outside in the <u>past seven days</u>?

0
1
2
3
4
5
6
7

Additional variables, conditions

This study is part of a larger project to understand the impact of the coronavirus outbreak on Americans' perceptions of inequality. The survey included many other questions related to respondents' general perceptions regarding opportunity, inequality, and redistribution that are not directly relevant to this paper. The survey also had experimental conditions that are completely unrelated to coronavirus (internet; natural inequality without reference to coronavirus; class inequality without reference to coronavirus). The researcher is writing another paper in parallel based on these results and is happy to share any materials, data, and/or results if requested.

Appendix 4.2: Sample size calculations and sample characteristics

Sample size calculations

Sample size calculations were made with the aim of being able to detect a small effect in one-way ANOVA. The `pwr.anova.test` function in the **R** package `pwr` was used for this end. Using this function, and assuming a small effect size (Cohen's d = 0.075), a total of 2,250 respondents gives us more than 90% power to be able to detect the effect of interest. (Note that Cohen's d is defined as `mu/sigma`, where `mu` is the raw effect size in the original scale and `sigma` is the standard deviation of the outcome variable.) The specific function that was run was this: `pwr.anova.test(k=3, f=0.075, sig.level=0.05, power=0.9)`.

Sample characteristics

Table A4.2.1 presents sample sizes by condition; since the variable indicating whether the respondent or someone in the respondent's family is at risk is used to show effect heterogeneity in the main text, sample sizes disaggregated by this additional variable are also presented in parentheses. Table A4.2.2 presents summary demographics by condition, and Table A4.2.3 presents the distribution of respondents across states by condition.

Table A4.2.1. Number of respondents by condition.

	Number of respondents
Control	870 (not at risk: 597; at risk: 273)
Natural inequality	880 (not at risk: 641; at risk: 239)
Class inequality	867 (not at risk: 589; at risk: 278)
Total	2,617 (not at risk: 1,827; at risk: 790)

Table A4.2.2. Demographics by condition.

	Control	Natural inequality	Class inequality
Age	45.6	44.9	45.8
Gender			
Male	0.452	0.464	0.490
Female	0.539	0.527	0.504
Other	0.009	0.009	0.006
Marital status			
Single	0.509	0.523	0.521
Married	0.491	0.477	0.479
Has children living with them			
No	0.624	0.649	0.645
Yes	0.376	0.351	0.355
Ethnicity/race			
European American/White	0.697	0.667	0.691
African American/Black	0.125	0.119	0.119
Hispanic/Latino	0.091	0.111	0.095
Asian/Asian American	0.053	0.073	0.053

Other	0.035	0.030	0.043
Religion			
Christian (Protestant)	0.268	0.244	0.254
Christian (Catholic)	0.239	0.243	0.255
Christian (Mormon)	0.025	0.017	0.015
Christian (Other)	0.130	0.157	0.137
Jewish	0.032	0.027	0.042
Muslim	0.018	0.014	0.019
Hindu	0.006	0.015	0.003
Buddhist	0.012	0.013	0.008
Other religion	0.048	0.052	0.048
No religion	0.222	0.218	0.219
Highest level of education			
Eighth Grade or Less	0.005	0.003	0.006
Some High School	0.025	0.036	0.017
High School Degree/GED	0.193	0.207	0.203
Some College	0.224	0.232	0.209
2-year College Degree	0.110	0.112	0.104
4-year College Degree	0.264	0.226	0.283
Master's Degree	0.136	0.134	0.131
Doctoral Degree	0.015	0.014	0.020
Professional Degree (JD, MD, MBA)	0.028	0.035	0.028
Employment status			
Full-time employee	0.390	0.417	0.403
Part-time employee	0.106	0.101	0.105
Self-employed or small business owner	0.066	0.066	0.070
Unemployed and looking for work	0.107	0.097	0.105
Student	0.060	0.057	0.058
Not in labor force (for example: retired, or full-time parent)	0.272	0.262	0.260
Total household income before taxes			
$0 - $9,999	0.069	0.076	0.060
$10,000 - $14,999	0.064	0.049	0.039
$15,000 - $19,999	0.055	0.047	0.053
$20,000 - $29,999	0.087	0.103	0.103
$30,000 - $39,999	0.107	0.094	0.125
$40,000 - $49,999	0.093	0.089	0.093
$50,000 - $74,999	0.184	0.195	0.183
$75,000 - $99,999	0.139	0.122	0.128
$100,000 - $124,999	0.064	0.077	0.075
$125,000 - $149,999	0.055	0.061	0.047
$150,000 - $199,999	0.047	0.057	0.044
$200,000+	0.035	0.030	0.050
Income volatility			
Income is about the same each month	0.634	0.611	0.612
Income varies somewhat from month to month	0.282	0.280	0.293
Income varies a lot from month to month	0.084	0.109	0.095
Liberal/conservative spectrum			
Very conservative	0.113	0.130	0.104
Conservative	0.205	0.188	0.204
Moderate	0.410	0.432	0.443
Liberal	0.186	0.176	0.153
Very liberal	0.086	0.075	0.096
Party identity			

Republican	0.330	0.318	0.343
Democrat	0.393	0.375	0.322
Independent	0.236	0.234	0.263
None	0.041	0.073	0.073
Frequency of following news			
Never	0.025	0.030	0.022
Less than once a week	0.084	0.073	0.077
Once a week	0.095	0.103	0.116
A few times a week	0.240	0.275	0.268
Every day	0.555	0.519	0.517
Confidence in the scientific community			
Hardly any confidence at all	0.070	0.080	0.087
Only some confidence	0.437	0.436	0.449
A great deal of confidence	0.493	0.484	0.465

Age is in years. All other numbers presented are proportions.

Table 4.2.3. Number of respondents in each state by condition.

State	Control	Natural inequality	Class inequality
Alabama	12	14	18
Alaska	1	1	2
Arizona	14	26	20
Arkansas	10	8	6
California	95	87	86
Colorado	14	15	12
Connecticut	6	14	12
Delaware	4	4	1
District of Columbia	4	3	3
Florida	71	76	85
Georgia	21	29	33
Hawaii	3	4	2
Idaho	7	1	4
Illinois	41	41	37
Indiana	32	10	7
Iowa	5	8	6
Kansas	8	3	10
Kentucky	12	13	6
Louisiana	13	7	10
Maine	5	5	5
Maryland	9	22	19
Massachusetts	22	16	14
Michigan	22	35	22
Minnesota	14	8	14
Mississippi	10	8	3
Missouri	11	16	9
Montana	3	1	5
Nebraska	5	4	2
Nevada	7	10	13
New Hampshire	2	3	2
New Jersey	28	33	31
New Mexico	5	6	6
New York	76	54	77
North Carolina	26	23	28
North Dakota	0	2	0
Ohio	23	23	27
Oklahoma	6	4	13

Oregon	15	13	9
Pennsylvania	49	51	36
Rhode Island	4	4	1
South Carolina	19	17	10
South Dakota	2	1	0
Tennessee	13	16	15
Texas	47	75	67
Utah	5	8	4
Vermont	1	1	2
Virginia	23	24	31
Washington	17	18	24
West Virginia	7	0	4
Wisconsin	19	12	14
Wyoming	2	1	0
Respondent does not reside in the United States	0	2	0
Total	870	880	867

Appendix 4.3: Regression results

Table A4.3.1 presents linear regression results for the outcome "coronavirus serious threat," while Table A4.3.2 presents linear regression results for the outcome "economy must be saved." Table A4.3.3 presents additional results for these outcomes based on ordinal logistic regression models. Table A4.3.4 presents estimates disaggregated by respondent's at risk status, and Table A4.3.5 presents estimates based on models that include interactions between the at-risk variable and the experimental conditions. Finally, Table A4.3.6 presents estimates from models that treat the two inequality conditions as the reference category to report an effect estimate for the equal pandemic condition, and Table A4.3.7 presents results from additional outcomes related to respondent's level of satisfaction with the way their city, state, and the federal government has been handling the coronavirus situation.

Table A4.3.1. Coronavirus serious threat.

Models w/o any demographic covariates

	Coefficient estimate	Standard error	p-value
Natural inequality	-0.166	0.052	0.001
Class and race inequality	-0.067	0.052	0.199

Models w/ demographic covariates

	Coefficient estimate	Standard error	p-value
Natural inequality	-0.141	0.046	0.002
Class and race inequality	-0.019	0.046	0.689

Table A4.3.2. Economy must be saved.

Models w/o any demographic covariates

	Coefficient estimate	Standard error	p-value
Natural inequality	0.201	0.062	0.001
Class and race inequality	0.138	0.062	0.027

Models w/ demographic covariates

	Coefficient estimate	Standard error	p-value
Natural inequality	0.182	0.058	0.002
Class and race inequality	0.092	0.058	0.112

Table A4.3.3. Estimates from the ordinal logistic regression models.

	Coronavirus serious threat	Economy must be saved
Natural inequality	-0.261 (0.088)**	0.286 (0.086)***
Class and race inequality	-0.130 (0.088)	0.194 (0.087)*

Coefficient estimates are in log-odds. The numbers inside the parentheses are standard errors. Estimates are based on models without any demographic covariates. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A4.3.4. Estimates disaggregated by respondent's at risk status.

At risk

	Coronavirus serious threat	Economy must be saved
Natural inequality	-0.061 (0.082)	0.101 (0.108)
Class and race inequality	-0.071 (0.079)	0.089 (0.104)

Not at risk

	Coronavirus serious threat	Economy must be saved
Natural inequality	-0.184 (0.064)**	0.226 (0.075)**
Class and race inequality	-0.068 (0.065)	0.163 (0.077)*

The numbers inside the parentheses are standard errors. Estimates are based on models without any demographic covariates. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A4.3.5. Estimates from models with interactions.

	Coronavirus serious threat	Economy must be saved
Natural inequality	-0.184 (0.061)**	0.226 (0.073)**
Class and race inequality	-0.068 (0.062)	0.163 (0.075)*
At risk	0.358 (0.078)***	-0.214 (0.094)*
Natural inequality x At risk	0.123 (0.112)	-0.124 (0.136)
Class and race inequality x At risk	-0.002 (0.110)	-0.075 (0.133)

The numbers inside the parentheses are standard errors. Estimates are based on models without any demographic covariates. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A4.3.6. Equal pandemic effect.

	Coronavirus serious threat	Economy must be saved
Equal pandemic	0.117 (0.045)**	-0.170 (0.054)**

The numbers inside the parentheses are standard errors. Estimates are based on models without any demographic covariates. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A4.3.7. Other outcomes.

	Satisfied with city	Satisfied with state	Satisfied with federal govt
Natural inequality	0.028 (0.052)	-0.039 (0.059)	0.035 (0.065)
Class and race inequality	-0.044 (0.053)	-0.111 (0.059)˙	-0.060 (0.065)

The numbers inside the parentheses are standard errors. Estimates are based on models without any demographic covariates. Stars denote p-values: ˙ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.