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The Consequences of Inequality: Beliefs and Redistributive Preferences

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The Consequences of Inequality: Beliefs and Redistributive Preferences

Abstract

What matters for individuals' preferences for redistribution? In this paper we show that consequentialist beliefs about inequality — beliefs about how economic inequality changes the crime rate or the quality of democratic institutions, for example — have a large causal impact on individuals' redistributive preferences. Using two representative surveys of a combined 6,731 U.S. citizens, we show that a majority of respondents believe that inequality leads to a wide range of negative societal outcomes. We establish a causal link from such beliefs to individuals' redistributive preferences by using exogenously provided video information treatments. With this and other methods we show that inequality externality beliefs impact redistributive preferences on the same order of magnitude as broad economic fairness views. These inequality externality beliefs are relatively equally held across political affiliations as well as incomes. We discuss whether a focus on inequality's consequences could shape a distinct conversation about redistribution.

JEL-Codes: C830, C900, D630, H200.

Keywords: inequality, inequality externalities, surveys, redistribution.

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1 Introduction

What motivates people’s willingness to redistribute? A large empirical literature has explored this question, proposing various reasons from individual income maximization (Durante et al., 2014) to broad fairness concerns (Cappelen et al., 2007) to governmental trust (Kuziemko et al., 2015). In this paper we quantify a novel motive within this literature, namely individuals’ beliefs about the *consequences of inequality*. Such consequences occur when economic inequality affects something that in turn affects individuals, for example the level of social unrest, the economic growth rate, or the general trust between people. Importantly for the present paper, people’s beliefs about these consequences can vary, which can in turn affect overall demands for redistribution and even political landscapes. If there is a societal consensus that large economic differences lead to violent revolutions, for example, then a consensus for redistributive policy may be achieved simply by highlighting these risks.

We call these consequences *inequality externalities*, based on the idea that they occur as a side effect of the economic inequality that all of us contribute to through market actions (Støstad and Cowell, 2021).¹ Beliefs in these ideas can affect people’s willingness to redistribute without requiring any altruism or other-regarding preferences, as even selfish people can be concerned about inequality’s consequences. Perhaps surprisingly, then, very few surveys have explored what individuals believe these consequences are. This contrasts to the wealth of information we have about individuals’ other attitudes to inequality and redistribution. We only know of two externality-focused questions in the United States, the most relevant of which comes from the General Social Survey (GSS). Respondents were asked whether they agreed that “*large differences in income are necessary for America’s prosperity*” over five survey waves between 1987 and 2021. The share who agreed with the statement steadily fell from 34% in 1987 to 12% in 2021.² Beyond this, very little has been uncovered about individuals’ inequality externality beliefs in either the United States or the wider world. As such, any empirical connection to individuals’ preferences for redistribution has also not been explored.

Following these observations, this paper poses two main questions. First, do U.S. citizens expect economic inequality to change society – and if so, how? Second, to what extent do such beliefs causally impact citizens’ redistributive preferences? To answer these questions we conduct two novel representative surveys of the U.S. population, sampling a total of 4,371 and 2,360 distinct U.S. citizens with the professional survey providers Lucid and Dynata. These two surveys allow us to create the first comprehensive data sets of U.S. citizens’ *inequality externality beliefs*. We explore the link between these beliefs and redistributive preferences using several methods, the most important of which is a video-based information experiment. This information experiment is designed to isolate the causal effect of inequality externality beliefs on redistributive preferences; it also allows us to explore how inequality externality beliefs affect redistributive preferences as compared to broad economic fairness views. Finally, we discuss

¹Calling these effects inequality externalities is a shorthand we use for simplicity. Strictly speaking it is inequality itself that is the externality.

²The trend is monotonically decreasing over five waves of the GSS. The second question is from the 1991 wave of the International Social Justice Project (ISJP), which asked respondents whether they agreed to the statement that “*there is an incentive for individual effort only if differences in income are large enough*”. 63% of respondents agreed. See Appendix A for more details and other countries.

structural differences between these two determinants of redistributive preferences.

Our results show that most U.S. citizens believe in the *negative* externality dimension. This contrasts to the way these issues have often been discussed; the GSS question mentioned earlier assumes a positive externality dimension, for example. Nearly every individual ($\sim 90\%$) believes inequality affects society in some way, and a consistent majority ($\sim 60\%$) believes that economic inequality has overall harmful societal consequences.³ We delve into the potential reasons and find strong beliefs in specific channels; 74% of respondents think more economic inequality increases the amount of crime, for example, and 67% think it deteriorates the overall level of societal trust. Whereas only 23% think more economic inequality *increases* the amount of economic growth (reminiscent of the GSS question), 52% think the converse, namely that more economic inequality *decreases* the amount of economic growth. Respondents have similarly negative beliefs about the effect of economic inequality on the prevalence of corruption or social unrest, the quality of democratic institutions, and more. This represents our first main finding, which is that most U.S. citizens believe that economic inequality has a wide range of negative effects on society. Results are robust to different methodologies, question phrasings, and are nearly identical across different representative samples.⁴

Who, then, believes in these harmful consequences of inequality? While Democrats are ~ 15 -20 percentage points more likely than Republicans to hold negative externality beliefs,⁵ the main conclusions are similar across political camps. Regardless of party affiliation, respondents are more likely to believe that economic inequality harms rather than helps society for every outcome we elicit. Democrats, Independents, and Republicans are all more likely to believe that more economic inequality *decreases* rather than *increases* economic growth and innovation, for example. That these beliefs are relatively similar across party lines is especially evident when we compare them to broad economic fairness views, which we use as a benchmark throughout the paper. Such equity-based concerns – regarding whether the current income distribution is *fair*, for example – are significantly more divided by party lines (~ 30 -35 p.p.). Indeed, two typical respondents in each political party are more likely to agree on any elicited externality question than on any elicited fairness question in either of our two samples. This non-polarization in externality beliefs is striking, and might reflect an intrinsic difference between externality beliefs and fairness concerns. We also see a similar non-polarization of externality beliefs in economic status; while the least well-off are much more likely to say that the economic distribution is unfair, the rich and poor are roughly equally likely to believe in the negative consequences of inequality.

Having established the existence of widespread inequality externality beliefs in the U.S. population, we then move to the implications of these beliefs. Specifically, we test whether such beliefs constitute a causal determinant for redistributive preferences, adding to the large literature exploring determinants for individuals' preferences for redistribution. Our main method

³ $\sim 10 - 15\%$ believe the net effect to be positive, $\sim 15 - 20\%$ believe the positive and negative effects “cancel each other out”, and $\sim 5 - 15\%$ do not believe inequality affects society.

⁴Respondents answer similarly to very simple and very detailed questions, and to questions early or late in the survey flow. Substituting the word “inequality” for “equality” or “differences in income and wealth” does not change overall results.

⁵78% of Democrat-leaning respondents believe inequality increases social unrest against 62% of Republican-leaning respondents, for example.

to explore this question is an exogenously provided information experiment. We use four easily digestible videos to inform respondents about four different sets of empirical relationships, focusing on either economic inequality’s strong correlations with crime and/or trust (three separate externality videos) or on the trends of the median wage, productivity, and the top 1% income share since 1980 (one fairness video). The main purpose of the video treatment is to explore whether shifting individuals’ inequality externality beliefs causally changes individual preferences for redistribution. If so, inequality externality beliefs are a causal determinant of redistributive preferences – a significant finding, given the prominence of the question of redistribution in both the public domain and the academic sphere. As such, the externality treatments are our main focus, while the fairness video functions as a positive benchmark. The externality videos are designed to exogenously shift various inequality externality beliefs, whereas the fairness video is designed to exogenously shift beliefs about productivity gains and distributional wage growth. We make several design choices to avoid survey demand and priming effects, including what we call a *secondary survey* (a structural, well-explained gap between the treatment and outcomes of interest) and *dual control groups* (using both an active and a passive control group and merging them on pre-specified criteria).

Both the main externality video and the fairness video have strong causal effects on redistributive preferences ($p < 0.01$).⁶ As mentioned, the former is most important for our purposes, as it shows that learning about inequality’s consequences increases individuals’ preferences for redistribution. This result is robust to an array of different specifications. First-stage effects and mediation analysis indicate that each video affected redistributive preferences through the expected mechanism (learning about the expected belief) with limited spillovers and priming. While the fairness video has a stronger effect on redistributive preferences, the overall order of magnitude is similar, which is notable given the comparative attention given to these two determinants in the academic literature. This is further confirmed by two other methods, where we (i) simply ask respondents what determines their redistributive preferences, and (ii) perform a classical regression horse-race to explore predictive power on redistributive preferences. We also note that inequality externality beliefs explain variation in redistributive preferences that cannot be explained by economic fairness views. Our second main result, which summarizes the above discussion, is thus that inequality externality beliefs are a causal and strong determinant of individuals’ preferences for redistribution.

We then move on to discuss the structural differences in how redistributive arguments based on inequality externalities or fairness ideas operate. First, individuals who saw the fairness video are significantly more likely to report their reaction to the video as *anger*. Second, the treatment effect of the fairness video is split across incomes; while the externality videos affect individuals similarly across income brackets, low-income individuals are significantly more swayed by the fairness treatment than top income individuals. These two points corroborate the polarization story we find in the descriptive data, which is also intuitively appealing. Externality-based redistributive arguments are by nature consequentialist, and could influence individuals who are not necessarily swayed by arguments based on deservedness or altruism. Indeed, externality-based arguments are less focused on normative concepts in general. It follows that externality-

⁶The effects of the two smaller externality videos are not statistically significant, but are in the expected direction of increased redistributive preferences.

based arguments could present an opportunity for consensus among groups that often disagree on normative ideals.

This paper is to the best of our knowledge among the first to explicitly study the idea of inequality externality beliefs, and the first to directly empirically link stated externalities beliefs to individuals' preferences for redistribution. An extensive literature has examined various other determinants of redistributive preferences, in particular people's fairness ideals and their concerns about the efficiency costs of redistribution (e.g. Cappelen et al., 2007; Durante et al., 2014). Of the two, fairness ideals are often found to be the stronger motivator (Almås et al., 2020), although there is some variation across various groups within the population (Fisman et al., 2015). Papers have also explored the connection between redistributive preferences and beliefs about one's relative position (Cruces et al., 2013; Karadja et al., 2017), information about the level of inequality and the functioning of tax systems (Kuziemko et al., 2015; Stantcheva, 2021), and beliefs about social mobility (Alesina et al., 2018; Gärtner et al., 2019), among many other topics. Citizens' concerns about the consequences of inequality are rarely discussed in this broad literature, despite having been proposed as a possible motive behind redistributive preferences (Alesina and Giuliano, 2011). One exception is work by Rueda and Stegmueller (2016) who present correlational evidence of an association between the fear of crime and preferences for redistribution among high-income individuals in Western Europe, and explain the association through an externality-based theoretical argument. As a final link to the redistributive preference literature, we note that the consensus we find in inequality externality beliefs is reminiscent of the across-party consensus Norton and Ariely (2011) find for a reduced level of wealth inequality in the no-friction case.

Our work creates a survey-based background to the vast literature attempting to establish connections between economic inequality and various societal outcomes. In short, there exists correlational evidence indicating that inequality is an externality across various dimensions (Wilkinson and Pickett, 2011; Rufrancos et al., 2013; Bergh et al., 2016), but large-scale causal evidence is unlikely to be forthcoming due to the lack of exogenous variation of economic inequality.⁷ In smaller settings, causal evidence can exist; economic inequality has been convincingly shown to affect subjective well-being (Card et al., 2012) and productivity (Breza et al., 2018) in the workplace through relative income concerns, and trust in laboratory and survey experiments (Gallego, 2016; Fehr et al., 2020). A full examination of this literature is beyond the scope of this paper.

We also connect to the theoretical literature on inequality as an externality, which occasionally argues that such beliefs could affect redistributive preferences. The first work that considers economic inequality's societal effects in a welfarist framework is Thurow (1971), who argues that the first welfare theorem fails if the income distribution is a pure public good. Alesina and Giuliano (2011) note that economic inequality can affect individual consumption and thus redistributive preferences, while Rueda and Stegmueller (2016) consider crime as an inequality externality and show theoretically how this has an effect on the preferred redistribution of the rich. A certain section of the inequality aversion literature specifically discusses how such inequality externalities could influence individual preferences (Carlsson et al., 2005)

⁷As well as other intrinsic concerns and insufficient data – see Støstad (2019) for a discussion.

and thus optimal income taxation (Aronsson and Johansson-Stenman, 2018, 2020). The most related paper on this dimension is Støstad and Cowell (2021), which formalizes a framework around inequality as an externality, establishes micro-foundations for various inequality externalities, and shows that an inequality externality can substantially influence well-known results from optimal taxation theory. This paper, in summarizing the widespread public beliefs in such effects – nearly every U.S. citizen believes that *some kind* of inequality externality exists – shows that a social planner who aggregates individual preferences might wish to include such externality considerations into its optimization problem. It might thus be prudent to more seriously consider the robustness – or fragility – of standard individualist frameworks to such externality effects.

The rest of the paper is organized as follows. Section 2 describes the theoretical framework behind the analysis. Section 3 presents the survey sampling methodology. Section 4 analyzes individuals’ inequality externality beliefs, while Section 5 extends the analysis of the results to the redistributive preference dimension. Section 6 discusses the implications of the findings and Section 7 concludes.

2 Theoretical Framework

The central idea behind this work is that economic inequality itself can affect society through various channels. This work discusses several of these specific channels, which we define as *inequality externalities*. The externality framing is motivated by Støstad and Cowell (2021), who argue that inequality itself is an externality if it affects pertinent societal outcomes.⁸ We define an inequality externality as the concept of economic inequality affecting some relevant societal variable, such as crime, social unrest, or economic growth.⁹

Exploring every potential causal channel through which economic inequality could affect society is beyond the scope of this paper. Still, it is useful to note two points briefly. First, the majority of the externality channels we focus on could be caused by several different mechanisms. Second, inequality externalities are relatively simple to micro-found and can be *mechanical* in nature; in other words, they do not need to depend on perceived inequality. Assuming that incomes causally affect political opinions in one direction can be enough to micro-found an effect of income inequality on political polarization, for example. We discuss these two points further in Appendix B.1.

Optimal redistribution We will now motivate the empirical analysis through a simple optimal taxation model in the vein of Saez and Stantcheva (2016) and Støstad and Cowell (2021).

⁸We all affect inequality through our market actions, so if inequality affects other factors we care about it is immediately an externality.

⁹We define inequality’s effects on other outcomes as “inequality externalities” as a simplification, even though it is debatable whether these concepts are technically externalities themselves. The definition of an externality in Buchanan and Stubblebine (1962) states that an externality is present when $u^A = u^A(\dots, Y)$ and Y is by definition under control by another individual. Under this definition it is clear that inequality itself is an externality if it affects society (Støstad and Cowell, 2021). It could also be argued, however, that crime is an externality *through* inequality; other individuals affect the level of crime through inequality, which they have changed with their market actions.

Individuals' utility function U depends on their post-tax income x_i , labor effort h_i , and individuals' experienced state of the world $\Gamma_i(\cdot)$. This $\Gamma_i(\cdot)$ vector includes anything else the individual cares about, for example the level of crime, political polarization, or innovation. At least some of these factors may depend on economic inequality θ , which we assume to be a function of all post-tax incomes \mathbf{x} .¹⁰

The exact dependence of Γ_i on $\theta(\mathbf{x})$ is unknown. For optimal taxation purposes, the crucial aspect is the total effect of inequality θ on utility through these externality channels, which can be captured in this model by the marginal rate of substitution between income and income inequality $\eta_i = MRS_{x_i, \theta} = -\frac{\partial U_i / \partial \theta}{\partial U_i / \partial x_i}$, which is potentially heterogeneous between individuals. If η_i is positive, inequality has a negative externality effect on agent i (a public bad effect). If η_i is negative, inequality has a positive externality effect on agent i (a public good effect). To illustrate the key channels we assume a utility function such that $U = x_i - v(h_i) - \eta_i \theta$. This already illustrates a key point about inequality externalities, which is that self-interested agents can have preferences over inequality without any other-regarding preferences.¹¹

The social planner, who uses generalized social welfare weights to value one unit of income to agent i at g_i (Saez and Stantcheva, 2016), sets the tax rate such that no small change around the optimum leads to welfare gains. As we show in Appendix B.2, this implies the following optimal marginal income tax rate at a pre-tax income z near the top:

$$\tau(z) = \frac{1 + \eta\Omega(z)}{1 + \alpha(z)\epsilon(z) + \eta\Omega(z)}, \quad (1)$$

where $\epsilon(z)$ is the earnings elasticity with respect to $1 - \tau(z)$, the local Pareto parameter is denoted by $\alpha(z)$, and the net inequality externality magnitude is a combination of the individual effects such that $\eta = \int_j \eta_j g_j dj$. Finally, $\Omega(z) = \alpha(z)\epsilon(z)\kappa(z) + \bar{\kappa}(z)$ contains the remaining effect of the inequality externality where $\kappa(z)$ denotes the weight of the individual in the inequality metric at z and $\bar{\kappa}(z)$ denotes the average of this weight above z .¹² For a more thorough explanation of these terms see Appendix B.2 and Støstad and Cowell (2021).

For our purposes it is sufficient to note that the extent of inequality externalities (η) strongly affects the optimal top tax rate. If we assume a negative externality for simplicity, there is an implicit trade-off between (i) the maximization of tax revenue to maximize social welfare, denoted by the no-externality optimal tax rate $\frac{1}{1 + \alpha(z)\epsilon(z)}$ and (ii) the minimization of inequality to account for the effect of inequality externalities, denoted by $\eta\Omega(z)$. The former portion is well-known and motivates the discussion on individuals' preferences for redistribution in e.g. Stantcheva (2021). The latter is the channel we will explore in the remainder of the paper. We

¹⁰ Γ could also depend on different inequality metrics θ_i , perceived inequality $\tilde{\theta}(\mathbf{x})$, the individuals' income x_i , and more – we will abstract from these factors for simplicity. We briefly explore which inequality metrics individuals believe are important in Section 4.

¹¹In this expository model we assume that individuals have no pure other-regarding preferences. If so, and if these are taken into account by the social planner, inequality also affects utility through this channel.

¹²The inequality metrics we use are written as

$$\bar{\theta}(z, H) = \int_z^{\bar{x}} \kappa(z)x(z)dH(z), \quad (2)$$

where $H(z)$ is the cumulative distribution of all pre-tax incomes z . The inequality weight $\kappa(z)$ is non-decreasing, continuous, positive near the top of the income distribution and negative near the bottom, and otherwise general. For example, the (absolute) Gini coefficient in post-tax income has a weight $\kappa_G(z) = 2H(z) - 1$.

will not attempt to measure η directly; instead we intend to explore whether this channel is meaningful in actual redistributive debates.

We thus have two main goals. First, to descriptively explore whether inequality externality beliefs are widely held. Second, to determine whether they are likely to be impactful in actual redistributive debates.

3 Sampling Methodology

Our empirical results are based on two independent pre-specified surveys.¹³ We will call these surveys Survey 1 ($N_1 = 4,371$) and Survey 2 ($N_2 = 2,360$).

Survey 1 had two primary goals. First, to collect descriptive data on individuals' inequality externality beliefs. Second, to conduct an information experiment to connect these beliefs to individuals' redistributive preferences.

Survey 2, which was designed after receiving results from Survey 1, aimed solely to re-measure and improve the robustness of the descriptive findings from Survey 1. The survey populations are distinct, and Survey 2 has no connection to the information experiment in Survey 1.

Where possible we merge the results from the two surveys. In practice, this means that we merge the control group of Survey 1 and all of Survey 2 for most of our descriptive results.¹⁴ This merged descriptive sample has a total of 3,292 respondents.

Survey respondents were collected through the survey providers Lucid (Survey 1) and Dynata (Survey 2). Both Lucid and Dynata are commonly used by economic researchers (see e.g. Haaland and Roth, 2021; Andre et al., 2022).¹⁵

3.1 Survey 1

Data for Survey 1 were collected between December 6th and 24th 2021 through the survey provider Lucid. 5,007 subjects completed the survey, which is reduced to 4,371 after routine data quality checks. The average survey duration for these respondents was 19 minutes and 11 seconds. Methodological details and the full questionnaire can be found in Appendix C.1.

3.2 Survey 2

Data for Survey 2 were collected between August 9th and October 8th 2022 through the survey provider Dynata. 2,479 subjects completed the survey, which is reduced to 2,360 after routine data quality checks. Survey 2 had the goal of further delving into the main *descriptive* results from Survey 1 with an independent sample from a distinct survey provider. The average survey duration for these respondents was 20 minutes and 31 seconds. Methodological details and the full questionnaire can be found in Appendix C.2.

¹³See AsPredicted.org #82083 and #104271.

¹⁴This does not significantly affect our results and is done to improve precision. We pre-specified that results from each survey would be shown side-by-side in the text where possible; as results are very similar across surveys, we instead merge the samples and show the side-by-side results in the Appendix.

¹⁵Lucid and Dynata both collect respondents from several distinct sources. These sources are partly chosen to collect a wide variety of respondents to ensure representativity in surveys like ours. They include brand loyalty programs, targeted online advertisements, and institutional partnerships.

3.3 Respondent characteristics

In both surveys we used quotas to aim for representativity along the dimensions of age, gender, geographical region and political affiliation (Democrat, Independent and Republican). These dimensions are therefore largely representative of the 2021 U.S. population by design. We also have a wide range of incomes in both surveys. As with other studies using online access panels, both surveys somewhat oversample white respondents and college-educated respondents.¹⁶ Re-weighting respondents for full representativity on these dimensions does not change reported results significantly. We discuss sample representativity further in Appendix C.3.

4 Descriptive Characterization of Inequality Externality Beliefs

The first main objective of this paper is to explore U.S. citizens' beliefs in the consequences of inequality. This section will descriptively analyze respondents' answers to various survey modules, and in doing so examine individuals' *general* and *specific* externality beliefs. General inequality externality beliefs concern whether inequality has negative or positive consequences for society as a whole. Specific externality beliefs concern whether inequality changes specific outcomes (crime, trust, and so on).

Prior to delving into the results it is important to acknowledge the limitations to our approach. We focus on *economic* inequality loosely defined as "differences in income and wealth". Although we explore different phrasings and whether inequalities near the top or bottom matters more, we abstract from other relevant dimensions such as whether income or wealth inequality is more meaningful, gender inequality, racial inequality, and so on. We also abstract from the *origin* of the inequality, the difference between perceived and actual inequality, and meritocratic and non-meritocratic inequality.

4.1 General externality beliefs

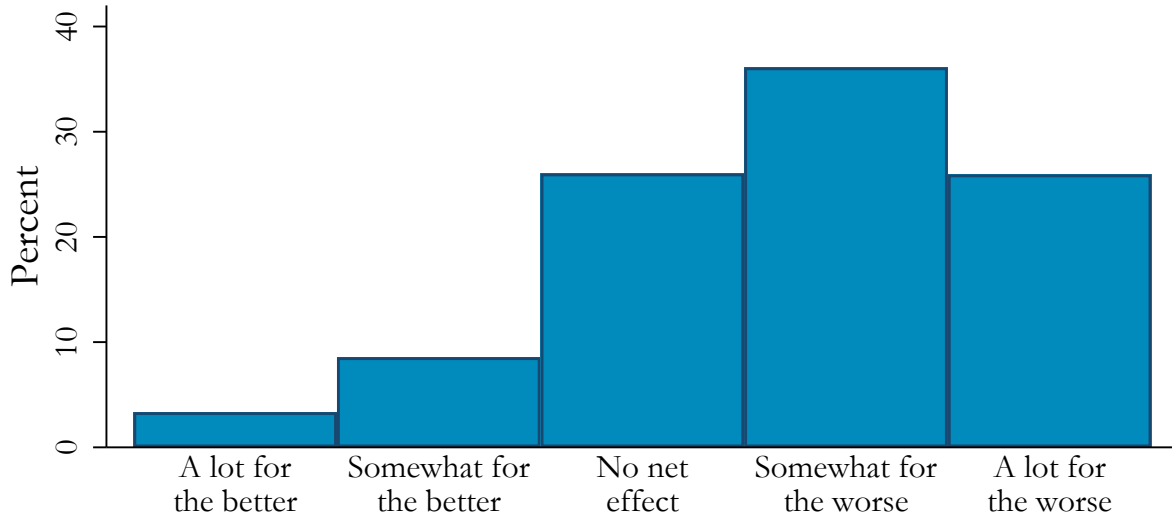
First we explore subjects' general externality beliefs. General externality beliefs concern how respondents think more economic inequality *generally* changes how society functions. Answer options range from an overall positive fashion to an overall negative fashion. A separate question specifies whether individuals believe inequality affects society *at all*. The design of these questions, which varied across surveys in detail and accompanying explanation, are detailed in Appendix C.5. The main conclusions are similar across surveys despite different design choices.¹⁷

Results Figure 1 illustrates the answer distribution. 62% of respondents believe that inequality changes society somewhat or a lot for the worse, or constitutes a negative externality. Only a small minority (12%) states that inequality has positive societal effects, or constitutes an overall positive externality. The accompanying data is shown in Table C2.

¹⁶These disparities are typical for similar studies, see e.g. Stantcheva (2021).

¹⁷Survey 1 did not give respondents examples of potential channels. Survey 2 had a clearer definition of "changes society", including the specific examples of "*economic growth, crime, general trust, innovation, the quality of democratic institutions, and so on.*"

Figure 1: How does more inequality change society overall?



Note. 3,292 respondents across Survey 1 and Survey 2 (pooled sample). See Appendix C.5 for more details on questions.

A total of 26% of respondents believe that there is no net effect of inequality on society. This is divided between 13% of respondents who believe good and bad effects cancel each other out, and 13% who believe that inequality has no effect on society at all.¹⁸ The belief that inequality affects society in one way or the other is thus shared by $\sim 87\%$ of respondents. The overall results do not change depending on whether we use different phrasing (“inequality”, “equality” or “differences in income and wealth”)¹⁹

We elicit two further general externality beliefs towards the end of Survey 1. Here respondents are asked (i) whether they believe that inequality changing society for the worse through externality channels is a “very serious issue”,²⁰ and (ii) whether “extremely high inequality levels would significantly increase the chances of a societal collapse”. A majority of respondents answer that inequality changing society through externality channels is a “serious” (29%) or “extremely serious” (22%) issue.²¹ A large majority of respondents also answer “Yes, definitely” (25%) or “Yes, maybe” (47%) to whether extremely high inequality would significantly increase the chances of a societal collapse.

4.2 Specific externality beliefs

Design Our main battery of inequality externality questions asks how respondents think inequality affects different aspects of society. We elicit opinions on whether inequality affects crime, corruption, political polarization, unemployment, innovation, economic growth, the quality of local public goods such as schools or libraries, people’s overall quality of life (comparing people

¹⁸This divide is the only aspect of the question that had significantly different responses across surveys, as we show in Table C2. The difference is most likely from a question-specific ordering effect that we discuss in Appendix C.5.

¹⁹See Appendix D.1.

²⁰Full question text: “Overall, do you think economic inequality changing society for the worse through one or more of the channels we discussed earlier - for example through increased crime / social unrest / corruption, or through decreased social cohesion - is a very serious issue?”

²¹42% answer that this is “not a very large” or “small” issue, and 7% do not believe it is an issue at all.

with the same income in more or less unequal societies), the quality of democratic institutions, and generalized trust.²² Survey 2 also asks a Placebo question where the sensible answer is that inequality does not affect this outcome (the number of daylight hours within a country). The standard question asks: “*How does more economic inequality change the [amount of crime / overall level of trust / ...] in a country?*”. Questions were always symmetric around a neutral option and answer order was randomly flipped.²³ We also generally added a short definition to the outcome in question; see Tables F11-F12 for these definitions and the full wording of each variable in each survey.²⁴

Results Figure 2 characterizes the responses to these questions from the merged descriptive sample.²⁵ The immediate takeaway from this exercise is that respondents are most likely to believe inequality has a negative societal consequence for any outcome we survey – including economic outcomes such as economic growth and innovation. This can be observed through the structural break between *unemployment* and *innovation* in Figure 2. Every outcome above this point is harmful (crime, social unrest...) and is believed to increase with more economic inequality. Every outcome below is beneficial (trust, the quality of democratic institutions...) and is believed to decrease with more economic inequality. Note that all questions are perfectly symmetric and that design choices do not drive these results.²⁶

We will now discuss the specific inequality externalities. First, there is a strong belief that economic inequality increases crime, which is a canonical inequality externality studied in previous research (Becker, 1968; Kelly, 2000; Fajnzylber et al., 2002). 74% of respondents believe more economic inequality increases the amount of crime. This is the most agreement we find on inequality’s effect on any specific variable. Similar but somewhat smaller figures are found for the percentage of respondents believing inequality increases the negative outcomes of social unrest (70%), corruption (66%), and political polarization (66%). A majority of respondents seem to believe that more unequal societies are less stable and law-abiding in general.

We also elicit individuals’ beliefs on how inequality affects positive outcomes such as generalized trust or the quality of democratic institutions. Generalized trust presents the most agreement; 67% believe inequality decreases the overall level of trust in a country. Then follows quality of life, where we specifically ask respondents to compare between people with the same income in more equal or unequal societies. Under this definition, 59% believe inequality worsens quality of life generally speaking – more strong evidence that individuals believe inequality itself is a negative externality. A clear majority believes inequality deteriorates the functioning of the collective parts of society, as observed through the number of respondents who believe inequality decreases the quality of local public goods (58%) and the quality of democratic institutions

²²We selected outcomes that have previously appeared in public and academic discussion about inequality’s societal effects. All these outcomes were elicited in Survey 1; in Survey 2 we did not elicit unemployment, the quality of local public goods, and overall quality of life.

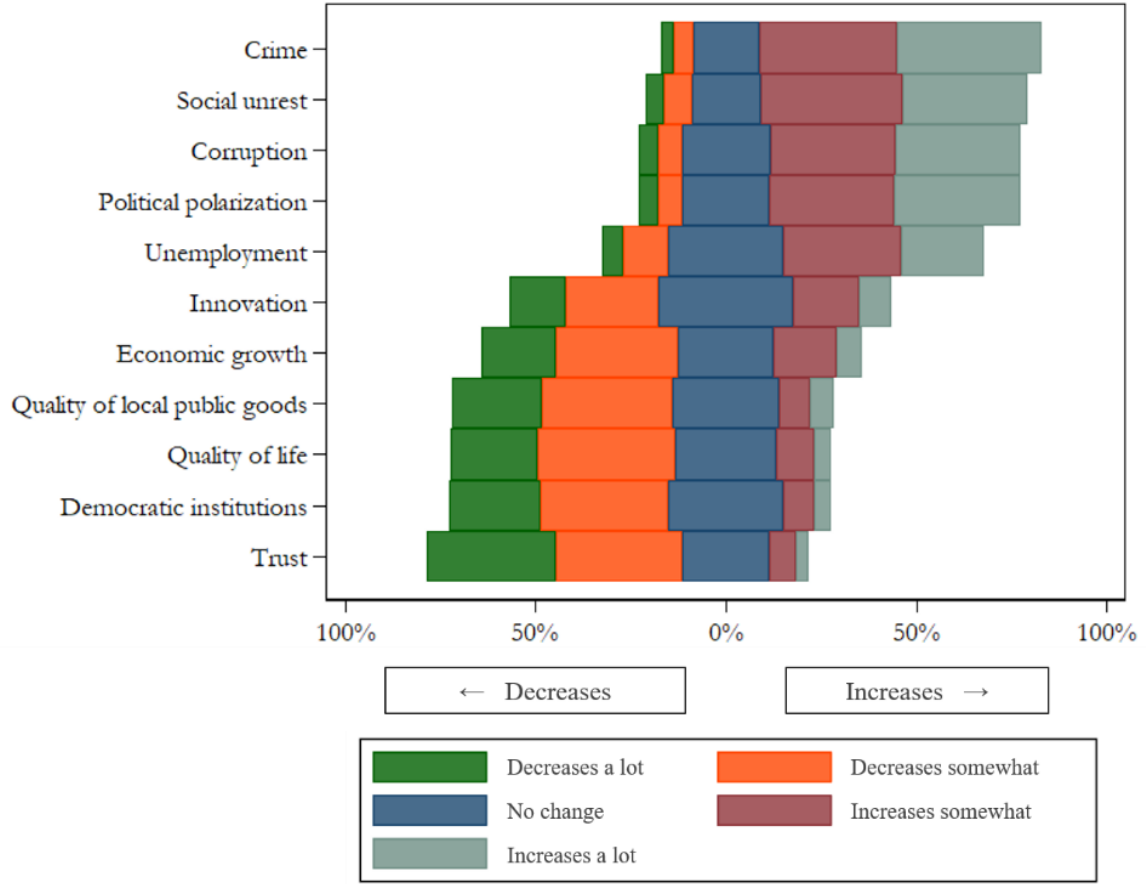
²³To ensure that respondents understood the direction of their answer each answer option re-iterated the causal channel, as for example “More inequality → a lot more crime”.

²⁴To assess whether subtle differences between questions matter for responses, 25% of questions in Survey 2 were randomly presented in the identical way as in Survey 1. This does not affect results.

²⁵The exact numbers are shown in Table F1.

²⁶We discuss robustness checks in Section 4.2.1. Notably, the placebo question is exactly centered around “No change”.

Figure 2: Specific Externality Beliefs: How Does More Economic Inequality Change Outcomes?



Note. Answers to questions about how “more economic inequality” changes the designated outcomes. Full question example: “How does more economic inequality change the amount of crime in a country?” Answer option example: “More inequality → a lot more crime”. Specific externality beliefs in the merged descriptive data. The bars on the right-hand-side indicate the share of respondents that believe that inequality increases the outcome in question, while the bars on the left-hand-side indicate the opposite. The bars centered around zero indicate the share of subjects that answered that inequality does not affect this variable. Questions are ordered according to which portion of respondents believe that inequality increases the variable. Answers are pooled from Survey 1 and Survey 2. $N \in \{628, 3292\}$. For the equivalent figures using only data from Survey 1 or 2, see Figures and E2 respectively. The exact numbers are shown in Table F1.

(57%).

The three last outcomes we elicit are on inequality’s effect on economic growth, innovation, and unemployment. Inequality’s effects on economic performance are more ambiguous than the other variables we survey.²⁷ On one hand, one could argue that inequality promotes growth by strengthening incentives. On the other, one could argue that inequality reduces economic performance through aggregate demand, poverty traps, or the many potential negative effects we already discussed – on trust, criminal activity, democratic institutions, and so on.

Between these two arguments, U.S. citizens’ beliefs clearly point towards the latter. A majority of respondents believe that inequality generally increases unemployment (53%) and reduces growth (51%). Somewhat less than a majority also believe that inequality decreases innovation (40%). The converse for these three figures – that inequality *decreases* unemployment

²⁷While one could conceivably argue that inequality has a positive effect on the other outcomes we study, e.g. crime or trust – say that inequality decreases crime through gated communities, for instance – the academic literatures on these outcomes have typically highlighted inequality’s negative effects (see e.g. Wilkinson and Pickett, 2011).

and *increases* growth and innovation – is only believed by 17%, 23% and 26% respectively. As far as we know this represents the first systematic exploration of these beliefs in the American public. Before moving on we note that some caution should be taken in interpreting these results, as respondents may confuse the effects of inequality itself on these factors with the effects coming from a redistributive tax system (classic incentive effects). Open-ended text questions indicate that such confusion is very rare, however.²⁸

Finally, we note that percentage of respondents who believe that economic inequality does not affect society in any way is consistently low, aligned with the results on general externality beliefs. Only 4.2% of respondents in Survey 1 and 3.7% of respondents in Survey 2 consistently chose “No change” to all questions they were asked.²⁹

4.2.1 Robustness of specific externality beliefs

The specific inequality externality beliefs we outlined above are very robust to different specifications. This is illustrated in the Appendix Figures E3-E4. The results change only minimally across the two surveys, and stay very similar when we (i) restrict to respondents who succeeded on every attention check, (ii) change the words “more inequality” to “larger differences in income and wealth”, (iii) explain what the initial reference point of inequality is through diagrams and words, and explain explicitly what “more inequality” means, (iv) ask respondents to confirm their choice, (v) inform respondents that their answer is important and implore them to carefully consider their choice, (vi) impose a waiting period before an answer is possible, (vii) ask any given question at the beginning or the end of the survey, or (viii) weight respondents for full representativity to the 2021 U.S. population. The robustness check that has the largest effect is to change the phrasing from “more inequality” to “more equality” ($\sim 12\%$ difference), and to a lesser extent to “larger differences in income and wealth”. Still, main results remain similar under all phrasing specifications. See Appendix D.1 for further details.

Finally, we note that a placebo question (“*How do you think more economic inequality changes the number of daylight hours?*”) posed in Survey 2 had a nearly perfectly symmetric answer distribution around the 89% of respondents who answered “No change”, and that 93% and 94% of our final respondent sample succeeded on two simple attention checks designed to look similar to the specific externality questions (for example, “*How do you think more economic inequality – could you please click the first answer option?*”).³⁰ This can be seen in Figure E2.

4.2.2 Heterogeneity in inequality externality beliefs

We next explore which parts of the population have which inequality externality beliefs. This section is based on Tables F13-F15, which regress different externality beliefs on sociodemographic variables, trust in government, and general fairness views for our main sample.³¹

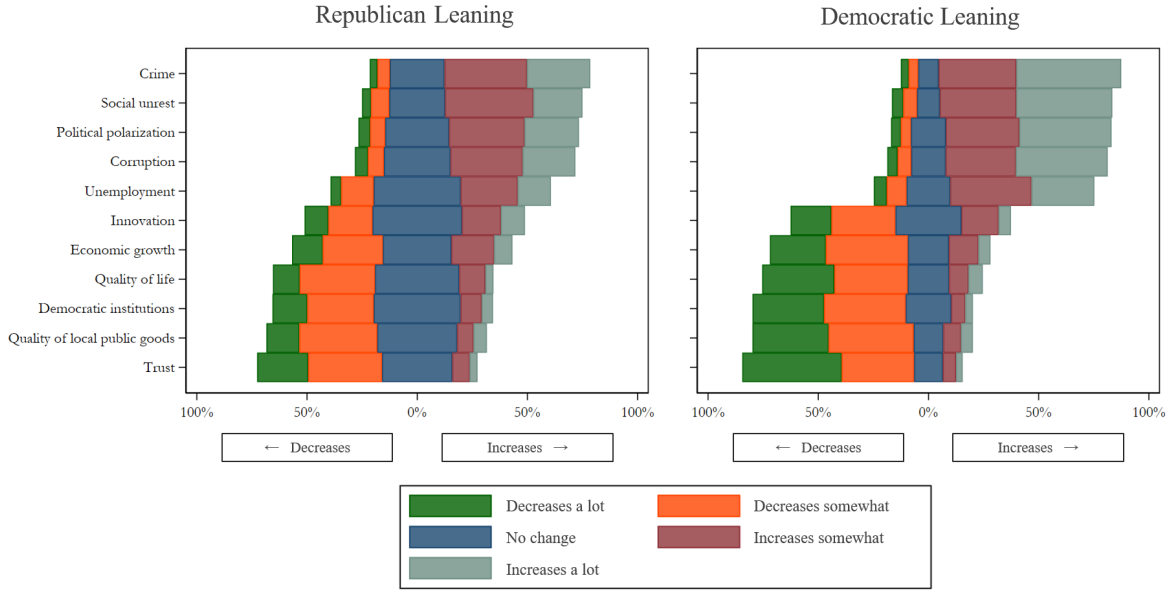
²⁸In a sample of 226 text responses on inequality’s effect on economic growth, only one answer clearly confuses these issues. In general, answers discussing incentive issues generally focus on the incentive effects of inequality itself, e.g. “*Income inequality creates competition, which creates the incentive and motivation to improve oneself.*”

²⁹Taken at face value this indicates that roughly 96% of respondents believe in at least one inequality externality.

³⁰The last two statistics are partly by design, as respondents were screened on failing too many attention checks.

³¹Note that the set of controls we use was specified in our pre-analysis plan. Here we have also included dummies for each treatment group (not shown) to allow the use of the full sample. The demographic correlations

Figure 3: Distribution of Externality Beliefs in Democrat- and Republican-Leaning Respondents



Note. Questions are ordered according to which portion of respondents believe that inequality increases the variable. Full question example: “How does more economic inequality change the amount of crime in a country?” Answer option example: “More inequality → a lot more crime”. To determine whether a respondent is Democrat- or Republican-leaning, respondents are asked whether they identify more strongly as Democrat, Republican or Independent. Those who identify as Independent are asked a follow-up question to clarify whether they feel closer to the Democrat or Republican party. $n \in \{1598, 1694\}$.

Individuals who identify as or lean Republican are significantly less likely to believe in negative externalities (that inequality reduces trust, increases crime...) than individuals who identify as or lean Democrat; we replicate Figure 2 for these two groups in Figure 3. Still, these beliefs are relatively similar across party affiliation. Between the three potential answers for each channel – negative inequality externality, no externality, and positive inequality externality – self-reported Republicans (excluding Republican-leaning Independents) are most likely to choose the negative externality option for every outcome we elicit except for innovation, where the no-externality option is most likely (the negative externality option being second).

Notably, Republicans choose the negative externality option more frequently than the positive externality option for all three economic outcomes we elicit. Republicans are more likely to believe that more economic inequality *decreases* (41.9%) rather than *increases* (28.2%) economic growth, *increases* (41.0%) rather than *decreases* (18.0%) the level of unemployment, and *decreases* (31.6%) rather than *increases* (27.6%) the amount of innovation. These results are surprising, but are generally robust across our two surveys with distinct respondents and question methodology.³² We show this in Figure E5. We will further discuss and contextualize these party differences shortly.

The gender and age of the respondent does not generally predict their externality beliefs. College-educated individuals are consistently more likely to believe in negative externalities,³³

do not change notably if excluding the fairness and government trust variables.

³²The innovation result is the most fragile of the three, and changes under different question phrasing methods and robustness specifications. The growth result is largely robust, but marginally flips under specific question phrasing (substituting “equality” and “differences in income and wealth” for “inequality”). See Figure E5 for details.

³³Note that this exaggerates the descriptive results somewhat, as our data has a larger share of college graduates than a fully representative national sample. This effect is relatively small, however. College-educated respondents

despite income and wealth generally not being significantly predictors.³⁴ Finally, respondents who live in the *West* U.S. Census region have stronger negative inequality externality beliefs than the remainder of our sample (a difference of ~ 5 p.p.), and income inequality on the state level does not correlate with externality beliefs (not shown).

Overall, this analysis shows that beliefs in inequality externalities are widely held across every demographic group.

4.2.3 The varying polarization of fairness views and externality beliefs

As we described above, party allegiance is a determinant for inequality externality beliefs, but perhaps a smaller one than might have been expected. To get a sense of the magnitude of this heterogeneity, we compare it to heterogeneity in broad economic fairness views. We find that the predictive power of party allegiance on externality beliefs is weaker than the predictive power of party allegiance on fairness views; this is illustrated in Figure 4 for two example questions.³⁵ We show the consistency of this result in Figure 5, which shows party differences in responses across every fairness- and externality-based question posed in Survey 1. Every fairness question is more party-polarized than every externality question; the same strong result largely holds in Survey 2, which we show in Figure E8.³⁶ Generally speaking, that fairness views are more polarized across party affiliation than externality beliefs is an extremely consistent result across both surveys.

A similar result can be found in both wealth and income, which we show an example of in Figure 6. Externality beliefs are relatively independent of income and wealth, unlike fairness views which are more conservative in both.³⁷ This is again a general result in our full samples, which we show in Figure 7 for Survey 1. Within Survey 1, the drop in anti-inequality sentiment for respondents with an income above 100,000\$ is larger for every fairness question than for any inequality externality question. In Survey 2 (Figure E9) a similar pattern holds, although there is some overlap between questions.³⁸

are, on average, ~ 5 p.p. more likely to hold a negative externality viewpoint than non-college graduates. Our merged sample has 19 p.p. more college-graduates than a fully representative sample; taken at face value, the net effect of this oversampling is a roughly 1 p.p. increase in the share of negative externality beliefs in our data. As expected from this calculation, our results do not change significantly if we re-weight the sample to be fully representative on education levels (see Figures E3-E3).

³⁴In the few exceptions, higher-income respondents generally believe somewhat less in inequality’s negative consequences. This shift is significant for innovation, corruption, and economic growth. In each of these three cases, moving from the lowest (\$0-\$25k) to the highest (\$100,000+) income-bracket decreases the likelihood of believing in the negative externality by 8-9 percentage points; see Figure 7.

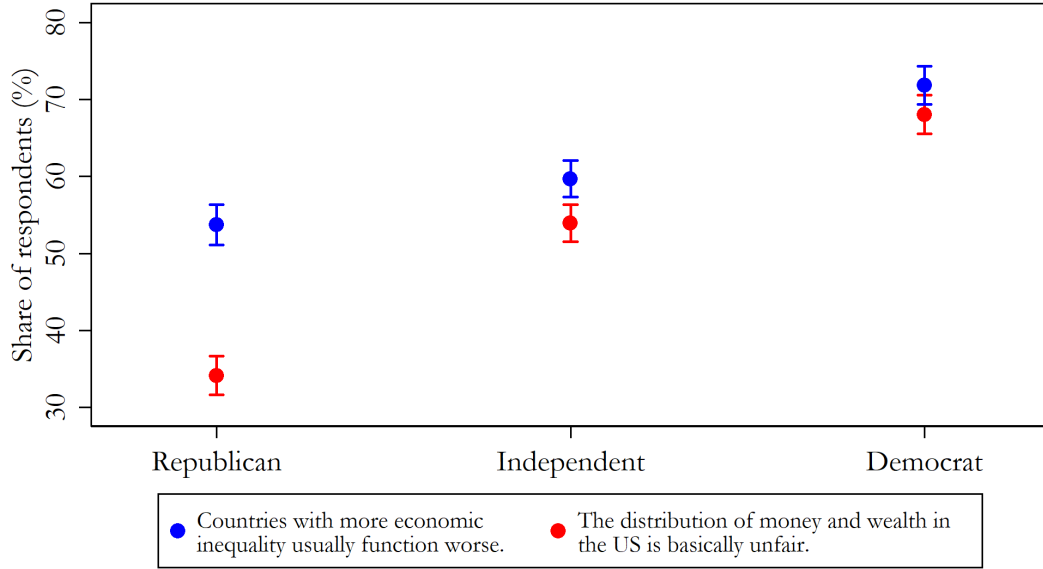
³⁵These two questions ask respondents to agree or disagree with the statements that (i) “*The distribution of money and wealth in the U.S. is basically fair, because everybody has an equal opportunity to succeed*” and (ii) “*Countries with more economic inequality generally function worse*”. These questions were chosen as an example as they were posed *before* the treatment intervention, allowing us to use the full Survey 1 sample ($N = 4,371$). The figure and values are without controls. The result is robust to adding a standard set of controls.

³⁶The general externality question in Survey 2 has a 0.12 p.p. larger response share difference between parties than the least polarized fairness question in either sample. If splitting instead by Republicans and Democrats (ignoring Independents), the result holds for every question in both surveys. The result is fully robust to adding controls within Survey 1. Within Survey 2, one additional externality question (on democratic institutions) becomes more party-polarized than the least polarized fairness question.

³⁷The latter follows the literature; generally, individuals with higher economic status believe that the distribution is more fair (e.g. Valero, 2021). The figure is without controls. The result is robust to adding a standard set of controls.

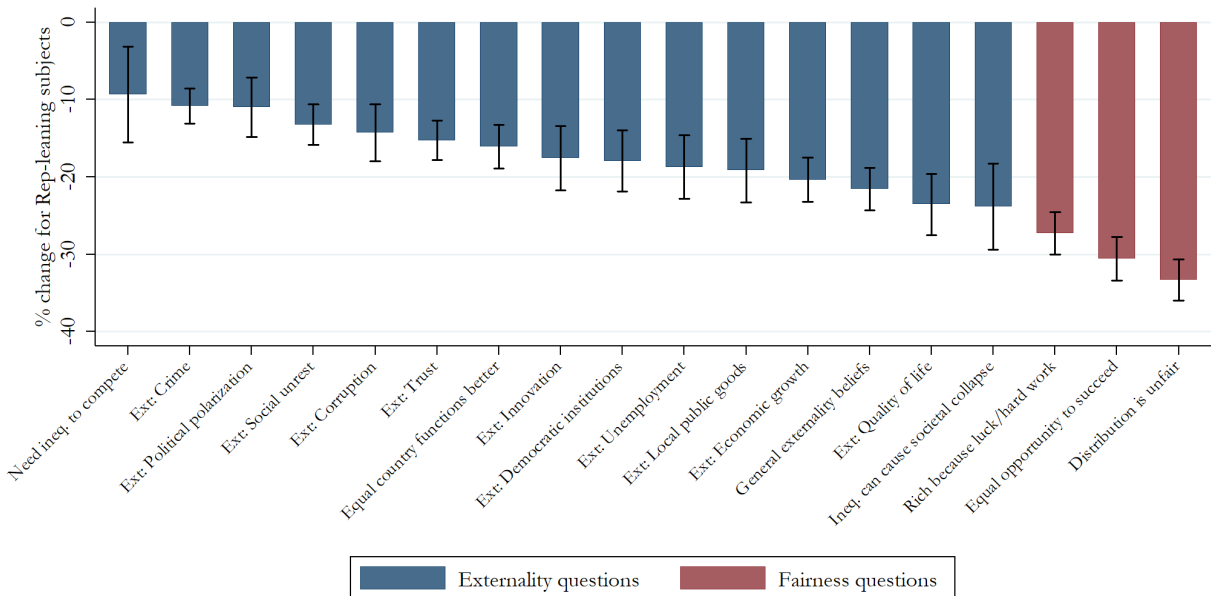
³⁸This strong result is robust to adding demographic controls within Survey 1. In Survey 2, adding demographic controls makes three more externality questions overlap.

Figure 4: An Example of Externality Beliefs and Fairness Views over Party Affiliation



Note. This graph uses the pre-treatment externality and fairness questions with the full sample ($N = 4,371$). Respondents are asked to agree or disagree with the following two statements: “*The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity to succeed*” and “*Countries with more economic inequality usually function worse*”. The equivalent graph for Survey 2 respondents is Figure E6

Figure 5: The Effect of Party Affiliation on Fairness Views and Inequality Externality Beliefs



Note. Drop in anti-inequality sentiment for Republican-leaning respondents across every externality and fairness question in Survey 1 without any controls. With a standard set of controls the same relation holds (all fairness questions have larger polarization than any externality question). Questions are largely split on pre-specified criteria or natural binary points (e.g. agree/disagree), keeping total shares close to 50% where possible. The equivalent graph for Survey 2 respondents is Figure E7. Error bars are 95% confidence intervals.

Measurement error presents a potential caveat for these results. Suppose there are systematic differences in the amount of measurement error in individuals’ responses to these questions across question type. For example, individuals could be more familiar with fairness questions and thus answer these questions more accurately based on their genuine beliefs. This could explain the low polarization of externality questions; in the extreme case where there is no signal in respondents’ answers, one would expect no aggregate differences across groups. We find some evidence of larger measurement error in externality questions, as respondents’ answers are less strongly correlated across externality questions than fairness questions. However, we also find that the vast majority of respondents confirm their externality beliefs when asked in a follow-up open-ended text question in Survey 2. The relatively small party and income differences for externality views persist across these open-ended text answers.³⁹

4.2.4 The importance of each inequality externality channel

That economic inequality affects a variable does not necessarily imply that the effect of economic inequality on the variable is *important*. To examine this dimension we ask respondents to delegate 100 points to the externality channels that “matter the most”, separating respondents who believe in an overall positive or negative inequality externality.⁴⁰ The average responses to this question are shown in Figure 8.

Respondents indicate that crime and corruption are the most important negative externalities, and that economic growth and innovation are the most important positive externalities. We note that answers to the negative and positive externality versions are clearly different. Most notably, the economic factors are deemed the least important negative externalities and most important positive externalities.

In Survey 2 we also ask respondents whether they deem a specific inequality externality “meaningful”.⁴¹ Results here are qualitatively similar.⁴² Answers also indicate that respondents believe these issues are important; a majority of respondents believe the given externality is “generally meaningful” (30%) or “very meaningful” (32%).

4.2.5 Top- or bottom-inequalities

Inequality externalities could depend heavily on the *type* of inequality. Specifically, inequalities near the bottom – the amount of relative poverty⁴³ – and inequalities near the top could affect

³⁹This will be added in an Appendix at a later date.

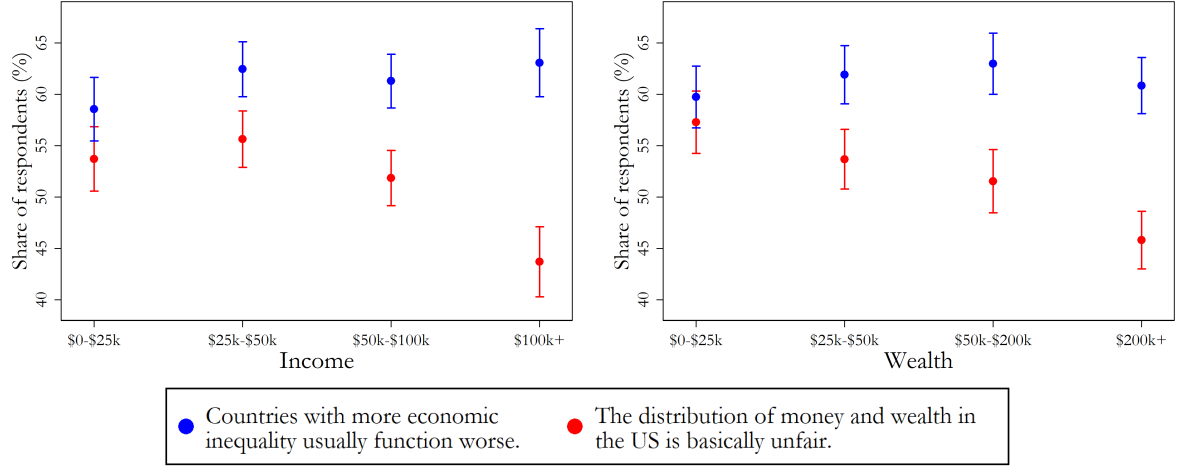
⁴⁰Full question: “*When thinking about how inequality [negatively / positively] affects society, which dimensions do you think matter the most, generally speaking? Please indicate what dimensions you think matter the most by giving scores below that add up to 100*”. This question only makes sense if the respondent thinks inequality has at least one negative or positive externality, and we only ask the negative or positive externality version to those who answered that inequality generally affects society negatively or positively, respectively, in the general externality question shown in Table C2. We also allow respondents to self-select out of the question by stating that changed their mind. There is thus a selection effect; each group should not be seen as a representative sample, but instead as the subsection of respondents who believe inequality affects society negatively or positively, respectively.

⁴¹In the sense that it is “*something politicians and policy-makers should be focused on, or [...] ultimately not very important*”.

⁴²The three most meaningful channels are considered to be the quality of democratic institutions (70%), crime (67%), trust (66%), and corruption (65%). Note that the quality of democratic institutions is not included in Figure 8 due to a coding error.

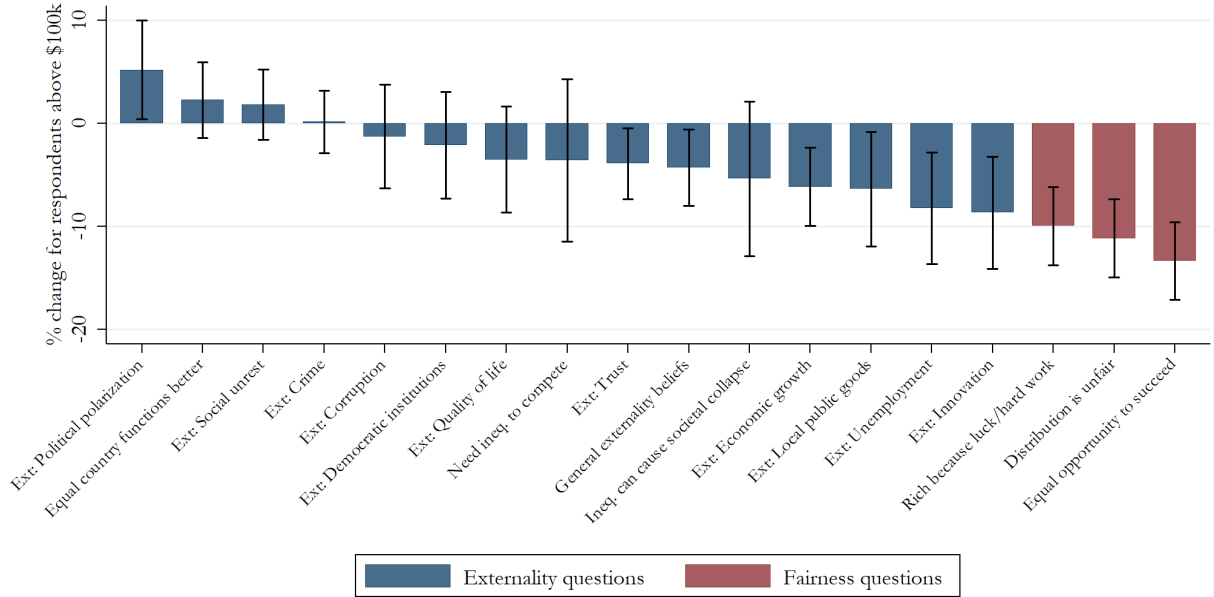
⁴³We note that relative poverty, unlike absolute poverty, is an inequality metric.

Figure 6: An Example of Externality Beliefs and Fairness Views over Income and Wealth



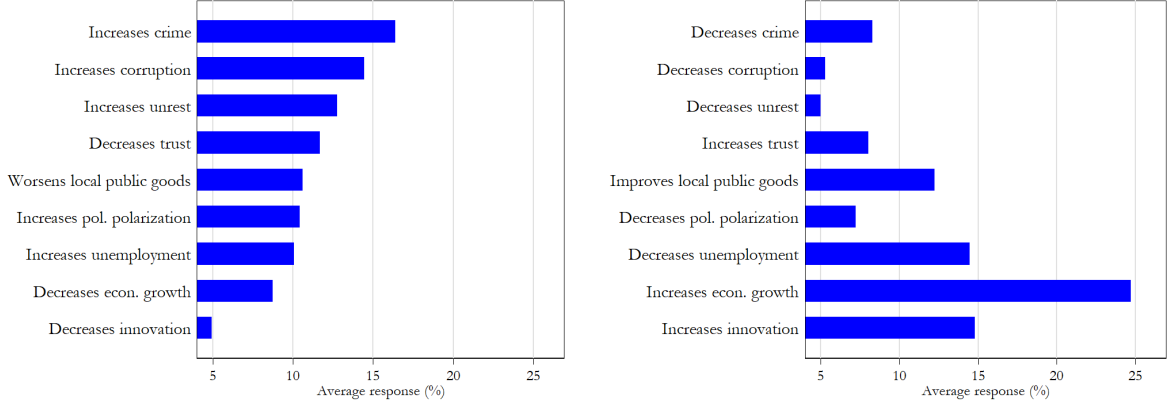
Note. These graphs use the pre-treatment externality and fairness questions with the full sample ($N = 4,371$). Respondents are asked whether they agree with the following statements: “The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity to succeed.” and “Countries with more economic inequality usually function worse.”. For the equivalent graph from Survey 1, see Figure E7.

Figure 7: The Effect of Income Level on Fairness Views and Inequality Externality Beliefs



Note. Drop in anti-inequality sentiment for respondents with incomes above \$100,000 across every externality and fairness question in Survey 1 without any controls. With a standard set of controls the three most income-dependent externality questions (those on innovation, local public goods, and unemployment) are roughly as income-dependent as the fairness questions. Questions are largely split on pre-specified criteria or natural binary points (e.g. agree/disagree), keeping total shares close to 50% where possible. Error bars are 95% confidence intervals.

Figure 8: Comparative Magnitudes of Externality Channels



Note. These questions were only asked to those in the control groups who also (i) answered that inequality is a negative (left) or positive (right) externality, and (ii) did not answer that they changed their mind when posed this question. Sample size is $n = 472$ (left) and $n = 100$ (right).

different channels differently. To explore this we ask respondents in Survey 2 what type of inequality matters more for any given externality.⁴⁴

Table 1: What Matters: Inequalities Near the Top or Bottom?

	Pol. polar.	Crime	Corr- uption	Inno- vation	Social unrest	Econ. growth	Trust	Dem. inst.
Both	48%	40%	41%	39%	36%	36%	36%	40%
Bottom inequality	33%	49%	22%	38%	51%	38%	43%	36%
Top inequality	11%	7%	29%	15%	9%	14%	9%	18%
Don't know	8%	4%	8%	8%	4%	12%	11%	6%
Sample size	247	251	228	212	249	226	226	214

Note. The share of respondents who think economic differences near the bottom or top matters more for one randomly chosen inequality externality; data is from Survey 2.

The answers are shown in Table 1. Generally respondents believe both top- and bottom-inequalities matter. However, bottom-inequalities are generally considered more important than top-inequalities, particularly for trust and crime. The only exception is corruption, where top inequalities are deemed most impactful. We also show that respondents generally have similar externality beliefs regardless of whether the initial inequality level is described as “initially very equal, very unequal, or anything else” in Appendix D.2. Finally, in Appendix D.3 we discuss the results from a question allowing individuals to predict the amount of various outcomes (crime, trust, and so on) given the level of average income and economic inequality.

This section has explored U.S. citizens’ beliefs about the consequences of economic inequality. To summarize, U.S. citizens strongly believe that economic inequality has negative societal

⁴⁴Example question text: *What do you think matters more for how economic inequality changes the amount of social unrest? · Economic differences near the bottom, meaning how many relatively poor people there are and how little they have, or · Economic differences near the top, meaning how many relatively rich people there are and how much they have.*

consequences, even (generally) on economic outcomes. This is our first main result. These beliefs are broadly shared across party affiliation and income. They are also less polarized on these dimensions than comparable economic fairness views. Having established these facts, we move to the next main objective of the paper – exploring the effect of these inequality externality beliefs on individuals’ redistributive preferences.

5 Redistributive Preferences and Inequality Externality Beliefs

The second main objective of this paper is to analyze how inequality externality beliefs relate to individuals’ preferences for redistribution. This section explores this relationship using various methods; our main analysis centers on a video-based information provision experiment in Survey 1, which intends to establish a causal effect of inequality externality beliefs on redistributive preferences. The primary purpose of the information experiment is to robustly establish that inequality externality beliefs partly determine individuals’ preferences for redistribution. We also explore (i) correlational horse-race analyses, and (ii) a customized survey module that assesses the self-reported relative importance of various redistributive motives. Throughout the section we will compare the effect of inequality externality beliefs to that of broad economic fairness concerns, which are well-known strong determinants of redistributive preferences (Almås et al., 2020).

5.1 Experimental design

To study the causal effect of inequality externality beliefs on redistributive preferences an information provision experiment was integrated into Survey 1. The structure of Survey 1 is shown in Figure 9. The survey is divided into three parts. In Part 1, we elicit sociodemographic information that is needed to check for representativeness.^{45,46} Part 2 presents subjects the intended information. Part 3 elicits respondents’ preferences for redistribution and inequality externality beliefs, which constitute our main reduced-form and first-stage outcomes respectively.

5.1.1 Information treatment

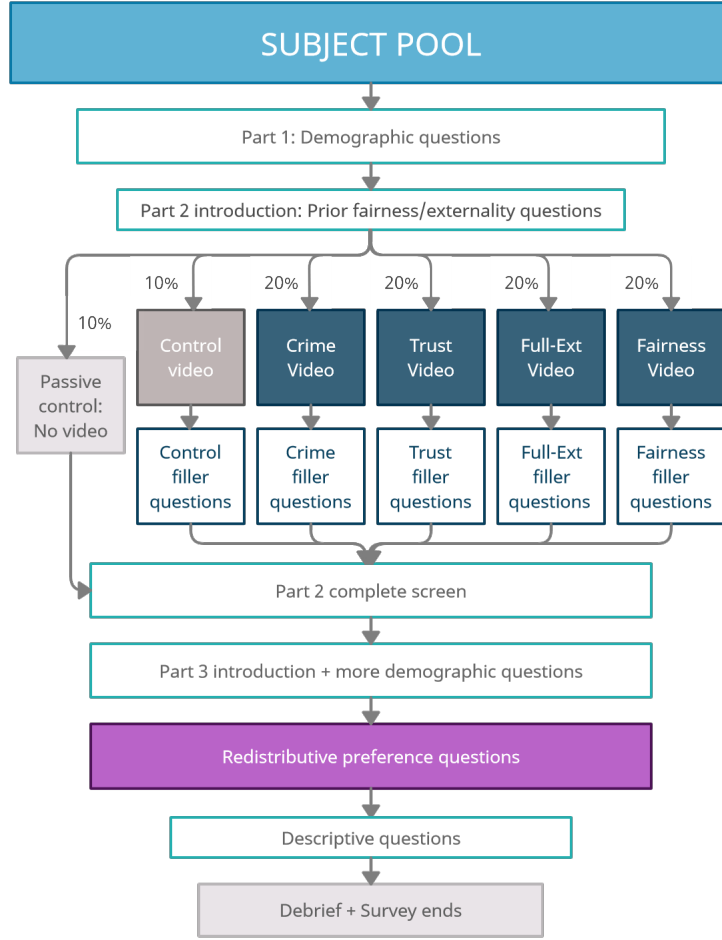
The information intervention in Part 2 is our main treatment variation. All subjects are first asked to answer two questions about (i) their general inequality externality beliefs, and (ii) their broad fairness beliefs.⁴⁷ These questions have two functions. First, to measure pre-treatment first-stage outcomes (when they should be equal across groups). Second, as a lead-in to the

⁴⁵This information is also used to check for selective attrition across the treatment groups, which we do not find any significant evidence of.

⁴⁶We also elicit respondents’ trust in the federal government and beliefs about whether people work less when taxed more. These latter attitudes have shown to be important drivers of redistributive policy preferences that are independent of fairness concerns or externality beliefs. For that reason, we elicit these views *before* the information intervention.

⁴⁷These questions are “*How much do you agree with the following statement? Working-class Americans are generally paid less than their productivity.*” and “*How much do you agree with the following statement? Countries with more economic equality usually function worse.*”

Figure 9: Survey Flow of Survey 1



video information treatment.⁴⁸

After these questions the sample is split into four treatment groups and two control groups. Videos are shown to the subjects in all four treatment groups and in the active control group. Each treatment video aims to shift *either* respondents' inequality externality beliefs *or* economic fairness views. We will discuss these in further detail shortly; for now it is sufficient to note that there are three inequality externality treatment groups and one fairness treatment group. The two control groups (one “passive” with no stimuli, one “active” with a control video) are as large as each of the four treatment groups when combined, which they eventually are on pre-specified criteria. We detail this *dual control group approach* further below.

We note that the short survey time (~ 19 minutes on average) and lack of a follow-up survey presents potential issues with external validity for our survey experiment. The treatment effects we present are within-survey estimates, and we do not know whether these shifts are temporary or permanent in nature. This is an intrinsic limitation of our work which is also shared by other survey projects where obfuscated follow-ups (Haaland and Roth, 2020) are not possible. To remedy this we have taken steps to create an approximation of an obfuscated follow-up survey

⁴⁸Subjects in treatment groups are introduced to the video with the following prompt: “We will now show you some information regarding the last question you answered. Please watch the video below.”

within the survey itself. We thus introduce what we call the *secondary survey* approach.

Secondary survey Experimenter demand and priming effects are key problems in traditional video information treatments. These issues are often partly circumvented by adding unrelated questions between the information treatment and outcome variables. However, this introduces a different issue; respondents who are presented the information do not understand *why* they were presented the information. This could lead to confusion or suspicion of future questions which could bias outcome data. To avoid this we design what we call a *secondary survey* – a logical flow of questions that explains the information treatment while disguising the true purpose of the survey.

In the present experiment, the secondary survey relies on what we call “filler questions”. These “filler questions” immediately follow the video and are directly related to the video content. All these questions focus on personal experiences related to the video topic. In the crime treatment, an example of one such question is the following: “*Have you lived in more than one place in your life? If so, think back – do you think the places with more economic inequality had more crime, generally speaking?*” These questions are designed to hide the purpose of the study by being directly related to the videos (and so explaining why the respondents had to watch them) while being unrelated to the true intent of the survey.⁴⁹ They thus create the impression that the videos are shown to lead into these filler questions and have no direct link with the rest of the survey.

To emphasize this connection we immediately end Part 2 of the survey after the filler questions, notifying respondents of this. We then start Part 3 with an introduction screen, upon which we continue with several unrelated demographic questions to create the appearance of each survey part being functionally independent. Our true treatment effects are all based on questions in Part 3 (see Figure 9). The respondents have thus seen a self-contained *secondary survey*, which should minimize experimenter demand and priming effects. While many survey experiments employ some structural break between treatment and outcome, we believe the formalization of this broader concept is a beneficial addition to the literature.

We note that we cannot guarantee that the filler questions themselves do not change individuals’ beliefs about the video topic. The crime question above could conceivably change individuals’ beliefs about how economic inequality affects crime, for example. This is less problematic than it might seem, however, as the origin of respondents’ opinion change – whether from the video or the filler questions – is of second-order importance to our research questions.⁵⁰

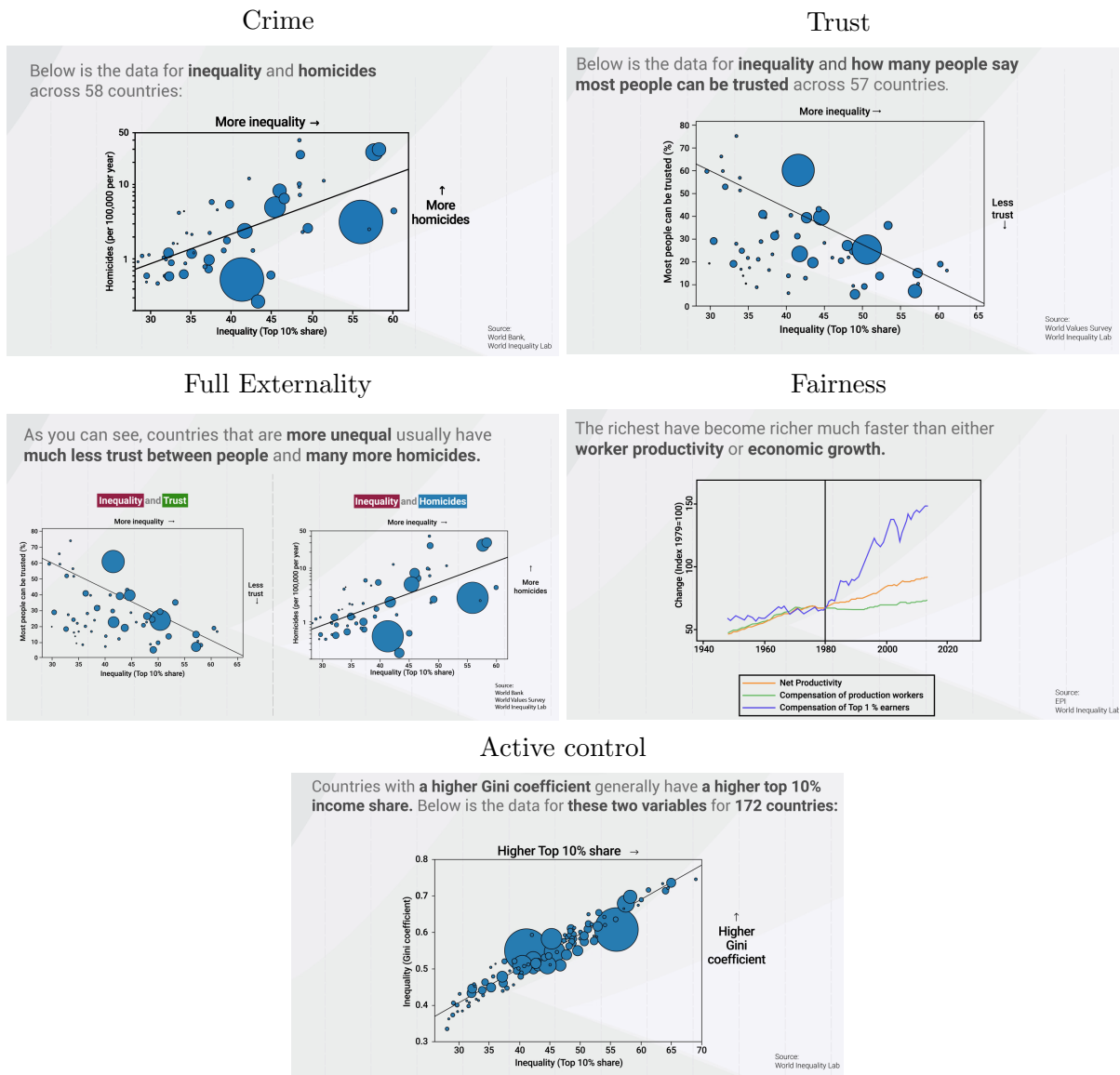
5.1.2 Video treatments

Each video is based on animated motion graphics that present information in an easily digestible way, preventing survey fatigue. Screenshots and links to the full videos are shown in Figure 10. After watching the video, respondents answer three very simple control questions to ensure that they understood the information provided in the video. We require respondents to answer these

⁴⁹The specific filler questions are discussed further in Appendix C.6.

⁵⁰First-stage responses in Section 5.2.1 and mediation analysis in Appendix D.6 show that the treatment mechanism appears to go through a shift of the intended beliefs.

Figure 10: Treatment Video Screenshots



Note. These are screenshots from the five videos used in the survey experiment. One video was shown to each respondent, except for the 10% of respondents in the passive control group. Click the following links for the full videos: Crime – Trust – Full externality – Fairness – Active control

questions correctly to proceed with the survey.⁵¹ They are then showed the filler questions discussed above.

The below section briefly describes the content of each video. For more information, including information on the filler questions, see Appendix C.6. The videos can also be watched through the hyperlinks in Figure 10.

Treatment group 1: Crime as an inequality externality This treatment group receives information on the positive correlation between crime and inequality using data from the World Bank and the World Inequality Database.

⁵¹ Respondents who answer incorrectly are able to change their answers after being presented the video screen again (with the option to re-watch).

Treatment group 2: Trust as an inequality externality This treatment is structurally similar; the treatment group receives information on the negative correlation between generalized trust and inequality using data from the World Values Survey and the World Inequality Database.

Treatment group 3: Full externality treatment This treatment is designed as an all-encompassing externality treatment. It combines the information from Treatment 1 and 2 with additional correlational evidence which shows that inequality is not correlated to economic growth and innovation. It also includes a more comprehensive discussion on economists’ opinions on how inequality could change societies. By presenting broad evidence that highlights the *negative* effects of inequality and by showing that the evidence for positive externalities is rather limited, the treatment makes the strongest case for the negative consequences of inequality between our three videos.

Treatment group 4: Fairness treatment This treatment group receives information on how U.S. manufacturing workers’ wages have stagnated while their productivity has increased between 1980-2019, using data from the Economic Policy Institute. This is contrasted to the growth of the top 1% income share in the U.S. from the World Inequality Database.

Dual control groups There are benefits and drawbacks to both a passive control group, where respondents see nothing, and an active control group, where respondents see information on a similar but unrelated topic. The main drawbacks of a passive control group are two-fold. First, if respondents see no stimuli/video, their overall attention and survey fatigue will likely differ from respondents who saw treatments in the post-treatment part of the survey. This could bias results due to attention issues and cause attrition problems. Second, priming from the information treatments – hearing the word “inequality” in our treatments, for example – could conceivably drive outcome differences. In surveys with only a passive control group these are untestable hypotheses. From these perspectives, an active control group is preferred. However, an active control group, no matter how well-designed, could always unintentionally convey information that affects the outcomes of interest. In experiments with only an active control group the existence of such unintentionally conveyed information is itself an untestable hypothesis. It is clear, then, that either method has significant drawbacks.

To solve these issues we introduce what we call *dual control groups*. This involves splitting the control into two groups; one active control and one passive control. The main outcomes are then compared across these two control groups and checked against pre-specified merging criteria. In the case that there are no significant differences across the control groups, the concerns stated above can safely be ignored and the two control groups can be merged.⁵² The researcher can thus test whether attention effects and priming about the relevant concept (inequality, in our case) have important effects, which presents an improvement over the uncertainty involved in single control groups.

⁵²The only exception to this is in the very unlikely case that the attention effects from the passive control and unintentional information from the active control group has exactly the same effect.

The idea of several control groups is not new; multiple control groups in observational studies have been discussed extensively (Rosenbaum, 2002). Intentionally designed dual control groups in information experiments are less common, however, and have as far as we know not been employed or formalized before. The usage of pre-specified merging criteria in such groups is also as far as we know novel. Below we present a short description of each control group.

Control group 1: Active control The active control group receives a video that is structured similarly to those on trust and crime, informing subjects about the difference between the Gini index and the top 10% income share. The video does not contain any information that is strictly speaking relevant for redistributive preferences, but does give individuals stimuli about inequality itself.

Control group 2: Passive control This group does not receive any stimuli or filler questions. Outcome differences between the two control groups were small and satisfied the pre-specified merging criteria. The control groups were thus merged and will be discussed as one larger control group for the remainder of the paper. The merging criteria and further discussion can be found in Appendix D.4.

Summary of the variation induced through the treatments Treatment groups 1 to 3 provide information about inequality’s externality properties while attempting to avoid fairness-related topics. Comparing redistributive preferences of these groups to the baseline thus gives us insights into the effect of information on the consequences of inequality on preferences for redistribution under the assumption of no other redistribution-related spillovers. Treatment group 4 informs subjects about the fairness of existing inequality but makes no reference to the consequences of inequality. Comparing the redistributive preferences of this group to the baseline thus allows us to identify how information on distributive fairness shapes preferences for redistribution, again under the assumption of no other redistribution-related spillovers. Comparing the magnitude of the treatment effects yields indicative evidence on which type of information has a larger impact on redistributive preferences.

It should be noted that all our treatments are designed to have weakly positive effects on beliefs, in the sense that the induced variation in beliefs should always lead to a weakly larger demand for redistribution. This feature is implemented by design for two main reasons. First, it enables us to form clear hypotheses for potential treatment effects. Second, there are no clear existing correlations between inequality and societal outcomes that imply positive externalities. The correlational evidence we present on crime and trust is strong and robust to different specifications; this is not the case for any potential positive externalities we know of.⁵³

We compare observable characteristics across the treatment and control groups across in Appendix D.5. Overall, the groups are, as expected from our research design, well-balanced. Though there are small differences in observables, these seem fairly spurious and do not reveal any systematic changes across treatment groups.⁵⁴ This also indicates limited differential

⁵³There is no strong cross-country correlation between inequality and economic growth, for example.

⁵⁴Note that our regressions control for observable characteristics; including or excluding these regressors does not change the results, underlining the that they do not mirror relevant variation across treatment groups.

attrition across treatments which we discuss further in Appendix C.

5.1.3 Outcome variables

Preferences for redistribution Our main treatment outcome variables are various measures of preferences for redistribution. We elicit redistributive preferences on both general and specific levels. The most general question asks respondents to choose the level of redistribution that they prefer on a scale from no redistribution to full redistribution.⁵⁵ This question does not mention government involvement, and is complemented by a question that asks subjects whether they prefer the government to take measures to reduce inequality on a Likert scale (the same as used by the European Social Survey). We further ask the respondent whether they believe that inequality is a very serious issue in the United States (used in Stantcheva, 2021, among others). To assess whether our results also apply to specific policy preferences, we elicit respondents' preferred average tax rate for the so called "Top 10%" over seven different options.⁵⁶

We pre-specified these four outcomes (general redistributive preferences, government should reduce inequalities, inequality is a serious issue, top tax rates) as well as a main outcome index (RP-index). This main outcome index was pre-specified as the standardized sum of dummy versions of all the four outcomes.⁵⁷

Externality beliefs and fairness views After the redistributive preference outcomes we elicited respondents' externality beliefs and fairness views, which represent our first-stage beliefs. Respondents are first asked the general externality belief question discussed in Section 4.1, then an open-ended text question on their externality beliefs, then specific externality belief questions on crime and trust (discussed in Section 4.2).⁵⁸

Afterwards respondents' fairness views are elicited with two questions. The first asks whether the current distribution of income and wealth in the US is fair because everybody gets what they are entitled to or whether some get much more than what they are entitled to, while others get too little. The second asks whether hard work or luck "has more to do with" why a person is rich.⁵⁹

The last part of the survey includes further questions on various externality channels and final debrief questions.

⁵⁵See pages 14-15 in the questionnaire for the full phrasing (Appendix C.1).

⁵⁶These options are 0%, 0-15%, 15-25%, 25-35%, 35-45%, 45-70%, and 70-100%. Each option contains a short explanation (e.g. "35-45%: *I want to tax them at a higher rate than now, but not very high*").

⁵⁷Where to split the four questions into dummy variables – e.g. 35-45% and above for the tax question – was also pre-specified, intending to split each question into roughly equal fractions.

⁵⁸We note that respondents were asked to be particularly attentive when shown the questions regarding crime and trust. This can be seen in the questionnaire (Appendix C.1). We made this design choice to maximize attention and minimize measurement error. We acknowledge that this reminder may induce a demand effect in these responses. This would not affect any questions placed earlier in the questionnaire, notably (i) our main outcome questions (on redistributive preferences), and (ii) the open-ended text question detailed below, which provides further support that the videos targeted the relevant belief.

⁵⁹We note that our fairness questions are broad in the sense that they can include (i) beliefs about the origin of economic inequalities, and/or (ii) views on economic fairness given a known economic process and distribution. In this case the first question includes both (i) + (ii), whereas the second question only includes (i).

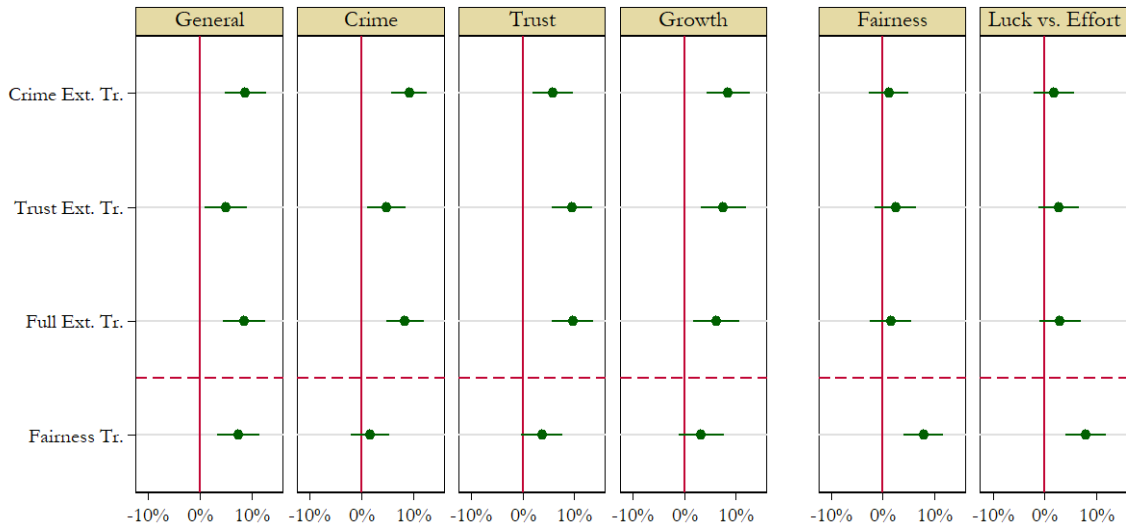
5.2 Experimental results

We will now discuss the results from the main information experiment. In Appendix D.7 we present a theoretical framework to clarify when these results allow us to assert that inequality externality beliefs causally affect redistributive preferences. In short, we require at least one externality treatment to have (i) first-stage effects on externality beliefs with limited spillovers on fairness views, and (ii) significant treatment effects on redistributive preferences.

5.2.1 First-stage outcomes

The first-stage outcomes of the experiment are shown in Figure 11. As can be seen in the first four columns, each of the three externality treatments significantly change respondents' general and specific externality views. The targeted specific externality concern is most affected; as an example, the crime externality belief is the most affected by the crime and full externality treatments.⁶⁰ None of the externality treatments significantly affect fairness views (last two columns), showing that fairness-based spillovers are limited for the externality treatments. Indeed, this provides some evidence that the two concepts are relatively independent.

Figure 11: First-stage Effects of Treatments



Note. This figure reports results from a pre-specified regression of different externality beliefs and fairness views on the treatment dummies as compared to the control group. The general and specific externality belief variables (left) are discussed in Section 4. The fairness variables (right) indicate whether the respondents believe the distribution of income in the U.S. is generally unfair (*Fairness*) and whether the respondents believe high-income individuals became rich mainly due to luck or effort (*Luck vs. effort*). Controls include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, and geographic region. Error bars characterize 95% confidence intervals. Table F20 presents the point estimates and standard errors. *Significance levels:* *10%, **5%, ***1%.

The fairness treatment strongly affects fairness views (bottom-right). It also has a significant effect on general externality views (bottom-left), indicating that some externality-based spillovers exist for the fairness treatment. This is noteworthy, but does not influence our main research question of whether inequality externality beliefs affect redistributive preferences.⁶¹

⁶⁰We do note that there seems to be externality-based spillovers; the crime treatment changes their trust externality belief, for example.

⁶¹The main consequence of these spillovers is that the fairness treatment's reduced form coefficient could be

Overall, the first stage treatment effects are strong. Each video increases beliefs in the intended direction by roughly 10 percentage points. These are sizable effects, particularly given that the control means for negative externality beliefs are already high – the crime- and trust-externality beliefs in the control group are at 76% and 68% respectively, for instance. To put it differently, the treatments help to raise negative externality beliefs to near-consensus levels on these matters.

The strong first stage effect of our externality video on externality beliefs is further corroborated when analyzing the post-treatment open-ended text question about externality opinions. This question asks respondents to write about how they think inequality changes society without prompting them specifically in any further direction. Table F19 in the Appendix presents the share of answers that include the words “crime” or “trust” by treatment group. We find a strong effect of the respective treatments on respondents mentioning “crime” or “trust” in their answers. As an example, the word “crime” is used by about 15% of the crime and full externality treatments, and only about 4% of any other treatment or control group. To ensure that this is not driven by respondents simply describing the video, we check the equivalent for the word “video”, which was barely mentioned by respondents in any group (0.18% of all respondents).⁶² This highlights that the video as such is barely discussed in the answers; instead respondents discuss the informational content itself. This indicates the success of the *secondary survey* we describe in Section 5.1, softening concerns about experimenter demand.

In sum, we find that the externality treatments strongly affect externality beliefs and do not significantly affect fairness views. The fairness treatment strongly affects fairness views and only affects externality beliefs in a limited fashion.

5.2.2 The effect of the information treatments on redistributive preferences

Table 2 shows the effect of the different treatments on the pre-specified redistributive preference index as well as the four outcomes from which it originates (see Section 5.1.3).

The full externality treatment has a significant and, for this kind of study, reasonably large effect. Three of our four pre-specified measures of redistributive preferences are significant and the effect on the aggregate redistributive preference index is significant at the 1% level.⁶³ The index increases by 11 percent of a standard deviation in response to the treatment. This *marginal* effect is on the same order of magnitude as the *net* effect of gender (which is comparable to that found in other studies),⁶⁴ or about $\frac{1}{8}$ of the difference between Republican- and Democrat-leaning subjects. Following the experiment design, this represents our second main finding; inequality externality beliefs causally affect redistributive preferences.

inflated. This would affect the relative magnitude of the externality and fairness treatment effects.

⁶²The same is true for other related words, such as “Youtube”, “infographic”, and so on.

⁶³We do not find any effect of any externality treatments on preferences for top-income taxation. As the other treatment effects from the full externality treatment are strongly robust, this is somewhat surprising. This can be due to the respondents not fully internalizing the connection between higher top tax rates and lower inequality, or because respondents believe that the effect of inequality on trust and crime is primarily affected by inequalities near the bottom as corroborated by Table 1. We also note that the active control showed a surprisingly high treatment effect for this variable (see Appendix D.4) – the non-result from the externality treatments could also be driven by this anomaly.

⁶⁴The higher preferences for redistribution among women in our sample represent a replication of a frequent finding (e.g. Alesina and Giuliano, 2011).

Table 2: Main Treatment Effects of Video Information Experiment

	(1)	(2)	(3)	(4)	(5)
	RP Index (st. dev)	Wants redistribution (0-1)	Increase top taxes (0-1)	Gov. reduce ineq. (0-1)	Ineq. is serious issue (0-1)
Crime Ext. Tr.	0.037 (0.036)	0.031 (0.020)	-0.005 (0.021)	0.007 (0.020)	0.020 (0.019)
Trust Ext. Tr.	0.043 (0.037)	0.006 (0.021)	0.004 (0.022)	0.036* (0.020)	0.017 (0.020)
Full Ext. Tr.	0.107*** (0.037)	0.050** (0.021)	-0.012 (0.022)	0.048** (0.020)	0.069*** (0.020)
Fairness Tr.	0.208*** (0.037)	0.052** (0.021)	0.065*** (0.021)	0.067*** (0.020)	0.115*** (0.019)
Leans Republican	-0.635*** (0.030)	-0.190*** (0.017)	-0.210*** (0.016)	-0.264*** (0.016)	-0.249*** (0.016)
Prior belief unfair	0.707*** (0.027)	0.146*** (0.015)	0.260*** (0.015)	0.260*** (0.014)	0.350*** (0.015)
Male	-0.138*** (0.026)	-0.056*** (0.015)	-0.061*** (0.015)	-0.036*** (0.014)	-0.046*** (0.013)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.391	0.169	0.170	0.293	0.313
Observations	4371	4371	4371	4371	4371

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. The RP index is normalized on the sample and has units of the number of standard deviations. The remaining variables are binary (0-1). Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

The crime and trust externality treatments have only weak and mostly insignificant effects on redistributive preferences. There is only one 10%-significant result, namely that of the trust treatment on the demand for government to reduce inequalities. In general, it appears that information about inequality externalities is more convincing when given in a comprehensive fashion. In other words, discussing the widespread effects of inequality is more impactful than focusing on any single type of externality.⁶⁵

The fairness treatment shows strong and significant shifts on all measures of redistributive preferences. The effect on the redistributive preference index is approximately two times larger than that of the full externality treatment (1.94 times the size).⁶⁶ This represents our first indication of the relative strength of the fairness and externality arguments, although caution should be employed due to the marginal nature of these effects.⁶⁷ We re-visit this topic in Section 5.3.

We note that one of our outcomes – inequality being a serious issue – was also asked in Stantcheva (2021). The treatment effects of the videos in that work (2% - 9%) are similar to those we find (2% - 12%). The main redistribution treatment effect in Stantcheva (2021) is 9%, which is similar to the treatment effect in our fairness treatment (12%).

⁶⁵Theoretically this is sensible. Say that individuals are rational and that a fraction α of their redistributive preferences are determined by inequality externality beliefs. If trust is a fraction γ of their overall inequality externality concerns, the concern for the inequality externality of trust is only a fraction $\gamma\alpha$ of their total redistributive preferences. This could be a small number; from the incomplete subset presented in Figure 8, we can estimate that $0.12 \geq \gamma \geq \gamma\alpha$. Although we do observe some limited spillovers to other externality beliefs – which would increase this number – we consider this the most likely reason for why the Crime and Trust videos have non-significant treatment effects.

⁶⁶This corresponds to about $\frac{1}{4}$ of the difference between Republican- and Democrat-leaning subjects.

⁶⁷Their relative magnitude is also determined by the malleability of these views in respondents and video design choices.

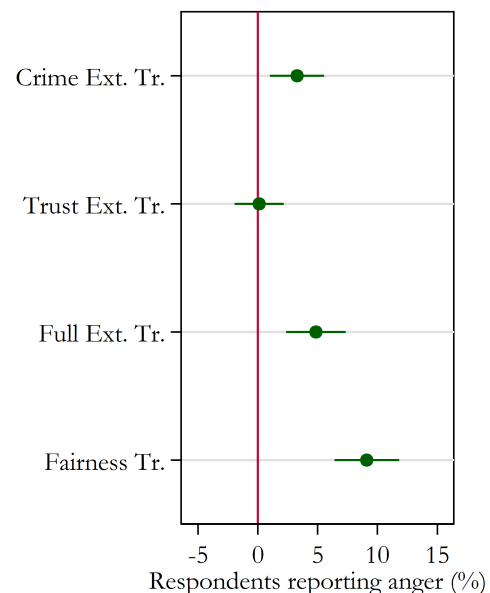
Mechanism We have already seen that the treatments shift redistributive preferences and the targeted first-stage belief. We now show four results that indicate that the mechanism through which redistributive preferences change are these first-stage externality beliefs / fairness views.⁶⁸

First, we conduct a mediation analysis and show that the magnitude of the treatment effects are reduced after controlling for the matching first-stage beliefs. This is as expected if redistributive preferences are shifted through these beliefs, and is shown in Table F24 and detailed in Appendix D.6. Second, instrumenting redistributive preferences through first-stage effects yields significant treatment effects that are very similar to the main specification.⁶⁹ Third, the externality treatment effects are strongest for individuals who did not believe in negative inequality externalities at the beginning of the survey (Table F25). Fourth, respondents that self-reported that they learned something new are more likely to have changed their redistributive preferences (Table F26).

Emotional reactions To explore the psychological channel through which the treatments operate we asked respondents to self-report their emotional reaction to the video at the end of the survey. Respondents were able to answer (potentially several of) anger, interest, surprise, indifference, confusion, or concern.

The most striking difference is on anger, which we show in Figure 12. Respondents who are shown the fairness video are significantly more likely to respond anger than those who have seen any other video. While the absolute percentage of such respondents is relatively small (11.7%), the increase from the control video (2.8%) is almost two times as large ($p < 0.0001$, t-test) as for any other video.⁷⁰ Respondents in the fairness treatment group are also significantly more likely to respond with *anger* compared to those in the full externality group ($p = 0.001$, t-test). This asymmetry is not carried over for other emotions; the equivalent differences between the fairness and full externality videos are not statistically significant for *concern*, *surprise*, *indifference* and *confusion*.⁷¹

Figure 12: Treatment Effects on Anger



Note. Error bars depict 95% confident intervals. The full distribution of emotional reactions by treatment is found in Table F22

⁶⁸Note that we did not include this analysis in our pre-analysis plan.

⁶⁹This is not our preferred specification as the first-stage questions are an imperfect measure of compliers.

⁷⁰The second-highest video is the full externality video (7.8%); third-highest is the crime video (6.1%); fourth-highest, roughly equal to the control, is the trust video (2.9%).

⁷¹The difference between the fairness and full externality treatments is significant at the 5% level in *interest*. Due to the high levels of *interest* in the active control and trust treatments, where other emotions were less frequently reported, we suspect that this option is to some extent used as a “neutral answer” by respondents who did not have a strong emotional reaction to the video (and thus did not know which other emotion to respond). While respondents were not required to enter any emotion, this is not explicitly stated, and most respondents seem to have thought at least one emotion was required – only 27/3833 (1%) respondents left the question blank. The average number of emotions per respondent is also very similar in all videos (between 1.09 and 1.15). The difference in *interest*, then, most likely follows from a zero-sum effect as the fairness video provokes more emotions overall (specifically anger).

This leads us to hypothesize that part of the difference in efficacy between these two videos, and thus potentially the two type of arguments, come from the extent to which they invoke anger in respondents. We thus find indicative evidence for a trade-off between efficacy (fairness arguments) and broad support and low polarization (externality arguments) in redistributive arguments. We further discuss this in Section 6.

Interaction effect in income We pre-specified certain interactions between our treatment dummies and a battery of baseline characteristics. For brevity, most of these pre-specified interactions are shown as tables in the Appendix. We note here that there is a compelling heterogeneous treatment effect through income.⁷²

Individuals with a yearly income below \$50,000 react very strongly to the fairness treatment ($\beta_G = 0.308$, compared to $\beta_G = 0.208$ in the full sample) and have a slightly weaker than average full externality treatment effect ($\beta_\eta = 0.099$, compared to $\beta_\eta = 0.107$ in the full sample). Top-income individuals ($> \$100k$), on the other hand, react more strongly to the externality treatment ($\beta_\eta = 0.188$) than to the fairness treatment ($\beta_G = 0.143$). In total, the larger average fairness treatment effect is driven entirely by individuals with incomes below \$50k. This is shown in Table F28. This has intriguing consequences for the efficacy of each argument for different income groups, which is further discussed in Section 6.

Robustness of treatment effects The conclusions from the information experiment are generally very robust to various specifications. In Appendix D.8 we discuss the robustness of the treatment effects to (i) fully representative population weights, (ii) keeping respondents with very fast/slow survey completion times or unusual text answers, (iii) excluding all respondents who failed at least one attention check, (iv) using only one control group, (v) not controlling for observable characteristics, (vi) using different sets of control variables, (vii) using non-dichotomized outcome variables, and (viii) multiple hypothesis testing. Point estimates do not change in a noteworthy fashion to any of these checks. Standard errors, however, increase significantly under some of these procedures – specifically reweighting for representativity, dropping all attention check failures, and using only one control group. As a result, certain treatment effects that are statistically significant in the original data no longer reach this threshold under these specifications. We discuss this further in Appendix D.8.

Survey bias At the end of the survey, respondents were asked whether they considered the survey biased in an either left-wing or right-wing fashion. The large majority of respondents (72.0%) did not think the survey was biased in either direction. More respondents answered that the survey was left-wing biased (21.5%) than right-wing biased (6.5%).⁷³

⁷²Note that we did not pre-specify this interaction, but include it as it is both robust and of particular interest.

⁷³The percentage of respondents who believe the survey was left-wing biased is lower in the control groups (19.1%) than in the treatments (22.3%), but there is no statistically significant difference over treatment groups. All treatment groups are between 21% and 23%. This is shown in Table F51. All main treatment effects are robust to including a dummy for left-wing bias as a control. The corresponding statistics in the Survey 2 is 16.6% left-wing biased, 5.4% right-wing biased, and 78.1% unbiased.

5.3 Comparing the importance of externality beliefs and fairness views as determinants of redistributive preferences

In the preceding section, we showed that both fairness views and inequality externality beliefs are causal determinants of preferences for redistribution. This section characterizes the relative importance of inequality externality concerns and fairness views on redistributive preferences. We undertake this exercise because fairness views have been identified as a crucial motive behind preferences for redistribution (Almås et al., 2020; Alesina and Giuliano, 2011), thus serving as a useful benchmark. To that end, we pre-specified three different approaches.

Comparing the effects of the information treatments First, as we have already discussed, we can compare the treatment effects of our information treatments characterized by Table 2. The fairness video has about twice the effect on our pre-specified index of redistributive preferences as the full externality video.⁷⁴ As these are marginal effects that are also dependent on the efficacy of the treatment video, this is only indicative evidence for the relative strength of these arguments as a whole.⁷⁵

Predictive power of externality beliefs, political leaning, and fairness views In the second method we explore the predictive power of each type of belief on redistributive preferences. We run descriptive pre-specified regressions that include either fairness views, externality beliefs, political preferences, or “economist determinants”⁷⁶ as regressors. We then compare the explanatory power of these models using the adjusted R^2 .

Table 3 displays the results of these regressions.⁷⁷ Fairness variables have the most predictive power in this specification; demographic controls and two fairness survey questions explain 38% of the variance in respondents’ redistributive preferences. This is followed by the externality beliefs and political views, which are equally predictive at $\sim 30\%$. The “economist” regression has a relatively low predictive power of 15%, only slightly above the only-controls regression (10%).⁷⁸

We can also explore whether externality views provide any *additional* predictive power to a fairness-based model of preferences for redistribution. Model (5) indicates that it may; when including all variables into a single regression, all fairness- and externality-variables remain strong predictors of redistributive preferences with somewhat lower point estimates. This indicates that

⁷⁴We can reject equality of the two coefficients at the 5% significance level ($p = 0.012$, t-test).

⁷⁵As discussed in Section 5.2.1, the fairness treatment effect could be somewhat overestimated due to spillovers into inequality externality beliefs.

⁷⁶Respondents’ trust in government and belief that higher taxes lead to efficiency losses.

⁷⁷Column (1) characterizes a regression that only includes demographic controls; Column (2) includes our two main fairness variables, the belief that society is unfair because some get much more than they are entitled to and some get too little, and the belief that one gets rich due to luck rather than hard work; Column (3) includes our two main externality variables, the belief that unequal countries generally function worse, and the belief that inequality generally affects society in a negative way; Column (4) includes the strict political variables of whether the respondent leans Republican and whether the respondent supports Kamala Harris and Bernie Sanders (rather than Mitt Romney or Donald Trump); Column (5) includes two variables economists often consider as potential determinants for redistributive preferences, namely whether the respondent generally trusts the government to do the right thing and whether the respondent agrees that higher taxes make people work much less; Column (6) displays the results of a regression that includes all variables from regressions (1) through (5). Note that all regressions only include observations from the baseline control group.

⁷⁸Note that these results are replicated in Survey 2, see Table F31.

Table 3: Horse-Race: Predictive Power of Beliefs on Redistributive Preferences

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich because of luck		0.624*** (0.060)				0.401*** (0.057)
Society is unfair		0.620*** (0.059)				0.416*** (0.056)
Belief uneq. countr. worse			0.434*** (0.058)			0.269*** (0.050)
Neg. externality belief			0.640*** (0.058)			0.272*** (0.054)
Leans Republican				-0.429*** (0.084)		-0.245*** (0.072)
Sanders/Harris supporter				0.533*** (0.085)		0.260*** (0.075)
Trusts the government					0.436*** (0.066)	0.131** (0.054)
Taxation reduces work					-0.115* (0.061)	-0.004 (0.048)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.104	0.382	0.297	0.296	0.148	0.494
Observations	932	932	932	932	932	932

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Controls not listed include gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

while these views are to some extent correlated to each other, each still captures *independent* correlation with redistributive preferences.⁷⁹ We note that the externality questions perform much better in this exercise than the “economist” determinants; the individual’s opinion on whether taxation reduces work effort is no longer significant in the combined regression, for example.

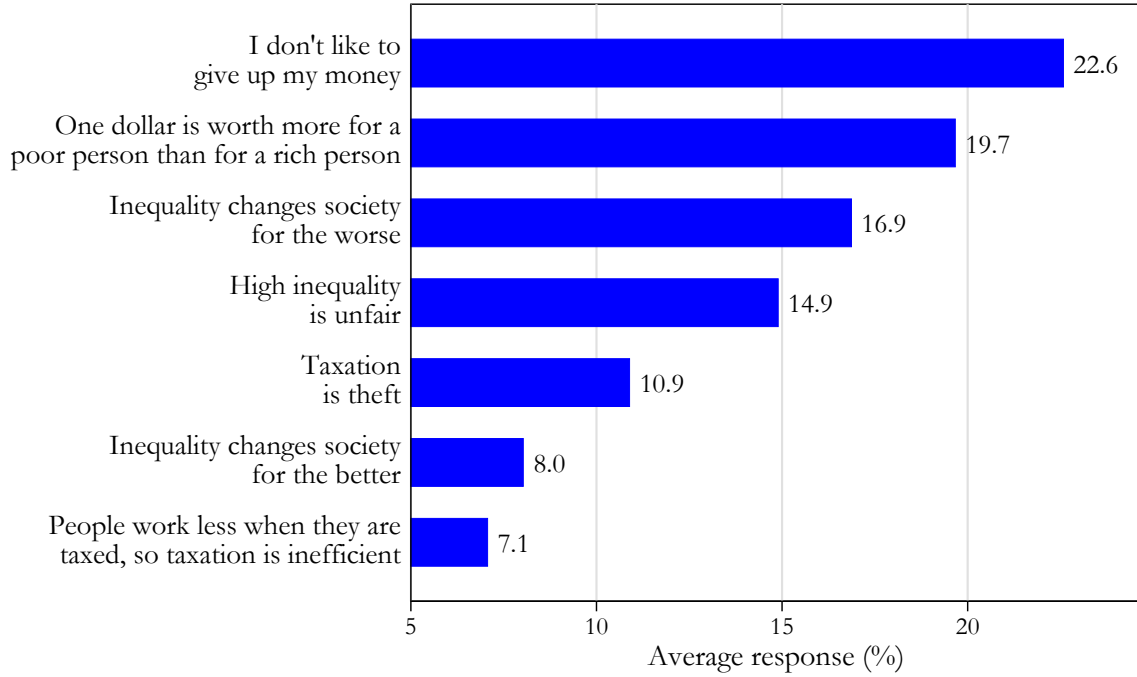
This method allows us to make two separate conclusions. First, the predictive power of inequality externality beliefs on redistributive preferences is strong; the total predictive power is only somewhat less than that of fairness views, and roughly the same as that of political affiliation. Second, externality beliefs capture information on redistributive preferences that is not included in individuals’ fairness views.

Ranking motives behind preferences for redistribution In a more direct approach, subjects were asked to allocate 100 points across different motives behind preferences for redistribution. This survey-item provides direct evidence of respondent’s redistributive motives under the assumption that they are able to discern and report these motives.

Figure 13 characterizes the mean weight put on the respective motives for the merged descriptive sample. The motive that attains the highest average support is income maximization. This is closely followed by a diminished marginal utility (DMU) argument that a dollar is worth more to the rich than to the poor. Negative externalities (“*Inequality changes society for the worse (more inequality → a worse society through various ways)*”) are the third most important

⁷⁹Similar results hold when exploring three-variable versions of the fairness and externality modules.

Figure 13: Directly Elicited Motives of Preferences for Redistribution



Question text: *When thinking about your preferred level of redistribution, what matters most to you? Please indicate what dimensions matter by giving scores below that add up to 100.* Answer option texts are identical to graph labels. Standard errors are approximately 0.6%. Sample is the merged descriptive sample ($N=3,292$). Results are very similar across surveys.

motive, attaining an average of 18 points. A broadly framed fairness motive (“*High inequality is unfair*”) actually ranks slightly behind the inequality motive ($p = 0.001$, t-test). A general aversion against taxation, positive externality concerns, and efficiency concerns attain only weak average support from our sample.⁸⁰ This last point on efficiency concerns is consistent with the findings in Table 3 and in Stantcheva (2021), among others; efficiency concerns do not seem to be strong determinants of U.S. citizens’ redistributive preferences.

What does this tell us about the relative importance of externality concerns and fairness views? First, we note that inequality externality concerns rank as one of the most important motives within our sample. This is remarkable, and cannot be explained by experimenter demand given that this is essentially the first question about inequality externalities that respondents are faced with in the survey.⁸¹ Second, negative externality concerns are similar in

⁸⁰One may argue that the presented averages just reflect idiosyncratic noise and not clear motives behind preferences for redistribution. This is, however, unlikely to be the case. Figure E11 in the Appendix shows the share of subjects that weakly rank a given motive first for the same question; the distribution strongly resembles that in Figure 13. One can also replicate Figure 13 while only including the sub-populations of subjects that rank a given motive first. This is presented in Figure E12. This decomposition shows both that subjects have consistent views – the positive externality answer is at the bottom for the negative externality group and vice versa, for example – and that respondents can be described as having one primary motive and other secondary motives.

⁸¹In Survey 1 (for which only the control group is included), respondents have been shown one pre-treatment externality question (“*Do more equal countries function worse?*”). It is the first time the concept is mentioned in Survey 2 (about 2/3 of the respondents in Figure 13). Results are very similar across samples, see Figure E10 for only Survey 2 data.

magnitude as broad but explicit fairness views. When comparing a combining positive and negative externality grouping with a combined “broad” fairness classification including both DMU and the fairness motive, general externality concerns are about three-quarters (74%) as important as fairness motives as a redistributive determinant - thus echoing the results from the two methods described above.⁸²

Motives behind partisan redistributive split One might also ask how much of the partisan split in redistributive beliefs is explained by variation in externality and fairness views respectively. To explore this question we employ a Gelbach decomposition (Gelbach, 2016). We use the decomposition to illustrate which portion of the partisan gap in the redistributive preference index goes through either the two main externality variables, the two main fairness variables, governmental trust, or efficiency concerns.⁸³ In total, 54% of Republicans’ lower support for redistribution can be explained by these variables or a list of standard controls. About half of this can be accounted for by fairness views (27% of the partisan gap). Then comes externality beliefs (12%), demographic controls (10%), and governmental trust (5%). Efficiency concerns are not a relevant factor ($\sim 0\%$). This indicates that while externality beliefs explain part of the partisan split in redistributive preferences, a larger portion is driven by fairness views. This is consistent with the stronger partisan split across fairness views than externality beliefs shown in Figure 4.

This marks the end of our empirical analysis. Before discussing potential structural differences between inequality externality beliefs and fairness views we repeat our two main results. (1) Beliefs in the negative consequences of economic inequality are widely held in the U.S. population. (2) Inequality externality beliefs are a strong determinant of redistributive preferences – on the same order of magnitude, but somewhat weaker, than broad fairness concerns, and significantly stronger than traditional efficiency concerns.

6 Discussion

Preferences for redistribution and polarization In the preceding discussion we have shown several structural differences between fairness concerns and inequality externality concerns. In this section we discuss potential implications of these differences.

Two immediate differences between these types of arguments are who is *at fault* and who is *affected*. Consider an argument for redistribution based on the unfairness of the income distribution. Whenever one person deserves more of an existing pie, another who already has resources must be deserving of less. Such arguments can be more or less explicit; arguing that the poor deserve more is different from arguing that the rich are rent-seeking, which is again different from assigning blame for the perceived unfair system. However, any fairness-based argument maintains a necessarily oppositional aspect. Somewhat stylized, there must always be a *victim* or a *villain*; often there are both.

⁸²If including “Taxation is theft” as a fairness motive, this falls to 60%.

⁸³The variables are the same as in Table 3.

Inequality externality concerns are fundamentally different. First, the “villain” is often a common enemy, namely *economic inequality itself*. Inequality externality arguments are, broadly speaking, about the unintended consequences of economic inequality. These unintended consequences do not have to be the fault of any particular individual, nor do they require “winners and losers” in a traditional sense (though they can have them). If higher inequality leads to less trust, for example, it is undoubtedly unfortunate – but it is difficult to argue that any one portion of society has sole responsibility for such a development. The villain in inequality externality arguments, as far as there is one, is often inequality itself, and the victim is often all of us. As an example, note that arguing that the economic system is unfair can be perceived to discredit those with high incomes. Arguing that high-inequality societies function worse, on the other hand, is not particularly targeted at anyone.

The importance of this distinction can be summarized in two main points. First, fairness arguments could be more polarizing than externality arguments, creating divisions and fostering anger in a way that externality arguments avoid. Second, these two types of arguments could have different target audiences, with fairness arguments being more effective near the bottom of the income distribution and externality arguments being more broadly applicable.

Our survey results underline these ideas. Some relatively simple information about the evolution of wages and productivity made one out of every eight people report anger, for example – significantly higher than for any of the externality treatments, even those discussing homicides.⁸⁴ This indicates the high potential for affective polarization in fairness arguments. Heterogeneous treatment effects in income for the fairness group also indicate that the externality argument may be more universal. While the fairness treatment was significantly less effective on respondents who earn more than \$50,000 a year, the effect of the full externality treatment was similar across the income distribution – if anything increasing at top incomes.⁸⁵

As we discussed in Section 4.2.3, fairness views are also more polarized across income, wealth and party affiliation than externality views. Fairness views, such as the view that the economic system is unfair, are significantly more common among low-income and low-wealth individuals, both in our sample and in other surveys (Valero, 2021). Externality views, on the other hand, have either weak or non-existent correlations with income and wealth. This is particularly notable given that inequality externalities present a theoretical incentive for self-interested individuals with high incomes to care about economic inequality. A similar pattern is true for party affiliation; the heterogeneity between Democrats, Independents, and Republicans is much stronger in fairness views than for externality beliefs. Overall, fairness views are polarized across various dimensions whereas externality views are relatively similar across the population.

In sum, our results indicate that inequality externality beliefs may be structurally distinct from existing redistributive motives in ways that could affect cross-country political landscapes and economic outcomes.

Welfare-theoretical ramifications Welfare theory is often based on individualist utility functions, which usually assumes no relevant externalities. Even if some externalities do exist,

⁸⁴The equivalent numbers for the three externality treatments is one in 13, 16 and 34 respondents.

⁸⁵This is in line with theoretical analysis and correlational evidence by Rueda and Stegmueller (2016) who study the relationship between preferences for redistribution and (fear of) crime.

they can usually be ignored either because they are not of a macroeconomic scale or not explicitly economic in nature (and potentially based on concepts deemed philosophically challenging in a welfare setting, such as altruism or jealousy). If economic inequality has externalities, however, it presents difficulties to this framework that most other externalities do not. It is a resource-based externality which is influenced by any individual's resources that also exists on a macroeconomic scale. It also does not rely on feelings or other arguments that can be disregarded on philosophical grounds.⁸⁶ Beyond rare exceptions (Thurow, 1971; Alesina and Giuliano, 2011; Støstad and Cowell, 2021) the idea that inequality could be an externality has been largely ignored in economic theory, likely because causal evidence of inequality's societal effects is empirically difficult to produce.

This paper shows that the vast majority of individuals believe that economic inequality has such externalities and that they believe these externalities are of a significant magnitude. Following this, inequality externality ideas present a novel reason for why governments differ in policy choices. Consider two governments whose subjects have equal (or even non-existent) aversions to economic inequality for fairness reasons. Differing inequality externality beliefs among these populations could lead these two governments to widely different policy goals; one might stridently reduce economic inequality while the other intentionally increases it, for example. This could be a large driver of policy differences between countries and cultures. In terms of economic theory, the social planners' inequality externality belief could have a large impact on optimal policy. In general, our results indicate that the ramifications of treating economic inequality as an externality in welfarist models might require further study.

7 Conclusion

This paper marks the first positive analysis of individuals' inequality externality beliefs as a determinant for redistributive preferences. Using two representative surveys of a total of 6,731 U.S. citizens we find that individuals believe inequality affects society through various ways, and that individuals largely believe that inequality has *negative* rather than *positive* effects on society. A large majority of individuals believe economic inequality increases crime (74%), decreases trust (67%), decreases economic growth (51%), for example. In collecting these and other data points, this paper has thus created the first extensive data set of inequality externality beliefs in any country.

The paper has also shown that inequality externality beliefs are a strong determinant for redistributive preferences, using an exogenously provided information treatment to establish causality. Three separate methods indicate that externality beliefs are on the same order of magnitude and somewhat weaker than broad fairness views in determining redistributive preferences. We also find indicative evidence that inequality externality beliefs are stronger determinants of redistributive preferences than traditional efficiency concerns about the distortive effects of taxation. As such, this paper presents the first strong evidence that individuals' beliefs about the *consequences of inequality* are impactful for their redistributive preferences.

The work further discussed the main differences between fairness arguments and inequality

⁸⁶The main other externality that meet all these requirements is global climate change.

externality arguments. While fairness arguments necessarily create opposition – by pointing out who does or does not deserve their incomes, for example – externality arguments focus on a shared enemy of inequality’s unintended consequences. As such, fairness arguments are more prone to polarization and have a more variable efficacy across the income distribution than externality arguments. Overall, we argue that these two types of arguments have significant structural differences.

Finally, these results have a broader dimension of academic and policymaking value. When economic inequality is an externality, the core problem of economics becomes not just to maximize resources efficiently but also to find the correct trade-off between more resources and less inequality. This presents serious trade-offs that are arguably more complex than those posed in the existing literature around redistributive preferences. We hope future work will explore these issues further.

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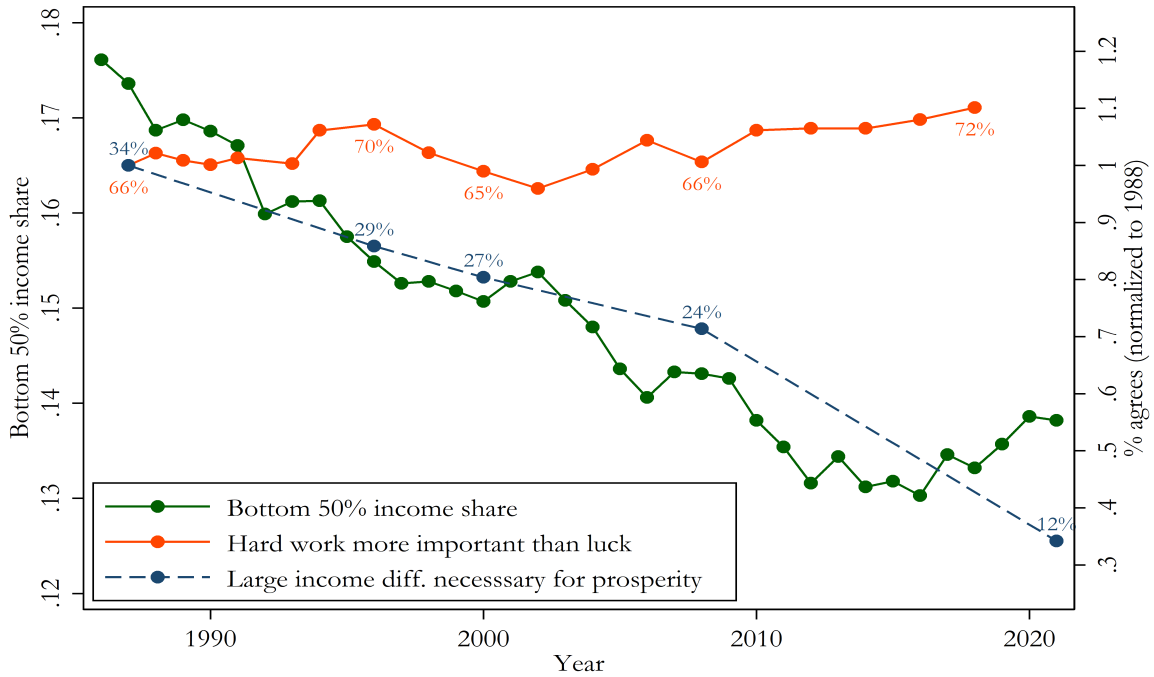
Appendix

A Prior questions about inequality externality beliefs

As far as we know there are two prior questions in the United States on individuals' beliefs about inequality's externality effects. The first is a question in the General Social Survey asking respondents if they agree with the statement that “*large income differences are necessary for America's prosperity*”. We show the trend of this question in Figure 14, overlaid with the bottom 50% income share and a measure of broad economic fairness views in the share of individuals who agree that “*hard work is more important than luck*”.

Among larger representative surveys, the International Social Justice Project has asked individuals whether they agree that “*There is an incentive for individual effort only if differences in income are large enough*”. This question was also asked in the United States. In Table 4 we show the results from this question in their 1991 and 1996 waves (Wegener et al., 2010) across different countries.

Figure 14: U.S. Inequality Externality Beliefs over Time.



Note. Data from the General Social Survey and the World Inequality Database (wid.world)

Outside of Europe and North America there is scarce evidence on the topic. Surveys conducted in China by Whyte (2010) is the sole exception we know of, with three questions on the topic with the following results;

- 51% of respondents agree that “*income gaps threaten stability*”,
- 20% of respondents agree that “*income gaps aid national wealth*”, and
- 50% of respondents agree that “*income gaps foster hard work*”.

Table 4: Inequality Externality Beliefs from the ISJP (1991-1996)

	Bulgaria	E. Ger	W. Ger	Hungary	Japan	N.lands	Poland	G.B.	U..	Russia	Slovenia	Cz. Rep.	Estonia	Slovakia
Does not agree	43%	38%	31%	68%	48%	47%	27%	34%	37%	38%	36%	35%	49%	38%
Agree	57%	62%	69%	32%	52%	53%	73%	66%	63%	62%	64%	65%	51%	62%
Respondents	2,628	2,063	1,787	1,837	708	2,532	1,389	1,246	1,370	2,816	1,294	1,987	1,930	345

Note. Percentage who agree with the statement that “*There is an incentive for individual effort only if differences in income are large enough*”. Data from (Wegener et al., 2010). Data was collected from Bulgaria, E. Germany, W. Germany, Hungary, the Netherlands, Russia, the Czech Republic, and Estonia in both 1991 and 1996. For these countries, data was merged across both years. Options were “Strongly agree”, “Somewhat agree”, “Neither agree nor disagree”, “Somewhat disagree”, and “Strongly disagree”. The first two are merged into the column “Agree” for this table. Respondents who answered “Don’t know” were excluded (on average 6% of respondents).

B Inequality as an Externality: Theory

B.1 The existence of inequality externalities

Section 2 asserts two points. First, that each inequality externality channel can be caused by several different mechanisms. Second, that these channels are potentially simple and can be micro-founded with few assumptions.

To establish the first point we will use the existing literature on economic inequality’s impact on crime. Following Kelly (2000), three main theories of how economic inequality increases crime can be sketched:

1. *The economic theory of crime* poses that individuals rationally optimize their resources, allocating time between market labor and criminal activity. Higher economic inequality leads to a higher relative return to crime for the majority of the population. Thus individuals substitute into criminal activity when inequality increases.
2. *The strain theory of crime* poses that individuals who struggle in more unequal economic systems are increasingly frustrated by what they see as their relative (and potentially unjust) failure compared to those around them. This causes stress, alienation, and finally leads at least some individuals to criminal activity.
3. *The social disorganization theory of crime* posits that inequality could decrease family and institutional stability, increase relative poverty, and weaken social networks. If so, there could be both more opportunities for and less risk from criminal activity – thus increasing the amount of crime.

One could also find causal channels for why economic inequality *decreases* crime; suppose that higher economic inequality leads to more segregation or stricter policing, for instance, which leads to less criminal opportunity.

The above is a very brief overview of the nuanced and varied hypotheses that underpin one potential inequality externality, that of crime. We believe this illustrates both the complexity of such theories and why it is infeasible to discuss the mechanisms behind each causal channel in detail.

The second assertion from the main text is that inequality externalities can be relatively simple to micro-found and can be mechanical in nature. Mechanical in this context means that they

do not require other-regarding preferences, emotional reactions, or changes in perceived inequality to exist. We illustrate this through the example of political polarization taken from Støstad and Cowell (2021). Begin with assuming that political opinions O_i are an increasing function of individual income x_i and no other factors (for simplicity). Political polarization is formalized as $\bar{P} = \varphi(\mathbf{O})$ and is an increasing function of a distributional metric φ of all opinions in the population \mathbf{O} . Polarization \bar{P} increases as well. This generally increases \bar{P} and thus affects $U_i(\dots)$. It follows that inequality causes more pronounced political polarization and thus also affects individual utility.

For further micro-foundations on inequality externalities we refer to Støstad and Cowell (2021). Several of the inequality externalities we explore in this work, including crime, trust, political polarization, innovation, and economic growth, are micro-founded there.

B.2 Inequality externalities and redistribution in optimal taxation

The following is a mildly modified version of the optimal tax problem found in Støstad and Cowell (2021).

We suppose that the inequality metric θ can be written as

$$\bar{\theta}(z, H) = \int_{\underline{x}}^{\bar{x}} \kappa(z) x(z) dH(z), \quad (3)$$

where $\kappa(z)$ is the weight of the agent in the inequality metric and the cumulative distribution of all pre-tax incomes z is $H(z)$. The inequality weight $\kappa(z)$ is non-decreasing, continuous, positive near the top of the income distribution and negative near the bottom, and otherwise general. For example, the (absolute) Gini coefficient in post-tax income has a weight $\kappa_G(z) = 2H(z) - 1$.

The social planner sets an income tax $T(z)$ dependent on pre-tax incomes z such that $x_i = z_i - T(z_i)$. This is done through finding the tax schedule $T(z)$ from which no given small perturbation ϵ which changes the tax schedule as $T(z) + \epsilon \Delta T(z)$ leads to welfare improvements. We denote the resulting change in the inequality metric from the small tax increase by $\Delta \bar{\theta}$. The local optimal tax criterion is thus defined as the tax schedule $T(z)$ for which any small budget neutral tax reform in direction $\Delta T(z)$ has $\int_i g_i [\Delta T(z_i) + \eta_i \Delta \bar{\theta}] di = 0$, where g_i is the SWW of individual i .

We thus introduce a small tax reform $dT(z)$ where the marginal income tax is increased by $\partial \tau$ in a small band from z to $z + dz$. The reform mechanically increases average tax rates on everyone above this band. This is the mechanical effect of taxation, and collects $dz \partial \tau$ from $1 - H(z)$ agents above z under our utility function (which has no income effects). Thus it collects $[1 - H(z)] dz \partial \tau$ revenue. For each $dz \partial \tau$ collected, however, inequality also changes. The magnitude of this change per agent above differs based on which agent is considered. Noting that income rank $\kappa(z)$ does not change given that second-order conditions hold, each decrease in one unit of post-tax income at z changes absolute post-tax income inequality by $\kappa(z)h(z)$. The mechanical effect thus has a differing equality effect of $-\kappa(z_j)h(z_j)dz \partial \tau$ at each point j above z , where z_j is the income of the agent and $h(z_j)$ is the number of agents at this point, and $\kappa(z_j)$ is that agent's weight in the inequality metric. As the income change of each agent above z is equal,

we can define the average inequality weight above as $\bar{\kappa}(z) [1 - H(z)] = \int_{\{j: z_j > z\}} \kappa(z) h(z) dj$ and write that the mechanical effect changes income inequality by $d\bar{\theta}_M = -\bar{\kappa}(z) [1 - H(z)] dz \partial \tau$.⁸⁷

Those who are located in the small band between z to $z + dz$ have a behavioral response to the tax change. They work less, and reduce their pre-tax earnings by an amount $\partial z = -\epsilon(z) z \partial \tau / (1 - \tau(z))$. $\epsilon(z)$ is the elasticity of earnings z with respect to $1 - \tau(z)$. There are $h(z) dz$ individuals in the tax bracket who were taxed at $\tau(z)$ before the perturbation, so total revenue decreases by $-dz \partial \tau \cdot \epsilon(z) z h(z) \tau(z) / (1 - \tau(z))$. This change in total earnings is moderated by an effect $(1 - \tau) / \tau$ for the inequality effect, as we are interested in the post-tax income decrease and not the tax revenue decrease.⁸⁸ Additionally we must multiply by the agents' weight in the inequality metric $\kappa(z)$. The behavioral response thus has an effect on the post-tax income inequality metric as $d\bar{\theta}_B = -\kappa(z) \cdot dz \partial \tau \cdot \epsilon(z) z h(z)$.

The total revenue effects are:

$$dR = dz \partial \tau (1 - H(z) - \epsilon(z) z h(z) \tau(z) / (1 - \tau(z)))$$

The direct welfare effect through the individual income channels is $\int_j g_j dR dj$ for $z_j \leq z$ and $-\int_j g_j (\partial \tau dz - dR) dj$ for $z_j > z$. Thus the net individual income-based welfare effect is $dM + dB + dW = dR \cdot \int_j g_j dj - dz \partial \tau \int_{\{j: z_j \geq z\}} g_j dj$.

The total equality effect is $\partial \bar{\theta} = d\bar{\theta}_M + d\bar{\theta}_B$:

$$\partial \bar{\theta} = dz \partial \tau (-\bar{\kappa}(z) [1 - H(z)] - \kappa(z) \epsilon(z) z h(z)) \quad (4)$$

In terms of welfare, the effect is $\int_j g_j \frac{\partial U}{\partial \bar{\theta}} d\bar{\theta} dj$. We have that $\eta_i = MRS_{x_i, \bar{\theta}} = -\frac{\partial U / \partial \bar{\theta}}{\partial U / \partial x_i}$, and thus the total welfare effect of the inequality change is $dI = \int_j (-g_j \eta_j) \partial \bar{\theta} \cdot dj = -\partial \bar{\theta} \int_j \eta_j g_j dj$.

The total welfare change, including all channels, is equal to zero at the optimum:

$$dM + dB + dW + dI = 0 \quad (5)$$

Note that in the main text we denote $dI = dI_B + dI_M$ where dI_B and dI_M correspond to the welfare-weighted versions of $d\bar{\theta}_B$ and $d\bar{\theta}_M$ respectively. Thus, using the expressions for dR and dI , and the expression $\bar{G}(z) (1 - H(z)) = \int_{\{j: z_j \geq z\}} g_j dj / \int_j g_j dj$, we have:

$$\begin{aligned} dz \partial \tau \int_j g_j dj \left[1 - H(z) - h(z) \epsilon(z) z \frac{\tau(z)}{1 - \tau(z)} \right] - dz \partial \tau \bar{G}(z) (1 - H(z)) \int_j g_j dj \\ + \int_j \eta_j g_j dj \cdot [dz \partial \tau (\bar{\kappa}(z) [1 - H(z)] + \kappa(z) \epsilon(z) z h(z))] = 0 \end{aligned}$$

Dividing by $z h(z) \epsilon(z) \int_j g_j dj \cdot dz \partial \tau$ and re-arranging, we find:

$$\frac{\tau(z)}{1 - \tau(z)} = \eta \cdot \kappa(z) + \frac{1 - H(z)}{z \cdot h(z)} \frac{(1 - \bar{G}(z) + \eta \bar{\kappa}(z))}{\epsilon(z)}, \quad (6)$$

where we have used the weighted average of the externality $\eta = \int_i g_i \eta_i di / \int_i g_i di$. By using the local Pareto parameter $\alpha(z) = \frac{z \cdot h(z)}{1 - H(z)}$ and assuming that the social value of one dollar at

⁸⁷In the absolute Gini, $\bar{\kappa}(z) = H(z)$.

⁸⁸For the mechanical effect, the tax revenue increase and the individual post-tax income decreases are identical.

the top is zero (due to the diminishing marginal utility of income) we find the optimal marginal income tax rates as specified in (1).

C Survey Details

C.1 Survey 1

The survey flow is shown in Figure 9. The survey was divided into three main parts:

1. A demographic section, asking standard questions on gender, age, party affiliation and so on.
2. A video treatment section. This begins with our pre-treatment fairness and externality questions, then sends the respondents into one of six randomly assigned groups. Four of these are shown 1-2 minute treatment videos on externality concepts (3 videos) or fairness concepts (1 video). One group is shown a video on how academics measure inequality using the top 10% income share and the Gini index (active control video). One group does not receive any stimulus (passive control group). Each video group sees “filler questions” after the video.
3. A section including additional demographic questions, redistributive question questions (main treatment outcomes), and further descriptive questions (including first-stage outcomes).

The full Survey 1 questionnaire can be found [here](#).

C.1.1 Data quality and attrition

11,540 respondents land on our consent page. 10,992 respondents consented to taking the survey, and 8,551 pass the required U.S. citizen screening test. Of these, a total of 5,007 respondents (58.6% of initial U.S. respondents) finished the survey. Simple data quality checks remove 7.64% of these respondents from the sample during the demographic section.⁸⁹

Further, 33.9% of respondents end the survey on their own before completion; we will now briefly discuss these respondents. 7.9% of respondents drop out before the information treatment. 17.9% of respondents drop out during the information treatment.⁹⁰ While the passive control group had less attrition at this stage – as there is no video treatment and no required

⁸⁹Respondents are required to pass two of three simple attention checks to continue past the demographic section. All attention check removals happened before any topic-specific questions. These attention checks are very simple and designed to sieve out individuals that do not read the questions at all. We believe relatively rigorous but simple attention check requirements are necessary to optimize the signal from online panel surveys, which are prone to inattentive and non-human respondents. Individuals who failed one attention check but still finished the survey add weight to this argument. These respondents exhibit generally lower correlations across similar questions (e.g. similar externality questions or similar fairness questions) than respondents who did not fail any attention checks. Still, our overall results are similar whether including or excluding these individuals from the final sample. We discuss this for the treatment effect in D.8 and show it for the main descriptive results in Figures E3-E4.

⁹⁰This attrition during the treatment is most likely due to either (i) technical issues from the Youtube video, for example accidentally exiting the survey screen, or (ii) inability or unwillingness to correctly answer the simple factual questions that we require respondents to correctly answer before continuing. We are unable to disentangle these two effects.

questions for these respondents, see Section 5.1.2 – the differences in attrition between the active control and the remaining treatments are small.⁹¹ The remaining 8.2% who dropped out did so after the treatment.

This leaves the 5,007 respondents who finished the survey. We pre-specified to drop the fastest 5% of the fastest respondents at this stage, as is often done in the literature and by survey companies (see e.g. Bellani et al., 2021).⁹² We also exclude subjects that spend less time on the screen with the video treatment than the duration of the video, as well as those who were in treatment groups that nonetheless self-reported that they did not watch a video. We also added two extra data quality checks that were not pre-specified. First, we deleted 237 respondents that dropped out of the survey in the middle and then retook the survey, which we identify due to identical IP-addresses. Second, we drop 109 subjects that were flagged due to providing “nonsense” answers to text-based questions (e.g. spam, vulgar phrases or the same non-topical copy-pasted text to all answers). Our results are not fragile to these two steps, which were taken to improve overall data quality.

Overall, this leaves a final sample of 4,371 respondents.

C.2 Survey 2

The full Survey 2 questionnaire can be found [here](#).

We conducted Survey 2 as a secondary “robustness” survey with Dynata to ensure the validity of our original results. The survey was conducted between August 7th and October 8th 2022. The main structure of the Survey 2 was a simple questionnaire, where towards the end of the survey respondents were funnelled into one of eight channels on a specific inequality externality.

The first part of the survey asked similar externality-based questions as to those in Survey 1 changed in various ways to explore the robustness of our initial results. We explain the concept of inequality in-depth to respondents, for example, and substitute any mention of “inequality” for “equality” or “differences in income and wealth” for two-fifths of respondents (one-fifth for each). We also asked respondents a simple question to gauge their understanding of inequality itself and explicitly specify our definition of “more inequality” (i.e. we set a reference level of inequality for the externality questions) for the entire survey.

In the latter part of the survey each individual was funneled into a channel focusing on one specific inequality externality. These externality-specific questions included re-asking a specific externality question to check for consistency, allowing individuals more time to ensure a high-quality answer, asking respondents to explain their answer with an open-ended text question, asking whether top- or bottom-based inequalities matter more for the externality, several questions designed to find out whether the reference level changes the direction of the individuals’ externality beliefs, and a question which explores whether average income or income

⁹¹Individuals who dropped out during the treatment are not significantly more likely to be Democratically leaning or have prior externality beliefs across video groups, for example. Tables F4-F10 shows that demographic controls and pre-treatment questions are balanced across treatment groups, further indicating that any selective attrition is limited. We also control for pre-treatment beliefs and demographic controls in our main treatment results.

⁹²Since different treatment groups watch different videos, we drop the 5% fastest subjects within each treatment group.

inequality is deemed a larger predictor of the outcome in question. The eight externality channels we elicit for in this study are crime, trust, economic growth, innovation, political polarization, corruption, the quality of democratic institutions, and social unrest.

C.2.1 Data quality and attrition

The sampling methodology in Survey 2 is similar to that of Survey 1. We use a similar attention check procedure to ensure high-quality responses. Respondents who fail either the U.S. citizen screening at the beginning of the survey or at least two later attention checks were removed from the survey. As before, these attention checks are very simple and designed to sieve out individuals that do not read the questions at all. Unlike Survey 1, we included two attention checks in the middle of the descriptive data collection to further ensure data quality.⁹³

6,980 respondents landed on our survey consent page. A total of 6,471 respondents consented to taking the survey, and 5,474 move past the U.S. citizen screening. A total of 2,479 respondents (45.3% of initial U.S. respondents) finish the survey. The data quality checks remove 30.9% of respondents from the sample. A further 23.9% of respondents end the survey on their own before completion. These numbers are likely inflated due to the repetitive nature of the survey. After dropping the fastest 5% of respondents per survey arm the final sample is 2,360 respondents.

C.3 Representativity

Table C1 displays the observable characteristics of our two samples. Both surveys explicitly targeted representative quotas for gender, age, political affiliation, and geographical region.

To elicit political preferences, we used the same question that is used by Gallup to monitor political preferences in America.⁹⁴ All three final distributions mirror the November 2021 Gallup poll quite closely (31% Republican, 27% Democrat, 41% Independent), each marginally undersampling Independents.⁹⁵ Figures C1-C2 show that political affiliation is representative across the 50 U.S. states, although this was not explicitly targeted. On the other dimensions we targeted, Survey 1 is completely balanced on gender and census region, and matches the age-group distribution of the overall population well. Survey 2 slightly oversamples men, older individuals, and those who live in the Census region *West*.⁹⁶

⁹³These questions were in the same format as other externality questions and asked respondents to answer a specific choice option. We added these questions to ensure that our descriptive statistics had the least amount of noise possible. The question text of the first question;

This question is about the same increase in inequality. Here we just want you to click the answer option at the top. In other words, how do you think more inequality – could you please click the first answer option? Note: Here we just want you to choose the top option to show that you are reading the questions. Thank you.

⁹⁴“In politics, as of today, do you consider yourself a Republican, a Democrat or an independent?”

⁹⁵This poll was the most recent poll when the first survey was conducted. Note that there is significant fluctuation in this distribution on a month-to-month basis (c.f. <https://news.gallup.com/poll/15370/party-affiliation.aspx>). The year-long average is 27% Republican, 30% Democrat, and 43% Independent.

⁹⁶These discrepancies as well as the undersampling of non-white respondents in Survey 2 come from a technical quota error on the survey providers’ part. In short, the survey provider accidentally increased the sample size from 1,700 to 2,360 but did not keep quotas in mind for these extra respondents. This made the total sample from Survey 2 somewhat less representative, as the additional respondents were not subject to the designed quotas. We decided to keep the larger sample as it is balanced on political affiliation, and the over-sampled observables (male and white respondents, specifically) generally do not have large effects on our outcome variables. Our results are robust to re-weighting for full representativity.

Table C1: Survey demographics compared to the 2021 U.S. adult population

	2021 U.S. share	Survey 1	Survey 2	Merged sample
Republican	31%	32%	33%	32%
Democrat	27%	30%	28%	29%
Independent	41%	39%	39%	39%
Male	49%	50%	54%	53%
Female	51%	50%	45%	46%
White	64%	77%	75%	76%
Black	12%	9%	8%	8%
Neither black nor white	24%	14%	17%	16%
Income: 0-25k	18%	22%	18%	19%
Income: 25-50k	20%	29%	23%	25%
Income: 50-100k	29%	30%	33%	32%
Income: 100k and more	33%	19%	26%	25%
Age 18-29	18%	14%	11%	12%
Age 30-39	17%	17%	16%	17%
Age 40-49	16%	17%	17%	17%
Age 50-59	16%	14%	16%	16%
Age 60-69	17%	17%	22%	21%
Age 70 and above	17%	21%	17%	19%
4-year college degree or more	36%	50%	58%	55%
Employed	59%	47%	51%	50%
Unemployed	4%	9%	7%	8%
Outside the labor force	38%	43%	42%	43%
South	38%	38%	30%	32%
West	24%	24%	32%	30%
North-East	17%	16%	16%	16%
Midwest	21%	21%	22%	22%
Respondents		4371	2360	3922

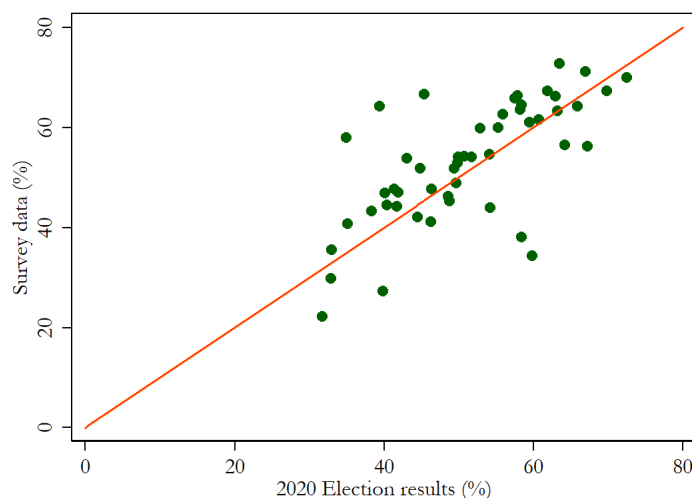
Note. This table represents respondent demographics of Survey 1 ($N=4,371$), Survey 2 ($N=2,360$), and the merged descriptive sample (the control group of Survey 1 and all of Survey 2) compared to the share among 2021 U.S. adults for the respective characteristic. Data on the U.S. population is from the U.S. Census Bureau, the U.S. Bureau of Labor Statistics, Gallup,

Though we did not explicitly target these dimensions, we are also interested in having diverse socio-economic representation. We have significant variation in household incomes, and particularly Survey 2 approximates the U.S. income distribution well. Our surveys are less representative on racial dimensions, as they oversample white Americans. Hispanics and Latinos are particularly underrepresented in our study (16.8% in the overall population versus 7.0% in our pooled sample). Similar to other studies using similar access-panels, our samples are more educated than the average American, as roughly half of the respondents have at least a college degree versus 36% in the overall population.

C.4 Eliciting externality beliefs

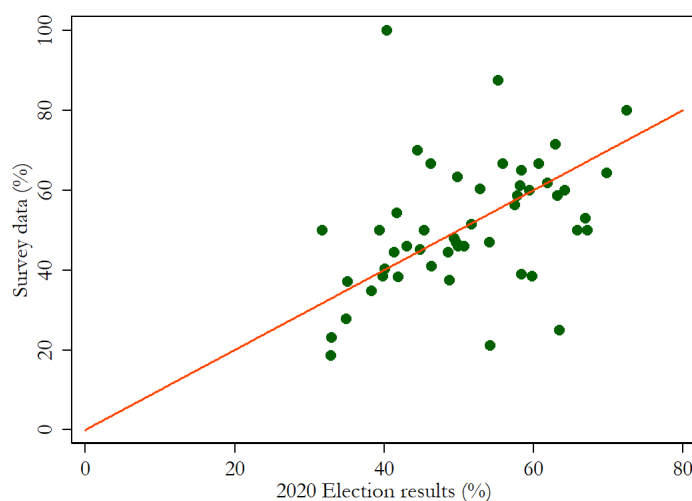
We elicit externality beliefs using various methods. Questions on individuals' *general* and *specific* inequality externality beliefs are asked in both Survey 1 and Survey 2. These beliefs are explored

Figure C1: Relationship between state-level political leaning in the survey and 2020 state-level election outcomes in Survey 1



Note. This figure plots state-level shares of respondents stating that they lean towards the Republican party in Survey 1 against the state-level share of votes going to the Republican party. Washington D.C. is included (the left-most data point). The diagonal line characterizes the points where both would coincide. 43 of 50 data points from the survey are within 1.96 standard errors to the 2020 election result. In making this comparison, note that we collect responses two years after the election and do not screen on likely voters.

Figure C2: Relationship between state-level political leaning in the survey and 2020 state-level election outcomes in Survey 2



Note. This figure plots state-level shares of respondents stating that they lean towards the Republican party in Survey 2 against the state-level share of votes going to the Republican party. Washington D.C. is included (the left-most data point). The diagonal line characterizes the points where both would coincide. 47 of 50 data points from the survey are within 1.96 standard errors to the 2020 election result. In making this comparison, note that we collect responses two years after the election and do not screen on likely voters. Note also that the sample size is significantly lower in this survey ($N = 1873$), and that some data points have very few associated respondents. Delaware, the data point at 100% in survey share, has only two respondents.

in closed-form multiple choice questions and open-ended text questions. Most questions are asked to all or a majority of respondents within a survey. The main exception is in Survey 2, which also funnels respondents into eight different survey strands. Each survey strand poses detailed questions on one specific externality channel.

In eliciting externality beliefs we took extensive measures to avoid biases arising from respondents misunderstanding the question, anchoring, or phrasing. In general, questions are always designed to be unbiased and symmetric around a neutral answer option. The order of multiple choice answers was randomly flipped on the question level to avoid anchoring bias whenever possible. We also varied the phrasing respondents face on a question-by-question basis in Survey 1 and throughout the survey in Survey 2. The standard phrasing in both surveys uses the word “inequality”. In Survey 1, respondents instead saw the phrasing “differences in income and wealth” in one-third of the specific externality questions. In Survey 2, 20% of subjects received identical questions but with “inequality” changed to “differences in income and wealth.” An additional 20% of subjects had “inequality” changed to “equality”.⁹⁷ Survey 2 also rigorously defines the chosen distributional concept (e.g. inequality) to respondents, including a small quiz which allows us to check respondent comprehension of distributional concepts.⁹⁸ Survey 2 also ensured that respondents answer accurately by asking them to confirm their previous choice; almost all respondents ($> 97\%$) confirm their choice.⁹⁹ We discuss these and other robustness checks in Section 4.2.1 and Appendix D.1.

C.5 Designing the general externality question

We ask somewhat different questions to elicit general externality beliefs in each survey.

Due to the complex nature of the views we wish to elicit, the two questions of *does inequality affect society* and *is this effect positive or negative* were asked separately in both surveys. The order between these two questions was swapped between surveys to minimize any potential effect of noise or phrasing. In Survey 1, respondents were first asked to choose between 5 options, ranging from “A lot to the better” over “Neither / no change” to “A lot for the worse.” If subjects chose “Neither / no change”, they were asked a follow-up question to find out whether they chose this option because they believe that inequality has “no effect on society” or because they think the “good and bad effects cancel each other out”.¹⁰⁰ In Survey 2, this order was swapped; respondents were immediately asked a “Yes”/“No” question about whether inequality affected society. If subjects chose “Yes, economic inequality affects society”, they were then asked whether the changes would be overall positive or negative.

The way these questions were phrased also differed across surveys. The main Survey 1 question is short, designed to be easily understood, and reads “Generally speaking, do you think **more economic inequality** changes society **for the better** or **for the worse**?”. This brevity comes at the cost of imprecision; in Survey 2, the question is much longer and clarifies any unclear points explicitly (see Appendix C.5.1 for the question in verbatim).¹⁰¹

⁹⁷Many of our questions ask how “more inequality” changes society; these respondents are instead asked how “more equality” changes society.

⁹⁸The share of individuals who believe in a given negative externality is often somewhat stronger (5 – 10%) among those who answer this quiz correctly.

⁹⁹We ask subjects to confirm both their general and specific externality beliefs. These questions allow respondents to state that they either agree with their choice, that they disagree with it and want to change it, or that they answered randomly.

¹⁰⁰They were also able to answer “I don’t know”; the 12 respondents who answered this were pooled with the “Good

¹⁰¹Specifically (i) the initial inequality level, shown through diagrams and words to be roughly at a Scandinavian level (without explicitly naming countries), (ii) the level of the change in inequality, which is an inequality increase

We show the separate results for each survey in Table C2.

Table C2: General Externality Beliefs: How Does More Economic Inequality Change Society?

	Survey 1	Survey 2
A lot for the better	4.1%	3.1%
Somewhat for the better	10.9%	7.6%
Good and bad effects cancel	20.8%	10.3%
Somewhat for the worse	34.8%	36.7%
A lot for the worse	24.8%	26.4%
Inequality does not affect society	4.5%	15.9%
Total respondents	919	2360

Note. Survey 1 question text: “Generally speaking, do you think *more economic inequality* changes society *for the better or for the worse?*” For Survey 1, only data from the control group is shown. For Survey 2, the table contains data from all respondents (i.e. “inequality” phrasing, “differences” phrasing, and “equality” phrasing). See Appendix C.5.1 for the full question text.

C.5.1 Survey 2 General externality belief question (Part 1)

Note: This question comes directly after a question which introduces the distributional concept (inequality, equality, etc.) and shows the income distributions shown below. Randomized phrasing is shown in brackets.

This question is about how **you think economic [inequality/equality/differences] changes society.**

Below we are showing you the same two income distributions as earlier. The correct answer was that society (B) [is more unequal/ is more equal¹⁰²/has more economic differences].

[Insert figure]

Here’s some more information: **Society A** has a large middle class and few with relatively small or large incomes. The richest tenth of society earns 5 times as much as the poorest tenth of society.

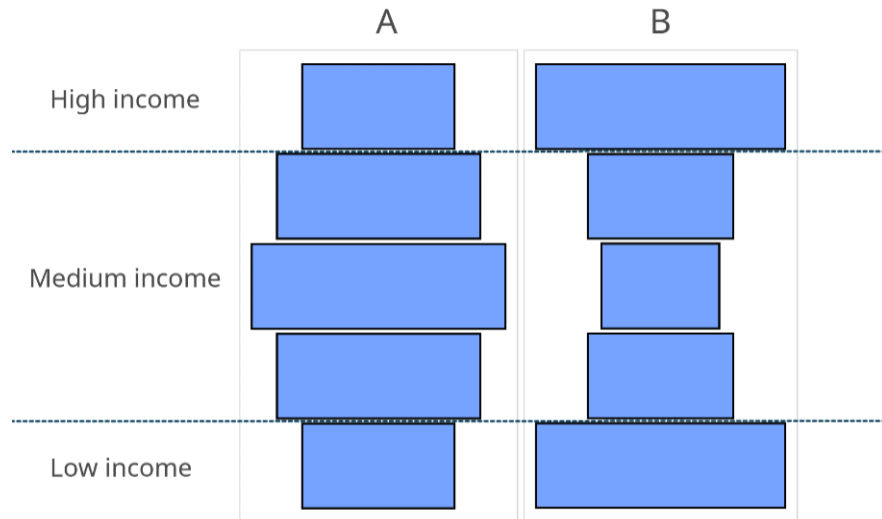
Society B has a small middle class and many with relatively small or large incomes. The richest tenth of society earns 30 times as much as the poorest tenth of society.

There is a low amount of extreme poverty in both countries.

Now imagine that the income distribution in a society moves from (A) to (B). In other words, the society becomes **[more economically unequal / more economically equal / has larger economic differences]**. The change is because of something outside the society, such as technological change in another country.

to roughly the level of the United States, (iii) the exogenous nature of the inequality shock, (iv) explicitly noting that we are interested in changes in factors of society, using examples such as crime and economic growth, (v) explicitly noting that the question is not about individual income or fairness concerns.

¹⁰²Order of the distributions is switched for the equality-phrasing



One could imagine that this either **changes** or **does not change factors in society** - such as economic growth, crime, general trust, innovation, the quality of democratic institutions, and so on. Note that this question is not about whether you think the new distribution is more or less unfair, or about the direct changes in individuals' economic situation, but about **potential changes in how the society functions as a result of [increased economic inequality / more economic equality / increased economic differences]** .

We are interested in whether you think **any** such changes occur (whether they are positive or negative).

All in all, do you think society would function differently **at all** after [becoming more economically unequal/becoming more economically equal/such an increase in economic differences within the population]?

- Yes, economic [inequality/equality/differences] affects society. The society would change
- No, economic [inequality/equality/differences] does not affect society. The society would remain the same.

Survey 2 General externality belief question (Part 2) *Note: Randomized phrasing is shown in brackets. This question was only shown to respondents that clicked “Yes” in the previous question.*

This question is about the same increase in economic [inequality / equality / differences within society] (the transition from society A to B).

All in all, do you think that the changes in society as a result of such an increase in economic [inequality / equality / differences within society] would be positive or negative?

(When thinking about your answer, try to ignore the direct effects on individuals' economic situation and focus on changes to society as a whole. Also note that this question is not about whether new distribution is more or less unfair. If you do not believe economic [inequality / equality / differences within society] affects society, select the answer option in the middle here and in subsequent questions.)

- [More economic inequality / More economic equality / Larger differences in income and wealth] → Society functions **much better**
- [More economic inequality / More economic equality / Larger differences in income and wealth] → Society functions **somewhat better**
- [More economic inequality / More economic equality / Larger differences in income and wealth] → Society functions **as well as before**
- [More economic inequality / More economic equality / Larger differences in income and wealth] → Society functions **somewhat worse**
- [More economic inequality / More economic equality / Larger differences in income and wealth] → Society functions **much worse**

C.6 Treatment videos

This section describes the video content in each of the treatment arms and control groups.

Treatment group 1: Crime as an inequality externality This treatment group receives information on the relationship between crime and inequality using data from the World Bank and the World Inequality Database. As shown in the screenshot in Figure 10, the video first presents subjects with a scatter plot and a fitted line that characterizes the relationship between inequality and homicides. The next graphic characterizes the magnitude of the correlation. It shows that very equal countries have, on average, between one and two homicides per year per 100,000 people, while very unequal countries have, on average, between ten and twenty homicides per year per 100,000 people. The respondents are then told that researchers still argue about whether this means that inequality *causes* more crime – highlighting that these correlations need not imply causation. The video ends with a statement that most research on this topic has confirmed the correlational relationship and finds that it holds for alternative metrics of crime such as property crime and robberies.

The filler questions ask the respondents about whether they experienced or perceived more crime in places they lived or travelled to with higher levels of inequality. It thus creates a direct link to the video by asking the subjects whether they themselves experienced this relationship.

Treatment group 2: Trust as an inequality externality This treatment is structurally similar and uses a correlation between inequality and generalized trust (the number of individuals that say that most people can be trusted in their country) using data from the World Inequality Database and the World Value Survey. The remainder of the video and the filler questions are intentionally similar to the crime video; the style and phrasing remains the same (with modified numbers and alternative metrics) at all times.

Treatment group 3: Full externality treatment While treatment groups 1 and 2 tackle one externality channel each, treatment group 3 is designed as an all-encompassing externality treatment. It thus aims at providing more comprehensive information on whether societies with

high economic inequality usually function better or worse. By presenting broad evidence that highlights the *negative* effects of inequality and by showing that the evidence for positive externalities is rather limited, the treatment makes the strongest case for the negative consequences of inequality between our three videos. As shown in the screenshot in Figure 10, the first part of the video shows the same information that we present in treatments 1 and 2. It then shows that there is no relationship between inequality and economic growth nor between inequality and innovation (measured by the number of patents).¹⁰³ Respondents are then told that these correlations need not imply any causal relationship, and that researchers disagree on the topic – some do not believe inequality causes society to function worse, while others believe economic inequality harms society through these and other channels (the video briefly mentions social unrest, corruption, and political polarization). The video ends with a quote from Amartya Sen, quoted as a nameless Nobel-winning economist, that “*virtually all the problems in the world come from inequality of one kind or another.*” Following the video, the filler questions in this treatment ask respondents whether they have generally experienced that more unequal places function better or worse than more equal places.

The full externality treatment is designed as the strongest externality treatment at the cost of precision. Realistically, redistributive preferences are composed of fairness concerns, externality beliefs, and several other factors. Crime or trust are only one part of each of these externality concerns. If our respondents are rational, even a large shift in the belief in a crime externality might be an overall small shift in their redistributive preferences which is not detectable even with a large sample size. The full externality treatment solves this issue by informing subjects about inequality externalities on a broader scale.

Treatment group 4: Fairness treatment The fourth treatment group receives information on how the wage-productivity gap has evolved since 1975, as shown in the screenshot in Figure 10, using data from the Economic Policy Institute. The stimulus includes information that blue-collars’ wages stagnated while their productivity increased since the 1980s. The income share of the top 1% earners (from the World Inequality Database), on the other hand, increased sharply, indicating that the economic gains from the increase in productivity went for the most part to the richest Americans. The filler questions ask subjects to recall whether people they know that were employed in 1950 and 1980, and whether they thought these people were paid closer to what they produced than people with similar jobs today.

The treatment intends to give respondents information about the *fairness* of the economy, and functions as a comparable benchmark to the inequality externality treatments.

Control group 1: Active control The active control group receives a video that is structured similarly to those on trust and crime. The general video topic is how economic inequality metrics differ and how this can affect research on economic inequality. The video informs subjects about the difference between the Gini index and the top 10% income share. The video does not contain any information that is strictly speaking relevant for redistributive preferences, but does give individuals stimuli about inequality itself. Thus subjects are primed to think about inequality

¹⁰³This was included due to our original assumption that individuals would believe in a positive association between these variables.

without revealing any information about inequality externalities or the fairness of the prevailing income distribution. The comparison across this control group and the treatments thus seeks to isolate the role of information. Filler questions to this treatment ask subjects to reflect on whether they (i) have already thought about the measurement of inequality and (ii) whether they knew that researchers had different ways of measuring inequality before the survey began.

D Further Results

D.1 Phrasing results

The robustness check that has the largest effect on the descriptive statistics is to substitute out the phrase “more inequality” with “more equality”. Under this phrasing the share of negative specific externality beliefs are on average $\sim 12\%$ lower.¹⁰⁴ This does not seem to be due to the word “inequality” itself, as using “differences in income and wealth” has a much smaller effect, but rather due to the difference in the distributional change (the effect of “more equality” vs. “more inequality”).

This section discusses this change. We modify the word “inequality” in the survey for various subsets of respondents to explore whether the word itself (and its potentially loaded nature) affects results. Instead we use either “equality” or “differences in income and wealth” throughout the survey for 20% each of Survey 2 respondents, and “differences in income and wealth” for one-third of questions in Survey 1 on a question-by-question basis.

In Survey 1, one-third of respondents per question saw the phrasing “*How do larger differences in income and wealth within the population...*” instead of “*How does more economic inequality...*”. This phrasing was randomly assigned on a question-by-question basis with the goal of exploring whether the phrasing of the question significantly impacted answers.

We further explored this topic in Survey 2. There 20% of respondents were shown an “equality” phrasing and 20% were shown a “differences in income and wealth” phrasing throughout. Respondents in the “differences in income and wealth” phrasing strand, for example, do not see the word “inequality” anywhere in the survey. Respondents were explained each concept using the same diagrams.

Note that respondents who received the “equality” phrasing were asked how “more equality” changes the relevant factors, which changes the *direction* of the question. As an example, a negative externality belief under the “inequality” phrasing would be “*More inequality \rightarrow More crime*”. The same belief under the “equality” phrasing would be “*More equality \rightarrow Less crime*”.

General externality beliefs Neither the “differences” nor the “equality” phrasing had a significant effect on general externality beliefs (statistically insignificant > 2 p.p. changes).

Specific externality beliefs Specific externality beliefs are generally not constant across phrasing choices. We show this in Figures E3-E4 and detail the results below.

¹⁰⁴The share believing in negative inequality externalities of political polarization and corruption are particularly impacted. These shares decrease from 71% to 44% (political polarization) and 69% to 47% (corruption) respectively.

First, the “differences” phrasing. This phrasing choice has a small but non-negligible effect on results in Survey 1 (where it was used on a question-by-question basis). In most questions it shifts averages by roughly 2-4 percentage points. The largest phrasing effect is for economic growth, where about 8% of individuals shift their response away from inequality decreasing growth under the “larger differences” phrasing (55% to 47%). In Survey 2, where the phrasing change was employed throughout the survey, changes are similar or smaller. No specific externality belief average shifted more than 5% from the baseline under this phrasing in Survey 2.

The “more equality” phrasing has a larger effect. It particularly affects the inequality externality beliefs regarding political polarization and corruption, where the proportion of those believing in the negative externality change from 70% to 44% and 68% to 47% respectively when changing “more inequality” to “more equality” (a decrease of 26% and 21%).¹⁰⁵ Despite this, the negative externality belief is still held by close to a majority in both cases. Other shifts are generally smaller and always below 15%. Respondents are less likely to choose the negative externality option in this setting for six out of eight outcomes (with a small effect in the opposite direction for the two economic outcomes).

Although phrasing choices have a significant effect on our results, the negative externality option is still the most popular for any combination of phrasing choice and outcome. It follows that our main results are robust to these changes. This exercise also implies that our results are not caused by the nature of the word “inequality” itself, as the “differences in income and wealth” phrasing do not change results in a noteworthy way. We hypothesize that the larger effect of the “equality” phrasing could be at least partly due to respondents thinking these problems are persistent and that an increase in economic equality – in other words, a reduction in economic inequality – is unlikely to solve them immediately or at all.

D.2 Consistent inequality externality beliefs across inequality levels

A key assumption for our main results to be easily interpretable is that the direction of individuals’ inequality externality beliefs do not vary across the level of inequality. If they are not, a respondent might for example think that more economic inequality increases the amount of economic growth if inequality is low and decreases the amount of economic growth if inequality is high. To explore this we ask Survey 2 respondents directly whether they think the same relationship holds “*no matter whether the country is initially very equal, very unequal, or anything else*”. The large majority (81%) expresses unchanging externality beliefs. The externalities with the largest share of changing beliefs are innovation (25%) and economic growth (24%). The full data is shown in Table F16.¹⁰⁶

D.3 Inequality or average income?

A pertinent question is whether respondents think the average income or level of economic inequality is more impactful in determining the levels of the outcomes we elicit. It is difficult to create an easily understood question on this topic; such a question would also be prone to

¹⁰⁵Believing in the negative externality in this case implies that respondents answer that more equality leads to less political polarization or less corruption.

¹⁰⁶Among respondents who have changing beliefs in innovation and growth, follow-up questions indicate that they believe in a positive externality at low inequality levels and a negative externality at high inequality levels.

experimenter demand. We thus examine this topic from an indirect approach. To do so we ask respondents to predict the level of a given outcome in an average country with a [low/high] level of average income and a [low/high] level of economic inequality. We then analyze how the changes in given average income/inequality changes the predicted outcome.

We show the results in Figure E13. Both the average income and level of inequality are generally strong predictors for the outcomes we elicit (crime, trust, and so on). Economic inequality is particularly predictive for the level of political polarization and corruption, whereas the average income is particularly predictive for economic factors. Indeed, high economic inequality is on average *positively* correlated to high economic growth and innovation, in opposition to our prior results. Meanwhile, respondents do not take the level of average income into account when predicting the level of political polarization.

In sum, respondents believe that both the level of average income and the level of economic inequality are strong and distinct predictors for other outcomes. Note, however, that this analysis does not explore respondents' *causal* beliefs and is a purely correlational exercise. For example, respondents believe high-income countries have high economic growth and not necessarily that high incomes *cause* high economic growth. As such we suggest interpreting Figure E13 with caution.

D.4 Dual control groups

In this section, we compare the respondents' characteristics and outcomes across the two control groups. We pre-specified to merge these to groups conditional on being sufficiently similar. Specifically, we pre-specified the following decision rule:

"If the active and passive control group are sufficiently similar, we will merge them for the main analysis. This decision will be made upon not reaching all the three following criteria.

- There is no 1% statistical difference in the index outcome variable between the active and passive control.
- There is not a 5% statistical difference in at least three of the four redistribution dummy variables listed above.
- There is not a 5% statistical difference in at least three of the four externality dummy variables listed above.

If one of these criteria are reached, we will present regressions with both control groups as separate categories."

Table D1: Dual control: Balance table for redistributive preferences

	(1)	(2)	(3)
Variable	Passive Control	Active Control	Difference
RP Index	-0.111 (0.965)	-0.045 (0.984)	0.067 (0.065)
Wants redistribution	0.370 (0.483)	0.360 (0.481)	-0.009 (0.032)
Increase top taxes	0.537 (0.499)	0.622 (0.486)	0.085*** (0.033)
Gov. reduce ineq.	0.480 (0.500)	0.508 (0.501)	0.028 (0.033)
Ineq. is serious issue	0.515 (0.500)	0.508 (0.501)	-0.007 (0.033)
Observations	538	394	932

Note. This table represents mean (standard deviations) for redistributive preference measures of respondents in the active (column 1) and passive (column 2) control groups. Column (3) characterizes the difference across the two. The pre-specified criteria to merge these two control groups for the main analysis is satisfied. *Significance levels:* *10%, **5%, ***1%.

As shown in Table D1 the index is not significantly different across the two groups. From the redistributive preference variables, only the variable on top tax-rates is significantly different across the two groups.¹⁰⁷ The other variables are not significantly different between control groups; the differences are also relatively small and in opposing directions. As pre-specified, we thus merge the two groups for the main analysis.

We also compare first-stage post-treatment outcomes (inequality externality beliefs and fairness views) across the two groups and find no significant difference between the two groups on any of these outcomes (see Table F4). This indicates that the difference for the top tax rate could be spurious, as other strong predictors of redistributive preferences such as fairness views are balanced across the two groups. It is also possible, however, that the quantification of the top 10% income share in the active control video made respondents who saw this video prefer a higher income tax rate for the same top 10%. If so, this would bias our main treatment effects on this variable downwards. We note that such unexpected effects are a good motivation to use dual control groups.

As shown in Table F5, there are no significant differences between the two groups on any pre-treatment dimension. Table F6 shows that they are also comparable on various sociodemographic characteristics.¹⁰⁸

Overall, the results show that the two control groups are sufficiently similar to be merged and can be treated as one control group. While there are few idiosyncratic differences across the two groups, they are non-systematic and likely to be spurious, reflecting the fact that we

¹⁰⁷This could be simple statistical noise; it is also possible that mentioning the top 10% income share shifted individuals' top tax rate preferences. We note that unexpected discrepancies like these are a strong motivation for the dual control group method.

¹⁰⁸We find that the two groups are mostly balanced apart from a few exceptions. Subjects in the active control group are less likely to be neither black nor white, and are somewhat differently allocated into the three income groups. Note that these differences are not large and including them as control variables does not affect the differences in redistributive preferences or first-stage outcomes. Beyond that, passive control group subjects are not more or less likely to pass all three attention checks build into the survey than active control groups. Neither are they more nor less likely to pass an attention check that was administered *after* the treatment.

are testing many hypotheses at once.¹⁰⁹ Following our pre-analysis plan, we thus merge the two groups.

D.5 Balance across control and treatment groups

This section checks the pre-treatment balance of control and treatment groups. As shown in Table F7, the crime and control groups are balanced on nearly every dimension. There is one important exception; subjects in the crime treatment group have significantly higher perceptions that unequal countries usually function worse. However, including this perception as a control variable in outcome regressions does not affect the results of the analysis.

Table F8 compares observable characteristics across the trust and control groups. The two groups are completely balanced on observables.

Table F9 compares observable characteristics across the full externality treatment and the control group. The full externality group has somewhat fewer individuals in high income households but more individuals from middle-income households. They are also slightly more likely to believe that working-class Americans are paid less than their productivity.¹¹⁰

Table F10 compares observables across Fairness and Control group. The two groups are balanced on all covariates with the exception of gender (slightly more in the Fairness group) and the number of individuals from middle-income households (slightly more in the Fairness group).

D.6 Mediation analysis

This section describes the mediation analysis results in Table F24. This table includes post-treatment first stage outcomes in the regression of redistributive preferences on treatment variables. Compared to the treatment effect of a regression without post-treatment beliefs, the coefficients of the treatment dummies decrease when the post-treatment first stage outcomes are included. For each treatment effect this effect is driven by the post-treatment belief in question (e.g. externality belief for the externality treatment). This strongly indicates that at least part of the treatment effect is driven by changes in first-stage beliefs.

More concretely, the treatment effect of the full externality treatment on our redistributive preference index was 10.7 percent of a standard deviation if we do not control for post-treatment externality beliefs and decreases to 5.5 percent of a standard deviation once we control for post-treatment externality beliefs. This implies a reduction in the magnitude of the treatment effect of nearly 50% ($p = 0.002$, t-test). The reduction in the magnitude of the fairness-treatment's treatment effect is similarly large. Before controlling for beliefs, the magnitude of the fairness treatment was 20.8 percent of a standard deviation and then decreased to 13.5 percent of a standard deviation ($p=0.000$, t-test). This provides evidence that our reduced form treatment effect is mediated through a shift in beliefs, as intended by the treatment itself.¹¹¹ When also

¹⁰⁹The potential exception to this being the top tax result.

¹¹⁰We note that this group does not statistically differ from the control group in other fairness views elicited either pre- or post-treatment, indicating that this correlation could be spurious. The group also does not have a statistically significant difference in externality beliefs from the control group pre-treatment. This correlation is also less significant and smaller in magnitude than the upcoming first-stage and outcome treatment effects for this group. Main results are also robust to including this variable as a control.

¹¹¹A complete disappearance of the treatment effect is unlikely given that beliefs are generally measured with noise and that our first-stage belief measurements are bounded. An example of this would be an individual who

including the opposite post-treatment belief (e.g. externality beliefs for the fairness treatment) treatment effects remain similar ($5.5 \rightarrow 5.8$ for the full externality treatment and $13.5 \rightarrow 12.2$ for the fairness treatment), indicating that the treatment effect is not driven by spillovers.

D.7 Theoretical mechanism of the information experiment

What theoretical mechanism drive a potential treatment effect? Assume that individual i 's stated redistributive preferences $RP_i(\eta_i, G_i, X_i)$ are a function of fairness views G_i , a set of other characteristics X_i including attention, mood, immutable characteristics, and so on, and potentially the individual's inequality externality beliefs η_i (if such externality beliefs are a determinant for redistributive preferences). A video information treatment T_q , where q determines the type of information treatment, can affect any of these three determinants;

$$\frac{dRP_i(\eta_i, G_i, X_i)}{dT_q} = \frac{\partial RP_i}{\partial \eta_i} \frac{\partial \eta_i}{\partial T_q} + \frac{\partial RP_i}{\partial G_i} \frac{\partial G_i}{\partial T_q} + \frac{\partial RP_i}{\partial X_i} \frac{\partial X_i}{\partial T_q} \quad (7)$$

These are the three different channels through which any of our treatments can affect redistributive preferences. We are specifically interested in whether $\frac{\partial RP_i}{\partial \eta_i} \neq 0$, which would imply that inequality externality beliefs are a causal determinant of redistributive preferences. As the active and passive control are similar in outcomes (see Appendix D.4), we can be confident that the presence of a video treatment (as a general concept) generally did not affect redistributive preferences. This implies that $\frac{\partial RP_i}{\partial X_i} \frac{\partial X_i}{\partial T_q} \approx 0$. In other words, showing respondents a video about inequality-related issues does not significantly change their redistributive preferences due to attention effects, priming, or any other change to the broad set of characteristics defined as X_i . If the externality treatments T_η have limited spillovers on fairness views, we also have that $\frac{\partial G_i}{\partial T_\eta} \approx 0$. If finally the externality treatments affect externality beliefs themselves, we have that $\frac{\partial \eta_i}{\partial T_\eta} \neq 0$. From Equation 7 we can then conclude that,

$$\frac{dRP_i(\eta_i, G_i, X_i)}{dT_\eta} \propto \frac{\partial RP_i(\eta_i, G_i, X_i)}{\partial \eta_i}, \quad (8)$$

and a significant treatment effect – or that $\frac{dRP_i}{dT_\eta} \neq 0$ – would imply that $\frac{\partial RP_i}{\partial \eta_i} \neq 0$, or that inequality externality beliefs causally affect redistributive preferences. The same argument is also true for fairness views (swapping η for G).

D.8 Robustness of treatment effects

Population weights Even though we targeted representativity along several observable dimensions, we slightly over- or under-sample populations with some characteristics as described in Section 3.3. To establish representativity ex-post, we replicate our key analyses by reweighting along gender, race, age-groups, party, holding a college degree, income group, and geographic

already thought inequality increases crime before the survey; after watching the full externality video she becomes increasingly convinced of the importance of this causal channel, which shifts her redistributive preferences. Her response to the first-stage crime question is the same ("*More inequality* \rightarrow *A lot more crime*"). However, her beliefs have changed, which then affect her redistributive preferences.

region. Regressions in Table F21 regress redistributive preferences on our treatments; Regressions in Table F23 regress posterior beliefs on treatments; and Regressions in Table F32 replicate the horse-race regressions using population weights. The results for the latter two regressions are nearly identical. For the former, reweighting has only small effects on the magnitude of the significant treatment effects. As standard errors increase under the reweighting procedure, certain clearly significant treatment effects in the original weighting are, however, no longer 5% significant in the reweighted data.

Keeping all respondents As prespecified, we dropped the 5% fastest and slowest respondents, as well as those that spent less time watching the video than the length of the video. Additionally, we dropped respondents with unusual or strange responses to open text questions. We replicate our main regressions keeping these respondents. As shown in Tables F33, F34 and F35, we do not find any meaningful differences compared to the analyses using our main sample.

Failing any attention check We also replicate our main regressions while excluding all respondents that failed *at least* one attention check. While the first-stage effects and the horserace regressions remain very similar to our main specification (Table F37 and F38 respectively), the effect of the full externality treatment on RP-Index becomes marginally significant as shown in Table F36. Given that controlling for passing or failing an attention check does not result in any differences, as shown in Table F39, this is likely due to the lack of power that results from dropping one-third of our sample.

Specifying only one control group As shown in Appendix D.4 we merge our two control groups given that they are sufficiently similar on a set of pre-specified criteria. As a robustness check, we first-stage and reduced form treatment effect regressions but drop either the active or the passive control group in Tables F40, F41, F42, and F43. The treatment effects are slightly stronger when only considering the passive control group as the baseline compared to when only specifying the active control group as the baseline, and overall results are robust to either specification.

We briefly discuss the full externality group specifically as this is important for our main hypothesis. Results are qualitatively unchanged and slightly stronger in magnitude when using only the passive control group. When using only the active control group, treatment effects still go in the expected direction. The RP-index treatment effect of the full externality treatment is only marginally significant in this setting, however, due to lower statistical power from the smaller control group. The magnitude of the effect is slightly smaller but comparable to the standard full control group specification.¹¹²

Not controlling for observable characteristics We replicate our main regressions without controlling for any observable characteristics. As shown in Tables F44 and F45 reduced form and first-stage treatment point-estimates are nearly identical to our main specification in magnitude and significance. This is expected given our randomized treatment design.

¹¹²We note that the high top tax rate result from the active control group leads to a *negatively* significant top tax rate result for the full externality treatment.

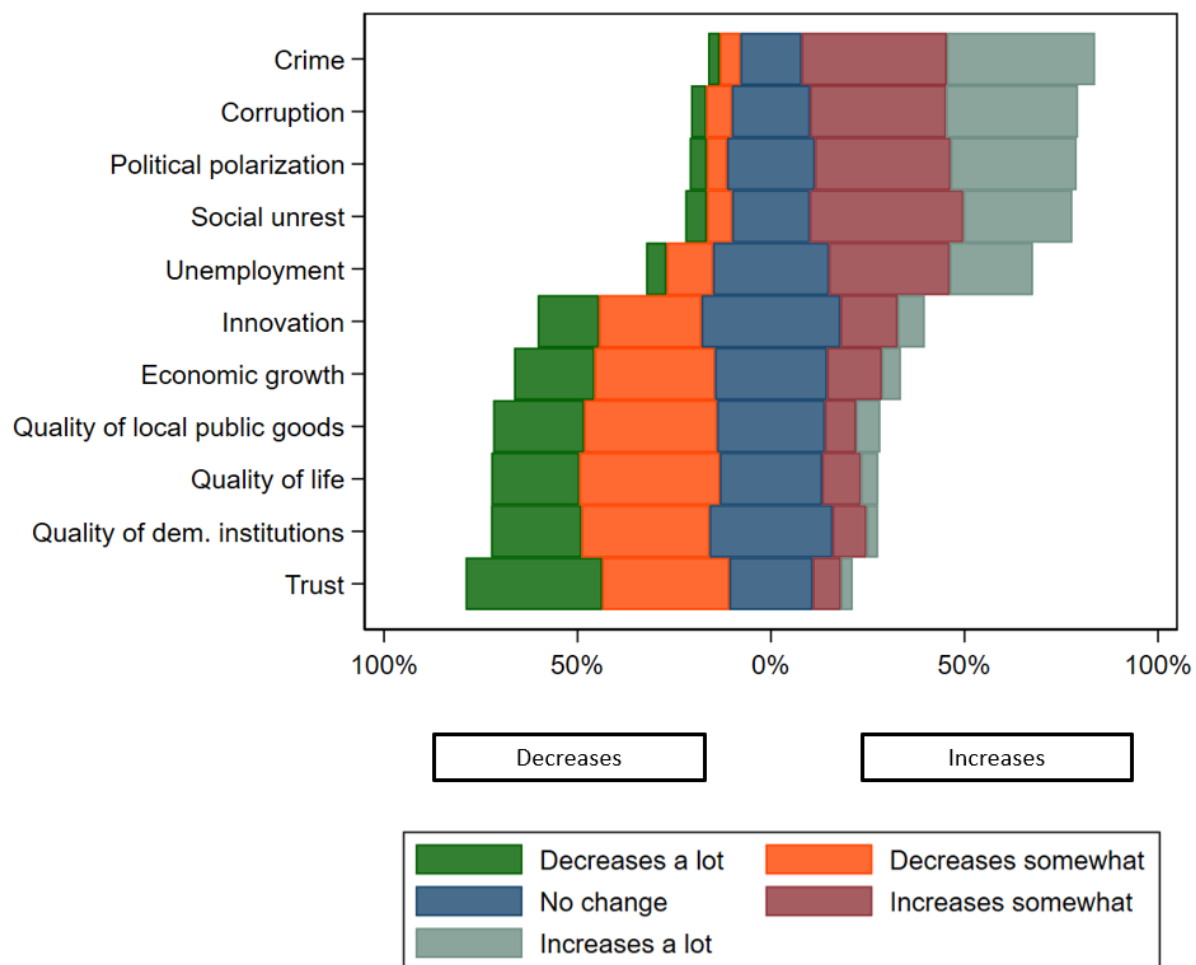
Different sets of controls We pre-specified a vector of control variables to evaluate the treatment effects. The results do not change significantly when we change this vector to any other reasonable permutation (as expected from our randomized experiment design). Notably, our results do not change if we include prior externality beliefs in the set of controls. Due to the large number of such permutations we do not explicitly show these results.

Using non-dichotomized outcome variables In our main specifications, we dichotomize our outcomes and explanatory variables when applicable. In Tables F46 and F47 we replicate our main regressions without dichotomizing any outcomes or control variables and, furthermore, we recompute the RP-Index based on non-dichotomized beliefs. As shown in the tables, the results are nearly identical to those presented previously.

Multiple hypothesis testing In the main regression tables (Table 2) we run a total of twenty tests for statistical significance. On this scale, Type I errors can become a serious problem and lead to erroneous inference of statistical significance. To correct for this we use the false discovery rate (FDR) sharpened q-values as described in Anderson (2008). FDR sharpened q-values are classical p-values that are corrected for the expected number of significant treatment effects that are truly null effects. Where a p-value threshold of 0.05 gives a false positive rate of 5% among all treatment effects that are truly null, a q-value threshold of 0.05 gives a false *discovery* rate of 5% among all *significant* treatment effects. This correction has no significant effect on our conclusions. None of the treatment effects with $p < 0.05$ in our original specifications have q-values above 0.05, indicating that this is a negligible concern. The results of this correction are shown in Table F50.

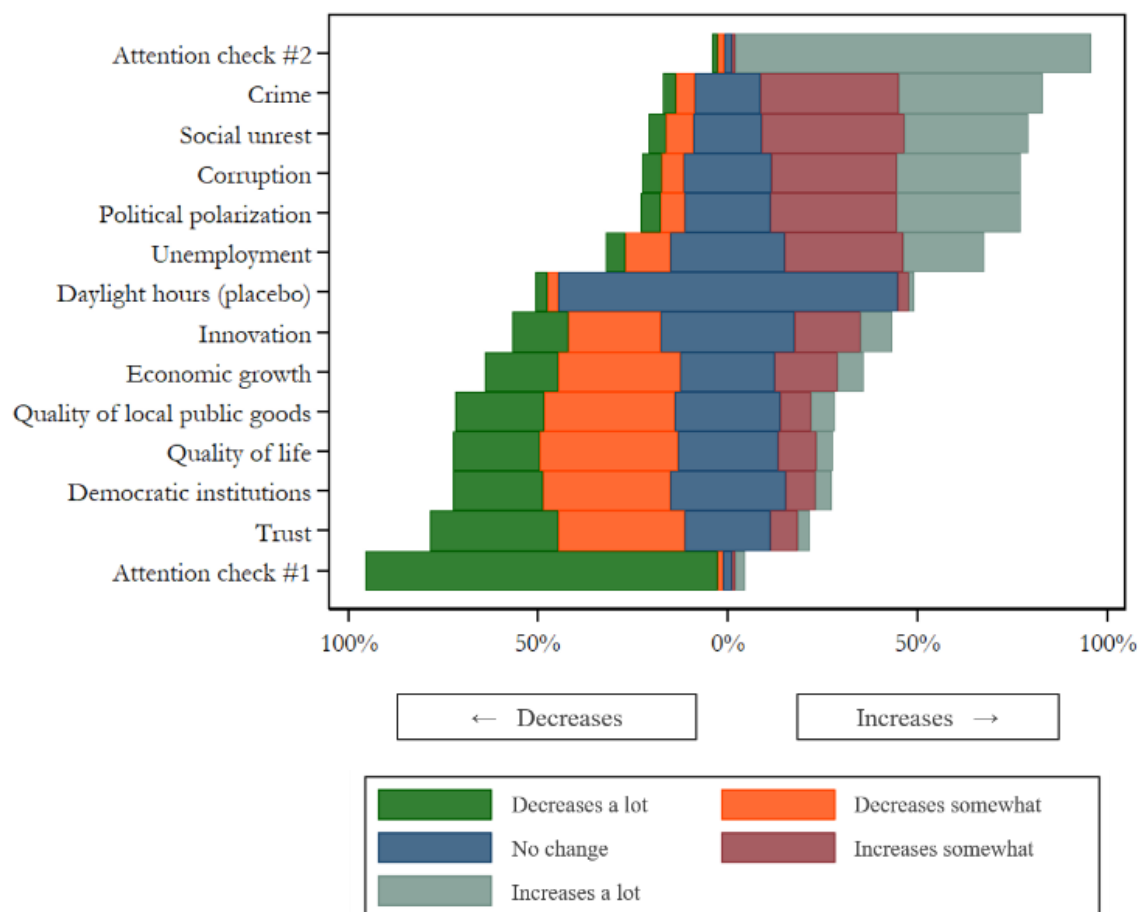
E Figures

Figure E1: Distribution of Externality Beliefs in Survey 1 Control Group



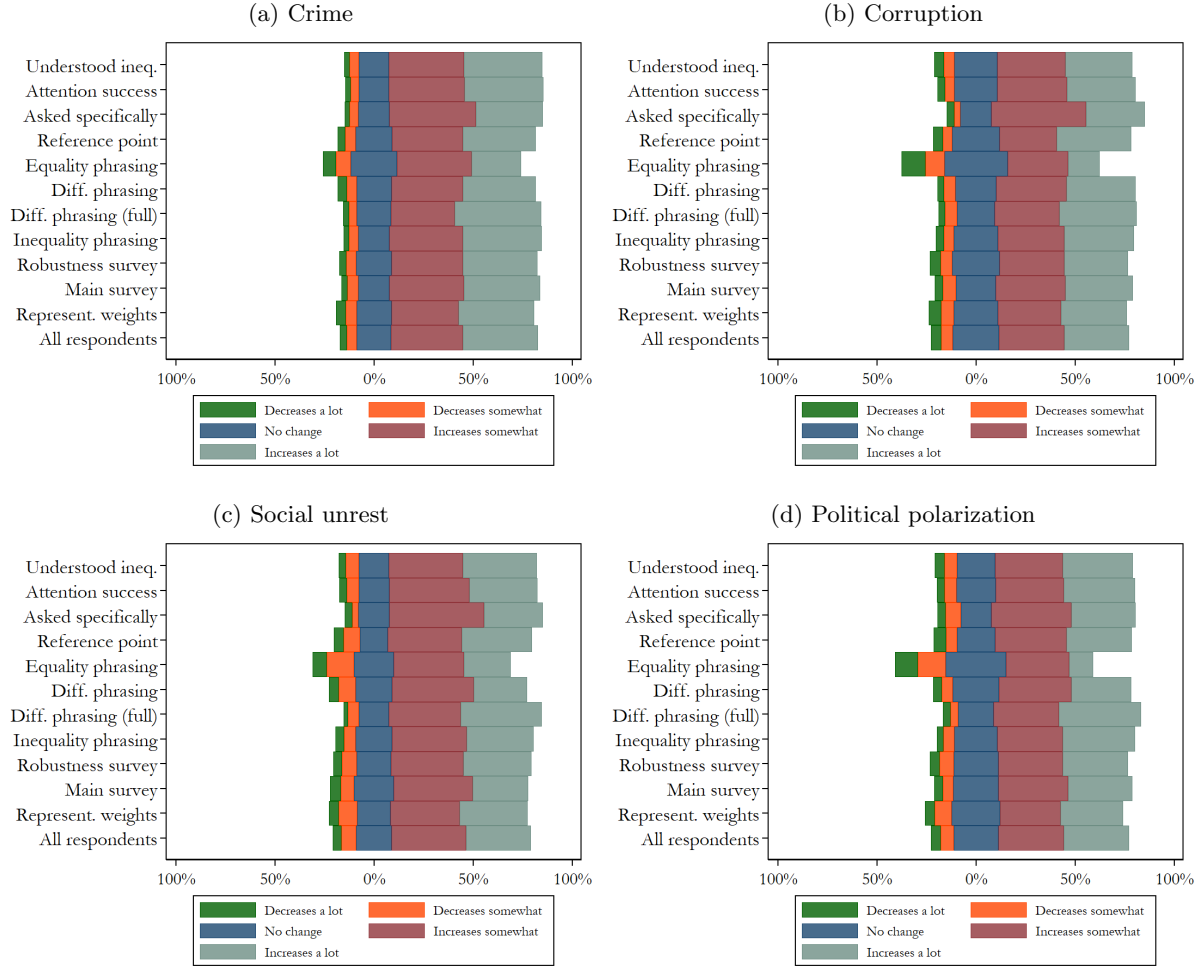
Note. Specific externality beliefs for control group in Survey 1. Questions are ordered according to which portion of respondents believe that inequality increases the variable. Full question example: “How does more economic inequality change the amount of crime in a country?” Answer option example: “More inequality → a lot more crime”. Answers are pooled from Survey 1 and Survey 2 if applicable $n \in \{628, 3, 292\}$. For the equivalent figure using pooled data or only data from Survey 2 respectively, see Figures 2 and E2.

Figure E2: Distribution of Externality Beliefs in Survey 2: How Does More Inequality Change Various Outcomes?



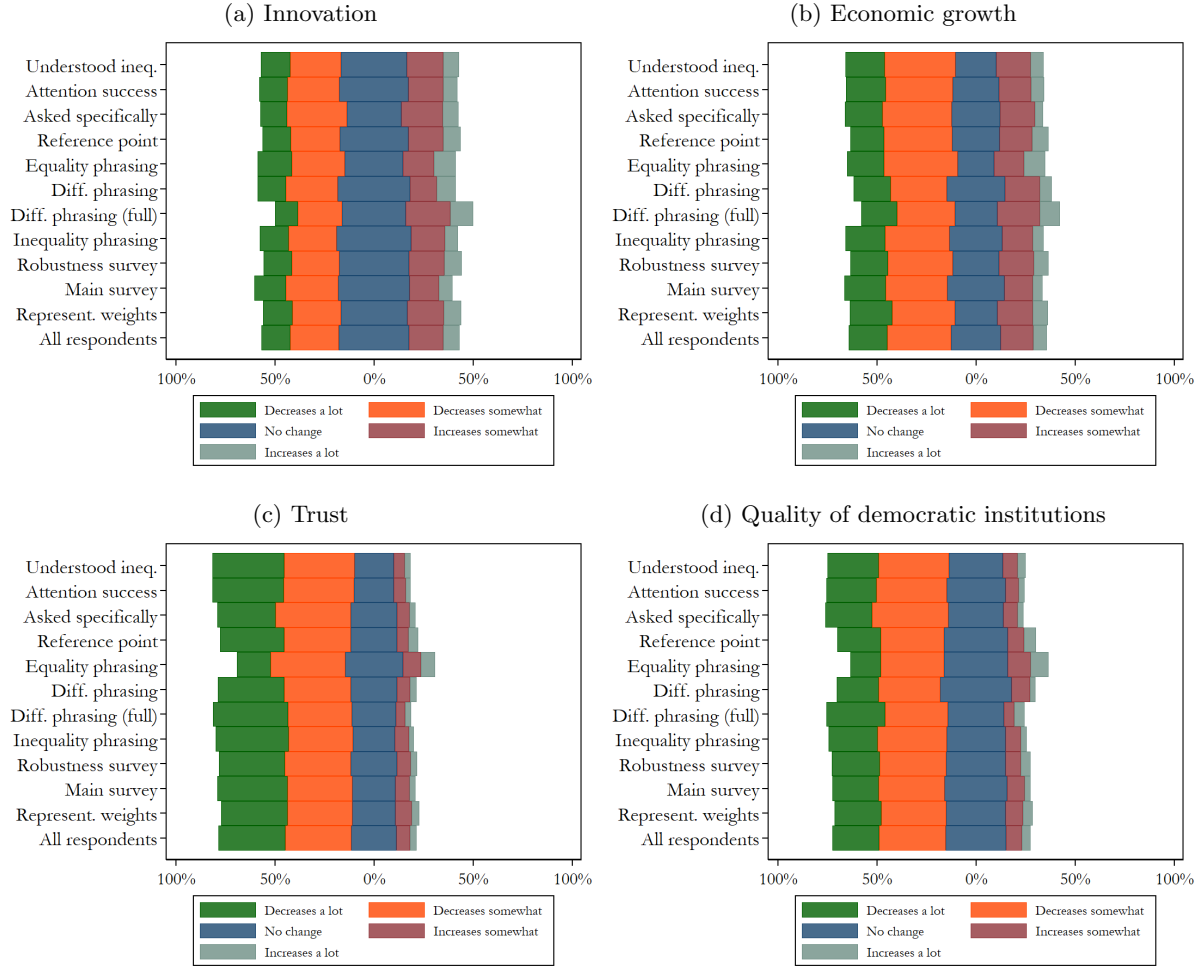
Note. Questions are ordered according to the net share of respondents who believe that inequality increases minus decreases the variable. Full question example: “How does more economic inequality change the amount of crime in a country?” Answer option example: “More inequality → a lot more crime”. The placebo question asks respondents how they think more economic inequality would change the amount of daylight hours in a country. The two attention check questions ask the respondents explicitly to answer either “Decreases a lot” (Attention check #1) or “Increases a lot” (Attention check #2). The high share of respondents who correctly answer these questions is partly mechanical, as individuals who incorrectly answered at least two attention checks were removed from the sample. Every respondent was asked every question. Order was fully randomized. For 20% of respondents, any mention of “more inequality” was substituted with “larger differences in income and wealth” throughout the survey. For 20% of respondents, any mention of “more inequality” was substituted with “more equality” throughout the survey. A respondent answering “decreases” to an equality-based question is coded as “increases” in the graph and vice versa. $n = 2,360$. For the equivalent figures using pooled data or only data from Survey 1, see Figures 2 and E1 respectively.

Figure E3: Robustness of externality beliefs I



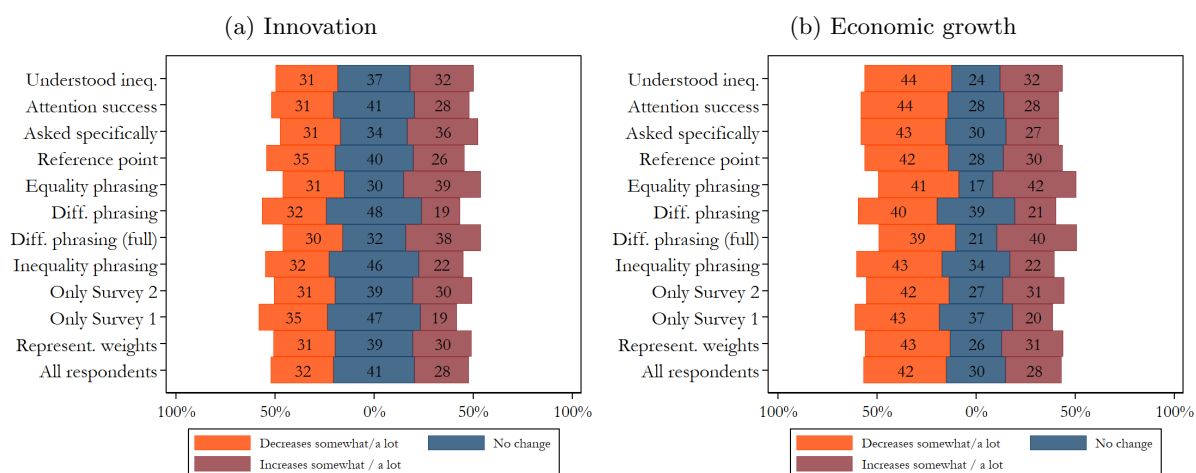
Note. Answers to *How does more economic inequality change the amount of [outcome] in a country?*, indicating inequality externality beliefs, across various specifications. The different specifications are, from bottom to top; (i) All respondents and all phrasings in both surveys, $N \in \{2990, 3292\}$, (ii) All respondents weighted for full representativity on age, gender, race, college attendance, income, region, and party affiliation, $N \in \{2990, 3292\}$, (iii) Only respondents from Survey 1 $N \in \{630, 932\}$, (iv) Only respondents from the Survey 2 $N = 2360$, (v) Only respondents who saw an “inequality” phrasing in either survey, $N \in \{2043, 2345\}$, (vi) Only respondents who saw a “differences in income and wealth” phrasing for the full survey (only in Survey 2), $N = 472$, (vii) Only respondents who saw a “differences in income and wealth” phrasing for this question, but were generally asked about inequality otherwise (only in Survey 1), $N \in \{219, 332\}$, (viii) Only respondents who saw an “equality” phrasing for the full survey (only in Survey 2), $N = 475$, (ix) Only respondents who were explicitly told the reference point of inequality and the magnitude of inequality change (only in Survey 2), $N \in \{1748, 1777\}$, (x) Only respondents who were explicitly asked to think through their answer and were given 15 seconds to do so, then asked to confirm their answer or change it, if they wished (only in Survey 2, and at the end of the survey), $N \in \{292, 298\}$ (xi) All respondents restricted to those who succeeded on every attention check, $N \in \{1677, 1873\}$, (xii) All respondents from Survey 2 who correctly answered a simple question on distributional concepts $N = 1571$. Treatment groups from Survey 1 are always excluded.

Figure E4: Robustness of externality beliefs II



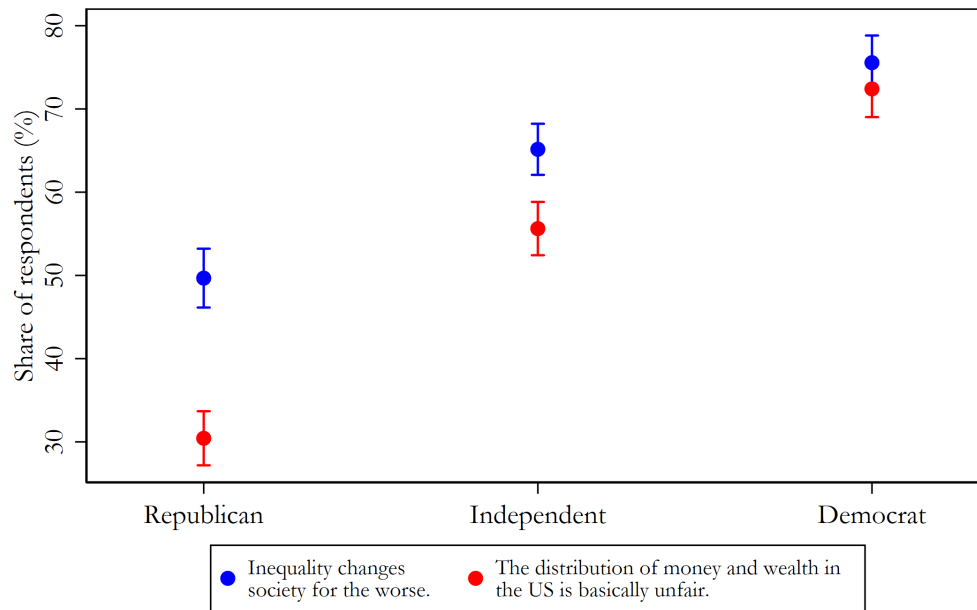
Note. Answers to *How does more economic inequality change the amount of [outcome] in a country?*, indicating inequality externality beliefs, across various specifications. The different specifications are, from bottom to top; (i) All respondents and all phrasings in both surveys, $N \in \{2990, 3292\}$, (ii) All respondents weighted for full representativity on age, gender, race, college attendance, income, region, and party affiliation, $N \in \{2990, 3292\}$, (iii) Only respondents from Survey 1 $N \in \{630, 932\}$, (iv) Only respondents from the Survey 2 $N = 2360$, (v) Only respondents who saw an “inequality” phrasing in either survey, $N \in \{2043, 2345\}$, (vi) Only respondents who saw a “differences in income and wealth” phrasing for the full survey (only in Survey 2), $N = 472$, (vii) Only respondents who saw a “differences in income and wealth” phrasing for this question, but were generally asked about inequality otherwise (only in Survey 1), $N \in \{219, 332\}$, (viii) Only respondents who saw an “equality” phrasing for the full survey (only in Survey 2), $N = 475$, (ix) Only respondents who were explicitly told the reference point of inequality and the magnitude of inequality change (only in Survey 2), $N \in \{1748, 1777\}$, (x) Only respondents who were explicitly asked to think through their answer and were given 15 seconds to do so, then asked to confirm their answer or change it, if they wished (only in Survey 2, and at the end of the survey), $N \in \{292, 298\}$ (xi) All respondents restricted to those who succeeded on every attention check, $N \in \{1677, 1873\}$, (xii) All respondents from Survey 2 who correctly answered a simple question on distributional concepts $N = 1571$. Treatment groups from Survey 1 are always excluded.

Figure E5: Robustness of innovation and economic growth result for Republican respondents



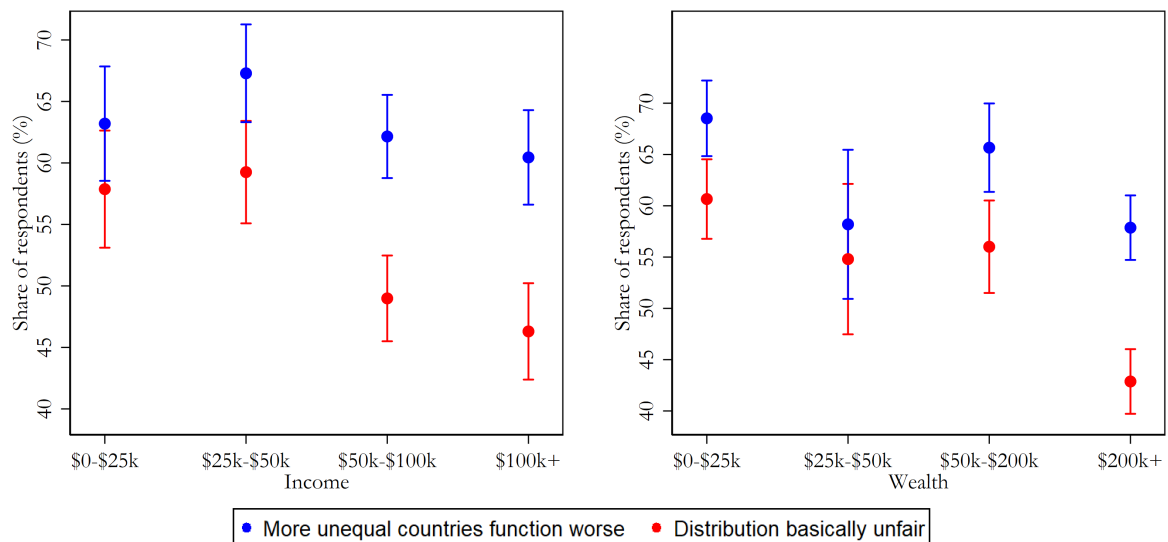
Note. Answers to *How does more economic inequality change the amount of [outcome] in a country?* for innovation and economic growth among Republicans across various specifications. The result that negative externality beliefs are more commonly held than positive externality beliefs among Republicans for economic growth holds across all specifications except when the full survey used the phrasing “equality” or “differences in income and wealth”. In the case of innovation, the same result does not hold for these two groups, or those who answer a question on understanding of distributional concerns correctly, or those who are specifically asked to think through their answer and given time to do so (sample size $N < 125$). Note that “No change” is often the highest-scoring value for innovation. The different specifications are, from bottom to top, only including Republican-identified respondents; (i) All respondents and all phrasings in both surveys, (ii) All respondents weighted such that the full sample to have full representativity on age, gender, race, college attendance, income, region, and party affiliation, (iii) Only respondents from Survey 1, (iv) Only respondents from the Survey 2, (v) Only respondents who saw an “inequality” phrasing in either survey, (vi) Only respondents who saw a “differences in income and wealth” phrasing for the full survey (only in Survey 2), (vii) Only respondents who saw a “differences in income and wealth” phrasing for this question, but were generally asked about inequality otherwise (only in Survey 1), (viii) Only respondents who saw an “equality” phrasing for the full survey (only in Survey 2), (ix) Only respondents who were explicitly told the reference point of inequality and the magnitude of inequality change (only in Survey 2), (x) Only respondents who were explicitly asked to think through their answer and were given 15 seconds to do so, then asked to confirm their answer or change it, if they wished (only in Survey 2, and at the end of the survey) (xi) All respondents restricted to those who succeeded on every attention check, (xii) All respondents from Survey 2 who correctly answered a simple question on distributional concepts. Sample sizes in all cases are slightly less than one-third of those in Figure E3-E4. Treatment groups from Survey 1 are always excluded.

Figure E6: Externality Beliefs and Fairness Views over Party Affiliation (Survey 2)



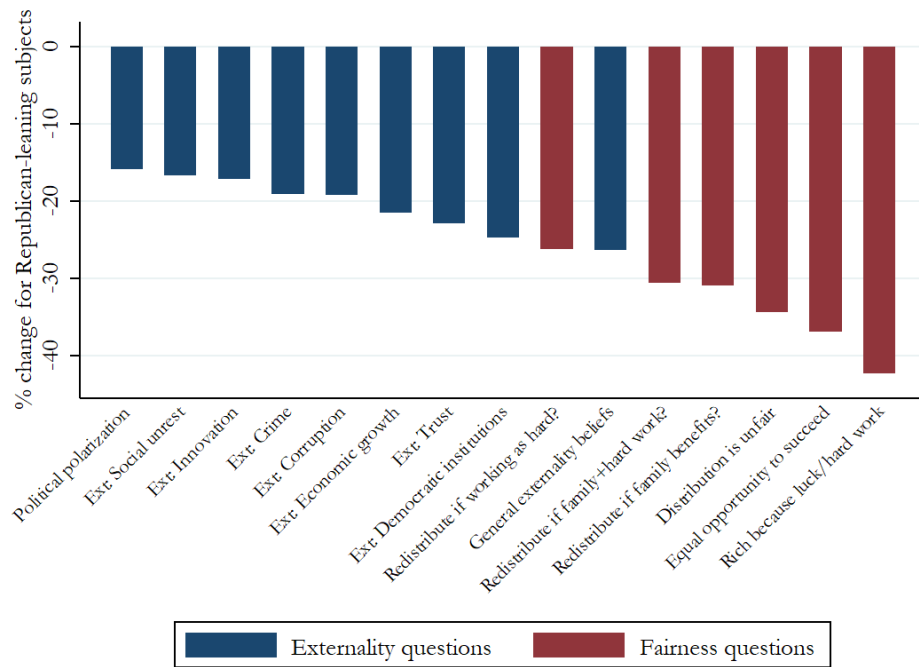
Note. This graph uses the pre-treatment externality and fairness question with Survey 2 respondents (n=2,360). Respondents are asked to agree or disagree with the following two statements: “*The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity to succeed*” and answer the question “*Does economic inequality change society for the better or the worse?*”. The equivalent graph for Survey 1 respondents is Figure 4

Figure E7: Externality Beliefs and Fairness Views over Income and Wealth (Survey 2)



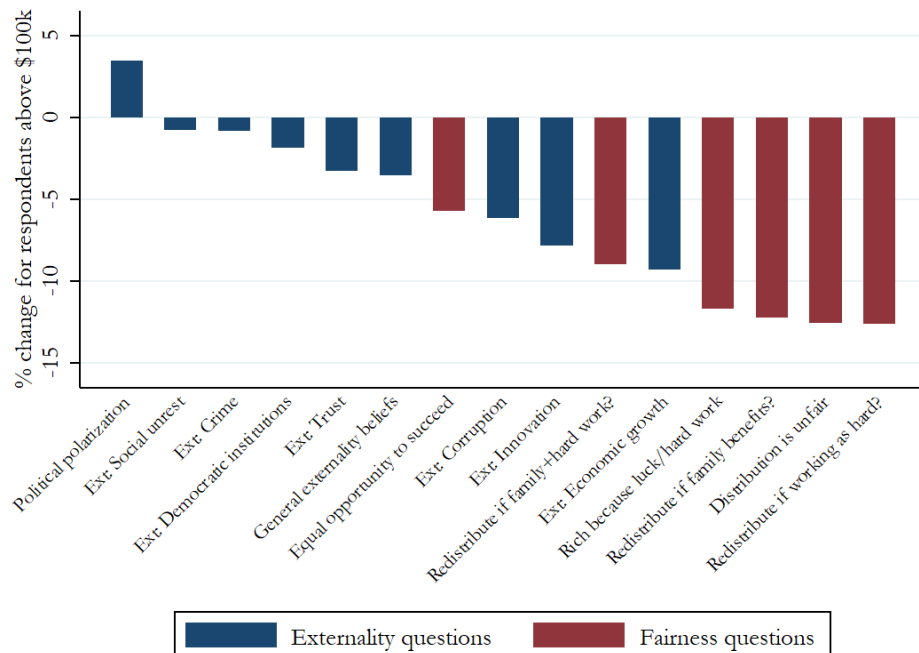
Note. This graph uses the pre-treatment externality and fairness question with Survey 2 respondents (n=2,360). Respondents are asked to agree or disagree with the following two statements: “*The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity to succeed*” and answer the question “*Does economic inequality change society for the better or the worse?*”. For the equivalent graph from Survey 1, see Figure 6.

Figure E8: Party affiliation polarization across questions (Survey 2)



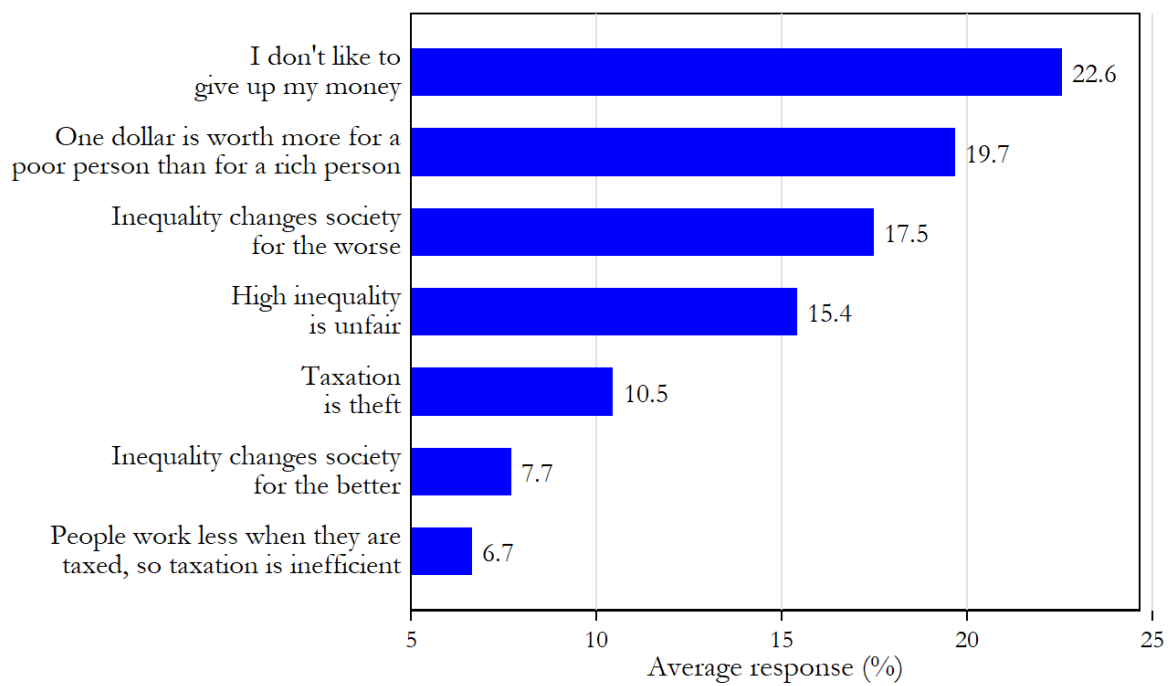
Note. Drop in anti-inequality sentiment for Republican-leaning respondents across every externality and fairness question in Survey 2 without any controls. With a standard set of controls the same relation holds (all fairness questions have larger polarization than any externality question). Questions are largely split on pre-specified criteria or natural binary points (e.g. agree/disagree), keeping total shares close to 50% where possible.

Figure E9: Income polarization across questions (Survey 2)



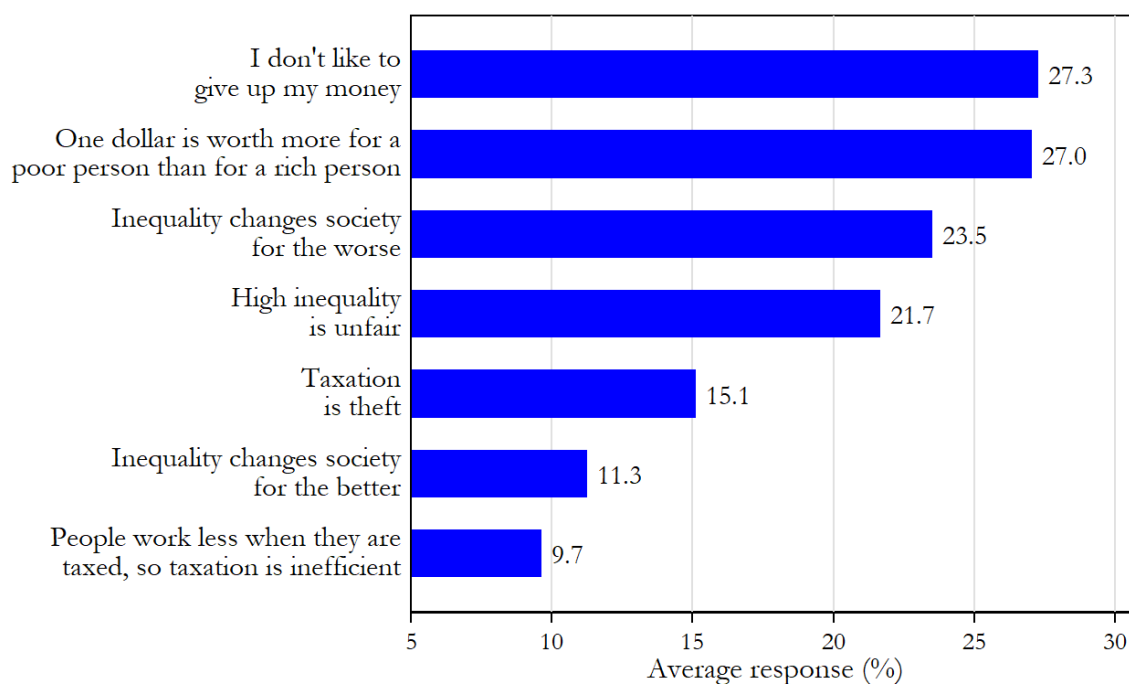
Note. Drop in anti-inequality sentiment for respondents with incomes above \$100,000 across every externality and fairness question in Survey 2 without any controls. With a standard set of controls there is more variation, although the average slope of the fairness questions stay significantly lower than that of the externality questions. Questions are largely split on pre-specified criteria or natural binary points (e.g. agree/disagree), keeping total shares close to 50% where possible.

Figure E10: Mean share for each motive behind preferences for redistribution in Survey 2



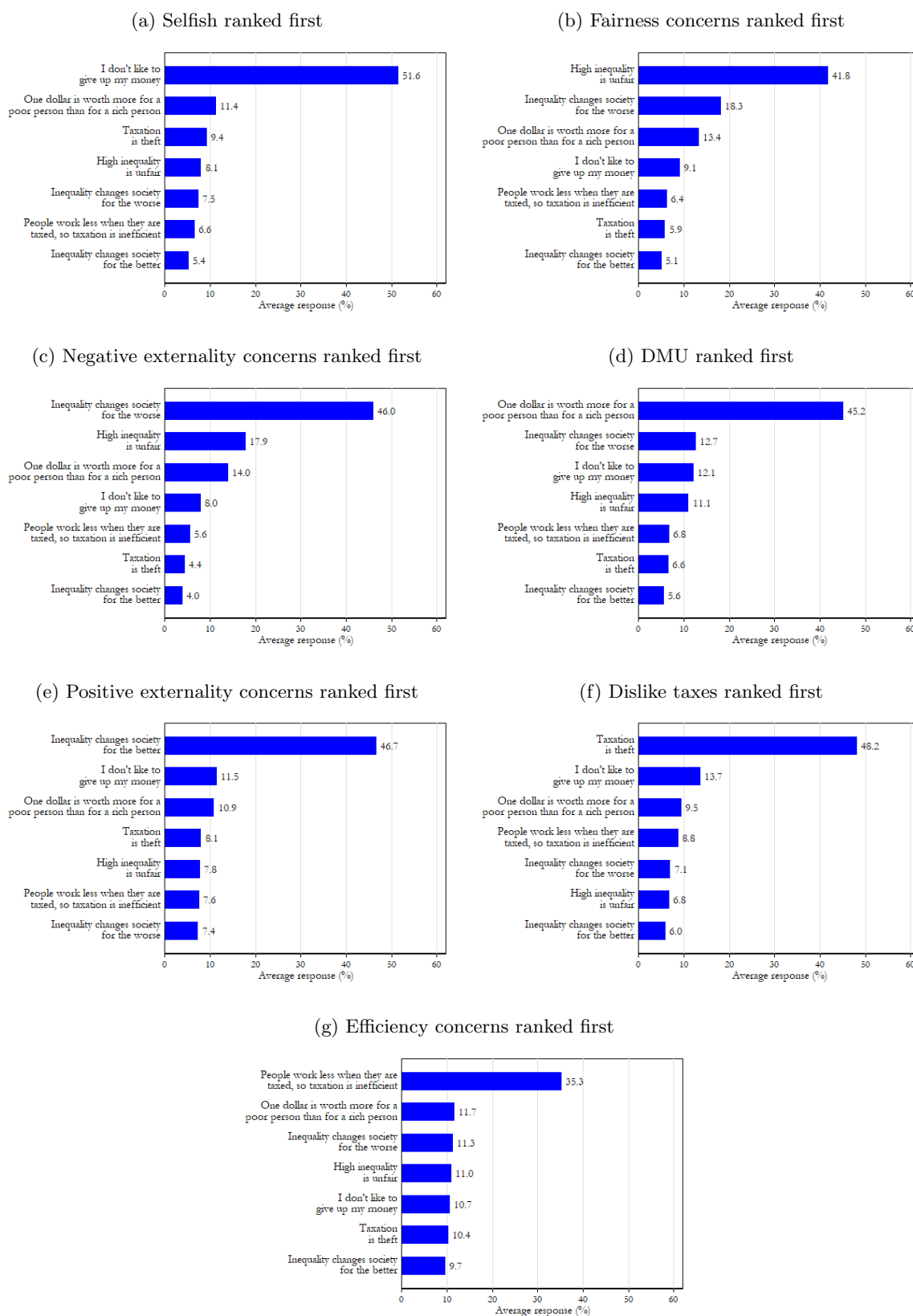
Question text: *When thinking about your preferred level of redistribution, what matters most to you? Please indicate what dimensions matter by giving scores below that add up to 100.* Answer option texts are identical to graph labels. Standard errors are approximately 0.5%.

Figure E11: Share of subjects that rank a given motive first



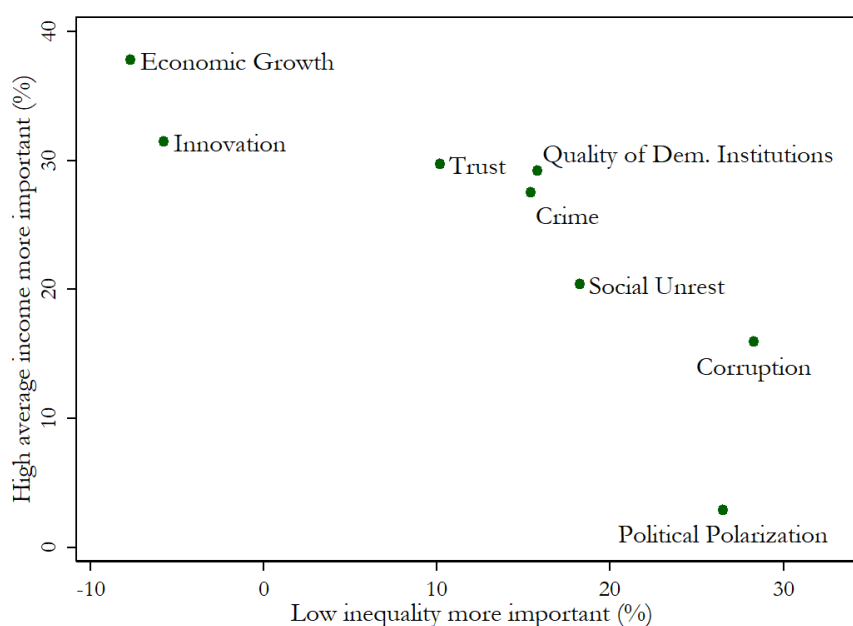
Note. Share of respondents who ranked the given motive weakly first. When a respondent ranked several motives equally, all are counted (which means the total percentage is above 100%). Question text: *When thinking about your preferred level of redistribution, what matters most to you? Please indicate what dimensions matter by giving scores below that add up to 100.* Answer option texts are identical to graph labels.

Figure E12: Share of points going to each motive conditional on the given motive attaining the highest share of points



Question text: *When thinking about your preferred level of redistribution, what matters most to you? Please indicate what dimensions matter by giving scores below that add up to 100.* Answer option texts are identical to graph labels.

Figure E13: Predictive power of average income and economic inequality in explaining respondents' predicted outcomes



Note. This graph is based on a set of questions of the type; *Do you think an average society with a high level of average income and a low level of economic inequality would have (compared to other countries): A [very low/low/average/high/very high] level of [output].* Each respondent in Survey 2 was shown one of these questions for the output they were specifically asked to evaluate; the level of inequality and average income was randomized. The graph illustrates the increased share of respondents who answer a negative outcome (e.g. “a high” or “a very high” level of crime) when told that the average income is [low/high] (the y-axis) or that the level of income inequality is [low/high] (the x-axis). The graph thus shows how the level of average income or inequality affects the prediction of respondents for the outcome. Each data point is elicited from ~ 200 respondents. This means that approximately 50 respondents received each type of question (where a type of question indicates an outcome, an income level, and an inequality level – for example crime with low inequality and high average income). Standard errors roughly are 0.5%.

F Tables

Table F1: Distribution of Inequality Externality Beliefs

	Crime	Corr- uption	Pol. polar.	Social unrest	Unemp- loyment	Inno- vation	Econ. growth	Public goods	Quality of life	Dem. inst.	Trust
Increases	74%	66%	66%	70%	53%	26%	23%	14%	14%	12%	10%
No change	17%	23%	23%	18%	30%	35%	25%	28%	26%	30%	23%
Decreases	9%	11%	12%	12%	17%	40%	51%	58%	59%	57%	67%
Respondents	3,292	2,994	2,990	3,292	641	3,017	3,292	643	628	3,065	3,292

Note. The corresponding table to Figure 2. Shows the distribution of specific externality beliefs for the full sample (control group of Survey 1 and all of Survey 2). “Increase” is the share of respondents that state that inequality “increases a lot” or “increases somewhat” the outcome. “No change” is the share of respondents that state that inequality does not induce a change on the outcome. “Decrease” is the share of respondents that state that inequality “decreases a lot” or “decreases somewhat” the outcome. Passive control respondents were asked every question, while active control respondents were asked the crime, trust, social unrest, and economic growth questions along with a random subset of three additional questions. For the equivalent table using only data from Survey 1 or see Tables F2 and F3.

Table F2: Distribution of Externality Beliefs in Survey 1 (Control Group)

	Crime	Corr- uption	Pol. polar.	Social unrest	Unemp- loyment	Inno- vation	Econ. growth	Public goods	Quality of life	Dem. inst.	Trust
Increases	76%	69%	68%	68%	53%	22%	19%	14%	14%	12%	10%
No change	16%	20%	23%	20%	30%	36%	29%	28%	26%	32%	22%
Decreases	8%	11%	10%	12%	17%	42%	52%	58%	59%	56%	68%
Respondents	932	634	630	932	641	657	932	643	628	705	932

Note. This is the corresponding table to Table F1 for Survey 1 respondents, control group only. Shows the distribution of specific externality beliefs in Survey 1. “Increase” is the share of respondents that state that inequality “increases a lot” or “increases somewhat” the outcome. “No change” is the share of respondents that state that inequality does not induce a change on the outcome. “Decrease” is the share of respondents that state that inequality “decreases a lot” or “decreases somewhat” the societal outcome. Passive control respondents were asked every question, while active control respondents were asked the crime, trust, social unrest, and economic growth questions along with a random subset of three additional questions. For the equivalent table in Survey 2 see Table F3.

Table F3: Distribution of Externality Beliefs in Survey 2

	Crime	Corr- uption	Pol. polar.	Social unrest	Inno- vation	Econ. growth	Dem. inst.	Trust	Placebo	Attent. Incr.	Attent. Decr.
Increases	74%	65%	65%	71%	27%	25%	13%	10%	5%	95%	3%
No change	16%	24%	23%	17%	35%	23%	30%	23%	89%	2%	2 %
Decreases	9%	11%	12%	12%	38%	52%	58%	67%	6%	3%	95%
Respondents	2,360	2,360	2,360	2,360	2,360	2,360	2,360	2,360	2,360	2,360	2,360

Note. This is the corresponding table to Table F1 for Survey 2 respondents. Shows the distribution of specific externality beliefs in Survey 2. “Increase” is the share of respondents that state that inequality “increases a lot” or “increases somewhat” the outcome. “No change” is the share of respondents that state that inequality does not induce a change on the outcome. “Decrease” is the share of respondents that state that inequality “decreases a lot” or “decreases somewhat” the outcome. For the equivalent table in Survey 1 see Table F2.

Table F4: Balance table for posterior externality beliefs

Variable	(1) Passive Control	(2) Active Control	(3) Difference
General neg. ext.	0.582 (0.494)	0.614 (0.487)	0.032 (0.032)
Ineq. incr. crime	0.757 (0.430)	0.761 (0.427)	0.005 (0.028)
Ineq. red. trust	0.669 (0.471)	0.698 (0.460)	0.029 (0.031)
Ineq. incr. growth	0.190 (0.392)	0.193 (0.395)	0.003 (0.026)
Society is unfair (post)	0.587 (0.493)	0.609 (0.489)	0.022 (0.033)
Rich because of hard work	0.392 (0.489)	0.383 (0.487)	-0.009 (0.032)
Observations	538	394	932

Note. This table represents mean (standard deviations) for posterior externality beliefs of respondents in the active (column 1) and passive (column 2) control groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F5: Balance table for prior views and values

Variable	(1) Passive Control	(2) Active Control	(3) Difference
Prior belief fair	0.481 (0.500)	0.492 (0.501)	0.011 (0.033)
Belief uneq countr. worse.	0.584 (0.493)	0.617 (0.487)	0.033 (0.032)
Trusts the government	0.288 (0.453)	0.327 (0.470)	0.039 (0.031)
Belief work less if tax	0.400 (0.490)	0.376 (0.485)	-0.024 (0.032)
Observations	538	394	932

Note. This table represents mean (standard deviations) for posterior fairness views of respondents in the active (column 1) and passive (column 2) control groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F6: Balance table for observable characteristics

Variable	(1) Passive Control	(2) Active Control	(3) Difference
Leans Republican	0.532 (0.499)	0.492 (0.501)	-0.039 (0.033)
Prior belief unfair	0.519 (0.500)	0.508 (0.501)	-0.011 (0.033)
Trusts the government	0.288 (0.453)	0.327 (0.470)	0.039 (0.031)
Male	0.498 (0.500)	0.495 (0.501)	-0.003 (0.033)
Black	0.087 (0.283)	0.081 (0.274)	-0.006 (0.018)
Neither black or white	0.162 (0.369)	0.107 (0.309)	-0.055** (0.022)
Income: 0-25k	0.214 (0.410)	0.236 (0.425)	0.022 (0.028)
Income: 25-50k	0.331 (0.471)	0.249 (0.433)	-0.082*** (0.030)
Income: 50-100k	0.257 (0.437)	0.312 (0.464)	0.056* (0.030)
Income: 100k and more	0.199 (0.400)	0.203 (0.403)	0.004 (0.027)
Age 30-39	0.164 (0.370)	0.188 (0.391)	0.024 (0.025)
Age 40-49	0.182 (0.386)	0.150 (0.357)	-0.032 (0.025)
Age 50-59	0.128 (0.335)	0.147 (0.355)	0.019 (0.023)
Age 60-69	0.175 (0.380)	0.162 (0.369)	-0.012 (0.025)
Age 70 and above	0.206 (0.405)	0.223 (0.417)	0.017 (0.027)
4-year college degree or more	0.459 (0.499)	0.513 (0.500)	0.054 (0.033)
Unemployed	0.099 (0.298)	0.107 (0.309)	0.008 (0.020)
Outside the labor force	0.457 (0.499)	0.431 (0.496)	-0.026 (0.033)
West	0.258 (0.438)	0.206 (0.405)	-0.053* (0.028)
North-East	0.138 (0.345)	0.190 (0.393)	0.053** (0.025)
Midwest	0.238 (0.426)	0.228 (0.420)	-0.009 (0.028)
Observations	538	394	932

Note. This table represents mean (standard deviations) for sociodemographic variables of respondents in the active (column 1) and passive (column 2) control groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F7: Balance table Trust vs. Control

Variable	(1) Control	(2) Crime	(3) Difference
Leans Republican	0.515 (0.500)	0.525 (0.500)	0.010 (0.023)
Prior belief unfair	0.514 (0.500)	0.529 (0.499)	0.016 (0.023)
Trusts the government	0.305 (0.461)	0.285 (0.452)	-0.020 (0.021)
Male	0.497 (0.500)	0.466 (0.499)	-0.031 (0.023)
Black	0.085 (0.279)	0.095 (0.294)	0.011 (0.013)
Neither black or white	0.138 (0.346)	0.128 (0.334)	-0.011 (0.016)
Income: 0-25k	0.223 (0.417)	0.235 (0.424)	0.012 (0.019)
Income: 25-50k	0.296 (0.457)	0.267 (0.443)	-0.029 (0.021)
Income: 50-100k	0.280 (0.449)	0.307 (0.461)	0.026 (0.021)
Income: 100k and more	0.201 (0.401)	0.192 (0.394)	-0.009 (0.018)
Age 30-39	0.174 (0.379)	0.158 (0.365)	-0.016 (0.017)
Age 40-49	0.168 (0.374)	0.166 (0.372)	-0.002 (0.017)
Age 50-59	0.136 (0.343)	0.144 (0.351)	0.007 (0.016)
Age 60-69	0.170 (0.375)	0.182 (0.386)	0.013 (0.018)
Age 70 and above	0.214 (0.410)	0.211 (0.408)	-0.002 (0.019)
4-year college degree or more	0.482 (0.500)	0.498 (0.500)	0.017 (0.023)
Unemployed	0.102 (0.303)	0.093 (0.291)	-0.009 (0.014)
Outside the labor force	0.446 (0.497)	0.426 (0.495)	-0.021 (0.023)
West	0.236 (0.425)	0.269 (0.444)	0.033 (0.020)
North-East	0.160 (0.367)	0.166 (0.372)	0.006 (0.017)
Midwest	0.234 (0.424)	0.175 (0.380)	-0.059*** (0.019)
Prior belief unfair	0.514 (0.500)	0.529 (0.499)	0.016 (0.023)
Belief work less if tax	0.389 (0.488)	0.372 (0.484)	-0.018 (0.022)
Trusts the government	0.305 (0.461)	0.285 (0.452)	-0.020 (0.021)
Belief pay less than prod.	0.734 (0.442)	0.741 (0.439)	0.007 (0.020)
Belief uneq countr. worse.	0.598 (0.491)	0.643 (0.479)	0.045** (0.022)
Observations	932	933	1,865

Note. This table represents mean (standard deviations) for pre-treatment beliefs and characteristics in the Control (column 1) and Crime (column 2) groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F8: Balance table Trust vs. Control

Variable	(1) Control	(2) Trust	(3) Difference
Leans Republican	0.515 (0.500)	0.527 (0.500)	0.012 (0.024)
Prior belief unfair	0.514 (0.500)	0.526 (0.500)	0.012 (0.024)
Trusts the government	0.305 (0.461)	0.325 (0.469)	0.020 (0.022)
Male	0.497 (0.500)	0.476 (0.500)	-0.020 (0.024)
Black	0.085 (0.279)	0.103 (0.304)	0.018 (0.014)
Neither black or white	0.138 (0.346)	0.127 (0.333)	-0.011 (0.016)
Income: 0-25k	0.223 (0.417)	0.227 (0.419)	0.003 (0.020)
Income: 25-50k	0.296 (0.457)	0.320 (0.467)	0.024 (0.022)
Income: 50-100k	0.280 (0.449)	0.282 (0.450)	0.002 (0.022)
Income: 100k and more	0.201 (0.401)	0.171 (0.377)	-0.030 (0.019)
Age 30-39	0.174 (0.379)	0.172 (0.378)	-0.002 (0.018)
Age 40-49	0.168 (0.374)	0.166 (0.372)	-0.002 (0.018)
Age 50-59	0.136 (0.343)	0.145 (0.353)	0.009 (0.017)
Age 60-69	0.170 (0.375)	0.164 (0.370)	-0.006 (0.018)
Age 70 and above	0.214 (0.410)	0.213 (0.410)	-0.000 (0.020)
4-year college degree or more	0.482 (0.500)	0.468 (0.499)	-0.014 (0.024)
Unemployed	0.102 (0.303)	0.099 (0.299)	-0.003 (0.014)
Outside the labor force	0.446 (0.497)	0.455 (0.498)	0.008 (0.024)
West	0.236 (0.425)	0.248 (0.432)	0.012 (0.021)
North-East	0.160 (0.367)	0.162 (0.369)	0.003 (0.018)
Midwest	0.234 (0.424)	0.215 (0.411)	-0.019 (0.020)
Prior belief unfair	0.514 (0.500)	0.526 (0.500)	0.012 (0.024)
Belief work less if tax	0.389 (0.488)	0.364 (0.481)	-0.026 (0.023)
Trusts the government	0.305 (0.461)	0.325 (0.469)	0.020 (0.022)
Belief pay less than prod.	0.734 (0.442)	0.772 (0.420)	0.038* (0.021)
Belief uneq countr. worse.	0.598 (0.491)	0.636 (0.481)	0.039* (0.023)
Observations	932	825	1,757

Note. This table represents mean (standard deviations) for pre-treatment beliefs and characteristics in the Control (column 1) and Trust (column 2) groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F9: Balance table Full ext. vs. Control

Variable	(1) Control	(2) FullExt	(3) Difference
Leans Republican	0.515 (0.500)	0.507 (0.500)	-0.008 (0.024)
Prior belief unfair	0.514 (0.500)	0.523 (0.500)	0.009 (0.024)
Trusts the government	0.305 (0.461)	0.303 (0.460)	-0.002 (0.022)
Male	0.497 (0.500)	0.497 (0.500)	0.000 (0.024)
Black	0.085 (0.279)	0.091 (0.288)	0.007 (0.014)
Neither black or white	0.138 (0.346)	0.158 (0.365)	0.020 (0.017)
Income: 0-25k	0.223 (0.417)	0.216 (0.412)	-0.007 (0.020)
Income: 25-50k	0.296 (0.457)	0.290 (0.454)	-0.006 (0.022)
Income: 50-100k	0.280 (0.449)	0.335 (0.472)	0.055** (0.022)
Income: 100k and more	0.201 (0.401)	0.158 (0.365)	-0.042** (0.018)
Age 30-39	0.174 (0.379)	0.168 (0.374)	-0.006 (0.018)
Age 40-49	0.168 (0.374)	0.180 (0.385)	0.012 (0.018)
Age 50-59	0.136 (0.343)	0.133 (0.340)	-0.003 (0.016)
Age 60-69	0.170 (0.375)	0.177 (0.382)	0.007 (0.018)
Age 70 and above	0.214 (0.410)	0.188 (0.391)	-0.026 (0.019)
4-year college degree or more	0.482 (0.500)	0.533 (0.499)	0.051** (0.024)
Unemployed	0.102 (0.303)	0.083 (0.276)	-0.019 (0.014)
Outside the labor force	0.446 (0.497)	0.403 (0.491)	-0.043* (0.024)
West	0.236 (0.425)	0.245 (0.430)	0.009 (0.021)
North-East	0.160 (0.367)	0.153 (0.360)	-0.007 (0.017)
Midwest	0.234 (0.424)	0.227 (0.419)	-0.006 (0.020)
Prior belief unfair	0.514 (0.500)	0.523 (0.500)	0.009 (0.024)
Belief work less if tax	0.389 (0.488)	0.350 (0.477)	-0.040* (0.023)
Trusts the government	0.305 (0.461)	0.303 (0.460)	-0.002 (0.022)
Belief pay less than prod.	0.734 (0.442)	0.776 (0.417)	0.042** (0.021)
Belief uneq countr. worse.	0.598 (0.491)	0.616 (0.487)	0.018 (0.023)
Observations	932	809	1,741

Note. This table represents mean (standard deviations) for pre-treatment beliefs and characteristics in the Control (column 1) and Full Externality (column 2) groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F10: Balance table Fairness vs. Control

Variable	(1) Control	(2) Fairness	(3) Difference
Leans Republican	0.515 (0.500)	0.526 (0.500)	0.011 (0.024)
Prior belief unfair	0.514 (0.500)	0.500 (0.500)	-0.014 (0.024)
Trusts the government	0.305 (0.461)	0.275 (0.447)	-0.029 (0.021)
Male	0.497 (0.500)	0.540 (0.499)	0.043* (0.024)
Black	0.085 (0.279)	0.096 (0.295)	0.012 (0.014)
Neither black or white	0.138 (0.346)	0.148 (0.355)	0.010 (0.017)
Income: 0-25k	0.223 (0.417)	0.208 (0.406)	-0.016 (0.019)
Income: 25-50k	0.296 (0.457)	0.271 (0.445)	-0.025 (0.021)
Income: 50-100k	0.280 (0.449)	0.321 (0.467)	0.041* (0.022)
Income: 100k and more	0.201 (0.401)	0.201 (0.401)	0.000 (0.019)
Age 30-39	0.174 (0.379)	0.159 (0.366)	-0.014 (0.018)
Age 40-49	0.168 (0.374)	0.175 (0.381)	0.007 (0.018)
Age 50-59	0.136 (0.343)	0.151 (0.359)	0.015 (0.017)
Age 60-69	0.170 (0.375)	0.178 (0.383)	0.008 (0.018)
Age 70 and above	0.214 (0.410)	0.206 (0.405)	-0.007 (0.019)
4-year college degree or more	0.482 (0.500)	0.514 (0.500)	0.032 (0.024)
Unemployed	0.102 (0.303)	0.094 (0.292)	-0.008 (0.014)
Outside the labor force	0.446 (0.497)	0.436 (0.496)	-0.011 (0.023)
West	0.236 (0.425)	0.221 (0.415)	-0.015 (0.020)
North-East	0.160 (0.367)	0.156 (0.363)	-0.004 (0.017)
Midwest	0.234 (0.424)	0.212 (0.409)	-0.022 (0.020)
Prior belief unfair	0.514 (0.500)	0.500 (0.500)	-0.014 (0.024)
Belief work less if tax	0.389 (0.488)	0.354 (0.479)	-0.035 (0.023)
Trusts the government	0.305 (0.461)	0.275 (0.447)	-0.029 (0.021)
Belief pay less than prod.	0.734 (0.442)	0.740 (0.439)	0.006 (0.021)
Belief uneq countr. worse.	0.598 (0.491)	0.576 (0.495)	-0.022 (0.023)
Observations	932	872	1,804

Note. This table represents mean (standard deviations) for pre-treatment beliefs and characteristics in the Control (column 1) and Fairness (column 2) groups. Column (3) characterizes the difference across the two. *Significance levels:* *10%, **5%, ***1%.

Table F11: Definitional text for externality questions

Externality	Additional definition
The amount of crime	<i>Note: When we say the amount of crime we mean the overall crime rate, including homicides, robberies, property crime and more.</i>
The overall level of trust	<i>Note: When we say the total level of trust we mean the strength of a country's social fabric. Some examples are whether most people trust others, whether people cooperate with each other, how many people return lost wallets, and so on.</i>
The amount of social unrest	None
The rate of economic growth	None
The amount of corruption	None
The overall amount of unemployment	None
The overall amount of innovation	None
The overall quality of life	<i>Note: Here we want you to compare between people with the same incomes living in more or less unequal societies.</i>
The overall amount of political polarization	<i>Note: When we say political polarization we mean to what extent people's and politicians' opinions are divided on political issues, as well as how strong these divisions are.</i>
The quality of democratic institutions	<i>Note: When we say the quality of democratic institutions we mean the capable and equitable functioning of the political system, the avoidance of abuses of power, the equality of the rule of law, whether civil liberties are respected, and so on.</i>
The quality of local public goods	<i>Note: When we say the quality of local public goods we mean the quality of things like schools, local government services, parks, youth centers and more.</i>

Table F12: Definitional text for externality questions, secondary study

Externality	Additional definition
The amount of crime	<i>Note: When we say the amount of crime we mean the overall crime rate, including homicides, robberies, property crime and more.</i>
The overall level of trust	<i>Note: When we say the total level of trust we mean the strength of a country's social fabric. Some examples are whether most people trust others, whether people cooperate with each other, and so on.</i>
The amount of social unrest	<i>Note: By social unrest we mean unconventional and sometimes violent forms of collective behavior that disrupt the typical social order in society.</i>
The rate of economic growth	<i>Note: By economic growth we mean the increase in the production of goods and services in the society.</i>
The amount of corruption	<i>Note: By corruption we mean dishonest or fraudulent acts committed by those in power, usually in the form of accepting bribes.</i>
The overall amount of innovation	<i>Note: By innovation we mean how many new technologies and products that are developed in the society.</i>
The overall amount of political polarization	<i>Note: When we say political polarization we mean the extent to which opinions are divided on political issues, both among most people and politicians, in addition to how strong these differences are and whether people with different views speak together. Increasing polarization means that there is generally less agreement in society.</i>
The quality of democratic institutions	<i>Note: When we say the quality of democratic institutions we mean the capable and equitable functioning of the political system, the avoidance of abuses of power, the equality of the rule of law, whether civil liberties are respected, and so on.</i>
Daylight (placebo) hours	<i>Note: By the number of daylight hours we mean the number of hours when the sun is visible within a country on an average day.</i>
Attention check #1	<i>Note: Here we just want you to choose the top option to show that you are reading the questions. Thank you.</i>
Attention check #2	<i>Note: Here we just want you to choose the bottom option to show that you are reading the questions. Thank you.</i>

Table F13: Main correlations of sociodemographic and externality beliefs

	(1) General neg. ext. b/se	(2) Ineq. incr. crime b/se	(3) Ineq. red. trust b/se	(4) Ineq. red. growth b/se
Leans Republican	-0.134*** (0.018)	-0.091*** (0.017)	-0.118*** (0.018)	-0.120*** (0.020)
Prior belief unfair	0.299*** (0.018)	0.193*** (0.016)	0.239*** (0.017)	0.210*** (0.019)
Trusts the government	0.037** (0.018)	0.051*** (0.017)	0.040** (0.018)	0.051** (0.020)
Male	-0.032* (0.017)	0.002 (0.016)	0.007 (0.017)	-0.044** (0.018)
Black	-0.072** (0.031)	-0.073** (0.029)	-0.016 (0.031)	-0.022 (0.034)
Neither black nor white	-0.047** (0.022)	-0.044** (0.021)	-0.016 (0.022)	0.027 (0.024)
Income: 25-50k	0.041 (0.025)	0.036 (0.023)	0.020 (0.025)	0.003 (0.027)
Income: 50-100k	0.034 (0.025)	0.031 (0.023)	0.015 (0.024)	-0.006 (0.027)
Income: 100k and more	-0.005 (0.027)	0.007 (0.026)	-0.030 (0.027)	-0.079*** (0.029)
Age 30-39	-0.013 (0.029)	-0.009 (0.028)	0.014 (0.030)	-0.017 (0.032)
Age 40-49	-0.026 (0.031)	-0.002 (0.029)	0.038 (0.030)	0.035 (0.033)
Age 50-59	0.016 (0.031)	0.003 (0.029)	0.043 (0.031)	0.058* (0.033)
Age 60-69	-0.053* (0.031)	-0.021 (0.030)	-0.004 (0.031)	-0.006 (0.034)
Age 70 and above	-0.052 (0.035)	-0.036 (0.032)	0.027 (0.035)	-0.003 (0.037)
4-year college degree or more	0.045** (0.018)	0.050*** (0.017)	0.042** (0.018)	0.010 (0.019)
Unemployed	-0.039 (0.031)	-0.040 (0.031)	0.002 (0.031)	-0.066* (0.034)
Outside the labor force	-0.000 (0.020)	0.000 (0.019)	-0.013 (0.020)	-0.019 (0.021)
West	0.041** (0.020)	0.043** (0.019)	0.046** (0.020)	0.015 (0.021)
North-East	0.032 (0.024)	0.030 (0.023)	-0.000 (0.025)	0.007 (0.026)
Midwest	-0.023 (0.022)	0.014 (0.021)	0.022 (0.022)	-0.036 (0.023)
Constant	0.527*** (0.038)	0.634*** (0.035)	0.539*** (0.038)	0.497*** (0.040)
Adjusted R2	0.160	0.087	0.111	0.094
Observations	3292	3292	3292	3292

Note. This table reports results from regressions that regress externality beliefs on sociodemographic variables. Sample is composed of Survey 1 control group respondents and Survey 2 respondents. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F14: Correlations of sociodemographic and externality beliefs, 2

	(1)	(2)	(3)	(4)
	Ineq. red. inno. b/se	Ineq. incr. unrest b/se	Ineq. worsens dem. inst. b/se	Ineq. worsens public goods b/se
Leans Republican	-0.093*** (0.020)	-0.082*** (0.018)	-0.159*** (0.020)	-0.098** (0.044)
Prior belief unfair	0.177*** (0.019)	0.211*** (0.017)	0.224*** (0.019)	0.200*** (0.041)
Trusts the government	0.028 (0.022)	0.062*** (0.018)	0.026 (0.020)	-0.035 (0.044)
Male	-0.007 (0.019)	-0.010 (0.016)	0.022 (0.018)	-0.059 (0.042)
Black	-0.081** (0.035)	-0.082*** (0.031)	-0.071** (0.034)	0.045 (0.074)
Neither black nor white	0.014 (0.025)	-0.061*** (0.022)	-0.034 (0.024)	0.073 (0.055)
Income: 25-50k	-0.021 (0.028)	0.008 (0.024)	0.009 (0.027)	-0.004 (0.053)
Income: 50-100k	-0.016 (0.028)	0.034 (0.024)	0.006 (0.027)	-0.012 (0.057)
Income: 100k and more	-0.086*** (0.030)	-0.007 (0.026)	-0.027 (0.030)	-0.088 (0.066)
Age 30-39	-0.026 (0.034)	-0.063** (0.029)	0.024 (0.033)	-0.066 (0.070)
Age 40-49	0.019 (0.035)	0.009 (0.030)	0.032 (0.033)	0.015 (0.069)
Age 50-59	-0.053 (0.035)	0.006 (0.030)	0.063* (0.034)	0.004 (0.078)
Age 60-69	-0.051 (0.035)	-0.002 (0.030)	0.040 (0.034)	0.004 (0.074)
Age 70 and above	-0.050 (0.039)	0.042 (0.033)	0.007 (0.038)	0.028 (0.076)
4-year college degree or more	0.021 (0.019)	0.068*** (0.017)	0.055*** (0.019)	0.094** (0.042)
Unemployed	0.028 (0.036)	-0.045 (0.032)	0.007 (0.035)	-0.130* (0.070)
Outside the labor force	-0.018 (0.022)	-0.020 (0.020)	0.002 (0.022)	0.022 (0.049)
West	0.010 (0.022)	0.064*** (0.020)	0.020 (0.022)	0.013 (0.050)
North-East	-0.001 (0.027)	0.039 (0.024)	-0.006 (0.027)	-0.028 (0.060)
Midwest	-0.038 (0.024)	0.030 (0.022)	-0.040* (0.024)	-0.057 (0.052)
Constant	0.407*** (0.043)	0.571*** (0.037)	0.478*** (0.041)	0.548*** (0.084)
Adjusted R2	0.067	0.095	0.105	0.070
Observations	3017	3292	3065	643

Note. This table reports results from regressions that regress externality beliefs on sociodemographic variables. Sample is composed of Survey 1 control group respondents and Survey 2 respondents. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F15: Correlations of sociodemographic and externality beliefs, 3

	(1)	(2)	(3)	(4)
	Ineq. incr. corruption	Ineq. incr. pol. pol.	Ineq. incr. unemp.	Ineq. decr. QoL
	b/se	b/se	b/se	b/se
Leans Republican	-0.097*** (0.019)	-0.103*** (0.020)	-0.172*** (0.044)	-0.202*** (0.044)
Prior belief unfair	0.216*** (0.019)	0.159*** (0.019)	0.213*** (0.041)	0.242*** (0.041)
Trusts the government	0.023 (0.020)	0.032* (0.020)	0.020 (0.046)	0.063 (0.043)
Male	-0.019 (0.018)	0.032* (0.018)	-0.013 (0.041)	0.041 (0.040)
Black	-0.057* (0.032)	-0.130*** (0.034)	0.032 (0.078)	-0.140* (0.075)
Neither black nor white	0.012 (0.023)	-0.019 (0.023)	-0.029 (0.055)	-0.018 (0.056)
Income: 25-50k	-0.017 (0.026)	-0.005 (0.027)	0.007 (0.054)	0.024 (0.054)
Income: 50-100k	-0.034 (0.026)	0.041 (0.027)	-0.009 (0.060)	0.010 (0.058)
Income: 100k and more	-0.085*** (0.029)	0.024 (0.029)	-0.039 (0.068)	0.004 (0.067)
Age 30-39	-0.041 (0.032)	-0.035 (0.033)	0.008 (0.070)	0.103 (0.072)
Age 40-49	-0.013 (0.032)	-0.008 (0.033)	0.027 (0.068)	0.113 (0.071)
Age 50-59	0.027 (0.032)	0.035 (0.034)	0.005 (0.073)	0.037 (0.082)
Age 60-69	-0.038 (0.034)	0.022 (0.034)	-0.074 (0.072)	0.097 (0.074)
Age 70 and above	-0.026 (0.037)	0.027 (0.037)	-0.005 (0.075)	0.048 (0.080)
4-year college degree or more	0.040** (0.019)	0.075*** (0.019)	-0.028 (0.043)	0.034 (0.042)
Unemployed	-0.075** (0.034)	0.003 (0.035)	0.029 (0.070)	0.089 (0.072)
Outside the labor force	-0.045** (0.022)	-0.003 (0.021)	0.027 (0.050)	0.070 (0.049)
West	0.025 (0.021)	0.064*** (0.022)	0.111** (0.049)	-0.004 (0.050)
North-East	0.017 (0.026)	-0.010 (0.027)	-0.020 (0.064)	-0.059 (0.057)
Midwest	-0.039* (0.024)	0.013 (0.024)	0.051 (0.051)	0.001 (0.050)
Constant	0.656*** (0.040)	0.532*** (0.041)	0.485*** (0.084)	0.419*** (0.088)
Adjusted R2	0.092	0.069	0.095	0.128
Observations	2994	2990	641	628

Note. This table reports results from regressions that regress externality beliefs on sociodemographic variables. Sample is composed of Survey 1 control group respondents and Survey 2 respondents. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F16: Non-monotonic beliefs

	Crime	Trust	Social unrest	Pol. polar.	Corr uption	Dem. inst.	Inno- vation	Econ. growth
Yes (<i>monotonic</i>)	87%	82%	82%	83%	80%	79%	75%	76%
No (<i>non-monotonic</i>)	13%	18%	18%	17%	20%	21%	25%	24%
Sample size	244	242	239	236	243	236	228	222

Note. The share of respondents who think their expressed inequality externality is non-monotonic in inequality level. Full question text: *In the earlier question you answered that “[Answer]”. Do you think this is true in any kind of country – no matter whether the country is initially very equal, very unequal, or anything else?*

Table F17: Treatment effects without controls

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Increase top taxes b/se	(4) Gov. reduce ineq. b/se	(5) Ineq. is serious issue b/se
Crime Ext. Tr.	0.036 (0.046)	0.031 (0.022)	-0.006 (0.023)	0.005 (0.023)	0.022 (0.023)
Trust Ext. Tr.	0.055 (0.047)	0.010 (0.023)	0.005 (0.024)	0.041* (0.024)	0.023 (0.024)
Full Ext. Tr.	0.124*** (0.048)	0.059** (0.023)	-0.014 (0.024)	0.056** (0.024)	0.078*** (0.024)
Fairness Tr.	0.173*** (0.047)	0.042* (0.023)	0.053** (0.023)	0.052** (0.024)	0.102*** (0.023)
Controls	No	No	No	No	No
R2	0.004	0.002	0.002	0.002	0.006
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from regressions that regress preferences for redistribution on treatment variables *without* controlling for other factors. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F18: Treatment effects with controls

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Increase top taxes	Gov. reduce ineq.	Ineq. is serious issue
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.037 (0.036)	0.031 (0.020)	-0.005 (0.021)	0.007 (0.020)	0.020 (0.019)
Trust Ext. Tr.	0.043 (0.037)	0.006 (0.021)	0.004 (0.022)	0.036* (0.020)	0.017 (0.020)
Full Ext. Tr.	0.107*** (0.037)	0.050** (0.021)	-0.012 (0.022)	0.048** (0.020)	0.069*** (0.020)
Fairness Tr.	0.208*** (0.037)	0.052** (0.021)	0.065*** (0.021)	0.067*** (0.020)	0.115*** (0.019)
Leans Republican	-0.635*** (0.030)	-0.190*** (0.017)	-0.210*** (0.016)	-0.264*** (0.016)	-0.249*** (0.016)
Prior belief unfair	0.707*** (0.027)	0.146*** (0.015)	0.260*** (0.015)	0.260*** (0.014)	0.350*** (0.015)
Trusts the government	0.174*** (0.028)	0.070*** (0.017)	0.016 (0.016)	0.115*** (0.015)	0.050*** (0.015)
Male	-0.138*** (0.026)	-0.056*** (0.015)	-0.061*** (0.015)	-0.036*** (0.014)	-0.046*** (0.013)
Black	0.016 (0.045)	0.081*** (0.028)	-0.124*** (0.026)	0.000 (0.026)	0.066*** (0.023)
Neither black or white	0.077** (0.037)	0.060*** (0.021)	-0.009 (0.021)	0.038* (0.020)	0.022 (0.019)
Income: 25-50k	0.018 (0.036)	-0.011 (0.020)	0.039* (0.020)	0.009 (0.019)	-0.012 (0.018)
Income: 50-100k	-0.084** (0.036)	-0.038* (0.020)	0.008 (0.020)	-0.038** (0.019)	-0.052*** (0.019)
Income: 100k and more	-0.131*** (0.042)	-0.055** (0.024)	-0.004 (0.024)	-0.048** (0.022)	-0.082*** (0.022)
Age 30-39	0.103** (0.046)	0.021 (0.027)	0.050* (0.026)	0.060** (0.025)	0.018 (0.024)
Age 40-49	0.024 (0.046)	-0.014 (0.027)	0.091*** (0.026)	-0.029 (0.025)	-0.013 (0.024)
Age 50-59	-0.046 (0.049)	-0.090*** (0.028)	0.114*** (0.027)	-0.055** (0.027)	-0.036 (0.026)
Age 60-69	-0.170*** (0.048)	-0.147*** (0.028)	0.119*** (0.027)	-0.132*** (0.026)	-0.084*** (0.025)
Age 70 and above	-0.274*** (0.050)	-0.183*** (0.028)	0.112*** (0.027)	-0.225*** (0.027)	-0.098*** (0.026)
4-year college degree or more	-0.041 (0.027)	-0.001 (0.015)	-0.012 (0.015)	-0.029** (0.014)	-0.018 (0.014)
Unemployed	0.029 (0.047)	-0.003 (0.026)	0.032 (0.026)	0.000 (0.025)	0.012 (0.024)
Outside the labor force	-0.029 (0.030)	-0.024 (0.017)	0.046*** (0.017)	-0.021 (0.016)	-0.042*** (0.016)
West	-0.018 (0.032)	-0.016 (0.018)	0.006 (0.018)	0.000 (0.017)	-0.016 (0.017)
North-East	0.113*** (0.036)	0.033 (0.021)	0.057*** (0.020)	0.051*** (0.019)	0.022 (0.019)
Midwest	0.010 (0.032)	-0.017 (0.018)	0.044** (0.018)	-0.010 (0.017)	-0.003 (0.017)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.391	0.169	0.170	0.293	0.313
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from regressions that regress preferences for redistribution on treatment variables and reporting all controls. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F19: Share of subjects mentioning “crime”, “trust” or “video” when asking them why they hold a certain general externality belief.

	Mentioned crime (%)	Mentioned trust (%)	Mentioned video (%)
Crime tr.	17.04	0.32	0.43
Trust tr.	4.48	6.30	0.12
Full ext tr.	13.23	3.71	0.37
Fairness tr.	4.13	0.23	0.00
Control (passive)	4.46	0.32	0.00
Control (active)	4.57	0.00	0.00

Table F20: First-stage effects of treatments

	(1) General neg. ext. b/se	(2) Ineq. incr. crime b/se	(3) Ineq. red. trust b/se	(4) Ineq. red. growth b/se	(5) Society unfair (post) b/se	(6) Rich b/c hard work b/se
Crime Ext. Tr.	0.088*** (0.021)	0.093*** (0.018)	0.059*** (0.020)	0.086*** (0.022)	0.012 (0.020)	-0.018 (0.020)
Trust Ext. Tr.	0.050** (0.021)	0.048** (0.019)	0.096*** (0.020)	0.076*** (0.023)	0.025 (0.020)	-0.028 (0.020)
Full Ext. Tr.	0.085*** (0.021)	0.084*** (0.019)	0.097*** (0.020)	0.062*** (0.023)	0.016 (0.020)	-0.030 (0.020)
Fairness Tr.	0.075*** (0.021)	0.017 (0.019)	0.037* (0.021)	0.033 (0.022)	0.079*** (0.020)	-0.079*** (0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.159	0.084	0.093	0.102	0.239	0.241
Observations	4371.000	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F21: Treatment effects with controls and population weights.

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Gov. reduce ineq. b/se	(4) Ineq. is serious issue b/se	(5) Increase top taxes b/se
Crime Ext. Tr.	-0.007 (0.051)	0.021 (0.029)	-0.023 (0.027)	0.006 (0.026)	-0.015 (0.028)
Trust Ext. Tr.	-0.008 (0.056)	-0.007 (0.031)	0.015 (0.029)	-0.005 (0.029)	-0.015 (0.030)
Full Ext. Tr.	0.091* (0.051)	0.064** (0.030)	0.035 (0.028)	0.052** (0.027)	-0.019 (0.030)
Fairness Tr.	0.148*** (0.050)	0.023 (0.030)	0.055** (0.027)	0.089*** (0.026)	0.045 (0.029)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.386	0.184	0.273	0.309	0.181
Observations	4363.000	4363.000	4363.000	4363.000	4363.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. Observations are reweighted to match representativity by gender, race, age, political affiliation, college degree, income-group, and geographic region. *Significance levels:* *10%, **5%, ***1%.

Table F22: Emotional reactions to treatments

	(1)	(2)	(3)	(4)	(5)
	Active control	Crime	Trust	Full externality	Fairness
Anger	2.8%	6.2%	2.9%	7.8%	11.8%
Concern	19.5%	37.2%	28.2%	32.0%	32.9%
Surprise	10.8%	13.9%	12.5%	13.0%	12.9%
Interest	41.5%	37.1%	42.2%	37.8%	34.0%
Indifference	17.7%	17.7%	19.2%	17.5%	17.9%
Confusion	16.9%	4.2%	6.0%	5.8%	4.5%
Observations	390	927	822	806	867

Table F23: First-stage effects of treatments with population weights

	(1)	(2)	(3)	(4)	(5)	(6)
	General neg. ext. b/se	Ineq. incr. crime b/se	Ineq. red. trust b/se	Ineq. red. growth b/se	Society unfair (post) b/se	Rich b/c hard work b/se
Crime Ext. Tr.	0.061** (0.028)	0.074*** (0.025)	0.031 (0.027)	0.049* (0.029)	0.014 (0.026)	0.006 (0.027)
Trust Ext. Tr.	0.003 (0.031)	0.059** (0.027)	0.090*** (0.028)	0.058* (0.031)	0.033 (0.028)	0.036 (0.028)
Full Ext. Tr.	0.088*** (0.028)	0.101*** (0.026)	0.106*** (0.027)	0.057* (0.031)	0.015 (0.027)	0.037 (0.028)
Fairness Tr.	0.073*** (0.028)	0.019 (0.027)	0.033 (0.029)	0.003 (0.030)	0.062** (0.027)	0.086*** (0.026)
Controls						
R2	0.170	0.091	0.097	0.103	0.246	0.233
Observations	4363.000	4363.000	4363.000	4363.000	4363.000	4363.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. Observations are reweighted to match representativity by gender, race, age, political affiliation, college degree, income-group, and geographic region. *Significance levels:* *10%, **5%, ***1%.

Table F24: Mediation analysis: Treatment effects including beliefs as regressors

	(1)	(2)	(3)	(4)
	RP Index b/se	RP Index b/se	RP Index b/se	RP Index b/se
Crime Ext. Tr.	0.037 (0.036)	-0.018 (0.035)	0.024 (0.034)	-0.008 (0.033)
Trust Ext. Tr.	0.043 (0.037)	0.013 (0.036)	0.019 (0.034)	0.004 (0.034)
Full Ext. Tr.	0.107*** (0.037)	0.055 (0.036)	0.087** (0.034)	0.058* (0.034)
Fairness Tr.	0.208*** (0.037)	0.170*** (0.035)	0.135*** (0.034)	0.122*** (0.033)
General neg. ext.		0.468*** (0.029)		0.301*** (0.028)
Ineq. incr. crime		0.149*** (0.032)		0.076** (0.030)
Society is unfair (post)			0.508*** (0.029)	0.414*** (0.030)
Rich because of hard luck			0.406*** (0.029)	0.372*** (0.029)
Controls	Yes	Yes	Yes	Yes
R2	0.391	0.442	0.489	0.507
Observations	4371	4371	4371	4371

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and post-treatment inequality beliefs and fairness views, as well as socio-economic control variables. Controls not listed include pre-treatment fairness views, race, income-group, age-group, gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F25: Treatment effects interacted with prior externality belief

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Increase top taxes b/se	(4) Gov. reduce ineq. b/se	(5) Ineq. is serious issue b/se
Crime Ext. Tr.	0.121** (0.059)	0.062* (0.032)	0.021 (0.035)	0.023 (0.032)	0.068** (0.031)
Trust Ext. Tr.	0.090 (0.059)	0.017 (0.033)	-0.018 (0.035)	0.059* (0.032)	0.072** (0.032)
Full Ext. Tr.	0.137** (0.058)	0.074** (0.033)	0.001 (0.035)	0.030 (0.032)	0.091*** (0.032)
Fairness Tr.	0.220*** (0.056)	0.045 (0.031)	0.069** (0.034)	0.063** (0.031)	0.139*** (0.030)
Crime*Unequal countries function worse	-0.153** (0.074)	-0.054 (0.042)	-0.049 (0.044)	-0.031 (0.041)	-0.086** (0.040)
Trust*Unequal countries function worse	-0.091 (0.075)	-0.022 (0.043)	0.028 (0.045)	-0.042 (0.041)	-0.096** (0.041)
Full Ext*Unequal countries function worse	-0.056 (0.075)	-0.041 (0.043)	-0.025 (0.044)	0.026 (0.041)	-0.040 (0.041)
Fairness*Unequal countries function worse	-0.013 (0.073)	0.014 (0.043)	-0.004 (0.043)	0.010 (0.040)	-0.038 (0.039)
Belief uneq countr. worse.	0.314*** (0.051)	0.081*** (0.030)	0.116*** (0.031)	0.102*** (0.028)	0.153*** (0.028)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.405	0.173	0.181	0.302	0.323
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and their interaction with pre-treatment externality view. Controls not listed include pre-treatment fairness views, race, income-group, age-group, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F26: Treatment effects interacted with those that say they learned something new in the video

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Increase top taxes b/se	(4) Gov. reduce ineq. b/se	(5) Ineq. is serious issue b/se
Crime Ext. Tr.	-0.152*** (0.055)	-0.049* (0.029)	-0.052 (0.032)	-0.064** (0.029)	-0.054* (0.029)
Trust Ext. Tr.	-0.046 (0.056)	-0.053* (0.031)	0.033 (0.034)	-0.024 (0.030)	-0.022 (0.030)
Full Ext. Tr.	-0.057 (0.059)	-0.060* (0.032)	-0.009 (0.036)	-0.025 (0.032)	0.011 (0.032)
Fairness Tr.	0.012 (0.057)	0.006 (0.031)	0.028 (0.033)	-0.053* (0.030)	0.036 (0.030)
Learned something new	0.097* (0.053)	-0.010 (0.031)	0.088*** (0.031)	0.033 (0.029)	0.029 (0.030)
Crime*Learned something new	0.220*** (0.077)	0.121*** (0.043)	0.021 (0.044)	0.085** (0.041)	0.090** (0.041)
Trust*Learned something new	0.077 (0.079)	0.090** (0.045)	-0.087* (0.046)	0.069 (0.042)	0.039 (0.043)
FullExt*Learned something new	0.174** (0.080)	0.156*** (0.045)	-0.053 (0.047)	0.082* (0.043)	0.064 (0.044)
Fairness*Learned something new	0.231*** (0.078)	0.071 (0.045)	0.008 (0.045)	0.156*** (0.042)	0.098** (0.041)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.403	0.176	0.175	0.305	0.320
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and their interaction with self-reported indicator to have learned something new. Controls not listed include pre-treatment fairness views, race, income-group, age-group, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F27: Treatment effects interacted with male dummy

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Increase top taxes	Gov. reduce ineq.	Ineq. is serious issue
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.024 (0.051)	0.049* (0.030)	-0.019 (0.029)	0.014 (0.028)	-0.010 (0.027)
Trust Ext. Tr.	0.012 (0.051)	0.005 (0.031)	-0.012 (0.030)	0.036 (0.029)	-0.011 (0.028)
Full Ext. Tr.	0.010 (0.052)	0.048 (0.032)	-0.088*** (0.030)	0.016 (0.029)	0.039 (0.028)
Fairness Tr.	0.194*** (0.053)	0.072** (0.032)	0.052* (0.030)	0.071** (0.029)	0.085*** (0.028)
Male	-0.198*** (0.051)	-0.041 (0.030)	-0.108*** (0.030)	-0.044 (0.028)	-0.092*** (0.028)
CrimeXmale	0.024 (0.073)	-0.038 (0.041)	0.027 (0.042)	-0.015 (0.040)	0.060 (0.039)
TrustXmale	0.062 (0.075)	0.003 (0.043)	0.032 (0.043)	-0.001 (0.040)	0.056 (0.040)
FullExtXmale	0.196*** (0.075)	0.004 (0.043)	0.153*** (0.043)	0.065 (0.040)	0.060 (0.040)
FairnessXmale	0.030 (0.074)	-0.038 (0.043)	0.029 (0.042)	-0.006 (0.040)	0.059 (0.039)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.392	0.169	0.173	0.294	0.314
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and their interaction with a male dummy. Controls not listed include pre-treatment fairness views, race, income-group, age-group, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F28: Treatment effects interacted with having a yearly income above \$50,000

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Ineq. is serious issue	Gov. reduce ineq.	Increase top taxes
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.041 (0.052)	0.016 (0.030)	0.035 (0.027)	-0.006 (0.029)	0.013 (0.029)
Crime*Income50k	-0.009 (0.073)	0.030 (0.041)	-0.033 (0.039)	0.027 (0.040)	-0.037 (0.042)
Trust Ext. Tr.	0.067 (0.053)	-0.001 (0.030)	0.043 (0.028)	0.024 (0.029)	0.030 (0.030)
Trust*Income50k	-0.049 (0.074)	0.015 (0.043)	-0.055 (0.040)	0.026 (0.040)	-0.055 (0.043)
Full Ext. Tr.	0.099* (0.053)	0.041 (0.031)	0.091*** (0.029)	0.019 (0.029)	-0.009 (0.031)
Full*Income50k	0.017 (0.074)	0.019 (0.043)	-0.046 (0.040)	0.058 (0.040)	-0.007 (0.043)
Fairness Tr.	0.308*** (0.054)	0.091*** (0.032)	0.168*** (0.028)	0.080*** (0.029)	0.106*** (0.030)
Fairness*Income50k	-0.197*** (0.073)	-0.076* (0.043)	-0.105*** (0.039)	-0.023 (0.040)	-0.080* (0.042)
Controls	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.388	0.165	0.310	0.289	0.166
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and their interaction with having a yearly income above \$50,000. Controls not listed include pre-treatment fairness views, race, income-group, age-group, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F29: Treatment effects interacted with Republican leaning dummy

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Increase top taxes	Gov. reduce ineq.	Ineq. is serious issue
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.039 (0.051)	0.068** (0.032)	0.009 (0.029)	-0.026 (0.030)	0.006 (0.028)
Trust Ext. Tr.	0.063 (0.052)	0.033 (0.034)	0.023 (0.029)	0.033 (0.030)	0.002 (0.029)
Full Ext. Tr.	0.192*** (0.051)	0.116*** (0.033)	0.020 (0.029)	0.061** (0.029)	0.079*** (0.028)
Fairness Tr.	0.218*** (0.051)	0.069** (0.033)	0.061** (0.028)	0.069** (0.029)	0.115*** (0.027)
Leans Republican	-0.592*** (0.053)	-0.135*** (0.031)	-0.188*** (0.031)	-0.273*** (0.029)	-0.256*** (0.029)
CrimeXRepublicanLeaning	-0.005 (0.073)	-0.071* (0.041)	-0.028 (0.042)	0.065 (0.040)	0.027 (0.039)
TrustXRepublicanLeaning	-0.039 (0.074)	-0.054 (0.043)	-0.035 (0.043)	0.005 (0.040)	0.028 (0.040)
FullExtXRepublicanLeaning	-0.165** (0.074)	-0.128*** (0.043)	-0.062 (0.043)	-0.027 (0.040)	-0.020 (0.040)
FairnessXRepublicanLeaning	-0.020 (0.073)	-0.034 (0.043)	0.008 (0.042)	-0.003 (0.040)	0.000 (0.039)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.391	0.171	0.171	0.294	0.313
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and their interaction with an indicator that the respondent leans republican. Controls not listed include pre-treatment fairness views, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F30: Treatment effects interacted with dummy indicating that the subject believes that the current economic system in the US is unfair

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Increase top taxes	Gov. reduce ineq.	Ineq. is serious issue
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.022 (0.051)	-0.007 (0.027)	0.006 (0.032)	0.024 (0.028)	0.009 (0.028)
Trust Ext. Tr.	0.039 (0.052)	-0.011 (0.028)	0.036 (0.033)	0.036 (0.028)	-0.004 (0.029)
Full Ext. Tr.	0.091* (0.053)	0.038 (0.028)	-0.011 (0.033)	0.041 (0.029)	0.064** (0.030)
Fairness Tr.	0.147*** (0.052)	0.009 (0.028)	0.088*** (0.032)	0.035 (0.028)	0.080*** (0.029)
Prior belief unfair	0.669*** (0.051)	0.103*** (0.030)	0.286*** (0.030)	0.251*** (0.028)	0.322*** (0.028)
CrimeXdPriorUnfair	0.030 (0.073)	0.073* (0.041)	-0.021 (0.042)	-0.031 (0.040)	0.022 (0.039)
TrustXdPriorUnfair	0.008 (0.074)	0.033 (0.042)	-0.062 (0.044)	0.001 (0.040)	0.040 (0.040)
FullExtXdPriorUnfair	0.031 (0.075)	0.024 (0.043)	-0.002 (0.043)	0.013 (0.041)	0.010 (0.040)
FairnessXdPriorUnfair	0.119 (0.073)	0.084** (0.042)	-0.046 (0.042)	0.065* (0.039)	0.068* (0.039)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.391	0.170	0.171	0.294	0.314
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes on the treatment indicators and their interaction with pre-treatment fairness views. Controls not listed include, political leaning, pre-treatment fairness views, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F31: Predictive power of various beliefs in Survey 2

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich because of luck		0.681*** (0.044)				0.394*** (0.045)
Society is unfair		0.648*** (0.044)				0.500*** (0.043)
Ineq. incr. crime			0.324*** (0.046)			0.102** (0.040)
Neg. externality belief			0.515*** (0.044)			0.151*** (0.039)
Leans Republican				-0.502*** (0.053)		-0.261*** (0.052)
Sanders/Harris supporter				0.618*** (0.055)		0.415*** (0.052)
Trusts the government					0.370*** (0.047)	0.090** (0.039)
Taxation reduces work					-0.181*** (0.043)	0.028 (0.035)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Only Control Group	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.133	0.404	0.234	0.358	0.165	0.486
Observations	2360.000	2360.000	2360.000	2360.000	2360.000	2360.000

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Controls not listed include gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. Observations are reweighted to match representativity by gender, race, age, political affiliation, college degree, income-group, and geographic region. Data is from Survey 2 only. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F32: Predictive power of various beliefs with population weights

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich because of hard work		-0.612*** (0.084)				-0.398*** (0.081)
Society is unfair (post)		0.546*** (0.083)				0.360*** (0.077)
Belief uneq countr. worse.			0.510*** (0.080)			0.344*** (0.072)
General neg. ext.			0.555*** (0.082)			0.256*** (0.081)
Leans Republican				-0.335** (0.137)		-0.215* (0.110)
SandersKamala				0.573*** (0.138)		0.268** (0.111)
govtrust					0.228*** (0.054)	0.064 (0.048)
Agrees/disagrees that people work much less if taxed more					-0.054 (0.038)	-0.001 (0.032)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Only Control Group	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.092	0.328	0.277	0.263	0.131	0.448
Observations	929.000	929.000	929.000	929.000	929.000	929.000

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Controls not listed include gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. Observations are reweighted to match representativity by gender, race, age, political affiliation, college degree, income-group, and geographic region. *Significance levels:* *10%, **5%, ***1%.

Table F33: Treatment effects with controls using all completed responses

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Gov. reduce ineq.	Ineq. is serious issue	Increase top taxes
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.039 (0.035)	0.034* (0.020)	0.013 (0.019)	0.023 (0.019)	-0.015 (0.020)
Trust Ext. Tr.	0.055 (0.036)	0.012 (0.021)	0.049** (0.019)	0.014 (0.020)	0.004 (0.021)
Full Ext. Tr.	0.098*** (0.036)	0.046** (0.020)	0.047** (0.019)	0.060*** (0.019)	-0.013 (0.021)
Fairness Tr.	0.202*** (0.036)	0.056*** (0.021)	0.070*** (0.019)	0.106*** (0.019)	0.056*** (0.020)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.360	0.169	0.272	0.277	0.159
Observations	4865.000	4865.000	4865.000	4865.000	4865.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F34: First-stage effects of treatments using all completed responses

	(1)	(2)	(3)	(4)	(5)	(6)
	General neg. ext.	Ineq. incr. crime	Ineq. red. trust	Ineq. red. growth	Society unfair (post)	Rich b/c hard work
	b/se	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.083*** (0.020)	0.092*** (0.018)	0.060*** (0.019)	0.087*** (0.021)	0.014 (0.019)	0.017 (0.019)
Trust Ext. Tr.	0.042** (0.020)	0.042** (0.019)	0.090*** (0.020)	0.081*** (0.022)	0.020 (0.020)	0.021 (0.020)
Full Ext. Tr.	0.073*** (0.020)	0.074*** (0.018)	0.087*** (0.020)	0.058*** (0.022)	0.009 (0.019)	0.027 (0.019)
Fairness Tr.	0.066*** (0.020)	0.018 (0.019)	0.038* (0.020)	0.035* (0.021)	0.067*** (0.019)	0.071*** (0.019)
Controls						
R2	0.162	0.092	0.100	0.097	0.234	0.219
Observations	4865.000	4865.000	4865.000	4865.000	4865.000	4865.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F36: Treatment effects with controls using only respondents that passed all attention checks

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Gov. reduce ineq.	Ineq. is serious issue	Increase top taxes
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.027 (0.043)	0.038 (0.025)	0.011 (0.024)	0.007 (0.023)	-0.016 (0.025)
Trust Ext. Tr.	0.021 (0.045)	0.008 (0.026)	0.039 (0.024)	0.009 (0.024)	-0.026 (0.026)
Full Ext. Tr.	0.075* (0.044)	0.045* (0.026)	0.056** (0.024)	0.050** (0.024)	-0.040 (0.026)
Fairness Tr.	0.185*** (0.043)	0.050* (0.026)	0.081*** (0.024)	0.094*** (0.023)	0.047* (0.026)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.436	0.201	0.335	0.360	0.192
Observations	2892.000	2892.000	2892.000	2892.000	2892.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F35: Predictive power of various beliefs using all completed responses

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich because of luck		-0.550***				-0.337***
		(0.059)				(0.056)
Society unfair (post)		0.628***				0.440***
		(0.058)				(0.056)
Belief uneq. countr. worse			0.457***			0.298***
			(0.056)			(0.050)
Neg. externality belief			0.600***			0.224***
			(0.055)			(0.052)
Leans Republican				-0.361***		-0.194***
				(0.084)		(0.072)
Sanders/Harris supporter				0.592***		0.331***
				(0.085)		(0.076)
Trusts the government					0.220***	0.081**
					(0.036)	(0.033)
Taxation reduces work					-0.097***	-0.012
					(0.025)	(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Only Control Group	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.090	0.346	0.274	0.279	0.138	0.465
Observations	1026.000	1026.000	1026.000	1026.000	1026.000	1026.000

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Controls not listed include gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F37: First-stage effects of treatments using only respondents that passed all attention checks

	(1)	(2)	(3)	(4)	(5)	(6)
	General neg. ext.	Ineq. incr. crime	Ineq. red. trust	Ineq. red. growth	Society unfair (post)	Rich b/c hard work
	b/se	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.096***	0.099***	0.058**	0.107***	-0.000	0.011
	(0.025)	(0.021)	(0.024)	(0.027)	(0.024)	(0.024)
Trust Ext. Tr.	0.046*	0.047**	0.096***	0.062**	0.036	0.000
	(0.025)	(0.023)	(0.024)	(0.028)	(0.024)	(0.024)
Full Ext. Tr.	0.075***	0.081***	0.102***	0.054*	0.016	0.020
	(0.025)	(0.022)	(0.024)	(0.028)	(0.024)	(0.024)
Fairness Tr.	0.084***	0.035	0.044*	0.037	0.082***	0.054**
	(0.025)	(0.023)	(0.025)	(0.027)	(0.024)	(0.024)
Controls						
R2	0.169	0.086	0.095	0.121	0.278	0.284
Observations	2892.000	2892.000	2892.000	2892.000	2892.000	2892.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F38: Predictive power of various beliefs using only respondents that passed all attention checks

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich because of luck		-0.642*** (0.080)				-0.383*** (0.075)
Society unfair (post)		0.624*** (0.076)				0.396*** (0.072)
Belief uneq. countr. worse			0.448*** (0.073)			0.281*** (0.067)
Neg. externality belief			0.681*** (0.073)			0.298*** (0.068)
Leans Republican				-0.366*** (0.114)		-0.195** (0.099)
Sanders/Harris supporter				0.629*** (0.114)		0.328*** (0.104)
Trusts the government					0.256*** (0.047)	0.043 (0.045)
Taxation reduces work					-0.090*** (0.033)	-0.003 (0.025)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Only Control Group	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.130	0.411	0.344	0.337	0.188	0.525
Observations	597.000	597.000	597.000	597.000	597.000	597.000

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Controls not listed include gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F39: Treatment effects with controls and controlling for passing attention checks

	(1)	(2)	(3)	(4)	(5)	(6)
	General neg. ext.	Ineq. incr. crime	Ineq. red. trust	Ineq. red. growth	Society unfair (post)	Rich b/c hard work
	b/se	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.086*** (0.020)	0.091*** (0.018)	0.056*** (0.020)	0.084*** (0.022)	0.010 (0.020)	0.018 (0.020)
Trust Ext. Tr.	0.046** (0.021)	0.044** (0.019)	0.090*** (0.020)	0.072*** (0.023)	0.021 (0.020)	0.026 (0.020)
Full Ext. Tr.	0.078*** (0.021)	0.079*** (0.019)	0.090*** (0.020)	0.057** (0.023)	0.011 (0.020)	0.028 (0.020)
Fairness Tr.	0.070*** (0.021)	0.013 (0.019)	0.033 (0.021)	0.029 (0.022)	0.076*** (0.020)	0.078*** (0.020)
Controls						
R2	0.167	0.091	0.101	0.106	0.244	0.242
Observations	4371.000	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region, failing or passing any attention check. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F40: Treatment effects, dropping active control group

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Gov. reduce ineq.	Ineq. is serious issue	Increase top taxes
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.058 (0.042)	0.024 (0.024)	0.016 (0.023)	0.014 (0.023)	0.029 (0.025)
Trust Ext. Tr.	0.064 (0.043)	-0.001 (0.025)	0.044* (0.023)	0.012 (0.023)	0.037 (0.026)
Full Ext. Tr.	0.127*** (0.043)	0.043* (0.025)	0.056** (0.024)	0.064*** (0.023)	0.021 (0.026)
Fairness Tr.	0.228*** (0.042)	0.045* (0.025)	0.076*** (0.023)	0.110*** (0.022)	0.099*** (0.025)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.392	0.172	0.292	0.317	0.167
Observations	3977.000	3977.000	3977.000	3977.000	3977.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F41: First-stage effects, dropping active control group

	(1)	(2)	(3)	(4)	(5)	(6)
	General neg. ext.	Ineq. incr. crime	Ineq. red. trust	Ineq. red. growth	Society unfair (post)	Rich b/c hard work
	b/se	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.098*** (0.024)	0.092*** (0.021)	0.067*** (0.024)	0.095*** (0.025)	0.020 (0.023)	0.022 (0.023)
Trust Ext. Tr.	0.060** (0.025)	0.048** (0.022)	0.105*** (0.024)	0.084*** (0.026)	0.033 (0.024)	0.031 (0.023)
Full Ext. Tr.	0.095*** (0.025)	0.084*** (0.022)	0.106*** (0.024)	0.071*** (0.026)	0.024 (0.024)	0.032 (0.024)
Fairness Tr.	0.084*** (0.024)	0.016 (0.022)	0.045* (0.024)	0.042 (0.026)	0.087*** (0.023)	0.082*** (0.023)
Controls						
R2	0.158	0.084	0.093	0.103	0.237	0.245
Observations	3977.000	3977.000	3977.000	3977.000	3977.000	3977.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F42: Treatment effects, dropping passive control group

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Gov. reduce ineq.	Ineq. is serious issue	Increase top taxes
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.010 (0.047)	0.039 (0.027)	-0.004 (0.025)	0.028 (0.026)	-0.049* (0.026)
Trust Ext. Tr.	0.016 (0.048)	0.014 (0.028)	0.024 (0.026)	0.025 (0.026)	-0.040 (0.027)
Full Ext. Tr.	0.081* (0.048)	0.058** (0.028)	0.036 (0.026)	0.079*** (0.026)	-0.056** (0.027)
Fairness Tr.	0.180*** (0.048)	0.060** (0.028)	0.055** (0.025)	0.123*** (0.025)	0.020 (0.027)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.390	0.170	0.294	0.314	0.176
Observations	3833.000	3833.000	3833.000	3833.000	3833.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F43: First-stage effect, dropping passive control group

	(1) General neg. ext. b/se	(2) Ineq. incr. crime b/se	(3) Ineq. red. trust b/se	(4) Ineq. red. growth b/se	(5) Society unfair (post) b/se	(6) Rich b/c hard work b/se
Crime Ext. Tr.	0.075*** (0.027)	0.093*** (0.024)	0.047* (0.026)	0.077*** (0.028)	-0.000 (0.026)	0.015 (0.026)
Trust Ext. Tr.	0.037 (0.027)	0.048** (0.024)	0.083*** (0.026)	0.066** (0.029)	0.013 (0.026)	0.024 (0.026)
Full Ext. Tr.	0.072*** (0.027)	0.084*** (0.024)	0.086*** (0.026)	0.053* (0.029)	0.004 (0.026)	0.027 (0.026)
Fairness Tr.	0.061** (0.027)	0.017 (0.025)	0.025 (0.027)	0.023 (0.029)	0.066*** (0.026)	0.075*** (0.026)
Controls						
R2	0.155	0.085	0.090	0.098	0.234	0.243
Observations	3833.000	3833.000	3833.000	3833.000	3833.000	3833.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F44: Treatment effects without controls

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Gov. reduce ineq. b/se	(4) Ineq. is serious issue b/se	(5) Increase top taxes b/se
Crime Ext. Tr.	0.036 (0.046)	0.031 (0.022)	0.005 (0.023)	0.022 (0.023)	-0.006 (0.023)
Trust Ext. Tr.	0.055 (0.047)	0.010 (0.023)	0.041* (0.024)	0.023 (0.024)	0.005 (0.024)
Full Ext. Tr.	0.124*** (0.048)	0.059** (0.023)	0.056** (0.024)	0.078*** (0.024)	-0.014 (0.024)
Fairness Tr.	0.173*** (0.047)	0.042* (0.023)	0.052** (0.024)	0.102*** (0.023)	0.053** (0.023)
Controls	No	No	No	No	No
R2	0.004	0.002	0.002	0.006	0.002
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F45: First-stage effects of treatments without controls

	(1) General neg. ext. b/se	(2) Ineq. incr. crime b/se	(3) Ineq. red. trust b/se	(4) Ineq. red. growth b/se	(5) Society unfair (post) b/se	(6) Rich b/c hard work b/se
Crime Ext. Tr.	0.092*** (0.022)	0.094*** (0.018)	0.059*** (0.021)	0.089*** (0.023)	0.015 (0.023)	0.018 (0.022)
Trust Ext. Tr.	0.052** (0.023)	0.049** (0.020)	0.096*** (0.021)	0.079*** (0.024)	0.030 (0.023)	0.033 (0.023)
Full Ext. Tr.	0.088*** (0.023)	0.084*** (0.019)	0.101*** (0.021)	0.069*** (0.024)	0.019 (0.023)	0.032 (0.023)
Fairness Tr.	0.068*** (0.023)	0.012 (0.020)	0.033 (0.022)	0.028 (0.024)	0.067*** (0.023)	0.066*** (0.022)
Controls						
R2	0.005	0.009	0.007	0.005	0.002	0.002
Observations	4371.000	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F46: Treatment effects with controls using non-dichotomized variables

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Gov. reduce ineq.	Ineq. serious issue	Increase top taxes
	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.026 (0.036)	0.107 (0.069)	0.024 (0.048)	0.039 (0.044)	-0.043 (0.059)
Trust Ext. Tr.	0.033 (0.036)	-0.006 (0.071)	0.066 (0.049)	0.040 (0.045)	0.031 (0.061)
Full Ext. Tr.	0.098*** (0.036)	0.179** (0.071)	0.106** (0.049)	0.140*** (0.045)	0.007 (0.062)
Fairness Tr.	0.209*** (0.035)	0.288*** (0.071)	0.180*** (0.048)	0.263*** (0.044)	0.182*** (0.058)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.422	0.318	0.386	0.357	0.142
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different redistributive preference outcomes and the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F47: First-stage effects of treatments using non-dichotomized variables

	(1)	(2)	(3)	(4)	(5)	(6)
	General neg. ext.	Ineq. incr. crime	Ineq. red. trust	Ineq. red. growth	Society unfair (post)	Rich b/c hard work
	b/se	b/se	b/se	b/se	b/se	b/se
Crime Ext. Tr.	0.194*** (0.046)	0.238*** (0.044)	-0.162*** (0.046)	-0.173*** (0.050)	0.047 (0.050)	-0.018 (0.020)
Trust Ext. Tr.	0.119** (0.048)	0.138*** (0.045)	-0.252*** (0.047)	-0.118** (0.051)	0.017 (0.052)	-0.028 (0.020)
Full Ext. Tr.	0.201*** (0.048)	0.223*** (0.046)	-0.265*** (0.048)	-0.127** (0.052)	0.013 (0.053)	-0.030 (0.020)
Fairness Tr.	0.170*** (0.047)	0.079* (0.047)	-0.104** (0.048)	-0.049 (0.051)	0.204*** (0.051)	-0.079*** (0.020)
Controls						
R2	0.163	0.096	0.108	0.091	0.269	0.241
Observations	4371.000	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports results from a regression of different externality beliefs and fairness views on the treatment dummies, as well as socio-economic control variables. Controls not listed in the table include political leaning, gender, trust in government, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F48: Predictive power of various beliefs without controls

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich because of luck		-0.655***				-0.418***
		(0.063)				(0.060)
Society unfair (post)		0.646***				0.445***
		(0.063)				(0.060)
Belief uneq. countr. worse			0.422***			0.249***
			(0.063)			(0.054)
Neg. externality belief			0.622***			0.217***
			(0.063)			(0.057)
Leans Republican				-0.458***		-0.292***
				(0.089)		(0.079)
Sanders/Harris supporter				0.581***		0.334***
				(0.089)		(0.081)
Trusts the government					0.216***	0.029
					(0.040)	(0.036)
Taxation reduces work					-0.092***	-0.004
					(0.028)	(0.021)
Controls	No	No	No	No	No	No
Only Control Group	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.104	0.317	0.183	0.249	0.046	0.443
Observations	932.000	932.000	932.000	932.000	932.000	932.000

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F49: Predictive power of various beliefs using RP-index based on non-dichotomized variables

	(1)	(2)	(3)	(4)	(5)	(6)
	RP Index	RP Index	RP Index	RP Index	RP Index	RP Index
	b/se	b/se	b/se	b/se	b/se	b/se
Rich b/c hard work		-0.657*** (0.063)				-0.388*** (0.055)
Society unfair (post)		0.280*** (0.025)				0.214*** (0.022)
Belief uneq. countr. worse			0.254*** (0.031)			0.142*** (0.024)
General neg. ext.			0.265*** (0.031)			0.077*** (0.025)
Leans Republican				-0.374*** (0.084)		-0.149** (0.065)
Sanders/Harris supporter				0.690*** (0.085)		0.359*** (0.068)
Trusts the government					0.322*** (0.039)	0.156*** (0.032)
Taxation reduces work					-0.120*** (0.028)	-0.036* (0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Only Control Group	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.114	0.455	0.323	0.350	0.207	0.593
Observations	932.000	932.000	897.000	932.000	932.000	897.000

Note. This table reports results from a regression of different redistributive preference outcomes on fairness views, political views, externality beliefs and attitudes towards the government, as well as socio-economic control variables. Controls not listed include gender, race, income-group, age-group, education, employment status, geographic region. Standard errors are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F50: Treatment effects with FDR sharpened q-values

	(1)	(2)	(3)	(4)	(5)
	RP Index	Wants redistribution	Increase top taxes	Gov. reduce ineq.	Ineq. is serious issue
Crime Ext. Tr.	0.037	0.031	-0.005	0.007	0.020
p-value	(.308)	(.127)	(.817)	(.705)	(.313)
q-value	(.288)	(.147)	(.610)	(.597)	(.288)
Trust Ext. Tr.	0.043	0.006	0.004	0.036*	0.017
p-value	(.244)	(.800)	(.842)	(.075)	(.407)
q-value	(.256)	(.610)	(.610)	(.091)	(.324)
Full Ext. Tr.	0.107***	0.050**	-0.012	0.048**	0.069***
p-value	(.004)	(.019)	(.572)	(.018)	(.001)
q-value	(.011)	(.032)	(.475)	(.032)	(.004)
Fairness Tr.	0.208***	0.052**	0.065***	0.067***	0.115***
p-value	(.000)	(.015)	(.002)	(.001)	(.000)
q-value	(.001)	(.032)	(.007)	(.004)	(.001)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4371.000	4371.000	4371.000	4371.000	4371.000

Note. This table reports FDR sharpened q-values from the regression in Table 2. p-values and q-values are in parentheses. *Significance levels:* *10%, **5%, ***1%.

Table F51: Respondents' belief about the survey bias by treatment group

	Right-Wing Bias (%)	No Bias (%)	Left-Wing Bias (%)
Crime tr.	5.68	71.49	22.83
Trust tr.	5.21	73.45	21.33
Full ext tr.	7.66	70.33	22.00
Fairness tr.	6.19	70.87	22.94
Control (passive)	7.81	73.98	18.22
Control (active)	6.85	72.84	20.30