

# The Consequences of Inequality: Beliefs and Redistributive Preferences\*

Max Lobeck<sup>†</sup> and Morten Nyborg Støstad<sup>‡★</sup>

\* Job Market Paper (Click Here for latest version)

## Abstract

What are the societal consequences of economic inequality, and how do concerns for these consequences affect individuals' redistributive preferences? This paper examines beliefs about how economic inequality changes society and establishes a causal link between such *inequality externality beliefs* and 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 negative societal outcomes through channels such as increased crime, deteriorating democratic institutions, and diminished economic growth. Such beliefs are similar among Democrats and Republicans, unlike broad economic fairness views which are more polarized. We establish a causal link from individuals' inequality externality beliefs to their redistributive preferences by using exogenously provided video information treatments. With this and other methods we estimate that inequality externality beliefs are about two-thirds as impactful for individuals' redistributive preferences as broad fairness views. Inequality externality-based arguments cause less anger among respondents than fairness-based arguments, however. Our results thus indicate a potential trade-off in debates on redistributive ideas; whereas fairness-based arguments are somewhat more persuasive, externality-based arguments seem to be less polarizing and present an opportunity for consensus-building.

---

\*We thank Emmanuel Saez, Marc Fleurbaey, Urs Fischbacher, Stéphane Gauthier, Claudia Senik and Nicolas Jacquemet for invaluable comments and discussions. We have also benefited from suggestions from Regina Anselm, Viola Asri, Luis Bauluz, Marcelo Bergolo, Thomas Blanchet, Antoine Bozio, Stefano DellaVigna, Amory Gethin, Shachar Kariv, Sébastien Laffitte, Clara Martínez-Toledano, Karine Nyborg, Thomas Piketty, Katrin Schmelz, Maj-Britt Sterba, Robert Stüber, Julia Werner, and Irenaeus Wolff. We also thank participants at the 2022 EEA-ESEM Congress, the 2022 IIPF Annual Congress, the 2022 LAGV Conference, the 2022 European ESA Meeting, the 2022 ASFEE Conference, Doctorissimes 2022, the JEM workshop at the University of Milan, the Fairness and the Moral Mind workshop at the Norwegian School of Economics (NHH), and seminar participants at UC Berkeley, the Paris School of Economics, the University of Konstanz and the Thurgau Institute of Economics, for helpful comments and suggestions. This work has been funded by CEPREMAP program 3, the UC Berkeley James M. and Cathleen D. Stone Center on Wealth and Income Inequality, two French government subsidies managed by the Agence Nationale de la Recherche under the framework of the Investissements d'avenir programme reference ANR-17-EURE-001, and the Deutsche Forschungsgemeinschaft (DFG - German Research Foundation) under Germany's Excellence Strategy - EXC-2035/1 - 390681379. This study was pre-registered under AsPredicted #82083 (Survey 1) and #104271 (Survey 2). Version: November 11, 2022.

<sup>†</sup>University of Konstanz, Universitätsstrasse 10, 78464 Konstanz, Germany, Cluster of Excellence "The Politics of Inequality", Thurgau Institute of Economics, e-mail: max.lobbeck@uni-konstanz.de

<sup>‡</sup>Paris School of Economics, 48 Boulevard Jourdan, 75014 Paris, France. Phone: +33766142152. e-mail: morten.stostad@psemail.eu.

# 1 Introduction

*Why do we care about economic inequalities?* The question has been debated since the dawn of institutions. Differing answers have lead to oppression or revolution, polarization or reform. The debate is still raging in modern times; in an age of rising inequality, the question of *whether we should care* is the contentious driver of many political schisms. Arguments are often centered on fairness concerns with no objective answer, partly explaining why these debates have continued for millennia.

But such arguments do not need to be about fairness, and they do not need to be divisive. In this paper we quantify a strong motivation for redistributive preferences that could foster wide-scale agreement, namely concerns for the *consequences of inequality*. Such consequences occur when economic inequality changes something we care about, for example the level of social unrest, economic growth, or the general trust between people. 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.<sup>1</sup> Such inequality externalities could help or harm society as a whole; more economic inequality could increase or decrease the rate of economic growth, for example.<sup>2</sup> Importantly for the present paper, people's beliefs about these consequences can vary, which can in turn affect overall demands for redistribution. 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. In general, what people believe about these inequality externalities – what they are and how impactful they are – could shape political landscapes and re-frame our understanding of why we should care about inequality.

Following these observations, this paper poses two main questions. First, do 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 links 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 compare the redistributive effect of inequality externality beliefs to that of broad fairness views. Finally, we show the practical implications of our results by discussing the differing polarization effects of fairness-based and inequality externality-based redistributive arguments.

Due to their potentially large societal ramifications, beliefs in inequality externalities are

---

<sup>1</sup>This is a shorthand; technically it is economic inequality *itself* that is an externality (Støstad and Cowell, 2020).

<sup>2</sup>We do not know the true effects due to intrinsic empirical difficulties, in particular the lack of exogenous variation of macroeconomic inequality. Despite this there is strong indicative evidence that such large-scale externalities do exist, at least to some degree, including evidence from the lab (Fehr et al., 2020), surveys (Gallego, 2016), in macroeconomic models (Bénabou, 1996; Auclert and Rognlie, 2018), and at the workplace (Card et al., 2012; Breza et al., 2018). There are also several strong cross-country correlations between economic inequality and various outcomes (see Section 5.1.1 for examples on trust and crime).

intriguing in themselves. Yet, before this work there was very little known about them. While individuals' broad thoughts on inequality, redistribution and economic fairness are extensively studied in international surveys – thousands of survey questions explore these topics – questions about the *consequences* of inequality are extremely rare. The few questions that have been posed to representative samples usually pertain to positive inequality externalities, and specifically to the idea that economic inequality increases the amount of economic growth or innovation. The only existing example we know of in the United States comes from the General Social Survey (GSS), which asked respondents in four waves between 1987 and 2008 whether they agreed that “*large differences in income are necessary for America’s prosperity*”. The implicit assumption has been that if such externalities exist, the positive externality dimension is the most meaningful.

In contrast to this idea, our results show that most U.S. citizens believe in the *negative* externality dimension. A consistent majority ( $\sim 60\%$ ) believes that economic inequality has overall harmful societal consequences, and nearly every individual believes inequality affects society in some way.<sup>3</sup> We delve into the potential reasons and find strong beliefs in specific channels; 76% of respondents think more economic inequality increases the amount of crime, for example, and 68% think more economic inequality deteriorates the overall level of societal trust. 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. Results are robust to different methodologies, question phrasings, and are nearly identical across different representative samples.<sup>4</sup>

Beliefs in such negative externalities extend to the economic realm. Citizens generally do not subscribe to the argument that more economic inequality helps the economy through more economic growth or innovation; instead they believe the opposite. About half of our respondents believe that more economic inequality *decreases* both economic growth and innovation, which contrasts with only one in five respondents who believe that more economic inequality would improve these economic outcomes. As these are our most mixed responses – every other outcome is seen as a negative externality by a larger proportion of the population – it is clear that the U.S. population strongly believes in the harmful consequences of economic inequality. The three channels judged as “most meaningful” all affect safety and political institutions; individuals are particularly concerned about the effect of inequality on crime, corruption, and the quality of democratic institutions.

Who, then, believes in these harmful consequences of inequality? While Democrats are more likely than Republicans to hold negative externality beliefs, the general descriptive results are consistent across political camps. Regardless of party affiliation, every respondent is more likely

---

<sup>3</sup>Approximately 15% believe the net effect to be positive, and approximately 10 – 20% believe the positive and negative effects “cancel each other out”, where different question specifications account for the percentage differences. Respondents are told to focus on societal changes and to disregard fairness concerns in their answers.

<sup>4</sup>All questions are specifically designed to avoid leading respondents to any one answer; most questions are symmetric, allowing for respondents to answer that more inequality leads to *more* or *less* of the outcome variable. The order of potential answers is also randomly flipped on a question-by-question basis. Furthermore, respondents 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 (where the “equality” phrasing implies that more equality leads to for example less crime). In a placebo question where the sensible answer is “no change”, almost every respondent chooses this option.

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 stable across party lines is especially evident when we compare them to the broad economic fairness views we elicit as a benchmark.<sup>5</sup> Such largely philosophical concerns – regarding whether the current income distribution is *fair*, for example – are starkly divided by party lines, which contrasts to the elicited externality beliefs. 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 both 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 explore whether such beliefs constitute a causal determinant for redistributive preferences. As inequality externality beliefs are essentially unmentioned in the large redistributive preferences literature, conclusively proving such a causal channel would be a significant finding. Our main method 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; (i) income inequality’s cross-country correlation with crime (crime externality treatment), (ii) income inequality’s cross-country correlation with trust (trust externality treatment), (iii) combined information from these two videos, coupled with broader empirical evidence<sup>6</sup> and general arguments about how inequality could affect society (full externality treatment), and (iv) the decoupling of wages and productivity since roughly 1980 and rising top incomes in the period thereafter (fairness treatment). The first three treatments are designed to exogenously shift various inequality externality beliefs, whereas the last is designed to exogenously shift fairness views as a benchmark. We formalize several novel survey design methodologies 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).

We find that the full externality treatment and the fairness treatment are both significant predictors of higher post-treatment redistributive preferences when compared to the control group ( $p < 0.01$ ). These results are robust to an array of different specifications. The effects of the crime and trust externality treatment are not statistically significant, but are in the expected direction of increased redistributive preferences. The magnitude of the full externality treatment effect is roughly half that of the fairness treatment. Further tests show that each video affected redistributive preferences through the expected mechanism with limited spillovers and

---

<sup>5</sup>We use fairness concerns as a benchmark throughout the study due to the status of such fairness views as well-known determinants of redistributive preferences in the literature (Almås et al., 2020).

<sup>6</sup>We show respondents that there are no significant correlations between income inequality and either innovation or economic growth.

priming, meaning that the experiment can be interpreted causally. In summary, we establish that inequality externality beliefs are causal determinants of redistributive preferences with a significant magnitude.

We further explore the relative importance of inequality externality beliefs and fairness concerns with two other methods. First, in a classical horse-race, we compare the predictive power of externality beliefs, fairness views, political preferences, and efficiency concerns in estimating redistributive preferences. Second, we simply ask respondents what they take into account when thinking about their preferred level of redistribution. These approaches both find that externality beliefs are roughly two-thirds as important as fairness views in determining redistributive preferences, a magnitude which is generally consistent with the video treatment effects. Thus our results show that inequality externality beliefs are a causal determinant of redistributive preferences with a magnitude somewhat smaller than, but on the same scale as, fairness views – which is notable given the comparative attention given to these two determinants in political rhetoric and in the academic literature.

Strict magnitude comparisons overlook important distinctions, however, as our results also indicate structural differences in how these two types of arguments 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; low-income individuals are significantly more swayed by the fairness treatment than top income individuals. This contrasts with the externality treatment, which is broadly convincing across the distribution. These two points corroborate the polarization story we find in the descriptive data, which is also intuitively appealing. Fairness arguments require, almost by definition, either a *victim*, a *villain*, or *both* – someone who deserves more and someone who deserves less. Externality arguments, meanwhile, focus on an unintended *common enemy*. These structural differences are fundamental; whereas fairness arguments are normative and deontological, externality arguments are positive and consequentialist. In essence, externality arguments are efficiency-based, which contrasts to principle-based fairness arguments. We thus hypothesize that there is a salient trade-off between these two types of arguments; while fairness-based arguments are somewhat more effective in general, they also cause more polarization. Externality-based arguments, meanwhile, have the potential for consensus-building at the cost of somewhat lower efficacy.

Beyond their effects on redistributive preferences, inequality externality beliefs are intriguing to study due to their influence on economic theory. As discussed in Thurow (1971) and Støstad and Cowell (2020), the externality effects of inequality have large ramifications for individualist economic theory. This paper, in summarizing the widespread public beliefs in such effects – nearly every U.S. citizen believes that *some kind* of inequality externalities exist – shows that a social planner who aggregates individual preferences might wish to include such externality considerations into its optimization problem. Standard individualist frameworks should thus consider more seriously their robustness – or fragility – to such externality effects. When economic inequality has externalities, 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 – to chart a steady path forward while preserving and improving society as we know it. What amount of resource inequality is safe and sustainable? Should we forego available efficiency

gains if they are too narrow to retain an equitable distribution? These questions are relevant questions when economic inequality has externalities and go beyond simple egalitarianism.

This paper relates to several strands of research. First, the literature on the motives behind redistributive preferences and the origins of the heterogeneity in these preferences. Past work asks whether fairness ideals and efficiency costs of redistribution motivate inequality acceptance (Cappelen et al., 2007; Almås et al., 2020), whether beliefs about one's relative position affects redistributive preferences (Cruces et al., 2013; Karadja et al., 2017), whether redistributive preferences are elastic to information about the level of inequality and taxes (Kuziemko et al., 2015), and whether beliefs about social mobility affect redistributive preferences (Alesina et al., 2018; Gärtner et al., 2019). Surprisingly, given its intuitive nature and important implications, citizens' concerns about the damaging effects of inequality have not been studied by this literature, even though it has been proposed as a possible motive behind redistributive preferences in the prior literature (Alesina and Giuliano, 2011). One exception is work by Rueda and Stegmüller (2016) who present correlational evidence of an association between fear of crime and preferences for redistribution and link the two through an externality-based argument in their theoretical framework. This paper is among the first to explicitly study these inequality externality beliefs, as well as the first to directly empirically link stated externalities beliefs to individuals' preferences for redistribution.

The second strand of related research is the theoretical work on the externality effects of inequality. The theoretical strand of this literature begins with 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 Stegmüller (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) and thus optimal income taxation in the first-best and discrete-agent settings (Aronsson and Johansson-Stenman, 2018, 2020). Støstad and Cowell (2020) formalize the framework around inequality as an externality, create micro-foundations for various inequality externalities, and show the large extent to which an inequality externality influences well-known optimal income taxation theory.

The third related strand of literature is the large empirical body of work on the various societal effects of inequality. In terms of establishing causal connections, this literature is troubled by intrinsic empirical concerns and insufficient data – see Støstad (2019) for a discussion. Despite these concerns, there is a large literature attempting to empirically estimate the consequences of economic inequality on a vast array of outcomes. A full examination of this literature is beyond the scope of this paper. 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. 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).

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. The existence of such societal effects of inequality implies that economic inequality itself is an externality, as individual choices affect economic inequality which then affects other individuals. This work discusses several of these specific channels, which we define as *inequality externalities*. We define an inequality externality as the concept of economic inequality affecting some relevant societal variable, such as crime, social unrest, or economic growth.<sup>7</sup>

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, each of the 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. We discuss these two points further in Appendix A. In sum, inequality externalities are theoretically complex to fully model, and there exists an academic literature pursuing this goal. However, their existence does not rely on individual irrationality, and specific causal channels can easily be microfounded. The large underlying complexity gives way to a simplicity that non-experts can both understand and opine on.

### 2.1 Inequality externalities and redistribution

A simple model of individual preferences for redistribution takes into account the individual's expected post-tax income  $x_i$  and broad fairness concerns. We introduce such fairness concerns through generalized social welfare weights  $g_{i,j}(z)$  – indicating the value individual  $i$  would put on another individual  $j$  at pre-tax income  $z$  receiving one extra unit of income if individual  $i$  was the social planner (normalized to one), *absent any effect on economic inequality*. For example, if individual  $i$  only cares about maximizing their own income even if they were the social planner, then  $g_{i,i} = 1$  and  $g_{i,j} = 0 \forall j \neq i$ . This remains true even if the respondent might selfishly want to reduce inequality due to the presence of inequality externalities (thus in practice having preferences for other agents' incomes).  $\bar{G}_i(z)$  indicates the average weight respondent  $i$  sets above income  $z$ . In the theoretical framework we consider fairness concerns to be perfectly captured by the social weights and thus assume no strict other-regarding preferences (altruism, jealousy, relative income concerns, and so on).<sup>8</sup>

---

<sup>7</sup>We note that this is strictly speaking a shorthand. If such channels exist, it is technically economic inequality *itself* that is an externality. In this paper we instead define inequality's effects on other outcomes as "inequality externalities", even though such abstract concepts are technically not externalities themselves. It simplifies the discussion significantly to consider these concepts as "inequality externalities", however, and as such we use these terms interchangeably in the remainder of the paper.

<sup>8</sup>Such other-regarding preferences would be introduced through an economic inequality term directly inserted into the utility function such that  $U_i(x_i, \theta, \dots)$ . Other-regarding preferences impact the optimal taxation problem

In addition to their individual income we posit that individuals might also have preferences over their *preferred state of the world*  $\vec{\Gamma}$ , where  $\vec{\Gamma}$  represents a vector of various utility-pertinent variables – the crime rate, the level of generalized trust, and so on. The question we pose in this paper is whether individuals believe that economic inequality affects this  $\vec{\Gamma}$  such that this vector – and potentially individual income  $x_i$  – is a function of economic inequality  $\theta$ . If so, these *inequality externality beliefs* should affect total redistributive preferences. In this case individual utility can be written and simplified as,

$$U_i(x_i(\theta), \vec{\Gamma}(\theta)) \rightarrow \tilde{U}_i(x'_i, \theta), \quad (1)$$

where we simplify the utility function to  $\tilde{U}_i$  by removing the parts of the  $\vec{\Gamma}$  function not related to the economic variables  $x_i$  or  $\theta$ , and defining  $x'_i$  as the part of post-tax income that is independent of economic inequality.

The remaining  $\theta$  indicates how inequality indirectly affects individuals through any inequality externalities. The effect of this  $\theta$  on utility depends on the marginal rate of substitution between income and income inequality  $\eta_i = MRS_{x\theta} = -\frac{dU_i/d\theta}{dU_i/dx}$ . If  $\eta_i$  is positive, inequality has a negative externality effect on agent  $i$  (a public bad effect). If  $\eta_i$  is negative, inequality has a positive externality effect on agent  $i$  (a public good effect).

In the descriptive part of the paper we will explore individuals' inequality externality beliefs. These beliefs determine the functions  $E_i(\vec{\Gamma}(\theta))$ , which in turn determine the perceived  $\eta_i$ .

### 2.1.1 Respondents' preferred redistribution

When faced with a redistributive preference setting, individual  $i$  determines their preferred state of the world by trading off (a) the maximization of the incomes  $z$  they care about – their fairness-based choice is denoted by  $\bar{G}_i(z)$ , (b) revenue collection efficiency concerns,<sup>9</sup> denoted by the individual's estimated earnings elasticity  $\epsilon_i(z)$  (with respect to  $1 - \tau_i(z)$ ), and the individual's estimated local Pareto parameter  $\alpha_i(z)$  (a variable denoting the thickness of the income distribution), and (c) the individual's belief in inequality externalities, denoted by  $\eta_i$ .

Note that all the variables in the preceding section do not represent true empirical values but rather individual's  $i$  estimates of the true values. We do not expect individuals to have a perfect understanding of the trade-offs behind the optimal redistributive problem. The crucial part is instead the marginal effects. If an individuals' estimate of inequality externality effects  $\eta_i$  increases, for example, this framework would allow us to see how a rational agent changes their redistributive preferences.

The trade-off above implies a maximization problem.<sup>10</sup> Respondent  $i$  solves this maximization problem and sets the following marginal income tax rate  $\tau_i(z)$  at income  $z$ ,

---

in a mathematically similar way to inequality externality issues. For simplicity we assume that they do not exist here; the further analysis is not affected by this choice. In practice broad fairness concerns are most likely a composite of social weights and other-regarding preferences.

<sup>9</sup>These variables determine how much tax revenue can be extracted from higher taxes. In short, they indicate how much individuals shift their labor supply when taxed and the shape of the income distribution, respectively. For a longer discussion see Saez (2001).

<sup>10</sup>See Støstad and Cowell (2020) for derivation and details.

$$\tau_i(z) = \frac{1 + \eta_i \Omega_i(z) - \bar{G}_i(z)}{1 + \eta_i \Omega_i(z) + \alpha_i(z) \epsilon_i(z) - \bar{G}_i(z)}, \quad (2)$$

where  $\Omega_i(z) = \alpha_i(z) \epsilon_i(z) \kappa_i(z) + \bar{\kappa}_i(z)$ . The parameter  $\kappa_i(z)$  denotes the weight of the individual at the tax bracket  $z$  in the chosen inequality metric, and  $\bar{\kappa}_i(z)$  denotes the average of this weight above  $z$ .

In the externality arm of the information experiment we will aim to inform individuals about inequality externalities to change their perceived  $E(\vec{\Gamma}(\theta))$ . A successful intervention would thus change redistributive preferences through changing  $\eta_i$  in the respondent's maximization problem. Similarly, the fairness treatment of the information experiment will aim to inform individuals about income distribution dynamics to change their social weights  $g_{i,j}$ , thus also changing the maximization problem. This breakdown also shows that a key question within our treatments is one of spillovers; if the externality videos affect fairness views or vice versa, the causal effect will not be clearly identified.

How do well-identified changes in individuals' externality or fairness views change redistributive preferences? To illustrate this we use top tax rates. Above the individuals' estimated inequality-neutral income, defined as where  $\kappa_i = 0$ , we have that  $\frac{\partial \tau_i}{\partial \eta_i} \geq 0$ . As more *negative* inequality externality beliefs indicate a higher  $\eta_i$ , more negative externality beliefs thus indicate a higher  $\tau_i(z)$  near the top – or higher redistributive preferences more generally. Similarly, we have that  $\frac{\partial \tau_i}{\partial G_i(z)} \geq 0 \forall z$ . Thus, stronger fairness concerns for bottom-incomes indicates higher preferred top tax rates and vice versa.

To sum up, stronger beliefs in negative externalities imply more demand for redistribution and stronger beliefs in positive externalities imply less demand for redistribution for rational individuals. Similarly, a change in fairness views that increases concerns for bottom incomes implies more demand for redistribution and vice versa.

The preceding discussion clarifies the two main unresolved questions we focus on in the empirical analysis. First, do people have inequality externality beliefs – and if so, what are they? Second, do any such externality beliefs actually influence people's redistributive preferences (as they theoretically should)? The remainder of the paper will expand on these ideas. We will continue to compare our findings to fairness concerns in order to contextualize our conclusions. The remainder of the paper will first explore individuals' externality beliefs, then examine how these beliefs relate to individuals' preferences for redistribution.

### 3 Sampling Methodology

Our empirical results are based on two independent pre-specified surveys. Data for the first survey (henceforth called Survey 1) were collected between December 6th and 24th 2021, where respondents were recruited through the survey provider Lucid. Data for the second survey (henceforth called Survey 2) were collected between August 9th and October 8th 2022, where respondents were recruited through the survey provider Dynata. Both Lucid and Dynata are commonly used by economic researchers (see e.g. Haaland and Roth, 2021; Andre et al., 2022).<sup>11</sup>

---

<sup>11</sup>Lucid 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

The planned analyses for each survey were pre-specified.<sup>12</sup>

### 3.1 Survey 1

Survey 1 had two primary goals. The first was to establish whether individuals think economic inequality itself changes various societal outcomes – in other words, to gather descriptive evidence on individuals’ inequality externality beliefs. The second goal was to conduct a video information experiment to explore how *changing* these inequality externality beliefs affects individuals’ redistributive preferences.<sup>13</sup> Due to the complexity of the information experiment, the relevant methodology and results will be discussed together in Section 5.

5,007 subjects completed the survey, which is reduced to 4,371 after routine data quality checks.<sup>14</sup> Methodological details and the full questionnaire can be found in Appendix B.1.

### 3.2 Survey 2

Survey 2 was designed after we analyzed results from Survey 1. It had the goal of further delving into the main *descriptive* results from Survey 1 with an independent sample. Note that Survey 2 does not affect the information experiment in Survey 1.

A total of 2,478 respondents completed the survey, which is reduced to 2,360 after routine data quality checks.<sup>15</sup> Further methodological details can be found in Appendix B.2.

### 3.3 Merged descriptive sample

We have two sources of descriptive results on individuals’ inequality externality beliefs. The first is the control group in Survey 1.<sup>16</sup> The second is Survey 2, which was intentionally designed to improve our understanding of these initial descriptive results. To ensure robustness of our results, 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.<sup>17</sup> The descriptive results in Section 4 thus uses the merged sample of the control group of Survey 1 and all of Survey 2. It has a total of 3,292 respondents.<sup>18</sup>

---

programs, targeted online advertisements, and institutional partnerships.

<sup>12</sup>See AsPredicted.org #82083 and #104271.

<sup>13</sup>Individuals were shown either no relevant information (control group) or one of four short videos that should change either their inequality externality beliefs (three treatment groups) or their broad economic fairness concerns (one treatment group). We then measured redistributive preferences and other outcomes within treatment groups.

<sup>14</sup>We targeted this sample size to ensure roughly 1000 respondents in each of the five experimental treatment groups in the final sample. This gives us sufficient power to identify even small treatment effects (see Haaland et al., 2020).

<sup>15</sup>The final part of the survey is split into eight arms to explore different externality channels (crime, trust, social unrest...). We targeted a sample size that would be able to provide sufficient descriptive evidence for each of these eight survey arms.

<sup>16</sup>There were four treatment groups and one control group in Survey 1. Individuals in the treatment groups were given information that affected their inequality externality beliefs before the majority of these beliefs were elicited by design. Individuals in these groups are therefore not included in the descriptive results. See further details in Section 5.1.

<sup>17</sup>The exception is for general externality beliefs, where there are small but noteworthy differences across samples due to the question ordering (see Section 4.1)

<sup>18</sup>This includes the 932 respondents in the control group of Survey 1 and all 2,360 respondents from Survey 2.

### 3.4 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 generally representative of the 2021 U.S. population. We also have a wide range of incomes in both surveys. Similarly to other studies using access-panels, both surveys oversample white respondents and college-educated respondents.<sup>19</sup> Re-weighting respondents for full representativity on these dimensions does not change reported results significantly.

In general, we believe the sample is a reasonable approximation of U.S. society as a whole. This is particularly true as we have a fully representative range of political views, which are the most statistically significant determinant for inequality externality beliefs among our observables. We discuss sample representativity further in Appendix B.4.

## 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).

### 4.1 General externality beliefs

First we explore subjects' general externality beliefs in two sets of questions, one from Survey 1 and one from Survey 2.<sup>20</sup> General externality beliefs concern how respondents think more economic inequality *generally* changes society. 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 are detailed in Appendix B.6.

**Results** Table 1 illustrates answer distributions to this question. Only a small minority (10-15%) in each sample states that inequality has positive societal effects, or constitutes an overall positive externality. This stands in stark contrast with the 60-63% of respondents who state that inequality changes society somewhat or a lot for the worse, or constitutes a negative externality. In other words, a majority of subjects believe that inequality has negative societal effects.

A very low proportion of respondents believe that inequality does not affect society at all. This ranges between 3.2% and 15.9% in the two surveys. The difference is most likely from a question ordering effect detailed in Appendix B.6, and the two numbers can be seen as lower and upper bounds. The belief that inequality affects society in one way or the other is thus shared by 86-97% of respondents, a large majority. All results discussed above remain very similar when

---

<sup>19</sup>These disparities are typical for similar studies, see e.g. Stantcheva (2021).

<sup>20</sup>This is a rare case where result differ across samples due to the question design. We thus present both answer distributions separately.

Table 1: 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	11.1%	7.6%
Good and bad effects cancel	21.1%	13.0%
Somewhat for the worse	35.2%	36.7%
A lot for the worse	25.1%	26.4%
Inequality does not affect society	3.2%	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 B.6.1 for the full question text.

changing the question phrasing<sup>21</sup> and reference level of inequality.<sup>22</sup>

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”,<sup>23</sup> 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” (28.8%) or “extremely serious” (22.4%) issue.<sup>24</sup> A large majority of respondents also answer “Yes, definitely” (24.7%) or “Yes, maybe” (46.5%) 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 with the same income in more or less unequal societies), the quality of democratic institutions, and generalized trust.<sup>25</sup> Survey 2 also asks a Placebo question where the sensible answer is

<sup>21</sup>Specifically changing “inequality” to “equality” or “differences in income and wealth”, see Appendix C.1.

<sup>22</sup>In Survey 1, no explicit reference level of inequality was given. In Survey 2, we describe and show figures depicting the “less equal” and “more equal” income distributions. In a separate question in Survey 2 we also ask respondents how they think more economic inequality would change the United States, specifically creating a well-known reference point. Answers remain similar, although the share of respondents that state that inequality changes the US for the better (for the worse) increases (decreases) by 10 percentage points.

<sup>23</sup>Full 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?”

<sup>24</sup>42% answer that this is “not a very large” or “small” issue, and 7% do not believe it is an issue at all.

<sup>25</sup>We 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

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?*”<sup>26</sup> When the variable in question was difficult to accurately define in a few words, we also added a short definition before the question. See Tables E10-E11 for these definitions and the full wording of each variable in each survey.<sup>27</sup>

**Results** Figure 1 characterizes the responses to these questions from the merged descriptive sample.<sup>28</sup> 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 1. 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 this result.<sup>29</sup>

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 (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.<sup>30</sup> On one hand, one could argue that inequality promotes growth by quality of local public goods, and overall quality of life.

<sup>26</sup>To 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”. Questions are symmetric around a neutral option and answer order was randomly flipped.

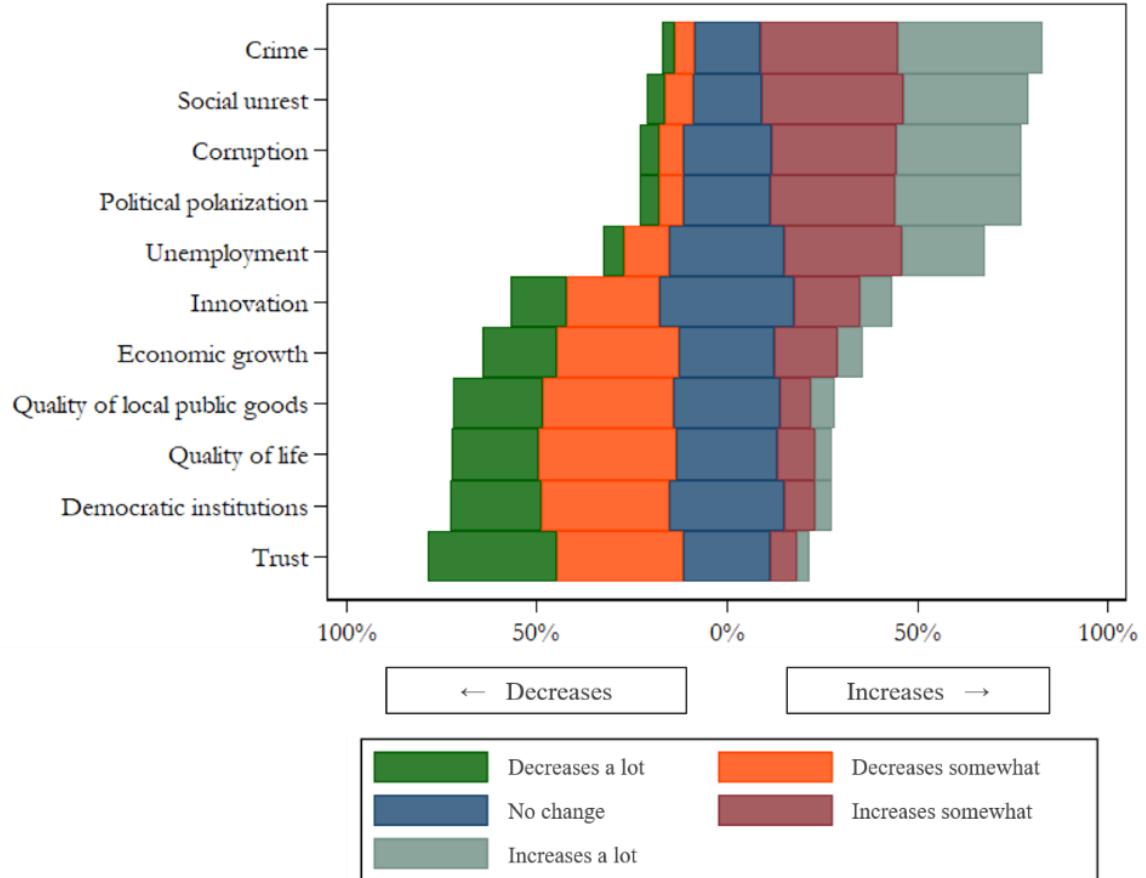
<sup>27</sup>To assess whether subtle differences between questions matter for responses, 25% of questions in Survey 2 were randomly presented in the identical way as in Survey 1. This does not affect results.

<sup>28</sup>The exact numbers are shown in Table D2.

<sup>29</sup>To rule out any unknown design feature affecting results we discuss a placebo question in Section 4.2.1. This question is exactly centered around “No change”.

<sup>30</sup>While one could conceivably argue that inequality has a positive effect on the other outcomes we study,

Figure 1: 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 D2 respectively. The exact numbers are shown in Table D2.

strengthening incentives. This has been a traditional argument for maintaining high inequality levels, as this Boris Johnson quote from 2013 exemplifies:

“I stress – I don’t believe that economic equality is possible; indeed some measure of inequality is essential for the spirit of envy and keeping up with the Joneses that is, like greed, a valuable spur to economic activity.”

On the other hand, 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. Martin Wolf, the chief economics commentator of the *Financial Times*, wrote such an argument in 2019:

---

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).

“[Inequality] makes politics far more fractious, undermines social mobility; weakens aggregate demand and slows economic growth.”

Between these two arguments, Americans’ 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 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.

Finally, we note that percentage of respondents who believe that economic inequality does not affect society in any way is consistent with the results from the preceding section 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.<sup>31</sup>

#### 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 D3-D4. 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 changes descriptive statistics the most 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.<sup>32</sup> This does not seem to be due to the word “inequality” itself, as using “differences in income and wealth” has an almost negligible effect. Still, we note that the negative externality option is always more popular with respondents than the no-externality or positive externality options under any phrasing choice – even in the “equality” setting – and thus that our general results are robust to these changes. See Appendix C.1 for more details on our phrasing variations.

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%

---

<sup>31</sup>Taken at face value this indicates that roughly 96% of respondents believe in at least one inequality externality, closer to the upper bound (97%) than the lower bound (85%) shown in Table 1. The respondents who answer this way in the general externality and specific externality questions do not perfectly overlap, however. In the most conservative case, where all the individuals who fall into one of these two groups in fact do not believe in any inequality externalities, the total share of the population who believes economic inequality affects society is 93.3% in Survey 1 and 83.0% in Survey 2.

<sup>32</sup>The 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.

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?*”). This can be seen in Figure D2.

#### 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 E12-E14, which regress different externality beliefs on sociodemographic variables, trust in government, and general fairness views for our main sample.<sup>33</sup>

The results show that 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 1 for these two groups in Figure 2.

These beliefs are still relatively homogeneous across party affiliation, however. 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.<sup>34</sup> We show this in Figure D5. We will further discuss and contextualize these party differences shortly.

We also find that individuals who believe that the current economic system is unfair (because people do not have the same opportunities to succeed) are much more likely to believe in negative externalities. Respondents who trust the government are also somewhat more likely to believe in negative externalities.

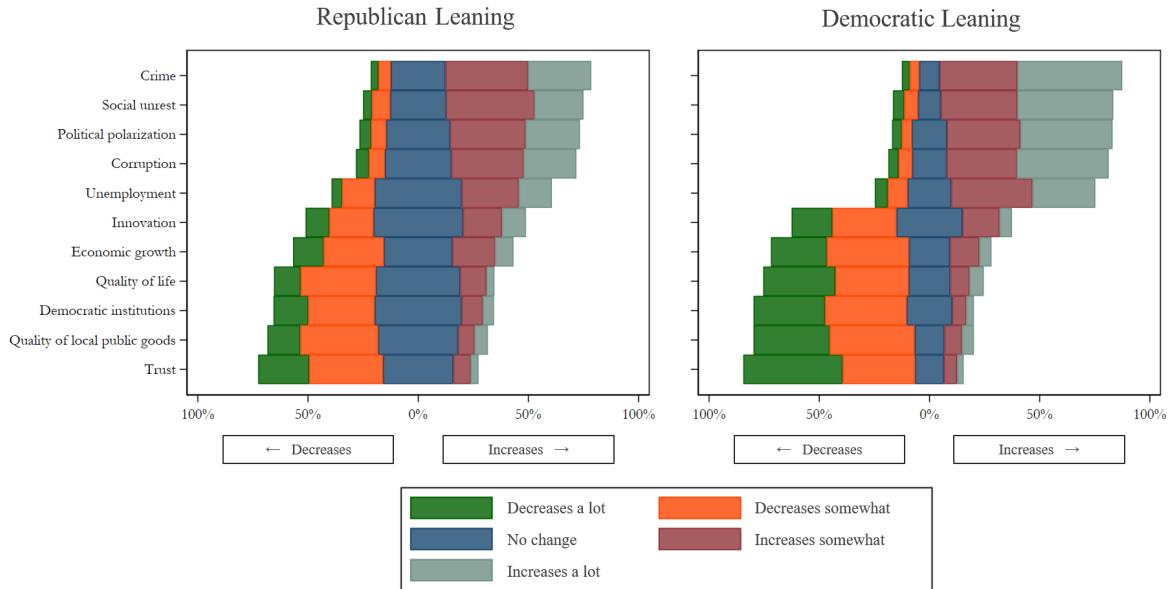
Gender does not have a large correlation with externality beliefs, although men are on average slightly less likely to think inequality reduces economic growth. Respondents who self-identify as black, however, are significantly less likely to believe most inequality externalities. It is difficult to say why this is so; we hypothesize that racial inequalities and their associated externalities may partly supplant the economic dimension for these individuals.

Income and wealth generally have no strong correlation with inequality externality beliefs. In the few exceptions, higher-income respondents generally believe somewhat less in inequality externalities. This shift is significant for innovation, corruption, and economic growth. In each of

<sup>33</sup>Note 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 do not change notably if excluding the fairness and government trust variables.

<sup>34</sup>The 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 D5 for details.

Figure 2: 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.  $n \in \{1598, 1694\}$ .

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.

College-educated individuals are consistently more likely to believe in negative externalities.<sup>35</sup> Age does not have a large effect on externality beliefs in our sample. 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  $\sim 5$  p.p.). Furthermore, income inequality on the state level does not correlate with externality beliefs (not shown).

#### 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. We illustrate this in Figure 3 by using the questions on fairness and externalities that were posed *before* the treatment intervention, allowing us to use the full Survey 1 sample ( $N = 4371$ ). These 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,*

<sup>35</sup>Note 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 are, on average,  $\sim 5$  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 D3-D3).

*because everybody has an equal opportunity to succeed*" and (ii) "*Countries with more economic inequality generally function worse*". A similar share of Democrats indicate anti-inequality fairness views (68%) and externality beliefs (71%). The same shares for Independents keep a similar distance, at 57% and 54%. For Republicans, however, those believing the economic system is unfair drops to 34% while the share believing more unequal countries function worse stays close to the result for Independents at 51%, indicating less polarization in the externality question.<sup>36</sup>

The above is only one question. However, the main sentiment – that the drop in anti-inequality sentiment by party affiliation is larger for fairness questions than for externality questions – is very consistent. Indeed, when splitting the sample by Republican- or Democrat-leaning respondents, this is true for nearly any combination of fairness and externality questions across both our samples. 23 out of 24 inequality externality belief questions have less party-based polarization than *every* single fairness questions we have data on (9 questions). The last externality question is still less party-polarized than 8 of the 9 fairness questions.<sup>37</sup> This is illustrated for the 18 fairness- and externality-based questions in Survey 1 in Figure 4, and for the remaining questions in Survey 2 in Figure D8. This already strong result is generally robust to adding a set of demographic controls.<sup>38</sup> Generally speaking, that fairness views are more polarized across party affiliation than externality beliefs is an extremely consistent result.

A similar result can be found in both wealth and income, which we show an example of in Figure 5. Inequality externality beliefs, again measured with the pre-treatment externality question, are relatively constant across both the wealth and income distribution at approximately the average 61%. Indeed, externality beliefs slightly increase in income. Fairness views, however, shifts from 54% (income < \$25k) and 57% (wealth < \$25k) to 44% (income > \$100k) and 46% (wealth > \$200k). Both shifts are strongly statistically significant.<sup>39</sup> This is again a general result in our full samples. Fairness views are strongly correlated to both wealth and income.<sup>40</sup> Inequality externality beliefs, however, are generally not strongly correlated to either income or wealth (with certain exceptions as noted above for innovation). 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 a similar pattern holds, although there is some overlap between questions. This is illustrated for Survey 1 in Figure 6, and for Survey 2 in Figure D9.<sup>41</sup>

---

<sup>36</sup>The figure and values are without controls. The result is robust to adding a standard set of controls.

<sup>37</sup>The general externality question in Survey 2 is the most party-polarized externality question. It has a 0.12 p.p. larger response share difference between parties than the least polarized fairness question in either sample, a question on the fairness of redistribution if two individuals work equally hard from Survey 2.

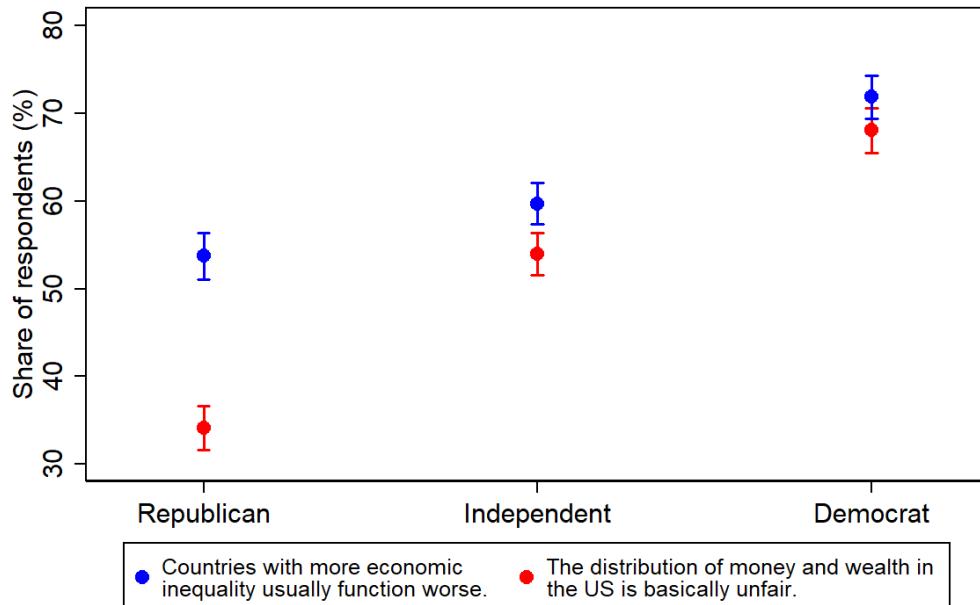
<sup>38</sup>The result is fully robust to adding controls within Survey 1. Within Survey 2, one additional externality question (on democratic institutions) becomes more party-polarized than the least polarized fairness question.

<sup>39</sup>The figure and values are without controls. The result is robust to adding a standard set of controls.

<sup>40</sup>This follows the literature; generally, individuals with higher economic status believe that the distribution is more fair (Valero, 2021).

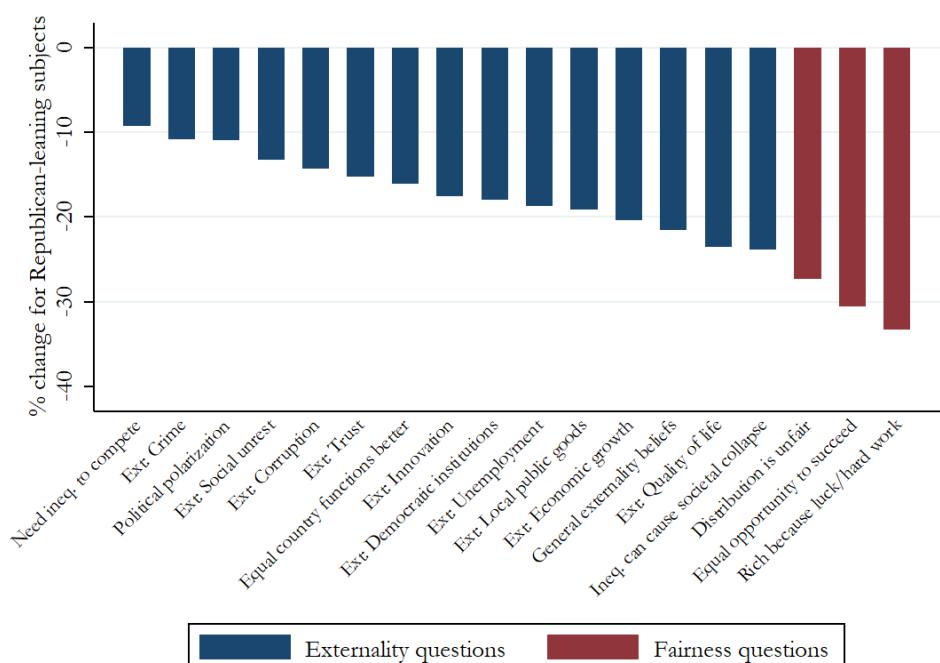
<sup>41</sup>This strong result is robust to adding demographic controls within Survey 1. In Survey 2, adding demographic controls makes three more externality questions overlap.

Figure 3: An Example of Externality Beliefs and Fairness Views over Party



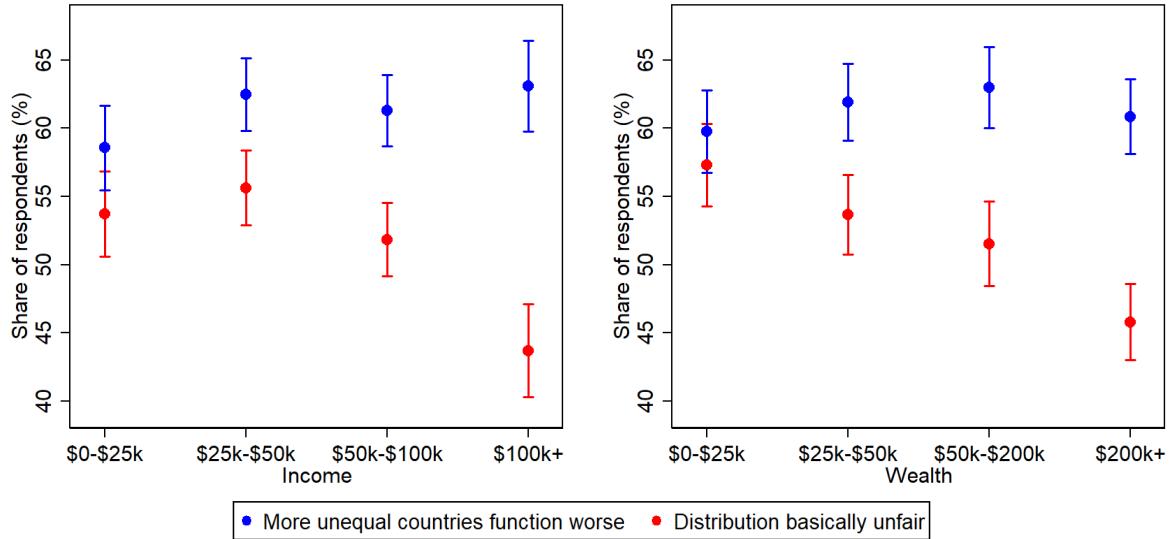
**Note.** This graph uses the pre-treatment externality and fairness questions with the full sample (n=4371). Respondents are asked to agree or disagree with the following two statements: “*The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity to succeed*” and “*Countries with more economic inequality usually function worse*”. The equivalent graph for Survey 2 respondents is Figure D6

Figure 4: The Effect of Party Affiliation on Fairness Views and Inequality Externality Beliefs



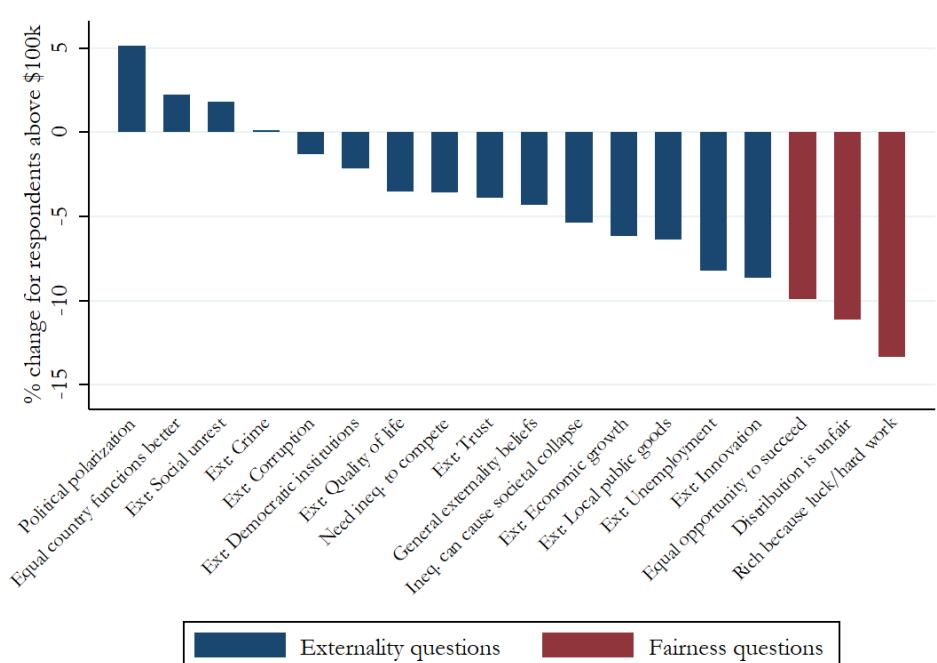
**Note.** Drop in anti-inequality sentiment for Republican-leaning respondents across every externality and fairness question in Survey 1 without any controls. With a standard set of controls the same relation holds (all fairness questions have larger polarization than any externality question). Questions 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 D7

Figure 5: 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 ( $n=4371$ ). 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 D7.

Figure 6: 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.

#### 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 which externality channels are deemed most important by respondents we ask them the following 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*” in Survey 1.

This question only makes sense if the respondent thinks inequality has at least one negative or positive externality. As such, 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 1.<sup>42</sup> There is thus a selection effect; each group should not be seen as a representative sample, but rather an indication of which externalities matter to the subsection of respondents who believe inequality affects society negatively or positively, respectively.

The average responses to this question are shown in Figure 7. There is significant noise in these estimates – the question is complicated and was towards the end of an intellectually tiring survey – but the answers are notably different between the negative and positive externality versions. Most of the options we give are deemed somewhat important as negative externalities. Crime (16.4%) and corruption (14.5%) are highest on the list, followed by social unrest (12.8%) and trust (11.7%). Indeed, every option except innovation (4.9%) is within 8 percentage points of each other. However, the positive externalities are dominated by economic factors. Economic growth (24.7%) is first, then innovation (14.8%) and unemployment (14.5%). As these economic factors are relatively less important in the negative externality version – so among those who thought inequality negatively affects society – this is a potentially important extension to the descriptive results in Figure 1.

In Survey 2 we also ask respondents whether they deem a specific inequality externality “meaningful” in the sense that it is “*something politicians and policy-makers should be focused on, or [...] ultimately not very important*”. Results here are qualitatively similar; the three most meaningful channels are considered to be the quality of democratic institutions (70%), crime (67%), trust (66%), and corruption (65%).<sup>43</sup> A majority of respondents believe the given externality is “generally meaningful” (30%) or “very meaningful” (32%).

#### 4.2.5 Top- or bottom-inequalities

One might think some inequality externalities depend heavily on inequalities near the bottom – the amount of relative poverty<sup>44</sup> – and that other inequality externalities depend more on inequalities near the top. To explore this we ask respondents in Survey 2 what type of inequality matters more for a given externality.<sup>45</sup>

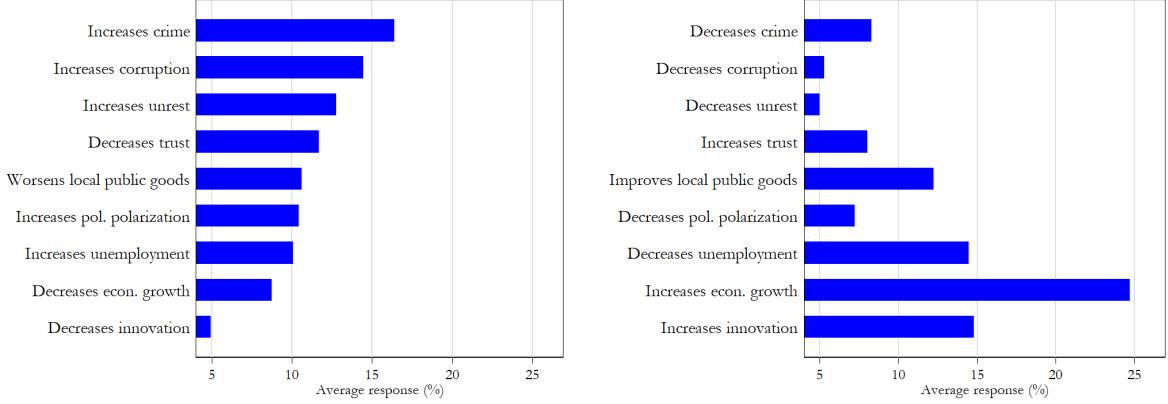
<sup>42</sup>We also allow respondents to self-select out of the question by stating that changed their mind.

<sup>43</sup>The quality of democratic institutions is not included in Figure 7 due to a coding error.

<sup>44</sup>We note that relative poverty, unlike absolute poverty, is an inequality metric.

<sup>45</sup>Example 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.*

Figure 7: 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).

Table 2: What Matters: Inequalities Near the Top or Bottom?

	Pol. polar.	Crime	Corrup- tion	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 2. 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 externality where top inequalities matter more than bottom inequalities is that of corruption.

#### 4.2.6 Non-monotonous inequality externality beliefs

A key assumption for our main results to be easily interpretable is that inequality externality beliefs are monotonous in 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*”. This is a complicated question; about 20% of respondents answer that it is too complicated for them. Of the remaining respondents, the large majority (81%) expresses monotonic externality beliefs. The externalities with the largest share of non-monotonic beliefs are economic growth (24%) and innovation (25%). The full table is shown in Table E15. Among respondents who have non-monotonic beliefs in

innovation and growth, follow-up questions indicate that the non-monotonicity implies a positive externality at low inequality levels and a negative externality at high inequality levels. In other words, these respondents believe that more economic inequality increases economic growth and innovation when economic inequality is low, but decreases economic growth and innovation when economic inequality is high.

We also elicit individuals' predicted levels of outcomes (crime, trust, and so on) when the level of average income or economic inequality changes. The results from this exercise are discussed in Appendix C.2. Respondents believe the level of economic inequality is particularly predictive for political polarization and corruption, whereas the level of average income in a society is particularly predictive for economic outcomes.

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 consequences, even on economic outcomes. 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 which, when combined, allow for a complete assessment of the topic. 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. 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 8. The survey is divided into three parts. In Part 1, we elicit sociodemographic information that is needed to check for representativeness.<sup>46,47</sup> Part 2 presents subjects the intended information. Part 3 elicits respondents' preferences for redistribution and inequality externality

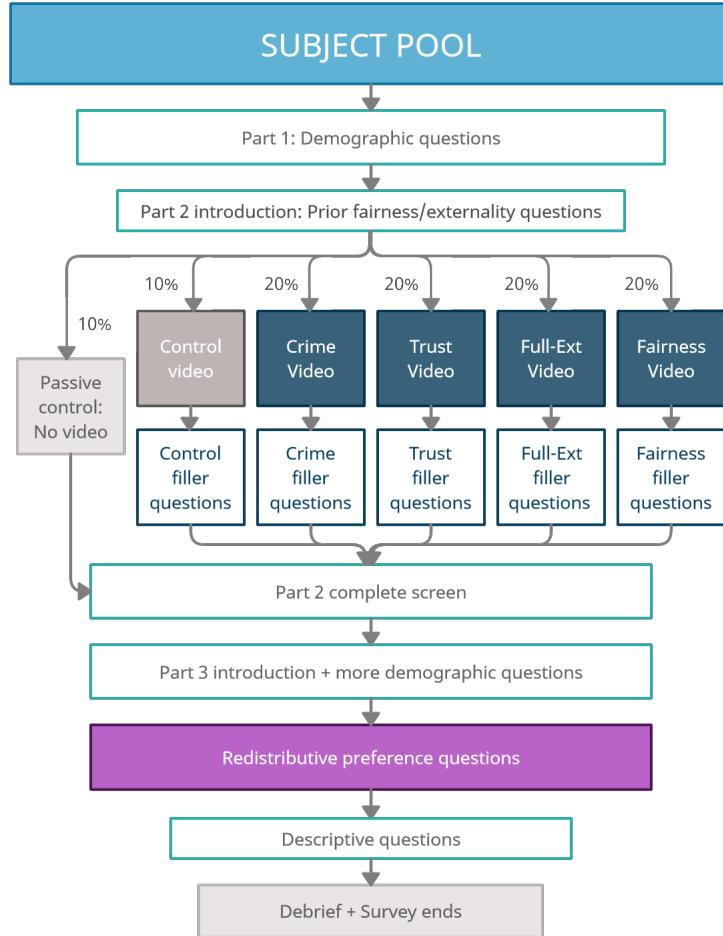
---

<sup>46</sup>This information is also used to check for selective attrition across the treatment groups, which we do not find any significant evidence of.

<sup>47</sup>We 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.

beliefs, which constitute our main reduced-form and first-stage outcomes respectively.

Figure 8: Survey Flow of Survey 1



### 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.<sup>48</sup> 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 video information treatment.<sup>49</sup>

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

<sup>48</sup>These 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.*”

<sup>49</sup>Subjects 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.*”

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.

Before discussing the video treatments explicitly we introduce what we call our *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, suspicion of future questions, or attrition problems, all of 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.<sup>50</sup> 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 8). 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. First-stage responses on externality beliefs and fairness views are also measured directly in Section 5.2.1, preventing concerns of a reverse effect (where the filler questions change opinions in the opposite way of what was intended).

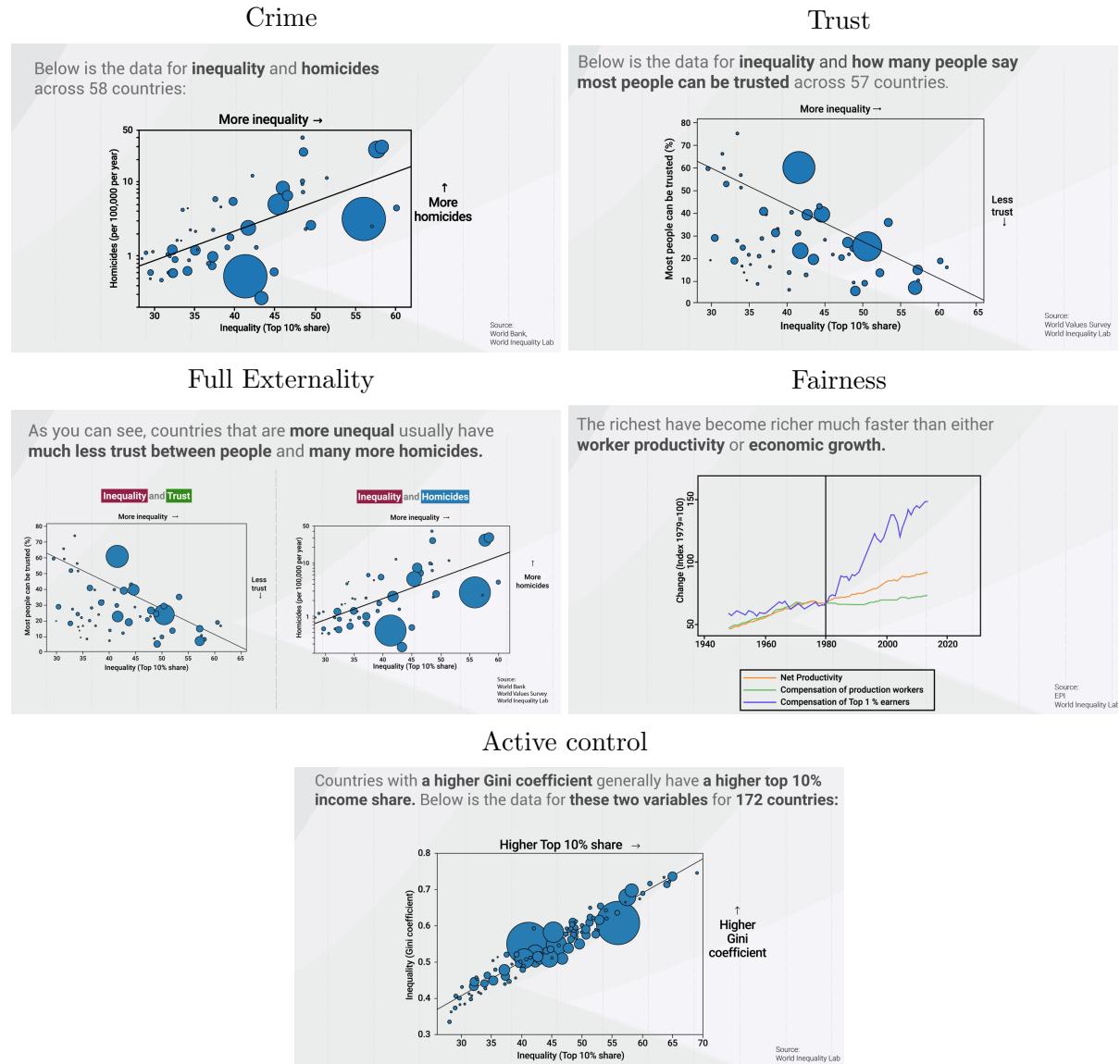
---

<sup>50</sup>The specific filler questions are discussed further below.

### 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 9. 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 questions correctly to proceed with the survey.<sup>51</sup> They are then showed the filler questions discussed above.

Figure 9: 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

**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 9, the video first

<sup>51</sup> Respondents who answer incorrectly are able to change their answers.

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 9, 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). 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 9. The stimulus includes information that blue-collars' wages stagnated while their productivity increased since the 1980s. Wages of the top 1% earners, 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.

**Dual control groups** The second methodological contribution we make is the concept of a *dual control group*.

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.<sup>52</sup> The researcher can thus *test* whether attention effects and priming about the relevant concept (inequality, in our case) have important effects, which presents a large improvement over the uncertainty involved in single control groups.

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

---

<sup>52</sup>The 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.

employed or formalized within internet-based video information surveys. The usage of pre-specified merging criteria in such groups is also rare and as far as we know novel within the economics literature. 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. 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. This is shown in the screenshot in Figure 9. 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 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 (a) have already thought about the measurement of inequality and (b) whether they knew that researchers had different ways of measuring inequality before the survey began.

**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 C.3.

**Summary of the variation induced through the treatments** Treatment groups 1 to 3 shape respondents' beliefs about externalities without referring to anything related to fairness. Comparing redistributive preferences of these groups to the baseline thus gives us insights into the effect of (different) information on the consequences of inequality on preferences for redistribution. 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. Comparing the magnitude of the treatment effects allows inferring what type of information has a larger effect in affecting preferences for redistribution.

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, because it enables us to form clear hypotheses for potential treatment effects. Second, because 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. There is no strong cross-country correlation between inequality and economic growth, for example, which prevents us from designing a positive externality-based growth-treatment similar to the crime or trust treatments. Furthermore, such externalities are easily confused

with labor supply effects (i.e. lower taxes lead to a high labor supply) which would complicate our design significantly.

We compare observable characteristics across the treatment and control groups across in Appendix C.4. 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.<sup>53</sup> This also indicates a limited effect of attrition across treatments.

### 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. 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.<sup>54</sup>

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.<sup>55</sup> The index was pre-specified as our main outcome variable.

**Externality beliefs** The next battery of questions elicit respondents' externality beliefs. These are composed of the general externality belief question discussed in Section 4.1 and the many specific externality belief questions discussed in Section 4.2.<sup>56</sup>

**Fairness views** The final module of the survey elicits respondents' fairness views using 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. Note that this question is deliberately asked in a way that relates directly to our fairness treatment. It, thus, serves as a first-stage outcome for our fairness treatment. We supplement this question with a more classical question that elicits subjects' perception of whether one gets rich through hard work or luck.

---

<sup>53</sup>Note that our regressions control for observable characteristics; including or excluding these regressors does not change the results, underlining the fact that they do not mirror relevant variation across treatment groups.

<sup>54</sup>These 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").

<sup>55</sup>Where 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.

<sup>56</sup>General externality question: "Generally speaking, do you think more economic inequality changes society for the better or for the worse? Specific externality question example: "How does more economic inequality change the amount of crime in a country?"

#### 5.1.4 Theoretical mechanism

Before presenting the experimental results it is worth taking a step back and think about the theoretical mechanism that drives 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  $\eta_i$  (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;

$$\frac{dRP_i(\eta_i, G_i, X_i)}{dT_q} = \frac{\partial RP_i}{\partial \eta_i} \frac{\partial \eta_i}{\partial T_q} + \frac{\partial RP_i}{\partial G_i} \frac{\partial G_i}{\partial T_q} + \frac{\partial RP_i}{\partial X_i} \frac{\partial X_i}{\partial T_q} \quad (3)$$

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 C.3), 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  $T_\eta$  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 3 we can then conclude that,

$$\frac{dRP_i(\eta_i, G_i, X_i)}{dT_\eta} \propto \frac{\partial RP_i(\eta_i, G_i, X_i)}{\partial \eta_i}, \quad (4)$$

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  $\eta$  for  $G$ ).

We will now discuss the first-stage of the experiment, which is the effect of the treatment  $T_q$  on externality beliefs  $\eta_i$  or fairness views  $G_i$  ( $\frac{\partial \eta_i}{\partial T_q}$  and  $\frac{\partial G_i}{\partial T_q}$ , respectively). Afterwards we will discuss the reduced-form, which is the effect of the treatment on redistributive preferences ( $\frac{dRP_i}{dT_q}$ ).

## 5.2 Experimental results

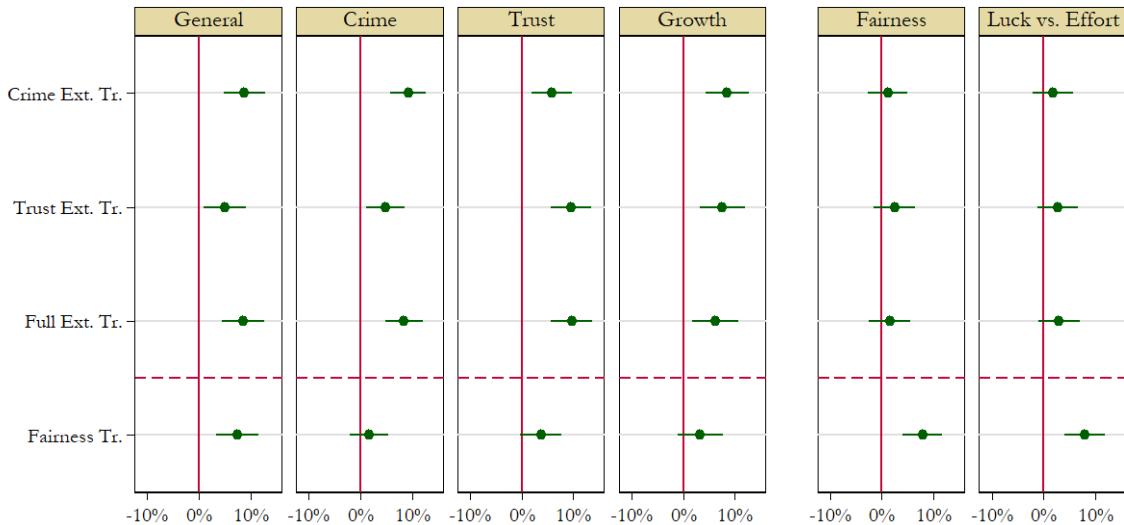
Below we discuss the results from the main information experiment. This experiment aims to (i) establish a causal link between inequality externality beliefs and redistributive preferences, and (ii) to compare the redistributive effect of inequality externality beliefs to economic fairness views, which is a well-known determinant of redistributive preferences. We first discuss how the information treatment affected first-stage outcomes.

### 5.2.1 First-stage outcomes

The first-stage outcomes of the experiment are the information treatments' effects on externality beliefs or fairness views. They are characterized by Figure 10.

As can be seen in the first four columns of Figure 10, 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. None of the externality treatments significantly affect fairness views (last two columns), showing that the fairness-based spillovers discussed in Section 5.1.4 are limited for the externality treatments.<sup>57</sup> Indeed, this provides some evidence that the two concepts are relatively independent.

Figure 10: 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 E19 presents the point estimates and standard errors. *Significance levels:* \*10%, \*\*5%, \*\*\*1%.

The fairness treatment, meanwhile, 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.<sup>58</sup>

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-

<sup>57</sup>We do note that there seems to be externality-based spillovers; the crime treatment changes their trust externality belief, for example.

<sup>58</sup>The main consequence of these spillovers is that the fairness treatment's reduced form coefficient could be inflated. This would affect the relative magnitude of the externality and fairness treatment effects.

externality beliefs are at 75% and 67% 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 questions about externality opinions. These questions ask respondents to write about how they think inequality changes society without prompting them specifically in any further direction. Table E18 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).<sup>59</sup> 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, which softens 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. These results show that the prerequisites for Equation 4 substantially hold for the externality treatments. In other words, any significant externality treatment effect would strongly imply that externality beliefs is a causal determinant of redistributive preferences.<sup>60</sup>

### 5.2.2 The effect of the information treatments on redistributive preferences

We now study whether the treatments shifted respondents’ demand for redistribution. Table 3 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 for three of our four measures of redistributive preferences. The effect on the aggregate redistributive preference index is significant at the 1% level; 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).<sup>61</sup> Following the careful experiment design and the discussion in Section 5.1.4, this represents one of our main findings; inequality externality beliefs causally affect redistributive preferences.

---

<sup>59</sup>The same is true for other related words, such as “Youtube”, “infographic”, and so on. As the question is specifically about externality beliefs, this text analysis cannot be conducted on fairness views.

<sup>60</sup>As there is a significant effect of the fairness treatment on general externality beliefs, this relation is slightly weaker for the fairness treatment. As we do not aim to establish a causal effect of fairness views on redistributive preferences – this is already well-known in the literature – this is less crucial for our main analysis.

<sup>61</sup>The higher preferences for redistribution among women in our sample represent a replication of a frequent finding (e.g. Alesina and Giuliano, 2011). We can also compare the treatment effects with the correlation between redistributive preferences and political leaning or fairness views. The full externality treatment effect corresponds to about  $\frac{1}{6}$  of the difference between Republican- and Democrat-leaning subjects, and about a  $\frac{1}{7}$  of the difference between those who think the economic system is fair or unfair (when including both as control variables). If only including political affiliation and demographic controls, the treatment effect is similarly sized and  $\frac{1}{8}$  of the difference between Republican- and Democrat-leaning subjects.

Table 3: Main Treatment Effects of Video Information Experiment

	(1) RP Index (st. dev)	(2) Wants redistribution (0-1)	(3) Increase top taxes (0-1)	(4) Gov. reduce ineq. (0-1)	(5) 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.<sup>62</sup>

We 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 – which we will discuss shortly – 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 might believe that the effect of inequality on trust and crime is primarily affected by inequalities near the bottom, on which top tax rates might not be an effective tool.<sup>63</sup> We also note that the active control showed a surprisingly high treatment effect for this variable (see Appendix C.3) – the non-result from the externality treatments could also be driven by this anomaly.

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

<sup>62</sup>Theoretically this is sensible. Say that individuals are rational and that a fraction  $\alpha$  of their redistributive preferences are determined by inequality externality beliefs. If trust is a fraction  $\gamma$  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 7, 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.

<sup>63</sup>This is somewhat but not entirely corroborated by Table 2, which shows that more than half of respondents believe that top inequality matters for crime rates and generalized trust levels.

than that of the full externality treatment (1.94 times the size).<sup>64</sup> This represents our first indication of the relative strength of the fairness and externality arguments. However, caution should be employed in interpreting these numbers directly. The treatment effects are *marginal* effects, and their relative magnitude is determined not only by the intrinsic importance of these arguments in shaping redistributive beliefs, but also by the malleability of these views in respondents and video design choices (see Equation 3). We re-visit this topic in Section 5.3.

It is also notable that, unlike for the externality treatments, we find a strong and significant effect of our fairness treatment on respondents' preferred top tax rate. Comparing this to the full externality treatment, it indicates that explicit information about the evolution of top incomes can be more effective in gathering support for increasing top taxation than informing about inequality's effects on trust and crime. If the goal is instead to change broader views on inequality or redistribution, inequality externality arguments may approach the same level of efficacy as fairness arguments, and may indeed be less polarizing and more effective across the income distribution. We return to the topic of polarization in Section 6.

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 this work (2% - 9%) are similar to those we find (2% - 12%) – where all treatment effects use a dummy variable to indicate inequality being a “serious” or “very serious” problem. The main redistribution treatment effect in Stantcheva (2021) is 9%, which is similar to the treatment effect in our fairness treatment (12%).

**Mechanism** We have already seen that the treatments shift the targeted first-stage belief. The next question we can ask is whether the reduced-form treatment effect is mediated through a shift in these first stage beliefs.<sup>65</sup> Our treatment is designed to induce a shift in externality beliefs and fairness views which yields a change in preferences for redistribution. Thus, the magnitude of the treatment effects should be reduced after controlling for externality and fairness views elicited *after* the treatment. This is what we find, which is shown in Table E23 and detailed in Appendix C.5. This further shows that the mechanism is the expected one.

**Emotional reactions** To explore the psychological channel through which the treatments operate and which could explain the differences in their effects we asked respondents to state their emotional reaction to the video. At the end of the survey we ask respondents to recall which emotion they felt after watching their given video; respondents were able to answer (potentially several of) anger, interest, surprise, indifference, confusion, or concern.

These emotional reactions to the video also differ across treatments. The most striking finding is on anger, which we show in Figure 11. 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

---

<sup>64</sup>This corresponds to about  $\frac{1}{3}$  of the difference between Republican- and Democrat-leaning subjects under standard controls, or  $\frac{1}{4}$  under only demographic controls (excluding prior fairness views).

<sup>65</sup>Note that we did not include this analysis in our pre-analysis plan. Nonetheless, we believe it is of interest for the reader.

other video.<sup>66</sup> 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*.<sup>67</sup>

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 effects** Which individuals reacted particularly strong to the information treatments? To explore this we pre-specified certain interactions between our treatment dummies and a battery of baseline characteristics. Below we discuss two interactions we deem to be of particular importance.

The first compelling heterogeneous treatment effect is through income.<sup>68</sup> 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). These individuals 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 ( $\beta_G = 0.143$ ). The two treatment effects are essentially equal above \$50k.<sup>69</sup> In total, the larger size of the fairness treatment effect is driven entirely by individuals with incomes below \$50k. This is shown in Table E26. This has intriguing consequences for the efficacy of each

Figure 11: Treatment Effects on Anger



This figure characterizes the treatment effect of regressions regressing the respondents reporting that they experienced anger after watching the video. Error bars depict 95% confident intervals. Standard errors are clustered at the respondent levels. The distribution of emotional reactions by treatment is found in Table E21

<sup>66</sup>The 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%).

<sup>67</sup>The 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).

<sup>68</sup>Note that we did not pre-specify this interaction, but include it as it is both robust and of particular interest.

<sup>69</sup>In this sample  $\beta = 0.111$  for the full externality treatment and  $\beta = 0.117$  for the fairness treatment.

argument for different income groups, which is further discussed in Section 6.

The second compelling heterogeneous treatment effect is through the self-reported variable that indicates whether respondents learned something new. This is conceptually linked to the preceding discussion of the mechanism; the intuition is that respondents who learned something new are also more likely to adjust their beliefs conditional on receiving the information. Table E27 displays the results of such a regression. As expected, respondents who indicate to have learned something new in the video have significantly higher treatment effects for the redistributive index than those who did not learn something new in the crime, full externality, or fairness treatment groups. This corroborates the findings from the previous section that our reduced form effects can be explained through a shift in actual beliefs.

**Robustness of treatment effects** The conclusions from the information experiment are generally very robust to various specifications. In Appendix C.6 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 C.6.

**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%).<sup>70</sup>

There is no statistically significant difference in the percentage of respondents who believe the survey was left-wing biased over treatment groups. The control group is only mildly different (19.1%) to the treatment average (22.3%). All treatment groups are between 21% and 23%. This is shown in Table E50. All main treatment effects are robust to including a dummy for left-wing bias as a control.

### 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

---

<sup>70</sup>The corresponding statistics in the Survey 2 is 16.6% left-wing biased, 5.4% right-wing biased, and 78.1% unbiased.

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 3. The fairness video has about twice the effect on our pre-specified index of redistributive preferences as the full externality video. We can reject equality of the two coefficients at the 5% significance level ( $p = 0.012$ , t-test). 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.<sup>71</sup>

**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 regressions that include fairness views, externality beliefs, political preferences, and “economist determinants” as regressors, and we compare the explanatory power of these models using the adjusted  $R^2$ . These regressions were all pre-specified.

Table 4: Horse-Race: Predictive Power of Beliefs on Redistributive Preferences

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index 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%.

Table 4 displays the results of these regressions. Note that all regressions only include observations from the baseline control group. Column (1) characterizes a regression that only includes the control vector; Column (2) includes our two main fairness variables: the belief

<sup>71</sup>As discussed in Section 5.2.1, the fairness treatment effect could be somewhat overestimated due to spillovers into inequality externality beliefs.

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).

The controls include groupings for gender, age, income, employment status, education and region, which explains only about 12% of variation in the redistributive preference index. All models explain more variation than the only-control regression, as expected. Focusing on the adjusted  $R^2$ , it becomes clear that the fairness variables have the most predictive power for preferences for redistribution with an  $R_{adj}^2 = 0.382$ . This is followed by the externality beliefs and political views, which are essentially equally predictive at  $R_{adj}^2 = 0.297$  and  $R_{adj}^2 = 0.296$  respectively. Last is the “economist” regression, with a relatively low predictive power of  $R_{adj}^2 = 0.148$ .

The first three of these models all have relatively strong predictive power. Including two simple fairness, externality or political dummies leads to a 20-30 percentage point increase in explaining variation in redistributive preferences. This is a sizable increase. The fairness module is clearly strongest; the externality or political modules explain about two thirds of the variation the fairness module does. However, it is also clear that the externality module itself explains a sizable portion of variation.<sup>72</sup>

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 variables remain strong predictors of redistributive preferences except for the taxation reduces work-variable, which is no longer significant. The point estimates drop for all variables, indicating that while they are to some extent correlated with each other, each still captures *independent* correlation with redistributive preferences. This is further reflected in the increase of the adjusted  $R^2$  of nearly 10 percentage points compared to a model that only includes classical fairness views and sociodemographic control variables. 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. We further this type of analysis in the Appendix by exploring three-variable versions of the fairness and externality modules (without the other two modules); the findings there confirm that the externality variables are weaker predictors than the fairness variables but capture variation that is not explained by the fairness variables.

---

<sup>72</sup>Note that these results are replicated in Survey 2 using a slightly different set of explanatory variables externality beliefs (the belief whether unequal countries generally function worse was not elicited in Survey 2). See Table E30 in the Appendix for details.

This method on its own does not show that inequality externality beliefs are a determinant for preferences for redistribution. Consider the possibility that externality beliefs are not determinants in themselves but rather simply correlated to redistributive preferences (similar to, for example, being Republican). Intrinsic weaknesses like these are why we explore the topic through different methods, and in particular induce exogenous variation in our information experiment. Nonetheless, this method allows us to make two separate conclusions. First, externality beliefs are somewhat weaker predictors of redistributive preferences as compared to classical fairness views in our survey. Second, externality views are similarly important in predicting redistributive preferences as compared to political affiliation. Putting the two together, the horse-race regressions strengthen the conclusion from the experimental analysis; while fairness views are overall stronger determinants of redistributive preferences than externality beliefs, such beliefs remain an important determinant of redistributive preferences.

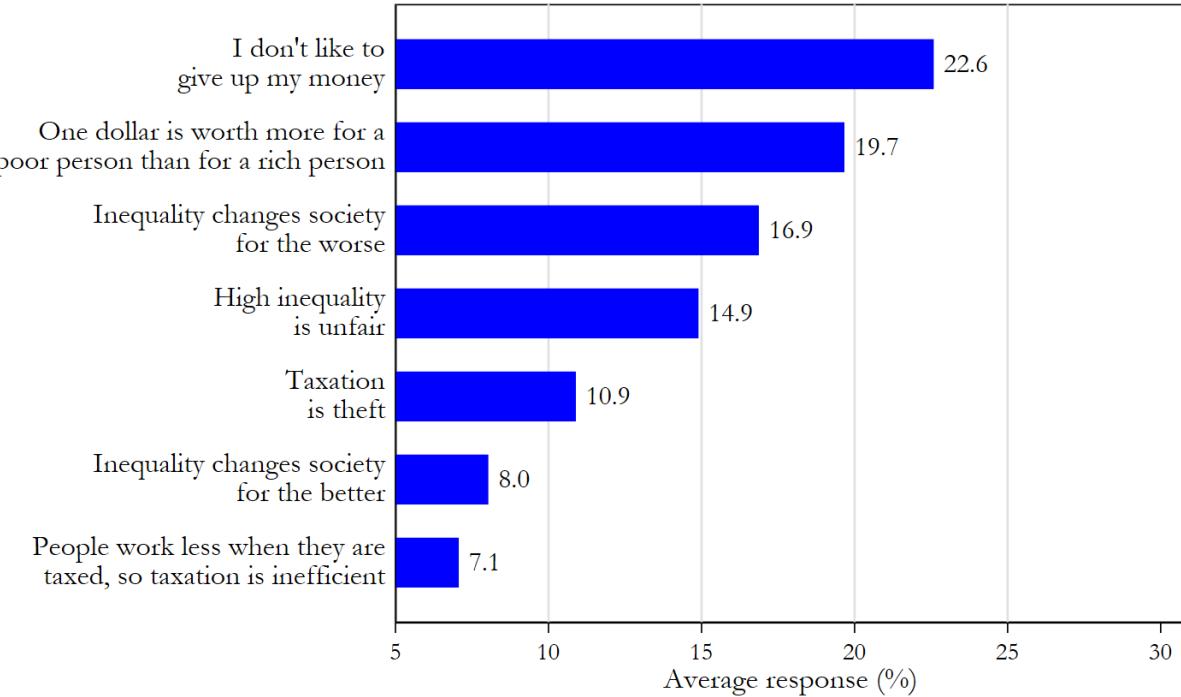
**Ranking motives behind preferences for redistribution** While the treatment effects characterizes the importance of each motive at the margin and the horse-race regression describe the predictive power of each motive, we now study the absolute relative importance of different motives behind redistributive preferences. To this end, we analyze the results from a survey-item conceptualized for this question. In this item subjects were asked to allocate 100 points across different motives behind preferences for redistribution. For example, a respondent who only cares about maximizing her own income should allocate 100 points to “I do not like to give up money”; a respondent who cares equally about inequality’s negative effects on society, the fairness of the post-tax income distribution, and maximizing her own income should allocate 33 points to each motive; etc. The survey-item is particularly useful to assess the relative importance of externality beliefs and fairness views in influencing preferences over redistribution, as it forces the respondents to trade these motives off against each other. Furthermore, it provides direct evidence of respondent’s motives behind their preferences over redistribution.

Figure 12 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 motive, attaining an average of 18 points. A broadly framed fairness motive (“High inequality is unfair”) actually ranks slightly behind the inequality motive and the mean weights are different from each other ( $p = 0.00$ , t-test). A general aversion against taxation, positive externality concerns, and efficiency concerns attain only weak average support from our sample.<sup>73</sup> This last point on efficiency concerns is consistent with the findings in Table 4 and in Stantcheva (2021), among others; efficiency concerns do not seem to be strong determinants of

---

<sup>73</sup>One 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 D11 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 12. One can also replicate Figure 12 while only including the sub-populations of subjects that rank a given motive first. This is presented in Figure D12. 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.

Figure 12: 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 ( $N=3,292$ ) discussed in Section 3.3. Results are very similar across surveys.

U.S. citizens' redistributive preferences.

What does this tell us about the relative importance of externality concerns and fairness views? First, one should note that inequality externality concerns rank as one of the most important motives within our control group. This is remarkable given that this is the first time most of these respondents are faced with the idea of inequality's consequences in our survey.<sup>74</sup> Second, negative externality concerns are similar in magnitude as broad but explicit fairness views. When adding up positive and negative externality concerns, externality beliefs become the strongest of all motives, even surpassing the selfish motive. If one adds-up the DMU and the fairness motives to one broad other-regarding motive, general externality concerns are about three-quarters (74%) as important as other-regarding motives as a redistributive determinant - thus echoing the results from the two methods described above.<sup>75</sup>

**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

<sup>74</sup>It is the first time the concept is mentioned in Survey 2 (about 2/3 of the respondents in Figure 12). In 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?*”). Results are very similar across samples.

<sup>75</sup>If including “Taxation is theft” as a fairness motive, this falls to 60%.

preference index goes through either the two main externality variables or the two main fairness variables (seen in Table 4), governmental trust, or efficiency concerns. In total, 46% of Republicans' lower support for redistribution cannot be explained by these variables or a list of standard controls. Of the remaining 54%, most can be accounted for mainly by fairness views (27% of the partisan gap). Then comes externality beliefs (12%), individual controls (10%), and governmental trust (5%). Efficiency concerns are not a relevant factor ( $\sim 0\%$ ). Overall, this indicates that the partisan split in redistributive preferences is driven by fairness views more so than externality beliefs – which is consistent with the stronger partisan split across fairness views than externality beliefs shown in Figure 3.

Two consistent patterns emerge from this analysis: (1) Fairness concerns are generally more powerful determinants of individuals' preferences for redistribution. (2) Inequality externality beliefs are still strong predictors of redistributive preferences, and seem to be between half and two-thirds as important as fairness views, broadly speaking.

## 6 Discussion

**Preferences for redistribution and polarization** There are structural differences between redistributive arguments based on either (a) fairness concerns or (b) inequality externality concerns. In this section we discuss some implications of these differences. The two most crucial differences 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 polarized; 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.

Inequality externality concerns are fundamentally different. First, the “villain” is 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 thus usually inequality itself, and the victim is often all of us.

As an example of this difference; arguing that the economic system is unfair can be perceived to discredit those with high incomes. Arguing that higher-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. This is most clearly seen in the likelihood of respondents reporting *anger* after watching the fairness treatment. Some relatively simple information about the evolution of wages and productivity made one out of every eight people report this emotion – significantly higher than for any of our other treatments, even those discussing homicides.<sup>76</sup> This indicates the high potential for 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.<sup>77</sup>

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. 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.

This discussion implies a salient trade-off between maximum efficacy (fairness arguments) and low polarization (externality arguments). Politicians and policy leaders who wish to create more demand for redistribution, yet want to keep polarization low, could do well by using externality arguments instead of fairness arguments. This may come at the cost of lower efficacy, however. We believe this trade-off is a notable finding in an increasingly polarized world.

**Welfare-theoretical ramifications** Welfare theory is often based on individualist utility functions, which usually assumes no relevant externalities. Even if some externalities do exist, they can usually be ignored either because they are not of a macroeconomic scale or not explicitly economic in nature (and potentially based on welfare-irrelevant concepts 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, largely independent of how they were procured, which exists on a macroeconomic scale. It also does not rely on feelings or other arguments that can be disregarded on philosophical grounds.<sup>78</sup> Beyond rare exceptions (Thurow, 1971; Alesina and Giuliano, 2011; Støstad and Cowell, 2020) this has been largely ignored in economic theory, likely because causal evidence of inequality's societal effects is empirically difficult to produce.<sup>79</sup>

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.

---

<sup>76</sup>The equivalent numbers for the three externality treatments is one in 13, 16 and 34 respondents.

<sup>77</sup>This is in line with theoretical analysis and correlational evidence by Rueda and Stegmüller (2016) who study the relationship between preferences for redistribution and (fear of) crime.

<sup>78</sup>The main other externality that meet all these requirements is global climate change.

<sup>79</sup>These difficulties are intrinsic, such as the lack of exogenous variation in inequality; the difficulties would remain even if externality effects were large.

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 which we hope to explore in a future study. 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 (66%), decreases economic growth (52%), for example. In collecting these and other data points, this paper has thus created the first extensive dataset 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 between a half to two-thirds as important as fairness views in determining redistributive preferences. As such, this paper presents the first strong evidence that individuals' beliefs about the *consequences of inequality* is impactful for their redistributive preferences.

The work further discussed the main differences between fairness arguments and inequality externality arguments. The three main distinguishing differences were argued to be the overall efficacy of the argument first, the potential polarization of the argument second, and the target audience of these arguments third. Generally speaking, fairness arguments are somewhat more effective than externality arguments. However, 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. We empirically demonstrate this argument through various survey results; the fairness treatment leads to more anger in respondents and was more effective on lower income individuals, for example. In addition, fairness views are more heavily correlated to income and party affiliation than externality beliefs. 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 has externalities, 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 further work will explore

these issues more thoroughly.

## References

- Alesina, Alberto and Paola Giuliano. Chapter 4 - Preferences for Redistribution. In Benhabib, Jess, Alberto Bisin, and Matthew O. Jackson, editors, *Handbook of Social Economics*, volume 1, pages 93–131. North-Holland, 2011.
- Alesina, Alberto, Stefanie Stantcheva, and Edoardo Teso. Intergenerational Mobility and Preferences for Redistribution. *American Economic Review*, 108(2):521–554, 2018.
- Almås, Ingvild, Alexander W. Cappelen, and Bertil Tungodden. Cutthroat Capitalism versus Cuddly Socialism: Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians? *Journal of Political Economy*, 128(5):1753–1788, 2020.
- Anderson, Michael L. Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and early training projects. *Journal of the American Statistical Association*, 103(484):1481–1495, 2008.
- Andre, Peter, Carlo Pizzinelli, Christopher Roth, and Johannes Wohlfart. Subjective Models of the Macroeconomy: Evidence From Experts and Representative Samples. *The Review of Economic Studies*, 2022.
- Aronsson, Thomas and Olof Johansson-Stenman. Inequality Aversion and Marginal Income Taxation. In *Proceedings. Annual Conference on Taxation and Minutes of the Annual Meeting of the National Tax Association*, volume 111, pages 1–32. JSTOR, 2018.
- Aronsson, Thomas and Olof Johansson-Stenman. Optimal Second-Best Taxation When Individuals Have Social Preferences. *Umeå Economic Studies*, (973), 2020.
- Auclert, Adrien and Matthew Rognlie. Inequality and Aggregate Demand. Working Paper 24280, National Bureau of Economic Research, 2018.
- Becker, Gary S. Crime and Punishment: An Economic Approach. In *The economic dimensions of crime*, pages 13–68. Springer, 1968.
- Bellani, Luna, Nona Bledow, Marius R Busemeyer, and Guido Schwerdt. When everyone thinks they're middle-class:(mis-) perceptions of inequality and why they matter for social policy. 2021.
- Bénabou, Roland. Inequality and Growth. *NBER Macroeconomics Annual*, 11:11–74, 1996.
- Bergh, Andreas, Therese Nilsson, and Daniel Waldenström. *Sick of Inequality?: An Introduction to the Relationship between Inequality and Health*. Edward Elgar Publishing, 2016.
- Breza, Emily, Supreet Kaur, and Yogita Shamdasani. The morale effects of pay inequality. *The Quarterly Journal of Economics*, 133(2):611–663, 2018.
- Cappelen, Alexander W, Astri Drange Hole, Erik Ø Sørensen, and Bertil Tungodden. The pluralism of fairness ideals: An experimental approach. *The American Economic Review*, 97(3):818–827, 2007.
- Card, David, Alexandre Mas, Enrico Moretti, and Emmanuel Saez. Inequality at work: The effect of peer salaries on job satisfaction. *The American Economic Review*, 102(6):2981–3003, 2012.
- Carlsson, Fredrik, Dinky Daruvala, and Olof Johansson-Stenman. Are People Inequality-Averse, or Just Risk-Averse? *Economica*, 72(287):375–396, 2005.
- Cruces, Guillermo, Ricardo Perez-Truglia, and Martin Tetaz. Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. *Journal of Public Economics*, 98:100–112, 2013.
- Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. Inequality and Violent Crime. *The Journal of Law and Economics*, 45(1):1–39, 2002.
- Fehr, Dietmar, Hannes Rau, Stefan T. Trautmann, and Yilong Xu. Inequality, fairness and social capital. *European Economic Review*, 129:103566, 2020.
- Gallego, Aina. Inequality and the erosion of trust among the poor: Experimental evidence. *Socio-Economic Review*, page mww010, 2016.
- Gärtner, Manja, Johanna Möllerström, and David Seim. Income Mobility, Luck/Effort Beliefs, and the Demand for Redistribution: Perceptions and Reality. *Working Paper*, 2019.

- Gelbach, Jonah B. When do Covariates Matter? And Which Ones, and How Much? *Journal of Labor Economics*, 34(2):509–543, 2016.
- Haaland, Ingar and Christopher Roth. Beliefs about Racial Discrimination and Support for Pro-Black Policies. *The Review of Economics and Statistics*, pages 1–38, 2021.
- Haaland, Ingar, Christopher Roth, and Johannes Wohlfart. Designing Information Provision Experiments. *Working Paper*, 2020.
- Karadja, Mounir, Johanna Mollerstrom, and David Seim. Richer (and Holier) Than Thou? The Effect of Relative Income Improvements on Demand for Redistribution. *The Review of Economics and Statistics*, 99(2):201–212, 2017.
- Kelly, Morgan. Inequality and crime. *Review of Economics and Statistics*, 82(4):530–539, 2000.
- Kuziemko, Ilyana, Michael I Norton, Emmanuel Saez, and Stefanie Stantcheva. How elastic are preferences for redistribution? Evidence from randomized survey experiments. *The American Economic Review*, 105(4):1478–1508, 2015.
- Rosenbaum, Paul R. *Multiple Control Groups*, pages 253–275. Springer New York, New York, NY, 2002.
- Rueda, David and Daniel Stegmüller. The Externalities of Inequality: Fear of Crime and Preferences for Redistribution in Western Europe. *American Journal of Political Science*, 60(2):472–489, 2016.
- Rufrancos, Hector, Madeleine Power, Kate E Pickett, and Richard Wilkinson. Income Inequality and Crime: A Review and Explanation of the Time Series Evidence. *Sociology and Criminology-Open Access*, 2013.
- Saez, Emmanuel. Using Elasticities to Derive Optimal Income Tax Rates. *The review of economic studies*, 68(1):205–229, 2001.
- Stantcheva, Stefanie. Understanding tax policy: How do people reason? *The Quarterly Journal of Economics*, 136(4):2309–2369, 2021.
- Støstad, Morten Nyborg. *Inequality as an Externality: Existence and Consequences*. PhD thesis, Master’s thesis at Paris School of Economics, 2019.
- Støstad, Morten Nyborg and Frank Cowell. Inequality as an Externality: Consequences for Tax Design. *Working Paper*, 2020.
- Thurow, Lester C. The income distribution as a pure public good. *The Quarterly Journal of Economics*, pages 327–336, 1971.
- Valero, Vanessa. Redistribution and beliefs about the source of income inequality. *Experimental Economics*, pages 1–26, 2021.
- Wilkinson, Richard and Kate Pickett. *The Spirit Level: Why Greater Equality Makes Societies Stronger*. Bloomsbury Publishing USA, 2011.

# Appendix

## A Inequality as an Externality: Theory

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.

However, it is also worth noting that the existence of inequality externalities does not generally have to rely on such complex causal channels. To establish the second point from the main text – that inequality externalities can be relatively simple to micro-found and can be mechanical in nature – we refer to Støstad and Cowell (2020). Several of the inequality externalities we explore in this work, including crime, trust, political polarization, innovation, and economic growth, are microfounded there. It is also argued that certain externalities are *mechanical* in nature, in other words that a population of purely self-interested rational agents could experience them. The economic theory of crime is one such example, where self-interested agents rationally react to higher economic inequality. There are also many more. A more unequal society would have a more difficult time funding public goods if public good preferences are diverging in income, for example. In other words, individuals do not have to have other-regarding preferences of any kind for inequality externalities to exist.

## B Survey Details

### B.1 Survey 1

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

#### B.1.1 Methodology and data quality

To ensure sufficient data quality we pre-specified the following measures. First, we drop 5% of the fastest respondents as a rule of thumb, as is often done in the literature and by survey companies (see e.g. Bellani et al., 2021).<sup>80</sup> Second, we exclude subjects that spend less time on the screen with the video treatment than the duration of the video, as well as those that indicate to not have watched the video. Third, our survey contains several attention checks. Subjects can only start the survey after passing the first attention check (immediately after the consent page) and are required to pass two of the next three attention checks. These attention checks are very simple and designed to sieve out individuals that do not read the questions at all.<sup>81</sup> We also added two extra data quality checks that were not pre-specified; we deleted respondents that dropped out of the survey in the middle and then retook the survey (who we identify due to identical IP-addresses) and we drop 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).<sup>82</sup> Overall, this leaves a final sample of 4,371 respondents. The average survey duration for these respondents was 19 minutes and 11 seconds.

### B.2 Survey 2

The full survey questionnaire will be included shortly. In Appendix B.6.1 below we show the general externality question.

#### B.2.1 Methodology and data quality

The sampling methodology in Survey 2 is similar to that of Survey 1. We dropped the 5% fastest respondents per survey arm and use a similar attention check procedure to ensure high-quality responses. Respondents who fail either the first attention check 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.<sup>83</sup> A total of 2,478 respondents passed our attention checks and finished the survey; after dropping the fastest 5% of respondents per survey arm the final sample is 2,360 respondents. The average survey duration for these respondents was 20 minutes and 31 seconds.

---

<sup>80</sup>Since different treatment groups watch different videos, we drop the 5% fastest subjects within each treatment group.

<sup>81</sup>Our conclusions do not change when including respondents who failed attention checks.

<sup>82</sup>This was done to optimize data quality. All results are robust to including these responses.

<sup>83</sup>Our conclusions do not change when including respondents who failed attention checks.

### B.3 Further Survey 2 details

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.

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 ensured that respondents understand the externality question, explicitly set a reference level of inequality for the hypothetical society wherein inequality increases, ask respondents to confirm their choices, and so on.

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 an open-ended text question, a question on whether top- or bottom-based inequalities matter more for the externality, several questions designed to find potential non-monotonicities in externality beliefs, and a question which explores whether average income or income 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.

More details will be included soon.

### B.4 Representativity

Table B1 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.<sup>84</sup> All three final distributions mirror the November 2021 Gallup poll quite closely (31% Republican, 27% Democrat, 41% Independent), each marginally undersampling Independents.<sup>85</sup> Figures B1-B2 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*.<sup>86</sup>

---

<sup>84</sup>“In politics, as of today, do you consider yourself a Republican, a Democrat or an independent?”

<sup>85</sup>This 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.

<sup>86</sup>These 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 B1: 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	

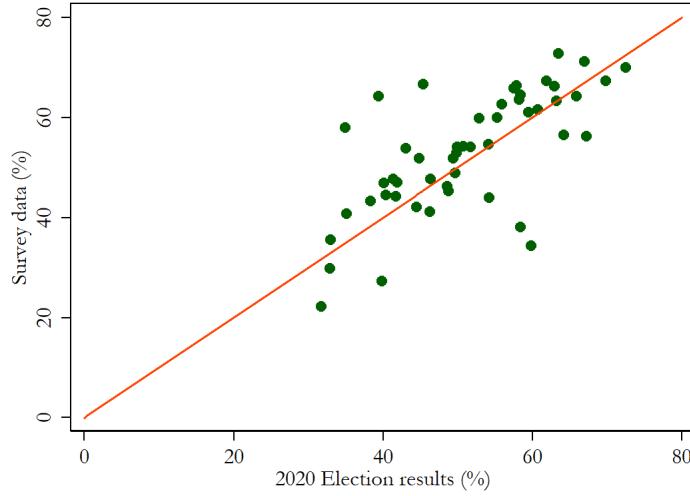
**Note.** This table represents respondent demographics of Survey 1 ( $N= 4,371$ ), Survey 2 ( $N=2,360$ ), and the merged descriptive sample (the control group of Survey 1 and all of Survey 2) compared to the share among 2021 U.S. adults for the respective characteristic. Data on the U.S. population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

## B.5 Eliciting externality beliefs

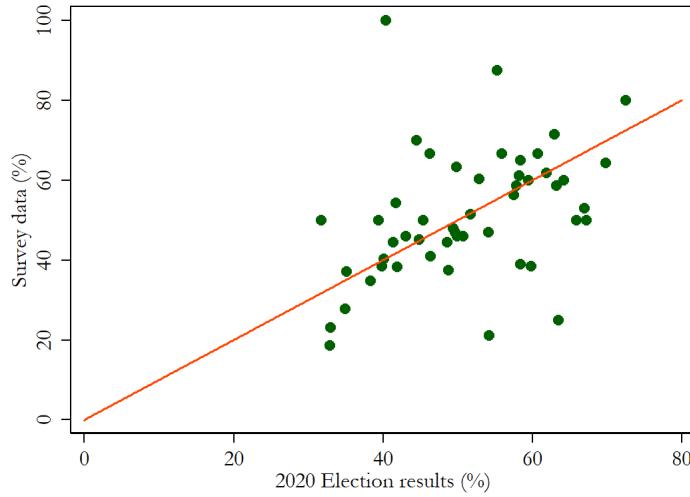
We elicit externality beliefs using various methods. Questions on individuals' *general* and *specific* inequality externality beliefs are asked in both Survey 1 and Survey 2. These beliefs are explored

Figure B1: 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 B2: 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”.<sup>87</sup> 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.<sup>88</sup> Survey 2 also ensured that respondents answer accurately by asking them to confirm their previous choice; almost all respondents (> 97%) confirm their choice.<sup>89</sup> We discuss these and other robustness checks in Section 4.2.1 and Appendix C.1.

## B.6 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”. 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 B.6.1 for the question in verbatim).<sup>90</sup>

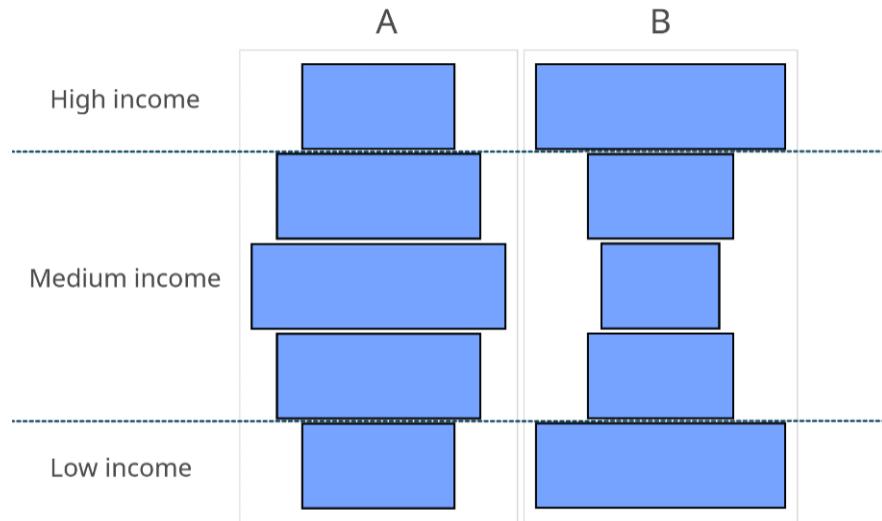
---

<sup>87</sup>Many of our questions ask how “more inequality” changes society; these respondents are instead asked how “more equality” changes society.

<sup>88</sup>The share of individuals who believe in a given negative externality is often somewhat stronger (5 – 10%) among those who answer this quiz correctly.

<sup>89</sup>We 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.

<sup>90</sup>Specifically (a) the initial inequality level, shown through diagrams and words to be roughly at a Scandinavian level (without explicitly naming countries), (b) the level of the change in inequality, which is an inequality increase to roughly the level of the United States, (c) the exogenous nature of the inequality shock, (d) explicitly noting that we are interested in changes in factors of society, using examples such as crime and economic growth, (e)



### B.6.1 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 equal<sup>91</sup>/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.

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]**.

---

explicitly noting that the question is not about individual income or fairness concerns.

<sup>91</sup>Order of the distributions is switched for the equality-phrasing

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 Further Results

### C.1 Phrasing results

We change the word “equality” 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. Below we further detail these changes and their effects on results.

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 → More crime*”. The same belief under the “equality” phrasing would be “*More equality → 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 D3-D4 and detail the results below.

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%).<sup>92</sup> 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

---

<sup>92</sup>Believing in the negative externality in this case implies that respondents answer that more equality leads to less political polarization or less corruption.

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.

## C.2 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 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 D13. 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 D13 with caution.

## C.3 Dual control groups

In this section, we compare the respondents’ characteristics and outcomes across the two control groups. We pre-specified to merge these two 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 C1: Dual control: Balance table for redistributive preferences

Variable	(1) Passive Control	(2) Active Control	(3) 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 C1 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.<sup>93</sup> 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 E3). 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 E4, there are no significant differences between the two groups on any pre-treatment dimension. Table E5 shows that they are also comparable on various sociodemographic characteristics.<sup>94</sup>

---

<sup>93</sup>This 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.

<sup>94</sup>We 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.

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 are testing many hypotheses at once.<sup>95</sup> Following our pre-analysis plan, we thus merge the two groups.

#### C.4 Balance across control and treatment groups

This section checks the pre-treatment balance of control and treatment groups. As shown in Table E6, 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 E7 compares observable characteristics across the trust and control groups. The two groups are completely balanced on observables.

Table E8 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.<sup>96</sup>

Table E9 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).

#### C.5 Treatment effect mechanism

This section describes the results in Table E23, which includes post-treatment first stage outcomes in a regression on treatment variables. Compared to the treatment effect of a regression without post-treatment beliefs, the coefficients of the treatment dummies decreases for the treatments that significantly affected redistributive preferences, i.e. the full externality treatment and the fairness treatment.

More concretely, the treatment effect of the full externality treatment on our redistributive preference index was 10 percent of a standard deviation if we do not control for post-treatment externality beliefs and decreases to 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 12.2 percent of a standard deviation ( $p=0.000$ , t-test). Similar reductions in the treatment effect can be observed

---

<sup>95</sup>The potential exception to this being the top tax result.

<sup>96</sup>We 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.

for the other redistributive preference. This provides evidence that our reduced form treatment effect is mediated through a shift in beliefs, as intended by the treatment itself.<sup>97</sup>

## C.6 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 3.4. 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 region. Regressions in Table E20 regress redistributive preferences on our treatments; Regressions in Table E22 regress posterior beliefs on treatments; and Regressions in Table E31 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 E32, E33 and E34, 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 E36 and E37 respectively), the effect of the full externality treatment on RP-Index becomes marginally significant as shown in Table E35. Given that controlling for passing or failing an attention check does not result in any differences, as shown in Table E38, 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 C.3 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 E39, E40, E41, and E42. 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.

---

<sup>97</sup>A 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 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 → A lot more crime"). However, her beliefs have changed, which then affect her redistributive preferences.

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.<sup>98</sup>

**Not controlling for observable characteristics** We replicate our main regressions without controlling for any observable characteristics. As shown in Tables E43 and E44 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.

**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 E45 and E46 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.

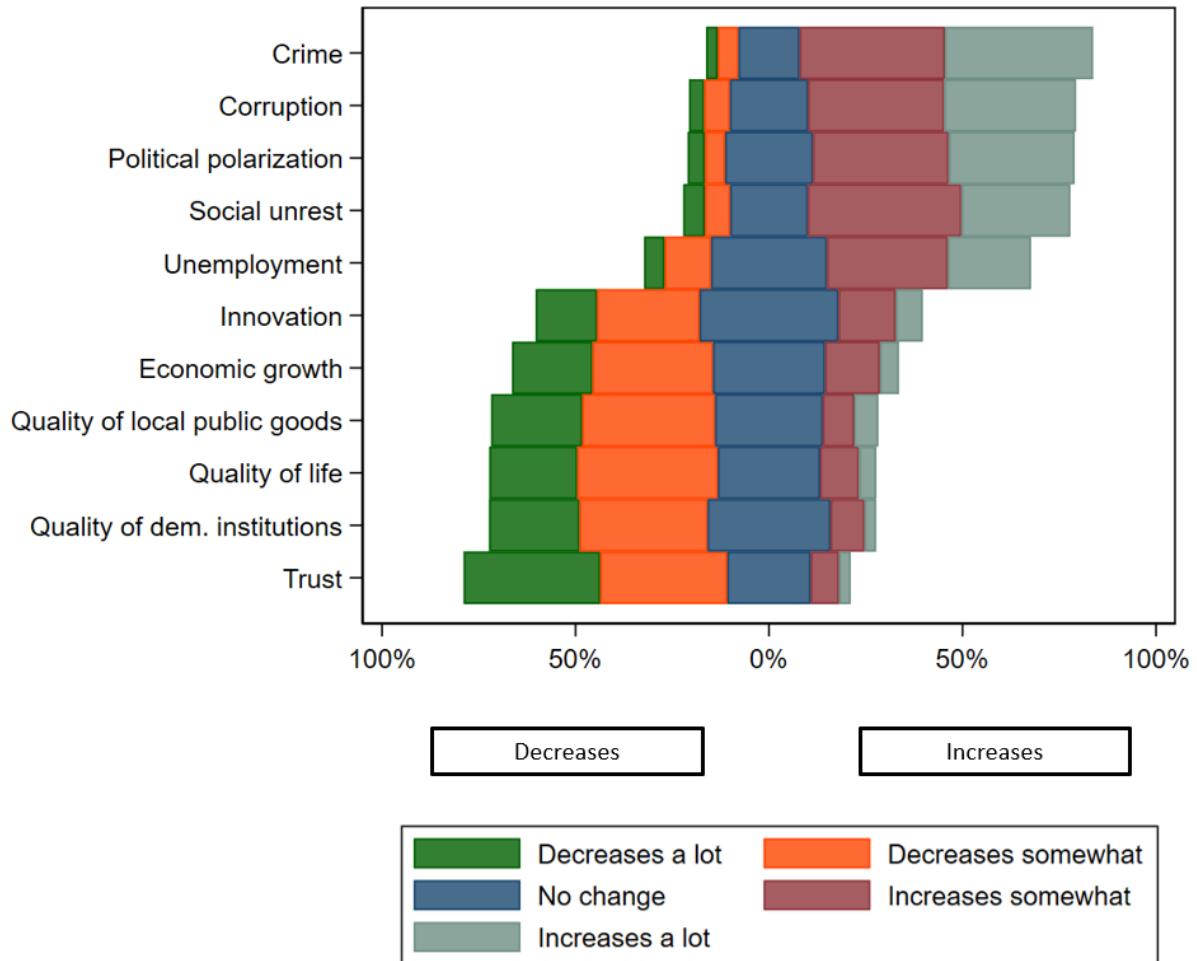
**Multiple hypothesis testing** In the main regression tables (Table 3) we run a total of twenty tests for statistical significance. On this scale, Type I errors can become a serious problem and lead to erroneous inference of statistical significance. To correct for this we use the false discovery rate (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 E49.

---

<sup>98</sup>We 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.

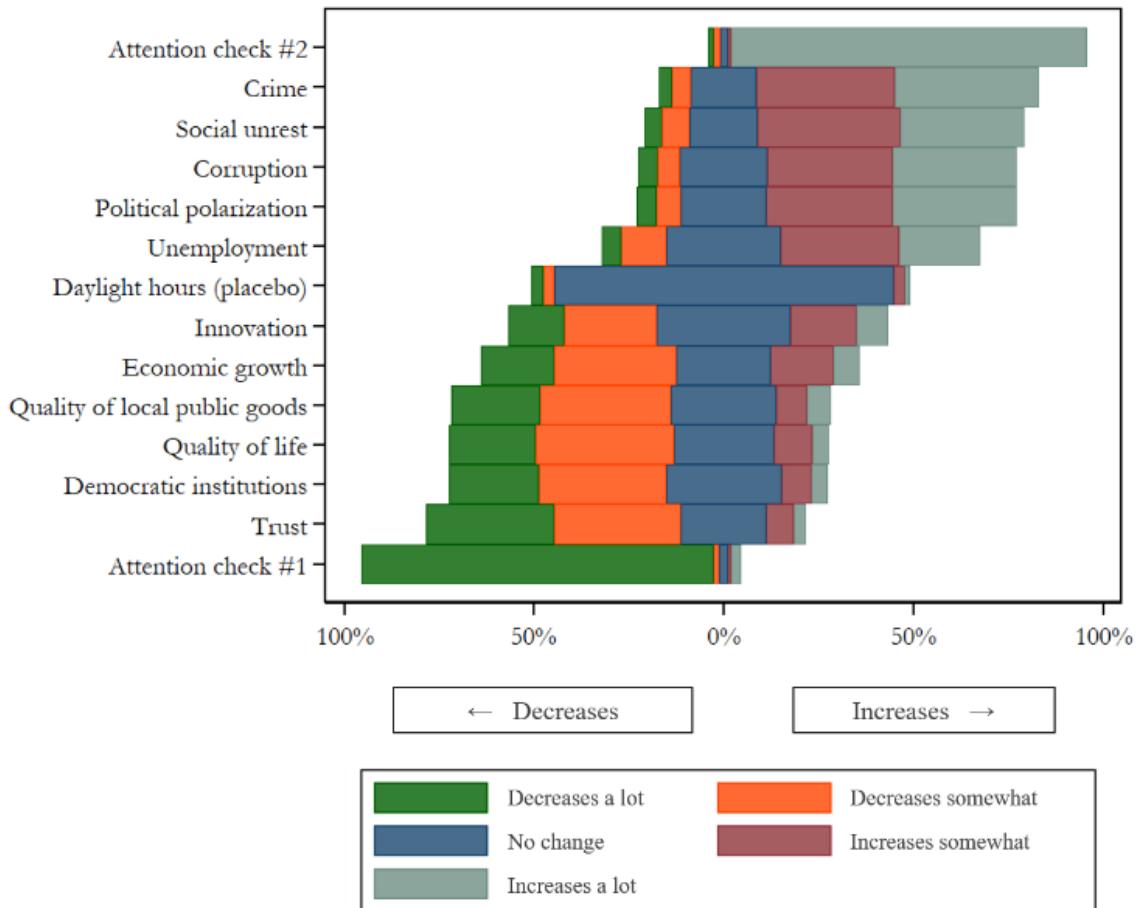
## D Figures

Figure D1: Distribution of Externality Beliefs in Survey 1 Control Group



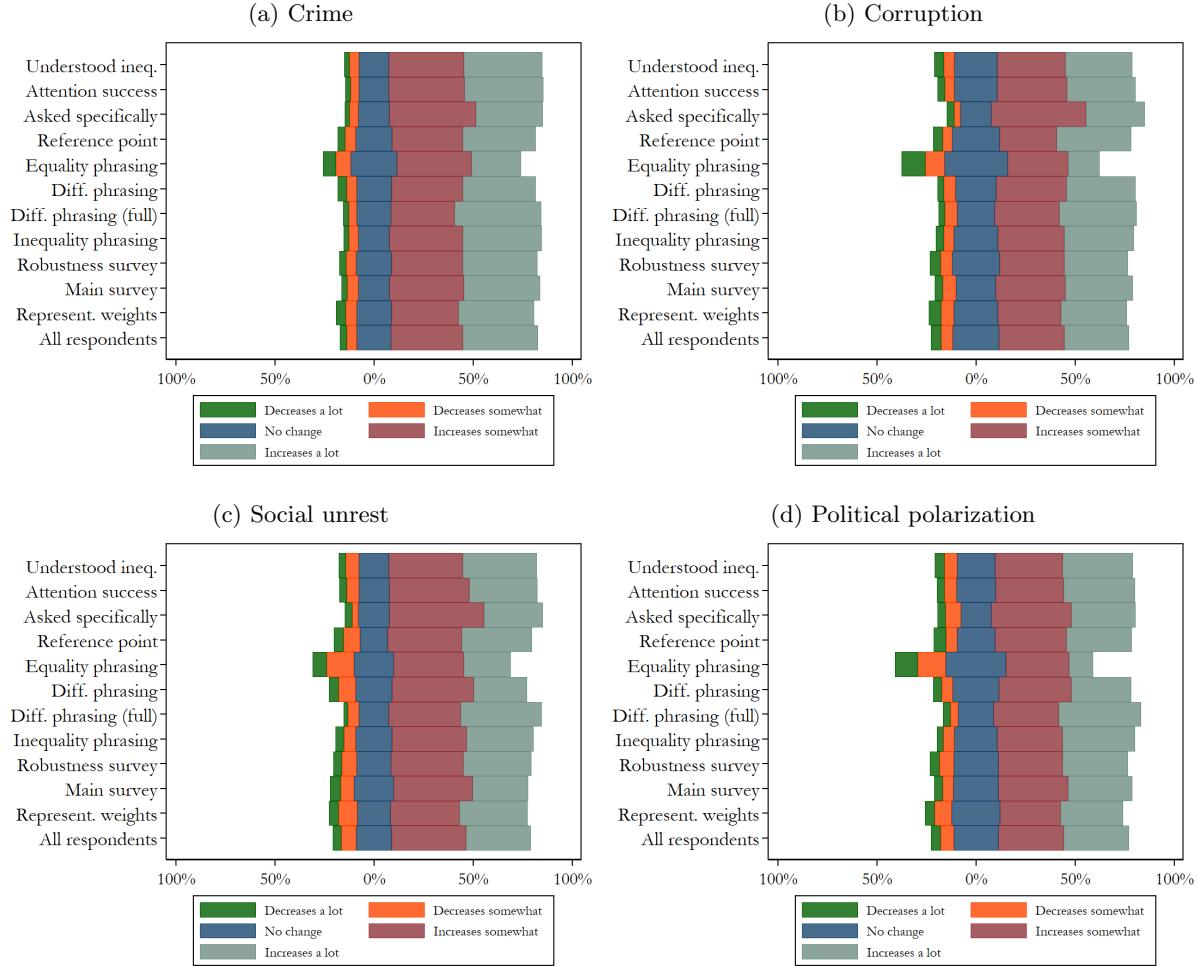
**Note.** Specific externality beliefs for control group in Survey 1. Questions are ordered according to which portion of respondents believe that inequality increases the variable. Full question example: “How does more economic inequality change the amount of crime in a country?” Answer option example: “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 1 and D2.

Figure D2: Distribution of Externality Beliefs in Survey 2: How Does More Inequality Change Various Outcomes?



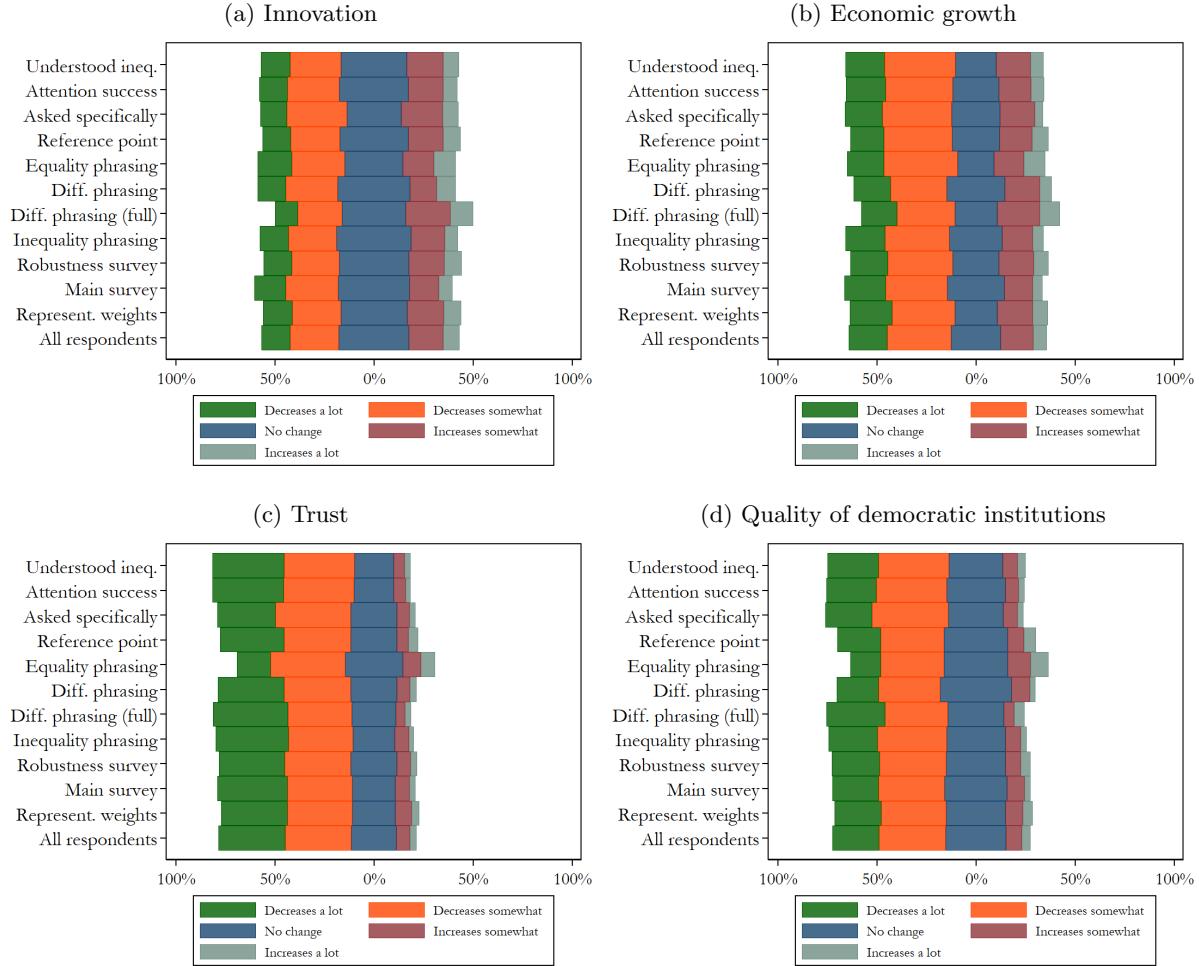
**Note.** Questions are ordered according to the net share of respondents who believe that inequality increases minus decreases the variable. Full question example: “How does more economic inequality change the amount of crime in a country?” Answer option example: “More inequality → a lot more crime”. 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). 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 1 and D1 respectively.

Figure D3: Robustness of externality beliefs I



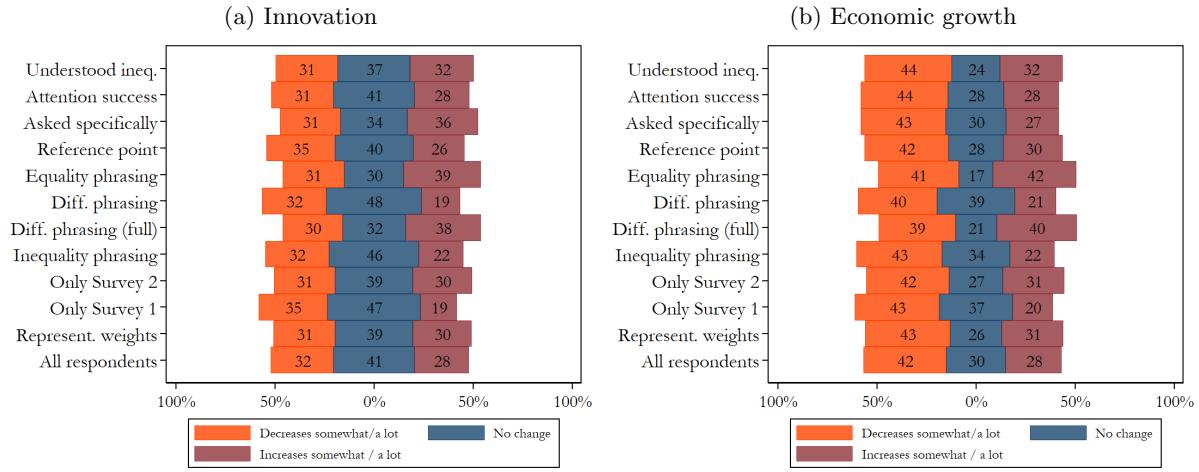
**Note.** Answers to *How does more economic inequality change the amount of [outcome] in a country?*, indicating inequality externality beliefs, across various specifications. The different specifications are, from bottom to top; (i) All respondents and all phrasings in both surveys,  $N \in \{2990, 3292\}$ , (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 D4: Robustness of externality beliefs II



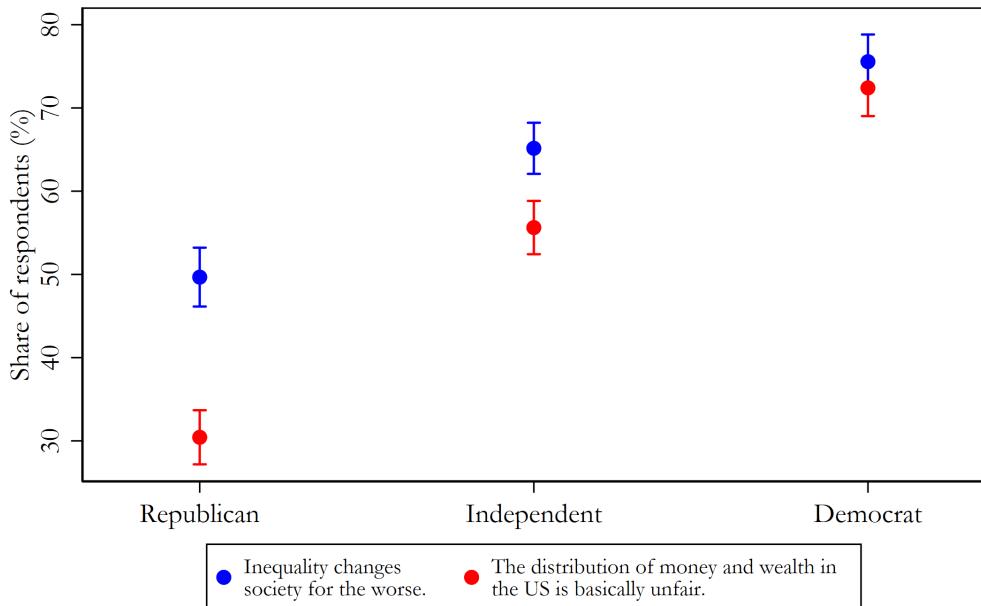
**Note.** Answers to *How does more economic inequality change the amount of [outcome] in a country?*, indicating inequality externality beliefs, across various specifications. The different specifications are, from bottom to top; (i) All respondents and all phrasings in both surveys,  $N \in \{2990, 3292\}$ , (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 D5: Robustness of innovation and economic growth result for Republican respondents



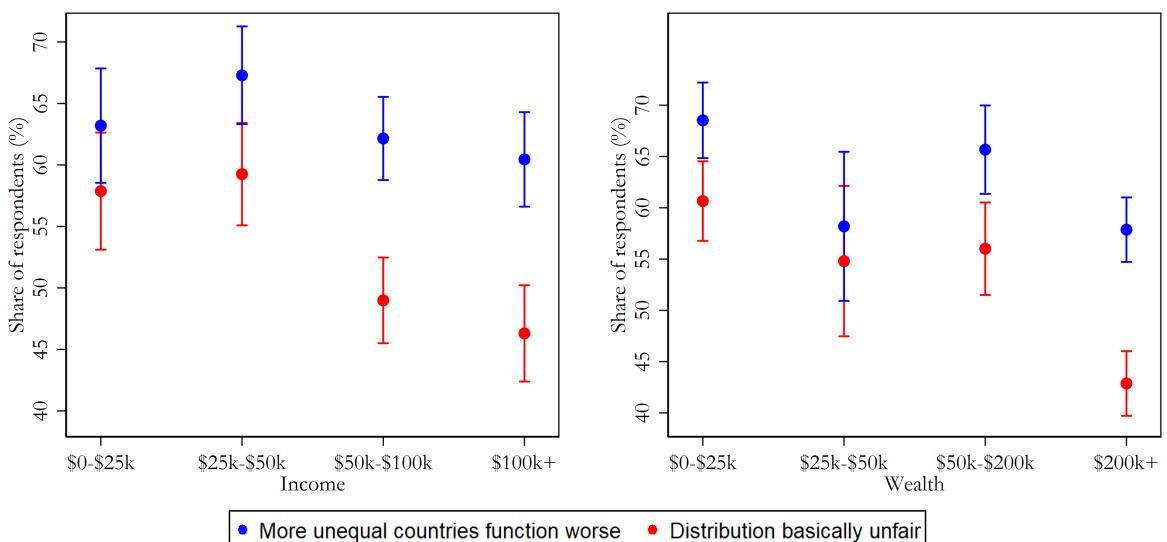
**Note.** Answers to *How does more economic inequality change the amount of [outcome] in a country?* for innovation and economic growth among Republicans across various specifications. The result that negative externality beliefs are more commonly held than positive externality beliefs among Republicans 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 D3-D4. Treatment groups from Survey 1 are always excluded.

Figure D6: Externality Beliefs and Fairness Views over Party (Survey 2)



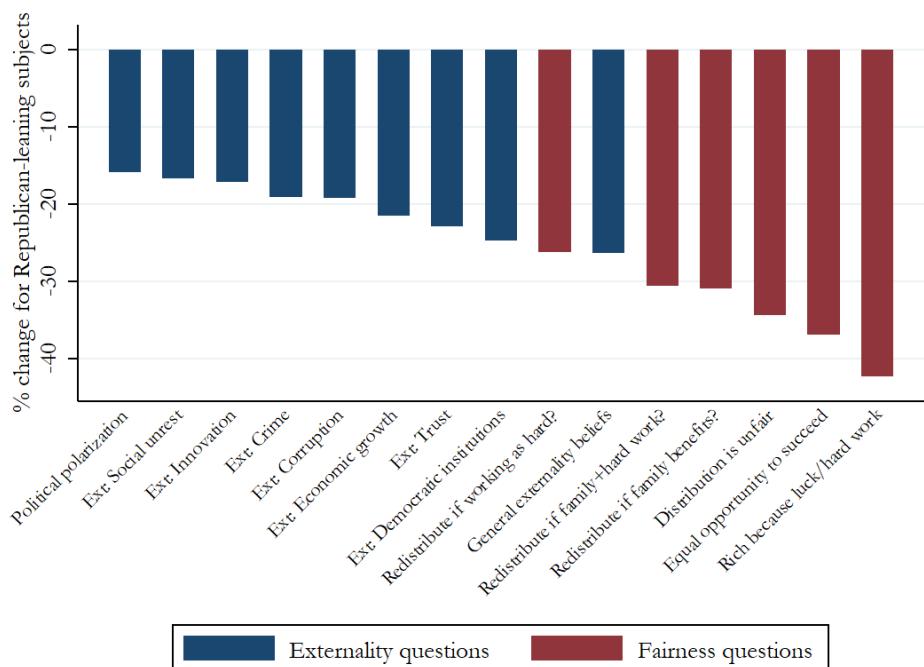
**Note.** This graph uses the pre-treatment externality and fairness question with Survey 2 respondents ( $n=2,360$ ). Respondents are asked to agree or disagree with the following two statements: “*The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity 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 3

Figure D7: Externality Beliefs and Fairness Views over Income and Wealth (Survey 2)



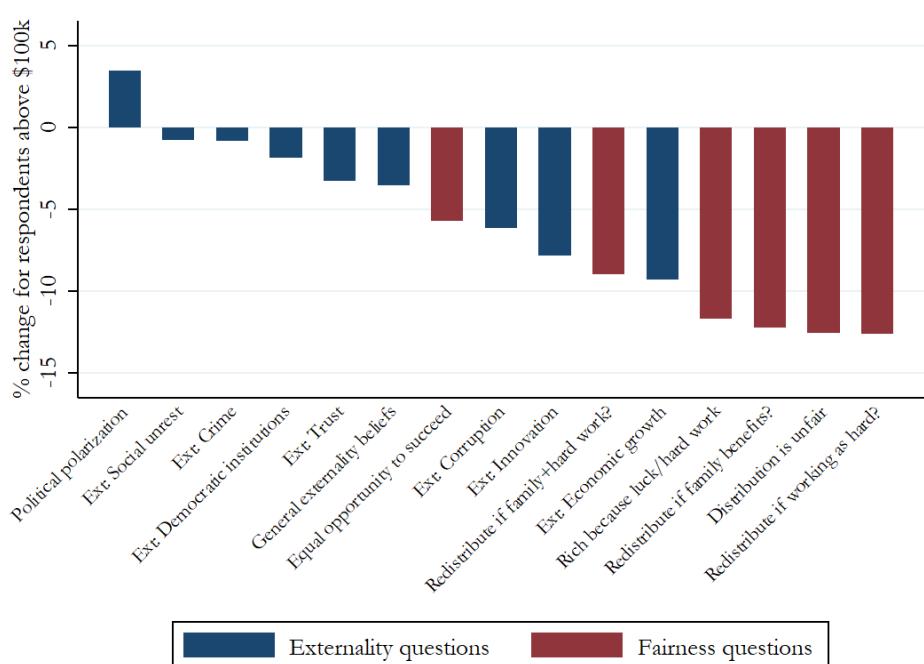
**Note.** This graph uses the pre-treatment externality and fairness question with Survey 2 respondents ( $n=2,360$ ). Respondents are asked to agree or disagree with the following two statements: “*The distribution of money and wealth in the US is basically fair, because everybody has an equal opportunity 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 5.

Figure D8: Party affiliation polarization across questions (Survey 2)



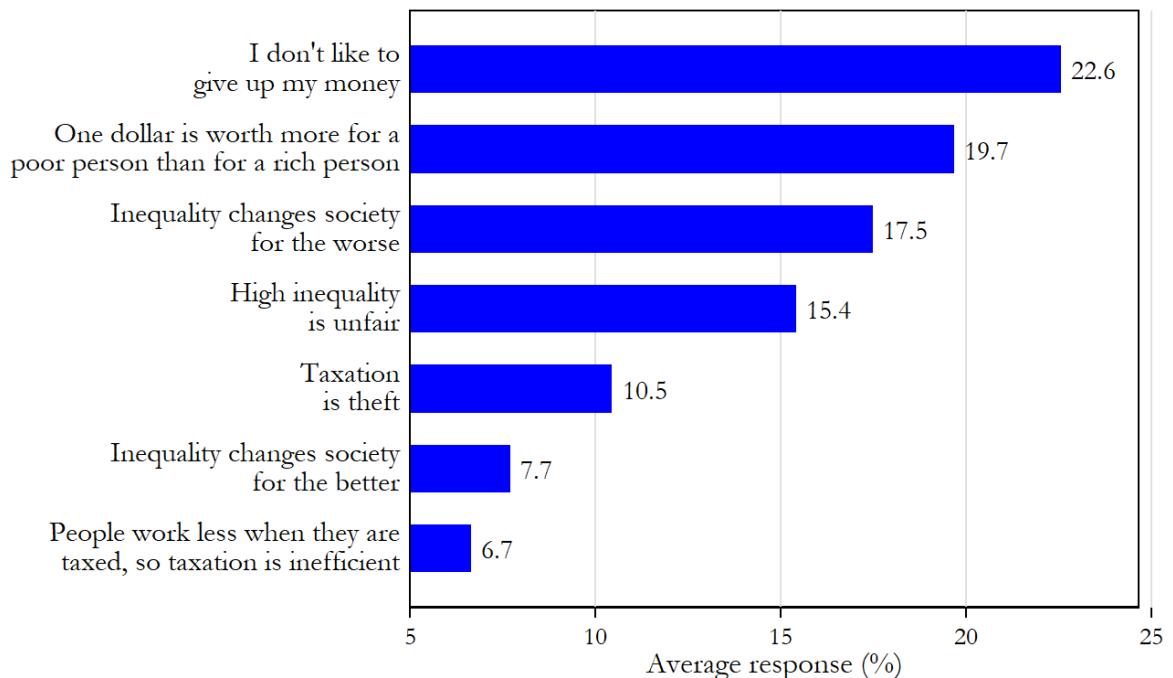
**Note.** Drop in anti-inequality sentiment for Republican-leaning respondents across every externality and fairness question in Survey 2 without any controls. With a standard set of controls the same relation holds (all fairness questions have larger polarization than any externality question). Questions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

Figure D9: Income polarization across questions (Survey 2)



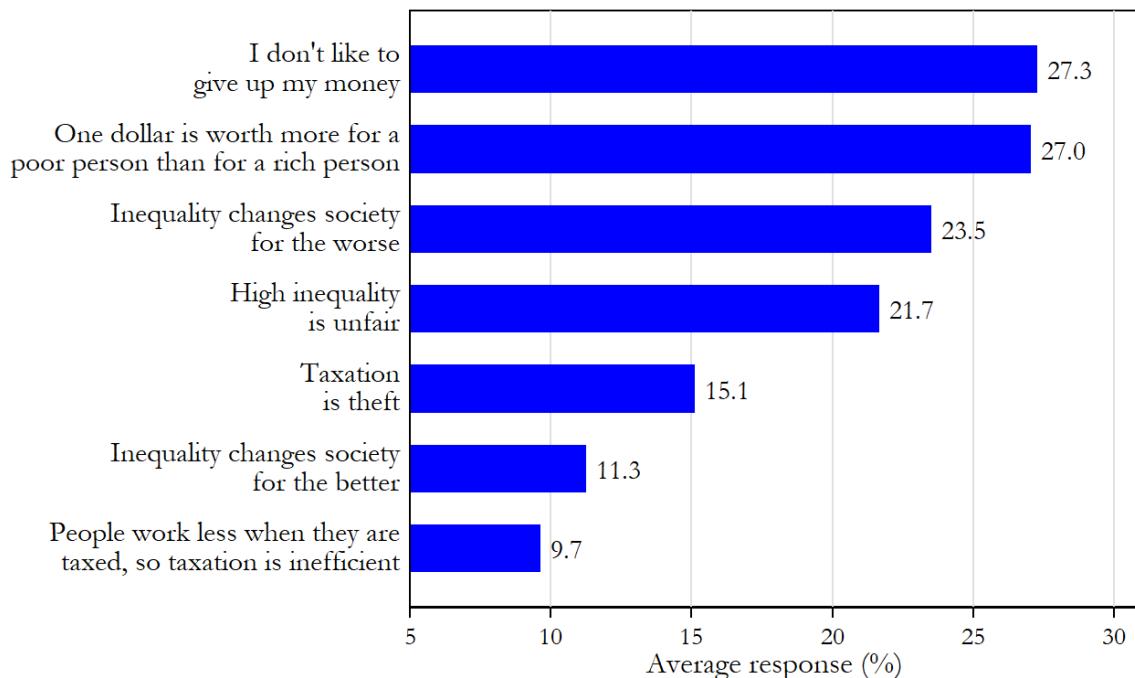
**Note.** Drop in anti-inequality sentiment for respondents with incomes above \$100,000 across every externality and fairness question in Survey 2 without any controls. With a standard set of controls there is more variation, although the average slope of the fairness questions stay significantly lower 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 D10: Mean share for each motive behind preferences for redistribution in Survey 2



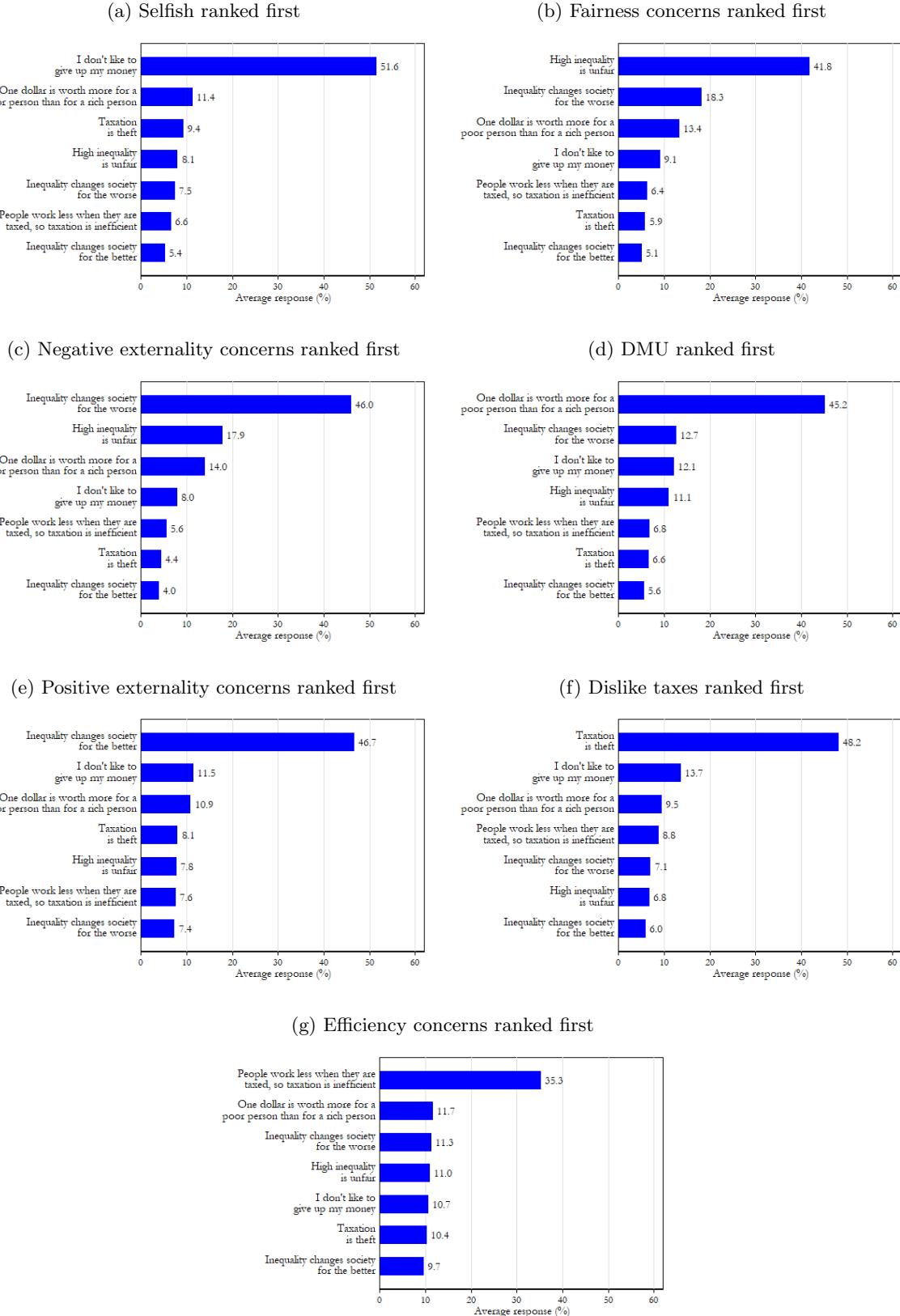
Question text: *When thinking about your preferred level of redistribution, what matters most to you? Please indicate what dimensions matter by giving scores below that add up to 100.* Answer option texts are identical to graph labels. Standard errors are approximately 0.5%.

Figure D11: Share of subjects that rank a given motive first



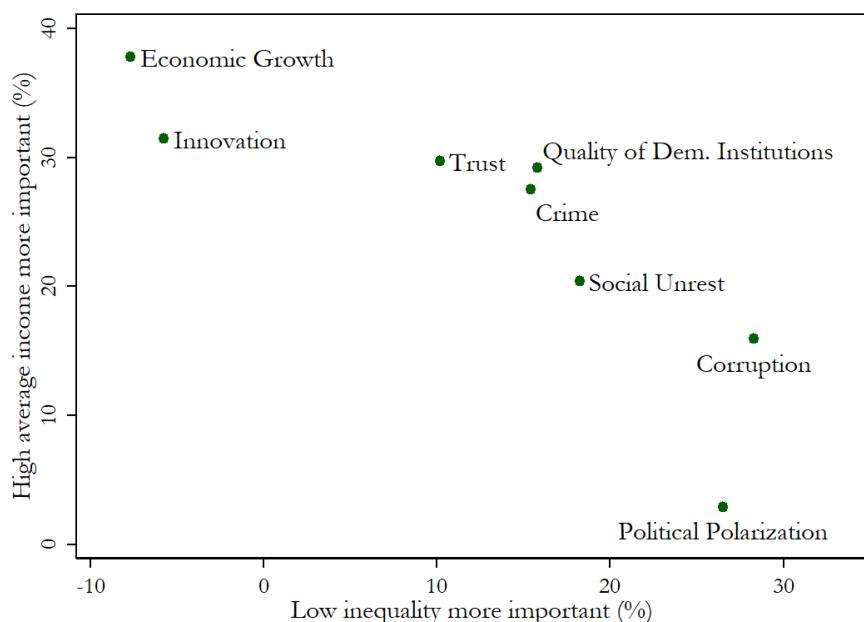
**Note.** Share of respondents who ranked the given motive weakly first. When a respondent ranked several motives equally, all are counted (which means the total percentage is above 100%). Question text: *When thinking about your preferred level of redistribution, what matters most to you? Please indicate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

Figure D12: 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 D13: 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  $\sim 200$  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%.

Table D2: Distribution of Externality Beliefs in Control Group

	Crime	Corr- ruption	Pol. polar.	Social unrest	Unemp- loyment	Inno- vation	Econ. growth	Public goods	Quality of life	Dem. inst.	Trust
Increases	74%	67%	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 1. 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 societal factor of interest. “No change” is the share of respondents that state that inequality does not induce a change on the factor of interest. “Decrease” is the share of respondents that state that inequality “decreases a lot” or “decreases somewhat” the societal factor of interest. 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 E1 and E2.

## E Tables

Table E1: Distribution of Externality Beliefs in Survey 1 (Control Group)

	Crime	Corr- ruption	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 D2 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 societal factor of interest. “No change” is the share of respondents that state that inequality does not induce a change on the factor of interest. “Decrease” is the share of respondents that state that inequality “decreases a lot” or “decreases somewhat” the societal factor of interest. 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 E2.

Table E2: Distribution of Externality Beliefs in Survey 2

	Crime	Corr- ruption	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 D2 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 societal factor of interest. “No change” is the share of respondents that state that inequality does not induce a change on the factor of interest. “Decrease” is the share of respondents that state that inequality “decreases a lot” or “decreases somewhat” the societal factor of interest or the placebo question. For the equivalent table in Survey 1 see Table E1.

Table E3: 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 E4: 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 E5: 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 E6: 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 E7: 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 E8: 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 E9: 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 E10: 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 E11: 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 hours (placebo)	<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 E12: 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 E13: Correlations of sociodemographic and externality beliefs, 2

	(1) Ineq. red. inno. b/se	(2) Ineq. incr. unrest b/se	(3) Ineq. worsens dem. inst. b/se	(4) 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)	0.069*** (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 E14: Correlations of sociodemographic and externality beliefs, 3

	(1)	(2)	(3)	(4)
	Ineq. incr. corruption b/se	Ineq. incr. pol. pol. b/se	Ineq. incr. unemp. b/se	Ineq. decr. QoL 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 E15: Non-monotonic beliefs

	Crime	Trust	Social unrest	Pol. polar.	Corr uptron	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-monotonous in inequality level. Full question: *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 E16: 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 E17: Treatment effects with 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.	0.037 (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 E18: 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.	<b>17.04</b>	0.32	0.43
Trust tr.	4.48	<b>6.30</b>	0.12
Full ext tr.	<b>13.23</b>	<b>3.71</b>	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 E19: 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 E20: 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 E21: Emotional reactions to treatments

	(1) Active control	(2) Crime	(3) Trust	(4) Full externality	(5) 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 E22: First-stage effects of treatments with population weights

	(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.	0.061** (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 representativeness by gender, race, age, political affiliation, college degree, income-group, and geographic region. *Significance levels:* \*10%, \*\*5%, \*\*\*1%.

Table E23: Treatment effects including beliefs as regressors

	(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.	-0.009 (0.033)	0.021 (0.020)	-0.030 (0.020)	-0.006 (0.019)	0.003 (0.018)
Trust Ext. Tr.	-0.001 (0.034)	-0.004 (0.021)	-0.020 (0.021)	0.022 (0.019)	0.001 (0.019)
Full Ext. Tr.	0.054 (0.034)	0.039* (0.021)	-0.043** (0.021)	0.032* (0.019)	0.050*** (0.019)
Fairness Tr.	0.122*** (0.033)	0.032 (0.021)	0.031 (0.020)	0.034* (0.019)	0.079*** (0.018)
General neg. ext.	0.285*** (0.028)	0.055*** (0.017)	0.127*** (0.018)	0.097*** (0.016)	0.130*** (0.016)
Ineq. incr. crime	0.050 (0.032)	0.030 (0.018)	0.052*** (0.020)	-0.006 (0.018)	-0.004 (0.018)
Ineq. red. trust	0.076** (0.031)	-0.002 (0.018)	0.093*** (0.019)	0.006 (0.017)	0.012 (0.017)
Society is unfair (post)	0.407*** (0.030)	0.110*** (0.017)	0.114*** (0.019)	0.170*** (0.017)	0.191*** (0.018)
Rich because of hard work	-0.367*** (0.029)	-0.088*** (0.017)	-0.138*** (0.018)	-0.163*** (0.017)	-0.139*** (0.017)
Controls	Yes	Yes	Yes	Yes	Yes
R2	0.508	0.197	0.254	0.365	0.396
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 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 E24: Treatment effects interacted with male dummy

	(1)	(2)	(3)	(4)	(5)
	RP Index b/se	Wants redistribution b/se	Increase top taxes b/se	Gov. reduce ineq. b/se	Ineq. is serious issue 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 E25: Treatment effects interacted with prior externality belief

	(1)	(2)	(3)	(4)	(5)
	RP Index b/se	Wants redistribution b/se	Increase top taxes b/se	Gov. reduce ineq. b/se	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 E26: Treatment effects interacted with having a yearly income above \$50,000

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Ineq. is serious issue b/se	(4) Gov. reduce ineq. b/se	(5) Increase top taxes 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 E27: 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 E28: Treatment effects interacted with Republican leaning dummy

	(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.	0.039 (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 E29: Treatment effects interacted with dummy indicating that the subject believes that the current economic system in the US is unfair

	(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.	0.022 (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 E30: Predictive power of various beliefs in Survey 2

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index 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)
Trusted 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 E31: Predictive power of various beliefs with population weights

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index 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 E32: Treatment effects with controls using all completed responses

	(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.	0.039 (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 E33: First-stage effects of treatments using all completed responses

	(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.	0.083*** (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 E35: Treatment effects with controls using only respondents that passed all attention checks

	(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.	0.027 (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 E34: Predictive power of various beliefs using all completed responses

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index b/se
Rich because of luck		-0.550*** (0.059)				-0.337*** (0.056)
Society unfair (post)		0.628*** (0.058)				0.440*** (0.056)
Belief uneq. countr. worse			0.457*** (0.056)			0.298*** (0.050)
Neg. externality belief				0.600*** (0.055)		0.224*** (0.052)
Leans Republican					-0.361*** (0.084)	-0.194*** (0.072)
Sanders/Harris supporter					0.592*** (0.085)	0.331*** (0.076)
Trusts the government						0.220*** (0.036)
Taxation reduces work						-0.097*** (0.025)
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 E36: First-stage effects of treatments using only respondents that passed all attention checks

	(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.	0.096*** (0.025)	0.099*** (0.021)	0.058** (0.024)	0.107*** (0.027)	-0.000 (0.024)	0.011 (0.024)
Trust Ext. Tr.	0.046* (0.025)	0.047** (0.023)	0.096*** (0.024)	0.062** (0.028)	0.036 (0.024)	0.000 (0.024)
Full Ext. Tr.	0.075*** (0.025)	0.081*** (0.022)	0.102*** (0.024)	0.054* (0.028)	0.016 (0.024)	0.020 (0.024)
Fairness Tr.	0.084*** (0.025)	0.035 (0.023)	0.044* (0.025)	0.037 (0.027)	0.082*** (0.024)	0.054** (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 E37: Predictive power of various beliefs using only respondents that passed all attention checks

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index 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)
Taxation reduces work						-0.090*** (0.033)
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 E38: Treatment effects with controls and controlling for passing attention checks

	(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.	0.086*** (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 E39: Treatment effects, dropping active control group

	(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.	0.058 (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 E40: First-stage effects, dropping active 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.	0.098*** (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 E41: Treatment effects, dropping passive control group

	(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.	0.010 (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 E42: 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 E43: 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 E44: 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 E45: Treatment effects with controls using non-dichotomized variables

	(1) RP Index b/se	(2) Wants redistribution b/se	(3) Gov. reduce ineq. b/se	(4) Ineq. serious issue b/se	(5) Increase top taxes 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 E46: First-stage effects of treatments using non-dichotomized variables

	(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.	0.194*** (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 E47: Predictive power of various beliefs without controls

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index b/se
Rich because of luck		-0.655*** (0.063)				-0.418*** (0.060)
Society unfair (post)		0.646*** (0.063)				0.445*** (0.060)
Belief uneq. countr. worse			0.422*** (0.063)			0.249*** (0.054)
Neg. externality belief			0.622*** (0.063)			0.217*** (0.057)
Leans Republican				-0.458*** (0.089)		-0.292*** (0.079)
Sanders/Harris supporter				0.581*** (0.089)		0.334*** (0.081)
Trusts the government					0.216*** (0.040)	0.029 (0.036)
Taxation reduces work					-0.092*** (0.028)	-0.004 (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 E48: Predictive power of various beliefs using RP-index based on non-dichotomized variables

	(1) RP Index b/se	(2) RP Index b/se	(3) RP Index b/se	(4) RP Index b/se	(5) RP Index b/se	(6) RP Index 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 E49: Treatment effects with FDR sharpened q-values

	(1) RP Index	(2) Wants redistribution	(3) Increase top taxes	(4) Gov. reduce ineq.	(5) 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 3. p-values and q-values are in parentheses. *Significance levels:* \*10%, \*\*5%, \*\*\*1%.

Table E50: 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