

Fairness Beliefs Strongly Affect Perceived Economic Inequality

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Fairness Beliefs Affect Perceived Economic Inequality*

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Abstract

This paper establishes a causal link from fairness beliefs to perceived economic inequality. I conduct an experiment where participants are asked to estimate various income inequality measures of hypothetical societies. While the true income distributions of the societies remain identical and simple, the description of the societies varies to indicate “fair” and “unfair” inequality across respondents. Describing the society as “unfair” increases the incentivized estimated top 10% income share as much as the actual difference between Denmark and the United States. Other inequality metrics are similarly affected. The findings imply that ideological beliefs fundamentally alter how people perceive economic inequality.

1 Introduction

What drives economic fairness perceptions and redistributive preferences? The actual level of economic inequality is a natural candidate, an intuition which has been formalized by among others [Meltzer and Richard \(1981\)](#) and [Benabou and Ok \(2001\)](#). In recent years, however, various studies have argued that *perceived* and not *actual* economic inequality matters for how much people want to redistribute, or how fair they believe society is. This is motivated by the strong correlation between such outcomes and perceived – but not actual – economic inequality.¹ Following this, influential papers have noted that “*most theories about political effects of inequality need to be reframed as theories about effects of perceived inequality*” ([Gimpelson and Treisman, 2018](#)), that “*(mis)perceptions of inequality — but not actual levels of inequality — drive behavior and preferences for redistribution*” ([Hauser and Norton, 2017](#)), and that “*redistributive preferences are influenced less by actual distribution than by perceived inequality*” ([Niehues, 2014](#)). Such assertions, if true, are meaningful. Substantial effort has been made in connecting actual economic inequality to redistributive preferences through various dimensions ([Meltzer and Richard, 1981](#); [Fehr and Schmidt, 1999](#); [Benabou and Ok, 2001](#); [Alesina and Giuliano, 2011](#); [Roth and Wohlfart, 2018](#); [Lobeck and Støstad, 2023](#)). If *perceived* inequality is ultimately a more meaningful driver of fairness beliefs and redistributive preferences, the way we understand inequality dynamics could shift substantially.

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¹I show an example of this in [Figure C1](#).

In this paper I present evidence indicating that these conclusions may be premature. This is because fairness beliefs appear to affect perceived inequality, a relationship I identify in a controlled setting. Although the originally posited causal effect of perceived inequality on fairness beliefs may still exist, the reverse causality prevents strong conclusions to be drawn from correlational evidence. My findings also have implications for the debate on actual inequality levels across countries, where persistent disagreements may be explained by ideological differences. In short, if the way we perceive economic inequality is affected by our fairness beliefs *even in the ideal case when the income distribution is known*, the literature on inequality measurement may always be contentious.

To test the hypothesis that fairness beliefs affect perceived economic inequality I conducted two pre-specified survey experiments on the survey platform *Prolific* with a total of 1003 U.S. participants. In either survey, respondents were presented with identical income distributions under one of two different fairness contexts (“fair” and “unfair”) and asked to estimate the actual level of income inequality. The true income distribution was salient to respondents, simple, and identical across treatments. Individuals’ estimated level of income inequality was elicited through six commonly used perceived inequality metrics.

The two survey experiments show large causal effects of the (irrelevant) fairness information on five of six measures of perceived inequality. The only incentivized outcome was the estimated top 10% income share, where respondents’ otherwise fixed survey payout was significantly increased with accuracy.² The “unfair” treatment increased the estimated top 10% income share by 11 percentage points. This is roughly equal to the real-world difference between Denmark and the United States and strongly statistically significant ($p < 0.000001$).

The treatment has statistically significant effects for every directly estimated income inequality estimate. The only inequality metric that was not significantly affected was an *indirect* estimate, where an inequality metric was calculated from respondents’ estimates of the average incomes of various groups. Overall, the results indicate that perceived inequality is strongly affected by fairness beliefs independent of the actual level of income inequality.

This paper is not the first to argue that perceived inequality could be affected by ideology. The idea has been explored by among others [Chambers et al. \(2014\)](#); [Willis et al. \(2015\)](#); [Kteily et al. \(2017\)](#); [García-Sánchez et al. \(2018, 2019\)](#); [Du and King \(2022\)](#) and [Weisstanner and Armingeon \(2022\)](#). Causal evidence, however, has not yet been firmly established. These papers largely use correlational evidence (e.g. mediation analysis) or focus on correcting individuals’ misperceptions of their own income ([Weisstanner and Armingeon, 2022](#)). I contribute by cleanly showing that fairness beliefs causally affect economic inequality in an experimental setting even when the true underlying economic distribution is known. My work is also related to [Heiserman and Simpson \(2021\)](#), who show that individuals struggle to consistently and accurately respond to survey questions about perceived inequality.

The work also connects to the literature on a *polarization of reality* across ideological camps ([Alesina et al., 2020](#)). In this literature it is often unclear whether ideology causally changes (factual) beliefs or vice versa.³ I contribute by showing a specific example where ideological

²The otherwise fixed survey payout was increased by 55% if the guess was within 5 p.p. of the actual value.

³In [Alesina et al. \(2020\)](#), the authors note that “*The direction of causality is unclear: On the one hand, individuals could select into political affiliation based on their perceptions of reality. On the other hand, political*

beliefs affect the perception of objective reality. This has broader implications. An example is the recent debate on whether income inequality in the United States has increased in recent decades, where trends from [Saez and Zucman \(2020\)](#) and [Auten and Splinter \(2023\)](#) differ. My results indicate that individuals' ideological biases contribute to which of these results they are more likely to believe. Although such a bias is intuitive, my paper is to the best of my knowledge the first to show causality on the matter.

The paper also relates to a larger recent literature on mis-perceptions about incomes, wealth, and their distributions ([Cruces et al., 2013](#); [Kuziemko et al., 2015](#); [Karadja et al., 2017](#); [Ballard-Rosa et al., 2021](#); [Hvidberg et al., 2022](#); [Fehr et al., 2022](#)). First, this literature has found that informing survey respondents about true aggregate inequality levels has only small effects on redistributive preferences ([Kuziemko et al., 2015](#); [Ballard-Rosa et al., 2021](#)). My findings indicate that this muted effect could be because underlying fairness beliefs are not equally shifted from the information. Second, to explore whether income rank misperceptions could also be affected by fairness beliefs – as mentioned as a possibility by among others [Fehr et al. \(2022\)](#) – respondents in each treatment group were also asked how their own income would rank in the hypothetical society. I find no evidence of a fairness bias in this setting.

Section 2 outlines the survey methodology, Section 3 details results, and Section 4 concludes.

2 Methodology

I ran two pre-specified studies (Survey 1 and Survey 2) on the survey provider Prolific.⁴ Data was collected on November 22nd and 29th 2023. 501 respondents completed Survey 1 (S1) and 502 respondents completed Survey 2 (S2). All respondents were U.S. citizens. The median Survey 1 duration was 7 minutes and 32 seconds. The median Survey 2 duration was 4 minutes 58 seconds. Each sample is representative on gender and political affiliation. The full questionnaires can be found in Appendix A.

Survey 2 was designed to explore the robustness of Survey 1 with an independent sample.⁵ As such, the two surveys are structured similarly. They explore the same hypothesis of whether exogenously changing fairness beliefs affects perceived income inequality. The main difference is that Survey 2, which was designed after collecting results from Survey 1, presents a different income distribution in a different format and elicits more perceived inequality metrics.⁶ The below description is otherwise identical across surveys.

After demographic questions, respondents are introduced to the fictional country Surveyland. There are two treatment groups; the Fair and Unfair groups. Both groups are shown the same income distribution with additional varying information about how individuals earn their incomes. The structure in Survey 1 is shown below, where [TEXT] indicates the location of the exogenous variation:

affiliation affects the information one receives, the groups with which one interacts, and the media to which one is exposed, all of which can shape perceptions of reality.”.

⁴Respondents were not permitted to take both surveys. Pre-registered as AsPredicted #152146 and #152961.

⁵Particularly the non-significant result of the ISSP measure in Survey 1 – see Section 3 – which was hypothesized to be partly due to the presentation format of the income distribution.

⁶Survey 2 also does not explain and elicit individuals' estimated income ranks, explaining the duration difference.

- The richest 3 people, [TEXT1], earn \$375,000 each.
- The next 17 people, [TEXT2], earn \$121,000 each.
- The next 30 people, [TEXT3], earn \$41,300 each.
- The next 44 people, [TEXT4], earn \$24,400 each.
- The remaining 6 people, [TEXT5], earn \$12,500 each.

The information in [TEXT] is designed to evoke perceptions of unfair inequality in the Unfair treatment and fair inequality in the Fair treatment. The full variation is shown below:

	Unfair treatment	Fair treatment
1	who rule Surveyland by birthright and ruthlessly exploit workers	who are top experts in their fields and contribute significantly to the economy
2	who serve the top three rulers without question	who are highly skilled professionals with advanced education and significant experience
3	who are high-skilled individuals limited by unequal opportunities	comprising individuals in various trades and services who work regular hours
4	who work painfully long hours to produce most of Surveyland’s output	who chose entry-level positions to have more leisure time
5	who do Surveyland’s most unpleasant jobs	who preferred simple lives and mostly travel

Respondents are also informed that there are no other sources of income in Surveyland.

The information in Survey 2 is identical and the income distribution is still divided into five groups. There are two minor changes; the format of the text is presented in one paragraph instead of bullet points, and the number of individuals per group is different (indicating slightly higher inequality).⁷ Survey 2 also elicits further perceived inequality metrics (see below).

The income distribution (including fairness text) remains on-screen for every relevant question except where direct calculation is possible (e.g. for the top 10% income share and the perceived income rank). For such questions, respondents were informed immediately prior to carefully read the text again because it would be the focus of the next question.

In both surveys I first elicit pre-specified first-stage outcomes, which are individuals’ general fairness beliefs and redistributive preferences in the context of Surveyland. These outcomes are elicited through questions on whether (i) the economic differences between the poor and the rich are unfair (both S1 + S2), (ii) whether the rich are rich because of luck (only S1), and (iii) whether the national government should aim to reduce the economic differences between the rich and the poor (only S1). The questions all specify that the focus is Surveyland.⁸

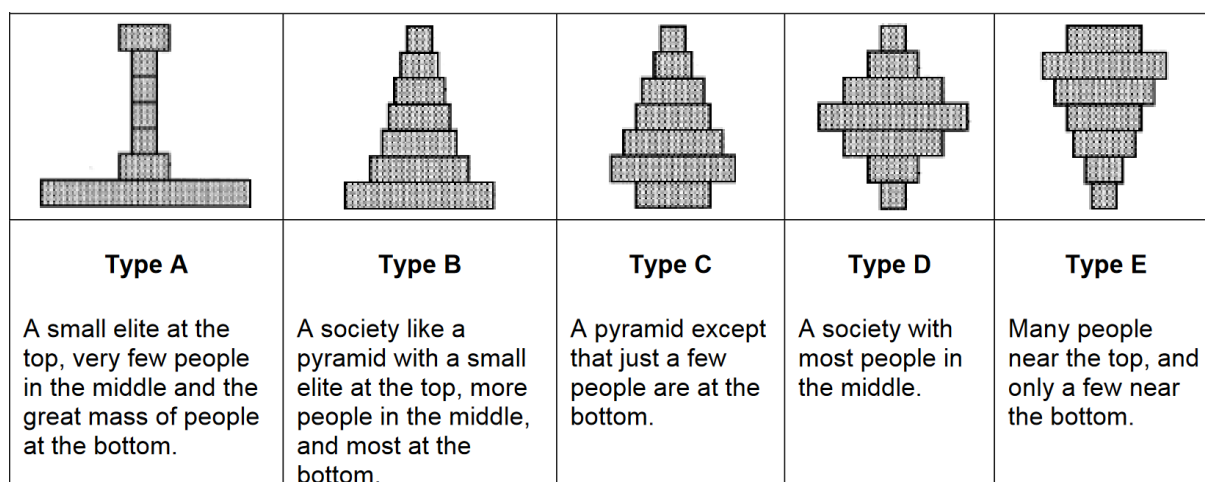
I then elicit perceived income inequality through six measures (varying across surveys):

1. (S1) Respondents’ were asked to estimate the top 10% income share. This outcome was incentivized; a guess within 5 percentage points would increase the survey payout by £0.5,

⁷Text: *The richest 15 people, [TEXT], earn \$375,000 each. The next 10 people, [TEXT], earn \$121,000 each. The next 20 people, [TEXT], earn \$41,300 each. The next 25 people, [TEXT], earn \$24,400 each. The remaining 15 people, [TEXT], earn \$12,500 each.*

⁸I also elicited the first of the two fairness questions in the context of the United States in both surveys. This question was pre-specified as a control in all main regressions.

Figure 1: ISSP pyramids for perceived inequality



or 55% of the otherwise fixed survey pay.^{9,10}

2. (S1+S2) Respondents are asked to “describe the level of income inequality in Surveyland” from 1 (very low income inequality) to 10 (very high income inequality).
3. (S1+S2) Respondents are asked to pick between different graphical representations of the income distribution, taken from the International Social Survey Programme (ISSP). Illustrated in Figure 1.¹¹
4. (S2) “How would you say income in Surveyland is distributed?” from “Rather equally” to “Extremely unequally”.¹²

These four outcomes indicate *directly* estimated inequality metrics. Respondents were also asked to recall the incomes of the top and bottom groups (S1), the average income of the top 10% (S1+S2), the average income of a CEO (S2), the average income of an unskilled worker (S2), and the average income (S2). These income estimates allow the calculation of two *indirectly* estimated inequality metrics:

5. (S2) The indirectly estimated pay ratio between a CEO and an unskilled worker.¹³
6. (S2) The indirectly estimated top 10% income share.¹⁴

Most of the related literature focuses on one of these six perceived inequality measurements.

⁹To prevent direct calculation the income distribution was not shown on the same screen. Prior to the elicitation respondents were shown the income distribution, told the next question pertained to it, and that it could increase the survey payout by £0.5. They were also informed that the next question would have a time limit of 60 seconds. The preparation screen allowed respondents to study the income distribution with no time limit. On the next screen, respondents were explained the top 10% income share and informed that the survey bonus would be paid if their estimate was within 5 p.p. of the true value. A timer was shown on the screen. 483 of 501 respondents responded within the time limit. The median time spent on the question was 37 seconds.

¹⁰This metric is similar to the wealth shares used in Norton and Ariely (2011).

¹¹The question text was also taken directly from the ISSP. Used by e.g. Niehues (2014); Gimpelson and Treisman (2018).

¹²Used by Knell and Stix (2021).

¹³Used by e.g. Osberg and Smeeding (2006); Gimpelson and Treisman (2018); Knell and Stix (2020).

¹⁴Calculated from respondents’ estimates of the average income of the population and the top 10%. Internally inconsistent estimates (below 10% or above 100%) were removed.

Finally, respondents in Survey 1 were explained the concept of an income ladder and income ranks and asked to estimate their position in Surveyland’s income distribution following methodology from Hvidberg et al. (2022).

For all regressions, a standard set of controls include demographic variables, political affiliation, and beliefs about the fairness of the U.S. income distribution.

3 Results

How does information about economic fairness affect perceived income inequality when the true income distribution is simple and salient?

I first show in Table 1 that respondents’ fairness beliefs and redistributive preferences in the setting of Surveyland were strongly shifted by the treatment. These were pre-specified first-stage outcomes, designed to explore whether the treatment affects fairness views and redistributive preferences as expected. All outcomes are statistically significant at very high levels ($p < 10^{-22}$).

Table 1: Treatment effects on fairness beliefs and redistributive preferences

	(1)	(2)	(3)	(4)
	Unfair S1	Unfair S2	Rich b/c luck	Redist. pref
Unit	1-5	1-5	1-5	1-5
Treatment	1.286*** (0.094)	1.235*** (0.094)	1.202*** (0.114)	0.980*** (0.093)
Controls	Yes	Yes	Yes	Yes
N	501	502	501	501

Note. Outcomes are, in order, (i+ii) whether the respondent agrees “the economic differences between the rich and the poor are unfair” in Surveyland, (iii) whether the rich in Surveyland became rich due to luck, (iv) whether the respondent agrees that “the economic differences between the rich and the poor should be reduced” in Surveyland. When a question is posed in both surveys, data from Survey 2 are denoted as S2. For all measures, higher values indicate higher perceived unfairness / more redistribution. All outcomes are from from “Strongly disagree” to “Strongly agree”. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

I then show the effect of the treatment on respondents’ perceived inequality in Table 2.

The treatment had a significant effect on almost all perceived inequality measures. The shifts are large and generally very statistically significant. The shift to the top 10% income share is statistically significant on the .0001% level, for example. The coefficient magnitude for the direct top 10% elicitation is 11 percentage points, or roughly the actual difference in the top 10% post-tax income share in Denmark and the United States in 2018.¹⁵ The treatment shifted perceived inequality across all inequality metrics commonly used in the related literature in at least one of the two surveys, generally by large magnitudes.

Two outcomes had no statistically significant treatment effects. The most meaningful of these is the indirect elicitation of the top 10% income share from Survey 2. This *indirect* estimate of the top 10% income share is calculated from individuals’ estimates of the average income of the top 10% and the average income of the population. There are no direct associations to inequality in these income elicitation questions.

¹⁵27.6% in Denmark and 39.2% in the United States, as measured through the wid.world database. Both the Fair and Unfair groups overestimate the level of inequality in the society. The true level was 39% – modelled off the top 10% income share in the U.S. in 2018 – and the group averages were 49% and 60% respectively.

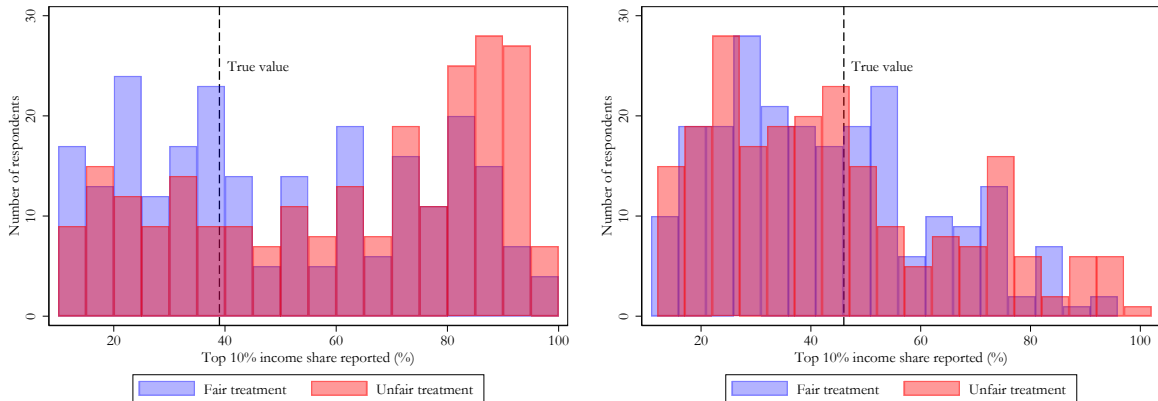
Table 2: Treatment effects on perceived inequality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unit	Top 10% 10-100	Likert 1-10	Likert S2 1-10	ISSP 1-5	ISSP S2 1-5	MCQ 1-4	CEO Ratio	Top 10% (ind) 10-100
Treatment	10.904*** (2.346)	1.683*** (0.217)	1.435*** (0.237)	0.122 (0.085)	0.208** (0.096)	0.865*** (0.074)	4.674*** (0.822)	1.367 (2.007)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	483	501	502	501	502	502	501	447

Note. Outcomes are, in order, (1) directly estimated top 10% income share, (2+3) Likert scale from 1-10 “describing the level of income inequality” from “very low” to “very high”, (4+5) ISSP “pyramid” question (see Figure 1), (6) multiple choice question letting the respondent choose how they “would say income in Surveyland is distributed” from “Extremely unequally” to “Rather equally”, (7) the ratio of CEO pay over unskilled worker pay – note that this is presented as a ratio for simplicity in contrast to the pre-specified $\ln(\text{ratio})$, although results do not differ in either case – and finally, (8) the “indirect” top 10% income share, where the income share was calculated from respondents’ estimated average *incomes* of the population and the top 10%. When a question is posed in both surveys, data from Survey 2 are denoted as S2. For all measures, higher values indicate higher perceived inequality. The top 10% income share has fewer observations as some respondents did not answer before the time limit. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

Unlike the direct top 10% share, the indirect top 10% share was unaffected by the treatment. Indirect top 10% share estimates are also significantly more accurate (closer to the true value) than direct top 10% share estimates. I show this in Figure 2.¹⁶

Figure 2: Directly estimated (left) and indirectly estimated (right) top 10% income share



Note. Left: Direct top 10% share estimates, found by asking respondents directly to estimate the top 10% income share in Survey 1. Respondents were incentivized for accuracy. Right: Indirect top 10% estimates, calculated with respondents’ estimates of the average income of the top 10% and the average income of the population in Survey 2. Note that the top 10% in Survey 2 were all contained within the top income group; as such, the top income group question was pure recall. *Significance levels:* *10%, **5%, ***1%.

A similar metric for the bottom 50% share (which was not pre-specified as a main outcome) was also unaffected by the treatment. Both these zero-results are due to the fact that perceived *incomes* were not strongly affected by the treatment. This is independent of whether the question was direct recall (what was the income of the top group?) or an estimation of a group not directly shown (e.g. the bottom 50%). I show this in Table D2.

This indicates that indirect inequality elicitation, where researchers elicit the expected incomes of various groups, may be a suitable path forward for research on perceived inequality. However, I indicate caution as it is particularly simple to estimate the incomes of groups in

¹⁶The average directly estimated top 10% share was 56%, compared to the true 39% (S1). The average indirectly estimated top 10% share was 43%, compared to the true 44% (S2).

this design. In real-world applications such an approach may be more similar to Column (7) in Table 2 which uses estimated incomes of a CEO and an unskilled factory worker in Surveyland to calculate a pay ratio, a perceived inequality metric used by e.g. Gimpelson and Treisman (2018) and Knell and Stix (2020). This pay ratio is significantly affected by the treatment.

The second insignificant outcome was the ISSP metric in Survey 1, where the same question was significantly affected in Survey 2. The difference was most likely due to the presentation of the income distribution in a pyramid-like shape in Survey 1.¹⁷

Mechanism The first-stage treatment effects indicate that the mechanism is through the expected fairness beliefs. To further explore this angle, I show in Table D1 that the treatment effect is strongly mediated through fairness beliefs; when including measures of the respondent’s beliefs about economic fairness in Surveyland, treatment effects become much weaker and often insignificant.¹⁸

The above analysis constitutes my main result:

Result #1

In a controlled experiment, changing individuals’ fairness beliefs strongly and causally changes their direct estimates of economic inequality.

Why do individuals’ fairness beliefs affect their perceived inequality (and not their income estimates)? Overall, the evidence indicates that the fairness bias enters through the error term of individuals’ estimation process when asked to estimate inequality itself (e.g. the top 10% income share or the “overall level of income inequality”). This could be due to either (i) the complexity of the estimation process introducing bias, or (ii) the respondent understanding that the question pertains to economic inequality and thus using the fairness of the economic system as a (potentially unsuitable) proxy. While the experiment does not allow these potential causes to be disentangled, I note that both are likely to be present in real-world cases.

I also show that the effect of the treatment was homogeneous across political affiliation and education in Tables D3 and D4. Finally, I show in Appendix B that individuals’ estimated income rank is unaffected by the treatment.

4 Conclusion

This study shows that information designed to affect economic fairness beliefs causally changes individuals’ perceptions of objective economic inequality. When presented with identical income distributions and narratives describing either conventionally fair or unfair situations in a controlled experiment, U.S. citizens showed a marked difference in their perception of objective

¹⁷The Survey 1 distribution was presented in bullet points which form the shape of a pyramid (the number of people in each group being 3, 17, 30, 39, and 11). The vast majority of respondents (70%) picked pyramid C or D, the pyramids most closely resembling this shape. In Survey 2, where the shape was less clear, the treatment was effective. Examining this hypothesis was part of the pre-specified motivation of Survey 2.

¹⁸Due to imperfect capture of fairness beliefs such a mediation analysis is unlikely to make all treatment effects insignificant.

income inequality. The difference in perceived inequality between the “fair” and “unfair” treatments is as large as the real-world difference between Denmark and the United States despite the true (identical) income distribution being simple and salient.

This outcome challenges the idea that views on fairness and redistributive preferences are predominantly shaped by perceived economic inequality. Instead, they indicate a reciprocal relationship where fairness beliefs actively shape perceptions of economic disparity and vice versa. As this may limit the usefulness of many perceived inequality measures, I also suggest *indirect* measures of inequality as a potentially useful substitute.

The existence of a causal link from ideology to perceived inequality has significant implications for the current political debate. Suppose, as this study indicates, that perceived economic inequality is strongly affected by ideology even when the objective level of inequality is known. The same relationship would likely be stronger when actual levels of inequality are uncertain – as they are in actuality (Saez and Zucman, 2020; Auten and Splinter, 2023). In debates on these topics, then, individuals could be strongly biased towards whichever inequality measure is more closely aligned to their ideological beliefs. This frames the debate on inequality measurement as a partly ideological debate. Taken at face value, my results pessimistically imply that this is unlikely to change – the amount of economic inequality may continue to be a subject of debate even as the true underlying distribution becomes increasingly well-measured.

Finally, my results may explain why interventions aimed at correcting misperceptions of economic inequality have a limited impact on willingness to redistribute (Kuziemko et al., 2015; Ballard-Rosa et al., 2021). Suppose that perceived inequality is, in reality, a weak determinant of redistributive preferences. If so, such interventions may not sufficiently affect individuals’ underlying fairness beliefs to also affect redistributive willingness.

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A Survey details

The full Survey 1 questionnaire can be found [here](#). The full Survey 2 questionnaire can be found [here](#).

Treatment variation The full description of the income distribution in the Unfair treatment was,

- The richest 3 people, who were born into the royal family of Surveyland and never work, earn \$386,000 each.
- The next 17 people, who serve the top three rulers without question, earn \$121,000 each.
- The next 30 people, who are high-skilled individuals limited by unequal opportunities, earn \$42,000 each.
- The next 39 people, who work long hours and produce most of Surveyland’s output, earn \$24,600 each.
- The remaining 11 people, who do Surveyland’s most unpleasant jobs, earn \$12,300 each.

The full description of the income distribution in the Fair treatment was,

- The richest 3 people, who are top experts in their fields and contribute significantly to the economy, earn \$386,000 each.
- The next 17 people, who are highly skilled professionals with advanced education and significant experience, earn \$121,000 each.
- The next 30 people, comprising individuals in various trades and services who work regular hours, earn \$42,000 each.
- The next 39 people, who chose entry-level positions to have more leisure time, earn \$24,600 each.
- The remaining 11 people, who preferred simple lives and mostly travel, earn \$12,300 each.

B Income rank

I also pre-specified a check on whether income rank depends on fairness beliefs. Respondents in Survey 1 were surveyed on their expected income rank within Surveyland. They were first told that the income distribution was actually “smooth” such that “*people are uniformly and randomly distributed. For example, the richest three people make \$121,000, \$248,000, and \$375,000.*”. They were then shown the “income ladder” used in [Hvidberg et al. \(2022\)](#) and explained the concept of income rank. After an understanding-based question, I asked them to estimate their own income rank in Surveyland through the following phrasing:

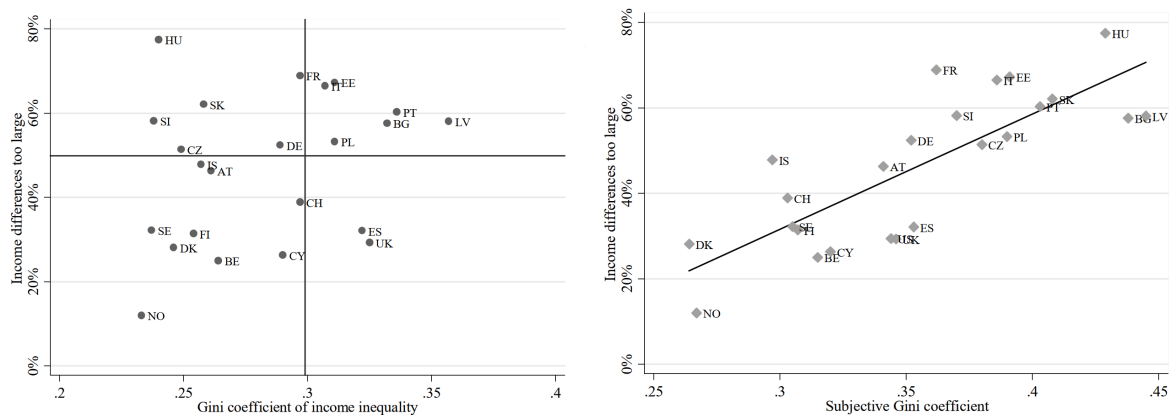
Suppose you lived in this version of Surveyland with your actual income in 2022 (as you reported earlier). We will now ask you to report where you think this income places you on the income ladder of Surveyland. [IMAGE OF LADDER] Which rank would your income in 2022 put you at in Surveyland’s income distribution? Remember, a higher rank means you had a higher income. Use the slider to select your position.

As I show in Table [D5](#), there were no significant differences between these groups' perceived income rank in Surveyland. There are also no differences across treatments for respondents' understanding of the rank methodology or individual incomes.

This indicates – though, due to the stylized setting, cannot conclude – that perceived income rank may not be significantly causally affected by ideology.

C Figures

Figure C1: Actual and perceived inequality's correlation to belief that differences are too large



Note. Actual and perceived inequality correlated to the belief that income differences are too large. Graph taken from Niehues (2014). Data from 2009. Perceived inequality created from a survey module in the ISSP (see Figure 1).

D Tables

Table D1: Mediation analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unit	Top 10% 10-100	Likert 1-10	Likert S2 1-10	ISSP 1-5	ISSP S2 1-5	MCQ 1-4	CEO Ratio	Top 10% (ind) 10-100
Treatment	4.334 (2.866)	0.527** (0.248)	0.339 (0.239)	-0.069 (0.104)	0.036 (0.117)	0.420*** (0.081)	3.381*** (0.960)	2.183 (2.341)
Unfair	2.867** (1.219)	0.593*** (0.113)	0.888*** (0.109)	0.101** (0.044)	0.139*** (0.047)	0.360*** (0.040)	1.048*** (0.356)	-0.641 (0.936)
Rich b/c luck	2.483** (1.034)	0.328*** (0.090)		0.051 (0.038)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	483	501	502	501	502	502	501	447

Note. Table 2 with first-stage effects for mediation analysis. For all measures, higher values indicate higher perceived inequality. Note that the first-stage questions do not perfectly capture compliers. As such, significant treatment effects may still occur if only fairness beliefs drive the treatment effect. Also note that the “Rich because luck” question was not posed in Survey 2, dampening the possible mediation. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

Table D2: Treatment effect on direct income elicitation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Top 10%	Top group	Bottom group	Average	Top 10% S2	Bottom 50%	CEO	Worker
Treatment	8.449 (6.574)	0.113 (2.302)	-0.122** (0.060)	2.627 (6.798)	-3.996 (8.119)	2.645 (5.536)	-10.243 (9.996)	-7.327 (4.619)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	501	501	501	502	502	502	502	502

Note. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

Table D3: Survey 1: Het. treatment effects by political affiliation

	(1)	(2)	(3)	(4)	(5)	(6)
	Unfair	Rich b/c luck	Redist. pref	Top 10% share	Unequal scale 1-10	ISSP
Treatment	1.391*** (0.117)	1.142*** (0.159)	1.018*** (0.117)	9.389*** (3.124)	1.779*** (0.293)	0.184* (0.109)
Treat*RepLean	-0.232 (0.190)	0.131 (0.227)	-0.083 (0.188)	3.358 (4.751)	-0.210 (0.444)	-0.136 (0.172)
RepLean	0.008 (0.152)	-0.381** (0.157)	-0.318** (0.146)	-5.202 (3.426)	0.022 (0.327)	0.207* (0.126)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	501	501	501	483	501	501

Note. All respondents are asked whether they are Republican-leaning or Democrat-leaning. Columns (1), (2), and (3) have answer options ranging from Strongly Disagree (1) to Strongly Agree (5). For all perceived inequality measures, higher values indicate higher perceived inequality – for the ISSP metric, the Type A society is coded as 5 (see Figure 1). The top 10% income share has fewer observations as some respondents did not answer before the time limit. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

Table D4: Survey 1: Het. treatment effects by education

	(1)	(2)	(3)	(4)	(5)	(6)
	Unfair	Rich b/c luck	Redist. pref	Top 10% share	Unequal scale 1-10	ISSP
Treatment	1.113*** (0.168)	1.252*** (0.207)	0.852*** (0.166)	11.116*** (4.201)	1.877*** (0.424)	0.249 (0.160)
Treat*College	0.253 (0.201)	-0.074 (0.247)	0.187 (0.200)	-0.310 (5.120)	-0.283 (0.491)	-0.185 (0.188)
College	-0.091 (0.161)	0.189 (0.162)	-0.099 (0.150)	-0.191 (3.616)	0.361 (0.357)	-0.005 (0.134)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	501	501	501	483	501	501

Note. All respondents are asked if they have a college degree. Columns (1), (2), and (3) have answer options ranging from Strongly Disagree (1) to Strongly Agree (5). For all perceived inequality measures, higher values indicate higher perceived inequality – for the ISSP metric, the Type A society is coded as 5 (see Figure 1). The top 10% income share has fewer observations as some respondents did not answer before the time limit. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

Table D5: Survey 1: Perceived income rank and income

	(1)	(2)	(3)	(4)
	Perceived rank	Rank understanding	Income (groups)	Income (exact)
	b/se	b/se	b/se	b/se
Treatment	0.950 (1.912)	-0.094 (0.058)	2.317 (4.376)	5.940 (6.402)
Controls	Yes	Yes	Yes	Yes
N	501	501	501	457

Note. Perceived income measures respondents' perceived rank in Surveyland. "Rank understanding" denotes whether respondents answered correctly to the factual rank question ("What is the P50 in Surveyland?"). Income (groups) use average incomes taken from 10 groups (e.g. "0-\$5,000" is \$2,500). Income (exact) is for respondents who reported their exact income. Robust standard errors in parenthesis. *Significance levels:* *10%, **5%, ***1%.

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