LEARNING ABOUT INEQUALITY AND DEMAND FOR REDISTRIBUTION: A META-ANALYSIS OF IN-SURVEY INFORMATIONAL EXPERIMENTS

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Acknowledgements

Abstract

A growing body of literature studies the effect of providing information about inequality to respondents of surveys on their preferences for redistribution. We provide a meta-analysis combining the results from 84 information treatments coming from 36 studies in Economics, Political Science, Psychology and Sociology. This meta-analysis complements and informs a broader project on perceptions of inequality and preferences for redistribution (OECD, 2021[1]). In the meta-analysis, we focus on in-survey experiments where a randomly selected group of respondents receive either information about the overall extent of inequalities, or about their position in the income distribution. The results show that providing information on inequality has a sizeable impact on people's perceptions and concerns about inequality, but a rather small effect on their demand for redistribution. Inspecting the heterogeneity across treatments and outcomes helps explaining the small average effect on demand for redistribution, but the evidence is not yet conclusive about the potential explanations. We further show that correcting respondents' misperceptions about their own position in the income distribution increases the preferences for redistribution for those who previously overestimated their position and decreases it for those who underestimated, although the effects are, on average, small.

Résumé

Un nombre croissant de publications étudient l'effet de fournir des informations sur les inégalités aux personnes interrogées dans le cadre d'enquêtes sur leurs préférences en matière de redistribution. Nous proposons une méta-analyse combinant les résultats de 84 traitements informationnels provenant de 36 études en économie, science politique, psychologie et sociologie. Cette méta-analyse complète et informe un projet plus large sur les perceptions de l'inégalité et les préférences pour la redistribution (OECD, 2021[1]). Dans notre méta-analyse, nous nous concentrons sur les expériences menées dans le cadre d'enquêtes où un groupe de répondants sélectionnés de manière aléatoire reçoit soit des informations sur l'ampleur globale des inégalités, soit des informations sur leur position dans la distribution des revenus. Les résultats montrent que le fait de fournir des informations sur les inégalités a un impact considérable sur la perception et les préoccupations des sujets quant aux inégalités, mais un effet plutôt faible sur leur demande de redistribution. Examiner l'hétérogénéité entre les traitements et les résultats permet d'expliquer le faible effet moyen sur la demande de redistribution, mais les preuves ne sont pas encore concluantes quant aux explications potentielles. Nous montrons en outre que la correction des perceptions erronées des répondants quant à leur propre position dans la distribution des revenus augmente les préférences pour la redistribution pour ceux qui surestimaient auparavant leur position et les diminue pour ceux qui la sous-estimaient, bien que les effets soient, en moyenne, faibles.

Table of contents

Tables

Figures

1. Introduction

1.   A majority of OECD countries have experienced a strong rise in income inequality in the last thirty years (OECD, 2015[2]; OECD, 2011[3]), while social mobility has slowed down (OECD, 2018[4]) and the middle class has been squeezed by rising costs, employment uncertainty and stagnating income (OECD, 2019[5]). Despite these trends, demand for redistribution seems to have stagnated on average across OECD countries. According to data from the International Social Survey Programme (ISSP), the fraction of respondents who agree with the statement that it is the responsibility of the government to reduce income differences has remained similar to the levels observed in the late 80s (Figure 1.1); the fraction that strongly agrees has only mildly increased (OECD, 2021[1]; Giger and Lascombes, 2019[6]).

Figure 1.1. Preferences for redistribution have stagnated in the last decades

Average across OECD countries of the percentage share of respondents who agree that it is the responsibility of the government to reduce differences in income
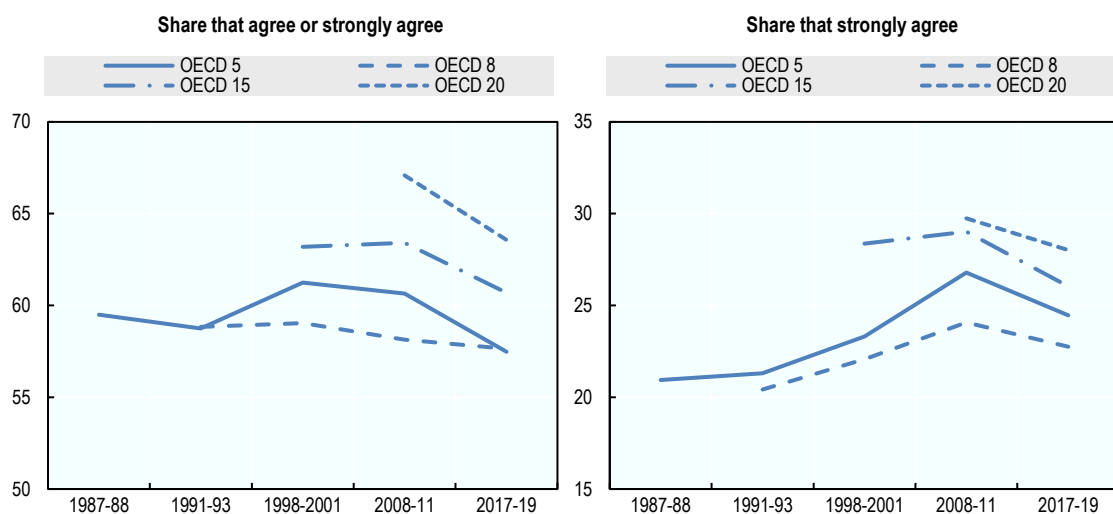
Note: Unweighted average across countries of the share of respondents who agree or strongly agree with the statement "It is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes". Despite of the availability of data, the figure does not include Germany in 1987 (only West Germany was surveyed), the Czech Republic and the Slovak Republic in 1992 (they were still part of Czechoslovakia; despite the availability of separate samples, the question in 1992 referred to the whole Czechoslovakia). As the aim is tracking the evolution over time, countries that have gaps (Italy and Switzerland) or do not appear in ISSP 2017 are not included. OECD 5: Australia, Austria, Great Britain, Hungary, United States; OECD 8: + Germany, New Zealand, Sweden; OECD 15: + Czech Republic, Denmark, Spain, France, Israel, Japan, Slovak Republic; OECD 20: + Switzerland, Finland, Iceland, Lithuania, Turkey.
Source: Calculations based on ISSP 1987, 1992, 1999, 2009, 2017.

2.   One potential explanation for the absence of a strong increase in demand for redistribution following the rise in income inequality is that people fail to connect their concerns about the rising disparities with the political reforms needed to tackle them. Bartels (2005[7]), studying the wide support for the tax cuts

enacted in the early 2000s in the United States, argues that individuals find it difficult to evaluate the broad range of tax policy effects, and therefore build their opinion on a short-sighted "unenlightened self-interest". Another possible explanation, discussed by Bechert and Osberg (2016[8]) among others, is that people's beliefs about the "feasible and legitimate role of government" (idem, p. 1) vary across countries and over time. A final explanation is that only a fraction of the population incorporates correctly the relevant information, and most of the people are actually misinformed about the true extent of income inequality. Gimpelson and Treisman (2018[9]) argue in favour of this last interpretation, showing that people's perceptions of inequality are widely inconsistent with the reality. Alesina, Stantcheva and Teso (2018[10]) show that US residents are on average over-optimistic with regard to social mobility, while Europeans tend to be more pessimistic, and that the difference at least partially explains international differences in preferences for redistribution.

3. These observations – and their possible explanations – raise two policy-relevant questions. The first is to what extent people incorporate information regarding income inequality in their perceptions and concerns about it. The second is whether such information changes their preferences for redistribution. Is the lack of support for redistribution due to the limited ability of people to process the available information, or to the limited connections between their beliefs/concerns and their demand for policy action?

4. The cross-country evidence from ISSP (OECD, 2021[1]; Giger and Lascombes, 2019[6]), suggests that in the long run people – on average – adjusted upward their perceptions of earnings disparities and their concerns about income differences, somehow in line with the rise in earnings and income inequality. However, the adjustment of beliefs and concerns does not seem to have translated into increased demand for redistribution. This suggests that people's views about the role of government in tackling inequalities, or their inability to connect concerns with policy action, play an important role in explaining the limited increase in preferences for redistribution. In fact, Giger and Lascombes (2019[6]) show that the distance between perceived disparities – what people think they are – and legitimate ones – what people think they should be – has become less relevant for driving demand of redistribution in recent years.

5. Observational evidence on the relevance of these channels suffers from two main limitations. First, the source of variation is common to all individuals from the same country, as all survey respondents are exposed to the same macro-economic environment. Secondly, issues of reverse causality emerge (from perceptions and opinions to policy), which are hard to tackle with country-level regressions.

6. To isolate the impact of new information about inequality on preferences for redistribution, a growing body of literature has built upon informational experiments. In the experiments, a randomly selected proportion of participants is provided with information regarding the distribution of outcomes or opportunities in the population, or about their own position within the distribution of income. As this information is provided randomly to participants, comparing the answers of the two groups (those with and without information) allows testing the questions raised in para. 3 by comparing the perceptions, concerns and preferences for redistribution of the two groups. Some experiments go further by examining the role of other issues, such as trust in government and the perceived effectiveness of policies, to explore different explanations for the results.

7. As part of a broader OECD report on perceptions of income inequality and preferences for redistribution (OECD, 2021[1]), this paper provides a quantitative meta-analysis of the available experiments. In line with the available evidence, we focus on two kinds of information provided to respondents:

   i.   The extent of current inequalities;
   ii.   The respondent's position in the income distribution of each country.

8. In the remainder of this paper, we refer to the provision of information to a (randomly selected) subgroup of respondents as an "information treatment". Information treatments of type (i), which relates to the overall distribution of outcomes/opportunities, speak closely to the literature suggesting a weak

connection between actual inequality and redistributive preferences. The second type of information treatment (type (ii)), which is about the respondent's relative income status, relates more to the literature about self-interested – or pocketbook – reasons for demanding, or not, more redistribution (Alesina and Giuliano, 2011[11]; Rueda and Stegmueller, 2019[12]). Both kinds of information play a crucial role in explaining preferences for redistribution.

9. To provide a quantitative review of the research at hand we start from a structured meta-analysis, in which we assess survey experiments, making them comparable on a common quantitative and standardised scale. In line with our two main research questions, we select only experiments focusing on the effect of information on people's beliefs about inequality, their concerns with it, and their preferences for redistribution. To match closely the observational evidence from long standing surveys, such as the ISSP, we focus only on experiments where the outcomes regard people's opinions about redistributive policies, excluding therefore those who focus on how people decide how to split income in a hypothetical situation or in a laboratory setting.

10. A quantitative meta-analysis has two main advantages. The first is that the weight given to available experiments does not depend on whether results are published or not, nor on the prestige of the journal that published them. The second is that we can compare the magnitude of the effects across studies and provide a non-subjective quantitative summary of the literature. This also helps to improve over the statistical power of individual studies, so to better discriminate between small and zero effects.

11. The main challenge in combining experiments from this stream of literature is the very large heterogeneity in methods, information provided, focus and outcomes. Among the studies collected in this meta-analysis, there is indeed evidence of a strong between-study variability. That said, we are not only interested in the "average" effect, but in the overall distribution of the results, and in explaining their heterogeneity. Given the large variability of results collected, even singling out studies with "stronger" effects requires putting the estimates on a common scale. Secondly, combining all the available experiments helps to highlight the missing evidence: Which kind of information matters the most in shaping people's preferences for redistribution? Is this information more effective in changing preferences for some policies than for others?

12. We pay particular attention to heterogeneity of results. First, we attempt as much as possible to collect comparable estimates not only by translating them on a common scale (by expressing them in terms of standard deviations of the control group, i.e. the Glass' Delta), but also by re-computing indices that aggregate multiple outcomes, wherever not already available in the original study. Secondly, we collect data on a large variety of characteristics of the studies and of the information received by the respondents, and we correlate them with effects size, through meta-regressions. Finally, we provide a systematic review of individual papers that shed light on specific theoretical explanations for the results.

13. Section 2 discusses the selection of studies and the meta-analysis method used in this paper in detail. It also explains how we make the estimates from different studies comparable by using the Glass' Delta measure. Section 3 briefly presents the characteristics of the different studies. Section 4 discusses the main quantitative results from informational treatments regarding inequality, and discusses the main theoretical explanations by means of a systematic review. Section 5 presents results from informational treatments on people's own position along the income distribution. Section 6 concludes.

2. Meta-analysis method

2.1. Collection and selection of studies

14.   Three main inclusion criteria guided our search. First, we selected only studies where participants were provided with a randomised informational treatment. Focusing on such experimental data allows us to draw a more robust causal inference regarding the effect of providing information on perceptions of inequality and preference for redistribution.

15.   Second, we chose to include informational treatments providing real-life data, which seem to be most relevant to answer the research questions at stake. This kind of treatments provide the respondents with data on social mobility, on economic inequality, or social outcomes in the respondents' country. Alternatively, they inform participants about their own situation, e.g. on their position in the income distribution. They differ from experiments based on games, where the information provided regards the rules of the game and the knowledge (or not) of the other participants' conditions within the game. Thus, we excluded this type of experimental studies from our meta-analysis.

16.   Thirdly, two types of outcomes are key to answer our research questions. On the one hand, we collected data capturing people's perceptions or concerns about inequalities. Measures of concerns with the extent of inequality fell under this category. These outcomes refer to our first question: to which extent do people incorporate the information regarding rising inequality? On the other hand, we collected data on preferences for redistribution, i.e. support for general, or specific redistributive policies. Therefore, we do not focus on data about political preferences per se, for instance regarding the vote for a specific party or coalition, even if we recognise that partisan identification may be one of the triggers of preferences for redistribution.

17.   Following these criteria, we started from searching on academic databases: Google Scholar, Mendeley, Ideas/Repec, SSRN, and the Social Science Registry. Table A A.1 in the Annex reports the list of keywords used in the search. We included working papers, both to avoid publication bias, and because this literature is relatively new and several papers have not yet reached publication. At this stage, we only conducted a loose assessment about whether the paper was likely to satisfy our selection criteria.

18.   To ensure that we are not excluding any relevant paper, we went through the references mentioned in the papers we collected through search engines. We did not iterated the process further. Finally, we randomly allocated the papers to review between the three authors of this paper. Each author decided whether to include or not a paper based on the three requirements listed above. Another author reviewed the decision made by other. If the situation was ambiguous, the choice was discussed and revised among the team members.

2.2. Data collection

19.   For each study, we collected information on treatment effects (and their standard errors) for all outcomes presented in the paper or mentioned therein. To avoid publication bias, we included outcomes from the appendices of the papers when it was clear that the outcomes shown in the main tables were only

a selection of all the outcomes collected in the original study. In some disciplines, it is common to present multiple specifications to estimate the treatment effects. In particular, some studies estimate the treatment effects using multivariate regressions, where pre-determined characteristics, such as age and gender of participants, are included as covariates. Although covariates should not matter in randomised experiments, some authors prefer to control for pre-determinate characteristics, both to correct for some degree of unbalancedeness in the pool of participants and to improve precision of the estimates. In case of multiple specifications, we collected estimates from the authors' preferred specification; in virtually all instances, estimates with and without covariates are similar, and therefore our choice should be a minor concern.

20.   The main issue to address in our meta-analysis was the large heterogeneity of outcomes considered in individual experiments, as the questions posed to participants did not align with those used in international surveys, but were mostly designed ad-hoc for each study. We addressed this issue as follows.

21.   First, we distinguished outcomes relating to perceptions/concerns, on one side, and to redistributive preferences, on the other. In the former group, we grouped together beliefs about the actual extent of inequality – which we call 'perceptions' (e.g. how many children from the poorest income quintile will still be poor once they grow up) – and opinions about whether inequality is too large – which we call 'concerns' (e.g. whether the respondents agree that income disparities are too large). We group together these outcomes because these terms are usually employed in the studies to assess whether respondents did, or did not, incorporate the information. In some studies, particularly in the psychological literature, this is just a manipulation check, to ensure that the treatment worked as expected. Nevertheless, we find these outcomes interesting per se, as it is not necessarily true that people incorporate new information, and that this fact affects their concerns. In fact, in some detailed analysis we also distinguished outcomes relating to "pure" perceptions from those relating to concerns, because the latter actually involve a normative assessment, which can be informative about changes in people's broader attitudes to inequality (e.g. about their preferences for more or less inequality). The second group of outcomes includes, instead, only those that refer to political interventions, whether general (e.g. the government should reduce income disparities) or specific (e.g. the top income tax should be increased).

22.   Secondly, we recoded the data, and reversed the scale where necessary, such that a positive effect implies always more inequality perceived, more concerns with inequality, or more redistribution. This procedure was performed by each reviewer, during data collection.

23.   Thirdly, we standardised all the estimated effects in order to make them comparable (i.e effect size). All the effects refer to the difference in an outcome between the treated and the control group. This standardisation can be done in different ways. The first methodological option is to refer to Cohen's d, by which the treatment is normalized by the standard deviation of the pooled sample. We choose, instead, to use the Glass's Delta measure, which standardises using the standard deviation of the control group. We preferred this option because different people might incorporate differently the information provided, and therefore the treatment may influence the dispersion of outcomes in the treatment group as well. In these circumstances, normalising the effects using the pooled standard deviation could be misleading.[1]

24.   Lastly, we selected for each study two main outcomes, one for perceptions/concerns and the other for redistributive preferences. Not all studies provide information about the outcome of the experiments on both dimensions. When a summary index is available for either dimension in the original study, we use the summary index. Otherwise, we focus on the authors' presentation and select the outcome that appears most relevant or general. We also use the entire list of outcomes when we run meta-regressions.

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[1] We lose 2 studies (4 interventions) for which we are not able to recover enough information to standardize the results. These are Kalleitner and Kittel (2019[62]) and Fehr, Mollerstrom and Perez-Truglia (2019[63]).

25.   Some experiments present evidence on a wide range of outcomes. For instance, several papers collect redistributive preferences for a wide range of political outcomes, e.g. tax rate, inheritance tax, food stamps. In order to perform the meta-analysis, for each dimension, a single outcome was needed to account for multiple testing. In this respect, we follow Alesina, Stantcheva and Teso (2018[10]) (who themselves follow Kling, Liebman and Katz (2007[13])) and focus on z-score indices that sum up multiple outcomes relative to the same dimension. In doing so, each outcome is standardised using the mean and standard deviation of the control group, and then all the outcomes are summed up with equal weights.[2] The effect is calculated using this aggregate outcome. Some papers already present this first-best statistics. As z-score indices are already standardised by the standard deviation(s) of the control group, the interpretation is the same. Therefore, we use them in the meta-analysis without further standardisation.[3]

26.   In the final meta-analysis dataset, each observation corresponds to a single treatment within each study and country. When studies include multiple countries, we consider each country as a separate intervention. The rationale is that each sample is independent from those for other countries, and there are important contextual differences among them. Each intervention is thus an independent treatment. In the remainder of the paper we use the terms "observation" and "intervention" interchangeably.

27.   When some information was missing in the original paper (in particular, the standard deviation of the control group or the effect on summary z-score indices) we run the calculations using the replication files (as supplied by the authors) or asked the authors to provide us with the additional results.

2.3. Meta-analysis method

28.   Considering that each intervention differs along several characteristics, and that our observations are defined at the intervention-country level, we allow for heterogeneity by assuming that each estimated Glass' delta effect $\widehat{\theta_j}$ is the sum of an intervention-specific effect plus the sampling error:

$$\widehat{\theta_j} = \theta_j + \epsilon_j \qquad\qquad (1)$$

where θ_j is the intervention specific effect, $\epsilon_j \sim N(0, \widehat{\sigma_j}^2)$ is the sampling error, and $\widehat{\sigma_j}$ is the (estimated) standard error of the effect. For the rest of the paper we use "effect" and "Glass' Delta effect" interchangeably.

29.   One of our aims is to estimate the average effects, hence we rewrite equation (1) as

$$\widehat{\theta_j} = \theta + u_j + \epsilon_j \qquad\qquad (2)$$

where u_j is the intervention specific effect expressed as a deviation from the average effect.

30.   As in Card, Kluve and Weber (2017[14]), we focus on describing the effects in the sample of available studies. To this purpose, different meta-analysis methods can be used. The most popular one is to use precision weights. These weights take into account the sampling error in equation (2), giving a higher weight to more precise estimates. However, this method is efficient only where there is no substantial variation in the intervention specific effect, i.e. if we can assume that $u_j = 0$. This is hardly the

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[2] If an outcome has missing values, we replaced the missing values with a zero when these refer to the control group, and with the standardized average among the treated observations when they refer to the treated group.

[3] Their standard deviation can be smaller than one because some of the components have more missing values, and the correction described in footnote 3 lowers their standard deviation (as the value is fixed for some observations). Furthermore, some components might be correlated, therefore reducing the standard deviation of the average index. Nevertheless, the interpretation is still in terms of standardized effects, and therefore no further standardization is warranted.

case in our context, where the type of intervention as well as the setting changes across studies, resulting in large heterogeneity of results. In the main analysis, therefore, we mostly focus on the simplest, unweighted estimates, but we discuss precision-weighted results too.

31.　　In the meta-analysis literature, an alternative estimation method is often used in cases where the between-studies heterogeneity is large. This method, usually called "Random-Effects" (RE), assumes that the sample of studies is a random draw from the population of possible studies. It further assumes that $u_j \sim N(0, \tau^2)$ and $\epsilon_j \sim N(0, \widehat{\sigma_j}^2)$, and that u_j and ϵ_j are independent. Using the "random-effects" method, the average effect is estimated as:

$$\hat{\theta} = \frac{\sum_{j=1}^{K} w_j \widehat{\theta_J}}{\sum_{j=1}^{K} w_j} \text{ with } w_j = \frac{1}{(\widehat{\sigma_j}^2 + \hat{\tau}^2)} \tag{3}$$

32.　　The only parameter to be estimated is the between-study variance $\hat{\tau}^2$, which is estimated using the normality assumptions through a Restricted Maximum-Likelihood method (REML). For completeness, we also present results using this method.

3. Characteristics of the studies

33.   The 36 studies included in our meta-analysis include 84 country-intervention pairs. Of these, 54 interventions provide information on inequality, 9 provide information on respondents' personal position in the national income distribution, and 22 provide a combination of the two (although in some cases with a stronger focus on the personal position). While a majority of the studies included were from economics, the topic has brought the attention from researchers from a wide range of disciplines: sociology, political science, psychology, and social policy. The treatments were led across a large number of countries, although mainly in the United States (38 of the treatments) and other OECD countries (Figure 3.1).

Figure 3.1. Geographical distribution of the interventions reviewed in this paper

Countries by number of interventions
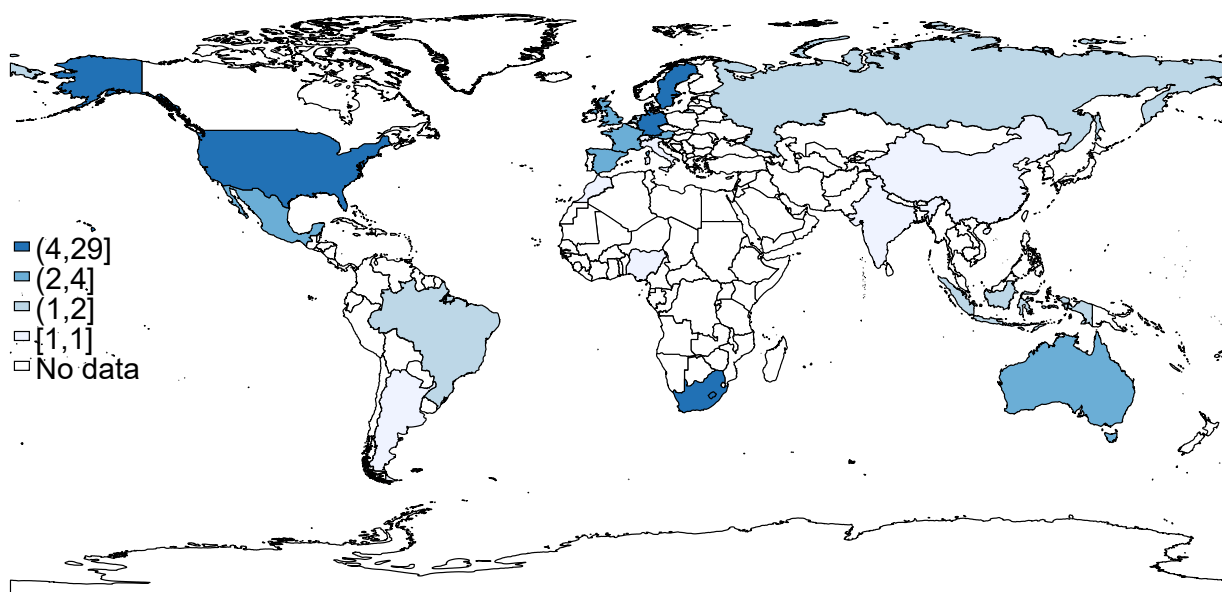
34.   Most of the interventions focus on providing information on country-level inequalities (Table 3.1). For instance, McCall et al. (2017[15]), in the first of their interventions, provide half of the respondents with an article about the rise of the inequalities in the United States since the 1990s. Hubers (2018[16]) showed half of the respondents a video about poverty in Singapore, while the control group was shown another video (about Panda's in China and tourism in Singapore). 28 interventions showed to respondents their position along the national income distribution, with higher or lower detail. For instance, Fernández-Albertos and Kuo (2018[17]), after eliciting respondents' perceptions about how many households have higher and lower income than them, provide them with the latest estimate based on nationally representative surveys. Three studies actually provide both types of information. We include Cansunar (2018[18]) and Kuziemko et al. (2015[19]) among those studies providing info on the general level of inequality, as the treatments and the analysis are mostly focused on that kind of information. We instead

include Bublitz (2020[20]) among the second group of studies, because it is built mostly around the role of perceived self-position along the income distribution. There is no simple way to show which study does what, but the data-collection table with the details of each study is available from the authors.

Table 3.1. Interventions reviewed by key characteristics

Media		Type of information and comparison		Reference		Type of inequality displayed		Type of information displayed	
Panel a. Interventions providing information about inequality (n=55)									
Image	15	Quantitative; info vs no info	15	National	43	Income	22	Actual	44
Image + Text	22	Quantitative; high vs low inequality	9	Sub-national	12	Mobility	2	Fake	11
Text	18	Qualitative; info vs no info	11			Poverty	12		
		Qualitative + Quantitative; info vs no info	19			Wealth	4		
		Qualitative; high vs low	1			Income + Wealth	10		
						Wealth + Mobility	5		
Panel b. Interventions providing information about personal position (n=29)									
Image	1	Quantitative; info vs no info	28	National	28	Income	29	Actual	28
Image + Text	19	Quantitative; high vs low relative position	1	Sub-national	1			Fake	1
Text	9								

35.   The content and type of information proved to participants also varies substantially across studies. Most studies use a graphical representation, either alone or combined with text, but some also present short animations or videos. All studies that focus on personal position within the distribution provide quantitative information based on the respondents' income. Among those focused on the overall level of inequality, the vast majority of studies presents quantitative information, either alone (e.g. a graph depicting income trends for different deciles of the distribution) or combined with qualitative information. Only some studies present purely qualitative results, for instance telling the respondents that scientific studies show that children born from poor families are more likely to be poor once they become adults. Qualitative information, either by itself or combined with quantitative info, always point towards a specific interpretation, i.e. that the inequality of outcomes is high or that intergenerational mobility is low. Purely quantitative information is instead left to the respondents' interpretation. However, it is usually intended to shift perceptions towards higher inequality and lower social mobility. Furthermore, while in most cases with quantitative information the control group receives no info, in others the control group receives different information that points to a lower level of inequality. For instance, Settele (2021[21]) provides respondents in the treatment and control groups with two different (actual) estimates for the gender wage gap in the United States, one significantly higher than the other.

36.   The focus of the information is mostly on income, but several interventions also focus on wealth and on mobility in terms of different outcomes. Finally, most studies present actual information on the state of the world; in few of them, instead, the treatment consists in showing fake high-inequality information to the treated group, and fake low-inequality information to the control group. In studies that provide information about personal position, provision of "fake information is used only by Brown-Iannuzzi et al. (2014[22]); the authors, after eliciting the respondents' assets, randomly provide them with either a feedback stating that they had more discretionary income than others individuals similar to them, or a feedback stating that they had less.

4.   The effect of information about inequality of outcomes and opportunities

4.1. Does new information shift perceptions and concerns?

37.   In the majority of interventions focusing on the overall inequality of outcomes and opportunities, the information provided points towards high level of inequality and/or low social mobility. When some qualitative information is provided, either alone or jointly with some statistics, it explicitly stresses the width of economic disparities and the strength of social immobility, with the purpose of inducing more pessimism with regards to inequalities. When the information is quantitative, it is such that it would usually point to higher inequality or social immobility than the majority of the participants thinks. This is the case of the high income of the top 0.1% in the United States in Zilinsky (2014[23]), pay gaps in Sweden and the United States in Trump (2018[24]), income inequality in South Africa in Pellicer et al. (2019[25]), wealth inequality in Indonesia in Hoy et al. (2020[26]). More neutral quantitative information is provided only in Cansunar (2018[18]), Martinangeli and Windsteiger (2019[27]), Becker (2020[28]) and Ballard-Rosa et al. (2020[29]).

38.   Given that the vast majority of treatments were intended to provide pessimistic information, we expect that, if people interpret correctly the information and incorporate it in their beliefs, the treatment should have a positive effect on perception and concern. Indeed, Figure 4.1 shows that information has a sizeable positive impact on perceptions and concerns about inequality. The figure focuses on all studies who present actual information on inequality.[4] We do not consider here those studies providing fake information, as the manipulation was designed to push beliefs to one direction and it is therefore not suited to address our first research question (whether perceptions react to information on actual inequality).

39.   The average Glass' Delta is 0.15 in EU countries, 0.17 in the United States and 0.16 in other countries. Overall, the information treatments lead, on average, to a change larger than 0.15 standard deviations in the perception or concern about inequality (recall that we measure effect sizes in terms of standard deviations in the control group). The 95% confidence intervals are consistent with the conclusion that, on average, the effect is positive and non-negligible. Results are slightly larger if we look at the precision-weighted estimates. However, they are similar if we use the weights produced by the RE method, which also accounts for the large variability between studies.

40.   The estimated average effect is not affected by single studies, even when they include more than one country-intervention (there are 18 studies for 38 country-interventions). If we re-estimate it by omitting

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[4] From the studies presenting both kinds of information, we exclude only Bublitz (2016[50]) because the study focuses mostly around the role of perceived self-position along the income distribution (including it does not change our overall conclusions); the study is included in the analysis in Section 5.

one study at a time, the effect spans from 0.14, when removing McCall et al. (2017[30]), to 0.16, when removing Hoy and Mager (2020[31]).[5]

41.   Looking at the distribution of single studies, most of the estimates are positive, even if to a different extent.[6] There are some exceptions. Cansunar (2018[18]) provides the respondent with the correct threshold to distinguish the poor, the middle class and the rich (top 10%). Treated respondents reduce their agreement with the statement that inequality is a problem. This result is actually consistent with the way in which the information provided changes perceptions: respondents tended to report very high thresholds for defining the top 10%. As a result, the information provided actually shows them *less* inequality than they expected. The negative result is, therefore, in line with the interpretation that people correctly interpret the information and revise their concerns accordingly. Hoy and Mager (2020[31]) find that providing US respondents with information about the actual level of both inequality and social mobility lowers their agreement with the statement that income differences are too large. The authors show that this result is mostly driven by respondents who, before the treatment, said that they did not prefer lower inequality. For this reason, they interpret it as evidence that people actually increase their preferred level of inequality after knowing its true extent. This is related to the results by Trump (2018[24]), who finds that informing respondents about the true extent of inequality has a statistically insignificant (although not very small) effect on the agreement with the statement that income differences are too large in both Sweden and the United States. We will get back to this point in Section 4.3.

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[5] Notice that these are not the studies including the minimum and maximum effect, because some studies include multiple interventions. Taking the union of the 95% confidence intervals, the (lowest of the) lower bound is 0.10, while the (largest of the) upper bound is 0.22.

[6] The interventions from Ballard-Rosa et al. (2020[29]) are somewhat different from the others, as the outcome refers to "quasi identification" of certain income thresholds. A positive effect still means that individuals who receive the information are more likely to hold correct knowledge and therefore is in line with the main qualitative conclusions. Excluding these 4 interventions has small effects on the estimated average effects.

Figure 4.1. Effects of providing information about inequality on participants' perceptions and concerns
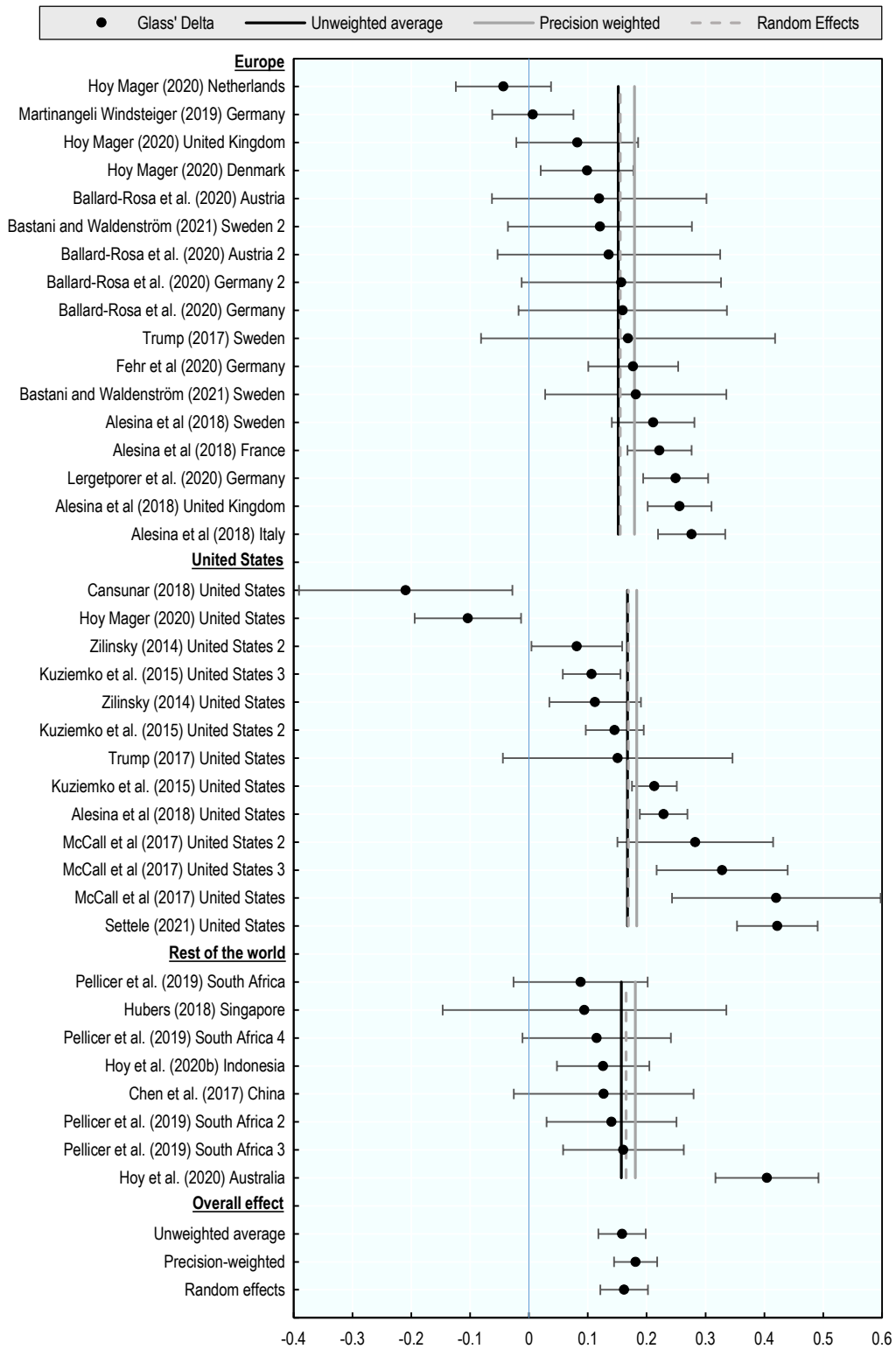
Note: Glass' Delta effect size (effect in terms of standard deviations). Positive effects mean higher perceived inequality or greater concerns.

42.   Because the studies are rather heterogeneous, we also investigate the distribution of the treatment effects. Figure 4.2, panel a, shows the estimated distribution of effects. Interestingly, there does not seem to be much of a publication bias. Publication bias usually occurs if researchers are more likely to report large and statistically significant estimates and not those that are insignificant and close to zero. This would skew the distribution of the estimates towards large values. The distribution of the effects in our sample is instead quite symmetrical around the average and there is no sign of an over-reporting of large estimates. Another sign that publication bias holds is when effects in smaller studies, which have higher variance, differ systematically from those in larger studies. This could happen when authors only report statistically significant estimates (the publication bias), so that larger effects are more likely to be associated with smaller – and less precise – studies. This does not seem to be the case. In the panel b, the Funnel plot (relating each estimated effect size with its precision $(1/\hat{\sigma}_j^2)$ shows that estimates with lower precision do not seem to be systematically stronger or above the 95% significance area.

Figure 4.2. Distribution of effects of information about inequality on perceptions and concerns
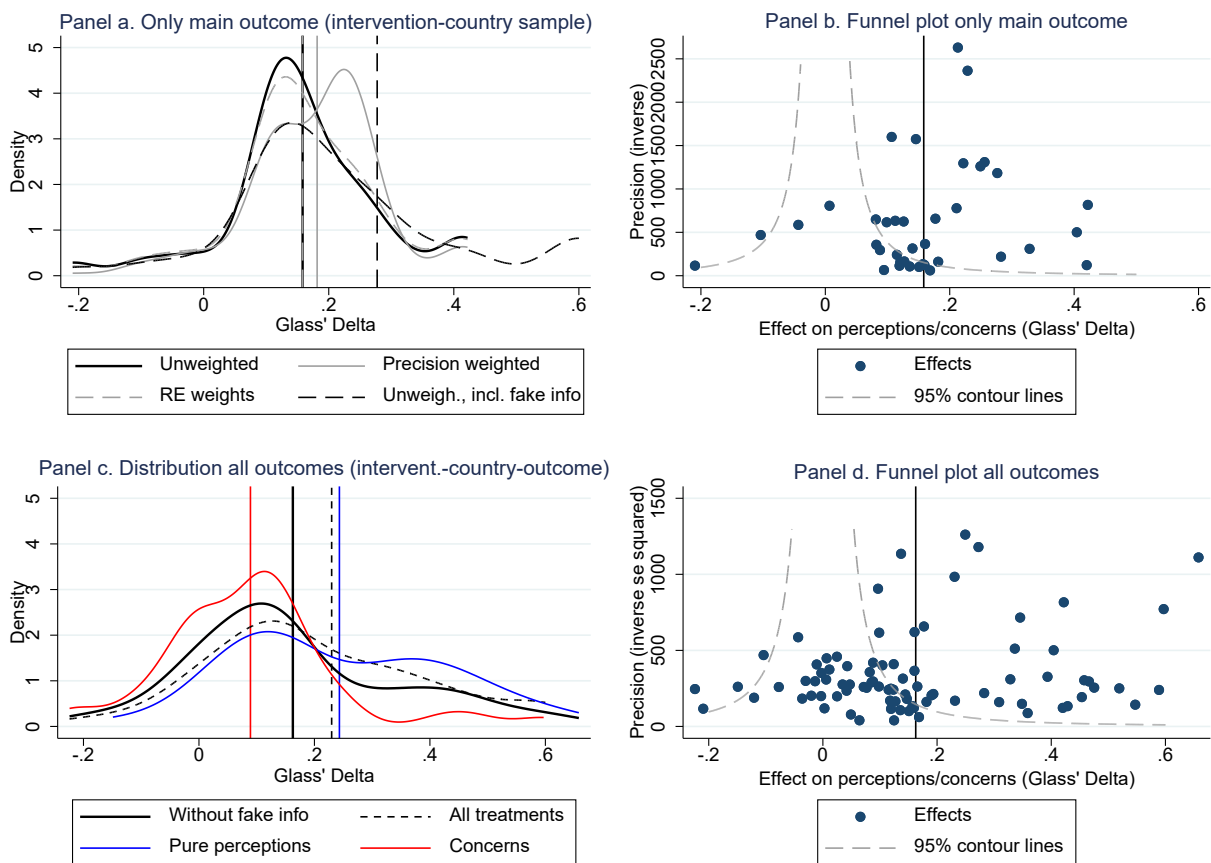
Note: Vertical lines are averages. For panel a and b, n=38 without fake information treatment; n=47 when including them (only panel a). Precision weights are the inverse of the variance of the estimate $(1/\hat{\sigma}_j^2)$; Random effects weights account also for between studies variance $(1/(\hat{\sigma}_j^2 + \hat{\tau}^2))$. In panels c and d, observations are not weighted and only non averaged outcomes are included, n=86 without fake information treatments, n=116 with all treatments (only panel c); the distribution of all treatments includes 5 cases which have been censored at 0.6. The densities are estimated using an Epanechnikov kernel and the Silverman's rule of thumb as bandwidth. The 95% c.i. lines in panels b and d represent the relation implied by a t-statistic equal to 1.96 (effect^2=1.96^2/precision); effects above the lines are statistically significant at the 95% level.

43.   If we include the effects from studies providing "fake" information to participants, the distribution and the average effect shift to the right. This is expected, because these studies present information

tailored to manipulate respondents' perceptions. In most of these studies the control group also receives information, which is of the opposite nature (for instance displaying low inequality instead of high inequality).[7] There are five instances in which the effect is larger than 0.6 (Côté, House and Willer, 2015[32]; Davidai, 2018[33]; Browman, Destin and Miele, 2021[34]); we censor them at 0.6 in the picture for presentational reasons.

44.   Studies often analyse the effect on multiple outcomes. While 45% of the interventions considered here have only one perceptions/concern outcome, 25% collect two, and the remaining 30% collect up to seven outcomes. To assess whether the average effect and their heterogeneity would change using all the available estimates, we expanded the sample by using, as unit of observation, the triplet intervention-country-outcome.[8] Figure 4.2, panel c, shows the distribution of the effects, estimated by weighting the estimate for each outcome by the inverse of the estimated variance of the effect. The average effect is very close to the one estimated using only one (usually averaged) outcome per study, but the heterogeneity is much larger. There is still no evidence of a strong publication bias (from the Funnel plot in Figure 4.2, panel d).The effect is also larger on pure perceptions (e.g. the fraction of children from a poor family that will remain poor once they grow adult) than on concerns (e.g. agreement with the statement "income differences are too large").

45.   To analyse the determinants of the strong differences in the effects on participants' perceptions and concerns, we run meta-regressions. We start from analysing the role of intervention characteristics by focusing on the main intervention-country sample. We focus on a linear model:

$$\hat{\theta}_j = \beta_0 + \beta'_{intervention}x_j + \beta_\sigma\hat{\sigma}_j + u_j + \epsilon_j \qquad (4)$$

which we estimate by OLS, assuming that $Cov(x_j, u_j + \epsilon_j) = 0$. As common in the meta-analysis literature, we include the standard error of the estimate among the regressors. This aims at testing whether publication bias occurs, because $\beta_\sigma > 0$ implies that less precise estimates are systematically associated with stronger effects (this check is usually called the Egger's test). In line with the estimation of the average effect, we also show precision-weighted results, which are however efficient only insofar $u_j = 0$, and RE estimates, which employ a restricted maximum-likelihood estimator based on the normality assumptions for u_j and ϵ_j. We exclude the result from Cansunar (2018[18]) because, as we discussed above, it can be interpreted as if respondents received information that inequality is lower than what participants expected.[9] Among the characteristics, we do not consider whether the studies also provide information on own position (as only one study in the sample does, Kuziemko et al. (2015[19])). As it is extremely relevant, we keep separate the dummy for quantitative treatments where both groups are provided with estimates of inequality, one high (for the treated group) and one low. In the sample excluding fake-info interventions, this amounts at singling out the paper by Settele (2021[21]), which has quite strong effects on perceptions/concerns (we do not display the s.e. for obvious reasons).

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[7] These studies are Côte, House and Willer (2015[32]) (1 intervention), Shariff, Wiwad D and Aknin (2016[51]) (1 intervention), Davidai (2018[33]) (4 interventions), Browman, Destin and Miele (2021[34]) (3 interventions).

[8] To avoid duplication, in this sample we include only the original outcomes, and not the average indices summing up different outcomes.

[9] The alternative would be to recode the outcome. However, given that the information presented there is neutral and not explicitly directed to point out high or low inequality, it seems quite arbitrary to do so.

Table 4.1. Differences in the effect of inequality information on participants' perceptions and concerns

Impact of the characteristics of the treatment

Characteristics of the treatment		(1)	(2)	(3)	(4)
		Dependent variable: Glass' Delta effect			
		Unweighted	Precision weight.	RE weighted	Unweighted
Type of information (ref. actual)	Fake				0.16
					(0.27)
Media (reference: Image or video)	Image + text	0.15*	0.06	0.13	0.09
		(0.09)	(0.07)	(0.08)	(0.15)
	Text	0.08	0.03	0.06	-0.01
		(0.07)	(0.06)	(0.08)	(0.17)
Reference (ref. national level)	Subnational	0.00	-0.06	-0.02	0.22
		(0.05)	(0.06)	(0.10)	(0.21)
Type of information (ref. Quantitative; info vs no info)	Quantitative; high vs low inequality	0.31	0.35	0.32	0.35***
		.	.	.	(0.07)
	Qualitative; info vs no info	0.11*	0.17**	0.13**	0.12*
		(0.05)	(0.07)	(0.06)	(0.06)
	Qualitative + Quantitative; info vs no info	0.06	0.09**	0.07	0.06
		(0.04)	(0.04)	(0.05)	(0.05)
	Qualitative; high vs low inequality				0.03
					.
Type of inequality displayed (ref.: mobility)	Outcome	0.09	0.06	0.09	0.04
		(0.10)	(0.09)	(0.07)	(0.11)
Country (reference: EU)	US	0.02	-0.00	0.02	0.04
		(0.07)	(0.05)	(0.05)	(0.08)
	Rest of the world	0.06	0.09	0.07	-0.03
		(0.08)	(0.08)	(0.06)	(0.11)
$\hat{\sigma}_j$		-0.01	-0.41	0.03	0.96
		(0.94)	(1.12)	(0.88)	(1.78)
Constant		-0.07	0.00	-0.07	0.00
		(0.10)	(0.10)	(0.11)	(0.18)
Number of observations (interventions)		37	37	37	46
R2		0.35	0.45		0.66

Note: * significant at the 10% level; ** 5% level; *** 1% level. Meta-regression on a sample of country-intervention observations, excluding Cansunar (2018[18]); unweighted OLS in columns (1) and (4); weighted by inverse of variance of the estimate in column (3) and via the random effects approach in column (3). Standard errors in parentheses are heteroscedasticity robust for unweighted OLS and precision-weighted; the standard errors of the RE weighted regressions are based on the normality assumptions. Only treatments about inequality, excluding fake info in columns (1)-(3).

46.   Results are displayed in Table 4.1. There is no evidence of a publication bias, as $\widehat{\beta_\sigma}$ is close to zero and we fail to reject the null that $\beta_\sigma = 0$ at conventional significance levels. Purely quantitative treatments tend to have a smaller impact on perceptions and concerns, and using some sort of text accompanying graphs or videos is associated with a stronger effect. The impact is larger if the information provided is about outcomes, rather than mobility; the difference is sizeable, though not statistically significant at conventional levels. Conditional on the intervention characteristics, the effect seems to be lower in the EU (the reference group) and in the United States than in other countries, although the difference is not appear to be statistically significant. Results are similar using different methods. They are also similar adding fake-info interventions, which, as expected, are associated with a strong effect. The only noticeable difference is that the treatments providing information about inequality at the sub-national

level (regions, or states for the United States) seem to have a stronger effect, which however should be interpreted with caution because, among the non-fake info interventions, only two (both from Pellicer et al. (2019[25])) display sub-national info.

47.   Results might differ also with respect to the characteristics of the perception/concern outcome being assessed, which varies considerably across studies. To study this, we run meta-regressions on the expanded sample (intervention-country-outcome triplet). Formally, we estimate the regression:

$$\widehat{\theta_{ij}} = \beta_0 + \beta'_{intervention}x_j + \beta'_{outcome}x_i + \beta_\sigma \hat{\sigma}_{ij} + u_j + \epsilon_{ij} \qquad (5)$$

where *i* refers to the specific outcome, *j* refers to the country-intervention pair, x_j' are characteristics of the intervention and x_i' of the outcome. ϵ_{ij} includes both sampling error related to the specific outcome and outcome specific differences that are not accounted for by x_i'. We assume that u_j and ϵ_{ij} are uncorrelated with (x_j', x_i') and estimate the regression by unweighted OLS, although as before we also show precision-weighted estimates where each observation is weighted by the inverse of the (estimated) variance of each effect. Standard errors are always clustered at the intervention level, to account for the presence of u_j. We include the same x_j' characteristics as in Table 4.1, but we do not show them because the results might be distorted by the fact that – in the expanded intervention-country-outcome sample – some interventions are over-represented because the authors collected information on more outcomes. Results for the $\beta'_{intervention}$ are nevertheless in line with those shown in Table 4.1. [10]

48.   Results are in Table 4.2. Importantly, even conditional on other characteristics, the effect is confirmed to be stronger for pure perceptions (such as the extent of the income gap between the poor and the rich) than for concerns (such as concern over income disparities; column 2). Providing information about the poor, rather than just about disparities, seem to be more effective in changing perceptions/concerns, although the effect is not statistically significant and rather unstable across the different specifications. The effect seems to be also stronger on outcomes that assess people's perceptions of concerns on both inequality of outcomes and opportunities combined. In column 2 we also introduce interactions between the type of information provided and the type of outcome. Unsurprisingly, information on social mobility has stronger effects on outcomes that regard social mobility, while information on inequality of outcomes have stronger impact on outcomes in the same dimension. Finally, purely quantitative answers also seem to be associated with a stronger effect, although the opposite holds once we include in the sample fake-info interventions.

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[10] An alternative would have been to reweight each observations by the inverse of the number of outcomes reported in intervention *j*. This is the method used by Havránek (2015[54]). However, in this case we prefer to keep the two regressions separated, to better stress the source of variation that identifies the different components.

Table 4.2. Differences in the effect of inequality information on participants' perceptions and concerns

Impact of the characteristics of the outcome

Characteristics of the outcome		(1)	(2)	(3)	(4)
		Dependent variable: Glass' Delta effect			
		Unweighted	Unweighted	Precision weight.	Unweighted, including fake info treatments
Concern/Perception (reference: concern)	Perception	0.12**	0.14**	0.11***	0.24***
		(0.05)	(0.05)	(0.04)	(0.06)
On mobility/merit/outco. (ref.: mob. or merit)	Outcomes	0.00			
		(0.06)			
	Combined	0.12*			
		(0.07)			
Qualitative answer/numerical answer	Numerical answer	0.05	0.04	0.06	-0.12*
		(0.08)	(0.08)	(0.06)	(0.06)
On gaps/the poor/ the rich (ref: the gap)	Poor	0.06	0.04	0.02	0.10*
		(0.06)	(0.06)	(0.05)	(0.06)
	Rich	-0.04	-0.03	-0.02	0.06
		(0.08)	(0.08)	(0.06)	(0.11)
Interactions between info and outcome (ref. info on mobility -> percept./concern about mobility)	info on mobility -> percept./ concern about outcomes		-0.09	-0.15**	-0.04
			(0.07)	(0.06)	(0.08)
	info on mobility -> percept./ concern combined				0.12***
					(0.04)
	info on outcomes -> perc./ concern about mobility		-0.01	-0.03	-0.14
			(0.09)	(0.08)	(0.11)
	info on outcomes -> perc./ /concern about outcomes		0.04	-0.02	0.07
			(0.09)	(0.08)	(0.10)
	info on outcomes -> perc./concern combined		0.13	0.07	0.25
			(0.10)	(0.08)	(0.16)
$\hat{\sigma}_j$		0.04	0.15	-0.91	0.51
		(0.94)	(0.95)	(0.96)	(1.16)
Characteristics of the treatment		X	X	X	X
Number of observations (outcomes)		85	85	85	115
Number of clusters (interventions)		37	37	37	46
R2		0.54	0.55	0.68	0.56

Note: * significant at the 10% level; ** 5% level; *** 1% level. Meta-regression on a sample of country-intervention observations, excluding Cansunar (2018[18]); unweighted OLS in columns (1), (2) and (4); weighted by inverse of variance of the estimate in column (3). Standard errors in parentheses are clustered at the intervention-country level. Only treatments about inequality, excluding fake info in columns (1)-(3). Only outcomes referring to perceptions or concerns, excluding summary indices. All the regressions include also the characteristics of the intervention from Table 4.1.

4.2. Does the information provided change participants' preferences for redistribution?

49.   Even when people correctly incorporate the information provided by the experiment, this does not necessarily lead to higher demand for redistribution. In Figure 4.3 we summarize the evidence about information treatment effects on preferences for redistribution, where a positive effect means a stronger demand for the government to intervene. Here we include experiments providing "fake" information, because they are suited to assess the impact of inequality information on demand for redistribution.

Figure 4.3. Effects of providing information about inequality on participants' preferences for redistribution
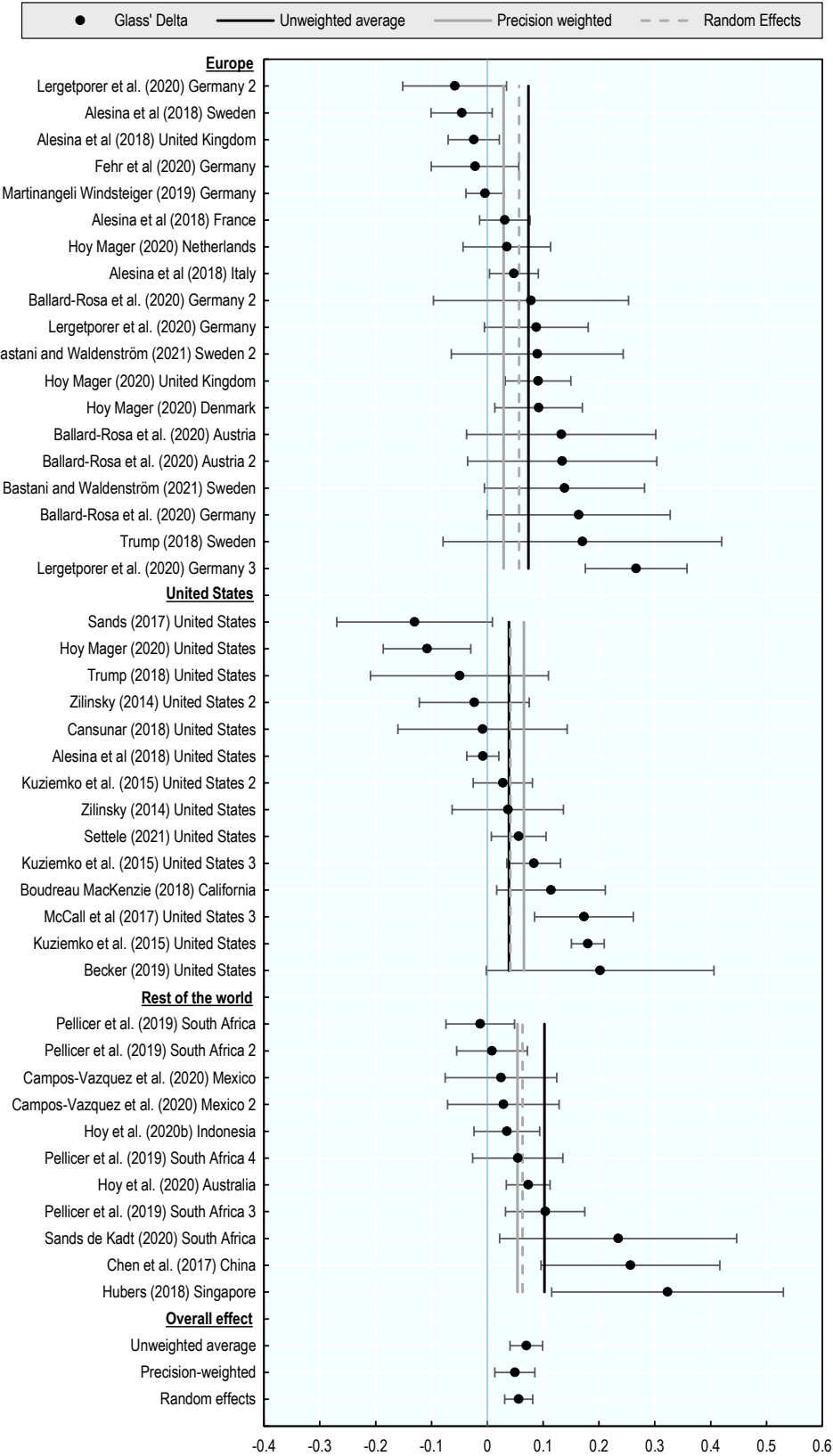
50.　　On average, the treatment effects are quite small. The increase in power due to the meta-analysis confirms that the average effect is positive and statistically different from zero, but small. This suggests that the increased perceptions and concerns translate only partially into higher preferences for redistribution. As suggested by Bartels (2005[7]), concerns are at least partially muted when it comes to political action.

51.　　The effects are quite small in the three set of countries shown in Figure 4.3. The unweighted averages are larger in the European countries and in the "other countries" group, although they seem to be driven by less precise estimates in some specific studies, as both the precision-weighted and the RE averages are smaller. Nevertheless, re-estimating the model by omitting one study out at a time, the average effect remains quite similar, spanning from 0.064, when removing Hubers (2018[16]), to 0.079, when removing Alesina, Stantcheva and Teso (2018[10]).

52.　　The distribution of the effects on demand for redistribution is less dispersed and more concentrated around zero than the effects on perceptions/concerns (Figure 4.4, panel a). It is also quite symmetric, suggesting that there is no strong evidence of publication bias, which would usually leads to a distribution strongly skewed far away from zero. The distribution and the average effect are only sightly affected by the choice of weights; precision-weights, in particular, give less emphasis to some larger estimates. Results are virtually identical when removing the two studies providing "fake" information.[11] Differently from the distribution of the effects on concerns/perceptions, here the distribution is quite compressed around a small positive effect also if we look at the entire sample of estimated effects, including all outcomes in the intervention-country-outcome sample (Figure 4.4, panel c)

53.　　There is some evidence of publication bias, as the Funnel plots for the main and expanded samples show that less-precise estimates are slightly more likely on the right of the 95% confidence interval threshold. If anything, this would further confirm our main conclusion that the effect of providing information on participants' redistributive preferences is rather small.

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[11] It should be noted that in the case of the studies included here, the label "fake" information might be misleading, because the authors exposed passers-by to visual depiction of either poor people (as portrayed by an actor on a rich neighbourhood street; Sands and de Kadt (2020[53]) or great wealth (an expensive car in a poor neighbourhood; Sands (2017[52])).

Figure 4.4. Distribution of effects of information about inequality on participants' preferences for redistribution
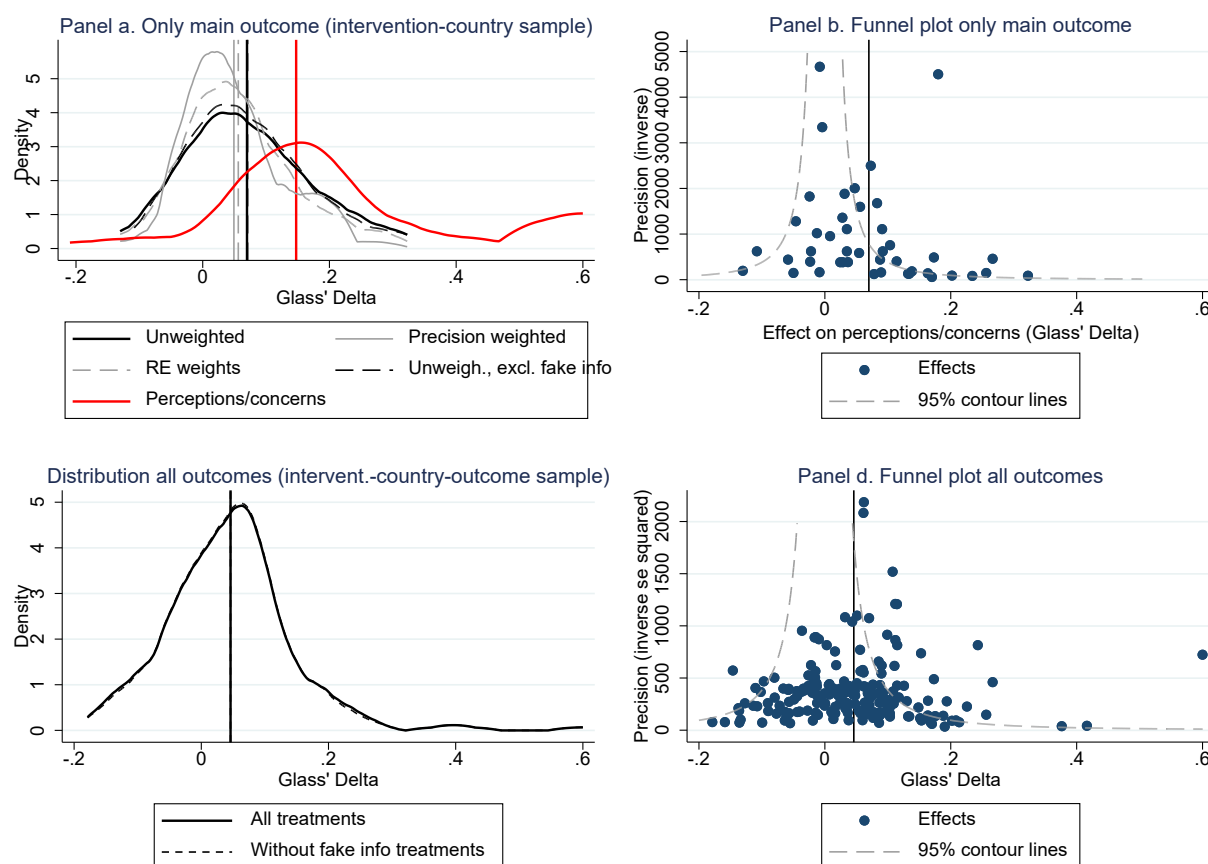
Note: Vertical lines are averages. For panel a and b, n=38 with fake information treatment; n=47 excluding them (only panel a); the distribution of perceptions/concerns includes 5 cases which have been censored at 0.6 and refers only to interventions where also the effects on preferences for redistributions are collected (so it differs from Figure 4.2). Precision weights are the inverse of the variance of the estimate ($1/\hat{\sigma}_j^2$); random effects weights account also for between studies variance ($1/(\hat{\sigma}_j^2 + \hat{\tau}^2)$). In panels c and d, observations are not weighted and only non averaged outcomes are included, n=86 with all treatments, n=116 excluding those with fake information (only panel c). The densities are estimated using an Epanechnikov kernel and the Silverman's rule of thumb as bandwidth. The 95% c.i. lines in panels b and d represent the relation implied by a t-statistic equal to 1.96 (effect²=1.96²/precision); effects above the lines are statistically significant at the 95% level.

54.   For 36 intervention-country pairs (18 studies), we observe both an effect on perceptions/concerns and on redistributive preferences. The two are positively related and the size of the impact on perceptions/concerns explains around one fourth of the total variation in redistributive preferences (Figure 4.5). However, the estimated linear fit does not have a strong slope (0.155, s.e. 0.099): one standard deviation increase in beliefs/concerns is associated with only one fifth of a standard deviation stronger effect on preferences for redistribution.[12] Below we also estimate this relation accounting for other characteristics of the interventions/outcomes, by means of meta-regressions. This estimate confirms that only part of the increased perceptions/concerns translate into higher demand for redistribution. We refrain from interpreting this as the effect of perceptions on redistribution (which would mean interpreting it as an IV estimate), because information about inequality likely influences other factors, such as beliefs about

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[12] In this sample of intervention-country pairs, 4 are from Ballard-Rosa et al. (2020[29]), in which the effect on perceptions captures the "near identification" of certain thresholds of the distribution. This might complicate the interpretation, but removing these observations has almost no impact on the estimate (0.158, s.e. 0.094).

policy effectiveness and trust in government, which have a strong effect on redistributive preferences (see Section 4.3).

55. The distribution of estimates in Figure 4.5 also suggests that the provision of information likely moves preferences for redistribution not only through increased perceptions or concerns, as some effects are remarkably large even in some cases in which perceptions and concerns do not move by much.

Figure 4.5. The effect on perceptions is only mildly related to the effect on preferences for redistribution
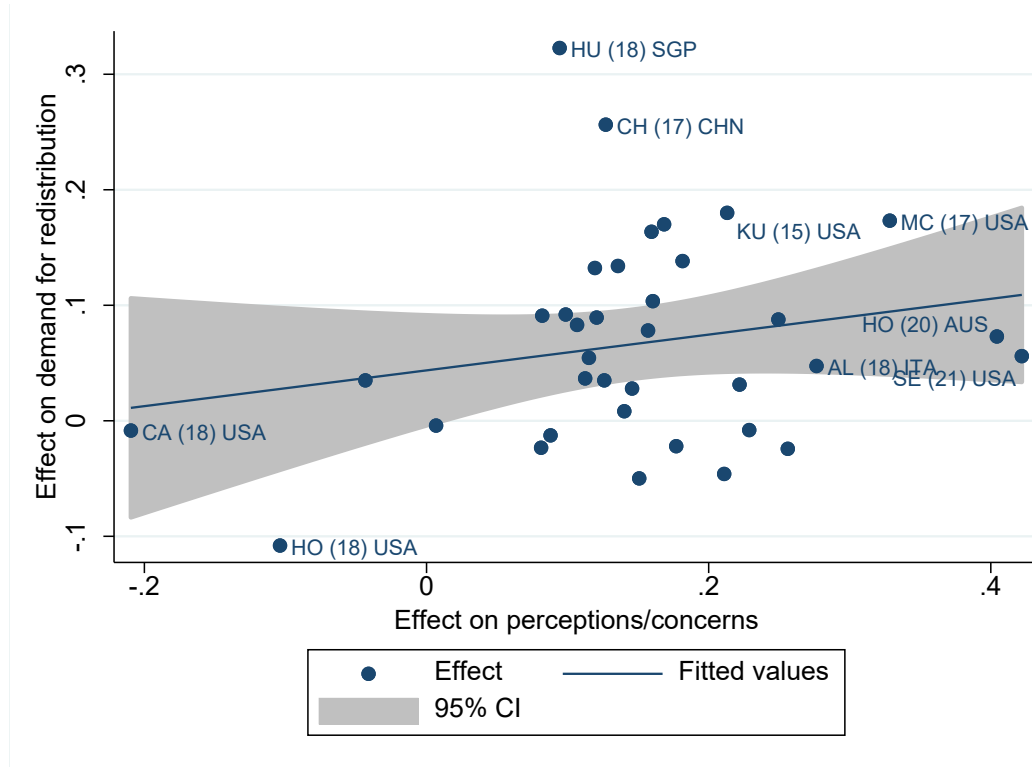
Note: Unweighted.

56. Looking at the expanded dataset (defined by the triplet intervention-country-outcome), in Table 4.3 we take a closer look at the different effect on pure perceptions, concerns and preferences for redistribution. The effect is largest on pure perceptions, milder for concerns, and significantly weaker for redistributive preferences. Differences are all statistically significant and are larger when estimated with precision weights and when considering only at the sample of interventions in which all the three outcomes are collected. Depending on weights and sample, the effect on preferences for redistribution is 12-19% of the effect on pure perceptions, and 28-52% of the effect on concerns.

Table 4.3. Average effects of proving inequality information on participants' perceptions, concerns and preferences for redistribution

	All interventions with demand for redistribution as outcome			Only interventions that collect all three outcomes		
	Preferences for redistribution	Concerns about inequality	Perceptions of inequality	Preferences for redistribution	Concerns about inequality	Perceptions of inequality
Panel a. Unweighted						
Average Glass' Delta	0.046	0.089	0.238	0.037	0.132	0.294
95% confidence interval	[0.022, 0.070]	[0.039,0.138]	[0.167,0.312]	[-0.003, 0.081]	[0.057, 0.214]	[0.162, 0.429]
H_0: different from eff. on pref. for red. (p-val)		0.075	0.000		0.002	0.001
Panel b. Precision-weighted						
Average Glass' Delta	0.050	0.129	0.273	0.054	0.167	0.315
95% confidence interval	[0.017,0.087]	[0.040,0.215]	[0.181,0.379]	[-0.001, 0.1144]	[0.062, 0.307]	[0.176, 0.475]
H_0: different from eff. on pref. for red. (p-val)		0.026	0.000		0.000	0.001
Obs (outcomes)	206	45	39	91	25	20
# clusters	44	25	22	11	11	11

Note: Confidence intervals are calculated clustering the standard errors at the intervention-country level and using a wild-bootstrap procedure (999 replications); see Roodman et al. (2019[35]).

57.   We further inspected the heterogeneity in the estimated effects using meta-regressions. Table 4.4, columns (1)-(3) starts from intervention-level characteristics. Providing textual information and providing both qualitative and quantitative information is associated with stronger effects on preferences for redistribution. The effect in the United States is slightly smaller than elsewhere, with small differences with respect to European countries and larger ones with other countries. There seems to be evidence of a publication bias, as $\beta_\sigma > 0$, although it becomes not significant in precision-weighted estimates.

58.   In columns (4)-(6) we re-estimate the relation between effects on perceptions/concerns and on redistribution, netting out for other characteristics of the intervention. The relation is slightly larger than the univariate regression from Figure 4.5, although the results still confirm that the two effects are not strongly associated.

59.   We then looked at whether the effect depends on the type of outcome, exploiting the expanded intervention-country-outcome sample (Table 4.5). The effect is slightly larger – but the difference is not statistically significant – when the outcome refers to stated preferences for policy intervention than when it refers to the respondents being willing (or choosing) to take an action themselves, like signing a petition. The effect on specific policies (e.g. a progressive tax on income) is, in most cases, weaker than for general preferences for redistribution (e.g. agreement with the statement that the government should reduce income differences), although these differences are not statistically significant apart from the case of subsidies and non-specified tax instruments. However, a weaker effect on subsidies characterizes the United States and the 'other' countries, rather than the European countries. The relation with the (main) effect on perceptions/concerns is now stronger (column 4), possibly because in this expanded sample we give more weight to some specific studies. It is, however, still far from a 1:1 relation, in line with the conclusion of a weaker effect of information about inequality on redistributive preferences than on perceptions and concerns about inequality.

Table 4.4. Differences in the effect of inequality information on participants' preferences for redistribution

Impact of the characteristics of the treatment

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent variable: Glass' Delta effect					
		Unweighted	Precision weight.	RE weighted	Unweighted	Precision weight.	RE weighted
Effect on perceptions (main outcome; Glass' Delta)					0.19	0.27**	0.21*
					(0.14)	(0.13)	(0.12)
Media (reference: Image or video)	Image + text	0.04	0.01	0.04	-0.01	-0.03	-0.01
		(0.05)	(0.05)	(0.06)	(0.04)	(0.04)	(0.05)
	Text	0.08*	0.10**	0.08	0.04	0.04	0.05
		(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)
Reference (ref. national level)	Subnational	0.01	-0.03	0.00	-0.11*	-0.08*	-0.09
		(0.07)	(0.05)	(0.06)	(0.06)	(0.04)	(0.06)
Type of information (ref. Quantitative; info vs no info)	Quantitative; high vs low	0.05	-0.03	0.02	-0.00	-0.10*	-0.04
		.	.	.	(0.06)		(0.08)
	Qualitative; info vs no info	0.05	0.02	0.04	0.05	0.00	0.03
		(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
	Qual. + Quant.; info vs no info	0.06	0.05	0.05	0.07*	0.04	0.06*
		(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Type of inequality displayed (ref.: mobility)	Outcome	0.03	0.02	0.03	0.04	0.05	0.05
		(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Type of information (ref. actual)	Fake	-0.11	-0.04	-0.10			
		.	.	.			
Country (reference: EU)	US	-0.01	-0.02	-0.02	-0.03	-0.03	-0.04
		(0.04)	(0.03)	(0.04)	(0.03)	(0.02)	(0.03)
	Rest of the world	0.06*	0.03	0.04	0.06*	-0.01	0.03
		(0.04)	(0.02)	(0.05)	(0.03)	(0.04)	(0.04)
$\hat{\sigma}_j$		1.88***	0.57	1.52**	1.50**	0.37	1.02*
		(0.61)	(0.83)	(0.70)	(0.55)	(0.66)	(0.61)
Constant		-0.13	-0.03	-0.10	-0.10	-0.03	-0.08
		(0.08)	(0.09)	(0.09)	(0.07)	(0.08)	(0.08)
Number of observations (interventions)		43	43	43	35	35	35
R2		0.42	0.43		0.64	0.64	

Note: * significant at the 10% level; ** 5% level; *** 1% level. Meta-regression on a sample of country-intervention observations, excluding Cansunar (2018[18]); unweighted OLS in columns (1) and (4); weighted by inverse of variance of the estimate in columns (2) and (5) and via the random effects approach (MLE based on normality assumptions) in column (4) and (6). Standard errors in parentheses are heteroscedasticity robust for unweighted OLS and precision-weighted; the standard errors of the RE weighted regressions are based on the normality assumptions.

Table 4.5. Differences in the effect of inequality information on participants' preferences for redistribution

Impact of the characteristics of the outcome

		(1)	(2)	(3)	(4)
		Dependent variable: Glass' Delta effect			
		Unweighted	Unweighted	Precision weight.	Unweighted
Effect on perceptions (main outcome; Glass' Delta)					0.36**
					(0.14)
Individual action or stated preference (ref. ind. action)	Stated pref. for redistr.	0.03	0.03	0.03	0.05
		(0.03)	(0.04)	(0.05)	(0.06)
Type of policy (reference: general redistri. policy)	Regulation (including minimum wage)	-0.02	-0.03	-0.04	-0.04
		(0.04)	(0.05)	(0.03)	(0.03)
	services	-0.02	0.00	-0.03	-0.04
		(0.03)	(0.03)	(0.03)	(0.03)
	subsidies	-0.07**	0.01	-0.02	-0.03*
		(0.03)	(0.04)	(0.02)	(0.02)
	tax on income	-0.03	-0.04	-0.01	-0.01
		(0.02)	(0.03)	(0.01)	(0.01)
	inheritance or wealth	-0.01	-0.06	0.09	0.09
		(0.06)	(0.04)	(0.12)	(0.12)
	tax not specified	-0.09**	-0.15**	-0.03	-0.06***
		(0.04)	(0.07)	(0.02)	(0.02)
Policy on outcome or opportunities (ref: opp.)	Policy on outcomes	0.04	0.04	-0.01	0.00
		(0.03)	(0.03)	(0.03)	(0.03)
Interaction between country and type of policy	US X services		-0.09*		
			(0.05)		
	US X subsidies		-0.09		
			(0.06)		
	US X tax on income		0.01		
			(0.03)		
	US X tax on wealth/inheritance		0.12		
			(0.16)		
	US X tax not specified		0.08		
			(0.10)		
	other countries X services		0.02		
			.		
	other countries X subsidies		-0.17**		
			(0.08)		
	other countries X tax on income		-0.02		
			(0.07)		
	other c. X tax on wealth/inheritance		0.36		
			.		
$\hat{\sigma}_i$		0.47	0.27	-0.53	-0.15
		(0.62)	(0.53)	(0.59)	(0.55)
Number of observations (outcomes)		204	204	204	188
Number of clusters (interventions)		43	43	43	35
R2		0.19	0.27	0.19	0.25

Note: * significant at the 10% level; ** 5% level; *** 1% level. Meta-regression on a sample of country-intervention observations, excluding Cansunar (2018[18]); unweighted OLS in columns (1), (2) and (4); weighted by inverse of variance of the estimate in column (3). Standard errors in parentheses are clustered at the intervention-country level. Only treatments about inequality. Only outcomes referring to preferences for redistribution, excluding summary indices. All the regressions include also the characteristics of the intervention from Table 4.4.

4.3. Possible explanations for the limited effect of information on redistributive preferences

60.　　The smaller effect of informational treatments on participants' preferences for redistribution supports the hypothesis that people fail to connect inequality with policy action (Bartels, 2005[7]). Kuziemko et al. (2015[36]) provide a separate treatment which, after providing information on inequality, also highlights how the different policies could benefit people on low income. For instance, it refers to the income of households benefitting from minimum income or food stamps. The inclusion of this information, in line with Bartel's (2005[7]) hypothesis, increases the effect of the treatment on preferences for redistribution. The increase is even larger taking into account that this "policy" treatment has smaller effects on perceptions/concerns.[13] Nevertheless, not all the experimental evidence agrees. Lergetporer, Werner and Woessmann (2020[37]), studying the impact of educational inequalities, also provide some of the treated participants with information linking the proposed policies with educational inequalities. They find that this additional "connecting" information does not rise the effect.

61.　　The lack of connection can also be explained by heterogeneous preferences with regard to specific policies. People might agree that *some policy* action is needed to reduce inequality, but do not agree on specific actions. This interpretation is consistent with our main result that the average effect is small, particularly when the experiments focus on several different interventions. This interpretation is consistent with the finding from the meta-regressions that effects on demand for redistribution tend to be smaller when respondents are asked about specific policies. Some experiments point in this direction. Settele (2021[21]) exposes respondents to two different estimates of the gender gap in the United States, one larger (females earn 74 percent with respect to males) and the other smaller (94 percent). She finds that the treatment has very large effects on perceptions, as well as on a general demand for government intervention (Glass' Delta 0.243, s.e. 0.035), but a relatively small effect on specific policies. The index averaging across different policies has an effect of 0.056, s.e. 0.025. The minimum effect is -0.015 on a rule requiring more wage transparency, and 0.115 on a stricter equal pay legislation. Zilinsky (2014[23]) shows that respondents receiving information about the extent of inequality raise their demand for redistribution in terms of general preferences, but do not change their opinions about tax policy.

62.　　People might also fail to link inequality with policy action because they lack an emotional connection with the potential beneficiaries of the policy. In principle, people might be more responsive to emotional treatments that help them empathize with the beneficiaries. The evidence on this is mixed and points to multiple directions. Kuziemko et al. (2015[36]) designed a specific informational treatment aimed at increasing respondents' empathy towards poor people, by showing how little households similar to theirs earn. This treatment, however, led to even smaller effects on preferences for redistribution. Hubers (2018[16]) finds that showing a video about a poor man's daily life in Singapore has strong effects on demand for redistribution, even if it does not raise concerns with inequality. Sands (2017[38]) shows that exposure to real-world poverty might reduce the rich's demand for redistribution. Passers-by in affluent US neighbourhoods were asked to sign a petition for a million-dollar tax, after being (inadvertently) exposed to a professional actor. When the actor was portraying a poor person, passers-by were less willing to sign the petition (contrary to the pre-registered hypothesis).

63.　　A different, though related hypothesis, is based on the heterogeneity in people's beliefs about the "feasible and legitimate role of government" (Bechert and Osberg, 2016, p. 1[8]). People might believe that there is limited scope for policies to change the current degree of inequality, or they may distrust the

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[13] In Figure 4.1 and Figure 4.3 this treatment is the "Kuziemko et al. (2015) 3" (Glass' Delta 0.11 and 0.08 on perceptions/concerns and preferences for redistribution, respectively). The informational content of the treatment was for the rest similar to the "emotional" treatment described in the next paragraph, and appearing as "Kuziemko et al. (2015) 2" in the pictures (Glass' Delta 0.15 and 0.03 on perceptions/concerns and preferences for redistribution, respectively).

government. Kuziemko et al. (2015[36]) show that providing information about policy effectiveness increases the effect of the informational treatment on redistributive preferences. Lergetporer, Werner and Woessmann (2020[37]) confirm the result, thought they show that the information on effectiveness does not increase the effect of providing inequality information but rather works as a separate channel. Settele (2021[39]) shows that giving people the facts about the wide gender wage gap has only a limited impact on demand for concrete policies to reduce that gap, because a sizeable share of respondents believe that such policies are ineffective. The importance of beliefs about the costs of policies is also underlined in other studies. Hayes and Guay (2020[40]) show that providing information about the costs of inequality-reducing policies reduces support, while paradoxically providing information about the benefits has no effect. The effect of information about costs is similar whether the costs are general or are specifically borne by the respondent. Mishagina and Montmarquette (2018[41]) show that providing respondents information about the costs of minimum wage reduces support for it.

64.   Kuziemko et al. (2015[36]) also point out the relevance of trust in government. Their results show that the inequality information treatment reduces trust in government. Using a different experiment in which they prime respondents to be less confident in government, they also show that reducing trust has an independent negative effect on demand for redistribution. Conversely, Lergetporer, Werner and Woessmann (2020[37]) argue that reduced trust does not explain the limited effect of the inequality information treatment on preferences for redistribution, because the effect is driven by those groups that trust the government less.

65.   Another possible channel for explaining the limited effect of information treatment on redistributive preferences is that people might increase their tolerance for inequality (or justify it). Trump (2018[24]) finds that providing information about the high level of inequality also raises the level of disparities that people deem as fair. Campos-Vazquez et al. (2020[42]) use a similar question in Mexico, but find opposite results, i.e. that providing respondents with information on the actual extent of income inequality, or about the level of intergenerational mobility, does not affect people's *desired* level of inequality and intergenerational mobility. Overall, this is still a largely unexplored avenue in this stream of research, as most studies do not collect information on the respondents' *preferred* level of inequality.

5. The effect of information about own position in the distribution on redistributive preferences

66.   Empirical evidence based on surveys supports the hypothesis that the individual position along the income distribution matters for participants' redistributive preferences: the rich, who lose from redistribution, are less likely to agree that the government should intervene to reduce income disparities (see Rueda and Stegmueller (2019[12]), for a more estensive discussion and summary of the literature).

67.   People, however, tend to misperceive their personal position in the distribution. Most people, in fact, believe that they belong to the middle class (OECD, 2019[5]). This raises the question whether people would become more supportive of redistribution if they learned their true position in the income distribution.

68.   Informational treatments assess this by providing a randomly selected subset of respondents with explicit information about their true position along the income distribution. One could use this information to ask two questions:

- Were all people correctly informed of their true position, would demand for redistribution grow or fall?
- Do respondents who overestimate their position react to the information by increasing their demand for redistribution? Does the opposite happen for people who underestimate their position?

69.   Figure 5.1 shows that the effect is, on the aggregate, very close to zero. The effects are noisily estimated. There seems to be heterogeneity, but given the noisiness of most estimates the between-study variance amounts to only 38%. The 95% confidence interval average, as estimated by the random-effects method, which is the most suited in the case of such a large variance across studies, is quite small around zero. However, the overall pattern might just be the result of heterogeneous responses by individuals with a positive (overestimate) or negative (underestimate) bias: in other words, if participants correctly revise their expectations, the two margins could compensate each other.

Figure 5.1. Effects of providing information about the respondents' position in the income distribution on participants' preferences for redistribution
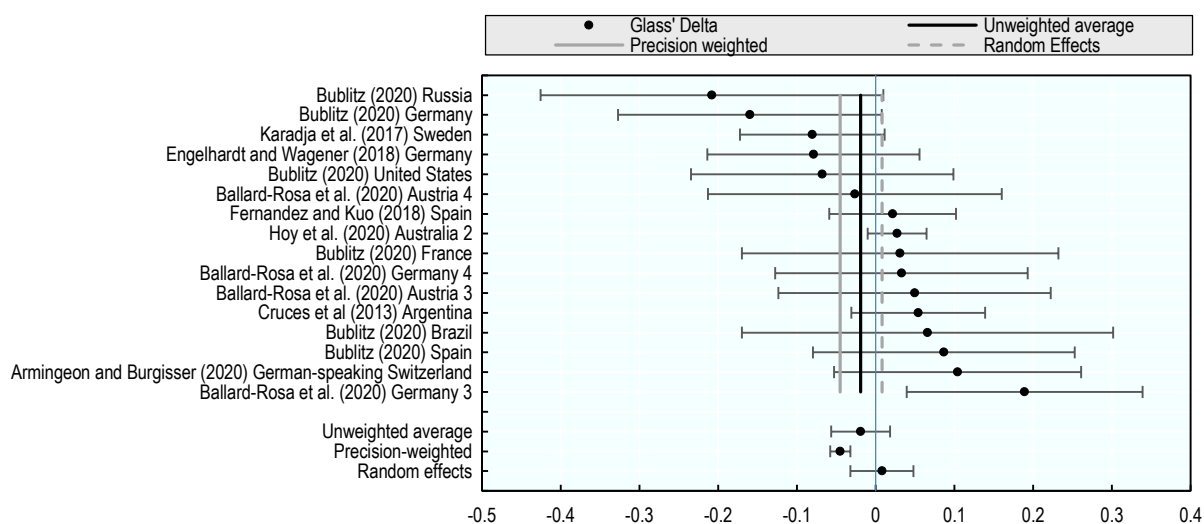
70.   Most of the studies do not report heterogeneous results depending on prior bias. For those that do, we find that results are consistent with the hypothesis above, i.e. that people who realise that they overestimate their position increase their demand for redistribution (Figure 5.2), and the other way round for those who underestimate it. The average effects are, however, very small. The first – for those who overestimate – is quite precisely estimated once we use precision weights or the RE methods, also because the between study heterogeneity is quite low. The latter – for those who underestimate – is statistically significant only using precision weights (at the 5% level) or using the RE method (at the 10% level) and displays more between-study heterogeneity. The meta-analysis helps in discriminating between zero results and small effects: in fact, most studies find insignificant effects close to zero (apart from Cruces, Perez-Truglia and Tetaz (2013[43])); only when looking at the average across studies we are able to discriminate between statistical insignificance and small effects.

Figure 5.2. Effects of providing information about the respondents' position in the income distribution on participants' preferences for redistribution by prior bias
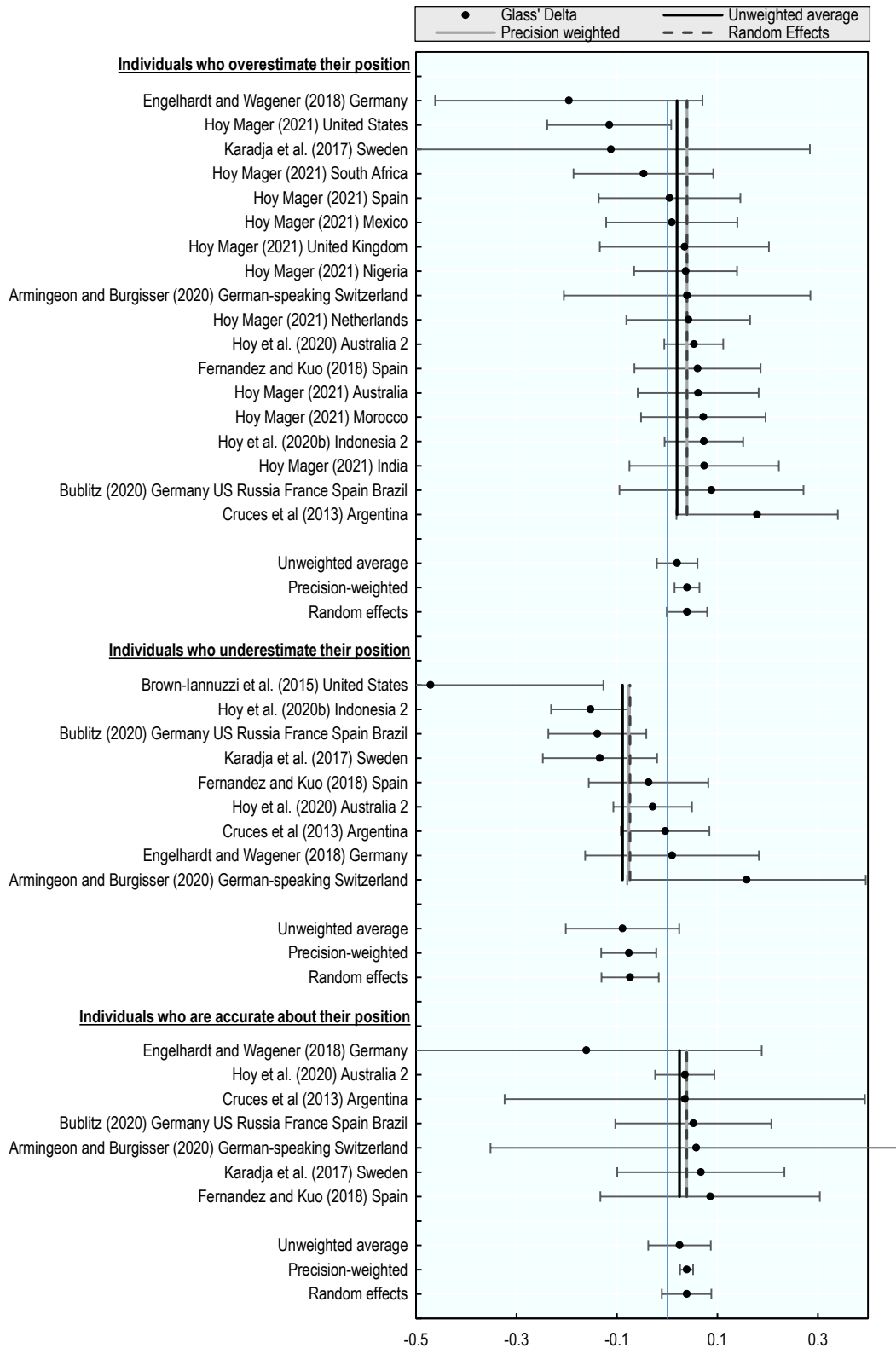
71.   For the poor, Hoy and Mager (2021[44]) suggest that the effect on redistributive preferences might be driven by respondents using their income as a reference. Before the information, they believe their income is a benchmark for the middle class. After discovering that they are actually poor, they also realise that there are fewer poor people than they expected. In fact, Hoy and Mager (2021[44]) show that in most of the countries of their study, poor people who overestimate their position in the income distribution reduce their concerns with inequality when they are provided with information. However, their preferences for redistribution do not change, a result closer to the one from inequality information treatments discussed in Section 4.3.

6. Discussion and conclusions

72.   Overall, the results of our review lend support to the conclusion that people correctly elaborate the information received, and that this affects not only their beliefs but also their concerns with the extent of economic disparities. This result is in line with evidence from observational studies, based on survey data across time (OECD, 2021[1]). Obviously, it does not tell us whether people, in the absence of an informational treatment, would ever get correct information about inequality in their countries. However, it suggests that providing information can successfully affect beliefs and concerns in the expected direction.

73.   However, our results also show that preferences for redistribution are much less reactive to information about the overall level of inequality or social mobility. Also information on the own income position has small effects on demand for redistribution, although in different directions, depending on whether they over- or under-estimate their actual position in the income distribution.

74.   We do not find much evidence of publication bias in this stream of literature. There appear to be no systematic selection of results with regards to the size and direction of effects among published studies. There seems to be some minor degree of publication bias when it comes to the effect on preferences for redistribution, as less precise estimates seem to be more likely to be positive and larger. However, this does not undermine our main conclusion, i.e. that the effect on demand for redistribution is quite small, with few exceptions.

75.   The meta-analysis highlights large heterogeneity across studies. This is not only due to differences in the setting and in how the information is communicated, but also to a very large heterogeneity in the types of outcomes collected. On the one hand, it is clear that it is hard to reach an agreement among researchers, as there is no consensus on how to measure people's perceptions of inequality (Jachimowicz et al., 2020[45]) or their concerns or redistributive preferences. On the other hand, some practices might facilitate comparison with other experiments as well as with observations data:

- International surveys, such as the Social Inequality module of the International Social Survey Programme or the World Values Survey, provide long-standing and agreed formulation of different questions on these issues. Although they are not necessarily the best measures, they can provide a benchmark and facilitate the comparison with observational evidence as well.

- Given the wide variety of outcomes, standardizing them by the control group standard deviation – or at least providing the necessary information to do so – is key to allow comparability. Similarly, as studies differ in terms of the number of outcomes collected, it is crucial to calculate indices that average many of them. In this respect, the literature seems to agree on standardizing – in the microdata – each outcome by the control group standard deviation and then take the simple average.

- More clarity in the meaning of different outcomes would facilitate comparisons. In particular, it would be useful to separate, even in the construction of indices, the following dimensions: *perceived* inequality (what people think the level of inequality is); *preferences* (what people think the level of inequality should be); *concern* (to which extent people think inequality is larger than it should be); and *demand for policy intervention* (what people think the government should do).

76.　　The paper also highlights different aspects on which evidence is still missing. First of all, the evidence is sparse, and not always conclusive, about the mechanisms that prevent information about inequality to lead to a stronger demand for redistribution:

- People's beliefs about the effectiveness of policies seem to matter. However, it is not clear whether this is a matter of baseline beliefs (results from Settele (2021[21]) suggest so, but those from Lergetporer et al. (2020[37]) do not) nor whether providing information on policy effectiveness increases the effect of inequality information (as suggested by Kuziemko et al. (2015[36])) or whether it rather has a direct effect which operates independently from inequality information (Lergetporer, Werner and Woessmann, 2020[37]).

- Related to policy effectiveness beliefs, Pellicer et al. (2019[25]) argue that one of the reasons behind the scarce elasticity of redistributive preferences is that people think inequality is inevitable, but more evidence is needed.

- Few papers try to examine whether the information influences *preferred* inequality (Trump, 2018[24]; Engelhardt and Wagener, 2018[46]; Campos-Vazquez et al., 2020[42]). If people adapt their preferences to the higher level of inequality usually associated with the information, then this might explain the scarce elasticity.

77.　　Second, after receiving the information, people may agree on the need to take some policy action, but disagree on the specific action. This is consistent with findings of average small effects across multiple policies. One way to look for this is to compare preferences for general intervention and for specific policies (as in Settele (2021[21])). Another possibility would be to look at the effect on agreement to *at least* one policy action, or to fully inspect the heterogeneity of the effects (e.g. via causal forest methods) to capture groups that raise preferences for different specific policies.

78.　　Few studies elicit both prior and posterior beliefs, so little is known about learning rates (for exceptions, see Becker (2020[28]); Settele (2021[21]); Lergetporer et al. (2020[37])). Furthermore, none of the studies tries to assess whether the information changes the *salience* of inequality vis-à-vis other societal issues (such as crime or immigration). Salience is often measured in surveys (Pontusson et al., 2020[47]) and could be thought of as an outcome.

79.　　Finally, few studies try to reconstruct how people interpret the information provided, going past the fact that they might adjust their own numerical estimates. Researchers should examine what it means to provide "true" information. For instance, when providing individuals' "true" income position along the income distribution, we should recognize that the metric used to estimate it (e.g. disposable equivalised income) and the reference group used by participants for that assessment (e.g. the country or the region) may not correspond to what participants consider. From the side of the respondent, adjusting numerical estimate does not necessarily imply that they change their view of the society. It would be important to learn more about how people interpret and read the information provided. One solution, following Stantcheva (2020[48]), would be to make more use of open ended questions and text analysis. Another, coming from Sociology, would be to use focus groups to understand how people – collectively or autonomously – read and interpret the information (Summers et al., 2021[49]).

Annex A. Additional table

Table A A.1. Keywords used in the search for in-survey experiments

Academic search engine	Keywords
Google Scholar	"information treatment survey inequality"; "information treatment preference redistribution"; "information on preferences and redistribution experiment", "experiment perception preference for redistribution", "experiment information inequalities preference redistribution", "experiment information redistribution", "experiment information position redistribution", "experiment information gini redistribution", "experiment perception inequality", "experiment belief inequality", "experiment preference redistribution"; ""information preference redistribution experiment".
Mendeley	"experiment perception inequality", "experiment belief inequality"; "experiment preference redistribution"; "information treatment survey inequality"; "information treatment preference redistribution"
Ideas/repec	"experiment preference redistribution" "experiment belief inequality" "experiment perception inequality", "information treatment survey inequality"; "information treatment preference redistribution
SSRN	"experiment preference redistribution" "experiment belief inequality" "experiment perception inequality", "information treatment survey inequality"; "information treatment preference redistribution"
Social Science Registry	"inequality perception" "redistributive preferences" "redistributive" "redistribution" "belief inequality";

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