Perceptions of own social class and local affluence: Effects on preferences for redistribution*

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Abstract

This paper studies how perceptions of own social class and local affluence shape preferences for redistribution. We conduct a survey experiment with 1,293 respondents in Lima Metropolitana, randomly correcting beliefs either about respondents' own socio-economic status (SES) under Peru's official five-class classification, or about the share of affluent households in their district. Misperceptions are strongly asymmetric: about 70% of respondents overestimate their own SES and about 50% overestimate the local share of affluent households. The effects of information, however, are not uniform. The clearest evidence comes from respondents who substantially overestimate local affluence: correcting this belief increases support for redistribution. For own SES, effects are concentrated among respondents who overestimate their position by at least two classes (34% of the relevant sample); among respondents whose misperceptions are smaller, the evidence is weaker. Neither treatment shifts support for a wealth tax, and perceived fairness of the income distribution is largely unaffected. Using geocoded data, we further show that local SES and local affluence are positively associated with respondents' perceptions, while local inequality itself is only weakly related to redistributive preferences in the control group. The results suggest that corrective information can affect redistributive attitudes when perceived-actual gaps are large, but that these effects depend on the type and magnitude of misperceptions.

Key words: Preferences for redistribution, inequality perceptions, beliefs, wealth taxes, Peru

JEL-classification: H24, D31, D63, E62, H53

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1 Introduction

How people form their preferences for redistribution is a central question in political economy. In the canonical framework of Meltzer and Richard (1981), preferences follow from self-interest given the income distribution. A substantial body of evidence shows, however, that they also depend on beliefs about that distribution: on perceptions of own income position (Cruces et al., 2013; Karadja et al., 2017; Hauser and Norton, 2017; Gimpelson and Treisman, 2018; Hvidberg et al., 2023), on beliefs about the origins of inequality (Alesina and Angeletos, 2005; Cappelen et al., 2013, 2020), and on misperceptions about the design of redistributive instruments (Kuziemko et al., 2015; Sides, 2016; Stantcheva, 2021). A growing literature corrects such beliefs experimentally and traces the resulting shifts in policy support (Cruces et al., 2013; Karadja et al., 2017; Bublitz, 2022; Bussolo and Dixit, 2023; Hvidberg et al., 2023; Hoy et al., 2024a,b).

Most of this evidence comes from settings where respondents underestimate their own income position, or where perceptual biases are roughly mean-zero around the truth. Less is known about contexts in which many individuals misperceive upward, where inequality is experienced through strong residential segregation, and where social position is commonly expressed through broad socio-economic groups rather than income percentiles. We provide evidence on these margins from Metropolitan Lima, the largest urban area of Peru and a setting where socio-economic groups are widely used in political polling, market research, and public debate.

We implement an information-provision experiment with 1,293 respondents in Metropolitan Lima between October and November 2021. At baseline, respondents are asked to locate their household in one of the five socio-economic groups used by Peru's National Institute of Statistics and Informatics (INEI): Upper, Upper-middle, Middle, Lower-middle, and Low. These categories are income-based in INEI's official classification, but they are also familiar and socially salient in the Peruvian context. Respondents are also asked to estimate the share of affluent households, defined as upper-middle and upper-class households, living in their district. They are then randomly assigned to one of three arms. In the control group (C), no further information is provided. In the first treatment arm (T1), respondents see their self-assessed SES next to the SES implied by their household income under INEI's official classification. In the second treatment arm (T2), respondents see their estimate of the district share of affluent households next to the actual share. Post-treatment outcomes include preferences for redistribution, support for state action to reduce income inequality, support for a wealth tax on assets above one million Peruvian soles, support for reducing inequality of opportunity, and a composite index of these four outcomes; we also elicit perceived fairness of the income distribution. For 937 respondents, corresponding to 72 percent of the analytical sample, we obtain valid geographical coordinates of the dwelling, which allows us to link each respondent to inequality measures computed within buffer zones of 300, 500, and 1,000 metres around the household.

A first result is descriptive: misperceptions in Lima are strongly one-sided. About 70% of respondents over-estimate their own SES relative to INEI's classification, while only 13% under-estimate it. For the share of affluent households in the respondent's district, 50% over-estimate and 34% under-estimate. This pattern differs from the centre bias that Hvidberg et al. (2023) document in Danish administrative data, where lower-ranked individuals over-estimate, higher-ranked individuals under-estimate, and the bias is roughly mean-zero in expectation. It also differs from the under-estimation documented for Argentina by Cruces et al. (2013), for Sweden by Karadja et al. (2017), and for a cross-section of low- and middle-income economies by Bussolo and Dixit (2023). The closest antecedent is the over-estimation that Fernandez-Albertos and Kuo (2015) report for Spain, although the magnitude we document is larger and arises in a different context: a high-inequality Latin American metropolis.

The experiment shows that corrective information raises demand for redistribution mainly among respondents who over-estimate either their own SES or the local share of affluent households. The SES treatment increases support for reducing income inequality and inequality of opportunity when the correction is large, namely when respondents learn that their household is at least two SES levels lower than they believed. The local-affluence treatment produces a stronger and more robust response: among respondents who over-estimate the district share of affluent households by at least ten percentage points, information increases preferences for redistribution and raises the composite index of redistributive preferences. By contrast, neither treatment shifts support for the proposed wealth tax.

The pattern of results is consistent with a self-interest channel. The treatments change what respondents want the state to do, but they do not change whether respondents perceive the income distribution as fair. This precisely estimated null on perceived fairness suggests that respondents are not revising their normative assessment of the distribution; rather, positively biased respondents update their policy preferences when they learn that their own position, or the affluence of their local environment, is lower than they believed. Heterogeneity by prior beliefs points in the same direction. Treatment effects are concentrated among respondents whose pre-treatment views are typically associated with weaker support for redistribution, such as right-leaning respondents, individuals with low trust in government, those with more individualistic views, and those who attribute income differences to effort.

Recent studies show that local socio-economic composition can shape inequality perceptions and redistributive attitudes.[1] Our geocoded data allow us to examine this local dimension directly. Building on Domènech-Arumí (2025) and Hoy et al. (2024a), we link each geocoded respondent to buffer-zone inequality measures constructed from INEI's block-level SES classification: the local share of affluent

[1] For instance, Minkoff and Lyons (2019) link local income diversity to perceived income gaps and support for redistribution; Hoy et al. (2024a) show stronger information effects among right-leaning individuals in high-inequality areas; while Domènech-Arumí (2025) finds that local inequality in Barcelona predicts perceived inequality but not redistributive preferences.

households, the coefficient of variation of household per-capita income, and the Generalised Entropy index. In the control group, we reproduce the central pattern documented by Domènech-Arumí (2025) for Barcelona: local inequality predicts perceived inequality and the magnitude of perceptual bias, but is only weakly associated with redistributive preferences. Conditional on a positive bias, however, local inequality matters for treatment responses. Correcting over-estimation of local affluence raises support for redistribution more clearly among respondents living in higher-inequality areas. Thus, local inequality appears less important as an unconditional predictor of redistributive preferences than as a context that shapes how individuals respond to corrective information.

The paper contributes to the literature in three ways. First, it studies misperceptions using income-based SES groups rather than income deciles or percentiles. The SES treatment should not be interpreted as separately identifying the role of education, occupation, or other non-income dimensions of status. Nevertheless, the distinction is substantively important. Social class identification is not only an economic ranking but also a social self-assessment that may incorporate education, occupation, consumption patterns, and other status cues. In Peru, socio-economic groups are also familiar categories in public opinion and market research, where households are commonly classified into NSE groups using information on income-related conditions, education, housing characteristics, services, and household assets (APEIM, 2021). The gap between perceived class and INEI's income-based official classification is therefore informative about how individuals map their lived social position onto a familiar class scheme.

Second, the paper introduces a local-affluence information treatment. Existing experiments have mainly corrected beliefs about own position in the income distribution or about national inequality. Our second treatment instead corrects beliefs about the share of affluent households in the respondent's own district. This is a more local object of perception and one that is likely to be especially relevant in a segregated city such as Lima, where daily social comparison is shaped by neighbourhood composition and visible differences across urban space.

Third, the paper combines experimental information provision with geocoded measures of local inequality. This allows us to connect the reference-group dimension emphasised by Hvidberg et al. (2023) with the local-inequality dimension studied by Domènech-Arumí (2025) and Hoy et al. (2024a). In doing so, we show that local inequality is not simply a background condition: it is related to perceptual bias and conditions the effect of information about local affluence. The combination of upward SES misperceptions, a local-affluence treatment, and buffer-zone inequality measures is, to our knowledge, new in this literature.

Section 2 describes the data, the experimental design, and the empirical strategy, including the geocoding procedure, the construction of local-inequality measures, the approach to multiple-hypothesis testing, and the hypotheses guiding the analysis. Section 3 presents the baseline bias patterns, the average treatment effects, and heterogeneous responses by political and ideological priors. Section 4 discusses mechanisms, Section 5 examines the role of local inequality, and Section 6 concludes.

2 Data and empirical strategy

2.1 The online survey

We designed an online survey to examine how perceived social class and local inequality shape individual attitudes toward economic redistribution. The survey was implemented by Pulso-PUCP in Metropolitan Lima between October and November 2021.[2] The study focuses on the Lima Metropolitan Area (Lima Metropolitana), which comprises the 43 districts of Lima Province and the 7 districts of the adjacent Callao Province. It is the largest metropolitan region in Peru, with 11.2 million inhabitants out of a national population of 33.4 million. Its districts are commonly grouped into five subregions according to location: Northern Lima, Southern Lima, Eastern Lima, Central Lima, and Callao.

Lima Metropolitana is a particularly useful setting for studying perceived social class and local affluence because inequality is not only high but also strongly spatial. The city combines affluent districts with large lower- and lower-middle-income areas, and the five INEI socio-economic groups are visibly clustered across the urban space.[3] This structure makes local reference groups especially salient: a household classified as low or lower-middle according to the official income-based classification may still compare itself with neighbours in a similarly disadvantaged area and therefore perceive itself as closer to the middle of the local distribution. At the same time, daily mobility across the city exposes residents to sharp contrasts in housing quality, services, consumption, and public infrastructure, which may shape perceptions of both own class and the affluence of nearby districts. Lima therefore differs from the mostly high-income-country settings studied in the existing literature, as well as from experiments based on national income percentiles or deciles. In this context, upward bias in perceived SES may reflect not only imperfect knowledge of the income distribution, but also the interaction between residential segregation, local comparison groups, and the socially salient nature of broad socio-economic categories.

The survey targeted Peruvian citizens residing in Metropolitan Lima aged between 18 and 64. Pulso-PUCP recruited participants under quota sampling designed to match the population distribution of Metropolitan Lima by district, sex, and age group (18–29, 30–44, and 45–64), with INEI population

[2] Pulso-PUCP is a spin-off company of the Pontificia Universidad Católica del Perú (PUCP) dedicated to conducting market and opinion studies as well as strategic data analysis.

[3] A well-known illustration of this socio-spatial divide is Lima's so-called "Muro de la Vergüenza", which separates affluent residential areas from poorer settlements and has become a symbol of urban segregation in the city (Campoamor, 2019). More broadly, studies of Lima document sharp inequalities in urban infrastructure, public investment, and neighbourhood conditions across the metropolitan area (Wiese et al., 2016; Espinoza and Fort, 2017).

projections providing the sampling frame. The frame covered residents aged 18 to 64 in 2021 across 35 districts; fifteen smaller or more remote districts (seaside or near-mountain areas) were excluded, together about 5.6% of the metropolitan population.

The analytical sample consists of 1,293 individuals.[4] As Table A–1 shows, the sample is well balanced across the five subregions of Metropolitan Lima and broadly representative at the district level, although some districts are slightly over- or under-represented. Table A–2 reports baseline balance across treatment arms. Covariates are well balanced across arms, with no significant difference between the control group and the affluent-share arm; the few weak differences elsewhere are consistent with chance given the number of balance tests. We nevertheless include these variables, together with other pre-treatment controls, in our main robustness specifications. The results are qualitatively unchanged by their inclusion (Appendix Tables G–5 and G–6).

The first block of the survey collected socio-demographic information. Reported household income and household size were later used to construct household income per capita, which determined the personalised SES feedback shown to respondents in the SES-treatment arm. The questionnaire also measured baseline beliefs and attitudes, including views on fairness, trust in the government's capacity to manage and redistribute public funds, perceptions of individual versus governmental responsibility for supporting others, political ideology, and risk preferences.

Two additional baseline questions capture the perceptions manipulated in the experiment. For own status, respondents were asked: "The National Institute of Statistics (INEI) classifies households in Lima into five levels according to their income. Which group do you think your household belongs to?", with the response options Upper, Upper-middle, Middle, Lower-middle, and Low. For local affluence, respondents used a slider to report the percentage of households in their district that they believed belonged to the Upper or Upper-middle classes.[5]

Respondents were then randomly assigned to one of three groups: the control group (C), the SES-treatment group (T1), or the affluent-share-treatment group (T2). Pre-treatment questions and outcome measures were identical across all groups; the arms differed only in whether respondents received corrective information about one of the two baseline perceptions (Table 1). In T1, respondents were shown

[4] The full ladder of sample exclusions is as follows. Pulso-PUCP recorded 2,364 raw responses; 134 were excluded by the survey firm's field-quality validation, leaving 2,230. Of these, 911 belonged to two arms of an unrelated experimental block and are dropped, leaving 1,319 respondents in the redistribution arm. We further trim 26 interviews with durations outside the 1st and 99th percentiles of the sample duration distribution, leaving the analytical sample of 1,293. The SES-treatment regressions (Table 3) use the control group and the T1 subsample ($N = 822$); the affluent-share-treatment regressions (Table 4) use the control group and the T2 subsample ($N = 867$). Adding the covariates in the appendix robustness specifications reduces the sample to $N = 793$ for T1 and $N = 833$ for T2 through listwise deletion (Appendix Tables G–5 and G–6); common-sample replications of the no-covariate specifications, restricted to respondents with complete information on all outcomes ($N = 810$ for T1 and $N = 849$ for T2), are reported in Appendix Tables G–10 and G–11.

[5] INEI (2021) provides maps and estimates of the SES distribution for all districts and blocks of Metropolitan Lima, based on data from the 2017 National Census and large-scale household surveys conducted between 2017 and 2020. INEI first estimates per-capita income for sampled households within each block, and then computes the SES distribution at block and district levels using income thresholds specific to Metropolitan Lima.

their self-assessed SES next to the SES implied by their household income per capita under INEI's official classification, and were told whether their household's official SES was higher, lower, or the same as the group they had reported. In T2, respondents were shown their estimate of the district share of affluent households next to the actual district share from INEI statistics, and were told whether the true share was higher, lower, or about the same as they had believed. The control group received no corrective information.

Two features of the SES measure are important for interpretation. First, the official classification used to define actual SES is income-based: it is obtained by combining respondents' reported household income and household size, and then applying INEI's income thresholds for Metropolitan Lima. The treatment therefore does not separately identify the role of education, occupation, or other non-income dimensions of socio-economic status. Second, respondents' self-assessed SES may nevertheless reflect a broader understanding of social class, including income, occupation, education, consumption patterns, neighbourhood, and aspirational identity. The gap between perceived class and the official classification is therefore informative about how respondents map their lived social position onto a familiar SES scheme. We return to this distinction when interpreting the SES-treatment effects in Section 3.2.

Table 1: Description of treatments

Group	**Treatment description**	**Sample size**
Baseline	1. "The National Institute of Statistics (INEI) classifies the households in Lima into 5 levels according to their incomes. Choose the group you think your household belongs to: *<Upper>, <Upper-Middle>, <Middle>, <Lower-Middle>, <Low>*." 2. "In the district where you live, what percentage of households do you think are Upper or Upper-Middle level? Please slide the bar until you reach the percentage you think is appropriate."	1,293
T1	"According to your income and according to studies by the National Institute of Statistics, your household would belong to level **L**, and you said it was level **M**. That is, the level of your household is *<higher>* *<lower>* *<the same>* than you thought."	426
T2	"According to studies performed by the National Institute of Statistics, in your district there are **R**% of Upper and Upper-Middle households, and you said there were **S**%. That is, there is *<a higher>* *<a lower>* *<about the same>* percentage of affluent households in your district than you thought."	471
C	The control group does not receive any bias correction	396

Notes: All individuals answered the baseline questions, while respondents were randomly assigned to the control and treatment groups (C=396, T1=426, T2=471).

2.2 Variables of interest

We measure four post-treatment outcomes capturing preferences for redistribution and combine them into a summary index.

- *Preferences for redistribution*. Respondents place themselves on a 1-to-10 scale between two opposing statements: 1 means strong agreement that incomes should be made more equal, while 10 means strong agreement that larger income differences are necessary to reward individual effort. This question is widely used in the World Values Survey (WVS) and the European Values Survey (EVS). We reverse-code and rescale the variable so that higher values indicate stronger preferences for redistribution.
- *Reduce income inequality*. Respondents state how much they agree with the statement: "The state must implement strong policies to reduce income inequality between the rich and the poor." Answers are reported on a 4-point Likert scale: strongly disagree (1), disagree (2), agree (3), and strongly agree (4).
- *Wealth tax*. Respondents state how much they agree with the statement: "The state should levy a tax on wealth exceeding one million Soles", approximately USD 260,000 at the time of the survey. Answers are reported on the same 4-point Likert scale.
- *Reduce inequality of opportunity*. Respondents state how much they agree with the statement: "The state must reduce the differences in opportunities between children from rich and poor neighbourhoods." Answers are again reported on the same 4-point Likert scale.
- *Index of redistribution*. We construct an overall index of redistributive preferences by averaging the standardised values of the four outcomes above, following the composite-index approach used by Cruces et al. (2013).

All outcome variables are rescaled to range between 0 and 1, with higher values denoting stronger support for redistribution. In addition to these main outcomes, we use perceived fairness of the income distribution as a mechanism variable in Section 4. Table A–3 in the Appendix reports descriptive statistics for all outcomes and covariates.

2.3 Local inequality

Geocoding was performed ex post and is used only for the buffer-zone inequality analysis reported in Section 5. It did not affect treatment delivery, which was implemented in SurveyCTO using respondents' self-reported district, household income, and household size. Nor does it affect the construction of the main analytical sample of 1,293 respondents.

The survey asked each respondent to report their household address or, when this was not possible, the closest identifiable point to the dwelling, such as a street intersection, park, or bus stop. We first geocoded these references using automated searches through the Google API. We then manually verified the resulting coordinates and discarded inconsistent or low-quality entries. This procedure yields valid coordinates for 937 respondents, corresponding to 72% of the analytical sample. Appendix B describes the procedure in detail and presents the corresponding maps. The district distribution of the geocoded subsample closely matches that of the full sample, with one exception, San Martín de Porres (Table B–1). To preserve comparability with the full sample, we re-calibrate the survey weights for the geocoded subsample using the same variables as in the original design: age group, sex, and district. The control and treatment groups are also well spread across the city, as shown in Figure B–1.

The georeferenced data allow us to link each respondent to measures of inequality in their immediate residential environment. We construct circular buffer zones of 300, 500, and 1,000 metres around each respondent's dwelling. The local data come from the *Stratified Plans of Metropolitan Lima by Income at the Block Level* published by INEI (2021). These data report, for 2020 and at the block level, household income per capita, the share of households and persons in each of the five SES groups, and the coefficient of variation (CV) of income. We aggregate block-level information within each buffer zone and construct three local measures: the Generalised Entropy index with $\alpha = 2$ (GE(2)), the CV, and the local share of affluent households, defined as the sum of the upper-middle and upper SES groups.[6]

2.4 Methods

The empirical analysis proceeds in three steps. We first document the distribution of baseline misperceptions about own socio-economic status and about the share of affluent households in the respondent's district, using descriptive figures and regressions. We then estimate the effects of the information treatments on each redistributive-preference outcome, allowing treatment effects to differ by the sign and magnitude of the respondent's baseline misperception. Finally, for the geocoded subsample, we link respondents to local inequality measures computed within buffer zones of 300, 500, and 1,000 metres around their dwelling. These measures are used to interpret the observed biases and to examine whether responsiveness to information varies with local inequality.

Our main specification estimates treatment effects separately for each information treatment, pooling the relevant treatment group with the control group. For outcome j and respondent i, we estimate:

[6]Because the INEI data report the CV, mean income, and the number of persons and households per block, we can derive GE(2) for each buffer zone using the standard within-between decomposition of the GE(2) index. The buffer-zone CV is also computed from the block-level information.

$$y_{ji} = \beta_0 + \beta_1 T \times negbias_i + \beta_2 T \times posbias_i + \beta_3 T \times nobias_i + \beta_4 negbias_i + \beta_5 posbias_i + \varepsilon_i \qquad (1)$$

where y_{ji} is one of the redistributive-preference outcomes and T_i equals one for respondents assigned to the treatment arm and zero for respondents in the control group. The indicators $posbias_i$, $negbias_i$, and $nobias_i$ classify respondents according to their baseline misperception about the object treated in that specification: own SES in the T1 regressions and the district share of affluent households in the T2 regressions.

We define the bias indicators using a common structure across the two treatments. A respondent has a positive bias when the perceived value of the treated object exceeds the actual value by at least a pre-specified threshold, and a negative bias when the perceived value falls short of the actual value by at least that threshold. Respondents whose perceived–actual gap lies within the threshold are classified as having no bias. For the SES treatment, our primary threshold is a gap of at least two SES levels, and the sensitivity threshold is a gap of at least one level. For the affluent-share treatment, our primary threshold is a gap of at least ten percentage points, and the sensitivity threshold is a gap of at least five percentage points.

The omitted reference group in equation (1) is therefore composed of unbiased respondents in the control group. Their mean outcome is captured by β_0. The coefficients β_4 and β_5 capture baseline differences in outcomes between negatively or positively biased respondents and unbiased respondents within the control group. The coefficients of interest are β_1, β_2, and β_3, which estimate the treatment effect among respondents with a negative bias, a positive bias, and no bias, respectively. Because treatment assignment is random, these coefficients can be interpreted as average treatment effects within each pre-treatment bias group. We expect the strongest responses among positively biased respondents, for whom the information reveals that own SES or local affluence is lower than initially believed; by contrast, little or no response is expected among respondents classified as unbiased.

The stricter thresholds define our main specification. They identify cases in which the informational correction is large enough to plausibly affect respondents' underlying assessment of their own position or of local affluence, and therefore their stated preferences for redistribution. The looser thresholds are used as sensitivity checks. This choice also anticipates the framework developed in the next section: if information matters through belief updating, larger gaps between perceived and actual values should generate stronger responses. The primary thresholds therefore focus on the subsamples for which the treatment is expected to be most consequential.

Because we estimate effects on several related outcomes, we report multiplicity-adjusted inference for our main family of tests: the four redistributive-preference outcomes in the positive-bias subsample at the primary threshold, corresponding to Hypothesis H1 in Section 2.5. We first use the redistribution index as a summary outcome, following the standard practice of aggregating correlated outcomes into a single composite measure (Kling et al., 2007; Anderson, 2008). We then report results for the four component outcomes, adjusting inference with the Romano-Wolf step-down procedure (Romano and Wolf, 2005; Clarke et al., 2020) and with sharpened two-stage q-values (Benjamini et al., 2006; Anderson, 2008). As an additional check, we report a joint Wald test that all four treatment effects in the positive-bias subsample are zero. Details of the implementation and the full set of adjusted results are reported in Appendix G.1, with headline results in Appendix Table G–1 and a robustness check in Appendix Table G–2.[7]

2.5 Hypotheses

We organize the empirical analysis around three hypotheses derived from a simple framework that combines material self-interest with fairness concerns, following Cappelen et al. (2020), and from the empirical evidence on information provision about inequality, own position, and redistributive preferences (Cruces et al., 2013; Karadja et al., 2017; Kuziemko et al., 2015; Hvidberg et al., 2023; Bussolo and Dixit, 2023; Hoy et al., 2024a). The framework is deliberately stylized. Its purpose is not to model all channels through which information may affect preferences, but to clarify the predictions that guide the empirical analysis. In particular, it distinguishes between factual beliefs about position or local affluence, which the treatments directly target, and more fundamental normative views about fairness or deservingness, which may be less responsive to factual corrections (Stantcheva, 2021; Haaland et al., 2023).

Let y_i denote respondent i's actual socio-economic position and $\tilde{y}_i$ her perceived position before the information treatment. The respondent's preferred level of redistribution r combines a material self-interest component and a fairness component:

$$V_i(r;,y_i,m_i) = u(y_i,r) - \frac{\beta_i}{2}(r-m_i)^2. \tag{2}$$

The first term captures the material payoff from redistribution, given the respondent's position y_i. Under standard assumptions, the marginal material benefit of redistribution is higher for lower-ranked respondents. The second term captures the loss from deviating from the respondent's fairness ideal m_i, with $\beta_i \geq 0$ measuring the weight attached to fairness relative to self-interest. The preferred policy satisfies:

$$r_i^*(y_i,m_i) \;=\; m_i \;+\; \frac{1}{\beta_i}\frac{\partial u}{\partial r}(y_i,r_i^*). \tag{3}$$

[7] The robustness check applies the experiment-specific family-wise procedure of List et al. (2019).

This expression separates the fairness ideal, m_i, from the self-interest component, which depends on the marginal material payoff to redistribution at the respondent's perceived position.

Before treatment, respondents form their stated preferences using their perceived position $\tilde{y}_i$ rather than their actual position y_i. Define the perceptual bias as $b_i = \tilde{y}_i - y_i$, so that $b_i > 0$ denotes overestimation. The information treatment provides a signal that moves beliefs toward the actual value. In the limiting case of full updating, the change in the preferred level of redistribution is:

$$\Delta r_i^* = r_i^*(y_i, m_i) - r_i^*(\tilde{y}_i, m_i) = \frac{1}{\beta_i}\left[\frac{\partial u}{\partial r}(y_i) - \frac{\partial u}{\partial r}(\tilde{y}_i)\right]. \tag{4}$$

The same logic applies to both treatments. In T1, the correction concerns the respondent's own SES. In T2, it concerns the share of affluent households in the respondent's district, which may affect the perceived material return to redistribution by changing how respondents assess their local reference group and relative position. The information-provision literature provides the corresponding empirical regularity: respondents who learn that they are lower in the distribution than they believed tend to move toward redistribution, while respondents who receive the opposite news tend to move away from it (Cruces et al., 2013; Karadja et al., 2017; Hvidberg et al., 2023). This motivates the following hypotheses.

H1 (sign). Among respondents with a positive bias, in either treatment arm, corrective information increases stated support for redistribution. A respondent who learns that her household belongs to a lower SES group than she believed, or that her district contains a smaller share of affluent households than she believed, should revise upward the expected material benefit from redistribution and update her stated preference accordingly.

H2 (magnitude). The treatment effect increases with the size of the corrected misperception. A larger gap between perceived and actual values implies a larger informational surprise and, under belief updating, a larger revision in redistributive preferences. This dose-response logic motivates the primary bias thresholds defined in Section 2.4.

H3 (heterogeneity). Treatment effects are larger among respondents whose prior beliefs are usually associated with weaker support for redistribution: right-leaning respondents, individuals with more individualistic views, respondents with low trust in government, and those who believe that income differences mainly reflect effort. For these groups, the corrective signal carries more news relative to their initial priors. This prediction is consistent with Hoy et al. (2024a), who show that information about inequality has stronger effects among right-wing respondents in Australia.

Additional implications. The same framework also clarifies where treatment effects should be limited. First, the treatments should not substantially change perceived fairness of the income distribution. They provide information about the respondent's own position or about local affluence, but not about the fairness ideal m_i or about the deservingness of different groups. This distinction is consistent with information-provision experiments showing that factual corrections can shift policy preferences while leaving more fundamental normative views, such as fairness concerns, trust in government, or beliefs about deservingness, less affected (Kuziemko et al., 2015; Stantcheva, 2021; Haaland et al., 2023). Second, the treatments should have little effect on support for the wealth tax. The proposed tax applies to wealth above one million soles, a threshold that affects only a very small fraction of Peruvian households. For most respondents, the personal material stake in this instrument is therefore limited, regardless of whether they update their perceived SES or their beliefs about local affluence.

3 Results

3.1 Bias analysis

We begin by documenting baseline misperceptions about own socio-economic status (SES) and about the share of affluent households, defined as upper-middle and upper households, in the respondent's district. For own SES, bias is defined as the difference between perceived and actual SES, with positive values indicating overestimation.[8] Figure 1a shows that the distribution is strongly tilted toward positive values: 69.9% of respondents place themselves in a higher SES group than the one implied by the official classification, 17.5% report their SES accurately, and only 12.6% underestimate it.[9] Appendix Table C–1 reports the bias distribution by actual SES group. Appendix Figure C–1 shows that the distribution of perceived social class in our survey is broadly similar to that observed in the Peruvian sample of Latinobarómetro, a cross-country public-opinion survey conducted regularly across Latin America.

This pattern differs markedly from the evidence in most previous studies. Karadja et al. (2017) report that 86% of Swedish respondents under-estimate their position in the national income distribution. In the Buenos Aires field experiment of Cruces et al. (2013), 30% of subjects over-estimate their income decile, while 55% under-estimate it and 15% report it correctly. In a large-scale survey experiment in India, Bussolo and Dixit (2023) find that 70% of respondents under-estimate their income decile and only 17% over-estimate it. The closest comparison is Fernandez-Albertos and Kuo (2015), who report, using an online survey experiment in Spain, that 45% of respondents perceive themselves as

[8] Assigning numerical values from 1 (low) to 5 (upper) to the SES groups, the absolute bias has a median of 1.0 and a mean of 0.89.

[9] Using the stricter two-class threshold, 33.8% of respondents are classified as having positive bias, 5.2% as having negative bias, and 61.0% as having no large bias.

richer than they are, while 40% perceive themselves as poorer.[10] One reason for this difference may be measurement: perceived SES in our survey is a broader self-assessment than an income decile and may reflect local reference groups, consumption patterns, neighbourhood, and other dimensions of status. At the same time, the magnitude of the gap suggests substantial uncertainty about the income distribution and respondents' position within it. The empirical question is whether providing accurate information about these gaps changes preferences for redistribution.

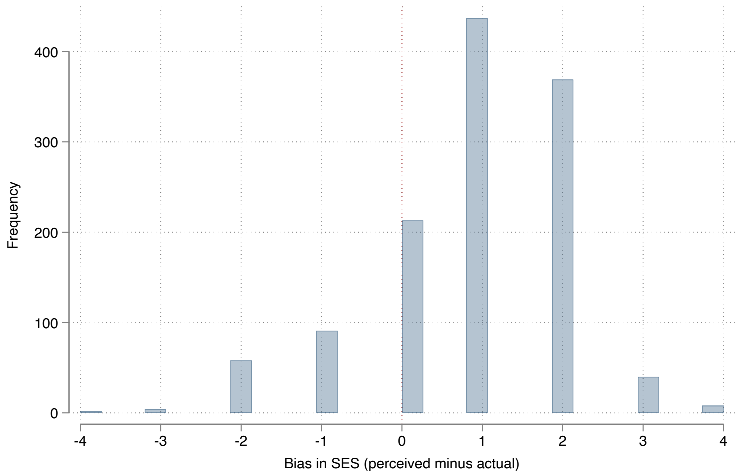
Misperceptions about the district share of affluent households display a similar, though less pronounced, asymmetry. Figure 1b shows that the signed bias is also tilted toward positive values. Overall, 57.1% of respondents report a share of affluent households above the actual district value, while 41.2% report a lower share. When perceptions within ± 5 percentage points of the actual value are classified as unbiased, 50% of respondents over-estimate the affluent share and 34% under-estimate it.[11] The direction of this bias varies sharply with the respondent's district of residence. As Appendix Table C–2 shows, 93% of respondents living in low-income districts over-estimate the district share of affluent households, whereas 96% of respondents in high-income districts under-estimate it. Appendix Figure C–2 illustrates these deviations from the unbiased benchmark across districts.

[10] In a study covering ten countries, Hoy and Mager (2021) focus on the two poorest quintiles and find that between 67% in the United Kingdom and 94% in Nigeria over-estimate their relative position.

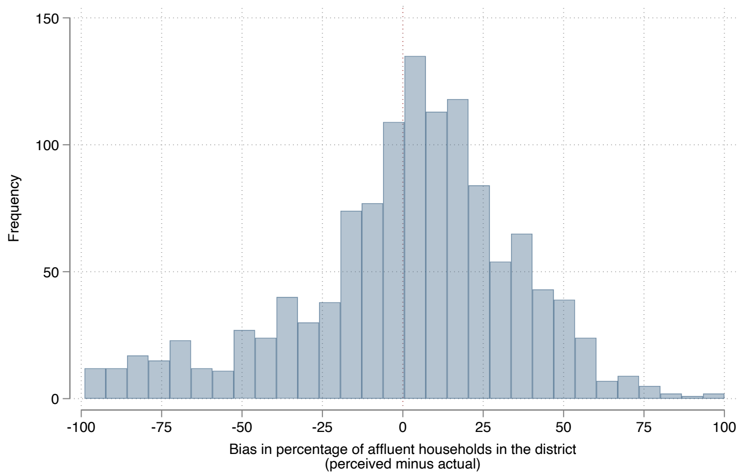
[11] Using the stricter ± 10 percentage points threshold, 41.4% of respondents are classified as having positive bias, 29.2% as having negative bias, and 29.4% as having no large bias.

Figure 1: Distribution of bias in the sample

(a) Perceived and actual SES



(b) Perceived and actual share of affluent households in the district



Notes: The top panel shows the distribution of SES bias, defined as the difference between perceived and actual SES group. The bottom panel shows the distribution of affluent-share bias, defined as the difference between the perceived and actual percentage of affluent households, i.e. upper- and upper-middle-class households, in the respondent's district. The analysis is based on 1,222 observations with complete covariate information.

These biases vary systematically across respondents. Table 2 reports linear regressions of the two bias measures on individual covariates.[12] Columns (1)–(2) refer to the bias in perceived own SES, while columns (3)–(4) refer to the bias in the perceived district share of affluent households. Columns (1) and (3) use the signed bias, defined as the difference between perceived and actual values, so that positive values indicate over-estimation. Columns (2) and (4) use the absolute value of the bias. To account for differences in respondents' actual positions, the SES-bias regressions include fixed effects for actual SES, while the affluent-share regressions include fixed effects for the actual district share of

[12] We include respondents from both the control and treatment groups, since perceptions of own SES and of the district share of affluent households were elicited at baseline, before any informational intervention.

affluent households. The estimates therefore show how individual characteristics are associated with misperceptions, conditional on the relevant actual benchmark.

Table 2 confirms the positive average bias documented above for both own SES and local affluence. It also shows that these biases are correlated with several respondent characteristics. Higher-income respondents are more likely to over-estimate their own SES, while respondents with right-wing political orientations are more likely to over-estimate both their own SES and the share of affluent households in their district. Respondents who attribute the wealth of the rich to greater advantages tend to under-estimate their own SES. By contrast, respondents who trust the government to use public resources for the benefit of the poor tend to over-estimate both own SES and local affluence.

The regressions using absolute bias capture the size of misperceptions, irrespective of their direction. For own SES, misperceptions are larger among respondents with higher income, right-wing political orientations, greater trust in the state, and stronger agreement that the government should bear responsibility for ensuring everyone a livelihood. They are smaller among respondents who follow the news daily, attribute the position of the rich to advantages, and report greater willingness to take risks. For perceptions of the district share of affluent households, individual covariates explain less of the variation in absolute bias; the only clear association is that tertiary education is linked to smaller misperceptions.

Table 2: Determinants of bias

	(1) SES bias	(2) SES bias (abs value)	(3) Affluent-share bias	(4) Affluent-share bias (abs value)
Age 14-29	0.138**	0.079	1.064	0.370
	(0.06)	(0.06)	(1.73)	(1.53)
Age 30-44	-0.021	0.013	-0.437	1.975
	(0.05)	(0.05)	(1.56)	(1.40)
Male	-0.004	0.028	0.572	0.386
	(0.04)	(0.04)	(1.24)	(1.09)
Married	0.087*	0.073	1.069	1.106
	(0.05)	(0.05)	(1.47)	(1.31)
Per capita household income	0.164***	0.159***	-0.859	0.369
	(0.05)	(0.05)	(0.80)	(0.71)
Secondary education	-0.013	-0.026	-2.855	-1.818
	(0.09)	(0.09)	(2.35)	(2.07)
Tertiary education	0.098	-0.002	-3.116	-4.861**
	(0.10)	(0.10)	(2.54)	(2.25)
Employed	0.028	-0.028	-2.103	-0.407
	(0.06)	(0.06)	(1.68)	(1.46)
Self-employed	0.057	0.045	-2.278	0.905
	(0.06)	(0.06)	(1.69)	(1.49)
Informed	-0.021	-0.080*	0.148	-1.426
	(0.04)	(0.04)	(1.28)	(1.15)
Right-wing	0.150***	0.121***	4.465***	1.507
	(0.04)	(0.04)	(1.25)	(1.12)
Poor due to circumstances	-0.068	-0.064	-2.084	-0.137
	(0.05)	(0.05)	(1.39)	(1.26)
Rich due to more advantages	-0.181***	-0.144***	-1.990	-1.709
	(0.05)	(0.05)	(1.39)	(1.26)
Trust in the state	0.136***	0.105**	2.621**	0.030
	(0.05)	(0.05)	(1.32)	(1.18)
Responsibility of the state	0.047	0.101**	1.285	-0.729
	(0.04)	(0.04)	(1.25)	(1.12)
Take risks	-0.051	-0.078*	1.239	-0.758
	(0.05)	(0.05)	(1.29)	(1.15)
Constant	-0.222	0.267	7.607	26.957***
	(0.30)	(0.30)	(5.44)	(4.88)
Observations	1,222	1,222	1,222	1,222
Mean	0.90	1.27	1.23	26.34

Notes: Robust standard errors are reported in parentheses. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$ denote statistical significance levels. The dependent variable in columns 1 and 2 is the bias in the respondent's SES, calculated as the difference between the perceived SES and the actual SES (measured in SES group levels). In columns 3 and 4, the dependent variable is the bias in the estimated percentage of upper or upper-middle-class households in the respondent's district, defined as the difference between the perceived and actual percentages. Regressions in columns 1 and 2 include fixed effects for the actual SES group, while regressions in columns 3 and 4 include fixed effects for the actual share of affluent households in the district.

We finally examine whether these misperceptions are related to the local inequality surrounding respondents' place of residence. Recent work shows that neighbourhood-level inequality can shape perceived inequality and, in some cases, redistributive attitudes (Minkoff and Lyons, 2019; Hoy et al., 2024a; Domènech-Arumí, 2025). Using the geocoded subsample, we augment the specifications in Table 2 with local inequality measures computed within buffer zones of 300, 500, and 1,000 metres around each household. Appendix Table C–3 shows that local inequality is significantly associated with bias in perceived own SES, but not with bias in the perceived district share of affluent households. Given that SES misperceptions are predominantly positive in our sample, this pattern suggests that residents of more unequal local environments tend to assess their own social position less accurately.

3.2 Main results

Tables 3 and 4 report the average treatment effects at the main bias thresholds: at least two SES levels for the SES treatment (T1), and at least ten percentage points for the affluent-share treatment (T2). Results using the looser sensitivity thresholds (at least one SES level for T1 and five percentage points for T2) are reported in Appendix Tables D–1 and D–2. These appendix tables allow us to assess whether the direction and concentration of the treatment effects are robust to a more permissive definition of bias.

Table 3 reports the effects of the SES treatment. Among respondents who over-estimate their SES by at least two levels, the treatment increases support for redistribution. Support for reducing income inequality and for reducing inequality of opportunity both rise by about five percentage points, while the composite redistribution index increases by 3.9 percentage points ($p = 0.037$). This pattern is consistent with a self-interest interpretation (Hypothesis H1): respondents become more supportive of redistribution when they learn that their household is classified lower than they believed. It is also in line with previous information-provision experiments. Cruces et al. (2013) find that respondents who learn that their income decile is lower than expected become more supportive of redistribution, while Karadja et al. (2017) document the reverse pattern among individuals who learn that they are better off than they thought. The magnitude of the correction also matters. When we use the looser one-level threshold (Appendix Table D–1, Panel A), the signs remain similar but the coefficients are smaller, with only weak evidence of an effect on support for reducing income inequality.

The table also shows a positive effect on support for reducing inequality of opportunity among respondents with a negative SES bias ($\beta = 0.123$, $p = 0.027$). This estimate is not predicted by the framework and should be interpreted cautiously. First, baseline support for this outcome is already high: 81% of control respondents agree or strongly agree that the state should reduce differences in opportunities between children from rich and poor neighbourhoods. Second, the estimate is based on a small number of respondents with a two-level negative bias: 23 in the control group and 22 in the treatment group. Finally, the treatment has no detectable effects among respondents classified as unbiased, suggesting that the estimated responses are driven by the informational content of the correction rather than by exposure to the treatment screen itself.

Table 3: Average effects of the SES treatment at the primary bias threshold

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Treated x Neg. bias	-0.116	0.086	0.050	0.123**	0.063
	(0.076)	(0.060)	(0.062)	(0.055)	(0.057)
Treated x Pos. bias	-0.004	0.052**	0.025	0.048**	0.039**
	(0.030)	(0.021)	(0.022)	(0.021)	(0.019)
Treated x No bias	0.036	-0.005	-0.009	-0.007	0.002
	(0.024)	(0.016)	(0.018)	(0.017)	(0.015)
Neg. bias	0.101*	-0.037	-0.060	-0.067*	-0.031
	(0.056)	(0.036)	(0.045)	(0.037)	(0.034)
Pos. bias	0.031	-0.018	-0.011	-0.049**	-0.017
	(0.027)	(0.019)	(0.021)	(0.020)	(0.018)
Constant	0.455***	0.689***	0.669***	0.762***	0.563***
	(0.018)	(0.012)	(0.014)	(0.013)	(0.012)
Observations	815	816	816	818	810

Notes: The table reports estimated average effects of the SES treatment by baseline bias type, using the main threshold of at least two SES levels. A negative (positive) bias indicates that the respondent's perceived SES is at least two levels below (above) the SES implied by the official classification. Robust standard errors are reported in parentheses. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$ indicate statistical significance levels.

Table 4 reports the effects of the affluent-share treatment, using the main threshold of 10 percentage points. Among respondents who over-estimate the district share of affluent households by at least this amount, the correction increases support for redistribution. The largest and most precise effects are found for the preferences for redistribution item, which rises by 6.9 percentage points ($p = 0.009$), and for the composite redistribution index, which rises by 5.4 percentage points ($p = 0.003$). Support for reducing income inequality and for reducing inequality of opportunity also increases, with the latter effect statistically significant. We find no comparable response among respondents who under-estimate the local share of affluent households. The results are similar, though somewhat smaller in magnitude, when we use the looser 5 percentage-point threshold (Appendix Table D–2, Panel A), indicating that the main pattern is not driven by the exact cut-off. Appendix Tables G–5 and G–6 show that adding the full set of covariates does not qualitatively alter the main results at either bias threshold.[13]

[13] Appendix G reports additional robustness checks for the main average treatment effects. Tables G–8 and G–9 use district-clustered standard errors. Tables G–10 and G–11 re-estimate the main specifications on a common sample of respondents with complete outcome information ($N = 810$ for T1 and $N = 849$ for T2). Table G–7 replaces the categorical bias indicators with the continuous signed gap, measured in percentage points, between the perceived and actual share of affluent households. The qualitative pattern of the results is unchanged across these checks.

Table 4: Average effects of the affluent-share treatment at the primary bias threshold

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Treated x Neg. bias	0.057*	-0.013	-0.026	0.002	-0.000
	(0.033)	(0.021)	(0.025)	(0.020)	(0.020)
Treated x Pos. bias	0.069***	0.034*	0.025	0.041**	0.054***
	(0.026)	(0.019)	(0.022)	(0.020)	(0.018)
Treated x No bias	-0.002	0.008	-0.029	0.010	-0.007
	(0.035)	(0.022)	(0.025)	(0.023)	(0.021)
Neg. bias	-0.047	0.012	-0.046*	-0.021	-0.033
	(0.036)	(0.022)	(0.026)	(0.024)	(0.021)
Pos. bias	-0.020	-0.030	-0.056**	-0.063***	-0.061***
	(0.033)	(0.022)	(0.025)	(0.024)	(0.021)
Constant	0.495***	0.690***	0.699***	0.773***	0.591***
	(0.027)	(0.017)	(0.019)	(0.018)	(0.016)
Observations	858	857	857	859	849

Notes: The table reports estimated average effects of the affluent-share treatment by baseline bias type, using a threshold of 10 percentage points. A negative (positive) bias indicates that the respondent's perceived district share of affluent households is at least 10 percentage points below (above) the actual share. Robust standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$ indicate statistical significance levels.

Taken together, the two treatments show that effects are concentrated among respondents with a positive baseline bias. Under the main thresholds, the SES treatment increases support for reducing income inequality, support for reducing inequality of opportunity, and the composite redistribution index among respondents who over-estimate their own SES. The affluent-share treatment increases the preference for redistribution measure, the two inequality outcomes, and the composite index among respondents who over-estimate the share of affluent households in their district. Across both the main and sensitivity bias thresholds, the signs are stable and the effects remain concentrated in the positive-bias subsample, although magnitudes are smaller under the more permissive definitions of bias. This pattern is consistent with the magnitude prediction in H2: larger corrections generate stronger responses. We find no evidence that either treatment affects support for the wealth tax.

We also report inference adjusted for multiple hypothesis testing within this focal family (Appendix G.1 and Table G–1). Used as the aggregate test of H1, the composite redistribution index is significant for both treatments, at the five percent level for the SES treatment and at the one percent level for the affluent-share treatment. Among the four component outcomes, the affluent-share effect on the preference for redistribution survives both the Romano-Wolf step-down family-wise correction (Romano and Wolf, 2005; Clarke et al., 2020) and the Anderson sharpened q-value false-discovery adjustment (Benjamini et al., 2006; Anderson, 2008), and the SES effects on reducing income inequality and reducing inequality of opportunity survive these corrections at conventional levels. A joint Wald test that all four positive-bias effects are zero rejects for the affluent-share treatment but not for the SES treatment; because this omnibus test loses power when effects are concentrated in a subset of correlated outcomes, we treat it as a conservative check. The experiment-specific procedure of List et al. (2019) (Appendix Table G–2) leads to the same substantive conclusion.

The stronger evidence for the affluent-share treatment is consistent with the distinction between the two interventions. The SES treatment compares respondents' self-assessed class with an income-based official classification. Because self-assessed class may also reflect occupation, education, consumption, neighbourhood, and other status cues, this correction is necessarily noisier. The affluent-share treatment instead corrects a well-defined cardinal belief: the share of upper- and upper-middle-class households in the respondent's district. The perceived–actual gap is therefore more precisely measured, and the informational signal is sharper. We therefore give particular weight to the affluent-share results, while interpreting the SES results as evidence that upward misperceptions of own status are common and that large corrections move preferences in the direction predicted by self-interest.

3.3 Heterogeneous treatment effects

We examine heterogeneous treatment effects using beliefs and attitudes elicited before the information intervention. Since the average effects are concentrated among respondents with a positive bias, the analysis is restricted to individuals who over-estimated either their own SES or the district share of affluent households. The heterogeneity dimensions include fairness views, risk preferences, political ideology, trust in government, and beliefs about individual versus government responsibility. For each outcome, we estimate a separate linear regression on the positive-bias subsample:

$$y_{ji} = \beta_0 + \beta_1 T_i + \beta_2 T_i \times belief_i + \beta_3 belief_i + \varepsilon_i, \tag{5}$$

where y_{ji} denotes outcome j for respondent i, T_i equals one for respondents assigned to the corresponding treatment arm, and $belief_i$ is a pre-treatment indicator for the relevant belief or attitude.

The interaction terms are not statistically significant across treatment arms or threshold definitions; the full results are reported in Appendix E. This absence of statistical significance is likely related to the limited size of the positive-bias subsamples once they are further divided by baseline beliefs. The point estimates should therefore be interpreted cautiously. Still, their direction is informative. For the SES treatment, the estimated effects are larger among respondents whose pre-treatment beliefs are typically associated with weaker support for redistribution. As shown in Figure E–2, right-leaning respondents and respondents with low trust in government increase support for reducing both income inequality and inequality of opportunity after learning that they had over-estimated their SES by at least two levels. Similar directional patterns appear among risk-seeking respondents, those who emphasise individual responsibility over government intervention, and those who attribute wealth to hard work or poverty to lack of effort. The composite redistribution index follows the same broad pattern.

The affluent-share treatment displays a similar profile. Figure E–4 shows that, among respondents who over-estimated the local affluent share by at least 10 percentage points, the estimated effects are larger for risk-seeking respondents, those who emphasise individual responsibility, and those with low trust in government. Respondents who attribute wealth to hard work also show larger increases in the preference for redistribution item, support for reducing income inequality, and the composite index. Those who attribute poverty to lack of effort show a similar pattern for the preference for redistribution item and for the composite index. As in the average treatment effects, we do not observe comparable responses for the wealth-tax outcome.

Overall, the heterogeneity analysis is consistent with the interpretation developed above, but it should not be read as definitive evidence of heterogeneous treatment effects. The point estimates suggest that corrective information may also shift preferences among respondents who, based on their priors, would usually be less favourable to redistribution. This is in line with a self-interest and belief-updating mechanism: when positively biased respondents receive information indicating that their own SES or the affluence of their district is lower than they believed, they tend to move toward greater support for redistribution. However, because the interaction terms are not statistically significant, these results are best interpreted as suggestive patterns that complement the average treatment effects rather than as stand-alone evidence.

4 Mechanisms

Section 2.5 suggests that the treatments should affect redistributive preferences mainly through belief updating and self-interest. This section examines three implications of that interpretation: whether the treatments change perceived fairness of the income distribution, whether responses are stronger among respondents whose priors are less favourable to redistribution, and whether the treatments affect support for a wealth tax.

Perceived fairness of the income distribution. If the treatments operate mainly by changing respondents' perceived position or perceived local affluence, they need not change their assessment of whether the income distribution is fair. This distinction is consistent with the broader information-provision literature: corrective information can move policy preferences by updating factual beliefs, while more fundamental normative views, such as fairness concerns and trust in government, are often more deeply rooted and less directly affected by factual corrections (Kuziemko et al., 2015; Stantcheva, 2021; Haaland et al., 2023). We test this implication by re-estimating the main specification using perceived fairness of the income distribution in Peru as the outcome. The variable is rescaled to range between 0 and 1, with higher values denoting a stronger perception that the distribution is unfair. The $Treat \times Positive$ bias coefficient is small and statistically indistinguishable from zero in both treatment arms, at both bias thresholds, and in specifications with and without covariates (Appendix Tables G–13, G–14, G–15, and G–16). Thus, the treatments move some redistributive preferences without detectably changing perceived fairness. This pattern is consistent with a self-interest interpretation and makes it less likely that the main effects are driven by changes in fairness perceptions.

Heterogeneity by prior beliefs. Hypothesis H3 predicts larger responses among respondents whose pre-treatment beliefs are usually associated with weaker support for redistribution. The heterogeneity analysis in Section 3.3 is broadly consistent with this prediction, although the interaction terms are not statistically significant and should therefore be interpreted cautiously. The point estimates suggest larger treatment effects among right-leaning respondents, respondents with low trust in government, those with more individualistic views, and those who attribute income differences mainly to effort. For these respondents, the information may carry more news relative to their priors, making the correction more consequential for stated preferences. The continuous-bias specification in Appendix Table G–7 provides complementary evidence for the affluent-share treatment: treatment effects increase with the size of the signed gap, measured in percentage points, between the perceived and actual share of affluent households.

The wealth-tax null. The wealth-tax outcome provides an additional check on the self-interest interpretation. The survey asked about support for a tax on wealth above one million Soles. According to the World Inequality Database, the 99.1st percentile of Peru's net wealth distribution in 2021 was 994,754 Soles. The proposed threshold would therefore apply only to approximately the top 0.9% of the wealth distribution. For the overwhelming majority of respondents, the direct personal cost of this tax is close to zero, regardless of whether they update their perceived SES or their beliefs about local affluence. The absence of treatment effects on support for the wealth tax is consistent with this logic: when the information does not meaningfully change respondents' expected personal gains or losses from a specific policy instrument, self-interest predicts little movement in support for that instrument.

Status concerns as a complementary channel. A related mechanism that our design cannot separately identify is status concern. Respondents who learn that their position is lower than they had believed may revise their redistributive preferences not only because the information changes the expected material returns to redistribution, but also because it alters their perceived relative status. In the present design, these channels are difficult to disentangle. Both imply stronger responses among positively biased respondents, both are consistent with the absence of treatment effects on perceived fairness, and both could plausibly contribute to the heterogeneous responses by political and ideological priors. Distinguishing material self-interest from status concerns would require either an experimental manipulation that varies perceived status independently of expected material returns, or baseline measures of respondents' social-comparison orientation. We therefore interpret status concerns as a plausible complementary mechanism, while acknowledging that the experiment is not designed to isolate this channel.

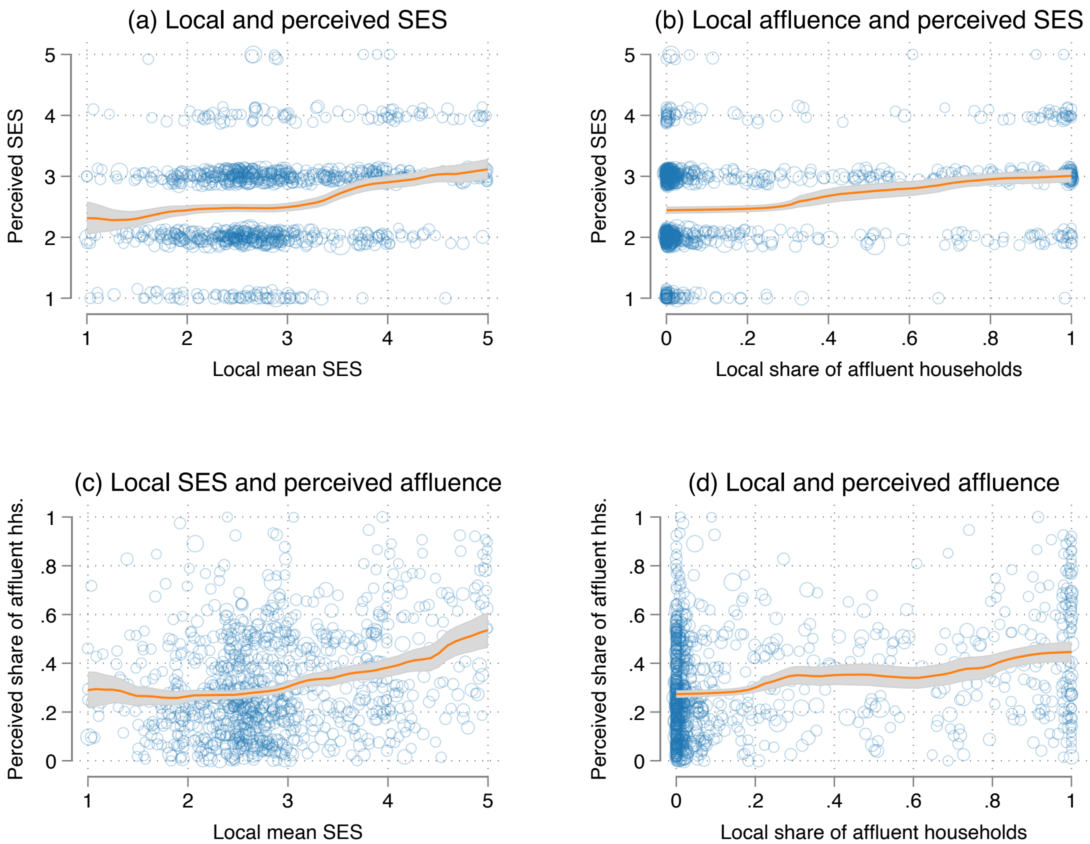
5 The role of local inequality

We now turn to the role of local inequality. This dimension is central to our setting because social comparison in Lima is likely to be shaped not only by national or district-level distributions, but also by the socio-economic composition of the immediate residential environment. The geocoded subsample allows us to examine this directly by linking respondents' perceptions to local measures of SES and affluence computed within buffer zones around their dwellings. Before studying whether local inequality conditions treatment effects, we first ask a simpler descriptive question: are respondents' perceptions of their own SES and of local affluence related to the socio-economic composition of the area in which they live?

Figure 2 provides this first descriptive step, using local indicators computed within 500 meter buffers around each respondent's dwelling. Perceived own SES is positively associated with both the local mean SES score and the local share of affluent households, although the relationship is moderate and there is substantial dispersion at each level of local SES. Perceived local affluence displays a similar pattern: respondents living in areas with higher local SES or a larger share of affluent households tend to report higher perceived affluence in their district, but the association remains imperfect. These patterns suggest that local conditions enter respondents' perceptions, but only imperfectly.[14] This is consistent with the idea that perceptions are shaped by local reference groups and visible neighbourhood composition, while also reflecting broader district images, mobility across the city, and individual socio-economic identity (Hvidberg et al., 2023). The analysis below builds on this descriptive evidence by examining whether local inequality is associated with perceptual bias and whether it conditions responses to the information treatments.

[14] Appendix Table G–17 reports the weighted rank correlations underlying these patterns. The association between perceptions and the local socio-economic environment is positive but moderate, and it is robust: it holds for both perceived SES and perceived affluence, and remains stable across the buffer sizes considered and at the district level.

Figure 2: Individual perceptions and local measures of SES and affluence



Notes: The figure shows the relationship between respondents' perceptions and factual measures of SES and affluence in their immediate residential environment. Perceived SES refers to respondents' self-assessed SES group, while perceived affluence refers to their estimate of the share of affluent households, defined as upper-middle and upper households, in their district of residence. Local measures are computed within 500 meter buffers around each respondent's dwelling. The fitted lines are local polynomial smooths, with shaded areas indicating pointwise confidence intervals. Estimates are based on the geocoded subsample ($N = 937$) and use recalibrated survey weights to match the full analytical sample.

5.1 Inclusion of local inequality levels

We next examine whether local inequality is associated with redistributive preferences. This exercise is descriptive: local inequality is not experimentally assigned, and the coefficients should therefore be interpreted as conditional associations rather than causal effects. Table 5 reports the coefficients on alternative local inequality measures, each included separately in the main regression specification. We use the main bias thresholds: at least two SES levels for the SES treatment and at least 10 percentage points for the affluent-share treatment.

Panel A reports the results for the SES-treatment sample. Local income inequality, measured by GE(2) or by the coefficient of variation, is negatively associated with the preference for redistribution item and, more weakly, with support for the wealth tax. The local share of affluent households displays a similar negative association with these two outcomes, while its association with support for reducing inequality of opportunity is positive but weak. Panel B reports the corresponding estimates for the affluent-share-treatment sample. The negative association between local inequality and redistributive preferences is again present and tends to be stronger, especially for the composite redistribution index and for support for the wealth tax.

To make the comparison with the existing observational literature clearer, Appendix Table G–18 repeats the analysis using only respondents in the control group. This restriction is useful because control respondents did not receive corrective information, making the exercise closer to studies that relate local inequality to perceptions and preferences without experimentally changing beliefs. In this sample, the unconditional association between local inequality and redistributive preferences is weak and generally not statistically distinguishable from zero across the three inequality measures and the three buffer sizes. This pattern is consistent with the evidence in Domènech-Arumí (2025) for Barcelona, where local inequality predicts perceived inequality but has at most a small direct association with demand for redistribution. In our setting, local inequality appears more relevant for understanding perceptions and perceptual biases than for directly predicting redistributive preferences in the absence of corrective information.

Table 5: Coefficients of local inequality on redistributive preferences

Buffer zone	Inequality metric	Pref. for redist.	Reduce inequality	Wealth tax	Reduce IneqOpp	Index redist.
Panel A: SES treatment equation						
300 mts.	GE(2)	-0.057	0.002	-0.014	0.009	-0.030
	Coef. Var.	-0.012	0.000	-0.005	0.003	-0.006
	%Rich	-0.066**	-0.015	-0.039*	0.040**	-0.021
500 mts.	GE(2)	-0.093**	-0.006	-0.055*	0.024	-0.042
	Coef. Var.	-0.018**	-0.001	-0.010	0.005	-0.007
	%Rich	-0.060 **	-0.020	-0.043*	0.039 *	-0.021
1000 mts.	GE(2)	-0.090*	-0.025	-0.057*	0.013	-0.052*
	Coef. Var.	-0.017*	-0.005	-0.011*	0.003	-0.009*
	%Rich	-0.051*	-0.022	-0.043*	0.040*	-0.020
Panel B: Rich share treatment equation						
300 mts.	GE(2)	-0.057	-0.050*	-0.058*	-0.036	-0.068**
	Coef. Var.	-0.012	-0.009	-0.011*	-0.007	-0.013**
	%Rich	-0.072**	-0.027	-0.071***	0.003	-0.051**
500 mts.	GE(2)	-0.086*	-0.067*	-0.081**	-0.051	-0.096***
	Coef. Var.	-0.017*	-0.012*	-0.015**	-0.009	-0.018***
	%Rich	-0.067 *	-0.038	-0.085***	-0.000	-0.059**
1000 mts.	GE(2)	-0.119**	-0.084***	-0.111***	-0.065*	-0.123***
	Coef. Var.	-0.023**	-0.016**	-0.022***	-0.012*	-0.024***
	%Rich	-0.070*	-0.048	-0.095***	0.002	-0.067**

Notes: The table reports coefficients on local inequality metrics, each included as a covariate in separate regressions estimated from equation 1. Each cell corresponds to a distinct regression. Inequality metrics are computed using households within buffer zones of 300, 500, or 1,000 metres around the respondent's location. In Panel A, a negative (positive) bias is defined as perceiving one's SES to be at least two categories lower (higher) than the actual SES. In Panel B, a negative (positive) bias is defined as perceiving the district share of affluent households to be at least 10 percentage points lower (higher) than the actual share. All regressions use calibrated weights to account for observations with no geolocation data. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$ indicate statistical significance levels.

5.2 Treatment effects across inequality levels

The results so far suggest that individuals exposed to higher local inequality may be less supportive of redistribution. We next ask whether local inequality also shapes how respondents update when given corrective information. To do so, we estimate heterogeneous treatment effects on redistributive attitudes among respondents with a positive bias in either their SES or the perceived share of affluent households:

$$y_{ji} = \beta_0 + \beta_1 T + \beta_2 (T \times highineq) + \beta_3 highineq + \varepsilon_i \tag{6}$$

where y_{ji} denotes one of the j outcome variables capturing redistributive preferences for individual i, and $T = 1$ if the respondent received the treatment and 0 otherwise. The variable *highineq* takes the value 1 when the level of local inequality within the respondent's buffer zone is above the sample median, and 0 otherwise. Local inequality is calculated for buffer zones of 300, 500, and 1,000 metres around each respondent's dwelling. The results are reported in Appendix F.

For the SES treatment, we observe no differential effects across low- and high-inequality areas, whether inequality is measured by GE(2) (Figure F–1) or by the local share of affluent households (Figure F–2).

For the affluent-share treatment, by contrast, we do find heterogeneity by local inequality. Respondents living in higher-inequality areas are more likely to revise their views about the local share of affluent households and, in turn, to increase their stated support for redistribution (the *preference for redistribution* outcome). Since we focus on individuals with a positive bias, this means correcting overestimated local affluence raises support for redistribution mainly among those in high-inequality areas.[15]

Local inequality therefore seems to matter for how individuals revise their beliefs about the local share of affluent households. The effect is not strong, however: it does not appear consistently across outcomes, it is present only under the 10 pp definition of bias, and the experimental subsamples involved are small, so it should be read with caution.

These local-inequality results connect directly to one of the studies closest to ours, Domènech-Arumí (2025). Recall from Section 5.1 that, among control-group respondents, the association between local inequality and redistributive preferences is small and not statistically significant across the three inequality measures and buffer sizes (Appendix Table G–18). This unconditional null is consistent with the central finding in Domènech-Arumí (2025) for Barcelona: neighbourhood inequality is related to perceived inequality, but has limited direct association with preferences for redistribution. The comparison is useful because Lima differs sharply from Barcelona in its degree of residential segregation and urban

[15] The differences between respondents in low- and high-inequality areas are statistically significant when bias is defined as overestimating the district share of affluent households by at least 10 percentage points and inequality is measured by GE(2) within a 1 km buffer around the respondent's home (see Figure F–3). A similar difference appears when we use the local share of affluent households within a 500-metre buffer as a proxy for inequality (see Figure F–4).

inequality. Taken together, the two settings point to a common pattern: local inequality appears more closely linked to perceptions of inequality than to redistributive preferences in the absence of additional information.

The second closely related study is Hoy et al. (2024a). In their Australian experiments, information about inequality increases support for redistribution more strongly in high-inequality areas, but only among right-leaning voters. Our affluent-share treatment points to a related, though not identical, pattern. Among respondents who overestimate local affluence, the correction appears to raise support for redistribution mainly when respondents live in more unequal local environments. The comparison is necessarily qualitative. In their setting, the relevant heterogeneity is political; in ours, it is perceptual, defined by the direction and size of the respondent's prior bias. Their measure of local inequality is also based on Local Government Areas (LGA), a much broader spatial unit than the residential buffers used here. Still, the mechanism they emphasize is relevant for our interpretation: information about inequality may be more persuasive when it is consistent with what respondents can observe in their surrounding environment.

Taken together, these studies help clarify the contribution of our local-inequality analysis. Like Domènech-Arumí (2025), we find little evidence that local inequality is directly associated with redistributive preferences in the control group. Like Hoy et al. (2024a), however, our results suggest that local inequality may matter for how respondents respond to information. In our case, this operates through perceptual bias: among respondents who overestimate local affluence, the correction appears more consequential in more unequal local environments. The contribution of the Lima evidence is therefore to link local inequality, misperceptions, and information effects within the same design. Appendix Table G–19 places the main features of the three studies side by side.

6 Limitations and conclusion

6.1 Limitations

Several features of the design should be kept in mind when interpreting the results. A first set of limitations concerns measurement. The classification of respondents into actual SES groups relies on self-reported household income and household size. Both variables are subject to reporting error, and this matters most for the SES treatment, where the treatment is defined relative to the gap between perceived SES and the income-based official classification. To the extent that this error is classical, it weakens the distinction between respondents who are genuinely positively biased and those who are not. Some respondents classified as overestimating their SES may simply have under-reported income, making the treatment informationally noisier for them. This source of error is therefore likely to attenuate the estimated effects of the SES treatment, especially in the positive-bias subsample. A related issue is that the categorical thresholds used to define bias are partly conventional. We address this by reporting results for both stricter and looser thresholds, and, for the affluent-share treatment, by estimating a continuous-bias specification that does not impose a discrete cut-off. The qualitative pattern is robust across these alternatives.

The local-inequality analysis is also subject to measurement and sample-selection concerns. Our GE(2), coefficient of variation, and affluent-share measures are constructed from INEI block-level SES data, which are themselves based on income imputations and categorical classifications. Measurement error in these local variables is likely to attenuate the estimated associations between local inequality, perceptions, and preferences. The geocoded analysis is further restricted to the 937 respondents for whom we could assign valid residential coordinates. Although we recalibrate the survey weights to account for missing geolocation data, the geocoded subsample is not perfectly balanced: the joint balance test is marginal, with some imbalance in gender and in the share of respondents who overestimate local affluence. For this reason, the local-inequality results should be interpreted as descriptive evidence on spatial heterogeneity rather than as causal estimates of the effect of local inequality. At the same time, the availability of three complementary local metrics, computed at different spatial scales, allows us to assess whether the observed associations between local inequality, perceptual biases, and redistributive preferences are consistent across alternative ways of characterising the local environment.

A second set of limitations concerns the interpretation of the information treatments. Our outcomes are stated preferences rather than revealed choices with material stakes. Stated support for redistribution may differ from behaviour in settings where respondents face actual costs, although the implication for treatment effects is less clear than for outcome levels. The experiment also does not re-elicit perceived SES or perceived district affluence after the information provision. We therefore cannot directly observe belief updating, nor can we decompose the treatment effect into the first-stage revision of perceptions and the subsequent revision of redistributive preferences. For this reason, we interpret the estimates as effects of providing corrective information to respondents with large perceived-actual gaps, rather than as direct estimates of belief updating.

Finally, some channels and subgroups remain difficult to separate with the available design. As discussed in Section 4, the experiment cannot distinguish material self-interest from status concerns: learning that one ranks lower than previously believed may change expected material returns to redistribution, perceived relative status, or both. The heterogeneity results should also be read cautiously, since some contrasts, especially those involving the small negative-bias group in the SES treatment, are underpowered. The clearest results are those for respondents with positive bias, particularly in the affluent-share treatment. External validity is another natural concern. Lima Metropolitana is a setting with steep spatial inequality and and SES categories are widely used as social classification. The qualitative finding that corrective information increases redistributive support among positively biased respondents is consistent with related evidence from other contexts, including Hoy et al. (2024a) and Hvidberg et al. (2023). The size of these effects, however, is likely to be context-dependent. In particular, it may vary with the steepness of local inequality, the visibility of affluence in everyday environments, and the extent to which class categories carry social meaning.

6.2 Conclusion

This paper studies how correcting perceptions of own socio-economic status and local affluence affects preferences for redistribution in Lima Metropolitana. Most respondents in our survey overestimate their own SES, and many also overestimate the share of affluent households in their district. Providing accurate information shifts redistributive preferences mainly among respondents with large positive misperceptions. The clearest effects arise in the affluent-share treatment: respondents who substantially overestimated the share of affluent households become more supportive of redistribution, and the effect on the preference for redistribution item survives both Romano-Wolf family-wise adjustment and Anderson sharpened-q adjustment. The SES treatment also increases support for reducing income inequality and inequality of opportunity among respondents who overestimated their own SES by at least two classes, although these effects are somewhat less robust. Neither treatment changes support for the wealth tax.

The pattern of results is consistent with an interpretation based on belief updating and self-interest, while leaving room for complementary status concerns. Effects are concentrated among positively biased respondents, as expected if the information lowers perceived relative position or increases the perceived tax base available for redistribution. The responses are also present among groups that are typically less favourable to redistribution, including right-leaning, individualistic, and low-trust respondents, although these heterogeneity patterns should be interpreted as suggestive. The geocoded data add a further dimension to the analysis. Respondents' perceptions are related to the socio-economic composition of their immediate residential environment, and local inequality appears more informative for understanding perceptions and perceptual bias than for directly predicting redistributive preferences in the absence of corrective information.

The findings highlight the importance of studying perceived class and local affluence, rather than only perceived position in the national income distribution. In settings such as Lima, broad SES categories are familiar social classifications, and local reference groups are likely to shape how individuals understand their own position and the extent of inequality around them. Information that speaks to these salient categories can shift redistributive attitudes when misperceptions are large. At the same time, the absence of effects on wealth-tax support suggests that simple factual corrections may be insufficient for policy instruments that depend on broader views about fairness, opportunity, trust, and the use of revenues. Future research could examine the durability of these updates, compare alternative framings, introduce behavioural outcomes with material stakes, and test more directly how local inequality and social comparison shape responses to information.

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Appendix

A Sample selection and descriptive statistics

Table A–1: Population and sample distribution by districts of Metropolitan Lima

Sub-area	District	Population		Sample		(%n)/(%N)
		N	%	n	%	
Lima Centro	Brena	66,737	1.01	13	0.99	0.98
Lima Centro	Lima	208,133	3.14	40	3.03	0.97
Lima Centro	Jesus Maria	61,901	0.93	8	0.61	0.65
Lima Centro	La Victoria	132,117	1.99	26	2.01	1.01
Lima Centro	Lince	44,876	0.68	9	0.68	1.01
Lima Centro	Magdalena del Mar	48,541	0.73	7	0.53	0.72
Lima Centro	Miraflores	83,418	1.26	18	1.36	1.08
Lima Centro	Pueblo Libre	67,929	1.02	13	1.01	0.98
Lima Centro	Rimac	131,167	1.98	30	2.32	1.17
Lima Centro	San Borja	92,849	1.40	15	1.14	0.81
Lima Centro	San Isidro	50,512	0.76	8	0.61	0.80
Lima Centro	San Luis	40,804	0.62	6	0.46	0.75
Lima Centro	San Miguel	122,417	1.85	27	2.05	1.11
Lima Centro	Santiago de Surco	260,245	3.93	45	3.41	0.87
Lima Centro	Surquillo	72,870	1.10	16	1.21	1.10
Total Lima Centro		1,484,516	22.39	281	21.73	0.97
Lima Este	Ate	422,579	6.37	82	6.34	0.99
Lima Este	El Agustino	144,046	2.17	31	2.35	1.08
Lima Este	La Molina	110,896	1.67	21	1.62	0.97
Lima Este	Lurigancho	165,874	2.50	31	2.40	0.96
Lima Este	San Juan de Lurigancho	742,577	11.20	152	11.76	1.05
Lima Este	Santa Anita	144,253	2.18	27	2.05	0.94
Total Lima Este		1,730,225	26.10	344	26.60	1.02
Lima Norte	Carabayllo	226,536	3.42	43	3.26	0.95
Lima Norte	Comas	378,048	5.70	80	6.19	1.08
Lima Norte	Independencia	154,467	2.33	26	1.97	0.85
Lima Norte	Los Olivos	245,638	3.71	48	3.71	1.00
Lima Norte	Puente Piedra	225,150	3.40	39	2.96	0.87
Lima Norte	San Martin de Porres	484,503	7.31	87	6.73	0.92
Total Lima Norte		1,714,342	25.86	323	24.98	0.97
Lima Sur	Chorrillos	232,700	3.51	50	3.79	1.08
Lima Sur	San Juan de Miraflores	263,905	3.98	51	3.94	0.99
Lima Sur	Villa El Salvador	279,389	4.21	62	4.80	1.14
Lima Sur	Villa Maria del Triunfo	282,596	4.26	48	3.64	0.85
Total Lima Sur		1,058,590	15.97	211	16.32	1.02
Callao	Callao	329,648	4.97	61	4.72	0.95
Callao	Bellavista	57,924	0.87	13	1.01	1.11
Callao	La Perla	47,570	0.72	11	0.85	1.19
Callao	Ventanilla	206,538	3.12	49	3.79	1.22
Total Callao		641,680	9.68	134	10.36	1.07
TOTAL	Total	6,629,353	100.00	1,293	100.00	1.00

Notes: The table reports the actual distribution of population by district and the distribution of the individuals in our selected sample in Metropolitan Lima. The population used in the sampling framework consists of people aged between 18 and 64 in 2020 living in 35 districts. 15 other districts were left out of the framework because they have small populations or are located in remote areas (seaside locations or near mountain areas). The population of these dropped districts represents 5.6% of the total population of Metropolitan Lima.

Table A–2: Balance of covariates

Variables	Control vs SES treatment		Control vs Affluent-share treatment		SES treatment vs Affluent-share treatment	
Male	0.053	(0.035)	0.039	(0.034)	0.015	(0.033)
Age 18-29	-0.014	(0.033)	0.016	(0.032)	-0.030	(0.031)
Age 30-44	0.048	(0.034)	-0.005	(0.032)	0.053	(0.032)
Age 45-64	-0.035	(0.032)	-0.012	(0.032)	-0.023	(0.031)
Married	0.058*	(0.035)	-0.011	(0.034)	0.069**	(0.033)
Per capita household income	0.037	(0.063)	-0.037	(0.062)	0.074	(0.061)
Primary education	0.001	(0.020)	0.013	(0.020)	-0.012	(0.019)
Secondary education	-0.063*	(0.035)	-0.045	(0.034)	-0.018	(0.034)
Tertiary education	0.063*	(0.034)	0.032	(0.033)	0.030	(0.033)
Employed	-0.005	(0.034)	0.014	(0.033)	-0.020	(0.033)
Self-employed	-0.013	(0.034)	-0.029	(0.033)	0.015	(0.033)
Student	-0.006	(0.022)	-0.022	(0.021)	0.017	(0.020)
Unemployed	-0.012	(0.011)	0.013	(0.012)	-0.024**	(0.011)
Other labour status	0.036	(0.023)	0.024	(0.022)	0.011	(0.023)
Informed	0.032	(0.035)	0.005	(0.034)	0.027	(0.033)
N	817		862		891	

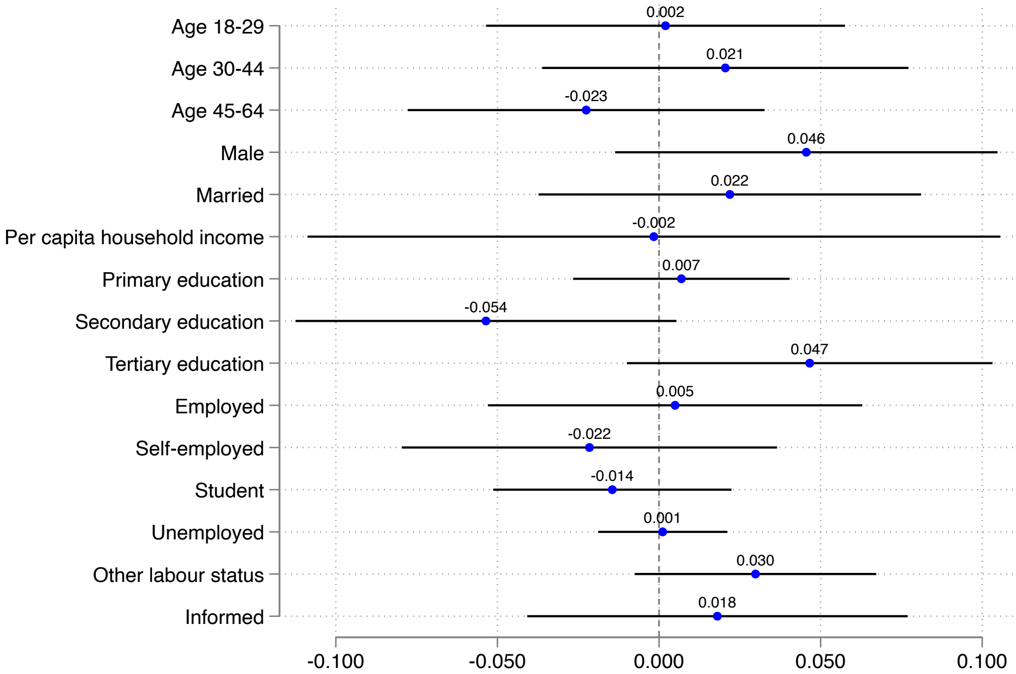
Notes: The table shows differences in means of covariates between experimental groups. Robust standard errors are shown in parentheses. *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$ indicate statistical significance.

Table A–3: Descriptive statistics

Variables	Control		SES treatment		Affluent-share treatment	
Preference for redistribution	0.473	(0.013)	0.486	(0.012)	0.517	(0.012)
Reduce income inequality	0.680	(0.009)	0.699	(0.009)	0.694	(0.008)
Introduce wealth tax	0.661	(0.010)	0.667	(0.009)	0.658	(0.009)
Reduce inequality of opportunity	0.740	(0.010)	0.761	(0.008)	0.763	(0.008)
Redistribution index	0.554	(0.008)	0.572	(0.008)	0.577	(0.008)
Poor due to circumstances	0.523	(0.025)	0.577	(0.024)	0.518	(0.023)
Rich due to advantages	0.462	(0.025)	0.506	(0.024)	0.474	(0.023)
Trust in Government	0.330	(0.024)	0.364	(0.023)	0.343	(0.022)
Govt take more responsibility	0.544	(0.025)	0.542	(0.024)	0.533	(0.023)
Right-leaning	0.483	(0.025)	0.520	(0.024)	0.451	(0.023)
Risk lover	0.622	(0.024)	0.673	(0.023)	0.595	(0.023)
Male	0.477	(0.025)	0.531	(0.024)	0.516	(0.023)
Age 18-29	0.326	(0.024)	0.312	(0.022)	0.342	(0.022)
Age 30-44	0.348	(0.024)	0.397	(0.024)	0.344	(0.022)
Age 45-64	0.326	(0.024)	0.291	(0.022)	0.314	(0.021)
Married	0.475	(0.025)	0.533	(0.024)	0.464	(0.023)
Per capita household income	6.216	(0.045)	6.253	(0.044)	6.180	(0.042)
Primary education	0.084	(0.014)	0.085	(0.014)	0.097	(0.014)
Secondary education	0.586	(0.025)	0.522	(0.024)	0.541	(0.023)
Tertiary education	0.330	(0.024)	0.392	(0.024)	0.362	(0.022)
Employed	0.374	(0.025)	0.368	(0.024)	0.388	(0.023)
Self-employed	0.387	(0.025)	0.373	(0.024)	0.358	(0.022)
Student	0.111	(0.016)	0.105	(0.015)	0.088	(0.013)
Unemployed	0.028	(0.008)	0.017	(0.006)	0.041	(0.009)
Other labour status	0.101	(0.015)	0.136	(0.017)	0.125	(0.015)
Informed	0.426	(0.025)	0.459	(0.024)	0.432	(0.023)

Notes: The table shows means of outcomes, beliefs, and covariates across experimental groups. Standard errors are shown in parentheses.

Figure A–1: Balance of covariates



Notes: The figure shows differences in means of covariates between the control group and both treatment groups. The intervals are based on a 95% level of confidence.

B Geo-referenced households and maps

Georeferencing

The survey asked each respondent to provide their household address or, alternatively, the closest identifiable point, such as a street intersection, park, or bus stop. This question was not mandatory, and some individuals chose not to provide any location information.

Starting from the initial sample of 1,319 individuals, 212 respondents did not report an address or local reference point. Geo-coordinates for the remaining 1,107 observations were obtained using the Google API. Because this tool is not entirely reliable, a manual verification and correction process was carried out. A total of 236 addresses were corrected because their initial coordinates fell outside the respondent's district (i.e., errors due to the Google API, while misspecified addresses in the survey were discarded). The cases of low precision addresses were also manually verified and corrected.

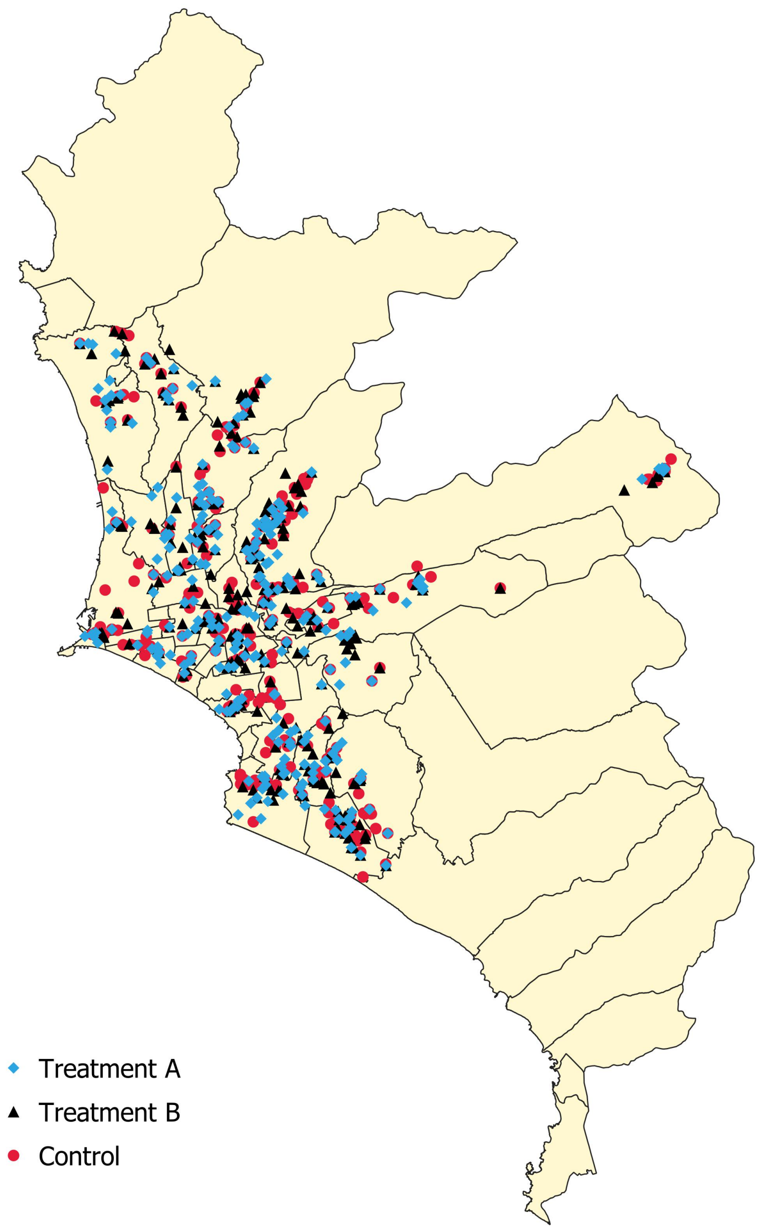
Subsequently, 39 addresses were discarded due to insufficient information (e.g., incomplete or vague entries), and 117 were excluded because they were located outside the respondent's declared district. After these adjustments, the sample included 951 observations. After further excluding cases with extreme interview durations, the final sample used for the local inequality analysis consisted of 937 individuals.

Table B–1: Distribution of observations with geolocation

District	All		With geo-data (unweighted)		With geo-data (weighted)
	N	%	N	%	%
Ate	82	6.34	54	5.76	6.37
Bellavista	13	1.01	6	0.64	0.96
Breña	13	1.01	5	0.53	0.98
Callao	61	4.72	40	4.27	4.76
Carabayllo	43	3.33	32	3.42	3.37
Chorrillos	50	3.87	36	3.84	3.86
Comas	80	6.19	42	4.48	6.27
El Agustino	31	2.40	19	2.03	2.42
Independencia	26	2.01	25	2.67	2.02
Jesus-Maria	8	0.62	5	0.53	0.63
La Molina	21	1.62	16	1.71	1.62
La Perla	11	0.85	6	0.64	0.88
La Victoria	26	2.01	25	2.67	2.02
Lima	40	3.09	35	3.74	3.12
Lince	9	0.70	7	0.75	0.70
Los Olivos	48	3.71	30	3.20	3.71
Lurigancho	31	2.40	27	2.88	2.41
Magdalena del Mar	7	0.54	6	0.64	0.54
Miraflores	18	1.39	17	1.81	1.40
Pueblo Libre	13	1.01	6	0.64	0.99
Puente Piedra	39	3.02	36	3.84	3.03
Rímac	30	2.32	17	1.81	2.29
San Borja	15	1.16	9	0.96	1.22
San Isidro	8	0.62	4	0.43	0.59
San Juan de Lurigancho	152	11.76	127	13.55	11.85
San Juan de Miraflores	51	3.94	45	4.80	3.97
San Luis	6	0.46	5	0.53	0.47
San Martín de Porres	87	6.73	25	2.67	6.20
San Miguel	27	2.09	25	2.67	2.10
Santa Anita	27	2.09	20	2.13	2.10
Santiago de Surco	45	3.48	39	4.16	3.50
Surquillo	16	1.24	10	1.07	1.24
Ventanilla	49	3.79	40	4.27	3.82
Villa El Salvador	62	4.80	54	5.76	4.82
Villa María del Triunfo	48	3.71	42	4.48	3.74
Total	1293	100.00	937	100.00	100.00

Notes: The table reports the distribution of the sample by districts before and after the processing of geolocation data.

Figure B–1: Distribution of control and treatment groups



C Tables and figures of bias analysis

Table C–1: Objective and perceived own-SES, and bias by SES

Actual SES	Perceived SES	Actual SES (level)	Bias	Proportion with positive bias	Proportion with negative bias
Low	2.434	1	1.434	0.897	0.000
Lower-middle	2.624	2	0.624	0.591	0.043
Middle	2.879	3	-0.121	0.169	0.274
Upper-middle	3.040	4	-0.960	0.013	0.747
Upper	3.234	5	-1.766	0.000	0.953

Notes: The bias is defined as the difference between the perceived and actual SES.

Table C–2: Objective and perceived percentage of affluent households, and bias by district

District affluence level	Perceived % affluent hhs	Objective % affluent hhs	Bias	Proportion with positive bias	Proportion with negative bias
Low (<10%)	26.553	2.707	23.846	0.933	0.041
Middle(10-50%)	29.835	23.048	6.787	0.562	0.421
High (>50%)	44.075	89.717	-45.642	0.034	0.958

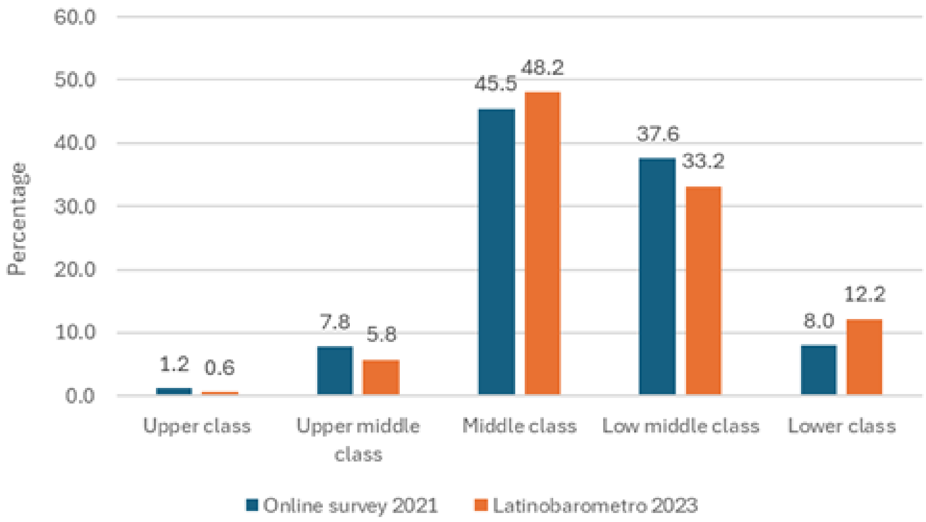
Notes: The bias is defined as the difference between the perceived and actual percentages of affluent households (upper and upper-middle classes) in each district. Districts are classified by their actual share of affluent households: *low* (below 10%), *middle* (10–50%), and *high* (above 50%).

Table C–3: Local inequality metrics and bias

	SES bias	SES bias (abs value)	Affluent-share bias	Affluent-share bias (abs value)
Buffer 300 mts.				
GE2	0.270 ***	0.074	3.323	-1.317
Coef. Var.	0.050 ***	0.013	0.556	-0.250
%Affluent households	0.366 ***	0.100	4.256	-5.770 **
Buffer 500 mts.				
GE2	0.271 **	0.114	3.025	1.289
Coef. Var.	0.050 **	0.021	0.624	0.190
%Affluent households	0.396 ***	0.128 *	4.200	-6.167 **
Buffer 1 Km.				
GE2	0.203 *	0.125	3.614	3.890
Coef. Var.	0.039 *	0.025	0.867	0.695
%Affluent households	0.434 ***	0.146 *	2.267	-4.119

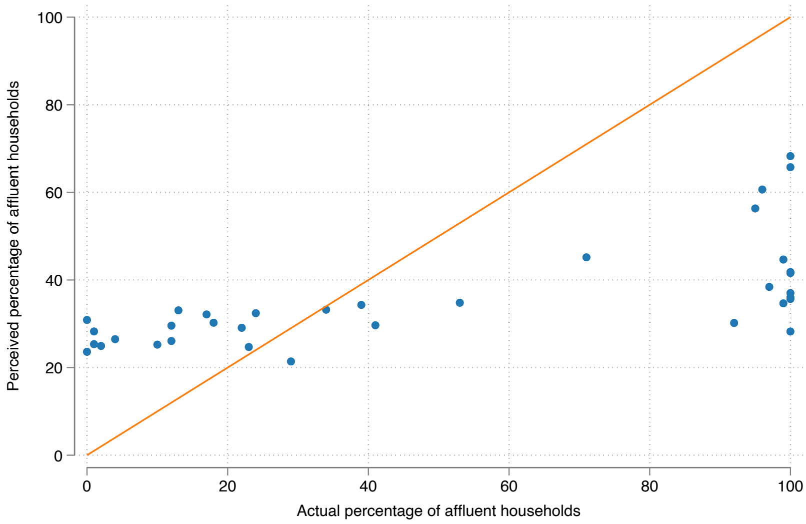
Notes: The table reports the coefficients of inequality metrics included as a covariate in the bias regressions reported in Table 2. Each cell displays the results of a regression that incorporates a single type of inequality metric alongside the other covariates included in the bias regressions. These inequality metrics are calculated for households within buffer zones of 300, 500, or 1,000 meters around the respondent's location. The dependent variable in columns 1 and 2 is the bias in the respondent's SES, calculated as the difference between the perceived SES and the actual SES (measured in SES group levels). In columns 3 and 4, the dependent variable is the bias in the estimated percentage of upper or upper-middle-class households in the respondent's district, defined as the difference between the perceived and actual percentages. Regressions in columns 1 and 2 include fixed effects for the actual SES group, while regressions in columns 3 and 4 include fixed effects for the actual share of rich households in the district. All regressions use calibrated weights to account for observations with no geolocation data. The regressions use robust standard errors, and *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$ indicate statistical significance.

Figure C–1: Self-reported social class in Lima



Notes: The figure shows the distribution of self-reported social classes in our online survey (Oct-Nov-2021) and in the 2023 wave of the Latinobarómetro (see www.latinobarometro.org). The Latinobarómetro values refer to respondents aged 18–64 residing in Lima and are weighted using survey weights. Data were collected between February and March 2023.

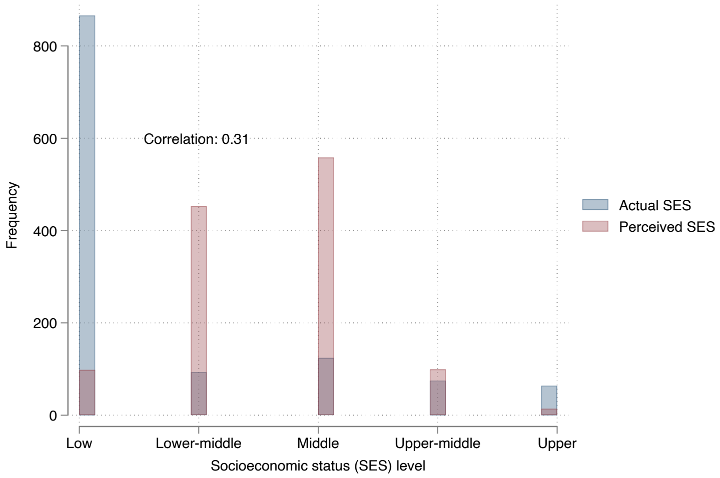
Figure C–2: Actual and perceived percentage of affluent households by district



Notes: The figure compares district-level averages of actual and perceived proportions of affluent households (upper and upper-middle classes).

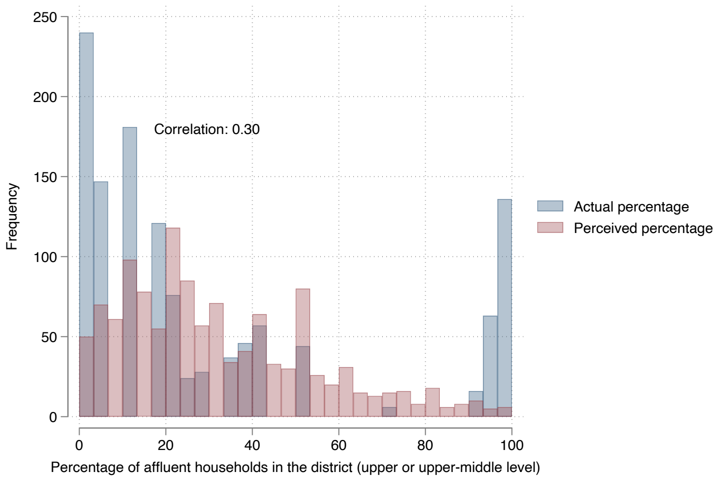
Figure C–3: Actual and perceived relative economic position

(a) Objective and Perceived SES



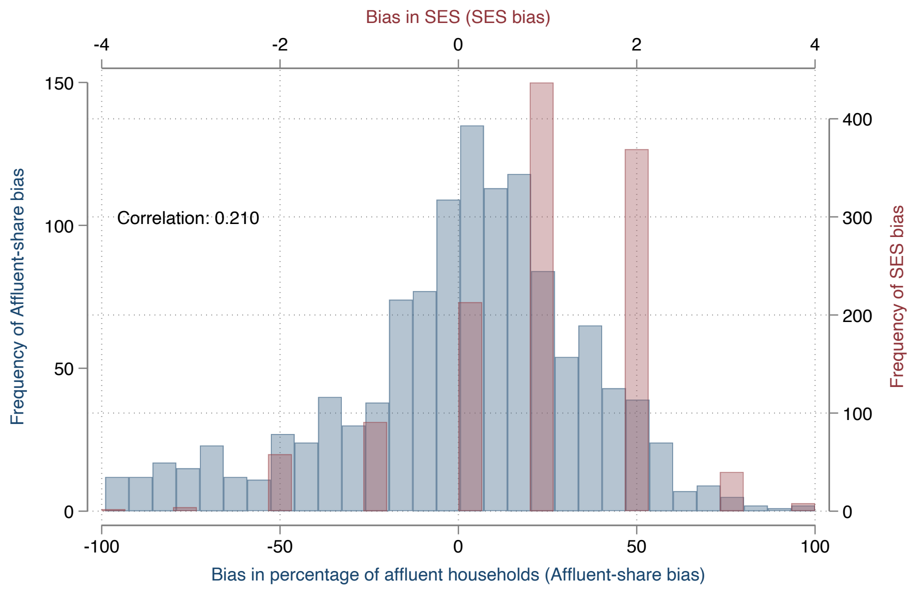
Notes: The figure displays the distributions for perceived and objective own SES in their district. The number of observations is 1,222. A significant portion of low SES individuals place themselves in higher positions than they actually occupy, while only Upper SES individuals underestimate their rank (similar results to Cruces et al. (2013)) and reverse results to Karadja et al. (2017)

(b) Objective and perceived % of rich households



Notes: The figure displays the relation between perceived and objective percentages of upper and upper-middle households in the district. The number of observations is 1,222.

Figure C–4: Correlation of two types of biases



D Average effects - extended regressions

This section reports extended versions of the main average-effects tables. For each treatment arm, the tables show results using both the main bias threshold and the more permissive sensitivity threshold.

Table D–1: Average effects of the SES treatment, extended (both bias thresholds)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual SES differ by at least one class					
Treated x Neg. bias	-0.011	0.029	0.035	0.044	0.033
	(0.049)	(0.039)	(0.040)	(0.037)	(0.038)
Treated x Pos. bias	0.012	0.026*	0.003	0.023	0.020
	(0.021)	(0.014)	(0.016)	(0.015)	(0.013)
Treated x No bias	0.036	-0.017	-0.005	-0.006	-0.003
	(0.045)	(0.030)	(0.034)	(0.030)	(0.029)
Neg. bias	0.044	-0.037	-0.108***	-0.048	-0.054*
	(0.051)	(0.032)	(0.038)	(0.035)	(0.032)
Pos. bias	0.051	-0.021	-0.042	-0.038	-0.021
	(0.037)	(0.024)	(0.030)	(0.026)	(0.025)
Constant	0.432***	0.700***	0.705***	0.773***	0.576***
	(0.034)	(0.022)	(0.027)	(0.023)	(0.023)
Observations	815	816	816	818	810
Panel B: Bias when perceived and actual SES differ by at least two classes					
Treated x Neg. bias	-0.116	0.086	0.050	0.123**	0.063
	(0.076)	(0.060)	(0.062)	(0.055)	(0.057)
Treated x Pos. bias	-0.004	0.052**	0.025	0.048**	0.039**
	(0.030)	(0.021)	(0.022)	(0.021)	(0.019)
Treated x No bias	0.036	-0.005	-0.009	-0.007	0.002
	(0.024)	(0.016)	(0.018)	(0.017)	(0.015)
Neg. bias	0.101*	-0.037	-0.060	-0.067*	-0.031
	(0.056)	(0.036)	(0.045)	(0.037)	(0.034)
Pos. bias	0.031	-0.018	-0.011	-0.049**	-0.017
	(0.027)	(0.019)	(0.021)	(0.020)	(0.018)
Constant	0.455***	0.689***	0.669***	0.762***	0.563***
	(0.018)	(0.012)	(0.014)	(0.013)	(0.012)
Observations	815	816	816	818	810

Notes: This table is an extended version of Table 3. In Panel A bias is defined as perceiving one's SES to be at least one level lower (negative) or higher (positive) than the actual SES. Panel B applies the primary threshold of at least two SES levels and reproduces Table 3. Robust standard errors are in parentheses. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$ indicate statistical significance.

Table D–2: Average effects of the affluent-share treatment, extended (both bias thresholds)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual share differ by at least 5%					
Treated x Neg. bias	0.037	-0.019	-0.019	0.002	-0.004
	(0.031)	(0.019)	(0.023)	(0.019)	(0.019)
Treated x Pos. bias	0.060**	0.025	0.012	0.037**	0.042***
	(0.024)	(0.017)	(0.020)	(0.018)	(0.016)
Treated x No bias	0.010	0.045	-0.020	0.021	0.013
	(0.048)	(0.033)	(0.036)	(0.033)	(0.031)
Neg. bias	-0.027	0.047	-0.011	0.018	0.006
	(0.044)	(0.029)	(0.033)	(0.031)	(0.028)
Pos. bias	-0.013	0.016	-0.009	-0.023	-0.016
	(0.042)	(0.029)	(0.032)	(0.031)	(0.028)
Constant	0.489***	0.656***	0.670***	0.746***	0.561***
	(0.038)	(0.027)	(0.028)	(0.027)	(0.025)
Observations	858	857	857	859	849
Panel B: Bias when perceived and actual share differ by at least 10%					
Treated x Neg. bias	0.057*	-0.013	-0.026	0.002	-0.000
	(0.033)	(0.021)	(0.025)	(0.020)	(0.020)
Treated x Pos. bias	0.069***	0.034*	0.025	0.041**	0.054***
	(0.026)	(0.019)	(0.022)	(0.020)	(0.018)
Treated x No bias	-0.002	0.008	-0.029	0.010	-0.007
	(0.035)	(0.022)	(0.025)	(0.023)	(0.021)
Neg. bias	-0.047	0.012	-0.046*	-0.021	-0.033
	(0.036)	(0.022)	(0.026)	(0.024)	(0.021)
Pos. bias	-0.020	-0.030	-0.056**	-0.063***	-0.061***
	(0.033)	(0.022)	(0.025)	(0.024)	(0.021)
Constant	0.495***	0.690***	0.699***	0.773***	0.591***
	(0.027)	(0.017)	(0.019)	(0.018)	(0.016)
Observations	858	857	857	859	849

Notes: This table is an extended version of Table 4. In Panel A bias is defined using a ±5 percentage point threshold: negative (positive) bias if the perceived district share of affluent households is at least 5 pp lower (higher) than the actual share. Panel B applies the primary threshold of ±10 percentage points and reproduces Table 4. Robust standard errors are in parentheses. *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$ indicate statistical significance.

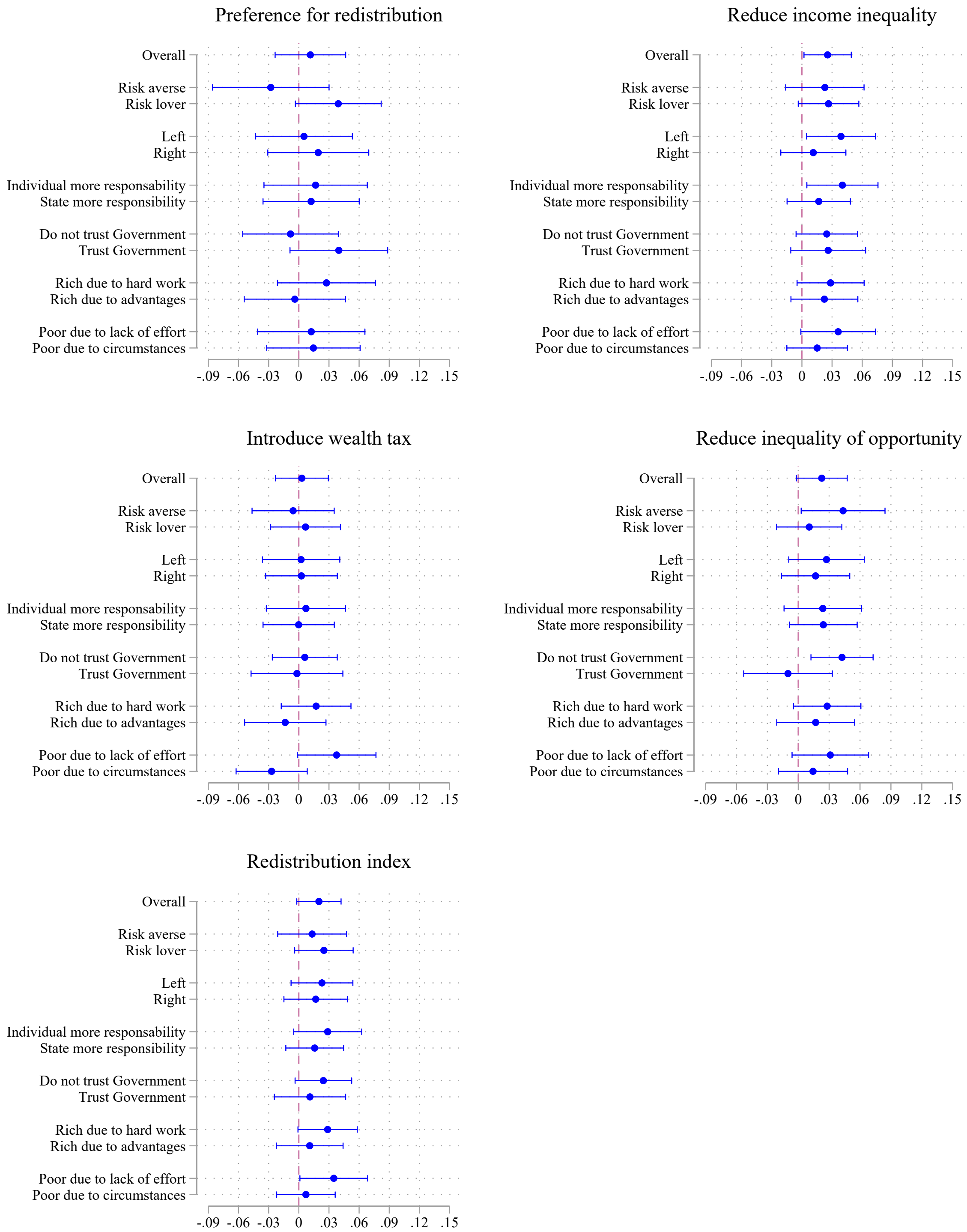
E Heterogeneous effects by basic beliefs (sample of respondents with positive bias)

This section reports the heterogeneous treatment effects on redistributive attitudes, focusing on individuals who overestimated either their socio-economic status (SES) or the proportion of affluent households in their district (positive bias). The analysis explores how these effects vary across key pre-treatment beliefs, including fairness views, risk preferences, political ideology, trust in government, and perceptions of individual responsibility. Heterogeneous effects are estimated using separate linear regression models for each outcome variable. Each specification includes the treatment indicator, an indicator capturing the specific belief, and an interaction term between these two variables. The estimated equation is as follows:

$$y_{ji} = \beta_0 + \beta_1\ T + \beta_2 T \times belief_i + \beta_3 belief_i + \varepsilon_i \tag{D.1}$$

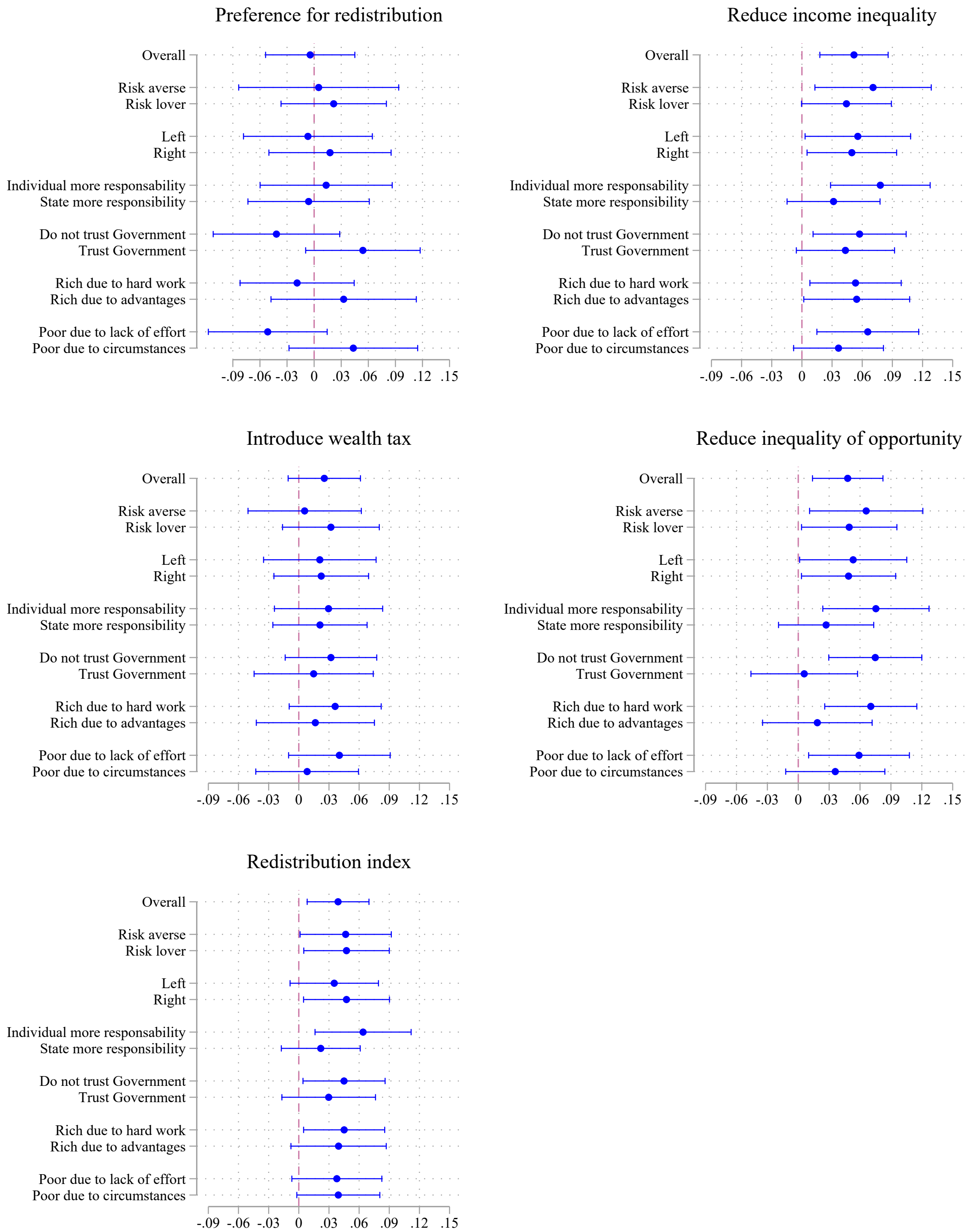
where y_{ji} denotes one of the j outcome variables measuring preferences for redistribution for individual i, and T equals 1 if the respondent received the treatment and 0 otherwise. The regressions are estimated on the subsample of individuals exhibiting a positive bias.

Figure E–1: Effects of the SES treatment by prior beliefs (sample of subjects with positive bias of at least one SES group)



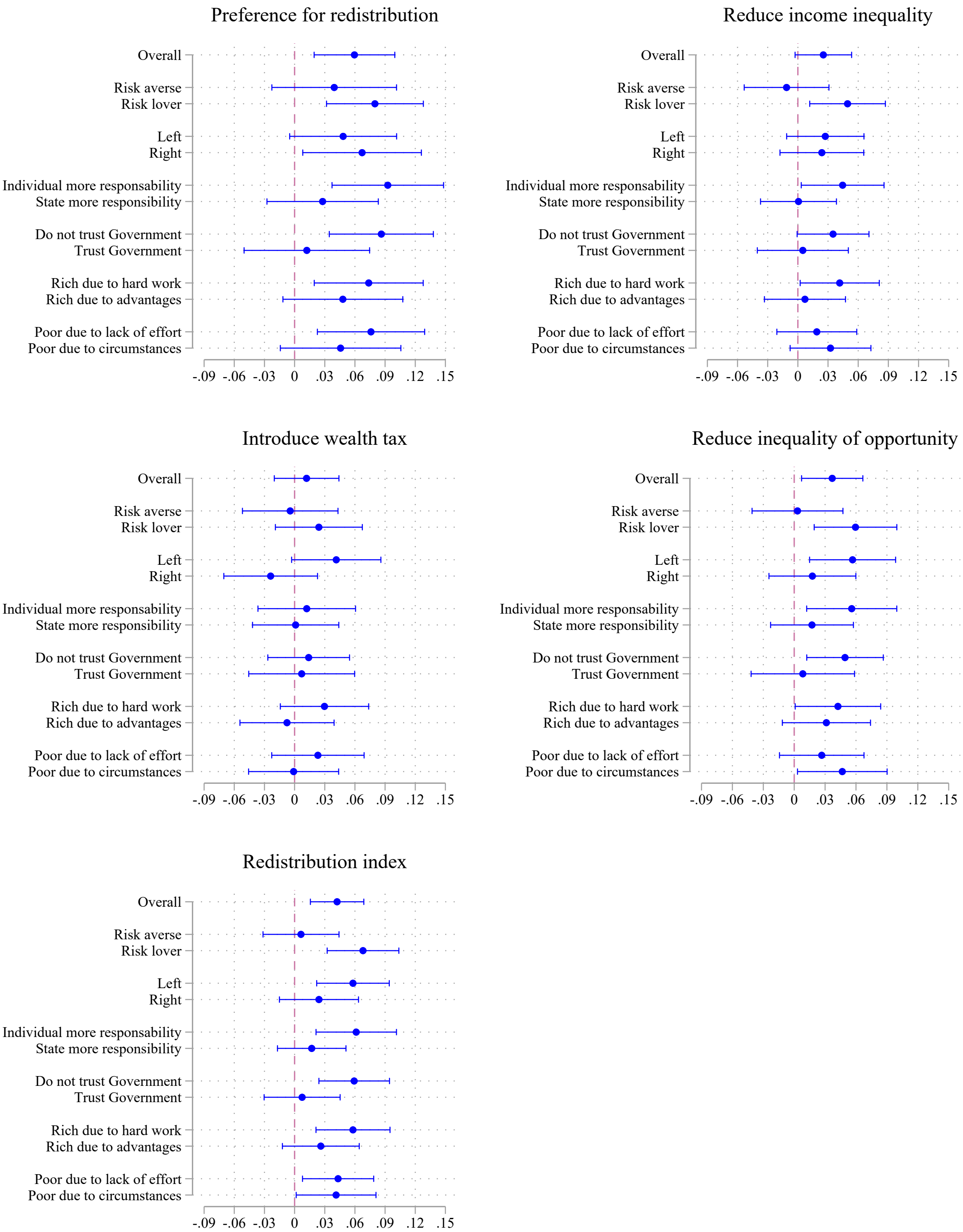
Notes: The figure plots the heterogeneous effects of the SES treatment on outcomes capturing preferences for redistribution. The sample consists of individuals who overestimated (positive bias) their SES by at least one SES group. The heterogeneous effects are estimated from separate linear regressions for each type of prior belief. The equations regress the outcome on the treatment, the belief and the interaction of these two variables. The bars indicate 90% confidence intervals.

Figure E–2: Effects of the SES treatment by prior beliefs (sample of subjects with positive bias of at least two SES groups)



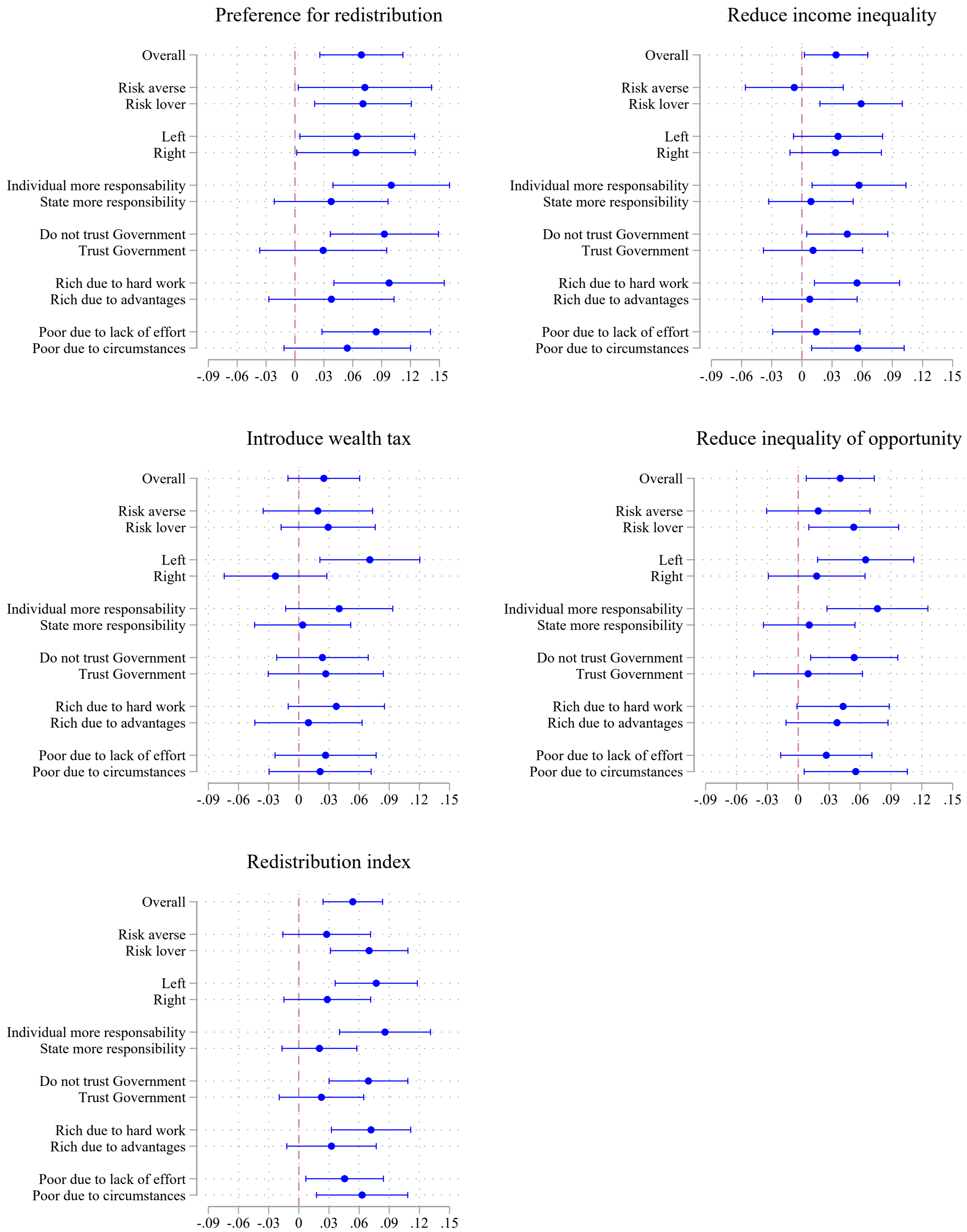
Notes: The figure plots the heterogeneous effects of the SES treatment on outcomes capturing preferences for redistribution. The sample consists of individuals who overestimated (positive bias) their SES by at least two SES groups. The heterogeneous effects are estimated from separate linear regressions for each type of prior belief. The equations regress the outcome on the treatment, the belief and the interaction of these two variables. The bars indicate 90% confidence intervals.

Figure E–3: Effects of the affluent-share treatment by prior beliefs (sample of subjects with positive bias of at least 5%)



Notes: The figure plots the heterogeneous treatment effects of the district share of high class people on outcomes capturing preferences for redistribution. The sample consists of individuals who overestimated (positive bias) the share of upper and upper-middle class in their district of residence by at least 5%. The heterogeneous effects are estimated from separate linear regressions for each type of prior belief. The equations regress the outcome on the treatment, the belief and the interaction of these two variables. The bars indicate 90% confidence intervals.

Figure E–4: Effects of the affluent-share treatment by prior beliefs (sample of subjects with positive bias of at least 10%)



Notes: The figure plots the heterogeneous treatment effects of the district share of high class people on outcomes capturing preferences for redistribution. The sample consists of individuals who overestimated (positive bias) the share of upper and upper-middle class in their district of residence by at least 10%. The heterogeneous effects are estimated from separate linear regressions for each type of prior belief. The equations regress the outcome on the treatment, the belief and the interaction of these two variables. The bars indicate 90% confidence intervals.

F Heterogeneous treatment effects by level of local inequality (sample of respondents with positive bias)

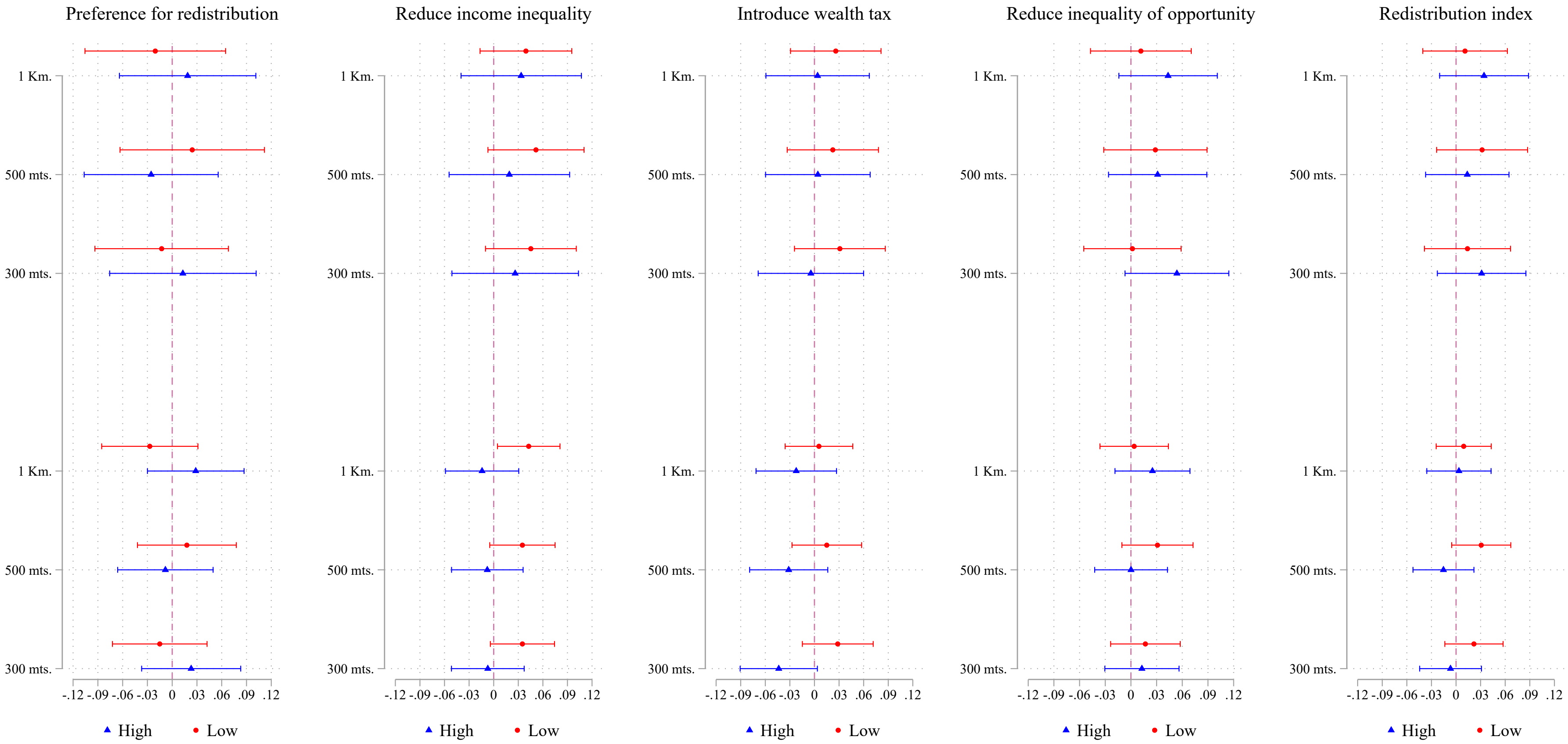
This section reports the results on heterogeneous treatment effects in redistributive attitudes, focusing on individuals who exhibited a positive bias, that is, those who overestimated either their socio-economic status (SES) or the proportion of affluent households in their district. The analysis investigates how these effects vary with the degree of local inequality, classified as *high* when the inequality metric within a given buffer zone exceeds the sample median across comparable zones, and *low* otherwise. Heterogeneous effects are estimated using separate linear regression models for each outcome variable, including the treatment indicator, an indicator for high local inequality, and their interaction term. The estimated equation is as follows:

$$y_{ji} = \beta_0 + \beta_1 T + \beta_2 T \times highineq_i + \beta_3 highineq_i + \varepsilon_i \tag{F.1}$$

where y_{ji} denotes one of the j outcome variables capturing preferences for redistribution for individual i, and T equals 1 if the respondent received the treatment and 0 otherwise. The variable *highineq* takes the value 1 when the level of inequality within the participant's buffer zone is above the sample median and 0 otherwise. Local inequality metrics are computed for three alternative buffer zones: 300, 500, and 1,000 meters around each respondent's household.

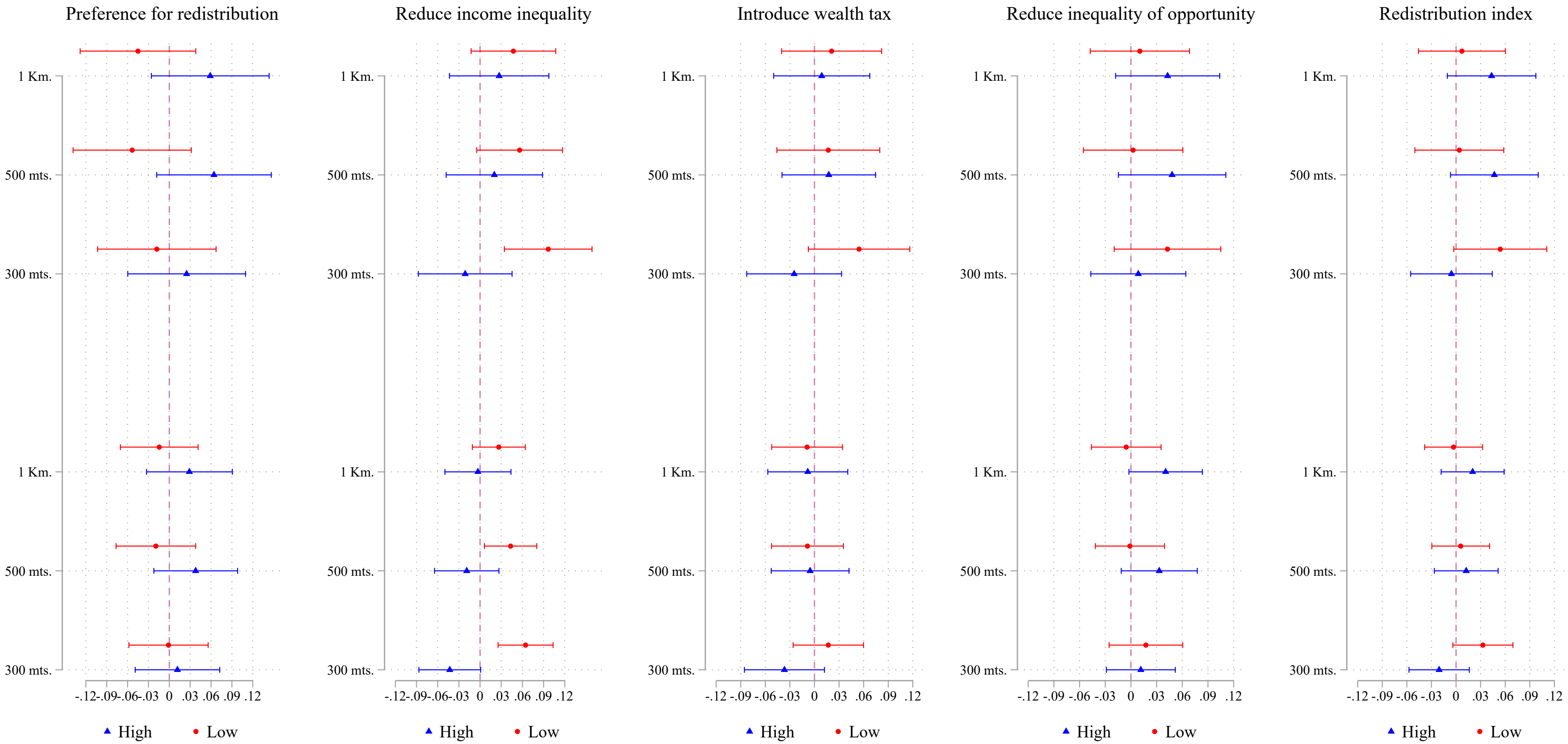
This appendix reports results using GE(2) and the local share of affluent households (upper and upper-middle class). Results using the coefficient of variation (CV) are not shown, as they are effectively the same as those for GE(2) due to the high correlation between the two measures.

Figure F–1: Effects of the SES treatment by levels of inequality (using the GE(2) of the buffer)



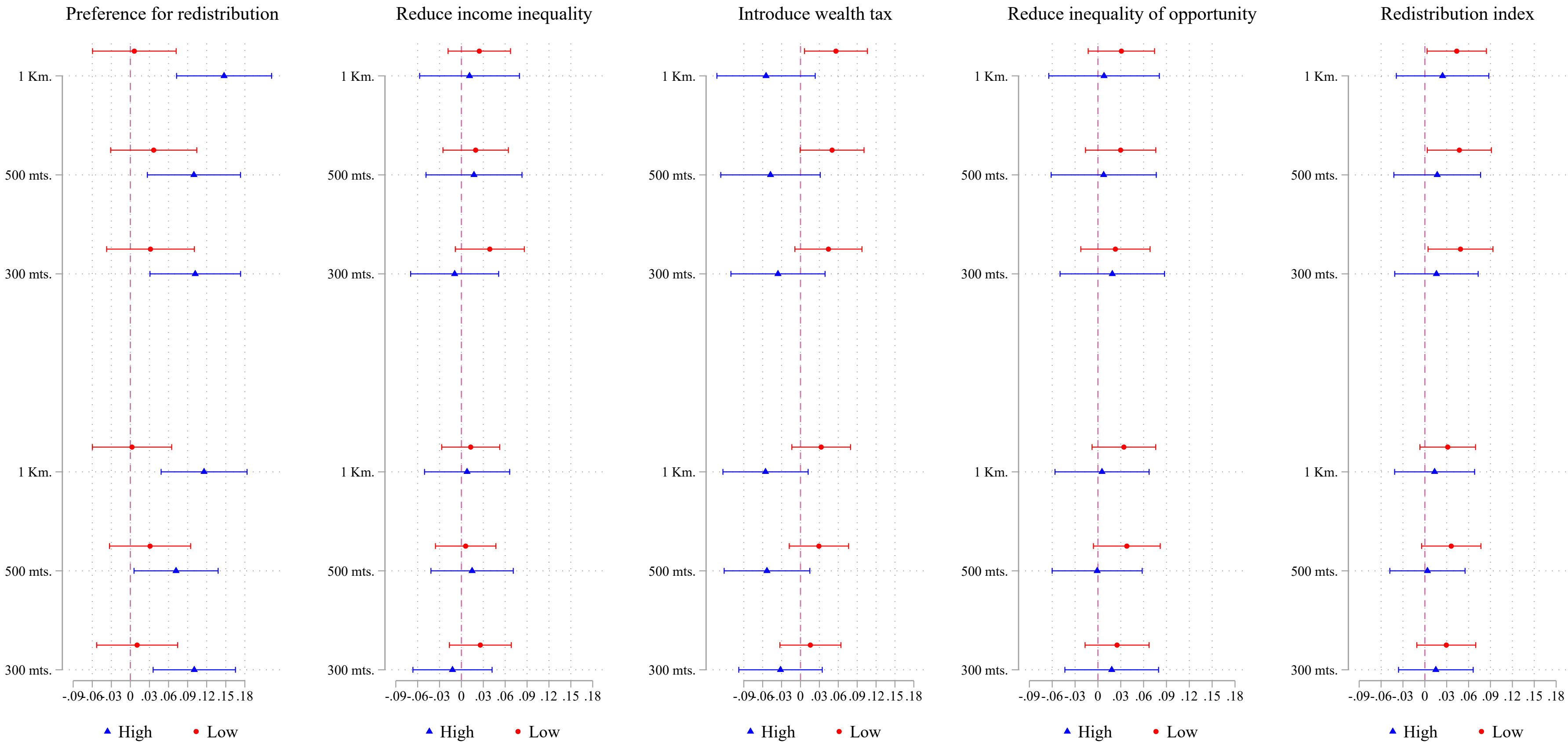
Notes: The figure shows the heterogeneous effects of the SES treatment on outcomes related to preferences for redistribution, focusing on individuals who overestimated their SES (positive bias). The heterogeneous effects are estimated from separate linear regressions for each outcome. Each regression includes the treatment indicator, an indicator for the level of local inequality (high or low), and their interaction term. The level of inequality is classified as *high* when the inequality metric within a given buffer zone exceeds the sample median for zones of equivalent size, and *low* otherwise. The bottom panels correspond to cases where positive bias is defined as overestimating one's own SES by at least one level; the top panels correspond to cases where positive bias is defined as overestimating by at least two levels. Bars indicate 90% confidence intervals. All regressions use calibrated weights to account for respondents without geolocation data.

Figure F–2: Effects of the SES treatment by levels of inequality (using the share of rich households in the buffer)



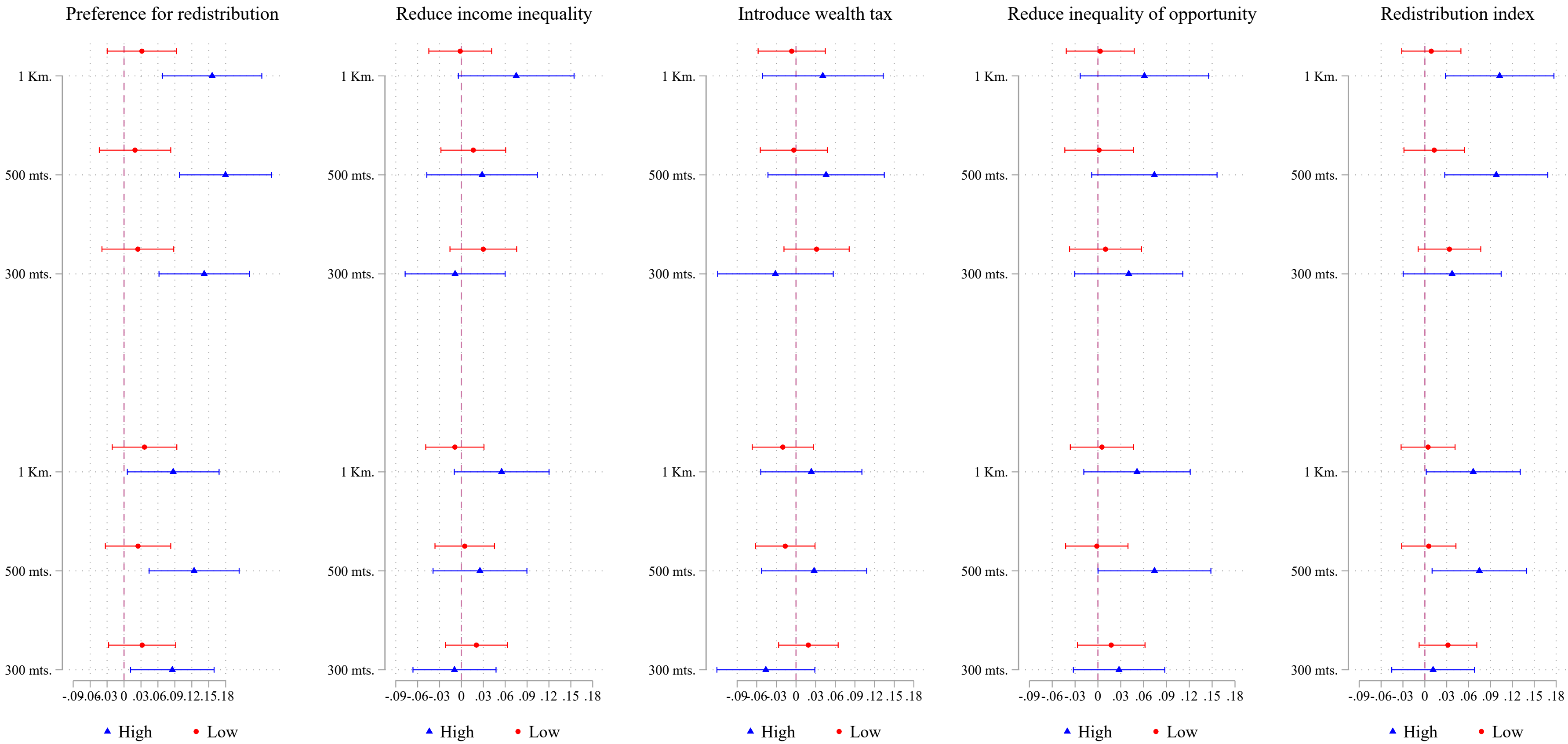
Notes: The figure shows the heterogeneous effects of the SES treatment on outcomes related to preferences for redistribution, focusing on individuals who overestimated their SES (positive bias). The heterogeneous effects are estimated from separate linear regressions for each outcome. Each regression includes the treatment indicator, an indicator for the level of local inequality (high or low), and their interaction term. The level of inequality is classified as *high* when the inequality metric within a given buffer zone exceeds the sample median for zones of equivalent size, and *low* otherwise. The bottom panels correspond to cases where positive bias is defined as overestimating one's own SES by at least one level; the top panels correspond to cases where positive bias is defined as overestimating by at least two levels. Bars indicate 90% confidence intervals. All regressions use calibrated weights to account for respondents without geolocation data.

Figure F–3: Effects of the affluent-share treatment by levels of inequality (using the GE(2) of the buffer)



Notes: The figure shows the heterogeneous effects of the affluent-share treatment on outcomes related to preferences for redistribution, focusing on individuals who overestimated the share of affluent households in their district (positive bias). The heterogeneous effects are estimated using separate linear regressions for each outcome. Each regression includes the treatment indicator, an indicator for the level of local inequality (high or low), and their interaction term. The level of inequality is classified as *high* when the inequality metric within a given buffer zone exceeds the sample median for zones of equivalent size, and *low* otherwise. The bottom panels correspond to cases where positive bias is defined as overestimating the share of rich households in the district by at least 5 pp; the top panels correspond to cases where positive bias is defined as overestimating by at least 10 pp. Bars represent 90% confidence intervals. All regressions use calibrated weights to account for respondents without geolocation data.

Figure F–4: Effects of the affluent-share treatment by levels of inequality (using the share of rich households in the buffer)



Notes: The figure shows the heterogeneous effects of the affluent-share treatment on outcomes related to preferences for redistribution, focusing on individuals who overestimated the share of affluent households in their district (positive bias). The heterogeneous effects are estimated using separate linear regressions for each outcome. Each regression includes the treatment indicator, an indicator for the level of local inequality (high or low), and their interaction term. The level of inequality is classified as *high* when the inequality metric within a given buffer zone exceeds the sample median for zones of equivalent size, and *low* otherwise. The bottom panels correspond to cases where positive bias is defined as overestimating the share of rich households in the district by at least 5 pp; the top panels correspond to cases where positive bias is defined as overestimating by at least 10 pp. Bars represent 90% confidence intervals. All regressions use calibrated weights to account for respondents without geolocation data.

G Robustness checks and additional results

G.1 Focal-family multiple-hypothesis testing

Our confirmatory multiple-hypothesis-testing procedure is defined by the focal family associated with hypothesis H1. This family consists of the four pre-specified redistributive attitude outcomes for respondents with a positive baseline bias at the main threshold: at least two SES levels for the SES treatment (T1), and at least ten percentage points for the affluent-share treatment (T2). We implement inference in two steps. First, we use the composite redistribution index as an aggregate test of H1. The positive-bias coefficient on the redistribution index is significant for both treatments under the same no-covariate specification as the main regressions: $p = 0.037$ for T1 (five percent) and $p = 0.003$ for T2 (one percent). Second, we use the four component outcomes of the index and adjust inference within this focal family. Our main family-wise correction is the Romano-Wolf step-down bootstrap (Romano and Wolf, 2005; Clarke et al., 2020), implemented separately by treatment arm. As a companion false-discovery-rate adjustment, we report the sharpened two-stage q-values of Benjamini et al. (2006), following Anderson (2008). We also report a four-degree-of-freedom joint Wald test of the null that all four positive-bias coefficients are zero, estimated by seemingly unrelated regressions with cross-equation robust standard errors. The joint test is a quadratic statistic that loses power when effects are concentrated in a subset of correlated outcomes, so we gate on the one-degree-of-freedom index and report multiplicity-adjusted p-values per outcome. Both treatments retain significant redistributive effects after correction: the affluent-share (T2) effect on the preference for redistribution clears the Romano-Wolf family-wise correction at five percent ($p = 0.041$) and the conservative twelve-hypothesis bound at ten percent ($p = 0.093$), while the SES (T1) effects on reducing inequality and on reducing inequality of opportunity clear at five and ten percent (Romano-Wolf $p = 0.046$ and 0.066; Anderson $q = 0.044$).

As an additional robustness check, Section G.2 reports the experiment-specific multiple-testing procedure of List et al. (2019), implemented with `mhtexp` in Stata. This procedure is tailored to multi-arm experimental designs, but its implementation relies on bivariate treatment-versus-control comparisons without heteroskedasticity-robust standard errors, so its specification differs slightly from the main multiplicity-adjusted table.

Table G–1: Focal-family multiple-hypothesis testing, positive-bias respondents

Outcome	Coef. (1)	SE (2)	Unadj. p-value (3)	RW p-value (4)	Anders. p-value (5)
Panel A: SES treatment (T1), ≥ 2 levels, positive bias ($N = 284$)					
Preference for redistribution	-0.004	0.030	0.886	0.872	0.443
Reduce inequality	0.052	0.021	0.012	**0.046****	**0.044****
Wealth tax	0.025	0.022	0.246	0.429	0.164
Reduce inequality of opportunity	0.048	0.021	0.021	**0.066***	**0.044****
Panel B: Affluent-share treatment (T2), ≥ 10 pp, positive bias ($N = 360$)					
Preference for redistribution	0.069	0.026	0.009	**0.041****	**0.036****
Reduce inequality	0.034	0.019	0.076	0.141	**0.076***
Wealth tax	0.025	0.022	0.252	0.247	0.113
Reduce inequality of opportunity	0.041	0.020	0.042	0.101	**0.064***

Notes: Column 1 reports the `Treat` × `Positive bias` coefficient from the main specification without covariates, identical to the main average-effects tables (Tables 3 and 4) estimated on each outcome's own sample, while column 2 and 3 report their corresponding robust standard errors and unadjusted p-value. column 4 reports Romano-Wolf step-down adjusted p-values (Romano and Wolf, 2005; Clarke et al., 2020). Column 5 reports sharpened two-stage q-values (Benjamini et al., 2006; Anderson, 2008) computed by the BKY adaptive linear step-up procedure over the same four-outcome family. * $p < 0.10$, ** $p < 0.05$ indicate statistical confidence levels under the indicated correction. The four-degree-of-freedom joint Wald test that all four positive-bias coefficients are zero gives $p = 0.126$ for T1 and $p = 0.010$ for T2. For reference, applying the same Romano-Wolf step-down procedure over a broader twelve-hypothesis grid that also includes the negative-bias and no-bias treatment-interaction coefficients not predicted by H1 still leaves the T2 preference-for-redistribution effect significant at the ten percent level ($p = 0.093$); none of the T1 coefficients clears that wider correction.

G.2 LSX robustness for the focal family

As a robustness check we also report the experiment-specific family-wise correction of List et al. (2019), implemented with `mhtexp` in Stata (with 3,000 bootstrap replications), applied to the same four-outcome positive-bias family. The Stata implementation of List et al. (2019) does not accept robust variance options, so the LSX p-values reported in Table G–2 are computed from bivariate treatment-vs-control differences in means with an observation-level bootstrap, rather than from the robust full-interaction specification used in Table G–1. Both specifications are estimated without covariates. The LSX procedure is a refinement of the same Romano-Wolf step-down max-t family used in Table G–1, tailored to multi-arm experimental designs. The conclusions agree with the Romano-Wolf bound on every binding outcome.

Table G–2: Focal-family LSX robustness, positive-bias respondents

Outcome	Mean diff (1)	Unadj. p-value (2)	LSX p-value (3)	BH p-value (4)	BY p-value (5)
Panel A: SES (T1), ≥ 2 levels, positive bias ($N = 284$)					
Preference for redistribution	0.004	0.884	0.884	0.884	1.000
Reduce inequality	0.052	0.014	**0.052***	0.051	0.106
Wealth tax	0.025	0.239	0.418	0.319	0.664
Reduce inequality of opportunity	0.048	0.025	**0.071***	0.051	0.106
Panel B: Affluent share (T2), ≥ 10 pp, positive bias ($N = 360$)					
Preference for redistribution	0.069	0.009	**0.033****	0.035	0.072
Reduce inequality	0.034	0.076	0.136	0.101	0.210
Wealth tax	0.025	0.239	0.239	0.239	0.497
Reduce inequality of opportunity	0.041	0.045	0.118	0.089	0.186

Notes: The family is the same as Table G–1: the four substantive outcomes among positive-bias respondents at the primary threshold. Column 1 reports the difference in means between treated and control in the positive-bias subsample, with no covariate adjustment. Column 2 reports the corresponding unadjusted bootstrap p-value. Colums 3 reports the List et al. (2019) family-wise adjusted p-value. Column 4 and 5 are the Benjamini-Hochberg and Benjamini-Yekutieli false-discovery-rate adjusted p-values (Benjamini and Hochberg, 1995; Benjamini and Yekutieli, 2001) over the same unadjusted bootstrap p-vector, respectvely. * $p < 0.10$, ** $p < 0.05$ under the LSX correction.

G.3 Symmetry check: negative-bias respondents

Hypothesis H1 is directional for positive-bias respondents, so the focal family of Table G–1 is defined on over-estimators. As a specificity and symmetry check we repeat the same four-outcome focal-family inference for negative-bias respondents, that is, those who under-estimated their own SES (T1) or the share of affluent households in their district (T2) at the primary threshold. Because the framework's H2 makes the predicted update monotone in the absolute size of the perceptual gap, a symmetric design would show an effect of the opposite sign for under-estimators. We find no such effect. The aggregate index gate is flat in both arms (no covariates, HC1 robust: $p = 0.270$ for T1 at the two-class threshold, $p = 0.997$ for T2 at the ten percentage-point threshold), and no outcome clears the focal-family Romano-Wolf correction or the Anderson sharpened q at the ten percent level in either arm (Table G–3). The two coefficients with an unadjusted p below 0.10, the SES effect on reducing inequality of opportunity and the affluent-share effect on the preference for redistribution, share the sign of their positive-bias counterparts rather than the opposite sign a symmetric channel would predict, and neither survives the multiplicity correction applied to the family. The redistributive responses documented in the paper are thus specific to over-estimators, consistent with a correction-of-overestimation mechanism rather than a generic response to any belief revision. The SES negative-bias subsample at the primary threshold is small ($N = 45$), so Panel A is reported for completeness but is underpowered; the affluent-share subsample ($N \approx 239$) supports a better-powered and cleanly null test. The LSX correction applied to the same negative-bias family (Table G–4) likewise leaves no outcome significant in either arm.

Table G–3: Focal-family multiple-hypothesis testing, negative-bias respondents

Outcome	Coef. (1)	SE (2)	Unadj. p-value (3)	RW p-value (4)	Anders. p-value (5)
Panel A: SES treatment (T1), ≥ 2 levels, negative bias ($N = 45$)					
Preference for redistribution	-0.116	0.076	0.126	0.360	0.150
Reduce inequality	0.086	0.060	0.150	0.360	0.150
Wealth tax	0.050	0.062	0.415	0.448	0.249
Reduce inequality of opportunity	0.123	0.055	0.027	0.125	0.120
Panel B: Affluent-share treatment (T2), ≥ 10 pp, negative bias ($N = 239$)					
Preference for redistribution	0.057	0.033	0.080	0.260	0.469
Reduce inequality	-0.013	0.021	0.529	0.747	0.529
Wealth tax	-0.026	0.025	0.293	0.594	0.469
Reduce inequality of opportunity	0.002	0.020	0.924	0.925	0.693

Notes: The family is the four substantive outcomes among negative-bias respondents at the primary threshold (at least two SES levels for T1, at least ten percentage points for T2). This table is the symmetry counterpart of Table G–1 and is not a confirmatory test of H1, which is directional for positive-bias respondents. Column 1 reports the `Treat × Negative bias` coefficient from the main specification without covariates, identical to the main average-effects tables (Tables 3 and 4) estimated on each outcome's own sample, while column 2 and 3 report their corresponding robust standard errors and unadjusted p-value. Column 4 reports Romano-Wolf step-down adjusted p-values (Romano and Wolf, 2005; Clarke et al., 2020). Column 5 reports sharpened two-stage q-values (Benjamini et al., 2006; Anderson, 2008) computed by the BKY adaptive linear step-up procedure over the same four-outcome family. The negative-bias SES subsample at the primary threshold is small ($N = 45$), so Panel A is reported for completeness but is underpowered; the affluent-share subsample ($N \approx 239$) is well populated. * $p < 0.10$, ** $p < 0.05$ under the indicated correction.

Table G–4: Focal-family LSX robustness, negative-bias respondents

Outcome	Mean (1)	Unadj. p-value (2)	LSX p-value (3)	BH p-value (4)	BY p-value (5)
Panel A: SES (T1), ≥ 2 levels, negative bias ($N = 45$)					
Preference for redistribution	0.116	0.139	0.328	0.225	0.469
Reduce inequality	0.086	0.169	0.284	0.225	0.469
Wealth tax	0.050	0.427	0.427	0.427	0.890
Reduce inequality of opportunity	0.123	0.035	0.121	0.140	0.292
Panel B: Affluent share (T2), ≥ 10 pp, negative bias ($N = 239$)					
Preference for redistribution	0.057	0.074	0.250	0.297	0.619
Reduce inequality	0.013	0.534	0.774	0.712	1.000
Wealth tax	0.026	0.293	0.621	0.585	1.000
Reduce inequality of opportunity	0.002	0.923	0.923	0.923	1.000

Notes: Negative-bias counterpart of Table G–2, computed on the negative-bias subsample at the primary threshold (at least two SES levels for T1, at least ten percentage points for T2). Column 1 reports the difference in means between treated and control, with no covariate adjustment. Column 2 reports the unadjusted bootstrap p-value. Column 3 reports the List et al. (2019) family-wise adjusted p-value. Column 4 and 5 are the Benjamini-Hochberg and Benjamini-Yekutieli false-discovery-rate adjusted p-values (Benjamini and Hochberg, 1995; Benjamini and Yekutieli, 2001) over the same unadjusted bootstrap p-vector, respectively. As with the negative-bias focal-family table, this is a symmetry check rather than a confirmatory test of H1. The SES (T1) negative-bias subsample is small ($N = 45$), so Panel A is reported for completeness but is underpowered. * $p < 0.10$, ** $p < 0.05$ under the LSX correction.

G.4 Inclusion of covariates

As a robustness check, the tables below report specifications that include age, sex, marital status, log household income per capita, college attendance, and employment status as additional covariates. The estimates are very similar to those reported in the main text, where no covariates are included.

Table G–5: Average effects of the SES treatment, with covariates (both bias thresholds)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual SES differ by at least one class					
Treated x Neg. bias	-0.013	0.035	0.043	0.040	0.036
	(0.050)	(0.040)	(0.040)	(0.037)	(0.038)
Treated x Pos. bias	0.018	0.019	-0.004	0.020	0.018
	(0.021)	(0.015)	(0.016)	(0.015)	(0.014)
Treated x No bias	0.025	-0.008	-0.005	-0.003	-0.002
	(0.045)	(0.029)	(0.035)	(0.030)	(0.029)
Neg. bias	0.094*	-0.039	-0.112***	-0.067*	-0.051
	(0.052)	(0.034)	(0.041)	(0.037)	(0.033)
Pos. bias	0.022	-0.029	-0.048	-0.035	-0.033
	(0.039)	(0.024)	(0.031)	(0.027)	(0.025)
Constant	0.757***	0.701***	0.689***	0.683***	0.618***
	(0.111)	(0.068)	(0.075)	(0.075)	(0.069)
Observations	798	797	797	799	793
Panel B: Bias when perceived and actual SES differ by at least two classes					
Treated x Neg. bias	-0.122	0.096	0.059	0.117**	0.065
	(0.078)	(0.060)	(0.062)	(0.054)	(0.057)
Treated x Pos. bias	-0.002	0.044**	0.024	0.044**	0.037**
	(0.030)	(0.021)	(0.022)	(0.021)	(0.019)
Treated x No bias	0.038	-0.004	-0.013	-0.006	0.001
	(0.023)	(0.016)	(0.019)	(0.017)	(0.016)
Neg. bias	0.181***	-0.030	-0.048	-0.079**	-0.009
	(0.058)	(0.038)	(0.048)	(0.038)	(0.036)
Pos. bias	0.012	-0.014	-0.010	-0.041**	-0.020
	(0.027)	(0.019)	(0.021)	(0.020)	(0.018)
Constant	0.774***	0.684***	0.674***	0.696***	0.618***
	(0.090)	(0.058)	(0.065)	(0.062)	(0.059)
Observations	798	797	797	799	793

Notes: The table presents the estimated average effects of the SES treatment by bias type, controlling for a set of covariates, including age, sex, marital status, logarithm of household income per capita, college attendance, and employment status. In Panel A, bias is defined as perceiving one's SES to be at least one level lower (negative) or higher (positive) than the actual SES. Panel B applies the primary threshold of at least two SES levels used in the body table 3 and in the multiplicity-adjusted analysis. Robust standard errors are indicated in parentheses. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$ indicate statistical significance.

Table G–6: Average effects of the affluent-share treatment, with covariates (both bias thresholds)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual share differ by at least 5%					
Treated x Neg. bias	0.042	-0.024	-0.018	0.005	-0.004
	(0.031)	(0.019)	(0.023)	(0.019)	(0.018)
Treated x Pos. bias	0.061**	0.028	0.013	0.034*	0.043***
	(0.024)	(0.017)	(0.020)	(0.018)	(0.016)
Treated x No bias	0.002	0.042	-0.032	0.016	0.012
	(0.047)	(0.032)	(0.037)	(0.033)	(0.032)
Neg. bias	-0.014	0.045	-0.014	0.011	0.012
	(0.042)	(0.029)	(0.033)	(0.030)	(0.028)
Pos. bias	-0.029	0.012	-0.015	-0.019	-0.015
	(0.040)	(0.029)	(0.033)	(0.030)	(0.029)
Constant	0.824***	0.650***	0.701***	0.591***	0.586***
	(0.084)	(0.057)	(0.067)	(0.058)	(0.055)
Observations	842	839	840	841	833
Panel B: Bias when perceived and actual share differ by at least 10%					
Treated x Neg. bias	0.063*	-0.020	-0.028	0.006	0.000
	(0.033)	(0.021)	(0.026)	(0.020)	(0.020)
Treated x Pos. bias	0.069***	0.037*	0.027	0.037*	0.054***
	(0.026)	(0.019)	(0.022)	(0.020)	(0.018)
Treated x No bias	-0.004	0.008	-0.033	0.007	-0.006
	(0.034)	(0.022)	(0.025)	(0.023)	(0.021)
Neg. bias	-0.029	0.012	-0.044*	-0.028	-0.027
	(0.035)	(0.021)	(0.026)	(0.024)	(0.021)
Pos. bias	-0.034	-0.032	-0.060**	-0.057**	-0.061***
	(0.032)	(0.022)	(0.025)	(0.024)	(0.022)
Constant	0.826***	0.685***	0.727***	0.613***	0.615***
	(0.080)	(0.054)	(0.063)	(0.054)	(0.051)
Observations	842	839	840	841	833

Notes: The table reports the estimated average effects of the affluent-share treatment by bias type, controlling for covariates such as age, sex, marital status, logarithm of household income per capita, college attendance, and employment status. In Panel A, bias is defined using a ±5 pp threshold: negative (positive) bias if the perceived district share of affluent households is at least 5 pp lower (higher) than the actual share. Panel B applies the primary threshold of ±10 percentage points used in the body table 4 and in the multiplicity-adjusted analysis. Robust standard errors are indicated in parentheses. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$ indicate statistical significance.

G.5 Continuous-bias specification

The continuous-bias specification is a coding clarification for the affluent-share treatment (T2), where the perceived-minus-actual gap is a cardinal quantity measured in percentage points. We do not construct a continuous-bias measure for the SES treatment (T1), because the SES gap is a difference of ordinal class categories. For T1 the magnitude of misperception is captured by the categorical threshold contrast (±1 versus ±2 classes) reported in the main tables. The ±5 pp and ±10 pp categorical thresholds for T2 are robust to replacing the bias indicator with the signed continuous gap.

Table G–7: Continuous bias × treatment, affluent-share treatment (T2)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Without covariates					
Treated (T_B)	0.044**	0.014	-0.004	0.023*	0.022*
	(0.018)	(0.012)	(0.014)	(0.012)	(0.011)
Bias T_B (standardized, SD units)	-0.001	-0.008	0.000	-0.020**	-0.010
	(0.013)	(0.009)	(0.011)	(0.009)	(0.008)
T_B x bias (standardized)	0.017	0.019*	0.025*	0.026**	0.030***
	(0.017)	(0.011)	(0.014)	(0.011)	(0.011)
Observations	858	857	857	859	849
Panel B: With covariates					
Treated (T_B)	0.045**	0.013	-0.005	0.022*	0.022*
	(0.018)	(0.012)	(0.014)	(0.012)	(0.011)
Bias T_B (standardized, SD units)	-0.016	-0.008	-0.003	-0.013	-0.013
	(0.013)	(0.009)	(0.011)	(0.009)	(0.009)
T_B x bias (standardized)	0.018	0.023**	0.029**	0.021*	0.031***
	(0.017)	(0.011)	(0.014)	(0.011)	(0.011)
Observations	842	839	840	841	833

Notes: The continuous bias is the signed gap between the perceived and actual share of affluent households, in percentage points.

G.6 District-clustered standard errors

Standard errors clustered at the district level. The qualitative pattern of the main results is unchanged.

Table G–8: Average effects of the SES treatment, district-clustered standard errors (T1)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual SES differ by at least one class					
Treated x Neg. bias	-0.011	0.029	0.035	0.044	0.033
	(0.046)	(0.037)	(0.043)	(0.035)	(0.037)
Treated x Pos. bias	0.012	0.026	0.003	0.023	0.020
	(0.022)	(0.018)	(0.016)	(0.014)	(0.016)
Treated x No bias	0.036	-0.017	-0.005	-0.006	-0.003
	(0.057)	(0.027)	(0.034)	(0.030)	(0.036)
Neg. bias	0.044	-0.037	-0.108***	-0.048	-0.054*
	(0.049)	(0.037)	(0.036)	(0.034)	(0.031)
Pos. bias	0.051	-0.021	-0.042	-0.038	-0.021
	(0.042)	(0.022)	(0.028)	(0.026)	(0.026)
Constant	0.432***	0.700***	0.705***	0.773***	0.576***
	(0.044)	(0.019)	(0.024)	(0.022)	(0.022)
Observations	815	816	816	818	810
Panel B: Bias when perceived and actual SES differ by at least two classes					
Treated x Neg. bias	-0.116	0.086	0.050	0.123*	0.063
	(0.078)	(0.063)	(0.060)	(0.061)	(0.059)
Treated x Pos. bias	-0.004	0.052**	0.025	0.048**	0.039*
	(0.031)	(0.023)	(0.023)	(0.019)	(0.023)
Treated x No bias	0.036	-0.005	-0.009	-0.007	0.002
	(0.026)	(0.016)	(0.013)	(0.017)	(0.015)
Neg. bias	0.101**	-0.037	-0.060	-0.067	-0.031
	(0.049)	(0.043)	(0.039)	(0.044)	(0.034)
Pos. bias	0.031	-0.018	-0.011	-0.049***	-0.017
	(0.029)	(0.017)	(0.019)	(0.017)	(0.018)
Constant	0.455***	0.689***	0.669***	0.762***	0.563***
	(0.023)	(0.011)	(0.009)	(0.012)	(0.011)
Observations	815	816	816	818	810

Notes: Specification mirrors Table D–1 with district-clustered standard errors.

Table G–9: Average effects of the affluent-share treatment, district-clustered standard errors (T2)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual share differ by at least 5%					
Treated x Neg. bias	0.037	-0.019	-0.019	0.002	-0.004
	(0.034)	(0.019)	(0.018)	(0.022)	(0.020)
Treated x Pos. bias	0.060**	0.025	0.012	0.037**	0.042***
	(0.024)	(0.017)	(0.017)	(0.016)	(0.011)
Treated x No bias	0.010	0.045	-0.020	0.021	0.013
	(0.059)	(0.033)	(0.038)	(0.034)	(0.039)
Neg. bias	-0.027	0.047	-0.011	0.018	0.006
	(0.044)	(0.031)	(0.035)	(0.034)	(0.034)
Pos. bias	-0.013	0.016	-0.009	-0.023	-0.016
	(0.038)	(0.025)	(0.033)	(0.034)	(0.030)
Constant	0.489***	0.656***	0.670***	0.746***	0.561***
	(0.038)	(0.026)	(0.032)	(0.029)	(0.030)
Observations	858	857	857	859	849
Panel B: Bias when perceived and actual share differ by at least 10%					
Treated x Neg. bias	0.057*	-0.013	-0.026	0.002	-0.000
	(0.032)	(0.020)	(0.018)	(0.019)	(0.020)
Treated x Pos. bias	0.069***	0.034	0.025	0.041**	0.054***
	(0.024)	(0.021)	(0.024)	(0.017)	(0.014)
Treated x No bias	-0.002	0.008	-0.029	0.010	-0.007
	(0.040)	(0.019)	(0.019)	(0.022)	(0.022)
Neg. bias	-0.047	0.012	-0.046*	-0.021	-0.033
	(0.036)	(0.021)	(0.023)	(0.023)	(0.023)
Pos. bias	-0.020	-0.030	-0.056**	-0.063**	-0.061***
	(0.026)	(0.021)	(0.023)	(0.030)	(0.021)
Constant	0.495***	0.690***	0.699***	0.773***	0.591***
	(0.028)	(0.015)	(0.019)	(0.020)	(0.018)
Observations	858	857	857	859	849

Notes: Specification mirrors Table D–2 with district-clustered standard errors.

G.7 Common-sample replication

Because item-level missingness in the outcome variables makes the estimation sample vary slightly across columns, we re-estimate the main (no-covariate) specifications on a single common sample of respondents with complete information on all five outcomes: $N = 810$ for T1 and $N = 849$ for T2.

Table G–10: Average effects of the SES treatment, common sample (T1, $N = 810$)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual SES differ by at least one class					
Treated x Neg. bias	-0.016	0.029	0.033	0.045	0.033
	(0.049)	(0.039)	(0.040)	(0.038)	(0.038)
Treated x Pos. bias	0.013	0.023	0.002	0.022	0.020
	(0.021)	(0.014)	(0.016)	(0.015)	(0.013)
Treated x No bias	0.044	-0.021	-0.001	-0.017	-0.003
	(0.044)	(0.030)	(0.034)	(0.029)	(0.029)
Neg. bias	0.052	-0.037	-0.103***	-0.056	-0.054*
	(0.050)	(0.032)	(0.038)	(0.034)	(0.032)
Pos. bias	0.059	-0.020	-0.037	-0.045*	-0.021
	(0.037)	(0.024)	(0.030)	(0.025)	(0.025)
Constant	0.423***	0.700***	0.700***	0.781***	0.576***
	(0.034)	(0.022)	(0.027)	(0.022)	(0.023)
Observations	810	810	810	810	810
Panel B: Bias when perceived and actual SES differ by at least two classes					
Treated x Neg. bias	-0.116	0.086	0.050	0.123**	0.063
	(0.076)	(0.060)	(0.062)	(0.055)	(0.057)
Treated x Pos. bias	-0.004	0.047**	0.024	0.043**	0.039**
	(0.030)	(0.020)	(0.022)	(0.021)	(0.019)
Treated x No bias	0.039	-0.007	-0.008	-0.008	0.002
	(0.024)	(0.016)	(0.018)	(0.016)	(0.015)
Neg. bias	0.103*	-0.037	-0.059	-0.068*	-0.031
	(0.056)	(0.036)	(0.045)	(0.037)	(0.034)
Pos. bias	0.032	-0.015	-0.008	-0.047**	-0.017
	(0.027)	(0.018)	(0.021)	(0.020)	(0.018)
Constant	0.453***	0.689***	0.667***	0.764***	0.563***
	(0.018)	(0.012)	(0.014)	(0.013)	(0.012)
Observations	810	810	810	810	810

Notes: The sample is restricted to respondents with complete information on all five outcomes. This table reports the common-sample counterpart of Table D–1.

Table G–11: Average effects of the affluent-share treatment, common sample (T2, $N = 849$)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.
Panel A: Bias when perceived and actual share differ by at least 5%					
Treated x Neg. bias	0.035	-0.020	-0.019	0.003	-0.004
	(0.031)	(0.019)	(0.023)	(0.019)	(0.019)
Treated x Pos. bias	0.064***	0.026	0.013	0.036**	0.042***
	(0.025)	(0.017)	(0.020)	(0.018)	(0.016)
Treated x No bias	0.010	0.037	-0.023	0.012	0.013
	(0.048)	(0.032)	(0.036)	(0.032)	(0.031)
Neg. bias	-0.027	0.040	-0.014	0.009	0.006
	(0.044)	(0.029)	(0.033)	(0.030)	(0.028)
Pos. bias	-0.016	0.008	-0.014	-0.031	-0.016
	(0.042)	(0.029)	(0.032)	(0.030)	(0.028)
Constant	0.489***	0.664***	0.673***	0.755***	0.561***
	(0.038)	(0.026)	(0.029)	(0.026)	(0.025)
Observations	849	849	849	849	849
Panel B: Bias when perceived and actual share differ by at least 10%					
Treated x Neg. bias	0.054	-0.015	-0.027	0.002	-0.000
	(0.033)	(0.021)	(0.025)	(0.020)	(0.020)
Treated x Pos. bias	0.076***	0.035*	0.026	0.040**	0.054***
	(0.026)	(0.019)	(0.022)	(0.020)	(0.018)
Treated x No bias	-0.003	0.004	-0.030	0.005	-0.007
	(0.035)	(0.022)	(0.025)	(0.023)	(0.021)
Neg. bias	-0.047	0.008	-0.047*	-0.026	-0.033
	(0.036)	(0.021)	(0.026)	(0.023)	(0.021)
Pos. bias	-0.023	-0.034	-0.060**	-0.066***	-0.061***
	(0.033)	(0.022)	(0.025)	(0.024)	(0.021)
Constant	0.495***	0.694***	0.701***	0.778***	0.591***
	(0.027)	(0.016)	(0.019)	(0.018)	(0.016)
Observations	849	849	849	849	849

Notes: The sample is restricted to respondents with complete information on all five outcomes. This table reports the common-sample counterpart of Table D–2.

G.8 Geocoded subsample balance

Comparison-of-means between the geocoded subsample ($N = 937$) and the full redistribution sample ($N = 1{,}293$), with calibrated inverse-probability weights for the geocoded subsample.

Table G–12: Balance of covariates: geocoded subsample versus full sample

	Full sample	Geocoded (unweighted) mean	Geocoded (weighted) mean	Non-geocoded mean	p-value
Age	37.195	37.123	37.200	37.382	0.725
Male	0.509	0.487	0.491	0.565	0.011
Married or cohabiting	0.490	0.489	0.491	0.492	0.940
Income	6.215	6.230	6.225	6.177	0.351
Secondary education	0.549	0.544	0.541	0.559	0.640
Tertiary education	0.362	0.375	0.379	0.329	0.116
Employed	0.377	0.375	0.376	0.383	0.787
Self-employed	0.372	0.370	0.371	0.375	0.891
Informed	0.439	0.445	0.448	0.424	0.509
Right-wing	0.484	0.480	0.469	0.494	0.643
Poor due to circumstances	0.539	0.546	0.553	0.521	0.427
Trust in the state	0.346	0.343	0.347	0.352	0.775
Received Covid social assistance or social program	0.479	0.466	0.470	0.515	0.124
perceived socioeconomic status of the household	2.566	2.575	2.571	2.543	0.524
actual socioeconomic status of the household	1.667	1.677	1.667	1.643	0.643
Own SES overestimated (positive bias)	0.699	0.710	0.710	0.671	0.183
Affluence in the district overestimated (positive bias)	0.500	0.525	0.505	0.435	0.004
Modern Area	0.110	0.112	0.110	0.103	0.625

Notes: Columns report sample means for the full sample, the unweighted geocoded subsample, the weighted geocoded subsample, and the non-geocoded subsample. The last column reports the p-value of the difference between the full sample and the unweighted geocoded subsample.

G.9 Perceived fairness as a sixth outcome

The treatment effects are extended to a sixth outcome, the respondent's perceived fairness of the income distribution in Peru, rescaled to $[0, 1]$ so that one denotes the strongest perception of unfairness. The coefficient on `Treat` $\times$ `Positive bias` is small and statistically indistinguishable from zero across all four panels, consistent with the null prediction in Section 2.5.

Table G–13: Perceived fairness as a sixth outcome, SES treatment (T1, no covariates)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.	(6) Income distr. unfair
Panel A: Bias when perceived and actual SES differ by at least one class						
Treated x Neg. bias	-0.011	0.029	0.035	0.044	0.033	-0.016
	(0.049)	(0.039)	(0.040)	(0.037)	(0.038)	(0.036)
Treated x Pos. bias	0.012	0.026*	0.003	0.023	0.020	0.011
	(0.021)	(0.014)	(0.016)	(0.015)	(0.013)	(0.016)
Treated x No bias	0.036	-0.017	-0.005	-0.006	-0.003	0.000
	(0.045)	(0.030)	(0.034)	(0.030)	(0.029)	(0.035)
Neg. bias	0.044	-0.037	-0.108***	-0.048	-0.054*	0.007
	(0.051)	(0.032)	(0.038)	(0.035)	(0.032)	(0.039)
Pos. bias	0.051	-0.021	-0.042	-0.038	-0.021	-0.017
	(0.037)	(0.024)	(0.030)	(0.026)	(0.025)	(0.029)
Constant	0.432***	0.700***	0.705***	0.773***	0.576***	0.707***
	(0.034)	(0.022)	(0.027)	(0.023)	(0.023)	(0.027)
Observations	815	816	816	818	810	817
Panel B: Bias when perceived and actual SES differ by at least two classes						
Treated x Neg. bias	-0.116	0.086	0.050	0.123**	0.063	-0.013
	(0.076)	(0.060)	(0.062)	(0.055)	(0.057)	(0.055)
Treated x Pos. bias	-0.004	0.052**	0.025	0.048**	0.039**	-0.004
	(0.030)	(0.021)	(0.022)	(0.021)	(0.019)	(0.024)
Treated x No bias	0.036	-0.005	-0.009	-0.007	0.002	0.008
	(0.024)	(0.016)	(0.018)	(0.017)	(0.015)	(0.017)
Neg. bias	0.101*	-0.037	-0.060	-0.067*	-0.031	0.016
	(0.056)	(0.036)	(0.045)	(0.037)	(0.034)	(0.041)
Pos. bias	0.031	-0.018	-0.011	-0.049**	-0.017	-0.036*
	(0.027)	(0.019)	(0.021)	(0.020)	(0.018)	(0.020)
Constant	0.455***	0.689***	0.669***	0.762***	0.563***	0.709***
	(0.018)	(0.012)	(0.014)	(0.013)	(0.012)	(0.013)
Observations	815	816	816	818	810	817

Notes: The last column reports the treatment effects on the perceived fairness of the income distribution.

Table G–14: Perceived fairness as a sixth outcome, affluent-share treatment (T2, no covariates)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.	(6) Income distr. unfair
Panel A: Bias when perceived and actual share differ by at least 5%						
Treated x Neg. bias	0.037	-0.019	-0.019	0.002	-0.004	0.015
	(0.031)	(0.019)	(0.023)	(0.019)	(0.019)	(0.023)
Treated x Pos. bias	0.060**	0.025	0.012	0.037**	0.042***	0.015
	(0.024)	(0.017)	(0.020)	(0.018)	(0.016)	(0.019)
Treated x No bias	0.010	0.045	-0.020	0.021	0.013	-0.035
	(0.048)	(0.033)	(0.036)	(0.033)	(0.031)	(0.036)
Neg. bias	-0.027	0.047	-0.011	0.018	0.006	-0.057*
	(0.044)	(0.029)	(0.033)	(0.031)	(0.028)	(0.033)
Pos. bias	-0.013	0.016	-0.009	-0.023	-0.016	-0.044
	(0.042)	(0.029)	(0.032)	(0.031)	(0.028)	(0.032)
Constant	0.489***	0.656***	0.670***	0.746***	0.561***	0.738***
	(0.038)	(0.027)	(0.028)	(0.027)	(0.025)	(0.029)
Observations	858	857	857	859	849	860
Panel B: Bias when perceived and actual share differ by at least 10%						
Treated x Neg. bias	0.057*	-0.013	-0.026	0.002	-0.000	0.015
	(0.033)	(0.021)	(0.025)	(0.020)	(0.020)	(0.024)
Treated x Pos. bias	0.069***	0.034*	0.025	0.041**	0.054***	0.017
	(0.026)	(0.019)	(0.022)	(0.020)	(0.018)	(0.020)
Treated x No bias	-0.002	0.008	-0.029	0.010	-0.007	-0.015
	(0.035)	(0.022)	(0.025)	(0.023)	(0.021)	(0.026)
Neg. bias	-0.047	0.012	-0.046*	-0.021	-0.033	-0.038
	(0.036)	(0.022)	(0.026)	(0.024)	(0.021)	(0.026)
Pos. bias	-0.020	-0.030	-0.056**	-0.063***	-0.061***	-0.027
	(0.033)	(0.022)	(0.025)	(0.024)	(0.021)	(0.024)
Constant	0.495***	0.690***	0.699***	0.773***	0.591***	0.719***
	(0.027)	(0.017)	(0.019)	(0.018)	(0.016)	(0.019)
Observations	858	857	857	859	849	860

Notes: The last column reports the treatment effects on the perceived fairness of the income distribution.

Table G–15: Perceived fairness as a sixth outcome, SES treatment (T1, with covariates)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.	(6) Income distr. unfair
Panel A: Bias when perceived and actual SES differ by at least one class						
Treated x Neg. bias	-0.013	0.035	0.043	0.040	0.036	-0.013
	(0.050)	(0.040)	(0.040)	(0.037)	(0.038)	(0.037)
Treated x Pos. bias	0.018	0.019	-0.004	0.020	0.018	0.010
	(0.021)	(0.015)	(0.016)	(0.015)	(0.014)	(0.016)
Treated x No bias	0.025	-0.008	-0.005	-0.003	-0.002	0.006
	(0.045)	(0.029)	(0.035)	(0.030)	(0.029)	(0.036)
Neg. bias	0.094*	-0.039	-0.112***	-0.067*	-0.051	0.045
	(0.052)	(0.034)	(0.041)	(0.037)	(0.033)	(0.040)
Pos. bias	0.022	-0.029	-0.048	-0.035	-0.033	-0.027
	(0.039)	(0.024)	(0.031)	(0.027)	(0.025)	(0.029)
Constant	0.757***	0.701***	0.689***	0.683***	0.618***	0.874***
	(0.111)	(0.068)	(0.075)	(0.075)	(0.069)	(0.077)
Observations	798	797	797	799	793	798
Panel B: Bias when perceived and actual SES differ by at least two classes						
Treated x Neg. bias	-0.122	0.096	0.059	0.117**	0.065	-0.014
	(0.078)	(0.060)	(0.062)	(0.054)	(0.057)	(0.056)
Treated x Pos. bias	-0.002	0.044**	0.024	0.044**	0.037**	-0.007
	(0.030)	(0.021)	(0.022)	(0.021)	(0.019)	(0.024)
Treated x No bias	0.038	-0.004	-0.013	-0.006	0.001	0.010
	(0.023)	(0.016)	(0.019)	(0.017)	(0.016)	(0.018)
Neg. bias	0.181***	-0.030	-0.048	-0.079**	-0.009	0.066
	(0.058)	(0.038)	(0.048)	(0.038)	(0.036)	(0.044)
Pos. bias	0.012	-0.014	-0.010	-0.041**	-0.020	-0.043**
	(0.027)	(0.019)	(0.021)	(0.020)	(0.018)	(0.020)
Constant	0.774***	0.684***	0.674***	0.696***	0.618***	0.871***
	(0.090)	(0.058)	(0.065)	(0.062)	(0.059)	(0.065)
Observations	798	797	797	799	793	798

Notes: With-covariates counterpart of Table G–13.

Table G–16: Perceived fairness as a sixth outcome, affluent-share treatment (T2, with covariates)

	(1) Pref. for redist.	(2) Reduce inequality	(3) Wealth tax	(4) Reduce IneqOpp	(5) Index redist.	(6) Income distr. unfair
Panel A: Bias when perceived and actual share differ by at least 5%						
Treated x Neg. bias	0.042	-0.024	-0.018	0.005	-0.004	0.020
	(0.031)	(0.019)	(0.023)	(0.019)	(0.018)	(0.023)
Treated x Pos. bias	0.061**	0.028	0.013	0.034*	0.043***	0.013
	(0.024)	(0.017)	(0.020)	(0.018)	(0.016)	(0.019)
Treated x No bias	0.002	0.042	-0.032	0.016	0.012	-0.036
	(0.047)	(0.032)	(0.037)	(0.033)	(0.032)	(0.037)
Neg. bias	-0.014	0.045	-0.014	0.011	0.012	-0.060*
	(0.042)	(0.029)	(0.033)	(0.030)	(0.028)	(0.034)
Pos. bias	-0.029	0.012	-0.015	-0.019	-0.015	-0.046
	(0.040)	(0.029)	(0.033)	(0.030)	(0.029)	(0.032)
Constant	0.824***	0.650***	0.701***	0.591***	0.586***	0.763***
	(0.084)	(0.057)	(0.067)	(0.058)	(0.055)	(0.065)
Observations	842	839	840	841	833	842
Panel B: Bias when perceived and actual share differ by at least 10%						
Treated x Neg. bias	0.063*	-0.020	-0.028	0.006	0.000	0.019
	(0.033)	(0.021)	(0.026)	(0.020)	(0.020)	(0.025)
Treated x Pos. bias	0.069***	0.037*	0.027	0.037*	0.054***	0.015
	(0.026)	(0.019)	(0.022)	(0.020)	(0.018)	(0.021)
Treated x No bias	-0.004	0.008	-0.033	0.007	-0.006	-0.012
	(0.034)	(0.022)	(0.025)	(0.023)	(0.021)	(0.026)
Neg. bias	-0.029	0.012	-0.044*	-0.028	-0.027	-0.038
	(0.035)	(0.021)	(0.026)	(0.024)	(0.021)	(0.026)
Pos. bias	-0.034	-0.032	-0.060**	-0.057**	-0.061***	-0.025
	(0.032)	(0.022)	(0.025)	(0.024)	(0.022)	(0.024)
Constant	0.826***	0.685***	0.727***	0.613***	0.615***	0.744***
	(0.080)	(0.054)	(0.063)	(0.054)	(0.051)	(0.060)
Observations	842	839	840	841	833	842

Notes: With-covariates counterpart of Table G–14.

G.10 Local inequality: additional results

This appendix reports three additional results on the relationship between perceived SES, local inequality, and redistributive preferences: the rank correlations between respondents' perceptions and their local socio-economic environment (Table G–17); the association between local inequality and redistributive preferences within the control group, where no corrective information was provided (Table G–18); and a comparison of our local-inequality findings with the two closest studies in the literature (Table G–19).

Table G–17: Rank correlations between perceptions and the local socioeconomic environment

Local measure	Perceived SES	Perceived affluent share
Local mean SES, 300 m buffer	0.236	0.231
Local mean SES, 500 m buffer	0.238	0.236
Local mean SES, 1000 m buffer	0.247	0.224
Affluent share, 300 m buffer	0.240	0.233
Affluent share, 500 m buffer	0.256	0.231
Affluent share, 1000 m buffer	0.258	0.242
District mean SES	0.256	0.196
District affluent share	0.262	0.214

Notes: Each cell reports Spearman rank correlation between a perception (columns) and a feature of the respondent's local environment (rows). The local measures are the average local socioeconomic status and the share of affluent households, evaluated at alternative distances around the dwelling and at the district level. Recalibrated survey weights are applied to account for missing geolocation data. All correlations are positive and significant at the 1% level.

Table G–18: Local inequality and redistributive preferences, control group only

Buffer zone	Inequality metric	Pref. for redist.	Reduce inequality	Wealth tax	Reduce IneqOpp	Index redist.
300 mts.	GE(2)	-0.067	-0.016	-0.020	-0.014	-0.039
	Coef. Var.	-0.015	-0.003	-0.003	-0.003	-0.008
	%Rich	-0.081**	-0.021	-0.074**	0.024	-0.043*
500 mts.	GE(2)	-0.096	-0.012	-0.021	-0.041	-0.056
	Coef. Var.	-0.019	-0.002	-0.003	-0.007	-0.010
	%Rich	-0.074 *	-0.025	-0.077**	0.026	-0.043
1000 mts.	GE(2)	-0.126*	-0.035	-0.040	-0.074	-0.088**
	Coef. Var.	-0.024*	-0.007	-0.007	-0.013	-0.016**
	%Rich	-0.062	-0.027	-0.075**	0.027	-0.040

Notes: The table uses respondents in the control group with valid geolocation data ($N = 289$). Each cell reports the coefficient from a separate regression in which the corresponding local inequality metric is included as the main covariate. Local inequality is computed using households within buffer zones of 300, 500, or 1,000 metres around the respondent's dwelling. All regressions include the standard set of covariates used in the main specifications: age, sex, marital status, log household income per capita, college attendance, and employment status. Recalibrated survey weights are applied to account for missing geolocation data, and robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$ indicate statistical significant levels.

Table G–19: Comparison of local-inequality findings with the closest studies

Paper	Setting	Local-inequality measure	Heterogeneity dimension	Differential effect on redistribution
This paper	Lima 2021	GE(2), CV, and affluent share at 300/500/1,000 m buffers	Positive bias in own SES or in perceived affluent share	SES treatment (T1): no differential effect by local inequality. Affluent-share treatment (T2): effect on stated preference for redistribution concentrated in high-inequality (above-median GE(2)) blocks, significant at the 10% level at the 1 km buffer
Hoy et al. (2024a)	Australia 2017–19 (3 RCTs)	Local Government Area (LGA) Gini, high vs. low (top 40%)	Right-wing voters	In high-inequality LGAs, right-wing support for redistribution rises by 18.7 pp under the national-inequality treatment and by 24.2 pp under the own-position treatment; $\sim$0 pp in low-inequality LGAs
Domènech-Arumí (2025)	Barcelona 2020 (descriptive and quasi-experimental)	Neighbourhood Gini at 100–1,000 m (cadastral data)	None (unconditional)	Unconditional effect of local inequality on redistribution is small ($\sim$1.3% of an SD) and not significant

Notes: Hand-built comparison. The Hoy et al. (2024a) magnitudes are from their Section 5.4 and the Domènech-Arumí (2025) estimate from his Tables 2 and 3.

H Questionnaire (English version)

A. Socio-demographic Questions

NOTE: Questions 1-7 are mandatory.

1. Does your usual place of residence in Lima or Callao?
   ☐ Yes ☐ No [-> End of the survey]
2. In which district do you currently live? [scroll through the list of 49 districts of Lima Metropolitana]
3. How old are you in completed years?
4. Are you
   ☐ Male ☐ Female
5. How many people permanently live in your household? [check min=1; max=10]
6. Which type of housing does your house resemble the most?
   ☐ Apartment ☐ House
7. In which range does the TOTAL monthly household income fall, including all wages, salaries, pensions, remittances, and government transfers? [Please scroll through the list]
   ☐ Less than 1,000 soles
   ☐ 1,000 to 1,500 soles
   ☐ 1,500 to 2,000 soles
   ☐ 2,000 to 2,500 soles
   ☐ 2,500 to 3,000 soles
   ☐ 3,000 to 3,500 soles
   ☐ 3,500 to 4,000 soles
   ☐ 4,000 to 4,500 soles
   ☐ 4,500 to 5,000 soles
   ☐ 5,000 to 6,000 soles
   ☐ 6,000 to 7,000 soles
   ☐ 7,000 to 8,000 soles
   ☐ 8,000 to 9,000 soles
   ☐ 9,000 to 10,000 soles
   ☐ 10,000 to 12,500 soles
   ☐ 12,500 to 15,000 soles
   ☐ More than 15,000 soles
8. What is your marital status?
   ☐ Cohabiting
   ☐ Married
   ☐ Widowed
   ☐ Divorced
   ☐ Separated
   ☐ Single
9. What is your relationship to the head of household?
   ☐ I am the head of household
   ☐ Spouse or partner
   ☐ Son or daughter or stepson
   ☐ Son-in-law or daughter-in-law
   ☐ Grandchild

- [ ] Father/mother or parent-in-law
- [ ] Other relative
- [ ] Domestic worker
- [ ] Pensioner
- [ ] Other

10. What is the highest level of education you have completed?
    - [ ] No education
    - [ ] Incomplete primary education
    - [ ] Completed primary education
    - [ ] Incomplete secondary education
    - [ ] Completed secondary education
    - [ ] Special basic education
    - [ ] Incomplete non-university higher education
    - [ ] Completed non-university higher education
    - [ ] Incomplete university education
    - [ ] Complete university education
    - [ ] Master's or doctorate degree

11. Select the employment status that best matches your current situation (choose one option)
    - [ ] Employer or boss
    - [ ] Self-employed
    - [ ] Employee
    - [ ] Laborer
    - [ ] Unpaid family worker
    - [ ] Domestic worker
    - [ ] Unemployed
    - [ ] Retired
    - [ ] Student
    - [ ] Other

12. How often do you follow the news through television, radio, newspapers, social media, or the internet?
    - [ ] Daily
    - [ ] Several times a week
    - [ ] Several times a month
    - [ ] Several times a year
    - [ ] Never

13. Did you or someone in your household receive any government assistance/benefits to cope with the COVID-19 pandemic (such as the "Bono Yo Me Quedo en Casa" or "Bono Familiar Universal", etc.)?
    ☐ Yes ☐ No ☐ Don't know

14. Have you or someone in your household received or used any of the government social programs (such as Quali Warma, Juntos, Pensión 65, Beca 18, Cuna Más, etc.)?
    ☐ Yes ☐ No ☐ Don't know

15. Do you consider your economic situation to be better, the same, or worse than it was twelve months ago?
    - [ ] Better
    - [ ] Same
    - [ ] Worse

B. Opinion and beliefs questions

16. In your opinion, why is a person poor? Choose the most important reason
    - ☐ Due to their lack of effort
    - ☐ Due to circumstances beyond their control

17. In your opinion, why is a person rich? Choose the most important reason
    - ☐ Because they worked harder than others
    - ☐ Because they had more advantages than others

18. Speaking about the people in your neighbourhood, would you say they are very trustworthy, somewhat trustworthy, not very trustworthy, or not trustworthy?
    - ☐ Very trustworthy
    - ☐ Somewhat trustworthy
    - ☐ Not very trustworthy
    - ☐ Not trustworthy

19. In the past 12 months, have you participated in a public demonstration or protest?
    - ☐ Yes, I have participated
    - ☐ No, I have not participated

20. Are you very satisfied, satisfied, dissatisfied, or very dissatisfied with the quality of public schools?
    - ☐ Very satisfied
    - ☐ Satisfied
    - ☐ Dissatisfied
    - ☐ Very dissatisfied

21. And with the quality of public medical and healthcare services?
    - ☐ Very satisfied
    - ☐ Satisfied
    - ☐ Dissatisfied
    - ☐ Very dissatisfied

22. In political matters, people often talk about the left and the right. On the following scale, where 1 represents the left and 10 represents the right, where would you place yourself in general terms?

1	2	3	4	5	6	7	8	9	10

Left   Right

23. Indicate how willing you are, in general, to take risks. Use the following scale, where 1 indicates that you are not willing to take risks, and 10 indicates that you are very willing:

1	2	3	4	5	6	7	8	9	10

Not willing to take risks at all   Very willing to take risks

24. Indicate how much trust you have in the government to use public resources for the benefit of the poorest. Use the following scale, where 1 indicates that you have no trust at all, and 10 indicates that you have a lot of trust:

1	2	3	4	5	6	7	8	9	10

No trust at all   Complete trust

25. Some people believe that the government should have more responsibility to ensure that everyone has a livelihood, while others believe that individuals should have more responsibility to support themselves. Mark the option that best reflects your opinion
    ☐ The government should have more responsibility to ensure that everyone has a livelihood
    ☐ Individuals should have more responsibility to support themselves

26. The National Institute of Statistics and Informatics (INEI) classifies households in Lima into 5 income levels. Mark the group you believe your household belongs to
    ☐ High
    ☐ Upper Middle
    ☐ Middle
    ☐ Lower Middle
    ☐ Low

27. In the district where you live, what percentage of households do you believe belong to the High or Upper Middle-income level? Please slide the bar until you reach the appropriate figure. [The bar moves from 1 to 100 in increments of 1, and the text changes accordingly].
    In my district, X% of households belong to the High or Upper Middle-income level.

C. Treatments

T1. According to your income and according to studies by the National Institute of Statistics, your household would belong to level L, and you said it was level M... [1/2/3].

(1) “That is, the level of your household is higher than you thought.”
(2) “That is, the level of your household is lower than you thought.”
(3) “That is, the level of your household is the same than you thought.”
Now that you know this, we will ask you a few final questions.

T2. According to studies by the National Institute of Statistics, in your district, there are R% of Upper and Upper-Middle households, and you said there were S%...[1/2/3].

(1) “That is, there is a higher percentage of rich households in your district than you thought.”
(2) “That is, there is a lower percentage of rich households in your district than you thought.”
(3) “That is, there is about the same percentage of rich households in your district as you thought.”
Now that you know this, we will ask you a few final questions.

D. Outcome Measurement Questions

28. How much do you agree with the following contrasting ideas? 1 means "Strongly disagree" with the first statement, and 10 means "Strongly agree" with the second statement. If your opinion falls somewhere else on the scale, please choose the corresponding number.

1	2	3	4	5	6	7	8	9	10

Incomes should be made more equal

Greater income differences are needed as an incentive for individual effort

29. How fair do you believe the income distribution in Peru is?
    ☐ Very fair
    ☐ Fair

☐ Unfair
☐ Very unfair

30. How much do you agree or disagree with each of the following statements?

Statement	Strongly agree	Agree	Disagree	Strongly disagree
A. The state must implement strong policies to reduce income inequality between the rich and the poor.				
B. The state should levy a tax on wealth exceeding one million Soles.				
C. The state must reduce the differences in opportunities between children from rich and poor neighbourhoods.				

31. If you have any comments about the questions, please write them down below.

32. Your participation in the survey has helped us a lot in our study, we really appreciate it, may we contact you at a later date to continue with a second phase of our study?
☐ Yes [-> 33. Could you please provide us your cell phone number and/or email address?]
☐ No