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ISSN: 2365-9793

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ABSTRACT

What Others Need: Misperceptions of Well-Being Norms and Support for Redistribution*

People often misjudge what others need to live well. We introduce and measure well-being norms - the income people believe others require for a good life - and show that these beliefs are systematically underestimated. In a preregistered U.S. survey, 85–86% of respondents reported thresholds below what others say they themselves need. Two randomized survey experiments corrected these misperceptions. Respondents updated their beliefs considerably, yet support for redistribution and donation behavior remained unchanged. This null average effect, however, masks substantial heterogeneity. Among those who found the information credible and personally relevant, we observe redistribution support increasing by approximately 20% of a standard deviation, especially when the information referred to low-income families rather than the average American. Among those who dismissed it, we observe support decreasing by similar magnitudes - a pattern consistent with motivated reasoning and backlash. The main insight is that belief updating alone does not, on average, change policy preferences. Information influences redistribution attitudes only when perceived as morally important and legitimate.

JEL Classification: D31, D63, H23, H24, I31

Keywords: redistribution preferences, inequality, beliefs, information treatments, income, subjective well-being

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* We gratefully acknowledge the valuable comments and suggestions of Giorgia Menta. All remaining errors are our own. This research was supported by NTU Start-up Grant (022674-00001). Ethical approval was obtained from the Nanyang Technological University Institutional Review Board (IRB-2025-602). The authors declare no conflict of interest.

1. Introduction

Support for redistributive policies is systematically weaker than income positions alone would predict. Even among lower-income individuals—those who stand to gain most materially—support is far from universal, and instead reflects beliefs about fairness, mobility, and deservingness. For example, Fong (2001) shows that redistribution preferences depend heavily on attributions of poverty, while Kuziemko et al. (2015) find that supplying information about inequality raises concern but produces only modest shifts in policy support. More broadly, perceived rank and political identity often explain more variation in attitudes than actual economic position (Bénabou & Ok, 2001; Alesina & La Ferrara, 2005; Petersen et al., 2012; Weisstanner & Armingeon, 2022; Valero, 2022).

This disconnect suggests that redistribution is shaped not only by economic self-interest or misperceived rank, but also by deeper assumptions about what others need to live well. We introduce a new behavioural channel: misperceptions of well-being norms—the income levels people believe others require for a good life. Our construct is second-order: respondents estimate the income **others** require to achieve high life satisfaction, not what they need themselves. Redistribution preferences operate on beliefs about others' well-being thresholds, making this construct inherently interpersonal. Unlike inequality misperceptions, which concern relative position (Hauser & Norton, 2017; Gimpelson & Treisman, 2018; Stantcheva, 2021), well-being norms thus reflect others' needs-based thresholds. Underestimating these thresholds can make inequality appear benign; overestimating them can make redistribution seem futile or excessive. Both distortions may suppress support, even among those who stand to benefit.

This mechanism is conceptually distinct yet complementary to the growing literature on inequality misperceptions (Cruces et al., 2013; Hauser & Norton, 2017; Stantcheva, 2021), which finds that misperceived rank predicts redistribution attitudes better than actual

inequality—but that corrective information often has weak or heterogeneous effects (Kuziemko et al., 2015; Henkel et al., 2025). We build on this literature by examining a different dimension of belief: what people think *others* consider adequate for a good life, and how these shape judgments of social needs.

Such misperceptions probably stem from individuals having to infer what others need based on limited and unrepresentative information. While people directly perceive their own income needs through consumption and budgeting choices, beliefs about others' needs are based on partial social samples that may not represent the entire population (Galesic et al., 2012). Furthermore, unlike inequality data that are regularly featured in the media, needs-based sufficiency thresholds are rarely part of public discussion (Bartels, 2005; Gilens, 2001), and people do not often explicitly talk about what income they consider necessary for a satisfied life (Kahneman & Deaton, 2010). This lack of shared knowledge (Bursztyn et al., 2020) means individuals miss out on feedback that could correct their initial misperceptions. Whether these errors influence redistribution preferences—and whether correcting them affects policy support—remains an open empirical question.

We are, to our knowledge, the first to systematically measure well-being norm misperceptions and assess the effects of correcting them. In a nationally representative U.S. survey (N = 1,074), 85–86% of respondents underestimated the income needed for high life satisfaction. Two preregistered survey experiments (N = 1,600 and N = 1,606) provided factual benchmarks. Respondents significantly updated their beliefs, reducing misperceptions by $2,900–$3,700 on average. However, support for redistribution did not increase on average, despite clear evidence of belief revision.

This exposes the core puzzle of the paper: **people learn, but they do not necessarily change what they think should be done**. Belief updating is real and significant, but preference updating depends on conditions rather than happening automatically. This null average effect

masks substantial heterogeneity in how respondents process corrective information. We observe that redistribution support increases among those who perceive information as credible and personally meaningful, while it decreases among those who dismiss it. This pattern suggests that the main constraint is not informational inaccuracy alone, but whether corrective messages achieve normative resonance and moral uptake.

This heterogeneity pattern is particularly evident when examining how different messages were received. Among respondents who found the information credible and relevant, we observe redistribution support increasing by ~20% of a standard deviation, including in revealed donations. Among those who dismissed it, we observe support decreasing by similar magnitudes. Information referencing low-income families generated stronger heterogeneity in responses than information about the average American, consistent with the possibility that identifiable need and fairness concerns, rather than factual updating alone, shape how information influences preferences.

These results challenge the common assumption that correcting factual misperceptions naturally increases demand for redistribution. Information can shift beliefs—and sometimes preferences—but appears to influence redistribution primarily when moral engagement is present, and may backfire when it is not (Nyhan & Reifler, 2010; Druckman & McGrath, 2019). The political feasibility of redistribution therefore depends not only on inequality beliefs, but on how social needs are perceived, framed, and morally interpreted (Fong, 2001; Kuziemko et al., 2015; Stantcheva, 2021).

The remainder of the paper proceeds as follows. Section 2 develops a theoretical framework linking well-being norm misperceptions to redistribution preferences. Section 3 describes the survey design and measurement strategy. Section 4 presents experimental results. Section 5 discusses implications for research on redistribution and belief formation. Section 6 concludes.

2. Theoretical framework and hypotheses

We develop a simple framework to formalize how misperceptions of *well-being norms*–the income levels people believe others require for a good life–affect preferences for redistribution. The aim is not to provide a full model of redistribution, but to highlight a neglected mechanism and generate testable predictions.

2.1. Basic setup

Let the true income threshold for a "good life" be θ. Individual i believes others require income θ_i, which may deviate from θ due to misperception. Let redistribution involve a transfer T, financed by a distortionary taxation, that lifts recipients' incomes. Let $F(y)$ be the income distribution. The utility of individual i from redistribution is:

$$U_i = \alpha_i \cdot B(\theta_i, T) - \gamma_i.C(T), \quad (1)$$

where α_i captures the weight placed on others' well-being (altruism or fairness concern), $B(\theta_i, T)$ is the perceived benefit from helping others, γ_i. is sensitivity to efficiency or tax costs, and $C(T)$ represents the cost of redistribution (e.g., deadweight loss or self-interest costs).

We build on two complementary traditions. First, needs-based approaches to poverty measurement (Atkinson, 1970; Sen, 1976; Foster et al., 1984) treat social welfare as kinked around salient thresholds: marginal gains from transfers are highest when they lift individuals across needs-based cutoffs. Second, reference-dependent preference models (Kőszegi & Rabin, 2006) suggest that people evaluate outcomes relative to salient benchmarks. When applied to redistribution, these frameworks imply that transfers are valued most when they move recipients from below to above a perceived sufficiency standard θ_i. That is,

$$B(\theta_i, T) = \int_{\theta_i - T}^{\theta_i} f(y)dy. \quad (2)$$

Support is strongest when many are seen to be on the edge of needing help, as transfers at that point seem more effective and well-targeted. Crucially, an individual i's support for redistribution depends on their belief θ_i, which may systematically deviate from the true threshold θ. We now examine how such misperceptions distort perceived benefits.

2.2. Belief distortions and redistribution preferences

The perceived benefit $B(\theta_i, T)$ is maximized when $\theta_i \approx \theta$. Misperceptions reduce the marginal perceived return from redistribution in two ways:

- **Underestimation** ($\theta_i < \theta$): The need threshold is seen as low; most people appear already "fine." Redistribution seems unnecessary.
- **Overestimation** ($\theta_i > \theta$): The need threshold is seen as high; transfers are perceived as too small to matter. Redistribution seems futile or indulgent.

This yields a *hump-shaped* relation between belief accuracy and support for redistribution. The optimal belief is the true threshold; deviations in either direction reduce perceived impact.[1]

2.3. Information, updating, and heterogeneity

Suppose individuals receive a signal θ^* about the true threshold. They update beliefs depending on credibility ρ_i and relevance δ_i:

$$\theta_i^{post} = (1 - \kappa_i)\theta_i^{prior} + \kappa_i\theta^*, \quad (3)$$

where $\kappa_i = \rho_i \cdot \delta_i$.[2] This updating rule—pre-registered in our design—implies: If κ_i is high, there will be a strong belief updating towards θ^*. If κ_i is low, there will be weak or no updating.

[1] This specification contrasts with shortfall-reduction models, in which overestimation would increase support (since more people appear below the need line). However, we believe the threshold-crossing framework better reflects how social norms are constructed and communicated – through salient cutoff points like "poverty lines" or "living wages." It also aligns with evidence from psychology on pluralistic ignorance and categorical norms (Prentice & Miller, 1993; Marks & Miller, 1987; Krueger & Clement, 1994).

[2] This linear form captures the intuitive idea that both credibility and personal relevance act as multiplicative weights on how much information is absorbed. While real-world belief updating may be nonlinear (e.g., Bayesian updating with priors, motivated reasoning models), the linear form suffices for organizing testable implications

In case where $\kappa_i < 0$, belief updating moves *away* from the signal θ^*, indicating reactance or backfiring.

Thus, redistribution effects should be conditional rather than automatic: belief updating is necessary for preference change, but not sufficient without normative uptake. Corrective information may therefore:

- **Increase support for redistribution**, when the message is believed and personally meaningful;
- **Produce backlash**, when the information is viewed as implausible or threatening; or
- **Leave preferences unchanged**, when it is noticed but discounted as irrelevant.

While the preregistered model specifies that behavioural effects depend on $\kappa_i = \rho_i \cdot \delta_i$, it does not pre-specify which informational frame should generate higher credibility or relevance. One theoretical possibility is that messages highlighting the needs of low-income families may evoke greater moral salience than more abstract benchmarks such as "the average American," thereby increasing δ_i and, in turn, κ_i. We treat this framing asymmetry as a theory-driven but non-directionally preregistered implication, which we explore further in our empirical analysis.

2.4. Hypotheses

Across the two preregistered designs (Studies 1–2 preregistration; Study 3 preregistration), our framework yields **five confirmatory hypotheses**:

- **H1: Belief Gap.** Most individuals misperceive well-being norms, such that their beliefs θ_i systematically deviate from the empirical benchmark θ.

and matching observed heterogeneity. Extensions incorporating asymmetries in prior strength or motivated rejection (e.g., reactance) are a promising direction for future work.

- **H2: Belief–Preference Gradient.** Larger misperception $|\theta_i - \theta|$ is associated with weaker support for redistribution, reflecting a diminished perceived marginal benefit of transfers.
- **H3. Average Treatment Effect.** If norm misperceptions depress support for redistribution, then correcting those misperceptions should raise redistribution support on average.
- **H4: Belief Updating.** Exposure to norm-correcting information increases belief accuracy on average; that is, $\theta_i^{post} \rightarrow \theta^*$ when the signal is processed.
- **H5: Conditional Behavioural Effects.** Corrective information affects redistribution only when updated beliefs are accepted as credible and personally relevant ($\kappa_i = \rho_i \cdot \delta_i$). We predict heterogeneous treatment effects conditional on post-treatment reception measures:
    - **Increase support** if κ_i is high (accepted and normatively integrated)
    - **No change** if $\kappa_i \approx 0$ (discounted or irrelevant)
    - **Backfire** if $\kappa_i < 0$ (reactance or motivated resistance)

Because reception is measured post-treatment, this hypothesis characterizes the heterogeneity pattern we expect to observe rather than establishing causal effects of credibility per se.

In addition to the pre-registered hypotheses above, we conduct exploratory analyses to probe the mechanisms underlying conditional updating. These analyses were not specified *ex ante* as confirmatory predictions, but they help illuminate when belief revision does—and does not—translate into redistribution preferences.

- **E1: Mediation.** We explore whether belief updating mediates the relationship between information exposure and redistribution outcomes. If updating is necessary for

preference change, then treatment effects should operate through revisions in θ_i, conditional on uptake.

- **E2: Moral-Belief Moderation.** We examine whether treatment effects vary with beliefs about the deservingness of the poor. Individuals who attribute poverty to structural rather than effort-based causes may be more responsive once beliefs are updated.
- **E3: Treatment-Arm Asymmetry.** Although the core model predicts that behavioural effects depend on $\kappa_i = \rho_i \cdot \delta_i$, it does not pre-specify which informational frame should generate higher relevance or credibility. We therefore explore whether messages highlighting low-income needs yield stronger effects than those referencing the average American threshold.
- **E4: Heterogeneous Responsiveness.** We assess variation in responsiveness by income, ideology, and baseline misperception, consistent with the view that informational uptake depends on priors and motivational orientation.

3. Empirical strategy

3.1. Samples, pre-registration, and design

The empirical analysis draws on three original online surveys conducted in close succession – on July 28, July 31, and August 3, 2025 – using the Prolific Academic platform. Each sample was quota-matched to the U.S. adult population by age, sex, and simplified ethnicity, using U.S. Census Bureau benchmarks. While not strictly nationally representative, this design approximates demographic representativeness along key dimensions. The respective sample sizes are 1,074, 1,600, and 1,606 respondents. All studies received ethics approval from Nanyang Technological University (IRB-2025-602) and obtained informed consent from participants.

All three studies were pre-registered on the Open Science Framework (Studies 1 and 2: https://osf.io/fktd2; Study 3: https://osf.io/2cswk). These registrations specify hypotheses, sample sizes, treatment arms, and analysis plans. Note that data-quality variables such as reCAPTCHA, RelevantID, attention-check outcomes, and completion-time metadata were not recorded in this survey version. Prolific's internal participant verification system—combining CAPTCHA screening and unique ID restrictions—served as the primary fraud-prevention measure. As a result, no additional respondents were removed for suspected duplication, automation, or inattentive completion.

3.2. Survey structure

Each survey included the same core modules. Respondents first completed standard sociodemographic questions and a self-assessment of overall life satisfaction (0-10 scale). They then reported the minimum pre-tax monthly household income (in US dollars) that they personally would need to feel *highly satisfied with life* (defined as 8-10 on the scale). This self-assessed threshold is a first-order belief.

Next, respondents estimated the minimum income that they believed (i) the *average* American, (ii) *people like them* (same age, gender, race, education, and income), and (iii) people from low-income families (household income $\leq$ \$2,500 per month) would require to feel highly satisfied. These perceived norm beliefs are second-order: they reflect beliefs about others' minimum income for a "good life" thresholds rather than respondents' own.

To measure preferences for redistribution, respondents rated (0-10 scale) their support for:

1. Raising income taxes on the highest earners;
2. Introducing a Universal Basic Income (UBI)—a fixed monthly payment to all citizens regardless of income or employment status; and
3. Having the government do more to reduce income inequality.

Respondents could also select "don't know/prefer not to answer," which we coded as missing. As a result, the number of observations varies across regressions depending on the dependent variable used. We complemented these attitudinal measures with a revealed-preference task. Participants were asked to complete a donation task where they could allocate part or all of a potential $10 bonus to one or more charities—Helping low-income families (Feeding America), Investing in public services (DonorsChoose), or Supporting economic growth (Kiva)—or keep the entire amount. To ensure incentive compatibility, participants were informed that a randomly selected subset of respondents would have their decisions implemented in real-world settings.

3.3 Information treatments

Studies 2 and 3 introduced randomized information treatments designed to correct respondents' misperceptions about well-being norms. The treatments were presented after the respondents reported the minimum incomes they believed were needed for high life satisfaction, and before the redistribution questions, thus ensuring that the interventions targeted prior beliefs rather than downstream attitudes.

The information explicitly referenced Survey 1: the baseline study establishing the empirical benchmarks for perceived well-being norms (θ). All messages were displayed on a single screen for a fixed minimum of 10 seconds, after which respondents could proceed. The verbatim treatment texts are reproduced in Appendix A1 for full transparency.

Study 3 additionally measured respondents' immediate reactions to the treatment, including whether they found the information believable and personally relevant, providing empirical measures for the theoretical parameters ρ_i (credibility) and δ_i (relevance).

They were then asked again to report (i) the minimum income they personally would need for high life satisfaction and (ii) their beliefs about what others require, both for the average

American and for low-income families. These repeated measures serve as manipulation checks: they help us assess whether respondents understood and internalized the information, updated their beliefs as expected, and distinguished between personal needs and perceived social norms. Study 3's design allows us to directly evaluate both *belief updating* and *information processing*, connecting the theoretical and empirical parts of the study.

3.4 Descriptive statistics and balance checks

Descriptive statistics for all three surveys are provided in Appendix Tables B1–B3. Appendix Table B1 summarizes the main outcome variables—redistribution attitudes and donation behavior—while Appendix Table B2 reports sociodemographic characteristics. As expected, variables measured **before** the information treatments—such as demographic traits and initial income thresholds—show very similar means across the three studies. In contrast, variables measured **after** the interventions in Studies 2 and 3 display notable variation. Specifically, redistribution attitudes and donation behavior differ significantly between treatment and control groups, aligning with the intended effects of the information treatments. Appendix Table B3 confirms that covariates are well balanced across treatment and control groups within each experimental sample. No significant differences are observed for pre-treatment variables, supporting the validity of random assignment.

3.5. Power and compliance.

The study was powered to detect effect sizes of approximately Cohen's $d \approx 0.23$, based on a G*Power sensitivity analysis using standard assumptions for linear regression. With 95% power at $\alpha = 0.05$ and balanced randomization, the sample sizes in Studies 2 and 3 are sufficient

to identify small-to-moderate shifts in redistribution preferences and belief accuracy. No respondents were excluded post-randomization for failing attention checks.

4. Results

4.1. Study 1: Documenting well-being norms

We begin by using the Study 1 sample to estimate well-being norms—the income respondents believe others require to live a good life—and compare them to empirical benchmarks. Let θ denote the self-reported income needed for respondents themselves to achieve high life satisfaction (8/10 on the Cantril ladder). While not a population benchmark, θ provides a transparent, data-driven measure of perceived sufficiency. For each respondent, θ_i captures their belief about the income required by (a) the average American and (b) low-income families. The benchmark θ for low-income needs is computed from respondents with household incomes below $2,500/month.

Testing H1: Do individuals misperceive well-being norms?

Table 1 shows clear and sizable misperceptions. Respondents report needing a monthly income of $13,592.22 (S.D. = $20,197.24) to live well, while low-income respondents report needing $8,832.47 (S.D. = $16,955.77)**.** Yet they believe that the average American needs only $11,959.13 and low-income families just $7,624.75. Even beliefs about similar respondents fall short at $12,347.86**,** which is $1,244.36 below the benchmark. Column (2) confirms that all differences are statistically significant—at the 1% level for the average and low-income thresholds and 5% for similar others—providing strong support for H1.

A further asymmetry emerges. In the full sample, respondents believe *people like them* require slightly more than the average American. Among low-income respondents, the pattern reverses: those with very low household income believe similar households need less

than the average American. Perceptions of sufficiency are shaped not only by factual beliefs about others but also by one's own economic position.

Figures B1 and B2 visualize the full belief distribution. In both panels, perceived norms fall heavily below benchmarks. Respondents generally assume others need only 50–75% of the actual reported income. In total:

- **85%** underestimate the needs of an average American
- **86%** underestimate the needs of low-income families

Overestimation is rare. Misperceptions are not marginal variations; they are broad, systematic, and left-skewed across the distribution, reinforcing H1.

Who underestimate more?

Table 2 examines how well-being norms—and the gaps between perceived and empirical thresholds—vary with individual characteristics. We estimate a series of OLS regressions controlling for age, gender, citizenship, post-secondary education, white racial identity, and household size. Our primary predictors of interest are log-equivalent household income, political orientation, and self-reported confidence in one's estimates.

Columns (1)–(4) show results for four reference groups: respondents' own sufficiency thresholds, beliefs about *the* average American, similar others, and low-income families. Income is consistently the strongest predictor of higher sufficiency thresholds, but the effect is highly asymmetric across targets. Higher-income respondents report substantially higher thresholds for themselves and for similar others, and to a lesser degree for the average American. Yet when estimating what *low-income families* require, the income coefficient is small, positive, and statistically indistinguishable from zero. In effect, higher-income individuals scale up perceived needs for peers and for the population in general, but leave their

estimates of low-income needs largely anchored at a lower level. This pattern is consistent with an implicit assumption that poor households either require less or should get by with less.

These asymmetries help explain why income widens misperception gaps for the average American and similar respondents, but not for low-income families. Columns (8)–(10) further examine predictors of absolute belief gaps. Only one variable—self-reported confidence—is marginally associated with smaller misperceptions (10% level), and even then, the effect is modest. Other sociodemographic characteristics have limited explanatory power, underscoring the diffuse and widespread nature of belief errors across the population.

Testing H2: Do misperceptions predict redistribution preferences?

Table 3 evaluates whether well-being norms—and the gaps between perceived and empirical thresholds—predict respondents' life satisfaction, redistribution attitudes, or donation behaviour in Study 1, holding log household income constant. Across all specifications, the associations are weak, small, and statistically insignificant. At face value, this appears inconsistent with H2, which posits that individuals whose beliefs deviate more from benchmark norms ($| \theta_i - \theta |$) should exhibit weaker support for redistribution. This is particularly visible in the lower panels of Table 3, which isolate absolute misperception gaps: even substantial deviations from benchmark norms are only weakly related to policy preferences.

However, two interpretations follow. First, Table 3 is purely observational: belief variation is not experimentally induced. As such, coefficients cannot be interpreted causally, and null patterns do not falsify H2. Differences in belief gaps across individuals reflect priors, ideology, identity, and unobserved moral and political dispositions, all of which simultaneously shape redistribution preferences. In short, passive correlations are confounded and hard to interpret.

Second, this pattern may signal something deeper: **belief gaps in isolation may not matter**. Most respondents lack knowledge of empirical benchmarks, so misperceptions are not experienced as errors. They are simply unexamined estimates, not beliefs that individuals recognize as incorrect nor beliefs linked to real needs. Our framework suggests that belief deviations influence redistribution only once they are made salient, credible, and normatively meaningful. Neutral or uninformed beliefs are inert.

Thus, the observational nulls in Table 3 set the stage for experimental tests. If belief correction matters, it should matter when beliefs are shifted exogenously and made cognitively available for judgment. Studies 2 and 3 provide this variation by supplying norm-correcting information based on benchmark thresholds from Study 1: $13,592 for the average American and $8,832 for low-income families.

4.2. Studies 2 and 3: Correcting well-being norms and redistribution

We now test whether providing respondents with empirically derived well-being norms influences redistribution preferences. The key identifying variation comes from the randomized assignment of corrective information about the income required by (1) the average American or (2) low-income families. For respondent i, we estimate:

$$Y_i = \beta_1 \text{Treat\_Avg}_i + \beta_2 \text{Treat\_Poor}_i + \beta_3 X_i + \varepsilon_i \qquad (4)$$

where Y_i denotes either (a) redistribution attitudes or (b) the share of money donated to redistribution-aligned charities. Redistribution outcomes reflect stated policy preferences, while donation shares provide a behavioural measure. In all cases, higher values imply stronger support for redistribution.

Treat_Avg_i and Treat_Poor_i equal 1 if respondents received corrective information about the average American or low-income Americans. The reference group is a control condition that received no norm information, only a reminder of their earlier estimates. The covariate vector X_i contains preregistered controls: age, gender, citizenship, post-secondary education, white racial identity, political affiliation, household size, log-equivalized household income, and confidence in sufficiency estimates. Randomization ensures covariates are balanced; estimates with and without controls are near-identical (Appendix).

Testing H3: Does corrective information increase support for redistribution on average?

The top panel of Table 4 presents the treatment effects of providing corrective information on redistribution attitudes and donation behavior, based on Study 2 data (*N*=1,600). None of the coefficients, though positive, are statistically significant, indicating that simply presenting benchmark information does not shift either stated or revealed support for redistribution. These null results offer little support for H3: the hypothesis that providing accurate benchmark information improves belief accuracy and, on average, increases support for redistribution.

There are several possible explanations for this null result. First, respondents may have ignored or skimmed the information, resulting in limited belief updating. Second, even if they engaged with the content, they may have dismissed it as untrustworthy or irrelevant. Study 3 was explicitly designed to probe these mechanisms. It allows us to restrict the analysis to respondents who demonstrably engaged with the treatment, operationalized as correctly answering a comprehension check identifying the treatment content. Among the 863 respondents assigned to a treatment arm, 78 (approximately 9%) failed the check, suggesting limited engagement with the vignette. The lower panel of Table 4 focuses on those who passed.

However, even among attentive respondents, we continue to find statistically insignificant treatment effects on redistribution preferences or donation behavior.[3]

Another possibility is that respondents paid attention to the information but did not update their well-being norms accordingly. Study 3 (N = 1,606) enables us to test this directly, as it both re-elicited beliefs after the intervention and measured perceived credibility and personal relevance of the information.

Testing H4: Does corrective information reduce perceived norm gaps?

Table 5 evaluates whether providing corrective information changed respondents' beliefs about the income required for a good life in Study 3. Columns (1)–(2) estimate within-person changes in raw misperception gaps. Across both treatment arms, belief updating is positive and statistically meaningful: respondents revised their thresholds upward after receiving benchmark information. This is consistent with the fact that 82% of participants in Study 3 initially underestimated the benchmark values derived from Study 1**.** For instance, the coefficient on Correction: Average American is $1,804.75 (S.E. = $750.79, p = .016), while Correction: Low-Income *yields* $918.04 (S.E. = $453.12, p = .043). In both cases, respondents moved measurably closer to the true income thresholds they previously understated.

Columns (3)–(4) show treatment effects on the absolute belief gap: our preferred accuracy measure. Benchmark information meaningfully reduced misperceptions by $3,713 (S.E. = $650.52, p < .001) for the average-American threshold and $2,908 (S.E. = $403.31, p < .001) for the low-income benchmark. For comparison, baseline gaps were $9,151 and $5,811, implying reductions of roughly 40%–50% of initial misperception.

[3] We find the same absence of results when looking at life satisfaction as an outcome. See the results in Appendix Table B4.

Figure B3 in the Appendix visualizes this shift: both distributions move rightward toward the empirical anchor, narrowing dispersion and shrinking the left tail of extreme underestimates.

Taken together, these findings strongly support H4. Information was not ignored: respondents absorbed and updated their beliefs in the intended direction. This rules out inattentiveness or non-responsiveness as explanations for the null behavioral effects observed under H3. The null average effect, therefore, raises a puzzle: what explains why belief updating does not translate into preference change? We address this by examining heterogeneity in how respondents received the information (H5).

Understanding the Null Average Effect: Heterogeneity in Information Reception (H5)

Table 6 investigates this puzzle by analyzing differences in how people receive information. After viewing the benchmark income data, respondents rated its believability and personal relevance. These two ratings are closely connected: respondents usually saw the message as both credible and relevant or dismissed both. In the average-American group, 31% rated the information as neither believable nor relevant, 38% were undecided, and 31% endorsed both. The pattern in the low-income group is very similar: 27%, 41%, and 32%, respectively. Because of this clustering, we categorize respondents into three reception profiles—Dismissive, Mixed, and Receptive—and evaluate treatment effects separately for each group. We emphasize that these reception measures were gathered after the treatment and therefore reflect observed heterogeneity rather than proving causal effects of credibility itself. (The detailed versions using believability and relevance separately are shown in Appendix Table B5 and lead to the same conclusions.)

The most striking pattern in Table 6 concerns participants who received the low-income norm information but did not find it believable or relevant. Among this group, treatment effects are negative across all redistribution outcomes and statistically significant in

every case. Support for UBI falls by 1.235 points ($p < .001$), support for higher top tax rates falls by 0.922 ($p = .010$), support for reducing inequality falls by 1.138 ($p < .001$), and donations fall by 0.113 of the $10 endowment ($p = .007$).

When converted to standard deviations, these shifts roughly equate to 25–40% of a standard deviation, which is substantial given the minimal intervention. This pattern is consistent with the conditional logic outlined in H5: among those who perceived information as neither trustworthy nor relevant ($\kappa_i \approx 0$ or negative), we observe decreased redistribution support rather than merely null effects.

The same message is associated with the opposite pattern among those who found it credible. Among respondents who judged the low-income correction as both believable and personally relevant, support for UBI rises by 0.653 points ($p = .002$), support for higher taxes increases by 0.636 ($p = .049$), support for reducing inequality increases by 0.362 ($p = .170$), and donations increase by 0.083 ($p = .014$). These effects are smaller than the backlash estimates but remain meaningful at 11–22% of a standard deviation, consistent with the pattern predicted when κ_i is high.

Because Table 6 reports 24 treatment-by-reception coefficients, some significant effects might occur by chance. We therefore apply the Westfall & Young (1993) resampling correction and report FWER-adjusted p-values in brackets. Of the seven coefficients significant at conventional levels, two remain below $p < .05$ after adjustment, both indicating backlash among dismissive low-income recipients. The remaining effects weaken under correction, which is consistent with the conservative nature of FWER control and reinforces the need for cautious interpretation.

To compress the outcome space, Table B6 in the Appendix constructs a redistribution index using the first factor of the three attitudinal items. Re-estimating heterogeneous effects using this single index yields the same qualitative conclusion: receptive respondents move upward, dismissive respondents move downward, and the pattern is exclusive to low-income information. Because this specification requires only eight comparisons instead of 24, adjusted p-values stabilize accordingly.

A final point concerns the treatment-arm asymmetry. Information about the average American significantly narrowed belief gaps but produced no differential behavioral responses, even among respondents who found it credible and relevant. In contrast, while low-income information also reduced misperceptions, only this treatment generated heterogeneous patterns in redistribution outcomes, and only in the low-income condition does heterogeneity remain after multiple-testing correction. This asymmetry suggests that redistribution preferences may be more responsive to information about low-income needs than about median income levels. Information about typical households updated beliefs without generating differential responses across reception profiles, while information focused on low-income families produced heterogeneous patterns consistent with the activation of fairness concerns among receptive respondents and backlash among dismissive ones.

In summary, Table 6 shows that the null average effect masks substantial heterogeneity. Among those who found the information credible and relevant, we see increased support for redistribution; among those who dismissed it, we see decreased support. Importantly, this varied pattern mainly appears in the low-income treatment group, indicating that message content—not just belief accuracy—affects how information is received and whether it influences preferences. These patterns align with H5's prediction of different effects depending on reception: positive associations when reception is good, negative associations when

information is rejected. The asymmetry is notable: backlash among dismissive respondents is stronger and more statistically robust than the support observed among receptive ones. This suggests that the same corrective information can lead to opposite responses depending on reception, with negative reactions being especially strong.

4.3. Exploratory analysis

Heterogeneity beyond message reception

While our core results document heterogeneous responses conditional on how information was received, they do not indicate whether treatment effects vary systematically with baseline individual characteristics.

Tables B7 and B8 in the Appendix present these results. Table B7 investigates whether treatment effects vary based on deservingness beliefs — the moral view that poverty results from structural disadvantage rather than personal failure. If redistribution preferences are influenced by moral judgments in addition to factual updates, then people who see poverty as undeserved might be more affected by information about unmet needs. However, the data do not support this idea. The interaction terms are small, have inconsistent signs, and are never statistically different from zero.

Table B8 examines two additional moderators: (i) the level of baseline misperception and (ii) respondents' confidence in their initial estimates. Once again, we observe no consistent patterns. Treatment effects stay near zero across different groups, and none of the interactions reach statistical significance. Overall, these findings indicate that heterogeneity in treatment effects is not systematically predicted by baseline characteristics—an outcome consistent with the observed pattern that responses vary primarily with post-treatment reception rather than pre-existing individual traits.

Is belief updating the mechanism?

Tables 4 and 5 show that corrective information significantly reduces misperceptions, especially in the low-income group. However, behavioral effects only occur among individuals who accept the information. If belief updating is the mechanism through which information influences redistribution preferences, we should observe mediation: the treatment should affect outcomes through changes in perceived well-being norms.

To test this, we estimate causal mediation models following the approach of Imai et al. (2010). Tables B9 and B10 report the natural indirect effects using the post-treatment absolute belief gap as the mediator. Across all redistribution outcomes, these indirect effects are small, inconsistent in direction, and never statistically different from zero. In short, correcting beliefs alone does not explain downstream changes in support for redistribution.

Table B11 clarifies the mechanism: nearly all treated respondents, including those who rejected the information, report significant changes in their stated income thresholds. For receptive individuals, these changes likely represent genuine updates. For those who dismissed it, however, the apparent shifts may instead reflect superficial adjustment or experimental-demand compliance rather than true belief revision. In other words, belief measures capture movement, but not necessarily the meaningful normative understanding needed to influence redistribution choices. The implication is that **updating is necessary but not sufficient**. What matters is whether updated information becomes something respondents care about, not whether they can recall it.

Across all exploratory analyses, we can conclude that:

- **E1 (mediation):** Not supported. Incorrect normative beliefs can be corrected, but updating does not mediate treatment effects.

- **E2 (deservingness moderation):** Not supported. Moral beliefs do not enhance the effect of information.
- **E3 (treatment asymmetry):** Supported. Only low-income information changes redistribution behavior.
- **E4 (heterogeneity by priors/ideology):** Not supported. Responsiveness does not differ by misperception level, income, or ideology.

Together, these findings suggest that belief updating alone does not automatically translate into preference change. We observe behavioral responses primarily among those who found the information credible, relevant, and morally salient, particularly when the message highlighted low-income needs rather than average benchmarks. This pattern is consistent with the possibility that redistribution attitudes respond to corrective information that activates normative concerns about material sufficiency for the least fortunate, though we cannot establish whether the reception itself is exogenous to prior preferences.

5. Discussion and Conclusion

This paper explored whether misperceptions of *well-being norms*—beliefs about the income *others* require to live a good life—serve as an independent factor influencing support for redistribution. Study 1 showed that these misperceptions are common and sizable: 85% of respondents underestimated the income required for the average American to feel highly satisfied with life, and 86% underestimated what low-income families need. On average, perceptions were $1,200–$1,600 below actual sufficiency thresholds, showing a distorted view of what constitutes a minimally adequate standard of living for others.

Studies 2 and 3 then asked whether correcting these misperceptions changes redistributive preferences. Aggregate results suggest not. Across both attitude measures and

incentivized donation choices, corrective information did not, on average, increase support for redistribution. A naïve interpretation would be that beliefs about others' material sufficiency carry little persuasive weight. However, the pattern revealed by heterogeneity analyses suggests a very different conclusion.

First, information shifted beliefs. Across both corrective treatments, participants exposed to the information increased their estimates of required income and significantly reduced the absolute belief gap. For the low-income framing, misperceptions narrowed by nearly $3,000 on average compared to baseline—substantial movement given the brevity of the intervention and the lack of incentives to update. This rules out inattentiveness or shallow processing as explanations for the null average treatment effect on redistribution preferences.

Second, the null average effect masks substantial heterogeneity in how people respond. Among participants who found the corrective information both credible and personally relevant, we observe increased support for redistribution; among those who viewed it as neither credible nor relevant, we see decreased support. The same message is thus associated with opposing responses depending on how it is received. These opposite patterns offset each other, resulting in a null average effect. In other words, the null ATE hides important variation in how different individuals interpret the same information.

Third, message content appears to matter for how information is received. While both treatment groups successfully updated beliefs (with messages about low-income situations causing slightly larger reductions in misperceptions), only the low-income correction led to diverse behavioral responses, and only this pattern remained significant after Westfall–Young FWER adjustment. This asymmetry suggests that information emphasizing low-income needs may better activate normative concerns than abstract benchmarks about the average American. The pattern of heterogeneity—positive associations among receptive respondents and negative associations among dismissive ones—supports the idea that informational interventions can

shape attitudes toward redistribution when they resonate with the moral reasons for redistribution, but may provoke backlash if seen as irrelevant or implausible.

While our design does not identify the psychological roots of misperceptions, the heterogeneous treatment effects offer clues about potential mechanisms. The fact that corrections backfire among dismissive respondents suggests that motivated reasoning may play a role—individuals might underestimate others' needs to justify existing distributions—rather than simple information deficits from limited social sampling (Galesic et al., 2012) or lack of public discourse (Bartels, 2005; Gilens, 2001). If misperceptions only resulted from insufficient exposure to population-wide patterns, we would expect corrections to uniformly shift preferences once beliefs are updated. Instead, the polarized response pattern is more consistent with motivated cognition, where recognising others' needs threatens beliefs about distributive justice. Future work could directly test whether motivated reasoning, social distance, or information deficits best explain these systematic biases.

Taken together, our findings show that well-being norms are a unique and policy-relevant belief area. Misperceptions are common and significant, but they can be effectively corrected through informational interventions. However, changing beliefs does not necessarily lead to a shift in redistribution preferences: even when beliefs are updated significantly, the average treatment effect on policy support remains zero. This overall null effect masks heterogeneous responses that depend on whether people see the information as credible, relevant, and morally resonant. These patterns underscore the importance of including well-being norms along with inequality misperceptions in models of redistribution preferences. They also point out the limits of purely informational approaches: corrective information influences behavior only when it gains normative acceptance, not just factual acknowledgment.

For policymakers, the results caution against the assumption that providing factual clarification automatically increases support for redistribution. We find null average effects on

redistribution preferences despite significant belief updating, with heterogeneous responses depending on reception: the same message is associated with increased support among those who found it credible and relevant, and decreased support among those who dismissed it. Messages highlighting low-income needs generated stronger heterogeneous responses than those referencing average Americans, suggesting that content emphasizing unmet needs may resonate more effectively than abstract statistical benchmarks. These patterns indicate that informational interventions face inherent limitations: corrective information may fail to influence preferences—or even produce backlash—when recipients perceive it as neither credible nor personally relevant.

This study is not without limitations. First, our treatments were brief, text-only, and delivered online; real-world persuasion is usually more sustained, visual, and socially connected. External validity may therefore be limited, especially regarding benchmark values from the study sample. Future research should examine messenger credibility, narrative framing, and whether well-being norms can be shifted more durably and on a larger scale.

Second, we cannot know how long belief changes or attitude shifts last. Corrections may fade quickly or grow stronger with repetition. Long-term follow-ups would help distinguish temporary updates from enduring normative change.

Third, a key limitation concerns our measurement of how people receive information. In Study 3, credibility and relevance were rated after the treatment, which raises concerns about post-treatment selection bias and reverse causality: dismissive respondents might reject information because it conflicts with their preferences, rather than adjusting their preferences because they find it implausible. We cannot fully eliminate this endogeneity. However, several patterns suggest it does not entirely explain our findings: baseline characteristics do not predict treatment effects (Tables B7-B8); the asymmetry between treatment groups—where average-American corrections update beliefs without behavioral changes while low-income corrections

provoke opposite responses—indicates reception depends on message content, not just prior beliefs; and receptive respondents show revealed-preference effects (donations) alongside attitudinal shifts. The ideal approach would be to randomize credibility primes before exposure to information, which we leave for future research.

Fourth, the experiments use U.S. income thresholds and U.S. respondents. Well-being norms are culturally dependent: what qualifies as "enough" varies across welfare systems, inequality levels, and social comparison groups. The extent of misperception and whether corrections backfire might differ in low-inequality or high-solidarity contexts.

Finally, our benchmark—the income needed to achieve high life satisfaction (8/10)—reflects flourishing rather than mere subsistence. One could argue that redistribution debates often focus on minimum needs. However, in most wealthy democracies, political conflict over redistribution centers on dignity, security, and what it means to live well, not just survive. A high-well-being benchmark therefore captures a psychologically meaningful idea of adequacy. That misperceptions remain large even at this higher standard strengthens our core argument: public beliefs about what others need are systematically distorted, and when information is credible and relevant, correcting those beliefs can influence political attitudes.

In conclusion, attitudes toward redistribution depend not only on perceived inequality or self-interest but also on beliefs about what others need to live well. We show that these beliefs are systematically biased: most people significantly underestimate material needs. Corrective information updates beliefs but not preferences; the overall null effect hides opposing responses that vary based on how the information is received. Therefore, understanding norms of well-being may be key for redistribution policies, although purely informational approaches face inherent limitations.

Declaration of Generative AI and AI-assisted technologies in the writing process

The authors used AI tools (ChatGPT, Claude, and Grammarly) to enhance the clarity and readability of the manuscript. All research, analysis, and intellectual content are solely the authors' own work.

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Table 1: Descriptive Statistics

	All sample		Respondents with household income below $2,500
	Average level	Difference with norms	Average level
First-order belief:			
Personal threshold	13592.22		8832.47
	[20197.24]		[16955.77]
Second-order beliefs:			
Income of average American	11959.13	-1633.08***	10448.86
	[17158.03]	(524.05)	[16955.16]
Income of similar respondent	12347.86	-1244.36**	8645.46
	[17227.14]	(526.16)	[15076.19]
Income of low-income families	7624.75	-1207.72***	6653.60
	[12142.62]	(370.86)	[11153.34]

Notes: This table shows descriptive statistics from Study 1. The first row indicates the average personal income needed to reach 8 out of 10 on the life satisfaction scale, serving as the benchmark for well-being standards. The remaining rows display respondents' second-order beliefs about how much income others need. Column (2) shows differences in means compared to the actual benchmarks. Standard deviations are in square brackets, and standard errors are in parentheses. Significance levels are indicated as follows: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 2: Determinants of Belief–Personal and Belief-Actual Gaps (Study 1)

	Beliefs about:				Gap btw Beliefs and Personal		
	Personal threshold	Average American	Similar respondent	Low-income Families	Average American	Similar respondent	Low-income Families
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Confidence	0.720	0.713	0.948*	0.546	-0.006	0.229	-0.174
	(0.659)	(0.568)	(0.562)	(0.400)	(0.587)	(0.520)	(0.555)
Eq. HH Income	3.865***	1.432**	2.948***	0.615	-2.433***	-0.916	-3.249***
	(0.826)	(0.712)	(0.705)	(0.502)	(0.736)	(0.652)	(0.697)
Political Orientation:							
Republican	-0.757	-1.489	-1.736	-0.802	-0.732	-0.979	-0.045
	(1.518)	(1.309)	(1.296)	(0.923)	(1.352)	(1.197)	(1.279)
Other	0.273	0.329	0.716	0.752	0.056	0.443	0.479
	(1.492)	(1.286)	(1.274)	(0.907)	(1.329)	(1.177)	(1.258)
Observations	1072	1072	1072	1072	1072	1072	1072
R-squared	0.041	0.012	0.040	0.019	0.034	0.015	0.040
Average of DV	13.592	11.959	12.348	7.625	-1.633	-1.244	-5.967
SD of DV	20.197	17.158	17.227	12.143	17.922	15.721	17.017

	Absolute Gap btw Beliefs and Empirical Benchmark		
	Average American	Similar respondent	Low-income Families
	(8)	(9)	(10)
Confidence	0.973*	0.576	0.881*
	(0.550)	(0.475)	(0.524)
Eq. HH Income	-0.469	-0.533	0.776
	(0.730)	(0.586)	(0.732)
Political Orientation:			
Republican	-1.176	-0.191	-0.886
	(1.060)	(0.783)	(1.034)
Other	0.179	0.569	0.834
	(1.133)	(0.807)	(1.128)
Observations	1072	1072	1072
R-squared	0.019	0.020	0.039
Average of DV	9.523	6.169	8.249
SD of DV	14.363	10.526	14.453

Notes: This table presents OLS estimates. All regressions include controls for age, gender, citizenship, partnership status, a missing-income indicator, postsecondary education, race (white), and household size. Robust standard errors are shown in parentheses. Significance levels are indicated as follows: * p < .10, ** p < .05, *** p < .01.

Table 3: Determinants of attitudes and behaviour (Study 1)

	Life satisfaction	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
		Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)	(5)
Beliefs about:					
Personal threshold	-0.002	0.007	0.012*	0.008	0.001
	(0.004)	(0.006)	(0.007)	(0.006)	(0.001)
Average American	-0.009	0.008	-0.002	0.012	0.001
	(0.007)	(0.009)	(0.010)	(0.009)	(0.001)
Similar respondent	-0.006	-0.001	-0.011	-0.016*	-0.000
	(0.007)	(0.010)	(0.011)	(0.009)	(0.001)
Low-income families	0.003	-0.015	0.006	-0.009	-0.003**
	(0.008)	(0.011)	(0.013)	(0.011)	(0.001)
Equivalent HH Income	0.846***	-0.350***	-0.795***	-0.537***	0.006
	(0.091)	(0.121)	(0.141)	(0.120)	(0.015)
Observations	1072	1065	1059	1052	1072
R-squared	0.247	0.230	0.193	0.256	0.053
Gap in wellbeing norm w.r.t					
Average American	-0.013**	0.011*	-0.003	0.007	0.001
	(0.006)	(0.006)	(0.008)	(0.007)	(0.001)
Low-income families	-0.000	-0.012	0.008	-0.012	-0.002**
	(0.008)	(0.009)	(0.010)	(0.009)	(0.001)
Equivalent HH Income	0.829***	-0.333***	-0.781***	-0.543***	0.009
	(0.096)	(0.124)	(0.135)	(0.115)	(0.015)
Observations	1072	1065	1059	1052	1072
R-squared	0.246	0.229	0.190	0.253	0.052
Absolute gap in wellbeing norm w.r.t					
Average American	-0.010	0.009	-0.007	0.004	0.001
	(0.007)	(0.006)	(0.009)	(0.008)	(0.001)
Low-income families	0.003	-0.009	0.010	-0.010	-0.002*
	(0.009)	(0.010)	(0.011)	(0.010)	(0.001)
Equivalent HH Income	0.807***	-0.325***	-0.779***	-0.544***	0.008
	(0.097)	(0.123)	(0.135)	(0.115)	(0.015)
Observations	1072	1065	1059	1052	1072
R-squared	0.240	0.229	0.190	0.253	0.051

Notes: This table shows OLS estimates. All regressions account for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and confidence in responses. Robust standard errors are given in parentheses. Significance levels are indicated as follows: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 4: Reactions to corrective treatments, redistribution items, and donation (Study 2) – OLS results

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Estimation Sample: Study 2				
Correction: average	-0.273*	-0.151	-0.192	-0.005
	(0.165)	(0.210)	(0.170)	(0.023)
Correction: low-income	0.200	0.109	0.099	0.009
	(0.165)	(0.209)	(0.169)	(0.023)
Observations	1587	1571	1577	1599
R-squared	0.232	0.164	0.256	0.056
Estimation Sample: Study 3				
Correction: average	0.022	-0.025	-0.167	-0.027
	(0.166)	(0.205)	(0.167)	(0.023)
Correction: low-income	-0.224	0.143	-0.191	0.006
	(0.165)	(0.204)	(0.166)	(0.023)
Observations	1597	1580	1589	1606
R-squared	0.249	0.224	0.312	0.045
Estimation Sample: Study 3 excluding those who failed comprehension check				
Correction: average	0.056	0.028	-0.167	-0.016
	(0.170)	(0.211)	(0.172)	(0.024)
Correction: low-income	-0.216	0.123	-0.185	0.009
	(0.170)	(0.212)	(0.172)	(0.024)
Observations	1519	1502	1511	1528
R-squared	0.253	0.226	0.317	0.046

Notes: This table reports OLS estimates of the treatment effects of the corrective information on each redistribution items separately and the percentage of the $10 dollar lottery-reward respondents would be willing to donate to charities. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < .10$, ** $p < .05$, *** $p < .01$

Table 5: Beliefs updating of well-being norms (Study 3) – OLS results

	Raw change in well-being norm		Absolute change in well-being norms	
	Average American	Low-income families	Average American	Low-income families
	(1)	(2)	(3)	(4)
Correction: average	1804.745**	1420.643**	-3712.898***	-901.266*
	(750.788)	(556.39)	(650.524)	(477.433)
Correction: low-income	-1087.589	918.043**	-2493.411***	-2908.303***
	(693.139)	(453.119)	(583.475)	(403.310)
Observations	1528	1528	1528	1528
R-squared	0.037	0.022	0.048	0.044
Misperceptions in well-being norm beliefs pre-treatment:				
Average	-2003.94	-1800.47	9151.56	5811.35
Standard deviation	16188.55	10744.70	13501.17	9214.04

Notes: This table presents OLS estimates of how corrective information affects updates to well-being norms, based on data from Study 3 and excluding respondents who failed the comprehension check. The dependent variable captures the change in the gap between respondents' beliefs about the income needed for a good life and the benchmark norms from Study 1, comparing responses before and after the treatment. Coefficients are reported in U.S. dollars. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are shown in parentheses. Significance levels are indicated as follows: * p < .10, ** p < .05, *** p < .01.

Table 6: Reactions to corrective treatments, redistribution items and donation (Study 3) – OLS results

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Joint reaction to "Correction: average"				
Don't believe it & Not relevant	-0.186	0.222	-0.313	-0.078**
	(0.262)	(0.338)	(0.292)	(0.039)
	[0.997]	[0.997]	[0.982]	[0.541]
Mix	0.122	-0.017	-0.150	-0.005
	(0.228)	(0.306)	(0.233)	(0.034)
	[0.997]	[0.999]	[0.999]	[0.999]
Believe it & Relevant	0.240	-0.080	-0.021	0.033
	(0.257)	(0.335)	(0.269)	(0.033)
	[0.990]	[0.999]	[0.999]	[0.990]
Reaction to "Correction: low-income"				
Don't believe it & Not relevant	-1.235***	-0.922***	-1.138***	-0.113***
	(0.338)	(0.358)	(0.309)	(0.042)
	[0.010]	[0.175]	[0.010]	[0.143]
Mix	-0.245	0.378	-0.008	0.028
	(0.246)	(0.298)	(0.239)	(0.033)
	[0.990]	[0.946]	[0.999]	[0.990]
Believe it & Relevant	0.653***	0.636**	0.362	0.083**
	(0.213)	(0.324)	(0.263)	(0.034)
	[0.057]	[0.541]	[0.928]	[0.237]
Observations	1519	1502	1511	1528
R-squared	0.266	0.233	0.325	0.059

Notes: This table reports OLS estimates of the treatment effects of the corrective information on each redistribution item separately and on donations. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political

affiliation, household size, and self-assessed confidence in responses to beliefs. Robust standard errors are in parentheses, and p-values adjusted for multiple hypothesis testing are in square brackets. Significance levels are indicated as follows: * $p < .10$, ** $p < .05$, *** $p < .01$.

Supplementary Materials

Online Appendix A: Additional details about the experiment

Verbatim of the information treatment:

Respondents were randomly assigned (equal probability) to one of three groups:

1. **Average-American correction:** Participants saw a personalized message reminding them of their own earlier estimate and presenting factual information about what other Americans actually reported in Survey 1. The message read, in part:

   > "Previously, you estimated that **the average American** needs ['value reported above'] of pre-tax household income per month to feel highly satisfied with life (a rating of 8–10 on a 0–10 scale).
   >
   > You might be interested to know that, in **a recent nationally representative survey of 1,072 Americans,** the average respondent reported needing **$13592** of pre-tax household income per month to feel highly satisfied with life.
   >
   > In fact, we found that **85%** of the people we surveyed **underestimated** the amount of income **other Americans** actually need to feel highly satisfied with their lives."

   The text concluded by summarizing the implication that "most people believe others require less income than they actually report."

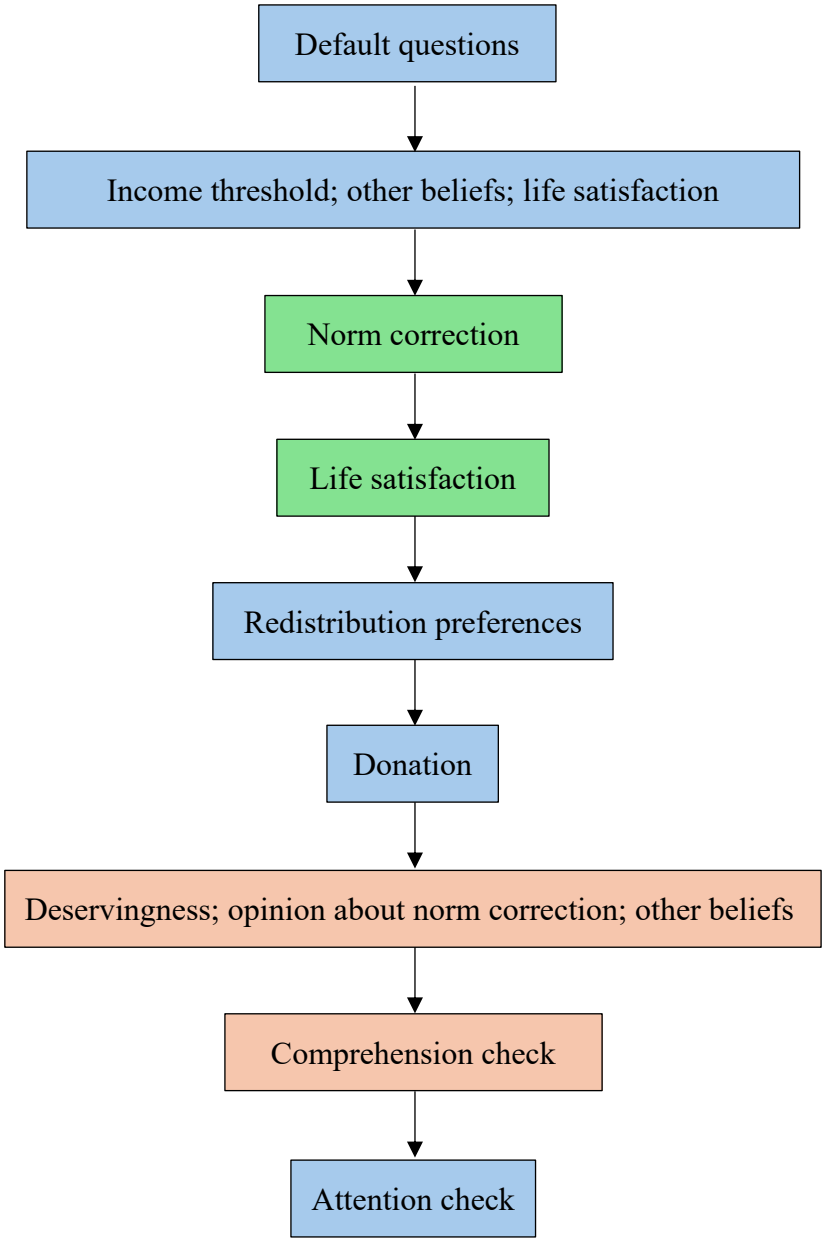
2. **Low-income family correction:** This treatment was identical in structure, but referred specifically to people from low-income families—defined as those with household incomes of $2,500 or less per month—and used the benchmark from Study 1:

> "Respondents estimated that individuals from low-income families need **$8,832 per month** to feel highly satisfied. In fact, **86%** of people underestimated the amount of income people from **low-income families** actually need to feel highly satisfied with their lives."

The framing emphasized that others perceived the needs of people from low-income families to be substantially higher than respondents typically assumed.

3. **Control condition**: Participants in the control group saw only the introductory sentence ("Previously, you estimated that the average American […]/people from low-income families […] need [respondent's value] per month …") but no factual information. This design ensured that any effects could be attributed to the informational content rather than message format or cognitive salience.

Figure A1: Structure of the questionnaires

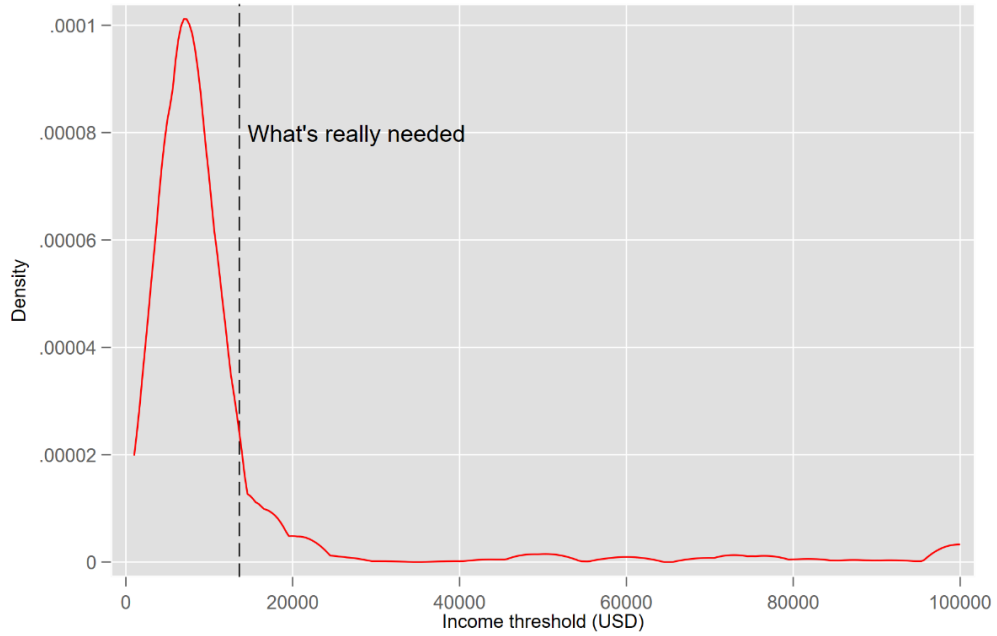


Notes: This figure illustrates the structure of the questionnaires across the three studies. Modules shown in blue were included in all studies, modules in green were introduced in Studies 2 and 3, and modules in orange were specific to Study 3 only. In Studies 2 and 3, respondents were randomly assigned to one of the information treatments ("norm correction") or to the control condition. Study 3 additionally included follow-up questions on deservingness, opinions about the norm correction, and other beliefs, as well as a comprehension check and, finally, an attention check to verify recall of the treatment information.

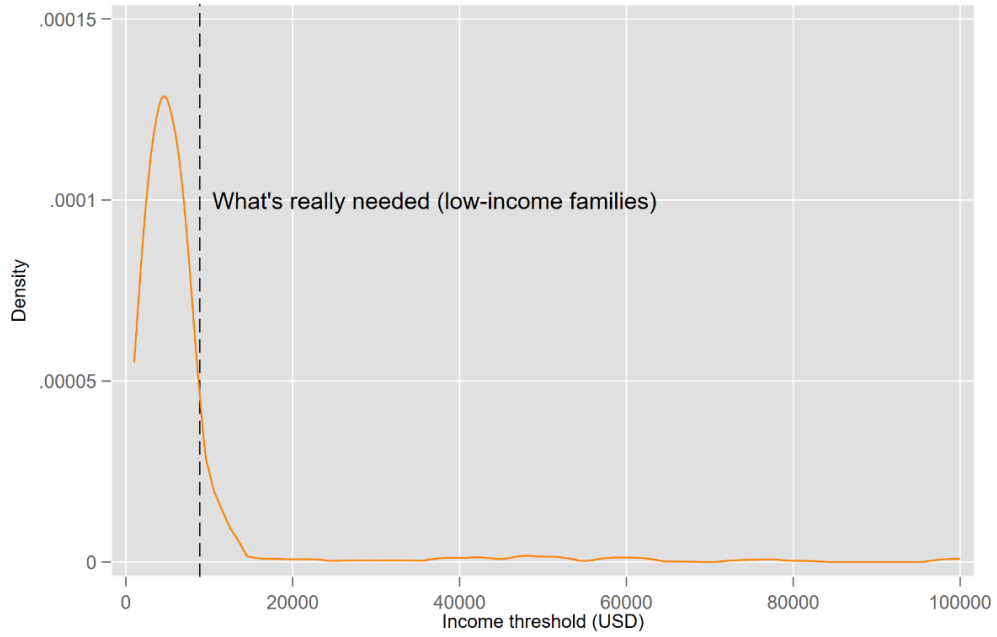
Online Appendix B: Exploratory and complementary pre-registered analyses

Figure B1: Misperceptions in well-being norms

Panel (A) – Beliefs about the average American



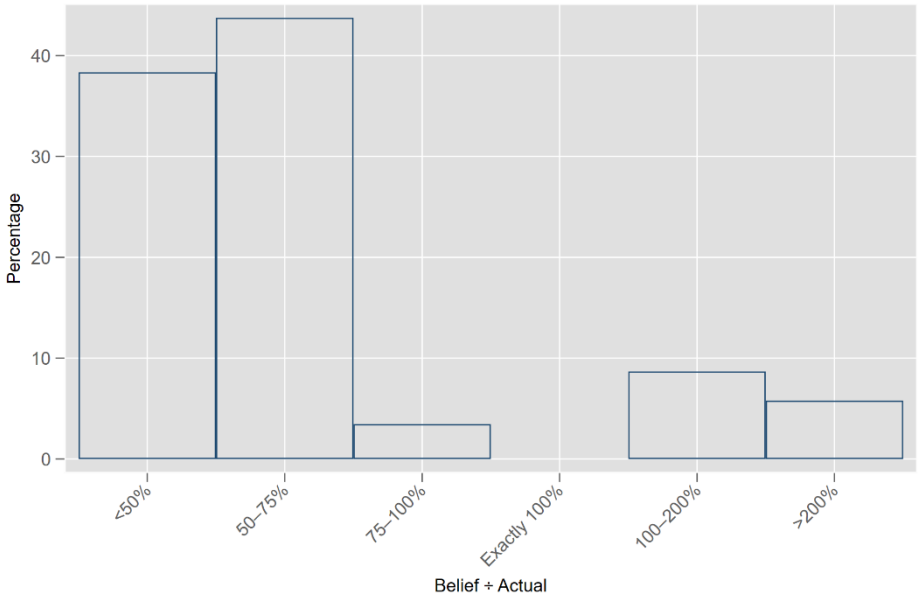
Panel (B) – Beliefs about people from low-income families



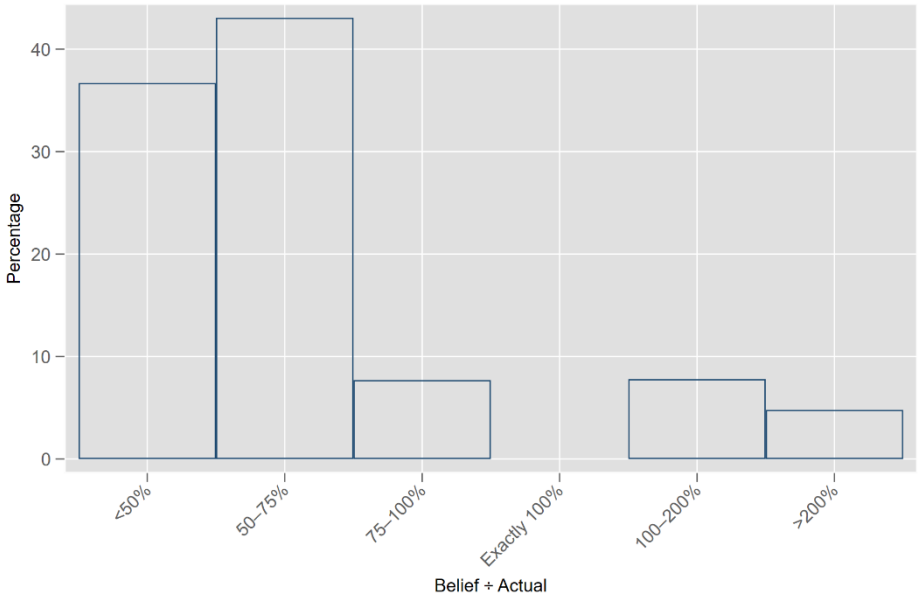
Notes: The figure plots kernel density estimates of respondents' beliefs about the minimum monthly pre-tax household income required to feel highly satisfied with life (8 out of 10 on a life satisfaction scale), using data from Study 1. Panel (A) displays beliefs about the average American (red line), while Panel (B) displays beliefs about individuals from low-income families with household incomes of $2,500 or less per month (orange line). The vertical dashed black line in each panel marks the corresponding benchmark level actually reported by respondents for themselves in Study 1, which we interpret as the true well-being norm. In both panels, the bulk of the distribution lies to the left of the benchmark, showing that most respondents systematically underestimate how much income is really needed for a good life.

Figure B2: Proportions between beliefs and actual income needed

Panel (A) – Beliefs about the average American



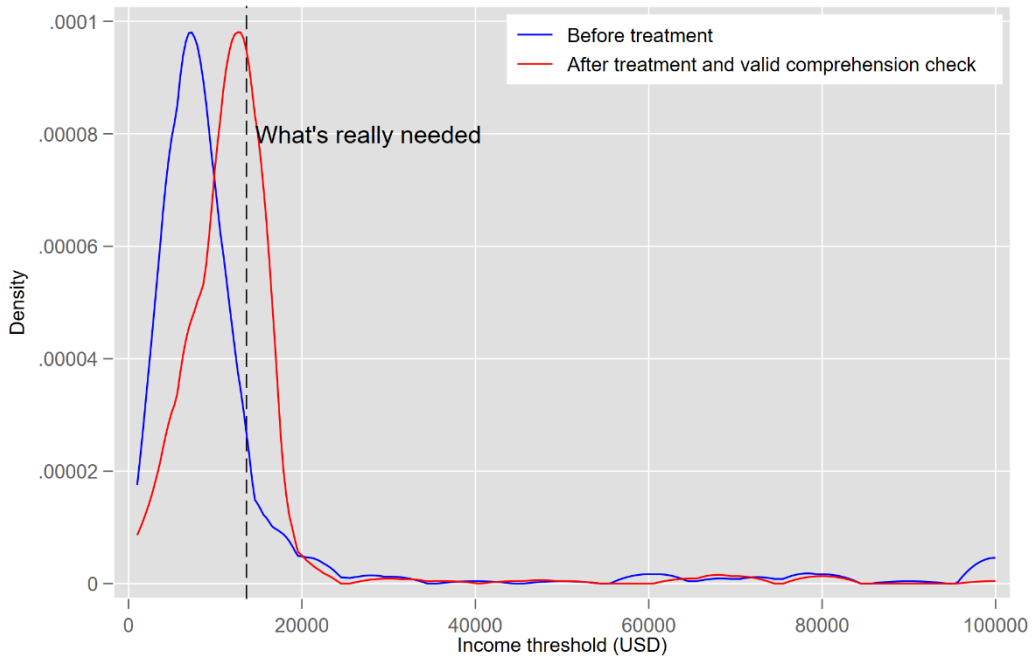
Panel (B) – Beliefs about people from low-income families



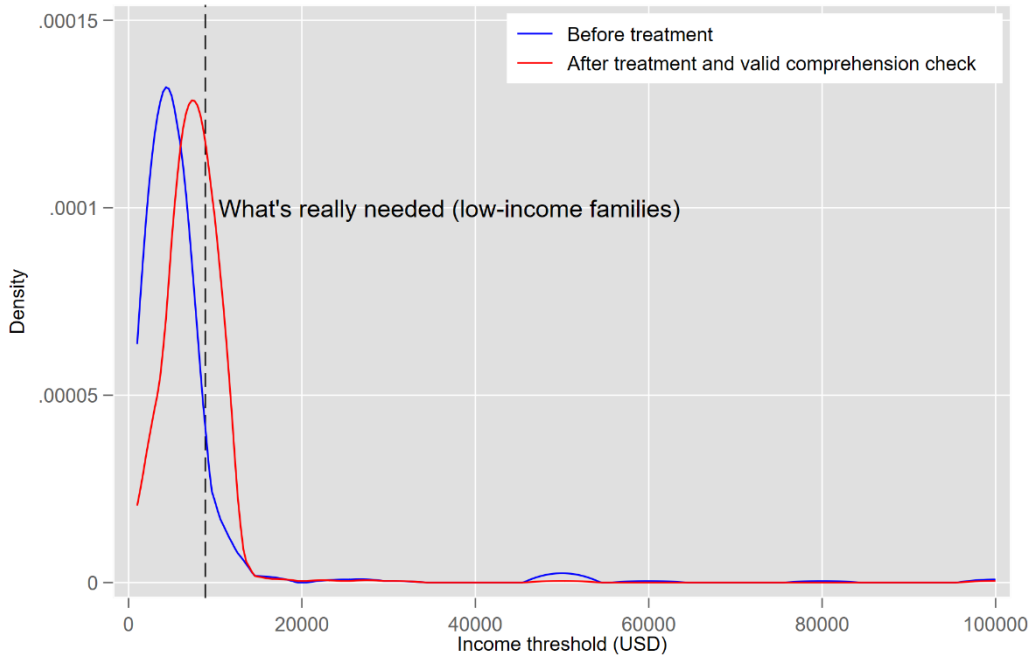
Notes: The figure shows the distribution of the ratio between respondents' beliefs about the minimum monthly pre-tax household income needed for a good life (8 out of 10 on the life satisfaction scale) and the actual benchmark reported in Study 1. Panel (A) reports results for beliefs about the average American, while Panel (B) reports results for beliefs about individuals from low-income families (with household incomes of $2,500 or less per month). Values below 100 percent indicate underestimation of the true benchmark, while values above 100 percent indicate overestimation. In both panels, the majority of respondents fall in the 50–75 percent range, confirming that most systematically underestimate the income required for a good life.

Figure B3: Shift of well-being norms

Panel (A) – Beliefs about the average American



Panel (B) – Beliefs about people from low-income families



Notes: This figure plots kernel density estimates of respondents' beliefs about the minimum monthly pre-tax household income required to feel highly satisfied with life (8 out of 10 on the life satisfaction scale), before and after receiving the corrective information in Study 3. Only respondents who passed the comprehension check are included in the post-treatment distributions. Panel (A) shows beliefs about the average American, and Panel (B) shows beliefs about individuals from low-income families (with household incomes of $2,500 or less per month). The vertical dashed black lines mark the benchmark norms established in Study 1 ($13,592 for the average American and $8,832 for low-income families). In both panels, the red distribution (post-treatment) shifts toward the benchmark relative to the blue distribution (pre-treatment), indicating that respondents updated their beliefs in the expected direction after receiving the information.

Table B1: Descriptive statistics of the three samples and differences in means

	Study 1	Study 2	Study 2 – Study 1	Study 3	Study 3 – Study 1	Study 3 – Study 2
	(1)	(2)	(3)	(4)	(5)	(6)
Personal threshold	13592.215	14552.827	960.611	14169.710	577.494	-383.117
	(20197.237)	(21991.929)	[840.407]	(20980.994)	[815.256]	[759.241]
Beliefs:						
Income of average American	11959.134	12453.004	493.870	11696.473	-262.661	-756.531
	(17158.034)	(18140.259)	[700.775]	(16394.472)	[658.813]	[610.730]
Income of low-income families	7624.750	7695.128	70.378	7163.011	-461.739	-532.118
	(12142.620)	(13046.905)	[501.000]	(11110.741)	[454.931]	[428.012]
Redistribution items:						
Introduction of a universal basic income $^{+}$	7.369	7.711	0.342***	7.472	0.102	-0.239**
	(3.209)	(3.033)	[0.123]	(3.088)	[0.124]	[0.108]
Increasing taxes on high earners^{+}	5.538	5.796	0.257*	5.437	-0.102	-0.359***
	(3.674)	(3.677)	[0.146]	(3.733)	[0.147]	[0.132]
Doing more to reduce income inequality^{+}	7.194	7.412	0.218*	7.157	-0.037	-0.255**
	(3.239)	(3.161)	[0.127]	(3.245)	[0.129]	[0.114]
Money donated^{+}	4.832	4.969	0.137	5.360	0.528***	0.391***
	(3.676)	(3.763)	[0.147]	(3.791)	[0.148]	[0.133]
Help low-income families (e.g., Feeding America)$^{+}$	2.940	3.022	0.082	3.386	0.447***	0.365***
	(2.846)	(2.889)	[0.113]	(3.098)	[0.118]	[0.106]
Improve public services (e.g., DonorsChoose) $^{+}$	1.079	1.098	0.019	1.106	0.027	0.008
	(1.471)	(1.424)	[0.057]	(1.448)	[0.057]	[0.051]
Support economic growth (e.g., Kiva)$^{+}$	0.814	0.849	0.036	0.868	0.054	0.019
	(1.210)	(1.246)	[0.049]	(1.329)	[0.051]	[0.046]

Notes: This table reports descriptive statistics of the three survey samples (Studies 1–3) and differences in means across them. Columns (1)–(2) present averages for Studies 1 and 2, with Column (3) reporting their difference. Columns (4)–(6) present analogous comparisons for Study 3. Standard deviations are reported in square brackets, while standard errors of the mean are reported in parentheses. Variables marked with a "+" were collected after the information treatments in Studies 2 and 3 and are therefore potentially affected by them. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Balance of covariates across studies

	Study 1 (1)	Study 2 (2)	Study 2 – Study 1 (3)	Study 3 (4)	Study 3 – Study 1 (5)	Study 3 – Study 2 (6)
Age	45.685	44.951	-0.733	45.610	-0.074	0.659
	(15.711)	(15.393)	[0.613]	(15.388)	[0.612]	[0.544]
Male	0.479	0.488	0.008	0.473	-0.006	-0.015
	(0.500)	(0.500)	[0.020]	(0.499)	[0.020]	[0.018]
Female	0.500	0.495	-0.005	0.509	0.009	0.015
	(0.500)	(0.500)	[0.020]	(0.500)	[0.020]	[0.018]
Non-binary	0.021	0.018	-0.003	0.017	-0.003	-0.000
	(0.142)	(0.131)	[0.005]	(0.131)	[0.005]	[0.005]
US citizen	0.983	0.979	-0.004*	0.991	0.007*	0.011***
	(0.129)	(0.142)	[0.005]	(0.096)	[0.004]	[0.004]
Partnered	0.456	0.508	0.052***	0.570	0.114***	0.063***
	(0.498)	(0.500)	[0.020]	(0.495)	[0.020]	[0.018]
Post-secondary education	0.852	0.824	-0.028*	0.842	-0.009	0.019
	(0.356)	(0.381)	[0.015]	(0.364)	[0.014]	[0.013]
White	0.719	0.740	0.021	0.767	0.048***	0.027*
	(0.450)	(0.439)	[0.017]	(0.423)	[0.017]	[0.015]
Household size	2.749	2.773	0.024	2.778	0.029	0.005
	(1.472)	(1.425)	[0.057]	(1.363)	[0.056]	[0.049]
Democrat	0.377	0.369	-0.008	0.384	0.007	0.015
	(0.485)	(0.483)	[0.019]	(0.487)	[0.019]	[0.017]
Independent	0.312	0.311	-0.000	0.301	-0.011	-0.011
	(0.463)	(0.463)	[0.018]	(0.459)	[0.018]	[0.016]
Republican	0.312	0.320	0.008	0.315	0.004	-0.005
	(0.463)	(0.466)	[0.018]	(0.465)	[0.018]	[0.016]
Log equivalized income	7.875	7.854	-0.020	7.942	0.067**	0.088***
	(0.793)	(0.813)	[0.032]	(0.777)	[0.031]	[0.028]

Notes: This table reports descriptive statistics of sociodemographic covariates across the three survey samples (Studies 1–3) and differences in means between them. Columns (1)–(2) present averages for Studies 1 and 2, with Column (3) reporting their difference. Columns (4)–(6) present analogous comparisons for Study 3. Standard deviations are reported in square brackets, while standard errors of the mean are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Balance of covariates across groups

	Control groups (1)	Treatment groups (2)	Difference in means (3)
Age	45.741	44.821	-0.919*
	(15.321)	(15.454)	[0.544]
Male	0.481	0.480	-0.000
	(0.500)	(0.500)	[0.018]
Female	0.504	0.500	-0.005
	(0.500)	(0.500)	[0.018]
Non-binary	0.015	0.020	0.005
	(0.121)	(0.140)	[0.005]
US citizen	0.981	0.989	0.007*
	(0.136)	(0.105)	[0.004]
Partnered	0.532	0.547	0.015
	(0.499)	(0.498)	[0.018]
Post-secondary education	0.833	0.833	0.000
	(0.373)	(0.373)	[0.013]
White	0.755	0.752	-0.003
	(0.430)	(0.432)	[0.015]
Household size	2.792	2.759	-0.033
	(1.423)	(1.365)	[0.049]
Democrat	0.380	0.374	-0.006
	(0.485)	(0.484)	[0.017]
Independent	0.302	0.310	0.007
	(0.459)	(0.463)	[0.016]
Republican	0.318	0.317	-0.001
	(0.466)	(0.465)	[0.016]
Log equivalized income	7.898	7.898	0.000
	(0.802)	(0.791)	[0.028]
Observations	1604	1601	

Notes: This table reports descriptive statistics of sociodemographic covariates across control and treatment groups, pooling respondents from Studies 2 and 3. Columns (1)–(2) present averages for the control and treatment groups, and Column (3) reports differences in means. Standard deviations are reported in square brackets, while standard errors of the mean are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Corrective treatments and change in life satisfaction – OLS results

	Within-subject change	Between-group comparison post-treatment
	(1)	(2)
Estimation Sample: Study 2		
Correction: average	-0.039	-0.087
	(0.050)	(0.133)
Correction: low-income	-0.046	-0.051
	(0.050)	(0.133)
Observations	1599	1599
R-squared	0.019	0.258
Estimation Sample: Study 3		
Correction: average	0.005	-0.199
	(0.046)	(0.130)
Correction: low-income	-0.028	-0.076
	(0.046)	(0.129)
Observations	1606	1606
R-squared	0.024	0.223
Estimation Sample: Study 3 excluding those who failed comprehension check		
Correction: average	-0.020	-0.218
	(0.047)	(0.133)
Correction: low-income	-0.042	-0.025
	(0.047)	(0.133)
Observations	1528	1528
R-squared	0.025	0.217

Notes: This table reports OLS estimates of the treatment effects of the corrective information on within-subject changes and between-group comparisons post-treatment of life satisfaction All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Reactions to corrective treatments, redistribution items and donation – OLS results

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Reaction to "Correction: average"				
Don't believe it	-0.045	0.168	-0.219	-0.064*
	(0.236)	(0.319)	(0.257)	(0.036)
Believe it	0.124	-0.042	-0.128	0.013
	(0.193)	(0.247)	(0.199)	(0.026)
Reaction to "Correction: low-income"				
Don't believe it	-1.043***	-0.567*	-0.981***	-0.101***
	(0.307)	(0.343)	(0.281)	(0.039)
Believe it	0.135	0.420*	0.158	0.056**
	(0.188)	(0.241)	(0.197)	(0.026)
Observations	1519	1502	1511	1528
R-squared	0.260	0.229	0.323	0.057
Reaction to "Correction: average"				
Not relevant	-0.080	0.104	-0.268	-0.040
	(0.194)	(0.248)	(0.206)	(0.029)
Relevant	0.300	-0.092	0.013	0.025
	(0.231)	(0.316)	(0.240)	(0.031)
Reaction to "Correction: low-income"				
Not relevant	-0.664***	-0.227	-0.462**	-0.024
	(0.219)	(0.252)	(0.211)	(0.029)
Relevant	0.591***	0.750**	0.316	0.069**
	(0.206)	(0.308)	(0.244)	(0.033)
Observations	1519	1502	1511	1528
R-squared	0.262	0.229	0.320	0.051

Notes: This table reports OLS estimates of the treatment effects of the corrective information on each redistribution item separately and donation. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B6 Beliefs updating of well-being norms – OLS results

	Redistribution attitude index (1)	Donation to charities (% of the $10 lottery-reward) (2)
Joint reaction to "Correction: average"		
Don't believe it & Not relevant	-0.048	-0.078**
	(0.090)	(0.039)
	[0.990]	[0.316]
Mix	-0.008	-0.005
	(0.069)	(0.034)
	[0.998]	[0.998]
Believe it & Relevant	0.024	0.033
	(0.082)	(0.033)
	[0.995]	[0.928]
Reaction to "Correction: low-income"		
Don't believe it & Not relevant	-0.393***	-0.113***
	(0.102)	(0.042)
	[0.004]	[0.060]
Mix	0.003	0.028
	(0.074)	(0.033)
	[0.998]	[0.943]
Believe it & Relevant	0.196***	0.083**
	(0.071)	(0.034)
	[0.006]	[0.124]
Observations	1528	1528
R-squared	0.037	0.059

Notes: This table reports OLS estimates of the treatment effects of the corrective information on a redistribution attitude index (first component of a factor analysis considering the three redistribution items used in the main analysis) and donation. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: Reactions to corrective treatments, redistribution items and donation – OLS results

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Correction: average	0.187	0.184	-0.831	0.063
	(0.795)	(0.757)	(0.728)	(0.096)
Correction average*Poor deserve help from govt.	-0.025	-0.031	0.122	-0.015
	(0.134)	(0.137)	(0.122)	(0.017)
Correction: low-income	-0.977	-0.388	-1.496**	0.006
	(0.795)	(0.675)	(0.607)	(0.099)
Correction: low-income* Poor deserve help from govt.	0.152	0.112	0.258**	0.001
	(0.137)	(0.121)	(0.105)	(0.018)
Observations	1519	1502	1511	1528
R-squared	0.352	0.349	0.490	0.073
Correction: average	-0.253	-0.441	-0.238	0.066
	(0.508)	(0.584)	(0.519)	(0.063)
Correction average*Poor because of bad luck	0.071	0.107	0.013	-0.019
	(0.099)	(0.125)	(0.100)	(0.014)
Correction: low-income	-1.224**	-0.077	-0.439	-0.018
	(0.527)	(0.567)	(0.527)	(0.064)
Correction: low-income* Poor because of bad luck	0.246**	0.053	0.068	0.007
	(0.104)	(0.118)	(0.104)	(0.014)
Observations	1519	1502	1511	1528
R-squared	0.299	0.278	0.374	0.055
Correction: average	-0.007	0.758	-0.099	0.036
	(0.433)	(0.589)	(0.430)	(0.071)
Correction average*Govt assistance only for those trying to improve their situation	0.018	-0.148	-0.006	-0.011
	(0.096)	(0.124)	(0.097)	(0.014)
Correction: low-income	-0.263	0.795	-0.089	0.085
	(0.453)	(0.592)	(0.431)	(0.069)
Correction: low-income* Govt assistance only for those trying to improve their situation	0.009	-0.152	-0.026	-0.017
	(0.103)	(0.125)	(0.099)	(0.014)
Observations	1519	1502	1511	1528
R-squared	0.259	0.253	0.342	0.047

Notes: This table reports OLS estimates of the treatment effects of the corrective information on each redistribution item separately and donation. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: Reactions to corrective treatments, redistribution items and donation – OLS results

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Correction: average	0.036	0.004	-0.206	-0.017
	(0.166)	(0.213)	(0.173)	(0.023)
Correction average*Baseline gap in average well-being norm	-0.012	-0.017	-0.019*	-0.000
	(0.010)	(0.013)	(0.010)	(0.001)
Correction: low-income	-0.250	0.149	-0.183	0.012
	(0.178)	(0.222)	(0.185)	(0.024)
Correction: low-income* Baseline gap in poor well-being norm	-0.015	0.008	-0.004	0.001
	(0.017)	(0.024)	(0.023)	(0.002)
Observations	1519	1502	1511	1528
R-squared	0.254	0.227	0.320	0.047
Correction: average	0.434	0.011	-0.546	-0.034
	(0.439)	(0.569)	(0.459)	(0.065)
Correction average*Confidence in responses	-0.159	0.008	0.160	0.007
	(0.178)	(0.220)	(0.186)	(0.025)
Correction: low-income	-0.731	-0.580	-0.613	0.030
	(0.484)	(0.570)	(0.484)	(0.065)
Correction: low-income* Confidence in responses	0.218	0.296	0.181	-0.009
	(0.194)	(0.225)	(0.203)	(0.025)
Observations	1519	1502	1511	1528
R-squared	0.254	0.227	0.317	0.046

Notes: This table reports OLS estimates of the treatment effects of the corrective information on each redistribution item separately and donation. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Causal mediation analysis – Natural indirect effects of gaps in well-being norms for average American

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Joint reaction to "Correction: average"				
Don't believe it & Not relevant	0.009	0.001	0.001	-0.000
	(0.017)	(0.006)	(0.005)	(0.001)
Mix	0.015	0.002	0.002	-0.000
	(0.016)	(0.011)	(0.015)	(0.001)
Believe it & Relevant	0.024	0.003	0.002	-0.000
	(0.020)	(0.016)	(0.016)	(0.002)
Reaction to "Correction: low-income"				
Don't believe it & Not relevant	0.035	0.004	0.002	-0.000
	(0.027)	(0.021)	(0.020)	(0.002)
Mix	0.051	0.006	0.003	-0.000
	(0.033)	(0.033)	(0.030)	(0.004)
Believe it & Relevant	0.073	0.008	0.004	-0.000
	(0.050)	(0.048)	(0.043)	(0.006)
Observations	1519	1502	1511	1528

Notes: This table reports the natural indirect effects of the absolute change in well-being norms for low-income families using the causal mediation framework of Imai et al., (2010) associated with each joint reaction to corrective information treatments Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B10: Causal mediation analysis – Natural indirect effects of gaps in well-being norms for average American

	Support for (on a 0-10 scale)			Donation to charities (% of the $10 lottery-reward)
	Introduction of a universal basic income	Increasing taxes on high earners	Doing more to reduce income inequality	
	(1)	(2)	(3)	(4)
Joint reaction to "Correction: average"				
Don't believe it & Not relevant	0.009	-0.001	-0.028	-0.003
	(0.018)	(0.019)	(0.022)	(0.003)
Mix	0.016	-0.003	-0.057	-0.005
	(0.032)	(0.037)	(0.037)	(0.004)
Believe it & Relevant	0.016	-0.002	-0.052	-0.004
	(0.033)	(0.036)	(0.035)	(0.003)
Reaction to "Correction: low-income"				
Don't believe it & Not relevant	0.007	-0.001	-0.020	-0.002
	(0.014)	(0.015)	(0.015)	(0.002)
Mix	0.009	-0.001	-0.027	-0.003
	(0.017)	(0.037)	(0.021)	(0.002)
Believe it & Relevant	0.012	-0.002	-0.036	-0.004
	(0.023)	(0.036)	(0.023)	(0.003)
Observations	1519	1502	1511	1528

Notes: This table reports the natural indirect effects of the absolute change in well-being norms for average American using the causal mediation framework of Imai et al., (2010) associated with each joint reaction to corrective information treatments Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: Beliefs updating of well-being norms – OLS results

	Raw change in well-being norm		Absolute change in well-being norms	
	Average American	Low-income families	Average American	Low-income families
	(1)	(2)	(3)	(4)
Joint reaction to "Correction: average"				
Don't believe it & Not relevant	252.542	322.410	-2454.325**	-504.806
	(1169.398)	(917.078)	(998.227)	(788.305)
Mix	2571.844	2020.774**	-4138.449***	-909.112
	(1057.021)	(884.016)	(942.808)	(782.127)
Believe it & Relevant	2398.696	1769.710**	-4438.334***	-1318.535**
	(1093.135)	(721.723)	(995.139)	(601.469)
Reaction to "Correction: low-income"				
Don't believe it & Not relevant	-2345.558**	-15.080	-1796.176**	-1775.063***
	(948.988)	(639.166)	(767.937)	(540.753)
Mix	-929.052	995.366	-2398.006***	-2779.413***
	(943.163)	(687.680)	(812.306)	(646.648)
Believe it & Relevant	-256.950	1587.055***	-3204.174***	-4007.139***
	(991.593)	(462.766)	(853.970)	(400.879)
Observations	1528	1528	1528	1528
R-squared	0.037	0.025	0.050	0.047
Average misperceptions in well-being norm beliefs pre-treatment per group:				
Joint reaction to "Correction: average"				
Don't believe it & Not relevant	-3432.924	-1861.231	9522.797	6643.049
Mix	679.893	212.428	10367.860	7112.070
Believe it & Relevant	-49.011	-595.017	9643.932	6187.873
Reaction to "Correction: low-income"				
Don't believe it & Not relevant	-4391.279	-3631.785	8881.011	5488.053
Mix	-2796.674	-2272.597	9100.37	5429.897
Believe it & Relevant	-1972.301	-1912.639	8381.719	5387.391

Notes: This table reports OLS estimates of the treatment effects of the corrective information on the update of well-being norms, using data from Study 3 and excluding respondents who failed the comprehension check. The dependent variable measures the change in the size of the gap between respondents' beliefs about the income needed for a good life and the benchmark norms from Study 1, comparing responses after and before the treatment. Coefficients are expressed in U.S. dollars. All regressions control for age, gender, citizenship, partnership status, log equivalized household income, a missing-income indicator, postsecondary education, race (white), political affiliation, household size, and self-assessed confidence in the responses to beliefs. Robust standard errors are reported in parentheses. Significance levels are reported as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.