Who Should Get Money? Estimating Welfare Weights in the U.S.*

Francesco Capozza†
Krishna Srinivasan‡

August 12, 2025

Abstract

Evaluating the desirability of a reform typically involves weighing the gains of winners against the losses of losers using welfare weights, which measure the value that society places on a $1 increase in an individual's consumption. We elicit the welfare weights of the U.S. general population using experiments and show their robustness, validity, and temporal stability. We estimate an income elasticity of welfare weights between -0.78 and -0.70, which is roughly 5-9 times more progressive than the weights implied by U.S. tax and transfer policies. We use the estimated welfare weights to derive optimal income taxes.

Keywords: Welfare Weights, Policy Views, Income Taxation
JEL Classification: D63, H21, I31

*This work was supported by Grant 2104-31756 from the Russell Sage Foundation, by the University Research Priority Program "URPP Equality of Opportunity" of the University of Zurich, and by WZB. Any opinions expressed are those of the principal investigators alone and should not be construed as representing the opinions of the Russell Sage Foundation. We thank Sandro Ambühl, Aurelien Baillon, Björn Bartling, Katy Bergstrom, Alexander Cappelen, William Dodds, Ernst Fehr, Manuel Grieder, Nathaniel Hendren, Luca Henkel, Michael Kurschilgen, Michel Maréchal, Morten Støstad, Nina Serdarevic, Florian Scheuer, Itai Sher, Stefanie Stantcheva, Dmitry Taubinsky, Bertil Tungodden, and Roberto Weber for their helpful comments. We also thank participants at various conferences and seminars for their helpful comments. This study was pre-registered: https://doi.org/10.1257/rct.16001-1.1

†UB, IEB, CESifo. Email: francescocapozza94@gmail.com
‡UniDistance Suisse. Email: krishna.srinivasan@unidistance.ch.

1 Introduction

Most policy reforms create winners and losers. Evaluating the desirability of such reforms commonly involves weighing the gains of the winners against the losses of the losers using welfare weights. Welfare weights measure the value that society places on providing an additional dollar of consumption to any given individual. But which welfare weights should society use to evaluate reforms? Previous studies generally assume welfare weights (e.g., Saez 2001) or indirectly infer them from existing policies (e.g., Hendren 2020, Lockwood & Weinzierl 2016). In contrast, our paper takes a direct empirical approach by eliciting the welfare weights of the U.S. general population using experiments.

We develop a portable method to elicit welfare weights and apply this method using online experiments with samples of the U.S. general population ($N \approx 2000$). In the experiments, participants in the role of "Social Architects" face pairs of participants in the role of "Recipients." A Social Architect makes several real-stakes decisions that redistribute specified monetary amounts between the pairs of Recipients, with the real-world disposable incomes of the Recipients differing across pairs. The Recipients' disposable incomes span the U.S. income distribution. Social Architects' decisions reveal the welfare weights they implicitly assign to the Recipients.

Welfare weights are a reduced-form representation of underlying normative ideals, such as equality of opportunity, utilitarianism, distribution based on the source of income, and poverty alleviation (Saez & Stantcheva 2016). For example, a utilitarian Social Architect would assign welfare weights proportional to the Recipients' marginal utility of consumption. A Social Architect guided by equality of opportunity might assign higher welfare weights to Recipients from disadvantaged backgrounds. Our approach allows for evaluating policies conditioned on income using welfare weights—also conditioned on incomes—without the need to specify and uncover the underlying ideals.

Pooling responses of all Social Architects, we find that the median welfare weights are progressive: the median income elasticity of welfare weights is $\nu = -0.63$. This estimate implies that if a Social Architect assigns a welfare weight of 1 to a Recipient, they would assign a weight of 0.37 (about one-third) to a Recipient earning twice as much. In monetary terms, the Social Architect is indifferent between taking \$1 from a Recipient and giving \$0.37 to a Recipient earning half as much. Since these welfare weights are elicited based on Recipients' disposable incomes resulting from the current tax and transfer system, progressive welfare weights indicate a desire for redistribution beyond that achieved by the current system.

While the aggregate welfare weights are progressive, there is considerable heterogene-

ity. This heterogeneity is predicted by the Social Architects' background characteristics. Republicans have less progressive welfare weights relative to Democrats and Independents, consistent with the partisan gap in support for government redistribution documented in the literature (e.g., Kuziemko et al. 2015, Stantcheva 2021). We also find that Social Architects with above-median age have less progressive welfare weights.

We conduct several tests to assess the robustness, validity, and temporal stability of the elicited welfare weights. We assess the robustness of welfare weights to individual-level response quality using several proxies and estimate quality-adjusted measures of the progressivity, following Luttmer & Samwick (2018). We find that high-quality responses are more progressive; consequently, the quality-adjusted progressivity estimate is slightly more progressive than our unadjusted baseline estimate.

We also assess the robustness of welfare weights to variation in the features of the experimental design. First, we vary three features of the decision environment: (i) the visual placement of the two Recipients on the screen, (ii) the order in which Recipient pairs are presented, and (iii) the income of the Recipient common across the pairs of Recipients. Only the third feature affects welfare weights: weights are more progressive when the common Recipient earns \$60,000 rather than \$120,000. Second, we vary features of the elicitation method—a "staircase method" that presents participants with decisions in an adaptive manner. (iv) Randomizing the first decision reveals anchoring: a more progressive first decision leads to more progressive weights. (v) Randomizing whether Social Architects are prompted to consider the consequences of their choices affects welfare weights. Prompted Social Architects have slightly more progressive welfare weights, suggesting that some regressive choices are mistakes. We adjust our progressivity estimates for anchoring and mistakes and derive bounds from the treatments that vary the common Recipient's income.

The progressivity estimates obtained from our various robustness checks form a relatively tight bound: the implied income-elasticity of welfare weights lies in the interval $[-0.78, -0.70]$. Optimal policy formulas can implement these welfare weights using the parametric function c^{ν}, where c represents consumption and ν is a parameter governing the progressivity of the weights, with $\nu \in [-0.78, -0.70]$ or using constant relative risk aversion (CRRA) utilities with a coefficient of relative risk aversion given by $\gamma = [0.70, 0.78]$.

To validate the welfare weights, we test whether they correlate with two survey items measuring (i) general support for government redistribution and (ii) support for government redistribution at the margin. Social Architects with more progressive welfare weights express stronger support for government redistribution on both measures. When comparing levels, we find that 78% of Social Architects assign progressive welfare weights—implying a preference to redistribute at the margin—closely matching the 74% who support additional

progressive government redistribution at the margin. The close alignment between welfare weights and support for government redistribution, both in individual-level correlations and in aggregate shares, supports the validity of our measure of welfare weights. It also suggests that calibrating optimal policy formulas using the elicited welfare weights would likely result in policies with broad public acceptance.

As a second validation check, we ask whether the elicited welfare weights capture fairness concerns, as intended. We measure fairness concerns as perceived fairness of the current income distribution. We find that welfare weights capture fairness concerns, validating our measure of welfare weights. However, welfare weights also capture beliefs about taxes hurting the economy and views about the scope of the government. Reassuringly, welfare weights do not capture misperceptions about the level of taxes paid by individuals and the share earning low incomes (below $15,000).

To gauge the temporal stability of welfare weights, we re-elicited Social Architects' welfare weights in a second survey wave conducted four weeks after the first. The elicited welfare weights are temporarily stable, both at the individual and aggregate levels. The across-wave correlation in welfare weights is comparable to the correlations documented in the broader preference-elicitation literature and to the correlations of two survey measures of support for government redistribution that we fielded in both waves. Our finding of a high degree of temporal stability at the aggregate level supports using the elicited aggregate progressivity measures to calibrate optimal policy formulas.

We compare the welfare weights obtained from our experiment to the weights implied by the U.S. income tax schedule (obtained from Hendren (2020)) and tax and transfer policies (obtained from Hendren & Sprung-Keyser (2020)). These weights—referred to as "inverse optimum weights"—represent politicians' aggregation of citizens' welfare weights, possibly influenced by political economy considerations. The welfare weights obtained from our experiment are about 8 to 9 times more progressive than the inverse optimum weights implied by the tax schedule and about 5 times more progressive than those implied by tax and transfer policies.

Finally, we explore the implications of the elicited welfare weights for optimal non-linear income taxes in the U.S. We calibrate the optimal non-linear labor income tax formula provided by Saez (2001) using the estimates of welfare weights from our experiment. We find that the optimal marginal tax rates obtained based on our experimental estimates of welfare weights are 28-30 percentage points higher, on average, than the current tax rates in the U.S.

Our paper is related to four strands of literature. The first is the experimental literature that aims to identify the ideals that guide people's support for redistribution (e.g., Drenik &

Perez-Truglia 2018, Almås et al. 2020, Saez & Stantcheva 2016). With the exception of Saez & Stantcheva (2016), these papers do not elicit welfare weights. Saez & Stantcheva (2016) elicit welfare weights as a function of disposable income and taxes and use them to calibrate optimal linear income taxes with no behavioral responses. Our paper estimates weights as a function of disposable income alone, making them portable across policy domains, and uses them to calibrate optimal non-linear income taxes that incorporate behavioral responses. We also document robustness, validity, and temporal stability of welfare weight, supporting their use in optimal policy formulas.[1]

A related strand of literature aims to identify distributional preferences (e.g., Fisman et al. 2007, 2023, Fehr et al. 2024), and preferences for government redistribution (Alesina & Angeletos 2005, Luttmer & Singhal 2011, Cruces et al. 2013, Durante et al. 2014, Karadja et al. 2017, Kuziemko et al. 2015, Stantcheva 2021).[2]

The second strand of literature identifies the inverse-optimum welfare weights implied by the tax schedule (e.g., Hendren 2020, Lockwood & Weinzierl 2016, Jacobs et al. 2017, Bourguignon & Spadaro 2012) or by tax and transfer policies (Hendren & Sprung-Keyser 2020). In contrast, our paper adopts an experimental approach to directly elicit the welfare weights of citizens. An important limitation of inverse-optimum welfare weights is that they may not be normatively appealing if they are influenced by political economy considerations, such as lobbying (Stantcheva 2016, Lockwood & Weinzierl 2016).

The third strand of literature aims to incorporate normative ideals in optimal policy formulas by modifying individuals' utilities or the objective function (e.g., Weinzierl 2014, Fleurbaey & Maniquet 2006). In contrast, our paper elicits the welfare weights of the general population, which can capture various underlying ideals, and uses these weights to calibrate "standard" optimal policy formulas.

The paper is organized as follows. Section 2 outlines the theoretical framework. Section 3 describes the experimental design. Section 4 reports the experimental results. Section 5 compares the elicited welfare weights to those implied by tax and transfer policies. Section 6 examines their implications for optimal income taxes. Section 7 presents a discussion.

[1]There are also two methodological differences. First, Saez & Stantcheva (2016) rely on a non-representative sample recruited from Amazon Mechanical Turks, while our paper recruits samples broadly representative on age, gender, income, education, and region. Second, their elicitation uses hypothetical decisions, whereas ours involves real-stakes decisions. Our paper shows the importance of incentivizing choices.

[2]Papers such as Fisman et al. (2007) and Fisman et al. (2023) elicit the weights assigned by participants on themselves relative to a stranger using choices that involve a tradeoff between equity and efficiency. However, these weights conflate the role of self-interest motives, which are normatively unappealing in the welfare economics tradition (e.g., Cappelen et al. 2013). Moreover, these welfare weights are not elicited as a function of income (since the income of the stranger is not specified); consequently, they cannot be directly used to evaluate policies conditioned on incomes.

2 Theoretical Framework

We present a simple theoretical framework, adapted from Saez & Stantcheva (2016), to conceptualize welfare weights and describe the approach to elicit them through experiments.

2.1 Setup

Consider a population of N *Recipients* indexed by j. A Recipient earns income z_j, pays taxes $T(z_j)$ as a function of income, and consumes $c_j = z_j - T(z_j)$. A Recipient j's utility function is quasi-linear in consumption and given by $u_j = z_j - T(z_j) - v(z_j, \alpha_j)$, where v represents Recipients' disutility of work as a function of their income and various personal characteristics α_j (e.g., disability status). We consider the simple case where earnings are completely inelastic to taxes and transfers to focus on redistributive issues (the focus of our paper) rather than behavioral responses (absent in the experiment).

2.2 Welfare Weights

The *Social Architect* assigns *generalized social marginal welfare weights* (henceforth *welfare weights*) to the Recipients. The welfare weight $g_j = g(c_j, \theta_j)$ measures the Social Architect's assessment of the value of increasing the consumption of Recipient j by \$1.[3] These welfare weights are a function of the Recipients' consumption c_j and characteristics contained in θ_j (e.g., disability status or parental income). Welfare weights are defined up to a multiplicative constant, as they measure the relative value of consumption of Recipient j.

Welfare weights are a reduced-form representation of underlying normative ideals rather than being deep structural primitives. They can capture various ideals such as equality of opportunity, utilitarianism, redistribution based on the source of income, and poverty alleviation. For example, a utilitarian Social Architect would assign welfare weights proportional to the Recipients' marginal utility of consumption (captured by c_j). A Social Architect guided by equality of opportunity would assign higher welfare weights to Recipients from disadvantaged backgrounds (captured by θ_j).

Although welfare weights may vary with characteristics θ_j, we focus on the average welfare weights at each consumption level c, since reforms are conditioned solely on incomes. A Social Architect's average welfare weight, $\bar{g}_j$, at income level $c = c_j$ equals

$$\bar{g}_j = \frac{\sum_{j:c_j=c} g_j}{h(c)} \tag{1}$$

[3]In particular, a Social Architect maximizes a social welfare function (not necessarily welfarist), and the welfare weight g_j measures the marginal increase in the Social Architect's objective function that arises from increasing Recipient j's consumption by \$1.

where $h(c)$ is the number of Recipients with income $c = c_j$. A distribution of mean welfare weights can be consistent with multiple underlying ideals. For example, under certain assumptions, welfare weights decreasing with Recipients' incomes can be consistent with utilitarianism and equality of opportunity.

2.3 Identifying Welfare Weights

Consider a setting with two Recipients with consumption levels c_l and c_h, such that $c_h > c_l$. Consider a "small" (not necessarily budget-neutral) reform $(r_l, -r_h)$ where r_l is the amount given to the low-income Recipient and r_h is the amount taken from the high-income Recipient. We assume that the reform does not affect earnings (which are assumed to be inelastic to taxes) or tax liabilities: consequently, a \$1 transfer translates to a \$1 increase in consumption.[4]

If a Social Architect assigns average welfare weights $\bar{g}_l$ and $\bar{g}_h$ to the low-income and high-income Recipients, respectively, she values the reform at $\bar{g}_l \cdot r_l - \bar{g}_h \cdot r_h$. To identify the ratio of welfare weights $\tilde{g} = \frac{\bar{g}_h}{\bar{g}_l}$, we identify the reform $(r_l, -r_h)$ that makes the Social Architect indifferent between implementing it and maintaining the status quo $(0, 0)$. Equating the Social Architect's valuation of this reform to her valuation of the status quo, we have $\bar{g}_l \cdot r_l - \bar{g}_h \cdot r_h = 0$. This implies,

$$\tilde{g} = \frac{\bar{g}_h}{\bar{g}_l} = \frac{r_l}{r_h}. \tag{2}$$

The ratio of the welfare weights is inversely proportional to the ratio of the reform amounts. Equation (2) allows us to identify $\bar{g}_h$ and $\bar{g}_l$ up to a multiplicative constant. If $\tilde{g} < 1$, the welfare weight on the high-income Recipient is lower than the welfare weight on the low-income Recipient ($\bar{g}_h < \bar{g}_l$), which corresponds to *progressive* welfare weights. Analogously, $\tilde{g} > 1 (\bar{g}_h > \bar{g}_l)$ corresponds to *regressive* welfare weights and $\tilde{g} = 1$ ($\bar{g}_h = \bar{g}_l$) implies equal weights.

Equation (2) shows that a Social Architect's welfare weights can be elicited using their choices between various reform amounts by finding their preferred reform that leaves them indifferent between the reform and the status quo. This approach allows us to elicit welfare weights without uncovering the underlying normative ideals guiding the Social Architect. The next section presents the experimental design used to elicit welfare weights.

[4] Assuming that reforms do not affect tax liabilities (and that Social Architects believe this) helps identify welfare weights unconfounded by the effect of the reforms on government revenue.

3 Experimental Design and Sample

This section describes the sample and experimental design used to elicit the welfare weights of the U.S. general population and to test their robustness, validity, and temporal stability. The experimental design, sample restriction, and analyses were pre-registered.[5] Minor deviations from the pre-registration are discussed in Appendix Section B. The complete set of instructions can be found in Appendix Section G.

3.1 Eliciting Welfare Weights

3.1.1 Approach

Participants in our experiment are assigned to one of two roles: *Social Architect* or *Recipient*. Social Architects decide whether to implement various "reforms" that redistribute specified monetary amounts between pairs of Recipients, with the real-world disposable incomes of the Recipients differing across pairs. Their decisions identify the welfare weights they implicitly assign to Recipients. Social Architects' decisions may have real consequences: one randomly selected decision made by one randomly selected Social Architect is implemented. Social Architects also learn that the Recipients (i) will be randomly drawn from the U.S. general population, (ii) are at least 18 years old, and (iii) are U.S. citizens. Recipients are passive subjects who receive payments based on the Social Architects' choices.

Our approach has four key features. First, following the small reform approach to taxation (Saez 2001, Saez & Stantcheva 2016), the reforms in our experiment constitute a small one-time transfer. Second, to ensure that welfare weights are unconfounded by factors such as trust in government, the reforms in the experiment make no references to the government or real-world policies. Third, the reforms are presented in a gain-loss frame—one Recipient loses while the other gains—to mirror trade-offs in policy evaluation (Saez & Stantcheva 2016, Hendren & Sprung-Keyser 2020). Finally, to capture Social Architects' assessments of the value of transfers given the current tax and transfer system, we present them with Recipients' disposable incomes accrued from the current system.[6]

3.1.2 Decisions

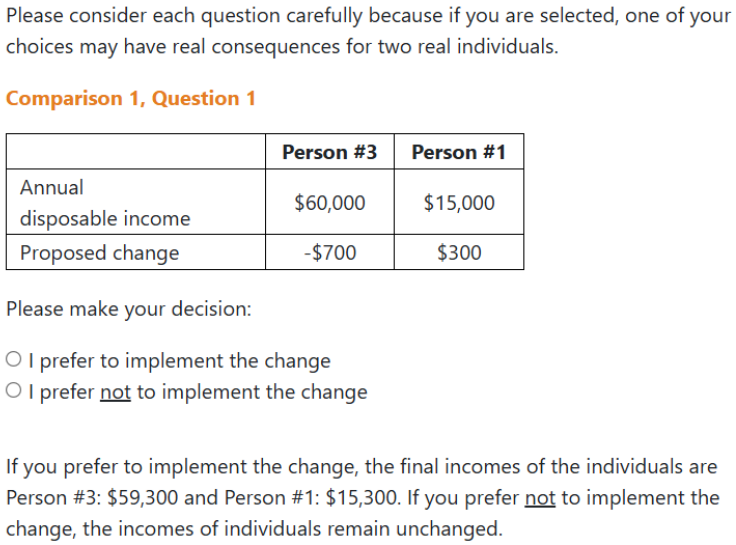
To elicit the welfare weights assigned by a Social Architect to a pair of Recipients, we present the Social Architect with several binary choices between implementing a reform and maintaining the status quo. Implementing a reform $(r_l, -r_h)$ would increase the income of the low-income Recipient by r_l and decrease the income of the high-income Recipient by r_h,

[5] See https://doi.org/10.1257/rct.16001-1.1.

[6] Eliciting welfare weights based on pre-tax incomes is challenging because it requires Social Architects to ignore the current disposable incomes of the Recipients, despite their incentivized decisions affecting these disposable incomes.

while maintaining the status quo leaves both incomes unchanged. To allow for negative transfers, we endow both Recipients with an initial endowment of $1,000, which is incorporated in their disposable incomes, and explicitly disclose this to the Social Architects. As discussed in the previous section, the reform $(r_l, -r_h)$ that makes a Social Architect indifferent between implementing it and maintaining the status quo, when plugged into Equation (2), reveals their welfare weights $\tilde{g} = \frac{\bar{g}_h}{\bar{g}_l} = \frac{r_l}{r_h}$.

Figure 1 presents a screenshot of one of the decisions presented to the Social Architects. We randomize the placement of the Recipients on the screen (left vs. right) across Social Architects. We refer to a reform as a "proposed change" instead of "reform" or "policy" to avoid any references to the government. At the bottom of the screen, we highlight how the reform affects the Recipients' incomes.


Please consider each question carefully because if you are selected, one of your choices may have real consequences for two real individuals.

Comparison 1, Question 1

	Person #3	Person #1
Annual disposable income	$60,000	$15,000
Proposed change	-$700	$300

Please make your decision:

○ I prefer to implement the change
○ I prefer not to implement the change

If you prefer to implement the change, the final incomes of the individuals are Person #3: $59,300 and Person #1: $15,300. If you prefer not to implement the change, the incomes of individuals remain unchanged.

Figure 1: Screenshot of a Decision Presented to Social Architects

Table 1 presents the full set of reforms $(r_l, -r_h)$ used to elicit welfare weights and the corresponding $\tilde{g}$. To cover a wide range of $\tilde{g}$ without requiring extremely large transfers, we simultaneously vary r_l and r_h across the reforms.[7] The reform amounts range from $50 to $950 in increments of $50, with the average amount ($500) comparable to several tax and transfer policies in the U.S. (see Appendix Table A2). The reform in Row 10 corresponds to $\tilde{g} = 1$; the reforms above Row 10 correspond to $\tilde{g} < 1$, while those below Row 10 correspond to $\tilde{g} > 1$. The reforms above Row 10 are symmetric to those below Row 10, ensuring that the set of progressive weights is the inverse of the set of regressive weights: $\tilde{g}$ in Row $10 - n$ is equal to $\frac{1}{\tilde{g}}$ in Row $10 + n$, $\forall n \in 1, \ldots, 9$.

[7]For instance, to obtain $\tilde{g} = 19$ (the largest $\tilde{g}$ in the table) with a fixed $r_h = \$500$ requires $r_l = \$9{,}500$.

Table 1: Set of Reforms Used to Elicit Welfare Weights

Row	Reform $(r_l, -r_h)$	$\tilde{g}$
1	(\$50, −\$950)	0.05
⋮	⋮	⋮
6	(\$300, −\$700)	0.43
⋮	⋮	⋮
8	(\$400, −\$600)	0.67
9	(\$450, −\$550)	0.82
10	(\$500, −\$500)	1
11	(\$550, −\$450)	1.22
12	(\$600, −\$400)	1.5
⋮	⋮	⋮
14	(\$700, −\$300)	2.33
⋮	⋮	⋮
18	(\$900, −\$100)	9
19	(\$950, −\$50)	19

Notes: The table presents the set of reforms used to elicit welfare weights. A reform $(r_l, -r_h)$ increases the income of the low-income Recipient by r_l and decreases the income of the high-income Recipient by r_h. $\tilde{g} = \frac{r_l}{r_h}$ is the ratio of the reform amounts.

We aim to identify a Social Architect's switch-point—the row where they switch from preferring the status quo (where no reform is implemented) to preferring the reform. The mid-point of the reforms in this row and the previous row represents the reform at which a Social Architect is just indifferent between implementing it and maintaining the status-quo.[8] To identify a Social Architect's switch-point, we use a "staircase method," which presents them with three to five decisions that are adaptively selected based on their previous choices. This method allows us to obtain precise weights with few decisions per participant.[9] We

[8]If a Social Architect always prefers a reform (i.e., switches in the first row), we take the mid-point of (\$50, −\$950) and (\$0, −\$1000), with the latter (corresponding to $\tilde{g} = 0$) giving the minimum possible amount to the low-income Recipient. If a Social Architect always prefers the status quo (i.e., never switches), we take the mid-point of (\$950, −\$50) and (\$1000, −\$0), with the latter reform (corresponding to $\tilde{g} = \infty$) taking the minimum possible amount from the high-income Recipient.

[9]Compared to the Multiple Price List (MPL) method, which presents all decisions simultaneously on a single page, the staircase method offers several advantages. First, the staircase method is simpler for the general population to understand; its simplicity is evidenced by Falk et al. (2018), who implemented the method to elicit risk and time preferences in nationally representative samples across the world. Second, the staircase method can pin down preferences with fewer decisions, reducing the burden on Social Architects. Third, it avoids the bias associated with the order of presentation of the decisions on the screen—a bias

randomize the first decision in the staircase to assess the sensitivity of the results to the starting point, following the approach of Luttmer & Samwick (2018) and Bursztyn et al. (2023). For half the Social Architects, the reform in the first decision is ($300, −$700), while for the other half, it is ($700, −$300). We discuss this and other randomizations in more detail in the next section. Appendix Figures A2 and A3 present a graphical representation of the staircase method with the two different starting points.

3.1.3 Recipients' Incomes and Comparisons

Social Architects are presented with six Recipients whose disposable incomes are chosen to span the U.S. disposable income distribution while satisfying three design constraints: (i) the highest income is capped at $500,000 to keep the recruitment of high-earners feasible, (ii) the ratios of neighboring incomes are constant to minimize cognitive burden, and (iii) incomes are rounded to the nearest $10,000 to reduce errors due to numeracy (e.g., List et al. 2023, Strulov-Shlain 2023). The resulting incomes range from $15,000 to $480,000, with each subsequent income double the previous one (see Appendix Figure A4 for a graphical representation).

A Social Architect is presented with five comparisons, with the incomes of the pairs of Recipients varying across the comparisons (see Table 2). To check the robustness of the welfare weights to the order in which comparisons are presented, we randomize the order—half the Social Architects see income differences in ascending order (as per the table), while the other half see them in descending order. A common Recipient appearing across all comparisons allows us to identify the relative welfare weights assigned to the six Recipients. To check the robustness to the common Recipient, we randomize the income of the common Recipient to be either $60,000 (as per the table) or $120,000.

Table 2: Recipients' Incomes Across Comparisons

	Comparison				
Recipient	1	2	3	4	5
Low-Income	$15,000	$30,000	$60,000	$60,000	$60,000
High-Income	$60,000	$60,000	$120,000	$240,000	$480,000

3.1.4 Incentives

To incentivize thoughtful decisions, Social Architects are informed that one participant in the study would be randomly selected at the end of the study, and one of their decisions would be randomly selected and implemented. The decision could be selected from either

observed in the MPL method (e.g., Jack et al. 2022). The limitations of the staircase method are discussed in Section 3.2.2.

of the two waves of data collection (discussed in the next section). Each decision saliently reminds Social Architects of the incentives (see Figure 1). Results from an additional study confirm the importance of incentivizing Social Architects' decisions.[10]

3.2 Assessing Robustness, Validity, and Temporal Stability of Welfare Weights

To assess the robustness, validity, and temporal stability of the elicited welfare weights, we conduct various tests.

3.2.1 Assessing Robustness to Quality of Responses

We assess the robustness of welfare weights to individual-level response quality using proxies of response quality. First, we flag Social Architects failing one or more comprehension questions on the first attempt as having low-quality responses. We included three comprehension questions that test whether Social Architects have understood (i) that they will make decisions regarding six Recipients, (ii) that they will be presented with disposable incomes (rather than pre-tax incomes), and (iii) that their decisions may have real stakes. Second, we elicit Social Architects' confidence in their decisions and classify those with low confidence as having low response quality. Third, we check decision consistency by including a sixth comparison identical to the third comparison (Recipients earning \$60,000 and \$120,000) and flag those whose welfare weights are inconsistent across comparisons as having low-quality responses. Finally, we flag completion times beyond two standard deviations from the mean within each treatment as an indicator of low response quality. Similar proxies have been used in the literature to assess response quality (e.g., Luttmer & Samwick 2018, Enke et al. 2023, Stantcheva 2023).

3.2.2 Assessing Robustness to Design Features

We assess whether the estimated welfare weights are robust to features of the experimental design by introducing several treatments that vary these features across Social Architects.

The first three treatment dimensions vary features of the decision environment, including (i) the placement of the Recipients on the screen (left vs. right), (ii) the order of presentation of the comparisons (ascending vs. descending order of income differences), and (iii) the Recipient common across the comparisons (\$60,000 vs. \$120,000). The set of high-to-low income ratios of Recipients is identical across treatments that vary the common Recipient, which allows us to isolate the role of the common Recipient.

The remaining two treatments vary features of the staircase method, addressing two potential limitations of the method. We randomize the reform in the first decision in the

[10] In a study conducted on Prolific ($N = 1965$), we find that Social Architects' welfare weights are more progressive with hypothetical decisions compared to real stakes decisions, consistent with an explanation of Social Architects considering the trade-offs more carefully in the latter. Appendix Section F presents the details of the study and analysis.

staircase to be either (\$300, −\$700) or (\$700, −\$300), following Luttmer & Samwick (2018) and Bursztyn et al. (2023). This allows us to test whether Social Architects anchor to the first decision. Finally, we vary whether Social Architects are prompted to consider the consequences of their choices to test whether their choices are based on mistakes. Prompted Social Architects are asked to choose between two final income distributions, which reflect the consequences of the two options they were presented with. They can only proceed if their preference on whether to implement the reform aligns with their preferred final income distribution.[11] To minimize the burden on Social Architects, they are only prompted in the first decision of each comparison because a mistake in the first decision leads to the largest distortion in the estimated welfare weights.[12] Prompted Social Architects are informed that they will be prompted in the first decision of each comparison. Appendix Figure A1 presents a screenshot of the prompt.

3.2.3 Correlation with Support for Redistribution

We validate our measure of welfare weights by testing whether they correlate with measures of support for government redistribution. We measure general support for government redistribution with a question used in the General Social Survey (GSS) and by Alesina et al. (2018) and support for government redistribution at the margin (i.e., beyond the current tax and transfer system). The order of the questions is randomized across Social Architects.

3.2.4 Do Welfare Weights Capture Fairness Concerns?

We test whether welfare weights capture fairness concerns (as intended) or other concerns using additional survey questions, drawn from Stantcheva (2021), capturing (i) fairness of the current income distribution, (ii) trickle-down economics, (iv) beliefs about labor distortions from taxes, (v) trust in government, and (vi) views about the scope of government. The order of the questions is randomized across Social Architects. Motivated by recent work documenting misperceptions (e.g., Hvidberg et al. 2023, Rees-Jones & Taubinsky 2020, Kuziemko et al. 2015), we also test whether welfare weights capture misperceptions about the level of taxes paid by individuals and the share of low-income individuals. Detailed descriptions of the measures are presented in Appendix Section A.

[11] This approach builds on Bursztyn et al. (2023), who ask participants for their agreement with a statement that reformulates their choices and provides them with the opportunity to revise their choices, and papers that nudge participants to reconsider their choices (e.g., Burchardi et al. 2021, Berry et al. 2020, Abdellaoui et al. 2019).

[12] As the staircase converges toward a Social Architect's preference, mistakes made later in the sequence lead to smaller distortions in the true welfare weights. When a mistake occurs, the true ratio of welfare weights $\tilde{g}$ is scaled by a factor of x and the observed ratio is $x\tilde{g}$. If a mistake occurs in the first decisions (with no subsequent mistakes), the variance of x is 17.98. The variance drops to 6.20, 1.53, 0.59, and 0.51 when the mistake occurs in the second, third, fourth, or fifth decision, respectively. Thus, mistakes in earlier decisions lead to larger deviations between observed and true welfare weights.

3.2.5 Temporal Stability of Welfare Weights

To gauge the temporal stability of welfare weights, we re-elicited Social Architects' welfare weights in a second survey wave conducted four weeks after the first. Participants with complete responses in Wave 1 were invited to Wave 2. The welfare weights elicitation protocol (including treatment assignments) was identical across waves. The short interval limits the scope for external shocks or changes in personal circumstances to alter preferences (Chuang & Schechter 2015). We benchmark the stability of welfare weights against the stability of our two measures of support for government redistribution, which were included in both waves.

3.3 Data Collection

We conducted the study across two waves. In Wave 1 of data collection, we recruited participants in the role of Social Architects from the data collection provider Prolific.[13] Recruitment was based on three demographic quotas available on Prolific: gender, political affiliation, and age. We implemented the experiment using oTree (Chen et al. 2016). The data collection for Wave 1 began on 12 May 2025 and lasted three days. Our final sample includes 1,996 participants.[14] The median completion time is 15.7 minutes.

We conducted the second wave of data collection roughly four weeks after the completion of the first wave. We invited 1986 participants to Wave 2, including those who completed the survey in the first wave and had a valid ID prior to data collection. The data collection for Wave 2 began on 9 June 2025 and lasted three days. With a follow-up rate of 70%, our final sample includes 1397 participants.[15] The median completion time is 9 minutes.

3.4 Summary Statistics

Table 3 presents the sample averages of Social Architects' background characteristics and the population averages. The Wave 1 sample broadly resembles the population but under-represents very high-income earners ($\geq$ \$150,000) and individuals with a high-school education or less, while over-representing very low-income earners ($<$ \$30,000) and those with a Bachelor's degree. We use sampling weights that help align sample means with population means; after weighting, the absolute differences in the sample and population means for all background characteristics are less than 0.1 percentage points. The Wave 1 and Wave 2 samples are also very similar.[16]

[13] Prolific has been used in several recent studies (e.g., Bursztyn et al. 2023, Enke et al. 2023).

[14] We recruited 2,194 participants in Wave 1. Following our pre-registered sample selection, we drop participants with multiple survey responses (0.18%), who failed the attention check (0.42%), and who did not complete the study.

[15] We invited 1,986 participants in Wave 2. Out of these, 1,447 participants took our survey (response of 73%). Following our pre-registered sample selection, we drop participants with multiple survey responses (0.35%), who failed the attention check (0.14%), and who did not complete the survey.

[16] Many characteristics are not balanced across treatment arms within each wave. Appendix Table A1 reports these balance tests. We control for background characteristics in the regressions exploring treatment

Table 3: Summary Statistics of Sample and Population

Variable	Wave 1	Wave 2	Population
Male	0.49	0.49	0.49
Income $<$ 30,000	0.26	0.28	0.13
Income 30–59,999	0.29	0.28	0.18
Income 60–99,999	0.26	0.26	0.23
Income 100–149,999	0.12	0.12	0.20
Income $\geq$ 150,000	0.07	0.07	0.26
Education: High school or less	0.11	0.11	0.37
Education: Some college	0.18	0.19	0.20
Education: Bachelor or Associate	0.45	0.47	0.30
Education: Masters or above	0.26	0.24	0.13
Age: 18–24	0.12	0.09	0.11
Age: 25–34	0.18	0.16	0.17
Age: 35–44	0.17	0.17	0.17
Age: 45–54	0.15	0.16	0.16
Age: 55–64	0.25	0.27	0.16
Age: $\geq$ 65	0.13	0.14	0.23
Region: Northeast	0.16	0.17	0.17
Region: Midwest	0.18	0.18	0.20
Region: South	0.44	0.43	0.39
Region: West	0.22	0.22	0.24
Republican	0.32	0.30	0.28

Notes: The table presents the average background characteristics of our sample and the U.S. population. Columns (2) and (3) report the sample demographics based on 1996 participants in Wave 1 and 1397 participants in Wave 2, respectively. Column (4) reports U.S. population demographics, computed using the 2023 American Community Survey (ACS) 1-year estimates for individuals aged 18 and older. The population share of Republicans is the average share of individuals identifying as Republican, based on the Gallup poll in 2024.

We also test representativeness using two survey items fielded in our experiment with wording identical to that used in large national polls. First, we find that 20% of the weighted Wave 1 sample report that the government can be trusted "most of the time" or "just about always," closely matching the share (22%) in the Pew Research Center's 2024 national poll. Second, 56% of the weighted Wave 1 sample supports reducing income differences between the rich and the poor, which is very similar to the corresponding share (53%) in the 2024 General Social Survey (GSS).[17] These close parallels to large-scale national surveys,

effects.

[17]Responses range on a scale from 1 to 7, with higher values indicating stronger preferences for reducing income differences between the rich and poor. We code values above 4 as supporting reductions in income differences. Unlike the GSS question, our question does not include a "Don't know" option. Appendix

particularly along preferences for redistribution, provide additional reassurance regarding the representativeness of our sample.

4 Welfare Weights of the General Population

4.1 Data Description

We begin by exploring the distribution of $\tilde{g}$—the ratio of welfare weights assigned to the high-income and low-income Recipients—across the six pairs of Recipients. We infer $\tilde{g}$ from each Social Architect's switching behavior using Equation (2). Figure 2a presents the distribution of $\tilde{g}$ using data from Wave 1 in treatments where the common Recipient across comparisons earns \$60,0000, while Figure 2b displays the analogous distribution when the common Recipient earns \$120,000.

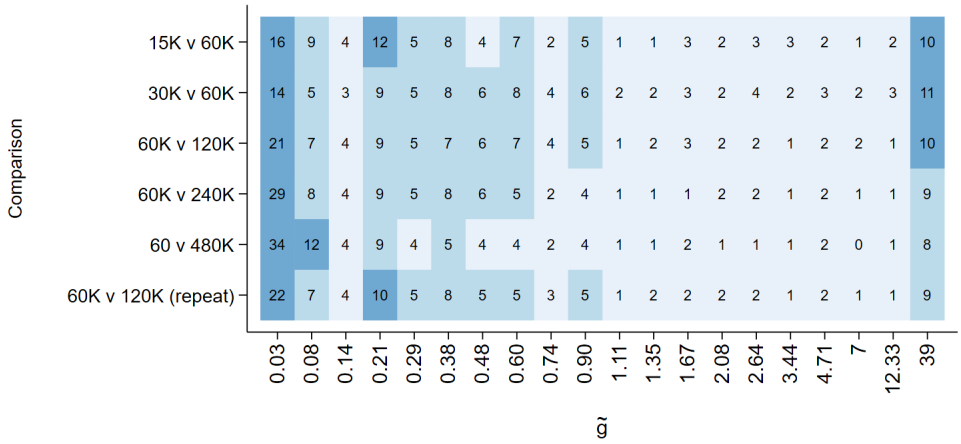
There are several patterns in the data. First, there is substantial heterogeneity in $\tilde{g}$: within each comparison, $\tilde{g}$ ranges from 0.03 (most progressive) to 39 (most regressive). Second, despite the heterogeneity, the distribution of $\tilde{g}$ skews toward low values of $\tilde{g}$ in every comparison—skews toward progressivity. Third, this progressive skew is stronger in comparisons featuring larger income differences between Recipients, implying that the aggregate welfare weights are progressive. Fourth, there is bunching at the extremes: 14–34% of Social Architects bunch at the progressive extreme ($\tilde{g} = 0.03$), while 8–15% bunch at the regressive extreme ($\tilde{g} = 39$); relatively few bunch at the "egalitarian" values $\tilde{g} = 0.09$ or $\tilde{g} = 1.11$. Finally, the distribution of $\tilde{g}$ is similar across identical comparisons: in the comparison that appears in both treatments (incomes \$60,000 and \$120,000), a Kolmogorov–Smirnov test fails to reject the equality of the two distributions ($D = 0.024$, $p = 0.929$). We also see that the distributions for the third and sixth comparisons (with Recipients earning \$60,000 and \$120,000) are indistinguishable in treatments where the common Recipient earns \$60,000 (Kolmogorov–Smirnov test; $D = 0.021$, $p = 0.979$) and \$120 000 ($D = 0.016$, $p = 1.000$).

4.2 Progressivity of Welfare Weights

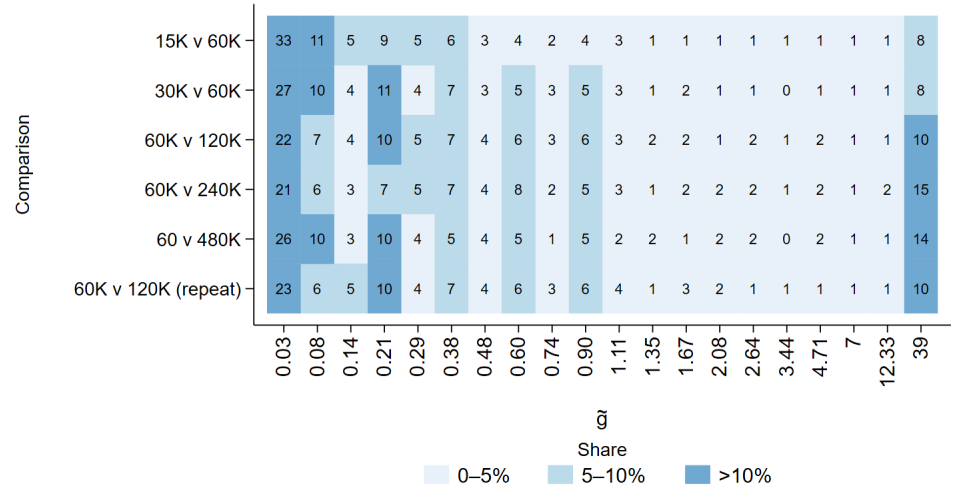
We find that 40% of Social Architects assign welfare weights that are weakly decreasing in Recipients' incomes (i.e., weakly progressive), while 8% assign weights that are weakly increasing (i.e., weakly regressive).[18] We estimate the progressivity of the Social Architects' welfare weights using a commonly employed parametric function (e.g., Saez 2002, Allcott et al. 2019): welfare weights g_r assigned by a Social Architect to Recipients (indexed by r) with disposable incomes c_r is proportional to c_r^{ν}, where ν is a parameter governing the

Figure A6 presents the distribution of responses of our sample.

[18]Non-monotonic weights are not necessarily due to measurement error. They could also be due to various normative ideals. For example, some Social Architects may be broadly progressive but assign a lower weight to very low-income individuals, whom they perceive as "lazy" (Drenik & Perez-Truglia 2018).



(a) Common Recipient earns \$60K



(b) Common Recipient earns \$120K

Figure 2: Distribution of $\tilde{g}$ across Comparisons and Treatments

Notes: The figures present the distribution of $\tilde{g}$—the ratio of welfare weights assigned to the high-income and low-income Recipients—across the six comparisons with the six pairs of Recipients. Each cell presents the share of participants with a given $\tilde{g}$ in a given comparison. The shares sum to 100% in each row. The figures in both panels use data from Wave 1; Panel (a) uses data from treatments in which the common Recipient across pairs earns \$60,000, while Panel (b) uses data from treatments in which the common Recipient earns \$120,000.

progressivity of the welfare weights. A negative value ($\nu < 0$) indicates progressive weights, while a positive value ($\nu > 0$) implies regressive weights. With a utility function that is additively separable in consumption and leisure and exhibits constant relative risk aversion in consumption, $-\nu$ corresponds to the coefficient of relative risk aversion.

To estimate ν for each Social Architect, we model the conditional expectation of the assigned welfare weights as

$$E[g_r \mid c_r] = exp(\alpha + \nu log(c_r)), \tag{3}$$

where the parameter α is a constant. The progressivity parameter ν can be interpreted as the income elasticity of welfare weights. We estimate ν using a Poisson Pseudo Maximum Likelihood (PPML) estimation.[19] We aggregate the welfare weights across Social Architects by computing the median estimate of ν—the median preference corresponds to the aggregate preference under the median voter theorem (Downs 1957).

Pooling the data across all treatments in Wave 1 and weighting the sample using sampling weights to match the sample with the population, we find a median progressivity estimate of $\nu = -0.63$ (see Figure 3).[20] In practical terms, this elasticity implies that if a Social Architect assigns a welfare weight of 1 to a Recipient, they would assign a weight of 0.37 (about one-third) to a Recipient earning twice as much. In monetary terms, this suggests that the Social Architect is indifferent between taking \$1 from the higher-income Recipient (decreasing welfare by $0.37 \cdot \$1$) and giving \$0.37 to the lower-income Recipient (increasing welfare by $1 \cdot \$0.37$). Since these welfare weights were elicited given the current tax and transfer system, our finding of progressive welfare weights implies that the Social Architects desire additional redistribution at the margin.

While the aggregate welfare weights are progressive, there is considerable heterogeneity across Social Architects. Specifically, 78% of Social Architects assign progressive weights ($\nu < 0$), while 22% assign regressive weights ($\nu > 0$). Applying sampling weights, these proportions remain similar: 75% progressive and 25% regressive. Additionally, there is significant variation in welfare weights within both of these groups (see Appendix Figure A5 for the distribution of ν). The minimum observed value of ν in our data is -4.18, and the maximum is 4.

4.3 Welfare Weights and Background Characteristics

We explore whether the observed heterogeneity in welfare weights is predicted by Social Architects' background characteristics. Table 4 presents median regression estimates of progressivity (ν) on background characteristics.

Our results indicate that Republicans assign less progressive welfare weights relative to

[19] We employ PPML estimation rather than OLS estimation (by log-linearizing Equation (3)), as the latter can be severely biased in the presence of heteroskedasticity (Silva & Tenreyro 2006).

[20] The unweighted median is slightly more progressive: $\nu = -0.71$, bootstrap SE $= 0.02$, 95% normal CI $= [-0.75, -0.66]$.

Democrats and Independents: the median ν among Republicans is 0.12 points higher than among non-Republicans. This result is consistent with the partisan gap in support for government redistribution observed in the literature (e.g., Alesina et al. 2018, Stantcheva 2021, Kuziemko et al. 2015). While Republicans assign less progressive weights, their weights are progressive, indicating that Republicans also desire additional redistribution at the margin.

Table 4: Welfare Weights and Background Characteristics

	(1)
Republican	0.118**
	(0.051)
High Income	0.076
	(0.049)
Male	0.023
	(0.047)
High Age	0.138***
	(0.048)
High Education	-0.093*
	(0.055)
Constant	-0.791***
	(0.044)
Observations	1998

Notes: The table presents coefficient estimates from a median regression. The dependent variable is the progressivity of the welfare weights (ν). *Republican* equals 1 for Republicans and 0 otherwise. *High Income* equals 1 for Social Architects with above median income and 0 otherwise. *High Age* equals 1 for Social Architects with above median age and 0 otherwise. *High Education* equals 1 for Social Architects with above median education and 0 otherwise. *Male* equals 1 if a Social Architect is male and 0 otherwise. The regression uses data from Wave 1. Standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Social Architects with above median income (above $54,000) assign less progressive weights, consistent with the income gap in support for redistribution observed in the literature (e.g., Singhal 2008, Cohn et al. 2023). However, the observed effect of income is not statistically significant. Although income is a weak predictor of progressivity, it predicts the weight assigned to the Recipient with similar incomes. Social Architects generally assign a higher weight to Recipients with incomes similar to their own relative to other Recipients, with the effects being largest for lower-income Social Architects (see Appendix Table A3).[21]

[21]The pattern is consistent with an explanation of self-interest motives. However, we cannot rule out alternative explanations, such as Social Architects having different preferences regarding Recipients with similar incomes.

Above-median aged (55 years or older) Social Architects assign less progressive welfare weights. We do not find a significant difference across education groups or across genders.

4.4 Assessing Robustness to Quality of Responses

We assess the robustness of the estimated welfare weights to individual-level response quality using several indicators of response quality. We flag potentially low-quality responses when participants (i) fail one or more comprehension questions on the first attempt, (ii) exhibit extreme completion times, (iii) provide internally inconsistent welfare weights (type clarification—i.e., progressive or regressive—differs across the identical third and sixth comparisons), or (iv) report low confidence in their decisions. About half the Social Architects are flagged by one or more indicators, with most of these Social Architects being flagged by only one indicator.[22] Reassuringly, Social Architects' welfare weights exhibit a high degree of internal consistency: about 90% of Social Architects have the same type classification (progressive or regressive weights) across identical comparisons. Furthermore, the distributions of $\tilde{g}$ are similar across the identical comparison (see Section 4.1).

Table 5 presents median regression estimates of progressivity (ν) on indicators of response quality. The explanatory variables also include treatment indicators and demographic controls. The median value of ν is 0.14 points higher among those who fail at least one comprehension question on their first try and 0.50 points higher among those with inconsistent welfare weights; Social Architects with low-quality responses tend to assign less progressive weights. We do not find a statistically significant effect of the other indicators.

We adjust the progressivity estimates for response quality following Luttmer & Samwick (2018). Each coefficient in Table 5 represents the median bias introduced by having a low-quality response. For each Social Architect whose answer is flagged as low-quality by a given indicator (indicator takes a value of 1), we subtract that indicator's estimated bias (the coefficient estimate) from the estimated ν. This correction yields unbiased estimates under two conditions: (1) differences in true underlying progressivity between those with high- and low-quality responses are fully captured by demographic controls (rather than by the quality indicators), and (2) the median ν among those with high-quality responses is measured without error.[23] Our adjustment uses the two proxies that have a statistically significant effect on progressivity. Applying this adjustment yields a weighted median progressivity estimate of $\nu = -0.71$, which is 0.08 points lower than our unadjusted baseline estimate (see

[22] About 37% of Social Architects are flagged by only one response-quality indicator, 11% by two indicators, 2% by three indicators, and 0.2% by all four indicators. Overlap across pairs of indicators is also limited (see Appendix Figure A8).

[23] Theoretical work shows that additive errors with a median of zero leave the population median unbiased (Schennach 2022, Hausman et al. 2021). Empirically, several studies report that the median error of estimates of interest is close to zero (Bollinger 1998, Bound & Krueger 1991).

Table 5: Welfare Weights and Response Quality

	(1)
Fail comprehension	0.145**
	(0.066)
Inconsistent	0.501***
	(0.067)
Extreme time	-0.057
	(0.096)
Low confidence	0.074*
	(0.043)
Observations	1996
Controls?	Yes

Notes: The table presents coefficient estimates from a median regression. The dependent variable is the progressivity of the welfare weights (ν). *Fail comprehension* equals 1 if a Social Architect failed one or more comprehension questions in the first try and 0 otherwise. *Inconsistent* equals 1 if, in the identical third and sixth comparisons, a Social Architect assigns progressive welfare weights ($\tilde{g} < 1$) in one and regressive welfare weights ($\tilde{g} > 1$) in the other. *Extreme time* equals 1 if a Social Architect's time spent on the survey lies beyond two standard deviations of the mean. *Low confidence* equals 1 if a Social Architect reports confidence levels lower than the highest category of "Very Much." Controls include treatment indicators (those specified in Table 6) and demographic controls (those specified in Table 4). The regression uses data from Wave 1. Standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Figure 3). The response-quality adjusted estimates are more progressive since low-quality responses exhibit less progressive welfare weights.

4.5 Assessing Robustness to Design Features

To assess the robustness of the welfare weights to the features of the experimental design, we implemented several treatments that vary the features across Social Architects. Table 6 presents median regression estimates of progressivity (ν) on treatment indicators. Column (1) includes only treatment indicators as explanatory variables, while Column (2) additionally includes background characteristics as controls.

The first three treatment dimensions vary the features of the decision environment. The placement of the Recipients on the screen (left vs. right) does not have a significant effect on the progressivity of weights. The order of the comparisons (ascending vs. descending order of income differences) has a small but statistically insignificant effect on the progressivity of the weights. In contrast, the common Recipient across comparisons matters: welfare weights are more progressive (ν is 0.13 to 0.15 points lower) when the common Recipient earns $60,000

rather than \$120,000, even though the set of high-to-low income ratios is identical across the two treatments. A plausible mechanism is the common Recipient's relative position: the \$60,000 common Recipient is the lower income Recipient in three comparisons, while the \$120,000 common Recipient is the lower income Recipient in only two comparisons.[24]

The remaining two treatment dimensions vary features of the staircase method, addressing two potential limitations of the method. We find that welfare weights are more progressive (ν is 0.18 points lower) when the first decision in the staircase is more progressive (\$300, −\$700) than when it is less progressive (\$700, −\$300), consistent with Social Architects anchoring to the first decision. We also find that welfare weights are more progressive (ν is 0.07 to 0.09 points lower) when Social Architects are prompted to consider the consequences of their choices compared to when they are unprompted, indicating that some regressive choices in the unprompted treatments are likely mistakes.[25] This effect is only marginally significant without controls but becomes significant at the 5% level once demographic controls are included.

We find very similar results when considering univariate regression in which the treatment indicators enter the regressions separately (see Appendix Table A4).

Reconciling Estimates Across Frames

Guided by the framework of Bernheim & Rangel (2009) and subsequent empirical studies, we apply several strategies to reconcile progressivity estimates across "frames" (treatments). The resulting alternative progressivity estimates are presented in Figure 3.

When frames differ in whether Social Architects are prompted to consider the consequences of their choice, we treat the unprompted frame as one in which some choices reflect mistakes and the prompted frame—because the prompt plausibly reduces such mistakes—as the "welfare-relevant" frame. This follows the common practice of recovering choice-mappings from frames in which mistakes are corrected (e.g., Allcott & Taubinsky 2015, Chetty et al. 2009, Ambuehl et al. 2022). The weighted median value of ν in the prompted frame is -0.75, about 0.12 points lower than our baseline estimate ($\nu = -0.63$).

For frames that vary the first decision of the staircase, we interpret anchoring to the first decision as a bias and correct the progressivity estimates for this bias (see Allcott et al. (2022)

[24]If people are more sensitive to disadvantageous than to advantageous inequality (Fehr & Schmidt 1999), the greater share of comparisons in which the common Recipient is worse off in treatments with the \$60,000 common Recipient relative to those with the \$120,000 common Recipient can tilt choices toward more progressive weights in the former.

[25]An alternative explanation is that the prompt induces experimental demand. To mitigate demand effects, we explicitly informed Social Architects before they began that they would be prompted in the first decision of each comparison. Moreover, we did not explicitly ask them to change their choice when there was an inconsistency (which might induce demand effects). Instead, we asked them to make their choice and indicate their preferred final distribution once again.

Table 6: Treatment Effects

	(1)	(2)
Low-income Recipient on Left	-0.044	-0.026
	(0.045)	(0.047)
Common recipient 60K	-0.131***	-0.150***
	(0.045)	(0.047)
Descending order of income diff	-0.076*	-0.045
	(0.045)	(0.047)
First decision (300, -700)	-0.188***	-0.182***
	(0.045)	(0.047)
Prompted	-0.075*	-0.093**
	(0.045)	(0.047)
Constant	-0.448***	-0.545***
	(0.055)	(0.071)
Observations	1996	1996
Controls?	No	Yes

Notes: The table presents coefficient estimates from a median regression. The dependent variable is the progressivity of the welfare weights (ν). The explanatory variables include treatment indicators and demographic controls (those specified in Table 4). The regressions use data from Wave 1. Standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

and Chetty et al. (2009) for a similar approach of inferring bias parameters). Following the adjustment strategy of Luttmer & Samwick (2018) and Bursztyn et al. (2023), we assume that the true values of the reform amounts that make Social Architects indifferent between the reform and the status quo are a weighted average of reported reform amounts and the reforms amounts in the first decision (details in Appendix Section E.1). We find that the extent of anchoring in each comparison is small; consequently, the median progressivity estimate after correcting for anchoring is $\nu = -0.66$, which is very similar to the baseline estimate.

Finally, when the frames vary the income of the common Recipient across comparisons, there is no obvious "welfare-relevant" frame. This is supported by results showing that indicators of response quality and temporal stability are not predicted by the frame (see Appendix Table A6). We embrace the normative ambiguity about the welfare-relevant frame and obtain bounds on the progressivity estimates (see Goldin & Reck (2022) for a similar approach). We estimate the weighted median value of ν in each frame that varies the common Recipient, and additionally, focus on the subset of prompted treatments to correct for mistakes. The weighted median value of ν when the common Recipient earns \$60,000 is

-0.78 and when the common Recipient earns \$120,000 is -0.70.[26]

Overall, the bounds on progressivity estimates, determined by the frames that vary the common Recipient and frames that prompt Social Architects to consider the consequences of their mistakes, are given by $\nu \in [-0.78, -0.70]$.

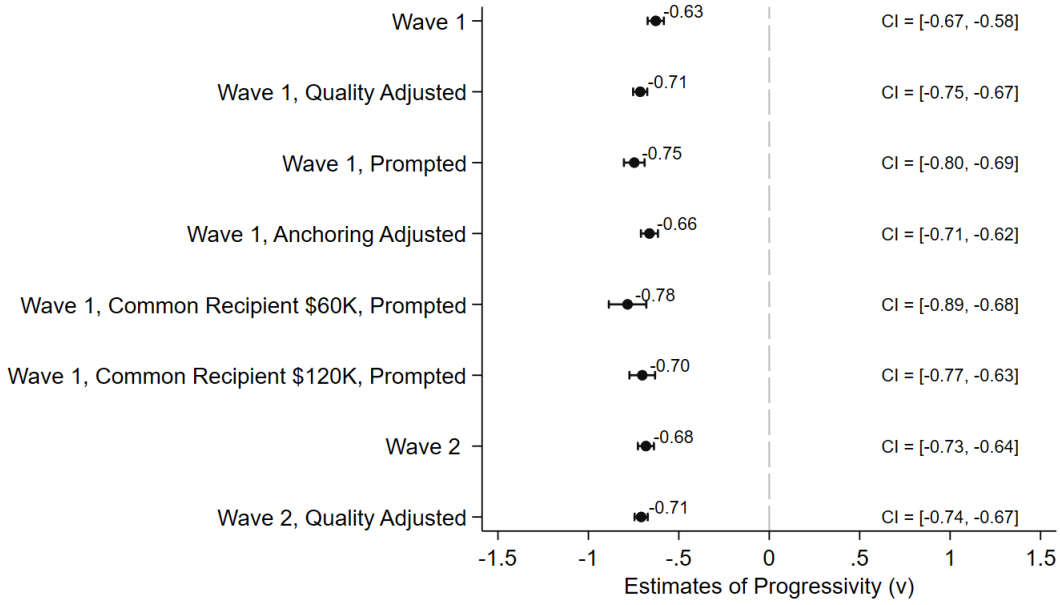


Figure 3: Estimates of Progressivity (ν)

Notes: The figure presents progressivity estimates ν from various specifications. All estimates are weighted using sampling weights to match the averages of the specified sample to the population averages. Confidence intervals are 95% normal confidence based on bootstrap standard errors.

4.6 Welfare Weights and Support for Redistribution

We validate our measure of welfare weights by testing whether it correlates with two measures of support for government redistribution. Social Architects with more progressive welfare weights should, all else equal, exhibit stronger support for government redistribution. We test these correlations using linear regressions presented in Table 7. In Column (1), the outcome variable is the Social Architects' support for reducing income differences between the rich and the poor (ranging from 1 to 7), with higher values indicating stronger support for redistribution. In Column (3), the outcome variable is support for redistribution beyond that achieved by the current tax and transfer system (ranging from -2 to 2), with positive (negative) values indicating redistribution from high-income (low/middle-income)

[26]Pooling over the prompted and unprompted treatments, the weighted median value of ν when the common Recipient earns \$60,000 is $\nu = -0.73$ (bootstrap SE $= 0.04$, 95% normal CI $= [-0.81, -0.65]$), and when the common Recipient earns \$120,000 is $\nu = -0.55$ (bootstrap SE $= 0.03$, 95% normal CI $= [-0.60, -0.49]$).

individuals to low/middle-income (high-income) individuals. The explanatory variable in both columns is the progressivity of the weights (ν). On average, Social Architects support increasing government redistribution on both measures (See Appendix Figures A6 and A7).

Social Architects with more progressive welfare weights (lower values of ν) exhibit stronger support for redistribution on both measures, and the associated coefficients are highly statistically significant. Based on the R^2, welfare weights account for 11–16% of the variation in support across the two measures.[27] We find similarly strong relationships when we use binary versions of the outcomes (split at each outcome's midpoint) and ν (split at 0), presented in Columns (2) and (4).

Table 7: Correlation with Support for Redistribution

	General redistribution		Redistribute at margin		
	(1)	(2)	(3)	(4)	(5)
ν	-0.884***		-0.370***		-0.349***
	(0.051)		(0.022)		(0.023)
$1(\nu > 0)$		0.369***		0.443***	
		(0.026)		(0.025)	
Fair distribution of income					-0.316***
					(0.060)
Constant	4.550***	0.371***	0.967***	0.399***	1.031***
	(0.052)	(0.023)	(0.023)	(0.023)	(0.026)
Observations	1996	1996	1996	1996	1996
R^2	0.158	0.104	0.112	0.177	0.126
Outcome var	Continous	Binary	Continous	Binary	Continous
Explanatory var	Continous	Binary	Continous	Binary	Binary

Notes: The table presents coefficient estimates from linear regressions. The dependent variable in Columns (1) and (2) takes values from 1 through 7, with higher values indicating stronger support for the government to do something to reduce income differences between the rich and poor. The dependent variable in Column (3), (4), and (5) takes values from -2 to $+2$, where positive (negative) values indicate redistribution from high-income (low/middle-income) individuals to low/middle-income (high-income) individuals. A value of zero indicates a desire for no additional redistribution. ν is the progressivity of Social Architects' welfare weights and $1(\nu > 0)$ is an indicator variable that equals 1 if ν is greater than 0 (progressive weights) and 0 otherwise. *Fair distribution of income* equals 1 if a Social Architect indicated that the current income distribution is "somewhat fair" or "very fair" and 0 otherwise. The regression uses data from Wave 1. HC3 standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

[27]We benchmark the predictive power of welfare weights against their stated political affiliation—an important predictor of support for redistribution (e.g., Stantcheva 2021, Kuziemko et al. 2015). We find that political affiliation (an indicator variable that equals 1 for Republicans and 0 otherwise) explains 3-8% of the variation in support for redistribution across the two measures (see Appendix Table A5). Thus, welfare weights have a stronger predictive power than political affiliation.

Having examined individual-level correlations, we now compare aggregate shares. About 78% of Social Architects assign progressive welfare weights—implying a preference to redistribute at the margin—closely matching the 74% of Social Architects supporting additional progressive government redistribution at the margin.[28]

Overall, the close alignment between welfare weights and support for government redistribution, both in individual-level correlations and in aggregate shares, helps validate our measure of welfare weights. It also suggests that calibrating optimal policy formulas using the elicited welfare weights would likely result in policies with broad public acceptance.

4.7 Do Welfare Weights Capture Fairness Concerns?

We also validate our measure of welfare weights by testing whether it captures fairness concerns, as intended. Building on the previous section—which showed that variation in welfare weights predicts variation in support for government redistribution—we explore whether the policy-relevant variation in welfare weights (the part that predicts policy preferences) is explained by fairness concerns.[29] We elicit Social Architects' fairness concerns by asking them to rate the fairness of the current disposable incomes on a five-point scale from "Very unfair" to "Very fair." This measure likely captures several fairness ideals, including redistribution based on the source of income, but it does not capture some important ones, such as equality of opportunity.

Table 7 presents the results. In Columns (3) and (5), the dependent variable is support for increasing government redistribution at the margin. In Column (3), the coefficient on ν is -0.37: Social Architects with more progressive weights (lower values of ν) express stronger support for increasing redistribution at the margin. In Column (5), we add an indicator for fairness perceptions that equals 1 if a Social Architect views the current income distribution as fair and 0 otherwise. Those who perceive the current distribution as fair have lower support for additional redistribution. Importantly, the coefficient on ν declines from -0.37 in Column (3) to -0.35 in Column (5) once fairness concerns are included, suggesting that fairness concerns partially mediate the effect of ν.

However, fairness concerns do not explain all of the policy-relevant variation in welfare

[28] Support for redistribution at the margin (rather than general support for redistribution) is the right measure for comparison to welfare weights because the former, similar to the welfare weights measure, is measured at the margin and allows for regressive redistribution. We find slightly lower support for our general redistribution measure: 66% indicate levels of redistribution above the mid-point of 4 on the 7-point scale. However, this share rises to 78% if we include Social Architects with a response of 4 as supporting redistribution.

[29] A simple correlation of welfare weights and fairness concerns would uncover whether the total variation in welfare weights (not just the policy-relevant variation) captures fairness concerns. We find that the correlation between our measure of fairness concerns and support for government redistribution at the margin is -0.34 ($p < 0.01$).

weights: the coefficient on ν remains statistically significant in Column (5). This residual likely reflects both the difficulty in obtaining a broad measure of fairness and the role of other concerns. Indeed, we find that beliefs about taxes hurting the economy and views about the scope of the government are also captured by welfare weights (see Appendix Section E.2).

We also measured Social Architects' perceptions about the level of taxes paid by individuals and the share earning low incomes (below $15,000). While Social Architects do have misperceptions along these margins, the policy-relevant variation in welfare weights does not capture these misperceptions.

4.8 Temporal Stability

To examine the temporal stability of welfare weights, we conducted our study across two waves, fielded four weeks apart. Figure 4 presents a scatter plot of ν across the two waves, using the sample of Social Architects with valid responses in both waves. The points cluster around the 45-degree line, highlighting the temporal consistency in the weights. The Pearson correlation coefficient of ν across the waves is 0.55 ($p < 0.01$), which is similar to the estimates found in the literature.[30] We benchmark the temporal stability of welfare weights against the stability of two measures of support for redistribution collected in both waves. The cross-wave correlation for welfare weights lies between the across-wave correlation for general support for redistribution ($\rho = 0.72, p < 0.01$) and support for redistribution at the margin ($\rho = 0.44, p < 0.01$). We also find a high degree of temporal consistency in types: 81% of Social Architects retain the same type classification (progressive or regressive weights) across waves. Among the remaining 19% who have inconsistent type classifications, half transition from regressive weights in Wave 1 to progressive weights in Wave 2, and the other half transition in the other way.

Next, we explore the aggregate temporal stability of welfare weights, an important input to policy evaluation.[31] The weighted median progressivity in Wave 2 is -0.68, which is slightly more progressive than the Wave 1 baseline estimate of -0.63 (see Figure 3). Because both estimates are weighted, differences in observable sample composition across waves are unlikely to explain the gap. Restricting the sample to Social Architects who provided valid responses in both waves yields very similar aggregate progressivity estimates, providing further evidence that the observable sample composition across waves does not drive the dif-

[30] Most evidence on the stability of social preferences comes from studies that track individuals over relatively long horizons. These studies find modest temporal correlations—0.28 in rural Paraguay (Chuang & Schechter 2015), 0.12–0.28 in Vietnam (Carlsson et al. 2014), and 0.39–0.46 in the United States (Fisman et al. 2023). By contrast, work that looks at much shorter intervals has focused on risk and time preferences, which appear considerably more stable, with correlations of 0.55 (Schoemaker & Hershey 1992) and 0.50 (Wölbert & Riedl 2013) observed for risk preferences and 0.64 for time (Dean & Sautmann 2021)

[31] To the extent that aggregate measures are less affected by individual-level measurement, stable aggregates can inform policy even when individual preferences are measured with noise.

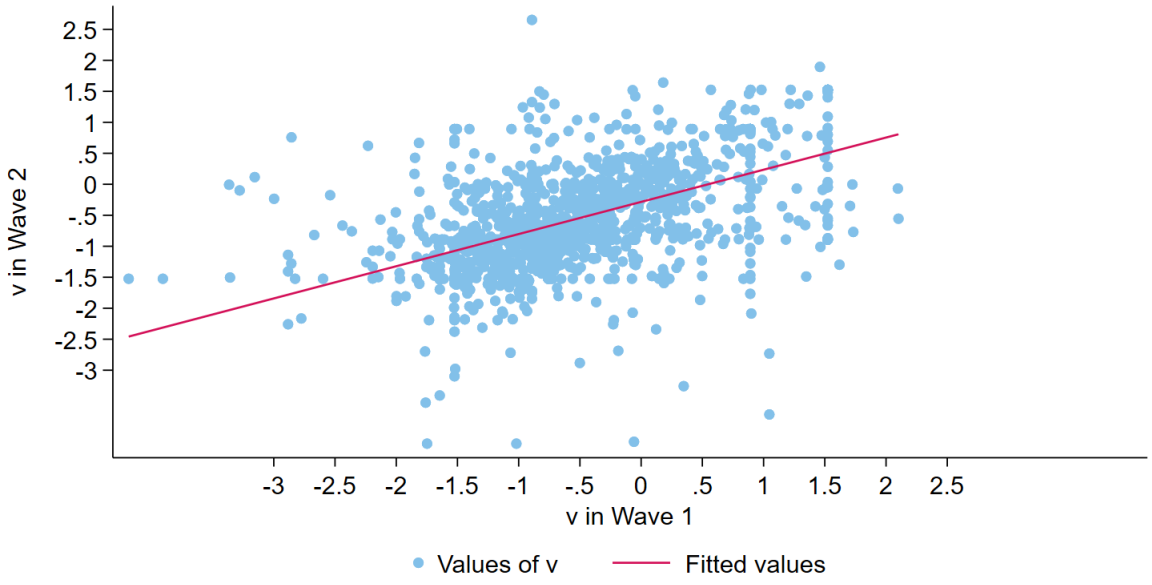


Figure 4: Temporal Stability of Welfare Weights

Notes: The figure presents a scatter plot of the progressivity estimates ν in Wave 1 and Wave 2 using the 1937 sample of Social Architects with valid responses in both waves.

ference in estimates across waves.[32] A more plausible explanation is lower response quality in Wave 1.[33] Consistent with this explanation, the Wave 2 estimate of -0.68 is much closer to the Wave 1 quality-adjusted progressivity estimate of -0.71 (see Section 4.4 for details on quality adjustment). If we additionally adjust responses in Wave 2 for response quality, the progressivity estimate is $\nu = -0.71$, which is even closer to the quality-adjusted wave 1 estimate (see Figure 3).[34] In addition to finding that the median progressivity estimate is stable across waves, we find that the distribution of progressivity estimates is also stable: a Kolmogorov-Smirnov test fails to reject the equality of the distributions of ν across waves ($D = 0.025$, $p = 0.697$).

Overall, we find that welfare weights are temporally stable across waves at the individual and aggregate levels. A high degree of temporal stability at the aggregate level supports using the elicited aggregate progressivity measures to calibrate optimal policy formulas.

[32]The unweighted estimates are: Wave 1: $\nu = -0.66, N = 1397$, bootstrap SE $= 0.03$, 95% normal CI $=$ [–0.72, –0.61]; Wave 2: $\nu = -0.68, N = 1397$, bootstrap SE $= 0.02$, 95% normal CI $=$ [–0.73, –0.64].

[33]The share of Social Architects flagged as having low-quality responses is higher in Wave 1 relative to the Wave 2 for several indicators: failing any comprehension question (7% in Wave 2 and 11% in Wave 1), inconsistent weights (8% in Wave 2 and 11% in Wave 1), and low confidence (34% in Wave 2 and 38% in Wave 1). See Figures A8 and A9 for the shares.

[34]The response quality adjustment in Wave 2 follows the same procedure as that used in Wave 1. Regression estimates of the effect of response quality indicators on the progressivity estimates can be found in Appendix Table A7.

4.9 Summary of Progressivity Estimates

In the previous sections, we found that the weighted median progressivity estimate in Wave 1—our baseline estimate—is given by $\nu = -0.63$. We assessed the robustness of welfare weights to individual-level response quality and corrected for low-quality responses. Since low-quality responses tended to be less progressive, the corrected progressivity estimate of $\nu = -0.71$ is more progressive. We also assessed the robustness of the welfare weights to variation in frames. The progressivity estimates are affected by (i) the first decision, with evidence of anchoring to the first decision, (ii) prompts to consider the consequences of choices, indicating evidence of mistakes, and (iii) the common Recipient across the comparisons, with no evidence of differential response quality across the treatments. We embrace the normative ambiguity across frames that vary the common Recipient, treating both frames as welfare relevant, and additionally, correct these estimates for mistakes based on the prompted treatments. The resulting bounds on the progressivity estimates are given by: $\nu \in [-0.78, -0.70]$. Our response-quality adjusted progressivity estimate of $\nu = -0.71$ also lies within these bounds. Finally, we find that the aggregate progressivity estimates are similar across the two waves of data collection, which supports using the elicited aggregate progressivity measures to calibrate optimal policy formulas.

Comparing our progressivity estimates $\nu \in [-0.78, -0.70]$ to the benchmarks in the literature—which classifies $\nu = |0.25|$ as "weak" redistributive tastes, $\nu = |1|$ as "fairly strong," and $\nu = |4|$ as "extremely strong" (Saez 2002, Allcott et al. 2019)—suggests that our elicited welfare weights fall slightly below the "fairly strong" threshold.

Optimal policy formulas can implement these welfare weights using the parametric function c^{ν} with $\nu \in [-0.78, -0.70]$ or using constant relative risk aversion (CRRA) utilities with a coefficient of relative risk aversion given by $\gamma = -\nu = [0.70, 0.78]$. Our finding of a close alignment between welfare weights and support for government redistribution suggests that calibrating optimal policy formulas with the elicited welfare weights would likely result in policies with broad public acceptance. In Section 5, we compare our estimates of welfare weights to other estimates in the literature, and in Section 6, we highlight the implications of the estimates for optimal income taxes.

Finally, we note that the reforms in our experiment are elicited in a gain-loss frame—one Recipient loses while the other gains—to mirror real policy trade-offs (Saez & Stantcheva 2016, Hendren & Sprung-Keyser 2020). However, we also test whether welfare weights are sensitive to framing in an additional experiment (see Appendix Section F for details and results). We find that welfare weights are slightly more progressive in a gain-gain frame compared to a gain-loss frame.

5 Comparing Welfare Weights

In this section, we compare the welfare weights elicited in our experiment to those implied by the U.S. income tax schedule and by broader tax and transfer policies. The welfare weights implied by policies—referred to as "inverse optimum weights"—represent politicians' aggregation of citizens' welfare weights, possibly influenced by political economy considerations. Details on the estimation of the inverse optimum weights and the comparisons can be found in Appendix Section E.3.

We obtain the inverse optimum weights implied by the U.S. income tax schedule from Hendren (2020). This paper "reveals" the implicit welfare weights from the marginal tax rates (MTRs), the observed income distribution, and estimates of the elasticity of taxable income (ETI). The baseline estimate of inverse-optimum weights uses MTRs in 2012 and mid-range ETI estimates. The resulting weights are decreasing with incomes but increase at the top percentile (see Appendix Figure A10).

The inverse optimum weights implied by U.S. tax and transfer policies are derived using the framework outlined in Hendren & Sprung-Keyser (2020). In this framework, the welfare weight $g(z)$ assigned to beneficiaries of a policy earning z is the inverse of the policy's Marginal Value of Public Funds (MVPF)—the beneficiaries' welfare gain from \$1 of government spending. Intuitively, a planner is willing to incur efficiency losses by implementing a policy with a low MVPF if the beneficiaries are assigned a high welfare weight. We use the MPVF estimates of tax and transfer policies in Hendren & Sprung-Keyser (2020) to infer the welfare weights implied by these policies. The resulting weights are roughly decreasing with the beneficiaries' incomes (See Appendix Figure A11).

We estimate the progressivity of each set of welfare weights using the parametric function c^ν, where c denotes beneficiaries' income and ν is a parameter guiding the progressivity of the weights.[35] The estimated progressivity of the inverse-optimum welfare weights implied by the income tax schedule is $\nu = -0.09$ (SE $= 0.01, 95\%$ CI $= [-0.12, -0.07]$) and for those implied by tax and transfer policies is $\nu = -0.15$ (SE $= 0.08, 95\%$ CI $= [-0.31, -0.00]$). The median welfare weights elicited using our experiment ($\nu \in [-0.78, -0.70]$) are about 8 to 9 times more progressive than the inverse-optimum weights implied by the income tax schedule and about 5 times more progressive than those implied by tax and transfer policies. Figure 5 presents the distribution of welfare weights implied by these estimates against the disposable income distribution.

There are several possible explanations for why the welfare weights we elicit in the experi-

[35]The functional form c^ν, which depends solely on income, can be applied either to pre-tax or post-tax income. Let z denote pre-tax income and $c = h(z)$ the corresponding post-tax income. If welfare weights are based on post-tax income as $g(c)$, then the corresponding weights based on pre-tax income are $g(h(z))$.

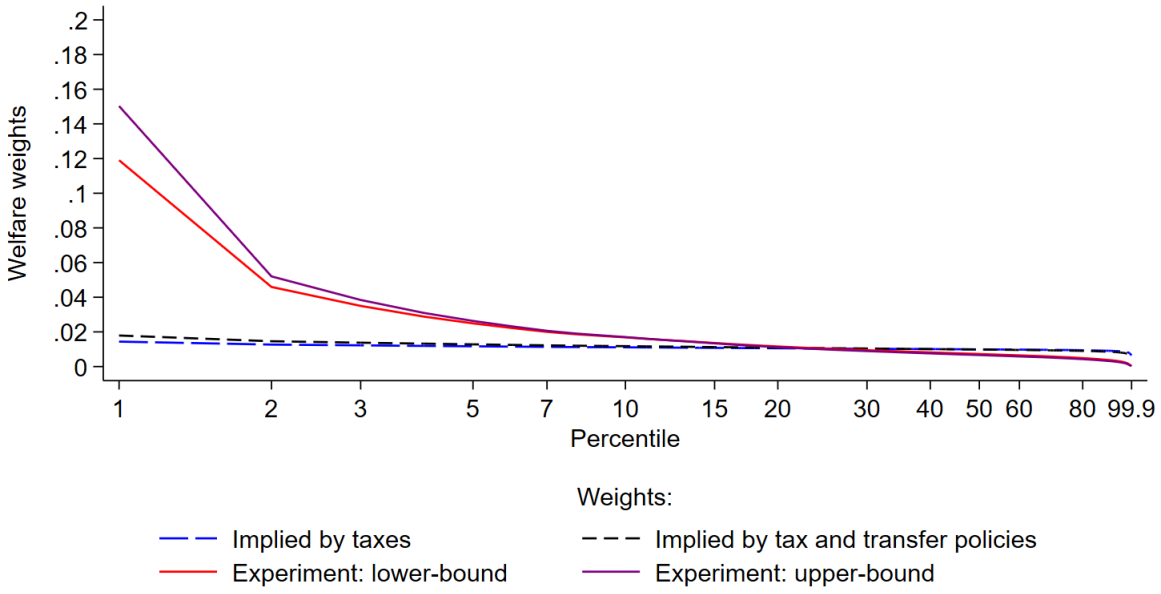


Figure 5: Comparison of Welfare Weights

Notes: The figure plots welfare weights against percentiles of the disposable income distribution. The x-axis has a natural log spacing. Welfare weights are interpolated using c^{ν}, where c is disposable income, and re-normalized to sum to 1. The figure plots the inverse-optimum weights implied by the income tax schedule ($\nu = -0.09$), inverse-optimum weights implied by tax and transfer policies ($\nu = -0.15$), and lower bound ($\nu = -0.70$) and upper bound ($\nu = -0.78$) of the progressivity estimates obtained from the experiment. Data on disposable incomes (which includes in-cash and in-kind transfers) is obtained from Piketty et al. (2018). Individuals with negative disposable income are excluded, and each individual is treated as a single filer.

ment are more progressive than those implied by existing tax and transfer policies. First, political economy considerations—such as politicians aggregating preferences only over certain subgroups (e.g., voters) or overweighting the preferences of high-income individuals—may lead to less progressive inverse-optimum weights. Second, politicians may be aggregating welfare weights that include other considerations, such as self-interest motives, that are absent or less pronounced in our experiment. Finally, differences may arise from assumptions about efficiency costs. In particular, high ETI estimates (than those used by Hendren (2020)) or high perceived efficiency costs by politicians can lead to less progressive inverse-optimum weights.

6 Implications for Optimal Labor Income Taxes

We investigate the implications of the welfare weights elicited in our experiment for optimal non-linear labor income taxes in the U.S. Our calibration applies the optimal tax formula derived by Saez (2001) and adapts the simulation procedures of Mankiw et al. (2009)

and Støstad & Cowell (2024). The optimal tax formula expresses the optimal marginal tax rate (MTR) schedule as a function of (i) the distribution of ability (wages), (ii) the elasticity of taxable income (ETI), and (iii) welfare weights. The calibration relies on the 2019 income distribution obtained from Piketty et al. (2018) to infer the underlying ability distribution, adopts a mid-range ETI of 0.25 (Saez et al. 2012), and uses estimates of welfare weights obtained in our experiment. Additional details are provided in Appendix Section E.4.

Table 8 presents the resulting average optimal MTRs. Using the lower-bound progressivity estimate ($\nu = -0.70$) yields an average optimal MTR of 56%, while the upper-bound estimate ($\nu = -0.78$) yields 58%. For comparison, the average MTR in 2019 was 28%, implying that prevailing rates are roughly 28–30 percentage points below the optimal rates implied by our welfare weights estimates. Closing the gap between existing rates and optimal rates requires more progressive tax rates or higher estimates of the elasticity of taxable income.

Table 8: Average Optimal Marginal Tax Rates (MTRs)

Case	E[MTR]	Guaranteed Income Share
Experiment: lower-bound ($\nu = -0.70$)	56%	57%
Experiment: upper-bound ($\nu = -0.78$)	58%	59%
Current MTRs (2019)	28%	-

Notes: The table presents average optimal MTRs and the guaranteed income transfer to bottom earners (as a share of the average income) calibrated using the lower bound ($\nu = -0.70$) and upper bound ($\nu = -0.78$) of the progressivity estimates obtained from the experiment. The last row presents the average MTRs in the U.S. in 2019. See Appendix Section E.4 for details.

7 Discussion

We elicit the welfare weights assigned by the general population of the U.S. using real-stakes online experiments. In our experiment, participants in the role of Social Architects make several real-stakes redistributive decisions that are used to identify the welfare weights assigned to participants in the role of Recipients. Social Architects' welfare weights are a reduced-form representation of underlying normative ideals, such as equality of opportunity, utilitarianism, redistribution based on the source of income, and poverty alleviation

The aggregate welfare weights are progressive: the median income elasticity of welfare weights is $\nu = -0.63$. We conduct several tests to assess the robustness, validity, and temporal stability of the elicited welfare weights. We assess the robustness of welfare weights to individual response quality and variation in the features of the experimental design. With various

adjustments, we provide bounds on the progressivity estimates, given by $\nu \in [-0.78, -0.70]$. We validate our measure of welfare weights by showing that it correlates with two survey measures of support for government redistribution and captures fairness concerns. Comparing responses across the two waves of data collections, four weeks apart, shows that welfare weights are temporally stable, both at the individual and aggregate levels.

Comparing the estimates of welfare weights obtained from our experiment to the weights implied by policies, we find that the welfare weights elicited in the experiment are about 8 to 9 times more progressive than the inverse-optimum weights implied by the income tax schedule and about 5 times more progressive than those implied by tax and transfer policies. Calibrating optimal income taxes with our estimates of welfare weights shows that the prevailing marginal tax rates are roughly 28–30 percentage points below the optimal rates implied by our welfare weights estimates.

Our reduced-form approach to eliciting welfare weights and the "small-reform" approach to taxation used by Saez & Stantcheva (2016) have a few limitations.

First, the welfare weights estimated in our paper cannot be used to evaluate non-marginal reforms.[36] For non-marginal reforms, the marginal value of the first dollar may differ from the marginal value of the last dollar. We assume a parametric form for the weights—in which welfare weights are a function of consumption—which allows evaluating large reforms, under the assumption that shifts in consumption induced by the reform capture the relevant shifts in Recipient characteristics that Social Architects value.

Second, the welfare weights estimated in our paper can only be used to evaluate policies conditioned on incomes. Future research can explore how welfare weights differ across non-income characteristics, such as age, gender, or ethnicity.

Finally, the elicited welfare weights may not be applicable across time, countries, and policy domains. There is evidence in the literature suggesting that people's support for redistribution may differ over time (Fisman et al. 2015) and across countries (e.g., Almås et al. 2020, Falk et al. 2018). Future work can test whether welfare weights differ across time, countries, and policy domains.

References

Abdellaoui, M., Kemel, E., Panin, A. & Vieider, F. M. (2019), 'Measuring time and risk preferences in an integrated framework', *Games and Economic Behavior* **115**, 459–469.

Alesina, A. & Angeletos, G.-M. (2005), 'Fairness and redistribution', *American economic review* **95**(4), 960–980.

[36]It is worth noting that alternative approaches, such as using the inverse-optimum weights implied by policies, also cannot be used to evaluate non-marginal reforms (Hendren 2020).

Alesina, A., Stantcheva, S. & Teso, E. (2018), 'Intergenerational mobility and preferences for redistribution', *American Economic Review* **108**(2), 521–554.

Allcott, H., Kim, J., Taubinsky, D. & Zinman, J. (2022), 'Are high-interest loans predatory? Theory and evidence from payday lending', *The Review of Economic Studies* **89**(3), 1041–1084.

Allcott, H., Lockwood, B. B. & Taubinsky, D. (2019), 'Regressive sin taxes, with an application to the optimal soda tax', *The Quarterly Journal of Economics* **134**(3), 1557–1626.

Allcott, H. & Taubinsky, D. (2015), 'Evaluating behaviorally motivated policy: Experimental evidence from the lightbulb market', *American Economic Review* **105**(8), 2501–2538.

Almås, I., Cappelen, A. & Tungodden, B. (2020), 'Cutthroat Capitalism versus Cuddly Socialism: Are Americans more Meritocratic and Efficiency-seeking than Scandinavians?', *Journal of Political Economy* **128**, 1753–1788.

Ambuehl, S., Bernheim, B. D. & Lusardi, A. (2022), 'Evaluating deliberative competence: A simple method with an application to financial choice', *American Economic Review* **112**(11), 3584–3626.

Bernheim, B. D. & Rangel, A. (2009), 'Beyond revealed preference: choice-theoretic foundations for behavioral welfare economics', *The Quarterly Journal of Economics* **124**(1), 51–104.

Berry, J., Fischer, G. & Guiteras, R. (2020), 'Eliciting and utilizing willingness to pay: Evidence from field trials in northern ghana', *Journal of Political Economy* **128**(4), 1436–1473.

Bollinger, C. R. (1998), 'Measurement error in the current population survey: A nonparametric look', *Journal of Labor Economics* **16**(3), 576–594.

Bound, J. & Krueger, A. B. (1991), 'The extent of measurement error in longitudinal earnings data: Do two wrongs make a right?', *Journal of labor economics* **9**(1), 1–24.

Bourguignon, F. & Spadaro, A. (2012), 'Tax–benefit revealed social preferences', *The Journal of Economic Inequality* **10**(1), 75–108.

Burchardi, K. B., De Quidt, J., Gulesci, S., Lerva, B. & Tripodi, S. (2021), 'Testing willingness to pay elicitation mechanisms in the field: Evidence from uganda', *Journal of Development Economics* **152**, 102701.

Bursztyn, L., Handel, B. R., Jimenez, R. & Roth, C. (2023), 'When product markets become collective traps: The case of social media'. NBER Working Paper no. 31771.

Cappelen, A. W., Konow, J., Sørensen, E. Ø. & Tungodden, B. (2013), 'Just luck: An experimental study of risk-taking and fairness', *American Economic Review* **103**(4), 1398–1413.

Carlsson, F., Johansson-Stenman, O. & Nam, P. K. (2014), 'Social preferences are stable over long periods of time', *Journal of public economics* **117**, 104–114.

Chen, D. L., Schonger, M. & Wickens, C. (2016), 'oTree—An open-source platform for laboratory, online, and field experiments', *Journal of Behavioral and Experimental Finance* **9**, 88–97.

Chetty, R., Looney, A. & Kroft, K. (2009), 'Salience and taxation: Theory and evidence', *American Economic Review* **99**(4), 1145–1177.

Chuang, Y. & Schechter, L. (2015), 'Stability of experimental and survey measures of risk, time, and social preferences: A review and some new results', *Journal of Development Economics* **117**, 151–170.

Cohn, A., Jessen, L. J., Klašnja, M. & Smeets, P. (2023), 'Wealthy americans and redistribution: The role of fairness preferences', *Journal of Public Economics* **225**, 104977.

Cruces, G., Perez-Truglia, R. & Tetaz, M. (2013), 'Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment', *Journal of public Economics* **98**, 100–112.

Dean, M. & Sautmann, A. (2021), 'Credit constraints and the measurement of time preferences', *Review of Economics and Statistics* **103**(1), 119–135.

Downs, A. (1957), 'An economic theory of political action in a democracy', *Journal of Political Economy* **65**(2), 135–150.

Drenik, A. & Perez-Truglia, R. (2018), 'Sympathy for the diligent and the demand for workfare', *Journal of Economic Behavior & Organization* **153**, 77–102.

Durante, R., Putterman, L. & Van der Weele, J. (2014), 'Preferences for redistribution and perception of fairness: An experimental study', *Journal of the European Economic Association* **12**(4), 1059–1086.

Enke, B., Graeber, T. & Oprea, R. (2023), 'Confidence, self-selection, and bias in the aggregate', *American Economic Review* **113**(7), 1933–1966.

Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D. & Sunde, U. (2018), 'Global evidence on economic preferences', *The Quarterly Journal of Economics* **133**(4), 1645–1692.

Fehr, E., Epper, T. & Senn, J. (2024), 'Social preferences and redistributive politics', *Review of Economics and Statistics* pp. 1–45.

Fehr, E. & Schmidt, K. M. (1999), 'A Theory of Fairness, Competition, and Cooperation', *Quarterly journal of Economics* pp. 817–868.

Fisman, R., Jakiela, P. & Kariv, S. (2015), 'How did distributional preferences change during the great recession?', *Journal of Public Economics* **128**, 84–95.

Fisman, R., Jakiela, P., Kariv, S. & Vannutelli, S. (2023), 'The distributional preferences of americans, 2013–2016', *Experimental Economics* **26**(4), 727–748.

Fisman, R., Kariv, S. & Markovits, D. (2007), 'Individual preferences for giving', *American Economic Review* **97**(5), 1858–1876.

Fleurbaey, M. & Maniquet, F. (2006), 'Fair income tax', *The Review of Economic Studies* **73**(1), 55–83.

Goldin, J. & Reck, D. (2022), 'Optimal defaults with normative ambiguity', *Review of Economics and Statistics* **104**(1), 17–33.

Hausman, J., Liu, H., Luo, Y. & Palmer, C. (2021), 'Errors in the dependent variable of quantile regression models', *Econometrica* **89**(2), 849–873.

Hendren, N. (2020), 'Measuring economic efficiency using inverse-optimum weights', *Journal of Public Economics* **187**, 104198.

Hendren, N. & Sprung-Keyser, B. (2020), 'A unified welfare analysis of government policies', *The Quarterly Journal of Economics* **135**(3), 1209–1318.

Hvidberg, K. B., Kreiner, C. T. & Stantcheva, S. (2023), 'Social positions and fairness views on inequality', *Review of Economic Studies* **90**(6), 3083–3118.

Jack, B. K., McDermott, K. & Sautmann, A. (2022), 'Multiple price lists for willingness to pay elicitation', *Journal of Development Economics* **159**, 102977.

Jacobs, B., Jongen, E. L. & Zoutman, F. T. (2017), 'Revealed social preferences of dutch political parties', *Journal of Public Economics* **156**, 81–100.

Karadja, M., Mollerstrom, J. & Seim, D. (2017), 'Richer (and holier) than thou? The effect of relative income improvements on demand for redistribution', *Review of Economics and Statistics* **99**(2), 201–212.

Kuziemko, I., Norton, M. I., Saez, E. & Stantcheva, S. (2015), 'How elastic are preferences for redistribution? Evidence from randomized survey experiments', *American Economic Review* **105**(4), 1478–1508.

List, J. A., Muir, I., Pope, D. & Sun, G. (2023), 'Left-digit bias at lyft', *Review of Economic Studies* **90**(6), 3186–3237.

Lockwood, B. B. & Weinzierl, M. (2016), 'Positive and normative judgments implicit in us tax policy, and the costs of unequal growth and recessions', *Journal of Monetary Economics* **77**, 30–47.

Luttmer, E. F. P. & Singhal, M. (2011), 'Culture, context, and the taste for redistribution', *American Economic Journal: Economic Policy* **3**(1), 157–179.

Luttmer, E. F. & Samwick, A. A. (2018), 'The welfare cost of perceived policy uncertainty: Evidence from social security', *American Economic Review* **108**(2), 275–307.

Mankiw, N. G., Weinzierl, M. & Yagan, D. (2009), 'Optimal taxation in theory and practice', *Journal of Economic Perspectives* **23**(4), 147–174.

Piketty, T., Saez, E. & Zucman, G. (2018), 'Distributional national accounts: methods and estimates for the united states', *The Quarterly Journal of Economics* **133**(2), 553–609.

Rees-Jones, A. & Taubinsky, D. (2020), 'Measuring "schmeduling"', *The Review of Economic Studies* **87**(5), 2399–2438.

Saez, E. (2001), 'Using elasticities to derive optimal income tax rates', *The review of economic studies* **68**(1), 205–229.

Saez, E. (2002), 'Optimal income transfer programs: intensive versus extensive labor supply responses', *The Quarterly Journal of Economics* **117**(3), 1039–1073.

Saez, E., Slemrod, J. & Giertz, S. H. (2012), 'The elasticity of taxable income with respect to marginal tax rates: A critical review', *Journal of economic literature* **50**(1), 3–50.

Saez, E. & Stantcheva, S. (2016), 'Generalized social marginal welfare weights for optimal tax theory', *American Economic Review* **106**(1), 24–45.

Schennach, S. (2022), 'Measurement systems', *Journal of Economic Literature* **60**(4), 1223–1263.

Schoemaker, P. J. & Hershey, J. C. (1992), 'Utility measurement: Signal, noise, and bias', *Organizational Behavior and Human Decision Processes* **52**(3), 397–424.

Silva, J. S. & Tenreyro, S. (2006), 'The log of gravity', *The Review of Economics and statistics* **88**(4), 641–658.

Singhal, M. (2008), 'Quantifying preferences for redistribution'. Working Paper.

Stantcheva, S. (2016), 'Comment on "Positive and normative judgments implicit in us tax policy and the costs of unequal growth and recessions" by Benjamin Lockwood and Matthew Weinzierl', *Journal of Monetary Economics* **77**, 48–52.

Stantcheva, S. (2021), 'Understanding tax policy: How do people reason?', *The Quarterly Journal of Economics* **136**(4), 2309–2369.

Stantcheva, S. (2023), 'How to run surveys: A guide to creating your own identifying variation and revealing the invisible', *Annual Review of Economics* **15**(1), 205–234.

Strulov-Shlain, A. (2023), 'More than a penny's worth: Left-digit bias and firm pricing', *Review of Economic Studies* **90**(5), 2612–2645.

Støstad, M. N. & Cowell, F. (2024), 'Inequality as an externality: Consequences for tax design', *Journal of Public Economics* **235**, 105139.

Weinzierl, M. (2014), 'The promise of positive optimal taxation: normative diversity and a role for equal sacrifice', *Journal of Public Economics* **118**, 128–142.

Wölbert, E. & Riedl, A. (2013), 'Measuring time and risk preferences: Reliability, stability, domain specificity'. Working Paper.

Appendix

Who Should Get Money? Estimating Welfare Weights in the U.S.

Francesco Capozza

Krishna Srinivasan

Contents

A Variable Definitions

Misperceptions

We elicit Social Architects' perceptions about the level of taxes paid by individuals, and the share of individuals with incomes below $35,000.

Overestimate the level of taxes: We ask Social Architects four questions designed to elicit their perceptions about the level of taxes paid by individuals in society. In particular, Social Architects are asked about their beliefs regarding (i) the share of households in the top tax bracket, (ii) the average tax rate of those in the top tax bracket, (iii) the share of households who pay no taxes, and (iv) the average tax rate of households with the median income. We focus on perceptions along these four dimensions because they were the most predictive of people's support for redistribution in Stantcheva (2021). Social Architects can select a number from 0 to 100 using a slider for each of the four questions. We identify misperceptions in each of the four variables as follows.

- *Gap in top-taxes* = *Beliefs about top-taxes* - 32.7
- *Gap in top-share* = *Beliefs about top-share* - 0.73
- *Gap in non-filers* = 44 - *Beliefs about non-filers*
- *Gap in median-taxes* = *Beliefs about median-taxes* - 13

We take the actual values from Stantcheva (2021). We orient the gap in non-filers such that a lower gap in non-filers corresponds to an overestimation in the level of taxes paid. We standardize each of the four misperception variables such that they have a mean of 0 and a standard deviation of 1. Then, we create an index by taking the equally weighted average of the four standardized variables and standardizing the resulting variable.

Overestimate share earning $<$ *15K*: We elicit Social Architects' beliefs about the share of households earning less than $15,000. This income level corresponds to the income of the Recipient with the lowest income in our experiment. Social Architects can select a number from 0 to 100 using a slider. We identify Architects' misperceptions by subtracting the actual value (11) from their responses. We obtain the actual value by looking at the share of individuals whose disposable income is below $15,000 in the data obtained from Piketty et al. (2018) (variable *diinc*). Finally, we standardize the misperceptions.

Views about Taxes and Government

We ask Social Architects several questions that elicit their views about the tax system and their trust in government, each capturing a unique mechanism that may explain people's support for redistribution. Many of these questions are drawn from Stantcheva (2021).

Behavioral responses high earners: Takes a value of 1 if a Social Architect indicates that the extent to which taxing high-income earners would encourage them to work less is "A moderate amount," "A lot," or "A great deal," and a value of 0 if the Social Architect indicates "A little," or "None at all."
Higher taxes high-incomes hurt economy: Takes a value of 1 if a Social Architect indicates that taxing high-income earners would "Hurt economic activity in the U.S.," and a value of 0 if the Social Architect indicates "Not have an effect on economic activity in the U.S." or "Help economic activity in the U.S."
No Belief trickle down: Takes a value of 1 if a Social Architect indicates that the lower class and working class would "Mostly lose" or "Neither lose nor win" if taxes on high-income earners were cut and a value of 0 if the Social Architect indicates that they would "Mostly win."
Fair distribution of income: Takes a value of 1 if a Social Architect indicates that the current income distribution is "Somewhat fair" or "Very fair" and a value of 0 if the Social Architect indicates "Neither unfair nor fair," "Somewhat unfair," or "Very unfair."
Low trust in government: Takes a value of 1 if a Social Architect trusts the government "Only some of the time" or "Never" and a value of 0 for responses "Most of the time" or "Just about always"
Govt should do less: Takes a value of 1 if a Social Architect indicates thar "Government is doing too much" or Government is doing just the right amount" to solve the country's problems and a value of 0 for "Government should do more."

B Pre-registration

The experimental design, sample restrictions, and analyses were pre-registered. We list the following deviations from the pre-analysis plan:

1. We pre-registered the inclusion of participants with an approval rating between 95 and 100 on Prolific. However, the "representative sample" option on Prolific does not permit screening by approval rate. Matching our data with Prolific's data on participants shows that our sample in Wave 1 includes participants with approval ratings between 93 and 100.

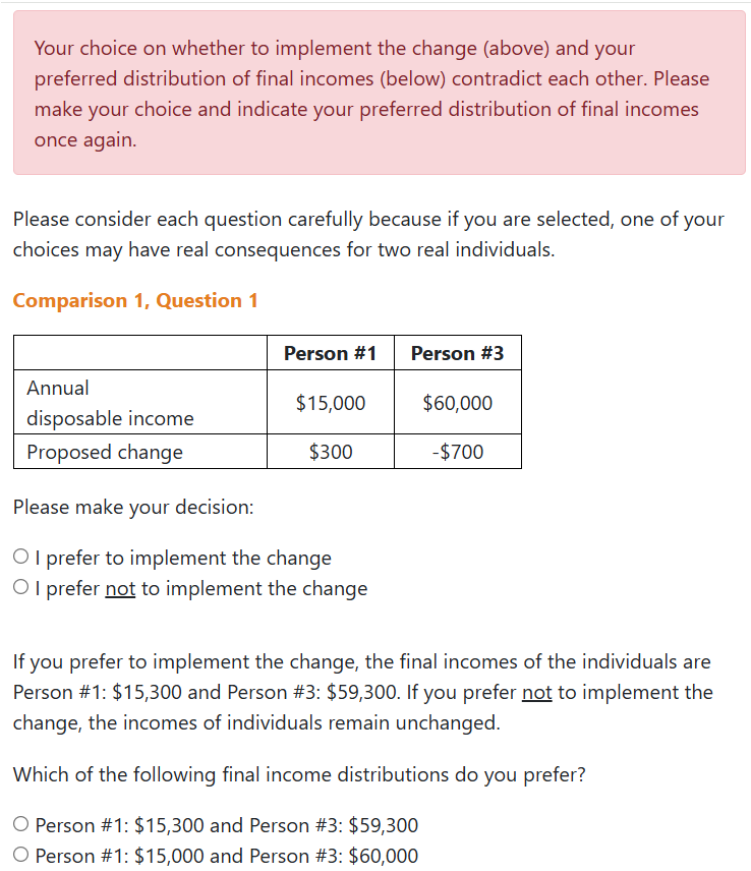
2. For our response-quality adjustment, we pre-registered three separate proxies indicating failure on each of the three comprehension checks. The proportions failing each check are 8%, 3%, and 2%, respectively. To improve power, we instead use a single indicator that flags failure on any of the three checks.

3. We pre-registered flagging respondents with unusual completion times ($\pm$ 2 standard

deviations from the mean) separately by treatment. We create this indicator variable separately for participants in the soft launch (about 250 participants) and the full launch; the median completion time in the soft launch (24.4 minutes) is longer than the completion time in the full launch (14 minutes), because of server congestion during the soft launch.

4. The following exploratory analyses were not pre-registered:
    - Table A5 on the effect of political affiliation on support for redistribution.
    - Table A6 on the effect of the common Recipient on response quality indicators
5. The regression in Column (5) of Table 7 on the mediation analysis was not pre-registered. The pre-registered mediation analysis is discussed in Appendix Section E.2.
6. Following Stantcheva (2021), we reverse code some variables in the mediation analysis discussed in Appendix Section E.2 to ensure that all coefficients have the same sign. This can help understand the overall share explained by the variables.

C Additional Figures



Your choice on whether to implement the change (above) and your preferred distribution of final incomes (below) contradict each other. Please make your choice and indicate your preferred distribution of final incomes once again.

Please consider each question carefully because if you are selected, one of your choices may have real consequences for two real individuals.

Comparison 1, Question 1

	Person #1	Person #3
Annual disposable income	$15,000	$60,000
Proposed change	$300	-$700

Please make your decision:

○ I prefer to implement the change
○ I prefer not to implement the change

If you prefer to implement the change, the final incomes of the individuals are Person #1: $15,300 and Person #3: $59,300. If you prefer not to implement the change, the incomes of individuals remain unchanged.

Which of the following final income distributions do you prefer?

○ Person #1: $15,300 and Person #3: $59,300
○ Person #1: $15,000 and Person #3: $60,000

Figure A1: Screenshot of a Decision Presented to Social Architects in Treatments Prompted

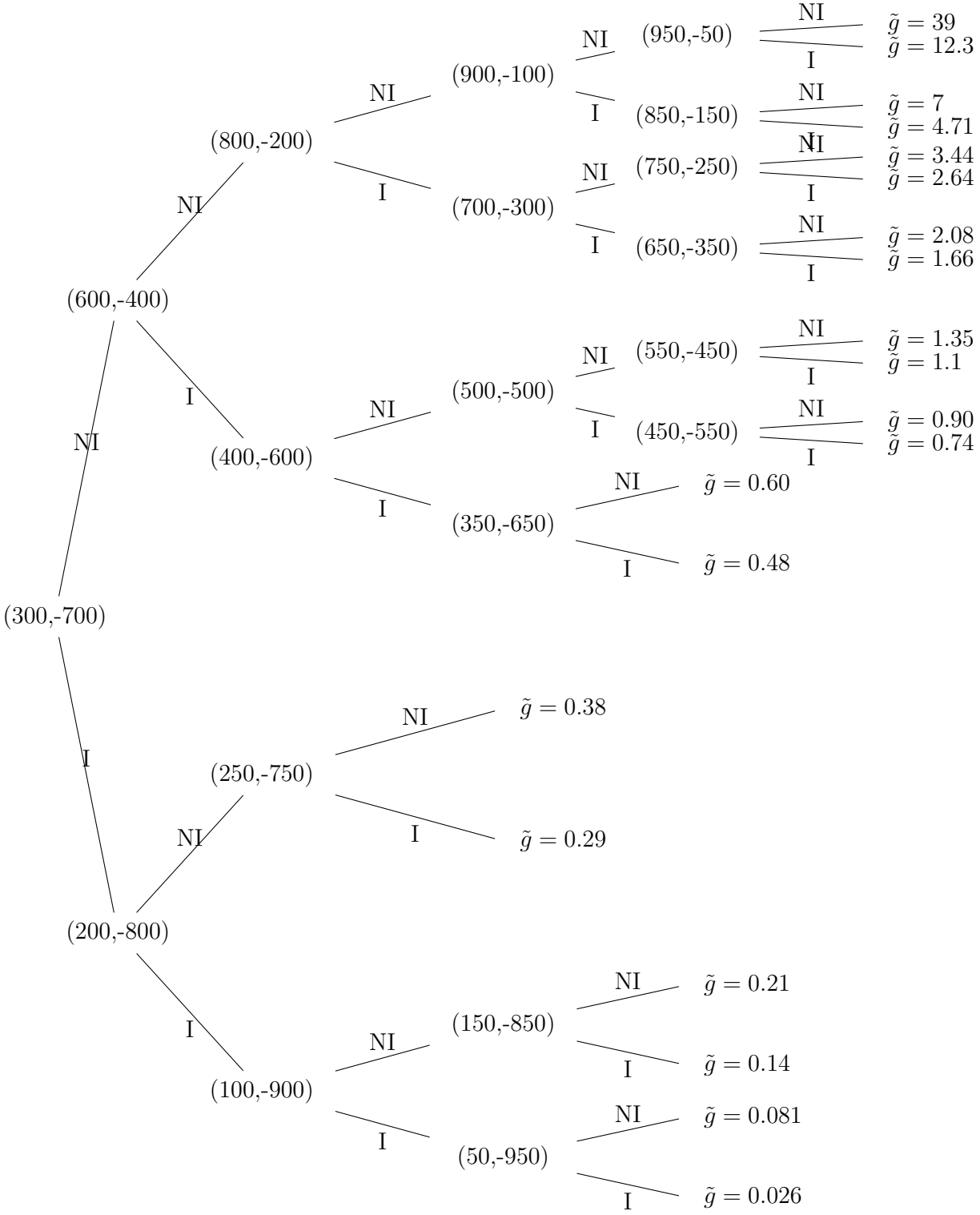


Figure A2: Reforms Selected by the Staircase Method, Starting at (300, −700)

Notes: The figure presents the reforms selected by the staircase method at each node, depending on whether the reform in the previous node was implemented ("I") or not implemented ("NI"). This staircase was implemented in treatments in which the reform in the first decisions is (300, −700). The parameter $\tilde{g}$ is the ratio of the weight assigned to the higher-income Recipient and lower-income Recipient.

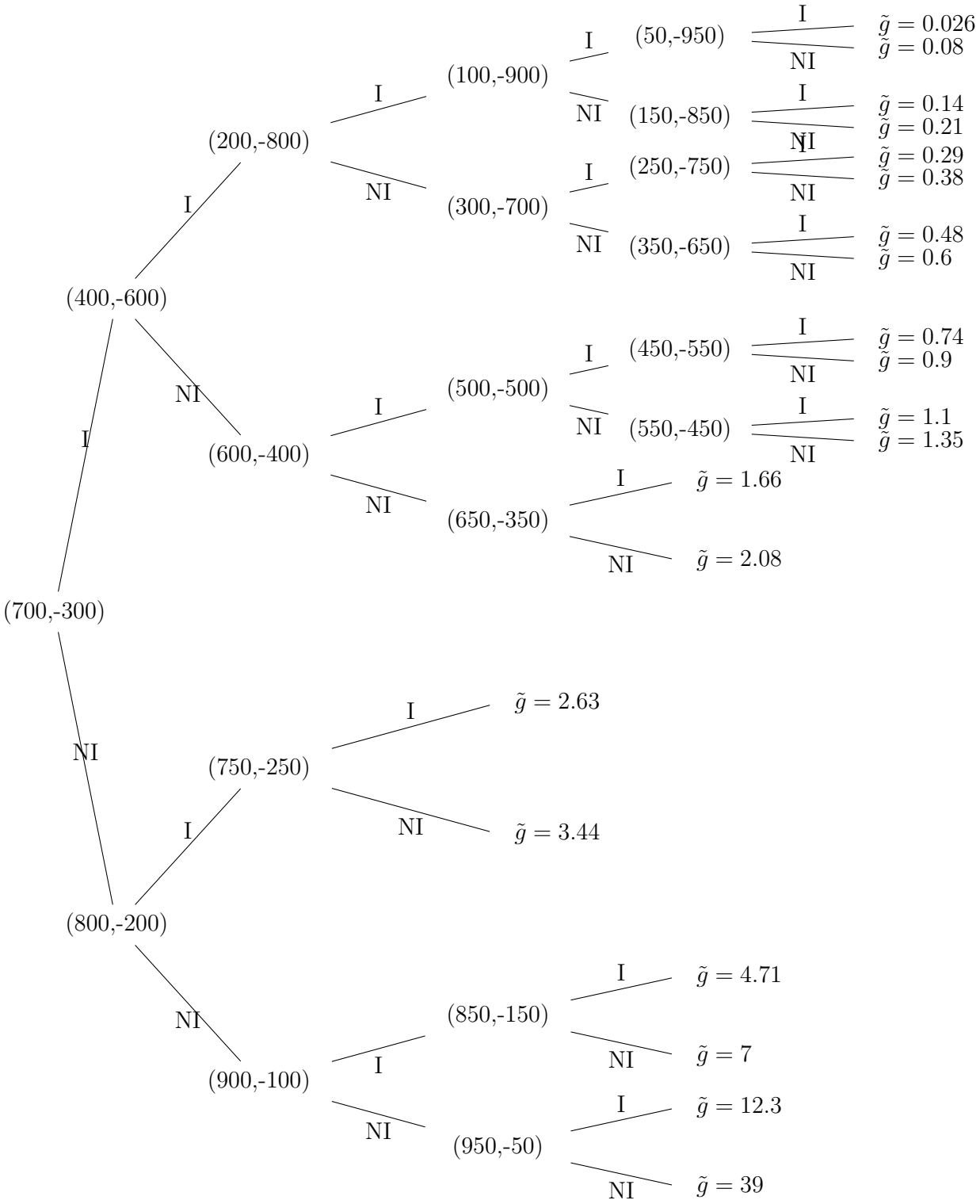


Figure A3: Reforms Selected by the Staircase Method, Starting at (700, −300)

Notes: The figure presents the reforms selected by the staircase method at each node, depending on whether the reform in the previous node was implemented ("I") or not implemented ("NI"). This staircase was implemented in treatments in which the reform in the first decisions is (700, −300). The parameter $\tilde{g}$ is the ratio of the weight assigned to the higher-income Recipient and lower-income Recipient.

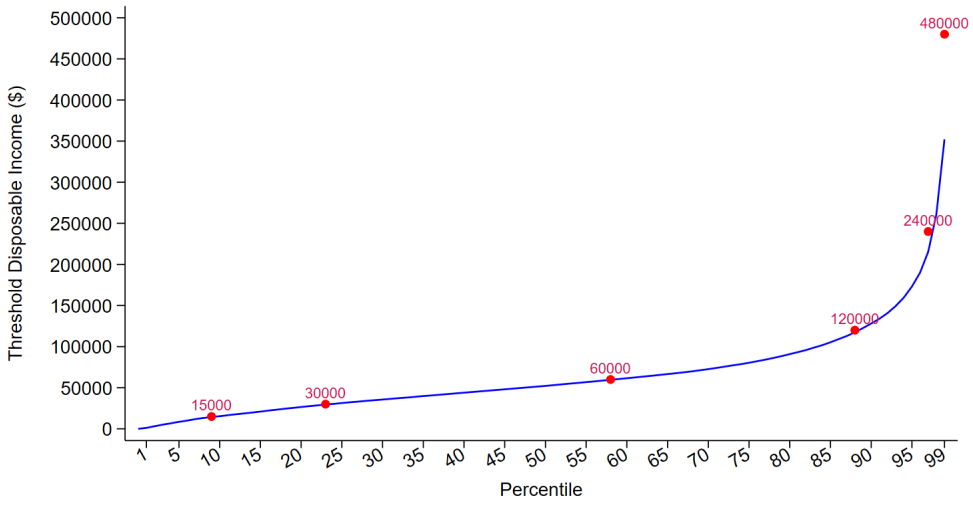


Figure A4: Disposable Incomes of the Six Recipients

Notes: The figure plots the incomes of the six Recipients (dots) against the disposable income distribution (line) in the U.S. in 2019. The horizontal axis indicates the percentiles, and the vertical axis indicates the threshold annual disposable incomes corresponding to the percentiles. Data on disposable incomes (which includes in-cash and in-kind transfers) is obtained from Piketty et al. (2018). Individuals with negative disposable income are excluded, and each individual is treated as a single filer.

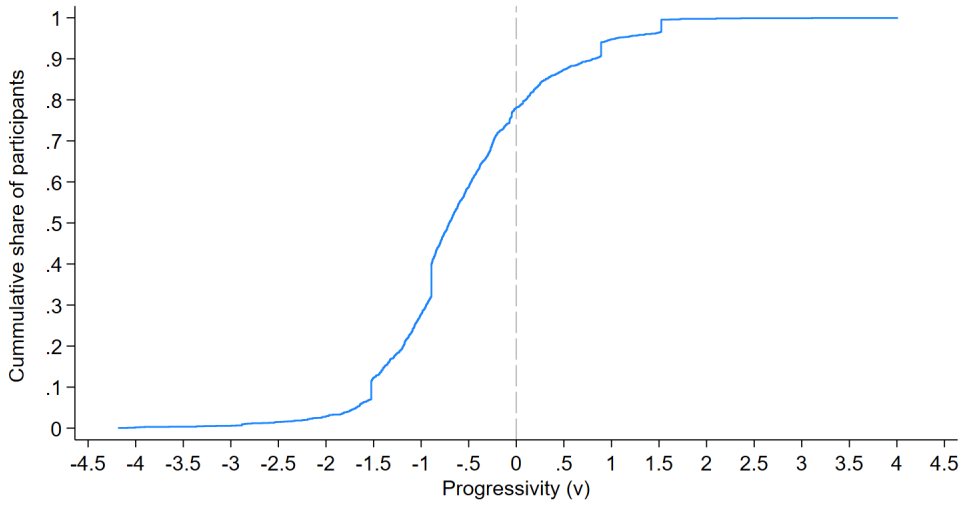


Figure A5: Distribution of Progressivity of Welfare Weights

Notes: The figure presents the cumulative distribution function (CDF) of the progressivity of welfare weights (ν) using data from Wave 1.

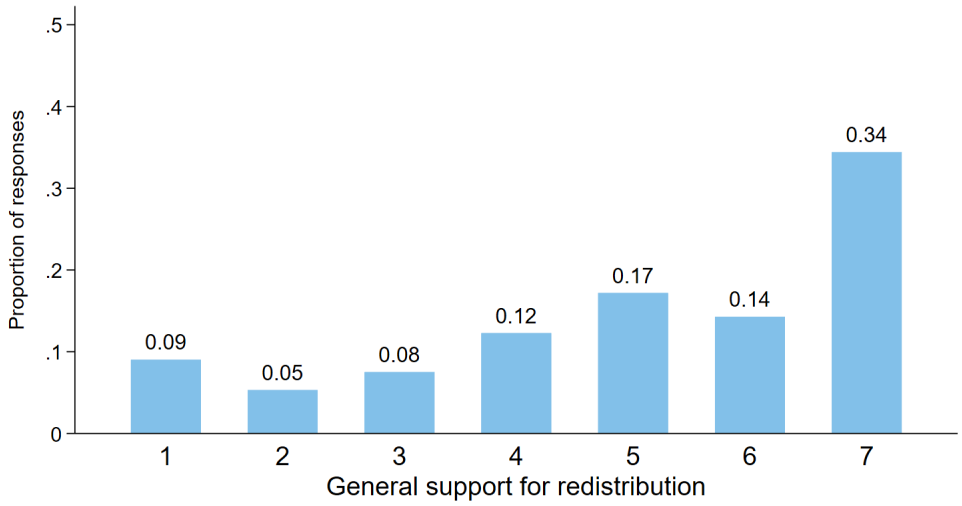


Figure A6: Distribution of General Support for Redistribution

Notes: This figure presents the frequency of responses for the general support for redistribution question. Responses take values from 1 through 7, with higher values indicating stronger support for the government to do something to reduce income differences between the rich and poor. The figure uses data from Wave 1.

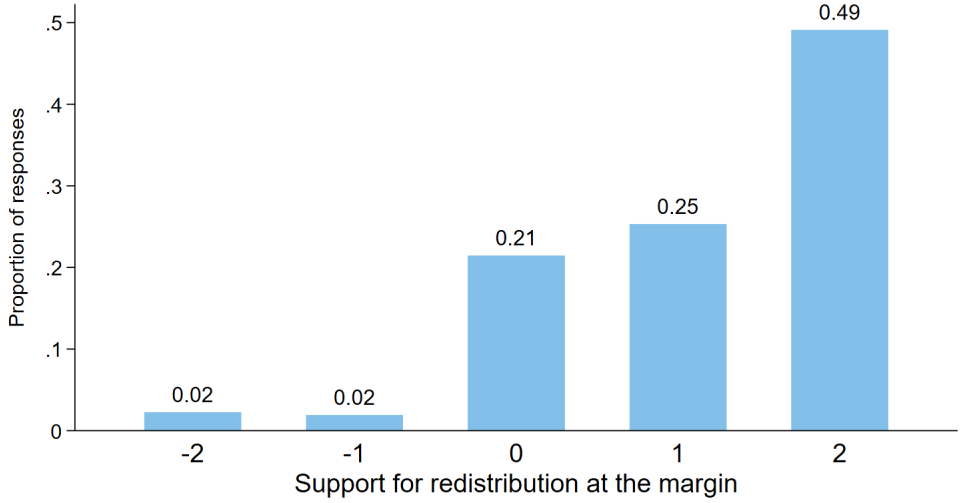


Figure A7: Distribution of Support for Redistribution at the Margin

Notes: This figure presents the frequency of responses for the support for redistribution at the margin question. Responses take values from -2 to $+2$, where positive (negative) values indicate redistribution from high-income (low/middle-income) individuals to low/middle-income (high-income) individuals. A value of zero indicates a desire for no additional redistribution. The figure uses data from Wave 1.

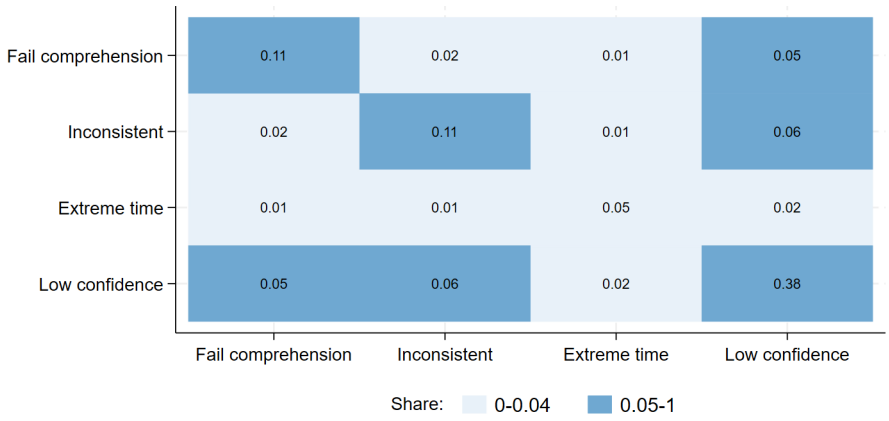


Figure A8: Share Flagged as Low-Quality Based on Indicators in Wave 1

Notes: This figure presents the share of observations where each indicator pair simultaneously equals 1. Diagonals show marginal shares. *Fail comprehension* equals 1 if a Social Architect failed one or more comprehension questions in the first try and 0 otherwise. *Inconsistent* equals 1 if a Social Architect's $\tilde{g}$ in the third and sixth comparisons do not match in sign. *Extreme time* equals 1 if a Social Architect's time spent on the survey lies beyond two standard deviations of the mean. *Low confidence* equals 1 if a Social Architect reports confidence levels lower than the highest category of "Very Much." The figure uses data from Wave 1.

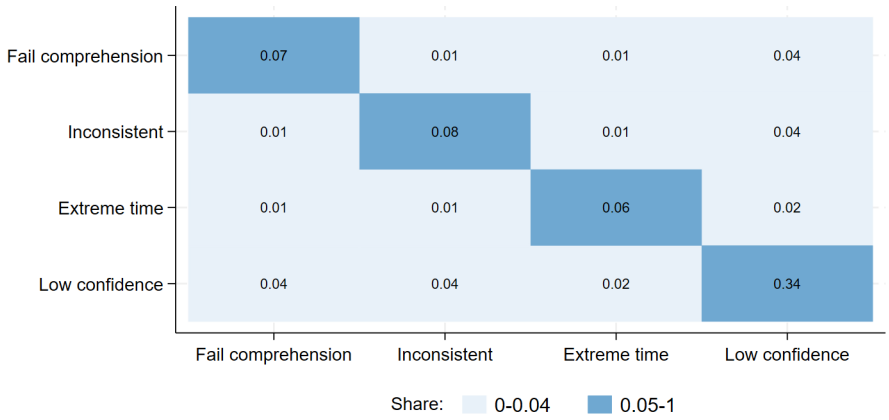


Figure A9: Share Flagged as Low-Quality Based on Indicators in Wave 2

Notes: This figure presents the share of observations where each indicator pair simultaneously equals 1. Diagonals show marginal shares. *Fail comprehension* equals 1 if a Social Architect failed one or more comprehension questions in the first try and 0 otherwise. *Inconsistent* equals 1 if, in the identical third and sixth comparisons, a Social Architect assigns progressive welfare weights ($\tilde{g} < 1$) in one and regressive welfare weights ($\tilde{g} > 1$) in the other. *Extreme time* equals 1 if a Social Architect's time spent on the survey lies beyond two standard deviations of the mean. *Low confidence* equals 1 if a Social Architect reports confidence levels lower than the highest category of "Very Much." The figure uses data from Wave 2.

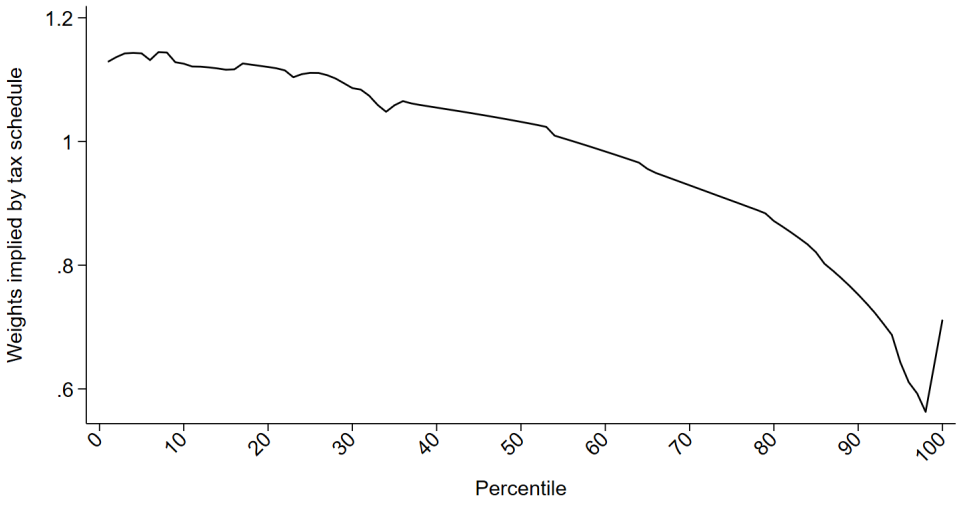


Figure A10: Welfare Weights Implied by the Tax Schedule

Notes: The figure plots the inverse-optimum welfare weights implied by the income tax schedule, obtained from Hendren (2020), for each percentile of the income distribution. We exclude the quintile with negative incomes.

Figure A11: Welfare Weights Implied by Tax and Transfer Policies

Notes: The figure plots the inverse-optimum welfare weights implied by tax and transfer policies against the approximate incomes of the beneficiaries of the policies. The welfare weight of a policy is the inverse of its marginal value of public funds (MVPF). Data on the MVPF of the policies is obtained from Hendren & Sprung-Keyser (2020). See Appendix Section E.3 for details.

D Additional Tables

Table A1: Randomization Check

Variable	p-value Wave 1	Wave 2
Male	0.000	0.000
Income $<$ 30,000	0.536	0.536
Income 30–59,999	0.001	0.001
Income 60–99,999	0.000	0.000
Income 100–149,999	0.005	0.005
Income $\geq$ 150,000	0.000	0.000
Education: High school or less	0.005	0.005
Education: Some college	0.000	0.000
Education: Bachelor or Associate	0.000	0.000
Education: Masters or above	0.094	0.094
Age: 18–24	0.000	0.000
Age: 25–34	0.002	0.002
Age: 35–44	0.000	0.000
Age: 45–54	0.573	0.573
Age: 55–64	0.000	0.000
Age: $\geq$ 65	0.002	0.002
Region: Northeast	0.003	0.003
Region: Midwest	0.000	0.000
Region: South	0.071	0.071
Region: West	0.000	0.000
Republican	0.000	0.000

Notes: The table presents presents the p-values from an F-test from a regression of the particular characteristic on treatment indicators. The second column uses data from Wave 1 and the third column uses data from Wave 2.

Table A2: Example U.S. Tax and Transfer Policies

Policy	**Amount**
Massachusetts one-time electric-bill credit	$50 per residential account
Credit for Other Dependents	Maximum $500
Energy-Efficient Home Improvement Credit	Range from $150 to $1500
Georgia 2025 surplus tax rebate	Maximum range $250-$500
North Dakota Primary Residence Credit	Maximum $1600
NYC Enhanced Real-Property Tax Credit	Maximum $500
Earned Income Tax Credit (no qualifying children, 2024)	Maximum $632

Notes: The table presents several U.S. tax and transfer policies with amounts around $500.

Table A3: Role of Self-Interest Motives

	(1)	(2)
Income near Recipient earning 15K	0.187***	0.199***
	(0.013)	(0.029)
Income near Recipient earning 30K	0.197***	0.106***
	(0.013)	(0.013)
Income near Recipient earning 60K	0.183***	-0.018**
	(0.010)	(0.008)
Income near Recipient earning 120K	0.146***	-0.062***
	(0.013)	(0.014)
Income near Recipient earning 240K	0.166***	-0.027
	(0.036)	(0.043)
Income near earning 480K	0.153*	-0.006
	(0.091)	(0.077)
Constant	0.137***	0.165***
	(0.001)	(0.001)
Observations	11976	11976

Notes: The table presents coefficient estimates from linear regressions. The dependent variable $g(R_j)_i$ is the weight assigned by Social Architect i to Recipient j. In Equation (1), the indicator variables *Income near Recipient earning 15K* through *Income near Recipient 480K* equal 1 if Social Architect i's income lies in each of the following brackets, respectively: [0, $22,500), [$22,500, $45,000), [$45,000, $90,000), [$90,000, $180,000), [$180,000, $360,000), and [$360,000, ∞). In Equation (2), the indicator variables *Income near Recipient earning 15K* through *Income near Recipient 480K* equal 1 if Social Architect i's income is within $\pm$ 20% of the income of the six Recipients, respectively. The regressions use data from Wave 1. Both regressions include Social Architect fixed effects. Robust Standard errors (HC1) in parentheses.
*p<0.1, **p<0.05, ***p<0.01

Table A4: Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Low-income Recipient on Left	-0.024					-0.044
	(0.047)					(0.045)
Common recipient 60K		-0.138***				-0.131***
		(0.046)				(0.045)
Descending order of income diff			-0.081*			-0.076*
			(0.047)			(0.045)
First decision (300, -700)				-0.171***		-0.188***
				(0.045)		(0.045)
Prompted					-0.066	-0.075*
					(0.047)	(0.045)
Constant	-0.689***	-0.654***	-0.668***	-0.607***	-0.668***	-0.448***
	(0.034)	(0.033)	(0.033)	(0.032)	(0.033)	(0.055)
Observations	1996	1996	1996	1996	1996	1996

Notes: The table presents coefficient estimates from a median regressions. The dependent variable is the progressivity of the welfare weights (ν). The explanatory variables include treatment indicators. The regressions use data from Wave 1. Standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Table A5: Support for Redistribution and Political Affiliation

	General redistribution	Redistribute at margin
	(1)	(2)
Republican	-1.210***	-0.332***
	(0.097)	(0.048)
Constant	5.428***	1.278***
	(0.048)	(0.025)
Observations	1996	1996
R^2	0.083	0.025

Notes: The table presents coefficient estimates from linear regressions. The dependent variable in Column (1) takes values from 1 through 7, with higher values indicating stronger support for the government to do something to reduce income differences between the rich and poor. The dependent variable in Column (2) takes values from -2 to $+2$, where positive (negative) values indicate redistribution from high-income (low/middle-income) individuals to low/middle-income (high-income) individuals. A value of zero indicates a desire for no additional redistribution. *Republican* equals 1 for Republicans and 0 otherwise. The regression uses data from Wave 1. HC3 standard errors in parentheses.
*p<0.1, **p<0.05, ***p<0.01

Table A6: Quality Indicators and Treatments

	Fail comprehension	Inconsistent	Extreme Time	Low Confidence	Stable
	(1)	(2)	(3)	(4)	(5)
Common recipient 60K	-0.019	0.002	-0.008	0.029	0.033
	(0.014)	(0.014)	(0.010)	(0.022)	(0.021)
Constant	0.120***	0.109***	0.054***	0.368***	0.795***
	(0.010)	(0.010)	(0.007)	(0.015)	(0.015)
Observations	1996	1996	1996	1996	1397

Notes: The table coefficient estimates from linear regressions. *Fail comprehension* equals 1 if a Social Architect failed one or more comprehension questions in the first try and 0 otherwise. *Inconsistent* equals 1 if, in the identical third and sixth comparisons, a Social Architect assigns progressive welfare weights ($\tilde{g} < 1$) in one and regressive welfare weights ($\tilde{g} > 1$) in the other. *Extreme time* equals 1 if a Social Architect's time spent on the survey lies beyond two standard deviations of the mean and 0 otherwise. *Low confidence* equals 1 if a Social Architect reports confidence levels lower than the highest category of "Very Much." *Stable* equals 1 if a Social Architect's welfare weights across the two waves do not match in sign. The explanatory variable is a treatment indicator. The regressions use data from Wave 1. HC3 standard errors in parentheses.
*p<0.1, **p<0.05, ***p<0.01

Table A7: Welfare Weights and Response Quality in Wave 2

	(1)
Fail comprehension	-0.050
	(0.095)
Inconsistent	0.417***
	(0.089)
Extreme time	0.053
	(0.103)
Low confidence	0.089*
	(0.052)
Observations	1397
Controls?	Yes

Notes: The table presents coefficient estimates from a median quantile regression. The dependent variable is the progressivity of the welfare weights (ν). *Fail comprehension* equals 1 if a Social Architect failed one or more comprehension questions in the first try and 0 otherwise. *Inconsistent* equals 1 if, in the identical third and sixth comparisons, a Social Architect assigns progressive welfare weights ($\tilde{g} < 1$) in one and regressive welfare weights ($\tilde{g} > 1$) in the other. *Extreme time* equals 1 if a Social Architect's time spent on the survey lies beyond two standard deviations of the mean. *Low confidence* equals 1 if a Social Architect reports confidence levels lower than the highest category of "Very Much." Controls include treatment indicators (those specified in Table 6) and demographic controls (those specified in Table 4). The regression uses data from Wave 2. Standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

E Additional Analysis

E.1 Correcting for Anchoring

We find that the progressivity of the welfare weights is affected by the first decisions of the staircase method, consistent with Social Architects' anchoring to the first decision. To account for this, we adjust our progressivity estimates following Luttmer & Samwick (2018) and Bursztyn et al. (2023).

Let r_c^r denote the reported reform amount accruing to the low-income Recipient at the point of indifference in comparison c, for $c \in \{1, \ldots, 5\}$. We assume r_c^r is a weighted average of the starting value r^s (either \$300 or \$700, depending on the treatment) and the actual value r_c^a:

$$r_c^r \;=\; \beta_c\, r^s \;+\; (1-\beta_c)\, r_c^a. \tag{A1}$$

By rearranging the terms, we obtain the actual value of the reform:

$$r_c^a \;=\; r_c^r \;-\; \frac{\beta_c}{1-\beta_c}\big(r^s - r_c^r\big). \tag{A2}$$

We estimate β_c by regressing the reported value r_c^r on the starting value r^s, including controls for response quality (described in Section 4.4) and demographics (described in Section 4.3). Table A8 reports the regression results. We find only moderate effects of anchoring, with values of β_c ranging from 0.06 to 0.11. The coefficients in only two of the five regressions are statistically significant at conventional levels ($p < 0.05$).

Using the estimates β_c for each comparison $c \in \{1, \ldots, 5\}$, we compute the adjusted reform amounts using Equation A2. We truncate these adjusted reform amounts at \$25 and \$975—the bounds of the unadjusted reform amounts. We then estimate welfare weights with these adjusted reform amounts using the procedures described in the main text.

E.2 What Do Welfare Weights Capture?

In the main text, we showed that the relationship between welfare weights and support for redistribution is mediated by fairness concerns. Here, we test whether other concerns and misperceptions also mediate the relationship. We proceed in two steps. First, we estimate the overall variation in support for redistribution at the margin that can be explained by welfare weights using a linear regression. Some of this overall variation may be explained by various concerns, which "mediate" the effect of welfare weights. In the second step, we decompose this overall variation in support for redistribution explained by welfare weights into the variation explained by each concern using a Gelbach decomposition (Gelbach 2016).

Table A8: Adjustment of Starting Point

	r_1^r (1)	r_2^r (2)	r_3^r (3)	r_4^r (4)	r_5^r (5)
Starting value 300	0.071**	0.115***	0.066*	0.068*	0.062*
	(0.034)	(0.034)	(0.034)	(0.036)	(0.036)
Observations	1996	1996	1996	1996	1996
Controls?	Yes	Yes	Yes	Yes	Yes

Notes: The table reports coefficient estimates from linear regressions. The dependent variables in Columns (1)–(5) are the reported reform amounts accruing to the low-income Recipient at the point of indifference in each of the five comparisons: $r_c^r \, \forall \, c \in \{1, \ldots, 5\}$. *Starting value 300* equals 300 if a Social Architect is assigned to a treatment in which the first decision is ($300, −$700) and 700 if the first decision is ($700, −$300). Controls include response quality indicators (described in Section 4.4) and demographic indicators (described in Section 4.3). The regressions use data from Wave 1. HC3 standard errors in parentheses.
*p<0.1, **p<0.05, ***p<0.01

Table A10 presents linear regressions in which the dependent variable is Social Architects' support for redistribution at the margin (taking values from −2 to +2). In Column (1), the explanatory variable is $1(\nu > 0)$, which is an indicator variable taking a value of 1 if a Social Architect has progressive weights and 0 otherwise. The coefficient estimate of $1(\nu > 0)$, is 0.44: this is the overall variation in support for redistribution at the margin explained by welfare weights. In Column (2), we add additional variables to the regression, which capture various concerns and perceptions. Definitions of the variables can be found in Appendix Section A. When we add other variables to the regression, the coefficient estimate of $1(\nu > 0)$ drops to 0.32. This suggests that about 29% of the overall variation in support for redistribution that is explained by welfare weights goes through these other variables.

To identify how much of the overall variance is explained by each of the other variables, we conduct a decomposition procedure proposed by Gelbach (2016). Table A9 presents the results. We find that fairness concerns explain 3% of the overall variation in support for redistribution that is explained by welfare weights; beliefs about higher taxes hurting the economy explain 9% and views about the scope of government explain 14%. Social Architects' misperceptions about the level of taxes paid by individuals and the share of individuals with incomes below $15,000 are including their

We also measured Social Architects' misperceptions about the level of taxes paid by individuals and the share of individuals with incomes below $15,000. While Social Architects do have misperceptions along these margins, their welfare weights do not capture their misperceptions.

Table A9: Decomposition of Treatment Effects

	(1)
Fair distribution of income	3.01
Behavioral responses high-earners	0.83
High taxes high-incomes hurt economy	8.33
No belief trickle down	0.20
Low trust in government	0.09
Govt is doing too little	14.07
Overestimate taxes	0.01
Overestimate low-income share	0.13
Share unexplained	70.94

Notes: The table presents the Gelbach decomposition of the treatment effects following Gelbach (2016). Each value indicates the share of the treatment effects explained by each of the factors, where the dependent variable is the share of policies supported. Share unexplained is the share of the treatment effects not explained by the factors.

Table A10: Support for Redistribution and Mechanisms

	Redistribute at margin	
	(1)	(2)
If Progressive ($\nu > 0$)	0.443***	0.314***
	(0.021)	(0.021)
Fair distribution of income		-0.110***
		(0.024)
Behavioral responses high-earners		-0.069***
		(0.019)
High taxes high-incomes hurt economy		-0.154***
		(0.022)
No belief trickle down		-0.038
		(0.023)
Low trust in government		-0.064***
		(0.021)
Govt Should do less		-0.203***
		(0.019)
Overestimate taxes		0.002
		(0.009)
Overestimate low-income share		0.004
		(0.008)
High Income		-0.018
		(0.017)
High Age		-0.056***
		(0.017)
Low Education		-0.024
		(0.020)
Female		-0.042**
		(0.016)
Republican		-0.044**
		(0.019)
Constant	0.399***	0.815***
	(0.019)	(0.041)
Observations	1996	1996

Notes: The table coefficient estimates from linear regressions. The dependent variable takes values from -2 to $+2$, where positive (negative) values indicate redistribution from high-income (low/middle-income) individuals to low/middle-income (high-income) individuals. A value of zero indicates a desire for no additional redistribution. Definitions of the explanatory variables can be found in Appendix Section A. HC3 standard errors in parentheses.
*p<0.1, **p<0.05, ***p<0.01

E.3 Comparison to Inverse-Optimum Weights

Inverse-Optimum Weights Implied by Tax Schedule: We obtain the inverse optimum welfare weights implied by the U.S. income tax schedule from Hendren (2020). Hendren (2020) computes MTRs using the universe of tax returns in 2012, incorporating ordinary income taxes, the alternative minimum tax (AMT), the earned income tax credits (EITC), and state, local, and Medicare taxes. The baseline estimates of ETI are: top earners (0.30); bottom earners subject to EITC in the phase-in region (0.31) and in the phase-out region (0.14); mid-range estimate elsewhere (0.30). We exclude the bottom quintile with negative incomes from our analysis. Figure A10 plots the inverse-optimum weights implied by the tax schedule against the quintiles of the income distribution.

Inverse-Optimum Weights Implied by Tax Schedule: The inverse optimum weights implied by U.S. tax and transfer policies are derived using the framework outlined in Hendren & Sprung-Keyser (2020). In this framework, the welfare weight $g(z)$ assigned to beneficiaries of a policy earning z is the inverse of the policy's Marginal Value of Public Funds (MVPF)—the beneficiaries' welfare gain from \$1 of government spending. The MVPF of a policy affecting beneficiaries with incomes z^* is defined as the beneficiaries' willingness to pay for the policy (s^*) divided by the net cost (c) accrued from the policy to the government. To replicate this policy (with benefits s^*) through adjustments to the tax schedule, the policy would cost $s^*g(z^*)$, where $g(z^*)$ is the welfare weight. It would be cheaper to achieve s^* through the policy than through adjustments to the tax schedule if and only if $c \leq s^*g(z^*)$. Rewriting this expression yields the following equation: $MVPF = s^*/c \geq 1/g(z^*)$. We focus on tax and in-cash transfer policies, which are the closest analogues to the reform in our experiment: a \$1 spending costs the government roughly \$1, generates benefits of \$1, and is typically conditioned only on income. These include policies in Figure 6 (Panel A) of Hendren & Sprung-Keyser (2020). Additionally, we drop policies with an MVPF of infinity or a negative MVPF, as our theoretical framework and experimental design cannot accommodate such values. Figure A10 plots the inverse-optimum weights implied by the tax and transfer policies against the income distribution.

Estimating the Progressivity of Welfare Weights: We estimate the progressivity of the inverse-optimum welfare weights implied by the tax schedule by regressing the welfare weights on the log of income, and estimating this regression using a Poisson pseudo–maximum likelihood (PPML) estimation. We estimate the progressivity of the inverse-optimum welfare weights implied by tax and transfer policies similarly.

Plotting Welfare Weights: Figure 5 in the main text plots the three sets of welfare weights against the disposable income distribution. The x-axis plots the average disposable income of each percentile. For each set of weights, we interpolate using the function c^{ν}, where c denotes disposable income and ν is the estimated progressivity parameter. Finally, each set of welfare weights is normalized to sum to one. Disposable income (including in-cash and in-kind transfers) is taken from the World Inequality Database (WID). We exclude individuals with negative disposable income and treat each individual as a single filer.

E.4 Calibration of Optimal Income Taxes

We explore the implications of the welfare weights elicited in the experiment for the optimal non-linear labor income taxes in the U.S. We use the optimal tax formula derived in Saez (2001), and follow the simulations presented in Mankiw et al. (2009) and Støstad & Cowell (2024).

The optimal tax formula solves a social planner's problem of maximizing social welfare subject to constraints. It expresses optimal marginal tax rates (MTRs) as a function of (i) the shape of the ability (wage) distribution, (ii) the elasticity of taxable income, and (iii) welfare weights. We estimate the ability distribution directly from the observed U.S. income distribution, apply elasticity estimates from existing literature, and incorporate welfare weights estimated in our paper.

Optimal Tax Formula

We assume that individuals are on a continuum of abilities (wage) w with densities $f(w)$ and cumulative distribution function $F(w)$. Individuals earn income $z = wl$, where l denotes labor supply. Taxes are denoted by $T(z)$, and consumption by $c = z - T(z)$. The planner observes only income z and labor supply l, but not abilities w. Individuals' utilities are given by

$$U(c, l) = u(c) + v(l) = \frac{c^{1-\gamma}}{1-\gamma} - \frac{l^{1+\frac{1}{E_L}}}{(1+\frac{1}{E_L})}, \tag{A3}$$

which are additively separable in consumption and labor. The utility function exhibits constant relative risk aversion (CRRA) in consumption, with the coefficient of relative risk aversion given by γ. Furthermore, it is isoelastic in labor, with E_L denoting the elasticity of earnings with respect to the retention rate $1 - T'(z)$. We denote the utility of an individual with wages w as $U(w)$.

The planner maximizes a lienar utilitarian social welfare function given by $W = \int U(\theta) f(\theta) d\theta$, where θ indexes wages in the integral. Following Saez (2001), the planner's first-order condition is given by

$$\frac{T'(z(w))}{1-T'(z(w))} = \left(\frac{1+E_L^U(w)}{E_L^C(w)}\right)\frac{u'(c(w))}{wf(w)}\int_w^\infty \frac{f(\theta)}{u'(c(\theta))}d\theta - (1-F(w))\frac{1}{p}, \tag{A4}$$

where $T'(z(w))$ is the optimal MTR at income level $z(w)$ at ability level w.

The term $u'(c(w))$ is the marginal utility of consumption or welfare weights. With CRRA utilities, $u'(c(w)) = c(w)^{-\gamma}$. Different estimates of welfare weights can be implemented using different values of γ. The progressivity of the optimal MTRs is increasing with the progressivity of the welfare weights.

The optimal MTRs are a function of $E_L^u(w)$ and $E_L^C(w)$, which are the average uncompensated and compensated elasticity of earnings with respect to the retention rate $1-T'(z)$. The elasticity of earnings is assumed to be driven by the substitution effect (people work less due to increased taxes) and assumes no income effects (people work more due to increased taxes). Thus, we assume that $E_L^U(w) = E_L^C(w) = E_L$. The optimal MTR is decreasing in the elasticity of earnings to reduce the extent to which taxes distort people's labor supply.

In Equation (A4), p is the marginal value of public funds. It measures the increase in social welfare obtained when the planner loosens the budget constraint. The cost in consumption terms of a marginal increase in utility for an individual with wage w is $\frac{1}{u'(c(w))}$. The cost of a marginal increase in average utility is $\int_0^\infty \frac{1}{u'(c(w))}f(w)dw$. The value to the planner of a marginal unit of public funds is the inverse of this cost, which is given by

$$p = \frac{1}{\int_0^\infty \frac{1}{u'(c(w))}f(w)dw} \tag{A5}$$

Estimating the Wage-Ability Distribution

We estimate the ability (wage) distribution from the current income distribution and use this exogenous ability distribution when we calibrate the tax formula in Equation (A4). Data on the income distribution is obtained from the Distributional National Accounts micro-files of Piketty et al. (2018). Each observation in the data corresponds to a tax unit.

Step 1: Use the NBER TAXSIM model to find the marginal tax rate for each tax unit. The tax rates are calculated based on the available information about the tax units, which include the number of dependents, the age of the primary filer, and marital status. Add a 5% state tax rate, a 2.9% tax rate for Medicare, and a 2.3% sales tax rate.
Step 2: Assuming that individuals have correctly optimized according to their utility function in Equation (A3), back out the resulting ability (w) of each tax unit.

Step 3: Create a smooth ability distribution using a Kernel density estimator with a bandwidth of \$5000. The smooth distribution has 50,000 observations.
Step 4: Replace the top 0.5% of the distribution with a Pareto distribution. The Pareto parameter is the value of the Pareto parameter $\alpha(z(w)) = \frac{z(w)\cdot f(z(w))}{1-F(z(w))}$ just before the top 0.5%.

Calibrating Optimal Income Taxes

We use an updating rule to find the fixed-point tax schedule, drawing from Mankiw et al. (2009) and Støstad & Cowell (2024). We assume an initial tax schedule. Given the tax schedule, we estimate individuals' labor supply and utilities. Given the utilities, we calculate the resulting optimal MTRs at each wage level using Equation (A4). We iterate on this process until an optimum is found. We check if the second-order condition holds at the optimum. This condition states that the pre-tax incomes are non-decreasing with wages.
Step 1: Start with an initial flat tax rate of 35%.
Step 2: Compute individuals' labor choices based on this tax rate, assuming that they have correctly optimized their utilities in Equation (A3). Computing the derivative of Equation (A3) with respect to l and setting it to 0 yields $l = (w \cdot (1 - T'(z)))^{E_L}$. Set $E_L = 0.25$, which is a mid-range estimate for the elasticity of taxable income (Saez et al. 2012).
Step 3: Based on the optimal labor choices computed in Step 2, calculate the optimal income choices $z = wl$ and the resulting utilities based on Equation (A3). Different values of γ in Equation (A3) lead to different estimates of welfare weights.
Step 4: Calculate the resulting optimal tax rate at each ability level based on Equation (A4).
Step 5: Repeat the previous steps until the tax rates converge to a fixed point.

F Additional Studies

We conducted two additional studies (Study 2 and Study 3), prior to conducting Study 1. All three studies are broadly similar, but with some important differences. First, in Study 1, the Social Architects are asked to decide between implementing a reform (r_l, $-r_h$) or maintaining the status quo (0, 0), while those in the additional studies are asked to decide between implementing a reform (\$500, -\$500) or implementing ($r_l + 500$, $-r_h - 500$). We hypothesize that the simplification of the task in Study 1 likely led to improvements in data quality. However, we are unable to test this because we did not include proxies of response quality in the additional studies. Second, Study 1 has six Recipients earning disposable incomes \$15,000, \$30,000, \$60,000, \$120,000, \$240,000, and \$480,000, while the additional studies have seven Recipients with disposable incomes \$8,000, \$35,000, \$70,000, \$100,000, \$170,000, \$250,000, and \$500,000. Third, Study 1 included proxies of response quality and randomizations, including the randomization of the starting decision and prompts to consider the consequences of choices, which were not included in the additional studies. Overall, Study 1 was designed to be simpler and included several additional checks of robustness. The complete set of instructions for Studies 2 and 3 can be found in Appendix Section H and Section I, respectively.

F.1 Study 2

Data Collection and Sample: In Study 2, we recruited participants in the role of Social Architects from the general population of the U.S. using the data collection provider Lucid. Participants first answer questions about their demographics and political affiliation. We define quotas for recruitment based on gender, age, education, individual income, and region. The quotas are designed to match the sample to the population of the U.S. Participants had to pass one attention check and two comprehension checks to continue with the study. We implemented the survey using Qualtrics. The data collection for Study 2 began on 8 December 2021 and lasted approximately two weeks. Our final sample includes 1965 participants.

Design: The welfare weights elicitation procedure in Study 2 is broadly similar to that in Study 1. In Study 2, Social Architects are randomly assigned to one of four treatments in a 2×2 design. The first treatment dimension tests if Social Architects' welfare weights are sensitive to the framing of the reforms. While Treatments Loss involve taking money away from the higher-income Recipient and giving money to the lower-income Recipient, Treatments Gain involve giving money to both Recipients in the pair. In Treatments Loss, each Recipient is given an initial endowment of \$1500, and a Social Architect decides between

the reforms (r_l, $-r_h$) and (\$500, $-$\$500). In Treatments Gain, the endowment is included in the reform amounts: a Social Architect decides between ($\$1500 + \$r_l, \$1500 - \$r_h$) and ($\$2000, \$1000$). The framing of the reform should not affect a Social Architect's assigned welfare weights since the welfare weights depend on Recipients' consumption, which is the same in the two treatments. The second treatment dimension tests if Social Architects' welfare weights are sensitive to the income of the Recipient common across the decision screens. In Treatments 70K, the Recipient common across the decision screens has an income of \$70,000. In contrast, in Treatments 500K, the Recipient common across the decisions screens has an income of \$500,000.

Results: Column (1) of Table A11 presents estimates from a median regression. The dependent variable is Social Architects' progressivity of the weights (ν). The explanatory variable includes treatment indicators, with the base category being Treatment Loss x 70K. We find that Social Architects have more progressive welfare weights in Treatment Gain $\times$ 70K (ν is 0.12 lower) relative to Treatment Loss $\times$ 70K, and this effect is statistically significant. We do not find an effect across treatments Loss x 500K and Gain x 500K

Table A11: Support for Redistribution and Mechanisms

	(1)	(2)
Gain x 70K	-0.125***	
	(0.038)	
Loss x 500K	0.203***	
	(0.038)	
Gain x 500K	0.203***	
	(0.038)	
Hypothetical		-0.307***
		(0.096)
Constant	-0.203***	-0.185***
	(0.027)	(0.068)
Observations	1965	997
Study:	Study 2	Study 3

Notes: The table presents coefficient estimates from a median regression. The dependent variable is the progressivity of the welfare weights (ν). The explanatory variables include treatment indicators. The regression in Column (1) uses data from Study 2 while the regression in Column (2) uses data from Study 3. Standard errors in parentheses.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

F.2 Study 3

Data Collection and Sample In Study 3, we recruited participants in the role of Social Architects from the data collection provider Prolific. The recruitment procedure is similar to the procedure used in Study 2. Participants were recruited based on three quotas available in Prolific: age, sex, and ethnicity. We implemented the survey using oTree (Chen et al. 2016). The data collection for Study 3 began on 14 December 2022 and lasted eight days. Our final sample includes 1992 participants.

Design: The welfare weights elicitation procedure in Study 3 is very similar to that in Study 2. In Study 3, Social Architects are randomly assigned to one of four treatments. The first two treatments test whether Social Architects' welfare weights are sensitive to the existence of real stakes, while the last two treatments focus on the role of self-interest motives. Here, we only discuss the results on the role of stakes. In Treatment Real, Social Architects make real decisions regarding real Recipients, while in Treatment Hypothetical, Social Architects make hypothetical decisions regarding hypothetical Recipients.

Results: Column (2) of Table A11 presents estimates from a median regression. The dependent variable is Social Architects' progressivity of the weights (ν). The explanatory variable is an indicator variable taking a value of 1 if a Social Architect is in Treatment Hypothetical and 0 if the Social Architect is in Treatment Real. We find that the welfare weights are more progressive with hypothetical stakes (ν is 0.30 lower) than with real stakes. This result is consistent with an explanation of Social Architects considering the trade-offs more carefully in the latter.

G Instructions - Study 1

G.1 Wave 1

Bold text, underlining, tables, etc. appear as in the original screen.

Screen Break

[Consent screen]

Introduction
Welcome! This study is conducted by Unidistance Suisse, Switzerland and WZB, Germany. Our goal is to understand the views of U.S. residents on various topics. By carefully completing this survey, you are helping us to understand these views.

Requirements
To participate in this study, you must be a U.S. resident and at least 18 years old.

Time required
This study will take around **12 minutes**.

Compensation
You will receive **$2.5** for completing the study. The payment will be made in the next few days.

Checks
Our survey includes attention checks to test whether participants take our survey carefully. Additionally, we have implemented measures to ensure that participants do not use AI assistance during the surveys. Participants must complete the survey independently, without the help of AI tools. Participants who fail these checks cannot proceed with the survey, and will be asked to return the survey.

Follow-up study
You may be contacted for a follow-up study approximately four weeks from now. We will notify you of the study via Prolific. Your participation in the follow-up is very important to us.

Confidentiality

Your answers will remain anonymous and will be used for scientific purposes only. Strict confidentiality is guaranteed, and your identity can never be associated with your answers.

Voluntary participation
Participation in this study is voluntary. You may withdraw from the study at any time.

Questions about the survey
If you have questions about this study or your rights, please get in touch with us at krishna.srinivasan@unidistance.ch

Consent
I have received the above information and I am willing to participate in the study.

[Yes; No]

What is your Prolific ID?

Screen Break

[Screen shown if participant does not provide consent]

End of survey

You did not give your consent to continue with the study.

Please close this survey and return your submission on Prolific by selecting the "Stop without completing" button.

Screen Break

[Block 1: Background Questions]

What is your gender?

[Female; Male; Non-binary; Prefer not to say]

How old are you?

[18 years old - 24 years old; 25 years old - 34 years old; 35 years old - 44 years old; 45 years old - 54 years old; 55 years old - 64 years old; 65 years old or above]

In which state do you currently reside?

[Alabama; ...; Wyoming; I do not reside in the U.S.]

What is the highest level of education you have completed?

[Primary education or less; Some high school; High school degree/GED; Some college; 2-year college degree; 4-year college degree; Master's degree; Doctoral degree; Professional degree (e.g., JD, MD, MBA)]

The next question is about your **total individual income**, before taxes, last year. This figure should include income from all sources, including salaries, wages, pensions, social security, dividends, interest, and all other income.

What was your total individual income, before taxes, last year?

[$29,999 and below; $30,000 to $59,999; $60,000 to $99,999; $100,000 to $149,999; $150,000 and above]

——— Screen Break ———

[Displayed if $29,999 and below is chosen]

You have reported that your total individual income, before taxes, last year was $29,999 and below.

[Displayed if $30,000 to $59,999 is chosen]

You have reported that your total individual income, before taxes, last year was $30,000 to $59,999.

[Displayed if $60,000 to $99,999 is chosen]

You have reported that your total individual income, before taxes, last year was $60,000 to $99,999.

[Displayed if $100,000 to $149,999 is chosen]

You have reported that your total individual income, before taxes, last year was $100,000 to $149,999.

[Displayed if $150,000 and above is chosen]

You have reported that your total individual income, before taxes, last year was $150,000 and above.

[Displayed in all cases]

Could you provide your best guess of what your total individual income, before taxes, last year was?

Screen Break

In politics, as of today, do you consider yourself a Republican, a Democrat or an independent?

[Republican; Democrat; Independent]

Screen Break

[Screen shown if participant does not reside in the U.S.]

End of survey

Unfortunately, you do not fulfill the requirements of this study since you do not reside in the U.S.

Thank you for your time.

Please close this survey and return your submission on Prolific by selecting the "Stop without completing" button.

Screen Break

[Attention check]

In surveys like ours, some participants do not carefully read the questions. This means that there are a lot of random answers that can compromise the results of research studies. To show that you read our questions carefully, please choose "Not at all interested" below:

[Extremely interested; Very interested; A little bit interested; Almost not interested; Not at all interested]

Screen Break

[Screen shown if participant failed the attention check]

End of survey

Unfortunately, you failed the attention check.

For this reason, you cannot continue the study and will not receive a payment.

Please close this survey and return your submission on Prolific by selecting the "Stop without completing" button.

Screen Break

[**Block 2: Eliciting Welfare Weights**]

Instructions

In this section, **you will be asked to decide whether you want to change the incomes of six real individuals in society.** These real individuals will be recruited from the U.S. general population. They are above the age of 18 and are U.S. citizens. As we will explain below, your decisions may have real consequences for two of these individuals.

You will be presented with several questions. In each question, you will be presented with two individuals and learn their disposable incomes. **Disposable income is defined as income after all taxes have been paid and transfers have been received (including federal and state taxes and transfers).**

In each question, you will be presented with a proposed change to the incomes of the individuals, and will be asked to indicate whether or not you prefer to implement this change. **If you prefer to implement the change, the income of the lower-income individual in the pair will increase, and the income of the higher-income individual in the pair will decrease by the amounts proposed in the change.** If you prefer not to implement the change, the incomes of the two individuals will remain unchanged. We will describe below how these changes are implemented.

Example

Here is an example of a question that you will see in the survey:

	Person #3	**Person #6**
Annual disposable income	\$60,000	\$120,000
Proposed change	\$500	-\$500

Please make your decision:

- ◦ I prefer to implement the change
- ◦ I prefer not to implement the change

In this example, there are two individuals: Person #3 with an income of \$60,000 and Person #6 with an income of \$120,000.

The proposed change involves increasing the income of Person #3 by $500 and decreasing the income of Person #6 by $500. If you prefer to implement the change, the final incomes of the individuals are Person #3: $60,500 and Person #6: $119,500. If you prefer not to implement the change, the incomes of the individuals remain unchanged.

Comparisons

You will face several questions like the one above, with the amounts in the proposed change varying across the questions. You will face several comparisons, with the income of the individuals varying across them. The following table indicates the comparisons.

Comparison	Annual disposable income of individuals
1	$15,000 vs. $60,000
2	$30,000 vs. $60,000
3	$60,000 vs. $120,000
4	$60,000 vs. $240,000
5	$60,000 vs. $480,000

Incentives and Implementation

At the end of the study, one participant will be randomly selected. If you are selected, one question will be randomly chosen from either this survey or the follow-up survey, and your choice on that question will be implemented. The two individuals involved in the selected question will then be affected by your decision. **Thus, if you are randomly selected, one of your choices may have real consequences for two other individuals.**

If you are selected, a $1,000 bonus will be transferred to the two individuals affected by your choice. The disposable incomes shown above for each person already include this $1,000 bonus, under the assumption that they receive it. The amounts specified in your proposed change will be added to or subtracted from that bonus.

[If Treatment Prompted] For the first question in each comparison, you will be presented with two distributions of income for the pairs of individuals, which reflect the consequences of the two choices you were presented with. You will then be asked to decide which distribution you

prefer. This is meant to help you consider the consequences of your choice before proceeding.

Please answer the following questions to show that you have understood the instructions. You can read the instructions above again if needed.

How many individuals will you make decisions regarding?

[Three; Six]

We will present you with the incomes of several individuals. What type of income will we present to you?

[Pre-tax income, Disposable income]

Please state True or False: "If you are randomly selected, one of your choices may have real consequences for two other individuals."

[True; False]

Screen Break

If a participant fails any of the three comprehension checks, they are taken back to the instruction screen and informed which checks they failed. The error message reads: "In your first try, you answered the [] question[s] incorrectly. Please try again." Any check answered correctly is locked, while those failed require a new response. Participants must pass the remaining checks before proceeding. On subsequent attempts, if a participant fails a check, they receive the following error message: "In your previous try, you answered the [] question[s] incorrectly. Please try again."

Screen Break

[We present the proposed change in each question as ($\$r_l$, $-\$r_h$), where $\$r_l$ and $-\$r_h$ refer to the amounts accrued to the lower-income Recipient and high-income Recipients in the pair, respectively. Participants are randomized into one of two treatments that vary the amounts in the first question to be either ($300, −$700) or ($700, −$300). Below we describe the questions in both treatments.]

[C1Q1: Comparison 1 Question]

Comparison 1/5, Question 1

[If first decision is ($300, −$700)]

Please consider each question carefully because if you are selected, one of your choices may have real consequences for two real individuals.

	Person #1	**Person #3**
Annual disposable income	$15,000	$60,000
Proposed change	$300	-$700

Please make your decision:

- ◦ I prefer to implement the change
- ◦ I prefer not to implement the change

If you prefer to implement the change, the final incomes of the individuals are Person #1: $15,300 and Person #3: $59,300. If you prefer not to implement the change, the incomes of individuals remain unchanged.

[If first decision is ($700, −$300)]

Please consider each question carefully because if you are selected, one of your choices may have real consequences for two real individuals.

	Person #1	**Person #3**
Annual disposable income	$15,000	$60,000
Proposed change	$700	-$300

Please make your decision:

- ◦ I prefer to implement the change
- ◦ I prefer not to implement the change

If you prefer to implement the change, the final incomes of the individuals are Person #1: $15,700 and Person #3: $59,700. If you prefer not to implement the change, the incomes of individuals remain unchanged.

Screen Break

[If first decision is ($300, −$700)]

[C1Q2.1: If change implemented in C1Q1, choose whether to implement ($200, −$800)]

[C1Q2.2: If change not implemented in C1Q1, choose whether to implement ($600, −$400)]

[If first decision is ($700, −$300)]

[C1Q2.1: If change implemented in C1Q1, choose whether to implement ($400, −$600)]

[C1Q2.2: If change not implemented in C1Q1, choose whether to implement ($800, −$200)]

Screen Break

[If first decision is ($300, −$700)]

[C1Q3.1: If change implemented in C1Q2.1, choose whether to implement ($100, −$900)]

[C1Q3.2: If change not implemented in C1Q2.1, choose whether to implement ($250, −$750)]

[C1Q3.3: If change implemented in C1Q2.2, choose whether to implement ($400, −$600)]

[C1Q3.4: If change not implemented in C1Q2.2, choose whether to implement ($800, −$200)]

[If first decision is ($700, −$300)]

[C1Q3.1: If change implemented in C1Q2.1, choose whether to implement ($200, −$800)]

[C1Q3.2: If change not implemented in C1Q2.1, choose whether to implement ($600, −$400)]

[C1Q3.3: If change implemented in C1Q2.2, choose whether to implement ($750, −$250)]

[C1Q3.4: If change not implemented in C1Q2.2, choose whether to implement ($900, −$100)]

Screen Break

[If first decision is ($300, −$700)]

[C1Q4.1: If change implemented in C1Q3.1, choose whether to implement ($50, −$950)]

[C1Q4.2: If change not implemented in C1Q3.1, choose whether to implement ($150, −$850)]

[If change implemented in C1Q3.2, participant indifferent between ($225, −$775)]

[If change not implemented in C1Q3.2, participant indifferent between ($275, −$725)]

[C1Q4.3: If change implemented in C1Q3.3, choose whether to implement ($350, −$650)]

[C1Q4.4: If change not implemented in C1Q3.3, choose whether to implement ($500, −$500)]

[C1Q4.5: If change implemented in C1Q3.4, choose whether to implement ($700, −$300)]

[C1Q4.6: If change not implemented in C1Q3.4, choose whether to implement ($900, −$100)]

[If first decision is ($700, −$300)]

[C1Q4.1: If change implemented in C1Q3.1, choose whether to implement ($100, −$900)]

[C1Q4.2: If change not implemented in C1Q3.1, choose whether to implement ($300, −$700)]

[C1Q4.3: If change implemented in C1Q3.2, choose whether to implement ($500, −$500)]

[C1Q4.4: If change not implemented in C1Q3.2, choose whether to implement ($650, −$350)]

[If change implemented in C1Q3.3, participant indifferent between ($725, −$275)]

[If change not implemented in C1Q3.3, participant indifferent between ($775, −$225)]

[C1Q4.5: If change implemented in C1Q3.4, choose whether to implement ($850, −$150)]

[C1Q4.6: If change not implemented in C1Q3.4, choose whether to implement ($950, −$50)]

Screen Break

[If first decision is ($300, −$700)]

[If change implemented in C1Q4.1, participant indifferent between ($25, −$975)]

[If change not implemented in C1Q4.1, participant indifferent between ($75, −$925)]

[If change implemented in C1Q4.2, participant indifferent between ($125, −$875)]

[If change not implemented in C1Q4.2, participant indifferent between ($175, −$825)]

[If change implemented in C1Q4.3, participant indifferent between ($325, −$675)]

[If change not implemented in C1Q4.3, participant indifferent between ($375, −$625)]

[C1Q5.1: If change implemented in C1Q4.4, choose whether to implement ($450, −$550)]

[C1Q5.2: If change not implemented in C1Q4.4, choose whether to implement ($550, −$450)]

[C1Q5.3: If change implemented in C1Q4.5, choose whether to implement ($650, −$350)]

[C1Q5.4: If change not implemented in C1Q4.5, choose whether to implement ($750, −$250)]

[C1Q5.5: If change implemented in C1Q4.6, choose whether to implement ($850, −$150)]

[C1Q5.6: If change not implemented in C1Q4.6, choose whether to implement ($950, −$50)]

[If first decision is ($700, −$300)]

[C1Q5.1: If change implemented in C1Q4.1, choose whether to implement ($50, −$950)]

[C1Q5.2: If change not implemented in C1Q4.1, choose whether to implement ($150, −$850)]

[C1Q5.3: If change implemented in C1Q4.2, choose whether to implement ($250, −$750)]

[C1Q5.4: If change not implemented in C1Q4.2, choose whether to implement ($350, −$650)]

[C1Q5.5: If change implemented in C1Q4.3, choose whether to implement ($450, −$550)]

[C1Q5.6: If change not implemented in C1Q4.3, choose whether to implement ($550, −$450)]

[If change implemented in C1Q4.4, participant indifferent between ($625, −$375)]

[If change not implemented in C1Q4.4, participant indifferent between ($675, −$325)]

[If change implemented in C1Q4.5, participant indifferent between ($825, −$175)]

[If change not implemented in C1Q4.5, participant indifferent between ($875, −$125)]

[If change implemented in C1Q4.6, participant indifferent between ($925, −$75)]

[If change not implemented in C1Q4.6, participant indifferent between ($975, −$25)]

Screen Break

[If first decision is ($300, −$700)]

[If Yes chosen in C1Q5.1, participant indifferent between ($425, −$575)]

[If No chosen in C1Q5.1, participant indifferent between ($475, −$525)]

[If Yes chosen in C1Q5.2, participant indifferent between ($525, −$475)]

[If No chosen in C1Q5.2, participant indifferent between ($575, −$425)]

[If Yes chosen in C1Q5.3, participant indifferent between ($625, −$375)]

[If No chosen in C1Q5.3, participant indifferent between ($675, −$325)]

[If Yes chosen in C1Q5.4, participant indifferent between ($725, −$275)]

[If No chosen in C1Q5.4, participant indifferent between ($775, −$225)]

[If change implemented in C1Q5.5, participant indifferent between ($825, −$175)]

[If No chosen in C1Q5.5, participant indifferent between ($875, −$125)]

[If Yes chosen in C1Q5.6, participant indifferent between ($925, −$75)]

[If No chosen in C1Q5.6, participant indifferent between ($975, −$25)]

[If first decision is ($700, −$300)]

[If Yes chosen in C1Q5.1, participant indifferent between ($25, −$975)]

[If No chosen in C1Q5.1, participant indifferent between ($75, −$925)]

[If Yes chosen in C1Q5.2, participant indifferent between ($125, −$875)]

[If No chosen in C1Q5.2, participant indifferent between ($175, −$825)]

[If change implemented in C1Q5.3, participant indifferent between ($225, −$775)]

[If change not implemented in C1Q5.3, participant indifferent between ($275, −$725)]

[If change implemented in C1Q5.4, participant indifferent between ($325, −$675)]

[If change not implemented in C1Q5.4, participant indifferent between ($375, −$625)]

[If change implemented in C1Q5.5, participant indifferent between ($425, −$575)]

[If change not implemented in C1Q5.5, participant indifferent between ($475, −$525)]

[If change implemented in C1Q5.6, participant indifferent between ($525, −$475)]

[If change not implemented in C1Q5.6, participant indifferent between ($575, −$425)]

Screen Break

[Comparisons 2-5 are identical to Decision Screen 1, with the below exceptions]

[In Comparison 2, the Recipients are Person #2: $30,000 and Person #3: $60,000]

[In Comparison 3, the Recipients are Person #3: $60,000 and Person #4: $120,000]

[In Comparison 4, the Recipients are Person #3: $60,000 and Person #5: $240,000]

[In Comparison 5, the Recipients are Person #3: $60,000 and Person #6: $480,000]

Screen Break

[Consistency check]

In this final comparison, all participants will see a pair of individuals that they have encountered before. It is very important for us that you once again consider each question carefully. Thank you very much.

[In this Decision Screen, the Recipients are Person #3: $60,000 and Person #4: $120,000]

Screen Break

[Treatments]

[Participants are randomized to various treatments. The first treatment dimension varies the order of the Comparisons. For half the participants, the order is as presented above, while

for the other half, the order of the first five Comparisons is reversed. The sixth Comparison is identical across the two treatments. The second treatment dimension varies whether the Recipient common across the Comparisons earns an income of \$60,000 (as shown above) or \$120,000. The third treatment dimension varies whether the low-income Recipient is presented on the left (as shown above) or the right of the screen.

The fourth treatment dimension varies whether participants are prompted to consider the consequences of their decisions prior to proceeding. Participants are asked to choose between two final income distributions that reformulate the consequences of their decision. Figure A1 presents a screenshot. If their preference on whether to implement the reform and their preferred final income distribution are consistent, they can proceed with the survey. If there is an inconsistency, they see the following error message: "Your choice on whether to implement the change (above) and your preferred distribution of final incomes (below) contradict each other. Please make your choice and indicate your preferred distribution of final incomes once again.'

The fifth treatment varies the questions in the staircase (explained above), with the reform in the first decision being either (\$300, −\$700) or (\$700, −\$300). The first four treatment dimensions have implications for the instructions and decisions, while the fifth dimension only has implications for the decisions.]

Screen Break

How confident are you that the choices you made in the previous screens reflect what you really think?

[Not at all; Very little; Little; Somewhat; Very much]

Screen Break

[**Block 3: Support for Redistribution**]

[The order of the two questions is counterbalanced across participants.]

In the following screens, we would like to ask you some general questions about your views on society. Your opinion and thoughts are important to us.

Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor.

Here is a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel?

[1: Government should do something to reduce income differences between rich and poor; 2; 3; 4; 5:; 6:; 7: Government should not concern itself with income differences]

Screen Break

Consider the current **disposable incomes** of individuals in society. Disposable income is defined as **income after all taxes have been paid and transfers have been received.**

Do you think that, given the current disposable incomes of individuals in society, incomes should be further redistributed or should they remain as they are?

Please provide your answer on a scale from -2 to $+2$.

- A -2 means that income should be further redistributed by taking from lower/middle-income individuals and giving to higher-income individuals.
- A $+2$ means that income should be further redistributed by taking from higher-income individuals and giving to lower/middle-income individuals.

Given the current disposable incomes of individuals in society ...

[-2: Incomes should be further redistributed by taking from lower/middle-income individuals and giving to higher-income individuals; -1:; +0: Incomes should remain as they are; +1:; +2: Income should be further redistributed by taking from higher-income individuals and giving to lower/middle-income individuals].

Screen Break

[**Block 4: Knowledge**]

The next set of questions is about the income tax system in the United States. In order for your answers to be most helpful to us, it is really important that you provide your best guesses to these questions. Although you may find some questions difficult, it is very important for our research that you try your best. Thank you very much!

In 2024, individuals (single filers) with income over $609,350 were in the top federal personal income tax bracket.

Out of every 100 households in the U.S., how many are in the top federal personal income tax bracket?

[slider 0-100]

What is the marginal income tax rate applied to incomes at the top federal personal income tax bracket?

[slider 0%-100%]

What share of their total income do people in the top federal personal income tax bracket pay in taxes?

[slider 0-100]

Out of every 100 U.S. households, how many pay no federal income taxes?

[slider 0-100]

Imagine a middle class household that is right at the middle of the income distribution, such that half of all households in the U.S. earn more than this household and half earn less.

What share of their income do you think such a household pays in federal income taxes?

[slider 0-100]

Out of every 100 individuals in the U.S., how many earn a disposable income below $15,000?

[slider 0-100]

Screen Break

[**Block 5: Mechanisms**]

[The order of the questions is randomized across participants]

Please answer the following last set of questions.

If the federal personal income tax rate were to increase for the richest people in the economy, to what extent would it encourage them to work less?

[A great deal; A lot; A moderate amount; A little; None at all]

Do you think that increasing income taxes on high-income households would hurt economic activity, not have an effect on economic activity, or help economic activity in the U.S.?

[Hurt economic activity in the U.S.; Not have an effect on economic activity in the U.S.; Help economic activity in the U.S.]

Typically, when the top federal income tax rate on high earners is cut, do you think that the lower class and working class mostly win or mostly lose from this change?

[Mostly lose; Neither lose nor win; Mostly win]

Consider the current disposable incomes of individuals in society, defined as income after all taxes have been paid and transfers have been received. Do you think that the current distribution of disposable incomes in society is unfair or fair?

[Very unfair; Somewhat unfair; Neither unfair nor fair; Somewhat fair; Very fair]

How much of the time do you think you can trust the federal government to do what is right?

[Never; Only some of the time; Most of the time; Just about always]

Some people think the government is trying to do too many things that should be left to individuals and businesses. Others think that the government should do more to solve our country's problems. Which comes closer to your own view?

[Government is doing too much; Government is doing just the right amount; Government should do more]

Screen Break

End of survey

Thank you for your time!

We will pay you your $2.5 participation payment in the following days.

Please click the following link to finish the survey:

[link]

G.2 Wave 2

[Consent screen]

Introduction
Welcome! This study is conducted by Unidistance, Switzerland and WZB, Germany. You previously participated in our survey. We invite you to participate in our follow-up survey.

Time required
This study will take around **5 minutes**.

Compensation
You will receive **$1** for completing the study. The payment will be made in the next few days.

Checks
Our survey includes attention checks to test whether participants take our survey carefully. Additionally, we have implemented measures to ensure that participants do not use AI assistance during the surveys. Participants must complete the survey independently, without the help of AI tools. Participants who fail these checks cannot proceed with the survey, and will be asked to return the survey.

Confidentiality
Your answers will remain anonymous and will be used for scientific purposes only. Strict confidentiality is guaranteed, and your identity can never be associated with your answers.

Voluntary participation
Participation in this study is voluntary. You may withdraw from the study at any time.

Questions about the survey
If you have questions about this study or your rights, please get in touch with us at krishna.srinivasan@unidistance.ch

Consent
I have received the above information and I am willing to participate in the study.

[Yes; No]

What is your Prolific ID?

Screen Break

[Screen shown if participant does not provide consent]

End of survey

You did not give your consent to continue with the study.

Please close this survey and return your submission on Prolific by selecting the "Stop without completing" button.

Screen Break

[Attention check]

In surveys like ours, some participants do not carefully read the questions. This means that there are a lot of random answers that can compromise the results of research studies. To show that you read our questions carefully, please choose "Not at all interested" below:

[Extremely interested; Very interested; A little bit interested; Almost not interested; Not at all interested]

Screen Break

[Screen shown if participant failed the attention check]

End of survey

Unfortunately, you failed the attention check.

For this reason, you cannot continue the study and will not receive a payment.

Please close this survey and return your submission on Prolific by selecting the "Stop without completing" button.

Screen Break

In the next part, we will present you with several decisions. **These decisions may look very similar to the ones you faced in our previous survey.**

However, it is very important for us that you consider these questions carefully.

Screen Break

[**Block 1: Eliciting Welfare Weights**]

[Participants are presented with questions designed to elicit their welfare weights. The questions are identical to those in Wave 1. Each participant is assigned to the same treatment group in both waves. Only one sentence in the instructions for Wave 2 differs from those in Wave 1. The sentence in Wave 1 "If you are selected, we will randomly select one question (from this survey or the follow-up survey) and implement your choice on this question." is replaced in Wave 2 with "If you are selected, we will randomly select one question (from this survey or the previous survey) and implement your choice on this question."]

Screen Break

[**Block 2: Support for Redistribution**]

[Participants are presented with both questions about preferences for redistribution.]

H Instructions - Study 2

Bold text, underlining, tables, etc., appear as on the original screens.

H.1 Treatment Loss x 70K

[Consent screen]

Introduction
Welcome to this research study. We appreciate your participation. We are a non-partisan group of researchers from University of Zurich and Erasmus University Rotterdam. This study contains real choices and questions regarding your demographic characteristics. No matter what your political views are, by completing this survey you are contributing to our knowledge as a society.

Time required
Approximately **10 minutes.** You will have a maximum of one hour to finish the survey after starting it.

Requirements
You must be a **U.S. resident** to participate in this study. You must also be above the age of 18. The survey contains attention checks. You must pass these check in order to proceed with the survey.

Confidentiality
All data obtained from you will be used for research purposes only. Data will be anonymized immediately after collection. Researchers will at no point have access to any information that could be used to personally identify you.

Voluntary participation
It is voluntary to participate in the project, and you can at any time choose to withdraw your consent without stating any reason.

Questions about the Survey
If you have questions about this study or your rights, please get in touch with us at Krishna.srinivasan@econ.uzh.ch

Consent

I have received the above information about the project and am willing to participate.

[Yes; No]

Screen Break

[Screen shown if participant does not provide consent]

End of survey

You did not give your consent to continue with the study.

Thank you for your time.

You will be automatically redirected in 5 seconds.

Screen Break

[Block 1: Background Questions]

What is your sex?

[Male; Female]

How old are you?

[18 years old - 34 years old; 35 years old - 44 years old; 45 years old - 54 years old; 55 years old - 64 years old; Above 65 years old]

In which state do you currently reside?

[Northeast (ME, NH, VT, MA, CT, RI, NY, PA, NJ); Midwest (OH, MI, IN, WI, IL, MN, IA, MO, ND, SD, NE, KS); South (DE, MD, DC, VA, WV, NC, SC, GA, FL, KY, TN, AL, MS, AR, LA, OK, TX); Pacific (MT, WY, CO, NM, ID, UT, AZ, NV, WA, OR, CA, AK, HI); I do not reside in the US]

What is the highest level of education you have completed?

[Less than High School; High School/GED; Some College; Associate's Degree; Bachelor's degree; Master's degree; Doctoral or Profession Degree (PhD, ED.D, JD, DVM, DO, MD, DDS, or similar)]

As of today, do you consider yourself a Republican, a Democrat, or an Independent?

[Republican; Democrat; Independent]

The next question is about your **total individual income in 2020 before taxes**. This figure should include income from all sources, including salaries, wages, pensions, Social Security, dividends, interest, and all other income. What was your total individual income (USD) in 2020?

[$29,999 and below; $30,000 to $59,999; $60,000 to $99,999; $100,000 to $149,999; $150,00 and above

[Displayed if $29,999 and below is chosen]

You have reported that your total individual income in 2020 before taxes was $29,999 and below.

[Displayed if $30,000 to $59,999 is chosen]

You have reported that your total individual income in 2020 before taxes was between $30,000 and $59,999.

[Displayed if $60,000 to $99,999 is chosen]

You have reported that your total individual income in 2020 before taxes was between $60,000 and $99,999.

[Displayed if $100,000 to $149,999 is chosen]

You have reported that your total individual income in 2020 before taxes was between $100,000 and $149,999.

[Displayed if $150,000 and above is chosen]

You have reported that your total individual income in 2020 before taxes was above $150,000.

[Displayed in all cases]

Could you provide your best guess of what your **total individual income** was?

Screen Break

[If quotas are full]

End of survey

Unfortunately, we already have the number of participants needed for this study.

Thank you for your time.

You will be automatically redirected in 5 seconds.

Screen Break

[If a participant does not reside in the U.S.]

End of survey

Unfortunately, you do not fulfil the requirements of this study since you do not reside in the U.S.

Thank you for your time.

You will be automatically redirected in 5 seconds.

Screen Break

[Attention check]

In surveys like ours, some participants do not carefully read the questions. This means that there are a lot of random answers that can compromise the results of research studies. To show that you read our questions carefully, please choose both "Extremely interested" and "Not at all interested" below:

[Extremely interested; Very interested; A little bit interested; Almost not interested; Not at all interested]

Screen Break

[Screen shown if participant failed the attention check]

End of survey

Sorry, you failed the attention check. You were supposed to select both "Extremely interested" and "Not at all interested."

You cannot continue with the study.

Thank you for your time.

You will be automatically redirected in 5 seconds.

Screen Break

[Block 2: Eliciting Welfare Weights]

[Instructions screen]

Instructions

In this study, you will make several choices involving **seven real people**. These people will be selected at random from a survey panel and will not participate in the same survey as you. These people are above the age of 18 and are U.S. citizens. The incomes of the seven people are as follows:

Person	**After-tax annual income**
Person A	$8000
Person B	$35,000
Person C	$70,000
Person D	$100,000
Person E	$170,000
Person F	$250,000
Person G	$500,000

Here is an example of a question that you will see in the survey:

	Person C	Person G
After-tax annual income	**$70,000**	**$500,000**

Question 2/4: Please choose your preferred alternative

Person C: +$750 Person G: -$1250	Person C: +$500 Person G: -$500

In this question, if you choose the option on the left, then $1250 will be taken away from Person G and $750 will be given to Person C. If you choose the option on the right, then $500 will be taken away from Person G and $500 will be given to Person C.

If you choose the option on the left, the final incomes of the two people **(including an initial $1500 bonus)** will be Person C: $72,250 and Person G: $500,250. If you choose the option on the right, the final incomes of the two people (including an initial $1500 bonus) will be Person C: $72,000 and Person G: $501,000.

You will face four questions like the one you saw above in each "decision screen." **Overall, you will face six decision screens with four questions in each.** In each question, you will see a different amount in the option on the left. In each decision screen, you will see a different pair of people.

There is a chance that you may be randomly selected in this study. If you are randomly selected, your choice on one randomly selected question on one randomly selected decision screen will be implemented. **This means that if you are randomly selected, one of your choices will have real consequences for two other people.** The final bonus of these two people will be transferred to them at the end of the study.

Please answer the following questions to demonstrate that you have understood the instructions. You can read the instructions above again if you feel the need to.

Please state True or False: "In this study, you will make several choices involving seven real people."

[True; False]

Please state True or False: "If you are randomly selected, one of your choices will have real consequences for two other people."

[True; False]

(You will be allowed to move to the next screen in 30 seconds)

Screen Break

[If a participant fails the comprehension check]

End of survey

The correct answers were "True" and "True". You answered incorrectly.

You cannot continue with the study.

Thank you for your time.

You will be automatically redirected in 5 seconds.

Screen Break

[C1Q1: Comparison 1 Question 1]

Decision Screen 1/6

Please consider each question carefully because if you are selected, one of your choices will have real consequences for two other persons.

	Person A	Person G
After-tax annual income	**$8,000**	**$500,000**

Question 1/4: Please choose your preferred alternative

Person A: +$1000 Person G: -$1000	Person A: +$500 Person G: -$500

Screen Break

[All questions hereafter in Decision Screen 1 look like C1Q1]

[C1Q2.1: If $(500, -500)$ chosen in C1Q1, choose between $(1250, -750)$ and $(500, -500)$]

[C1Q2.2: If $(1000, -1000)$ chosen in C1Q1, choose between $(750, -1250)$ and $(500, -500)$]

Screen Break

[C1Q3.1: If $(500, -500)$ chosen in C1Q2.1, choose between $(1375, -625)$ and $(500, -500)$]

[C1Q3.2: If $(1250, -750)$ chosen in C1Q2.1, choose between $(1125, -875)$ and $(500, -500)$]

[C1Q3.3: If $(500, -500)$ chosen in C1Q2.2, choose between $(875, -1125)$ and $(500, -500)$]

[C1Q3.4: If $(750, -1250)$ chosen in C1Q2.2, choose between $(625, -1375)$ and $(500, -500)$]

Screen Break

[C1Q4.1: If $(500, -500)$ chosen in C1Q3.1, choose between $(1450, -550)$ and $(500, -500)$]

[C1Q4.2: If $(1375, -625)$ chosen in C1Q3.1, choose between $(1300, -700)$ and $(500, -500)$]

[C1Q4.3: If $(500, -500)$ chosen in C1Q3.2, choose between $(1200, -800)$ and $(500, -500)$]

[C1Q4.4: If $(1125, -875)$ chosen in C1Q3.2, choose between $(1050, -950)$ and $(500, -500)$]

[C1Q4.5: If $(500, -500)$ chosen in C1Q3.3, choose between $(950, -1050)$ and $(500, -500)$]

[C1Q4.6: If $(875, -1125)$ chosen in C1Q3.3, choose between $(800, -1200)$ and $(500, -500)$]

[C1Q4.7: If $(500, -500)$ chosen in C1Q3.4, choose between $(700, -1300)$ and $(500, -500)$]

[C1Q4.8: If $(625, -1375)$ chosen in C1Q3.4, choose between $(550, -1450)$ and $(500, -500)$]

Screen Break

[Comparisons 2-6 are identical to Decision Screen 1, with the following exceptions]

[In Comparison 2, the Recipients are: Person B: $35,000 and C: $70,000]

[In Comparison 3, the Recipients are Person C: $70,000 and D: $100,000]

[In Comparison 4, the Recipients are Person C: $70,000 and. E: $170,000]

[In Comparison 5, the Recipients are Person C: $70,000 and F: $250,000]

[In Comparison 6, the Recipients are Person C: $70,000 vs. G: $500,000]

[For half the participants, the order of the Comparisons is reversed. The pair of Recipients are as follows: C: $70,000 vs. G: $500,000 (Comparison 1), C: $70,000 vs. F: $250,000 (Comparison 2), C: $70,000 vs. E: $170,000 (Comparison 3), C: $70,000 vs. D: $100,000 (Comparison 4), B: $35,000 vs. C: $70,000 (Comparison 5), and A: $8,000 vs. C: $70,000 (Comparison 6).]

Screen Break

[Block 3: Support for Redistribution]

[The order of the two questions is counterbalanced across participants.]

We have some final questions. It is important for us that you answer them carefully.

The top income tax category in 2020 includes those with an annual individual income of over $518,400. Do you think that income taxes levied on these people in the top income category should be increased, stay the same, or decreased?

[1: Increased a lot; 2:; 3:; 4: Stay the same; 5; 6; 7: Decreased a lot]

Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor.

Here is a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel?

[1: Government should do something to reduce income differences between rich and poor; 2; 3; 4; 5:; 6:; 7: Government should not concern itself with income differences]

Screen Break

End of survey

Thank you for your time!

You will be automatically redirected in 5 seconds.

H.2 Treatment Loss x 500K

[All questions are identical to those in Treatment Loss x 70K. The incomes of the Recipients in the comparisons are as follows:]

[In Comparison 2, the Recipients are: Person A: $8,000 vs. G: $500,000]

[In Comparison 2, the Recipients are: Person B: $35,000 vs. G: $500,000]

[In Comparison 3, the Recipients are Person C: $70,000 vs. G: $500,000]

[In Comparison 4, the Recipients are Person D: $100,000 vs. G: $500,000]

[In Comparison 5, the Recipients are Person E: $170,000 vs. G: $500,000]

[In Comparison 6, the Recipients are Person F: $250,000 vs. G: $500,000]

[For half the participants, the order of the Comparisons is reversed]

H.3 Treatment Gain x 70K

[All questions, with the exceptions of those listed below, are identical to those in Treatment Loss x 70K]

[Instructions screen]

Instructions

In this study, you will make several choices involving **seven real people**. These people will be selected at random from a survey panel and will not participate in the same survey as you. These people are above the age of 18 and are U.S. citizens. The incomes of the seven people are as follows:

Person	After-tax annual income
Person A	$8000
Person B	$35,000
Person C	$70,000
Person D	$100,000
Person E	$170,000
Person F	$250,000
Person G	$500,000

Here is an example of a question that you will see in the survey:

	Person C	Person G
After-tax annual income	**$70,000**	**$500,000**

Question 2/4: Please choose your preferred alternative

Person C: +$2250 Person G: +$250	Person C: +$2000 Person G: +$1000

In this question, if you choose the option on the left, then $250 will be given to Person G and $2250 will be given to Person C. If you choose the option on the right, then $1000 will be given to Person G and $2000 will be given to person C.

If you choose the option on the left, the final incomes of the two people will be Person C: \$72,250 and Person G: \$500,250. If you choose the option on the right, the final incomes of the two people will be Person C: \$72,000 and Person G: \$501,000.

You will face four questions like the one you saw above in each "decision screen." **Overall, you will face six decision screens with four questions in each.** In each question, you will see a different amount in the option on the left. In each decision screen, you will see a different pair of people.

There is a chance that you may be randomly selected in this study. If you are randomly selected, your choice on one randomly selected question on one randomly selected decision screen will be implemented. **This means that if you are randomly selected, one of your choices will have real consequences for two other people.** The final bonus of these two people will be transferred to them at the end of the study.

Please answer the following questions to demonstrate that you have understood the instructions. You can read the instructions above again if you feel the need to.

Please state True or False: "In this study, you will make several choices involving seven real people."

[True; False]

Please state True or False: "If you are randomly selected, one of your choices will have real consequences for two other people."

[True; False]

(You will be allowed to move to the next screen in 30 seconds)

Screen Break

[The incomes of the Recipients in the six Comparisons are identical to the incomes of the Recipients in Treatment Loss x 70K.]

[C1Q1: Architect chooses between $(2500, 500)$ and $(2000, 1000)$]

Screen Break

[C1Q2.1: If $(2000, 1000)$ chosen in C1Q1, choose between $(2750, 750)$ and $(2000, 1000)$]

[C1Q2.2: If $(2500, 500)$ chosen in C1Q1, choose between $(2250, 250)$ and $(2000, 1000)$]

Screen Break

[C1Q3.1: If $(2000, 1000)$ chosen in C1Q2.1, choose between $(2875, 875)$ and $(2000, 1000)$]

[C1Q3.2: If $(2750, 750)$ chosen in C1Q2.1, choose between $(2625, 625)$ and $(2000, 1000)$]

[C1Q3.3: If $(2000, 1000)$ chosen in C1Q2.2, choose between $(2375, 375)$ and $(2000, 1000)$]

[C1Q3.4: If $(2250, 250)$ chosen in C1Q2.2, choose between $(2125, 125)$ and $(2000, 1000)$]

Screen Break

[C1Q4.1: If $(2000, 1000)$ chosen in C1Q3.1, choose between $(2950, 950)$ and $(2000, 1000)$]

[C1Q4.2: If $(2875, 875)$ chosen in C1Q3.1, choose between $(2800, 800)$ and $(2000, 1000)$]

[C1Q4.3: If $(2000, 1000)$ chosen in C1Q3.2, choose between $(2700, 700)$ and $(2000, 1000)$]

[C1Q4.4: If $(2625, 625)$ chosen in C1Q3.2, choose between $(2550, 550)$ and $(2000, 1000)$]

[C1Q4.5: If $(2000, 1000)$ chosen in C1Q3.3, choose between $(2450, 450)$ and $(2000, 1000)$]

[C1Q4.6: If $(2375, 375)$ chosen in C1Q3.3, choose between $(2300, 300)$ and $(2000, 1000)$]

[C1Q4.7: If $(2000, 1000)$ chosen in C1Q3.4, choose between $(2200, 200)$ and $(2000, 1000)$]

[C1Q4.8: If $(2125, 125)$ chosen in C1Q3.4, choose between $(2050, 50)$ and $(2000, 1000)$]

[The questions in the other Comparisons are identical to those in Comparison 1]

H.4 Treatment Gain x 500K

[All questions are identical to those in Treatment Gain x 70K, with the following exceptions: The incomes of the Recipients in the six Comparisons are identical to the incomes of the Recipients in Treatment Loss x 500K.]

I Instructions - Study 3

Bold text, underlining, tables, etc., appear as on the original screen.

I.1 Treatment Real

[Consent screen]

Introduction
Welcome to this research study. We appreciate your participation. We are a non-partisan group of researchers from University of Zurich and Erasmus University Rotterdam. This study contains real choices and questions regarding your demographic characteristics. No matter what your political views are, by completing this survey you are contributing to our knowledge as a society.

Time required
Approximately **12 minutes.**

Requirements
You must be a U.S. resident to participate in this study. You must also be above the age of 18. The survey contains attention checks. You must pass these check in order to proceed with the survey.

Confidentiality
All data obtained from you will be used for research purposes only. Data will be anonymized immediately after collection. Researchers will at no point have access to any information that could be used to personally identify you.

Voluntary participation
It is voluntary to participate in the project, and you can at any time choose to withdraw your consent without stating any reason.

Questions about the Survey
If you have questions about this study or your rights, please get in touch with us at Krishna.srinivasan@econ.uzh.ch

Consent
I have received the above information about the project and am willing to participate.

[Yes; No]

What is your prolific ID?

Screen Break

[Screen shown if participant does not provide consent]

You did not give your consent to continue with the study.

Thank you for your time.

Please return your submission on Prolific by selecting the 'Stop without completing' button.

Screen Break

[Block 1: Background Questions]

What is your sex?

[Male; Female]

How old are you?

[18 years old - 34 years old; 35 years old - 44 years old; 45 years old - 54 years old; 55 years old - 64 years old; 65 years old or above]

In which state do you currently reside?

[Alabama; ...; Wyoming; I do not reside in the U.S.

In which ZIP code do you live? (5 digits)

What is the highest level of education you have completed?

[Less than High School; High School/GED; Some College; Associate's Degree; Bachelor's degree; Master's degree; Doctoral or Profession Degree (PhD, ED.D, JD, DVM, DO, MD, DDS, or similar)]

As of today, do you consider yourself a Republican, a Democrat, or an Independent?

[Republican; Democrat; Independent]

The next question is about your **total individual income in 2021 before taxes**. This figure should include income from all sources, including salaries, wages, pensions, social security, dividends, interest, and all other income. What was your total individual income (USD) in 2021?

[$29,999 and below; $30,000 to $59,999; $60,000 to $99,999; $100,000 to $149,999; $150,00 and above]

Screen Break

[Displayed if $29,999 and below is chosen]

You have reported that your total individual income in 2021 before taxes was $29,999 and below.

[Displayed if $30,000 to $59,999 is chosen]

You have reported that your total individual income in 2021 before taxes was $30,000 to $59,999.

[Displayed if $60,000 to $99,999 is chosen]

You have reported that your total individual income in 2021 before taxes was $60,000 to $99,999.

[Displayed if $100,000 to $149,999 is chosen]

You have reported that your total individual income in 2021 before taxes was $100,000 to $149,999.

[Displayed if $150,000 and above is chosen]

You have reported that your total individual income in 2021 before taxes was $150,000 and above.

[Displayed in all cases]

Could you provide your best guess of what your **total individual income** was?

Screen Break

[If a participant does not reside in the U.S.]

End of survey

Unfortunately, you do not fulfil the requirements of this study since you do not reside in the U.S.

Thank you for your time.

Please return your submission on Prolific by selecting the 'Stop without completing' button.

Screen Break

[Attention check]

In surveys like ours, some participants do not carefully read the questions. This means that there are a lot of random answers that can compromise the results of research studies. To show that you read our questions carefully, please choose both "Extremely interested" and "Not at all interested" below:

[Extremely interested; Very interested; A little bit interested; Almost not interested; Not at all interested]

Screen Break

[Block 2: Eliciting Welfare Weights]

[Instructions screen]

Instructions

In this study, you will make several choices involving **seven real people**. These people will be selected at random from a survey panel and will not participate in the same survey as

Person	After-tax annual income
Person A	$8,000
Person B	$35,000
Person C	$70,000
Person D	$100,000
Person E	$170,000
Person F	$250,000
Person G	$500,000

you. These people are above the age of 18 and are U.S. citizens. The incomes of the seven people **after all taxes paid and transfers received** are as follows:

Here is an example of a question that you will see in the survey:

	Person C	**Person G**
After-tax annual income	**$70,000**	**$500,000**

Question 2/4: Please choose your preferred alternative

Person C: +$750 Person G: -$1250	Person C: +$500 Person G: -$500

In this question, if you choose the option on the left, then $1250 will be taken away from Person G and $750 will be given to Person C. If you choose the option on the right, then $500 will be taken away from Person G and $500 will be given to Person C.

If you choose the option on the left, the final incomes of the two people **(including an initial $1500 bonus)** will be Person C: $72,250 and Person G: $500,250. If you choose the option on the right, the final incomes of the two people (including an initial $1500 bonus) will be Person C: $72,000 and Person G: $501,000.

You will face four questions like the one you saw above in each "decision screen." **Overall, you will face six decision screens with four questions in each.** In each question, you will see a different amount in the option on the left. In each decision screen, you will see a different pair of people.

One participant in this study will be randomly selected. If you are randomly selected, your

choice on one randomly selected question on one randomly selected decision screen will be implemented. **This means that if you are randomly selected, one of your choices will have real consequences for two other people.** The final bonus of these two people will be transferred to them at the end of the study.

Please answer the following questions to demonstrate that you have understood the instructions. You can read the instructions above again if you feel the need to.

Please state True or False: "In this study, you will make several choices involving seven real people."

[True; False]

Please state True or False: "If you are randomly selected, one of your choices will have real consequences for two other people."

[True; False]

(You will be allowed to move to the next screen in 30 seconds)

[The timer updates dynamically. When the time elapses, the text disappears.]

Screen Break

[If a participant fails at least two out of three checks (one attention check and two comprehension checks)]

End of survey

Sorry, you answered at least two out of three comprehension/attention checks incorrectly.

You cannot continue with the study.

Thank you for your time.

Please return your submission on Prolific by selecting the 'Stop without completing' button.

[If a participant fails only one out of three checks (one attention check and two comprehension checks)]

End of survey

Thank you for your time.

We will pay you your £2 participation fee in the following days.

Please click the following link to finish the survey.

Screen Break

[C1Q1: Comparison 1 Question]

Decision Screen 1/6

Please consider each question carefully because if you are selected, one of your choices will have real consequences for two other persons.

	Person A	Person C
After-tax annual income	**$8,000**	**$70,000**

Question 1/4: Please choose your preferred alternative:

Person A: +$1000 Person C: -$1000 ○	Person A: +$500 Person C: -$500 ○

Screen Break

[All questions hereafter in Comparison 1 look like C1Q1]

[C1Q2.1: If $(500, -500)$ chosen in C1Q1, choose between $(1250, -750)$ and $(500, -500)$]

[C1Q2.2: If $(1000, -1000)$ chosen in C1Q1, choose between $(750, -1250)$ and $(500, -500)$]

Screen Break

[C1Q3.1: If $(500, -500)$ chosen in C1Q2.1, choose between $(1375, -625)$ and $(500, -500)$]

[C1Q3.2: If $(1250, -750)$ chosen in C1Q2.1, choose between $(1125, -875)$ and $(500, -500)$]

[C1Q3.3: If $(500, -500)$ chosen in C1Q2.2, choose between $(875, -1125)$ and $(500, -500)$]

[C1Q3.4: If $(750, -1250)$ chosen in C1Q2.2, choose between $(625, -1375)$ and $(500, -500)$]

Screen Break

[C1Q4.1: If $(500, -500)$ chosen in C1Q3.1, choose between $(1450, -550)$ and $(500, -500)$]

[C1Q4.2: If $(1375, -625)$ chosen in C1Q3.1, choose between $(1300, -700)$ and $(500, -500)$]

[C1Q4.3: If $(500, -500)$ chosen in C1Q3.2, choose between $(1200, -800)$ and $(500, -500)$]

[C1Q4.4: If $(1125, -875)$ chosen in C1Q3.2, choose between $(1050, -950)$ and $(500, -500)$]

[C1Q4.5: If $(500, -500)$ chosen in C1Q3.3, choose between $(950, -1050)$ and $(500, -500)$]

[C1Q4.6: If $(875, -1125)$ chosen in C1Q3.3, choose between $(800, -1200)$ and $(500, -500)$]

[C1Q4.7: If $(500, -500)$ chosen in C1Q3.4, choose between $(700, -1300)$ and $(500, -500)$]

[C1Q4.8: If $(625, -1375)$ chosen in C1Q3.4, choose between $(550, -1450)$ and $(500, -500)$]

Screen Break

[Comparisons 2-6 are identical to Decision Screen 1, with the following exceptions]

[In Comparison 2, the Recipients are: Person B: \$35,000 and C: \$70,000]

[In Comparison 3, the Recipients are Person C: \$70,000 and D: \$100,000]

[In Comparison 4, the Recipients are Person C: \$70,000 and. E: \$170,000]

[In Comparison 5, the Recipients are Person C: \$70,000 and F: \$250,000]

[In Comparison 6, the Recipients are Person C: \$70,000 vs. G: \$500,000]

[For half the participants, the order of the Comparisons is reversed. The pair of Recipients are as follows: C: \$70,000 vs. G: \$500,000 (Comparison 1), C: \$70,000 vs. F: \$250,000 (Comparison 2), C: \$70,000 vs. E: \$170,000 (Comparison 3), C: \$70,000 vs. D: \$100,000 (Comparison 4), B: \$35,000 vs. C: \$70,000 (Comparison 5), and A: \$8,000 vs. C: \$70,000 (Comparison 6).]

Screen Break

How confident are you that the choices you made in the previous screens reflect what you really think?

Please provide your answer on a scale of 1 to 5. A 1 indicates "Not confident all," and a 5 indicates "Completely confident."

[5: Completely confident; 4:; 3:; 2:; 1: Not confident at all]

Screen Break

[**Block 3: Welfarist and Non-Welfarist Concerns**]

In the following screens, we would like to ask you some general questions about your views on society. Your opinion and thoughts are important to us.

Consider the current incomes of individuals in society obtained after all taxes are paid and transfers received.

Which of the following statements comes closest to how you feel?

High-income individuals ...

[do not deserve their current income and do not need their current income; deserve their current income but do not need their current income; do not deserve their current income but need their current income; deserve their current income and need their current income]

Which of the following statements comes closest to how you feel?

Low-income individuals ...

[do not deserve their current income and do not need their current income; deserve their current income but do not need their current income; do not deserve their current income but need their current income; deserve their current income and need their current income]

Screen Break

Consider the current incomes of individuals in society obtained after all taxes are paid and transfers received.

Do you think that, given the current incomes of individuals in society, incomes should be further redistributed or should not be further redistributed?

Please provide your answer on a scale from -2 to +2 where a +2 means that income should be further redistributed by taking from the higher-income individuals and giving to the lower/middle-income individuals while a -2 means that income should be further redistributed by taking from the lower/middle-income individuals and giving to the higher-income individuals.

[-2: Incomes should be further redistributed by taking from the lower/middle-income individuals and giving to the higher-income individuals; -1:; +0: Incomes should **not** be further redistributed; +1:; +2: Incomes should be further redistributed by taking from the higher-income individuals and giving to the lower/middle-income individuals]

Screen Break

[**Block 4: Knowledge**]

The next set of questions is about the income tax system in the United States. These are questions for which there are right or wrong answers.

In order for your answers to be most helpful to us, it is really important that you answer these questions as accurately as you can. Although you may find some questions difficult, it is very important for our research that you try your best. Thank you very much!

Out of 100 households in the U.S., how many are in the top federal personal income tax bracket?

[slider 0-100]

What share of their total income do people in the top federal personal income tax bracket

pay in taxes?

[slider 0-100]

Out of 100 U.S. households, how many pay no federal income taxes?

[slider 0-100]

Imagine a middle class household that is right at the middle of the income distribution, such that half of all households in the U.S. earn more than this household and half earn less. What share of their income do you think such a household pays in federal income taxes?

[slider 0-100]

Out of every 100 individuals in the U.S., how many earn an income (after all taxes paid and transfers received) below $35,000?

[slider 0-100]

We would now like to ask you what you think about the life opportunities of children from very poor families.

For the following question, we focus on 500 families that represent the U.S. population. We divide them into five groups on the basis of their income, with each group containing 100 families. These groups are:

- The poorest 100 families
- The second poorest 100 families
- The middle 100 families
- The second richest 100 families
- The richest 100 families

How many out of 100 children coming from the poorest 100 families will grow up to be among the richest 100 families?

Screen Break

[**Block 4: Mechanisms**]

We would like to ask you what you think the distribution of after-tax income in the U.S. should be.

There are **7 tax groups** (tax brackets) in the U.S. Group 1 includes households with the lowest incomes and Group 7 includes households with the highest incomes. Groups 2 through 6 include households with incomes in the middle.

Column 2 of the table below lists the **CURRENT** average annual after-tax income of all households in each group. The after-tax income is obtained by subtracting all federal income taxes (e.g., ordinary income taxes, alternative minimum taxes) from the pre-tax income and adding all federal transfers (e.g., tax credits) to the pre-tax income.

In Column 3 of the table below, we list the average federal income tax rate of each group. This rate was determined based on the ordinary income taxes that households paid. As an example, if a household with a pre-tax income of $80,000 has an average tax rate of 15%, they would pay 80000*0.15 = $12,000 in taxes.

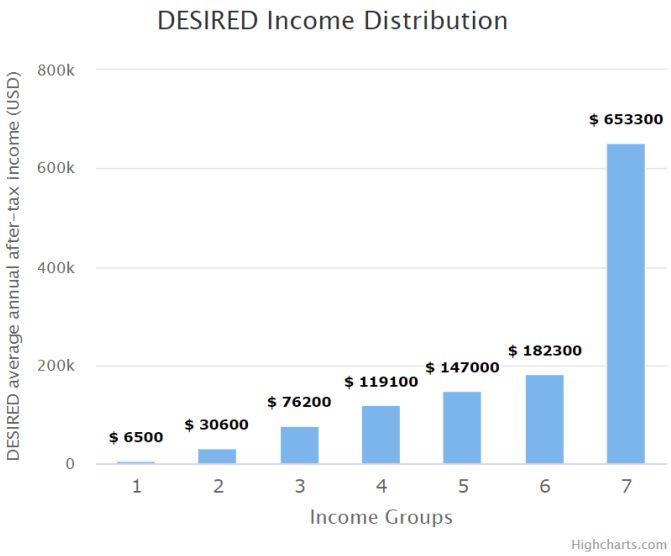
We would like you to indicate what you think the average tax rate for each tax group in the U.S. should be. This can be done as follows. **You can increase or decrease the average tax rates of the first six groups. The average tax rate of group 7 adjusts automatically so that all seven groups together pay as much taxes as they currently do.**

Column 4 of the table below and the figure below indicate your **DESIRED** average annual after-tax incomes. The numbers in the table as well as the figure update automatically as you change the average tax rates.

Your choices will sometimes be limited for a variety of reasons. For example, you cannot set the tax rate for a group such that their average after-tax income becomes lower than the average after-tax income of the group below them or higher than the average after-tax income of the group above them.

Note also that there may be rounding-off errors in various calculations.

You can go back to the initial situation by refreshing the page.



Income group	Annual after-tax income **(CURRENT)**	Average tax rate	Annual after-tax income **(DESIRED)**
1	$6,500	9%	$6,500
2	$30,600	11%	$30,600
3	$76,200	15%	$76,200
4	$119,100	19%	$119,100
5	$147,000	21%	$147,000
6	$182,300	25%	$182,300
7	$653,300	31%	$653,300

Screen Break

Please answer the following last set of questions.

Which has more to do with why a person is rich?

[Because she or he worked harder than others; Because she or he had more advantages than others]

If the federal personal income tax rate were to increase for the richest people in the economy, to what extent would it encourage them to work less?

[A great deal; A lot; A moderate amount; A little; None at all]

Do you think that increasing income taxes on high-income households would hurt economic activity, not have an effect on economic activity, or help economic activity in the U.S.?

[Hurt economic activity in the U.S.; Not have an effect on economic activity in the U.S.; Help economic activity in the U.S.]

Typically, when the top federal income tax rate on high earners is cut, do you think that the lower class and working class mostly win or mostly lose from this change?

[Mostly lose; Neither lose nor win; Mostly win]

Some people think that income inequality in society can affect the level of crime, trust, corruption, and social unrest in society.

How big of an issue do you think income inequality is in America?

[Not an issue at all; A small issue; An issue; A serious issue; A very serious issue]

How much of the time do you think you can trust the federal government to do what is right?

[Always; Most of the time; Only some times; Never]

Screen Break

End of survey

Thank you for your time!

We will pay you your £2 participation fee in the following days.

Please click the following link to finish the survey.

I.2 Treatment Hypothetical

[All questions, with the exceptions of those listed below, are identical to those in Treatment Real]

[Block 2: Eliciting Welfare Weights]

[Instructions screen]

Instructions

In this study, you will make several choices involving **seven hypothetical people**. These people are not real but you should imagine them as above the age of 18 and U.S. citizens. The incomes of the seven people **after all taxes paid and transfers received** are as follows:

Person	**After-tax annual income**
Person A	$8,000
Person B	$35,000
Person C	$70,000
Person D	$100,000
Person E	$170,000
Person F	$250,000
Person G	$500,000

Here is an example of a question that you will see in the survey:

	Person C	**Person G**
After-tax annual income	**$70,000**	**$500,000**

Question 2/4: Please choose your preferred alternative

Person C: +$750 Person G: -$1250	Person C: +$500 Person G: -$500

In this question, if you choose the option on the left, then $1250 will be taken away from Person G and $750 will be given to Person C. If you choose the option on the right, then $500 will be taken away from Person G and $500 will be given to Person C.

If you choose the option on the left, the final incomes of the two people **(including an initial $1500 bonus)** will be Person C: $72,250 and Person G: $500,250. If you choose the option on the right, the final incomes of the two people (including an initial $1500 bonus) will be Person C: $72,000 and Person G: $501,000.

You will face four questions like the one you saw above in each "decision screen." **Overall, you will face six decision screens with four questions in each.** In each question, you will see a different amount in the option on the left. In each decision screen, you will see a different pair of people.

The choices you make in the survey will not have real consequences.

Please answer the following questions to demonstrate that you have understood the instructions. You can read the instructions above again if you feel the need to.

Please state True or False: "In this study, you will make several choices involving seven hypothetical people."

[True; False]

Please state True or False: "Your choices will **not** have real consequences."

[True; False]

(You will be allowed to move to the next screen in 30 seconds)

[The timer updates dynamically. When the time elapses, the text disappears.]

Screen Break

[C1Q1: shown to all participants]

Decision Screen 1/6

Please consider each question carefully.

	Person A	Person C
After-tax annual income	**$8,000**	**$70,000**

Question 1/4: Please choose your preferred alternative:

Person A: +$1000 Person C: -$1000 ○	Person A: +$500 Person C: -$500 ○

[All Comparisons and questions and identical to those in Treatment Real. Only the first sentence differs between the two treatments]

I.3 Treatment No Self-Interest

[All questions, with the exceptions of those listed below, are identical those in Treatment Real]

[**Block 1: Background Questions**]

[In the Demographics screen, all questions, with the exception of the question on own income, is the same as in Treatment Real]

The next question is about your **total individual income in 2021 before taxes**. This figure should include income from all sources, including salaries, wages, pensions, Social Security, dividends, interest, and all other income. What was your total individual income (USD) in 2021?

[$22,000 and below; $22,000 to $53,000; $53,000 to $85,000; $85,000 to $135,000; $135,000 to $210,000; $210,000 to $375,000; $375,000 and above]

Screen Break

[Displayed if $22,000 and below is chosen]

You have reported that your total individual income in 2021 before taxes was $22,000 and below.

[Displayed if $22,000 to $53,000 is chosen]

You have reported that your total individual income in 2021 before taxes was $22,000 to $53,000.

[Displayed if $53,000 to $85,000 is chosen]

You have reported that your total individual income in 2021 before taxes was $53,000 to $85,000.

[Displayed if $85,000 to $135,000 is chosen]

You have reported that your total individual income in 2021 before taxes was $85,000 to $135,000.

[Displayed if $135,000 to $210,000 is chosen]

You have reported that your total individual income in 2021 before taxes was $135,000 to $210,000.

[Displayed if $210,000 to $375,000 is chosen]

You have reported that your total individual income in 2021 before taxes was $210,000 to $375,000.

[Displayed if $375,000 and above is chosen]

You have reported that your total individual income in 2021 before taxes was $375,000 and above.

[Displayed in all cases]

Could you provide your best guess of what your **total individual income** was?

Screen Break

[**Block 2: Eliciting Welfare Weights**]

[Instructions screen]

Instructions

In this study, you will make several choices involving **seven real people**. These people will be selected at random from a survey panel and will not participate in the same survey as you. These people are above the age of 18 and are U.S. citizens. The incomes of the seven people **after all taxes paid and transfers received** put them in the following income brackets:

Here is an example of a question that you will see in the survey:

	Person C	**Person G**
After-tax annual income	**$53,000 to $85,000**	**$375,000 and above**

Person	After-tax annual income
Person A	$22,000 and below
Person B	$22,000 to $53,000
Person C	$53,000 to $85,000
Person D	$85,000 to $135,000
Person E	$135,000 to $210,000
Person F	$210,000 to $375,000
Person G	$375,000 and above

Question 2/4: Please choose your preferred alternative

Person C: +$750 Person G: -$1250	Person C: +$500 Person G: -$500

In this question, if you choose the option on the left, then $1250 will be taken away from Person G and $750 will be given to Person C. If you choose the option on the right, then $500 will be taken away from Person G and $500 will be given to Person C.

If you choose the option on the left, the final income brackets of the two people **(including an initial $1500 bonus)** will be Person C: $55,250 to $87,250 and Person G: $375,250 and above. If you choose the option on the right, the final incomes of the two people (including an initial $1500 bonus) will be Person C: $55,000 to $87,000 and Person G: $376,000 and above.

You will face four questions like the one you saw above in each "decision screen." **Overall, you will face six decision screens with four questions in each.** In each question, you will see a different amount in the option on the left. In each decision screen, you will see a different pair of people.

One participant in this study will be randomly selected. If you are randomly selected, your choice on one randomly selected question on one randomly selected decision screen will be implemented. **This means that if you are randomly selected, one of your choices will have real consequences for two other people.** The final bonus of these two people will be transferred to them at the end of the study.

Please answer the following questions to demonstrate that you have understood the instruc-

tions. You can read the instructions above again if you feel the need to.

Please state True or False: "In this study, you will make several choices involving seven real people."

[True; False]

Please state True or False: "If you are randomly selected, one of your choices will have real consequences for two other people."

[True; False]

(You will be allowed to move to the next screen in 30 seconds)

[The timer updates dynamically. When the time elapses, the text disappears.]

Screen Break

[C1Q1: shown to all participants]

Decision Screen 1/6

Please consider each question carefully because if you are selected, one of your choices will have real consequences for two other persons.

	Person A	**Person C**
After-tax annual income	**$22,000 and below**	**$53,000 to $85,000**

Question 1/4: Please choose your preferred alternative:

Person A: +$1000 Person C: -$1000 ○	Person A: +$500 Person C: -$500 ○

[All questions are identical to those in Treatment Real. Comparisons 1 to 6 are identical to the corresponding Comparisons in Treatment Real, with the exception that the incomes of the Recipients are different. The pair of Recipients they view is as follows:

In Comparison 2, the Recipients are Person B: $22,000 to $53,000 and C: $53,000 to $85,000

In Comparison 3, the Recipients are Person C: $53,000 to $85,000 and D: $85,000 to $135,000

In Comparison 4, the Recipients are Person C: $53,000 to $85,000 and E: $135,000 to $210,000

In Comparison 5, the Recipients are Person C: $53,000 to $85,000 and F: $210,000 to $375,000

In Comparison 6, the Recipients are Person C: $53,000 to $85,000 and G: $375,000 and above

[For half the participants, the order of the Comparisons is reversed]

I.4 Treatment Self-Interest

[All questions, with the exception of those listed below, are identical to those in Treatment No Self-Interest]

[Block 2: Eliciting Welfare Weights]

[Instructions screen]

Instructions

In this study, you will make several choices involving **six real people** and you. These six people will be selected at random from a survey panel and will not participate in the same survey as you. These people are above the age of 18 and are U.S. citizens. The incomes of the six people **after all taxes paid and transfers received** put them in the following income brackets:

Note that in this study, you are Person [A/B/C/D/E/F/G] earning [income].

Person	**After-tax annual income**
Person A	$22,000 and below
Person B	$22,000 to $53,000
Person C	$53,000 to $85,000
Person D	$85,000 to $135,000
Person E	$135,000 to $210,000
Person F	$210,000 to $375,000
Person G	$375,000 and above

Here is an example of a question that you will see in the survey:

	Person C	**Person G**
After-tax annual income	**$53,000 to $85,000**	**$375,000 and above**

Question 2/4: Please choose your preferred alternative

Person C: +$750 Person G: -$1250	Person C: +$500 Person G: -$500

In this question, if you choose the option on the left, then $1250 will be taken away from Person G and $750 will be given to Person C. If you choose the option on the right, then $500 will be taken away from Person G and $500 will be given to Person C.

If you choose the option on the left, the final income brackets of the two people **(including an initial $1500 bonus)** will be Person C: $55,250 to $87,250 and Person G: $375,250 and above. If you choose the option on the right, the final incomes of the two people (including an initial $1500 bonus) will be Person C: $55,000 to $87,000 and Person G: $376,000 and above.

You will face four questions like the one you saw above in each "decision screen." **Overall, you will face six decision screens with four questions in each.** In each question, you will see a different amount in the option on the left. In each decision screen, you will see a different pair of people.

Remember that in this study, you are Person [A/B/C/D/E/F/G] earning [income].

One participant in this study will be randomly selected. If you are randomly selected, your choice on one randomly selected question on one randomly selected decision screen will be implemented. **This means that if you are randomly selected, one of your choices will have real consequences. If the selected question involves a payment to you, then we will pay out the bonus to you and to the other person. If the selected question involves a payment to two other persons, then we will pay out the bonus to these two other persons.** The final bonus will be transferred at the end of the study. If you are among the winners, we will contact you in a few months and pay out your bonus via prolific.

Please answer the following questions to demonstrate that you have understood the instructions. You can read the instructions above again if you feel the need to.

Please state True or False: "In this study, you will make several choices involving six real people and you."

[True; False]

Please state True or False: "If you are randomly selected, one of your choices will have real consequences for two other people or for you and one other person."

[True; False]

(You will be allowed to move to the next screen in 30 seconds)

[The timer updates dynamically. When the time elapses, the text disappears.]

Screen Break

[C1Q1: Comparison 1 Question]

Decision Screen 1/6

Please consider each question carefully because if you are selected, one of your choices will have real consequences.

	Person A	**Person C**
After-tax annual income	**$22,000 and below**	**$53,000 to $85,000**

Question 1/4: Please choose your preferred alternative:

Person A: +$1000 Person C: -$1000 ○	Person A: +$500 Person C: -$500 ○

[All questions and Comparisons are identical to those in Treatment No Self-Interest except that in the relevant Comparisons, we replace "Person [A/B/C/D/E/F/G]" with "You." Furthermore, the first sentence in all Comparisons is different.]

References

Bursztyn, L., Handel, B. R., Jimenez, R. & Roth, C. (2023), 'When product markets become collective traps: The case of social media'. NBER Working Paper no. 31771.

Chen, D. L., Schonger, M. & Wickens, C. (2016), 'oTree—An open-source platform for laboratory, online, and field experiments', *Journal of Behavioral and Experimental Finance* **9**, 88–97.

Gelbach, J. B. (2016), 'When do covariates matter? And which ones, and how much?', *Journal of Labor Economics* **34**(2), 509–543.

Hendren, N. (2020), 'Measuring economic efficiency using inverse-optimum weights', *Journal of Public Economics* **187**, 104198.

Hendren, N. & Sprung-Keyser, B. (2020), 'A unified welfare analysis of government policies', *The Quarterly Journal of Economics* **135**(3), 1209–1318.

Luttmer, E. F. & Samwick, A. A. (2018), 'The welfare cost of perceived policy uncertainty: Evidence from social security', *American Economic Review* **108**(2), 275–307.

Mankiw, N. G., Weinzierl, M. & Yagan, D. (2009), 'Optimal taxation in theory and practice', *Journal of Economic Perspectives* **23**(4), 147–174.

Piketty, T., Saez, E. & Zucman, G. (2018), 'Distributional national accounts: methods and estimates for the united states', *The Quarterly Journal of Economics* **133**(2), 553–609.

Saez, E. (2001), 'Using elasticities to derive optimal income tax rates', *The review of economic studies* **68**(1), 205–229.

Saez, E., Slemrod, J. & Giertz, S. H. (2012), 'The elasticity of taxable income with respect to marginal tax rates: A critical review', *Journal of economic literature* **50**(1), 3–50.

Stantcheva, S. (2021), 'Understanding tax policy: How do people reason?', *The Quarterly Journal of Economics* **136**(4), 2309–2369.

Støstad, M. N. & Cowell, F. (2024), 'Inequality as an externality: Consequences for tax design', *Journal of Public Economics* **235**, 105139.