HOW ELASTIC ARE PREFERENCES FOR REDISTRIBUTION? EVIDENCE FROM
RANDOMIZED SURVEY EXPERIMENTS

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ABSTRACT

We develop online survey experiments to analyze how information about inequality and taxes affects preferences for redistribution. Approximately 4,000 respondents were randomized into treatments providing interactive, customized information on U.S. income inequality, the link between top income tax rates and economic growth, and the estate tax. An additional 6,000 respondents were randomized into follow-up treatments to explore mechanisms underlying the initial results. The treatment has very large effects on whether respondents view inequality as a problem. By contrast, it only slightly moves policy preferences (e.g., top income tax rates and transfer programs). An exception is the estate tax—informing respondents of the small share of decedents who pay it more than doubles support for it and this effect persists in a one-month follow-up. We explore several explanations for our results. Extreme ex-ante misinformation appears to drive the large estate tax results. The small effects for all other policies can be at least partially explained by respondents' low trust in government—indeed, we show that priming people to think negatively about the government substantially reduces support for transfer programs—as well as a disconnect between concerns about social issues and the public policies that aim to address them.

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1   Introduction

Over the past several decades, there has been a large increase in income concentration in the United States. While the top one percent of families captured 9.0 percent of total pre-tax income in 1970, that share rose to 22.4 percent by 2012.[1] More recent work has documented a corresponding trend for wealth concentration: the top 0.1 percent share of wealth has grown from 8 percent in the mid-1970s to 22 percent in 2012.[2] These trends have not gone unnoticed, at least by some. The Occupy Wall Street movement popularized the term "the one percent." Recently, President Obama has called "a dangerous and growing inequality" the "defining challenge of our time," a sentiment echoed by the CEO of Goldman Sachs, who told an interviewer that "too much of the GDP of the country has gone to too few of the people."[3]

The standard political economy model suggests that tax and transfer policy will react to rising market income inequality. Specifically, the median-voter theorem predicts that an increase in the demand for redistribution—and thus an increase in top tax rates, as politicians respond to voters' wishes—should accompany this rise in income concentration (Meltzer and Richard, 1981).[4] By contrast, top income tax rates as well as inheritance tax rates have fallen in the United States during this period.[5] While for institutional reasons the policy views of the majority might be ignored by policy-makers (Bartels, 2009), even more challenging to the model's predictions is that survey respondents themselves show no increased demand for redistribution during this period.[6] If anything, the General Social Survey shows there has been a slight decrease in stated support for redistribution in the United States since the 1970s, even among those who self-identify as having below-average income (see Figure 1). These trends have led commenters to suggest that Americans simply do not care about

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[1]See the online updates to Piketty and Saez (2003), Table A3 at
`http://elsa.berkeley.edu/~saez/TabFig2012prel.xls`

[2]See `http://gabriel-zucman.eu/files/SaezZucman2014Slides.pdf`.

[3]See `http://www.whitehouse.gov/the-press-office/2013/12/04/remarks-president-economic-mobility` and `http://thinkprogress.org/economy/2014/06/13/3448679/goldman-sachs-income-inequality/`, respectively.

[4]Note that the self-interested voter might not be a good model of policy preferences. After all, self-interested individuals would not vote in the first place if voting is privately costly.

[5]For top income tax rates, see Piketty, Saez, and Stantcheva (2014) and for estate taxes see IRS calculations at `http://www.irs.gov/pub/irs-soi/ninetyestate.pdf`.

[6]See, e.g., Kenworthy and McCall (2008), for evidence from a variety of OECD countries that saw increases in inequality but no corresponding increase in redistributive demand.

rising inequality.[7]

There are alternative explanations: Americans may be unaware of the extent or growth of inequality (see Kluegel and Smith, 1986 and Norton and Ariely, 2011), this information may not be sufficiently salient, or they are skeptical about the government's ability to redistribute effectively. In this paper, we extensively examine these explanations. We conduct a series of randomized survey experiments using Amazon's Mechanical Turk (mTurk). mTurk is a rapidly growing online platform that can be used to carry out social and survey experiments (see Horton, Rand, and Zeckhauser, 2011 and Paolacci, Chandler, and Ipeirotis, 2010). In our initial set of experiments, comprising just over 4,000 respondents, half of respondents were randomized into an "omnibus" treatment providing interactive, personalized information on U.S. income inequality, the historical correlation between top income tax rates and economic growth, and the incidence of the estate tax. Both control and treatment groups then reported their views on inequality, redistributive policies, and government more generally. We then conducted follow-up experiments with about 6,000 new respondents to analyze potential mechanisms behind the initial results, for a total of approximately 10,000 respondents.[8]

Our treatments exploit the flexibility of the mTurk platform to include several features that heighten the salience of the information we present. First, some of the information we present is *customized*. For example, we ask individuals their household income, allowing us to show them their place in the income distribution, as well as their counterfactual income level had aggregate income growth since 1980 been distributed more equally (so as to leave inequality unchanged). In other parts of the survey, we customize information based on respondents' own household composition.[9] Second, some of the information is interactive— for example, our survey allows respondents to enter different household income levels and the software survey application provides the corresponding percentile, so that the income distribution can be transparently explored.

The initial survey experiment provides several findings we believe to be novel relative to existing literature; the first part of the paper provides a descriptive analysis of these results. First, we find that respondents' concern about inequality is very elastic to information—

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[7]As *Newsweek* put it in 2001: "If Americans couldn't abide rising inequality, we'd now be demonstrating in the streets."

[8]Survey questions and treatments are all available online at
`https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`.

[9]Recent work has highlighted the potential power of customizing information in interventions. For example, Hoxby and Turner (2013) credit the customized nature of the information they present to students for the large effects their intervention had on college application decisions.

for example, the treatment increases the share agreeing that inequality is a "very serious problem" by over 35 percent. Put differently, the treatment effect is equal to roughly 36 percent of the gap between self-identified liberals and self-identified conservatives on this question. By contrast, while there are some effects on policy preferences such as top income tax rates, the minimum wage and food stamps (always in the "expected" direction), they are small and often insignificant despite the large sample size.

The only exception is the estate tax—we find that providing information on the (small) share of estates subject to the tax more than doubles respondents' support for increasing it. Focusing on the estate tax result, we attempt to make progress on two long-standing critiques about survey analysis: that the effects are ephemeral and unrelated to actual behavior. We benefit from the mTurk technology and re-survey respondents one month later: the estate tax effect is virtually unchanged. We also find that the treatment significantly increases the share of respondents who say they would send a petition to one of the U.S. Senators from their state to raise the estate tax.

The second half of the paper explores the mechanisms behind the large estate-tax effects and the muted response for any other policy outcome. Of course, other explanations may exist and as such we do not view our attempts to tease out the mechanisms behind the main results as definitive.

Consistent with past work, we find that respondents are wildly misinformed about the share of decedents subject to the estate tax, which appears to account for the large effects. We further show that the estate tax effect remains strong even when we take steps to decrease the salience and emotional content of the information provided, further proof that basic information on this issue has large effects.

We test three potential explanations for the small effects for other policies: limited trust in government; an overly "clinical" presentation of information; and respondents' inability to connect their concerns on a given issue with the public policies meant to address it.

The first potential explanation is that distrust in government inhibits respondents from translating concern for inequality into support for redistribution by the government. Several results from the original survey experiment point in this direction. First, our initial treatment significantly decreases trust in government. When reminded of the extent of inequality (which even control group respondents view as a problem), those in the treatment group appear to at least partially blame the government, perhaps thinking that if politicians "let things get this bad" they cannot be trusted. Second, beyond any treatment effect, the level

of government trust among our sample of mTurk respondents are very low: over 89 percent agree that "Politicians in Washington work to enrich themselves and their largest campaign contributors, instead of working for the benefit of the majority of citizens," with 47 percent "strongly" agreeing. It is thus perhaps not surprising that even when the treatment increases respondents' concern with inequality, they remain reluctant to increase support for government redistributive policies.

In a follow-up survey experiment, we provide direct evidence for the effect of trust in government on respondents' policy preferences. We first asked a small pilot group to answer open-ended questions on their views of government—the main theme that emerges is that politicians are believed to work to enrich themselves and their wealthiest campaign donors. We then used these answers to develop primes (e.g., asking respondents' opinions about lobbyists or the Wall Street bailout) that significantly lowered trust in government *without* significantly changing views about the extent of inequality or poverty. Therefore, the treatment isolates the causal effect of decreasing trust in government. We find that the treatment significantly lowers support for all poverty-alleviation policies, with the exception, interestingly, of the minimum wage—a program that does not involve direct transfers from the government. Support for top tax rates generally falls as well (though only some of these effects are significant) and respondents elevate "private charity" over government policies in a list of the best ways to combat inequality. To the best of our knowledge, this analysis provides the first evidence on the casual effects of trust in government on policy preferences, and is particularly relevant given the historically low regard with which Americans currently view their government.[10]

Besides distrust muting the policy effects of our treatment, we explore two other potential explanations for the small results in the original survey experiment. As Brader (2005) and others argue, policy preferences might respond more to emotional than factual appeals. We thus develop a treatment—again, interactive and customized—designed to evoke empathy for households at the poverty line. Just as in the initial survey experiment, the treatment significantly increases respondents' tendency to view inequality and poverty as "serious problems" but has almost no effect on policy preferences.

We find more support for a third explanation, the idea in Bartels (2005) that the public fails to connect concern for inequality with actual public policy measures. To test this idea,

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[10]See analysis from Gallup: `http://www.gallup.com/poll/164663/americans-trust-government-generally-down-year.aspx`. The General Social Survey also shows a strongly negative trend.

we repeat much of the information in the "emotional appeal" treatment, but then show respondents concretely the resources provided to such families through government programs including the minimum wage and food stamps. Therefore, the treatment directly connects poverty and inequality with policies meant to address them. Emphasizing this connection appears important: treatment respondents significantly increase their support for the minimum wage as well as most of the poverty-alleviation programs that we survey. We view this result as potentially complementary to the trust results: given the low baseline levels of trust, it appears to be the case that policy preferences can only be moved if respondents are explicitly reminded of efficacious examples of government activity.

We believe our findings make several contributions to the understanding of how individuals form—and change—their redistributive preferences. Compared to most informational interventions that merely provide a fixed set of facts to respondents, our informational treatments were interactive and customized—while perhaps not providing a strict upper bound on the effects of information on preferences, our results do suggest that most policy preferences are hard to move. This finding echoes Luttmer and Singhal (2011) that redistributive preferences may have "cultural" determinants that are very stable over time.

Our results also highlight the potential role of mistrust of government in limiting the public's enthusiasm for policies they would otherwise appear to support, a subject that has garnered limited attention in the economics literature. An exception is Sapienza and Zingales (2013), who find that a major reason respondents support auto fuel standards over a gasoline-tax-and-rebate scheme is not because they misunderstand the incidence of fuel standards but because they simply do not trust the government to actually rebate them their money.

More generally, our paper relates to the literature on the determinants of redistributive preferences, to which political scientists, sociologists, psychologists and public economists have all contributed. Many papers in this literature use survey data to relate individual traits to redistributive preferences and do not, as we do, take an experimental approach. Alesina and Ferrara (2005), Alesina and Giuliano (2011), and Fong (2001) show that, respectively, prospects for future income mobility, past experience of misfortune, and beliefs about equality of opportunity predict redistributive preferences. Other papers have examined how situational factors (employment status, neighborhood characteristics) predict preferences (see, e.g., Margalit 2013, Luttmer 2001). Singhal (2008) uses OECD survey data to show that people do not necessarily favor low tax rates at income levels close to theirs, suggesting that redistributive preferences are not completely determined by self-interest.

As in our paper, some researchers have estimated the effects of randomized informational treatments on policy preferences.[11] The evidence from these efforts is mixed. Sides (2011) finds that providing information on the very small number of individuals affected by the estate tax drastically decreases support for its repeal, results that we replicate with our data. Cruces, Perez-Truglia, and Tetaz (2013) find that showing poor individuals their actual place in the income distribution increases their support for policies that target poverty, as most overestimate their income. On the other hand, Kuklinski et al. (2003) find that providing (accurate) information on the demographic composition of welfare recipients and the share of the federal budget dedicated to welfare payments has no effect on respondents' preferences, despite the fact that their initial beliefs are wildly incorrect.[12] We examine a wide variety of redistributive policy outcomes; indeed, we find that the responsiveness of views on the estate tax appears to be an outlier and other outcomes suggest a far more modest effect of information on redistributive preferences.

As noted, our research is part of a small but growing set of papers using online platforms. Researchers have used these platforms—most often, mTurk—to have respondents play public goods games (e.g., Rand and Nowak, 2011, Suri and Watts, 2010), interact in online labor markets (Amir, Rand, and Gal 2012; Horton, Rand, and Zeckhauser 2011), or simply answer non-experimental survey questions on views about policy and social preferences (Weinzierl 2012; Saez and Stantcheva 2013). We summarize our experience conducting survey experiments on mTurk in the online Methodological Appendix, which we hope can be of use to future researchers utilizing this platform.

The paper proceeds as follows. Section 2 introduces the initial survey instrument and data collection procedures. Section 3 describes the data. Section 4 presents the main results of the survey experiment. In Section 5, we explore mechanisms behind the large effects of information on views about the estate tax and why most other effects were so limited, reporting methods and results from four follow-up survey experiments. Finally, in Section 6, we suggest directions for future work and offer concluding thoughts.

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[11]While not related to policy preferences, there is a small literature on how information treatments affect individuals' ability to better *navigate* policies such as Social Security (Liebman and Luttmer, 2011).

[12]Related but distinct from informational treatments are priming and presentational treatments (see, e.g., Savani and Rattan 2012 on the effect of priming about free-will and McCaffery and Baron 2006 on the effects of presenting taxes in absolute or percentage terms).

2 The Main Survey Experiment

The main experiment was implemented in four separate rounds from January 2011 to August 2012. For expositional clarity, to distinguish this initial experiment from the follow-up work we describe in Section 5, we refer to these four initial rounds of surveys as the "omnibus" treatment surveys.

The omnibus treatment surveys had the following structure: (1) background socio-economic questions including typical demographic questions as well as political leanings; (2) randomized treatment providing information on inequality and tax policy (shown solely to the treatment group); (3) questions on views on inequality, tax and transfer policies, and government more generally.[13]

2.1 Data collection

Surveys were openly posted on mTurk with a description stating that the survey paid $1.50 for approximately 15 minutes, i.e., a $6 hourly wage. Respondents were free to drop out any time or take up to one hour to answer all questions. As a comparison, the average effective wage on mTurk according to Amazon is around $4.80 per hour and most tasks on mTurk are short (less than one hour).[14]

Several steps were taken to ensure the validity of the results. First, there are many foreign workers on mTurk, especially from Asia. In addition to requiring respondents to confirm their U.S. residency on the consent form, we also had Amazon show the survey only to workers who had U.S. addresses. Second, to further discourage foreign workers, we tried to launch our surveys during East Coast daylight hours (and, to reduce heterogeneity, only on workdays). Third, only workers with a past completion rate of at least ninety percent were allowed to take the survey, in order to exclude robots. Fourth, as our survey comprises many rounds, we block workers who had participated in a previous round of the survey. Fifth, respondents were told that payment would be contingent on completing the survey

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[13]The Appendix provides a complete description of the experiment with the questions for each round of the main experiments, as well as the follow-up experimental rounds that we describe later in the paper.

[14]In order to gauge the generalizable of results on mTurk to results on other platforms, we gathered data for round 3 using C&T Marketing (`http://www.ctmarketinggroup.com/`). As noted in Section 4, effects are stable across rounds, suggesting that respondents from the two platforms respond similarly to the treatment. See the Methodological Appendix for more detail. Note that per-participants costs for C&T are roughly five times higher than for mTurk.

and providing a password visible only at completion. Finally, to discourage respondents from skipping mindlessly through the pages, pop-up windows with an encouragement to answer all questions appeared as prompts whenever a question was left blank.

2.2 The omnibus information treatment

In general, the goal of the information treatments was to provide a large "shock" to individuals' information about inequality and redistributive policies, rather than to provide a Ph.D-level, nuanced discussion about, say, the underlying causes of inequality or the trade-off between equality and efficiency. Hence, some of the treatments we display will seem overly simplified to an economics audience, but it should be kept in mind that our goal in the initial experiment is to test whether *any* treatment can move redistributive preferences; thus we erred on the side of presenting information we thought would indeed move those preferences. As noted in the introduction, we took steps to make the information both interactive and customized to each respondent.

The treatment had three basic parts. First, treatment respondents saw interactive information on the *current* income distribution—they were asked to input their household income and were then told what share of households made more or less than their household. We also asked them to find particular points in the distribution—they were asked to find the median and the 90$^{\text{th}}$ and 99$^{\text{th}}$ percentiles and were encouraged to "play around" with the application. Appendix Figure 1 presents a screen shot.[15]

The second part focused not on the current distribution but a counterfactual: respondents entered their current income and were then shown what they "would have made" had economic growth since 1980 had been evenly shared across the income distribution (i.e., had the level of inequality stayed the same as in 1980). Of course, this exercise abstracts away from the trade-off between efficiency (economic growth) and equality that would certainly exist at very high levels of taxation. The interactive application allowed them to find this counterfactual value for any point of the current income distribution. Appendix Figure 2 presents a screen shot.

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[15]As detailed in Appendix Table 1 we also ask treatment respondents six "basic comprehension" questions to determine if the information was confusing. With one exception, each question exhibits at least eighty percent comprehension. Moreover, more than 74 percent of respondents answer at least five of the six questions correctly. There are no differential treatment effects by comprehension level (results available upon request), not surprising given that comprehension is at a fairly high (and uniform) level.

The third part of the treatment focused on redistributive policies. To emphasize that higher income taxes on the well-off need not always lead to slower economic growth, we presented respondents a figure showing that, at least as a raw correlation, economic growth, measured by average real pre-tax income per family from tax return data, has been slower during periods with low top tax rates (1913 to 1933 and 1980 to 2010) than with high top tax rates (1933 to 1980). Appendix Figure 3 presents a screen shot. Similarly, we also presented a slide on the estate tax, emphasizing that it currently only affects the largest 0.1 percent of estates. Appendix Figure 4 presents a screen shot.

Readers can directly experience these informational treatments online at the link below.[16] We describe the additional treatments in the follow-up surveys in Section 5.

3 Data

3.1 Summary statistics

Table 1 shows characteristics of the sample who completed the omnibus treatment survey rounds (we discuss attrition below). We compare these summary statistics to a nationally representative sample of U.S. adults contacted by a CBS poll in 2011, which we choose both because it was conducted around the same time as our surveys and asks very similar questions.[17] We also compare it to a more representative (though far more expensive) online panel survey gathered by RAND, the American Life Panel.[18]

Our sample is younger, more educated and has fewer minorities. It is more liberal, with a higher fraction reporting having supported President Obama in the 2008 election.[19]

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[16]See https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn. Note that the control group went straight from the background questions to the outcomes measures (starting with the preferred tax rates sliders).

[17]Note that the CBS sample is not as representative as the traditional surveys used by economists such as the Current Population Survey or the American Community Survey. However, these two surveys do not have questions on past voting behavior or political preferences, so we rely on the admittedly less representative CBS survey.

[18]The American Life Panel currently costs researchers $3 per subject per minute, compared to roughly $.10 per subject per minute for our mTurk surveys. The ALP survey is also limited in sample size.

[19]As a robustness check, we created weights to match our mTurk sample in col. (1) to the CBS poll in col. (2) with respect to the 32 cells based on: gender (2) × age brackets (2) × white versus non-white (2) × college degree indicator (2) × Supported Obama in 2008 (2). Reweighting has no appreciable effects on the results in Tables 4 and 5 (results available upon request) and thus we focus on the unweighted results in the paper.

Table 2 shows summary statistics on demographic and policy views for self-reported liberals (col. 1) and conservatives (col. 2) from our control group (so that responses are not contaminated by the information treatment), as well as the entire control group (col. 3). As expected, conservatives are older, more white, and more likely to be married. They prefer lower taxes on the rich and a less generous safety net. Such contrasts are useful to scale the magnitude of our effects. We will often discuss treatment effects both in absolute terms and as a percentage of the liberal-versus-conservative differences reported in Table 2. For convenience, we refer to this difference as the "political gap" for a given outcome variable.

3.2 Survey attrition

The omnibus survey experiment had an overall attrition rate of 22 percent, which includes those who attritted as early as the consent page. For those who remained online long enough to be assigned a treatment status, attrition was 15 percent.

As Appendix Table 2 shows, attrition is not random, though it is unrelated to 2008 voting preferences and liberal versus conservative policy views (the variables most highly correlated with our outcome variables). The online survey for the treatment group was, by necessity, different from the online survey for control group. Therefore, a key concern is differential attrition between those assigned to the treatment versus control arms. As the final row of Table 2 shows, attrition is higher among the treatment group (twenty percent, versus nine percent for the control group).[20]

Importantly, however, conditional on finishing the survey, assignment to treatment appears randomly assigned. That is, while the treatment induces attrition, it does not induce certain groups to differentially quit the survey more than others. Table 3 shows the results from estimating (using the sample who complete the survey) 14 separate regressions of the form: $Treatment_{ir} = \beta Covariate_{ir} + \delta_r + \epsilon_{ir}$, where i indexes the individual, r the survey rounds, and δ_r are survey-round fixed effects.[21] For each regression, one of the control variables in Table 2 serves as $Covariate$. Of the 14 regressions, only two (for the $black$ and $Hispanic$ indicators) yield significant coefficients on the $Covariate$ variable. However,

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[20]For this comparison, we can obviously only include individuals who remained in the survey long enough to have been assigned a treatment status. The other comparisons in this table include all those who remained long enough to answer the given covariate question.

[21]We include round fixed effects δ_r because in one round we assigned more than half the respondents to the treatment. As such, without round fixed effects, $Treatment$ becomes mechanically correlated to the characteristics of respondents in this round.

given that their point-estimates have opposite signs, it does not seem that, say, minorities systematically attrit from the sample if they are assigned to the treatment.

While we will control for these covariates as well as perform additional checks of attrition in the analysis that follows, it is reassuring to see that, conditional on finishing the survey, there does not appear to be a discernible pattern in the types of respondents assigned to treatment. We are quite fortunate in this regard, as one might have expected that groups pre-disposed against reading about inequality—perhaps conservatives or wealthier people— would have been "turned off" by the treatment and differentially attired. Note also that the follow-up surveys discussed in Sections 5 have essentially zero differential attrition by treatment status (see Appendix Table 3), most likely because the treatments in the follow-up surveys are much shorter (making the treatment and control arms of the survey much closer in length).

4   Results from the omnibus treatment

We present three sets of results. Our first set of results captures how the treatment affects respondents' answers to questions related to inequality *per se*, not policies that might affect it. Our second set of results relate to specific policies—e.g., raising taxes or increasing the minimum wage. Our final set of results relate to views about government and respondents' political involvement.

4.1   Views on inequality

Table 4 presents the effect of the omnibus treatment on questions related to inequality and income distribution. Odd-numbered columns do not include any controls outside of round fixed effects, while even-number columns include standard controls (essentially, those listed in Table 3).[22]

Col. (1) shows that the treatment is associated with a ten-percentage-point (or 36 percent) increase in the share agreeing that inequality is a "very serious" problem. Similarly, dividing the point-estimate by the 'political gap' (i.e., the liberal-conservative control group difference for the outcome variable) suggests that the treatment effect is equal to 36 percent of the

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[22]Specifically, we include fixed effects for racial/ethnic categories, employment status, and state of residence; indicator variables for voting for Barack Obama in 2008, being married, gender, and native-born status; continuous controls for age; and categorical variables for the liberal-to-conservative self-rating, household income, and education.

political gap on this question (which, as Table 2 shows, is 38 percentage points). While a convenient scaling, dividing by the political gap is hardly a perfect metric—while political views are highly predictive of many of our outcomes, this tendency varies and therefore some questions have larger political gaps than others. We thus report both the absolute and scaled effects for all regressions. Adding covariates in col. (2) has no effect on the estimated treatment effect.

The effects on the outcome "did inequality increase since 1980?" are presented in columns (3) and (4) and are even larger both in absolute percentage points and when scaled by the political gap (54 percent of the conservative-liberal difference), likely because the informational treatment presented information directly related to the question.

The effects on respondents' opinion of whether the rich are deserving of their income are presented in column (5). They are statistically significant, but markedly smaller in magnitude—equal to about five percentage points, or one-sixth the political gap. Therefore, it does not seem that treatment respondents' concern about inequality is being driven primarily by a vilification of the rich.

In no case does the choice to exclude or include controls change the results (consistent with the results from Table 3 that conditional on finishing the survey, there was little correlation between treatment status and standard covariates). Therefore, to conserve space and reduce noise, we show all results with covariates in the rest of the analysis.

Overall, our omnibus treatment generated a very strong "first stage," significantly shifting views about inequality and its increase in recent decades.

4.2   Views on public policy

Table 5 presents results for questions related to income and estate taxation. The first two columns report results from the two questions on income taxation—a continuous variable asking respondents to choose an ideal average tax rate for the richest one percent and a categorical variable asking them whether taxes on millionaires should be raised—show statistically significant effects of the treatment, in the "expected" direction.[23] However, these magnitudes are small, equal to about ten percent of the liberal-conservative gap in both cases. For example, the treatment increases the preferred top 1% average tax rate by 0.93 percentage points, whereas the gap between liberals and conservatives on this question is slightly over ten percentage points (see Table 2). The omnibus treatment was hardly subtle

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[23]See Appendix Figure 5 for the screen respondents used to choose their ideal tax rates.

in its discussion of income taxes, focusing on how income growth might be shared more equitably through higher taxation and illustrating the temporal correlation between periods of high top tax rate and strong economic growth. We also asked the income tax question in two different ways, so the small magnitude of the results is unlikely to be an artifact of framing.

By contrast, there are very large effects for the estate tax (col. 3), consistent with Sides (2011). The treatment triples the share of respondents reporting a desired increase in the estate tax, and the effect size is more than double the liberal-conservative gap on this question.

A common critique of survey experiments that find large effects on opinion is that one cannot know how these effects impact actual behavior. We try to partially bridge this gap by asking individuals whether they would send a petition to their U.S. Senator asking either to raise or lower the estate tax. We provided a link to Senators' emails and also provided sample messages both for and against raising estate taxes. We then asked if the respondent would send a petition for higher taxes, would send a petition against higher taxes, or would do nothing at all.

We report these results in col. (4). The treatment significantly increases the propensity of respondents to say they would petition their U.S. Senator to raise the estate tax (though, not surprisingly, this effect is smaller than the pure opinion question, suggesting attenuation from belief to action). Naturally, we recognize that we must take respondents' word that they will send the email and thus this outcome is not as concrete as, for example, knowing with certainty how they would vote in the next election. At the very least, this result confirms the strong effect of the treatment on views about the estate tax. In Section 5, we probe the robustness of this result and offer some thoughts on why it is so different from the income tax. For now we merely note that these large results serve to dismiss a potential explanation of why the income tax results were so small—that there is something inherent in the mTurk experience that mutes respondents' policy responses.

While so far we have focused on policies that affect the well-off, we also asked a series of questions about policies that impact the bottom of the income distribution. While the treatment induces significant but small (less than ten percent of the political gap) effects for the minimum wage (col. 5), it induces no significant increase in support for food stamps or the Earned Income Tax Credit (EITC) (cols. 6 and 7).[24] The results thus suggest a contrast

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[24]In later follow-up work, we asked a small pilot group to write open-ended responses to many of

between direct transfer policies such as the EITC and food stamps and indirect transfer policies such as the minimum wage, a theme that will also emerge in some of the follow-up work discussed in next section.[25]

4.3 Views of government and political involvement

Columns (8)-(10) of Table 5 reports results on the effect of the treatment on opinions about government. The first question asked respondents: "How much of the time do you think you can trust government in Washington to do what is right?" and we code a respondent as trusting government if she answers "always" or "most of the time" as opposed to "only some of the time" and "never."[26] Col. (8) reports a large decrease in the share of treatment respondents agreeing that the government can be trusted. The treatment is equal to the entire liberal-conservative gap, but operates in the *opposite* direction to the other outcomes, in that it makes respondents take the more conservative—and less trusting—view on this question.[27] Note also that, consistent with the trends noted in the Introduction, the control group has a very low level of trust in government—only about 16 percent are trusting of government, by our definition—and that the contrast of liberals and conservatives about trusting government is fairly small (17% vs. 14.5%, see Table 2). The low baseline level of trust in the control group suggests that the treatment effect we observe might in fact understate the true effect experienced by the treatment group, as their ability to express an even lower opinion of government is limited by floor effects.

The second question assesses respondents' preferred scope of government: "Next, think more broadly about the purpose of government. Where would you rate yourself on a scale of 1 to 5, where 1 means you think the government should do only those things necessary to provide the most basic government functions, and 5 means you think the government should take active steps in every area it can to try and improve the lives of its citizens?" Intriguingly,

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our outcome variables. Many respondents had little familiarity with the EITC (though we always provided a description) so the non-result for that outcome might need to be interpreted more cautiously. No respondent indicated unfamiliarity with food stamps or the minimum wage, however.

[25]There are other possible distinctions between these policies. For example, respondents may have stronger racial stereotypes of food-stamp recipients than they do of minimum-wage workers.

[26]These questions were taken from Gallup.

[27]We say that being less trusting of government is the "conservative" view because in our data as well as GSS data from the same time period, conservatives indeed report lower trust in government. However, these tendencies are very sensitive to the party in power (e.g., in the GSS, during the George W. Bush administration, self-described conservatives were more trusting of the executive branch than were liberals).

the treatment significantly moves people toward wanting a more active government (col. 9). Providing information about the growth of inequality and the ability of the government to raise taxes and redistribute have complicated effects on views of government. It appears to make respondents see more areas of society where government intervention may be needed but simultaneously make them trust government less. We return to these results linking trust in government to preferences on government scope in Section 5.2.1.

Finally, as shown in col. (10), the treatment has almost no effect on respondents' planned voting choice for the 2012 Presidential elections (recall that the omnibus-treatment surveys were completed before the November 2012 election). There is at best a marginal effect in the direction of supporting President Obama. This result is consistent with the relatively mild policy effects overall. The treatment may simultaneously make individuals want the more redistributive policies of the Democratic party *and* distrust the party in power (the Democrats under Obama, at least in the executive branch and the Senate).[28]

4.4   Robustness checks

Persistence of effects. Before mTurk, recontacting survey respondents was onerous, and thus few papers were able to test the duration of effects from informational survey experiments. None of the papers cited in the Introduction on the effect of information on redistributive preferences follows up with respondents to measure the duration of the effects.[29]

The evidence from the few papers that do test persistence is not encouraging. Luskin, Fishkin, and Jowell (2002) find that even the immediate effects of an extreme intervention— in which British participants spent a weekend with experts with the goal of debunking misconceptions about crime and prison policy—do not persist ten months later. Indeed, in a similarly intense intervention focused on issues related to campaign finance, Druckman and Nelson (2003) find that their results dissipate within ten days. While not a survey experiment *per se*, Gerber et al. (2011) use variation in the location of campaign television advertising to show that persuasive effects are strong the week the ad airs but has little persistence beyond the first week. Perhaps closest to our methodology, Lecheler and Vreese (2011) sample Dutch respondents to test for the effect of informational treatments on opinions about economic

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[28]In the interest of space, there are some outcome variables we relegate to the Appendix. The full set of all results from the omnibus survey are found in Appendix Tables 4 and 5.

[29]In their review of the use of survey experiments, Gaines, Kuklinski, and Quirk (2007) names measuring duration effects as their first recommendation for future work on survey experiments.

aid to Bulgaria and Romania; while the treatment effect persisted after one week, it was insignificant after two.

The flexibility of the mTurk platform offers the possibility of resurveying participants months after the original survey. In the third round of the omnibus survey, we attempted to recontact respondents one month after taking the survey. Out of 1087 respondents who completed the original survey, 187 (17%) completed the follow-up survey, though this number is slightly reduced if we drop people with some missing values for control variables. The follow-up survey asked most of the outcome questions in the original survey, but did not include the informational treatment.

With a relatively low take-up rate, a concern is that follow-up respondents are differentially selected. Appendix Table 6 suggests that while some selection takes place (a marginal effect for whites and a more significant effect by age) the most important variables in terms of predicting preferences (support of Obama and overall liberal-versus-conservative policy views) show no differential selection into the follow-up sample. Nor does initial treatment status predict take-up and thus we have a roughly equal number of control and treatment observations in the follow-up sample.

We compare the original results for these 187 observations to their responses one month later for selected outcomes in Table 6 and for all other outcomes in Appendix Tables 7 and 8. As only some outcomes show a substantial initial treatment effect for the $N = 187$ subsample, it is not feasible to have meaningful tests of persistence for all outcomes.

Cols. (1) and (2) show that our most robust outcome result from the original survey—support for increasing the estate tax—is strongly persistent. In absolute terms, 64 percent of the effect size remains one month later, more than doubling the share who support the policy. And the effect one month later remains highly statistically significant.

Cols. (3) and (4) show similarly strong results for views on the proper scope of government. For the subsample, the treatment effect on this outcome is even larger than for the entire sample (col. 9 of Table 5). The follow-up result in col. (4) actually shows an increase in the point-estimate, though it is within the confidence interval of the result in col. (3).

The next four columns show results that (for the subsample) are sizable but not statistically significant in the initial survey and examine their persistence. As col. (5) shows, the initial treatment effect on "trust in government" is slightly larger than for the full sample, with most of the effect persisting one month later. Like the "scope of government" outcome, the initial effect on the top tax rate is slightly larger than for the full sample, and

in fact grows after one month (though, again, the relatively large standard errors mean the point-estimates are within each other's confidence intervals).

Unfortunately, as cols. (9) and (10) show, one of the main outcome variables from the omnibus survey—concern for inequality—yields an initial treatment effect of essentially zero for the subsample, and thus testing for persistence is not particularly meaningful. Given that our initial treatment often had small effects for the entire sample, it is not surprising that only some outcomes yield substantial initial treatment effects for the subsample. We thus relegate the follow-up results for all these outcomes to Appendix Tables 7 and 8.

Overall, the follow-up analysis shows, once again, that the estate tax emerges as the policy most robustly and significantly affected by our omnibus treatment.

Bounding the effects of differential attrition. While we showed in Table 3 that, conditional on finishing the survey, assignment to the treatment appears as good as random, here we further probe the potential effects of attrition. To create a demanding bounding exercise, we assume either that (1) attriters would have all had the average "liberal" view for each outcome; or (2) they would have had the average "conservative" view for each outcome. Given that attrition does not actually vary by political views (see Appendix Table 2) but outcome values vary substantially by political views (see Table 2), this test should provide generous upper bounds on the potential effects of attrition. The results in Appendix Table 9 shows that no signs flip for any of our main outcomes variables under either the conservative (columns labeled "C") or liberal (columns labeled "L") attrition assumptions.

Next, we examine how the level of differential attrition affects our results: do our results only hold in survey rounds with high differential attrition between the control and treatment group? The first three rounds of the omnibus surveys had very similar differential attrition rates (between 12 and 16 percent), whereas the fourth had a substantially lower attrition rate (five percent). Appendix Tables 10 and 11 show that our main results on concern for inequality, support for the estate tax, and trust in government are robust and at times stronger for the low-differential-attrition round, the round where we expect our identification assumptions to be most robust. As before, the "non-results" for other outcomes remain (not shown).[30] In Sections 5 and 6, we analyze follow-up surveys where the treatment is much shorter and

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[30]Note that comparing across high- and low-attrition rounds follows the current gold-standard in dealing with differential attrition issues: re-weighting the sample based on the propensity score for attrition using the Dinardo-Fortin-Lemieux (DFL) reweighing method. In our context, the only observable variable that is correlated with differential attrition is the round of the experiment. Hence, our comparison across rounds is the simplest and most transparent non-parametric form of DFL reweighting.

where there is virtually no differential attrition by treatment status (see Appendix Table 3). The fact that these follow-up surveys largely confirm our omnibus treatment results provides further re-assurance that differential attrition is not driving our results.

Robustness across rounds. As our rounds took place at different dates with different stories dominating the news cycle, we might worry that the treatment effects are being driven by a single round. We verified that dropping rounds one by one does not change the sign or significance of the main results.

Survey fatigue. Finally, "survey fatigue" would not seem to explain our results. For example, the question "is inequality a serious problem" comes before top tax rates, which precedes the estate tax question, our strongest effect. Therefore, there is no monotonic relationship between the strength of the treatment effect and the order of the outcome variables.[31]

5 Understanding our Results with Follow-up Surveys

The follow-up surveys share the following structure. While we repeated most of the outcome questions used in the omnibus surveys, there are some differences (based on input from referees and others). For example, we ask respondents to report whether "poverty is a serious problem," as well as rank "private charity" and "education" in a list of tools to address inequality (so as to gauge whether respondents react to the treatments by turning to options—some non-governmental—more often advocated by political conservatives).[32] We also replace the question about the EITC (which we feared might not be sufficiently familiar to respondents) with a general question about "aid to the poor" and a specific question about public housing, while retaining the minimum wage and food stamp questions.

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[31]A potential bias that is more difficult to measure is differential experimenter demand effects—perhaps it is the case that a variable such as "inequality is a serious problem" is more susceptible to demand effects than concrete policy questions such as "preferred top income tax rates." An indirect test is to examine gender differences by outcome variable, as women appear more likely to give the "desired" answer (see, e.g., Bernardi, 2006, Dalton and Ortegren, 2011 and citations therein). We find very small gender differences overall, and no pattern whereby they are larger for women for the "first-stage" outcomes (results available upon request). Recent work argues that demand effects are likely muted with internet surveys (see, e.g., Kreuter, Presser, and Tourangeau, 2008 and Gelder, Bretveld, and Roeleveld, 2010).

[32]For example, McCall and Kenworthy (2009) argue that Americans care about inequality but prefer policy levers such as education to combat it, not income redistribution. In our data, (control group) conservatives are indeed more likely to rank education and private charity above tools that more directly involve government redistribution.

For the sake of completeness, we used the same battery of outcome questions for all follow-up surveys, even when certain follow-up treatments were unlikely to affect a given outcome. For the sake of brevity and exposition, we only discuss in the main text those outcomes that are relevant to a given treatment, but all other outcomes are reported in the Appendix for each treatment.

Section 5.1 explores why the estate tax appears to be an anomaly, first verifying that the effect is robust to changes in presentation and then measuring the informational impact of our treatment. Section 5.2 explores potential mechanisms for why most other policies are more impervious to informational interventions.

5.1   Why are views about the estate tax so elastic to information?

In this section, we present two types of follow-up analysis on the estate tax. First, we verify whether the estate tax treatment effect—an outlier among the policy outcomes analyzed in the previous section—is truly robust. After showing that it withstands several significant modifications of the treatment, we then present evidence as to *why* this effect is so strong. Our view is that misinformation about the estate tax is far greater than for the other policies we surveyed, such that the informational treatment has an especially large impact.

Verifying the large estate tax effects. Recall that the omnibus treatment includes not only information about the incidence of the estate tax, but several other components as well (e.g., the interactive feature showing where respondents are in the income distribution, among the others we described in Section 2). To gauge the sensitivity of the estate tax effect to this additional information, we redid the experiment with a treatment that *only* included the slide on the estate tax.

Furthermore, the original estate tax treatment shows a picture of a mansion and notes that the estate tax can help "level the playing field" (see Appendix Figure 4). We thus formulated a treatment that decreased the emotional impact of the estate-tax treatment and that only mentions the incidence in dry, factual terms (see Appendix Figure 6). We call the first version the "emotional estate tax treatment" and the second the "neutral estate tax treatment." Again, neither of these treatments contains the other, non-estate-tax components of the omnibus treatment.

Table 7 displays results for the key outcome variables. In contrast to the omnibus treatment, the "emotional" estate tax treatment has no effect on views about whether inequality is a problem, whether it has increased, or whether the rich are "deserving" (cols. 1, 2 and

3). The "neutral" treatment appears to have countervailing effects: increasing concern for inequality (though this effect is much smaller than that of the omnibus treatment) while decreasing the sense that inequality has increased. This much weaker effects are not surprising because these two treatments provide no information about income inequality or its trends.

However, col. (4) shows that the effect of even these more limited treatments on opinions about the estate tax remain strong. The point-estimate for the "emotional treatment" is nearly as large as that of the omnibus treatment (0.289 versus 0.357). The "neutral" effect is smaller (0.109), though both in absolute and scaled terms swamps any policy effect (excluding the estate tax itself) associated with the omnibus treatment. Recall that the omnibus treatment provided extensive interactive and personalized information on income inequality and income tax rates and typically produced scaled effects on the income tax outcomes of ten percent. The "neutral" estate tax treatment consisted of a total of four sentences but nonetheless produced a scaled effect on the estate tax four times as large (40.8 percent of the liberal-conservative gap). This stark contrast highlights how much more elastic to information views about the estate tax are than those about the income tax and other policies.

As shown in col. (5), both treatments make respondents more likely to say they will petition their Senator (scaled effects greater than 0.2), but this effect is not statistically significant. The significant effect with the omnibus treatment suggests that the background information on growing inequality might be required to induce more respondents to connect their policy views with political activism. Col. (6) shows that the effect on trust in government is negative (as in the omnibus treatment) but not significant.[33]

Overall, the large effects of information about the estate tax remain robust after two major modifications to its presentation.

Why are estate tax preferences so malleable to information? At first, we attributed this finding to our treatment having larger effects for topics that held little ex-ante salience for our respondents. However, recent polling data suggests the estate tax is very salient to respondents—in 2010, Gallup respondents named averting an increase in the estate tax as their *top* priority for the lame-duck session of Congress, above extending unemployment benefits and the Bush income tax cuts.[34] Moreover, there were no more "missing" responses

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[33]Given that the omnibus treatment had, at best, small effects on income tax and transfer policies, it is not surprising that the estate-tax-only treatments do not produce consistently significant effects on these outcomes either (Appendix Table 12 and 13).

[34]See `http://www.gallup.com/poll/144899/Tax-Issues-Rank-Top-Priority-Lame-Duck-Congress.aspx`.

on the estate tax question than on other policies questions in the control group, further evidence that the estate tax is not an obscure issue to respondents in our sample.

A more promising explanation is that while respondents may view the estate tax as a salient issue, they may hold misinformed views on the topic. Indeed, as documented by Slemrod (2006), 82 percent of respondents favor estate tax repeal but 49 percent of respondents believe that most families have to pay it, compared to 31 percent who believe only a few families have to pay, and 20 percent who admit to not knowing. As a result, providing basic information on how the current federal estate tax is limited to the very wealthiest families might serve as a large informational shock.

We directly tested this hypothesis by adding a question on the incidence of the estate tax to the follow-up surveys. Respondents were asked to choose the share of decedents subject to the estate tax from among the following options: less then 1%, 1%, 10%, 20%, 40%, 60% and 100%. If anything, the greater detail offered for choices below twenty percent would seem to tip-off respondents that the answer is a small number, but only 12 percent of control group respondents answered correctly (random guessing would be correct 14 percent of the time) and accuracy varied substantially by political orientation (16 percent of liberals versus 6 percent of conservatives).[35]

Col. (7) of Table 7 shows the effect of the two estate tax treatments on respondents' tendency to choose the true incidence. The two treatments roughly triple the likelihood of answering correctly, strongly suggesting that information is a key mechanism behind the large effects of the omnibus treatment.[36] It is important to note that misinformation is not a sufficient condition for an informational treatment to have large effects. As noted earlier, Kuklinski et al. (2003) found that correcting substantially misinformed views on welfare was not sufficient to change respondents' support, though perhaps the lack of elasticity is due to the racial stereotypes the world "welfare" brings to mind (Gilens, 1996). Tthe estate tax may be one of a few issues on which voters are highly misinformed but their ignorance is not linked to racial or other stereotypes. In any case, extrapolating from the estate tax effects

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[35]Though it might be an artifact of offering more small than large choices interacting with random guessing, only 34 percent of our control respondents report that at least 40 percent of decedents pay the estate tax, suggesting that perhaps mTurkers are less misinformed than other survey takers such as those polled by Slemrod (2006). If true, this difference would suggest that the informational impact of the treatment might be even larger on a different sample.

[36]Our results offer experimental support for the observational regression analysis presented by Slemrod (2006) showing that support for the estate tax is lower when respondents believe that most families have to pay it.

would give vastly biased views of the ability of information to move other redistributive policy preferences, as we saw in the previous section and as we further document below.

5.2 Exploring the limited treatment effects on policy preferences

We explore three potential explanations for why the omnibus treatment had small policy-preference effects (aside from the estate tax). Of course, other explanations may exist, so one should not not view our analysis of the mechanisms behind the policy "non-results" as definitive or exhaustive.

5.2.1 Does government distrust explain limited treatment effects?

As documented in Section 4, the omnibus treatment significantly reduces trust in government. It is perhaps not surprising that an informational treatment emphasizing a dramatic increase in income inequality would lower respondents' view of government. But, to our knowledge, it remains an open question whether lowering trust in government has a causal effect on policy preferences. This question has perhaps never been more relevant in the U.S. context, given that Americans' trust in government is at historically low levels, as noted in the Introduction.

To test the causal effect of trust in government on policy preferences, we devised a treatment that lowers trust but does not affect views on other factors that might affect policy preferences. This task is not easy—as we saw with the omnibus treatment, primes about inequality reduce trust in government, but they also increase concern about inequality, meaning the omnibus treatment effects on the policy outcomes are the joint effect of increasing concern about inequality (which we hypothesize would increase support for government action) and reducing trust in government (which we hypothesize would decrease it).

We began by collecting a small pilot group ($N \approx 150$) on mTurk where we asked people to answer our basic trust question (how often they can trust the government to do what is right) but then to *explain* why they answered the way they did. Note that they answer this question directly after answering the demographic questions and are thus not being primed to think about inequality. There is no "treatment" in this pilot—we are merely asking people to explain their views.

The pilot group provided a fascinating look into why trust in government is currently so low. Respondents feel politicians are out to enrich themselves and their wealthiest donors. "Money," "corporations" and "special interests" are some of the most commonly used words and phrases in these answers, as Appendix Figure 7 shows. The detailed descriptions given by

respondents allowed us to develop primes we thought could lower trust in government without necessarily affecting other factors that would have a direct effect on policy preferences.

Our treatment consists of several multiple-choice questions that induce respondents to reflect on aspects of government they dislike. For example, we asked if they agree that "Politicians in Washington work to enrich themselves and their largest contributors, instead of working for the benefit of the majority of citizens" (90% do). We also showed them results from a ranking of OECD countries in terms of government transparency in which the US was categorized in the bottom quartile (see Appendix Figure 8 for a screenshot and Appendix A for the full description).

Results. Table 8 shows the results of this treatment on a variety of outcomes. Col. (1) shows that the first stage "works"—the treatment significantly decreases respondents' stated trust in government, by 5.8 percentage points in absolute terms or 1.78 times the liberal-conservative gap on this question. This effect is slightly larger than the effect of the omnibus treatment (roughly 1.1 times the liberal-conservative gap), not surprising given that the goal of this treatment was to lower trust. As we saw with the omnibus treatment, respondents appear to separate how much they trust the government with what they view as its proper scope, as the treatment has no effect on that outcome (col. 2). The treatment makes them more likely to view the government as wasteful, but the effect is not significant (col. 3).

Cols. (4) through (6) suggest that we were largely successful in devising a treatment that isolates the effect of trust, at least with respect to our standard questions on income inequality and poverty. There is a marginal effect of the treatment in increasing concern about inequality, but no effect on the sense it has increased or the sense that poverty is a problem. The results from the omnibus treatment suggest that, if anything, the uptick in concern about inequality should have a mildly positive effect on treatment respondents' tendency to support redistributive policies. As such, it would mask the effects of decreasing trust in government on support for redistributive policies, which we hypothesize to be negative.

Table 9 displays those results. The treatment decreases support for a tax on millionaires, though this result is not quite statistically significant (when the continuous top tax rate is used instead as the outcome, the coefficient is positive but essentially zero, see Appendix Tables 14 and 15 for this and other results not discussed in the main text). While stated support for expanding the estate tax is essentially unchanged by the treatment, the stated willingness to petition for its expansion is significantly reduced (a scaled effect of 0.588). Taken together, cols. (1) through (3) suggest that the treatment slightly reduces support for

sending more money (even the money of the well-off) to Washington.

The estimated effects of trust on support for transfer programs to the poor are much less equivocal. While support for the minimum wage is unaffected (col. 4), treatment respondents significantly reduce their support for "aid to the poor" generally (col. 5), and food stamps and public housing specifically (cols. 6 and 7). Finally, some interesting results emerge when respondents are asked to rank a list of options for addressing inequality (a higher number here means more support). Col. (8) shows the treatment causes respondents to elevate a non-governmental solution to inequality—private charity. Col. (9) shows a similar (though statistically insignificant) effect for education, which, as noted at the beginning of the Section, is generally preferred by more conservative respondents.

Discussion. Decreasing respondents' trust in government appears to have a strong, negative effect on support for direct government transfers. As further support for the trust mechanism, the treatment has no effect on support for the minimum wage, which is an *indirect* transfer that does not involve the government receiving and redistributing tax dollars. Recall also that the omnibus treatment failed to increase support for direct transfer programs (the EITC and food stamps) but did increase support for the minimum wage. As Table 9 shows that support for the minimum wage appears unaffected by changes in trust, trust emerges as a plausible mediating variable that can explain the pattern of results for the omnibus treatment.

5.2.2 Emotional versus factual appeals

There is a long psychology literature that suggests that for many subjects, emotional appeals produce larger changes in attitudes than more factual presentations.[37] Indeed the estate tax follow up experiment described in Section 5.1 showed that the neutral treatment had a smaller effect than the emotional treatment. While our omnibus treatment provided extensive interactive and personalized information, it was mostly numeric in nature, which may have limited its ability to move policy preferences. Similarly, the focus on the "top one percent" might be less effective than focusing more intensely on the bottom of the distribution.

To test this idea, we developed a treatment meant to create empathy between the respondent and low-income families. Again, the treatment was personalized and interactive. For example, we asked respondents to "[t]hink about a family of X_1 with X_2 parent(s) working full time...and X_3 kids....What would be the minimal monthly expenses that such a family

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[37]See, e.g., Edwards (1990), Rosselli, Skelly, and Mackie (1995), Loef, Antonides, and Raaij (2001), Huddy and Gunnthorsdottir (2000), and citations therein.

would have to make to afford living where you live?" The values $X_1, X_2,$ and X_3 were interactively matched to the household composition that the respondent earlier gave in the demographic module at the start of the survey. The respondent then filled out dollar amounts for monthly rent, utilities, transportation, food, and expenses related to children. We then showed them how the budget they devised compared with the income at the poverty line (based on the respondents' household size), emphasizing to them that the budget did not even include items such as health care, clothing, furniture, and costs related to schooling (see the Appendix for a complete description of the treatment and Appendix Figure 9 for a screenshot).[38] The slides with this information also included photos of low-income families.

Results. Overall, the results track very closely to those from the original omnibus treatment. We obtain large "first stage" effects on concern for poverty and inequality, but little movement on policy preferences.

Table 10 presents the key outcomes. The treatment has significant effects on concern about inequality (col. 1), and, not surprisingly, large effects on whether poverty is a serious problem (equal to over 30 percent of the political gap for this outcome).[39] However, just like the omnibus treatment, this follow-up "emotional" treatment has limited effect on policy preferences. Of the four poverty-policy questions we asked, only one exhibits a significant treatment effect (food stamps, and even then just ten percent of the political gap). Similar to the omnibus treatment, this follow-up treatment reduces trust in government, though the effect is smaller and not significant.[40]

Discussion. As readers can see from experiencing these two treatments, the omnibus treatment and this "emotional" follow-up are very different in spirit. The omnibus treatment focused largely on the top one percent and was more factual in nature, whereas the follow-up treatment focused on the disadvantaged and sought to create empathy both with our "put yourselves in their shoes" exercise as well as photographs of low-income families.

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[38]The large majority (76 percent) devise a budget in excess of the income at the poverty line for a household of their type.

[39]As detailed in Appendix Table 16, the treatment increased the likelihood of correctly choosing the actual poverty rate from multiple choices, though, because many control respondents over-estimated the poverty rate, it did not on net increase their estimate of the poverty rate. The fact that the treatment had such a large effect on perceiving poverty as a "very serious problem" likely works through the intensive margin: perhaps through the creation of empathy, the treatment highlights how difficult it would be to manage with limited income.

[40]The treatment had very small but positive effects on taxes on the well-off, always below ten percent of the political gap and typically not significant. See Appendix Tables 16 and 17 for these and all other outcomes not displayed in Table 10.

Despite these stark differences, the results are very similar. It is relatively easy for treatments to affect how much individuals are "concerned" with any issue, but much harder to increase their support for policies that would seem directly related to addressing said issues. Our final follow-up survey attempts to make the connection to policy measures more explicit.

5.2.3 Connecting "concerns" with policy measures

Bartels (2005) documents the seemingly odd result that even though the individuals in his 2002 sample appeared worried about inequality and aware that the tax cuts proposed by the Bush Administration in 2001 favored the wealthy, they still supported them by a large margin. He concludes that "Americans support tax cuts not because they are indifferent to economic inequality, but because they largely fail to connect inequality and public policy."

We directly test this notion—that respondents do not connect their "concerns" with policies meant to address them—in our final follow-up survey. In this version, we largely repeat the low-income "emotional" treatment described in Section 5.2.2, but also add slides showing how current government programs help these households. First, after entering in the expenses in the budget exercise, the treatment describes a family earning one full-time full-year minimum-wage income, making a salient connection between the level of the minimum wage and family income at the bottom of the income distribution. Second, respondents are told that "The food stamps program helps many low income families, such as those earning only one minimum wage. It provides $150/month per person to help with food expenses." Hence, the connection between poverty and a government program is made explicit. Appendix Figure 10 provides a screenshot.

Results. We repeat the results for the key outcome variables in Table 11. The "first-stage" effects of this treatment are smaller and not significant. It is not surprising that we do not see much movement in variables related to inequality, since the treatment did not provide any direct information on the topic. Despite the focus on the situation of a low-income household, treatment respondents do not view poverty as a more serious problem. We speculate that emphasizing the efficacy of a government poverty program might have the effect of making poverty and inequality seem less severe.

Despite the somewhat smaller first-stage effects, the effects on our policy outcomes are consistently positive and significant. Support rises for the minimum wage (col. 4), food stamps (col. 6), and public housing (col. 7). The effect on "aid to the poor" is positive but not significant. It should be noted, however, that with the exception of the minimum wage,

these effects are still relatively small (roughly about ten to fifteen percent of the political gap). It is also the case that the treatment does not consistently increase support for actually sending money to Washington to pay for these policies: the effects on income and estate taxes for the well-off are mixed (see Appendix Tables 18 and 19).

However, it does appear that the interaction of the empathic portrayal of low-income families along with a prime emphasizing the efficacy of a transfer program has a meaningful effect on the policies respondents support. Col. (8) suggests that, in contrast to the trust treatment, this treatment reduces the relative attractiveness of the non-governmental solution to inequality: private charity. The effect on support for education over more direct redistribution is in the same direction, but not statistically significant (col. 9). However, as col. (10) shows, the effect of this treatment on trust in government remains negative: respondents apparently still in part blame the government after being primed to think about the challenges facing low-income families, though the effect is only marginally significant.

Discussion. While this treatment indeed moved policy preferences, it is worth noting its highly explicit nature. After completing an exercise where they contemplated the budget constraints of a low-income family, respondents were shown in concrete terms how a government program helps such a household make ends meet. Even then, while support for many poverty-related programs significantly increased, the largest effect remains the minimum wage, an *indirect* transfer program. Moreover, the treatment does not have a consistent effect in terms of inducing treatment participants to desire higher taxes (even if only on the wealthy) to pay for these programs.

Taken together, the results from these three follow-up surveys suggest the difficulty in moving most policy preferences. While concern for an issue is highly elastic to information, translating this concern into a change in policy preference appears very difficult, with the consistent exception of the estate tax.

6   Conclusion

Standard models predict that support for redistribution should increase with income inequality, yet there has been little evidence of greater demand for redistribution over the past thirty years in the United States—despite historic increases in income concentration. A possible explanation is that people are unaware of the increase in inequality, such that greater information would substantially move redistributive preferences. We gather over 10,000 observations using Amazon's Mechanical Turk to conduct a series of survey experiments to

extensively explore this hypothesis.

Our results suggest that, generally, greater information can increase respondents' sense of concern about an issue, but not necessarily their support for policies that might ameliorate it. Information about income inequality and poverty has only a limited (and typically statistically insignificant) ability to increase support for higher income taxes on the well-off or transfer programs for the disadvantaged.

We present evidence that extreme distrust of government appears to explain part of this null effect. First, trust in government is very low in our sample, as it currently is among Americans more generally. Second, many of our treatments appear to further reduce this already low level of trust. Third, and most novel relative to the literature, we show that decreasing trust has a causal effect on diminishing support for redistribution. We develop a prime that reduces trust in government without significantly moving respondents' concern for inequality and poverty: respondents exposed to this prime significantly reduce their support for programs that involve the government directly redistributing tax dollars, while increasing their support for non-governmental solutions such as private charity.

Only when we explicitly show individuals the concrete effects of government poverty policies do we observe consistent, statistically significant increases in support for such policies. Even so, such effects are small, and are largest for indirect transfer programs such as the minimum wage that do not involve the government collecting and redistributing tax dollars.

Future work might further probe the connection between government trust and policy preferences. Underlying mistrust might help explain the reluctance to support policies that would seem to be in the majority's self-interest. Relatedly, distrust could explain why minimum wage hikes typically enjoy 70-80 percent support in surveys. Many economists assume that respondents simply misunderstand the incidence of the minimum wage. Instead, it might be the case that they view the minimum wage as imperfect, but better than other redistributive policies that involve sending money to Washington.

As we extensively document, the estate tax is the exception to the generally small effects of information on policy: even a four-sentence description providing information on its incidence significantly increases support for the policy. At least part of this effect appears due to vast misinformation—many respondents both in our survey and past work on the estate tax believe a majority of families are subject to it, when in fact roughly 0.1% of decedents currently pay it.

It remains an open question if misinformation fully explains the difference. For example,

Americans might view the moral claims to inheritance versus income differently. If the goal of the estate tax is to prevent the self-perpetuation of extreme wealth, then respondents might still support it even if, say, the government merely burns the money it collects. Therefore, low levels of trust in government may not inhibit support for the estate tax as much as for other policies where efficiency is a more salient goal. Independent of the origin of the treatment effect, the large elasticity of support for the estate tax in response to basic information that we and past work have documented is highly policy-relevant, given the recent rise in inheritance flows in many developed countries.[41]

Randomized online surveys are a powerful and convenient tool for studying the effects of information treatments on attitudes and behaviors, one we imagine can be used to extend the results we have documented. The tool is powerful because it can reach large samples of U.S. residents (in the thousands) at fairly low cost ($1-$2 per respondent). It is convenient because, using widely available software, online surveys are now very easy to design. Hence, it becomes more feasible to explore mechanisms behind results. For example, we were able to easily design companion experiments to test mechanisms potentially underlying the effects in our original, omnibus treatment. Therefore, in contrast to field experiments which are very costly to set-up and replicate, online survey experiments lend themselves naturally to conducting series of experiments where results from an initial experiment lead to new experiments to cast light on potential mechanisms.

Such flexibility will allow researchers to gain a more nuanced understanding of redistributive preferences. Largely due to the growth of Medicare costs, the U.S. government faces a long-run fiscal imbalance that will require raising taxes, cutting spending, or both.[42] European countries face similar challenges. The distributional effects of this future fiscal rebalancing will depend in large part on voters' redistributive preferences, how strongly they hold them, and whether they act on them. As such, these questions are of first-order importance in public economics. We believe that the methodology we employed in this paper can be used in future research to better understand how individuals form—and change—their redistributive preferences.

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[41] See Piketty (2014), Ch. 11. Unfortunately, data limitations make it difficult to know whether this trend applies specifically to the United States.

[42] See, e.g., the Congressional Budget Office's projections (www.cbo.gov/publication/43288).

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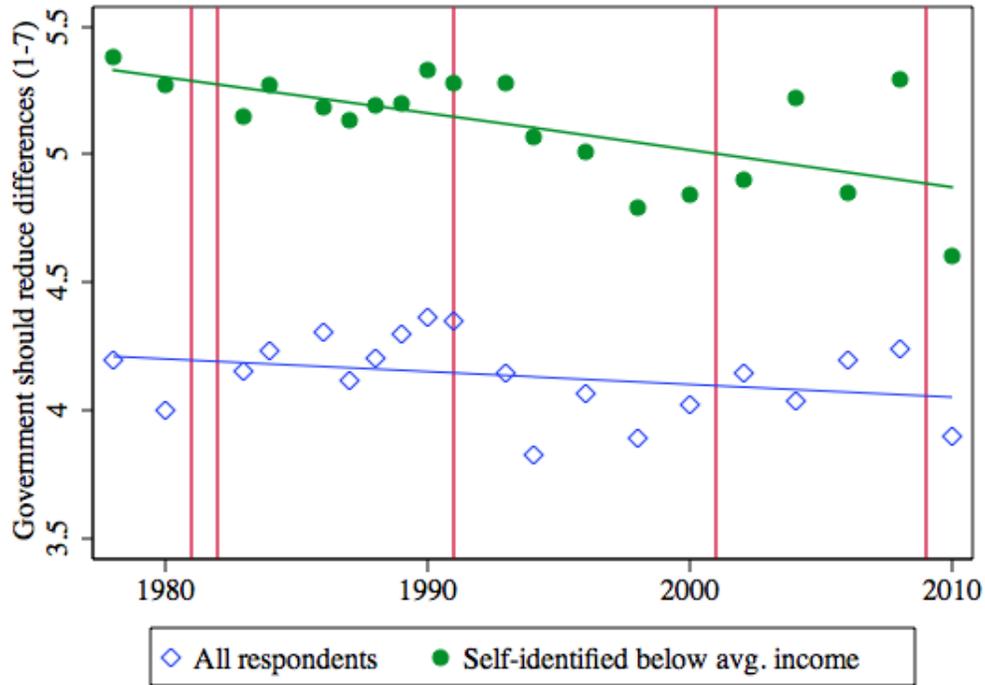
Figure 1: The government should reduce income differences (scale from 1–7, GSS)

This figure depicts responses since 1978 in the US General Social Survey (GSS) on whether the government should reduce income differences. The empty diamond series is for all respondents while the full circle series if for respondents with below average income. Regression fits are depicted for each series. The graph uses the *eqwlth* variable from the GSS (though subtracts it from eight so that it is increasing in support for redistribution).

Table 1: Summary statistics and comparison to other polling and online data

	(1) mTurk sample	(2) CBS election poll	(3) American Life Panel
Male	0.428	0.476	0.417
Age	35.41	48.99	48.94
White (non-Hisp)	0.778	0.739	0.676
Black	0.0756	0.116	0.109
Hispanic	0.0444	0.0983	0.180
Other racial/ethnic group	0.0759	0.0209	0.0410
Employed (full or part)	0.465	0.587	0.557
Unemployed	0.123	0.104	0.103
Married	0.397	0.594	0.608
Has college degree	0.433	0.318	0.309
Voted for Obama	0.675	0.555	0.559
Political views, cons. (1) to lib. (3)	2.176	1.586	
Observations	3741	808	1002

Notes: This table displays summary statistics from our mTurk omnibus surveys in column (1) along with (weighted) averages based on a 2011 CBS news survey in column (2) and RAND's online American Life Panel (ALP) in column (3). We are grateful to Ray Fisman for providing us with summary statistics from the ALP.

Table 2: Summary statistics for the Control Group, split by Liberals and Conservatives

	(1) Liberals	(2) Conservatives	(3) All
Male	0.407	0.472	0.422
Age	32.618	39.823	35.557
White	0.752	0.838	0.776
Black	0.090	0.063	0.085
Hispanic	0.039	0.027	0.037
Asian	0.090	0.053	0.078
Married	0.302	0.543	0.402
Has college degree	0.462	0.455	0.430
Unemployed	0.140	0.076	0.121
Not in labor force	0.093	0.208	0.144
Voted for Obama in 2008	0.914	0.303	0.674
Ineq. has increased	0.836	0.615	0.738
Ineq. v. serious	0.414	0.129	0.285
Top Tax Rate	34.181	23.996	30.205
Increase Millionaire Tax	0.904	0.469	0.745
Increase Estate Tax	0.254	0.080	0.171
Increase min wage	0.822	0.496	0.690
Support food stamps	0.850	0.446	0.686
Support EITC	0.722	0.418	0.611
Trust Govt	0.168	0.145	0.155
Purpose of Govt is broad	3.552	2.349	3.076
Said would petition for higher inc taxes (early rounds only)	0.288	0.118	0.238
Send petition for high estate tax	0.305	0.141	0.234
Vote democrat 2012	0.800	0.182	0.529
Observations	821	475	1976

Notes: This table displays summary statistics based on control respondents from the omnibus surveys, stratified by self-reported liberal vs. conservative status (on a five-point scale, very liberal, liberal, center of the road, conservative, very conservative). Column (1) is for liberals (less than three on the scale) while column (2) is for conservatives (more than three on the scale). Col. (3) shows summary statistics for the entire control group. The complete wording of these survey questions is reported in the Appendix.

Table 3: Ability of covariates to predict treatment status, conditional on finishing the survey

Variable	Coefficient	P-value
Voted for Obama in 2008	0.003	0.856
Age	-0.001	0.479
Liberal policy view	0.002	0.751
Household income	0.005	0.109
Married	-0.013	0.434
Education	-0.003	0.575
Male	0.013	0.447
Black	-0.066	0.031
Hispanic	0.091	0.021
Native	-0.043	0.201
Employed full time	-0.012	0.502
Unemployed	0.015	0.539
Not in labor force	0.021	0.376
Student	-0.027	0.235

Notes: For each row, the coefficient and p-value are from regressions of the form $Assigned\ to\ treatment_{ir} = \alpha + \beta Covariate_i + \delta_r + \epsilon_{ir}$, where $Covariate$ is listed to the left in the row and δ_r are survey round fixed effects. Those tests are used to detect selective attrition (as treatment respondents are approximately ten percentage points less likely to complete the omnibus survey than are control respondents, see appendix Table 3). If we regress treatment status jointly on all covariates and survey round fixed effects, we obtain a p-value for joint significance of 0.12.

Table 4: Effect of omnibus treatment on opinions about inequality ("first-stage" outcomes)

	Ineq. v. serious		Ineq. increased		Rich deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.102***	0.104***	0.119***	0.120***	-0.0500***	-0.0526***
	[0.0154]	[0.0144]	[0.0130]	[0.0128]	[0.0119]	[0.0114]
Cont gp. mean	0.285	0.285	0.738	0.738	0.180	0.180
Scaled Effect	0.357	0.365	0.539	0.540	0.173	0.182
Covariates?	No	Yes	No	Yes	No	Yes
Obs.	3703	3703	3704	3704	3690	3690

Notes: The three outcome variables are binary indicator variables, coded as one if the respondent says that "inequality is a very serious problem," "inequality has increased," and "the rich are deserving of their income," respectively. All regressions have round fixed effects, even those labeled as including "no" covariates. Controls for covariates further include all variables in the randomization table (Table 3), plus state-of-residence fixed effects. "Scaled effect" is the coefficient on $Treated$ divided by the difference between control group liberals and conservatives. The row "Cont pg. mean" reports the mean of the outcome variable for the entire control group. $^*p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01$

Table 5: Effect of omnibus treatment on policy preferences

	Top rate	$1M tax	Estate	Petition	Min. wage	Food st.	EITC	Trust	Scope	Dem 2012
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.931*	0.0463***	0.357***	0.0648***	0.0325**	0.0149	0.0212	-0.0253**	0.132***	0.0152
	[0.549]	[0.0125]	[0.0140]	[0.0156]	[0.0141]	[0.0141]	[0.0151]	[0.0112]	[0.0339]	[0.0125]
Cont gp. mean	30.21	0.745	0.171	0.234	0.690	0.686	0.611	0.155	3.076	0.529
Scaled Effect	0.0914	0.106	2.043	0.394	0.0995	0.0369	0.0698	1.105	0.110	0.0246
Obs.	3741	3741	3673	3060	3690	3690	3690	3739	3704	3703

Notes: "Top rate" is continuous (respondents' preferred average tax rate on the richest one percent). "Scope" is also continuous (a 1-5 variable, increasing in the preferred scope of government activities). All other variables are binary. "$1M tax" and "Estate" indicate the respondent wants income taxes on millionaires and the estate tax to increase, respectively. "Petition" indicates she would write her Senator to increase the estate tax. "Min wage," "Food st." and "EITC" indicate support for increasing the minimum wage, and funding for food stamps (SNAP) and EITC, respectively. "Trust" indicates trust in government and "Dem 2012" indicates the respondent plans to vote for the Democrat (Obama) in the 2012 Presidential election. "Covariates" and "scaled effects" are as specified in the notes to Table 4. The row "Cont pg. mean" reports the mean of the outcome variable in the control group. All regressions in this and subsequent tables include control variables as defined in Table 4. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 6: Results from the follow-up survey one month after

	Increase Estate Tax		Govt scope		Trust govt		Ineq v. Serious		Top tax	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	First	Follow-up	First	Follow-up	First	Follow-up	First	Follow-up	First	Follow-up
Treated	0.295***	0.177**	0.329*	0.365*	-0.0490	-0.0339	2.213	3.373	-0.00715	0.0407
	[0.0878]	[0.0845]	[0.187]	[0.186]	[0.0576]	[0.0532]	[3.169]	[3.239]	[0.0708]	[0.0741]
Cont gp. mean	0.181	0.184	2.995	2.874	0.123	0.126	32.61	29.99	0.288	0.230
Obs.	168	168	168	168	168	168	167	167	169	169

Notes: All outcomes and terms are as defined in Tables 4 and 5. For each dependent variable, Column "First" is the result from the first survey, while Column "Follow-up" is the result from the follow-up survey one month after the initial survey. We use a more limited set of control variables given the small sample size. All regressions are run on the subsample of respondents who accepted to do the follow up survey. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 7: Results from the estate tax survey

	Ineq. v. ser.	Ineq. inc.	Deserving	Estate tax	Petition	Trust	Estate tax corr.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated (Emotional)	0.0381	-0.00239	-0.0312	0.289***	0.0313	-0.0164	0.316***
	[0.0258]	[0.0243]	[0.0328]	[0.0258]	[0.0208]	[0.0205]	[0.0263]
Treated (Neutral)	0.0511**	-0.0501**	-0.0429	0.109***	0.0239	-0.00558	0.375***
	[0.0259]	[0.0244]	[0.0329]	[0.0259]	[0.0209]	[0.0205]	[0.0264]
Cont gp. mean	0.307	0.771	1.997	0.210	0.132	0.153	0.120
Scaled Emot. Effect	0.118	0.0106	0.0818	1.085	0.265	0.235	3.386
Scaled Neutral Effect	0.159	0.223	0.113	0.408	0.202	0.0803	4.014
Obs.	1777	1777	1777	1777	1762	1756	1773

Notes: The "emotional" treatment repeats the estate tax slide from the omnibus treatment, but eliminates the rest of the treatment. The "neutral" treatment is a version of the "emotional" treatment that attempts to remove any framing effects or emotional appeals to focus solely on the information. The outcomes in cols. (1) through (6) are as defined in Tables 4 and 5. "Estate tax corr." indicates that the respondent chose the correct multiple-choice outcome for a question asking what share of estates are subject to the estate tax. $^*p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 8: Effect of negative trust prime on "first stage" variables

	Trust	Scope	Efficient	Ineq. v. ser.	Ineq. inc.	Pov. v. ser.
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.0582***	0.0236	-0.0278	0.0547*	0.0119	-0.00257
	[0.0203]	[0.0688]	[0.0346]	[0.0311]	[0.0289]	[0.0313]
Cont gp. mean	0.125	3.031	1.423	0.343	0.755	0.383
Scaled Trust Effect	1.730	0.0170	0.109	0.182	0.341	0.00828
Obs.	899	899	898	899	899	899

Notes: The negative trust prime treatment consists of several multiple-choice questions that made respondents reflect on aspects of government they dislike. For outcomes, "Efficient" is taken from a 1-3 scale of how much respondents think the government wastes money (we "flip" it so that it is increasing with perceived government efficiency). "Pov. v. ser." is an indicator variable for whether the respondent views poverty as a very serious problem. All other outcomes are as defined previously. $^*p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 9: Effect of negative trust prime on outcome variables

	$1M tax	Est. tax	Petition	Min. wage	Aid poor	Food st.	Housing	Priv. char.	Educ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	-0.0421	-0.00168	-0.0602**	-0.00428	-0.139**	-0.153**	-0.163***	0.187**	0.0615
	[0.0275]	[0.0266]	[0.0236]	[0.0902]	[0.0616]	[0.0673]	[0.0614]	[0.0791]	[0.0885]
Cont gp. mean	0.722	0.204	0.174	2.673	2.675	2.454	2.581	1.800	3.732
Scaled Trust Effect	0.0949	0.00728	0.580	0.00531	0.128	0.119	0.133	0.169	0.265
Obs.	899	895	899	899	899	899	899	850	874

Notes: The negative trust prime treatment consists of several multiple-choice questions that made respondents reflect on aspects of government they dislike. Outcome variables are defined as follows. "Aid poor" is a 1-4 categorical variable increasing in support for programs that aid poor households. "Housing" is a 1-4 categorical variable increase in support for funding public housing programs. "Priv. char." is an indicator of where (among a list of five policy approaches) the respondent puts "private charity" as a preferred method for addressing inequality (the variable increases in relative support for private charity with respect to the other outcomes). "Educ." is defined in a parallel manner for education spending. All other outcomes are as defined previously. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 10: Effect of "emotional" treatment on outcome variables

	Ineq. v. ser.	Ineq. inc.	Pov. v. ser.	Min. wage	Aid poor	Food st.	Housing	Trust gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.0778***	0.0374	0.0971***	0.0280	0.147	0.189*	0.0473	-0.0132
	[0.0278]	[0.0256]	[0.0315]	[0.0993]	[0.0936]	[0.101]	[0.0944]	[0.0215]
Cont gp. mean	0.307	0.771	0.316	2.529	2.127	1.811	2.064	0.144
Scaled Poverty Effect	0.211	0.179	0.293	0.0283	0.0689	0.0973	0.0249	0.264
Obs.	1825	1824	1204	899	899	899	899	1825

Notes: The "emotional" treatment aimed at creating empathy between the respondent and families living in poverty. Respondents were told about poverty rates and had to fill out a minimum budget for a family like theirs living in the same city. Respondents were then shown how their minimum budget compared to the poverty line. All outcomes are as defined previously. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 11: Effect of policy treatment on outcome variables

	Ineq. v. ser.	Ineq. inc.	Pov. v. ser.	Min. wage	Aid poor	Food st.	Housing	Priv. char.	Educ.	Trust gov.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.0408	0.0289	-0.00872	0.304***	0.149	0.299***	0.244***	-0.130*	-0.117	-0.0349*
	[0.0268]	[0.0255]	[0.0269]	[0.0960]	[0.0909]	[0.0977]	[0.0906]	[0.0704]	[0.0782]	[0.0206]
Cont gp. mean	0.307	0.771	0.316	2.529	2.127	1.811	2.064	2.026	3.728	0.144
Scaled Policy Effect	0.111	0.138	0.0263	0.308	0.0698	0.154	0.128	0.0731	0.155	0.698
Obs.	1835	1834	1315	906	906	906	906	1266	1285	1835

Notes: The policy treatment aimed at creating empathy between the respondent and families living on a minimum wage. Respondents had to fill out a minimum budget for a family like theirs living in the same city. Respondents were then shown how their minimum budget compared to the minimum wage and how food stamps adds \$150 per person/month to the budget of such a family. All outcomes are as defined previously. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Online Appendix (Not for Publication)

A Detailed Description of the Experiment

A.1 Omnibus Treatment Description

This Section describes in detail the omnibus experiment. The omnibus treatment experiment was carried out in 4 separate rounds from January 2011 to August 2012. The structure of the experiment was as follows:

(1) Background socio-economic questions including age, gender, race, marital status, children, state of residence, education, household income, work status, whether the respondent considers himself conservative or liberal on economic policy, voting choice in 2008 presidential election.

(2) Randomized treatment showing information on inequality, the estate tax, and tax policy shown solely to the treatment group. Those treatments are illustrated through screenshots in appendix Figures 1, 2, 3, and 4.

(3) Set of questions on inequality, taxes and transfers, policy views and 2012 voting plans. Those questions are listed in detail after the screenshots.

Surveys were openly posted on mTurk and their description stated that they would pay $1.50 for approximately 15 minutes of survey time, i.e., a $6 hourly wage. People were free to drop out any time or take up to one hour to answer all questions.

Round 3 was conducted with CT Marketing Group instead of mTurk at a cost of $5 per respondent. The survey software remained exactly the same.

The link to this survey is: `https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`

List of all Outcome Questions

Note: Sentences in italic were not seen by the respondents. Bold fonts are just labels for each section. Sentences in normal font are exactly as they appeared in the survey.

Tax rate questions:

1. *Choose the tax rate on the top 1%, next 9%, next 40% and bottom 50% (see screenshot).*

2. Do you think top income tax rates were higher in the 1950s and 1960s than they are today?

3. As you may know, there have been proposals recently to decrease the federal deficit by raising income taxes on millionaires. Do you think income taxes on millionaires should be increased, stay the same or decreased?

4. The Federal Estate tax (also known as the Death Tax) is a tax imposed on the transfer of wealth from a deceased person to his or her heirs. Do you think the Federal Estate tax should be decreased, left as is or increased?

First stage questions on knowledge and perceptions of inequality:

5. Do you think annual economic growth was faster in the period 1980-2010 than in the period 1933-1980?

6. Do you think inequality is a serious problem in America?

7. Do you think income inequality in the US has increased or decreased in recent decades?

8. Are you satisfied with your current income?

9. Do you think that the very high earners in our society deserve their high incomes?

10. Which statement do you agree with most? (A: "One's income and position in society is mostly the result of one's individual effort"/ B:"One's income and position in society is to a large extent the outcome of elements outside of one's control (for example, including but not limited to family background, luck, health issues, etc..)?

Political Outcomes:

11. Which party do you plan to support in the 2012 presidential elections?

12. How much of the time do you think you can trust government in Washington to do what is right? (Just about always/Most of the time/ Only some of the time/ Never)

13. Next, think more broadly about the purpose of government. Where would you rate yourself on a scale of 1 to 5, where 1 means you think the government should do only those things necessary to provide the most basic government functions, and 5 means you think the government should take active steps in every area it can to try and improve the lives of its citizens?

14. What do you think the most important goal of the federal income tax should be? (Raise money for infrastructure projects such as roads and bridges/ Raise money for universal socia services such as Social Security and Medicare/ Raise money from the wealthiest citizens to support programs that aid low-income citizens, such as Medicaid and Food Stamps).

Poverty Reduction Policies Outcomes:

15. The minimum wage is currently $7.25 per hour. Do you think it should be decreased, stay the same or increased?

16. Do you support or oppose the Earned Income Tax Credit (EITC) program?

17. Do you support or oppose the Food Stamps program?

18. In the next month, do you plan to donate any money to an organization that supports those in need?

19. In the past month, have you donated any money to an organization that supports those in need?

20. In the next month, do you plan to donate some of your time to an organization that supports those in need?

21. In the past month, have you donated some of your time to an organization that supports those in need?

Real Outcome: Petition for Estate Tax

Writing to the Senators of your state gives you an opportunity to influence taxation policy. Few citizens email their elected officials, therefore Senators and their staff take such emails from their constituents very seriously. If you would like to write to your Senator, go to the official US Senate list and click on your Senator's contact webpage. Two sample letters are provided below, one asking for higher estate taxes on the rich, one asking not to increase estate taxes on the rich. Feel free to cut-and-paste and edit the text before sending it to your Senator. Most Senators' websites ask for your name and address to avoid spam. We are not able to record what you write on the external (Senator's) website, so your letter and private information are kept fully confidential.

For the purpose of our survey, we would just like to know from you: I sent or will send an email to my Senator asking for higher estate taxes on the rich/ I sent or will send an email to my Senator asking to not increase estate taxes on the rich/ I do not want to email my Senator

Sample letter for higher estate taxes on the rich: Dear Senator, In the coming months as you debate the federal budget, one of the priorities for Congress should be raising estate taxes on the wealthiest Americans so that they pay their fair share to fund government programs and help solve our federal budget deficit problem.

Sample letter for not increasing estate taxes on the rich: Dear Senator, In the coming months as you debate the federal budget, one of the priorities for Congress should be keeping estate taxes on the wealthiest Americans low. The government should not punish people who are financially successful or well-off.

A.2 Estate Tax Experiment Description

The follow up estate tax experiment was conducted in March 2014 on a sample of 1800 respondents. Respondents were randomized into one of two treatment groups, or into the

control group. The first treatment group saw the same screen as in the omnibus treatment (only the part regarding the estate tax), as in appendix Figure 4. The second group saw a plain explanation for the estate tax, as in appendix Figure 6.

The link to this experiment is: `https://hbs.qualtrics.com/SE/?SID=SV_0vOecp2MDPBpInb`

List of all Outcome Questions

Note: Sentences in italic were not seen by the respondents. Bold fonts are just labels for each section. Sentences in normal font are exactly as they appeared in the survey.

1. *Choose the tax rate on the top 1%, next 9%, next 40% and bottom 50% (see screenshot).*
2. Do you think income inequality in the United States has increased or decreased since 1980?
3. Do you think inequality is a serious problem in America?
4. The Federal Estate Tax is a tax imposed on the transfer of wealth from a deceased person to his or her heirs. What percentage of people who die have to pay the Federal Estate tax? If you don't know, just give your best guess.
5. Do you think poverty is a serious problem in America?
6. Do you think that the very high earners in our society deserve their high incomes?
7. Do you think the federal estate tax should be decreased, left as is or increased? (Recall that the federal estate tax is a tax imposed on the transfer of wealth from a deceased person to his or her heirs.)
8. As you may know, there have been proposals recently to decrease the federal deficit by raising income taxes on millionaires. Do you think income taxes on millionaires should be increased, stay the same or decreased?
9. Which of the tools below do you consider the best to address inequality in the United States? Please drag and drop the items to the box on the right and rank them in your preferred order. Your preferred method for addressing inequality should be at the top, your least preferred one at the bottom. [Education Policies, Private Charity, Progressive Taxes, Government Transfers (e.g., food stamps, Medicaid,..), Government regulation (e.g., min wage, caps on top compensation,...)]
10. Should the federal government increase or decrease spending on aid to the poor?
11. The federal minimum wage is currently $7.25 per hour. Do you think it should be decreased, stay the same or increased?
12. Should the federal government increase or decrease its spending on public housing for low income families?
13. Should the federal government increase or decrease its spending on food stamps? (Food stamps provide financial assistance for food purchasing to families and individuals with low or no income.)
14. *Petition for Estate Tax*: same as in A.1
15. Which party do you plan to support in the 2014 congressional elections?
16. How much of the time do you think you can trust government in Washington to do what is right?
17. Next, think more broadly about the purpose of government. Where would you rate yourself on a scale of 1 to 5, where 1 means you think the government should do only those things necessary to provide the most basic government functions, and 5 means you think the government should take active steps in every area it can to try and improve the lives of its citizens?
18. Do you think that people in the government waste a lot of money we pay in taxes, waste some of it, or don't waste very much of it?"
19. How do you feel about the following statement: "Currently, the federal government is very effective in limiting fraud, waste and abuse in the programs it administers"?
20. How do you feel about the following statement: "Politicians in Washington work to enrich themselves and their largest campaign contributors, instead of working for the benefit of the majority of citizens"?

A.3 Emotional Poverty Experiment

The emotional poverty treatment was conducted in March 2014 on a sample of 1200 respondents. Respondents were first as usual asked for their demographic information. Depending on their answers to the questions about their marital status and whether they have children living with them, they were redirected to a specific, customized branch. They were then randomized into treatment and control group. The treatment group saw information about poverty that was adapted to his family situation, without knowing that the information was actually customized. We asked respondents to estimate the basic expenses that a family of the configuration of his own family would have to incur for rent, utilities, transportation to work, food, and, depending on the family situation, expenses for children. Appendix Figure 9 shows the screenshot where respondents were asked to estimate the minimal budget of a family similar to theirs. Between this page and the next, the program computed the total expenses estimated. If those were above the actual poverty threshold for the family, the next page told respondents that given their estimates, the family would fall short by such and such amount. If the total was below the poverty threshold, the next page told respondents that given their estimates, the family would only be left with such and such amount.

The link to the survey is `https://hbs.qualtrics.com/SE/?SID=SV_1B8MczKSMIvWaqx`.

The outcome questions were the same as in section A.2, except that the question about how many people pay the estate tax was not asked. Instead the following three questions were added:

1. What percentage of Americans live in poverty? If you don't know, just give your best guess. (Poverty is officially defined as having monthly resources below $970 for a single person, $1310 for a two person family, $1650 for a three person family, etc...)

2. What percentage of children under age 18 live in poverty in the US, approximately? If you don't know, just give your best guess.

3. How many workers in the US earn the minimum wage? If you don't know, just give your best guess.

A.4 Policy Experiment

The policy experiment was conducted in March 2014 on a sample of 1200 respondents. Respondents were randomized into a control group or a treatment group. The treatment group was again redirected to a customized branch, depending on their answers to the questions about their marital status and whether they had children living with them. In the treatment screens, they were asked about and show information about a family whose configuration was similar to theirs. First, respondents were asked to estimate the minimal expenses necessary for a family earning the minimum wage, for rent, utilities, transportation to work, food, and, if applicable, children expenses. That screen is shown in appendix Figure 10. Between this page and the next, the program computed the total expenses implied and informed the respondents either of the shortfall that the family living on a single minimum wage would have to face, or the surplus that would be left over, if they had to incur the expenses as estimated by the respondent. That second screen also showed information about the food stamps program, informing respondents that: "The Food stamps program helps many low income families, such as those earning only one minimum wage. It provides $150/month per person to help with food expenses."

The link to the experiment is `https://hbs.qualtrics.com/SE/?SID=SV_1B8MczKSMIvWaqx`.

The outcome questions were exactly the same as in section A.3.

A.5 Trust Experiment

The trust treatment was conducted in March 2014 on a sample of 1000 respondents. The treatment showed people information about how the United States is ranked in terms of corruption among a list of countries of similar income and development levels (the United

States is ranked in the worst group of countries given income and development levels), as shown in appendix Figure 8.

The link to this survey is in: `https://hbs.qualtrics.com/SE/?SID=SV_bgEuJfl1Y3UreKh`

Respondents were also asked questions that are supposed to elicit negative reactions regarding the government. The full list of questions were:

1. What is your view of the "Wall Street bailout", the legislation signed into law by President Bush in 2008 whereby the federal government lent $700 billion to banks and other financial institutions that faced bankruptcy?

2. Because of a recent Supreme Court decision, for the first time in decades there are now no limits on the amount of money that corporations and other special interests can give to political campaigns for President or Congress. Do you agree with the following statement: "Corporations and other specials interests have far too much influence on politicians in Washington"?

3. How do you feel about the following statement: "Currently, the federal government is very effective in limiting fraud, waste and abuse in the programs it administers"?

4. How much confidence do you have in the federal government's administering of economic and military aid to foreign countries (which totals over $50 billion annually)?

5. How do you feel about the following statement: "Politicians in Washington work to enrich themselves and their largest campaign contributors, instead of working for the benefit of the majority of citizens"?

The outcome questions were the same as in section A.2, except that we did not ask any of the knowledge questions about poverty rates, child poverty rates, minimum wage earners, or how many people pay the estate tax.

B   Methodological issues related to online survey experiments

In this section, we share what we have learned through our experience with these mTurk survey experiments to highlight both the advantages and disadvantages of using online surveys for research as well as provide advice for future researchers.

The most obvious advantage is the ease and speed of gathering relatively large samples. At a per-survey price of $1.50 it typically took at most five or six days to gather a sample of 1,000 U.S. residents, and samples of 300-400 could usually be gathered within 24 hours. An important piece of advice for future researchers studying American domestic policy questions is to ask mTurk to limit the sample to U.S. residents (Amazon documents this information for tax purposes). In pilot surveys we conducted before becoming aware of this problem, over forty percent of our respondents turned out to have non-U.S. IP addresses (likely an ever greater share were foreign residents since respondents can connect through remote servers and appear to be in the US). Launching surveys at night (during the workday in Asia) exacerbates this problem.

B.1   How representative is the mTurk sample?

As noted earlier, respondents to our surveys are not representative of the U.S. population. However, this lack of representativeness does not appear substantially worse than in other surveys. For example, the mTurk sample is about 13 years younger and 13 percentage points more likely to have a college degree than the representative sample of U.S. adults in our national CBS sample (see Table 1). But that same CBS poll we use as a basis for comparison is itself weighted. The raw CBS sample is about nine years older than the weighted sample and, like our mTurk sample, about 13 percentage points more likely to have a college degree. It under-represents Hispanics to the same extent as our sample and is even worse than our sample in terms of the under-representation of men. As such, while the mTurk sample is not

representative, neither are other standard polls (though some of the biases, such as age, are in opposite directions from standard polls).

As noted in the text, the American Life Panel is more representative than the mTurk sample. However, this sample is much smaller and roughly 30 times more expensive than mTurk.

B.2   How serious is attrition in the mTurk sample?

As noted earlier, for the omnibus treatment, we have an overall attrition rate (the share of respondents who start but do not finish the survey) of 22 percent: a 21 percent rate for the CT Marketing Group survey and a 22 percent rate for the mTurk surveys. In fact, when we compare surveys 4 and 5—which were in the field at the same time, with the former on mTurk and the latter on CT Marketing Group—attrition is actually lower on mTurk. Even Gallup has an attrition rate of nine percent on its 15-minute Daily Poll.[43] Given that our survey requires people to read through informational treatments, we suspect that a pure opinion survey conducted on mTurk would have a lower attrition rate.

The omnibus survey has differential attrition between treatment and control, probably because of the relatively long length of the treatment (respondents had to scroll through 6 treatment pages in the omnibus survey). Importantly, the follow-up surveys do not have such a differential attrition because the treatment was much shorter (2 pages at most) so that there was no noticeable length difference between the treatment and control group.

A related point is that researchers using this methodology should ask background questions first. In settings with little to no attrition, background questions can ideally be asked at the very end to avoid priming, but given the importance of assessing non-random attrition, it may be preferable for online survey experiments to ask demographic and background questions at the outset.

B.3   How many unique observations can one collect on mTurk?

Although the exact number of workers registered on mTurk is not publicly reported, the pool is thought to be quite large, approximately 500,000.

However, the pool available for any given survey experiment is more limited. First, workers select into tasks voluntarily, choosing among types of tasks. As a result, the pool of workers interested in survey experiments is more limited. Indeed survey experiments are typically longer than a standard task on mTurk and might require more attention. Second, we filter workers based on past ratings: this is to avoid careless workers who consistently do not complete tasks properly and have been rated badly by previous amazon mTurk requesters (i.e., employers on mTurk). Third, the pool of registered workers is not online all the time. Many workers could be working for a few weeks, then switch to another occupation, and potentially come back later or only work during some times of the year, month, or week. Finally, there is competition between tasks. Many tasks are posted simultaneously on the platform and workers have to search for each individual task. Large sample survey experiments sometimes fall lower in the list and are less visible to workers (although, one way to make it more visible and which we employ, is to stop and relaunch the survey several times so that it moves back up to the top of the list of tasks).

We always exclude workers who have already taken one of our survey experiments in the past to avoid any spillovers from previous treatments. Hence, we need a fresh sample every time. Accordingly, we noticed that when we launch a survey experiment a couple of weeks after a previous one, we easily get a large sample very quickly. However, when we repeatedly launch experiments one after the other, the rate at which we get workers slows down.

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[43]Email correspondence when Jeffrey Jones at Gallup, January 16, 2013. The nine-percent figure refers to all Daily Polls from 2012.

To mitigate the constraint of being unable to reach a sufficiently large sample of workers, researchers can do all of the following: a) leave sufficient times between survey dates, b) pay sufficiently high wages to make the tasks competitive relative to others, c) keep survey time short, d) choose more generic titles for the task, such as "Opinion Survey" rather than "Academic Research Survey in Economics." However, researchers should be warned that even after taking these steps, survey samples are not unlimited in mTurk and if "fresh" respondents are needed for each survey date a large share of respondents clicking on the task will be screened out (or will need to be dropped ex-post based on repeated mTurk worker IDs).

Appendix Figure 1: Inequality Component: "Where are you in the income distribution?"

Please enter your annual household income* in the box below:

$ [25000]

39% of US households earn less than your household
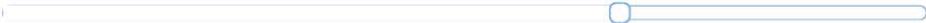
We now encourage you to move the blue slider above (by clicking on the line) to explore the US income distribution on your own and to answer the questions below.

79% of households earn less than **$73,000** .

Notes: This interactive slider allows people to explore the income distribution in the US and to determine their position in it. Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`

Appendix Figure 2: Inequality component: "Where would you have been in the income distribution?"

Income Inequality has increased dramatically in the United States since 1980.
Incomes of poorer and middle-income families have grown very little while top incomes have grown a lot.

How would YOU be doing if inequality had not increased?

The slider below shows how much each group would make if incomes had grown by the same percentage since 1980 for all groups: the poor, the middle class, and the rich. Use the slider to answer the questions below.

A household making **$25,800** today would instead be making
$35,200 if inequality had not changed since 1980.
In other words, if growth had been evenly shared, this household would have earned
37% more.

Notes: This interactive slider shows the "counterfactual" household income had income growth been shared equally since 1980. Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`

Appendix Figure 3: Taxes component: Correlation between growth and top tax rates over time
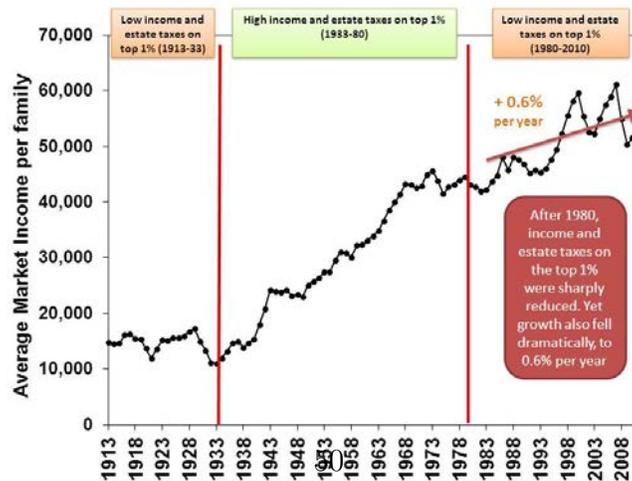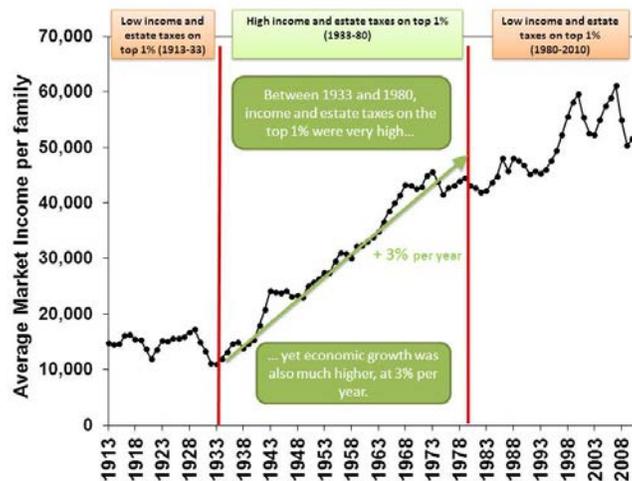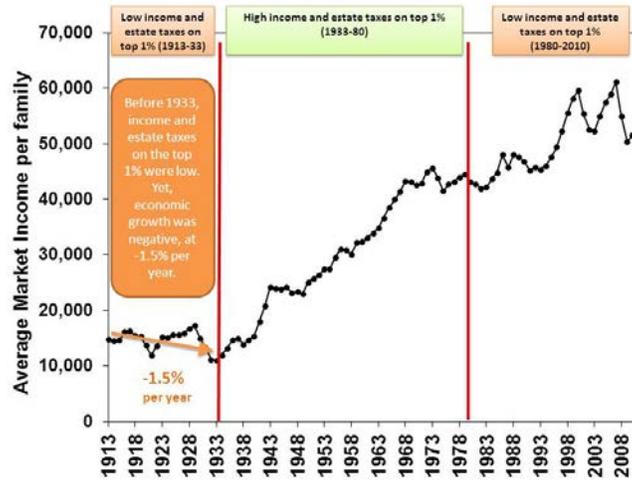
Appendix Figure 4: Showing information about the estate tax

Besides the income tax, the government can also level the playing field with **the federal estate tax.**

The **Federal Estate Tax** (also known as the **Death Tax**) applies when a deceased person leaves **more than $5 million** in wealth to his or her heirs. Wealth left to a spouse or charitable organizations is exempt from estate tax.

Only 1 person out of 1000 is wealthy enough to face the estate tax.

Average Americans do not have anything close to $5 million in wealth, so the estate tax does not affect them and they can pass on their property to their children tax-free.

Eliminating the estate tax would allow the very richest families to pass down all of their wealth to their children tax-free. Hence, children of rich people would also start off very rich themselves.

Increasing the estate tax is a way to level the playing field between the children of wealthy parents and children of middle-class parents.

Notes: Available online at
https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn

Appendix Figure 5: Preferred tax rates outcome

Now we would like to ask you about the income tax rates* that you think different people *SHOULD* pay.

The income tax rate is the percentage of your income that you pay in federal income tax. For example, if you earn $30,000 and you pay $3,000 in income taxes, your income tax rate is 10%.

Please use the sliders below to tell us how much you think each of the following groups should pay as a percentage of their total income.

The Top 1% (richest)   10%

The next 9% (1% of Households earn more than them, 90% earn less)   17%

The next 40% (10% earn more than them, 50% earn less)   5%

The Bottom 50% (poorest)   13%

Notes: Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`

52

Appendix Figure 6: Neutral information about the estate tax

The Federal Estate Tax applies when a deceased person leaves more than $5 million in wealth to his or her heirs. Wealth left to a spouse or charitable organizations is exempt from estate tax.

Only 1 person out of 1000 is wealthy enough to face the estate tax.

Average Americans have far less than $5 million in wealth when they die, so the estate tax does not affect them and they can pass on their property to their children tax-free.

Notes: Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_0vOecp2MDPBpInb`

Appendix Figure 7: A word cloud based on open-ended responses to the question "How much of the time can you trust the government in Washington to do what is right?"

Notes: Based on the pilot group survey on mTurk described in Section 5.2.

Appendix Figure 8: Negative information about corruption in the U.S.

Each year, the non-partisan organization Transparency International rates countries based on the amount of government corruption. When the US is compared to countries with similar levels of income and development how do you think it ranks?

The best 6:
The least corrupt group of countries

Better than average:
Half of countries have *more* corruption than these countries

Worse than average:
Half of countries have *less* corruption than these countries

The worst 6:
The most corrupt group of countries

The best 6	Better than average	Worse than average	The worst 6
○	○	○	○
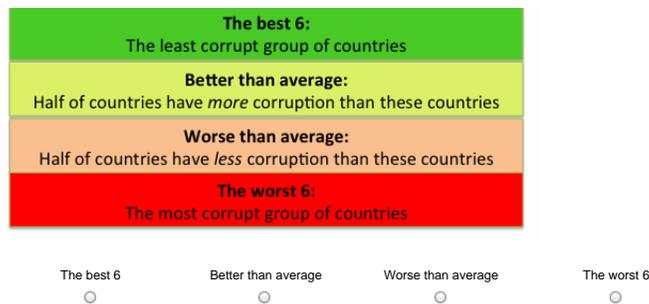
- **Page Break** -

These are the actual results from the report:

The best 6:
The least corrupt group of countries

Better than average:
Half of countries have *more* corruption than these countries

Worse than average:

The worst 6:
The most corrupt group of countries

The US is in the most corrupt group of countries among countries with similar levels of income and development.
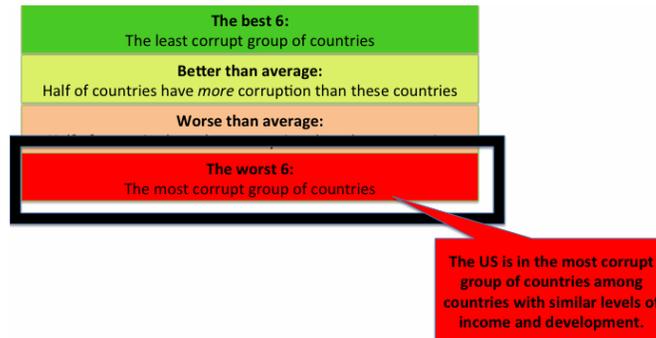
Notes: Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_bgEuJfl1Y3UreKh`

Appendix Figure 9: Customized information about poverty

The picture of poverty in a rich country like the United States is striking.

16% of Americans live in **poverty.**

20% of American **children** live in poverty.

25% of the **disabled** Americans live in poverty.

Think about a family of four with two parents working full time at low pay and two kids.

What would be the minimal monthly expenses that such a family would have to make to afford living in your city?

Please enter numbers only, with no "$" sign and no commas, e.g., 1000.

Rent	
Utilities (electric, heating, cable/phone)	
Car payment + car insurance + gas to go to work	
Food	
Child care while working	

Notes: Available online at
https://hbs.qualtrics.com/SE/?SID=SV_1B8MczKSMIvWaqx

Appendix Figure 10: Customized information about policies

Think about a family of four with one parent working full time at the minimum wage and two kids.

What would be the minimal monthly expenses that such a family would have to make to afford living where you live?

Please enter numbers only, with no "$" sign and no commas, e.g., 1000.

Rent	
Utilities (electric, heating, cable/phone)	
Transportation (public transit fare and/or car payments, insurance, gas…)	
Food	
Expenses related to children	

Notes: Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_1B8MczKSMIvWaqx`

Appendix Table 1: Share of treatment respondents correctly answering basic comprehension questions

	(1) Percent that answered correctly
Pct of households earn less than 386,000	0.852
Pct of households earn less than 108,000	0.843
Pct of households earn less than 33,800	0.826
Household now making 386,000 would be making...	0.464
Household now making 108,000 would be making...	0.843
Household now making 33,800 would be making...	0.800
Observations	1142

Notes: This table displays the percent of respondents who answered correctly to the comprehension questions during the treatment (see the treatment screenshots in the Appendix for the complete wording and outlay of these questions.

Appendix Table 2: Ability of covariates to predict whether respondents' finish the survey

Variable	Coeff	P-val
Voted for Obama in 2008	0.014	0.227
Age	-0.003	0.000
Liberal policy view	0.002	0.676
Household income	0.001	0.449
Married	0.008	0.480
Education	-0.001	0.724
Male	0.018	0.105
Black	-0.006	0.765
Hispanic	0.072	0.007
Native	-0.011	0.624
Employed full time	-0.008	0.483
Unemployed	0.024	0.145
Not in labor force	-0.029	0.055
Student	0.023	0.137
Treatment Group	-0.113	0.000

Notes: For each row, the coefficient and p-value are from regressions of the form $Finished_{ir} = \alpha + \beta Covariate_i + \delta_r + \epsilon_{ir}$, where $Covariate$ is listed to the left in the row and δ_r are survey round fixed effects.

Appendix Table 3: Attrition by survey round

Survey	Attrition			Obs. (Completed)
	Control	Treatment	Differential	
Omnibus	0.09	0.21	0.11	4045
Estate Tax	0.06	0.03	-0.03	1760
Trust	0.03	0.02	-0.01	901
Poverty	0.06	0.02	-0.04	1825
Policy	0.06	0.04	-0.02	1837

Notes: Note that the attrition numbers count those respondents who started and continued long enough to be at least assigned a treatment status. Respondents could drop out before being assigned to treatment for a variety of reasons, including that they were not U.S. citizens (albeit U.S. residents) or did not agree with the consent form.

Appendix Table 4: Effect of omnibus treatment on all outcomes, part 1

	Ineq vs.	Ineq. inc.	Deserving	Top tax	Mill. tax	Estate tax	Petition	Min. wage	Food stamps	EITC	Trust	Active.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.104***	0.120***	-0.127***	0.931*	0.0463***	0.357***	0.0648***	0.0325**	0.0149	0.0212	-0.0253**	0.132***
	[0.0144]	[0.0128]	[0.0188]	[0.549]	[0.0125]	[0.0140]	[0.0156]	[0.0141]	[0.0141]	[0.0151]	[0.0112]	[0.0339]
Cont gp. mean	0.285	0.738	2.002	30.21	0.745	0.171	0.234	0.690	0.686	0.611	0.155	3.076
Scaled Effect	0.365	0.540	0.312	0.0914	0.106	2.043	0.394	0.0995	0.0369	0.0698	1.105	0.110
Obs.	3703	3704	3690	3741	3741	3673	3060	3690	3690	3690	3739	3704

Notes: The first three outcome variables are binary indicator variables, coded as one if the respondent says that "inequality is a very serious problem," "inequality has increased," and "the rich are deserving of their income," respectively. "Top rate" is continuous (respondents' preferred average tax rate on the richest one percent). "Mill. tax" and "Estate" indicate the respondent wants income taxes on millionaires and the estate tax to increase, respectively. "Petition" indicates she would write her Senator to increase the estate tax. "Min wage," "Food st." and "EITC" indicate support for increasing the minimum wage, and funding for SNAP and EITC, respectively. "Trust" indicates trust in government and "Active" indicates the respondent favors a broad range of interventions by the government. "Covariates" and "scaled effects" are as specified in the notes to Table 4. The row "Cont pg. mean" reports the mean of the outcome variable in the control group. $^*p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table 5: Effect of omnibus treatment on all outcomes, part 2

	Dem. 2012	Taxes redistr.	Growth	Tax 1950	Satisfied	Money charity	Money given	Time charity	Time given
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	0.0152	0.0396***	0.137***	-0.0114*	-0.0266	-0.00717	-0.00565	-0.00188	0.00655
	[0.0125]	[0.0132]	[0.0160]	[0.00646]	[0.0264]	[0.0157]	[0.0158]	[0.0160]	[0.0157]
Cont gp. mean	0.529	0.199	0.437	0.0616	2.881	1.441	1.477	1.520	1.622
Scaled Effect	0.0246	0.314	0.946	0.487	0.102	0.167	0.0823	0.0219	0.255
Obs.	3703	3700	3696	3698	3690	3681	3683	3684	3684

Notes: "Dem 2012" indicates the respondent plans to vote for the Democrat (Obama) in the 2012 Presidential election. "Taxes redistr." indicates that the respondent thinks the role of income taxes is to redistribute income. "Growth" indicates that the respondent correctly replied to the question of whether growth was higher in 1933-1980 than in 1980-2000. "Tax 1950" indicates that the respondent correctly replied to the question whether top income taxes were higher in the 1950s than today. "Satisfied" indicates whether the respondent is satisfied with his current income. "Money charity" (respectively, "Time charity") is a dummy variable for whether the respondent plans to give money (respectively, time) to charity in the next month and "Money given" (respectively, "Time given") indicates whether he has already given money (respectively, time) in the past month. "Covariates" and "scaled effects" are as specified in the notes to Table 4. The row "Cont pg. mean" reports the mean of the outcome variable in the control group. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 6: Selection into the follow-up

Variable	Coefficient	P-value
Voted for Obama in 2008	-0.012	0.605
Age	0.003	0.005
Liberal policy view	-0.015	0.149
Household income	0.006	0.097
Married	0.051	0.027
Education	0.012	0.143
Male	-0.014	0.539
Black	0.060	0.163
Hispanic	0.067	0.242
Native	-0.077	0.093
Employed full time	0.015	0.506
Unemployed	-0.006	0.843
Not in labor force	0.074	0.051
Student	-0.066	0.019

Notes: For each row, the coefficient and p-value are from regressions of the form $Followup_{ir} = \alpha + \beta Covariate_i + \epsilon_{ir}$, where $Covariate$ is listed to the left in the row. Those tests are used to detect selection into the follow-up survey.

Appendix Table 7: Effect of omnibus treatment one month later

	Ineq. v. serious		Ineq. inc.		Deserving rich		Top tax rate	
	(1) First	(2) Follow-up	(3) First	(4) Follow-up	(5) First	(6) Follow-up	(7) First	(8) Follow-up
Treated	-0.00715 [0.0708]	0.0407 [0.0741]	0.0581 [0.0626]	-0.0161 [0.0712]	0.112 [0.0943]	0.0883 [0.112]	2.213 [3.169]	3.373 [3.239]
Cont gp. mean	0.288	0.230	0.785	0.747	1.986	1.885	32.61	29.99
Obs.	169	169	169	169	168	168	167	167

Notes: The top tax rate is continuous. "Rich deserving" and "Increase Estate Tax" are binary, while "Govt Purpose" is a categorical variable taking five values with one being the most limited and five the most comprehensive purpose for the government (the variable is rescaled by subtracting the sample mean). For each dependent variable, Col "First" is the result from the first survey, while Col "Follow-up" is the result from the follow-up survey. We use a more limited set of control given the small sample size. Controls for each regression are as usual. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 8: Effect of omnibus treatment one month later (cont.)

	Mill. tax		Estate tax		Trust govt		Govt scope	
	(1) First	(2) Follow-up	(3) First	(4) Follow-up	(5) First	(6) Follow-up	(7) First	(8) Follow-up
Treated	-0.00661 [0.0705]	0.0385 [0.0749]	0.295*** [0.0878]	0.177** [0.0845]	-0.0490 [0.0576]	-0.0339 [0.0532]	0.329* [0.187]	0.365* [0.186]
Cont gp. mean	0.756	0.770	0.181	0.184	0.123	0.126	2.995	2.874
Obs.	168	168	168	168	168	168	168	168

Notes: All outcomes are as defined previously. For each dependent variable, Col "First" is the result from the first survey, while Col "Follow-up" is the result from the follow-up survey. All regressions include our standard controls. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 9: Bounding effects of attrition (using "liberal" and "conservative" values for outcomes)

	Ineq. v. serious		Increase Mill. Tax		Increase Estate Tax		Trust Gov	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	C	L	C	L	C	L	C	L
Treated	0.0666***	0.0980***	0.0194	0.0518***	0.284***	0.310***	-0.00692	-0.0198**
	[0.0129]	[0.0128]	[0.0118]	[0.0117]	[0.0122]	[0.0121]	[0.00979]	[0.00970]
Cont gp. mean	0.267	0.293	0.717	0.744	0.170	0.192	0.172	0.161
Obs.	4547	4547	4546	4546	4519	4519	4546	4546

Notes: All outcomes are as defined previously. No controls are included. For each dependent variable, Col "C" assumes that all attritors gave the average answer among those who label themselves as conservative or very conservative. Col "L" assumes that all attritors would have given the average answer among those who label themselves as liberal or very liberal. *$p < 0.1$,** $p < 0.05$,*** $p < 0.01$

Appendix Table 10: Views on inequality and taxes for high- and low-attrition rounds in the omnibus surveys

	Ineq. v. serious		Ineq. increased		Rich deserving		Top tax rate		Millionaire tax		Estate tax	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.0794***	0.117***	0.0849***	0.138***	-0.00327	-0.0672***	0.155	2.003***	0.0154	0.0504***	0.421***	0.365***
	[0.0248]	[0.0219]	[0.0222]	[0.0176]	[0.0199]	[0.0169]	[1.031]	[0.716]	[0.0207]	[0.0185]	[0.0247]	[0.0212]
Cont gp. mean	0.276	0.289	0.767	0.780	0.160	0.180	34.40	28.88	0.800	0.758	0.179	0.178
Scaled Effect	0.278	0.387	0.475	0.615	0.0118	0.209	0.0131	0.203	0.0446	0.100	1.761	2.396
Differential attrition?	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Obs.	1260	1635	1260	1636	1254	1631	1271	1650	1271	1650	1250	1626

Notes: All outcomes are as defined previously. "Differential attrition?" separates observations according to whether they were in "Low" differential attrition round (round 4) or in "High" differential attrition rounds (rounds 1, 2 and 3). Otherwise, all terminology follows that in previous tables. *$p < 0.1$,** $p < 0.05$,*** $p < 0.01$

Appendix Table 11: Opinions on other policies and views of government for high- and low-attrition rounds in the omnibus surveys

	Min. wage		EITC		Food stamps		Trust gov.		Active gov.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.00695	0.0408*	-0.0199	0.0260	0.0203	0.0197	-0.0553***	0.00269	0.0576	0.248***
	[0.0244]	[0.0210]	[0.0257]	[0.0230]	[0.0240]	[0.0210]	[0.0199]	[0.0166]	[0.0579]	[0.0494]
Cont gp. mean	0.708	0.706	0.683	0.589	0.710	0.702	0.174	0.132	3.140	2.997
Scaled Effect	0.0227	0.129	0.0679	0.0808	0.0490	0.0451	2.329	0.102	0.0441	0.195
Differential attrition?	Low	High	Low	High	Low	High	Low	High	Low	High
Obs.	1254	1631	1254	1631	1254	1631	1270	1649	1260	1636

Notes: See previous table. *$p < 0.1$,** $p < 0.05$,*** $p < 0.01$

Appendix Table 12: Effect of estate-tax-only treatments on outcome variables

	Ineq. v. ser.	Ineq. inc.	Estate corr.	Pov. vs.	Deserving	Top tax	Mill. tax	Estate	Petition	Charities	Educ. pol
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treated (Emotional)	0.0381	-0.00239	0.316***	0.0224	-0.0247	1.595	0.00123	0.289***	0.0313	0.0421	-0.0319
	[0.0258]	[0.0243]	[0.0263]	[0.0274]	[0.0206]	[1.121]	[0.0243]	[0.0258]	[0.0208]	[0.0699]	[0.0755]
Treated (Neutral)	0.0511**	-0.0501**	0.375***	0.0191	-0.0244	2.280**	-0.00456	0.109***	0.0239	-0.0214	0.0157
	[0.0259]	[0.0244]	[0.0264]	[0.0276]	[0.0206]	[1.124]	[0.0244]	[0.0259]	[0.0209]	[0.0701]	[0.0757]
Cont gp. mean	0.307	0.771	0.120	0.285	0.174	33.67	0.717	0.210	0.132	1.945	3.611
Scaled Emot. Effect	0.118	0.0106	3.386	0.0858	0.0984	0.142	0.00251	1.085	0.265	0.0329	0.0524
Scaled Neutral Effect	0.159	0.223	4.014	0.0732	0.0974	0.202	0.00934	0.408	0.202	0.0167	0.0257
Obs.	1777	1777	1773	1508	1777	1785	1764	1777	1762	1709	1732

Notes: The "emotional" treatment repeats the estate tax slide from the omnibus treatment, but eliminates the rest of the treatment. The "neutral" treatment is a version of the "emotional" estate tax treatment that attempts to remove any framing effects. All outcomes are defined as previously. "Charities" and "Educ. pol" refer to the ranking (from 1 to 5, with 1 being the highest ranking) that respondents attribute to private charities and education policies as means to redistribute income.*$p < 0.1$,** $p < 0.05$,*** $p < 0.01$

Appendix Table 13: Effect of estate-tax-only treatment on outcome variables (cont.)

	Min. wage	Aid	Food stamps	Housing	Trust	Govt scope	No waste	Dem. 2014
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated (Emotional)	0.0529	0.109	0.0982	0.236***	-0.0164	0.0172	-0.0281	0.0344
	[0.0865]	[0.0837]	[0.0861]	[0.0824]	[0.0205]	[0.0585]	[0.0303]	[0.0213]
Treated (Neutral)	-0.0838	0.0273	0.0628	0.127	-0.00558	0.0259	-0.0587*	-0.00310
	[0.0870]	[0.0842]	[0.0866]	[0.0829]	[0.0205]	[0.0587]	[0.0303]	[0.0213]
Cont gp. mean	2.608	2.130	1.797	1.927	0.153	3.074	1.456	0.470
Scaled Emot. Effect	0.0913	0.0624	0.0534	0.140	0.235	0.0150	0.0896	0.0490
Scaled Neutral Effect	0.145	0.0156	0.0341	0.0755	0.0803	0.0225	0.187	0.00441
Obs.	1495	1495	1495	1495	1756	1756	1754	1756

Notes: See above. "No waste" takes values from 1 to 3, with 1 representing that the government is wasting a lot of tax revenue and 3 that the government is not wasting much tax revenue at all. Note that the only statistically significant effect for a poverty-related outcome is for increased public housing. We speculate that this effect is due to seeing the picture of the mansion in the "emotional" estate tax treatment (but not in the neutral treatment). $^*p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table 14: Effect of trust treatment on outcome variables

	Ineq. v. ser.	Ineq. inc.	Pov. v. ser.	Deserving	Top tax	Mill. tax	Estate	Petition
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.0547*	0.0119	-0.00257	-0.0555**	0.490	-0.0421	-0.00168	-0.0602**
	[0.0311]	[0.0289]	[0.0313]	[0.0239]	[1.326]	[0.0275]	[0.0266]	[0.0236]
Cont gp. mean	0.343	0.755	0.383	0.185	34.70	0.722	0.204	0.174
Scaled Trust Effect	0.182	0.341	0.00828	0.204	0.0452	0.0949	0.00728	0.580
Obs.	899	899	899	899	898	899	895	899

Notes: The negative trust prime treatment consists of several multiple-choice questions that made respondents reflect on aspects of government they dislike. All terms are defined as previously. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 15: Effect of trust treatment on outcome variables (cont.)

	Min wage	Aid	Food st.	Housing	Trust	Active	No waste	Dem. 2014	Charity	Educ. pol.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	-0.00428	-0.139**	-0.153**	-0.163***	-0.0582***	0.0236	-0.0278	-0.0462*	0.187**	0.0615
	[0.0902]	[0.0616]	[0.0673]	[0.0614]	[0.0203]	[0.0688]	[0.0346]	[0.0258]	[0.0791]	[0.0885]
Cont gp. mean	2.673	2.675	2.454	2.581	0.125	3.031	1.423	0.479	1.800	3.732
Scaled Trust Effect	0.00531	0.128	0.119	0.133	1.730	0.0170	0.109	0.0686	0.169	0.265
Obs.	899	899	899	899	899	899	898	899	850	874

Notes: See above. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 16: Effect of "emotional" treatment on outcome variables

	Ineq. v. ser.	Ineq. inc.	Pov. corr.	Childpov. corr.	Min wage corr.	Pov. vs.	Deserving	Top tax	Mill. tax	Estate	Petition
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treated	0.0778***	0.0374	0.254***	-0.0751**	0.0701**	0.0971***	-0.0146	0.494	0.0343	0.0116	0.00721
	[0.0278]	[0.0256]	[0.0312]	[0.0299]	[0.0342]	[0.0315]	[0.0229]	[1.150]	[0.0255]	[0.0270]	[0.0251]
Cont gp. mean	0.307	0.771	0.417	0.373	0.355	0.316	0.183	30.96	0.721	0.256	0.198
Scaled Poverty Effect	0.211	0.179	216.6	1.667	2.227	0.293	0.0461	0.0428	0.0700	0.0384	0.0484
Obs.	1825	1824	1302	1299	999	1204	1825	1812	1824	1402	1824

Notes: The "emotional" treatment aimed at creating empathy between the respondent and families living in poverty. Respondents were told about poverty rates and had to fill out a minimum budget for a family like theirs living in the same city. Respondents were then shown how their minimum budget compared to the poverty line. All outcomes are as defined previously. $^*p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table 17: Effect of "emotional" treatment on outcome variables (cont.)

	Min wage	Aid	Food st.	Housing	Trust	Active	No waste	Dem. 2014	Charity	Educ. pol.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.0280	0.147	0.189*	0.0473	-0.0132	0.0557	-0.0657**	0.00856	0.0634	-0.209**
	[0.0993]	[0.0936]	[0.101]	[0.0944]	[0.0215]	[0.0622]	[0.0325]	[0.0232]	[0.0813]	[0.0916]
Cont gp. mean	2.529	2.127	1.811	2.064	0.144	3.065	1.384	0.468	2.026	3.728
Scaled Poverty Effect	0.0283	0.0689	0.0973	0.0249	0.264	0.0383	0.207	0.0125	0.0356	0.275
Obs.	899	899	899	899	1825	1825	1407	1407	1165	1180

Notes: See above. $^*p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table 18: Effect of policy treatment on outcome variables

	Ineq. v. ser.	Ineq. inc.	Pov. corr.	Childpov. corr.	Min wage corr.	Pov. vs.	Deserving	Top tax	Mill. tax	Estate	Petition
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treated	0.0408	0.0289	0.0305	0.0383	0.228***	-0.00872	-0.00557	2.099*	0.0319	-0.0251	-0.0227
	[0.0268]	[0.0255]	[0.0306]	[0.0301]	[0.0300]	[0.0269]	[0.0219]	[1.097]	[0.0243]	[0.0256]	[0.0239]
Cont gp. mean	0.307	0.771	0.417	0.373	0.355	0.316	0.183	30.96	0.721	0.256	0.198
Scaled Policy Effect	0.111	0.138	25.99	0.852	7.237	0.0263	0.0176	0.182	0.0650	0.0832	0.152
Obs.	1835	1834	1311	1313	1110	1315	1835	1822	1833	1414	1834

Notes: The policy treatment aimed at creating empathy between the respondent and families living on a minimum wage. Respondents had to fill out a minimum budget for a family like theirs living in the same city. Respondents were then shown how their minimum budget compared to the minimum wage and how food stamps adds \$150 per person/month to the budget of such a family. All outcomes are as defined previously. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 19: Effect of policy treatment on outcome variables (cont.)

	Min wage	Aid	Food st.	Housing	Trust	Scope	No waste	Dem. 2014	Charity	Educ. pol.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	0.304***	0.149	0.299***	0.244***	-0.0349*	0.0515	-0.0490	0.0273	-0.130*	-0.117
	[0.0960]	[0.0909]	[0.0977]	[0.0906]	[0.0206]	[0.0602]	[0.0308]	[0.0224]	[0.0704]	[0.0782]
Cont gp. mean	2.529	2.127	1.811	2.064	0.144	3.065	1.384	0.468	2.026	3.728
Scaled Policy Effect	0.308	0.0698	0.154	0.128	0.698	0.0354	0.154	0.0398	0.0731	0.155
Obs.	906	906	906	906	1835	1835	1417	1417	1266	1285

Notes: See above. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$