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ABSTRACT

How Elastic are Preferences for Redistribution? Evidence from Randomized Survey Experiments*

This paper analyzes the effects of information about inequality and taxes on preferences for redistribution using randomized online surveys on Amazon Mechanical Turk (mTurk). About 5,000 respondents were randomized into treatments providing interactive information on U.S. income inequality, the link between top income tax rates and economic growth, and the estate tax. We find that the informational treatment has very large effects on whether respondents view inequality as an important problem. By contrast, we find quantitatively small effects of the treatment on views about policy and redistribution: support for taxing the rich increases slightly, support for transfers to the poor does not, especially among those with lower incomes and education. An exception is the estate tax---we find that informing respondents that it affects only the very richest families has an extremely large positive effect on estate tax support, even increasing respondents' willingness to write to their U.S. senator about the issue. We also find that the treatment substantially decreases trust in government, potentially mitigating respondents' willingness to translate concerns about inequality into government action. Methodologically, we explore different strategies to lower attrition in online survey platforms and show our main results are robust across methods. A small follow-up survey one month later reveals that our results persist over time. Finally, we compare mTurk with other survey vendors and provide suggestions to future researchers considering this platform.

JEL Classification: D63, H2 and I3
Keywords: inequality, online survey, randomized experiment and taxation

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1 Introduction

Over the past several decades, the US has witnessed a large increase in income concentration. While the top one (0.1) percent of tax-filers captured 9.0 (2.8) percent of total pre-tax income in 1970, in 2007, on the eve of the financial crisis, the share had risen to 23.5 (12.3) percent.[1] The median-voter theorem predicts that an increase in the demand for redistribution would accompany this rise in income concentration (Meltzer and Richard, 1981). However, time-series evidence from survey data does not support this prediction. If anything, the General Social Survey shows there has been a slight decrease in stated support for redistribution in the US 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 rising inequality.[2] An alternative explanation is that Americans are unaware of the extent or growth of inequality (see Kluegel and Smith, 1986 and Ariely and Norton, 2011), that this information is not sufficiently salient, or that they are skeptical about the government's ability to redistribute effectively.

In this paper, we explore whether providing information on inequality and tax policy affects respondents' views of whether inequality is a problem and beliefs about what, if anything, the government should do about it. We conducted a series of online randomized experiments using Amazon Mechanical Turk (mTurk). mTurk has been a rapidly growing new laboratory to carry out social experiments (see Horton *et al.*, 2011 and Paolacci *et al.*, 2010, as well as our review below). We show that such an online laboratory can be fruitfully used to test the effects of information on policy preferences. In our experiments, approximately 5,000 respondents were randomized into treatments providing interactive information on U.S. income inequality, historical data on the link between top income tax rates and economic growth, and the estate tax. Both control and treatment groups then reported their views on inequality, government, taxes, transfers, and redistribution.[3]

We find that respondents' views about inequality are very elastic to information—for example, the treatment increases the share agreeing that inequality is a "very serious prob-

[1] See the online updates to Piketty and Saez (2003), Table A3 at `http://elsa.berkeley.edu/~saez/TabFig2010.xls`

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

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

lem" by forty percent. Put differently, the treatment closes roughly forty percent of the gap between self-identified liberals and conservatives on this question. By contrast, while there are some effects on policy preferences (always in the "expected direction"), these effects are much smaller. The effects on respondents' opinions about increasing taxes on millionaires, preferred top tax rates, and the minimum wage are significant but small in magnitude. For example, our treatment closes only about ten percent of the gap in top tax rate preferences between liberals and conservatives. Treatment effects on attitudes towards transfer programs such as the Earned Income Tax Credit (EITC) and Food Stamps are small and not statistically significant. An exception to this general trend is the estate tax—we find that providing information on the (small) share of estates subject to the tax dramatically increases support for the tax.

We explore possible explanations for respondents' general unwillingness to connect their concern about inequality with government action. We find that information about inequality also makes respondents trust government less—the treatment decreases by nearly twenty percent the share of respondents who "trust government" most of the time. Hence, emphasizing the severity of a social or economic problem appears to undercut respondents' willingness to trust the government to fix it—the existence of the problem could act as evidence of the government's limited capacity to improve outcomes more generally.

Interestingly, we find that there is a substantial contrast in treatment effects between low income/low education respondents and high income/high education respondents. Low income/education respondents are less likely to support transfers to the poor after being treated with information about inequality while high income/education respondents are more likely to support such transfers after treatment. This result is consistent with recent findings by Kuziemko *et al.* (2011) suggesting that status anxiety can make low-status individuals wary of redistributive policies that may help groups below them. Given that our treatment focused on the income distribution, status concerns related to income are likely heightened by the treatment. We further explore whether financial anxiety has different effects on lower- and higher-income respondents' support for redistribution in a separate experiment that randomizes respondents to reading either positive or negative news about the economy (and does not present any information about inequality). We find a very similar pattern of results where negative (positive) news decreases (increases) low-income respondents support for redistribution relative to other respondents.

We also attempt to address two common critiques of survey experiments—that their effects are ephemeral and that they tell us little about "real" changes in how individuals actually behave. We find that many of our effects persist a month after the survey experiment, though these results are limited by only twenty percent of original respondents participating in a follow-up we conducted for one of the experiments. We also find that our treatment induces individuals to state they will send an email to their U.S. Senator in favor of maintaining the estate tax, though we note that we are unable to verify that emails were actually sent.

Besides documenting these results, a second goal of this paper is to describe in detail the data-gathering process on mTurk, to serve as a guide for future research in public economics using this platform. To this end, we compare attrition on mTurk to other survey providers (C & T Marketing group and Gallup, Inc.). We explore different methods for decreasing attrition generally, as well as decreasing the difference in attrition between those in the treatment and control arms of the survey experiment. In general, attrition does appear to respond to some of our attempts to reduce it, such as varying the pay or the relative time length of the treatment and control experiments. Importantly, the estimated treatment effects for our main results is very stable across all waves, suggesting that they are not sensitive to attrition levels. Overall, the quality of responses and attrition rates in mTurk compare well to other, more expensive survey platforms.

Our survey results echo Bartels (2005), in that the public fails to connect concern for inequality with public policy. Even with our relatively aggressive informational treatment—which is successful in changing attitudes towards inequality—policy preferences move only slightly. Similarly, by showing that such preferences are relatively "sticky," even in the short-run, our work is consistent with the results in 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 the lack of trust in 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. In preliminary work, Sapienza and Zingales (2013) 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. Trust in government tends to be lower among less-educated and poorer individuals,

and could help explain the seeming paradox that a substantial fraction of low and moderate income earners support politicians who advocate lower taxes on the rich.

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. Mueller (1963) is an early example. In more recent work, Alesina and Glaeser (2004) perform an extensive analysis of how attitudes toward redistribution differ in the US versus Europe, Alesina and Ferrara (2005) show that prospects for future income mobility predict redistributive preferences in the General Social Survey, and Alesina and Giuliano (2009) compare how background characteristics predict these preferences differently in the US versus other countries in the World Values Survey. Fong (2001) uses Gallup data to show that beliefs about the causes of poverty and equality of opportunity predict redistributive preferences even after controlling for many background variables. Similarly, Reeskens and Van Oorschot (2011) and León (2012) use the European Social Survey to relate personality traits and personal beliefs to redistributive preferences. In addition, Margalit (2013) explores how individuals preferences on welfare policy shift in response to changes in their personal economic circumstances during the Great Recession. Other papers in this category relate local ethnic heterogeneity to support for transfer programs, generally finding that support falls as minorities make up a greater share of potential beneficiaries (e.g., Luttmer, 2001 and Eger, 2010). Singhal (2008) uses OECD survey data to quantify redistributive preferences and shows that people do not necessarily favor low tax rates at income levels close to theirs.

Some previous research has randomized respondents into experiencing framing or priming treatments and then examined the effects on policy preferences. Hite and Roberts (1991), McCaffery and Baron (2006), McCaffery and Baron (2005) and Baron and McCaffery (2004) present extensive evidence that views on taxation are highly sensitive to framing (e.g., whether a tax is described in absolute or percentage terms, or framed as a penalty or a loss of a credit). Savani and Rattan (2012) find that priming individuals about choice and free will has large effects on whether they support redistributive policies or view inequality as a problem. Similarly, Naoi and Kume (2011) find that priming with visual cues affects support for agricultural subsidies.

As in our paper, some researchers have estimated the effects of randomized *information*

treatments on policy preferences.[4] The evidence from these efforts is mixed. Sides (2011) carries out an information experiment with the Cooperative Congressional Election Surveys on the estate tax and finds that providing (accurate) 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 *et al.* (2013) find that showing poor individuals their actual place in the income distribution increases their support for policies that target poverty, as most incorrectly view themselves as closer to the average than they are. On the other hand, Kuklinski *et al.* (2003) finds 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 respondents' initial beliefs are wildly incorrect.[5] 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. To the best of our knowledge, no paper has examined the effect of randomized information treatments on preferred federal income tax rates, despite the fact that the income tax is much larger than the estate tax or welfare policy in terms of its effect on the federal budget.[6]

As noted, our research is part of a small but growing set of papers using online survey platforms. Of the papers noted above, McCaffery and Baron (2006), Naoi and Kume (2011), and Savani and Rattan (2012) were performed online. Outside of informational survey experiments, authors have used online survey platforms—most often, mTurk—to have respondents play public goods games (e.g., Rand and Nowak, 2011, Suri and Watts, 2010),

[4]While not related to policy preferences, there is a small literature on how information treatments affect individuals' ability to better *navigate* policies such as the EITC (Chetty and Saez, 2013) and Social Security (Liebman and Luttmer, 2011).

[5]While less related to redistributive preferences, Gilens (2001) finds significant effects on support for foreign aid and reductions in prison spending using information treatments on the size of the foreign aid budget and decreasing crime rates. Howell *et al.* (2011) analyze views on teachers' pay and finds that support for increases in teachers' pay raises erodes when respondents are told about average teacher salary in their state.

[6]For example, in 1999, before the large decrease in the estate tax under President George W. Bush, the Estate and Gift tax together brought in $27 billion, compared to $879 billion brought in by the individual income tax. See `http://www.whitehouse.gov/sites/default/files/omb/budget/fy2013/assets/hist.pdf` Tables 2.2 and 2.5. An early paper that examines the effect of information on tax preferences is Fishkin (1997), but the design is not that of a randomized informational treatment. He analyzes the support for various policy options before and after a weekend of intensive learning and deliberation (designed to be neutral). He finds that support for a flat tax falls from 44 percent at the beginning of the weekend to 30 percent at the end.

interact in online labor markets (Amir *et al.*, 2012 and Horton *et al.*, 2011), study the effects of pay inequality on job satisfaction and turnover (Card *et al.* (2012)), or simply to gather non-experimental survey data on respondents' views about policy (Huber and Paris, 2013) and social preferences (Weinzierl, 2012; Saez and Stantcheva, 2013). The methodological papers by Berinsky *et al.* (2013), Rand (2012), and Mason and Suri (2012) focus on evaluating mTurk as a tool for research. We follow as best we can the advice in these papers as well as draw lessons from our own experience that might help future researchers working on this platform.

The remainder of the paper is organized as follows. Section 2 introduces the 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 report results from robustness checks and extensions and discuss potential explanations for our results. In Section 6, we discuss the advantages and disadvantages of using online survey-experiment methodology, and provide advice for future researchers. Finally, in Section 7, we suggest directions for future work and offer concluding thoughts.

2 The Survey Experiment

The experiment was carried out in six separate waves from January 2011 to August 2012 with a common core structure with variations. The Appendix provides a complete description of the experiment with the questions for each wave. Each wave had the following common structure: (1) background socio-economic questions including age, gender, race, education, household income, work status, conservative-versus-liberal views on economic policy, preferred candidate in the 2008 presidential election; (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 preferred candidate in the upcoming 2012 presidential election. The experiment was designed to test the effect of the information treatment on views about inequality and government, while being able to stratify the analysis by socio-economic characteristics.

The online survey for the treatment group was, by necessity, different from the online survey for control group. Therefore, a key challenge that arises in such a setting is differential attrition between the control and treatment groups. Five of the six waves were carried out

with mTurk while one wave (wave 5) was carried out with CT Marketing group survey company. Our full sample consists of the six waves.

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). The mTurk population is 55% female, with a median age of 30 years and a median annual income of $30,000 (Mason and Suri (2010)).

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 our consent form which asked people to confirm that they were U.S. residents in order to take the survey, we limited access to the survey to U.S. residents only, which is checked by Amazon itself. Second, only workers with a good track-record (those with a past completion rate of at least ninety percent) were allowed to take the survey, in order to exclude robots. Thirdly, after each wave of the survey, we blocked respondents for that wave before launching the next one, to ensure a fresh, non-contaminated sample each time. Fourth, respondents were told that payment would be contingent on completing the survey 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 information treatments

In general, the goal of the information treatments were 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. As such, some of the treatments we display will seem overly-simplified to an economics audience, but it should be kept in mind that our goal was 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.

The information varied slightly across surveys waves, but there was substantial overlap. In all waves, respondents saw interactive information on the current income distribution—they were asked to input their household income and were then told what share 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 99^{th} percentile and were encouraged to "play" around with the application. Appendix Figure 2 presents a screen shot.

A second application related to the income distribution allowed respondents to see what they "would have made" had economic growth since 1980 had been evenly shared across the income distribution. 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 3 presents a screen shot.

The second type of treatment focused on policy. To emphasize that higher taxes on the wealthy 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 4 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 5 presents a screen shot.

Readers can directly experience these informational treatments online at `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).

3 Data

3.1 Summary statistics

Table 1 shows characteristics of the sample who completed the survey (we discuss issues related to 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.

The differences between our mTurk sample and the CBS sample tend to conform to our *a priori* expectations of the groups most likely to use the Internet. 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.

In the third column, we reweight our sample, so that it matches the CBS sample in terms of the 32 cells based on: gender (2) $\times$ age brackets (2) $\times$ white versus non-white (2) $\times$ college degree indicator (2) $\times$ Supported Obama in 2008 (2). As we shall see, in general, reweighing makes little difference to the estimated treatment effects and we focus on the unweighted results.

Table 2 shows summary statistics on demographic and policy views for liberals and conservatives separately for our control group (so that responses are not contaminated by the information treatment). As expected, conservatives are older, more white, more likely to be married, and 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 typically report treatment vs. control differences both in absolute terms and as a percentage of the liberal vs. conservative differences reported in Table 2.

3.2 Survey attrition

Our surveys 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 1 shows, attrition is not random, though is only marginally related to 2008 voting preferences and not related to liberal versus conservative policy views (the variable most highly correlated with our outcome variables). As the final row of the Table shows, attrition is higher among the treatment group (twenty percent, versus nine percent for the control group).[7]

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 regressing $Treatment_{ir} = \beta Covariate_{ir} + \delta_r + \epsilon_{ir}$ for the main control variables we have

[7]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.

in our data (i indexes the individual, r the survey wave, and δ_r are survey-wave fixed effects).[8] Each row reports results from a separate regression. Of 14 outcomes, only one, the indicator for Hispanic origin, is significant ($p = 0.026$). However, given that the point-estimate for *Black* is of the opposite direction, it does not seem that, say, minorities systematically attrit from the sample if they are assigned to treatment.

While we will show results with and without the controls in this table, 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 hearing about inequality—perhaps conservatives or wealthier people—would have been turned off by the treatment. Section 6 provides greater detail on our attempts to reduce attrition and to reduce the difference in attrition rates between the control and treatment groups. We also present a number of tests to show that our results are not driven by differential attrition.

4 Results

We present several sets of results. Our first set of results captures how the treatment affected 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 treatment on questions related to inequality and income distribution. Odd-numbered columns do not include any controls outside of wave fixed effects and those listed in the table, while even-number columns include standard controls (essentially, the ones listed in Table 3).[9]

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

[9] Specifically, we include fixed effects for racial/ethnic categories, employment status, and state of residence; indicator variables for voting for President 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.

Column (1) shows that the treatment is associated with a forty percent increase in the share agreeing that inequality is a "very serious" problem. Similarly, dividing the point-estimate by the liberal-conservative difference suggests that the treatment makes up just under forty percent of that gap. The effects on the outcome "did inequality increase since 1980" are presented in column (3). The effects are even larger (54 percent of the conservative-liberal gap), likely because the informational treatment presented information so 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—only making up about one-sixth the liberal-conservative gap. As such, it does not seem that treatment respondents' concern about inequality is being driven primarily by a heightened feeling that the rich have not deserved their income.

In all cases, the results are robust to inclusion of standard controls (cols. 2, 4 and 6). Given the results from Table 3 that conditional on finishing the survey, there was little correlation between treatment status and standard covariates, the robustness to including controls is not surprising.

Overall, our basic information 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 shows parallel results for questions related to income and estate taxation. While the results related to income taxation are in the expected direction and statistically significant, their magnitudes are small. While the treatment closed a large share of the liberal-conservative difference in views about inequality, it makes only a small dent in the corresponding differences for income taxation. Whether we ask individuals to set their preferred top tax rate (cols. 1 and 2) or simply ask them whether taxes on millionaires should be increased (cols. 3 and 4), the point-estimates are small in magnitude. For example, the treatment increases the preferred top 1% average tax rate by just 1.1 percentage points, and hence closes only eleven percent of the ten-percentage-point gap between conservatives and liberals on this issue (see Table 2). Similarly, the treatment closes less than twelve percent of the conservative-liberal gap with respect to raising income taxes on millionaires. The infor-

mation treatment was hardly subtle in its discussion of taxes, focusing on how growth might be shared more equitably and noting the correlation between high-top tax rate periods and strong economic growth. We also asked the income tax question in two different ways, so the non-result is unlikely to be an artifact of framing.

In contrast, there are very large effects for the estate tax (cols. 5 and 6), consistent with the results in Sides (2011). At first, we attributed this finding to our treatment having larger effects for topics that held little initial 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.[10] Moreover, there were no more "missing" responses on the estate tax question than on other policies questions among our control group respondents, further evidence that the estate tax is not an obscure issue to respondents in our sample.

A potential 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 in the survey carried out 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. Consistent with our experimental results, observational regression analysis presented by Slemrod shows that support for the estate tax is lower when respondents believe that most families have to pay it. 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. On the other hand, as noted earlier, Kuklinski *et al.* (2003) found that correcting wildly 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" bring to mind (Gilens, 1996). It may be the case that the estate tax is one of a few issues on which voters are highly misinformed but is not linked to racial or other stereotypes. In any case, extrapolating from the estate tax would give vastly biased views of the ability of information treatments to move other redistributive policy preferences.

Indeed, Table 6 presents evidence of the limited effect of the treatment on support for

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

policies that target poverty. The results display a contrast between direct transfer policies such as the EITC and food stamps and indirect transfer policies such as the minimum wage. Columns (1) and (2) of Table 6 show a fairly modest but statistically significant effect of the treatment in terms of increasing support for the minimum wage—it closes about 13 percent of the liberal-conservative gap. In contrast, the effects for the EITC and food stamps are essentially zero and insignificant. Hence, learning about inequality does not seem to translate into more support for direct transfers to the poor through the EITC or food stamps although it does lead to a small increase in support of indirect transfers such as the minimum wage.[11] We come back to the interpretation of those results in Section 5.

4.3 Views of government

Table 7 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 they answer "always" or "most of the time" as opposed to "only some of the time" and "never."[12] Cols. (1) and (2) show there is a large decrease in the share 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. Note also that, consistent with previous research, the control group has a very low level of trust in government—only about 17 percent are trusting of government, under our definition—and that the contrast of liberals and conservatives about trusting government is fairly small (18.3% vs. 16.1%, see Table 2).

The second question assessed 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,

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

[12] These questions were taken from Gallup.

the treatment significantly moves people toward wanting a more active government (cols. 3 and 4). Similarly, in cols. 5 and 6, the treatment had a large effect on the share of people who prioritize the redistributive role of the federal income tax, as opposed to its role in raising money for (1) infrastructure projects or (2) universal transfer programs like Medicare and Social Security.

In general, 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 them simultaneously see more areas of society where government intervention may be needed but simultaneously make them trust government less. We return to these results in the next section.

4.4 Effects on political involvement

A major critique of survey analysis is that it is difficult to relate respondents' stated views on surveys to whether they will act on these views in the "real world." We made some effort to bridge this gap, though certainly recognize that our attempt is but a small step toward addressing this important issue. We ask individuals whether they would send a petition to their U.S. Senator asking either to raise or lower top tax rates; in some survey waves we asked about the estate tax. We provided a link to Senators' emails and also provided sample messages both for and against raising 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 Table 8. Consistent with the modest effects we find on preferred top income tax rates, there is no significant effect of the treatment for the surveys in which the petition focused on top income tax rates (columns 1 and 2). By contrast and again consistent with previous results, there is a large effect (in the direction of increasing taxes) for the surveys in which the petition focused on the estate tax (columns 3 and 4). We recognize that we are taking respondents' word that they will send the email such that this outcome is not as concrete as, say, knowing with certainty how they would vote in the next election. At the very least, this result confirms the strong effect that the treatment has on views about the estate tax. Note also that, scaled by the liberal vs. conservative contrast, the effect on the estate tax petition (equal to 40% of the liberal/conservative gap) is smaller

than the effect on views about the estate taxes (equal to 204% of the liberal/conservative gap). As such, there is indeed attenuation from attitudes to action, as one would expect.

Interestingly, as shown in columns (5) and (6), the treatment has almost no effect on respondents' choice for president in 2012 (recall that all 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. It might also be the case that the treatment simultaneously makes individuals want the more redistributive policies of the Democratic party *and* distrust the party in power (the Democrats under Obama).

5 Discussion of results

We first report the results from several robustness checks and extensions of the original results, and then offer potential explanations for the pattern of results we reported in the previous section.

5.1 Robustness checks

External validity and re-weighting. As noted earlier, the mTurk sample is not representative of the U.S. population in some important ways. For example, there is a disproportionate number of Asians, younger people and women. We thus used a 2011 CBS political poll to reweight our sample, so that it matched the CBS sample in terms of cells based on the following 32 cells: gender (2) $\times$ age brackets (2) $\times$ white versus non-white (2) $\times$ college degree indicator (2) $\times$ Supported Obama in 2008 (2).

Reweighting does not affect our main results (results available upon request). Mechanically, this result suggests that in general there are limited differential treatment effects. Indeed, when we split the data by gender, political preferences, racial background and age, we find no consistent pattern of certain groups responding more strongly to the treatment in an absolute sense. In a relative sense, conservatives respond more strongly to the treatment because they tend to have a lower baseline agreement with the policy positions we surveyed. There are some differential treatment effects by income and education for certain outcomes, to which we return later in this section.

Bounding the effects of differential attrition. While we showed in Table 3 that, condi-

tional on finishing the survey, assignment to the treatment appears as good as random, here we further probe the potential effects of attrition. To create lower and upper bounds on our treatment effect, we estimate our usual equations when (1) attriters are added back into the sample and we impute for them the largest possible value of the outcome and (2) attriters are added back into the sample and we impute for them the smallest possible value of the outcome. Those bounding results are presented in Appendix Table 2. For each outcome, we present both the lower bound (column L) and the upper bound (column U).

Even under this extreme formulation, support for the "is inequality a very serious problem" question and an increase in the estate tax is positive and statistically significant under both bounds. The millionaire's tax and trust in government effects flip signs depending on the bound.

We also performed another demanding but not quite as extreme bounding exercise, comparing the effects if we assume that everyone who attrits would have the average "liberal" view versus assuming they would have the average "conservative" view. The results in Appendix Table 3 shows that no signs flip under either the conservative (columns labeled "C") or liberal (columns labeled "L") assumptions. Given that attrition does not actually vary by political views (see Appendix Table 1) but that outcome values vary substantially by political views (see Table 2), this test would seem to provide a generous upper bound on the effects of attrition.

Finally, we verify that our main results are not being driven by waves in which differential attrition between the treatment and control groups were especially high. To that end, we split waves into high- and low-differential attrition categories and compare the main treatment effects (see Appendix Table 4 for attrition statistics by wave).[13] Appendix Table 5 show that the treatment effect for "inequality is a problem" and the estate tax are nearly identical across these two groups, while the top tax rate is no longer significant when considering only low-differential-attrition waves. Appendix Table 6 shows that the result for the minimum wage and the (non-)results for the other poverty programs are also robust to the choice of wave.

Cols. (7) and (8) of Appendix Table 6 shows that the negative effect of the treatment on trust in government is substantially larger when looking at only low-differential-attrition

[13]Specifically, waves 1, 2 and 6 are in the low-differential-attrition group and others are in the high-differential-attrition group.

waves. In fact, the somewhat puzzling results in Table 7—that the treatment both decreased trust in government while making respondents want a more activist and redistributive government—appear largely reconciled. The treatment only triggers activist and redistributive preferences in rounds with more differential attrition (cols. 10 and 12), and thus we conclude that the negative effect of the treatment on trust in government is the most robust of our results relating to views on government.

In summary, in the low-differential-attrition waves (where our identification is likely most robust), the main set of results we have presented are even stronger—that while relatively easy to move opinions about inequality, the effects on policy (outside of the estate tax) are more modest, which can potentially be explained by a substantial decrease in respondents' trust in government.

The most widely used method to tackle differential attrition issues is to re-weight 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 wave of the experiment. Hence, our comparison across waves is the simplest and most transparent non-parametric form of DFL reweighing.

Robustness across waves. We also verified that no single wave of the survey was driving our results. Appendix Figure 1 depicts the coefficient of interest for "inequality is a serious problem," increasing taxes on millionaires, and increasing the estate tax, by wave. As we discuss in the next section, there were some significant methodological differences across waves and of course waves took place at different times with different stories dominating the news cycle—as such, the stability of the treatment effect is encouraging.

Survey fatigue. Finally, "survey fatigue" would not seem to explain our results. While "do you think inequality is a serious problem" was typically the first outcome question, there does not appear to be any pattern between the order of the questions and the tendency for the treatment to affect how respondents answer them. For example, the preferred top tax rate question was always asked before the estate tax question, yet the latter saw much larger effects. While we did not have the sample size to randomize outcome question order, at the very least there does not appear to be a monotonic relationship between question order and the magnitude of the treatment effect.

5.2 Are treatment effects ephemeral?

One common critique of survey experiments is that they can only measure the immediate effect of an informational treatment. In survey wave 4, we attempted to recontact respondents one month after taking the survey. Out of 1087 respondents who completed the original survey, 187 completed the follow-up survey. The follow-up survey asked the same outcome questions as the original survey, but did not include the informational treatment. Of our main covariates, whites and married individuals are less likely to complete the follow-up, but, perhaps surprisingly, political identification and voting for Obama in 2008 do not predict whether individuals complete the follow-up.[14] Those assigned to the treatment arm in the first wave are no more or less likely to respond to the follow-up survey than those assigned to the control arm.

We compare the answers for these 187 respondents in the original and follow-up survey in Table 9. Given the small sample size, the precision of the estimates fall. However, the table shows that the results for the preferred tax rate on the top 1%, whether the rich deserve their income, the role of the government, and whether the estate tax should be increased remain significant in the follow-up survey. In contrast, for this small sample, the effect of the treatment on whether inequality is a serious problem is not significant in either wave, though in both they are of similar magnitude and in the expected direction. Given the small sample size, we view these results as quite encouraging of the notion that even brief information treatments can have lasting effects.

5.3 What aspects of the treatment move preferences?

Our main motivation was to present a variety of information in our information treatment to in some sense estimate an upper bound on how much policy preferences can be moved. Outside of the estate tax, this upper bound appears quite small.

We were also interested in what aspects of our treatment appeared to move preferences the most. In wave 6, we broke down our treatment by creating four experimental groups: a control, a treatment group who only saw information on the level and growth of inequality (including the fact that the estate tax is paid by a very small fraction of families), a treatment

[14]Because of the small sample size, we do not include our usual battery of control variables. Because of the differential take-up, we do include race and marital status. We also include gender and political orientation since these variables have such large effects on many outcome variables.

group who only saw information on the empirical correlation between high top tax rates and robust economic growth, and a treatment group that saw both informational treatments. In our previous analyses reported above, we included only the pure control group and the full treatment group so as to be comparable to the other waves.

Appendix Table 7 presents these results. For our main outcomes (inequality is a problem, trust in government, and the estate tax), more information appears to create stronger results. However, in most cases, the information about the level and growth of inequality produces almost the same effect as the combined informational treatment.

5.4 Differential treatment effects by socio-economic status

One of the more puzzling results from the previous section is the lack of effect on policies related to poverty—a small effect on increasing the minimum wage, and no effect on the support for food stamps or the EITC. To try and understand better those counter-intuitive findings, we estimate treatment effects by socio-economic status based on reported household income or education.

We restrict the sample to those at least 23 years old, to exclude college students, who may not yet have completed their education and whose household income measure may not reflect their actual economic well-being, though results are robust to including them. We categorize someone as "poor" if they report household income below $20,000, a definition 23.5 percent of our usual sample and 21.3 percent of our over-age-22 sample meet.[15] For education we use as our marker of low-status not having either a two- or four-year college degree (forty percent of our sample meets this definition).[16]

The first three columns of Table 10 show the results when the usual controls are included,

[15] As a comparison, in 2011, the 20th percentile of the income distribution for all U.S. households was $20,262 (see `http://www.census.gov/prod/2012pubs/p60-243.pdf`, Table A-2.) As such, along this measure, about the same share of families would be "poor" in our sample and in the overall U.S. population.

[16] In our sample there is almost no income difference between "high school degree" and "some college" among those over age 22 (4.84 versus 4.87 in our categorical income measure, p=0.777), whereas there is a significant difference between "some college" and "two-year degree" (4.87 versus 5.43, $p < 0.001$), which is why we choose the latter as our threshold. A concern is that our market for education status is mechanically related to age, even after we restrict the sample to those at least 23 years old. In fact, within this sample, those with no degree are two years older. Some of this effect could be driven by cohort differences, but when we restrict the sample to those aged 23 to 29, the difference in age between those with and without a degree is essentially zero (0.16 years).

as well as an indicator for being "poor" and its interaction with treatment status. In contrast to the earlier results on poverty policies, the main effect of the treatment is now significant for the food stamps and EITC outcomes. That is, if we had eliminated "poor" respondents from our sample, we would have seen the positive effect of the treatment on these poverty-reduction policies that we had *a priori* anticipated. The interaction term suggest that poor respondents have different—in some cases significantly different—responses to the treatment than do other respondents. They respond less to the treatment than do other respondents and in the case of the EITC in the opposite direction. We repeat this analysis using education as our marker for status in the final three columns. Again, the main effects of the treatment are all significant and in the direction of encouraging support for poverty programs. The interactions with not having a degree are all negative, in the case of food stamps significantly so and in the case of the EITC with a p-value of 0.126.

We take from this analysis that individuals who likely feel more confident about their socio-economic status react as we expected by becoming more supportive of transfer programs for the poor after learning about inequality. In contrast, those with low socio-economic status become less supportive to transfer programs. Importantly, those in the poor category show no consistent difference with others in terms of the treatment effects of "inequality is very serious," the preferred top tax rate and the estate tax outcomes. And if anything, those without a degree respond more strongly to the treatment on these measures than do other respondents, making their lack of response to poverty-reduction policies even more striking.

To further explore the role of financial anxiety in shaping redistributive preferences, we conducted a related mTurk experiment between January and March of 2012. In this experiment, respondents were randomized into reading either good or bad news about the economy (the positive-information and negative-information treatments can be found in Appendix Figures 7 and 8, respectively). Given the mixed state of the economy during this period, it was easy to find compelling facts supporting both sides. Our claim is that negative (positive) information about the economy diminishes (improves) respondents' views of both their absolute *and relative* future position in the income distribution. Indeed, using GSS data, we find that during periods of high unemployment, respondents' view of their *relative* place in the income distribution falls—the point-estimates suggest that a five-percentage-point increase in unemployment such as the one witnessed during the most recent recession increases by roughly 25 percent the share of respondents who feel they have "below average"

income (results available upon request).

Presumably because the treatments are very short, there is almost no attrition (2.8 percent). Appendix Table 8 shows how background characteristics (gathered before assignment to treatment) differ based on treatment status.[17] Because there is essentially zero differential attrition (col. 1), any non-uniform distribution of characteristics across treatment arms is due to random sampling variation. Only one out of twelve outcomes shows significant differences across treatment arms.

Our first outcome on redistributive preferences uses a question from the Gallup Poll: "Do you feel that the distribution of money and wealth in the United States is fair, or do you think money and wealth should be more evenly distributed among a larger percentage of people?" The second outcome question comes from the General Social Survey, asking respondents to place themselves on a one-to-seven scale based on relative agreement with the following positions: "Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor." The third question we devised ourselves in order to connect with a current policy debate: "Politicians are currently debating how the United States should address its long-run deficit problem. Which statement comes closest to the way you feel: The deficit should be mainly addressed with higher taxes on the wealthy or it should be mainly addressed by cutting social services such as Medicare, Medicaid, Unemployment Insurance and Food Stamps."

To test whether reading negative information about the economy as opposed to positive information affects the preferences of those with high and low family income, we divide respondents based on whether they are above or below a family income of $50,000 (roughly the median in our data). We use a higher cut-off here than with the main survey experiment because given the GSS results, the negative treatment would presumably make many below-median-income respondents worry that they might fall into a poorer part of the distribution in the near future.

The first column of Table 11 shows regression results when the Gallup question is re-

[17]The table shows regressions using all observations, regardless of whether they finished the survey, though given the low attrition rates re-estimating these regressions among those who finished yield almost identical results (and are available up on request).

gressed against a dummy for being assigned the negative-information treatment, its interaction with a below-median-income dummy, and income-category fixed effects. The positive and significant coefficient on the main effect of the negative-information treatment dummy suggests that for those with above-median income, learning negative as opposed to positive information about the economy heightens their sense that the current income distribution is unfair and should be changed to the benefit of the less well-off. However, the large, negative and significant coefficient on the interaction term suggests that lower-income respondents react in the opposite direction.

The result is strengthened by adding as controls all the outcomes in Table 3 plus state fixed effects (col 2) and dropping those under age 26 (col. 3).[18] In fact, in cols. (2) and (3), summing the main effect and the interaction suggests that low-income respondents significantly reduce their redistributive sentiment in an *absolute* sense upon hearing negative as opposed to positive news, not just relative to other respondents.

The results are slightly weaker for the GSS question on whether the government should reduce income differences, though the patterns are the same and the interaction term coefficient is significant once controls are added. In col. (6), summing coefficients yields a negative and significant absolute reaction against redistribution for low-income respondents assigned to the negative treatment.

The results on policies to reduce the deficit are the most striking. Learning negative information about the economy has a strong, positive effect on higher-income respondents' tendency to say that deficit reduction should be mostly accomplished via increasing taxes on the wealthy (as opposed to cutting social services). By contrast, adding the main effect and interaction coefficients yields a negative and significant effect of the treatment for lower-income respondents. The baseline share of low-income respondents saying that deficit reduction should be mostly accomplished via cutting social services is 17 percent, so taking the coefficients in col. (8) the negative-information treatment represents a $\frac{(0.176-0.0993)}{0.17} = 45$ percent increase in the share of low-income respondents favoring social-service cuts versus tax increases on the rich.

Several (non-exclusive) mechanisms could explain those heterogeneous treatment effects by socio-economic status.

First, social-desirability survey bias might cause low-income individuals to turn against

[18] In this survey, we only asked age in brackets (e.g., 18-25).

redistribution in a survey after having their low status emphasized so as not to appear too selfish by supporting self-serving policies. For example, those likely to benefit from means-tested transfers are less likely to report supporting such policies if the treatment has emphasized that they are current likely beneficiaries (due to their low position in the income distribution revealed by the inequality treatment) or likely to become beneficiaries (due to the deteriorating economic news treatment).

Second, Kuziemko *et al.* (2011) argue with both observational and experimental evidence that individuals are "last-place" averse, and thus status concerns make low-income individuals wary of certain redistributive policies, lest they differentially help the group below them. Given that our treatment focused on the income distribution, status concerns related to income are likely heightened by the treatment. Similarly, the negative economic news treatment likely increases status concerns. Therefore, our treatments could undermine the support for poverty-reduction policies and diminish the redistributive sentiments more generally among those with low socio-economic status. Last-place aversion would thus suggest that lower-status respondents would respond less favorably to the treatment in terms of support for poverty-reduction than would other respondents.

6 Methodological issues related to online survey experiments

In this section, we share what we have learned through our experience with these 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.

6.1 How representative is the mTurk sample?

As noted earlier, selection into our survey is not representative of the U.S. population. However, it 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).

6.2 How serious is attrition in the mTurk sample?

As noted earlier, 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 wave and a 22 percent rate for the Amazon waves. In fact, when we compare waves 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.[19] 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.

We created and tested several different methods intended to either decrease overall attrition or decrease the differences in attrition between the treatment and the control. Appendix Table 4 describes these attempts and their relative success. We caution readers not to read these results too literally, as the surveys varied in other ways as well (e.g., their length, the dates they were in the field).

We first experimented with "attrition inducement devices." The idea is to induce respondents who are unlikely to finish the survey to drop out before they are assigned to a

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

treatment status, decreasing differential attrition by treatment status.[20] We implemented this method by asking all respondents to read through a lengthy academic paragraph on the teaching of the scientific method and write a few sentences sharing their reaction. We then administered the usual survey. As Appendix Table 4 shows, it appears to have had the desired effect of narrowing attrition between the treatment and control group.

We also experimented with making the control group experience an informational treatment, but on a topic unrelated to the later outcome questions. To this end, we showed information on climate change to the control group, with the idea that the same types of respondents who would be turned off by information on inequality would be turned off by information on climate change. This method had no effect on the differential attrition rate.

We also tried varying respondents' payment for finishing the survey. This method also had no effect on attrition. In fact, taken literally, paying slightly more seems to increase attrition.

However, as shown in the previous section, the main results are stable across waves, despite different overall and relative attrition rates. As demonstrated in Section 3, attrition generally does not seem to exhibit much *selection* and in particular selective attrition conditional on being assigned to treatment does not appear to be a serious concern. All of these results suggest that attrition was due mostly to the extra time required to complete the survey in the treatment group rather than a reaction to the information being presented in the treatment. Indeed, our follow-up experiment exploring the effects of positive and negative information about the economy—for which the treatments were markedly shorter—had an attrition rate of under three percent. Given that our treatment in the main survey experiment is probably on the more sensitive side—people tend to have strong views about taxes and fairness—the lack of selective attrition conditional on treatment in our experiment bodes well for future work on less controversial subjects.

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.

[20]We thank Todd Rogers for this suggestion.

6.3 Can researchers use mTurk to conduct longitudinal analysis?

One disadvantage of mTurk is the difficulty in following up with survey respondents. We attempted to do so for wave 4, but only twenty percent (187 observations) of the original respondents participated in the follow-up, and, of course, this group is a selected sample (though, importantly, they were not selected by political views, the factor most likely to cause selection bias). We informed workers during the first survey that there would be a follow up in a month's time and sent multiple reminders through the mTurk messaging platform once the follow-up was posted.

There are several pitfalls that future researchers can avoid in order to maximize take-up in a follow-up survey. First, if the survey data is collected on another platform and mTurk just serves as a sampling device, then researchers should ask workers to explicitly provide their mTurk Worker IDs during the survey in the survey platform—having the worker IDs on the mTurk platform, separate from the answers each worker gave on the survey will not be sufficient to jointly analyze the data with the follow-up data. Second, given the very rapid turnover on mTurk, it is important to keep the time span between the two waves of the survey short, between two to four weeks at most. Third, it is difficult to email reminders to workers: their email addresses are not provided, such that the only options are to write a script which will send message reminders through the mTurk platform (but that requires workers to log in in the first place to see their messages), or to ask workers for their email addresses, which in many cases violates mTurk's current privacy policies. Finally, researchers should also bear in mind that workers on mTurk like to proceed quickly from task to task and often work on a large number of tasks per day. Unless the follow-up survey offers a significant financial incentive, workers will likely not go to the trouble of searching for it on the scheduled date. In general, follow-up surveys will be much easier with a stable panel provided by a survey company. As an example, the take-up on a typical follow-up for the survey company's panel that we used in wave 4 is 70%.

7 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 US—despite historic increases in income concentration. A possible

explanation is that people are unaware of the increase in inequality and greater information would substantially move redistributive preferences. In this paper, we use a novel methodology to examine the determinants of redistributive preferences—randomized online survey experiments with Amazon Mechanical Turk. We find that providing information about the rise of inequality has very large effects on whether respondents' view inequality as an important problem. However, this information has only small effects on their policy views, especially policies targeting poverty. A possible explanation for this seeming disconnect is that the treatment also decreases trust in government, potentially mitigating respondents' willingness to connect concern about inequality into government action. Interestingly, we observe stark contrasts in treatment effects regarding transfer programs when we consider the socio-economic status of respondents. Low income and education groups appear to be less willing to support transfer programs for the poor after our information treatment and in a follow-up experiment we found that below-median-income respondents decrease their support for redistribution after hearing negative information about the economy. Such results are consistent with the "last-place aversion" hypothesis proposed by Kuziemko *et al.* (2011), though future work might explore alternative hypotheses for these surprising divergences by income.

We have also found, consistent with past work, very large effects of information on the estate tax. Informing respondents that the estate tax only affects the top one thousandth richest families dramatically increases support for the estate tax. This result is likely explained at least in part by the enormous misconception of the public regarding the fraction of families subject to the estate tax (Slemrod (2006), Sides (2011)). While in other contexts—most notably welfare policy—information has not been shown to move preferences even when individuals are wildly misinformed, the estate tax may be a special case of an issue where voters are very misinformed and yet not emotionally attached to their position due to ethnic or socioeconomic stereotypes.

We also made some attempt to assess the "real world" effects of survey information. First, we found that many of our main results persist in a follow-up survey a month after the respondents received our information treatments. Second, we found that the treatments increased respondents' willingness to send an email to their U.S. Senator to increase the estate tax (though, again, we cannot verify that these emails were sent). Future research is needed to build on these attempts. For example, an information treatment could be launched

a few weeks before an election; respondents could then be asked after the election if and for whom they voted.

Randomized surveys are a powerful and convenient tool for studying the effects of information treatments on attitudes and beahviors. 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. As such, it becomes more feasible to tease out mechanisms behind results. For example, we were able to easily design a companion experiment examining the effect of financial anxiety on redistributive preferences. Similarly, we have argued that the negative effect of our treatment on trust in government might have dampened the treatment effects on policy preferences—future research could test this hypothesis with an alternative experiment where an additional treatment negatively or positively impacts trust in government and then measures support for various policies. 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.

Largely due to the growth of Medicare costs, the US faces a long-run fiscal imbalance that will require raising taxes, cutting spending, or both.[21] European countries face similar challenges. The distributional effects of tax hikes or spending cuts 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.

[21] See, for example, the Congressional Budget Office's projections (`www.cbo.gov/publication/43288`).

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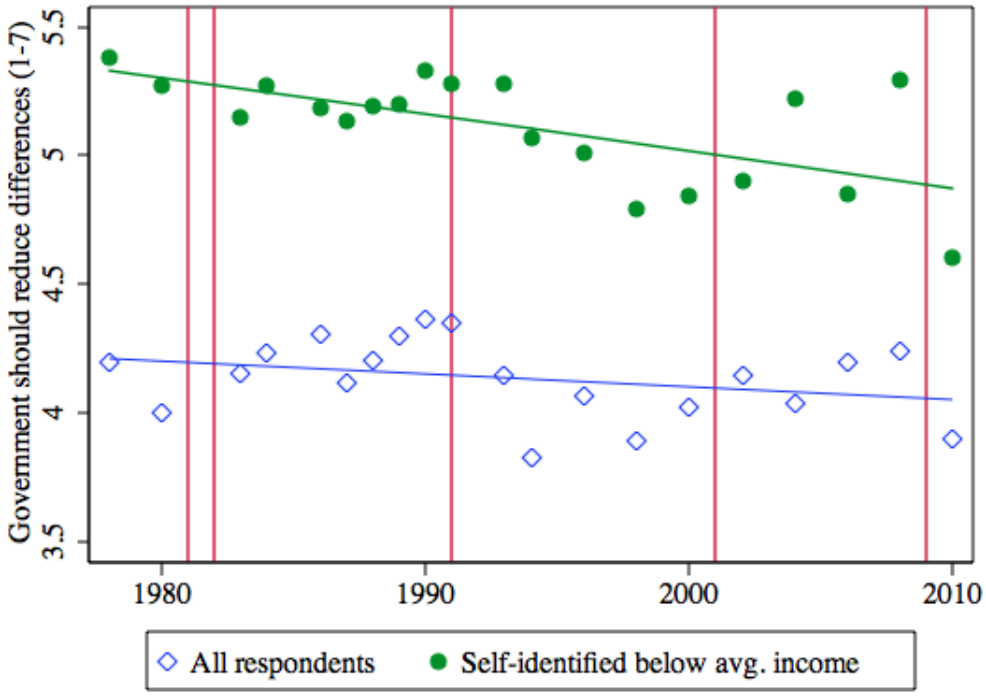


Figure 1: The government should reduce income differences (scale from 1–7, GSS)

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: Unweighted mTurk, CBS News and weighted mTurk samples

	(1) mTurk (unwgted)	(2) CBS	(3) mTurk (wgted)
Male	0.430	0.476	0.476
Age	35.03	48.99	42.82
White	0.784	0.739	0.739
Black	0.0725	0.116	0.102
Hispanic	0.0420	0.0983	0.0550
Asian	0.0729	0.0209	0.0707
Married	0.387	0.594	0.457
Has college degree	0.448	0.318	0.318
Unemployed	0.127	0.104	0.129
Not in labor force	0.140	0.309	0.213
Voted for Obama in 2008	0.670	0.555	0.555
Pol. views, 1 (Cons) to 3 (Lib)	2.176	1.586	1.998
Observations	4527	808	4527

Notes: This table displays summary statistics of survey respondents in our mTurk experiment in column (1) along with national representative averages based on the CBS news surveys in column (2). Column (3) displays the re-weighted summary statistics of our mTurk sample after re-weighting our original sample so that it matched the CBS sample in terms of the 32 cells based on: gender (2) $\times$ age brackets (2) $\times$ white versus non-white (2) $\times$ college degree indicator (2) $\times$ Supported Obama in 2008 (2).

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

	(1) Liberals	(2) Conservatives
Male	0.406	0.474
Age	32.452	39.137
White	0.761	0.848
Black	0.081	0.055
Hispanic	0.040	0.024
Asian	0.085	0.050
Married	0.301	0.540
Has college degree	0.480	0.460
Unemployed	0.140	0.083
Not in labor force	0.091	0.202
Voted for Obama in 2008	0.918	0.287
Top Tax Rate	32.668	23.069
Increase Millionaire Tax	0.901	0.465
Increase Estate Tax	0.254	0.080
Increase min wage	0.814	0.471
Support EITC	0.679	0.402
Support Food Stamps	0.844	0.441
Trust Govt	0.164	0.139
Purpose of Govt is broad	3.537	2.314
Taxes serve to redistribute	0.242	0.102
Send petition for high inc tax	0.286	0.122
Send petition for high estate tax	0.305	0.141
Vote democrat 2012	0.796	0.170

Notes: This table displays summary statistics of survey respondents in the control group 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 (3) is for conservatives (more than three on the scale). The complete wording of those survey questions is reported in appendix.

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

Variable	Coefficient	P-value
Voted for Obama in 2008	0.009	0.564
Age	-0.000	0.552
Liberal policy view	0.002	0.767
Household income	0.003	0.207
Married	-0.014	0.357
Education	-0.004	0.443
Male	0.006	0.666
Black	-0.041	0.156
Hispanic	0.083	0.026
Native	-0.023	0.469
Employed full time	-0.008	0.616
Unemployed	0.014	0.516
Not in labor force	0.011	0.623
Student	-0.017	0.403

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 wave fixed effects. Those tests are used to detect selective attrition (as treatment respondents are approximately ten percentage points less likely to complete the survey than are control respondents).

Table 4: Effect of treatment on opinions about inequality

	Ineq. v. serious		Ineq. increased		Rich deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.113***	0.115***	0.118***	0.118***	-0.0479***	-0.0491***
	[0.0140]	[0.0131]	[0.0117]	[0.0115]	[0.0109]	[0.0103]
Cont gp. mean	0.280	0.280	0.746	0.746	0.180	0.180
Scaled Effect	0.378	0.384	0.539	0.539	0.164	0.168
Covariates?	No	Yes	No	Yes	No	Yes
Obs.	4477	4477	4478	4478	4464	4464

Notes: All outcome variables are binary. "Ineq. v. serious" is a dummy equal to one if respondent says inequality is a "very serious" problem. "Ineq. increased" is a dummy equal to one if respondent says inequality has increased in the US. "Rich deserving" is a dummy for whether respondent think the high earners in our society deserve their pay. All regressions have wave 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. "Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. The row "Cont pg. mean" reports the mean of the outcome variable in the control group. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 5: Effect of treatment on opinions about taxes

	Top tax rate		Millionaire tax		Estate tax	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	1.131**	1.096**	0.0477***	0.0485***	0.360***	0.357***
	[0.485]	[0.472]	[0.0125]	[0.0113]	[0.0145]	[0.0140]
Cont gp. mean	28.96	28.96	0.742	0.742	0.171	0.171
Scaled Effect	0.118	0.114	0.109	0.111	2.063	2.043
Covariates?	No	Yes	No	Yes	No	Yes
Obs.	4521	4521	4521	4521	3673	3673

Notes: "Top rate" is continuous (respondents' preferred average tax rate on the richest one percent) and other outcome variables are binary ("millionaire tax" and "estate tax" coded as one if respondents wants taxes on millionaires and the estate tax to increase, respectively). All regressions have wave 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. "Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. The row "Cont pg. mean" reports the mean of the outcome variable in the control group. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 6: Effect of treatment on opinions about other policies

	Min. wage		EITC		Food stamps	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.0439***	0.0461***	0.0206	0.0229*	0.0138	0.0156
	[0.0137]	[0.0128]	[0.0144]	[0.0138]	[0.0138]	[0.0129]
Cont gp. mean	0.679	0.679	0.577	0.577	0.681	0.681
Scaled Effect	0.128	0.134	0.0743	0.0826	0.0342	0.0386
Covariates?	No	Yes	No	Yes	No	Yes
Obs.	4464	4464	4464	4464	4464	4464

Notes: All outcomes are binary and indicate respondents wish those policies are increased. All regressions have wave 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. Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. The row Cont pg. mean reports the mean of the outcome variable in the control group. The row "Cont pg. mean" reports the mean of the outcome variable in the control group. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 7: Effect on views of government

	Trust gov.		Active gov.		Redistr. taxes	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.0247**	-0.0255**	0.113***	0.117***	0.0378***	0.0385***
	[0.0102]	[0.0101]	[0.0353]	[0.0305]	[0.0122]	[0.0119]
Cont gp. mean	0.153	0.153	3.054	3.054	0.194	0.194
Scaled Effect	1.018	1.050	0.0925	0.0961	0.271	0.276
Covariates?	No	Yes	No	Yes	No	Yes
Obs.	4519	4519	4478	4478	4474	4474

Notes: "Trust gov" is binary indicator for trusting government at least some of the time, "Active gov" is a one-to-five variable for how active you think the government should be, and "Redist. taxes" is a binary variable for whether you believe the primary goal of the federal income tax is redistribution. All regressions have wave 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. "Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. The row Cont pg. mean reports the mean of the outcome variable in the control group. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 8: Effect on political involvement

	Petition, inc. tax		Petition, est. tax		Democrat 2012	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.0321	0.0370	0.0666***	0.0648***	0.0231	0.0206*
	[0.0232]	[0.0230]	[0.0159]	[0.0156]	[0.0149]	[0.0113]
Cont gp. mean	0.235	0.235	0.234	0.234	0.520	0.520
Scaled Effect	0.196	0.226	0.405	0.394	0.0369	0.0329
Cont gp. st dev	0.424	0.424	0.424	0.424	0.500	0.500
Covariates?	No	Yes	No	Yes	No	Yes
Obs.	1390	1390	3060	3060	4477	4477

Notes: "Petition, inc. tax" is a dummy equal to 1 if respondent wants to send a petition to his State Senator asking to increase the income tax. "Petition, est. tax" is a dummy equal to 1 if respondent wants to send a petition to his State Senator asking to increase the estate tax. "Democrat 2012" is a dummy equal to 1 if respondent expresses intention to vote for the Democratic party in 2012 presidential elections. All regressions have wave 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. "Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 9: Results from Follow-up Survey on Selected Variables

	Top Tax		Rich deserving		Govt Purpose		Increase Estate Tax		Ineq v. Serious	
	(1) First	(2) Follow-up	(3) First	(4) Follow-up	(5) First	(6) Follow-up	(7) First	(8) Follow-up	(9) First	(10) Follow-up
Treated	3.786	5.353**	0.149*	0.171*	0.208	0.330**	0.228***	0.203***	0.0505	0.0438
	[2.491]	[2.557]	[0.0901]	[0.0974]	[0.162]	[0.149]	[0.0670]	[0.0658]	[0.0615]	[0.0586]
Cont gp. mean	32.61	29.99	1.986	1.885	-0.0819	2.874	0.181	0.184	0.288	0.230
Obs.	167	167	168	168	168	168	168	168	169	169

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 include race, marital status, age, gender and political orientation. We choose the first two because they marginally predict take-up of the follow-up survey and the last three because they are highly predictive of the outcomes. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 10: Do poor and less-educated respondents respond differently?

	By income			By Education		
	(1) F. Stamps	(2) EITC	(3) Min wage	(4) F. Stamps	(5) EITC	(6) Min wage
Treated	0.0304*	0.0438***	0.0571***	0.0481***	0.0487**	0.0652***
	[0.0158]	[0.0170]	[0.0157]	[0.0179]	[0.0192]	[0.0178]
Poor	0.0627**	-0.0181	-0.0266			
	[0.0296]	[0.0318]	[0.0294]			
Treat*Poor	-0.0293	-0.0666*	-0.000615			
	[0.0345]	[0.0370]	[0.0342]			
No degree				0.0885***	0.0382	0.0115
				[0.0309]	[0.0332]	[0.0307]
Treat*No degree				-0.0607**	-0.0491	-0.0215
				[0.0289]	[0.0311]	[0.0287]
Cont gp. mean	0.681	0.577	0.679	0.681	0.577	0.679
Obs.	3721	3721	3721	3721	3721	3721

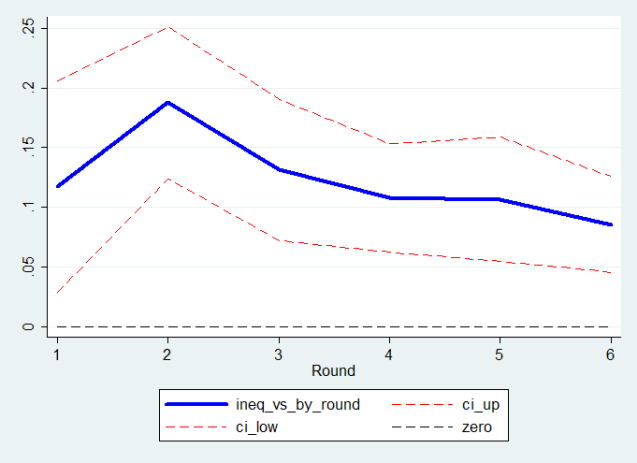
Notes: All outcomes are binary and indicate respondents' wish those policies or programs are made more generous. Controls for each regression' include all variables in the randomization table (Table 3), plus state-of-residence and survey wave fixed effects. The "By education" columns distinguish respondents without a two-year College degree from respondents with a two-year or higher degree. The "By Income" columns distinguish respondents with less than $20,000 dollars annual household income ("Poor") from households with higher income. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

Table 11: Effect of negative information about the economy on support for redistribution

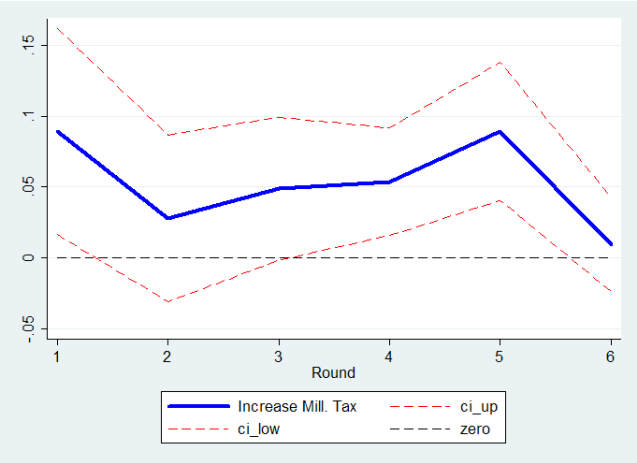
	Distribution unfair			Gov. reduce inc. diffs			Fix deficit by taxing rich		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neg. treat x	-0.150***	-0.169***	-0.216***	-0.428	-0.518**	-0.789**	-0.167***	-0.176***	-0.242***
Below-med. inc.	[0.0565]	[0.0530]	[0.0672]	[0.264]	[0.241]	[0.314]	[0.0546]	[0.0503]	[0.0622]
Negative-info.	0.119***	0.111***	0.113**	0.349*	0.348*	0.345	0.111***	0.0993***	0.123***
treatment	[0.0426]	[0.0398]	[0.0498]	[0.200]	[0.182]	[0.233]	[0.0412]	[0.0379]	[0.0461]
Mean, dept. var.	0.773	0.771	0.753	4.734	4.715	4.672	0.796	0.794	0.790
Controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Ages	All	All	Over 25	All	All	Over 25	All	All	Over 25
Observ.	899	886	583	902	889	585	902	889	585

Notes: All regressions include income-category fixed effects. "Controls" include in addition all variables in the randomization table (Appendix Table 8), plus state-of-residence and survey wave fixed effects. See Appendix Figures 7 and 8 for the information treatments. "Distribution unfair" refers to the question "Do you feel that the distribution of money and wealth in the United States is fair, or do you think money and wealth should be more evenly distributed among a larger percentage of people?" "Gov. reduce inc. diffs" is a one-to-seven scale question on whether the government should aim at reducing differences in income between rich and poor. "Fix deficit by taxing rich" refers to the question "The deficit should be mainly addressed with higher taxes on the wealthy or it should be mainly addressed by cutting social services such as Medicare, Medicaid, Unemployment Insurance and Food Stamps." $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

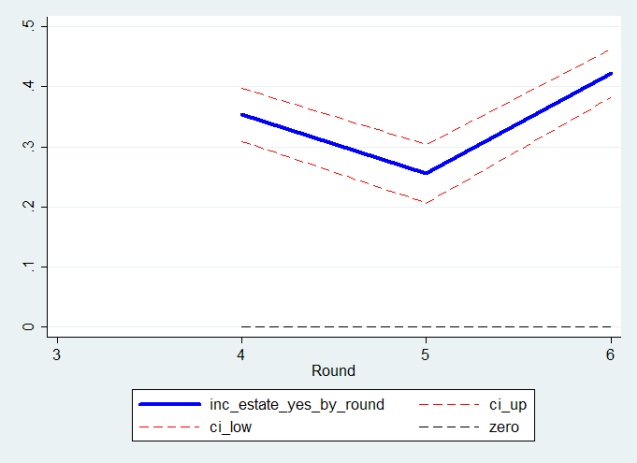
(a) Inequality is a very serious problem



(b) Increase taxes on millionaires



(c) Increase the estate tax



Appendix Figure 1: Treatment effects plotted separately by wave

Notes: The three figures display the coefficients on the variable "Treated" across all six waves. The coefficients are stable across waves, which is reassuring. Still, we add wave fixed effects in all regressions displayed in the tables. The estate tax treatment and questions were only asked in waves 4, 5, and 6.

Appendix Table 1: Ability of covariates to predict whether respondents' finish the survey

Variable	Coeff	P-val
Voted for Obama in 2008	0.017	0.094
Age	-0.003	0.000
Liberal policy view	0.004	0.392
Household income	0.002	0.235
Married	0.006	0.556
Education	-0.000	0.959
Male	0.019	0.055
Black	0.006	0.748
Hispanic	0.057	0.020
Native	-0.002	0.910
Employed full time	-0.006	0.527
Unemployed	0.025	0.088
Not in labor force	-0.032	0.022
Student	0.027	0.049
Treatment Group	-0.110	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 wave fixed effects.

Appendix Table 2: Bounding effects of attrition (using minimum and maximum values for outcomes)

	Ineq. v. serious		Increase Mill. Tax		Increase Estate Tax		Trust Gov	
	(1) L	(2) U	(3) L	(4) U	(5) L	(6) U	(7) L	(8) U
Treated	0.0614***	0.171***	-0.0407***	0.0694***	0.256***	0.367***	-0.0387***	0.0710***
	[0.0121]	[0.0132]	[0.0128]	[0.0109]	[0.0124]	[0.0133]	[0.00888]	[0.0119]
Cont gp. mean	0.249	0.341	0.663	0.755	0.150	0.260	0.143	0.236
Obs.	5458	5458	5456	5456	4639	4639	5457	5457

Notes: "Ineq. v. Serious" is a dummy equal to 1 if the respondent thinks Inequality is a very serious problem. "Top tax" is the respondent's preferred tax rate on the top 1 percent richest. "Estate Tax" is a binary variable equal to 1 if the respondent thinks the estate tax should be increased. "Trust gov" is binary indicator for trusting government at least some of the time. No controls are included. For each dependent variable, Col "L" denotes the lower bound, assuming all attritors replied "No" or chose 0 tax. Col "U" is the upper bound, assuming all attritors would have answered "Yes" or 100 tax. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 3: Bounding effects of attrition (using "liberal" and "conservative" values for outcomes)

	Ineq. v. serious		Increase Mill. Tax		Increase Estate Tax		Trust Gov	
	(1) C	(2) L	(3) C	(4) L	(5) C	(6) L	(7) C	(8) L
Treated	0.0772***	0.109***	0.0217**	0.0540***	0.277***	0.302***	-0.00864	-0.0206**
	[0.0118]	[0.0117]	[0.0108]	[0.0107]	[0.0120]	[0.0118]	[0.00886]	[0.00878]
Cont gp. mean	0.262	0.289	0.715	0.742	0.171	0.195	0.168	0.158
Obs.	5458	5458	5456	5456	4639	4639	5457	5457

Notes: "Ineq. v. Serious" is a dummy equal to 1 if the respondent thinks Inequality is a very serious problem. "Top tax" is the respondent's preferred tax rate on the top 1 percent richest. "Estate Tax" is a binary variable equal to 1 if the respondent thinks the estate tax should be increased. "Trust gov" is binary indicator for trusting government at least some of the time. 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 4: Attrition by wave

Wave	Company	Attempt to lower attrition	Attrition			Obs.	Avg. duration for Tr. gp (mins)
			Tr.	Cont.	Diff.		
1	mTurk	Attrition inducement	0.13	0.07	0.06	502	22
2	mTurk	Climate change tutorial	0.21	0.10	0.11	590	14
3	mTurk	Paid $2	0.28	0.16	0.12	863	17
4	mTurk	Nothing	0.20	0.05	0.15	1189	16
5	C & T	Nothing	0.28	0.15	0.13	1401	16
6	mTurk	Nothing	0.11	0.06	0.05	1566	14
All			0.20	0.09	0.11	6151	16

Notes: Note that the attrition numbers count those respondents who started and continued long enough to be at least assigned a treatment status. Attrition beyond that point is 15% overall (9% in the control group, 20% in treatment group). If we instead consider all respondents (even those who dropped out at the consent page and thus were never assigned a treatment status), attrition is 22 percent. Those respondents could drop out 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 5: Views on inequality and taxes for high- and low-attrition waves

	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	0.114***	0.117***	0.0968***	0.138***	-0.0188	-0.0672***	0.828	2.003***	0.0333**	0.0555***	0.421***	0.365***
	[0.0193]	[0.0219]	[0.0168]	[0.0176]	[0.0155]	[0.0169]	[0.697]	[0.716]	[0.0166]	[0.0187]	[0.0247]	[0.0212]
Cont gp. mean	0.269	0.289	0.775	0.780	0.168	0.180	30.00	28.88	0.766	0.754	0.179	0.178
Scaled Effect	0.355	0.387	0.517	0.615	0.0651	0.209	0.0792	0.203	0.0823	0.108	1.761	2.396
Differential attrition?	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Obs.	2034	1635	2034	1636	2028	1631	2051	1650	2034	1636	1250	1626

Notes: "Ineq. v. serious" is a dummy equal to one if respondent says inequality is a "very serious" problem. "Ineq. increased" is a dummy equal to one if respondent says inequality has increased in the US. "Rich deserving" is a dummy for whether respondent think the high earners in our society deserve their pay. "Top rate" is continuous (respondents' preferred average tax rate on the richest one percent) and other outcome variables are binary ("millionaire tax" and "estate tax" coded as one if respondents wants taxes on millionaires and the estate tax to increase, respectively). "Differential attrition?" separates observations according to whether they were in "Low" differential attrition waves (waves 1, 2, and 6) or in "High" differential attrition waves (waves 3, 4, and 5). All regressions have wave fixed effects and controls for covariates (which include all variables in the randomization table (Table 3), plus state-of-residence). Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. 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: Opinions on other policies and views of government for high- and low-attrition waves

	Min. wage		EITC		Food stamps		Trust gov.		Active gov.		Redistr. taxes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.0468**	0.0408*	0.00939	0.0260	0.0183	0.0197	-0.0511***	0.000987	0.0476	0.248***	0.0169	0.0762***
	[0.0192]	[0.0210]	[0.0205]	[0.0230]	[0.0189]	[0.0210]	[0.0155]	[0.0169]	[0.0442]	[0.0494]	[0.0175]	[0.0194]
Cont gp. mean	0.676	0.706	0.580	0.589	0.690	0.702	0.166	0.133	3.064	2.997	0.191	0.167
Scaled Effect	0.131	0.129	0.0373	0.0808	0.0447	0.0451	2.191	0.0311	0.0361	0.195	0.117	0.496
Differential attrition?	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Obs.	2028	1631	2028	1631	2028	1631	2033	1635	2034	1636	2032	1635

Notes: The first three outcomes are binary and indicate respondents wish those policies are increased. "Trust gov" is binary indicator for trusting government at least some of the time, "Active gov" is a one-to-five variable for how active you think the government should be, and "Redist. taxes" is a binary variable for whether you believe the primary goal of the federal income tax is redistribution. "Differential attrition?" separates observations according to whether they were in "Low" differential attrition waves (waves 1, 2, and 6) or in "High" differential attrition waves (waves 3, 4, and 5). All regressions have wave fixed effects and controls for covariates (which include all variables in the randomization table (Table 3), plus state-of-residence). Scaled effect" is the coefficient on treated scaled by the difference in mean between liberals and conservatives for each corresponding dependent variable. 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 7: Main outcome variables by Treatment Type for Wave 6

	(1) Ineq. v. Serious	(2) Deserve	(3) Trust Gov	(4) Active Gov	(5) Top Tax	(6) Millionaire Tax	(7) Estate Tax	(8) EITC
Pure Inequality Treatment	0.0841***	-0.0206	-0.0239	0.00542	-0.247	-0.00900	0.318***	-0.0198
	[0.0245]	[0.0195]	[0.0199]	[0.0556]	[0.978]	[0.0206]	[0.0247]	[0.0254]
Pure Growth Treatment	0.0576**	0.00520	-0.0381*	0.0202	-1.227	-0.00137	0.0577**	-0.0247
	[0.0242]	[0.0193]	[0.0196]	[0.0547]	[0.964]	[0.0203]	[0.0243]	[0.0251]
Inequality and Growth Treatment	0.0885***	-0.0141	-0.0669***	0.0310	0.104	0.0134	0.420***	-0.0200
	[0.0246]	[0.0195]	[0.0199]	[0.0556]	[0.979]	[0.0206]	[0.0247]	[0.0255]
Obs	2531	2523	2530	2531	2546	2531	2514	2523

Notes: This table shows the effects of two aspects of the treatment that were tested separately in wave 6. The inequality treatment showed the interactive information on inequality as well as the estate tax information while the growth treatment showed that high top income tax rates are positively correlated with economic growth in the U.S. historical record. Wave 6 had a control group, an inequality treatment group, a growth treatment group, and a joint inequality and growth treatment group. The table reports the coefficients on dummies for each of those three treatment groups. Control group is the omitted category. Controls for each regression' include all variables in the randomization table (Table 3), plus state-of-residence and survey-wave fixed effects. Outcome variables are all as described in previous tables.

Appendix Table 8: Randomization across treatment arms in the negative- and positive-information experiment

	(1) Attrit.	(2) Obama	(3) Dem.	(4) Male	(5) Age	(6) Income	(7) Educ.	(8) White	(9) Black	(10) Hisp.	(11) Student	(12) Unemp.
Negative-info.	0.00215	0.0258	0.0108	-0.0173	0.0452	-0.459**	0.0774	-0.0151	0.0194	-0.00430	-0.0259	-0.00864
treatment	[0.0114]	[0.0309]	[0.0319]	[0.0329]	[0.125]	[0.210]	[0.0896]	[0.0250]	[0.0155]	[0.0116]	[0.0240]	[0.0230]
Mean	0.0279	0.670	0.405	0.512	2.894	5.589	5.044	0.815	0.0580	0.0358	0.169	0.138
Observ.	930	930	928	926	930	928	930	930	930	930	926	926

This table tests whether the randomization appears "successful." Outcomes are whether the respondent (1) did not finish the survey; (2) supported Barack Obama in the 2008 presidential election; (3) is male; (8) is white; (9) is black; (10) is Hispanic; (11) is a student; (12) is unemployed. Outcomes. (5) through (7) are categorial measures for age, family income and education. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

A Detailed Description of the Experiment

This Section describes in detail the experiment. The experiment was carried out in 6 separate waves from January 2011 to August 2012 with slight variations. Each wave had the following common structure:
(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 conservativ or liberal on economic policy, voting choice in 2008 presidential election.
(2) Randomized treatment showing information on inequality and tax policy shown solely to the treatment group. Those treatments are summarized in Appendix Figure 9 and illustrated through screenshots in 2, 3, 4, and 5.
(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.

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

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% imposing budget balance (see screenshot).*

2. *Choose the tax rate on the top 1%, next 9%, next 40% and bottom 50% imposing budget balance, and showing actual tax rates.*

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

4. 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?

5. 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:

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

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

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

9. Are you satisfied with your current income?

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

11. 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:

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

13. 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)

14. 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?

15. 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:

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

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

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

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.

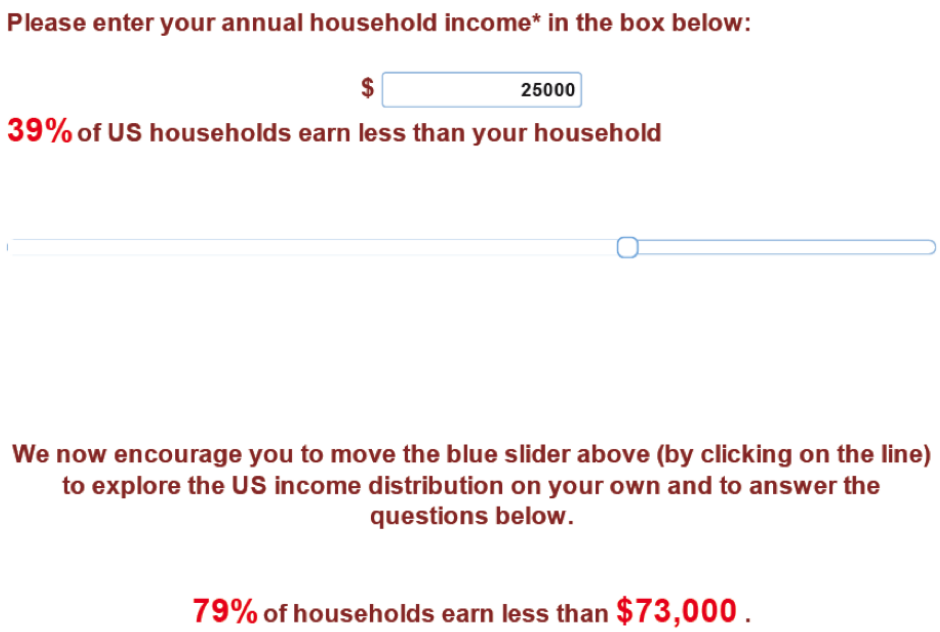
Sample letter for not increasing estate taxes on the rich**:** Dear Senator, In the coming months as you debate the federal budget, one of the priorities for Congress should be keeping estate taxes on the wealthiest Americans low. The government should not punish people who are financially successful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

Appendix Table 9: Summary of all waves

Wave	Date	Sample Size	Treatment Description	Attrition Reducing Method
1	1/30/12	502	1. "Where are you in the distribution" slider. 2. "Where would you have been?" slider 3. Growth and taxes slides	Long, neutral, passage to read before starting survey
2	2/15/12	590	1. "Where are you in the distribution" slider. 2. "Where would you have been?" slider	Climate Change slides for Control Group
3	2/30/2012	863	1. "Where are you in the distribution" slider. 2. "Where would you have been?" slider 3. Growth and taxes slides 5. Estate tax slide	Paid $2
4	5/2/12	1189	1. "Where are you in the distribution" slider. 2. "Where would you have been?" slider 3. Growth and taxes slides 4. Estate tax slide	None
5	4/15/12	1401	1. "Where are you in the distribution" slider. 2. "Where would you have been?" slider 3. Growth and taxes slides 4. Estate tax slide	None
6	8/20/12	1566	A. Pure Inequality Treatment: 1. "Where are you in the distribution" slider 2. "Where would you have been?" slider 4. Estate tax slide B. Pure Growth Treatment 3. Growth and taxes slides C. Inequality and Growth Treatment: All elements of treatments A and B	None

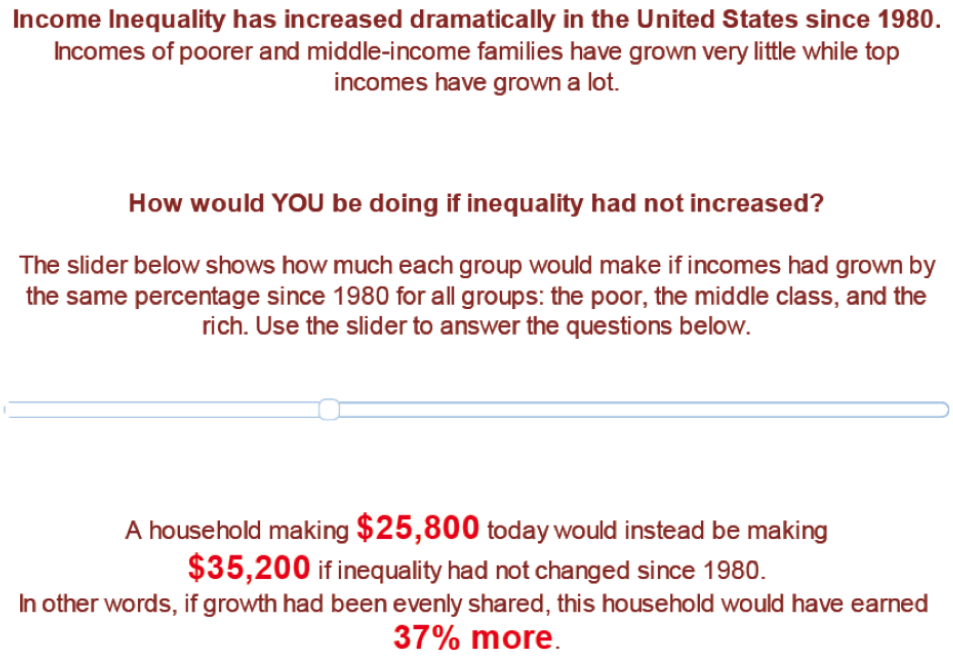
Notes: Background questions asked at the beginning of each survey round were always the same.
Outcome questions are listed next.
Treatments can be seen on the screen shots over the next pages.

Appendix Figure 2: Treatment 1. "Where are you in the income distribution?"



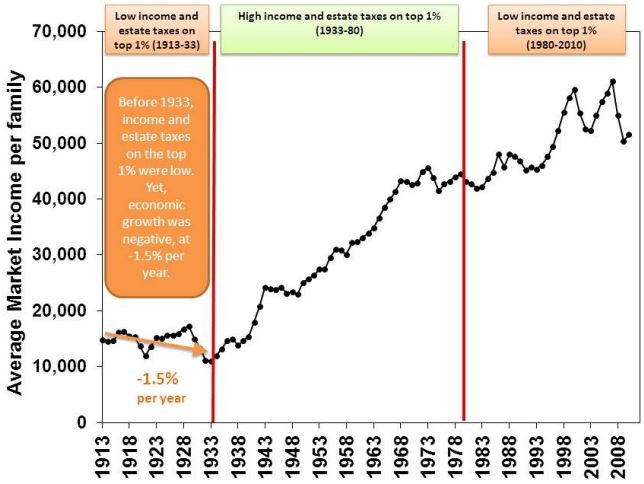
Notes: This interactive slider allows people to explore the income distribution in the US and to determine their position in it. Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`

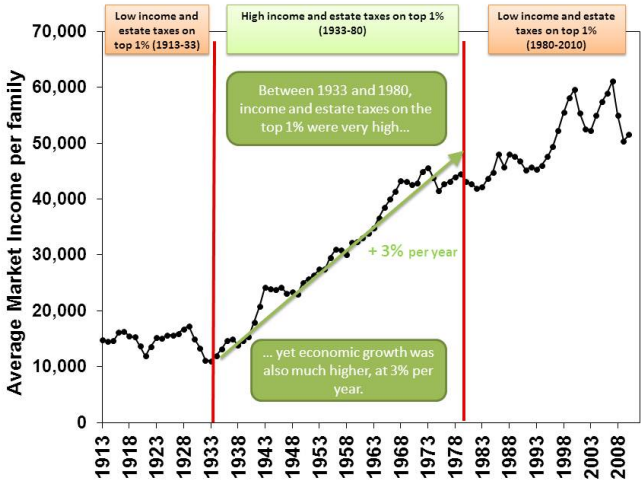
Appendix Figure 3: Treatment 2. "Where would you have been in the income distribution?"

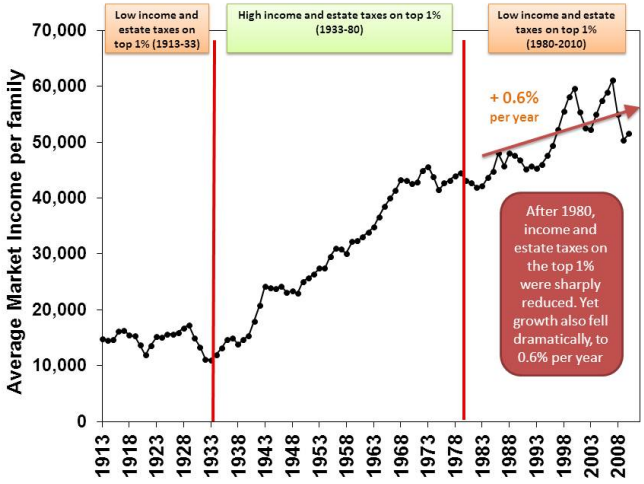


Notes: This interactive slider shows the "counterfactual" household income had income growth been shared equally since 1980. Available online at
`https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`

Appendix Figure 4: Treatment 3. Correlation between growth and top tax rates over time







Appendix Figure 5: Treatment 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.

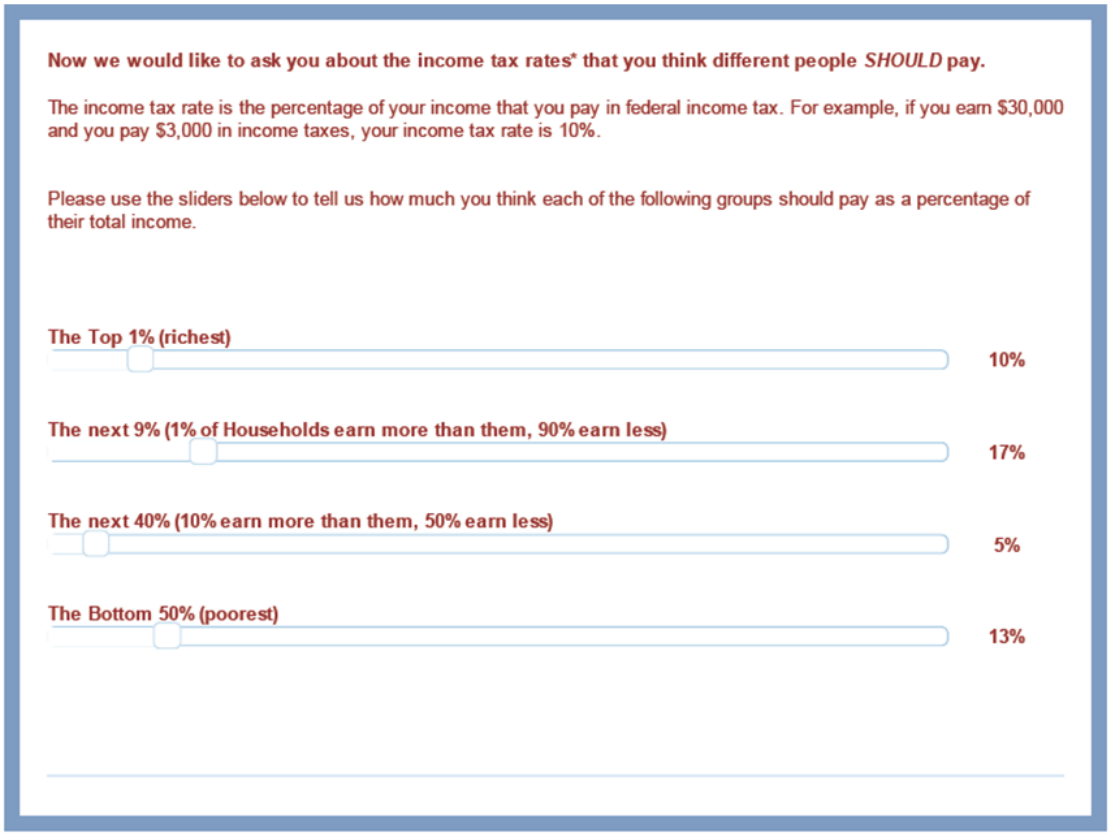
Average Americans do not have anything close to $5 million in wealth, so the estate tax does not affect them and they can pass on their property to their children tax-free.

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 6: Preferred tax rates outcome



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

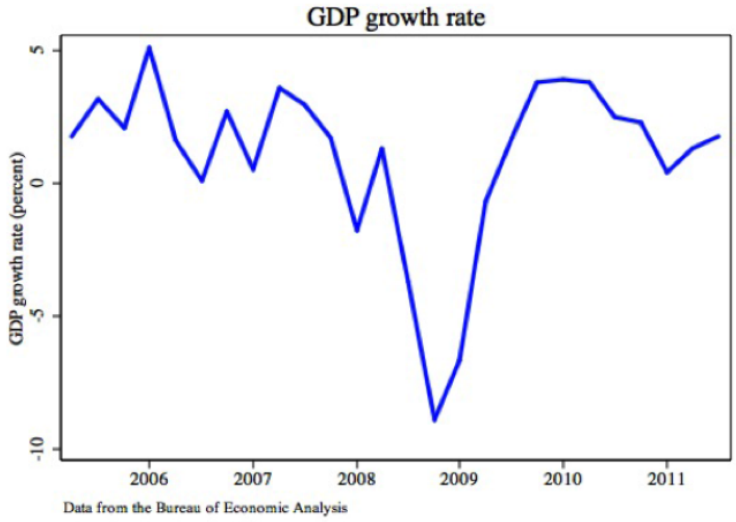
Appendix Figure 7: Business-cycle survey experiment, positive-information treatment

For the remainder of this study we will ask you about your opinions and the current state of the economy in the United States.

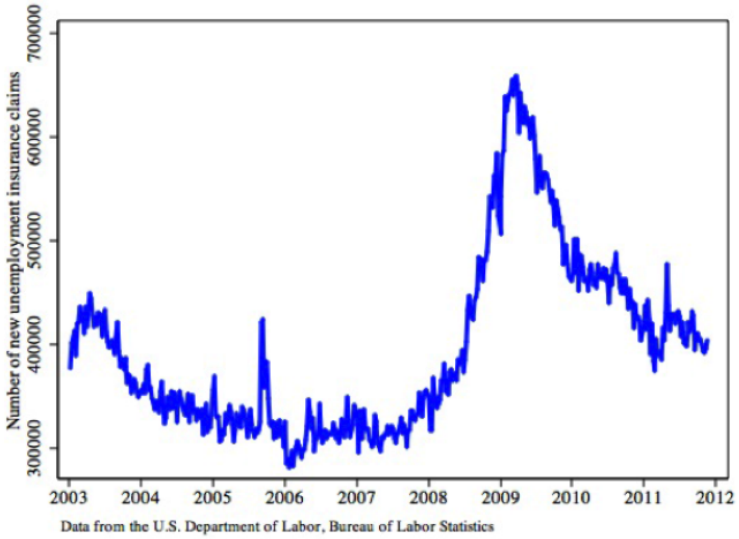
Please read the information carefully and answer the questions that follow.

The U.S. economy has experienced a strong recovery since the financial downturn in 2008.

For example, the growth rate of gross domestic product (GDP) – the market value of all final goods and services produced within a country – has returned to its pre-Recession rate.



The job market has also been steadily improving. The number of new workers applying for Unemployment Insurance—a key measure of employment—has been falling steadily and is nearly at its pre-recession level.



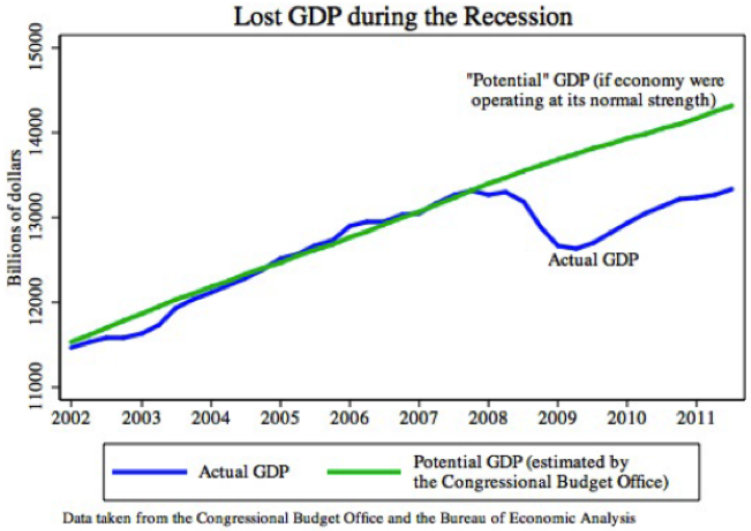
Has the number of Americans applying for Unemployment Insurance been increasing or decreasing recently?

- ○ Increasing
- ○ Decreasing

Appendix Figure 8: Business-cycle survey experiment, negative-information treatment

The United States economy is currently in its worst downturn since the Great Depression.

Gross domestic product (GDP) – the market value of all final goods and services produced within a country – has fallen significantly from its pre-Recession trend, with no indication that it will make up its lost ground anytime soon.



The percent of the U.S. population that is employed has plummeted since 2008, with no recent signs of improvement.

Has the percent of Americans that are employed been increasing or decreasing since 2008?

○ Increasing