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How Elastic Are Preferences for Redistribution? Evidence from Randomized Survey Experiments†

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We analyze randomized online survey experiments providing interactive, customized information on US income inequality, the link between top income tax rates and economic growth, and the estate tax. The treatment has large effects on views about inequality but only slightly moves tax and transfer policy preferences. An exception is the estate tax—informing respondents of the small share of decedents who pay it doubles support for it. The small effects for all other policies can be partially explained by respondents' low trust in government and a disconnect between concerns about social issues and the public policies meant to address them. (*JEL* D31, D72, H23, H24)

The past several decades have seen a large increase in income concentration in the United States. While the top 1 percent of families captured 9.0 percent of total pretax income in 1970, that share rose to 22.4 percent by 2012.[1] More recent work has documented a corresponding trend for wealth concentration: the top 0.1 percent share of wealth has grown from 8 percent in the mid-1970s to 22 percent in 2012 (Saez and Zucman 2014). These trends have not gone unnoticed, at least by some. The Occupy Wall Street movement popularized the term "the 1 percent." Recently, President Obama has called "a dangerous and growing inequality" the "defining challenge of our time," a sentiment echoed by the CEO of Goldman Sachs, who told an interviewer that "too much of the GDP of the country has gone to too few of the people."[2]

There is a large theoretical literature on the link between inequality and redistribution. The most widely used median-voter model predicts that a widening gap between the average and the median income should lead to an increase in redistribution, as politicians respond to the median voter's preferences (Meltzer and Richard 1981).

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

[2] See http://www.whitehouse.gov/the-press-office/2013/12/04/remarks-president-economic-mobility (accessed December 1, 2014) and http://thinkprogress.org/economy/2014/06/13/3448679/goldman-sachs-income-inequality/ (accessed December 1, 2014), respectively.

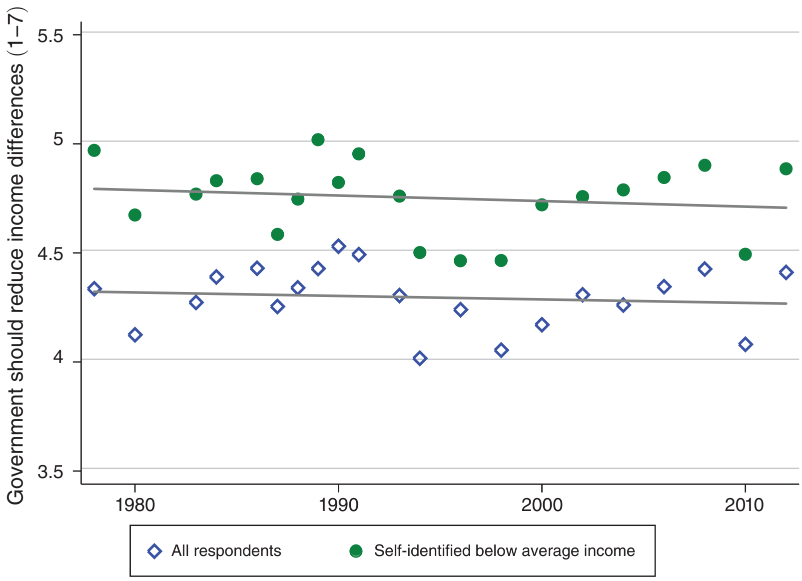

Figure 1. The Government Should Reduce Income Differences (*Scale from 1–7, GSS*)

Notes: 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 is 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 8 so that it is increasing in support for redistribution).

By contrast, top income tax rates as well as inheritance tax rates have fallen in the United States during this period.[3] While for institutional reasons the policy views of the majority might be ignored by policymakers (Bartels 2009), even more challenging to the model's predictions is that survey respondents themselves show no increased demand for redistribution since the 1970s.[4] If anything, the General Social Survey shows that there has been a slight decrease in stated support for redistribution in the United States since the 1970s, even among those who self-identify as having below average income (see Figure 1). These trends have led commentators to suggest that US residents simply do not care about rising inequality.[5]

There are alternative explanations: Americans may be unaware of the extent or growth of inequality (see Kluegel and Smith 1986 and Norton and Ariely 2011), this information may not be sufficiently salient, or they are skeptical about the government's ability to redistribute effectively.[6] In this paper, we extensively examine

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

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

[5] As *Newsweek* put it in 2001: "If Americans couldn't abide rising inequality, we'd now be demonstrating in the streets." Samuelson, Robert J. 2001. "Indifferent to inequality?" *Newsweek*, May 7, p. 45.

[6] A number of alternative theoretical models make different predictions than the median voter model. Corneo and Grüner (2000) propose a model with status effects where the middle class opposes redistribution when this leads to more social competition with bottom income earners. Alesina and Angeletos (2005) and Piketty (1995) show that different beliefs on the role of effort versus luck in success can lead to multiple redistributive equilibria. Bénabou and Ok (2001) show that the prospect for upward mobility can limit the desire for redistribution. There is also a wide empirical literature on the determinants of preferences for redistribution (see, e.g., Corneo and Grüner 2002; Guillaud 2013; and Senik 2009 for evidence from cross-country survey data).

these explanations. We conduct a series of randomized survey experiments using Amazon's Mechanical Turk (mTurk). mTurk is a rapidly growing online platform that can be used to carry out social and survey experiments (see Horton, Rand, and Zeckhauser 2011 and Paolacci, Chandler, and Ipeirotis 2010). In our initial set of experiments, comprising just over 4,000 respondents, one-half of respondents were randomized into an "omnibus" treatment providing interactive, personalized information on US income inequality, the historical correlation between top income tax rates and economic growth, and the incidence of the estate tax. Both control and treatment groups then reported their views on inequality, redistributive policies, and government more generally. We then conducted follow-up experiments with about 6,000 new respondents to analyze potential mechanisms behind the initial results, for a total of approximately 10,000 respondents.[7]

Our treatments exploit the flexibility of the mTurk platform to include several features that heighten the salience of the information we present. First, some of the information we present is *customized.* For example, we ask individuals their household income, allowing us to show them their place in the income distribution, as well as their counterfactual income level had aggregate income growth since 1980 been distributed more equally (so as to leave inequality unchanged). In other parts of the survey, we customize information based on respondents' own household composition.[8] Second, some of the information is interactive—for example, our survey allows respondents to enter different household income levels and the software survey application provides the corresponding percentile, so that the income distribution can be transparently explored.

The initial survey experiment provides several findings we believe to be novel relative to existing literature; the first part of the paper provides a descriptive analysis of these results. First, we find that respondents' concern about inequality is very elastic to information: for example, the treatment increases the share agreeing that inequality is a "very serious problem" by over 35 percent. Put differently, the treatment effect is equal to roughly 36 percent of the gap between self-identified liberals and self-identified conservatives on this question. By contrast, while there are some effects on policy preferences such as top income tax rates, the minimum wage, and food stamps (always in the "expected" direction), they are small and often insignificant despite the large sample size.

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

[7] Survey questions and treatments are all available in the online Appendix.

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

The second half of the paper explores the mechanisms behind the large estate tax effects and the muted response for any other policy outcome. Of course, other explanations may exist and as such we do not view our attempts to tease out the mechanisms behind the main results as definitive. Consistent with past work, we find that respondents are wildly misinformed about the share of decedents subject to the estate tax, which appears to account for the large effects. We further show that the estate tax effect remains strong even when we take steps to decrease the salience and emotional content of the information provided, further proof that more information on this issue has large effects.

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

The first potential explanation is that distrust in government inhibits respondents from translating concern for inequality into support for redistribution *by the government*. Several results from the original survey experiment point in this direction. First, our initial treatment significantly decreases trust in government. When reminded of the extent of inequality (which even control group respondents view as a problem), those in the treatment group appear to at least partially blame the government, perhaps thinking that if politicians "let things get this bad" they cannot be trusted to fix it. Second, beyond any treatment effect, the level of government trust among our sample of mTurk respondents is very low: over 89 percent agree that "Politicians in Washington work to enrich themselves and their largest campaign contributors, instead of working for the benefit of the majority of citizens," with 47 percent "strongly" agreeing. It is thus perhaps not surprising that even when the treatment increases respondents' concern with inequality, they remain reluctant to increase support for government redistributive policies.

In a follow-up survey experiment, we provide direct evidence for the effect of trust in government on respondents' policy preferences. We first asked a small pilot group to answer open-ended questions on their views of government: the main theme that emerges is that politicians are believed to work to enrich themselves and their wealthiest campaign donors. We then used these answers to develop "primes" (e.g., asking respondents' opinions about lobbyists or the Wall Street bailout) that significantly lowered trust in government *without* significantly changing views about the extent of inequality or poverty. Therefore, the treatment isolates the causal effect of decreasing trust in government. We find that the treatment significantly lowers support for all poverty-alleviation policies, with the exception, interestingly, of the minimum wage—a program that does not involve direct transfers from the government. Support for top tax rates generally falls as well (though only some of these effects are significant) and respondents elevate "private charity" over government policies in a list of the best ways to combat inequality. This analysis provides, to our knowledge, the first direct evidence on the causal effects of trust in government on policy preferences, and is particularly relevant given the historically low regard with which Americans currently view their government.[9]

[9] See analysis from Gallup: http://www.gallup.com/poll/164663/americans-trust-government-generally-down-year.aspx (accessed December 1, 2014). The General Social Survey also shows a strongly negative trend. Related

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

We find more support for a third explanation, the idea in Bartels (2005) that the public fails to connect concern for inequality with actual public policy measures. To test this idea, we repeat much of the information in the "emotional appeal" treatment, but then show respondents concretely the resources provided to such families through government programs including the minimum wage and food stamps. Therefore, the treatment directly connects poverty and inequality with policies meant to address them. Emphasizing this connection appears important: treatment respondents significantly increase their support for the minimum wage as well as most of the poverty-alleviation programs that we survey. We view this result as potentially complementary to the trust results: given the low baseline levels of trust, it appears to be the case that policy preferences can only be moved if respondents are explicitly reminded of efficacious examples of government intervention.

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

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

More generally, our paper relates to the literature on the determinants of redistributive preferences, to which political scientists, sociologists, psychologists, and public economists have all contributed. Many papers in this literature use survey data to relate individual traits to redistributive preferences and do not, as we do, take an experimental approach. Alesina and Ferrara (2005); Alesina and Giuliano (2011); and Fong (2001) show that, respectively, prospects for future income mobility, past experience of misfortune, and beliefs about equality of opportunity predict redistributive preferences. Other papers have examined how situational factors (employment status, neighborhood characteristics) predict preferences (see, e.g., Margalit 2013;

to our finding, Yamamura (2014) finds that above average income respondents in Japan are more likely to support redistribution when trust in government in their residential area is high.

Luttmer 2001). Singhal (2008) uses OECD survey data to show that people do not necessarily favor low tax rates at income levels close to theirs, suggesting that redistributive preferences are not completely determined by self-interest.

As in our paper, some researchers have estimated the effects of randomized informational treatments on policy preferences.[10] The evidence from these efforts is mixed. Sides (2011) finds that providing information on the very small number of individuals affected by the estate tax drastically decreases support for its repeal, results that we replicate with our data. Cruces, Perez-Truglia, and Tetaz (2013) find that showing poor individuals their actual place in the income distribution increases their support for policies that target poverty, as most overestimate their income. On the other hand, Kuklinski et al. (2003) find that providing (accurate) information on the demographic composition of welfare recipients and the share of the federal budget dedicated to welfare payments has no effect on respondents' preferences, despite the fact that their initial beliefs are wildly incorrect.[11] We examine a wide variety of redistributive policy outcomes; indeed, we find that the responsiveness of views on the estate tax appears to be an outlier and other outcomes suggest a far more modest effect of information on redistributive preferences.

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

The paper proceeds as follows. Section I introduces the initial survey instrument and data collection procedures. Section II describes the data. Section III presents the main results of the survey experiment. In Section IV, we explore mechanisms behind the large effects of information on views about the estate tax and why most other effects were so limited, reporting methods and results from four follow-up survey experiments. Finally, in Section V, we suggest directions for future work and offer concluding thoughts. All our online surveys, data, and programs are available in the online Appendix.

I. The Main Survey Experiment

The main experiment was implemented in four separate rounds from January 2011 to August 2012. For expositional clarity, to distinguish this initial experiment from the follow-up work we describe in Section IV, we refer to these four initial rounds of surveys as the "omnibus" treatment surveys. The omnibus treatment surveys had the following structure: (i) background socioeconomic questions including

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

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

typical demographic questions as well as political leanings; (ii) randomized treatment providing information on inequality and tax policy (shown solely to the treatment group); and (iii) questions on views on inequality, tax and transfer policies, and government more generally.[12]

A. *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).[13]

Several steps were taken to ensure the validity of the results. First, there are many foreign workers on mTurk, especially from Asia. In addition to requiring respondents to confirm their US residency on the consent form, we also had Amazon show the survey only to workers who had US addresses. Second, to further discourage foreign workers, we tried to launch our surveys during East Coast daylight hours (and, to reduce heterogeneity, only on workdays). Third, to exclude robots, only workers with a past completion rate of at least 90 percent were allowed to take the survey. Fourth, as our survey comprises many rounds, we screen out workers who had participated in a previous round of the survey. Fifth, 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.

B. *The Omnibus Information Treatment*

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

The treatment had three basic parts. First, treatment respondents saw interactive information on the *current* income distribution—they were asked to input their household income and were then told what share of households made more or less than their household. We also asked them to find particular points in the

[12] The online Appendix provides a complete description of the experiment with the questions for each round of the main experiments, and the follow-up experimental rounds discussed later in the paper.

[13] To gauge the external validity of the mTurk results, we gathered data for round 3 using C&T Marketing (http://www.ctmarketinggroup.com/) (accessed December 1, 2014). As noted in Section III, effects are stable across rounds, suggesting that respondents from the two platforms respond similarly to the treatment. Per-participant costs for C&T are roughly five times higher than for mTurk.

distribution—they were asked to find the median and the ninetieth and ninety-ninth percentiles and were encouraged to "play around" with the application. Online Appendix Figure 2 presents a screen shot.[14]

The second part focused not on the current distribution but a counterfactual: respondents entered their current income and were then shown what they "would have made" had economic growth since 1980 been evenly shared across the income distribution (i.e., had the level of inequality stayed the same as in 1980). Of course, this exercise abstracts away from the trade-off between efficiency (economic growth) and equality that would certainly exist at very high levels of taxation. The interactive application allowed them to find this counterfactual value for any point of the current income distribution. Online Appendix Figure 4 presents a screen shot.

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

Readers can directly experience these informational treatments online at the link below.[15] We describe the additional treatments in the follow-up surveys in Section IV.

II. Data

A. *Summary Statistics*

Table 1 shows characteristics of the sample who completed the omnibus treatment survey rounds (we discuss attrition below). We compare these summary statistics to a nationally representative sample of US adults contacted by a Columbia Broadcasting Company (CBS) poll in 2011, which we choose both because it was conducted around the same time as our surveys and asks very similar questions.[16] We also compare it to a more representative (though far more expensive) online panel survey gathered by RAND, the American Life Panel (ALP).[17]

[14] As detailed in online Appendix Table 1 we also ask treatment respondents six "basic comprehension" questions to determine if the information was confusing. With one exception, each question exhibits at least 80 percent comprehension. Moreover, more than 74 percent of respondents answer at least five of the six questions correctly. There are no differential treatment effects by comprehension level (results available upon request), not surprising given that comprehension is at a fairly high (and uniform) level.

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

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

[17] The ALP currently costs researchers $3 per subject per minute, compared to roughly $0.10 per subject per minute for our mTurk surveys. The ALP survey is also limited in sample size.

Table 1—Summary Statistics and Comparison to Other Polling and Online Data

	mTurk sample (1)	CBS election poll (2)	American Life Panel (3)
Male	0.428	0.476	0.417
Age	35.41	48.99	48.94
White (non-Hispanic)	0.778	0.739	0.676
Black	0.0756	0.116	0.109
Hispanic	0.0444	0.0983	0.180
Other racial/ethnic group	0.0759	0.0209	0.0410
Employed (full or part)	0.465	0.587	0.557
Unemployed	0.123	0.104	0.103
Married	0.397	0.594	0.608
Has college degree	0.433	0.318	0.309
Voted for Obama	0.675	0.555	0.559
Political views, conservatives (1) to liberals (3)	2.176	1.586	
Observations	3,741	808	1,002

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

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 presidential election.[18]

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

B. *Survey Attrition*

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

As online Appendix Table 2 shows, attrition is not random, though it is unrelated to 2008 voting preferences and liberal versus conservative policy views (the variables most highly correlated with our outcome variables). The online survey for the treatment group was, by necessity, different from the online survey for

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

TABLE 2—SUMMARY STATISTICS FOR THE CONTROL GROUP, SPLIT BY LIBERALS AND CONSERVATIVES

	Liberals (1)	Conservatives (2)	All (3)
Male	0.407	0.472	0.422
Age	32.618	39.823	35.557
White	0.752	0.838	0.776
Black	0.090	0.063	0.085
Hispanic	0.039	0.027	0.037
Asian	0.090	0.053	0.078
Married	0.302	0.543	0.402
Has college degree	0.462	0.455	0.430
Unemployed	0.140	0.076	0.121
Not in labor force	0.093	0.208	0.144
Voted for Obama in 2008	0.914	0.303	0.674
Inequality has increased	0.836	0.615	0.738
Inequality is a very serious problem	0.414	0.129	0.285
Top tax rate	34.181	23.996	30.205
Increase millionaire tax	0.904	0.452	0.740
Increase estate tax	0.254	0.080	0.171
Increase min. wage	0.822	0.496	0.690
Support food stamps	0.850	0.446	0.686
Support EITC	0.722	0.418	0.611
Trust government	0.171	0.148	0.158
Scope of government is broad	3.552	2.349	3.076
Said would petition for higher income taxes (early rounds only)	0.288	0.118	0.238
Send petition for high estate tax	0.305	0.141	0.234
Plan to vote Democrat 2012	0.800	0.182	0.529
Observations	821	475	1,976

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

the control group. Therefore, a key concern is differential attrition between those assigned to the treatment versus control arms. As the final row of Table 2 shows, attrition is higher among the treatment group (20 percent, versus 9 percent for the control group).[19]

Importantly, however, conditional on finishing the survey, assignment to treatment appears randomly assigned. That is, while the treatment induces attrition *overall*, it does not induce *certain groups* to differentially quit the survey more than others. Table 3 shows the results from estimating (using the sample who complete the survey) 14 separate regressions of the form: $Treatment_{ir} = \beta Covariate_{ir} + \delta_r + \epsilon_{ir}$, where i indexes the individual, r the survey rounds, and δ_r are survey-round fixed effects.[20] For each regression, one of the control variables in Table 2 serves as *Covariate*. Of the 14 regressions, only two (for the *black* and *Hispanic* indicators) yield significant coefficients on the *Covariate* variable. However, given that their

[19] 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.

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

Table 3—Ability of Covariates to Predict Treatment Status, Conditional on Finishing the Survey

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

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

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

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

III. Results from the Omnibus Treatment

We present three sets of results. First, we analyze how the treatment affects respondents' answers to questions related to inequality per se, not policies that might affect it. Second, we analyze specific policies, e.g., raising taxes or increasing the minimum wage. Third, we analyze respondents' views about government as well as their political engagement.

A. *Views on Inequality*

Table 4 presents the effect of the omnibus treatment on questions related to inequality. Odd-numbered columns do not include any controls outside of round

TABLE 4—EFFECT OF OMNIBUS TREATMENT ON OPINIONS ABOUT INEQUALITY
(*"First-Stage" outcomes*)

	Inequality very serious		Inequality increased		Rich deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.102***	0.104***	0.119***	0.120***	−0.0500***	−0.0526***
	[0.0154]	[0.0144]	[0.0130]	[0.0128]	[0.0119]	[0.0114]
Control mean	0.285	0.285	0.738	0.738	0.180	0.180
Scaled effect	0.357	0.365	0.539	0.540	0.173	0.182
Covariates?	No	Yes	No	Yes	No	Yes
Observations	3,703	3,703	3,704	3,704	3,690	3,690

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

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

fixed effects, while even-numbered columns include standard controls (essentially, those listed in Table 3).[21]

Column 1 of Table 4 shows that the treatment is associated with a 10-percentage-point (or 36 percent) increase in the share agreeing that inequality is a "very serious" problem. Similarly, dividing the point-estimate by the "political gap" (i.e., the liberal-conservative control group difference for the outcome variable) suggests that the treatment effect is equal to 36 percent of the political gap on this question (equal to 38 points in Table 2). While a convenient scaling, dividing by the political gap is hardly a perfect metric—while political views are highly predictive of many of our outcomes, this tendency varies and therefore some questions have larger political gaps than others. We thus report both the absolute and scaled effects for all regressions. Adding covariates in column 2 has no effect on the estimated treatment effect.

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

The effects on respondents' opinion of whether the rich are deserving of their income are presented in columns 5 and 6. They are statistically significant, but markedly smaller in magnitude—equal to about 5 percentage points, or one-sixth of the political gap. Therefore, it does not seem that treatment respondents' concern about inequality is being driven primarily by a vilification of the rich.

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

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

Table 5—Effect of Omnibus Treatment on Policy Preferences

	Top rate (1)	$1M tax (2)	Estate (3)	Petition (4)	Min. wage (5)	Trust (6)	Scope (7)	Dem 2012 (8)
Treated	0.931*	0.0502**	0.357***	0.0648***	0.0325**	−0.0292**	0.132***	0.0152
	[0.549]	[0.0126]	[0.0140]	[0.0156]	[0.0141]	[0.0115]	[0.0339]	[0.0125]
Control mean	30.21	0.740	0.171	0.234	0.690	0.158	3.076	0.529
Scaled effect	0.0914	0.111	2.043	0.394	0.0995	1.250	0.110	0.0246
Observations	3,741	3,704	3,673	3,060	3,690	3,702	3,704	3,703

Notes: "Top rate" is continuous (respondents' preferred average tax rate (in percent) on the richest 1 percent). "Scope" is also continuous (a 1–5 variable, increasing in the preferred scope of government activities). All other variables are binary. "$1M tax" and "Estate" indicate the respondent wants income taxes on millionaires and the estate tax to increase, respectively. "Petition" indicates she would write her Senator to increase the estate tax. "Min. wage" indicates support for increasing the minimum wage. "Trust" indicates trust in government and "Dem 2012" indicates the respondent plans to vote for the Democrat (Obama) in the 2012 presidential election. "Covariates" and "scaled effects" are as specified in the notes to Table 4. The row "Control mean" reports the mean of the outcome variable in the control group. All regressions in this and subsequent tables include control variables as defined in Table 4.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

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

B. *Views on Public Policy*

Table 5 presents results for questions related to income and estate taxation. The first two columns report results from the two questions on income taxation—a continuous variable asking respondents to choose an ideal average tax rate for the richest 1 percent and a categorical variable asking them whether taxes on millionaires should be raised—show statistically significant effects of the treatment, in the "expected" direction.[22] However, these magnitudes are small, equal to about 10 percent of the liberal-conservative gap in both cases. For example, the treatment increases the preferred top 1 percent average tax rate by 0.93 percentage points, whereas the gap between liberals and conservatives on this question is slightly over 10 percentage points (see Table 2). Hence, we can rule out an effect on the tax rate for the top 1 percent larger than 20 percent of the liberal conservative gap.

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

By contrast, there are very large effects for the estate tax (column 3 of Table 5), consistent with Sides (2011). The treatment triples the share of respondents supporting an increase in the estate tax, and the effect size is more than double the liberal-conservative gap on this question. We explore in the next section the reasons behind the large estate tax effects. We show that both the informational and the emotional content of the estate

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

tax treatment matter (a purely clinical information treatment with no emotional component increases support by 50 percent instead of more than doubling it).

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

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

While so far we have focused on policies that affect the well-off, we also asked a series of questions about policies that impact the bottom of the income distribution. While the treatment induces significant but small (less than 10 percent of the political gap) effects for the minimum wage (column 5), it induces no significant increase in support for food stamps or the Earned Income Tax Credit (EITC) (results reported in online Appendix Table 4).[23] The results thus suggest a contrast between direct transfer policies such as the EITC and food stamps and indirect transfer policies such as the minimum wage, a theme that will also emerge in some of the follow-up work discussed in the next section.[24]

C. *Views of Government and Political Involvement*

Columns 6–8 of Table 5 reports results on the effect of the treatment on opinions about government. The first question asked respondents: "How much of the time do you think you can trust government in Washington to do what is right?" and we code a respondent as trusting government if she answers "always" or "most of the time" as opposed to "only some of the time" and "never."[25] Column 6 reports a large decrease in the share of treatment respondents agreeing that the government can be trusted. The treatment is equal to the entire liberal-conservative gap, but operates

[23] In later follow-up work, we asked a small pilot group to write open-ended responses to many of our outcome variables. Many respondents had little familiarity with the EITC (though we always provided a description) so the non-result for that outcome might need to be interpreted more cautiously. No respondent indicated unfamiliarity with food stamps or the minimum wage, however.

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

[25] This question is taken from the American National Election Studies (ANES): http://www.electionstudies.org/nesguide/toptable/tab5a_1.htm (accessed December 1, 2014).

in the *opposite* direction to the other outcomes, in that it makes respondents take the more conservative—and less trusting—view on this question.[26] Note also that, consistent with the trends noted in the introduction, the control group has a very low level of trust in government—only about 16 percent are trusting of government, by our definition—and that the contrast of liberals and conservatives about trusting government is fairly small (17 versus 14.5 percent: see Table 2). The low baseline level of trust in the control group suggests that the treatment effect we observe might in fact understate the true effect experienced by the treatment group, as their ability to express an even lower opinion of government is limited by floor effects.

The second question assesses respondents' preferred scope of government: "Next, think more broadly about the purpose of government. Where would you rate yourself on a scale of 1 to 5, where 1 means you think the government should do only those things necessary to provide the most basic government functions, and 5 means you think the government should take active steps in every area it can to try and improve the lives of its citizens?"[27] Intriguingly, the treatment significantly moves people toward wanting a more active government (column 7 of Table 5). Providing information about the growth of inequality and the ability of the government to raise taxes and redistribute have complicated effects on views of government. It appears to make respondents see more areas of society where government intervention may be needed but simultaneously make them trust government less. We return to these results linking trust in government to preferences on government scope in Section IV.

Finally, as shown in column 8, the treatment has almost no effect on respondents' planned voting choice for the 2012 presidential elections (recall that the omnibus-treatment surveys were completed before the November 2012 election). There is at best a marginal effect in the direction of supporting President Obama. This result is consistent with the relatively mild policy effects overall. The treatment may simultaneously make individuals want the more redistributive policies of the Democratic party *and* distrust the party in power (the Democrats under Obama, at least in the executive branch and the Senate).[28]

D. *Robustness Checks*

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

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

[27] This question comes from Gallup.

[28] In the interest of space, there are some outcome variables we relegate to the online Appendix. The full set of all results from the omnibus survey are found in online Appendix Tables 4, 5, 6.

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

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

The flexibility of the mTurk platform offers the possibility of resurveying participants months after the original survey. In the third round of the omnibus survey, we attempted to recontact respondents one month after taking the survey. Out of 1,039 respondents who completed the original survey, 145 (14 percent) completed the follow-up survey. The follow-up survey asked most of the outcome questions in the original survey, but did not include the informational treatment.

With a relatively low take-up rate, a concern is that follow-up respondents are differentially selected. Online Appendix Table 7 suggests that while some selection takes place (by age, marital status, and employment status) the most important variables in terms of predicting preferences (support of Obama and overall liberal-versus-conservative policy views) show no differential selection into the follow-up sample. Nor does initial treatment status predict take-up and thus we have a roughly equal number of control and treatment observations in the follow-up sample.

We compare the original results for these 145 observations to their responses one month later for selected outcomes in Table 6 and for all other outcomes in online Appendix Tables 8 and 9. As only some outcomes show a substantial initial treatment effect for the $N = 145$ subsample, it is not feasible to have meaningful tests of persistence for all outcomes.

Columns 1 and 2 show that our most robust outcome result from the original survey—support for increasing the estate tax—is strongly persistent. In absolute terms, 58 percent of the effect size remains one month later, more than doubling the share who support the policy. And the effect one month later remains highly statistically significant.

Columns 3 and 4 show similarly strong results for views on the proper scope of government. The follow-up result in column 4 actually shows an increase in the point-estimate, though it is within the confidence interval of the result in column 3.

Columns 5 and 6 show that the initial treatment effect on "trust in government" is slightly larger than for the full sample, with a negative, but now insignificant effect persisting one month later.

Unfortunately, as columns 7 and 8 show, one of the main outcome variables from the omnibus survey—concern for inequality—yields an initial treatment effect of essentially zero for the subsample, and thus testing for persistence is not particularly meaningful. Given that our initial treatment often had small effects for the entire sample, it is not surprising that only some outcomes yield substantial initial

Table 6—Results from the Follow-Up Survey One Month Later

	Increase estate tax		Government scope		Trust government		Inequality very serious	
	First (1)	Follow-up (2)	First (3)	Follow-up (4)	First (5)	Follow-up (6)	First (7)	Follow-up (8)
Treated	0.337***	0.195***	0.259	0.364*	−0.122**	−0.0691	0.00833	0.102
	[0.0953]	[0.0910]	[0.207]	[0.200]	[0.0611]	[0.0582]	[0.0809]	[0.0770]
Control mean	0.180	0.179	2.995	2.910	0.122	0.128	0.283	0.218
Observations	145	145	145	145	145	145	145	145

Notes: All outcomes and terms are as defined in Tables 4 and 5. For each dependent variable, column "First" is the result from the first survey, while column "Follow-up" is the result from the follow-up survey one month after the initial survey. We use a more limited set of control variables given the small sample size. All regressions are run on the subsample of respondents who entirely completed the follow up survey.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

treatment effects for the subsample. We thus relegate the follow-up results for all these outcomes to online Appendix Tables 8 and 9.

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

Bounding the Effects of Differential Attrition.—While we showed in Table 3 that, conditional on finishing the survey, assignment to the treatment appears as good as random, here we further probe the potential effects of attrition. To conduct a bounding exercise, we assume either that (i) attriters would have all had the average "liberal" view for each outcome; or (ii) they would have had the average "conservative" view for each outcome. Given that attrition does not actually vary by political views (see online Appendix Table 2) but outcome values vary substantially by political views (see Table 2), this test should provide generous upper bounds on the potential effects of attrition. The results in online Appendix Table 10 shows that no signs flip for any of our main outcome variables under either the conservative (columns labeled "C") or liberal (columns labeled "L") attrition assumptions.

Next, we examine how the level of differential attrition affects our results: do our results only hold in survey rounds with high differential attrition between the control and treatment group? The first three rounds of the omnibus surveys had very similar differential attrition rates (between 12 and 16 percent), whereas the fourth had a substantially lower attrition rate (5 percent). Online Appendix Tables 11 and 12 show that our main results on concern for inequality, support for the estate tax, and trust in government are robust and at times stronger for the low-differential-attrition round, the round where we expect our identification assumptions to be most robust. As before, the "non-results" for other outcomes remain (not shown).[30] In Section IV, we analyze follow-up surveys where the treatment is much shorter and where there is virtually no differential attrition by treatment status (see online

[30] In our context, the only observable variable that is correlated with differential attrition between control and treatment is the round of the experiment. Hence, our comparison across rounds is the simplest and most transparent nonparametric form of DiNardo-Fortin-Lemieux (DFL) reweighting.

Appendix Table 3). The fact that these follow-up surveys largely confirm our omnibus treatment results provides further reassurance that differential attrition is not driving our results.

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

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

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

IV. Understanding Our Results with Follow-up Surveys

The follow-up surveys share the following structure. While we repeated most of the outcome questions used in the omnibus surveys, there are some differences (based on input from referees and others). For example, we ask respondents to report whether "poverty is a serious problem," as well as rank "private charity" and "education" in a list of tools to address inequality (so as to gauge whether respondents react to the treatments by turning to options—some nongovernmental—more often advocated by political conservatives).[31] We also replace the question about the EITC (which we feared might not be sufficiently familiar to respondents) with a general question about "aid to the poor" and a specific question about public housing, while retaining the minimum wage and food stamp questions.

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

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

outcomes are reported in the online Appendix for each treatment. Importantly, in this section, we use data solely from the follow-up surveys because the control groups in the omnibus survey and the follow-up surveys are not directly comparable, and because the wording of some of the questions has changed.

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

A. *Why Are Views about the Estate Tax So Elastic to Information?*

In this section, we present two types of follow-up analysis on the estate tax. First, we verify whether the estate tax treatment effect—an outlier among the policy outcomes analyzed in the previous section—is truly robust. After showing that it withstands several significant modifications of the treatment, we then present evidence as to *why* this effect is so strong. Our view is that misinformation about the estate tax is far greater than for the other policies we surveyed, such that the informational treatment has an especially large impact.[32]

Verifying the Large Estate Tax Effects.—Recall that the omnibus treatment includes not only information about the incidence of the estate tax, but several other components as well (e.g., the interactive feature showing respondents' place in the income distribution, among the others we described in Section I). To gauge the sensitivity of the estate tax effect to this additional information, we redid the experiment with a treatment that *only* included the slide on the estate tax. Furthermore, the original estate tax treatment shows a picture of a mansion and notes that the estate tax can help "level the playing field" (see online Appendix Figure 4). We thus formulated a treatment that decreased the emotional impact of the estate tax treatment and that only mentions the incidence in dry, factual terms (see online Appendix Figure 6). We call the first version the "emotional estate tax treatment" and the second the "neutral estate tax treatment." Again, neither of these treatments contains the other, non-estate-tax components of the omnibus treatment.

Table 7 displays results for the key outcome variables. In contrast to the omnibus treatment, the "emotional" estate tax treatment has no effect on views about whether inequality is a problem, whether it has increased, or whether the rich are "deserving" (columns 1, 2, and 3). The "neutral" treatment appears to have countervailing effects: increasing concern for inequality (though this effect is much smaller than that of the omnibus treatment) while decreasing the sense that inequality has increased. These much weaker effects are not surprising because these two treatments provide no information about income inequality or its trends.

However, column 4 shows that the effect of even these more limited treatments on opinions about the estate tax remains strong. The point estimate for the "emotional

[32] Theoretically, the prospect for upward mobility mechanism of Bénabou and Ok (2001) that limits the desire for redistribution might become irrelevant when respondents realize that only 1 decedent out of 1,000 pays the estate tax. Following Alesina and Angeletos's (2005) theory, respondents might also support the estate tax on the very wealthy as they realize that receiving a very large inheritance is due entirely to luck and not effort of the inheritor.

Table 7—Results from the Estate Tax Survey

	Inequality very serious (1)	Inequality increased (2)	Deserving (3)	Estate tax (4)	Petition (5)	Trust (6)	Estate tax corr. (7)
Treated (emotional)	0.0381	−0.00239	−0.0247	0.289***	0.0313	−0.0164	0.316***
	[0.0258]	[0.0243]	[0.0206]	[0.0258]	[0.0208]	[0.0205]	[0.0263]
Treated (neutral)	0.0511**	−0.0501**	−0.0244	0.109**	0.0239	−0.00558	0.375***
	[0.0259]	[0.0244]	[0.0206]	[0.0259]	[0.0209]	[0.0205]	[0.0264]
Control mean	0.307	0.771	0.174	0.210	0.132	0.153	0.120
Scaled emotional effect	0.118	0.011	0.098	1.085	0.265	0.235	3.386
Scaled neutral effect	0.159	0.223	0.097	0.408	0.202	0.080	4.014
p-value	0.612	0.049	0.991	0.000	0.722	0.598	0.026
Observations	1,777	1,777	1,777	1,777	1,762	1,756	1,773

Notes: The “emotional” treatment repeats the estate tax slide from the omnibus treatment, but eliminates the rest of the treatment. The “neutral” treatment is a version of the “emotional” treatment that attempts to remove any framing effects or emotional appeals to focus solely on the information. The outcomes in columns 1–6 are as defined in Tables 4 and 5. (“Deserving” is the abbreviation for “the rich are deserving of their income”). “Estate tax corr.” indicates that the respondent chose the correct multiple-choice outcome for a question asking what share of people who die are subject to the estate tax. *p*-value is the *p*-value of the test that the coefficients on the treated “emotional” and the treated “neutral” are the same.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

treatment” is nearly as large as that of the omnibus treatment (0.289 versus 0.357). The “neutral” effect is smaller (0.109), though both in absolute and scaled terms swamps any policy effect (excluding the estate tax itself) associated with the omnibus treatment. Recall that the omnibus treatment provided extensive interactive and personalized information on income inequality and income tax rates and typically produced scaled effects on the income tax outcomes of 10 percent. The “neutral” estate tax treatment consisted of a total of four sentences but nonetheless produced a scaled effect on the estate tax four times as large (40.8 percent of the liberal-conservative gap). This stark contrast highlights how much more elastic to information views about the estate tax are than those about the income tax and other policies.

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

Why are Estate Tax Preferences So Malleable to Information?— At first, we attributed this finding to our treatment having larger effects for topics that held little ex ante salience for our respondents. However, recent polling data suggests the

[33] Given that the omnibus treatment had, at best, small effects on income tax and transfer policies, it is not surprising that the estate-tax-only treatments do not produce consistently significant effects on these outcomes either (online Appendix Table 13 and 14).

estate tax is very salient to respondents—in 2010, Gallup respondents named averting an increase in the estate tax as their *top* priority for the lame-duck session of Congress, above extending unemployment benefits and the Bush income tax cuts.[34] Moreover, there were no more "missing" responses on the estate tax question than on other policy questions in the control group, further evidence that the estate tax is not an obscure issue to respondents in our sample.

A more promising explanation is that while respondents may view the estate tax as a salient issue, they may hold misinformed views on the topic. Indeed, as documented by Slemrod (2006), 82 percent of respondents favor estate tax repeal but 49 percent of respondents believe that most families have to pay it, compared to 31 percent who believe only a few families have to pay, and 20 percent who admit to not knowing. In contrast, the public appears much better informed about policies such as the minimum wage or the individual income tax.[35] As a result, providing basic information on how the current federal estate tax is limited to the very wealthiest families might serve as a large informational shock.

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

Column 7 of Table 7 shows the effect of the two estate tax treatments on respondents' likelihood to choose the correct response. Both treatments roughly triple the likelihood of answering correctly, strongly suggesting that information is a key mechanism behind the large effects of the omnibus treatment.[36] Importantly, misinformation is not a sufficient condition for an informational treatment to have large effects. As noted earlier, Kuklinski et al. (2003) found that correcting substantially misinformed views on welfare was not sufficient to change respondents' support, though perhaps the lack of elasticity is due to the racial stereotypes the world "welfare" brings to mind (Gilens 1996). The estate tax may be one of a few issues on which voters are highly misinformed but their ignorance is not linked to racial or other stereotypes. In any case, extrapolating from the estate tax effects would give vastly biased views of the ability of information to move other redistributive policy preferences, as we saw in the previous section and as we further document below.

[34] See http://www.gallup.com/poll/144899/Tax-Issues-Rank-Top-Priority-Lame-Duck-Congress.aspx (accessed December 1, 2014).

[35] A recent Pew survey shows that 73 percent of respondents could identify the correct current minimum wage (see http://www.people-press.org/2014/1%2/from-isis-to-unemployment-what-do-americans-know/) (accessed December 1, 2014). For the individual income tax, we asked respondents in an earlier pilot to give us their best guess of actual average tax rates by income brackets. On average, respondents came fairly close to actual tax rates both in level and in terms of progressivity. For example, for top bracket taxpayers with income above \$379,150, they guessed a tax rate of 29.1 percent on average when the actual tax rate based on 2012 IRS statistics is 26.4 percent. Consistent with these results, Fujii and Hawley (1988) find that, on average, survey respondents perceive fairly accurately the marginal tax rates they face.

[36] Our results offer experimental support for the observational regression analysis presented by Slemrod (2006), none showing that support for the estate tax is lower when respondents believe that most families have to pay it.

B. *Exploring the Limited Treatment Effects on Policy Preferences*

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

Does Government Distrust Explain Limited Treatment Effects?—As documented in Section III, the omnibus treatment significantly reduces trust in government. It is perhaps not surprising that an informational treatment emphasizing a dramatic increase in income inequality would lower respondents' view of government. But, to our knowledge, it remains an open question whether lowering trust in government has a causal effect on policy preferences. This question has perhaps never been more relevant in the US context, given that Americans' trust in government is at historically low levels, as noted in the introduction.

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

We began by collecting a small pilot study ($N \approx 150$) on mTurk where we asked people to answer our basic trust question (how often they can trust the government to do what is right) but then to *explain* why they answered the way they did. Note that they answer this question directly after answering the demographic questions and are thus not being primed to think about inequality. There is no "treatment" in this pilot—we are merely asking people to explain their opinion. The pilot group cast light into why trust in government is currently so low. Respondents feel politicians are out to enrich themselves and their wealthiest donors. "Money," "corporations," and "special interests" are some of the most commonly used words and phrases in these answers, as online Appendix Figure 7 shows. The detailed descriptions given by respondents allowed us to develop primes we thought could lower trust in government without necessarily affecting other factors that would have a direct effect on policy preferences.

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

Results.—Table 8 shows the results of this treatment on a variety of outcomes. Column 1 shows that the first stage "works"—the treatment significantly decreases

Table 8—Effect of Negative Trust Prime on "First-Stage" Variables

	Trust (1)	Scope (2)	Efficient (3)	Inequality very serious (4)	Inequality increased (5)	Poverty versus serious (6)
Treated	−0.0582***	0.0236	−0.0278	0.0547*	0.0119	−0.00257
	[0.0203]	[0.0688]	[0.0346]	[0.0311]	[0.0289]	[0.0313]
Control mean	0.125	3.031	1.423	0.343	0.755	0.383
Scaled trust effect	1.730	0.0170	0.109	0.182	0.341	0.00828
Observations	899	899	898	899	899	899

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

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

respondents' stated trust in government, by 5.8 percentage points in absolute terms or 1.78 times the liberal-conservative gap on this question. This effect is slightly larger than the effect of the omnibus treatment (roughly 1.1 times the liberal-conservative gap), not surprising given that the goal of this treatment was to lower trust. As we saw with the omnibus treatment, respondents appear to separate how much they trust the government with what they view as its proper scope, as the treatment has no effect on that outcome (column 2). The treatment makes them more likely to view the government as wasteful, but the effect is not significant (column 3).

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

Table 9 displays those results. The treatment decreases support for a tax on millionaires, though this result is not quite statistically significant (when the continuous top tax rate is used instead as the outcome, the coefficient is positive but essentially zero, see online Appendix Tables 15 and 16 for this and other results not discussed in the main text). While stated support for expanding the estate tax is essentially unchanged by the treatment, the stated willingness to petition for its expansion is significantly reduced (a scaled effect of 0.588).

The estimated effects of trust on support for transfer programs to the poor are much less equivocal. While support for the minimum wage is unaffected (column 4), treatment respondents significantly reduce their support for "aid to the poor" generally (column 5), and food stamps and public housing specifically (columns 6 and 7). Finally, some interesting results emerge when respondents are asked to rank a list of options for addressing inequality (a higher number here means more

Table 9—Effect of Negative Trust Prime on Outcome Variables

	\$1M tax (1)	Estate tax (2)	Petition (3)	Min. wage (4)	Aid poor (5)	Food stamps (6)	Housing (7)	Private charity (8)
Treated	−0.0421	−0.00168	−0.0602*	−0.00428	−0.139**	−0.153**	−0.163***	0.187**
	[0.0275]	[0.0266]	[0.0236]	[0.0902]	[0.0616]	[0.0673]	[0.0614]	[0.0791]
Control mean	0.722	0.204	0.174	2.673	2.675	2.454	2.581	1.800
Scaled trust effect	0.0949	0.00728	0.580	0.00531	0.128	0.119	0.133	0.169
Observations	899	895	899	899	899	899	899	850

Notes: The negative trust prime treatment consists of several multiple-choice questions that made respondents reflect on aspects of government they dislike. Outcome variables are defined as follows. "Min. wage" is a 0–4 categorical variable increasing in support for the minimum wage (0 indicates most opposition and 4 indicates most support). "Food stamps" is a 0–4 categorical variable increasing in support for food stamps. "Aid poor" is a 0–4 categorical variable increasing in support for programs that aid poor households. "Housing" is a 0–4 categorical variable increasing in support for funding public housing programs. "Private charity" is an indicator of where (among a list of five policy approaches) the respondent puts "private charity" as a preferred method for addressing inequality (the variable increases with relative support for private charity). All other outcomes are as defined previously.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

support). Column 8 shows the treatment causes respondents to elevate a nongovernmental solution to inequality—private charity, which, as noted at the beginning of the Section, is generally preferred by more conservative respondents.

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

Emotional versus Factual Appeals.—There is a long psychology literature that suggests that for many issues, emotional appeals produce larger changes in attitudes than more factual presentations.[37] Indeed, the estate tax follow-up experiment described in Section IVA showed that the neutral treatment had a smaller effect than the emotional treatment. While our omnibus treatment provided extensive interactive and personalized information, it was mostly numeric in nature, which may have limited its ability to move policy preferences. Similarly, the focus on the "top 1 percent" might be less effective than focusing more intensely on the bottom of the distribution.

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

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

Table 10—Effect of "Emotional" Treatment on Outcome Variables

	Inequality very serious (1)	Inequality increased (2)	Poverty very serious (3)	Min. wage (4)	Aid poor (5)	Food stamps (6)	Housing (7)	Trust government (8)
Treated	0.0783***	0.0410	0.0885***	0.0469	0.117*	0.177*	0.0397	−0.00979
	[0.0292]	[0.0258]	[0.0313]	[0.0989]	[0.0665]	[0.101]	[0.0670]	[0.0211]
Control mean	0.337	0.775	0.296	2.546	2.559	1.832	2.539	0.124
Scaled poverty effect	0.221	0.225	0.257	0.0449	0.0714	0.0866	0.0291	0.0931
Observation	1,002	1,001	799	799	799	799	799	1,002

Notes: The "emotional" treatment aimed at creating empathy between the respondent and families living in poverty. Respondents were told about poverty rates and filled out a minimum budget for a family like theirs living in the same city. Respondents were then shown how their minimum budget compared to the poverty line. All outcomes are as defined previously. "Min. wage," "Aid poor," "Food stamps," "Housing" are all categorical 0–4 and increasing in support as in Table 9. The lower number of observations in columns 3–7 is due to the fact that these questions were not asked in one smaller wave (sample of 200).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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

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

Table 10 presents the key outcomes. The treatment has significant effects on concern about inequality (column 1), and, not surprisingly, large effects on whether poverty is a serious problem (equal to over 30 percent of the political gap for this outcome).[39] However, just like the omnibus treatment, this follow-up "emotional" treatment has limited effect on policy preferences. Of the four poverty-policy questions we asked, only two exhibit a marginally significant treatment effect (food stamps and aid to the poor, and even then just below 8 percent of the political gap). Similar to the omnibus treatment, this follow-up treatment reduces trust in government, though the effect is smaller and not significant.[40]

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

[39] As detailed in online Appendix Table 17, the treatment increased the likelihood of correctly choosing the actual poverty rate from multiple choices, though, because many control respondents overestimated the poverty rate, it did not on net increase their estimate of the poverty rate. The fact that the treatment had such a large effect on perceiving poverty as a "very serious problem" likely works through the intensive margin: perhaps through the creation of empathy, the treatment highlights how difficult it would be to manage with limited income.

[40] The treatment had very small but positive effects on taxes on the well-off, always well below 10 percent of the political gap and not significant. See online Appendix Tables 17 and 18 for these and all other outcomes not displayed in Table 10.

Discussion.—As readers can verify by taking the surveys themselves, the omnibus treatment and this "emotional" follow-up are very different in spirit. The omnibus treatment focused largely on the top 1 percent and was more factual in nature, whereas the follow-up treatment focused on the disadvantaged and sought to create empathy both with our "put yourselves in their shoes" exercise as well as photographs of low income families.

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

Connecting "Concerns" with Policy Measures.—Bartels (2005, p. 16) documents the seemingly odd result that even though the individuals in his 2002 sample were worried about inequality and aware that the tax cuts proposed by the Bush administration in 2001 favored the wealthy, they still supported them by a large margin. He concludes that "Americans support tax cuts not because they are indifferent to economic inequality, but because they largely fail to connect inequality and public policy."

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

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

Despite the somewhat smaller first-stage effects, the effects on our policy outcomes are consistently positive and significant. Support rises for the minimum wage (column 3), aid to the poor (column 4), food stamps (column 5), and public housing (column 6). The effect on "aid to the poor" is positive but not significant. It should be noted, however, that with the exception of the minimum wage, these effects are still relatively small (roughly about 10–15 percent of the political gap). It is also the case that the treatment does not consistently increase support for actually sending

Table 11—Effect of Policy Treatment on Outcome Variables

	Inequality very serious (1)	Poverty very serious (2)	Min. wage (3)	Aid poor (4)	Food stamps (5)	Housing (6)	Private charity (7)	Trust government (8)
Policy treatment	0.0405	−0.00637	0.323***	0.133**	0.313***	0.176***	−0.137*	−0.0325
	[0.0279]	[0.0272]	[0.0949]	[0.0638]	[0.0970]	[0.0636]	[0.0709]	[0.0207]
Control mean	0.343	0.326	2.546	2.559	1.832	2.539	2.025	0.149
Scaled policy effect	0.108	0.0196	0.310	0.0811	0.153	0.129	0.0740	0.654
Observations	1,111	1,111	806	806	806	806	1,068	1,111

Notes: The policy treatment aimed at creating empathy between the respondent and families living on a minimum wage. Respondents filled out a minimum budget for a family like theirs living in the same city. Respondents were then shown how their minimum budget compared to the minimum wage and how food stamps add $150 per person/month to the budget of such a family. All outcomes are as defined previously. "Min. wage," "Aid poor," "Food stamps," "Housing" are all categorical 0–4 and increasing in support as in Table 9. The lower number of observations in columns 3–7 is due to the fact that these questions were not asked in one smaller wave (sample of 300).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

money to Washington to pay for these policies: the effects on income and estate taxes for the well-off are mixed (see online Appendix Tables 19 and 20).

However, it does appear that the interaction of the empathic portrayal of low income families along with information emphasizing the efficacy of a transfer program has a meaningful effect on the policies respondents support. Column 7 suggests that, in contrast to the trust treatment, this treatment reduces the relative attractiveness of the nongovernmental solution to inequality: private charity. Finally, column 8 shows that there is no significant effect of the treatment on trust in government, though the point estimate is negative.

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

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

V. Conclusion

The standard median-voter model predicts that support for redistribution should increase with income inequality, yet there has been little evidence of greater demand for redistribution over the past 30 years in the United States—despite historic increases in income concentration. A possible explanation is that people are unaware of the increase in inequality, such that greater information would substantially move redistributive preferences. We gather over 10,000 observations using

Amazon's Mechanical Turk to conduct a series of survey experiments to extensively explore this hypothesis.

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

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

Only when we explicitly show individuals the concrete effects of government poverty policies do we observe consistent, statistically significant increases in support for such policies.[41] Even so, such effects are small, and are largest for indirect transfer programs such as the minimum wage that do not involve the government collecting and redistributing tax dollars.

Future work might further probe the connection between government trust and policy preferences. Underlying mistrust might help to explain the reluctance to support policies that would seem to be in the majority's self-interest. Relatedly, distrust could explain why minimum wage increases typically enjoy 70–80 percent support in surveys. Many economists assume that respondents simply misunderstand the incidence of the minimum wage. Instead, it might be the case that they view the minimum wage as imperfect, but better than other redistributive policies that involve sending money to Washington, DC.[42] In future work, it would be valuable to test whether treatments that *increase* trust in the government also increase support for redistributive policies.

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

It remains an open question if misinformation fully explains the difference. For example, Americans might view the moral claims to inheritance versus income differently. If the goal of the estate tax is to prevent the self-perpetuation of extreme wealth, then respondents might still support it even if, say, the government merely

[41] Future work could also explore how emphasizing the negative aspects of redistribution, such as reduced labor supply, could affect preferences for redistribution.

[42] Notably, voters in several conservative states passed minimum wage hikes in the 2014 midterm elections, consistent with their not viewing the minimum wage as a "government program."

burns the money it collects. Therefore, low levels of trust in government may not inhibit support for the estate tax as much as for other policies where efficiency is a more salient goal. Independent of the origin of the treatment effect, the large elasticity of support for the estate tax in response to basic information that we and past work have documented is highly policy-relevant, given the recent rise in inheritance flows in many developed countries.[43]

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

Such flexibility will allow researchers to gain a more nuanced understanding of redistributive preferences. While projections are by nature uncertain, the US government is expected to face a long-run fiscal imbalance, largely due to the aging of the population and rising health care costs.[44] European countries face similar challenges. The distributional effects of any future fiscal rebalancing—raising taxes, cutting spending, or both—will depend in large part on voters' redistributive preferences, how strongly they hold them, and whether and how 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' redistributive preferences are formed and shaped.

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

[44] See, e.g., the Congressional Budget Office's 2014 long-run budget projections (www.cbo.gov/publication/45471) (accessed December 1, 2014).

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