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Christopher Roth
Sonja Settele
Johannes Wohlfart

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CEBI
Department of Economics
University of Copenhagen
www.cebi.ku.dk

Beliefs About Public Debt and the Demand for Government Spending*

Christopher Roth† Sonja Settele‡ Johannes Wohlfart§

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Abstract

We examine how beliefs about the debt-to-GDP ratio affect people's attitudes towards government spending and taxation. Using representative samples of the US population, we run a series of experiments in which we provide half of our respondents with information about the debt-to-GDP ratio in the US. Based on a total of more than 4,000 respondents, we find that most people underestimate the debt-to-GDP ratio and reduce their support for government spending once they learn about the actual amount of debt, but do not substantially alter their attitudes towards taxation. The treatment effects seem to operate through changes in expectations about fiscal sustainability and persist in a four-week follow-up.

JEL Classification: P16, E60, Z13.

Keywords: Government Debt, Political Attitudes, Beliefs, Expectations, Information.

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†Christopher Roth, Department of Economics, University of Warwick, CESifo, email: christopher.roth@warwick.ac.uk.

‡Sonja Settele, CEBI and Department of Economics, University of Copenhagen, e-mail: sonja.settele@econ.ku.dk.

§Johannes Wohlfart, CEBI and Department of Economics, University of Copenhagen, CESifo, e-mail: johannes.wohlfart@econ.ku.dk.

1 Introduction

Government debt in many of the largest economies in the world has increased over the last few decades. For example, the debt-to-GDP ratio in the United States reached a level of 104.81 percent in 2016.[1] High levels of government debt can have important implications for the tax burden of future generations, the sustainability of public finances, and the possibility of a fiscal crisis. While the ultimate effects of government debt on the economy are still being debated among economists, much less attention has been devoted to people's beliefs and preferences regarding government debt. Are individuals' estimates of the debt-to-GDP ratio in line with underlying facts? Do voters have a preference for lowering levels of debt? And how do beliefs about the level of government debt affect attitudes towards government spending and taxation? Answering these questions has important implications for understanding voting behavior, patterns of debt accumulation and the optimal design of government policies.

In this paper, we conduct several online experiments in the United States in which we measure people's beliefs and preferences regarding government debt. We first elicit people's beliefs about the debt-to-GDP ratio. Then, we provide a random subset of our respondents with information about the debt-to-GDP ratio and study how this affects their attitudes towards government spending and taxation measured using both self-reports and behavioral measures. Overall, we recruit 3,350 respondents from online panels that are representative of the US population in terms of age, income, gender and region. Moreover, we recruit 800 individuals on Amazon Mechanical Turk (MTurk), whom we re-survey four weeks after the main experiment.

We start by documenting a series of stylized facts about people's beliefs and preferences regarding government debt: Most individuals underestimate the degree of indebtedness of the US government. The median respondent's estimate of the debt-to-GDP ratio is 60 percent, i.e. far below the actual debt-to-GDP ratio in 2016 (104.81 percent). Moreover, the median respondent thinks that the government should aim to achieve an even lower debt-to-GDP ratio of 25 percent.

Individuals who receive information about the true debt-to-GDP ratio become more likely to consider the prevailing level of government debt as too high and become approximately 0.21 of a standard deviation more supportive of cutting the overall amount of debt. Moreover, people who receive the information also become significantly less supportive of government spending

[1] It has remained at a stable level since then, amounting to 103.36 in 2017, 104.19 in 2018 and 103.80 in the first half of 2019 (Federal Reserve Bank of St. Louis, 2018).

in all spending categories. Our estimated effect sizes for views on government spending are considerable and correspond to approximately 0.10 of a standard deviation or to one third of the Democrat-Republican gap on average. In contrast, we find no strong evidence of a treatment effect on people's views on taxation. People's beliefs about debt also affect their political preferences as measured with a behavioral measure. Specifically, respondents provided with the information donate 0.15 of a standard deviation more to a think tank that advocates downsizing the government. This is a large effect size, corresponding to 54 percent of the gap in donations between Democrats and Republicans. However, we find no evidence that treated respondents change their willingness to sign a petition in favor of a balanced budget rule.

Do treatment effects persist over time? Using data from the four-week follow-up survey we show that the information about government debt persistently shifts people's views on cutting government debt and total government spending. The patterns of persistence for individual spending categories are more noisily measured but similar. The follow-up also shows that respondents in the treatment group have significantly lower biases in beliefs about the debt-to-GDP ratio. This suggests that a substantial part of the effects operate through genuine learning about the debt-to-GDP ratio rather than through short-lived emotional responses to the treatment.

We also shed light on the mechanisms through which the perceived level of government debt reduces people's demand for government spending. We find that beliefs about public debt causally affect people's expectations regarding the sustainability of public finances, leading to a 0.1 standard deviation difference between the treatment and the control group. There is a similar effect on people's expectations of government spending in ten years, but it is smaller in size and more noisily measured. By contrast, we find no strong evidence of changes in expected future taxation. We interpret this as suggestive evidence that people demand immediate spending reductions as a result of a desire to smooth the consumption of public goods over time. We find no evidence that beliefs about government debt causally affect people's trust in the government or their beliefs about rent-seeking and inefficiencies in the public sector, which could similarly lead to a reduction in desired spending levels.

We contribute to a literature on the role of people's beliefs about economically relevant facts in driving their demand for government spending and taxation.[2] Alesina et al. (2018b) find that

[2]A broader literature on the determinants of people's attitudes towards the size of the government and redistributive policies studies various other drivers such as people's equality-efficiency preferences or personal experiences (Alesina and La Ferrara, 2005; Alesina and Giuliano, 2010; Cappelen et al., 2018; Fisman et al., 2017,

(left-wing) survey respondents increase their support for equality of opportunity policies when exposed to information about low intergenerational mobility. Kuziemko et al. (2015) show that people's demand for redistribution is fairly inelastic to information about inequality. Karadja et al. (2016) and Cruces et al. (2013) provide evidence that people change their demand for redistribution in response to information about their relative position in the income distribution. Whereas all these experiments provide information that should change people's beliefs about the societal or private benefits of government spending, ours is one of the first to provide information that should affect people's concerns about the *financing* of such spending.

In concurrent work, and most closely related to our paper, Lergetporer et al. (2018) show that informing people about current levels of education spending and teacher salaries, or various other individual spending categories, sharply reduces support for spending increases in these categories. By contrast, our study provides information that abstracts from spending flows on individual categories and focuses on the stock of government debt. The stock of government debt should be a more relevant metric for judging the sustainability of public finances than spending on individual categories. Another novel aspect of our paper is that we shed light on people's concerns about the inter-temporal allocation of funds in our analysis of mechanisms.

On a methodological level, we follow Grigorieff et al. (2019) in their use of signatures of real online petitions and donations. We introduce two novel behavioral measures to the literature on attitudes towards the size of the government, which are tailored to capture attitudes to certain policies. First, we measure people's willingness to donate money to an NGO advocating the downsizing of the government, which captures a general desire for a smaller state. Second, we elicit people's willingness to sign a real online petition in favor of a balanced budget rule, which measures preferences about one specific policy that is tightly linked with concerns about government debt and fiscal sustainability.

Our paper adds to the literature on the political economy of government debt, which is concerned with the question why governments tend to accumulate (excessive) debt levels (Alesina and Passalacqua, 2016; Alesina and Tabellini, 1990; Battaglini and Coate, 2008; Cukierman and Meltzer, 1989; Müller et al., 2016; Persson and Svensson, 1989; Song et al., 2012).[3] Part of this literature provides indirect evidence on voters' concerns about government debt. On the one hand, reducing government debt could be punished by voters because of its contractionary effect

2015; Giuliano and Spilimbergo, 2014; Roth and Wohlfart, 2018; Weinzierl, 2017).

[3]Battaglini et al. (2018) provide evidence from a laboratory experiment which studies political distortions in the accumulation of public debt.

on the economy (Alesina and Tabellini, 1990). On the other hand, if voters are aware of the intertemporal government budget constraint and care about future spending and taxation, this could lead to a punishment of excessive debt accumulation. Correlational studies have found at most a small positive correlation between episodes of debt reduction and the likelihood of the government to be re-elected (Alesina et al., 2013, 1998; Brender and Drazen, 2008), and a zero correlation between whether spending is debt-financed or not on voting of the US population in presidential, senatorial and gubernatorial elections (Peltzman, 1992). Our paper speaks to this literature by isolating the causal effect of people's beliefs about the level of government debt on attitudes towards government spending and taxation. Our results imply that biased beliefs about the level of government debt can make voters prefer higher levels of government spending than if they were aware of the true level of debt.

Finally, our study relates to a literature using survey methods to study people's mental model of the economy. Existing evidence suggests that people are inattentive to macroeconomic conditions such as inflation, house prices and income, and have a rough but imperfect understanding of macroeconomic relationships (Andre et al., 2019; Armona et al., 2019; Coibion et al., 2018; Fuster et al., 2019; Kumar et al., 2015; Roth and Wohlfart, 2019). In the context of public debt, the assumption that consumers understand the intertemporal government budget constraint is at the core of many macroeconomic models and is one of the key assumptions underlying the Ricardian Equivalence Theorem (Barro, 1974). A small correlational literature testing the assumptions underlying the Ricardian Equivalence Theorem provides suggestive evidence of a limited awareness of the level of public debt (Allers et al., 1998)[4] and finds little support that individuals' consumption and savings decisions are influenced by the perceived level of debt (Allers et al., 1998; Hayo and Neumeier, 2017; Heinemann and Hennighausen, 2012). To the best of our knowledge, our paper is the first to provide systematic evidence on beliefs about the debt-to-GDP ratio using a representative sample of the US population, and on how an exogenous shift in these beliefs causally translates into people's attitudes towards government policies. Our experimental results are consistent with the idea that voters take into account the intertemporal budget constraint of the government when forming their demand for government policies. We provide suggestive evidence on the role of expectations in mediating these effects: Individuals who learn about the high level of public debt change their expectations

[4]Allers et al. (1998) rely on a selected sample of Dutch newspaper readers, who are more educated than the average population, and ask them about their perceived level of government debt in billions of the national currency.

about fiscal sustainability, even though only respondents with higher levels of education change their expectations about the level of government spending in ten years.

The rest of this paper is structured as follows. In section 2 we provide some background on the intertemporal budget constraint of the government and develop the hypotheses we test in the experiment. Section 3 presents the design as well as the setting and samples used in the information experiments. In section 4 we provide evidence on our respondents' prior beliefs about debt and changes in beliefs in response to the information. We present our main results in section 5 and provide evidence on mechanisms and robustness in section 6. Section 7 concludes.

2 Conceptual framework

In this section we present a simple conceptual framework which motivates the experiment and the main hypotheses on how voters should adjust their policy demand when updating their beliefs about the amount of government debt. Voters form their expectations about future government spending and taxation and their policy demand subject to the perceived intertemporal government budget constraint:

$$\sum_{t=1}^{\infty} \frac{p_t}{(1+r)^t} = \alpha \left[B_0 + \sum_{t=1}^{\infty} \frac{T_t}{(1+r)^t} \right]$$

where p_t is public good provision in period t, T_t is total tax revenue collected in period t and B_0 is net wealth of the government in period 0.[5] α lies in the interval $[0, 1]$ and captures the efficiency of the bureaucratic process. In our experiment we create exogenous variation in our respondents' perceived level of government debt, $-B_0$. If respondents understand the intertemporal budget constraint of the government, then an increase in the perceived level of government debt, $\Delta B_0 < 0$, should lead to a decrease in the perceived net present value of the stream of public good provision, $\sum_{t=1}^{\infty} \frac{p_t}{(1+r)^t}$, or to an increase in the perceived net present value of tax revenue, $\sum_{t=1}^{\infty} \frac{T_t}{(1+r)^t}$.

If respondents expect that adjustments in spending or taxation will be necessary during their lifetimes, or if they care about the utility of future generations, then an inclination to smooth the consumption of public goods over time could lead them to immediately demand lower

[5] For simplicity we abstract from the distributional aspects of taxation and from how the tax burden is spread across the electorate.

levels of government spending in response to learning that debt is higher then they previously thought. Alternatively, respondents could favor immediate tax increases in order to smooth the tax burden for themselves and their children. If respondents do not expect that adjustments will be necessary during their lifetimes and if they do not care about future generations, they may not respond to the treatment and may want to postpone the necessary adjustments in spending or taxes.

In addition, our respondents could adjust their beliefs about the wastage that occurs in the bureaucratic process. Specifically, upon learning that government debt is higher than they previously thought, respondents could hold more pessimistic beliefs about the rate at which the government transforms tax revenue into public goods, α. Consequently, respondents may want to shift consumption away from public goods towards private goods, and therefore reduce the size of the government.

In online Appendix A we demonstrate how an increase in the perceived level of government debt leads to a lower demand for government spending and to an increase in the net present value of total tax revenue collected in a simple two-period model.

3 Experimental design

We conducted a total of four very similar experiments, summarized in Table A.1 in the online appendix. One of the four experiments included a follow-up survey. In this section we present our experimental design and explain the structure of the main experiment and the follow-up survey. The full experimental instructions for all four experiments are available at `https://www.dropbox.com/s/9o0a5gk14x8c19m/01_instructions_v3.pdf?dl=0`.[6]

3.1 Main experiment

3.1.1 Belief elicitation

All four experiments are structured as follows: We start by asking all respondents questions about some of their demographics, namely gender, age, region and income, and about their

[6]We pre-registered our experimental design, the sample sizes, as well as the specifications estimated in the paper on the AEA RCT registry before conducting Experiments 1 and 2. Detailed pre-analysis plans are available for these two experiments at `https://www.socialscienceregistry.org/trials/1960`. Experiments 3 and 4 are highly similar to Experiments 1 and 2. In the pre-registered trial entry we also pre-specified an experiment examining how people's support for government spending programs varies by the proposed mode of financing, which will be used as the basis for a separate paper and which is not included in this paper for brevity's sake. Results are available upon request.

political affiliation.

Next, we elicit all participants' beliefs about the current US debt-to-GDP ratio. We chose to focus on beliefs about the debt-to-GDP ratio as it offers several advantages relative to alternative measures of public debt: First, according to economic theory it is a meaningful measure of government debt as it sets its level in relation to the tax base. Second, alternative measures, such as the debt-per-capita ratio, may entail the (potentially misleading) connotation that people have to pay back their personal share of government debt within their lifetime. We therefore view information on the debt-to-GDP ratio as a conservative way of measuring the effect of perceived debt levels on attitudes towards government spending and taxation.

Although the debt-to-GDP ratio is frequently discussed in the news, some respondents may not be familiar with the concept. We therefore explain that government debt refers to the "total amount owed by the Federal government" and that GDP refers to the "market value of all final goods and services that are produced by an economy within one year". Moreover, we provide our respondents with a numerical anchor in order to inform them about the order of magnitude they should think of and to thereby enable them to make a meaningful estimate of a statistic they are not regularly confronted with (Ansolabehere et al., 2013). As we describe in more detail in section 4.1, evidence from a pilot experiment indicates that the provision of an anchor reduces respondents' uncertainty about their responses and suggests that the provision of an objective benchmark indeed allows respondents to express their qualitative beliefs in a quantitatively meaningful way, in line with the evidence presented by Ansolabehere et al. (2013).

Our four experiments differ in the exact anchor we provide. In Experiments 1 and 2 we inform our respondents about the debt-to-GDP ratio in the US in 1970 (34.78 percent) before asking them to estimate the current debt-to-GDP ratio in the US. Specifically, they receive the following instructions:

> In 1970 the debt-to-GDP ratio was 34.78 percent. This means that the Federal Government owed around a third of what the country produced within one year. What do you think was the debt-to-GDP ratio in 2016?

One concern could be that this anchor in combination with our information treatment about the actual debt-to-GDP ratio not only shifts people's beliefs about the level of debt but also about the increase in debt over time. We therefore conduct two additional experiments which provide alternative anchors. In Experiment 3 we inform respondents about the average debt-

to-GDP ratio in the US over the past 100 years (55.2 percent), and therefore do not shift beliefs about the specific increase in debt since any particular point in time. Finally, in Experiment 4, we provide the median debt-to-GDP ratio currently prevailing in OECD countries (52.4 percent) as a point of orientation, which makes no historical reference.

It is no coincidence that all our anchors are smaller in size than the most recent US debt-to-GDP ratio, which corresponds to 104.81 percent (104.19 percent) in Experiments 1 and 2 (3 and 4). Given that the current level of US government debt is high both by historical as well as by international standards, any representative anchor that provides a realistic impression of the order of magnitude will be lower than the current level of debt.

We deliberately chose not to monetarily incentivize our respondents' prior beliefs, given that the current debt-to-GDP ratio can easily be looked up online.[7] Instead, the framing of our belief elicitation task strongly suggests that we ask for an objective statistic and might therefore work as an accuracy incentive in the sense of Prior et al. (2015). Moreover, Bullock et al. (2015) show that not incentivizing (prior) beliefs is unlikely to lead to partisan bias in beliefs about levels of public debt.

3.1.2 Information treatment

Thereafter, respondents in the treatment group receive information about the actual most recent debt-to-GDP ratio in the US, while respondents in the control group do not receive any information. Treated respondents in Experiments 1 and 2 receive the following message:

> We now would like to provide you with information about the debt-to-GDP ratio in the US. In 2016, the federal debt-to-GDP ratio was 104.81 percent. This means that the Federal Government owed a bit more than what the country produced within one year.

Our respondents are also shown a figure contrasting the debt-to-GDP ratio in 1970 with the debt-to-GDP ratio in 2016. Experiments 3 and 4 differ from Experiments 1 and 2 in i) the year of reference (2018 vs 2016), ii) the exact treatment value (104.19 vs. 104.81) and iii) the fact that the treatment value is graphically contrasted with the respondents own prior estimate rather than with the anchor. The latter difference arguably makes the anchor less salient than in Experiments 1 and 2. For an illustration of the treatment screens see Figure A.1 (Figures

[7] Grewenig et al. (2019) show that incentives in online experiment have similar effects on reported beliefs as a prompt to Google for a statistic.

A.2 and A.3) in the online appendix for Experiments 1 and 2 (3 and 4). To ensure a high level of trust of our respondents in the information we provide them with the official source of the data (the Federal Reserve Bank of St. Louis) in all four experiments.

3.1.3 Measuring political preferences: Survey measures

After the information treatment, we ask all of our respondents whether they think that there is too much government debt in the US and whether the government should reduce the amount of debt. We measure people's agreement to these statements on 5-point Likert scales reaching from "Strongly Agree" to "Strongly Disagree". Thereafter, we ask them a series of questions on their attitudes towards government spending. They first answer a question on whether they would like the overall level of government spending to be increased or decreased. Then we provide our respondents with explanations of several spending categories. For each category, we ask them whether they would like to increase or decrease spending.[8] The answer categories for these questions range from 1 "It should be increased a lot" to 5 "It should be decreased a lot". Subsequently, our respondents answer a series of questions on whether income taxes of different income groups should be increased or decreased, whether the government should introduce a wealth tax and whether the estate tax should be increased or decreased.

3.1.4 Measuring political preferences: Behavioral measures

To examine whether the information also affects political behavior, we employ revealed preference measures of political attitudes.[9] Specifically, we elicit two behavioral measures that capture people's attitudes towards government spending and the size of the government in our sample from Amazon Mechanical Turk.[10]

Our respondents can make a donation to the Cato Institute which we describe as an NGO that advocates downsizing the government. We provide a brief description of the self-declared mission of the Cato Institute, based on its official website:

[8] We focus on the following spending categories: defense, infrastructure, schooling, social security, social insurance, health, and environment.

[9] Ultimately, support for political parties or officials, for instance as proxied by voting decisions, would be the most relevant outcome in a representative democracy. In our context, however, there is no clear hypothesis on how perceived levels of government debt should affect party support: While the Republican party is generally associated with less public spending, Republican administrations have, in fact, systematically increased the debt-to-GDP ratio relative to when the president was a Democrat (Müller et al., 2016). With the Trump administration, this perception may arguably be quite salient in the population, not least due to the recent public discussion and corresponding media coverage (see `e.g.https://www.nytimes.com/2019/10/25/us/politics/us-federal-budget-deficit.html`).

[10] We could not include these behavioral measures in the experiment with the representative online panels due to constraints from our online panel providers.

> "The Cato Institute seeks to help policymakers and the public understand where federal spending goes and how to reform each government department. It describes the failings of agencies and identifies specific programs to cut. We believe that cutting the federal budget would enhance personal freedom, increase prosperity, and leave a positive fiscal legacy to the next generation."

Our respondents learn that one out of 20 participants will receive an additional $5 at the end of the experiment, and they have to decide how much to keep for themselves and how much to donate in case they are selected. We believe that this is a particularly suitable behavioral measure as donations to political organizations are an important real-life tool for people to support particular political causes. Donations to political NGOs and campaigns have been used previously to measure political preferences (Grigorieff et al., 2019; Perez-Truglia and Cruces, 2016).

Moreover, we give all of our respondents the opportunity to sign a real online petition on the White House Petition Website in favor of introducing a balanced-budget rule. The petition is described as follows in the survey:

> "We propose the introduction of a balanced budget amendment. A balanced-budget amendment is a constitutional rule requiring that the government cannot spend more than its income. It requires a balance between the projected receipts and expenditures of the government. A balanced budget rule is designed to prevent the government from accumulating debt."

Individuals in the treatment and in the control group who express their intention to sign the petition receive different links to identical petitions. This allows us to observe the actual numbers of signatures for the petition separately for respondents in the treatment group and for respondents in the control group.

3.1.5 Mechanisms: Post-treatment beliefs

To understand why people may change their views on government spending and taxation in response to information about the level of public debt, we collect a rich set of post-treatment beliefs. Specifically, we measure our respondents' expectations about taxation and government spending in ten years as well as for future generations. We also elicit their beliefs about the sustainability of public finances through their agreement with the statement "the current levels

of government spending and taxation are not sustainable". Thereafter, we measure our respondents' trust towards the government and their beliefs about the effectiveness of the government and about the corruption of politicians. For example, we ask them whether they agree that "the government makes good use of taxpayers' money". Finally, in Experiments 3 and 4, we also elicit our respondents' posterior beliefs about the debt-to-GDP ratio by again asking for their individual estimate of the US debt-to-GDP ratio in 2018.

3.2 Follow-up survey

One concern could be that responses to the information treatment are subject to short-lived (emotional) responses and not driven by an actual learning about relevant facts. To examine the persistence of effects over time, we conduct a follow-up survey in Experiment 2, which takes place four weeks after the main experiment. We do not administer any additional treatment but ask respondents the same set of questions on their views regarding government spending and taxation as in the main survey. At the very end of the follow-up survey we also ask respondents about their estimate of the current debt-to-GDP ratio to test whether beliefs about the debt-to-GDP ratio persistently respond to information.

3.3 Setting and sample size

3.3.1 Experiment 1: Representative online panel

Our main evidence comes from an experiment with a representative online sample that we collected in February 2017 in collaboration with the market research company Research Now, which is widely used in economic research (Almås et al., 2020; de Quidt et al., 2018; Enke, 2018). The resulting sample of 813 respondents is representative of the adult US population in terms of region, income, age, and gender. Table A.7 displays the corresponding summary statistics for the sample and the American Community Survey. 55 percent of our respondents are female, a slightly larger fraction than among the American population (51 percent). Moreover, our sample is very similar to the US population in terms of age profile and regions. While the mean household income in our sample ($62,488) is lower than that of the US population ($84,568), the median household income in our sample ($62,500) is very close to that of the US population ($59,039).

A remaining concern could be that our online sample is, by definition, selected from the online population. Using German data, Grewenig et al. (2018) show that the online and the

offline population hardly differ in terms of survey responses in the context of political views and opinions, once the survey method and observable respondent characteristics are controlled for. Lastly, the attrition rate in our experiment is very low and does not differ across treatment arms.

3.3.2 Experiment 2: Amazon Mechanical Turk

In addition to conducting experiments with representative online panels, we also recruited participants on Amazon Mechanical Turk (MTurk), an online labor market which is increasingly used for experimental research (Cavallo et al., 2017; Kuziemko et al., 2015).[11] We conducted our experiment on the MTurk platform for two reasons: First, it allows us to employ behavioral measures which are difficult to implement with representative online panels. Second, it enables us to conduct a four-week follow-up with a much higher recontact rate than commonly achieved in representative online panels.

Data collection on MTurk took place on the 27$^{\text{th}}$ of January 2017, resulting in a sample of 802 participants.[12] Table A.3 in the online Appendix summarizes the sample characteristics. 56 percent of our respondents are male, the median income in our sample is $62,500, which is only slightly higher than the median income in the US population. Respondents from the MTurk sample are more educated than respondents from the general population and there are more Democrats in our MTurk sample compared to the US population. The attrition rate was below 2 percent and not statistically different for respondents in the treatment and the control group. 74 percent of the respondents who completed the main experiment also completed our four-week follow-up survey. The sample composition is virtually unchanged compared to the main experiment (see Table A.4 in the online appendix) and the attrition rate from the main experiment to the follow-up is not statistically different between treatment and control group.

3.3.3 Experiments 3 and 4: Representative online panels

We conducted Experiments 3 and 4 in collaboration with a market research company, Lucid (Coppock and McClellan, 2019; Wood and Porter, 2019), similar to Experiment 1. Data collection took place in December 2019, resulting in two samples of 1,488 respondents (Experiment

[11] Coppock (2018) conducts 15 replication experiments and finds a very high degree of replicability of survey experiments in the field of political science with MTurk as compared to nationally representative samples. Rand (2012) uses IP address logging and repeated surveys to show that the vast majority of MTurk workers self-report characteristics such as their country of residence and other demographic variables truthfully.

[12] It was restricted to participants currently living in the United States who have completed at least 500 tasks with an overall rating of more than 95 percent.

3) and 1,049 respondents (Experiment 4), respectively.[13] Both samples are representative of the adult US population in terms of gender, region, income, and age profile, as illustrated by Tables A.9 and A.10 in the online appendix. As in Experiment 1, our respondents have a somewhat lower average household income than the population but the median household income is fairly similar.

3.3.4 Integrity of randomization

We provide evidence that our four main samples and the follow-up sample are balanced between treatment and control group in terms of observables. Specifically, in Tables A.11-A.15 in the online appendix we show p-values for separate t-tests for differences in individual observables between the treatment and control group. As expected given the large number of t-tests, a few differences turn out statistically significant, such as the higher fraction of Republicans in the control group of Experiment 1 (see Table A.11).[14] We control for such minor imbalances through the pre-specified set of control variables in all regressions. In addition to separate t-tests, we run one joint F-test per experiment in which we regress the treatment indicator on all covariates. For our four main samples as well as the follow-up sample, these tests confirm that our covariates are globally balanced.

4 Beliefs about the debt-to-GDP ratio

4.1 Prior beliefs

The US debt-to-GDP substantially increased over the last decades from about 35 percent in the 1960s and 1970s to more than 100 percent today (see Figure A.4 in the online appendix). Figure 1 illustrates people's beliefs about the debt-to-GDP ratio in the US, pooling Experiments 1-4. On average, people widely under-estimate the true value of around 104 percent. The median respondent in our full sample believes that the debt-to-GDP ratio is 60 percent and more than 90 percent of our respondents underestimate the debt-to-GDP ratio.[15] These findings are consistent

[13] Note that the sample size in Experiment 3 ($N \approx 1,500$) is larger than that of the other experiments due to an accidental over-recruiting of respondents through the survey company.

[14] Note that the larger fraction of Republicans in the control group works against finding a significant effect of the information on views on government spending, given that Republicans are generally more likely to demand spending cuts. In section B.4 of the online Appendix we show robustness of our main findings to dropping the pre-specified control variables.

[15] As pre-specified, we winsorize people's beliefs about the debt-to-GDP ratio at 200 in order to deal with outliers. This affects very few observations above the 98th percentile of the prior as well as the posterior belief distribution in all our samples. Our results are fully robust to applying symmetric winsorizing at the top and bottom 1 or 2% of the belief distribution, as we show in Table A.19 in the online appendix.

with previous evidence that voters have incorrect perceptions of the level of government debt (Allers et al., 1998; Mayer, 1995). Figure A.8 in the online appendix displays the distribution of prior beliefs separately for each of our four experiments. Both the mean and the median prior belief are fairly similar across experiments, additionally corroborating our finding that respondents generally underestimate the debt-to-GDP ratio.[16]

How externally valid is our finding that respondents underestimate the current debt-to-GDP ratio in the US? To get an understanding of the extent to which our historical anchor affects reported beliefs about the current debt-to-GDP ratio, we ran a pilot experiment on MTurk in early 2017 in which we elicited the beliefs of around 200 respondents about the debt-to-GDP ratio. In this pilot experiment, half of the respondents are given information about the debt-to-GDP ratio in 1970, while the remaining respondents do not receive this information. As expected, the dispersion in beliefs is higher in the "no anchor"-setting. At the same time, measures of central tendency of prior beliefs are not too different (see Figure A.5 in the online appendix): While the median respondent thinks that the debt-to-GDP ratio is 63 percent when not provided with an anchor, the median respondent who is given the historical anchor reports an estimate of 56.23 percent. This difference amounts to only a tenth of a standard deviation of the perceived debt-to-GDP ratio across the two samples and is statistically insignificant.[17] Comparing mean beliefs, which are more sensitive to outliers, the anchor causes a somewhat larger difference of 13 percentage points (p-value 0.04) or around one fourth of a standard deviation.

Lastly, we find that respondents in the "no anchor"-condition are significantly more likely to round to multiples of 10 or 5 and to report beliefs above 200.[18] Following Ruud et al. (2014) and Binder (2017), we interpret these patterns as evidence that our respondents, in the absence of an anchor, are more uncertain about the range in which the debt-to-GDP ratio generally moves. In sum, our pilot study supports the view that the historical anchor reduces noise and allows our respondents to express their beliefs on a "common scale" (Ansolabehere et al., 2013),

[16]The median belief in experiments 1, 2, 3 and 4 is 60, 55, 60 and 60 and the mean is 65, 64, 63 and 62. Finally, the share of under-estimators of the debt-to-GDP ratio in the four experiments is 93 percent, 92 percent, 95 percent and 94 percent, respectively.

[17]A nonparametric two-sample test on the equality of medians gives a p-value of 0.37.

[18]Whereas 43 percent of the respondents round to multiples of 10 in the absence of an anchor, only 29 percent do so under the 1970 anchor. Similarly, 75 percent of the respondents round to multiples of 5 without an anchor and only 55 percent do so with the anchor. The described differences are significant at the five percent and the one percent significance level, respectively. Finally, in the absence of an anchor, 9 percent of the responses exceed a debt-to-GDP ratio of 200 percent and this share is 7 percentage points lower in the presence of the anchor (p-value 0.04).

while at the same time having only a minor effect on the median belief.

4.2 Correlates of beliefs about the debt-to-GDP ratio

To shed light on the determinants of respondents' beliefs about the level of debt, we regress people's perceived debt-to-GDP ratio on a set of demographics. Men, older individuals, those with higher education and those with a higher household income report higher estimates of the debt-to-GDP ratio, even though these effects are significant only in one or two out of the four subsamples, respectively (see Table A.16 in the online Appendix).[19]

As more than 90 percent of our respondents underestimate the debt-to-GDP ratio, one might expect higher estimates to correspond to less biased beliefs. Table A.17 in the online appendix reports predictors of people's misperceptions of public debt, defined as the absolute deviation of the prior belief (winzorised at 200) from the true value as of 2016 (104.81 percent). The results reveal that in the cases of older individuals, those with higher education and those with higher household income, higher beliefs at the same time reflect significantly more precise beliefs about the debt-to-GDP ratio. In contrast, the beliefs of male and female respondents are similarly precise, despite the fact that males hold higher beliefs about the debt-to-GDP ratio.

4.3 What is our respondents' desired debt-to-GDP ratio?

In the pilot experiment on MTurk we also ask people about their views on what level of the debt-to-GDP ratio the government should aim to achieve. People answer this question after estimating the current debt-to-GDP ratio in the United States. Figure A.6 displays the distribution of beliefs about the debt-to-GDP ratio as well as the desired debt-to-GDP ratio in the group of respondents who have received the historical anchor. While the median respondent's estimate of the debt-to-GDP ratio is 56.23 percent, she thinks that the government should aim to achieve a debt-to-GDP ratio of 25 percent.[20] Figure A.7 directly illustrates the distribution of desired changes in the debt-to-GDP ratio, which is defined as the difference between people's desired debt-to-GDP ratio and their belief about the actual debt-to-GDP ratio. The figure highlights that 94% of individuals want to reduce the amount of debt in the US.

[19] The difference in average beliefs between Democrats and Republicans is small and insignificant in the pooled sample and points into different directions in different subsamples.

[20] People's stated preference over their desired debt-to-GDP ratio could be higher if people fully understood what moving to a lower debt-to-GDP ratio would entail in terms of spending cuts or tax increases.

4.4 Do respondents update their beliefs?

Do our respondents persistently update their beliefs about the debt-to-GDP ratio in response to the provision of official statistics? Experiment 2 sheds light on this question, based on data from the four week follow-up survey on MTurk. The treatment durably shifts people's beliefs about the debt-to-GDP ratio. The distribution of posterior beliefs is described in Figure 2. Specifically, people in the treatment group report significantly higher estimates of the debt-to-GDP ratio ($p<0.01$). The median belief in the treatment group is that the debt-to-GDP ratio is 75 percent, while it is 62 percent in the control group. Figure 3 shows treatment effects on posterior beliefs depending on our respondents' prior beliefs. The figure highlights that treated subjects who under-estimated the debt-to-GDP ratio strongly shift their belief upward, while treated respondents who over-estimated the debt-to-GDP ratio shift their belief downward (although this effect is noisily measured). This evidence strongly suggests that the information treatment leads to genuine updating of beliefs (Cavallo et al., 2017).

We use the data from the MTurk follow-up survey to quantify the extent to which the respondents persistently update their beliefs towards the signal they receive during the information treatment using the following specification:

$$\text{Updating}_i = \beta_0 + \beta_1 \text{Treatment}_i \text{Perc.-gap}_i + \beta_2 \text{Treatment}_i + \beta_3 \text{Perc.-gap}_i + \varepsilon_i$$

where Updating_i is defined as the difference between the respondent's posterior and prior about the debt-to-GDP ratio. The perception gap, Perc.-gap_i, is the difference between the objective signal of 104.81% and the respondent's prior belief. Our coefficient of interest, β_1, measures the extent to which respondents in the treatment group update their belief toward the provided signal, on top of any updating that also happens for respondents in the control group, who were not provided with information. β_2 is the average treatment effect on respondents' beliefs, regardless of individual priors. It captures, e.g., salience effects. β_3 measures changes in beliefs depending on the perception gap in the control group.[21] Our estimated coefficient of interest, β_1, amounts to 0.21, i.e. four weeks after the information respondents in the treatment

[21] Note that the estimated coefficient β_3 on the non-interacted perception gap is mechanically different from zero for two reasons: First, given that $\text{Updating}_i = \text{posterior}_i - \text{prior}_i$ and $\text{Perc.-gap}_i = 104.81 - \text{prior}_i$, updating and the perception gap are mechanically correlated through the prior belief. Second, as pointed out by Fuster et al. (2019), "endogenous spurious learning" is likely, in the sense that a given respondent might e.g. make a typo in the prior belief elicitation which leads to a significant deviation of the reported prior from the true belief. If the individual does not make the typo again in the posterior belief elicitation it will look like she is learning from a signal she has never received. For comparison of our estimated coefficient β_3, see for instance Table A.18 in Roth and Wohlfart (2019). Following our pre-analysis-plan, all beliefs are winsorized at 200.

group have persistently updated their beliefs towards the signal at a rate of 0.21, while putting a weight of 0.79 on their prior (see Table A.18 Panel B column 1 in the online appendix). Moreover, the effect of the treatment on people's beliefs fully operates through its interaction with the extent that respondents' priors differed from the information.[22]

5 The causal effect of information about government debt

In this section we shed light on the causal effect of beliefs about the debt-to-GDP ratio on people's views on government debt, public spending and taxation.

5.1 Empirical specification

We regress our outcome variables y_i on a treatment indicator, Treatment_i, which takes the value one for people who receive the information treatment, and zero otherwise. We estimate the following equation using OLS:

$$y_i = \pi_0 + \pi_1 \text{Treatment}_i + \Pi^T \mathbf{X_i} + \varepsilon_i$$

where X_i is a vector of control variables, including all of the variables we use in the baseline balance check[23] and ε_i is an individual-specific error term. We include control variables as this increases our power to precisely estimate treatment effects and to account for small imbalances in individual covariates. We report robust standard errors for all estimations.

We report results for all pre-specified outcome variables which are normalized using the mean and standard deviation from the control group. To deal with the issue of multiple hypothesis testing we follow the approach described in Anderson (2008): We first create summary indices

[22] In Experiments 3 and 4 we also elicit posterior beliefs about the debt-to-GDP ratio at the end of the main survey. Using these data we find an immediate learning rate of 0.62. Given that the posterior simply measures very short-term recall of a fact, the extent to which the learning rate is smaller than one indicates inattention to the survey. Assuming that the immediate learning rate was similar in Experiment 2 on MTurk, this implies a recall rate of about one third after four weeks. This magnitude is in line with the persistence of updating documented by previous literature in the context of macroeconomic expectations (Armantier et al., 2016; Coibion et al., 2018; Fuster et al., 2019; Roth and Wohlfart, 2019), which we summarize in Table A.20 in the online appendix. Columns 2-4 of Table A.18 show that our estimated learning rate is robust to the specification used e.g. in Coibion et al. (2018).

[23] Specifically, we control for the belief about the debt-to-GDP ratio pre-treatment, gender, age, log income, the number of children, dummies for employment status, whether the respondent has a college degree and whether the respondent is a Republican. In all pooled specifications we also include dummies for the Experiments 1 to 4. For ease of interpretation and to take care of outliers we deviate in some minor ways from the pre-specified set of controls. Namely, we include a dummy for "other employment status" and we top-code the number of children at five. We also include a measure of trust in statistics and a dummy variable for Independents. The two latter control variables help us to increase efficiency, while not affecting the coefficient estimates. Results without controls are presented in the online appendix.

for our three main families of outcomes i) views on government debt, ii) attitudes towards government spending and iii) attitudes towards taxation. When constructing an index, we weight its inputs by the inverse of the covariance matrix of the standardized outcomes such that outcomes that are highly correlated with each other receive less weight, while outcomes that are uncorrelated, and thus contain new information, receive more weight. Second, when looking at more detailed outcomes within each family, we control for the false discovery rate (FDR) or the proportion of rejections in the family of outcomes that are "false discoveries", i.e. Type I errors (Benjamini et al., 2006). Compared to more conservative methods, such as a classical Bonferroni correction, this method allows for more power in exchange for a small number of Type I errors. We present FDR-adjusted p-values for all detailed outcomes within the three families. The interpretation of these adjusted p-values is similar to standard p-values: They represent the proportion of Type I errors as a share of all rejections of null hypothesis in the family that has to be allowed such that the respective null hypothesis can still be rejected.

5.2 Does the information affect views on government debt?

Do people's subjective views on government debt respond to factual information about the debt-to-GDP ratio? Table 1 shows that respondents who receive information about the true debt-to-GDP ratio become 0.24 of a standard deviation more likely to think that there is too much debt and 0.20 of a standard deviation more inclined to think that the government should reduce the amount of government debt. The latter effect corresponds to 91 percent of the greater support for debt reduction among Republicans than among Democrats.

5.3 Does the information affect policy views?

After establishing that people who receive the information want to reduce government debt, we now turn to the question whether people would like to achieve the reduction in debt through spending cuts or through tax increases. Table 2 highlights that participants who were provided with the information are 0.15 of a standard deviation less supportive of overall government spending (see Panel A, column 1). Moreover, they become significantly less supportive of spending in all subcategories, namely defense, infrastructure, schooling, social security, social insurance, health and the environment. We find a highly significant overall effect of about 0.1 of a standard deviation, based on our summary index measure. Our treatment shifts policy preferences by one third of the preference gap for these variables between Republicans and

Democrats. Our evidence highlights that beliefs about the debt-to-GDP ratio strongly affect people's views on government spending.

Moreover, we examine people's views on taxation. People who learn about the true debt-to-GDP ratio become significantly more likely to favor an increase in the overall amount of taxes collected by the government (Table 3, column 1). The treatment effects on attitudes towards specific types of taxation are weaker, less stable across the four experiments and less robust to adjusting p-values for multiple hypothesis testing. Namely, while treated respondents in Experiments 1 and 4 favor an increase in the estate tax, treated respondents in Experiments 2 and 3 become more supportive of increasing income taxes for the bottom 50 percent. Moreover, the treatment effect on the summary index is only half as large as on the summary index for government spending, suggesting a cautious interpretation. All in all, our results imply that learning that the debt-to-GDP ratio is higher than previously thought makes people less supportive of government spending, but does not strongly change their support for changes in taxation.

The differential responses for government spending and taxation could be due to several factors. First, the perceived marginal disutility of a tax increase could be higher than the perceived marginal disutility of a government spending cut. For instance, this could be due to people's belief that a large fraction of government spending is wasteful. Second, tax increases affect some people's income with certainty, while it is less clear whether individuals will be directly affected by cuts in government spending.[24]

5.4 Does the information affect behavior?

To examine whether the information also changes actual political behavior we analyze our respondents' inclination to donate to a political NGO advocating government spending cuts, and their willingness to sign a real online petition (Grigorieff et al., 2019). In response to the treatment, donations to the NGO increase by 15 Cents or 0.15 of a standard deviation on average (Table 4). This treatment effect corresponds to 54 percent of the gap in donations between Republicans and Democrats. Whereas individuals in the control group donate on average around 58 cents of their $5 endowment, respondents in the treatment group on average donate around 72 cents, i.e. the treatment effect on donations corresponds to 24 percent of the

[24]In unreported regressions we examined whether treated respondents favor tax increases in other income groups than their own. However, we found no strong evidence of such an effect. These results are available upon request.

control group mean.

We also examine the distribution of donations in the treatment vs. the control group and find that the treatment effect on average donations is driven by the intensive rather than the extensive margin (Figure 4). 41% of the respondents in the control group decide to make a positive donation and the treatment increases this share by a statistically insignificant four percentage points. At the intensive margin, however, the information treatment has a substantial effect for the range of donations up to half of our respondents' $5 endowment, as illustrated by Figure 4 and by Figure A.9 in the online appendix.

By contrast, we find that treated respondents do not become significantly more willing to sign a petition in favor of introducing a balanced budget rule. Table 4 highlights that the treatment effect is small and statistically insignificant for the self-reported intention to sign the petition (column 1). We also calculate the proportion of actual signatures on the petition website for the treatment and the control group, which confirms the conclusions from the self-reports (column 4). One possibility as to why treatment effect sizes on self-reported behavior, donation behavior and petition signatures differ could be that the treatment was less effective in changing beliefs for respondents at the margin of changing their petition signatures than for respondents at the margin of changing their self-reports and donation behavior.[25]

5.5 Do the treatment effects persist?

One concern with survey experiments is that treatment effects could reflect short-lived emotional responses to the information or experimenter demand rather than true changes in beliefs and policy views. Following Cavallo et al. (2017), we address these concerns by examining the persistence of our main results in Experiment 2 (MTurk sample, 1970 anchor) in a four-week follow-up. We first show that the treatment effect on views regarding government debt persist and remains large in magnitude. As shown in Panel D of Table 1, even four weeks after receiving the treatment, respondents remain 0.16 standard deviations more likely to think that there is too much debt and 0.18 standard deviations more inclined to think that the government should reduce the amount of government debt.[26] On average, the treatment effect on respondents' attitudes towards government debt persists at 64 percent of the effect size in the main study.

[25] For a discussion on how treatments effects in information experiments depend on the density of respondents at the margin of taking an action and how much the beliefs of those at the margin are moved, see Coffman et al. (2015).

[26] In Tables A.26 - A.28 in the online Appendix we provide evidence on the robustness of these results to sample composition effects.

Moreover, we find a persistent treatment effect of 0.16 of a standard deviation on people's attitudes towards cutting the overall amount of government spending (Panel D of Table 2, column 1). This effect size corresponds to 98 percent of the effect size in the main study. Even though the effects become weaker and are not significantly different from zero for the individual spending categories, they are statistically indistinguishable from the effects in the main experiment. It is worth noting that the effect sizes on individual spending categories estimated in the main study were slightly smaller in the MTurk sample of Experiment 2 than in the three representative samples to begin with.[27] Since we only successfully recontacted 75 percent of the original sample in the follow-up on MTurk, we are naturally less powered to detect small effect sizes.

Finally, we find little persistence of the initially small effects on people's views on whether to increase the overall amount of taxes (Table 3, Panel D, column 1). There is suggestive evidence that treated respondents favor an introduction of a wealth tax and an increase in taxes for the bottom 50 percent when re-interviewed after four weeks. However, none of these results are robust to adjusting p-values for multiple hypothesis testing.

In Table A.29 in the online appendix we put the persistence of our estimated treatment effects into perspective by systematically comparing it to related literature studying the role of people's beliefs about relevant facts in shaping their demand for government intervention. The average persistence of around 60% of our treatment effect is very much in line with the majority of similar information experiments.[28]

Taken together, the fact that our findings on people's beliefs about the amount of government debt and their attitudes towards debt reduction and government spending persist in a four-week follow-up suggests that these results reflect true updating of beliefs and policy views, and that short-lived responses to our treatment are not the main driver behind these effects.

[27] We discuss this point in detail in online appendix D.

[28] Alesina et al. (2018b), Haaland and Roth (2019a) and Haaland and Roth (2019b) find effects of information treatments on low social mobility, the degree of racial discrimination and labor market effects of immigrants that persist at around 60% after one week. Similarly, Alesina et al. (2018a) and Settele (2019) find a persistence of 50% of effects of information about the characteristics of immigrants and about the size of the gender wage gap on related policy demand after two weeks. Kuziemko et al. (2015) and Grigorieff et al. (2019) conduct 4-week follow-ups and find a larger persistence of their initial treatment effects, reaching more than 100% in some cases. The fact that the persistence of our overall treatment effects (62% of the initial effects on average) is larger than the persistence of the estimated learning about the debt-to-GDP ratio which we document in Section 4.4 (around 30%) is in line with recent evidence in the context of inflation expectations and consumption (Coibion et al., 2019), and may indicate that individuals permanently adjust their plans and policy views, while forgetting about exact numerical values of information.

5.6 Is there a heterogeneous response to the information?

Heterogeneity by prior beliefs about the debt-to-GDP ratio Our information treatment is designed to be more effective for people who have highly biased beliefs about the debt-to-GDP ratio. Indeed, we find that our average effects are driven by respondents who reported lower estimates of the debt-to-GDP ratio ex-ante (see Figures 5 and 6).[29] Respondents with prior beliefs of a debt-to-GDP ratio below 50 percent respond strongly to the information in terms of their views on debt reduction and government spending. For respondents who initially over-estimated the debt-to-GDP ratio and receive the treatment, on the other hand, we find noisily measured null effects. The fact that the treatment effects are driven by individuals with a lower prior belief suggests that our results reflect true updating of beliefs and that emotional responses and priming effects are less important. In Tables A.30 - A.32 we present more detailed results on heterogeneous treatment effects according to respondents' prior beliefs on our three families of outcomes. We estimate specifications of the following form:

$$y_i = \pi_0 + \pi_1 \text{Treatment}_i + \pi_2 \text{Treatment}_i H_i + \Pi^T \mathbf{X_i} + \varepsilon_i$$

where H_i is the dimension of heterogeneity of interest, which is also included, in its non-interacted form, in the vector of controls $\mathbf{X_i}$. In panel A of each table we interact the treatment dummy with a continuous measure of overestimation of the debt-to-GDP ratio, while in panels B, C and D we use different binary measures of prior underestimation of the debt-to-GDP ratio. In line with the graphical analysis presented above, the treatment effects are generally driven by those who initially underestimated the level of debt. However, the relationship is not perfectly linear and noisily measured, which results in insignificant coefficient estimates on the interaction terms. We believe that there are two main reasons for this. First, there is measurement error in prior beliefs as there is substantial variation in people's ability to estimate abstract statistics. Second, the size of the bias is correlated with many unobserved variables which could affect the response to the information treatment. For example, one could imagine that more highly biased respondents are also less numerate and thus less capable of using our information to change their policy demand. Both described scenarios would lead to a downward bias of the estimated

[29] This graphical analysis was not pre-specified. The pre-specified regression analysis follows in the remainder of this section as well as in section B.6 of the online Appendix and confirms the graphical results.

coefficient on the interaction term.[30] All in all, however, the results in Tables A.30 - A.32 are consistent with our findings on people's belief updating described in section 4.4 and with a role for information as compared to pure priming effects in driving our results.

Heterogeneity by demographics We also test whether our treatment has heterogeneous effects across different groups. Among others, we find no differential reaction to the information according to political affiliation. Most strikingly, younger individuals and those who have children are significantly more likely to change their views on whether the government should reduce debt, which is consistent with the idea that these individuals should have the strongest concerns for the future and therefore should care more about high levels of government debt. The heterogeneity by age and parenthood is less pronounced for treatment effects on attitudes towards government spending, but individuals with children are significantly more in favor of increasing taxes of the top 10 percent and the next 40 percent income earners. The patterns of heterogeneity by other demographics, namely education and income are less pronounced, as illustrated in Tables A.33, A.34 and A.35. We provide a more detailed discussion of these results in section B.6 in the online Appendix.

5.7 OLS estimates

Are correlational estimates based on our control group consistent with the experimental results discussed so far? In line with our experimental estimates, people who think that the debt-to-GDP ratio is higher are more likely to think that the government should reduce the amount of public debt and government spending and increase taxation in general (see Table 5). Unlike in the experiment, respondents who think that there is more debt are significantly more likely to sign the petition for the introduction of a balanced budget-rule. Lastly, beliefs about the debt-to-GDP ratio and donations to the Cato institute are positively correlated as expected, but this correlation is insignificant. The differences in significance of experimental and correlational estimates for the behavioral outcomes could be due to endogeneity of the OLS results or differential effects of beliefs on policy preferences for the compliant subpopulation of respondents who update their beliefs in response to the information.[31]

[30] The only way we see to mitigate the described downward bias is to reduce measurement error in prior beliefs. In fact, our choice of providing all respondents with a historical anchor to allow them to more meaningfully estimate the current US debt-to-GDP ratio is one step in this direction. Of course, it does not completely solve the problem, which is why the estimated heterogeneity in the treatment effect by prior beliefs should be interpreted cautiously.

[31] We discuss the latter point above in section 5.4.

Taken together, the fact that we find significant correlations between beliefs about the debt-to-GDP ratio and individuals' general attitudes towards debt reduction, government spending and taxation, and that all correlations have the same sign as our experimental estimates, reassures us of the external validity of our experimental findings.

6 Mechanisms and robustness

6.1 Why do respondents want to decrease government debt?

In what follows, we examine mechanisms through which our information intervention may increase people's willingness to reduce government debt and to cut government spending.

6.1.1 Intertemporal government budget constraint

We start by examining the role of people's awareness of the intertemporal budget constraint (ITBC) of the government. As discussed in section 2, if people form their beliefs in line with the ITBC, learning that government debt is higher than previously thought should lead them to expect higher taxes or lower government spending in the future. An inclination to smooth the consumption of public goods or the tax burden over time could then lead them to demand immediate cuts in government spending. Our finding of partially stronger treatment effects among respondents who are younger or have children (see online appendix B.6), and should therefore care more about future government finances, is suggestive of this mechanism.

To examine this channel more directly, we ask our respondents whether they think that the current levels of spending and taxation are sustainable, whether they expect changes in spending and taxation for future generations, and how they expect the level of government spending and the tax burden to change between the time of the survey and ten years after the survey.[32] To increase statistical power, we also generate a summary index over the two questions on expected government spending (in ten years and for future generations), a summary index over the two questions on expected taxes, and another one summarizing all four questions indicating any expected adjustment in spending or taxes.

We find evidence consistent with the idea that our main findings operate through changes in expectations about future government spending and taxation (Table 6). In response to the

[32]We chose the time span of ten years because at this point a new administration will be in office, and we wanted participants to abstract from specific goals of the current government. Moreover, participants should still be able to form meaningful expectations over this time span, while ten years seems to be far enough in the future that spending cuts or tax increases may become necessary.

information treatment, expectations about spending and taxation in ten years and for future generations generally move in the expected directions, although they only do so significantly for expected government spending in ten years (column 1), the spending index (column 3) and the combined index (column 7). The treatment effects on these outcomes are small, but precisely estimated. Moreover, respondents who receive information about the level of government debt become significantly more likely to agree that current public finances are not sustainable (column 8) and that it will become more expensive for the government to borrow in the future (column 9). The effects on these two outcomes are large in size, robust to adjustment for multiple hypothesis testing and, at least qualitatively, present in all four experiments (see Table A.25 in the online appendix).

Consistent with recent evidence of a role of cognitive abilities for people's beliefs about the macroeconomy (D'Acunto et al., 2019), we find that highly educated individuals seem to adjust their expectations more in line with the ITBC. Individuals with at least a Bachelor degree significantly downward-adjust their expectations about government spending in ten years by 0.10 of a standard deviation and increase their beliefs about the costs of refinancing by 0.14 of a standard deviation (Panel B of Table 6). Individuals with lower educational attainment, in contrast, do not significantly adjust expectations of future government spending, taxation or the costs of refinancing (Panel C), although they do significantly change their beliefs about fiscal sustainability.

In sum, this suggests that our findings operate through the perceived ITBC of the government and a consumption-smoothing motive. Treated individuals expect some adjustment of public finances to become necessary in the future. While the exact changes they expect as a result of a shift of the perceived level of debt are more clear-cut for the more highly educated, those with lower educational attainment and likely lower cognitive abilities potentially form their policy views based on simple heuristics which are better captured by the more general question about fiscal sustainability.

6.1.2 Beliefs about wastage and government efficiency

Alternatively, our results could work through reduced trust towards the US government and changes in beliefs about the efficiency of the government. First, after learning that the debt-to-GDP ratio has reached a higher level than they previously thought respondents could become

less likely to think that the government can be trusted to do what is right.[33] More specifically, they could become less likely to think that the government makes good use of tax money or that the government is forward-looking in its spending and taxation. Second, once people learn about the large amount of government debt, they may update their beliefs about the wastage that occurs in the bureaucratic process. Such wastage could occur through general inefficiencies in the public sector or through rent-seeking activities of politicians. Less trust towards the government and a lower perceived efficiency of the public sector could make our respondents more favorable to downsizing the government. As shown in Table 7, we can rule out with high precision that the information treatment changes people's trust in the government or their beliefs about wastage in the bureaucratic process.

All in all, these results suggest that it is more likely that our effects operate through the perceived intertemporal government budget constraint and a desire to smooth the consumption of public goods over time rather than through changes in beliefs about wastage and government efficiency.

6.2 Robustness

6.2.1 Differences in effect sizes across samples

We ran our experiments on four samples, and there are slight differences in estimated effect sizes across these samples. First, Experiment 1 on a representative sample and Experiment 2 on the MTurk sample used identical instructions and were both run in early 2017. While the effect sizes for individual attitudes on government spending presented in Table 2 are larger for Experiment 1 than for Experiment 2, these differences are not statistically distinguishable. In online Appendix Section D we show, by means of a reweigthing exercise, that these differences are likely driven by the different demographic composition of the MTurk sample.

Second, in December 2019 we ran Experiments 3 and 4 which differ in several aspects from Experiments 1 and 2. For our main findings on attitudes towards government spending presented in Table 2, the effect sizes in Experiments 3 and 4 are virtually unchanged compared to Experiments 1 and 2. For views on government debt, effect sizes are smaller in Experiments 3 and 4, but still economically large and statistically significant (Table 1). Potential explanations for the differences in effect sizes include i) sampling variation, ii) changes in the economic

[33] Kuziemko et al. (2015) find that providing people with information about high levels of inequality reduces their trust towards the US government, explaining why support for government policies aimed at reducing inequality does not respond strongly to their information treatment.

environment within the more than two years between the two sets of experiments, iii) differences in the way the treatment information was presented, and iv) the provision of different anchors.[34]

6.2.2 Experimenter demand effects

We believe that it is unlikely that our results are driven by experimenter demand for at least three reasons. First, we collected data on whether people thought that our survey was politically biased. Overall, 87 percent of respondents felt that the survey was not politically biased. Moreover, our treatment did not shift people's beliefs about whether the survey was politically biased in Experiments 1 and 2. In Experiments 3 and 4 the treatment slightly increases the likelihood that respondents perceive the survey as left-wing biased (see Table A.39 in the online Appendix). The direction of the effects suggests that, if anything, experimenter demand effects should lead to a downward bias of our estimated treatment effect of interest. Second, the treatment effects persist in a four-week follow-up, which is much less likely plagued by demand effects. Third, de Quidt et al. (2018) find that respondents in online experiments change their behavior in standard preference measures only very moderately in response to explicit demand manipulations that signal the experimental hypothesis to subjects.

7 Conclusion

We provide evidence on people's beliefs and preferences regarding government debt. Respondents who learn that the debt-to-GDP ratio in the US is higher than they thought want the government to reduce the amount of debt, become less supportive of government spending and donate significantly more to an NGO lobbying for downsizing the government. By contrast, people provided with the information do not alter their views on taxation substantially, nor do they become more likely to support a petition in favor of a balanced budget rule. Taken together, our results suggest that learning about the actual amount of government debt lowers people's demand for state-financed public good provision.

Our results have several implications. First, our findings indicate that information about statistics that are relevant for future government spending and taxation can persistently change

[34]The historical anchors make the growth of debt-to-GDP quite salient. This raises the question whether our effects could be purely driven by perceived growth rates of the debt-to-GDP ratio rather than by perceptions of the level of the debt-to-GDP ratio. The fact that we find similar treatment effects using the "OECD anchor" (in which no historical reference is mentioned) and using the "historical anchors" is suggestive of the fact that perceptions about the level of debt per se play an important role above and beyond perceptions of the growth rate.

people's attitudes towards current levels of spending. This suggests that voters are at least to some extent forward-looking when forming their views on government policies, and care not only about the expected benefits but also about the financing side of government spending. Our results are consistent with other findings showing that people's policy preferences can be responsive to new information (Karadja et al., 2016; Lergetporer et al., 2018). Second, our finding that voters demand higher levels of spending when they underestimate the level of debt suggests that biased beliefs could contribute to the accumulation of high levels of debt as observed in many industrial countries. Finally, our results suggest that support for spending increases could diminish during times in which voters update their beliefs about government debt, which could restrict the political feasibility of implementing fiscal stimulus programs during a fiscal crisis such as the recent crises in Europe.

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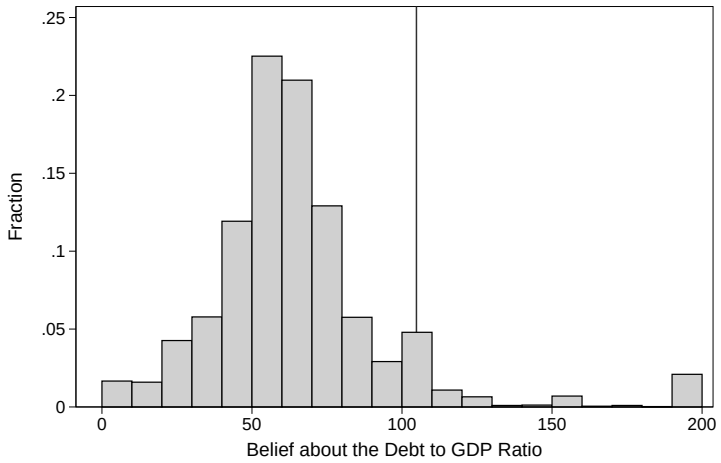
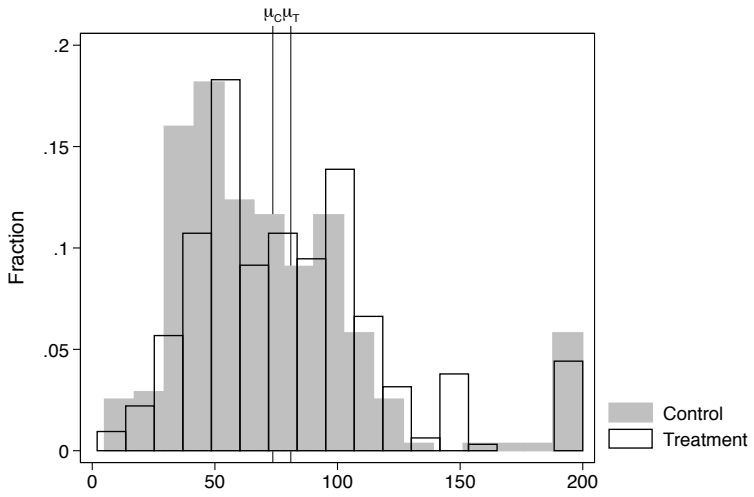
Main figures

Figure 1: Beliefs about the debt-to-GDP ratio (pooled sample)



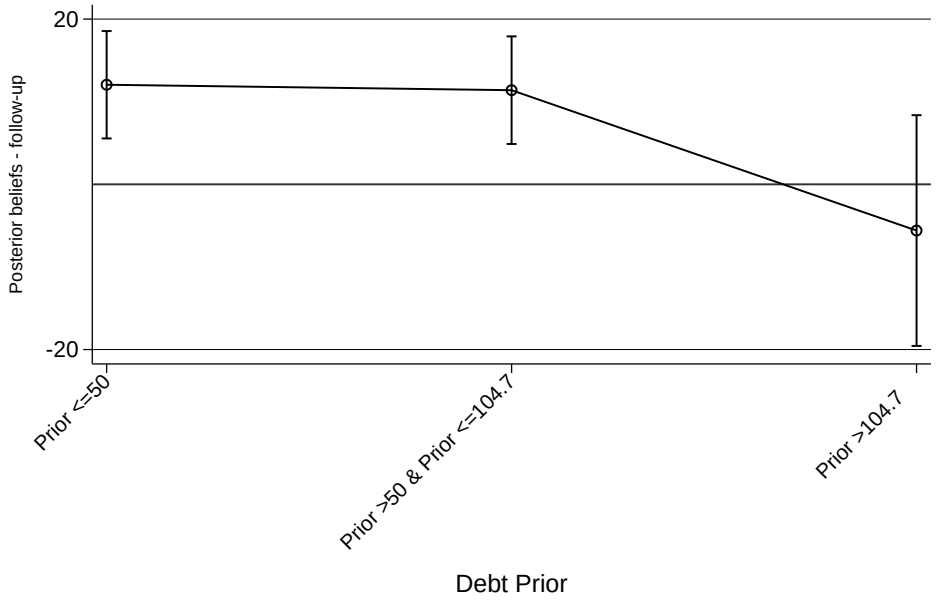
Notes: In this figure we display people's beliefs about the current debt-to-GDP ratio using data from our full sample of 4152 respondents from Experiments 1-4. Beliefs are winsorized at a debt-to-GDP ratio of 200 percent.

Figure 2: Beliefs about the debt-to-GDP ratio in the four-week follow-up



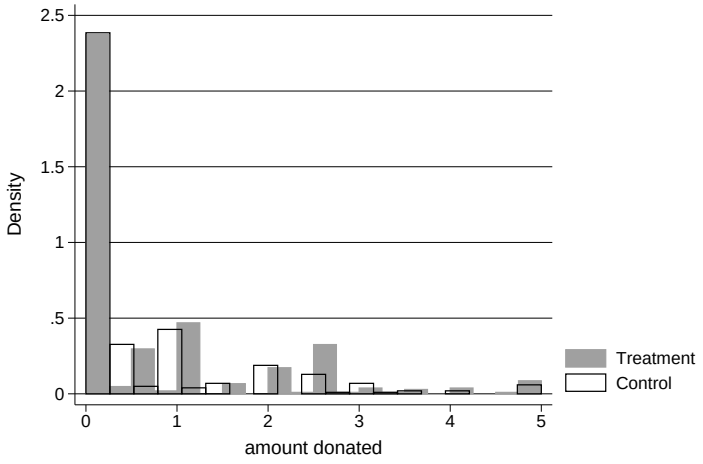
Notes: This figure describes the distribution of beliefs about the debt-to-GDP ratio in the four-week follow-up experiment for the treatment and the control group. It is based on 592 respondents who completed the follow-up. The estimates are winsorized at a debt-to-GDP ratio of 200 percent. The median belief in the treatment group is that the debt-to-GDP ratio is 75 percent, while it is 62 percent in the control group. A Kolmogorov–Smirnov test reveals that the distribution of beliefs is statistically different between the treatment and control group (p=0.018). Also the mean belief about the debt-to-GDP ratio in the treatment group is statistically different from the mean in the control group (p=0.001).

Figure 3: Heterogeneous effects on posterior beliefs about the debt-to-GDP ratio: by prior beliefs



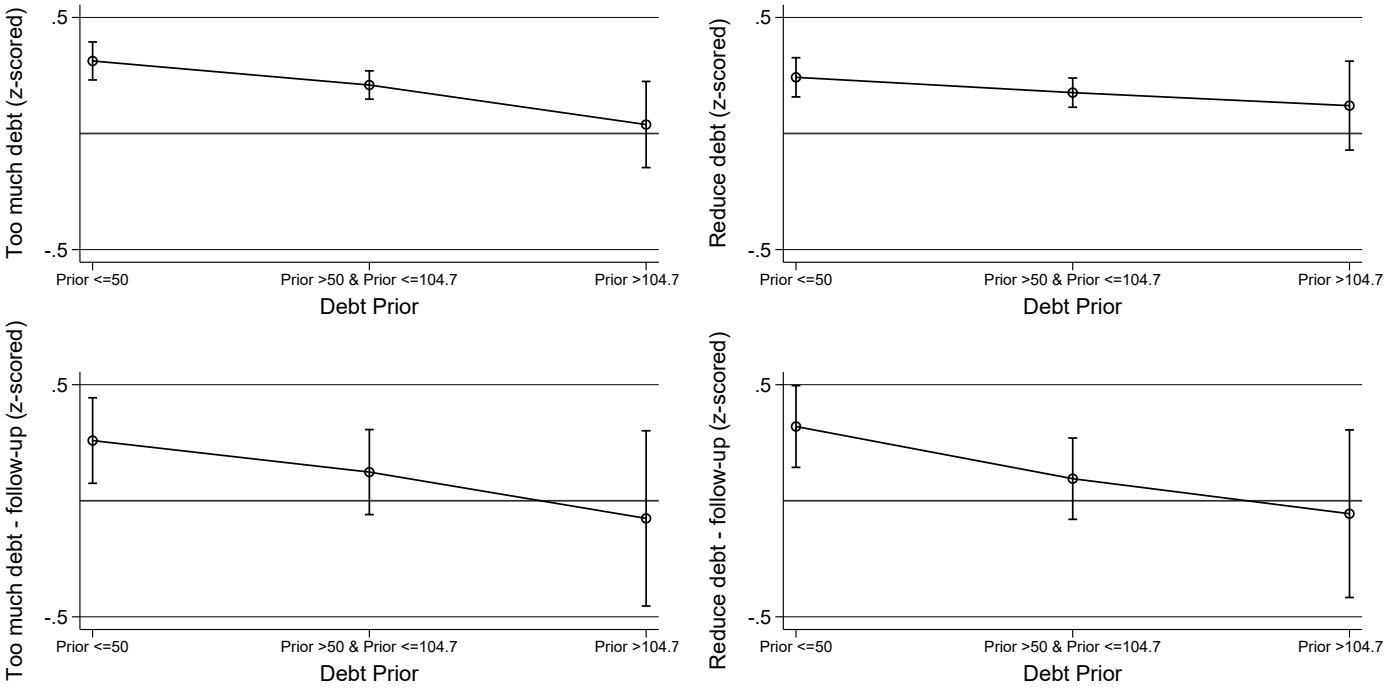
Notes: This figure describes treatment effects on posterior beliefs about the debt-to-GDP ratio by people's prior beliefs about the debt-to-GDP ratio. The figure displays the point estimate of the treatment effects with 90 percent confidence interval estimated on data from the follow-up survey on Experiment 2 (1970 anchor, MTurk sample). The treatment effect estimates control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, and the respondent's number of children (top-coded at five).

Figure 4: Donation decisions in the treatment group and in the control group



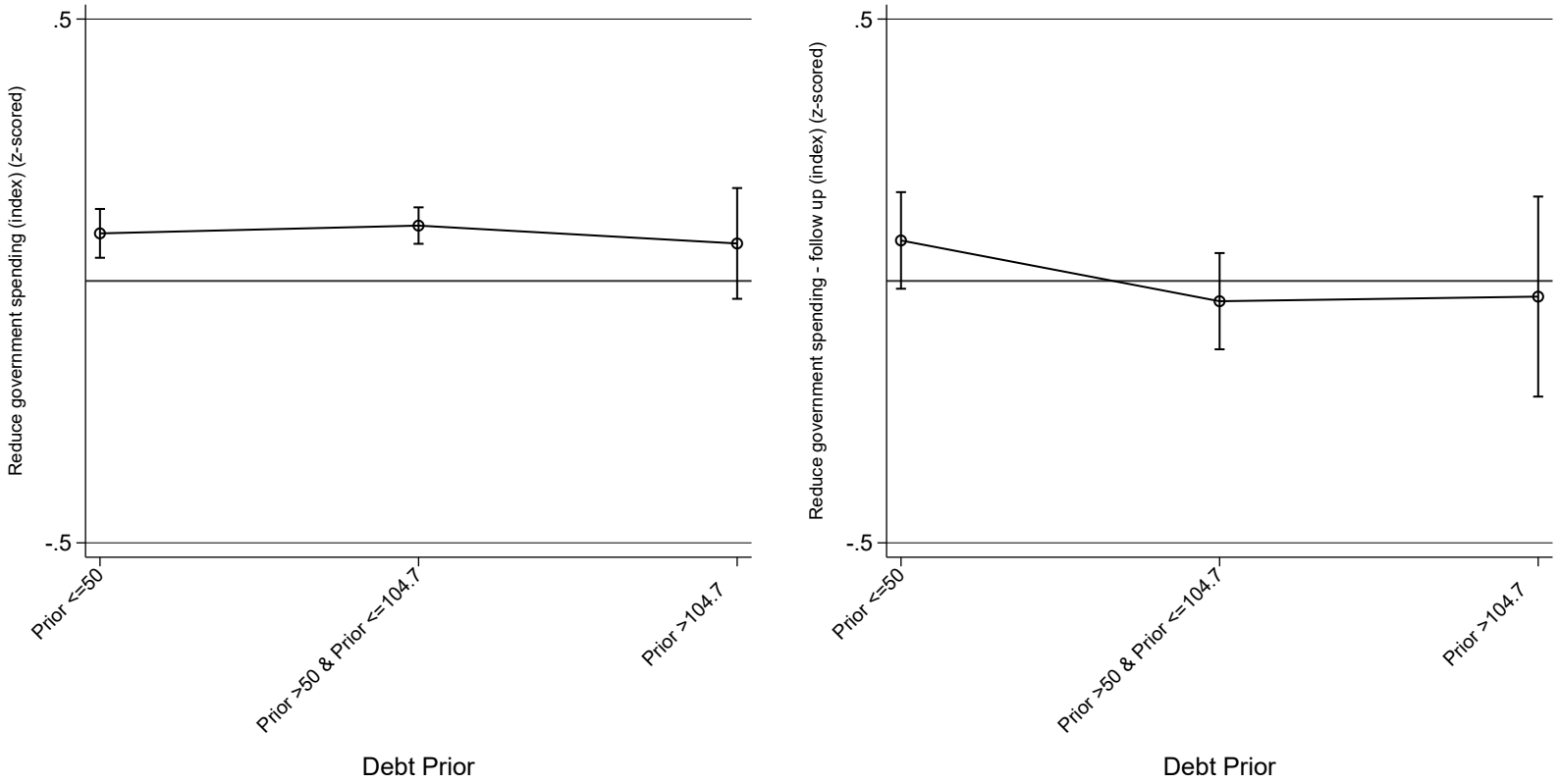
Notes: This figure shows histograms of the respondents' donation decisions in the treatment and the control group. It is based on Experiment 2 (1970 anchor, MTurk sample, N=802).

Figure 5: Heterogeneous effects on views on government debt: by prior beliefs



Notes: This figure describes treatment effects on views on government debt by people's prior beliefs about the debt-to-GDP ratio. The outcome variables are z-scored using the mean and standard deviation in the control group. The figure displays the point estimate of the treatment effects with 90 percent confidence intervals. The figures on the top are based on pooled data from Experiments 1-4 (N=4151, different anchors, representative and MTurk samples), while the figures on the bottom are based on the follow-up survey for Experiment 2 (N=597, 1970 anchor, MTurk sample). The treatment effect estimates control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five) and dummies for Experiment 1, 2, 3 and 4.

Figure 6: Heterogeneous effects on attitudes towards government spending (index): by prior beliefs



Notes: This figure describes treatment effects on an index of attitudes towards government spending by people's prior beliefs about the debt-to-GDP ratio. The outcome variable is z-scored using the mean and standard deviation in the control group. The figure displays the point estimate of the treatment effects with 90 percent confidence intervals. The figure on the left is based on pooled data from Experiments 1-4 (N=4151, different anchors, representative and MTurk samples), while the figure on the right is based on the follow-up survey for Experiment 2 (N=597, 1970 anchor, MTurk sample). The treatment effect estimates control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five) and dummies for Experiment 1, 2, 3 and 4.

Main Tables

Table 1: Views on government debt

	There is too much debt	Gov. should reduce debt	Index
Panel A: Pooled (B,C,E,F)			
Treatment	0.236***	0.196***	0.216***
	(0.029)	(0.030)	(0.028)
Adjusted p-value	[0.001]	[0.001]	
Observations	4151	4151	4151
Panel B: Exp. 1 (1970, rep. sample)			
Treatment	0.346***	0.293***	0.320***
	(0.063)	(0.062)	(0.060)
Adjusted p-value	[0.001]	[0.001]	
Observations	812	812	812
Panel C: Exp. 2 (1970, MTurk sample)			
Treatment	0.303***	0.221***	0.262***
	(0.063)	(0.066)	(0.061)
Adjusted p-value	[0.001]	[0.001]	
Observations	802	802	802
Panel D: Exp. 2 follow-up (1970, MTurk)			
Treatment	0.159**	0.176**	0.168**
	(0.075)	(0.073)	(0.070)
Adjusted p-value	[0.033]	[0.033]	
Observations	599	599	599
Panel E: Exp 3 (Hist. mean, rep. sample)			
Treatment	0.167***	0.148***	0.157***
	(0.050)	(0.052)	(0.048)
Adjusted p-value	[0.001]	[0.002]	
Observations	1488	1488	1488
Panel F: Exp 4 (OECD median, rep. sample)			
Treatment	0.230***	0.206***	0.218***
	(0.058)	(0.059)	(0.055)
Adjusted p-value	[0.001]	[0.001]	
Observations	1049	1049	1049

Notes: The outcome variables in column 1-2 are z-scored using the mean and standard deviation in the control group. The outcome in column 3 is a weighted average of those in columns 1-2, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on the follow-up of Experiment 2, Panel E is based on Experiment 3 (rep. sample, historical mean anchor) and Panel F is based on Experiment 4 (rep. sample, OECD median anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also include a set of dummies for the four subsamples. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table 2: Attitudes towards government spending

	Reduce Overall Sp.	Reduce Defense Sp.	Reduce Infrastr. Sp.	Reduce Schooling Sp.	Reduce Social Sec. Sp.	Reduce Social Ins. Sp.	Reduce Health Sp.	Reduce Environm. Sp.	Index
Panel A: Pooled (B,C,E,F)									
Treatment	0.154***	0.091***	0.115***	0.115***	0.125***	0.075***	0.107***	0.102***	0.101***
	(0.029)	(0.029)	(0.031)	(0.029)	(0.029)	(0.029)	(0.029)	(0.027)	(0.016)
Adjusted p-value	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel B: Exp. 1 (1970, rep. sample)									
Treatment	0.184***	0.045	0.217***	0.219***	0.212***	0.137**	0.191***	0.164**	0.138***
	(0.068)	(0.066)	(0.070)	(0.069)	(0.068)	(0.066)	(0.067)	(0.065)	(0.036)
Adjusted p-value	[0.007]	[0.065]	[0.005]	[0.005]	[0.005]	[0.015]	[0.005]	[0.009]	
Observations	811	811	811	811	811	811	811	811	811
Panel C: Exp. 2 (1970, MTurk sample)									
Treatment	0.160**	0.000	0.058	0.125**	0.104*	0.154**	0.096	0.092	0.065**
	(0.063)	(0.063)	(0.068)	(0.064)	(0.063)	(0.063)	(0.063)	(0.060)	(0.031)
Adjusted p-value	[0.064]	[0.333]	[0.207]	[0.109]	[0.148]	[0.064]	[0.148]	[0.148]	
Observations	802	802	802	802	802	802	802	802	802
Panel D: Exp. 2 follow-up (1970, MTurk)									
Treatment	0.156**	-0.023	0.049	0.023	0.080	0.085	0.073	-0.042	0.018
	(0.073)	(0.073)	(0.082)	(0.074)	(0.079)	(0.074)	(0.071)	(0.070)	(0.038)
Adjusted p-value	[0.349]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
Observations	599	597	597	597	597	597	597	597	597
Panel E: Exp 3 (Hist. mean, rep. sample)									
Treatment	0.158***	0.168***	0.073	0.090*	0.118**	-0.001	0.120**	0.065	0.099***
	(0.050)	(0.048)	(0.051)	(0.048)	(0.049)	(0.049)	(0.048)	(0.045)	(0.029)
Adjusted p-value	[0.006]	[0.004]	[0.094]	[0.051]	[0.026]	[0.208]	[0.026]	[0.094]	
Observations	1488	1488	1488	1488	1488	1488	1488	1488	1488
Panel F: Exp 4 (OECD median, rep. sample)									
Treatment	0.149**	0.100*	0.137**	0.062	0.073	0.093	0.029	0.113**	0.094***
	(0.059)	(0.059)	(0.063)	(0.056)	(0.060)	(0.057)	(0.057)	(0.056)	(0.034)
Adjusted p-value	[0.112]	[0.126]	[0.112]	[0.196]	[0.196]	[0.126]	[0.296]	[0.112]	
Observations	1049	1049	1049	1049	1049	1049	1049	1049	1049

Notes: The outcome variables in column 1-8 are z-scored using the mean and standard deviation in the control group. The outcome in column 9 is a weighted average of those in columns 2-8, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on the follow-up of Experiment 2, Panel E is based on Experiment 3 (rep. sample, historical mean anchor) and Panel F is based on Experiment 4 (rep. sample, OECD median anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also include a set of dummies for the four subsamples. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table 3: Attitudes towards taxation

	Increase overall amount of taxes	Increase income tax: top 10	Increase income tax: next 40	Increase income tax: bottom 50	Introduce wealth tax	Increase estate tax	Index
Panel A: Pooled (B,C,E,F)							
Treatment	0.120***	0.038	0.039	0.067**	0.020	0.077**	0.056***
	(0.029)	(0.029)	(0.030)	(0.030)	(0.029)	(0.030)	(0.018)
Adjusted p-value	[0.001]	[0.130]	[0.130]	[0.037]	[0.300]	[0.026]	
Observations	4150	4149	4149	4149	4148	4147	4147
Panel B: Exp. 1 (1970, rep. sample)							
Treatment	0.061	-0.027	-0.045	-0.036	0.037	0.134**	0.010
	(0.068)	(0.069)	(0.069)	(0.072)	(0.066)	(0.068)	(0.040)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.399]	
Observations	811	810	810	810	809	809	809
Panel C: Exp. 2 (1970, MTurk sample)							
Treatment	0.131**	0.068	0.054	0.136**	-0.099	0.001	0.048
	(0.062)	(0.064)	(0.066)	(0.068)	(0.069)	(0.067)	(0.039)
Adjusted p-value	[0.162]	[0.406]	[0.449]	[0.162]	[0.260]	[0.766]	
Observations	802	802	802	802	802	801	801
Panel D: Exp. 2 follow-up (1970, MTurk)							
Treatment	0.031	-0.138*	0.013	0.152*	0.144*	-0.078	0.028
	(0.074)	(0.082)	(0.075)	(0.079)	(0.080)	(0.079)	(0.032)
Adjusted p-value	[0.676]	[0.225]	[0.751]	[0.225]	[0.225]	[0.323]	
Observations	597	597	597	597	596	594	594
Panel E: Exp 3 (Hist. mean, rep. sample)							
Treatment	0.176***	0.063	0.105**	0.119**	0.085*	0.043	0.099***
	(0.049)	(0.048)	(0.049)	(0.049)	(0.049)	(0.051)	(0.030)
Adjusted p-value	[0.002]	[0.128]	[0.048]	[0.040]	[0.066]	[0.152]	
Observations	1488	1488	1488	1488	1488	1488	1488
Panel F: Exp 4 (OECD median, rep. sample)							
Treatment	0.069	0.017	-0.017	0.011	0.030	0.117*	0.038
	(0.059)	(0.059)	(0.062)	(0.059)	(0.059)	(0.061)	(0.035)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.503]	
Observations	1049	1049	1049	1049	1049	1049	1049

Notes: The outcome variables in column 1-5 are z-scored using the mean and standard deviation in the control group. The outcome in column 6 is a weighted average of those in columns 2-5, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on the follow-up of Experiment 2, Panel E is based on Experiment 3 (rep. sample, historical mean anchor) and Panel F is based on Experiment 4 (rep. sample, OECD median anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also include a set of dummies for the four subsamples. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table 4: Behavioral measures

	Petition in favor of a balanced budget rule				Donation to
	Want to sign	Report: Signed	Index: Self-reports	Actual signatures	Cato Institute
	(1)	(2)	(3)	(4)	(5)
Treatment	0.0419	-0.0105	0.0343	0.031	0.152**
	(0.032)	(0.028)	(0.067)	(0.019)	(0.077)
Control group mean	0.26	0.19	0.01	0.060	0.00
Observations	802	802	802	802	802

Notes: The Table is based on data from Experiment 2 (MTurk sample, 1970 anchor). The outcome variables in columns 1 and 2 are dummies, the one in column 3 is the average of the z-scored measures from columns 1 and 2. The outcome in column 5 is z-scored using the mean and standard deviation in the control group. All estimations are based on the MTurk sample. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table 5: Correlates of the demand for government spending and taxation

	Debt Reduction Index	Reduce Total sp.	Increase Total taxes	Petition Index	Donation to Cato Inst.
(Prior - 104.8) / 100	0.264***	0.232***	0.129*	0.297**	0.092
	(0.072)	(0.072)	(0.074)	(0.127)	(0.136)
Male	-0.030	-0.156***	0.092**	0.049	0.077
	(0.042)	(0.044)	(0.044)	(0.102)	(0.106)
Age	0.010***	0.009***	-0.000	-0.001	-0.009
	(0.002)	(0.002)	(0.002)	(0.005)	(0.006)
Log(Income)	0.053*	0.042	0.004	-0.036	0.161**
	(0.029)	(0.028)	(0.029)	(0.072)	(0.078)
Number of children	-0.004	0.032*	-0.044**	0.067	0.058
	(0.016)	(0.017)	(0.017)	(0.044)	(0.054)
Employed Full-Time	-0.053	-0.013	0.003	0.039	-0.059
	(0.079)	(0.085)	(0.093)	(0.225)	(0.177)
Employed Part-Time	-0.177*	-0.115	0.121	-0.090	0.056
	(0.093)	(0.094)	(0.101)	(0.249)	(0.226)
Unemployed	-0.021	-0.019	-0.105	0.339	-0.182
	(0.099)	(0.112)	(0.121)	(0.289)	(0.276)
Retired	-0.132	-0.100	0.233**	0.429	-0.165
	(0.098)	(0.103)	(0.111)	(0.524)	(0.312)
Student	-0.072	0.060	-0.019	0.357	0.477
	(0.135)	(0.125)	(0.134)	(0.404)	(0.469)
High Education	-0.066	-0.081*	0.087*	-0.144	-0.064
	(0.046)	(0.046)	(0.047)	(0.103)	(0.095)
Republican	0.176***	0.414***	-0.500***	0.140	0.172
	(0.047)	(0.046)	(0.047)	(0.118)	(0.128)
Observations	2073	2072	2072	384	384

Notes: The outcome variables are z-scored using the mean and standard deviation in the control group. Each column shows one estimation. All estimations are based on the control group in the pooled data from our four main experiments. The perceived debt-to-GDP ratio is winsorized at 200 percent, the respondent's number of children is top-coded at five. In addition to the coefficients that are shown, all regressions contain dummies for each of the four experiments and a dummy for other employment status, the respondent's trust in official US government statistics and a dummy for being a self-reported Independent. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table 6: Expectations about future government spending and taxation and beliefs about fiscal sustainability

	Exp: Decrease future gov. spending	Less gov. spending future generation	Exp. spend. index	Exp: Increase future taxes	More taxes for future generation	Exp. tax index	Exp. spend and tax index	Levels of spend. not sustainable	More expensive to refinance
Panel A: Average efffect									
Treatment	0.054*	0.039	0.047*	0.046	-0.005	0.021	0.034**	0.105***	0.067**
	(0.031)	(0.031)	(0.026)	(0.031)	(0.031)	(0.024)	(0.014)	(0.030)	(0.030)
Observations	4146	4144	4144	4146	4144	4144	4144	4143	4143
Panel B: Bachelor +									
Treatment	0.100**	0.072	0.086**	0.074	-0.005	0.034	0.060***	0.150***	0.139***
	(0.048)	(0.050)	(0.041)	(0.047)	(0.045)	(0.036)	(0.022)	(0.046)	(0.048)
Observations	1667	1667	1667	1667	1667	1667	1667	1667	1667
Panel C: < Bachelor									
Treatment	0.018	0.019	0.018	0.027	-0.004	0.011	0.015	0.076*	0.020
	(0.041)	(0.040)	(0.033)	(0.041)	(0.041)	(0.032)	(0.018)	(0.039)	(0.039)
Observations	2479	2477	2477	2479	2477	2477	2477	2476	2476
p-value(T[Bachelor +] - T[< Bachelor]) = 0	0.19	0.41	0.20	0.46	0.99	0.63	0.11	0.23	0.05

Notes: The outcome variables are z-scored using the mean and standard deviation in the control group. The outcome variable in column 3 (6) is a summary index of those in column 1 and 2 (4 and 5). In column 7 the dependent variable is a summary index of columns 1,2,4 and 5. All regressions pool observations from Experiments 1, 2, 3 and 4. Panel A shows the average treatment effect. Panel B (Panel C) restricts the sample to respondents with a Bachelor degree or more (less than a Bachelor degree). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five) and a set of dummies for the four subsamples. Robust standard errors are in parentheses. The bottom row shows p-values of χ^2-tests on the equality of the treatment coefficient in Panel B vs. C based on seemingly unrelated regressions. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table 7: Trust in the government and beliefs about government efficiency

	Trust the Gov.	Gov. makes good use of tax money	Gov. is forward-looking	Gov. bureaucracy not efficient	Politicians do not work for public
Panel A: Pooled (B,C,D,E)					
Treatment	-0.007	-0.025	-0.015	-0.032	0.011
	(0.028)	(0.028)	(0.030)	(0.031)	(0.031)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Observations	4143	4143	4143	4143	4142
Panel B: 1970 (representative)					
Treatment	-0.049	-0.069	-0.104	0.063	-0.028
	(0.065)	(0.065)	(0.067)	(0.070)	(0.074)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Observations	804	804	804	804	803
Panel C: 1970 (MTurk)					
Treatment	0.068	0.039	0.017	-0.091	0.011
	(0.062)	(0.062)	(0.066)	(0.068)	(0.066)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Observations	802	802	802	802	802
Panel D: Historical mean					
Treatment	0.014	-0.008	-0.014	-0.026	0.016
	(0.046)	(0.047)	(0.050)	(0.052)	(0.053)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Observations	1488	1488	1488	1488	1488
Panel E: OECD median					
Treatment	-0.056	-0.078	0.049	-0.049	0.045
	(0.055)	(0.054)	(0.058)	(0.061)	(0.058)
Adjusted p-value	[0.776]	[0.776]	[0.776]	[0.776]	[0.776]
Observations	1049	1049	1049	1049	1049

Notes: The outcome variables are z-scored using the mean and standard deviation in the control group. Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on Experiment 3 (rep. sample, historical mean anchor) and Panel E is based on Experiment 4 (rep. sample, OECD median anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also include a set of dummies for the four subsamples. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Online Appendix: Beliefs about Public Debt and the Demand for Government Spending

Christopher Roth, Sonja Settele, Johannes Wohlfart

Summary of the online appendix

In Section A we formally show how an increase in people's beliefs about government debt affects their attitudes towards government spending and taxation in a simple two-period model.

In Section B.2 we display summary statistics and provide evidence on the integrity of the randomization by showing that respondents in the treatment and control groups are balanced in terms of observables. In Section B.3 we provide evidence on correlates of prior beliefs and belief updating.

In Section B.4 we replicate our main results from the information experiments without control variables and present our evidence on mechanisms separately for our four experiments. In Section B.5 we present results on the persistence of treatment effects accounting for sample composition and systematically compare the persistence of our effects with existing related evidence. In Section B.6 we describe heterogeneous treatment effects in response to the information about the debt-to-GDP ratio. In Section B.7 we replicate our main results, using population-based probability weights for age, gender and income in the MTurk sample. In Section B.8 we provide additional results on beliefs about political bias, and correlates of beliefs about the debt-to-GDP ratio.

In Section C we illustrate the treatment screen and the actual evolution of the debt-to-GDP ratio in the US (Section C.1) as well as beliefs about debt-to-GDP, the desired debt-to-GDP ratio and updating in response to the information C.2. In Section C.3 we present the treatment effect on the distribution of the donation decisions. Section D demonstrates the robustness of the results in the MTurk sample to a reweighting exercise. Finally, in Section E we provide additional evidence on the effect of information about the debt-to-GDP ratio on debt-vs. tax-financed spending programs.

A Theoretical appendix

In this section we demonstrate how an increase in the perceived level of government debt affects people's policy preferences in a simple two-period model.

A representative voter who lives for two periods, $t = 1, 2$, has utility over private consumption, c_t, and over consumption of public goods, p_t. We assume that utility is separable between private and public good consumption, and allow for different discount factors for consumption of private and public goods, β and γ:

$$U = u(c_1) + \beta u(c_2) + v(p_1) + \gamma v(p_2)$$

The government can raise revenue by taxing labor income, w_t, in the two periods, which we assume to be exogenous. Given taxes, τ_t, and public good provision by the government, the voter chooses private consumption such as to maximize utility subject to the voter's intertemporal budget constraint. We assume that the voter can borrow and save at the rate $1 + r$:

$$\max_{c_1,c_2} U \qquad \text{s.t.}$$
$$c_1 + \frac{c_2}{1+r} \leq w_1 (1-\tau_1) + \frac{w_2 (1-\tau_2)}{1+r}$$

The voter believes that the government faces the following intertemporal budget constraint:

$$p_1 + \frac{p_2}{1+r} + \frac{\underline{B_3}}{(1+r)^2} \leq \alpha \left[\tau_1 w_1 + \frac{\tau_2 w_2}{1+r} + B_1 \right]$$

where B_1 is the ex-ante net wealth of the government at the beginning of the first period (the negative of government debt), $\underline{B_3}$ is an exogenous lower bound to net wealth of the government at the end of the second period.[1] α lies in the interval $[0, 1]$ and captures the efficiency of the bureaucratic process. We assume that the government can borrow and save at the same rate as the voter, $1 + r$, i.e. that there are no general equilibrium effects on the interest rate.[2]

The voter forms his or her policy preferences by choosing public good provision and taxes in the two periods such as to maximize utility, taking into account the government intertemporal budget constraint and that private consumption will be chosen optimally given taxes and public good provision.

Assuming log utility for the consumption of private and public goods, $u_t = \log c_t$ and $v_t = \log p_t$, it can be shown that the voter's preferred levels of consumption of private and public goods are given by:

$$\begin{aligned}
c_1^\star &= \frac{1}{2+\beta+\gamma} \left[w_1 + \frac{w_2}{1+r} + B_1 - \frac{\underline{B_3}}{\alpha (1+r)^2} \right] \\
c_2^\star &= \frac{\beta (1+r)}{2+\beta+\gamma} \left[w_1 + \frac{w_2}{1+r} + B_1 - \frac{\underline{B_3}}{\alpha (1+r)^2} \right] \\
p_1^\star &= \frac{1}{2+\beta+\gamma} \left[\alpha \left(w_1 + \frac{w_2}{1+r} + B_1 \right) - \frac{\underline{B_3}}{(1+r)^2} \right] \\
p_2^\star &= \frac{\gamma (1+r)}{2+\beta+\gamma} \left[\alpha \left(w_1 + \frac{w_2}{1+r} + B_1 \right) - \frac{\underline{B_3}}{(1+r)^2} \right]
\end{aligned}$$

Thus, the voter's demand for public spending is increasing in the perceived level of net wealth of the government, B_1, i.e. decreasing in the level of government debt that is inherited in the first period. It is also decreasing in the exogenous lower bound on government net wealth at the end of the second period, B_3, increasing in exogenous labor income in both periods, w_t, and in the efficiency of the government, α.

If the perceived efficiency of the government, α, positively depends on the perceived level of government net wealth, B_1, this will amplify the negative effect of updating beliefs about the level of debt on the voter's demand for public spending:

$$\frac{\delta \mathbf{p_t^\star}}{\delta \mathbf{B_1}} = \left. \frac{\delta p_t^\star}{\delta B_1} \right|_{\Delta\alpha=0} + \frac{\delta p_t^\star}{\delta \alpha} \frac{\delta \alpha}{\delta B_1} > 0$$

[1] The constraint that debt cannot exceed a certain threshold at the end of the second period captures in a stylized fashion considerations such as constraints to the government's ability to refinance when debt reaches a level that is too high.

[2] One motivation of this is that the government can borrow in international markets.

The net present value of the total tax revenue raised by the government is given by:

$$\tau_1 w_1 + \frac{\tau_2 w_2}{1+r} = \frac{1+\gamma}{2+\beta+\gamma}\left[w_1 + \frac{w_2}{1+r}\right] - \frac{1+\beta}{2+\beta+\gamma}B_1 + \frac{1+\beta}{\alpha\,(2+\beta+\gamma)}\frac{\underline{B_3}}{(1+r)^2}$$

The specific timing of taxes is indeterminate in this model. However, the net present value of taxes increases in the level of government debt at the beginning of the first period. Moreover, in this model a reduced perceived efficiency of the government, $\Delta\alpha < 0$, leads to an increase in total tax revenue collected. Intuitively, if the government works less efficiently, a higher level of taxes will be required for the government to respect the exogenous upper bound on government debt at the beginning of the third period, $-\underline{B}_3$.[3]

Taken together, in a simple two-period model with a representative voter who has log utility over the consumption of public and private goods, an increase in the perceived level of government debt leads to an immediate reduction in the preferred level of government spending. In addition, there is an increase in the net present value of total tax revenue collected. If voters update their beliefs about the efficiency of the government upon learning that government debt is higher than they thought, then this reinforces both the negative effect on the demand for government spending and the positive effect on the net present value of total taxes.

[3]The efficiency of the government affects the relative price of public good consumption, which should lead to both income and substitution effects. Assuming log utility these effects cancel out. The only channel through which the perceived efficiency of the government affects optimal public good provision and taxes is that it makes it more or less difficult to achieve the exogenous lower bound on government net wealth at the end of the second period.

B Additional tables

B.1 Timeline and overview

Table A.1: Overview of Experiments

Experiment	**Sample**	**Details**	**Anchor**
Experiment 1 (February 2017)	Research Now (N=812)	Full experiment	US debt-to-GDP ratio in 1970 (34.78%)
Experiment 2 (January 2017)	MTurk (N=802)	Full experiment	US debt-to-GDP ratio in 1970 (34.78%)
Follow-up Experiment 2 (February 2017)	MTurk (N=599)	Only outcomes	No priors
Experiment 3 (December 2019)	Lucid (N=1,488)	Full experiment	Mean US debt-to-GDP ratio last 100 years (55.2%)
Experiment 4 (December 2019)	Lucid (N=1,049)	Full experiment	Median current debt-to-GDP ratio across OECD countries (52.4%)
Pilot experiment (January 2017)	MTurk (N=207)	Only priors and desired debt ratio	Randomize 1970 anchor (34.78%) vs no anchor

Notes: This table provides an overview of the different experiments conducted.

B.2 Summary statistics and balance

Table A.2: Summary statistics: Experiment 1 (rep. sample, 1970 anchor)

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.45	0.50	0.00	0.00	1.00	813
Income	62487.70	49004.72	62500.00	0.00	250000.00	813
Age	42.32	15.69	35.00	21.00	70.00	813
Any Children	0.61	0.49	1.00	0.00	1.00	813
Full-time Employed	0.40	0.49	0.00	0.00	1.00	813
Part-time Employed	0.10	0.30	0.00	0.00	1.00	813
Unemployed	0.08	0.27	0.00	0.00	1.00	813
At Least Bachelor	0.40	0.49	0.00	0.00	1.00	813
Republican	0.36	0.48	0.00	0.00	1.00	813
Belief about the debt-to-GDP Ratio	64.78	32.73	60.00	0.00	200.00	813

Belief about the debt-to-GDP ratio stands for the (winzorized) prior belief about the US debt-to-GDP ratio in 2016, multiplied by 100.

Table A.3: Summary statistics: Experiment 2 (MTurk sample, 1970 anchor)

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.56	0.50	1.00	0.00	1.00	802
Income	58513.09	39100.10	62500.00	0.00	250000.00	802
Age	33.97	10.62	28.00	21.00	70.00	802
Any Children	0.44	0.50	0.00	0.00	1.00	802
Full-time Employed	0.63	0.48	1.00	0.00	1.00	802
Part-time Employed	0.14	0.34	0.00	0.00	1.00	802
Unemployed	0.07	0.26	0.00	0.00	1.00	802
At Least Bachelor	0.51	0.50	1.00	0.00	1.00	802
Republican	0.28	0.45	0.00	0.00	1.00	802
Belief about the debt-to-GDP Ratio	64.28	36.97	55.10	0.00	200.00	802

Belief about the debt-to-GDP ratio stands for the (winzorized) prior belief about the US debt-to-GDP ratio in 2016, multiplied by 100.

Table A.4: Summary statistics: Experiment 2 follow-up (MTurk sample)

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.57	0.50	1.00	0.00	1.00	594
Income	58059.76	38875.32	62500.00	0.00	250000.00	594
Age	34.73	10.90	28.00	21.00	70.00	594
Any Children	0.45	0.50	0.00	0.00	1.00	594
Full-time Employed	0.61	0.49	1.00	0.00	1.00	594
Part-time Employed	0.14	0.35	0.00	0.00	1.00	594
Unemployed	0.08	0.27	0.00	0.00	1.00	594
At Least Bachelor	0.52	0.50	1.00	0.00	1.00	594
Republican	0.27	0.45	0.00	0.00	1.00	594
Prior Belief About Debt-to-GDP Ratio	65.15	37.81	56.50	0.00	200.00	594

Prior Belief about the debt-to-GDP ratio stands for the (winzorized) prior belief about the US debt-to-GDP ratio in 2016, multiplied by 100.

Table A.5: Summary statistics: Experiment 3 (rep. sample, hist. mean anchor)

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.46	0.50	0.00	0.00	1.00	1488
Income	67439.52	53058.41	62500.00	0.00	250000.00	1488
Age	42.63	16.37	35.00	21.00	70.00	1488
Any Children	0.57	0.50	1.00	0.00	1.00	1488
Full-time Employed	0.43	0.49	0.00	0.00	1.00	1488
Part-time Employed	0.13	0.34	0.00	0.00	1.00	1488
Unemployed	0.07	0.25	0.00	0.00	1.00	1488
At Least Bachelor	0.37	0.48	0.00	0.00	1.00	1488
Republican	0.36	0.48	0.00	0.00	1.00	1488
Belief about the debt-to-GDP Ratio	62.52	26.89	60.00	0.00	200.00	1488

Belief about the debt-to-GDP ratio stands for the (winzorized) prior belief about the US debt-to-GDP ratio in 2018, multiplied by 100.

Table A.6: Summary statistics: Experiment 4 (rep. sample, OECD median anchor)

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.45	0.50	0.00	0.00	1.00	1049
Income	68882.27	55104.82	62500.00	0.00	250000.00	1049
Age	44.40	16.82	45.00	21.00	70.00	1049
Any Children	0.62	0.49	1.00	0.00	1.00	1049
Full-time Employed	0.41	0.49	0.00	0.00	1.00	1049
Part-time Employed	0.13	0.33	0.00	0.00	1.00	1049
Unemployed	0.06	0.24	0.00	0.00	1.00	1049
At Least Bachelor	0.36	0.48	0.00	0.00	1.00	1049
Republican	0.38	0.49	0.00	0.00	1.00	1049
Belief about the debt-to-GDP Ratio	61.86	29.59	60.00	0.00	200.00	1049

Belief about the debt-to-GDP ratio stands for the (winzorized) prior belief about the US debt-to-GDP ratio in 2018, multiplied by 100.

Table A.7: Demographics in sample 1 compared to the American Community Survey

	Mean: Sample	Mean: ACS
Female	0.55	0.51
Age 18-24	0.11	0.13
Age 25-34	0.21	0.18
Age 35-44	0.19	0.17
Age 45-54	0.17	0.17
Age 55-64	0.19	0.17
Age 65+	0.13	0.19
Northeast	0.19	0.18
Midwest	0.23	0.21
South	0.37	0.38
West	0.22	0.23
HH Inc <15.000	0.10	0.11
HH inc 15-25.000	0.12	0.09
HH inc 25-50.000	0.26	0.22
HH inc 50-75.000	0.21	0.17
HH inc 75-100.000	0.14	0.13
HH inc 100-150.000	0.10	0.15
HH inc 150-200.000	0.03	0.06
HH inc 200.000+	0.02	0.07
Total HH income	62487.70	84568.00

Notes: This table summarizes the characteristics of our sample from Experiment 1 as well as the characteristics of the 2015 American Community Survey.

Table A.8: Demographics in sample 2 compared to the American Community Survey

	Mean: Sample	Mean: ACS
Female	0.44	0.51
Age 18-24	0.10	0.13
Age 25-34	0.45	0.18
Age 35-44	0.23	0.17
Age 45-54	0.13	0.17
Age 55-64	0.07	0.17
Age 65+	0.02	0.19
Northeast	0.20	0.18
Midwest	0.23	0.21
South	0.35	0.38
West	0.22	0.23
HH Inc <15.000	0.07	0.11
HH inc 15-25.000	0.11	0.09
HH inc 25-50.000	0.28	0.22
HH inc 50-75.000	0.25	0.17
HH inc 75-100.000	0.16	0.13
HH inc 100-150.000	0.09	0.15
HH inc 150-200.000	0.02	0.06
HH inc 200.000+	0.01	0.07
Total HH income	58513.09	84568.00

Notes: This table summarizes the characteristics of our sample from Experiment 2, collected on MTurk, as well as the characteristics of the 2015 American Community Survey.

Table A.9: Demographics in sample 3 compared to the American Community Survey

	Mean: Sample	Mean: ACS
Female	0.54	0.51
Age 18-24	0.11	0.13
Age 25-34	0.22	0.18
Age 35-44	0.19	0.17
Age 45-54	0.14	0.17
Age 55-64	0.16	0.17
Age 65+	0.16	0.19
Northeast	0.19	0.18
Midwest	0.24	0.21
South	0.38	0.38
West	0.19	0.23
HH Inc <15.000	0.09	0.11
HH inc 15-25.000	0.11	0.09
HH inc 25-50.000	0.27	0.22
HH inc 50-75.000	0.19	0.17
HH inc 75-100.000	0.15	0.13
HH inc 100-150.000	0.12	0.15
HH inc 150-200.000	0.04	0.06
HH inc 200.000+	0.03	0.07
Total HH income	67439.52	84568.00

Notes: This table summarizes the characteristics of our sample from Experiment 3 as well as the characteristics of the 2015 American Community Survey.

Table A.10: Demographics in sample 4 compared to the American Community Survey

	Mean: Sample	Mean: ACS
Female	0.55	0.51
Age 18-24	0.12	0.13
Age 25-34	0.17	0.18
Age 35-44	0.17	0.17
Age 45-54	0.18	0.17
Age 55-64	0.16	0.17
Age 65+	0.20	0.19
Northeast	0.19	0.18
Midwest	0.20	0.21
South	0.40	0.38
West	0.21	0.23
HH Inc <15.000	0.12	0.11
HH inc 15-25.000	0.10	0.09
HH inc 25-50.000	0.22	0.22
HH inc 50-75.000	0.20	0.17
HH inc 75-100.000	0.12	0.13
HH inc 100-150.000	0.15	0.15
HH inc 150-200.000	0.04	0.06
HH inc 200.000+	0.03	0.07
Total HH income	68882.27	84568.00

Notes: This table summarizes the characteristics of our sample from Experiment 4 as well as the characteristics of the 2015 American Community Survey.

Table A.11: Balance: Experiment 1 (rep. online panel)

	Treatment	Control	P-value(Treatment - Control)	Observations
Prior Belief About Debt-to-GDP Ratio	63.98	65.50	0.507	813
Male	0.44	0.45	0.780	813
Age	42.10	42.51	0.711	813
Log(Income)	10.54	10.63	0.414	813
Number of Children	1.26	1.29	0.734	803
Unemployed	0.07	0.09	0.398	813
Part-time Employed	0.10	0.10	0.853	813
Full-time Employed	0.41	0.40	0.660	813
Retired	0.18	0.20	0.395	813
Student	0.05	0.05	0.713	813
Other Employment Status	0.09	0.09	0.898	813
At Least Bachelor	0.41	0.40	0.819	813
Republican	0.30	0.41	0.002	813

The p-value of a joint F-test when regressing the treatment dummy on all covariates is 0.4680.

Table A.12: Balance: Experiment 2 (MTurk sample)

	Treatment	Control	P-value(Treatment - Control)	Observations
Prior Belief About Debt-to-GDP Ratio	62.71	65.99	0.211	802
Male	0.59	0.53	0.120	802
Age	34.74	33.12	0.029	802
Log(Income)	10.66	10.52	0.199	802
Number of Children	0.90	0.89	0.892	802
Unemployed	0.08	0.06	0.191	802
Part-time Employed	0.14	0.13	0.583	802
Full-time Employed	0.61	0.66	0.135	802
Retired	0.03	0.02	0.100	802
Student	0.03	0.03	0.858	802
Other Employment Status	0.06	0.06	0.768	802
At Least Bachelor	0.50	0.52	0.552	802
Republican	0.28	0.28	0.974	802

The p-value of a joint F-test when regressing the treatment dummy on all covariates is 0.2025.

Table A.13: Balance: Experiment 2 follow-up (MTurk sample)

	Treatment	Control	P-value(Treatment - Control)	Observations
Prior Belief About Debt-to-GDP Ratio	63.99	66.49	0.424	594
Male	0.59	0.54	0.278	594
Age	35.18	34.21	0.276	594
Log(Income)	10.65	10.55	0.413	594
Number of Children	0.88	0.91	0.728	594
Unemployed	0.09	0.07	0.500	594
Part-time Employed	0.16	0.12	0.206	594
Full-time Employed	0.57	0.66	0.018	594
Retired	0.04	0.02	0.128	594
Student	0.03	0.02	0.212	594
Other Employment Status	0.06	0.07	0.638	594
At Least Bachelor	0.50	0.53	0.476	594
Republican	0.26	0.28	0.581	594

The p-value of a joint F-test when regressing the treatment dummy on all covariates is 0.3627.

Table A.14: Balance: Experiment 3 (rep. online panel)

	Treatment	Control	P-value(Treatment - Control)	Observations
Prior Belief About Debt-to-GDP Ratio	62.10	62.94	0.549	1488
Male	0.45	0.47	0.261	1488
Age	43.14	42.12	0.228	1488
Log(Income)	10.71	10.73	0.734	1488
Number of Children	1.33	1.23	0.176	1488
Unemployed	0.08	0.05	0.056	1488
Part-time Employed	0.13	0.13	0.683	1488
Full-time Employed	0.40	0.45	0.051	1488
Retired	0.20	0.18	0.257	1488
Student	0.05	0.05	0.763	1488
Other Employment Status	0.09	0.08	0.509	1488
At Least Bachelor	0.37	0.37	0.928	1488
Republican	0.37	0.34	0.225	1488

The p-value of a joint F-test when regressing the treatment dummy on all covariates is 0.5279.

Table A.15: Balance: Experiment 4 (rep. online panel)

	Treatment	Control	P-value(Treatment - Control)	Observations
Prior Belief About Debt-to-GDP Ratio	62.13	61.59	0.767	1049
Male	0.49	0.42	0.031	1049
Age	43.46	45.35	0.069	1049
Log(Income)	10.67	10.61	0.603	1049
Number of Children	1.33	1.35	0.825	1049
Unemployed	0.08	0.05	0.109	1049
Part-time Employed	0.12	0.14	0.281	1049
Full-time Employed	0.43	0.39	0.150	1049
Retired	0.20	0.23	0.199	1049
Student	0.06	0.04	0.413	1049
Other Employment Status	0.07	0.08	0.534	1049
At Least Bachelor	0.38	0.34	0.270	1049
Republican	0.38	0.39	0.953	1049

The p-value of a joint F-test when regressing the treatment dummy on all covariates is 0.1095.

B.3 Prior beliefs and belief updating

Table A.16: Correlates of beliefs about the debt-to-GDP ratio

	Outcome variable: Prior				
	Pooled Sample	Exp. 1 (rep.)	Exp. 2 (MTurk)	Exp 3 (rep.)	Exp. 4 (rep.)
Male	3.757***	9.347***	3.886	4.049***	0.939
	(1.004)	(2.448)	(2.808)	(1.466)	(1.959)
Age	0.174***	0.142	0.373**	0.172***	0.110
	(0.042)	(0.102)	(0.173)	(0.055)	(0.074)
Log(Income)	1.688***	0.758	2.326	1.484	2.328**
	(0.604)	(1.439)	(1.792)	(0.915)	(1.065)
Number of children	-0.144	-1.057	-1.211	0.106	0.471
	(0.377)	(0.970)	(1.469)	(0.515)	(0.625)
Employed	-4.162	3.916	0.840	-6.402	-7.306
Full-Time	(2.797)	(5.605)	(5.733)	(4.625)	(5.990)
Employed	-3.031	13.760**	3.192	-7.257	-8.633
Part-Time	(3.024)	(6.852)	(6.444)	(4.774)	(6.499)
Unemployed	-4.695	5.267	-5.829	-4.104	-7.908
	(2.963)	(6.132)	(6.355)	(4.758)	(6.338)
Retired	-4.820	2.893	-3.240	-5.415	-7.996
	(3.304)	(6.785)	(11.728)	(5.333)	(6.730)
High Education	2.772***	-0.620	1.070	5.175***	3.846*
	(1.066)	(2.540)	(2.764)	(1.529)	(2.137)
Republican	-1.249	3.259	2.144	-3.835**	-2.883
	(1.057)	(2.478)	(3.121)	(1.502)	(2.031)
Observations	4152	813	802	1488	1049
Anchor	Pooled	1970 US	1970 US	Hist. mean US	OECD median

Notes: The outcome variable is the (winzorized) self-reported prior belief about the debt-to-GDP ratio in percent, multiplied by 100. Column 1 shows the estimation on the pooled sample and columns 2-5 show the same specification based on our four subsamples. Column 2 is based on Experiment 1 (rep. sample, 1970 anchor), column 3 on Experiment 2 (MTurk sample, 1970 anchor), column 4 on Experiment 3 (rep. sample, historical mean anchor) and column 5 on Experiment 4 (rep. sample, OECD median anchor). In addition to the independent variables shown in the table, the specification controls for the respondent's trust in official US government statistics, a dummy for "other" political orientation which includes Independents (the omitted category being Democrats) and "other" employment status (the omitted category being full-time student). The estimation on the pooled sample also controls for a set of dummies for the four subsamples. The respondent's number of children is top-coded at five. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.17: Correlates of (absolute) misperceptions of the debt-to-GDP ratio

	Outcome variable: Absolute bias in prior belief				
	Pooled Sample	Exp. 1 (rep.)	Exp. 2 (MTurk)	Exp 3 (rep.)	Exp. 4 (rep.)
Male	-0.325	-1.191	-1.210	-1.076	1.402
	(0.702)	(1.701)	(1.839)	(1.057)	(1.398)
Age	-0.134***	-0.037	0.005	-0.187***	-0.201***
	(0.030)	(0.076)	(0.105)	(0.042)	(0.057)
Log(Income)	-1.503***	-1.135	-1.724	-2.226***	-0.498
	(0.435)	(1.004)	(1.289)	(0.666)	(0.786)
Number of children	0.147	0.750	0.469	0.185	-0.521
	(0.263)	(0.647)	(0.867)	(0.367)	(0.509)
Employed	-1.173	-6.459*	2.801	-1.226	1.074
Full-Time	(1.779)	(3.366)	(5.340)	(2.824)	(3.640)
Employed	-0.241	-7.302*	3.087	-1.904	5.551
Part-Time	(1.933)	(4.149)	(5.647)	(3.004)	(3.919)
Unemployed	-0.384	-5.537	5.684	-3.365	3.822
	(1.910)	(3.806)	(5.637)	(2.974)	(3.945)
Retired	-0.447	-7.189	0.235	-0.018	4.822
	(2.173)	(4.478)	(8.566)	(3.315)	(4.317)
High Education	-1.565**	-0.572	-0.911	-2.095*	-1.453
	(0.763)	(1.789)	(1.849)	(1.154)	(1.572)
Republican	1.223	-2.680	-0.277	2.843**	2.913*
	(0.771)	(1.783)	(2.118)	(1.142)	(1.537)
Observations	4152	813	802	1488	1049
Anchor	Pooled	1970 US	1970 US	Hist. mean US	OECD median

Notes: The outcome variable is the absolute bias in the (winzorized) self-reported prior belief about the debt-to-GDP ratio in percent, multiplied by 100. Column 1 shows the estimation on the pooled sample and columns 2-5 show the same specification based on our four subsamples. Column 2 is based on Experiment 1 (rep. sample, 1970 anchor), column 3 on Experiment 2 (MTurk sample, 1970 anchor), column 4 on Experiment 3 (rep. sample, historical mean anchor) and column 5 on Experiment 4 (rep. sample, OECD median anchor). In addition to the independent variables shown in the table, the specification controls for the respondent's trust in official US government statistics, a dummy for "other" political orientation which includes Independents (the omitted category being Democrats) and "other" employment status (the omitted category being full-time student). The estimation on the pooled sample also controls for a set of dummies for the four subsamples. The respondent's number of children is top-coded at five. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.18: Learning rate

	Posterior - Prior Full sample	Posterior Treat.Group	Posterior Cont. Group	Posterior Full sample
Panel A: Main Survey (Exp.3,4)				
Prior		0.219***	0.861***	0.852***
		(0.039)	(0.037)	(0.036)
Treat. x perc. gap	0.621***			
	(0.052)			
Treatment	3.764			68.888***
	(2.399)			(3.308)
Treat. x prior				-0.621***
				(0.052)
Perc. gap (104.81 - Prior)	0.148***			
	(0.036)			
Observations	2537	1276	1261	2537
Panel B: Follow-up Survey (Exp.2)				
Prior		0.267***	0.464***	0.474***
		(0.071)	(0.091)	(0.089)
Treat. x perc. gap	0.208*			
	(0.114)			
Treatment	0.484			22.240***
	(5.766)			(7.353)
Treat. x prior				-0.208*
				(0.114)
Perc. gap (104.81 - Prior)	0.526***			
	(0.089)			
Observations	593	319	274	593

Notes: Panel A pools data from Experiments 3 and 4 (representative samples, historical mean and OECD median anchor) and Panel B is based on the follow-up of Experiment 2 (MTurk sample, 1970 anchor). The dependent variable in column 1 is the learning rate, i.e. the respondent's posterior belief (winsorized at 200) minus her prior belief (winsorized at 200 and always based on the main survey). The independent variable "perception gap" corresponds to the update that respondents in the treatment group receive, i.e. the signal (104.81) minus the prior belief (based on the main survey and winsorized at 200). All specifications control for age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations in Panel A also control for whether the respondent is part of Experiment 3 or 4.

Table A.19: Robustness of est. learning rate to different forms of winsorizing beliefs

	Outcome: Posterior - Prior (Learning)		
	Win 200 (Main spec.)	Win 1 perc. (top and bottom)	Win 2 perc. (top and bottom)
Panel A: Main Survey (Exp.3,4)			
Treat. x perc. gap	0.621***	0.433***	0.479***
	(0.052)	(0.057)	(0.041)
Treatment	3.764	12.507***	10.354***
	(2.399)	(2.647)	(1.831)
Perc. gap (104.81 - Prior)	0.148***	0.435***	0.312***
	(0.036)	(0.046)	(0.028)
Observations	2537	2537	2537
Panel B: Follow-up Survey (Exp.2)			
Treat. x perc. gap	0.208*	0.216*	0.227**
	(0.114)	(0.128)	(0.110)
Treatment	0.484	-0.416	-0.296
	(5.766)	(6.394)	(5.618)
Perc. gap (104.81 - Prior)	0.526***	0.624***	0.501***
	(0.089)	(0.109)	(0.088)
Observations	593	593	593

Notes: Panel A pools data from Experiments 3 and 4 (representative samples, historical mean and OECD median anchor) and Panel B is based on the follow-up of Experiment 2 (MTurk sample, 1970 anchor). The dependent variable is the learning rate, i.e. the respondent's posterior belief minus her prior belief. The independent variable "perception gap" corresponds to the update that respondents in the treatment group receive, i.e. the signal (104.81) minus the prior belief (based on the main survey and winsorized at 200). Column 1 shows our standard specification, i.e. beliefs are winsorized at 200. Column 2 winsorizes beliefs below the 1st and above the 99th percentile of the distribution. The cutoffs correspond to 3.799 and 350 for the prior belief (based on Experiments 2,3 and 4), to 4 and 200 for the posterior belief in the main survey (Experiments 3 and 4) and to 15 and 300 for the posterior belief in the follow-up survey (Experiment 2). In column 3 we winsorize beliefs below the 2nd and above the 98th percentile of the distribution. The corresponding cutoffs are prior beliefs of 10 and 180 (based on Experiments 2,3 and 4), posterior beliefs of 15 and 132 in the main survey (Experiments 3 and 4) and to posterior beliefs of 15.5 and 200 in the follow-up survey (Experiment 2). All specifications control for age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations in Panel A also control for whether the respondent is part of Experiment 3 or 4. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.20: Our estimated learning rate compared to existing literature

Authors, year, journal	Context	Type of information	Immediate LR	Time lapse	Follow-up LR	Persistence
This paper	Beliefs about the debt-to-GDP ratio	Factual info provided by Fed. of St. Louis	0.621	4 weeks	0.208	33%
Roth and Wohlfart (2019) REStat (Table 1)	Recession expectations	Professional forecasts (SPF)	0.318	2 weeks	0.129	41%
Armona et al. (2019) REStud (Table 9)	House price expectations	1-year and 5-year past house price growth	0.18 (1-year) 0 (5-year)	2 months	0.13	72%
Fuster et al. (2019) WP (Figure 5)	House price expectations	Choice btw. expert forecast, past 1 year and past 10 year house price growth	0.380 (based on preferred source of information)	4 months	0.173	46%
Armantier et al. (2016) REStat (Table 2)	1-year inflation expectations	SPF forecast (average)	0.393	no follow-up	NA	
Coibion et al. (2018) AER (Table 6 and 7)	Firms' expectation of inflation unempl. rate and GDP growth	Infl.: SPF forecast, CB target or both. Unemp and GDP growth: Past 12 months	infl: 0.66 (pooled) UE: 0.35 GDP: 0.44	6 months	0	0%
Cavallo et al. (2017) AEJ: Macro (Table 1)	1-year inflation expectations	Statistics such as the current inflation rate or price changes for selected products	Statistics: 0.84 /0.43 (US/Arg.) Prices: 0.69 /0.46 (US/Arg.)	2 months/ 4 months (US/ Argentina)	Statistics: 0.36 /NA (US/Arg.) Prices:0.336/ 0.208 (US/Arg.)	43% 49% 45%

Notes: This table provides an overview of the related literature on macroeconomic expectation formation that provides respondents with information and studies i) their immediate belief updating and ii) the decay of the estimated learning rate over time.

B.4 Main tables without controls

Table A.21: Views on government debt: Without controls

	There is too much debt	Gov. should reduce debt	Index
Panel A: Pooled (B,C,E,F)			
Treatment	0.227***	0.190***	0.209***
	(0.029)	(0.030)	(0.028)
Adjusted p-value	[0.001]	[0.001]	
Observations	4151	4151	4151
Panel B: Exp. 1 (1970, rep. sample)			
Treatment	0.295***	0.245***	0.270***
	(0.063)	(0.063)	(0.060)
Adjusted p-value	[0.001]	[0.001]	
Observations	812	812	812
Panel C: Exp. 2 (1970, MTurk sample)			
Treatment	0.272***	0.197***	0.234***
	(0.066)	(0.068)	(0.064)
Adjusted p-value	[0.001]	[0.001]	
Observations	802	802	802
Panel D: Exp. 2 follow-up (1970, MTurk)			
Treatment	0.117	0.138*	0.128*
	(0.080)	(0.076)	(0.074)
Adjusted p-value	[0.158]	[0.158]	
Observations	599	599	599
Panel E: Exp 3 (Hist. mean, rep. sample)			
Treatment	0.177***	0.151***	0.164***
	(0.051)	(0.052)	(0.048)
Adjusted p-value	[0.001]	[0.001]	
Observations	1488	1488	1488
Panel F: Exp 4 (OECD median, rep. sample)			
Treatment	0.211***	0.198***	0.204***
	(0.058)	(0.059)	(0.056)
Adjusted p-value	[0.001]	[0.001]	
Observations	1049	1049	1049

Notes: The outcome variables in column 1-2 are z-scored using the mean and standard deviation in the control group. The outcome in column 3 is a weighted average of those in columns 1-2, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on the follow-up of Experiment 2, Panel E is based on Experiment 3 (rep. sample, historical mean anchor) and Panel F is based on Experiment 4 (rep. sample, OECD median anchor). The estimations on the pooled sample include a set of dummies for the four subsamples.

Table A.22: Attitudes towards government spending: Without controls

	Reduce Overall Sp.	Reduce Defense Sp.	Reduce Infrastr. Sp.	Reduce Schooling Sp.	Reduce Social Sec. Sp.	Reduce Social Ins. Sp.	Reduce Health Sp.	Reduce Environm. Sp.	Index
Panel A: Pooled (B,C,E,F)									
Treatment	0.142***	0.095***	0.107***	0.109***	0.114***	0.058*	0.097***	0.094***	0.095***
	(0.031)	(0.031)	(0.032)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.017)
Adjusted p-value	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.007]	[0.001]	[0.001]	
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel B: Exp. 1 (1970, rep. sample)									
Treatment	0.114	0.103	0.187***	0.131*	0.161**	0.026	0.105	0.067	0.107***
	(0.072)	(0.070)	(0.072)	(0.072)	(0.070)	(0.071)	(0.072)	(0.070)	(0.037)
Adjusted p-value	[0.168]	[0.168]	[0.079]	[0.166]	[0.084]	[0.238]	[0.168]	[0.238]	
Observations	811	811	811	811	811	811	811	811	811
Panel C: Exp. 2 (1970, MTurk sample)									
Treatment	0.141**	0.001	0.030	0.152**	0.102	0.149**	0.101	0.096	0.063**
	(0.068)	(0.070)	(0.071)	(0.069)	(0.067)	(0.068)	(0.070)	(0.069)	(0.032)
Adjusted p-value	[0.114]	[0.327]	[0.289]	[0.114]	[0.163]	[0.114]	[0.163]	[0.163]	
Observations	802	802	802	802	802	802	802	802	802
Panel D: Exp. 2 follow-up (1970, MTurk)									
Treatment	0.105	-0.012	0.014	0.007	0.054	0.040	0.047	-0.073	0.001
	(0.077)	(0.083)	(0.083)	(0.079)	(0.083)	(0.082)	(0.081)	(0.081)	(0.038)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
Observations	599	597	597	597	597	597	597	597	597
Panel E: Exp 3 (Hist. mean, rep. sample)									
Treatment	0.178***	0.142***	0.086	0.115**	0.120**	0.017	0.142***	0.098*	0.112***
	(0.052)	(0.052)	(0.053)	(0.051)	(0.052)	(0.052)	(0.052)	(0.051)	(0.030)
Adjusted p-value	[0.005]	[0.014]	[0.045]	[0.025]	[0.025]	[0.131]	[0.014]	[0.041]	
Observations	1488	1488	1488	1488	1488	1488	1488	1488	1488
Panel F: Exp 4 (OECD median, rep. sample)									
Treatment	0.115*	0.096	0.135**	0.049	0.079	0.072	0.021	0.107*	0.084**
	(0.061)	(0.062)	(0.065)	(0.061)	(0.061)	(0.061)	(0.060)	(0.061)	(0.036)
Adjusted p-value	[0.280]	[0.280]	[0.280]	[0.337]	[0.280]	[0.280]	[0.466]	[0.280]	
Observations	1049	1049	1049	1049	1049	1049	1049	1049	1049

Notes: The outcome variables in column 1-8 are z-scored using the mean and standard deviation in the control group. The outcome in column 9 is a weighted average of those in columns 2-8, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on the follow-up of Experiment 2, Panel E is based on Experiment 3 (rep. sample, historical mean anchor) and Panel F is based on Experiment 4 (rep. sample, OECD median anchor). The estimations on the pooled sample include a set of dummies for the four subsamples. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.23: Attitudes towards taxation: Without controls

	Increase overall amount of taxes	Increase income tax: top 10	Increase income tax: next 40	Increase income tax: bottom 50	Introduce wealth tax	Increase estate tax	Index
Panel A: Pooled (B,C,E,F)							
Treatment	0.125***	0.042	0.047	0.059*	0.024	0.082***	0.057***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.019)
Adjusted p-value	[0.001]	[0.131]	[0.131]	[0.083]	[0.227]	[0.022]	
Observations	4150	4149	4149	4149	4148	4147	4147
Panel B: Exp. 1 (1970, rep. sample)							
Treatment	0.102	0.044	0.011	-0.059	0.092	0.193***	0.040
	(0.071)	(0.073)	(0.070)	(0.073)	(0.069)	(0.069)	(0.042)
Adjusted p-value	[0.437]	[0.574]	[0.771]	[0.574]	[0.437]	[0.032]	
Observations	811	810	810	810	809	809	809
Panel C: Exp. 2 (1970, MTurk sample)							
Treatment	0.154**	0.078	0.064	0.120*	-0.106	0.010	0.046
	(0.068)	(0.069)	(0.069)	(0.070)	(0.072)	(0.071)	(0.043)
Adjusted p-value	[0.171]	[0.358]	[0.400]	[0.282]	[0.314]	[0.634]	
Observations	802	802	802	802	802	801	801
Panel D: Exp. 2 follow-up (1970, MTurk)							
Treatment	0.089	-0.102	0.045	0.155*	0.122	-0.054	0.036
	(0.080)	(0.086)	(0.079)	(0.079)	(0.084)	(0.083)	(0.032)
Adjusted p-value	[0.499]	[0.499]	[0.665]	[0.439]	[0.499]	[0.665]	
Observations	597	597	597	597	596	594	594
Panel E: Exp 3 (Hist. mean, rep. sample)							
Treatment	0.159***	0.050	0.093*	0.116**	0.061	0.022	0.084***
	(0.052)	(0.050)	(0.050)	(0.051)	(0.051)	(0.052)	(0.031)
Adjusted p-value	[0.013]	[0.235]	[0.086]	[0.060]	[0.216]	[0.504]	
Observations	1488	1488	1488	1488	1488	1488	1488
Panel F: Exp 4 (OECD median, rep. sample)							
Treatment	0.074	0.004	-0.004	0.021	0.019	0.137**	0.042
	(0.062)	(0.061)	(0.063)	(0.061)	(0.062)	(0.064)	(0.037)
Adjusted p-value	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.236]	
Observations	1049	1049	1049	1049	1049	1049	1049

Notes: The outcome variables in column 1-5 are z-scored using the mean and standard deviation in the control group. The outcome in column 6 is a weighted average of those in columns 2-5, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on the follow-up of Experiment 2, Panel E is based on Experiment 3 (rep. sample, historical mean anchor) and Panel F is based on Experiment 4 (rep. sample, OECD median anchor). The estimations on the pooled sample include a set of dummies for the four subsamples. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.24: Behavioral measures: Without controls

	Petition in favor of a balanced budget rule				Donation to
	Want to sign	Report: Signed	Index: Self-reports	Actual signatures	Cato Institute
	(1)	(2)	(3)	(4)	(5)
Treatment	0.0386	-0.0105	0.00554	0.031	0.141*
	(0.032)	(0.027)	(0.027)	(0.019)	(0.077)
Control group mean	0.26	0.19	0.01	0.060	0.00
Observations	802	802	802	802	802

Notes: The Table is based on data from Experiment 2 (MTurk sample, 1970 anchor). The outcome variables in columns 1 and 2 are dummies, the outcome in column 3 is the average of the z-scored measures from columns 1 and 2. The outcome in column 5 is z-scored using the mean and standard deviation in the control group. All estimations are based on the MTurk sample. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.25: Expectations about future government spending and taxation and beliefs about fiscal sustainability

	Exp: Increase future taxes	Exp: Decrease future gov. spending	More taxes for future generation	Less gov. spending for future generation	Levels of spend. not sustainable	More expensive to refinance	Exp: increase tax to gdp	Exp: decrease spend. to gdp	Exp: GDP growth (reverse)
Panel A: Pooled (B,C,D,E)									
Treatment	0.054*	0.039	0.047*	0.046	-0.005	0.021	0.034**	0.105***	0.067**
	(0.031)	(0.031)	(0.026)	(0.031)	(0.031)	(0.024)	(0.014)	(0.030)	(0.030)
Adjusted p-value	[0.161]	[0.123]	[0.320]	[0.192]	[0.002]	[0.067]			
Observations	4146	4144	4144	4146	4144	4144	4144	4143	4143
Panel B: 1970 (representative)									
Treatment	-0.025	0.043	0.009	0.115	0.044	0.079	0.044	0.169**	0.039
	(0.071)	(0.074)	(0.060)	(0.071)	(0.068)	(0.053)	(0.031)	(0.068)	(0.068)
Adjusted p-value	[0.365]	[0.927]	[0.843]	[0.843]	[0.085]	[0.843]			
Observations	807	805	805	807	805	805	805	804	804
Panel C: 1970 (MTurk)									
Treatment	0.107	0.045	0.076	0.028	0.015	0.022	0.049	0.122*	0.072
	(0.073)	(0.069)	(0.062)	(0.072)	(0.069)	(0.058)	(0.030)	(0.067)	(0.067)
Adjusted p-value	[1.000]	[0.735]	[1.000]	[1.000]	[0.735]	[0.736]			
Observations	802	802	802	802	802	802	802	802	802
Panel D: Historical mean									
Treatment	0.115**	0.058	0.086**	0.007	-0.044	-0.019	0.034	0.065	0.031
	(0.053)	(0.052)	(0.043)	(0.052)	(0.052)	(0.040)	(0.023)	(0.052)	(0.051)
Adjusted p-value	[1.000]	[0.228]	[0.998]	[0.776]	[0.776]	[1.000]			
Observations	1488	1488	1488	1488	1488	1488	1488	1488	1488
Panel E: OECD median									
Treatment	-0.001	0.038	0.019	0.083	-0.004	0.039	0.029	0.126**	0.158***
	(0.061)	(0.060)	(0.049)	(0.062)	(0.061)	(0.047)	(0.027)	(0.059)	(0.059)
Adjusted p-value	[0.315]	[0.982]	[0.982]	[0.642]	[0.084]	[0.049]			
Observations	1049	1049	1049	1049	1049	1049	1049	1049	1049

Notes: The outcome variables are z-scored using the mean and standard deviation in the control group. The outcome variable in column 3 (6) is a summary index of those in column 1 and 2 (4 and 5). In column 7 the dependent variable is a summary index of columns 1,2,4 and 5. Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on Experiment 3 (rep. sample, historical mean anchor) and Panel E is based on Experiment 4 (rep. sample, OECD median anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also include dummies for each of the four experiments. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

B.5 Persistence of the effect accounting for sample composition

Table A.26: Views on government debt: Sample composition effects

	There is too much debt	Gov. should reduce debt	Index
Panel A: MTurk Main			
Treatment	0.301***	0.218***	0.259***
	(0.063)	(0.065)	(0.061)
Adjusted p-value	[0.001]	[0.001]	
Observations	802	802	802
Panel B: MTurk Main (follow-up sample)			
Treatment	0.378***	0.287***	0.333***
	(0.074)	(0.075)	(0.070)
Adjusted p-value	[0.001]	[0.001]	
Observations	599	599	599
Panel C: MTurk Follow-up			
Treatment	0.159**	0.177**	0.168**
	(0.075)	(0.073)	(0.070)
Adjusted p-value	[0.033]	[0.033]	
Observations	599	599	599

Notes: All regression are based on Experiment 2 (MTurk sample, 1970 anchor). The outcome variables in column 1-2 are z-scored using the mean and standard deviation in the control group. The outcome in column 3 is a weighted average of those in columns 1-2, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the MTurk sample from the main Experiment, Panel B shows estimations on the results from the main experiment based on those MTurk respondents who completed the follow-up survey and Panel C shows results from the follow-up experiment. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.27: Attitudes towards government spending: Sample composition effects

	Reduce Overall Sp.	Reduce Defense Sp.	Reduce Infrastr. Sp.	Reduce Schooling Sp.	Reduce Social Sec. Sp.	Reduce Social Ins. Sp.	Reduce Health Sp.	Reduce Environm. Sp.	Index
Panel A: MTurk Main									
Treatment	0.160**	0.000	0.058	0.125**	0.104*	0.154**	0.096	0.092	0.065**
	(0.063)	(0.063)	(0.068)	(0.064)	(0.063)	(0.063)	(0.063)	(0.060)	(0.031)
Adjusted p-value	[0.064]	[0.333]	[0.207]	[0.109]	[0.148]	[0.064]	[0.148]	[0.148]	
Observations	802	802	802	802	802	802	802	802	802
Panel B: MTurk Main (follow-up sample)									
Treatment	0.151**	-0.041	0.093	0.166**	0.104	0.141*	0.136*	0.100	0.061*
	(0.071)	(0.071)	(0.078)	(0.074)	(0.072)	(0.074)	(0.072)	(0.069)	(0.035)
Adjusted p-value	[0.137]	[0.252]	[0.154]	[0.137]	[0.137]	[0.137]	[0.137]	[0.137]	
Observations	599	597	597	597	597	597	597	597	597
Panel C: MTurk Follow-up									
Treatment	0.156**	-0.023	0.049	0.023	0.080	0.085	0.073	-0.042	0.018
	(0.073)	(0.073)	(0.082)	(0.074)	(0.079)	(0.074)	(0.071)	(0.070)	(0.038)
Adjusted p-value	[0.349]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
Observations	599	597	597	597	597	597	597	597	597

Notes: All regression are based on Experiment 2 (MTurk sample, 1970 anchor). The outcome variables in column 1-8 are z-scored using the mean and standard deviation in the control group. The outcome in column 9 is a weighted average of those in columns 2-8, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the MTurk sample from the main Experiment, Panel B shows estimations on the results from the main experiment based on those MTurk respondents who completed the follow-up survey and Panel C shows results from the follow-up experiment. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.28: Attitudes towards taxation: Sample composition effects

	Increase overall amount of taxes	Increase income tax: top 10	Increase income tax: next 40	Increase income tax: bottom 50	Introduce wealth tax	Increase estate tax	Index
Panel A: MTurk Main							
Treatment	0.131	0.067	0.050	0.135	-0.096	-0.003	0.047
	(0.062)	(0.064)	(0.066)	(0.068)	(0.069)	(0.067)	(0.039)
Adjusted p-value	[0.171]	[0.422]	[0.489]	[0.171]	[0.280]	[0.803]	
Observations	802	802	802	802	802	801	801
Panel B: MTurk Main (follow-up sample)							
Treatment	0.155	0.000	0.101	0.160	-0.105	0.010	0.054
	(0.071)	(0.073)	(0.077)	(0.080)	(0.081)	(0.079)	(0.046)
Adjusted p-value	[0.154]	[0.499]	[0.246]	[0.154]	[0.246]	[0.499]	
Observations	597	597	597	597	596	593	593
Panel C: MTurk Follow-up							
Treatment	0.030	-0.138	0.012	0.151	0.144	-0.085	0.017
	(0.074)	(0.082)	(0.075)	(0.079)	(0.080)	(0.079)	(0.039)
Adjusted p-value	[0.691]	[0.222]	[0.733]	[0.222]	[0.222]	[0.268]	
Observations	597	597	597	597	596	593	593

Notes: All regression are based on Experiment 2 (MTurk sample, 1970 anchor). The outcome variables in column 1-5 are z-scored using the mean and standard deviation in the control group. The outcome in column 6 is a weighted average of those in columns 2-5, following the weighting procedure described in Anderson (2008). Panel A shows estimations on the MTurk sample from the main Experiment, Panel B shows estimations on the results from the main experiment based on those MTurk respondents who completed the follow-up survey and Panel C shows results from the follow-up experiment. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.29: Persistence of treatment effect compared to existing literature

Authors, year	Context	Type of info	Outcome variable	Immediate effect	Time lapse	Follow-up effect	Persistence in percent	Additional info
This paper	Beliefs about the debt-to-GDP ratio and demand for gov. spend.	Factual info by Fed. of St. Louis	Summary index on: Views on debt (index) Demand for gov. spending Demand for taxation	 0.26 0.160 0.131	4 weeks	 0.168 0.156 0.031	**Avg:** 62% 64% 98% 24%	not obfuscated
Kuziemko et al. (2015) (Table 6)	Elasticity of preferences for redistribution	Factual info on income inequality and link btw. inc. tax rate & growth	 Support for estate tax Pref. government scope	 0.337 0.259	4 weeks	 0.195 0.364	**Avg:** 99.5% 58% 141%	not obfuscated
Alesina et al. (2018b) (Table 5)	Beliefs about social mobility and demand for gov. intervention	Qualitative info on low social mobility	Belief about likelihood of remaining in bottom income quartile	9.25	1 week	5.67	61%	not obfusc. (more similar outcomes in paper)
Grigorieff et al. (2019) (Tables 2,3,4)	Beliefs about immigrants and support for immigration	Info on immigrants: Share of population Unempl. rate Incarceration rate English speakers	Summary Index on: Beliefs about charact. Bel. about effect on US Policy preferences	 0.368 0.121 0.060	4 weeks	 0.213 0.139 0.116	**Avg:** 153% 58% 151% 195%	not obfuscated
Haaland and Roth (2019a) (Table A.25)	Beliefs about racial discrim. and support for pro-black policy	Factual, research-finding on extent of racial discrim. in labour market	View that racial disc. is a serious problem	0.11	1 week	0.068	62%	obfuscated (most outcomes elicited only in follow-up)
Haaland and Roth (2019b) (Table 2)	Labor market concerns and support for immigration	Factual, causal evidence on effect of imm. on US labor markets	Positive attitudes towards low-skilled immigrants (index)	0.15	1 week	0.1	66%	obfuscated
Alesina et al. (2018a) (Table 7)	Beliefs about immigrants and demand for redistribution	Info on origins and share of immigrants anecdote on hard work of immigrants	 Beliefs about origins (by origin) Beliefs about share Beliefs about hard work	1-3 weeks -7.22, 15.12 -3.44, 5.46 -7.05 -0.09		 -2.81, 7.23 -0.57, 2.15 -1.37 0.08	**Avg:** 49% 36% (avg.) 19% 92%	not obfuscated
Settele (2019) (Table 7)	Beliefs about the gender wage gap and demand for gov. intervention	(Noisy) statistical information about the size of the gender wage gap	 Belief that GWG = problem Gov. should intervene Policy demand	2 weeks 0.422 0.243 0.12, 0.11		 0.186 0.183 0.10, 0.01	**Avg:** 55% 44% 75% 46% (avg.)	obfuscated

Notes: This table provides an overview of the related literature in political economy that provides respondents with relevant information and studies i) their immediate response in terms of policy views and ii) the persistence of this immediate response over time.

B.6 Heterogeneous effects

In what follows, we discuss the heterogeneous treatment effects by respondents' personal characteristics presented in Tables A.33 to A.36.

One could imagine that people's political affiliation plays an important role in shaping their response to our information treatment. In particular, Republicans already have a very strong preference for downsizing the government, which reduces the available variation to change their preferences. However, it is also possible that Republicans could engage in motivated reasoning and use the high levels of debt as an excuse to demand further decreases in government spending. Our results in Tables A.33 to A.35 are generally more in line with the first of these two explanations, i.e., if anything, Republicans' views in the context of public debt are less elastic to the information treatment. However, most interaction terms are small and insignificant.

Next, people with different levels of educational attainment may respond differently to information about the debt-to-GDP ratio. On the one hand, it is possible that people with more education respond less to the information treatment because they are less biased about the true statistic than are people with low levels of education or because their political views are more substantiated. On the other hand, they could respond more strongly to the treatment as they are more numerate and more able to interpret the information. (See Gilens (2001) as well as our evidence on the perceived ITBC for high and lower educated individuals, which we present in Section 6.1.1 of the paper.) Empirically, we find no heterogeneity by education in the elasticity of our respondents' demand for taxation and government spending to the treatment information (Panel B of Tables A.33 to A.35).

The treatment elasticity could also differ by household income, for instance based on the extent to which different income groups are affected by government spending and taxation. Empirically, however, we do not find evidence in support of a self-interested reaction to the information treatment, nor of a stronger or weaker response in terms of general attitudes towards government debt (Panel C of Tables A.33 to A.35).

We also study heterogeneous treatment effects by age (Panel D of Tables A.33 to A.35). We expect young individuals to respond more strongly to the treatment as they are more likely to see higher taxes and lower government spending in the future which might become necessary in order to reduce government debt. Panel D of Table A.33 shows that younger individuals react significantly more strongly in terms of their general perceptions, but this heterogeneity does not translate into similar heterogeneity concerning the demand for government spending and taxation, with the exception of the taxation of high income earners (Panel D of Table A.35).

Similarly, in order to test whether concerns for future generations mediate the response to our treatment, we also examine heterogeneity by a dummy variable taking value one if the respondent reports to have at least one child (Panel E of Tables A.33 to A.35). We find that those with children update their general perception of debt more strongly (Table A.33). Regarding preferences on government spending and taxation, the heterogeneous treatment effect by parenthood is more nuanced. The information treatment increases parents' demand for taxation of the top 10 percent and of the next 40 percent of income earners significantly more strongly than for non-parents, but parents do not react significantly differently when it comes to their demand for government spending (Table A.34).

Finally, in addition to our pre-specified dimensions of heterogeneity, we consider differences in the treatment effect by gender (Panel F of Tables A.33 to A.35). This exercise is motivated by Section 6.2.1 in the main text and appendix B.5 where we discuss the role of the different demographic composition of the MTurk sample as compared to our representative samples. We find that male survey participants react less strongly than females in terms of their demand for debt reduction (Table A.33) and their demand for a reduction of government spending on

infrastructure, social security and health (Table A.34). When it comes to the demand for taxation, the treatment effects are similar for males and females.

In Table A.36 we control for the different pre-specified dimensions of heterogeneity at the same time. The results should be interpreted cautiously due to the large number of interaction terms leading to low power. However, it is reassuring that they are generally in line with the evidence in Tables A.33 to A.35, confirming the nuanced patterns of heterogeneity described above.

Table A.30: General views: Heterogeneity by Prior

	There is too much debt	Gov. should reduce debt	Index
Panel A: Est. Debt-to-GDP (continuous)			
Treatment ×	-0.002	-0.000	-0.001
(Prior - 104.8)	(0.001)	(0.001)	(0.001)
Treatment	0.227***	0.224***	0.172***
	(0.069)	(0.065)	(0.049)
p-value [T + T × x P-104.8]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel B: Underestimators			
Treatment ×	0.266	0.091	0.138
(Prior < 104.8)	(0.171)	(0.159)	(0.122)
Treatment	0.064	0.159	0.086
	(0.166)	(0.154)	(0.119)
p-value [T + T × x P<104.8]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel C: Low est. Debt-to-GDP			
Treatment ×	0.066	0.001	0.029
(Prior < 90)	(0.138)	(0.125)	(0.096)
Treatment	0.254*	0.243**	0.189**
	(0.132)	(0.118)	(0.092)
p-value [T + T × x P<90]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel D: Below Median est. Debt-to-GDP			
Treatment ×	0.133*	0.073	0.084
(Prior < 60)	(0.078)	(0.075)	(0.055)
Treatment	0.244***	0.205***	0.171***
	(0.052)	(0.049)	(0.036)
p-value [T + T × x P<60]	0.00	0.00	0.00
Observations	4151	4151	4151

Notes: The outcome variables in columns 1 and 2 are z-scored using the mean and standard deviation in the control group. The outcome in column 3 is a weighted average of those in columns 1-2, following the weighting procedure described in Anderson (2008). Each column shows one estimation and every estimation is done on the pooled sample. All specifications control for the corresponding dimension of heterogeneity in its non-interacted form, (linear) prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.31: Views on spending: Heterogeneity by Prior

	Reduce Overall Sp.	Reduce Defense Sp.	Reduce Infrastr. Sp.	Reduce Schooling Sp.	Reduce Social Sec. Sp.	Reduce Social Ins. Sp.	Reduce Health Sp.	Reduce Environm. Sp.	Index
Panel A: Est. Debt-to-GDP (continuous)									
Treatment ×	-0.002*	-0.001	-0.001	-0.001	-0.000	0.000	0.001	0.000	-0.001
(Prior - 104.8)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment	0.076	0.061	0.075	0.080	0.118**	0.090*	0.163***	0.104**	0.077**
	(0.055)	(0.053)	(0.055)	(0.055)	(0.054)	(0.052)	(0.054)	(0.052)	(0.031)
p-value [T + T × x P-104.8]	0.19	0.27	0.19	0.15	0.03	0.09	0.00	0.05	0.02
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel B: Underestimators									
Treatment ×	0.123	0.099	0.091	0.030	0.013	-0.046	-0.201	-0.043	0.040
(Prior < 104.8)	(0.133)	(0.131)	(0.129)	(0.132)	(0.130)	(0.125)	(0.131)	(0.128)	(0.072)
Treatment	0.037	0.003	0.028	0.083	0.110	0.113	0.290**	0.136	0.061
	(0.130)	(0.127)	(0.125)	(0.129)	(0.127)	(0.122)	(0.127)	(0.124)	(0.070)
p-value [T + T × x P<104.8]	0.00	0.00	0.00	0.00	0.00	0.03	0.00	0.00	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel C: Low est. Debt-to-GDP									
Treatment ×	0.091	0.046	0.125	0.096	0.017	-0.112	-0.202**	0.050	0.031
(Prior < 90)	(0.101)	(0.102)	(0.104)	(0.104)	(0.101)	(0.099)	(0.102)	(0.098)	(0.058)
Treatment	0.071	0.055	0.003	0.026	0.107	0.169*	0.282***	0.052	0.072
	(0.097)	(0.097)	(0.099)	(0.099)	(0.096)	(0.094)	(0.097)	(0.093)	(0.055)
p-value [T + T × x P<90]	0.00	0.00	0.00	0.00	0.00	0.07	0.01	0.00	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel D: Below Median est. Debt-to-GDP									
Treatment ×	0.068	0.069	-0.005	-0.026	0.015	0.054	-0.063	-0.045	0.010
(Prior < 60)	(0.060)	(0.061)	(0.062)	(0.059)	(0.060)	(0.060)	(0.060)	(0.060)	(0.034)
Treatment	0.116***	0.060	0.117***	0.122***	0.114***	0.043	0.130***	0.118***	0.093***
	(0.040)	(0.043)	(0.043)	(0.043)	(0.042)	(0.042)	(0.043)	(0.042)	(0.024)
p-value [T + T × x P<60]	0.00	0.00	0.01	0.02	0.00	0.02	0.12	0.09	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150

Notes: The outcome variables in column 1-8 are z-scored using the mean and standard deviation in the control group. The outcome in column 9 is a weighted average of those in columns 2-8, following the weighting procedure described in Anderson (2008). Each column shows one estimation and every estimation is done on the pooled sample. All specifications control for the corresponding dimension of heterogeneity in its non-interacted form, (linear) prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.32: Views on taxes: Heterogeneity by Prior

	Increase overall amount of taxes	Increase income tax: top 10	Increase income tax: next 40	Increase income tax: bottom 50	Introduce wealth tax	Increase estate tax	Tax Index
Panel A: Est. Debt-to-GDP (continuous)							
Treatment ×	-0.000	0.000	-0.001	-0.001	0.001	-0.002	-0.000
(Prior - 104.8)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment	0.115**	0.046	0.005	0.019	0.049	0.013	0.042
	(0.054)	(0.054)	(0.055)	(0.055)	(0.052)	(0.054)	(0.035)
p-value [T + T × x P-104.8]	0.04	0.41	0.94	0.75	0.35	0.83	0.24
Observations	4150	4149	4149	4149	4148	4147	4147
Panel B: Underestimators							
Treatment ×	0.088	-0.027	0.115	0.280**	0.023	0.238*	0.138
(Prior < 104.8)	(0.134)	(0.134)	(0.128)	(0.130)	(0.126)	(0.132)	(0.084)
Treatment	0.041	0.068	-0.066	-0.197	0.002	-0.142	-0.071
	(0.131)	(0.131)	(0.124)	(0.126)	(0.122)	(0.128)	(0.082)
p-value [T + T × x P<104.8]	0.00	0.19	0.12	0.01	0.42	0.00	0.00
Observations	4150	4149	4149	4149	4148	4147	4147
Panel C: Low est. Debt-to-GDP							
Treatment ×	0.052	-0.023	0.115	0.206**	0.020	0.112	0.073
(Prior < 90)	(0.101)	(0.105)	(0.101)	(0.104)	(0.099)	(0.102)	(0.065)
Treatment	0.078	0.063	-0.061	-0.118	0.006	-0.019	-0.007
	(0.096)	(0.101)	(0.096)	(0.099)	(0.093)	(0.096)	(0.062)
p-value [T + T × x P<90]	0.00	0.21	0.09	0.00	0.42	0.00	0.00
Observations	4150	4149	4149	4149	4148	4147	4147
Panel D: Below Median est. Debt-to-GDP							
Treatment ×	-0.031	-0.022	-0.004	-0.083	-0.104*	0.001	-0.053
(Prior < 60)	(0.060)	(0.061)	(0.061)	(0.061)	(0.061)	(0.061)	(0.036)
Treatment	0.138***	0.052	0.043	0.106***	0.073*	0.081*	0.083***
	(0.042)	(0.042)	(0.042)	(0.041)	(0.042)	(0.044)	(0.026)
p-value [T + T × x P<60]	0.01	0.50	0.38	0.61	0.48	0.06	0.24
Observations	4150	4149	4149	4149	4148	4147	4147

Notes: The outcome variables in columns 1-6 are z-scored using the mean and standard deviation in the control group. The outcome in column 7 is a weighted average of those in columns 2-6, following the weighting procedure described in Anderson (2008). Each column shows one estimation and every estimation is done on the pooled sample. All specifications control for the corresponding dimension of heterogeneity in its non-interacted form, (linear) prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.33: General views: Heterogeneity by Demographics

	There is too much debt	Gov. should reduce debt	Index
Panel A: Republican			
Treatment ×	-0.086	-0.011	-0.037
Republican	(0.081)	(0.079)	(0.058)
Treatment	0.344***	0.249***	0.229***
	(0.049)	(0.047)	(0.035)
p-value [T + T × x Rep.]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel B: Education			
Treatment ×	0.049	0.046	0.037
Bach. +	(0.079)	(0.076)	(0.056)
Treatment	0.295***	0.227***	0.201***
	(0.051)	(0.049)	(0.036)
p-value [T + T × x Bachelor+]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel C: Income			
Treatment ×	0.064	-0.010	0.023
High Income	(0.078)	(0.076)	(0.056)
Treatment	0.280***	0.251***	0.204***
	(0.060)	(0.057)	(0.042)
p-value [T + T × x HI]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel D: Age			
Treatment ×	-0.007***	-0.006**	-0.005***
Age	(0.002)	(0.002)	(0.002)
Treatment	0.601***	0.486***	0.421***
	(0.110)	(0.106)	(0.077)
p-value [T + T × Age]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel E: Children			
Treatment ×	0.150*	0.142*	0.111**
Parent	(0.079)	(0.076)	(0.056)
Treatment	0.229***	0.165***	0.153***
	(0.060)	(0.058)	(0.043)
p-value [T + T × Parent]	0.00	0.00	0.00
Observations	4151	4151	4151
Panel F: Gender			
Treatment ×	-0.052	-0.140*	-0.074
Male	(0.078)	(0.075)	(0.055)
Treatment	0.339***	0.312***	0.251***
	(0.052)	(0.050)	(0.036)
p-value [T + T × x Male]	0.00	0.00	0.00
Observations	4151	4151	4151

Notes: The outcome variables in columns 1 and 2 are z-scored using the mean and standard deviation in the control group. The outcome in column 3 is a weighted average of those in columns 1-2, following the weighting procedure described in Anderson (2008). Each column shows one estimation and every estimation is done on the pooled sample. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.34: Views on spending: Heterogeneity by Demographics

	Reduce Overall Sp.	Reduce Defense Sp.	Reduce Infrastr. Sp.	Reduce Schooling Sp.	Reduce Social Sec. Sp.	Reduce Social Ins. Sp.	Reduce Health Sp.	Reduce Environm. Sp.	Index
Panel A: Republican									
Treatment ×	-0.099*	-0.025	-0.070	-0.005	-0.068	-0.063	0.021	-0.074	-0.047
Republican	(0.060)	(0.059)	(0.063)	(0.061)	(0.063)	(0.061)	(0.062)	(0.058)	(0.035)
Treatment	0.189***	0.100***	0.140***	0.117***	0.149***	0.097***	0.099***	0.128***	0.117***
	(0.038)	(0.037)	(0.039)	(0.034)	(0.036)	(0.035)	(0.035)	(0.034)	(0.020)
p-value [T + T × x Rep.]	0.05	0.10	0.17	0.03	0.12	0.50	0.02	0.25	0.02
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel B: Education									
Treatment ×	-0.005	-0.037	-0.009	-0.045	-0.027	0.028	-0.016	-0.001	-0.013
Bach. +	(0.061)	(0.060)	(0.062)	(0.058)	(0.060)	(0.058)	(0.058)	(0.055)	(0.034)
Treatment	0.156***	0.106***	0.119***	0.134***	0.136***	0.063*	0.113***	0.103***	0.106***
	(0.037)	(0.037)	(0.040)	(0.037)	(0.038)	(0.037)	(0.038)	(0.036)	(0.021)
p-value [T + T × x Bachelor+]	0.00	0.14	0.02	0.05	0.02	0.04	0.03	0.02	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel C: Income									
Treatment ×	0.011	0.082	-0.027	-0.038	-0.080	0.001	-0.053	0.018	0.018
High Income	(0.059)	(0.058)	(0.062)	(0.057)	(0.059)	(0.057)	(0.058)	(0.055)	(0.033)
Treatment	0.148***	0.047	0.129***	0.137***	0.170***	0.074*	0.136***	0.092**	0.091***
	(0.044)	(0.043)	(0.046)	(0.042)	(0.043)	(0.043)	(0.042)	(0.042)	(0.024)
p-value [T + T × x HI]	0.00	0.00	0.01	0.01	0.03	0.05	0.03	0.00	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel D: Age									
Treatment ×	-0.001	0.001	0.001	0.001	0.001	0.001	-0.000	-0.003	0.000
Age	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Treatment	0.187**	0.037	0.076	0.087	0.087	0.013	0.126	0.206***	0.097**
	(0.083)	(0.081)	(0.084)	(0.079)	(0.083)	(0.079)	(0.079)	(0.076)	(0.045)
p-value [T + T × Age]	0.02	0.63	0.35	0.26	0.28	0.85	0.10	0.01	0.03
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel E: Children									
Treatment ×	-0.007	-0.039	0.053	0.056	-0.041	-0.016	-0.019	-0.060	-0.019
Parent	(0.059)	(0.058)	(0.062)	(0.057)	(0.059)	(0.057)	(0.057)	(0.055)	(0.033)
Treatment	0.158***	0.114***	0.085*	0.085**	0.148***	0.082*	0.116***	0.136***	0.112***
	(0.045)	(0.044)	(0.046)	(0.043)	(0.044)	(0.043)	(0.042)	(0.041)	(0.024)
p-value [T + T × Parent]	0.00	0.06	0.00	0.00	0.01	0.08	0.01	0.04	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150
Panel F: Gender									
Treatment ×	0.001	0.010	-0.102*	-0.087	-0.110*	-0.055	-0.141**	-0.058	-0.056*
Male	(0.059)	(0.058)	(0.062)	(0.057)	(0.059)	(0.057)	(0.057)	(0.055)	(0.033)
Treatment	0.153***	0.087**	0.164***	0.158***	0.177***	0.099***	0.172***	0.130***	0.128***
	(0.038)	(0.039)	(0.042)	(0.037)	(0.039)	(0.038)	(0.039)	(0.037)	(0.022)
p-value [T + T × x Male]	0.00	0.02	0.18	0.10	0.13	0.29	0.46	0.08	0.00
Observations	4150	4150	4150	4150	4150	4150	4150	4150	4150

Notes: The outcome variables in column 1-8 are z-scored using the mean and standard deviation in the control group. The outcome in column 9 is a weighted average of those in columns 2-8, following the weighting procedure described in Anderson (2008). Each column shows one estimation and every estimation is done on the pooled sample. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.35: Views on taxes: Heterogeneity by Demographics

	Increase overall amount of taxes	Increase income tax: top 10	Increase income tax: next 40	Increase income tax: bottom 50	Introduce wealth tax	Increase estate tax	Tax Index
Panel A: Republican							
Treatment ×	0.014	-0.078	0.040	0.014	0.029	0.007	-0.003
Republican	(0.061)	(0.064)	(0.064)	(0.063)	(0.065)	(0.063)	(0.039)
Treatment	0.115***	0.066*	0.025	0.061*	0.009	0.075**	0.057***
	(0.037)	(0.034)	(0.036)	(0.037)	(0.034)	(0.037)	(0.020)
p-value [T + T × x Rep.]	0.01	0.82	0.22	0.14	0.49	0.11	0.11
Observations	4150	4149	4149	4149	4148	4147	4147
Panel B: Education							
Treatment ×	0.056	0.036	0.004	0.002	-0.035	-0.095	-0.002
Bach. +	(0.060)	(0.059)	(0.061)	(0.061)	(0.060)	(0.061)	(0.036)
Treatment	0.098***	0.024	0.037	0.066*	0.034	0.115***	0.056**
	(0.038)	(0.039)	(0.039)	(0.039)	(0.037)	(0.039)	(0.022)
p-value [T + T × x Bachelor+]	0.00	0.18	0.37	0.15	0.98	0.67	0.06
Observations	4150	4149	4149	4149	4148	4147	4147
Panel C: Income							
Treatment ×	0.049	0.055	-0.033	-0.042	-0.055	0.004	-0.001
High Income	(0.059)	(0.059)	(0.060)	(0.061)	(0.059)	(0.060)	(0.035)
Treatment	0.095**	0.009	0.057	0.091**	0.051	0.076*	0.057**
	(0.044)	(0.046)	(0.045)	(0.046)	(0.043)	(0.045)	(0.026)
p-value [T + T × x HI]	0.00	0.09	0.55	0.22	0.90	0.05	0.02
Observations	4150	4149	4149	4149	4148	4147	4147
Panel D: Age							
Treatment ×	-0.000	-0.003*	0.001	0.001	0.002	0.002	0.000
Age	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Treatment	0.133	0.177**	0.005	0.005	-0.057	0.002	0.045
	(0.081)	(0.081)	(0.082)	(0.084)	(0.082)	(0.082)	(0.049)
p-value [T + T × Age]	0.09	0.03	0.94	0.94	0.49	0.96	0.34
Observations	4150	4149	4149	4149	4148	4147	4147
Panel E: Children							
Treatment ×	0.031	0.101*	0.127**	-0.005	0.093	0.046	0.064*
Parent	(0.059)	(0.059)	(0.060)	(0.061)	(0.059)	(0.060)	(0.035)
Treatment	0.103**	-0.019	-0.033	0.068	-0.034	0.050	0.019
	(0.044)	(0.044)	(0.044)	(0.045)	(0.044)	(0.044)	(0.026)
p-value [T + T × Parent]	0.00	0.04	0.02	0.12	0.13	0.02	0.00
Observations	4150	4149	4149	4149	4148	4147	4147
Panel F: Gender							
Treatment ×	0.068	-0.002	-0.010	-0.023	0.007	0.022	0.011
Male	(0.059)	(0.059)	(0.060)	(0.061)	(0.059)	(0.060)	(0.036)
Treatment	0.088**	0.038	0.043	0.076*	0.015	0.066	0.050**
	(0.040)	(0.039)	(0.040)	(0.040)	(0.040)	(0.040)	(0.022)
p-value [T + T × x Male]	0.00	0.41	0.46	0.24	0.61	0.05	0.03
Observations	4150	4149	4149	4149	4148	4147	4147

Notes: The outcome variables in columns 1-6 are z-scored using the mean and standard deviation in the control group. The outcome in column 7 is a weighted average of those in columns 2-6, following the weighting procedure described in Anderson (2008). Each column shows one estimation and every estimation is done on the pooled sample. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.36: Heterogeneity by all dimensions: Horserace

	Debt Reduction Index	Reduce Overall Sp.	Increase overall Amount of taxes
Treatment ×	-0.072	-0.160	-0.059
(Prior - 104.8) / 100	(0.100)	(0.108)	(0.103)
Treatment ×	-0.040	-0.103*	0.006
Republican	(0.059)	(0.061)	(0.062)
Treatment ×	0.048	-0.005	0.051
High Education	(0.058)	(0.064)	(0.064)
Treatment ×	0.008	0.024	0.031
High Income	(0.058)	(0.062)	(0.063)
Treatment ×	-0.006***	-0.000	-0.001
Age	(0.002)	(0.002)	(0.002)
Treatment ×	0.162***	0.005	0.031
Children	(0.059)	(0.062)	(0.062)
Treatment	0.332***	0.120	0.065
	(0.094)	(0.103)	(0.102)
Observations	4151	4150	4150

Notes: The outcome variables in column 1 is a weighted average of two (z-scored) variables, following the weighting procedure described in Anderson (2008). The outcome variables in columns 2 and 3 are z-scored using the mean and standard deviation in the control group. All estimations are done on the pooled sample. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent, the respondent's number of children (top-coded at five), and a set of dummies for Experiments 1 to 4. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

B.7 Main results based on reweighted MTurk sample

Table A.37: Views on government debt (Table 1) reweighted

	There is too much debt	Gov. should reduce debt	Index
Panel A: Rep. Sample			
Treatment	0.346***	0.293***	0.309***
	(0.063)	(0.062)	(0.060)
Observations	812	812	812
Panel B: MTurk Sample			
Treatment	0.303***	0.221***	0.262***
	(0.063)	(0.066)	(0.061)
Observations	802	802	802
Panel C: MTurk (age x gender)-weights			
Treatment	0.260***	0.219**	0.239***
	(0.089)	(0.104)	(0.092)
Observations	802	802	802
Panel D: MTurk (age x gender x inc)-weights			
Treatment	0.269***	0.229**	0.249***
	(0.095)	(0.105)	(0.096)
Observations	790	790	790

Notes: The outcome variables in column 1-2 are z-scored using the mean and standard deviation in the control group. The outcome in column 3 is a weighted average of those in columns 1-2, following the weighting procedure described in Anderson (2008). Panel A shows estimations based on Experiment 1 (rep. sample, 1970 anchor), Panel B shows estimations on experiment 2 (MTurk sample, 1970 anchor) Panel C shows estimations on the MTurk sample reweighed by six age categories and gender, Panel D shows estimations on the MTurk sample reweighed by six age categories, gender and two household income groups (above and below $50,000 annual income). The probability weights for Panels C and D were constructed based on the ACS 2016. Note that for Panel D women aged 65 or older in the high income group were dropped from the ACS because the corresponding cell in our MTurk sample is empty. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct. level.

Table A.38: Attitudes towards government spending (Table 2) reweighted

	Reduce Overall Sp.	Reduce Defense Sp.	Reduce Infrastr. Sp.	Reduce Schooling Sp.	Reduce Social Sec. Sp.	Reduce Social Ins. Sp.	Reduce Health Sp.	Reduce Environm. Sp.	Index
Panel A: Rep. Sample									
Treatment	0.184***	0.045	0.217***	0.219***	0.212***	0.137**	0.191***	0.164**	0.171***
	(0.068)	(0.066)	(0.070)	(0.069)	(0.068)	(0.066)	(0.067)	(0.065)	(0.039)
Observations	811	811	811	811	811	811	811	811	811
Panel B: MTurk Sample									
Treatment	0.160**	0.000	0.058	0.125**	0.104*	0.154**	0.096	0.092	0.065**
	(0.063)	(0.063)	(0.068)	(0.064)	(0.063)	(0.063)	(0.063)	(0.060)	(0.031)
Observations	802	802	802	802	802	802	802	802	802
Panel C: MTurk (age x gender)-weights									
Treatment	0.227**	0.070	0.181*	0.070	0.174**	0.196*	0.134	0.072	0.113*
	(0.088)	(0.096)	(0.108)	(0.087)	(0.086)	(0.107)	(0.091)	(0.090)	(0.060)
Observations	802	802	802	802	802	802	802	802	802
Panel D: MTurk (age x gender x inc)-weights									
Treatment	0.179**	0.039	0.094	0.077	0.148*	0.162*	0.149*	0.053	0.080
	(0.085)	(0.091)	(0.094)	(0.085)	(0.079)	(0.095)	(0.088)	(0.081)	(0.052)
Observations	790	790	790	790	790	790	790	790	790

Notes: The outcome variables in column 1-8 are z-scored using the mean and standard deviation in the control group. The outcome in column 9 is a weighted average of those in columns 2-8, following the weighting procedure described in Anderson (2008).Panel A shows estimations based on Experiment 1 (rep. sample, 1970 anchor), Panel B shows estimations on Experiment 2 (MTurk sample, 1970 anchor) Panel C shows estimations on the MTurk sample reweighed by six age categories and gender, Panel D shows estimations on the MTurk sample reweighed by six age categories, gender and two household income groups (above and below $50,000 annual income). The probability weights for Panels C and D were constructed based on the ACS 2016. Note that for Panel D women aged 65 or older in the high income group were dropped from the ACS because the corresponding cell in our MTurk sample is empty. All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct.

B.8 Other

Table A.39: Beliefs about political bias

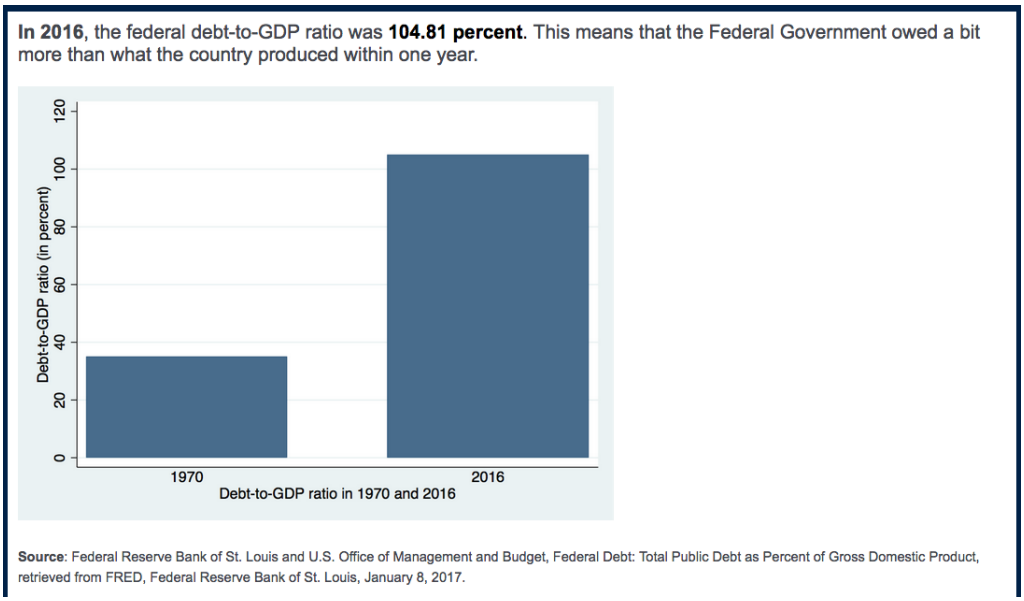
	Left-wing Biased	Right-wing Biased	No political Bias
Panel A: Pooled (B,C,E,F)			
Treatment	0.016**	0.002	-0.018*
	(0.007)	(0.008)	(0.010)
Adjusted p-value	[0.073]	[0.382]	[0.079]
Observations	4152	4152	4152
Panel B: Exp. 1 (1970, rep. sample)			
Treatment	-0.017	0.020	-0.003
	(0.013)	(0.016)	(0.021)
Adjusted p-value	[0.486]	[0.486]	[0.486]
Observations	813	813	813
Panel C: Exp. 2 (1970, MTurk sample)			
Treatment	0.019	0.007	-0.026
	(0.017)	(0.023)	(0.028)
Adjusted p-value	[1.000]	[1.000]	[1.000]
Observations	802	802	802
Panel D: Exp 3 (Hist. mean, rep. sample)			
Treatment	0.023*	-0.002	-0.021
	(0.012)	(0.011)	(0.016)
Adjusted p-value	[0.244]	[0.403]	[0.244]
Observations	1488	1488	1488
Panel E: Exp 4 (OECD median, rep. sample)			
Treatment	0.029*	-0.017	-0.012
	(0.015)	(0.014)	(0.019)
Adjusted p-value	[0.183]	[0.283]	[0.495]
Observations	1049	1049	1049

Notes: The outcome variables are binary, taking on value zero or one. Panel A shows estimations on the pooled sample (excluding the MTurk Follow-up), Panel B is based on Experiment 1 (rep. sample, 1970 anchor), Panel C is based on Experiment 2 (MTurk sample, 1970 anchor), Panel D is based on Experiment 3 (rep. sample, historical mean anchor) and Panel E is based on Experiment 4 (rep. sample, OECD median anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also include dummies for Experiments 1, 2, 3 and 4. Robust standard errors are in parentheses. False-discovery rate adjusted p-values are in brackets. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct.

C Additional figures

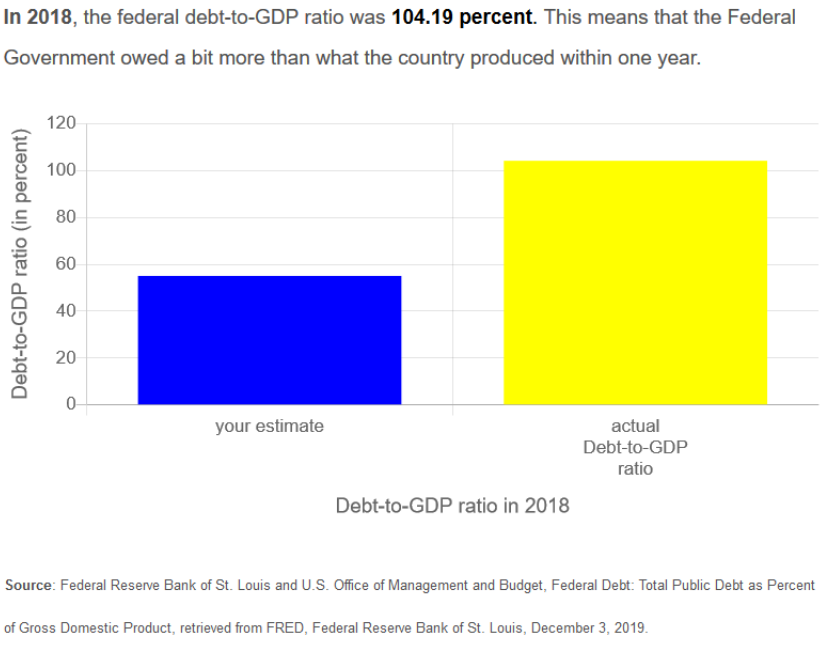
C.1 Treatment screen

Figure A.1: Treatment screen Experiments 1 and 2



Notes: This is the treatment screen shown to respondents in the treatment group of Experiment 1 and 2.

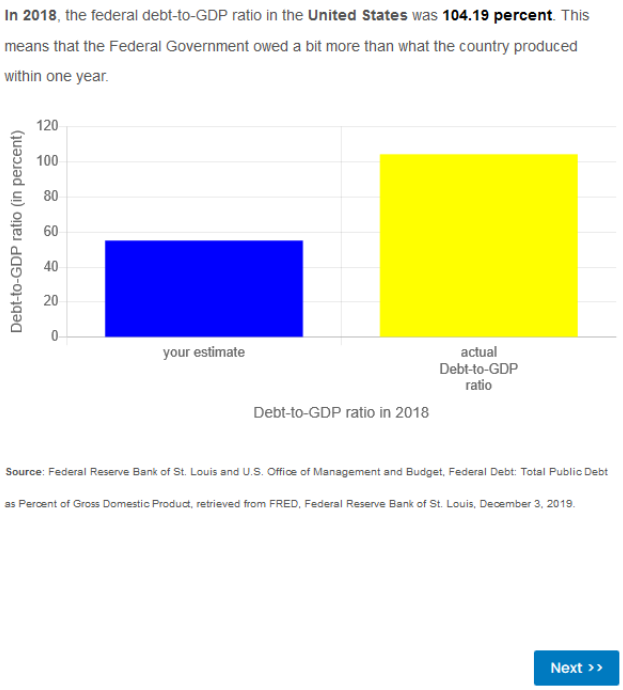
Figure A.2: Treatment screen Experiment 3



Notes: This is the treatment screen shown to respondents in the treatment group of Experiment 3. The left bar dynamically shows the respondents own prior belief (in the exemplary case displayed here 55 percent)

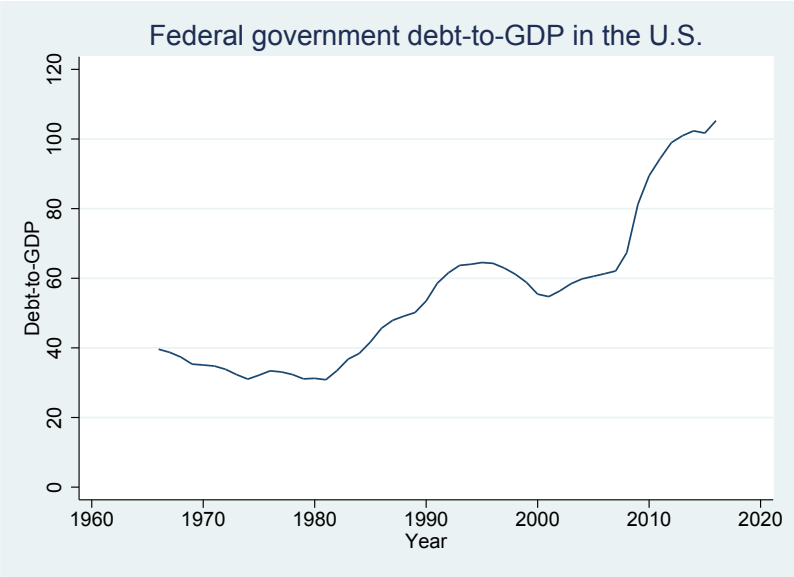
C.2 Beliefs about the debt-to-GDP ratio

Figure A.3: Treatment screen Experiment 4



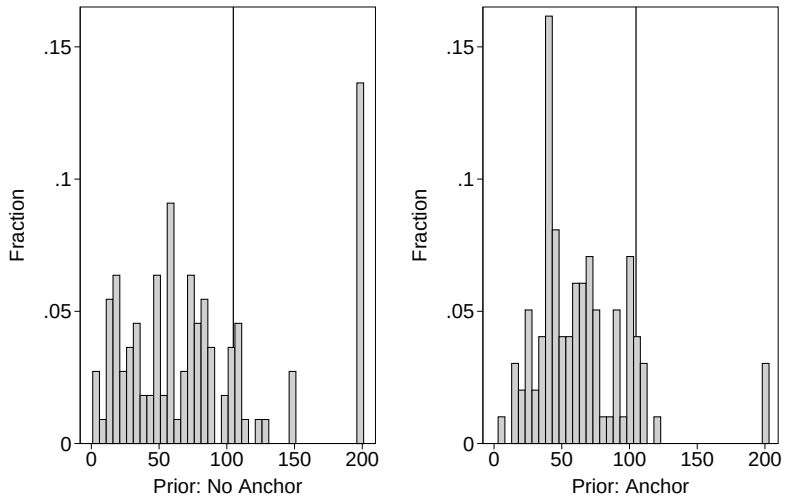
Notes: This is the treatment screen shown to respondents in the treatment group of Experiment 4. The left bar dynamically shows the respondent's own prior belief (in the exemplary case displayed here 55 percent).

Figure A.4: Evolution of debt-to-GDP ratio



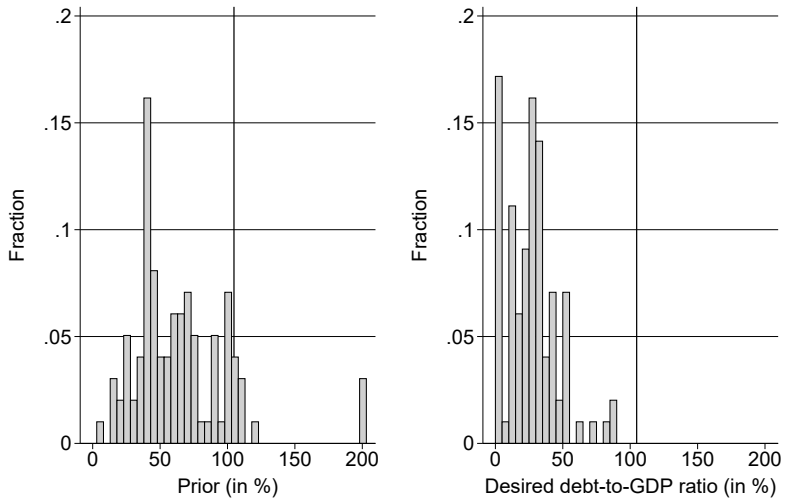
Notes: In this figure we display the evolution of the federal government debt-to-GDP ratio in the US from 1965 until 2016. Source: FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/GFDEGDQ188S, July 24, 2017.

Figure A.5: Beliefs about the debt-to-GDP ratio with and without anchor (pilot experiment)



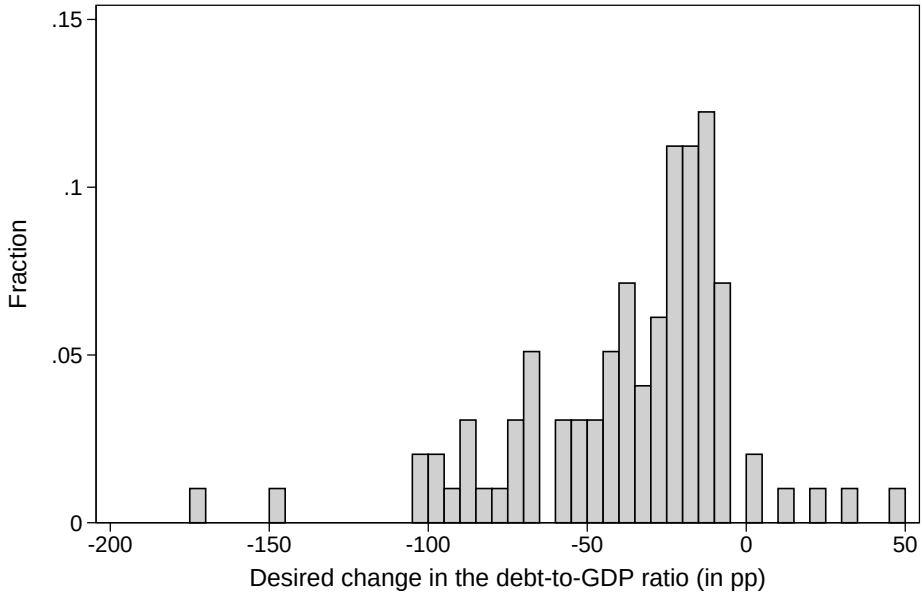
Notes: This figure describes the distribution of beliefs about the debt-to-GDP ratio (in %) in a pilot experiment with 200 respondents on MTurk. On the left we display people's beliefs when they are not given an anchor. On the right we display their beliefs when they are given a historical anchor. The estimates are winsorized at a debt-to-GDP ratio of 200 percent. The median estimate is 61.5 percent (56.23 percent) without (with) historical anchor. The difference in medians is statistically insignificant (based on a nonparametric two-sample test on the equality of medians).

Figure A.6: Beliefs about the debt-to-GDP ratio and desired debt-to-GDP ratio (pilot experiment)



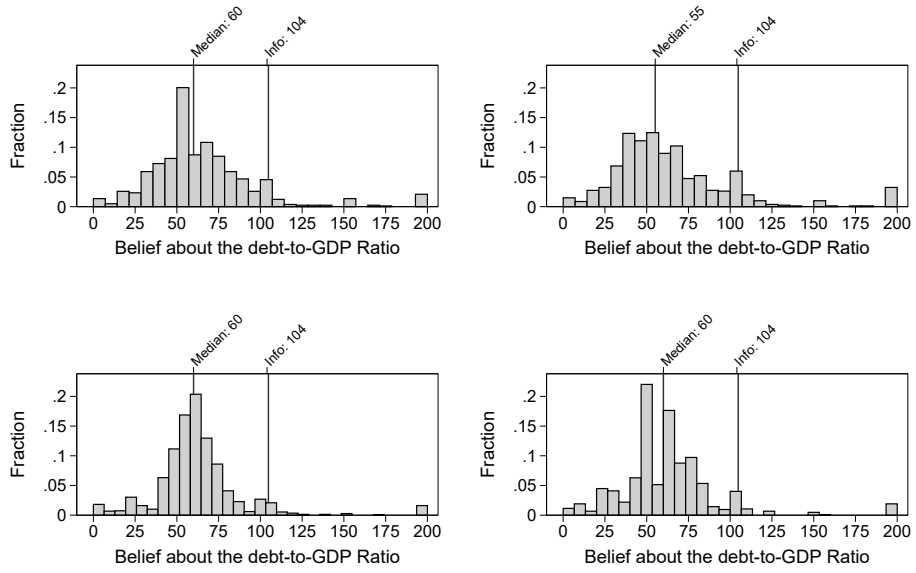
Notes: This figure describes the distribution of beliefs about the debt-to-GDP ratio in a pilot experiment with 200 respondents on MTurk. The figure is based on data from half of the respondents which we provided with a historical anchor. On the left we display people's beliefs about the debt-to-GDP ratio. On the right we show people's desired debt-to-GDP ratio. The estimated and desired debt-to-GDP ratios are winsorized at 200 percent. The median estimate is 56.23 percent and the median desired debt-to-GDP ratio is 25 percent.

Figure A.7: Desired change in debt-to-GDP ratio (pilot experiment)



Notes: This figure describes the distribution of desired changes in the debt-to-GDP ratio in a pilot experiment with 200 respondents on MTurk. Desired change on the horizontal axis is calculated as the individually preferred debt-to-GDP ratio minus the perceived debt-to-GDP ratio (both in %, winzorized at 200). The figure is based on data from the half of the respondents that we provided with a historical anchor.

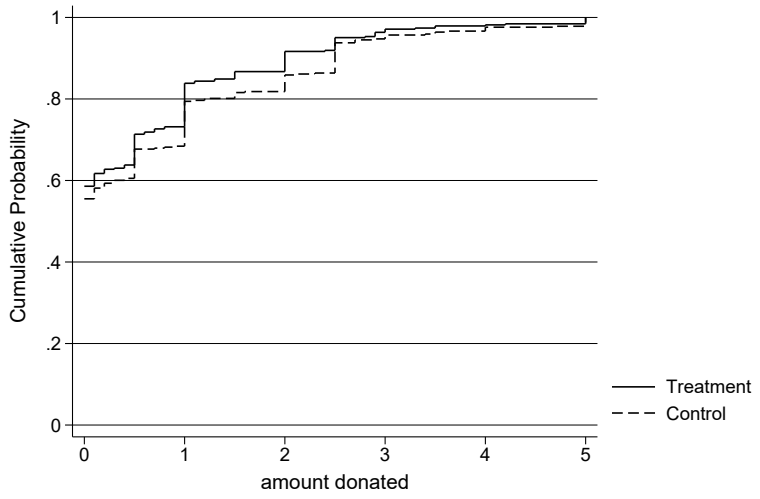
Figure A.8: Beliefs about the debt-to-GDP ratio, separately by experiment



Notes: This figure displays people's beliefs about the debt-to-GDP ratio separately for Experiments 1 (1970 anchor, representative sample), 2 (1970 anchor, MTurk sample), 3 (Historical mean anchor, representative sample) and 4 (OECD median anchor, representative sample). The estimates are winsorized at a debt-to-GDP ratio of 200 percent.

C.3 Additional evidence on actual political behavior

Figure A.9: Donation decisions in the treatment group and in the control group (CDFs)



Notes: This figure shows, separately for the treatment and control group, the cumulative distribution of the respondents' donation decisions. It is based on Experiment 2 (MTurk sample, 1970 anchor).

D Sample composition effects MTurk sample vs representative sample

In this appendix section, we discuss differences between results in Experiment 1, which was based on a representative online sample, and Experiment 2, which was based on an MTurk sample, and which were both run in early 2017 and have the same design (including the 1970 anchor). We also demonstrate the robustness of the MTurk results to a reweighting exercise.

Whereas the estimated treatment effect on people's general attitudes towards government debt are statistically similar in Experiment 1 and Experiment 2 (Table 1, Panels B and C), the treatment effects on attitudes towards government spending in specific categories are somewhat larger in Experiment 1 than in Experiment 2. This is true especially for attitudes towards government spending on infrastructure, schooling, social security and health (compare Panels B and C of Table 2). One potential reason for the slight difference in magnitudes is the different sample composition, i.e. our MTurk sample employed in Experiment 2 is younger, has a higher share of males and fewer individuals with very high income than the general population and our representative sample employed in Experiment 1. As shown in Table A.34, males have a significantly lower elasticity of demand for government spending to the information, which could be driving some of the reduction in size of the treatment effects in the male-dominated MTurk sample.

In a reweighting exercise we explore whether the difference in the magnitude of the estimated treatment effect may be due to the described difference in sample composition. Table A.38 replicates our main results for attitudes towards government spending based on the representative sample (Panel A) and the MTurk sample (Panel B). Panel C shows the MTurk results including probability weights to adjust for the sample differences by age and gender. The results should be interpreted cautiously because in some demographic cells very few underrepresented

individuals receive a very high weight[4], resulting in more noisily measured treatment effects in general. In addition, Solon et al. (2015) recommend a cautious interpretation of weighted regression results particularly in cases where treatment effects are heterogeneous across (demographic) cells. That said, the reweighted MTurk-based results on attitudes towards government spending approach the representative results in magnitude, suggesting that the initial difference in estimated treatment effects may indeed be the result of the different sample composition.

In Panel D we go one step further and extend the reweighting to income. We distinguish between household incomes of less than and of more than $50,000, which is the closest match of our income categories to a median split in the population. Even with this crude measure of income the additional sample split brings us to the limit in terms of the number of cells our sample allows for.[5] In general, however, the results based on the more detailed reweighting are similar to our preferred choice of weights in Panel C.

For completeness, in Table A.37 we apply the same set of weights in our estimation of the treatment effect on people's general attitudes towards government debt, even though the treatment effect on these attitudes is already statistically similar in the representative and the MTurk sample in the absence of any reweighting (See Panel A compared to B). The reweighting in Panels C and D leaves the treatment effect on people's views on whether the government should reduce debt (column 2) unchanged, and leads to a somewhat smaller treatment effect on perceptions of whether government debt is too high (column 1). However, the effect sizes remain statistically indistinguishable between the two samples. Overall, the described reweighting exercise suggests that the minor differences in the treatment effects on attitudes towards government spending across the two samples are the result of differences in sample composition.

E Additional results

E.1 Effects of information on support for tax- vs. debt-financed spending program

We also ask our respondents about their support for an infrastructure program and randomly assign whether this program is tax-financed or debt-financed. This allows us to identify whether people's beliefs about government debt affect their support for new spending programs depending on the proposed mode of financing.

To analyze whether our information treatment has differential effects depending on whether a proposed spending program is tax-financed or debt-financed, we create the dummy variable $Debt_i$, which takes value one for participants who are asked about support for a debt-financed infrastructure program and value zero for participants who are asked about support for a tax-financed program. We estimate the following specification:

$$y_i = \pi_0 + \pi_1 Treatment_i \times Debt_i + \pi_2 Treatment_i + \pi_3 Debt_i + \Pi^T \mathbf{X_i} + \varepsilon_i$$

The coefficients π_1 and π_2 capture effects of our treatment on support for the program that potentially differ depending on the mode of financing. The coefficient π_3 captures whether people in the control group differentially support tax-financed and debt-financed infrastructure investments.

[4]For instance, 1.8% of the MTurk sample represent 19% of the population aged 65 or older.

[5]Note that even with only two income categories, we end up with no females above 65 in the high income category. To accommodate the missing cell, we drop females above 65 also from the ACS and create probability weights based on the remaining population. Note that the smaller sample size in Panel D of Table A.38, however, is due to the fact that 12 individuals in the MTurk sample did not report their income.

Table A.40 shows that people are more likely to support a government infrastructure investment program if it is financed by a temporary tax increase rather than by issuing new debt. We find no evidence that learning about the actual debt-to-GDP ratio affects people's support for this investment program – irrespective of the mode of financing.

This result differs from our previous finding that learning about the debt-to-GDP ratio decreases people's demand for government spending. We believe that this could be the case as (i) we have less variation available in the measure of people's support for the infrastructure program, (ii) we have less statistical power and (iii) the framing of the question on infrastructure spending is different.

Table A.40: Debt vs tax-based financing of an infrastructure investment program

	Support Infr. Inv. Program		
	Pooled Sample	Rep. Sample	MTurk Sample
Debt-financed × Treatment	-0.011	0.035	-0.083
	(0.097)	(0.139)	(0.137)
Treatment	0.055	0.118	0.011
	(0.069)	(0.101)	(0.097)
Debt-financed	-0.266***	-0.341***	-0.186*
	(0.069)	(0.096)	(0.099)
Observations	1611	809	802

Notes: The outcome variables are z-scored using the mean and standard deviation in the control group. Column 1 pools Experiments 1 and 2, column 2 shows the estimation based on Experiment 1 (representative sample, 1970 anchor) and column 3 shows the estimation based on Experiment 2 (MTurk sample, 1970 anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also control for whether the respondent is part of the representative sample or the MTurk sample. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct.

Table A.41: Debt vs. tax-based financing of an infrastructure investment program: Heterogeneity in control group

	Support Infr. Inv. Program		
	Pooled Sample	Rep. Sample	MTurk Sample
Debt- vs. Tax-based Financing			
Debt-financed × Debt-to-GDP	-0.394**	-0.451*	-0.406
	(0.188)	(0.267)	(0.255)
Debt-financed × Republican	-0.074	-0.038	-0.000
	(0.148)	(0.198)	(0.223)
Debt-financed × High Education	-0.069	0.011	-0.113
	(0.145)	(0.207)	(0.206)
Debt-financed × High Income	-0.026	-0.148	0.055
	(0.147)	(0.206)	(0.210)
Debt-financed × Age	0.002	0.004	0.010
	(0.004)	(0.006)	(0.009)
Debt-financed × Children	-0.057	0.144	-0.335
	(0.146)	(0.203)	(0.212)
Debt-financed	-0.374*	-0.652*	-0.413
	(0.214)	(0.335)	(0.322)
Debt-to-GDP	0.134	0.221	0.021
	(0.141)	(0.209)	(0.170)
Republican	-0.260**	-0.085	-0.531***
	(0.105)	(0.142)	(0.156)
High Education	0.066	-0.048	0.149
	(0.105)	(0.151)	(0.145)
High Income	0.177	0.156	0.268
	(0.136)	(0.192)	(0.198)
Age	-0.003	-0.004	-0.005
	(0.004)	(0.005)	(0.006)
Children	0.028	0.026	0.079
	(0.105)	(0.148)	(0.155)
Observations	811	427	384

Notes: The outcome variables are z-scored using the mean and standard deviation in the control group. Column 1 pools Experiments 1 and 2, column 2 shows the estimation based on Experiment 1 (representative sample, 1970 anchor) and column 3 shows the estimation based on Experiment 2 (MTurk sample, 1970 anchor). All specifications control for prior beliefs about the debt-to-GDP ratio (winsorized at 200 percent), age, gender, a dummy for whether the respondent has at least a bachelor degree, the log of total household income, dummies for full-time employment, part-time employment, unemployment, retirement, full-time education and other employment status, the respondent's trust in official US government statistics, dummies for being a Republican or an Independent as well as the respondent's number of children (top-coded at five). The estimations on the pooled sample also control for whether the respondent is part of the representative sample or the MTurk sample. Robust standard errors are in parentheses. * denotes significance at the 10 pct., ** at the 5 pct., and *** at the 1 pct.