Do Americans Want to Tax Wealth? Evidence from Online Surveys

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Abstract

A vast theoretical literature in public finance has studied the desirability of capital taxation. This discussion largely ignores the political feasibility of taxing wealth. We provide, to our knowledge, the first investigation of individuals' preferences over jointly taxing income *and* wealth. We provide subjects with a set of hypothetical individuals' incomes and wealth and elicit subjects' preferred (absolute) tax bill for each individual. Our method allows us to unobtrusively map both income earned and accumulated wealth into desired tax levels. Our regression results yield roughly linear desired tax rates on income of about 14 percent. Respondents' suggested tax rates indicate positive desired wealth taxation. When we distinguish between sources of wealth we find that, in line with recent theoretical arguments, subjects' implied tax rate on wealth is three percent when the source of wealth is inheritance, far higher than the 0.8 percent rate when wealth is from savings. Textual analysis of respondents' justifications for their tax rates imply limited concern for the elasticity of tax bases with respect to net-of-tax rates.

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1 Introduction

Wealth taxes are levied on the stock of private assets such as real estate, cash holdings, and financial assets (e.g., stocks and bonds). While the idea of taxing assets is not a new one, the rise of wealth-to-income ratios around the world (Piketty, 2014) and increased wealth inequality in the United States (Saez and Zucman, 2016; Smith *et al.*, 2019) has led to increased discussion of wealth taxation among both academics and policymakers. Recent academic research, in particular, has focused on wealth tax experiences in other countries, using identifying variation in wealth tax rates and bases to quantify the behavioral responses to wealth taxes in countries such as Denmark (Jakobsen *et al.*, 2018), Sweden (Seim, 2017), Colombia (Londoño-Vélez and Ávila, 2018), and Switzerland (Brülhart *et al.*, 2017). In a widely debated proposal, Saez and Zucman (2019) summarize the recent evidence in advocating for the creation of a progressive wealth tax in the U.S., which, as they observe, has been proposed by prominent candidates for the 2020 Democratic presidential nomination.[1]

The questions of incidence and implementation of a wealth tax are distinct from questions of *political feasibility*, i.e., whether the practical political economy of tax setting would allow for wealth taxation. Putting aside legal impediments, behavioral responses, and the practical challenges of implementation, there is the separate issue of whether a wealth tax is even desired by the electorate. And if it is, what are the wealth tax parameters that a responsive legislator would aim to translate into policy? Do citizens understand the difference between taxing stocks and taxing flows? And if they do, do they consider stocks of wealth acquired from saving as normatively different from those acquired via inheritance?[2]

We provide, to our knowledge, the first investigation into individuals' preferences toward *jointly* taxing (net) wealth and income, via surveys on Amazon's Mechanical Turk (MTurk) and by adding questions to the (more representative) *Understanding America Study* administered by the University of Southern California.[3] Each subject in our study is confronted with scenarios describing a hypothetical individual's income *and* wealth. For each scenario, the subject then chooses a tax bill for the individual to pay. By asking for *absolute* levels

[1] See Brookings Institute discussions by Mankiw and Kopczuk attached to Saez and Zucman (2019) as well as the column by Larry Summers and Natasha Sarin and associated Twitter debate archived here `https://twitter.com/LHSummers/status/1113991280026103813` (last accessed September 20, 2019). Both Elizabeth Warren and Bernie Sanders have advocated creation of a wealth tax.

[2] The latter is of particular relevance for proposals to increase the estate tax, which has been suggested as a means of financing the "Medicare for All" policy endorsed by some Democratic Party candidates for the 2020 election. See `https://www.sanders.senate.gov/download/options-to-finance-medicare-for-all?inline=file`, last accessed September 20, 2019.

[3] Unless otherwise specified, "wealth" refers to "net wealth" throughout this paper.

of taxation in response to a hypothetical individual's (multi-dimensional, in our case two-dimensional) financial situation, we believe our approach is less likely to lead subjects to use misplaced heuristics (for example, to choose current levels or to confuse marginal and average rates). We also argue that, by asking for desired absolute tax levels rather than rates on income and wealth *per se*, our methodology is unobtrusive—the implied $T(income, wealth)$ function, which we trace out from individual responses, may be a complicated nonlinear function that would be much more costly to elicit, for example, by asking for separate tax rates on a large set of income and wealth brackets (at the same time). Finally, past work has shown that this methodology produces lower and less progressive tax schedules than asking for preferences as rates, which means it is a demanding test of the claim that Americans prefer a non-zero tax on wealth.[4]

Our empirical findings are as follows: First, subjects' tax recommendations over income versus wealth are, roughly speaking, "sensible." Respondents seem to understand intuitively the difference between a stock and a flow and choose implied wealth tax rates that are typically an order of magnitude smaller than those on income. Second, their chosen tax bills imply a linear tax rate on income of approximately 13–15 percent, in line with past work, another sign that our respondents appear to be roughly representative in their views and to have taken the task seriously.

Third, and of greater interest, subjects' choices imply positive rates of wealth taxation. When we restrict the relationship of the tax bill and wealth to be linear, the implied average tax rate on wealth is about 1.2 percent in our baseline estimate. In follow-up sessions, we tell subjects the *source* of the hypothetical wealth. In one treatment they are told it is from saving past income, while in another treatment they are told it is from a bequest from a deceased relative. Preferred taxes on wealth from savings are 0.8 percent, versus over three percent on wealth from inheritance.

Finally, we examine the reasoning provided by our subjects when asked to justify (in their own words) their chosen tax levels, and in particular how their explanations compare to concerns raised in the optimal tax literature. In Mirrlees (1971), the optimal income tax rate is inversely proportional to the elasticity of the tax base with respect to the net-of-tax rate. Similarly, the classic contributions of Atkinson and Stiglitz (1976), Judd (1985), and Chamley (1986) argue that the tax on capital income should be zero because of the costs

[4]As McCaffery and Baron (2006) show, desired income tax rates differ when elicited in absolute versus percentage terms—subjects tend to choose higher taxes when asked to give a percentage as opposed to an absolute tax bill, and this difference widens for higher incomes. As such, our methodology is less likely to exhibit *progressive* tax schedules than the more traditional method of asking subjects to give preferred rates and will likely lead to an underestimate of desired tax rates more generally. We return to discuss this limitation later in the paper.

resulting from behavioral responses of taxed wealth holders.[5] Our subjects do not, however, express concerns for such behavioral responses (e.g., that higher labor income taxes would discourage work, or that higher taxes on wealth would reduce savings or induce capital flight). Simplicity of the tax *schedule* (e.g., a flat tax) is attractive to many. Also, "double taxation" is often noted as an objection to taxing wealth, with respondents saying it was "already taxed" at the time it was earned. These considerations are quite removed from the tradeoffs that economists weigh in the classic optimal tax framework.

We view our main contribution as two-fold. First, to the best of our knowledge we are among the first to directly elicit preferences for wealth taxation from prospective voters.[6] While there are no immediate payoff consequences for survey respondents, the sensible estimates we obtain on income taxation suggest that subjects exert effort in providing responses. While objections to wealth taxation on theoretical grounds or owing to legal or logistical impediments (which we discuss in the conclusion) may still stand, our findings indicate that there appears to be support among the electorate for such policies.

The credibility of our estimates on desired wealth taxation is bolstered by our methodology, which we view as our second contribution. Since we elicit subjects' preferred tax rates through their (absolute) tax choices over a number of hypothetical income/wealth pairs we avoid, for example, leading subjects to gravitate toward responses that reflect current tax rules. With sufficient data, this methodology could be extended across many tax-relevant characteristics (for example, consumption, real estate holdings, and age) to elicit the full tax schedule preferred by respondents. The disadvantage, as we have noted above, is that respondents are typically unaccustomed to thinking in terms of absolute tax bills, and based on past work our methodology will tend to give *lower* and *less progressive* rates than when choices are framed as percentages. While bias in any direction is not ideal, we note that this bias pushes against finding our key result: that Americans prefer a positive tax on wealth.

The remainder of the paper proceeds as follows. Section 2 outlines our experimental design. Section 3 describes our data collection procedures and provides summary statistics on our resulting sample of subjects. Section 4 describes results from the baseline experiment, in which we do not specify to subjects the source of the wealth values they are asked to

[5]More recently, these classic results in optimal capital income taxation have been challenged by, among others, Piketty and Saez (2013), Diamond and Saez (2011), Kopczuk (2013), Farhi and Werning (2010), Guvenen *et al.* (2019), and Straub and Werning (2014).

[6]See, for example, McCaffery and Baron (2006), Singhal (2008), and Kuziemko *et al.* (2015) for attempts at quantifying *income* tax preferences. More commonly, researchers have explored the determinants of redistributive preferences using responses to attitudinal questions on whether there should be, for example, more or less equal incomes in society (see, for example, Alesina and Ferrara (2005) and Ashok *et al.* (forthcoming) and citations therein). We discuss how our estimates compare to these in Section 4.

consider, and then Section 5 shares results from the surveys that compare responses for wealth accumulated via saving past income versus wealth gained via a bequest. Section C uses our results, past estimates of relevant elasticities, and recent models of optimal capital taxation to calculate the implied social welfare weights our subjects' place on individuals with varying levels of wealth. Section 6 concludes and offers suggestions for future work.

2 Experimental Design

We developed our survey experiment with two main goals in mind. First, we wanted to be as unobtrusive as possible, allowing subjects to consider both income and wealth levels when choosing their desired tax but not asking them explicitly how much they wanted to tax income versus wealth. We worried that asking for specific rates on income and wealth would prime them, perhaps toward submitting the current tax rate on income or, more worrisome in our context, presuming that there *should* be a non-zero rate on wealth. Second, we wanted to gather the most information on individuals' preferred tax schedule with minimal cost to our subjects. Asking for absolute tax bills under varying levels of income and wealth allows us to trace out the implied schedule of tax rates on both bases. We worried that the most obvious alternative—asking subjects to explicitly set the rate *and* bracket structure—would be tedious.

In each experiment, subjects were asked how much hypothetical individuals should pay in taxes, based on their income and wealth levels. In the first two survey dates (both in 2014), subjects were provided the following definitions:

> *Wealth* is the total amount of assets an individual owns minus any debt. Examples of assets include money in savings or retirement accounts, stocks, and the value of real estate owned; examples of debt include remaining mortgages, credit card balances, and student loans.
>
> *Income* is the amount of money an individual earns in a year. Examples of income include salary from employment, interest on savings accounts, and stock dividends.

Subjects were then asked to consider a hypothetical individual with a certain amount of income and wealth. These values were randomized within and across subjects (so, subjects do not all see the same sequence of wealth and income values). Specifically, subjects were confronted with a sequence of ten questions that all had the following form (note that the underlining appears in the original):

> Consider a person who, at the end of 20XX, had $X in wealth. His 20XX income was $Y. How much should this person pay in taxes for the year?

The "20XX" value is set to the previous year (e.g., was equal to 2013 for surveys conducted in 2014). Subjects were asked to type in the amount. The field into which they typed was formatted so that only numeric values could be entered. If a subject typed more than three digits, a comma automatically appeared, to help subjects see exactly the amount entered. The comma was not pre-populated, so as to avoid priming subjects that they "should" enter a value of at least a thousand. Subjects answer ten iterations of this question. Interested readers can take the survey themselves at the following link: `https://az1.qualtrics.com/jfe/preview/SV_cOqUTFMhLulW3dP`.[7]

In the surveys we fielded in 2015 and later, subjects were randomized into a "savings" and "inheritance" treatment. For those who were randomized into the "inheritance" treatment, questions took the form:

> Consider a person who, at the end of 20XX, had $X in wealth, accumulated mostly from inheritance received from a deceased relative. His 20XX income was $Y. How much should this person pay in taxes for the year?

For those who were randomized into the "savings" treatment, questions took the form:

> Consider a person who, at the end of 20XX, had $X in wealth, accumulated mostly by saving his past earnings. His 20XX income was $Y. How much should this person pay in taxes for the year?

Subjects answered seven iterations of each of these questions. We collect fewer iterations for each question, because they then went on to answer seven iterations of whichever version they did not initially encounter (i.e., the "reverse experiment," savings questions for those randomized to encounter the inheritance questions first, and vice versa). We selected the wealth levels presented to subjects to be below the estate tax thresholds. In comparing tax preferences on wealth from savings versus inheritance, we focus on the between-subject

[7] As we only allowed numerical entries, respondents are not able to enter negative values (which, in full disclosure, we had not anticipated when originally designing the survey), implicitly disallowing transfers. However, not a single respondent complained about this restriction in the open-ended responses. There was no mention of the EITC; the only subsidy mentioned, noted by one subject, was for food. The one relevant mention of "negative" was a respondent who wrote that: "[o]ne's total wealth should not factor in since people that have negative wealth due to student loan debts etc do not get a credit." Future work may wish to allow for negative taxation, but given the responses of our subjects it appears this constraint was rarely binding.

variation driven by initial randomization, though we also show that results hold when we instead use the within-person variation that also uses data from the reverse experiment.

In order to test robustness, we vary slightly (by survey date) the distributions from which income and wealth values were randomly drawn. In early rounds, wealth values were drawn at random from \$50,000, \$100,000, \$200,000, \$500,000, \$1,000,000, and \$2,000,000; income values were drawn from \$13,000, \$27,000, \$50,000, \$86,000, and \$210,000. While the wealth values were chosen in order to capture salient levels of wealth, the income values were chosen to roughly match the tenth, twentieth, fiftieth, seventy-fifth and ninety-fifth percentiles in the U.S. income distribution.

To "fill out" the distribution, in our first November 2015 wave, we added two new wealth values, \$300,000 and \$750,000. Finally, in late November 2015 and December 2015, we "jittered" both the wealth and income values to ensure we were not picking up "round number" effects from, for example, very high tax rates on wealth values of \$1,000,000. In this wave, wealth and income figures were generated by (a) drawing a value at random from the same distribution as earlier experiments; (b) adding or subtracting 5 percent (with equal probability) of the parameter value, rounded to the nearest thousand. So, for example, \$100,000 would be 'jittered' to either \$95,000 or \$105,000, and \$86,000 would be jittered to \$82,000 and \$90,000. In a separate wave in December of 2015, we sampled from the joint distribution of income and wealth in the 2013 Survey of Consumer Finance (SCF). As such, in this survey, wealth and income were *not* drawn independently, as they were in all the others. In 2018 and 2019, we performed additional variants, which we describe in Section 5.4.

Following the tax scenarios, subjects were asked whether they believe the government should redistribute from the rich to the poor (the wording of this question is taken from the General Social Survey), the importance of luck in life's outcomes, whom they supported in the most recent presidential election, as well as basic socio-demographic data, such as gender, household income, age and marital status. We also asked respondents if they felt the survey was biased. Finally, we gave respondents the chance to respond to open-ended questions on whether the survey was confusing and also invited them to share, in words, how they made their tax decisions.[8]

[8]The exact wording of this question is: "Please describe how you decided on the level of tax payments for the hypothetical individuals in the survey."

3 Data

For the most part, we recruited and compensated our subjects through Amazon's Mechanical Turk (MTurk) market place, but redirected them to surveys that we built with Qualtrics' online survey software. The exception is a round of data collection ($N = 306$) using the *Understanding America Study* (UAS) run by the Center for Economic and Social Research at the University of Southern California. This platform is more representative of the U.S. adult population, but also substantially more expensive. The experiments were conducted over several waves spread out from November 2014 through July 2019, with the UAS survey fielded at the latter part of this period. The full set of dates, along with details on the differences in survey design across waves, are included in Appendix Tables A.1 and A.2.

3.1 Data collection procedures

Amazon Mechanical Turk (MTurk) allows "requesters" to post "human intelligence tasks" (HITs) and associated levels of compensation for "workers" to complete. Over the past few years, social scientists have increasingly used MTurk to perform experiments and collect survey data (see Kuziemko *et al.*, 2015 and papers cited therein for a review). We registered as a requester and posted a HIT with the following description: "The survey asks your opinion on a variety of topics. There are no right or wrong answers." We tried to use a neutral description that would limit selection bias while also giving workers an honest depiction of the task. As we are interested in respondents' *preferences*, we also emphasized that there were "no right or wrong answers," to limit social-desirability bias to the extent possible. This wording also aimed to convey that subjects' answers should reflect their *opinion*, not their guess of how much individuals actually pay in taxes. Indeed, when we ask individuals how they decided on the tax bill in our open-ended question, none suggested that they were trying to give the actual amount the individual would pay under the current tax schedule.

Each MTurk worker logs in with a unique ID. Because we collected data across multiple dates, we drop any worker who had taken a previous survey with the same ID, to ensure that we gather a fresh set of participants each time (though we will show that our estimated tax preferences change little when we do not drop repeat-takers from the sample).[9]

[9]If workers maintain multiple MTurk IDs then some individuals remaining in our main sample may have participated in a previous session. Outside of surveys (which appear to make up a very small fraction of all tasks), in which case requesters may want unique workers, there is little incentive for workers to create multiple IDs. It is not possible to rule out the possibility that some workers may have done so, however, and thus could have passed through our screening process.

To limit heterogeneity of the sample, we collect all data on workdays during daylight hours on the East Coast of the United States. Given our focus on American tax policy, we limited the survey's availability to those with U.S. billing addresses; we also asked respondents to confirm their residency in the United States.

The data pass basic reality checks (for example, subjects that report having supported Mitt Romney in 2012 tend to be white and male, mirroring patterns observed in polling data). Almost all respondents went on to answer open-ended "feedback" questions. In particular, we asked whether any part of the survey had been confusing, and the vast majority wrote that no part had been unclear to them.

In Appendix B, we provide greater detail on compensation, and our efforts to limit "bots" (algorithms that masquerade as human subjects). In particular, the need to screen for bots increases after the summer of 2018 (many social scientists termed this period an "MTurk quality crisis").[10] In general, we set a compensation level that is high by MTurk standards to limit selection into our sample.

3.2 Data sample and randomization check

Table 1 provides detail on the respondents who completed our surveys. We pool all MTurk workers from 2014 to 2015 (col. 1) as well as those from 2018 (col. 2), when we performed additional robustness checks on our 2014-2015 baseline results. Col. 3 describes the *Understanding America Study* sample; we defer discussion of these data until Section 5.4. Col. 4 provides summary statistics from the 2014-2018 General Social Survey (GSS), which is representative of the U.S. adult population.

Consistent with past work using MTurk, we find that male, white, college-educated and young subjects are over-represented in our sample. Despite being more likely to have a college degree, they are nonetheless poorer than the average household (presumably in part due to being younger). Interestingly, the demographics of our 2014-2015 and our 2018 MTurk samples are very similar.

While the MTurk samples differ demographically from the representative American, the political and redistributive views of our samples match those in the GSS very closely. In both our 2014-2015 MTurk data and the 2014-2016 GSS, just under two-thirds of respondents preferred Barack Obama in the 2012 election (included in this share for both our sample and the GSS are those who did not vote but nonetheless report having preferred Obama to Romney or other choices at the time). A similar share (54.5 and 49.8) supported Hillary

[10]See Kennedy *et al.* (2018), Dennis *et al.* (2018), as well as our more detailed discussion in Appendix B.

Clinton in our 2018 MTurk data and the 2018 GSS, respectively.

To gauge redistributive preferences, we asked our MTurk sample a question taken verbatim from the GSS:

> Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Here is a card with a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel?

In both samples, we flip this question so that it is increasing in the redistributive position. The average responses in the GSS fall between our two MTurk samples, and all are very close to each other (on the slightly more redistributive side of the neutral answer of 4.0). Just under a third of our MTurk respondents say that "luck and help from others" is more important than hard work in determining success.[11]

In general, the redistribution question and presidential election questions left us encouraged by how representative our sample appears to be in terms of political ideology. Additionally, we will show that our results are robust to weighting along the dimensions in which our sample and the GSS sample differ the most (gender, age and attainment of a college degree) and compare them to data collected in 2019 using the more representative UAS sample.

Table 1 also shows the average wealth and income values that our subjects are asked to consider in the tax scenarios. The average income value our respondents evaluate is roughly $83,000 and the average wealth value is roughly $648,000 (though the median is only $44,000, as both in our experiment and in reality, the actual wealth distribution is extremely right-skewed). The average income and wealth values are comparable to average family income ($87,200) and net wealth ($534,600) in 2013, though our survey question was vague on whether the income and wealth of the hypothetical individual was personal or household.[12]

In the Appendix, we show that our randomization rendered treatment status uncorrelated with subjects' observable characteristics. Appendix Tables A.3 and A.4 show that in our

[11]While we took this question from the GSS as well, we inadvertently did not include a "both" option as the GSS did, so we cannot make a direct comparison.

[12]Statistics on U.S. averages can be found here: `http://www.federalreserve.gov/pubs/bulletin/2014/pdf/scf14.pdf` (last accessed September 20, 2019).

baseline surveys (in which the source of wealth is unspecified), the demographic and other characteristics of our subjects have no ability to predict the levels of income and wealth they evaluated. Appendix Table A.5 and Appendix Table A.6 show that this experimental balance also holds in the surveys in which wealth sources are specified. Finally, Appendix Table A.7 shows that subjects randomized to see the savings questions first appear no different on observables than those who initially saw inheritance questions.

4 Baseline results when the source of wealth is not specified

We begin with an analysis of preferred tax schedules using the surveys in which the source of the hypothetical individuals' wealth was unspecified. These results will serve as a baseline for examining how preferred tax rates are affected by the source of wealth.

4.1 Graphical evidence

Before estimating the regression equations, we show the relationship between the preferred total tax bill and hypothetical income and wealth values graphically. Figure 1 depicts vintiles of the chosen tax bills as a function of vintiles of the wealth values. We residualize these values by survey date and income value and then add back in the means of the tax bill and the hypothetical wealth values. We fit a quadratic line through the scatter plot to allow the data to display non-linear relationships. In fact, the graph shows that the relationship between chosen tax bill and wealth levels is similar throughout the wealth distribution. Note that the fitted line does not hit (0,0), nor would we expect it to do so. As $T = T(income, wealth)$, for low values of $wealth$ the average income is still considerable (around \$100,000) and thus not surprisingly respondents on average choose non-zero tax bills in such scenarios.

Figure 2 performs the same analysis for the hypothetical income values, this time absorbing the wealth values. The relationship is again quite linear, with a steeper slope, consistent with a higher rate on flows than stocks. The intercept on this graph is notably lower than that in Figure 1. As we will see in the regression analysis, respondents put far greater weight on income than on wealth in determining the total annual tax bill, and thus for individuals with very low income, respondents indeed choose very low tax bills, so the implied intercept is much closer to zero.[13]

[13]When we estimate regressions of the tax bill on wealth and income, we do not constrain the intercept to be zero. However, the implied intercept in the tax schedule when we assume a linear functional form (the implied amount owed when income and wealth are set to zero) cannot be distinguished from zero at standard levels of significance. This result holds as well for the data in the next section of the paper when we specify whether wealth comes from inheritance or own savings.

4.2 Regression results

Given the evidence of linear relationships in the graphs above, in our initial specification we assume that the tax-income and tax-wealth relationships are linear in levels:

$$Tax_{ij} = \alpha + \beta^W Wealth_{ij} + \beta^I Income_{ij} + \gamma X_{ij} + e_{ij},$$

where i indexes the subject and j the question order, Tax_{ij} is the chosen tax bill, $Wealth_{ij}$ is the wealth level subject i encounters in question j, $Income_{ij}$ is the income level subject i considers in question j, and X_{ij} are additional covariates that vary to probe robustness. The coefficients on the wealth and income levels will be the implied linear tax rates on these two tax bases.

Col. (1) of Table 2 shows results from estimating the above regression, including only survey-date fixed effects as controls. Subjects choose tax bills that yield a 1.2 percent linear tax on wealth and a 15.8 tax on income. This result is precisely estimated and essentially unchanged when we include fixed effects for each of the ten iterations the subject completes and subject-specific fixed effects (cols. 2 and 3, respectively).

Given that our focus is on preferences over wealth taxation, in col. (4) we absorb fixed effects for each of the income values that subjects encounter, essentially treating income as a nuisance variable. Since wealth and income values are chosen independently, it is unsurprising that controlling more flexibly for income has no effect on the wealth coefficient. In col. (5), we drop subjects who completed the survey in less than four minutes (the fifth percentile of survey duration); in col. (6) we drop answers that give a tax bill of zero. Neither of these sample restrictions affect the coefficient of interest.

A common worry in repeated survey experiments is anchoring bias: in later rounds, respondents may be unconsciously influenced by responses given in early rounds.[14] A simple version of anchoring bias (anchoring, in levels, to the first-round response) would drive our coefficients of interest toward zero, as it makes respondents less responsive to the wealth and income values in subsequent vignettes.[15] In col. (7) we simply use the very first observation from each respondent. While the coefficient is somewhat smaller (0.074) it is statistically indistinguishable from our estimate derived from the larger sample.[16]

[14]See Green *et al.* (1998) for evidence of anchoring bias in respondents' valuation of policy (in their case environmental protection) and Beggs and Graddy (2009) for evidence of anchoring bias even in high-stakes settings (art auctions).

[15]Of course, one could imagine more complicated versions of anchoring bias—e.g., anchoring to the *rate* chosen in the first round, which might over or under-state the preferred rate if individual's true (non-anchored) preferred rates are non-linear.

[16]In fact, when we include the first (*income*, *wealth*) pair as an explanatory variable in a regression using data from the subsequent nine questions, coefficients on these variables are small and

In col. (8) we use the GSS to generate weights that correct for our under-representation of women and individuals over age 30, and over-representation of those with a college education (i.e., we weight observations in our sample so that the proportions in the eight cells defined by these three binary variables are the same as in the GSS). Weighting makes almost no difference to the coefficient of interest.

4.3 Reliability of survey answers

There are inherent challenges in interpreting hypothetical survey results; our experiment is no exception. We ask unfamiliar and potentially challenging questions to subjects who have no direct monetary incentive to exert cognitive effort. Some respondents may have low levels of numeracy. Given that the U.S. does not have, strictly speaking, a wealth tax, respondents may have been especially unfamiliar with the concepts we seek to study (though most likely have some familiarity with the property tax). While we do not believe we can ever fully dispel these worries, we provide some evidence that respondents in fact understood our questions and took the survey seriously.

First, we find very few "reversals" in our data. For any pair of scenarios in which the income and wealth levels are *both* higher in one scenario than in the other, we define a "reversal" as an occasion where the subject chooses a larger tax bill in the scenario in which the hypothetical individual is strictly poorer. For the ten scenarios each subject confronts, there are $\binom{10}{2} = 45$ pairs, though not all will be comparable (e.g., within a pair, one could have a higher income level but a lower wealth level than the other). On average, our subjects confront 15 comparable pairs, ranging from zero to 35. We find that fewer than five percent of comparable pairs indicate "reversals" of the form described above. Not surprisingly, we find that reversals are more common among those who finish the survey in an unrealistically short amount of time.[17]

While the small number of reversals suggest that respondents understood the questions, we also directly asked subjects at the survey's conclusion to tell us if any part of it was confusing. While almost all respondents answer this question (usually with some variant of "no," "nope") less than four percent tell us they felt confused at any point.[18]

insignificant (see Appendix Tables B.3 and B.4).

[17]If we all subjects that ever exhibit a reversal (about a sixth of our sample), our point estimates are unchanged. Another check of data quality is prevalence of round numbers. We check that our results are robust to dropping subjects who give a majority of their responses as multiples of $10,000 (as with reversals, these subjects are more likely to finish the survey in unrealistically short amounts of time). See Appendix Tables B.3 and B.4 for both of these results .

[18]We include in this number those who describe not so much being confused but just challenged by the task ("just the estimating of taxes—a calculation chart would be helpful") and those who

Third, social-desirability bias (see, e.g., Bernardi, 2006, Dalton and Ortegren, 2011) is a concern in our context, though some work suggests that web-based surveys may be less prone to it than tradition in-person interviews (Kreuter *et al.*, 2008). We did our best to remind readers that *any* answer they gave was valid by stressing in the introduction to the task that it was an *opinion* survey with no "right or wrong answers." Nonetheless, we might still worry that respondents attempt to provide the responses that they perceive researchers want. For this reason, at the end of our survey, we ask respondents whether they perceived it to be biased in any manner. The vast majority (85 percent) indicate that they perceived no bias, eleven percent a left-wing bias, four percent a right-wing bias, and less than one percent a bias of some other type.

Finally, respondents' open-ended answers to how they chose their preferred tax bills suggest that they took the task seriously. After all decisions were made and demographic questions posed, we asked respondents to explain in words their general thinking when choosing the hypothetical tax bills. More than 99 percent of respondents wrote at least something in response to this question, which we take to be a testament to the care they seemed to devote to our survey. Moreover, answers were written in colloquial American English, indicating that our attempts to screen out non-American residents were largely successful. While somewhat subjective, our read of the responses to the open-ended questions indicate that less than one percent of respondents are potential bots (see Appendix B for more detail). We discuss the actual responses in the next subsection.

4.4 Discussion of results

Are the coefficients that we estimate based on respondents' answers "reasonable" in a public-finance sense? To the extent that our methodology unobtrusively tests individuals' general understanding of the difference between income and wealth (that wealth, a stock, would quickly disappear if taxed at the same rate as income, a flow), the results are encouraging. The tax rate on income is more than an order of magnitude larger than that on wealth. While our question ("how much should this person pay in taxes?") abstracts from federal versus state tax,15.8 percent is very close to the average federal plus state income tax rate in the U.S. (the actual value is 15.5 percent).[19]

Since we are, to our knowledge, the first to estimate wealth tax preferences, we cannot

had temporary problems with the interface ("the first question i couldn't type in a number," or "they [*sic*] way you had to enter the money amounts. it took me a few questions just to figure that out").

[19]The most recent year available is 2012 for the federal (12.5) and 2008 for the state (3.0). See `http://users.nber.org/~taxsim/allyup/ally.html` (last accessed September 20, 2019).

compare our wealth tax estimates to past work. We can, however, compare our implied preferred average income tax rates to prior estimates. The most directly comparable paper is McCaffery and Baron (2006), in which researchers estimate income tax preference by asking subjects to give an absolute tax bill (as we do) for different values of income. They find that the implied preferred average rate is 16.8 percent on those making $200,000 and 11.7 percent on those making $50,000, so our point estimate falls in between these values. As we noted earlier, they find that preferred average tax rates are substantially higher when rates (rather than absolute amounts) are directly solicited. Their subjects give a preferred average rate of 24.6 percent on those making $200,000 and 13.0 percent for those making $50,000. Other recent work on income tax preferences tend to ask for preferred *rates* directly and also focus on top earners. These estimates are thus unsurprisingly higher than what we find (e.g., Kuziemko *et al.* (2015) and Charité *et al.* (2015) find that subjects choose average tax rates of around thirty percent on, respectively, those in the top one percent and those making $250,000 a year).

Finally, to gain a better understanding of respondents' tax preferences, we analyze their answers to the open-ended question: "Please describe how you decided on the level of tax payments for the hypothetical individuals in the survey." In Table 3 we report the most common two-word (bigram) and three-word (trigram) phrases that appear in these open-ended responses.[20] Simplicity, in the sense of a single bracket, appears attractive to many respondents, with "flat tax" and "everyone pay 10 [percent]" appearing frequently. We also note that respondents do not raise efficiency concerns (e.g., that high taxes would make individuals work or save less), a point we return to later.

In summary, we take away from our baseline results that our elicitation procedure produces reasonable differences between preferred levels of income and wealth taxation, with the preferred rate on wealth being much lower than the preferred rate on income, and the preferred rate on income matching well with past work. In the next section we focus on how preferred taxes differ when subjects are told that wealth comes from savings versus bequests.

5 Results when the source of wealth is specified

We now turn to data from surveys in which we specify the source of the hypothetical individual's wealth. We first analyze data collected in 2015. At the end of this section we turn to analyses of data collected in 2018 and 2019, when we tested variants of our original surveys,

[20] We use the "tm" package in R to process the text of the responses to this question. We convert all text to lowercase, strip punctuation and common English stopwords, and stem words with a Porter stemmer. We then take all 2-word (bigram) and 3-word (trigram) sequences in the remaining text, and calculate frequencies across subject responses.

and replicated the survey experiment on a different, more representative survey vendor.

Before describing our 2015 "inheritance versus savings" results, we note that these data pass the same "quality checks" as the 2014 data described in the previous section. We find that only five percent of comparable pairs of $(income, wealth)$ values lead to reversals (in the sense that a subject chooses a lower tax bill in a scenario A versus B when the individual is strictly richer in A). A somewhat higher share, eight percent, tell us that they were confused at some point in the survey, though the increase relative to the baseline survey is driven by a handful of respondents who appeared to have trouble with the interface. Similar to the baseline surveys, 83 percent of respondents felt the surveys were unbiased, with eleven, five and one percent indicating they perceived bias in the left-wing, right-wing, or "other" direction, respectively. Over 98 of respondents go on to explain in the open-ended question how they made their decisions and our reading of these answers suggest less than one percent were "bots" (again, see Appendix B for more detail).

5.1 Assuming a linear functional form

The results in the previous section showed that our estimated wealth coefficients were robust to a variety of specification checks. For brevity, we will present a more limited set of specifications for the results in this section. Our preferred specification, which we present first, controls for question-order and subject fixed effects (as in col. 3 of Table 2). Col. (1) of Table 4 is identical to col. (3) of Table 2 except that we include only observations for which (a) wealth is specified as coming from savings *and* (b) the subject was randomized into seeing the savings questions first (that is, we do not use the reverse experiment).

The coefficient on income in col. (1) in Table 4 is slightly smaller than that in col. (3) of Table 2, 13.2 versus 15.7 percent. Of greater interest, the coefficient on wealth in Table 4 is over a third smaller (though still precisely estimated and highly significant) than its analogue in Table 2: 0.766 versus 1.17 percent. Subjects in these surveys appear to reward wealth from savings with a lower implied tax rate relative to surveys in which the source of wealth is unspecified. Col. (2) is identical to col. (1) except that income is treated as a nuisance variable and fully absorbed; the results remain unchanged.

The next two columns perform the parallel analysis for observations in which wealth was specified as coming from inheritance (and in which subjects were randomized to see these questions first). The coefficients on income are nearly identical to the wealth-from-savings observations. However, the coefficient on wealth is over four times larger, at just over three percent. Interestingly, the implied tax from wealth when the source of wealth was left unspecified (roughly 1.1 percent, as in Table 2) falls between that on savings and that

on inheritance. While few respondents spelled out their assumptions on the source of wealth in the baseline survey, of the five that did, four mention they assume it came from savings of past earnings.[21] As such, it is not surprising that the results on generic wealth are closer to those from savings.

The final columns test whether the large differences in preferred tax rates on wealth from savings versus inheritance can be detected based on within-person variation as well, using the reverse experiment and comparing, for a given respondent, whether higher taxes are chosen for wealth-from-inheritance scenarios. The differences are still significant and in the expected direction, but smaller than those implied by the between-subject identification of this difference. For example, the first four columns imply a difference of about 2.2 percentage points ($3.03 - 0.77$), whereas the difference identified within-person is only 1.3 percentage points.

Further analysis suggests that the smaller within-person estimates result from some anchoring bias on the first set of questions that the respondent encounters. While (as shown in Table 4) respondents choose a tax on savings of 0.766 percent when they encounter these questions first, this figure rises to 1.3 percent when they encounter them *after* the inheritance questions, consistent with subjects being primed to respond with relatively larger tax bills (see Appendix Table A.8). Similarly, while respondents who see the inheritance questions first choose to tax wealth from inheritance at three percent, those who first view the savings questions choose to then tax wealth from inheritance at 1.7 percent. Nonetheless, even those who are "anchored" to give a lower inheritance tax (because they see the savings questions first) give higher inheritance questions than those who are anchored to give a higher savings tax (because they see the inheritance questions first). This type of anchoring bias makes us prefer the between-person estimates, on which we focus for the rest of the paper.

In summary, we find a robust, average difference in respondents' willingness to tax wealth from bequests versus wealth from their own past savings. On the one hand, this result is not surprising, given the large literature from lab experiments showing that subjects acting as social planners are more willing to redistribute endowments gained via luck versus those gained through effort or skill (see, e.g., Cherry *et al.* (2002) and Oxoby and Spraggon (2008)). On the other hand, it is surprising given survey evidence showing that large majorities of Americans are opposed to the estate tax. Consistent with past surveys, a recent Gallup poll showed that 54 percent of Americans favor eliminating the estate tax, relative to 19 percent

[21]For example, one person in the open-ended answers to the baseline surveys wrote: "After the first one, I set it at 10% of income, regardless of wealth, because the wealth should have been taxed in the year it was earned." Another wrote: "People should only be taxed in [*sic*] annual income. They've already been taxed once on the money they earned in the past."

who oppose its elimination.[22] Whereas we do not use the term estate tax, our results in fact imply robust support for taxes on inheritance. In that sense, it echoes results in Kuziemko *et al.* (2015) that Americans' views on an inheritance tax may be sensitive to framing and information.

5.2 Exploring non-linear functional form

Figures 3 and 4 show the shape of the implied tax schedule over wealth separately for the savings and inheritance scenarios (we relegate the analogous figures for income to Appendix Figures A.1 and A.2.) Again, we fit quadratic lines to see whether the data imply a linear or non-linear relationship. The solid gray line is the fitted quadratic line for the full distribution of wealth values and appears linear. Cognizant that our methodology likely understates progressivity for large values of the tax base (relative to asking for tax preference as rates), we also estimate fitted lines in which we exclude wealth values above $2 million (long dashed line) and $1.75 million (short dashed line). For these truncated distributions, some (weak) evidence of progressivity emerges. In Appendix Table A.10, we formally test whether the progressivity depicted in Figure 3 (over wealth from savings) can be statistically distinguished, and find that linear schedules cannot be rejected at conventional levels of significance.

For the wealth-from-inheritance data depicted in Figure 4, the tax bill appears very well-explained as a linear function throughout the wealth distribution. Our respondents appear willing to tax even modest amounts of inherited wealth at the same rate they would tax, say, $2,000,000 in inherited wealth. Finally, in Appendix Table A.11, we explore whether individuals consider the interaction between wealth and income when setting the total tax bill, and similarly find no evidence of any significant or even consistently signed interaction between wealth and income, suggesting that separability of wealth and income in the tax schedule might be warranted.

We may further ask whether respondents' views on taxing the hypothetical individual's wealth differ if the individual's *income* is modest. To that end, in the even-numbered columns of Table A.11, we interact wealth with a dummy variable denoting whether the hypothetical individual's income is below $50,000. While the point estimates are negative in all cases (as one would expect), they are small and insignificant, with all p-values greater than 0.45. Again, the coefficient on the wealth main effect is essentially unaffected across all specifications.

[22] See `http://www.gallup.com/poll/190067/americans-react-presidential-candidates-tax-proposals.aspx`, last accessed September 21, 2019.

We find these results to be somewhat surprising, as standard models in which only consumption enters the utility function would imply that wealth is merely a source of capital income, and is substitutable for labor income in generating consumption. In such cases, a social planner would generally impose a low tax rate on individuals with limited income, regardless of wealth holdings, whereas our subjects continue to impose significant wealth taxes on these individuals. We return to this point in Section C.

5.3 Heterogeneity in tax preferences

How do demographic and political characteristics mediate the relationship between preferred taxation over wealth levels, and, further, is any difference mediated by whether wealth is gained via savings or inheritance? We explore these questions using the following regression specification:

$$Tax_{isj} = \beta Wealth_j \times Inheritance_s \times X_i + \lambda_{ijs} + e_{isj},$$

where Tax_{isj} is subject i's preferred tax on the wealth observed in question j when the source of the wealth is $s \in \{inheritance, savings\}$; $Wealth$ is the amount of wealth being considered in question j, $Inheritance_s$ is a dummy variable denoting whether the source of wealth is from inheritance; X_i is a given set of individual characteristics; and λ_{isj} is a vector of all lower-order terms of the triple interaction term. As usual, we only use observations that come from the first set of questions each subject encounters.

The results are displayed in Table 5 for individual characteristics related to political views. Note that for readability, the coefficients in this table are multiplied by 100,000. In col. (1) we see that all subjects (regardless of whether they supported Obama) prefer higher tax rates on wealth derived from inheritance than own savings (a positive and significant coefficient on $Wealth \times Inheritance$. However, this tendency is significantly stronger among Obama supporters (a positive and significant coefficient on the triple interaction term). Otherwise, Obama supporters appear similar to other subjects. Belief that the government should redistribute income and wealth is associated with higher preferred wealth tax rates more generally, and also—as with Obama supporters—a preference for taxing inherited wealth more than other types of wealth.

In the final column, we include interactions with a dummy variable indicating that the respondent feels luck is more important than effort in determining success. The triple interaction is small and insignificant in this specification, perhaps consistent with these respondents believing that luck also determines *past savings* as well, as would be the case with uninsurable and idiosyncratic rates of return to past savings or luck being integral in determining

past income flows.

Finally, we find no mediating effect for gender, age, own household income, race or parenthood (see Appendix Table A.9).

5.4 Additional data collection for further robustness checks

Perhaps the key concern about MTurk and thus our results so far is that subjects are not fully representative of the U.S. population. To address this concern, we re-run the survey on a different platform. Specifically, we paid to add our questions to the *Understanding America Study* (UAS) at USC. This round of data collection took place in July of 2019, and we present summary statistics in col. (3) of Table 1. Recall that MTurk workers skewed educated, young, male, and low-income, relative to the nationally representative GSS. On all four margins the UAS data are far closer to the GSS, though they are still somewhat more likely to have a college degree (45 percent versus 32 percent in the GSS). The UAS respondents are also more likely to be white than those in the GSS.

Despite the different data vendor and the four years of time that had elapsed, our results (shown in Table 6) are quite similar to their 2015 MTurk analogues (cols. 1–4 of Table 4). Our key results—that subjects choose a positive and significant tax on wealth whether it is from savings or inheritance, but nonetheless a substantially larger one on wealth from inheritance—holds. In fact, the difference in the preferred tax rate on wealth from inheritance versus savings is slightly larger in the UAS data. In the UAS sample, respondents choose lower *income* tax rates when wealth comes from savings than inheritance (12.7 versus 20.1 percent, respectively), though this difference is not significant, nor is either significantly different from their MTurk analogues (13.2 and 13.5, respectively).

A second concern about the surveys from 2014-2015 is that our definition of income includes some sources of capital income. We used this wording because it gives the clearest answer to the question of whether subjects want to add a wealth tax on top of the *current* tax schedule (which does in fact tax some capital income). But this wording makes it more complicated to infer individuals' preferences about taxing capital versus labor *per se*. For example, if some respondents' preferred tax bill does not vary with wealth (and thus the coefficient on wealth is zero), it might mean that they are against taxing wealth on principle. But it might instead mean that they are not against taxing wealth, but simply feel it is sufficiently taxed via the current income tax (which includes in its base some capital income sources and thus indirectly taxes wealth already).

To address these ambiguities, we re-ran the experiment in the Fall of 2018, randomizing between two variants of the income definition given to subjects: the original definition (as

given in Section 2) versus the following: "Labor Income is the amount of money an individual earns from work in a year. Examples include salary from employment, tips, and bonuses." In the "labor income" variant, we also change the description of the hypothetical individuals to the following: "Consider a person who, at the end of 20XX, had $X in wealth, accumulated mostly from inheritance received from a deceased relative. His 20XX labor income was $Y. How much should this person pay in taxes for the year?" Wording for the savings treatment was changed similarly, again specifying income as labor income.

As shown in Appendix Table A.12, the results are very similar whether our income definition includes capital income or whether we explicitly limit it to labor income. Our key results from the original 2015 MTurk data holds under both variants: a positive and significant tax on wealth regardless of source, but a substantially higher tax on wealth from inheritance than from savings. Under both variants, respondents choose an income tax rate of between nine and 16 percent (again, not distinguishable from the MTurk analogues, columns 1–4 in Table 4). The only appreciable difference (marginally significant at the ten percent level) is a lower preferred tax on wealth from savings in the "labor income" variant than in the variant using the original income definition, but this difference is driven by the fact that the tax on savings in the variant that uses the original language is higher than in the 2015 version of the exact same experiment (0.0135 in 2018 versus 0.0077 originally). In general, the results from each of these variants not only look similar to each other, but also look very similar to the results from the 2015 data collection, suggesting that even with four years between surveys, preferences as measured by our MTurk subjects appear to hold steady.

A third concern involves the potential positive framing associated with the savings treatment. Perhaps the large differences in implied tax rates between wealth from inheritance versus wealth from savings is sensitive to our exact wording, in that subjects might be primed by the positive valence of the word "savings." To confirm that subjects are not simply primed by the virtue associated with savings, in November of 2018 we conducted another variant of our survey experiment. For a random half of subjects we change the inheritance treatment wording from "Consider a person who, at the end of 2017, had $X in wealth, accumulated mostly from inheritance received from a deceased relative" to "Consider a person who, at the end of 2017, had $X in wealth, accumulated mostly from inheritance received from the savings of a deceased relative." The other half is provided the original language, without "savings of a" added in front of "deceased relative."

In comparing these two variants, Appendix Table A.13 shows that none of the four coefficients of interest differ appreciably from each other (and these small differences are all statistically insignificant). Thus, even when we emphasize that in both scenarios the wealth

was due to savings (just varying which generation did the saving), our respondents continue to tax wealth from inheritance substantially more than wealth from *own* savings.

5.5 Textual analysis

Returning to the original waves of 2015 data, Table 7 displays the most common bigrams and trigrams in the open-ended answers in the surveys that specify the source of wealth. Interestingly, phrases (e.g., "alreadi [*sic*] tax," "alreadi [*sic*] paid tax") often suggest an aversion to "double taxation," which did not emerge as a key concern in the baseline survey. It appears that specifying the source of wealth reminds individuals that taxes may already have been paid on it (recall that in the savings treatment, respondents are told that wealth comes from saving past *earnings*). As economists focus almost entirely on the elasticity of relevant tax bases to determine efficiency consequences of taxation, "double taxation" is merely an accounting issue, and yet it appears very salient to our respondents.

Bigrams and trigrams allow us to derive some broad patterns from the universe of responses, but obviously subtle meanings are lost. We therefore randomly sampled 100 of these responses for further scrutiny. As a very rough count, approximately 14 percent of respondents stated explicit opposition to a wealth tax (e.g., "how much savings or inheritance should not determine the amount of tax paid. I used their income and took 5% ; the govn't [*sic*] is too big and needs to get out of our business."). Another 12 percent suggested decision rules that did not include wealth, but also did not state explicitly any opposition to the concept (e.g., "10% of all income earned in the year").[23]

The remaining explanations were either too vague to classify (e.g, "I kept taxes low for everyone, as they should be" or "I took mental evaluations and gave a good answer" or "Randonmly [*sic*]") or explicitly supported including wealth in the tax base at least under some circumstances (e.g., "I taxed people with inheritance more because it's not like they had that money before so how much could it harm them?" or "The most important factor was the income to determine the tax amount though in some cases I also took into account a person's wealth").

Notable in both the bigrams and trigrams and our close reading of the random sample of responses was the absence of efficiency concerns (in both the baseline survey in Section 4 and

[23]For the purposes of this definition, we do not code as one if the respondent left the door open for *some* wealth taxation even if for the most part they felt income was more important. For example, "to be honest I don't know how taxes are decided but I was going off their income of that year *more so* [emphasis added]" was not coded as implicitly excluding wealth from their calculation. Similarly, "I really couldn't decide how to factor in wealth so I decided to focus more on income" was not coded as explicitly or implicitly against a wealth tax.

in the current section in which wealth sources are specified). No one argued that taxation would reduce savings or work effort. Those who voiced general opposition to taxes did not rely on efficiency arguments but instead made more moral claims (e.g., it's not "fair" to be taxed twice) or libertarian ones (e.g., the government needs to "get out of our business"). Neither sentiment is easily embodied in familiar social welfare functions, but are suggestive of more complex normative theories that can provide rationales for limited redistribution even in the presence of no incentive effects (Saez and Stantcheva, 2016a; Weinzierl, 2014).

In Appendix C we calibrate an optimal linear wealth tax model, building on Saez and Stantcheva (2016b), to subjects' preferred tax rates under a range of capital supply elasticities, recovering the implied normative weights subjects put on the welfare of wealth-holders (as well as separately for savers and heirs). If the textual evidence is any guide, our subjects seem to be unconcerned about supply responses. The moderate taxes reported by our subjects are then surprisingly low, and could reflect express concern for the welfare of wealth holders. On the other hand, if we take values greater than one as the high-end of the existing elasticity estimates, the taxes on inheritances are high enough to imply *negative* welfare weights on inheritors, reminiscent of experimental ultimatum game outcomes where subjects are willing to bear costs in order to punish unfair divisions of unearned resources (Fehr and Gächter, 2000).

6 Discussion and Conclusion

A recent literature documents the increasing importance of wealth and wealth inequality. Saez and Zucman (2016) find that 20% of American wealth is held by the top 0.1% of owners, a share that has doubled in the last forty years.[24] Piketty (2014) documents a secular increase in wealth-income ratios over the same period. In eras of high wealth inequality and high wealth-income ratios, it is perhaps not surprising for wealth taxes to enter the political debate. We elicit taxes over joint income and wealth holdings using an online survey. We find that Mechanical Turk subjects appear to understand the difference between stocks and flows, choosing wealth tax rates that are an order of magnitude smaller than those on income. Our estimates indicate that on average subjects prefer a 0.8% tax rate on saved wealth, a 3% tax rate on inherited wealth, and a 13-15% percent tax on income. Desired wealth taxes remain at the same rate even at low income levels.

Were they to be implemented, the budget implications of these taxes would be substantial.

[24]Wolff (2014) also finds an increase in wealth inequality since 1962, even in the Survey of Consumer Finance, which is generally viewed as unable to sample very large wealth-holders and thus under-states inequality.

Aggregate net wealth in the United States at the end of 2016 was 93 trillion dollars, and Davies and Shorrocks (2000) estimates that between 35-45% of wealth is inherited. Ignoring supply responses, our subjects' implied tax rates would result in an extra 1.11 trillion dollars in government revenue if no consideration were given to the source of wealth, and between 1.4 and 1.6 trillion dollars if preferred inherited and saved wealth taxes were levied separately. This sum is substantial, well over a quarter of the United States federal government budget.[25]

Our results also suggest that much of the theoretical literature on wealth taxation, with the exception of Piketty and Saez (2013) and Kopczuk (2013), does not capture the intuitive tastes individuals have for taxing wealth. Far from the prescribed zero capital tax or positive subsidy predicted by various models, it appears that respondents have a preference for positive wealth taxation, even for wealth accumulated out of savings and even for low-income individuals. Indeed, some of our calibrations suggest that for plausible wealth bequest elasticities, the implied welfare weight put on inherited wealth would be negative. However, none of our subjects list bequests, or indeed any type of wealth supply response, as their justification for their chosen tax rate. As they express no concern about supply response and yet support relatively modest tax rates, our respondents appear to have limited redistributive preferences.

As noted in the introduction, separate from the proscriptions of economic theory or the extent of popular support, questions about the practical and legal feasibility of wealth taxes remain. Legal scholars have debated the constitutionality of a wealth tax in the United States (see Bankman and Shaviro (2014)). Assessing the value of different forms of wealth may be difficult, particularly given sophisticated tax-sheltering services or tax havens, for complex financial contracts or assets that are not transacted very often (although see Posner and Weyl (2016) for both why low transaction rates imply optimal positive wealth taxes as well as how technological changes may erode this problem in the future). Wealth taxes may also be inferior to capital income taxation when rates of return vary widely and unpredictably, as they would exempt transitory changes in returns on wealth. But the infeasibility of wealth taxes in the United States should not be taken for granted: while wealth taxes have only recently re-entered the political discourse, they were more commonly discussed in the nineteenth century (Einhorn, 2008). While feasibility issues arose then as well, thick financial markets, cross-border information sharing, and modern digital records may improve enforcement. Furthermore, given our findings, the wealth tax may be a policy option Americans are be willing to entertain again. Additional research on the costs, benefits,

[25]Recall from Section 4 that their preferred income tax rates are slightly above the average state plus federal income tax rates, so if anything our subjects appear open to a larger total tax share of GDP.

and political economy of wealth taxation may become of increasing policy relevance.

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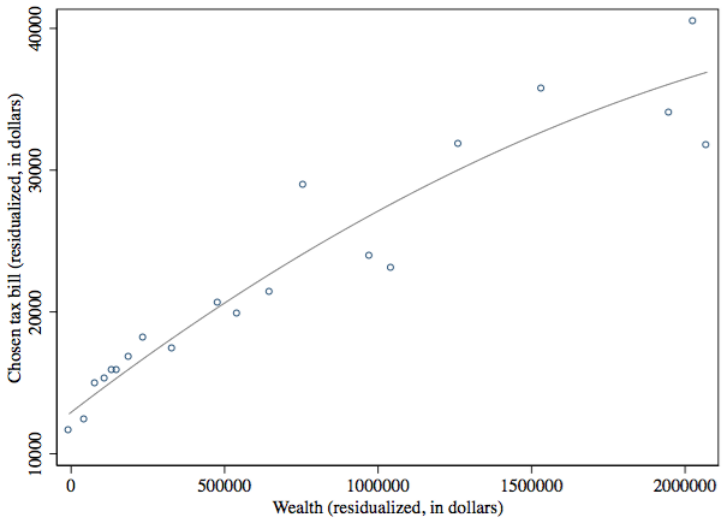
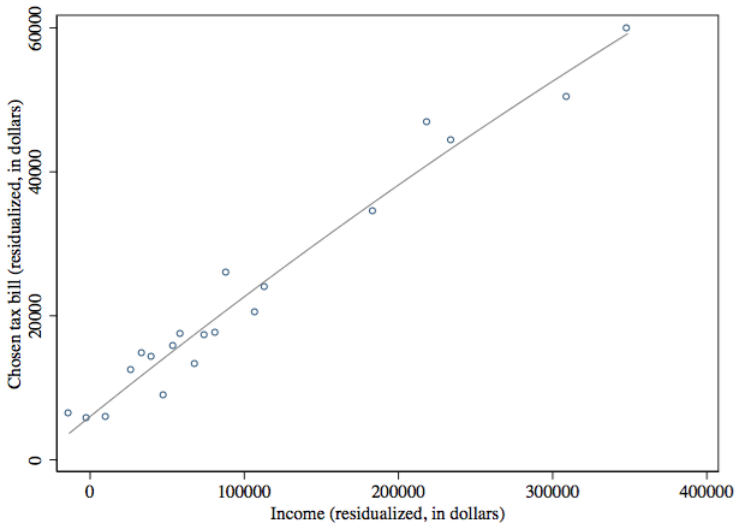
Figure 1: Tax bill as a function of wealth (source of wealth unspecified)



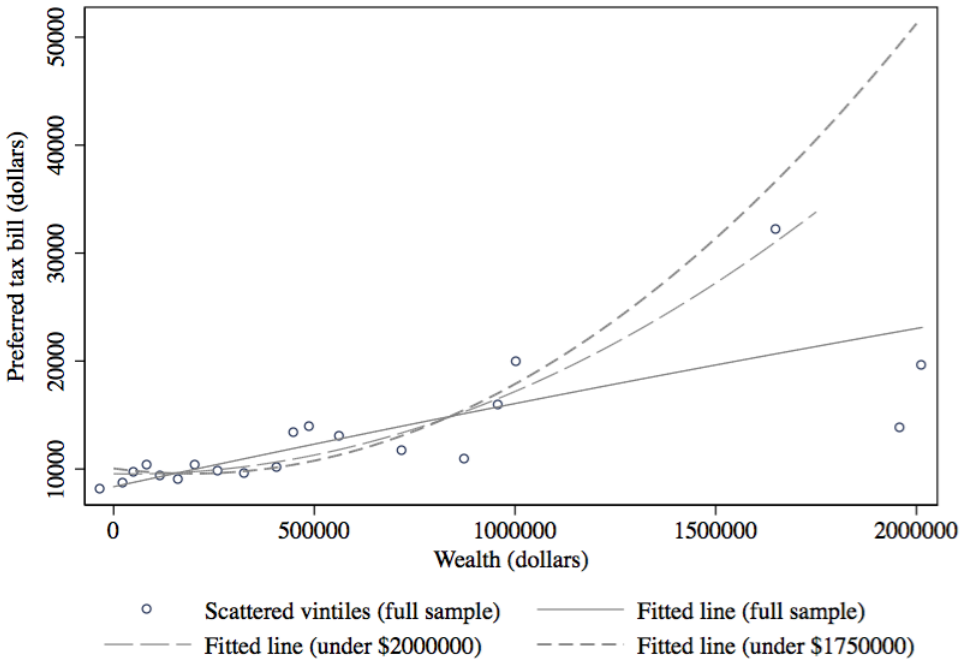
Notes: Data are taken from MTurk surveys in which we do not specify to respondents the source of wealth in the vignettes. The figure shows residualized vintiles of the tax and wealth data using the Stata binscatter package. The tax choices have been adjusted for income decile fixed effects and survey date fixed effects. We then add back in the means of the x- and y-axis variables. Note that the scatter points are collapsed to vintiles; subjects were confronted with *more* than the twenty wealth choices plotted in the figure. Fitted lines are based on the underlying data, not the scatter points.

Figure 2: Tax bill as a function of income (source of wealth unspecified)



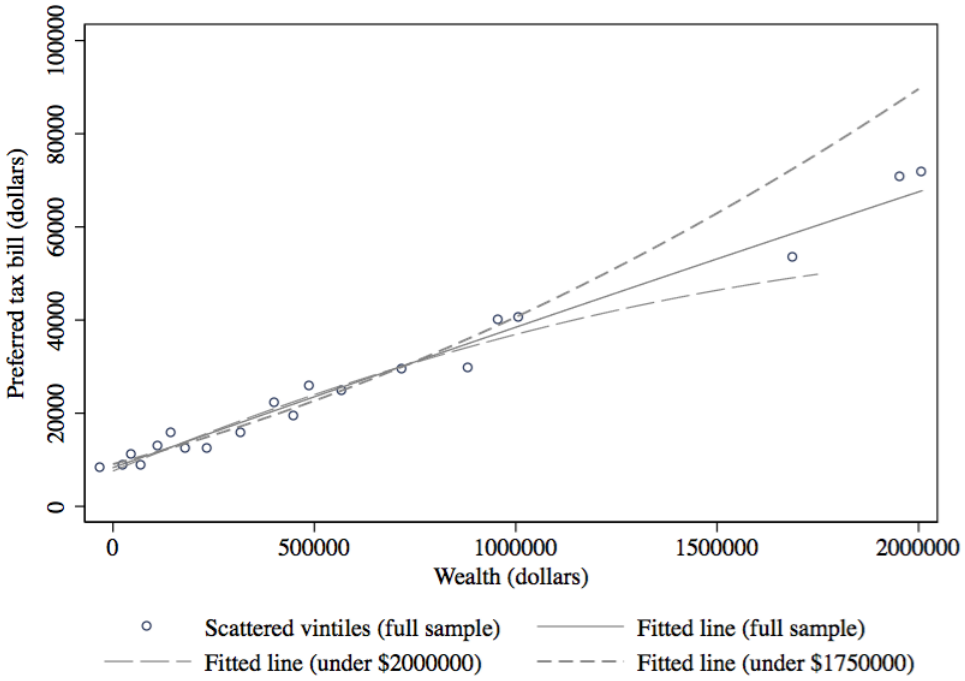
Notes: Data are taken from MTurk surveys in which we do not specify to respondents the source of wealth in the vignettes. The figure shows residualized vintiles of the tax and income data using the Stata binscatter package. The tax choices have been adjusted for wealth decile fixed effects and survey date fixed effects. We then add back in the means of the x- and y-axis variables. Note that the scatter points are collapsed to vintiles; subjects were confronted with *more* than the twenty income choices plotted in the figure. Fitted lines are based on the underlying data, not the scatter points.

Figure 3: Tax bill as a function of wealth (wealth from savings)



Notes: Data are taken from MTurk surveys in which we specify to respondents the source of wealth in the vignettes. This figure uses only those subjects who encountered the *savings* vignettes first and uses their preferred tax bills when wealth comes from savings. The figure shows residualized vintiles of the tax and wealth data using the Stata binscatter package. The tax choices have been adjusted for income decile fixed effects and survey date fixed effects. We then add back in the means of the x- and y-axis variables. Note that the scatter points are collapsed to vintiles; subjects were confronted with *more* than the twenty wealth choices plotted in the figure. Fitted lines are based on the underlying data, not the scatter points.

Figure 4: Tax bill as a function of wealth (wealth from inheritance)



Notes: Data are taken from MTurk surveys in which we specify to respondents the source of wealth in the vignettes. This figure uses only those subjects who encountered the *inheritance* vignettes first and uses their preferred tax bills when wealth comes from inheritance. The figure shows residualized vintiles of the tax and wealth data using the Stata binscatter package. The tax choices have been adjusted for income decile fixed effects and survey date fixed effects. We then add back in the means of the x- and y-axis variables. Note that the scatter points are collapsed to vintiles; subjects were confronted with *more* than the twenty wealth choices plotted in the figure. Fitted lines are based on the underlying data, not the scatter points.

Table 1: Summary statistics in our samples compared to the General Social Survey

	(1) mTurk (2014-15)	(2) mTurk (2018)	(3) UAS	(4) GSS
Female	0.422	0.466	0.566	0.546
	(0.494)	(0.499)	(0.496)	(0.498)
Age	33.17	36.67	53.43	47.44
	(10.29)	(10.71)	(15.96)	(17.77)
White	0.754	0.783	0.875	0.734
	(0.431)	(0.412)	(0.331)	(0.442)
Non-Hispanic White	0.754	0.783	0.809	0.646
	(0.431)	(0.412)	(0.393)	(0.478)
Has at least a college education	0.605	0.681	0.451	0.320
	(0.489)	(0.466)	(0.498)	(0.466)
Household income	49869.1	58320.5	72845.4	79633.1
	(37047.7)	(40828.6)	(50250.4)	(74347.8)
Supported Obama in 2012	0.646			0.619
	(0.478)			(0.486)
Supported Clinton in 2016		0.498	0.481	0.545
		(0.500)	(0.501)	(0.498)
Supports gov't redistribution	4.345	4.528		4.384
(scale 1-7)	(1.962)	(1.964)		(1.988)
Luck, help from others more	0.329	0.299		
important to success than hard work	(0.470)	(0.458)		
Wealth value considered in	647997.4	705008.7	652000	
tax scenarios	(649790.5)	(607119.3)	(609577.3)	
Income value considered in	82775.7	48703.1	79012.7	
tax scenarios	(78186.1)	(39203.8)	(75361.0)	
Observations	1899	1145	306	7753

Notes: The MTurk 2014-2015 sample pools subjects from all survey dates in 2014 and 2015. The MTurk 2018 sample pools subjects from survey rounds conducted in October of 2018. The UAS (Understanding American Study) data were collected in June and July 2019. The GSS sample is taken from the 2014, 2016, and 2018 GSS (though only the first two surveys are used for the Obama question, and the last one for the Clinton question). GSS surveys weights are used. "Supported" a candidate in the MTurk and the GSS is coded as one if the person voted for the candidate or if they expressed support but did not actually vote. In the UAS data, subjects are only asked which candidate they supported if they voted.

Table 2: Relationship between total tax bill and income and wealth values (surveys where source of wealth not specified)

	Dep't variable: Subjects' chosen total tax bill (dollars)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wealth (dollars)	0.0119***	0.0119***	0.0117***	0.0117***	0.0124***	0.0117***	0.00741**	0.0114***
	[0.00301]	[0.00300]	[0.00271]	[0.00271]	[0.00289]	[0.00272]	[0.00351]	[0.00261]
Income (dollars)	0.158***	0.158***	0.157***					
	[0.00897]	[0.00896]	[0.00920]					
Dept. var. mean	22415.1	22415.1	22415.1	22415.1	22821.0	22514.8	20668.8	22413.7
Question order FE?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
mTurk ID FE?	No	No	Yes	Yes	Yes	Yes	No	Yes
Income FE?	No	No	No	Yes	Yes	Yes	Yes	Yes
Ex. short	No	No	No	No	Yes	No	No	No
Ex. zero tax bills?	No	No	No	No	No	Yes	No	No
First obs. only?	No	No	No	No	No	No	Yes	No
Weighted?	No	No	No	No	No	No	No	Yes
Observations	5420	5420	5420	5420	5060	5396	542	5410

Notes: Data taken from MTurk surveys where the source of wealth in the vignettes is not specified. Question order fixed effects include ten dummies for each of the ten iterations of the question each respondent encountered. MTurk ID fixed effects include a fixed effect for each unique MTurk id (roughly speaking, for each subject, unless they take the survey with multiple IDs). "Ex. short" drops subjects who complete the survey in less than four minutes, roughly the fifth percentile of the duration distribution. "Ex. zero tax bills" drops those who enter a preferred tax bill of zero. "First obs. only" includes only the very first iteration that each subject encounters, in order to address concerns about anchoring bias. "Weighted" shows results after weighting our MTurk observations to match the 2014 GSS in terms of the $2 \times 2 \times 2$ weights based on dummies for being greater than thirty, female and having a BA (those characteristics where our MTurk and GSS samples differ the most). Standard errors (clustered by MTurk ID) are reported in brackets.
$^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Table 3: Most common bigrams and trigrams (surveys where source of wealth not specified)

Bigrams		Trigrams	
Phrase	Count	Phrase	Count
base incom	48	base incom wealth	7
incom wealth	40	flat tax rate	6
incom tax	36	base incom year	5
tax rate	32	incom made year	5
flat tax	28	tax rate incom	5
incom year	22	think flat tax	5
year incom	22	base incom level	4
pay tax	20	base much wealth	4
10 incom	19	everyon pay 10	4
incom level	19	flat tax incom	4
tax incom	19	incom accumul wealth	4
wealth incom	19	incom high wealth	4
percentag incom	18	pay amount tax	4
amount wealth	16	percentag incom tax	4
annual incom	13	take wealth consider	4
everyon pay	13	tax base incom	4
much wealth	13		
person wealth	13		
tax bracket	13		
thought fair	13		

Notes: Data are taken from MTurk surveys were we do not specify the source of wealth in the vignettes. At the end of these surveys (after all chosen tax bills and entered and demographic questions are asked) we ask respondents "Please describe how you decided on the level of tax payments for the hypothetical individuals in the survey." We use the "tm" package in R to process the text of the responses to this question. We convert all text to lowercase, strip punctuation and common English stopwords, and stem words with a Porter stemmer. We then take all 2-word (bigram) and 3-word (trigram) sequences in the remaining text, and calculate frequencies across subject responses.

Table 4: Chosen tax bill as a function of income and wealth (source of wealth specified)

	Dependent variable: Total tax bill (dollars)					
	Wealth from savings		Wealth from inherit.		Pooled, w/in-subject	
	(1)	(2)	(3)	(4)	(5)	(6)
Wealth (dollars)	0.00766***	0.00758***	0.0303***	0.0306***	0.0112***	0.0111***
	[0.00218]	[0.00216]	[0.00354]	[0.00357]	[0.00187]	[0.00181]
Income (dollars)	0.132***		0.135***		0.136***	
	[0.00759]		[0.0178]		[0.00793]	
Wealth is from inheritance					36.99	35.95
					[715.1]	[713.3]
Wealth x Inheritance					0.0130***	0.0129***
					[0.00243]	[0.00242]
Dept. var. mean	13008.0	13008.0	26570.0	26570.0	19125.0	19125.0
Question order FE?	Yes	Yes	Yes	Yes	Yes	Yes
mTurk ID FE?	Yes	Yes	Yes	Yes	Yes	Yes
Inc. decile FE?	No	Yes	No	Yes	No	Yes
Observations	4503	4503	4802	4802	18572	18572

Notes: Data from MTurk surveys. All regressions include fixed effects for survey date, question order and MTurk ID. In the first four columns, only the subjects' first seven questions are used in the sample. Half the sample was randomized so that the first seven questions involve wealth from *savings* and half so that the first seven questions involve wealth from *inheritance*. So, one set of individuals is sampled in cols. (1) and (2) and another set in cols. (3) and (4). In cols. (5) and (6) we combine both sample *and* use all 14 questions (so the sample size increases by a factor of four). As we retain the MTurk ID fixed effects, identification of the Wealth x Inheritance coefficient is coming from contrasting how the same person answers the first set of seven questions versus the second set of seven questions. Standard errors (clustered by MTurk ID) are in brackets. $^{*}p < .1,^{**} p < .05,^{***} p < 0.01$

Table 5: Testing for heterogeneous tax preferences on wealth

	(1) Var: Obama supporter	(2) Var: Supports redistr.	(3) Var: Luck important
Var x Wealth x Inheritance	0.0151*	0.00308*	0.000526
	[0.00811]	[0.00179]	[0.00814]
Wealth x Inheritance	0.0120*	0.00715	0.0249***
	[0.00628]	[0.00734]	[0.00507]
Inheritance question	1264.8	67.27	-1726.2
	[2090.0]	[2195.5]	[1347.5]
Var x Wealth	-28.31	151.9*	465.1
	[510.8]	[80.41]	[367.2]
Var x Inheritance questions	-1452.4	43.85	2822.8
	[2558.9]	[543.2]	[2603.3]
Wealth (dollars)	0.00777	0.000487	0.00461***
	[0.00473]	[0.00244]	[0.00121]
Var	2384.5	290.8	350.6
	[1896.8]	[330.2]	[1586.6]
Dept. var. mean	20006.9	20006.9	20025.3
Observations	9305	9305	8290

Notes: Data from MTurk surveys. All regressions include fixed effects for survey date, question order, MTurk ID and income decile fixed effects. Support for Obama and belief that luck is more important than hard work are binary variables. Support for redistribution is on a 1-7 scale. There are fewer observations in col. 3 than in the first two columns because of a greater share missing for the "luck" question. Standard errors (clustered by MTurk ID) are in brackets. Coefficients are multiplied by 100,000 for readability. $^{*}p < .1$,$^{**}p < .05$,$^{***}p < 0.01$

Table 6: Chosen tax bill as a function of income and wealth, *Understanding America* data

	Wealth from savings		Wealth from inherit.	
	(1)	(2)	(3)	(4)
Wealth (Dollars)	0.0133**	0.0131**	0.0493***	0.0485***
	[0.00584]	[0.00572]	[0.0124]	[0.0122]
Income (Dollars)	0.127***		0.201***	
	[0.0162]		[0.0769]	
Dept. var. mean	15200.8	15200.8	38224.7	38224.7
Income FE?	No	Yes	No	Yes
Observations	1127	1127	1037	1037

Notes: This table shows results from data collected on June 24 to July 19, 2019. For this round, we included our survey as part of the *Understanding America Study* run by the University of Southern California, in order to obtain a more nationally representative sample (see Table 1 for summary statistics). Otherwise the experiment is the same as the survey experiment described in Section 5. Question-order fixed-effects are included in all regressions and standard errors are clustered by subject. $^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

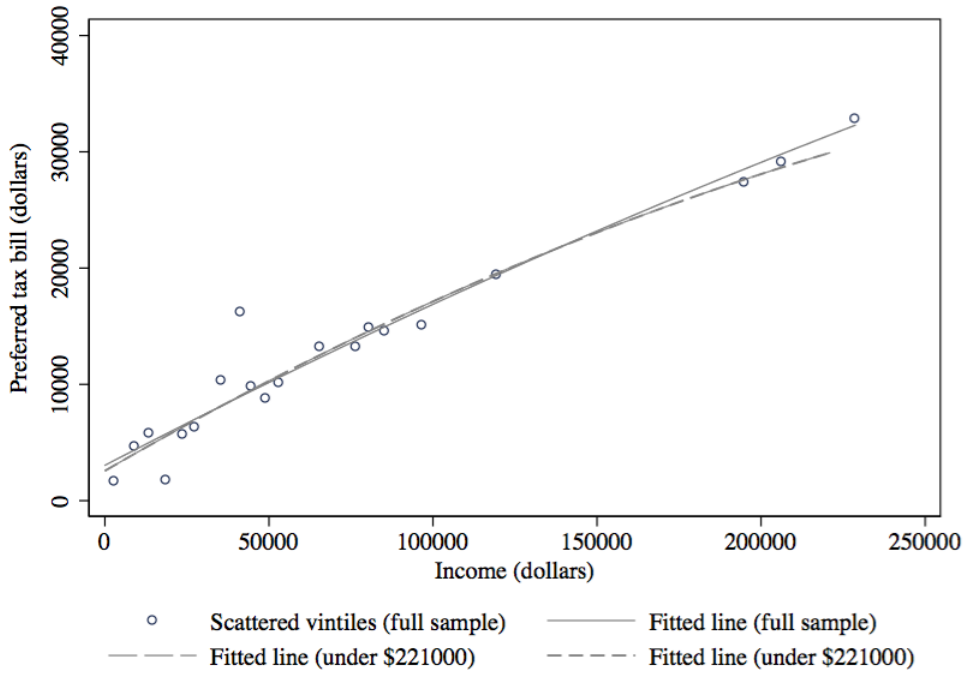
Table 7: Most common bigrams and trigrams (surveys where source is specified)

Bigrams		Trigrams	
Phrase	Count	Phrase	Count
base incom	100	went gut feel	12
incom tax	87	base incom wealth	11
pay tax	80	just went gut	11
tax rate	77	10 tax incom	10
flat tax	68	alreadi paid tax	10
tax incom	64	base tax payment	10
year incom	49	flat tax rate	10
alreadi tax	46	money alreadi tax	10
inherit money	45	tax base incom	10
inherit tax	42	10 flat tax	9
incom wealth	41	level tax payment	9
10 incom	40	base incom level	8
tax payment	37	flat 10 tax	8
incom year	35	incom regardless wealth	8
earn year	34	most base incom	8
save inherit	34	peopl pay tax	8
tax inherit	34	think peopl pay	8
10 tax	32	think peopl tax	8
incom level	32	believ flat tax	7
peopl pay	32	decid base incom	7
tax money	31	flat tax 10	7
percentag incom	30	money save inherit	7
save money	29	peopl inherit money	7
decid base	28	percentag base incom	7
tax peopl	28	tax money save	7
wealth incom	28		

Notes: Data are taken from MTurk surveys in which we specify the source of wealth in the vignettes. At the end of these surveys (after all chosen tax bills are entered and demographic questions are asked) we ask respondents "Please describe how you decided on the level of tax payments for the hypothetical individuals in the survey." We use the "tm" package in R to process the text of the responses to this question. We convert all text to lowercase, strip punctuation and common English stopwords, and stem words with a Porter stemmer. We then take all 2-word (bigram) and 3-word (trigram) sequences in the remaining text, and calculate frequencies across subject responses.

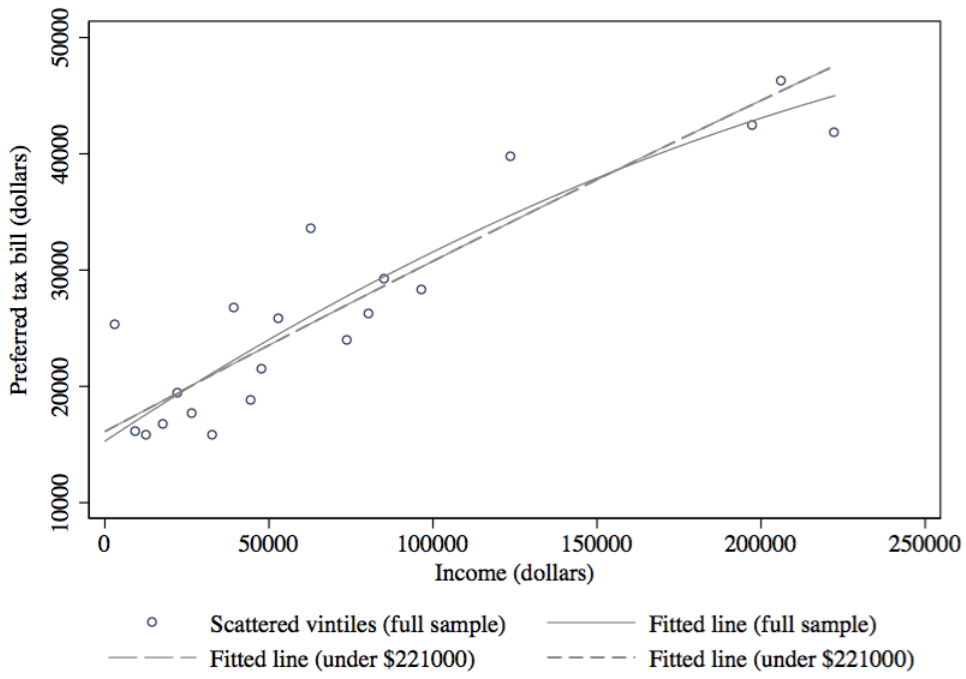
Appendix A Additional tables and figures not for publication

Appendix Figure A.1: Tax bill as a function of income (wealth from savings)



Notes: The figure shows residualized vintiles of the tax and wealth data. The tax choices have been adjusted for income decile fixed effects and survey date fixed effects. Note that as the scatter points are collapsed to vintiles, subjects were confronted with *more* than the twenty wealth choices plotted in the figure. Fitted lines are based on the underlying data, not the scatter points.

Appendix Figure A.2: Tax bill as a function of income (wealth from inheritance)



Notes: The figure shows residualized vintiles of the tax and wealth data. The tax choices have been adjusted for income decile fixed effects and survey date fixed effects. Note that as the scatter points are collapsed to vintiles, subjects were confronted with *more* than the twenty wealth choices plotted in the figure. Fitted lines are based on the underlying data, not the scatter points.

Appendix Table A.1: Survey rounds where the source of wealth is not specified

Round	Date	Sample size	Wealth distribution
1	2014-11-11	2,740	Basic
2	2014-12-12	2,680	Extended Wealth Support

Notes: Note that "sample size" refers to the unit of observation in the main regressions: subject-question level (each subject encounters ten scenarios for which they choose their preferred tax bill). "Basic" indicates that wealth values were drawn from the distribution {$50,000, $100,000, $200,000, $500,000, $1,000,000, $2,000,000}. Extended Wealth Support adds values $300,000, $750,000 and $1,500,000 to the support of the wealth distribution.

Appendix Table A.2: Survey rounds where the source of wealth is specified

Round	Date	Sample size	Wealth distribution
1	2015-04-06	2,618	Basic
3	2015-04-14	3,010	Basic
4	2015-11-10	3,962	Extended Wealth Support
5	2015-11-24	4,214	Jittered
6	2015-12-22	4,200	Jittered
7	2015-12-23	4,228	SCF Values
8	2018-10-9	8,400	Jittered
9	2018-10-10	8,400	Jittered
10	2018-10-29	8,400	Jittered
11	2019-6-24	4,312	Jittered

Notes: Note that "sample size" refers to the unit of observation in the main regressions: subject-question level (each subject encounters fourteen scenarios for which they choose their preferred tax bill). "Basic" indicates that wealth values were drawn from the distribution {$50,000, $100,000, $200,000, $500,000, $1,000,000, $2,000,000}. Extended Wealth Support adds values $300,000, $750,000 and $1,500,000 to the support of the wealth distribution. Jittered means that wealth values from the extended distribution were additionally given a 5% addition or subtraction. SCF values means that wealth, income pairs were drawn from 2010 Survey of Consumer Finance survey. Round 2 (N=32) was dropped due to implementation problems.

Appendix Table A.3: Verifying that levels of wealth subjects are asked to consider are uncorrelated with their characteristics (in surveys where source of wealth is unspecified)

	(1) Female	(2) Obama	(3) Age	(4) College	(5) White	(6) Black	(7) Own HH inc.	(8) Married	(9) Emp. FT
Wealth/100,000	0.000692	0.000227	-0.00210	-0.000289	-0.000122	-0.000290	-16.66	-0.000564	0.0000903
	[0.000967]	[0.000931]	[0.0190]	[0.000983]	[0.000881]	[0.000493]	[62.38]	[0.000940]	[0.00100]
Observations	5420	5420	5410	5420	5410	5410	5410	5420	5410

Notes: Data are taken from surveys where the source of wealth is not specified. Each coefficient is from a regression of the form $Var_i = \beta Wealth_{it} + \lambda_t + e_{it}$, where Var take the values of the characteristics listed in the columns. These regressions include survey date fixed effects (λ_t), given our different sampling rules for income and wealth employed across survey dates. As in our main regressions, we cluster standard errors by MTurk ID. $^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Appendix Table A.4: Verifying that levels of income subjects are asked to consider are uncorrelated with their characteristics (in surveys where source of wealth is unspecified)

	(1) Female	(2) Obama	(3) Age	(4) College	(5) White	(6) Black	(7) Own HH inc.	(8) Married	(9) Emp. FT
Income/10,000	-0.000290	-0.000159	0.0101	-0.000759	0.000171	-0.000352	-29.12	0.000370	-0.000162
	[0.000681]	[0.000694]	[0.0139]	[0.000677]	[0.000614]	[0.000366]	[38.11]	[0.000665]	[0.000692]
Observations	5420	5420	5410	5420	5410	5410	5410	5420	5410

Notes: Data are taken from surveys where the source of wealth is not specified. Each coefficient is from a regression of the form $Var_i = \beta Income_{it} + \lambda_t + e_{it}$, where Var take the values of the characteristics listed in the columns. These regressions include survey date fixed effects (λ_t), given our different sampling rules for income and wealth employed across survey dates. As in our main regressions, we cluster standard errors by MTurk ID. $^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Appendix Table A.5: Checking randomization of wealth values within survey date (surveys where source of wealth is specified)

	(1) Female	(2) Obama	(3) Age	(4) College	(5) White	(6) Black	(7) Own HH inc.	(8) Married	(9) Emp. FT
Wealth/100,000	-0.000285	-0.000242	0.0106	0.0000184	0.000382	-0.0000928	1.645	-0.000227	-0.00101
	[0.000710]	[0.000711]	[0.0152]	[0.000730]	[0.000627]	[0.000331]	[57.45]	[0.000691]	[0.000740]
Observations	9305	9305	9305	9305	9305	9305	9305	9305	9305

Notes: Data are taken from surveys where the source of wealth is specified. Each coefficient is from a regression of the form $Var_i = \beta Wealth_{it} + \lambda_t + e_{it}$, where Var take the values of the characteristics listed in the columns. These regressions include survey date fixed effects (λ_t), given our different sampling rules for income and wealth employed across survey dates. As in our main regressions, we cluster standard errors by MTurk ID. $^{*}p < .1$,$^{**}p < .05$,$^{***}p < 0.01$

Appendix Table A.6: Checking randomization of income values within survey date (surveys where source of wealth is specified)

	(1) Female	(2) Obama	(3) Age	(4) College	(5) White	(6) Black	(7) Own HH inc.	(8) Married	(9) Emp. FT
Income/10,000	-0.000388	-0.000271	-0.00435	-0.00106	-0.000167	-0.000128	-27.76	0.000582	0.000771
	[0.000647]	[0.000653]	[0.0131]	[0.000656]	[0.000548]	[0.000325]	[53.47]	[0.000626]	[0.000657]
Observations	9305	9305	9305	9305	9305	9305	9305	9305	9305

Notes: Data are taken from surveys where the source of wealth is specified. Each coefficient is from a regression of the form $Var_i = \beta Income_{it} + \lambda_t + e_{it}$, where Var take the values of the characteristics listed in the columns. These regressions include survey date fixed effects (λ_t), given our different sampling rules for income and wealth employed across survey dates. As in our main regressions, we cluster standard errors by MTurk ID. $^{*}p < .1$,$^{**}p < .05$,$^{***}p < 0.01$

Appendix Table A.7: Comparing respondents randomized into first seeing savings versus inheritance questions

	(1) Income	(2) Wealth	(3) Female	(4) Obama	(5) Age	(6) College	(7) White	(8) Black	(9) Own HH inc.	(10) Married	(11) Emp. FT
Saw savings	749.4	1907.9	-0.00419	0.000184	0.320	-0.0220	0.00515	-0.00706	-242.1	-0.0257	-0.00295
questions first	[1328.7]	[12369.2]	[0.0269]	[0.0259]	[0.558]	[0.0265]	[0.0233]	[0.0140]	[2119.9]	[0.0258]	[0.0271]
Mean, inheritance	72664.7	608291.3	0.429	0.652	33.18	0.618	0.757	0.0743	51027.2	0.356	0.541
Observations	9305	9305	9305	9305	9305	9305	9305	9305	9305	9305	9305

Notes: Data taken from surveys where the source of wealth is specified. Each coefficient is from a regression of the form: $Var_{it} = \beta Saw\ savings\ first_{it} + \lambda_t + e_{it}$, where $Saw\ savings\ first$ is a dummy for having been randomized to see first the vignettes where wealth is due to savings. $^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Appendix Table A.8: Do respondents' answers depend on which types of vignette (savings versus inheritance) they encounter first?

	Wealth from savings		Wealth from inherit.	
	(1)	(2)	(3)	(4)
Wealth (dollars)	0.00766***	0.0134***	0.0303***	0.0170***
	[0.00218]	[0.00318]	[0.00354]	[0.00254]
Income (dollars)	0.132***	0.149***	0.135***	0.122***
	[0.00759]	[0.0227]	[0.0178]	[0.0111]
Dept. var. mean	13008.0	17080.1	26570.0	19367.4
Module viewed first	Savings	Inheritance	Inheritance	Savings
Round FE?	Yes	Yes	Yes	Yes
mTurk ID FE?	Yes	Yes	Yes	Yes
Observations	4503	4570	4802	4697

Notes: Data taken from surveys where the source of wealth is specified. All regressions include question-order and MTurk ID fixed effects. The headings indicate the type of vignette (wealth from savings or wealth from inheritance) the respondents are answering. Standard errors clustered by MTurk ID. $^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Appendix Table A.9: Testing for heterogeneous tax preferences on wealth (additional results)

	(1) Var: Female	(2) Var: Older than 30	(3) Var: Above med. inc.	(4) Var: Has kids	(5) Var: Has BA	(6) Var: White
Var x Wealth x Inheritance	-0.00196	0.000316	0.000243	0.00685	-0.00365	-0.0105
	[0.00845]	[0.00802]	[0.00784]	[0.00805]	[0.00864]	[0.0105]
Wealth x Inheritance	0.0227***	0.0216***	0.0218***	0.0171***	0.0244***	0.0300***
	[0.00453]	[0.00631]	[0.00526]	[0.00638]	[0.00746]	[0.00965]
Inheritance question	-1128.5	604.1	819.1	546.5	519.5	-2122.1
	[1313.3]	[1974.8]	[1667.6]	[1732.8]	[2101.5]	[2705.8]
Var x Wealth	453.7	-263.7	-239.7	-125.2	-389.8	-189.3
	[474.3]	[429.6]	[352.9]	[385.3]	[457.5]	[441.4]
Var x Inheritance questions	3357.2	-474.1	-1269.4	-328.4	-504.4	3145.6
	[2566.2]	[2406.6]	[2278.8]	[2293.7]	[2521.0]	[3000.7]
Wealth (dollars)	0.00570***	0.00904**	0.00851***	0.00847***	0.00988**	0.00906**
	[0.00151]	[0.00398]	[0.00307]	[0.00282]	[0.00419]	[0.00373]
Var	-3966.2**	685.2	1286.2	338.4	2588.2	380.0
	[1726.3]	[1597.7]	[1343.5]	[1468.4]	[1670.5]	[1723.6]
Dept. var. mean	20006.9	20006.9	20006.9	20006.9	20006.9	20006.9
Observations	9305	9305	9305	9305	9305	9305

Notes: Data taken from surveys where the source of wealth is specified. All regressions include income fixed effects. Standard errors clustered by MTurk ID. Coefficients are multiplied by 100,000 for readability. $^{*}p < .1,^{**} p < .05,^{***} p < 0.01$

Appendix Table A.10: Testing for convexity of tax schedule over wealth (wealth from savings)

	Dependent variable: Chosen tax bill (in dollars)					
	(1)	(2)	(3)	(4)	(5)	(6)
Wealth (dollars)	0.00575*	-0.00135	-0.00800	0.00380	0.00132	0.00242
	[0.00336]	[0.00747]	[0.0131]	[0.00296]	[0.00434]	[0.00369]
Wealth squared / 10M	0.00902	0.0794	0.146			
	[0.0236]	[0.0696]	[0.132]			
Max(Wealth - 500,000, 0)				0.00481	0.0136	0.0115
				[0.00581]	[0.0108]	[0.0103]
Dept. var. mean	13008.0	12118.1	11697.3	13008.0	12118.1	11697.3
Question order FE?	Yes	Yes	Yes	Yes	Yes	Yes
mTurk ID FE?	Yes	Yes	Yes	Yes	Yes	Yes
Ex. if wealth above...	N/A	2M	1.75M	N/A	2M	1.75M
Observations	4503	4011	3932	4503	4011	3932

Notes: In this table, we test for whether our subjects' preferred tax bills exhibit convexity as captured in two specifications: a quadratic function (first three columns) or a spline with a knot at $500,000 (final three columns). In cols. 2-3 and 5-6 we drop observations with large wealth amounts, given past work showing that asking for tax preferences in levels produces lower implied rates than asking directly for rates, especially for large values of a tax base. All regressions include fixed effects for survey date, question order, MTurk ID and income decile fixed effects. Squared terms are scaled for readability. Standard errors (clustered by MTurk ID) are in brackets. $^{*}p < .1$,$^{**}p < .05$,$^{***}p < 0.01$

Appendix Table A.11: Testing for separability between wealth and income tax preferences

	Dependent variable: Total tax bill (dollars)					
	Generic wealth		Wealth from savings		Wealth from inherit.	
	(1)	(2)	(3)	(4)	(5)	(6)
(Income x Wealth)/100M	1.129		-0.431		1.468	
	[1.046]		[1.662]		[3.591]	
Wealth (dollars)	0.0105***	0.0122***	0.00797**	0.00877***	0.0292***	0.0308***
	[0.00285]	[0.00281]	[0.00312]	[0.00315]	[0.00446]	[0.00419]
Income (dollars)	0.147***	0.157***	0.135***	0.124***	0.126***	0.139***
	[0.0106]	[0.00954]	[0.0105]	[0.0128]	[0.0173]	[0.0221]
Wealth x (Inc. <50,000)		-0.00143		-0.00273		-0.00116
		[0.00194]		[0.00388]		[0.00471]
Income <50,000		1397.5		-29.20		1399.8
		[1595.8]		[1453.1]		[2616.2]
Dept. var. mean	22415.1	22415.1	13008.0	13008.0	26570.0	26570.0
Observation	5420	5420	4503	4503	4802	4802

Notes: Data for the first two columns are taken from the baseline surveys (where the source of wealth is not specified). Data for the middle two columns uses the surveys where the source is specified, but only use the answers to the vignettes where wealth comes from savings (and only from those who saw the savings vignettes first). The last two columns are analogues to the middle two but use the questions where wealth comes from inheritance. $^{*}p < .1,^{**} p < .05,^{***} p < 0.01$

Appendix Table A.12: "Income" vs. "labor income" wording

	"Income"		"Labor Income"		"Income"		"Labor Income"	
	Savings	Inherit.	Savings	Inherit.	Savings	Inherit.	Savings	Inherit.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wealth (Dollars)	0.0135**	0.0419***	0.00354***	0.0523***	0.0137**	0.0415***	0.00358***	0.0518***
	[0.00586]	[0.00727]	[0.00128]	[0.00890]	[0.00592]	[0.00722]	[0.00129]	[0.00882]
Income (Dollars)	0.0927***	0.164***	0.155***	0.108*				
	[0.0356]	[0.0471]	[0.0156]	[0.0650]				
Dept. var. mean	11893.2	29999.8	9620.8	34128.5	11893.2	29999.8	9620.8	34128.5
Income FE?	No	No	No	No	Yes	Yes	Yes	Yes
Observations	2051	2128	2170	2065	2051	2128	2170	2065

Notes: This table presents results from data collected from MTurk on October 9-10, 2018. In this round, we randomized the wording used in the definition of income. Half of respondents saw the original definition (which is essentially the actual definition of U.S. taxable income, i.e., including some forms of capital income). The other half saw a modified definition (essentially earned or labor income). For the exact wording, see Section 5.4. Otherwise the experiment is the same as the baseline experiment in Section 5. Question-order fixed-effects are included in all regressions and standard errors are clustered by subject. $^{*}p < .1$,$^{**}p < .05$,$^{***}p < 0.01$

Appendix Table A.13: Original ("inheritance") versus modified wording ("savings of a deceased relative")

	Original Wording				New Wording			
	Savings		Inherit.		Savings		Inherit.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wealth (Dollars)	0.00786*	0.00824*	0.0366***	0.0369***	0.00675***	0.00681***	0.0346***	0.0351***
	[0.00410]	[0.00417]	[0.00937]	[0.00912]	[0.00241]	[0.00241]	[0.00881]	[0.00897]
Income (Dollars)	0.134***		0.108		0.127***		0.213***	
	[0.0338]		[0.0887]		[0.0216]		[0.0807]	
Dept. var. mean	11469.9	11469.9	34756.9	34756.9	8951.8	8951.8	32816.3	32816.3
Income FE?	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1008	1008	1057	1057	1148	1148	1099	1099

This table presents results from data collected on October 29, 2018. In this round, we randomized the wording used in the definition of inherited wealth. Half of respondents saw the original definition (see Section 2). The other half saw a modified definition: "Consider a person who, at the end of 2017, had $X in wealth, accumulated mostly from inheritance received from the savings of a deceased relative." Otherwise the experiment is the same as the baseline experiment in Section 5. Question-order fixed-effects are included in all regressions and standard errors are clustered by subject. $^{*}p < .1$,$^{**}p < .05$,$^{***}p < 0.01$

Appendix Table A.14: Implied redistributive motives, given subjects' preferred tax rates and select elasticity assumptions

	Supply elasticity					
Subjects' chosen τ...	e=0	e=0.1	e=0.25	e=0.5	e=1.0	e=2.0
Wealth elasticities						
...on generic wealth (23.3%)	1	0.970	0.924	0.848	0.696	0.391
...on wealth from savings (15.2%)	1	0.982	0.955	0.911	0.821	0.643
...on wealth from bequests (61.3%)	1	0.842	0.604	0.208	-0.584	-2.167
Income elasticities						
...on generic income (14.4%)	1	0.983	0.958	0.916	0.832	0.664

Notes: See Section C for a full definition of redistributive motives. Briefly, the redistributive motive is defined as $\frac{\int_i g_i k_i}{\int_i k_i}$, where g_i is the welfare weight put on an individual i with wealth k_i. By assumption, $\int_i g_i = 1$, so that if welfare weights are *negatively* correlated with wealth then $\frac{\int_i g_i k_i}{\int_i k_i} < 1$. For $\frac{\int_i g_i k_i}{\int_i k_i} < 0$ welfare weights on the wealthy are negative. Subjects' preferred wealth tax rates are taken from col. (4) of Table 2 and cols. (2) and (4) of Table 4.
$^{*}p < .1,^{**} p < .05,^{***} p < 0.01$

Appendix B Additional details on data collection

The data collection for this project began on MTurk. We collected over 2,100 subjects in 2014–2015 used for the analysis in this paper. We followed past papers to ensure we surveyed human subjects (as opposed to "bots") who took the task seriously. For example, we begin each survey with a "captcha" (non-standard writing difficult for computers to interpret), followed by a series of animal sketches (hand-drawn by one of the authors) that subjects needed to identify before starting the experiment, in case robots have been trained to read captchas.[26] We also required that MTurk workers have at least a ninety percent satisfaction rate on past HITs. As noted in the paper, nearly all (99 percent) of respondents answer open-ended questions and all but one or two give responsive, sensible answers in colloquial English.

In 2018, we sought to replicate our original results as well as perform several variants of the original survey experiments. As has been well documented, MTurk data quality declined markedly in the summer of 2018 (see, e.g, Kennedy *et al.* (2018) and Dennis *et al.* (2018), who refer to this episode as the "MTurk data quality crisis"). We were, in fact, unaware of this literature, but noticed this problem ourselves by reading through the answers to our open-ended questions. While the majority of responses provided a sensible answer to the question in colloquial English (as in the 2014–2015 data) about one-quarter were suspicious. Most frequently, they would contain a paragraph on the topic of the question (taxes) but were otherwise non-responsive and nonsensical. A few illustrative examples are: "Because the Making Work Pay Tax Credit was integrated into the tax system.The income effect shows the variation of y good quantity given by the change of real income;" "Recent fiscal policies, including the 2008 stimulus payments and the 2009 Making Work Pay Tax Credit, aimed to increase household spending. This paper quantifies the spending response to these policies and examines differences in spending by whether the stimulus was delivered as a one-time payment or as a flow of payments from reduced withholding;" and "The three types of taxes are the proportional tax, the progressive tax, and the regressive tax." We suspect that these responses were generated by an algorithm that searches online for the words included in our open-ended question and then outputs one of the results.

For each round of data collected, we inspect by hand the open-ended responses and create a dummy for the share of suspected bots. Appendix Table B.1 shows these results, along with other variables related to data collection. In the 2014-2015 data, we detect essentially zero bots. However, in 2018, when we used the same selection criteria (i.e., a minimum ninety-percent approval from past HITs) as we did in 2014-2015, the bot share is much higher (29.7 percent), consistent with the "MTurk quality crisis" documented in the papers above.

Our first reaction was to substantially raise the past approval rate (to 100 percent) as well as require a minimum of 1,000 past HITs. However, this set of requirements slowed data collection to a trickle, and despite keeping the survey open for seven days we collected only 123 responses. Such a slow rate was not only impractical, but we also worried the sample would be significantly differently selected than our previous samples. As a compromise, we

[26]Examples of "captchas" can be found here: `http://www.fileflash.com/graphics/screens/Captcha_Creator_PHP_Script-69.gif`.

settled on a minimum approval rate of 98 percent with at least 1,000 past HITs (sessions run in October of 2018). While some bots (as proxied by our inspection of open-ended responses) slip through (as can be seen in Appendix Table B.1) we were satisfied that the vast majority of subjects (more than ninety percent) were humans who took the survey in good faith. Note that when we analyze these data in the paper, we do not drop bots (we trust that their minimal level will only mildly affect the results), as doing so would mean that we hand-select which observations to include.[27]

While we were reasonably satisfied that these sessions successfully eliminated most bots, as they are still drawn from MTurk, they are not representative of the U.S. adult population. We thus decided to re-run the survey experiment (the version in Section 5, which randomizes between wealth from savings and inheritance) on a platform that advertised access to a nationally representative sample. Unfortunately, the first two platforms we tried had bot shares (again, as proxied by our reading of the open-ended responses) of 14.5 and 47.1 percent. Obviously, given the bot issue in these data, claims of being nationally representative are likely inaccurate.

Our ultimate solution to this problem was to use a much more expensive survey platform. Specifically, we paid to add our survey questions to the *Understanding America Study* at USC. While much more expensive and thus not an option for all researchers, it provides us a substantially more nationally representative sample (as we show in Table 1) and we found no bot-like responses to the open-ended questions.

We hope the information in this appendix can potentially inform other researchers about various trade-offs in online survey data collection.

[27]We show that even in the more bot-infested rounds of data-collection (when bot rates are high enough that we prefer to exclude those rounds of data collection entirely), our results still look similar to those in the main text, as we would suspect because by our measure even in these rounds of data collection more than two-thirds of respondents are humans taking the survey in good faith. We show results for the data collected on August 20 and November 27, 2018 in Appendix Table B.2.

Appendix Table B.1: Data quality and other information for all rounds of data collection

Vendor	Round Start Date	Round End Date	Wealth source specified?	Labor Income Randomization?	Inheritance Randomization?	mTurk Sample Restrictions	Sample Size	Share Bots	Price Estimate
mTurk	11/11/14	11/11/14	No	No	No	>90% approved	274	<1%	$330
mTurk	12/12/14	12/12/14	No	No	No	>90% approved	268	<1%	$330
mTurk	4/2/15	4/2/15	Yes	No	No	>90% approved	187	<1%	$206
mTurk	4/14/15	4/14/15	Yes	No	No	>90% approved	215	<1%	$242
mTurk	11/10/15	11/10/15	Yes	No	No	>90% approved	281	1%	$393
mTurk	11/24/15	11/24/15	Yes	No	No	>90% approved	301	1%	$420
mTurk	12/22/15	12/22/15	Yes	No	No	>90% approved	300	0%	$420
mTurk	12/23/15	12/23/15	Yes	No	No	>90% approved	300	0%	$420
mTurk	1/21/16	1/21/16	Yes	No	No	>90% approved	101	0%	$140
mTurk	2/8/16	2/8/16	Yes	No	No	>90% approved	200	1%	$280
mTurk	8/20/18	8/20/18	Yes	Yes	No	>90% approved	300	29.7%	$420
mTurk	9/17/18	9/24/18	Yes	Yes	No	>99%+ approved, > 1000 HITs approved	124	<1%	$350
mTurk	10/9/18	10/9/18	Yes	Yes	No	>97% approved, > 1000 HITs approved	600	5.0%	$840
mTurk	10/10/18	10/10/18	Yes	Yes	No	>97% approved, > 1000 HITs approved	600	7.7%	$840
mTurk	10/29/18	10/29/18	Yes	No	Yes	>97% approved, > 1000 HITs approved	600	1.7%	$840
CINT	11/27/18	11/28/18	Yes	No	No	N/A	300	14.5%	$1,200
SSI	1/10/19	1/10/19	Yes	No	No	N/A	17 (300)	47.1%	$1,500
UAS	6/24/19	7/19/19	Yes	No	No	N/A	289	0.0%	$9,200

Notes: See Section 3 for further detail on data collection. Because of the high bot rate, we do not use data from the August 20th 2018 MTurk round, the CINT round or the SSI round in the main part of the paper (though Appendix Table B.2 shows results for the the August 2018 MTurk and CINT rounds.) For the SSI data collection round beginning on January 10, 2019, we aborted the data collection after 17 subjects out of a planned 300 because of a high detected bot rate. The price listed in the table was the one quoted to us had we completed the full 300-subject sample. MTurk sample restrictions refer to standards that requesters can set on eligible workers (e.g., the share of their past human intelligence tasks, for HITs, for which they received a positive rating or the number of past HITs they completed).

Appendix Table B.2: Baseline results estimated on data with high rates of detected "bots"

	Wealth from savings		Wealth from inherit.	
	(1)	(2)	(3)	(4)
Demeaned Wealth	0.0158*	0.0244*	0.0351***	0.0509***
	[0.00937]	[0.0137]	[0.00905]	[0.0191]
Demeaned Income	0.231	0.277**	0.108**	0.0678
	[0.166]	[0.118]	[0.0530]	[0.130]
Dept. var. mean	14754.3	20340.6	26030.7	30300.7
Data Source	mTurk	CINT	mTurk	CINT
Observations	1015	1064	1085	1015

Appendix Table B.3: Additional Robustness Exercises (Main Effect of Wealth)

Dependent variable: Total tax bill (dollars)

	Dep't variable: Subjects' chosen total tax bill (dollars)			
	(1)	(2)	(3)	(4)
Wealth (dollars)	0.0114***	0.0121***	0.0113***	0.0110***
	[0.00259]	[0.00293]	[0.00260]	[0.00325]
Dept. var. mean	22413.7	22605.2	22373.3	21603.2
Excl. Reversals?	No	No	No	Yes
Excl. All Round?	No	No	Yes	No
Excl. First?	No	Yes	No	No
Question order FE?	Yes	Yes	Yes	Yes
mTurk ID FE?	Yes	No	Yes	Yes
Income FE?	Yes	Yes	Yes	Yes
Ex. short	Yes	Yes	Yes	Yes
Ex. zero tax bills?	No	No	No	No
First obs. only?	No	No	No	No
Weighted?	Yes	Yes	Yes	Yes
Observations	5410	4869	5380	3890

Notes: Column 1 shows the main specification. Column 2 shows the specification omitting the first question, and including the income and wealth from the first question as controls. Column 3 excludes all observations where the individual reported any reversal (e.g. higher wealth and income but lower tax bill). Column 4 excludes all subjects who report every tax bill as divisible by $10,000.
$^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Appendix Table B.4: Additional Robustness Exercises (Wealth By Source)

Dependent variable: Total tax bill (dollars)

	Dep't variable: Subjects' chosen total tax bill (dollars)			
	(1)	(2)	(3)	(4)
Wealth (dollars)	0.0111***	0.00989***	0.00960***	0.0112***
	[0.00181]	[0.00166]	[0.00176]	[0.00185]
Wealth is from inheritance	35.95	-655.3	-541.1	188.4
	[713.3]	[734.2]	[678.9]	[719.8]
Wealth x Inheritance	0.0129***	0.0147***	0.00815***	0.0130***
	[0.00242]	[0.00247]	[0.00215]	[0.00247]
First Income Asked		-0.00794		
		[0.0131]		
First Wealth Asked		0.00110		
		[0.00102]		
Dept. var. mean	19125.0	19054.8	17401.3	19458.1
Excl. Reversals?	No	No	Yes	No
Excl. All Round?	No	No	No	Yes
Excl. First?	No	Yes	No	No
Question order FE?	Yes	Yes	Yes	Yes
mTurk ID FE?	Yes	No	Yes	Yes
Inc. decile FE?	Yes	Yes	Yes	Yes
Observations	18572	15858	12584	18106

Notes: Column 1 shows the main specification. Column 2 shows the specification omitting the first question, and including the income and wealth from the first question as controls. Column 3 excludes all observations where the individual reported any reversal (e.g. higher wealth and income but lower tax bill). Column 4 excludes all subjects who report every tax bill as divisible by $10,000.
$^{*}p < .1,^{**}p < .05,^{***}p < 0.01$

Appendix C Calibrating with Optimal Tax Formulae

In a standard optimal tax model, the observed tax preferences will be a function of the normative weights the social planner puts on wealth holders as well as the elasticities of wealth and income with respect to net-of-tax rates. For the purposes of this section, we assume agents see the economic environment just as a traditional, Mirleesian social planner would, and know the supply elasticity of wealth perfectly. Saez and Stantcheva (2016b) simplify the taxation of capital income literature by putting wealth directly into the utility function (with linear utility in consumption), and show that numerous other models can be embedded in this framework. This approach sacrifices detailed study of dynamic responses, but yields tractable analysis of a variety of normative views embedded in flexible welfare weights. Putting wealth directly in the utility function is motivated, in their paper, by bequest motives, the non-pecuniary value of entrepreneurship, service flows from wealth (e.g., the peace of mind of potential liquidity, housing services), or motivated beliefs and social norms. The resulting optimal tax formulas are transparent and easy to calculate, and can incorporate a wide variety of normative justifications.

C.1 Modification of Saez and Stantcheva (2016b) with direct wealth taxes

We begin by deriving the optimal linear wealth tax formula with arbitrary welfare weights from the Saez and Stantcheva (2016b) model, using a linear wealth tax instead of a linear capital income tax, and show that it yields a formula that is identical to a "capitalized" version of the capital income tax formula. Given the results in Appendix Table A.11 that our subjects do not consider *interactions* between income and wealth in setting the total tax bill, we also use the version of the formula that assumes separability of wealth and income taxes. Suppose mass 1 of agents with instantaneous utility:

$$u(c, k, z) = c + a(k) - h(z), \tag{1}$$

with c consumption, k wealth, and z taxable labor income. Dynamic utility is given by:

$$\int_0^\infty (c + a(k) - h(z))e^{-\delta t}dt, \tag{2}$$

subject to the dynamic budget constraint:

$$\dot{k} = (1 - \tau_L)z - c + rk - \tau_W k + G, \tag{3}$$

where $G = \tau_W k_m + \tau_l z_m$ is total tax revenue. Contrast this equation with the dynamic budget constraint in Saez and Stantcheva (2016b):

$$\dot{k} = (1 - \tau_L)z - c + (1 - \tau_K)rk + \tau_K r k_m + \tau_l z_m. \tag{4}$$

In Saez and Stantcheva (2016b) the tax is paid only on capital income, and so the after-tax price of capital is $(1 - \tau_K)r$, while in a "pure" wealth tax application the after-tax price of

capital is $r-\tau_W$. This adjustment simply implies that the wealth tax needs to be capitalized by r to yield an identical formula to equation (5) in Saez and Stantcheva (2016b). If rates of return were heterogeneous, however, both welfare weights as well as the relevant aggregate elasticity could generate differences between the optimal wealth tax and the optimal capital income tax, and assumptions would have to be made about whether unusually high returns were rents or reflect productivity as in Guvenen *et al.* (2019).

The Hamiltonian is given by:

$$H(c,k,z,\lambda) = c + a(k) - h(z) + \lambda((1-\tau_L)z - c + rk - \tau_W(k) + G). \tag{5}$$

First-order conditions imply $\lambda(t) = 1$ for all t. In particular the first-order condition for capital is:

$$a'(k_i) = \delta - (r - \tau_W). \tag{6}$$

Hence the aggregate response of capital to the net of tax price of capital is given by $\frac{dk_m}{d(r-\tau_W)}$. Again contrast to Saez and Stantcheva (2016b), who have the same net of tax derivative given by $\frac{dk_m}{dr(1-\tau_K)}$.

Following the argument in Saez and Stantcheva (2016b), we have the equivalent static utility representation:

$$U_i = c_i + u(k_i) - h(z_i) + \delta(k_i^{init} - k_i), \tag{7}$$

subject to $c = (1-\tau_L)z + rk - \tau_W k + G$.

We now have the social welfare function given by (with $\int_i \omega_i di = 1$):

$$SWF = \int_i \omega_i U_i di. \tag{8}$$

At the optimum, we have that $\frac{dSWF}{d\tau_W} = 0$. The envelope theorem implies $dU_i/d\tau_W = -k_i$, while the marginal revenue effect is direct revenue raised plus loss due to the behavioral response of wealth: $k_m - \tau_W \frac{dk_m}{d(r-\tau_W)}$. Hence, we have:

$$\frac{dSWF}{d\tau_W} = \int_i \omega_i \cdot \Big(- k_i + k^m - \tau_W \frac{dk^m}{d(r-\tau_W)}\Big) d_i. \tag{9}$$

Collect terms and divide throughout by k_m to get:

$$\frac{dSWF}{d\tau_W} = \int_i \omega_i \cdot \Big(1 - \frac{k_i}{k_m} di - \frac{\tau_W}{k_m} \frac{dk^m}{d(r-\tau_W}) \Big). \tag{10}$$

Defining $e_K = \frac{r-\tau_W}{k_m}\frac{dk_m}{d(t-\tau_W)}$ and $\bar{g_K} = int_i \omega_i \frac{k_i}{k_m}$ yields:

$$\frac{dSWF}{d\tau_W} = 1 - \bar{g_K} - \frac{\tau_W}{r-\tau_W} e_K. \tag{11}$$

So at the optimum we have:

$$\tau_K = \frac{1 - \bar{g_K}}{1 - \bar{g_K} + e_K}, \tag{12}$$

where $\tau_K \equiv \frac{\tau_W}{r}$. Thus we can see that capitalizing the wealth tax gives the same formula as the Saez and Stantcheva (2016b) optimal capital income tax, under the assumption of no heterogeneity in rates of return, where e_K is the supply elasticity of aggregate K, and $\bar{g}_K = \int_{i=0}^{1} g_i \frac{k_i}{K} d_i$ is the inner product of the welfare weights (normalized so that $\int_i g_i di = 1$) with wealth shares.

It is easy to show that $\bar{g}_K$ is equal to the covariance of welfare weights with the wealth shares plus 1.[28] Redistributive motives can be captured with $g_i(k_i)$ decreasing in k_i. If $\bar{g}_K = 0$, then the optimal tax rate is the revenue maximizing one (equivalent to no consideration being given to wealth-holders and thus the only objective being to maximize the demogrant, which benefits the worst off). If taxes are chosen by a social planner who cares equally about the wealthy and the poor (so the covariance of the weight with wealth shares is 0) then $\bar{g}_K = 1$ and the optimal tax is 0. The formula also reflects the standard result that the optimal tax declines with the supply elasticity of the tax base, and it is immediate that an infinite elasticity implies a zero optimal tax.

We are primarily interested in the implicit welfare weight put on owners of capital under the assumption that our survey subjects' preferred tax rates are optimal. Rewriting equation (12), this weight is given by:

$$\bar{g_K} = 1 - \frac{\tau_W}{r - \tau_W} e_K = 1 - \frac{\frac{\tau_W}{r}}{1 - \frac{\tau_W}{r}} e_K. \tag{13}$$

Our estimates for $\bar{g}_K$ obviously depend on the estimate for e_K. The empirical literature estimating e_K is sparse and unsettled, reflects both avoidance and evasion, and indeed e_K may differ for savings and bequests. Further, micro short-run elasticities will be different from the macro or long-run elasticities, which may be much larger. Jakobsen *et al.* (2018) use changes and kinks in the Danish wealth tax schedule on couples and high-wealth households to estimate wealth responses (both avoidance and evasion) among the moderate and very wealthy. Their simulated long-run elasticities with respect to the net of tax rate of return are 0.5 in the middle and 2 at the top. Seim (2017) uses kinks in the Swedish wealth tax schedule to estimate e_K, finding small responses of wealth and no response of labor income.[29] An exceptional estimate of e_K, larger than 20, is estimated by Brülhart *et al.* (2017) using cross-canton variation in Swiss data, but this exercise is likely less informative about the effects of a national wealth tax in a large country like the US.

Table A.14 shows the resulting $\bar{g}_K$ for our different sources of wealth and different assumptions about e_K. Unsurprisingly, the higher the elasticity, the higher the redistributive motive required to rationalize a given tax rate, and thus the lower $\bar{g}_K$. For comparison,

[28] By definition, $Cov(g_i, \frac{k_i}{K}) = \int g_i \frac{ki}{K} d_i - \frac{1}{K}\mathbf{E}(g_i) \cdot \mathbf{E}(k_i)$. Recall that over an interval of measure one, the average and aggregate values are equal, so $\mathbf{E}(g_i) = \int g_i d_i$, which equals 1 by assumption, and $\mathbf{E}(k) = \int k_i d_i$. As such, $\bar{g}_K = \int g_i \frac{k_i}{K} d_i = Cov(g_i, \frac{k_i}{K}) + 1$.

[29] Seim (2017) also estimates the cross-elasticity of labor income with respect to capital income taxes, and finds a zero, which justifies the use of the separable utility formula.

the bottom row of Table A.14 shows the corresponding welfare weights implied by subjects' preferred income tax rates under different taxable income elasticities. Again unsurprisingly, these are higher than the comparable welfare weights generated from subjects' preferred wealth tax rates, but not implausibly different in magnitude.

An important special case is $e_K = 0$, which implies that there is no supply response of wealth-holders to taxation. Given the evidence above that none of our subjects mentioned any evasion or avoidance margin as a reason for limiting wealth taxes, it could be that our subjects believe the true elasticity is zero. If this is the case, traditional utilitarian welfare weights would imply that all wealth should be equalized, with tax rates set at one, making our observed tax rates *below* what would be expected, suggesting that subjects might put additional weight on the welfare of the wealthy or believe that taxation is unjustly punitive. Given the high mean wealth rates subjects observed, a standard (e.g. utilitarian with log utility over wealth) parameterization of $\bar{g}_K = \int \frac{1}{k} dF(k)$ would imply that one dollar transferred to the rich delivers on the order of 10^{-6} amount of social planner, and is basically worthless as a result. With no supply response our subjects are much more sympathetic towards the rich than benchmark redistributive weights would imply.

Consider the other extreme, in which our subjects are internalizing a much higher capital supply elasticity of $e_K = 1$ or higher. In contrast to the $e_K = 0$ case, the implied welfare weights here are extremely punitive toward wealth-holders, particularly inheritors. With decreasing welfare weights, this implies a willingness to actively punish large inheritors, which rules out Pareto-optimal allocations of inherited wealth: there is no regular social welfare function that implements allocations consistent with these weights (Saez and Stantcheva, 2016a). This result is consistent with evidence on fairness preferences from ultimatum games, in which receiving subjects sacrifice their own payoffs in order to deny the giver too unequal a share of unearned income (Fehr and Gächter, 2000), as well as recent experimental evidence that individuals seek to lower the income of the richest person in larger distributions (Fisman *et al.*, 2017).

Finally, recall from Section 5 that our subjects imposed a non-zero wealth tax even on individuals with limited income. Saez and Stantcheva (2016b) offer one interpretation for our result above that wealth and income taxes seem to be set independently. In their model, if wealth and income both enter the utility function, but are independent (i.e. additively separable) sources of utility, then the optimal wealth tax does not take income into account. Another interpretation is that subjects believe idle wealth should be mobilized into productive uses that generate income, or be taxed.[30]

Two key results from the paper are that respondents have non-trivial but modest preferred tax rates on wealth and income *and* that they have little (stated) concern about the elasticity of the tax base with respect to the net of tax rate. In an optimal tax framework, these two conditions imply that respondents must *also* have relatively limited redistributive motives (as, given an inelastic tax base, even a utilitarian would choose full redistribution). This section has allowed us to formalize that intuition. Indeed our respondents (again, as-

[30]Guvenen *et al.* (2019) formalize and quantify this argument, which is associated with Allais (1977). Those wealth holders who can only manage, say, a two percent annual return would quickly see their wealth taxed away, presumably redistributed to sectors of the economy that enjoy higher returns.

suming, as they seem to suggest in their open-ended responses, that they believe there is no or very limited aggregate capital elasticity) appear orders of magnitude less redistributive than common baselines assumed in the literature. Of course, this exercise requires that we shoehorn our respondents' preferences into an optimal tax model (when perhaps they think of the problem in a completely different manner), but it is still noteworthy that they seem to have the opposite concerns of the classical social planner, who has strong redistributive motives due to diminishing marginal utility of consumption but is constrained by efficiency concerns.