The Causal Effects of Wealth on Political Participation and Political Preferences*

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Abstract

We test whether random, positive wealth shocks in the form of lottery prizes induce changes in political participation and political preferences. We find no effects on participation: Compared to suitably matched controls, large-prize winners are no more likely to cast votes in national elections or run for political office, nor are their children. But preferences change, in the sense that lottery winners become more negative toward taxes on wealth, real estate and inheritances. Consistent with material self-interest influencing preferences, these effects diminish over time as lottery wealth dissipates. Effects on other political preferences are statistically insignificant, though they often go in the direction of a more right-wing political orientation. We find no evidence that lottery wealth changes moral values or strengthen beliefs in the importance of hard work for success in life.

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1 Introduction

The wealthy participate more in politics than the poor (Verba, Schlozman, and Brady, 1995; Fox and Lawless, 2005; Blais, 2006; Ojeda, 2018). For example, people at the top of the income distribution in Sweden are 30 percentage points more likely to vote in parliamentary elections and almost six times more likely to run for political office compared to people at the bottom. Such vast differences in participation may be of little consequence if wealth and income were unrelated to political preferences, yet observational studies document a strong positive relationship between economic status and support for right-of-center economic policy (e.g. Lewis-Beck and Nadeau, 2011; Persson and Martinsson, 2018). A recent analysis of post-election survey data from several countries by Piketty (2020, p. 766) concluded that "property ownership appears as an almost irresistible determinant of political attitude: the wealthiest asset holders virtually never vote for the left, while those who do not own anything seldom vote for the right."

Do these wealth-gradients for participation and preferences reflect causal effects of wealth? If so, the distribution of income will affect political outcomes through two different channels. First, by influencing people's political preferences. And second, by affecting which opinons are represented in the political system. Political theorists have indeed often emphasized the causal role of economic resources. For example, a standard assumption in political economy, at least in since Downs (1957), is that people vote based on their material self-interest. Political scientists have theorized that higher income and wealth could affect political participation by allowing human capital investments with downstream effects on the cost of participation (Verba, Schlozman, and Brady, 1995; Dawes et al., 2014; Denny and Doyle, 2008), or by boosting social status which in turn facilitates participation in political activities (Verba, Schlozman, and Brady, 1995; Wolfinger and Rosenstone, 1980). However, assessing the causal effect of changes to wealth or income is challenging. Economic status is correlated with a multitude of other factors – for example education or class background – that may determine opinions and political engagement. Separating the causal effect of wealth from these other channels therefore requires suitable experimental data.

In this paper, we exploit the randomized assignment of prizes in four Swedish lotteries to estimate causal effects of wealth on political participation and preferences. In each of our lotteries, some players were randomly assigned large monetary prizes. By comparing their long-run outcomes to those of ex ante identical players who did *not* win a large prize, our study design comes very close to replicating the conditions of a randomized experiment where the treatment consists of being assigned a large amount of wealth. The lottery data comprise over 400,000 people who in total won approximately one billion dollars.

We match our lottery data to detailed information about political participation and political

attitudes. To measure political participation, we use digitized records of individual-level turnout in elections to the Swedish national assembly and the European parliament. Because turnout is much higher in elections to the general assembly (91% in our sample) than the European parliament (54%), these two types of elections capture different margins of participation. To capture more active political participation, we use records from candidacy for political office at the local, regional and national level. Because our election data span many decades, we can measure participation for both the lottery players and their children as adults.

To measure attitudes, we sent a survey with detailed questions about political preferences, moral values, and beliefs to a subset of the lottery sample. The high response rate (69%) gave us a large sample of survey respondents (N = 3,362) who won prizes with a combined value of $277 million.

Prior to accessing the matched data, we pre-specified our main analyses in separate pre-analysis plans for the participation (Brännlund et al., 2020) and survey data analyses (Cesarini, Lindqvist, and Östling, 2016).[1] Any additional analyses in the paper that were not specified in the analysis plan are clearly marked as post hoc.

We estimate precise null effects of lottery wealth on all measures of political participation. Except for children's political candidacy (where the gradient is near zero), we strongly reject the cross-sectional gradients between income and political participation. An exception to the overall pattern of null results is that the effect on children's turnout is significantly *more negative* when parents are relatively poor, strengthening the conclusion that lack of turnout is not due to growing up with low-income parents. Our results do thus not lend support to theories that suggest a casual role for economic status on political participation.

Turning to attitudes, we find winners of larger prizes become more negative towards taxes on wealth, real estate and inheritances. Overall, we find little evidence of heterogeneous effects, but we do find suggestive evidence that effects are larger for winners who won closer in time to answering the survey. One interpretation of this finding consistent with the self-interested voter hypothesis is that effects fade over time as lottery wealth is dissipated and the self-interest motive weakened. We also compare the estimated effect to cross-sectional income and wealth gradients. We find the effect of lottery wealth on preferences for capital taxes to be of similar magnitude as the cross-sectional gradients. Our results are thus consistent with self-interest being a key explanation between the wealth gradient with respect to capital taxation.

The estimated effects on other political attitudes – privatization, redistribution and placement on the left-right scale – are not statistically significant. It might be tempting to conclude from these null results that lottery wealth only affects opinions related to a narrow self-interest, without inducing a wider attitudinal shift. Though zero effects cannot be ruled out, the pattern

[1]The plans can be publicly accessible via the URLs `https://osf.io/kzc6e` and `https://osf.io/t3qb5/`.

of results is generally consistent with lottery wealth inducing a limited shift toward increased support for policy proposals and parties associated with the political right.

We also do not find statisticatically significant effects on measures of beliefs that previous literature have identified as potential determinants of political attitudes, such as beliefs about the functioning of markets and belief in meritocracy. Finally, we find no evidence that lottery players' generalized trust is affected by a lottery win or that moral values change in a self-serving manner.

Our paper contributes to a quasi-experimental literature that studies the effects of economic shocks on political attitudes and voting behavior (surveyed in Margalit, 2019).[2] While we unaware of any previous paper that estimates the effect of lottery wealth on political participation, three previous papers study positive economic shocks in the form of lottery winnings (Doherty, Gerber, and Green, 2006; Peterson, 2015; Oswald and Powdthavee, 2014). In line with our results, Doherty, Gerber, and Green (2006) failed to detect any changes in overall political attitudes, but found clear evidence that players who won larger prizes became more hostile toward estate taxes. Peterson (2015) studied partisanship and found large lottery wins increase the propensity to register as Republican. We provide an in-depth comparison to the previous lottery studies in Section 6.4 below.[3]

The next section gives an overview of our lottery data and identification strategy. Section 3 presents our data on political participation and the results from the corresponding analyses are provided in Section 4. We turn to our survey in Section 5 and the results from the analyses of the survey data in Section 6. Section 7 concludes the paper.

[2]Tests of the self-interested voter hypothesis go beyond studies of economic shocks. For example, a number of quasi-experimental studies have found that targeted public spending increase political support among benefiting groups (e.g. Levitt and Snyder Jr., 1997; Pop-Eleches and Pop-Eleches, 2012; Elinder, Jordahl, and Poutvaara, 2015). Another approach is to exploit within-twin variation in wealth (Ahlskog and Brännlund, 2021). At a more general level, a literature spanning several disciplines have studied the malleability of political preferences. Examples of life events that have been shown to influence political preferences include: experiencing a recession during early adulthood (Giuliano and Spilimbergo, 2013); living in a communist regime (Alesina and Fuchs-Schündeln, 2007); participating in the Vietnam draft lottery (Erikson and Stoker, 2011); being exposed to a new curriculum (Cantoni et al., 2017); teaching disadvantaged children (Mo and Conn, 2018), and participating in financial markets (Jha and Shayo, 2019; Margalit and Shayo, 2021).

[3]Our study is also related to Bagues and Esteve-Volart (2016) who study province-level effects of winning the Spanish Christmas Lottery. Lottery winners are geographically clustered and winning provinces receive prizes corresponding to up to 3 percent of GDP. Bagues and Esteve-Volart (2016) find that support for political incumbents increase in winning provinces. The effect is larger when the incumbent is from a right-wing party, but the difference compared to left-wing incumbents is not statistically significant. Because lottery prizes have such a large aggregate economic effect at the province level, it is likely that different mechanisms are at play compared to studies of individual lottery winners.

2 The Lottery Data

We here describe the three lotteries to which we match data on political participation. The survey on political attitudes is matched to a subset of lottery players, described in Section 5.

2.1 The Lotteries

The first lottery, *Triss*, is a popular scratch-off lottery run by *Svenska Spel*, the Swedish government-owned gaming operator. We have data on two types of Triss prizes which qualify the winner to a daily TV show. At the show, winners of the Triss-Lumpsum prize (1994 to 2011) scratch a ticket which award them between $7,000 and $700,000. Triss-Monthly winners (1997 to 2011) win a monthly installment. The size ($1,400 to $7,000 per month) and duration (10 to 50 years) of the installments are determined by two separate and independently drawn tickets. To make the installments comparable to lump-sum prizes, we convert them to net-present value using a discount rate of 2 percent. All lottery prizes are net of taxes and have been inflation-adjusted to 2011 SEK and converted using the USD-SEK exchange rate at the end of 2011 (6.89 SEK per USD).

The third lottery, *Kombi*, is a monthly subscription lottery run by a company owned by the Swedish Social Democratic Party, the main political party in Sweden over the last 100 years. The administrative sample contains information on the number of lottery tickets bought by all Kombi participants (about 500,000 people) between 1998 and 2011 and large prizes (>1M SEK) won during this period.

The final lottery, *PLS*, are savings accounts in Swedish banks which paid interest but also gave the opportunity to win cash prizes. Our data includes information about all prizes won in this program between 1986 and 2003 and microfiche images with information about the account balance of all accounts participating in the draws between December 1986 and December 1994 (the "fiche period") and the account owner's personal identification number (PIN). Matching the prize-list data with the microfiche data enable us to identify PLS winners between 1986 and 2003 who held an account during the fiche period.

In our adult analyses, our basic estimation sample consists of lottery players who were above 18 years of age at the time of the lottery event. Our intergenerational analyses are conducted in a sample of players' children who were born or conceived before the time of the lottery event but below the age of 18.

2.2 Identification Strategy

Our identifying assumption is that lottery prizes are randomly assigned conditional on observed player characteristics. We estimate the long-run causal impact of lottery wealth by estimating

the following equation using ordinary least squares:

$$y_i = \alpha L_i + \mathbf{Z}_i\boldsymbol{\gamma} + \mathbf{X}_i\boldsymbol{\beta} + \varepsilon_i,$$

where y_i is an outcome variable measuring election turnout, political candidacy, or a survey reponse. L_i is the lottery prize (in \$100,000) awarded to individual i, and $\mathbf{Z}_i$ is a vector of baseline characteristics measured at the end of the year prior to the lottery event. The baseline characteristics are only included to increase precision of our estimates and differ somewhat between the adult and intergenerational analyses (Brännlund et al., 2020). Following our previous work, we assume that the effect is linear in lottery wealth, but we complement our main analyses with robustness checks omitting large prizes (see Lindqvist, Östling, and Cesarini, 2020 for further discussion of the linearity assumption). $\mathbf{X}_i$ is a set of indicator variables for groups of lottery players within which lottery prizes are randomly assigned.

The construction of the group identifiers $\mathbf{X}_i$ vary by lottery and closely mirrors the approach used in our previous studies. In the two Triss lotteries, players are assigned to the same group if they won the same type of prize (Lumpsum or Monthly) in the same year and under the same prize plan. The prize distribution is given by the prize plan, so conditional on the same prize plan the size of the lottery win is random.

In the Kombi lottery, we assign each large-prize winner to up to 100 matched controls to each large-prize winner in Kombi. For each winner, we identify all non-winning players whose sex, year of birth and number of tickets purchased in the month of win matches that of the winner. If the number of non-winners satisfying these criteria is less than 100, we retain them all as matched controls. If the number of suitable controls exceeds 100, we randomly sample 100 of them and retain them as the winner's matched controls.

In PLS, there were two types of prizes: fixed prizes and odds prizes. Odds prizes are multiples of the account balance (up to a maximal threshold). Following our previous work, we assign winners that won exactly one odds prize to the same group as winners that won exactly one prize (fixed or odds) in the same draw and had a similar account balance (using the same criteria as in our previous work, see p. 59-60 the Online Appendix of Cesarini et al., 2016). If an odds prize group constructed in this way contains fewer than five observations or contains a total prize sum of less than 100,000 SEK, we omit that group. All fixed-prized winners that are not used to construct the odds prize groups are assigned to the same group if they won exactly one fixed prize in the same draw.

The cell construction for the child sample is identical, except that the unit of observation is a child of a lottery-winning parent.

Because there are a few individuals that appear several times in the data, we report analytical standard errors that are clustered at the level of the individual. We also report non-parametric p-values that are constructed by permuting the distribution of prizes within groups

and re-estimating the main estimating equation 10,000 times (Young, 2018). Finally, in our main analyses of primary outcomes, we also report family-wise error rate-adjusted p-values using the free step-down resampling method of Westfall and Young (1993).

2.3 Exogeneity Tests

To verify that this holds also for the estimation sample of this study, we follow the pre-analysis plan and regress the lottery prizes on the vector of baseline characteristics. Conditional random assignment implies that, when the cell fixed effects are controlled for, we should not be able to reject the null that all of the coefficients of the covariates in the vector of baseline characteristics are zero.

Table A1 shows that we cannot reject the null of joint insignificance for the adult sample, thus supporting the assumption of conditional random assignment. However, as shown in Table A2, for two of the three different types of p-values imply we reject joint insignificance for the child sample. Though our previous studies on the same lottery data have consistently found evidence consistent conditional random assignment of lottery prizes, the amount won may be correlated with covariates by chance. In the event that random assignment would be rejected, our pre-analysis plan stated we would impose sample restriction in order to achieve covariate balance. The main driver of the rejection, it seems, is that family size is correlated with amount won in the PLS sample. Removing families with four children or more from the PLS sample implies we only reject random assignment when using analytical standard errors (p-value 0.034), but not when using any of the permutation-based p-values. We therefore chose to exclude PLS-families with four children or more from our estimation sample.

3 Data on Political Participation

3.1 Voter turnout

Starting in 1994, Swedish general elections have been held in September every four years (before 1994, the elections were also held in September, but every three years). The general elections determine the makeup of legislative bodies at three levels of administrative division: the national parliament, the county councils and the municipal assemblies. Each eligible voter can cast one ballot for each legislative body. Sweden also holds elections to the European Parliament in June every five years, most recently in May 2019. Before an election, all eligible voters are assigned to a voting district based on their region of residence. Our information about individual-level voting decisions from multiple national elections are based on district-level electoral rolls which lists the name and unique personal identification number (henceforth PIN) of each voter eligible

to participate in the election. On the rolls, election officials also manually record whether an individual voted. The PINs enable matching the information about eligibility and actual voting from the electoral rolls with other government registers and the lottery data. Lindgren and Oskarsson requested all rolls for the Swedish general elections (parliament, county and municipality) held in 1970, 1994 and 2010, as well as the European Parliament Election in 2009.[4] Since 2018, Statistics Sweden digitize all election rolls, allowing us to measure turnout in four general elections – held in 1970, 1994, 2010 and 2018 – and the two European Parliamentary elections in 2009 and 2019.

Even though each general election technically consists of three separate elections, one for each of the three legislative bodies elected, nearly all voters choose to either cast a ballot in either all three elections or to abstain from all three.[5] In what follows, we therefore adopt the convention of defining an individual as a voter in a general election if they cast a ballot for the national parliament.

Our pre-anaysis plan defined two primary outcomes for election turnout. *General Turnout* is an indicator equal to 1 for having voted in the first national parliamentary election after the lottery event for which we have digitized information. For example, for a player who participated in a lottery event in 1997 the variable is constructed using information from the digitized information about the 2010 election. *EU Turnout* in an indicator equal to 1 for having voted in the first election to the European Parliament held after the lottery event for which digitized turnout data is available (2009 or 2019). Both variables are set to missing for individuals whose PIN could not be identified on the electoral rolls.

3.2 Candidacy

Our second set of variables measures a much more time-consuming act of political participation: running for political office. These variables are derived from the Register of Nominated and Elected Candidates, which contains information about all nominated and elected candidates in the eleven parliamentary, county council, and municipal elections in the period 1982–2018. Using these data, we can generate the pre-specified indicator variable *Political Candidacy* equal to 1 for individuals nominated for political office (parliament, county council or municipal

[4]The rolls were digitized using procedures described on pp. 10-13 in the Online Appendix of Lindgren, Oskarsson, and Persson (2019). Lindgren, Oskarsson and Persson (2019) report that the procedure used allowed them to reliably classify the turnout of 96.5% of eligible voters in the 2010 parliamentary election. In a 1% random population sample whose turnout in 2010 was manually classified by Statistics Sweden through visual inspection of the rolls, the automated procedure generated a turnout variable that agreed with Statistics Sweden's classification in 99.7% of the cases.

[5]For example, in the digitized rolls for the 1994 election, 98.9% (99.3%) of individuals recorded as having voted in the general election are also recorded as having voted in the county council (municipal) elections. And conversely, 99.9% (99.9%) of individuals who voted in the county council (municipal) election are recorded as having voted in the general election.

council) in at least one election after the lottery event. Because the nomination process typically takes some time, we only include elections taking place the year after the lottery event or later. The variable is set to 0 for individuals who were Swedish citizens and at least 18 years old in the year prior to at least one election after the lottery event, but who did not run for political office. The variable is set to missing for individuals that were not both Swedish citizens and above age 18 the year prior to at least one election after the lottery event.

3.3 Sample Representativeness

Table A3 shows baseline pre-lottery characteristics for each adult lottery sample separately as well as for the pooled sample (with each lottery weighted by its share of the treatment variation). To gauge sample representativeness, we compare the pooled lottery sample to a representative sample matched on sex and age. Table A3 shows lottery players are less likely to have immigrated to Sweden and to hold a college degree, but somewhat more likely to have voted in the previous election (71.7% vs. 67.4%) or have run for political office (3.0% vs. 2.4%).

Table A4 shows a corresponding set of baseline characteristics for the children in our child sample, and for their parents. Children of lottery-playing parents have similar birth order, while the parents are less likely to be immigrants and college educated, but have somewhat higher incomes and political participation.

Another potential barrier to generalizability is that the impact of a change in lottery wealth may differ from the impact of an equivalently sized change in wealth from some other source (such as a change in house or stock prices). A common intuition is that lottery wealth is special in that it is often squandered. Our previous research has found little support for this intuition. In our Swedish samples, winning players invest a substantial share of wins in safe assets with modest but stable returns (Briggs et al., 2021); smooth their labor supply responses over a long time-horizon (Cesarini et al., 2017), and are more satisfied with their personal finances many years after winning (Lindqvist, Östling, and Cesarini, 2020). Still, it is conceivable that lottery wealth matter in ways different from other types of wealth such as, for example, appreciation of real estate values (cf. Ansell, 2014).

4 The Effect of Wealth on Political Participation

4.1 Adult analyses

As shown in Table 1, we estimate winning $100K increases turnout in national elections by 0.099 percentage points, decreases turnout in elections to the EU parliament by 0.630 percentage points, and decreases the probability for political candidacy by 0.064 percentage points. In

Table 1: Effects of Lottery Wealth on Political Participation (Adults)

	Turnout National	Turnout EU	Political Candidacy
Effect ($100K)	0.097	-0.627	-0.064
	(0.241)	(0.425)	(0.096)
p (analytical)	0.688	0.140	0.502
p (rand-c)	0.705	0.152	0.536
p (rand-t)	0.698	0.148	0.508
Avg. dep. var (%)	90.6	54.1	2.1
R^2	0.100	0.105	0.196
N	335,989	288,205	413,349

This table reports the treatment effect of $100K SEK on the three primary participation outcomes in the adult sample. All three outcomes are indicator variables. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual. The resampling-based p-values are obtained by simulating the distribution of coefficient estimates under the null hypothesis of zero treatment effects, as described in the main text.

line with our expectations listed in the pre-analysis plan, none of these effects are statistically significant.

We now turn to a set of pre-specified exploratory analyses. Table A5 shows the results when the estimation samples are split by income, education or previous voter turnout. We find lottery wealth reduces the propensity to vote in elections to the EU parliament for people who abstained from voting in the previous EU election, but there is no corrsponding difference at the national level. For players with a college degree, lottery wealth increases turnout in national elections, but decrease their propensity to run for political office. Notably, we find no heterogenous effects by income. Table A6 shows there is no evidece of heterogeneous treatment effects by lottery. Table A7 shows the effect over time, indicating more negative effects of lottery wealth in the third national election following the lottery win, but the effect is only borderline significant. Table A8 shows there is no clear evidence for non-linear effects.

To help readers interpret our treatment effect estimates, we benchmark them against disposable household income gradients estimated from observational data, following a procedure laid out in detail in the pre-analysis plan. In short, we match each player aged 25 or above in the estimation sample to 10 randomly chosen individuals from the general population of the same age and sex in the year of the lottery event. To reduce attenuation bias caused by transitory fluctuations in year-to-year income, we use the average disposable household income in the five years prior to the year of the lottery event. To make our lottery estimates comparable to the income gradients, we rescale the treatment-effect estimates into annuity equivalents using

the procedure in Cesarini et al. (2016, pp. 726-730). The basic idea is to calculate the annual payout that each lump-sum prize would sustain if it were annuitized over a 20-year period at an actuarially fair price and a real return of 2%. The left panel of Figure 1 shows the lottery-based estimates for both turnout and candidacy are much smaller than the corresponding gradients, suggesting the strong relationship between political participation and turnout income does not reflect a causal effect of income.

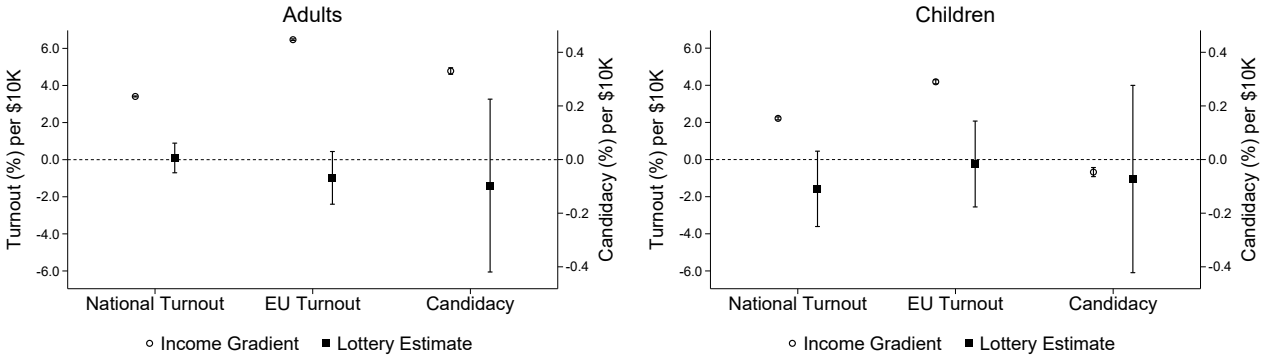

Figure 1: Benchmarking Participation Estimates: Income Gradients

4.2 Intergenerational Analyses

We now turn to intergenerational analyses of the effect of lottery wealth on the lottery winners children. Table 2 shows we estimate negative but statistically insignificant effects of lottery wealth on turnout and political candidacy. Our analyses of heterogenous effects in Table A9 indicates the effect is more negative when parents are poor, quite strongly suggesting the negative relationship between childhood poverty and turnout does not reflect a causal effect of low parental incomes. We further see more negative effects when children are older at the time of winning and when parents voted in the previous election.

The right panel of Figure 1 compares our lottery-based estimates to parental-income gradients. We clearly reject the gradient for turnout in both types of elections. For candidacy, both the gradient and the lottery-based estimate are close to zero and not statistically different from each other.

5 The Survey

The data on political attitudes come from a survey conducted on our behalf during the fall of 2016 by Statistics Sweden. Survey responses were combined with administrative variables and anonymized before Statistics Sweden delivered it to us. Note that lottery prizes were awarded

Table 2: Effects of Lottery Wealth on Political Participation (Children)

	Turnout National	Turnout EU	Political Candidacy
Effect ($100K)	-0.946	-0.144	-0.044
	(0.621)	(0.707)	(0.107)
p (analytical)	0.127	0.839	0.684
p (rand-c)	0.101	0.845	0.773
p (rand-t)	0.152	0.841	0.720
Avg. dep. var (%)	87.4	49.1	1.5
R^2	0.030	0.054	0.017
N	92,999	93,878	97,159

This table reports the treatment effect of $100K on the three primary participation outcomes in the child sample. All three outcomes are indicator variables. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual. The resampling-based p-values are obtained by simulating the distribution of coefficient estimates under the null hypothesis of zero treatment effects, as described in the main text.

between 1994 and 2011, so 5 to 22 years had past between the lottery event and the time of the survey.

5.1 Survey Design

We first identified a *Survey Population* from three administrative samples of lottery players discussed in Section 2.1, Triss-Lumpsum, Triss-Monthly and Kombi. A primary goal when selecting the survey population was to retain as much as possible of the lottery-prize variation.

When selecting the Survey Population, we started from the set of Triss lottery winners and large-prize winners in Kombi (prizes of 1M SEK or more). We then imposed a number of sample restrictions summarized in Table A1, the most important being that we excluded all winners who were above age 75 at the time of the survey (2016). We further dropped a smaller number of lottery players because we were uncertain about their identity, because basic socioeconomic characteristics were missing from the government registers, or because prizes were shared.[6] We also imposed a number of additional minor sample restrictions.

The sample restrictions left us with 259 large prizes from Kombi, 3,294 Triss-Lumpsum

[6] "Shared prizes" here refer to the ownership of the lottery ticket prior to the realization of the prize. Ownership of lottery tickets is never shared in Kombi, but about 7% of Triss prizes are co-owned by a set of friends, co-workers or relatives. The data provided to us from Svenska Spel include information on such co-ownership, and in principle these prizes could be used in estimation along with the non-shared prizes. However, because the amount won per person is lower for shared prizes and because we had a limited budget for the survey, we abstained from surveying winners of shared prizes.

prizes and 608 Triss-Monthly prizes. In the final step, Statistics Sweden first dropped individuals who were deceased or lacked an official Swedish address of residence in 2016. They then added four controls for each large-prize winner in Kombi to the Survey Population. The controls were randomly selected from the set of non-winning Kombi players who had the exact same sex, year of birth and number of tickets as the winner in the month of win. This leaves our Survey Population of 4,840 observations: 241 Kombi large-prize events and 964 (241×4) matched controls, 3,065 Triss-Lumpsum prizes and 570 Triss-Monthly prizes. Because a small number of individuals appear more than once in the data, the 4,840 observations correspond to 4,820 different individuals.

Statistics Sweden sent the survey via mail to all members of the Survey Population (see Figure A1 for the exact timeline). Along with the survey, we included an invitation letter, a return envelope, and a 100 SEK gift certificate. The invitation letter made no mention of lotteries.[7] The gift card was included to increase the willingness to return the survey. Subjects who did not return the survey were sent three reminders by mail, the last two of which included the survey. Statistics Sweden also contacted Triss-Monthly players who had failed to return a survey after the third reminder by telephone and asked them to return the mail-in survey. After the survey-data collection via mail had ended, Statistics Sweden tried to contact 501 randomly selected non-respondents by telephone. Subjects who answered the phone were invited to participate in an shortened version of the survey via telephone.

The total response rate, including 111 respondents that took part in the abbreviated telephone survey, was 69%. Because not all respondents answered all questions, the effective response rate varies between outcomes (between 63% and 68% for the primary outcomes). We refer to the respondents of the survey as the *Respondents Sample*. Table A4 shows the prize distribution and response rate separately for each lottery and for the pooled sample. Most prizes are relatively modest, but the largest prizes contribute most to the identifying variation. For example, if we drop the 662 prizes above $100,000, the overall variation in lottery prizes drops by 99 percent. Although our sample is small compared to previous studies using all available data on Swedish lottery winners, the identifying variation is about one third of that in Cesarini et al. (2016).

The pre-analysis plan pre-specified three diagnostic tests for the conditional exogeneity of lottery wealth. In the survey sample, a further potential threat to our identification strategy

[7]The final data set delivered to us contains subjects' survey responses and some basic socioeconomic variables from administrative registers. Statistics Sweden required that information about these registers be available to interested subjects, along with information about selection into the study. The cover letter therefore referred survey invitees interested in learning more to a website with additional information. The URL on each letter was unique and we therefore have data on which subjects accessed the website. Only six subjects accessed the website, so any bias arising from being aware that selection was based on having played the lottery is likely to be negligible.

compared to the participation sample is that lottery wealth influences survey participation. As reported in our previously published work based on the same survey (Lindqvist, Östling, and Cesarini, 2020; Östling, Cesarini, and Lindqvist, 2020), we pass all three tests. For completeness, the three tests are reproduced in Table A5, A6 and A7 in the Appendix.

5.2 Outcome Variables

The pre-analysis plan specified seven primary outcomes. The definitions of our primary outcomes are shown in Table A2 and their pairwise correlations in Table A3. Three primary outcomes (1, 3 and 4), as well as three additional outcomes, are indexes derived from subjects' responses to a list of 21 policy proposals included in our survey. Respondents were asked to rate each proposal on a 5-point Likert scale ranging from "Very Poor Proposal" to "Very Good Proposal" which we assign the numerical values 1 to 5. The 21 survey items are listed in Table 4 and their pairwise correlations are shown in Figure A2.

Our first two primary outcomes concern capital taxation, which we hypothesized would be most directly influenced by wealth shocks. Primary Outcome 1 is based on questions about reintroducing bequest, wealth and property taxes. These taxes were abolished in Sweden in 2005-2008, but a potential reintroduction of these taxes has remained on the political agenda. The index is also based on a question about whether the capital income tax should be increased. Responses to the questions about taxing the capital stock, i.e. the questions about bequest, wealth and real estate taxes are strongly correlated (0.55 to 0.68), whereas the correlations between attitudes to these taxes and the attitude to taxing the flow of capital income are substantially smaller (0.27 to 0.37).

Primary Outcome 2 is derived from two questions about the preferred tax rates on labor and capital income. Because lottery winners increase their capital income but reduce their labor supply (Cesarini et al., 2017), a self-interested winner should prefer a lower tax on capital income, but a higher tax on labor income. We therefore construct the outcome variable by first subtracting the respondent's ideal capital income tax from their ideal labor income tax, and then calculating the sample percentile of this difference.

The third to fifth primary outcomes are intended to capture broader attitudes toward economic policy. Primary Outcome 3 measures individual attitudes toward the appropriate scope and size of government whereas Primary Outcome 4 measures attitudes toward redistribution. Primary Outcome 5 is based on a single question adapted from the World Values Surveys that asks the respondent to position her political views on a Likert scale from 0 ("Left") to 10 ("Right").

Primary Outcome 6 measures belief in meritocracy using the following question adapted from the World Values Surveys: "To what extent do you think success in life is determined

by luck and connections rather than hard work?". Previous research has shown that stronger beliefs in the importance of luck in determining individual success are positively correlated with preferences for redistribution (Fong, 2001; Alesina, Glaeser, and Sacerdote, 2001; Alesina and Angeletos, 2005). Because a lottery win represents a stroke of luck, it might seem natural to expect lottery winners to increase their belief in the importance of luck. This could in turn lead to a stronger preference for redistribution. Models of motivated reasoning (e.g. Benabou and Tirole, 2006), however, suggest that beliefs may change in the opposite direction and two previous papers have found support for motivated reasoning in related settings (Di Tella, Galiani, and Schargrodsky, 2007; Andersen et al., 2020).

Primary Outcome 7 is motivated by literature about the relationship between affluence and ethical behavior (e.g. Östling, 2009; Piff et al., 2012; Andreoni, Nikiforakis, and Stoop, 2017) and is specifically designed to test an hypothesis put forth by Östling (2009). In his model, unanticipated wealth shocks induce increased consumption of "immoral goods" and this change in consumption is accompanied by a self-serving softening of moral attitudes toward the consumption of such goods. To measure moral values we combine seven item-level responses adapted from the World Values Survey about the moral defensibility of different behaviors.

Additional Outcomes (Post Hoc). Based on the policy items not included in the construction of primary outcomes we code three additional indexes about immigration, environmentalism and globalization. Table 4 lists which questions are included in the construction of each index. A further set of additional outcomes are three related measures of beliefs about the nature of reality: belief in free will (based on three questions), belief in fate, and belief in unpredictability. These five questions are taken from the 27-item battery of questions used by Paulhus and Carey (2010). Paulhus and Carey's factor analysis uncovered four distinct factors which they label belief in free will, fatalistic determinism, scientific determinism and unpredictability. In our survey, we included one question with a strong factor loading for each dimension, and two more questions that load heavily on the free will dimension. Third, motivated by Di Tella, Galiani, and Schargrodsky (2007), we use three questions to construct an index of pro-market beliefs. The index is coded so that higher values denote more trust, a stronger belief that money is important for happiness and a stronger belief that one can be successful without the support of a large group.[8]

[8]In Di Tella, Galiani, and Schargrodsky (2007), an individual's score on the index of market beliefs was derived from responses to four questions and interpreted as a measure of the extent to which the respondent holds beliefs conducive to capitalism. We only include the three items on which Di Tella, Galiani, and Schargrodsky (2007) found evidence of a treatment effect.

5.3 Representativeness

How representative are the surveyed lottery players of the Swedish population? The top panel of Table A8 compares pre-lottery baseline characteristics in the Respondents Sample and the Survey Population with a representative sample of Swedish adults. To avoid differences related to sex and age, we re-weight the representative sample to match the sex- and age distribution in the Respondents Sample. The starkest difference shown in Table A8 is that lottery players are more likely to be born in Sweden (92.4% versus 83.8%). But because the representative sample was drawn in 2010 and the share foreign-born increased substantially in the lottery years, the difference in the share foreign-born understates the representativeness of the lottery players at the time of winning. Lottery players are also less likely to have attended college and have higher labor incomes on average, though differences are modest in both cases (25.8% versus 30.1% and $35,000 versus $32,000, respectively). Players are similar to the Swedish population in terms of marital status and number of children residing in their household.

The bottom panel of Table A8 compares political attitudes of lottery players to the nationally representative SOM survey conducted by University of Gothenburg during the fall of 2016. Table A8 only includes questions that were identically or very similarly worded in the two surveys. Because lottery players are surveyed after winning the lottery, the table also includes a separate column reporting the average only for those who won small prizes (below $20K). Table A8 reveals that the political attitudes of our pooled sample are overall similar to the nationally representative survey. One exception is that Kombi players are more likely to support left-wing parties, which is unsurprising given that the lottery is owned by the largest left-wing party (the Social Democrats). Another exception is that lottery players also appears to be somewhat less supportive of reducing the size of the government. Overall, the similarity in baseline characteristics and political attitudes is reassuring, though we cannot rule out that players who select into the lottery differ from the population in unobservables in ways that could impair the generalizability of our findings.

6 The Effect of Wealth on Political Preferences

6.1 Main Results

Table 3 displays the estimated long-run treatment effects of lottery wealth on each of the primary outcomes (scaled so that higher values are more right-wing or more self-serving for winners). The most immediate hypothesized effect of a wealth shock is that winners should favor lower taxes on capital. We indeed find that the effect on the capital taxation index is statistically distinguishable from zero; the index increases by 0.044 SD units per $100,000 won.

Post hoc analyses reported in Table 4 shows the effects on the sub-components of the capital taxation index. The effect is largest for the wealth tax question, but there are also statistically significant effects for attitudes toward taxes on inheritances and real estate. Attitudes toward the capital income tax does not seem to be significantly affected by winning the lottery. Given that we find no effect on attitudes to the capital income tax, it is unsurprising that our second primary outcome, the difference in preferred tax rates on capital and labor income, is not significantly affected by lottery wealth.

The fact that winners become more negative toward taxes on wealth, but do not change attitudes to income taxation, might be related to how different types of taxes are perceived. In Sweden, income taxes are typically withheld by employers, whereas taxes on wealth, property and inheritances constituted annual payments, potentially making them more salient – especially after winning a lottery prize (Cabral and Hoxby, 2012). An endowment effect (Kahneman, Knetsch, and Thaler, 1990) might also be at play: taxation of existing lottery wealth (or property bought from it) may be perceived more negatively than taxation of future income flows. The strength of the self-interest motive could also matter. Table A7 shows that a \$100K lumpsum prize increases the winner's registered net wealth by on average \$53,523. For the same subsample (lumpsum winners with observable wealth), capital income the year following the win increases by \$1,144, whereas labor income decreases by \$1,216. For a winner of a \$100K prize, the capital income tax would increase by approximately \$343, whereas the labor income tax would fall by \$365. If the old Swedish wealth tax was reintroduced, however, a \$100K winner would pay up to \$803 in wealth tax.[9]

[9]We base these calculations on the Swedish wealth tax in 2006 that was 1.5 percent for wealth exceeding 1.5 million SEK, the current capital income tax of 30 percent for interest rate payments and dividends, and the current marginal labor income tax rate for median earners of approximately 30 percent.

Table 3: Effects of Lottery Wealth on Primary Outcomes

	Taxation		Political Attitudes			Beliefs and Values	
	Capital Taxation	Capital vs Labor	Public vs Private	Redistri-bution	Left-Right Placement	Meritocratic Beliefs	Moral Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect ($100K)	0.044	0.006	0.009	-0.001	0.022	-0.028	0.005
SE	(0.016)	(0.015)	(0.017)	(0.016)	(0.015)	0.018	0.015
p (analytical)	0.005	0.694	0.584	0.936	0.132	0.107	0.734
p (resampling)	0.003	0.704	0.542	0.931	0.138	0.056	0.715
FWER p	0.023	0.917	0.917	0.932	0.496	0.278	0.917
N	3227	3029	3204	3256	3219	3308	3180
Higher value implies...	Lower taxes on capital		More "right-wing" attitudes			Believe more in hard work	Self-serving for the rich

This table reports the treatment effect of $100K on the seven primary outcomes. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual. The resampling-based p-values are obtained by simulating the distribution of coefficient estimates under the null hypothesis of zero treatment effects, as described in the main text. The family-wise error rate (FWER) is calculated using the free step-down resampling method of Westfall and Young (1993). All outcomes are measured in SD units. Higher values of outcome variables indicate attitudes that are self-serving for large-prize winners.

We find little evidence of a broader attitudinal shift following lottery wins. As shown in Table 3, the estimated effects on attitudes toward redistribution (Primary Outcome 3) and private ownership (Primary Outcome 4) are close to zero (0.009 and -0.001 SD units per $100,000 won). The null results for these two indexes are not masked by counteracting effects on the underlying survey items (shown in Table 4). Lottery wealth does tend to push winners to identify more with the political right (Primary Outcome 5), 0.022 SD units per $100,000 won, but the effect is not statistically distinguishable from zero. We do not find that large-prize winners self-servingly adapt beliefs (Primary Outcome 6). To the contrary, Table 3 shows large-prize winners believe less in meritocracy (-0.028 SD units per $100,000 won). Though this effect is statistically insignificant, our 95 percent confidence interval allows us to rule out all but tiny changes the opposite direction. Hence, unlike Di Tella, Galiani, and Schargrodsky (2007) and Andersen et al. (2020), we find no evidence of motivated reasoning in the sense that lottery winners reduce their belief in the importance of luck for economic success. Finally, we find no evidence that lottery winners self-servingly adjust moral values (Primary Outcome 7).

The pre-analysis plan specified two robustness tests. The first test adjusts for selective non-response by weighting respondents to the telephone survey to match the share of mail-survey non-respondents. This implies that we weigh each of the 111 telephone respondents by $1,589/111 = 14.3$ to account for the 1,589 non-respondents in the mail-in survey. We did

Table 4: Effects of Lottery Wealth on Attitudes to Policy Proposals (Post Hoc)

	Effect ($100K)	SE		Effect ($100K)	SE
	(1)	(2)		(3)	(4)
Capital Taxation Index	0.044	(0.016)	Public vs Private Index	0.009	(0.017)
Real Estate Tax*	-0.030	(0.015)	Smaller Government	-0.015	(0.017)
Wealth Tax*	-0.057	(0.014)	Reduce Labor Tax	0.004	(0.018)
Inheritance Tax*	-0.036	(0.015)	Privatize	0.029	(0.017)
Reduce Capital Income Tax	0.009	(0.017)	More Private Care	0.012	(0.016)
			Prohibit Profits	0.001*	(0.016)
Redistribution Index	-0.001	(0.016)	Immigration Index	-0.008	(0.015)
Reduce Inequality*	-0.021	(0.017)	Language Test*	0.025	(0.015)
Rural Support*	-0.011	(0.019)	Reduce Foreign Aid*	-0.010	(0.017)
Six-hour Work Day*	0.023	(0.016)	Fewer Refugees*	0.006	(0.015)
Gender Equality*	0.005	(0.016)			
Environment Index	0.004	(0.018)	Globalization Index	0.023	(0.016)
Environment Investments	0.006	(0.017)	Leave the EU*	-0.006	(0.016)
Reduce CO2	0.000	(0.019)	Join NATO	0.022	(0.016)
			Free Trade	0.015	(0.016)

This table reports the treatment effect of $100K on the average response to the 21 policy proposals included in the survey and the outcome indexes constructed from these questions. A higher value indicates stronger support for the listed policy proposal; the complete wording of the questions can be found in the Appendix. Questions marked with asterisks have been reverse-coded when constructing the indexes. All outcomes are measured in SD units. We control for baseline controls measured at t = −1 and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

not ask the questions about moral values in the telephone survey. Table A9 shows that this re-weightening results in a larger effect on left-right placement, but similar estimates for the other outcomes. The larger estimate for left-right placement is largely due to the high weight given to a few large-prize winners in the telephone survey. For example, excluding the largest winner in the telephone survey reduces the estimated coefficient from 0.070 to 0.052.

In our second pre-specified robustness test (reported in Table A9), we drop prizes above 4 million SEK (approximately $580,000). Dropping large prizes results in larger standard errors, but also a somewhat smaller effect on the capital taxation index. The fact that the point estimate is smaller suggests that the per-dollar effect is larger for large prizes. To further explore such non-linearities, in post hoc analyses we varied the threshold above which prizes were omitted. Figure A3 in the Appendix shows that the estimate for the capital taxation index is particularly sensitive to setting the threshold at 4 or 5 million SEK. In additional post

hoc analyses, we re-estimated the effect on the capital taxation index using categorical dummy variables for prize amount. The results are shown in Figure A4 and suggests that effects are approximately linear in prize size.

In the analyses discussed above, we have treated survey responses as if they measured on an interval scale, although all primary outcomes except the question about capital and labor income taxes are based on ordinal Likert scales. To analyze whether our results are sensitive to this assumption, Figure A5-A7 show the results from post hoc analyzes with binary variables for all possible cutoffs of the underlying response scale. For example, Figure A5 shows there is a statistically significant effect on attitudes to the wealth tax irrespective of how the binary outcome variable is constructed. The estimated effects for the questions about real estate and inheritance taxation always have the same sign regardless of how the cutoff is chosen, but are not always statistically significant. For the questions underlying the other primary outcomes, estimates are almost without exception close to zero and statistically insignificant.

6.2 Heterogeneous Effects

The pre-registered analyses of heterogeneous effects are shown in Figure A8 and Table A10 in the Appendix. We stratify the sample by years-since-win (before or after 2005), sex, age-at-win (below or above median), pre-lottery income (below or above median) and type of prize (Triss-Monthly vs Triss-Lumpsum). Overall, there is little evidence of heterogeneous effects. Among the 35 different tests of equal effects reported in Table A10, only four are nominally significant at the 5 percent level, three of which indicate a stronger shift toward the political right among high-income winners. Notably, mode of payment does not seem to affect the effect of lottery wins in the Triss lottery. Because participants in the Kombi lottery are selected on political support for the Social Democrats, in post hoc analyses we analyze this subsample separately. Effect sizes are generally somewhat larger for Kombi players, but it is only the effect on the capital taxation index that is statistically different from zero at the 5 percent significance level (the effect size is 0.098, $p = 0.047$).

The pre-registered analyses of heterogeneous effects also show that effects tend to be larger for more recent winners. Because we only survey winners at one point in time, we cannot rule out unobserved heterogeneity correlated with the year of winning. With this caveat in mind, we perform post hoc analyses to further explore heterogeneity with respect to time since win by splitting the sample into six categories depending on year of win. The left panel of Figure 2 shows the effect on the capital taxation index appears to fade with time since the lottery event. A linear extrapolation of the time trend (the dashed line) suggests the effect would have been 0.116 SD units per \$100K if winners had been surveyed in the year of winning. Figure A10 in the Appendix shows the time trend is similar for the index subcomponents regarding taxation

of real estate, inheritances and wealth, while there is no apparent time trend for the capital income tax. Figure A9 shows the tendency for effect sizes to weaken with time is present for all other primary outcomes except meritocratic beliefs.

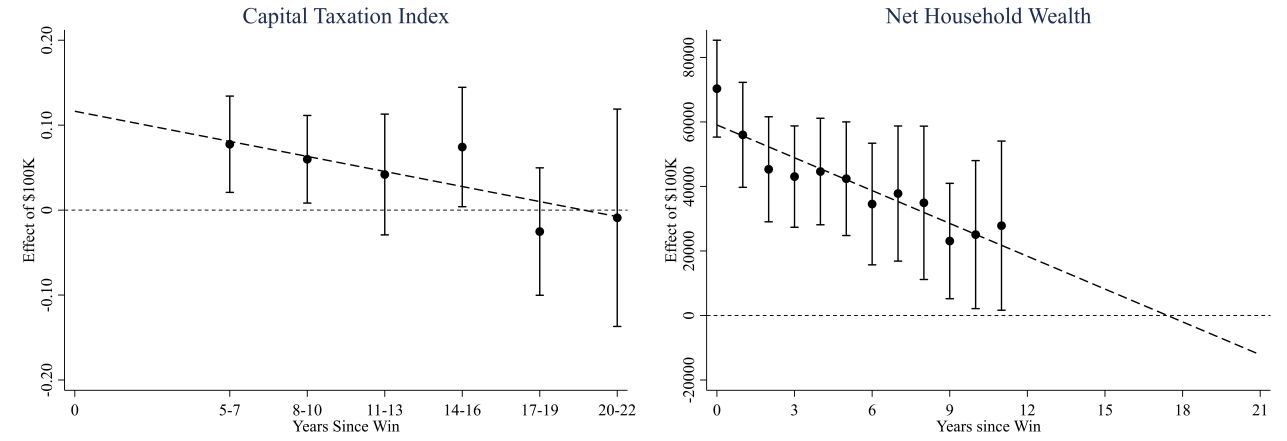

Figure 2: Treatment-Effect Heterogeneity by Year Won (Post Hoc)

The left panel depicts estimates from post hoc analyses of treatment-effect heterogeneity by year won for the capital taxation index. The dashed line is based on a regression of the treatment-effect estimates weighting each point in proportion to the inverse of the variance of the estimate. The right panel shows the estimated effect of lottery wealth on registered household wealth in the Swedish Wealth Registry. The sample is restricted to lumpsum prize-winners and only includes winners for which wealth could be observed in the registry (wealth recorded between 1999 and 2007).

Because lottery winners spend down lottery winnings over time, more recent winners retain more lottery wealth at the time of the survey. To illustrate, the right panel of Figure 2 shows the effect of lump-sum lottery prizes on net wealth by year since the lottery event. Because data on household wealth is only available between 1999 and 2007, the sample size is limited, but the point estimates are similar to what we find for the full administrative lottery data set (Cesarini et al., 2016). After 10 years, about 20 percent of a lottery prize shows up in the Wealth Registry. Because not all changes in wealth are captured by the Wealth Registry (e.g. cars, paintings or home improvements), this is a lower bound of the amount of remaining lottery wealth.[10] However, if taxes on wealth, inheritance and real estate were to be reintroduced, the wealth reported in the Wealth Registry is what is likely to be taxed. A linear extrapolation based on the right panel of Figure 2 suggests that (taxable) lottery wealth is down to zero after approximately 20 years. If we assume that 5 percent of a lottery prize is consumed or invested in non-registered assets every year, we can multiply the lottery wealth variable by $\max\{0, 1 - 0.05t\}$ (where t is the number of years that passed between winning and the survey) and re-estimate the effect on the capital taxation index to get the effect of current

[10]Real estate is included in the registry, but not all home improvements are likely to affect the valuation of the home that is reported in the wealth registry.

wealth. Doing so, we get an estimate of 0.113 SD units per $100K, which corresponds closely to the linear extrapolation based on the left panel of Figure 2 (0.116). The similarity of these two crude estimates could be a coincidence, but it is consistent with a simple static model in which attitudes to capital taxes are proportional to the current level of taxable wealth.

6.3 Additional Outcomes

We now turn to some post hoc analyses of additional outcomes. Table 4 shows that the estimated effect of lottery wealth on our policy indexes for environment, immigration and globalization are all statistically insignificant. The pattern of null results holds also for the response items underlying the indexes. In fact, among the 21 policy questions in Table 4, only the questions about wealth, property and inheritance taxes are nominally significant at the five percent level and only the effect on the wealth tax question survives adjustment for multiple hypothesis testing across all 21 outcomes (adjusted $p = 0.006$).

Even if the effects on individual questions are all statistically insignificant, they may follow a distinctive pattern. Suppose voters are constrained to maintain an ideological belief system that is internally consistent (Converse, 1964). A change in attitudes toward the wealth tax may then require changing attitudes on related questions. To gauge whether the pattern of responses is consistent with such belief-updating constraints, in post hoc analyses we test whether the estimated effects on the questions in Table 4 vary depending on their respective correlation with the wealth tax question. Specifically, we regress each of the policy questions (except the wealth tax question) on the wealth tax question using data on small-prize winners only (below $20K), thus obtaining a gradient between the response to each question and attitudes toward the wealth tax. We then compute the "predicted" effect for each question by multiplying the gradient with the estimated effect of lottery wealth on the wealth tax question (i.e., -0.057). If we interpret the gradients in the first step as casual, the predicted effects equal the true wealth effects if attitudes were mediated only through attitudes toward the wealth tax. Figure 3 shows predicted and estimated effects are positively correlated ($\sigma = 0.637, p = 0.003$), although the correlation is weaker if the components of the capital taxation index (indicated by hollow circles) are excluded ($\sigma = 0.368, p = 0.147$). Most policy questions are in the lower-left or upper-right quadrants, i.e. the sign of the estimated effect correspond to the sign of the predicted effect. There are a few questions that stand out from this pattern, but only one does so significantly: Winners of larger lottery prizes are more positive towards implementing a six-hour work day, whereas the predicted effect is the opposite (because it is respondents on the political left that tend to be in favor of a six-hour work day). Another question that deviates from the overall pattern is the question about attitudes to government size. Winners of large lottery prizes are predicted to prefer a smaller government, whereas the estimated wealth effect is the opposite;

winners of large amounts tend to favor a bigger government. Both of these deviations from the overall pattern are possible to rationalize. Large-prize winners reduce their labor supply more (Cesarini et al., 2017), which might have made them more favorable toward working fewer hours. Similarly, winning the lottery may increase demand for public goods such as theaters, parks, etc., thereby changing preferences for government spending.

Overall, the results in Figure 3 shows that the effects of lottery wealth follow a similar pattern as the correlations between attitudes to the wealth tax and the other questions. This suggests that winning the lottery may result in a broader attitudinal shift induced by a change in self-interest, but that the effect sizes are too small to be statistically detected. For most of the outcomes the predicted effect sizes are around +/- 0.01 SD units per $100K won, which is too small to be detected with the current study design.

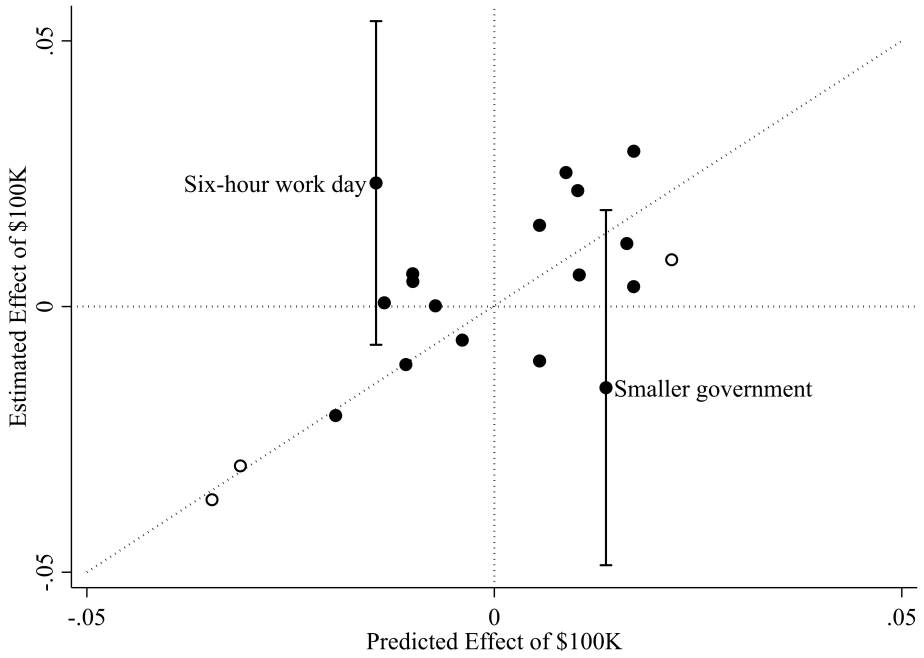

Figure 3: Predicted vs Estimated Effects on Attitudes to Policy Proposals (Post Hoc)
This figure displays the estimated effects of lottery wealth on 20 different policy proposals against the predicted effect under the assumption that the effect is completely mediated by the effect on attitudes to reintroducing the wealth tax. The dotted lines indicate predicted/estimated effects of 0 and a 45 degree line showing where the predicted effect equals the estimated effect. Hollow circles indicate the questions that are included in the capital taxation index.

In post hoc analyses, we also investigated whether lottery wealth affects support for political parties, using a question in our survey about which political party respondents identified most closely with. The results shown in Table 5 are broadly consistent with increased support for the political right. Lottery wealth increases support the Center Party (to the right) and reduces support for the Social Democrats (to the left) and the Christian Democrats (to the right),

Table 5: Effects of Lottery Wealth on Political Partisanship (Post Hoc)

	Left of Center			Right of Center				Nationalist
	Left Party	Social Democrats	Green Party	Center Party	Liberal Party	Moderate Party	Christian Democrats	Sweden Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect ($100K)	0.040	-1.384	0.175	1.324	0.029	0.311	-0.336	-0.407
SE	(0.301)	(0.696)	(0.222)	(0.526)	(0.399)	(0.710)	(0.134)	(0.542)
p (analytical)	0.895	0.047	0.432	0.012	0.941	0.662	0.012	0.453
p (resampling)	0.904	0.064	0.452	0.001	0.927	0.658	0.072	0.447
Support (%)	5.74	40.07	2.55	4.50	4.06	23.2	1.58	12.9
N	2,977	2,977	2,977	2,977	2,977	2,977	2,977	2,977

This table reports the treatment effect of $100K on support for political parties. The outcome is measure in percentages, so a coefficient of 1.00 means that $100K increases the support for the party by one percentage point. We control for baseline controls measured at t $= -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

though the latter two effects are not statistically significant at the 5 percent level based on the resampling-based p-values. If we instead use an indicator for supporting either of the four parties to the right as the dependent variable, we estimate an increase in the support of right-wing parties of 1.7 percentage points per $100K won ($p = 0.035$). If the nationalist party Sweden Democrats is included among the right-wing parties, the estimated effect is instead 1.1 percentage points ($p = 0.127$).[11]

Table 6 presents post hoc analyses further exploring the effect of wealth on beliefs. The first column shows the effect on the index of market beliefs similar to the index used by Di Tella, Galiani, and Schargrodsky (2007). There is no effect on this index or its subcomponents. It is noteworthy that there is no effect on generalized trust, which is one component of the index, despite previous studies showing not only a positive correlation between income and trust (Alesina and La Ferrara, 2002), but also that the relationship is causal (Ananyev and Guriev, 2019). Table 6 also shows the effect on metaphysical beliefs related to free will and determinism. We included these items in our survey to study whether winning the lottery might have wider implications on winner's perception of the world. The estimated effects are very close to zero for all three measures of metaphysical beliefs.

[11]In a related paper, Bagues and Esteve-Volart (2016) find that support for the incumbent party increases in provinces that win more in the Spanish Christmas Lottery. Table 5 shows no increased support for the incumbent parties in the national government at the time of the survey (Social Democrats and Green Party). We also do not find that support for the incumbent party at the time of the lottery win (the Social Democrats between 1994 and 2006, and a coalition of the four right-wing parties up until 1994 and from 2006 to 2014).

Table 6: Effects of Lottery Wealth on Beliefs (Post Hoc)

	Market Beliefs				Metaphysical Beliefs		
	Index	Trust	Money	Teamwork	Free will	Fate	Unpredictability
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect ($100K)	0.008	-0.008	0.011	0.012	0.000	0.013	-0.016
SE	(0.017)	(0.016)	(0.017)	(0.017)	(0.018)	(0.016)	(0.015)
p (analytical)	0.605	0.612	0.533	0.494	0.997	0.419	0.294
p (resampling)	0.572	0.586	0.488	0.457	0.997	0.420	0.301
N	3,215	3,211	3,210	3,201	3,133	3,132	3,134

This table reports the treatment effect of $100K on an index of market beliefs and its subcomponent, as well as questions about metaphysical beliefs. All outcomes are measured in SD units. We control for baseline controls measured at t $= -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

6.4 Benchmarking Effect Sizes

In order to assess the magnitude of the estimated effects of lottery wealth, we compare our lottery estimates to cross-sectional income gradients and gradients between our primary outcomes and demographic characteristics, as well as to estimates from previous lottery studies.

6.4.1 Income Gradients

In the pre-analysis plan we expressed our intention to benchmark our lottery-based estimates to household-income gradients. Following the plan, we convert lottery prizes to the annual payouts they would generate if they were annuitized over a 20-year period. For example, a $100,000 prize corresponds to an increase in net annual income of $5,996. The top row in Table 7 shows the results when our lottery estimates are re-scaled accordingly. We compare these annuity-rescaled effects to income gradients estimated using average disposable household income over the period 2004-2014, controlling for sex, a fourth-order polynomial in age and sex-by-age interactions. Annual income is left-censored at $6,000 before calculating average disposable income. Because income is affected by winning the lottery (Cesarini et al., 2017), we estimate the gradients only for respondents who won prizes below $20K.

Table A8 shows the gradients between all primary outcomes and income are positive, although the gradient for moral values is weaker and not statistically significant. We can only reject that our annuity-rescaled re-scaled treatment effects are equal to the income gradients for two outcomes, the redistribution index and meritocratic beliefs. For these two outcomes, we estimate negative treatment effects whereas the cross-sectional gradients are strongly positive.

Table 7: Comparison to Household Income Gradients

	Taxation		Political Attitudes			Beliefs and Values	
	Capital Taxation	Capital vs Labor	Public vs Private	Redistri-bution	Left-Right Placement	Meritocratic Beliefs	Moral Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect ($10K)	0.073	0.010	0.016	-0.002	0.037	-0.047	0.008
SE	(0.026)	(0.025)	(0.028)	(0.027)	(0.025)	0.029	(0.025)
Income ($10K)	0.035	0.042	0.038	0.062	0.056	0.042	0.013
SE	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
p equal	0.169	0.213	0.457	0.021	0.458	0.003	0.873

This table compares the effect of lottery wealth to household-income gradients estimated using small-prize winners (below $20K). Treatment effects are re-scaled assuming lottery prizes are annuitized over 20 years at a 2 percent real rate. Gradients are estimated using average annual household disposable income between 2004 and 2014 (left censored at $6K) controlling controlling flexibly for age and sex. "p equal" refers to the p-value obtained from a Wald test that the lottery estimate and the gradient estimate are equal. Standard errors are clustered at the level of the individual.

One concern with the comparison to household-income gradients is that wealth and income are likely to impact especially the first two primary outcomes differently – attitudes to capital taxes are more closely tied to wealth than income, and income taxes more closely tied to income. However, because wealth is measured almost ten years prior to the survey and lottery wealth disspates over time, both wealth gradients and our lottery estimates need to be rescaled to make an appropriate comparison. We report the results from this post-hoc comparison in the Appendix. The results show, after appropriate rescaling, we cannot statistically reject that lottery estimates and wealth gradients are equal for any of our primary outcomes.

6.4.2 Demographic Differences

As an additional benchmark, in post hoc analyses we compare our estimates to differences in political attitudes between demographic groups. In our data, females, older respondents and respondents living outside metropolitan areas are more left-leaning for all primary outcomes, except that females and older respondents tend to believe more in meritocracy. Figure 4 compares the effect of $100K in lottery wealth to differences between these demographic groups for all primary outcomes. For the capital taxation index, the estimated effect of $100K (0.044) is smaller than the difference between men and women, about half the size of the difference between old and young (defined as above/below the median age in the sample) and about a third of the difference between respondents living in metropolitan regions compared to the rest of the country. If we instead were to use the rescaled lottery estimate from Section 6.2 (0.113), the effect of $100K in lottery wealth would be larger than the differences between the sexes,

about the same as the difference between the young and the old, and roughly two thirds of the difference due to location.

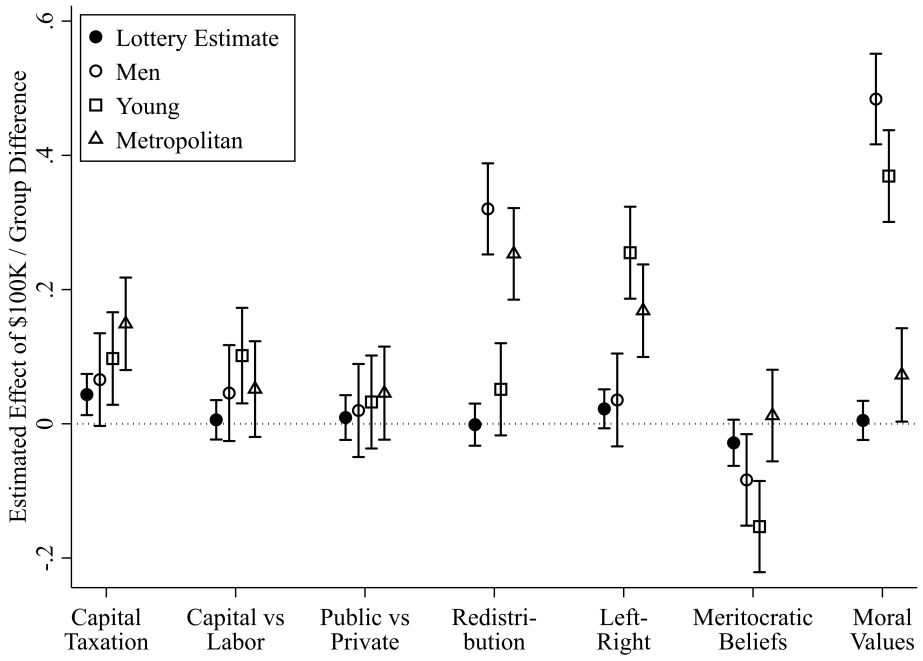

Figure 4: Comparison between Causal Estimates and Group Differences (Post Hoc)
This figure displays the estimated effects of lottery wealth for the primary outcomes and compares them to the average difference between men and women, young and old, and the difference between respondents living in metropolitan regions (Stockholm-Uppsala, Västra Götaland or Skåne) and outside these regions.

We also compare the effect of lottery wealth to the difference in average attitudes between supporters of left- and right-wing political parties (excluding the nationalistic party in this comparison). Compared to party affiliation, the effect of lottery wealth is small. For example, the left-right difference for the capital taxation index is 0.99 SD units, implying that changing an average left-wing supporter's attitude to capital taxation to that of an average right-wing supporter requires a lottery prize of $2.3M, or $880,000 if we use the rescaled estimate from Section 6.2 (in both cases subject to the caveat that effect sizes can be linearly extrapolated outside the observed prize range).

6.4.3 Previous Lottery Studies

We now compare our estimates to three previous papers on lottery winners' political attitudes. The study most closely related to ours is Doherty, Gerber, and Green (2006) who surveyed 342 U.S. lottery winners. All winners participated in the same lottery, but some purchased tickets individually and others as a part of a group. The prizes ranged from $48K to $15.1M and were awarded between 1983 and 2000; winners were surveyed in 2002 and prize amounts

are expressed in 2002 dollars. Lottery prizes were paid out as annual installments over twenty years. The individual prizes in their data were substantially larger than the prizes in our lotteries: 83 percent of prizes were larger than $1M whereas only 2 percent of our prizes are larger than $1M. The group prizes are more similar in magnitude to the prizes in our data (7 percent above $1M). Identification in Doherty et al (2006) relies on comparing winners of different amounts, but for group prizes part of the variation is due to the size of the group splitting the prize. Doherty, Gerber, and Green (2006) report a statistically significant positive effect of lottery wealth on support for the elimination of the estate tax, but no statistically significant effects on five other economic-political questions. Because they only report ordered probit estimates, it is difficult to make a direct quantitative comparison to our findings. To make a crude comparison, we use descriptive statistics on attitudes toward the estate tax by prize distribution quintiles reported in the working paper version of Doherty, Gerber, and Green (2006). We set all prizes in each quintile equal to the midpoint of the corresponding prize range. The exception is the top quintile, for which the maximum prize is not stated. As the general prize level is lower for the group prizes, we set the maximum individual prize to $8M and the maximum group prize to $1M, thereby generating different top quintile midpoints for each type of prize. The resulting average prize is very similar to that reported in the paper ($1.8M). Regressing their binary indicator for elimination of the estate tax on the individual prizes gives an estimate of 0.003 per $100K won ($SE = 0.001$) whereas the estimate for group prizes is 0.023 per $100K ($SE = 0.014$). Because group prizes are smaller, the larger estimate for group prizes suggests diminishing marginal effects of wealth. If we recode our question about reintroducing the inheritance tax in our data as a binary variable where 1 denotes not supporting a reintroduction of the tax, the estimated effect varies between 0.002 and 0.023 per $100K depending on which value on the five-point scale we use as cutoff.

In another study of U.S. lottery winners, Peterson (2015) estimates the effect of lottery winnings between 2000 and 2012 on party registrations in 2013. His sample includes 1,933 lottery prizes between $10K to $1.7M, which is similar to the prize range in our data. Just like Doherty, Gerber, and Green (2006), identification relies on comparing winners of different amounts in the same lottery. Peterson (2015) estimates the effect of log winnings on an indicator for Republican registration to be 0.015 ($SE = 0.008$). The median prize was $113,898, so the marginal effect per $100K evaluated at the median prize is $100,000 \times 0.015/113,898 \approx 0.013$.[12] He also reports the results from a regression using the dollar value of prizes where the estimated effect is substantially smaller, 0.0035 per $100K won ($SE = 0.0035$). These estimates can be compared to our estimate for supporting right-wing political parties (see Section 6.3) of 0.017 ($SE = 0.008$). We prefer a specification with prizes in dollars instead of logarithms as we find a

[12]Peterson (2015) does not report whether nominal or real amounts are used, so we do not try to adjust his estimates for inflation.

logarithmic functional form implausible for the range of prizes in our and Peterson's study. For example, a logarithmic specification implies that the marginal effect of the last lottery dollar for someone who won a $10,000 prize is 170 times larger than the effect for a winner of a $1.7M prize.

Whereas Peterson's main estimate is smaller than ours, Oswald and Powdthavee (2014) estimate a substantially larger effect of lottery wealth. Their main outcome is a measure of strength of political partisanship on a seven-point scale ranging from strong identification with Labour to strong identification with the Conservative party. They use data from the British Household Panel Survey which includes winners of lottery prizes from different lotteries ranging from £1 to £185K during the years 1996 and 2009. There are 8,984 lottery prizes awarded, of which 541 are above £500. The total prize sum is about £2M, which is order of magnitudes smaller than the current study ($277M) and the other two lottery studies discussed above ($613M expressed in 2002 dollars in Doherty, Gerber, and Green, 2006; a lower bound of $231M in Peterson, 2015). In one of their main specifications, Oswald and Powdthavee (2014) estimate that winners of prizes above £500 increase their strength of right-wing political partisanship by 0.124 ($SE = 0.044$). Their dependent variable has a standard deviation of 1.46, so the standardized effect is 0.085. The paper does not report the average prize for winners above £500, but in a related paper Apouey and Clark (2015) also use data on lottery winners from BHPS and report that the average lottery prize below £500 is £61.64. Under the assumption that the same average holds for Oswald and Powdthavee (2014), the average large prize is £2,745, implying the effect size is 3.17 SD units per £100K won ($SE = 1.13$). Expressed in 2011 US dollars, the effect is 1.50 SD units per $100K. This estimate can be compared to our most similar outcome, the question about left-right placement, for which the estimated effect is 0.022 ($SE = 0.015$) per $100K won. The large standard errors in Oswald and Powdthavee (2014) implies that they were underpowered to detect effect sizes of similar magnitude to what we find in this paper.

7 Concluding Discussion

In this paper, we seek to make progress on the question whether there are casual pathways from wealth to political participation and political attitudes. We find large, positive wealth shocks in the form of lottery prizes do not affect political participation in terms of voter turnout or candidacy for political office, neither for the lottery players and nor for their children.

In terms of attitudes, our main finding is that, relative to controls, large-prize winners become more negative towards proposals to reintroduce taxes on wealth, real estate and inheritances. For these outcomes, our estimated wealth effects are similar in magnitude to income

and wealth gradients estimated in the cross section. The wealth effects decline over time at a rate that is roughly proportional to the rate at which the windfall wealth is spent down. For other outcomes, we generally fail to detect any statistically significant wealth effects, though the point estimates suggest a small rightward shift in political attitudes. Our estimates have high internal validity and were obtained through pre-registered analyses. Overall, our results lend support to the self-interested voter hypothesis, which has been disputed on theoretical grounds. As pointed out by Downs (1957), the fact that any individual voter's probability of casting the pivotal vote in a large-scale election is effectively zero implies that incentives to make well-informed voting decisions are limited. In a similar vein, Akerlof (1989) argues that biased beliefs are optimal if there is just a slight psychological benefit from holding a particular opinion; "the price of ideological loyalty is close to zero", as Caplan (2007, p. 18) succinctly puts it. Our findings suggest that despite such incentives, attitudes – especially with respect to the taxation of capital – are impacted by changes in wealth in a direction consistent with the self-interest hypothesis.

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"The Causal Effects of Wealth on Political Participation and Political Attitudes"

Online Appendix

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1 Additional Tables: Participation

Table A1: Test of Conditional Random Assignment of Lottery Prices (Adults)

	No Cell FE	Cell FE
Age	0.000	0.711
Age^2	0.003	0.549
Age^3	0.015	0.456
Female	0.166	0.697
Immigrant	0.004	0.650
Married	0.113	0.737
Income (logs)	0.000	0.168
College	0.000	0.555
College missing	0.020	0.509
Previous turnout	0.000	0.436
Below 18	0.000	0.548
Abstained	0.000	0.535
Previous nomination	0.486	0.274
Below 18	0.001	0.715
Not Swedish	0.002	0.410
F (analytical)	32.745	0.729
p (analytical)	0.000	0.757
p (rand-c)	0.000	0.831
p (rand-t)	0.000	0.806
R^2	0.001	0.484
N	413,940	413,940

This table reports the p-values of individual coefficients and tests of joint significance from a regression with lottery wins as the dependent variable. The sample consists of adult players for whom we observe at least one outcome. Standard errors are clustered at the level of the individual. The resampling-based p-values are obtained by simulating the distribution of coefficient estimates under the null hypothesis of zero treatment effects, as described in the main text.

Table A2: Test of Conditional Random Assignment of Lottery Prices (Children)

	Full Sample		No Large PLS Families	
	No Cell FE	Cell FE	No Cell FE	Cell FE
Age	0.083	0.438	0.078	0.395
Age^2	0.377	0.111	0.438	0.111
Age^3	0.541	0.072	0.657	0.079
Female	0.016	0.039	0.020	0.048
First-born	0.273	0.000	0.001	0.037
Second-born	0.440	0.000	0.000	0.045
Third-born	1.000	0.049	0.000	0.086
Parent age	0.923	0.704	0.464	0.833
Parent age^2	0.610	0.760	0.168	0.918
Parent age^3	0.338	0.883	0.060	0.930
Parent female	0.437	0.301	0.352	0.347
Parent immigrant	0.021	0.350	0.012	0.364
Parent married	0.002	0.173	0.002	0.188
Log income	0.000	0.757	0.000	0.768
Parent college	0.000	0.356	0.000	0.321
Parent college missing	0.000	0.825	0.000	0.916
Parent turnout	0.001	0.240	0.002	0.206
Parent below 18	0.000	0.284	0.000	0.325
Parent abstained	0.034	0.558	0.029	0.540
Parent candidacy	0.580	0.566	0.664	0.697
Parent below 18	0.471	0.226	0.756	0.302
Parent not Swedish	0.281	0.281	0.325	0.279
F (analytical)	5.812	2.020	6.146	1.617
p (analytical)	0.000	0.003	0.000	0.034
p (rand-c)	0.000	0.112	0.000	0.144
p (rand-t)	0.270	0.007	0.265	0.065
R^2	0.004	0.557	0.005	0.560
N	110,392	110,392	97,914	97,914

This table reports the p-values of individual coefficients and tests of joint significance from a regression with parental lottery wins as the dependent variable. The sample consists of players' children for whom we observe at least one outcome. Standard errors are clustered at the level of the individual. The resampling-based p-values are obtained by simulating the distribution of coefficient estimates under the null hypothesis of zero treatment effects, as described in the main text.

Table A3: Representativeness (Adults)

	PLS	Kombi	Triss Lumpsum	Triss Monthly	Pooled	Matched Sample
Age	57.690	61.736	51.001	50.020	53.948	53.948
Female	0.524	0.427	0.507	0.469	0.493	0.493
Immigrant	0.027	0.016	0.058	0.053	0.043	0.085
Married	0.587	0.558	0.508	0.511	0.537	0.534
Log Income	9.962	10.168	10.091	10.179	10.086	10.046
College	0.187	0.174	0.180	0.212	0.189	0.235
Turnout	0.643	0.846	0.735	0.714	0.717	0.674
Candidacy	0.023	0.062	0.025	0.030	0.030	0.024
N	367,737	41,163	4,338	702	413,940	4,139,400

This table reports mean values for demographic and political variables for the sample of adult players, by lottery (columns 1-4) and pooled (column 5). All time-varying variables are measured prior to the lottery event. The pooled sample has been weighted by each lottery's share of the identifying variation (defined as the total within-cell variation in prize amount). The matched sample in column 6 is generated by matching each player to ten individuals randomly selected individuals from the total population with the same sex and birthyear, and then weighting by the corresponding lottery's share of the identifying variation.

Table A4: Representativeness (Child Sample)

	PLS	Kombi	Triss Lumpsum	Triss Monthly	Pooled	Matched Sample
Age	9.402	12.387	10.270	11.325	10.442	10.442
Female	0.491	0.471	0.479	0.543	0.497	0.497
First-born	0.482	0.402	0.487	0.489	0.482	0.485
Second-born	0.392	0.370	0.358	0.343	0.363	0.345
Third-born	0.124	0.173	0.111	0.125	0.121	0.124
Fourth-born	0.002	0.055	0.043	0.043	0.035	0.046
Parent age	40.325	44.701	40.606	42.389	41.186	40.552
Parent female	0.493	0.496	0.483	0.421	0.471	0.471
Parent immigrant	0.041	0.034	0.084	0.118	0.080	0.137
Parent married	0.732	0.565	0.566	0.582	0.607	0.625
Log Income	10.842	11.007	10.928	11.018	10.935	10.877
Parent college	0.341	0.283	0.216	0.189	0.241	0.299
Parent turnout	0.471	0.818	0.717	0.650	0.651	0.569
Parent candidacy	0.028	0.055	0.019	0.025	0.024	0.019
N	92,605	3,035	1,994	280	97,914	979,140

This table reports mean values for demographic and political variables for the sample of players' children, by lottery (columns 1-4) and pooled (column 5). All time-varying variables are measured prior to the lottery event. The pooled sample has been weighted by each lottery's share of the identifying variation (defined as the total within-cell variation in prize amount). The matched sample in column 6 is generated by matching each player to ten individuals randomly selected individuals from the total population with the same sex and birthyear, and then weighting by the corresponding lottery's share of the identifying variation.

Table A5: Heterogeneous Effects of Lottery Wealth on Political Participation (Adult Sample)

	Income		Education		Turnout	
	Poor	Rich	No College	College	Abstained	Voted
Turnout National						
Effect ($100K)	0.107	0.002	-0.126	0.610	1.622	0.072
	0.449	0.279	0.303	0.242	1.721	0.237
p	0.811	0.996	0.678	0.012	0.346	0.762
Heterogeneity p	0.842		0.058		0.372	
R^2	0.115		0.112		0.146	
N	323,341		323,341		243,887	
Turnout EU						
Effect ($100K)	-0.380	-0.752	-0.760	-0.102	-2.713	-0.211
	0.669	0.587	0.499	0.923	0.981	0.519
p	0.570	0.200	0.127	0.912	0.006	0.684
Heterogeneity p	0.677		0.530		0.024	
R^2	0.120		0.118		0.130	
N	275,054		275,054		202,590	
Candidacy						
Effect ($100K)	-0.139	-0.026	0.068	-0.695	-0.258	-0.039
	0.096	0.155	0.108	0.261	0.154	0.112
p	0.149	0.867	0.527	0.008	0.092	0.728
Heterogeneity p	0.535		0.007		0.248	
R^2	0.208		0.213		0.208	
N	399,563		399,563		295,508	

This table reports the treatment effect of $100K on the three primary participation outcomes for the adult sample split by pre-lottery income, education and voter turnout. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

Table A6: Effects of Lottery Wealth on Political Participation by Lottery (Adult Sample)

	PLS	Kombi	Triss-Lumpsum	Triss-Monthly
Turnout National				
Effect ($100K)	0.070	0.359	-0.430	0.418
	0.417	0.611	0.484	0.429
p	0.867	0.557	0.375	0.330
Heterogeneity p	0.587			
R^2	0.101			
N	335,989			
Turnout EU				
Effect ($100K)	-0.781	1.353	-1.446	-0.624
	0.896	1.141	0.687	0.796
p	0.383	0.236	0.035	0.433
Heterogeneity p	0.218			
R^2	0.106			
N	288,205			
Candidacy				
Effect ($100K)	0.023	-0.251	-0.044	-0.075
	0.174	0.274	0.173	0.174
p	0.893	0.360	0.800	0.665
Heterogeneity p	0.866			
R^2	0.198			
N	413,349			

This table reports the treatment effect of $100K on the three primary participation outcomes for each lottery sample separately. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

Table A7: Effects of Lottery Wealth on Political Participation over Time (Adult Sample)

	Second National	Third National	Second EU
Effect ($100K)	-0.016	-1.266	0.150
	(0.271)	(0.741)	(0.517)
p	0.954	0.088	0.771
R^2	0.110	0.111	0.113
N	250,241	108,266	207,059

This table reports the treatment effect of $100K on voter turnout after the first post-lottery election. See Brännlund et. al. (2020) for a detailed description of how these variables are constructed. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

Table A8: Non-linear Effects of Lottery Wealth on Political Participation (Adult Sample)

	Turnout National	Turnout EU	Political Candidacy
Omitting large prizes			
Effect ($100K)	0.114	-0.218	-0.180
	(0.319)	(0.611)	(0.126)
p	0.721	0.721	0.154
R^2	0.100	0.105	0.196
N	335,796	288,010	413,122
Squared winnings			
Effect ($100K)	-0.139	-0.340	-0.137
	(0.374)	(0.759)	(0.155)
p	0.709	0.654	0.378
Prize^2	3.529	-3.972	1.087
	(3.903)	(9.066)	(1.563)
p	0.366	0.661	0.487
R^2	0.100	0.105	0.196
N	335,989	288,205	413,349

This table reports the treatment effect of $100K on the three primary participation outcomes. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

Table A9: Heterogeneous Effects of Lottery Wealth on Political Participation (Child Sample)

	Household Income		Child Age		Parental Turnout	
	Poor	Rich	0-9	9-18	Abstained	Voted
Turnout National						
Effect ($100K)	-2.376	0.138	-0.151	-1.472	1.740	-2.051
	0.946	0.695	0.842	0.810	1.374	0.968
p	0.012	0.842	0.858	0.069	0.205	0.034
Heterogeneity p	0.027		0.226		0.024	
R^2	0.041		0.041		0.061	
N	92,999		92,999		45,279	
Turnout EU						
Effect ($100K)	-0.850	0.162	0.117	0.088	0.205	-1.784
	0.960	1.075	1.139	0.856	1.831	0.928
p	0.376	0.880	0.919	0.918	0.911	0.055
Heterogeneity p	0.484		0.983		0.332	
R^2	0.066		0.065		0.082	
N	93,878		93,878		45,432	
Candidacy						
Effect ($100K)	0.008	-0.052	0.066	-0.103	-0.103	-0.247
	0.131	0.142	0.219	0.082	0.201	0.119
p	0.949	0.717	0.764	0.207	0.608	0.038
Heterogeneity p	0.756		0.468		0.538	
R^2	0.026		0.029		0.036	
N	97,159		97,159		46,670	

This table reports the treatment effect of $100K on the three primary participation outcomes for the child sample split by pre-lottery household income, child age at the time of the lottery event, parental voter turnout. We control for baseline controls measured at $t = -1$ and group-identifier fixed effects in all specifications. Standard errors are clustered at the level of the individual.

2 Additional Tables and Figures: Survey

Table A1: Selecting Sample of Survey Respondents

	Kombi	Triss-Lumpsum	Triss-Monthly	Total
	(1)	(2)	(3)	(4)
Time Period	1998-2011	1994-2011	1997-2011	1994-2011
# Prizes Awarded	499	5,057	824	6,380
Original Restrictions				
# Quality Control	7	190	36	233
# Share Prize	0	342	61	403
# Multiple Winners in Group	0	8	0	8
# Age <18 at Win	0	19	0	19
# Born < 1941	230	12	119	1552
# <4 Valid Controls (Kombi)	3	0	0	3
# Deceased Before 2011	0	1	0	1
Statistics Sweden				
# Deceased, Emigrated, No Address	18	229	38	285
Survey Population				
# Prizes	241	3065	570	3876
# Controls	964	0	0	964
N	1,205	3,065	570	4,840
# Unique Individuals	1,196	3,061	570	4,820
Respondents Sample				
Survey Respondents	909	1,977	365	3,251
Abbreviated Survey	20	78	13	111
N	929	2,055	378	3,362
# Unique Individuals	920	2,051	378	3,344

This table summarizes the procedure by which we arrived at our final Survey Population. Failed quality control includes winners without information about ticket balance (Kombi only), missing or incorrect personal identification number, uncertainty about the identity of the winner, and so on. The table also reports survey participation by lottery (columns 1-3) and overall (column 4) and the number of players who participated who responded to the abbreviated telephone survey. We dropped prizes if the winning player's personal identification number ("PIN") could not be reliably determined or if key covariates (e.g., information about the number of tickets owned in Kombi) were missing. From each of the two Triss samples, we dropped subjects for whom we had indications that the winning ticket was jointly owned. Such players constitute ∼7% of the sample (for details on joint ownership, see Section IV in the Online Appendix of Cesarini et al. (2016)). We also dropped a small number of Triss players who won multiple prizes under the same prize plan. We restricted the sample to prizes won by players aged 18 or above at the time of win and who were at most 75 years of age when surveyed. For each large-prize event in Kombi, we sought to identify suitable experimental controls. A non-winning player was deemed a suitable control if their sex, year of birth and number of tickets owned (in the month of win) were identical to that of the winner. For three large-prize winners, we were unable to identify four controls satisfying these criteria; we therefore dropped them. In a final step, we added four experimental controls for each large-prize winner in Kombi.

Table A2: Definition of Primary Outcomes

Primary Outcome	Variable Definition
1. Capital Taxation	Index of three questions about reintroducing bequest, wealth and property taxes and one question about reducing the capital income tax.
2. Capital vs Labor	Constructed from two questions asking respondents to give the percentage rate at which they would like capital income and labor income to be taxed. The outcome variable is the sample percentile of the difference between the stated preferred labor income tax and preferred capital income tax (both constrained to be integers between 1% and 99%).
3. Public vs Private	Index of five questions about attitudes to the size of the public sector, reduction of labor taxes, privatization of publicly owned companies, private provision of health care, and a prohibition for private providers of tax-funded welfare services to pay dividends.
4. Redistribution	Index of four questions about reduction of income inequality, increased support to rural areas, introduction of a six-hour work day for all workers and investments to achieve parity between women and men.
5. Left-Right	"In political matters, people talk of left and right. How would you place your views on this scale, generally speaking?" Likert scale from 0 ("Left") to 10 ("Right").
6. Meritocratic Beliefs	"To what extent do you think success in life is determined by luck and connections rather than hard work?" Likert scale from 0 ("Success only depends on luck and connections") to 10 ("Success only depends on hard work").
7. Moral Values	Index of seven questions that ask the respondent to rate the moral defensibility of different behaviors on a Likert scale from 0 ("Never defensible") to 10 ("Always Defensible"). Questions about goods that Östling (2009) classifies as inferior (benefit fraud and avoiding public transport fares) are reverse-coded. The index is defined as the sum of the seven numerically coded responses.

Index outcome variables are calculated by summing the responses to the items included in the index. We set the variable to missing if more than one of the items it is composed of is missing. If exactly one item is missing, we replace the missing value by the sample mean and then generate the variable. All outcomes are standardized and scaled so that a higher value indicates attitudes that are self-serving for a lottery winner and/or more to the political right: lower taxes on capital, less redistribution, more private ownership, stronger belief in meritocracy etc.

Table A3: Pairwise Correlations Between Primary Outcomes

	Capital Taxation	Capital vs Labor	Public vs Private	Redistri-bution	Left-Right Placement	Meritocratic Beliefs
Capital vs Labor	0.421					
Public vs Private	0.464	0.186				
Redistribution	0.353	0.230	0.350			
Left-Right	0.472	0.253	0.513	0.413		
Meritocratic Beliefs	0.117	0.123	0.144	0.048	0.128	
Moral Values	0.160	0.025	0.147	0.212	0.187	-0.046

This table shows pairwise correlations between the primary outcomes.

Table A4: Distribution of Prizes Awarded

	Survey Population				Respondents Sample			
			Triss...				Triss...	
	All	Kombi	Lumpsum	Monthly	All	Kombi	Lumpsum	Monthly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0	964	964	0	0	747	747	0	0
5K to 10K	811	0	811	0	554	0	554	0
10K to 50K	1,896	0	1,896	0	1,261	0	1,261	0
50K to 100K	211	0	211	0	138	0	138	0
100K to 200K	340	213	42	85	247	163	27	57
200K to 400K	322	21	43	258	216	14	34	168
400K to 600K	149	4	26	119	104	4	18	82
600K to 1M	135	2	36	97	87	0	23	64
>1M	12	1	0	11	8	1	0	7
Prize Sum ($M)	410.7	44.4	128.3	237.9	277.2	33.3	86.1	157.8
% of Survey Pop.					67%	75%	67%	66%
N	4,840	1,205	3,065	570	3,362	929	2,055	378
% of Survey Pop.					69%	77%	67%	66%

This table compares the distribution of prizes in the Respondents Sample and in the Survey Population. In Triss-Monthly, prize amount is defined as the net present value of the monthly installments won, assuming the annual discount rate is 2%.

Table A5: Testing Endogenous Selection into the Respondents Sample

	Mail-in Survey		Phone Survey	Mail-in or Phone
	(1)	(2)	(3)	(4)
Effect ($100K)	-0.0057	-0.0024	0.0077	-0.0024
SE	(0.0040)	(0.0059)	(0.0183)	(0.0058)
p (analytical)	0.154	0.677	0.675	0.682
p (resampling)	0.150	0.681	0.635	0.674
N	4,840	4,840	501	4,840
Proportion	67.2%	67.2%	22.2%	69.5%
Group FEs	No	Yes	Yes	Yes

This table reports the results from Diagnostic Test 1 in the pre-analysis plan. The first two columns report coefficient estimates from a regression of an indicator variable equal to 1 for subjects who returned a mail-in survey and 0 for subjects who did not, on prize amount won. The results without group identifier fixed effects are shown in column 1 and the results with the group identifier fixed effects are in column 2. Column 3 shows the results from an analogous specification estimated among players invited to the abbreviated telephone survey (see Figure A1). Here, the dependent variable is an indicator equal to one for subjects who agreed to participate. Finally, column 4 shows the results from a specification in which survey participation is defined as either having returned the mail-in survey or having answered the abbreviated telephone survey. Across all specifications, we fail to see any indications that survey participation was impacted by the outcome of the lottery.

Table A6: Testing for Conditional Random Assignment of Lottery Prizes

	Survey Population				Respondents Sample			
Kombi	X	X	X		X	X	X	
Triss-Monthly	X	X		X	X	X		X
Triss-Lumpsum	X	X		X	X	X		X
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fixed Effects	None	Group ID	Group ID	Group ID	None	Group ID	Group ID	Group ID
N	4,840	4,840	1,205	3,635	3,362	3,362	929	2,433
Pre-Lottery Characteristics								
Age (Beta/SE)	0.525	1.049	N/A	1.045	0.274	0.798	N/A	0.709
p (analytical)	0.599	0.294	N/A	0.296	0.784	0.425	N/A	0.478
Age^2 (Beta/SE)	-0.710	-0.782	N/A	-0.809	-0.366	-0.550	N/A	-0.485
p (analytical)	0.478	0.435	N/A	0.419	0.714	0.582	N/A	0.628
Female (Beta/SE)	0.952	0.792	N/A	0.809	1.006	0.959	N/A	1.002
p (analytical)	0.341	0.429	N/A	0.418	0.314	0.338	N/A	0.317
College (Beta/SE)	0.750	1.516	-0.278	1.732	1.150	1.508	0.086	1.619
p (analytical)	0.453	0.130	0.781	0.083	0.250	0.132	0.932	0.106
Married (Beta/SE)	0.118	-0.594	-0.971	-0.290	0.127	-0.769	-1.375	-0.303
p (analytical)	0.906	0.552	0.332	0.772	0.899	0.442	0.169	0.762
Swedish (Beta/SE)	-1.197	-1.060	-1.091	-0.844	-1.497	-1.318	-1.503	-1.028
p (analytical)	0.231	0.289	0.275	0.399	0.135	0.187	0.133	0.304
# Children (Beta/SE)	-0.080	0.836	1.552	0.437	0.297	-0.049	0.599	-0.210
p (analytical)	0.936	0.403	0.121	0.662	0.766	0.961	0.549	0.833
Capital Income (Beta/SE)	0.098	-0.043	-1.609	0.157	-0.290	-0.593	-1.649	-0.446
p (analytical)	0.922	0.965	0.108	0.876	0.772	0.553	0.100	0.656
Labor Income (Beta/SE)	0.839	0.382	-0.314	0.477	1.199	0.652	-0.244	0.748
p (analytical)	0.402	0.702	0.754	0.633	0.230	0.514	0.808	0.455
Joint Test of Baseline Covariates								
F-statistic	0.716	1.247	1.054	1.262	0.889	1.256	1.021	1.265
p (analytical)	0.694	0.261	0.389	0.253	0.535	0.256	0.410	0.251
p (resampling)	0.635	0.324	0.375	0.337	0.365	0.219	0.415	0.323

This table reports results from Diagnostic Test 2 in the pre-analysis plan. Each column reports results from a regression in which the dependent variable is the lottery prize. In all specifications, we control for baseline characteristics measured at $t = -1$. Under the null hypothesis of conditional random assignment, variables determined before the lottery should not have any predictive power conditional on the group-identifier fixed effects. The table shows t-statistics, that is, coefficient estimates divided by their standard errors. The resampling-based p-values are constructed by performing 10,000 simulations to approximate the distribution of covariate coefficients under the null hypothesis of zero treatment effects, as described in the main text.

Table A7: Treatment-effect Estimates in the Survey Population and Respondents Sample

	$t = 0$ Net Wealth		$t = 0$ Total Debt		$t = 1$ Capital Income		$t = 1$ Labor Income	
	Survey Population	Respondents Sample	Survey Population	Respondents Sample	Survey Population	Respondents Sample	Survey Population	Respondents Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect ($100)	53.256	53.523	-2.392	-1.178	0.722	0.532	-1.196	-1.211
SE	(3.836)	(5.110)	(0.978)	(1.500)	(0.197)	(0.178)	(0.180)	(0.219)
p (analytical)	<0.001	<0.001	0.015	0.433	<0.001	0.003	<0.001	<0.001
p (resampling)	<0.001	<0.001	0.001	0.185	<0.001	0.003	<0.001	<0.001
Mean	84,637	90,951	43,387	44,482	-331	-195	32,857	33,966
SD	137,053	141,577	54,072	53,325	8,329	8,918	22,677	23,175
N	1,976	1,403	1,976	1,403	4,129	2,901	4,129	2,901
Years-of-Win Restrictions	2000-2007		2000-2007		1994-2009		1994-2009	

This table reports the results from Diagnostic Test 3 in the pre-analysis plan. We estimated the treatment effect of lottery wealth on a number of register-based outcome variables in the Survey Population and examined whether the coefficients moved appreciably when the estimation sample was restricted to the Respondents Sample. In all specifications, we control for baseline characteristics measured at $t = -1$ and the lag of the dependent variable. The sample restrictions in columns 1-4 are needed because the outcomes – wealth and debt at year-end in the year of the lottery event – are only available in government registers 1999-2007. Hence, the analyses are restricted to the subset of players who won during this period. Estimates in column 1-4 data are restricted to the lotteries that pay lump-sum prizes (Kombi and Triss-Lumpsum). The sample restrictions in 5-8 reflect the fact that the last year for which we have income data for the Survey Population is 2010. As discussed in the Analysis Plan, evidence of systematic differences between the two sets of coefficient estimates could, but need not, be an indication of endogenous selection into the Respondents Sample. We find no evidence of systematic differences in the coefficient estimates. In columns 1, 3, 5, and 7 we report estimates from the Survey Population (the smaller sample sizes in columns 1 and 3 reflect the fact that financial variables are only available 2000-2007 and net wealth and debt at year-end in the year of the lottery event is only defined for players who won in these years). In columns 2, 4, 6, and 8, we report the results from exactly analogous analyses conducted with non-respondents omitted from the estimation sample. For all pre-specified outcomes – $t = 0$ net wealth, $t = 0$ debt, $t = 0$ capital income, and $t = 0$ labor income – the estimated treatment effects are similar in magnitude.

Table A8: Representativeness of Survey Respondents

	Respondents Sample					
	Kombi	Triss-Lumpsum	Triss-Monthly	Pooled	Survey Population	Representative Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Year of Birth	1951.1	1957.2	1957.5	1955.6	1957.3	1955.6
Female	40.0%	52.1%	49.2%	48.4%	46.5%	48.4%
College	24.0%	26.1%	28.0%	25.8%	22.1%	30.1%
Swedish-born	95.2%	91.2%	91.5%	92.4%	90.7%	83.8%
Married	53.3%	53.8%	53.7%	53.7%	48.4%	51.0%
# Children	0.33	0.69	0.62	0.58	0.62	0.56
Capital Income ($)	-625	-978	-691.4	-848	-964	-26
Labor Income ($)	37,454	33,431	37,160	34,963	33,874	32,074
N	929	2,055	378	3,362	4,840	373,276
	Kombi	Triss-Lumpsum	Triss-Monthly	Pooled	Small-prize Winners	National SOM Survey
	(7)	(8)	(9)	(10)	(11)	(12)
Support Left	64.8%	42.8%	36.1%	48.4%	50.8%	42.3%
Support Right	21.1%	37.2%	43.7%	33.3%	31.0%	40.1%
Support Nationalist	9.4%	14.3%	14.1%	12.9%	12.9%	15.6%
Smaller Government (1-5)	2.20	2.41	2.43	2.35	2.33	2.82
Private Care (1-5)	2.13	2.38	2.35	2.31	2.88	2.38
Reduce Inequality (1-5)	4.07	3.81	3.74	3.88	3.90	3.89
Rural Support (1-5)	4.09	3.93	3.88	3.97	4.00	4.20
Gender Equality (1-5)	4.33	4.20	4.21	4.24	4.25	4.30
Reduce Foreign Aid (1-5)	2.78	2.91	2.82	2.87	2.87	2.74
Fewer Refugees (1-5)	3.40	3.62	3.59	3.56	3.55	3.60
Leave EU (1-5)	2.53	2.60	2.51	2.57	2.60	2.50
Join NATO (1-5)	2.67	2.99	2.87	2.88	2.87	3.01
Trust (0-10)	5.85	5.81	6.09	5.86	5.82	6.69
N	854	1,796	327	2,977	1,893	1,487

This table reports descriptive statistics for the baseline controls and selected survey items in the Respondents Sample. The baseline controls are also reported separately for the Survey Population. Because the survey items are measured after the lottery event, column (11) also report averages for lottery players that won less than $20K. To help gauge representativeness, column (6) provides descriptive statistics for the baseline covariates for a representative sample draw in in 2010 after reweighting to match the sex and age distribution of the Respondents Sample. All time-varying variables are measured the year prior to the lottery event. Similarly, column (12) reports survey items from the representative national SOM survey conducted during the fall of 2016 after reweighting to match the sex and age distribution of the Respondents Sample. Only survey items that are available in both surveys are included in the table. The number of observations varies for each survey item, the total observation count on the bottom row shows the survey item with the fewest respondents. The survey question about party preferences and the trust question were phrased somewhat differently in the two surveys.

Table A9: Robustness Analyses

	Capital Taxation	Capital vs Labor	Public vs Private	Redistri-bution	Left-Right Placement	Merit. Beliefs	Moral Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Original Estimate							
Effect ($100K)	0.044	0.006	0.009	-0.001	0.022	-0.028	0.005
SE	(0.016)	(0.015)	(0.017)	(0.016)	(0.015)	0.018	0.015
Reweighted Estimate							
Effect ($100K)	0.052	0.001	0.018	0.001	0.070	-0.020	
SE	(0.017)	(0.015)	(0.019)	(0.018)	(0.021)	(0.025)	
p (analytical)	0.002	0.925	0.343	0.962	0.001	0.408	
p (resampling)	0.012	0.942	0.455	0.969	0.004	0.420	
N	3,227	3,029	3,204	3,256	3,219	3,308	
Drop Large Prizes (above $580K)							
Effect ($100K)	0.032	-0.012	-0.019	-0.024	0.003	-0.016	0.018
SE	(0.023)	(0.027)	(0.024)	(0.025)	(0.026)	(0.026)	(0.027)
p (analytical)	0.164	0.654	0.426	0.343	0.907	0.535	0.510
p (resampling)	0.214	0.657	0.458	0.342	0.902	0.519	0.439
N	3,132	2,939	3,109	3,161	3,123	3,211	3,084

This table reports the results from two pre-registered robustness analyses. In the first robustness analyses, we weight each abbreviated survey respondent such that the weighted fraction of mail-in survey respondents in the estimation sample matches the population fraction of 31%. This robustness check is not feasible for the two outcomes that were measured in the abbreviated survey. The second robustness check reports the results when excluding very large prizes, define as a price above 4M SEK in the pre-analysis plan.

Table A10: Heterogeneous Effects

		Year of Lottery		Prize Type		Disp. Income		Age		Sex	
		1994 to 2004	2005 to 2011	Lumpsum	Monthly	Below Median	Above Median	Below Age 51	At least 51	Male	Female
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Capital Taxation	Effect ($100K)	0.016	0.068	0.041	0.034	0.033	0.050	0.050	0.033	0.047	0.040
	SE	0.024	0.019	0.025	0.018	0.024	0.016	0.018	0.021	0.019	0.019
	p	0.505	0.000	0.108	0.063	0.165	0.002	0.005	0.124	0.013	0.037
	p equal	0.089		0.814		0.458		0.430		0.722	
	N	1,610	1,617	1,971	365	1,366	1,857	1,630	1,597	1,690	1,537
Capital vs Labor	Effect ($100K)	-0.008	0.024	0.005	0.011	0.001	0.009	0.005	0.007	-0.005	0.024
	SE	0.023	0.020	0.022	0.022	0.021	0.017	0.018	0.019	0.017	0.020
	p	0.739	0.215	0.818	0.623	0.967	0.603	0.774	0.723	0.783	0.241
	p equal	0.288		0.859		0.727		0.943		0.191	
	N	1,510	1,519	1,869	341	1,259	1,765	1,558	1,471	1,633	1,396
Public vs Private	Effect ($100K)	-0.001	0.017	0.011	-0.001	-0.017	0.025	0.015	0.004	0.004	0.022
	SE	0.023	0.025	0.025	0.025	0.021	0.019	0.020	0.023	0.023	0.020
	p	0.965	0.501	0.651	0.977	0.425	0.199	0.454	0.869	0.869	0.284
	p equal	0.603		0.733		0.072		0.684		0.296	
	N	1,600	1,604	1,956	361	1,350	1,850	1,624	1,580	1,580	1,682
Redistribution	Effect ($100K)	-0.007	0.002	0.017	-0.010	-0.033	0.019	0.006	-0.010	-0.010	0.003
	SE	0.019	0.026	0.024	0.025	0.020	0.019	0.020	0.020	0.020	0.020
	p	0.721	0.938	0.477	0.684	0.105	0.317	0.758	0.594	0.594	0.866
	p equal	0.782		0.433		0.025		0.474		0.757	
	N	1,628	1,628	1,992	368	1,380	1,872	1,643	1,613	1,613	1,693

Table A7: Heterogeneous Effects (Continued)

		Year of Lottery		Prize Type		Disp. Income		Age		Sex	
		1994 to 2004	2005 to 2011	Lumpsum	Monthly	Below Median	Above Median	Below Age 51	At least 51	Male	Female
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Left-Right Placement	Effect ($100K)	0.010	0.027	0.016	0.031	-0.013	0.044	0.034	0.012	0.016	0.033
	SE	0.020	0.021	0.023	0.020	0.020	0.017	0.017	0.020	0.017	0.019
	p	0.608	0.197	0.480	0.109	0.512	0.008	0.046	0.547	0.365	0.085
	p equal	0.566		0.613		0.009		0.323		0.415	
	N	1,609	1,610	1,965	361	1,361	1,854	1,621	1,598	1,684	1,535
Meritocratic Beliefs	Effect ($100K)	-0.047	-0.015	-0.033	-0.029	-0.025	-0.032	-0.041	-0.011	-0.030	-0.030
	SE	0.022	0.027	0.026	0.028	0.020	0.022	0.023	0.019	0.022	0.021
	p	0.031	0.572	0.202	0.290	0.228	0.139	0.071	0.579	0.783	0.147
	p equal	0.350		0.928		0.750		0.207		0.992	
	N	1,647	1,661	2,025	372	1,417	1,887	1,661	1,647	1,710	1,598
Moral Values	Effect ($100K)	-0.021	0.037	-0.016	0.018	-0.018	0.023	0.011	0.001	0.008	0.002
	SE	0.019	0.022	0.024	0.017	0.019	0.016	0.017	0.019	0.017	0.019
	p	0.271	0.084	0.498	0.304	0.330	0.165	0.509	0.970	0.629	0.922
	p equal	0.043		0.249		0.036		0.596		0.747	
	N	1,583	1,597	1,941	357	1,360	1,816	1,579	1,601	1,644	1,536

This table reports the results from the five pre-registered heterogeneity analyses. Columns 1 and 2 show results separately for winners before or after January 1, 2005. Columns 3 and 4 show the results separately for Triss-Lumpsum and Triss-Monthly winners. Columns 5 and 6 display results separately for those above or below the median income in a representative sample. This analysis is based on individual disposable income (in the pre-lottery year) and compared to the population median which is calculated conditional on the respondent's sex and age category (18-27, 28-37,..., 68+) in the year prior to the win. Column 7 and 8 show the result for winners above or below the median age in the sample. Finally, columns 9 and 10 show the results separately for men and women. All regressions include the baseline control variables measured at $t = -1$, indicator variable for the dimension of heterogeneity being examined (e.g. 1 if female in the sex-heterogeneity analyses), and interactions between the indicator and the baseline characteristics. We also control for group identifier fixed effects and their interactions with the indicator.

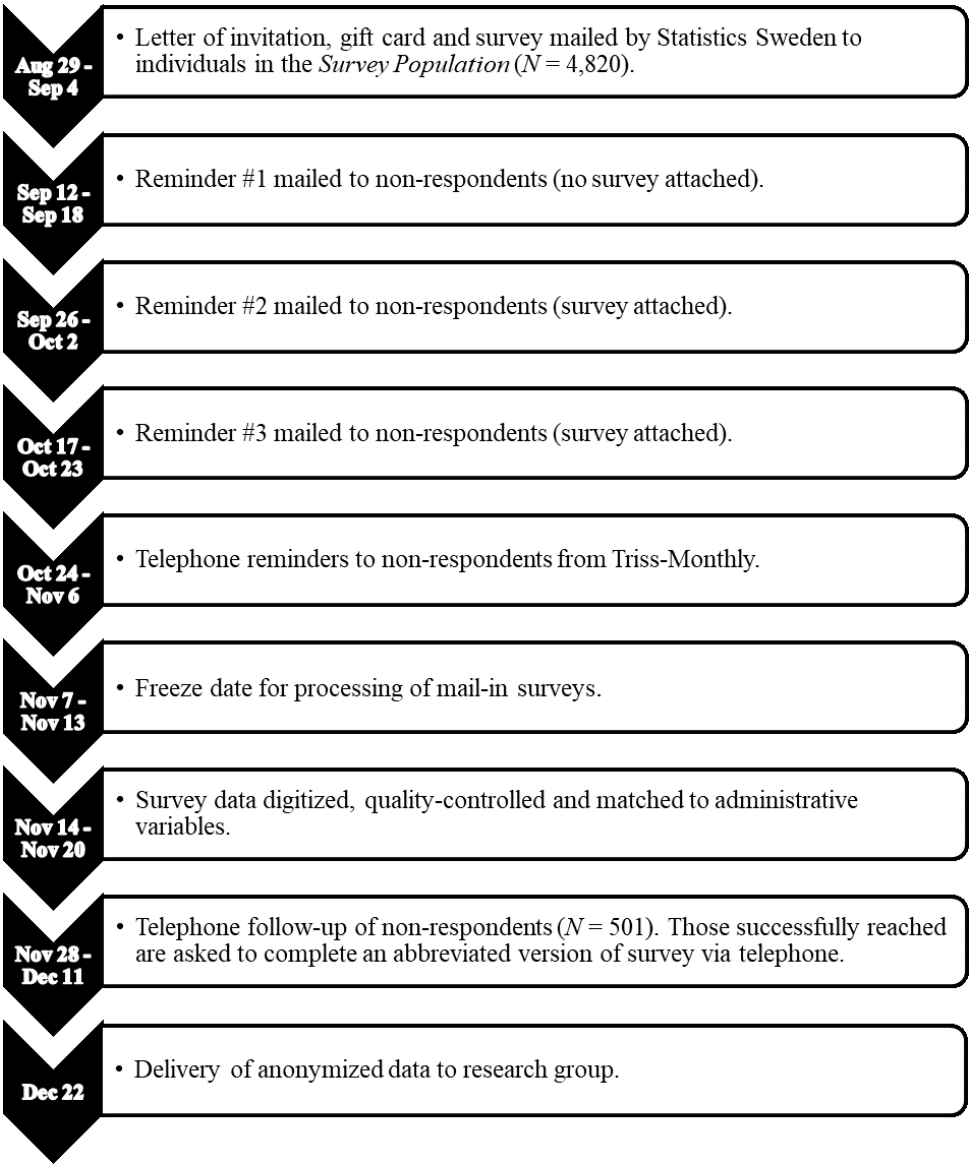

Figure A1: Schematic overview of survey-data-collection timeline.

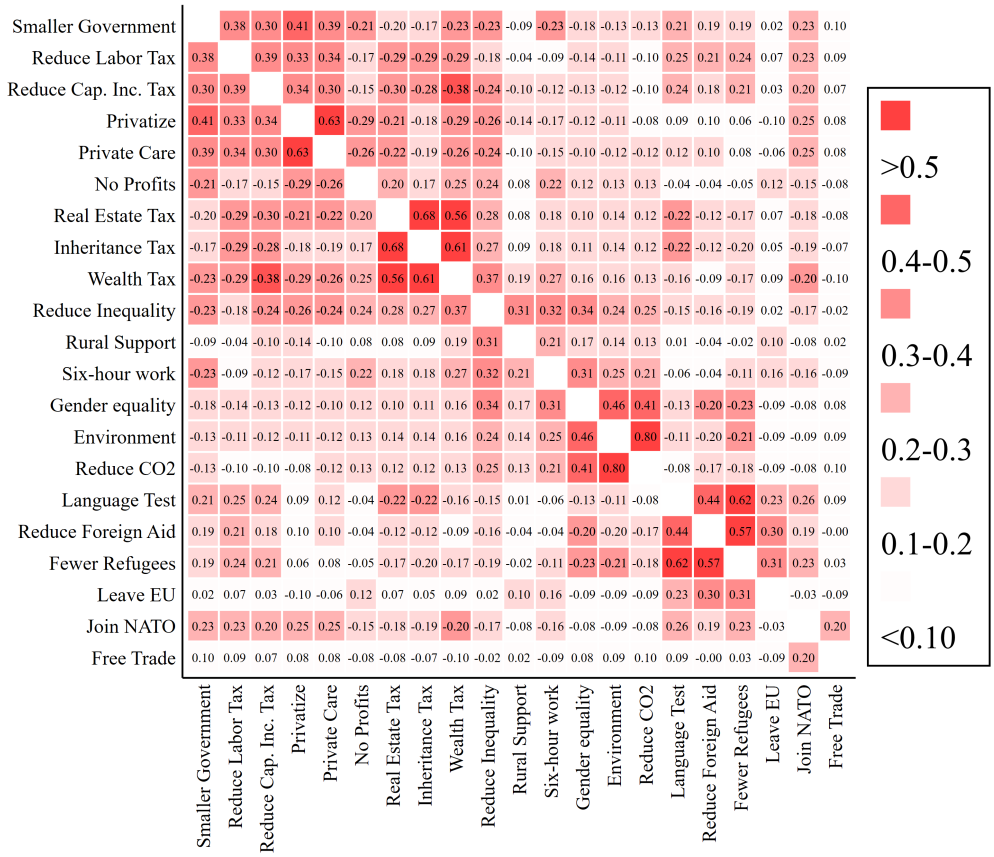

Figure A2: Pairwise Correlations Between Attitudes to Policy Proposals

The figure shows the pairwise correlation between all 21 policy proposals included in the survey. The proposals are shown in the order of appearance in the survey and labeled so that a higher value indicates support for the listed proposal.

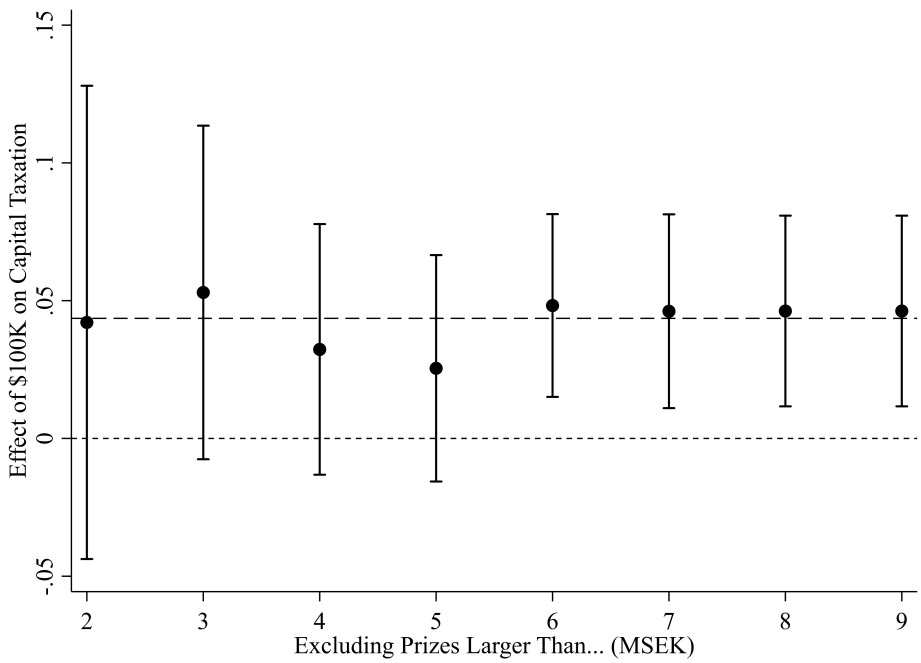

Figure A3: Effect of Lottery Wealth on Capital Taxation Index Excluding Large Prizes (Post Hoc)

The figure shows the effect of lottery wealth on the capital taxation index when excluding prizes above 2,3,...,9 million SEK. The dashed line correspond to the baseline estimate when all prizes are included.

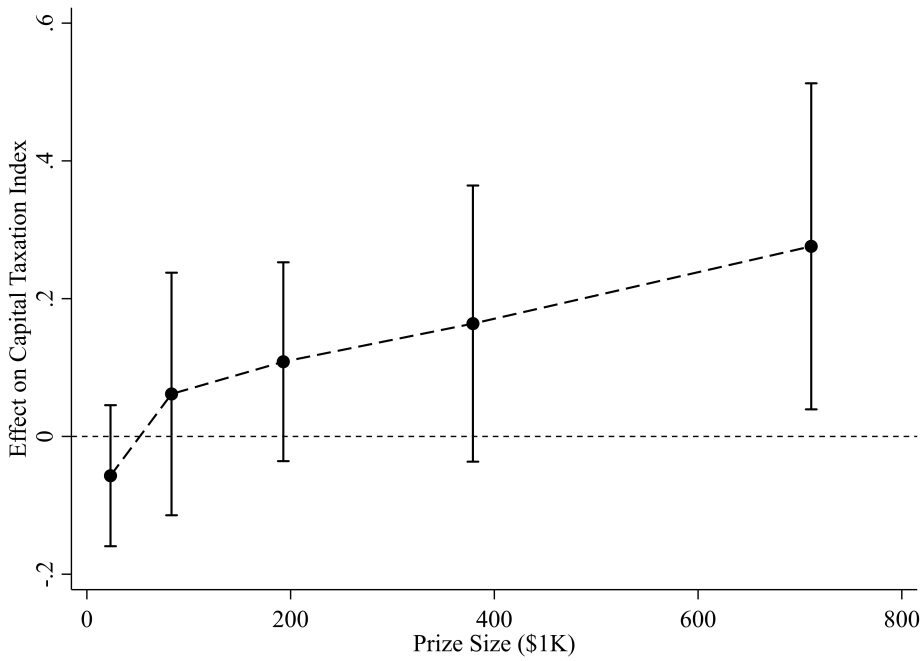

Figure A4: Effect of Lottery Wealth on Capital Taxation Index

The figure shows the results when regressing the capital taxation index on six categorical dummy variables for prize size (0 to 10K, 10 to 50K, 50K to 150K, 150 to 300K, 300 to 500K, and above 500K). The omitted category are lottery winnings of less than 10K (including the controls in Kombi that do not win). The coefficients are plotted at the average prize size within each category.

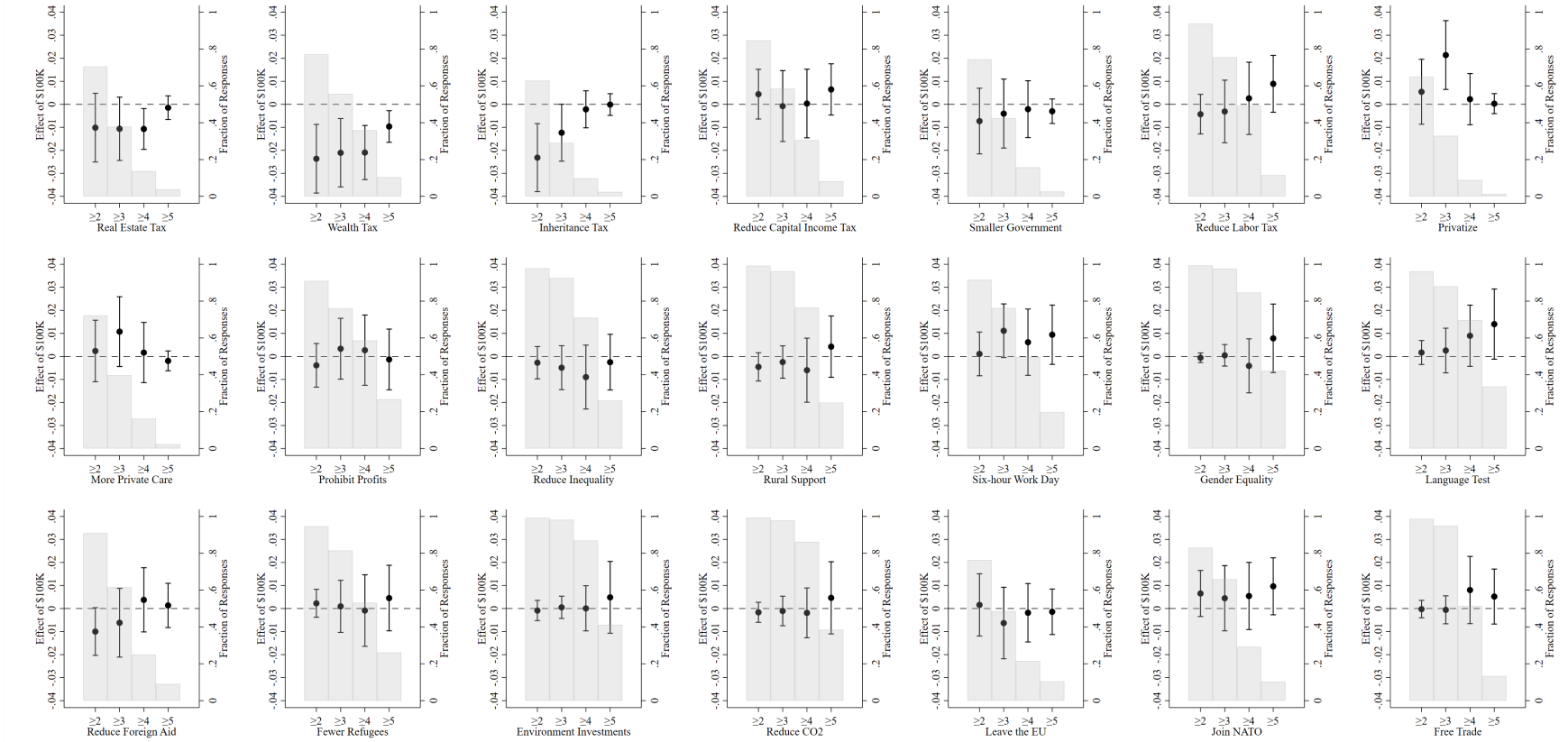

Figure A5: Effect of Lottery Wealth on Policy Proposals Defined as Binary Outcomes (Post Hoc)

The survey contained 21 questions asking about support for different policy proposals allowing answers ranging from "Very bad proposal" (coded as 1) to "Very good proposal" (coded as 5). The figure shows the estimated effect of lottery wealth when support for each policy proposal is coded as an indicator variable set equal to 1 for responses at or above each value of the response scale, and zero otherwise. The grey bars shows the share of respondents who report support for to the policy proposal equal to or higher than the value stated on the horizontal axis.

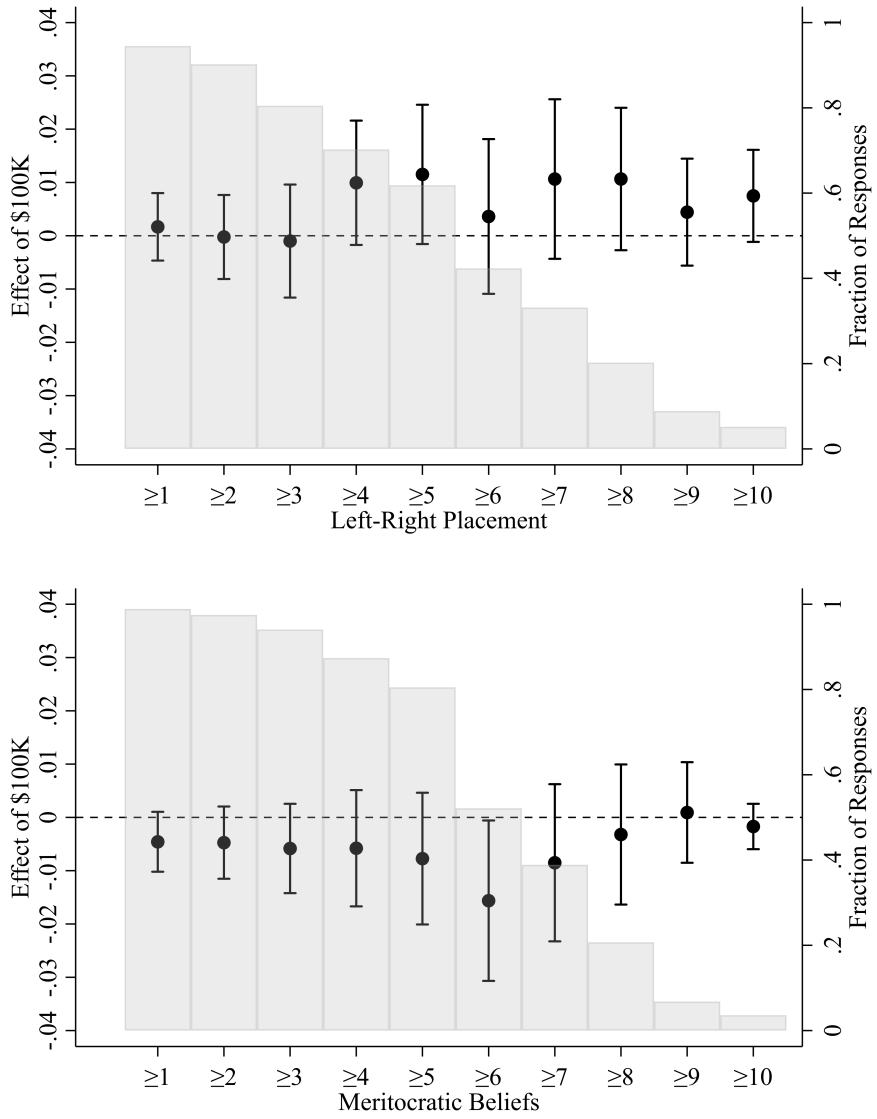

Figure A6: Effect of Lottery Wealth on Left-Right Placement and Meritocratic Beliefs Defined as Binary Outcomes (Post Hoc)

Left-right placement and meritocratic beliefs are based on a survey question allowing responses from 0 to 10. The figure shows the estimated effect of lottery wealth when the outcome is coded as an indicator variable set equal to 1 for responses at or above each value of the response scale, and zero otherwise. The grey bars shows the share of respondents who gave a response equal to or higher than the value stated on the horizontal axis.

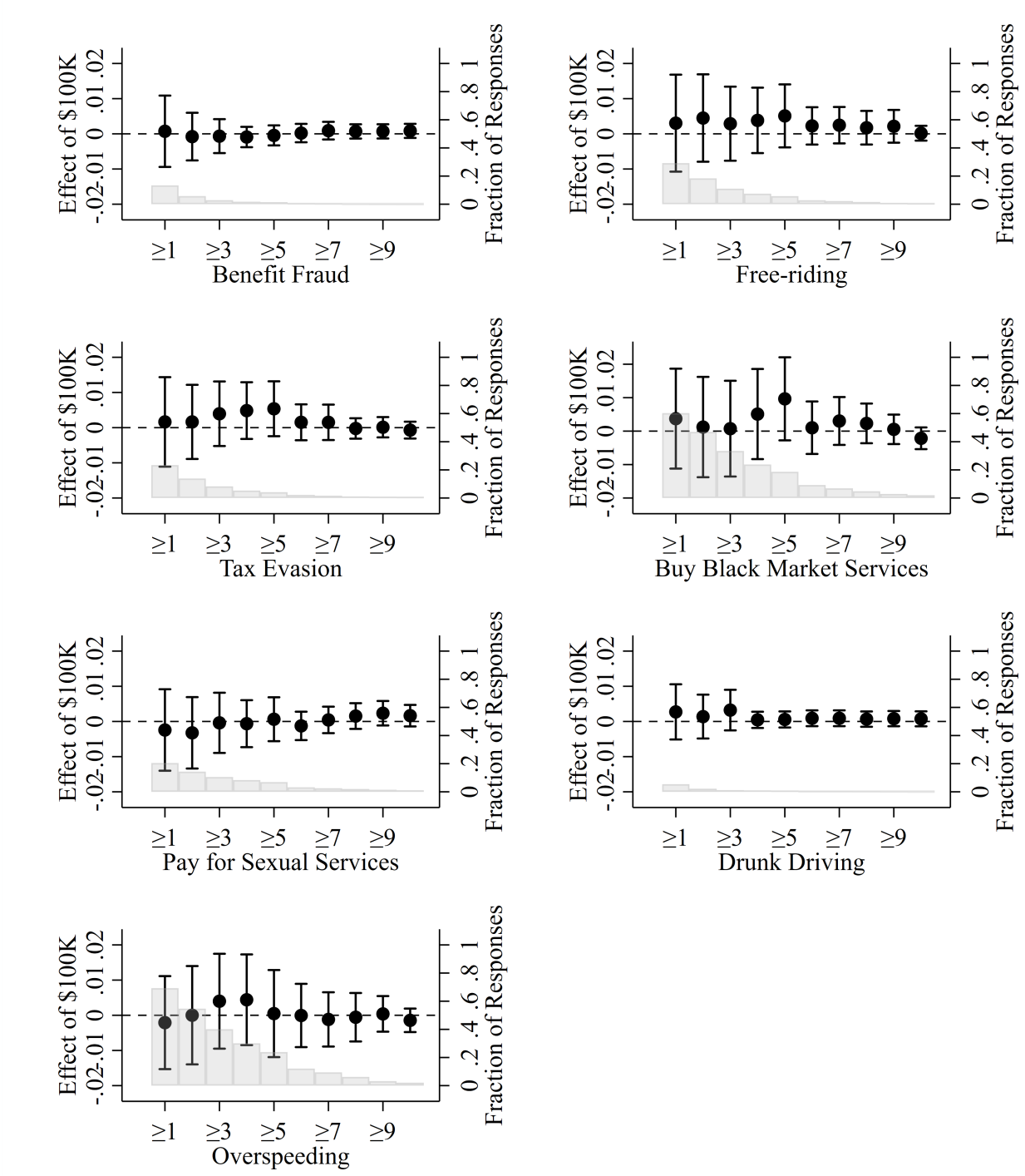

Figure A7: Effect of Lottery Wealth on Moral Values Defined as Binary Outcomes (Post Hoc)
The index of moral values is constructed from underlying questions asking about the moral acceptability of different behaviors allowing responses ranging from 0 ("Can never be defended") to 10 ("Can always be defended"). The figure shows the estimated effect of lottery wealth for each question when the outcome is coded as an indicator variable set equal to 1 for responses at or above each value of the response scale, and 0 otherwise. The grey bars shows the share of respondents who gave a response equal to or higher than the value stated on the horizontal axis.

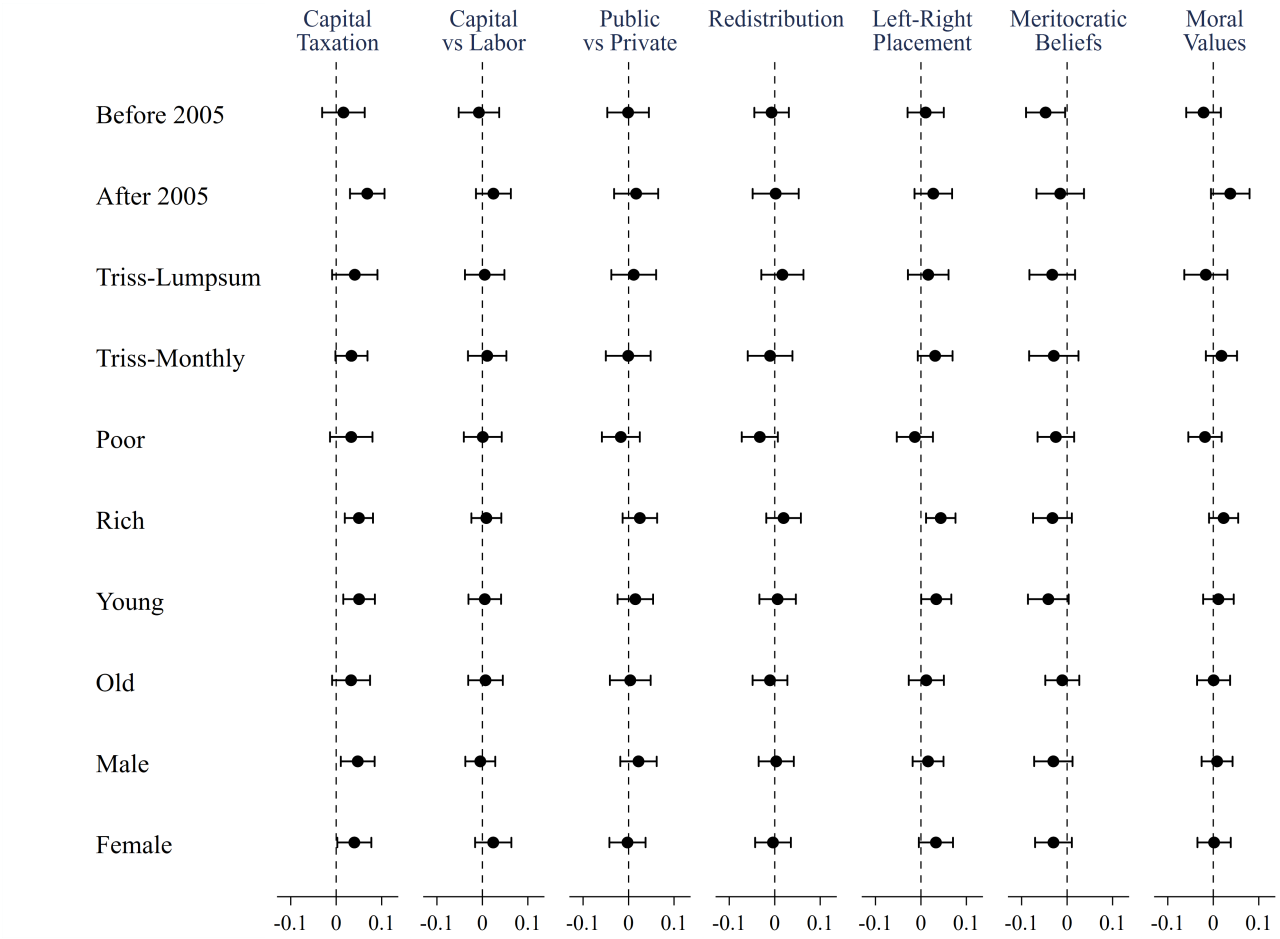

Figure A8: Treatment-Effect Heterogeneity

The figure shows estimated treatment effects of $100,000 in subsamples defined in the pre-analysis plan. For underlying data, see Table A10.

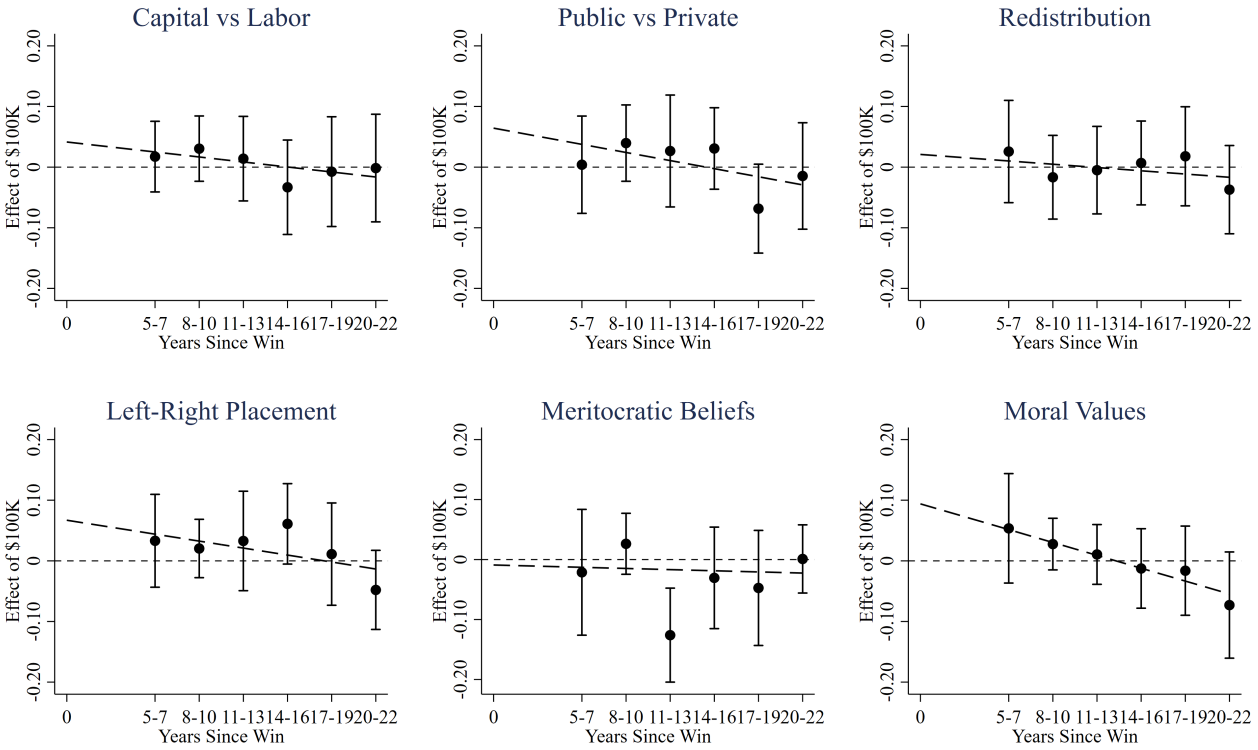

Figure A9: Treatment-Effect Heterogeneity by Year Won (Post Hoc)

This figure depicts estimates from post hoc analyses of treatment-effect heterogeneity by year won for all primary outcomes. The line shown is from a regression of the treatment-effect estimates weighting each point in proportion to the inverse of the variance of the estimate.

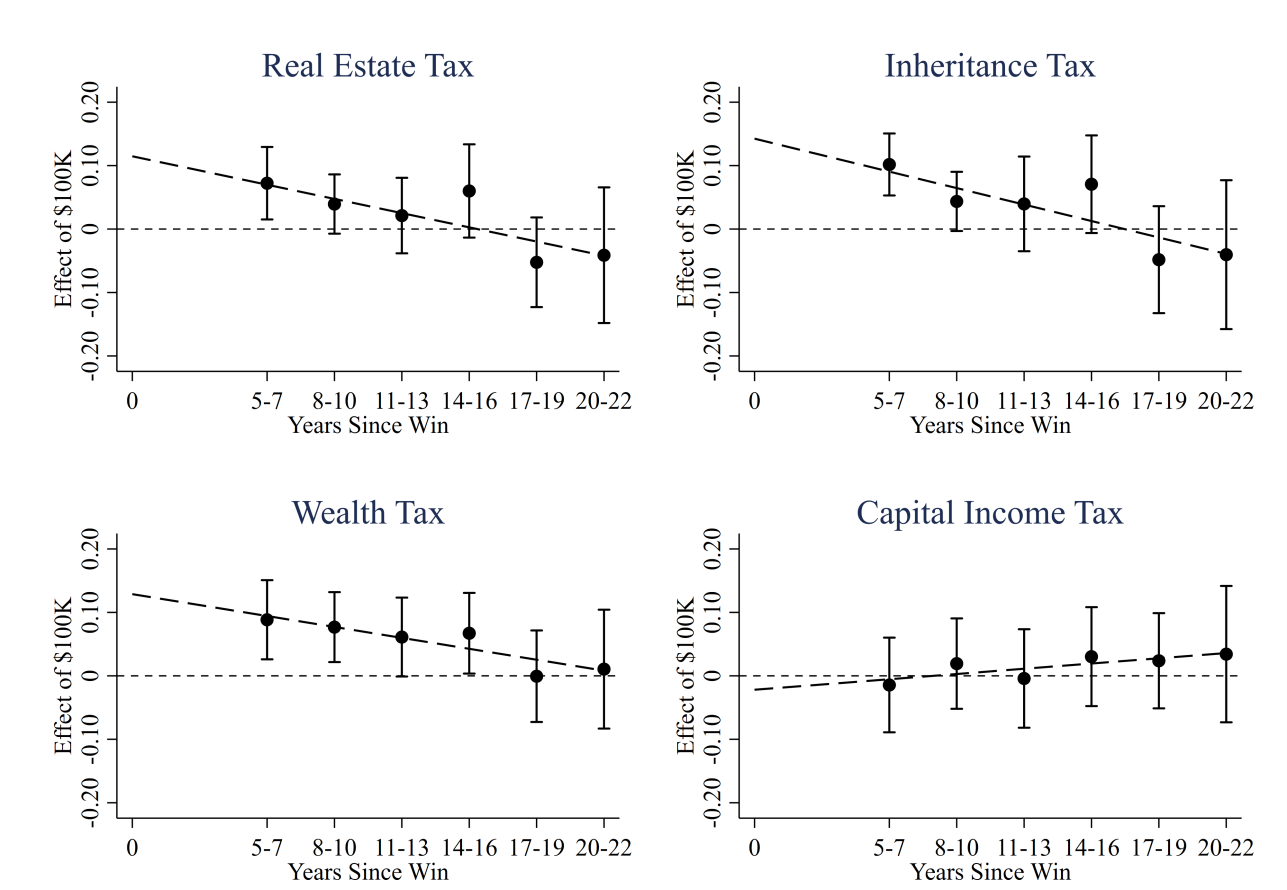

Figure A10: Treatment-Effect Heterogeneity by Year Won (Post Hoc)

This figure depicts estimates from post hoc analyses of treatment-effect heterogeneity by year won for the sub-components of the capital taxation index. The line shown is from a regression of the treatment-effect estimates weighting each point in proportion to the inverse of the variance of the estimate.

3 Translation of Survey Questions

Below we provide English translations of the survey questions used in this paper. The original Swedish version of the survey is provided in the pre-analysis plan.

1. Which of the political parties is closest to you?

- Centerpartiet (The Centre Party)
- Liberalerna (f.d. Folkpartiet) (The Liberals)
- Kristdemokraterna (The Christian Democrats)
- Moderaterna (The Moderate Party)
- Socialdemokraterna (The Social Democratic Party)
- Vänsterpartiet (The Left Party)
- Miljöpartiet (The Green Party)
- Sverigedemokraterna (The Sweden Democrats)
- Piratpartiet (The Pirate Party)
- Feministiskt initiativ (The Feminist Initiative)
- Junilistan (The June List)
- Annat parti (Other party)

2. In political matters, people talk of left and right. How would you place your views on this scale, generally speaking? 0: Left ... 10: Right.

3. To what extent do you believe that people in general can be trusted? 0: You cannot be careful enough ... 10: Most people can be trusted.

4. To what extent do you think success in life is determined by luck and connections rather than hard work? 0: Success only depends on luck and connections ... 10: Success only depends on hard work.

5. To what extent do you believe that cooperation is needed in order to become successful? 0: Cooperation is necessary for success ... 10: It is possible to become successful on one's own

6. Do you believe that having money is important to be happy? 0: Money is not indispensable for happiness ... 10: Money is indispensable for happiness

7. Below are some proposals that have figured in the political debate. What is your opinion about each proposal? Very bad proposal / Bad proposal / Neither good nor bad proposal / Good proposal / Very good proposal.

- Reduce the public sector
- Reduce the tax on labor income
- Reduce the tax on capital income
- Sell government-owned companies and businesses to private buyers
- Privatize more health care services
- Prohibit paying dividends in tax-financed health care, schools, elderly care and child care
- Reintroduce the property tax
- Reintroduce the inheritance tax
- Reintroduce the wealth tax
- Reduce income differences in the society
- Increase economic support to rural areas
- Introduce six-hour workday for all paid work
- Invest in increasing the equality between women and men
- Invest more in preventing environmental degradation
- Reduce carbon dioxide emissions
- Introduce language tests to be eligible for Swedish citizenship
- Reduce aid to developing countries
- Admit fewer refugees to Sweden
- Sweden should leave the EU
- Sweden should apply for a NATO membership

- Sweden should work for increased free trade in the world

8. The average municipal tax on labor income is currently 32 percent. How high do you think that the municipal income tax should be? ___ percent

9. The tax on capital income, such as interest income and dividends, are in most cases 30 percent. How high do you think that the tax on capital income should be? ___ percent

10. How well do the following statements fit your views? 1: Fits very well ... 5: Fits very poorly

- My future is already determined by fate
- People can overcome any obstacle if they really want to
- Life is hard to predict since it is often completely random
- Like other animals, human behavior follows natural laws
- People have a completely free will
- Willpower can always overcome bodily urges

11. To what extent do you believe that the following behaviors are morally defensible? 0: Can never be defended ... 10: Can always be defended

- Collect sickness benefit without being sick
- Not paying when riding the bus, train or subway
- Avoid to pay tax when one is bound to pay tax by law
- Buy services on the black market
- Pay for sexual services
- Drive a car while intoxicated
- Drive a car faster than the current speed limit

4 Comparison to Wealth Gradients (Post Hoc)

As mentioned in the main text, we also compare our lottery estimates to wealth gradients. The results of this comparison are shown in Table A8 and Figure A11.

Table A8: Comparison to Wealth Gradients (Post Hoc)

	Taxation		Political Attitudes			Beliefs and Values	
	Capital Taxation	Capital vs Labor	Public vs Private	Redistri-bution	Left-Right Placement	Meritocratic Beliefs	Moral Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect	0.044	0.006	0.009	-0.001	0.022	-0.028	0.005
SE	(0.016)	(0.015)	(0.017)	(0.016)	(0.015)	(0.018)	(0.015)
Wealth 2007	0.106	0.116	0.023	0.089	0.075	0.007	0.042
SE	(0.017)	(0.015)	(0.016)	(0.014)	(0.015)	(0.015)	(0.015)
p equal	0.007	<0.001	0.558	<0.001	0.013	0.118	0.081
Effect 2016	0.113	0.027	0.033	0.008	0.056	-0.049	0.049
SE	(0.029)	(0.030)	(0.038)	(0.039)	(0.033)	(0.043)	(0.033)
Wealth 2016	0.079	0.087	0.017	0.067	0.056	0.006	0.031
SE	(0.012)	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
p equal	0.285	0.055	0.680	0.138	0.994	0.211	0.612

This table compares the effect of lottery wealth to wealth gradients estimated using small-prize winners (below \$20K). Effects and gradients are expressed per \$100K. Gradients in the top panel are estimated using registered net wealth in 2007 (winsorized at the 0.5th and 99.5th percentile). The wealth gradients are estimated controlling flexibly for age and sex. The bottom panel show gradients divided by 1.33 and estimated effects of lottery prizes after multiplying the prize variable by $\max\{0, 1 - 0.05t\}$, where t is the number of years that passed between winning and the survey. Because the "$p$equal" refers to the p-value obtained from a Wald test that the lottery estimate and the gradient estimate are equal. Standard errors are clustered at the level of the individual.

The filled circles in Figure A11 show estimated wealth gradients for our primary outcomes based on household net wealth in 2007. The gradients are estimated controlling for sex, a fourth-order polynomial in age and sex-by-age interactions. For example, \$100K higher wealth is associated with a 0.106 SD units higher capital taxation index. This is considerably larger than our lottery-based estimate of 0.044 SD units per \$100K won. However, both the lottery estimate and the gradient reflects a relationship with wealth measured many years prior to the survey. Further, while lottery wealth is spent down over time (see Figure 2), the opposite is true for wealth that players have accumulated prior to winning. For example, between 1999 and 2007, the net wealth of households that had not yet won increased by 33%.[13] Assuming the

[13]The fact that agents accumulate wealth over the life cycle while an exogenous wealth shock is consumed might appear contradictory, but it is consistent with a simple life-cycle model (see e.g. Cesarini et al., 2017).

same rate of increase, \$100K in 2007 corresponds to \$133K at the end of 2016, the year of the survey. Rescaling the gradient for the capital taxation index accordingly results in a gradient with respect to current wealth of $0.106/1.33 = 0.080$ SD units per \$100K (indicated by the leftmost filled square in Figure A11). This re-scaled gradient can be compared to the re-scaled lottery-based estimate in Section 6.2 of 0.113 SD units per \$100K (indicated by the leftmost hollow square in Figure A11). Although this comparison relies on strong assumptions, we note that it is possible to rationalize the entire gradient between wealth and the capital taxation index as a causal effect of wealth.

For the other primary outcomes, Figure A11 shows that the wealth gradients have the expected signs, whereas lottery estimates are smaller (or of the opposite sign) compared to the gradient. Performing the corresponding re-scaling of both the gradient and lottery estimates as for the capital taxation index, however, tend to move the gradient and causal estimates closer to each other. After re-scaling, we cannot reject that gradients and treatment effects are statistically distinguishable for any of the primary outcomes (see Table A8).

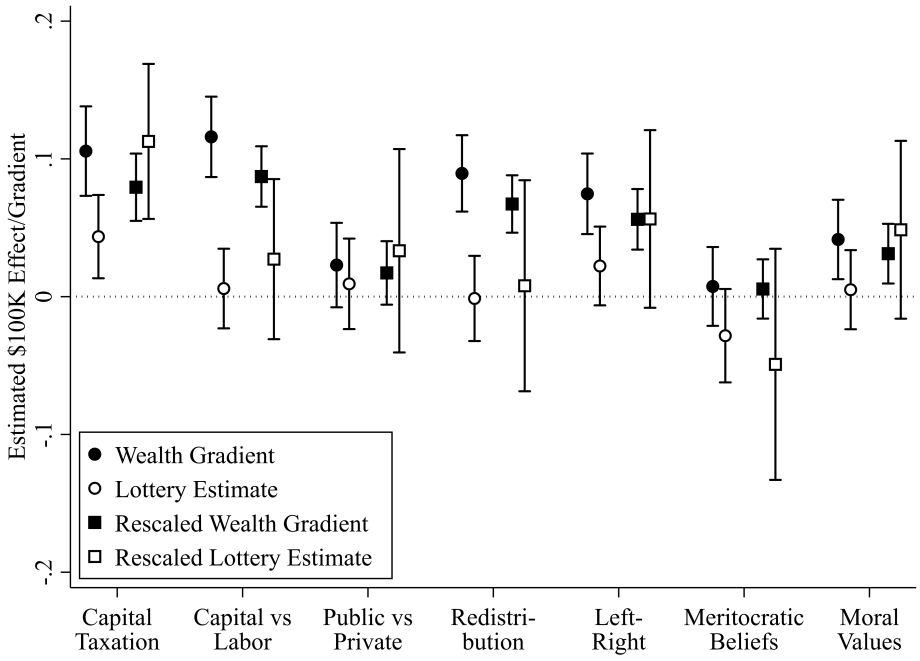

Figure A11: Comparison to Wealth Gradients (Post Hoc)

This figure compares the effects of lottery wealth to wealth gradients estimated using small-prize winners (below \$20K). Effects and gradients are expressed per \$100K. The solid circles show gradients estimated using registered net wealth in 2007 (winsorized at the 0.5th and 99.5th percentile) controlling flexibly for age and sex. The solid squares shows the same gradients divided by 1.33. The hollow circles shows the baseline lottery estimates and the hollow squares show estimated effects of lottery prizes when the prize variable is multiplied by $\max\{0, 1 - 0.05t\}$, where t is the number of years that passed between winning and the survey.