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POCKETBOOK POLITICS: THE IMPACT OF WEALTH ON POLITICAL PREFERENCES AND PARTICIPATION

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Pocketbook Politics: The Impact of Wealth on Political Preferences and Participation
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ABSTRACT

The rich tend to support policies favoring the affluent and are over-represented among both voters and legislators. This paper investigates whether these correlations reflect causal effects of wealth by leveraging random, positive wealth shocks in the form of lottery prizes. Compared to suitably matched controls, large-prize winners are no more likely to cast votes in national elections or run for political office. We also find no significant effects of parents' lottery winnings on their children's political participation. But winners of large lottery prizes become more negative toward taxes on wealth, real estate and inheritances. Although we do not detect any statistically significant effects on other political preferences, effects tend to 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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A data appendix is available at http://www.nber.org/data-appendix/w32777
A Pre-Analysis Plan for Participation Outcomes is available at https://osf.io/kzc6e
A Pre-Analysis Plan for Political Preferences is available at https://osf.io/t3qb5r

1 Introduction

A key promise of modern democracy, when first established, was that it would break the link between economic wealth and political power, ensuring that the preferences of the poor carry the same weight as those of the rich. Yet there is little indication that any democratic country has realized this lofty ideal. Instead, the economically well-off have been shown to be over-represented among both voters and legislators across democracies in the Americas and Europe (Verba, Schlozman, and Brady, 1995; Carnes and Lupu, 2015; Blais, 2006; Dal Bó et al., 2017; Carnes, 2013). From a democratic perspective, this representational bias is problematic if political preferences are structured along economic lines. There is large a literature showing that income and wealth are indeed strongly associated with various forms of political attitudes. For example, studies report that affluent individuals are less likely to favor economic redistribution (Mengel and Weidenholzer, 2023; Corneo and Grüner, 2002; Alesina and Giuliano, 2009), more likely to support right-wing political parties (Lewis-Beck and Nadeau, 2011; Persson and Martinsson, 2018), and have more positive outlooks on free trade and globalization (Mayda and Rodrik, 2005).

While income cleavages in political attitudes and the over-representation of the rich among voters and politicians are well-documented, it is less clear whether these observed differences reflect causal effects of economic resources. Credibly estimating the causal relationships is important in light of concerns that increased economic inequality in the Western world over the past decades may have fostered political inequality (Solt, 2008; Bartels, 2018) and that "we may increasingly get government by the wealthy (Kenworthy, 2022, p. 6)" as income inequality grows. Such fears are easy to justify if economic affluence affects citizens' political preferences or their likelihood of engaging politically. In this paper, we estimate the causal impact of a positive wealth shock on individual political attitudes, and the likelihood that individuals, and their children, vote or pursue political office.

The main reason previous evidence is scant is the methodological challenges involved in identifying the effect of changes to wealth or income. Economic status is correlated with a multitude of other factors – for example education and family background – that may determine opinons and political engagement. We overcome the identification challenge by exploiting the randomized assignment of prizes in four Swedish lotteries. In each lottery, some players were randomly assigned large monetary prizes. By comparing their outcomes to those of ex ante identical players who won smaller prizes or did not win at all, our study design comes very close to replicating a randomized experiment. The lottery data comprise over 400,000 people who in total won approximately one billion dollars. Our main analyses were pre-specified in separate pre-analysis plans for political participation (Brännlund et al., 2020) and political preferences

(Cesarini, Lindqvist, and Östling, 2016).[1] Any additional analyses in the paper that were not specified in the analysis plans are clearly marked as post hoc.

We first examine whether lottery wealth alter political preferences. Based on standard political economy models with self-interested voters, we expect wealth gains to change political attitudes, as people benefit from different types of policies depending on their economic standing. However, research on political socialization indicate that political attitudes and behavior may be less amenable to change than self-interest models suggest. One reason is that political values and outlooks tend to form during adolescence, after which they crystallize and become difficult to change (Neundorf and Smets, 2017).

We study the impact of wealth on political attitudes using data from a survey with detailed questions about political preferences, moral values, and beliefs that we administered 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. To address concerns about priming effects (cf. Rohrer, Pashler, and Harris, 2015), our survey was administered by a third party, and the survey instructions made no mention of lotteries.

Our survey shows winners of large prizes are more negative towards taxes on wealth, real estate and inheritances. Overall, there is little evidence of heterogeneous effects, but post hoc analyses suggest effects are larger for winners who won closer in time to answering the survey. One interpretation of this finding is that treatment effects fade over time as lottery wealth is dissipated and the self-interest motive weakened. We benchmark our estimated effects to cross-sectional income and wealth gradients and find the effect of lottery wealth on preferred capital taxes to be similar to the gradients. Our results are thus consistent with self-interest being a key explanation for why the wealthy prefer lower taxation on capital.

In contrast, we find no statistically significant effects on attitudes toward other aspects of economic policy, such as privatization, redistribution, and self-placement on the left-right scale. Likewise, lottery winnings do not significantly affect beliefs in meritocracy or attitudes toward the consumption of "immoral goods". In post hoc analyses, we detect no effects of lottery winnings on attitudes toward globalization, immigration, or the environment. However, though the effects on specific policy questions are statistically insignificant, the general pattern of estimated effects is consistent with a shift toward the political right. Consistent with such a shift, we find suggestive evidence that lottery winners become more likely to support right-wing political parties.

Having estimated the effect of lottery wealth on political preferences, we ask whether financial wealth encourages higher voter turnout, as argued by many political scientists (e.g. Lijphart, 1997). To study this question, we match the lottery data to digitized records of

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

individual-level turnout in elections to the Swedish national parliament and the European parliament. Because turnout is much higher in elections to the national parliament (91% in our sample) than the European parliament (54%), these two types of elections capture different margins of participation. In the cross-section, turnout is strongly positively correlated with both own and parental household income. In sharp contrast, we estimate precise zero effects of lottery wealth on turnout in both types of elections and for both players and their children. We strongly reject effect sizes of the same magnitude as the corresponding income-turnout gradients.

To study entry into politics, we utilize population-wide data encompassing all political nominations for elections at the municipal, regional, and national levels spanning over three decades. In the Swedish electoral system, candidates are nominated by political parties, making a nomination contingent on both the willingness to participate and the approval of a political party. Dal Bó et al. (2017) document that Swedish political parties employ a screening process for prospective candidates that results in positive selection regarding personal income, while maintaining representativeness in terms of parental socioeconomic background.

Our estimates of the causal effect of wealth on candidacy are close to zero and lack statistical significance, for both winners and their offspring. For instance, our point estimate suggests a $\$100,000$ increase in wealth reduces the propensity of winners to be nominated by a mere 0.064 percentage points, constituting a 3% reduction relative to the baseline probability. At least in Sweden, the widely discussed over-representation of the wealthy among voters and politicians does thus not appear to be driven by economic resources *per se*, but by other factors related to economic affluence.

Our findings contribute to the small quasi-experimental literature that studies the effects of economic shocks on political attitudes and behavior (surveyed in Margalit, 2019). In particular, our paper relates to three previous studies that examine how positive economic shocks in the form of lottery winnings impact individual political preferences (Doherty, Gerber, and Green, 2006; Peterson, 2016; Oswald and Powdthavee, 2014). In line with our results, Doherty, Gerber, and Green (2006) did not detect any changes in overall political attitudes but found evidence that players who won large prizes became more hostile toward estate taxes. Also consistent with our findings, Oswald and Powdthavee (2014) and Peterson (2016) report that lottery winners are more likely to identify with parties to the political right, although Peterson (2016) only found an effect among those not previously registered with a party.[2] We provide an in-

[2]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 increases 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

depth comparison to the previous lottery studies in Section D of the Appendix, but in short we complement these earlier lottery studies by studying a larger sample and a broader set of outcomes.

We are the first to study the effect of lottery wealth on political participation, but a couple of recent studies use other sources of variation to estimate the effect of income on political activity. Akee et al. (2020) found no effect of unconditional cash transfers stemming from the opening of a casino in a Native American reservation on turnout of adult beneficiaries, but their results suggest increased turnout of children raised in poor families. Hirvonen, Schafer, and Tukiainen (2022) found an increase in turnout among benefit recipients in a basic income experiment in Finland, though they attribute this finding to improved social capital rather than income per se. Our results differ from these previous studies in that we consistently find no effect of lottery wealth on political activity.[3]

2 Data and Identification Strategy

This study uses data from four administrative samples of lottery players previously used to study the effects of lottery wealth on health and children's outcomes (Cesarini et al., 2016), labor supply (Cesarini et al., 2017) and participation in financial markets (Briggs et al., 2021). We match the lottery data to adminstrative data on socioeconomic variables of both lottery players and their children. We begin by a brief description of the lottery data.

2.1 Lottery Data

The first two lottery data sets come from *Triss*, 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).

compared to studies of individual lottery winners.

[3]Another related study is Schaub (2021), who found a drop in reported vote intentions among survey respondents in Germany during what he refers to as long bank months, i.e., months with a longer spacing between monthly salary payments. There is also a literature that studies the effect of conditional cash transfer programs on support for incumbent politicians, see Araújo (2021) for a review.

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.

We refer to Cesarini et al. (2016) for additional details about the lottery data.

2.2 Survey Data

To measure political preferences, we use a survey fielded to a subset of the participants in the Triss and Kombi lotteries by Statistics Sweden during the fall of 2016. We choose not to include PLS in the survey because most of the large prizes were awarded in the 1980s and 1990s, making it less likely to detect treatment effects on outcomes measured in 2016. Because prizes in the Triss and Kombi lotteries were awarded between 1994 and 2011, 5 to 22 years had past between the lottery event and the time of the survey. Survey responses were matched to administrative variables and pseudonymized before Statistics Sweden delivered them to us. The survey have previously been used to study subjective well-being (Lindqvist, Östling, and Cesarini, 2020) and subjective health (Östling, Cesarini, and Lindqvist, 2020).[4]

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. 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.[5] We

[4]Because these papers use the same data and are based on the same pre-analysis plan, the description of the data in this section bears a strong resemblance with these previously published papers.

[5]"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.

also imposed a number of additional minor sample restrictions.

The sample restrictions left us with 259 large prizes from Kombi, 3,294 Triss-Lumpsum 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 (to be described below) for each large-prize winner in Kombi to the Survey Population. 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.[6] 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 the survey after the third reminder by telephone and asked them to return the mail-in survey. Moreover, after the survey-data collection via mail had ended, Statistics Sweden randomly selected 501 non-respondents (from all lotteries) and contacted them by telephone. Subjects whom they managed to reach 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 A2 shows the prize distribution and response rate for both the the Survey Population and Respondents Sample.

2.3 Political Participation Data

In our analyses of political participation, we include lottery players from all four lotteries 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.

[6]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.

Swedish general elections are held in September every four years (before 1994, 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. Elections to the European Parliament are held in June every five years. Our information about individual-level voting decisions are based on district-level electoral rolls which lists the name and unique personal identification number of each eligible voter. In a recent effort, the complete electoral rolls for the 1970, 1994, and 2010 general elections in Sweden as well as the Swedish 2009 European Parliament election have been scanned and digitized, resulting in validated information on turnout for all the individuals who were eligible to vote. The reliability of this digitized turnout data have been shown to be very high (Lindgren, Oskarsson, and Persson, 2019). 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, nearly all voters either cast a ballot in all three elections or abstain from all three.[7] 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.

To measure entry into politics, we rely on data from the Register of Nominated and Elected Candidates. This register contains information about all nominated and elected candidates in the eleven parliamentary, county council, and municipal elections in the period 1982–2018.

We define the *Winner Participation Sample* and *Child Participation Sample* as the samples for which we observe either data on turnout or candidacy for winners and their children, respectively. Moreover, as described in Section 2.5, we restrict the Child Participation Sample to winners with up to three children at the time of winning. Table B1 shows the prize distribution for the two samples.

2.4 Identification Strategy

We gauge the causal impact of lottery wealth by estimating the following equation using ordinary least squares:

$$y_{it} = \alpha L_{i,0} + \mathbf{Z}_{i,-1}\boldsymbol{\gamma} + \mathbf{X}_i\boldsymbol{\beta} + \varepsilon_i,$$

where y_{it} is an outcome variable measuring election turnout, political candidacy, or a survey

[7] 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.

reponse measured t years after the lottery event (the time of the lottery event is normalized to $t = 0$). $L_{i,0}$ denote the lottery prize (in \$100,000) awarded to individual i, and $\mathbf{Z}_{i,-1}$ is a vector of pre-specified 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. Following previous work using the same data, 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 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 matched controls. 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. The survey was sent to four such controls per winner, whereas we use up to 100 controls in our analyses of participation. If there were fewer than 100 non-winners, 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 construction of the lottery group identifiers for the Child Participation Sample is identical to the adult samples, except that the unit of observation is a child of a lottery-winning parent.

As shown in Table A2 and B1, most lottery prizes in our data are relatively modest. However, the largest prizes contribute most to the identifying variation. For example, if we drop the 662 prizes above \$100,000 in the Respondents Sample, the overall variation in lottery prizes drops by 99 percent. Although the Respondents Sample is relatively small, the identifying

variation is about one third of that in the Winner Participation Sample.

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 (in our analyses of winners) or extended family (in our intergenerational analyses). Because clustered standard errors can be unreliable when variables are skewed (as lottery prizes are), we also report non-parametric p-values constructed by permuting the distribution of prizes within lottery groups and re-estimating the main estimating equation 10,000 times (Young, 2018).[8] 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).

2.5 Exogeneity Tests

The pre-analysis plans described a set of tests of the conditional random assignment of lottery prizes. In these tests, we regress lottery prizes on the vector of baseline characteristics. Conditional random assignment implies that, when the lottery group identifiers are controlled for, we should not reject the null that all of the coefficients of the covariates in the vector of baseline characteristics are zero.

As reported in the previously published work based on the same survey (Lindqvist, Östling, and Cesarini, 2020; Östling, Cesarini, and Lindqvist, 2020), we pass the test of conditional random assignment in both the Survey Population and Respondents Sample (see Table A3) and find no evidence that lottery wealth influence survey participation (see Table A4). We also document the treatment effect estimates of lottery wealth on financial outcomes are similar in the Survey Population and Respondents Sample (Table A5). The fact that lottery winnings do not predict survey participation and that the effect on financial outcomes are similar among respondents and non-respondents show that endogenous selection is unlikely to bias our estimates, but bias cannot be completely ruled out (for example, the absence of an aggregate effect of wealth on survey participation could mask heterogeneous effects correlated with unobservables in turn related to outcomes).

Table B2 shows that we do not reject the null hypothesis of joint insignificance in the Winner Participation Sample, thus supporting the assumption of conditional random assignment. However, as shown in Table B3, we reject the null hypothesis for the winners' children. Previous studies using the same lottery data have not found any evidence of randomization failure. However, as the control variables and the estimation sample differ across studies, it is unsurprising that conditional random assignment could be rejected in some subsamples. In the event this

[8] We pre-registered slightly different methods for our analyses of survey outcomes and participation. In our analyses of survey outcomes, we compute p-values based on estimated coefficients, whereas we base the p-values on observered test statistics in our our analyses of participation. The two methods correspond to "randomization-c" and "randomization-t" in Young (2018).

would happen, our pre-analysis plan stated we would impose sample restrictions in order to achieve covariate balance. As it turns out, the main driver of the imbalance is that family size is correlated with amount won.[9] When we remove families with four children or more, we pass the test of conditional random assignment (i.e., we no longer reject the null hypothesis that the coefficients on all covariates are zero). We therefore exclude families with four children or more (about 12% of the sample) from the Child Participation Sample. There are other ways to make the sample balanced and Table B4 shows we pass the test of conditional random assignment for two alternative specifications. Table B10 shows our main results are qualitatively unchanged if we instead use the unrestricted sample (including families with four or more children) or the the two alternative specifications.

2.6 Representativeness and External Validity

A potential threat to our study's external validity is that lottery players might be unrepresentative of the general population. In Section E of the Appendix, we therefore compare lottery players to representative samples matched on sex and age. Compared to the representative sample, the lottery players are less likely to have immigrated to Sweden and to hold a college degree, but have slightly higher incomes. Lottery players in the Winner Participation Sample are more likely to being eligible and having voted in previous elections (71.7% vs. 67.4%) and to have run for political office (3.0% vs. 2.4%). The Respondents Sample is overall quite similar to the representative samples in terms of political preferences, but are less supportive of reducing the size of the government and more likely to support left-wing parties (48.4% vs. 42.3%). The stronger support for left-wing parties is mainly due to players in the Kombi lottery, which is unsurprising given that the Kombi lottery is owned by the largest left-wing party (the Social Democrats). Overall, the similarity in baseline characteristics and modest differences in terms of political participation and political preferences 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.

Another potential barrier to generalizability is that the impact of 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). We cannot rule out that the effect of lottery wealth is different,

[9] As shown in Table B3, our rejection of the null hypothesis is due to the birth order variables. Birth order strongly correlates with family size, which in turn is associated with the amount won, especially in PLS. This pattern does not appear in the Winner Participation Sample. In unreported analyses we have explored many potential explanations, such as parents holding PLS accounts in their children's names or children's winnings being reported on their parents' account. We have not been able to find a plausible explanation for the correlation between family size and amount won. In addition, the problem is compounded by analytical standard errors being underestimated in our analyses of children due to the smaller sample size and some lottery groups with few individuals.

but we can partly address two common concerns. First, a common notion is that lottery wealth is often squandered. However, previous research on Swedish lottery players have found that winners spend down their winnings over the course of several decades (Cesarini et al., 2016); 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 more than a decade after winning (Lindqvist, Östling, and Cesarini, 2020). Survey evidence indicates social status considerations play little role when Swedish lottery winners spend their winnings (Larsson, 2011). A second concern is that lottery winners might feel they have unjustly received the prize and do not deserve their lottery wealth. However, Cesarini et al. (2017) have documented that married winners keep most of the prize money for themselves and reduce labor supply more than their non-winning spouses, suggesting they feel entitled to their prize.

3 Political Preferences

We begin by reporting the results from our analyses of political preferences. Our pre-analysis plan specified seven primary outcomes, see Table A6 for detailed definitions and summary statistics and Table A7 for their pairwise correlations. Three of our primary 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 2 and their pairwise correlations are illustrated in Figure A2.

In the pre-analysis plan, we hypothesized that attitudes toward capital taxation were most likely to be influenced by a wealth shock. We pre-specified two primary measures of attitudes to capital taxes. The first is an index 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 taxes on wealth and real estate were levied annually and based on value of assets, whereas the inheritance tax was levied at the time of distributing the deceased's estate. The index is also based on a question about whether the capital income tax should be increased. The second primary outcome 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.

Table 1 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). We 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. The effect is also statistically significant at the five-percent level after our pre-specified correction for multiple hypothesis testing. Post hoc analyses reported in Table 2 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 on attitudes toward taxes on inheritances and real estate. However, 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 paid annually and typically withheld by employers, whereas taxes on wealth and property constituted annual payments, potentially making the latter 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 income flows. The strength of the self-interest motive could also matter. Table A5 shows that a $100K lumpsum prize increases winners' 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. These effects corresponds to a movement in the wealth distribution by 4.4 percentiles, but only 2.3 percentiles in the capital income distribution and 0.4 percentiles in the labor income distribution. Moreover, for a winner of a $100K prize, the capital income tax would increase by approximately $343, whereas the labor income tax would fall by approximately $365. If the old Swedish wealth tax was reintroduced, however, a $100K winner would pay up to $803 in wealth tax every year.[10]

[10]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 1: Effects of Lottery Wealth on Primary Survey 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
p (FWER)	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 survey 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 10,000 times 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.

Our next three primary outcomes were included to test whether lottery wealth generates a broader attitudinal shift. As shown in Table 1, we do not find statistically significant evidence that this is the case. The third outcome measures individual attitudes toward the appropriate scope and size of government whereas the fourth oucome measures attitudes toward redistribution. The estimated effects on these outcomes 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 2).

The fifth outcome 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"). The point estimate suggest lottery wealth does tend to push winners to identify more with the political right, 0.022 SD units per $100,000 won, but the effect is not statistically distinguishable from zero.

Our final two outcomes were included to test whether lottery wealth impact beliefs and moral values. We measure 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?". Belief in meritocracy have previously been shown to be positively correlated with preferences for redistribution (Fong, 2001; Alesina, Glaeser, and Sacerdote, 2001; Alesina and Angeletos, 2005). We do not find that large-prize winners

self-servingly adapt beliefs. To the contrary, Table 1 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. (2023), we find no evidence of motivated reasoning in the sense that lottery winners reduce their belief in the importance of luck for economic success.

Our final primary outcome 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. As shown in Table 1, we find no evidence that lottery winners self-servingly adjust moral values.

3.1 Robustness

The pre-analysis plan specified two robustness tests. The first test adjusts for selective non-response by weighting respondents to the abbreviated 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 not ask the questions about moral values in the telephone survey. Table A8 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 A8), 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. To further explore 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 do not provide evidence that contradicts our baseline linear specification.

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

Table 2: 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.

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.

3.2 Heterogenous Effects

The pre-registered analyses of heterogeneous effects are shown in Figure A10 and Table A9 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 A9, 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 0.099, $p = 0.047$).

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 where we split the sample into six categories depending on year of win. The left panel of Figure 1 shows the effect on the capital taxation index appears to fade with time since the lottery event. To assess whether this trend is statistically significant, we residualize lottery prizes and the capital taxation index and estimate one treatment effect for each lottery group. We then regress the treatment effect estimates on years since winning weighted by the total variation in lottery prizes within each group. This analysis shows that the intercept term, which corresponds to the predicted effect of winning at the time of the survey, is 0.108 ($SE = 0.041$) SD units per \$100K won. The relationship between estimated treatment effects and years since winning is negative, but the relationship is not statistically significant at the five percent level ($p = 0.088$). Figure A8 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.

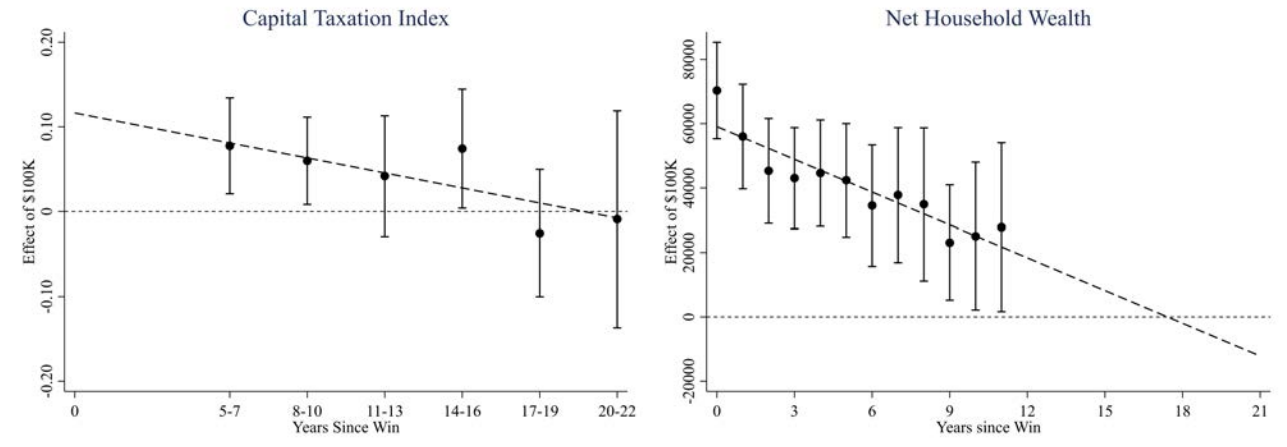

Figure 1: Treatment-Effect Heterogeneity by Year Won (Post Hoc)

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 1 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.[11] 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 1 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 wealth. Doing so, we get an estimate of 0.113 SD units per $100K, which corresponds closely to the predicted effect of winning at the time of the survey (0.108). The similarity of these two 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.

[11] 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.

3.3 Additional Outcomes

We now turn to some post hoc analyses of additional outcomes in our survey. Based on the policy items in our survey that were not included in the construction of primary outcomes, we code three additional indexes about immigration, environmentalism and globalization. Table 2 lists which questions are included in the construction of each index. As also shown in Table 2, 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 2, 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 2 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 2 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 large lottery prizes are more positive towards implementing a six-hour work day, whereas the predicted effect is the opposite (because respondents on the political left 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, Figure 2 suggests the effects of lottery wealth on a given question depend on the correlation with the wealth tax question. This suggests 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 we are underpowered to detect with the current study design.

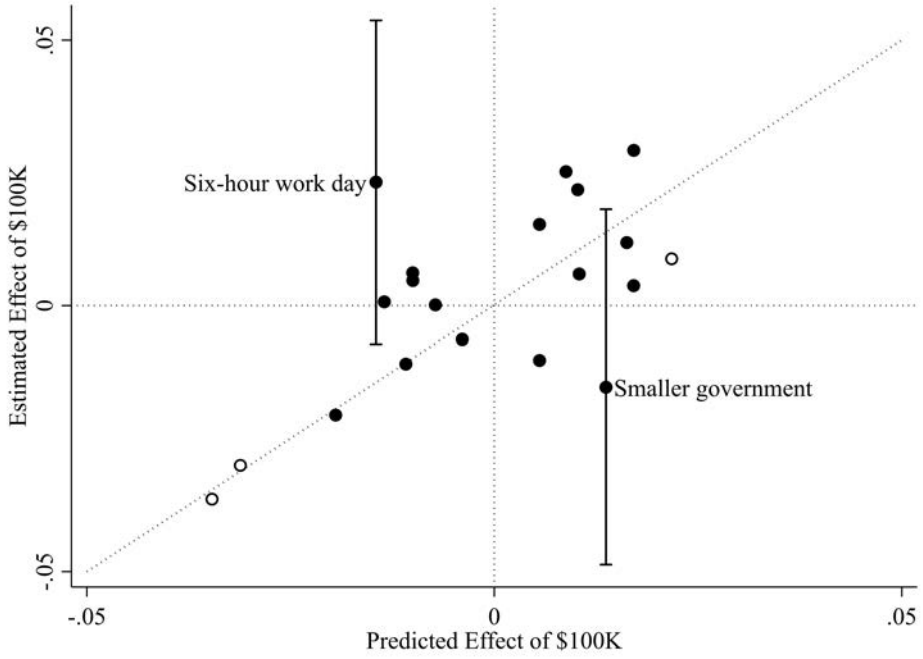

Figure 2: Predicted vs Estimated Effects on Attitudes to Policy Proposals (Post Hoc)
This figure displays the estimated effects of lottery wealth on 20 different policy proposals against the predicted effect under the assumption that the effect is completely mediated by the effect on attitudes to reintroducing 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.

Table 3 displays post hoc analyses of the effect of wealth on some additional belief measures. The first column shows the effect on the index of market beliefs similar to the index used by Di Tella, Galiani, and Schargrodsky (2007). Our index is based on three questions and constructed 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.[12] There is no effect on this index or its subcomponents. It is noteworthy that there is no

[12]In Di Tella, Galiani, and Schargrodsky (2007), an individual's score on the index of market beliefs was

Table 3: 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.

effect on generalized trust, which is one component of the index, despite previous studies not only showing a positive correlation between income and trust (Alesina and La Ferrara, 2002), but also suggesting the relationship is causal (Ananyev and Guriev, 2019).

Table 3 also shows the effect on metaphysical beliefs related to free will and determinism. Apart from the question about belief in luck vs effort for success discussed above, we also included survey items related to measures of beliefs about the nature of reality: belief in free will (based on three questions), belief in fate, and belief in unpredictability.[13] As shown in Table 3, the estimated effects are very close to zero for all three measures of metaphysical beliefs.

3.4 Benchmarking Effect Sizes

All our effects are scaled in units of $100K. When assessing the magnitudes of the estimated effects, keep in mind that a $100K wealth increase is relatively small from a life-cycle perspective. If $100K is annuitized over a 20-year period, it increases net annual income by $5,996 (assuming a discount factor of 2 percent). This corresponds to a movement of less than one decile in the distribution of household disposable income. Another possible comparison is that the annual net full-time wage difference between a Swedish physician and nurse of about $25K in 2011 is approximately equivalent to a $400K prize.

To further assess effect size magnitudes, we follow the pre-analysis plan and compare our lot-

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.

[13] We included five questions from the 27-item battery of questions used by Paulhus and Carey (2010) in our survey. 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.

tery estimates to cross-sectional income gradients and estimates from previous lottery studies. The top row in Table 4 shows the results when our lottery estimates are converted to annual 20-year income streams. For example, the re-scaled lottery estimates shows $10K higher annual income correspond to 0.073 S.D. increase in the capital taxation index. 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 4 shows the gradients between all primary outcomes and income are positive, although the gradient for moral values is not statistically significant. The rescaled lottery estimate for the capital taxation index is twice as large as the corresponding gradient, but we cannot reject the null hypothesis that the lottery estimate and the gradient are equal ($p = 0.169$). The gradient is larger than the rescaled lottery estimates for all other outcomes, but we can can only reject that the lottery estimate and the gradients are equal for the redistribution index and meritocratic beliefs.

Table 4: Comparison to Household Income Gradients

	Taxation		Political Attitudes			Beliefs and Values	
	Capital Taxation	Capital vs Labor	Public vs Private	Redistribution	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 benchmarking lottery estimates to household-income gradients is that wealth and income likely impact political preferences differentially. This is particularly the case for attitudes toward taxation – attitudes to capital taxes are arguably more closely tied to wealth than income, while the opposite is true for income taxes. In Section F of the Appendix, we therefore report the results from a post hoc comparison of our lottery estimates to wealth

gradients. As for the income gradient, the appropriately rescaled lottery estimate for the capital taxation index is larger than the corresponding wealth gradient. Our findings are thus consistent with self-interest fully explaining why the wealthy prefer lower taxation on capital.

In Section D of the Appendix, we also compare our estimates to those in previous lottery studies. It is difficult to make exact quantitative comparisons, but our best attempt for the outcomes that are most comparable to previous studies suggests our effect sizes are of similar magnitude to those reported in Doherty, Gerber, and Green, 2006 and Peterson, 2016. In contrast, our estimates are substantially smaller than Oswald and Powdthavee (2014) who estimates effects orders of magnitude larger than the gradients.

4 Political Participation

We now turn to our second question whether lottery wealth impacts the propensity to vote and be nominated for political office. We start by analyzing turnout.

4.1 Turnout

Our pre-anaysis plan defined two primary outcomes for voter turnout. Turnout in national elections is measured by an indicator equal to 1 for having voted in the first national parliamentary election after the lottery event for which we have digitized information (1994, 2010 or 2018). For example, for a player who participated in a lottery event in 1997, the variable reflects turnout in the 2010 election. Turnout in EU elections is 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.

Table 5 shows the effect of winning \$100K on turnout of both winners and their children. The table also shows the results for political candidacy which we describe separately in the following subsection. We estimate winning \$100K increases winners' turnout in national elections by 0.097 percentage points and decreases turnout in elections to the EU parliament by 0.627 percentage points, but none of these estimates are statistically significant. The effects of lottery winnings on children are negative, but close to zero and not statistically significant. Our estimates are quite precisely estimated. For lottery players, the standard error for national turnout is 0.241 percentage points for a lottery win of \$100K. For children, we have a smaller sample and somewhat lower precision ($SE = 0.664$), but we can still reject positive effects on turnout in parliamentary elections larger than 0.601 percentage points.

To put these results into context, we benchmark them against disposable household income gradients following a procedure laid out in the pre-analysis plan. In short, we match each player

Table 5: Effects of Lottery Wealth on Political Participation

	Winners			Children		
	Turnout National	Turnout EU	Political Candidacy	Turnout National	Turnout EU	Political Candidacy
	(1)	(2)	(3)	(4)	(5)	(6)
Effect ($100K)	0.097	-0.627	-0.064	-0.700	-0.193	-0.047
SE	(0.241)	(0.425)	(0.096)	(0.664)	(0.774)	(0.122)
p (analytical)	0.688	0.140	0.502	0.292	0.803	0.700
p (resampling)	0.698	0.148	0.508	0.325	0.811	0.728
p (FWER)	0.924	0.894	0.998	0.878	0.852	0.999
Avg. dep. var. (%)	90.6	54.1	2.1	87.4	49.2	1.5
N	335,989	288,205	413,349	92,043	92,925	96,205

This table reports the treatment effect of $100K on indicator variables for turnout and political candidacy for winners and winners' children. The estimates have been multiplied by 100, so a coefficient of 1.00 means that $100K increases participation 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 in the analyses of lottery winners and at the family level in the intergenerational analyses. The resampling-based p-values are obtained by simulating the distribution of t statistics 10,000 times under the null hypothesis of zero treatment effects. The family-wise error rate (FWER) is calculated separately for winners and children using the free step-down resampling method of Westfall and Young (1993).

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. Figure 3 shows the results from a (post hoc) binscatter plot of household income against turnout. The upper panel of Figure 3 shows turnout in both national and EU elections increases dramatically from the bottom to the top of the income distribution, from about 65% to 95% in national elections and from 25% to 80% in EU elections. The bottom panel shows there are steep gradients also for the relationship between voting and parental household income. The voting gradients are steep despite the cost of voting being very low in Sweden, both in terms of money, time and effort. There is no registration required, election day is always on a Sunday and voting typically only takes a couple of minutes. For those who cannot vote on election day, it is also possible to participate by voting up to a few weeks before election day.

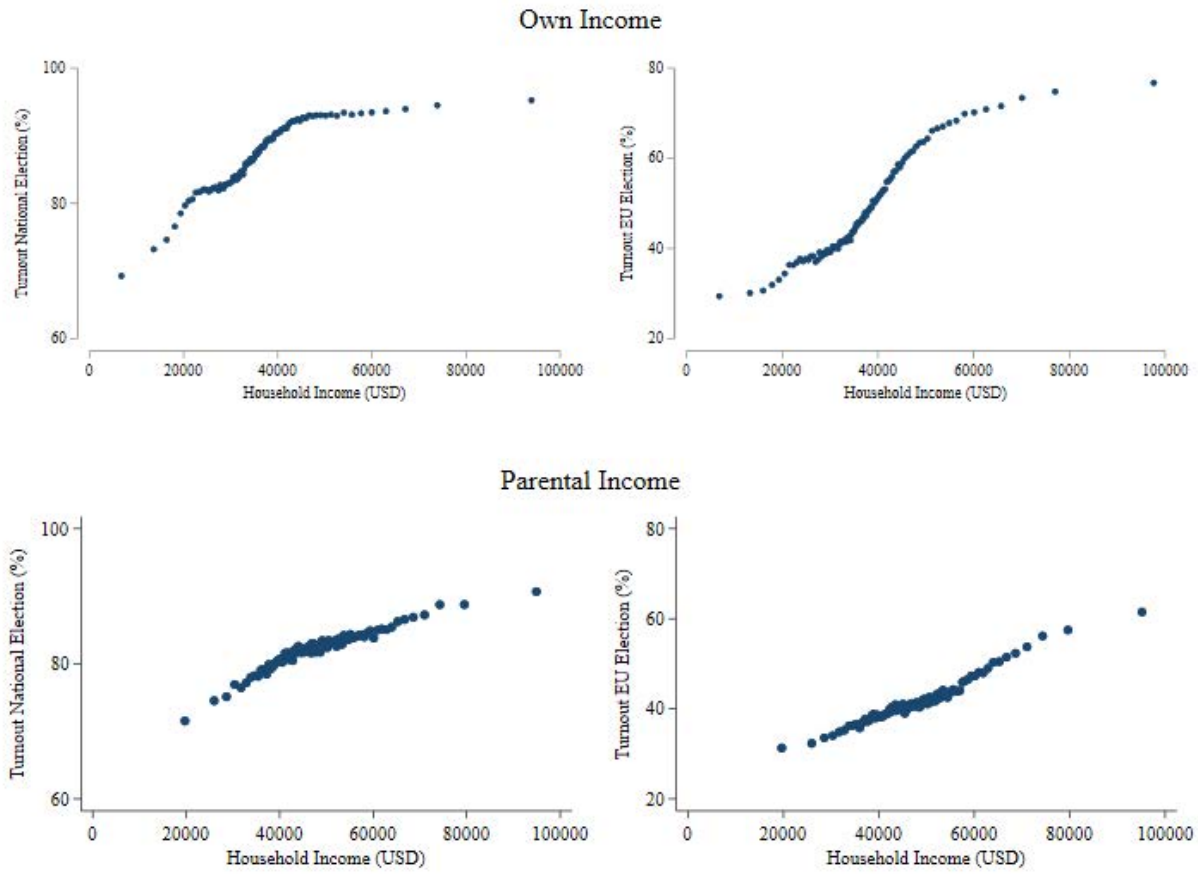

Figure 3: Nonparametric Income-turnout Gradients (Post Hoc)

This figure shows the propensity to vote in national and EU elections for each percentile of the income distribution for the matched lottery sample described in the text. For the adult sample, we control for age, sex, year and marital status, and household income is the average disposable household income five years prior to the year of the lottery event. For children, we control for parental age, year and sex, and income is the average of the sum of both parents disposable income 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 same procedure as described in Section 3.4. Figure 4 shows the lottery-based estimates are much smaller than the corresponding gradients. For lottery players, we can reject causal effects about one quarter (national elections) and one tenth (EU elections) as large as the cross-sectional gradients. For players' children we can reject causal effects about half as large as the gradient for turnout in both types of elections. In sum, our results suggest the strong relationship between turnout and income does not reflect a causal effect of income.

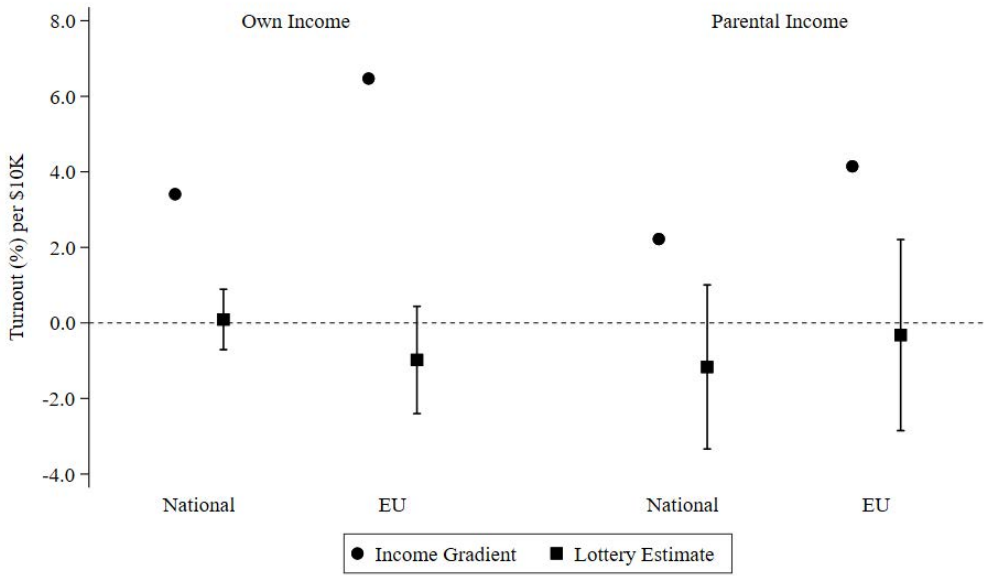

Figure 4: Comparison of Income-turnout Gradients and Lottery Estimates

This figure compares the effect of winning the lottery to the income-turnout gradients in the matched lottery sample described in the text. Lottery estimates are re-scaled assuming lottery prizes are annuitized over 20 years at a 2 percent real rate. Estimates have been multiplied by 100, so a coefficient of 1.00 means that $10K in additional annual income increases participation by one percentage point. For the adult sample, we control flexibly for age and sex, and household income is the average disposable household income five years prior to the year of the lottery event. For children, we control flexibly for parental age, and income is measured as the average of the sum of both parents' disposable income five years prior to the year of the lottery event. Standard errors are clustered at the level of the individual in the analyses of lottery winners and at the family level in the intergenerational analyses.

Robustness and Heterogenous Effects

We now turn to a number of robustness checks and heterogeneity analyses listed in our pre-analysis plan. We pre-specified two analyses to test for non-linear effects in the sample of lottery players – one set of analyses which omit large prize and one that include squared winnings. We see no evidence of non-linear effects (see Table B5). We also pre-specified analyses of turnout in the second and third election after winning the lottery. Table B6 indicates a stronger negative effects of lottery wealth in the third national election following the lottery win, but none these effect are statistically significant.

Table B7 shows the results for lottery players when the estimation samples are split by income, education and previous voter turnout. We only find statistically significant evidence of heterogenous effects with respect to previous abstention – lottery wealth appears to reduce 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. Because statistical precision in the subgroup of players who abstained is low, we interpret this treatment effect difference cautiously. Table B8 shows there is no evidece of heterogeneous

treatment effects by lottery.

We did not pre-specify any robustness analyses for our analysis of winners' children, but performing the same tests as for adults do not suggest the effect of lottery wealth on children's turnout is non-linear. In our pre-registered analyses of heterogenous effects, we split the sample by household income, age at the time of winning and whether the parents abstained in previous elections. The results shown in Table B9 suggest the effect on turnout in national elections is *more negative* when parents are poor ($p = 0.051$), thus strenghtening our conclusion that childhood poverty does not cause low voter turnout. We also observe that the effects on turnout in the national election are more negative for parents that voted in the election preceeeding the lottery win. Finally, Table B10 shows we estimate statistically insignificant effects also with the alternative specifications discussed in Section 2.5.

4.2 Political Candidacy

Compared to voting, running for political office is much more time-consuming. It also requires a nomination from a political party, so it is jointly determined both by an individual's willingness to seek political office and parties' screening of candidates. To analyze political candidacy, we use data on the universe of nominated candidates to the parliament, county councils and municipal assemblies between 1982 and 2018, in total about 250,000 individuals. We code an indicator variable equal to 1 for individuals nominated for political office 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 the year prior to at least one post-lottery election, but who did not run for political office. The variable is set to missing for individuals who were not Swedish citizens or had turned 18 the year prior to at least one post-lottery election.

Figure 5 shows (post hoc) binscatter plots of the relationship between own and parental household income and our political candidacy variable. There is a very strong positive gradient in terms of own income – the probabability of being nominated is almost six times larger for the highest income percentile compared to the lowest percentile. In sharp contrast, but consistent with the characterization of the Swedish political system as an "inclusive meritocracy" (Dal Bó et al., 2017), there is no apparent relationship between parental income and political candidacy. The absence of a correlation between *candidacy* and parental household income does not necessarily imply the absence of a relationship between income and the *willingess* to seek political office. For example, it is possible that the willingness to seek candidacy is negatively related to parental income, but parties screen in a way that compensates for this. In line with this, Dal Bó et al. (2017) show that candidates from weaker socioeconomic backgrounds are more

positively selected.

We now turn to the effect of winning the lottery on political candidacy. Table 5 shows that winning \$100K decreases winners' probability of political candidacy by 0.064 percentage points and decreases children's probability of political candidacy by 0.047 percentage points. None of these estimated effects are statistically significant. If we assume that winning the lottery does not influence how political parties select candidates, our findings suggest that economic resources do not affect the willingness to seek political office.

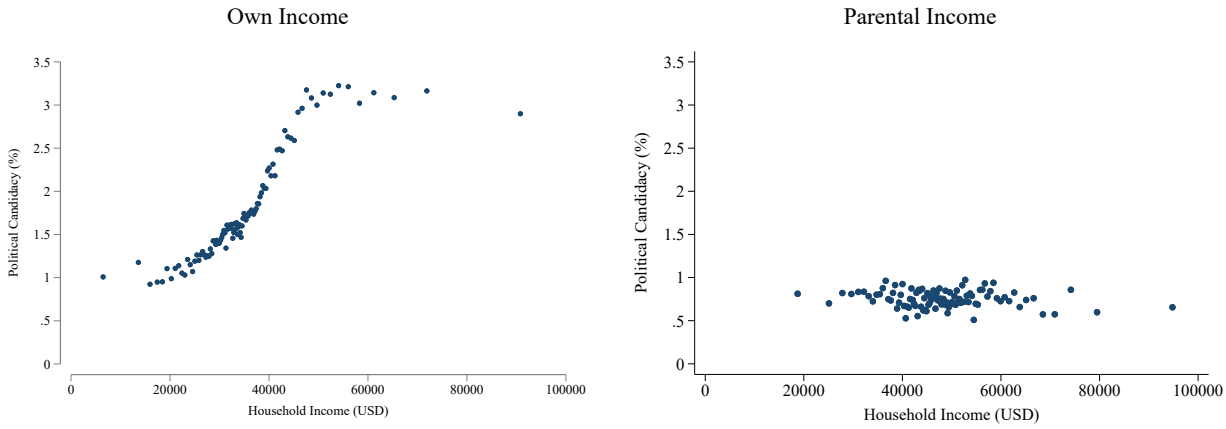

Figure 5: Nonparametric Income-candidacy Gradients (Post Hoc)

This figure shows the propensity to be nominated to a political party for each percentile of the income distribution for the matched lottery sample described in the text. For the adult sample, we control for age, sex, year and marital status, and household income is the average disposable household income five years prior to the year of the lottery event. For children, we control for parental age, year and sex, and income is the average of the sum of both parents disposable income five years prior to the year of the lottery event.

We compare our lottery estimates to income gradients following the same procedure as for turnout.[14] Figure 6 shows that we can reject the strong positive gradient with respect to own income, but unsurprisingly not the close-to-zero gradient with respect to parental income. The parental income gradient is negative in Figure 6, which might appear to contradict the pattern in Figure 5. The estimate is very small, however, and can end up on either side of zero depending on which controls are included.

[14]We pre-registered this comparison although the income-candidacy gradient is a arguably a less relevant benchmark for political candidacy than for turnout. Holding political office comes with a significant time commitment, especially since most political work is done outside regular office hours. The effect of a corresponding wage increase might therefore be different compared to a wealth shock due to the change in the opportunity cost of time as the wage increases.

Figure 6: Benchmarking Participation Estimates: Income Gradients

This figure compares the effect of winning the lottery to the income-candidacy gradients in the matched lottery sample described in the text. Lottery estimates are re-scaled assuming lottery prizes are annuitized over 20 years at a 2 percent real rate. Estimates have been multiplied by 100, so a coefficient of 1.00 means that $10K in additional annual income increases the probability to be nominated by one percentage point. For the adult sample, we control flexibly for age and sex, and household income is the average disposable household income five years prior to the year of the lottery event. For children, we control flexibly for parental age, and income is measured as the average of the sum of both parents' disposable income five years prior to the year of the lottery event. Standard errors are clustered at the level of the individual in the analyses of lottery winners and at the family level in the intergenerational analyses.

We ran the same pre-specified robustness analyses and tests of heterogenous effects for political candidacy as for turnout. The only statistically significant finding is that the effect of winners' candidacy differ by educational background – the effect on candidacy is negative for winners with college education (see Table B7).

5 Political Partisanship (Post Hoc)

One mechanism through which wealth can influence political outcomes is that parties more to the right gain political support. We analyze this question in post hoc analyses using data from both the survey and the administrative data on political candidacy. In our survey, we asked respondents which political party they identified most closely with. Table 6 shows the effect of winning on indicator variables for supporting any of the three conventional left parties, the four conventional right parties and the radical right party Sweden Democrats. We find lottery wealth increases support for the conventional right-wing parties ($p = 0.048$), but we cannot reject equal effects on the support for left and right-wing parties ($p = 0.055$).

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

	Left Parties	Right Parties	Sweden Democrats
	(1)	(2)	(3)
Effect ($100K)	-1.139	1.548	-0.410
SE	(0.745)	(0.774)	(0.588)
p (analytical)	0.127	0.046	0.486
p (resampling)	0.152	0.048	0.462
p (Left = Right)		0.055	
Avg. dep. var. (%)	51.15	35.24	13.61
N	2,815	2,815	2,815

This table reports the treatment effect of $100K on support for political parties in the survey of lottery winners. The estimates have been multiplied by 100, so a coefficient of 1.00 means that $100K increases the support for that party bloc by one percentage point. Conventional left parties include the Left Party, Social Democrats and the Green Party, wherease the conventional right parties are the Center Party, the Liberal Party, the Moderate Party and the Christian Democrats. 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 coefficients 10,000 times under the null hypothesis of zero treatment effects.

The turnout data does not contain information about the party voted for, but the candidacy data does contain information about which party the candidate was nominated by. We use this data to analyze the effect on being nominated by a particular party. Occasionally, the same person is nominated by different parties and in such cases we use the nominating party in the first election after winning. As displayed in Table 7, the pattern of results show the same shift towards the conventional right as in the survey data, but none of these effects are statistically significant.

Table 7: Effects of Lottery Wealth on Party Candidacy (Post Hoc)

	Winners			Children		
	Left Parties	Right Parties	Sweden Democrats	Left Parties	Right Parties	Sweden Democrats
	(1)	(2)	(3)	(4)	(5)	(6)
Effect ($100K)	-0.074	0.037	-0.021	-0.016	0.001	-0.005
SE	(0.044)	(0.071)	(0.020)	(0.062)	(0.099)	(0.008)
p (analytical)	0.091	0.606	0.297	0.800	0.992	0.554
p (resampling)	0.113	0.613	0.412	0.871	0.993	0.761
p (Left = Right)	0.206			0.885		
Avg. dep. var. (%)	0.70	1.11	0.05	0.48	0.82	0.07
N	413,349			96,205		

This table reports the treatment effect of $100K on support for political parties. The estimates have been multiplied by 100, so a coefficient of 1.00 means that $100K increases the support for that party bloc by one percentage point. Conventional left parties include the Left Party, Social Democrats and the Green Party, wherease the conventional right parties are the Center Party, the Liberal Party, the Moderate Party and the Christian Democrats. 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 in the analyses of lottery winners and at the family level in the intergenerational analyses. The resampling-based p-values are obtained by simulating the distribution of t statistics 10,000 times under the null hypothesis of zero treatment effects.

6 Concluding Discussion

In this study, we conduct pre-registered analyses examining the causal impact of an exogenous positive wealth shock on individuals' political preferences, as well as their own and their children's political participation. We find that large, positive wealth shocks in the form of lottery prizes make winners more negative towards proposals to reintroduce taxes on wealth, real estate, and inheritances. Our estimated wealth effects for these outcomes are similar in magnitude to the income and wealth gradients estimated in the cross-section. Moreover, there is suggestive evidence that wealth effects decline over time at a rate roughly proportional to the rate at which the lottery wealth is spent down. These results lend some credibility to the standard political economy assumption that self-interested voters form policy opinions based on their economic standing (Downs, 1957; Meltzer and Richard, 1981). Although self-interest is commonly assumed in political economy models, it is not an uncontroversial assumption. Because any individual voter's probability of casting the pivotal vote in a large election is effectively zero, the benefit of making well-informed voting decisions is negligible (Downs, 1957), and holding on to biased beliefs can be optimal if they offer the slightest psychological benefit (Akerlof, 1989). Or as Caplan (2007, p. 18) succinctly put it: "the price of ideological loyalty is

close to zero". In contrast, our findings show that the self-interest motive is sufficiently strong to change political views in a self-serving direction.

However, we do not detect any statistically significant effect of lottery winnings on preferences concerning other aspects of economic policy, such as privatization, redistribution, and ideological self-placement. We also detect no effects on attitudes towards meritocracy, immoral behavior, globalization, immigration, and the environment. Though we cannot reject zero effects, the general pattern of estimated coefficients are consistent with a re-alignment of opinions toward the political right. One possibility is thus that winning the lottery results in a broader attitudinal shift, but that effect sizes are too small to be statistically detected.

One reason to expect small effects of wealth on political preferences is that politics seems to have become increasingly multidimensional over time and fewer political issues are now structured along an economic dimension (Gethin, Martínez-Toledano, and Piketty, 2022). For instance, individual attitudes toward immigration and environmental policies typically reflect both material and non-material (cultural) values (Besley and Persson, 2023). While a positive income shock of the type examined here will affect the economic standing of an individual, it is unlikely to substantially alter an individual's cultural and moral outlooks. Thus, we have less reason to expect income shocks to impact individual preferences on policy issues that span multiple ideological dimensions. This may explain why lottery winnings primarily influence tax attitudes, as taxes are a key example of a fairly unidimensional policy issue.

An alternative explanation for the lack of detectable effects on broader political attitudes is that attitudes are formed early in life, after which they become sticky and difficult to change. If so, only external events of substantive magnitude with direct and clear policy implications will induce preference changes, such as attitudes to capital taxation following a large lottery win. To explore this possibility further, we follow the methodology of Ahlskog and Brännlund (2022) and use data on Swedish twins to study how much of income and wealth gradients that are due to confounding factors shared by twins. The results are reported in Section G in the Appendix and show that income and wealth gradients are substantially smaller in within-twin comparisons, suggesting that income and wealth gradients are partly confounded by shared genetic and environmental factors. However, for several of our preference measures, including the capital taxation index, income and wealth gradients remains positive and statistically significant also when comparing monozygotic twins, suggesting that political preferences are malleable also later in life.

Turning to our analyses of political participation, we find winning large sums in the lottery appears to have no effect on political participation, neither in terms of voting nor candidacy. We are able to reject causal effects much smaller than the cross-sectional gradients between income and political participation. Similarly, we find no evidence that children of lottery winners grow up to become more politically active than their less fortunate peers. These findings

suggest the political overrepresentation of the wealthy, among both voters and politicians, is not attributable to income levels per se. Increased economic inequality need therefore not widen the gap in political participation, as conventional resource-based theories of political engagement suggest (Verba, Schlozman, and Brady, 1995). Our result also provide another example of the difficulties in uncovering the mechanisms underlying the political clout of the affluent. While previous empirical studies have questioned the hypotheses that the influence gap can be attributed to the impact of lobbying and campaign contributions (Kenworthy, 2022), our study casts doubt on the view that the gap is simply a function of higher income levels fostering greater political participation.

Contrary to the hopes of many democratic thinkers, the association between economic and political power did not dissipate with the advent of democracy. By demonstrating how positive economic shocks can alter political preferences, our study underscores the risk that growing income and wealth disparities might exacerbate political polarization. But our results also indicate that this effect mainly applies to policy issues where economic self-interest is salient, such as tax policies. The fact that politics seems to have become increasingly multidimensional over time (Gethin, Martínez-Toledano, and Piketty, 2022; Besley and Persson, 2023) can therefore serve to offset the polarizing effect of increasing income inequality.

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