Dutiful Altruism and Propensities to Donate from Wealth

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Abstract

Do individuals feel a sense of duty to share good fortune with others? We test this prominent theory of prosocial behavior by estimating the effects of a variety of wealth shocks on donations. Our first design compares lottery winners in IRS microdata with a control group of future lottery winners. We find that a few lottery winners make very large donations but that the vast majority donate less than one percent of their earnings, the median guess of surveyed nonexperts. Consistent with a short-lived sense of duty, donations only increase in the year of the win despite long-run effects on other forms of consumption. Turning to survey data that capture other forms of wealth, we find a smaller level of donations from inheritance income and no significant effect of portfolio shocks that do increase other household expenditures. This finding is consistent with evidence from the lotteries that donations increase convexly with the amount of the wealth shock. Though aggregate donations by US households are consistently around two percent of income, our findings suggest that this has resulted not because many dutifully give two percent but because a small number of households make donations that are large and convex in wealth. **Keywords**: charity, wealth, tax policy. **JEL**: D14, D31, D64, H24, H31, H41, L31

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1 Introduction

In the United States, there is a robust and obvious connection between wealth and philanthropy. Virtually all high-wealth households give to charity, and the average amounts that they give are an order of magnitude larger than the average household's charitable donations (Osili et al., 2023). A small number of wealthy households are disproportionately responsible for charitable giving and philanthropy in the US (Schervish and Havens, 2001; Duquette and Mayo, 2021), where the top 1% of earners accounted for nearly 30% of all tax-deductible charitable giving in 2012.

The relationship between wealth and giving could be causal, or it could arise from a personality type that drives individuals to both obtain wealth and make donations. The extent to which wealth causes giving is an important parameter for evaluating spillover effects of policies that affect wealth. Estimating this parameter can also provide information about donor preferences, which can benefit charities that are increasingly dependent on high-net-worth large-dollar donors (Nwakpuda, 2020; Duquette, 2021; Osili et al., 2023).

In this paper, we measure the magnitude of the causal effect of changes in wealth on charitable giving. We identify these causal effects using two identification strategies. First, we use IRS microdata to study how winnings in state lotteries affect the donations deducted from taxable income. We compare lottery winners to (stacked) control groups who win similar amounts years later to account for any selection into playing the lottery and obtain estimates of the causal effect of random lottery wins.

Our second empirical strategy uses data from a panel survey with information on both donations and assets to estimate the effect of changes in household portfolios in-

duced by changes in asset market prices. Returns to various asset classes, such as homes or stocks, are unpredictable in an efficient market, providing variation in wealth that should be as good as random. We construct panel wealth predictions in two ways: by means of 1) a shift-share (Bartik) instrument computed from average market returns and 2) the post-LASSO selection algorithm of Belloni et al. (2014), which we use to generate efficient predictions from a machine-learning selection process.

We find extreme heterogeneity in the effect of wealth shocks on charitable contributions. When we winsorize donations in the tax data or look at lottery winnings of less than $1 million, we generally find that donations increase by around 1 percent of the amount won in the year of the lottery win. However, when we relax the winsorization or look at larger win amounts, we find average effects on donations as large as 8 percent of the increase in wealth. In contrast, our instrumental variable (IV) estimates from asset price changes find precise zero effects of changes in the value of assets. The upper end of the 95% confidence interval for both IV approaches is less than $121 per year for a wealth shock of $100,000. Thus, we find that short-run giving responses to wealth shocks can be large, moderate, or negligibly small, depending on the size and nature of the wealth shock.

To understand these findings further, we follow Samek and Longfield (2023) in using an incentivized survey to elicit beliefs from the US general public about the causal effect of wealth on charitable giving and other forms of spending. We find that survey responses are good estimates of other household responses to a wealth shock in the panel survey data but overestimate the effect on charitable giving, offering beliefs in magnitudes that are quite close to our estimated short-run lottery effects. Our survey

asked participants about the effect of an *unexpected* increase in wealth, offering a possible explanation for the difference in magnitudes of the charitable responses to the two types of wealth shock. We also confirm that the panel survey data non-effect on donations is not simply due to household inattention to asset value, as the same wealth shocks cause large and significant effects on spending on cars, clothing, and other categories of discretionary household consumption, as well as non-spending outcomes like fertility, health, and college enrollment. We also confirm the robustness of the survey-data findings through a battery of tests presented in our appendix, including changing the specification to a Poisson model, testing for time-varying treatment effects to rule out macroeconomic confounders, and using a different survey panel dataset that includes a larger share of high-income respondents; the appendix also presents alternative approaches to estimating the effect of a lottery shock, such as implementing tighter sampling restrictions. None of these checks suggest alternative interpretations of our findings.

This paper's primary contribution is to offer novel insight on the causal relationship between wealth and charitable contributions in the real world; existing works have largely studied either noncausal relationships between wealth and giving or the giving effects of a wealth shock in laboratory games. In nonexperimental settings, evidence on the wealth–giving relationship is mixed, with some papers finding large positive effects of wealth on giving, some finding small positive effects, and others finding no relationship at all.[1] Lab experiments featuring lump-sum endowments with the option to give

[1]Stock market rises are positively correlated with charitable giving levels (List and Peysakhovich, 2011; Clark et al., 2019). Joulfaian (2001) finds that wealth is positively correlated with lifetime giving in a microdataset of matched estate and income tax returns. Meer and Priday (2021) find that giving rises approximately linearly with wealth after outliers are winsorized. However, Do and Paley (2012) find that

to a real-world charity under varying conditions have found that larger endowments cause more giving (Exley, 2016; Ottoni-Wilhelm et al., 2017; Duquette and Hargaden, 2021). However, it is not clear whether these findings are externally valid for larger wealth shocks in the real world. Field experiments on high-wealth university donors find that the wealthy respond strongly to solicitations emphasizing donor agency and that the instrumental value of the gift can have large effects in terms of dollars given (Levin et al., 2016; Kessler et al., 2019). However, these studies do not vary wealth itself as part of their experimental design and so do not distinguish between the effect of wealth on donor psychology and underlying differences that may have led donors to have become wealthy in the first place. Therefore, the present research fills a gap in the literature on the effects of changes in wealth on giving.

Second, this paper extends and refines quantitative methods for estimating causal wealth shock effects. The existing literature has used changes in the value of specific asset classes to create shift-share instruments for understanding wealth effects on other outcomes. The shift-share strategy that we develop in this paper is akin to the approaches in an existing literature that looks at the effect of shocks to specific asset classes on household outcomes; for example, Schwandt (2018) uses stock market returns to find that retiree health is significantly affected by wealth shocks, and Lovenheim and Mumford (2013) find that housing price shocks affect fertility. We extend this method to the entire portfolio of household wealth, not just stocks or home equity, and we bolster the efficiency of the approach using causal machine learning.

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the relationship between home equity and charitable giving, while positive and statistically significant, is very small in magnitude. Moreover, in Swiss tax data, which include reported wealth, the conditional correlation between wealth and giving is negative and not robust (Lideikyte Huber and Pittavino, 2022).

Third, we contribute to the literature on charitable responses to lottery wins specifically as a form of wealth shock. Previous work in the United States has found that winning the lottery causes decreases in work income and increases in consumption, but do not substantially affect college attendance or physical health (Bulman et al., 2021; Golosov et al., 2024). For effects on charitable giving, Geys and Sørensen (2024) find that winning the lottery increases charitable giving in Norway, while Kotsadam and Somville (2024) find no effect of winning on giving in Ethiopia, though both of these studies have important institutional differences from our setting.[2]

Last, this paper contributes to an emerging literature on the effects of wealth on human behavior. Economists have debated the extent of wealth inequality, how it is changing over time, and what that change means for civil society.[3] Changes in wealth affect total consumption (Christelis et al., 2015) on both durables and nondurables (Mian et al., 2013; Graham and Makridis, 2023). Wealth shocks have clear effects on other kinds of behavior, including labor supply (Golosov et al., 2024), college attendance (Lovenheim, 2011), fertility (Lovenheim and Mumford, 2013), divorce (Cesarini et al., 2023), and intergenerational transfers (Schaller and Eck, forthcoming). Whether giving also responds to the growth and distribution of wealth has social and policy consequences; US tax benefits for charitable giving have always been designed (and reformed) with high-

[2]The Norwegian lottery data studied by Geys and Sørensen has very few jackpots over 1 million USD in value, which is where we find the largest effects in the US data. On the other hand, while the Ethiopian context studied by Kotsadam and Somville features (like the US data) very large wealth shocks relative to income, the prizes are in the form of houses, not cash, which means the liquidity of the prize is very different.

[3]This literature is quite large, and its findings and methods are diverse. Some papers on this topic include Kopczuk (2015); Zucman (2015); Boserup et al. (2016); Fagereng et al. (2016); Saez and Zucman (2016); Alvaredo et al. (2017); Boserup et al. (2018); Saez and Zucman (2020); Splinter (2020) and Smith et al. (2023).

wealth donors in mind (Duquette, 2019b), and well-targeted philanthropic investments by the very wealthy in future scientific and social progress may be an important contributor to long-run growth (Acs, 2013). Recent policy changes are likely to affect the total resources available to the nonprofit sector; for example, while a great deal of attention has focused on the 2017 Tax Cuts and Jobs Act's adjustments to the standard deduction, which greatly reduced the share of households likely to benefit from a tax deduction for giving, high-wealth households may have actually experienced more favorable tax treatment thanks to the near-elimination of the alternative minimum tax and higher deductible shares of adjusted gross income (Duquette, 2019a; Meer and Priday, 2020; Han et al., 2023). Given our finding that giving is convex in wealth, these effects at the top of the distribution may have offset some of the loss in donations caused by the reduction in the number of itemizers.

2 Data

This section describes the data used in this research. We use multiple panel datasets with different, complementary limitations to capture household wealth, charitable giving, income, and other relevant characteristics. Other variables are taken from additional sources or calculated from the values in the survey data.

2.1 IRS Microdata

We study lottery winners using the federal tax records of the Internal Revenue Service, accessed through the U.S. Department of Treasury Office of Tax Analysis. The primary

source is the Form 1040 filed by each taxpaying individual or household, and our outcome variable is charitable donations. Charitable donations are an itemized deduction that reduces taxable income one-for-one for all donations up to a percentage-of-income limit.[4] Because the Tax Cuts and Jobs Act of 2018 greatly reduced the number of taxpayers who itemized deductions, we restrict our sample to years before 2018. Because we only observe donations when donors itemize their deductions, we also restrict the sample to taxpayers who itemized in both baseline years leading up to the year of interest, and this ensures that winners of even small amounts are nearly all itemizers.

Our source of random variation in wealth that can be observed in the tax data is state lottery winnings. State lottery agencies are required to report to the IRS any awarded prizes of value exceeding $600 using Form W-2G. These reports are available starting in 1999 and include the year of the lottery and the amount that the individual won.

We use federal tax records for the U.S. population. All dollar amounts are adjusted to real 2010 dollars using the Consumer Price Index. The first year in which a lottery win is reported is classified as the "win year." Winners are linked to tax records back to 1996.

Table 1 provides summary statistics for the main sample. Because these taxpayers are regular itemizers, the vast majority have donations in each year. The share donating and median actually decrease slightly in the year of the lottery winnings, suggesting that most taxpayers' donations do not respond, though the mean of roughly $2600 increases by $345. We include only taxpayers who won at least $1000, and therefore the share with any winnings is 1. Winnings are highly skewed: the median amount won is less

[4]Deductions are limited to 50% of Adjusted Gross Income (AGI) for most donations and 30% of AGI for donations to private foundations. For both limits, deductions can be spread over 5 years.

Table 1: Summary statistics for lottery sample

	Share > 0	Median	Mean
Donations in year -1	.866	1300	2577
Donations in year 0	.815	1200	2922
Winnings in year 0	1		30,008

Notes: This table shows summary statistics for the main sample of lottery winners. Medians are rounded to avoid disclosing a value for any individual taxpayer.

than $4000, but the mean is more than 30,000.

2.2 Panel Study of Income Dynamics

The primary household-level survey dataset used in this research is the Panel Study of Income Dynamics/Center on Philanthropy Panel Study (PSID/COPPS). This dataset adds a module on charitable giving to the well-known PSID, a panel dataset on household behavior that has run continuously since 1968. Financial data on charitable giving and wealth for even-numbered years from 2000 to 2018 are captured in ten survey waves. [5] Because our identification strategy relies on exploiting changes across periods, observations are usable if the relevant variables are observed in the current and lagged (two years prior) waves. The usable dataset encompasses 46,095 observations for 10,926 households over nine survey waves. In addition to information on wealth, income, and demographics, the survey includes questions on giving: namely, whether respondents

[5] The survey waves themselves are implemented in odd-numbered years but ask about income and wealth information for the previous year. Thus, for example, the 2019 PSID wave has questions about 2018 income and wealth.

gave to charity and, if so, how much they donated for a list of eleven specific types of charitable causes and in total.

Descriptive statistics for the PSID data are presented in the appendix. Income and wealth are highly skewed, with the mean of each much larger than the medians. Just under half of all observations do not have enough charitable giving to exceed the PSID censorship threshold of $25. As in tax data, most of the observations in both datasets have an income-tax price of charitable giving equal to 1. That is, they receive no tax subsidy for their charitable giving; they do not itemize, and donating a dollar costs an entire dollar.

2.3 Wealth shock beliefs survey

The PSID analyses of this paper will report precise and very small or null effects of wealth shocks on charitable contributions. To benchmark these counterintuitively small effects against general expectations, we use the method of Samek and Longfield (2023), running an incentivized survey to assess how far our empirical results diverge from widely held beliefs about the effects of wealth on giving among a nonexpert US sample.

After obtaining participant consent, the survey explains what a wealth shock is, as distinct from an income shock or an endogenous change in wealth. It then asks respondents to guess how much a typical household would change its spending on five categories of expenditure, including charitable contributions, following an unexpected shock to wealth. For each of the five categories, respondents were told the mean of spending in that category and asked "After an unexpected $100,000 increase in wealth, how much more do you think they would spend on this each month?." The survey

Table 2: Selected quantiles, wealth shock predictions survey (N=498)

	Charitable giving	Eating out	Clothing	Leisure trips	Home furnishings
Minimum	0	0	0	0	0
25th percentile	300	1,200	1,200	1,800	720
Median	936	2,400	2,400	3,600	1,440
75th percentile	1,800	3,600	3,921	6,000	2,568
Maximum	240,000	600,000	840,000	600,000	840,000

also elicits self-reported confidence in the responses and basic self-reported assessments of income and wealth. Survey responses were incentivized with a gift card mechanism that rewarded three randomly selected participants with a cash-equivalent prize with a higher value for guesses closer to the real values obtained in this study.

The survey was implemented on the Prolific platform and approved by the University of Southern California IRB, with number UP-23-00728. We used answers to pilot versions of the survey to bootstrap an ex ante power analysis; we found that ≈ 500 responses would be sufficiently powered to deliver a sample mean within 10% of the population mean 80% of the time with 10,000 bootstrap replications.[6] Details on the power analysis and the survey implementation are reported in the appendix. The fielded survey had a sample size of 500 observations, of which 498 were complete and usable.

We present quantiles of the survey responses in table 2. The means and standard deviations are uninformative because four respondents gave implausibly large responses to each question. Instead, we present the medians, interquartile ranges, and minimum and maximum values.

[6]Specifically, a bootstrap sample of 500 respondents was within ±10% of the mean 82.6% of the time.

2.4 Asset price sources

We use changes in asset prices for broad market indexes to estimate a shift-share instrument for changes in wealth. For stock holdings, we use returns to the S&P 500 including dividend reinvestment and compounding (Shiller 2015, updated data). For housing, we use the rate of growth of the Bureau of Economic Analysis average home price in the state where the respondent resides.

2.5 Tax rates

Marginal tax subsidies for charitable contributions are estimated with the National Bureau of Economic Research's (NBER's) TAXSIM calculator (Feenberg and Coutts, 1993).[7] To avoid endogenous effects of greater giving on the marginal rate (since large gifts can bump filers down to a lower bracket), we calculate a "first-dollar" marginal subsidy with respect to a small cash gift as follows. Other tax return line-items are submitted to the calculator at reported values, and then the tax owed is calculated for zero giving and a gift of $10 in cash. The marginal tax subsidy rate is then derived as the change in tax divided by 10. For the great majority of observations, which correspond to respondents who take the standard deduction, the subsidy rate is zero.

3 Exogenous changes in household wealth

Wealth can undergo large, endogenous changes for reasons possibly correlated with charitable giving decisions. A large inheritance saved or savings draw down for a child's

[7] Specifically, Internet TAXSIM v35. See www.nber.org/taxsim/taxsim35.

college tuition will appear in the data as a substantial wealth change but will plausibly not come as a surprise to a forward-looking household. Households tend to accumulate wealth in middle age and to draw it down in old age, a lifecycle pattern of wealth changes that may be correlated with interest in giving.

These events may be correlated with giving, but not for causal reasons. For example, gifts to a college may be timed to coincide with a child's application process (Butcher et al., 2013), leading the reduction in wealth due to the drawing-down of a college savings account to be correlated with the giving decision. More mundanely, gifts out of wealth, or averted savings, create a mechanical relationship between wealth and giving that possibly double-counts the same dollars.

For these reasons, we exploit *unanticipated* wealth changes. We first do so using lottery winnings, comparing changes among current lottery winners to those of future lottery winners. We also construct changes in wealth that occur because of changes in the market prices of assets held by a household.

3.1 Tax data empirical strategy

Our first empirical strategy exploits the randomization conducted by state lotteries. Because winners are selected at random, comparisons among lottery participants would provide purely random variation. However, the tax data only identify lottery winners and not participants who do not win. For this reason, a simple comparison between lottery winners and other taxpayers or between those who win different amounts may not identify the causal effect of the lottery winnings because lottery ticket purchases, including of games with differing amounts of payouts, may be correlated with charitable

giving.

To address the sample selection issue, we compare individuals currently receiving lottery income to individuals receiving (similar amounts of) lottery income in the future. More specifically, to be able to study effects over the five years following the year of winnings, we use a control group of individuals with lottery winnings six years later. We match the control group to the treatment group by age, thereby comparing same-aged individuals who win the lottery in a given year with those who win similar amounts six years later. For example, we compare age-30 charitable donations between those with lottery winnings at age 30 in a given year and those with lottery winnings at age 36 six years in the future.

$$y_{it} = \beta_1(\text{winnings}_i \times \text{treat}_i) + \beta_2 \text{winnings}_i + \beta_3 \text{treat}_i + \delta_t + \delta_a + u_{it} \tag{1}$$

In this equation, $winnings_i$ is the pre-tax dollar amount won by individual i, and $treat_i$ indicates those winning in year zero, distinguishing them from the control group winning in year six. The interaction of these two variables gives the effect of additional winnings, using the future winners to control for selection into winning different amounts. We control for win year (or placebo win year for the control) by age fixed effects δ_t and δ_a.

To make results easier to read, we scale $winnings_i$ by \$100,000. Thus, an estimate of \$1000 shows that winners donate \$1000 per \$100,000 of winnings, i.e. they donate 1 percent of the wealth shock. We support the empirical strategy by showing that there are no "effects" of winnings in years before the winnings occur. We examine nonlinearity of the effects by varying the maximum amount of winnings in the sample.

Our primary outcome is the level of charitable donations. This is only observed for individuals who itemize deductions. This common empirical problem is less of an issue in our setting as demonstrate robustness to sample restrictions based on itemizing in years prior to the win. We also define indicators for donations that exceed various thresholds to describe the heterogeneity of the response.

3.2 Survey data empirical strategy

We use exogenous changes in wealth to estimate the effect of wealth on charitable giving. We construct an instrument for wealth in the current period by taking households' preexisting stock of various assets and debts in the previous period and predicting changes in the wealth that they would receive from asset returns over the time between the survey waves. While wealth itself may be endogenous to savings preferences and determinants of income, asset price returns are exogenous from the perspective of individual households and are not influenced by individual saving or dissaving decisions. Thus, comparison of households by similar preperiod net worth (but different asset class allocations), using predictions from unforeseeable asset price changes, should reveal the effect of exogenous wealth changes on household behavior.[8]

3.3 Shift-share IV

Our simplest instrumental variable uses changes in wealth that would occur if the household left its finances "on autopilot" between survey waves. That is, we calculate what

[8]This identification strategy is similar to the approach in an existing literature in labor economics that studies household and family decision-making in response to housing market fluctuations or stock market returns (Lovenheim, 2011; Lovenheim and Mumford, 2013; Schwandt, 2018).

would happen if the household made no nonmandatory additions to or withdrawals from asset or debt positions while also paying its debts on time and reinvesting distributions. This identifies the causal effect of wealth shocks on giving because the component of wealth changes from asset markets is unpredictable; households with similar levels of absolute wealth in the preperiod experience different exogenous changes to their predicted wealth because the returns to these asset classes that households hold in varying proportions differ with the returns to each asset market.

For each asset type a and pair of consecutive observation periods $(t, t-1)$, we construct a rate of return $R_{a,t,t-1}$ from the macroeconomic index return for a. Using these rates of return, we construct a *predicted* asset value for each asset class a and household i by multiplying the market rate of return by the lagged asset value,

$$\widehat{\text{Asset}}_{a,i,t} = \text{Asset}_{a,i,t-1} * R_{a,t,t-1} \tag{2}$$

In addition to asset information, both the PSID and SCF datasets include variables on the interest rate and payment amounts for first and second mortgages. For each mortgage debt d, we compute a predicted debt level $\widehat{\text{Debt}}_{d,i,t}$ by computing the reduction to lagged principal $\text{Debt}_{d,i,t-1}$ that would result from sticking with the regular payment schedule implied by the monthly payment and interest rate, with no prepayments, deferments, or delinquencies. Last, for miscellaneous assets and liabilities, we compute the residual net worth of the household after accounting for these assets and debts, and we assume that the residual net worth remains unchanged across consecutive survey waves.

For the primary shift-share instrument used in this paper, we use five lagged components of net worth: stocks, real estate, first mortgage, second mortgage, and residual net

worth. Stocks and real estate are two large and widely held asset classes that have been previously used in the wealth-shock literature, and whose value is reported precisely and well-predicted by market returns. (In a check later in this paper we will estimate the effect of wealth shocks with a broader set of assets and debts.)

All of these individual asset predictions are aggregated to construct the shift-share instrument for wealth, $\widehat{\text{Wealth}}_{it}$:

$$\widehat{\text{Wealth}}_{it} \equiv \sum_a \widehat{\text{Asset}}_{a,i,t} - \sum_d \widehat{\text{Debt}}_{d,i,t} \tag{3}$$

This is a shift-share strategy because two households with the same net worth in the previous period can have very different predictions of their wealth in the current period: the process creates a weighted average across all asset and debt types from the previous period. That is, we can restate the prediction as

$$\widehat{\text{Wealth}}_{i,t} = \sum_a \text{Wealth}_{i,t} \times \alpha_{i,a,t-1} \times R_{a,t,t-1} \tag{4}$$

where

$$\alpha_{i,a,t} \equiv \frac{\text{Asset}_{a,i,t}}{\text{Wealth}_{i,t}}$$

for the contribution of assets and, equivalently, $\frac{-\text{Debt}_{d,i,t}}{\text{Wealth}_{i,t}}$ for the contribution of debts to wealth changes.

This shift-share IV strategy requires only that one of the two pieces that predict the treatment satisfies the conventional requirements of an instrumental variable (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2021). Because the predictions are a product of pre-

existing shares and shocks, if we can argue that *either* is exogenous—influencing wealth today but not giving except via wealth today—then the product of the exogenous and endogenous ingredients is also exogenous, and the overall IV strategy satisfies the exclusion restriction.

Here, the predicted changes in wealth explained by these shocks are conditionally random and exogenous to the current period's giving because asset market returns are random and unpredictable. Household i's weights on asset and debt types a in the previous period $\alpha_{i,a,t}$ are endogenous. However, asset market returns $R_{a,t,t-1}$ are exogenous and unanticipated by households (Lovenheim, 2011; Schwandt, 2018). The key identifying assumption thus reduces to $\mathbb{E}\left[\varepsilon_{i,t}|X_{i,t-1}, R_{a,t,t-1}\right] = 0$.

3.4 Post-LASSO IV

An important limitation of the shift-share approach is that the rates of return are taken from changes in asset price time series on an annual basis, which may not accurately capture the true rate of return between survey waves for specific asset classes held by surveyed households. An alternative approach to modeling asset prices is to use a post-LASSO prediction IV to fit, but not overfit, wealth this period based on the levels of assets and debts by type from the previous period (Belloni et al., 2014). We use this post-LASSO instrument as the primary IV in the analyses to follow.

Specifically, we follow the steps below to perform the post-LASSO estimation:

1. Predict the level of wealth for each observation i with a LASSO regression. Specif-

ically, we estimate the following equation:

$$\text{Wealth}_{i,t} = X_{i,t-1}\beta + \theta \sum |\beta|$$

where $X_{i,t-1}$ is a set of lagged normalized asset and debt predictor variables and θ is a regularization penalty chosen by cross-validation.

2. Save the selected subset of predictors $X^{\star}_{i,t-1} \subseteq X_{i,t-1}$ with $\hat{\beta}^{LASSO}_{X_{i,t-1}} \neq 0$.

3. For each observation i, use unpenalized least-squares regression to estimate the relationship between the selected subset of predictors $X^{\star}_{-i,t-1}$ and the level of wealth, omitting observations for household i (denoted subscript $-i$):

$$\text{Wealth}_{-i,t} = X^{\star}_{-i,t-1}\beta_{-i} + \varepsilon$$

4. Create predictions of the level of wealth for each observation i with the estimated relationship and lagged values:

$$\widehat{\text{Wealth}}_{i,t} = X^{\star}_{i,t-1}\hat{\beta}_{-i}$$

5. Use this alternative set of predictions for IV estimation.

The set of candidate predictors X includes the same set of lagged wealth categories as the shift-share IV—stock holdings, gross home value, mortgages, and residual net worth—as well as the square of each asset and debt variable, year dummies, state dummies, interactions of each asset and debt variable with the dummy variables and with

each other.

The PSID post-LASSO IV is generated with 68 predictors selected by LASSO from 645 features. The selected variables are the core lagged wealth variables, plus interactions with years around the 2008 crisis and indicators for states with dysfunctional housing markets. The LASSO estimation is implemented in Python using Scikit-learn (Pedregosa et al., 2011).

3.5 Panel model of wealth and giving

All variables of interest have very skewed distributions, with significant masses at zero, and in the case of wealth, we see significant shares of negative values. Consistent with the previous literature, we transform and first-difference variables to create a pseudo log-log specification that allows for an elasticity-like interpretation of the coefficients of interest. To ensure that the limitations of this approach do not generate spurious results, we estimate alternative specifications in later sections.[9]

To estimate the pseudo log-log specification, we first transform the PSID data as follows. Dollar-valued variables are censored at low, positive values and then transformed with the natural logarithm. Where giving is not reported below the PSID's $25 cutoff, we impute giving amounts of $5. We also winsorize income and wealth at a minimum value of $1000. After censoring and logging, we calculate first-differenced values across consecutive observation periods. Observations are dropped if consecutive observations on the household are not observed or are incomplete for the needed variables (so the

[9]Taking the log of variables transformed to eliminate nonpositive values will not yield a consistent, coherently defined treatment parameter, and the results can depend substantially on the details of the chosen transformation (Chen and Roth, 2023).

change in the transformed variable is undefined).

The shift-share prediction of (net) wealth for household i in period t is the sum of predicted assets less the sum of predicted debts, and the predicted *change* in net wealth ($\Delta W_{it}^{\star}$), which is our instrumental variable, is these predictions net of the previous period's observed wealth.

$$\Delta W_{it}^{\star} \equiv \ln\left[\sum_{a} \widehat{\text{Asset}}_{a,i,t} - \sum_{d} \widehat{\text{Debt}}_{d,i,t}\right] - \ln\left[\sum_{a} \text{Asset}_{a,i,t-1} - \sum_{d} \text{Debt}_{d,i,t-1}\right] \quad (5)$$

We use $\Delta W_{it}^{\star}$ as an instrumental variable capturing the exogenous component of changes to the observed log of wealth ΔW_{it},

$$\Delta W_{it} \equiv \ln\left[\sum_{a} \text{Asset}_{a,i,t} - \sum_{d} \text{Debt}_{d,i,t}\right] - \ln\left[\sum_{a} \text{Asset}_{a,i,t-1} - \sum_{d} \text{Debt}_{d,i,t-1}\right] \quad (6)$$

When we use the post-LASSO instrument, $\Delta W_{it}^{\star}$ is predicted directly by the leave-one-out regression model on the omitted panel ID.

For both instruments, the estimation proceeds by 2SLS, with first stage

$$\Delta W_{it} = \alpha_1 + \Delta W_{it}^{\star}\delta + \Delta \mathrm{X_{it}}\boldsymbol{\lambda}_1 + u_{it} \quad (7)$$

and second stage

$$\Delta G_{it} = \alpha_2 + \widehat{\Delta W_{it}}\beta + \Delta \mathrm{X_{it}}\boldsymbol{\lambda}_2 + \varepsilon_{it} \quad (8)$$

where Δ is a first-difference operator, W is wealth, $\Delta W^{\star}$ is the constructed exogenous change in W used as an instrument, and $\widehat{\Delta W_{it}}$ is the predicted value of the change in

wealth fitted in the first stage. G is the censored logarithm of self-reported charitable giving, and X is a set of other controls, specifically the change in the logarithm of income and tax price of giving. All variables are indexed by household i and survey year t. Residual terms are denoted u_{it}, ε_{it}. Regression coefficients are estimated elasticities with respect to the censored variables. All standard errors are clustered at the 1968 PSID household level to adjust for serial correlation and correlation within kinship networks.[10]

3.6 IV strength and validity

First-stage results for each of the instruments are shown in the appendix. The pseudo-elasticity of the predictions on observed change in wealth are approximately 0.3 and 0.7, respectively. Both instruments are strongly predictive of actual changes in wealth. The F-statistics on each instrument both exceed 900, putting them well above the F=104 rule of thumb for conventional use of t-statistics for inference (Lee et al., 2022).

3.7 Untestable IV assumptions

The credibility of our identification strategy rests on whether the reader believes asset market movements really are unpredictable from the point of view of individual households and do not affect charitable giving decisions except through their effects on wealth. It is unlikely that households can accurately predict changes in efficient financial markets, so the primary threat to this strategy would be if asset price movements affected

[10]The original 1968 wave of the PSID has continued to the present by following descendants and other family members (e.g., by marriage) of the original survey-takers, with new waves of initial households added only to make the sample more representative of the US population. We use the identifier for original household observation as the clustering level since the giving decisions of extended families may be correlated within and across years.

giving through channels other than the resources available to individual donors—for example, if potential donors were inspired to give more when asset prices fall because they take asset price drops as a leading signal of a worsening economy and imminent need (or, more formally, $R_{a,t,t-1} \not\perp \Delta G_{i,t}$).[11] While the exclusion restriction cannot be tested directly, a check in a later section of the paper will confirm that the effects of wealth shocks are not heterogeneous by overall macroeconomic conditions, suggesting that this specific channel for exclusion restriction violation is not of concern.

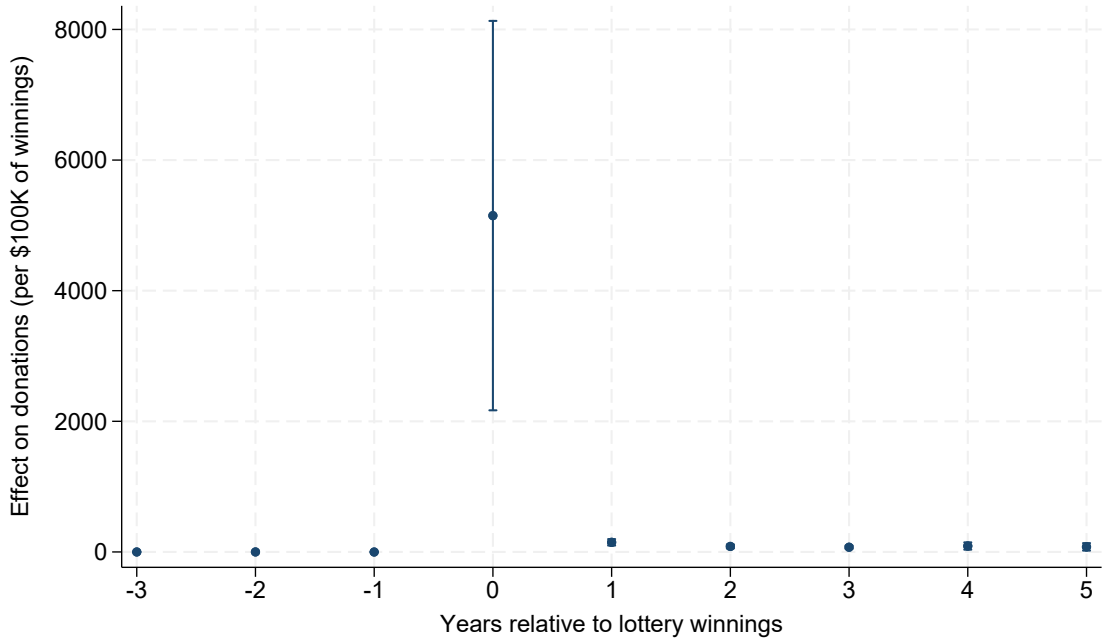
4 Results: Effect of wealth shocks on giving

4.1 Results in tax data

Figure 1 plots the treatment effect in each year among all taxpayers in the main sample. As expected, we find precisely zero effect in years before the win. In the year of the win, donations increase by an average of about $5000 per $100,000 of winnings, i.e. 5 percent of the increase in wealth. The effect on donations in the next year is smaller by an order of magnitude, and it is even smaller in subsequent years. In summary, the charitable effect of these large wealth shocks is short-lived and in present value does not much exceed one percent of the amount won. This contrasts with extant results on labor supply and other forms of consumption, which display a more permanent effect.

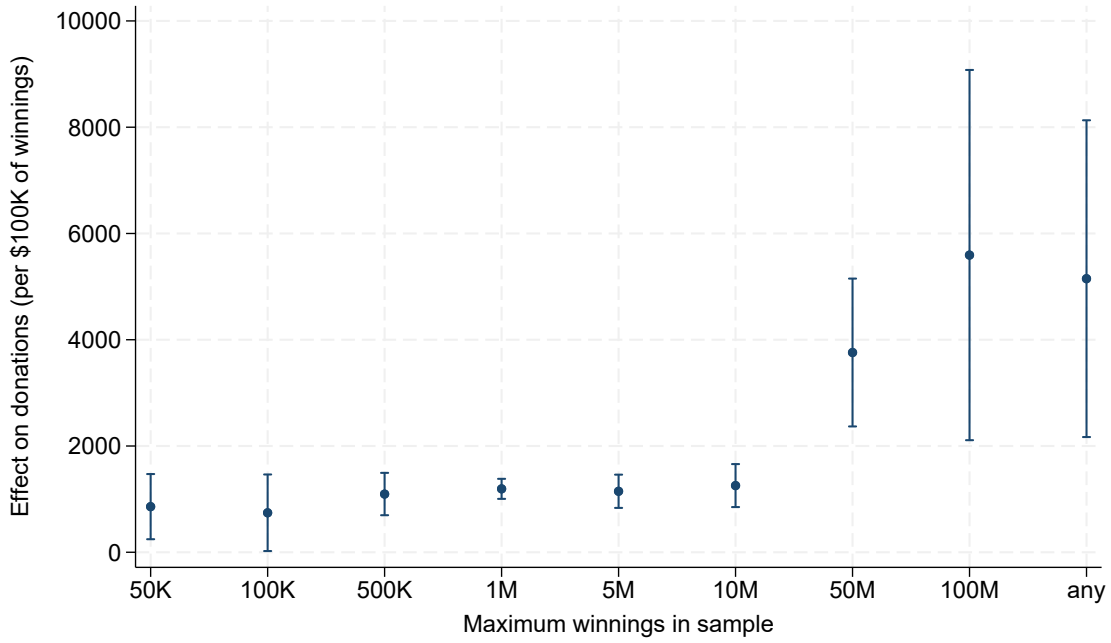
[11] An emerging literature suggests that this is unlikely: Exley et al. (2023) find that giving is strongly economically procyclical in general, while Karol (2023) finds that donations to food banks specifically decline in response to greater intensity of local demand for food assistance. These articles suggest that any backchannel effect of asset prices on giving is unlikely to be large and, if anything, should cause the estimates presented in this paper to be too large. On the other hand, Meer et al. (2017) find that giving over 2002–2012 tracked the GDP changes of the great recession even after they control for wealth and other individual factors.

Figure 1: Event study of donations around time of lottery win



Notes: This figure shows the estimated coefficient of interest from the difference-in-differences regression, estimated separately for each year relative to the winning year.

Figure 2: Donation effect in year of lottery win in by win amount



Notes: This figure shows the estimated coefficient of interest for the year of winnings, estimated separately for each sample defined by maximum win amount.

Figure 2 plots the initial donation response for different lottery winnings amounts. Allowing for winnings of any level up to $10 million, individuals give about 1 percent of the amount of their gain. However, if we consider wins of up to $50 million, donors give closer to 4 percent of their winnings. Allowing for any win size, the average increases to more than 5 percent of winnings, as seen in Figure 1. This convexity reveals that individuals view charitable giving is a luxury good that increases as a percentage of income only when the level of income reaches multiple millions.

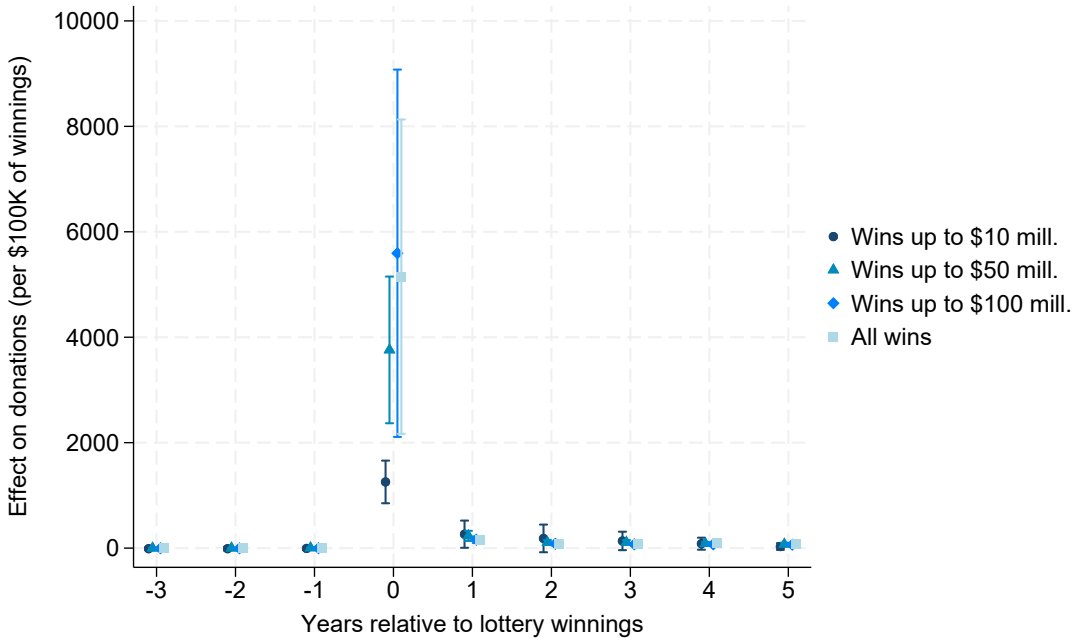
Figure 3 shows estimates of donation response over time for different lottery win-

nings thresholds. Across thresholds, estimated effects are precisely zero in the pre-period. Effects in the year of the win are as previously depicted in Figure 2, with donations per dollar of winnings increasing between wins of $10 million and $100 million. This is not the case, however, for donations in later years. Estimates for these years are actually decreasing in the win amount, though coefficients for larger amounts fall within the confidence intervals of those for smaller amounts. This pattern reinforces the idea, suggested by the mostly temporary nature of the response, that charitable giving does not provide the same type of utility as consumption goods that show persistent effects of wealth shocks. Our model that can explain these patterns will appear in our next draft.

The lottery winnings we observe in the tax data are reported to the IRS by the payer, rather than self-reported by the taxpayer as part of their taxable income. Appendix F re-estimates these event studies using an alternative independent variable: the amount of gross self-reported winnings observed on lottery winners' income tax returns. The resulting coefficients display a very similar pattern to those observed in Figures 1 and 3, although the magnitude is somewhat smaller.

We next estimate effects throughout the distribution of giving, using a more flexible binned function of win amount, to get a sense of heterogeneity. Table 3 shows results of win-size-bin regressions for various discrete levels of the donation outcome. For example, in the first row we find that winning $30,000 to $50,000 increases the probability of donating by nearly one percentage point relative to winning less than $30,000. Larger wins have a greater effect on the extensive margin up to $500,000, above which the share donating actually decreases, perhaps because a small donation is no longer worth

Figure 3: Donation effect by lottery win amount



Notes: This figure shows the estimated coefficient of interest for the year of winnings, estimated separately for each sample defined by maximum win amount.

one's time.

Moving down the rows of Table 3 shows the effect of a win of a given size throughout the distribution of donation amounts. The amount of a donation threshold can be compared to the size of the lottery win to see the share giving different percentages of their winnings. For example, a donation of $1000 would represent at least 2 percent of the winnings for taxpayers in the first column, and the estimate shows an increase of .022 in the share of taxpayers giving at this level. Across most of the columns, we see significant increases in the share donating more than 10 percent of the amount won. Comparing this with the mean effects found in the linear regression, as seen in Figure 2, reveals a substantial degree of heterogeneity in the giving response. Our next draft will describe this heterogeneity in terms of taxpayer characteristics.

4.2 Results in survey data

We present the main results from the PSID in table 4. The first two columns present the results from an OLS regression of the change in giving on the change in wealth, where we do not yet deal with the endogeneity of wealth. The first column is an unadajusted regression of the change in giving on the change of wealth with no other controls, while the second column includes the changes in the tax cost of giving and total household income as control variables. Standard errors, in parentheses, are clustered at the PSID 1968 household level to adjust for correlation over time and within extended family networks, and the p-values, reported below, for a two-sided test of the null that the coefficient differs from zero reflect this clustering adjustment.[12] The estimated relationship

[12] "1968 household" is an identifier for the original household sampled at the beginning of the PSID. As people form new households over time, the PSID tries to follow them, and this identifier links all

Table 3: Effect of lottery winnings up to 500K on discrete donation outcomes

	Won 30-50K	Won 50-100K	Won 100-500K	Won 500K-1M
give 1+	0.0095*	0.029***	0.038***	–0.028**
	(0.0057)	(0.0060)	(0.0053)	(0.013)
give 5+	0.0092	0.029***	0.037***	–0.028**
	(0.0057)	(0.0060)	(0.0053)	(0.013)
give 10+	0.0094*	0.029***	0.038***	–0.028**
	(0.0057)	(0.0060)	(0.0053)	(0.013)
give 50+	0.0075	0.030***	0.036***	–0.022*
	(0.0058)	(0.0061)	(0.0054)	(0.013)
give 100+	0.0099*	0.030***	0.037***	–0.021
	(0.0059)	(0.0062)	(0.0055)	(0.013)
give 500+	0.014**	0.041***	0.059***	0.033**
	(0.0068)	(0.0071)	(0.0064)	(0.015)
give 1K+	0.022***	0.052***	0.083***	0.086***
	(0.0071)	(0.0075)	(0.0069)	(0.016)
give 5K+	0.0092*	0.034***	0.088***	0.17***
	(0.0048)	(0.0051)	(0.0051)	(0.012)
give 10K+	0.0033	0.019***	0.061***	0.13***
	(0.0028)	(0.0030)	(0.0034)	(0.0096)
give 50K+	0.000093	0.00039	0.0034***	0.048***
	(0.00056)	(0.00054)	(0.00072)	(0.0050)
give 100K+	–0.00021	0.00037	0.00048	0.011***
	(0.00032)	(0.00031)	(0.00041)	(0.0026)
give 500K+	–0.000011	–0.000027	0.000046	3.5e-06
	(0.00014)	(0.00016)	(0.00016)	(0.000016)
give 1M+	3.4e-06	–0.00012	0.000052	9.5e-06
	(7.5e-06)	(0.00012)	(0.00016)	(8.0e-06)

Notes: This figure shows the estimates of separate regressions for each of the discrete donation outcomes shown in each row. Estimates display only the coefficient of interest, which is the interaction of win-size bin and an indicator for current treatment. Number of taxpayers = 1,060,882 in omitted group with win <30K; 18,332 with win of 30-50K, 16,848 with win of 50-100K; 20,648 with win of 100-500K; 3595 with win of 500K-1M. Standard errors, clustered by taxpayer ID, are reported in parentheses. *** p< 0.01 ** p< 0.05 * p< 0.1.

is positive and statistically significant, but very small in magnitude: if we interpret these coefficients as elasticity-like parameters, a 10% increase in wealth increases giving by only 0.4–0.5%. These small magnitudes are comparable with those reported by Do and Paley (2012).

Table 4: Log-log first-difference regressions

	OLS		Post-Lasso IV	Shift-share IV
Δ Wealth	0.053	0.044	0.055	-0.035
	(0.008)	(0.008)	(0.037)	(0.043)
p-value	0.000	0.000	0.138	0.418
Control variables	No	Yes	Yes	Yes
Marginal giving, $100,000 more wealth	45.57	37.03	46.69	-27.10
	[30.99, 60.54]	[23.44, 53.03]	[-10.87, 121.50]	[-85.07, 43.30]
Reject survey median?	Yes	Yes	Yes	Yes

Standard errors, reported in parentheses, are adjusted for clustering at the 1968 PSID household identifier level. "Control variables" include income and tax price of charitable contributions. "*p*-value" is reported for a two-sided hypothesis test of the null that the coefficient on wealth is equal to zero. "Marginal giving in dollars" reports the difference in predicted giving at the sample mean for baseline levels of wealth and for an additional $100,000 in wealth. Panel-bootstrapped 95% confidence intervals for marginal differences are presented in brackets (1000 bootstrap replications).

The second two columns of the table presents IV estimates to isolate the causal effect of plausibly exogenous changes in wealth on giving. The third column presents our preferred specification, a 2SLS IV estimation using the post-LASSO predictions of current wealth based on the composition of last period's wealth, fitted by leave-one-out regression on post-selection predictors. The last column uses the shift-share predictions from asset market return weights on last-period wealth. Both IV models control for the

households that have descended from the original sample. This includes households added in later survey waves, not solely the original households literally added in the original 1968 study. Thus, in addition to clustering for correlated errors over time as one would typically do in a panel regression, this clustering adjusts for correlated errors within an extended kinship network in the same period.

change in income and tax price.

Though the 2SLS estimates are not as precise as the OLS coefficients, of course, both are quite precisely estimated, reflecting the strength of the instruments. The coefficient on the post-LASSO instrument is almost indistinguishable from the corresponding OLS estimate, at 0.055, but it is not statistically different from zero. The estimated coefficient based on the shift-share instrument is actually negative, though also not statistically different from zero or from the OLS point estimate.

Because these pseudo-log coefficient estimates are not exactly elasticities, and because giving and wealth are of very different magnitudes, we translate all results in the table into average marginal effects in dollar terms.[13] At the bottom of the table, we present the average additional giving that the model predicts across the sample for a hypothetical $100,000 increase in wealth. Framing the results in this manner makes the very small magnitudes of the estimated effects tractable: A $100,000 increase in wealth increases average giving the following year by only $47 according to the post-LASSO specification, and the effect is negative under the shift-share IV. The panel-bootstrapped 95% confidence intervals presented in brackets below these marginal effects demonstrate that this small effect is also quite precisely estimated, ruling out increases in giving greater than $122 and $43 in response to a $100,000 wealth shock, respectively.

These magnitudes are much smaller than most people would expect. In the online

[13]Because all variables of interest in this work have very nonnormal distributions, these marginal effects are calculated as the *average of individual marginal effects within the sample*, rather than according to the common practice of estimating *marginal effects for a unit at the sample mean*. That is, we calculate the marginal effect for each observation and then take the mean of those marginal effects. This is a conservative approach—a hand-check of the sample-mean method shows that at-the-sample-mean marginal effects are smaller than the ones that I report here—but we believe that it better captures the actual marginal effect of interest: the average change in giving across the sample population for a hypothetical wealth change.

survey eliciting guesses at the giving response to a $100,000 wealth shock, the median guess is $936, more then ten times the upper end of the post-LASSO confidence interval, and more than twenty-five times the point estimate. Only 15.2% of survey respondents guessed a number for the wealth-shock effect that was within the confidence interval of the post-LASSO IV estimate. All four specifications firmly reject the median survey response as a plausible effect size.

Overall, these findings suggest (1) that the bias that endogenous changes in wealth introduce to an OLS estimate of the effect of wealth on giving is small and (2) that the effect of wealth changes on giving is very small or even (3) statistically indistinguishable from zero in the IV setting. These findings lend support to the existing literature that has found a low or null effect of wealth on giving, but they strongly contradict not only another literature that suggests wealth and giving are related but also widely held priors and cultural beliefs that wealth begets philanthropy.

For all these reasons, we must be sure that we have obtained the correct and true effect before inferring that the causal relationship between wealth and giving is so small. In the following sections, we will replicate this analysis using alternative datasets, functional forms, and instrumental variables. We will also make sure that the methods used here estimate the wealth effects on other categories of spending accurately.

5 Discussion

5.1 The effect of wealth shocks on giving are convex

To view the effect of wealth on giving in a manner that is comparable across our two research designs, figure 4 presents short-run wealth shock effects for both the PSID and lottery methods. For consistency with the lottery shocks, PSID samples are split based on whether the change is greater than +$10,000. We then estimate effects of wealth shocks up to +$500,000.[14]

In the region of common support, we obtain estimates that are essentially equal within each of the two research designs but different between designs. The PSID results are consistently indistinguishable from the null of zero for all shock sizes, including negative shocks, small positive shocks, and larger positive shocks. Where the estimates are more precise, we can reject that the effect of lottery winnings are the same as the effect of asset price shocks. This suggests a difference in liquidity or in the way that the two types of wealth shock are perceived.

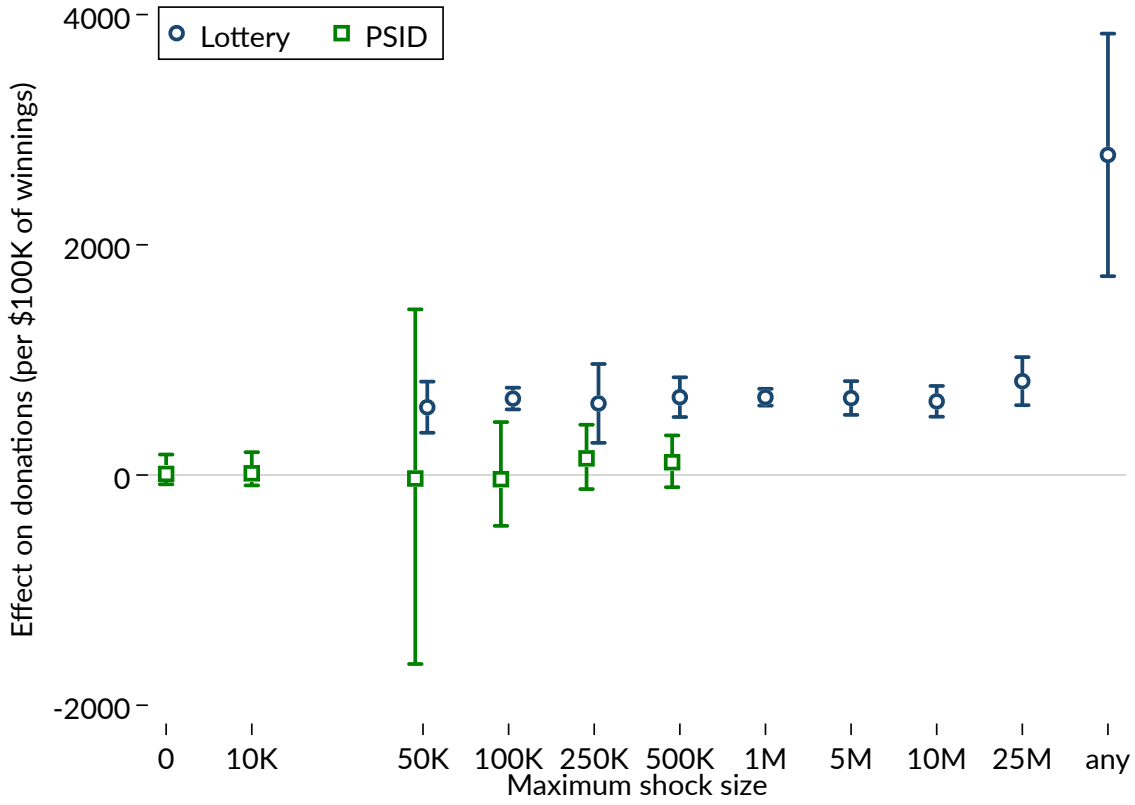
5.2 The effect of wealth shocks on giving are short-lived

As shown in Figure 1, the primary effect on giving of a lottery win happens in the year of the win. For the subsequent five years, the effect is smaller by an order of magnitude and is not increasing in the amount of winnings.

In the appendix, we estimate the effects of wealth shocks in the PSID for time win-

[14] While large wealth shocks are observed in the PSID, in our sample a wealth shock of +$500,000 is observed for only about 3.5% of observations. The effective region of common support between the two data sets is $10,000 to the low $100,000s.

Figure 4: Short-run wealth shock effects by wealth shock size



Notes: For lottery shocks, this figure shows the estimated coefficient for the year of winnings, estimated separately for each sample defined by maximum win amount, as in figure 2. The sample is not restricted based on past itemizing. For PSID regressions, presented estimates use the post-LASSO IV method (akin to table 4, column 3) for a sample that has been selected on wealth shock size. Estimates at left are for PSID wealth shocks ≤ 0 and $\leq \$10,000$. Estimates at right are for positive wealth shocks from $10,000 up to the amount stated on the horizontal axis.

dows longer than one survey wave—that is, the change in giving caused by the change in wealth over four, six, eight etc. years. None of those estimates suggest that PSID wealth shocks have an effect of giving that manifests over longer time periods.[15]

5.3 The effect of wealth shocks on giving is surprisingly small

As described above, we elicited estimates of the effect of a $100,000 wealth shock on charitable giving and four other spending categories (meals eaten out, clothing, leisure trips, and home furnishings). Survey respondents largely predicted giving effects that were much larger than the estimates from the PSID survey, but about the same size as the short-run lottery shock effect.

This raises the possibility that the PSID estimates are too low for some reason, for example, if our methodology is attenuating the causal estimates in some manner, or if the wealth shocks from asset price changes are not salient.

In the appendix, we check for this possibility with eight "nocebo" tests. That is, we estimate eight placebo tests, but in reverse: specifications in which we expect to see an effect to evaluate whether some error in the research process generates a small or null estimate instead.[16] Four of the nocebo tests estimate the causal effect of a wealth shock on the four other categories of spending included in our incentivized survey. The other

[15] One possible reason the PSID estimates are somewhat smaller than the lottery effects are the every-other-year design of the PSID survey. If the PSID asks respondents about their giving in year t and $t+2$, but the wealth shock occurs in period $t+1$ between survey waves, then we may be failing to observe about half of all PSID shocks to transitory giving.

[16] A *nocebo effect* in a double-blind medical trial refers to a negative placebo effect—that is, a situation where the patient experiences negative psychogenic effects from a sugar pill or similar nontreatment similar to the side effects of a real medication. In economics, we typically refer to the placebo test as a check for a spurious treatment effect in response to nontreatment, without distinguishing the direction of the effect. We leave to the reader to decide for his- or herself whether calling the tests in this section "nocebo" tests is an appropriate generalization of the placebo metaphor.

four use our methodology to estimate causal effects of a wealth shock on four other outcomes with well-established wealth effects estimated using other methods (college attendance, birth of a child, purchase of a new car, and a measure of physical health).

Our estimates for the wealth effects of each of the nocebo regressions is much closer to the predicted effects of the incentivized survey and to the prior wealth-shock literature, respectively, than our charitable giving PSID estimate is to the survey or to the short-term lottery effect. We conclude that our method is sound, and the PSID null results are credible.

5.4 Summary of additional checks

In the appendix, we present a variety of additional checks for the main results presented here. None of them change our intepretations of the presented results.

We present extensive-margin outcomes for whether lottery wins predict total giving above varying totals. Unsurprisingly, very large charitable gifts are more likely following very large lottery wins. The heterogeneous effect of lottery winnings applies throughout the giving distribution as well as the win distribution.

We confirm that a first-differenced research design is not driving our small PSID estimates by implementing an alternative approach, using panel Poisson regression. Our results are unchanged.

We implement the survey-data IV approach using the Survey of Consumer Finances panel as an alternative to the PSID. The SCF panel is much shorter and smaller than the PSID, but is a higher-income and -wealth population. The resulting estimates are less precise than those estimated with the PSID, but are consistent in magnitude.

We estimate heterogeneous effects for PSID wealth shocks from liquid vs. illiquid asset classes, and for positive vs. negative wealth shocks. We do not observe any noteworthy differences on these dimensions.

We estimate heterogeneous wealth-shock effects by observable demographics for both lottery and survey-data approaches. While we do observe statistically significant demographic heterogeneity in some settings, the differences in magnitudes across groups is never large enough to be material.

6 Conclusion

Cross-sectional associations and the mythos of the wealthy philanthropist in US culture would lead one to expect that wealth shocks would have a large positive effect on giving behavior. A survey of nonexpert US respondents about their expected effects of wealth shocks on charitable giving confirms that this view is widely held. These beliefs turn out to be accurate for lottery winnings, which the median belief and average lottery response both close to $1000 for a $100,000 win. In contrast, asset price shocks appear to have no effect on charitable giving. We rule out many potential explanations for the difference and conclude that the charitable response depends on the natures of the wealth shock.

We also show several unique features of the charitable response. We find that it is much more short-lived than effects on labor supply and many forms of consumption. It is also find that it is convex in the year of the win, with shocks in excess of $10 million having much larger proportional effects. By examining particular levels of giving, we

show that there is also considerable heterogeneity, with some individuals giving more than 10 percent of the amount won. Our next draft will explore these patterns in greater detail.

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