# How Americans Respond to Idiosyncratic and Exogenous Changes in Household Wealth and Unearned Income*

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**Abstract**

We study how Americans respond to idiosyncratic and exogenous changes in household wealth and unearned income. Our analyses combine administrative data on U.S. lottery winners with an event-study design. We first examine individual and household earnings responses to these windfall gains, finding significant and sizable wealth and income effects. On average, an extra dollar of unearned income in a given period reduces household labor earnings by about 50 cents, decreases total labor taxes by 10 cents, and increases consumption by 60 cents. These effects are heterogeneous across the income distribution, with households in higher quartiles of the income distribution reducing their earnings by a larger amount. Next, we examine margins of adjustment other than earnings and, in the course of doing so, address a number of important economic questions about how additional wealth or unearned income affect retirement decisions and labor market dynamics, family formation and dissolution, entrepreneurship and self-employment, and geographic mobility and neighborhood choice. Lastly, we carefully compare our findings to those reported in existing lottery studies. This comparison reveals that existing U.S. studies substantially underestimate wealth and income effects because they use measures that understate earnings responses and overstate wealth changes associated with lottery wins.

**JEL Codes:** D15, J22, H21, H31, H53

**Keywords**: income effects; labor supply elasticities; lottery winning; wealth effects

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# 1 Introduction

How do Americans respond to idiosyncratic, unanticipated, and exogenous changes in household wealth and unearned income? Economists and policymakers are keenly interested in this question. For example, the earnings responses to such shocks are important, both to infer income and wealth effects and to assess the effects of public policy such as income taxation and cash transfers like universal basic income. However, giving a credible answer to this question has proven difficult. A key challenge is to find variation in wealth or unearned income that is both as good as random and specific to an individual as opposed to economy-wide. Such variation is necessary to isolate the effects of changes in wealth or unearned income, holding fixed other determinants of behavior such as preferences and prices.

The goal of our paper is to address this challenge and offer a credible answer as to how Americans respond to idiosyncratic and exogenous changes in household wealth and unearned income. We analyze a wide range of individual and household responses to lottery winnings and explore the economic implications of these responses for a number of key questions that economists and policymakers are interested in. The analyses combine administrative data for the U.S. for the period 1999 to 2016 with an event-study design that compares household behavior before and after winning a lottery. Winning a lottery can be viewed as a shock to household wealth or, equivalently, a permanent shock to unearned income. Importantly, the win is an idiosyncratic change that is difficult to anticipate.

As described in Section 2, our analyses are based on a population-level panel data set which is constructed by combining the universe of worker tax records with third-party-reported lottery winnings. The worker data give us information about labor earnings and other sources of individual and household income, as well as various demographics. The data on lotteries contain a record for winnings in state lotteries, with information about the unique identifier of each winner and the winning amount.

In Section 3, we take advantage of the individual-level panel data to perform an event study that compares the earnings of lottery winners, before and after they win. These within-person comparisons let us eliminate unobserved time-invariant individual heterogeneity while controlling flexibly for age. We find evidence of sizable, swift, and persistent labor market responses to winning the lottery. A potential concern is that earnings may have changed over time for reasons other than the lottery winnings, such as calendar time effects. To address this concern, we show that our estimates barely move if we include a control group (e.g. individuals who later win the lottery) in the event-study regression to eliminate common time effects.

To interpret the magnitudes of these event-study estimates, we use the variation in the timing of lottery wins as an instrumental variable (IV) for lottery winnings. The resulting IV estimates tell us the individual and household responses per dollar of additional wealth due to the lottery winnings. We find that Americans respond to an exogenous increase in household wealth by significantly reducing their employment and labor earnings. For an extra 100 dollars in wealth, households reduce their annual earnings by approximately 2.3 dollars on average. The labor earnings responses per dollar of additional wealth are larger for higher income households as compared to lower income households. Households in the bottom quartile of the pre-win income distribution reduce their annual household labor earnings by 1.3 dollars per 100 dollars of additional wealth, whereas winners in the top quartile decrease their annual household labor earnings by 3.1 dollars per 100 dollars of additional wealth.

The size of these wealth effects can be hard to gauge as the observed responses to windfall gains should vary across individuals depending on a number of factors, such as the age at which the individual wins, her savings behavior, and the tax rates she faces. This issue motivates our analyses in Section 4. In this section, we first study how winning the lottery changes the unearned income that households allocate to consumption and leisure across years. We then estimate the share of yearly allocated unearned income that is spent on reducing labor versus increasing consumption. In other words, we estimate both the allocation of the windfall gains over time and the marginal propensities to earn (MPE) and consume (MPC) out of unearned income in a given period.

The analyses in Section 4 draw on two popular approaches to study the allocation of windfall gains over time: the annuitization method and the capitalization method.[1] Under the annuitization method, one assumes that households smooth winnings over the remaining lifetime, while under the capitalization method one does not make any assumptions about household behavior, but rather uses observed capital income and rates of return to compute unearned income directly. Since there are pros and cons to each method, we apply both approaches and find that they produce remarkably similar measures of per-period unearned income and consumption expenditure. This similarity not only increases confidence in the estimated MPEs and MPCs, but also indicates that American lottery winners save most of the windfall for future periods, as predicted by textbook models of consumption smoothing.

To draw causal inference about the MPEs and MPCs, we use variation in the timing of lottery wins as an instrument for yearly unearned income. On average, an extra dollar of unearned income in a given period reduces household labor earnings by about 50 cents, decreases total labor taxes by 10 cents, and increases consumption by 60 cents. Interestingly, the MPEs and MPCs vary systematically across the pre-win income distribution. For example, individuals in the bottom quartile of the pre-win income distribution use most of the increase in unearned income on consumption, while individuals in the top quartile prioritize reducing labor over increasing consumption.

The analyses in Sections 3 and 4 are centered around employment and earnings responses to exogenous changes in household wealth and unearned income. This focus is in line with the canonical models of labor supply where the individual's problem is restricted to choosing hours of work or earnings given a wage rate. In reality, however, individuals could be responding along several other margins. In Section 5, we therefore expand the analysis by examining margins of adjustment other than earnings and, in the course of doing so, address a number of important economic questions about how additional wealth or unearned income affect retirement decisions and labor market dynamics, family formation and dissolution, entrepreneurship and self-employment, and geographic mobility and neighborhood choice. In our investigation of these questions, we describe the key identification and measurement challenges that arise, and motivate how lottery winnings as a source of variation in wealth or unearned income can address these challenges. We discuss how our findings relate and contribute to the current evidence base, which is either by using a change in wealth and unearned income that plausibly meets the requirements of being exogenous, unanticipated and idiosyncratic, or by obtaining the first evidence for the U.S., or by achieving sufficiently precise estimates to draw firm

[1] Blundell, Pistaferri, and Preston (2008), Jappelli and Pistaferri (2010), and Blundell, Low, and Preston (2013) are some examples of papers that use the annuitization method, while Stewart (1939), Saez and Zucman (2016), and Smith, Zidar, and Zwick (2020) are examples of applications of the capitalization method.

conclusions about signs or magnitudes.

We conclude our paper with a comparison to existing work. The four most closely related studies to ours are, arguably, Imbens, Rubin, and Sacerdote (2001), Bulman, Fairlie, Goodman, and Isen (2021), Cesarini, Lindqvist, Notowidigdo, and Östling (2017), and Picchio, Suetens, and van Ours (2018), all of which estimate earnings responses to lottery winnings. The former two studies use data from the U.S., while the latter two studies use data from Sweden and the Netherlands, respectively. In Section 6, we carefully compare our estimates to those reported in these studies. This comparison leads to two key conclusions. First of all, once one performs apples-to-apples comparisons that use the same measures of earnings responses and wealth changes associated with lottery wins, we find similar estimates as reported by the existing U.S. studies. We argue an important limitation of the existing U.S. studies is that they use measures that understate earnings responses and overstate wealth changes associated with lottery wins. These problems lead them to substantially underestimate wealth and income effects.[2] Second, the estimates from the European studies are consistently and noticeably smaller than ours, even when we use a comparable measure of earnings and lottery winnings. These findings caution against the practice of using wealth effects or income effects from one country as inputs for models that are otherwise calibrated or estimated using data from other countries.[3]

Our paper is also related to two other sets of studies which attempt to isolate the labor market responses to an exogenous change in unearned income or wealth. The first utilizes various forms of natural experiments as sources of shocks to unearned income or wealth. Examples include changes in transfer income (e.g., Krueger and Pischke, 1992; Bengtsson, 2012; Jacob and Ludwig, 2012; Gelber, Isen, and Song, 2016; Feinberg and Kuehn, 2018; Jones and Marinescu, 2019) and inheritances and bequests (e.g., Holtz-Eakin, Joulfaian, and Rosen, 1993; Andersen and Nielsen, 2012). Such natural experiments are interesting because they present plausibly-exogenous variation in resources which can be used to draw causal inferences about labor market responses. However, responses to such shocks may be hard to gauge and economically interpret, as they should vary with individual characteristics as well as the nature of the shock, such as whether it is persistent or transitory, idiosyncratic or market-wide, and expected or unanticipated. For estimating responses to unearned income or wealth, lottery winning presents the advantage of being an idiosyncratic, plausibly-exogenous shock that is unlikely to be anticipated and, as we show in Section 4, the earnings responses to such shocks can be directly mapped into economically-relevant parameters.

The second set of studies uses models of labor supply to attempt to recover income effects and labor supply elasticities from observational variation in unearned income, wages, and tax rates. The models, data, and findings have been summarized and critiqued in multiple review articles including Pencavel (1986), Killingsworth and Heckman (1986), Blundell and Macurdy (1999), Keane (2011), and Saez, Slemrod, and Giertz (2012). As emphasized in these reviews, there is no consensus about the size of income effects. One reason is that it has been difficult to find an exogenous source of variation in unearned income. Many studies

[2]This conclusion, in turn, led a number of studies to build models specifically around the assumption that income effects are essentially negligible (Auclert and Rognlie, 2020; Airaudo, 2020; McKay and Wieland, 2021; Wolf, 2021). Moreover, Auclert and Rognlie (2020) show that standard New Keynesian models must fail in important ways if preferences are restricted such that wealth has no effect on labor supply. Thus, they argue, it is necessary to develop new classes of New Keynesian models.

[3]For example, a set of studies use these estimates to justify building models specifically around the assumption that income effects are essentially negligible (Auclert and Rognlie, 2020; Airaudo, 2020; McKay and Wieland, 2021; Wolf, 2021), whereas others use these estimates to directly calibrate their own models (Alon, Coskun, and Doepke, 2019; Kindermann, Mayr, and Sachs, 2020).

use observational variation in spousal or capital income over time and across households to instrument for changes in unearned income. The challenge, however, is that it is difficult to ensure that such variation is independent of other determinants of behavior such as preferences and wages. Other studies use variation in take-home pay that arises either from tax reforms or from changes in observed earnings or wages. However, it is often not clear that these changes allow one to separately identify income and substitution effects without strong assumptions on functional form and the distribution of unobservables.[4] By comparison, lottery winnings allow one to isolate the effects of changes in unearned income, holding fixed all other determinants of behavior such as preferences and wages. Thus, our paper offers credible evidence on income effects in the U.S., including how they vary across the income distribution.

## 2 Data and sample selection

In this section, we describe the institutional background and the data, explain the construction of the estimation sample, and define the key terms and conventions that we use throughout our paper.

### 2.1 Institutional background

Currently, 45 U.S. states conduct some type of state lottery. Any winning of at least $600 and at least 300 times the purchase price of a ticket triggers generation of Form W-2G. This form is provided to both the winner and to tax authorities, and is used for income tax filing. Form W-2G contains information about the amount of lottery winnings disbursed in a given year, a unique identification number for the winner, and her state of residence.[5] For lottery drawings with multiple winners, the winnings are split before a Form W-2G is issued, and separate W-2G forms are furnished to each winner.

In the U.S., lottery winnings are considered ordinary taxable income in the year the payment is made. The vast majority of winnings are paid lump sum but there is a small fraction of winnings that are paid in installments over time. The legal treatment of the ownership of lottery winnings for married households is determined by state laws. Although there is some variation across states, de facto most lottery winnings accrued during marriage are treated as being owned by spouses equally.[6]

### 2.2 Data sources

We begin with the universe of annual W-2G forms generated between 1999 and 2016. This data on W-2G forms is merged to two additional data sources. First, it is linked to each winner's data on wage earnings

[4]Reflecting this identification challenge, Kimball and Shapiro (2010) argue there is no consensus on the size of income and substitution effects in labor supply.

[5]Form W-2G is also used for other kinds of unusual payments, such as those from horse-race betting and casino gambling. However, we directly observe whether the Form W-2G is a payment from a state lottery for lottery winnings, and restrict attention to these payments.

[6]Broadly speaking, there are two groups of states: those that presume explicitly in their statutes that all property acquired during marriage should be divided equally in the event of a divorce, and those that do not have such a presumption explicitly stated (Hersch and Shinall, 2019). In practice, the prevailing tendency in all states is to treat lottery winnings as owned by spouses equally (see, e.g., In re Marriage of Mahaffey, 1990; Smith v. Smith, 1990; Ullah v. Ullah, 1990; In re Marriage of Swartz, 1993; DeVane v. DeVane, 1995; Thomas v. Thomas, 2003).

as reported on annual W-2 forms. Next, it is linked to each winner's annual federal tax returns (Form 1040 with various schedules), which contain data on income from other sources (self-employment, savings, Social Security, and unemployment insurance), on total federal income taxes owed, and on various household characteristics. We also observe data on spousal income for married winners who file tax returns. Most married households in the U.S. file tax returns jointly.

While we observe reported income and federal income taxes, we do not observe state income taxes directly. To compute income taxes incorporating both federal and state income taxation (total income taxes) and marginal tax rates, we use the tax calculator of Bakija (2019).[7] By comparing federal income taxes computed using this calculator to the observed federal income taxes owed, we confirm that the calculator is very accurate for our sample, with reported and calculated values being virtually identical.[8]

### 2.3 Terminology and key variables

We now define key variables and conventions that we use throughout our paper. At the outset, we note that many economic outcomes are reported at the household level and cannot be attributed to a specific individual for married households. Therefore, for consistent comparison of all outcomes across both single and married households, we report all variables, unless we explicitly state otherwise, on a per-adult basis by normalizing them by the number of adults in the household (one if single and two if married).

**Lottery win variables.** We define the *win year* for any individual as the first year in which her Form W-2G from a lottery winning appears in our sample. Similarly, whenever we refer to the year in which an individual wins a lottery we mean the win year for that individual. Unless we explicitly state otherwise, we generally measure the *size of the lottery win* on a post-tax, per-adult basis. To compute post-tax lottery winnings, we begin with the (pre-tax) amount reported on Form W-2G for the win year. We then calculate additional taxes from the lottery winnings in the win year in three steps. We take total taxable income observed two years before the win year (i.e., pre-win taxable income) and calculate total income taxes in the win year absent behavioral response and lottery winnings. We then add lottery winnings to pre-win taxable income and re-calculate total income taxes in the win year. The increase in taxes after adding winnings is our measure of additional taxes from lottery winnings. Post-tax lottery winnings, then, are pre-tax winnings in the win year net of the calculated additional taxes. Form W-2G does not indicate whether the lottery win is paid lump-sum or in installments. However, we can infer the latter case when we observe multiple consecutive years of W-2G forms with similar reported amounts. When this occurs, we take the amount reported on the first-observed Form W-2G and convert it to the lump-sum present value by assuming that installments are paid in equal amounts over 25 years, using a 2.5 percent interest rate for discounting.[9]

[7] We follow common practice (see, e.g., Brewer, Saez, and Shephard, 2010; Chetty, Friedman, Olsen, and Pistaferri, 2011; Saez, Slemrod, and Giertz, 2012; Kleven and Schultz, 2014) and use a tax calculator to impute marginal tax rates. Accounting for additional distortions from future transfers, such as social security payments, would require specifying a full structural life-cycle model (see, e.g., Altig, Auerbach, Kotlikoff, Ilin, and Ye, 2020).

[8] In our data, reported and calculated values have a correlation of 0.997 and a median absolute deviation of $47.

[9] There is some variation across installment-paid lotteries over whether the winner is required to take the winnings in installments or merely has an option to do so, the number of years over which the installments are paid, and whether and how nominal win amounts are adjusted for inflation. We do not have sufficient information to know which specific rules apply to a given lottery

**Tax return and earnings variables.** *Wage earnings* of any individual is the sum of pre-tax wages, tips, salary, taxable fringe benefits, and other forms of taxable compensation provided by all employers of that individual in a given year. *Self-employment income* consists of self-employment business income, farm income, and partnership income in a given calendar year. *Total labor earnings* is the sum of wage earnings and self-employment income. *Capital income* is the sum of dividend income, interest income, pension and annuity income, rental and royalty income, and non-labor income from estates, trusts, farms, and mortgage investments in a given calendar year. *Gross income* is the sum of total labor earnings, capital income, and Social Security and unemployment insurance payments. We report all monetary values in inflation-adjusted 2016 U.S. dollars using the Consumer Price Index to adjust.[10]

**Research design variables.** For our research design we generally use the following terminology and notation. We call all individuals who won a lottery in a given calendar year a *cohort*, and denote that year by $w$. The *baseline year* is defined as $s$ years prior to the lottery win and is used as the pre-treatment reference point in the event study. We report all our results for $s = 2$ but our findings are virtually unchanged for other choices of $s$. The *event time* $\ell$ for cohort $w$ corresponds to calendar year $w + \ell$, where $\ell$ can be positive or negative depending on whether we look at the outcomes which occur after or before winning a lottery.

**Variables measuring other margins of adjustment.** We use ancillary variables included on W-2 forms and household tax returns as measures of broader labor market responses. For employed winners, we define their pre-win *employer* as the linked firm observed in the baseline year on the W-2 form.[11] A *job mobility* indicator denotes calendar years when the employer differs from that in the baseline year for employed winners. A *geographic mobility* indicator denotes whether a winner's current-year Census tract differs from the previous year's tract. We provide additional details on these and all other key variables in our paper in Online Appendix C.

## 2.4 Descriptive statistics

**Summary and representativeness of lottery winners.** To construct our baseline estimation sample, we impose three restrictions. First, we require each individual to be of working age (that is, between age 21 and 64) in their win year. Second, we require each individual to be in the sample for at least two years prior to generating their first Form W-2G. This ensures that we observe pre-win economic outcomes for each individual, as needed in our research design. Lastly, we restrict our baseline estimation sample to lottery wins of at least $30,000 per winner. We choose this amount since, if smoothed over her remaining lifetime, it amounts to an economically-meaningful increase in income of approximately $1,000 per year for an average winner.[12]

win. In any case, inferred installment-paid lottery disbursements are rare, as they comprise around 2 percent of all winners in U.S. administrative data, and dropping them from our sample barely moves our results.

[10] In the main analysis, we use wage earnings as they are reported on Form W-2. We also explored winsorizing wage earnings and the results do not change. We report the results of this robustness check in Appendix Figure B.1.

[11] For individuals linked to multiple firms through Form W-2, this is the identity of the highest-paying employer.

[12] The point estimates of wealth effects and effects of unearned income in Sections 3.2 and 4.3, respectively, are somewhat larger if we include wins smaller than $30,000. However, including the smallest wins makes the sample of lottery winners less

Column 1 in Table 2.1 reports a set of key summary statistics for our sample. All summary statistics for the winners are measured in their baseline year, i.e., two years prior to their win year. Each statistic is calculated as a weighted average using cohort size as weights. We compare winners to the average working-age U.S. tax filer in column 2. We find that lottery winners have broadly-comparable wage earnings, employment status, and age as compared to an average tax filer. However, lottery winners are more likely to be single and male, and slightly less likely to own a home. The last four rows of Table 2.1 show how the income distribution of lottery winners compares to that of working-age tax filers. We make this comparison by calculating the share of individuals in our sample that falls into each quartile of adjusted gross income (AGI) in the working-age tax filer population. We find that lottery winners are well represented in each income quartile.

Table 2.1: Summary statistics of individual characteristics and labor market outcomes

| | | **Winners (Age 21-64)** | **Tax Filers (Age 21-64)** |
|---|---|---|---|
| | | (1) | (2) |
| *Covariate* | *Statistic* | | |
| Wage Earnings | Mean | $34,541 | $33,005 |
| Employment | Prop. | 0.79 | 0.80 |
| Age | Mean | 43.93 | 41.78 |
| Female | Prop. | 0.39 | 0.51 |
| Married | Prop. | 0.45 | 0.58 |
| Homeowner | Prop. | 0.45 | 0.49 |
| Relative Q1 AGI Share | | 0.28 | 0.25 |
| Relative Q2 AGI Share | | 0.21 | 0.25 |
| Relative Q3 AGI Share | | 0.24 | 0.25 |
| Relative Q4 AGI Share | | 0.27 | 0.25 |
| *N* | | 90,731 | 154,372,671 |

*Notes:* This table presents a summary of the descriptive statistics in our baseline estimation sample of working-age winners. All monetary values are reported in 2016 U.S. dollars, using the Consumer Price Index to adjust for inflation. In the first section of the table, we report mean characteristics. All values for the winners sample are measured two years prior to the win year and reported as cohort-size-weighted averages. The final column reports the same set of descriptive statistics for the universe of tax filers aged 21-64, taking a population-weighted average across the 1999 to 2015 tax years. In the second section of the table, we present a comparison of the pre-win distribution of per-adult adjusted gross income (AGI) among winners to that in the universe of tax filers aged 21-64. For each calendar year of tax-filer data, we map each winner to the corresponding quartile in the tax-filer AGI distribution. We then calculate the share of winners falling into each quartile of the tax-filer AGI distribution. Finally, we take the mean of the shares across calendar time (for each quartile). For the tax-filer population, this share is mechanically 0.25.

**Distribution of lottery winnings.** In Appendix Table A.1, we summarize the distribution of lottery winnings in our baseline estimation sample. In the first column, we report this summary for lottery winnings in our data measured at the *household* level and on a *pre-tax* basis. Next, in the second column, we adjust pre-tax household lottery winnings for taxation of winnings and number of adults (corresponding to a change in

comparable to the population at large in terms of observable characteristics. We thus prefer to focus on larger wins that yield observationally-representative samples and economically-meaningful increases in annual income.

wealth available to be spent or saved). Finally, in the third column, we convert the change in wealth into annuity payments and summarize the implied change in unearned annuity income in a given year.[13] The median change in wealth is roughly $44,000 ($1,900 annually), and only about 10 percent of wins exceed a wealth change of $200,000.

A natural question is how the distribution of lottery winnings compare between our study and other studies that estimate the earnings responses to lottery winnings. It is difficult to give a general answer to this question as we do not observe the distribution of lottery winnings in any study other than Imbens, Rubin, and Sacerdote (2001).[14] However, Appendix Figure B.2 shows the stark differences in the size of the pre-tax lottery winnings of a household in our data as compared to the two estimation samples of Imbens, Rubin, and Sacerdote (2001). Relative to our data, the estimation samples in Imbens, Rubin, and Sacerdote (2001) contain a lot more large prizes. Even in the preferred estimation sample of Imbens, Rubin, and Sacerdote (2001) (which excludes the biggest winners), the median prize is $750,000 and the vast majority (97 percent of the sample) has prize amounts over $200,000.[15] By comparison, the median pre-tax winning is merely $68,000 in our data, and only 22 percent of pre-tax winnings exceed $200,000.

**Policy relevance of lottery shocks.** In our baseline analysis, we follow the previous literature by focusing on average responses to winning the lottery. A natural question is how to think about the policy relevance of the average lottery shocks in our data. In general, one would like to have exogenous shocks to wealth and unearned income that are broadly comparable in magnitude to changes associated with policy reforms. The shocks that we study are comparable in magnitude to both typical shocks to labor income and to the permanent income changes associated with many common policy proposals. To see this, note that our average size of lottery win is approximately equivalent to a $8,000 post-tax annuity payment (Appendix Table A.1). Such a permanent change in annual income is comparable to several other relevant permanent income changes considered in the literature. For example, in the U.S., a 1 standard deviation shock to the permanent component of log earnings approximately corresponds to $6,000 annually, as follows from estimates in Meghir and Pistaferri (2004).

We can also look at reforms to the tax and transfer system as a comparison to our lottery shocks. In terms of transfer policy, there is a range of popular proposals to introduce a UBI, which is a lump-sum payment to each adult. The suggested values of these transfers range from $500 to $1000 tax-free per month (see, e.g., Stern, 2016, Lowrey, 2018, and Yang, 2018), which corresponds to a $6,000 to $12,000 recurring annual payment. In terms of tax policy, a much discussed policy lever to raise revenue is an increase to the top

[13]For a winner of age $k$ with change in wealth $L$ and $T-k$ remaining years of life, we calculate the change in unearned annuity income in a given year as $\frac{r}{1+r}\left(1-\left(\frac{1}{1+r}\right)^{T-k+1}\right)^{-1}L$. We use $r = 0.025$ (the average risk-free real interest rate in the U.S. for our period of observation) and $T = 80$ (an 80-year life expectancy).

[14]We are, however, able to calculate the average pre-tax lottery winnings across studies. This comparison reveals that Imbens, Rubin, and Sacerdote (2001) is the outlier with lottery wins that, on average, are several times larger than in both Bulman, Fairlie, Goodman, and Isen (2021), our study, Cesarini, Lindqvist, Notowidigdo, and Östling (2017), and Picchio, Suetens, and van Ours (2018). The latter two studies – using data from Sweden and the Netherlands – have the smallest average lottery winnings.

[15]Imbens, Rubin, and Sacerdote (2001) define the biggest winners as those with wins over $100,000 (in 1986 dollars) annually over 20 years (18% of their winners) . When we convert this to pre-tax winnings and to 2016 dollars, these correspond to winnings of approximately $2.8 million dollars. About 1 percent of winners in our sample fall into this category.

marginal tax rate on earnings. For each year that an individual works, an increase in the top marginal tax rate on earnings can be represented as an increase in the tax rate on *all* earnings plus an increase in post-tax unearned income equal in magnitude to the income cut-off for the top tax bracket scaled by the size of the tax rate increase (see Saez, 2001). Thus, under the current tax code, evaluating the effect of each percentage point increase in the top marginal tax rate for a single taxpayer requires evaluating the effect of an increase in the unearned income of around $5,000 for each year that an individual's earnings are in the top tax bracket.

# 3 Earnings responses to an exogenous change in wealth

In this section, we present and apply the event-study estimator that we use to draw causal inferences about how individuals and households respond to winning the lottery. We demonstrate how we arrive at this estimator, assess the threats to identification, and show that a variety of alternative estimators yield remarkably similar estimates. We focus first on how to recover the average earnings responses to winning the lottery, before showing how to translate these responses into estimates of wealth effects.

## 3.1 Earnings responses to winning the lottery

**Parameter of interest and research design.** We denote outcomes of interest by $Y_{i,t}$. For now, the outcome we focus on is wage earnings. As described in Section 2.3, we let $w$ index the calendar year that defines a particular cohort of lottery winners. Let $D_{i,t} = 1$ for individuals who first win the lottery in calendar year $t$ or earlier (i.e., those in cohort $w \leq t$), and $D_{i,t} = 0$ otherwise. Let $Y_{i,t}(1)$ denote the potential wage earnings of an individual that has experienced her first-observed win, and $Y_{i,t}(0)$ denote the potential wage earnings of an individual that has not experienced her first-observed win. Potential wage earnings are related to observed wage earnings through $Y_{i,t} = Y_{i,t}(0) + D_{i,t}(Y_{i,t}(1) - Y_{i,t}(0))$.

To define the parameter of interest, it is useful to consider a specific cohort of winners who win in year $w$. For this cohort, the parameter of interest is the cohort-specific average effect of the lottery win on $Y$ as measured in post-win year $w + \ell$,

$$\rho^{w,\ell} \equiv \mathbb{E}\left[Y_{i,w+\ell}(1) - Y_{i,w+\ell}(0) \middle| \, i \text{ won in } w\right]. \tag{3.1}$$

The identification challenge is that we do not observe $\mathbb{E}\left[Y_{i,w+\ell}(0) \middle| \, i \text{ won in } w\right]$, which is the average wage earnings of winners in year $w + \ell$ had they, counterfactually, not won. In the rest of the paper, we refer to $\mathbb{E}\left[Y_{i,w+\ell}(0) \middle| \, i \text{ won in } w\right]$ as the counterfactual outcome mean for winners in year $w + \ell$.

**First-difference estimator.** One natural approach to recovering our parameter of interest is to use a first-difference (FD) estimator,

$$\underbrace{\mathbb{E}\left[Y_{i,w+\ell} - Y_{i,w-s} \middle| \, i \text{ won in } w\right]}_{\text{difference over time for winners in } w}, \tag{3.2}$$

where $w - s$ is a baseline, pre-win year. This FD estimator is illustrated using our data in Figure 3.1a, which presents the wage earnings of winners in a randomly drawn year, 2003. In the graph, we adjust for common life-cycle effects on earnings by regressing wage earnings on a full set of dummies for each age. On the $y$-axis, we report mean annual wage earnings in a given time period. The running variable on the $x$-axis is event time, with 0 denoting the year of the lottery win.

The graphical evidence in Figure 3.1a highlights two important features of our data. First, there is a sizable, swift, and persistent change in wage earnings from before to after the year of the lottery win. Second, wage earnings change little if at all in the years prior to winning the lottery. This suggests that the FD estimator defined in expression (3.2) may produce credible estimates of the effects of winning the lottery. The reason is that this estimator produces consistent estimates under the assumption that wage earnings would not have changed from before to after the lottery win in the absence of winning the lottery (conditional on age). Under this assumption, the FD estimator (using $s = -2$ as the baseline year) suggests that the 2003 cohort of lottery winners reduced their wage earnings by \$3,211 (approximately 10 percent) in response to winning the lottery, as illustrated in Figure 3.1a.

**Controlling for time effects.** The key threat to the identifying assumption of the FD estimator defined in expression (3.2) is that economic outcomes may change over time (conditional on age) not only due to the lottery winnings but also due to other factors, such as contemporaneous aggregate changes in the economy. There are two ways to address this concern about confounding time effects.

One possibility is to take advantage of the fact that we observe many cohorts of lottery winners who win in different calendar years. In Figure 3.1b, we follow this approach, pooling the wage earnings data of winners across the 2001 to 2016 win years. We re-center the data so that period 0 is the year of win for all individuals (even if they win in different calendar years) and adjust for time effects in addition to age.

We report the estimates from this pooled approach in Figure 3.1b, using $s = -2$ as the baseline year. Estimates mirror the unadjusted estimates in Figure 3.1a for the subsample of 2003 winners. There is no evidence of significant changes in earnings prior to the year of win. By contrast, there is a sharp change in earnings from before to after the year of the lottery win. As illustrated in Figure 3.1b, the FD estimator net of calendar time effects suggests that lottery winners, on average, reduced their earnings by \$3,768 per year (approximately 12 percent) in response to winning the lottery.

**Difference-in-differences estimator.** An alternative approach to address concerns about time effects involves finding a control group that would arguably have experienced the same change in earnings from before to after the lottery win in the absence of winning the lottery. Such a control group allows one to difference out time effects by constructing a DiD estimator. A natural candidate for a control group is the individuals who first win the lottery in later years. Using such a control group, the resulting DiD estimator between the treatment and control groups for cohort $w$ is

$$\underbrace{\mathbb{E}\left[Y_{i,w+\ell} - Y_{i,w-s} \middle| \, i \text{ won in } w\right]}_{\text{difference over time for treatment group}} - \underbrace{\mathbb{E}\left[Y_{i,w+\ell} - Y_{i,w-s} \middle| \, i \text{ has not won by } w^* \geq w+\ell\right]}_{\text{difference over time for control group}}, \tag{3.3}$$

where $w^*$ reflects the choice of how much later we look to find a later-winning control group.

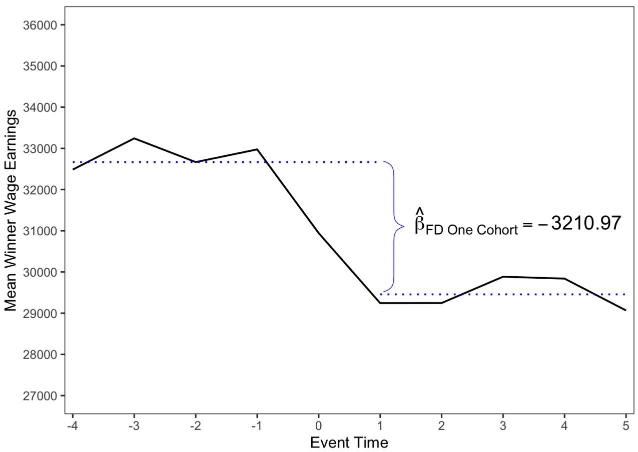
The DiD estimator eliminates unobserved time-invariant individual heterogeneity by comparing winners before and after they win, while accounting for year and event-time effects by using the later winners as a control group before they win. As long as the individuals in the treatment and control groups would have had a common trend between years $w - s$ and $w + \ell$ in the expected potential outcomes in the absence of the lottery wins $Y(0)$, the DiD estimator defined in expression (3.3) recovers the average impact of lottery winning for cohort $w$ in year $w + \ell$ for $\ell \geq 0$.

In Figure 3.1c, we take this DiD estimator to our data. The dark line shows the mean wage earnings of the treatment group of lottery winners before and after their own lottery win. The lighter line shows the mean wage earnings of the control group of later winners (i.e., $w^* = w + \ell$) in the years prior to their own lottery win. The pre-trends of the two groups are remarkably similar, suggesting that later winners are a suitable choice of control group.[16] As illustrated in the graph, the DiD estimate is given by the change in mean wage earnings before and after the win year of the treated individuals, in the treatment group relative to the control group. In our data, this estimate suggests that individuals, on average, reduce their earnings by approximately \$3,800 per year (approximately 11 percent) in response to winning the lottery, which is nearly identical to what we found using the FD estimator.

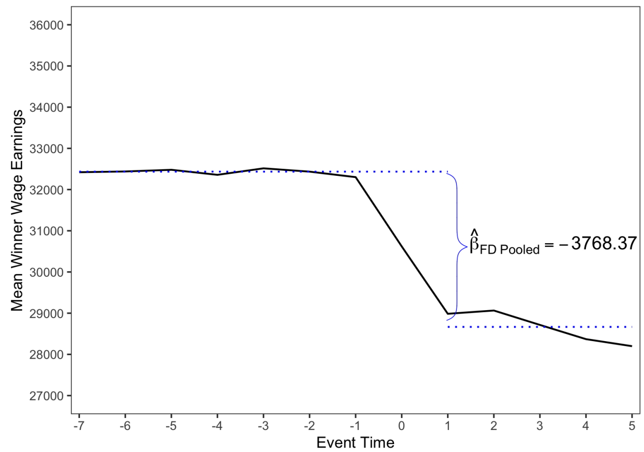
As an alternative control group, we consider individuals who did not win the lottery during the period we consider. In Figure 3.1d, we plot the mean earnings of this control group over time. The graph shows that the DiD estimate does not materially change if we use the never winners as the control group instead of the later winners.

**Graphical comparison of estimators.** In sum, we find that whether using a FD estimator (which relies on the absence of time effects) or a DiD estimator (which relies on a common trend between treatment and control groups), our conclusions are strikingly similar. The reason is simply that in this context, a control group is only needed to eliminate time effects. Our data, however, do not show any evidence of strong time effects (conditional on age), and, as a result, both the FD and DiD estimators produce remarkably similar results. This is true both if we use never winners or all later winners as the control group. We summarize these findings in Figure 3.2. In this graph, we plot the differences between the treatment and control group (if any) across event time, while normalizing the level of wage earnings of each group to be zero in event time $s = -2$. By examining the pattern of earnings in the years after the win, we can then directly compare the effects estimates across estimators, and they are remarkably similar. In light of this similarity, our baseline specification will be a DiD estimator using all available later winners as our preferred approach, given its attractive combination of flexibility in estimation and maximal use of later winners as control units.

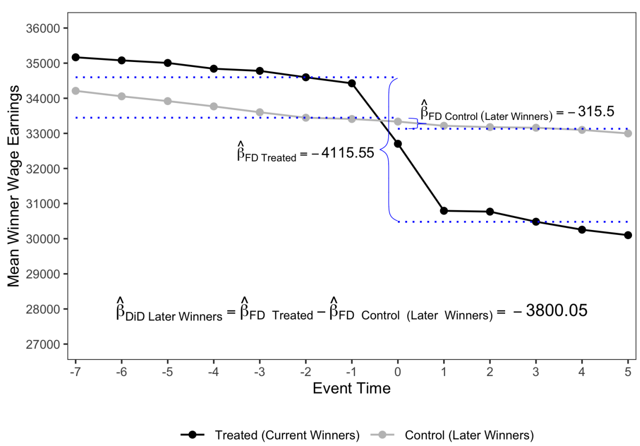
[16] As the control group includes all possible later winners at each event time, the set of calendar times represented at each event time will differ (relative to the treatment group), leading to a natural source of difference (in levels) between treatment and control groups. These differences are quite small – approximately \$1,000 between the treated and control groups. More importantly, our research design eliminates any differences in levels so this will not affect or bias our estimates.



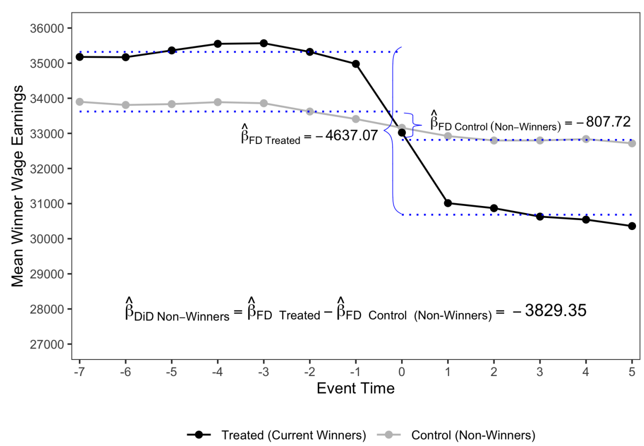

(a) First-Difference (Single Cohort)




(b) First-Difference (Pooled Across Cohorts)




(c) Difference-in-Differences (Using Later Winners as Controls)

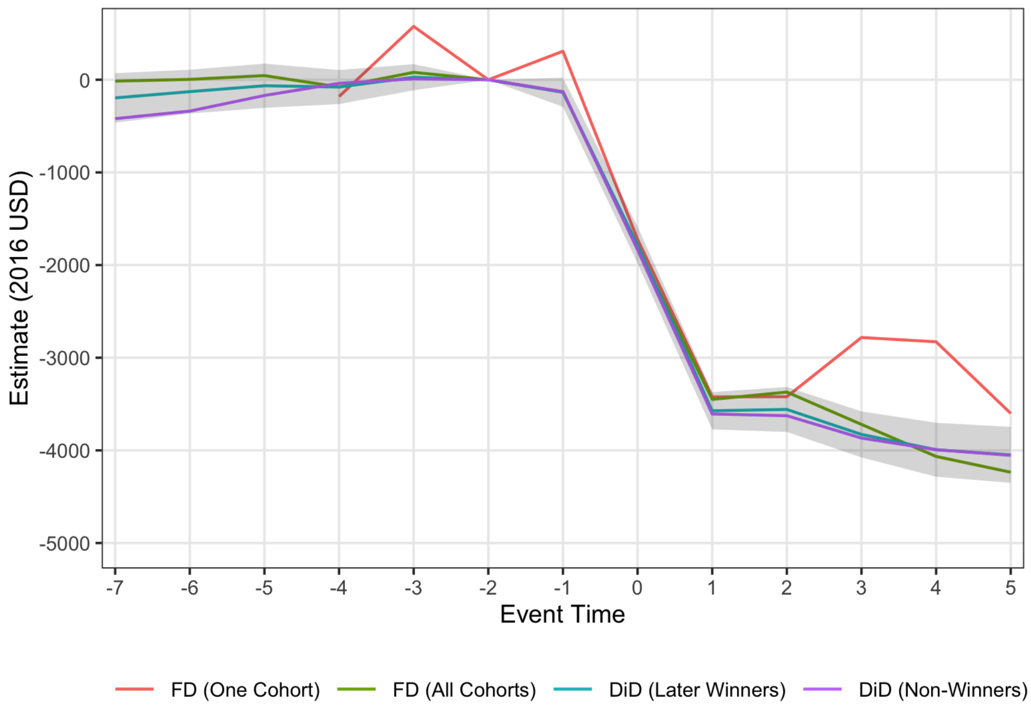



(d) Difference-in-Differences (Using Non-Winners as Controls)

Figure 3.1: Earnings paths across event time for treatment and control groups

*Notes:* This figure provides a comparison of various estimators for the effect of winning the lottery on winner wage earnings. In subfigure (a), we plot the life-cycle adjusted average wage earnings of the 2003 cohort of winners. We adjust for age effects by regressing winner wage earnings on a full set of dummies for each age. We then use the residual from this regression as our measure of earnings net of age effects, and add back in the population mean of winner wage earnings to get an intercept with the correct population average earnings level. In subfigure (b), we plot the average wage earnings of the pooled 2001-2016 cohort of winners. We adjust for age and time effects by regressing winner wage earnings on a full set of dummies for each age as well as a full set of time effects for each calendar year. In subfigure (c), we plot the cohort-weighted average of wage earnings of the treated group of winners (dark line) together with the cohort-weighted average of wage earnings of all later winners in the years before they win (lighter line). Finally, in subfigure (d), we produce an analog to subfigure (c), but only using individuals with control units who did not win the lottery during the period we consider. In each subfigure, we report the corresponding estimate of the effect of winning on winner wage earnings.

Figure 3.2: Comparison of estimators




*Notes:* In this figure, we summarize the estimates of the effect of winning the lottery on winner wage earnings at each event time corresponding to each of the four estimators discussed in Section 3.1. "FD (One Cohort)" corresponds to first-difference estimates using the 2003 cohort. "FD (All Cohorts)" corresponds to the pooled first-difference estimator using all 2001-2016 cohorts. "DiD (Later Winners)" corresponds to difference-in-differences estimates where the control group consists of all available later winners. "DiD (Non-Winners)" corresponds to difference-in-differences estimates where the control group consists of only using individuals who did not win the lottery during the period we consider. For the two difference-in-difference estimators, we take cohort-size-weighted averages for each event time. 90 percent confidence intervals are displayed, clustering on winner, for the "DiD (Later Winners)". Throughout, we use $w-2$ as the omitted event time.

**Regression model for the DiD estimator.** To implement the DiD estimator defined in expression (3.3), we use a regression to make it easier to include additional covariates and calculate standard errors. For each cohort $w$ and each event time $\ell$ we create a subsample of the treated individuals who won in period $w$ and a control group of individuals who have not won by period $w$ or $w+\ell$, whichever is greater. Using this subsample, we run the regression

$$Y_{i,t} = \alpha_1^{w,\ell} + \alpha_2^{w,\ell} 1\{i \text{ won in } w\} + \alpha_3^{w,\ell} 1\{t = w + \ell\} + \rho^{w,\ell} Z_{i,t} + u_{i,t}^{w,\ell} \tag{3.4}$$

where $Z_{i,t}$ represents the interaction term $1\{i \text{ won in } w\} \times 1\{t = w + \ell\}$. Here, $\alpha_1^{w,\ell}$ is the control group mean in the baseline year (i.e., $w-s$), $\alpha_2^{w,\ell}$ is a fixed effect for the treated individuals (cohort $w$), $\alpha_3^{w,\ell}$ is a time effect for event time $\ell$, and $\rho^{w,\ell}$ is an interaction effect and our parameter of interest as defined in expression (3.1). We estimate the model separately for each cohort $w$ and then take a weighted average of the estimates for each event time $\ell$, where the weights are determined by the cohort-size.[17]

[17] By first estimating the parameter $\rho^{w,\ell}$ separately for each cohort $w$ and then averaging these parameters across cohorts, we avoid the problems pointed out by de Chaisemartin and D'Haultfoeuille (2018), Callaway and Sant'Anna (2021), and Sun and Abraham

To control for age composition, we include a full set of dummies for each age in regression model (3.4).[18] These controls adjust for the fact that current winners are slightly older than later winners in year $w$, which is to be expected if the timing of win is as good as random. For other observables, current and later winners have very similar pre-win characteristics. This is shown in Appendix Table A.2, where we report a set of key summary statistics for the treatment-control sample. For each cohort $w$, we compute the average characteristics of individuals in the treatment and control group in the baseline year $w - s$ and report cohort-size-weighted averages of those values. In addition, we report the cohort-weighted average size of the lottery win in each group. Appendix Table A.2 shows that current and later winners (i.e., individuals in the treatment and control groups) have very similar pre-win characteristics, except current winners are slightly older. This is reassuring if one is worried that the changes in earnings over time could differ depending on the characteristics of individuals.

**Average effects of lottery winning: Graphical evidence** In Figure 3.3 we plot the estimated coefficients from regression (3.4). For each outcome and each event time $\ell$, we report a cohort-weighted average of regression coefficient $\rho^{w,\ell}$, with the baseline event time $w - 2$ normalized to zero. There is no evidence of differential trends between current and later winners in pre-win event times -7 to -1 for any of the outcomes. This is consistent with the outcomes of current winners and the later winners evolving in the same way across years in the pre-win period, providing support for the common trends assumption.

Figure 3.3 shows that various measures of labor market outcomes fall significantly for lottery winners (relative to later winners) after they win a lottery. Since our observations are at an annual frequency, it is difficult to interpret estimates in event time 0 since they are affected by the timing of the win within the year. For this reason we focus our discussion on years 1 through 5. The wage earnings of the winner fall on average by \$3,572 (approximately 10 percent) in the first year following the lottery win, and continue to decline slightly in subsequent years. Per-adult wage earnings have a similar pattern to winner wage earnings, but they decline by slightly less: \$3,234 (approximately 10 percent) in the first year after the win. Recall that per-adult wage earnings for married households is the average wage earnings of the winner and the spouse. The smaller decrease in per-adult wage earnings compared to winner wage earnings implies that the spouse of the winner decreases his or her wage earnings by a smaller amount. It is also evident that the winner is more likely to stop working, and this probability grows over time. This pattern also persists after accounting for spousal responses.

Per-adult total labor earnings also include household income from self-employment. Self-employment income declines following the lottery win, and, as a result, per-adult total labor earnings decline by more than per-adult wage earnings. The decline in self-employment income rules out substitution towards self-employment as a means of offsetting the reduction in wage earnings. By comparison, per-adult capital income

(2020), and we ensure that our event-study regressions produce positively-weighted averages of causal effects under the standard common trends assumption. To arrive at a joint variance-covariance matrix for all cohort-by-event time estimates, we estimate the model in a single, fully-interacted step.

[18] Including a full set of dummies for each age in our regression specification allows for parsimonious control for age differences between current and later winners, as well as potentially underlying life-cycle trends in earnings. In Appendix Figure B.3, we compare the results of our main specification to an non-parametric estimator due to Callaway and Sant'Anna (2021) which adjusts for age differences between current and later winners through an inverse-probability-weighted (IPW) DiD estimator. Results from the two approaches are virtually identical.

increases in the first year and then slightly declines over time. This pattern is consistent with lottery winners first expanding their savings and then gradually consuming out of their new wealth. Appendix Figure B.4 illustrates how various components of capital income such as interest payments, dividends, and other sources of capital income respond to winning.

### 3.2 IV estimates of the individual and household responses to increases in wealth

It is difficult to interpret the size of the effects reported in the previous subsection because the treatment variable captures whether a person wins but not the size of the lottery win. To get economically-interpretable estimates, we now shift to an IV model that uses variation in the timing of the lottery wins as an instrument for lottery winnings. The resulting IV estimates tell us individual and household responses per dollar of lottery winnings, which we will refer to as wealth effects.

We maintain the conventions and notation from our event-study regression model (3.4) in the prior subsection. For each cohort $w$ and each event time $\ell \geq 0$, we estimate the following IV model

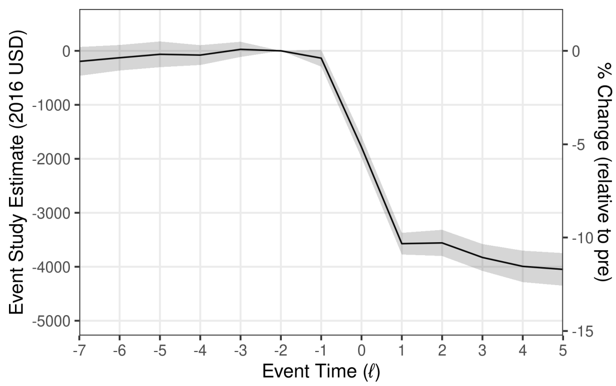
$$X_{i,t} = \mu_1^{w,\ell} + \mu_2^{w,\ell} 1\{i \text{ won in } w\} + \mu_3^{w,\ell} 1\{t = w + \ell\} + \phi^{w,\ell} Z_{i,t} + \epsilon_{i,t}^{w,\ell} \tag{3.5}$$

$$Y_{i,t} = \theta_1^{w,\ell} + \theta_2^{w,\ell} 1\{i \text{ won in } w\} + \theta_3^{w,\ell} 1\{t = w + \ell\} + \beta^{w,\ell} X_{i,t} + \nu_{i,t}^{w,\ell}. \tag{3.6}$$

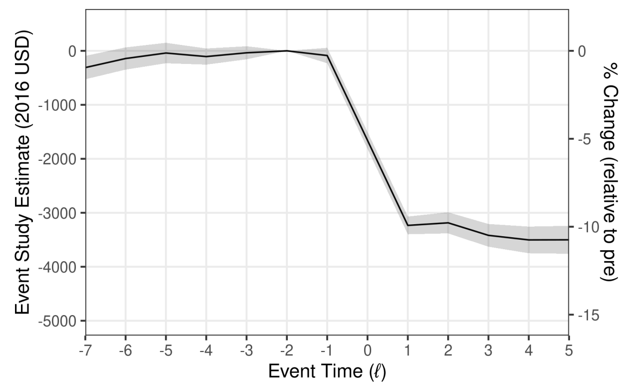
Starting with the first-stage equation (3.5), the parameters $\{\mu_1^{w,\ell}, \mu_2^{w,\ell}, \mu_3^{w,\ell}\}$ are cohort and time effects. The endogenous variable $X$ in our estimation is the lottery winnings in the win year. Thus, the first-stage coefficient $\phi^{w,\ell}$, which captures the impact of a lottery win on lottery winnings, does not change over time. The second-stage equation (3.6) relates our outcome of interest $Y$ to changes in $X$. Our parameter of interest is $\beta^{w,\ell}$, which measures the average response of outcome $Y$ to changes in $X$. The reduced form of the IV model is given by the event-study regression in (3.4).

Column 1 of Table 3.1 reports estimates of the average annual response to an additional dollar of wealth in the post-win period across several economic outcomes. The reported estimates are constructed by taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell \in \{1, ...., 5\}$, and then taking the average over event times $\ell$. Going forward, we will use a similar weighting for other estimates, so we adopt the convention of referring to them as weighted-average estimates. To ease the interpretation of responses, we scale earnings and capital income responses by 100 and employment responses by 100,000 dollars in Table 3.1.

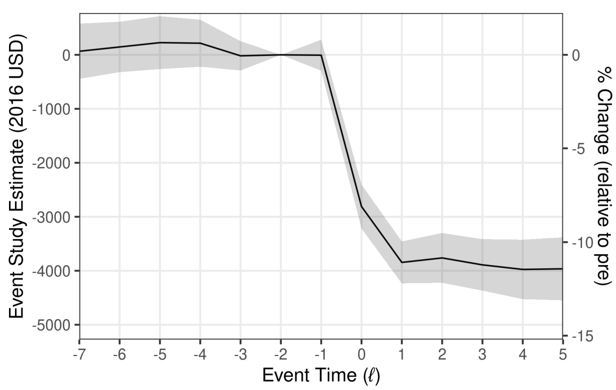
Figure 3.3: Effect of winning across outcomes



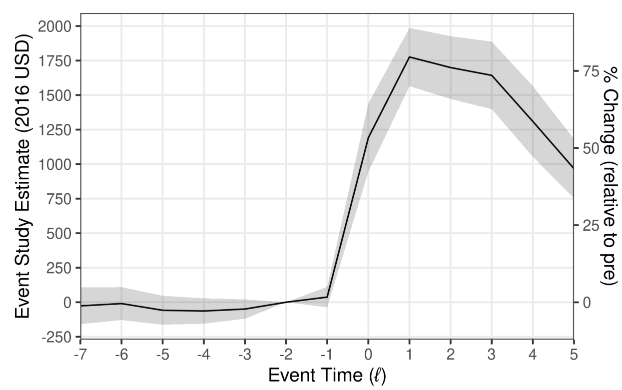

(a) Winner Wage Earnings



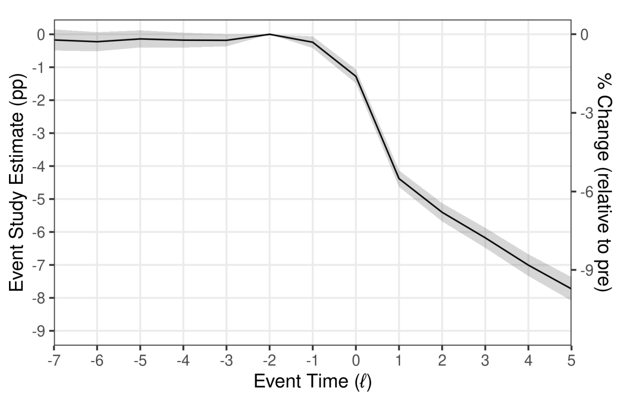

(b) Per-Adult Wage Earnings



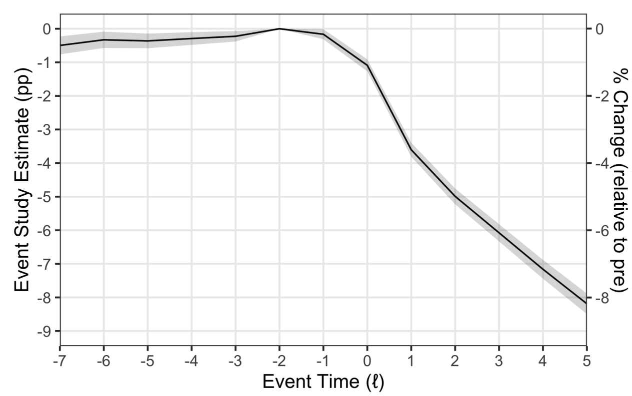

(c) Per-Adult Total Labor Earnings




(d) Per-Adult Capital Income




(e) Winner Employment




(f) Total Employment

*Notes:* This figure presents estimates of the impact of winning on six outcomes, based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. Throughout, we use $w - 2$ as the omitted event time. In addition to the cohort-size-weighted average effect in levels (left-hand axis), each subfigure also reports this average effect scaled by the mean of the outcome in omitted event time (right-hand axis) which can be interpreted as an average percentage change (relative to the baseline pre-win period) in the outcome.

Table 3.1: Wealth effects across outcomes

| Outcome | Sample | | | | |
|---|---|---|---|---|---|
| | Full Sample | Quartile 1 Pre-Win Income | Quartile 2 Pre-Win Income | Quartile 3 Pre-Win Income | Quartile 4 Pre-Win Income |
| | (1) | (2) | (3) | (4) | (5) |
| Winner Wage Earnings (per $100) | -2.2856 | -1.4003 | -2.2948 | -2.6196 | -3.0596 |
| | (0.0571) | (0.0628) | (0.0861) | (0.0935) | (0.1541) |
| Per-Adult Wage Earnings (per $100) | -2.0245 | -1.2514 | -2.0422 | -2.2590 | -2.7035 |
| | (0.0492) | (0.0589) | (0.0764) | (0.0813) | (0.1311) |
| Per-Adult Total Labor Earnings (per $100) | -2.3394 | -1.3339 | -2.2720 | -2.6450 | -3.1298 |
| | (0.0657) | (0.1051) | (0.0867) | (0.0996) | (0.1820) |
| Per-Adult Capital Income (per $100) | 0.8738 | 0.5784 | 0.7626 | 0.9658 | 0.9265 |
| | (0.0406) | (0.0540) | (0.0784) | (0.0709) | (0.0974) |
| Winner Employment (per $100,000) | -0.0368 | -0.0517 | -0.0444 | -0.0350 | -0.0231 |
| | (0.0008) | (0.0021) | (0.0019) | (0.0013) | (0.0010) |
| Total Employment (per $100,000) | -0.0361 | -0.0633 | -0.0421 | -0.0278 | -0.0196 |
| | (0.0008) | (0.0025) | (0.0017) | (0.0010) | (0.0008) |

*Notes:* This table presents estimates of the mean effect of an extra dollar of wealth on six outcomes. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, for each outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean wealth effect in the post-win period. Column 1 reports wealth effects for the full analysis sample. In columns 2 to 5, we report wealth effects for subsamples of winners falling into each quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, we scale earnings and capital income responses by $100. In the case of employment responses, we scale each estimate by $100,000.

These weighted-average estimates show that for an extra 100 dollars in wealth, winners reduce their earnings on average by 2.3 dollars in each of the five subsequent years. Per-adult total labor earnings similarly decrease by 2.3 dollars per 100 dollar of additional wealth. As both the earnings and winnings measures are per-adult, this 2.3 dollar response coincides with the mean response of total (not normalized per number of adults) household labor earnings. Capital income increases annually by 0.9 dollars per 100 dollars of additional wealth. Finally, the probability that the winner is employed decreases by 3.7 percentage points per 100,000 dollars of additional wealth. The response is similar for total employment, which also accounts for the employment of the spouse (if any).

**Heterogeneity in wealth effects across the income distribution** Columns 2 to 5 of Table 3.1 explore heterogeneity in responses across the income distribution.[19] To construct these estimates, we use the

[19] We explore how our estimates vary jointly over time and by pre-win income in Appendix Figure B.5. We report estimates for our two main measures of earnings responses: winner wage earnings and per-adult total labor earnings. Earnings responses increase over time, but not substantially so, and the magnitude of the increases is similar across income distribution.

distribution of adjusted gross income (AGI) in the baseline pre-win period to assign treated and control individuals into quartiles of pre-win income, and then estimate our IV model separately for each quartile. As one can see from these columns, the average wage earnings reduction per dollar of additional wealth is increasing in pre-win income. For example, whereas individuals in the first quartile of pre-win income reduce their own annual wage earnings by 1.4 dollars per 100 dollars of additional wealth, individuals in the fourth quartile decrease their annual wage earnings by over twice as much. This pattern does not materially change when we look at other measures of earnings responses. The lower-income households, however, are more likely to stop working – the reduction in the probability of employment for the winners in the lowest quartile is more than twice as large as that of the winners in the highest quartile. This difference increases even further after accounting for responses in spousal labor supply.

**Intensive- and extensive-margin responses.** Our finding of earnings responses that increase with pre-win income while employment responses decline with income raises a natural question: How much of the overall earnings response is attributable to the extensive margin, and does this also vary across the income distribution? To address this question, we decompose the earnings response into extensive- and intensive-margin contributions. Concretely, we take a standard statistical intensive-extensive decomposition of cross-sectional earnings effects (as in, e.g., Angrist, 2001) and adapt it to a DiD estimator (see Online Appendix D). Appendix Table A.3 shows the share of the observed earnings response that is attributable to the extensive-margin response. In aggregate, we find that the extensive margin explains roughly half of the winner wage earnings response and 40 percent of the per-adult total labor earnings response. The importance of the extensive margin, however, decreases with pre-win income. For example, whereas the extensive margin explains 58% of the observed per-adult total labor earnings response for low-income households, 36% of the response is explained by employment responses for households in the fourth quartile.

**Heterogeneity by prize size.** Lastly, we examine heterogeneity in responses by prize size. To concisely summarize results allowing for variation in prizes, we report, in Appendix Figure B.6, how average wealth effects on earnings and employment in the full analysis sample compare to a subset of smaller winners (winning $30,000 up to $300,000), a subset of larger winners (winning $300,000 to $1 million) and a subset of the largest winners (winning more than $1 million). We find that wealth effects for both earnings and employment decrease with prize size. For smaller winners, the average per-adult total earnings reduction per 100 dollars of additional wealth is in excess of 5.5 dollars, whereas for the largest winners, the reduction is less than 1 dollar per 100 dollars of additional wealth. The fact that wealth effects are declining in the lottery win is consistent with earnings being bounded below by zero. For sufficiently high winnings, the marginal effect on earnings of an extra dollar of windfall gain must be zero, which attenuate the estimated average earnings response per dollar won among large winners.

# 4 Propensities to earn and consume out of unearned income

The wealth effects that we reported in the previous section can be hard to gauge as the observed responses to windfall gains should vary across individuals depending on a number of factors, such as the age at which the individual wins, her pre-win income, her savings behavior, and the tax rates she faces. This issue motivates our analyses in this section. We begin by first studying how winning the lottery changes the unearned income that households allocate to consumption and leisure across years. Next, we estimate the share of yearly allocated unearned income that is spent on reducing labor versus increasing consumption. In other words, we estimate both the allocation of the windfall gains over time and the marginal propensities to earn (MPE) and consume (MPC) out of unearned income in a given period. For now, we focus on the key earnings and consumption responses, while we, in Section 5, analyze a more comprehensive set of labor market responses.

## 4.1 Approaches to allocate windfall gains over time.

An issue with the interpretation of the heterogeneity analysis in the previous section is that it could reflect a variety of underlying sources. For example, the heterogeneity along the income distribution could reflect the fact that responses truly depend on household income, per se. On the other hand, this same heterogeneity could reflect other factors correlated with household income, such as higher-income households also being older. For this reason, in this next section, we turn attention to studying how households allocate their winnings over time. We start with the household budget constraint. It will be convenient to write it in the following form:

$$c_t = y_t - \mathcal{T}(y_t) + \underbrace{(1+r)a_{t-1} - a_t - \mathcal{T}_a(ra_{t-1})}_{\text{unearned income} \equiv n_t}. \tag{4.1}$$

Here $c_t$, $y_t$, and $a_t$ denote consumption, labor earnings, and assets of the household in period $t$, $r$ is the interest rate, and $\mathcal{T}_a$ and $\mathcal{T}$ are taxes on capital income and labor earnings, respectively. The variable $n_t$ represents the total amount of unearned income used by the household in period $t$, or unearned income for short. Lottery winnings provide an exogenous increase in unearned income, and responses of earnings and consumption to this variation will be informative about the size of effects of unearned income (or income effects, for short).

There are two popular approaches to inferring the effect of lottery winnings on unearned income: the annuitization method and the capitalization method.[20] Under the annuitization method, one assumes that winnings are smoothed perfectly over the remaining lifetime. It is easy to show that if a $k$-year-old individual decides to smooth her lottery winning $L$ over remaining $T-k$ years of life using a post-tax interest rate $r^{\text{post-tax}}$, then unearned income must increase by

$$\frac{r^{\text{post-tax}}}{1+r^{\text{post-tax}}}\left(1-\left(\frac{1}{1+r^{\text{post-tax}}}\right)^{T-k+1}\right)^{-1} L. \tag{4.2}$$

[20] Blundell, Pistaferri, and Preston (2008), Jappelli and Pistaferri (2010), and Blundell, Low, and Preston (2013) are some examples of papers that use the annuitization method, while Stewart (1939), Saez and Zucman (2016), and Smith, Zidar, and Zwick (2020) are examples of applications of the capitalization method.

Under the capitalization method, one does not make any assumptions a priori about household behavior, but rather uses observed information about pre-tax capital income $ra_{t-1}$ and rate of return $r$ to impute ("capitalize") the value of wealth $a_{t-1}$. With this information in hand, one can then compute unearned income directly.

The two methods have strengths and weaknesses. The annuitization method has minimal data requirements and is easy to implement, but it relies on the assumption that households smooth their winnings perfectly. The capitalization method makes no assumptions about how agents smooth their winnings, but the imputation of wealth using asset returns can only be done approximately. Since a priori it is not clear which method is preferable, we estimate the average effect of winning a lottery on unearned income using both methods. For the annuitization method, we assume that all individuals live for $T = 80$ years, and set $r^{\text{post-tax}} = 2.5\%$, which is close to the average risk-free real interest rate in the U.S. for our period of observation.[21] For the capitalization method, we follow Saez and Zucman (2016) who calculate that the average pre-tax rate of return on taxable capital and business assets between 1999-2010 is approximately 5.4%. We observe capital income directly for each household and use it to impute a measure of beginning-of-period $t$ wealth, $a_{t-1}$. We calculate capital income taxes by applying the relevant marginal tax rate to each source of capital income and then summing them up. Using these measures of assets and capital income taxes we construct household-level unearned income, which we then convert to a per-adult measure for consistent comparison between single and married households.

## 4.2 Allocation of lottery winnings over time.

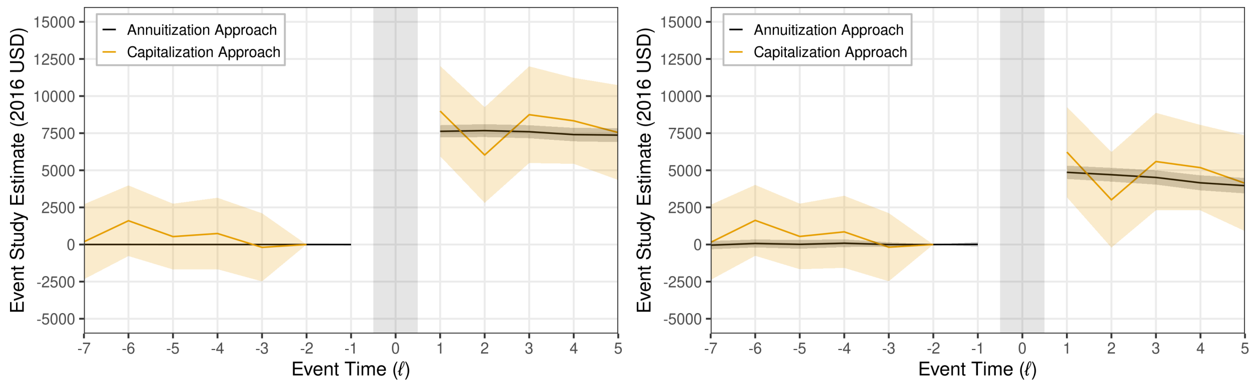
To study the allocation of lottery winnings over time, we estimate the regression model in equation (3.4) of Section 3.1, but now with the outcome variable specified as a measure of unearned income. In Figure 4.1a, we report the results of these event-study regressions for each of the two approaches outlined in the prior subsection. Under the annuitization method, the outcome variable in equation (3.4) is, by construction, zero in years prior to the win (as given by equation (4.2)) and equal to the annuitized size of the lottery win after the win. Under the capitalization method, the outcome variable in equation (3.4) is the imputed unearned income in all periods. Because the capitalization method places no assumptions on how agents smooth their winnings, the pre-win trend in this outcome variable is informative about the common trend assumption while the post-win trend is informative about the allocation of lottery winnings over time.[22]

The capitalization approach shows no evidence of differential trends in the allocation of lottery winnings in the pre-win period (no differential trends is mechanical under the annuitization approach), providing support for a common trend in unearned income between current and later winners in the absence of the lottery win. Looking in the post-win period, the key finding is that the two approaches produce remarkably similar estimates. On average in the post-win period, unearned income increases by \$7,497 per period using the annuitization method, and \$8,265 per period using the capitalization method. This similarity suggests

[21] The World Bank, DataBank (2020). Real interest rate (%) - United States. Retrieved from https://data.worldbank.org/indicator/FR.INR.RINR?locations=US.

[22] Under the capitalization method, it is not possible to get reliable estimates of imputed unearned income for $t = w - 1$ and $t = w$ because the timing of the lottery win within the year is unknown. For this reason, we exclude the corresponding event times in Figure 4.1a.

that households smooth their lottery winnings and that the simple life-cycle model and annuitization method provide a good approximation to households' savings behavior.[23] As expected, standard errors are larger under the capitalization method, likely because of the measurement error inherent with this approach.




(a) Unearned Income

(b) Implied Consumption Expenditure

Figure 4.1: Comparing across methods to measure unearned income

*Notes:* This figure presents estimates of the impact of winning on unearned income $(n_t)$ and implied consumption expenditure $(c_t)$. All estimates are based on estimating a version of equation (3.4) (as described in Section 3.1) and reporting cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. For each of unearned income and implied consumption expenditure, we plot the estimates for the annuitization and capitalization methods together to facilitate comparison. Due to our capitalization method inferring the change in wealth, the effect of winning on capital income in the win year (on-impact) directly affects the measure of wealth change in the win year as well as the prior year. For this reason, when estimating the effect of winning on $n_t$ and $c_t$ using the capitalization method, we require that not-yet treated cohorts win later than $\max\{w, w+\ell+1\}$ rather than $\max\{w, w+\ell\}$. For the same reason, we omit estimates for $\ell = -1$ and $\ell = 0$ under the capitalization method.

Next, we estimate the same event-study regression as above, but replacing the outcome with household labor earnings taxes, $\mathcal{T}(y_t)$. Together with the event-study estimates for labor earnings which we reported in Section 3, this allows us to impute the effect of winning a lottery on consumption expenditure using the budget constraint identity (4.1). We report the response of consumption expenditure, imputed under both capitalization and annuitization methods, in Figure 4.1b. Using the annuitization method, we find that consumption increases in the first post-win year by \$4,862 (approximately 17 percent). This effect declines slightly over time such that on average in the post-win period, consumption increases by \$4,413 (approximately 15 percent) per period. Estimates of the consumption response using the capitalization method are very similar, with a post-win average increase in consumption of \$5,176 (approximately 16 percent) per period.

[23] Cesarini, Lindqvist, Notowidigdo, and Östling (2017) find that earnings responses to lump-sum prizes are similar to earnings responses to installment prizes. This is a complementary approach for assessing the quality of the annuitization method as an approximation to households' savings behavior. The similarity of responses adds support to modeling lottery winners as behaving as predicted by textbook models of consumption smoothing.

### 4.3 Estimates of marginal propensities to earn and consume

Using the IV model introduced in Section 3.2, defined by equations (3.5) and (3.6), we can estimate how an extra dollar in unearned income translates into an increase in consumption (MPC), a decrease in earnings (MPE), and a change in labor earnings taxes (MPT). This is done by 2SLS estimation of the two-equation system with the endogenous variable being the unearned income in a given period, and the outcome variable being labor earnings, consumption, or labor earnings taxes. Before discussing the results, it is useful to note that the MPC and the MPE must satisfy the accounting identity,

$$\text{MPC} - \text{MPE} = 1 - \text{MPT},$$

and therefore MPC - MPE may exceed one.

**Main estimates and heterogeneity across the income distribution.** Table 4.1 presents the results both for the full sample and separately for each income quartile, similarly to Table 3.1. The estimates here are reported using the annuitization method; point estimates are very similar when we use the capitalization method and so we relegate them to the appendix (see Appendix Table A.4). Table 4.1 shows that labor earnings responses to a change in unearned income (i.e., MPEs) are quite large. An extra dollar in unearned income leads to a 52 cent reduction in labor earnings. Furthermore, there is substantial heterogeneity in MPEs across the income distribution. The MPE of households in the lowest quartile is -0.31 while the MPE of those in the highest quartile is -0.67.

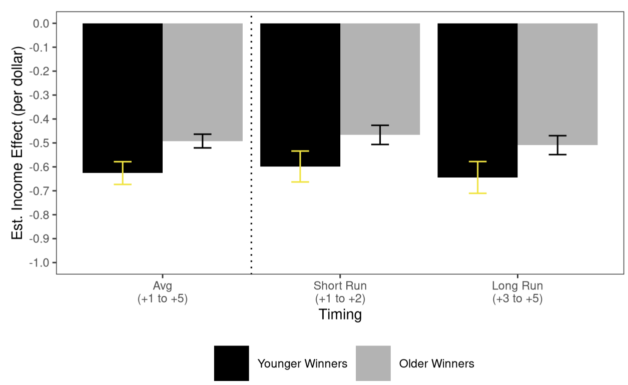
Table 4.1: IV estimates of the effect of exogenous change in unearned income

| | **Sample** | | | | |
|---|---|---|---|---|---|
| Outcome | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 2 Pre-Win Income (3) | Quartile 3 Pre-Win Income (4) | Quartile 4 Pre-Win Income (5) |
| Per-Adult Total Labor Earnings | -0.5227 | -0.3080 | -0.5204 | -0.5893 | -0.6735 |
| | (0.0146) | (0.0240) | (0.0197) | (0.0221) | (0.0389) |
| Per-Adult Labor Earnings Taxes | -0.1063 | -0.0395 | -0.0700 | -0.1254 | -0.1725 |
| | (0.0051) | (0.0063) | (0.0075) | (0.0063) | (0.0155) |
| Implied Consumption Expenditure | 0.5836 | 0.7315 | 0.5496 | 0.5361 | 0.4990 |
| | (0.0198) | (0.0417) | (0.0374) | (0.0339) | (0.0361) |

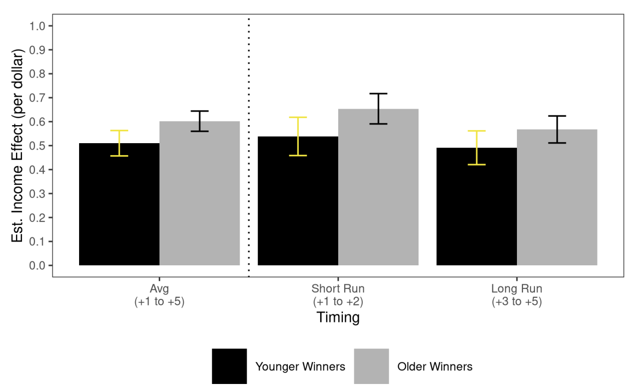
*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income $(n_{i,t})$ as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 to 5, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into each quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner.

The imputed consumption responses are of similar magnitude to earnings responses, and also display heterogeneity across the income distribution. Imputed consumption increases, on average, by 58 cents in response to an extra dollar in unearned income. This response is largest for households in the lowest pre-win income quartile and it declines with pre-win income. Finally, the reduction in earnings leads to a reduction in labor earnings taxes of about 11 cents per extra dollar of unearned income.

**Heterogeneity by age of the winner.** Consistent with the life-cycle hypothesis, Appendix Figure B.7 shows that observed earnings responses to windfall gains increase with age. The annuitization method maps a windfall gain into a per-period flow of additional unearned income. Under the annuitization method, we can expect the effects of an additional dollar of unearned income to not vary by age. Figure 4.2 illustrates how an extra dollar in unearned income translates into changes in earnings and consumption for younger and older winners. The similarity of the effects between these two groups provides an additional piece of support for the annuitization method beyond our earlier comparison with the capitalization approach shown in Figure 4.1.




(a) Per-Adult Total Labor Earnings




(b) Consumption Expenditure

Figure 4.2: Effects of exogenous change in unearned income by age of winner

*Notes:* This figure presents estimates of the mean effect of an extra dollar of unearned income on earnings and consumption expenditure. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income $(n_{i,t})$ as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for all post-win event times $\{1, 2, 3, 4, 5\}$ ("Avg (+1 to +5)"), a shorter-run set of post-win event times $\{1, 2\}$ ("Short Run (+1 to +2)"), and a longer-run set of post-win event times $\{3, 4, 5\}$ ("Long Run (+3 to +5)").. For each temporal average, we report effects of unearned income for the subsample of younger winners (age 30 - 46) and older winners (age 47 - 64). 90 percent confidence intervals are displayed, clustering on winner.

**Heterogeneity in responses by gender and marital status.** We now exploit the richness of our data to study additional dimensions of heterogeneity in earnings responses to an exogenous change in unearned income, beginning with gender. Appendix Figure B.8 shows the estimated coefficients from the earnings regression (3.4) when we split the sample by the gender of the winner. We find no evidence of differential trends between current and later winners in pre-win event times. The event study shows a sizable, swift, and persistent change in earnings for both male and female winners. However, the earnings of male winners tend to decrease more compared to female winners.

To study gender differences in the earnings response to an extra dollar of unearned income, we estimate the IV model described in Section 3.2 separately by gender of the winner. In Appendix Table A.5 we report these estimates. On average we find that labor earnings responses are larger for males. An extra dollar of unearned income leads to a 60 cent reduction in total labor earnings for male winners, whereas female winners reduce their total labor earnings by 38 cent. However, taking into account that earnings of females in the absence of winning are substantially lower on average, we find that the relative earnings responses are similar across gender. Just as in Table 4.1, we continue to find substantial heterogeneity in MPEs across the income distribution: the earnings reduction due to an extra dollar of unearned income is increasing in pre-win income regardless of the winner's gender.

We next turn to possible differences in earnings responses by marital status. Appendix Figure B.8 shows the estimated coefficients from the earnings regression (3.4) when we split the sample by the marital status of the winner. As before, we find no evidence of differential trends between current and later winners in pre-win event times. The event studies show a sharp and persistent change in earnings for both single and married winners. To interpret the magnitudes of these event-study estimates, we estimate the IV model described in Section 3.2. As shown in Appendix Table A.6, we find that married winners reduce their earnings by more than single winners for every additional dollar of unearned income. For example, whereas married individuals reduce their own annual wage earnings by 64 cents for an extra dollar of unearned income on average, singles decrease their annual wage earnings by 46 cents for an extra dollar of unearned income. However, taking into account that earnings of singles in the absence of winning are around 20 percent lower on average, we find that the relative earnings responses are similar across single and married winners. Irrespective of the marital status of the winner, the earnings reduction due to an extra dollar of unearned income is increasing in pre-win income.

**Heterogeneity in responses between winner and spouse.** Up to this point, our analysis has focused on differential responses across households. We now look across members within a household to analyze differences in earnings responses between winners and their spouses. We focus on married couples and on wage earnings (which can be separately attributed to each spouse). We begin by estimating regression (3.4) separately for wage earnings of the winner and his or her spouse. As shown by the event studies in Appendix Figure B.9, we find no evidence of differential trends between current and later winners in the years before the lottery win. This finding holds whether we look at winners or their spouses. The event studies show a sharp and persistent decline in wage earnings in response to winning, with winner wage earnings decline by more than spousal wage earnings.

We examine differences in earnings responses between winners and their spouses by estimating the IV model described in Section 3.2 separately for each household member. The estimated wage earnings responses are reported in Appendix Table A.7. For an additional dollar of unearned income, winners decrease their wage earnings by 64 cents on average, whereas the non-winning spouse reduces his wage earnings by only a third of that amount.

As discussed in Cesarini, Lindqvist, Notowidigdo, and Östling (2017), differential responses of winners and their non-winning spouses, such as those reported in Appendix Table A.7, may have implications for models of household behavior.[24] In particular, such differential responses are inconsistent with the unitary model of the household and the income pooling hypothesis provided that winning is randomly assigned within households.[25] However, as shown in Appendix Table A.8, we find that the winner within a household is systematically different from the non-winner.[26] This implies that differences in earnings responses between spouses may be due to either non-random assignment of winning within household or a violation of the unitary model, which prevents us from drawing any further conclusions.

# 5 Broader analysis of responses to changes in wealth and income

So far, the analysis has been narrowly focused on key earnings responses to exogenous changes in household wealth and unearned income. This focus is in line with the canonical models of labor supply where the individual's problem is restricted to choosing hours of work or earnings given a wage rate. In reality, however, individuals could be responding along several other margins. We now examine margins of adjustment other than earnings and, in the course of doing so, address a number of important related economic questions: Does a rise in unearned income make individuals more likely to choose jobs with lower wages in exchange for more favorable non-wage attributes? Are wealth effects important for retirement decisions and the design of public pension systems? Does a lack of wealth constitute an important barrier to entrepreneurship? To what extent does low wealth prevent households from moving to a different neighborhood? Are certain types of households more likely to move than others? Are the neighborhoods that people move to characterized by better local labor markets or better neighborhood quality (as typically measured)? And finally, how does an increase in household wealth affect the incentives to marry or divorce?

To answer these questions, we exploit variation in household wealth and unearned income from winning the lottery. For each question, we discuss how our findings relate and contribute to the current evidence base, which is either by using a change in wealth and unearned income that plausibly meets the requirements of being exogenous, unanticipated and idiosyncratic, or by obtaining the first evidence for the U.S., or by achieving sufficiently precise estimates to draw firm conclusions about signs or magnitudes. In cases where existing work emphasizes the role of fixed costs of adjustment or financial constraints, we present estimates of wealth effects. Conversely, in cases where existing work primarily focuses on changes in unearned income,

[24] For a recent overview of models of household behavior see, e.g., Chiappori and Mazzocco (2017).

[25] For earlier studies of the income pooling hypothesis see, e.g., Schultz (1990), Bourguignon, Browning, Chiappori, and Lechene (1993), Fortin and Lacroix (1997) and Attanasio and Lechene (2002).

[26] Cesarini, Lindqvist, Notowidigdo, and Östling (2017) also reject random assignment of winning within households even when the sample is restricted to couples in which both spouses participated in the winning draw.

we present estimates of income effects.

### 5.1 Labor market dynamics

In this section, we study the effects of changes in wealth or unearned income on dynamics in the labor market. We begin by estimating retirement responses, and then turn to studying the effects on the propensity to start a business and job mobility.

**Take-up of retirement benefits and labor market exit.** Assuming leisure is a normal good, economic theory predicts that an unanticipated increase in wealth or unearned income accelerates retirement and leads to a reduction in lifetime labor supply. Understanding the magnitude of this effect is key to assessing the effects of public policy such as reforms to the public pension system. For example, policies that increase the minimum retirement age or reduce the replacement rate of earnings within a pension system (either to encourage labor force participation or improve fiscal sustainability) result in direct changes to household wealth that loom largest for older individuals at the margin of retirement.

In our analysis we focus on winners aged 62 - 64 and follow two distinct approaches to define entry into retirement. The first approach is centered around the take up of Old-Age and Survivors Insurance (OASI) benefits, commonly known as Social Security retirement benefits. Depending on year of birth, Americans are eligible for full retirement benefits as early as age 65 or as late as age 67. However, individuals have the option to claim retirement benefits earlier than their full retirement age (but not earlier than age 62) at the cost of a lower actuarial value of the benefit stream.[27] Early retirement is common among Americans even though actuarial calculations suggest that there are strong financial disincentives to drawing benefits before the full retirement age. One possible explanation for this behavior is liquidity constraints: individuals may decide to claim benefits before the full retirement age due to a lack of alternative funds to finance early retirement. Winning the lottery eases these financial constraints, so the effect of winning on the timing of retirement benefit receipt is ambiguous. For this reason, our second approach to defining entry into retirement is based on the level of earnings rather than the receipt of benefits. Specifically, we use consecutive years with zero total labor earnings as a proxy for an exit from the labor force in our analysis of retirement responses to lottery winnings.

In Appendix Figure B.11a and B.11b we plot the estimated coefficients from regression (3.4), where the dependent variables are binary indicators for the receipt of OASI benefits and labor force exit (of one, two, and five-year duration), respectively.[28] There is no evidence of differential trends in these outcomes between current and later winners aged 62 - 64 in pre-win event times.[29] We find a small positive effect of winning the lottery on the propensity to receive OASI benefits in the first year post-win, but this effect does not persist

[27] Claiming retirement benefits at any point after the full retirement age yields a benefit stream that is intended to hold actuarial value fixed.

[28] When analyzing receipt of OASI benefits, we condition on having no Social Security retirement benefits pre-win. Results that do not condition on no receipt of Social Security retirement benefits pre-win are virtually identical, in part because the initiation of Social Security retirement benefit receipt in this older population is essentially always an absorbing state.

[29] Our design, which uses later winners as a control group, necessarily means that holding age fixed, earlier winners can leave the sample through mortality whereas later winners cannot until they win. For older winners, where this might be meaningful, we restrict the sample to the living and include individual fixed effects to account for compositional differences.

over time. In contrast, we find significant positive effects on the propensity to exit the labor force, and these effects increase over time.

To quantify the effects of additional wealth on retirement behavior, we estimate the IV model from Section 3.2. Table 5.1 reports the resulting IV estimates which inform us about the propensity to retire in response to an extra 100,000 dollars in wealth.[30] We find a small positive, but statistically insignificant, effect of additional wealth on the propensity to claim OASI benefits. For an extra 100,000 dollars in wealth, the propensity to claim OASI benefits increases by around 1.1 percentage points on average. In contrast, the propensity to leave the labor force for at least one, two, or five consecutive years increases by around 5 percentage points per $100,000 of additional wealth on average. These average effects on exit correspond to an 11-14 percent increase in the propensity to leave the labor force.

An extensive body of prior work has studied the wealth effect on retirement by focusing on changes in wealth stemming from inheritances (Joulfaian and Wilhelm, 1994; Brown, Coile, and Weisbenner, 2010), stock market and house price booms and busts (Sevak, 2002; Coronado and Perozek, 2003; Coile and Levine, 2006; Hurd, Reti, and Rohwedder, 2009; Gustman, Steinmeier, and Tabatabai, 2010; Zhao and Burge, 2017; Begley and Chan, 2018; Disney and Gathergood, 2018), and changes in public pension system design (Atalay and Barrett, 2015; Gelber, Isen, and Song, 2016). This literature faces two main challenges. The first challenge is that it may be difficult to empirically isolate variation in wealth that is unanticipated, and doing so is important because individuals adjust their behavior prior to an expected change in wealth. Ignoring this behavioral response and treating all wealth changes as unexpected will tend to *understate* the true effect of wealth on retirement behavior. The second challenge is the endogeneity problem arising from the possible contemporaneous correlation between determinants of labor market conditions and determinants of wealth. Studies leveraging natural experiments that alter household wealth through changes in pension program design can sometimes overcome these two challenges, but their impact on retirement responses may be hard to gauge and economically interpret, as it should vary with individual characteristics as well as the precise nature of the shock, such as whether it is persistent or transitory, and idiosyncratic or market-wide.

In light of these challenges, it is not surprising that the prior literature has found contrasting results for the effect of wealth on retirement decisions, with estimates varying in magnitude and even sign. We summarize these results in Panel A of Appendix Table A.9. We include among these results wealth effect estimates from Cesarini, Lindqvist, Notowidigdo, and Östling (2017) as an important prior use of older lottery winners to study retirement in the Swedish context. The magnitude of our wealth effect estimates on labor market exit are similar to those reported in Joulfaian and Wilhelm (1994), Sevak (2002), Gelber, Isen, and Song (2016), Cesarini, Lindqvist, Notowidigdo, and Östling (2017), and Disney and Gathergood (2018). However, the precision of our estimates reduces the uncertainty around the magnitude of these effects for the U.S., allowing us to rule out effects on retirement much smaller than, much larger than, or of a different sign from ours (as in, e.g., Hurd, Reti, and Rohwedder, 2009, Brown, Coile, and Weisbenner, 2010, and Zhao and Burge, 2017).[31]

[30]Effects of unearned income on retirement behavior are reported in Appendix Table A.10.

[31]We omit Coronado and Perozek (2003), Coile and Levine (2006), Gustman, Steinmeier, and Tabatabai (2010), Atalay and Barrett (2015), and Begley and Chan (2018) from Appendix Table A.9 as they do not report wealth effects for propensity to retire directly and do not report enough information with which to calculate them. Broadly speaking, Coronado and Perozek (2003), Coile and Levine (2006), and Gustman, Steinmeier, and Tabatabai (2010) all leverage shocks to stock values and conclude, in contrast to our findings, that wealth effects are small, likely indistinguishable from zero. Atalay and Barrett (2015) and Begley and Chan (2018)

Table 5.1: Effects of wealth on take-up of retirement benefits and labor market exit

| Outcome | Value | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 4 Pre-Win Income (3) |
|---|---|---|---|---|
| | | **Take-Up of Retirement Benefits** | | |
| Claiming OASI Benefits | *Estimate* | 0.0114 | 0.0224 | 0.0077 |
| | *Standard Error* | (0.0041) | (0.0122) | (0.0077) |
| | *Counterfactual Mean* | 0.77 | 0.74 | 0.73 |
| | *Percentage Change* | 1.5 | 3.0 | 1.1 |
| | | **Labor Market Exit** | | |
| One-Year Exit | *Estimate* | 0.0489 | 0.0361 | 0.0392 |
| | *Standard Error* | (0.0053) | (0.0095) | (0.0103) |
| | *Counterfactual Mean* | 0.43 | 0.63 | 0.33 |
| | *Percentage Change* | 11.3 | 5.8 | 11.7 |
| Two-Year Exit | *Estimate* | 0.0536 | 0.0477 | 0.0457 |
| | *Standard Error* | (0.0058) | (0.0106) | (0.0111) |
| | *Counterfactual Mean* | 0.40 | 0.58 | 0.30 |
| | *Percentage Change* | 13.4 | 8.3 | 15.0 |
| Five-Year Exit | *Estimate* | 0.0490 | 0.0599 | 0.0195 |
| | *Standard Error* | (0.0098) | (0.0265) | (0.0170) |
| | *Counterfactual Mean* | 0.35 | 0.48 | 0.31 |
| | *Percentage Change* | 14.0 | 12.6 | 6.4 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of wealth on take up of retirement benefits and labor market exit for winners aged 62-64. The dependent variables are binary indicators for the receipt of OASI benefits and labor force exit respectively. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, for each outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean wealth effect in the post-win period. Column 1 reports mean effects of an extra dollar of wealth for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of wealth for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by \$100,000. The counterfactual means correspond to the fraction of winners that would have taken up retirement benefits or exited the labor market in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per \$100,000 in percentage change terms relative to the counterfactual mean.

**Entrepreneurship and self-employment.** A vast empirical literature has documented the cross-sectional relationship between entrepreneurship and wealth.[32] Theoretical explanations for this cross-sectional relationship are organized around three broad classes of models, with some viewing entrepreneurship as an amenity (Hurst and Pugsley, 2015), some highlighting the presence of important financial barriers to entrepreneurship (Evans and Jovanovic, 1989; Cagetti and Nardi, 2006), and some positing a spurious relationship driven by unobservables such as risk aversion (Hurst and Lusardi, 2004). Precisely which of the three classes of model

study increases in pension eligibility age and decreases in housing wealth, respectively, and find that these negative wealth shocks decrease propensity to retire, consistent with leisure being a normal good.

[32] See, for example, Evans and Jovanovic (1989), Evans and Leighton (1989), Fairlie (1999), Quadrini (1999), Hurst and Lusardi (2004), Nykvist (2008), and Fairlie and Krashinsky (2012).

governs entry into self-employment is key to informing policymakers of the likely consequences of policy reforms such as tax breaks, grants, training programs, and preferential loan and bankruptcy terms for small businesses.[33]

We contribute to this literature by studying the role of a plausibly-exogenous idiosyncratic shock to household wealth on the transition from paid employment to either low- or high-paying self-employment in the U.S. We distinguish between these two types of self-employment by defining two binary outcomes. One outcome indicates receiving income from self-employment of $15,000 or less; this variable serves as a proxy for starting a low-paying business. The other outcome indicates self-employment income in excess of $15,000.[34] Appendix Figure B.11c shows the estimated coefficients from regression (3.4) with the two indicators as dependent variables. We find no evidence of differential trends for either of these outcomes between current and later winners in pre-win event times. The event study shows that the propensity to start a low-paying business increases significantly in the years following the lottery, whereas winning the lottery appears to have no effect on the propensity to start a business with annual profits of at least $15,000.

To quantify the role of wealth in business creation, we now shift to the estimates of the IV model described in Section 3.2. Table 5.2 reports the effects of an additional $100,000 in wealth on the propensity to start a business.[35] On average we find an increase of around 0.5 percentage points (a change of approximately 14 percent) in the probability of transitioning from employee to self-employed with income of $15,000 or less. In contrast, we find no effect on the probability to start a business associated with annual profits of more than $15,000.

Taken together, these two findings suggest several conclusions for the literature on entrepreneurship. First, given the plausibly-exogenous nature of our wealth shock, our finding of economically-meaningful transition to self-employment (albeit with low earnings) is at odds with models suggesting that unobservables spuriously drive the empirical relationship between wealth and self-employment. Second, we observe a lack of transition into higher-paying self-employment, which is consistent with both the liquidity and amenity classes of models. The first finding is qualitatively consistent with much past work which has studied changes in entrepreneurship in response to changes in wealth (Panel B of Appendix Table A.9). Nonetheless, our estimates are generally smaller in magnitude than those in comparable past studies.[36] For example, when Holtz-Eakin, Joulfaian, and Rosen (1994) study transitions into entrepreneurship, the authors find overall transition rates substantially larger than ours (3.3 percentage points). The second finding is relatively novel, although in the case Holtz-Eakin, Joulfaian, and Rosen (1994), the authors consider an outcome similar to our high-paying transition outcome and, again, find a larger estimate (1.1 percentage points).

As with our study of retirement responses, we note here the potential advantage of lottery shocks (relative to, for instance, inheritances or asset price shocks used in some prior work) in generating exogenous variation

[33] See OECD (2010) for an overview of policies across several countries.

[34] The $15,000 threshold approximately corresponds to the amount an individual would earn if working full time at U.S. federal minimum wage.

[35] Effects of unearned income on the propensity to become self-employed are reported in Appendix Table A.11.

[36] Given our definition of outcomes (defined by self-employment income thresholds), the comparison between our estimates and prior work found in Appendix Table A.9 can only be done approximately. Three important past studies excluded from the table are Blanchflower and Oswald (1998), Taylor (2001), and Fairlie and Krashinsky (2012). These studies do not report wealth effects for transition to self-employment and do not report enough information with which to calculate them. Nonetheless, discussions in all three papers suggest responses to modest changes in wealth (much less than $100,000) that are much larger than our estimates.

in wealth. For example, Hurst and Lusardi (2004) find, using U.S. data from the Panel Study of Income Dynamics, that future inheritances predict current propensity to form a business which should not be the case if future inheritances are an exogenous source of change in household wealth.[37] Beyond this advantage, our estimates also allow for richer insights as we can look at entrepreneurship responses across the income distribution. In particular, the relative homogeneity in wealth effects across the income distribution (a proxy for liquidity constraints prior to winning the lottery) is suggestive that the marginal entrepreneur may be seeking non-pecuniary benefits from entrepreneurship at the cost of pecuniary benefits, in line with amenity models (Hurst and Pugsley, 2011; Hurst and Pugsley, 2015).

Table 5.2: Effects of wealth on entrepreneurship and self-employment

| Outcome | Value | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 4 Pre-Win Income (3) |
|---|---|---|---|---|
| Transition to Low-Paying SE | *Estimate* | 0.0047 | 0.0037 | 0.0053 |
| | *Standard Error* | (0.0003) | (0.0010) | (0.0004) |
| | *Counterfactual Mean* | 0.03 | 0.05 | 0.03 |
| | *Percentage Change* | 13.7 | 6.8 | 19.4 |
| Transition to High-Paying SE | *Estimate* | -0.0002 | -0.0005 | -0.0005 |
| | *Standard Error* | (0.0002) | (0.0005) | (0.0003) |
| | *Counterfactual Mean* | 0.01 | 0.01 | 0.02 |
| | *Percentage Change* | -1.3 | -3.2 | -3.0 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of wealth on the propensity to start a business associated with annual profits of $15,000 or less (low-paying SE), or a business with profits of more than $15,000 (high-paying SE). The estimation sample is restricted to winners and not-yet winners in paid-employment at event time $w - 2$. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, for each outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean wealth effect in the post-win period. Column 1 reports mean effects of an extra dollar of wealth for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of wealth for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by $100,000. The counterfactual means correspond to the fraction of employed winners that would have received income from self-employment in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per $100,000 in percentage change terms relative to the counterfactual mean.

**Job mobility.** Non-wage job characteristics are important determinants of job mobility. In addition to their wage, jobs differ in their non-wage attributes, such as the level of fringe benefits, flexibility of work schedules, the type of tasks performed, and the amount of effort required. Workers treat these as consumption goods, and thus face a trade off between the wage- and non-wage attributes in their labor supply decision.[38] Employers with undesirable working conditions then must pay a compensating wage premium in order to attract labor, whereas employers that offer favorable job amenities can attract labor at lower than average wages. Differences in wages between otherwise identical workers then reflect differences in the value of

[37] As a reminder, in our context, the analogous issue would arise if future lottery winning predicted current entrepreneurship. Practically, this issue would appear as differential trends in entrepreneurship between current and later winners. As mentioned earlier, we find no evidence for such differential trends in Appendix Figure B.11c.

[38] For example, survey and experimental evidence shows that workers are willing to take lower pay in exchange for more job flexibility, e.g., Hamermesh (1999), Eriksson and Kristensen (2014), Mas and Pallais (2017), Wiswall and Zafar (2017), Maestas, Mullen, Powell, von Wachter, and Wenger (2018), Katz and Krueger (2019) and Chen, Ding, List, and Mogstad (2020).

non-wage characteristics between different jobs in a competitive labor market (Rosen, 1986).

Assuming that job amenities are a normal good, we would expect an increase in wealth or unearned income to induce workers to move to employers that pay lower wages on average, but offer more favorable job amenities in exchange. We explore this channel by studying the job mobility decisions of individuals who were employed prior to winning the lottery and continue to be employees post-win. To study the effect of winning on the job mobility rate, we define an indicator $J_{i,t}$ equal to 1 if household $i$'s employer in year $t$ is different from the employer in year $w-2$, i.e. two years prior to winning the lottery, and 0 otherwise. In order to explore the effect of winning on the direction of job moves, we rank employers according to the mean wage paid to their employees.[39] By definition, we can then decompose the total probability of changing jobs as follows:

$$\underbrace{\mathbb{P}\left[J_{i,t}=1\right]}_{\text{total prob. of job change}} = \underbrace{\mathbb{P}\left[J_{i,t}=1 \,\cap\, \Psi_{i,t} \geq \Psi_{i,w-2}\right]}_{\text{prob. of upward job move}} + \underbrace{\mathbb{P}\left[J_{i,t}=1 \,\cap\, \Psi_{i,t} < \Psi_{i,w-2}\right]}_{\text{prob. of downward job move}}, \tag{5.1}$$

where $\Psi_{i,t}$ denotes the wage rank of household $i$'s employer in year $t$.

Appendix Figure B.11d shows the estimated coefficients from regression (3.4) for the total moving probability and each of its components. We find no evidence of differential trends in all of these outcomes between current and later winners in pre-win event times.[40] The event study shows a small decline in the probability of having changed job by the first year post-win, but this effect does not persist over time. Interestingly, we find an increase in the probability of a downward job move in the first year post-win, whereas the probability of an upward job move declines. The gap between the estimated effects is growing over time, which is consistent with frictional labor markets in which moving to the preferred job takes time.

To study the effects of changes in unearned income, we estimate the IV model described in Section 3.2, only now using unearned income as the endogenous variable. In Table 5.3 we report the estimated effects of an additional $10,000 in unearned income on both the frequency and the direction of job moves.[41] On average and across the income distribution, we do not find an effect on the frequency of job moves, consistent with the prior finding of Cesarini, Lindqvist, Notowidigdo, and Östling (2017). However, behind this overall job move propensity lies important heterogeneity in the *direction* of job mobility. Specifically, we find a significant increase of around 2 percentage points (roughly a 10 percent change) in the probability of moving to an employer that pays lower wages on average, whereas the probability of moving to an employer that pays higher wages on average decreases by around the same amount. This finding is suggestive that job amenities are a normal good, and that individuals with exogenously higher levels of unearned income are systematically sorting into jobs with better amenities at the expense of lower wages, consistent with earlier work using survey data for the UK (Haywood, 2016). Looking across the income distribution, we find that

[39] Specifically, we calculate a time-invariant measure of firm-level mean wages per employee by taking raw mean wage earnings per employee and removing aggregate time effects to construct residual mean wage earnings for each firm in each calendar year. We then take an average of the residuals for each firm across calendar years. Our results barely change if we do not adjust for aggregate time effects in the construction of time-invariant firm-level mean wages.

[40] By construction of the outcome variables, $\mathbb{P}\left[J_{i,w-2}=1\right]=0$ for both current and later winners. To allow for potentially different annual job-to-job transition rates between current and later winners, we then normalize the effect in event time $w-1$ to be zero in these regressions.

[41] Wealth effects on job mobility are reported in Appendix Table A.12.

high-earning individuals respond to changes in unearned income by reducing their efforts to move upward rather than increasingly moving downward.

Table 5.3: Effects of unearned income on job mobility

| Outcome | Value | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 4 Pre-Win Income (3) |
|---|---|---|---|---|
| Any Job-to-Job Move | *Estimate* | 0.0010 | 0.0081 | -0.0094 |
| | *Standard Error* | (0.0042) | (0.0127) | (0.0066) |
| | *Counterfactual Mean* | 0.46 | 0.67 | 0.36 |
| | *Percentage Change* | 0.2 | 1.2 | -2.6 |
| Downward Move | *Estimate* | 0.0222 | 0.0305 | 0.0025 |
| | *Standard Error* | (0.0039) | (0.0131) | (0.0058) |
| | *Counterfactual Mean* | 0.23 | 0.31 | 0.19 |
| | *Percentage Change* | 9.8 | 9.9 | 1.3 |
| Upward Move | *Estimate* | -0.0212 | -0.0224 | -0.0118 |
| | *Standard Error* | (0.0037) | (0.0127) | (0.0052) |
| | *Counterfactual Mean* | 0.24 | 0.36 | 0.17 |
| | *Percentage Change* | -9.0 | -6.2 | -6.9 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on the frequency and direction of job-to-job moves. The estimation sample is restricted to winners and not-yet winners in paid-employment pre- and post-win. Outcomes are defined as binary and equal to 1 if the firm is either different from, or higher or lower ranked than the firm prior to winning the lottery. Firms are ranked by the mean wage paid to its employees. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by $10,000. The counterfactual means correspond to the job mobility rates that would have occurred in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per $10,000 in percentage change terms relative to the counterfactual mean.

## 5.2 Geographic mobility

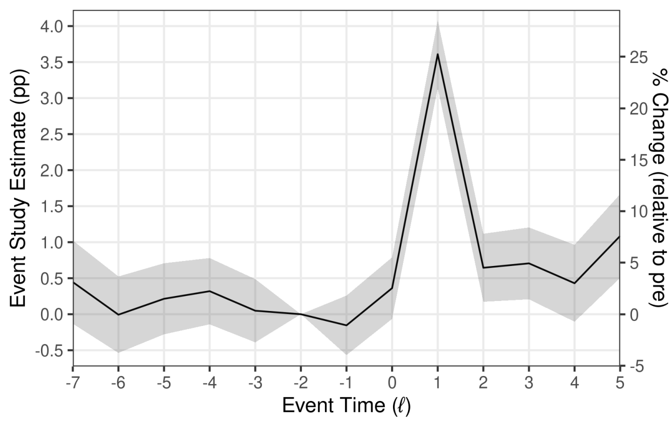
In the prior section we documented a variety of labor market moves in response to an exogenous change in wealth or unearned income, whether to a different job, a different type of employment, or out of the labor market entirely. Another type of move that households may make in response to winning the lottery is a move to a different geography. Such geographic mobility may arise for a variety of reasons. For example, a large body of evidence has documented substantial and persistent geographic disparities in local labor market outcomes, living standards, and intergenerational mobility.[42] These descriptive findings raise the question of why more households do not move in order to improve the local area to which they and their families are exposed. This pattern of immobility even arises when relatively short-distance moves would result in greater

[42] See for example Wilson (1987), Rosenbaum (1995), Ludwig, Liebman, Kling, Duncan, Katz, Kessler, and Sanbonmatsu (2008); Ludwig, Duncan, Gennetian, Katz, Kessler, Kling, and Sanbonmatsu (2013), Chetty, Hendren, and Katz (2016a), De La Roca and Puga (2016), Chetty and Hendren (2018), Chyn (2018) and Aliprantis and Richter (2020).

access to opportunity (Chetty, Friedman, Hendren, Jones, and Porter, 2020). With this as motivation, we now explore the effects of changes in wealth on geographic mobility and neighborhood choice.

**Effect of wealth on geographic mobility.** To measure geographic mobility, we use year-to-year changes in a household's Census tract. Our outcome of interest is a binary indicator equal to 1 if the household's Census tract in year $t$ is different from that in year $t-1$, and 0 otherwise. We study the effects of winning the lottery on geographic mobility by estimating regression model (3.4). The estimated coefficients of this regression are summarized in Figure 5.1. We find no evidence of differential trends in geographic mobility between current and later winners in years prior to winning. Winning the lottery leads to an immediate and sharp increase in the annual moving rate of approximately 3.5 percentage points (approximately 25 percent). As of the second year post-win, however, the effect of winning on annual moving rates largely dissipates, although annual moving rates of lottery winners remain elevated relative to those who have not yet won the lottery. Taken together, this temporal pattern suggests that winning the lottery induces households to move once shortly after winning.

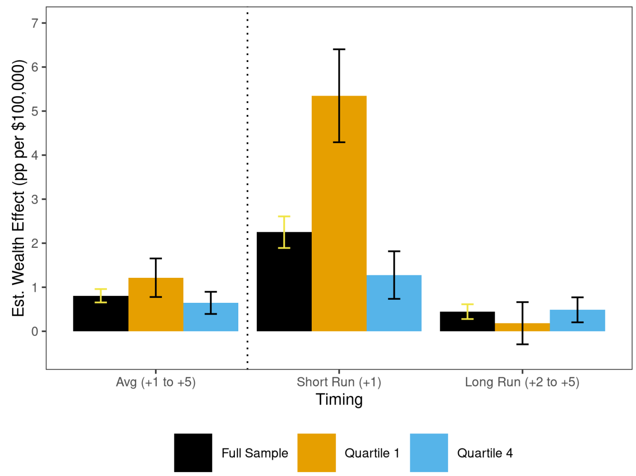
Figure 5.1: Effect of winning on moving




*Notes:* This figure presents estimates of the impact of winning on the propensity to move across Census tracts, based on estimating a version of equation (3.4) (as described in Section 3.1). The outcome is a binary indicator equal to 1 if the household has moved from their Census tract in that year (i.e., that the current Census tract is different from that in the prior year), and 0 otherwise. We then take cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. Throughout, we use $w-2$ as the omitted event time. In addition to the cohort-size-weighted average effect in levels (left-hand axis), we also reports the average effect scaled by the mean of the outcome in omitted event time (right-hand axis) which can be interpreted as an average percentage change (relative to the baseline pre-win period) in the outcome.

To study the importance of wealth for geographic mobility, we turn to estimating the IV model that we describe in Section 3.2. The resulting IV estimates are reported in Figure 5.2, where we scale responses per 100,000 dollars of additional wealth. The estimated effects mirror the temporal pattern discussed above: moving responses predominantly occur immediately after winning. Focusing on the first year post-win, we find that the propensity to move increases by approximately 2 percentage points for an extra 100,000 dollars in wealth. However, this average wealth effect masks a striking difference between households across the income distribution. Lower-income households are much more likely to move: the increase in the probability of moving for winners in the lowest quartile is around five times as large as that of the winners in the highest quartile for an extra 100,000 dollars in wealth.

Figure 5.2: Effect of wealth on geographic mobility




*Notes:* This figure presents estimates of the mean effect of an extra dollar of wealth on the propensity to move Census tract. The estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2 for the binary outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for all post-win event times $\{1, 2, 3, 4, 5\}$ ("Avg (+1 to +5)"), a shorter-run set of post-win event times $\{1\}$ ("Short Run (+1)"), and a longer-run set of post-win event times $\{2, 3, 4, 5\}$ ("Long Run (+2 to +5)"). In addition, for each temporal average, we report wealth effects for the full analysis sample ("Full Sample") as well as for the subsample of winners falling in the first ("Quartile 1") and fourth ("Quartile 4") quartile of the pre-win distribution of per-adult adjusted gross income. To ease interpretability, we scale moving responses by $100,000.

Next, we explore the spatial dimension of the induced moves. We use the fact that Census tracts are nested within Census counties, and Census counties are nested within U.S. state boundaries. We use these three levels of geography to summarize the distance of a typical move. Let $d \in \{1, 2, 3\}$ denote three mutually exclusive and exhaustive types of moves, with $d = 1$ corresponding to a move across tracts but within county, $d = 2$ corresponding to a move across counties but within state, and $d = 3$ corresponding to a move across states. Let $M_{i,t}$ be a binary indicator equal to 1 if the household's Census tract in year $t$ is different from that in year $t - 1$, and 0 otherwise. Furthermore, let $M_{i,t}^d$ denote an indicator corresponding to a move of type $d$. By definition, we can decompose the total probability of moving as follows:

$$\underbrace{\mathbb{P}\left[M_{i,t} = 1\right]}_{\text{total prob. of moving}} = \underbrace{\mathbb{P}\left[M_{i,t}^1 = 1\right]}_{\substack{\text{prob. of moving} \\ \text{across tract \& within county}}} + \underbrace{\mathbb{P}\left[M_{i,t}^2 = 1\right]}_{\substack{\text{prob. of moving} \\ \text{across county \& within state}}} + \underbrace{\mathbb{P}\left[M_{i,t}^3 = 1\right]}_{\substack{\text{prob. of moving} \\ \text{across states}}}$$

It is straightforward to show that the total effect of winning on geographic mobility can be decomposed into the effect on each of the move types.[43]In Appendix Figure B.11 we focus on the first year post-win and quantify the contribution of each type of move $d$ on the total effect of winning on geographic mobility. On average and across the income distribution, we find that more than 80 percent of moves are within state, and the majority of moves occur across quite nearby locations, i.e., across tracts within the same county.

[43]See Online Appendix F for a formal discussion of this decomposition.

Taken together, the above results suggest that winning the lottery leads to a sizable, swift, and non-recurring moving response, especially among lower-income households. Among these moves, the vast majority takes place across locations close to the winner's current home. This observation guides the subsequent analysis in two ways. First, it motivates our choice of Census tract as the main geographic unit of interest when defining the relevant characteristics of a local area. Second, it sharpens our focus on the first year following the lottery win, corresponding to the period in which households predominantly respond to the change in wealth.

**Who moves in response to windfall gains?** Are certain types of households more likely to move than others in response to a wealth shock? Motivated by empirical evidence that suggests a relationship between certain demographic characteristics and moving costs and attitudes (see, e.g., Koşar, Ransom, and van der Klaauw, 2021), we estimate the IV model introduced in Section 3.2 separately by demographic groups.

Figure 5.3a presents the IV estimates that correspond to the first year post-win. As discussed above, we find that lower-income households are about five times more likely to move than higher-income households in response to an unexpected change in wealth. Closely related, households without a strong attachment to the work force are also significantly more likely to move.

Motivated by the literature on the impact of neighborhoods on long-term economic outcomes of young children (Chetty, Hendren, and Katz, 2016b; Chetty and Hendren, 2018), we also explore whether the presence of young children leads to differential moving responses. Figure 5.3a shows that additional wealth makes parents with young children slightly more likely to move compared to other parents, but the difference is not significant at conventional levels.

Past work has found that young households are more mobile, suggestive of moving costs that increase with age (Molloy, Smith, and Wozniak, 2011; Koşar, Ransom, and van der Klaauw, 2021). When we compare young and old winners, we find that young winners tend to be slightly more responsive, but the difference is not statistically significant. Lastly, we find that renters are more than 3 times more likely to move their location in response to an additional dollar of wealth, in line with evidence that suggests that moving costs are considerably higher for homeowners (Oswald, 2019).

**Where do people move in response to windfall gains?** Are households moving to places with higher quality than their previous locations? How do local labor market attributes of the destination compare to their origin location? We conduct a decomposition of the moving response using origin and destination characteristics to shed light on these questions.

Let $C_{i,t}$ denote a continuous-valued attribute of household $i$'s tract in $t$. As a convention, we will refer to moves to tracts with lower $C$ as downward moves, and moves to tracts with the same or higher $C$ as upward moves. By definition, we can decompose the total probability of moving as follows:

$$\underbrace{\mathbb{P}\left[M_{i,t}=1\right]}_{\text{total prob. of moving}} = \underbrace{\mathbb{P}\left[M_{i,t}=1 \,\cap\, C_{i,t} \geq C_{i,t-1}\right]}_{\text{prob. of moving upward}} + \underbrace{\mathbb{P}\left[M_{i,t}=1 \,\cap\, C_{i,t} < C_{i,t-1}\right]}_{\text{prob. of moving downward}}. \tag{5.2}$$

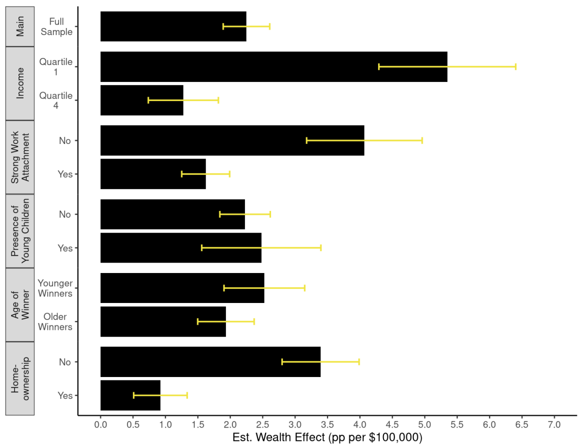
For each characteristic $C$, we estimate the IV model introduced in Section 3.2 for each component in expression (5.2), allowing us to quantify the contribution of each to the total moving effect and thereby address whether the overall moving response is driven by moves upward versus moves downward.

In Figure 5.3b we report the results of this decomposition for various attributes of local labor markets. Overall, we do not find evidence that households are disproportionately moving to areas with stronger local labor markets as measured by, for example, local wage growth and employment growth. We do, however, find some evidence that households are moving increasingly to less densely-populated areas, and local labor markets that are characterized by a longer typical commute from home to work and fewer jobs. For instance, close to 65% of the increase in moving propensity is due to moves to locations with a longer typical commute to work, whereas more than 60% is due to moves to less densely-populated areas. Overall, this pattern is suggestive of households moving towards less urban areas.

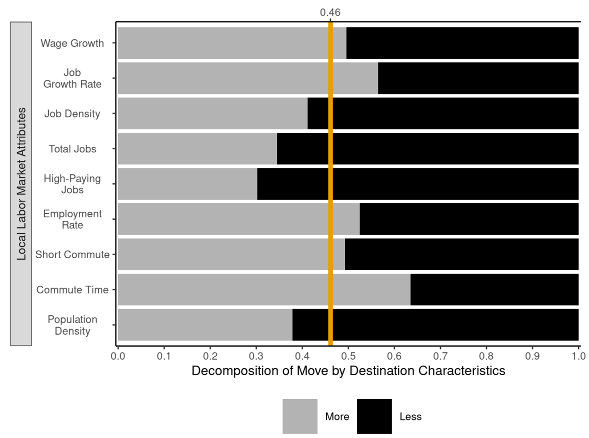
In Figure 5.3c we explore whether the overall moving response is driven by a reallocation to to neighborhoods that are typically-measured as having higher quality. We judge the quality of neighborhoods based on measures used explicitly in past work, such as the Opportunity Atlas (Chetty, Friedman, Hendren, Jones, and Porter, 2020), Childhood Opportunity Index (Noelke, McArdle, Baek, Huntington, Huber, Hardy, and Acevedo-Garcia, 2020; Aliprantis and Martin, 2020), Area Deprivation Index (Kind and Buckingham, 2018), poverty rate (Wilson, 1987), and college attainment (Couture, Gaubert, Handbury, and Hurst, 2019) among others. Overall, we do not find strong evidence that the overall moving response is driven by moves to higher-quality neighborhoods. In Figure 5.3d, we shift attention to parents with young kids which is a group of particular interest given past research on the impact of neighborhoods on intergenerational mobility. We find that, if anything, they are even less likely to move to a higher-quality neighborhood than the other winners without young kids.

Existing work in this literature has considered three distinct forces which may play a role in the apparent lack of moving to higher-quality neighborhoods. First, there may be preference-based explanations for the pattern of geographic sorting. For example, the presence of natural amenities (such as proximity to oceans, hills, and lakes) or lower costs of living may compensate residents in locations of lower quality in other dimensions. Second, households that are willing and able to move to a better neighborhood may not due to lack of information on the attributes of other neighborhoods, or on the potential benefits of moving to higher-quality locations (Bergman, Chetty, DeLuca, Hendren, Katz, and Palmer, 2020; Bergman, Chan, and Kapor, 2020). Lastly, households may wish to move to higher-quality neighborhoods, but face financial or non-financial frictions preventing them from doing so. For example, past work on internal migration in the U.S. argues that moving costs must be on the order of hundreds of thousands of dollars for the average household in order to rationalize the persistent differences in wages across places (Davies, Greenwood, and Li, 2001; Kennan and Walker, 2011; Bayer and Juessen, 2012).

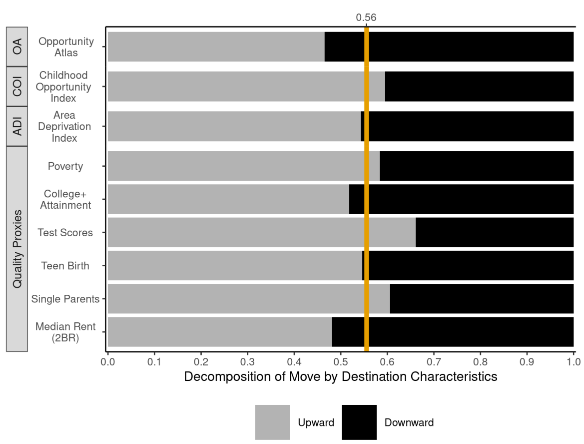
Our data has two advantages that allow us to contribute to this literature. First, we have a long and large panel data set of households. This enables us to study who moves and where, including features of both the origin and destination of the moves. Second, the lottery winnings allow us to infer how a change in wealth, in and of itself, affects geographic mobility. This variation allows us to isolate the effects of changes in wealth, holding fixed other determinants of mobility such as prices, preferences, information sets, and local economic conditions. In contrast to place-based policies and mobility vouchers, the variation in wealth induced by lottery winnings is specific to a household and not tied to geographic relocation. Taken as a whole, the evidence on geographic mobility of lottery winners suggests that pure unconditional cash transfers do not lead households to systematically move to locations with more active local labor markets or to locations of higher quality, consistent with the argument in Bergman, Chetty, DeLuca, Hendren, Katz, and Palmer (2020) on the importance of non-financial barriers to moving to better neighborhoods.



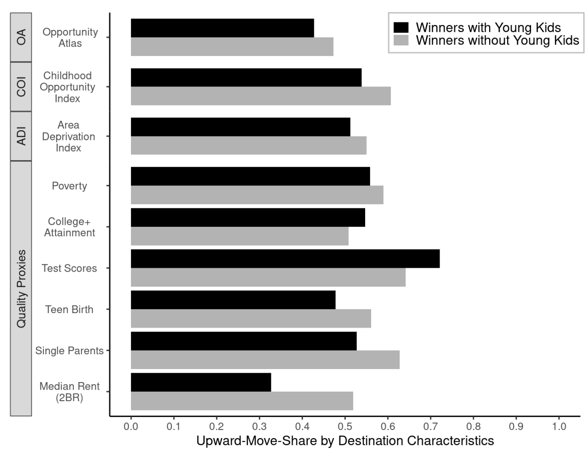

(a) Winner Characteristics




(b) Destination Characteristics: Local Labor Market




(c) Destination Characteristics: Neighborhood Quality




(d) Neighborhood Quality for Winners with Young Kids

Figure 5.3: Decomposition of the moving response by characteristics

*Notes:* Subfigure (a) presents an analysis of heterogeneity in the mean effect of an extra dollar of wealth on moving by characteristics of the winner. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, separately for each demographic group. The dependent variable is a binary indicator for moving Census tract. We then take cohort-size-weighted averages of $\beta^{w,\ell}$ for event time $\ell = 1$. To ease interpretability, each estimate is scaled by $100,000. Subfigures (b) and (c) show a decomposition of the overall effect of winning on moving by characteristics of the destination. These estimates are calculated by estimating a version of equation (3.4) (as described in Section 3.1), focusing on effects in the first year post-win. The dependent variables are binary indicators for moving Census tract and moving upwards and moving downwards along a given measure of local labor market attribute or neighborhood quality. The resulting estimates are then scaled by the total moving response. The vertical line in subfigures (b) and (c) corresponds to the unweighted mean of estimates in the "More" / "Upward" direction. Subfigure (d) shows a comparison between winners with and without young kids in terms of the share of the total moving response that is due to a move upward.

### 5.3 Family formation and stability

Conventional approaches to studying household labor supply treat household formation as exogenous and then model the labor supply decision for a given household member.[44] However, a change in unearned income or wealth can affect the likelihood of marriage and divorce for a number of reasons. Conceptually, an increase in unearned income raises income in both the married and unmarried states, yielding ambiguous predictions on both margins. From the perspective of singles, while an increase in wealth makes singles more attractive as potential marriage partners, it also increases the option value of remaining single (Becker, 1973; Becker, Landes, and Michael, 1977). Similarly, while additional wealth can have a stabilizing effect on existing marriages, it can also help to cover the legal costs of a divorce for otherwise financially-constrained households (Burstein, 2007).

We shed light on the forces above by studying the effect of winning the lottery on family formation (marriage) and dissolution (divorce). To study the effect of winning the lottery on marriage, we restrict our sample of treated winners to tax filers that were unmarried 2 years prior to winning the lottery. Our outcome of interest is a time-varying indicator equal to 1 if the winner is married. The control group is comprised of tax filers who were unmarried in the same year as the treated winners, but won a lottery in a later year. Using the same DiD estimator as before, such a control group allows one to difference out common time effects, including common mean reversion in marital status. Any differential time effect (e.g., due to differential mean reversion) between the two groups would be reflected as differential trends between current and later winners prior to winning. In Appendix Figure B.12, we report the estimated coefficients from regression (3.4) with the outcome defined as above. We find no evidence of differential trends in marital status between current and later winners in pre-win event times, providing support for the common trends assumption. The event study shows that winning the lottery increases the propensity to get married for single lottery winners (relative to single non-yet-winners).

To study the effect of winning the lottery on divorce, we follow a similar sample construction as above, only with tax filers that were *married* 2 years prior to winning the lottery. Here, our outcome of interest is a time-varying indicator for singlehood. Appendix Figure B.12 shows the estimated coefficients from regression (3.4) using singlehood as the outcome. Here, also, we find no evidence of differential trends between current and later winners in pre-win event times. The event study shows the likelihood for married winners to get divorced (relative to married non-yet-winners) decreases on average.

To quantify the role of an increase in wealth on family formation and stability, we estimate the IV model from Section 3.2. We find that on average, marriage and divorce responses to an exogenous change in wealth are economically modest. Table 5.4 shows that for every $100,000 of additional wealth, the propensity to marry increases by around 0.8 percentage points, while the likelihood of divorce decreases by around 0.7 percentage points on average.[45] In other words, wealth does appear to be a barrier to family formation and a stabilizer of existing families, in line with correlational evidence (Lundberg and Pollak, 2014; Lafortune and Low, 2017). These two findings hold especially for low-income households.

[44] A growing body of work (such as Low, Meghir, Pistaferri, and Voena, 2020) endogenizes the household formation problem when studying household labor market responses.

[45] The effects of changes in unearned income on family formation and stability are reported in Appendix Table A.13.

Table 5.4: Effects of wealth on marriages and divorce

| Outcome | Value | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 4 Pre-Win Income (3) |
|---|---|---|---|---|
| New Marriage | *Estimate* | 0.0077 | 0.0167 | 0.0006 |
| | *Standard Error* | (0.0008) | (0.0024) | (0.0013) |
| | *Counterfactual Mean* | 0.14 | 0.14 | 0.15 |
| | *Percentage Change* | 5.5 | 11.7 | 0.4 |
| Divorce | *Estimate* | -0.0067 | -0.0146 | -0.0058 |
| | *Standard Error* | (0.0010) | (0.0041) | (0.0015) |
| | *Counterfactual Mean* | 0.11 | 0.17 | 0.09 |
| | *Percentage Change* | -5.9 | -8.6 | -6.3 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of wealth on the propensity to enter or leave marriage. The estimation sample is restricted to winners and not-yet winners that are tax filers. When we study the effect on new marriages (divorce), we further restrict the sample to individuals that were not married (married) in $w-2$. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, for each outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for post-win event times $\{1,2,3,4,5\}$ to recover the mean wealth effect in the post-win period. Column 1 reports mean effects of an extra dollar of wealth for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of wealth for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by \$100,000. The counterfactual means correspond to the marriage and divorce rates that would have occurred in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per \$100,000 in percentage change terms relative to the counterfactual mean.

To our knowledge, the only directly comparable estimates to ours come from two prior studies.[46] The first study, Hankins and Hoekstra (2011), utilizes data on marriage and divorce records from two Florida counties that are manually linked (using name and county of residence) to administrative data on lottery winners. Our estimated wealth effects are in line with estimates in Hankins and Hoekstra (2011) but substantially more precise, providing confidence in both the sign and modest magnitude of our estimates for the U.S. The second study, Cesarini, Lindqvist, Notowidigdo, and Östling (2017), only explores effects on divorce risk of married lottery winners (their Appendix Figure A21). Cesarini, Lindqvist, Notowidigdo, and Östling (2017) finds, if anything, a modest *increase* in divorce risk, although estimates are relatively imprecise and so could not be distinguished from a divorce risk decrease of the magnitude in our data.

# 6 Comparison with existing work

Our paper is related to several other studies that estimate the earnings responses to lottery winnings. The four most closely related ones are Imbens, Rubin, and Sacerdote (2001), Bulman, Fairlie, Goodman, and Isen

[46] Three related past studies are Rainer and Smith (2010), Farnham, Schmidt, and Sevak (2011), and Klein (2017) which all study marriage dissolution in response to changes in housing wealth. These studies do not report wealth effects for divorce or enough information with which to calculate them. Rainer and Smith (2010) and Farnham, Schmidt, and Sevak (2011) find evidence that negative wealth shocks increase divorce risk, but neither finds evidence for positive wealth shocks decreasing divorce risk, in contrast to our findings. Klein (2017), on the other hand, finds that, in line with our results, positive wealth shocks decrease divorce risk, with mixed evidence on the relationship between negative wealth shocks and divorce.

(2021), Cesarini, Lindqvist, Notowidigdo, and Östling (2017), and Picchio, Suetens, and van Ours (2018). The former two studies use data from the U.S., while the latter two studies use data from Sweden and the Netherlands, respectively. In this section, we carefully compare our estimates to those reported in these studies.

This comparison leads to two key conclusions. First of all, once we perform apples-to-apples comparisons that use the same measures of earnings responses and wealth changes associated with lottery wins, we find similar estimates as reported by the existing U.S. studies. We show that an important limitation of the existing U.S. studies is that they use measures that understate earnings responses and overstate the wealth changes associated with lottery wins. These problems lead them to substantially underestimate wealth effects and MPEs. Second, the estimates from the European studies are consistently and noticeably smaller than ours, even when we use a comparable measure of earnings and lottery winnings. These findings caution against the practice of using wealth effects or MPEs from one country as inputs for models that are otherwise calibrated or estimated using data from other countries.

## 6.1 Description of studies

The second column of Tables 6.1 and 6.2 summarize the data source and population of study in each paper. As evident from this column, both Cesarini, Lindqvist, Notowidigdo, and Östling (2017) and Picchio, Suetens, and van Ours (2018) as well as our study use administrative data for the population at large to estimate individual and household labor market responses to lottery winnings. Bulman, Fairlie, Goodman, and Isen (2021) also use administrative data, but their population of study is rather different. The goal of their study is to estimate the effect of an increase in parental wealth on college attendance of children and they, therefore, restrict the estimation sample to lottery winners with children of various ages (ranging from childhood to early adulthood). For these parents, they report earnings responses per dollar of lottery winnings in footnote 41. The primary data source of Imbens, Rubin, and Sacerdote (2001) is a mail-in survey of people playing the (Megabucks) state lottery in Massachusetts during the years 1984 through 1988 and winning a major prize. Imbens, Rubin, and Sacerdote (2001) combines this survey with administrative data on each respondent's earnings and lottery winnings. Their headline estimates come from a subsample of 194 respondents that excludes the biggest winners.

The other columns of Tables 6.1 and 6.2 are populated with estimates of wealth effects and MPEs from the various studies. As evident from these columns, direct comparisons across studies are challenging for three reasons. First, Imbens, Rubin, and Sacerdote (2001) only report MPEs while Bulman, Fairlie, Goodman, and Isen (2021) and Picchio, Suetens, and van Ours (2018) only report wealth effects. Second, different studies use alternative measures of both earnings responses and the size of the lottery win. As a result, some of the cells of Tables 6.1 and 6.2 do not contain any estimates (indicated by NA). Yet it is still possible to compare the estimate(s) of each study to one or more of our estimates. Third, the identification strategies differ across studies. The approach taken varies naturally across studies depending on the available data.

The ideal identification strategy would be to regress earnings on lottery winnings among individuals who participate in the same lottery draw while conditioning on the number of lottery tickets bought. Doing so would allow the researcher to be entirely confident that she has isolated the exogenous variation in lottery winnings. A key advantage of the two European studies is that their data allow them to effectively use such an identification strategy. Such a strategy is not used (or feasible) in the studies that use U.S. data as it does not let one restrict the sample to individuals who participate in the same lottery draw. Due to these data limitations, the U.S. studies use a different identification strategy. Both Imbens, Rubin, and Sacerdote (2001) and our study take advantage of individual-level panel data to compare the earnings of lottery winners, before and after they win.[47] The key difference between the baseline specification of Imbens, Rubin, and Sacerdote (2001) and the one used in our study is arguably that they use a first-difference approach, comparing winners before and after the winnings, while we add as second difference to this estimator to eliminate common time effects.[48] It is reassuring to find that this difference in the choice of estimator does not materially affect the empirical findings.[49]

## 6.2 Comparison of wealth effects

In Table 6.1, we compare estimates of wealth effects as measured by the annual earnings responses per 100 dollars of lottery winnings. The top panel presents the estimates from the U.S. while the bottom panel reports the estimates from the two European countries.

We begin, in the third column, by reporting estimates of the *total labor earnings* (including wage earnings and self-employment income) responses of *households* per 100 dollars in *post-tax* lottery winnings. The measures of earnings and lottery winnings used in the third column are arguably preferable, since they capture the full earnings responses to an increase in the wealth available to be spent or saved. Our study then finds that for an extra 100 dollars in wealth, households reduce their annual earnings by approximately 2.3 dollars on average (as reported earlier in Table 3.1). The estimates of wealth effects of Cesarini, Lindqvist, Notowidigdo, and Östling (2017) for Sweden and Picchio, Suetens, and van Ours (2018) for the Netherlands are about 40 percent smaller than ours, even when we use a comparable measure of earnings and lottery winnings. As shown in the fourth column, this conclusion does not change if we instead consider *individual earnings* responses per 100 dollars in post-tax lottery winnings.

The fifth column of Table 6.1 presents estimates of wealth effects in terms of *household earnings* with lottery wins measured on a *pre-tax* basis. This is the only estimate of earnings responses per dollar of lottery winnings that is reported by Bulman, Fairlie, Goodman, and Isen (2021). It is remarkably similar to the

[47] The identification strategy that Bulman, Fairlie, Goodman, and Isen (2021) use to estimate wealth effects does not use individual-level panel data and, thus, cannot eliminate unobserved time-invariant heterogeneity. This is because their study is centered around the question of whether children whose parents won the lottery are more likely to attend college. To answer this question, they compare differences in this outcome between children whose parents won large and small amounts before high school graduation to those between children whose parents won large and small amounts after high school graduation (i.e., too late for college attendance in the year of high school graduation to be affected). They use the same identification strategy to analyze earnings responses per dollar of lottery winnings, except the outcome variable now is parental earnings in the year of the child's high school graduation.

[48] Our sample selection also mirrors Imbens, Rubin, and Sacerdote (2001) in that they also exclude individuals with small lottery winnings from their estimation sample.

[49] As discussed in Section 3, we find that the first-difference estimator (assuming no change over time) and the difference-in-differences estimator (assuming common changes in outcomes between treatment and control group, in the absence of treatment) give very similar results, no matter the choice of the control group. To directly compare how the findings of Imbens, Rubin, and Sacerdote (2001) depend on methodology, we have also used their own micro data (which is publicly available) to show that their finding of relatively small MPEs is not due to their estimation method. If we apply our difference-in-differences estimator to their data, we obtain similar estimates to those in Imbens, Rubin, and Sacerdote (2001) (see Appendix Table A.14).

wealth effects that we find when we use the same measure of earnings and lottery winnings. However, lottery winnings in the U.S. are taxed as ordinary income and the change in wealth of the household is equal to the *post-tax* value of the win. Using the pre-tax value underestimates the wealth effects by almost a factor of two.

Table 6.1: Estimates of wealth effects: annual earnings changes per 100 dollars in lottery winnings

| | | Post-Tax Winnings | | Pre-Tax Winnings |
|---|---|---|---|---|
| Study<br>(1) | Primary Data Source and Population of Study<br>(2) | Household Total Labor Earnings<br>(3) | Individual Earnings<br>(4) | Household Earnings<br>(5) |
| *Panel A: U.S. Studies* | | | | |
| IRS (2001) | Respondents to a Survey in Massachusetts | NA | NA | NA |
| BFGI (2021) | Administrative Data on Parents with Young to Early-Adult Children | NA | NA | -1.171<br>(0.215) |
| Our estimate | Administrative Data on Working-Age Individuals and Households | -2.339<br>(0.066) | -1.717<br>(0.032) | -1.288<br>(0.042) |
| *Panel B: European Studies* | | | | |
| CLNÖ (2017) | Administrative Data on Working-Age Individuals and Households | -1.306<br>(0.194) | -0.964<br>(0.151) | NA |
| PSO (2018) | Administrative Data on Working-Age Individuals and Households | -1.348<br>(-0.748) | -1.003<br>(-0.630) | NA |

*Notes:* This table presents a comparison of our estimated wealth effects with the ones reported in four closely related studies: Imbens, Rubin, and Sacerdote (2001) (IRS (2001)), Cesarini, Lindqvist, Notowidigdo, and Östling (2017) (CLNÖ (2017)), Picchio, Suetens, and van Ours (2018) (PSO (2018)), and Bulman, Fairlie, Goodman, and Isen (2021) (BFGI (2021)). Panel A corresponds to studies with populations in the U.S., while Panel B corresponds to studies with populations in two European countries, Sweden and the Netherlands. The first two columns in the table report the study identifier and the primary data source and population considered in each study. The remaining columns are populated with the relevant estimates from the various studies. Values of NA correspond to cases where such an estimate was not reported in that study. All studies report a wealth effect estimate except for IRS (2001) where the authors only report MPE estimates. For BFGI (2021), we report the estimate in their Appendix Table A35, column 1 (and also footnote 41). For CLNÖ (2017), we report the estimates in their Table 3, column 2 (individual earnings) and Table 6, column 5 (household total labor earnings). Finally, PSO (2018) report unconditional wealth effect estimates for individual earnings in their Appendix D Table D1, separately in each of the three years post-win (their columns 2 to 4). We report the mean of these estimates and their standard errors for consistency with the estimates of the other studies we consider and ours.

### 6.3 Comparison of MPEs

In Table 6.2, we compare estimates of MPEs as measured by the annual earnings responses per extra dollar of unearned income in a given year (for details on how we do this comparison, see Online Appendix G). The headline estimates of Imbens, Rubin, and Sacerdote (2001) and Cesarini, Lindqvist, Notowidigdo, and Östling (2017) are -0.11 and -0.14, respectively. To the casual eye, these estimates may seem at odds with our headline MPE estimate of -0.52 (see Table 4.1). Indeed, existing work frequently refers to the estimates of Imbens, Rubin, and Sacerdote (2001) and Cesarini, Lindqvist, Notowidigdo, and Östling (2017) as evidence of negligible income effects on labor supply, often to motivate a quasi-linear specification of preferences. We now show that such a conclusion is misguided for the U.S., and that studies of lottery winnings give a consistent picture of not only wealth effects but also MPEs once one performs apples-to-apples comparisons.

We begin, in columns three to six, by comparing our estimates of MPEs to those reported in Cesarini, Lindqvist, Notowidigdo, and Östling (2017). To do so, we progressively align assumptions to go from our estimate of the MPE to the headline number of Cesarini, Lindqvist, Notowidigdo, and Östling (2017). In the third column, we use our specification from Table 4.1 of the *total labor earnings* (including wage earnings and self-employment income) responses of *households* to an extra dollar of *post-tax* unearned income with a discount rate of 2.5 percent (equal to the average real interest rate in the period of study). Our study then finds that per extra dollar in unearned income available to be spent or saved, households reduce their annual earnings by approximately 52 cents on average (as reported earlier in Table 4.1). The estimates for Sweden are about 40 percent smaller than ours, consistent with the differences in wealth effects reported in Table 6.1.

To arrive at the headline estimate of Cesarini, Lindqvist, Notowidigdo, and Östling (2017) of -0.14, we next consider i) individual as opposed to household responses (column four), ii) replace our discount rate of 2.5 percent with the discount rate of Cesarini, Lindqvist, Notowidigdo, and Östling (2017) of 1.5 percent (column five), and iii) change the measure of labor market responses from labor earnings to *net labor income* after the deduction of taxes (column six).[50] The only change that materially affects the estimates is the use of net labor income as the outcome variable. The progressive labor income taxation in Sweden reduces the estimated MPEs from -0.28 in column five to -0.14 in column six. By way of comparison, the MPE in terms of net labor income remains relatively large in the U.S., around -0.41. Thus, we conclude that MPEs in our data are non-negligible both if we use our baseline specification or if we use the specification that delivers the headline estimate of Cesarini, Lindqvist, Notowidigdo, and Östling (2017). It is also evident that MPEs in Sweden are considerably lower than in the U.S., even when we use comparable measures of both earnings and unearned income.

The purpose of the final three columns of Table 6.2 is to reconcile the apparent differences between our estimates of MPEs and those reported by Imbens, Rubin, and Sacerdote (2001). In these three columns, the lottery winnings are measured on a *pre-tax* basis, as this is done throughout the study of Imbens, Rubin, and Sacerdote (2001). As shown in the final column, they then find that the lottery winners, on average, reduce their individual annual earnings by 11.2 cents per dollar of additional pre-tax unearned income. These MPEs appear to be much lower than our estimate in the third column of 52 cents. However, as shown in the final column, when we use comparable measures of earnings, lottery winnings, and discount rates, our estimate of the MPE becomes much smaller and economically similar to those reported in Imbens, Rubin, and Sacerdote (2001). Comparing the estimates in columns three and seven shows that failing to measure the full earnings responses to the lottery winnings remaining after deduction of taxes leads one to underestimate the MPE by a factor of two. The results in columns eight and nine show that if we use the same earnings concept (individual earnings) and the same discount rate (10 percent) as Imbens, Rubin, and Sacerdote (2001), the

[50] Cesarini, Lindqvist, Notowidigdo, and Östling (2017) report responses both in terms of labor earnings and net labor income. None of the other studies consider wealth effects or MPEs on net labor income. The choice between pre and post-tax measures of labor earnings depends on the question of interest and the model of labor supply. Pre-tax labor earnings effects are the relevant statistics to evaluate several policy reforms, such as the introduction of universal basic income or changes in the top marginal tax rates (Golosov, Graber, Mogstad, and Novgorodsky, 2021). On the other hand, the effects on net labor income map easily to labor supply elasticities in certain labor supply models, such as in a model where households have Stone-Geary preferences, face a linear tax system, and are working both before and after winning the lottery (similar to those found in Imbens, Rubin, and Sacerdote (2001) and Cesarini, Lindqvist, Notowidigdo, and Östling (2017)).

estimated MPE becomes even smaller, declining first to -0.24 and then further to -0.13. This discount rate is much higher than what Cesarini, Lindqvist, Notowidigdo, and Östling (2017) use as well as the observed real interest rate in the period of study.

Table 6.2: Estimates of MPEs: annual earnings changes per dollar of extra unearned income

| Study | Primary Data Source and Population of Study | Post-Tax Unearned Income | | | | Pre-Tax Unearned Income | | |
|---|---|---|---|---|---|---|---|---|
| | | Household Total Labor Earnings discount rate: 2.5% | Individual Total Labor Earnings discount rate: 2.5% | Individual Total Labor Earnings discount rate: 1.5% | Individual Net Labor Income discount rate: 1.5% | Household Total Labor Earnings discount rate: 2.5% | Individual Earnings discount rate: 2.5% | Individual Earnings discount rate: 10% |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *Panel A: U.S. Studies* | | | | | | | | |
| IRS (2001) | Respondents to a Survey in Massachusetts (Sample Excluding Large Winners) | NA | NA | NA | NA | NA | NA | -0.112 |
| BFGI (2021) | Administrative Data on Parents with Young to Early-Adult Children | NA | NA | NA | NA | NA | NA | NA |
| Our estimate | Administrative Data on Working-Age Individuals and Households | -0.523 | -0.433 | -0.487 | -0.405 | -0.332 | -0.241 | -0.133 |
| *Panel B: European Studies* | | | | | | | | |
| CLNÖ (2017) | Administrative Data on Working-Age Individuals and Households | -0.333 | -0.267 | -0.284 | -0.144 | NA | NA | NA |
| PSO (2018) | Administrative Data on Working-Age Individuals and Households | NA | NA | NA | NA | NA | NA | NA |

*Notes:* This table presents a comparison of our estimated effects of additional unearned income (or MPEs) with the ones reported in four closely related studies: Imbens, Rubin, and Sacerdote (2001) (IRS (2001)), Cesarini, Lindqvist, Notowidigdo, and Östling (2017) (CLNÖ (2017)), Picchio, Suetens, and van Ours (2018) (PSO (2018)), and Bulman, Fairlie, Goodman, and Isen (2021) (BFGI (2021)). Panel A corresponds to studies with populations in the U.S., while Panel B corresponds to studies with populations in two European countries, Sweden and the Netherlands. The first two columns in the table report the study identifier and the primary data source and population considered in each study. The remaining columns are populated with the relevant estimates from the various studies. Values of NA correspond to cases where such an estimate was not reported in that study. Two studies do not report an MPE estimate: PSO (2018) and BFGI (2021). For IRS (2001), we report the estimate for their preferred sample of winners with annual pre-tax prize payouts less than $100,000 (Sample Excluding Large Winners) which corresponds to IRS (2001) Table 4, Specification VIII. For this estimate, we take the estimate reported in IRS (2001) Table 4 and divide by 0.9 to reflect the fact that annual prize payments in IRS (2001) are for 20 years and not for the remaining lifetime of the winner, consistent with IRS (2001)'s approach. For CLNÖ (2017), the estimate in column 6 corresponds to the estimate reported in their Table 5 (panel C; assumed age-at-win of 30). For the estimates in columns 3 to 5, we utilize their model code (which is publicly available) and the wealth effects reported in their Figures 1 and 5 to calculate the lifetime MPE for a 30-year-old winner across various choices of earnings concept and discount rates. For additional details see Appendix G.

# 7 Conclusion

The goal of our paper was to study how Americans respond to idiosyncratic and exogenous changes in household wealth and unearned income. Our analyses combined administrative data on U.S. lottery winners with an event-study design. We first examined individual and household earnings responses to these windfall gains, finding significant and sizable wealth and income effects. On average, an extra dollar of unearned income in a given period reduces household labor earnings by about 50 cents, decreases total labor taxes by 10 cents, and increases consumption by 60 cents. These effects are heterogeneous across the income distribution, with households in higher quartiles of the income distribution reducing their earnings by a larger amount. Next, we examined margins of adjustment other than earnings and, in the course of doing so, address a number of important economic questions about how additional wealth or unearned income affect retirement decisions and labor market dynamics, family formation and dissolution, entrepreneurship and self-employment, and geographic mobility and neighborhood choice. Lastly, we carefully compared our findings to those reported in existing lottery studies. This comparison revealed that existing U.S. studies substantially underestimate wealth and income effects because they use measures that understate earnings responses and overstate wealth changes associated with lottery wins.

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# Online Appendix

## A Appendix Tables

Table A.1: Distribution of lottery winnings

| | Pre-Tax Household Winnings | Post-Tax Per-Adult Winnings | |
|---|---|---|---|
| | Size of win | Size of win | Additional unearned annuity income |
| *Statistic* | (1) | (2) | (3) |
| 1st percentile | \$30,400 | \$11,300 | \$500 |
| 5th percentile | \$31,400 | \$13,400 | \$600 |
| 10th percentile | \$33,400 | \$16,700 | \$800 |
| 25th percentile | \$45,600 | \$25,800 | \$1,100 |
| Median | \$67,800 | \$43,600 | \$1,900 |
| Mean | \$359,500 | \$181,200 | \$8,100 |
| 75th percentile | \$150,000 | \$87,200 | \$3,900 |
| 90th percentile | \$331,800 | \$202,900 | \$9,300 |
| 95th percentile | \$847,400 | \$394,200 | \$18,100 |
| 99th percentile | \$3,969,800 | \$1,795,500 | \$82,400 |

*Notes:* The table summarizes the distribution of the size of lottery wins in our baseline estimation sample of working-age winners, measured in three ways. In Column 1, we report a summary of the overall size of the win measured at the household level and on a pre-tax basis. In Column 2, we report a summary of the overall size of the win measured on a post-tax, per-adult basis (corresponding to a change in wealth available to be spent or saved). In Column 3, we report a summary of the annuitized size of the (post-tax, per-adult) win, taking into account the winner's age and assuming a life expectancy of 80 years and an interest rate of 2.5% (the average risk-free real interest rate in the U.S. for our period of observation). All monetary values are reported in 2016 U.S. dollars, using the Consumer Price Index to adjust for inflation. All values are rounded to the nearest hundred.

Table A.2: Summary statistics of treatment-control sample

| | | **Treatment Group (Current Winners)** | **Control Group (Later Winners)** |
|---|---|---|---|
| | | (1) | (2) |
| *Covariate* | *Statistic* | | |
| Wage Earnings | Mean | $34,649 | $34,278 |
| Employment | Prop. | 0.80 | 0.80 |
| Age | Mean | 43.94 | 41.84 |
| Female | Prop. | 0.39 | 0.39 |
| Married | Prop. | 0.45 | 0.45 |
| Homeowner | Prop. | 0.45 | 0.44 |
| Size of the Lottery Win | Mean | $182,902 | $184,184 |

*Notes:* This table presents a summary of the descriptive statistics in our treatment-control sample. All monetary values are reported in 2016 U.S. dollars, using the Consumer Price Index to adjust for inflation. The treatment group is the collection of all cohorts of working-age winners in our sample with later-treated cohorts that can serve as a control group. The control group is the collection of all control units used across all treated units. All values are measured two years prior to the treated group's win year with the exception of the size of the lottery win, which is measured in each individual's win year.

Table A.3: Extensive-margin share of earnings response

| Outcome | Quartile 1 Pre-Win Income (1) | Quartile 2 Pre-Win Income (2) | Quartile 3 Pre-Win Income (3) | Quartile 4 Pre-Win Income (4) | Aggregate (5) |
|---|---|---|---|---|---|
| Winner Wage Earnings | 0.65 | 0.50 | 0.52 | 0.50 | 0.53 |
| Per-Adult Total Labor Earnings | 0.58 | 0.41 | 0.34 | 0.36 | 0.40 |

*Notes:* This table presents the share of the observed earnings responses that is attributable to the extensive margin as defined in Online Appendix D.

Table A.4: IV estimates of the effect of an exogenous change in unearned income (capitalization approach)

| | **Sample** | | | | |
|---|---|---|---|---|---|
| Outcome | Full Sample<br>(1) | Quartile 1 Pre-Win Income<br>(2) | Quartile 2 Pre-Win Income<br>(3) | Quartile 3 Pre-Win Income<br>(4) | Quartile 4 Pre-Win Income<br>(5) |
| Per-Adult Total Labor Earnings | -0.5182<br>(0.0815) | -0.2925<br>(0.0669) | -0.4278<br>(0.0874) | -0.5283<br>(0.1041) | -0.7385<br>(0.3306) |
| Per-Adult Labor Earnings Taxes | -0.1077<br>(0.0201) | -0.0404<br>(0.0221) | -0.0609<br>(0.0244) | -0.1143<br>(0.0277) | -0.1941<br>(0.0892) |
| Implied Consumption Expenditure | 0.5882<br>(0.1689) | 0.7463<br>(0.2294) | 0.6318<br>(0.2280) | 0.5858<br>(0.2159) | 0.4533<br>(0.4781) |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income. These estimates are calculated by first estimating separate regressions for the first- and second-stage model as described in Section 3.2, only now using unearned income as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\rho^{w,\ell}$ and $\phi^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$, and then form the ratio to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 to 5, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into each quartile of the pre-win distribution of adjusted gross income. We use the delta method to calculate two-sample IV standard errors as in Pacini and Windmeijer (2016) (reported in parenthesis), clustering on winner. We use a ratio of weighted-averages due to the additional imprecision in the cohort-by-event-time ratios when using the capitalization method.

Table A.5: Effects of unearned income on earnings by gender of the winner

| Group | Value | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 4 Pre-Win Income (3) |
|---|---|---|---|---|
| | | **Winner Wage Earnings (per dollar)** | | |
| Male Winner | *Estimate* | -0.5733 | -0.3934 | -0.6839 |
| | *Standard Error* | (0.0186) | (0.0197) | (0.0442) |
| | *Counterfactual Mean* | 38235.23 | 14288.29 | 66641.39 |
| | *Percentage Change* | -1.5 | -2.8 | -1.0 |
| Female Winner | *Estimate* | -0.4113 | -0.2277 | -0.6936 |
| | *Standard Error* | (0.0144) | (0.0179) | (0.0436) |
| | *Counterfactual Mean* | 26576.05 | 10946.22 | 47967.19 |
| | *Percentage Change* | -1.5 | -2.1 | -1.4 |
| | | **Per-Adult Total Labor Earnings (per dollar)** | | |
| Male Winner | *Estimate* | -0.6031 | -0.4047 | -0.7683 |
| | *Standard Error* | (0.0190) | (0.0290) | (0.0437) |
| | *Counterfactual Mean* | 36131.60 | 15015.03 | 62948.19 |
| | *Percentage Change* | -1.7 | -2.7 | -1.2 |
| Female Winner | *Estimate* | -0.3817 | -0.1905 | -0.5588 |
| | *Standard Error* | (0.0234) | (0.0442) | (0.0856) |
| | *Counterfactual Mean* | 31112.13 | 12875.22 | 58251.54 |
| | *Percentage Change* | -1.2 | -1.5 | -1.0 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on earnings separately by gender of the winner. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. The counterfactual means correspond to the average earnings the winner would have received in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per $1,000 in percentage change terms relative to the counterfactual mean.

Table A.6: Effects of unearned income on earnings by marital status of the winner

| Group | Value | Full Sample<br>(1) | Quartile 1 Pre-Win Income<br>(2) | Quartile 4 Pre-Win Income<br>(3) |
|---|---|---|---|---|
| | | **Winner Wage Earnings (per dollar)** | | |
| Married Winner | *Estimate* | -0.6376 | -0.5456 | -0.6728 |
| | *Standard Error* | (0.0254) | (0.0471) | (0.0633) |
| | *Counterfactual Mean* | 38275.25 | 11892.97 | 62152.53 |
| | *Percentage Change* | -1.7 | -4.6 | -1.1 |
| Single Winner | *Estimate* | -0.4579 | -0.2982 | -0.6507 |
| | *Standard Error* | (0.0138) | (0.0147) | (0.0375) |
| | *Counterfactual Mean* | 29816.89 | 13198.26 | 57815.81 |
| | *Percentage Change* | -1.5 | -2.3 | -1.1 |
| | | **Per-Adult Total Labor Earnings (per dollar)** | | |
| Married Winner | *Estimate* | -0.6009 | -0.4818 | -0.6892 |
| | *Standard Error* | (0.0300) | (0.1096) | (0.0759) |
| | *Counterfactual Mean* | 38033.72 | 12365.40 | 63576.97 |
| | *Percentage Change* | -1.6 | -3.9 | -1.1 |
| Single Winner | *Estimate* | -0.5051 | -0.3117 | -0.6836 |
| | *Standard Error* | (0.0158) | (0.0222) | (0.0408) |
| | *Counterfactual Mean* | 30994.56 | 14686.77 | 58302.67 |
| | *Percentage Change* | -1.6 | -2.1 | -1.2 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on earnings separately by marital status of the winner. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. The counterfactual means correspond to the average earnings the winner would have received in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per $1,000 in percentage change terms relative to the counterfactual mean.

Table A.7: Effects of unearned income on wage earnings of winners and their spouse

| Group | Value | Full Sample<br>(1) | Quartile 1<br>Pre-Win Income<br>(2) | Quartile 4<br>Pre-Win Income<br>(3) |
|---|---|---|---|---|
| Winner | *Estimate* | -0.6376 | -0.5456 | -0.6728 |
| | *Standard Error* | (0.0254) | (0.0471) | (0.0633) |
| | *Counterfactual Mean* | 38275.25 | 11892.97 | 62152.53 |
| | *Percentage Change* | -1.7 | -4.6 | -1.1 |
| Spouse | *Estimate* | -0.2249 | -0.0706 | -0.3706 |
| | *Standard Error* | (0.0221) | (0.0452) | (0.0668) |
| | *Counterfactual Mean* | 27141.85 | 6727.89 | 46890.46 |
| | *Percentage Change* | -0.8 | -1.0 | -0.8 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on wage earnings for winners and non-winning spouses. The estimation sample is restricted to married couples. Estimates are calculated by first estimating separate regressions for the first- and second-stage model as described in Section 3.2, only now using unearned income as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\rho^{w,\ell}$ and $\phi^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$, and then form the ratio to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. The counterfactual means correspond to the average earnings the winner and spouse would have received in the absence of winning (see Online Appendix E for details). The row "Percentage Change" reports the effect per $1,000 in percentage change terms relative to the counterfactual mean.

Table A.8: Summary statistics for winning member of the household

| | | **Winner** | **Spouse** |
|---|---|---|---|
| | | (1) | (2) |
| *Covariate* | *Statistic* | | |
| Wage Earnings | Mean | \$42,465 | \$33,037 |
| Employment | Prop. | 0.80 | 0.73 |
| Age | Prop. | 47.07 | 46.65 |
| Female | Prop. | 0.36 | 0.64 |
| Primary Earner | Prop. | 0.62 | 0.38 |
| Older Member | Prop. | 0.50 | 0.50 |
| Same Age | Prop. | 0.11 | 0.11 |
| | | $p < 0.001$ | |

*Notes:* This table presents a summary of the descriptive statistics in our sample of married winners, separately for the winner and spouse. All monetary values are reported in 2016 U.S. dollars, using the Consumer Price Index to adjust for inflation. All values are measured two years prior to the win year. At the bottom of the table, we report the $p$-value from an $F$-test that one or more of the covariate means is different between winners and their spouses.

Table A.9: Estimates of wealth effects: responses per 100,000 dollars in additional wealth

| Study<br>(1) | Outcome<br>(2) | Country<br>(3) | Primary Data Source(s) and Population<br>(4) | Time Period<br>(5) | Nature of Shock<br>(6) | Units of Effect Estimate<br>(7) | Effect Estimate (SE)<br>(8) | Source<br>(9) |
|---|---|---|---|---|---|---|---|---|
| *Panel A: Take-up of Retirement Benefits and Labor Market Exit* | | | | | | | | |
| Joulfaian and Wilhelm (1994) | Indicator for retirement | U.S. | Panel Study of Income Dynamics (PSID); 55 to 60 year old males | 1988 | Change in wealth due to inheritance | change in retirement propensity | 3.80 pp (NA) | Page 1219 |
| Sevak (2002) | Indicator for transitioning into retirement | U.S. | Health and Retirement Study (HRS); 55 to 60 year old males | 1992-1998 | Change in wealth due to change in values of stocks | change in retirement propensity | 3.88 pp (1.62) | Table 14, column 1 |
| Hurd, Reti, and Rohwedder (2009) | Reported probability of working beyond age 62 | U.S. | Health and Retirement Study (HRS); Stockholders aged 50 to 60 in 1992 | 1992-2002 | Change in wealth due to change in values of stocks | reported probability of working beyond age 62 | 1.48 pp (NA) | Table 4.10, stock owners in both waves, Waves 3-5 |
| Brown, Coile, and Weisbenner (2010) | Indicator for retiring earlier than expected | U.S. | Health and Retirement Study (HRS); 55 to 65 year old males | 1992-2002 | Change in wealth due to unexpected inheritance | change in retirement propensity | 10.33 pp (NA) | Table 3, column 5 |
| Gelber, Isen, and Song (2016) | Indicator for positive earnings | U.S. | Social Security Master Earnings File (MEF) and Master Beneficiary Record (MBR); 1916 to 1923 birth cohorts | 1978-2012 | Change in Old Age and Survivors Insurance (OASI) benefit formula | change in propensity for positive earnings | 6.5 pp (1.7) | Table 6, column 3 |
| Cesarini, Lindqvist, Notowidigdo, and Östling (2017) | Indicator for earnings above minimum threshold | Sweden | Administrative earnings and lottery winnings registers; 55 to 65 year olds | 1991-2010 | Change in wealth from winning a lottery | change in propensity for earnings above threshold | 2.36 pp (1.00) | Table 4, column 5 |
| Zhao and Burge (2017) | Indicator for labor force participation | U.S. | Health and Retirement Study (HRS); Several birth cohorts (born between 1920s and 1950s) | 1991-2010 | Change in housing wealth | change in labor force participation | 0.10 pp (0.03) | Table 8, column 1 |
| Disney and Gathergood (2018) | Indicator for retirement | U.K. | British Household Panel Survey (BHPS); 55 to 75 year old males | 1991-2009 | Change in housing wealth | change in retirement propensity | 6.23 pp (NA) | Table 8, columns 3 and 4 |
| *Panel B: Entrepreneurship and Self-Employment* | | | | | | | | |
| Holtz-Eakin, Joulfaian, and Rosen (1994) | Transition to Schedule C tax return | U.S. | Matched federal estate and income tax returns; Tax filers | 1981-1985 | Change in wealth due to inheritance | change in presence of Schedule C | 3.3 pp (NA) | Page 342 |
| Holtz-Eakin, Joulfaian, and Rosen (1994) | Transition to Schedule C tax return above $5,000 (1981) | U.S. | Matched federal estate and income tax returns; Tax filers | 1981-1985 | Change in wealth due to inheritance | change in presence of Schedule C above $5,000 (1981) | 1.19 pp (NA) | Table 3, column 1, 3rd set of results |
| Lindh and Ohlsson (1996) | Indicator for being self-employed | Sweden | Swedish Level of Living Survey; Individuals aged 15 to 75 | 1981 | Change in wealth due to inheritance or lottery | change in self-employment propensity | 17.7 pp (NA) | Page 1518 |
| Cesarini, Lindqvist, Notowidigdo, and Östling (2017) | Indicator for self-employment earnings above minimum threshold | Sweden | Administrative earnings and lottery winnings registers; 55 to 65 year olds | 1991-2010 | Change in wealth from winning a lottery | change in propensity for self-employment earnings above threshold | -0.10 (0.14) | Table 4, column 5 |
| Harding and Rosenthal (2017) | Transition to self-employment | U.S. | American Housing Survey (AHS); Household heads aged 20-65 with earnings above $5,000 (2014) | 1985-2013 | Change in housing wealth | change in self-employment propensity | 1.08 (NA) | Table 5, column 5 |

*Notes:* This table reports a summary of wealth effect estimates across past studies. In Panel A, we summarize the past literature estimating wealth effects on take-up of retirement benefits and labor market exit for older individuals. In Panel B, we summarize the past literature estimating wealth effects on entry into entrepreneurship and transition into self-employment. For each study, we report the relevant outcome studied (column 2), the country (column 3), the primary data sources and populations studied in the analysis (column 4), the time period in the study (column 5), the nature of the shock to wealth (column 6), the units of the response to the wealth shock (column 7), and the wealth effect estimate with the standard error in parenthesis (column 8), and the source of the estimate in the relevant study (column 9). In cases where a standard error is not available for the study, we report “(NA)”.

Table A.10: Effects of unearned income on take-up of retirement benefits and labor market exit

| Outcome | Value | Full Sample<br>(1) | Quartile 1<br>Pre-Win Income<br>(2) | Quartile 4<br>Pre-Win Income<br>(3) |
|---|---|---|---|---|
| | | **Take-Up of Retirement Benefits** | | |
| Claiming OASI Benefits | *Estimate* | 0.0169 | 0.0333 | 0.0114 |
| | *Standard Error* | (0.0061) | (0.0181) | (0.0115) |
| | *Counterfactual Mean* | 0.77 | 0.74 | 0.73 |
| | *Percentage Change* | 2.2 | 4.5 | 1.6 |
| | | **Labor Market Exit** | | |
| One-Year Exit | *Estimate* | 0.0720 | 0.0532 | 0.0581 |
| | *Standard Error* | (0.0078) | (0.0140) | (0.0152) |
| | *Counterfactual Mean* | 0.43 | 0.63 | 0.33 |
| | *Percentage Change* | 16.7 | 8.5 | 17.4 |
| Two-Year Exit | *Estimate* | 0.0790 | 0.0703 | 0.0678 |
| | *Standard Error* | (0.0086) | (0.0156) | (0.0164) |
| | *Counterfactual Mean* | 0.40 | 0.58 | 0.30 |
| | *Percentage Change* | 19.7 | 12.2 | 22.3 |
| Five-Year Exit | *Estimate* | 0.0721 | 0.0879 | 0.0290 |
| | *Standard Error* | (0.0144) | (0.0390) | (0.0251) |
| | *Counterfactual Mean* | 0.35 | 0.48 | 0.31 |
| | *Percentage Change* | 20.6 | 18.5 | 9.5 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on take up of retirement benefits and labor market exit for winners aged 62-64. The dependent variables are binary indicators for the receipt of OASI benefits and labor force exit respectively. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income ($n_t$) as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\rho^{w,\ell}$ and $\phi^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$, and then form the ratio to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by $10,000. The counterfactual means correspond to the fraction of winners that would have taken up retirement benefits or exited the labor market in the absence of winning (see Appendix E for details). The row "Percentage Change" reports the effect per $10,000 in percentage change terms relative to the counterfactual mean.

Table A.11: Effects of unearned income on entrepreneurship and self-employment

| Outcome | Value | Full Sample<br>(1) | Quartile 1 Pre-Win Income<br>(2) | Quartile 4 Pre-Win Income<br>(3) |
|---|---|---|---|---|
| Transition to Low-Paying SE | *Estimate* | 0.0107 | 0.0092 | 0.0115 |
| | *Standard Error* | (0.0006) | (0.0025) | (0.0009) |
| | *Counterfactual Mean* | 0.03 | 0.05 | 0.03 |
| | *Percentage Change* | 31.1 | 17.1 | 42.1 |
| Transition to High-Paying SE | *Estimate* | -0.0004 | -0.0009 | -0.0011 |
| | *Standard Error* | (0.0004) | (0.0012) | (0.0006) |
| | *Counterfactual Mean* | 0.01 | 0.01 | 0.02 |
| | *Percentage Change* | -2.9 | -6.3 | -6.3 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on the propensity to start a business associated with annual profits of \$15,000 or less (low-paying SE), or a business with profits of more than \$15,000 (high-paying SE). The estimation sample is restricted to winners and not-yet winners in paid-employment at event time $w-2$. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income $(n_t)$ as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\rho^{w,\ell}$ and $\phi^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1,2,3,4,5\}$, and then form the ratio to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by \$10,000. The counterfactual means correspond to the fraction of employed winners that would have received income from self-employment in the absence of winning (see Appendix E for details). The row "Percentage Change" reports the effect per \$10,000 in percentage change terms relative to the counterfactual mean.

Table A.12: Effects of wealth on job mobility

| Outcome | Value | Full Sample (1) | Quartile 1 Pre-Win Income (2) | Quartile 4 Pre-Win Income (3) |
|---|---|---|---|---|
| Any Job-to-Job Move | *Estimate* | 0.0004 | 0.0033 | -0.0043 |
| | *Standard Error* | (0.0018) | (0.0052) | (0.0030) |
| | *Counterfactual Mean* | 0.46 | 0.67 | 0.36 |
| | *Percentage Change* | 0.1 | 0.5 | -1.2 |
| Downward Move | *Estimate* | 0.0096 | 0.0123 | 0.0011 |
| | *Standard Error* | (0.0017) | (0.0053) | (0.0026) |
| | *Counterfactual Mean* | 0.23 | 0.31 | 0.19 |
| | *Percentage Change* | 4.3 | 4.0 | 0.6 |
| Upward Move | *Estimate* | -0.0092 | -0.0090 | -0.0054 |
| | *Standard Error* | (0.0016) | (0.0052) | (0.0023) |
| | *Counterfactual Mean* | 0.24 | 0.36 | 0.17 |
| | *Percentage Change* | -3.9 | -2.5 | -3.2 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of wealth on the frequency and direction of job-to-job moves. The estimation sample is restricted to winners and not-yet winners in paid-employment pre- and post-win. Outcomes are defined as binary and equal to 1 if the firm is either different from, or higher or lower ranked than the firm prior to winning the lottery. Firms are ranked by the mean wage paid to its employees. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2. For each outcome, we then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3, 4, 5\}$ to recover the mean effect of an extra dollar of wealth. Column 1 reports mean effects of an extra dollar of wealth for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of wealth for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by \$100,000. The counterfactual means correspond to the job mobility rates that would have occurred in the absence of winning (see Appendix E for details). The row "Percentage Change" reports the effect per \$100,000 in percentage change terms relative to the counterfactual mean.

Table A.13: Effects of unearned income on marriages and divorce

| Outcome | Value | Full Sample<br>(1) | Quartile 1 Pre-Win Income<br>(2) | Quartile 4 Pre-Win Income<br>(3) |
|---|---|---|---|---|
| New Marriage | *Estimate* | 0.0174 | 0.0388 | 0.0009 |
| | *Standard Error* | (0.0018) | (0.0056) | (0.0029) |
| | *Counterfactual Mean* | 0.14 | 0.14 | 0.15 |
| | *Percentage Change* | 12.3 | 27.1 | 0.6 |
| Divorce | *Estimate* | -0.0143 | -0.0309 | -0.0122 |
| | *Standard Error* | (0.0022) | (0.0087) | (0.0032) |
| | *Counterfactual Mean* | 0.11 | 0.17 | 0.09 |
| | *Percentage Change* | -12.4 | -18.1 | -13.4 |

*Notes:* This table presents estimates of the mean effect of an extra dollar of unearned income on the propensity to enter or leave marriage. The estimation sample is restricted to winners and not-yet winners that are tax filers. When we study the effect on new marriages (divorce), we further restrict the sample to individuals that were not married (married) in $w-2$. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using unearned income $(n_t)$ as the endogenous variable. For each outcome, we then take cohort-size-weighted averages of $\rho^{w,\ell}$ and $\phi^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1,2,3,4,5\}$, and then form the ratio to recover the mean effect of an extra dollar of unearned income. Column 1 reports mean effects of an extra dollar of unearned income for the full analysis sample. In columns 2 and 3, we report mean effects of an extra dollar of unearned income for subsamples of winners falling into the first and fourth quartile of the pre-win distribution of per-adult adjusted gross income. We use the delta method to calculate standard errors (reported in parenthesis), clustering on winner. To ease interpretability, each estimate is scaled by $10,000. The counterfactual means correspond to the marriage and divorce rates that would have occurred in the absence of winning (see Appendix E for details). The row "Percentage Change" reports the effect per $10,000 in percentage change terms relative to the counterfactual mean.

**Robustness of MPE estimates in IRS (2001) across estimators.** To explore whether the finding of relatively small MPE estimates in Imbens, Rubin, and Sacerdote (2001) is robust to the choice of estimator and methodology, we use their micro data (which is publicly available) to estimate and compare their estimates of MPEs to those obtained using our estimator.

Imbens, Rubin, and Sacerdote (2001) run a first-difference (FD) regression that specifies a linear relationship between winners' earnings and lottery winnings. Abstracting from time-varying controls, their regression model can be expressed as

$$\Delta Y_i = \alpha + \beta X_i + \varepsilon_i, \tag{A.1}$$

where $\Delta Y_i$ is the difference in earnings of individual $i$ between the periods after and before the lottery win, $X_i$ is the annuitized pre-tax lottery payment, and $\beta$ is the coefficient of interest (i.e., the MPE). The FD approach is based on a comparison of winners before and after they win. We add a second difference to this estimator to eliminate common time effects. More specifically, as explained in detail in Section 3, we use a difference-in-difference instrumental variable (DiD-IV) estimator that uses the pre-win earnings of individuals who later win the lottery to take a second difference to eliminate the common time effects.

Table A.14 presents a comparison of MPE estimates across the two estimators. For Imbens, Rubin, and Sacerdote (2001), we present results for both their full winners sample and their preferred estimation sample that excludes the largest winners. The first two columns replicate the findings reported in Imbens, Rubin, and Sacerdote (2001). The second column shows their headline estimate for the MPE which comes from a subsample that excludes the biggest winners.[51] The third and fourth columns show the estimates we obtain when we apply our DiD-IV estimator to their data. By comparison, we find that the choice of estimator does not materially affect the finding of relatively small MPE estimates.

Table A.14: Robustness of MPE estimates in IRS (2001) across estimators

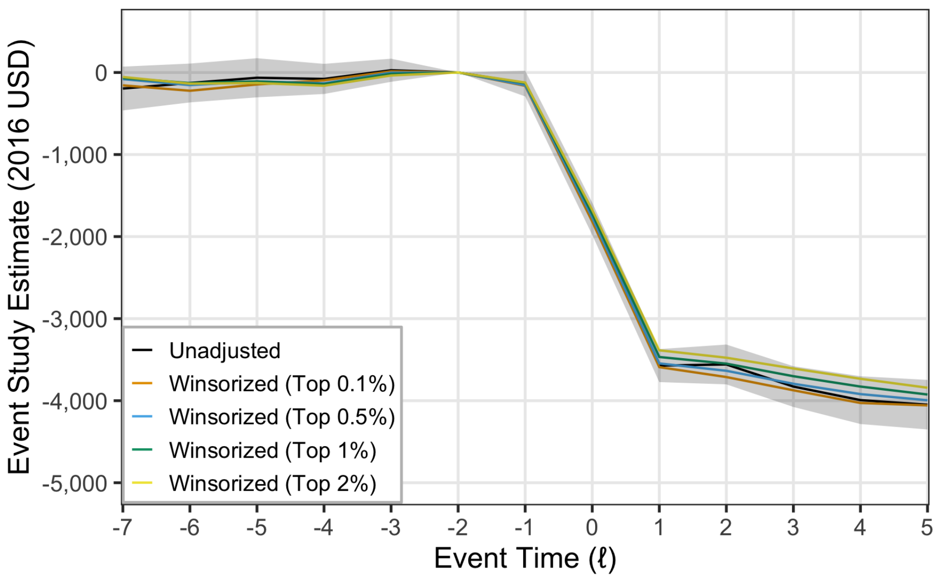
| | FD Estimator | | DiD-IV Estimator | |
|---|---|---|---|---|
| | Full Winners Sample | Sample Excluding Large Winners | Full Winners Sample | Sample Excluding Large Winners |
| | (1) | (2) | (3) | (4) |
| Effect Estimate | -0.048 | -0.112 | -0.068 | -0.080 |
| (SE) | (0.010) | (0.029) | (0.024) | (0.041) |

*Notes:* This table presents a comparison of MPE estimates from Imbens, Rubin, and Sacerdote (2001) (IRS (2001)) across two different choices of estimator. Columns 1 and 2 correspond to estimates using the first-difference (FD) estimator of IRS (2001) when we re-estimate their model specifications. The specification for the sample of all winners (Full Winners Sample) corresponds to IRS (2001) Table 4, Specification VI while the specification for the sample of winners with annual pre-tax prize payouts less than $100,000 (Sample Excluding Large Winners) corresponds to IRS (2001) Table 4, Specification VIII. The FD estimates are identical to the estimates reported in Imbens, Rubin, and Sacerdote (2001) (IRS (2001)) Table 4, Specifications VI and VIII, divided by 0.9 to reflect the fact that annual prize payments in IRS (2001) are for 20 years and not for the remaining lifetime of the winner. This is consistent with IRS (2001)'s approach and explained in their footnote 20. Columns 3 and 4 correspond to estimates using our difference-in-difference instrumental variable (DiD-IV) estimator, estimated on the same samples as above. Using the data of IRS (2001), these estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, using the annuitized pre-tax lottery payment as the endogenous variable. We then take cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, and then take the mean across estimates for post-win event times $\{1, 2, 3\}$ to recover the mean effect of an extra dollar of unearned income. We are constrained to three post-win event times by the number of distinct cohorts of winners in the data of IRS (2001).

[51]The FD estimates we report are identical to the estimates in Imbens, Rubin, and Sacerdote (2001) (IRS (2001)) Table 4, Specifications VI and VIII, divided by 0.9 to reflect the fact that annual prize payments in IRS (2001) are for 20 years and not for the remaining lifetime of the winner. This is consistent with IRS (2001)'s approach as explained in their footnote 20.

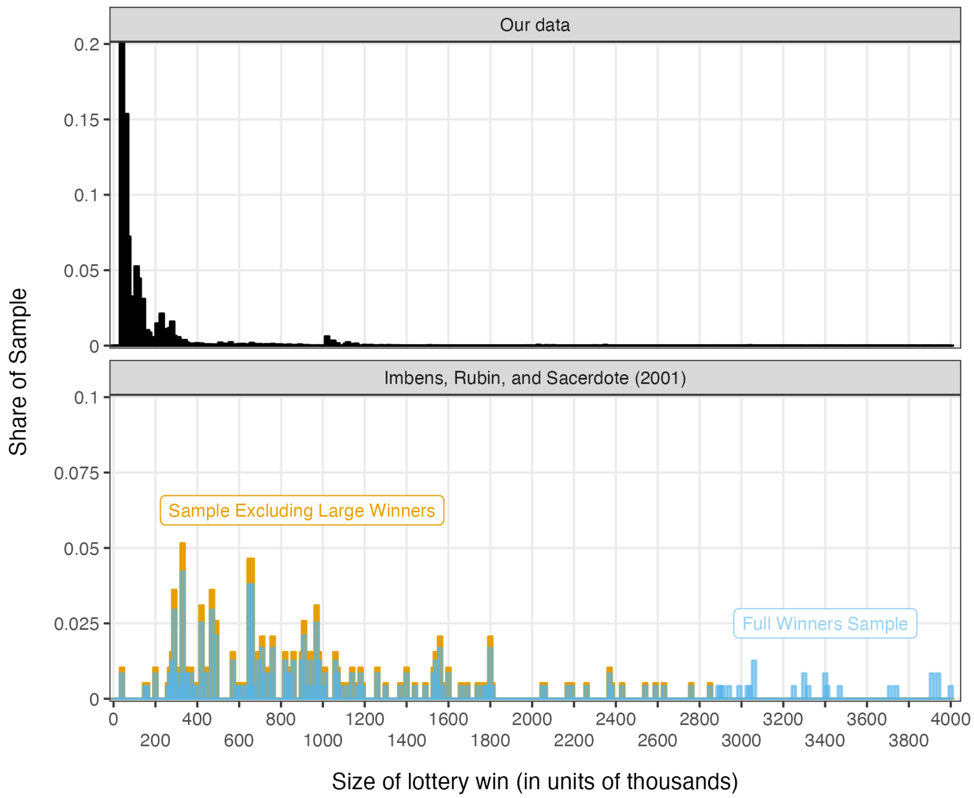
# B Appendix Figures

Figure B.1: Unadjusted versus winsorized estimates of effect of winning



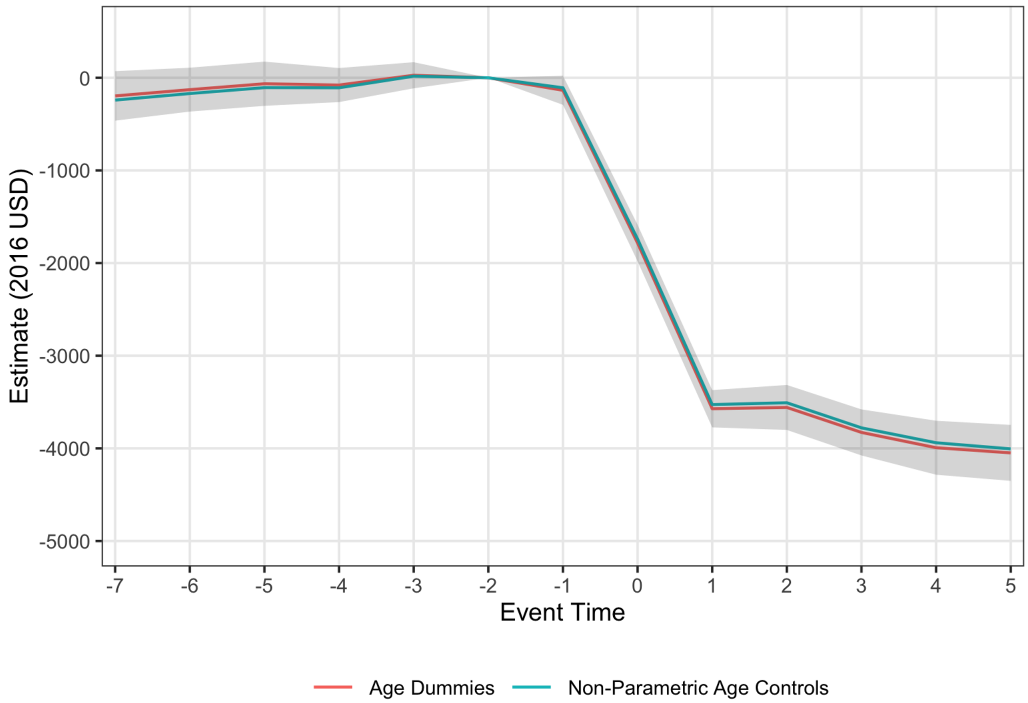

*Notes:* In this figure, we summarize the estimates of the effect of winning the lottery on winner wage earnings corresponding to an unadjusted measure of wage earnings ("Unadjusted") as well as various winsorized measures. Each set of estimates is based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed for the "Unadjusted" estimates, clustering on winner. Throughout, we use $w-2$ as the omitted event time.

Figure B.2: Distribution of pre-tax winnings: our data and Imbens, Rubin, and Sacerdote (2001)



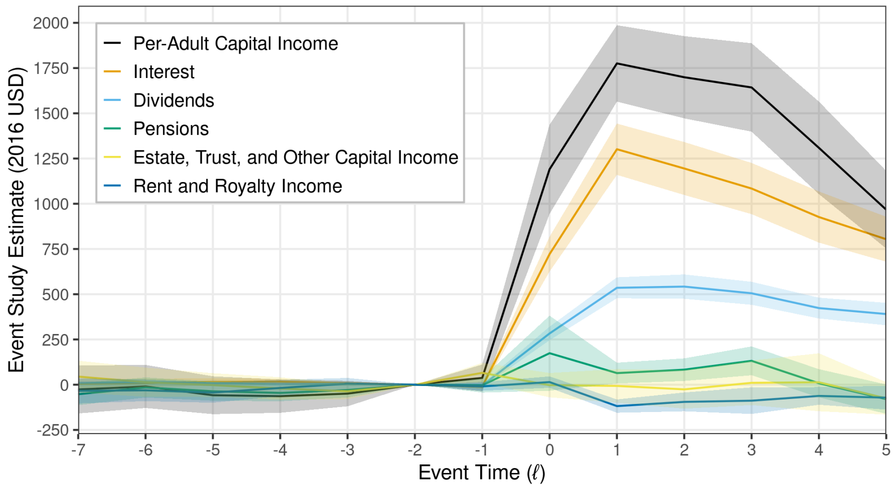

*Notes:* This figures presents the distribution of pre-tax winnings of the household in our baseline estimation sample (top panel; also summarized in column 1 of Appendix Table A.1) as well as in two key estimation samples of Imbens, Rubin, and Sacerdote (2001) (bottom panel). Each bar corresponds to the share of the overall sample found in a particular $10,000 interval. In the bottom panel, the "Full Winners Sample" data series corresponds to the (calculated) pre-tax amounts won by all winners in the sample of Imbens, Rubin, and Sacerdote (2001); the "Sample Excluding Large Winners" restricts attention, following Imbens, Rubin, and Sacerdote (2001), to winners with annual pre-tax payouts of less than $100,000 (in 1986 U.S. dollars). All values on the $x$-axis are in 2016 U.S. dollars. We convert units in Imbens, Rubin, and Sacerdote (2001) from pre-tax annual prize payouts in 1986 U.S. dollars to pre-tax winnings in 2016 U.S. dollars in two steps. First, we convert the annual pre-tax payouts to total pre-tax winnings by calculating the net present value of the 20-year payment stream using the average real interest rate from 1986 to 2006, the relevant time period for the annual payouts in their study. Next, we convert from 1986 U.S. dollars into 2016 U.S. dollars using the Consumer Price Index.

Figure B.3: Comparison of approaches to control for life-cycle effects

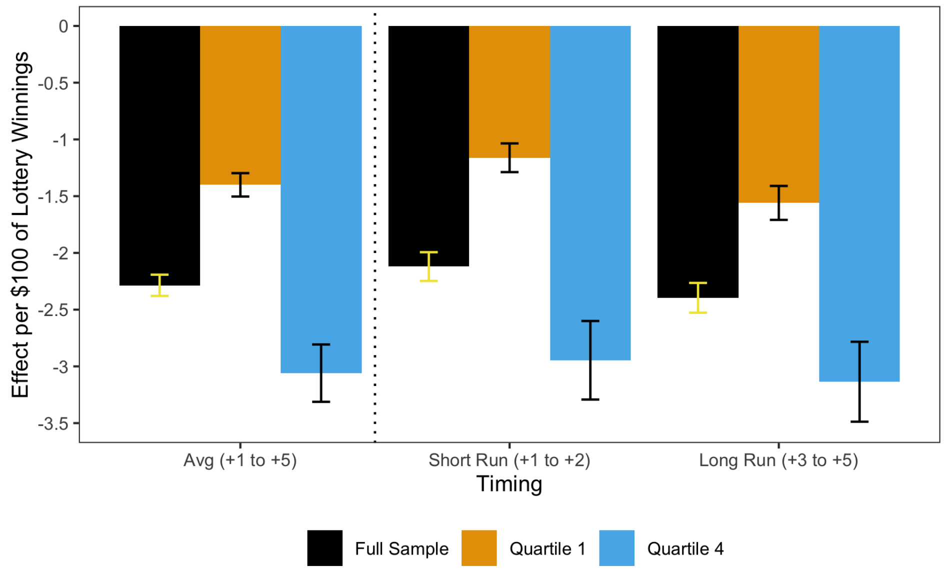



*Notes:* This figure presents estimates of the impact of winning on winner wage earnings. For the red series, each set of estimates is based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. For the blue series, we instead use a non-parametric estimator due to Callaway and Sant'Anna (2021) which adjusts for age differences between current and later winners through an inverse-probability-weighted (IPW) DiD estimator. 90 percent confidence intervals are displayed for the red series, clustering on winner. Throughout, we use $w-2$ as the omitted event time.

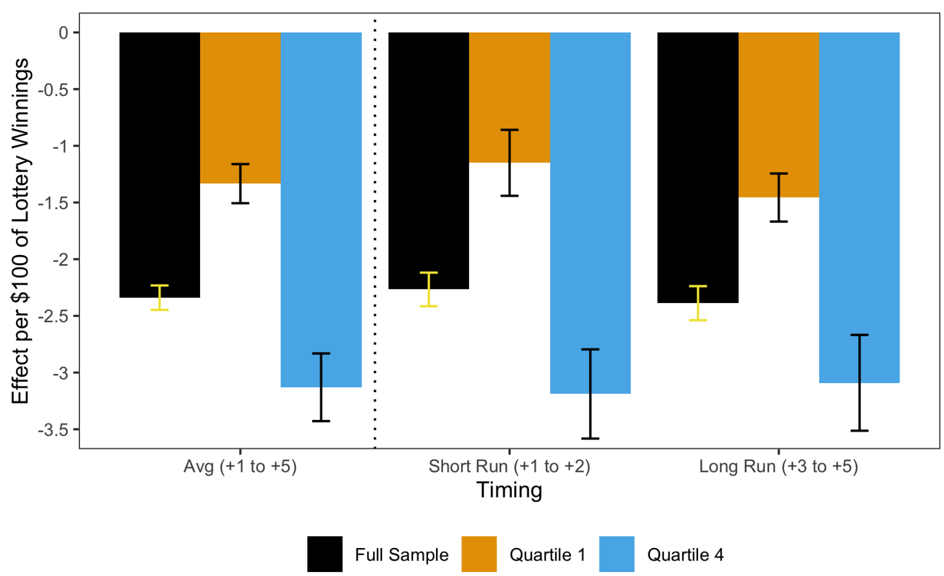
Figure B.4: Effect of winning on per-adult capital income and its components




*Notes:* This figure presents estimates of the impact of winning on per-adult capital income, as well as all of the components comprising capital income. Each set of estimates is based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. Throughout, we use $w - 2$ as the omitted event time.




(a) Winner Wage Earnings

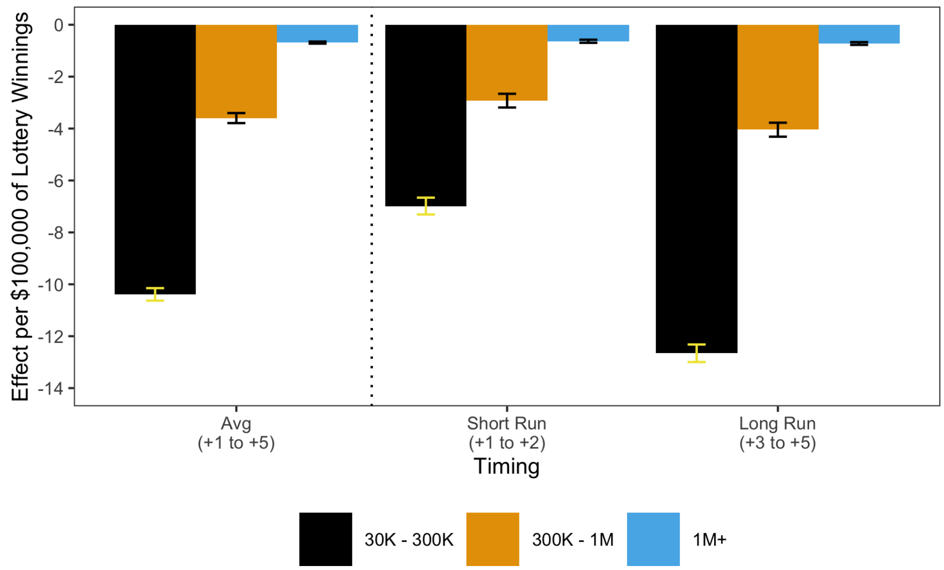



(b) Per-Adult Total Labor Earnings

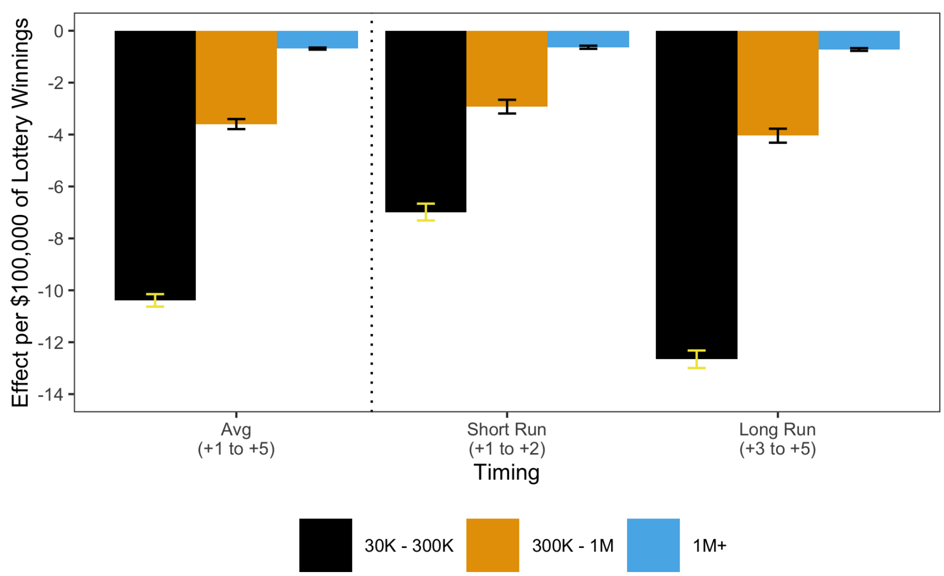
Figure B.5: Wealth effects across time and pre-win income

*Notes:* This figure presents estimates of the mean effect of an extra dollar of wealth on (a) winner wage earnings and (b) per-adult total labor earnings. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, for each outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for all post-win event times $\{1, 2, 3, 4, 5\}$ ("Avg (+1 to +5)"), a shorter-run set of post-win event times $\{1, 2\}$ ("Short Run (+1 to +2)"), and a longer-run set of post-win event times $\{3, 4, 5\}$ ("Long Run (+3 to +5)"). In addition, for each temporal average, we report wealth effects for the full analysis sample ("Full Sample") as well as for the subsample of winners falling in the first ("Quartile 1") and fourth ("Quartile 4") quartile of the pre-win distribution of per-adult adjusted gross income. To ease interpretability, we scale earnings responses by $100. 90 percent confidence intervals are displayed, clustering on winner.

Figure B.6: Wealth effects by prize size over time

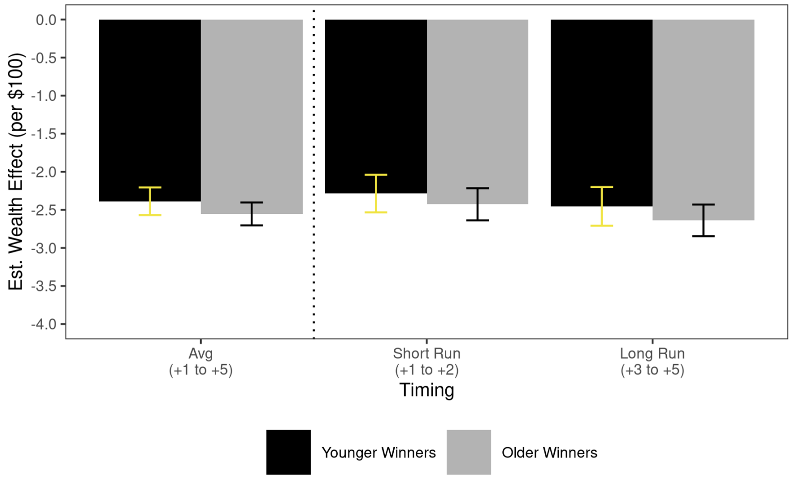



(a) Per-Adult Total Labor Earnings




(b) Total Employment

*Notes:* This figure presents estimates of the mean effect of an extra dollar of wealth on a) per-adult total labor earnings and b) total employment. These estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2, for each outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for all post-win event times $\{1, 2, 3, 4, 5\}$ ("Avg (+1 to +5)"), a shorter-run set of post-win event times $\{1, 2\}$ ("Short Run (+1 to +2)"), and a longer-run set of post-win event times $\{3, 4, 5\}$ ("Long Run (+3 to +5)"). In addition, for each temporal average, we report wealth effects separately by prize size. To ease interpretability, we scale earnings responses by \$100. In the case of employment responses, we scale each estimate by \$100,000. 90 percent confidence intervals are displayed, clustering on winner.

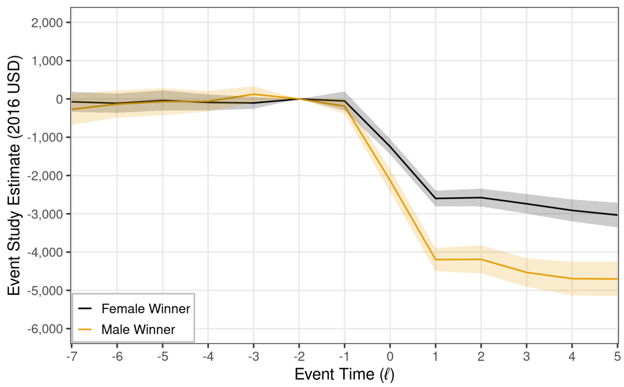



(c) Per-Adult Total Labor Earnings

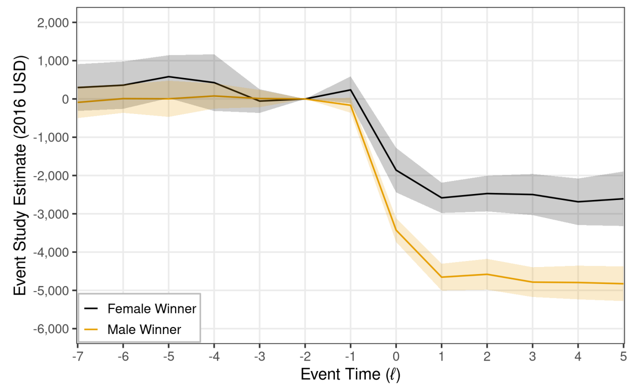
Figure B.7: Wealth effects by age of winner

*Notes:* This figure presents estimates of the mean effect of an extra dollar of wealth on per-adult total labor earnings. The estimates are calculated by first estimating a 2SLS regression, as described in Section 3.2 for the binary outcome, then taking cohort-size-weighted averages of $\beta^{w,\ell}$ for each event time $\ell$, then taking the mean across estimates for all post-win event times $\{1, 2, 3, 4, 5\}$ ("Avg (+1 to +5)"), a shorter-run set of post-win event times $\{1, 2\}$ ("Short Run (+1 to +2)"), and a longer-run set of post-win event times $\{3, 4, 5\}$ ("Long Run (+3 to +5)"). For each temporal average, we report wealth effects for the subsample of younger winners (age 30 - 46) and older winners (age 47 - 64). 90 percent confidence intervals are displayed, clustering on winner.

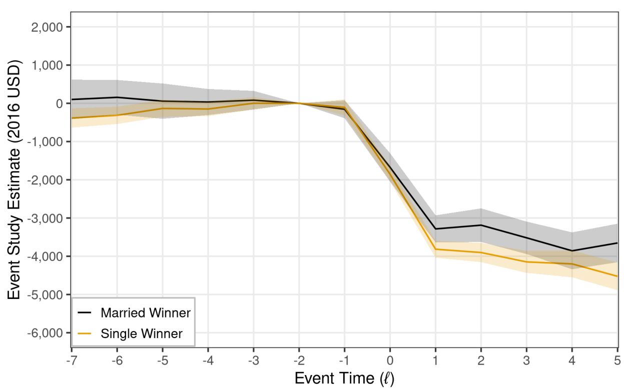
Figure B.8: Effect of winning by gender and marital status



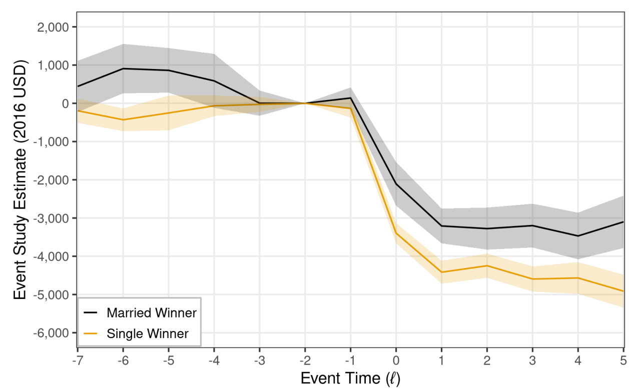

(a) Wage earnings of male and female winners




(b) Total per-adult labor income of male and female winners




(c) Wage earnings of single and married winners



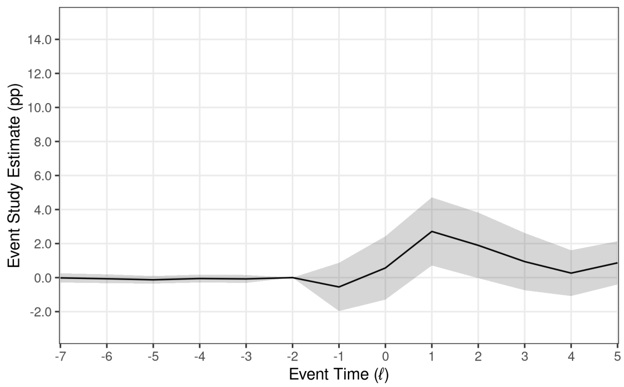

(d) Total per-adult labor income of single and married winners

*Notes:* This figure presents estimates of the impact of winning on earnings by gender and marital status of the winner. Each set of estimates is based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. Throughout, we use $w-2$ as the omitted event time.

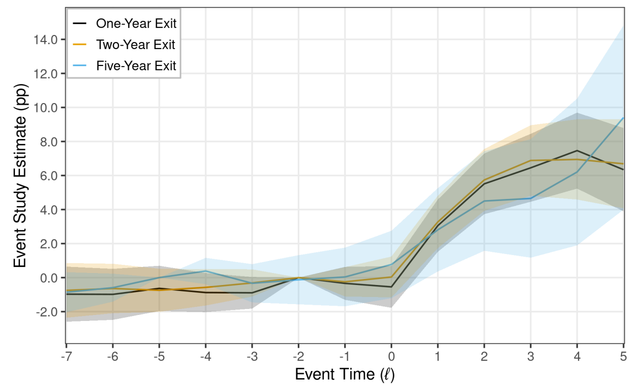
Figure B.9: Effect of winning on wage earnings of the winner and his or her spouse


*Notes:* This figure presents estimates of the impact of winning on wage earnings for winners and non-winning spouses. The estimation sample is restricted to married couples. Each set of estimates is based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. Throughout, we use $w-2$ as the omitted event time.

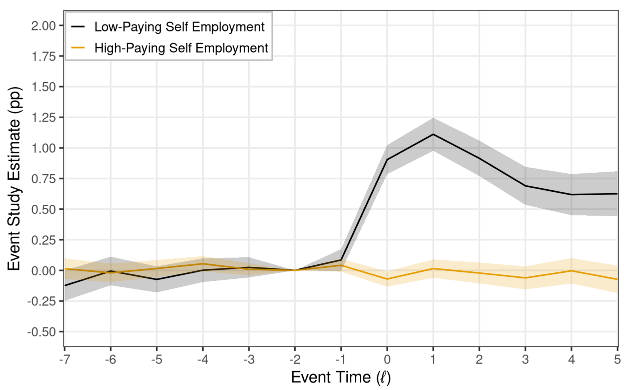
Figure B.10: Effect of winning on labor market dynamics



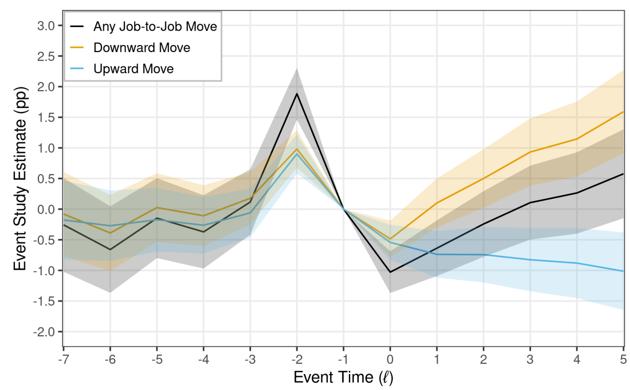

(a) Claiming social security benefits




(b) Labor market exit



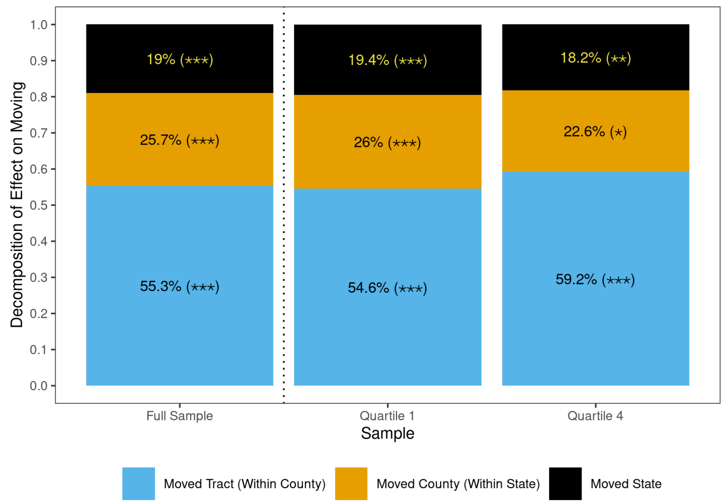

(c) Transition from paid employment into self-employment




(d) Job-to-job transitions

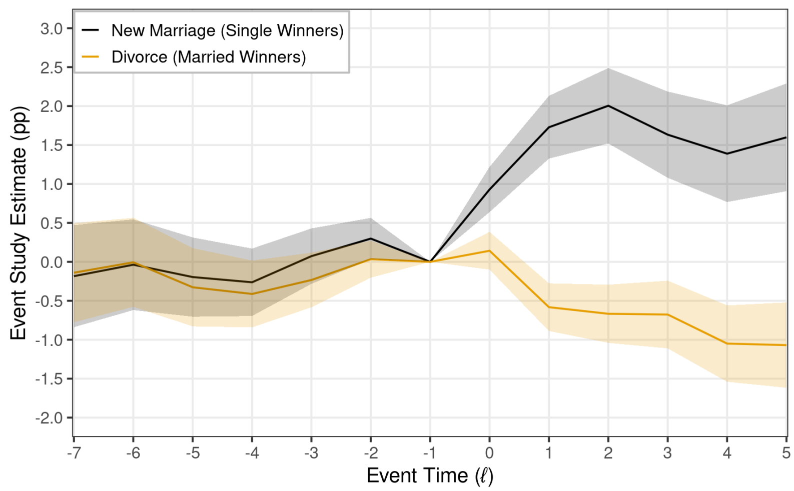
*Notes:* This figure presents estimates of the impact of winning on various outcomes related to labor market dynamics, based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. In the regressions for subfigures (a)-(c),, we use $w-2$ as the omitted event time. In the regressions for subfigure (d), by construction of the outcome variables, the regression coefficients in $w-2$ are zero. To allow for potentially different annual transition rates between current and later winners, we then normalize the effect in event time $w-1$ to be zero in these regressions.

Figure B.11: Decomposition of the moving response by geographic unit




*Notes:* This figure presents a decomposition of the impact of winning on the propensity to move Census tracts in terms of the distance moved. Given that Census tracts are nested within Census counties, and Census counties are nested within U.S. states, we decompose overall Census tract moving into a) moves across Census tract but within Census county, b) moves across Census county but within state, and c) moves across state. For each, we define a corresponding binary outcome and estimate a version of equation (3.4) (as described in Section 3.1), focusing on effects in the first year post-win. We then divide each estimate by the total effect on across-tract moving, as reported in Figure 5.1, to arrive at the share of the total moving effect. We report this decomposition separately for the full analysis sample ("Full Sample") as well as for the subsample of winners falling in the first ("Quartile 1") and fourth ("Quartile 4") quartile of the pre-win distribution of per-adult adjusted gross income. Next to each share, we also summarize the statistical significance of the underlying estimate, with ⋆ ⋆ ⋆ for significance at the 99% level, ⋆⋆ for significance at the 95% level, and ⋆ for significance at the 90% level.

Figure B.12: Effect of winning on family formation and stability




*Notes:* This figure presents estimates of the impact of winning on family formation and stability. The estimation sample is restricted to winners and not-yet winners that are tax filers. When we study the effect on new marriages (divorce), we further restrict the sample to individuals that were not married (married) in $w-2$. Each set of estimates is based on estimating a version of equation (3.4) (as described in Section 3.1) for each outcome, and then taking cohort-size-weighted averages of $\rho^{w,\ell}$ for each event time $\ell$. 90 percent confidence intervals are displayed, clustering on winner. Throughout, we use $w-2$ as the omitted event time.

## C Definition of variables and their sources

- **Age:** Age of an individual in calendar year $t$ is measured as the difference between $t$ and birth year reported for each de-identified Taxpayer Identification Number (TIN) on Data Master-1 (DM-1) from the Social Security Administration.

- **Gender:** Gender of an individual is reported for each de-identified TIN on DM-1 from the Social Security Administration.

- **Marital status:** For tax filers, marital status is determined based on the filing status observed on Form 1040 at the tax-paying unit (TPU) level. All non-filers are treated as single, in line with Cilke (1998), Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011), and subsequent work utilizing administrative tax records in the U.S.

- **Form W-2G reported gross winnings**: Reported in box 1 ("Gross winnings") of Form W-2G for each de-identified TIN. In the administrative data, the source of the winnings is labeled, with a separate category for payments from state lotteries. We only utilize Form W-2G issued by state lotteries for lottery payments.

- **Wage earnings**: Reported in box 1 of Form W-2 for each de-identified TIN. For individuals receiving multiple W-2s in a given calendar year (from multiple employers), we sum across all W-2s in the same calendar year. This measure of wage earnings corresponds to total taxable remuneration for labor services of a direct employee, and includes wages, tips, salary, and taxable fringe benefits. For individuals with no Form W-2 in a given calendar year, wage earnings are set to 0.

- **Employment**: A binary indicator for having positive wage earnings (as defined above) for each de-identified TIN in each calendar year.

- **Employer:** For individuals linked to a single firm through Form W-2, this is the identity of their employer (which is a de-identified employer ID number, or EIN). For individuals linked to multiple firms through Form W-2, this is the identity of the highest-paying employer.

- **Per-adult wage earnings**: For single workers, this is equivalent to wage earnings as defined above. For married workers, this is the sum of own and spouse wage earnings in a given calendar year, divided by 2 (that is, per-adult). We use Form 1040 filing in order to identify married workers and make spousal links.

- **Self-employment income**: For single tax filers, we define self-employment income as the sum of self-employment business income (Form 1040), farm income (Form 1040), and partnership income (Schedule E) in a given calendar year. For married tax filers, this is self-employment income of the TPU, divided by 2 (that is, per-adult). For non-filers, self-employment income is set to 0.

- **Total labor earnings**: The sum of per-adult wage earnings and self-employment income, both as defined above.

- **Total employment**: A binary indicator for having non-zero total labor earnings (as defined above) for each de-identified TIN in each calendar year.
- **Capital income**: For single tax filers, we define capital income as the sum of dividend income (Form 1040), interest income (Form 1040), pension and annuity income (Form 1040), rental and royalty income (Form 1040 Schedule E), and non-labor income from estates, trusts, farms, and mortgage investments (Form 1040 Schedule E) in a given calendar year. For married tax filers, this is capital income of the TPU, divided by 2 (that is, per-adult). For non-filers, capital income is set to 0.
- **Social Security benefit payments:** For single tax filers, we define Social Security benefit payments as the gross Social Security benefit payments reported on Form 1040 in a given calendar year. For married tax filers, this is Social Security benefit payments of the TPU, divided by 2 (that is, per-adult). For non-filers, Social Security benefit payments is set to 0.
- **Unemployment insurance payments:** For single tax filers, we define unemployment insurance payments as the gross unemployment insurance payments reported on Form 1040 in a given calendar year. For married tax filers, this is unemployment insurance payments of the TPU, divided by 2 (that is, per-adult). For non-filers, unemployment insurance payments is set to 0.
- **Gross income**: For single tax filers, this is the sum of total labor earnings, capital income, Social Security payments, and unemployment insurance payments. For married tax filers, this is the gross income of the TPU, divided by 2 (that is, per-adult).
- **Adjusted gross income**: For single tax filers, this is adjusted gross income as reported on Form 1040. For married tax filers, this is the adjusted gross income reported for the TPU, divided by 2 (that is, per-adult). For non-filers, adjusted gross income is set to 0.
- **Homeownership:** A binary indicator for the receipt of at least one Form 1098 for each de-identified TIN in each calendar year. All individuals paying mortgage interest in excess of $600 (per mortgage) in a calendar year receive a Form 1098 . Using such a binary indicator as a proxy for homeownership is in line with past work utilizing tax return data in the U.S. (e.g., Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan, 2011).
- **Total income taxes**: For single tax filers, this is the combined federal and state income taxes owed, as calculated using the combined federal and state tax calculator of Bakija (2019). For married tax filers, this is the combined federal and state income taxes owed for the TPU, divided by 2 (that is, per-adult). For non-filers, total income taxes are set to 0.
- **Marginal tax rate:** The change in total income taxes of the TPU (as defined above) from a mechanical $1 increase in wage earnings (i.e., a forward difference approximation), using the combined federal and state tax calculator of Bakija (2019).
- **Census tract:** The 2010 Census tract (Census-defined geographic aggregation) corresponding to the ZIP Code of an individual in calendar year $t$. To map ZIP Code to 2010 Census tract, we use

quarterly crosswalk files provided in HUD crosswalk files, accessible here: https://www.huduser.gov/portal/datasets/usps_crosswalk.html.

- **Census county:** The 2010 Census county corresponding to the ZIP Code of an individual in calendar year $t$. A Census county nests multiple Census tracts.

- **Tract-level local labor market measures:** We consider several measures of local labor markets, each defined as a time-invariant measure for each 2010 Census tract. The source of these tract-level measures is the Opportunity Atlas, as introduced in Chetty, Friedman, Hendren, Jones, and Porter (2020). Details on each measure, and the crosswalk files, are accessible here: https://opportunityinsights.org/paper/the-opportunity-atlas/. We briefly describe each measure below, taking descriptions from: https://opportunityinsights.org/wp-content/uploads/2019/07/Codebook-for-Table-9.pdf

  - *Wage growth*: Wage growth for high school graduates. Wages are constructed by dividing the average high school graduate annual earnings by the product of overall average weekly hours worked and 52. High school graduate wage growth is then computed as the difference in logarithms between high school graduate wages in 2010-2014 and school graduate wages in 2005-2009. Wages are measured in the 2005-2009 and 2010-2014 American Community Surveys.
  - *Job growth*: Average annualized job growth rate over the time period 2004 to 2013. Constructed using LODES - WAC data files provided by the Census Bureau. Data unavailable for Massachusetts and Washington D.C.
  - *Job density*: Number of jobs per square mile in each tract. Constructed using LODES - WAC data files provided by the Census Bureau.
  - *Total jobs*: Total number of jobs in own and neighboring tracts whose centroids fall within a radius of 5 miles from own tract centroid. Constructed using information from the Workplace Area Characteristics (WAC) data files in the LEHD Origin-Destination Employment Statistics (LODES) provided by the Census Bureau.
  - *High-paying jobs*: Number of jobs with earnings greater than $3,333 per month in own and neighboring tracts whose centroids fall within a radius of 5 miles from own tract centroid. Constructed using LODES - WAC data files provided by the Census Bureau.
  - *Employment rate*: The rate of employment computed as total employed population (the sum of employed females and employed males) divided by the total population 16 years and over. Obtained from 2000 Decennial Census
  - *Short commute*: Share of workers 16 years and over who do not work at home whose commute is shorter than 15 minutes. Measured in the 2006-2010 ACS.
  - *Commute time*: Mean commute time for workers over 16 years old in the tract, as measured in the 2000 Decennial Census.

- *Population density*: Number of residents per square mile, calculated by dividing the total tract level population in the Decennial Census from 2010 with tract land area given in square miles from the 2010 Census Gazetteer Files

- **Tract-level neighborhood quality measures:** We consider several measures of neighborhood quality, each defined as a time-invariant measure for each 2010 Census tract. The descriptions and sources of these tract-level measures are as follows:
  - *Opportunity Atlas* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): Average family income for children with parents at the 25th percentile of income; source:`https://opportunityinsights.org/paper/the-opportunity-atlas/`
  - *Childhood Opportunity Index* (Noelke, McArdle, Baek, Huntington, Huber, Hardy, and Acevedo-Garcia, 2020): Omnibus index of neighborhood quality, with a focus on conditions that encourage upward mobility of children; source: `https://data.diversitydatakids.org/dataset/coi20-child-opportunity-index-2-0-database`
  - *Area Deprivation Index* (Kind and Buckingham, 2018): Omnibus index of neighborhood disadvantage, with a focus on susceptibility to disease and poor health; source: `https://www.neighborhoodatlas.medicine.wisc.edu/`
  - *Poverty rate* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): Share of individuals in the tract below the federal poverty line, measured in the 2006-2010 ACS; source: `https://opportunityinsights.org/paper/the-opportunity-atlas/`
  - *College attainment* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): Number of people aged 25 or older who have a bachelor's degree, master's degree, professional school degree, or doctorate degree, divided by the total number of people aged 25 or older in a Census tract. Estimated using the 2006-2010 ACS; source: `https://opportunityinsights.org/paper/the-opportunity-atlas/`
  - *Test scores* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): Mean 3rd grade math test scores in 2013; source: `https://opportunityinsights.org/paper/the-opportunity-atlas/`
  - *Teen birth* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): Fraction of women who grew up in the given tract who ever claimed a child who was born when they were between the ages of 13 and 19 as a dependent at any point; source: `https://opportunityinsights.org/paper/the-opportunity-atlas/`
  - *Single parents* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): The number of households with females heads (and no husband present) or male heads (and no wife present) with own children under 18 years old present divided by the total number of households with own children present. Estimated using the 2006-2010 ACS; source: `https://opportunityinsights.org/paper/the-opportunity-atlas/`
  - *Median rent (2BR)* (Chetty, Friedman, Hendren, Jones, and Porter, 2020): The median gross rent for renter-occupied housing units with two bedrooms that pay cash rent (from the 2011-2015

ACS); source: https://opportunityinsights.org/paper/the-opportunity-atlas/

## D Decomposition into intensive and extensive margin contributions

**Preliminaries.** For an earnings variable $Y$, let $Y_{i,t}(1)$ and $Y_{i,t}(0)$ be the potential outcomes of an individual that has experienced or has not experienced her first-observed win, respectively. Furthermore, let $\Lambda_{i,t} \equiv \mathbf{1}\{Y_{i,t} > 0\}$ be a binary random variable indicating whether an individual works. Observed outcomes are linked to potential outcomes as follows

$$\begin{aligned} Y_{i,t} &= Y_{i,t}(0) + D_{i,t}(Y_{i,t}(1) - Y_{i,t}(0)) \\ \Lambda_{i,t} &= \Lambda_{i,t}(0) + D_{i,t}(\Lambda_{i,t}(1) - \Lambda_{i,t}(0)), \end{aligned} \tag{D.1}$$

where $D_{i,t}$ is an indicator variable that takes value 1 if individual $i$ experiences the first-observed lottery win by year $t$.

For simplicity, we focus on a particular cohort $w$ and event time $\ell > 0$. Specifically, we are interested in decomposing the effect of winning in year $w$ on earnings $Y$ in the event time $\ell > 0$ into extensive- and intensive-margin responses. It is well known that under a common trends assumption, the average effect of winning in $w$ in the event time $\ell$ can be identified with a DiD estimator as follows:

$$\begin{aligned} \rho^{w,\ell} &\equiv \mathbb{E}\left[Y_{i,w+\ell}(1) - Y_{i,w+\ell}(0) \middle| \tilde{D}_{i,w} = 1\right] \\ &= \mathbb{E}\left[Y_{i,w+\ell} - Y_{i,w-s} \middle| \tilde{D}_{i,w} = 1\right] - \mathbb{E}\left[Y_{i,w+\ell} - Y_{i,w-s} \middle| \tilde{D}_{i,w} = 0\right] \quad (\equiv \text{DiD}(Y_{w+\ell})) \\ &= \underbrace{\mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 1\right] - \mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 0\right]}_{(\mathbf{A})} - \underbrace{\left(\mathbb{E}\left[Y_{i,w-s} \middle| \tilde{D}_{i,w} = 1\right] - \mathbb{E}\left[Y_{i,w-s} \middle| \tilde{D}_{i,w} = 0\right]\right)}_{(\mathbf{B})}, \end{aligned}$$

where

$$\tilde{D}_{i,w} = \begin{cases} 1 & \text{if } i \text{ won in } w \\ 0 & \text{if } i \text{ has not won by } w + \ell \end{cases}.$$

**Main decomposition of** $\text{DiD}(Y_{w+\ell})$. We decompose each of the terms $(\mathbf{A})$ and $(\mathbf{B})$:

$$\begin{aligned} \mathbf{A} &= \mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 1, \Lambda_{i,w+\ell} = 1\right] \mathbb{P}\left\{\Lambda_{i,w+\ell} = 1 \middle| \tilde{D}_{i,w} = 1\right\} \\ &\quad - \mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 0, \Lambda_{i,w+\ell} = 1\right] \mathbb{P}\left\{\Lambda_{i,w+\ell} = 1 \middle| \tilde{D}_{i,w} = 0\right\} \\ &= \underbrace{\left(\mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 1, \Lambda_{i,w+\ell} = 1\right] - \mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 0, \Lambda_{i,w+\ell} = 1\right]\right) \mathbb{P}\left\{\Lambda_{i,w+\ell} = 1 \middle| \tilde{D}_{i,w} = 0\right\}}_{\alpha_{w+\ell} \equiv \Delta \text{ intensive margin (post-win)}} \\ &\quad + \underbrace{\mathbb{E}\left[Y_{i,w+\ell} \middle| \tilde{D}_{i,w} = 1, \Lambda_{i,w+\ell} = 1\right] \left(\mathbb{P}\left\{\Lambda_{i,w+\ell} = 1 \middle| \tilde{D}_{i,w} = 1\right\} - \mathbb{P}\left\{\Lambda_{i,w+\ell} = 1 \middle| \tilde{D}_{i,w} = 0\right\}\right)}_{\beta_{w+\ell} \equiv \Delta \text{ extensive margin (post-win)}} \end{aligned}$$

and analogously,

$$\mathbf{B} = \underbrace{\left(\mathbb{E}\left[Y_{i,w-s} \middle| \tilde{D}_{i,w} = 1, \Lambda_{i,w-s} = 1\right] - \mathbb{E}\left[Y_{i,w-s} \middle| \tilde{D}_{i,w} = 0, \Lambda_{i,w-s} = 1\right]\right) \mathbb{P}\left\{\Lambda_{i,w-s} = 1 \middle| \tilde{D}_{i,w} = 0\right\}}_{\alpha_{w-s} \equiv \Delta \text{ intensive margin (pre-win)}}$$

$$+ \underbrace{\mathbb{E}\left[Y_{i,w-s} \middle| \tilde{D}_{i,w} = 1, \Lambda_{i,w-s} = 1\right] \left(\mathbb{P}\left\{\Lambda_{i,w-s} = 1 \middle| \tilde{D}_{i,w} = 1\right\} - \mathbb{P}\left\{\Lambda_{i,w-s} = 1 \middle| \tilde{D}_{i,w} = 0\right\}\right)}_{\beta_{w-s} \equiv \Delta \text{ extensive margin (pre-win)}}$$

Then, bringing the two separate decompositions together, we have

$$\text{DiD}(Y_{w+\ell}) = \underbrace{\alpha_{w+\ell} - \alpha_{w-s}}_{\text{intensive margin}} + \underbrace{\beta_{w+\ell} - \beta_{w-s}}_{\text{extensive margin}},$$

We perform these decompositions for each cohort $w$ and each event time $\ell$. In Appendix Table A.3, we report the share of the observed earnings response that is attributable to the extensive-margin response for each quartile,

$$\frac{\sum_{\ell(w)} \sum_w \omega_w (\beta_{w+\ell} - \beta_{w-s})}{\sum_{\ell(w)} \sum_w \omega_w \text{DiD}(Y_{w+\ell})}.$$

We then construct the aggregate share of the observed earnings response that is attributable to the extensive-margin as the mean of the weighted-average extensive contribution $\sum_{\ell(w)} \sum_w \omega_w (\beta_{w+\ell} - \beta_{w-s})$ across quartiles divided by the mean of the weighted-average total response $\sum_{\ell(w)} \sum_w \omega_w \text{DiD}(Y_{w+\ell})$ across quartiles.

## E Recovering counterfactual means with DiD

In this section, we discuss how we can recover the counterfactual mean of an outcome for lottery winners had they not won. Throughout, for simplicity, we focus on a particular cohort $w$ and event time $\ell > 0$, and so the discussion below is implicitly conditional on $w$ and $\ell$.

For an economic outcome variable $Y$, let $Y_{i,t}(1)$ and $Y_{i,t}(0)$ be the potential outcomes of an individual that has experienced or has not experienced her first-observed win, respectively. We can represent observed $Y_{i,t}$ as

$$Y_{i,t} = Y_{i,t}(0) + D_{i,t}(Y_{i,t}(1) - Y_{i,t}(0)), \tag{E.1}$$

where $D_{i,t}$ is an indicator variable that takes value 1 if individual $i$ experiences the first-observed lottery win by year $t$. We are interested in estimating the average effect of winning in year $w$ on outcome $Y$ in the event time $\ell \geq 0$, which we define as $\rho^{w,\ell} \equiv \mathbb{E}\left[Y_{i,w+\ell}(1) - Y_{i,w+\ell}(0)\right| i \text{ won in } w]$. The obvious difficulty is that while $\mathbb{E}\left[Y_{i,w+\ell}(1)\right| i \text{ won in } w]$ is observed directly, the counterfactual $\mathbb{E}\left[Y_{i,w+\ell}(0)\right| i \text{ won in } w]$ is not. Under an identifying common trend assumption

$$\mathbb{E}\left[Y_{i,w+\ell}(0) - Y_{i,w-s}(0)\right| i \text{ won in } w] = \mathbb{E}\left[Y_{i,w+\ell}(0) - Y_{i,w-s}(0)\right| i \text{ has not won by } w+\ell], \tag{E.2}$$

standard arguments imply that DiD estimator (3.3) recovers $\rho^{w,\ell}$. Under the same common trend assumption, we can also identify $\mathbb{E}\left[Y_{i,w+\ell}(1)\right| i \text{ won in } w]$ with a simple rearrangement of terms: $\mathbb{E}\left[Y_{i,w+\ell}(0)\right| i \text{ won in } w] = \mathbb{E}\left[Y_{i,w+\ell}\right| i \text{ won in } w] - \rho^{w,\ell}$.

When we implement this approach, we calculate $\mathbb{E}\left[Y_{i,w+\ell}(1)\right| i \text{ won in } w]$ directly as the observed mean for cohort $w$ in event time $\ell$, and we use our cohort-specific event-study estimates of equation (3.4) as our estimate of $\rho^{w,\ell}$.

# F Decomposition of moving probabilities

In this section, we discuss how we can decompose the total effect of moving into the contribution from mutually exclusive and exhaustive types of moves. Throughout, for simplicity, we focus on a particular cohort $w$ and event time $\ell > 0$, and so the discussion below is implicitly conditional on $w$ and $\ell$.

Following the discussion in Section 5.2, let $M_{i,t}$ be a binary indicator equal to 1 if the household's Census tract is different from that in the prior year, and 0 otherwise. Let $d \in \{1, 2, 3\}$ denote three mutually exclusive and exhaustive types of moves. Let$M_{i,t}^d$ denote an indicator corresponding to a move of type $d$. By definition, we can decompose the total probability of moving as follows:

$$\underbrace{\mathbb{P}\left[M_{i,t}=1\right]}_{\text{total probability of moving}} = \underbrace{\mathbb{P}\left[M_{i,t}^1=1\right]}_{\text{probability of type-1 move}} + \underbrace{\mathbb{P}\left[M_{i,t}^2=1\right]}_{\text{probability of type-2 move}} + \underbrace{\mathbb{P}\left[M_{i,t}^3=1\right]}_{\text{probability of type-3 move}} \tag{F.1}$$

Now, we consider the total effect on moving for cohort $w$ in event time $l$. As elsewhere, we define potential outcomes for moving: $M_{i,t}(1)$ and $M_{i,t}(0)$ are the potential outcomes of an individual that has experienced or has not experienced her first-observed win, respectively. We define analogous potential outcomes for each type-$d$ move. The average effect of winning in year $w$ on moving probability in the event time $\ell \geq 0$ is

$$\mathbb{E}\left[M_{i,w+\ell}(1) - M_{i,w+\ell}(0) \middle| \, i \text{ won in } w\right]. \tag{F.2}$$

By substituting (F.1) into (F.2), we can re-write as follows:

$$\begin{aligned}\underbrace{\mathbb{E}\left[M_{i,w+\ell}(1) - M_{i,w+\ell}(0) \middle| \, i \text{ won in } w\right]}_{\text{total effect on probability of moving}} &= \underbrace{\mathbb{E}\left[M_{i,w+\ell}^1(1) - M_{i,w+\ell}^1(0) \middle| \, i \text{ won in } w\right]}_{\text{effect on type-1 move}} \\ &+ \underbrace{\mathbb{E}\left[M_{i,w+\ell}^2(1) - M_{i,w+\ell}^2(0) \middle| \, i \text{ won in } w\right]}_{\text{effect on type-2 move}} \\ &+ \underbrace{\mathbb{E}\left[M_{i,w+\ell}^3(1) - M_{i,w+\ell}^3(0) \middle| \, i \text{ won in } w\right]}_{\text{effect on type-3 move}}.\end{aligned}$$

Finally, we can calculate the contribution of each component to the total moving effect by dividing the component effect by the total effect (F.2).

# G Details to Table 6.2

**Imbens, Rubin, and Sacerdote (2001) (IRS (2001)).** For IRS (2001), we report their headline MPE estimate which is based on their preferred sample of winners with annual pre-tax prize payouts less than \$100,000 (Sample Excluding Large Winners). This estimate corresponds to the estimate reported in their Table 4, Specification VIII, divided by 0.9 to reflect the fact that annual prize payments in IRS (2001) are for 20 years and not for the remaining lifetime of the winner. This is consistent with IRS (2001)'s approach as explained in their footnote 20. The underlying assumption is that the real interest rate $r$ and the discount rate $\delta$ are both equal to 10 percent, and that the lifespan is an additional 30 years after winning the lottery (corresponding to a total lifespan of 80 years).

To explore whether the IRS (2001)'s headline MPE estimate is robust to their preferred choice of 10 percent, we consider two alternatives: (i) if the discount rate and real interest rate is 5 percent (close to the average risk-free interest rate for their period of observation), the MPE estimate becomes -0.125, (ii) if the discount rate and real interest rate is 2.5 percent (close to the average risk free interest rate for our period of observation, and our maintained assumption), the MPE estimate becomes -0.136.[52]

**Our estimate.** In columns 3, 4, 7 and 8, we report MPE estimates based on the IV model introduced in Section 3.2 and defined by equations (3.5) and (3.6). We obtain these estimates by 2SLS estimation of the two-equation system with the endogenous variable being the (pre- or post-tax) measure of unearned income in a given period, and the outcome variable being a (pre- or post-tax) measure of individual or household total labor earnings. To calculate unearned income, we annuitize the (pre- or post-tax) lottery winnings as explained in Section 4.1, assuming that both the interest rate $r$ and discount rate $\delta$ are 2.5 percent, and that the total lifespan is 80 years.

The remaining MPE estimates (reported in columns 5, 6, and 9) are simple back-of-the-envelope transformations of the existing estimates to account for a different discount rate. Specifically, we follow Imbens, Rubin, and Sacerdote (2001) and Cesarini, Lindqvist, Notowidigdo, and Östling (2017), and assume a per-period utility function that takes the Stone-Geary form. It is then easy to show then that a $k-$year old household with a remaining lifetime of $T-k$ periods allocates a share

$$\lambda(r,\delta) = \sum_{t=1}^{5} \left(\frac{1+r}{1+\delta}\right)^{t} \frac{\delta}{1+\delta} \left(1 - \left(\frac{1}{1+\delta}\right)^{T-k+1}\right)^{-1}, \tag{G.1}$$

of the lottery prize as unearned income to the first five years after winning the lottery.[53] Given that winners in our sample are on average 46 years old in the year of winning, we can use expression (G.1) to scale an estimate of the MPE to account for a different discount rate in the underlying annuitization. For example, to go from column 4 to 5 in Table 6.2, we calculate $-0.433 \times \lambda(0.025, 0.025)/\lambda(0.025, 0.015) = -0.433 \times 1.125 = -0.487$.[54]

[52] See Imbens, Rubin, and Sacerdote (2001) for details on how to adjust their MPE estimates.

[53] The focus on the five years following the win year is in line with our empirical analysis in Section 4.

[54] The corresponding calculations for columns 6 and 9 are $-0.360 \times \lambda(0.025, 0.025)/\lambda(0.025, 0.015) = -0.405$ and $-0.241 \times \lambda(0.025, 0.025)/\lambda(0.025, 0.1) = -0.133$, respectively.

**Cesarini, Lindqvist, Notowidigdo, and Östling (2017) (CLNÖ (2017)).** For CLNÖ (2017), the MPE estimate in column 6 corresponds to the estimate reported in their Table 5 (panel C; assumed age-at-win of 30). We obtain the remaining estimates (columns 3, 4 and 5) by utilizing their model code (which is publicly available). To calculate pre-tax MPEs, we extend their model to include a linear tax system, and set the tax rate equal to 30 percent in line with the average tax rates reported in Appendix Figure A6 in CLNÖ (2017). To calculate MPEs for household total labor earnings, we also increase CLNÖ (2017)'s choice for the subsistence level of consumption and maximum hours of work by a factor of two. In columns 3 to 5, we then report estimates of MPEs (i.e., the lifetime MPEs at age 30) that we obtain by running their model code (extended for linear income taxation and, if applicable, household responses) to match the earnings responses reported in their Figures 1 and 5, respectively. For columns 3 and 4, we fix the discount rate in their model code to be 2.5 percent in line with our maintained assumption. In column 5, we fix the discount rate to be 1.5 percent which corresponds to the estimate reported in CLNÖ (2017)'s Table 5.

To investigate the robustness of the comparison with our estimates, we also run the above calculations under our assumption of a 2.5 percent real interest rate and find very similar results. As an additional robustness check, we also scale the MPE estimate in column 5 based on expression (G.1) and compare it to the MPE estimate that we obtain when we run the model code with a discount rate of 2.5 percent. Our back-of-the-envelope calculation (given an average age-at-win of 49 years in CLNÖ (2017) and a total lifespan of 80 years) implies an MPE of $-0.283 \times \lambda(0.02, 0.015)/\lambda(0.02, 0.025) = -0.283 \times 0.899 = -0.255$, compared to the model-based MPE estimate of $-0.267$ reported in column 4.