Parental Resources and College Attendance: Evidence from Lottery Wins*

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Abstract

We examine U.S. children whose parents won the lottery to trace out the effect of financial resources on college attendance. The analysis leverages federal tax and financial aid records and substantial variation in win size and timing. While per-dollar effects are modest, the relationship is weakly concave, with a high upper bound for amounts greatly exceeding college costs. Effects are smaller among low-SES households, not sensitive to how early in adolescence the shock occurs, and not moderated by financial aid crowd-out. The results imply that households derive consumption value from college and household financial constraints alone do not inhibit attendance.

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I. Introduction

Given the large college wage premium and sizable gaps in college-going by socioeconomic background, understanding the relationship between household financial resources and college attendance is an important step to addressing inequality of opportunity.[1] In theory, absent financial frictions, the decision to attend college should primarily reflect its net return. Indeed, a longstanding objective of higher education policy has been to abate or remove such frictions from this decision.[2] Nonetheless, many households cite cost as a major factor in the decision not to attend (Federal Reserve Board, 2017), suggesting that resources may directly influence college-going. If so, households must still be financially constrained, must derive consumption value from college beyond its human capital benefits, or both.

Understanding this relationship requires overcoming the inherent identification challenge that resources and schooling decisions are correlated with children's ability levels, household preferences, and other factors. To address this challenge, a recent quasi-experimental literature exploits income differences generated by, for example, housing prices, job loss, oil revenue, and tax credits.[3] The resulting estimates vary greatly in magnitude, ranging from less than 1 percentage point (p.p.) per \$100,000 to over 1 p.p. per \$1,000. This wide range likely reflects differences in the research designs—such as, the identifying assumptions, the affected populations, the size and salience of the changes in resources, the timing of the changes in children's lives, and offsetting effects of changes in financial aid eligibility—with the amount of weight attributable to any one factor difficult to ascertain. Altogether, despite considerable research in this area, there is a lack of consensus on 1) the degree to which household resources affect children's college-going and 2)

[1] Several studies, using distinct data sources, have identified sizable gaps in college attendance by socioeconomic background in the U.S. (e.g., Pallais and Turner, 2006; Bailey and Dynarski, 2011). Internationally, the average difference in the enrollment rate between children whose parents did or did not attend college is 38 percentage points for developed countries (OECD, 2017). College offers both pecuniary and non-pecuniary benefits (see Oreopoulos and Salvanes (2011) for an overview on non-pecuniary returns to higher education).

[2] See Hanushek, Leung, and Yilmaz (2014) and Page and Scott-Clayton (2016) for, respectively, a theoretical examination of and a review of the effectiveness of such policies in the U.S.

[3] Studies of the effect of resources on college attendance are described in the appendix and include Coelli (2011), Lovenheim (2011), Lovenheim and Reynolds (2013), Pan and Ost (2014), Hilger (2016), Bastian and Michelmore (2018), and Manoli and Turner (2018). Related literatures consider educational attainment (Shea, 2000; Akee et al., 2010; Bleakley and Ferrie, 2016; Loken, 2010), academic performance (Blau, 1999; Maurin, 2002; Ananat et al., 2011; Milligan and Stabile, 2011; Rege, Tella, and Votruba, 2011; Dahl and Lochner, 2012; Cesarini et al., 2016), and future economic outcomes (Bratberg, Nilsen, and Vaage, 2008; Oreopoulos, Page, and Stevens, 2008; Aizer et al., 2016).

the extent to which these effects stem from easing financial constraints or consumption preferences.

This study pursues resolution to these questions by comprehensively examining the attendance effects of a clean resource shock within a single framework. Specifically, using federal tax records, we examine the college outcomes of children whose parents won a state lottery (ranging from $600 to tens of millions of dollars) between 2000 and 2013. It is the first study to exploit variation among lottery winners to examine post-secondary attendance, a setting that offers several advantages relative to prior research in this area.[4] Lottery wins are pure income shocks that do not load other factors that might confound interpretation, and the amount of the win is salient to the household. The wide range of lottery win amounts and the diversity of households affected allow us to paint a rich picture of the magnitude of resources needed to generate significant changes in college outcomes, to assess the degree of concavity and upper bound of these effects, and to document heterogeneity in the response by household socioeconomic status (SES) and children's ages at the time of the shock. Our national, third-party reported data also allow us to generate precise estimates and examine the representativeness of our population of study, the extent to which households spend or save lottery winnings, and potential mechanisms underlying the pattern of results.

The analysis uses the full set of tax filings associated with each lottery winning household, including enrollment records that colleges must file with the IRS, and a separate linkage to children's federal financial aid records. The empirical strategy leverages both the amount of the lottery win and, to address concerns that there may be unobserved differences between households that experience larger and smaller lottery wins, the timing of the win with respect to the child's age. Specifically, our strategy examines college attendance in the year of high school graduation. Then, estimates are derived by comparing 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).[5] The resulting design is balanced across a rich set of household characteristics and is robust to falsification tests. We estimate both

[4] This study is also the first to examine *any* outcome using data on the population of U.S. lottery winners.

[5] Studies examining the effects of lotteries on labor supply, health, consumption, and cognitive development have taken various approaches to controlling for potential differences in household characteristics across lottery win amounts (e.g., see Imbens, Rubin, and Sacerdote, 2001; Lindahl, 2005; Hankins, Hoekstra, and Skiba, 2011; Kuhn et al., 2011; Powdthavee and Oswald, 2014; Apouey and Clark, 2015; Cesarini et al., 2016, 2017).

a step function and a linear specification and also examine effects on enrollment in later years, time spent in college, and the types of colleges attended.

Our analysis reveals that small-to-moderate increases in resources, which should ease most immediate household financial constraints, have little effect on attendance. For example, we can rule out that lottery wins averaging $50,000 (before taxes) increase enrollment by more than 0.4 percentage point (p.p.). However, our results do indicate a clear causal relationship between household resources and college attendance that is not highly concave.[6] The per-dollar effect indicates a 0.6 p.p. increase in attendance per $100,000 of additional household resources, which, while modest in comparison to many prior estimates, explains about one-third of the cross-sectional relationship between household income over childhood and four-year college attendance in the tax data (Table 1).[7] Further, the effect of resources achieves a high upper bound at amounts greatly exceeding the cost of college—averaging 10 p.p. for wins of $1,000,000 or more. The pattern of results is robust to alternative specifications, adjusting win amounts for taxes, and expanding the definition of enrollment to also include the year after high school graduation. Enrollment increases are concentrated at four-year colleges, at those with higher expenditures on student amenities, and among children from neighborhoods with higher rates of college attendance. They are also evident in each of the four years after high school, consistent with a persistent effect. While moderate wins increase attendance at public colleges, the largest wins increase attendance at both public and private colleges.

Extended analyses produce several results that help shed additional light on the roles of financial constraints and consumption as mechanisms. First, we find that responsiveness is, if anything, smaller among households with lower earnings, less wealth, and with the highest propensity to be credit constrained. Second, the diversity of children's ages at which a win occurs allow us to examine the role of binding short-run constraints and constraints that limit complementary parental investment earlier in childhood. We find no evidence that resource shocks that occur earlier in a child's life have larger effects, either in general or for lower-SES households

[6] To examine concavity, we can observe households with the same baseline levels of income receiving vastly different-sized resource shocks and thus do not need to leverage the amount of households' prior resources or to assume that there is no treatment heterogeneity by socioeconomic background.

[7] Table 1 presents estimates of the OLS relationship between total household income during childhood and college attendance, over increasing ranges of average household income. Coefficients range from 1.2 to 1.7 p.p. per $100,000 for four-year college attendance and 2.1 to 2.6 p.p. for any college attendance. We do not observe stronger correlations for income in the years immediately prior to high school graduation.

in particular.[8] Third, we develop a test that exploits the timing of wins with respect to key features of the federal financial aid formula – i.e., that wins that occur in the year prior to high school graduation are treated as income, which is heavily penalized in this formula, while wins that occur earlier than that year are treated as wealth, which is not – to account for potentially offsetting effects of changes in need-based financial assistance. This reveals that additional resources reduce need-based financial assistance but that crowd-out of such assistance does not significantly moderate the attendance estimates. Finally, the rich set of outcomes available in the tax data enable us to examine alternative margins of response, revealing that lower-SES households exhibit smaller reductions in earnings and smaller increases in savings, suggesting a higher marginal propensity to consume non-college goods that could reflect other spending priorities.

Altogether, our estimates are not particularly consistent with children forgoing college due to a lack of household resources alone. Specifically, the analysis reveals modest effects for wins sufficient to cover the cost of college and for households that are most likely to face constraints.[9] In contrast, our results are consistent with households consuming college in part as a normal good, such that fundamentally altering a household's financial status can have a large impact on college-going, as the effects increase approximately linearly in the size of the win and reach a high upper bound. Indeed, some prior studies have found that the consumption component of education is larger for higher-income households, which is consistent with our heterogeneity analyses (including evidence of outsized effects at colleges with a greater focus on amenities and from neighborhoods with higher rates of college attendance).[10]

The paper is organized as follows. Section II introduces the administrative data used for analysis and examines the representativeness of lottery winners. Section III details the empirical design, the underlying assumptions, and presents falsification tests. Section IV describes the results, robustness, and extensions (including an investigation of heterogeneity and financial aid). Section V explores alternative household responses. Section VI discusses potential mechanisms. Section VII describes the relation of our findings to the literature. Section VIII concludes.

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[8] In addition, we find that increases in financial resources during the calendar year of high school graduation have little effect on college attendance, which has implications for the design of policies seeking to improve college affordability (e.g., student aid offers and education tax credits).

[9] The muted response among children from more constrained households may partially reflect other factors, such as a lack of academic readiness for college, which we explore in Section VI.

[10] See Arcidiacono (2004), Alstadsæter (2011), Alter and Reback (2014), Jacob, McCall, and Stange (2018), and Gong et al. (2019).

II. Sample Construction and Lottery Winner Characteristics

a. Sample Construction

We use the universe of federal tax records for the U.S. population to identify 1.5 million individuals who turned 18 between 1999 and 2013 and had a parent with a state-reported lottery win over those years. We then link their tax records to their college enrollment records, federal financial aid records, and parents' tax records using social security numbers.[11] Throughout the analysis, dollar amounts are adjusted for inflation and denoted in real 2010 dollars.

To construct the sample, we first identify any individual with a state lottery win reported on the third-party reporting Form W-2G. This form is reported by the relevant state agency to the IRS, required for all prizes in excess of $600, and first available in 1999. In addition to the amount of the win, the form indicates the state and year of the lottery. The first calendar year that we observe an individual receiving lottery income is designated as the "win year," which is used to classify household treatment.[12] In a small fraction of cases, assumptions are required to identify the precise year or amount of an individual's first win.[13] These ambiguous cases are excluded from the baseline sample, but we show that the estimates are robust to their inclusion.

To form "households," lottery winners are linked to their full set of tax records back to 1996 (the first year such data are available), which includes identifying information for any dependents they claimed on their tax return (Form 1040) over that window. Parent-child matches are only included if the child is claimed prior to the win, and, if the win occurred after the child turned 19

[11] Table A1 summarizes the merging process. The use of social security numbers as unique identifiers largely eliminates issues of mismatch. Of all lottery winners, 99.8 percent are linked to their Social Security Administration records, and 93.4 percent file a tax return as a primary or secondary filer prior to the win. Non-filers who win the lottery tend to be older, as retirees who depend on social security payments are not required to complete tax returns.

[12] Because we cannot observe whether lottery income received in 1999 is part of a multi-year payout stemming from an earlier win, we only include wins in the analysis that occurred in 2000 or later. The first win year is preferred because subsequent wins could be endogenous to the size of the initial win and hence contaminate the assignment of win size. The probability that a household experienced a large win prior to the first observable year, 1999, is small. To verify this, we note that only a small fraction of those with wins in the second half of the sample period experienced a large win in the first half and that estimates are robust to restricting attention to first wins that occurred in the second half of the sample period.

[13] This occurs when an individual is observed receiving supplemental income that matches their win amount in the year prior to state reporting (indicating that they may have been collecting the win before it was reported), when an individual has multiple wins in the same year and it is unclear which occurred first, and when a win is paid out over multiple years (which may not be fully observed during the sample period) and must be converted to a lump sum.

years old, prior to turning 19.[14] These links will include birth parents, step parents, and adoptive parents who are financially responsible for a child and whose income and assets are likely to be considered for the purposes of educational grants and loans provided by the federal government, states, and academic institutions. As an alternative to relying on claimed dependents to form our sample, we present results derived from birth parents as determined by Social Security Administration records.

The primary outcome of interest is whether children transition to college immediately after high school. We measure attendance with Form 1098-T data, a mandatory third-party reporting form filed by post-secondary institutions. This form yields each college and university a student attends in each calendar year starting in 1999. Both the act of and the timing of high school graduation may be endogenous to financial resources, so, consistent with the literature, the sample is not restricted to children who complete high school. We instead examine all children whose parents won the lottery, approximating the year of high school graduation using each child's exact birthdate (via social security card applications), the state in which the child was born, and the corresponding school entry age laws for that state. As Form 1098-T is filed by calendar year and not by academic year, children with 1098-Ts for their predicted high school graduation year are classified as transitioning to college immediately.[15]

We also examine the characteristics of colleges children attend as well as their financial aid receipt using data from the U.S. Department of Education. For the former, Form 1098-T data are linked by college identifiers to college characteristics maintained by the National Center for Education Statistics Integrated Postsecondary Education Data System (IPEDS), including whether the college is a two- or four-year and public, private, or for-profit institution. For the latter, we match via social security number student federal financial aid records within the National Student Loan Data System, an administrative database of student-level records describing their Title IV loans and grants. These data contain an array of financial aid information for each academic year, including application for aid (i.e., filing a FAFSA), subsidized and unsubsidized loan amounts, Pell Grant amounts, and expected family contribution (EFC), a Department of Education concept

[14] Claiming children after a win could be endogenous to the extent that tax filing and tax liability are influenced by lottery wins, and eligibility for claiming children aged 19 and over is mostly limited to those who are attending college.
[15] To address the concerns that this approach may misclassify some children's graduation cohorts (e.g., if they enter elementary school late or are held back) or count children that only briefly delay their matriculation to college as not attending, we present estimates using an alternative outcome variable which counts children as attending if they are enrolled in either their predicted high school graduation year or the subsequent year.

that is designed to approximate a family's ability to pay for college. Because realized changes in aid can only be observed for federal sources, we supplement the analysis by imputing changes in state and institutional aid for our sample using restricted-use data from the National Postsecondary Student Aid Study (NPSAS), a research dataset that includes survey and administrative records of financial aid application and receipt for a nationally representative sample of students attending Title IV postsecondary institutions.

Finally, we examine several measures from tax records as outcomes of interest and draw on a number of pre-win child, parent, and household characteristics to test for balance in the research design, to include as controls, and to examine heterogeneity. Most notably, tax forms reveal earned income, investment income from interest and dividends, the presence of a mortgage, retirement contributions, and household composition. Many of the variables used are reported on Form 1040 and third-party reporting forms (e.g., W-2, 1099, 1099-int, and 1099-div), while some demographic information (e.g., gender, citizenship) comes from social security records.[16] In addition to measures available in the tax records, the heterogeneity analyses split the sample by measures that are imputed using corresponding waves of the Federal Reserve Board's Survey of Consumer Finances (SCF), a triennial cross-sectional survey of U.S. families that includes information on their debt, assets, income, and demographic characteristics. Specifically, household liquidity, net worth, payment-to-income ratio, debt-to-asset ratio, propensity to be rejected for credit, and propensity to make late payments are all imputed from the SCF using a rich set of characteristics common to both datasets. Finally, heterogeneity by economic conditions is analyzed using business cycle timing and local measures of poverty, insurance coverage, and housing values from the U.S. Department of Agriculture, American Community Survey, and Zillow.

b. Lottery Winner Characteristics

The baseline analysis restricts the sample to children from families that experienced a lottery win within six years before or after their expected high school graduation (though we evaluate the robustness of the estimates to alternative windows).

Table 2 describes lottery wins and college attendance for the resulting population. Approximately 96,000 children are from households that win over $10,000, nearly 14,000 are from

[16] Form 1040 is first available in 1996, and the first cohort of interest is 1999, so household characteristics are based on three pre-win years.

households that win over $100,000, and 1,300 are from households that win over $1,000,000. Taking into account taxes reduces lottery win amounts by an average of 18 to 35 percent, with the reduction increasing in the size of the win. Only 3 to 4 percent of winners in any category subsequently win $10,000 or more in a future lottery, easing concerns that households spend a significant fraction of their wins on additional lottery tickets or that total win amounts are misclassified. Approximately 35 percent of our sample attends college in their predicted high school graduation year (our main outcome of interest) and 45 percent attend by the following year (presented as an alternative specification). Table 3 indicates that, prior to a lottery win, the average household in the sample has wages of approximately $52,000, and 57 percent of households file as married. The homeownership rate is about 56 percent, and slightly less than half of the households have interest or dividend income.

An important consideration for interpreting the estimates in this study is how representative lottery-winning households are of the population of U.S. households. As detailed in Appendix A, surveys indicate that up to 50 percent of the population plays the lottery. Further, lottery-playing households in the Consumer Expenditure Survey (CEX) closely resemble non-playing families in terms of family size, education, race, earnings, sector of employment, and assets, and they allocate $266 (0.4 percent of annual spending) to buying lottery tickets (Table A2).

Given that our analysis examines lottery *winners with children*, we compare our sample to the population of tax-filing parents who have same-aged children. Relative to the parents in our sample, these parents have higher average earnings and are more likely to be married, though there is significant common support and their median earnings are nearly identical (Table A3). These results are consistent with other analyses of the comparability of lottery winners to a more-general population in the literature (e.g., Imbens, Rubin, and Sacerdote, 2001; Hankins, Hoekstra, and Skiba, 2011; Cesarini et al., 2017). While it is straightforward to account for differences in observable characteristics – we later show our results are robust to weighting the sample to resemble the overall population – one concern for generalizability specific to our setting is whether children from lottery-winning households have lower propensities to attend college due to unobserved factors such as academic readiness or parental support. To shed light on this, Table A3 also compares the enrollment rates of children whose parents won the lottery after they would have graduated from high school (and whose decisions to attend college were thus unaffected by the lottery wins) to the rates of same-aged children. This comparison reveals that, after conditioning on household characteristics, the children of lottery winners are only 1 percentage point less likely to attend college. In sum, while we cannot fully rule out differences in behavior and preferences, lottery winners appear to be only modestly different from non-winners in terms of observable characteristics and, once we account for these differences, the propensity of their children to attend college is nearly identical.

III. Empirical Strategy

Our empirical framework exploits within-state-year variation in both win size and timing. While the majority of the variation stems from the randomized process behind a lottery win, comparisons across win size alone would require an assumption that winners of different-sized payouts are not different along unobserved dimensions correlated with attendance. In our setting, variation in the payout is also an artifact of the type of lottery played and when the lottery is played, two factors for which it is difficult to control directly. (We are unaware of any state that collects data on the specific day and type of lottery tickets purchased by each individual, as tickets are sold by a variety of stores and are often paid for in cash.) Moreover, the prior literature has documented differences in the types of households that play particular lotteries (Oster, 2004), and, within our data, there are observable differences in household characteristics by win size.

To abstract from this assumption, we also leverage variation in the timing of wins with respect to the child's age. Specifically, we focus on outcomes that occur within a particular time frame relative to high school graduation and use the experiences of children who were "too old" at the time of the win to absorb unobserved differences between households that experience larger and smaller wins. For example, whether a child transitions to college immediately from high school cannot be affected by wins that occur in the years after high school graduation.[17] The identifying assumption is that unobserved differences in the propensity to attend college across lottery win

[17] The main specification excludes wins that occur the year of high school graduation, although we demonstrate results are not sensitive to their inclusion. It is unclear if, and to what extent, children whose parents won that year can respond. A win in the fall is generally too late to change enrollment decisions. If the win occurs earlier in the year, it may also be too late to take the necessary steps to enroll in college (e.g., taking the SAT or ACT, meeting application deadlines), and the individual may have made other arrangements, such as taking a job or enlisting in the military.

sizes are the same for children whose parents win before or after they graduate from high school.[18,19]

The sample includes wins as high as tens of millions of dollars; thus, how we choose to parameterize win size is important. For example, the effect of each dollar will necessarily decrease at some level, and a linear functional form will place the most weight on the largest wins.[20] The baseline strategy addresses this issue via a flexible "step function" approach that categorizes wins into bins and thereby allows effects to vary across win ranges without imposing a strong functional form assumption. We classify wins according to five thresholds: \$10,000, \$30,000, \$100,000, \$300,000, and \$1,000,000. (We show that the results are not sensitive to these thresholds.)

The step function specification is:

$$y_{i,c,s,y} = \delta_{s,y} + \delta_c + \boldsymbol{X}_i\gamma + \theta PreHSG_i + \sum_j \alpha_j(size_i = j) + \sum_j \beta_j PreHSG_i\,(size_i = j) + \varepsilon_{i,c,s,y}$$

The unit of observation is a child i in a high school cohort c in state s and win year y. The specification includes state-by-year of win and expected year of high school graduation cohort fixed effects, with standard errors clustered at the parent winner level. $\boldsymbol{X}_i$ is a vector of 15 pre-win household and child characteristics.[21] The α_j coefficients absorb fixed differences across households that experience different-sized wins, with wins between \$600 and \$10,000 (which average \$2,047) serving as the omitted group (though, in practice, the exact range of the omitted group is unimportant). The coefficient θ accounts for fixed differences between children who

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[18] The identifying assumption would fail if, for example, the children of large lottery winners before high school graduation are *differentially* more college ready than children of small lottery winners *relative* to the difference in college readiness of children of large versus small winners after high school. The design does not require that children whose parents win before or after their high school graduation have identical propensities to attend college but rather that the differences are the same across lottery win sizes. Nonetheless, as reported in Appendix B, there is no significant shift in the income or share of households that win lotteries before or after their children graduate from high school.

[19] An additional assumption implicit in this analysis is that responsiveness to resources is similar for households across the win size distribution. We explore this assumption in Section IV(a) and Appendices B and C and find evidence that it holds.

[20] Issues with imposing linearity on the effects of income have been noted by Loken, Mogstad, and Wiswall (2012).

[21] These characteristics are log wages, log adjusted gross income, presence of mortgage interest, claiming of the self-employment tax deduction, the presence of interest and dividends, the presence of SSA income (including disability), whether a 1040 is missing in any of the three years prior to winning the lottery (from which we derive these controls), household filing status, the number of children in the household, and parent and child gender and citizenship. Only households who do not file in any of the three years prior to the lottery win are excluded from the analysis. Data missing for a single year are treated as missing and are not assumed to be zero. Results are robust to alternative levels by which to cluster the standard errors.

graduate before and after their parents experience a win. The key parameters of interest are the β_j coefficients, which reflect the differential outcomes for children whose parents win a lottery of a given size relative to children whose parents won a small lottery, while accounting for fixed differences between these groups. A number of outcomes, each with a temporal component, are examined, including several variants of college attendance (e.g., sector-specific, level-specific, and different horizons relative to high school graduation), financial aid application and receipt, and parental labor force participation and savings.

In addition to the step function approach, we estimate two other specifications. The first relies on a continuous measure of win amounts, restricting attention to the range of wins over which the relationship appears to be linear (i.e., over which there is no evidence of concavity).[22] While this specification requires a strong functional form assumption and is sensitive to the range of wins included, it yields a per-dollar effect that helps us compare estimates in a parsimonious manner across pre- and post-tax lottery win amounts as well as by household SES and other dimensions of heterogeneity. We also use this linear specification to estimate changes in parental earnings and other outcomes after the lottery win. The second specification is a multinomial logit model, which we use to further explore the margins of attendance across two- and four-year colleges and by household income.

Before turning to the main results, we examine whether the comparisons relied upon for identification appear to successfully isolate changes in resources. First, we test for balance by estimating the specification with 15 different exogenous pre-win characteristics on the left hand side. Table 3 presents the results across each characteristic, including adjusted gross income, self-employment status, homeownership, and the presence of investment income (a proxy for savings). Among the 15 variables we consider, only 2 (self-employment and child gender) are jointly significant across win sizes at the 10 percent level, and among the 5 win size bins, none are jointly significantly different. An F-test across all win size bins and variables (the resulting 75 coefficients) is not significant (p-value=0.5098).[23] Further, the design is balanced across the

[22] The linear specification is $y_{i,c,s,y} = \delta_{s,y} + \delta_c + \boldsymbol{X}_i\gamma + \theta PreHSG_i + \alpha Amt_i + \beta PreHSG_i Amt_i + \varepsilon_{i,c,s,y}$, where the win amount is measured in hundreds of thousands of dollars.

[23] As an alternative, we estimate the propensity of each child to attend college based on the 15 household characteristics (with the predictive importance of each characteristic estimated using older, unaffected children). We find that this measure of college propensity is insignificant across each win size bin and has an overall F-statistic of 0.4798. A similar balance test of the linear specification (not shown) reveals that 14 out of the 15 coefficients are insignificant, with the other significant only at 10 percent.

household income distribution, college attendance and financial aid of older, unaffected siblings, and household characteristics when children are the same age (Tables A4 and A5). Altogether, there do not appear to be meaningful differences in observable child and household characteristics. Second, we conduct a placebo analysis that implements the design using only children whose parents won after their high school graduation year, reassigning the lottery win to a false prior year. This generates null effects, evidence that the timing of wins relative to high school graduation is not correlated with differential trends in college attendance across lottery win amounts (Table A6).

IV. The Effects of Household Financial Resources

a. College Attendance

Figure 1 offers a graphical depiction of attendance rates pre- and post-lottery win by win amount. For resource shocks of less than $100,000, differences in attendance by timing are small. In contrast, for larger wins, the differences are substantial, with the amount increasing in the size of the win. Note that children from households that experience larger wins are somewhat more likely to attend college, highlighting an identification challenge that, as Table 3 suggests, our design allows us to address.

We turn next to our main estimated effects of lottery wins on college attendance. Table 4 examines attendance in the year of high school graduation at any college, a four-year college, and a two-year college. The estimates reveal that moderate-sized shocks have little effect on attending any college. Wins between $10,000 and $30,000 and between $30,000 and $100,000 produce statistically insignificant effects, and we can rule out more than modest increases. For wins between $100,000 and $300,000, the coefficient is larger—on the order of 1 to 2 p.p.—but only marginally significant. The effect continues to increase above this level, with wins between $300,000 and $1,000,000 increasing attendance by an average of 5 p.p., and wins exceeding $1,000,000 increasing attendance by approximately 10 p.p., and both estimates are highly significant. These estimates are stable to the inclusion of controls for household, parent, and child characteristics.

Differentiating the effects by college level reveals that they are driven by four-year college attendance. The four-year estimates closely mirror those for any college attendance, with no detectable change for small wins and the response increasing with the amount of the win. The estimates for two-year college attendance are small and statistically insignificant for all win levels. (For smaller wins, the lack of a two-year enrollment effect has a straightforward interpretation: even when we focus on schools that are relatively less expensive and less selective, modest shocks to resources have no material effect on attendance. The interpretation of estimates for larger wins is complicated by the fact that they represent the net effect of competing margins, as some children may be induced to attend a two-year college instead of no college, while others may be induced to attend a four-year college instead of a two-year college.[24]) In the interest of space, except where noted, the remaining analyses focus on four-year attendance.

Our results suggest that attendance effects are not very concave in the size of the win. An alternative explanation for the relative magnitude of effects across lottery win size is that they could be driven by treatment heterogeneity along a) observable characteristics that differ between large and small winners or b) unobservable differences by win size in the propensity of children to attend college. We test the former by reweighting households such that each win size bin has the same average characteristics (estimating similar results in Table A11 as from our main specification) and the latter by examining the attendance rates of older, unaffected children (finding no difference in college enrollment by win size once we condition on observable characteristics in Table A7). Thus, differences across win amounts are too modest to meaningfully alter the pattern of responses.

The estimates thus far represent average effects for ranges of wins, abstracting from strong functional form assumptions. To explore the shape of the response further, we first estimate a variant of the baseline specification, whereby we increase the number of bins more than tenfold, and fit a Lowess plot over the estimates (Figure 2).[25] The effect appears to be approximately linear until win sizes reach nearly $5,000,000 and reach an upper bound of around 20 p.p. Then, Table 5 specifies win size continuously, interacting the amount of the win with whether the win occurred prior to high school graduation. The first column tests for concavity by including a linear and quadratic term and imposing various caps on the largest win size included in the sample. Consistent

[24] Because these competing margins cannot be observed directly, we examine if there are changes in the average characteristics of households with children attending two-year colleges. The resulting estimates (not shown) reveal no evidence that characteristics are changing, which is more consistent with no gross effects on attendance at two-year colleges.

[25] Bin increments are selected as follows: $5,000 up to $100,000, $25,000 up to $500,000, $100,000 up to $1,000,000, and $500,000 up to $5,000,000. Effect sizes are estimated relative to small wins of less than $1,000.

with the figure, the coefficient on the quadratic term is indistinguishable from zero when restricting attention to wins less than $5,000,000 (and any threshold below), suggesting little concavity in this region.[26] Note that the challenge of imposing a specific functional form on lottery wins is reflected in the sensitivity of the linear estimates to including large wins in the analysis. Because of the above findings, we only include lottery wins that are less than $5,000,000 when estimating the linear specification.

The linear specification facilitates comparisons across alternative constructions of key variables. Table 6 presents estimated linear effects of lottery wins on four-year college attendance before and after adjusting for income taxes, after broadening the enrollment measure to include the subsequent year, and as an elasticity with respect to average annual and total household income during childhood. The estimated linear effect is approximately 0.6 p.p. per $100,000 of pre-tax lottery winnings. To predict after-tax lottery winnings, we take each household's pre-win tax return, add the lottery win, and apply the lottery year's tax rules. This simulated tax liability approach produces an average reduction in the take-home amount of 30 percent. The resulting effects are commensurately larger, with attendance increasing by 0.9 p.p. per $100,000 of post-tax winnings. Table 2 indicated that approximately 10 percent of children initially enroll in college in the year after their predicted graduation, potentially due to factors such as entering elementary school late, being held back, not meeting graduation requirements, or choosing to delay matriculation. Thus, we replicate the design while expanding the definition of enrollment to include the year of expected graduation and the subsequent year, and find a similar-sized effect of 0.7 p.p. per $100,000. Estimating the response as an elasticity reveals that attendance increases by 0.22 percent for each 1 percent increase in total household income during childhood. In the appendix, we consider an alternative method of measuring treatment intensity. Parents may share lottery wins across children, and we find some evidence of smaller effects for households with more children (Table A29). Thus, we divide the win equally across children and estimate a win amount per child effect. This approach scales the estimate proportionally, resulting in an average increase in attendance of 1.2 p.p. per $100,000.

[26] Table A8 presents an alternative test of linearity across win amounts. Incorporating indicators for win size bins into the linear specification reveals that they are not statistically significant, implying that the linear coefficient is sufficient to capture the relationship between the lottery win and the college attendance response. We also estimate the degree of concavity while taking into account aggregate household income during childhood (using parents' pre-lottery average AGI multiplied by 18 as a proxy). The results (not shown), which load treatment heterogeneity, suggest linearity up to $10,000,000 in resources.

Figure 3 plots estimated differences in attendance between children whose parents won large versus small lotteries on a year-by-year basis both before and after the children graduate from high school—where "large wins" in the top panel are defined as wins greater than $100,000 and "large wins" in the bottom panel wins greater than $300,000 (excluding those between $100,000 and $300,000)—and reveals several key results. First, estimated differences are generally positive for children whose parents win the lottery in any of the six years prior to their high school graduation year but not for children who graduate prior to the win. This pattern affirms the timing exploited by the design. (Table A10 further confirms the sharpness of the pre-post transition by replicating the baseline specification while sequentially reducing the window of lottery win years before and after graduation.) Second, the magnitudes are similar across years, so effects do not appear to grow or diminish depending on the time between a lottery win and high school graduation. And third, the steep drop-off in effects in the year of high school graduation suggests that the year in which policies that seek to improve college affordability become salient to households is material to their effectiveness.

The pattern and magnitude of the results are robust to a number of alternative methods of classifying treatment and constructing key variables (described more fully in Appendix C). The estimates are stable when linking children to their birth parents and when using alternative win ranges for the control and treatment groups.[27] The results are also robust to designs based solely on lottery win sizes or the timing of the wins relative to high school graduation (Tables A15 and A16).[28] Using the lottery win to instrument for a household's net change in income as a way to take into account any corresponding changes in household earned income in response to a win also reveals an average effect of 0.6 p.p. per $100,000, suggesting any such responses are small relative to the win amount (Table A17). Restricting attention to within-household comparisons of siblings results in estimates that are similar in magnitude but less precise than those in the primary design.[29]

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[27] Estimates for alternative samples and control groups are presented in Table A11 and narrower win ranges in Table A12. In addition, we present estimates for alternative measures of college enrollment in Table A13 and levels of clustering in Table A14.

[28] An across win size design produces estimates of 2, 5, and 8 p.p. for the largest three win size bins, while a design exploiting only the timing of the win generates effects of 1, 5, and 11 p.p. (Tables A15 and A16). The linear estimates (not shown) for these designs are 0.5 and 0.6 p.p. per $100,000, respectively. Neither the binned nor the linear estimates are statistically significantly different from those in the primary design.

[29] Within-household comparisons require that at least one child graduates before and after the win. As shown in Table A11, this discards 74 percent of the sample, including all one-child households, significantly reducing precision and placing greater weight on households with many children. These estimates are not statistically different from those in

From the magnitude and pattern of results thus far, it appears unlikely that a large fraction of students are deterred from attending college solely due to binding borrowing constraints at the household level. Nonetheless, the estimates reveal a role for household resources in shaping college outcomes. The average effect of a pure resource shock in our context explains about one-third of the 1.7 p.p. per $100,000 relationship between four-year college attendance and total childhood household income observed in the cross-section (Table 1). Likewise, the 10 p.p. effect found for the largest win bin represents a 30 percent increase in any college attendance and a 50 percent increase in four-year college attendance with respect to baseline rates in our data. To put this in perspective, this effect matches the overall growth in the attendance rate that occurred during the rapid expansion in college enrollment between the late 1970s and the mid 1990s.

b. Enrollment in Later Years and by College Type

Additional household financial resources could raise children's enrollment in years after high school graduation, increase time in college, or change the types of colleges attended. Table 7 presents the effects of lottery wins on enrollment in the four years after high school graduation, as well as the cumulative change in years of attendance over this period. Following the logic of our main specification, the control group is restricted to children whose parents won after the year being analyzed and thus could not have been affected by the win.[30] For smaller wins, there is no evidence of increased attendance in any year or total years in college. However, large wins increase attendance in each of the four years after high school graduation and raise cumulative attainment by as much as 0.6 year. Thus, it does not appear to be the case that wins generate a temporary increase in attendance that rapidly fades. Still, these effects could partially reflect increased persistence among students who would have initially attended regardless of the lottery win.

Table A19 extends the analysis to examine if children are induced to attend college by wins that occur after high school graduation. Replicating Table 4 for children whose parents won in the four years after their expected high school graduation and similarly restricting the control group to children whose parents won after the year being analyzed indicates that effects are attenuated

the primary step function design, with joint test p-values of 0.29 and 0.70, nor do they differ when we estimate the effects in a linear specification.

[30] Table A18 replicates this design, restricting the control group to those whose parents won the lottery four or more years after high school graduation. While this eliminates the children who are most similar in terms of win timing, it maintains a consistent control group across each post-high school year analyzed. The results are similar.

for later wins, suggesting that attendance behaviors are more meaningfully influenced by resource shocks that occur before children graduate from high school.[31]

Turning to college composition, we do not find a clear shift in the sector of attendance for smaller wins, so the lack of an effect on overall attendance is not obscuring offsetting changes in private and public college attendance (Table A21). Likewise, when we examine college quality (measured either as a binary variable denoting whether college attendees have above median earnings or as the average earnings of attendees), the effects for smaller wins are not statistically significant. For wins less than $1,000,000, the enrollment effects are concentrated at public colleges, while wins exceeding $1,000,000 increase enrollment at both private and public colleges, and at colleges whose attendees subsequently have higher earnings.

c. Heterogeneity

Households of varying means and financial constraints might differ in their responsiveness to resource shocks. For example, households may differ in their access to credit, their consumption preferences, or the extent to which their children are college ready. Table 8 presents tests for heterogeneous effects on four-year attendance using the linear specification and, one-by-one, interacting the pre-college win amount term with various measures of constraints. Throughout the table, the coefficient on the interaction term quantifies the differential effect for the *less* constrained group.

The top panel delineates households based on financial characteristics available in (or that can be imputed from) the tax data, measured prior to the lottery win. The first column reveals a large and statistically significant coefficient on this term, indicating that children from households that have above the median income in our sample (about $45,000) appear to be driving the positive effect of lottery wins on attendance. This result is also evident in the step function specification and a multinomial logit model.[32] Responses are also statistically larger for households that have

[31] Only the largest wins generate a statistically significant increase and only for any college enrollment. Tables A20 shows there are no notable attendance effects among parents.

[32] Table A24 presents the step function specification for households with incomes above and below the sample median ($45,000). The estimates do not reveal significant changes in enrollment at two-year colleges for lower- or higher-income households. The effects for larger wins are concentrated at four-year colleges and above median income households. Though sizable four-year college responses to the largest wins cannot be ruled out for lower-income households due to a lack of precision, the estimates are statistically significantly smaller. These results are confirmed by a multinomial logit model that simultaneously estimates two-year and four-year attendance for lower- and higher income households (Table A25). Tests of heterogeneity by household income are unchanged after adding terms

interest or dividend income (a proxy for savings), make voluntary retirement account contributions (given their tax advantages, those who do not contribute to them are more likely to be financially constrained), and for households with above median wealth, which is estimated following Saez and Zucman (2016).[33] There does not appear to be a differential effect in either direction for homeowners.

The effect of financial resources on attendance may vary along other segments of the income distribution or administrative measures of ability to pay for college (Belley and Lochner, 2007; Stinebrickner and Stinebrickner, 2008; Lochner and Monge-Naranjo 2011; Brown, Scholz, and Seshadri, 2012; Johnson, 2013; Cowan, 2016; Marx and Turner, 2018). A more refined split by household income reveals that enrollment responses are modest for the lowest two quartiles of household income, and are larger and statistically significant for the top two quartiles (Table A27). After applying the federal aid formula to each household's pre-win income, imputed wealth, and composition to predict the administrative measure of ability to pay for college (i.e., its EFC), we find larger enrollment responses for households with a higher ability to pay and for those with too high of an ability to pay to receive a Pell Grant.

To capture other aspects of a household's financial position, we use the Survey of Consumer Finances (SCF) to construct measures of the probability a household has been denied credit, the probability a household has made late payments, whether a household has high debt service (in relation to its income or assets), and whether a household has low liquidity.[34] Across each of these measures, less constrained households are more responsive to lottery wins. Finally, the bottom panel of Table 8 differentiates the effects by local poverty rates, health insurance coverage, and housing values as well as the business cycle (as the Great Recession years of 2008 to 2012 represented a period of particularly tightened access to credit). These aggregate measures could capture household constraints that are not evident in the tax data, but reveal no evidence of larger effects for households that live in more disadvantaged communities or whose children graduated

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allowing for heterogeneous responses by investment income, homeownership, and debt cancellation status (Table A26).

[33] See Appendix D for a discussion of how wealth is imputed using tax variables.

[34] The SCF includes questions about whether or not a household has been denied credit or deterred from applying for credit in the last year (Japelli, 1990; Crook, 1996; Japelli, Pischke, and Souleles, 1998; Bostic, Gabriel, and Painter, 2009). It also includes information about average monthly payments, late payments, debt, and assets. Appendix D presents the procedure used to impute these values for households in our sample using variables that are observable in both the tax data and the SCF such as earned income, business income, investment income, retirement accounts, homeownership, marital status, and household size.

from high school during the Great Recession. (Table A23 presents more refined splits, by comparing the top to bottom quartile for each non-binary measure of being constrained from Table 8. This division also generally reveals a pattern of larger responses for the least constrained households relative to the most constrained households.)

The tax treatment of lottery wins and more expansive definitions of enrollment may be especially relevant for heterogeneity analysis, as lower-income households face lower marginal tax rates and their children are less likely to graduate from high school on time. Replicating the linear design for post-tax win amounts reveals that effects are again concentrated at higher incomes, with little evidence of changes in enrollment for households with below median income (Table A28). Results are similar when the effects of lottery wins are measured as the elasticity of college attendance with respect to annual or lifetime income. More notably, the effects for lower income households become marginally significant under the broader definition of enrollment; still, they are only one-third as large as those for higher income households. Extending the heterogeneity analysis by income to the types of colleges attended, the estimates for children from higher-income households fully mirror those using all households from earlier, while there is some evidence that very large wins increase public enrollment for lower-income households (Table A30).

We next build on Figure 3 and further examine heterogeneity by win timing. If additional resources are spent down prior to the college transition and are unavailable to cover tuition (though we later do not find evidence consistent with this phenomenon), then the effects of earlier wins could be muted. Alternatively, if parents are financially constrained from allocating income to inputs that are complementary to college-going (e.g., if there is a critical period for investment at younger ages), then earlier resource shocks could produce larger effects.[35] Table 9 presents a specification that includes the interaction of a time trend, an indicator for whether the win occurred prior to graduation (i.e., treatment), and a continuous measure of the lottery win amount. The resulting coefficient on the fully interacted trend is statistically indistinguishable from zero, which, consistent with the earlier evidence, suggests equally large effects across the six years prior to high school graduation. Column 2 examines resource shocks earlier in childhood by expanding the

[35] A large literature has found significant effects of in-kind early childhood resources, including parental leave generosity (e.g., Dustmann and Schonberg, 2012; Carneiro, Loken, and Salvanes, 2015) and preschool programs (e.g., Deming, 2009; Heckman, Pinto, Savelyev, 2013). See Almond, Currie, and Duque (2018) for a recent assessment of the literature. Our context lacks the statistical power to generate precise estimates for income shocks affecting very young children. However, we are able to test for strong trends across the 14 years prior to high school graduation.

sample to include all children from households that won the lottery prior to their high school graduation that we can observe in our sample period.[36] The interaction term of interest is insignificant, indicating positive effects of similar magnitude across children aged 3 to 17. The final two columns combine the analyses by SES and timing and test, in both the baseline sample and the expanded sample, whether earlier wins generate relatively larger responses among lower-income households, which would be expected if borrowing constraints were inhibiting important earlier investments. There is no evidence that earlier wins generate larger (or smaller) effects for lower-income households.

d. Federal Financial Aid

Enrollment responses to lottery wins may be attenuated by the crowding out of financial aid. In particular, additional resources may reduce need-based assistance for children from lower-SES households. We first descriptively examine changes in financial aid within our design, revealing that the largest lottery wins reduce FAFSA application rates (despite higher rates of attendance), and wins of all sizes increase the average expected family contribution used for determining aid eligibility, and reduce loan and grant amounts (Table A32).[37]

To investigate if crowd-out of aid is meaningfully moderating our attendance estimates, we leverage a useful institutional feature of the primary formulas used for financial aid determination – that the marginal effective tax rate for parents' income can be quite high (20 to 50 cents on the dollar) whereas the effective tax rate for assets is far lower (several cents on the dollar) (Dynarski, 2004).[38] These rates imply a substantially different loss of aid depending on whether or not the win occurred in the year before high school graduation and is thus counted as income for determinations of financial aid eligibility for the first year of college (the "FAFSA Year"). To

[36] Figure A2 presents estimates for wins that occur in each of the 12 years prior to high school graduation. The point estimates in each treated year are positive and there is no clear change in magnitude across wins affecting younger and older children. Due to data limitations, the effects of wins in early childhood are not precisely estimated.
[37] This analysis is descriptive in nature because financial aid outcomes are endogenous to, among other things, college attendance, composition of attendees, aid application, and parental responses to lottery wins that affect income and asset holdings. There is no significant change in taking tax credits or deductions for tuition in response to lottery wins. In contrast to federal aid, eligibility for tax benefits is not a direct function of wealth (other than through investment income) and phases out at much higher income levels.
[38] There are two formulas used to determine aid eligibility, the Federal Methodology (FM) and the Institutional Methodology (IM). The FM, used by the Federal Government and most colleges, relies exclusively on information from the FAFSA, while the IM, used by some private colleges, relies on the FAFSA and supplementary information such as home equity. Both formulas treat assets much more favorably than income: the FM assesses parental income up to 47 percent and assets up to 6 percent, and the IM assesses parental income and assets up to 46 and 5 percent, respectively.

exploit this feature, we re-estimate Pell Grant receipt, first excluding and then restricting attention to wins that occurred the year before a child graduates from high school. As shown in Table 10, when lottery wins that occur in the FAFSA Year are excluded, grant reductions are small, but when attention is restricted to this year, the crowd-out of grants is large, especially for lower-income households. (Likewise, Table A33 shows imputed changes in state and institutional aid are concentrated in the FAFSA Year.[39]) However, the pattern of attendance estimates when excluding and restricting attention to the FAFSA Year are similar to the baseline estimates, both on average and for lower-income households.

Still, effects in the FAFSA Year may not be fully comparable to effects in all other years if there is treatment heterogeneity in the timing of the win. (Note that the prior results on timing are not prima facie consistent with such heterogeneity.) To consider this possibility, we test for a differential effect in the FAFSA Year after adding a linear time trend interacted with the interaction of amount of the win and whether the win occurred before high school graduation. The differential effect is small and statistically insignificant in all cases for the FAFSA Year (Table A34), including when examining only lower-income households. Altogether, these results imply that while a reduction in financial aid is a natural byproduct of winning the lottery, crowd-out does not attenuate the overall effect or explain heterogeneous responses observed across the income distribution.[40]

V. Alternative Household Responses

Households may also respond to resource shocks by altering their labor supply, savings, homeownership, and geography. To provide context for the college enrollment estimates against other potential spending priorities, we probe effects on these outcomes. Further, to shed light on

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[39] Because the observed financial aid outcomes are restricted to federal sources, we impute the effect of lottery wins on state and institutional aid for all households in the sample using the NPSAS. Appendix D presents details of the imputation procedure.

[40] These results do not imply that college enrollment is unaffected by financial aid more generally due to several mitigating factors. First, the change in aid is concurrent with a large increase in household financial resources, resulting in an atypical context for evaluating price effects that must assume that effects of resources and aid do not interact. Second, the resulting changes in aid from the resource shock may be exceptionally difficult to anticipate. And third, the exercise does not produce a precise enough estimate of the difference in attendance scaled by the loss of aid in the FAFSA Year relative to other years to rule out changes that are consistent with the price effects literature.

the external validity of lottery wins, we explore the degree to which observed household responses to lottery income are unusual.

a. Main Effects on Non-College Outcomes

Table 11 presents estimated effects on parental outcomes. For consistency, each outcome is examined within the same framework as attendance, focusing on the year a child graduates from high school. The labor supply results reveal evidence of reduced earnings, and, for larger resource shocks, a reduction on the extensive margin.[41] Interestingly, we find little effect on self-employment earnings. Perhaps the (implied) increase in leisure among lottery winners offsets the relaxation of financial frictions.[42] Large wins also generate increases in savings (i.e., interest and dividend income), but the effects of lottery wins for homeownership are more nuanced. For those without a mortgage prior to winning, there is an increase in having a mortgage even for moderate-sized wins, with the size of the effect increasing to 25 p.p. for very large wins. For those with mortgages already, households appear to use large wins to pay them off. All told, homeownership appears to be a significant spending priority. Households with large wins also move to slightly wealthier neighborhoods and those with modestly higher rates of college-going. However, when neighborhoods are classified on the basis of mobility by county (Chetty and Hendren, 2018), there is no evidence that these moves are to areas with greater upward mobility.

Table A36 briefly explores effects on children's labor supply. Results indicate that lottery wins reduce earnings in the year after high school graduation, with the effects increasing in the size of the win, broadly consistent with the pattern we saw earlier with respect to college attendance. That said, there is little evidence of an effect along the extensive margin, except in the largest win category, indicating that they are generally still employed in some capacity but less intensively than in the counterfactual state.[43] Note that, beyond a reallocation of time from work to college, the earnings reduction may partly reflect increased consumption of leisure. The final column

[41] The linear specification in Table A35 reveals that households reduce annual earnings by $1,170 in response to a win of $100,000. This is consistent with the estimate in Cesarini et al. (2017), which finds that earnings decrease annually by 1 percent of the prize amount in Sweden. Imbens, Rubin, and Sacerdote (2001) estimate elasticity of earnings with respect to payments of -0.10 match our estimates under the assumption of a discount factor of approximately 9 percent.

[42] Evidence in the literature on the effect of resource shocks on self-employment is mixed. While Holtz-Eakin, Joulfaian, and Rosen (1994), Lindh and Ohlsson (1996), Taylor (2001), and Andersen and Nielsen (2012) find positive effects, Cesarini et al. (2017) find a negative effect on self-employment and self-employment income.

[43] This is consistent with the finding in Keane and Wolpin (2001) that relaxing borrowing constraints does not change attendance decisions but does cause students to work less while they are enrolled.

examines children's earnings several years down the road—when they are 27 years old—with the pattern of results consistent with children induced to attend college by a lottery win having higher earnings in early adulthood, though the estimates are too imprecise to be conclusive.[44]

b. Persistence of Household Responses to Lottery Wins

While in standard economic models, a household's response to a resource shock does not depend on its source, in practice, one may be concerned that households differentially allocate prize money in a manner that could lead our estimates to misstate the effect of resources more generally on college attendance. (See Appendix E for a full exposition of this analysis.) In particular, households might "over-consume" lottery winnings, resulting in few resources being available for potentially high return college investment. A rapid depletion of lottery wins would manifest itself in short-lived changes in wealth and parents' earned income (and the presumed impact on leisure). Figure 4 and Table A38 examine the persistence in response in each of the five years following a lottery win for wealth, earnings, homeownership, and debt cancellation. Using our tax capitalization measure of household wealth, we find that approximately half of the post-tax lottery win is retained as wealth after five years. Further, estimated effects on earned income and new homeownership are stable over time, consistent with the predictions of a wealth shock in a standard lifecycle model. In the final column, we examine debt cancellation (a proxy for bankruptcy)—whereby increasing coefficients over time could signal that lottery winners are overspending, falling behind on payments, and going bankrupt—and instead observe a modest and persistent *reduction* in debt cancellation. These findings echo Cesarini et al. (2017), which finds evidence of persistent changes in earnings and wealth in response to lottery wins in Sweden. The estimates are also consistent with our earlier findings that enrollment effects persist over the four years after high school and are not concentrated among wins occurring shortly before high school graduation. Overall, we find evidence that supports the generalizability of our lottery-win-based estimates to other types of resources.[45]

[44] We examine the earnings of 27 years olds as a compromise between selecting an age that is correlated with later-life earnings and restricting the size of the sample. Note that, by this point, all children in the sample will be "post-win," although comparisons are still derived by the timing of the wins relative to high school graduation.

[45] We similarly find that, by examining the effect of lottery wins on the ownership of businesses and other risky assets in Table A39, households do not appear to be averse to investing their winnings in enterprises with relatively uncertain returns (which are similar to college in that respect).

VI. Potential Mechanisms

This section describes two leading (non-mutually exclusive) mechanisms that might explain why additional resources influence attendance and whether our findings are consistent with each.[46]

First, households may face financial frictions—such as a lack of access to credit or aversion to debt—that restrict college access for children who would otherwise earn high returns.[47,48] Much of the structural literature finds evidence that only a small fraction of households are constrained from sending their children to college due to financial factors alone (Cameron and Heckman, 1998, 2001; Keane and Wolpin, 2001; Carneiro and Heckman, 2002; Cameron and Taber, 2004). However, the rising cost of college and limited growth in the generosity of federal aid has increased reliance on private borrowing and may have increased the role of constraints (Belley and Lochner, 2007; Lochner and Monge-Naranjo, 2011).

If binding financial constraints restrict college access for children from a significant fraction of households, the estimated effect of an income shock on college-going would likely be concave, with moderate increases in resources leading to economically significant increases in attendance. For example, median tuition and room and board in 2011 was about $7,800 at two-year public colleges and $16,900 at four-year public universities, while average debt among four-year public college graduates was $25,600.[49] Thus, a $50,000 lottery win should eliminate the need to incur debt for most students; moreover, a $100,000 win is sufficient to fully cover four years at the

[46] We assess each explanation in isolation. While there are other potential mechanisms, they are unlikely to be primary explanations for the relationship we obtain. For example, additional resources could increase attendance by insuring against the risk of college investment. The results are not prima facie consistent with such an explanation, as effects would likely be larger among lower-SES households than higher-SES households, but we cannot rule out that complementarity between parental resources and children's attendance is a factor (though we would again expect similar heterogeneity responses).

[47] In addition to financial returns, a number of non-pecuniary returns to college may be valued by prospective students, including improved health, marriage opportunities, and workplace quality and benefits (Hamermesh, 1999; Pierce, 2001; Glied and Lleras-Muney, 2008; Chiappori, Iyigun, and Weiss, 2009; Silles, 2009; Oreopoulos and Salvanes, 2011; Lafortune, 2013).

[48] The U.S. is not unique in having a strong correlation between household SES and the college outcomes of children. Among the 32 OECD countries, the U.S. has the 13th largest gap in tertiary enrollment between children whose parents did or did not attend college (OECD, 2017). However, the U.S. is exceptional in terms of its high tuition levels and the fraction of educational costs that are borne by households; it has the highest average college tuition levels and the 3rd highest share of tertiary education funded by households rather than by public sources. Thus, the context of our analysis is one in which the role of household resources is not mitigated by universal subsidies for higher education. In many other countries, intergenerational persistence is unlikely to stem from the cost of college, but rather may be due to tracking into college and non-college streams at an early age (Orr et al, 2017; Crosier et al., 2018).

[49] Table A40 presents the distribution of tuition by college type, and Table A41 differentiates average costs and parental support by household income.

median college, even after taxes have been deducted. If attendance is deterred by a lack of cash-on-hand for incidental expenses, then even smaller resource shocks could be sufficient to generate a response.[50] However, the results are only weakly concave, and $100,000 is estimated to increase college going by less than one percentage point.

Further, if binding financial constraints were a primary factor in deterring college, we might expect the most financially constrained households to be the most responsive. Yet, households that have low income, low wealth, and are most likely to be denied credit seem, if anything, less responsive than other households. Reductions in financial aid that result from the lottery win and adversely affect constrained households the most do not appear to explain this difference. Likewise, some lower-income parents appear to reduce their labor supply in response to a win, which would be unusual if they were very financially constrained. Note that a variant of this mechanism—that households face financial constraints, but they bind earlier by limiting parental investment that is complementary with college—also appears to be inconsistent with the estimates.[51] Specifically, when we expand the sample to include a wide range of win timing prior to high school graduation, the effects are relatively stable, and earlier resource shocks are no more important for lower SES households. In sum, a "financial frictions" explanation does not appear to fit the overall pattern of results.

Second, households may derive consumption value from college, much as they do from normal goods. The consumption value of education has long been hypothesized in the literature, incorporated in human capital models, and is often needed to fit the data (Schaafsma, 1976; Lazear,

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[50] Some prior literature, including Stinebrickner and Stinebrickner (2008), Lochner and Monge-Naranjo (2011), and Cowan (2016), consider how, while loan and grant programs help finance the direct costs of attending school, prospective students may have difficulty smoothing non-schooling consumption. Consistent with increased resources reducing constraints on non-schooling consumption, we find in Table A36 a reduction in earnings among below-median income college students (who we can examine because there is no evidence of selection into college due to lottery wins). Still, reducing any such constraints does not appear to increase their attendance outcomes by much.

[51] We note three caveats: 1) we cannot test for very early life constraints, 2) our results are not inconsistent with an under-investment in schooling deriving from financial frictions in combination with other features, raising the possibility that policies that relax both financial constraints and, for example, informational frictions may still be effective, and 3) one piece of evidence that could be consistent with constraints inhibiting high return investment that is complementary with college is the finding that lottery winner's move to modestly wealthier and more educated neighborhoods. With respect to the third caveat, the effect on neighborhood could also be due to a consumption story (or possibly both) and appears too small to explain more than a fraction of the main effect (namely, even under the strong assumption that children adopt the same college-going rate as those from their new neighborhood, such neighborhood effects could explain only a fraction of the primary estimates). We also note that, while earlier wins may allow greater complementary investment, the additional resources from a lottery win may have been partially spent down by high school graduation, thus creating offsetting effects.

1977, Kodde and Ritzen, 1984; Eckstein and Wolpin, 1999; Keane and Wolpin, 1997, 2000; Carneiro and Heckman, 2002). More recent empirical research has documented evidence in the context of college (Arcidiacono, 2004; Alstadsæter, 2011; Alter and Reback, 2014; Jacob, McCall, and Stange, 2018, Gong et al., 2019). Predictions from a consumption channel are consistent with our findings. Namely, we would expect to see college attendance increasing in the size of the resource shock, with a high upper bound that is achieved at values that exceed the cost of college, which is indeed what we observe. We also find evidence of a disproportionate increase in attendance at four-year colleges that are likely to have higher consumption value, which we measure using the level of student services spending and the ratio of student services to total spending in the style of Jacob, McCall, and Stange (2018), and in zip codes with higher rates of college attendance (even conditional on zip code average income).[52,53]

That said, a potential tension with a consumption-based interpretation of our results is that typically one would expect lower-SES households to have a higher marginal propensity to consume, yet the heterogeneity analysis finds larger effects on attendance among higher-SES households.[54] However, there is evidence in the literature that higher income households may derive more consumption value from education (e.g., Carneiro and Heckman, 2002; Jacob and Lefgren, 2007; Jacob, McCall, and Stange, 2018) and that parental willingness to provide support for college may increase with SES (e.g., Brown, Scholz, and Seshadri, 2012), which could mean that lower-SES households have higher marginal propensities to consume but simply derive relatively more marginal utility from non-college consumption. To analyze differences in the marginal propensity to consume by SES, we implement an accounting exercise that digs deeper into key non-college outcomes. Specifically, under the assumption that the residual of earnings and savings responses to lottery wins is a consumption response, Table 12 explores potential

[52] The details of these exercises are presented in Appendix G. The estimates in Table A42 reveal statistically significant increases in attendance at colleges with higher levels of student services spending. These effects are driven by children from households with above median incomes. Table A43 presents evidence that attendance responses are larger for households residing, prior to the win, in communities with higher rates of college attendance (which could reflect unobserved preferences for consuming education), even after accounting for heterogeneity by average income in the community.

[53] The consumption value of college may accrue to children and/or parents. If it primarily accrues to parents, they may use college-contingent transfers to induce their children to attend, creating a direct link between parental resources and children's college choices (e.g., Keane and Wolpin, 2001; Weinberg, 2001; Brown, Scholz, and Seshadri, 2012).

[54] Several studies examining tax rebate windfalls document higher rates of spending for lower-income households (Souleles, 1999; Johnson, Parker, and Souleles, 2006; Parker et al., 2013) and others find no, or an opposite signed, difference (Shapiro and Slemrod, 1995, 2003; Parker, 1999; Agarwal, Liu, and Souleles, 2007).

heterogeneity in effects on earnings and savings by pre-win household income.[55] In addition to smaller increases in college-going, lower-income households have both a smaller earnings reduction and smaller savings increase, suggesting that, even though they are less responsive with respect to college attendance, such households consume a higher fraction of their lottery winnings.[56]

Several factors may help to explain why lower-SES households appear to place lower consumption value on college (or, more generally, be less responsive along this margin). First, children from lower-income households are less likely to be college ready than their higher-income peers.[57] While three-quarters of students from below median income households graduate from high school on time, less than half satisfy each of three measures of academic readiness that are highly correlated with college enrollment in the Education Longitudinal Study: graduating on time, having a 2.0 or higher GPA, and scoring above the bottom quartile on the PISA test (Table A44). To try to adjust for differences in college readiness, we scale the estimates by the fraction of students who could feasibly be induced to attend college based on several measures of readiness across the household income distribution. Scaling produces larger average treatment effects but does not eliminate the statistically significant gap by household income (Table A45). Second, prior to the win, lower-income households likely faced higher borrowing costs for other forms of consumption, thus altering the relative returns of allocating resources toward college versus paying off debt. Indeed, the SCF confirms that credit card and mortgage interest rates are modestly negatively correlated with income, although lower-income households have less debt on both the

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[55] Specifically, one can simply assume a consumption response = winnings + earnings response – savings response. For the purposes of this exercise, we are interested in the level effect on earnings, not the relative effect (which could be a proxy for the increase in leisure).

[56] We probe a couple alternative explanations for these results. First, we have ignored the role of housing, which has both consumption and investment qualities. Lower-SES households are much less likely to have a mortgage prior to their lottery win, and given the stark differences we found by whether there was a mortgage in the pre-period, they are more likely to have acquired a mortgage and less likely to have paid one off (results not shown). To address this issue, we estimate regressions where we allow for both differential effects of lottery wins by pre-win income and differential effects of lottery wins by the pre-win presence of a mortgage (to hold constant any differential effects due to the latter). The results continue to show smaller earnings reductions and savings increases (Table 12, columns 2 and 4). Second, the exercise could be confounded by differential investment returns within the class of investments we use to construct our savings proxy or the failure to include alternative classes of investments that might be more prevalent among lower-SES households. As discussed in Appendix F, we do not find evidence consistent with these issues and conclude that they are unlikely to overturn the results. In sum, the available evidence indicates that lower-SES households appear to consume a higher fraction of their lottery winnings but with less of this spending dedicated to children's higher education

[57] However, it is not clear if the overall set of potential compliers (i.e., college ready children who do not go to college at baseline) is higher among higher or lower-SES households, given the offsetting fact that higher-SES children are both more college ready but also much more likely to enroll in college absent treatment.

extensive and intensive margins (Table A46). Finally, lower-income households may inflate the price of college against alternative types of spending for which prices are more salient. There is evidence in the literature that the timing and complexity of financial aid causes a significant fraction of lower-income households to incorrectly perceive list tuition as the net cost of college (e.g., Dynarski and Scott-Clayton, 2006; Levine, 2014).

Overall, the results are not particularly consistent with financial constraints alone deterring investments in college that are then eased by the increased resources made available by a lottery win. Instead, the effects we detect are most consistent with spending on college increasing with resources in a fashion similar to normal goods.

VII. Relation to Prior Literature

Lottery wins generate pure resource shocks that are salient to households and easily measured by the researcher. As presented in Table 1, $100,000 more household income prior to college is associated with approximately 2.5 p.p. higher attendance at any college and 1.7 p.p. at four-year colleges (correlations that are likely upwardly biased by omitted factors such as college readiness and parental preferences), and our estimates explain one-quarter and one-third of these relationships, respectively. By tracing out causal effects across a wide range of resource shocks, for a wide range of household types, and across children of varying ages, our analysis provides useful context for interpreting estimates in the literature. For example, our results suggest that the wide range in prior estimates does not appear to be driven by the timing or amount of the shock or the population affected by the shock.

The estimated effects of household resources on college enrollment differ dramatically across studies, including across studies that exploit the same source of identifying variation (Table A48). Hilger (2016) finds that parental job loss reduces lifetime earnings by about $100,000, but college-going by less than 0.5 p.p,, while Coelli (2011) and Pan and Ost (2014) find job loss effects of about 10 p.p. Bastian and Michelmore (2018) report no significant effect of the Earned Income Tax Credit (EITC) on initial college attendance, while Manoli and Turner (2018) find an effect of 1.3 p.p. per $1,000 at the low income end of EITC eligibility (but an insignificant effect of 1.0 at the high end). Lovenheim (2011) finds that housing equity increases college attendance by 0.7 p.p. per $10,000. That is, the effects documented in several studies are similar to or smaller than those in our analysis, while others exceed cross-sectional differences by an order of magnitude.

Differentiating effects by household SES has been challenging in the literature due to the sources of variation used for identification. Analyses of the EITC are restricted to relatively small post-tax changes in income for lower-income households. Changes generated by job loss or housing equity are inherently tied to baseline income or housing wealth. The resulting findings with respect to heterogeneity are mixed. For example, Coelli (2011) and Hilger (2016) find smaller effects for lower-income households, while Lovenheim (2011) finds larger effects. Our findings over a wide range of resource shocks and households reveal that the effects are not highly concave and that the modest effects for lower-income households are not due to the magnitude of the change in resources they experience. Similarly, few papers that examine college-going differentiate effects by child age or grade, with most restricting attention to shocks that occur during high school. In cases where age is considered, the evidence is also mixed. For example, Manoli and Turner (2018) find significant effects if the EITC shock occurs in grade 12 but not 11; Bastian and Michelmore (2018) find effects of the EITC on high school and college graduation for exposure when the child is 13-18 years old but not earlier; and, Coelli (2011) finds effects if children are 16 or 17, but not 18. We are able to explicitly estimate effects by the timing of the win, revealing significant effects in each year prior to high school graduation.

The per-dollar relationships between income and college-going documented in this paper (and in the cross-section) are an order of magnitude smaller than the effects of changes in college price documented in much of the literature.[58] Nonetheless, different-sized estimates derived from price and income changes are not contradictory for several reasons. First, large price effects are consistent with education being a normal good and do not necessarily imply that households are credit constrained (Carneiro and Heckman, 2002; Dynarski, 2003).[59] Second, income effect studies generally examine the outcomes of all children affected by a household resource shock, while price effect estimates are often based on students who have applied to college or are eligible for merit-based grants. And third, if parents are not fully altruistic towards their children, then easing

[58] While some studies find modest changes in enrollment on the extensive margin, others find positive effects ranging from 1 to 5 p.p. per $1,000 (Card and Lemieux, 2000; Dynarski, 2003; Abraham and Clark, 2006; Kane, 2007; Cohodes and Goodman, 2014; Castleman and Long, 2016; Denning, 2017; Bettinger et al., 2019).

[59] Nielsen, Sorensen, and Taber (2010) and Fack and Grenet (2015) find substantial price effects in France and Denmark which they argue are highly unlikely to stem from easing of credit constraints. Fack and Grenet (2015) note that if a significant fraction of households are credit constrained, that could actually attenuate the effect of modest changes in college price on enrollment as households could not respond.

household income constraints will have more modest effects than reducing the prices faced by children.

VIII. Conclusion

This paper estimates the effect of household resources on college outcomes. It is the first study to exploit the universe of lottery wins in the U.S., and to leverage a wide range of resource shocks across a diverse population of households to examine changes in college-going. The analysis reveals several important results. Additional financial resources, including those at levels sufficient to cover college costs, have a modest effect on attendance. However, the effects are not highly concave and continue to increase for large resource shocks, reaching a high upper bound at win amounts far exceeding the cost of college. We also find that additional resources generate effects across each year prior to a student's high school graduation. The effects are concentrated at four-year institutions and are not temporary, as we observe significant increases in enrollment for several years after high school graduation. Households that are most likely to face financial constraints are not more responsive than wealthier households, and there is no evidence that financial aid crowd-out is shaping this result. Finally, winning parents also decrease labor supply and increase housing consumption and savings, with some evidence that lower-income households have a higher marginal propensity to consume. These findings provide valuable context for interpreting existing studies of college access.

In the current policy environment, parental financial frictions alone do not appear to hinder college attendance for a significant fraction of households. This conclusion has several implications. First, the current set of subsidies available for higher education may be sufficient to overcome market failures stemming from financial frictions, at least at current tuition prices. However, to the extent that parents are not fully altruistic towards their children, our results may still be consistent with *children* facing binding borrowing constraints. Second, redistribution of income towards lower-SES households is unlikely to be sufficient to meaningfully close enrollment gaps, unless the transfers are far larger than what could conceivably operate through the tax system. Policies seeking to raise educational attainment by distributing resources likely need to incorporate features that address other potential obstacles in the transition to college (e.g., college and career counseling, remedial programs). Third, the results raise a new question of why increasing the resources of lower-SES households appears to be especially ineffective. Such households may have weaker preferences for post-secondary education, larger academic or informational constraints, different norms about who is responsible for financing higher education (Sallie Mae, 2015), and other financial priorities that inhibit their responsiveness. Future work should explore which channels operate and how policy can remedy these gaps.

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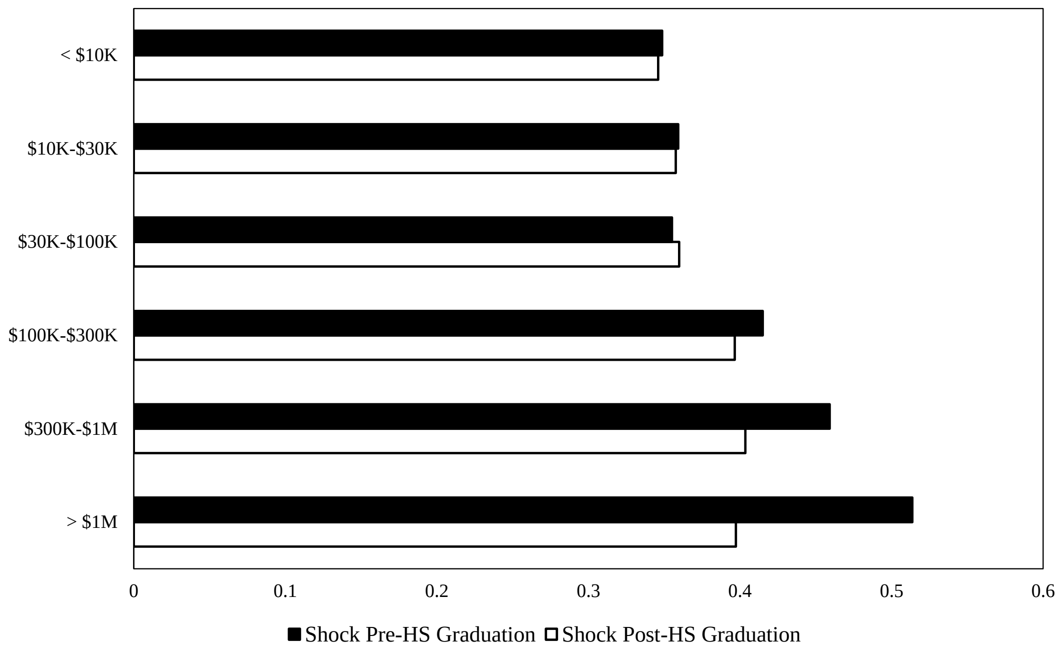
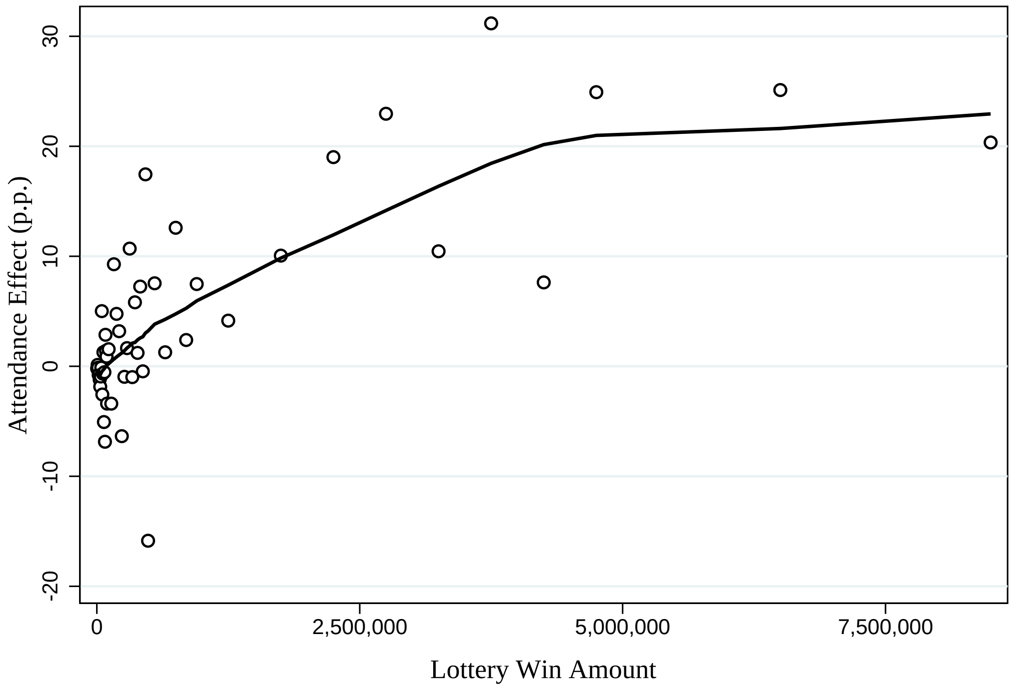
Figure 1: College Attendance Within One Year of High School Graduation
by the Amount and Timing of the Lottery Win



Note: This figure presents the average rate of attending any college for children who graduate before and after their parent wins a lottery. Attendance is measured in the year of expected high school graduation and rates are adjusted for cohort fixed effects. Win sizes are adjusted to 2010 dollars and are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, and $1,000,000.

Figure 2: Four-Year College Attendance: LOWESS Plot of Lottery Win Effects



Note: This figure presents a LOWESS plot fitted to the effects of lottery wins on four-year college attendance in the year of high school graduation. The effects are in percentage points and are plotted for increments of $5,000 up to $100,000, $25,000 up to $500,000, $100,000 up to $1,000,000, and $500,000 up to $5,000,000. Effect sizes are estimated relative to small wins of less than $1,000. Smoothing is based on a bandwidth of 0.8.

Figure 3a: Estimated Effects on Four-Year Attendance for Lottery Wins > $100,000 by Timing of Win Relative to High School Graduation

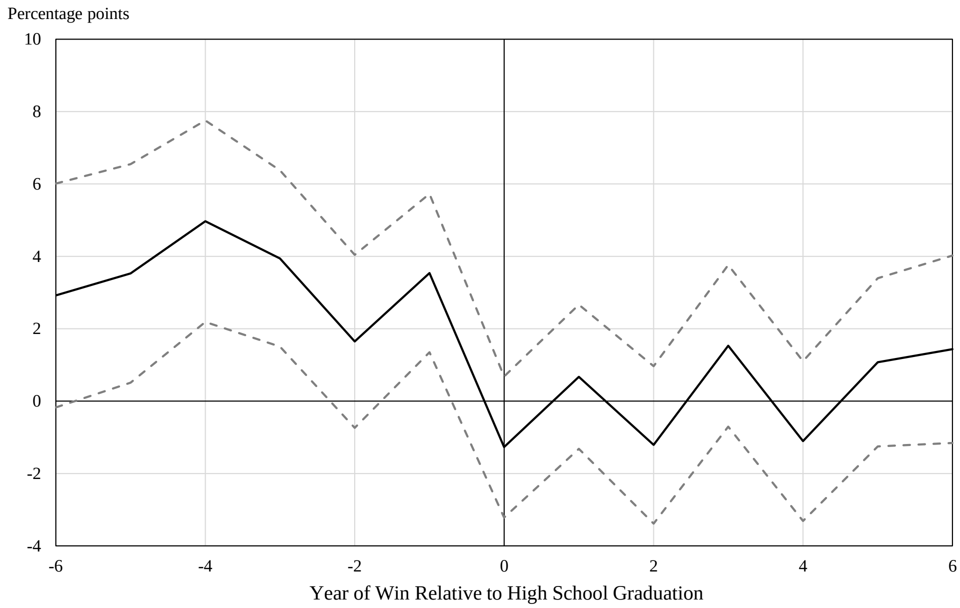
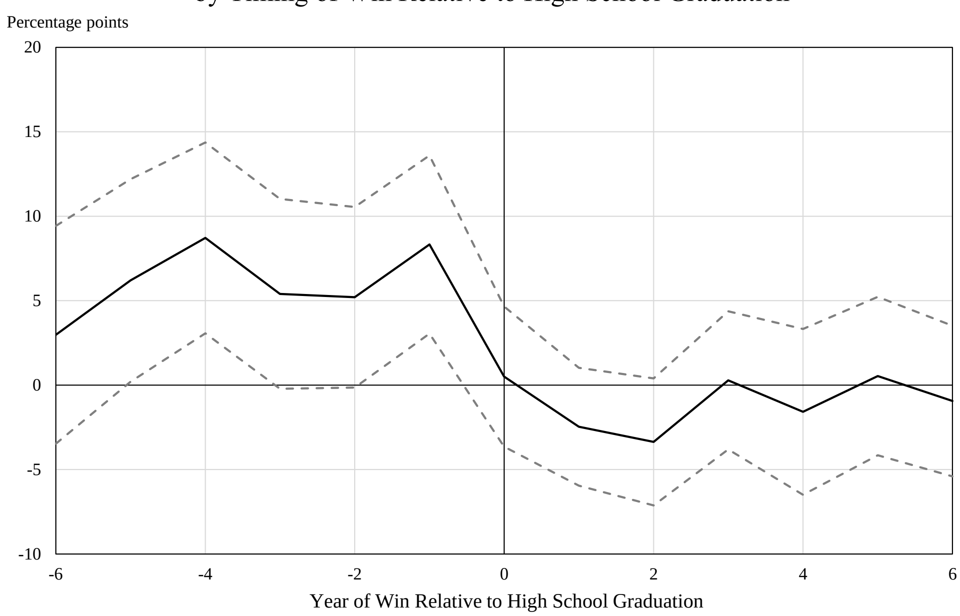
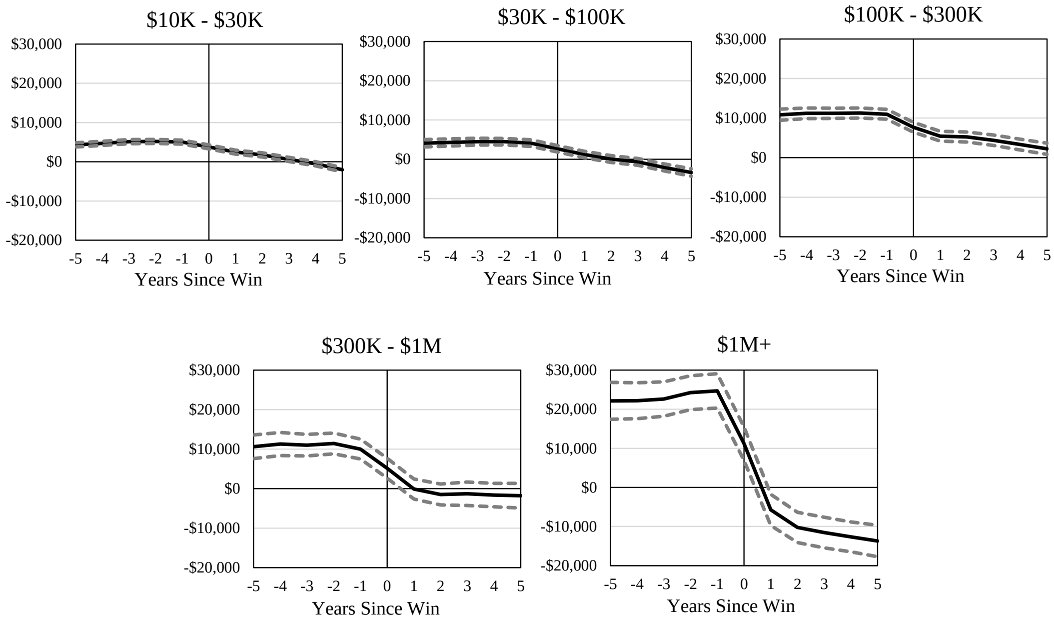


Figure 3b: Estimated Effects on Four-Year Attendance for Lottery Wins > $300,000 by Timing of Win Relative to High School Graduation



Note: This figure presents the estimated percentage point difference in four-year college attendance for children whose parents won a large lottery relative to those whose parents won a small lottery in each year before and after the expected year of high school graduation. The estimates account for state-by-year of win fixed effects, cohort fixed effects, parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, household number of children, and child gender, citizenship, and an indicator for Social Security birth match to parent, with the omitted variables being a large win and one of the year effects. All student and parent controls are based on pre-win measures. In the top figure, large wins are defined as those exceeding $100,000, while small lotteries are those of less than $100,000 (which average $3,294). In the bottom figure, large wins are those exceeding $300,000 and wins between $100,000 and $300,000 are excluded. Dashed lines depict the 95 percent confidence interval.

Figure 4: Parental Earnings Before and After Lottery Wins



Note: These figures present changes in parental earnings in the years before and after a lottery win relative to households with wins of less than $10,000. Estimates are presented for each of five larger win groups, corresponding to cutoffs of $30,000, $100,000, $300,000, and $1,000,000 or more. Earnings and win sizes are adjusted to 2010 dollars. Year 0 is the year during which the win occurred and thus is likely to represent partial treatment.

Table 1: Cross-Sectional Relationship Between College Enrollment and Household Income

	(1)	(2)	(3)	(4)	(5)
	Average Annual Income				
Four-Year College	$0-25k	$0-50k	$0-100k	$0-150k	$0-200k
Total Household Income Age 0-18 ($100k)	0.0123***	0.0153***	0.0166***	0.0172***	0.0168***
	(0.0006)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Mean Dep	0.107	0.136	0.183	0.205	0.211
Observations	377,386	840,957	1,316,239	1,446,270	1,474,902
	(6)	(7)	(8)	(9)	(10)
	Average Annual Income				
Any College	$0-25k	$0-50k	$0-100k	$0-150k	$0-200k
Total Household Income Age 0-18 ($100k)	0.0206***	0.0251***	0.0259***	0.0242***	0.0225***
	(0.0007)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Mean Dep	0.185	0.233	0.307	0.334	0.341
Observations	377,386	840,957	1,316,239	1,446,270	1,474,902

Note: This table presents the cross-sectional relationship between college attendance and household income during childhood in 2010 dollars, over increasing ranges of average household income. Total household income during childhood (in hundreds of thousands of dollars) is computed by taking the average of all available years of income from tax returns before the lottery win and multiplying by 18. The sample is restricted to households with at least five years of income data. Estimates are differentiated for households with average annual incomes of less than $25,000, $50,000, $100,000, $150,000, and $200,000. The top panel presents the relationship for four-year college attendance and the bottom panel presents the relationship for any college attendance. Errors are clustered at the household level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 2: Summary Statistics: Lottery Wins and College Attendance

Win Size Distribution	Number Children	Median Win	Mean Win	Mean After Tax	Subsequent Win >10k
Income Shock 600 to 10,000	1,365,498	1,189	2,047	1,671	0.03
Income Shock 10,000 to 30,000	62,239	11,900	15,252	12,034	0.04
Income Shock 30,000 to 100,000	19,608	50,000	52,152	40,319	0.04
Income Shock 100,000 to 300,000	10,318	153,421	169,383	121,551	0.04
Income Shock 300,000 to 1,000,000	2,301	525,000	568,269	381,548	0.04
Income Shock 1,000,000 or more	1,298	2,082,322	7,704,497	4,953,009	0.03
Cumulative College Attendance	Year 0	Year 1	Year 2	Year 3	Year 4
Any College	0.35	0.45	0.50	0.53	0.55
Four-Year College	0.22	0.27	0.30	0.33	0.35
Two-Year College	0.14	0.23	0.28	0.31	0.34

Note: This table presents summary statistics for the lottery wins that affect each child, as well as the timing of their initial college attendance. Column 1 of the top panel presents the number of children affected by wins in each of six size ranges: $600 to $9,999, $10,000 to $29,999, $30,000 to $99,999, $100,000 to $299,999, $300,000 to $999,999, and $1,000,000 or more. Columns 2 and 3 present the median and mean of these wins. Column 4 presents the mean lottery win amount after deducting income taxes, which are estimated using each household's income level in the year prior to the win. Win range categories in Column 4 are based on pre-tax amounts. Column 5 presents the fraction of children whose parents experience total lottery wins exceeding $10,000 in the four years after the initial win. The bottom panel presents the cumulative fraction of children in the sample who have attended college by each year after the expected year of high school graduation. Cumulative attendance is presented for any college, four-year college, and two-year college.

Table 3: Lottery Wins and Covariate Balance

Covariate		Mean	Win size (dollars)					
			10-30k	30-100k	100-300k	300k-1mil	1mil or more	F-test p-value
Children's characteristics								
Male	(1)	0.511	-0.0022	0.0061	-0.0125	0.0420*	-0.0496*	0.0848
			(0.0041)	(0.0073)	(0.0099)	(0.0216)	(0.0274)	
Citizen	(2)	0.964	-0.0005	0.0010	-0.0015	-0.0014	-0.0027	0.9918
			(0.0016)	(0.0030)	(0.0036)	(0.0079)	(0.0082)	
Parent and household characteristics								
Male	(3)	0.533	0.0019	0.0061	0.0010	0.0071	-0.0491*	0.6141
			(0.0045)	(0.0081)	(0.0107)	(0.0231)	(0.0298)	
Citizen	(4)	0.913	-0.0028	-0.0004	-0.0018	0.0090	0.0042	0.8563
			(0.0025)	(0.0047)	(0.0059)	(0.0121)	(0.0155)	
Birth Parent	(5)	0.633	0.0096**	0.0013	-0.0025	0.0126	0.0114	0.3610
			(0.0043)	(0.0077)	(0.0106)	(0.0226)	(0.0286)	
Number Children	(6)	3.454	0.0253*	0.0385	0.0149	0.0673	-0.0009	0.2698
			(0.0140)	(0.0256)	(0.0334)	(0.0709)	(0.0826)	
Married	(7)	0.569	0.0045	0.0122	0.0119	0.0304	0.0219	0.2186
			(0.0044)	(0.0079)	(0.0104)	(0.0225)	(0.0270)	
Missing 1040	(8)	0.030	0.0002	-0.0001	-0.0015	0.0037	-0.0005	0.7602
			(0.0006)	(0.0011)	(0.0014)	(0.0033)	(0.0030)	
Ln(Wages)	(9)	51,791	0.0121	0.0065	0.0221	-0.0418	0.0360	0.5544
			(0.0091)	(0.0163)	(0.0226)	(0.0471)	(0.0614)	
Ln(AGI)	(10)	60,467	0.0089	0.0030	0.0182	0.0107	0.0419	0.8943
			(0.0104)	(0.0183)	(0.0276)	(0.0558)	(0.0614)	
Self Employed	(11)	0.203	0.0011	0.0125*	0.0178*	0.0097	0.0502**	0.0474
			(0.0037)	(0.0068)	(0.0093)	(0.0200)	(0.0252)	
SSA Income	(12)	0.071	0.0029	-0.0013	-0.0079	0.0177	-0.0036	0.2388
			(0.0022)	(0.0041)	(0.0053)	(0.0111)	(0.0123)	
Parent College	(13)	0.088	-0.0036	-0.0012	0.0051	-0.0180	-0.0294	0.2714
			(0.0026)	(0.0046)	(0.0065)	(0.0144)	(0.0196)	
Mortgage	(14)	0.560	0.0031	0.0020	0.0120	0.0267	0.0010	0.6559
			(0.0044)	(0.0080)	(0.0104)	(0.0221)	(0.0275)	
Investment Income	(15)	0.487	0.0011	-0.0001	0.0097	0.0172	0.0051	0.9176
			(0.0044)	(0.0079)	(0.0107)	(0.0230)	(0.0281)	
F-test p-value			0.1652	0.8210	0.7748	0.3142	0.3693	0.5098
Predicted propensity to attend college								
College Propensity	(16)	0.3412	0.0013	0.0001	0.0058	-0.0028	0.0112	0.4798
			(0.0015)	(0.0027)	(0.0037)	(0.0079)	(0.0098)	

Note: This table applies the empirical design to child, parent, and household characteristics to test for balance. Each row represents a separate variable. The specification includes state-by-year of win and student cohort fixed effects. Household characteristics are based on the three years prior to the lottery win. All characteristics are binary variables except number of children, log income, log wages, and college propensity. Whether an individual is married is derived from filing status, number of children is derived from children ever claimed as a dependent, gender and citizenship status are based on Social Security records, parental college enrollment is based on the Form 1098-T, and wages, adjusted gross income, Social Security Income, having a mortgage, and investment income are derived from the Form 1040 and third-party mandatory reporting forms. Column 1 presents wage and income means in levels for ease of interpretation. F-tests of joint significance for each covariate are presented at the bottom of every column and across win sizes at the end of every row. An F-test for the joint significance of all covariates across all win sizes is presented at the bottom of the last column of the second panel. The measure of the propensity to attend college is the predicted probability that a student attends college based on coefficients derived from a cross-sectional regression of attendance on all 15 household characteristics for children too old to be affected by the lottery win. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 4: College Attendance in the Year of High School Graduation

	(1)	(2)	(3)	(4)	(5)	(6)
	Any College		Four-Year College		Two-Year College	
Win 10-30k Pre-HS Grad	-0.0022	-0.0033	-0.0016	-0.0025	-0.0023	-0.0026
	(0.0041)	(0.0038)	(0.0035)	(0.0034)	(0.0030)	(0.0029)
Win 30-100k Pre-HS Grad	-0.0101	-0.0100	-0.0075	-0.0075	-0.0043	-0.0042
	(0.0073)	(0.0067)	(0.0062)	(0.0059)	(0.0054)	(0.0053)
Win 100-300k Pre-HS Grad	0.0169*	0.0109	0.0188**	0.0143*	-0.0025	-0.0044
	(0.0101)	(0.0093)	(0.0091)	(0.0086)	(0.0074)	(0.0072)
Win 300k-1.0m Pre-HS Grad	0.0559***	0.0590***	0.0539***	0.0566***	0.0076	0.0082
	(0.0217)	(0.0200)	(0.0195)	(0.0185)	(0.0160)	(0.0159)
Win 1.0m or more Pre-HS Grad	0.1039***	0.0928***	0.1184***	0.1097***	-0.0005	-0.0036
	(0.0279)	(0.0267)	(0.0257)	(0.0246)	(0.0227)	(0.0226)
Child, Parent, and Family Controls		X		X		X
State-by-Year and Cohort	X	X	X	X	X	X
R-Squared	0.015	0.142	0.018	0.104	0.021	0.049
Mean Dep	0.348	0.348	0.219	0.219	0.144	0.144
Observations	1,461,262	1,461,262	1,461,262	1,461,262	1,461,262	1,461,262

Note: Estimates show the percentage point effect of income shocks on attending any college, a four-year college, or a two-year college in the year of high school graduation. Students for whom the win occurs prior to high school graduation are potentially affected. Columns 1, 3, and 5 include state-by-year of win and cohort fixed effects. Columns 2, 4, and 6 add parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, the number of children in the household, child gender, citizenship, and an indicator for Social Security birth match to parent. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 5: Four-Year College Attendance: Linear Estimates and Test For Concavity

		(1) Quadratic	(2) Linear
Range: 0 to 50 million	Pre-HS Grad * Win Amt ($100k)	0.003338***	0.000743**
		(0.000659)	(0.000311)
	Pre-HS Grad * Win Amt ($100k) ^{2}	-0.0000100***	
		(0.0000024)	
Range: 0 to 25 million	Pre-HS Grad * Win Amt ($100k)	0.005205***	0.001632***
		(0.000938)	(0.000446)
	Pre-HS Grad * Win Amt ($100k) ^{2}	-0.000025***	
		(0.000006)	
Range: 0 to 10 million	Pre-HS Grad * Win Amt ($100k)	0.007079***	0.004082***
		(0.001656)	(0.000765)
	Pre-HS Grad * Win Amt ($100k) ^{2}	-0.000048*	
		(0.000026)	
Range: 0 to 5 million	Pre-HS Grad * Win Amt ($100k)	0.007133***	0.005948***
		(0.002384)	(0.001209)
	Pre-HS Grad * Win Amt ($100k) ^{2}	-0.000049	
		(0.000082)	
Range: 0 to 2.5 million	Pre-HS Grad * Win Amt ($100k)	0.006362*	0.006655***
		(0.003360)	(0.001760)
	Pre-HS Grad * Win Amt ($100k) ^{2}	0.000026	
		(0.000262)	
Range: 0 to 1 million	Pre-HS Grad * Win Amt ($100k)	0.005155	0.008476***
		(0.005178)	(0.002590)
	Pre-HS Grad * Win Amt ($100k) ^{2}	0.000523	
		(0.000879)	
Range: 0 to 500k	Pre-HS Grad * Win Amt ($100k)	0.002671	0.006374*
		(0.008221)	(0.003752)
	Pre-HS Grad * Win Amt ($100k) ^{2}	0.001473	
		0.003037)	

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of high school graduation. The linear specification interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. A quadratic in win amount is used to test for concavity over various income shock ranges. Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects, cohort fixed effects, parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, the number of children in the household, and child gender, citizenship, and an indicator for Social Security birth match to parent. All student and parent controls are based on pre-win measures. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 6: Four-Year College Attendance: Alternative Specifications of Key Variables

	(1) Pre-Tax Income	(2) Post-Tax Income	(3) Enrollment Within Two Years	(4) Elasticity (Annual)	(5) Elasticity (Lifetime)
Win Amount ($100k) * Pre-HS Grad	0.0060***	0.0089***	0.0073***	0.0122***	0.2195***
	(0.0012)	(0.0018)	(0.0013)	(0.0025)	(0.0446)
Mean Dep	0.219	0.219	0.270	0.219	0.219
Observations	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890

Note: This table presents estimates of the effect of income shocks on four-year college attendance when accounting for taxes, when expanding the definition of enrollment to include the year after the expected high school graduation year, and as the elasticity of attendance with respect to household income. Estimates in columns 1 and 2 are the percentage point effect of income shocks on college attendance before and after taxes are deducted from lottery winnings, respectively. Taxes are estimated based on household income in the year prior to the lottery win. Column 3 presents estimates based on attendance in the year of expected high school graduation or in the subsequent year. The linear specifications in columns 1, 2, and 3 interact the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Columns 4 and 5 present the elasticity of attendance with respect to estimated average annual household income and total household income earned when the child is aged 1 to 18, respectively. Attention in each column is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively. See Appendix Table A9 for the full set of interaction terms and main effects for each specification.

Table 7: Four-Year College Attendance in Later Years and Cumulatively

	(1) Year 0	(2) Year 1	(3) Year 2	(4) Year 3	(5) Year 4	(6) Total
Win 10-30k Pre-HS Grad	-0.0016	-0.0036	-0.0015	-0.0025	-0.0027	-0.0331
	(0.0035)	(0.0037)	(0.0039)	(0.0043)	(0.0047)	(0.0208)
Win 30-100k Pre-HS Grad	-0.0075	0.0002	0.0011	0.0029	0.0011	0.0072
	(0.0062)	(0.0064)	(0.0068)	(0.0075)	(0.0083)	(0.0365)
Win 100-300k Pre-HS Grad	0.0188**	0.0244***	0.0259***	0.0225**	0.0255**	0.1249**
	(0.0091)	(0.0094)	(0.0099)	(0.0108)	(0.0119)	(0.0527)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0343*	0.0309	0.0444**	0.0397	0.2583**
	(0.0195)	(0.0194)	(0.0206)	(0.0227)	(0.0250)	(0.1091)
Win 1.0m or more Pre-HS Grad	0.1184***	0.1221***	0.1371***	0.1451***	0.0956***	0.6070***
	(0.0257)	(0.0272)	(0.0294)	(0.0316)	(0.0353)	(0.1545)
Mean Dep	0.215	0.242	0.238	0.238	0.219	1.162
Observations	1,461,262	1,461,262	1,292,594	1,060,514	840,030	840,030

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the years after high school graduation and the cumulative number of years of enrollment during this period. Year 0 refers to the calendar year in which a student is expected to graduate from high school based on his or her state and date of birth (baseline specification). Years 1 to 4 correspond to the subsequent calendar years. Students for whom the win occurs prior to high school graduation are potentially affected. Students who could endogenously change their enrollment decision are excluded in each column (e.g. the children of parents who won in Year 1 and Year 2 are excluded when estimating the change in enrollment in Year 3). The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 8: Four-Year College Attendance: Heterogeneity by Financial Constraints

Household tax records	(1) Income	(2) Investment Income	(3) Retirement Contribution	(4) Mortgage	(5) Imputed Wealth	(6) Pell Eligible	(7) Imputed EFC
Win Amt ($100k) Pre-HS Grad	0.0065***	0.0049*	0.0044*	0.0035	0.0070***	0.0051**	0.0048**
* Less Constrained	(0.0024)	(0.0025)	(0.0026)	(0.0026)	(0.0025)	(0.0024)	(0.0024)
SCF imputations	(8) Denied Credit	(9) Late Payment	(10) Payment -to-Income Ratio	(11) Debt-to-Asset Ratio	(12) Liquid Assets	(13) Liquid Assets + Home Equity	(14) Net Worth
Win Amt ($100k) Pre-HS Grad	0.0092***	0.0082***	0.0027	0.0045*	0.0077***	0.0087***	0.0078***
* Less Constrained	(0.0023)	(0.0023)	(0.0026)	(0.0026)	(0.0025)	(0.0025)	(0.0025)
Economic environment	(15) County Poverty Rate	(16) County Food Stamp Rate	(17) County Insured Rate	(18) County Medicaid Rate	(19) Zip House Values	(20) Zip House Values (w/ House)	(21) Great Recession Years
Win Amt ($100k) Pre-HS Grad	0.0042	0.0000	0.0065***	-0.0006	-0.0029	0.0015	0.0005
* Less Constrained	(0.0027)	(0.0028)	(0.0024)	(0.0025)	(0.0027)	(0.0036)	(0.0028)
	Mean Dep = 0.219		Observations = 1,460,890				

Note: Estimates show the differential effect of income shocks on attending a four-year college for households that are less financially constrained relative to those that are more constrained. The estimates are based on a linear specification that interacts the pre-tax win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation and a measure of household financial constraints. Estimates in the top panel are differentiated based on pre-win characteristics available in the tax records or estimated by applying the FAFSA formula to tax data. Less constrained households are those with above median income, any investment income, any voluntary retirement contributions, a mortgage, above median wealth, Pell Grant ineligible, or above median expected household contribution (EFC). The second panel differentiates the estimates by measures of financial constraints imputed using the Survey of Consumer Finances. Less constrained households are those that have below median probability of being denied credit, probability of making late payments, monthly payment-to-debt ratio, debt-to-asset ratio, or have above median liquid assets, liquid assets and home equity, or total net worth. In the bottom panel, less constrained households are those that live in counties with below median poverty, food stamp, and Medicaid rates, above median health insurance coverage rates, that live in zip codes with above median housing values, or who have children who graduate before or after the Great Recession years of 2008 to 2012. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively. See Appendix Table A22 for the sub-interaction term for each specification.

Table 9: Four-Year College Attendance: Heterogeneity by Timing

	(1)	(2)	(3)	(4)
	Average Trends		Trends by Income	
	6 Years	All Years	6 Years	All Years
Above Med Inc * Yrs Pre Grad * Win Amt ($100k) Pre-HS Grad			-0.0008	-0.0010
			(0.0014)	(0.0009)
Yrs Pre Grad * Win Amt ($100k) Pre-HS Grad	0.0001	-0.0001	0.0012	0.0007
	(0.0007)	(0.0005)	(0.0010)	(0.0007)
Mean Dep	0.219	0.224	0.219	0.224
Observations	1,460,890	1,902,983	1,460,890	1,902,983

Note: This table presents a test of whether the effect of a lottery win varies with the timing of the win relative to a child's high school graduation. The number of years between the win and the year of high school graduation is interacted with the win amount (in hundreds of thousands of dollars). Estimates are presented for wins that occur in the six years prior to graduation and for all lottery wins, extending as far back as 14 years prior to graduation. Columns 3 and 4 differentiate the effects for households with above and below median income ($44,699). Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively. See Appendix Table A31 for the full set of interaction terms and main effects for each specification.

Table 10: College Attendance and Federal Aid: Critical FAFSA Year

	(1)	(2)	(3)	(4)
	Non-FAFSA Year		FAFSA Year	
All Households	Attend 4-Yr	Pell Grants	Attend 4-Yr	Pell Grants
Win 10-30k Pre-HS Grad	-0.0020	-53.47	0.0002	-461.95***
	(0.0038)	(40.56)	(0.0058)	(55.23)
Win 30-100k Pre-HS Grad	-0.0055	-103.08	-0.0149	-817.47***
	(0.0067)	(71.17)	(0.0102)	(91.43)
Win 100-300k Pre-HS Grad	0.0193*	-108.10	0.0158	-556.27***
	(0.0099)	(93.86)	(0.0149)	(121.91)
Win 300k-1.0m Pre-HS Grad	0.0521**	261.11	0.0594*	-613.61**
	(0.0211)	(200.69)	(0.0339)	(252.73)
Win 1.0m or more Pre-HS Grad	0.1057***	-514.94**	0.1673***	-1,295.88***
	(0.0273)	(258.46)	(0.0489)	(240.61)
Mean Dep	0.219	1,577.16	0.219	1,577.16
Observations	1,317,523	1,317,523	961,290	961,290
	(5)	(6)	(7)	(8)
	Non-FAFSA Year		FAFSA Year	
Below Median Income Households	Attend 4-Yr	Pell Grants	Attend 4-Yr	Pell Grants
Win 10-30k Pre-HS Grad	0.0048	-54.18	-0.0030	-965.16***
	(0.0046)	(102.62)	(0.0070)	(144.42)
Win 30-100k Pre-HS Grad	-0.0098	-259.90	-0.0044	-1,791.89***
	(0.0082)	(183.33)	(0.0129)	(239.59)
Win 100-300k Pre-HS Grad	-0.0127	-129.53	-0.0057	-1,353.28***
	(0.0126)	(270.01)	(0.0197)	(351.55)
Win 300k-1.0m Pre-HS Grad	0.0199	401.76	0.0568	-1,505.48**
	(0.0272)	(563.83)	(0.0445)	(743.53)
Win 1.0m or more Pre-HS Grad	0.0355	-1,593.27**	0.0464	-2,841.31***
	(0.0402)	(811.50)	(0.0748)	(947.44)
Mean Dep	0.138	3,531.71	0.138	3,531.71
Observations	657,385	657,385	469,214	469,214

Note: Estimates show changes in the rate of four-year college attendance and receiving federal grants for all households in the top panel and households with below median income ($44,699) in the bottom panel. The first two columns exclude lottery wins in the critical FAFSA year (the year prior to high school graduation) and the next two columns only include the critical FAFSA year and post-graduation control years. Pell Grants are scaled by baseline attendance to reflect per-student changes. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 11: Parental Responses to Income Shocks

	(1) Earnings	(2) Any Work	(3) Self Employ Earnings	(4) Investment Income	(5) Mortgage (prior=0)	(6) Mortgage (prior=1)	(7) Zip Code Income	(8) Zip Code Coll Rate	(9) County Mobility
Win 10-30k Pre-HS Grad	-435.22	-0.0014	60.80	-23.73	0.0081**	0.0076*	101.83	-0.0005	0.0023
	(408.37)	(0.0031)	(89.35)	(41.24)	(0.0040)	(0.0042)	(227.80)	(0.0006)	(0.0021)
Win 30-100k Pre-HS Grad	-1,594.93**	0.0021	107.44	145.88*	0.0404***	0.0128*	252.69	0.0013	0.0020
	(703.14)	(0.0056)	(164.23)	(85.31)	(0.0079)	(0.0071)	(390.75)	(0.0010)	(0.0038)
Win 100-300k Pre-HS Grad	-904.11	0.0008	-313.72	267.42	0.0941***	-0.0049	617.34	0.0000	-0.0016
	(1,050.67)	(0.0073)	(228.89)	(172.57)	(0.0134)	(0.0092)	(530.38)	(0.00)	(0.0051)
Win 300k-1.0m Pre-HS Grad	-6,694.80***	-0.0501***	-201.55	762.31***	0.1336***	-0.0874***	1,025.37	0.0058*	0.0093
	(2,124.24)	(0.0168)	(504.01)	(219.83)	(0.0309)	(0.0218)	(1,179.12)	(0.0031)	(0.0105)
Win 1.0m or more Pre-HS Grad	-26,287.33***	-0.2401***	927.07	18,177.40***	0.2640***	-0.1634***	10,990.25***	0.0263***	0.0155
	(3,604.14)	(0.0237)	(770.96)	(1,876.93)	(0.0463)	(0.0293)	(2,205.51)	(0.0059)	(0.0166)
Mean Dep	51,281.80	0.824	2,086.84	428.92	0.028	0.910	51,427.58	0.287	0.006
Observations	1,461,262	1,461,262	1,390,302	1,461,262	643,511	817,751	1,390,355	1,369,923	1,383,768

Note: This table presents alternate parental responses to lottery wins, including earnings, employment, self-employment, investment income, having a mortgage, and zip code characteristics. Mortgage results are differentiated between those who have mortgages and may pay them off and those who do not have mortgages and may buy a house. Zip code income is the average zip code level adjusted gross income and zip code college attendance rate is the proportion of 17-year-old residents of the zip code that attend four-year colleges in the year of their expected high school graduations. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 12: Earnings and Savings: Heterogeneity Tests of Implied Marginal Propensity to Consume

	(1)	(2)	(3)	(4)
	Earnings		Investment Income	
Above Med Inc * Win Amt ($100k) Pre-HS Grad	-606.10**	-470.07**	157.15*	166.82**
	(237.27)	(221.61)	(81.48)	(67.37)
Home Owner * Win Amt ($100k) Pre-HS Grad		-303.34		-24.35
		(228.49)		(71.13)
Mean Dep	51,275.28	51,275.28	428.51	428.51
Observations	1,460,890	1,460,890	1,460,890	1,460,890

Note: This table presents tests of whether the effects of lottery wins on earnings and investments vary with household income. An indicator for a household having above median income ($44,699) is interacted with the win amount (in hundreds of thousands of dollars). Results are presented with and without including an interaction for home ownership. The specifications include state-by-year of win fixed effects and cohort fixed effects. Attention is restricted to lottery wins of 5 million dollars or less. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively. See Appendix Table A37 for the full set of interaction terms and main effects for each specification.

Appendix

Appendix A: Characteristics of Lottery Players and Lottery Winners

The analysis in this paper is based on households that won a state lottery of 600 dollars or more. However, lottery-winning households may differ in terms of observable and unobservable characteristics relative to the population, and these differences may affect the magnitude and pattern of college enrollment responses estimated in our analysis. This appendix provides additional details behind the examination of lottery winners in Section II. First, we document the approaches to examining the representativeness of lottery winners that have been adopted in the literature, and discuss how they inform our analysis and how we expand on them. Second, we carefully examine microdata from 10 years of the Consumer Expenditure Survey (CEX) to compare lottery-playing households to non-playing households on a rich array of characteristics, shedding light on the representativeness of lottery players in terms of household income, composition, race, education, debt, and expenditures. Third, we examine the characteristics of lottery-winning households relative to non-winning households in the tax data. In addition, we examine the college enrollment rates of the older, unaffected children of lottery winners to reveal the extent to which the children of lottery winners may have lower propensities to attend college due to unobservable factors (e.g., weaker academic preparation or less parental financial support).

a. Lottery Literature

Studies exploiting lottery wins have been conducted in various states and countries, relying on data from both surveys and administrative sources (Lindh and Ohlsson, 1996; Imbens, Rubin, and Sacerdote, 2001; Lindahl, 2005; Hankins, Hoekstra, and Skiba, 2011; Kuhn et al., 2011; Apouey and Clark, 2015; Cesarini et al., 2016; Cesarini et al., 2017). The literature primarily appeals to two classes of evidence to support the generalizability of lottery-based estimates: 1) the high rate of lottery playing in the population; and 2) the relative similarity of lottery winners and non-winners in terms of observable characteristics. For example, Kuhn et al. (2011) report that 25 percent of Dutch households play the postcode lottery, and Apouey and Clark (2015) note that 57 percent of the population plays the UK National Lottery. In the U.S., Kearney (2005) finds that 56 percent of people play the lottery and that spending on lottery tickets is approximately equal across the lowest, middle, and highest income groups, across men and women, and across racial groups. These findings are consistent with national surveys such as the National Opinion Research Center Survey on Gambling and the Gallup Poll, which report that 51 and 57 percent of adults buy at least one lottery ticket each year, respectively.

With respect to characteristics, Imbens, Rubin, and Sacerdote (2001) compare the sex, age, education, and earnings of lottery winners who responded to their survey to Current Population Survey respondents, and find that lottery winners are, on average, older and have higher education and earnings. Although the differences are not very large, these findings highlight the need to compare lottery winners to a relevant subgroup of the population. Hankins, Hoekstra, and Skiba (2011) note that the bankruptcy rate observed among lottery winners (1 percent) is similar in magnitude to the rate for the population overall. Cesarini et al. (2016) and Cesarini et al. (2017) compare Swedish lottery winners to a sample of the population that has the same distribution of age and sex, revealing that winners have relatively similar rates of education, earnings, and health to non-winners.

We follow the literature in documenting the rate of lottery playing and examining how the observable characteristics of lottery winners compare to those of the broader population. To generate valid comparisons, we focus on households in the population with same-aged children. With respect to the representativeness of our sample, the analysis benefits from exploiting the universe of lottery winners across every state, resulting in a sample that is geographically diverse and large enough to generate precise estimates across the socioeconomic distribution. We are also able to examine if the children of lottery winners have different propensities to attend college than the population due to unobservable factors by considering the college outcomes of their older, unaffected children.

b. Lottery Players

We analyze microdata from 10 years (2005 to 2014) of the Consumer Expenditure Survey (CEX) to document the characteristics of lottery playing households relative to non-playing households. While our analysis is based on lottery winners, and not players, this survey enriches the analysis due to the availability of demographic characteristics that are not available in the tax data, as well data on saving and monthly expenditures. Within the CEX, we find that approximately 1 out of every 4 families reports playing the lottery, and those that do spend an average of $266 on tickets per year. As shown in Table A2, families that purchase tickets do not differ substantially from families that do not across a wide range of characteristics, including race, highest education, family size, and age of children. The only dimension along which they noticeably differ is income, with lottery playing families generally earning more than non-playing families.

Of particular concern is that lottery players may be more likely to spend more and save less than typical households. This could occur if, for example, playing the lottery is a response to financial problems, or if the types of people who play the lottery have lower financial literacy. The data indicate that differences on these margins are not large. Lottery playing households have slightly higher monthly expenditures, which is consistent with having higher incomes, as well as higher levels of savings. They spend slightly more on housing and education, have more savings and nearly identical levels of stock holdings, and are more likely to own a home. Overall, there is no evidence that lottery playing households are substantially different from other households in terms of their propensities to spend and save.

c. Lottery Winners

We compare lottery winners to the population using the tax data. We select a large random sample of families with children of the same age from the population in order to generate a suitable group for comparison. Parent-child matches and household characteristics are constructed in the same way as for lottery winners. Table A3 presents household characteristics, including marital status, mean and median earnings, adjusted gross income, and number and gender of children for lottery winners and the random sample of the population. Lottery winning households are 3 percentage points less likely to be married, have nearly identical median earnings, and have lower average earnings than the random sample. That is, the highest income households are somewhat less likely to have a state-reported lottery win, but lottery winners have significant common support with the population. If the primary differences between winning and non-winning households are captured by these observable characteristics, then reweighting the estimates to reflect population characteristics will generate an estimate of the population average response to income shocks. The wide range of households observed winning lotteries allows us to implement this exercise, which is discussed in Appendix C.

However, the estimates in this study may understate the population response to the extent that the children of lottery winners are less responsive to resources for unobservable reasons, which could be true if they have lower propensities to attend college due to lower academic aptitude or preparation, or due to the children or parents having weaker preferences for education. As documented in the second panel of Table A3, the children of lottery winners are somewhat less likely to attend college. Approximately 35 percent of the children of lottery winners attend a two-year or four-year college in the expected year of high school graduation, while 39 percent of same-aged children in the population attend. This gap is likely to be driven in part by differences in household characteristics, such as the lower average income of lottery winners. To examine how much of the enrollment gap remains after accounting for these differences in characteristics, we first restrict attention to the older, unaffected children of lottery winners in order to abstract from the effect of lottery wins on attendance. We then estimate the difference in the college attendance rate between these older children and the random sample (similarly limited to older children) while controlling for observable characteristics (marital status, earnings, income, household size, and child gender). This reveals modest differences in the college enrollment rate, as the unaffected children of lottery winners are only about 1 percentage point less likely to attend college than the broader population. Thus, there is little evidence to suggest that the estimates in our analysis are significantly attenuated by the unobservable characteristics of lottery winners. While lottery winners have somewhat lower average income than the population of households with same-aged children, they share significant common support and are not fundamentally different in terms of the propensity of their children to attend college.

d. Cross-sectional Relationship between Household Income and Attendance

Table 1 presents the cross-sectional relationship between college attendance and household income for lottery winners in order to provide context for our estimates and those in the related literature. Of particular importance is the relevant time frame over which to measure household income. A lottery win is a one-time increase in income and thus is most naturally compared to total income over some relevant time period rather than to permanent average income. Thus, we present the effect of $100,000 of total pre-college income, that is, the sum of household income from the time of a child's birth until he or she graduates from high school at age 18. We estimate total pre-college income using all available years of tax data and then extrapolating to 18 years. As reported in Chetty, Hendren, and Katz (2016) and Chetty and Hendren (2018), five years of income after age 30 provides an accurate measure of lifetime income when mapped to a lifetime profile of earnings between age 26 and 65.[1] Thus, we limit this exercise to households for whom we observe at least five years of tax returns. The estimates indicate a correlation between college attendance and household income during childhood of about 2.6 p.p. for any college and 1.7 p.p. for four-year colleges. The relationships for households with average incomes of less than $25,000 are slightly smaller, averaging 2.1 and 1.2 p.p. Estimates based on enrollment within two years of high school graduation (an expanded measure presented throughout the paper) are approximately the same, while those based on the random

[1]We cannot observe income for some households when the child is very young, a time when income may be relatively lower. However, accounting for the life cycle income profile suggests the effect of this on our measure of total childhood income is modest.

sample of the population are slightly larger. These correlations will overstate the causal effect of income on college outcomes to the extent that household income is positively correlated with other determinants of college enrollment such as child aptitude, local school quality, parental expectations and preferences (e.g., parents with a college degree are both higher income and plausibly more likely to want their children to attend college), and support in the application process. In case liquidity is an important determinant, we consider whether a cash-on-hand measure is more influential than a total childhood income measure and test whether income in the year immediately prior to college has a stronger correlation with college attendance than average income in all pre-college years by including both in the specification. The resulting coefficient on income in the year prior to college is small in magnitude and insignificant.

Appendix B: Design Validity

The difference-in-differences design used in our analysis exploits both variation in the size and the timing of lottery wins. The college outcomes of children are compared across households that win larger and smaller amounts and as a function of whether the parents won the lottery before or after the children's high school graduation years. The identifying assumption is that the differential propensity to attend college across lottery win amounts is the same for children whose parents win the lottery before or after they graduate from high school. Table 3 presents evidence of the balance of the design for a rich set of student, household, and parent characteristics. This section presents the details of four additional tests of the validity of the design. First, we examine the rate of lottery winning by parents before and after their children graduate from high school, as well as the characteristics of these households. Second, we replicate the balance test for the distribution of parental income (as log income may obscure differences across the distribution), the college outcomes of older, unaffected siblings, and restricting comparisons of financial characteristics to households in which children are the same age. Third, we conduct a placebo test using a false lottery win date prior to the true date. And fourth, we test for differences in the propensity of children to attend college across lottery bins, as heterogeneous responses across win amounts due to unobservables could shape the pattern of results.

a. Lottery Win Timing

Identification is based in part on comparing children who have not yet graduated from high school when their parents win the lottery to those who have already graduated and made their initial enrollment decisions. Ideally, households that win the lottery when their children are in high school would be similar to those that win in subsequent years (though this is not an assumption of the design). This might not be the case if, for example, some households are induced to play in anticipation of college costs. Figure A1 presents the rate at which households win in each year before and after high school graduation. The rate of winning is measured relative to the population of households with same-aged children and is indexed to 1 in the expected year of high school graduation. The figure reveals no discontinuous changes associated with the child graduating from high school. Specifically, the rate of lottery winning increases gradually by about 2 percent per year as children age. Likewise, there is no evidence of a systematic change in the median income for households that win before or after their children complete high school. It is important to note that shifts in the rate of playing or differences in the characteristics of households that win lotteries before and after their children graduate from high school would not invalidate the design. That is, identification only requires that pre-post differences have the same magnitude across win sizes, as pre-post differences common to all win amounts will be differenced out in the design.

b. Extended Balance Tests

Table 3 examines balance in log earnings in the design, but may not detect differences in the distribution of earnings. For example, households that win large lotteries after their children graduate from high school may have higher variance in earnings than households that win large lotteries prior to their children graduating. This is a concern because propensities to attend college do not vary linearly across the income distribution. Thus, we replicate the balance test for dummy variables representing whether a household falls into various parts of the income distribution. Specifically, households are classified as having income that fall into each of the following percentile ranges: 0-10, 10-25, 25-50, 50-75, 75-90, and 90-100. The baseline distribution for comparison is households that win the lottery prior to their children graduating from high school. As reported in the top panel of Table A4, the test is balanced across each part of the distribution and overall, with a p-value of 0.56.

The college outcomes of older, unaffected siblings provide a unique opportunity to test for bias. Specifically, significant differences between these older siblings, none of whose college outcomes in the year of high school graduation should be influenced by lottery wins, could suggest that unobserved factors may invalidate the comparisons exploited in the design. Thus, for each child in the sample, we identify whether or not they have an older sibling in the sample who completed high school prior to the lottery win, how many such siblings they have, whether the siblings attended college, the types of colleges they attend, and whether the siblings received Pell Grants or subsidized loan aid. For children with multiple older siblings, the fraction of siblings attending college or receiving aid is used as the sibling outcome. The bottom panel of Table A4 shows that each of these sibling measures is balanced and that the overall p-value is 0.32.

The household financial characteristics used in Table 3 are measured in the three years prior to the lottery win. As a result, the parents of control children, defined as those for whom the win occurs after high school graduation, are, on average, older and thus may have higher earnings. This systematic difference in the timing of when financial characteristics are measured does not generate imbalance in the design because covariates are measured at the same ages for households who win small and large amounts in each difference. Nonetheless, it is informative to examine if balance is maintained when household characteristics are measured when children are of a fixed age. To implement this test, we measure household characteristics 7 years prior to each child's high school graduation, a window just outside the criterion for a child in a lottery-winning household to be included in our sample. Measuring characteristics closer to graduation would be problematic, as it would include households after they have won the lottery (as we use a 6-year window for the baseline sample), which has a direct effect on financial measures. However, we note that some households are not observed 7 years prior to when their children graduate from high school due to data limitations (i.e., earnings are first observed in 1996). Table A5 presents the results of this fixed age test, revealing balance across financial measures, with a p-value of 0.71.

c. Falsification Test

We implement a placebo test for the primary design by assuming that lottery wins occur three years prior to the actual date among households in our sample that won the lottery after their child graduates from high school, effectively splitting the control group in two. That is, we assume that children who graduated 1, 2, or 3 years prior to the win are treated, while those who graduated 4, 5, or 6 years prior to the win are not. If this test reveals significant effects on college attendance, it suggests that there are differential trends in college outcomes across larger and smaller lottery winners that are correlated with the timing of parental lottery wins. Table A6 presents the results of the test with and without the inclusion of child, parent, and family controls. The estimates reveal no evidence of statistically significant effects for any college, four-year, or two-year college attendance. More refined placebo tests conducted on shorter windows also produce statistically insignificant effects, but are too imprecisely estimated to be informative.

d. College Attendance Propensities across Win Amounts

As noted in Section II, households that win smaller lotteries tend to have lower incomes, which is consistent with the finding in Oster (2004) that larger prizes make lotteries less regressive. This is not a challenge to identification as long as the differences are the same for households that win lotteries before and after their children graduate from high school, which the balance tests help confirm. However, differences across win amounts could pose a challenge for the interpretation of the pattern of estimates due to treatment heterogeneity. Specifically, if households that win smaller lotteries tend to be less responsive in terms of college attendance than households that win larger lotteries, then the effects will be muted for smaller win amounts.

In Appendix C, we examine the effect of weighting households such that observable characteristics match across lottery win amounts. This exercise will make the estimates across win size bins comparable if the issue is solely due to differences in observables, and the results indicate that any difference in observables across win amounts is not influencing our pattern of estimates. However, it will not address treatment heterogeneity due to any unobserved differences, including in the propensity to attend college across win amounts.

To examine if children in households that win smaller lotteries have lower propensities to attend college that are not captured by observables (e.g., due to academic preparation or parental support), we examine the attendance rates of older children who could not have been affected by their parents lottery wins at the time of their college enrollment decisions. That is, we regress the college outcomes of children in the control group on their lottery win size indicators while controlling for household characteristics. The results of this exercise are presented in Table A7, revealing no statistically significant differences in attendance for any of the win size bins once we condition on observables. Overall the bin indicators are jointly insignificant with a p-value of 0.27. This suggests that any differences in the propensity to attend college across the win size distribution are not large. These exercises indicate that the pattern of results across win amounts will not be distorted by treatment heterogeneity due to differences in observable household characteristics or by unobserved differences in propensities to attend college.

Appendix C: Robustness

This appendix examines the robustness of the estimates to alternative methods of constructing the sample and defining the treatment. We also examine how the estimates vary with alternative designs that exploit only the variation in win size, variation in win timing, and that scale to account for endogenous changes in household income.

a. Alternative Samples and Specifications

Table A10 presents estimates using lottery wins that occur in varying bandwidths around a child's predicted year of high school graduation. This exercise has two primary purposes. First, it reveals whether the lack of large effects for modest resource shocks is due to households spending down lottery wins prior to when they would be used for college. This could result in larger effects when restricting attention to wins that occur close to high school graduation and smaller effects for wider treatment windows. We do not observe this pattern of results, as even restricting the sample to wins within 1 or 2 years of high school graduation does not produce significant positive effects for smaller wins. Additionally, restricting attention to households whose parents win a lottery close to the year of their child's graduation addresses concerns about the comparability of the treatment and control groups in the time dimension. For example, households who win a lottery when their children are 17 and 19 may be better matched in terms of unobservables than households that win when their children are 16 and 20. However, we observe a similar pattern of results with wider and narrower bandwidths.

Table A11 presents an array of alternative sample choices. Column 2 presents the estimates when children are matched to parents based on Social Security Card Applications to focus on birth parents. The primary disadvantage of this approach is that data are first available in 1983, so the sample is significantly smaller. An additional concern is that a birth parent may no longer be involved in a child's life by the time of high school graduation. This disconnect could differentially attenuate the estimates for children from socioeconomic groups with higher rates of divorce or absentee parents. The estimates generated by matching children to birth parents are insignificant for wins of less than $100,000, and 3, 7, and 10 p.p. for wins of $100,000 to $300,000, $300,000 to $1,000,000, and $1,000,000 or more, respectively. Thus, on average, the estimated responses are not smaller than those based on matching lottery winners to claimed children.

In some cases, it is not possible to determine with certainty the year or size of a household's first lottery win and thus assumptions must be made in order for the winner to be included. Such households are omitted from the baseline sample presented in the paper, as they may introduce measurement error, but estimates that include them are presented in column 3 of Table A11. This sample includes cases in which it is not possible to determine which of multiple wins in the same year occurred first, so we assume the largest win is the first win. The sample also includes cases in which a win is paid out over multiple years (which constitute about 2 percent of all wins) by predicting their lump sum equivalent. For wins that may be truncated by the last observed year of data, we project the expected number of years that payments would be received. Projections are based on annual payouts that occur early in our observed period and thus for which we have a relatively complete picture of the typical pattern of payouts. We note that it is sometimes the case that lottery winners have the choice between one-time and annual payouts and that they may have different totals. Finally, in a handful of instances, there is ambiguity with respect to the true year in which a lottery win occurred, as indicated by the presence of supplemental income in the year prior to state reporting that is equal in size to the win. Including each of these three cases results in a sample that is 15 percent larger, and estimates that are similar to those for the baseline sample. Specifically, we find no statistically significant effect for moderate wins, and effects of 2, 7, and 10 p.p. for the largest lottery win ranges.

The estimates throughout the paper exploit comparisons between larger and smaller lottery wins (and account for fixed differences between these households using unaffected, older children). The small win control group used in the main specification consists of wins of less than $10,000 but greater than $600 (the IRS mandatory reporting threshold), which average about $2,000. There is a fundamental trade-off between increasing the minimum win included in the control range. A higher cutoff may result in households that are more similar to those with large lottery win households (in terms of observable characteristics), but the control group is treated by a more substantial win and thus could attenuate the relative treatment effect of larger wins. Columns 4 through 6 of Table A11 present estimates for three alternative control ranges where the lower and upper bound are adjusted. Since the $600 dollar reporting threshold for the IRS is arbitrary, we increase the lower bound to $1,000 and then $5,000 in columns 4 and 5, resulting in, on average, larger win households as the control group. Then, Column 6 uses only the smallest wins of less than $1,000 as controls. While these alternatives dramatically change the size of the sample (since small wins are common), they have essentially no effect on the point estimates: wins of less than $100,000 remain insignificant, and larger wins have effects of 2, 5, and 12 p.p., respectively.

Column 7 of Table A11 includes wins that occurred in the year of a child's high school graduation, which are omitted from the main analysis since it is not clear if such wins are too late to have an effect. This results in slightly attenuated estimates, which is consistent with misclassifying treatment status. Column 8 relaxes the school entry age cutoff in each state by two months. This will capture, for example, children whose school entry dates are misclassified due to starting school in states other than the ones in which they are born or whose parents did not enroll them in the expected year. The resulting estimates of 2, 5, and 14 p.p. are slightly larger than those in the baseline sample.

Columns 9 and 10 present estimates for specifications that include winner fixed effects, such that the variation stems from children born before and after a lottery win within the same family. The primary challenge of this approach is that it can only leverage children from households for whom the win occurs after one child graduates from high school and before another graduates. This approach drops all one-child households, 67 percent of two-child households, and 43 percent of three-child households. Thus, the resulting estimates are based on a sample that is only 26 percent of the size of the full sample and is mechanically weighted toward larger families, making it less representative of the overall population. The standard errors from this approach are substantially larger. The point estimates for the three largest win ranges are 1, 5, and 8 p.p. for attendance in the year of high school graduation and 1, 5, and 14 p.p. for attendance in the year of high school graduation or the subsequent year. Under the linear specification, the estimates are 0.4 and 0.6 p.p. per $100,000 for attendance in the year of high school graduation and in the expanded definition of attendance, respectively. (In the interest of space, these results are not presented in the exhibits.) In no case can we reject the null hypothesis that the estimates from this sample are the same as those from our main sample.

The final two columns involve reweighting the sample of households. In column 11, households are reweighted such that the sample of lottery winners matches the characteristics of the population of households with children of college-going age. Because lottery winners are reasonably similar to the population, this results in no meaningful change in the estimates. Column 12 reweights households such that each lottery win size bin has the same average characteristics. This helps assess whether the relative magnitudes of effects across win sizes are driven by treatment heterogeneity due to differences in observable characteristics across win sizes (e.g., if higher income households tend to have larger enrollment responses to resource shocks and are more likely to win larger lotteries). The resulting estimates are similar to our main estimates (insignificant for moderate wins and 2, 5, and 11 p.p. for the largest three win ranges, respectively).

There are a sufficient number of individuals with smaller lottery wins to generate precise estimates for narrower win ranges in the step function specification. Table A12 presents estimates for 10 win size ranges relative to the smallest win size (compared to 5 in our main analysis). The resulting estimates reveal that there is no pattern of positive effects for wins of less than $100,000 that is being obscured by the specific choice of cutoffs. The table also presents a separate estimate for very large wins exceeding $3,000,000, which reveals even larger point estimate than those for wins greater than $1,000,000 and is consistent with the lack of concavity discussed in Section IV.

Some colleges may not report 1098Ts (from which we observe enrollment) to the IRS for students whose grants meet or exceed tuition billed because they are not eligible for tax credits. Thus, non-classical measurement error from potential underreporting could bias the estimates to overstate the effects of additional resources, particularly among lower-SES households (who are most likely to receive full scholarships). To mitigate these concerns, we conduct two exercises. Here, we diverge from the rest of the robustness exercises and present results for any college attendance instead of four-year attendance (which, as Table 4 and other exercises in the paper make clear, drive the effects on any college in the main analysis): 1) because there is difficulty in inferring the school level from the Federal aid data and thus classifying the attendance level for the students we can only observe via this method, and 2) because it is probably more likely that grant aid would fully cover tuition at community colleges and so we would want to be as general as possible in how we measure attendance to examine whether fully covered students are biasing our results.

As presented in Table A13, we first omit colleges that appear not to report (or appear to under-report) students that receive full grant aid (i.e., do not pay tuition) in the tax data. Column 2 sets attendance to 0 for all students attending colleges that seem most likely to not be filing 1098-Ts for students receiving full scholarships. These colleges are identified as having close to 0 percent of students with 1098-Ts that show grants equaling total tuition billed. To verify this approach, we identify colleges that have stated explicitly that they do not file a Form 1098-T for students with full scholarships, and confirm that our method correctly identifies these colleges. This approach will necessarily attenuate estimates since all attendance, including causal increases, can no longer contribute to the estimated response. Nonetheless, the estimates are insignificant for modest wins and 2, 5, and 9 p.p. for the larger wins. Column 3 omits these students from the sample and generates estimates of 2, 6, and 10 p.p. for the larger wins. Second, we supplement attendance as measured using only the tax data with attendance that can be observed through federal grant aid receipt. Specifically, we exploit the fact that the Department of Education financial aid data reveal students who are receiving federal grants. These students are those most likely to be omitted when relying exclusively Form 1098-T. Thus, we construct a new measure where a student is classified as enrolled if they have a Form 1098-T or are observed receiving federal grant aid. Column 4 presents the resulting estimates, which again reveal a similar pattern of results.

Table A14 examines the sensitivity of the standard errors to alternative levels of clustering for the step function and linear specifications. Column 1 presents standard errors clustered at the winner level, which is used throughout the paper. Columns 2 through 4 cluster at the state-by-year, state, and win amount levels, respectively. For the linear specification, the standard errors do not change across these alternatives, with a value of 0.0012 in each case. In the step function specification, the level of clustering has only modest effects on the standard errors for each win size range. Further, only in the case of wins of $100,000-$300,000 does statistical significance vary across the options, as clustering at the winner family, state-by-year, and win amount levels produces nearly identical standard errors but state-level clustering generates larger standard errors and an insignificant estimate for that win range.

b. Alternative Designs

The difference-in-differences design implemented in the paper exploits variation across lottery win sizes and across the timing of the win relative to children's expected years of high school graduation. This section presents estimates based on alternative designs that exploit only the size of the win or only the timing of the win. We also present estimates for a design in which we instrument for net changes in household income using lottery wins, scaling the attendance responses as a way to account for endogenous changes in household earnings.

Table A15 presents estimates based on an across win size design, relying only on children that could potentially be treated by an income shock. Thus, attention is restricted to children who graduate from high school after the lottery win, resulting in a sample that is approximately half of the size of the full sample. As discussed in Section III, a comparison across lottery win sizes requires the assumption that there are no unobservable differences that affect college attendance across larger and smaller winners. The specification progressively adds controls for state-by-year fixed effects, cohort effects, and a rich set of parent, child, and household characteristics. The most comprehensive specification produces estimates of the effect of income on four-year attendance that are insignificant for modest wins and 2, 5, and 8 p.p. for the three largest win size bins. We cannot reject the null that these estimates are the same as those from our main estimation strategy.

The distribution of the timing of wins and households' characteristics before and after a child graduates from high school are examined in Appendix B. A design that exploits only differences in the timing of a win requires the assumption that there are no unobservable differences that affect college attendance across households that win before or after a child graduates from high school. The timing only design presented in Table A16 reports the estimated effect on enrollment for each win size bin, including for the lowest win bin, which was the omitted group in our main specification. As above, the specification progressively adds controls for state-by-year fixed effects, cohort effects, and a rich set of parent, child, and household characteristics. The specifications produce estimates of the effect of income on four-year attendance that are insignificant for modest wins (including for the new smallest win bin). The resulting estimates for the three largest win size bins are 1, 5, and 11 p.p.

As discussed in Section V, some households reduce labor supply on the extensive margin in response to a lottery win and, on average, household earnings decrease. Due to such responses, the net effect of winning the lottery on household income will not exactly match the win amount. To examine how accounting for these endogenous adjustments affects the estimates, we instrument for net income using the lottery win. In column 1 of Table A17, we instrument for total income in the 4 years after the lottery win. In column 2, we instrument for all household income earned prior to a student's high school graduation year. These approaches generate estimated effects of net income on four-year attendance of 0.62 p.p. per $100,000, which is very closely aligned with the baseline linear estimate.

c. Fixed Control Group

As detailed in Section IV, lottery wins generate changes in enrollment that are apparent in each of the four years after high school graduation. Households that win the lottery after high school graduation but prior to the year of interest are omitted from the control group, as they may be partially treated by the lottery win. As a result, the control group changes across each column in Table 7. This slightly complicates the comparison of effects across each post-high school year, as they are based on comparisons of the treatment group to different control groups.

Table A18 presents an alternative approach in which only households that win the lottery four or more years after high school graduation are included in the control group regardless of the year of interest. The advantage of this approach is that it generates an apples-to-apples comparison across each column, as the control group is fixed. The trade-off is that this restriction discards a significant fraction of the control group (four of the six control years). Also, to the extent that households that win the lottery when their children are similar ages are most likely to be similar in terms of unobservables, this approach discards the best available control households for the years immediately after high school. Specifically, the design compares the outcomes of children whose parents won the lottery when they were less than 18 years old to children whose parents won when they were 22 or 23. Still, the magnitude and pattern of results in Table A18 closely mirrors Table 7, with nearly all estimates statistically significant in each of the four years after high school graduation for larger wins and no evidence of positive effects for smaller wins. Further, applying the linear specification to each post-high school year using the constant sample generates effects of 0.55, 0.61, 0.69, 0.62, and 0.50 p.p. in Years 0 to 4, respectively, supporting the evidence of persistent effects (results not shown).

Appendix D: Imputations for Heterogeneity and Financial Aid Analyses

In the literature, heterogeneity analysis has primarily been based on income, which is frequently observable in data and is likely to be correlated with other types of financial resources and access to credit. As summarized in Section IV, we test for heterogeneity using several classes of measures: 1) financial characteristics observed directly in the tax data; 2) imputed wealth based on Saez and Zucman (2016); 3) estimated eligibility for federal financial aid using the FAFSA; 4) imputed measures of credit access and liquidity from the Survey of Consumer Finances (SCF); and 5) measures of the economic conditions in each household's neighborhood or cohort. This appendix details about how we estimate and impute measures of financial constraints that are not observed directly in the data, as well as how we impute changes in state and institutional financial aid in response to lottery wins. We also discuss the methods used to estimate elasticities with respect to household income, heterogeneity as function of household size, and to differentiate effects across college levels using a multinomial logit model.

a. Imputing Wealth and Pell Eligibility

Household wealth is not directly observable in the tax data. However, a measure of wealth is useful for examining heterogeneity in household resources, for estimating each household's expected family contribution (EFC) and Pell eligibility using the FAFSA, and for determining the extent to which lottery wins are spent down versus saved. We impute wealth (as well as levels of particular asset classes required for calculations below) using tax return data and the multipliers derived in Saez and Zucman (2016). The year-specific multipliers reported in that study are applied to the following variables observed in the tax data: interest income, dividend income, rental income, sole proprietorship income, pass-through business income, retirement and pension distributions, and property taxes. While this approach is sensitive to various factors, such as variation across households in their returns on investments, and has been subject to critiques in Bricker et al. (2016) and Auten and Splinter (2018), it provides an objective approximation of household wealth in each year that is suitable for the needs of our analysis.

With imputed values of assets in hand, we can estimate each household's EFC and Pell Grant eligibility using the FAFSA. Specifically, the EFC takes into consideration earned income, savings and investments, household composition, parent age, and state of residence. We run each household's characteristics from the year prior to the win through the FAFSA formula, generating an estimate of EFC and Pell eligibility used in the heterogeneity analysis presented in Section IV. Parental income on the FAFSA is the sum of adjusted gross income, income from Worksheet A (including the Earned Income Tax Credit, Additional Child Tax Credit, and untaxed Social Security income), and from Worksheet B (including payments to 401ks and IRAs and untaxed interest and IRA distributions). Total income is adjusted downward by income taxes paid, state-specific percentage allowances, a Social Security percentage allowance, an income protection allowance based on household size and number of students, and an employment expense allowance. FAFSA assets include the values generated for taxed and untaxed interest and dividends, rent, and some business income, but do not include retirement savings or the value of a household's primary residence. Asset protection levels are based on the marital status of the parents and the age of the older parent.

b. Imputing Credit Constraints and Liquidity

A challenge in the literature has been identifying households that are relatively more and less credit constrained, as few data sources include measures of interactions with credit markets (including denials). However, the SCF includes information about whether a household has recently been denied credit or discouraged from applying for credit out of fear of denial, as well as other useful measures of a household's broad financial position that can be used to differentiate households by the extent to which they are constrained (Japelli, 1990; Crook, 1996; Japelli, Pischke, and Souleles, 1998; Bostic, Gabriel, and Painter, 2009). Specifically, the SCF allows us to examine heterogeneity by timely debt repayment history, payment-to-income ratios, debt-to-asset ratios, liquid assets, liquid assets and home equity (a broader measure of liquidity), and net worth.

To impute these measures for each household in our sample, we first identify the subset of SCF households that most closely resemble the ones we analyze in the tax data. Namely, we restrict the 2001, 2004, 2007, 2010, and 2013 survey samples to tax-filing households with children under 24 years old in which the respondent is between 30 and 70 years old. From this subsample, we identify variables common to both the tax data and the SCF (i.e., the presence and amount of wage earnings, business and self-employment income, investment income, and Social Security income; indicators for joint filing, itemizing deductions, filing particular schedules, and homeownership; age; and family size). Next, within the SCF, we regress each measure noted in the prior paragraph on this set of variables and a survey year fixed effect. The resulting values will capture the extent to which the household characteristics common to both datasets are predictive of specific measures of financial constraints. For example, homeownership and having investment income are negatively correlated with credit denial, while having a larger family is positively correlated with it. We then apply the resulting coefficients to the tax data, generating an imputed value of each measure for each household in our sample. We differentiate the attendance responses for children from households that are above and below the median for each measure in Table 8 and Table A22 and in the top and bottom quartile in Table A23. These results are discussed in Section IV.

c. Federal, State, and Institutional Aid

The modest effects for lower-SES households underscore the need to examine the role of financial aid offset. Table A32 presents estimates of the (endogenous) change in FAFSA applications, EFC, Pell Grants, subsidized and unsubsidized loans, and higher education tax benefits, and we see reductions in grants and loans but not education tax benefits. The lack of an effect on education tax benefits is likely due to how eligibility is determined. Namely, income eligibility is based off of the year in which the student attends school, so the lottery money won before the high school graduation year will not directly crowd out any benefits (at most it can only indirectly through returns on investing the lottery money), and the benefits also phase out at much higher incomes than Pell (e.g., for the largest tax benefit, the American Opportunity Tax Credit, eligibility is exhausted at incomes of $90,000 if single and $180,000 if married filing jointly).

As presented in Table 10, reductions in aid are largest for wins that occur in the year prior to high school graduation, which is the income year used for financial aid determination. Children of lottery winners in this year may remain eligible for financial aid if the lottery win is modest or if they are no longer a dependent of the winner (e.g., due to divorce). Wins that occur in prior years have less of an effect on eligibility, as assets are less heavily taxed than income by the financial aid formula. Further factors that could moderate the effects for prior year wins include: the household spending the lottery winnings or investing them in exempt assets (e.g., a primary residence or a retirement account), the asset protection allowance, parents reducing earnings in response to the lottery win, or households not accurately report their assets on the FAFSA.

As discussed in Section IV, the administrative financial aid records maintained by the Department of Education only include aid from federal sources, and thus would not reveal changes in state and institutional aid and may understate the total reduction in financial aid. Thus, we impute these changes using the 2011-2012 National Postsecondary Student Aid Study. Specifically, among all undergraduate financial aid applicants under 24 years old, we regress state and institutional (need-based) grant aid on EFC, controlling for academic level effects and school effects, separately for above and below-median income students. Then, we apply these estimates to a simulated EFC variable (derived from pre-win tax return information and the win amount) to impute corresponding changes in need-based state and institutional aid in response to a lottery win for households in our data.

Table A33 presents the estimated change in state and institutional grant aid for lottery wins that occur in the year before college (the FAFSA Year), when the win amount is treated as income, and for wins that occur in prior years, when the win amount only affects aid through changes in wealth. The left-hand-side columns report estimates using all households in our sample and the right-hand-side columns using only below median income households. This set of results displays a similar pattern as is found for Pell Grants in Table 10. For non-FAFSA years, changes in state and institutional grant aid are only meaningful for the largest lottery wins. This is because a smaller lottery win will usually not increase a household's wealth by enough to significantly alter aid eligibility. In contrast, more modest lottery wins in the FAFSA Year meaningfully reduce state and institutional grant aid (and the largest wins reduce aid by between two and three times the amount of the reduction in the non-FAFSA years), suggesting corresponding reductions in total need-based grant aid are larger than the estimates presented in Table 10. For wins that occur in the FAFSA Year, reductions in state and institutional aid are larger among lower-income households.

d. College Type

Table A24 presents the step function specification split by households with above and below median income for four-year and two-year college attendance. These estimates reveal no evidence of changes in two-year college attendance for lower- or higher-SES households. As discussed in Section IV, changes in two-year college attendance involve multiple margins, as some children may be induced to attend a two-year college instead of no college while others may be induced to attend a four-year college instead of a two-year college. A multinomial logit model exploits both of these margins of change when generating estimates (see Lovenheim and Reynolds (2013) for an application of a multinomial logit to college choice). Thus, we supplement our analysis by estimating the model for two- and four-year colleges and differentiating the effects for households with above and below median income. The results are shown in Table A25. They reveal no statistically significant change in two-year college attendance on average or for households with lower or higher income. We also find evidence of statistically significant increases in four-year college enrollment for the highest three win ranges, with the effects largely driven by households with above median income. Overall, the estimates from this model are consistent with those from when we consider each margin of college in isolation.

Neither the linear probability model nor the multinomial logit model reveals whether competing margins explain the lack of an effect on two-year enrollment. To explore this further, we estimate whether there are changes in the composition of students who attend two-year colleges as a result of lottery wins. That is, using characteristics measured prior to lottery wins, we estimate whether there is a reduction in the household income of children attending two-year colleges, which would suggest that lower-income children are shifting from no college to two-year colleges or higher income children are shifting from two-year to four-year colleges. The results of this exercise (not shown) are statistically insignificant and do not reveal sizable changes.

e. Elasticity with Respect to Household Income

Table 6 presents estimates of the elasticity of attendance with respect to household income, and Table A28 differentiates these elasticities for households with above and below median income. The elasticities are estimated by scaling lottery win amounts by median income and scaling the enrollment responses by the baseline rate of attendance. For the heterogeneity analysis, the scaling of lottery wins and attendance is done separately for lower- and higher-income households. The results are presented for two measures of the percent change in household income. In the first, the lottery win is measured as a percent change in annual income. In the second, the lottery win is measured as a percent change relative to total household income earned when the child is aged 0 to 18. The latter captures the fact that lottery wins are one-time increases in income and thus may be most naturally compared to pre-college total income. The estimates reveal statistically significant elasticities for households with above median income. A lottery win representing a 1 percent change in total pre-college income increases attendance by 0.35 percent, while a win equal to 1 percent of average annual income increases enrollment by 0.02 percent. In contrast, there is no evidence of statistically significant elasticities for below median income households.

f. Household Composition

Table A29 presents estimates differentiated by the number of children in a household. If a household receives an income shock that is only sufficient to cover tuition (or ease binding financial constraints) for one child, but they have additional children who are college ready, then the enrollment response could be attenuated. Likewise, parents may allocate the lottery win equitably to all of their children, regardless of college enrollment choices or age, reducing the size of the treatment for each child in proportion to the number of children. Alternatively, if only one child in a household is likely to change their college enrollment decision due to the lottery win, and paying for college is a high priority for the parents, then the potential crowding out of effects may be modest.

We explore the implications of this issue in three ways. First, we differentiate the effects by the number of children in the household (regardless of age), revealing somewhat smaller effects for households with more total children (Table A29). Second, if wins are shared among children, then the household, and not the child, may be the relevant treated unit and the baseline approach in the paper will give too much weight to households that have more children in the sample window (i.e., households with children whose expected high school graduation year is within 6 years of the lottery win). To examine if this has meaningful implications for the estimates, we replicate the primary design while giving each household equal weight and report the results in column 5 of Table A29. This also produces an average enrollment effect of approximately 0.6 p.p. per $100,000. Third, we assume that households divide all of the winnings across their children and use per-child win amount as the explanatory variable. The per-child win amount estimates in column 6 are mechanically larger than the baseline estimates, reflecting the fact that the explanatory variable has been, on average, scaled down.

Appendix E: Exploration of Lottery Wins as a Special Type of Resource

An important consideration with respect to the external validity of our estimates is whether responses to lottery wins can be generalized to other types of resource shocks. While in standard economic models, a household's response to a resource shock does not depend on its source, in practice, one may be concerned that households differentially allocate prize money in a manner that could lead our estimates to misstate the effect of resources more generally on college attendance. In particular, if households over-consume lottery winnings, resulting in few, if any, resources being available for potentially high return college investment, we may be understating the effects of other types of resources.[2] This section builds on the lottery literature and provides additional evidence supporting that our estimates generalize to other types of resources.

Several lottery-based studies have examined whether wins generate short-lived or persistent changes in labor supply and savings. Imbens, Rubin, and Sacerdote (2001) find that winning the Massachusetts Megabucks Lottery generates reductions in labor earnings that persist over time. They also find a marginal propensity to save that is consistent with a life-cycle model based on reasonable assumptions of the discount factor, interest rate, and life expectancy. Similarly, Cesarini et al. (2017) find evidence of persistent reductions in earnings and increases in wealth for lottery winners in Sweden.

Turning to our setting, first note that the pattern of college attendance effects in our analysis does not appear to be consistent with the rapid spending of lottery wins. Consuming winnings quickly would likely manifest itself in the form of larger effects for wins that occur close to high school graduation and smaller effects for wins that occur several years earlier. In practice, the attendance effects are similar when we shift the focus to wins that occur several years prior to high school graduation (Table A10 and Figure 3). Likewise, rapid spending of the lottery win could result in children initially attending college and then dropping out when financial resources are depleted, which is not consistent with the persistence in effects over the four years after graduation (Table 7).

Second, we use the tax data to infer savings by imputing wealth using the tax capitalization method of Saez and Zucman (2016).[3] We then estimate changes in imputed wealth in the years following a win

[2] Of course, the amount that households consume is endogenous to whether there are high return college investment opportunities.

[3] This approach will understate wealth due to an inability to account for wealth holdings that do not generate taxable income, such as contributions to retirement accounts, the purchase of annuities, and investments in durable goods. Additionally, if winning the lottery induces households to make investments in new businesses or properties with long time horizons, the initial returns might understate their value. Finally, the measure we use here will not reflect changes in debt, including the act of paying off a mortgage or acquiring a new one.

relative to the year before the win, using our linear specification. The results, which are presented in the first column of Table A38, reveal that $100,000 of post-tax lottery winnings increase imputed wealth by about $70,000 in the year after the lottery win. While the estimated change in wealth decreases over time, it does so slowly, with nearly $50,000 remaining after 5 years. Similar to the evidence from Sweden, these findings are not consistent with a high fraction of U.S. households rapidly depleting lottery wins; indeed, the amount of the win that households in our sample appear to save is somewhat larger than in Sweden (Cesarini et al., 2017).

Third, a useful benchmark for considering how resources are treated is the canonical lifecycle or permanent income framework, which is based on the idea that households make consumption decisions in response to their lifetime income (Friedman, 1957; Ando and Modigliani, 1963). Under this framework, households smooth their consumption over time through savings and credit, and all sources of income produce the same effect on household decisions. Estimated consumption responses to lottery wins that are consistent with the model's predictions–namely, that wins generate a persistent increase in consumption–would provide evidence that lottery winnings are treated similarly to other resources.[4] While the tax data are not well suited to measuring consumption, we can examine earnings responses (and presumed change in the consumption of leisure) and housing (which yields consumption value, though not exclusively). Figure 4 reveals that household earnings decrease in the year of a lottery win, and remain at this lower level. Were households to quickly consume their lottery wins, they might reverse these earnings reductions out of necessity, but this is not observed empirically. Table A38 reports the effects on earnings and new mortgages in each of the 5 years following a lottery win. The observed reduction in wages and increase in homeownership exhibit a great deal of persistence. For example, the estimated reduction in earnings is $1,138 in the year after the win and is $1,120 after 5 years. The rate of new homeownership increases by 1.4 p.p. per $100,000 in the year after the win and remains 1.6 p.p. higher 5 years later.

Finally, we examine whether households appear to exhibit other unusual patterns of spending with respect to lottery prize money that could result in estimates that understate effects of other types of resources on college attendance. At the extreme, rapid consumption could lead lottery winners to overspend and end up in financial distress. To examine this narrative within our data, we can estimate the effect of lottery wins on debt cancellation (as reported to the IRS by creditors on Form 1099-C), which occurs in bankruptcy or

[4] A failure to validate consumption smoothing would not necessarily imply that lottery money is treated differently than other resources. That is, if this test fails, it would not necessarily be informative.

other periods of financial distress when a creditor relieves a debtor from a debt obligation. In contrast to this story, there is a small and persistent *reduction* in debt cancellation of about 0.8 percentage point in each year after the win (Table A38), which is not consistent with lottery winners rapidly spending their windfall gains, finding themselves in financial trouble, and falling behind on payments or filing for bankruptcy.[5]

Alternatively, lottery-winning households may be reluctant to use their prize money to undertake investments that have uncertain returns (e.g., college, businesses). In Table A39, we examine whether lottery wins induce households in our sample to make investments in a range of risky assets. We find that lottery wins increase investment in every category that we can examine in the tax data, including partnerships (1065 Schedule K-1), s-corporations (1120S Schedule K-1), c-corporations (1099-DIV), rental properties (1040 Schedule E), and retirement accounts (5498 and W-2s). The evidence is not consistent with households being averse to investing their winnings in enterprises with relatively uncertain returns.

In sum, we find evidence supporting the generalizability of our lottery-win-based estimates to other types of resources.

[5]Hankins, Hoekstra, and Skiba (2011) find that lottery wins of $50,000 to $150,000 postpone bankruptcy for only a couple years, with no difference in bankruptcy rates through five years after the win.

Appendix F: Heterogeneity in the Household Propensity to Consume

This section presents a more detailed discussion of the household propensity to consume estimates in Table 12. While the percentage reduction in earnings is larger for lower-income households, the magnitude is smaller. However, there is little evidence of greater savings by these households. In conjunction, these estimates and the more modest college attendance effects suggest that lower-income households are spending a larger fraction of their lottery wins on non-college consumption.

Yet, there are several potential concerns with attempting to identify differences in the marginal propensity to consume across lower- and higher-SES households. First, within the class of investment income we use to proxy for savings, lower-SES households may earn a lower average return, which would understate the magnitude of their savings. However, when we parse investment income into interest income and dividend income, the results imply larger effects for higher-SES households for each investment type, although the effects are not all statistically significant. Second, our measure does not include alternative investments that could be more prevalent among lower-SES winners. While investments in real estate, sole proprietorships, or pass-throughs and contributions to some tax advantage retirement accounts are not subject to uniform third party reporting, we examine income responses from the Form 1040 and W-2, and find no evidence of larger increases in Schedule C income, Schedule F income, IRA contributions, or 401-k contributions.

Finally, while we cannot observe overall debt or the paying off of old debt in the data, Table A46 indicates that lower-SES households are less likely to have mortgage, credit card, and installment (e.g., car loans) debt and have less debt in each of these categories, on average, than higher-SES households. However, the SCF indicates that the lower-SES households that do have debt face modestly higher interest rates, and thus have a higher relative return to paying off debt. An alternative approach to understanding if debt can explain the results is to approximate the amount of debt that would need to be paid off to offset the observed changes in savings and earnings. Back of the envelope calculations reveal that the amount of debt needed to offset these changes is implausibly high, especially compared to the amounts indicated in Table A46. Overall, the available evidence indicates that lower-SES households consume a higher fraction of lottery winnings.

Appendix G: Measures of College Cost, Academic Readiness, and Consumption

This section provides additional details about the data used to document college costs and debt, academic readiness, and the consumption value of college in Section VI.

a. College Costs and Debt

A primary result in the paper is that reasonably large lottery wins have modest effects on college attendance. Comparing win sizes with the range of college costs can shed light on the magnitude of shocks needed to substantially reduce or eliminate the constraints associated with enrollment. Likewise, if children or families are debt averse, lottery wins can be compared to cumulative debt levels. Table A40 presents summary statistics, primarily from the Department of Education's National Center for Education Statistics Integrated Postsecondary Education Data System (NCES IPEDS) as reported in the Digest of Education Statistics. The remainder of the statistics come from The College Board's annual Trends in Student Aid publication. The data, including the distributions of tuition and fees (with and without room and board), are presented for the 2000-2001 and 2011-2012 academic years to provide a clear picture of costs, borrowing, and grant aid during the period of interest. The data are differentiated by four types of colleges: four-year non-profit, four-year public, two-year public, and for-profit.

Annual list tuition and fees (i.e., a college's official published price) vary considerably between public and private institutions and between two- and four-year colleges. For example, median tuition and fees in 2011-2012 were $7,175 for four-year public colleges and $28,310 for four-year private non-profit colleges. Factoring in room and board substantially increases price, raising these medians to $16,860 and $39,596, respectively. In practice, many students do not pay these list prices, as made evident by the large differences in average list tuition, fees, room, and board and average net price (which removes grant and scholarship aid from this figure for those who receive it) in the bottom two rows of the top panel. Still, list prices may be relevant, as list prices may be more salient to some households than the true price they face. Further, more specific to our setting, households that win large lotteries may not be eligible for grants.

The bottom half of the table presents more detailed statistics (that are only published for subsets of undergraduates), particularly with respect to financial aid participation. Interestingly, a substantial share of first-time students receive institutional aid from four-year private non-profit colleges. This may partially explain the large gap between average published and net prices at private non-profit colleges (relative to public colleges). Among program graduates in 2011-2012, 64 percent of those attending a public college and 74 percent of those attending a non-profit four-year college borrowed to finance their education, with average cumulative debt levels, conditional on borrowing, of \$25,640 and \$32,310, respectively (and each about \$10,000 more than corresponding debt levels a decade earlier).

Table A41 uses data from the 2011-2012 National Postsecondary Student Aid Study (NPSAS) to examine heterogeneity in net cost and parental support by household income. These data indicate average net costs of \$9,209 for two-year colleges and \$19,349 for four-year colleges, which expectedly fall between the published net price figures for each sector by level in Table A40. A large majority of college students–about 82 percent of two-year students and 93 percent of four-year students–are financial dependents of their parents, and a smaller majority–65 percent and 81 percent, respectively–receive parental support for college. Restricting attention to students for whom parental income is available (either through the corresponding NPSAS survey or through the financial aid applications of financially dependent students), which are almost all financial dependents, reveals that parental income and financial support are positively correlated. This may be due to several factors, including: children from lower-income households receive higher amounts of grant aid (which is evident from the growing discrepancy between total and net costs as income falls); children from lower-income households, both conditional on level and unconditionally (the latter of which is not explicitly shown in the exhibit), tend to attend lower sticker price colleges; lower-income parents may have less financial capacity to provide support; or differences in beliefs by income about the role of parents in paying for college.

b. College Readiness

The college readiness of high school students is documented using microdata from the Education Longitudinal Study of 2002. This panel dataset reports the academic performance, college plans, and realized college outcomes of a nationally representative sample of 10th graders. The study includes several variables that are likely to reflect a student's eligibility for, and interest in, college attendance. As discussed in Section VI, college readiness measures allow us to scale the estimates by the fraction of the population that could feasibly be treated–ignoring that this fraction would still include the always-takers–and provide evidence of the extent to which the effects for lower-income households may be attenuated by academic preparation and college eligibility. The measures of college readiness include the fraction of students who: graduate from high school on time, perform above the bottom quartile on the PISA exam, and have a high school grade point average of 2.0 or higher. In addition to these measures of academic readiness for college, we report student and parent interest in college and enrollment in college preparatory programs.

Table A44 reveals that 85 percent of students in the 2002 study graduated from high school on time, with the rate ranging from 74 percent for the lowest income households (with income of less than $5,000) to 96 percent for the highest (with income exceeding $200,000). However, adjusting our estimates by high school completion may understate differences in college readiness, as other measures exhibit higher levels of heterogeneity by income. For example, among students from households with income of less than $25,000, 59 percent score above the bottom quartile on the PISA, compared to 93 percent among students from households earning $100,000 or more. This should reduce the expected response to lottery wins for lower-income households to the extent that poor academic readiness reduces the return to college or limits admissions chances. When considering multiple measures of readiness in conjunction, the gap is even larger. For example, 45 percent of children from households with income below $25,000 graduate on time, perform above the bottom quartile on the PISA, and have a GPA of 2.0 or better. Conversely, over 87 percent of children from households with income exceeding $100,000 dollars meet this level of college readiness. The fraction of 10th graders who state that they plan to attend college after graduation also exhibits a strong correlation with income, as do parents' preferences for their child's future college attendance; that said, the discrepancy in parents' preferences across the income distribution is narrower. For example, among households with income of less than $25,000, 58 percent of students plan to attend college, while 82 percent of parents express an interest in their child attending. At the highest household income level, 84 percent of students plan to attend, and 97 percent of parents express an interest.

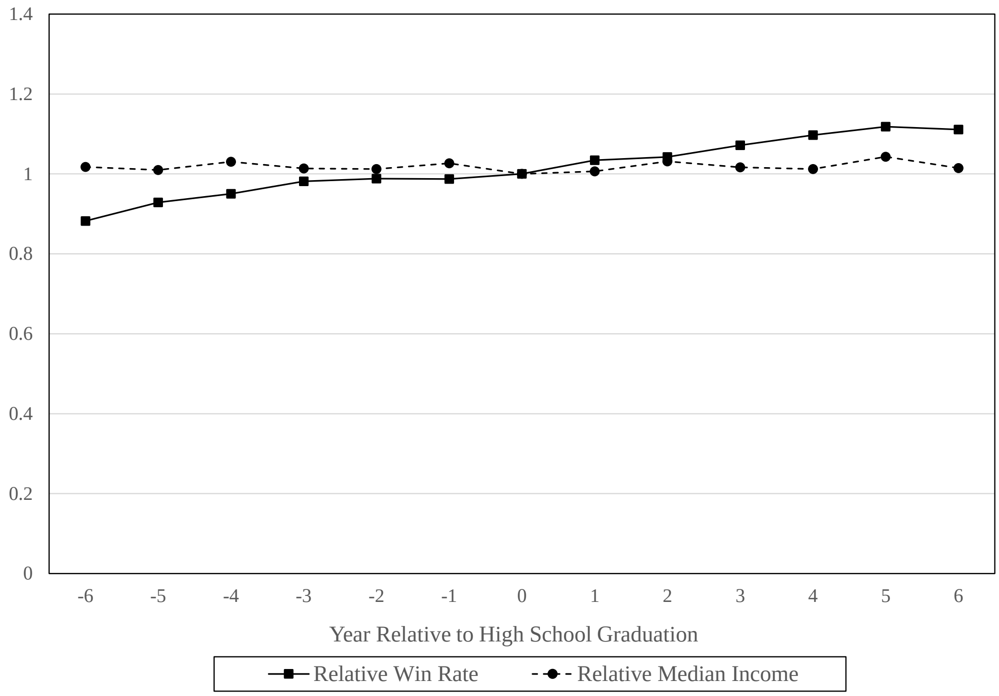
To scale the estimates, we divide the binary attendance decision of each child by the fraction of children meeting the measure of readiness for each household income range by sex before running regressions using the transformed dependent variable. This procedure is replicated for each measure of college readiness, and the results are reported in Table A45. The estimated effects of lottery wins increase as a result. For example, in column 2, children are considered feasibly treated as measured by meeting three measures of academic readiness, causing the estimated effect to increase to 0.74 p.p. per $100,000. Column 5 scales by the fraction of children who would not attend in the absence of a lottery win (based on older, unaffected children) and thus could feasibly be induced to attend (because the always-takers–i.e., children who would attend college whether or not their parents won the lottery–are effectively subtracted out), and produces an estimate of 0.86 p.p. Column 6 presents the log odds ratio of attendance based on a logistic regression. The bottom panel of Table A45 examines whether there is evidence of heterogeneous effects by income after scaling. This analysis reveals that the gap is still statistically significant after scaling by each measure of readiness.

c. College Consumption Value

The consumption value of college may vary with the type of college attended. Jacob, McCall, and Stange (2018) use student services expenditures as a measure of the extent to which colleges provide direct consumption value to students in the form of non-academic amenities. We differentiate increases in college attendance due to lottery wins across colleges with higher and lower levels of student services expenditures and higher and lower ratios of services to total spending. To achieve this, we merge college financial data from the NCES IPEDS. Student amenities spending includes "student services" and "auxiliary enterprises". Academic expenditures include "total instructional" and "academic support". Attention is restricted to four-year colleges, and colleges with missing data are not included in the analysis. The results are presented in Table A42 and reveal that increases in attendance are largely within colleges with higher levels of spending on student services and above median ratios of student services to total spending. For example, attendance at colleges in the top quartile of student services spending (measured in levels or as a ratio of total spending) exhibit a highly statistically significant increase in response to lottery wins, while attendance at colleges in the bottom quartile are small and insignificant (and we can reject the null that they are the same). The estimates also reveal that these effects are driven by children from households with above median income. Specifically, there was no statistically significant increase in attendance for any classification of college for children from lower-income households.

Households in communities with higher rates of college-going may value college more than other households. This would be the case if, for example, households that value education select into certain communities. Thus, the local college-going rate (conditional on income) reflects unobserved preferences for education. Alternatively, households may value college more if they are surrounded by other households that value education (i.e., the community could affect their preferences). Table A43 tests for evidence of this by considering heterogeneity by the pre-win zip code college attendance rate, with and without accounting for heterogeneity by zip code average income. The analysis reveals positive, statistically significant coefficients on the interaction between treatment and local college attendance rates in both specifications, providing suggestive evidence consistent with a consumption-based interpretation of the response.

Figure A1: Lottery Win Rates and Winner Median Income
Relative to Population by Year



Note: This figure presents the rate at which households win the lottery and their median income relative to the population in the years before and after their children graduate from high school, indexed to equal 1 in Year 0. Population is defined by year and includes all households in the tax records that have children in the same expected academic year relative to high school graduation.

Figure A2a: Estimated Effects on Four-Year Attendance for Lottery Wins > $100,000 by Timing of Win Relative to High School Graduation

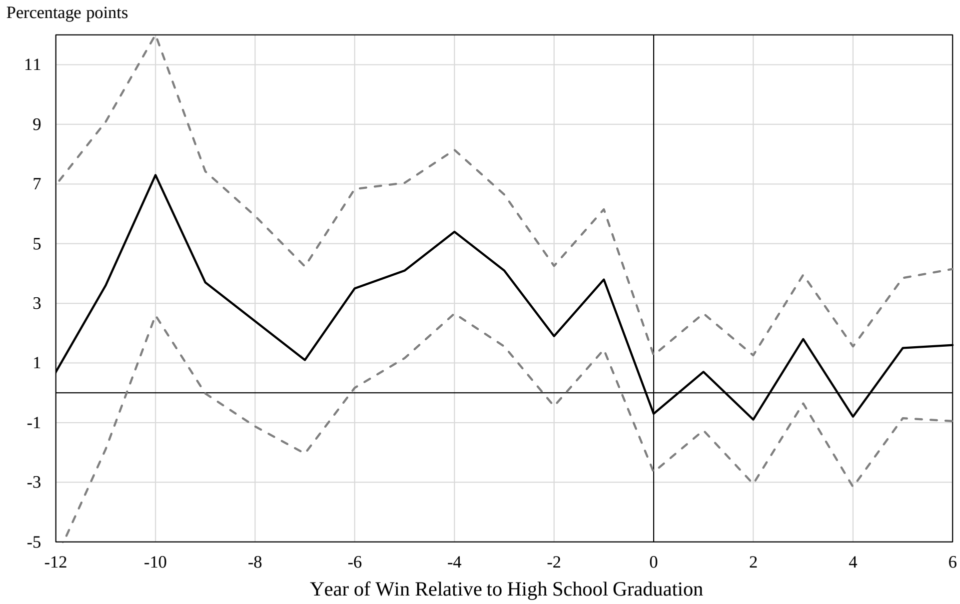
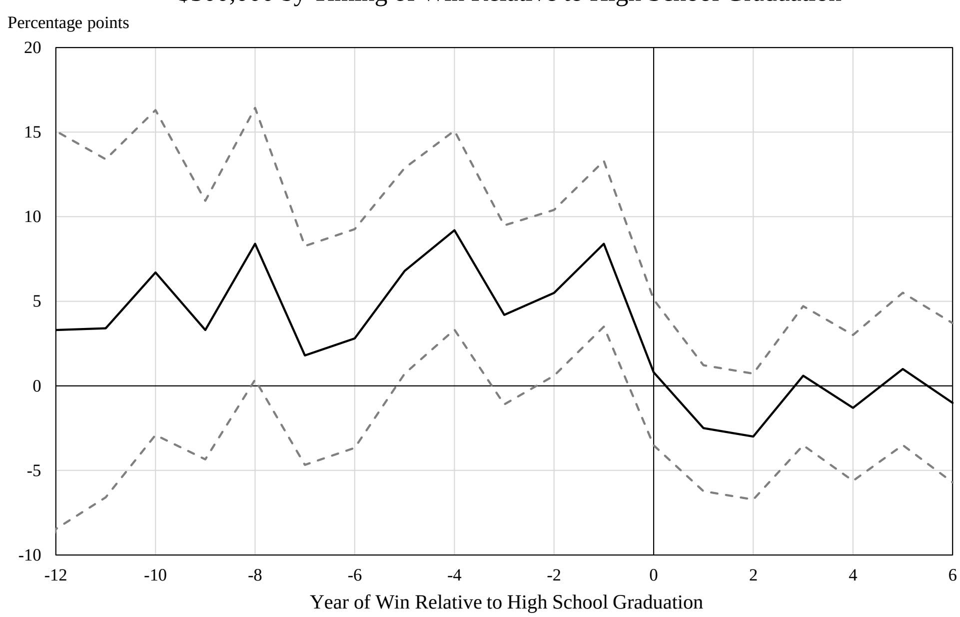


Figure A2b: Estimated Effects on Four-Year Attendance for Lottery Wins > $300,000 by Timing of Win Relative to High School Graduation



Note: This figure presents the estimated percentage point difference in four-year college attendance for children whose parents won a large lottery relative to those whose parents won a small lottery in each year before and after the expected year of high school graduation. The estimates account for state-by-year of win fixed effects, cohort fixed effects, parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, household number of children, and child gender, citizenship, and an indicator for Social Security birth match to parent, with the omitted variables being a large win and one of the year effects. All student and parent controls are based on pre-win measures. In the top figure, large wins are defined as those exceeding $100,000, while small lotteries are those of less than $100,000 (which average $3,294). In the bottom figure, large wins are those exceeding $300,000 and wins between $100,000 and $300,000 are excluded. Dashed lines depict the 95 percent confidence interval.

Table A1: Data Merging Process

Lottery winners and amounts
Lottery winners and amounts identified using state reported Form W-2G
Lottery winner characteristics
Lottery winners linked to Social Security master file
Lottery winners linked to pre-win Form 1040s
Lottery winners linked to pre-win Form 1098s and 1098-Ts
Children claimed by lottery winners
Lottery winners linked to claimed dependents using prior Form 1040s
Lottery winners linked to children using Social Security birth records
Child characteristics
Children linked to Social Security birth records
Children linked to state school entry age laws
College outcomes
Children linked to college and university reported Form 1098-Ts
Children linked to federal financial aid records
Supplemental data sources
NCES IPEDS - college characteristics linked to Form 1098-Ts
CEX - characteristics of lottery players
SCF - measures of financial constraints
NPSAS - levels of state and institutional aid
ELS - measures of college readiness by income
Zillow - housing values by zip code
USDA - county poverty rates and food stamp receipt
ACS - county insurance coverage rates

Note: This table presents the data sources and merging process used to construct the population of children affected by lottery wins. Forms W-2G, 1098, and 1098-T are reported to the IRS by third parties. The Form 1040 is filed by tax payers. The data reveal 5,372,900 lottery winners, of whom 5,364,579 (99.8%) are linked to the Social Security master file and 5,015,743 (93.4%) filed a tax return in the three years prior to the win. These winners claimed 1,617,679 children for whom the win occurred within 6 years of their expected year of high school graduation, representing 3.3% of all children in potentially affected birth cohorts. Federal financial aid records are maintained by the Department of Education. NCES IPEDS is the National Center for Education Statistics Integrated Postsecondary Education Data System, CEX is the Consumer Expenditure Survey, SCF is the Survey of Consumer Finances NPSAS is the National Postsecondary Student Aid Study, ELS is the Education Longitudinal Study, USDA is the United States Department of Agriculture, and ACS is the American Community Survey.

Table A2: Characteristics of Lottery Players and Non-Players: Consumer Expenditure Survey

	Lottery Players	Non-Players
Age	51.89	51.60
Family Size	2.50	2.52
Hours Worked Per Week	40.82	40.35
Marital Status		
Married	0.58	0.54
Widowed	0.08	0.12
Divorced	0.15	0.15
Seperated	0.02	0.03
Never married	0.16	0.17
Highest Education		
HS or Less	0.40	0.38
JC or Vocational	0.28	0.33
Bachelors	0.20	0.19
Masters / Professional / PhD	0.12	0.10
Race		
White, Non-Hispanic	0.76	0.72
Black	0.10	0.12
Hispanic	0.10	0.11
Asian	0.03	0.04
Other, Non-Hispanic	0.02	0.01
Family Income		
Less than $30,000	0.23	0.34
$30,000-$49,000	0.21	0.20
$50,000-$69,999	0.17	0.15
$70,000 and over	0.39	0.32
Type of Employment		
Private Business	0.74	0.72
Federal Government	0.04	0.03
State Government	0.06	0.07
Local Government	0.08	0.07
Self-Employed	0.09	0.11
Age of Oldest Child		
No Children	0.61	0.59
Less than 6	0.05	0.07
6-11	0.07	0.08
12-17	0.11	0.11
Greater than 17	0.17	0.15
Expenditures		
Total (monthly)	5,075	4,387
Housing (monthly)	1,577	1,481
Education (monthly)	91	84
Lottery (annual)	266	0.00
Investments		
Own Home	0.76	0.72
Savings	20,657	16,239
Stocks	30,822	30,565
Sample Size	11,308	34,958

Note: This table presents summary statistics for those who play and do not play the lottery. The analysis is based on the Bureau of Labor Statistics Consumer Expenditure Survey for Quarter 1 of 2005 to Quarter 4 of 2014. Households are identified as playing the lottery if they report any spending on lottery tickets in the prior year.

Table A3: Comparison of Lottery Winning Households and Households with Same-Aged Children

	Population	Lottery Winners
Parent and Child Characteristics		
Parent Married	0.60	0.57
Parent Median Wage	45, 291	44,699
Parent Mean Wage	59,184	51,790
Parent AGI	74,905	60,466
Number of Children	3.281	3.454
Child Male	0.51	0.51
Child Citizen	0.97	0.96
Child College Attendance		
Year HS Grad: Any	0.39	0.35
Year HS Grad: Four-Year	0.25	0.22
Year HS Grad: Two-Year	0.16	0.14
Child College Attendance: Adjusted for Characteristics		
Year HS Grad: Any	0.39	0.38
Year HS Grad: Four-Year	0.25	0.24
Year HS Grad: Two-Year	0.16	0.15

Note: This table presents summary statistics for parents and children who experience income shocks due to lottery wins and for a random sample of the population of parents with children of the same age. The population sample characteristics are shown for parents with children born between 1980 and 1994 to correspond to those in the lottery sample. Marital status and income are derived from the Form 1040, the number of children is derived from those claimed as a dependent in prior years, and child gender and citizenship are based on Social Security records. College attendance is based on the Form 1098-T. Attendance is reported for the calendar year of expected high school graduation, which is determined using each child's state and date of birth. Child college attendance adjusted for characteristics accounts for differences between the lottery winners and the population sample in terms of the child and household characteristics listed in the table and restricts attention to older children who are unaffected by the lottery win.

Table A4: Covariate Balance: Income Distribution and Sibling College Outcomes

Covariate		Mean	Win size (dollars)					
			10-30k	30-100k	100-300k	300k-1mil	1mil or more	F-test p-value
					Earnings distribution			
Percentile 0-10	(1)	0.103	-0.0047*	-0.0013	0.0004	0.0088	-0.0003	0.5785
			(0.0026)	(0.0047)	(0.0056)	(0.0136)	(0.0122)	
Percentile 10-25	(2)	0.156	0.0046	-0.0009	-0.0140**	-0.0116	0.0168	0.1510
			(0.0032)	(0.0057)	(0.0068)	(0.0141)	(0.0185)	
Percentile 25-50	(3)	0.257	-0.0007	-0.0098	-0.0009	-0.0094	-0.0512**	0.1908
			(0.0039)	(0.0072)	(0.0094)	(0.0206)	(0.0221)	
Percentile 50-75	(4)	0.249	0.0021	0.0117	0.0213**	0.0037	0.0081	0.1938
			(0.0040)	(0.0073)	(0.0099)	(0.0214)	(0.0269)	
Percentile 75-90	(5)	0.143	-0.0009	0.0004	-0.0095	-0.0035	0.0080	0.9150
			(0.0033)	(0.0058)	(0.0084)	(0.0184)	(0.0257)	
Percentile 90-100	(6)	0.091	-0.0005	0.0000	0.0027	0.0120	0.0185	0.9359
			(0.0027)	(0.0046)	(0.0073)	(0.0150)	(0.0269)	
F-test p-value			0.2873	0.7620	0.1749	0.9217	0.4085	0.5634
					Sibling college outcomes			
			10-30k	30-100k	100-300k	300k-1mil	1mil or more	F-test p-value
Any Sibling	(7)	0.273	-0.0060	-0.0119*	-0.0045	0.0247	-0.0084	0.2541
			(0.0041)	(0.0071)	(0.0097)	(0.0211)	(0.0270)	
Number Siblings	(8)	0.334	-0.0078	-0.0132	-0.0058	0.0349	-0.0099	0.3676
			(0.006)	(0.0096)	(0.0127)	(0.0300)	(0.0370)	
Any College	(9)	0.316	-0.0009	-0.0034	-0.0322*	0.0420	-0.0327	0.5193
			(0.0078)	(0.0141)	(0.0193)	(0.0421)	(0.0551)	
Four-Year College	(10)	0.201	0.0001	0.0203*	-0.0203	0.0392	-0.0385	0.2922
			(0.0067)	(0.0121)	(0.0168)	(0.0386)	(0.0431)	
Two-Year College	(11)	0.139	-0.0009	-0.0105	0.0035	0.0569*	-0.0398	0.4594
			(0.0057)	(0.0110)	(0.0146)	(0.0337)	(0.0444)	
Pell Grant	(12)	0.160	0.0036	-0.0010	-0.0113	0.0304	-0.0382	0.6343
			(0.0060)	(0.0113)	(0.0148)	(0.0301)	(0.0319)	
Sub Loan	(13)	0.142	-0.0086	-0.0113	-0.0217	0.0172	-0.0218	0.2875
			(0.0057)	(0.0102)	(0.0149)	(0.0297)	(0.0325)	
F-test p-value			0.4562	0.0732	0.3675	0.4549	0.8139	0.3246

Note: This table applies the empirical design to segments of the household earnings distribution and to older siblings college outcomes. Each row of the top panel represents a regression where the outcome variable is an indicator for household income falling into the specified percentile range. Household earnings are based on the three years prior to the lottery win and are derived from the Form 1040. Each row of the bottom panel represents the outcome for older siblings in the sample whose expected high school graduation years were prior to, and thus unaffected by, the lottery win. The college attendance of siblings is based on the 1098-T, and Pell Grant receipt and amount are based on federal aid records. The specification includes state-by-year of win and student cohort fixed effects. F-tests of joint significance for each covariate are presented at the bottom of every column and across win sizes at the end of every row. An F-test for the joint significance of all covariates across all win sizes is presented at the bottom of the last column for each panel. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A5: Lottery Wins and Covariate Balance: Fixed Age

Covariate		Mean	Win size (dollars)					
			10-30k	30-100k	100-300k	300k-1mil	1mil or more	F-test p-value
			Parent and household characteristics					
Married	(1)	0.576	-0.0011	0.0098	0.0108	0.0350	0.0030	0.5564
			(0.0050)	(0.0091)	(0.0120)	(0.0252)	(0.0320)	
Missing 1040	(2)	0.234	0.0029*	0.0034	0.0019	0.0125	0.0233***	0.0207
			(0.0017)	(0.0030)	(0.0038)	(0.0095)	(0.0085)	
Ln(Wages)	(3)	54,302	-0.0013	-0.0038	0.0123	0.0257	0.0358	0.9733
			(0.0100)	(0.0177)	(0.0245)	(0.0508)	(0.0674)	
Ln(AGI)	(4)	59,491	-0.0128	-0.0010	-0.0071	0.0437	0.0388	0.8813
			(0.0127)	(0.0221)	(0.0326)	(0.0641)	(0.0833)	
Self Employed	(5)	0.133	0.0011	0.0042	0.0118	-0.0027	0.0237	0.6805
			(0.0036)	(0.0065)	(0.0089)	(0.0189)	(0.0250)	
SSA Income	(6)	0.020	-0.0002	0.0005	-0.0006	0.0101	-0.0031	0.8654
			(0.0014)	(0.0024)	(0.0034)	(0.0079)	(0.0078)	
Parent College	(7)	0.058	0.0005	-0.0032	-0.0070	-0.0209	-0.0222	0.4950
			(0.0030)	(0.0048)	(0.0080)	(0.0159)	(0.0184)	
Mortgage	(8)	0.467	-0.0068	-0.0093	0.0059	0.0250	-0.0598	0.4446
			(0.0065)	(0.0109)	(0.0154)	(0.0305)	(0.0410)	
Invest Income	(9)	0.394	-0.0015	-0.0097	-0.0093	0.0311	0.0347	0.4987
			(0.0050)	(0.0087)	(0.0124)	(0.0256)	(0.0348)	
F-test p-value			0.8250	0.5195	0.4941	0.3945	0.2680	0.7076

Note: This table applies the empirical design to household financial characteristics measured 7 years prior to each child's expected high school graduation. Each row represents a separate variable. The specification includes state-by-year of win and student cohort fixed effects. Whether an individual is married is derived from filing status, and income and investments are derived from the Form 1040. Parent college attendance is based on the Form 1098-T. F-tests of joint significance for each covariate are presented at the bottom of every column and across win sizes at the end of every row. An F-test for the joint significance of all covariates across all win sizes is presented at the bottom of the last column. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A6: Falsification Test: Prior to Actual Win Year

	(1)	(2)	(3)	(4)	(5)	(6)
	Any College		Four-Year College		Two-Year College	
Win 10-30k Pre-HS Grad	-0.0035	-0.0058	-0.0039	-0.0053	0.0012	0.0002
	(0.0052)	(0.0049)	(0.0045)	(0.0043)	(0.0039)	(0.0038)
Win 30-100k Pre-HS Grad	0.0020	-0.0005	0.0037	0.0021	-0.0008	-0.0019
	(0.0090)	(0.0085)	(0.0077)	(0.0074)	(0.0068)	(0.0067)
Win 100-300k Pre-HS Grad	0.0102	0.0036	0.0065	0.0020	0.0060	0.0036
	(0.0129)	(0.0122)	(0.0114)	(0.0110)	(0.0093)	(0.0093)
Win 300k-1.0m Pre-HS Grad	-0.0094	-0.0182	0.0042	-0.0017	-0.0004	-0.0038
	(0.0256)	(0.0242)	(0.0224)	(0.0215)	(0.0200)	(0.0199)
Win 1.0m or more Pre-HS Grad	-0.0208	-0.0196	-0.0368	-0.0363	0.0278	0.0286
	(0.0340)	(0.0327)	(0.0288)	(0.0276)	(0.0266)	(0.0265)
Child, Parent, and Family Controls		X		X		X
State-by-Year and Cohort	X	X	X	X	X	X
R-Squared	0.015	0.143	0.019	0.105	0.023	0.052
Mean Dep	0.341	0.341	0.215	0.215	0.139	0.139
Observations	817,551	817,551	817,551	817,551	817,551	817,551

Note: This table presents the results of a falsification test that imposes a false lottery win date three years prior to the actual win. Children who graduate after the false lottery win date are labeled as treated, while those who graduate before the false date act as the control. Children who graduate after the actual lottery win date are excluded. Estimates show the percentage point effect of the false income shocks on attending any college, a four-year college, or a two-year college in the year of high school graduation. Columns 1, 3, and 5 include state-by-year of win and cohort fixed effects. Columns 2, 4, and 6 add parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, student gender and citizenship, the number of children in the household, and an indicator for Social Security birth match to parent. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A7: Across Win Size Comparison: Four-Year Attendance Rates of Unaffected Children

	(1) Without Covariates	(2) With Covariates
Win 10-30k	0.0126***	-0.0028
	(0.0029)	(0.0026)
Win 30-100k	0.0202***	0.0015
	(0.0049)	(0.0045)
Win 100-300k	0.0545***	0.0105
	(0.0069)	(0.0064)
Win 300k-1.0m	0.0620***	0.0107
	(0.0140)	(0.0127)
Win 1.0m or more	0.0568***	-0.0229
	(0.0193)	(0.0183)
F-test p-value	0.000	0.273
Mean Dep	0.215	0.215
Observations	817,551	817,551

Note: This table tests for differences in the rate of four-year college attendance across lottery win sizes for older, unaffected children. Estimates reveal the rate of attending a four-year college in the year of high school graduation relative to children whose households won less than $10,000. Attention is restricted to children who graduated from high school prior to the lottery win. The specification in column 1 includes state-by-year of win fixed effects and cohort fixed effects. Column 2 adds parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, the number of children in the household, and child gender, citizenship, and an indicator for Social Security birth match to parent. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A8: Four-Year Attendance: Comparison of Linear Effects Across Win Sizes

	(1)
Win Amount ($100k) * Pre-HS Grad	0.0055**
	(0.0027)
Win 10-30k Pre-HS Grad	-0.0023
	(0.0036)
Win 30-100k Pre-HS Grad	-0.0102
	(0.0064)
Win 100-300k Pre-HS Grad	0.0097
	(0.0101)
Win 300k-1.0m Pre-HS Grad	0.0232
	(0.0241)
Win 1.0m or more Pre-HS Grad	0.0060
	(0.0595)
Mean Dep	0.219
Observations	1,460,890

Note: This table examines whether the effects of lottery wins on four-year college attendance differ significantly across win sizes. The specification includes both the amount of the lottery win and indicators for each win size range. Estimates are presented for attending a four-year college in the year of high school graduation. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Attention is restricted to lottery wins of 5 million dollars or less. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A9: Four-Year Attendance: Alternative Specifications of Key Variables

	(1)	(2)	(3)	(4)	(5)
	Pre-Tax Income	Post-Tax Income	Enrollment Within Two Years	Elasticity (Annual)	Elasticity (Lifetime)
Win Amount ($100k) * Pre-HS Grad	0.0060***	0.0089***	0.0073***	0.0122***	0.2195***
	(0.0012)	(0.0018)	(0.0013)	(0.0025)	(0.0446)
Win Amount ($100k)	-0.0012*	-0.0020*	-0.0016**	-0.0025*	-0.0452*
	(0.0007)	(0.0010)	(0.0007)	(0.0014)	(0.0250)
Pre-HS Grad	-0.0022	-0.0023*	-0.0004	-0.0104	-0.0104
	(0.0014)	(0.0014)	(0.0015)	(0.0064)	(0.0064)
Mean Dep	0.219	0.219	0.270	0.219	0.219
Observations	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890

Note: This table presents estimates of the effect of income shocks on four-year college attendance when accounting for taxes and expanding the definition of enrollment to include the year after the (projected) high school graduation year, as well as the elasticity of attendance with respect to household income. Estimates in columns 1 and 2 are the percentage point effect of income shocks on college attendance before and after taxes are deducted from lottery winnings, respectively. Taxes are estimated based on household income in the year prior to the lottery win. Column 3 presents estimates based on attendance in the year of expected high school graduation or in the subsequent year. The linear specifications in columns 1, 2, and 3 interact the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Columns 4 and 5 present the elasticity of attendance with respect to average annual household income and total household income earned when the child is aged 0 to 18, respectively. Attention in each column is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A10: Four-Year Attendance: Alternative Bandwidths Before and After Graduation

	(1) 6 Years	(2) 5 Years	(3) 4 Years	(4) 3 Years	(5) 2 Years	(6) 1 Year
Win 10-30k Pre-HS Grad	-0.0016	-0.0010	-0.0004	-0.0012	-0.0010	0.0024
	(0.0035)	(0.0037)	(0.0040)	(0.0044)	(0.0052)	(0.0073)
Win 30-100k Pre-HS Grad	-0.0075	-0.0090	-0.0090	-0.0158**	-0.0155*	-0.0197
	(0.0062)	(0.0065)	(0.0070)	(0.0077)	(0.0092)	(0.0128)
Win 100-300k Pre-HS Grad	0.0188**	0.0183*	0.0180*	0.0102	0.0088	0.0097
	(0.0091)	(0.0095)	(0.0102)	(0.0113)	(0.0134)	(0.0184)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0465**	0.0529**	0.0583**	0.0595**	0.0475
	(0.0195)	(0.0206)	(0.0220)	(0.0247)	(0.0292)	(0.0405)
Win 1.0m or more Pre-HS Grad	0.1184***	0.1397***	0.1435***	0.1142***	0.1502***	0.1894***
	(0.0257)	(0.0273)	(0.0294)	(0.0313)	(0.0377)	(0.0558)
Mean Dep	0.219	0.219	0.218	0.218	0.218	0.217
Observations	1,461,262	1,289,589	1,087,709	857,883	598,923	312,407

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of high school graduation. Each column includes a different bandwidth of years around the lottery win, with column 1 including students who graduate within 6 years of the lottery win, column 2 including students who graduate within 5 years of the lottery win, etc. Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A11: Four-Year Attendance: Alternate Samples and Robustness Checks

	(1) Baseline	(2) Soc. Sec. Parent	(3) No Sample Restrictions	(4) Control 1,000-10,000	(5) Control 5,000-10,000	(6) Control 600-1,000
Win 10-30k Pre-HS Grad	-0.0016	-0.0051	0.0014	-0.0019	-0.0029	-0.0017
	(0.0035)	(0.0047)	(0.0032)	(0.0036)	(0.0042)	(0.0040)
Win 30-100k Pre-HS Grad	-0.0075	-0.0041	-0.0040	-0.0074	-0.0050	-0.0087
	(0.0062)	(0.0082)	(0.0058)	(0.0062)	(0.0066)	(0.0064)
Win 100-300k Pre-HS Grad	0.0188**	0.0265**	0.0225***	0.0176*	0.0154*	0.0188**
	(0.0091)	(0.0120)	(0.0085)	(0.0091)	(0.0094)	(0.0092)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0711***	0.0670***	0.0547***	0.0565***	0.0507***
	(0.0195)	(0.0260)	(0.0174)	(0.0195)	(0.0196)	(0.0195)
Win 1.0m or more Pre-HS Grad	0.1184***	0.1036***	0.0983***	0.1180***	0.1200***	0.1195***
	(0.0257)	(0.0329)	(0.0230)	(0.0258)	(0.0258)	(0.0257)
Mean Dep	0.219	0.258	0.220	0.216	0.221	0.228
Observations	1,461,262	914,841	1,691,357	1,138,097	222,840	416,035

	(7) Include Grad Yr	(8) Shift Cohort	(9) Household FE Yr 0	(10) Household FE Yr 0-1	(11) Population Weighted	(12) Bin Weighted
Win 10-30k Pre-HS Grad	-0.0024	-0.0025	-0.0098	-0.0084	-0.0011	-0.0023
	(0.0033)	(0.0036)	(0.0095)	(0.0100)	(0.0037)	(0.0038)
Win 30-100k Pre-HS Grad	-0.0076	-0.0048	-0.0240	-0.0228	-0.0069	-0.0087
	(0.0058)	(0.0063)	(0.0178)	(0.0184)	(0.0066)	(0.0067)
Win 100-300k Pre-HS Grad	0.0097	0.0200**	0.0064	0.0134	0.0194**	0.0181**
	(0.0084)	(0.0092)	(0.0243)	(0.0258)	(0.0098)	(0.0091)
Win 300k-1.0m Pre-HS Grad	0.0415**	0.0572***	0.0514	0.0465	0.0533***	0.0553***
	(0.0179)	(0.0196)	(0.0552)	(0.0542)	(0.0210)	(0.0195)
Win 1.0m or more Pre-HS Grad	0.1067***	0.1403***	0.0893	0.1426**	0.1186***	0.1066***
	(0.0240)	(0.0261)	(0.0695)	(0.0707)	(0.0257)	(0.0250)
Mean Dep	0.220	0.222	0.198	0.246	0.220	0.220
Observations	1,617,679	1,449,621	377,252	377,252	1,461,262	1,461,262

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment for alternate samples and specification choices. The columns in the top panel present: (1) the baseline sample and specification; (2) a sample that includes only individuals who are linked to the parent winner through Social Security birth records; (3) a sample that eliminates all sample restrictions (e.g. including individuals who appear to have won the lottery prior to the date on the W2G as revealed by a matching income amount in the prior year); (4) to (6) which use alternative control groups in the following ranges: $1,000 to $10,000, $5,000 to $10,000, and $600 to $1,000. The columns in the bottom present: (7) inclusion of wins that occur in a student's graduation year; (8) shifting the school entry age cutoffs for every state to be two months earlier; (9) lottery winner fixed effects; (10) lottery winner fixed effects for attendance in the year of high school graduation or the subsequent year (11) weighting the sample to represent the population; and (12) weighting each win size bin to have the same average household characteristics. Note that Social Security birth match records are first available for the 1983 cohort, not 1980, so the resulting sample is smaller. Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A12: Four-Year Attendance: Narrower Win Ranges For Income Shocks

	(1)	(2)
Win 1-3k Pre-HS Grad	0.0024	-0.0010
	(0.0019)	(0.0023)
Win 3-10k Pre-HS Grad	-0.0045*	-0.0021
	(0.0024)	(0.0023)
Win 10-20k Pre-HS Grad	-0.0007	-0.0021
	(0.0044)	(0.0042)
Win 20-30k Pre-HS Grad	-0.0049	-0.0084
	(0.0065)	(0.0062)
Win 30-50k Pre-HS Grad	-0.0017	0.0046
	(0.0105)	(0.0100)
Win 50-100k Pre-HS Grad	-0.0039	-0.0110
	(0.0122)	(0.0115)
Win 100-300k Pre-HS Grad	0.0249**	0.0168
	(0.0125)	(0.0118)
Win 300k-1.0m Pre-HS Grad	0.0525***	0.0526***
	(0.0203)	(0.0191)
Win 1.0m-3.0m Pre-HS Grad	0.0912***	0.0761**
	(0.0344)	(0.0325)
Win 3.0m or more Pre-HS Grad	0.1558***	0.1552***
	(0.0387)	(0.0375)
Child, Parent, and Family Controls		X
State-by-Year and Cohort	X	X
Mean Dep	0.219	0.219
Observations	1,461,262	1,461,262

Note: Estimates show the percentage point effect of income shocks on college enrollment in the year of high school graduation for narrower win ranges. Students for whom the win occurs prior to high school graduation are potentially affected. The specification in column 1 includes state-by-year of win fixed effects and cohort fixed effects. Column 2 adds parent wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, self-employment income, household number of children, and child gender, citizenship, and an indicator for Social Security birth match to parent. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: $1,000, $3,000, $10,000, $20,000, $30,000, $50,000, $100,000, $300,000, $1,000,000, $3,000,000 and exceeding $3,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A13: Any College Attendance: Alternative Measures

	(1) Baseline	(2) Set to Zero	(3) Set to Missing	(4) Form 1098-T or Fed Aid
Win 10-30k Pre-HS Grad	-0.0022	-0.0019	-0.0006	-0.0030
	(0.0041)	(0.0038)	(0.0042)	(0.0041)
Win 30-100k Pre-HS Grad	-0.0101	-0.0064	-0.0083	-0.0121*
	(0.0073)	(0.0068)	(0.0075)	(0.0073)
Win 100-300k Pre-HS Grad	0.0169*	0.0173*	0.0148	0.0155
	(0.0101)	(0.0095)	(0.0105)	(0.0102)
Win 300k-1.0m Pre-HS Grad	0.0559***	0.0470**	0.0561**	0.0464**
	(0.0217)	(0.0209)	(0.0229)	(0.0217)
Win 1.0m or more Pre-HS Grad	0.1039***	0.0919***	0.1027***	0.0941***
	(0.0279)	(0.0275)	(0.0303)	(0.0279)
Mean Dep	0.348	0.276	0.307	0.362
Observations	1,461,262	1,461,262	1,308,674	1,461,262

Note: This table presents estimates based on alternate methods of measuring college attendance, as some colleges may not file a Form 1040 for students receiving full grant aid. Column 1 presents estimates for all colleges. Column 2 sets enrollment to 0 for students attending colleges identified as being most likely not to file. Column 3 omits all students attending these colleges. Column 4 presents estimates from the union of 1098-T and federal aid enrollment reports. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A14: Four-Year Attendance: Level of Clustering

Step Function Specification	(1) Winner Family	(2) State by Year	(3) State	(4) Win Amount
Win 10-30k Pre-HS Grad	-0.0016	-0.0016	-0.0016	-0.0016
	(0.0035)	(0.0037)	(0.0039)	(0.0042)
Win 30-100k Pre-HS Grad	-0.0075	-0.0075	-0.0075	-0.0075
	(0.0062)	(0.0066)	(0.0062)	(0.0060)
Win 100-300k Pre-HS Grad	0.0188**	0.0188*	0.0188	0.0188**
	(0.0091)	(0.0097)	(0.0132)	(0.0092)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0539**	0.0539***	0.0539***
	(0.0195)	(0.0209)	(0.0177)	(0.0195)
Win 1.0m or more Pre-HS Grad	0.1184***	0.1184***	0.1184***	0.1184***
	(0.0257)	(0.0256)	(0.0272)	(0.0246)
Mean Dep	0.219	0.219	0.219	0.219
Observations	1,461,262	1,461,262	1,461,262	1,461,262
Linear Specification	(5) Winner Family	(6) State by Year	(7) State	(8) Win Amount
Win Amount ($100k) * Pre-HS Grad	0.0058***	0.0058***	0.0058***	0.0058***
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Win Amount ($100k)	-0.0007	-0.0007	-0.0007	-0.0007
	(0.0007)	(0.0008)	(0.0010)	(0.0010)
Pre-HS Grad	-0.0023*	-0.0023	-0.0023	-0.0023*
	(0.0014)	(0.0017)	(0.0014)	(0.0013)
Mean Dep	0.219	0.219	0.219	0.219
Observations	1,460,890	1,460,890	1,460,890	1,460,890

Note: This table shows the effect of different levels of clustering on the standard errors and statistical significance for estimates of the effect of income shocks on four-year college enrollment. Column 1 presents standard errors clustered at the lottery winner level, the level presented throughout the paper. Columns 2 through 4 present standard errors clustered at the state-by-year, state, and win amount levels. The top panel presents the step function specification with win sizes classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. The bottom panel presents estimates for a linear specification that interacts the pre-tax win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Attention is restricted to lottery wins of 5 million dollars or less for the linear specification. All specifications include state-by-year of win fixed effects and cohort fixed effects. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A15: Four-Year Attendance: Across Win Size Design

	(1)	(2)	(3)
Win 10-30k	0.0090***	-0.0019	-0.0030
	(0.0027)	(0.0026)	(0.0026)
Win 30-100k	0.0094*	-0.0025	-0.0038
	(0.0049)	(0.0047)	(0.0046)
Win 100-300k	0.0607***	0.0299***	0.0242***
	(0.0072)	(0.0068)	(0.0067)
Win 300k-1.0m	0.0851***	0.0518***	0.0496***
	(0.0164)	(0.0157)	(0.0154)
Win 1.0m or more	0.1433***	0.0904***	0.0830***
	(0.0212)	(0.0200)	(0.0200)
Child and Family Controls			X
Parental Controls		X	X
State-by-Year and Cohort	X	X	X
R-Squared	0.018	0.082	0.106
Mean Dep	0.223	0.223	0.223
Observations	643,711	643,711	643,711

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of high school graduation. The sample is restricted to children for whom the win occurred prior to high school graduation and thus may be affected. Column 1 includes state-by-year of win and cohort fixed effects. Column 2 adds parental controls, including wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, and self-employment income. Columns 3 adds student and family controls, including gender, citizenship, number of children, and an indicator for Social Security birth match to parent. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A16: Four-Year Attendance: Pre-Post High School Design

	(1)	(2)	(3)
Win 0.6-10k Pre-HS Grad	0.0004	-0.0021	-0.0019
	(0.0014)	(0.0014)	(0.0014)
Win 10-30k Pre-HS Grad	-0.0012	-0.0055	-0.0045
	(0.0037)	(0.0035)	(0.0035)
Win 30-100k Pre-HS Grad	-0.0071	-0.0113*	-0.0095
	(0.0063)	(0.0060)	(0.0060)
Win 100-300k Pre-HS Grad	0.0192**	0.0127	0.0125
	(0.0091)	(0.0087)	(0.0086)
Win 300k-1.0m Pre-HS Grad	0.0543***	0.0482**	0.0546***
	(0.0195)	(0.0188)	(0.0185)
Win 1.0m or more Pre-HS Grad	0.1188***	0.1112***	0.1079***
	(0.0257)	(0.0247)	(0.0246)
Child and Family Controls			X
Parental Controls		X	X
State-by-Year and Cohort	X	X	X
R-Squared	0.018	0.086	0.104
Mean Dep	0.219	0.219	0.219
Observations	1,461,262	1,461,262	1.461,262

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of high school graduation. The design exploits only the timing of the win relative to a students' year of expected high school graduation. Students for whom the win occurs prior to high school graduation are potentially affected. Column 1 includes state-by-year of win and cohort fixed effects. Column 2 adds parental controls, including wages, adjusted gross income, filing status (joint or single), gender, citizenship, missing returns, mortgage payments, Social Security income, and self-employment income. Columns 3 adds student and family controls, including gender, citizenship, number of children, and an indicator for Social Security birth match to parent. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A17: Four-Year Attendance: Instrumenting for Net Income Change

Second Stage	(1)	(2)
Four Years of Post-Win Income ($100k)	0.00621***	
	(0.00127)	
All Post-Win Income Prior to HS Grad ($100k)		0.00618***
		(0.00126)
Mean Dep	0.219	0.219
Observations	1,460,890	1,460,890
First Stage	Four Years Post-Win Income	Post-Win Pre-HS Grad Income
Win Amount ($100k)	0.9591***	0.9632***
	(0.0048)	(0.0113)
Observations	1,460,890	1,460,890

Note: Estimates show the percentage point effect of net income changes on attending a four-year college. The first row instruments for the net change in income (in hundreds of thousands of dollars) in the four years after a lottery win. The second row instruments for the net change in income in every year after the lottery win and prior to the student graduating from high school. The bottom panel presents the first stage effect of lottery wins on these measures of net income. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A18: Four-Year College Attendance in Later Years and Cumulatively: Constant Sample

	(1) Year 0	(2) Year 1	(3) Year 2	(4) Year 3	(5) Year 4	(6) Cumulative
Win 10-30k Pre-HS Grad	-0.0078*	-0.0096*	-0.0082*	-0.0048	-0.0027	-0.0331
	(0.0047)	(0.0049)	(0.0048)	(0.0048)	(0.0047)	(0.0208)
Win 30-100k Pre-HS Grad	-0.0053	0.0031	0.0038	0.0045	0.0011	0.0072
	(0.0081)	(0.0084)	(0.0085)	(0.0085)	(0.0083)	(0.0365)
Win 100-300k Pre-HS Grad	0.0221*	0.0254**	0.0324***	0.0195	0.0255**	0.1249**
	(0.0119)	(0.0123)	(0.0122)	(0.0121)	(0.0119)	(0.0527)
Win 300k-1.0m Pre-HS Grad	0.0628**	0.0457*	0.0567**	0.0535**	0.0397	0.2583**
	(0.0251)	(0.0255)	(0.0253)	(0.0254)	(0.0250)	(0.1091)
Win 1.0m or more Pre-HS Grad	0.1078***	0.1067***	0.1475***	0.1494***	0.0956***	0.6070***
	(0.0337)	(0.0354)	(0.0362)	(0.0354)	(0.0353)	(0.1545)
Mean Dep	0.219	0.245	0.241	0.238	0.218	1.160
Observations	840,030	840,030	840,030	840,030	840,030	840,030

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the years after high school graduation and the cumulative number of years of enrollment during this period. Year 0 refers the calendar year in which a student is expected to graduate from high school based on his or her state and date of birth. Years 1 to 4 correspond to the subsequent calendar years. Students for whom the win occurs prior to high school graduation are potentially affected. Students whose parents win the lottery in the year of high school graduation and the subsequent three years could endogenously change their enrollment decisions and thus are excluded from the control group. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A19: Matriculation in Subsequent Years: Wins After High School Graduation

	(1) Any College	(2) Four-Year	(3) Two-Year
Win 10-30k	-0.0019	-0.0013	-0.0025
	(0.0033)	(0.0029)	(0.0026)
Win 30-100k	-0.0027	-0.0028	-0.0013
	(0.0057)	(0.0049)	(0.0045)
Win 100-300k	0.0065	0.0072	-0.0031
	(0.0080)	(0.0071)	(0.0062)
Win 300k-1.0m	0.0256	0.0136	0.0122
	(0.0159)	(0.0140)	(0.0127)
Win 1.0m or more	0.0485**	0.0229	0.0299
	(0.0236)	(0.0206)	(0.0199)
Mean Dep	0.348	0.226	0.148
Observations	2,932,463	2,932,463	2,932,463

Note: Estimates show the percentage point effect of income shocks on attending any college, a four-year college, or a two-year college for wins occurring in the four years after the expected year of high school graduation. Attendance is compared in the year before and after the lottery win. Students for whom the win occurs prior to high school graduation are omitted. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A20: Parental Attendance

	(1) Any College	(2) Four-Year	(3) Two-Year	(4) Grad School
Win 10-30k	-0.0006	0.0000	-0.0007	0.0003
	(0.0012)	(0.0009)	(0.0010)	(0.0005)
Win 30-100k	0.0009	0.0013	-0.0006	0.0002
	(0.0020)	(0.0015)	(0.0015)	(0.0008)
Win 100-300k	-0.0000	0.0005	-0.0011	0.0013
	(0.0029)	(0.0020)	(0.0024)	(0.0012)
Win 300k-1.0m	-0.0039	0.0014	-0.0053	-0.0027
	(0.0061)	(0.0041)	(0.0047)	(0.0036)
Win 1.0m or more	-0.0097	-0.0019	-0.0077	0.0022
	(0.0091)	(0.0064)	(0.0064)	(0.0025)
Mean Dep	0.028	0.015	0.014	0.004
Observations	2,922,524	2,921,780	2,921,780	2,921,780

Note: Estimates show the percentage point effect of income shocks on attending any college, a four-year college, a two-year college, or graduate school for the lottery winning parents. Attendance is compared in the year before and after the lottery win. The specifications include state-by-year of win fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A21: Four-Year Attendance by College Type

	(1) Private	(2) Public	(3) For-Profit	(4) High Earn	(5) Avg Earn
Win 10-30k Pre-HS Grad	0.0031	-0.0058**	0.0011*	-0.002	-155.14
	(0.0022)	(0.0030)	(0.0007)	(0.003)	(211.77)
Win 30-100k Pre-HS Grad	-0.0060*	0.0004	-0.0018	0.002	-438.39
	(0.0036)	(0.0053)	(0.0012)	(0.005)	(374.05)
Win 100-300k Pre-HS Grad	0.0029	0.0170**	-0.0011	0.006	683.31
	(0.0056)	(0.0078)	(0.0016)	(0.008)	(526.37)
Win 300k-1.0m Pre-HS Grad	0.0108	0.0333**	0.0098**	0.025	2,798.71**
	(0.0119)	(0.0167)	(0.0044)	(0.017)	(1,119.81)
Win 1.0m or more Pre-HS Grad	0.0487***	0.0657***	0.0039	0.079***	5,055.47***
	(0.0176)	(0.0223)	(0.0057)	(0.024)	(1,527.29)
Mean Dep	0.072	0.137	0.006	0.162	17,893.18
Observations	1,461,262	1,461,262	1,461,262	1,461,262	1,461,262

Note: Estimates show the effect of income shocks on four-year college enrollment by sector (columns 1 to 3), enrolling at a college with high earning attendees (column 4), and the average earnings of attendees of the college attended (column 5). Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A22: Four-Year Attendance: Heterogeneity by Financial Constraints

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household tax records	Income	Investment Income	Retirement Contribution	Mortgage	Imputed Wealth	Pell Eligible	Imputed EFC
Win Amt ($100k) Pre-HS Grad	0.0019	0.0031	0.0037**	0.0039**	0.0004	0.0027	0.0029*
	(0.0017)	(0.0019)	(0.0019)	(0.0020)	(0.0020)	(0.0018)	(0.0017)
Win Amt ($100k) Pre-HS Grad * Less Constrained	0.0065***	0.0049*	0.0044*	0.0035	0.0070***	0.0051**	0.0048**
	(0.0024)	(0.0025)	(0.0026)	(0.0026)	(0.0025)	(0.0024)	(0.0024)
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
SCF imputations	Denied Credit	Late Payment	Payment -to-Income Ratio	Debt-to-Asset Ratio	Liquid Assets	Liquid Assets + Home Equity	Net Worth
Win Amt ($100k) Pre-HS Grad	0.0003	0.0006	0.0053***	0.0035*	0.0026	0.0014	0.0027
	(0.0016)	(0.0016)	(0.0019)	(0.0019)	(0.0017)	(0.0017)	(0.0017)
Win Amt ($100k) Pre-HS Grad * Less Constrained	0.0092***	0.0082***	0.0027	0.0045*	0.0077***	0.0087***	0.0078***
	(0.0023)	(0.0023)	(0.0026)	(0.0026)	(0.0025)	(0.0025)	(0.0025)
	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Economic environment	County Poverty Rate	County Food Stamp Rate	County Insured Rate	County Medicaid Rate	Zip House Values	Zip House Values (w/ House)	Great Recession Years
Win Amt ($100k) Pre-HS Grad	0.0040*	0.0070***	0.0025	0.0062***	0.0084***	0.0045	0.0057**
	(0.0021)	(0.0023)	0.0017	(0.0018)	(0.0021)	(0.0029)	(0.0022)
Win Amt ($100k) Pre-HS Grad * Less Constrained	0.0042	-0.0000	0.0065***	-0.0006	-0.0029	0.0015	0.0005
	(0.0027)	(0.0028)	(0.0024)	(0.0025)	(0.0027)	(0.0036)	(0.0028)
	Mean Dep = 0.219		Observations = 1,460,890				

Note: Estimates show the differential effect of income shocks on attending a four-year college for households that are less financially constrained relative to those that are more constrained. The estimates are based on a linear specification that interacts the pre-tax win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation and a measure of household financial constraints. Estimates in the top panel are differentiated based on pre-win characteristics available in the tax records or estimated by applying the FAFSA formula to tax data. Less constrained households are those with above median income, any investment income, any voluntary retirement contributions, a mortgage, above median wealth, any Pell Grant eligibility, or above median expected household contribution (EFC). The second panel differentiates the estimates by measures of financial constraints imputed using the Survey of Consumer Finances. Less constrained households are those that have below median probability of being denied credit, probability of making late payments, monthly payment-to-debt ratio, debt-to-asset ratio, or have above median liquid assets, liquid assets and home equity, or total net worth. In the bottom panel, less constrained households are those that live in counties with below median poverty, food stamp, and Medicaid rates, above median health insurance coverage rates, that live in zip codes with above median housing values, or who have children who graduate before or after the Great Recession years of 2008 to 2012. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A23: Four-Year Attendance: Heterogeneity by Financial Constraints for Top and Bottom Quartiles

Household tax records	(1) Income	(2) Investment Income	(3) Retirement Contribution	(4) Mortgage	(5) Imputed Wealth	(6) Pell Eligible	(7) Imputed EFC
Win Amt ($100k) Pre-HS Grad	0.0043	*Not*	*Not*	*Not*	0.0080**	*Not*	0.0024
* Least Constrained	(0.0034)	*Applicable*	*Applicable*	*Applicable*	(0.0032)	*Applicable*	(0.0039)
Mean Dep	0.249				0.230		0.250
Observations	730,412				822,224		730,855
SCF imputations	(8) Denied Credit	(9) Late Payment	(10) Payment -to-Income Ratio	(11) Debt-to- Asset Ratio	(12) Liquid Assets	(13) Liquid Assets + Home Equity	(14) Net Worth
Win Amt ($100k) Pre-HS Grad	0.0090**	0.0076**	0.0054	0.0113***	0.0055	0.0084**	0.0045
* Least Constrained	(0.0037)	(0.0036)	(0.0036)	(0.0039)	(0.0035)	(0.0033)	(0.0035)
Mean Dep	0.251	0.253	0.222	0.236	0.268	0.262	0.267
Observations	730,443	730,500	730,489	730,445	730,406	730,391	730,400
Economic environment	(15) County Poverty Rate	(16) County Food Stamp Rate	(17) County Insured Rate	(18) County Medicaid Rate	(19) Zip House Values	(20) Zip House Values (w/ House)	(21) Great Recession Years
Win Amt ($100k) Pre-HS Grad	0.0031	-0.0031	0.0058*	-0.0022	-0.0064	-0.0013	*Not*
* Least Constrained	(0.0040)	(0.0042)	(0.0035)	(0.0036)	(0.0039)	(0.0049)	*Applicable*
Mean Dep	0.227	0.221	0.225	0.224	0.226	0.253	
Observations	756,547	748,901	756,958	751,798	750,019	366,539	

Note: Estimates show the percentage point effect of income shocks on attending a four-year college using a linear specification that interacts the pre-tax win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. The effects are differentiated for households that are more or less likely to be constrained based on their financial characteristics and economic environment. The specification is not applicable for binary measures of constraints which cannot be divided into quartiles. Estimates are differentiated based on pre-win characteristics available in tax records, estimated using the FAFSA, imputed using the Survey of Consumer Finances, and economic conditions in the zip code, county, and year. In the top panel, the least constrained households are those in the top quartile of income, median wealth, and estimated expected household contribution (EFC) on the FAFSA. In the second panel, the least constrained households are those that are in the bottom quartile probability of having been denied credit, probability of having made a late payment, monthly payment-to-debt ratio, debt-to-asset ratio, or in the top quartile of median liquid assets, liquid assets and home equity, and total net worth. In the bottom panel, the least constrained households are those that live in counties in the bottom quartile of poverty, food stamp, and Medicaid rates, and the top quartile of health insurance coverage rates, or who live in zip codes with the top quartile of housing values. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A24: Two-Year and Four-Year Attendance: Heterogeneity by Household Income

	(1)	(2)	(3)	(4)
	Below Median Inc		Above Median Inc	
	Two-Year	Four-Year	Two-Year	Four-Year
Win 10-30k Pre-HS Grad	-0.0017	0.0030	-0.0026	-0.0059
	(0.0038)	(0.0043)	(0.0044)	(0.0053)
Win 30-100k Pre-HS Grad	-0.0027	-0.0088	-0.0078	-0.0115
	(0.0070)	(0.0077)	(0.0080)	(0.0093)
Win 100-300k Pre-HS Grad	-0.0001	-0.0111	-0.0057	0.0319**
	(0.0102)	(0.0116)	(0.0101)	(0.0125)
Win 300k-1.0m Pre-HS Grad	0.0163	0.0282	0.0020	0.0631**
	(0.0225)	(0.0259)	(0.0218)	(0.0264)
Win 1.0m or more Pre-HS Grad	0.0233	0.0370	-0.0150	0.1387***
	(0.0396)	(0.0372)	(0.0275)	(0.0318)
Mean Dep	0.104	0.138	0.184	0.299
Observations	730,632	730,632	730,630	730,630

Note: Estimates show the percentage point effect of income shocks on two-year and four-year college enrollment in the year after high school graduation. The results are presented for students from households with above and below median income ($44,699) measured prior to the lottery win. Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. The distribution of children by win size and above or below median household income are: $600 to 10k - 677,915 above and 687,583 below; $10k to 30k - 33,591 above and 28,648 below; $30k to 100k - 10,491 above and 9,117 below; $100k to 300k - 6,306 above and 4,012 below; $300k to 1m - 1,408 above and 893 below; and $1m or more - 919 above and 379 below. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A25: Two-Year and Four-Year Attendance: Multinomial Logit

	(1)	(2)	(3)	(4)	(5)	(6)
			Below Median Inc		Above Median Inc	
	Two-Year	Four-Year	Two-Year	Four-Year	Two-Year	Four-Year
Win 10-30k Pre-HS Grad	-0.0272	0.0042	-0.0246	0.0353	-0.0340	-0.0142
	(0.0253)	(0.0211)	(0.0416)	(0.0368)	(0.0326)	(0.0269)
Win 30-100k Pre-HS Grad	-0.0271	-0.0408	-0.0350	-0.0760	-0.0439	-0.0523
	(0.0438)	(0.0381)	(0.0731)	(0.0651)	(0.0557)	(0.0485)
Win 100-300k Pre-HS Grad	-0.0159	0.1032**	-0.0339	-0.0905	-0.0118	0.1574***
	(0.0603)	(0.0487)	(0.1060)	(0.0913)	(0.0748)	(0.0599)
Win 300k-1.0m Pre-HS Grad	0.0675	0.3070***	0.0747	0.2221	0.0671	0.3459***
	(0.1206)	(0.1049)	(0.1950)	(0.2073)	(0.1550)	(0.1281)
Win 1.0m or more Pre-HS Grad	0.1324	0.6342***	0.1583	0.3360	0.0944	0.6665***
	(0.1629)	(0.1338)	(0.2877)	(0.2969)	(0.1985)	(0.1542)
Mean Dep	0.144	0.219	0.104	0.138	0.184	0.299
Observations	1,461,262	1,461,262	730,632	730,632	730,630	730,630

Note: Estimates show the effect of income shocks on two-year and four-year college enrollment from a multinomial logit model. The results are presented for students from households with above and below median income ($44,699) measured prior to the lottery win. Students for whom the win occurs prior to high school graduation are potentially affected. The specifications includes win year fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. The distribution of children by win size and above or below median household income are: $600 to 10k - 677,915 above and 687,583 below; $10k to 30k - 33,591 above and 28,648 below; $30k to 100k - 10,491 above and 9,117 below; $100k to 300k - 6,306 above and 4,012 below; $300k to 1m - 1,408 above and 893 below; and $1m or more - 919 above and 379 below. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A26: Four-Year Attendance: Income Heterogeneity and Assets

	(1)	(2)	(3)	(4)
Above Med Inc * Win Amt ($100k) * Pre-HS Grad	0.0065***	0.0052**	0.0052*	0.0052*
	(0.0024)	(0.0026)	0.0028)	(0.0028)
Has Invest Inc * Win Amt ($100k) * Pre-HS Grad		0.0031	0.0031	0.0031
		(0.0027)	(0.0028)	(0.0028)
Home Owner * Win Amt ($100k) * Pre-HS Grad			0.0006	0.0004
			(0.0028)	(0.0028)
Debt Cancellation * Win Amt ($100k) * Pre-HS Grad				0.0000
				(0.0000)
Mean Dep	0.219	0.219	0.219	0.219
Observations	1,460,890	1,460,890	1,460,890	1,460,890

Note: This table examines the robustness of heterogeneity by income to the inclusion of the presence of investment income, homeownership, and debt cancellation. The linear specification interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation and and an indicator for the household having above median income ($44,699). Column 2 through 4 sequentially add interactions between the treatment and indicators for the household having any investment income, having a mortgage (a proxy for homeownership), and having previously experienced debt cancellation. Income, investment, mortgages, and debt cancellation are measured prior to the lottery win. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A27: Four-Year Attendance: Heterogeneity by Household Income Quartiles

	(1) Q1 <$24k	(2) Q2 $24-45k	(3) Q3 $45-78k	(4) Q4 >$78k+
Win 10-30k Pre-HS Grad	-0.0051	0.0102	0.0016	-0.0132*
	(0.0058)	(0.0062)	(0.0067)	(0.0079)
Win 30-100k Pre-HS Grad	-0.0260**	0.0071	-0.0002	-0.0224
	(0.0107)	(0.0109)	(0.0115)	(0.0144)
Win 100-300k Pre-HS Grad	-0.0021	-0.0191	0.0216	0.0443**
	(0.0170)	(0.0158)	(0.0166)	(0.0178)
Win 300k-1.0m Pre-HS Grad	0.0095	0.0443	0.0893**	0.0375
	(0.0345)	(0.0374)	(0.0377)	(0.0363)
Win 1.0m or more Pre-HS Grad	0.0424	0.0487	0.1428***	0.1228***
	(0.0549)	(0.0517)	(0.0468)	(0.0426)
Mean Dep	0.120	0.155	0.220	0.378
Observations	365,318	365,310	365,320	365,316

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year after high school graduation. The results are presented for students by household income quartiles. Average household income is measured in the three years prior to the lottery win. Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A28: Four-Year Attendance: Heterogeneity by Income for Alternative Specifications of Key Variables

	(1)	(2)	(3)	(4)	(5)
Above Median Income	Pre-Tax Income	Post-Tax Income	Enrollment Within Two Years	Elasticity (Annual)	Elasticity (Lifetime)
Win Amount ($100k) * Pre-HS Grad	0.0078***	0.0120***	0.0092***	0.0195***	0.3513***
	(0.0016)	(0.0024)	(0.0016)	(0.0040)	(0.0712)
Mean Dep	0.299	0.299	0.360	0.299	0.299
Observations	730,364	730,364	730,364	730,364	730,364
	(6)	(7)	(8)	(9)	(10)
Below Median Income	Pre-Tax Income	Post-Tax Income	Enrollment Within Two Years	Elasticity (Annual)	Elasticity (Lifetime)
Win Amount ($100k) * Pre-HS Grad	0.0015	0.0020	0.0031*	0.0028	0.0509
	(0.0016)	(0.0025)	(0.0018)	(0.0031)	(0.0552)
Mean Dep	0.138	0.138	0.180	0.138	0.138
Observations	730,526	730,526	730,526	730,526	730,526

Note: This table presents estimates of the effect of income shocks on four-year college attendance when accounting for taxes and expanding the definition of enrollment to include the year after the (projected) high school graduation year, as well as the elasticity of attendance with respect to household income. The top panel presents estimates for households with above median income ($44,699) and the bottom panel for households with below median income. The first two columns of each panel present estimates of the percentage point effect of income shocks on college attendance before and after taxes are deducted from lottery winnings, respectively. Taxes are estimated based on household income in the year prior to the lottery win. The third column presents estimates based on attendance in the year of expected high school graduation or in the subsequent year. Each of these linear specifications interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. The last two columns present the elasticity of attendance with respect to average annual household income and total household income earned when the child is aged 0 to 18, respectively. Attention in each column is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A29: Four-Year Attendance: Family Composition

	(1)	(2)	(3)	(4)	(5)	(6)
		Number Children			Family Size	Per Child
	One	Two	Three	Four+	Weighted	Win Amount
Win Amount ($100k) * Pre-HS Grad	0.0069*	0.0083***	0.0040*	0.0046**	0.0056***	0.0118***
	(0.0038)	(0.0024)	(0.0024)	(0.0020)	(0.0013)	(0.0027)
Mean Dep	0.265	0.295	0.251	0.148	0.246	0.219
Observations	132,619	361,710	333,175	633,386	1,460,890	1,460,890

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year after high school graduation. Columns 1-4 present results for students from households with 1, 2, 3, and 4 or more total children. Column 5 reweights the sample such that each lottery winning family, rather than each affected child (i.e., children who graduate within 6 years of the lottery win), gets equal weight. Column 6 divides the lottery win by the number of children in the household. The linear specification interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A30: Heterogeneity in Four-Year Attendance by College Type

Above Median Income	(1) Private	(2) Public	(3) For-Profit	(4) High Earn	(5) Avg Earn
Win 10-30k Pre-HS Grad	0.0035	-0.0097	0.0003	-0.0026	-417.41
	(0.0035)	(0.0045)	(0.0009)	(0.0048)	(308.51)
Win 30-100k Pre-HS Grad	-0.0083	-0.0031	-0.0001	-0.0048	-831.57
	(0.0056)	(0.0081)	(0.0017)	(0.0084)	(547.99)
Win 100-300k Pre-HS Grad	0.0010	0.0327***	-0.0018	0.0147	1,287.46*
	(0.0081)	(0.0109)	(0.0022)	(0.0111)	(702.83)
Win 300k-1.0m Pre-HS Grad	0.0075	0.0517**	0.0039	0.0348	3,341.81**
	(0.0177)	(0.0234)	(0.0042)	(0.0241)	(1,494.75)
Win 1.0m or more Pre-HS Grad	0.0672***	0.0600**	0.0114*	0.0796***	5,207.70***
	(0.0225)	(0.0285)	(0.0067)	(0.0303)	(1,873.24)
Mean Dep	0.105	0.188	0.006	0.235	24,626.36
Observations	730,630	730,630	730,630	730,630	730,630
Below Median Income	(6) Private	(7) Public	(8) For-Profit	(9) High Earn	(10) Avg Earn
Win 10-30k Pre-HS Grad	0.0018	-0.0010	0.0022**	-0.0019	125.18
	(0.0025)	(0.0035)	(0.0010)	(0.0035)	(260.78)
Win 30-100k Pre-HS Grad	-0.0063	0.0012	-0.0036**	0.0046	-405.25
	(0.0040)	(0.0065)	(0.0016)	(0.0064)	(460.73)
Win 100-300k Pre-HS Grad	0.0017	-0.0131	0.0003	-0.0142	-804.35
	(0.0069)	(0.0098)	(0.0024)	(0.0093)	(693.32)
Win 300k-1.0m Pre-HS Grad	0.0082	0.0015	0.0186**	0.0004	1,277.19
	(0.0118)	(0.0209)	(0.0090)	(0.0171)	(1,405.97)
Win 1.0m or more Pre-HS Grad	-0.0081	0.0580*	-0.0128	0.0561	2,767.90
	(0.0234)	(0.0300)	(0.0109)	(0.0343)	(2,324.54)
Mean Dep	0.043	0.088	0.007	0.088	11,576.40
Observations	730,632	730,632	730,632	730,632	730,632

Note: Estimates show the effect of income shocks on four-year college enrollment by sector (columns 1 to 3), enrolling at a college with high earning attendees (column 4), and the average earnings of attendees of the college attended (column 5). The top and bottom panels present the results for students from households with above and below median income ($44,699). Students for whom the win occurs prior to high school graduation are potentially affected. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A31: Four-Year Attendance: Heterogeneity by Timing

	(1)	(2)	(3)	(4)
	Average Trends		Trends by Income	
	6 Years	All Years	6 Years	All Years
Above Med Inc * Yrs Pre Grad * Win Amt ($100k) Pre-HS Grad			-0.0008	-0.0010
			(0.0014)	(0.0009)
Yrs Pre Grad * Win Amt ($100k) Pre-HS Grad	0.0001	-0.0001	0.0012	0.0007
	(0.0007)	(0.0005)	(0.0010)	(0.0007)
Pre-HS Grad * Win Amount ($100k)	0.0081***	0.0084***	0.0057	0.0077***
	(0.0028)	(0.0021)	(0.0037)	(0.0027)
Yrs Pre Grad * Win Amt ($100k)	-0.0003	-0.0003	-0.0012**	-0.0012**
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
Yrs Pre Grad * Pre-HS Grad	0.0043***	0.0040***	0.0037***	0.0034***
	(0.0004)	(0.0003)	(0.0005)	(0.0004)
Win Amt ($100k)	0.0016	0.0016	-0.0022	-0.0022
	(0.0014)	(0.0014)	(0.0019)	(0.0019)
Yrs Pre Grad	0.0017***	0.0013***	0.0031***	0.0026***
	(0.0003)	(0.0003)	(0.0004)	(0.0003)
Pre-HS Grad	-0.0017	-0.0009	-0.0019	-0.0010
	(0.0015)	(0.0012)	(0.0017)	(0.0015)
Above Med Inc * Pre-HS Grad * Win Amount ($100k)			0.0019	0.0006
			(0.0050)	(0.0038)
Above Med Inc * Yrs Pre Grad * Win Amount ($100k)			0.0011	0.0011
			(0.0008)	(0.0008)
Above Med Inc * Yrs Pre Grad * Pre-HS Grad			0.0004	0.0008
			(0.0008)	(0.0006)
Above Med Inc * Win Amt ($100k)			0.0020	0.0020
			(0.0027)	(0.0027)
Above Med Inc * Yrs Pre Grad			0.0010*	0.0009*
			(0.0005)	(0.0005)
Above Med Inc * Pre-HS Grad			-0.0014	-0.0024
			(0.0028)	(0.0024)
Above Med Inc			0.1585***	0.1582***
			(0.0019)	(0.0019)
Mean Dep	0.219	0.224	0.219	0.224
Observations	1,460,890	1,902,983	1,460,890	1,902,983

Note: This table presents a test of whether the effect of a lottery win varies with the timing of the win relative to a child's high school graduation. The number of years between the win and the year of high school graduation is interacted with the win amount (in hundreds of thousands of dollars). Estimates are presented for wins that occur in the six years prior to graduation and for all lottery wins, extending as far back as 14 years prior to graduation. Columns 3 and 4 differentiate the effects for households with above and below median income ($44,699). Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A32: Financial Aid: Expected Family Contribution, Grants, Loans, and Tax Benefits

	(1) FAFSA Application	(2) Expected Fam. Contr.	(3) Pell Grants	(4) Subsidized Loans	(5) Unsubsidized Loans	(6) Tax Credits & Deductions
Win 10-30k Pre-HS Grad	-0.0059	242.0450	-89.44***	-15.87	-111.59	-20.01
	(0.0039)	(184.7606)	(34.12)	(31.59)	(105.94)	(20.34)
Win 30-100k Pre-HS Grad	-0.0201***	1,620.0784***	-147.52**	-118.34**	4.52	44.75
	(0.0068)	(352.0651)	(62.18)	(55.62)	(187.50)	(37.18)
Win 100-300k Pre-HS Grad	-0.0083	3,860.7219***	-316.13***	-305.71***	371.30	52.20
	(0.0095)	(582.6894)	(71.70)	(70.21)	(249.64)	(45.32)
Win 300k-1.0m Pre-HS Grad	-0.0106	4,109.3422**	54.83	-204.98	-632.42	146.20
	(0.0204)	(1,614.9986)	(139.00)	(140.96)	(554.41)	(111.33)
Win 1.0m or more Pre-HS Grad	-0.1326***	2,607.4711	-603.06***	-1,003.23***	-1,909.92***	-43.05
	(0.0245)	(2,373.4101)	(181.60)	(166.47)	(719.99)	(108.18)
Mean Dep	0.294	7,380.04	1,163.24	1,550.34	2,985.71	876.19
Observations	1,461,262	466,280	319,341	319,341	319,341	319,341

Note: Estimates show changes in the rate of FAFSA filing, expected family contribution (EFC), Pell Grants, subsidized and unsubsidized loans, and postsecondary tax credits and deductions. Expected family contributions are conditional on filing a FAFSA, and grants, loans, and tax benefits are conditional on college attendance. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A33: Imputed Change in State and Institutional Financial Aid

	(1)	(2)	(3)	(4)
	All Households		Below Med Inc Households	
	Non-FAFSA Year	FAFSA Year	Non-FAFSA Year	FAFSA Year
Win 10-30k Pre-HS Grad	-2.39***	-57.15***	-1.25***	-65.91***
	(0.88)	(1.40)	(0.41)	(1.14)
Win 30-100k Pre-HS Grad	-2.96*	-279.43***	-1.94***	-372.00***
	(1.58)	(4.48)	(0.53)	(6.59)
Win 100-300k Pre-HS Grad	-8.89***	-950.71***	-9.3724***	-1,447.03***
	(2.47)	(17.60)	(1.15)	(25.27)
Win 300k-1.0m Pre-HS Grad	-47.52***	-1,612.25***	-59.41***	-2,203.37***
	(4.62)	(40.79)	(3.57)	(6.40)
Win 1.0m or more Pre-HS Grad	-563.84***	-1,403.13***	-863.89***	-2,169.36***
	(36.42)	(63.02)	(82.33)	(32.42)
Observations	1,316,754	960,702	656,921	468,841

Note: This table shows estimated changes in the amount of state and institutional aid for all households and households with below median income ($44,699). The values are imputed using the National Postsecondary Student Aid Study. Columns 1 and 3 exclude lottery wins in the critical FAFSA year (the year prior to high school graduation) and columns 2 and 4 only include the critical FAFSA year and post-graduation control years. The imputation is done for all households, regardless of whether children attended college. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A34: Four-Year Attendance: Critical FAFSA Year With Time Trends

	(1)	(2)	(3)	(4)
	All Households		Below Med Inc Households	
	Without Time Trend	With Time Trend	Without Time Trend	With Time Trend
FAFSA Year * Win Amt ($100k)	0.0015	0.0019	0.0014	0.0034
	(0.0027)	(0.0037)	(0.0036)	(0.0046)
Mean Dep	0.219	0.219	0.138	0.138
Observations	1,460,890	1,460,890	730,526	730,526

Note: Estimates show changes in the rate of four-year college attendance for all households and households with below median income. The estimates measure whether the critical FAFSA year has a differential effect on college attendance with and without controlling for time trends in the effect of income. The number of years between the win and the year of high school graduation is interacted with the win amount (in hundreds of thousands of dollars). The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A35: Parental Responses to Income Shocks: Linear Specification

	(1) Earnings	(2) Any Work	(3) Self Employ Earnings	(4) Investment Income	(5) Mortgage (prior=0)	(6) Mortgage (prior=1)	(7) Zip Code Income	(8) Zip Code Coll Rate	(9) County Mobility
Win Amount ($100k) * Pre-HS Grad	-1170.56 ***	-0.0062***	-4.09	310.75***	0.0155***	-0.0083***	216.76**	0.0010***	-0.0003
	(179.25)	(0.0011)	(33.31)	(45.79)	(0.0025)	(0.0015)	(104.93)	(0.0002)	(0.0008)
Mean Dep	51,275.28	0.824	2,086.84	428.51	0.028	0.91	51,425.55	0.287	0.006
Observations	1,460,890	1,460,890	1,389,936	1,460,890	643,408	817,482	1,389,989	1,369,561	1,383,402

Note: This table presents alternate parental responses to lottery wins, including earnings, employment, self-employment, investment, having a mortgage, and zip code characteristics. Mortgage results are differentiated between those who have mortgages and may pay them off and those who do not have mortgages and may buy a house. Zip code income is the average zip code level adjusted gross income and zip code college attendance rate is the proportion of 17-year-old residents of the zip code that attend four-year colleges in the year of their expected high school graduations. The linear specification interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A36: Children's Labor Supply and Earnings Responses to Income Shocks

	(1)	(2)	(3)	(4)	(5)
			Below Med Inc Households		
	Earnings Yr HS Grad	Any Work Yr HS Grad	Earnings (if attend)	Any Work (if attend)	Earnings Age 27
Win 10-30k Pre-HS Grad	-78.20*	-0.0035	-20.08	0.0005	-218.12
	(41.77)	(0.0037)	(124.03)	(0.0107)	(295.95)
Win 30-100k Pre-HS Grad	38.28	-0.0013	-46.11	-0.0101	195.47
	(82.35)	(0.0066)	(201.91)	(0.0184)	(478.52)
Win 100-300k Pre-HS Grad	-203.63**	0.0003	93.08	0.0253	1,128.69
	(95.59)	(0.0088)	(262.84)	(0.0270)	(694.78)
Win 300k-1.0m Pre-HS Grad	-503.04**	-0.0223	-1,005.39*	-0.0806	2,815.30*
	(199.84)	(0.0195)	(575.23)	(0.0519)	(1,613.11)
Win 1.0m or more Pre-HS Grad	-1,284.37***	-0.0880***	-1,625.91**	-0.1714**	1,935.31
	(318.76)	(0.0268)	(779.78)	(0.0859)	(1,809.84)
Mean Dep	4,082.21	0.741	4,003.81	0.785	20,932.83
Observations	1,461,262	1,461,262	170,138	170,138	816,342

Note: This table presents estimates of child earnings and employment in the year after high school graduation and at age 27 (for those whom we observe at this age). Column 3 and 4 restrict attention to students from below-median income households who are enrolled in college. The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A37: Earnings and Savings: Heterogeneity Tests of Implied Marginal Propensity to Consume

	(1)	(2)	(3)	(4)
	Earnings		Investment Income	
Above Med Inc * Win Amt ($100k) Pre-HS Grad	-606.10**	-470.07**	157.15*	166.82**
	(237.27)	(221.61)	(81.48)	(67.37)
Home Owner * Win Amt ($100k) Pre-HS Grad		-303.34		-24.35
		(228.49)		(71.13)
Pre-HS Grad * Win Amt ($100k)	-555.37***	-428.74***	204.25***	214.73***
	(102.50)	(147.17)	(53.59)	(76.52)
Pre-HS Grad * Above Med Inc	-5,952.04***	-5,977.45***	1.07	-35.54**
	(132.32)	(135.68)	(15.61)	(17.65)
Above Med Inc * Win Amt ($100k)	251.53	228.05	21.55	15.94
	(153.00)	(142.28)	(16.34)	(14.32)
Pre-HS Grad * Above Med Inc	-5,952.04***	-5,977.45***	1.07	-35.54**
	(132.32)	(135.68)	(15.61)	(17.65)
Win Amt ($100k)	338.52***	116.11	6.34*	-1.85
	(66.19)	(91.85)	(3.73)	(6.80)
Pre-HS Grad	1,857.82***	2,127.46***	-19.29	-44.34***
	(116.03)	(120.91)	(13.93)	(14.79)
Above Med Inc	56,102.23***	50,123.92***	483.66***	444.33***
	(90.27)	(91.37)	(10.47)	(12.46)
Home Owner * Win Amt ($100k)		234.50		16.04
		(147.55)		(13.44)
Home Owner * Pre-HS Grad		-623.74***		76.68***
		(126.98)		(17.19)
Home Owner		14,001.16***		92.29***
		(86.71)		(12.54)
Mean Dep	51,275.28	51,275.28	428.51	428.51
Observations	1,460,890	1,460,890	1,460,890	1,460,890

Note: This table presents tests of whether the effects of lottery wins on earnings and investments vary with household income. An indicator for a household having above median income ($44,699) is interacted with the win amount (in hundreds of thousands of dollars). Results are presented with and without including an interaction for home ownership. The specifications include state-by-year of win fixed effects and cohort fixed effects. Attention is restricted to lottery wins of 5 million dollars or less. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A38: Persistence of Parental Responses to Lottery Wins

Full Sample	(1) Imputed Wealth	(2) Earned Income	(3) Mortgage (prior=0)	(4) Debt Cancellation
Win Amount ($100k): Year 1	70,702.70***	-1,138.17***	0.0135***	-0.0004
	(7,437.44)	(83.01)	(0.0022)	(0.0005)
Win Amount ($100k): Year 2	56,103.06***	-1,205.40***	0.0154***	-0.0009**
	(8,482.55)	(95.87)	(0.0025)	(0.0005)
Win Amount ($100k): Year 3	54,474.21***	-1,169.13***	0.0171***	-0.0007*
	(10,600.02)	(107.19)	(0.0027)	(0.0004)
Win Amount ($100k): Year 4	44,853.83***	-1,118.70***	0.0171***	-0.0008**
	(11,220.08)	(117.93)	(0.0030)	(0.0004)
Win Amount ($100k): Year 5	47,621.63**	-1,120.06***	0.0157***	-0.0008**
	(19,453.30)	(132.22)	(0.0031)	(0.0004)

Note: This table presents the persistence of parental responses to lottery wins for imputed wealth, earned income, having a mortgage, and debt cancellation. Mortgage results are conditional on not having a mortgage prior to the lottery win. Each coefficient stems comes from a separate regression examining the change in the value of the outcome from the year prior to the lottery win to each subsequent year. In column 1, lottery winnings are adjusted for taxes based on household income in the year prior to the lottery win in order to match post-tax wealth. In column 2, pre-tax winnings are compared to pre-tax changes in earned income. Likewise, pre-tax winnings are also used in columns 3 and 4. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include winner household fixed effects and year of win fixed effects. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A39: Parental Investment Responses to Lottery Wins

	(1)	(2)	(3)	(4)	(5)
	Partnerships	S Corporations	C Corporations	Rental Inc	Retirement Contribution
Win Amount ($100k) * Year After	0.0107***	0.0013**	0.0493***	0.0047***	0.0118***
	(0.0039)	(0.0005)	(0.0053)	(0.0010)	(0.0020)

Note: This table presents estimates of having income from partnerships, S corporations, C corporations, rental properties, or making contributions to retirement accounts. The outcome variables in columns 1 through 3 are the number of partnerships and corporations from which the lottery winning household receives income. Columns 4 and 5 are binary variables indicating whether the household receives any income from rental properties or makes any contribution to a retirement account. The specification reveals the change per $100,000 of lottery winnings between the year before and after the lottery win. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include winner household fixed effects and year of win fixed effects. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A40: Published Statistics on College Cost, Aid, and Debt (current dollars)

	2000-2001				2011-2012			
	Four-Year Non-Profit	Four-Year Public	Two-Year Public	Private For-Profit	Four-Year Non-Profit	Four-Year Public	Two-Year Public	Private For-Profit
Full-time students								
List Tuition and Fees								
25th Percentile	11,920	2,520	724	8,202	20,640	5,765	1,600	11,415
50th Percentile	15,746	3,314	1,387	9,644	28,310	7,175	2,704	12,800
75th Percentile	19,730	4,094	1,799	12,090	36,130	9,367	3,542	15,605
List Tuition, Fees, Room and Board								
25th Percentile	17,714	7,347	3,804	15,778	31,460	14,478	6,703	15,827
50th Percentile	22,554	8,468	4,627	19,403	39,596	16,860	7,831	15,827
75th Percentile	27,476	9,816	5,750	21,400	49,146	19,137	9,674	25,518
Avg List Tuition, Fees, Room and Board (*College Board*)	22,842	8,671	7,191	–	38,501	17,401	10,446	–
Avg Net Price (*College Board*)	15,594	6,255	5,409	–	22,219	11,882	6,655	–
First-time, Full-time students								
Share enrolled in sector	21	41	27	10	16	41	27	11
Avg Estimated Total Cost (on campus residents)	–	–	–	–	41,418	20,997	12,823	28,921
Avg Net Price (conditional on Federal aid)	–	–	–	–	23,540	12,410	6,980	19,300*
Financial Aid Participation by Source								
Federal Grants	27	27	35	49	34	39	58	75
State Grants	32	37	29	15	27	37	33	8
Institutional Grants	70	30	12	6	81	42	11	15
Federal Loans (excl. Parent PLUS loans)	58	41	15	64	63	53	27	82
Program Graduates								
Share that borrowed (excl. Parent PLUS loans)	66	60	33	93*	74	64	42	88*
Average Cumulative Debt (excl. Parent PLUS loans)	19,620	16,210	8,060	13,440*	32,310	25,640	13,970	24,680*

Note: Data for this table were compiled from the U.S. Department of Education's Digest of Education Statistics, 2013, Table 331.20 (share enrolled; sources of financial aid), Table 330.30 (list tuition and fees), Table 330.40 (estimated total cost), and Table 331.30 (net price), and the Digest of Education Statistics, 2017, Table 331.95 (cumulative debt statistics among program graduates). Cumulative debt for earlier period pertains to 1999-2000 (rather than 2000-2001). Average cumulative debt is conditional on borrowing. Entries denoted with asterisks pertain to two-year for-profit institutions only. Tuition measures from the College Board reflect those published in *Trends in Student Aid* 2018, Table 7, and pertain to a different sample than the other measures in the table (4,000 schools in their Annual Survey of Colleges).

Table A41: College Cost and Parental Support by Household Income: NPSAS 2011-2012

	Total List Cost	Net Cost (less grants)	Financially Dependent	Receive Parental Support	Parental Support Amount
All Students					
Two-Year College	11,551	9,209	0.82	0.65	2,960
Four-Year College	27,261	19,349	0.93	0.81	9,677
Two-Year by Household Income (if reported)					
0-30K	11,553	7,734	0.91	0.56	1,712
30-60k	11,372	9,223	0.93	0.74	2,931
90-120k	11,937	10,962	0.94	0.84	4,924
120K+	12,093	11,392	0.94	0.87	7,796
Four-Year by Household Income (if reported)					
0-30K	24,396	13,191	0.94	0.62	3,646
30-60k	26,238	16,511	0.96	0.77	6,062
60-90k	26,666	19,467	0.98	0.88	9,511
90-120k	28,159	22,079	0.98	0.92	12,875
120K+	30,820	25,612	0.98	0.96	16,681

Note: Statistics are computed using the 2011-12 National Postsecondary Student Aid Study. The top panel presents average list tuition, net cost, financial dependence, parental support, and parental support amount for students attending two-year and four-year colleges. The middle and bottom panels differentiate the statistics by household income for students attending two-year and four-year colleges, respectively. Note that in some cases parental income is not reported and thus the student is included in the top panel but not in the middle or bottom panels.

Table A42: Four-Year Attendance by Level and Share of Student Services Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Level of Spending on Services				Share of Spending on Services			
	Bottom	Top	Below	Above	Bottom	Top	Below	Above
All Households	Quartile	Quartile	Median	Median	Quartile	Quartile	Median	Median
Win Amount ($100k)	-0.0004	0.0021***	0.0016**	0.0027***	0.0008	0.0017**	0.0018**	0.0026***
* Pre-HS Grad	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0008)	(0.0007)	0.0009	0.0010
Mean Dep	0.0464	0.0464	0.0927	0.0927	0.0464	0.0464	0.0927	0.0927
Observations	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Level of Spending on Services				Share of Spending on Services			
	Bottom	Top	Below	Above	Bottom	Top	Below	Above
Above Median Income	Quartile	Quartile	Median	Median	Quartile	Quartile	Median	Median
Win Amount ($100k)	-0.0006	0.0029***	0.0021*	0.0042***	0.0010	0.0028***	0.0027**	0.0039***
* Pre-HS Grad	(0.0008)	(0.0010)	(0.0011)	(0.0012)	(0.0009)	(0.0009)	(0.0011)	(0.0013)
Mean Dep	0.0606	0.0661	0.1249	0.1324	0.0711	0.0649	0.1369	0.1309
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	Level of Spending on Services				Share of Spending on Services			
	Bottom	Top	Below	Above	Bottom	Top	Below	Above
Below Median Income	Quartile	Quartile	Median	Median	Quartile	Quartile	Median	Median
Win Amount ($100k)	0.0000	0.0007	0.0010	0.0002	0.0007	-0.0001	0.0004	0.0006
* Pre-HS Grad	(0.0008)	(0.0008)	(0.0012)	(0.0012)	(0.0009)	(0.0009)	(0.0011)	(0.0013)
Mean Dep	0.0320	0.0266	0.0605	0.0529	0.0353	0.0278	0.0632	0.0544

Note: Estimates show the percentage point effect of income shocks on attending four-year colleges with higher and lower levels and shares of spending on student services (as defined in Jacob et al. (2018)). The first four columns estimate enrollment at colleges categorized by the total level of spending on students services. The next four columns estimate enrollment at colleges categorized by the share of spending on student services relative to total spending on student services and academics. Colleges are divided into above and below median and top and bottom quartile for both measures. The results are presented for all households (top panel), households with above median income (middle panel), and households with below median income (bottom panel). Only four-year colleges with spending data reported by the National Center for Education Statistics are included in the analysis. The linear specification interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A43: Four-Year Attendance by Zip Code College Attendance Rates

	(1)	(2)
Zip Coll Rate * Win Amt ($100k) * Pre-HS Grad	0.0187**	0.0285**
	(0.0093)	(0.0122)
Zip Income * Win Amt ($100k) * Pre-HS Grad		-0.0056
		(0.0047)
Mean Dep	0.219	0.219
Observations	1,431,157	1,431,157

Note: This table presents estimates of how the effect of income shocks on attending four-year colleges varies with the college attendance rate and average income in the students zip code. The linear specification interacts the win amount (in hundreds of thousands of dollars) with an indicator for the win occurring prior to high school graduation. Column 1 presents the baseline estimate, while column 2 controls for heterogeneity by the natural log of zip code income. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A44: College Readiness by Household Income: Education Longitudinal Study

Household Income	Graduate On Time	PISA Top 3 Quartiles	HS GPA 2.0 or Higher	Graduate, Score, GPA (All Three)	Student Plans 4-Yr Coll	Parent Plans 4-Yr Coll	HS Coll Prep Program	Number Students
0,000-5,000	0.74	0.46	0.67	0.35	0.55	0.79	0.47	563
5,001-15,000	0.76	0.59	0.70	0.45	0.56	0.82	0.42	1,050
15,001-25,000	0.78	0.63	0.72	0.49	0.60	0.82	0.44	1,782
25,001-35,000	0.83	0.71	0.76	0.59	0.63	0.85	0.49	1,894
35,001-50,000	0.85	0.76	0.78	0.64	0.66	0.86	0.50	3,022
50,001-75,000	0.90	0.83	0.84	0.73	0.72	0.88	0.55	3,316
75,001-100,000	0.93	0.88	0.88	0.81	0.78	0.92	0.60	2,178
100,001-200,000	0.94	0.93	0.91	0.86	0.83	0.96	0.68	1,810
200,001 or more	0.96	0.93	0.94	0.90	0.84	0.97	0.77	582
Total	0.85	0.75	0.80	0.65	0.69	0.87	0.55	16,200

Note: This table presents measures of college readiness from the Education Longitudinal Study of 2002. Students were initially surveyed in 2002 as 10th graders and in subsequent years. The college readiness measures include graduating from high school on time, scoring in the top 3 quartiles of the Programme for International Student Assessment (PISA), having a high school GPA of 2.0 or higher, satisfying all three prior criteria, the student planning to attend college (as measured in 10th grade), how much schooling the parent wants the child to achieve (as measured when the student is in 10th grade), and attending a college preparation program.

Table A45: Four-Year Attendance: Scaled by Feasibility of Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
Linear Specification	Grad On Time	Grad, Score, GPA	Plans 4-Yr Coll	Coll Prep Program	Non-Attend Rate	Log Odds Ratio
Win Amount ($100k) * Pre-HS Grad	0.0064***	0.0074***	0.0075***	0.0096***	0.0086***	0.0315***
	(0.0013)	(0.0017)	(0.0016)	(0.0021)	(0.0019)	(0.0064)
Mean Dep	0.247	0.317	0.303	0.391	0.312	0.219
Observations	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890
	(7)	(8)	(9)	(10)	(11)	(12)
Test for Heterogeneity	Grad On Time	Grad, Score, GPA	Plans 4-Yr Coll	Coll Prep Program	Non-Attend Rate	Log Odds Ratio
Win Amt ($100k) * Pre-HS Grad	0.0069***	0.0078**	0.0083**	0.0099**	0.0106***	0.0245*
* Above Med Inc	(0.0026)	(0.0036)	(0.0033)	(0.0043)	(0.0034)	(0.0141)
Win Amount ($100k) * Pre-HS Grad	0.0017	0.0022	0.0020	0.0030	0.0015	0.0129
	(0.0020)	(0.0030)	(0.0026)	(0.0034)	(0.0020)	(0.0118)
Mean Dep	0.247	0.317	0.303	0.391	0.312	0.219
Observations	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890	1,460,890

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of high school graduation after scaling by the fraction of children who could feasibly be treated. Columns 1 through 4 scale by measures of college readiness from the Education Longitudinal Study of 2002. The measures include the fraction of children who graduate from high school on time, satisfy three measures of college readiness (graduating on time, scoring in the top three quartiles of the PISA, and having a high school GPA of 2.0 or higher), fraction planning to attend college (as measured in 10th grade), and being enrolled in a college preparatory program. Column 5 scales by the fraction of students who would not attend college in the absence of treatment, which is measured using older, unaffected children. Column 6 presents the log odds ratio estimated using a logistic model. The bottom panel tests for heterogeneity by household income after scaling each household income range by the relevant measure of college readiness or treatment feasibility. Attention is restricted to lottery wins of 5 million dollars or less. The specifications include state-by-year of win fixed effects and cohort fixed effects. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A46: Measures of Debt and Debt Prices

	Below Median Income	Above Median Income
Share with Debt		
Any Debt	0.76	0.94
Mortgage Debt	0.35	0.75
Credit Card Debt	0.40	0.56
Installment Debt	0.49	0.63
Average Balances (incl. zeroes)		
All Debt	48,477	187,320
Mortgage Debt	33,653	144,763
Credit Card Debt	1,808	4,907
Installment Debt	9,498	17,078
Average Interest Rates for Those with Debt		
Mortgage Debt	6.48	6.02
Credit Card Debt	14.82	12.99
Share with Debt Cancellation	0.036	0.042

Note: Statistics (except in bottom row) derived from 2001, 2004, 2007, 2010, and 2013 Survey of Consumer Finances (SCF) data describing tax-filing households with at least one child under 24 years old. Bottom row derived from IRS tax records of lottery winners in our main sample from the year before the win. Mortgage debt refers to all debt (e.g., mortgage, home equity loans) associated with principal residence. Installment debt refers to vehicle loans, education loans, and other installment loans. Interest rate measures for mortgages and credit cards refer to rates associated with first reported mortgage and highest balance credit card, respectively. For SCF measures, median income defined as $45,000 to correspond to median income in the main sample. Balance figures are in 2016 dollars.

Table A47: Literature: Borrowing Constraints and College Outcomes

Data	Evidence of Borrowing Constraints	Additional Conclusions
Cameron and Heckman (1998)		
OCG, NLSY 79	not a primary factor	short-run constraints do not matter, long-run family factors are important
Acemoglu and Pischke (2001)		
NLS 72, HSB, NELS	potential factor	roll of income may be due to credit constraints or college as a consumption good
Cameron and Heckman (2001)		
NLSY 79	not a primary factor	racial disparities in college are due to long-run factors, not short-run constraints
Keane and Wolpin (2001)		
NLSY 79	not a primary factor	college contingent parental transfers matter, constraints affect working during college
Carneiro and Heckman (2002)		
NLSY 79	not a primary factor	constraints bind for small fraction of households, price effects do not imply constraints
Cameron and Taber (2004)		
NLSY 79	not a primary factor	instrumental variable and structural estimates do not reveal constraints in policy environment
Belley and Lochner (2007)		
NLSY 79, 97	primary factor	constraints matter more in NLSY97 than NLSY79, high income may have consumption value
Brown, Scholz, and Seshadri (2011)		
HRS	factor for some households	college contingent parental transfers matter, constraints bind for households that underinvest
Lochner and Monge-Naranjo (2011)		
NLSY 79, 97	primary factor	constraints matter more in NLSY97 than NLSY79 due to rising costs, stagnant federal aid

Table A48: Literature: Resource Shocks and Educational Attainment

Income shock	Dataset / country	Research design	Main estimates	Heterogeneity by resources / age
Outcome: College Attendance				
Bastian and Michelmore (2018)				
EITC exposure in childhood (maximum potential benefit)	1968-2013 waves of the Panel Study of Income Dynamics (PSID) / U.S.	Compares EITC exposure during childhood over time for three age ranges (0-5 y.o., 6-12 y.o., and 13-18 y.o.)	No significant effect on college attendance for any group. For other outcomes: effects are larger and only significant in the 13-18 y.o. group and imply a $1,000 increase in EITC exposure between 13 and 18 y.o. leads to a 1.2 p.p. increase in high school graduation, a 1.3 p.p. increase in college graduation, 0.08 more years of schooling, a 0.8 p.p. increase in employment, and $564 increase in annual earnings	Effects only appear for children 13-18 y.o. and not younger
Coelli (2011)				
Parental job loss via permanent layoff or employer going out of business	Canadian Survey of Labour and Income Dynamics / Canada	Compares childrens attendance outcomes through age 20 for those whose parent lost their job when they were 16-18 y.o. to those whose did not	Estimates imply $1,000 decrease in income reduces probabilities of attending any postsecondary and of attending a university, both by about 1 p.p.	Effects concentrated among high income parents (who saw larger changes in income) and job losses that occurred at age 16 or 17
Hilger (2016)				
Parental job loss (as identified by take-up of unemployment insurance in a given year after at least one year of non-receipt)	Administrative tax records of over 7 million laid off fathers (and over 30 million "survivor" fathers who worked at a firm with at least one laid off father) from 2000-2009 / U.S.	Nonparametric difference- in- differences comparing enrollment outcomes among children aged 18-22, exploiting both the timing of parental layoffs with respect to the child's age (between 12 and 29) and an additional control group formed by children of layoff survivor fathers	Layoffs (estimated to equal a net present value wealth shock of about $100,000) reduce annual college enrollment over ages 18-22 by less than half of 1 p.p. and have no significant impact on children's earnings up through age 25	Effects on enrollment are smallest at low incomes, largest at middle incomes, and declining at higher incomes; effects are similar in families with high and low financial asset holdings

Lovenheim (2011)				
Family housing wealth	1,497 panel records for households with 18-19 year olds that can be identified in the PSID 2001, 2003, and 2005 / U.S.	Compares enrollment (i.e., completion of at least 13 years of education within 2 years of the survey) and self-reported home equity levels, instrumenting for the latter with the change in the family's home equity or home value over the prior four years, conditioning on homeownership, family annual income, household characteristics, and economic conditions	A $10,000 home value increase raises the likelihood that a child attends college by between 0.56 and 0.71 p.p., depending on the instrument	Effect is strongest– 5.67 p.p. – for households earning less than $70,000 per year
Lovenheim and Reynolds (2013)				
Family housing wealth	2,801 restricted-use National Longitudinal Survey of Youth 1997 panel records for children who were 12-17 years old in 1997 and who attended college within two years of high school graduation, interviewed annually through 2008 / U.S.	Multinomial logit among homeowners estimating the likelihood a student attends a flagship public university, a private university, or a two- year college, relative to non-flagship public four-year schools (the omitted category), as a function of the level difference in real home prices over the four years prior to the student turning 18 years old	Relative to attending a non-flagship public university, a $10,000 four-year home price increase increases the rate of attending a public flagship by 0.19 p.p. (2.0 percent) and marginally significantly decreases community college attendance by 0.59 p.p. (1.6 percent), with no significant effect on four-year private attendance	Comparing effects among three income groups, they are largest for students from lower and middle- class households earning less than $75,000 per year
Manoli and Turner (2018)				
EITC	Over 2 million administrative tax records for individuals who were 17 or 18 (as well as records for tax-filing units claiming them as dependents) during the years 2001 to 2011 / U.S.	Regression Kink Design relating changes in tax refunds received during the spring of the high school senior year – exploiting two different kink points in the EITC benefit schedule – to changes in enrollment rates	For students in households at EITC Kink 1 (very low income), an additional $1,000 (real 2015 dollars) in cash-on-hand from tax refunds in the spring of the high school senior year increases college enrollment in the next year by 1.3 p.p.; insignificant effects of 0.8 and 1.0 the prior year and at the other kink (moderate income), respectively	

Pan and Ost (2014)				
Parental job loss	640 panel records of respondents born between 1970 and 1985 within biennial waves of PSID beginning in 1985 / U.S.	Compares enrollment (i.e., completion of at least 13 years of education) between children who were 15-17 years old during parental layoff and children who were 21-23 years old during parental layoff	Layoffs reduce enrollment by 10.1 to 11.1 p.p.	Suggestive evidence (given small sample) that children from families with more educated parents and home-owning parents are less negatively impacted; no evidence effects differ by income
		Outcome: Years of Education		
Akee et al. (2010)				
Casino profits for American Indians, averaging $4,000 (pre-tax) per adult per year since 1996	Great Smoky Mountains Study of Youth (GSMS) – a longitudinal study of 1,420 children in rural North Carolina that began in 1993 / U.S.	Compares children from poor American Indian households that receive per capita income transfer to non-American Indian households that do not, based on whether they were 9, 11, or 13 years old at intake	No significant effect on years of education (by 21 y.o.); 15 p.p. increase in probability of graduating high school (by 19 y.o.) among children who were 9 y.o. at intake relative to those who were 13 y.o. at intake	Among households previously in poverty, 1.127 additional years of education among children who were 9 y.o. at intake, and 40 and 30 p.p. increase in probability of graduating among those who were 9 and 11 y.o., respectively
Loken (2010)				
Oil boom	Administrative population records from Statistics Norway linked to Censuses in 1960, 1970, and 1980 for children born before and after oil boom in two counties / Norway	Instruments average family income between ages 1 and 13 years old with being born in the cohort-county that would have experienced early effects of the oil boom	No significant effect on years of education (from 11-12 percent increase in permanent family income)	No evidence effect differs by parental education
Shea (2000)				
Labor market luck	PSID	Instruments fathers' earnings using fathers' union, industry, and job loss experience / U.S.	No significant effect on years of education	No evidence effect differs by parental education