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ABSTRACT

We examine more than one million children whose parents won a state lottery to trace out the effect of financial resources on college attendance. The analysis links the universe of federal tax records to federal financial aid records and leverages substantial variation in the size and timing of lottery wins. The results reveal that, while the per-dollar effect is modest (i.e., about 0.6 percentage point for every $100,000 of winnings), the relationship between win size and attendance is only weakly concave, with a high upper bound for amounts that greatly exceed the cost of college. The effects are smaller among lower-SES households, are not sensitive to how early in adolescence the resource shock occurs, and persist through each of the four years after high school. Finally, while additional resources reduce financial aid, attendance patterns are not moderated by this crowd-out. Overall, the results imply that households derive consumption value from college and that, in the current policy environment, financial constraints alone do not inhibit college attendance.

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I. Introduction

Despite a large college wage premium and increased focus on inequality of opportunity, the relationship between household financial resources and college-going is not well understood. In theory, absent financial frictions, the decision to attend college should primarily reflect its net return. And, a longstanding objective of U.S. higher education policy has been to abate or remove such frictions from this decision. Yet, empirically, there are substantial and persistent gaps in college attendance by socioeconomic background, with many households citing 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.[1]

Understanding how household resources influence the decision to attend college is a critical step to developing sound policy, but it requires overcoming the inherent identification challenge that resources and schooling decisions are correlated with children's ability levels, household preferences, and other factors. Recent quasi-experimental work strives to find settings in which changes in income or wealth can be isolated, but the resulting estimates have varied greatly in magnitude, resulting in uncertainty around answers to several key questions.[2] First, how much does, on average—and can, at maximum—altering household resources affect college-going? Second, is attendance more sensitive for lower- or higher-SES households, and is the availability of resources more important at certain ages? And, finally, to what extent are the effects of resources moderated by offsetting reductions in financial aid?[3]

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[1] These financial constraints may bind at the time of the college decision or limit earlier investment in children that is complementary with college-going. Higher education subsidies would not alleviate the latter.

[2] Early studies considered cross-sectional and time variation in income (e.g., Acemoglu and Pischke, 2001; Blanden and Gregg, 2004), while more recent work has exploited differences generated by job loss (Coelli, 2011; Pan and Ost, 2014; Hilger, 2016), tax credits (Manoli and Turner, 2016; Bastian and Michelmore, 2016), casino revenue (Akee and Copeland, 2010), land grant lotteries (Bleakley and Ferrie, 2016), oil revenue (Loken, 2010), and housing prices (Lovenheim, 2011; Lovenheim and Reynolds, 2013). A number of these studies find attendance effects as large as 1 percentage point per $1,000 of income, while others find effects of less than 1 percentage point per $100,000. This wide range of estimates could reflect differences in the responsiveness of the affected populations, the levels of resource change, or other aspects of the research designs. Related studies with a similarly diverse set of findings consider the effect of income and job loss on cognitive development, health, and employment (e.g., Blau, 1999; Maurin, 2002; Bratberg, Nilsen, and Vaage, 2008; Oreopoulos, Page, and Stevens, 2008; Ananat et al., 2011; Rege, Tella, and Votruba, 2011; Dahl and Lochner, 2012; Aizer et al., 2016; and Cesarini et al., 2016).

[3] Policymakers might be particularly interested in whether borrowing constraints, in light of existing financial aid and educational tax credit policies, still hinder households from investing in human capital; therefore, analysis should explicitly account for how these policies interact with changes in resources.

To shed light on these questions, this study examines college outcomes for 1.5 million children whose households won a state lottery between 2000 and 2013. Specifically, we leverage the universe of federal tax records to identify individuals who won as little as $600 (the federal reporting minimum) to tens of millions of dollars over this period, match winners to their children, measure these children's college attendance and financial aid (via linked federal financial aid records), and place estimates in the context of household characteristics and post-win labor supply and investment.[4] The empirical analysis leverages the substantial variation in win size and timing. Our primary outcome of interest is the transition to college from high school, and comparisons are drawn between children from households with larger and smaller wins and, to account for potentially unobserved differences between these groups, whether their parents win the lottery before or after their predicted graduation year.[5] Lottery wins in this design are balanced across a rich set of household characteristics.

The estimates reveal several core findings. First, small-to-moderate increases in resources, which should ease most immediate financial constraints, have little effect on attendance. For example, the estimated effect of lottery wins averaging $50,000 on enrollment is not statistically significant and rules out an increase of as little as 0.4 percentage point (p.p.).[6] Second, the relationship is not highly concave, and the size of the effect continues to grow at amounts greatly exceeding the cost of college, preliminary indications that households may value college in a more complex fashion than simply an investment in human capital. Specifically, while the per-dollar effect of a win is not large (0.6 p.p. per $100,000), attendance increases by an average of 5 p.p. in response to lottery wins between $300,000 and $1,000,000 and by more than 10 p.p. in response to wins of $1,000,000 or more. These effects are concentrated at four-year colleges and persist for

[4] Survey data indicate that a substantial fraction of households play the lottery each year, with Gallup Polls indicating as many as half. In Section VI, we use survey and tax data to assess how lottery players and winners compare to the general population and find, at most, modest differences. We also show that the household earnings response to lottery wins over time is in line with the predicted response to an ordinary wealth shock in a standard lifecycle model.

[5] Studies examining the effects of lotteries on labor supply, health, and cognitive development have taken various approaches for controlling for potentially unobserved differences among winners (e.g., see Imbens, Rubin, and Sacerdote (2001), Lindahl (2005), Hankins, Hoekstra, and Skiba (2011), Powdthavee and Oswald (2014), Apouey and Clark (2015), Cesarini et al. (2015, 2016)). Notably, Cesarini et al. (2015, 2016), using Swedish lotteries, can observe those who play and do not win in some cases and otherwise can explicitly control for a household's probability of experiencing a win.

[6] We also find that moderate-sized wins have little or no effect on whether children attend a two-year college, on whether children attend a public or private college, and on persistence in college.

several years after high school graduation. Finally, the results point to a very high upper bound. Thus, large changes in resources can fundamentally alter a child's educational trajectory.

We extend the analysis to explore the role of financial constraints, to examine the importance of financial aid crowd-out, and to shed light on competing uses of resources. First, we investigate if there is heterogeneity in responsiveness across household economic well-being and find that effects are, if anything, smaller among households with lower earnings and households with less savings (i.e., investment income). Second, we find no evidence that earlier resource shocks have larger effects, either in general or for lower-SES households in particular. Third, since additional resources reduce eligibility for need-based financial assistance, we examine whether aid crowd-out is suppressing the response. Namely, we develop a test that exploits the fact that lottery wins that occur in the tax year prior to filing federal financial aid forms are treated as income, which is heavily penalized in financial aid formulas, while wins that occur prior to that year are treated as wealth, which is not. This exercise reveals no evidence that offsetting reductions in aid are moderating attendance effects. Finally, we find that, within our design, households decrease labor supply, increase savings, and are more likely to allocate money towards housing. Interestingly, lower-SES households exhibit smaller reductions in earnings and smaller increases in savings, suggesting that they are more likely to use additional resources for (non-college) consumption.

This study is the first to use data on the universe of lottery winners in the United States to examine any outcome and the first to exploit variation among lottery winners to examine the effects of household financial resources on college attendance. Relative to prior work, a research design using lotteries is well-suited to estimate the role of resources: the size of the shock is salient to the household, and, unlike in settings where factors that might affect educational outcomes help generate a study's identifying variation, lottery income generally does not load other factors that confound interpretation. Several features of our design, beyond both the breadth and scope of the data as well as the use of lottery wins for identifying variation, allow us to advance the literature and to help resolve the uncertainty among disparate existing estimates and potential mechanisms. First, there is extremely wide variation in the size of lottery wins, enabling us to paint a rich picture of the magnitude of additional resources needed to generate significant changes in college outcomes and to examine the degree of concavity and the upper bound.[7] Likewise, the diversity of

[7] In particular, to examine concavity, we observe similar households receiving vastly different-sized resource shocks and thus do not need to (unrealistically) assume that there is no treatment heterogeneity by socioeconomic background.

household types affected and the variation in the timing of wins (relative to a child's age) allows us to draw more-refined comparisons within a single context to ascertain which household types are most sensitive to additional resources, to investigate the relative importance of differently timed shocks, and to estimate the role of corresponding changes in need-based financial assistance.[8] We are also able to examine alternative margins of household response, such as earnings and savings, and to consider heterogeneity in the marginal propensity to consume across households to help contextualize the findings across other types of spending. Finally, our full collection of evidence enables us to argue that our main results are consistent with households deriving consumption value from college and are not consistent with households facing binding constraints that either make college unaffordable or limit complementary investment.

The paper is organized as follows. Section II introduces the administrative data used for analysis. Section III details the empirical design and the underlying assumptions. Section IV describes the results, robustness, and extensions (including an investigation of heterogeneity and financial aid). Section V discusses the mechanisms most consistent with the results. Section VI explores issues of external validity and the relation of our findings to the prior literature. Section VII concludes.

II. Sample Construction and Characteristics

The sample is formed by merging administrative records from two sources. First, we use the universe of federal tax records for the U.S. population to identify 1.5 million individuals who graduated from high school between 1999 and 2013 and had a parent with a state-reported lottery win. Their individual tax records are linked to their federal financial aid records and their parents' tax records. Throughout the analysis, all dollar amounts are adjusted for inflation and denoted in real 2010 dollars.

To construct the sample, we identify any individual with a state lottery win reported on the third-party reporting Form W-2G for the purposes of tax withholding. This form is first available in 1999. All prizes in excess of $600 must be reported by the relevant state agency to the IRS and indicate the state and year of the lottery. The first calendar year that we observe an individual

[8] In addition, observing the calendar year in which a win occurs allows us to examine whether the year in which financial resources become salient should be taken into account in the design of policies seeking to improve college affordability (e.g., student aid offers and education tax credits).

receiving lottery income is designated as the "win year," and is used to classify household treatment.[9] In a small fraction of cases, assumptions are required to identify the exact timing or amount of an individual's first win.[10] These ambiguous cases are excluded from the baseline sample, but we show that the estimates are essentially unchanged when they are included.

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 Form 1040 over that window. This approach offers an advantage over previous lottery studies that rely on matching win amount data from lottery agencies with outcome data from other sources – a process that is likely to introduce measurement error. Parent-child matches are only included if the child is claimed both prior to the win and to turning 19 years old.[11] 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 and academic institutions. (We also present results using birth parents as determined by Social Security Administration records.)

A primary outcome of interest is whether children transition immediately to college after high school, which we derive from several sources. We measure annual attendance with Form 1098-T data, a mandatory third-party reporting form filed by post-secondary institutions that is available beginning in 1999. High school graduation cohort is approximated from each child's exact birthdate (available via social security card applications), the state in which the child was born,

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[9] 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: 1) subsequent wins could be endogenous to the size of the initial win and hence contaminate the assignment of win size and 2) using subsequent wins would lead to the misclassification of treatment status whereby some parents whose later win occurred after their child graduated from high school will also have won before their child graduated from high school. A very small fraction of households have substantial lottery wins in the years after their first win, so subsequent wins are unlikely to significantly bias the estimates. For example, less than 4 percent of households win $10,000 or more in the four years after their initial win. Likewise, 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. Similarly, restricting attention to first wins that occurred in the second half of the sample period generates similar estimates to using the full sample.

[10] This occurs when an individual is observed receiving supplemental income that matches their win amount in the year prior to state reporting (suggesting that they were collecting the win before it was reported by the state), 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 and thus needs to be converted to an annualized equivalent and may not be fully observed during the data window.

[11] Claiming children after a win could be endogenous to the extent that tax filing is influenced by lottery wins, and claiming children aged 19 and over is correlated with whether or not they are attending college.

and the corresponding school entry age laws for that state.[12] Because Form 1098-T is filed by calendar (tax) year and not by academic year, children with 1098-Ts for their predicted high school graduation year are classified as transitioning to college immediately.

We also examine the characteristics of colleges children attend as well as their financial aid receipt. For the former, Form 1098-T data are linked to college characteristics maintained by the National Center for Education Statistics Integrated Postsecondary Education Data System, including whether the college is a two- or four-year and public, private, or for-profit institution. For the latter, colleges that participate in Title IV programs (e.g., Pell Grant, subsidized and unsubsidized Stafford Loans) are required to report student-level federal aid application and disbursement to the Department of Education, which we match via social security number to each child. These data contain an array of federal financial aid information for each academic year, including application for aid (i.e., filing a FAFSA), amount of subsidized loans, amount of unsubsidized loans, Pell Grant receipt and amount, and expected family contribution.

The baseline analysis restricts the sample to children from families that experienced a lottery win within six years of their expected high school graduation year (though we evaluate the robustness of the estimates to alternative windows). As shown in Table 1, the resulting population includes nearly 1.5 million children whose households experienced a win, with 44 percent of those wins occurring prior to their expected high school graduation year. Approximately 35 percent of our sample attends college in their expected high school graduation year and approximately 22 percent attends a four-year college. Table 1 also shows that the size of the wins varies widely. About 96,000 children are from households that win over $10,000, nearly 14,000 are from households that win over $100,000, and 1,300 are from households that win over $1,000,000.

Finally, we draw on a number of child, parent, and household characteristics to test for balance in the research design, to include as controls, and to examine heterogeneity. Many of these

[12] The outcome is measured for all children and not for high school graduates only (where graduating may be endogenous to financial resources and college plans). Note that the estimates will be attenuated by students who graduate in a year other than the predicted year, if at all. For example, students who dropped out, who are held back, or who move to a state with a different school entry age would not attend college in the expected year. Because these forms of measurement error are potentially endogenous, the predicted year of graduation represents the best available exogenous measure of treatment. For these reasons, our calculated proportion that attend college in the year of expected high school graduation will also be lower than figures the Department of Education publishes of the immediate college enrollment rates of recent high school completers. To capture students who graduate a year late, which is the likely alternative to graduating as predicted (among non-dropouts), we consider an alternative definition of attendance that also includes attendance in the following year.

variables (e.g., wages, adjusted gross income, filing status, number of dependents, marital status) are reported on Form 1040 and other tax forms, while demographic information (e.g., gender, citizenship) comes from social security records. Characteristics are based on pre-win years, as post-win characteristics may be endogenous to the win.[13] To examine heterogeneity by household economic well-being, the sample is split by adjusted gross income and the presence of savings (using investment income as a proxy). The presence of an interest-bearing mortgage is used as a proxy for homeownership. We also examine several of these measures, which are based on third-party reporting forms (e.g., W-2, 1099, 1099-int, and 1099-div), as outcomes (e.g., wages, investment income, homeownership). Table 2 indicates that, prior to a lottery win, the average household in the sample has wages of approximately $52,000, and 57 percent file as married. The homeownership rate is about 56 percent, and slightly less than half of the households report interest or dividends.

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 wins and attendance. In our setting, variation in the payout may also be 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, 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 leverage variation in the timing of wins. 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

[13] 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.

child transitions to college immediately from high school cannot be affected by wins that occur in the years after high school graduation.[14] Results from this design are similar to those from a design that relies solely on win size but includes household control variables.

Finally, our 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.[15] The main 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. A second strategy, which we also implement, relies upon a continuous measure of wins but estimates, and then restricts attention to, the range of wins over which the relationship appears to be linear (i.e., over which there is no evidence of concavity). For the main strategy, 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 baseline step function specification is:

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

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 that we include incrementally.[16] The α_j coefficients absorb fixed differences across households that experience different-sized wins, with wins between \$600 and \$10,000 serving as the omitted group (though, in practice, the exact range of the omitted group

[14] It is unclear if, and to what extent, children whose parents won in the year that they graduate can respond. A win in the fall is certainly too late to change a student's decision about whether or not to enroll in a four-year college for that year. If the win occurs earlier in the year, it may nonetheless be too late for a student to take the necessary steps to enroll in college (e.g., taking the SAT or ACT and meeting application deadlines) and the student may have made other arrangements such as taking a job or enlisting in the military. Thus the preferred specification excludes wins that occur in the year of graduation, but we demonstrate our results are not sensitive to its inclusion in Section IVb.

[15] Issues with imposing linearity on the effects of income have been noted by Loken, Mogstad, and Wiswall (2012). We show that a linear specification results in estimates that are sensitive to the range of win sizes considered.

[16] These characteristics include log wages, log adjusted gross income, claiming of the mortgage interest tax deduction, claiming of the self-employment tax deduction, the presence of interest and dividends, the presence of earnings from the Social Security Administration (including disability), whether a 1040 is missing for the household 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.

is unimportant). The coefficient θ accounts for fixed differences between children who 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.[17] A number of outcomes, each with a temporal component, are examined, including several variants of college attendance (e.g., sector-specific, level-specific, different horizons relative to high school graduation), financial aid application and receipt, and parent labor force and saving outcomes.

Before turning to the main results, we examine whether the comparisons relied upon for identification appear to successfully isolate changes in resources. Specifically we test for balance by estimating our baseline step function specification with 15 different exogenous pre-win characteristics on the left hand side. Table 2 presents the results across each characteristic, including adjusted gross income, self-employment status, homeownership, and the presence of savings. Among the 15 variables we consider, only 2 are jointly significant across win sizes at the 10 percent level (self-employment and child gender), and among the 5 win size bins, none are jointly significantly different across the 15 variables. An F-test across all win size bins and variables (the resulting 75 coefficients) is not significant (p-value=0.5098). Altogether, beyond statistical noise, there do not appear to be meaningful differences in observable child and household characteristics, lending credence to the validity of the design.

IV. The Effects of Household Financial Resources

a. College Attendance

To preview the empirical results, Figure 1 offers a graphical depiction of pre- and post-lottery win attendance averages. For resource shocks of less than $100,000, any differences in attendance by timing are small. In contrast, for larger wins, the differences are substantial, with the size of the gap increasing in the amount. Note that children from households that experience larger wins are

[17] The design estimates the effect of lottery wins that occur, on average, a couple of years before high school graduation and does not hold constant subsequent endogenous changes in financial resources leading up to graduation, such as those due to labor supply or consumption, neither do we explicitly take into account the prior amount of household resources (which would require strong assumptions about homogeneity across households); thus, the estimates do not necessarily recover the structural relationship between financial resources and college enrollment.

somewhat more likely to attend college, highlighting an identification challenge that, as shown in Table 2, our design allows us to address without the inclusion of household controls.

We turn next to our formal estimates of the effect of different-sized lottery wins on attendance at any college, a four-year college, and a two-year college in the year of high school graduation. Table 3 presents estimates for attendance at any college. The estimates reveal that moderate-sized shocks have little effect on attendance. Wins between $10,000 and $30,000 and between $30,000 and $100,000 produce small, insignificant estimates that rule out effects exceeding approximately one-half p.p.. For wins between $100,000 and $300,000, the response is larger—on the order of 1 to 2 p.p.—and is marginally significant. Above this level, there are more meaningful responses. Wins between $300,000 and $1,000,000 increase attendance by approximately 5 p.p., and very large wins exceeding $1,000,000 increase attendance by approximately 10 p.p.. Table 4 presents results for four-year college attendance, which closely mirror those for any college attendance and are slightly more precise. For both outcomes, the estimates are stable to the inclusion of controls for household, parent, and child characteristics.

Table 5 presents results for two-year college attendance. The estimates are close to zero and insignificant for all win levels. For some win sizes, the interpretation of these zeroes is difficult, as there may be offsetting responses underlying them. Specifically, among larger wins, for which there was an increase in four-year enrollment, the zeroes may represent the net effect of competing margins: 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. For smaller wins, which did not affect four-year attendance, the zeroes have a more straightforward interpretation: even when we zoom in on schools that are relatively less expensive and less selective, modest shocks to resources have no material effect on attendance. Given that enrollment responses appear to be driven by adjustments along the four-year margin, for brevity, the remaining analyses will focus on four-year attendance. The results are similar when the outcome of interest is any college.

The estimates thus far represent average effects for ranges of wins, abstracting from strong functional form assumptions. However, they suggest that the effect is not very concave in the size of the win. To explore this 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).[18] The effect appears to be approximately linear until win sizes reach about $5,000,000, after which the effect of marginal winnings is limited. They also indicate an upper bound of approximately 20 p.p..

We then, in Table 6, specify win size continuously by interacting the amount of the win with whether the win occurred prior to high school graduation. (Wins are scaled by $100,000 for ease of interpretation.) 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. Note that the challenge of imposing a specific functional form on lottery wins is reflected in the sensitivity of the estimates to the range included in the analysis.[19] Consistent with the figure, the coefficient on the quadratic term when restricting attention to wins of less than or equal to $5,000,000 is indistinguishable from zero, suggesting little concavity in this region.[20] When focusing on wins within the linear region (capped at $5,000,000), the estimated linear effect on attendance is approximately 0.6 p.p. per $100,000.

From the results thus far, it appears unlikely that a large fraction of students are deterred from attending college due to binding borrowing constraints. Additional resources of $30,000 to $100,000 should be large enough to place at least some college within reach for most households, but they generate little change. It is also clear that the effects are not highly concave. For example, wins averaging $500,000 generate only a fraction of the effect observed for the largest wins. In light of these empirical findings, subsequent analysis considers how the effects differ across households of different means, whether initial changes in enrollment persist beyond the first year after high school, the role of offsetting financial aid, and household responses along other margins. Before turning to these other analyses, we first explore the robustness of the results.

b. Robustness

In this section, we examine the sensitivity of our results to the timing of lottery wins relative to the year of a child's high school graduation and consider alternate sample restrictions, alternate measures of college attendance, and other variants of our specification. We begin by amending the

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[18] Bins are selected as follows: 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.

[19] The effects must become concave at some level and must be bounded since enrollment cannot exceed 100 percent. Thus, per-dollar effects are necessarily modest for very large lottery wins. Because least squares places the greatest weight on very large wins, including them will necessarily result in small estimated effects.

[20] We also estimate the degree of concavity attempting to take into account, not only the size of the win, but also the household's permanent income during childhood (using parents' pre-lottery average AGI multiplied by 18 as a proxy). This exercise, which loads treatment heterogeneity, suggests even more linearity (up to $10,000,000 in resources).

specification to estimate the effects separately for wins that occur in each year before and after a child graduates from high school. This exercise has several purposes. A by-year specification reduces to an event-study framework, which helps substantiate the timing assumed in the baseline specification and the validity of the design. The estimates should reveal a jump in attendance outcomes for lottery wins that occurred prior to graduation and not for wins that occurred when children were older. The exercise also provides a comparison of the relative effect sizes across the treatment years, which we revisit when we examine heterogeneity. Finally, the exercise delivers estimates for wins that occur in the year of graduation, which speak to the effectiveness of policies that reduce the cost of college, or the salience of that cost, when students are in their final semester of high school (or have already graduated).

Figure 3 plots the results for large lottery wins, defined as those that exceed $100,000 in the top panel and $300,000 in the bottom (given that smaller wins did not significantly affect college-going in the main specification), and reveals that students who graduate in any of the six years following a lottery win experience positive and significant enrollment effects. In addition, estimates are insignificant for wins that occur during the year of high school graduation or any of the six subsequent years. This pattern of results affirms the timing implied by the main specification and, because there is a steep drop-off in year zero, suggests that the year in which policies that seek to improve college affordability become salient to households is highly relevant. (Of course, there may be a difference between an unexpected change in income in the year of high school graduation and a subsidy that is anticipated in advance.)

We also sequentially reduce the window of lottery win years before and after graduation (Table A1). The resulting estimates reveal that moderate-sized lottery wins, even within narrow windows, do not appear to affect college-going. This finding suggests that the lack of large effects for resource shocks of this size is not due to households spending down lottery wins prior to when they would be used for college. In addition and consistent with Figure 3, large wins continue to have large effects on college-going, even in narrow windows of timing.

The pattern and magnitude of the results is robust to a number of alternative methods of classifying lottery win sizes, constructing key variables, and defining the sample (more fully described in Appendix 1). The first set of these results is presented in Table A2 and demonstrate that estimates are stable when: restricting attention to children who are linked to their birth parent by Social Security birth records; adjusting for predicted taxes based on each household's pre-win annual income and composition (since realized taxes are endogenous); using alternative omitted win ranges as the control group; defining college attendance within one year of graduation as the outcome (to capture students who graduate a year later than predicted); and weighting the sample by population average characteristics. Likewise, the estimates are largely unaffected by the inclusion of lottery wins that occurred during the year of high school graduation and wins that could not be definitively classified in terms of size or timing. Restricting attention to within-household comparisons of siblings based solely on variation in the timing of wins results in estimates that are similar in magnitude (column 9).[21] However, because this exercise discards 74 percent of the sample – i.e., all one-child households and the majority of two-child households – the resulting estimates are much less precise and are heavily weighted toward households with many children or children born many years apart. [22]

Table A3 presents estimates for narrower bins, which also reveal no evidence of positive effects for wins of less than $100,000 with and without the inclusion of controls for household characteristics. On the other hand, and consistent with Figure 2 and Table 6, a bin for wins exceeding $3,000,000 yields an effect of approximately 15 p.p., implying a high upper bound.

Some colleges may not submit enrollment data to the IRS for students whose grants meet or exceed tuition billed, as they are not eligible for tax credits. Thus, non-classical measurement error could bias the estimates to overstate the effects of additional resources, particularly among lower-SES households (who are most likely to receive full scholarships).[23] To mitigate these concerns, we verify that the results are robust to restricting attention to colleges that appear to report attendance for all students (see Table A4).[24] Further, an alternative construction of enrollment that relies on either Form 1098-T or students who are observed receiving federal grant aid, as reported to the Department of Education, generates a similar pattern of results.

[21] The point estimates show increases of 1, 5, and 9 p.p. for the three largest win ranges. When college attendance is defined as occurring in either the calendar year of high school graduation or the following calendar year, the estimated effects are 1, and 5, and a statistically significant 14 percent.
[22] Variation in a model with lottery winner fixed effects requires that at least one child in the household must graduate before and after the win. This eliminates all one-child households, two-thirds of two-child households, and nearly half of three-child households. The average number of children per household in the resulting sample grows by 50 percent.
[23] Note that children from lower-income households would be more susceptible to such measurement error, yet subsequent results reveal that the effects we detect are driven by children from higher-income households.
[24] Specifically, we omit colleges that appear not to report (or appear to under-report) students that receive full grant aid (i.e., do not pay tuition). 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.

Finally, we examine how our estimates compare to those from a design that is based exclusively on differences in lottery win sizes. Such a design requires the assumption that children from households with different win sizes have similar propensities to attend college, which does not seem to be the case in Figure 1 but may hold after controlling for observable household characteristics. Indeed, Table A5 indicates that the estimates from an across size design with a rich set of control variables are quite similar to those in Table 4.

c. Enrollment in Later Years and College Quality

Also of interest is whether resource shocks affect enrollment beyond the first year of college and intensive margin outcomes such as the type and quality of college attended. We first look to see whether the initial increase in enrollment is evident in later years, which, while we cannot examine it directly with tax data, would be consistent with lottery wins inducing students to not only enroll but also persist through college completion. Table 7 presents the effects of lottery wins on enrollment in each of the first four years after high school graduation as well as the cumulative change. For each successive year, children in the control group who could have been affected by a win are excluded (i.e., if their parents' win occurred before we observe that school year). For wins of less than $100,000, there is no evidence of higher enrollment rates in any year or in total years of college attendance. The effects of the largest wins are large and significant through the first three to four years after high school graduation. Thus, it is not the case that there is a temporary change in first year college enrollment that rapidly fades, although this result could be due to increased persistence in school or new enrollees at later ages. This generates an average increase in total years of college enrollment of 0.32 and 0.53 years of college for the largest two win ranges, respectively.

Turning to college composition, because wins of less than $100,000 do not result in changes in attendance on the extensive margin, any effects along the intensive margin for these win sizes have a straightforward interpretation. The extensive margin enrollment increases generated by larger wins complicate the interpretation of estimates at those levels, as potentially weaker marginal students have been added to the pool of college matriculates. With that said, Table A6 does not reveal a clear shift in the composition of colleges attended for wins of less than $100,000 in terms of sector (i.e., the lack of an effect on total enrollment is not masking offsetting changes in private and public college attendance rates). Likewise, when we examine college quality (measured by the later-life earnings of attendees), we find no significant changes. Thus, there is no evidence of extensive or meaningful intensive margin effects for wins of this size. The estimates indicate that the increases in enrollment for wins between $100,000 and $1,000,000 are concentrated at public colleges, while wins exceeding $1,000,000 increase enrollment at both private and public colleges.

d. Heterogeneity

Households of varying means might differ in their responsiveness to resource shocks, which could lend insight into the mechanisms that generate the main results, especially whether borrowing constraints are a prominent factor. Table 8 presents estimates after splitting the sample into households with annual incomes above and below the sample median (about $45,000) and with and without interest and dividends (a proxy for savings), all measured prior to the win. The estimates do not reveal statistically significant changes in enrollment in response to lottery wins of less than $100,000 for any of these subgroups, including the lower-SES households. On the other hand, the effects of larger wins is concentrated among households with *greater* prior resources.[25] (Table A8 decomposes the sample into income terciles and the lowest-income group is again the least responsive; see additional heterogeneity discussion in Appendix 2.)

We can jointly reject that the responsiveness between lower- and higher-SES households across lottery win sizes is the same. For clearer exposition, two variants of this test are included in Table A9. The first interacts a dummy variable for higher-income (or saving) households with the interaction of a continuous measure of lottery winnings in the segment of the win distribution where the effect appears to be linear (i.e., wins over $5,000,000 are excluded) and whether the win occurred prior to the year of high school graduation. The second interacts the above dummies with the interaction of whether or not the household experienced a win exceeding $100,000 and whether the win occurred prior to high school graduation. Both tests indicate that higher-SES households are more responsive.[26] Additionally, even when both income and savings are included in the

[25] Estimates for two-year college attendance also do not reveal larger effects for lower-SES households (see Table A7). That is, the null effects for two-year college attendance do not appear to be obscuring positive effects for households that are most constrained.

[26] The difference in responsiveness is much too large to be explained by the modest difference by SES in family size. On the other hand, these heterogeneity results are consistent with our examination of complier characteristics, which reveals they are more likely to come from higher income and positive savings households and from two-parent households (results available upon request). We estimate complier characteristics within the binary wins framework (assuming those with wins under 100k or wins over 100k but after the year of high school graduation are in the control group) and solve for their characteristics using the characteristics of the always-takers in the control group and the share of the treatment group attending college that is comprised of always-takers.

specification (column 3), neither coefficient is negative. Altogether, the main results are not driven by households that are the most likely to be financially constrained.

We next consider whether there is heterogeneity by the timing of the win. If parental inputs complementary to college-going are important (e.g., if there is dynamic complementarity or certain critical periods for investment), then earlier-timed resource shocks could produce larger effects. We consider a specification that includes a time trend interacted with an indicator for whether the win occurred prior to graduation (i.e., treatment) and each of two summary measures of wins: a continuous measure defined only over the linear region (as earlier) and an indicator variable for wins over $100,000 (Columns 1 and 2 of Table A10). In both cases the coefficient on the trend is statistically indistinguishable from zero, suggesting little difference in the size of the effect across the year of the win prior to high school graduation.

We also examine if there is evidence that a resource shock even earlier in childhood results in larger effects, as earlier wins may allow greater investment during a particularly formative time in a child's lifecycle. We explore this question by expanding the sample to include children from households that won the lottery up to 14 years prior to their year of expected high school graduation (as far as the data will allow). Again, both triple interaction terms are insignificant (Columns 3 and 4 of Table A10).

Finally, we combine an investigation of heterogeneity by SES and time and test whether there is any evidence that earlier wins generate relatively larger responses among lower-income households, which would be expected if borrowing constraints in early childhood were particularly inhibitive. We use both the baseline sample of up to six years prior to high school graduation and the expanded sample from the exercise above. The results, presented in the second panel of Table A10, reveal no evidence that earlier wins are more important for lower-income households.

e. Federal Financial Aid

The size of the estimates thus far may be blunted by the potentially important role that financial aid plays in college accessibility. In particular, if additional resources crowd out need-based assistance, we might not expect particularly large attendance responses to small-to-moderate lottery wins or among lower-SES households, which are eligible for the most aid. To examine this question, we exploit a unique feature of financial aid formulas, whereby the year in which the win occurs considerably influences the amount of aid for which a prospective student is eligible.

Before doing so, we first descriptively examine whether there are broad changes in financial aid within our design. (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.) Unsurprisingly, estimates reveal 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 (i.e., the EFC, which is a key metric used in determining aid eligibility) and reduce loan and grant amounts (Table A11).[27]

To investigate if crowd-out of aid is moderating attendance, 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).[28] These rates imply a substantially different loss of aid depending on whether or not the win occurred in the year before high school graduation, a critical year on which financial aid eligibility and generosity is based for the first year of college (the "FAFSA Year").[29] To exploit this feature, we re-estimate attendance and Pell Grant receipt (as a representative aid outcome), first excluding and then restricting attention to wins that occurred the year before a child graduates from high school. As shown in Table 9, when lottery wins that occur in the FAFSA Year are excluded, Pell Grant reductions are small, but the attendance estimates are unaffected. When attention is restricted to the FAFSA Year, the crowd-out of Pell Grants is large, but the attendance estimates are generally unchanged. The bottom panel replicates the exercise, restricting attention to below median income households. The results are even more striking: despite even greater reductions in grant aid for wins that occur in

[27] While additional forms of financial aid may be available to students, e.g., state or institutional grants, the majority is distributed through Federal programs (College Board, 2015). It is likely that institutional and state aid are crowded out by lottery wins as well, as they tend to use the same (or similar) eligibility formulas that the Federal government uses. Regression analyses using the restricted-access 2007-8 and 2011-2 NPSAS reveal a precisely-estimated negative relationship between expected family contribution and freshman year state and institutional aid: -0.016 (.002).

[28] 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 up to 46 and 5 percent, respectively.

[29] A simple comparison in a myopic model focusing on the first year of college, or a more sophisticated comparison over a full four year window of college attendance (properly discounted) reveal a difference in simulated crowd-out that is an order of magnitude larger or twice as large, respectively, if the win occurred in the year before high school graduation.

the FAFSA year among more financially constrained households, there is no change in estimated attendance effects.

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 results are presented in Table 10. The differential effect is small and statistically insignificant in all cases for the FAFSA Year, even when allowing for treatment heterogeneity in time (via the time trend interaction). Likewise, for lower-income households, there is no evidence that the attendance effects are attenuated by financial aid crowd-out, with and without controlling for a time trend in the treatment effect.[30] Altogether, these results imply that while a reduction in financial aid is a natural byproduct of winning the lottery, crowd-out is not altering children's attendance decisions on average and does not explain the heterogeneous responses observed across the population.[31]

f. Alternative Household Responses

Households may respond along other dimensions, including parents' earnings and labor supply, savings, homeownership, geographic mobility, and children's labor supply. In addition to estimating these alternate spending priorities, we differentiate the responses by household SES and conduct an accounting exercise to back out unobserved changes in consumption. For consistency and to contextualize the attendance results, these outcomes are examined within the same framework as attendance, focusing on the year a child graduates from high school.

The results presented in Table 11 reveal evidence of reduced earnings, and, for larger resource shocks, a reduction on the extensive margin of labor supply. Interestingly, though prior literature has suggested that self-employment increases after such shocks (e.g., Holtz-Eakin et al. 1994; Lindh and Ohlsson 1996), we find little effect on self-employment earnings. Perhaps the (implied) increase in leisure among lottery winners offsets the relaxation of financial frictions. Large wins also generate increases in our measure of savings (i.e., interest and dividends), but the effects of lottery wins for homeownership are more nuanced. For those without a mortgage prior to winning,

[30] To more flexibly allow for treatment heterogeneity by timing of the win, the same exercise is repeated for above median income households, who are less subject to financial aid crowd-out, and those estimates can be subtracted from the estimates for below median income households. Again, the estimates are not statistically significant.

[31] These results do not necessarily imply that college enrollment is unaffected by (anticipated) financial aid.

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, 2016), there is no evidence that these moves are to areas with greater upward mobility.

Having established average responses, we implement an accounting exercise to better understand differences in effects by SES and what they might imply about differences in the marginal propensity to consume. Specifically, we test for heterogeneity in the effects on earnings and savings, under the assumption that the residual of earnings and savings responses to lottery wins is a consumption response.[32] In addition to smaller increases in college-going, we find that lower-SES households have both a smaller earnings reduction and smaller savings increase, suggesting that such households consume a higher fraction of their lottery winnings (Table A12). Several caveats apply to 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 as expected given the stark differences we found by whether there was a mortgage in the pre-period, we find they are more likely to have acquired a mortgage and less likely to have paid one off. To address this issue, we estimate regressions where we include both differential effects of lottery wins by SES and differential effects of lottery wins by the presence of a mortgage to hold constant any differential effects due to the latter. The results, also in Table A12, continue to show smaller earnings reductions and savings increases. 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 3, 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 lottery winnings, which is consistent with findings in other literature on

[32] 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). Separately, we find mixed evidence on the effect on relative earnings, and if anything, a larger relative decrease for earnings among lower-SES households.

differential marginal propensities to consume by financial well-being (e.g., Keynes, 1936; Parker et al., 2013).

Finally, we examine labor market effects for the children themselves. Results for children's labor supply indicate that lottery wins are associated with reduced earnings, with the effects increasing in the size of the win (see Table A13). This is consistent with a higher fraction of children enrolling in college courses rather than being employed (though the effects could also reflect increased consumption of leisure). Interestingly, the reduction in labor supply primarily occurs on the intensive margin (working less) rather than on the extensive margin of not working at all.[33,34]

V. Potential Mechanisms

This section revisits the nature of the relationship between household resources and college attendance. We describe the two leading (non-mutually exclusive) mechanisms that might explain why additional resources influence attendance and whether our findings are consistent with each.[35]

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. In this case, the effect on college-going would likely be concave in the size of the resource shock, with even moderate increases leading to economically significant increases in attendance. But, the results are only weakly concave, and moderately sized shocks do not significantly affect college going rates. Further, we might expect the most financially constrained households to be the most responsive. Yet, low-earning and low-saving households seem, if anything, less responsive than other households, which held even when we attempted to account for both together (Table A9, column 3), and reductions in financial aid that might result from the lottery win and adversely affect

[33] 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.

[34] We also find some evidence that children whose parents won before they graduated from high school go on to have higher earnings in early adulthood (though the results are too imprecise to be conclusive). 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.

[35] 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).

constrained households the most do not appear to explain this difference. Finally, even when there is little enrollment response (both for smaller wins and among lower-SES households), parents appear to reduce their labor supply, which would be unusual if households were very financially constrained. In sum, a "financial frictions" explanation does not appear to fit the overall pattern of results.

Note that a variant of this mechanism—that households face financial constraints, but they bind earlier by limiting parental investment in children that is complementary with college-going (or require lead time for children to increase their preparation for college)—also appears to be largely inconsistent with our results.[36] For this explanation to hold, we would expect earlier lottery winnings to have larger effects, particularly among more constrained households. Yet, even 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.

Second, households may derive some consumption value from college, much as they do other normal goods. In addition to financial frictions being unable to explain our results, there are also particular predictions from this channel that are consistent with our findings. Namely, we would expect to see college attendance increasing in financial resources, with a high upper bound that is achieved at values that far exceed the cost of college, which is indeed what we find. Further, the largest responses should be concentrated among those who value college most. While theory is agnostic on this dimension, there is some evidence this applies to higher-SES households (e.g., Federal Reserve Board, 2017), which is also consistent with our accounting exercise (Table A12) that suggests lower-SES households derive relatively more marginal utility from (non-college) consumption.[37]

[36] We note three caveats: 1) we cannot test for very early life constraints, 2) while earlier wins may allow greater complementary investment in early childhood, which might be a particularly formative time, the additional resources from a lottery win may be more likely to be spent down by high school graduation, thus creating offsetting effects, and 3) one piece of evidence that could be consistent with constraints inhibiting high return investment complementary with college is the finding that lottery winner's move to modestly wealthier and more educated neighborhoods. However, 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 neighborhood, such neighborhood effects could explain only a fraction of the primary estimates).

[37] The larger response among higher-SES households could also be driven by potential complementarities between consumption value and prior ability and preparation (e.g., academic readiness).

VI. External Validity and Relation of Findings to Prior Literature

Thus far, we have provided strong evidence supporting the validity of our results within the context of our study. In this section, we discuss and address two natural concerns related to external validity – namely, the extent to which the study population resembles the overall population, and whether families treat lottery winnings similarly to resources from other sources. Then, we consider how our findings contrast with those from frontier studies in this area.

First, we evaluate the representativeness of the population of study. Survey data and tax records are used to compare lottery players with non-players and lottery winners with the tax-filing population. As detailed in Appendix 4, survey data indicate that up to 50 percent of the population plays the lottery and that lottery players are similar to non-players. For example, in the Consumer Expenditure Survey (CEX), lottery-playing families closely resemble non-playing families along demographic, earnings, and labor force dimensions (see Table A14). Relative to parents with lottery wins in the sample, average tax-filing parents who have children of the same age in the tax data have higher average earnings and are less likely to be married, though there is significant common support and these differences are small (see Table A15).[38] Children of lottery winners are less likely to attend college, though the difference again is small (about 4 p.p.) and they are similar in gender composition and citizenship. In sum, while we cannot fully rule out differences in behavior and preferences between winners and the general population, lottery players represent a substantial fraction of the population and lottery winners appear to be only modestly different in terms of observable characteristics, with substantial heterogeneity within the group. In addition, the pattern and magnitude of results are extremely similar when the sample of lottery-winning families is weighted to be representative of the tax-filing population (Table A2).

Second, we examine whether households appear to treat lottery wins differently than wealth from other sources and, as some anecdotes suggest, consume their winnings quickly. While this narrative deviates from standard economic models and we are aware of no systematic evidence of this phenomenon, we indirectly examine it by investigating the persistence of the effect of lottery winnings on parents' earnings. If this phenomenon were occurring, one might expect any earnings reduction (and presumed increase in leisure) to quickly dissipate as lottery wins are depleted.

[38] Note that lottery wins are censored by reporting requirement for amounts below an arbitrary reporting cutoff of $600. As we artificially increase the cutoff above that amount, the sample increasingly resembles the rest of the population. In earlier analysis, we show that the main results are not sensitive to the cutoff that is used.

Figure 4 presents an event-study framework relative to the timing of the win, which reveals a modest but persistent reduction in earnings in each year subsequent to the win among families with large wins. This pattern is inconsistent with the narrative that lottery income is spent particularly rapidly and is instead in line with a measured response, consistent with the predictions of a standard lifecycle model for ordinary wealth shocks.

It is also worth exploring how our estimates compare to those recovered in frontier studies within the literature. We have uncovered a modest per-dollar effect of lottery wins on college-going but one that is only weakly concave with a high upper bound. In general, these results are not consistent with many recent quasi-experimental studies that find that modest changes in household income (or wealth) have potentially large effects on human capital investment (e.g., Coelli, 2011; Lovenheim, 2011; Pan and Ost, 2014; Manoli and Turner, 2016; Bastian and Michelmore, 2016). Instead, our findings are most consistent with the negligible or modest effects estimated in Bleakley and Ferrie (2016), Cesarini et al. (2016), and Hilger (2016).[39] While Bleakley and Ferrie (2016) and Cesarini et al. (2016) focus on different outcomes and find little response when they examine an early 19th century Georgia land lottery and recent Swedish lotteries, respectively, their settings have fewer very large payouts that, in our context, are required to generate measurable effects.[40] Examining a context more similar to ours, Hilger (2016) finds that parental job loss only modestly reduces college-going, with smaller responses among lower income households, which he interprets as evidence that borrowing constraints are not binding and that households derive consumption value from college.[41] Our results are consistent with these conclusions as our per-dollar effect size for a pure resource shock is within the range of his implied magnitude (from the pecuniary effect of a job loss). But, within this setting, we are also able to document the degree of concavity of the response, that children from lower-SES households given additional resources are less responsive, that financial aid crowd-out does not appear to moderate responsiveness, and that lower-income households appear to have a greater marginal propensity to consume.

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[39] Cameron and Taber (2004) also find little evidence of borrowing constraints using quasi-experimental methods and structural estimation, and Carneiro and Heckman (2002) argue that the current policy environment results in a small fraction of households being constrained from attending college.

[40] The latter's main finding of no effect on children's intermediate development outcomes is also consistent with our finding that the effects are not driven by complementary parental inputs.

[41] Hilger (2016) notes that modest effects, particularly for low-income families, could stem from parents allocating a small share of marginal income to finance their children's college and offsetting effects of need-based financial aid.

VII. 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 United States, and to leverage an extremely 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 at most 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 income generates similar effects across years 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. Low-earning and low-saving households are not more responsive than wealthier households, and there is no evidence that financial aid dampens the response. 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 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 incent college investment and overcome any market failures stemming from financial frictions. However, to the extent that parents are not fully altruistic towards their children, our results are consistent with *children* facing binding borrowing constraints.[42] Second, redistribution of income towards lower-SES households is likely to be an inefficient method of closing enrollment gaps, unless the transfers are far larger than what could conceivably operate through the tax system. Policies seeking to raise educational attainment may need to turn to features that address other potential obstacles in the transition to college (e.g., information, college and career counseling, remedial

[42] This would be consistent with the college price effects literature that often finds large effects (Dynarski, 2003; Kane, 2003; Kane, 2007; Deming and Dynarski, 2010; Fack and Grenet, 2015; Castleman and Long, 2016; Denning, 2017). While we cannot test this directly, the finding that financial aid crowd-out does not influence enrollment suggests one of three possibilities: lower-SES parents do provide support to their children, which makes up for the deleterious effects of the removal of financial aid; these parents do not provide support but the removal of financial aid has little effect on college-going; or informational frictions about the operation of the financial aid system exist.

programs).[43] 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.

[43] Our results are also 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.

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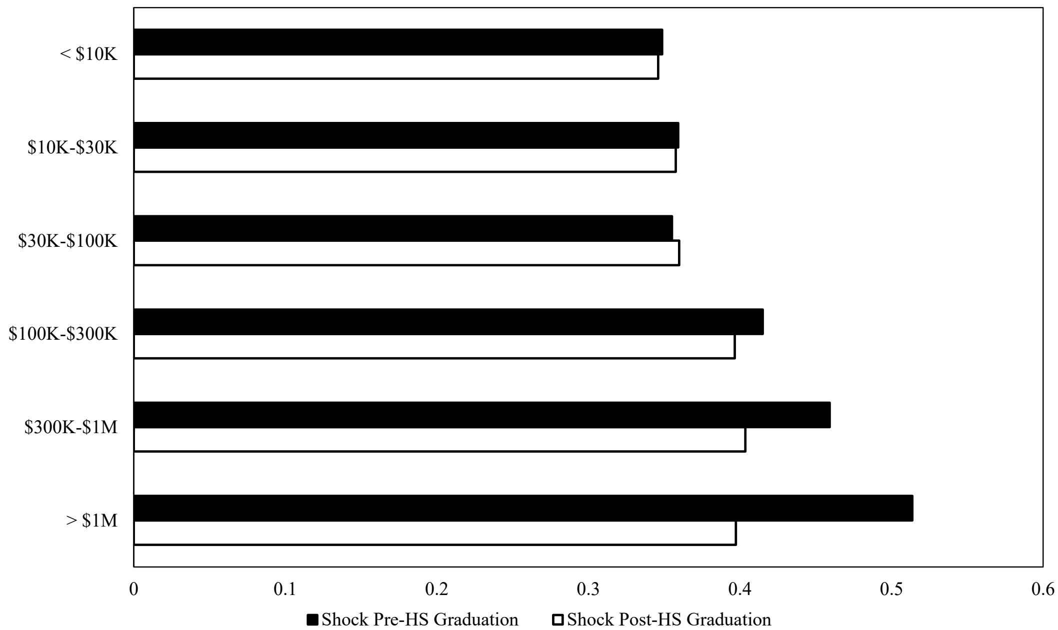
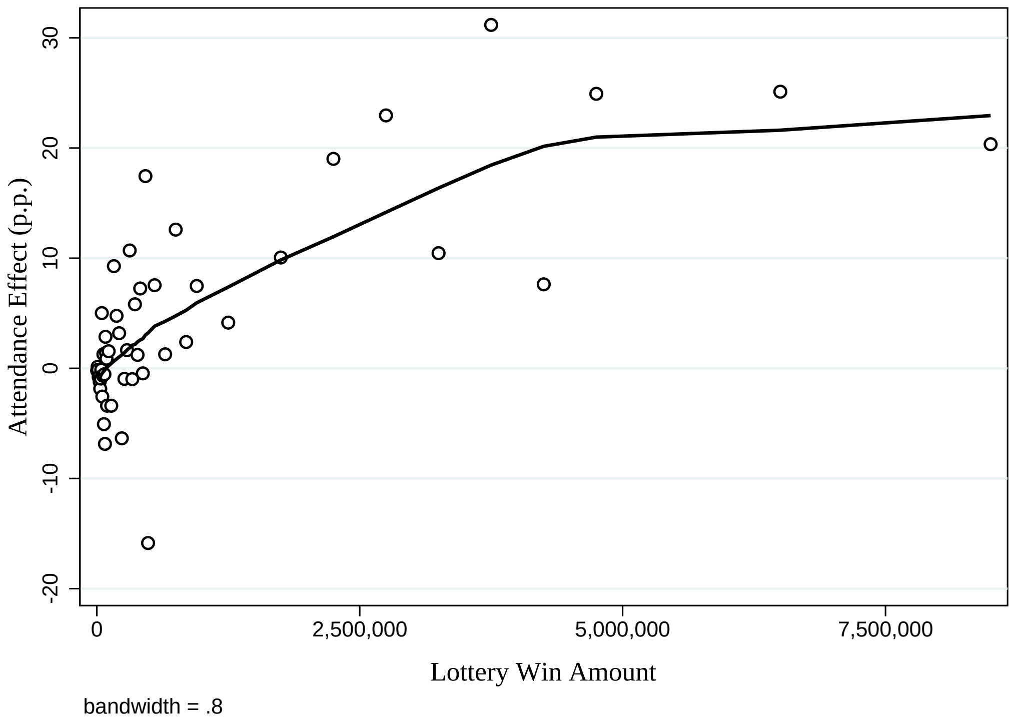
Figures and Tables

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 rates are adjusted for cohort fixed effects. Win sizes are adjusted to 2010 dollars and are classified according to six cutoffs: 10,000 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, and 1,000,000 dollars.

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.

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

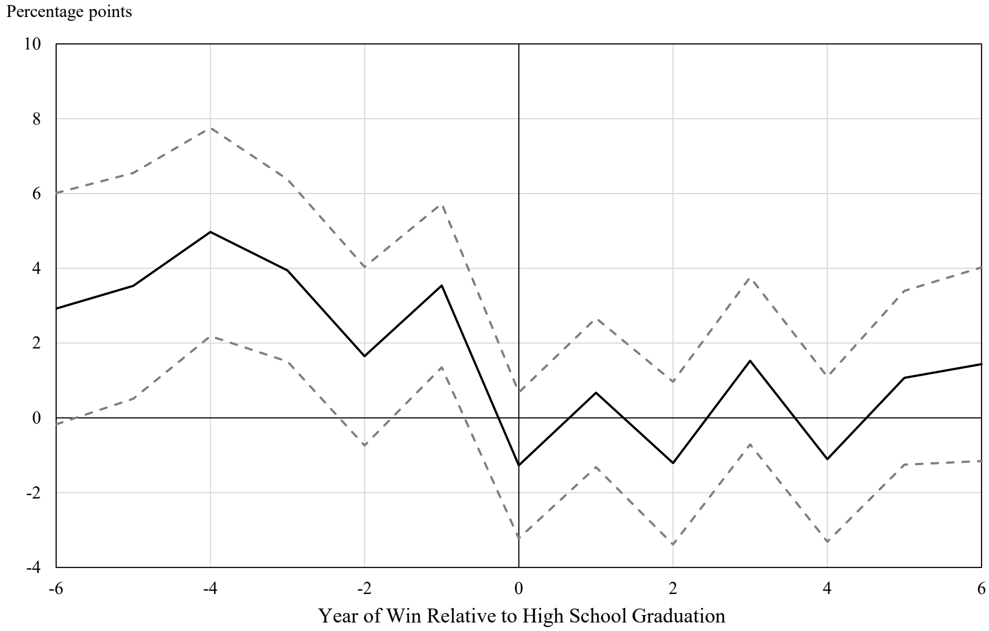
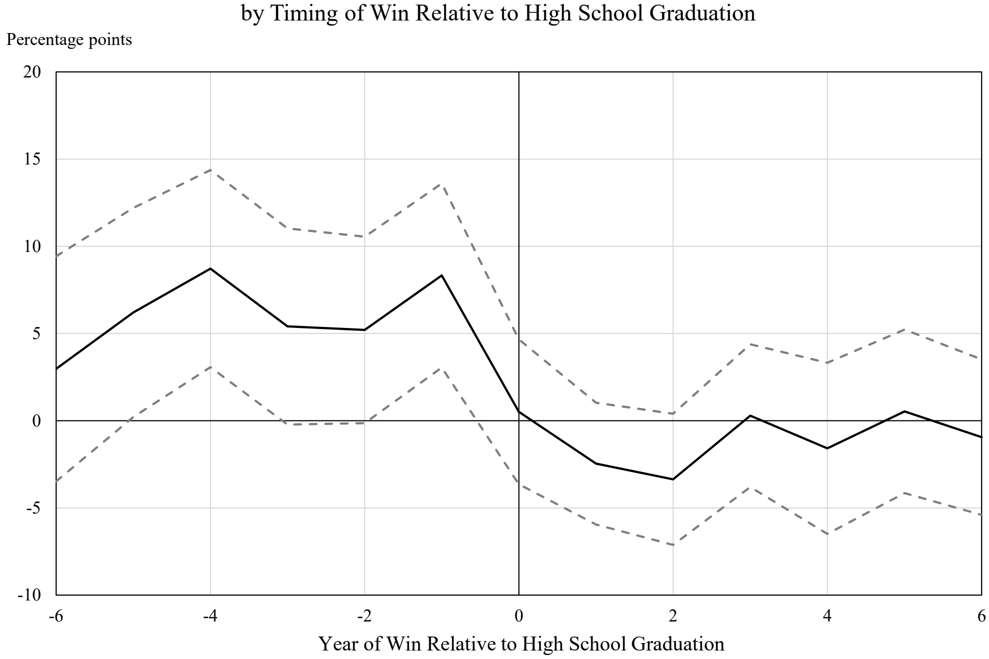
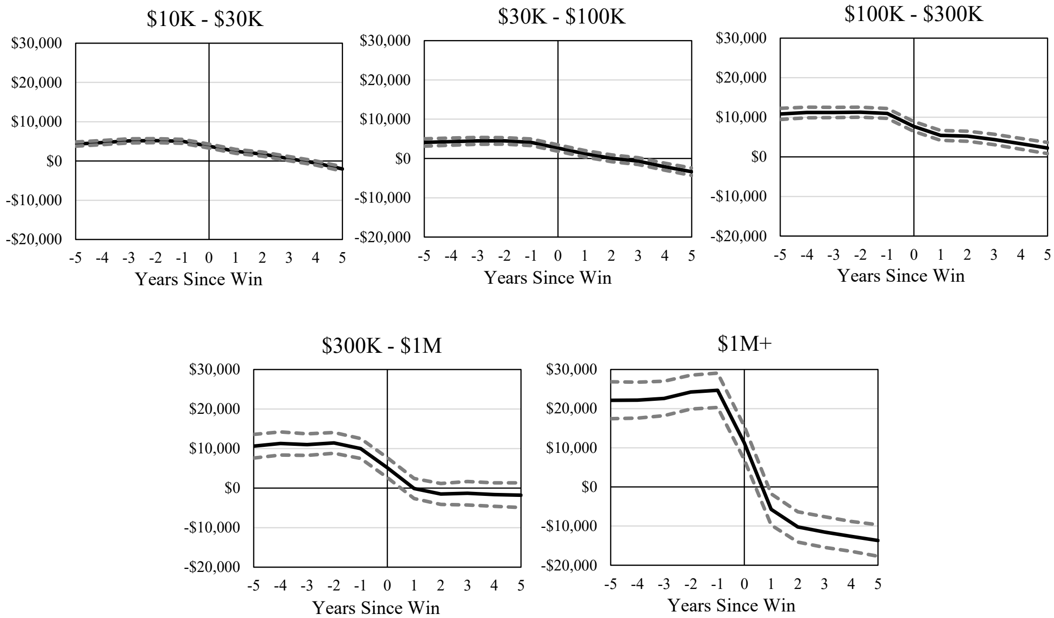


Figure 3b: Estimated Effects on Four-Year Attendance for Lottery Wins > $300,000
by Timing of Win Relative to High School Graduation



Note: This figure presents the percentage point change in four-year college attendance as a function of the timing of the win relative to the year of high school graduation. The graphed 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. All student and parent controls are based on pre-win measures. In the bottom panel, 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 dollars. Estimates are presented for each of five larger win groups, corresponding to cutoffs of 30,000 dollars, 100,000 dollars, 300,000 dollars, and 1,000,000 dollars or more. Year 0 is the year during which the win occurred and thus is likely to represent partial treatment.

Table 1: Summary Statistics: Lottery Wins and College Attendance

Win Size Distribution	Number Students	Median Win	Mean Win	Subsequent Win >10k
Income Shock 600 to 10,000	1,365,498	$1,189	$2,047	0.03
Income Shock 10,000 to 30,000	62,239	$11,900	$15,252	0.04
Income Shock 30,000 to 100,000	19,608	$50,000	$52,152	0.04
Income Shock 100,000 to 300,000	10,318	$153,421	$169,383	0.04
Income Shock 300,000 to 1,000,000	2,301	$525,000	$568,269	0.04
Income Shock 1,000,000 or more	1,298	$2,082,322	$7,704,497	0.03
Timing and Attendance	Mean	Std. Dev.		
Win Pre High School Graduation	0.44	0.50		
Attend Any Coll: Year of HS Grad	0.35	0.48		
Attend 4-Yr Coll: Year of HS Grad	0.22	0.41		
Attend 2-Yr Coll: Year of HS Grad	0.14	0.35		

Note: This table presents summary statistics for the lottery wins that affect each student, the fraction of students affected, and average college attendance rates. Column 1 of the top panel presents the number of students affected by wins in each of six size ranges: 600 to 9,999 dollars, 10,000 to 29,999 dollars, 30,000 to 99,999 dollars, 100,000 to 299,999 dollars, 300,000 to 999,999 dollars, and 1,000,000 dollars or more. Columns 2 and 3 present the median and mean of these wins. Column 4 presents the fraction of students whose parents experience total wins exceeding 10,000 dollars in the four years after the initial win. College attendance in the bottom panel is for the year of predicted high school graduation.

Table 2: 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)	
Num 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)	
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)	
Invest 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

Note: This table applies the empirical design to household characteristics as the dependent variable 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. Whether an individual is married is derived from filing status, number of children is derived from children ever claimed as a dependent, and income and investments are derived from the Form 1040. 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 3: Any College Attendance in Year of High School Graduation

	(1)	(2)	(3)
Win 10-30k Pre-HS Grad	-0.0022	-0.0032	-0.0033
	(0.0041)	(0.0039)	(0.0038)
Win 30-100k Pre-HS Grad	-0.0101	-0.0120*	-0.0100
	(0.0073)	(0.0069)	(0.0067)
Win 100-300k Pre-HS Grad	0.0169*	0.0113	0.0109
	(0.0101)	(0.0095)	(0.0093)
Win 300k-1.0m Pre-HS Grad	0.0559***	0.0518**	0.0590***
	(0.0217)	(0.0207)	(0.0200)
Win 1.0m or more Pre-HS Grad	0.1039***	0.0984***	0.0928***
	(0.0279)	(0.0272)	(0.0267)
Child and Family Controls			X
Parental Controls		X	X
State by Year and Cohort	X	X	X
Observations	1,461,262	1,461,262	1,461,262
Mean Dep	.341	.341	.341

Note: Estimates show the percentage point effect of income shocks on attending any college in the year of 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. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: 10,000 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 4: Four-Year College Attendance in Year of High School Graduation

	(1)	(2)	(3)
Win 10-30k Pre-HS Grad	-0.0016	-0.0024	-0.0025
	(0.0035)	(0.0034)	(0.0034)
Win 30-100k Pre-HS Grad	-0.0075	-0.0089	-0.0075
	(0.0062)	(0.0060)	(0.0059)
Win 100-300k Pre-HS Grad	0.0188**	0.0147*	0.0143*
	(0.0091)	(0.0087)	(0.0086)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0514***	0.0566***
	(0.0195)	(0.0189)	(0.0185)
Win 1.0m or more Pre-HS Grad	0.1184***	0.1138***	0.1097***
	(0.0257)	(0.0247)	(0.0246)
Child and Family Controls			X
Parental Controls		X	X
State by Year and Cohort	X	X	X
Observations	1,461,262	1,461,262	1,461,262
Mean Dep	.215	.215	.215

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of 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. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: 10,000 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 5: Two-Year College Attendance in Year of High School Graduation

	(1)	(2)	(3)
Win 10-30k Pre-HS Grad	-0.0023	-0.0025	-0.0026
	(0.0030)	(0.0029)	(0.0029)
Win 30-100k Pre-HS Grad	-0.0043	-0.0049	-0.0042
	(0.0054)	(0.0053)	(0.0053)
Win 100-300k Pre-HS Grad	-0.0025	-0.0043	-0.0044
	(0.0074)	(0.0073)	(0.0072)
Win 300k-1.0m Pre-HS Grad	0.0076	0.0057	0.0082
	(0.0160)	(0.0159)	(0.0159)
Win 1.0m or more Pre-HS Grad	-0.0005	-0.0016	-0.0036
	(0.0227)	(0.0229)	(0.0226)
Child and Family Controls			X
Parental Controls		X	X
State by Year and Cohort	X	X	X
Observations	1,461,262	1,461,262	1,461,262
Mean Dep	.139	.139	.139

Note: Estimates show the percentage point effect of income shocks on two-year college enrollment in the year of 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. All student and parent controls are based on pre-win measures. Win sizes are classified according to six cutoffs: 10,000 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. 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: 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. 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, 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. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 7: Persistence of Four-Year College Attendance Effects

	(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	-0.0033	-0.0024	-0.0027	-0.0031	-0.0369
	(0.0035)	(0.0039)	(0.0041)	(0.0047)	(0.0054)	(0.0240)
Win 30-100k Pre-HS Grad	-0.0075	0.0003	0.0021	0.0071	0.0015	-0.0116
	(0.0062)	(0.0068)	(0.0072)	(0.0082)	(0.0095)	(0.0419)
Win 100-300k Pre-HS Grad	0.0188**	0.0231**	0.0314***	0.0248**	0.0207	0.1072*
	(0.0091)	(0.0099)	(0.0105)	(0.0118)	(0.0134)	(0.0593)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0365*	0.0327	0.0507**	0.0495*	0.3219***
	(0.0195)	(0.0204)	(0.0218)	(0.0244)	(0.0281)	(0.1221)
Win 1.0m or more Pre-HS Grad	0.1184***	0.1127***	0.1388***	0.1458***	0.1014***	0.5294***
	(0.0257)	(0.0290)	(0.0307)	(0.0341)	(0.0381)	(0.1716)
Observations	1,461,262	1,292,594	1,135,772	916,781	710,403	710,403
Mean Dep	.215	.243	.239	.239	.221	1.17

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment in the years after high school graduation and in total. 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 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. 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 Household Resources

	(1) Lower Income	(2) Higher Income	(3) No Invest Income	(4) Invest Income
Win 10-30k Pre-HS Grad	0.0030	-0.0059	-0.0039	0.0000
	(0.0043)	(0.0053)	(0.0041)	(0.0054)
Win 30-100k Pre-HS Grad	-0.0088	-0.0115	-0.0122*	-0.0042
	(0.0077)	(0.0093)	(0.0072)	(0.0097)
Win 100-300k Pre-HS Grad	-0.0111	0.0319**	-0.0043	0.0329**
	(0.0116)	(0.0125)	(0.0116)	(0.0129)
Win 300k-1.0m Pre-HS Grad	0.0282	0.0631**	0.0536**	0.0473*
	(0.0259)	(0.0264)	(0.0263)	(0.0272)
Win 1.0m or more Pre-HS Grad	0.0370	0.1387***	0.0704**	0.1379***
	(0.0372)	(0.0318)	(0.0355)	(0.0334)
Observations	730,632	730,630	749,071	712,191
Mean Dep	.133	.292	.134	.298

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 from households with above and below median income and those with and without investment income (as measured by interest and dividend income). 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 9: College Attendance and Federal Aid: Critical FAFSA Year

All Households	(1)	(2)	(3)	(4)
	Non-FAFSA Year		FAFSA Year	
	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)
Observations	1,317,523	1,317,523	961,290	961,290
Mean Dep	.215	$1,577.16	.215	$1,577.16
Low Income Households	(5)	(6)	(7)	(8)
	Non-FAFSA Year		FAFSA Year	
	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)
Observations	657,385	657,385	469,214	469,214
Mean Dep	.133	$3,531.71	.133	$3,531.71

Note: Estimates show changes in the rate of four-year college attendance and receiving federal grants for all households (top panel) and households with below median income (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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table 10: Four-Year College Attendance: Critical FAFSA Year With Time Trends

	(1)	(2)	(3)	(4)
All Households	Without Time Trend		With Time Trend	
FAFSA Year * Win Amt ($100k)	0.0015		0.0019	
	(0.0027)		(0.0037)	
FAFSA Year * Win > $100k		0.0034		0.0187
		(0.0139)		(0.0188)
Observations	1,460,890	1,461,262	1,460,890	1,461,262
Mean Dep	.215	.215	.215	.215

	(5)	(6)	(7)	(8)
Low Income Households	Without Time Trend		With Time Trend	
FAFSA Year * Win Amt ($100k)	0.0014		0.0034	
	(0.0036)		(0.0046)	
FAFSA Year * Win > $100k		0.0124		0.0265
		(0.0184)		(0.0241)
Observations	730,526	730,628	730,526	730,628
Mean Dep	.133	.133	.133	.133

Note: Estimates show changes in the rate of four-year college attendance for all households (top panel) and households with below median income (bottom panel). 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) and with an indicator for the win exceeding $100,000. 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 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)
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
Mean Dep	$51,281.80	.824	$2,086.84	$428.92	.028	.910	$51,427.58	.287	.006

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 differentiate 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Appendix

Appendix 1: Robustness to Alternative Designs, Samples, and Outcomes

This section examines the robustness of the estimates to alternate windows of the lottery win relative to high school graduation, methods of determining parent-child matches, adjustment of lottery wins for taxes, and definition of win size ranges for treatment and control groups. Results are also presented for an alternate method of measuring college attendance, employing an across win size design, and differentiating by college sector and quality.

Table A1 presents estimates using lottery wins that occur in varying bandwidths around a student's predicted year of high school graduation. This exercise has two primary benefits. First, we examine 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. We find this is not the case. Additionally, by restricting attention to households whose parents won a lottery close to the year of their child's graduation, it deals with the concern about 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 more similar in terms of unobservables than households that win when their children are 16 and 20. We get similar results with smaller bandwidths, suggesting this is not an issue.

Table A2 considers an array of alternative sample and specification choices. The first column 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. However, the estimates have a similar pattern to those generated when matches are based on claimed children, with little or no effect for wins by birth parents of less than $100,000, and effects exceeding 10 percentage points for wins of $1,000,000 or more. The predicted after-tax value of each lottery win is estimated by taking each household's pre-win tax return, adding the lottery win, and computing the estimated reduction in the win due to taxes. This simulated tax liability approach avoids the issue of endogenous labor supply responses to earnings. The resulting win amounts are mechanically smaller and thus some wins are now classified in a smaller win range. As a result, the effects for each win range are slightly larger than for the pre-tax estimates. as shown in column 2.

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, 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. Thus columns 3 through 5 of Table A2 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 3 and 4, resulting in larger win households as the control group. Conversely, we use only the smallest wins of less than \$1,000 as controls in column 5. 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 percentage points respectively.

Some students may graduate from high school a year later than is predicted by their date and state of birth. This can occur if, for example, a student starts a year late by choice, is held back during their schooling, or moves to a state with a later entry age prior to starting school. Thus, we consider an alternative, more broadly defined measure of children's transitions to college that includes attendance in either the calendar year of high school graduation or the following year. This will, however, also capture students who graduate as expected and enroll one year later. The resulting estimates on four-year college attendance in column 6 are similar to those based on calendar year, with somewhat larger effects for the largest lottery wins.

In some cases it is not possible to determine with certainty the year or size of an individual's first lottery win and thus assumptions must be made in order for the winner to be included. These individuals 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 7 of Table A2. 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. Second, the sample includes cases in which a win is paid out over multiple years (which constitute a couple 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, the year of some lottery wins may not be accurately reported, which we identify 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 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 effect for moderate wins, and effects of 2, 7, and 10 percentage points for the largest lottery win ranges. Columns 8 includes wins that occurred in the year of a child's high school graduation, for which it is not clear if a win is too late to have an effect. This results in slightly attenuated estimates, which is consistent with misclassifying treatment status.

Column 9 presents estimates while including winner fixed effects. This approach does not exploit variation across larger and smaller wins. Instead, the variation stems solely 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. As a result, the average child in the sample comes from a household with 2.8 children graduating during the period of interest rather than 1.9 children for the full sample. Thus the resulting estimates are based on a sample that is only 26 percent of the size of the full sample and does not exploit any of the variation in lottery win size. The standard errors from this approach are substantially larger. Finally, column 10 weights the sample of lottery winners to reflect the characteristics of the population of households with children of college-going age. Because lottery winners are generally similar to the population, this results in no meaningful change in the estimates.

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 A3 presents estimates for 10 win ranges relative to the smallest wins (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, which is consistent with the lack of concavity discussed in Section III.

As detailed in the text, enrollment estimates based on the Form 1098T could be biased upward by income shocks due to the possibility that some colleges may not submit forms for students receiving full grant aid. This would be most likely to generate upward bias for students from lower income households who receive the most grant aid. Table A4 presents two approaches for addressing this concern. (Note that we diverge from the rest of the robustness exercises and present results for any attendance instead of four-year attendance – in this exercise 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.) Columns 2 sets attendance to 0 for all students attending colleges that seem most likely to not be filing 1098Ts for students receiving full scholarships. These colleges are identified as having close to 0 percent of students with 1098Ts that show grants equaling total tuition billed. This approach will necessarily attenuate estimates since all attendance, including causal increases, can no longer contribute to the estimated response. Nonetheless, the estimates reveal effects that have a similar pattern to those for the full sample, and are scaled downward in a way that is proportional to the fraction of attendance omitted from the analysis. Column 3 omits these students from the sample and generates similar results. As an alternative to these approaches, 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 by the Form 1098T. Thus we construct a new measure where a student is classified as enrolled if they have a Form 1098T or are observed receiving federal grant aid. Column 4 presents the resulting estimates, which reveal a similar pattern of college enrollment effects.

As detailed in Section III, a design based on comparisons across lottery win sizes requires the assumption that there are no unobservable differences that affect the outcome of interest. Because only children who graduate from high school after the lottery win are treated by the income shock, attention is restricted to these children. Thus the sample is approximately half of the size of the full sample, as shown in Table A5. This across size design controls for state-by-year fixed effects, cohort effects, and a rich set of parent, child, and household characteristics. The resulting estimates of the effect of income on four-year attendance are discussed in Section III and are quite similar to those in the primary specification.

Appendix 2: Heterogeneity by Household Financial Status and Financial Aid Outcomes

This section supplements the heterogeneity estimates by household earnings and savings presented in Table 8. Table A7 presents analogous estimates for two-year college attendance. They reveal that the null average effect for two-year attendance does not obscure larger effects for children with the least access to household financial resources. Table A8 divides households into the bottom, middle, and top third of earnings among lottery winners. This division isolates the effects for the lowest earning households who are most likely to be eligible for need-based grants and aid. Table A9 presents the result of a formal test of the difference in income effects for lower and higher income households. The interaction of having above median household income or above median savings with the size of the lottery win prior to graduation is statistically significant when wins are measured either as a continuous variable or an indicator for the win exceeding $100,000.

Table A10 presents a formal test for whether earlier or later wins have larger effects on college enrollment outcomes. Using a sample restricted to wins within 6 years of a student's high school graduation (columns 1 and 2) and wins extending as far back as 14 years prior to graduation (columns 3 and 4) does not reveal evidence of increasing or decreasing effects. The bottom panel tests whether earlier income shocks are disproportionately more important for households with below median income and reveals no evidence that this is the case.

The modest effects for lower-income households highlights the need for a close examination of the role of financial aid offset. A brief discussion of this issue is presented in Section IV. Estimates of the effect of lottery wins on financial aid must be interpreted carefully. Specifically, the estimated effect of lottery wins may reflect changes in enrollment and FAFSA filing on the extensive margin and may reflect changes in the composition of students who attend. Column 1 of Table A11 presents estimates of FAFSA filing. This reveals that rates of filing decreased for the largest wins despite higher rates of college attendance. Conditional on filing, the expected family contribution increases for each win size. Conditional on attendance, winning households tend to receive lower levels of grant aid, take out smaller levels of subsidized loans, and the winners of large lotteries take out lower levels of unsubsidized loans.

Appendix 3: Household Propensity to Consume

This section considers how lottery wins affect household labor supply and consumption. Table A12 examines if lower- and higher-income households experience differential changes in labor market earnings and investment. 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.

There are several potential concerns with attempting to identify differences in the marginal propensity to consume across 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 (results available upon request). Second, our measure does not include alternative investments (e.g., real estate, business investment, retirement accounts, paying off non-mortgage debt) that could be more prevalent among lower-SES winners (e.g., if such households previously faced borrowing constraints in these markets), though these investments could easily be more prevalent among higher-SES households as well. 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 (results available upon request), and find no evidence of larger increases in Schedule C or F income or IRA or 401-k contributions.

Finally, while we cannot observe overall debt or the paying off of old debt (or taking on new debt) in the data, we note that lower-SES households in the United States have, on average, less mortgage and student loan debt, have less credit card debt, and slightly lower credit card balances on delinquent accounts (e.g., https://www.valuepenguin.com/average-credit-card-debt), though interest rates on credit card debt may be higher for such households. An alternative approach to understanding if unobserved 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. Overall, the available evidence indicates that lower-SES households consume a higher fraction of lottery winnings.

Table A13 presents the effect of household lottery income on children's labor market outcomes in the year that they graduate from high school and at age 27. The estimates reveal statistically significant reductions in earnings after high school on the extensive margin for each of the three largest win ranges. At age 27, earnings estimates are positive and reasonably large in magnitude, but are too imprecisely estimated to be statistically significant. The lack of precision is due in large part to the limited number of treated children observed at this age. We note that interpreting these estimates as the causal effect of greater education is potentially problematic due to the fact that there could be other mechanisms by which the timing of parents' lottery wins could affect children's labor market outcomes.

Appendix 4: Characteristics of Lottery Players and Lottery Winners

To investigate whether families that play the lottery differ from the general population, we analyze microdata from 10 years (Quarter 1 of 2005 to Quarter 4 of 2014) of Consumer Expenditure Surveys (CEX). Within the CEX, we find that approximately 1 out of every 4 families report purchasing lottery tickets and those that do spend an average of roughly $250 on tickets. Families that purchase tickets do not differ substantially from families that do not along several demographic, income, and labor force dimensions (Table A14). Among those we consider, the only dimension along which they noticeably differ is income, with playing families generally earning more income than non-playing families. These differences, however, are not large. Across a wide range of other characteristics – including race, highest education, family size, and age of children – we do not find large differences between families that do and do not play the lottery.

It has been noted that the CEX likely underreports lottery playing (Kearney 2005), and higher levels of lottery playing are found in other surveys. For example, estimates from the 1998 NORC National Survey on Gambling indicate that 51 percent of adults report playing the lottery in the past year (Kearney 2005). Additionally, a Gallup Poll on gambling conducted in 1999 reported that 57 percent of adults buy at least one lottery ticket each year. Finally, recent data from a California Lottery Commission survey (2015) indicate that 64 percent of Californians play the lottery each year, which implies that it is not only the states that are otherwise known for their high gambling rates and casino presence (e.g., Nevada, Louisiana, New Jersey) that drive national participation estimates.

In addition to comparing players and non-players, we examine the characteristics of winners and non-winners using tax data. We select a random sample of non-winners with children of the same age 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. The resulting summary statistics are presented in Table A15 and are discussed in Section VI.

Table A1: Four-Year College 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)
Observations	1,461,262	1,289,589	1,087,709	857,883	598,923	312,407
Mean Dep	.215	.215	.214	.214	.214	.213

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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A2: Four-Year College Attendance: Alternate Samples and Robustness Checks

	(1) Soc. Sec. Parent	(2) Tax Adjusted	(3) Control 1,000-10,000	(4) Control 5,000-10,000	(5) Control 600-1,000
Win 10-30k Pre-HS Grad	-0.0051	-0.0085*	-0.0019	-0.0029	-0.0017
	(0.0047)	(0.0046)	(0.0036)	(0.0042)	(0.0040)
Win 30-100k Pre-HS Grad	-0.0041	0.0051	-0.0074	-0.0050	-0.0087
	(0.0082)	(0.0067)	(0.0062)	(0.0066)	(0.0064)
Win 100-300k Pre-HS Grad	0.0265**	0.0233**	0.0176*	0.0154*	0.0188**
	(0.0120)	(0.0117)	(0.0091)	(0.0094)	(0.0092)
Win 300k-1.0m Pre-HS Grad	0.0711***	0.0622***	0.0547***	0.0565***	0.0507***
	(0.0260)	(0.0220)	(0.0195)	(0.0196)	(0.0195)
Win 1.0m or more Pre-HS Grad	0.1036***	0.1584***	0.1180***	0.1200***	0.1195***
	(0.0329)	(0.0336)	(0.0258)	(0.0258)	(0.0257)
Observations	914,841	1,461,262	1,138,097	222,840	416,035
Mean Dep	.254	.215	.212	.217	.224
	(6) Attend Within Two Years	(7) No Sample Restrictions	(8) Include Grad Yr	(9) Household Fixed Effect	(10) Population Weighted
Win 10-30k Pre-HS Grad	-0.0021	0.0014	-0.0024	-0.0098	-0.0011
	(0.0038)	(0.0032)	(0.0033)	(0.0095)	(0.0037)
Win 30-100k Pre-HS Grad	-0.0016	-0.0040	-0.0076	-0.0240	-0.0069
	(0.0067)	(0.0058)	(0.0058)	(0.0178)	(0.0066)
Win 100-300k Pre-HS Grad	0.0209**	0.0225***	0.0097	0.0064	0.0194**
	(0.0096)	(0.0085)	(0.0084)	(0.0243)	(0.0098)
Win 300k-1.0m Pre-HS Grad	0.0555***	0.0670***	0.0415**	0.0514	0.0533***
	(0.0204)	(0.0174)	(0.0179)	(0.0552)	(0.0210)
Win 1.0m or more Pre-HS Grad	0.1373***	0.0983***	0.1067***	0.0893	0.1186***
	(0.0274)	(0.0230)	(0.0240)	(0.0695)	(0.0257)
Observations	1,461,262	1,691,357	1,617,679	377,252	1,461,262
Mean Dep	.268	.216	.216	.215	.216

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 presents: (1) a sample that includes only individuals who are linked to the parent winner through social security birth records; (2) estimates after adjusting lottery wins for federal income taxes; (3) to (5) which use alternative control groups in the following ranges: 1,000 to 10,000 dollars, 5,000 to 10,000 dollars, and 600-1,000 dollars. The columns in the bottom present: (6) attendance in the calendar year of high school graduation or the year after; (7) 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); (8) inclusion of wins that occur in a student's graduation year; (9) lottery winner fixed effects; and (10) weighting the sample to represent the population. 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. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A3: Four-Year College Attendance: Narrower Win Ranges For Income Shocks

	(1) No Controls	(2) With Controls
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)
Observations	1,461,262	1,461,262
Mean Dep	.219	.219

Note: Estimates show the percentage point effect of income shocks on college enrollment in the year of high school graduation. 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 nine cutoffs: 1,000 dollars, 3,000 dollars, 10,000 dollars, 20,000 dollars, 30,000 dollars, 50,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and 3,000,000 dollars or more. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A4: Any College Attendance: Alternative Measures

	(1) Baseline	(2) Set to Zero	(3) Set to Missing	(4) Form 1098T or Fed Grant 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)
Observations	1,461,262	1,461,262	1,308,674	1,461,262
Mean Dep	.341	.269	.300	.355

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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A5: Across Win Size Design: Four-Year College Attendance

	(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
Observations	643,711	643,711	643,711
Mean Dep	.223	.223	.223

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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A6: Four-Year Attendance by College Type

	(1) Private	(2) Public	(3) For-Profit	(4) Avg Earn
Win 10-30k Pre-HS Grad	0.0031	-0.0058**	0.0011*	-$155.14
	(0.0022)	(0.0030)	(0.0007)	(211.77)
Win 30-100k Pre-HS Grad	-0.0060*	0.0004	-0.0018	-$438.39
	(0.0036)	(0.0053)	(0.0012)	(374.05)
Win 100-300k Pre-HS Grad	0.0029	0.0170**	-0.0011	$683.31
	(0.0056)	(0.0078)	(0.0016)	(526.37)
Win 300k-1.0m Pre-HS Grad	0.0108	0.0333**	0.0098**	$2,798.71**
	(0.0119)	(0.0167)	(0.0044)	(1,119.81)
Win 1.0m or more Pre-HS Grad	0.0487***	0.0657***	0.0039	$5,055.47***
	(0.0176)	(0.0223)	(0.0057)	(1,527.29)
Observations	1,461,262	1,461,262	1,461,262	1,461,262
Mean Dep	.072	.137	.006	$17,893.18

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment by sector and average earnings. 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A7: Two-Year College Attendance: Heterogeneity by Household Resources

	(1) Lower Income	(2) Higher Income	(3) No Invest Income	(4) Invest Income
Win 10-30k Pre-HS Grad	-0.0017	-0.0026	0.0004	-0.0049
	(0.0038)	(0.0044)	(0.0038)	(0.0044)
Win 30-100k Pre-HS Grad	-0.0027	-0.0078	-0.0055	-0.0020
	(0.0070)	(0.0080)	(0.0069)	(0.0082)
Win 100-300k Pre-HS Grad	-0.0001	-0.0057	0.0054	-0.0098
	(0.0102)	(0.0101)	(0.0099)	(0.0105)
Win 300k-1.0m Pre-HS Grad	0.0163	0.0020	0.0045	0.0111
	(0.0225)	(0.0218)	(0.0227)	(0.0222)
Win 1.0m or more Pre-HS Grad	0.0233	-0.0150	0.0042	-0.0048
	(0.0396)	(0.0275)	(0.0372)	(0.0285)
Observations	730,632	730,630	749,071	712,191
Mean Dep	.098	.178	.104	.176

Note: Estimates show the percentage point effect of income shocks on two-year college enrollment in the year after high school graduation. The results are presented for students from households with above and below median income and those with and without investment income (as measured by interest and dividends). 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A8: Four-Year College Attendance: Heterogeneity by Household Resources

	(1) Income Tercile1	(2) Income Tercile2	(3) Income Tercile3
Win 10-30k Pre-HS Grad	0.0000	0.0035	-0.0079
	(0.0051)	(0.0056)	(0.0067)
Win 30-100k Pre-HS Grad	-0.0138	-0.0034	-0.0117
	(0.0091)	(0.0097)	(0.0120)
Win 100-300k Pre-HS Grad	0.0020	-0.0138	0.0467***
	(0.0142)	(0.0144)	(0.0152)
Win 300k-1.0m Pre-HS Grad	0.0347	0.0671**	0.0425
	(0.0314)	(0.0338)	(0.0312)
Win 1.0m or more Pre-HS Grad	0.0520	0.1050**	0.1495***
	(0.0466)	(0.0438)	(0.0369)
Observations	487,088	487,088	487,086
Mean Dep	.121	.178	.336

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 from households in three income terciles (where Tercile 1 is the lowest and Tercile 3 is the highest). 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A9: Four-Year College Attendance: Heterogeneity Tests

Linear Specification	(1)	(2)	(3)
Above Med Inc * Win Amt ($100k) Pre-HS Grad	0.0065***		0.0052**
	(0.0024)		(0.0026)
Has Invest Inc * Win Amt ($100k) Pre-HS Grad		0.0049*	0.0031
		(0.0025)	(0.0027)
Observations	1,460,890	1,460,890	1,460,890
Mean Dep	.215	.215	.215
Binary Specification	(1)	(2)	(3)
Above Med Inc * Win > $100k Pre-HS Grad	0.0526***		0.0433***
	(0.0148)		(0.0160)
Has Invest Inc * Win > $100k Pre-HS Grad		0.0393***	0.0208
		(0.0150)	(0.0163)
Observations	1,461,262	1,461,262	1,461,262
Mean Dep	.215	.215	.215

Note: This table presents the interaction of household resources with the size of the lottery win. The top panel uses a continuous measure of win amount while the bottom panel uses a binary measure for wins exceeding 100,000 dollars. Main effects are not shown. Estimates show the percentage point effect of income shocks on four-year college enrollment in the year of high school graduation. 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. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A10: Four-Year College Attendance: Heterogeneity by Timing

	(1)	(2)	(3)	(4)
Trend Test	Within 6 Years		All Years	
Years Pre Grad * Win Amt ($100k) Pre-HS Grad	0.0001		-0.0001	
	(0.0007)		(0.0005)	
Years Pre Grad * Win > $100k Pre-HS Grad		0.0026		0.0002
		(0.0046)		(0.0033)
Observations	1,460,890	1,461,262	1,902,983	1,903,457
Mean Dep	.215	.215	.215	.215

	(5)	(6)	(7)	(8)
Trend Test by Household Income	Within 6 Years		All Years	
Above Med Inc * Years Pre Grad * Win Amt ($100k) Pre-HS Grad	-0.0008		-0.0010	
	(0.0014)		(0.0009)	
Above Med Inc * Years Pre Grad * Win > $100k Pre-HS Grad		-0.0069		-0.0093
		(0.0088)		(0.0062)
Observations	1,460,890	1,461,262	1,902,983	1,903,457
Mean Dep	.215	.215	.215	.215

Note: This table presents a test of whether the effect of a lottery win varies with when the win occurred 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) and with an indicator for the win exceeding $100,000. Columns 1 and 2 present the estimates for wins that occur in the six years prior to graduation and columns 3 and 4 present estimates for all lottery wins, extending as far back as 14 years. 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 A11: Federal Financial Aid: FAFSA Filing, EFC, Pell Grants, and Loan Amounts

	(1) FAFSA Application	(2) Expected Fam. Contr.	(3) Pell Grants	(4) Subsidized Loans	(5) Unsubsidized Loans
Win 10-30k Pre-HS Grad	-0.0059	$242.0450	-$89.44***	-$15.87	-$111.59
	(0.0039)	(184.7606)	(34.12)	(31.59)	(105.94)
Win 30-100k Pre-HS Grad	-0.0201***	$1,620.0784***	-$147.52**	-$118.34**	$4.52
	(0.0068)	(352.0651)	(62.18)	(55.62)	(187.50)
Win 100-300k Pre-HS Grad	-0.0083	$3,860.7219***	-$316.13***	-$305.71***	$371.30
	(0.0095)	(582.6894)	(71.70)	(70.21)	(249.64)
Win 300k-1.0m Pre-HS Grad	-0.0106	$4,109.3422**	$54.83	-$204.98	-$632.42
	(0.0204)	(1,614.9986)	(139.00)	(140.96)	(554.41)
Win 1.0m or more Pre-HS Grad	-0.1326***	$2,607.4711	-$603.06***	-$1,003.23***	-$1,909.92***
	(0.0245)	(2,373.4101)	(181.60)	(166.47)	(719.99)
Observations	1,461,262	466,280	319,341	319,341	319,341
Mean Dep	.294	$7,380.04	$1,163.24	$1,550.34	$2,985.71

Note: Estimates show changes in the rate of FAFSA filing, expected family contribution (EFC), Pell Grants, and loan aid received. Expected family contributions are conditional on filing a FAFSA, and grants and loans received 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 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A12: Earnings and Savings: Heterogeneity Tests of Implied Marginal Propensity to Consume

	(1)	(2)	(3)	(4)
Linear Specification	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)
Observations	1,460,890	1,460,890	1,460,890	1,460,890
Mean Dep	$51,275.28	$51,275.28	$428.51	$428.51
	(5)	(6)	(7)	(8)
Binary Specification	Earnings		Investment Income	
Above Med Inc * Win > $100k Pre-HS Grad	-$3,428.19**	-3,638.65***	$1,236.58***	1,090.82***
	(1,359.63)	(1,398.30)	(428.90)	(410.05)
Home Owner * Win > $100k Pre-HS Grad		459.59		358.73
		(1,434.45)		(407.67)
Observations	1,461,262	1,461,262	1,461,262	1,461,262
Mean Dep	$51,281.81	$51,281.81	$428.93	$428.93

Note: This table presents a test of whether the effect of a lottery wins on earnings and investment varies with household income. The top panel uses a continuous measure of win amount while the bottom panel uses a binary measure for wins exceeding 100,000 dollars. Results are presented with and without including an interaction for home ownership. Main effects are not shown. 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 A13: Children's Labor Supply and Earnings Responses to Income Shocks

	(1) Earnings Yr HS Grad	(2) Any Work Yr HS Grad	(3) Earnings Age 27
Win 10-30k Pre-HS Grad	-$78.20*	-0.0035	-$218.12
	(41.77)	(0.0037)	(295.95)
Win 30-100k Pre-HS Grad	$38.28	-0.0013	$195.47
	(82.35)	(0.0066)	(478.52)
Win 100-300k Pre-HS Grad	-$203.63**	0.0003	$1,128.69
	(95.59)	(0.0088)	(694.78)
Win 300k-1.0m Pre-HS Grad	-$503.04**	-0.0223	$2,815.30*
	(199.84)	(0.0195)	(1,613.11)
Win 1.0m or more Pre-HS Grad	-$1,284.37***	-0.0880***	$1,935.31
	(318.76)	(0.0268)	(1,809.84)
Observations	1,461,262	1,461,262	816,342
Mean Dep	$4,082.21	.741	$20,932.83

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). The specifications include state-by-year of win fixed effects and cohort fixed effects. Win sizes are classified according to six cutoffs: 10,000 dollars, 30,000 dollars, 100,000 dollars, 300,000 dollars, 1,000,000 dollars, and exceeding 1,000,000 dollars. Errors are clustered at the winner level. The symbols *, **, and *** represent statistical significance at 10, 5, and 1 percent respectively.

Table A14: 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
Sample Size	11,308	34,958

Note: This table presents summary statistics for those who report playing or not playing the lottery in the prior year. The analysis is based on the Consumer Expenditure Survey for Quarter 1 of 2005 to Quarter 4 of 2014.

Table A15: Comparison of Lottery Winners and Parents With Same-Aged Children

	Population	Lottery Winners
Parent Married	0.62	0.57
Parent Wage	59,325	51,790
Parent AGI	75,280	60,466
Number of Children	3.233	3.454
Child Male	0.51	0.51
Child Citizen	0.95	0.96
Attend Any Coll: Year of HS Grad	0.39	0.35
Attend 4-Yr Coll: Year of HS Grad	0.25	0.22
Attend 2-Yr Coll: Year of HS Grad	0.15	0.14

Note: This table presents summary statistics for parents and children who experience an income shock due to lottery winnings and for a random sample 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. Whether an individual is married and income is derived from filing status, number of children is derived from children ever claimed as a dependent, and college attendance comes from the 1098-T.