Parental Resources and College Attendance: Evidence from Lottery Wins*

George Bulman
UC Santa Cruz

Robert Fairlie
UC Santa Cruz, NBER

Sarena Goodman
Federal Reserve Board of Governors

Adam Isen
Office of Tax Analysis, U.S. Department of the Treasury

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Abstract

We examine more than one million children whose parents won a state lottery to trace out the effect of additional household resources on college outcomes. The analysis draws on the universe of federal tax records linked to federal financial aid records and leverages substantial variation in the size and timing of wins. The results reveal modest, increasing, and only weakly concave effects of resources: wins less than $100,000 have little influence on college-going (i.e., effects greater than 0.3 percentage point can be ruled out) while very large wins that exceed the cost of college imply a high upper bound (e.g., wins over $1,000,000 increase attendance by 10 percentage points). The effects are smaller among low-SES households. Further, while lottery wins reduce financial aid, attendance patterns are not moderated by this crowd-out. Overall, the results suggest that households derive consumption value from college and, in the current policy environment, do not generally face binding borrowing constraints.

* Email: gbulman@ucsc.edu, rfairlie@ucsc.edu, sarena.f.goodman@frb.gov, and adam.isen@treasury.gov. We thank Nate Hilger and seminar participants at the 2016 NBER Summer Institute Education Meeting, the All California Labor Economics Conference 2016, American University, Census Bureau, George Mason University, Urban Institute, and the University of California at Santa Cruz for helpful comments and suggestions. The views expressed herein are those of the authors and do not necessarily reflect the views of the Federal Reserve Board of Governors, National Bureau of Economic Research, or U.S. Department of the Treasury. All errors are our own.

I. Introduction

There is a strong correlation between household resources and college-going. Today, children from above median income households are approximately twice as likely to attend college as children from below median income households. Given the critical role that higher education plays in the labor market, there is marked interest in the nature of this relationship and which, if any, causal factors govern it. It may be that: households face financial frictions—such as borrowing constraints—that restrict college access for children who would otherwise earn high returns from education; resources spur investment in children that is complementary with schooling (or alter preferences for schooling); or, households derive consumption value from higher education.

As resources and schooling decisions are also correlated with ability levels, preferences, and other aspects of the household that are difficult to observe, a primary challenge in the literature has been overcoming the identification concern that household incomes are not exogenously determined. Many recent studies have developed creative methods to attempt to isolate changes in resources that are not due to existing household conditions in a range of contexts. The estimates in this literature have varied greatly in magnitude, resulting in uncertainty around several fundamental questions regarding the nature of this relationship.[1,2] First, to what extent is this relationship causal: how much does, on average—and can, at maximum—increasing resources affect college-going? Second, is attendance more sensitive to income for some household types than for others? And, finally, how does increasing resources interact with the current policy environment, which subsidizes college attendance for particular income groups?[3]

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[1] The wide range of estimates in the literature could reflect the responsiveness of the populations the studies examine, the levels of resource change, and perhaps other aspects of the research designs. These studies exploit income and wealth differences generated by, for example, job loss (Coelli, 2011; Pan and Ost, 2014; Hilger, 2016), tax credits (Manoli and Turner, 2014; Bastian and Michelmore, 2015), union status (Shea, 2002), casino revenue (Akee and Copeland, 2010), historical land grant lotteries (Bleakley and Ferrie, 2016), oil revenue (Loken, 2010), and housing prices (Lovenheim, 2011). Prior studies have exploited cross-sectional differences and trends in income over time (e.g. Acemoglu and Pischke, 2001; Blanden and Gregg, 2004). Related studies examine the effect of income and job loss on various components of cognitive development, health, and employment (e.g. Blau, 1999; Maurin, 2002; Bratberg, Nilsen, and Vaage, 2008; and 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). In contrast to most studies, Cesarini et al. (2016) and Bleakley and Ferrie (2016) find no causal link of household resources on human capital outcomes while Hilger (2016) finds modest effects of job loss on college attendance.

[2] For a related literature examining borrowing constraints in the context of college outcomes, see, for example, Keane and Wolpin (2001), Carneiro and Heckman (2002), Cameron and Taber (2004), Brown, Scholz, and Seshadri (2011), and Lochner and Monge-Naranjo (2011).

[3] While policymakers might be particularly interested in whether financial constraints, in light of existing financial aid and educational tax credits policies, still hinder many households from investing in human capital, no quasi-

This study offers a setting in which to examine changes in college-going among children of varying ages and backgrounds, whose households receive a very wide range of shocks to their resources (from as little as $600 to more than $1,000,000), and thus generate answers to these questions. Specifically, we estimate the effects of unearned household income in late childhood from more than one million state lottery wins between 2000 and 2013. The analysis leverages the unique breadth and detail of the universe of federal tax records linked to the universe of federal financial aid records for college students. The information in these two data sources enable us to overcome several data challenges, including: observing individuals' lottery wins, matching these individuals to their children, measuring children's college attendance and financial aid receipt, and placing responses in the context of pre- and post-win household conditions.[4] Our primary empirical specification exploits two sources of variation. Following the approach taken in the lottery literature of comparing across win size, we compare children from households that experience larger wins to children exposed to smaller wins.[5] Then, to account for potentially unobserved differences between these groups in our setting (e.g., Oster, 2004), we leverage variation in the timing of the lottery win. Specifically, attention is restricted to time-sensitive outcomes (e.g., college attendance in the same year as high school graduation), whereby children whose parents won a same-sized lottery too late to affect the outcomes (in the absence of perfect foresight) become a second comparison group. Lottery wins in this design are balanced across a rich set of household characteristics measured prior to the lottery win.[6]

This framework reveals several interesting results. The estimated response is only weakly concave in the win amount. Specifically, for wins of less than $100,000, the estimated effect on college attendance is not statistically significant and rules out even modest effects (i.e., an effect greater than 0.3 percentage point). For larger wins, including those that vastly exceed the typical

experimental study we are aware of has been able to explicitly account for how these policies interact with the income effects that have been estimated, further complicating interpretation.

[4] Gallup Polls indicate that approximately 50 percent of adults in the United States participate in state lotteries each year. Tabulations from the Consumer Expenditure Survey, which has been noted to underreport lottery playing (Kearney, 2005), suggest that about 25 percent of households play the lottery each year. Analyses of both Consumer Expenditure Survey and other data suggest that characteristics of lottery players and non-lottery players do not differ substantially.

[5] See Imbens, Rubin, and Sacerdote (2001), Hankins, Hoekstra, and Skiba (2011), and Cesarini et al. (2015, 2016) for a few examples of previous studies using variation in win size.

[6] Specifically, conditional on the win amount, children affected by wins that occur before and after they complete high school come from households that are similar in terms characteristics such as wages, adjusted gross income, investment income, marital status, number of children, and citizenship.

cost of attending a four-year college, there are meaningful effects on attendance, with the degree of responsiveness increasing in win size and an implied upper bound that is quite large. For example, there are increases in attendance of approximately 5 percentage points for wins between $300,000 and $1,000,000, and 10 percentage points for wins exceeding $1,000,000.[7] These results are robust to a number of alternative specifications, including continuous win (rather than binned) specifications, alternative bin sizes, and a design that exploits the exact timing of the lottery win relative to high school graduation.[8]

To more fully examine whether binding financial constraints can explain our findings, we investigate if there is heterogeneity in responsiveness across household financial well-being and whether crowd-out of financial aid reducing responsiveness, especially for smaller wins and among lower-income households. The analysis reveals little evidence to support these explanations. Effects on college outcomes are, if anything, smaller among households with lower income or less savings to draw from. Still, if lottery winnings reduce eligibility for need-based financial assistance that would otherwise be available—e.g., college grants that do not need to be repaid— attendance response might be muted. To examine this concern explicitly, we develop a test that exploits the timing of the lottery win and the differential treatment of income and assets in standard financial aid formulas, and find no evidence that reduction in aid due to winning the lottery is moderating the attendance effects, even among lower income households.

To shed light on competing uses of new income, we apply the same empirical framework to additional dimensions of parental response, including earnings and labor supply, savings, and geographic mobility. The estimates reveal clear evidence that parents decrease labor supply and increase housing consumption and savings, and that very large winners move to modestly wealthier neighborhoods. Interestingly, for those without a mortgage prior to winning, there is an increase in having a mortgage *at every win level*, with the size of the effect growing in the amount of the win and on the order of 25 percentage points for very large wins. For those with a mortgage already, it appears that households use large winnings to pay it off and own their homes outright.

[7] We also find some evidence that very large resource shocks cause students to acquire more years of education, persist in college, and to attend private colleges..

[8] Children who graduate from high school in any of the six years after their parents win more than $300,000 exhibit significant changes in whether they attend college in the year of graduation, while children who graduate in the six years before their parents win do not. Notably, income shocks in the year of high school graduation appear to be too late to change college enrollment decisions within that year.

Altogether, the results are most consistent with households deriving consumption value from college, rather than a large fraction of households facing financial constraints that are overcome by an influx of income. There is also little evidence to support intermediate parental investments driving the observed changes in college-going. Namely, responses are modest, increasing, and only weakly concave, with a high upper bound, and the magnitude of the response is generally insensitive to timing. The households that are most likely to face meaningful constraints are less responsive, and the interaction of new income from a lottery win with financial aid eligibility does not appear to attenuate the effects of wins on average, for small wins, or for lower-income households.

This study makes several important contributions. It is the first to use data on the universe of lottery winners in the United States, and to exploit variation among U.S. lottery winners to examine the effect of household resources on post-secondary outcomes.[9] Relative to prior work, a research design using lotteries is well-suited to estimate income effects: the size of the income shock is salient to the household and is readily observed by the researcher, and the income from the win does not load onto other treatments that confound the interpretation. The resulting estimates advance the literature on post-secondary enrollment and household resources by providing a rich picture of the magnitude of resource shocks needed to generate significant changes in college outcomes as well as establishing the concavity and upper bound of such effects. We generate a range of estimates within one context—including across households of different means and at different points in time—and, for the first time to our knowledge, explicitly take into account corresponding changes in financial aid. As a result, the study sheds new light on the mechanisms that underlie the relationship between household resources and college-going. Finally, we are able to offer context for the estimates by studying alternative margins of response, including labor supply, housing consumption, and savings.

The paper is organized as follows. Section II introduces the administrative data used for analysis. Section III details the empirical strategy and the underlying assumptions. Section IV describes the results. Section V investigates the mechanisms most consistent with the results. Section VI concludes.

[9] To our knowledge, our study is the first to use the universe of lottery winnings from tax records to study income effects on *any* outcome.

II. Sample Construction

The sample is formed from confidential administrative records from two sources. First, we use the universe of federal tax records for the United States population. The analysis focuses on more than one million individuals who graduated from high school between 1999 and 2012 and had a parent with state-reported lottery winnings. We then link these individuals to their federal financial aid records. Throughout, all dollar amounts are adjusted for inflation and denoted in real 2010 dollars.

To construct the sample, we first identify any individual with state lottery income as reported on the third-party Form W-2G for the purposes of tax withholding and income declaration. This form is first available in 1999. All prizes in excess of $600 must be reported and indicate the state and year of the lottery. The first calendar year that an individual is observed with a win is designated as his "win year", which will be used to classify household treatment.[10] The main analysis sample excludes individuals that appear to have ``other'' income in prior years that matches lottery win payouts (which could suggest that they were collecting the win prior to it being reported by the state), that collect lottery wins paid out over multiple years, or that have multiple wins in the first year, as, in each case, identifying the timing and amount of the initial win requires additional assumptions. We show that the results are robust to the inclusion of these individuals.

Then, to identify "households," lottery winners are linked to their tax filings beginning in 1996 (the first year such data is available), including the dependents they claimed on their Form 1040. This match technique offers an advantage over most previous lottery studies, which rely on matching between win amount data from lottery agencies and outcome data from various sources and therefore likely entail more measurement error. Parent-child matches are only included in our sample if the child is claimed prior to the first lottery win and prior to graduating from high school (defined below).[11] This linkage will include birth parents, step parents, and adoptive parents who

[10] 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 high school will also have won before their child graduated high school, thus contaminating the timing aspect of the control group. A very small fraction of households, 3-4 percent, have wins exceeding $10,000 in the four years after the initial win. Thus, subsequent wins affect too small a fraction of the population to significantly alter the estimates. Likewise, the probability that a household experienced a large win prior to the first observable year, 1999, is quite small. To verify this, results are presented using only later lottery wins (that occurred to households we can verify did not have any lottery winnings going back to 1999) and the estimates are similar.

[11] Children claimed after the win could be endogenous to the extent that tax filing is influenced by lottery winnings and children claimed after high school will be selected on whether or not they are attending college.

are financially responsible for a child and whose income is likely to be considered for the purposes of financial aid by colleges and federal grants. (We also present results linking birth parents and children using records from the Social Security Administration, though these records are first available for children born in 1983 and thus results in a smaller sample.) The resulting population includes more than one million children linked to parents who experience income shocks.

Children's college outcomes are constructed from several sources. Attendance is based on the Form 1098-T, a mandatory reporting form filed by post-secondary institutions for informational purposes on behalf of their enrollees to assist them in claiming educational tax credits. The Form 1098-T is also available beginning in 1999 and can be linked to college characteristics maintained by the National Center for Education Statistics Integrated Postsecondary Education Data System, making it possible to ascertain if students attended two- or four-year colleges, whether the college is public, private, or for-profit, and the college's Carnegie Classification (a classification system used to designate undergraduate programs as more selective, selective, or inclusive). Important for the research design, the data reveal not only if children attend college but also when, since the goal is to capture immediate transitions to college. Thus, the expected high school graduation cohort for each child is determined using exact birthdates (available via social security card applications), the state in which the child is born, and the corresponding school entry age laws for that state.[12] Children whose parents won the lottery before they were due to graduate from high school could have been affected by the win, and children whose parents won the lottery later could not have been and act as controls.[13] As shown in Table 1, 44 percent of the children in the sample had parents who won early enough to potentially be affected. Because the 1098-T is filed by calendar

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[12] The outcome is measured for all children and not for high school graduates only (where graduating may be endogenous to household resources and college plans). Also note that the estimates will be attenuated by students who graduate in a year other than the predicted year. For example, a student who is held back will not attend college in the expected year. Likewise, a student who moves to a state with an earlier school entry age may appear to be treated pre-college when he or she is not. Because each of these forms of measurement error is potentially endogenous to household resources, the predicted year of graduation represents the exogenous measure of treatment. To capture students who may graduate a year late (which is the most likely alternative to graduating as predicted), we consider an alternative definition of attendance that includes attendance in the predicted year or one year later.

[13] In the preferred specification, children whose parents won in the year that they are expected to graduate are excluded, as it is unclear if, and to what extent, they can be affected. An increase in income late in the year is certainly too late to change a student's decision about whether or not to enroll in a four year college for that year. Further, even if the increase occurs earlier in the year, it may 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 (e.g., taking a job or enlisting in the military). The effects of wins that occur in the year of high school graduation are considered explicitly in the robustness section.

(tax) year and not by academic year, only children with 1098-T's for their predicted high school graduation year are treated as transitioning to college immediately. Approximately 35 percent of children in the sample are observed attending college in the year that they graduate from high school and approximately 22 percent attend a four-year college.[14]

Schools that participate in Title IV programs (e.g., Pell Grant, subsidized and unsubsidized Direct 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 the children in the sample. This information contains an array of federal financial aid outcomes, which, similar to the attendance measures, include a temporal dimension (e.g., total Pell Grant received within the academic year after high school graduation). Several outcomes of interest are constructed: application for financial aid (i.e., filing a FAFSA), amount of subsidized loans, amount of unsubsidized loans, Pell Grant receipt (both a binary variable indicating receipt of a Pell Grant and a continuous measure for the amount received), and expected family contribution.

Finally, we draw on a number of child, parent, and household characteristics to use as controls, to test for balance in the research design, and to examine heterogeneity. Many of these variables (e.g., wages, adjusted gross income, filing status, number of dependents, marital status) are reported on the Form 1040 and other tax forms, while demographic information (e.g., gender, citizenship) for both the lottery-winning parent and the child comes from social security records. Characteristics that come from tax forms are based on pre-win years, as post-win characteristics may be endogenous to the win.[15] To examine heterogeneity, the sample is split by adjusted gross income, the presence of savings (proxied by interest and dividend income), and homeownership (proxied by the presence of an interest-bearing mortgage). Table 2 indicates that the average household in the sample has wages of about $52,000 dollars and 57 percent file as married. The homeownership rate is about 56 percent, and a little less than half of the households report interest or dividend income.

[14] Note that these shares are not directly comparable to those tabulated from the Current Population Survey and published by the Department of Education. Those survey statistics represent the share of recent high school completers (not all children) enrolled in college in October.

[15] The 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. For continuous variables we use the average, and for binary variables, whether the value is ever 1 (i.e., the max).

We can compare characteristics of our sample to those of other populations. Overall, lottery players (or winners) are not that different than non-players (or non-winners) in terms of observable characteristics. Relative to tax-filing parents without lottery wins who have children of the same age, parents with lottery wins have lower average income, and are less likely to be married, though there is significant common support and these differences are quite small (see Table A1a of the appendix).[16] Children of lottery winners are less likely to attend college, but, similarly, the difference is a modest 3 percentage points. Appendix 1 describes lottery players using survey data, which indicate that up to 50% of the population play the lottery and that lottery players are not very different than non-players. In the Consumer Expenditure Survey, lottery-playing families closely resemble non-playing families along demographic, income, and labor force dimensions, save for having slightly higher income. While differences in behavior between winners and non-winners cannot be ruled out, lottery players represent a substantial fraction of the population and any differences between them are likely to be modest. Later in the paper, the pattern and magnitude of results hold when, using the characteristics from Table A1a, the sample of lottery winning families is weighted to be representative of the population.

III. Empirical Strategy

a. Lottery Wins as Identifying Variation

As noted in the introduction, because of the strong correlation between household resources and other factors that influence college-going, a growing number of studies attempt to estimate causal effects by identifying a source of variation in resources that is likely to be exogenous to college outcomes. Generally speaking, the primary challenge in this literature has been deriving estimates that are both credible and externally valid. Income and wealth are rarely fully exogenously determined: if extraneous factors that might affect educational outcomes help generate a study's identifying variation, additional treatments load onto the estimates and confound the interpretation. In addition, effects identified for one socio-demographic group may not be applicable to others, and the responsiveness of any one group may vary with the size of the shock. Moreover, other relevant household labor and consumption decisions usually cannot be observed

[16] Note that lottery wins are censored 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 later analysis, we show that our main results are not sensitive to the cutoff we choose.

and the landscape of college attendance, cost, and financial aid is constantly changing, altogether making it difficult to contextualize how additional resources affect college attendance and to generate coherent policy prescriptions.

Given these challenges, this setting offers many desirable characteristics for evaluating the effects of household resources on college outcomes. First, lottery wins vary significantly in size—from $600 dollars to more than $1,000,000—and affect a large segment of households that are diverse in terms of income, size, and the propensity of their children to pursue a college education. Thus, the analysis generates a range of estimates: both those that can be interpreted quite generally, as well as others that are local in terms of the magnitude of the shock and the type of households affected. This range makes it possible to explore the degree of concavity of the response and to identify the upper bound of possible effects. Likewise, the diversity of household types allow us to identify which households are most sensitive to changes in income, which sheds light on the role of borrowing constraints.

Second, the timing of when households experience the income shock varies. Thus, we can implement a research design that does not solely rely on comparisons across households with different lottery win sizes, which may differ in terms of unobservable characteristics. Variation in timing also makes it possible to examine whether the year in which new policies seeking to improve college affordability (e.g., student aid offers, education tax credits) become salient to households is relevant and should be taken into account in their design.[17] Third, the income shock is salient to the household and easily measured by the researcher in terms of size and duration, so there is no need to make strong assumptions about household expectations. Fourth, lottery wins are not preceded by other phenomena, such as changes in household structure, employment status, or earned income, and so they generally do not load on other treatments that would complicate the interpretation of estimates. Fifth, because student-level financial aid outcomes and the timing of the win are observed, the analysis can estimate the effects on financial aid receipt and examine whether crowd-out of aid could explain instances where college-going is not responsive to additional income. Finally, income shocks may alternatively influence other household behaviors – e.g., labor force and savings (Imbens, Rubin, Sacerdote, 2001; Holtz-Eakin, Joulfaian, Rosen, 1993) – which can be readily examined within this framework.

[17] Of course, there may be a difference between an unanticipated change in income in the year of high school graduation and a subsidy that is expected well in advance.

b. Research Design

Our empirical framework exploits comparisons across win sizes and in the timing of the win in a difference-in-difference design. Comparisons drawn across win sizes alone are common in the lottery literature, but require an assumption that winners that experience different-sized payouts are identical – or that the choice of controls is sufficient to account for all unobservable differences and render winners with different-sized payouts essentially identical – which may not hold for several reasons.[18] For state lotteries, variation in payouts can stem not only from random lottery variation but also from changes in prize pools over time due to "rollover" when there is no winner and from participation in different variants of the lottery, which have different potential payouts. However, the complexity and number of lotteries across states makes it effectively impossible to obtain individual lottery participation to isolate within-lottery randomization (and 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 purchased from a range of stores and are often paid for in cash). Thus, while it is possible to exploit variation in win sizes across individuals who participate in lotteries in the same year and in the same state, it may be the case that these individuals differ in unobservable ways (e.g., it is possible that individuals who participate in lotteries with larger potential payouts have children with higher propensities to attend college after conditioning on income and other observable characteristics). Prior literature has documented differences in the types of households that play particular lotteries (Oster, 2004) and we also note differences in characteristics across households with large and small wins.

Thus, relying on a second difference, over timing, enables us to abstract from this assumption. To generate variation in timing, we first infer from the tax data the year of each child's (expected) high school graduation relative to the lottery win. The analysis is then restricted to attendance outcomes that occur within particular time frames relative to graduation, primarily focusing on "immediate" transitions to college. Children whose parents win the lottery after they graduate from high school form a natural counterfactual. Logically, without perfect foresight, children whose

[18] Studies examining labor supply, health, and cognitive development have taken various approaches for controlling for unobserved differences among lottery winners. For examples, see Imbens, Rubin, and Sacerdote (2001), Lindahl (2005), Apouey and Clark (2010), Hankins, Hoekstra, and Skiba (2011), Powdthavee and Oswald (2014), Cesarini et al. (2015, 2016). The focus on lottery winners is necessary in these studies because of the lack of information on non-winners in household survey or lottery agency data. Cesarini (2015, 2016) provide a rare exception using Swedish lotteries, where they can observe non-winners in some cases or otherwise still explicitly account for the probability a household experiences a win.

parents win the lottery after they graduate have no scope to adjust their enrollment paths in response. To take a specific example, if the outcome of interest is "four-year college attendance in the year of graduation," children whose parents win the lottery the year after they graduate would be unable to retroactively begin school one year earlier. Intuitively, the design compares the outcomes of students whose parents experience larger or smaller wins and accounts for unobserved differences in the propensity to attend college across these households using older children who could not be affected. We compare the results from this difference-in-difference design to those from the more traditional across win size design.

Because households experience a very wide range of wins (from $600 to tens of millions of dollars), how wins are parameterized can greatly influence the estimates. For example, imposing a linear functional form is inherently problematic, as the effect of each dollar will necessarily decrease at some level, and such a specification will place the most weight on the largest wins.[19] This issue is addressed in two ways. First, in our baseline specification, wins are categorized into bins, which allow their estimated effects to vary flexibly without imposing a strong functional form assumption. Specifically, wins are classified according to six cutoffs: $10,000, $30,000, $100,000, $300,000, $1,000,000, and exceeding $1,000,000. The results are not sensitive to the specific choice of cutoff though using very narrow win ranges for large lottery wins results in too few observations to generate precise estimates. As shown in Table 1, more than one million children in the sample experience "small" parental wins, averaging about $2,000. Approximately 100,000 children experience parental wins of $10,000 or more, and about 15,000 experience wins exceeding $100,000. Conveniently, the number of individuals affected by wins of varying sizes is inversely related to the expected effect size, which means that we recover the most precise estimates for the smallest income shocks, which are likely to generate the smallest effects. Thus, the design is likely to have ample statistical power to examine the effects of large and small income shocks. Second, in an alternative design, we determine the range over which a linear specification is reasonable (i.e., over which there is no evidence of concavity) and conduct the analysis within this range.

[19] Issues with imposing linearity on the effects of income have been noted by Loken, Mogstad, and Wiswall (2012). We show that imposing a linear specification will result in estimates that are necessarily sensitive to the range of win sizes considered. We separately look at heterogeneity in the effects by the level of prior household income.

Before turning to the estimating equation, Figure 1 offers a graphical depiction of raw pre- and post-win attendance averages, unadjusted for cohort effects.[20] The raw averages reveal sizable differences in attendance for large wins due to timing, with the differences increasing in win size. For smaller wins, below $100,000, the differences by timing are quite small. These patterns preview the empirical results: the effect of income on attendance appears to be at most modest until the size of the win vastly exceeds the cost of college, and increases in the size of the win. Note that among children who could not have been affected, those from households with relatively large wins are more likely to attend college. This observation underscores the importance of comparisons within win-size.

Our baseline 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. A range of outcomes are presented, including college attendance (four-year, two-year, and any college), financial aid application and receipt, and parent labor force and saving behaviors, each with a temporal component that exploits households with children who were too old to respond to a win as an additional counterfactual. The baseline specification includes state-by-year of win fixed effects and high school cohort fixed effects, with standard errors clustered on the parent winner level. $\boldsymbol{X}_i$ is a vector of controls that varies across specifications and attempts to account for potential differences across winners that the fixed effects may not capture.[21]

The α_j coefficients and θ absorb fixed differences between children with households of different win sizes and timing of their wins (relative to graduation), respectively, and their interaction generates the identifying variation. More specifically, the α_j's absorb any differences in the propensity to attend college across children from households that experience different-sized

[20] Because the averages are not adjusted for changes in enrollment over time, we observe that students who graduate after small lottery wins are slightly more likely to attend college than those who graduate before the win. These small differences disappear in specifications that control for high school cohort fixed effects.

[21] Controls include log wages, log adjusted gross income, claiming of the mortgage interest tax deduction, claiming of self-employment tax deduction, parental college attendance, the presence of investment income, 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), as well as a rich set of child and household characteristics, including household filing status, the number of children in the household, and lottery-winning parent and child gender and citizenship.

wins, so that we are making within win size comparisons, and the omitted win size group is wins of between \$600 and \$10,000.[22] Likewise, the inclusion of θ accounts for fixed differences between children who graduate before and after their parents experience a win (though, in practice there is little difference in the rates of attendance for children whose parents win before and after they graduate from high school for small wins as well as the omitted group once cohort and state-year of win controls are added).

The key parameters of interest are the β_j coefficients, which reflect the differential college outcomes for children whose parents win the lottery of a given size before they graduated from high school relative to after and relative to the differential outcomes for the omitted group. As described above, the identifying assumption is that the college enrollment paths of children who graduate from high school after a win of a particular size would resemble those of children who graduate from high school before a win of that same size, save for the timing of the win.[23]

Before turning to the main results, we test if the comparisons used for identification are statistically valid, namely, that households that win similar amounts of money but differ in the timing of the win relative to their children's high school graduation appear observationally equivalent. Specifically, we estimate our baseline specification with exogenous characteristics on the left hand side to examine balance across these characteristics. Table 2 presents the results across each of 15 pre-win child and parent characteristics, such as adjusted gross income, self-employment status, homeownership, and the presence of savings. Among these 15 variables, only 2 are jointly significant across win sizes at the 10 percent level (self-employment and child gender). Further, none of the 5 win size bins 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 the characteristics of children whose parents won the lottery before graduation and children whose parents won the lottery after, lending credence to the validity of the design.

[22] We present robustness checks for alternative bin sizes, choice of the control group (the smallest win bin), and for tax adjusted wins based on predicted household earnings. We also present linear and quadratic specifications to examine the curvature of effects.

[23] Note that the design estimates the effect of lottery winnings that occur, on average, a couple years before high school graduation and does not hold constant subsequent and endogenous changes in resources in the lead up to graduation, such as those due to labor supply or consumption patterns. In other words, analysis does not reveal the structural relationship between resources and college enrollment during or across specific periods of time, although results later in the paper suggest that differences in timing may be relatively unimportant.

IV. The Effects of Lottery Income

a. College Attendance

Tables 3 and 4 present the main estimates of the effects of income shocks on four-year and any college attendance in the year that an individual graduates from high school. The four-year college attendance estimates, which are the focus of much of the analysis, reveal that modest income shocks have little effect on attendance. Wins between $10,000 and $30,000 (which average about $15,000) as well as wins between $30,000 and $100,000 (which average about $50,000) each produce insignificant point estimates close to zero, and rule out effects exceeding 0.5 percentage point (when pooled, effects greater than 0.3 percentage point can be ruled out). Beyond this range, income shocks begin to have more meaningful effects. Wins between $100,000 and $300,000 raise attendance by between 1 and 2 percentage points. Still-larger income shocks reveal the concavity and upper bound of such effects. Wins between $300,000 and $1,000,000 increase attendance by 5 to 6 percentage points. Interestingly, very large wins exceeding $1,000,000 result in an increase in attendance of approximately 10 percentage points. The estimates for any college attendance closely mirror the four-year college attendance results, and both are stable to specifications that add a rich set of controls for household, lottery-winning parent, and child characteristics.

The main takeaway of these results is that income effects do not appear to be highly concave. For example, households in the second highest bin receive an income shock averaging $500,000, which, even after taxes, would comfortably cover four years of tuition at a private college (potentially even for multiple children). Yet, the estimated effect on attendance is only half that of what we estimate for wins over $1,000,000. Likewise, the third largest income shock increases income by an average of more than $150,000 but generates only a fraction of the effect of larger wins.[24]

Table 5 presents analogous results for income shocks on two-year college attendance. The theoretical implications for two-year college attendance are ambiguous due to competing attendance margins. Greater financial resources may cause some students to attend a four-year college instead of a two-year college, which would result in a negative effect. By contrast, some students may be induced to attend a two-year college instead of no college, which would result in

[24] Comparing these estimates to those in the literature for college price effects (Dynarski, 2003; Kane, 2003; Kane, 2007; Deming and Dynarski, 2010; Castleman and Long, forthcoming; Fack and Grenet, 2015) suggests that changing the price of college may be orders of magnitude more effective than cash transfers for promoting attendance.

positive effects. Further, two-year college attendance may be less sensitive to household resources as tuition costs are quite modest. Consistent with competing theoretical predictions, the results do not reveal net responses in attendance. The estimates are close to zero and insignificant for all win levels. Because small wins had no effect on four-year college enrollment, the null result implies that there is no effect on two-year college enrollment (i.e., the null result is not due to offsetting margins). However, for larger wins, there is an increase in four-year enrollment. Thus, it is possible that the zero net effect for two-year college enrollment stems from some students switching from two-year to four-year colleges and others switching from no college to a two-year college. Given that responses appear to be on the four-year college margin, for brevity, the focus of the remaining analyses will be on this class of schools. (The results are extremely similar if the outcome of interest is any college.)

The primary specification abstracts from functional form assumptions by presenting average effects for ranges of treatment, made possible by the richness of the data. A natural alternative to this approach is a specification that relies on a continuous parameterization of wins. Table 6 presents specifications using continuous wins, which reveals the degree of concavity in the response. The specification includes linear and quadratic terms for wins, scaled by $100,000 for ease of interpretation, and tests the sensitivity of the coefficients to various caps on the largest win size included in the sample, both with and without controls. The challenges of imposing a specific functional form on lottery wins that vary dramatically in size are evident in the sensitivity of the estimates to the range included in the analysis.[25] Nonetheless, the coefficient on the quadratic term when including all wins capped at any level less than or equal to $5,000,000 is generally indistinguishable from zero, suggesting essentially no concavity in this region. It is only when extremely large wins are included in the estimation sample that there is clear evidence of concavity. Thus, these results are consistent with the step function model in showing weak concavity. When focusing on wins within the non-concave region (capped at $5,000,000), the estimated effect of income on attendance in a linear specification is approximately 0.6 to 0.7 percentage point per $100,000 dollars.

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[25] Income effects must become concave at some level (e.g., the marginal effect of an additional $100,000 is unlikely to be large for a household that experiences a shock of several million dollars) and must be bounded since enrollment cannot exceed 100 percent. Thus per-dollar effects are necessarily modest for very large income shocks. Ordinary least squares places the greatest weight on very large income shocks. In conjunction, these issues imply that including very large income shocks in this specification will necessarily result in small estimated effects.

From these results, it appears unlikely that a large fraction of potential students are unable to attend college due to affordability issues or borrowing constraints, or, more precisely, that these channels are the leading explanation for the income effects. For one, income shocks of $30,000 to $100,000 should be large enough to make the cost of some college tenable for most households, but they generate little change. In addition, one would expect the effects to diminish greatly when income shocks exceed the typical cost of college, but they do not.

b. Other College Outcomes

Also of interest is whether lottery wins affect intensive margin outcomes such as persistence in college or the type and quality of college attended. Because wins of less than $100,000 do not result in changes in attendance on the extensive margin, we are able to credibly estimate intensive margin effects for these win sizes. The extensive margin enrollment increases generated by larger wins complicates the interpretation of estimates at these levels, as potentially weaker marginal students have been added to the pool of college matriculates. However, the estimates can reveal whether the increase in enrollment generated by income shocks is short lived and fades in subsequent years.

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. That is, these wins do not generate effects on the extensive margin of enrollment in the year of high school graduation or on the margin of persistence in college. The net 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 large income shocks generate a temporary change in first year college enrollment and then fade. The net effect is an average increase in total years of college enrollment of 0.32 and 0.53 years of college for the largest two win bins, respectively.

Table A2 of the appendix does not reveal a clear shift in the composition of colleges attended for wins of less than $100,000 in terms of sector or selectivity (i.e., the zero effect on total enrollment is not masking offsetting changes in private and public college attendance rates). Likewise, when we examine selectivity and quality (measured by the later-life earnings of attendees), we find no significant changes. Thus, there is no evidence of extensive or intensive margin effects for wins of this size. The results indicate 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. The largest win category appears to disproportionately increase attendance at the most selective colleges, which is consistent with some students switching to more selective private colleges.[26] For expositional purposes, we re-estimate college quality, conditioning on college attendance (which is likely to bias the estimate downward by the addition of marginal students), and find that only the largest wins generate statistically significant shifts toward more selective and higher earning colleges.

c. Robustness

Our identification strategy is based in part on the observation that enrollment decisions made prior to a lottery win are independent of the shock, while those made after the shock are not. To examine the sensitivity of responses to the timing that the lottery win occurred, relative to high school graduation, we amend the specification to estimate the effects separately for shocks that occur in each of the six years before and after a child graduates from high school, including the full set of controls (to capture the differences in characteristics by win size). Based on the earlier estimates, a child is considered treated if her lottery win exceeds $100,000 or $300,000 dollars.

This exercise has three purposes. One, a by-year specification reduces to an event-study framework, which helps validate the timing assumed in the primary specification. The estimates should reveal a jump in attendance outcomes influenced by lottery wins that occurred prior to graduation and not for wins that occurred in later years. Two, the exercise provides a comparison of the relative effect sizes across the treatment years. If certain mechanisms, such as parental inputs complementary to college-going or if financial frictions play a meaningful role in other facets of human capital investment earlier in the pipeline, are operating—e.g., a child might attend a private high school or public school in a higher income community that provides better college preparation, or preparatory classes for admissions exams are attended, earlier-timed wins could produce larger effects. Three, the estimates reveal whether wins in the year of graduation affect college outcomes, which could yield insight into policies that reduce the cost of college, or the salience of that cost, when students are in the second half of their senior years of high school.

[26] Note that non-enrollees' college quality is assigned to be zero, which makes it difficult to interpret for larger wins, as the measure will increase mechanically through the increase in college enrollment on the extensive margin.

Figure 2 plots the results, revealing that students who graduate in each of the six years after a household has an income shock experience positive and significant enrollment effects. In addition, there is no evidence of significant effects for wins that occur the year of high school graduation or in subsequent years. This pattern of results affirms the timing implied by our main specification and further suggests that the year in which policies seeking to improve college affordability (e.g., student aid offers, education tax credits) become salient to households is relevant and should be taken into account in their design. Of course, there may be a difference between an unanticipated change in income in the year of high school graduation and a subsidy that is expected well in advance.

We consider two additional exercises to examine the role of timing. First, we begin with our main specification and sample that includes households that win 6 years before and after high school graduation and, year-by-year, narrow the window around graduation for households included in the sample (see Table A3 of the appendix). A couple of interesting results emerge. Modest lottery winnings, even within narrow windows, do not appear to affect college-going. This finding suggests that the results pertaining to modest wins, or lack thereof, are not driven by extremely credit constrained households spending down lottery winnings prior to when they would be used for college. In addition, large wins continue to have large effects on college-going even in narrow windows of timing.[27] Second, we consider a variant of the main 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: first, a continuous win measure concentrating on the segment of the win distribution where the effect appeared to be linear (wins under $5,000,000) and second, a dummy variable indicating a win over $100,000. In neither case is the coefficient on the trend statistically distinguishable from zero, further suggesting no differences across win years prior to high school graduation.

We next derive estimates using the more common across win size design. This approach ignores the timing aspect of lottery wins and simply compares attendance rates across households with larger and smaller wins.. For such a comparison to be valid, after controlling for observable characteristics, households of differing win sizes would need to have, on average, similar propensities to attend. Table A4 of the appendix presents the results. When controls are included,

[27] This can further address the concern that the households that are far apart in the timing of the win differ in unobservable ways, which would have complicated a comparison that includes such households.

estimates are quite similar to the preferred estimates. There is no evidence that income shocks of less than $100,000 generate significant increases in attendance. Wins between $100,000 and $300,000 increase attendance by about 2 percentage points, wins between $300,000 and $1,000,000 result in increases of about 5 percentage points, and wins exceeding $1,000,000 result in increases of nearly 10 percentage points. Thus, it seems that restricting attention to lottery winners and including a rich set of controls does a reasonably good job of accounting for differences in the propensity to attend college across win sizes.

The pattern and magnitude of results is also robust to a number of alternative methods of classifying income shocks, constructing key variables, and defining the sample (see Appendix Table A5). Alternative choices of the omitted range of wins that form the control group do not meaningfully alter the estimates (columns 3-5).[28] When wins are adjusted for predicted taxes based on each household's pre-win income and composition (since actual taxes paid are partly a function of endogenous responses in the year of the win), some larger wins are necessarily classified in lower win bins, thus increasing the point estimates at each level. The results are similar when restricting attention to the sample of children who are linked to their birth parent by Social Security birth records (which will include birth parents who are non-filers). Results are presented for several additional alternatives: defining attendance within a year of high school graduation as the outcome (which will capture students who graduate a year later than predicted); including lottery winners that were excluded because they could not be properly classified (e.g. those for whom there is evidence that the lottery win occurred prior to the year reported on the W2G or for whom it was not possible to identify the amount of the first win); including lottery wins that occur in the year of high school graduation; clustering the standard errors at the state-by-year level; and weighting the sample by population average characteristics. In each case the resulting estimates are similar to those in the baseline specification in terms of both magnitude and pattern.

Table A6 of the appendix replicates the design while employing narrower win bins. Precise estimates are possible due to the large number of smaller wins in the sample. The results generate no evidence that the preferred bin sizes obscure positive effects for income shocks of less than $100,000.

[28] The results thus far suggest that the omitted range will not affect the estimates, since wins ranging from $10,000 to $30,000 do not generate changes in college enrollment relative to the omitted group.

Finally, a potential concern for identification is that colleges may not submit enrollment data for students whose grants meet or exceed tuition billed. Thus, non-classical measurement error may be biasing the estimates to overstate the effects of lottery wins, particularly among low-income households (who are most likely to receive full scholarships).[29] To mitigate these concerns, we verify that the results are robust to restricting attention to colleges that appear to report fully by identifying colleges that have low rates of students reporting students receiving full grant aid (see Appendix Table A7).[30] Further, an alternative construction of enrollment that relies on both Form 1098-T and information reported to the Department of Education by Title IV institutions generates a similar pattern of results.

d. Heterogeneity

Households of varying means might differ in their responsiveness to income shocks, and understanding how would lend insight into the mechanisms that generate the results. First, it is possible that, within the population, households have different priorities for how to use additional income, such as reducing labor supply or purchasing a home, and understanding the extent to which children's education is a spending priority for different household types is useful for policy. In addition, insight into which household types are most responsive could point to whether constraints versus consumption preferences (or both) seem to be important determinants of the effects.

On the one hand, low-SES households may be more sensitive to income shocks if such households are relatively financially constrained – e.g., children from these households face more difficulty paying for college or are relied upon for household income – and if, as some papers have found, the marginal return to education is higher among this group. In addition, any-sized win will invariably represent a larger fraction of household resources for such households, which would also presumably correspond to increased sensitivity among this group. On the other hand, there could be more responsiveness among high-SES households if there is complementarity between the consumption value of college and prior ability (e.g., a higher fraction of children from such households are academically prepared, eligible (with a high school degree)) or such children are

[29] As a simple check on this measurement error concern, we note that low-income children would be more susceptible to this problem, yet the next section reveals that positive attendance estimates are primarily driven by children from higher-income households.

[30] To verify that non-reporting colleges are identified accurately, we identify a sample of colleges that state explicitly that they do not file a Form 1098T for students receiving full scholarships, and then verify that our method correctly identifies these colleges.

more interested in attending college, or prefer jobs that require college degrees. Further, if lower-SES households hold the view that children are primarily responsible for financing their own college (as opposed to parents), college-going among children from higher-SES households would be more sensitive to extra household income from lottery wins (Sallie Mae, 2015). Finally, increased income from lottery wins may simply crowd out need-based financial aid for children from low-SES households, compared to those from high-SES households who are generally ineligible for such aid, which would point to larger effects among high income households. (This explanation is examined more thoroughly in the next section using financial aid records.)

Fortunately, the breadth and detail of the tax data enable us to directly investigate this type of heterogeneity, and particularly whether responsiveness varies along either a household income or saving dimension. Specifically, we present responsiveness across two household types at a time – high-income versus low-income and high-saving versus low-saving.

Table 8 presents the results for households with incomes above and below the median (about 45,000 dollars) and with and without investment income (as a proxy for savings) for all affected households. (Because federal financial aid programs make the most relief available to the lowest income households, Table A8 of the appendix presents the responsiveness for terciles, thus isolating households with the lowest levels of resources.) Approximately half of the households in the sample have no investment income. The estimates do not reveal statistically significant changes in enrollment for students from households with below median income or less savings in response to lottery wins of less than $100,000, which is at least suggestive evidence that such households are not highly constrained. The results also indicate that effects for large wins appear to be concentrated among households with above median income rather than households with below median income and are certainly no larger for low-savings households than high-saving households: in fact, among households that experience wins exceeding $1,000,000, attendance increases by nearly 14 percentage points for both higher income households and higher saving households.[31] The estimates are similar when including the presence of a home mortgage in the proxy for savings.

The overall difference in the effects of lottery wins for high- and low-income households among the wins groups are statistically significant. For clearer exposition, two variants of this

[31] Differences are unlikely to stem from overstated relationships via an increased likelihood that birth parents are absentee within low-SES households, as matches in the sample rely on dependent tax claims rather than birth records.

specification are included in Table A9 of the appendix. The first variant interacts a dummy variable for high-income or high-savings households, per the above measures, with a continuous win size measure concentrating on the segment of the win distribution where the effect appeared to be linear (wins under $5,000,000). The second variant interacts the above dummies with whether or not the household experienced a large win exceeding $100,000. The results indicate that higher income households are more responsive, as are households that have savings. Interestingly, when both are included in the specification, the coefficient on savings is no longer significant, but the sign is positive, again inconsistent with larger effects for those with no prior wealth.

e. Federal Financial Aid

Our primary results may obscure the potentially important role that current financial aid policy plays in reducing credit constraints and improving college accessibility. For example, if lottery winnings crowd out need-based financial assistance, there may not be particularly large attendance responses to small wins that do not exceed the cost of college, or for low-income households who are likely most susceptible to this type of crowd out. Fortunately, this setting affords us the opportunity to examine these questions directly.

We begin by presenting, primarily for illustrative purposes, the effects on Federal financial aid outcomes over the full sample of lottery-winning households.[32] Financial aid is reported at the academic year level and thus the outcome of interest is receipt in the first year after high school graduation. It is important to note that the outcomes reflect financial aid receipt, which is endogenous to college attendance, aid application, and non-college responses to lottery wins that affect income and asset holdings (e.g., labor supply). In other words, the results will be estimates of the net effects on financial aid, including any differences in aid that result from lottery-induced increases in attendance, decreases in aid application or take-up, or compositional shifts in either in addition to any crowd out that may be occurring, and therefore should be interpreted with caution. In particular, these estimates are difficult to interpret for wins over $100,000, for which there is increased attendance.

Federal financial assistance does appear, on average, to be reduced by a lottery win, as shown in Table 9. (Table A10 separately presents results for aid applications and expected family contribution.) Interestingly, the estimates reveal reductions in aid among smaller winners for

[32] 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).

whom there is no effect on attendance: children from households who win $30,000 to $100,000, on net, receive less in Pell Grants and borrow less in subsidized loans, suggesting that some crowd-out of aid might be damping the effects among households with more modest wins. Even for large wins, which should be biased upward by increased enrollment, it appears that need-based aid (i.e., Pell Grants and subsidized loans) is reduced by lottery wins (though the pattern is less clear for non-need based unsubsidized loans).[33] To better understand the magnitudes within win size ranges, we replicate the exercise conditional on attendance (which is of course endogenous). With this restriction, the reduction in aid is, in some specifications, quite large, with estimated effect sizes nearly double those in the unconditional regressions. While attendance rates did not change much for these modest wins, aid amounts declined; thus, in the absence of federal financial aid programs, there could be larger responses to smaller lottery wins.

Is the crowd out of aid moderating attendance responses to lottery wins? To investigate the answer to this question, we leverage a useful institutional feature of the primary formulas used for financial aid determinations -- that the marginal tax rate on parents' income can be quite high (about 20 to 50 cents on the dollar) whereas the effective tax rate on assets far lower (only several cents on the dollar) (Dynarski, 2004). These different rates imply a substantially different loss of aid depending on whether the lottery win occurred in the year before high school graduation, a critical year on which financial aid eligibility is determined for the first year of college (hereafter "FAFSA Year"), versus in prior years.[34,35] To exploit this feature, we first re-estimate the attendance and aid outcomes, excluding and restricting to wins that occurred the year before a child graduates high school. As shown in Table 10, when lottery wins that occur in the key

[33] While they cannot be observed, it is extremely likely that institutional and state aid are crowded out by lottery wins as well, as they tend to rely on the same (or similar) eligibility formulas that the Federal government uses. Regression analyses using the restricted-access 2007-8 and 2011-2 NPSAS reveals an extremely precisely-estimated negative correspondence between expected family contribution (i.e., need) and total freshman year state and institutional aid – -0.016 (.002).

[34] 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.

[35] There are two formulas that can be employed 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 available on the FAFSA while the IM, used mostly by some private colleges, claims to take a more comprehensive stance on a family's ability to pay for school and relies on FAFSA information as well as supplementary information on, for example, home equity. For our purposes, both formulas are similar in that they treat assets much more favorably than income: in the FM, parental income can be assessed up to 47% and assets up to 6%, and in the IM, parental income can be assessed up to 46% and assets up to 5%. (See https://www.reed.edu/financialaid/pdfs/CSS_IMwhatisit.pdf, accessed on September 8, 2016).

financial aid year are excluded, the attendance results are unaffected and Pell Grant reductions are quite small. When only the FAFSA Year is included, attendance effects are similar, but the crowd out of Pell Grants is large. The bottom panel replicates the exercise while restricting attention to low-income households to further probe whether the (non-)effect on attendance stems from the crowd-out of need-based financial aid. The results are even more striking: there is no relative reduction in attendance effects for wins in the FAFSA YEAR, but the difference in the amount of aid that is crowded out is even larger compared to in the full population.

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, even though the prior results are not prima facie consistent with such heterogeneity. To relax this assumption, we estimate a regression discontinuity-like framework, allowing for a differential effect in the key year, whereby a dummy variable for the "FAFSA Year" is interacted with the amount of the win or an indicator for wins over $100,000 (as before), and then add a linear time trend interacted with whether the win occurred before high school graduation. The results are presented in Table 11. The "differential effect" is, in fact, *positive* in all cases (but also statistically insignificant); further, when allowing for treatment heterogeneity in time (via the time trend interaction), the coefficient actually grows. Likewise for low income households there is no evidence that attendance effects are reduced by financial aid crowd-out, with and without controlling for a time trend in the treatment effect.[36] Altogether, these results imply, while a reduction in financial aid is the natural byproduct of winning the lottery, whatever crowd-out is occurring is not altering children's attendance decisions, at least on average. Further, this type of crowd-out does not explain the heterogeneous responses observed across the population.[37]

f. Alternative Household Responses

Households may respond on other dimensions, including parents' earnings and labor supply, savings (e.g., investment income, mortgages), and mobility responses, which provide context for the attendance results and may reveal other spending priorities. This setting also allows us to

[36] To even 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 out from the estimates for below median income households. Again, the resulting estimates are insignificantly positive and therefore go in the opposite direction as would be expected if aid crowd-out was moderating responses.

[37] Note, these results do not necessarily imply that college enrollment decisions are unaffected by (anticipated) financial aid.

examine, albeit indirectly, whether individuals treat lottery winnings unlike other money. Each of these outcomes is considered in the same manner as attendance, i.e., estimating the effects on the outcome in the year a child graduates from high school. Results are presented in Table 12.

The estimates reveal evidence of lower earnings, and for the large wins, reductions on the extensive margin of labor supply. Interestingly, there appears to be no effect on self-employment earnings, even though prior literature has suggested income shocks will increase self-employment (e.g., Holtz-Eakin et al. 1994; Lindh and Ohlsson 1996). Perhaps the reduction in labor supply more generally offsets the relaxation of financial frictions. Likewise, large wins generate increases in interest and dividend income, a proxy for savings. The effects of additional income for investment in homeownership are more nuanced: for those without a mortgage prior to winning, there is an increase in having a mortgage *at every win level*, with the size of the effect growing in the amount of the win and on the order of 25 percentage points for very large wins. For those with a mortgage already, it appears that households use large winnings to pay it off. Thus, homeownership appears to be a significant use of additional income. Finally, there is evidence that only very large winners move to (slightly) wealthier neighborhoods and those with higher rates of college-going.[38] If neighborhoods are classified on the basis of recent causal estimates of income mobility by county (Chetty and Hendren, 2015), there is no evidence that these moves are to areas with more upward mobility. The results for children's labor supply (see Table A11 of the appendix) indicate that lottery wins are associated with reduced earnings, with the effects increasing in the size of the win. 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) than on the extensive margin of not working at all. We also examine earnings in early adulthood, selecting 27 years old as a representative age at which to measure earnings. (We choose 27 years old to strike a balance between selecting an age that is correlated with later-life earnings and restricting the size of our sample. Note that, by this point, all children in our sample will be "post-win," although comparisons are still derived by the timing of the wins relative to

[38] These outcomes are average zip code level adjusted gross income and the proportion of 17 year old residents of the zip code that then attend 4 year institution in the year of their expected high school graduation, respectively. As far as interpreting these magnitudes, they suggest that, under the arguably unrealistic scenario that children adopt the same college-going rate as those from their neighborhood, such neighborhood effects could explain only a fraction of the main effects.

high school graduation.) We find some evidence that children whose parents won before they graduated have higher earnings in early adulthood, but the results are inconclusive

Finally, we investigate the persistence of effects on earnings by win size, which helps us understand, at least coarsely, whether individuals appear to either consume away all of their winnings quickly (as some anecdotes suggest) or treat lottery winnings similar to other types of income. If the former were occurring, one might expect any earnings response to quickly dissipate as lottery winnings are depleted. Figure 3 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. These results are inconsistent with the narrative that lottery winnings are spent immediately and more in line with a measured response that most parameterizations of a lifecycle model would predict in response to a wealth shock.

V. Mechanisms and Related Literature

This section describes four prevailing (non-exclusive) mechanisms that could explain the relationship between resources and attendance and discusses whether our evidence from lottery wins is broadly consistent with each.[39]

First, differences in resources may reflect omitted factors—complementary to schooling investment—that parents transmit to children, such as ability or preferences. In this case, because the relationship is not causal in nature, lottery income of any amount should have no effect on attendance. Though omitted factors may still explain some of the correlation, the estimates in this study reveal responses to income, and in some instances, responses are quite large, implying that increased income can increase college-going and that there indeed exists a causal relationship. However, it is worth noting that there is little evidence of an effect for low-income households, consistent with the existence of any number of factors within such households that lead them to be not much affected by additional income, at least late in childhood.

Second, households may face financial frictions—such as borrowing constraints—that restrict college access for children who would otherwise earn high returns from education. In this case,

[39] There are other possible mechanisms that are unlikely to be primary channels. For example, the results are not consistent with resources increasing attendance by insuring against the risk of college investment (as the effect should be larger among those with fewer resources), but we cannot rule out that there is complementarity between parent resources and their children's attendance (though one might still expect the effect to be bigger for those that have the least experience with resources, i.e., lower-income families).

the effect on college-going should be concave in the size of the income shock, with even very modest increases in income leading to large changes in attendance and the largest responses among the most constrained households. But, in fact, the estimates do not reveal a large degree of concavity. Further, modest income shocks generally do not affect college going, and financial aid does not seem to be an important part of the story. Likewise, low-income and low-savings households, even conditional on each other, are no more, and indeed sometimes less, responsive than other types of households, which is inconsistent with a simple model of financial frictions. Finally, there is evidence that parents also (persistently) reduce their labor supply, which would be unusual if households were very financially constrained.[40]

Third, increased resources may spur investment in children that is complementary with schooling (or alter preferences towards schooling). In this case, the relationship is causal but requires intermediate investment; thus, increasing household income should increase attendance, but earlier interventions will likely generate more meaningful effects, especially compared to lottery wins that happen in the year before high school graduation. Yet, our results by timing indicate that earlier interventions do not have larger effects. Further, there is only slight evidence that households use their winnings to move to better neighborhoods, even among those with the largest wins who move to richer neighborhoods. Finally, if there is concavity in the effect of parental inputs on college attendance and parental inputs is an important channel, the effects are unlikely to be larger effects for higher-income households. While it is not possible to fully rule out this mechanism, there is little evidence in this context of parents making early-stage investments in the children that translate into increased college-going.[41]

[40] The array of evidence we present could be consistent with borrowing constraints that bind in early childhood. However, when we explore this question by taking full advantage of the data and including in the sample children from households that won the lottery up to 14 years prior to their year of expected high school graduation, we do not find evidence of larger effects for earlier wins. Specifically, the interactions of both win amount and an indicator for wins over $100,000 with a linear time trend are insignificant. Thus, there is no evidence that earlier lottery winnings have larger effects, which would be expected from borrowing constraints that bind in early childhood. The earlier results could similarly be consistent with *children* facing binding credit constraints, if their parents possess different norms over who is responsible for financing higher education. While we cannot test this directly, the finding that financial aid crowd-out does not influence enrollment suggests one of two possibilities: low SES parents do provide support to their children, which makes up for the deleterious effects of the removal of financial aid; or these parents do not provide support but the removal of financial aid has little effect on college-going (or support to their children is only provided when their winnings crowd-out financial aid).

[41] The effects on neighborhood are too small to explain more than a fraction of the effect, and the effects on attending more selective institutions have ambiguous implications; they could be consistent with a role for parental inputs, but they could also be due to financial frictions or increased consumption value from better colleges.

Finally, households may derive consumption value from college, whereby college is more or less a budget item that households exact preferences over and purchase accordingly. In this case, consumption of college should continue to increase with income, with the largest responses concentrated among those who presumably value college most. Indeed, our results indicate that income effects are not very concave, with a high upper bound at values that far exceed the cost of college. The pattern of results is quite similar to other forms of household response, such as labor supply, savings, housing consumption, and paying off mortgage debt. In this case, above-median income and high-saving households may be the most responsive because they have the fewest competing priorities, because they place a higher value on consuming college, or due to complementarities between consumption value and prior ability (e.g., academic readiness). On net, our results are fully consistent with households deriving consumption value from college.

We can also interpret our findings in the context of prior literature. In general, our results are not consistent with quasi-experimental studies, which tend to find large effects of modest income and wealth shocks, with Cesarini et al (2016) and Hilger (2016) being two notable exceptions.[42] The size of the income shock needed to generate meaningful effects in this context is quite large, which could explain why Cesarini et al. (2016) do not detect effects when they examine Swedish lotteries (which have fewer of the very large payouts that we observe). Additionally, their main finding of no effect on children's intermediate development outcomes is consistent with our conclusion that the relationship is likely driven by the consumption value of college (rather than parental inputs). Second, Hilger (2016) finds that parental job loss only modestly reduces college-going, with even smaller effects among lower income households.[43] His preferred interpretation of these findings is that financial constraints are not binding and that households derive consumption value from college. Our results lend credence to these conclusions because, not only is our effect size for a pure income shock within the range of his implied magnitude (from the pecuniary effect of a job loss), but we also document the degree of concavity of the response, that children from low-income households given *additional* resources are generally no more likely to attend college, and that financial aid crowd-out does not appear to moderate responsiveness.

[42] Our results are also consistent with Cameron and Taber (2004), who, through the use of both quasi-experimental methods and as well as structural estimation, find no evidence of binding borrowing constraints.

[43] His paper hypothesizes that effects are smaller for low-income households because need-based financial aid provides access to children who wish to attend school, particularly if children from these households (relative to those with more income) do not rely heavily on parents to finance college.

VI. Conclusion

This paper estimates the impact of one-time increases in income on college outcomes. It is the first study to exploit the universe of lottery wins in the United States, and to exploit a wide range of time-varying income shocks across different households to examine changes in college-going. Several important results are revealed. Modest shocks, including those at levels ample to cover college costs, have little effect on attendance. Effects increase with the size of the win and are not highly concave, with a large upper bound at levels that far exceed the cost of college. The timing of the win relative to the year of graduation does not appear to be important. Low-income and low-saving households are not relatively more responsive (and in fact appear to be less response) than other types of households and there is no evidence that financial aid moderates household responses. Finally, parents decrease labor supply, increase housing consumption, and increase net savings, but only move to wealthier areas when provided with a substantial income shock. The modest estimates for smaller wins and the upper bound effects provide valuable context for interpreting existing studies of household resources and college access.

The results have several implications for policy design. While wins of less than $100,000 should be sufficient to cover a significant fraction of tuition at a four-year public university and to ease most financial constraints associated with college attendance, attendance responses are not found either in aggregate or among lower-SES households, suggesting that, in the current policy environment, borrowing constraints are not explicitly hindering attendance. Thus, new policies seeking to raise educational attainment should not be primarily justified on the basis of the existence of such constraints. Further, redistribution of income towards low-SES households would likely be ineffective at generating enrollment changes, unless the transfers are far larger than what could conceivably operate through the tax system. Policymakers seeking to increase educational investment, particularly among such households, might instead see benefits from interventions that reduce the cost of college or strive to increase the consumption value households place on college.

Finally, our results raise a new question of why it is that increasing the resources of low-income households appears to be especially ineffective. Such households may have weaker preferences for post-secondary education, different norms over who is responsible for financing higher education, or other financial priorities that are inhibiting their responsiveness relative to higher-income households. Future work should explore which channels operate and how policy can remedy these gaps.

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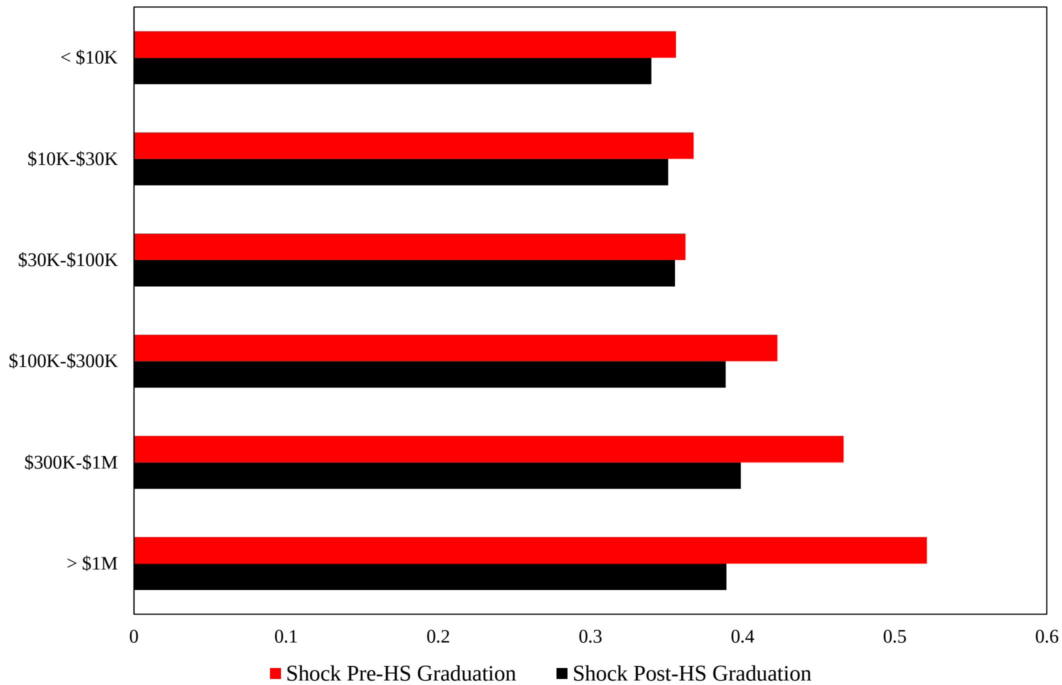
Figure 1: Likelihood a Child Attends College in Year of High School Graduation
by Size and Timing of Lottery Win (2010$)



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 not adjusted for graduating cohort fixed effects and thus will reflect time trends in college enrollment. 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.

Figure 2a: Estimated Effects on Four-Year Attendance for Large Wins (>$100,000)
by Timing of Win Relative to High School Graduation

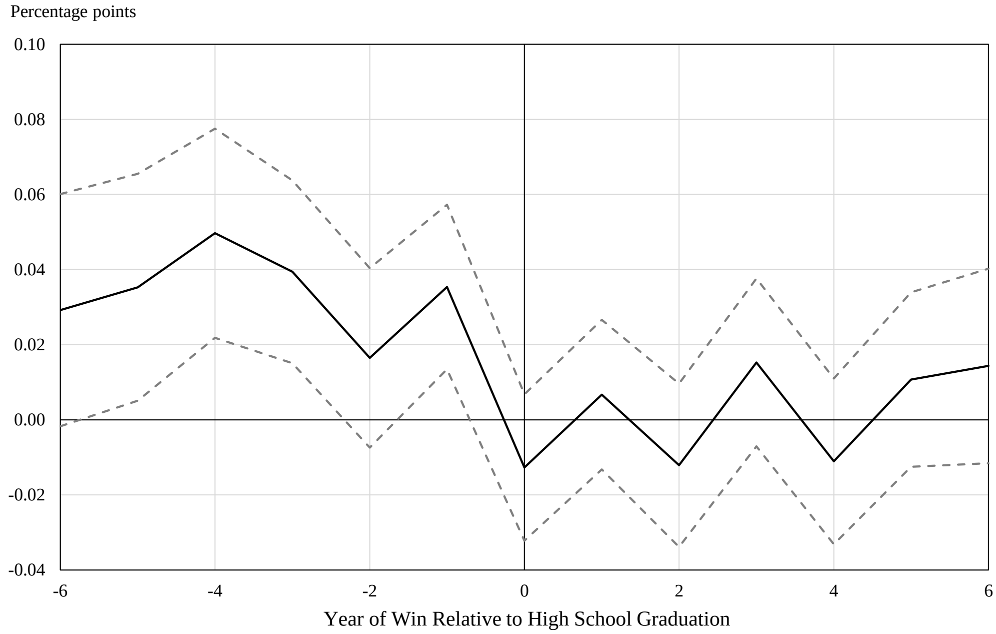
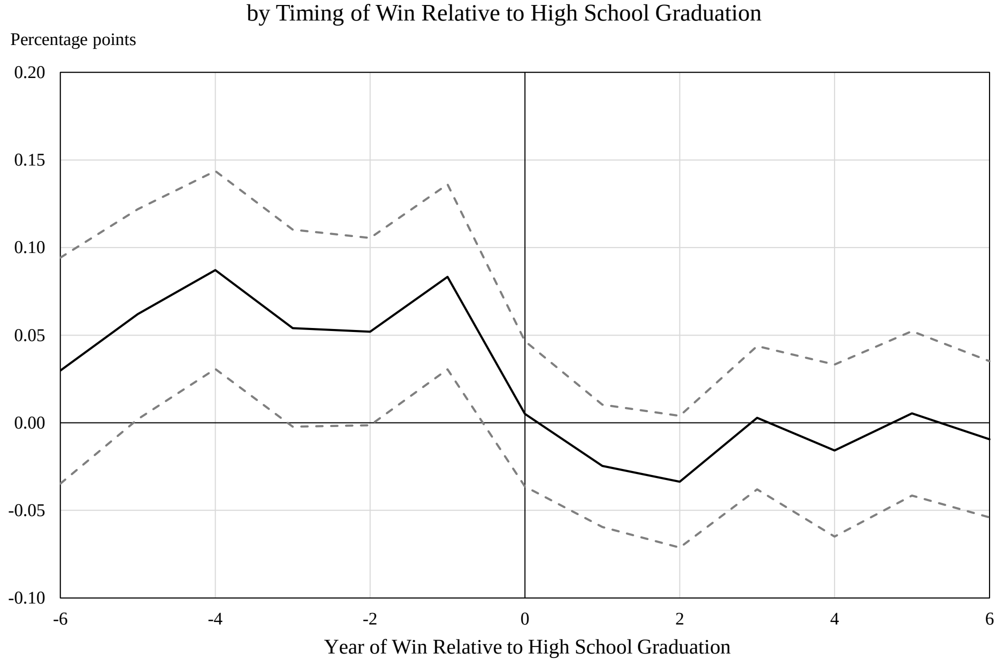
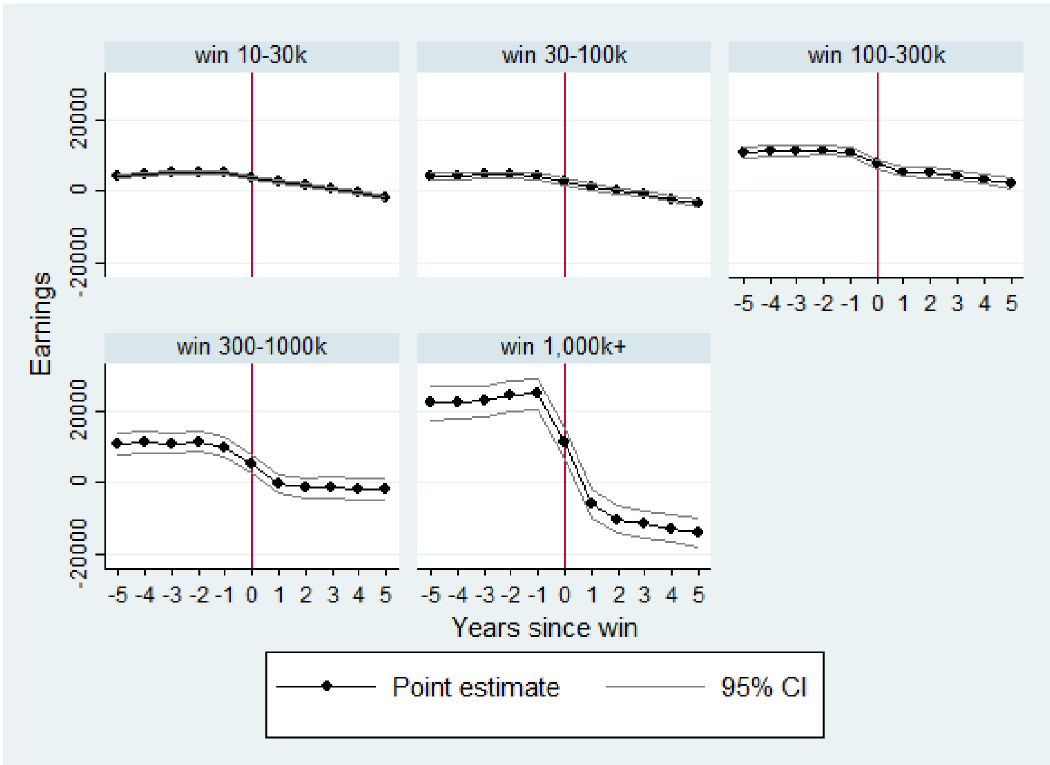


Figure 2b: Estimated Effects on Four-Year Attendance for Large Wins (>$300,000)
by Timing of Win Relative to High School Graduation



Note: This figure presents the percentage point change in 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. Dashed lines depict the 95 percent confidence interval.

Figure 3: Household 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 the 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 attendance rates. Column 1 of the top panel presents the number of students affected by first year 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 first year 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)	
Winner/parent 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 is 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: 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 4: 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 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: Quadratic Test For Concavity

		(1) Quadratic	(2) Quadratic with controls
Range: All	Pre-HS Grad * Win Amount ($100k)	0.0006811**	0.0005396**
		(0.0002784)	(0.0002601)
	Pre-HS Grad * Win Amount ($100k) ^{2}	-0.0000005**	-0.0000004*
		(0.0000002)	(0.0000002)
Range: 0 to 50 million	Pre-HS Grad * Win Amount ($100k)	0.0031833***	0.0033381***
		(0.0007294)	(0.0006592)
	Pre-HS Grad * Win Amount ($100k) ^{2}	-0.0000102***	-0.0000100***
		(0.0000028)	(0.0000024)
Range: 0 to 25 million	Pre-HS Grad * Win Amount ($100k)	0.005119***	0.005205***
		(0.001019)	(0.000938)
	Pre-HS Grad * Win Amount ($100k) ^{2}	-0.000026***	-0.000025***
		(0.000007)	(0.000006)
Range: 0 to 10 million	Pre-HS Grad * Win Amount ($100k)	0.007828***	0.007079***
		(0.001753)	(0.001656)
	Pre-HS Grad * Win Amount ($100k) ^{2}	-0.000057**	-0.000048*
		(0.000027)	(0.000026)
Range: 0 to 5 million	Pre-HS Grad * Win Amount ($100k)	0.008616***	0.007133***
		(0.002584)	(0.002384)
	Pre-HS Grad * Win Amount ($100k) ^{2}	-0.000078	-0.000049
		(0.000093)	(0.000082)
Range: 0 to 2.5 million	Pre-HS Grad * Win Amount ($100k)	0.008022**	0.006362*
		(0.003587)	(0.003360)
	Pre-HS Grad * Win Amount ($100k) ^{2}	-0.000004	0.000026
		(0.000283)	(0.000262)
Range: 0 to 1 million	Pre-HS Grad * Win Amount ($100k)	0.007592	0.005155
		(0.005496)	(0.005178)
	Pre-HS Grad * Win Amount ($100k) ^{2}	0.000169	0.000523
		(0.000947)	(0.000879)
Range: 0 to 500k	Pre-HS Grad * Win Amount ($100k)	0.004837	0.002671
		(0.008660)	(0.008221)
	Pre-HS Grad * Win Amount ($100k) ^{2}	0.001420	0.001473
		(0.003181)	(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. 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. 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, 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. 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) Low Inc	(2) High Inc	(3) No Invest Inc	(4) Invest Inc
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 two and four-year college enrollment in the year after high school graduation. The results are presented for students from households with above and below median income 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, 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. 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: Federal Financial Aid: Grants and Loans

Unconditional	(1) Pell Grants	(2) Subsidized Loans	(3) Unsubsidized Loans
Win 10-30k Pre-HS Grad	-\$49.50***	-\$3.58	\$0.30
	(12.31)	(10.89)	(29.80)
Win 30-100k Pre-HS Grad	-\$88.12***	-\$67.18***	-\$75.32
	(21.69)	(18.15)	(50.12)
Win 100-300k Pre-HS Grad	-\$71.48**	-\$83.24***	\$207.44**
	(28.50)	(26.24)	(83.01)
Win 300k-1.0m Pre-HS Grad	\$24.72	-\$11.57	-\$38.14
	(59.82)	(53.95)	(183.80)
Win 1.0m or more Pre-HS Grad	-\$227.93***	-\$305.14***	-\$501.10**
	(77.57)	(64.33)	(233.74)
Observations	1,461,262	1,461,262	1,461,262
Mean Dep	\$538.16	\$491.94	\$491.94
Conditional on 4-Yr Attendance	(4) Pell Grants	(5) Subsidized Loans	(6) Unsubsidized Loans
Win 10-30k Pre-HS Grad	-\$89.44***	-\$15.87	-\$111.59
	(34.12)	(31.59)	(105.94)
Win 30-100k Pre-HS Grad	-\$147.52**	-\$118.34**	\$4.52
	(62.18)	(55.62)	(187.50)
Win 100-300k Pre-HS Grad	-\$316.13***	-\$305.71***	\$371.30
	(71.70)	(70.21)	(249.64)
Win 300k-1.0m Pre-HS Grad	\$54.83	-\$204.98	-\$632.42
	(139.00)	(140.96)	(554.41)
Win 1.0m or more Pre-HS Grad	-\$603.06***	-\$1,003.23***	-\$1,909.92***
	(181.60)	(166.47)	(719.99)
Observations	319,341	319,341	319,341
Mean Dep	\$1,163.24	\$1,550.34	\$2,985.71

Note: Estimates show changes in the rate receiving federal grants and loans with and without conditioning on college attendance. 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. 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: College Attendance and Federal Aid: Critical FAFSA Year

All Households	(1) Attend 4Yr Non-FAFSA Yr	(2) Pell Grants Non-FAFSA Yr	(3) Attend 4Yr FAFSA Yr	(4) Pell Grants FAFSA Yr
Win 10-30k Pre-HS Grad	-0.0020	-$18.24	0.0002	-$157.63***
	(0.0038)	(13.83)	(0.0058)	(18.84)
Win 30-100k Pre-HS Grad	-0.0055	-$35.17	-0.0149	-$278.94***
	(0.0067)	(24.28)	(0.0102)	(31.19)
Win 100-300k Pre-HS Grad	0.0193*	-$36.88	0.0158	-$189.81***
	(0.0099)	(32.02)	(0.0149)	(41.59)
Win 300k-1.0m Pre-HS Grad	0.0521**	$89.09	0.0594*	-$209.38**
	(0.0211)	(68.48)	(0.0339)	(86.23)
Win 1.0m or more Pre-HS Grad	0.1057***	-$175.71**	0.1673***	-$442.18***
	(0.0273)	(88.19)	(0.0489)	(82.10)
Observations	1,317,523	1,317,523	961,290	961,290
Mean Dep	.215	$538.16	.215	$538.16

Low Income Households	(5) Attend 4Yr Non-FAFSA Yr	(6) Pell Grants Non-FAFSA Yr	(7) Attend 4Yr FAFSA Yr	(8) Pell Grants FAFSA Yr
Win 10-30k Pre-HS Grad	0.0048	-$12.11	-0.0030	-$215.84***
	(0.0046)	(22.94)	(0.0070)	(32.29)
Win 30-100k Pre-HS Grad	-0.0098	-$58.12	-0.0044	-$400.72***
	(0.0082)	(40.99)	(0.0129)	(53.58)
Win 100-300k Pre-HS Grad	-0.0127	-$28.96	-0.0057	-$302.64***
	(0.0126)	(60.38)	(0.0197)	(78.61)
Win 300k-1.0m Pre-HS Grad	0.0199	$89.84	0.0568	-$336.67**
	(0.0272)	(126.09)	(0.0445)	(166.27)
Win 1.0m or more Pre-HS Grad	0.0355	-$356.31**	0.0464	-$635.41***
	(0.0402)	(181.47)	(0.0748)	(211.88)
Observations	657,385	657,385	469,214	469,214
Mean Dep	.133	$789.81	.133	$789.81

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). Columns 1 and 2 exclude lottery wins in the critical FAFSA year (the year prior to high school graduation) and columns 3 and 4 only include the critical FAFSA year and post-graduation control years. 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. 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 11: Four-Year College Attendance: Critical FAFSA Year With Time Trends

All Households	(1) Without Time Trend	(2) With Time Trend
FAFSA Year * Win Amount ($100k)	0.0034	0.0187
	(0.0139)	(0.0188)
Observations	1,461,262	1,461,262
Mean Dep	.215	.215
Low Income Households	(3) Without Time Trend	(4) With Time Trend
FAFSA Year * Win Amount ($100k)	0.0124	0.0265
	(0.0184)	(0.0241)
Observations	730,628	730,628
Mean Dep	.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 estimate measure whether the critical FAFSA year has a differential effect on college attendance. Column 2 controls for time trends in the timing of the win relative to high school graduation. 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 12: 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. 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. 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: Lottery Playing

To investigate whether families that play the lottery differ from the general population, we analyzed microdata from 10 years (2005Q1 to 2014Q4) of Consumer Expenditure Surveys (CEX). Within the CEX, we find that roughly 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 key demographic, income, and labor force dimensions (Appendix Table A1b). 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 characteristicsincluding race, highest education, and family sizewe do not find large differences between families that do and do not play the lottery.

Still, it has been noted that the CEX likely underreports lottery playing (Kearney 2005). Much 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 indicates that 57 percent of adults buy a 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. Within these different surveys, lottery players again do not differ dramatically from non-players along most demographic and labor force dimensions.

Table A1a: Comparison: 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 College: Yr 1	0.39	0.35
Attend 4-Yr College: Yr 1	0.25	0.22
Attend 2-Yr College: Yr 1	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.

Table A1b: Characteristics of Lottery Players: Consumer Expenditure Surveys 2005-2014

	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
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 2005 to 2014.

Table A2: Four-Year Attendance by College Type

	(1) Private	(2) Public	(3) For-Profit	(4) More Selective	(5) Selective	(6) Not Selective	(7) Avg Earnings
Win 10-30k Pre-HS Grad	0.0031	-0.0058**	0.0011*	-0.0027	0.0005	0.0008	-$155.14
	(0.0022)	(0.0030)	(0.0007)	(0.0021)	(0.0026)	(0.0016)	(211.77)
Win 30-100k Pre-HS Grad	-0.0060*	0.0004	-0.0018	0.0029	-0.0037	-0.0050*	-$438.39
	(0.0036)	(0.0053)	(0.0012)	(0.0037)	(0.0045)	(0.0029)	(374.05)
Win 100-300k Pre-HS Grad	0.0029	0.0170**	-0.0011	0.0128**	0.0098	-0.0032	$683.31
	(0.0056)	(0.0078)	(0.0016)	(0.0057)	(0.0069)	(0.0039)	(526.37)
Win 300k-1.0m Pre-HS Grad	0.0108	0.0333**	0.0098**	0.0156	0.0156	0.0069	$2,798.71**
	(0.0119)	(0.0167)	(0.0044)	(0.0118)	(0.0144)	(0.0092)	(1,119.81)
Win 1.0m or more Pre-HS Grad	0.0487***	0.0657***	0.0039	0.0736***	0.0593***	-0.0148	$5,055.47***
	(0.0176)	(0.0223)	(0.0057)	(0.0177)	(0.0203)	(0.0094)	(1,527.29)
Observations	1,461,262	1,461,262	1,461,262	1,461,262	1,461,262	1,461,262	1,461,262
Mean Dep	.072	.137	.006	.064	.104	.040	$17,893.18

Note: Estimates show the percentage point effect of income shocks on four-year college enrollment by sector and selectivity as defined in the Carnegie Classification. 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. 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 A3: Alternative Bandwidths: Years 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, 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. 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 A4: 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 A5: 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 Acad Year Proxy	(7) No Sample Restrictions	(8) Include Grad Yr	(9) Cluster State-by-Year	(10) Population Weighted
Win 10-30k Pre-HS Grad	-0.0021	0.0014	-0.0024	-0.0016	-0.0011
	(0.0038)	(0.0032)	(0.0033)	(0.0037)	(0.0037)
Win 30-100k Pre-HS Grad	-0.0016	-0.0040	-0.0076	-0.0075	-0.0069
	(0.0067)	(0.0058)	(0.0058)	(0.0066)	(0.0066)
Win 100-300k Pre-HS Grad	0.0209**	0.0225***	0.0097	0.0188*	0.0194**
	(0.0096)	(0.0085)	(0.0084)	(0.0097)	(0.0098)
Win 300k-1.0m Pre-HS Grad	0.0555***	0.0670***	0.0415**	0.0539**	0.0533***
	(0.0204)	(0.0174)	(0.0179)	(0.0209)	(0.0210)
Win 1.0m or more Pre-HS Grad	0.1373***	0.0983***	0.1067***	0.1184***	0.1186***
	(0.0274)	(0.0230)	(0.0240)	(0.0256)	(0.0257)
Observations	1,461,262	1,691,357	1,617,679	1,461,262	1,461,262
Mean Dep	.268	.216	.216	.216	.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) a proxy for attendance in the academic year after high school graduation using the first two calendar years; (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) errors clustered at the state-by-year level; 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 A6: Four-Year College Attendance: Narrower Bins For Income Shocks

	(1)
Win 1-3k Pre-HS Grad	0.0024
	(0.0019)
Win 3-10k Pre-HS Grad	-0.0045*
	(0.0024)
Win 10-20k Pre-HS Grad	-0.0007
	(0.0044)
Win 20-30k Pre-HS Grad	-0.0049
	(0.0065)
Win 30-50k Pre-HS Grad	-0.0017
	(0.0105)
Win 50-100k Pre-HS Grad	-0.0039
	(0.0122)
Win 100-300k Pre-HS Grad	0.0249**
	(0.0125)
Win 300k-1.0m Pre-HS Grad	0.0525***
	(0.0203)
Win 1.0m-3.0m Pre-HS Grad	0.0912***
	(0.0344)
Win 3.0m or more Pre-HS Grad	0.1558***
	(0.0387)
Observations	1,461,262
Mean Dep	.219

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. 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. 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 A7: Four Year College Attendance: Robustness to Form 1098-T

	(1) Baseline	(2) Set to Zero	(3) Set to Missing	(4) Match Ed	(5) Match Ed Yr	(6) Ed Enroll
Win 10-30k Pre-HS Grad	-0.0016	-0.0017	-0.0009	0.0001	-0.0014	-0.0028
	(0.0035)	(0.0033)	(0.0036)	(0.0042)	(0.0032)	(0.0038)
Win 30-100k Pre-HS Grad	-0.0075	-0.0030	-0.0041	0.0011	-0.0186***	-0.0154**
	(0.0062)	(0.0058)	(0.0064)	(0.0075)	(0.0056)	(0.0067)
Win 100-300k Pre-HS Grad	0.0188**	0.0220***	0.0223**	0.0110	0.0002	0.0146
	(0.0091)	(0.0083)	(0.0093)	(0.0102)	(0.0080)	(0.0097)
Win 300k-1.0m Pre-HS Grad	0.0539***	0.0334*	0.0395*	0.0116	-0.0193	0.0383*
	(0.0195)	(0.0181)	(0.0202)	(0.0221)	(0.0168)	(0.0206)
Win 1.0m or more Pre-HS Grad	0.1184***	0.0963***	0.1093***	-0.1121***	-0.0734***	0.0801***
	(0.0257)	(0.0248)	(0.0279)	(0.0282)	(0.0203)	(0.0271)
Observations	1,461,262	1,461,262	1,308,674	1,461,262	1,461,262	1,461,262
Mean Dep	.215	.173	.193	.505	.179	.274

Note: The estimates examine the potential for bias generate by colleges that do not file a Form 1040 for students receiving a full scholarship. Column 1 presents estimates for all colleges. Column 2 sets enrollment to 0 for students attending non-filing colleges. Column 3 omits all students who attend non-filing colleges. Column 4 presents changes in enrollment reported to the Department of Education for financial aid reasons. Column 5 restricts reported enrollment to the year after high school graduation. Column 6 presents estimates from the union of 1098-T and Department of Education enrollment reports. 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. 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 two- and 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, 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. 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 Median Inc * Win Amount ($100k)	0.0065***		0.0052**
	(0.0024)		(0.0026)
Has Invest Inc * Win Amount ($100k)		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 Median Inc * Win > 100k	0.0526***		0.0433***
	(0.0148)		(0.0160)
Has Invest Inc * Win > 100k		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, 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 A10: Federal Financial Aid: Applications and Expected Family Contribution

	(1) FAFSA Application	(2) Expected Fam. Contr.
Win 10-30k Pre-HS Grad	-0.0059	$242.0450
	(0.0039)	(184.7606)
Win 30-100k Pre-HS Grad	-0.0201***	$1,620.0784***
	(0.0068)	(352.0651)
Win 100-300k Pre-HS Grad	-0.0083	$3,860.7219***
	(0.0095)	(582.6894)
Win 300k-1.0m Pre-HS Grad	-0.0106	$4,109.3422**
	(0.0204)	(1,614.9986)
Win 1.0m or more Pre-HS Grad	-0.1326***	$2,607.4711
	(0.0245)	(2,373.4101)
Observations	1,461,262	466,280
Mean Dep	.294	$7,380.04

Note: The estimates show changes in the rate of applying for federal aid and the expected family contribution. 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. 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 A11: Child 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, 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. 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.