USC Dornsife
Center for Economic and Social Research

USC Schaeffer
Leonard D. Schaeffer Center for Health Policy & Economics

Education, Decision-making, and Economic Rationality

James Banks, Leandro S. Carvalho, and Francisco Perez-Arce

Paper No: 2018-003

CESR-SCHAEFFER WORKING PAPER SERIES

Education, Decision-making, and Economic Rationality

*James Banks, Leandro S. Carvalho, and Francisco Perez-Arce**

This article studies the causal effect of education on decision-making. In 1972 England raised its minimum school-leaving age from 15 to 16 for students born after September 1, 1957. An online survey was conducted with 2,700 individuals born in a 36-month window on either side of this date. Participants made 25 incentivized risk choices that allow us to measure multiple dimensions of decision-making. Despite the policy having effects on education, educational qualifications, and income, we find no effects of the policy on decision-making or decision-making quality.

In many aspects of life, ranging from health to finances, the more educated have better outcomes than the less educated. One potential explanation is that education leads people to make better choices, a mechanism hypothesized for example to underlie the education-health gradient (Cutler and Lleras-Muney 2008, 2010) and consistent with correlational evidence that the more educated make higher-quality choices (Choi et al. 2014).

*Banks: Arthur Lewis Building-3.020, School of Social Sciences, The University of Manchester, Manchester, M13 9PL (email: james.banks@manchester.ac.uk); Carvalho: University of Southern California, Center for Economic and Social Research, 635 Downey Way, Los Angeles, CA 90089-3332 (leandro.carvalho@usc.edu); Perez-Arce: University of Southern California, 1909 K Street NW, Washington, DC, 20036 (perezarc@usc.edu). This paper benefited from discussions with Dan Benjamin, Shachar Kariv, Heather Royer, Dan Silverman, and participants in many seminars and conferences. A special thanks to Carla Blair, Samantha Luks, and Adrian Montero. This work was supported by the National Science Foundation (SES-1261040) and by the National Institute on Aging of the National Institutes of Health under Award Number 3P30AG024962-13W1. Banks is grateful to the ESRC-funded Centre for the Microeconomic Analysis of Public Policy at IFS (grant number RES-544-28-5001) for financial support. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

There are, however, two main challenges to determining whether more education leads to better choices. One is to make judgments about what good choices are. Differences in choices could reflect differences in decision-making ability but also differences in preferences, constraints, information, or beliefs. The second challenge is to isolate the causal effect of education on decision-making. There may be reverse causality – that is, better decision-makers may choose to invest more in education – while third factors, such as cognitive ability, may confound the relationship between past education choices and current decision-making.

In this paper, we investigate whether education improves decision-making by exploiting a well-known school-leaving age reform in England, using experimental risk choices to measure decision-making ability. We designed and administered an incentivized risk choice experiment that permits distinguishing differences in decision-making ability from differences in preferences or constraints. In order to exploit the school-leaving age reform, which affected only cohorts born after a specific date ("the cutoff"), we fielded this instrument via the Internet on a large general population sample born within three years around the cutoff. We study the causal effect of education on decision-making by comparing the decision-making of pre- and post-reform cohorts. Despite the schooling reform having effects on education and educational qualifications, and despite education and qualifications being (cross-sectionally) correlated with our measures of decision-making, we find no causal effects of education on decision-making or decision-making quality.

The 1972 Raising of the School Leaving Age Order (ROSLA) increased the minimum school-leaving age in England from 15 to 16. As a result, students born on or after September 1, 1957 had to stay in school until age 16 while students born before this date could leave at age 15. Previous studies have exploited compulsory schooling changes in England to study the causal effects of education on income (Oreopoulos 2006; Devereux and Hart 2010; Grenet 2013), health (Jürges et al. 2009; Clark and Royer 2013), and cognitive abilities (Banks and Mazzonna 2012). In order to exploit this natural experiment we carried out a study with 2,700 members of an Internet panel born between September 1, 1954 and August 31, 1960 and who left school at age 16 or younger. The study contained a module of incentivized experimental choices designed specifically to measure the impacts of the ROSLA on decision-making 40 years later.

Studies of individual differences in decision-making have used one of two approaches to assess poor decision-making. The "who is behavioral" approach (e.g., Benjamin et al. 2013) measures "behavioral anomalies", such as small-scale risk aversion, that are difficult to reconcile with rationality. The "who is rational" approach (e.g., Choi et al. 2014) measures decision-making quality by the consistency of choices with economic rationality. One desirable feature of the latter is that it enables one to "distinguish individual heterogeneity in decision-making ability from unobserved differences in preferences, constraints, information, or beliefs" (Choi et al. pg. 1520).

Our risk choice experiment was designed to combine these two approaches with the experimental choices yielding three different types of decision-making metrics. First, we can study the expected return and risk of the investment portfolios chosen by participants. Second, we can analyze measures of behavioral anomalies that are difficult to reconcile with rationality: small-scale risk aversion (Rabin 2000; Schechter 2007), the use of a 1/n heuristic (Benartzi and Thaler 2001; Huberman and Jiang 2006), and default effects (Madrian and Shea 2001; Choi et al. 2004). Finally, we measure decision-making quality by the consistency of choices with rationality, capturing both violations of the General Axiom of Revealed Preference (Choi et al. 2007a, 2007b, 2014; Echenique et al. 2011) and violations of monotonicity with respect to first-order stochastic dominance (Choi et al. 2014). We augment the set of measures of decision-making quality with a measure similar to "financial competence" proposed by Ambuehl et al. (2014) that is rooted in the principles of choice-based behavioral welfare analysis (Bernheim and Rangel 2004, 2009).

Consistent with previous studies (e.g., Clark and Royer 2013; Grenet 2013), our data show the reform increased educational attainment. The fraction of study participants staying in school until age 16 increased from 55 percent to 90 percent. The additional year of schooling kept students in high school courses for one more year and consequently more students received formal qualifications. The reform increased the fraction of study participants with a Certificate for Secondary Education (CSE) by 5.2 percentage points and the fraction with an O level by 6.5 percentage points (both of these qualification exams are typically taken at age 16). Overall, the fraction of participants without any formal qualification was reduced by 12 percentage points. Furthermore, we reproduce the finding, documented by previous studies, that the reform increased income (e.g., Harmon and Walker, 1995; Delaney & Devereux 2017; Dickson 2013; Grenet 2013) – an effect that persists more than four decades after the reform.

However, we do not find a causal effect of education on decision-making. Study participants born after September 1, 1957 make similar portfolio choices in terms of risk and return to those born before. There are also no differences in decision-making quality as defined by our various measures. In addition, "pre" and "post" reform groups are also equally likely to exhibit small-scale risk aversion, to remain at default portfolio allocations, or to use a 1/n heuristic. Not only do we find no significant effects, but also the confidence intervals around our estimates are tight enough to be informative.

Our results contribute to a growing literature investigating the characteristics associated with poor decision-making (e.g., Agarwal and Mazumder 2013; Choi et al. 2011; Benjamin et al. 2013; Choi et al. 2014; Cappelen et al. 2014; Stango et al. 2017). To the best of our knowledge, our study is the first to study the causal effect of general education on a large battery of measures of decision-making quality.[1] To the extent that our risk choice experiments are carried out in the context of financial portfolio decisions, our work is also related to studies such as Cole et al. (2014) and Black et al. (2015) that study the effects of education on financial portfolios. A contribution of our analysis over that of these studies is that our experimental methodology isolates the effect of education on decision-making quality, disentangling it from changes in underlying conditions or circumstances (for instance, Black et al. argue that impacts on risky asset ownership may arise from more educated being subsequently wealthier and thus more able or willing to take risks). Our work is also related to a growing literature on the effects of education on cognitive abilities (e.g., Banks and Mazzona 2012; Carlsson et al. 2015; Cascio and Lewis 2015; Lager et al. 2016; Gorman 2017).

The paper is structured as follows. Section I presents the study design and Section II evidence of the validity of the decision-making measures. Results of the effects of the 1972 reform on education and decision-making are presented in Section III. Concluding remarks are made in Section IV.

I. Study Design

To take advantage of the exogenous variation in education generated by the ROSLA, we

[1] Two studies examine the impacts of financial education on quality of decision-making. Ambuehl et al. (2014) study the impacts of short online educational videos about compound interest on financial competence while Lührmann et al. (2017) study the impact of financial education training on whether high school students making intertemporal choices allocate more money to the future in response to an increase in the interest rate.

surveyed approximately 2,700 members of the largest Internet panel in the UK, the YouGov Panel, between October 16, 2015 and February 1, 2016. In order to maximize statistical power, we recruited panel members more likely to have been affected by the policy: those who studied in England at age 14 and who dropped out at age 16 or younger.[2] We also restricted the sample to panel members born within a narrow window of three years around the cutoff date – namely those born between September 1, 1954 and August 31, 1960. With such a narrow window we are able to assume that there are no systematic birth cohort trends, which increases the effective sample size by a factor of 3 to 4 (Schochet 2009).[3]

There were two levels of screening. First, information that YouGov already had on file was

[2] Previous studies do not find an effect of the 1972 ROSLA on the likelihood of students staying in school until age 17 or older. According to Clark and Royer (2013), "the 1972 change had small, at best, effects on the fractions completing 11 or fewer years… one can view these law changes as forcing students that would previously have left at the earliest opportunity to stay in school for one more year." (pg. 2102) Banks and Mazzonna (2012) also focus on those who dropped out at 16 and younger to maximize power.

[3] Allowing for trends introduces a correlation between the running variable (i.e., date of birth) and the jumping variable (i.e., being born after September 1, 1957), which reduces the information contained in the jumping variable. In the results section we run such a specification as a robustness test.

used to determine which panel members should be invited to our survey.[4] Three screening questions (date of birth, school leaving age, and country of study at age 14) opened the survey and provided a second level of screening.[5] Respondents meeting the selection criteria made experimental risk choices (described in Section I.B) and answered a short survey containing five questions to assess understanding about the risk choice experiment, six questions to measure predetermined characteristics that should be balanced before and after September 1, 1957 (e.g., household size at age 10), and 4-5 questions to assess numeracy – see Appendix A for more details. All respondents received a £3 participation fee.

A. Risk Choice Experiment. Participants were presented with twenty-five choices, in each of which they had to allocate £25 among risky assets whose returns depended on a coin toss. They were shown the return per £1 invested depending on the outcome of the coin toss and were then asked to choose how much of the £25 they wanted to invest on each asset.

Appendix Figure 1 shows a screenshot of the interface presented to participants. The table shows the return per £1 invested for two assets – A and B – depending on the outcome of the coin toss. A graph below the table displays two bars: the first bar shows the amount invested on asset A and the second the amount invested on asset B. The starting level of the bars, which added to

[4] In the first level of screening the selection criteria were: 1) currently living in England; 2) born between September 1, 1954 and August 31, 1960; and 3) reported having left school at age 16 or younger.

[5] In the second level of screening the selection criteria were: 1) studied in England at age 14; 2) born between September 1954 and August 1960; and 3) reported having left school at age 16 or younger.

£25, was randomized.[6] Participants made their investments by either dragging the bars up and down or by clicking on the + and – buttons below the bars. When a participant changed the amount invested on one asset, the other bar automatically adjusted such that the total amount invested always equaled £25 – in other words, the participant could not keep any amount "uninvested."

One concern is that study participants often find risk choice experiments difficult (e.g., Eyster and Weizsäcker 2016). With this concern in mind, we designed a tutorial video to make the experiment as accessible as possible to study participants. The video, which was aimed at a general population, explained the experiment in non-technical terms and used animation to illustrate how to use the interface to make investment choices.[7] After the tutorial, participants had two rounds to practice. Even if the difficulty of the risk choice experiment may influence the *levels* of the decision-making quality measures, we are interested ultimately in *differences* of these measures between those born before and after September 1, 1957.

Participants were presented with 25 such choices (opportunity sets). The opportunity sets were designed such that they could be grouped into non-nested subsets of choices used to construct different measures of decision-making, as we explain in section I.B. The first 10 opportunity sets were presented in a simple frame where participants could invest in two assets only. In what follows, we refer to asset h as the asset that paid £x per £1 invested if the coin came up heads and £0 if it came up tails. We refer to asset t as the asset that paid £y per £1 invested if the coin came up tails and £0 if it came up tails. The returns x and y varied across opportunity sets. The order in

[6] For each opportunity set, two sets of starting levels for the bars were randomly drawn. Participants were randomly assigned to one of the two sets.

[7] http://youtu.be/VpUFDpdHlu8

which these two assets were presented on the screen from left to right was randomized, such that for half of the sample asset *h* showed up in the asset A column and for the other half asset *h* showed up in the asset B column.

The other 15 opportunity sets were presented in a more complex frame where participants could divide the investment amount across five assets (henceforth, the "complex frame"). In five of these, the opportunity sets were identical to some presented in the simple frame but with the addition of three superfluous assets produced from convex combinations of assets *h* and *t*.[8] This design, where new assets are introduced without effectively changing the investment opportunities, was proposed by Carvalho and Silverman (2017) and permits measuring "financial competence" using Ambuehl et al. (2014)'s measure of decision-making quality (discussed further in the next section). The remaining ten opportunity sets presented in the complex frame included assets *h* and *t* and three other assets that paid in both states of the world, where one or two of them lay below the efficient frontier and were therefore sub-optimal. The order in which the five assets were presented from left to right on the screen and the starting levels of the bars were randomized. Table 1 shows the 25 opportunity sets in the order they were presented to participants.[9]

Before participants started making their choices, they were shown a shorter second video explaining that 10% of participants would be randomly selected to receive an Amazon.co.uk Gift Certificate in the amount of the realized return of their investments in one of the 25 opportunity sets (randomly chosen).[10] All measures in monetary units were presented (and paid, where

---

[8] Additional details of the experiment are provided in Appendix B.

[9] We varied the columns in which assets were shown. The alternative presentation is shown in Appendix Table 1. Participants were randomly assigned to one of the two presentations.

[10] http://youtu.be/ZqVY8a_wmV8

relevant) to respondents in British pounds. For the purposes of exposition in this paper all amounts have been converted to US dollars using an exchange rate of $1.50 per pound. The average winnings amongst those selected to receive the gift voucher was $36.85, and the minimum and maximum were $0.90 and $135 respectively.

Previous studies have shown that even small-stakes experimental choices are predictive of real-life behaviors (Choi et al. 2014; Fisman et al. 2015). Moreover, Camerer and Hogarth (1999) review studies that varied the level of incentives and conclude that raising incentives does not change violations of rationality.

The median participant spent 6.8 minutes in the tutorials, 44 seconds in the two practice trials, and 13.7 minutes choosing their investments – compared to 11.3 minutes in Choi et al. (2013). The median duration of the entire survey was 32.75 minutes.

B. Decision-Making Measures The experimental choices are used to construct three types of decision-making measures. The first is the risk and return of portfolios. The second is measures of decision-making quality in the sense of consistency with rationality, irrespective of people's preferences. The third refers to well-documented behaviors that are hard to reconcile with rationality, such as the use of the 1/n heuristic, default stickiness, and small-scale risk aversion.

We examine five measures of quality of decision-making. First, we study whether the set of 25 choices violate the General Axiom of Revealed Preference (GARP). GARP requires that if a portfolio P_1 is revealed preferred to a portfolio P_2, then P_2 is not strictly and directly revealed preferred to P_1 (that is, at the prices at which P_2 is chosen, P_1 must cost at least as much as P_2). Choices that violate GARP are not consistent with rationality because there is no utility function that these choices maximize (Afriat 1972). We assess how closely individual choice behavior complies with GARP by using the Money Pump Index (Echenique et al. 2011), a metric commonly used in the microeconomics literature that captures the amount of money that could be arbitraged away from an individual whose choices violate GARP.

Choi et al. (2014) argue that consistency with GARP is a necessary but not sufficient condition for high quality decision-making. GARP-consistency does not rule out a choice of a portfolio that yields unambiguously lower payoffs than some available alternative portfolio. Violations of monotonicity with respect to first-order stochastic dominance (FOSD) provide another compelling criterion for decision-making quality. We use the difference between the maximal expected return (i.e., the highest expected return that can be achieved while holding the lowest payoff constant) and the expected return of the selected allocation to assess how closely individual choice behavior complies with the dominance principle (Hadar and Russell 1969). This measure is then averaged over opportunity sets.

Following Choi et al. (2014), we calculate a unified measure of violations of GARP and violations of FOSD by combining the 25 choices for a given participant with the mirror image of these data obtained by reversing the returns and the payoffs. We then compute the Money Pump Index for this combined dataset with 50 choices.

The fourth measure of quality of decision-making is financial competence (Ambuehl et al. 2014), a measure that compares the choices an individual makes when presented with the same opportunity set in a simple frame and in a complex frame. Following Carvalho and Silverman (2017), we conceptualize the complex frame as an investment problem where participants have a larger number of investment options but the opportunity set remains the same. Five opportunity sets were presented in both the simple and complex frames (see Section I.A). We calculate financial competence for a given opportunity set as the within-participant absolute difference in the amount invested in the high-paying state; this measure is then averaged over the five opportunity sets.

The fifth measure of decision-making quality captures whether participants failed to minimize portfolio risk. In two opportunity sets the return was the same for all assets, which implies that all portfolios yielded the same expected return. If a risk-averse rational agent were presented with a choice between portfolios with the same expected return, he would choose the one with the lowest risk. Given that risk-free portfolios were feasible in these two opportunity sets, we can use the portfolio risk (i.e., the standard deviation of portfolio returns) for the opportunity set as a measure of (low) quality of decision-making; this measure is then averaged over the two opportunity sets.

We also construct an overall index of decision-making quality, which is a simple respondent-level average of four measures of decision-making quality: GARP violations, FOSD violations, financial competence, and failure to minimize the portfolio risk.

Finally, we assess the occurrence of "behavioral anomalies", i.e. portfolio choices that are hard to reconcile with rationality. Expected utility theory predicts that individuals will be approximately risk-neutral when stakes are small. Empirically, however, it is often found that individuals are risk-averse even when stakes are small (Rabin 2000; Schechter 2007). Small-scale risk aversion is measured as the portfolio return in the low-paying state (a risk neutral agent would invest $0 in the low-paying state). Some investors may excessively diversify their portfolios by using a 1/n heuristic where they divide the investment amount evenly among the n investment options available (Benartzi and Thaler 2001; Huberman and Jiang 2006). Our measure of this is the fraction of times that the participant invested one half (fifth) of the endowment in each one of the two (five) assets available. Finally, a number of studies have shown that many people tend to stick to defaults. For example, defaults in 401(k) retirement plans have large effects on participation rates, contribution rates, and asset allocation choices (e.g., Madrian and Shea 2001; Choi et al. 2004).

We measure default stickiness by the fraction of times that the participant remained at the default (starting) allocation.

As explained in Section I.A, the opportunity sets were designed such that they could be grouped into non-nested subsets of choices used to construct the different measures of decision-making. Expected return, FOSD violations, and 1/n heuristic are constructed using 23 opportunity sets: the two opportunity sets where the expected return is the same for all assets are excluded. Portfolio risk, GARP violations, small-scale risk aversion, default stickiness, and the unified measure of GARP and FOSD violations are constructed using all 25 opportunity sets. See Appendix B for more details about the construction of the decision-making variables.

II. Descriptive Evidence on the Distribution of the Decision-making Outcomes

This section presents descriptive evidence on the decision-making outcomes. In order to avoid contamination by the education reform the analyses in this section are presented for the "pre-reform" sample only, i.e. those born before September 1, 1957 (N = 1,416).

We begin by discussing evidence about participants' understanding of the risk choice experiment. After making their investment choices, participants were asked five questions to assess their understanding. They were shown an example of an investment allocation on five assets and asked questions about the example. Participants who could correctly answer all five questions earned £1 to add to a £3 participation fee.

Most participants seem to have understood the risk choice experiment. More than ninety percent knew the amount they had to invest (93%) and the amount invested in the example on a particular asset (95%). More than sixty percent could correctly identify the state-specific return of investing £1 (77%) or £10 (64%) on an asset. More than half (51%) could calculate the state-specific return of the portfolio allocation shown in the example, which involved five multiplications and adding the five products. We find that cohorts born before and after September 1, 1957 exhibit similar understanding of the risk choice experiment – see Appendix Table 3.

Despite the fact that participants seemed to have understood the risk choice experiment, they often made suboptimal investment choices. Table 2 shows summary statistics – mean; 25th, 50th, and 75th percentiles; and standard deviation – of the different measures of decision-making quality described above. The values can be interpreted as the amount of money participants "left on the table" by making suboptimal investment choices, and are denoted as negative values such that higher values (closer to zero) correspond to higher decision-making quality.

We estimate that low quality decision-making cost study participants on average between $2.77 and $6.61, depending on the measure used. This corresponds to 7.4%-17.6% of the amount participants had to invest. It is interesting to note that for financial competence even the 75th percentile is high: a loss of $3.97.

Choi et al. (2014) propose two exercises to investigate whether decision-making quality measured from experimental choices reflect decision-making ability that affects real-world outcomes. First, they examine the correlation between decision-making quality and socioeconomic characteristics. Second, they investigate whether differences in decision-making quality explain differences in real-world outcomes, using wealth as a real-world economic outcome.

In keeping with the findings of Choi et al. (2014), our data show that decision-making quality is associated with education, numeracy, and income. Figure 1 shows cumulative distribution functions of decision-making quality – violations of GARP (row 1) and financial competence (row 2) – separately for those with and without a formal qualification (column A), low and high numeracy (column B), and low and high income (column C).[11]

Participants with more education, higher numeracy, and higher income make higher-quality choices than their peers. The relationships are stronger for GARP violations than for financial competence. These associations are even more striking if one considers that that the pre-reform subsample is considerably homogenous because of the sampling design: they were all born within a 36-month window, studied in England at age 14, and reported finishing continuous full-time education at age 16 or younger.

In rows 3 and 4 we conduct a similar analysis for two of our measures of behavioral anomalies, namely the 1/n heuristic and for default stickiness. The 1/n heuristic is measured as the fraction of times that the participant invested one half (fifth) of the endowment in each one of the two (five) assets available. Default stickiness is measured as the fraction of times that the participant remained at the default (starting) allocation.

The relationship of behavioral anomalies with education and numeracy are not as clear as for the measures of decision-making quality. While those with more education and higher numeracy are less likely to remain at the default portfolio allocation, they are also *more* likely to divide the investment amount evenly among the investment options. One possibility is that the more educated feel more confident to move away from default allocations but perceive the 1/n heuristic as a sophisticated investment strategy to diversify risk. This speculation illustrates the challenges in unambiguously characterizing behavioral anomalies as mistakes. It is also interesting to note that there is substantial variation across individuals in the frequency with which they use these

[11] "High income" is defined as having an annual household income of £25,000 or more. "High numeracy" is defined as having correctly answered the 3 numeracy questions.

strategies.

The measure of default stickiness is also a useful way to assess attention during the risk choice experiment. A disengaged participant could hit “next” without moving the bars, which happened in 9.4% of the choices. Another marker of inattention is the failure to minimize risk. The two assets available in the fifth choice had the same return, such that any portfolio yielded the same expected return. In other words, there was no risk-return tradeoff. A risk-averse agent would invest half of the endowment on each asset to minimize risk. Even though the bars started away from this allocation, 63% of the sample implemented it (notice risk-neutral agents would be indifferent between allocations). We can also easily reject the null that the decision-making quality of participants' choices is as good as if they had been choosing at random, a standard benchmark used in the literature (results available upon request).

Next, we investigate whether differences in decision-making quality are associated with differences in home ownership, our proxy for wealth.[12] Figure 2 shows that homeowners exhibit

[12] We do not have monetary measures of wealth available to us. Homeownership is often used as a proxy for wealth when only two groupings are required and the correlation is strong. Calculations from the 2014/15 English Longitudinal Study of Ageing, which contains detailed measures of both variables, confirm this. For the cohort born between 1955 and 1960, the 75th percentile of net financial assets (not including housing or pension wealth) for non-homeowners was £5,000, which compares to value of £6740 for the 25th percentile of net financial wealth for homeowners. The 90th percentile of this measure of wealth for non-homeowners is around 80% of the median wealth of home owners, so less than ten-percent of renters would be observed in the top half of the wealth distribution of homeowners.

higher decision-making quality measured in terms of GARP violations than renters. However, this is not true for financial competence.

Finally, in Appendix Table 2 we show that, even though the measures of decision-making quality capture different dimensions of poor decision-making, they are strongly correlated with each other. For example, violations of FOSD capture whether participants chose portfolios that yielded unambiguously lower payoffs, while our *failure to minimize risk* measure captures the average portfolio risk when all portfolios yielded the same expected return. There is no overlap in the opportunity sets used to compute these two decision-making quality measures, yet the correlation between them is 0.37.[13] The correlation coefficients between the different measures of decision-making quality range from 0.26 to 0.94, which indicates that there is some common component of decision-making ability that these different measures are capturing.

III. The Impacts of the Compulsory Schooling Changes

A. Impacts on Education. The ROSLA generated a discontinuous relationship between education and date of birth. Figure 3 shows the fraction of study participants that stayed in school until age 16 by quarter of birth (all other study participants dropped out at age 15 or younger). The 1972 ROSLA raised the compulsory school leaving age in England from 15 to 16 years. The vertical dotted line denotes students born between September and November of 1957, the first cohort subject to the change in the compulsory schooling law. The education reform increased the fraction of study participants who stayed in school until age 16 in 35 percentage points.

As discussed in Section I, we recruited panel members born within a narrow window of 72

[13] Failure to minimize risk is calculated using only the two opportunity sets where all portfolios yielded the same expected return.

months around September 1, 1957 because the effective sample size increases by a factor of 3-4 if the birth cohort trends can be assumed to be approximately zero. For most of the analysis here, we ignore the birth cohort trends and compare means for study participants born after September 1, 1957 to the means for study participants born before this date. In Table 6 we estimate regressions that allow for birth cohort trends (in all 27 specifications we cannot reject that there are no birth cohort trends).

One may worry that the ROSLA forced students to attend school, but that these students may not have learned much if they were not putting effort. The evidence does not support this hypothesis. Figure 4 shows the distribution of highest qualification, separately for study participants born before and after September 1, 1957. The reform reduced the fraction of study participants without a formal qualification and increased the fraction with a Certificate of Secondary Education (CSE) and the fraction with a General Certificate of Education (GCE) Ordinary Level (also known as an O level).[14]

[14] One feature of education data in this cohort in England is the rather coarse relationship between education as measured by self-reported "age left full-time education" and education as measured by highest qualification attained. Figure 4 shows that, even in the pre-reform cohort, roughly thirty percent of our sample achieved some higher qualifications (either A-levels, typically taken at age 18 if in full-time education, or some kind of college degree) despite the sample being selected on the basis of having left full-time education at age 16 or before. Calculations from other nationally representative surveys confirm this is a feature of the population not just our sample. In the 2015 Labour Force Survey, 26.0% of the individuals born between 1955 and 1960 who reported that they left full-time education at age 16 or before report

Table 3 estimates the effects of the compulsory schooling law change on educational attainment. Each row shows results from a separate regression. We run regressions of the educational attainment outcomes listed in the rows on a dummy for being born after September 1, 1957 and a constant. The first column shows the coefficient on the constant, which corresponds to the mean of the outcome variable among participants born before September 1, 1957. The second column shows the coefficient on the dummy for being born after September 1, 1957, which corresponds to the difference in means between those born before and after the cutoff birthdate.

The education reform increased the fraction of study participants staying in school until age 16 by 35 percentage points or 63%. It also increased the fraction of study participants with a CSE by 5.2 percentage points and the fraction of those with a GCE O level by 6.5 percentage points, reducing the fraction without any formal qualification by 12 percentage points. The CSE and the GCE O level were examinations that students would typically take around age 16 and hence are the qualifications that one would expect to be affected by the change in the compulsory school leaving age from 15 to 16.

We are also able to reproduce the finding of previous studies showing that the education reform increased income (e.g., Dickson 2013; Grenet 2013; Delaney & Devereux 2017). Figure 5 shows the cumulative distribution function of personal income, separately for study participants born before and after September 1, 1957. Appendix Figure 5 shows the cumulative distribution function of household income. In both cases, the distribution for those born after September 1, 1957 is further to the right, indicating that they have higher incomes than those born before September 1,

---

qualifications of A-level or higher (the equivalent proportion in the 2014/15 English Longitudinal Study of Ageing is 25.7%).

1957. The income data were collected between June 2013 and January 2016 – i.e., more than four decades after the reform. Despite some limitations of these data, we can reject the null of a Wilcoxon rank-sum test at the 5% significance level for both personal and household income.[15]

Before presenting the estimates of the effects on decision-making, we investigate whether predetermined characteristics (i.e., characteristics determined before the ROSLA) are balanced across study participants born before and after the cutoff birthdate. The identifying assumption is that study participants born before and after September 1, 1957 would have had similar decision-making ability had the latter group not been forced to stay in school until age 16.

We test this assumption in Table 4. Each row shows a separate regression. We run regressions of the variables listed on the rows on a dummy for being born after September 1, 1957 and a constant. The first column shows the coefficient on the constant, which corresponds to the mean of the dependent variable among participants born before September 1, 1957. The second column shows the coefficient on the dummy for being born after September 1, 1957, which corresponds to the difference in means between those born before and after this date.

Overall the predetermined characteristics are balanced across the two groups. For only one of 18 predetermined characteristics we can reject the null hypothesis that the mean is the same across

[15] We did not collect data on income in our survey, but YouGov had asked in previous waves about panel members' incomes. The data have a few shortcomings: the measures come from a single question about "total income"; respondents answered by choosing one of thirteen income brackets; approximately a fifth of the sample answered "do not know" or "prefer not to answer"; and income was measured at different points in time for different panel members. We would expect these issues to make it harder to detect an effect of the reform on income.

the two groups – participants born after September 1, 1957 are 2.6 percentage points more likely to have lived at age 10 in a place with enough books to fill two or more bookcases. This difference is statistically significant at 10%. Moreover, all variables in Table 4 cannot jointly predict whether a study participant was born after the cutoff birthdate (p-value 0.45). In Section III.B we show that estimates barely change, and our qualitative conclusions are unaffected, if we control for these predetermined variables.

Despite the internal validity of our estimates, one may worry about their external validity because of our online sample. In Appendix Table 5 we compare compliers in the YouGov sample to compliers in the English Longitudinal Study of Ageing (ELSA), a nationally representative survey, where we represent the compliers by restricting the sample to participants born before September 1, 1957 who left school before age 16 (after the education reform they would have been forced to stay in school until age 16). We compare the answers to questions that we borrowed from ELSA, such that there are no concerns about differences in the wording of the questions. Overall, the two samples look quite comparable.

In the next section, we investigate whether the exogenous increase in education generated by the education reform caused an increase in decision-making.

B. Impacts on Decision-Making. Figure 3 showed that the ROSLA created a discontinuous relationship between educational attainment and date of birth at September 1, 1957. If education has a causal effect on decision-making, we would expect to see a corresponding discontinuity between decision-making and date of birth at the same cutoff birthdate.

We first analyze portfolio choices, remaining agnostic about the quality of these decisions. Figure 6 shows the relationship between expected return and portfolio risk and date of birth. The circles show average expected return (left y-axis) by quarter of birth. The Xs show average portfolio risk (right y-axis) by quarter of birth. For both outcomes there is no apparent discontinuity at the birthdate cutoff. The figure also indicates that both at the left and at the right of the cutoff there is no clear birth cohort trends or aging effects, which lends support to the assumption that birth cohort trends are approximately zero for such a narrow window.

Figure 7 shows cumulative distributions of three measures of decision-making quality – violations of the GARP, financial competence, and the decision-making quality index – separately for those born before and after September 1, 1957. These numbers can be contrasted with the $37.50 participants had to invest. The red dashed lines show cumulative distributions for those born before September 1, 1957. The black solid lines show cumulative distributions for those born after September 1, 1957. In the top and bottom panels the two distributions are strikingly similar. The p-values of a Wilcoxon rank-sum test of equality of distributions are: 0.63 (GARP); 0.12 (financial competence); and 0.90 (decision-making quality index).

Table 5 presents estimates of the effects of education on decision-making. The first column shows the mean of the dependent variables in US dollars (with the exception of the last two rows) for the pre-reform sample. For the other columns, each cell corresponds to a separate regression with the dependent variables listed on the rows. The second and third columns show reduced-form estimates where the independent variable is an indicator for being born after September 1, 1957. The fourth and fifth columns show two stages least squares (2SLS) estimates, where we use the dummy for being born after September 1, 1957 to instrument for staying in school until age 16. The 2SLS estimator is the ratio of the reduced-form estimates shown in columns 2 and 3 and the first-stage estimate 0.348 (i.e., the first row of Table 3 showed that the school reform increased the fraction of study participants staying in school until age 16 in 34.8 percentage points). The second and fourth columns present estimates based on monetary amounts as in the previous descriptive analysis. For estimates in the third and fifth columns the dependent variables are in standard deviation units—constructed by subtracting off the mean, and dividing by the standard deviation, of the outcome variable in the pre-reform group.[16]

We find no effects of education on the portfolio choices. An additional year of schooling is associated with a *reduction* in the expected return in \$0.10 or 0.04 of a standard deviation. There is also no effect of education on portfolio risk.

We do not find evidence of effects of education on decision-making quality. For all six measures, the effect of education on decision-making quality is not statistically different from zero.[17] We also note that the point estimates do not all go in one direction. The unified measure of GARP and FOSD violations and the failure to minimize risk measure indicate that education *improves* decision-making quality while GARP, FOSD, and financial competence imply that education *deteriorates* decision-making quality. Similarly, we find no effect of education on small-scale risk aversion, the use of the 1/n heuristic, or default stickiness.

The 2SLS effects are estimated relatively precisely with a standard error of 0.09-0.12 of a standard deviation. The upper bounds of the 95% confidence intervals range from 0.04 to 0.27 of a standard deviation. In other words, we can rule out that an additional year of schooling improves

[16] For the decision-making quality index, we calculated the simple average of the standardized GARP violations, the standardized FOSD violations, the standardized financial competence, and the standardized failure to minimize risk.

[17] Afriat's Critical Cost Efficiency Index (CCEI) provides an alternative measure of how closely individual choice behavior complies with GARP. We also find no effect of education on violations of GARP if we use the CCEI instead of the Money Pump Index.

decision-making quality by 0.28 of a standard deviation or more. For the decision-making quality index, we can rule out effects greater than 0.09 of a standard deviation.

Consider the following back-of-envelope calculation to assess the potential consequences of these estimates for real-world outcomes. Choi et al. (2014) estimate that a one standard deviation increase in Afriat's Critical Cost Efficiency Index (CCEI) is associated with an 18% increase in wealth. If we use CCEI as our dependent variable, the upper bound of the 95% confidence interval implies that an additional year of schooling increases CCEI by 0.13 of a standard deviation (result not shown).[18] In other words, the improvement in decision-making quality caused by an additional of year schooling increases household wealth by *no more than* 2.3%. As a comparison, Black et al. (2015) estimate that an additional year of schooling increases wealth by 18. Taken at face value, these two set of results together suggest that less than 13% of the effect of education on wealth is due to the effect of education on decision-making quality.

Table 6 assesses the robustness of Table 5 results to alternative specifications. The last column of Table 5 is reproduced as the first column of Table 6 to facilitate the comparison. The estimates in the second column of Table 6 include the predetermined characteristics analyzed in Table 4. The third column adds a linear function of date of birth in days and an interaction of date of birth in days with a dummy for being born after September 1, 1957—that is, the birth cohort trends are allowed to be different for those born before and after September 1, 1957. Standard errors are clustered at the month-year of birth in the last two columns. Finally, the last column includes month-of-birth dummies to control for seasonality (these effects are assumed to be the same for

[18] Though we use the Money Pump Index throughout the paper, we use here the CCEI for comparability with the results in Choi et al (2014).

those born before and after September 1, 1957).

Estimates in the second column show that adding the predetermined characteristics has small effects on point estimates and on standard errors. Except for FOSD where the coefficient is significantly negative at the 10% level (which would suggest education worsens decision-making quality), all other coefficients remain insignificant. The estimates are more sensitive to the inclusion of birth cohort trends (third column). Violations of GARP, financial competence, and the decision-making quality index change from a negative to a positive sign. Including birth cohort trends increase the standard errors, from 0.09-0.12 of a standard deviation to 0.14-0.19 of a standard deviation. With just one exception, clustering at the month-year of birth reduces standard errors (fourth column). Finally, some results are also sensitive to the inclusion of month-of-birth dummies, as the last column shows. With just one exception (the effect on GARP violations in column 5), the effects remain statistically undistinguishable from zero. It is worth pointing out that in all 27 specifications the birth cohort trends are not jointly significant at a 10% significance level. In Appendix Table 4 we show our results are also robust to controlling for the amount of time (in levels or in logs) that study participants spent making their investment choices.

We also investigated whether our estimated effects vary by parental background. Our measure of parental background is the first principal component of a principal component analysis of several pre-determined characteristics.[19] We then broke the sample in two based on whether the

[19] The characteristics were: whether respondent lived most of her childhood with both parents, number of books in place respondent lived (at age 10), number of bedrooms in residence (at age 10), number of people lived with respondent (at age 10), whether parents were unemployed for more than 6 months (before age 14), and caregiver's main occupation (at age 14).

participant had a parental background below or above the median, and estimated separate effects for these two groups. We find no effect of staying in school until age 16 on decision-making quality for either group. We cannot reject the null hypothesis that that the effects are the same for the two groups (results available upon request).

IV. Conclusion

In many aspects of life, ranging from health to finances, the more educated have better outcomes than the less educated. One potential explanation is that education leads people to make better choices. To our knowledge this paper is the first to test this hypothesis. In this paper we combined the 1972 school leaving age reform in England with experimental choices to investigate whether education has a causal effect on decision-making quality. The risk choice experiment is designed to differences in decision-making ability from differences in constraints and preferences. We fielded this instrument on 2,700 members of an Internet panel born within three years on either side of the reform date, specifically to measure the impacts of the 1972 reform on decision-making. We study the causal effects of the reform by comparing the decision-making of the pre- and post-reform cohorts. While this identification strategy has been used to good effect to study the causal effects of education on other outcomes, this study is the first to provide evidence on the causal effect of education on decision-making ability.

Despite the policy having effects on education, and educational qualifications, and income (confirming well-known results of other studies), we find no effects of the policy on decision-making or decision-making quality. We can rule out that an additional year of schooling improves decision-making quality by more than 0.09 of a standard deviation.

Previous studies had documented associations between education and decision-making quality (Choi et al. 2014) and between education and behavioral anomalies (e.g., Benjamin et al. 2013;

Stango et al. 2017). Our contribution over these studies is to show that such correlations do not hold up in the specific causal setting we study.

To the extent that our risk choice experiments are carried out in the context of financial portfolio decisions, our work is also related to Cole et al. (2014) and Black et al. (2015) that find that compulsory schooling changes in the U.S. and in Sweden increased financial market participation and investments in risky assets. More educated individuals, who earn higher wages and accumulate more wealth, can afford to save and invest more, and may also have access to better investment opportunities. Looking at actual portfolio outcomes inevitably confounds these "wealth effects" with any effects education may have on decision-making ability and preferences. Our experimental methodology isolates the effect of education on decision-making quality, disentangling it from changes in underlying conditions or circumstances. Our results suggest that there is no evidence for causal effects of education on decision-making ability and hence that wealth effects may well be the channel via which the effects of education were appearing in previous studies.

Our results contrast with findings that education improves performance in cognitive ability tests (e.g., Banks and Mazzona 2012; Carlsson et al. 2015; Cascio and Lewis 2015; Lager et al. 2016; Gorman 2017). Although the capacity to make high-quality choices may partly depend on skills that are captured by these tests, Choi et al. (2014) argue that decision-making ability and cognitive abilities are conceptually different constructs and show that empirically a "measure of cognitive ability [performance in the Cognitive Reflection Test] cannot be used as a simple substitute for the CCEI for the purposes of explaining wealth." (pg. 1543)

It is important to note that our estimates apply to a specific education reform that took place at a particular point in time for pupils at a relatively young age. For policymakers and researchers interested in improving individuals' choices, there is still much more that needs to be understood about the potential effects of education outside of the context of this specific reform. In particular, other types of education or curriculum change might well have different effects to simply an additional year of high school education. Our measures are constructed from financial choices under uncertainty, so it is indeed possible that a curriculum targeted to financial capability or quantitative reasoning and decision-making more generally could yield different results. Thinking about how to design an education intervention that could deliver better decision-making, or indeed evaluating more targeted education interventions with a framework and methods such as we use here, would both be excellent directions for future research.

REFERENCES

Afriat, Sydney. 1972. "Efficiency Estimates of Production Functions." *International Economic Review* 13(3): 568-598.

Ambuehl, Sandro, B. Douglas Bernheim, and Annamaria Lusardi. 2014. "Financial Education, Financial Competence, and Consumer Welfare." NBER Working Paper 20618.

Banks, James, and Fabrizio Mazzonna. 2012 "The effect of education on old age cognitive abilities: evidence from a regression discontinuity design." *The Economic Journal* 122(560): 418-448.

Benartzi, Shlomo, and Richard H. Thaler. 2001. "Naïve Diversification Strategies in Defined Contribution Saving Plans." *American Economic Review* 91(1): 79-98.

Benjamin, Daniel J., Sebastian A. Brown, and Jesse M. Shapiro. 2013. "Who is 'Behavioral'? Cognitive Ability and Anomalous Preferences." *Journal of the European Economic Association* 11(6): 1231-1255.

Bernheim, B. Douglas and Antonio Rangel. 2004. "Addiction and Cue-triggered Decision Processes." *American Economic Review* 94(5): 1558-1590.

Bernheim, B. Douglas and Antonio Rangel. 2009."Beyond Revealed Preference: Choice-theoretic

Foundations for Behavioral Welfare Economics” *Quarterly Journal of Economics* 124(1):51-104.

Black, Sandra E., Paul J. Devereux, Petter Lundborg, and Kaveh Majlesi. 2015. “Learning to Take Risks? The Effect of Education on Risk-Taking in Financial Markets.” NBER Working Paper 21043.

Burks, Stephen V., Jeffrey P. Carpenter, Lorenz Goette, and Aldo Rustichini. 2009. “Cognitive Skills Affect Economic Preferences, Strategic Behavior, and Job Attachment.” *Proceedings of the National Academy of Sciences of the United States of America* 106(19): 7745-7750.

Camerer, Colin F., and Robin M. Hogarth. 1999. "The effects of financial incentives in experiments: A review and capital-labor-production framework." *Journal of risk and uncertainty* 19(1-3): 7-42.

Carlsson, M., Dahl, G.B., Öckert, B. and Rooth, D.O., 2015. The effect of schooling on cognitive skills. Review of Economics and Statistics, 97(3), pp.533-547.

Carvalho, Leandro S., and Dan Silverman. 2017. “Complexity and Sophistication.” Unpublished manuscript.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick. 2004. “For Better or Worse: Default Effects and 401(K) Savings Behavior.” In Perspectives on the Economics of Aging, ed. David A. Wise, 81–121. Chicago and London: University of Chicago Press.

Choi, Syngjoo, Raymond Fisman, Douglas Gale, and Shachar Kariv. 2007. "Consistency and Heterogeneity of Individual Behavior Under Uncertainty." *American Economic Review* 97(5): 1921-1938.

Choi, Syngjoo, Raymond Fisman, Douglas Gale, and Shachar Kariv. 2007. "Revealing Preferences Graphically: An Old Method Gets a New Tool Kit." *American Economic Review* 97(2): 153-158.

Choi, Syngjoo, Shachar Kariv, Wieland Müller, and Dan Silverman. 2014. “Who Is (More) Rational?” *American Economic Review* 104(6): 1518-1550.

Clark, Damon, and Heather Royer. 2013. "The effect of education on adult mortality and health and: Evidence from Britain." *American Economic Review* 103(5): 2087-2120.

Cole, Shawn, Ann Paulson, and Gauri K. Shastry. 2014. "Smart Money? The Effect of Education on Financial Outcomes." *The Review of Financial Studies* 27(7): 2022-2051.

Cutler, David M., and Adriana Lleras-Muney. 2008. Education and Health: Evaluating theories and evidence in Making Americans Healthier: Social and Economics Policy as Health Policy, Robert F. Schoeni, James S. House, George Kaplan and Harold Pollack, editors, New York: Russell Sage Foundation.

Cutler, David M., and Adriana Lleras-Muney. 2010. "Understanding Differences in Health Behaviors by Education." *Journal of Health Economics* 29(1): 1-28.

Delaney, J. and Devereux, P., 2017. More Education, Less Volatility? The Effect of Education on Earnings Volatility over the Life Cycle.

Devereux, Paul J. and Robert A. Hart. 2010. "Forced to be Rich? Returns to Compulsory Schooling in Britain." *The Economic Journal* 120(549): 1345-1364.

Dickson, M., 2013. The causal effect of education on wages revisited. Oxford Bulletin of Economics and Statistics, 75(4), pp.477-498.

Dohmen, Thomas, Armin Falk, David Huffman, Felix Marklein, and Uwe Sunde. 2009. "Biased probability judgment: Evidence of incidence and relationship to economic outcomes from a representative sample." *Journal of Economic Behavior & Organization* 72(3): 903-915.

Dohmen, Thomas, Armin Falk, David Huffman, and Uwe Sunde. 2010. "Are Risk Aversion and Impatience Related to Cognitive Ability?" *American Economic Review* 100(3): 1238-1260.

Echenique, Federico, Sangmok Lee, and Matthew Shum. 2011. "The Money Pump as a Measure of Revealed Preference Violations." *Journal of Political Economy* 119 (6): 1201–123.

Eyster, E. and Weizsacker, G., 2016. Correlation Neglect in Portfolio Choice: Lab Evidence.

Fisman, Raymond, Pamela Jakiela, Shachar Kariv, and Daniel Markovits. 2015. "The distributional preferences of an elite." *Science* 349(6254): aab0096.

Goda, Gopi S., Matthew R. Levy, Colleen F. Manchester, Aaron Sojourner, and Joshua Tasoff. 2015. “The Role of Time Preferences and Exponential-Growth Bias in Retirement Savings.” NBER Working Paper 21482.

Grenet, Julien. 2013. "Is extending compulsory schooling alone enough to raise earnings? Evidence from French and British compulsory schooling laws." *The Scandinavian Journal of Economics* 115(1): 176-210.

Hadar, Josef, and William R. Russell. 1969. “Rules for Ordering Uncertain Prospects.” *American Economic Review* 59(1): 25-34.

Harmon, C. and Walker, I., 1995. Estimates of the economic return to schooling for the United Kingdom. The American Economic Review, 85(5), pp.1278-1286.

Huberman, G., and W. Jiang. 2006. “Offering versus Choice in 401(k) Plans: Equity Exposure and Number of Funds.” *The Journal of Finance* 61(2): 763-801.

Jürges, Hendrik, Eberhard Kruk and Steffen Reinhold. 2013. “The effect of compulsory schooling on health—evidence from biomarkers.” *Journal of Population Economics* 26: 645–672.

Kariv, Shachar, and Dan Silverman. 2013. "An Old Measure of Decision-Making Quality Sheds New Light on Paternalism." *Journal of Institutional and Theoretical Economics* 169(1): 29-44.

Lager, A., Seblova, D., Falkstedt, D. and Lövdén, M., 2016. Cognitive and emotional outcomes after prolonged education: a quasi-experiment on 320 182 Swedish boys. International journal of epidemiology, 46(1), pp.303-311.

Lührmann, Melanie, Marta Serra-Garcia, and Joachim Winter. 2017. "The Impact of Financial

Education on Adolescents' Intertemporal Choices." Unpublished manuscript.

Lusardi, Annamaria, and Olivia S. Mitchell. 2014. "The economic importance of financial literacy: Theory and evidence." *Journal of Economic Literature* 52(1): 5-44.

Madrian, Brigitte, and Dennis F. Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *Quarterly Journal of Economics* 116(4): 1149–87.

Oreopoulos, Philip. 2006. "Estimating average and local average treatment effects of education when compulsory schooling laws really matter." *American Economic Review* 96(1): 152-175.

Rabin, Matthew. 2000. "Risk Aversion and Expected-Utility Theory: A Calibration Theorem," *Econometrica* 68(5): 1281-92.

Rabin, Matthew, and Georg Weizsacker. 2009. "Narrow Bracketing and Dominated Choices." *American Economic Review* 99(4): 1508–1543.

Schechter, Laura. 2007. "Risk Aversion and Expected-Utility Theory: A Calibration Exercise." *Journal of Risk and Uncertainty* 35(1): 67-76.

Schochet, Peter Z. 2009. "Statistical Power for Regression Discontinuity Designs in Education Evaluations." *Journal of Educational and Behavioral Statistics* 34(2): 238-266.

Stango, Victor, Joanne Yoong, and Jonathan Zinman. 2016. "We are all behavioral, more or less: New consumer-level summary statistics for multiple behavioral factors." Unpublished manuscript.

Stephens Jr., Melvin, and Dou-Yan Yang. 2014. "Compulsory Education and the Benefits of Schooling." *American Economic Review* 104(6): 1777-1792.

Tversky, Amos, and Daniel Kahneman. 1981. "The Framing of Decisions and the Psychology of Choice." *Science* 211(4481): 453–458.

Table 1: Opportunity Sets

	Contingent Asset Returns per £1 Invested									
	A		B		C		D		E	
	Heads	Tails	Heads	Tails	Heads	Tails	Heads	Tails	Heads	Tails
Opportunity Set										
1	£2.40	£0.00	£0.00	£0.80						
2	£0.00	£1.20	£2.40	£0.00						
3	£0.00	£0.80	£3.20	£0.00						
4	£0.00	£3.00	£1.50	£0.00						
5	£1.60	£0.00	£0.00	£1.60						
6	£0.00	£3.20	£0.80	£0.00						
7	£3.00	£0.00	£0.00	£1.50						
8	£1.20	£0.00	£0.00	£2.40						
9	£2.00	£0.00	£0.00	£1.80						
10	£0.00	£2.40	£0.80	£0.00						
11	£0.20	£1.80	£0.80	£0.00	£0.40	£1.20	£0.00	£2.40	£0.60	£0.60
12	£1.20	£0.50	£0.50	£1.00	£0.00	£2.00	£0.25	£1.40	£1.60	£0.00
13	£0.25	£1.00	£1.50	£0.40	£1.00	£0.60	£0.00	£1.60	£2.00	£0.00
14	£0.30	£0.30	£1.20	£0.00	£0.00	£3.60	£0.30	£2.70	£0.60	£1.80
15	£0.60	£0.60	£1.20	£0.40	£2.40	£0.00	£1.80	£0.20	£0.00	£0.80
16	£0.75	£1.50	£2.30	£0.25	£1.30	£0.80	£3.00	£0.00	£0.00	£2.00
17	£1.20	£0.00	£0.30	£0.90	£0.50	£0.50	£0.00	£1.20	£0.90	£0.10
18	£0.00	£1.20	£0.60	£0.90	£1.80	£0.30	£1.20	£0.60	£2.40	£0.00
19	£1.20	£0.40	£0.40	£1.20	£0.80	£0.80	£0.00	£1.60	£1.60	£0.00
20	£0.25	£2.30	£1.00	£1.50	£2.00	£0.00	£1.30	£0.50	£0.00	£3.00
21	£2.40	£0.00	£1.80	£0.60	£0.00	£2.40	£0.40	£1.80	£1.00	£1.00
22	£1.20	£0.50	£0.00	£1.60	£1.60	£0.25	£0.60	£1.20	£2.40	£0.00
23	£0.80	£1.20	£1.60	£0.00	£0.00	£2.40	£1.00	£0.40	£0.20	£1.80
24	£3.60	£0.00	£2.70	£0.30	£1.80	£0.60	£0.30	£0.30	£0.00	£1.20
25	£0.90	£0.60	£1.20	£0.00	£0.00	£2.40	£0.30	£1.80	£0.60	£1.20

Notes: The table shows the return per pound invested (depending on the outcome of the coin toss) for the different assets.

Table 2—Summary Statistics of Decision-making Quality Measures

	Mean	*Percentiles* 25th	Median	75th	Standard Deviation
GARP	-$2.77	-$3.07	-$1.19	-$0.55	$3.87
FOSD	-$3.68	-$4.42	-$3.36	-$2.52	$1.93
GARP and FOSD	-$4.49	-$5.53	-$2.45	-$1.43	$4.90
Financial competence	-$6.61	-$8.80	-$6.09	-$3.97	$3.73
Failure to minimize risk	-$3.32	-$4.61	-$0.90	$0.00	$4.91
Decision-making quality index	-$4.10	-$5.06	-$3.33	-$2.34	$2.66

Notes: This table shows summary statistics of decision-making quality measures for the "pre-reform" sample, i.e. those born before September 1 1957. The outcomes are denoted as negative values such that higher values (closer to zero) correspond to higher decision-making quality. N = 1,416.

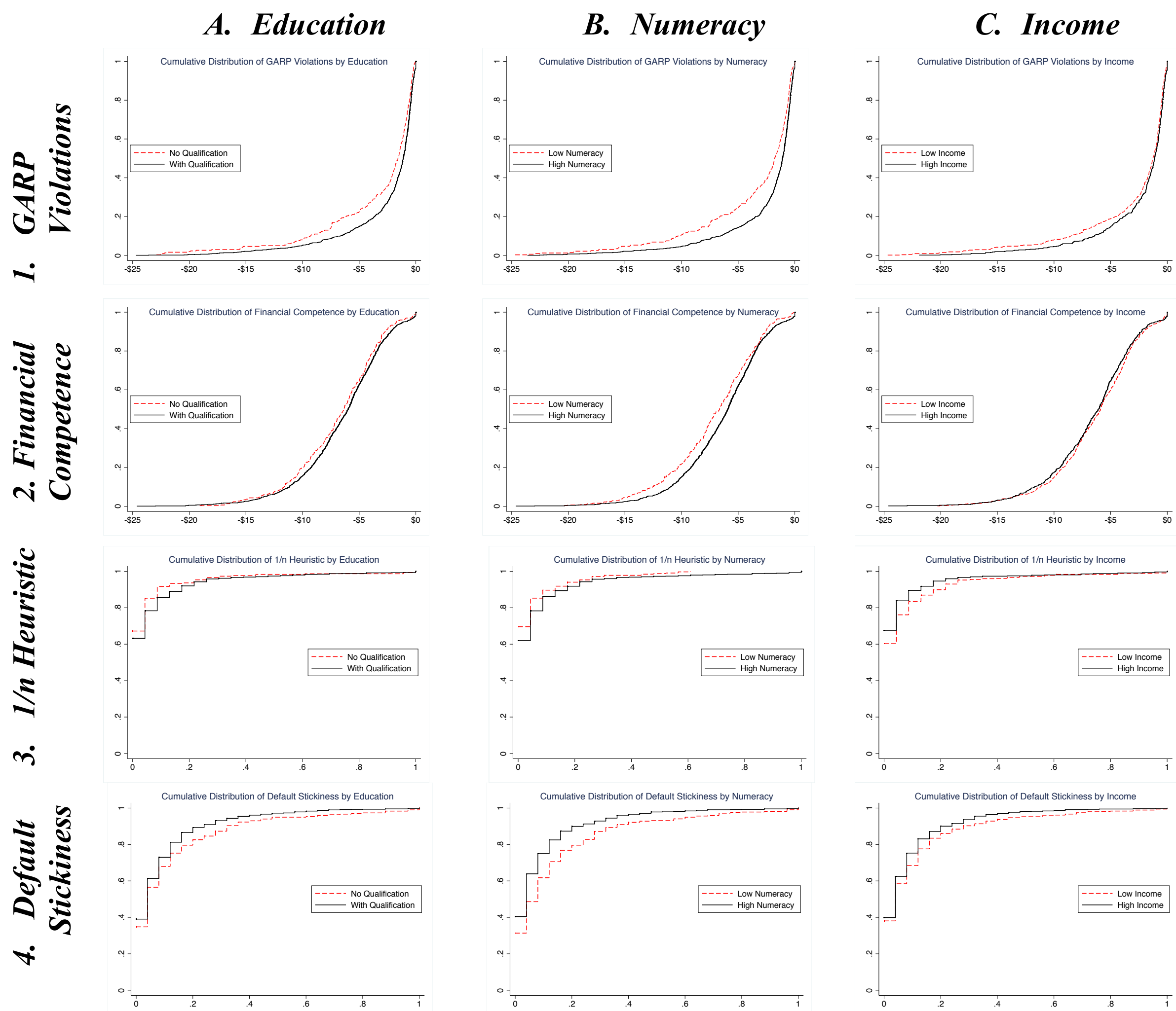

Figure 1: Decision-making, Education, Numeracy, and Income

Notes: Row 1 shows cumulative distributions of violations of the General Axiom of Revealed Preference (GARP). Row 2 shows cumulative distributions of financial competence, row 3 the cumulative distributions of violations of the 1/n heuristic and row 4 the cumulative distributions of default stickiness. Column 1 compares cumulative distributions for those with and without a formal qualification, column B for high and low numeracy, and column C low and high income. *N* = 1,375 (Column A), 1,383 (Column B), and 1,106 (Column C). The p-values of Wilcoxon rank-sum tests are: 0.00 (Panel 1A); 0.00 (1B); 0.03 (1C); 0.10 (2A); 0.00 (2B); 0.23 (2C); 0.08 (3A); 0.01 (3B); 0.00 (3C); 0.03 (4A); 0.00 (4B); and 0.09 (4C).

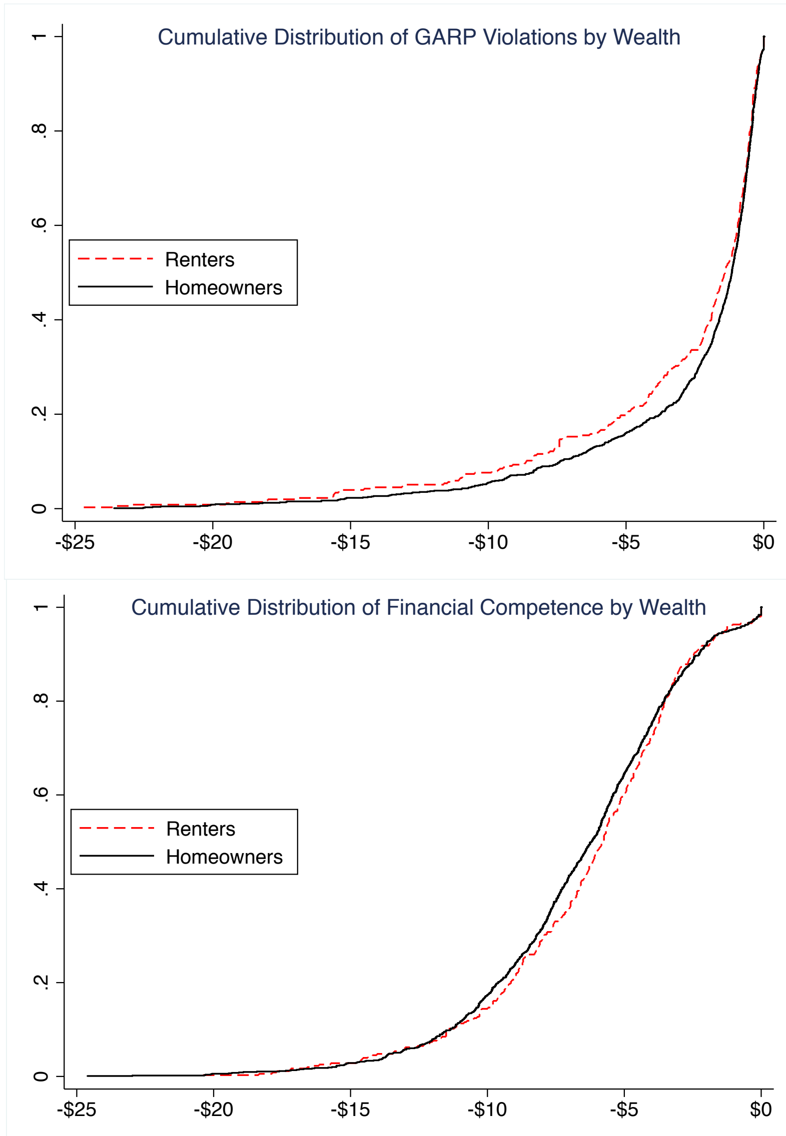

Figure 2: Decision-making Quality and Home Ownership

Notes: The top figure shows cumulative distributions of violations of the General Axiom of Revealed Preference (GARP). The bottom figure shows cumulative distributions of the decision-making quality index. The red dashed lines show cumulative distributions for renters. The black solid lines show cumulative distributions for homeowners. N = 1,402. The p-values of Wilcoxon rank-sum tests are: 0.05 (top) and 0.17 (bottom).

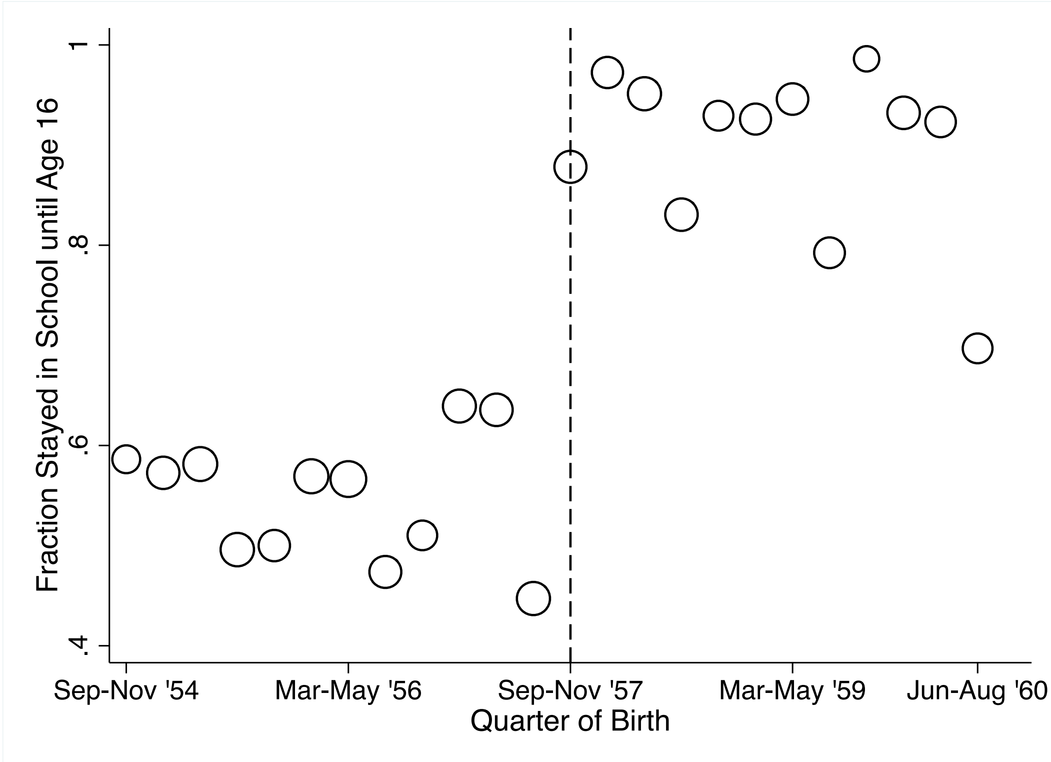

Figure 3: Fraction Stayed in School until Age 16 by Quarter of Birth

Notes: The points show the fraction of study participants in each quarter-year of birth cell that stayed in school until age 16. The vertical dashed line is the cutoff indicating the first cohort subject to the change in the compulsory schooling law. The balls' circumferences correspond to the number of study participants born in the quarter-year of birth cell. $N = 2{,}698$.

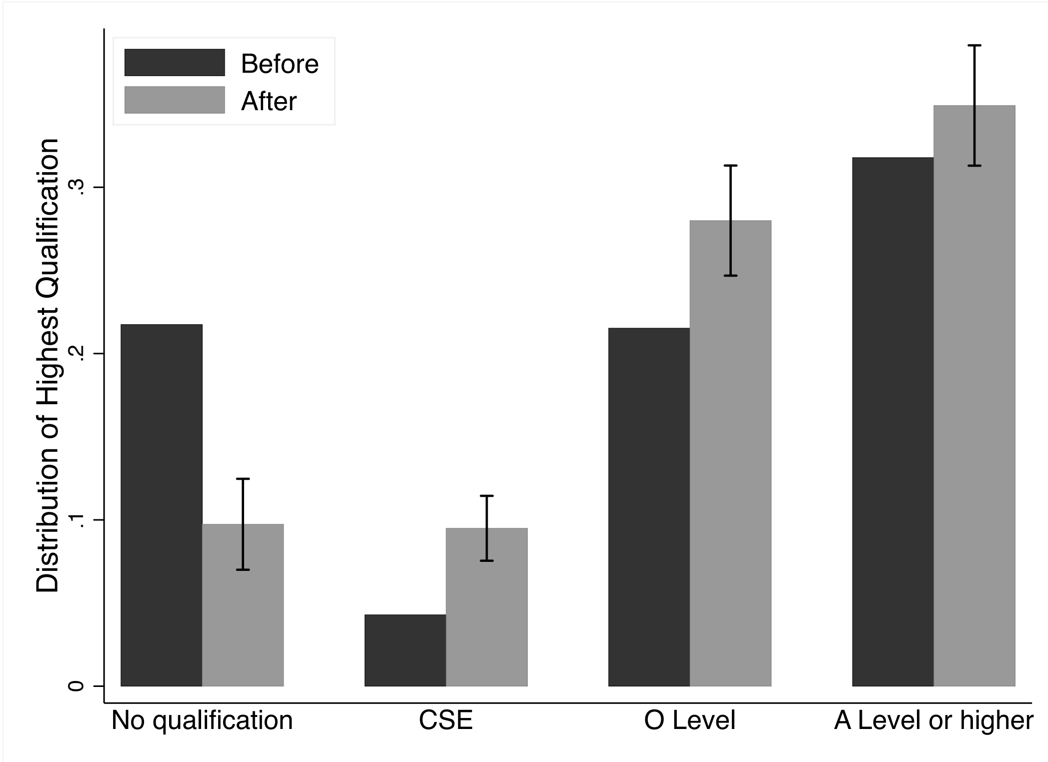

Figure 4: Highest Qualification

Notes: The figure shows the distribution of highest qualification, separately for those born before and after September 1, 1957. The black bars show the distribution for those born before September 1, 1957. The gray bars show the distribution for those born after September 1, 1957. The range plots show 95% confidence intervals of the difference between those born before and after September 1, 1957. $N = 2{,}618$ for highest qualification distribution. The figure omits lower level vocational and higher level vocational, which are shown in Table 3.

Table 3—Impacts of England's 1972 Compulsory Schooling Change on Qualifications

	Mean born before Sep 1, 1957	After-before difference
Stayed in school until age 16	0.55	0.348 (0.016)***
No formal qualification	0.22	-0.120 (0.014)***
Lower level vocational	0.13	-0.016 (0.013)
CSE	0.04	0.052 (0.010)***
O level	0.22	0.065 (0.017)***
Higher level vocational	0.08	-0.012 (0.010)
A level and higher	0.32	0.031 (0.018)*

Notes: This table shows the estimated effect of the change in England's compulsory schooling law in 1972 on educational attainment. Robust standard errors between parentheses. $N = 2{,}698$ (stayed in school until age 16); 2,618 (for all other outcomes).

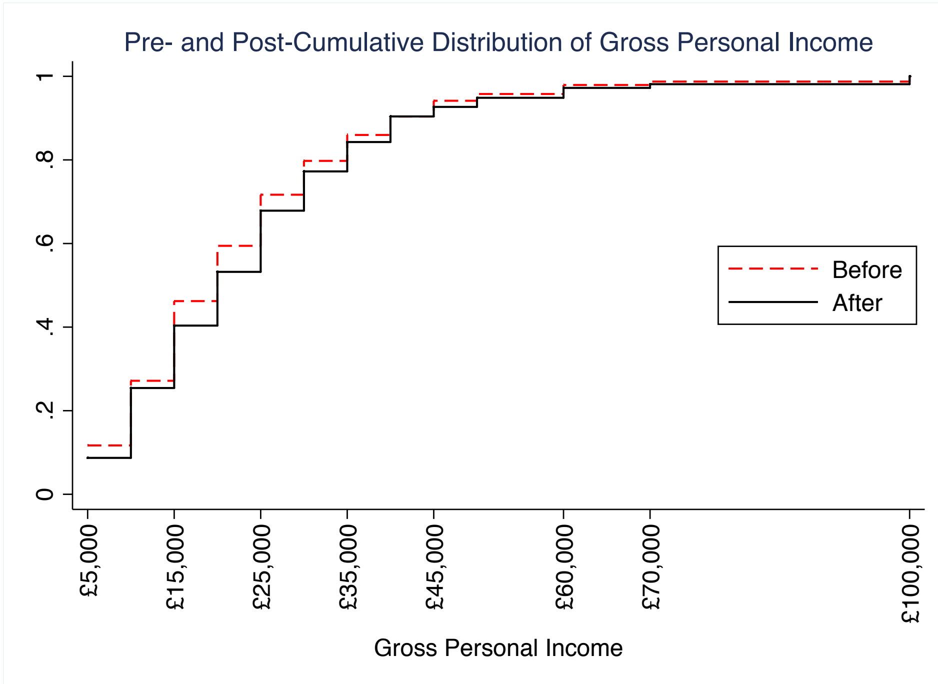

Figure 5: Cumulative Distributions of Gross Personal Income

Notes: The figure shows cumulative distributions of annual Gross Personal Income. The red dashed line shows the cumulative distribution for those born before September 1 September 1, 1957. The black solid line shows the cumulative distribution for those born after September 1, 1957. $N = 2{,}136$. The p-value of a Wilcoxon rank-sum test is 0.0107.

Table 4—Differences in Predetermined Characteristics of Those Born before and after 9/1/1957

	Mean born before Sep 1, 1957	After-before difference
Male	0.50	0.005
		(0.019)
British white	0.97	-0.009
		(0.007)
# of Bedrooms in residence respondent lived at age 10	2.95	0.04
		(0.03)
# of People lived with respondent at age 10	5.05	-0.10
		(0.08)
Parents unemployed for +6 months when respondent was < 14	0.08	0.015
		(0.011)
# of books in place respondent lived at age 10		
None or very few (0-10 books)	0.24	-0.003
		(0.017)
Enough to fill one shelf (11-25 books)	0.30	-0.014
		(0.018)
Enough to fill one bookcase (26-100 books)	0.34	-0.008
		(0.018)
Enough to fill two or more bookcases (101 books or more)	0.12	0.026
		(0.013)*
Respondent lived for most of his/her childhood with		
Both parents	0.87	-0.017
		(0.014)
Mother only	0.05	0.007
		(0.008)
Father only	0.01	0.002
		(0.004)
Other	0.08	0.007
		(0.011)
Caregiver's main occupation when respondent was 14		
Manager, run own business, professional or technical	0.20	0.024
		(0.016)
Admin., clerical, secretarial, caring, personal services, sales or customer service	0.10	0.007
		(0.012)
Skilled trade	0.29	-0.026
		(0.017)
Machine operator, casual jobs, other jobs	0.27	-0.009
		(0.017)
Other	0.14	0.004
		(0.014)

Notes: This table tests difference in means of predetermined characteristics between study participants born before and after September 1, 1957. The first column shows means for those born before September 1, 1957. The second column shows the after-before difference in means. N = 2,698 (male); 2,678 (white); 2,595 (# of bedrooms); 2,596 (household size); 2,658 (parents unemployed); 2,659 (# of books and upbringing); and 2,646 (caregiver's main occupation). Robust standard errors between parentheses.

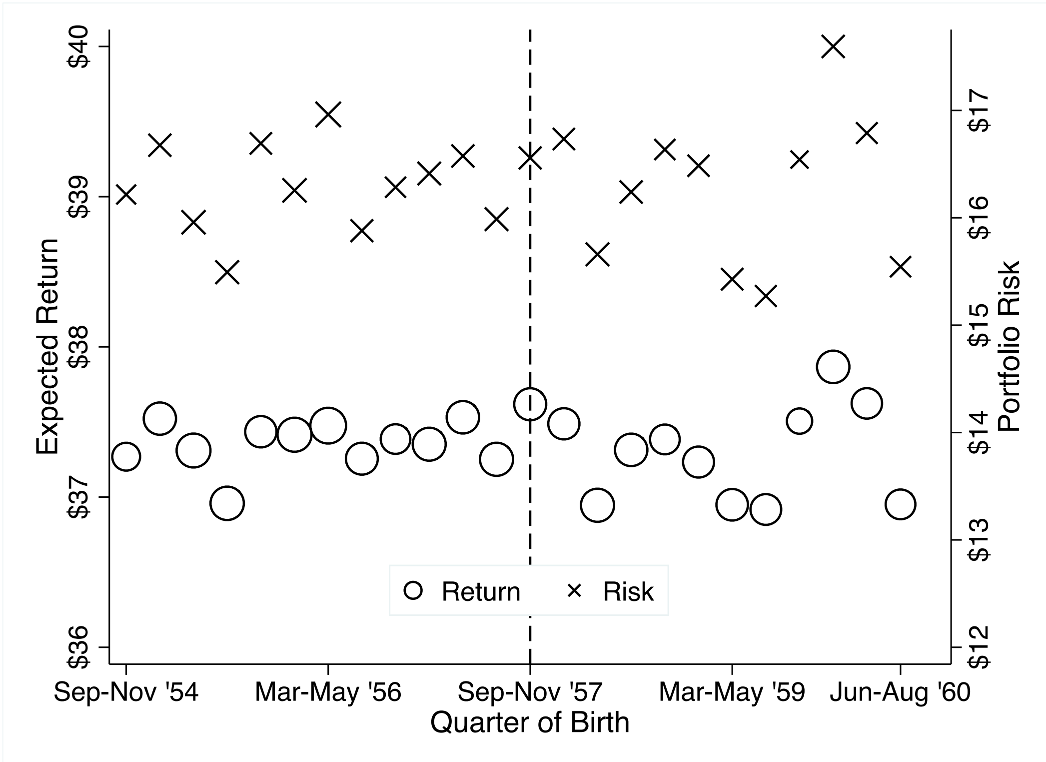

Figure 6: Expected Return and Portfolio Risk by Quarter of Birth

Notes: The circles show the average expected return for each quarter-year of birth cell (averaged over participants and over budget lines). The Xs show average standard deviation of portfolio return by quarter-year of birth. $N = 2{,}698$. The vertical dashed line is the cutoff indicating the first cohort subject to the new compulsory schooling law.

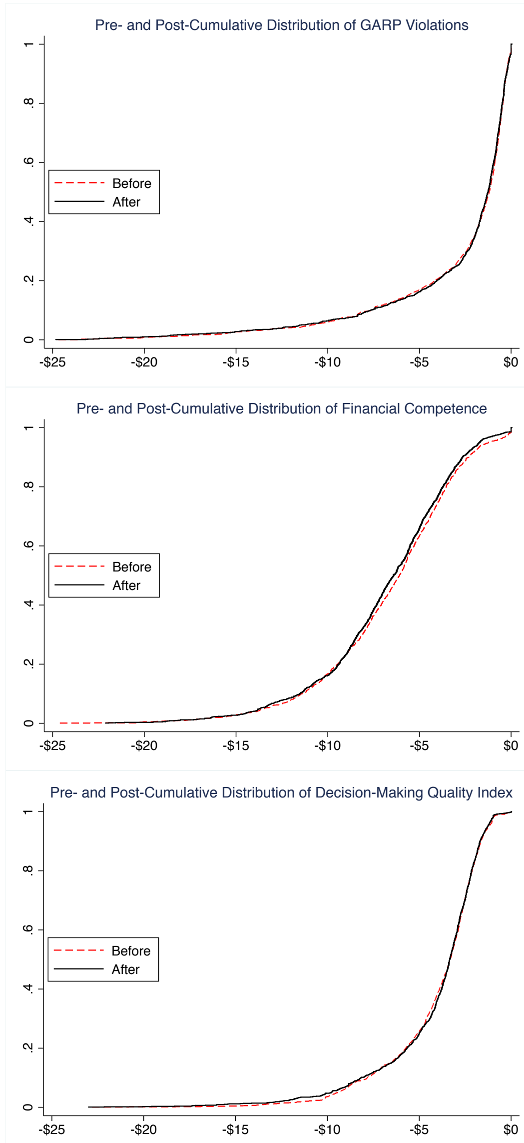

Figure 7: Cumulative Distributions of Decision-Making Quality

Notes: The top figure shows cumulative distributions of violations of the General Axiom of Revealed Preference (GARP). The middle figure shows cumulative distributions of financial competence. The bottom figure shows cumulative distributions of the decision-making quality index. The red dashed lines show cumulative distributions for those born before September 1, 1957. The black solid lines show cumulative distributions for those born after September 1, 1957. N = 2,698. The p-values of Wilcoxon rank-sum tests are: 0.63 (top); 0.125 (middle); and 0.99 (bottom).

Table 5—Effects on Decision-Making

Dependent Variable	Mean born before Sep 1, 1957	*Reduced Form* Born after Sep 1, 1957 *in $*	*in SD*	*2SLS* Stayed in school until 16 *in $*	*in SD*
		Portfolio performance			
Expected return	$37.35	-$0.03	-0.01	-$0.10	-0.04
		(0.10)	(0.04)	(0.29)	(0.11)
Portfolio risk	$16.29	$0.00	0.00	$0.00	0.00
		(0.22)	(0.04)	(0.62)	(0.11)
		Decision-making quality			
GARP	-$2.77	-$0.02	0.00	-$0.05	-0.01
		(0.15)	(0.04)	(0.43)	(0.11)
FOSD	-$3.68	-$0.12	-0.06	-$0.36	-0.19
		(0.08)	(0.04)	(0.24)	(0.12)
GARP and FOSD	-$4.49	$0.03	0.01	$0.10	0.02
		(0.19)	(0.04)	(0.54)	(0.11)
Financial competence	-$6.61	-$0.21	-0.06	-$0.60	-0.16
		(0.14)	(0.04)	(0.41)	(0.11)
Failure to minimize risk	-$3.32	$0.08	0.02	$0.24	0.05
		(0.19)	(0.04)	(0.56)	(0.11)
Decision-making quality index	-$4.10	-$0.07	-0.03	-$0.19	-0.08
		(0.11)	(0.03)	(0.31)	(0.09)
		Behavioral anomalies			
Small-scale risk aversion	$20.45	$0.02	0.01	$0.07	0.02
		(0.16)	(0.04)	(0.45)	(0.11)
1/n heuristic	0.055	-0.005		-0.015	
		(0.005)		(0.015)	
Default stickiness	0.097	-0.007		-0.020	
		(0.006)		(0.017)	

Notes: This table shows estimates of the effects on decision-making. The first column shows the mean of the dependent variable among those born before September 1, 1957. For all other columns, each cell corresponds to a separate regression. Columns (2) and (3) show reduced-form estimates, where the independent variable is a dummy for being born after September 1, 1957. Columns (4) and (5) show 2SLS estimates, where we use the dummy for being born after September 1, 1957 to instrument for staying in school until age 16. In columns (2) and (4) the dependent variable is in US dollars. In columns (3) and (5) the dependent variable is in standard deviation units. $N = 2{,}698$. Robust standard errors in parentheses.

Table 6—Robustness

	Effect of one additional year of schooling (*2SLS*)				
Dependent Variable in SD					
			Portfolio performance		
Expected return	-0.04	-0.05	-0.04	-0.04	-0.19
	(0.11)	(0.11)	(0.18)	(0.16)	(0.17)
Portfolio risk	0.00	-0.01	-0.01	-0.01	-0.17
	(0.11)	(0.11)	(0.18)	(0.16)	(0.17)
			Decision-making quality		
GARP	-0.01	-0.03	0.23	0.23	0.30
	(0.11)	(0.11)	(0.18)	(0.17)	(0.16)*
FOSD	-0.19	-0.21	-0.06	-0.06	-0.08
	(0.12)	(0.12)*	(0.19)	(0.16)	(0.17)
GARP and FOSD	0.02	0.00	0.13	0.13	0.18
	(0.11)	(0.11)	(0.17)	(0.15)	(0.16)
Financial competence	-0.16	-0.17	0.13	0.13	0.25
	(0.11)	(0.11)	(0.18)	(0.21)	(0.24)
Failure to minimize risk	0.05	0.03	0.09	0.09	0.09
	(0.11)	(0.11)	(0.18)	(0.17)	(0.19)
Decision-making quality index	-0.08	-0.09	0.10	0.10	0.14
	(0.09)	(0.09)	(0.14)	(0.13)	(0.14)
			Behavioral anomalies		
Small-scale risk aversion	0.02	0.03	0.08	0.08	0.22
	(0.11)	(0.11)	(0.19)	(0.15)	(0.14)
Control predetermined vars.		X	X	X	X
Linear functions of DoB			X	X	X
Cluster at month-year of birth				X	X
Month-of-birth fixed effects					X

Notes: This table assesses the sensitivity of the results shown in Table 5. Each cell corresponds to a separate 2SLS regression, where we use the dummy for being born after September 1, 1957 to instrument for staying in school until age 16. Dependent variable in standard deviation units listed on the rows. See text for explanations about different specifications. Missing values for the predetermined variables were replaced by zeroes; specifications include dummies for these missing values. N = 2,698. Robust standard errors in columns 1-3. Standard errors clustered at month-year of birth in columns 4-5.