Business Education and Portfolio Returns*

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Abstract

Using university admission cutoffs that generate exogenous variation in college-major choices, we provide causal evidence that enrollment in a business or economics program leads individuals to invest significantly more in the stock market, earn higher portfolio returns, and ultimately accumulate higher levels of wealth later in life. Underlying these effects, beyond differences in risk-taking, innate ability, labor market outcomes, or scale effects, is the enhanced ability of business educated individuals to acquire and process economic information and make informed investment decisions. Early investments in financial literacy thus play an important role in generating higher returns that significantly alter individuals' life-cycle wealth profiles.

JEL: G11, G51, G53, I26.

Keywords: Portfolio choice, financial literacy, portfolio returns, household wealth, returns to education, distribution of wealth.

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1 Introduction

Heterogeneity in returns to wealth is a key driver of household wealth inequality across the world, not the least in the thick right tail of the wealth distribution.[1] In principle, return heterogeneity can arise from differences in household risk taking (Bach, Calvet, and Sodini 2020; Campbell, Ramadorai, and Ranish 2019), innate ability (Barth, Papageorge, and Thom 2020; Fagereng et al. 2020), or financial knowledge (Jappelli and Padula 2013, 2017; Lusardi, Michaud, and Mitchell 2017).[2] For the latter, Lusardi, Michaud, and Mitchell (2017) show theoretically that differences in financial literacy can lead to large differences in household wealth, mainly through their effects on the returns to saving, and account for a sizeable portion of wealth inequality in the U.S. Despite the intuitive appeal of a financial literacy related explanation for return heterogeneity, there is still a lack of well-identified empirical evidence on the causal effects of increased financial sophistication on household portfolio returns and wealth accumulation.[3]

In this paper, we provide causal evidence that increased financial literacy contributes positively to the rate of return on risky investments, and thereby affects the dynamics of household wealth accumulation. Specifically, using exogenous variation generated by university-program admission thresholds, we document that relatively early investments in financial literacy, in the form of quasi-randomly enrolling in a business or economics program at the tertiary level, lead individuals to both invest more in the stock market, and more importantly, earn significantly higher returns on their stock investments over the short to medium term. Thus, individuals with similar initial characteristics in terms of preferences and abilities end up accumulating significantly different levels of wealth later in life, implying that early investments in financial sophistication alter the life cycle wealth profiles of individuals.

In our empirical analysis, we overcome the thorny problem of identifying the causal effect of financial education on household financial outcomes by employing a regression discontinuity design that exploits the quasi-random variation around the cutoffs for admission to business or economics university programs. In Sweden, where we base our empirical analysis, a centralized application and admissions process allocates applicants to university programs based on their academic performance and preferences. This system

[1] See, for example, Bach, Calvet, and Sodini (2020), Benhabib, Bisin, and Luo (2019), Benhabib, Bisin, and Zhu (2011), Campbell, Ramadorai, and Ranish (2019), Fagereng et al. (2020), Gabaix et al. (2016), and Hubmer, Krusell, and Smith Jr. (2021).

[2] See also the discussion in De Nardi and Fella (2017). In addition to differences in risk exposure, ability, and financial sophistication, return differences across households can also arise from other sources such as access to information (Kacperczyk, Nosal, and Stevens 2019; Peress 2004) or access to the stock market (Guvenen 2009).

[3] Bianchi (2018) and Gaudecker (2015) provide correlational evidence on the link between portfolio returns and financial literacy. For example, using administrative data from France, Bianchi (2018) finds that financially more literate investors earn 40 basis points larger annual returns on their investments than less literate investors after accounting for various measures of risk.

generates sharp admission cutoffs whenever a program is oversubscribed. Furthermore, the ranked list of university-program combinations that each applicant submits allows us to observe each applicant's counterfactual alternative, to which they would be assigned if not admitted to their preferred option.

From the universe of university applications submitted through the centralized system between 1977 and 1995, we first identify all applicants who apply to study business (including programs such as economics, finance, business administration, management, commerce, industrial economics, management, and organization) in an oversubscribed program and have a non-business major as their next best alternative. Building on the ideas as in Fagereng et al. (2020) and Haliassos, Jansson, and Karabulut (2020), we argue that individuals who were quasi-randomly assigned to a business or economics program acquire higher levels of financial literacy. We ensure the validity of this interpretation and provide strong evidence that our education-based measure does indeed capture improved financial literacy. Using a fuzzy regression discontinuity design, where business enrollment is instrumented with threshold-crossing, and an extensive individual-level panel dataset, we then estimate short- and medium-term effects. By contrasting individuals marginally above an admission threshold to those who are slightly below, we uncover the lasting causal effects of studying business on household financial outcomes.

Our empirical findings demonstrate that more financial education causes higher portfolio returns and improved financial and wealth outcomes over a period of 4 to 25 years after initial application. In particular, individuals marginally admitted to a business degree program invest more in the stock market and earn significantly higher returns on their stock investments than their otherwise similar peers who were slightly below the admission cutoff and thus not admitted to a business program. The documented effects are not only statistically significant, but also economically meaningful. For example, individuals with a business education hold about USD 7,000 more in stocks (i.e., 20% more relative to the mean) and earn about 15 basis points more in raw monthly stock portfolio returns than their non-business educated peers, which translates into an annualized return difference of 1.86 percentage points.

Having established a strong and lasting positive causal effect of business education on portfolio returns, we turn to identifying the driving mechanisms. First, we recognize that the documented return differences between business and non-business educated individuals may be due to differences in their willingness to take more risk, as shown by Bach, Calvet, and Sodini (2020) and Campbell, Ramadorai, and Ranish (2019). To address this explanation, the baseline analysis accounts for differences in portfolio beta and access to the menu of financial instruments across individuals in the sample as in Fagereng et al. (2020). To further refine our understanding of the role of risk-taking in our results, additional analyses control for the average size, momentum, and value loadings of the stock portfolio. Importantly, despite a 23% reduction in its economic magnitude,

the effect of business education remains strong even after controlling for differences in exposure to different sources of (compensated) risk.

Second, heterogeneity in innate ability across individuals could also explain the return results, if, for example, more skilled and talented individuals sort themselves into business programs (Barth, Papageorge, and Thom 2020; Fagereng et al. 2020). Because our empirical design ensures we are comparing individuals with the same observable (and unobservable) predetermined characteristics, our analyses account for such differences. Furthermore, when we exclude the most able and the least able applicants, as proxied by their high school GPA, and focus on applicants competing at cutoffs at the middle of the ability distribution, the positive impact of business education on portfolio returns retains both its statistical and economic significance.

In fact, our results point to differences in financial literacy as an important operative mechanism. More precisely, business or economics education improves individuals' ability and capacity to process relevant economic information, which facilitates more informed stock investment decisions, especially when the return to information is higher (Kacperczyk, Nosal, and Stevens 2019; Lusardi and Mitchell 2014). For example, we find significantly better relative portfolio performance of business educated individuals during market downturns and periods of high volatility, i.e., when the value of information acquisition and processing is higher (Grossman and Stiglitz 1980). In contrast, we do not observe a systematic effect during favorable market conditions, characterized by relatively higher market returns or lower aggregate volatility. Similarly, the documented positive effects are prevalent if the underlying assets of the individual stock portfolio exhibit relatively higher volatility and illiquidity, as measured by the idiosyncratic volatility and the Amihud ratio of the stock portfolio, respectively. Taken together, these results indicate that early investments in financial knowledge, beyond differences in risk exposure and innate ability, play a distinct role in explaining return heterogeneity.

To further substantiate the interpretation that enrolling in a business or economics program reflects improvements in financial literacy, we examine its effects on common investment mistakes, including portfolio underdiversification and the disposition effect. We find that individuals with a business education tend to hold better diversified portfolios and are less subject to the disposition effect relative to their non-business educated peers. These findings not only strengthen the increased financial literacy interpretation, but also highlight additional channels through which business education contributes to improved household financial behavior.

We then subject our arguments to various tests, and examine possible alternative explanations that could produce similar results. These include explanations based on educational attainment, labor market outcomes and career paths, scale effects, quantitative skills, and peer effects. The first concern is whether the documented effects might be driven by the level of education rather than its content. Our analysis shows that business

education has positive and significant effects on portfolio returns even when we restrict the sample to individuals with a college degree. Second, we investigate whether business education influences financial choices and outcomes of households through its impact on labor market outcomes and career paths, such as working in the finance industry, and show that the effects we uncover cannot be attributed solely to individuals' career choices or other relevant labor market outcomes. For example, we find no significant effect of business education on the unemployment risk of individuals, which may otherwise partly explain the higher returns. We also perform a mediation analysis and find that the effect of business education on portfolio returns does not appear to be mediated through labor market outcomes, particularly career paths. Third, it is possible that individuals with business education (and thus possibly higher wealth) may earn higher returns because they may have easier access to high-quality information or better investment opportunities, a mechanism called scale dependence (Bach, Calvet, and Sodini 2020). Several tests, such as directly accounting for scale effects in the portfolio return regressions or focusing on a time period when there are no wealth differences between business and non-business educated households, show that our return results are not simply an artifact of scale effects. Fourth, we confirm that it is the financial knowledge acquired through business education, rather than quantitative skills, that underlies the observed positive effects by focusing on a sample of applicants with a business or economics program as their preferred choice and a quantitative program such as science or technology as their next best alternative. Finally, we examine the potential role of peer effects, and find no support for this interpretation. Taken together, these sensitivity checks suggest that our results are not a pure artifact of potential differences in educational attainment, scale effects, quantitative skills, peers, or career trajectories between business and non-business educated individuals. Rather, it is the improved financial literacy that leads to improved portfolio decisions.

Additional empirical support for this interpretation comes from the analysis on the dynamic effects of majoring in business on portfolio returns. Our findings reveal that business educated households earn significantly higher returns on their stock investments in the short run (i.e., 4-14 years after the initial application), while this effect turns out to be insignificant in the medium run (i.e., 14-25 years after the initial application). This asymmetry is consistent with the model predictions of Lusardi, Michaud, and Mitchell (2017), which show that, similar to other human capital investments, financial literacy depreciates over time, and that the optimal financial literacy profile is hump-shaped over the life cycle. At the same time, non-business educated households can improve their financial sophistication either directly through endogenous investment in financial knowledge or indirectly through learning-by-doing, thereby reducing the initial financial literacy gap over time.

As a number of studies document the importance of intergenerational spillovers in earnings, wealth, and human capital formation (Björklund, Lindahl, and Plug 2006; Black

et al. 2020; Black, Devereux, and Salvanes 2005), we next turn to analyzing whether investment in financial literacy complements or substitutes for the intergenerational transmission of financial sophistication (Lusardi and Mitchell 2014). Interestingly, our results show that the positive contribution of business education to portfolio choice and portfolio returns is operative only for individuals with less educated parents. In contrast, we find no systematic effects in the sample of individuals with at least one college-educated parent. These cross-sectional results imply that business education serves as a substitute for intergenerational persistence in financial literacy and thus may play a key role in increasing intergenerational mobility. At the same time, the documented asymmetry across individuals based on parental sophistication also speaks to the external validity of our findings. Specifically, the observed disparities suggest that applicants from relatively more disadvantaged backgrounds may lack alternative access to financial knowledge, and thus financial education interventions may lead to more pronounced changes in their knowledge and financial behavior, increasing the potential for generalizability of our findings across different demographic groups.

The effects of business education extend beyond financial behavior to household wealth accumulation. We find that individuals with business education accumulate significantly more financial and net wealth later in life. In particular, enrolling in a business degree program results in an average increase of USD 11,600 (USD 28,155) in financial (net) wealth. To put this effect in context, it is equivalent to an increase of about 18% (16.5%) in the average financial (net) wealth of the sampled individuals, which is quite substantial. The analysis of the dynamics of wealth accumulation shows that these effects manifest gradually in the medium term, after which there is a monotonic increase in the wealth gap between business and non-business majors. We conclude that early investments in financial literacy alter life-cycle wealth profiles, and individuals with similar initial characteristics end up accumulating significantly different levels of wealth later in life. We also examine alternative mechanisms, such as the labor market, household debt behavior, or housing investments, that may affect wealth through channels other than the portfolio channel, and find little or no support for them.

Our paper relates to several strands of the literature. First, our causal empirical evidence on the impact of improved financial knowledge on portfolio returns and wealth accumulation directly links to the literature on financial literacy, and its implications for household wealth accumulation and wealth inequality (Behrman et al. 2012; Jappelli and Padula 2013, 2017; Lusardi, Michaud, and Mitchell 2017; Lusardi and Mitchell 2007; Van Rooij, Lusardi, and Alessie 2011). For example, Lusardi, Michaud, and Mitchell (2017) develop a dynamic stochastic intertemporal model of consumption and portfolio choice and show that endogenous investments in financial knowledge generate higher expected returns on savings and large differences in household financial wealth. Similarly, Jappelli and Padula (2017) document a positive link between financial sophistication and portfolio

returns and consumption growth of households using a life-cycle model that incorporates endogenous financial knowledge. Both Jappelli and Padula (2017) and Lusardi, Michaud, and Mitchell (2017) posit that improved financial knowledge allows individuals to use sophisticated, information intensive financial products (such as stocks), which enable them to earn higher returns on their investments. Our causal evidence supports the model predictions of Lusardi, Michaud, and Mitchell (2017) and Jappelli and Padula (2017) in that financial knowledge acquired through business education generates higher portfolio returns and alters the life cycle wealth profiles of individuals. Hence, our findings are relevant for the ongoing discussion on the policy tools to regulate wealth inequality (e.g., Calvet et al. 2023; Guvenen et al. 2023; Stiglitz 2015), indicating that financial literacy education can partly contribute to contain wealth inequality.

Second, we contribute to the current discussion on the effectiveness of financial literacy education to empower households to make better financial decisions (e.g., Campbell 2016; Kaiser et al. 2021). Specifically, there is an ongoing debate whether financial education is an effective policy tool that can improve economic choices of households (Campbell 2016; Fernandes, Lynch Jr, and Netemeyer 2014; Kaiser et al. 2021), partly due to the lack of well-identified evidence on the causal effects of financial education on household decision-making.[4] In a closely related paper, Hvidberg (2023) uses university admission discontinuities in Denmark to document that business education significantly lowers the probability of experiencing financial distress. Like us, Hvidberg (2023) studies the effect of enrolling in a post-secondary business program. Taken together, our findings suggest that financial education has the potential to improve financial choices and wealth outcomes through its effects on the asset side of the household balance sheet.

Our paper also links to the recent literature on the role of education in the distribution of wealth. For example, Girshina (2019) and Fagereng et al. (2019) document a positive association between educational attainment and returns on net wealth and on each of its components. Compared to these studies, which focus on the level of education, we consider the content of education and show that business education plays an important role in the wealth accumulation process of households through its effects on portfolio returns.

Finally, our paper contributes to the literature on returns to education, which typically focuses on the effects of college education and college major on individuals' labor market prospects and outcomes (some examples are Acemoglu, He, and le Maire 2022; Altonji, Arcidiacono, and Maurel 2016; Altonji, Blom, and Meghir 2012; d'Astous and Shore 2024; Delavande and Zafar 2019; Eika, Mogstad, and Zafar 2019; Hastings, Neilson,

[4]This discussion is of profound importance for policy choice in the presence of alternative policy options such as financial regulation, use of default options, and financial advice. See, for example, Alan and Ertac (2018), Boyer, d'Astous, and Michaud (2020), Brown et al. (2016), and Carpena et al. (2019) for existing evidence on the effects of financial education on individual decision-making. See also Fernandes, Lynch Jr, and Netemeyer (2014) and Kaiser et al. (2021) who evaluate the recent literature on financial education using meta-analysis techniques.

and Zimmerman 2014; Kirkebøen, Leuven, and Mogstad 2016). For example, Andrews, Imberman, and Lovenheim (2017) use a regression discontinuity design to establish the causal effect of majoring in business on earnings of individuals in the U.S., and identify returns to business majors of being approximately 80–130% over the medium term, i.e., more than 12 years after initial enrollment. We document that the causal effect of majoring in business extends beyond the labor market to financial behavior and wealth accumulation of households. An early paper focusing on the financial behavior of individuals with economics education is Christiansen, Joensen, and Rangvid (2008), which finds that being an economist is associated with an increased tendency to invest in the stock market. Our paper differs from this study in many aspects, including the empirical identification, the household outcomes considered, and the results. Notably, we find no systematic effect of business education on stock market participation, while we do identify positive causal effects of business education on both portfolio returns and household wealth accumulation.[5]

The remainder of the paper is structured as follows: Section 2 first provides background information on the Swedish education system and university admission process, and then describes the data sources and sample construction. In Section 3, we present our identification strategy. Section 4 presents the empirical analysis on household financial behavior, while Section 5 explores the implications of our findings on household wealth accumulation. Section 6 concludes.

2 Institutional Details and Data

In this section, we first provide information about the Swedish higher education system and university admission process, and then describe the data sources and the construction of the final sample for the empirical analysis.

2.1 University Admission Process in Sweden

In Sweden, where we base our empirical analysis, tertiary education is tuition-free and, with a few exceptions, state-run. All students are offered stipends and subsidized student loans. Similar to many other European countries, individuals apply by submitting a preference ranking of programs at specific institutions in which they would like to study. Each of these alternatives covers a specific field of study and, when completed, awards the student with a field-specific degree. If a program is oversubscribed, students are admitted on the basis of previous academic performance.

[5]We acknowledge that the discrepancy in the stockholding results could be due to differences in empirical identification strategy and samples. Specifically, our estimates represent local average treatment effects for applicants who have a revealed preference for studying business and thus are likely to have an above-average interest in finance or economics.

To be eligible for post-secondary education, applicants must have completed a university-preparatory high school program. Individuals from other programs, or those who have not completed the required courses, can supplement their high school diplomas with preparatory adult education to become eligible. University programs begin in either the fall or spring semester, and applications are made separately for each semester. Applicants submit ranked lists of up to 12 program-institution combinations, hereafter referred to as choices or alternatives.

All applicants to a given program-institution are ranked by their score in the admission groups for which they are eligible. Applicants often compete in multiple admission groups for a given alternative. For example, one admission group is based on high school GPA scores,[6] and another one on *Högskoleprovet* (a standardized admission exam similar to the SAT). Finally, applicants with prior work experience can apply in a separate group where their work experience is awarded with bonus points on top of their high school GPA. Note that applicants in each group are ranked separately based on their group-specific scores, and the number of spots available for different admission groups is proportional to the total number of eligible applicants who compete in each group. To make the admissions scores more comparable across groups, we standardize applicants' scores separately for each group and year. In all of our regressions, we include admission cutoff fixed effects and separate running variable polynomials for each admission group.

Each application period consists of two rounds.[7] During each round, applicants are offered admission to their highest-ranked program for which they are above the admission cutoff, while lower-ranked alternatives are automatically withdrawn. Applicants may choose to remain on a waitlist for any higher-ranked program to which they have applied but have not yet been admitted. Note that the first round offer will be withdrawn if waitlisted applicants are admitted to a higher ranked alternative in the second round.

The admission allocation mechanism can be described as a truncated multicategory serial dictatorship. Because of application list truncation, it is not strategy-proof. Moreover, when multiple applicants have exactly the same score, but there are not enough slots to admit them all, tie-breaking mechanisms are used. These include lotteries, gender priorities, and, for most programs in the period from 1977 to 1995, a priority for the applicants who ranked that alternative the highest on their preference lists. Such allocation mechanisms pose some risk to strategic considerations in the application process. For example, applicants for highly competitive programs may avoid ranking multiple such

[6]During a transition between two high school grading systems, separate groups are used for each grading system.

[7]After the second round, a third round of admissions may take place locally at each university, where students who are just below the cutoff at the end of the second round may be offered admission if other admits do not show up. We do not have data on this process. Therefore, admission status and cutoffs are calculated based on the results of the second round. Admission to a higher ranked program in the third round does not cancel offers made in the second round.

programs in their applications in case they may need a safe fallback option.[8] However, when zooming in on a pair of a preferred and a next-best alternative in an application, there is no reason for the applicant to reverse the order of these options from their true preference.

2.2 Data Sources

We focus on applications to Swedish universities made between 1977 and 1995 through the central application system.[9] The university application data come from the Swedish National Archives, specifically the A1 system (which covers the period 1977-1992) and the H97 system (which covers the period 1993-2005).[10] This dataset provides detailed information on the university applications of prospective students submitted through the centralized system.

In addition to the university application data, we make use of the Swedish Income and Wealth Registry, which was compiled by Statistics Sweden (SCB) using data on wealth taxation. The wealth tax was abolished in 2007, but the registry contains highly detailed information on real and financial wealth of every individual residing in Sweden between 1999 and 2007. The wealth information is highly accurate, as banks and financial institutions reported all asset holdings directly to the tax authorities. Specifically, the dataset provides information on global assets, disaggregated to the individual security or property level, held by residents as of December 31 of each year.[11]

We match these two datasets, using pseudonomized social security numbers. The SCB also provides detailed information on the demographic and labor market characteristics of all individuals residing in Sweden. The demographic data include variables such as university enrollment and graduation, high school performance, gender, age, marital status, labor income, employment status, and information on family ties—allowing us to measure the characteristics of the applicants' parents.

[8]This is especially important for highly selective programs like medicine. For several years, medical programs only admitted students with perfect GPA, which meant that all admitted students were subject to tie-breaking. When ties were broken based on how applicants ranked the alternative, the result was that only some of those who ranked the alternative as their first choice were admitted. In such situations, the incentive to include a safe option increases. However, for business programs during this period, admission cutoffs were almost never at the level of perfect scores.

[9]Institutions were not required to offer their programs through the centralized system until 2005. While most institutions participated from the beginning of our sample period in 1977, additional schools joined over time or included only a subset of their offered programs.

[10]Note that data are not available for the fall 1992 semester, when the newer admission system was implemented.

[11]The Swedish Income and Wealth Registry has been fruitfully used in earlier research for various purposes. See, e.g., Calvet, Campbell, and Sodini (2007), Calvet, Campbell, and Sodini (2009a), Betermier, Calvet, and Sodini (2017), Bach, Calvet, and Sodini (2020), and Bali et al. (2023) for a detailed description of the dataset.

To calculate stock portfolio characteristics, we use auxiliary information on daily and monthly data for returns, shares outstanding, share volume and balance sheet data on all companies listed on the Swedish stock market for the period from January 1988 to December 2018 from Thomson Reuters Datastream.[12] Using this information, we calculate for each individual in each year the stock portfolio returns,[13] and other portfolio characteristics such as the portfolio beta and the size, momentum, and value loadings over the period 2000-2007.[14]

When calculating the portfolio returns for each individual, we focus on their holdings in single stocks listed on the Swedish stock market, which is motivated by several reasons. First, this choice allows us to accurately measure and control for various sources of compensated risk factors such as market, size, value, and momentum in the return regressions. Second, direct stocks, unlike mutual funds, typically involve no substantial heterogeneous or hidden fee and expense structure that can affect returns and overall portfolio performance. Third, our focus on Swedish-domiciled stocks limits any concern that differences in portfolio performance may be partly due to differences in households' access to international markets or other alternative investment vehicles (e.g., private equity or venture capital investments). Nevertheless, we also use an alternative measure of portfolio returns, focusing on the returns on the total risky asset portfolio, in order to verify the robustness of our findings.

2.3 Sample Construction and Descriptive Statistics

When constructing the sample for our empirical analysis, we proceed as follows: First, we identify the admission cutoffs for each alternative. The cutoff is the lowest score among all admitted students in an admission group for a given alternative in a given application period. Note that cutoffs are defined for admissions only if there are both admitted and non-admitted applicants at the end of the application round. We exclude from the sample

[12]Daily and monthly returns for each stock are calculated using the total return index adjusted for stock splits and dividend payments. We report returns in US dollars. We also follow other international stock market studies such as Bekaert, Harvey, and Lundblad (2007) and Karolyi, Lee, and Van Dijk (2012) to screen the data and omit some of the data errors in Datastream reported in the prior literature. We refer the reader to Bali et al. (2023) for further details. In addition, the monthly returns are winsorized at the 1% (99%) level for the left (right) tail for each month. To ensure that our results are not driven by penny stocks, we exclude stocks trading below USD 1 per share.

[13]Note that we use end-of-period stock holdings in year t, i.e., measured on December 31 of each year, and average monthly stock return data from year $t+1$ to calculate the stock portfolio returns of individuals in year $t+1$. Hence, we focus on the time period between 2000 and 2007 in the return regressions.

[14]Stock-level sensitivities are calculated as the slope coefficients from rolling regressions of excess stock returns on the global Asness and Frazzini (2013) model with the global market, size, value, and momentum factors constructed using stocks traded in 22 developed countries with 36 months of data to month t. Using the value weights of the securities in a household's stock portfolio, we then aggregate them at the portfolio level for each household.

those applicants who were admitted in non-standard admission groups or to institutions that offer practice-based programs.[15]

We use the admission status and scores of applicants from the second round of admissions, while taking into account the preference rankings of the alternatives they submitted for the first round. The reason for this is that changes in preference ranking after the first round of admissions (withdrawing from a higher ranked alternative to which one was not admitted) may be influenced by the outcome of the initial allocation. Econometrically, such selection, if not accounted for, could lead to biased estimates. Because applicants who end up below the cutoff often decide to leave the waitlist, many applicants who remain on the waitlist end up being admitted. Thus, using first-round cutoffs would imply that many applicants who end up being admitted are incorrectly predicted to be below the cutoff. On the other hand, using the cutoffs from the second round with the rankings from the first round protects against manipulation while ensuring an adequate first stage.

Next, we collapse the admission groups for each alternative and use only the group in which a given applicant performed best, i.e., where they had the highest relative score. If above the cutoff, this is the admission group to which the applicant was admitted. If an applicant scored below the cutoff in all admission groups, we select the group where they would have been admitted if the cutoff had been slightly lower.

To identify the correct counterfactual, we drop dominated alternatives. These are program-institution combinations to which individuals apply, but where higher ranked alternatives have lower cutoffs. If the applicants are above the cutoffs to such alternatives, they are also above the cutoffs to the higher-ranked alternatives, making admission impossible.

Finally, we collapse the applications by field of study and consider only those cases where the consecutively ranked alternatives in the individual preference list are in different fields. For example, if an applicant first ranked two business programs, then three medicine programs, and finally one technology program, we collapse their ranking into (1) business, (2) medicine, and (3) technology. In each collapsed field of study, we keep the alternative where the applicant performed the best (they had the highest score relative to the cutoff). We then create observations of pairs of preferred (j) and counterfactual (k) fields. Since we are interested in understanding the causal effects of having a business or economics education on portfolio returns and household wealth, we restrict our sample to only those applications where the preferred alternative j is a business program and the counterfactual choice k is a non-business program. Programs may be offered at the same institution or at different institutions. Specifically, we use a broad definition of business education

[15]This includes admissions to programs that select on the basis of prior college credits and those who were readmitted after military service. Each year, a subset of applicants are drafted into military service and, if admitted, are allowed to defer the start of their studies. They must reapply after completing their service, but are then guaranteed admission through a special admission group.

that includes programs such as business, administration, economics, finance, commerce, management, organisation, and industrial economics.[16] Programs in all other fields are defined as non-business. The final sample comprises around 34,000 unique applicants who are observed at least once during 1999-2007, which results in more than 300,000 applicant-year observations.[17]

Table 1 reports summary statistics for a sample of college-educated individuals, as well as for individuals with business degrees and those in our final sample. Columns (1) and (2) present samples from the college-educated population, drawn to match the joint distribution of birth year, gender, and immigrant status of the individuals in our sample.[18] Not surprisingly, about 40% of our sample has a business degree, compared to about 17% of the population of university degree holders. As indicated by the standardized high school GPA and cognitive skills—as measured by IQ tests during military enlistment—our sample performs slightly better than the average, but the differences are small. Individuals in the study sample are also slightly more successful in the labor market, and somewhat more likely to have parents with a college education.

Overall, when compared to a randomly drawn group of university educated individuals of similar birth year, gender, and immigrant status (column 1), our study sample is somewhat positively selected on cognitive ability, labor market outcomes, and socioeconomic status. However, when we restrict our comparison to individuals with business degrees, most of the differences in means and dispersion disappear. As shown in Figure O.A.2 in the online appendix, the similarities persist across the distribution of average returns, net wealth, and earnings. Because the regression discontinuity design selects individuals who apply to competitive programs (where a cutoff exists), the distribution of cognitive skills shows that individuals in the sample are more likely to be found at the top.

While our study sample is very similar to the population of business educated individuals, and only somewhat positively selected when compared to university degree holders, the differences between the sample and the full population of Swedes are of course large. These differences could potentially limit the external validity of our results. In addition to sample differences, we also underscore that we are estimating local average treatment effects of a group of individuals who comply with treatment assignment. Although this design allows us to identify the causal effects cleanly, as in many natural or quasi-natural experimental settings, this comes at the potential cost of the generalizability. We will turn to this issue when we analyze the treatment effects heterogeneity in Section 4.5, where we provide evidence supporting the strength of our findings across different demographic groups. There we find the strongest effects for those with parents who have no university

[16]We use the SUN classification codes 340-345, 349, and 526 to identify business-related programs.

[17]Note that our dataset is a panel with multiple observations per treatment, as we include each observation-year separately both to increase the precision of our estimates and to study the dynamics of financial behavior and wealth accumulation of individuals.

[18]Figure O.A.1 in the online appendix shows how well the distributions match.

Table 1: Summary Statistics

	University educated	Business educated	Study sample
	(1)	(2)	(3)
High school GPA	0.57	0.70	0.84
	(0.92)	(0.89)	(0.82)
Cognitive skills (men only)	6.66	6.52	6.92
	(1.57)	(1.47)	(1.38)
Has university degree	100.00%	100.00%	73.81%
	(0.00)	(0.00)	(0.44)
Has business degree	17.38%	100.00%	38.25%
	(0.38)	(0.00)	(0.49)
Works in finance (age 35)	3.31%	11.87%	7.77%
	(0.18)	(0.32)	(0.27)
Unemployed (age 35)	10.49%	7.10%	5.97%
	(0.31)	(0.26)	(0.24)
Earnings percentile (age 31-35)	0.58	0.71	0.72
	(0.30)	(0.29)	(0.28)
Parental earnings percentile (age 14-18)	0.57	0.59	0.61
	(0.20)	(0.19)	(0.19)
Parent has university degree	35.73%	38.09%	41.98%
	(0.48)	(0.49)	(0.49)
Parent has business degree	3.10%	5.33%	4.80%
	(0.17)	(0.22)	(0.21)
Net wealth (USD)	111,379	175,078	175,059
	(196,236)	(245,557)	(235,583)
Financial wealth (USD)	44,736	66,304	65,924
	(73,165)	(94,681)	(90,838)
Homeowner	64.44%	67.38%	71.14%
	(0.38)	(0.37)	(0.36)
Stock market participation	75.07%	81.23%	83.10%
	(0.36)	(0.32)	(0.30)
Average portfolio returns	0.52%	0.44%	0.47%
	(0.02)	(0.02)	(0.02)
Observations	200,000	100,000	34,333

Notes: This table presents summary statistics for three samples. Columns (1) and (2) represent samples from the population drawn to match the birth, gender, and immigrant status of the study sample. Column (3) summarizes the characteristics of the study sample (within the bandwidth). Figure O.A.1 in the online appendix reports the accuracy of this matching. High school GPA is normalized by cohort, cognitive skills—tested in an IQ test during military enlistment—is reported on a standardized discrete scale between 1 and 9. Earnings percentiles are cohort percentiles based on the 5-year earnings averages. Average portfolio returns correspond to the (equally weighted) sample mean of the average monthly returns of the individual's direct equity portfolio over the 2000 and 2007 periods. All wealth variables are calculated as averages over the full 8-year period for which we have data (1999-2007). Figure O.A.2 in the online appendix reports distributions for key variables.

education. This speaks to the possibility that our reported estimates, based on a positively selected sample, could be understated, and that the treatment effect of providing business education to the general public could be even larger.

3 Empirical Strategy

To formally examine the effects of having business education on household financial behavior and wealth outcomes, we employ a regression discontinuity design (RDD) which allows us to identify the causal effects under fairly weak assumptions (Lee and Lemieux 2010).

As described in section 2.3, we consider applicants who prefer to study business at the university level and have a non-business program as their counterfactual alternative. We then compare the financial decisions and outcomes of those applicants who are slightly above the admission cutoff and those who are slightly below. As long as the control function is continuous at the cutoff, the allocation of business education among these applicants can be considered quasi-random. We exploit a large set of such cutoffs for different business programs at different institutions over several years. Hence, our empirical strategy can be considered as pooling of a large set (around 3,500 in total) of "natural experiments" of admission to business education programs with fixed effects for each such experiment.

Our estimation is based on the following reduced-form specification:

$$Y_{iT} = \beta \cdot \mathbf{1}(a_{ic} \geq 0) + f(a_{ic}, \theta^{\alpha}) + \gamma \cdot X_i + \tau_t + \tau_T + \tau_p + \tau_b + \tau_c + \varepsilon_{iT} \tag{1}$$

where Y_{iT} is the outcome of interest for applicant i in year $T \in \{1999, \ldots, 2007\}$. These outcomes are, in turn, stock market participation, value of stock market investments, and portfolio returns. Since the financial behavior considered is relevant for wealth accumulation, we also consider household-level wealth outcomes, such as the level of financial and net wealth and the percentile rank in the wealth distribution. Note that all these outcomes are observed t years after application, where t can take a value between 4 and 25.

$f(a_{ic}, \theta^{\alpha}) = \theta_0^{\alpha} a_{ic} + \theta_1^{\alpha} a_{ic} \mathbf{1}(a_{ic} \geq 0)$ is a linear polynomial of the cutoff-centered running variable, a_{ic}, that is estimated separately for each admission group α, above and below the cutoff. X_i is a vector of predetermined individual characteristics that includes indicator variables for the applicant's gender, and whether the applicant is foreign born. We also include fixed effects for year of birth (i.e., τ_b). To control for macroeconomic and other time-varying aggregate factors, we include additional time fixed effects: for the year of observation of the outcome variable, τ_T, and for the number of years since application, τ_t. Because we pool all individual observations and include fixed effects for t and T as well as for year of birth, our estimates should be interpreted as a weighted average of

the causal effect of business education on household outcomes measured 4–25 years after application during the 1999-2007 period. In addition, we include fixed effects for the priority ranking of the business alternative in the application, denoted by τ_p. Finally, τ_c are cutoff fixed effects, where each admission cutoff is a unique combination of semester, program, institution, and admission group. In all regressions, standard errors are two-way clustered by applicant and admission cutoff.

To estimate the causal effects of having a business or economics education on household financial behavior and outcomes, we use a "fuzzy" design and instrument enrollment in a business program within five years of application (*Enrolled*) by whether the applicant is above the admission cutoff. More formally, our regressions take the following form:

$$Y_{iT} = \beta \cdot Enrolled_{it_0} + f(a_{ic}, \theta^{\alpha}) + \gamma \cdot X_i + \tau_t + \tau_T + \tau_p + \tau_b + \tau_c + \varepsilon_{iT} \quad (2)$$

$$Enrolled_{it_0} = \pi \cdot \mathbf{1}(a_{ic} \geq 0) + f(a_{ic}, \theta^{\alpha}) + \omega \cdot X_i + \eta_t + \eta_T + \eta_p + \eta_b + \eta_c + u_{it_0} \quad (3)$$

We measure enrollment based on whether the applicants registered for at least one course in a business or economics program within five years of initial treatment.[19] In additional robustness checks, we use a different definition of treatment, namely an indicator variable for graduating from a business program within 8 years of application.[20]

Under the standard assumptions of the instrumental variable (IV) estimator, the parameter β captures the local average treatment effect (LATE) of enrolling in a business program on the outcome of interest (i.e., Y_{iT}).[21] Thus, we are able to estimate the impact of having a business education in a group of individuals who comply with the treatment assignment, i.e., enroll in a business program if they are above the cutoff and enroll in a non-business program if they are below the cutoff.

Since the sampled applicants, by construction, all prefer business relative to their next-best (non-business) alternative, there is likely a group of always-takers who will reapply and enroll in a business program at a later date. Since pairs of preferred and next-best alternatives should be ranked in order of relative preference, no individual becomes less inclined to enroll in a business program by crossing the threshold, meaning that the monotonicity, and thus the assumptions of the LATE theorem, should hold.

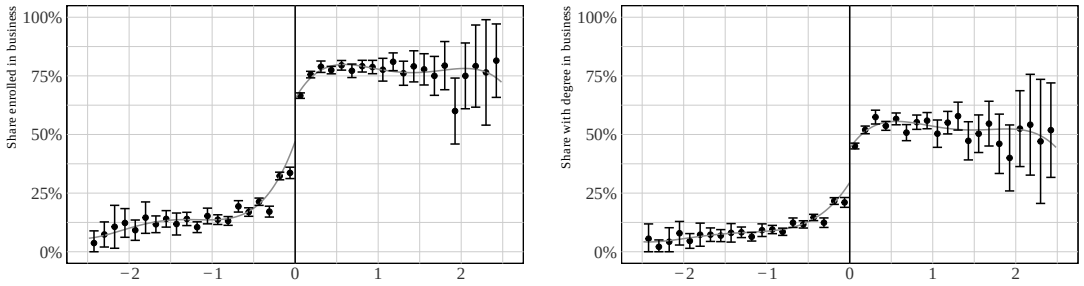
[19]We use a long period of five years to ensure that we correctly classify as always takers all applicants who were below the initial cutoff but then reapplied and were admitted to business in a later year.

[20]Note that the results of the analysis with a business degree (rather than enrollment in a business program) should be interpreted with caution. This specification may not satisfy the exclusion restriction, and the estimates may be biased because threshold-crossing is likely to affect household financial behavior in ways other than through degree completion.

[21]Independence is satisfied by quasi-random assignment and exclusion is satisfied since there are no other ways that threshold-crossing could affect our outcomes than through enrollment. Figure 1 shows the validity of the first stage. Monotonicity requires that threshold-crossing makes no applicant *more* inclined to enroll in the next-best option. This is ensured by the fact that for a pair of preferred and next-best alternatives the applicant has no reason to rank them in any order other than their true preference. We avoid studying the treatment effect of business education when it is the counterfactual alternative, for this reason, as it would likely violate the monotonicity assumption.

For the 2SLS estimator β to be an unbiased estimate of the LATE, however, recent research has identified additional requirements when covariates are included in the specification.Blandhol et al. (2022) show that if the estimated model is not saturated, the estimand will in fact contain negatively weighted always-takers. Since the assignment for each cutoff is quasi-random, including cutoff fixed effects ensures that the instrument is exogenous and thus that the model is saturated. Fort et al. (2022) make a similar argument, showing that cutoff-level fixed effects are required when pooling over multiple cutoffs for unbiased estimates of the ATE.

Figure 1: Enrollment and Degree in Business around the Admission Threshold



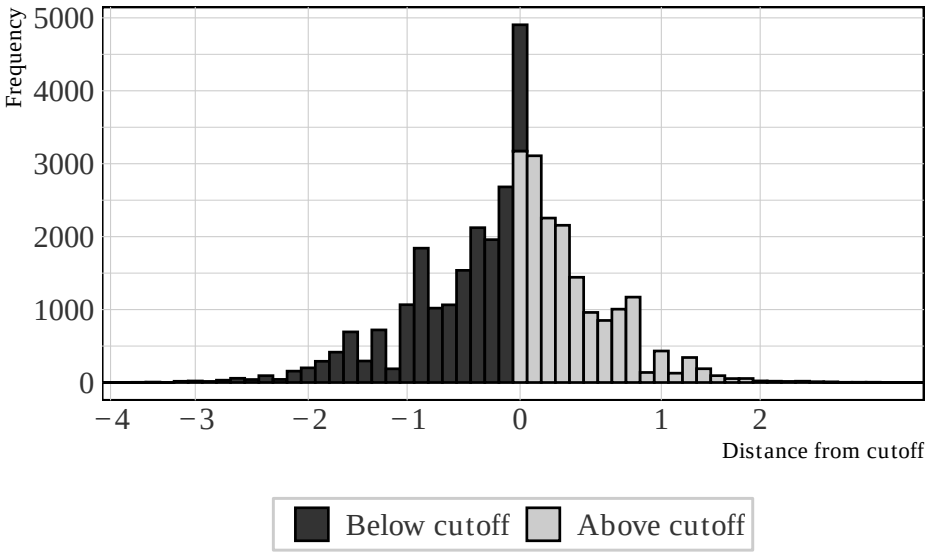
Notes: The left panel illustrates enrollment in a business program, and the right panel depicts the business degree completion around the admission cutoff among applicants who apply to a business program with a non-business counterfactual.

Figure 1 illustrates the first stage for both enrollment and business degree completion. We see a clear jump in the probability of enrolling in a business program in five years and in earning a business degree in 8 years around the admission cutoff. These results are also confirmed by the regression estimates reported in Table O.A.1 in the online appendix. Specifically, we estimate equation 3 and regress being enrolled or having a business degree on an indicator variable for threshold-crossing, individual demographic characteristics, and fixed effects for each cutoff. Being above the cutoff significantly increases the probability of enrolling in a business program by 54 to 56 percentage points, depending on the regression specification, which shows a strong first stage.

For RDD to properly identify a causal treatment effect, it should not be possible to precisely manipulate assignment around the cutoff. Since the cutoffs change each year depending on the scores of all applicants, an individual has no way of knowing ex ante whether they will be admitted, making such manipulation unlikely. We present two figures to confirm that this identifying assumption holds. Figure 2 shows that the running variable is evenly distributed around the cutoff, and Figure 3 further shows that the predetermined covariates are balanced.

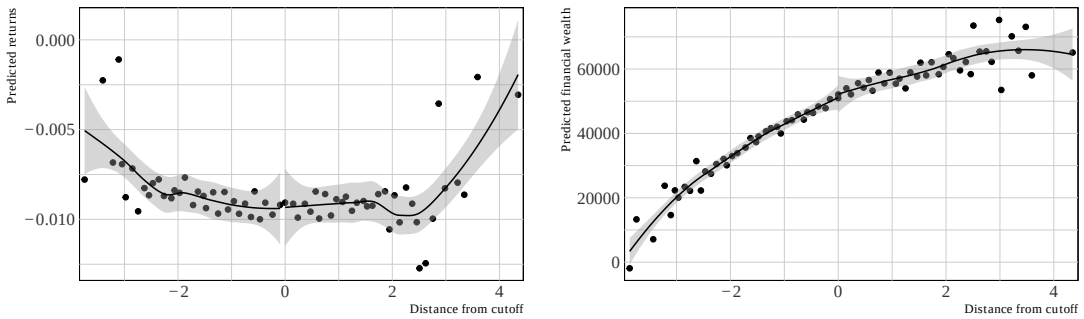
Finally, a key parameter in any regression discontinuity design is the bandwidth. Normally, optimal bandwidth algorithms can be used to find the best balance between

Figure 2: Distribution of Admission Scores around the Admission Threshold



Notes: This figure illustrates a histogram of the distribution of observations around the admission cutoff. Observations exactly at the cutoff are sorted in a separate bar. These individuals are admitted using different tie-breaking mechanisms, and are counted in the analysis as either above or below the cutoff depending on what their predicted admission status is. That the number of observations is balanced around the cutoff show that applicants cannot precisely influence admission.

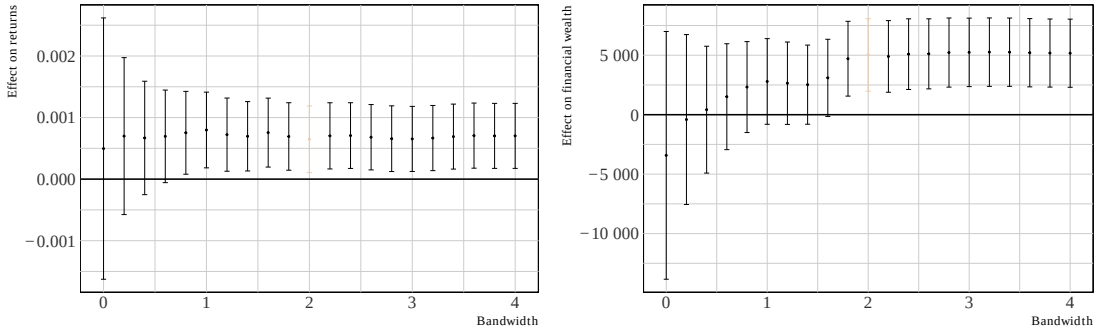
Figure 3: Covariate Balance around the Admission Threshold



Notes: This figure plot shows predicted levels of two outcomes used in the paper, portfolio returns and financial wealth, for different values of the running variable. Various predetermined characteristics are included in the regression, including admission score, gender, age, and parental education. That there are no discernible jump in the predicted value around the cutoff indicates that assignment to business education has not been manipulated.

bias and variance. However, because our analysis pools a large set of cutoffs, no chosen bandwidth will be optimal for all cutoffs. Instead, we use a bandwidth of 2 standard deviations throughout the paper, and show in Figure 4 that our key results are not sensitive to this choice—while a smaller sample obviously reduces statistical power, changing the bandwidth has little effect on the point estimates.

Figure 4: Bandwidth Selection



Notes: This figure shows the predicted results of some of the main outcome regressions, but for different bandwidths. Throughout the paper, we use a bandwidth of 2 standard deviations. The plot shows that the point estimates do not change much as the bandwidth changes, although we observe that, not surprisingly, a smaller sample leads to more noise in the estimates.

4 Business Education and Household Financial Behavior

This section examines the impact of quasi-random enrollment in a business or economics program on household financial outcomes.

4.1 Base Results

We begin our empirical analysis by estimating the causal effects of enrolling in a business-related program on several dimensions of household financial behavior, including stock market participation, the value of stock holdings, and the returns on the stock portfolio.

Table 2 presents the estimation results. For brevity, we report only the coefficient estimates on the variable for enrollment in a business or economics program. In all regressions, we include linear polynomials of the running variables, individual control variables, and a battery of fixed effects, including fixed effects for each admission cutoff, the number of years since application, the applicant's birth year, the priority ranking of the business alternative in the application, and the year of observation when the household's financial behavior is observed.

As a prelude to our instrumental variable estimates, Panel A of Table 2 reports the reduced form regressions as shown in Equation 1. Panel B, which is our preferred specification, and Panel C present the second-stage estimates from the IV regressions as outlined in Equation 2, where we instrument enrollment in a business program and business degree completion with being above the cutoff at the time of admission, respectively.

In column (1) of Table 2, we first estimate the causal effects of having a business education on the likelihood of investing in the stock market. The dependent variable is an indicator variable for whether the household holds stocks, either directly or indirectly through mutual funds, excluding holdings in retirement accounts.[22] As shown in Panel B, the coefficient on enrollment is estimated to be positive, but it is neither statistically nor economically significant at any conventional level. This result remains the same when we use different treatment definitions, as shown in Panels A and C, or when we consider the direct stock ownership as the outcome variable, as presented in Panel A of Table O.A.2 in the online appendix. In fact, the absence of a significant effect on households' stock market participation decisions in our setting is not so surprising. Stock ownership is widespread in Sweden, especially among households with some college education (see Table 1). In addition, our sample of applicants includes only individuals who have a preference for studying business or economics and are therefore likely to have an above-average interest in financial issues.

Next, we focus on the intensive margin of financial risk-taking, using the value of direct and indirect stock investments as the outcome variable. As shown in column (2) of Table 2, individuals with a business education have significantly greater exposure to the stock market. Specifically, conditional on stock market participation, business education leads to an increase in individuals' stock holdings of about USD 6,740 (t-stat. = 2.54), which corresponds to an increase in mean stock wealth of about 20%. In additional analysis presented in Panel B of Table O.A.2 in the online appendix, we consider the value of direct stock holdings as the outcome variable and find similar results. Specifically, we document that enrollment in a business program increases direct stock investments by about USD 4,550, which accounts for more than two-thirds (=4,550/6,740) of the total contribution to the total stock portfolio. Thus, the effect of business education on increased household exposure to the stock market operates primarily through its effect on direct stock investments.

An important question is whether individuals who have some business education, and thus likely a higher level of financial literacy, are able to earn higher returns on their risky investments. In other words, does the positive impact of business education extend to returns on stock investments? Our dataset provides a unique opportunity to address this

[22]As the wealth data were collected to assess wealth taxes, stock holdings under the mandatory first pillar of Social Security and in tax-deferred retirement accounts are not included in our data because they were not part of the tax base.

Table 2: Household Financial Behavior and Business Education

Panel A: Reduced Form				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Above_Cutoff	0.0004	2793.978**	0.066**	0.053**
	(0.08)	(2.54)	(2.38)	(2.11)
Obs	297,633	254,653	111,906	111,906
Panel B: IV Estimates: Enrollment as Treatment				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.001	6737.266**	0.155**	0.126**
	(0.08)	(2.54)	(2.35)	(2.09)
Obs	297,633	254,653	111,906	111,906
Panel C: IV Estimates: Degree as Treatment				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Degree	0.001	9217.377**	0.199**	0.162**
	(0.08)	(2.54)	(2.34)	(2.08)
Obs	297,633	254,653	111,906	111,906
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	Yes

Notes: This table presents regression estimates of household financial behavior. Panel A reports the reduced-form regressions as shown in Equation 1. Panel B and Panel C present the second-stage estimates from the IV regressions as outlined in Equation 2, where we instrument enrollment in a business program and obtaining a business degree with being above the cutoff at the time of admission, respectively. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions in column (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

question, which have important implications for the ongoing discussion of the importance of asset returns for wealth inequality and the various sources of return heterogeneity (Bach, Calvet, and Sodini 2020; Campbell, Ramadorai, and Ranish 2019; Fagereng et al. 2020; Lusardi, Michaud, and Mitchell 2017).

Following Fagereng et al. (2020), we incorporate the (one-year lagged) value of the equity portfolio, the interaction of the time-year dummies and the equity share of financial assets, in addition to other control variables and a full set of fixed effects in the return regressions. The former allows us to account for the effects of participation costs (Vissing-Jorgensen 2003) and scale effects, which can be highly relevant, for example, through easier access to high quality information or better investment opportunities to generate higher returns (Bach, Calvet, and Sodini 2020; Kacperczyk, Nosal, and Stevens 2019), while the latter controls for differences in access to the menu of financial instruments (Chien, Cole, and Lustig 2011; Fagereng et al. 2020). We also include the portfolio beta in the regressions to capture variation in risk exposure across individuals. When controlling for the differences in risk exposure and scale effects, we use the beginning-of-the-period portfolio size, stock share in financial wealth, and portfolio beta (all measured before portfolio returns) to limit the potential simultaneity bias. It is standard and essential to account for these effects, particularly for the scale effects, in the portfolio return analysis (e.g., Fagereng et al. 2020; Bianchi 2018). For example, Bach, Calvet, and Sodini (2020) quantify the relative contribution of scale effects to expected returns on gross wealth and show that scale dependence accounts for more than one-third of the variation in gross wealth returns. Finally, to alleviate any concerns that the return results are driven by small portfolios, we apply a size filter and include stock portfolios of at least USD 500, which corresponds to the 10th percentile of the portfolio size distribution.

The regression results are presented in column (3) of Table 2. We find positive and significant effects of business education on stock portfolio returns, with t-statistics ranging from 2.34 to 2.38 depending on the treatment definition. Specifically, based on the estimates of our preferred specification presented in Panel B, quasi-random enrollment in a business-related program increases the monthly stock portfolio returns by about 15 basis points. This effect corresponds to an annualized return differential of 1.86 percentage points between business and non-business educated individuals, highlighting the economic importance of financial education in generating higher portfolio returns.

We perform several sensitivity checks to ensure the robustness of our findings. First, we rerun the return regressions using different portfolio size filters. The results, presented in Table O.A.3 in the online appendix, show that our results are robust to relaxing or using alternative size filters. Since we so far have focused on the returns of the direct equity portfolio, we next use an alternative measure of returns where we calculate the raw returns of the entire portfolio of risky assets for which we are able to collect price data. As shown in Table O.A.4, we again find a positive effect of enrolling in a business program

on the returns to the entire risky portfolio, suggesting that our results are not limited to individuals' direct stock investments. Third, we recognize that including controls for risk exposure and scale effects in the return regressions, as they are defined post-treatment, carries the risk of introducing confounding effects if these variables are influenced by unobservable characteristics that are not captured by our regression specification. This creates a trade-off between controlling for important determinants of portfolio returns and using potentially endogenous right-hand-side variables in the analysis. To address this issue, in Table O.A.5 in the online appendix, we conduct a sensitivity analysis in which we remove all individual-level controls, including portfolio size and measures of risk, from the regression model. We further restrict the sample to the first 14 years after the initial enrollment, motivated by our finding in Section 4.3 that there exists no significant differences in wealth in the short run, thereby limiting any distortions of potential scale effects. Reassuringly, we still find that business education has a statistically significant and economically meaningful positive impact on portfolio returns even after excluding individual-level controls. The point estimates are very similar to those in the baseline analysis, with t-statistics ranging from 2.27 to 2.59. Thus, we conclude that our return results are not simply an artifact of potential bias introduced by the use of post-treatment controls or differences in wealth between business and non-business educated households. Finally, Kirkebøen, Leuven, and Mogstad (2016) note that, in an empirical setting similar to ours, it is important to control for the next best alternative for the identification and causal interpretation of the estimates. In Table O.A.6 in the online appendix, we present results controlling for next best major fixed effects. Since we, by construction, have applicants who have a business or economics program as their preferred major and a non-business program, such as engineering, science, or humanities, as their counterfactual one, these fixed effects are analogous to two-way interacted fixed effects for preferred and next best alternatives (Altmejd et al. 2021). As shown in the table, the results are very similar to those observed in the baseline analysis.

4.2 Understanding the Mechanism

What is the main mechanism through which having business education affects portfolio returns? First, we recognize that the documented return differences between business and non-business educated individuals may be due to business education making individuals more willing to take on financial risk, as shown by Bach, Calvet, and Sodini (2020) and Campbell, Ramadorai, and Ranish (2019). We directly address this explanation by controlling for portfolio beta and differences in access to the menu of financial instruments across sampled individuals in the baseline analysis. To further refine our understanding of the role of risk-taking in our findings, we next extend our return regressions by including the size, momentum, and value loadings of the equity portfolio to better capture differences

in exposure to different sources of (compensated) risk. The results, presented in column (4) of Table 2, closely mirror those from the baseline analysis. The economic magnitude of having a business education declines, however, by about 23% (from 15.5 to 12.6 basis points) once we account for these additional risk factors.

Second, heterogeneity in innate ability across households could also contribute to the documented return differences if more skilled and talented individuals sort themselves into business programs (Fagereng et al. 2020). Since we by design are contrasting the stock portfolio performance of individuals with similar initial abilities and preferences, we implicitly control for such heterogeneity in our empirical analysis. This means that our results cannot be biased by positive selection on ability to business programs. However, there is heterogeneity in admission requirements across business programs, and it is possible that our identified treatment effect is driven by outliers. In additional tests, we therefore exclude applicants who are in the top 10 percent and bottom 10 percent of the high school GPA distribution. As shown in Table O.A.7 in the online appendix, the positive coefficient on enrollment retains both its statistical and economic significance even after excluding the most and least able applicants from the sample.

If the observed effects cannot be fully attributed to heterogeneity in risk exposure or innate ability, what explains the positive contribution of business education to portfolio returns? One compelling explanation is that individuals, who quasi-randomly enroll in business, are likely to accumulate higher levels of financial literacy. This, in turn, improves individuals' ability and capacity to process economic information and allows them to make more informed investment decisions (Lusardi and Mitchell 2014). Although our dataset is rich in many dimensions, it does not provide any direct information on the level of financial literacy of individuals, which is typically elicited through the Big Three survey questions developed by Lusardi and Mitchell (2007).[23] Therefore, we take an indirect approach to examine the role of financial literacy in explaining the observed return differences between business and non-business educated individuals.

The enhanced ability of business educated individuals to acquire and process economic information is particularly important for generating higher returns when the return to information is higher, or in other words, when the price system is less informative (Grossman and Stiglitz 1980). To test this idea, we split the sample into relatively good and bad market conditions using the median values of market returns and annual volatility of the stock price index in Sweden between 2000 and 2007.[24] This analysis is based on the

[23]The survey work of Almenberg and Säve-Söderbergh (2011) provides some support for the increased financial literacy interpretation. The authors focus on a representative Swedish sample and document that education-based measures of financial literacy correlate closely with the financial literacy measure developed by Lusardi and Mitchell (2007). Almenberg and Säve-Söderbergh (2011) show that respondents with a major in economics are significantly more financially literate than households with other majors such as social sciences, arts and humanities, and medicine.

[24]We measure aggregate market returns using the MSCI Sweden return index (denominated in SEK) obtained from Datastream. The data for the volatility of the stock price index in Sweden are obtained

idea that the value of information acquisition and processing is higher particularly during market downturns and periods of high aggregate volatility (Kacperczyk, Nosal, and Stevens 2019). Table 3 reports the regression estimates. We find that the positive contribution of having a business education to portfolio returns is only confined to market downturns and when the aggregate market volatility is high. The estimated effect sizes are also large in magnitude, ranging from 21 to 28 basis points per month. On the other hand, the estimated effects are both economically and statistically insignificant in relatively better times, when the aggregate market returns are higher and the volatility is lower. The return regressions reported in Table O.A.8 in the online appendix, where we split the sample by the idiosyncratic volatility (IVOL) and liquidity of the direct stock portfolio, further support the increased financial literacy interpretation.[25] We find that having business education significantly increases the portfolio returns only when the underlying stocks have higher IVOL and lower liquidity. For portfolios with low IVOL and high liquidity, however, the effect is economically and statistically indistinguishable from zero. Taken together, the documented asymmetric effects strongly support the notion that business education increases the financial literacy of the treated individuals, thereby enhancing their ability to acquire, process, and use relevant information more effectively.

To further reinforce the interpretation that enrolling in a business program captures improvements in financial literacy, we examine its effect on common investment mistakes, including portfolio underdiversification and the disposition effect. We use the total number of stocks in the portfolio as a crude measure of portfolio diversification (Goetzmann and Kumar 2008). Following Odean (1998) and Calvet, Campbell, and Sodini (2009b), we measure the disposition effect, i.e., the tendency of individuals to hold losing stocks too long and sell winning stocks too early, as the difference between the proportion of realized stock gains and losses in a given year.[26] The results presented in Table O.A.9 in the online appendix show that individuals with business education tend to have more diversified portfolios and are significantly less subject to behavioral biases. These findings not only strengthen the interpretation of increased financial literacy, but also shed light

from the FRED database.

[25]To compute the portfolio IVOL, we first compute the IVOL of an individual stock (listed on the Swedish Stock Exchange) as the standard deviation of the residuals from time-series regressions of daily excess stock returns on daily excess market returns and daily size (SMB) and book-to-market (HML) factor returns in a month. Since we are only able to observe the stock investments of the sampled households at an annual frequency, we use the average IVOL of a stock in a given year. To compute the IVOL, we need at least 15 daily return observations (in a month). We then compute the value-weighted direct stock portfolio IVOL for each household in each year. Portfolio liquidity is constructed analogously to portfolio IVOL, where we first compute the illiquidity of an individual stock (listed on the Swedish Stock Exchange) as the absolute daily return divided by the daily dollar trading volume averaged over all trading days in each month. Using the portfolio IVOL and illiquidity of the sampled individuals, we divide the sample into three and define the portfolios in the upper (lower) tercile as high (low) IVOL and illiquidity portfolios.

[26]Our dataset does not record the purchase and sale prices of stocks. Therefore, following Calvet, Campbell, and Sodini (2009b), we define a stock as a winner (loser) if it had a higher (lower) average monthly return than the Swedish market returns over the past year.

on additional dimensions of household investment behavior that can be improved through financial education.

Overall, the causal evidence presented in this section on the sources of differential returns suggests that increased financial literacy plays an essential role in generating higher returns over and above differences in risk exposure and ability. Our results provide direct empirical support for the model predictions of Lusardi, Michaud, and Mitchell (2017) and Jappelli and Padula (2017), as well as, a credible micro foundation for the driving forces behind increased financial literacy.

4.3 The Effect of Business Education over Time

We now turn to analyzing the impact of business education on portfolio returns and household financial behavior over time. We do so by exploiting the unique feature of our dataset that allows us to observe the portfolio decisions and outcomes of sampled households up to 25 years after their initial application. We split the sample into two according to the median number of years since college application (i.e., 14 years) and estimate the effect of quasi-random enrollment in a business or economics program on household financial behavior in both the short run (4-14 years) and in the medium run (14-25 years).[27]

The regression results are reported in Table 4. First, similar to the baseline results, we observe no significant effect on household stock market participation in either the short or medium run. However, our analysis reveals an interesting pattern regarding the effect of business education on stock returns over time. Individuals with a business education earn about 26 basis points (t-stat. = 2.17) higher average monthly returns on the stock portfolio in the short run. In the medium term, however, this effect is economically and statistically indistinguishable from zero.

Despite the positive and significant effects of business education on portfolio returns in the short run, we find no systematic differences in the level of stock investments between business and non-business educated individuals over this period (t-stat. = 0.62).[28] As discussed in Section 4.1, this result provides additional evidence that the documented return differences cannot be fully attributed to scale effects (Bach, Calvet, and Sodini 2020; Gabaix et al. 2016), which in our context would suggest that differences in wealth and investment size between business and non-business educated individuals may, for example, lead to differential access to better investment opportunities or higher quality information, thereby generating the observed return differences. On the other hand, business education

[27]As we only have data on household portfolio holdings and wealth outcomes for the 1999-2007 period, the short-run and medium-run samples include those households that applied to a business program during the 1986-1995 and 1977-1993 periods, respectively.

[28]In unreported results, we also find no significant effect of business education on financial or net wealth of households in the short run.

Table 3: Household Financial Behavior and Business Education by Aggregate Market Conditions

Panel A: Good Market Conditions				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	-0.002	7010.125**	0.047	0.010
	(-0.12)	(2.47)	(0.64)	(0.14)
Obs	198,085	168,380	68,214	68,214
Panel B: Poor Market Conditions				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.006	6144.981**	0.275***	0.261***
	(0.42)	(2.57)	(2.89)	(2.92)
Obs	99,541	86,206	43,466	43,466
Panel C: High Aggregate Market Volatility				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.008	4940.976**	0.258***	0.210**
	(0.55)	(2.31)	(2.60)	(2.27)
Obs	135,370	114,356	44,093	44,093
Panel D: Low Aggregate Market Volatility				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	-0.004	8157.384**	0.089	0.078
	(-0.33)	(2.50)	(1.22)	(1.10)
Obs	162,239	140,216	67,534	67,534
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	Yes

Notes: This table presents the second-stage estimates of household financial behavior from the IV regressions as outlined in Equation 2, where we instrument enrollment in a business program with being above the cutoff at the time of admission. We split the sample into relatively good and bad market conditions using the median values of market returns and annual volatility of the stock price index in Sweden between 2000 and 2007. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions in column (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table 4: Household Financial Behavior and Business Education over Time

Panel A: Short-term Effects of Business Education (14 years$>t\geq$4 years)				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.005	1907.993	0.260**	0.261**
	(0.29)	(0.62)	(2.17)	(2.34)
Obs	133,427	111,917	42,737	42,737
Panel B: Medium-term Effects of Business Education (25 years$\geq t\geq$14 years)				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	-0.002	10096.126***	0.070	0.020
	(-0.13)	(2.91)	(0.92)	(0.29)
Obs	164,093	142,596	69,010	69,010
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	Yes

Notes: This table presents the second stage estimates of household financial behavior regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. In Panel A and B, we estimate the causal effect of enrolling in a business program on household financial behavior over the short run (4-14 years) and medium run (14-25 years), respectively. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions in column (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

leads to significant differences in stock wealth levels in the medium term. Specifically, households enrolled in a business or economics program have about USD 10,100 more in stock wealth than their non-business educated peers 14 to 25 years after enrollment (t-stat. = 2.91). This result highlights the potential dynamic effects of business education on wealth accumulation, which we discuss and analyze in more detail in section 5.

The observed asymmetry in the effects of business education on portfolio returns over time can be attributed to two factors that are also consistent with the interpretation of increased financial literacy as a result of enrolling in a business program. First, Lusardi, Michaud, and Mitchell (2017) show that the optimal financial literacy profile follows a hump-shaped pattern over the life cycle. This implies that having a business education can help households to build up financial knowledge in the short run, allowing them to earn higher returns on their investments. Over time, however, financial literacy, similar to any investment in human capital (e.g., Heckman 1976), may depreciate, for example, due to cognitive decline (Agarwal et al. 2009) or because the acquired knowledge becomes obsolete with the advent of new financial products (Lusardi, Michaud, and Mitchell 2017). Second, we do not observe significant differences at either the extensive or the intensive margin of stock investment in the early period. Thus, it is possible that non-business educated individuals improve their financial sophistication either directly through endogenous investments in financial knowledge or indirectly through learning-by-doing, reducing the initial financial literacy gap over time and earning comparable portfolio returns to their business-educated peers over the medium term.

4.4 Robustness to Alternative Interpretations

So far, we have interpreted the enrollment in a business or economics program as a direct investment in financial literacy, a view that is supported by our empirical findings. In what follows, we further scrutinize this interpretation and examine its robustness to alternative explanations.

First, one might worry that the documented positive effects may be driven by the level of education rather than its content. For example, recent studies find a positive association between educational attainment and returns to wealth (Fagereng et al. 2019; Girshina 2019).[29] This alternative could pose a threat to the interpretation of our results if individuals who were marginally admitted to a business program had higher college completion rates than those who were just below the admission cutoff. Indeed, we observe that the unconditional probability of earning a college degree within 8 years is significantly

[29]To provide a causal interpretation of the effects of educational attainment on returns, Fagereng et al. (2019) also use an exogenous increase in schooling requirements from 7 to 9 years. Interestingly, once the authors correct for the endogeneity of educational attainment, the correlation between educational attainment and returns disappears, which they interpret as the innate wealth management ability of households being the ultimate driver of higher returns to wealth and its components.

higher for individuals who were marginally admitted to a business program than for those who were not (0.79 versus 0.69). In the analysis presented in Table O.A.10 in the online appendix, we formally test this issue and find that, ceteris paribus, being above the admission cutoff significantly increases the probability of earning any college degree within 8 years by 3.2 percentage points.

To test for this alternative explanation, we next restrict the sample to those applicants who actually earned a college degree and re-estimate our regressions, reducing the sample size from 300,003 to 219,392 applicant-year observations. As reported in Panel A of Table 5, we obtain similar results. Specifically, being enrolled in a business or economics program increases average monthly portfolio returns by about 19 basis points (t-stat. = 2.67), suggesting that our results are not simply an artifact of potential differences in the level of education of the individuals in the sample. Rather, it is the content of the education that leads to better portfolio decisions.[30]

Second, the effect of business education on portfolio returns may also be manifested through the broader consequences of business education on individuals' labor market prospects, particularly through the unemployment risk and career paths (e.g., working in the financial industry). For example, Fagereng, Guiso, and Pistaferri (2017) notes that unemployment risk is one of the most important sources of background risk that can affect households' risk-taking and portfolio choices (Cocco, Gomes, and Maenhout 2005; Gomes, Jansson, and Karabulut 2024). If business education leads individuals to end up in jobs with greater job security and hence less uncertainty about labor income, it could allow them to take more financial risk and earn higher returns.[31] To address this concern, we define an indicator variable for being unemployed by using information on whether or not the individual received any unemployment benefits in a given year. We then regress this variable on quasi-random enrollment in a business program and individual controls and fixed effects. As shown in column (1) of Panel B of Table 5, we find no significant effect of business education on individual unemployment risk, although the point estimate is negative (-0.005; t-stat. = -0.25). This result suggests that the positive effect of business education on portfolio returns is not due to the differences in unemployment risk between individuals with and without business education. In contrast, as shown in columns (2) and (3) of Panel B, we do indeed find a positive and significant effect on the probability of working in finance and on individual earnings, raising the

[30]Of course, we acknowledge that conditioning on post-treatment outcomes introduces selection, and thus these results should be interpreted with caution. For example, individuals who respond to being below the cutoff by not completing college are likely to be those with the weakest connection to higher education and thus negatively selected in terms of financial returns. If anything, selection should bias these results downward.

[31]As noted by Ameriks, Caplin, and Leahy (2003), the willingness of households to take financial risks directly affects the returns to investment. Similarly, in a recent paper, d'Astous and Shore (2024) find that increased labor income uncertainty, identified by exogenous variation in college enrollment, affects stock market participation and household portfolio decisions.

Table 5: Household Financial Behavior and Business Education: Alternative Explanations

Panel A: University degree-holders only				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.004	9685.456***	0.189***	0.154**
	(0.25)	(3.05)	(2.67)	(2.40)
Obs	219,392	190,793	86,852	86,852
Panel B: Effect of business education on labor market outcomes				
	Unemployment	Works in Finance	Earnings (in SEK)	
	(1)	(2)	(3)	
Enrolled	-0.005	0.079***	29510.706***	
	(-0.25)	(4.52)	(4.45)	
Obs	300,003	300,003	277,333	
Panel C: Including only quantitative next-best fields				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	-0.002	5429.155	0.156*	0.128*
	(-0.11)	(1.48)	(1.90)	(1.74)
Obs	103,782	91,818	46,320	46,320
Panel D: Controlling for peer effects				
			Portfolio Returns (in %)	Portfolio Returns (in %)
			(1)	(2)
Enrolled			0.154**	0.126**
			(2.38)	(2.09)
Obs			111,906	111,906
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	Yes

Notes: This table presents the second stage estimates of household financial behavior and labor market regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. In Panel A, we focus only those applicants who complete their university education. Panel B considers the effects of having business education on labor market outcomes. Panel C focuses on applicants who have a business education as their preferred choice and a technology or science program as their next best alternative. In Panel D, we account for the portfolio-level overlap measure in the portfolio return regressions. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions in column (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

concern that the return effects of business education may be partly mediated through these channels. First, as discussed in Section 4.1, we recognize the importance of scale dependence in portfolio returns (Bach, Calvet, and Sodini 2020). Thus, we explicitly account for scale effects in all the return regressions, which would also serve to implicitly control for income differences between business and non-business educated households. Moreover, the positive and significant effects of business education on portfolio returns in the short run, where we find no differences in financial or net wealth (see Section 4.3), provide additional support for the notion that income differences are unlikely to drive the return results. In Table O.A.11 in the online appendix, we also perform a naïve mediation analysis, controlling for the level of earnings, unemployment risk, and working in the finance industry, and find very similar results. Interestingly, we see that the coefficient on working in finance turns out to be insignificant (t-stat. $= 0.87$) once we control for quasi-random enrollment in a business or economics program. In the untabulated results, we verify this finding using a causal mediation analysis similar to Dippel et al. (2022), and again find that working in the finance as a mediator does not significantly explain the total effect of having business education on portfolio returns. Taken together, these results suggest that while business education affects individuals' career paths and earnings, the causal effect of business education on portfolio returns is primarily a direct effect and is not mediated by labor market outcomes.

Third, an extensive literature documents that quantitative education and cognitive skills play an important role in household portfolio choice (Brown et al. 2016; Christelis, Jappelli, and Padula 2010; Grinblatt, Keloharju, and Linnainmaa 2011).[32] Against this background, we next turn to analyzing whether our results are primarily driven by improved financial literacy, or whether they are due to increased quantitative skills acquired through business or economics education. To do so, we focus on applicants holding a university degree, with a business or economics program as their preferred choice, and a technology or science program as their next best alternative. This subset of applicants in the control group allows us to focus on those individuals who were ultimately admitted to a non-business program where they can acquire and improve their quantitative skills, when they were just below the threshold for a business program. As shown in Panel C of Table 5, the coefficient on being enrolled in a business program retains its statistical and economic significance in the portfolio return analysis. In particular, individuals who are quasi-randomly assigned to a business program earn 13 to 16 basis points higher monthly returns relative to their peers who end up in a technology or science program. We find no systematic effects for the intensive and extensive margins of stock investments. In short, this result implies that increased financial literacy, rather than quantitative skills, is the key to higher portfolio

[32]For example, Brown et al. (2016) exploit variation in the adoption of financial and mathematics education reforms in U.S. high school curricula and show that increased mathematics education reduces the negative debt-related outcomes among young adults.

returns.

Finally, we consider peer effects as an alternative explanation. Individuals who study business may get access to financially more sophisticated peers, either through alumni networks or workplace associations, who can provide direct investment recommendations or pertinent information for stock investments. We address this explanation in several ways. First, although the existing literature documents positive peer effects on individuals' economic and financial decisions, such as stock market participation or saving for retirement (Duflo and Saez 2002; Haliassos, Jansson, and Karabulut 2020), it is worth noting that at the stock level Hvide and Östberg (2015) find that individuals do not earn significantly higher returns by investing in the stocks in which their (work) peers invest heavily.[33] If we were to take this evidence at face value, it would suggest that peer effects are unlikely to be the driving force behind our findings, since they go in the opposite direction of what we document in our return regressions.

In addition, we conduct a more direct test of peer effects by considering the entire population of households in Sweden. We first identify individuals who majored in a business-related program at either university or high school. We then exploit our ability to observe their stock investments at the security level and create a (stock-level) measure that captures the share of a given firm's outstanding stocks that is directly held by households with a business education. We construct the variable, Bus_Edu_Index, by sorting the stocks into percentile portfolios in ascending order based on the share of business educated investors in each year. By definition, higher values imply a higher concentration of business educated investors, and vice versa. Finally, using this stock-level measure, we compute a portfolio-level overlap score for each sampled household as follows:

$$Overlap^{P}_{i,t} = \sum_{j=1}^{N} Bus\_Edu\_Index_{j,t} \times \omega_{i,j,t} \tag{4}$$

where $Overlap^{P}_{i,t}$ is the stock portfolio overlap score of household i with other business educated individuals in year t, $Bus\_Edu\_Index_{j,t}$ is the overlap score of stock j in year t, and $\omega_{i,j,t}$ is the weight of stock j in the stock portfolio of household i in year t.

To test the potential role of peer effects in our findings, we then extend the regressions by including the portfolio-level overlap measure in the estimation model. The results, reported in Panel D of Table 5, show that the coefficient on enrollment in a business program retains its economic and statistical significance even after accounting for peer effects. In Table O.A.12 in the online appendix, we verify these findings by using alternative measures of peers.

Taken together, the numerous empirical findings presented in this section strongly support the interpretation that the estimated positive effects of business education are

[33]See, for example, Hwang (2023) for a recent review of the literature on peer effects and word-of-mouth communication in individual investment and financial decisions.

largely due to improved household financial literacy acquired through business education, rather than alternative explanations such as educational attainment, quantitative skills, unemployment risk, household career trajectories, or peer effects.

4.5 Effects of Business Education by Parental Background

A number of studies document significant intergenerational spillovers in both the educational attainment and earnings of individuals (Björklund, Lindahl, and Plug 2006; Black et al. 2020; Black, Devereux, and Salvanes 2005), which play a key role in intergenerational mobility (Black and Devereux 2010). In the context of financial literacy, there is evidence highlighting the importance of a link between one's own financial sophistication and that of one's parents.[34] Thus, it is important to understand whether increased financial literacy through business education complements or substitutes for the intergenerational transmission of financial sophistication.

Table 6 shows regressions of financial behavior for a sample breakdown based on whether at least one of the applicant's parents has some college education. We again consider three dimensions of financial behavior and portfolio choices, namely stock market participation, the value of stock market investments, and stock portfolio returns.

Importantly, we find that the positive contribution of business education to the level of equity investments and portfolio returns is only operative for households with less educated parents. In contrast, we find no systematic effects of business education on portfolio returns or any other dimension of financial behavior in the sample of households with more educated parents. For example, columns (3) and (4) in Panel B of Table 6 show that a business major leads to about 23 and 18 basis points higher monthly returns on stock investments for households with less educated parents, respectively, while the effect is statistically indistinguishable from zero for households with better educated parents.

The findings of this cross-sectional analysis suggest that having a business or economics education acts as a substitute for learning from parents, and thus for the intergenerational persistence of financial sophistication. This provides additional support for the improved financial knowledge interpretation of business education that we discussed in detail in the previous section. At the same time, the documented asymmetry across individuals based on parental sophistication also speaks to the external validity of our findings. Specifically, the observed disparities suggest that applicants from relatively more disadvantaged backgrounds may lack alternative access to financial knowledge, and thus financial education interventions may lead to more pronounced changes in their knowledge and financial behavior. The finding that treatment effects are not driven by those with higher parental

[34]See, e.g., Lusardi and Mitchell 2014 and the references therein.

Table 6: Household Financial Behavior and Business Education by Parental Background

Panel A: Parents with College Education				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.007	7349.597	0.071	0.064
	(0.40)	(1.52)	(0.72)	(0.72)
Obs	113,825	100,317	48,006	48,006
Panel B: Parents without College Education				
	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	-0.007	5492.548*	0.233**	0.176**
	(-0.41)	(1.71)	(2.48)	(2.08)
Obs	183,805	154,317	63,816	63,816
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	Yes

Notes: This table presents the second-stage estimates of the regressions of household financial behavior for a sample breakdown based on whether any of the parents of the sampled households have some college education, where we instrument enrollment in a business program with being above the cutoff at the time of admission. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions in column (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

human capital not only strengthens the internal validity of our study, but also increases the potential for generalizability across different demographic groups.

5 From Financial Behavior to Wealth Accumulation

This section examines the impact of quasi-random enrollment in a business or economics program on the (dynamics of) household wealth accumulation.

5.1 Business Education and Household Wealth

Our empirical analysis so far suggests that increased financial literacy through quasi-random enrollment in a business program leads individuals to increase their exposure to the stock market and to earn significantly higher returns on their stock investments. Building on these findings, we next examine whether the effects of business education extend beyond financial portfolio choices to household wealth accumulation.

Table 7: Business Education and Household Wealth

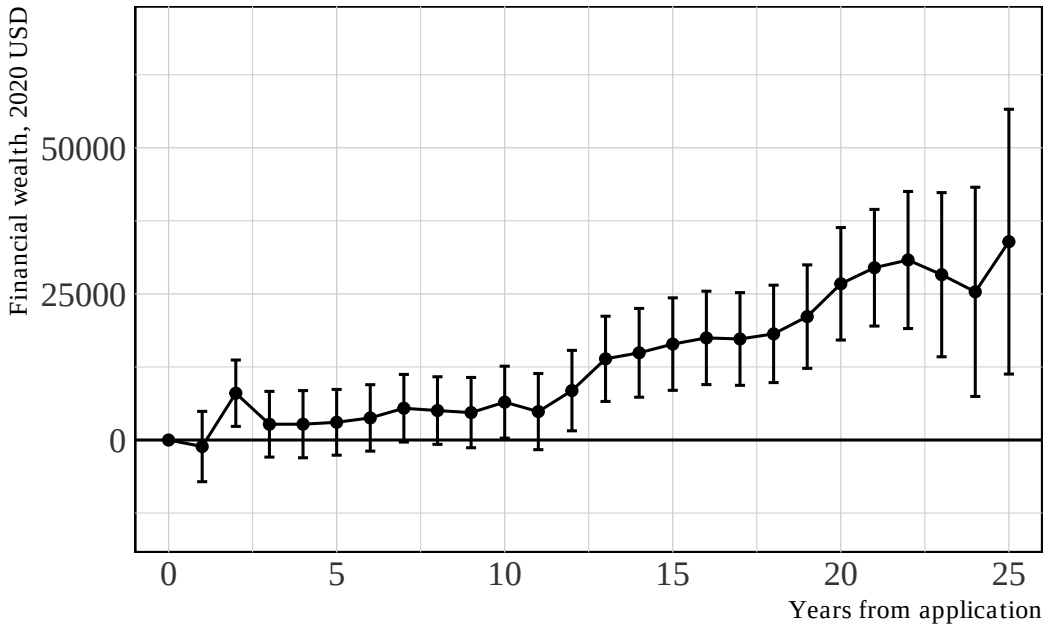
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	11633.380***	28155.212***	0.025**
	(3.06)	(2.87)	(2.06)
Obs	297,633	297,633	297,633
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes
FE: female	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes

Notes: This table presents the second stage estimates of household wealth regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. Household wealth variables are measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table 7 presents the estimation results for the wealth analysis. In column (1), we first use household financial wealth as the outcome variable, which is defined as the sum of the

value of direct and indirect stocks, bonds, bond and mixed mutual funds, and holdings in savings and checking accounts. In column (2), the dependent variable is household net wealth, which is calculated by subtracting household debt from the total sum of financial and real assets. In column (3), we examine the relative position of individuals in the wealth distribution, measured by their percentile rank in the net wealth distribution. Our empirical findings indicate that quasi-random enrollment in a business or economics program leads to significantly higher levels of financial and net wealth, as well as higher rank in the wealth distribution, 4 to 25 years after initial application. The estimated effects are economically significant, with business education contributing to an average increase of about USD 11,700 in financial wealth and USD 28,200 in net wealth. To put it in context, this effect is equivalent to a quite substantial increase of about 18% in financial wealth and 16.5% in net wealth.

Figure 5: Business Education and Household Financial Wealth over Time



Notes: This figure illustrates evolution of wealth effects of business education over time. Specifically, we augment our base regression model, as outlined in Equation 2, by including an interaction term of enrollment in a business program and number of years that has passed since application, and present the estimated coefficients along with their confidence banks over time. The x-axis reports the number of years that has passed since application to a business program while the y-axis presents the coefficient estimates on having business education interacted with each of these years (up to year 25) separately from the financial wealth analysis. Household wealth variables are measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007.

Next, we turn to an analysis of the evolution of wealth effects over time. To do so, we extend our base regression model, as outlined in equation 2, by including an interaction term of enrollment in a business program and number of years since application. Figure

5 illustrates the results. Specifically, the x-axis of Figure 5 denotes the number of years since applying to a business program, while the y-axis reports the coefficient estimates of having business education interacted with each of these years (up to year 25) separately from the financial wealth regression.[35]

The figure shows that increased financial literacy through business education does not have a significant effect on household financial wealth accumulation within the first 11 years after application. The lack of a systematic relationship in the short run is to a large extent to be expected, since wealth is a stock variable and therefore differences in wealth levels are expected to amplify over time. Accordingly, we do indeed find evidence of a significant positive causal wealth effect in the medium term. More importantly, the differences in wealth levels between business and non-business educated households increase monotonically up to 25 years, suggesting that early investments in financial literacy alter life-cycle wealth profiles, and that individuals with similar initial characteristics in terms of preferences and abilities end up accumulating significantly different levels of wealth later in their life.[36] In Figure O.A.3 in the online appendix, we repeat the same exercise using household net wealth as outcome variable, and we document a very similar pattern.

Overall, these results, together with the evidence presented in Section 4.1, provide direct empirical support for the theory model of Lusardi, Michaud, and Mitchell (2017), which formally shows that differences in financial literacy can generate large differences in household wealth accumulation through their effects on investment behavior and portfolio returns.

5.2 Alternative Channels of Influence

In what follows, we discuss and explore alternatives to household financial behavior through which enrollment in a business program may affect household wealth accumulation.

Labor Market Outcomes

One potential mechanism underlying the positive wealth effects of business education is the labor market channel. The existing literature shows that there is considerable heterogeneity in the labor market returns to different college majors, with differences in effect sizes even as large as the overall payoff from having a college degree (e.g., Altonji, Blom, and Meghir 2012; Hastings, Neilson, and Zimmerman 2014; Kirkebøen, Leuven, and Mogstad 2016). For example, Kirkebøen, Leuven, and Mogstad (2016) document that

[35]The figure can also be interpreted as the wealth effects of business education over the life cycle of households between the ages of 21 and 46, since the average age of the sampled household at the time of application is 21.

[36]The effect of business education on household financial wealth after year 20 is less precisely estimated, mainly due to the smaller number of observations for those years.

business education leads to significantly higher early career earnings than social sciences or humanities, while the authors find no significant differences compared to medicine, engineering, or law. In Section 4.4, we also find that individuals with a business education tend to have significantly higher earnings later in life. Thus, our wealth results may simply be an extension of the well-established effects of business education on earnings to household wealth accumulation. We address this explanation below.

Table 8: Household Wealth and Business Education: The Role of Earnings

	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	27330.891***	64317.183**	0.054**
	(2.99)	(2.59)	(1.99)
Obs	78,620	78,620	78,620
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes
FE: female	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes

Notes: This table presents the second stage estimates of household wealth regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. In this analysis, we restrict the sample to those individuals whose next-best alternative college major leads to similar earnings levels as business education, conditional on having positive earnings. Household wealth variables are measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Following Kirkebøen, Leuven, and Mogstad (2016), we first quantify the relative labor market payoffs of business education relative to alternative educational majors among households, conditional on having positive earnings.[37] As shown in Table O.A.13, we find no significant effects of business education on individual earnings compared to medicine, law, health, humanities, and other fields, while business education leads to higher earnings

[37]The alternative fields of study include health, humanities, law, medicine, other, science, social science, teaching, and technology.

compared to science, social science, technology and teaching.[38] We then restrict the sample to those individuals whose next-best college major leads to similar earnings levels as business education to mute the labor income channel, and re-estimate the wealth regressions. In principle, this sample restriction allows us to control for differences in labor income levels between households with business and non-business education later in life. Thus, we are able to isolate the wealth effects of business education that operate through channels other than the labor income channel.

Table 8 reports the coefficient estimates for the wealth regressions for this subsample of households. We document that enrolling in a business program still has positive and significant effects on household wealth accumulation, even when comparing households with similar levels of expected labor income after graduation. This result holds regardless of whether we consider households' net or financial wealth or their percentile rank in the wealth distribution. In other words, our findings are consistent with labor market effects not being the main driver of differences in accumulated wealth levels between business and non-business educated individuals.

Household Debt

Next, we explore the possibility that the documented wealth effects of business education may operate through its impact on the liability side of household balance sheets. For example, Hvidberg (2023) uses an identification strategy similar to ours and examines the effect of business education on the debt behavior of individuals in Denmark. The author documents that individuals with business education are significantly less likely to experience financial distress, primarily due to improved financial behavior rather than their labor market outcomes. To address this explanation, we partition household net wealth into its two broad components, gross assets and total liabilities, and re-run our regressions.

Columns (1) and (2) of Table 9 present the regression results for gross household assets and liabilities, respectively. We find positive and significant effects of enrolling in a business or economics program on household asset accumulation, with individuals having a business education accumulating about USD 35,000 more in total assets (t-stat. = 3.11). In contrast, the effect in the debt regression is not precisely estimated (t-stat. = 1.16), suggesting that household debt behavior does not seem to be an operative mechanism contributing to the wealth results. In unreported tests, we verify this finding using an alternative definition of household indebtedness, namely household leverage measured as the total household debt normalized by annual labor income. Overall, the results presented

[38]We acknowledge that some earnings estimates should be interpreted with caution. For example, the lack of (statistically) significant differences in earnings between business and humanities may be due in part to relatively small sample sizes and lack of variation in these subsamples.

Table 9: Business Education and Household Assets and Debt

	Total Assets	Total Liabilities
	(1)	(2)
Enrolled	34775.829***	5281.876
	(3.11)	(1.16)
Obs	297,633	297,633
FE: time	Yes	Yes
FE: data year	Yes	Yes
FE: cutoff	Yes	Yes
FE: birthyear	Yes	Yes
FE: female	Yes	Yes
FE: priority	Yes	Yes

Notes: This table presents the second stage estimates of household assets and debt regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. Household assets and debt variables are measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

in this section demonstrate that business education contributes to higher levels of wealth primarily through its effects on household assets.

Homeownership

For most households, housing is the primary savings instrument and high returns to housing, especially when purchased with leverage, can contribute significantly to household wealth accumulation (Happel et al. 2024). Against this background, we finally examine the role of individuals' housing tenure decisions for the wealth effects of business education. In Table O.A.14 in the online appendix, we first estimate the causal effects of enrolling in a business program on individuals' homeownership decisions and find no significant effect (t-stat. = -0.82). We then split the sample by the homeownership status of individuals and re-run the wealth regressions. The regression results are reported in Table 10. We find

Table 10: Household Wealth and Business Education: The Role of Housing Investments

Panel A: Homeowners			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	12606.956***	40245.811***	0.028**
	(2.72)	(3.36)	(2.39)
Obs	213,609	213,609	213,609
Panel B: Renters			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	12513.143***	14161.570*	0.046**
	(2.61)	(1.66)	(2.41)
Obs	83,847	83,847	83,847
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes
FE: female	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes

Notes: This table presents the second-stage estimates of household wealth regressions for a sample split based on whether the sampled household is a homeowner or a renter, where we instrument enrollment in a business program with being above the cutoff at the time of enrollment. Household wealth variables are measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

that enrolling in a business degree program leads to significantly higher levels of household financial and net wealth, as well as a higher rank in the wealth distribution, regardless of whether the individual is a homeowner or a renter. Interestingly, the economic magnitude of business education on financial wealth is comparable for renters and homeowners, while the estimated effect on total net wealth is significantly larger for homeowners (USD 40,000 vs. 14,000). Thus, we conclude that the positive contribution of business education to household wealth accumulation is not a mere outcome of differences in housing investment decisions between business and non-business educated households.

6 Conclusions

This paper provides causal evidence that increased financial literacy has a positive and significant effect on portfolio returns and household wealth outcomes. Using exogenous variation generated by university program admission thresholds, we show that early investments in financial literacy, in the form of quasi-random enrollment in a business or economics program, lead individuals to invest more in the stock market, earn significantly higher portfolio returns, and accumulate higher levels of wealth later in life. The estimated effects of having a business education are significant in economic terms. For example, individuals who are quasi-randomly enrolled in a business program earn 15 basis points higher monthly raw returns on their stock portfolio than their non-business educated peers, which translates into an annualized return difference of 1.86 percentage points.

We then analyze the potential mechanisms underlying the positive effect of business education on portfolio returns. First, we show that heterogeneity in risk exposure or innate ability between business and non-business educated individuals does not fully explain the documented return differences. Further analysis suggests that having a business education increases an individual's financial literacy, which enhances the individual's ability to acquire and process economic information and make more informed equity investment decisions, particularly during market downturns and periods of high volatility. These results suggest that, over and above heterogeneity in risk exposure and innate ability, differences in financial literacy are likely to play an important role in explaining return heterogeneity.

To provide further support for the increased financial literacy interpretation of having a business education, we then examine its effects on common investment mistakes. Individuals with business education have better diversified portfolios and are less subject to the disposition effect. We also examine possible alternatives to this interpretation that might produce similar effects, and find that it is indeed financial literacy that leads to better portfolio decisions, rather than potential differences in educational attainment, scale effects, quantitative skills, labor market outcomes including career trajectories, and peer effects across individuals.

Importantly, our results show that the positive contribution of formal financial education to portfolio choice and portfolio returns is operative only for individuals with less educated parents. In contrast, we find no systematic effects in the sample of individuals with at least one college-educated parent. These results suggest that financial education can substitute intergenerational transfers of financial knowledge and thus may play a key role in increasing intergenerational mobility.

The effects of business education extend beyond portfolio choice to the dynamics of household wealth accumulation. We find that individuals with business education accumulate significantly more financial and net wealth later in life. In particular, quasi-random enrollment in a business program leads to an increase in financial (net) wealth of about USD 11,600 (USD 28,155). To put this in context, this effect is equivalent to an increase of about 18% (16.5%) in the average financial (net) wealth of the individuals in the sample, which is quite substantial. We also examine alternative mechanisms, such as the labor market, household debt behavior, or housing investment, that may affect wealth through channels other than financial behavior. We find that the positive wealth effects of business education cannot be fully attributed to any of these alternative mechanisms.

We have shown that early investment in financial literacy plays an important role in generating higher returns and significantly alters life-cycle wealth profiles. In other words, individuals with similar initial characteristics—in terms of skills and preferences—end up accumulating significantly different levels of wealth later in life depending on whether they get the chance to study business or not.

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Appendix for Online Publication

"Business Education and Portfolio Returns"

This Online Appendix includes tables and figures referred to but not included in the main body of the paper, which provide robustness checks and additional findings.

Table O.A.1: First-stage Regressions

	Enrolled	Enrolled	Degree	Degree
	(1)	(2)	(3)	(4)
Above_Cutoff	0.547***	0.558***	0.393***	0.406***
	(68.07)	(70.49)	(44.22)	(45.50)
Obs	33,485	33,485	33,485	33,485
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	No	Yes	No	Yes
FE: female	No	Yes	No	Yes
FE: priority	No	Yes	No	Yes

Notes: This table presents first-stage regression estimates of being enrolled or having a degree in a business program on being above the admission cutoff. Standard errors are clustered at the cutoff level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.2: Business Education and Direct Stock Investments

Panel A: Direct Stock Ownership			
	Direct Stock Investments		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	0.008	0.020	0.028
	(1.10)	(1.10)	(1.10)
Obs	297,633	297,633	297,633
Panel B: Direct Stock Investments			
	Direct Stock Investments		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	1885.854***	4547.459***	6221.463***
	(2.64)	(2.64)	(2.64)
Obs	254,653	254,653	254,653
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes
FE: female	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes

Notes: This table reports the estimates of the regressions of household direct stock investment regressions. Column (1) reports the reduced form regressions as reported in equation 1. Columns (2) and (3) present the second-stage estimates from the IV regressions as described in Equation 2, where we instrument enrollment in a business program and obtaining a business degree with being above the cutoff at the time of admission, respectively. In Panel A, the dependent variable is an indicator variable that takes the value of one if the individual directly owns stocks and zero otherwise. In Panel B, the dependent variable is the amount of direct stocks held in USD. Direct stock wealth is measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.3: Household Financial Behavior and Business Education: Alternative Size Filters

	Portfolio Returns (in %)			
	(1)	(2)	(3)	(4)
Enrolled	0.142**	0.144**	0.151**	0.146**
	(2.22)	(2.24)	(2.33)	(2.12)
Obs	125,265	121,289	118,060	98,548
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: Birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Size filter	$>$ 0USD	$>$ 100USD	$>$ 250USD	$>$ 1000USD

Notes: This table presents second-stage estimates of regressions of household stock portfolio returns using different portfolio size filters, where we instrument enrollment in a business program by being above the cutoff at the time of admission. We control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.4: Business Education and Portfolio Returns: Returns on Risky Assets

Panel A: Full Sample			
	Returns on Risky Assets		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	0.004**	0.011**	0.014**
	(2.28)	(2.27)	(2.28)
Obs	162,735	162,735	162,735
Panel B: With Size Filter			
	Returns on Risky Assets		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	0.004**	0.010**	0.013**
	(2.06)	(2.05)	(2.06)
Obs	157,060	157,060	157,060
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes
FE: female	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes
Portfolio Chars	No	No	No

Notes: This table reports the estimates of the regressions of risky investment return regressions. Column (1) reports the reduced form regressions as reported in equation 1. Columns (2) and (3) present the second-stage estimates from the IV regressions as described in Equation 2. The dependent variable is the annual raw return on the full portfolio of risky assets for which we are able to collect price data. In these regressions, we include the (one-year lagged) value of the risky asset portfolio, the interaction of the time-year dummies and the risky share of financial assets, in addition to other control variables and a full set of fixed effects. Panel A considers all applicants, while Panel B applies a size filter and includes portfolios of at least USD 500. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.5: Portfolio Returns and Business Education: Scale Effects vs. Individual Controls

	Stock Investments	Portfolio Returns (in %)			
	(1)	(2)	(3)	(4)	(5)
Enrolled	1907.993	0.260**	0.234**	0.310***	0.278**
	(0.62)	(2.17)	(1.96)	(2.59)	(2.37)
Obs	111,917	42,737	48,534	43,329	49,295
FE: time	Yes	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	No	No
FE: female	Yes	Yes	Yes	No	No
FE: priority	Yes	Yes	Yes	No	No
Size Filter	No	Yes	No	Yes	No

Notes: This table presents second-stage estimates of regressions of household stock investments and portfolio returns over the short-term (i.e., 5–14 years after the initial application), where we instrument enrollment in a business program by being above the cutoff at the time of admission. In columns (2) and (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta, while columns (4) and (5) excludes all individual level controls. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.6: Business Education and Financial Behavior: Controlling for Next Best Major Fixed Effects

	Participation	Stock Investments	Portfolio Returns (in %)	Portfolio Returns (in %)
	(1)	(2)	(3)	(4)
Enrolled	0.004	6848.572**	0.142**	0.117*
	(0.28)	(2.56)	(2.15)	(1.94)
Obs	293,444	251,218	110,648	110,648
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: next best field	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	Yes

Notes: This table presents the second-stage estimates of household financial behavior from the IV regressions as outlined in Equation 2, where we instrument enrollment in a business program with being above the cutoff at the time of admission. In these regressions, we control for next best major fixed effects. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions in column (3), we control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.7: Business Education and Portfolio Returns: The Role of Ability

	Portfolio Returns (in %)	
	(1)	(2)
Enrolled	0.181**	0.149**
	(2.42)	(2.17)
Obs	89,943	89,943
FE: time	Yes	Yes
FE: data year	Yes	Yes
FE: cutoff	Yes	Yes
FE: birthyear	Yes	Yes
FE: female	Yes	Yes
FE: priority	Yes	Yes

Notes: This table presents second-stage estimates of regressions of household stock portfolio returns controlling for peer effects, where we instrument enrollment in a business program by being above the cutoff at the time of admission. In these regressions, we therefore exclude applicants who are in the top 10 percent and bottom 10 percent of the high school GPA distribution.. We control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (2), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.8: Business Education and Portfolio Returns: Learning and Information Processing

Panel A: Stock Portfolios by IVOL				
	Portfolio Returns (in %)			
	High IVOL		Low IVOL	
	(1)	(2)	(3)	(4)
Enrolled	0.317**	0.299**	0.099	0.043
	(2.30)	(2.19)	(1.57)	(0.74)
Obs	37,315	37,315	34,220	34,220
Panel B: Stock Portfolios by Illiquidity				
	Portfolio Returns (in %)			
	High Amihud		Low Amihud	
	(1)	(2)	(3)	(4)
Enrolled	0.309**	0.291*	0.079	-0.017
	(1.98)	(1.91)	(1.34)	(-0.66)
Obs	37,263	37,263	41,775	41,775
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	Yes	No	Yes

Notes: This table presents the second stage estimates of household financial behavior regressions. In Panels A and B, we split the sample by the idiosyncratic volatility (IVOL) and liquidity of the direct stock portfolio of applicants. In column (3), we control for the (one-year lagged) value of the stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.9: Business Education and Investment Mistakes

	Disposition Effect		Diversification	
	(1)	(2)	(3)	(4)
Enrolled	-0.024*	-0.024*	0.335**	0.298**
	(-1.79)	(-1.72)	(2.05)	(2.02)
Obs	45,622	43,904	147,686	123,208
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: Birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio size	No	Yes	No	Yes

Notes: This table presents the second stage estimates of household investment mistakes regressions where where we instrument enrollment in a business program with being above the cutoff at the time of admission. We use the total number of stocks in the portfolio as a crude measure of portfolio diversification as in Goetzmann and Kumar (2008). We measure the disposition effect, i.e., the tendency of individuals to hold losing stocks too long and sell winning stocks too early, as the difference between the proportion of realized stock gains and losses in a given year as in Calvet, Campbell, and Sodini (2009b). In these regressions, we condition on holding direct stocks in the current and previous periods (Calvet, Campbell, and Sodini 2009b). The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.10: Business Education and College Graduation

	Degree from any Program
	(1)
Above_Cutoff	0.032***
	(4.23)
Obs	300,003
FE: time	Yes
FE: data year	Yes
FE: cutoff	Yes
FE: Birthyear	Yes
FE: female	Yes
FE: priority	Yes

Notes: This table presents the results of an analysis in which we regress having any college degree on being above the admission cutoff. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.11: Business Education and Financial Behavior: Controlling for Labor Market Outcomes

	Portfolio Returns (in %)				
	(1)	(2)	(3)	(4)	(5)
Enrolled	0.1552**	0.1587**	0.1531**	0.1538**	0.1570**
	(2.35)	(2.31)	(2.33)	(2.31)	(2.28)
Earnings		-0.0001***			-0.0002***
		(-2.58)			(-3.35)
Unemployed			-0.0718***		-0.0833***
			(-3.84)		(-4.28)
Works in Finance				0.0205	0.0174
				(0.87)	(0.73)
Obs	111,906	108,851	111,906	111,906	108,851
FE: time	Yes	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes	Yes
FE: Birthyear	Yes	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes	Yes
Portfolio Chars	No	No	No	No	No

Notes: This table presents the second stage estimates of portfolio returns regressions where where we instrument enrollment in a business program with being above the cutoff at the time of admission. Panel A reports a results from a naïve mediation analysis where we control for same-year individual earnings. In Panel B, we extend our baseline model by including the labor market payoffs of business education relative to the applicant's next-best field of study in the application, and an interaction of this variable with the enrollment indicator. In column (1), we control for the (one-year lagged) value of the stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In column (2), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.12: Household Financial Behavior and Portfolio Returns: Is it Peer Effects? Alternative Portfolio Overlap Measure

	Portfolio Returns (in %)			
	(1)	(2)	(3)	(4)
Enrolled	0.154**	0.126**	0.154**	0.126**
	(2.38)	(2.09)	(2.40)	(2.10)
Obs	111,906	111,906	111,906	111,906
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes
Portfolio Chars	No	Yes	No	Yes

Notes: This table presents second-stage estimates of regressions of household stock portfolio returns controlling for peer effects, where we instrument enrollment in a business program by being above the cutoff at the time of admission. In these regressions, we control for the portfolio-level overlap measure that is constructed based on the stock investments of all individuals who had some business education at the college level. In (1) and (2), we use the the measure Bus_Edu_Index, and we use the continuous form of this variable in (3) and (4). We control for the (one-year lagged) value of the log stock portfolio, the interaction of the time-year dummies and the stock share in financial wealth, and the portfolio beta. In columns (2) and (4), we also control for the average size, momentum, and value loadings of the stock portfolio. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.13: Labor Market Payoffs of Business Education relative to Different Fields of Study

	Science	Medicine & Health	Humanities	Law	Other	Social Science	Teaching	Technology	Non-significant Fields
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Enrolled	94318.603*	-109320.391	27117.305	31725.468	31261.433	52729.611**	72925.393***	23409.738***	20271.311
	(1.73)	(-1.08)	(0.35)	(1.60)	(0.61)	(2.01)	(3.55)	(2.74)	(1.27)
Obs	12,742	12,202	11,066	43,945	9,204	36,170	30,836	117,155	76,431
FE: time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

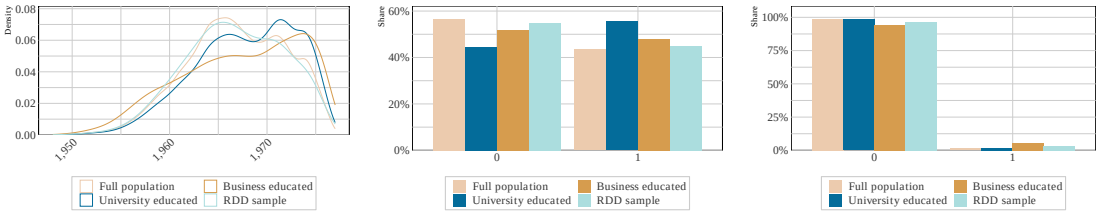
Notes: This table presents the second stage estimates of individual earnings regressions where where we instrument enrollment in a business program with being above the cutoff at the time of admission. Following Kirkebøen, Leuven, and Mogstad (2016), we quantify the relative labor market payoffs of business education relative to alternative educational majors among households with some college education. In each column, we estimate the impact of business education on earnings relative to an alternative field of study. The alternative fields of study include science, health & medicine, humanities, other, social sciences, teaching, and technology. In the last column, we pool all fields of study that produce statistically insignificant labor market payoffs relative to business. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Table O.A.14: Business Education and Homeownership

	Homeownership		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	-0.005	-0.012	-0.017
	(-0.82)	(-0.82)	(-0.82)
Obs	297,633	297,633	297,633
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: birthyear	Yes	Yes	Yes
FE: female	Yes	Yes	Yes
FE: priority	Yes	Yes	Yes

Notes: This table presents the estimates of homeownership regressions. Column (1) reports the reduced form regressions as reported in equation 1. Columns (2) and (3) present the second-stage estimates from the IV regressions as described in Equation 2, where we instrument enrollment in a business program and obtaining a business degree with being above the cutoff at the time of admission, respectively. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff) and fixed effects for each admission cutoff, applicant's year of birth, the priority ranking of the business alternative in the application, year of measurement, the number of years since the application, and indicator variables for whether the applicant is foreign-born and applicant's gender. Standard errors are two-way clustered at the cutoff and individual levels, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

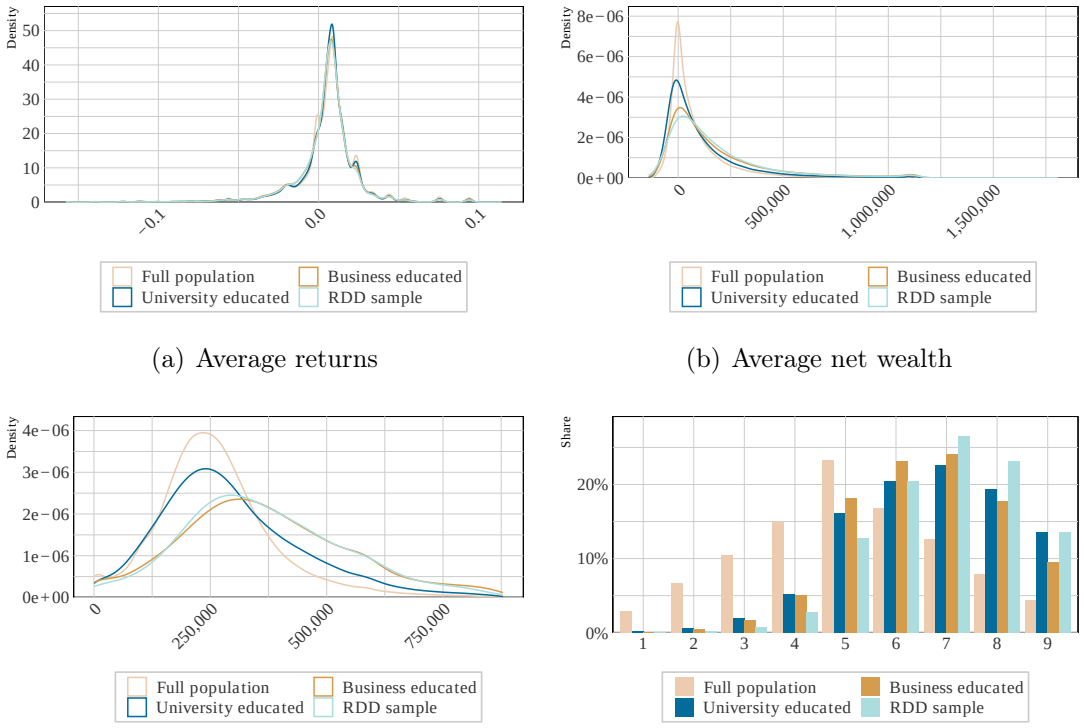
Figure O.A.1: Summary statistics: matched samples



(a) Birthyear (b) Women (c) Foreign born

Notes: This figure reports densities and histograms of the matched samples reported in Table 1. Each category has been samples from the full population to match the joint distribution of birth year, gender, and immigrant status in the study sample. In addition to the sample of university degree holders and business degree holders reported in Table 1, the figure also includes a matched sample from the full population of Swedes.

Figure O.A.2: Summary statistics: key metrics

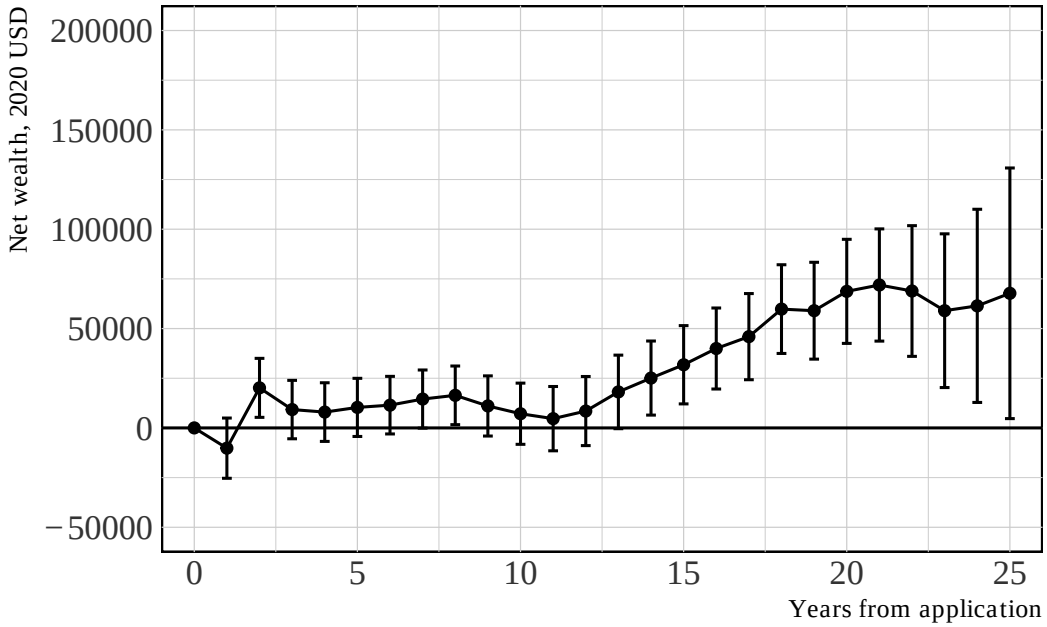


(a) Average returns (b) Average net wealth

(c) Average earnings, age 31-35 (d) Cognitive skills

Notes: This figure reports densities and histograms of key statistics reported in Table 1.

Figure O.A.3: Business Education and Household Net Wealth over Time



Notes: This figure illustrates evolution of wealth effects of business education over time. Specifically, we augment our base regression model, as outlined in Equation 2, by including an interaction term of enrollment in a business program and number of years that has passed since application, and present the estimated coefficients along with their confidence banks over time. The x-axis reports the number of years that has passed since application to a business program while the y-axis presents the coefficient estimates on having business education interacted with each of these years (up to year 25) separately from the financial wealth analysis. Household wealth variables are measured at the household level. The sample is restricted to individuals who apply to degree programs in business before 1995 and have a non-business counterfactual alternative. Portfolio and wealth outcomes are observed each year between 1999 and 2007.