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. Underlying these effects, beyond differences in risk taking, innate ability, labor market outcomes, or scale effects, is the improved 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.

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1 Introduction

Heterogeneity in returns to wealth is a key driver of rising wealth inequality, particularly 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) theoretically demonstrate that differences in financial sophistication can lead to large differences in household wealth, primarily by affecting the returns to saving, thereby accounting for a significant share of wealth inequality in the U.S. Despite this intuitive link, well-identified empirical evidence on the causal effects of increased financial sophistication on household portfolio returns and wealth accumulation remains scarce.[3]

This paper provides empirical evidence that financial education, by enhancing individuals' financial sophistication, causally increases returns on risky assets and positively influences the dynamics of household wealth accumulation over the short to medium term. Exploiting exogeneous variation in college majors from admission thresholds, we show that enrollment in a business-related program leads individuals to hold more stocks and earn higher returns on average on their stock investments. Thus, individuals with similar initial preferences and abilities accumulate different levels of wealth later in life, suggesting that early investments in financial sophistication fundamentally alter life-cycle wealth profiles.

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 leverages 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 system assigns applicants to university programs based on their academic performance and stated preferences. This system generates sharp admission cutoffs for oversubscribed programs. Moreover, the ranked list of university-program preferences submitted by each applicant allows us to observe their counterfactual alternative, that is, the program they would have been assigned to if not admitted to their preferred choice.

[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). Beyond differences in risk exposure, ability, and financial sophistication, return heterogeneity can also stem from factors 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 literate investors earn 40 basis points higher annual returns on their investments compared to less literate investors, even after controlling for various measures of risk.

Using the universe of applications submitted through the centralized system between 1977 and 1995, we identify applicants who apply to oversubscribed business programs, such as economics, finance, business administration, industrial economics, and related fields, and have a non-business major as their next best alternative. Using a fuzzy regression discontinuity design that instruments business program enrollment with threshold crossing and a rich individual-level panel dataset, we compare individuals just above and just below the admission threshold to estimate the causal effects of financial education on financial behavior. Our approach exploits a large set of such cutoffs for different business programs across institutions and years, pooling approximately 3,500 "natural experiments" of admission to business or economics programs, with fixed effects for each experiment.

Our results show that business education leads to higher portfolio returns and better financial and wealth outcomes over a period of 4 to 25 years after initial application. In particular, individuals marginally admitted to a business or economics program hold on average about USD 6,700 more in stocks (i.e., an increase of 20% over the mean) and earn 15 basis points more on average in raw monthly portfolio returns than their peers who were marginally not admitted and did not enroll. These effects are both statistically and economically significant. For example, the documented return difference translates into an annualized return difference of 1.86 percentage points, which, under modest assumptions, could lead to approximately 60% higher direct stock wealth or 20% greater overall financial wealth over 25 years. These results are robust to controlling for predetermined individual characteristics, as well as fixed effects for birth cohort-by-observation year, admission cutoff, and the priority ranking of the business alternative in the application.

Having established a strong causal effect of business education on portfolio returns, we next examine the potential channels underlying these findings. One strong explanation for the return gap is differences in risk-taking behavior between business and non-business educated individuals (Bach, Calvet, and Sodini 2020; Campbell, Ramadorai, and Ranish 2019). To address this, our baseline model incorporates portfolio beta and accounts for differences in access to the menu of financial instruments across individuals, following the approach in Fagereng et al. (2020). To further examine the role of risk-taking and to better capture exposure to different sources of compensated risk, we extend our return regressions to include portfolio loadings on the size, value, and momentum factors. We note that the economic magnitude of business education declines by about 14% when these additional risk factors are included, suggesting that risk-taking partially, but not fully, explains the observed effects. Second, heterogeneity in innate ability across individuals could also explain the return differences, if, for example, individuals with superior wealth management skills self-select into business programs (Barth, Papageorge, and Thom 2020; Fagereng et al. 2020). However, by design, our analysis compares the investment performance of individuals with similar initial skills (proxied by high school GPA) and preferences (inferred from their ranked lists of university-programs), thereby implicitly controlling for such heterogeneity in our empirical analysis. Moreover, when we exclude the most and the least able applicants and focus on those competing at admission cutoffs in the middle of the ability distribution, we obtain similar results. Thus, we can rule out the explanation that our findings are merely driven by positive selection into business programs based on ability.

An essential question is what explains the positive effect of business education on portfolio returns, if not risk-taking or innate ability? We argue and provide evidence that individuals who are enrolled in business or economics programs develop higher levels of financial sophistication, which enhances their ability to process economic information and ultimately leads to higher portfolio returns. Even though previous literature typically elicits financial literacy by the "Big Three" survey questions (Lusardi and Mitchell 2007), we measure it directly through actual portfolio decisions. Accordingly, we first show that business education improves portfolio diversification and reduces behavioral biases such as the disposition effect, strongly suggesting that individuals with business education end up being more financially sophisticated and, in turn, make more informed portfolio decisions. Second, we find that business educated individuals earn significantly higher returns relative to their non-business educated peers during market downturns and periods of high volatility, precisely when the value of acquiring and processing information is greatest (Grossman and Stiglitz 1980). In contrast, there is no systematic difference in returns during favorable market conditions, when the return to improved information processing ability is lower. Similarly, business education significantly increases portfolio returns only when the underlying stocks are relatively more volatile and illiquid, as measured by the idiosyncratic volatility and the Amihud ratio of the stock portfolio, respectively. Taken together, these results highlight the key role of business educated individuals' enhanced ability to acquire and process economic information in generating higher portfolio returns, beyond differences in risk exposure and innate ability. In this regard, our analysis provides a credible micro-foundation for the mechanisms by which financial literacy contributes to higher returns, as shown in the theoretical models of Lusardi, Michaud, and Mitchell (2017) and Jappelli and Padula (2017).

We subject our findings to a series of robustness checks and explore alternative explanations that could potentially account for the observed results. These include mechanisms related to educational attainment, labor market outcomes, scale effects, quantitative skills, peer effects, college quality, and elite school effects. First, we address the concern that the documented effects may be due to the level of education rather than its content. Our analysis shows that business education continues to have positive and significant effects on portfolio returns even when the sample is restricted to individuals with a college degree. Second, we examine whether business education affects households' financial decisions and outcomes through its effects on labor market outcomes and career paths, such as working in the financial 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 individuals' unemployment risk, which might 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 run through labor market outcomes, particularly career paths. Third, we examine scale dependence, that is, the possibility that individuals with business education earn higher returns due to better access to high-quality information or investment opportunities associated with greater wealth (Gabaix et al. 2016). Tests that explicitly account for scale effects, as well as analyses that focus on periods when wealth differences between business and non-business educated individuals are minimal, confirm that our results are not driven by scale dependence. Fourth, we confirm that the observed positive effects are driven by the financial knowledge gained through business education, rather than quantitative skills, by analyzing a sample of applicants who had a business program as their preferred field and listed a quantitative field, such as science or technology, as their next-best alternative. Fifth, we examine the potential role of peer effects and find no evidence to support this explanation. Finally, we show that enrollment in a business or economics program continues to improve investment performance even after controlling for heterogeneity in university quality or excluding the applicants to elite schools from the sample. Thus, we conclude that our findings are not simply artifacts of differences in college quality or elite school effects.

Our findings suggest that business education significantly increases financial sophistication, which leads to better investment performance. However, the external validity of these results warrants careful consideration. Our estimates are based on high school graduates who intend to pursue higher education, particularly in business or economics programs, and reflect local average treatment effects for individuals close to the admissions cutoffs. Nevertheless, comparisons of the background characteristics of our sample, such as high school GPA and cognitive ability, with the broader college-educated population suggest only small differences. Robustness checks excluding high-ability individuals further confirm that the positive effects of business education on financial behavior persist, supporting the broader applicability of our findings. We also find that the positive impact of business education on portfolio performance is significant only for individuals from less advantaged parental backgrounds. This suggests that business education can substitute for intergenerational persistence in financial sophistication, potentially playing a role in increasing intergenerational mobility. It also highlights the effectiveness of financial education for individuals with limited access to alternative sources of financial knowledge, underscoring the potential generalizability of our findings to 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 over time. In particular, enrolling in a business degree program increases financial wealth by an average of USD 11,600 and net wealth by USD 28,155, or approximately 18% (16.5%) relative to the average financial (net) wealth of the sampled individuals. The analysis of the dynamics of wealth accumulation shows that these effects manifest gradually in the medium term, followed by a monotonic increase in the wealth gap between business and non-business majors. We conclude that business education alters the life-cycle wealth profiles, and individuals with similar initial characteristics ultimately accumulating significantly different levels of wealth. We also examine alternative mechanisms, such as the labor market, household debt behavior, and 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 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, demonstrating that endogenous investments in financial knowledge lead to higher expected returns on savings and large differences in household financial wealth. Similarly, Jappelli and Padula (2017) document a positive link between financial sophistication, portfolio returns, and household consumption growth using a life-cycle model that incorporates endogenous financial knowledge. Both studies argue that improved financial knowledge allows individuals to use sophisticated, information-intensive financial products, such as stocks, thereby earning higher returns on their investments. Our causal evidence supports the model predictions of those papers in that financial sophistication acquired through business education leads to higher portfolio returns and alters the life cycle wealth profiles of individuals. Hence, our findings are relevant for the ongoing discussion on policy tools to regulate wealth inequality (e.g., Calvet et al. 2023; Guvenen et al. 2023; Stiglitz 2015), suggesting that financial education can partly contribute to contain wealth inequality.

Second, we contribute to the current debate on the effectiveness of financial literacy education to empower households to make better financial decisions (e.g., Campbell 2016; Kaiser et al. 2021). A central question in this discussion is whether financial education serves an effective policy tool for improving household economic choices (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.[4] We add to this

[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

discussion by providing causal evidence of the positive effects of financial education on portfolio returns and wealth accumulation, while also identifying the mechanisms behind these effects.

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 majors on individuals' labor market outcomes (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, 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 individual earnings and find that the return is approximately 80–130% over a period of more than 12 years. We document that the causal effect of business education extends beyond the labor market to financial behavior and wealth accumulation of households. An early paper focusing on the financial behavior of individuals with an 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. In another related paper, Hvidberg (2023) uses university admission discontinuities in Denmark to document that business education reduces the probability of experiencing financial distress. Extending these results, we show that business education, by enhancing individuals' financial sophistication, also improves the asset side of the household balance sheet by increasing returns to wealth, highlighting its broader implications.

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.

education using meta-analysis techniques.

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.

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,[5] 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.[6] During each round, applicants are

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

[6] 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

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 programs in their applications in case they may need a safe fallback option.[7] 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.[8] 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).[9] 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 income and wealth taxation. The wealth tax was abolished in 2007, but the registry contains

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.

[7]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.

[8]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.

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

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.[10]

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.

To calculate stock portfolio characteristics, we use auxiliary information on daily and monthly stock 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.[11] Using this information, we calculate for each individual in each year the stock portfolio returns,[12] and other portfolio characteristics such as the portfolio beta and the size, momentum, and value loadings over the period 2000-2007.[13]

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 fees and expense structures 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'

[10]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.

[11]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.

[12]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.

[13]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.

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 financial 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 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 he or she 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.[14]

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.

[14]We exclude applicants who were admitted in non-standard admission groups or to institutions that offer practice-based programs from the sample. 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.

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 accumulation, we restrict our sample to 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 that includes programs such as business administration, economics, finance, commerce, management, organisation, and industrial economics.[15] 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.[16]

Turning to sample characteristics, we find that about 74% of our sampled individuals graduate from college within eight years of their initial application. Not surprisingly, the proportion of individuals who earn a business degree is significantly higher in our sample than in the broader population of college graduates, 40% versus 17%. Later in life, 83% of the sampled individuals participate in the stock market (directly or indirectly) and about 70% become homeowners. Individuals in our sample also have somewhat better labor market and wealth outcomes, accumulating higher levels of financial and net wealth later in life. However, when comparing them to the larger population of individuals with university business education, most of the differences in means and variances diminish. As shown in Figure O.A.2 in the online appendix, these similarities extend to the distributions of average returns, net wealth, and earnings. We return to these issues in section 4.4 when discussing the external validity of our findings.

3 Empirical Strategy

To formally examine the effects of having a business or economics education on household financial behavior and wealth outcomes, we employ a regression discontinuity design (RDD)

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

[16] 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.

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 with those applicants 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 as 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:

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

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 holdings, 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 birth cohort-by-observation year fixed effects (i.e., τ_{bT}) to control for any systematic differences across birth cohorts in each calendar year, along with additional time fixed effects 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 wealth 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:

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

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

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.[17] 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.[18]

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}).[19] 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.

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.

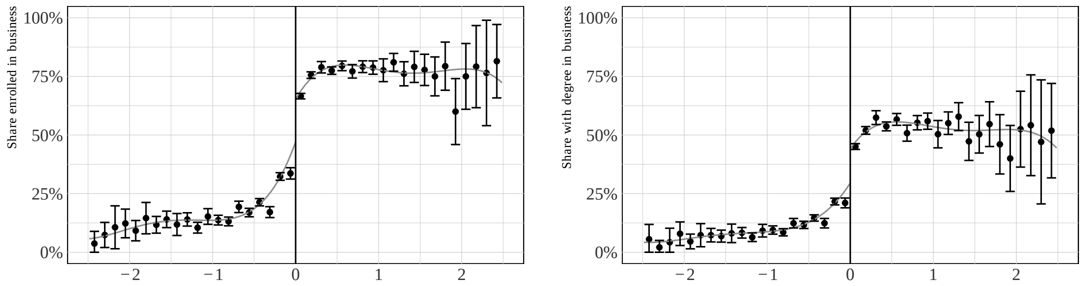
Figure 1 illustrates the first stage for both enrollment and business degree completion. We note a clear jump in the probability of enrolling in a business program within five

[17]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.

[18]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.

[19]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.

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



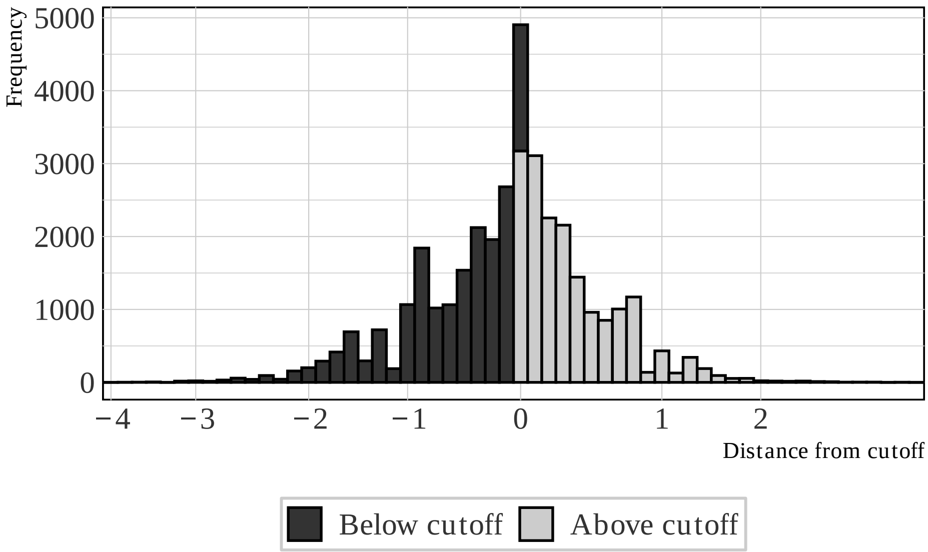
Notes: The left panel illustrates enrollment in a business program, and the right panel depicts the business degree completion around the admission cutoff among applicants who apply to a business program with a non-business counterfactual.

years and in earning a business degree within 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 indicates a strong first stage.

For RDD to properly identify a causal treatment effect, it should not be possible to precisely manipulate assignment around the admission cutoff. Since the cutoffs change each year depending on the scores of all applicants, an individual close to the cutoff has no way of knowing ex ante whether he or she 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.

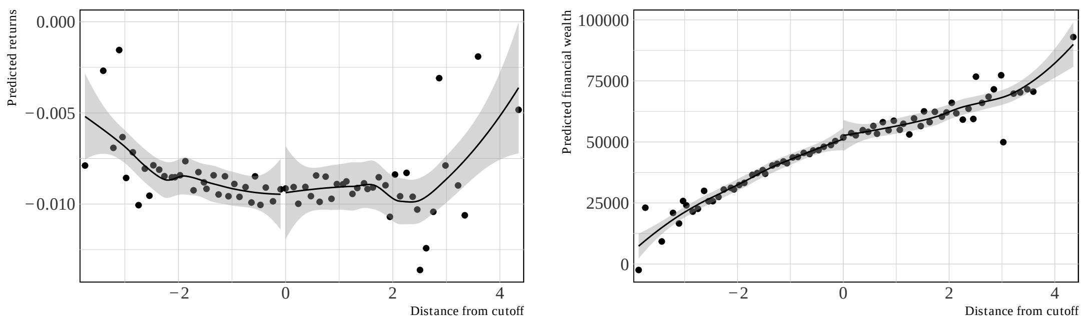
Finally, a key parameter in any regression discontinuity design is the bandwidth. Normally, optimal bandwidth algorithms can be used to find the best balance between bias and variance. However, because our analysis pools a large set of cutoffs, no chosen bandwidth will be optimal for all cutoffs. Instead, 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 2: Distribution of Admission Scores around the Admission Threshold



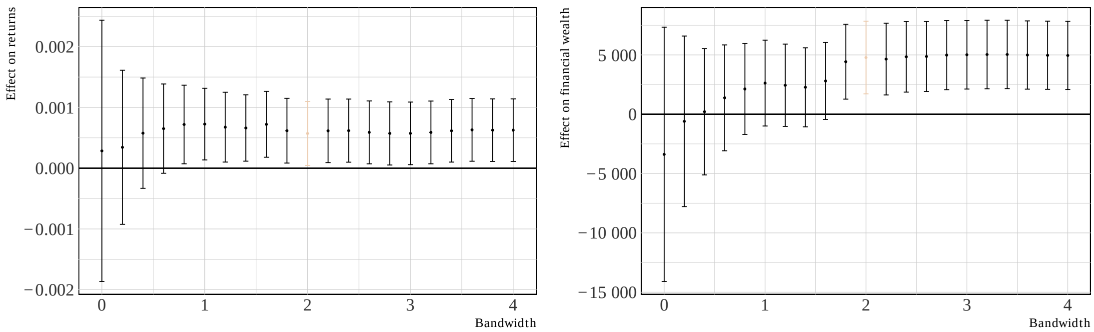
Notes: This figure illustrates a histogram of the distribution of observations around the admission cutoff. Observations exactly at the cutoff are sorted in a separate bar. These individuals are admitted using different tie-breaking mechanisms, and are counted in the analysis as either above or below 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.

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 1 presents the estimation results. For brevity, we report only the coefficient estimates on the variable for enrollment in a business program. In all regressions, we include linear polynomials of the running variables, a set of predetermined individual-level covariates, and a battery of fixed effects, including fixed effects for each admission cutoff, the number of years since application, the priority ranking of the business alternative in the application, and for the birth cohort-observation year.

As a prelude to our instrumental variable estimates, Panel A of Table 1 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 1, where the sample includes both stock market participants and non-participants, 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.[20] 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 only the direct stock ownership as the outcome variable, as presented in Panel A of Table O.A.2 in the online appendix. The lack of a significant effect on households' stock market participation decisions in our context is, in fact, not surprising. Stock ownership is widespread in Sweden - one of the highest levels in the world, especially among households with some college education. In addition, our sample includes only individuals with a stated preference for a business or economics major, suggesting that they are likely to have an above-average interest in financial matters, including stock market investing.

Next, we focus on the intensive margin of financial risk-taking, using the value of direct and indirect stock holdings as the outcome variable. As shown in column (2) of Table 1, among stock market participants, individuals with a business education have significantly greater exposure to the stock market. Specifically, business education leads to an increase in individuals' stock holdings of about USD 6,725 (t-stat. = 2.53), which represents an 18% increase in mean stock wealth. Further analysis, presented in Panel B of Table O.A.2 in the online appendix, focuses on direct stock holdings as the outcome variable and produces similar results. We find that enrolling in a business program increases direct stock holdings by about USD 4,540, which accounts for over two-thirds (=4,540/6,725) of the total increase in stock portfolio value. 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 and natural question is whether individuals with business or economics education not only invest more in the stock market but also 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 question by observing realized stock returns and accurately measuring differences in risk exposure, which allows us to identify the causal effects of financial education on portfolio returns. These, in turn, can inform the broader debate on the sources of return heterogeneity and the role of asset returns in wealth inequality more generally (Bach, Calvet, and Sodini 2020; Campbell, Ramadorai, and Ranish 2019; Fagereng et al. 2020; Lusardi, Michaud, and Mitchell 2017).

To examine the impact of business education on portfolio returns, we begin with a simple portfolio analysis. For each year from 2000 to 2007, we group individuals' stock

[20]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 1: Household Financial Behavior and Business Education

Panel A: Reduced Form			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Above_Cutoff	0.000	2789.563**	0.065**
	(0.07)	(2.53)	(2.36)
Obs	297633	254653	111903
Panel B: IV Estimates: Enrollment as Treatment			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.001	6726.712**	0.154**
	(0.07)	(2.53)	(2.33)
Obs	297633	254653	111903
Panel C: IV Estimates: Degree as Treatment			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Degree	0.001	9202.495**	0.197**
	(0.07)	(2.54)	(2.33)
Obs	297633	254653	111903
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	No	No	Yes
Portfolio beta	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 interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

portfolios into deciles based on either their high school GPA (used as a proxy for ability) or their account size. Within each decile, we further categorize individuals based on whether they were marginally above (i.e., *Treated*) or below (i.e., *Control*) the cutoff for admission to a business or economics program. We then compute the value-weighted returns for each portfolio and compare the average performance between the treated and control groups. The results show that, on average, individuals just above the admission cutoff earn higher monthly portfolio returns than those just below the cutoff, regardless of how the portfolios are sorted. Specifically, when sorted by ability, the return difference between the *Treated* and the *Control* is 0.20% per month (t-stat = 2.69), and when sorted by account size, the difference is 0.08% per month (t-stat = 3.54). This suggests that business education contributes positively to investment performance.

Building on these findings, we proceed to a more formal analysis to control for potential confounding factors and to establish a causal relationship between business education and portfolio returns. Following Fagereng et al. (2020), our return analysis includes fixed effects for deciles of stock portfolio value and the interaction between time-year dummies and equity share of financial assets, all measured prior to portfolio returns, along with additional control variables and a comprehensive set of fixed effects, as shown in Equation 2. The former allows us to account for the effects of participation costs (Vissing-Jorgensen 2003) and scale effects (Gabaix et al. 2016), which are particularly important for generating higher returns, for example, through easier access to high-quality information or better investment opportunities (Bach, Calvet, and Sodini 2020; Kacperczyk, Nosal, and Stevens 2019). Meanwhile, the latter controls for differences in access to the menu of financial instruments (Chien, Cole, and Lustig 2011; Fagereng et al. 2020).[21] In addition, our baseline model includes portfolio beta to capture the variation in risk exposure across individuals. To address concerns that small portfolios may drive the results, we apply a size filter, including only stock portfolios with a minimum value of USD 500 (corresponds to the 10th percentile of the portfolio size distribution).

The regression results are presented in column (3) of Table 1. We find positive and significant effects of business education on stock portfolio returns, with t-statistics ranging from 2.33 to 2.36 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, on average. This effect corresponds to an annualized return differential of 1.85 percentage points between business and non-business educated individuals, highlighting the economic importance of financial education in generating higher portfolio returns.

[21]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.

Next, we examine the effect of quasi-random enrollment in a business or economics program on household financial behavior in the short run (4-14 years) and in the medium run (14-25 years), with results reported in Table O.A.3. Consistent with the baseline findings, business education has no significant effect on stock market participation in either period. However, individuals with a business education earn 26 basis points (t-stat. = 2.22) higher monthly stock portfolio returns in the short run, an effect that becomes statistically and economically insignificant in the medium run. Notably, there are no significant differences in household stock holdings or in wealth levels in the short run (t-stat. = 0.63), but in the medium run, business educated households hold about USD 10,090 more in stock wealth than their non-business educated counterparts (t-stat. = 2.91). These results highlight the dynamic effects of business education on wealth accumulation, which we discuss in detail in Section 5.

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.4 in the online appendix, show that our results are robust to relaxing or using alternative size filters. Having focused on the returns of direct equity portfolios, we now broaden our analysis by using an alternative measure of portfolio returns. Specifically, we calculate the gross returns of the entire portfolio of risky financial assets for which price data are available. As shown in Table O.A.5, we again find a positive effect of enrolling in a business program on the returns to the full risky financial portfolio. This result suggests that the observed benefits of business education are not limited to direct stock investments but extend to a broader range of risky assets. Third, while it is important to control for risk exposure and scale effects in the return regressions, these variables are defined post-treatment and may introduce confounding effects if influenced by unobservable characteristics. To address this trade-off, we remove all individual-level controls and restrict the sample to the first 14 years after initial enrollment, a period in which there are no significant differences in wealth. Reassuringly, business education continues to have a statistically significant and economically meaningful positive impact on portfolio returns, with point estimates that are close to the baseline results. This analysis confirms the robustness of our findings and provides further evidence that the results cannot be fully attributed to scale effects (Gabaix et al. 2016). In this context, scale effects would imply that differences in wealth and investment size between business and non-business educated individuals lead to differential access to better investment opportunities or higher quality information, which could explain the observed return differences. Finally, Kirkebøen, Leuven, and Mogstad (2016) emphasize the importance of controlling for the next best alternative in settings like ours to ensure proper identification and causal interpretation of the estimates. In Panel A of Table O.A.7 in the online appendix, we present results controlling for next best major fixed effects. Because our sample includes applicants who list a business or economics program as their preferred major and a non-business program (e.g., engineering, science, or humanities) as their counterfactual choice, these fixed effects are equivalent to two-way interacted fixed effects for preferred and next-best alternatives (Altmejd et al. 2021). As shown in the table, the economic magnitude of the effects decreases slightly, but the coefficient on enrollment largely retains its economic and statistical significance.

4.2 Understanding the Mechanism

What is the primary mechanism through which business education affects portfolio returns? To explore this question, we investigate several potential channels. First, the documented return differences between business and non-business educated individuals may arise because business education increases individuals' willingness to take financial risk (Bach, Calvet, and Sodini 2020; Campbell, Ramadorai, and Ranish 2019). Our baseline model addresses this explanation by incorporating portfolio beta and accounting for differences in access to the menu of financial instruments across individuals. To further refine our understanding of the role of risk-taking in our results and to better capture differences in exposure to different sources of compensated risk, we extend our return regressions to include portfolio loadings on the size, value, and momentum factors. The results presented in Panel A of Table 2 show that the economic magnitude of having a business education declines by about 14% (from 15.4 to 13.2 basis points) once we account for these additional risk factors, suggesting that risk-taking only partially explains the documented effects.

Second, heterogeneity in innate ability across households could also contribute to the documented return differences if individuals with superior (wealth management) skills sort themselves into business or economics programs (Barth, Papageorge, and Thom 2020; Fagereng et al. 2020). By design, we contrast the stock portfolio performance of individuals with similar initial abilities and preferences, thereby implicitly controlling for such heterogeneity in our empirical analysis. Thus, our results are not biased by positive selection into business programs based on ability. However, the heterogeneity in admission requirements across business programs raises the possibility that our identified treatment effect is biased by outliers. To address this concern, we conduct additional tests excluding applicants in the top and bottom 10 percent of the high school GPA distribution. As shown in Panel B of Table 2, the effect of business education on portfolio returns increases, if anything, after excluding the most and least able applicants from the sample.

If the return differences cannot be fully attributed to heterogeneity in risk exposure or innate ability, what explains then the positive contribution of business education to portfolio returns? One compelling explanation is that individuals who are quasi-randomly enrolled in business or economics programs are more likely to develop higher levels of financial sophistication. That, in turn, improves their ability to process economic information and make more informed investment decisions, ultimately leading to higher portfolio returns

Table 2: Portfolio Returns and Business Education: Role of Risk Taking and Ability

Panel A: Controlling for Additional Risk Factors			
	Portfolio Returns (in %)		
	(1)	(2)	(3)
Enrolled	0.150**	0.139**	0.132**
	(2.40)	(2.30)	(2.18)
Obs	111903	111903	111903
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes
Portfolio beta	Yes	Yes	Yes
Size factor	Yes	Yes	Yes
Value factor	No	Yes	Yes
Momentum factor	No	No	Yes

Panel B: Exclude the Most and Least Able Applicants		
	Portfolio Returns (in %)	
	(1)	(2)
Enrolled	0.182**	0.157**
	(2.46)	(2.30)
Obs	89940	89940
FE: time; cutoff; priority	Yes	Yes
FE: cohort-obs year	Yes	Yes
Individual controls	Yes	Yes
Scale & Menu effects	Yes	Yes
Portfolio beta	Yes	Yes
Risk controls	No	Yes

Notes: This table presents second-stage estimates of regressions of household stock portfolio returns, where we instrument enrollment in a business program by being above the cutoff at the time of admission. In the portfolio return regressions, we control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, and the portfolio beta. In Panel A, the portfolio loadings on the size, value, and momentum factors are sequentially included in columns (1) through (3). In Panel B, the analysis excludes applicants in the top and bottom 10 percent of the high school GPA distribution. 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

(Lusardi and Mitchell 2014). We examine this explanation in more detail in the following analysis.

While we do not directly observe individuals' level of financial sophistication — commonly measured by the "Big Three" survey questions developed by Lusardi and Mitchell (2007) — we infer it through individuals' actual portfolio choices. Specifically, we examine two prevalent investment mistakes —portfolio underdiversification and the disposition effect — to assess whether business education enhances financial literacy. To measure portfolio diversification, we use the total number of stocks in the portfolio as a crude proxy (Goetzmann and Kumar 2008). The disposition effect, defined as the tendency to hold losing stocks too long and sell winning stocks too early, is measured by calculating the difference between the proportion of realized stock gains and losses within a given year (Odean 1998; Calvet, Campbell, and Sodini 2009b).[22] Table O.A.8 in the online appendix demonstrates that quasi-random enrollment in a business or economics program leads to greater portfolio diversification and a significant reduction in behavioral biases. These results imply that business education significantly increases individuals' financial sophistication, effectively improving their ability to process economic information and make more informed investment decisions.

To strengthen this interpretation, we next estimate the effect of business education on portfolio returns using a sample split into time periods of relatively good and of relatively bad market conditions, defined by the median values of market returns and annual volatility of the Swedish stock price index from 2000 to 2007.[23] This analysis is based on the idea that the value of acquiring and processing information is particularly high during market downturns and periods of elevated aggregate volatility (Kacperczyk, Nosal, and Stevens 2019). Under such conditions, the enhanced ability of business-educated individuals to process economic information becomes particularly important for generating higher returns, as the return to information increases when the price system is less informative (Grossman and Stiglitz 1980).

As shown in Table 3, the positive contribution of business education to portfolio returns is confined to periods of market downturns and high aggregate volatility. The estimated magnitudes are also large, ranging from 21 to 27 basis points per month. In contrast, during relatively favorable market conditions, characterized by higher aggregate returns and lower volatility, the estimated effects are both economically and statistically insignificant. This observed asymmetry between good and bad market conditions provides strong evidence of the enhanced ability of individuals with a business or economics

[22]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.

[23]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 from the FRED database.

Table 3: Portfolio Returns and Business Education by Aggregate Market Conditions

Panel A: Aggregate Market Returns

	Portfolio Returns (in %)			
	Higher Returns		Lower Returns	
	(1)	(2)	(3)	(4)
Enrolled	0.051	0.015	0.265***	0.259***
	(0.69)	(0.21)	(2.80)	(2.90)
Obs	68210	68210	43466	43466

Panel B: Aggregate Market Volatility

	Portfolio Returns (in %)			
	High Vola		Low Vola	
	(1)	(2)	(3)	(4)
Enrolled	0.248**	0.209**	0.091	0.087
	(2.51)	(2.27)	(1.24)	(1.24)
Obs	44092	44092	67531	67531
FE: time; cutoff; priority	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes	Yes
Portfolio beta	Yes	Yes	Yes	Yes
Risk controls	No	Yes	No	Yes

Notes: his table presents second-stage estimates of regressions of household stock portfolio returns, where we instrument enrollment in a business program by being above the cutoff at the time of admission. In the portfolio return regressions, we control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, and the portfolio beta. 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. 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

education to effectively acquire and process economic information, enabling them to make more informed investment decisions. Further support for this interpretation is provided by the analysis reported in Table O.A.9 in the online appendix. In particular, we rerun the portfolio return analysis for two subsamples, split by the idiosyncratic volatility (IVOL) and liquidity of the individuals' direct stock portfolios. The results show that business education significantly increases portfolio returns only when the underlying stocks exhibit higher IVOL and lower liquidity. In contrast, for portfolios characterized by low IVOL and high liquidity, the effect is both economically and statistically indistinguishable from zero.[24]

Overall, the evidence presented in this section highlights the key role of increased financial sophistication in generating higher portfolio returns, beyond differences in risk exposure and innate ability. Our findings provide direct empirical support for the model predictions of Lusardi, Michaud, and Mitchell (2017) and Jappelli and Padula (2017), while also providing a credible microfoundation for the mechanisms through which financial literacy contributes to higher returns.

4.3 Robustness to Alternative Interpretations

Our empirical findings so far suggest that enrolling in a business or economics program increases an individual's financial sophistication, which improves his or her ability to acquire, process, and use relevant information more effectively. In what follows, we further scrutinize this interpretation and examine its robustness to alternative explanations.

Is it level of education?

First, one might worry that the documented positive effects are 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).[25] This alternative explanation could challenge the interpretation of our results if

[24]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.

[25]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

individuals who were marginally admitted to a business program also had higher college completion rates than those just below the admission cutoff. Indeed, we observe that the unconditional probability of obtaining a college degree within 8 years of application is significantly 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 obtaining any college degree within 8 years by 3.2 percentage points (t-stat. = 4.23).

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 297,633 to 219,391 applicant-year observations. As reported in Panel A of Table 4, we obtain similar results. Specifically, being enrolled in a business or economics program increases average monthly portfolio returns by about 18 basis points (t-stat. = 2.57), 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.[26]

Is it the labor market?

Second, the effect of business education on portfolio returns may be manifested through the broader consequences of business education on individuals' labor market prospects, particularly through unemployment risk and career paths (e.g., working in the financial industry).[27] If business education leads to jobs with greater security and reduced uncertainty about labor income, it could enable individuals to take more financial risk and, consequently, earn higher returns.[28]

To address this concern, we first define an indicator variable for unemployment based on whether an individual received unemployment benefits in a given year. We then regress this variable on on an indicator for enrollment in a business program, along with individual controls and fixed effects. As shown in column (1) of Panel B of Table 4, we find no

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.

[26] Of course, we acknowledge that conditioning on post-treatment outcomes introduces selection, and thus that 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.

[27] 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).

[28] 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 4: Household Financial Behavior and Business Education: Alternative Explanations

Panel A: University degree-holders only			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.002	9684.111***	0.182**
	(0.13)	(3.05)	(2.57)
Obs	219391	190792	86848

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***	29610.392***
	(-0.25)	(4.52)	(4.46)
Obs	300003	300003	277333

Panel C: Including only quantitative next-best fields			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	-0.002	5394.718	0.149*
	(-0.11)	(1.47)	(1.82)
Obs	103767	91804	46309

Panel D: Controlling for peer effects			
	Portfolio Returns (in %)	Portfolio Returns (in %)	
	(1)	(2)	
Enrolled	0.154**	0.155**	
	(2.40)	(2.43)	
Overlap	-0.011***	-7.502***	
	(-13.81)	(-17.37)	
Obs	111903	111903	

Panel E: Controlling for Institutional Quality			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	-0.015	1245.874	0.158**
	(-0.90)	(0.37)	(2.25)
Obs	148111	126295	54532

Panel F: Excluding Elite Schools			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	-0.003	2320.851	0.109**
	(-0.21)	(0.89)	(2.05)
Obs	271899	232923	99523
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	No	No	Yes
Portfolio beta	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. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions, we control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, 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.

significant effect of business education on individual unemployment risk, with a negative point estimate (-0.002; t-stat. = -0.11). This result suggests that the positive effect of business education on portfolio returns is not driven by differences in unemployment risk between individuals with and without business education.

In contrast, as shown in columns (2) and (3) of Panel B, we find positive and significant effects on the probability of working in finance and on individual earnings, raising the possibility that the return effects of business education may be partly mediated through these channels. First, as discussed in Section 4.1, we acknowledge the importance of scale dependence in portfolio returns (Gabaix et al. 2016). To address this, we explicitly account for scale effects in all return regressions, which also 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 - when there are no differences in financial or net wealth - support the notion that income differences are unlikely to drive the return results. Furthermore, in Table O.A.11 in the online appendix, we perform a naïve mediation analysis, controlling for earnings, unemployment risk, and working in the finance industry, and find very similar results. In particular, the coefficient on working in finance becomes insignificant (t-stat. = 0.62) after controlling for quasi-random enrollment in a business or economics program. In untabulated analysis, we verify this finding using a causal mediation analysis similar to Dippel et al. (2022), which also shows that working in finance does not significantly explain the total effect of business education on portfolio returns.

Taken together, these additional empirical results suggest that while business education affects individuals' career paths and earnings, its causal effect on portfolio returns is predominantly direct and not mediated through labor market outcomes.

Is it quantitative education?

Third, an extensive literature highlights the role of quantitative education and cognitive skills in household portfolio choices (Brown et al. 2016; Christelis, Jappelli, and Padula 2010; Grinblatt, Keloharju, and Linnainmaa 2011).[29] Building on this, we analyze whether our results are primarily driven by improved financial sophistication or by improved quantitative skills acquired through business or economics education. To do this, we focus on college graduates with a business or economics major as their preferred choice, and a technology or science major as their next best alternative. This control group allows us to isolate individuals who were admitted to a non-business program where they could still develop quantitative skills after falling just below the threshold for admission to a business program.

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

As shown in Panel C of Table 4, 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 15 basis points higher monthly returns on average relative to their peers who are assigned to a technology or science program, with no systematic effects observed for the intensive or extensive margins of stock investments. These results suggest that increased financial sophistication, rather than quantitative skills, is the key driver of higher portfolio returns.

Is it peer effects?

Fourth, we consider peer effects as a possible alternative explanation. Business students may gain access to more financially sophisticated peers, either through alumni networks or workplace associations, who can provide direct investment recommendations or relevant information for stock investments. We address this explanation in several ways. First, while the existing literature documents positive peer effects on various economic and financial decisions, such as stock market participation or saving for retirement (Duflo and Saez 2002; Haliassos, Jansson, and Karabulut 2020), at the stock level, Hvide and Östberg (2015) find that individuals do not earn significantly higher returns by investing in stocks in which their (work) peers invest heavily.[30] Taken at face value, this evidence suggests that peer effects are unlikely to drive our findings, as they would predict outcomes opposite to the positive return effects documented in our analysis.

In addition, we conduct a more direct test of peer effects using the entire population of households in Sweden. First, we identify individuals who majored in a business-related program at either university or high school level. Using our ability to observe their stock investments at the security level, we create a stock-level measure, which captures the share of a given firm's outstanding stocks held directly 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. 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.

[30] 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.

To test the potential role of peer effects in our results, we extend the regressions by including the portfolio-level overlap measure in the estimation model. The results, reported in Panel D of Table 4, show that the coefficient on enrollment in a business program retains its economic and statistical significance even after accounting for peer effects, regardless of whether the ranked or continuous form of the portfolio overlap measure is used. In Table O.A.12 in the online appendix, we verify these results by using alternative measures of peers, i.e., those who had a business or economics education at the university level.

Is it college quality?

To this point, our analysis has not accounted for potential heterogeneity in institutional quality across universities where individuals apply to business and non-business programs. A well-established literature emphasizes that college quality significantly affects individuals' long-term economic outcomes. Given our focus on applicants to relatively competitive programs with admissions cutoffs (i.e., programs with more applicants than available slots), one might question whether our findings reflect differences in "college quality" rather than the specific content of business education.

To address this concern, we restrict our analysis to applicants whose preferred program (business) and next-best alternative (non-business) are offered at the same university. This approach ensures that individuals who are not admitted to their preferred business program and instead enroll in a non-business program experience the same institutional quality, thereby isolating the effect of educational content. Panel E of Table 4 reports the estimation results. Reassuringly, we find that enrollment in a business or economics program increases the average monthly portfolio returns by about 16 basis points (t-stat. = 2.25), even after accounting for heterogeneity in quality across universities. This finding suggests that our results are not simply an artifact of potential differences in college quality. Rather, it is the content of business education that leads to higher portfolio returns.

Is it elite school effects?

Finally, prior research highlights that an elite college background is a key determinant of labor market success, with graduates of highly selective universities disproportionately represented in managerial positions and at the top of the income distribution (Bertrand, Goldin, and Katz 2010; Michelman, Price, and Zimmerman 2022; Zimmerman 2019).[31] According to this interpretation, our results may simply be a (re-)manifestation of the well-documented effects of elite schools.

[31] For example, Zimmerman (2019) shows that graduates from a small number of elite, business-related programs account for a significant share of top income earners and leadership positions in large firms in Chile.

In Panel F of Table 4, we address this explanation by excluding from the sample applicants to the highly selective business programs at two top universities, the Stockholm School of Economics and Stockholm University. As shown in column (3), we find that the coefficient on business education in the return regressions declines slightly, but remains both economically and statistically significant. In particular, we find that individuals with a business education earn about 11 basis points (t-stat. = 2.05) more on their stock investments than their peers without a business education, even after excluding the elite schools from the sample. We further validate this finding using alternative definitions of elite schools, such as those based on the highest average GPA of the students enrolled in the school.

Taken together, the numerous empirical findings presented in this section show that alternative mechanisms, such as *educational attainment*, *quantitative skills*, *unemployment risk*, *career trajectories*, *peer effects*, *college quality*, and *elite schools*, provide little or no explanatory power.

4.4 External Validity

Our causal evidence thus far suggests that enrolling in a business or economics program significantly improves an individual's ability to process economic information and make more informed decisions, which ultimately leads to better investment performance. Taken at face value, these findings have important policy implications. Specifically, they suggest that financial education can be an effective policy tool for empowering households to make sound financial decisions. There is, however, an important caveat regarding the external validity of our findings when extending them to broader policy recommendations. Our sample consists of high school graduates who intend to pursue higher education, particularly in business or economics programs, and our estimates capture local average treatment effects for individuals complying with treatment assignment. Therefore, it is important to consider whether these estimated sample-average treatment effects can be generalized to a broader population.

To address this, we first compare the background characteristics, such as high school GPA and cognitive ability, of our sample with those of the broader college-educated and business-educated populations, matched on year of birth, gender, and immigrant status (see Table O.A.13 in the online appendix).[32] We note that our sample has slightly higher standardized high school GPA and cognitive ability (as measured by IQ tests during military enlistment) than the average college-educated individual, although these differences are small. Because the regression discontinuity design selects individuals who apply to competitive programs with defined admission cutoffs, these observations are not

[32]Figure O.A.1 in the online appendix illustrates the quality of the matching process.

unexpected. In addition, individuals in our sample are more likely to have parents with a college education, suggesting some degree of positive selection on cognitive ability and socioeconomic status.

Given this background, we next re-estimate the financial behavior analysis excluding applicants who fall within the top 10% or 25% of the high school GPA distribution respectively, which makes the sample more representative of the average college graduate in Sweden, as reflected in the closer match of average GPA and cognitive scores. Even after excluding the most able applicants from the sample, we observe significant and positive effects of business education on financial behavior, as reported in Table O.A.14 in the online appendix. This robustness analysis suggests that our results are not driven solely by high ability individuals, and supports the external validity of our findings.

Second, we examine the cross-section of applicants by their parental background. Extensive literature highlights significant intergenerational spillovers in educational attainment and earnings (Björklund, Lindahl, and Plug 2006; Black et al. 2020; Black, Devereux, and Salvanes 2005). Similarly, empirical evidence in the financial literacy literature emphasizes the link between an individual's financial sophistication and that of their parents (Lusardi and Mitchell 2014). This raises an important question: does business education complement or substitute for the intergenerational transmission of financial sophistication?

Table 5 presents regression results on financial behavior, splitting the sample based on whether at least one of the parents hold a college degree. Importantly, we find that the positive impact of business education on portfolio performance is significant only for individuals with less educated parents. For example, column (3) shows that a business major increases monthly stock returns by approximately 24 basis points (t-stat. = 2.32) for these individuals, while the effect is negligible for those with college-educated parents (0.03%, t-stat. = 0.30). This suggests that business education acts as a substitute for learning from parents and thus for the intergenerational persistence of financial sophistication.

These findings highlight an important asymmetry in treatment effects based on parental sophistication, which also has implications for the external validity of our study. In particular, applicants from relatively more disadvantaged backgrounds may lack alternative sources of financial knowledge, making financial education interventions particularly effective in improving their knowledge and financial behavior. In fact, our finding that treatment effects are not driven by individuals with higher parental human capital does not only strengthen the internal validity of our study, but also increases its 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.

Table 5: Household Financial Behavior and Business Education by Parental Background

Panel A: Parents with College Education			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.010	7303.359*	0.026
	(0.59)	(1.85)	(0.30)
Obs	159252	138414	64177

Panel B: Parents without College Education			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	-0.016	5113.904	0.236**
	(-0.80)	(1.36)	(2.32)
Obs	138365	116212	47636
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	No	No	Yes
Portfolio beta	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, we control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

5.1 Business Education and Household Wealth

Our empirical analysis shows that quasi-random enrollment in a business program increases stock market investment and improves investment returns. Building on these findings, we next examine whether the effects of business education extend beyond portfolio choices to household wealth accumulation.

Table 6: Business Education and Household Wealth

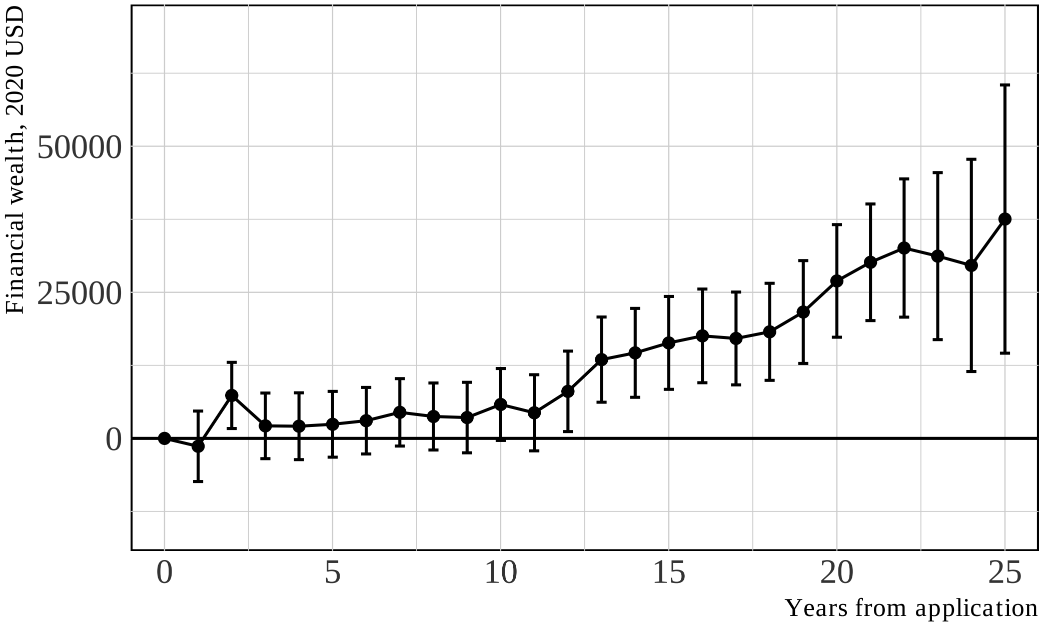
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	11628.782***	28185.946***	0.025**
	(3.06)	(2.88)	(2.05)
Obs	297633	297633	297633
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Notes: This table presents the second-stage estimates of household wealth regressions in which we instrument enrollment in a business program for 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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 6 reports the results of the wealth analysis. Column (1) examines household financial wealth, defined as the sum of direct and indirect stocks, bonds, mutual funds, and balances in savings and checking accounts. Column (2) focuses on household net wealth, calculated by subtracting household debt from total financial and real assets. Column (3) investigates individuals' relative position in the wealth distribution, measured by their percentile rank. Our findings show that quasi-random enrollment in a business or economics program leads to significantly higher levels of financial and net wealth, as well as improved wealth distribution rankings, 4 to 25 years after initial application. The effects are economically meaningful, with business education associated with an average increase of about USD 11,600 in financial wealth (18% relative to the mean) and USD 28,200 in net wealth (16.5% relative to the mean).

Next, we analyze the evolution of wealth effects over time. To do so, we extend our baseline regression model (Equation 2) by including an interaction term between enrollment in a business program and the number of years since application. Figure 5 shows the

Figure 5: Business Education and Household Financial Wealth over Time



Notes: This figure illustrates the evolution of the wealth effects of business education over time. Specifically, we extend our basic regression model, as outlined in Equation 2, by including an interaction term of enrollment in a business program and the number of years since application, and plot the estimated coefficients along with their confidence intervals over time. The x-axis reports the number of years since enrolling in 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 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.

results. The x-axis indicates the number of years since application to a business program, while the y-axis shows the coefficient estimates for the interaction of business education with each year (up to year 25) based on the financial wealth regression.

The figure shows that business education has no significant effect on household financial wealth accumulation within the first 10 years after application. This lack of a systematic relationship in the short run is largely expected, as wealth is a stock variable and differences in accumulation typically compound over time. Consistent with this, we find evidence of a significant positive causal wealth effect in the medium term. In particular, the wealth gap between educated and uneducated households grows monotonically over 25 years, suggesting that early investments in financial literacy reshape life-cycle wealth profiles. Individuals with similar initial preferences and abilities accumulate markedly different levels of wealth later in life.[33] A similar pattern emerges when household net wealth is analyzed as the outcome variable, as shown in Figure O.A.3 in the online appendix.

Taken together with the evidence presented in Section 4.1, these results provide direct

[33]Estimates beyond 20 years are less precise because of the smaller sample size at longer time horizons.

empirical support for the theoretical model of Lusardi, Michaud, and Mitchell (2017), which shows that differences in financial sophistication can lead to substantial differences in household wealth accumulation by affecting investment behavior and portfolio returns.

5.2 Alternative Channels of Influence

In the following, we explore alternative channels through which enrollment in a business program may influence household wealth accumulation beyond financial behavior.

Labor Market Outcomes

One important potential mechanism driving the positive wealth effects of business education is the labor market channel. The literature highlights substantial heterogeneity in labor market returns across college majors, with differences in effect sizes comparable to the overall returns to 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) find that business education leads to significantly higher early career earnings than social sciences or humanities, although no significant differences emerge compared to medicine, engineering, or law. Consistent with these findings, we show in section 4.3 that individuals with a business education experience significantly higher earnings later in life. Thus, the observed wealth effects may simply reflect an extension of the established impact of business education on earnings to household wealth accumulation. We explore this explanation below.

Following Kirkebøen, Leuven, and Mogstad (2016), we quantify the relative labor market returns to business education compared to other majors among households with positive earnings. As shown in Table O.A.15, we find no significant differences in earnings between business education and fields such as medicine, law, health, humanities, and other disciplines. However, business education does lead to higher earnings relative to majors in science, social science, technology, and teaching.[34] We then restrict the sample to individuals whose next best college major yields earnings similar to business education, effectively muting the labor income channel, and re-estimate the wealth regressions. This sample restriction controls for differences in labor income levels between households with and without business education later in life. In this way, we isolate the wealth effects of business education that operate through alternative channels beyond labor income.

Table 7 reports the coefficient estimates for the wealth regressions for this restricted subsample. The results indicate that enrolling in a business program still has positive and

[34] We acknowledge that some of the 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 7: Household Wealth and Business Education: The Role of Earnings

	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	27273.038***	64184.585***	0.054**
	(2.98)	(2.59)	(1.98)
Obs	78611	78611	78611
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Notes: This table presents the second-stage estimates of household wealth regressions in which we instrument enrollment in a business program for 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

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. These findings suggest that differences in accumulated wealth levels between business and non-business educated individuals are not primarily driven by labor market outcomes.

Household Debt

Next, we examine whether the documented wealth effects of business education operate through the liability side of household balance sheets. For example, Hvidberg (2023), who uses a similar identification strategy as ours to examine the impact of business education on debt behavior in Denmark, finds 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 examine this channel, we decompose household net wealth into its two main components - gross assets and total liabilities - and re-estimate our regressions.

Columns (1) and (2) of Table 8 present the regression results for gross household assets

Table 8: Business Education and Household Assets and Debt

	Total Assets	Total Liabilities
	(1)	(2)
Enrolled	34833.427***	5306.042
	(3.11)	(1.17)
Obs	297633	297633
FE: time; cutoff; priority	Yes	Yes
FE: cohort-obs year	Yes	Yes
Individual controls	Yes	Yes

Notes: This table presents the second-stage estimates of household asset and debt regressions in which we instrument enrollment in a business program for 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

and liabilities, respectively. The results show that enrolling in a business or economics program significantly increases household gross assets, with business-educated individuals accumulating approximately USD 35,000 more in total assets (t-stat. = 3.11). In contrast, the effect on household debt is not precisely estimated (t-stat. = 1.17), indicating that debt behavior is unlikely to be a primary mechanism underlying the observed wealth outcomes. In unreported tests, we confirm this conclusion using an alternative measure of household debt, defined as household leverage (total debt normalized by annual labor income). Overall, the empirical evidence suggests that business education enhances wealth accumulation primarily through its effects on household gross assets.

Homeownership

For most households, housing serves as a primary savings vehicle, and high returns on housing, especially when leveraged, can contribute significantly to household wealth accumulation (Happel et al. 2024). Against this background, we examine whether homeownership decisions mediate the wealth effects of business education. In Table O.A.16 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). Next, we stratify the sample by homeownership status and re-estimate the wealth regressions. The regression results presented in Table 9 show that enrolling in

Table 9: Household Wealth and Business Education: The Role of Housing Investments

Panel A: Homeowners			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	12617.428***	40199.140***	0.028**
	(2.72)	(3.36)	(2.37)
Obs	213603	213603	213603
Panel B: Renters			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	12562.536***	14375.211*	0.046**
	(2.63)	(1.68)	(2.42)
Obs	83834	83834	83834
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	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 in which we instrument enrollment in a business program for 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

a business program leads to significantly higher household financial and net wealth, as well as a higher rank in the wealth distribution, regardless of homeownership status. In particular, the effect of business education on financial wealth is comparable for renters and homeowners, while the effect on net wealth is substantially larger for homeowners (USD 40,000 vs. 14,000). These findings suggest that the positive contribution of business education to household wealth accumulation cannot be attributed solely to differences in housing investment decisions between business educated and non-business educated households.

6 Conclusions

This paper presents empirical evidence that business education, by increasing individuals' financial sophistication, causally improves returns on risky assets and positively affects household wealth accumulation in the short to medium term. Exploiting exogenous variation induced by university admission thresholds, we show that enrollment in a business-related program increases stock market participation, significantly improves portfolio returns, and leads to higher wealth accumulation later in life. The estimated effects of business education are economically significant: the observed return differential is equivalent to an annualized gain of 1.86 percentage points, which, under modest assumptions, could lead to about 60% higher direct stock wealth or 20% higher total financial wealth over 25 years.

We then examine the mechanisms driving the positive effects of business education on portfolio returns. Our analysis shows that differences in risk exposure and innate ability between business and non-business educated individuals cannot fully account for the observed return gap. Instead, further evidence suggests that business educated individuals exhibit greater financial sophistication, which enables them to make more informed portfolio decisions. In particular, these individuals earn significantly higher returns than their non-business educated peers during market downturns and periods of high volatility - precisely when the ability to acquire and process information is most valuable. In contrast, we find no systematic differences in returns during favorable market conditions, when the benefits of improved information processing are less pronounced. Taken together, these findings highlight the key role of enhanced economic information acquisition and processing skills in driving the superior portfolio performance of business educated individuals, over and above differences in risk exposure or innate ability. We also examine alternative explanations, such as educational attainment, quantitative skills, unemployment risk, career trajectories, peer effects, college quality, and attendance at elite schools. However, these factors provide little or no explanation for the documented effects.

The impact of business education extends beyond financial behavior to significantly affect household wealth accumulation. Specifically, enrolling in a business program increases financial wealth by an average of USD 11,600 and net wealth by USD 28,155. An analysis of the dynamics of wealth accumulation reveals that these effects emerge gradually over the medium term and are followed by a steady widening of the wealth gap between business majors and non-business majors. These findings suggest that business education reshapes life-cycle wealth trajectories, with individuals with similar initial characteristics ending up with substantially different levels of wealth. We also examine alternative mechanisms that could drive these effects, including labor market outcomes, household debt behavior, and housing investment. However, our results suggest that the positive wealth effects of business education cannot be fully explained by these alternative channels.

In conclusion, this paper shows that business education plays a critical role in shaping individuals' financial and wealth trajectories. By fostering greater financial sophistication, financial education improves portfolio performance and contributes positively to household wealth accumulation. These findings highlight the importance of financial education in empowering individuals to make informed financial decisions and accumulate substantial wealth over their lifetimes.

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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 Ownership		
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 Holdings			
	Direct Stock Holdings		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	1882.782***	4540.112***	6211.112***
	(2.64)	(2.63)	(2.63)
Obs	254,653	254,653	254,653
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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 over Time

Panel A: Short-term Effects of Business Education (14 years>$t\geq$4 years)			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.006	1941.805	0.263**
	(0.34)	(0.63)	(2.22)
Obs	133427	111917	42737
Panel B: Medium-term Effects of Business Education (25 years$\geq t \geq$14 years)			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	-0.002	10088.805***	0.067
	(-0.12)	(2.91)	(0.89)
Obs	164092	142596	69007
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	No	No	Yes
Portfolio beta	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 (3), we also control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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: Household Financial Behavior and Business Education: Alternative Size Filters

	Portfolio Returns (in %)			
	(1)	(2)	(3)	(4)
Enrolled	0.137**	0.144**	0.157**	0.141**
	(2.16)	(2.25)	(2.43)	(2.06)
Obs	125262	121287	118058	98543
FE: time; cutoff; priority	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes	Yes
Portfolio beta	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 also control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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: 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**	0.015**
	(2.30)	(2.29)	(2.30)
Obs	162,731	162,731	162,731
Panel B: With Size Filter			
	Returns on Risky Assets		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	0.004**	0.010**	0.013**
	(2.01)	(2.01)	(2.02)
Obs	157,056	157,056	157,056
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes
Portfolio beta	Yes	Yes	Yes

Notes: This table reports the estimates of the regressions of the return on risky assets. . 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 also control for the interaction of the time-year dummies and the risky share of financial wealth, fixed effects for deciles of the value of the full risky portfolio, 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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: Addressing Scale Effects vs. Individual Controls

	Stock Holdings	Portfolio Returns (in %)		
	(1)	(2)	(3)	(4)
Enrolled	1941.805	0.263**	0.332***	0.300**
	(0.63)	(2.22)	(2.69)	(2.47)
Obs	111917	42737	43329	49295
FE: time; cutoff; priority	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	No	No
Scale & Menu effects	No	Yes	No	No
Portfolio beta	No	Yes	No	No
Size filter	No	Yes	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 to 14 years after initial application), where we instrument for enrollment in a business program by being above the cutoff at the time of admission. In column (2), we also control for the interaction of the time-year dummies and the stock share of financial assets, fixed effects for deciles of stock portfolio value, and portfolio beta. Columns (3) and (4) exclude all individual-level controls, and we relax the size filter (4). 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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: Controlling for Next Best Major Fixed Effects

Panel A: Financial Behavior			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.004	6838.717**	0.141**
	(0.28)	(2.56)	(2.15)
Obs	293444	251218	110645
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
FE: next-best field	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes
Portfolio beta	Yes	Yes	Yes

Panel B: Wealth Outcomes			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	12004.131***	28424.884***	0.027**
	(3.14)	(2.88)	(2.21)
Obs	293444	293444	293444
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
FE: next-best field	Yes	Yes	Yes
Individual controls	Yes	Yes	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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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 Investment Mistakes

	Disposition Effect		Diversification	
	(1)	(2)	(3)	(4)
Enrolled=1	-0.024*	-0.023*	0.334**	0.272*
	(-1.79)	(-1.65)	(2.04)	(1.87)
Obs	45615	43897	147684	123206
FE: time; cutoff; priority	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Scale effects	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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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 Portfolio Returns: Learning and Information Processing

Panel A: Stock Portfolios by IVOL

	Portfolio Returns (in %)			
	High IVOL		Low IVOL	
	(1)	(2)	(3)	(4)
Enrolled	0.333**	0.316**	0.097	0.049
	(2.41)	(2.31)	(1.55)	(0.83)
Obs	37307	37307	34203	34203

Panel B: Stock Portfolios by Illiquidity

	Portfolio Returns (in %)			
	High Amihud		Low Amihud	
	(1)	(2)	(3)	(4)
Enrolled	0.321**	0.302**	0.061	-0.008
	(2.06)	(1.98)	(1.08)	(-0.30)
Obs	37250	37250	41761	41761
FE: time; cutoff; priority	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes	Yes
Portfolio beta	Yes	Yes	Yes	Yes
Risk controls	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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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
Above_Cutoff	0.032***
	(4.23)
Obs	300003
FE: time; cutoff; priority	Yes
FE: cohort-obs Year	Yes
Individual controls	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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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	0.1536**	0.1548**	0.1514**	0.1526**	0.1535**
	(2.33)	(2.26)	(2.31)	(2.30)	(2.24)
Earnings		-0.0001**			-0.0001***
		(-2.19)			(-2.93)
Unemployed			-0.0716***		-0.0817***
			(-3.84)		(-4.20)
Works in Finance				0.0144	0.0106
				(0.62)	(0.45)
Obs	111903	108848	111903	111903	108848
FE: time; cutoff; priority	Yes	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Scale & Menu effects	Yes	Yes	Yes	Yes	Yes
Portfolio beta	Yes	Yes	Yes	Yes	Yes

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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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 %)	Portfolio Returns (in %)
	(1)	(2)
Enrolled	0.154**	0.155**
	(2.40)	(2.43)
Overlap (I)	-0.011***	
	(-13.81)	
Overlap (II)		-7.502***
		(-17.37)
FE: time; cutoff; priority	Yes	Yes
FE: cohort-obs year	Yes	Yes
Individual controls	Yes	Yes
Scale & Menu effects	Yes	Yes
Portfolio beta	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 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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: External Validity: Summary Statistics

	Study sample	University educated	Business educated
	(1)	(2)	(3)
High school GPA	0.84	0.57	0.70
	(0.82)	(0.92)	(0.89)
Cognitive skills (men only)	6.92	6.66	6.52
	(1.38)	(1.57)	(1.47)
Has university degree	73.81%	100.00%	100.00%
	(0.44)	(0.00)	(0.00)
Has business degree	38.25%	17.38%	100.00%
	(0.49)	(0.38)	(0.00)
Works in finance (age 35)	7.77%	3.31%	11.87%
	(0.27)	(0.18)	(0.32)
Unemployed (age 35)	5.97%	10.49%	7.10%
	(0.24)	(0.31)	(0.26)
Earnings percentile (age 31-35)	0.72	0.58	0.71
	(0.28)	(0.30)	(0.29)
Parental earnings percentile (age 14-18)	0.61	0.57	0.59
	(0.19)	(0.20)	(0.19)
Parent has university degree	41.98%	35.73%	38.09%
	(0.49)	(0.48)	(0.49)
Parent has business degree	4.80%	3.10%	5.33%
	(0.21)	(0.17)	(0.22)
Net wealth (USD)	175059.07	111379.00	175078.28
	(235583.81)	(196236.26)	(245557.76)
Financial wealth (USD)	65924.56	44736.49	66304.69
	(90838.13)	(73165.55)	(94681.92)
Homeowener	71.14%	64.44%	67.38%
	(0.36)	(0.38)	(0.37)
Stock market participation	83.10%	75.07%	81.23%
	(0.30)	(0.36)	(0.32)
Portfolio returns	0.47%	0.52%	0.44%
	(0.02)	(0.02)	(0.02)
Observations	34333	200000	100000

Notes: This table presents summary statistics for three samples. Column (1) summarizes the characteristics of the study sample (within the bandwidth). Columns (2) and (3) represent samples from the population drawn to match the birth, gender, and immigrant status of the study sample. 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.

Table O.A.14: External Validity: Household Financial Behavior and Business Education

Panel A: Excluding top 10%			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.001	4761.480*	0.155**
	(0.09)	(1.70)	(2.13)
Obs	255286	218794	95070
Panel B: Excluding top 25%			
	Participation	Stock Holdings	Portfolio Returns (in %)
	(1)	(2)	(3)
Enrolled	0.001	4179.494	0.153*
	(0.06)	(1.40)	(1.83)
Obs	212828	181461	77684
FE: time; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Scale & Menu effects	No	No	Yes
Portfolio beta	No	No	Yes

Notes: This table presents the second-stage estimates of the regressions of household financial behavior, where we instrument enrollment in a business program with being above the cutoff at the time of admission. Panel A (B) excludes applicants who are in the top 10% (25%) of the GPA distribution. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions, we control for the interaction of the time-year dummies and the stock share in financial wealth, fixed effects for deciles of stock portfolio value, 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.15: 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	94581.417*	-108967.922	25815.732	31881.721	31961.620	52744.533**	73145.888***	23407.971***	20459.624
	(1.72)	(-1.07)	(0.33)	(1.60)	(0.62)	(2.01)	(3.55)	(2.74)	(1.28)
Obs	12742	12190	11037	43933	9182	36159	30828	117136	76422
FE: time; cutoff; priority	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

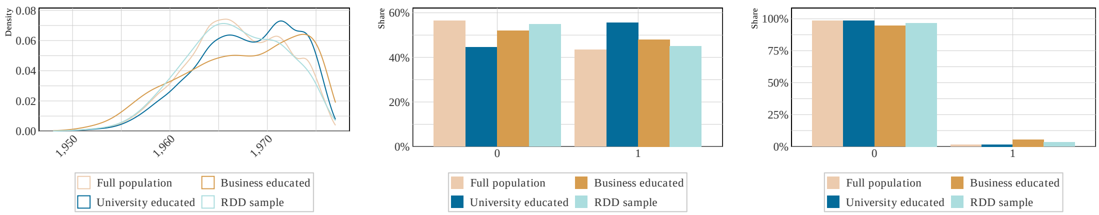
Notes: This table presents the second stage estimates of individual earnings regressions where where we instrument enrollment in a business program with being above the cutoff at the time of admission. Following Kirkebøen, Leuven, and Mogstad (2016), we quantify the relative labor market payoffs of business 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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.16: 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; cutoff; priority	Yes	Yes	Yes
FE: cohort-obs year	Yes	Yes	Yes
Individual controls	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, estimated separately above and below the cutoff for each admission group. Additionally, they include fixed effects for each admission cutoff, the priority ranking of the business program in the application, and the number of years since the application. We also control for birth cohort-by-observation year fixed effects and include indicator variables for the applicant's gender and whether the applicant is foreign-born. 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.

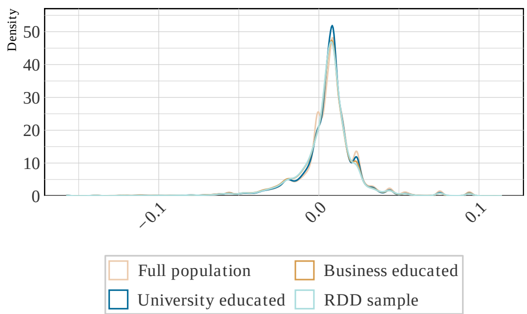
Figure O.A.1: Summary statistics: matched samples



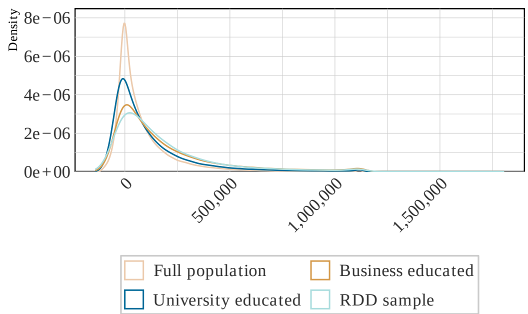
(a) Birthyear (b) Women (c) Foreign born

Notes: This figure reports densities and histograms of the matched samples reported in Table ??. 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 ??, the figure also includes a matched sample from the full population of Swedes.

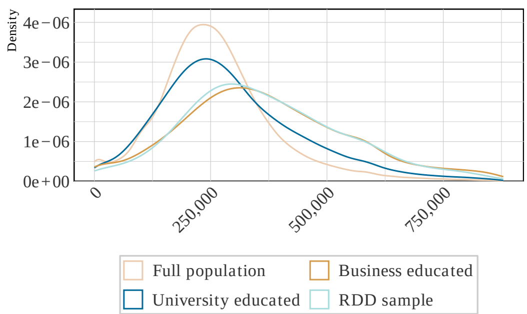
Figure O.A.2: Summary statistics: key metrics



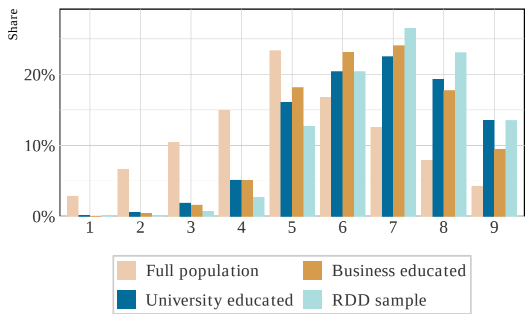
(a) Average returns



(b) Average net wealth



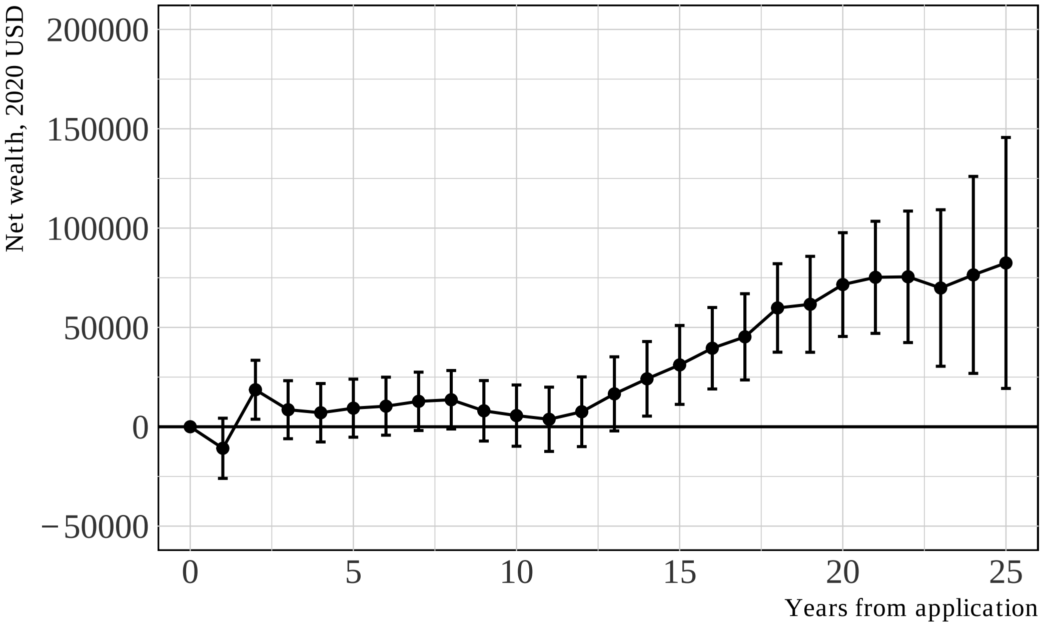
(c) Average earnings, age 31-35



(d) Cognitive skills

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

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



Notes: This figure illustrates the evolution of the wealth effects of business education over time. Specifically, we extend our basic regression model, as outlined in equation 2, by including an interaction term of enrollment in a business program and the number of years since application, and plot the estimated coefficients along with their confidence intervals over time. The x-axis reports the number of years since enrolling in 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 net wealth analysis. 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.