Business Education and Portfolio Returns*

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Abstract

We provide evidence of a positive causal link between financial knowledge acquired through business education and returns on stock investments. Using exogenous variation generated by admission thresholds to university business programs in Sweden, we document that early investments in financial sophistication causes individuals to invest significantly more in the stock market, to earn higher portfolio returns, and to end up accumulating higher levels of wealth. Investments in financial sophistication at the launch of economic life thus significantly alters the life cycle wealth profiles of individuals.

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1 Introduction

A budding literature in macroeconomics and household finance documents that cross-sectional heterogeneity in returns to wealth is a key driver of variation in household wealth, including the thick tail in the wealth distribution observed in many countries.[1] In principle, wealth 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 show that differences in financial knowledge can generate large differences in household wealth mainly through their effects on returns on savings, which can account for a sizeable portion of wealth inequality in the U.S. Despite the intuitive appeal of a financial sophistication based explanation for heterogeneous returns, there is still lack of well-identified evidence on the causal effects of improved financial sophistication on portfolio returns.[3]

In this paper, we provide causal evidence that improved financial knowledge acquired through formal business education positively contributes to rates of returns on risky investments, which also affects household wealth accumulation. Specifically, we use exogenous variation generated by university-program admission thresholds, and document that early investments in financial knowledge in the form of enrolling in a business-related program at the post-secondary level leads individuals both to invest more in the stock market, but also to earn significantly higher rates of returns on their stock investments. Thus, individuals with similar initial characteristics in terms of preferences and abilities end up accumulating significantly different levels of wealth later in their lives, implying that early differences in financial sophistication alter the life cycle wealth profiles of individuals.

In our empirical analysis, we overcome the thorny issue of causally identifying the effects of financial education on financial outcomes by employing a regression discontinuity design, taking advantage of the quasi-random variation around admission cutoffs to university business programs. Specifically, in Sweden, where we base our empirical analysis, there is a centralized application and admissions process that allocates applicants to a unique university-program alternative based on academic performance and preferences.

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

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

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

This system generates sharp admission cutoffs whenever a program is oversubscribed. Furthermore, the ranked list of university-program combinations that each applicant submits enables us to observe the next-best field of study to which an applicant would be allocated in case they are not admitted to a preferred alternative. From the universe of university applications made through the centralized system between 1977 to 1995, we identify all applicants who intend to study business (that includes programs such as economics, finance, business, administration, commerce, industrial economics, management, organisation) at an oversubscribed program, and have a non-business field of study as their next-best alternative. By construction, the allocation of business education to individuals is quasi-random among these applicants. Using a regression discontinuity design and an extensive individual-level panel dataset, we then contrast the short and medium term financial outcomes of business degree applicants who are marginally above the admission cutoff with those who are slightly below, to uncover the lasting causal effects of enrolling in a business degree program.

Following Fagereng et al. (2020) and Haliassos, Jansson, and Karabulut (2020), we measure improvements in financial knowledge by whether sampled individuals were quasi-randomly allocated to a business program as compared to a non-business field of study. Since our definition of business education includes programs such as economics, finance, business, or industrial economics, it is likely that individuals who enrolled in business programs have higher financial sophistication. We also scrutinize this interpretation, and provide supporting evidence that our education-based measure actually captures higher financial sophistication.[4]

We document causal evidence that financial knowledge acquired through enrolling in a business-related degree program positively contributes to the portfolio returns and financial outcomes of households. Specifically, we find that individuals who were marginally admitted to a business degree program tend to invest more in the stock market and achieve significantly higher returns on their stock investments than their otherwise similar peers who were slightly below the admission cutoff, and hence, not admitted to a business program. The effects are not only statistically significant but they are also meaningful in economic terms. For example, individuals who major in business earn approximately 10 basis points higher monthly raw returns on their stock portfolio than their non-business educated peers, which translates into an annualized rate of return difference of 1.2 percentage points.

We recognize that the documented return differences between business and non-business educated individuals can result from differences in their willingness to take more risk, as

[4]Additional support for this interpretation is provided by the survey work of Almenberg and Säve-Söderbergh (2011) who focus on a representative Swedish sample and document that education-based measure of financial sophistication correlates closely to measures based on the Big Three survey questions related to financial literacy developed by Lusardi and Mitchell (2007). Almenberg and Säve-Söderbergh (2011) document that respondents with a major in economics or engineering are significantly more financially literate than households with other majors such as social sciences, arts and humanities, and medicine.

shown by Bach, Calvet, and Sodini (2020) and Campbell, Ramadorai, and Ranish (2019). To control for this explanation, the base analysis accounts for portfolio beta and differences in access to menu of financial instruments across sampled individuals. To further refine our understanding on the role of risk taking in our results, we also control for various stock portfolio characteristics such as the average idiosyncratic volatility, size, momentum, and book-to-market. Importantly, the positive effect of majoring in business on portfolio returns remains even after accounting for differences in exposure to various sources of risk. In addition to differences in risk exposure, heterogeneity in innate ability across households could also contribute to the differences in portfolio returns, if, for example, more skilled and talented individuals sort themselves into business programs (Fagereng et al. 2020). Since by design, we contrast the stock portfolio performance of individuals with similar initial abilities and preferences, to a certain extent, we implicitly control for such differences in our empirical analysis. Taken these results together, we conclude that differences in financial sophistication – beyond heterogeneity in risk exposure and innate ability – are likely to play a distinct role in earning higher portfolio returns.

We then subject our argument, that enrolling in a business program reflects direct investments in financial knowledge, to various tests, and examine possible alternative explanations that could generate similar effects. These include level of education, career trajectory, and quantitative skills explanations. The first concern is that the positive effects of business education on household financial behavior can be driven by level of education rather than its content. Our analysis shows that business education displays positive significant effects on portfolio returns even when we restrict the sample to individuals with college education. Second, we check whether business education influences financial choices and outcomes of households through its impact on labor market outcomes and career trajectory, and show that the effects we uncover cannot be fully attributed to the career choices of individuals. Third, we confirm that it is the financial knowledge acquired through business education rather than quantitative skills that underlies the positive influence of business education. Overall, these sensitivity checks imply that our results are not a pure artifact of potential differences in level of education, quantitative skills, or career trajectory across business and non-business educated individuals. It is in fact the improved financial knowledge that leads to improved portfolio decisions.

Additional support for this interpretation comes from the analysis on the dynamic effects of majoring in business on portfolio returns. Specifically, we document that business educated households earn significantly greater returns on their stock investments in the short run, while this effect turns out to be insignificant over the medium run. This asymmetry is consistent with the model predictions of Lusardi, Michaud, and Mitchell (2017) who show that, similar to other human capital investments, financial literacy can depreciate over time, and that the optimal financial literacy profile is hump-shaped over the life cycle. At the same time, non-business educated households can improve their financial

sophistication either directly due to endogenous investments in financial knowledge or indirectly through "learning-by-doing", reducing the initial financial literacy gap over time.

As a number of studies documents the importance of intergenerational spillovers in earnings, wealth, and human capital formation (Björklund, Lindahl, and Plug 2006; Black, Devereux, and Salvanes 2005; Majlesi et al. 2019), we next turn to the analysis of whether investments in financial sophistication complements or substitutes the intergenerational transmission of financial sophistication. Interestingly, our results show that the positive contribution of business education to portfolio choice and portfolio returns is only operative for households with financially less literate parents. In contrast, we find no systematic effects in the sample of households where at least one of their parents is business educated. These cross-sectional results imply that financial education in the form of majoring in business serves as a substitute for intergenerational persistence in financial literacy, and hence, can play a key role in increasing intergenerational mobility (Black and Devereux 2010).

The effects of business education extend beyond financial behavior to household wealth accumulation. We find that individuals with business education accumulate significantly higher financial and net wealth later in their lives. In particular, enrolling in a business degree program leads to an increase of 87,000 Swedish Krona in financial wealth. To put this into context, the effect of business education corresponds to an approximately 18 percent increase in the mean financial wealth of the sampled households, which is quite sizeable. When we analyze the dynamics of wealth accumulation, we observe that the effects of business education becomes visible only over the medium run and thereafter there is a monotonic increase in the wealth gap between individuals with business and non-business majors. Hence, we conclude that early investments in financial knowledge alters life cycle wealth profiles, and individuals with similar initial characteristics end up accumulating significantly different levels of wealth later in their lives. We also investigate alternative mechanisms, such as labor market, debt behavior, or housing investments of households, that can affect wealth through channels other than financial behavior. We find that the positive wealth effects of business education cannot be fully attributed to any of these alternative mechanisms.

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

differences in household financial wealth. Similarly, Jappelli and Padula (2017) document a positive link between financial sophistication and portfolio returns and consumption growth of households using a life-cycle model that incorporates endogenous financial knowledge. Both Jappelli and Padula (2017) and Lusardi, Michaud, and Mitchell (2017) posit that improved financial knowledge allows individuals to use sophisticated, information intensive financial products (such as stocks), which enable them to earn higher returns on their investments. Our causal evidence supports the model predictions of Lusardi, Michaud, and Mitchell (2017) and Jappelli and Padula (2017) in that financial knowledge acquired through business education generates higher portfolio returns and alters the life cycle wealth profiles of individuals. Hence, our findings can be relevant for the ongoing discussion on the policy tools to regulate wealth inequality (e.g., Calvet et al. 2020; Guvenen et al. 2019; Stiglitz 2015), indicating that financial literacy education can partly contribute to contain wealth inequality.

Second, we contribute to the current discussion on the effectiveness of financial literacy education to empower households to make better financial decisions (e.g., Campbell 2016; Kaiser et al. 2021). Specifically, there is an ongoing debate as to whether financial education is an effective policy tool that can improve economic choices of households (Campbell 2016; Fernandes, Lynch Jr, and Netemeyer 2014; Kaiser et al. 2021), partly due to the lack of well-identified evidence on the causal effects of financial education on household decision making.[5] In a closely related paper, Hvidberg (2021), who uses a regression discontinuity design and university admission data from Denmark, documents that business education significantly lowers the probability of experiencing financial distress over the medium term. Even though the treatment, i.e., enrolling in a business program at the post-secondary level, in both papers represents (potentially) an upper bound for financial literacy education, our findings indicate that such policy interventions could have the potential to improve financial choices and outcomes of households along with their debt behavior.[6]

Our paper also links to the recent literature on the role of education in 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 also contributes to the literature on returns to education, which

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

[6]We acknowledge that our analysis is currently silent on the cost effectiveness of financial education, as we have so far focused only on its potential benefits without considering its costs.

typically focuses on the effects of college education and college major on labor market prospects and outcomes of individuals (some examples are Acemoglu, He, and le Maire 2022; Altonji, Arcidiacono, and Maurel 2016; Altonji, Blom, and Meghir 2012; d'Astous and Shore 2021; 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 the earnings of individuals in the U.S, and identify returns to business majors of being approximately 80–130% over the medium term, i.e., more than 12 years after initial enrollment. We document that the causal effect of majoring in business extends beyond the labor market to financial behavior and wealth accumulation of households. An early paper focusing on the financial behavior of individuals with economics education is Christiansen, Joensen, and Rangvid (2008) who show that economists are more likely to hold stocks than otherwise identical investors. Our paper differs from this study in many dimensions, including empirical identification and results. Mainly, we do not find any systematic effect on stock market participation from studying business.[7]

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

2 Institutional Details and Data

In this section, we first provide information about the Swedish 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, government run. All students are offered stipends and subsidized study-loans. Similar to many other European countries, individuals apply by submitting a preference ranking of programs at specific institutions where they would like to study. Each such alternative covers a particular field of study and, if completed, awards the

[7]We acknowledge that the discrepancy in the stockholding results could be due to sample differences. Specifically, our estimates are local average treatment effects on applicants who have a revealed preference to study business, and thus likely a larger-than-average interest in personal finances.

student with a field-specific degree. When a program is oversubscribed, students are admitted by academic performance.

To be eligible for post-secondary education, applicants must have completed a university-preparatory high school program. Individuals from other programs, or who have not taken the required courses, can supplement their high school diplomas with preparatory adult education to become eligible. University programs start either in fall or spring semesters and applications are made separately to each semester. Applicants submit ordered lists of up to 12 program-institution combinations, below 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 (but during a transition between two high school grading systems, separate groups are used for each grading system). There is another group for admission through Högskoleprovet (a standardized admission exam similar to the SAT). Finally, applicants who have prior work experience can compete in a separate group where their work experience is awarded with bonus points on top of the high school GPA. Note that applicants in each group are separately ranked based on their group-specific scores, and number of available spots for different admission groups is proportional to the total number of eligible applicants in each of those groups. To make admission scores more easily comparable across groups, we standardize the scores of applicants separately for each group and year. In all our regressions, we include admission cutoff fixed-effects and separate running variable polynomials for each admission group.

Each application period consists of two rounds.[8] During each round applicants are offered admission to their highest ranked program for which they are above the admission cutoff, while lower ranked alternatives are automatically withdrawn. The applicants can choose to stay on a waiting list for any higher ranked program to which they applied but were not yet admitted. Note that the offer from the first round is withdrawn if the applicants on the waiting list are admitted to a higher ranked alternative in the second round.

The admission allocation mechanism can be described as a truncated multi-category serial dictatorship. Because of application list truncation, it is not strategy proof. Moreover, when multiple applicants have the exact same score but there are not enough spots to admit them all, tie-breaking mechanisms are applied. These include lotteries and gender-based priorities, and, for most programs in the period from 1977 to 1995, a priority for the applicants who ranked the alternative in question the highest in their preference lists. Such

[8]After the second round, a third admission round can take place locally at each university, where students who are just below the cutoff at the end of the second round can be offered admission when other admits do not show up. We do not have any data for this process. Admission status and cutoffs are therefore calculated based on round two outcomes. Being admitted to a higher ranked program in the third round does not cancel offers made in the second round.

allocation mechanisms introduce some risk for 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.[9] Looking at a pair of preferred and next-best alternative in an application, there is however no reason for the applicant to hold a preference ordering that is different from the specific ranking.

We focus on applications to the Swedish universities made between 1977 and 1995 through the centralized application system.[10] The university application data come from the Swedish National Archives, particularly the A1 (that covers the period from 1977 to 1992) and H97 (that covers the period from 1993 to 2005) systems.[11] This dataset provides detailed information on the university applications of prospective students made through the centralized system.

In addition to the university application data, we make use of the Swedish Income and Wealth Registry that was compiled by Statistics Sweden (SCB) using data on wealth taxation. The wealth tax was abolished in 2007, but the registry contains highly detailed information on household wealth of every individual residing in Sweden between 1999 and 2007. The wealth information is also highly accurate, since banks and financial institutions reported all asset holdings to the tax authorities directly. Specifically, the dataset provides information on worldwide assets, disaggregated at the individual security or property level, held by residents as of December 31 of each year.[12]

We match these two datasets, using pseudonomized social security numbers. From SCB we also get detailed information about the demographics and labor market characteristics of all individuals residing in Sweden. The demographic data contains variables such as university enrollment and degree completion, high-school performance, gender, age, marital status, labor income, employment status as well as information about family ties - making it possible to measure the characteristics of applicants' parents.

[9]This matters the most for extremely selective programs like medicine. For some years, medical programs only admitted students with perfect GPA, meaning all admitted students were subject to tie-breaking. When ties were broken by how the applicants had ranked the alternative, this led to admission of only those who had the alternative as their first option. In such situations, incentives to include a safe option increases. For business programs during the period, admission cutoffs were almost never at perfect score levels, however.

[10]It became mandatory for institutions to offer their programs through the centralized system only in 2005. While most universities participated from the start of our sample period in 1977, additional schools joined over time or only included a subset of their offered programs.

[11]Note that no data is available for the fall semester of 1992 when the newer admission system was introduced.

[12]The Swedish Income and Wealth Registry has been fruitfully used in earlier research for different purposes. See, e.g., Calvet, Campbell, and Sodini 2007, Calvet, Campbell, and Sodini 2009, Betermier, Calvet, and Sodini 2017, and Bach, Calvet, and Sodini 2020 for a detailed description of the dataset.

2.2 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 specific alternative in a given application period. Note that cutoffs are only defined for admissions if there are both admitted and not admitted applicants at the end of the application round. We exclude from the sample those applicants who were admitted in non-standard admission groups or institutions that offer practical programs. This includes admission to programs that select based on previous university credits and those readmitted after military service.[13]

We use the admission status and scores of applicants from the second admission round, while we consider the preference ranking of alternatives that they submitted for the first round. The reason is that changes to the priority ranking after the initial admission round (withdrawing from a higher ranked alternative that one was not admitted to) can be influenced by the outcome of the initial allocation. Econometrically speaking, if not accounted for, such selection could lead to biased estimates. Since applicants who end up below the cutoff often do decide to drop out of the waiting list, many applicants who stay on the waiting list end up being admitted. Using the cutoffs from the first round would therefore imply that many applicants who end up being admitted are wrongly predicted to be below the cutoff. Using second round cutoffs with first-round rankings, on the other hand, protects against manipulation while ensuring an adequate first stage.

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

To identify the correct counterfactual alternative we then drop dominated alternatives. These are program-institution combinations that individuals apply to but where they have ranked other alternatives higher that end up having lower cutoffs. In such cases, if the applicants are above the cutoff to the alternatives, they are also above the cutoffs to the higher ranked alternatives, ensuring that the applicants will never be admitted to the lower ranked alternatives.

Finally, we collapse applications by field of study and look only at those cases where consecutively ranked alternatives in the individual preference list are in different fields. For example, if an applicant first ranks two business programs, then three medicine programs, and finally a 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

[13] A subset of applicants get conscripted for military service each year and are allowed to postpone the start of their studies if admitted. They need to reapply after completing their service but are then guaranteed admission through a special admission group.

applicant performed the best (they had the highest score relative to the cutoff). We proceed to create observations of pairs of preferred (j) and counterfactual (k) fields. Since we are interested in understanding the causal effects of business education on portfolio returns and financial behavior of households, we restrict our sample only to those applications where the preferred alternative j is a business program and the counterfactual choice k is a non-business program. The programs could be offered at the same 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.[14] Programs in all other fields are defined as non-business. We then contrast the short and medium term financial choices and outcomes of applicants in a business degree program who are marginally above the admission cut-off (the treatment group) with those who are marginally below the cutoff (the control group).

Finally, we complement the application data with information from the Swedish Income and Wealth Registry about wealth outcomes, financial and real investments during the 1999-2007 period, as well as with data from SCB on other individual demographic characteristics of the applicants and of their parents. Overall, the final sample comprises 35,111 unique applicants who are observed at least once during 1999-2007, which results in more than 300,000 applicant-year observations.[15]

Table 1 reports summary statistics for the full sample and for a sample breakdown based on whether the applicant was above or below the admission cutoff. As reported in Panel A, we observe that 45% of the applicants in our sample are female and the average age at the time of application is 21.6. As noted earlier, we are able to link the sampled applicants to their parents, and observe their parents' educational background. For example, we see that for 24% of the applicants at least one of the parents had some business education, which highlights the relevance of parental education for their children's college major choice. Columns (3)-(6) show that applicants who are above and below the admission cutoff points resemble each other in terms of age at application, gender, and parental background.

In Panel B of of Table 1, we note that the mean standardized score of individuals who are above the admission cutoff is significantly higher than those in the control group. Not surprisingly, we also observe that enrollment in a business degree program as well as business degree completion rates are higher among individuals in the treatment group. Finally, Panel C reports descriptive statistics for financial investments and wealth outcomes of applicants 4 to 25 years after their application. To limit the effect of potential outliers, we winsorize all the wealth measures at the first and ninety-ninth percentile; i.e., we set all observations that fall beyond these tolerances to the first and ninety-ninth percentile

[14] We use the SUN classification codes 340-345, 349, and 526 when identifying business-related programs.

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

Table 1: Summary Statistics

Panel A: Individual-level Characteristics						
	Full Sample		Above Cutoff		Below Cutoff	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.453	0.498	0.448	0.497	0.458	0.498
Birth year	1966.830	4.972	1966.406	5.234	1967.245	4.663
Age at Application	21.670	2.328	21.911	2.501	21.433	2.119
Foreign parent	0.061	0.238	0.055	0.229	0.066	0.248
Business-educated Parent	0.241	0.428	0.239	0.426	0.244	0.429
Panel B: Education-related Variables						
	Full Sample		Above Cutoff		Below Cutoff	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Score	0.749	0.814	1.135	0.699	0.371	0.739
Application Year	1988.500	4.422	1988.318	4.581	1988.679	4.252
Enrolled in Business	0.502	0.500	0.771	0.420	0.239	0.426
Enrolled in any Program	0.798	0.401	0.862	0.344	0.735	0.441
Degree in Business	0.307	0.461	0.478	0.500	0.139	0.346
Degree in any Program	0.662	0.473	0.703	0.457	0.622	0.485
Panel C: Outcome Variables						
	Full Sample		Above Cutoff		Below Cutoff	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Stock Market Participation	0.835	0.371	0.838	0.369	0.833	0.373
Stock Investments (in 1000s)	255.999	500.221	276.672	526.980	235.762	471.685
Stock Portfolio Returns	0.011	0.0391	0.011	0.0388	0.011	0.0394
Financial Wealth (in 1000s)	494.457	800.814	535.583	845.643	454.198	752.182
Net Wealth (in 1000s)	1279.259	2046.077	1375.755	2137.871	1184.797	1947.422
Net Wealth Rank	56.533	34.124	57.648	34.251	55.443	33.963

Notes: This table presents summary statistics for the variables employed in the empirical analysis for the applicants who are within a bandwidth of 2 standard deviations. Panel A reports the mean and standard deviation of the individual characteristics for the full sample and for a sample breakdown based on whether the applicant was above or below the admission cutoff. Panel B and C reports summary statistics for education-related variables, and financial investments and wealth outcomes of applicants 4 to 25 years after their applications, respectively.

values, respectively. Even though we observe virtually no differences in stock market participation rates, we observe that individuals in our treatment group accumulate on average higher levels of financial and net wealth later on in their lives than those in the control group, suggesting that there is a positive association between business education and wealth levels.

3 Empirical Strategy

To formally investigate the effect of business education on household financial behavior, we employ a regression discontinuity design (RDD) estimation strategy. Under fairly weak assumptions, RDD enables us to pin down the causal effects of business education on household economic behavior (Lee and Lemieux 2010).

As described in section 2.2 we consider applicants who all want to study business at the university level and have a program in a non-business field as their next-best alternative. We compare the financial choices and outcomes of those applicants who are marginally above the admission cutoff with those who are slightly below the cutoff. By construction, the allocation of business education to individuals can be considered quasi-random among these applicants. We exploit a large set of such cutoffs for different programs at specific institutions over multiple years. Our empirical strategy can be seen as pooling of a large set of "natural experiments" of admission to business education with fixed effects for each such experiment.

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

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

where y_{iT} is the outcome of interest for applicant i in year T where $T \in \{1999, \ldots, 2007\}$. These outcomes are, in turn, stock market participation, stock market investments, and returns on risky assets. Since the financial behavior considered is relevant for generating wealth, we also consider wealth outcomes at the household level such as the level of financial and net wealth and 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 individual characteristics that include an indicator variable for applicant gender and dummy variables for age at application. To control for macroeconomic and other time varying aggregate factors, we include further time effects: for the observation year of the outcome variable, τ_T, and for the number of years since application, τ_t. Since we pool all individual observations and include fixed effects for t and T as well as for age at the application, our estimates should be interpreted as weighted

average of the causal effect of business education on household outcomes measured 4–25 years after application during the 1999–2007 period. Finally, μ_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.

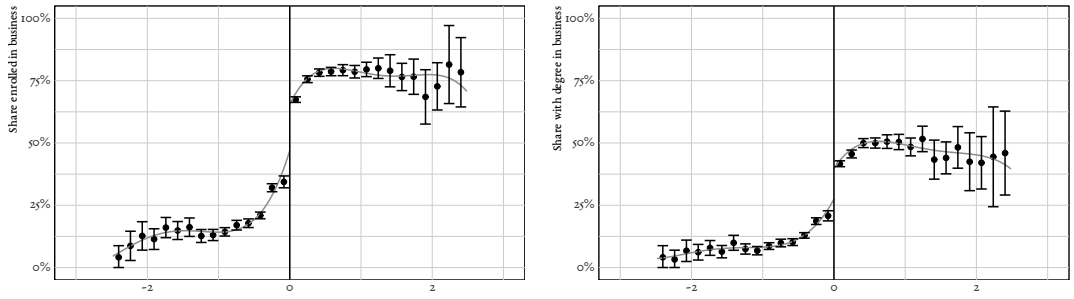
To pin down the causal effects of business education on portfolio returns and financial behavior of households, we use a "fuzzy" design and instrument enrollment in a business degree program within five years from the application by whether the applicant is above the admission cutoff or not. More formally, our regressions take the following form:

$$y_{iT} = \beta \cdot Enrolled_{it_0} + f(a_{ic}, \theta^{\alpha}) + \gamma \cdot X_i + \tau_t + \tau_T + \mu_c + \varepsilon_{iT} \tag{2}$$

$$Enrolled_{it_0} = \pi \cdot \mathbf{1}(a_{ic} \geq 0) + f(a_{ic}, \theta^{\alpha}) + \eta_1 \cdot X_i + \eta_{2t} + \eta_{3T} + \eta_{4c} + u_{it} \tag{3}$$

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

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.

Since the sampled applicants, by construction, all prefer to study business relative over their next-best non-business alternative, there is likely a group of always-takers who will re-apply and enroll in a business program at a later time. As rankings should approximately reflect true relative preferences of applicants, the monotonicity assumption should hold. The reason is that no individual becomes less inclined to enroll in a business program when they are above the cutoff. Hence, 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 actually contain negatively weighted always-takers. Since assignment is quasi-random for each cutoff, including cutoff fixed effects ensures the instrument is exogenous, and thus that the model is saturated.

To increase interpretability of our results, we also report results where we use a different treatment, that is, an indicator variable for completion of a business program within 8 years of application.[16]

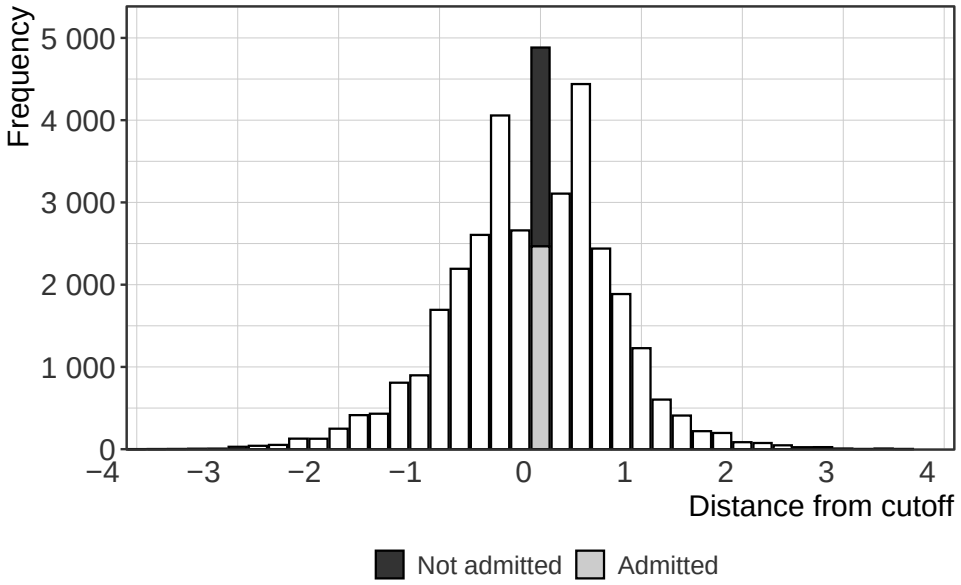
Figure 1 illustrates a clear jump in the probability of both enrolling in a business program and earning a business degree around the admission cutoff. These results are also confirmed by the formal econometric analysis, as reported in Table O.A.1 of the online appendix. Specifically, we estimate equation 3 and regress being enrolled or having a degree in business on an indicator variable of being above the cutoff and fixed effects for each cutoff. Being above the cutoff significantly increases the probability to enroll in a business program within 5 years by 54.1 percentage points.

For RDD to properly identify a causal treatment effect it should not be possible to precisely manipulate assignment around the cutoff. Since admission cutoffs change each year, depending on the scores of all applicants, an individual has no way of knowing if they will be admitted ex ante, making such manipulation unlikely. We present two figures to corroborate that this identifying assumption holds. Figure 2 shows that the running variable is evenly distributed around the cutoff, and Figure 3 further shows that pretermined 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, since our analysis is pooling 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 results are not sensitive to this choice - while a smaller sample does of course decrease statistical power, changing the bandwidth has little impact on the point estimates.

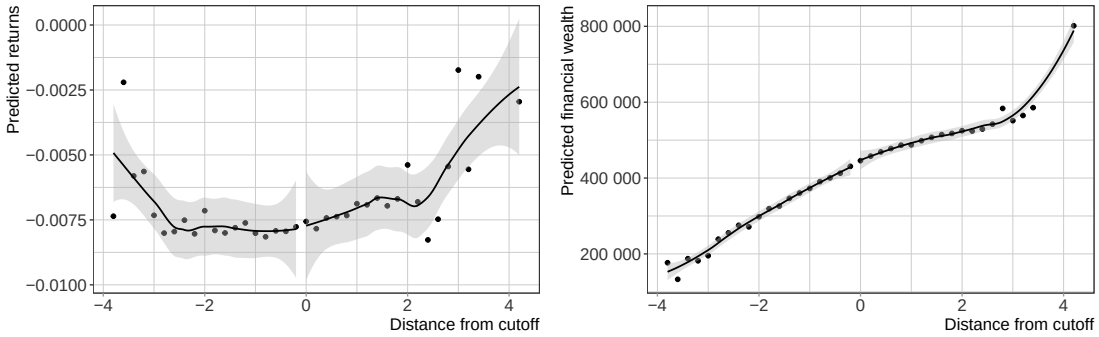
[16]Note that the results of the analysis with having a business degree (rather than enrolling in a business program) should be interpreted with caution. This specification may not fulfill the exclusion restriction and the estimates could be biased, since being above the cutoff is also likely to affect household financial behavior in other ways than through degree completion. For example, admitted students might take classes in finance and then drop out.

Figure 2: Distribution of Admission Scores around the Cutoff



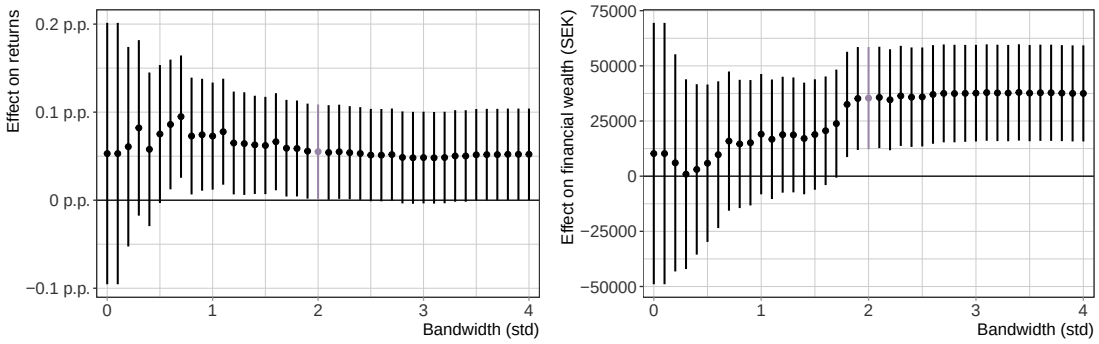
Notes: This figure illustrates a histogram of the distribution of observations around the admission cutoff. Observations exactly at the cutoff are sorted in a separate bar. These individuals are admitted using different tie-breaking mechanisms, and are counted in the analysis as either above or below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



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 predicted the results of some of the main results 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 even though we observe that, not surprisingly, a smaller sample leads to more noise in the estimates.

4 Household Financial Behavior: Stock Investments and Portfolio Returns

In this section, we first investigate the impact of majoring in business on portfolio returns and financial outcomes of households. We next focus on its effects on household financial outcomes over time. As we interpret the effect of enrolling in a business-related program as going through improved financial knowledge of treated individuals, we then subject this interpretation to various sensitivity tests and verify its robustness to alternative explanation. Finally, we analyze whether business education complements or substitutes the intergenerational transmission of financial knowledge.

4.1 Base Results

We begin our analysis by estimating the causal effects of enrolling in a business-related program, which we interpret as an investment in financial knowledge (Almenberg and Säve-Söderbergh 2011; Fagereng et al. 2020), on different dimensions of household financial behavior.

Table 2 presents the regression results for household financial behavior. For brevity, we only report the coefficient estimates for the business education variable. In all regressions, we account for linear polynomials of the running variables, individual control variables and a battery of fixed effects, including fixed effects for each admission cutoff, the number of years since application, the age at time of application, and the year of observation when household financial behavior is observed.

As a prelude to our instrumental variable estimates, Panel A of Table 2 reports the reduced-form regressions as shown in Equation 1. Panel B, which is our preferred

specification, and Panel C present the second-stage estimates from the IV regressions as outlined in Equation 2 where we instrument enrollment in a business program and obtaining a business degree with being above the cutoff at the time of admission, respectively.

In column (1) of Table 2, we first estimate the causal effects of business education on the likelihood of households to participate in the stock market. The dependent variable is an indicator variable of whether the household invests in the stock market, either directly or indirectly through mutual funds, excluding investments through retirement accounts.[17] As shown in Panel B, the coefficient on enrolling in a business program is estimated to be positive, but it is neither statistically nor economically significant at any conventional level. The results remain the same when we use different definitions of treatment for business education. In fact, the absence of a significant impact of business education on stock market participation decisions of households in our setting is not surprising mainly because stockholding is quite pervasive in Sweden, particularly among those with some college education. Moreover, our sample of applicants include only those who have a preference for studying business, and hence, are likely to have larger-than-average interest in financial matters.

In column (2) of Table 2, we focus on the intensive margin of financial risk taking where we use the Swedish Krona (SEK) value of direct and indirect stock investments as the outcome variable. We find that business educated households have a larger exposure to the stock market as they invest significantly more in stocks conditional on participating in the stock market. Specifically, households with some business education invest approximately 48,600 SEK more in stocks than their non-business educated peers (t-stat. $= 2.41$). To put this estimate into context, the estimated effect of business education corresponds to an approximately 19 percent increase in the mean stock wealth of the sampled households, which is quite sizeable. In additional analysis presented in Table O.A.2 of the online appendix, we consider investments in single stocks as the outcome variable, and find similar results. Specifically, we document that majoring in business increases the direct stock investments by approximately 30,000 SEK, which makes up more than 60% (=30,000/48,600) of its overall contribution to the total stock investments. Hence, the effect of business education on increased exposure of households to stock market is mainly operative through its impact on direct stock investments.

Motivated by recent theoretical models that highlights the relevance of investments in financial knowledge in portfolio returns (Lusardi, Michaud, and Mitchell 2017 and Jappelli and Padula 2017), we next ask whether (quasi-randomly) enrolling in a business program affects returns on risky investments of households. To answer this question, we make use of an auxiliary dataset provided by Statistics Sweden, which reports information, detailed

[17]Since the wealth data were collected to assess wealth taxes, stockholding under the mandatory first pillar of social security and in tax-deferred retirement accounts is not included in our data because they were not part of the tax base.

Table 2: Household Financial Behavior and Business Education

Panel A: Reduced Form				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Above_Cutoff	0.002	20077.783**	0.001**	0.001**
	(0.34)	(2.42)	(2.02)	(2.19)
Obs	305,061	260,787	127,443	127,443
Panel B: IV Estimates: Enrollment as Treatment				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	0.004	48604.028**	0.001**	0.001**
	(0.34)	(2.41)	(1.99)	(2.17)
Obs	305,061	260,787	127,443	127,443
Panel C: IV Estimates: Degree as Treatment				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Degree	0.007	71553.147**	0.002**	0.002**
	(0.34)	(2.41)	(1.99)	(2.16)
Obs	305,061	260,787	127,443	127,443
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
Portfolio beta	No	No	Yes	Yes
Portfolio chars	No	No	No	Yes

Notes: This table presents regression estimates of household financial behavior. Panel A reports the reduced-form regressions as shown in Equation 1. Panel B and Panel C present the second-stage estimates from the IV regressions as outlined in Equation 2 where we instrument enrollment in a business program and obtaining a business degree with being above the cutoff at the time of admission, respectively. Stock market participation and stock wealth are measured at the household level. In the portfolio return regressions, we control for (one-year lagged) value of stock portfolio, the interaction of time-year dummies and stock share in financial wealth, and portfolio beta. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

at the security level, on the financial wealth of sampled individuals over the sample period. We combine this information with stock level return data collected from Thomson Reuters Datastream, and calculate the stock portfolio returns of sampled households over the period between 2000 and 2007.[18]

When calculating the portfolio returns, we focus on household investments 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 stock beta, size, book-to-market, and momentum in the return regressions. Second, direct stock investments, as compared to investments in mutual funds, tend to involve no substantial heterogeneous fee or expense structure that can affect returns and overall portfolio performance. Third, our focus on stocks domiciled in Sweden limits any concerns that differences in portfolio performance can result in part from differences in access to international markets or other alternative investment vehicles (e.g., private equity or venture capital investments) across households. Still, we also use an alternative return measure where we focus on rates of returns on overall risky asset portfolio, and verify the robustness of our findings, as presented in Table O.A.3 of the online appendix.

In the portfolio return regressions, following Fagereng et al. (2020), we account for (one-year lagged) value of stock portfolio, and the interaction of time-year dummies and stock share in financial wealth along with other control variables. The former allows us to account for the effects of participation costs and scale effects, while the latter controls for differences in access to menu of financial instruments. Finally, we also include the beta of the stock portfolio in the return regressions to account for differences in risk exposure across households.

The regression estimates are reported in column (3) of Table 2. We document positive and significant effects of business education on returns on stock investments of households with t-statistics ranging from 1.99 to 2.02, depending on the definition of the treatment. Specifically, based on the estimates of our preferred specification presented in Panel B, we find that households who quasi-randomly enroll in a business-related program earn approximately 10 basis points higher returns in a given month on their stock portfolio than their non-business educated peers. This effect translates into an annualized rate of return difference of 1.2 percentage points that is highly meaningful in economic terms and highlights the importance of improved financial sophistication in generating higher portfolio returns.

We recognize that the documented return differences between business and non-business educated individuals can result from differences in their willingness to take more risk, as shown by Bach, Calvet, and Sodini (2020) and Campbell, Ramadorai, and Ranish

[18]Note that we use end-of-period stock holdings in year t, and 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.

(2019). To control for this explanation, the base analysis accounts for portfolio beta and differences in access to menu of financial instruments across sampled individuals. To refine our understanding on the role of risk taking in our results, we next augment our return regressions by including other stock characteristics in the estimation model, such as the average idiosyncratic volatility, size, momentum, and book-to-market of the stock portfolio to better capture the differences in exposure to different sources of (compensated) risk. As presented in column (4) of Table 2, we obtain similar results to what we document in the base analysis.

In addition to differences in risk exposure, heterogeneity in innate ability across households could also contribute to the differences in portfolio returns, if, for example, more skilled and talented individuals sort themselves into business programs (Fagereng et al. 2020). Since by design, we contrast the stock portfolio performance of individuals with similar initial abilities and preferences, we implicitly control for such differences in our empirical analysis. Hence, it seems that heterogeneity in risk exposure or innate ability does not fully explain the positive effects of business education on portfolio returns.[19] Taken as a whole, the causal evidence presented in this section provides direct support for the model predictions of Lusardi, Michaud, and Mitchell (2017) and Jappelli and Padula (2017) that improved financial sophistication generates higher portfolio returns.

4.2 The Effect of Business Education over Time

We next turn to the analysis of the effects of business education on portfolio returns and household financial behavior over time. We do so by exploiting the unique feature of our dataset that allows us to observe the portfolio choices and outcomes of sampled households up to 25 years from the year of university application. We split the sample into two by the median years passed since the university application (i.e., 14 years), and estimate the effect of business education on household financial behavior both over the short run (1-14 years) and medium run (14-25 years).[20]

The regression results are reported in Table 3. First, similar to the base results, we observe no significant effect of enrolling in a business program on stock market participation of households both over the short- and medium-run. Interestingly, we document that majoring in business leads to significantly greater returns on stock investments of households in the short run, while this effect turns out to be insignificant over the medium run. In

[19]We note that a full-fledged analysis to identify the mechanism through which improved financial sophistication affects portfolio returns is beyond the scope of our study, and we remain agnostic about its exact sources. For example, we acknowledge that factors such as better stock-picking or market-timing skills of business educated individuals may also be at play along with other sources.

[20]Since we only have data on portfolio holdings and wealth outcomes of households over the 1999-2007 sample, short-run and medium-run sample includes those households who applied to a business program between 1985-2006 and 1974-1992 periods, respectively.

Table 3: Household Financial Behavior and Business Education over Time

Panel A: Short-term Effects of Business Education				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	0.011	18388.541	0.003**	0.003***
	(0.61)	(0.75)	(2.35)	(2.91)
Obs	136,103	114,054	49,114	49,114
Panel B: Medium-term Effects of Business Education				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	0.0004	68341.712***	0.0003	0.0001
	(0.03)	(2.65)	(0.44)	(0.21)
Obs	168,855	146,604	78,191	78,191
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
Portfolio beta	No	No	Yes	Yes
Portfolio chars	No	No	No	Yes

Notes: This table presents the second stage estimates of household financial behavior regressions where 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 (1-14 years) and medium run (14-25 years), respectively. Stock market participation and stock wealth are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomesare observed each year between 1999 and 2007. In the portfolio return regressions, we control for (one-year lagged) value of stock portfolio, the interaction of time-year dummies and stock share in financial wealth, and portfolio beta. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cut-off and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

particular, majoring in business produces approximately 30 basis points (t-stat. = 2.35) larger average monthly returns on stock portfolio over the short term. By contrast, this effect is both economically and statistically indistinguishable from zero over the medium term.

The observed asymmetric effects of business education on portfolio returns over time can be driven by at least two factors. First, Lusardi, Michaud, and Mitchell (2017) show that the optimal financial literacy profile is hump-shaped over the life cycle. This implies that business education can help households to build up financial knowledge in the short run, allowing them to earn higher returns on their savings. Over time, though, financial literacy, similar to any investments in human capital (e.g., Heckman 1976), may

depreciate, for example, due to cognitive decline (Agarwal et al. 2009) or because the acquired knowledge becomes obsolete with the advent of new financial products (Lusardi, Michaud, and Mitchell 2017). Second, we do not observe any significant differences neither at the extensive nor at the intensive margin of stock investments in the early period. Hence, it is possible that non-business educated households can improve their financial sophistication either directly due to endogenous investments in financial knowledge or indirectly through "learning-by-doing", reducing the initial financial literacy gap over time and earning comparable portfolio returns as the business educated households in the medium term.

Still, we see that early investments in financial knowledge lead to significant differences in stock wealth levels between business educated and non-business educated individuals over the medium term. Specifically, households who quasi-randomly enrolled in a business program end up having 68,300 SEK higher stock wealth than their non-business educated peers 14 to 25 years removed from the year of university application (t-stat. = 2.65). This result highlights the potential wealth effects of business education in the medium term, which we discuss and analyze more in detail in Section 5.

4.3 Robustness to Alternative Interpretations

To this point, supported by our empirical findings, we have interpreted having business education as a direct investment in financial knowledge. In what follows, we further scrutinize this interpretation and examine its robustness to alternative explanations.

First, one may worry that the positive effects of business education on household financial behavior can be driven by level of education rather than its content. For example, recent studies document a positive association between level of education and returns on wealth (Fagereng et al. 2019; Girshina 2019).[21] This alternative could pose a threat for the interpretation of our findings if individuals who were marginally above the admission threshold for a business program have higher university completion rates than those who were just below the admission cutoff. Indeed, we observe in our data that the unconditional probability of earning a college degree within 8 years is significantly higher among individuals who were marginally admitted to business programs than those who were not (0.70 versus 0.62). In analysis presented in Table O.A.4 of the online appendix, we also formally test this issue in a multivariate regression setting and find that, ceteris paribus, being above the admission cutoff significantly increases the probability to earn any college degree within 8 years by 2 percentage points.

[21] To provide 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 level of education, the correlation between educational attainment and returns disappears, which they interpret as the innate wealth management ability of households being the ultimate driver of higher returns to wealth and its components.

To test whether it is the level of education that drives our results, we restrict the sample to those individuals who graduated from university and re-estimate our regressions. This restriction reduces the sample size from 305,061 to 201,906 applicant-year observations. As reported in Panel A of Table 4, reassuringly, we obtain similar results - that is, individuals with business education have significantly larger exposure to stock market and earn higher returns on their investments than their peers without business education in the sample of households with college degree. Specifically, having enrolled in a business program increases the monthly average portfolio returns by approximately 20 basis points (t-stat. = 2.22). This sensitivity analysis suggests that our results are not a pure artifact of potential differences in the level of education across sampled individuals. It is in fact the content of education, particularly, business studies, that leads to improved portfolio decisions. In Table O.A.5 of the online appendix, we verify the robustness of these results by restricting the sample to households who attended but not necessarily graduated from university.[22]

Second, it is possible that business education can affect financial behavior through its effects on the unemployment risk of households. As noted by Fagereng, Guiso, and Pistaferri (2017), unemployment risk represents one of the most important sources of background risk, which can affect household risk taking and portfolio choice (Cocco, Gomes, and Maenhout 2005). If business education leads households to end up having jobs with higher employment security, and hence, lower labor income uncertainty, this could allow them to take on more financial risk and achieve higher returns. As noted by Ameriks, Caplin, and Leahy (2003), willingness of households to take financial risk directly affects returns on investments. Similarly, in a recent paper, d'Astous and Shore (2021) find that increased labor income uncertainty, identified by exogenous variation in university program enrollment, affects stock market participation and portfolio decisions of households.

To test for this explanation, we define an indicator variable for being unemployed that is defined using information about whether the individual received any unemployment benefits in a given year. We then regress this variable on enrollment in a business program, and other household controls and fixed effects. As presented in column (1) of Panel B of Table 4, we find no significant effect of business education on the unemployment risk of households, even though the point estimate is negative (-0.014; t-stat. = -0.68). Next, we consider a related but distinct possibility, that is, whether majoring in business affects the occupational trajectory of individuals measured by their propensity to become entrepreneurs. As presented in Column (2) of Panel B, we again observe no significant effect of having business education on the occupational choice of individuals. Overall, these

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

Table 4: Household Financial Behavior and Business Education: Alternative Explanations

Panel A: University degree-holders only				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	0.014	69149.915***	0.002**	0.002**
	(0.98)	(2.86)	(2.22)	(2.41)
Obs	201,906	176,783	89,678	89,678
Panel B: Effect of business education on labor market conditions				
	Unemployment	Self-employment		
	(1)	(2)		
Enrolled	-0.014	0.003		
	(-0.68)	(0.17)		
Obs	312,426	312,426		
Panel C: Including only quantitative next-best fields				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	-0.010	31435.627	0.002**	0.002***
	(-0.70)	(1.42)	(2.37)	(2.73)
Obs	184,232	159,556	79,240	79,240
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
Portfolio beta	No	No	Yes	Yes
Portfolio chars	No	No	No	Yes

Notes: This table presents the second stage estimates of household financial behavior and labor market regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. In Panel A, we focus only those applicants who complete their university education. Panel C excludes from the sample those applicants who have business education as their preferred choice with the next-best alternative in a non-quantitative field. Stock market participation and stock wealth are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio, wealth, and labor market outcomes are observed each year between 1999 and 2007. In the portfolio return regressions, we control for (one-year lagged) value of stock portfolio, the interaction of time-year dummies and stock share in financial wealth, and portfolio beta. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

results suggest that potential differences in unemployment risk or occupational choices across households do not explain the positive impact of business education on portfolio returns and financial behavior of households.

Finally, an extensive literature documents that quantitative education and cognitive abilities play an important role in household portfolio choice and financial outcomes (Brown et al. 2016; Christelis, Jappelli, and Padula 2010; Cole, Paulson, and Shastry 2014; Grinblatt, Keloharju, and Linnainmaa 2011).[23] Given this background, we analyze whether the positive effects of business education on financial behavior are primarily driven by improved financial knowledge or whether they are due to increased quantitative skills acquired through business education. To do so, we exclude from the sample those applicants who have business education as their preferred choice with the next-best alternative in a non-quantitative field such as humanities, social science, medicine, health, or law. This subset of applicants in the control group allows us to focus on those individuals who ended up being admitted to a program such as science or engineering where they can acquire and improve their quantitative skills if they were marginally below the threshold for a business program.

The estimation results presented in Panel D of Table 4 show significant and positive effects of business education on portfolio returns. In particular, we find that households who quasi-randomly enrolled in a business program earn significantly higher returns on their stock investments when we contrast their portfolio performance with that of individuals who had quantitative training at the postsecondary level. Even though the coefficient estimate on business education is positive in the stock investment regressions, they are not precisely estimated. In short, this result implies that financial knowledge rather than quantitative abilities appears to be key for the positive effects of business education on household financial behavior.

Taken together, the numerous findings presented in this section provide strong support for the interpretation that the estimated positive effects of business education can be, to a large extent, attributed to improved financial knowledge of households acquired through majoring in business rather than alternative explanations such as level of education, quantitative skills, unemployment risk, or career trajectory of households.

4.4 Effects of Business Education by Parental Background

A number of studies document significant intergenerational spillovers in education levels and earnings of individuals (Björklund, Lindahl, and Plug 2006; Black, Devereux, and

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

Salvanes 2005; Majlesi et al. 2019), which play a key role in intergenerational mobility (Black and Devereux 2010). In the context of financial literacy, there is also evidence that highlights the importance of a link between one's own financial sophistication and that of one's parents.[24] Hence, it is important to understand whether investments in financial knowledge through business education complements or substitutes the intergenerational transmission of financial sophistication.

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

Panel A: Parents with Business Education				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	-0.024	57817.407	0.0001	0.0002
	(-0.87)	(1.23)	(0.10)	(0.19)
Obs	72,945	63,035	31,924	31,924
Panel B: Parents without Business Education				
	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	0.018	49574.704**	0.001*	0.001**
	(1.21)	(2.06)	(1.91)	(2.08)
Obs	228,721	195,390	94,454	94,454
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
Portfolio beta	No	No	Yes	Yes
Portfolio chars	No	No	No	Yes

Notes: This table presents the second stage estimates of household financial behavior regressions for a sample breakdown based on whether any of the parents of the sampled households has some business education 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 (1-14 years) and medium run (14-25 years), respectively. Stock market participation and stock wealth are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual 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 5 shows regressions of financial behavior for a sample breakdown based on whether any of the parents of the sampled households has some business education. We again consider three dimensions of financial behavior and portfolio choices that are stock market participation decisions, stock investments, and returns on stock portfolio.

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

Interestingly, we observe that the positive contribution of business education to stock investments and portfolio returns is only operative for those households with financially less literate parents. By contrast, we find no systematic effects of business education on portfolio returns or any other dimension of financial behavior in the sample of households with financially savvy parents. For example, columns (3) and (4) in Panel B of Table 5 show that majoring in business leads to 10 basis points higher monthly returns on stock investments among households with financially less literate parents. However, it is both economically and statistically indistinguishable from zero for households with financially sophisticated parents.

The results from this cross-sectional analysis imply that having business education serves as a substitute for learning from parents, and hence, for intergenerational persistence in financial sophistication. Furthermore, the documented asymmetry in the effects of business education across households by parental financial sophistication provides additional support for the improved financial knowledge interpretation of business education, which we discussed in detail in the previous section.

5 From Financial Behavior to Wealth Accumulation

5.1 Business Education and Household Wealth

Our analysis up to this point implies that improved financial knowledge through quasi-randomly enrolling in a business program leads individuals to increase their exposure to the stock market and to earn significantly higher rates of returns on their stock investments. Building on these results, we next investigate whether the effects of business education extend beyond the financial behavior to the wealth accumulation of households.

Table 6 presents the estimation results for the wealth analysis. In column (1), we first use household financial wealth as the outcome variable, which is defined as the sum of the value of direct and indirect stocks, bonds, bonds and mixed mutual funds, and cash holdings in savings and checking accounts. In column (2), the dependent variable is household net wealth, calculated by subtracting household debt from total household assets (i.e., the sum of all financial and real assets).

Consistent with the empirical findings presented in section 4.1, we document that households with some business education accumulate significantly larger financial and net wealth with t-statistics of 2.98 and 2.88, respectively. The estimated wealth effects of business education are not only statistically significant but they are also meaningful in economic terms. For example, based on the estimates in column (1), enrolling in a business program increases the financial wealth of households by approximately 87,000 SEK, on average. To put this into context, the effect of majoring in business corresponds

Table 6: Household Wealth and Business Education

	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	86692.570***	214430.996***	2.801**
	(2.98)	(2.88)	(2.32)
Obs	305,061	305,061	305,061
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the second stage estimates of household wealth regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. Household wealth variables are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

to an approximately 17.6 percent increase in the mean financial wealth of the sampled households, which is quite sizeable.[25]

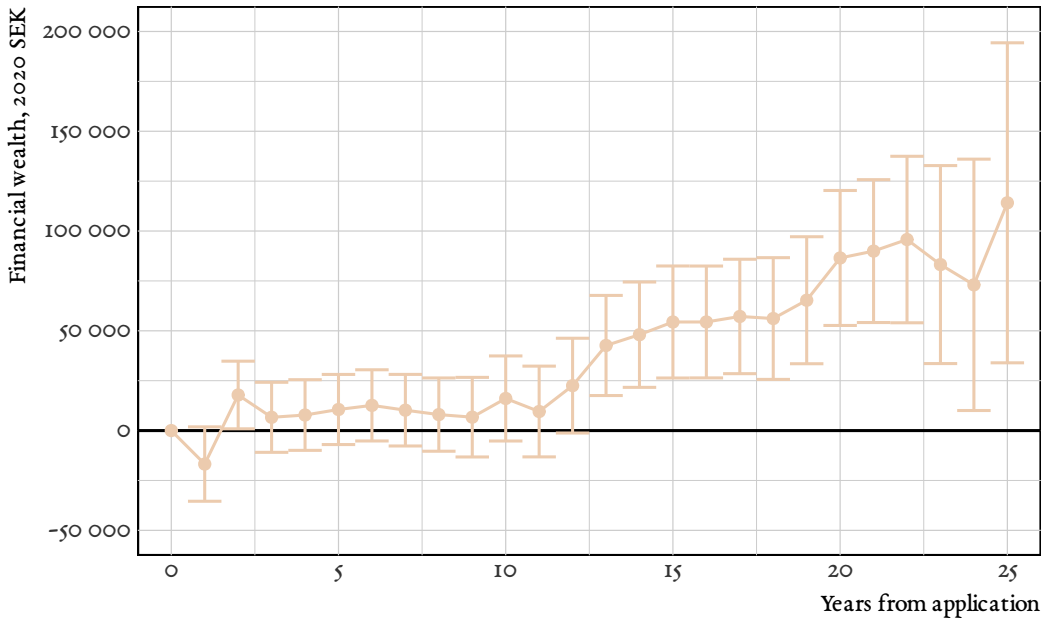
We next turn to the analysis of the evolution of wealth effects of business education over time. To do so, we augment our base regression model, as outlined in Equation 2, by including an interaction term of enrollment in a business program and number of years that has passed since application. Figure 5 illustrates the results. Specifically, the x-axis of Figure 5 reports the number of years that has passed since application to a business program while the y-axis presents the coefficient estimates on having business education interacted with each of these years (up to year 25) separately from the financial wealth analysis.[26]

As can be seen in the figure, majoring in business does not significantly affect household financial wealth within the first 13 years after the application. The lack of any systematic

[25]In addition, we estimate the causal effect of business education on the relative position of individuals in the wealth distribution, measured by their percentile rank in the net wealth distribution (see column (3) of Table 6). This variable allows us to focus on the relative rank of business educated households in comparison to their non-business educated peers within the overall wealth distribution (Black et al. 2020). We find that individuals with business education attain significantly higher positions in the wealth distribution, on average, than those without business education (t-stat. = 2.32).

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

Figure 5: Household Financial Wealth and Business Education over Time



Notes: This figure illustrates evolution of wealth effects of business education over time. Specifically, we augment our base regression model, as outlined in Equation 2, by including an interaction term of enrollment in a business program and number of years that has passed since application, and present the estimated coefficients along with their confidence banks over time. The x-axis reports the number of years that has passed since application to a business program while the y-axis presents the coefficient estimates on having business education interacted with each of these years (up to year 25) separately from the financial wealth analysis. Financial wealth is measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007.

relation between having business education and household wealth accumulation over the short term is, to some extent, not unexpected, mainly because wealth is a stock variable, and the differences in wealth levels should thus get larger over time. Consistent with this notion, we indeed find evidence of a significant positive causal link over the medium term. For example, households who enrolled in a business program accumulate approximately 50,000 SEK higher financial wealth as compared to their peers with a non-business education 15 years after their application to college. More importantly, the differences in financial wealth levels between households with business and non-business educational majors monotonically increases up to 20 years.[27] In Figure O.A.1 of the online appendix, we repeat the same exercise using household net wealth and document a very similar pattern.

Our results imply that early investments in financial sophistication alters the life cycle wealth profiles, and individuals with similar initial characteristics in terms of preferences

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

and abilities end up accumulating significantly different levels of wealth later in their lives. Overall, these results together with evidence presented in Section 4.1 provide direct empirical support for the theory model of Lusardi, Michaud, and Mitchell (2017) who formally show that differences in financial sophistication generate large differences in household wealth through their effects on portfolio returns.

5.2 Alternative Channels of Influence

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

Labor Market Outcomes

A powerful alternative mechanism underlying the positive effects of business education on household wealth is the labor market channel. A growing literature shows that there is considerable heterogeneity in labor market returns to different college majors with the differences in effect sizes being even as high as the overall payoff of having a college degree (e.g., Altonji, Blom, and Meghir 2012; Hastings, Neilson, and Zimmerman 2014; Kirkebøen, Leuven, and Mogstad 2016). For example, Kirkebøen, Leuven, and Mogstad (2016) document that business education leads to significantly higher earnings early in the working life than social sciences or humanities, while they find no significant differences in earnings between business and medicine, or technology, or law. Hence, our wealth results may simply be an extension of the well-documented effects of business education on earnings to the wealth accumulation of households. We address this explanation in the following way.

Following Kirkebøen, Leuven, and Mogstad (2016), we first quantify the relative labor market payoffs of business education relative to alternative educational majors among households with some college education.[28] As presented in Table O.A.6, we find no significant effects of business education on earnings of individuals relative to science, law, other, medicine, humanities and health, while business education leads to greater earnings as compared to social science, technology and teaching.[29] We then restrict the sample to those individuals whose next-best alternative college major leads to similar earnings levels as business education to mute the labor income channel, and re-estimate the wealth regressions. In principle, this sample restriction allows us to fix the differences in labor income levels across households with business and non-business education. Hence, we are

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

[29]We acknowledge that some of the earnings estimates should be interpreted with caution. For example, no (statistically) significant differences in earnings between business and humanities can be partly attributed to relatively smaller sample sizes and lack of variation in those subsamples.

Table 7: Household Wealth and Business Education: The Role of Earnings

	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	198403.779***	492919.272***	6.686**
	(2.78)	(2.62)	(2.54)
Obs	79,022	79,022	79,022
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the second stage estimates of household wealth regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. In this analysis, we restrict the sample to those individuals whose next-best alternative college major leads to similar earnings levels as business education. Household wealth variables are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. In the portfolio return regressions, we control for (one-year lagged) value of stock portfolio, the interaction of time-year dummies and stock share in financial wealth, and portfolio beta. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

able to isolate the wealth effects of business education that operate through channels other than the labor income channel.

Table 7 reports the coefficient estimates for the wealth regressions for this subsample of households. We document that enrolling in a business program still exhibits positive significant effects on household wealth even when we contrast households with similar levels of expected labor income after graduation. For example, majoring in business leads to 198,403 SEK greater financial wealth once we fix the differences in expected labor income levels across households. Note that we do not regard this finding as proof of the irrelevance of the income channel for wealth accumulation. We rather view our findings as consistent with labor market effects not being the main driver of differences in the wealth levels between treatment and control groups.

Household Debt

We next examine the possibility of whether the wealth effects of business education operate through its impact on the liability side of household balance sheets. For example,

Hvidberg (2021) uses an identification strategy similar to that of our paper, and studies the effect of business education on the debt behavior of individuals in Denmark. The author documents that business educated individuals have significantly lower tendency of experiencing financial distress, which, as the author argues, is primarily due to improved financial behavior rather than their labor market outcomes and prospects. To address this explanation, we partition household net wealth into its two broad components, gross assets and total liabilities, and rerun our regressions.

Table 8: Business Education and Household Assets and Debt

	Total Assets	Total Liabilities	Household Leverage
	(1)	(2)	(3)
Enrolled	256443.631***	30664.444	-0.0548
	(2.99)	(0.89)	(-0.03)
Obs	305,061	305,061	284,166
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the second stage estimates of household assets and debt regressions where we instrument enrollment in a business program with being above the cutoff at the time of admission. Household assets and debt variables are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

Columns (1) and (2) of Table 8 present the regression results for gross household assets and liabilities, respectively. We find positive and significant effects of enrolling in a business degree program on household assets, while we observe no systematic effects on the level of household liabilities. For example, our estimates show that enrolling in a business program leads households to attain 256,443 SEK higher total asset holdings (t-stat. = 2.99). In the debt regressions, however, the effect is not precisely estimated with a t-statistics of 0.89. These findings indicate that business education affects household net wealth primarily through its impact on the asset side of the household balance sheet. In additional tests, presented in column (3), we verify this finding using alternative definitions of household liabilities such as household leverage measured as the total household debt normalized by annual labor income. Overall, the results presented in this section reveals that business education leads to greater wealth levels mainly through its effects of household assets.

Homeownership and Housing Investments

For most households, housing represents the primary saving instrument, and high returns to housing, especially when purchased with leverage, can significantly contribute to the wealth accumulation of households (Happel et al. 2022). Given this background, we finally investigate the role of housing tenure decisions of individuals for the wealth effects of business education. In Table O.A.7 of the online appendix, we first estimate the effects of having business education on homeownership decisions of individuals and find no significant effect (t-stat. = -0.35). Then, we split the sample by homeownership status of individuals and re-run the wealth regressions.

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

Panel A: Homeowner Households			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	90546.608**	295973.120***	2.793**
	(2.55)	(3.28)	(2.40)
Obs	218,765	218,765	218,765
Panel B: Renter Household			
	Financial Wealth	Net Wealth	Net Wealth Rank
	(1)	(2)	(3)
Enrolled	98538.467***	103651.779	4.647**
	(2.72)	(1.58)	(2.44)
Obs	86,124	86,124	86,124
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the second stage estimates of household wealth regressions for a sample breakdown based on whether the sampled household is a homeowner or renter 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 for homeowner and renter households, respectively. Household wealth variables are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

The regression results are reported in Table 9. We see that enrolling in a business

degree program leads to greater household financial and net wealth regardless of whether the individual is a homeowner or renter. Interestingly, the economic magnitude of business education on financial wealth is slightly larger among renters than homeowners, while the overall net wealth effects of business education is greater among homeowners. Hence, we conclude that positive contribution of business education to household wealth is not a mere outcome of differences in housing investments across business and non-business educated households.

6 Conclusions

In this paper, we provide causal evidence that improved financial knowledge acquired through formal business education positively contributes both to portfolio returns and wealth outcomes of households. Using exogenous variation generated by university-program admission thresholds, we document that early investments in financial knowledge in the form of enrolling in a business-related program leads individuals to invest more in the stock market, but also to earn significantly higher rates of returns on their stock investments. The effects of majoring in business are also meaningful in economic terms. For example, individuals who quasi-randomly enrolled in a business program earn 10 basis points higher monthly raw returns on their stock portfolio than their non-business educated peers, which translates into an annualized rate of return difference of 1.2 percentage points. Further analysis reveals that heterogeneity in risk exposure or innate ability across households does not fully explain the documented positive effects of enrolling in a business program on portfolio returns. Hence, we conclude that improved financial knowledge through business education is likely to play a distinct role in earning higher portfolio returns.

We also subject our argument, that business education reflects direct investments in financial knowledge, to various tests and examine possible alternatives to this interpretation that could generate similar effects. Overall, the sensitivity checks imply that it is in fact the acquired financial knowledge that leads to improved portfolio decisions rather than alternative interpretations such as potential differences in level of education, quantitative skills, or labor market outcomes and career trajectories across individuals.

The effects of business education extend beyond the portfolio choice and outcomes to household wealth accumulation. We find that individuals with business education accumulate significantly higher financial and net wealth later in their lives. In particular, enrolling in a business degree program leads to an increase of 87,000 Swedish Krona in financial wealth. To put this into context, the effect of business education corresponds to an approximately 18 percent increase in the mean financial wealth of the sampled households, which is quite sizeable. We also investigate alternative mechanisms, such as labor market, debt behavior, or housing investments of households, that can affect wealth through channels other than financial behavior. We find that the positive wealth effects of

business education cannot be fully attributed to any of these alternative mechanisms.

In summary, our results indicate that early investments in financial knowledge alters the life cycle wealth profiles, and individuals with similar initial characteristics in terms of abilities and preferences end up accumulating significantly different levels of of wealth later in their lives.

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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	Degree
	(1)	(2)
Above_Cutoff	0.541***	0.358***
	(67.93)	(38.53)
Obs	34,342	34,342
FE: cutoff	Yes	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

	Direct Stock Investments		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	12348.460**	29892.988**	44007.409**
	(2.27)	(2.26)	(2.26)
Obs	260,787	260,787	260,787
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the estimates of household stock investment regressions. Column (1) reports the reduced-form regressions as shown in Equation 1. Column (2) and (3) 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 wealth is measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual 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.3: Business Education and Portfolio Returns: Returns on Risky Assets

	Returns on Risky Assets		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	0.004**	0.010**	0.014**
	(2.15)	(2.15)	(2.15)
Obs	166,540	166,540	166,540
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the estimates of risky investment return regressions. Column (1) reports the reduced-form regressions as shown in Equation 1. Column (2) and (3) 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. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual 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.4: Business Education and College Attendance/Graduation

	Enrolled in any Program	Degree from any Program
	(1)	(2)
Above_Cutoff	0.065***	0.019**
	(10.08)	(2.42)
Obs	312,426	312,426
FE: time	Yes	Yes
FE: data year	Yes	Yes
FE: cutoff	Yes	Yes
FE: age	Yes	Yes
FE: female	Yes	Yes

Notes: This table presents results of analysis where we regress being enrolled in any program and having any college degree on being above the admission cutoff, respectively. Both regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are clustered at the cutoff and individual level, and corresponding t-statistics are reported in parentheses. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respective

Table O.A.5: Household Financial Behavior and Business Education: Is it Level of Education? Households with some College Education

	Participation	Stock Investments	Portfolio Returns	Portfolio Returns
	(1)	(2)	(3)	(4)
Enrolled	0.006	37123.809*	0.001*	0.001*
	(0.47)	(1.69)	(1.80)	(1.92)
Obs	243,483	209,078	105,369	105,369
FE: time	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes
Portfolio chars	No	No	No	Yes

Notes: This table presents the second stage estimates of household financial behavior where where we instrument enrollment in a business program with being above the cutoff at the time of admission. We restrict the sample to those households with some college education. Stock market participation and stock wealth are measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual 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.6: Labor Market Payoffs of Business Education relative to Different Fields of Study

	Science	Medicine	Health	Humanities	Law	Other	Social Science	Teaching	Technology	All Insignificant
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enrolled	-84660.357	-1250737.041	34570.421	116390.147	38360.246	-1451.504	77859.828*	98023.172***	35981.122*	43165.174
	(-0.24)	(-1.16)	(0.28)	(0.87)	(0.53)	(-0.02)	(1.65)	(3.43)	(1.84)	(0.92)
Obs	10417	1452	8295	9201	40266	7190	33526	22420	102242	76829
FE: time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

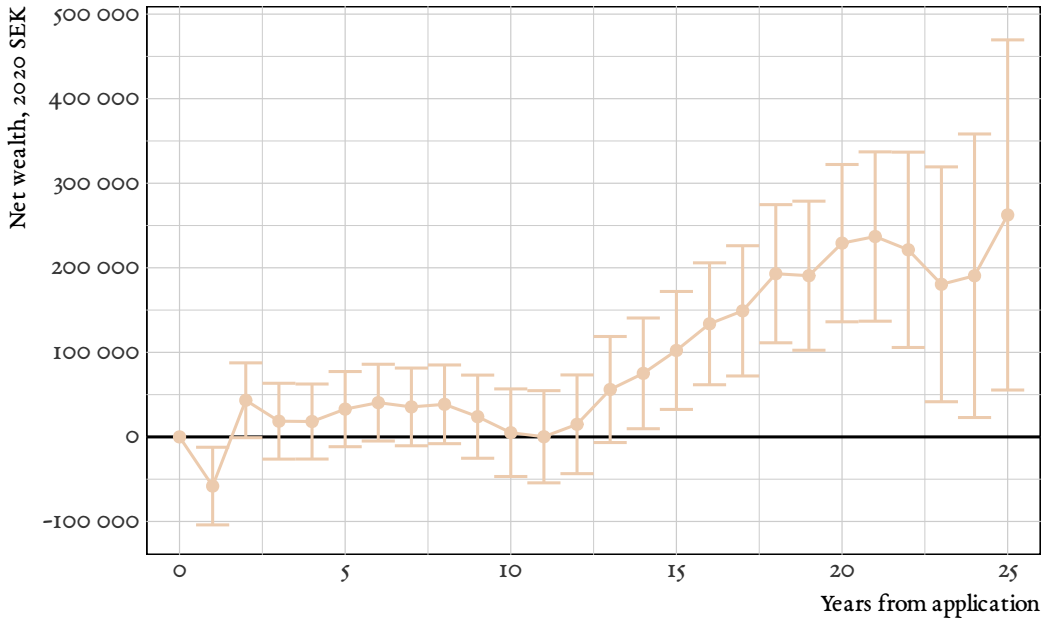
Notes: This table presents the second stage estimates of individual earnings regressions where where we instrument enrollment in a business program with being above the cutoff at the time of admission. Following Kirkebøen, Leuven, and Mogstad (2016), we quantify the relative labor market payoffs of business education relative to alternative educational majors among households with some college education. In each column, we estimate the effects of business education on earnings relative to a different field of study. The alternative fields of study include health, humanities, law, medicine, other, science, social science, teaching, and technology. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual 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.7: Business Education and Homeownership

	Homeownership		
Treatment:	Above_Cutoff	Enrolled	Degree
	(1)	(2)	(3)
Treatment	-0.002	-0.005	-0.008
	(-0.35)	(-0.35)	(-0.35)
Obs	305,061	305,061	305,061
FE: time	Yes	Yes	Yes
FE: data year	Yes	Yes	Yes
FE: cutoff	Yes	Yes	Yes
FE: age	Yes	Yes	Yes
FE: female	Yes	Yes	Yes

Notes: This table presents the estimates of homeownership regressions. Column (1) reports the reduced-form regressions as shown in Equation 1. Column (2) and (3) 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. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007. All regressions include linear polynomials of the running variables (one for each admission group, estimated separately above and below the cutoff), and fixed effects for each admission cutoff, applicant age, applicant gender, year of measurement and the number of years since application. Standard errors are two-way clustered at the cutoff and individual level, 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: Business Education and Household Net Wealth over Time



Notes: This figure illustrates evolution of wealth effects of business education over time. Specifically, we augment our base regression model, as outlined in Equation 2, by including an interaction term of enrollment in a business program and number of years that has passed since application, and present the estimated coefficients along with their confidence banks over time. The x-axis reports the number of years that has passed since application to a business program while the y-axis presents the coefficient estimates on having business education interacted with each of these years (up to year 25) separately from the net wealth analysis. Net wealth is measured at the household level. The sample is limited to individuals who apply to degree programs in business before 1997 and have a non-business counterfactual alternative. The portfolio and wealth outcomes are observed each year between 1999 and 2007.