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Peers and careers: unequal returns to elite alumni networks

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Abstract

Do alumni ties preserve economic advantage? Linking random tutorial-group assignments at a Danish business school to administrative career data, we show that students align careers more with group peers than others in the same cohort, particularly through shared workplaces. These effects are especially pronounced among students from the wealthiest families and concentrated in top-paying firms. More exposure to affluent peers raises earnings, access to top-paying jobs, and increases the probability of reaching the top income ranks for similarly privileged students, while not for others. Job transitions to group peers point to gains from peer-connected moves, again concentrated among wealthier students.

Key words: Social connections, Peer effects, Elite university, Social mobility.

JEL classification: I24, I26, J62.

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1 Introduction

The adage "it's not what you know, but who you know" resonates powerfully in the context of career progression. Access to professionally successful peers is often viewed as part of the value of an education program. Business schools, often seen as stepping stones into high-paying careers, emphasize alumni peer ties.[1] At the same time, programs that propel graduates to the upper echelons of the income ladder typically enroll a disproportionate share of students from high-income families (Chetty et al., 2026). Previous research has also shown that the returns to such education programs tend to favor students from wealthy backgrounds (Zimmerman, 2019). These facts raise the question of what role alumni ties play in shaping individual careers and reproducing economic elites. Specifically, do social connections to individuals from affluent backgrounds help students from less privileged upbringings achieve career success, or do these connections primarily benefit those who share a similar social standing?

Our paper benefits from a setting characterized by a unique combination of conditionally random assignment to tutorial groups and extensive data on students' careers and family backgrounds. We use administrative records on tutorial-group assignment in the Business Economics program at Copenhagen Business School (CBS) for cohorts entering between 1986 and 2006. This is a large business education program known for producing graduates who often rank among the highest earners in the country. It has also traditionally enrolled a disproportionate number of students from affluent families. We merge these records with Danish linked employer-employee data, which provide detailed information on students' individual career trajectories. Population-wide registers further allow us to link students to parents and observe parental income and employment histories.

The paper has three main findings. First, we show that peer exposure affects career trajectories: students' industries, occupations, and employers align more closely with tutorial group peers than with other cohort peers. This pattern is driven primarily by former students working together in the same workplaces. The effect is especially pronounced among students from the very top of the parental income distribution: pairs in which both students have fathers in the top 1% are much more likely to work together, and this excess tendency is particularly visible in top-paying firms. Second, despite high average career success, students from elite family backgrounds substantially outperform other students. Using exogenous variation in tutorial-group composition, we show that exposure to more elite peers generates substantial gains for students from top 1% families. These students earn more, are more likely to work in

[1] Promotional materials of world-leading business schools often explicitly mention career benefits from networking among students (e.g., University of Chicago Booth (2018)).

top-paying firms and occupations, and are more likely to reach the top income ranks themselves. We find no comparable gains for students from less affluent backgrounds. Third, we examine job transitions to firms where former peers work. Transitions to firms with group peers are associated with higher wages than transitions to firms with cohort peers. Overall, alumni networks in this setting reinforce advantages among affluent students rather than broadening access to elite careers.

The analysis follows in three steps. First, to identify the causal effect of peer exposure on post-graduation choices, we use a dyadic approach. We analyze pairs of students within the same matriculation cohort. We compare pairs assigned to the same tutorial group, or group peers, with pairs from the same cohort assigned to different groups, or cohort peers. We find that group peers tend to have more similar careers than cohort peers. They exhibit an "excess" tendency to work in the same occupations and industries and are more likely to be employed by the same firm and workplace. The workplace effect is particularly strong: a pair of group peers is over 37% more likely to work in the same workplace than a pair of cohort peers, compared with less than 3% for industries and occupations and 17% for employers.[2]

Although group-specific shocks could generate excess career similarities, several patterns point more directly to social interactions among tutorial-group peers. The excess similarity is concentrated at the most granular level: students are disproportionately likely to work at the exact same workplace, but not in the same narrow industry or even the same firm if there is no workplace overlap. It is also concentrated in overlapping employment spells, rather than in narrowly separated non-overlapping spells at the same workplace. Finally, students are also "excessively" more likely to work at jobs connected to group peers' parents, where links are determined before matriculation.

The effect is significantly stronger when both students come from the wealthiest families, as measured by fathers' income in the top 1% of the national income distribution. These differences fade under a broader definition of rich family: pairs with fathers between the 90th and 99th percentiles do not have stronger networking effects than pairs below the 90th percentile. Moreover, the workplace effect among students from the most affluent families is concentrated in top-paying firms.

Stronger effects for top 1% students can reflect stronger tie formation in response to group exposure, greater relevance of the induced ties for job search, or both. Although we cannot decompose these mechanisms precisely, we show that group exposure increases both academic and non-academic interactions among top 1% students. Academically, top 1% group peers are more likely to take shared elective courses and

[2] Sharing workplaces with tutorial group peers is not uncommon for students in our sample. Almost 8 percent of students work with at least one tutorial-group peer in their first post-graduation job, and around 22 percent do so at least once during the first ten years after graduation.

participate in other common academic activities. Non-academically, they are also more likely to cohabit during and after their studies.

Second, given the stronger peer ties observed among students from affluent families, we investigate whether these students also benefit from being assigned to groups with more peers from similarly privileged backgrounds. We first document substantial gaps in career achievement by family background. For example, students from top 1% families earn about 15% more ten years after graduation and are almost twice as likely to reach the top 1% of the earnings distribution. We then use a linear-in-means framework that compares students from the same cohort who were assigned to tutorial groups with different shares of highly affluent peers. Students from the most affluent families experience significant career gains when assigned to groups with a higher share of similarly privileged peers. They have higher earnings and wages, are more likely to work in top-paying firms and occupations, and are more likely to reach the top of the income distribution themselves. The magnitude is economically large: a 10 percentage point increase in the elite-peer share raises earnings for top 1% students by an amount equal to roughly one-third of the observed earnings gap between top 1% and other students across years after graduation. These career effects are specific to top 1% students and top 1% peers, and we find no evidence that they operate through educational outcomes.

Finally, we examine whether peer-connected job transitions are associated with career gains. The preceding results show that tutorial-group exposure shapes where students work and that exposure to peers from top 1% families improves long-run outcomes for students from similarly affluent backgrounds. One likely mechanism is improved access to higher-paying firms and occupations. However, it is unclear whether moving to a peer-connected firm is itself associated with better labor-market outcomes. We estimate a stacked event-study design comparing transitions to firms with one or more tutorial-group peers to transitions to firms with one or more cohort peers. We find that transitions to firms with group peers are followed by sizable wage gains relative to transitions to firms with cohort peers. The gains are largest in the year of transition and decline thereafter. The effects are again concentrated among students from the most affluent families, while we find no significant effect for other students. The results are robust to comparing transitions to the same destination firm at the same time and to restricting comparisons to workers moving from the same origin jobs.

Our findings contribute to several strands of research, starting with the literature on the labor-market effects of education networks.[3] Marmaros and Sacerdote (2002) study how Dartmouth seniors use fraternity and sorority connections to secure their

[3]While we focus specifically on social connections among university peers, a broader literature examines the role of peers in educational settings in shaping labor-market outcomes (Black et al., 2013; Anelli and Peri, 2017; Bertoni et al., 2020; Feld and Zölitz, 2022).

first jobs, relying on self-reported networking data. Zhu (2022) identifies referral networks among community college graduates in Arkansas, while Ost et al. (2026) study how public-university peers in Ohio affect re-employment after displacement. Hacamo and Kleiner (2021) show how firms use MBA-based social connections to attract managerial talent.[4] Related evidence on elite education shows that graduates from the same cohorts of elite Chilean universities are disproportionately likely to manage the same firms together (Zimmerman, 2019). Similarly, Kramarz and Thesmar (2013) show that CEOs from elite French universities are more likely to appoint board members from the same schools.

This paper differs from previous studies in several ways. First, we study an education program whose graduates often enter high-paying corporate positions, unlike settings focused on broader labor-market segments (Zhu, 2022; Ost et al., 2026). In this segment, the redistributive consequences of university networks may be different. At the same time, not all careers in our setting reach the top executive level, as in studies of corporate boards or senior managers (Kramarz and Thesmar, 2013; Shue, 2013). Unlike MBA students, whose networks may already be shaped by prior employers (Hacamo and Kleiner, 2021), our sample consists of Bachelor students who typically lack relevant professional connections before entering the program. Our setting is therefore well suited for studying the early-career formation of elite-oriented alumni networks. Second, our empirical strategy relies on assigned peer exposure that is both long-lasting and explicitly conditionally randomized. The exposure spans the full undergraduate program, unlike shorter course-level assignments (Feld and Zölitz, 2022; Zhu, 2022) and MBA sections (Lerner and Malmendier, 2013; Shue, 2013). It is also based on explicit conditional randomization, rather than within-school across-cohort variation (Zimmerman, 2019), job displacements (Eliason et al., 2023), or a combination of the two (Ost et al., 2026). These features allow us to identify the causal effect of assigned alumni-network exposure on corporate career development and to illuminate a key mechanism through which business education shapes labor-market outcomes.

Another branch of research studies elite educational programs, inequality, and social mobility (Dale and Krueger, 2002; Chetty et al., 2026). In contrast to much of this literature, we focus on how students' socioeconomic backgrounds interact with the returns to elite alumni connections. Our findings support the view that unequal access to, and unequal returns from, alumni networks may help explain why elite business degrees generate larger gains for students from privileged families (Zimmerman, 2019). Our study is closely related to Michelman et al. (2022).[5] While

[4]Several studies also leverage peer group randomizations in business school settings to identify effects on outcomes not directly related to labor-market careers (Lerner and Malmendier, 2013; Shue, 2013). Bjerge and Skibsted (2016) studies master program choices at CBS using a different data source.

[5]Cattan et al. (2025) uncover a comparable pattern in education decisions, showing that exposure to elite peers in Norwegian high schools increases enrollment in elite degree programs. Barrios-Fernández

we document conceptually similar patterns, our setting differs sharply: a contemporary Scandinavian business school rather than Harvard University nearly a century ago. This distinction matters because Denmark's redistributive institutions, relatively low inequality, and high social mobility might be expected to weaken elite peer networks. Our findings challenge this expectation, showing that social connections among affluent university peers can shape access to elite careers even in a comparatively egalitarian setting. We also contribute by using administrative records linking tutorial-group assignments, family backgrounds, and long-run career trajectories.

Recent work on social capital argues that cross-class connections are central to social mobility. Chetty et al. (2022a) show that economic connectedness strongly predicts upward mobility across areas, while Chetty et al. (2022b) distinguish between exposure to high-SES peers and friendship formation conditional on exposure, which they call friending bias. Exposure to elite peers in educational settings may therefore help students from less affluent backgrounds reach top positions. However, this need not occur if social ties fail to form across socioeconomic divisions, or if induced cross-class ties are less economically valuable than ties among students from similar backgrounds. We study this distinction in an elite education setting: students are exposed to affluent peers, but the career value of these ties depends strongly on their own socioeconomic background.

This paper also relates to the labor literature on networks, which shows that informal connections are pervasive in job search and hiring and may either amplify or mitigate inequality (Ioannides and Datcher Loury, 2004; Topa, 2011; Galenianos, 2021). Yet the distributional implications of referrals are theoretically and empirically ambiguous. Lester et al. (2021) emphasize that business-contact referrals and family-and-friend referrals play different roles: business referrals tend to benefit already high-productivity workers and increase inequality, whereas family-and-friend referrals can support workers with weaker labor-market prospects. We show that, in the context of elite business-school ties, alumni networks exacerbate inequality by generating the largest career gains for students from the most affluent families.

The remaining sections of this paper are structured as follows. The next section provides an overview of the institutional context of the program, outlines the data sources used, and presents descriptive statistics. In Section 3, we employ a dyadic regression framework to examine network effects by exploring "excess" career similarities. Section 4 adopts a linear-in-means approach to discern varying returns to elite peer exposure. Section 5 investigates the career implications of peer transitions. The paper concludes with a final section.

et al. (2026) show that admission to elite colleges in Chile improves the chances that students' children attend elite schools and colleges.

2 Data and Institutional Background

2.1 Business Economics at CBS 1986-2006

Copenhagen Business School (CBS) is a large public institution in Copenhagen, Denmark. Our study focuses on CBS's largest program: the three-year degree in Business Economics, known in Danish as HA almen. During the sample period, the degree was equivalent to a Bachelor's degree and allowed students to continue into Master's-level programs. Most graduates continued at CBS in either the Master of Science in Economics and Business Administration or the Master of Science in Business Economics and Auditing. As in other Danish higher-education programs, tuition was free, and students were eligible for government-funded stipends.

During the sample period, the Business Economics program admitted approximately 600 students annually. Applications were processed through Denmark's centralized higher-education admissions system, and admission was based primarily on high-school grade-point average. The program was the largest Business Economics program in Denmark and consistently among the most selective: throughout the period under study, applicants exceeded available seats, and the admission cutoff was the highest among Danish Business Economics programs.

The central institutional feature for our research design is the organization of students into tutorial groups. Incoming students were assigned to groups of approximately 35 before the start of the first semester. These groups were the primary unit for organizing academic and social life throughout the three-year program. The study guidelines described the tutorial group as the student's basic unit and closest point of contact, and the program was designed to foster a strong group atmosphere from the outset.[6] Although the guidelines emphasized that all student interactions, including those within and across cohorts, were important for academic success and networking, the tutorial group was the only group formally assigned upon entry. Students in the same group took the same mandatory exercise classes, were taught by the same teaching assistants in those classes, and participated in group-organized academic and social activities together.

Incoming students were assigned to tutorial groups by the CBS administration after the final application deadline, with the admitted cohort assigned in a single batch. Application timing therefore played no role in group assignment. The assignment procedure was manual and used only information derivable from the Danish civil registration number: gender, age, and Danish citizenship. The exact algorithm varied

[6] The closest setting in the literature is MBA sections; see, for example, Lerner and Malmendier (2013) and Shue (2013), who both study MBA sections at Harvard. CBS tutorial groups differ by being substantially smaller, fixed for longer, and characterized by more homogeneous teaching and grading (Harvard Business School, 2026; Copenhagen Business School Studievejledningen, 2004).

across cohorts, but the main objectives were to balance gender across groups, distribute non-Danish students across groups, and place older students in designated senior groups. High-school GPA, prior school, municipality of residence, parental background, and other socioeconomic characteristics were not used. Conditional on the information available to the administration, initial tutorial-group assignment was therefore as good as random.

Our empirical analysis relies on the initial tutorial-group assignment. Group composition was intended to remain fixed throughout the three-year program, and student-initiated group changes were rare. Changes were allowed only under exceptional circumstances, such as ongoing medical treatment, elite-athlete training commitments, or documented personal safety concerns. Scheduling preferences and personal compatibility were not accepted grounds for changing groups. If dropout substantially reduced group size, groups could be merged, but only after the first year. We therefore use initially assigned groups as the source of variation in peer exposure.

The curriculum was highly structured. The program consisted primarily of mandatory courses in social sciences, business economics, and quantitative or technical tools, with approximately 85 percent of required coursework fixed and shared across the cohort. The first year consisted entirely of mandatory courses. In later semesters, students could choose a limited number of electives, amounting to roughly 15–25 percent of the program. Teaching combined lectures for the full cohort with exercise classes organized within tutorial groups. Tutorial-group teaching was especially important in the first year, when students spent a large share of scheduled teaching time in their assigned groups. Attendance was not mandatory, but highly encouraged.

Teaching and assessment were standardized across tutorial groups. Students followed the same curriculum, worked on the same assignments, and took the same examinations. Exams were graded at the cohort level, and final grades were based on common assessment criteria. Teaching assistants had limited discretion over course content and were expected to cover material aligned with the lectures and course plan. They were typically former graduates selected by the course coordinator, with performance in their own studies as an important criterion. To align grading standards, course coordinators organized joint grading meetings in which teaching assistants graded a common set of exam responses before marking the full exams. This structure limited systematic differences in academic content, grading standards, and formal requirements across tutorial groups.

At the same time, tutorial groups generated intensive interaction among assigned peers. From the beginning of the first semester, students participated in a structured introductory period centered on socializing with group peers and learning to navigate the institution. The main event was an organized cabin trip for the tutorial group together with senior students who acted as guides. These senior students introduced new students to CBS and encouraged the formation of smaller reading groups.

The program also emphasized interpersonal skills through collaborative project work and long-term group activities. The curriculum required group-based written submissions, collaborative presentations, and peer-group opposition. Students were strongly encouraged to form reading groups of three to five students, which the study guidelines described as close to necessary for succeeding in the program. These groups were used to discuss course material, solve exercises, exchange notes, prepare for exams, and work on home and group assignments. Reading groups were not assigned by the administration and no administrative records of their membership exist. They were student-driven and could change over time. Thus, while tutorial-group assignment created the main institutional exposure, students endogenously formed more intensive academic and social ties within that environment.

Not all relevant activities were organized at the tutorial-group level. Electives in the final part of the program created opportunities for interaction outside the assigned group, and some student-initiated academic or social activities could involve students from other groups. Career-related activities were organized at the cohort, program, or institution level rather than by tutorial group. CBS held a yearly career day for the student body, firms sponsored graduation ceremonies and prizes for top Bachelor's theses, and career counseling was available through the school. Thus, students in different tutorial groups were exposed to the same formal curriculum, examinations, and institution-level career opportunities, while the main source of differential exposure came from peers assigned to their tutorial group.

2.2 Data Sources and Sample Selection

For this study, we combine administrative data from Copenhagen Business School with register data from Statistics Denmark.

Our study relies on official CBS records for students enrolled in the Business Economics program between 1986 and 2006. These records cover 21 complete cohorts and include matriculation and exmatriculation dates, reasons for exmatriculation, high-school GPA, high-school track, citizenship, gender, age, and, crucially, the initial peer-group assignment made by the CBS administration. We supplement them with information on group composition across semesters and academic activities during the Business Economics program and at the Master's level. Throughout the analysis, we retain all students regardless of graduation status.[7]

We link the CBS records to Danish register data. The registers provide demographic and background information on Danish residents, including age, gender, marital status,

[7]Throughout the paper, years after graduation refer to years after scheduled graduation, defined as four years after matriculation.

municipalities of birth and residence, educational attainment, taxable income, and links to parents. We also use matched employer-employee data covering all firms and workers in Denmark. Combining these sources allows us to construct variables for students and their parents, and to characterize the firms, workplaces, industries, and occupations in which students are employed after graduation.

The primary source for the labor-market analysis is the Danish matched employer-employee data. We use annual job records for 1980–2021, covering each worker's primary job in the last week of November. The data include employment status, occupation, industry, earnings, and firm and workplace identifiers. Firms are identified by the employer's tax identity, and we use the terms "firm" and "employer" interchangeably. Workplaces correspond to physical locations where employees work, such as offices or plants. A firm may have multiple workplaces, but each workplace belongs to a single firm. Occupations are defined at the 4-digit level of the ISCO classification,[8] and industries at the 4-digit level of the NACE classification.[9] We restrict the sample to observations with identified workplaces. If a worker holds multiple jobs in a year, we retain the employment spell with the highest annual earnings. We supplement the annual data with monthly spell data for 2008–2021. To study post-CBS educational trajectories, we use administrative data on students' Bachelor's and Master's programs in Denmark.

Several sample restrictions are worth noting. First, our data do not cover careers outside Denmark. International students who leave Denmark after their studies, as well as Danish students who work abroad, are therefore not observed in the labor-market analysis. We are also unable to identify parents for many international students, so analyses involving parental income are restricted to students with income data for at least one parent. Second, because we study labor-market networks among wage employees, we exclude observations outside wage employment. Workers who are non-employed or self-employed in a given year are therefore not included in the career sample for that year. Below, we document sample-selection patterns and assess whether these restrictions threaten the validity of our empirical strategy.

2.3 Summary Statistics

Table 1 presents summary statistics for the sample of 12,365 students. Approximately two thirds are male, and the average age at matriculation is just above 21. Foreign citizens account for a small share of the student body, at 4%. On average, students' high

[8]DISCO is the Danish version of the International Standard Classification of Occupations (ISCO). When studying occupational similarities, we restrict the sample to years with available occupational data (1994–2016) and use only non-imputed occupational codes.

[9]The first four digits of the Danish industrial classification (DB) correspond to the EU classification of industries (NACE).

TABLE 1
Student Characteristics, by Father's Income Rank

	All	Top 1%	Non Top 1%	Missing
Female	0.34	0.33	0.35	0.39
	(0.48)	(0.47)	(0.48)	(0.49)
Danish citizen	0.96	0.99	0.99	0.40
	(0.19)	(0.08)	(0.11)	(0.49)
Age at matriculation	21.43	20.99	21.41	23.62
	(2.33)	(1.66)	(2.16)	(4.91)
HS GPA	0.09	0.17	0.06	0.21
	(0.82)	(0.78)	(0.83)	(0.86)
Dropout	0.32	0.26	0.34	0.38
	(0.47)	(0.44)	(0.47)	(0.48)
Father's education (in years)	13.63	14.83	13.29	.
	(2.78)	(2.37)	(2.80)	(.)
Mother's education (in years)	12.89	13.62	12.71	.
	(2.71)	(2.44)	(2.74)	(.)
Father in top 1%	0.22	1.00	0.00	.
	(0.41)	(0.00)	(0.00)	(.)
Mother in top 1%	0.02	0.04	0.01	.
	(0.14)	(0.21)	(0.12)	(.)
Father in top 10%	0.67	1.00	0.58	.
	(0.47)	(0.00)	(0.49)	(.)
Mother in top 10%	0.22	0.27	0.21	.
	(0.42)	(0.44)	(0.41)	(.)
Group size	35.40	35.11	35.43	36.23
	(5.78)	(5.74)	(5.77)	(5.87)
Cohort size	592.48	595.13	592.12	587.18
	(48.12)	(48.66)	(48.29)	(41.88)
Observations	12,365	2,412	9,392	561

Notes: Descriptive statistics for CBS sample students based on fathers' income. Rows represent variables, columns - income groups. Cell values indicate variable means and standard deviations (in parentheses). Column definitions are as follows: All - entire CBS student sample; Top 1% - students with fathers in the top 1% of the national income distribution averaged across 5 available years prior the matriculation year; Non Top 1% - students without fathers in the Top 1% group, but with at least one parent having registered income in Denmark before matriculation; Missing - students lacking parental income data before matriculation (parental variables for this group are undefined). High School GPA is standardized based on the GPA distribution for all high school graduates from the academic high school track in the corresponding graduation year.

school GPA is only 0.1 standard deviations above that of their graduating cohort on the academic track.[10] The program dropout rate is 33%. Each student has, on average, around 35 group peers and nearly 600 cohort peers.

[10]Owing to data constraints, we standardize high school GPAs using the distribution of all graduates from the academic high school track in the corresponding year. Approximately 75 percent of students in our sample hold an academic-track high-school qualification.

Many students come from affluent backgrounds. More than two thirds have fathers in the top 10% of the income distribution, and approximately 22% have fathers in the top 1%. Father's income rank is defined relative to the national personal income distribution over the five years before matriculation. Students with fathers in the top 1% have higher high school GPAs and markedly lower dropout rates than students without fathers in this income group. A small fraction of students, about 4.5%, lacks parental income data because both parents were non-residents in Denmark in the year before matriculation.

In many contexts, the elite character of a university program—reflected in the high socioeconomic status of its students—is conflated with academic selectivity. Appendix Fig. A.1 helps separate these two dimensions by comparing students in CBS Business Economics with students in other bachelor programs in Denmark, including other Business Economics programs and other CBS programs. Although parental income and high school GPA are generally positively correlated across and within programs, CBS Business Economics is socioeconomically elite without being especially academically selective relative to other bachelor programs. Students with fathers in the top 1% are almost twice as likely to attend the program as their share among bachelor students would predict. By contrast, attendance is not concentrated among students with the highest high-school GPAs. At the same time, CBS Business Economics is the largest Business Economics program and has the highest admission cutoff among Danish Business Economics programs, underscoring that its distinctive feature is not low selectivity within its field.

Fig. 1 presents career outcomes by years since graduation, separately for students with fathers in the top 1% of the income distribution and all other students. Panel A shows that real annual earnings grow throughout the first 20 years after graduation, with the steepest growth during the first five years. Students with top 1% fathers have no earnings advantage immediately after graduation, but their earnings grow faster over time, producing a gap of about 15% in the second decade after graduation. Panel B shows that the daily wage gap in the second decade also exceeds 15%. Panel C plots the share of students reaching the top 1% of the earnings distribution within their birth cohort. This share rises rapidly during the first decade for both groups, but is eventually almost twice as high among students from top 1% families, at 18% compared with 9%. Panel D shows the corresponding dynamics for the top 10% earnings share, which grows most rapidly during the first five years and peaks at around 60% for top 1% students and 45% for other students. Panels E and F show that both groups move into higher-paying firms and occupations over time. However, students with top 1% fathers are consistently more likely to work at top 10% firms, measured by daily wages, and this gap persists throughout their careers. Differences in access to top 10% occupations are initially small but widen steadily over time.

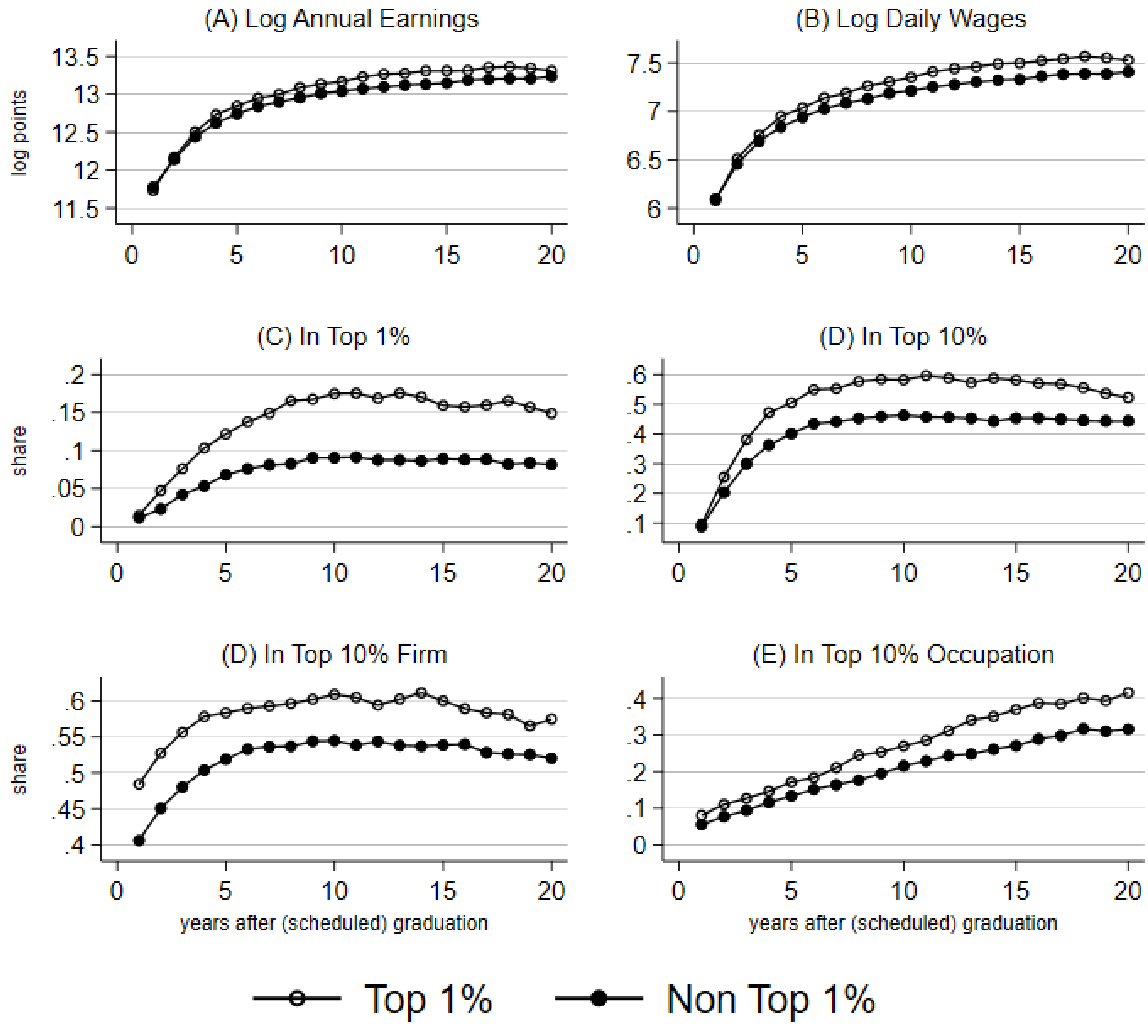

FIGURE 1
Career Dynamics: Top 1% vs Non Top 1% Students

Note: The figure plots the means of various career outcomes by years after scheduled graduation, separately for students with fathers in the top 1% and other students (with at least one parent in the tax records). Father's income rank is defined relative to the national personal income distribution over the five years before matriculation. The year after scheduled graduation is defined as the matriculation year plus 4. Earnings and daily wages are measured in 2015 Danish kroner. Students' top 10% and 1% statuses are based on earnings percentiles in a given year for their birth cohort. A top 10% firm and top 10% occupation are defined as one in the average daily wages distribution for a given year by firm and occupation, correspondingly.

Compared with students outside CBS Business Economics, students in this program have a clear advantage in reaching the top of the earnings distribution across various top earnings brackets (Appendix Fig. A.2). Even relative to students from other Business Economics programs who tend to have higher earnings, already in the first 10 years of their careers students from CBS Business Economics are more than 2 times more likely to be in 99-99.9% group and 3 times more likely in the top 0.1%. This advantage is visible both over the career cycle and across family-background groups. Appendix

Fig. A.3 shows that CBS Business Economics students are consistently more likely to reach the top 10% and top 1% of the earnings distribution throughout the post-graduation period. Furthermore, it shows that this advantage is present across the father's income distribution. However, the relative advantage is especially pronounced among students from the most affluent families.

Appendix Fig. A.4 shows dynamic sample selection patterns over time. The share of students who are not tax residents in Denmark post-graduation is low, rising from under 3% five years after graduation to around 6% after 15 years, with slightly higher attrition among top 1% students. Among tax residents, over 90% are wage-employed five years post-graduation. Many students pursue Master's degrees during the early years, while a growing share becomes entrepreneurs, reaching around 4% ten years post-graduation, with stronger growth among top 1% students.

3 Career Similarities & Networks

3.1 Identifying "Excess" Peer Similarities

To study whether social interactions among university peers shape later careers, we use a dyadic design. We construct unique student pairs (i, j)[11] within each matriculation cohort $c(i, j)$ and estimate

$$F_{ijt} = \beta I_{ij} + \gamma X_{ijt} + \epsilon_{ijt}, \tag{1}$$

where F_{ijt} indicates whether students i and j share the same job in year t. In the main analysis, this corresponds to working in the same workplace, firm, industry, or occupation. The variable I_{ij} indicates whether the students were assigned to the same tutorial group at matriculation. The vector X_{ijt} contains fixed effects for the variables used in the assignment process: matriculation cohort, calendar year, age at matriculation, gender, and Danish citizenship for both students in the pair.[12]

The coefficient β measures excess similarity: the additional career similarity of two students assigned to the same tutorial group relative to otherwise comparable students from the same cohort assigned to different groups. Since the comparison is within cohort, students in different tutorial groups face the same broad curriculum, graduate into the same labor market, and may still interact. The estimate therefore captures similarity in excess of that shared by cohort peers.

The first identifying condition is that tutorial-group assignment is conditionally

[11] Our primary analysis uses undirected dyads, so (i, j) and (j, i) are treated as the same pair. In supplementary analyses, we also use directed dyads. In that case, i denotes the focal student and j denotes the peer, which allows us to study events such as student i joining a workplace where student j is already employed.

[12] For cross-sectional outcomes, such as similarities in educational choices, we estimate the analogous specification without the t subscript.

exogenous. If students self-selected into groups, or if administrators used unobserved information related to future career choices when assigning groups, same-group pairs could exhibit greater career similarity even without any causal effect of tutorial-group assignment. The conditionally random nature of group assignment at CBS addresses this concern. Conditional on the variables used by the administration, pairs of students within a cohort were as good as randomly assigned to the same or different tutorial groups. Under this condition,

$$I_{ij} \perp \{F_{ijt}(1), F_{ijt}(0)\} \mid X_{ijt},$$

where $F_{ijt}(1)$ and $F_{ijt}(0)$ denote potential career similarity under same-group and different-group assignment. The balancing tests below provide evidence related to this condition.

Conditional exogeneity is necessary but not sufficient for interpreting β as evidence of social interactions. We also assume that group assignment affects later career similarity through the social ties it induces among students. Group assignment is not assignment of friendships, referrals, or professional contacts; it is assignment of exposure. Being placed in the same group gives two students repeated opportunities to meet, study together, exchange information, and remain salient to one another. Some assigned pairs form friendships, reading groups, or later professional connections, while others do not. Dropout, group mergers, and cross-group ties may further attenuate the contrast between same-group and different-group pairs. Thus, β is an intention-to-treat effect of assigned network exposure, not the effect of an observed realized tie.

Endogenous subgroup formation within tutorial groups does not invalidate the design. Students may form reading groups, project groups, friendships, or other ties within and across tutorial groups, and these ties are not randomly formed. However, treatment is defined by initial tutorial-group assignment, so these endogenous ties form after assignment and are part of the mechanism through which assigned exposure becomes salient social ties.

The main threat to the assumption that group-assignment effects operate exclusively through social ties is the presence of group-level common shocks. Even if purely random, such shocks could generate excess similarity without any meaningful role for social ties between students. These shocks may take many forms. Students in the same tutorial group share parts of the academic environment: the same TAs in mandatory courses, classrooms, schedule, daily study environment, and group culture. They may also be accidentally exposed to the same career ideas or idiosyncratic information that makes certain choices more salient. More generally, any group-specific experience not shared by other tutorial groups could, if accumulated over time, create excess within-group career similarity. Some shocks might shift broad career aspirations toward particular industries or occupations; others might affect choices of firms or workplaces. For example, a more competitive group environment could lead students toward investment banking or consulting, while exposure to a job ad near the classroom or a tip from a TA could channel students toward the same narrow firms or workplaces.

Conditional exogeneity does not rule out such post-assignment shocks. Since both social interactions and common shocks are nested within tutorial groups, Eq. 1 cannot identify them separately. The relevant question is therefore whether the empirical patterns are more consistent with induced social interaction or with group-specific shocks. Our interpretation follows an inference-to-the-best-explanation logic: we compare the implications of the two explanations and ask which better accounts for the full set of results (Spirling and Stewart, 2025). Several patterns are natural under a social-ties interpretation but harder to reconcile with common shocks. Excess similarity is concentrated at the most granular level: students are disproportionately likely to work at the same workplace, but not merely in the same narrow industry or even the same firm once workplace overlap is excluded. The timing also matters: the effect is concentrated in overlapping employment spells, rather than in closely timed but non-overlapping spells at the same workplace. Students are also more likely to work in jobs connected to group peers' parents than in jobs connected to cohort peers' parents, even though these parental links are determined before matriculation.

Although it is hard to imagine a setting completely immune to common shocks, our design substantially limits their scope relative to alternative designs. We compare students within the same matriculation cohort rather than across cohorts (Zimmerman, 2019; Ost et al., 2026), holding fixed program content, graduation timing, cohort-level labor-market conditions, and cohort-level employer exposure. Any remaining common shocks would therefore have to vary across tutorial groups within the same cohort. The institutional setting limits such variation: tutorial groups followed the same curriculum, completed the same assignments, and took the same cohort-graded exams; career events, employer interactions, student organizations, and clubs were organized at the university or cohort level rather than by tutorial group.[13]

Even if common group-level shocks are not the main concern, the ITT nature of β is important for interpretation. A larger effect of tutorial assignment for one group can arise through two margins. First, assignment may induce more social ties for that group: for example, students from affluent families may be more likely to convert exposure to other affluent students into friendships. This margin is closely related to the distinction between exposure and friending bias in Chetty et al. (2022b): institutions shape who is exposed to whom, but individual interactions determine whether exposure becomes a

[13]Previous evidence from a standardized teaching environment also suggests limited long-run effects of tutorial instructors on student outcomes (Feld et al., 2020).

salient tie. Second, the career consequences of a given tie may differ across groups. A friendship matters for careers only if it transmits career-relevant information, referrals, or access to job opportunities; such ties may be more valuable when students have similar career aspirations or access to similar labor-market segments.[14] Thus, stronger reduced-form effects among top 1% pairs may reflect stronger tie formation, larger career effects of the ties that form, or both. Because we do not observe the full set of social ties induced by assignment, we cannot decompose these channels. However, we provide suggestive evidence consistent with stronger tie formation among top 1% students: tutorial group assignment leads them to interact more along both academic and non-academic dimensions. From a policy perspective, this distinction matters because exposure is often the relevant intervention margin, but its effects depend on both whether ties form and whether the ties that form are economically valuable.

Lastly, both F_{ijt} and I_{ij} are indicator variables, making β a percentage point difference between relative dyadic frequencies. Understanding the magnitude of these differences in the dyadic setting can be challenging. Share of within-group pairs that work together depends not only on the share of students that work at the workplace with at least one peer, but also on the average number of group peers that those students work with relative to the number of students in a group. For example, in a group of 34 students where each student works together with exactly one group peer the share of students pairs that work together is just $\approx 3\%$. The share goes to 1 when all students in a group work in one workplace. To provide more insight, we calculate the effect as a percentage relative to a baseline frequency among cohort peers. We also discuss statistics like share of students working with at least one group peer.

Due to the nature of dyadic data, the same student may appear in multiple pairs, and pairs can include students from different peer groups. This results in complex patterns of error correlation within a cohort. To account for this, we cluster all dyadic regressions at the cohort level. To address potential inference issues arising from a small number of clusters, we implement a wild cluster bootstrap.[15] Additionally, in the

[14] A compact way to formalize this distinction is to introduce a latent realized-tie variable. Let $D_{ij}(I)$ indicate whether pair (i, j) forms a social tie under assignment status I. If assignment were interpreted as an instrument for this latent treatment, then for subgroup g the reduced form could be written as $ITT_g = \pi_g \cdot LATE_g$, where $\pi_g = E[D_{ij}(1) - D_{ij}(0) \mid G_{ij} = g]$ is the first stage from assignment to realized tie formation, and $LATE_g = E[F_{ijt}(D = 1) - F_{ijt}(D = 0) \mid D_{ij}(1) > D_{ij}(0), G_{ij} = g]$ is the effect of the realized tie for compliers: pairs whose tie is induced by same-group assignment. This decomposition is heuristic because D_{ij} is unobserved. A formal LATE interpretation would also require monotonicity, $D_{ij}(1) \geq D_{ij}(0)$, which rules out pairs that would form a relevant tie only if assigned to different tutorial groups. This assumption is not needed for the reduced-form estimates. Larger reduced-form effects among top 1% pairs may therefore reflect a stronger first stage, a larger complier treatment effect, or both. Even if average effects of ties are the same across groups, local average treatment effects may differ if the induced ties are selected differently—for example, if top 1% students are more likely to form ties with top 1% peers who also become valuable professional connections.

[15] Note that an alternative approach based on dyadic cluster-robust methods does not account for certain higher-order network effects, such as correlation across pairs that do not share a common unit

balancing tests presented in Section 3.2, we employ the permutation-based inference method similar to the one proposed by Shue (2013) to ensure that our primary approach to inference is not overly conservative.

3.2 Evaluation of the Empirical Strategy

To demonstrate the balancedness of our sample, we show that group peers are not initially more similar than cohort peers. This balancedness further supports the credibility of our identification strategy and ensures that any subsequent differences in career outcomes between group peers and cohort peers are driven by the effect of group assignment rather than pre-existing differences among students.

Table 2 presents the results of the balancing test. To assess the balance between group peers and cohort peers, we conduct a balancing test using variables measured before matriculation. We use a cross-sectional version of Eq. 1, where career outcomes are replaced with a set of variables determined prior to matriculation. These variables include the difference in standardized high school GPA, an indicator for both students being in the top 10% by GPA within their matriculation cohort, sharing the same high school track, both students having no parental tax record, living in the same municipality or postcode, being born in the same municipality, differences in average income rank within postcodes of residence, differences in parents' years of education and income ranks (for both mothers and fathers), indicators for both mothers or fathers being in the top 10% or top 1%, whether parents worked in the same industry or workplace within 10 years prior to matriculation, and whether students worked in the same industry or workplace within the same period.

To address concerns that using wild cluster bootstrap at the matriculation cohort level may lead to overly conservative inference, potentially masking imbalances, we also implement a permutation-based method to calculate alternative p-values.[16] In this method, students are randomly reassigned across groups within a matriculation cohort while preserving the distribution of age, gender, and Danish citizenship. P-values based on this permutation procedure appears to be close to the ones based on wild cluster bootstrap. We find that none of the variables appears unbalanced at the 10% significance level. Overall, these results support the validity of our identification strategy.

Although the balancing tests support conditional random assignment, missing observations could still matter because not all students are observed in the career sample every year. Students may leave Denmark, or they may remain in Denmark but

(Aronow et al., 2015).

[16]This approach is not applied throughout the paper due to its prohibitive computational cost in a dyadic setting.

TABLE 2

Dyadic Balancing Test

	HS GPA	High HS GPA	HS Track	No Parent Tax Record
Same group	-0.196	-0.006	0.088	0.003
	(0.217)	(0.024)	(0.115)	(0.005)
P-value (WCB)	0.400	0.775	0.449	0.662
P-value (Permut.)	0.289	0.737	0.357	0.621
Observations	3,238,962	3,238,962	3,397,100	3,397,855
	Municipality	Postcode	Place of Birth	Neighbors' Av. Income Rank
Same group	-0.002	0.003	0.023	-0.207
	(0.044)	(0.016)	(0.032)	(0.311)
P-value (WCB)	0.962	0.860	0.492	0.501
P-value (Permut.)	0.949	0.845	0.533	0.527
Observations	3,645,889	3,406,982	3,397,855	3,406,982
	Mother's Education	Father's Education	Mother's Income Rank	Father's Income Rank
Same group	0.185	-0.675	-4.031	-4.234
	(0.487)	(0.563)	(3.770)	(4.912)
P-value (WCB)	0.713	0.251	0.319	0.384
P-value (Permut.)	0.685	0.196	0.314	0.346
Observations	2,988,585	2,714,389	3,210,548	2,980,874
	Mother in Top 10%	Father in Top 10%	Mother in Top 1%	Father in Top 1%
Same group	0.013	0.135	0.005	-0.003
	(0.042)	(0.088)	(0.005)	(0.036)
P-value (WCB)	0.765	0.150	0.352	0.936
P-value (Permut.)	0.743	0.117	0.306	0.943
Observations	3,210,548	2,980,874	3,210,548	2,980,874
	Parental Industries	Parental Workplaces	Prior Industries	Prior Workplaces
Same group	0.031	0.037	0.018	-0.035
	(0.022)	(0.084)	(0.012)	(0.058)
P-value (WCB)	0.176	0.657	0.165	0.547
P-value (Permut.)	0.217	0.588	0.116	0.493
Observations	3,648,469	3,648,469	3,610,465	3,626,043

Notes: The table provides regression coefficients from the balancing test specified in Eq. 1, measuring predetermined student similarities. All coefficients are multiplied by 100 to represent percentage points. Variables used to measure similarities include the difference in standardized high school GPA, an indicator for both students being in the top 10% by GPA within their matriculation cohort, having the same high school track, both students having no parental tax record, residing in the same municipality or postcode, being born in the same municipality, differences in average income rank within postcodes of residence, differences in parents' years of education and income ranks (for both mothers and fathers), indicators for both mothers or fathers being in the top 10% or top 1%, whether parents worked in the same industry or workplace within 10 years prior to matriculation, and whether students worked in the same industry or workplace within the same time frame. Standard errors, shown in parentheses, are clustered at the cohort level. P-values reflect the significance of coefficients and are determined using two methods: wild cluster bootstrap (WCB) at the matriculation cohort level with 9,999 replications, and a permutation procedure with 999 replications that randomly reassigns students across groups within cohorts while maintaining the distribution of age, gender, and Danish citizenship. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

be non-employed, self-employed, or enrolled in further education.[17] Such missingness would threaten the interpretation of our estimates if group assignment affected sample inclusion in ways correlated with later career similarity. Appendix Table A.1 tests this directly, both overall and by top 1% status, by examining whether assignment to the same peer group affects joint observation in the career data, joint tax residency in Denmark, and, conditional on Danish residency, joint wage employment or continued education. We find little evidence of differential selection. The only statistically significant effect is small, appears at the 10% level, and is driven mainly by education in the first two years after potential graduation, when many students are still studying. Overall, sample selection does not appear to threaten the interpretation of the results.

3.3 Results

3.3.1 Main Results

We start by estimating the "excess" similarities formalized in Eq. 1. Panel A of Table 3 explores the "excess" similarities between group peers concerning their industry, occupation, firm, and workplace choices. Across all four outcomes, pairs of group peers are more likely than pairs of cohort peers to follow similar career paths. Because percentage-point estimates can be difficult to interpret in this setting, we also report effects relative to the baseline probability that cohort peers work in the same labor-market cell. These relative effects differ substantially across outcomes. Students assigned to the same peer group are about 3% more likely to work in the same industry or occupation, 17% more likely to work at the same firm, and 37% more likely to work at the same workplace after graduation.

We complement the dyadic regressions with a random-assignment benchmark (Appendix Table A.2). Holding fixed the realized distribution of workplaces, we repeatedly reassign students to placebo tutorial groups within cells defined by gender, Danish nationality, and age at matriculation, while preserving group sizes. The simulated mean captures the workplace overlap that would arise mechanically under the CBS assignment rule absent the realized tutorial-group links. The difference between the observed and simulated means therefore provides a simple student-level analogue to the dyadic estimates, measuring excess workplace overlap with actual group peers relative to comparable placebo peers. Observed peer workplace exposure is consistently higher than under simulated reassignment. Students are more likely to share their first workplace with a group peer (7.7% vs. 4.8%), to work with a group peer at least once within 10 years after graduation (22.3% vs. 17.9%), to spend a larger share of years with at least one group peer (5.9% vs. 4.6%).

[17] Figure A.4 in the Appendix summarizes sample selection patterns over 20 years after graduation, separately for top 1% and non-top 1% students.

Some group-specific common shocks could generate similarity in broad career trajectories, such as industries or occupations. To examine this possibility, we redefine each outcome to capture cases in which students share the same industry, occupation, or firm, but not the same workplace.[18] As shown in Panel B, once workplace overlap is excluded, the effects on sharing the same industry and firm are no longer statistically significant. This suggests that the broader similarities in industry and firm choices are largely driven by students working together at the same workplace, rather than by general convergence in career trajectories. The effect on occupational similarity remains statistically significant, but is relatively small.

Because workplaces are nested within industries, broad common shocks could in principle lead workers into the same industries and, mechanically, increase the probability that they work at the same workplaces. If this were the main channel, however, we would expect to see not only more peers working in the same industry and workplace, but also more peers working in the same industry at different workplaces. The data do not support this latter pattern. A common-shock explanation would therefore have to be much more granular: it would need to direct students to the same specific workplaces, without also increasing sorting into other workplaces within the same narrow industries or even the same firms. By contrast, this pattern follows naturally from a social-connection mechanism. If peers transmit information about vacancies or help secure referrals, the presence of a peer at the exact workplace is the relevant margin.

Developing this idea further, we ask whether it matters that a peer is present at the workplace *at the time* the focal student joins. This distinction is useful for separating social-connection mechanisms from group-specific factors that push group peers to a specific workplace. In the latter case the timing of peer presence need not be central: a student may be more likely to join a workplace that a group peer recently left, just as she may be more likely to join a workplace where a group peer is about to leave. By contrast, if the mechanism operates through information about vacancies, referrals, or coordination, the peer's contemporaneous presence at the workplace should matter.

Figure 2 examines this distinction using monthly spell data for 2008–2021. We start with a panel of events, where a focal student i joins a new workplace. In the directed-dyad version of Eq. 1, the treatment variable indicates whether the focal student i and peer student j were assigned to the same tutorial group. The outcome, however, is no longer simply whether i and j work at the same workplace. Instead, it indicates whether peer j worked at the workplace joined by i within a specified time

[18] This exercise differs from conditioning on students not working at the same workplace, which would drop these observations. The probability of working in the same industry, occupation, or firm can be decomposed into two mutually exclusive events: working in the same category at the same workplace and working in the same category at different workplaces. Here we use the latter as the outcome variable.

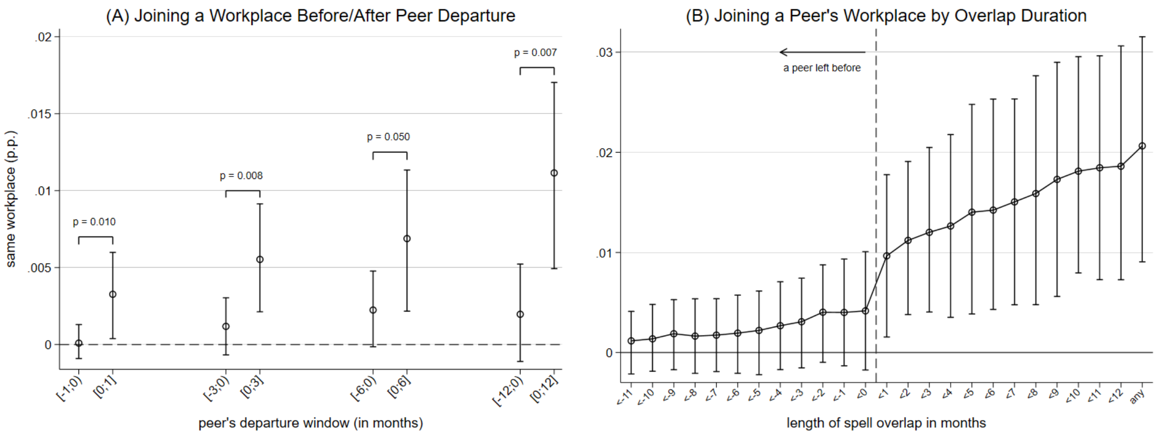

FIGURE 2
Joining a Peer-Connected Workplace: Job Spell Overlap with a Peer

Notes: The figure reports coefficients from directed-dyad regressions based on Eq. 1 on the spell data between 2008 and 2021. The unit of observation is a focal student–peer pair, where a focal students starts a new job spell. First post-graduation jobs are excluded from the analysis. In Panel A, the outcome is an indicator for whether focal student i joins a workplace from which peer j departed shortly before i's entry or shortly after i's entry. Event time is measured in months relative to the month in which i joins the workplace. Bracketed p-values test equality of the corresponding before- and after-entry coefficients. Panel B reports coefficients from regressions where the outcome is an indicator for whether focal student i joins a workplace where peer j works or previously worked, allowing for at most the indicated number of months of overlap between their workplace spells. The final coefficient, "Any," corresponds to the total probability that i joins a workplace where j works or has worked at any point, regardless of overlap length. Standard errors are clustered at the cohort level. Coefficients are multiplied by 100 and reported in percentage points. The 5% confidence intervals refer to point estimates and are established through wild cluster bootstrap at the matriculation cohort level (9999 replications).

window around i's start date. Panel A compares two types of outcomes: cases in which the focal student i joins a workplace shortly after peer j leaves, and cases in which peer j remains at the workplace shortly after the focal student joins. We define these outcomes using narrow windows of 1, 3, 6, and 12 months around the focal student's entry. Even within these short windows, the timing of peer presence matters: the coefficients are larger when the peer remains employed after the focal student joins, whereas the coefficients for workplaces the peer left shortly before entry are statistically insignificant.[19]

Panel B provides a complementary decomposition. The outcome is whether the focal student joins a workplace where the peer works or has worked before, allowing for different degrees of overlap between their workplace spells. Negative values correspond to cases in which the focal student joins after the peer has already left; positive values require the two spells to overlap for at least the indicated number of months. The final coefficient, "Any," captures the total probability of joining a workplace where the peer works or has worked at any point. The total effect is positive, but the timing decomposition shows that it is driven by the cases with actual spell overlap. The coefficients are small and statistically insignificant when the peer has already left, and rise when the peer is present at the workplace after the focal student's entry. This pattern further narrows the set of plausible common-shock explanations: the relevant group-specific factor would have to attract a student to a specific workplace only when an incumbent peer is still present. At the same time, the pattern is directly consistent with a social-connection mechanism in which the peer's contemporaneous presence facilitates information transmission or referrals.

Peer connections may operate not only through workplaces where peers later work themselves, but also through workplace links they bring into the program. If students are disproportionately likely to enter such peer-linked workplaces, this is difficult to explain by tutorial-group shocks alone as these workplaces are fixed before assignment. Students in our sample generally have little relevant labor-market experience before their studies and are therefore unlikely to connect peers to their past employers immediately after graduation, unlike MBA students, for example. (Hacamo and Kleiner, 2021). For young workers, however, parental employers may provide an important source of labor-market connections (Kramarz and Skans, 2014). Table 4 examines this channel directly. Students are 19% more likely to work at workplaces where a group peer's parent worked before matriculation than at workplaces where a cohort peer's parent worked. We also find a smaller effect on working at other workplaces within

[19]Appendix Table A.3 shows that the higher probability that group peers work at the same workplace is driven both by simultaneous moves and by focal students joining incumbent peers - the outcome we investigate here. Appendix Fig. A.5 shows that the effect on joining an incumbent peer remains significant through at least the first five post-graduation jobs.

TABLE 3
Career Similarities

	Same Industry	Same Occupation	Same Firm	Same Workplace
Panel A: Baseline Regressions				
Same Group	0.0537**	0.0853***	0.0548***	0.0517***
	(0.0248)	(0.0223)	(0.0107)	(0.0072)
P-value	0.041	0.000	0.000	0.000
Effect (in %)	2.93	2.64	17.14	36.94
Baseline	1.84	3.23	0.32	0.14
Observations	52,287,568	39,540,123	52,288,624	48,023,863
Panel B: Net of Workplace Effects				
Same Group	-0.0014	0.0561***	0.0016	—
	(0.0212)	(0.0214)	(0.0056)	—
P-value	0.947	0.009	0.768	—
Effect (in %)	-0.08	1.79	0.81	—
Baseline	1.72	3.13	0.19	—
Observations	48,023,863	36,676,726	48,023,863	—

Notes: This table combines estimates from two sets of regressions. Panel A presents baseline regressions, while Panel B reports estimates net of workplace effects. Occupations and industries are categorized at the 4-digit level. Observations for occupations are available only within the 1994-2016 period, and only non-imputed values are utilized. Standard errors, enclosed in parentheses, are clustered at the cohort level. In Panel B the outcome variables are redefined as indicator variables for instances where individuals work within the same industry, occupation, or firm, but not within the same workplace. Baseline values and coefficients have been magnified by a factor of 100 to represent percentage points. The reported p-values indicate the significance of coefficients, determined using wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 4
Career Similarities: Peer Parental Workplaces

	Peer Parent Workplace	Peer Parent Industry	Peer Parent Workplace w/ Peer	Peer Parent Workplace w/o Peer
Same Group	0.0171***	0.0406*	0.0044**	0.0127**
	(0.0066)	(0.0226)	(0.0021)	(0.0049)
P-value	0.010	0.088	0.019	0.018
Effect (in %)	19.25	1.71	112.60	14.92
Baseline	0.089	2.378	0.004	0.085
Observations	21,452,838	21,423,364	21,452,838	21,452,838

Notes: This table presents estimates from the linear probability model specified in Eq. 1 with directed dyads. The sample is restricted to dyads in which a peer has a parent with an observed workplace before matriculation and observations in the first 5 years after scheduled graduation. The column "Peer Parent Workplace" indicates that a student works at a workplace where the peer's parent worked within 10 years before matriculation. "Peer Parent Industry" indicates that a student works in the same industry as the peer's parental workplace, but at a different workplace. "Peer Parent Workplace with Peer" indicates that the student works at the peer's parental workplace together with the peer. "Peer Parent Workplace without Peer" indicates that the student works at the peer's parental workplace while the peer does not work there. Standard errors, shown in parentheses, are clustered at the cohort level. Coefficients and baseline values are multiplied by 100 and reported in percentage points. P-values are computed using wild cluster bootstrap at the matriculation-cohort level with 9,999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

peers' parental industries. Finally, decomposing peer-parent workplace matches by whether the peer is also present shows that group assignment affects both margins: students are more likely to enter peer-parent workplaces both with and without the peer herself being employed there.

This result is important because it points to the relevance of higher-order connections: peers' family networks. It also highlights interactions between connections of intrinsically different types—strong and weak ties (Granovetter, 1973).[20] Lester et al. (2021) show that professional contacts and family or friend ties are qualitatively different: they are used by different types of workers and have different effects. Our results show that the boundary between these types of connections can be blurred. University peers are, on one hand, "peers in careers": they share similar professional skills and qualifications and, as we show, may become each other's colleagues. On the other hand, intensive academic and non-academic interactions can create stronger friendship ties. The parental-workplace results show that alumni ties can also interact with family connections, further blurring this distinction.

Social interactions could also affect career trajectories through educational choices made before labor-market entry.[21] Appendix Table A.4 examines whether peer group assignment affects similarities in educational choices at the Bachelor's and Master's levels. We find no significant effects except for a small effect, significant at the 5% level, on graduating from the same Master's program.[22] Appendix Table A.5 assesses whether this Master's-program channel can explain our career-similarity results. Students who enroll in the same Master's program are more likely to make similar subsequent career choices, but this association may reflect both the causal effect of program choice and selection of similar students into the same program. To be conservative, we treat the association as an upper bound on the causal effect of Master's-program choice. We then impute the probability of working together from a model that uses same Master's program as the treatment and re-estimate the regressions using these imputed probabilities as outcomes. Even under this upper-bound assumption, the implied effect is very small: for example, the workplace effect is less than 1% of the original estimate. Educational choices are therefore unlikely to explain the main career-similarity results.

[20]There is an extensive literature studying strong family ties per se shape sorting of workers to firms (Corak and Piraino, 2011; Kramarz and Skans, 2014; Engzell and Wilmers, 2025). We show that parents of university peers—strong ties of weak ties—matter for employment at specific establishments. By contrast, Plug et al. (2018) finds no effect of parents' high-school peers—weak ties of strong ties—on own occupational choices.

[21]Even in this case, it is not obvious how similarities in educational choices would translate into workplace-level career similarities, since education is more likely to affect occupation or industry choice.

[22]There is limited variation in Master's degree choices in our sample. Among students who pursue a Master's degree, the overwhelming majority enroll in one of the two programs available at CBS at the time.

3.3.2 Networks and Elite Homophily

Students from affluent backgrounds may accumulate more valuable labor-market networks during elite education, helping explain why returns to such programs are often higher for students from privileged families (Zimmerman, 2019). To investigate this question, we analyze how being assigned to the same peer group affects the probability that student pairs work at the same workplace, classifying students by their fathers' income ranks.

Fig. 3 Panel A compares two definitions of "top" status: having a father in the top 1% and having a father between the 90th and 99th percentiles. Student pairs are divided into three groups: both students have fathers in the top group, only one does, or neither does. The networking effect is much stronger for students from the most affluent family backgrounds. Among pairs where both students have top 1% fathers, the effect is four times larger than for pairs where neither student has a top 1% father ($p = 0.006$ for equality) and twice as large as for mixed pairs ($p = 0.064$). When we exclude students with top 1% fathers and instead define affluent background as having a father between the 90th and 99th percentiles, the pattern disappears: the effect for pairs from top families is indistinguishable from the effect for pairs with fathers below the 90th percentile. The strongest networking effects are therefore specific to students from the very top of the family-income distribution.

Panel B shows that the stronger effect among students from elite backgrounds is mainly driven by joint employment at firms near the top of the average-pay distribution.[23] The effect on sharing a workplace at a top 5% firm is four times larger for top 1% pairs than for other pairs ($p = 0.018$ for equality). Thus, for top 1% students, the relevant peer links are closely connected to the high-paying segment of the labor market. This pattern differs from the broader use of social connections in the labor market, where lower-paying firms have been shown to rely more on connected hiring (Eliason et al., 2023). Appendix Table A.6 shows complementary evidence: among top 1% student pairs, the effect of group ties on joining an incumbent peer is concentrated in top 10% occupations, whereas among other students it is concentrated in bottom 90% occupations.

Network homophily appears along other dimensions as well (Appendix Fig. A.6). Mixed-gender pairs exhibit weaker effects of group assignment than male pairs ($p < 0.001$ for equality) and female pairs ($p = 0.001$ for equality). Students from the same country of origin also exhibit stronger effects than students from different countries ($p = 0.021$ for equality). This homophily, together with the gradual weakening of the effect as time since exposure increases, is consistent with social interactions (Eliason

[23]The probability of working together can be decomposed into probabilities of working together at firms with different pay ranks.

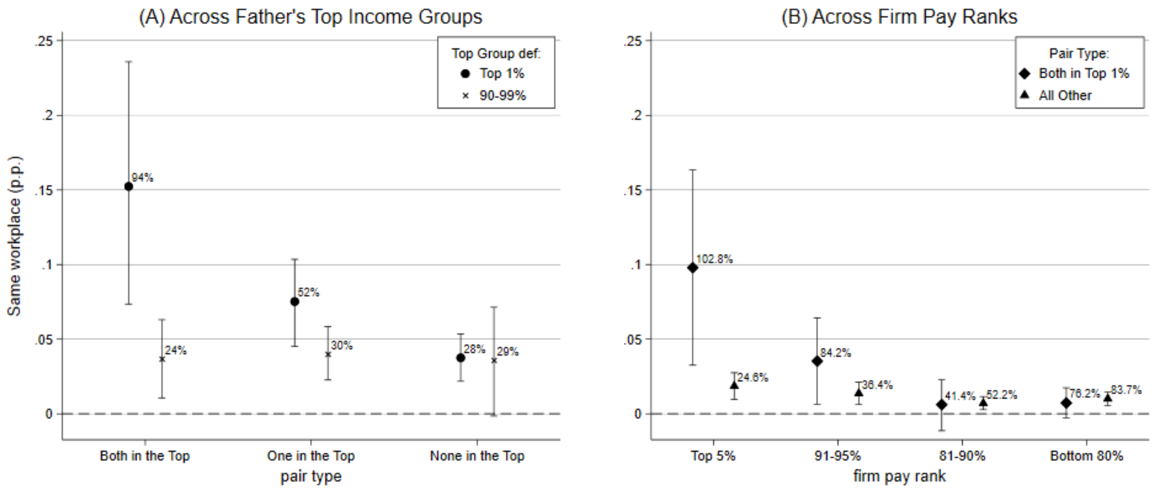

FIGURE 3
Same Workplace: by Father's Income Rank

Note: The figure reports coefficients from the linear probability model specified in Eq.~**??**{eq:1_2}. Panel A interacts treatment with father's income-group indicators. In the "Top 1%" model, "Both in Top," "One in Top," and "None in Top" refer to whether both, one, or neither student has a father in the top 1% of the national income distribution. In the "90–99%" model, students with top 1% fathers are excluded, and "Top" refers to fathers between the 90th and 99th percentiles. Father's income rank is based on years before matriculation. Panel B reports separate regressions for top 1% father pairs and all other pairs. The outcomes indicate whether the two students work together at the same workplace, separately by firm pay-rank range. Firm pay ranks are based on average daily wages across all firms in a given year. Relative effects, shown as percentages, are displayed on the graph. The 5% confidence intervals for the point estimates are established through wild cluster bootstrap at the matriculation cohort level (9999 replications).

et al., 2023). The effect of group peers is also stronger for students with high rather than low high-school GPA ($p = 0.007$ for equality). This, together with the fact that connected transitions are concentrated in higher-paying occupations, suggests that alumni networks in our setting resemble professional or business connections more than family-and-friend ties (Lester et al., 2021).

As discussed in Section 3.1, stronger effects among top 1% students can reflect two distinct margins. First, group assignment may generate more social ties among these students—a form of friending bias. Second, the ties induced by group exposure may have larger career consequences. As Algan et al. (2026) show, group interactions can reinforce preexisting similarities. If top 1% students initially have more similar career aspirations, for them social ties might have stronger effects on career similarities in the future.[24] Because we do not observe social ties directly, we cannot decompose the family-background heterogeneity into differences in tie formation and differences in the effects of ties. We can, however, examine whether the first margin appears in the available measures of exposure and interaction intensity.[25]

Table 5 examines the effect of initial assignment to the same tutorial group on several measures of exposure and interaction, both for all students and separately by fathers' top 1% status. First, if group peers affected each other's dropout behavior, and if this effect differed for top 1% students, assignment to the same tutorial group could mechanically lead top 1% students to spend more time exposed to one another. Panel A finds no significant effects of group assignment on program attrition, either overall or by top 1% status. Second, although group changes were rare and group mergers or enlargements should not be systematically related to group composition, we also test whether initial assignment to the same group affects the probability of being observed in the same group in a given semester. The effect of initial group assignment on later same-group exposure is statistically indistinguishable between top 1% students and other students. We therefore conclude that group assignment does not mechanically generate greater exposure among top 1% students through attrition or group changes.

Panel B asks whether same-group assignment leads to more intensive academic interaction among top 1% students than among other students. If initial group assignment generated stronger interactions among top 1% students, this could appear as a greater tendency to take more electives together. This is what we find: although students in our sample took fewer than three electives on average, the effect on the number of shared electives is significantly larger among top 1% pairs than among other pairs. Choices of academic activities vary more at the Master's level. When we add

[24]Appendix Table A.7 shows that, even among students assigned to different tutorial groups, pairs from top 1% families are significantly more likely than other pairs to work in the same industries and occupations, but not in the same firms or workplaces.

[25]Interpretations based on common shocks would also have to explain why top 1% students are differentially affected by them.

TABLE 5
Exposure and Interactions

	Both Still Enrolled			Still in Same Group		
	All	Top 1%	Other	All	Top 1%	Other
Panel A: Exposure						
Same Group	-0.030	0.192	-0.033	85.24***	84.44***	85.27***
	(0.064)	(0.467)	(0.068)	(1.579)	(2.774)	(1.557)
P-value	0.635	0.703	0.630	0.000	0.000	0.000
P-val: Top vs Other		0.649			0.648	
Baseline	49.66	55.28	49.50	2.122	2.131	2.122
Observations	16,837,914	480,756	16,357,158	8,364,260	265,846	8,098,414

	Shared Electives			+ Master's Lvl Activites		
	All	Top 1%	Other	All	Top 1%	Other
Panel B: Academic Interactions						
Same Group	0.067***	0.103***	0.066***	0.133***	0.211***	0.130***
	(0.007)	(0.020)	(0.007)	(0.012)	(0.046)	(0.012)
P-value	0.000	0.000	0.000	0.000	0.000	0.000
P-val: Top vs Other		0.049			0.098	
Baseline	0.407	0.475	0.404	0.707	0.767	0.704
Observations	3,454,340	136,320	3,318,020	3,454,340	136,320	3,318,020

	During Studies			After Graduation		
	All	Top 1%	Other	All	Top 1%	Other
Panel C: Cohabitation						
Same Group	0.0729***	0.210***	0.0681***	0.0884***	0.233*	0.0831***
	(0.00861)	(0.0572)	(0.00841)	(0.0140)	(0.107)	(0.0119)
P-value	0.000	0.00140	0.000	0.000	0.0594	0.000
P-val: Top vs Other		0.024			0.173	
Baseline	0.00306	0.00359	0.00300	0.00679	0.0134	0.00653
Observations	7,610,735	236,836	7,265,820	18,868,526	595,058	18,139,360

Notes: The table reports coefficients from the dyadic regression specified in Eq. 1. Regressions are estimated for all available observations ("All"), pairs in which both fathers are in the top 1% of the income distribution ("Top 1%"), and all other pairs ("Other"). Panel A uses data for matriculation cohorts 1996–2006, for which tutorial-group composition is observed across semesters. The unit of observation is a student pair by semester, and fixed effects are interacted with semester number. "Both Still Enrolled" indicates that at a given semester both students are still enrolled in the program. "Still in Same Group" indicates that both students are assigned to the same tutorial group. The variable is missing if one of the students dropped out. Panel B uses data on academic activities during the Business Economics program and CBS Master's programs. Activities include courses and graded activities inside or outside courses, such as group projects and exams, but exclude thesis work. "Shared Electives" is the number of elective academic activities taken together by the two students. "+ Master's Lvl Activities" additionally includes activities offered at the Master's level. Panel C uses data on spouses and cohabiting partners and is restricted to different-sex pairs. Two students are classified as married or cohabiting if they are observed as spouses or cohabiting partners either within five years after matriculation ("During Studies") or during the subsequent 15 years ("After Graduation"). Standard errors are shown in parentheses and clustered at the cohort level. In Panels A and C coefficients and baseline values are multiplied by 100 and reported in percentage points. P-values are computed using wild cluster bootstrap at the matriculation-cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

these activities to the measure of shared academic choices, the stronger effect among top 1% students remains.

Finally, we assess whether group exposure also leads to more intensive non-academic interaction among top 1% students. We use a narrow proxy for a strong social tie between two students: whether they are registered as a cohabiting or married couple.[26] Five years after matriculation, around 40% of students in our sample have a registered partner (Appendix Fig. A.7). Among partnerships formed within the matriculation cohort, roughly half are with someone from the same initial tutorial group. Top 1% students are more likely than other students to have a cohort peer as a partner: 3.7% versus 2.5%. We find that group assignment has a very strong effect on the probability of couple formation during the study period. Students assigned to the same group are almost 24 times more likely to start cohabitation than comparable students in different groups. The effect is significantly larger for top 1% pairs than for other pairs and remains larger after graduation.[27] Couple formation is a narrow and unusual proxy in the context of labor-market networks. Nevertheless, it provides more direct evidence that group exposure induces stronger social-tie formation among top 1% students.

4 Career Effect of Exposure to Top 1% Peers

4.1 Empirical Strategy

The dyadic analysis shows that exposure to tutorial-group peers affects the labor-market networks students later use in their careers. Students are more likely to work at workplaces connected to their assigned group peers, with especially pronounced effects among students from the wealthiest family backgrounds. These findings motivate a broader question: does exposure to a larger share of peers from top 1% families within the tutorial group affect students' subsequent career outcomes? To answer this, we use a linear-in-means framework that relates later career outcomes to the share of top 1% peers in the assigned group. This allows us to test whether exposure to affluent peers generates career gains, and whether these gains are concentrated among students from particular socioeconomic backgrounds.

We use a standard linear-in-means specification widely applied in the educational peer-effects literature (Sacerdote, 2001). We estimate the model separately for students

[26]Our definition includes married couples, couples cohabiting with a common child, and cohabiting couples without children. During this period, Statistics Denmark defined the latter group as two people of opposite sex, registered at the same address, with an age difference below 15 years, no common child, and not close relatives according to the population register. Since this last group is important for our analysis, we restrict the sample to different-sex pairs.

[27]For both academic and non-academic exposure, the baseline level among cohort pairs is higher for top 1% students.

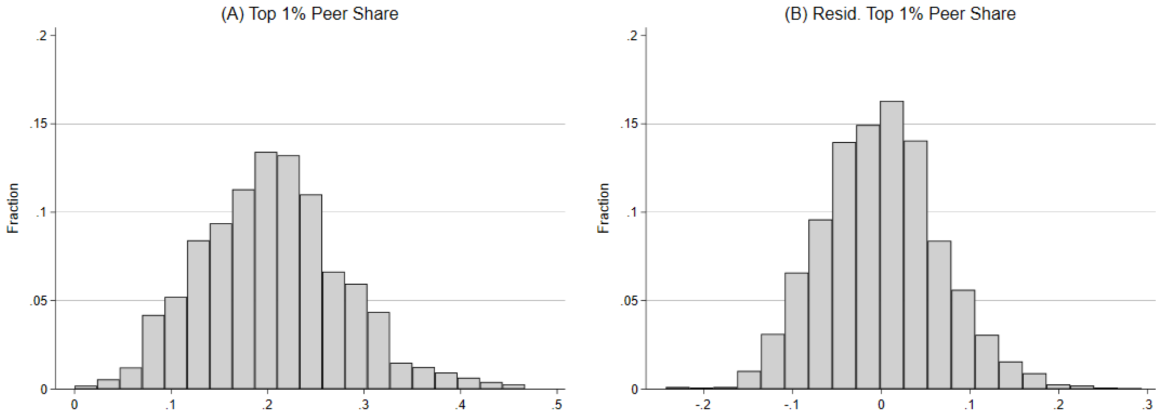

FIGURE 4
Top 1% Peer Group Shares

Notes: The figure displays distribution of tutorial group shares of peers with top 1% fathers that students face. Panel (A) shows raw (leave-one-out) shares of top 1% peers. Panel (B) shows the residualized peer shares used as identifying variation in Eq. 2.

with fathers in the top 1% of the income distribution (H) and all other students (L):

$$y_{it} = \alpha^G \times \overline{Top1\%}_{-ig} + \lambda^G_{ict} + \epsilon_{it}, \tag{2}$$

where $G \in H; L$.[28] The outcome y_{it} is one of the career outcomes of interest. $\lambda^G ict$ denotes fixed effects for each combination of age at matriculation, gender, Danish citizenship, matriculation cohort, and calendar year. Because the regression is estimated separately by top 1% status, α^G captures the group-specific causal effect of interest (Feigenberg et al., 2025). The specification compares students from the same matriculation cohort and calendar year, with the same assignment-relevant characteristics, who were assigned to tutorial groups with different shares of top 1% peers. $\overline{Top1\%} - ig$ is the leave-one-out share of top 1% students in the assigned group. Figure 4 plots the raw and residualized identifying variation. Standard errors are clustered at the peer-group level.

Fathers' top 1% status is defined using average income over the five years before matriculation. Because peer status is predetermined, this specification avoids the reflection problem and the common-shock problem emphasized by Manski (1993). Combined with conditionally exogenous assignment to tutorial groups, this supports a causal interpretation of α^G. To further assess the reliability of the randomization, beyond the evidence in Table 2, we conduct linear-in-means balancing tests.

As in the dyadic analysis, α^G should be interpreted as an intention-to-treat effect. Assignment to a group with more top 1% peers creates exposure; it does not assign social

[28]Here we restrict the sample to students for whom we observe parental income; see Table 1.

ties. Being assigned to a group with one additional top 1% peer does not mechanically imply one additional top 1% career-relevant contact. Students form different types of ties endogenously, and this sorting may differ by family background. Such within-group sorting does not invalidate the design, because it occurs after randomized assignment of peer composition. Instead, it is one channel through which assigned exposure may become career-relevant ties.

Random group-specific shocks of the type discussed for career similarities do not alter the interpretation of these estimates, since such shocks should be orthogonal to the treatment variable. However, group composition may affect students through channels unrelated to tie formation. For example, if a larger share of top 1% students changes the academic environment and thereby affects learning, α^G would also capture this channel. Although we cannot rule out all alternative mechanisms, we document that the share of top 1% peers does not affect students' academic outcomes.

The distinction between peer exposure and social connections is also central for interpreting heterogeneous effects. Even if exposure affects careers only through social ties, a larger effect of top 1% peer exposure among top 1% students can reflect either stronger conversion of exposure into ties or greater career value of the induced ties. The linear-in-means estimates do not separately identify these margins. They identify the reduced-form effect of assignment to a more elite peer environment, separately for students from top 1% and non-top 1% families.

Random group-specific shocks analogous to those discussed in relation to career similarities do not alter the interpretation of the results as these shocks should be orthogonal to the treatment variable. At the same time, it is possible that group composition affects students through channels unrelated to peer tie formation. For example, if the share of top 1% students in a group substantially alters the academic environment, thereby affecting student learning, α^G should also be interpreted as reflecting this channel. Although we cannot rule out all potential alternative mechanisms, we document that the share of top 1% peers does not affect students' academic outcomes.

The distinction between peer exposure and social connections is important for interpreting heterogeneous effects, even if the formation of social ties is the only way exposure manifests in career outcomes. A larger effect of top 1% peer exposure on students from top 1% families can reflect different margins: stronger conversion of exposure into social ties or greater career value of these induced ties. The linear-in-means estimates do not separately identify these channels. They identify the reduced-form effect of being assigned to a more elite peer environment, separately for students from top 1% and non-top 1% families.

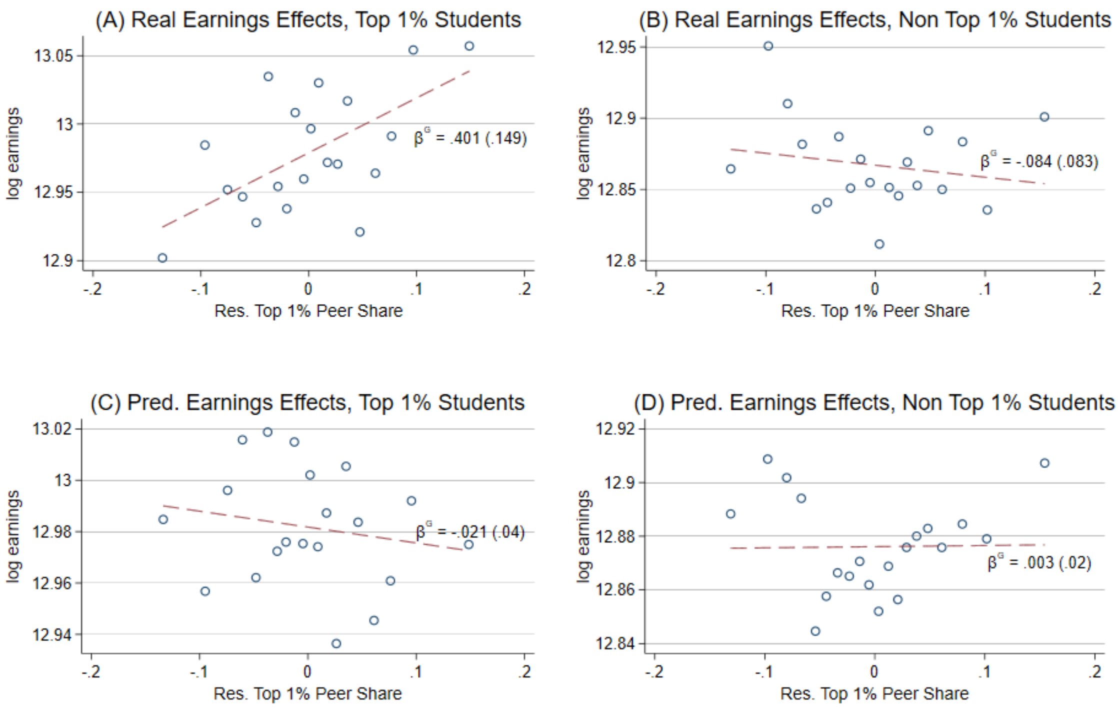

FIGURE 5
Effects of Top 1% Peers on Real vs Predicted Annual Earnings

Notes: The figure presents estimates derived from the model specified in Eq. 2 and run separately for top 1% and non-top 1% students. The sample is restricted to students with available income information for at least one parent, and the same set of students is used to compute the top 1% share. Real earnings are measured as the logarithm of annual earnings in 2015 Danish krona. Predicted earnings are constructed by regressing log real annual earnings on a set of controls. These controls include λ_{cit}^{G} from Eq. 2, second-degree polynomials of standardized high school GPA, log total parental income and years of education. Additional covariates comprise an indicator for being in the top 10% of high school GPA within the CBS matriculation cohort, as well as citizenship, high school track, municipality of residence, the number of gap years before matriculation fixed effects. The estimated coefficients from this regression are then used to predict log earnings. The graph displays a binscatter of real and predicted earnings against the top 1% peer share, residualized from λ_{cit}^{G}. Standard errors are clustered at the peer group level.

4.2 Results

Applying the framework in Eq. 2, Fig. 5 plots both real annual earnings and earnings predicted by a rich set of predetermined observables against the residualized top 1% peer share, separately for top 1% and non-top 1% students.[29] Panel (A) highlights the strong career effects of elite peers on students with similar backgrounds. Increasing the share of elite peers by 10 percentage points (equivalent to the interquartile range in our sample) corresponds to an increase in annual earnings by 4%. The magnitude is substantial when put into context of the gap between top 1% and non-top 1% students from Fig. 1. It implies that the 10 p.p. reduction in top 1% share leads to closing the 11% gap across the years by more than a third. To express the coefficient magnitude in terms of the absolute number of top 1% peers, we divide the coefficient by 34 (the average group size minus one). This calculation implies that having one additional elite peer in a tutorial group increases postgraduation annual earnings by approximately 1.2%. The relationship does not display evident non-monotonicities in the peer share. At the same time, the graph supports the hypothesis of asymmetric impacts on long-term career success based on students' social standing. Panel (B) shows no statistically significant effects for former CBS students without affluent family backgrounds. In line with the conditionally random nature of the assignment, using predicted annual earnings instead of real earnings provides additional evidence supporting the validity of our research design (Panels C and D). Regardless of elite background, the residualized elite peer share is not related to earnings, predicted by the set of covariates that on top of the fixed effect from Eq.2 includes second-degree polynomials of standardized high school GPA, parental income, years of education and indicator for being in the top 10% of high school GPA within the CBS matriculation cohort, citizenship, high school track, municipality of residence, and the number of gap years before matriculation fixed effects. Table A.8 in the Appendix further corroborates this finding by conducting a linear-in-means type balancing test, replacing career outcomes with various predetermined individual-level characteristics in Eq. 2.

Table 6 further paints a broader picture of the uneven career effects of elite peers by students' own family background. For students with top 1% fathers, a 10-percentage-point increase in the share of top 1% students in the assigned peer group raises real daily wages by almost 5% (1.4% per additional top 1% peer) and increases their rank in the within-birth-cohort earnings distribution by 1.1 points (.33 points per additional top 1% peer). Increased exposure to elite peers also moves top 1% students into higher-paying firms and occupations, which complements findings from Section 3.3.

Importantly, among students from wealthy family backgrounds, the share of top 1% peers has substantial effects on reaching the upper tail of the earnings distribution. A

[29] This figure follows the approach from Carrell et al. (2018).

TABLE 6
The Effect of Top 1% Peers on Career Outcomes

	Log Daily Wages	Earnings Rank	Top 10% Firm	Top 10% Occupation
Other x Peer Top 1% Share	-0.041	-1.658	0.072	-0.034
	(0.073)	(2.563)	(0.054)	(0.038)
Top 1% x Peer Top 1% Share	0.487***	11.380***	0.313***	0.215***
	(0.138)	(4.001)	(0.087)	(0.080)
Mean dep. var.: Top 1%	7.19	78.77	0.57	0.27
Mean dep. var.: Non Top 1%	7.08	75.54	0.51	0.21
$P(H_0 : \alpha^H = \alpha^L)$	0.001	0.005	0.021	0.011
Observations	218,401	220,937	215,623	190,827

	Top 10%		Top 1%	
	Early	Late	Early	Late
Other x Peer Top 1% Share	-0.007	-0.058	-0.022	-0.054
	(0.050)	(0.067)	(0.025)	(0.162)
Top 1% x Peer Top 1% Share	0.199***	0.187	-0.006	0.217**
	(0.084)	(0.123)	(0.063)	(0.098)
Mean dep. var.: Top 1%	0.42	0.56	0.11	0.16
Mean dep. var.: Non Top 1%	0.33	0.44	0.06	0.09
$P(H_0 : \alpha^H = \alpha^L)$	0.023	0.069	0.81	0.010
Observations	110,780	110,157	110,780	110,157

Notes: This table presents estimates derived from the model specified in Eq. 2. All regressions are estimated separately for students with top 1% fathers and without. The sample is restricted to students with income information for at least one parent, and the same set of students is used to compute the top 1% peer share. Panel A reports effects on log daily wages, earnings rank, and indicators for employment at top 10% firms and in top 10% occupations. Panel B reports effects on indicators for reaching the top 10% and top 1% of the earnings distribution, separately for early and late career periods. Earnings ranks are defined relative to the earnings distribution within a birth year cohort in a given year. Top firms and occupations are defined using average daily wages across firms and 4-digit occupations, respectively, in a given year. "Early" refers to the first 10 years after (potential) graduation. "Late" to the years after. Mean dependent variables are reported separately for students with fathers in the top 1% and for all other students. Standard errors are clustered at the peer-group level. *** p<0.01, ** p<0.05, * p<0.1.

10-percentage-point increase in top 1% peer share raises the probability of reaching the top 10% of the earnings distribution during the first 10 years after planned graduation by 2 percentage points, and raises the probability of reaching the top 1% later in the career by over 2 percentage points.[30] By contrast, students without top 1% fathers experience no comparable gains.

The magnitudes imply that elite-peer exposure can account for a sizable share of the career gaps documented in Fig. 1. A 10-percentage-point reduction in the top 1% peer share would reduce the earnings-rank gap across the years by more than a third, from 3.2 to 2.1; the daily-wage gap by around 40%, from almost 12% to 7%; the gap in top 10% firm employment by half, from 6 to 3 percentage points; the top 10% occupation employment by third, from 6 to 4 percentage points; the gap in early top 10% achievement by 23%, from 9 to 7 percentage points; and the gap in later top 1% achievement by 30%, from 7 to 5 percentage points.

As shown in Table 1, students with top 1% fathers tend to have a lower drop-out rate. It is possible that the career effects we observe are mediated by educational outcomes. On the other hand, being assigned to peers with very different social statuses could lead students to switch to another program. However, the results in Appendix Table A.9 do not provide evidence in support of this hypothesis. There are no significant effects on the final GPA at CBS graduation, the probability of graduating from the program, or any Bachelor or Master's program.[31]

We also test whether assignment to groups with a higher share of top 1% peers affects selection into the career sample. If lower-earnings-potential students were more likely to leave the sample when assigned to groups with more top 1% peers, our estimates could be biased upward. Appendix Table A.10 provides no evidence of such selection. The treatment has no discernible effect on inclusion in the career sample, registration as a Danish resident, or, conditional on residency, being either wage-employed or enrolled in education.

We next examine whether the estimated effect is specific to top 1% students and exposure to top 1% peers, rather than reflecting a broader income gradient. We divide students into quartiles of fathers' pre-matriculation income, where the top quartile overlaps heavily with fathers' national top 1% status, and construct peer shares separately for each group. With four income groups, the regression includes three peer-share variables. We then estimate the effects of these peer shares on future earnings separately for students in each quartile, using the same strategy as in Eq. 2. Appendix Table A.11 shows that the results are driven by exposure to students with fathers in the

[30] As Fig. A.8 shows, the early-career effect reflects a broad reallocation of students from lower to higher earnings brackets (but the very top), whereas the later-career effect is concentrated in the probability of reaching the top 1%.

[31] We find that this result is broadly in line with previous findings by Sacerdote (2001) and Michelman et al. (2022).

top quartile among students whose own fathers are also in the top quartile. Appendix Table A.12 further shows that the effect is specific to top 1% status defined through fathers' income; it is not significant when using top 1% mothers or top 10% fathers or mothers.

In the baseline specification, fathers' income rank is defined using average income over the five years before students' matriculation. Appendix Figure A.11 shows that introducing measurement error into fathers' income rank attenuates the estimated effect, either by reducing the number of years over which income is averaged (Panel A) or by adding noise directly to the income measure (Panel B). This pattern is consistent with random group assignment.[32] Panel C shows that the estimated effect also attenuates when parental income is measured further back in time from the year before matriculation. Panel D shows that the results are robust to excluding fathers in the lower and upper tails of the age distribution.

5 Wage Effects of Peer Transitions

5.1 Empirical Strategy

The previous sections show two related patterns. First, tutorial-group exposure generates career-relevant alumni links: students are more likely to work with assigned group peers, especially those from the wealthiest family backgrounds. Second, exposure to a larger share of top 1% peers generates substantial long-run career gains for students who themselves come from top 1% families. As shown above, one likely mechanism is improved access to higher-paying firms and occupations. However, it remains unclear whether obtaining a job through a peer connection is itself associated with benefits or losses, and whether these effects differ by students' backgrounds.

The existing literature does not provide a clear prediction for whether peer-connected job moves should raise or lower subsequent wages. Some studies find that network-based jobs are associated with higher wages or better matches (Marmaros and Sacerdote, 2002; Kugler, 2003; Dustmann et al., 2016), while others find wage penalties or mixed effects (Loury, 2006; Bentolila et al., 2010; Pellizzari, 2010). These differences may reflect distinct referral mechanisms: reducing uncertainty about worker *ex ante* productivity (Montgomery, 1991; Hensvik and Skans, 2016) or match quality (Simon and Warner, 1992; Brown et al., 2016; Dustmann et al., 2016; Glitz and Vejlin, 2021), improving incentives through monitoring or moral-hazard channels (Kugler, 2003; Heath, 2018), relaxing search frictions (Schmutte, 2015), or reflecting favoritism and access to

[32]As pointed out by Angrist (2014), measurement error in the linear-in-means setting can potentially bias peer-effect estimates in either direction. Feld and Zölitz (2017) show that under random group assignment, measurement error leads to attenuation bias. The test performed here is inspired by Carrell et al. (2018).

jobs workers would not otherwise obtain (Bandiera et al., 2009). Lester et al. (2021) show that referral effects depend on connection type: professional referrals primarily benefit higher-productivity workers and increase inequality, whereas family-and-friend referrals matter more for workers with weaker outside options and can reduce inequality. The alumni ties studied here do not map cleanly into either category, since they combine features of professional, friendship, and family-linked networks.

We examine whether transitions to firms where group peers work—peer transitions—are associated with wage gains or penalties. The key challenge is choosing an appropriate comparison group. Voluntary job-to-job transitions may generate wage gains even without peer interaction, so peer transitions must be compared to other job transitions. At the same time, if CBS graduates tend to work at high-paying firms, transitions to any CBS-connected firm may mechanically raise wages. We therefore use an event-study design comparing observably similar CBS graduates who move to firms with an incumbent group peer to those who move to firms with an incumbent cohort peer. This comparison holds fixed the presence of a CBS peer at the destination firm and exploits the fact that, ex ante, group peers and cohort peers work at similar firms.[33]

We implement a stacked event-study design around job-transition events. The event sample consists of transitions in which student i joins a firm where another student from the same cohort is employed. For each transition event e, we construct a panel in event time τ, covering five years before and five years after the transition. The same student may therefore appear in multiple events and, in some cases, multiple times in the same calendar year. We show below that the results are robust to restricting the treatment group to first-time group-peer transitions and the control group to never-treated students.

We estimate the following specification:

$$y_{eit} = \mu_e + \lambda_{\tau t} + \sum_{\substack{-5 \leq k \leq 5 \\ k \neq -1}} \gamma^k \mathbb{I}(\tau_{eit} = k)\text{GroupPeer}_e + \beta X_{et} + \epsilon_{eit} \tag{3}$$

where y_{eit} is the log daily wage for individual i in calendar year t around transition event e, and $\mathbb{I}(\tau_{eit} = k)$ is an indicator function equal to one when the observation occurs k years relative to the transition. $GroupPeer_e$ is an indicator equal to one if the destination firm contains at least one group peer, and zero if it contains cohort peers but no group peers. Event fixed effects, μ_e, absorb time-invariant differences across transition events, while calendar-by-event-time fixed effects, $\lambda_{\tau t}$, compare treated and control transitions observed in the same calendar year and at the same relative event time. The vector X_{et} includes second-degree polynomials in age and years since

[33] Appendix Fig. A.9 shows wage trajectories associated with different transition types. Group-peer transitions are associated with the largest daily-wage jump around the transition. They also exhibit higher wage growth before the transition and are more likely to occur early in the career.

matriculation, interacted with gender and Danish citizenship indicators.

The coefficients γ^{τ} trace the differential evolution of outcomes around group-peer transitions relative to cohort-peer transitions. Under parallel trends, post-transition coefficients, γ^{τ} for $\tau \geq 0$, capture the excess change associated with joining a group peer. The omitted category is the year before the transition, $\tau = -1$; absent anticipation, pre-transition coefficients should be statistically indistinguishable from zero. In some specifications, we summarize the estimates using the short-run effect γ^{0} and the average post-transition effect $\gamma^{\tau \in [1,5]}$. We restrict the sample to events at least five years after matriculation and require the student to be observed working in the year before the transition and at least five years after it. Transitions into first post-graduation jobs are therefore excluded. Standard errors are clustered at the individual level.

The central concern is non-random sorting of connected hires into firms (Dustmann et al., 2016). Both theory (Montgomery, 1991) and empirical evidence (Hensvik and Skans, 2016) suggest that workers hired through connections may differ from other hires, and this selection may vary by job and connection type (Lester et al., 2021). Event fixed effects account for worker heterogeneity that is invariant around the transition event.[34] The parallel-trends assumption further rules out shocks correlated with peer transitions. Although we cannot test this assumption directly, we report the conventional pre-trend test.

Destination firms may also be selected, both in theory (Galenianos, 2014) and empirically (Eliason et al., 2023). Comparing transitions to firms with CBS peers narrows the set of destinations. Given random group assignment, group peers should, on average, be located in similar firms as cohort peers, but this need not hold conditional on transition. Even among firms with CBS peers, firms relying more heavily on referrals may differ from others. To address this concern, we exploit cases in which multiple CBS students transition to the same firm at the same time, allowing us to compare group-peer and cohort-peer transitions to the same destination firm in the same period.[35]

Because we do not directly observe referrals or other peer interactions, this design should again be interpreted as an intention-to-treat strategy. We assume that transitions to firms with group peers are more likely to reflect such interactions than transitions to firms with cohort peers, and that this proxy affects outcomes only through that channel; see Dustmann et al. (2016) for a similar discussion. Although pre-matriculation group peers are not more similar than cohort peers, group exposure leads them to develop more similar careers. We therefore also test whether the effects hold when comparing former students transitioning *from* the same jobs.[36]

[34]Event fixed effects are more granular than worker fixed effects, which account only for heterogeneity that is time-invariant within the sample.

[35]This approach is more flexible than firm fixed effects alone because it also controls for firm-specific shocks that may be related to connected transitions.

[36]We conduct the full set of robustness checks with occupation, industry, firm, and workplace origin

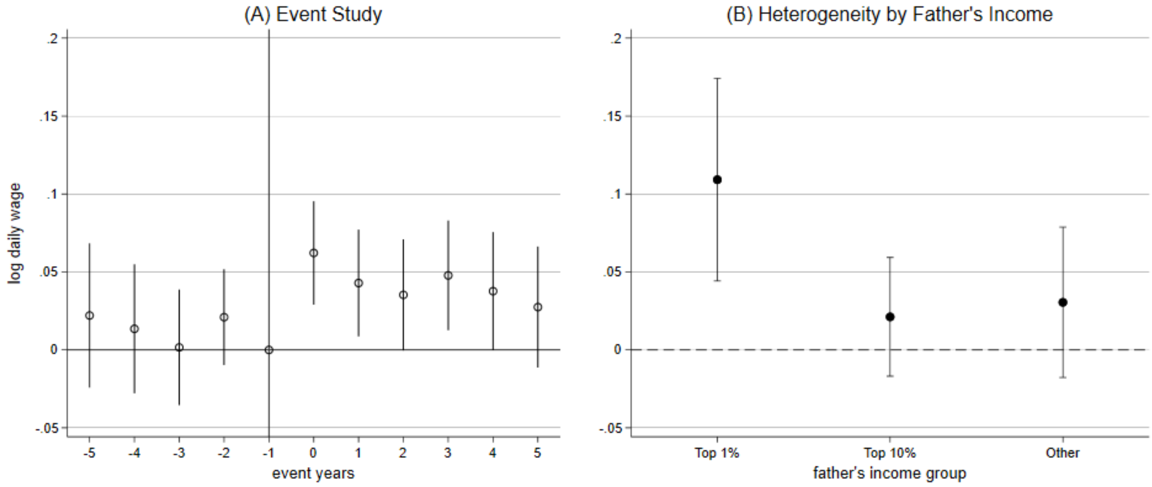

FIGURE 6
The Daily Wage Effect of Peer Transitions

Notes: Panel A presents coefficients from the event-study in Eq. 3 - the effect of joining a group peer on log daily real wages by a year relative to the transition. Panel B presents only the short run effect coefficients from the model where all treatment indicators and fixed effects are interacted with parental income group, based on father's income rank before matriculation. $p = 0.028$ for equality between "Top 1% and "Top 10%" effects and $p = 0.123$ for equality between "Top 1% and "Other". The sample is restricted to observations later than 4 years from matriculation and where student is observed employed at least between event years -1 and 5. The standard errors are clustered at the individual and the firm level. The vertical lines indicate 5% confidence intervals.

We are interested in whether returns to peer-connected transitions differ by family background. This heterogeneity requires care in interpretation. Even if the identifying assumptions hold, differences between top 1% and non-top 1% students may reflect two margins: differences in the payoff to an actual peer interaction, such as a referral, and differences in how well the group-peer transition proxy captures such an interaction. For example, if top 1% students are more likely to use peers in job search, or if they work in firms where referrals are more common and more consequential, then a transition to a firm with a group peer is likely to be a stronger proxy for an actual referral among top 1% students.

5.2 Results

In Fig. 6 (Panel A), we present the event study results using the specification in Eq. 3. The figure displays the estimated coefficients on the leads and lags of treatment. We find no statistically significant differences in pretrends between the treatment and control groups, while the treatment lags are significant and positive. This indicates that joining a firm with a group peer is associated with wage gains of around 6% compared

or destination fixed effects. Conditioning on the origin or destination job also makes the parallel-trends assumption less demanding.

to joining a firm with a cohort peer. The event-study graph suggests that the wage benefit is most pronounced in the year of the transition, gradually declining over the following 5 years.

We do not know which of the theoretical mechanisms discussed above is responsible for the observed pattern. However, if we interpret these findings through the lenses of Dustmann et al. (2016): referrals give employers more precise information about a worker's match-specific productivity before hiring. Referred workers are therefore initially better matched to firms, which generates higher starting wages and as firms and workers learn about true productivity over time, the informational advantage of the referral fades, so the wage premium should decline with tenure. Appendix Table A.13 presents complementary evidence showing that in line with this theory peer transitions also are associated with lower turnover.

Appendix Table A.14 examines the robustness of the observed wage effect. First, we demonstrate that the result is robust to different approaches for handling already-treated units. In our baseline specification of the stacked event study approach, the same student can appear in the treatment group multiple times, as they may join a firm with an incumbent group peer several times during their career path. Each of these instances is treated as a separate treatment event. Additionally, previously treated students can serve as controls; for example, a student who previously joined a firm with an incumbent group peer may later join a firm with an incumbent cohort peer. We show that the results remain robust even when we restrict treated events to only the first-time treatments and/or control events to never-treated students. Second, we find that even when treatment and control transition events lead to the same job (defined by occupation, industry, firm, or workplace), joining a group peer is still associated with wage gains. This alleviates concerns that the effects are driven by the composition of jobs where connected transitions occur. Lastly, the results are robust to restricting the comparison to transitions from the same job. This helps rule out the possibility that the observed pattern is explained by differences in previous career histories.

Most notably, the wage benefit is concentrated among workers from the wealthiest backgrounds, particularly those with a father in the top 1% of the Danish income distribution. The short-run effect for this group is higher than 10% for this group. However, for students with fathers in the top 10% and other students, we do not find any significant effect of peer transitions.[37] These findings might reflect both lower referral premiums for these groups (independently of the true mechanism behind these premiums) or lower reliance of non-top 1% students and firms where they tend to work on such hiring channels. In any case, these peer transitions premiums point to unequal

[37]We are not able to reject the hypothesis that the effects on turnover are the same for top 1% students and non top 1% students (Appendix Table A.13).

returns to alumni networks that reinforce inequality among students.[38]

6 Conclusion

What is the impact of social connections among business-school peers on individual career trajectories and mobility into top jobs? Do alumni networks primarily benefit students from similarly affluent backgrounds, or do they open paths to career success for students from less privileged families? To answer these questions, we leverage a unique research setting at Copenhagen Business School, where students were conditionally randomly assigned to tutorial groups. This assignment allows us to estimate causal effects of peer exposure on career outcomes. We combine these records with Danish administrative data containing detailed information on students' careers and family backgrounds.

We find substantial career similarity among former group peers, beyond the similarity observed among cohort peers. These excess similarities in industries and employers are driven by peers being more likely to work together at the same workplace, and the effects are especially pronounced among students from affluent families. Using a linear-in-means model, we further show that students with fathers in the top 1% experience significant career gains when assigned to tutorial groups with a higher share of peers from similarly privileged backgrounds. In contrast, we find no comparable effects for students from less privileged backgrounds. This suggests that the concentration of affluent students in the program primarily benefits students from the same socioeconomic background.

Further analysis of job transitions shows that alumni networks help students access higher-paying jobs. Comparing transitions to firms with group peers to transitions to firms with cohort peers, we find significant wage gains for students joining firms where group peers are already employed. These gains are again largest for students from top 1% families.

Overall, our study shows that social connections among university peers shape the career returns to business education. Prior work highlights that networks can play equalizing roles in other parts of the labor market: social ties matter in school-to-work transitions for lower-skill or vocational-track students (Kramarz and Skans, 2014; Ost et al., 2026), connected hiring is more prevalent in lower-paying labor-market segments (Eliason et al., 2023), and family-and-friend referrals may support workers with weaker employment prospects and reduce inequality (Lester et al., 2021). Our results highlight a different role of networks. In the context of elite alumni ties, social connections foster career advancement primarily among students from privileged backgrounds. This

[38]This also puts alumni ties studied here closer to business connections than family/friends ties in Lester et al. (2021) classification.

limits the equalizing potential of elite business education and may contribute to the persistence of advantage.

The findings suggest that expanding access to educational pathways leading to top jobs may not be sufficient for students without privileged family backgrounds. Social interactions among students shape the returns to education, and further research should examine interventions that make elite educational networks more valuable for less advantaged students.

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Supplementary Appendix

A Business Economics at CBS 1986–2006

Copenhagen Business School (CBS) is a large public institution in Copenhagen, Denmark. Established in 1917, it was regulated specifically as a business school rather than a university throughout the sample period.[39] Our study focuses on CBS's largest program, a three-year degree in Business Economics (Danish: HA almen). Business Economics has been offered at CBS since 1929 and as a three-year program since 1951. Throughout the sample period, the degree was equivalent to a Bachelor of Science in the United States, permitting admission into Master's programs; this equivalency was formally codified by the Bologna process in 1993. In 1986, 80–90 percent of graduates continued to a Master of Science in Economics and Business Administration or a Master of Science in Business Economics and Auditing (cand.merc. or cand.merc.aud.), programs at the same academic level as Master's degrees at Danish universities. Like all Danish study programs, the Business Economics program was tuition-free, and students were eligible for government-funded stipends.

During the sample period, CBS enrolled approximately 600 students in the program each year. Applications were processed through Denmark's centralized admissions system, which manages all applications to higher education in Denmark. Admission was based on high school grade-point average (GPA). The Business Economics program at CBS is the largest Business Economics program in Denmark and has consistently been the most selective: throughout the period under study, and continuing to the present day, the program received more applications than its capacity, and the admission cutoff GPA for this program has consistently been the highest among all Business Economics programs in Denmark.

Approximately 75 percent of our sample holds an academic-track high school qualification; around 96 percent have Danish citizenship, with approximately four percent from Norway or Sweden.[40] The program operated under two successive study regulations—a 1986 regulation governing the early cohorts and a 1995 regulation preserving the same overall structure.

Students at CBS were responsible for finding and arranging their own housing. CBS Academic Housing owned some housing units, and CBS disposed over rooms at a few dorms in Copenhagen. These units, however, were exclusively for international exchange students and visiting or newly appointed international faculty, not for regular students at CBS (Copenhagen Business School, 2026).

[39] CBS became one of Denmark's eight universities in 2007.

[40] Students with an AP degree in Marketing Management (*Markedsføringsøkonom*) could complete the degree in two years through credit transfer. CBS assigned these students to a dedicated tutorial group, and they are excluded from our sample.

The information in the remainder of this section is based on student handbooks for the Business Economics program at CBS during our period of study [41] and interviews we conducted with various current staff in the CBS administration, many of whom were also employed at CBS during our period of study.

Tutorial groups.

The central institutional feature of the Business Economics program is the organization of students into tutorial groups (Danish: *stamhold* or *studiehold*) of approximately 35 students. These groups serve as the primary unit for organizing both academic life and social life throughout the entire three-year program. The study guidelines in effect across the sample period describe the tutorial group as "the basic unit and thus your closest contact point throughout the study,"[42] and program design was explicitly oriented toward fostering a strong group atmosphere from the outset.

According to the study guides, group composition remained fixed for the full three-year program. Changes were permitted only in exceptional circumstances, such as ongoing medical treatment, elite-athlete training commitments, or documented personal safety concerns. Scheduling preferences and personal compatibility were not accepted grounds. When sustained dropout reduced a group's size materially, groups could be merged, but only after the first year of studies; no mergers took place during the first year itself. Group numbers carried no stable identity across cohorts: the same number in two consecutive years did not imply the same teaching assistants, rooms, or time slots.

We use the initial group assignment in our analysis, but our data allows us to observe which tutorial group students belong to by semester over time. We use information on subsequent group changes and mergers for cohorts 1996–2006 as for the earlier cohorts this data is less reliable. In this data we see that all group merges happen after the first and second year of studies and that only around 2–3 percent of students (excluding group mergers) ever change tutorial group. Thus the administrative data align well with the study handbooks and with interviews with CBS administrators.

Group assignment. Incoming students were assigned to tutorial groups by the CBS administration after the final application deadline, with the entire cohort placed in a single batch. The timing of a student's application, therefore, played no role in group assignment. The procedure was manual and relied only on the information derivable from each student's civil registration number (CPR number): gender, age, and whether the student held a Danish social security number (used as a proxy for nationality). The precise procedure varied across years, and no documentation of

[41] When we mention examples they are from Copenhagen Business School Studievejledningen (2004) which is the latest handbook that we have.

[42] This was also emphasized in interviews with current members of the CBS administration who were also employed in the administration during some of our sample period.

the specific algorithm survives, but the CBS administration has confirmed that the main objectives were to balance gender across groups, distribute non-Danish students across groups, and place older students in designated senior groups. High-school GPA, prior school, municipality of residence, parental background, and other socioeconomic characteristics were not used in group assignment. Repeating students were typically, though not always, placed in the same group number again for practical reasons, conditional on available capacity. We drop repeating students from our analysis sample.

Teaching assistants. Each course within the tutorial group was led by a teaching assistant (TA). TAs were selected by the course coordinator, with performance in their own studies likely an important criterion. TAs also typically worked full-time in a role relevant to the course subject and taught alongside their main employment. No administrative records of TA assignments to groups exist for our sample period.

TAs operated within clearly defined boundaries: they were required to cover material aligned with the course's lectures, and had limited discretion over content or assessment. There was a deliberate institutional effort to ensure teaching quality was equal across tutorial groups, and to ensure consistent grading, course coordinators held joint grading meetings with all TAs for a given course, in which a common set of exam responses was graded collectively to align standards before individual marking began.

Curriculum and teaching. The Business Economics curriculum was organized into four thematic blocks—social sciences, business economics, technical tools, and a free business-related block—with approximately 85 percent of required coursework fixed and shared across the cohort. The first year comprised strictly mandatory courses.[43] In the final semesters, students could choose three elective courses, covering approximately 15–25 percent of total program hours. Students who went on an exchange typically did so during the final semesters. Students finalized their education by writing a bachelor's thesis. During our study period, students were expected to spend approximately 40 hours per week on their studies, of which about 16 were scheduled teaching hours in the first year. The handbook explicitly mentions what students have to prepare at home for exercise sessions. It involves solving assignments at home, group projects, and preparation of presentations. Total in-class hours in the first academic year were approximately 470 in 1986 and 459 in 1995. Attendance in classes and lectures was not mandatory, but highly encouraged.

Teaching consisted of cohort-wide lectures and exercise classes within the tutorial groups. During the first year, the share of teaching that took place within tutorial

[43] For example the first-year curriculum in 2004/05 consisted of: Company Decision-Making Processes (15 ECTS), Empirical Economics (20 ECTS), Business Financial Management (8 ECTS), Information Technology (8 ECTS), Macroeconomics and Trade Theory (8 ECTS), Disciplinary Philosophy of Science (3.75 ECTS), and Business Law (9 ECTS).

groups in a classroom format was higher than in the second and third years. Teaching was standardized across all groups: the same curriculum, assignments, and examinations applied throughout the cohort, and exams were graded at the cohort level.

Tutorial groups were not assigned dedicated classrooms, and students had to move classrooms for the different courses. Scheduling was largely driven by TA availability rather than any systematic design: tutorial groups were assigned to either morning or afternoon slots depending on when TAs were available, a split that arose because CBS faced room shortages during the 1980s and 1990s.

The study handbook indicates that CBS students were in demand among employers during the period. Students were encouraged to orient themselves towards the labour market through media, project collaborations with firms, student jobs, and career events. It also mentions that CBS had the most extensive bank of student jobs in Denmark at the time, although students were encouraged to spend at most 10–15 hours a week on their student jobs, as it could negatively affect their learning and normal progression.

To measure students' involvement in various elective courses and academic activities we use a database that registered students' activities and grades during the program and the subsequent Master's studies. The primary use of the database was to generate diplomas at the end of studies and alongside courses it contains activities like exams, group projects and thesis work. We restrict our analysis to entries that correspond to specific activities that took place in group environments (like elective courses).

Within-group interactions. The CBS administration placed significant emphasis on social cohesion within tutorial groups from the very start of the program. Upon enrolling, new students participated in a *rus* period; a structured introductory phase at the beginning of the first semester that focused on socializing with group peers, receiving an introduction to business-economics problem formulation (including solving a business case), and learning to navigate the institution. The central event of this period was the *rustur*: an organized cabin trip, typically to a location elsewhere in Denmark, for the tutorial group together with a small number of senior students further along in the Business Economics program who served as guides (*rusvejledere*). The rusvejledere led students through the social and practical aspects of this introductory period and played an active role in initiating the formation of reading groups, by encouraging students to form groups and by assisting students who were unable to find one independently.

Within tutorial groups, students were strongly encouraged to form smaller reading groups of three to five students. The program guidelines across the sample period describe reading group membership as "almost a necessity to survive in the study" and encouraged students to "experiment with multiple group constellations throughout the first year." These groups served as platforms for collaborative problem-solving, discussion of course material, exchange of notes, and joint work on home assignments and group assignments. Reading groups were not organized by the CBS administration; the process was student-driven, although the administration actively supported it, both through the rusvejledere system at the outset and by assisting any student who struggled to find a group. No administrative records of reading group membership were collected. The study handbook describes some good uses for the reading groups as going through course material, sharing notes, planning projects, and sharing summaries when someone was absent. This highlights the importance of reading groups in daily activities.

In practice, reading groups are not fixed over the course of the program. Many students remained in the reading group they formed during the *rus* period throughout their studies, while others changed groups as their studies progressed. It was not uncommon for students to belong to one reading group for some courses and a different group for others. The reading groups often served as the unit for group projects and group assignments.

Employer-facing activities were organized at the institutional level rather than through tutorial groups. A yearly career day brought companies to CBS and was organized for the entire student body, with no group- or cohort-specific activities. Firms separately sponsored the graduation ceremony and awarded prizes for the best bachelor's theses; sponsors received information on the top-ten students by grade in return for this.[44] A career guidance counselor function existed at CBS throughout the period, but counseling was not organized by tutorial group.

Beyond the introductory activities, the recurrent exercise classes and project work tied to the curriculum, additional social and academic activities at the tutorial group level were student-initiated.

Comparison with US-style business school sections.

Tutorial groups at CBS share structural similarities with sections at US business schools and are closest to the MBA sections at Harvard Business School (HBS) (Lerner and Malmendier (2013) and Shue (2013) study these MBA sections). Our setting mainly differs in three ways.[45] First, CBS tutorial groups are smaller, with approximately 35 students compared with about 90 students per HBS section. Second, CBS tutorial groups remained together for more of the curriculum: students took nearly all classes together during the three year program, except a small number of third year electives.

[44] There are no records of the sponsor firms.

[45] See the study handbook for the 2005 cohort of the Business Economics program at CBS (Copenhagen Business School Studievejledningen, 2004) and the current study handbook for the HBS MBA program (Harvard Business School, 2026) for a detailed description of the programs.

Third, teaching and grading were more standardized at CBS. While lectures were for the full cohorts, during classes TAs go through exercises and other material related to the lectures, with little room to shape the material and teaching themselves. Grading is shared between course coordinators and TAs, final grades are only based on the final exams, and grading is highly standardized within the cohort, for example TAs meet with the course coordinator and grade a small set of exams to align grading before grading the full set of exams. The teaching and grading for HBS MBA sections while being standardized, is likely more varied than in our setting as grading is based on different activities such as class participation, midterms, and final exams and section faculty are encouraged to use "...their own research and real-world experience to introduce innovative ideas and approaches to learning" (Harvard Business School, 2026).

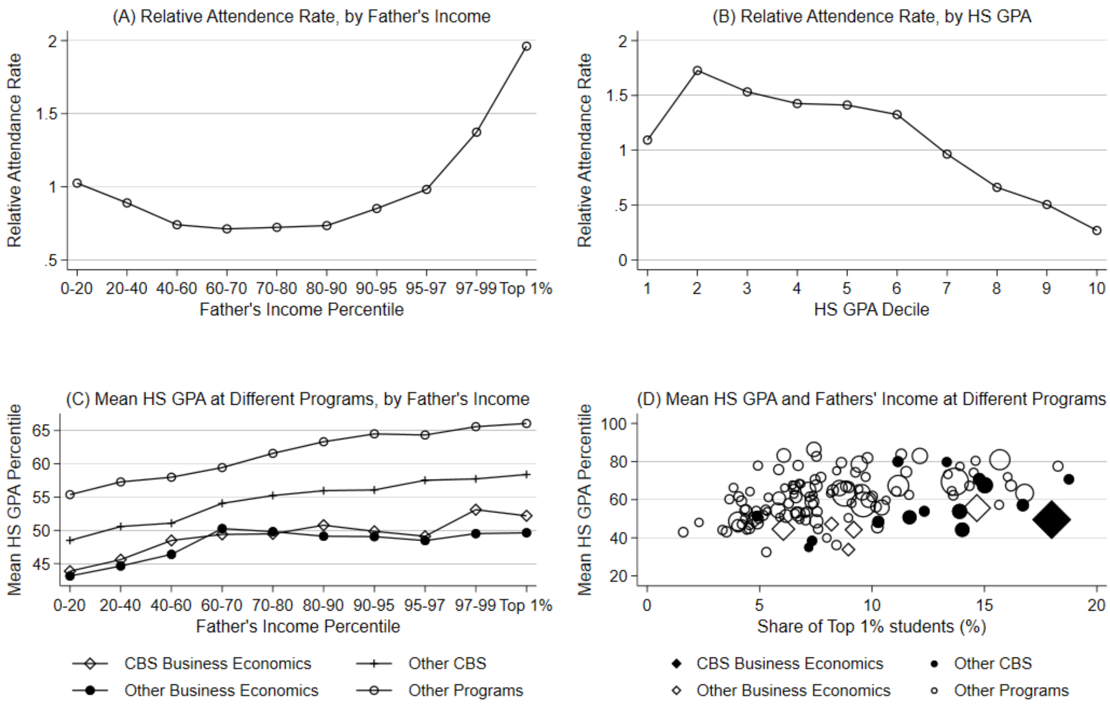

FIGURE A.1
Father's Income and High School GPA: CBS Business Economics vs Other Programs

Notes: The figure presents descriptive statistics for the father's income and high school GPA of students in CBS Business Economics program and other Bachelor programs in Denmark. Programs are defined irrespective of students' graduation status. The sample is restricted to students from matriculation cohorts 1998-2006, where the first cohort is restricted by the availability of comprehensive high school GPA data. Panel (A) shows the relative attendance rate at the program by father's income group and Panel (B) by high school GPA decile. Relative attendance rate is defined as ratio of the share of students in a given group at the program to the corresponding share in the population of bachelor students in the corresponding cohort. Panel (C) plots mean high school GPA by father's income group for different programs. Income groups are defined using average income across 5 years prior to matriculation. Panel (D) shows a scatter plot of mean high school GPA and share of students with fathers in top 1% at all Bachelor programs with more than 50 students in each cohort. The size of each dot is proportional to the average program size.

TABLE A.1

Career Similarities: Sample Selection

All Pairs	Sample	Registered in DK	Wage Employed	In Education	In Education (>2 years)
Same group	0.0620	0.0415	0.0330	0.0152*	0.00216
	(0.0364)	(0.0320)	(0.0319)	(0.00791)	(0.00437)
P-value	0.105	0.215	0.322	0.0766	0.645
Baseline	66.62	87.62	76.03	2.365	0.291
R-squared	0.123	0.213	0.0879	0.236	0.0362
Observations	74,740,981	74,740,981	65,499,572	65,499,572	58,429,385
Top 1% Pairs					
Same group	-0.432	-0.134	-0.367	0.0227	-0.0213
	(0.366)	(0.253)	(0.297)	(0.0530)	(0.0192)
P-value	0.262	0.611	0.232	0.668	0.288
Baseline	67.11	89.80	74.73	2.451	0.250
R-squared	0.148	0.161	0.148	0.307	0.0599
Observations	2,506,383	2,506,383	2,251,088	2,251,088	2,021,524
Other Pairs					
Same group	0.0791	0.0470	0.0475	0.0157*	0.00297
	(0.0456)	(0.0379)	(0.0392)	(0.00820)	(0.00412)
P-value	0.108	0.236	0.250	0.0735	0.496
Baseline	66.61	87.55	76.08	2.362	0.292
R-squared	0.126	0.217	0.0895	0.235	0.0364
Observations	72,234,598	72,234,598	63,248,484	63,248,484	56,407,861

Notes: The table presents the results of sample selection tests based on the baseline specification of Eq. 1, for all student pairs, as well as separately for pairs where both students belong to the Top 1% and for all other pairs. Baseline values and coefficients are multiplied by 100 to represent percentage points. Outcome variables include indicators equal to one if both students are: in our career sample during a specific year ('Sample'), registered as Danish residents ('Registered in DK'). Conditional on being Danish residents, additional indicators are: wage-employed ('Wage Employed'), in education ('In Education'), or in education, excluding the first two years after potential graduation from CBS Business Economics ('In Education (>2 years)'). Standard errors, shown in parentheses, are clustered at the cohort level. P-values indicate the significance of coefficients, calculated using wild cluster bootstrap with 9,999 replications at the matriculation cohort level. *** p<0.01, ** p<0.05, * p<0.1

TABLE A.2 Peer Workplace Exposure: Observed vs Simulated Means

	All	Within Top 1%	Within 91–99%	Within Bottom 90%
Panel A: First Workplace with Peer				
Sim. Mean	0.048	0.010	0.023	0.019
Sample Mean	0.077	0.027	0.038	0.033
P-value	0.000	0.000	0.000	0.000
Panel B: Ever with Peer, 10 Years After Graduation				
Sim. Mean	0.179	0.051	0.094	0.072
Sample Mean	0.223	0.084	0.116	0.088
P-value	0.000	0.000	0.000	0.000
Panel C: Share of Years with Peer				
Sim. Mean	0.046	0.012	0.022	0.017
Sample Mean	0.059	0.020	0.027	0.020
P-value	0.000	0.000	0.005	0.026
Panel D: Number of Peers per Workplace				
Sim. Mean	1.161	1.026	1.059	1.068
Sample Mean	1.192	1.015	1.052	1.091
P-value	0.064	0.527	0.587	0.222

Notes: This table compares observed peer workplace exposure with simulated exposure under random assignment. Columns report results for all students ("All"), for those with fathers in the top 1% of the national income distribution ("Top 1%"), between the 91st and 99th percentiles ("91–99%"), and below the 91st percentile ("Bottom 90%"). Panel A reports the probability that a student shares the first post-graduation workplace with a peer. Panel B reports the probability of working with a peer at least once. Panel C reports the share of observed post-graduation years spent working with peers. Panel D reports the average number of peers at the workplace conditional on having at least 1. "Sample Mean" is the observed mean in the data. "Sim. Mean" is the average of 9,999 simulated means obtained from tutorial-group reassignments that preserve group sizes and the joint distribution of gender, Danish nationality, and age at matriculation. P-values are upper-tail randomization p-values, defined as the share of simulated means that are weakly larger than the observed sample mean.

TABLE A.3
Career Similarities: New matches

	Baseline	Only New Matches	Joining an Incumbent	Simultaneous Move
Same group	0.0742***	0.0627***	0.0252***	0.0374***
	(0.0092)	(0.0086)	(0.0038)	(0.0067)
P-value	0.000	0.000	0.000	0.000
Effect (in %)	39.08	40.67	33.03	48.19
Baseline	0.19	0.15	0.08	0.08
R-squared	0.0035	0.0063	0.0056	0.0064
Observations	43,574,314	14,728,981	14,728,981	14,728,981

Notes: This table presents estimates from the linear probability model specified in Eq. 1 for directed dyads. Columns in the table correspond to regression results for distinct subsamples and outcome variables. "Baseline": all observations and an indicator for working together in the same workplace. "Only New Matches": only years when student i joins a new workplace and incorporates an indicator for working together in the same workplace. "Joining an Incumbent": only years when student i joins a new workplace and includes an indicator for joining a workplace where student j is an incumbent. "Simultaneous Move": only years when student i joins a new workplace and features an indicator for both students i and j joining a new workplace simultaneously. Standard errors, enclosed in parentheses, are clustered at the cohort level. Baseline values and coefficients have been magnified by a factor of 100 to represent percentage points. The reported p-values indicate the significance of coefficients, determined using wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE A.4
Career Similarities: Educational Choices

	Graduate from CBS HA	Graduate from any HA	Graduate from any Bachelor's	Switch to other program
Same group	0.0843	0.0797	0.0820	0.0115
	(0.0815)	(0.0828)	(0.0746)	(0.0158)
P-value	0.332	0.360	0.305	0.496
Effect (in %)	0.182	0.170	0.154	1.294
Baseline	46.43	46.98	53.34	0.89
Observations	7,219,546	7,219,546	7,219,546	7,219,546

	Master's start	Master's graduate	Master's program	Master's institution
Same group	0.0377	-0.0178	0.243**	0.116
	(0.0716)	(0.0491)	(0.101)	(0.0838)
P-value	0.602	0.728	0.0207	0.188
Effect (in %)	0.072	-0.059	0.825	0.284
Baseline	52.08	30.36	29.50	40.70
Observations	7,219,546	7,219,546	7,219,546	7,219,546

Notes: The table presents estimates derived from the linear probability model as specified in Eq. 1 for indicator variables equal to 1 if both students graduate from the program, graduate from any Business Economics program, graduate from any Bachelor's program, switch to another program, start any Master's program, graduate from any Master's program, start the same Master's program, and start any Master's program at the same institution. Standard errors, enclosed in parentheses, are clustered at the cohort level. Baseline values and coefficients have been magnified by a factor of 100 to represent percentage points. The reported p-values indicate the significance of coefficients, determined using wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE A.5
Career Similarities and Master's Degree Choices

	Same Industry	Same Occupation	Same Firm	Same Workplace
Panel A: Imputed Same Jobs				
Same Group	0.0031**	0.0082**	0.0005**	0.0005**
	(0.0013)	(0.0034)	(0.0002)	(0.0002)
P-value	0.034	0.025	0.030	0.026
Effect (in %)	0.17	0.25	0.15	0.33
Baseline	1.85	3.25	0.32	0.14
Observations	51,589,920	39,023,129	51,590,971	47,393,276
Panel B: Same Master's "Effect"				
Same Masters	1.111***	2.530***	0.173***	0.169***
	(0.083)	(0.179)	(0.020)	(0.015)
P-value	0.000	0.000	0.000	0.000
Effect (in %)	74.05	102.40	64.25	187.50
Baseline	1.50	2.47	0.27	0.09
Observations	51,609,031	39,039,953	51,610,082	47,411,257

Notes: This table combines estimates from two sets of regressions. Panel A presents the effect of being assigned to the same group on the probability of working in the same job imputed by the same Master's degree. Panel B shows the imputation stage - predicting working together by studying in the same Master's. Observations for occupations and industries are categorized at the 4-digit level. Standard errors, enclosed in parentheses, are clustered at the cohort level. Baseline values and coefficients have been magnified by a factor of 100 to represent percentage points. The reported p-values indicate the significance of coefficients, determined using wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.6 Career Similarities: Peer Transitions by Occupation Pay Rank

	Top 1% Pairs		Other Pairs	
	Top 10% Occup.	Other Occup.	Top 10% Occup.	Other Occup.
Same Group	0.0372**	0.00880***	0.0465	0.0218***
	(0.0161)	(0.00235)	(0.0276)	(0.00488)
P-value	0.0490	0.00400	0.106	0.000
Effect (in %)	93.45	27.48	45.87	26.54
Baseline	0.0398	0.0320	0.101	0.0822
Observations	816,752	23,067,498	816,752	23,067,498

Notes: This table presents estimates from the linear probability model specified in Eq. 1 with directed dyads. The table reports results separately for two samples: "Top 1% Pairs," which includes student pairs in which both fathers are in the top 1% of the national income distribution, and "Other Pairs," which includes all remaining student pairs. The outcome is an indicator for whether the focal student joins a workplace where a peer works. Columns distinguish whether the peer works in one of the top 10% occupations by daily wage ("Top 10% Occup.") or in any other occupation ("Other Occup."). Standard errors, shown in parentheses, are clustered at the cohort level. Coefficients and baseline values are multiplied by 100 and reported in percentage points. P-values are computed using wild cluster bootstrap at the matriculation-cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.7
Career Similarities by Parental Background

	Same Industry	Same Occupation	Same Firm	Same Workplace
Top 1% Pair	0.215**	0.569***	0.001	0.017
	(0.088)	(0.132)	(0.029)	(0.013)
P-value	0.024	0.000	0.969	0.190
Difference (in %)	11.740	17.680	0.331	12.530
Baseline	1.828	3.219	0.319	0.139
Observations	47,843,645	36,219,332	47,844,640	43,956,180

Notes: This table presents estimates from regressions analogous to Panel A of Table 3, but excluding all within tutorial group pairs. Occupations and industries are categorized at the 4-digit level. Observations for occupations are available only within the 1994–2016 period, and only non-imputed values are used. The treatment variable is an indicator equal to one if both students in the dyad have fathers in the top 1%. Standard errors, enclosed in parentheses, are clustered at the cohort level. Baseline values and coefficients have been multiplied by 100 to represent percentage points. The reported p-values indicate the significance of coefficients, determined using wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.8
The Linear-in-Means Balancing Test

	HS GPA	Top GPA	Academic HS Track	Business HS Track
Other x Peer Top 1% Share	0.050	0.017	-0.038	0.036
	(0.045)	(0.118)	(0.065)	(0.065)
Top 1% x Peer Top 1% Share	0.049	0.106	-0.020	0.082
	(0.081)	(0.196)	(0.114)	(0.110)
Observations	11,560	11,560	11,804	11,804
	Gap Years	**Av Municipal Inc Rank**	**Father's Education**	**Mother's Education**
Other x Peer Top 1% Share	-0.117	-0.730	-0.137	-0.250
	(0.147)	(0.480)	(0.456)	(0.400)
Top 1% x Peer Top 1% Share	0.058	-1.317	0.521	-0.345
	(0.217)	(0.992)	(0.700)	(0.694)
Observations	11,264	11,638	10,654	11,173

Notes: This table presents the results of a balancing test derived from the model as specified in Eq. 2 in a cross-section of students with predetermined students' characteristics as outcome variables. The sample consists solely of students with available income information for at least one parent. The same set of students is used to compute the top 1% share. Tests are conducted for various student background variables. HS GPA: high-school GPA, standardized using the distribution for high school graduates of the same year from the academic track. Top GPA: an indicator variable equals to 1 if student's high school GPA is in top 10% of the corresponding CBS matriculation cohort. Academic HS Track: an indicator for graduation from the academic high school track. Business HS Track: an indicator for graduation from the business high school track. Gap Years: the number of years between high school graduation and CBS matriculation. Average Municipal Income rank: average residents' income rank of the municipality residence before matriculation. Father's Education: the number of years of education completed by the father. Mother's Education: the number of years of education completed by the mother. All regressions are run separately by own top 1% status. Standard errors are clustered at the peer group level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE A.9

The Effect of Top 1% Peers on Education Outcomes

	CBS GPA	CBS Graduate	Any Bachelor Degree	Any Master Degree
Other x Peer Top 1% Share	0.031	0.036	0.047	-0.014
	(0.185)	(0.065)	(0.066)	(0.074)
Top 1% x Peer Top 1% Share	0.049	0.145	0.094	0.077
	(0.326)	(0.127)	(0.120)	(0.138)
$P(H_0 : \alpha^H = \alpha^L)$	0.961	0.447	0.729	0.550
Observations	7,608	11,295	11,295	11,295

Notes: This table presents estimates derived from the model as specified in Eq. 2 but in a cross-section of students. The sample consists solely of students with available income information for at least one parent. The same set of students is used to compute the top 1% share. The columns in this table correspond to distinct outcome variables. CBS GPA: the grade-point average for program graduates (not applicable to dropouts). CBS graduate: an indicator for program graduation at any point post the matriculation year. Any Bachelor Degree: graduating from any Bachelor program in Denmark following the matriculation year. Any Master Degree: graduating from any Master's program in Denmark after the matriculation year. All regressions are run separately by own top 1% status. Standard errors are clustered at the peer group level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.10

The Effect of Top 1% Peers, Sample Selection

	Sample	Registered in DK	Wage Employed	In Education
Other x Peer Top 1% Share	-0.019	-0.033	0.012	-0.011
	(0.043)	(0.027)	(0.032)	(0.015)
Top 1% x Peer Top 1% Share	-0.098	-0.071	-0.047	0.008
	(0.076)	(0.048)	(0.062)	(0.025)
Observations	202,294	202,294	194,626	194,045

Notes: The table presents estimates derived from the sample selection test, based on the model outlined in Eq. 2. The sample comprises only students with income information available for at least one parent, and this same set of students is employed to compute the top 1% share. Outcome variables include indicators equal to one if a student is: in our career sample during a specific year ('Sample'), registered as a Danish resident ('Registered in DK'). Conditional on being a Danish resident, additional indicators are: wage-employed ('Wage Employed') and in education ('In Education'). All regressions are run separately by own top 1% status. Standard errors are clustered at the peer group level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.11 The Effect of Peer Background Composition on Career Outcomes

	Father's Income Group			
	Q4	Q3	Q2	Q1
Peer Share Q4	0.416**	-0.165	-0.236	0.027
	(0.188)	(0.151)	(0.162)	(0.198)
	[0.027]	[0.276]	[0.146]	[0.892]
Peer Share Q3	0.215	0.067	-0.052	0.053
	(0.162)	(0.136)	(0.136)	(0.149)
	[0.186]	[0.626]	[0.700]	[0.723]
Peer Share Q2	-0.068	0.035	-0.014	-0.167
	(0.172)	(0.140)	(0.135)	(0.148)
	[0.693]	[0.801]	[0.917]	[0.259]
Observations	44,913	55,170	57,639	63,215
R-squared	0.431	0.420	0.406	0.394
F-statistic	2.956	0.878	0.857	0.882
P-value: joint test	0.032	0.452	0.464	0.451

Notes: This table presents estimates derived from the model specified in Eq. 2. Students are divided into four equally sized groups based on father's income before matriculation. Columns report separate regressions by the student's own father's income group. Q4 corresponds to the highest income group, Q1 to the lowest. The explanatory variables are leave-one-out peer-group shares of students from the second, third, and fourth father-income groups; the first group is the omitted category. Standard errors are shown in parentheses. Wild-cluster bootstrap p-values are shown in brackets. The sample is restricted to students with income information for at least one parent, and the same set of students is used to compute peer shares. Standard errors are clustered at the peer-group level. The F-statistic and corresponding p-value test the joint significance of the three peer-share variables. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.12

The Effect of Top 1% Peers on Career Outcomes, Alternative Treatment Definitions

	(1)	(2)	(3)	(4)
Other x Peer Top 1% Share (fathers)	-0.0842			
	(0.0841)			
Top 1% x Peer Top 1% Share (fathers)	0.401***			
	(0.147)			
Other x Peer Top 1% Share (mothers)		0.0288		
		(0.202)		
Top 1% x Peer Top 1% Share (mothers)		0.790		
		(1.090)		
Other x Peer Top 10% Share (fathers)			0.0494	
			(0.110)	
Top 10% x Peer Top 10% Share (fathers)			-0.0479	
			(0.0700)	
Other x Peer Top 10% Share (mothers)				0.0713
				(0.0720)
Top 10% x Peer Top 10% Share (mothers)				-0.0416
				(0.152)
Observations	220,937	220,937	220,937	220,937
R-squared	0.369	0.352	0.374	0.369

Notes: This table presents estimates derived from the model specified in Eq. 2 using alternative definitions of the treatment variable. The sample consists of students with available income information for at least one parent, and the same set of students is used to compute the peer variables. The outcome variable is earnings rank in all columns. Column (1) uses fathers' top 1% status to define both own top 1% status and the peer top 1% share. Column (2) uses mothers' top 1% status. Column (3) uses fathers' top 10% status instead of top 1% status. Column (4) uses mothers' top 10% status. Across all definitions, parental disposable income is averaged across all available observations in the five years before matriculation. All regressions are run separately by own top-income status. Standard errors are clustered at the peer-group level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.13 The Effect of Peer Transitions on Turnover, by Family Background

	Firm Change		
	All	Top 1%	Other
Year of Transition	0.000569	-0.0160	-0.00104
	(0.0113)	(0.0263)	(0.0112)
1–5 Years After	-0.0154**	-0.0138	-0.0146*
	(0.00725)	(0.0165)	(0.00752)
Observations	92,910	17,191	89,343

Notes: This table presents estimates derived from the model specified in Eq. 3. Columns report results separately for all students, students with fathers in the top 1% of the national income distribution, and all other students. The outcome variable is an indicator for changing firm in the following year. The reported coefficients correspond to peer transitions measured one year after the transition and two to five years after the transition. All regressions include transition-event fixed effects, calendar-year-by-event-year fixed effects, and second-degree polynomials in age and years since matriculation fully interacted with female and Danish citizenship indicators. The sample is restricted to observations more than four years after matriculation and to students observed in employment at least from event year −1 through event year 5. Standard errors, shown in parentheses, are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.14 The Effect of Peer Transitions, Robustness

Panel A: Alternative Treated and Control Groups				
	Baseline	First Time Treated	Never Treated Controls	Both
Year of Transition	0.0609***	0.0631***	0.0661***	0.0673***
	(0.0164)	(0.0182)	(0.0168)	(0.0186)
1–5 Years After	0.0372**	0.0507***	0.0402**	0.0525***
	(0.0146)	(0.0156)	(0.0157)	(0.0167)
Observations	100,200	95,554	83,168	78,522
Panel B: Destination Job Controls				
	Occupation	Industry	Firm	Workplace
Year of Transition	0.0591***	0.0518***	0.0513***	0.0595***
	(0.0167)	(0.0167)	(0.0194)	(0.0208)
1–5 Years After	0.0494***	0.0503***	0.0474***	0.0461**
	(0.0158)	(0.0157)	(0.0183)	(0.0199)
Observations	87,238	92,529	81,261	61,948
Panel C: Origin Job Controls				
	Occupation	Industry	Firm	Workplace
Year of Transition	0.0565***	0.0425**	0.0452**	0.0626***
	(0.0165)	(0.0173)	(0.0214)	(0.0225)
1–5 Years After	0.0402***	0.0294*	0.0374*	0.0413**
	(0.0155)	(0.0154)	(0.0197)	(0.0210)
Observations	82,405	86,764	52,655	46,736

Notes: This table presents estimates derived from the model specified in Eq. 3, with lag periods aggregated into the year of transition and the first to fifth full years after the transition. The outcome variable is log daily wages. All regressions include transition-event fixed effects, calendar-year-by-event-year fixed effects, and second-degree polynomials in age and years since matriculation fully interacted with female and Danish citizenship indicators. Panel A reports estimates from alternative definitions of treated and control events: restricting treated events to first-time treatments, restricting controls to never-treated students, and combining both restrictions. Panel B adds destination-job-by-calendar-year-by-event-year fixed effects, and Panel C adds origin-job-by-calendar-year-by-event-year fixed effects. Jobs are defined at the level of 4-digit occupation, 4-digit industry, firm, or workplace, as indicated by the column headers. The sample is restricted to observations more than four years after matriculation and to students observed in employment at least from event year −1 through event year 5. Standard errors, shown in parentheses, are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

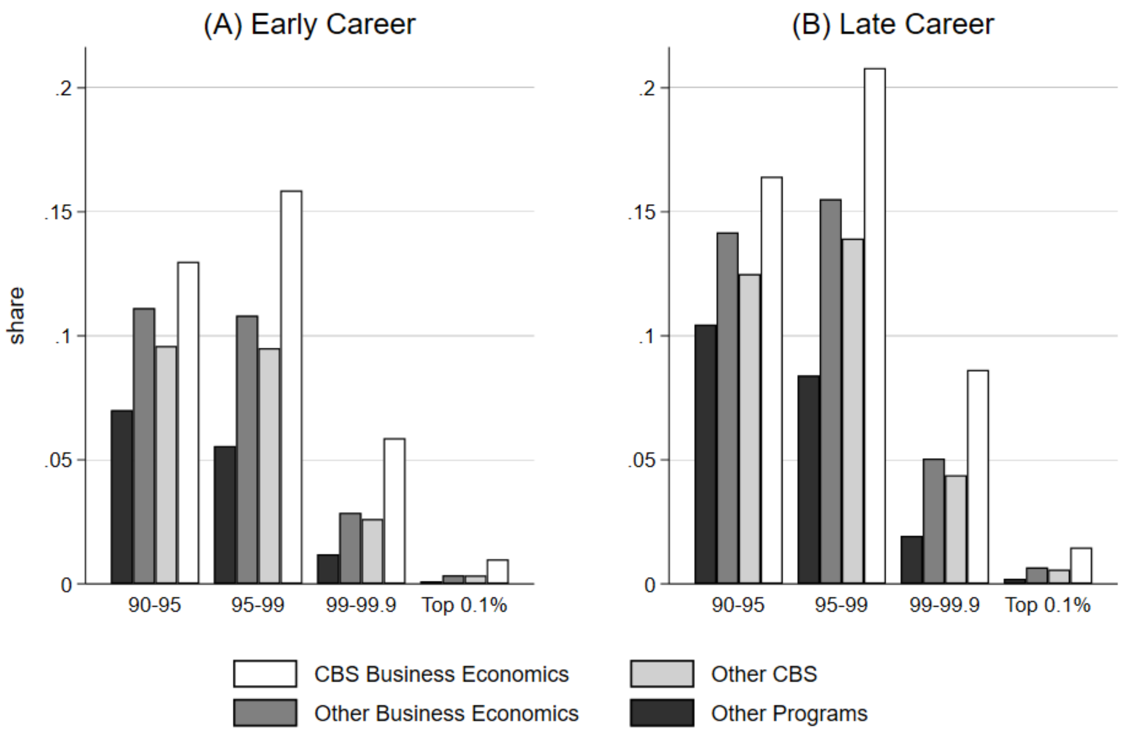

FIGURE A.2
Top Earnings Distribution: CBS Business Economics vs Other Programs

Notes: The figure presents shares of observations in different top earnings brackets of students in CBS Business Economics program and other Bachelor programs in Denmark. Programs are defined irrespective of the graduation status and post graduation years are calculated from the year 4 after matriculation. Top earnings brackets are based on national distribution of earnings in a given year for a given birth cohort. "Early Career" refers to students in the first 10 years after (potential) graduation. "Late Career" to the years after. The sample is restricted to students from matriculation cohorts 1993-2006.

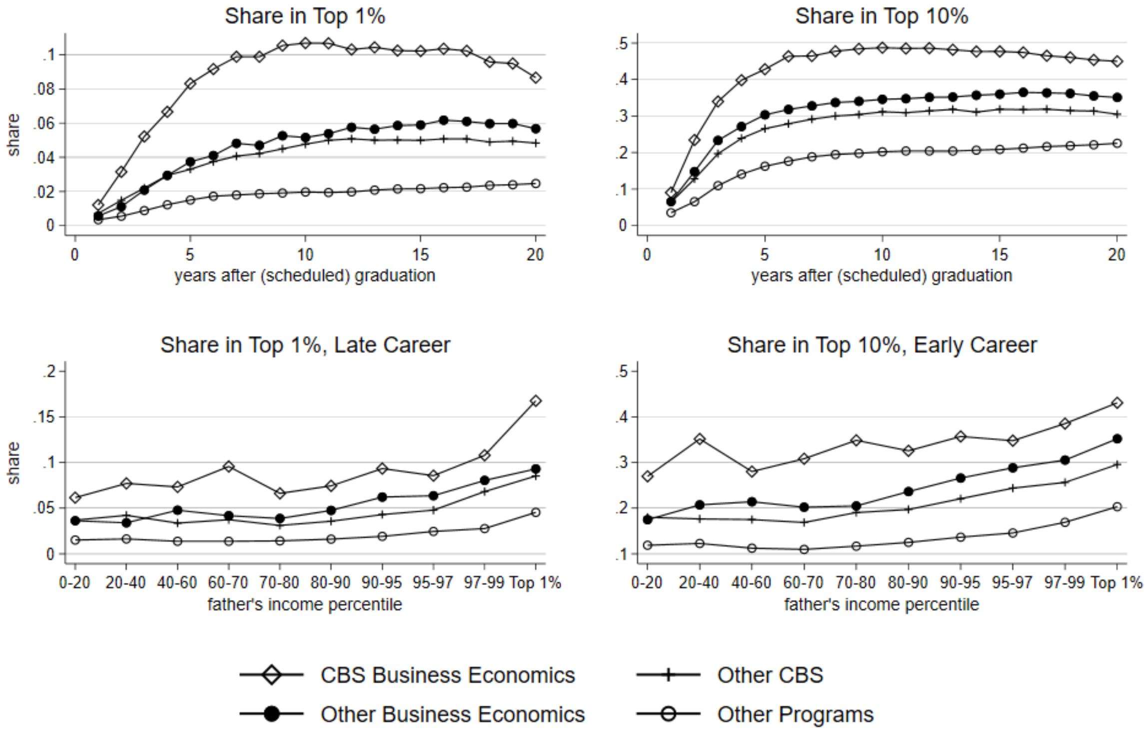

FIGURE A.3
Top Earnings Dynamics: CBS Business Economics vs Other Programs

Notes: The figure presents shares of observations in different top earnings brackets of students in the CBS Business Economics program and other Bachelor programs in Denmark. The top panels plot these shares over years after potential graduation, while the bottom panels plot them across father's income percentiles. Programs are defined irrespective of students' graduation status, and post-graduation years are calculated from year 4 after matriculation. Students' top 10% and 1% statuses are based on earnings percentiles in a given year for their birth cohort. "Early Career" refers to students in the first 10 years after (potential) graduation, and "Late Career" refers to the years following the first ten. The sample is restricted to students from matriculation cohorts 1993-2006.

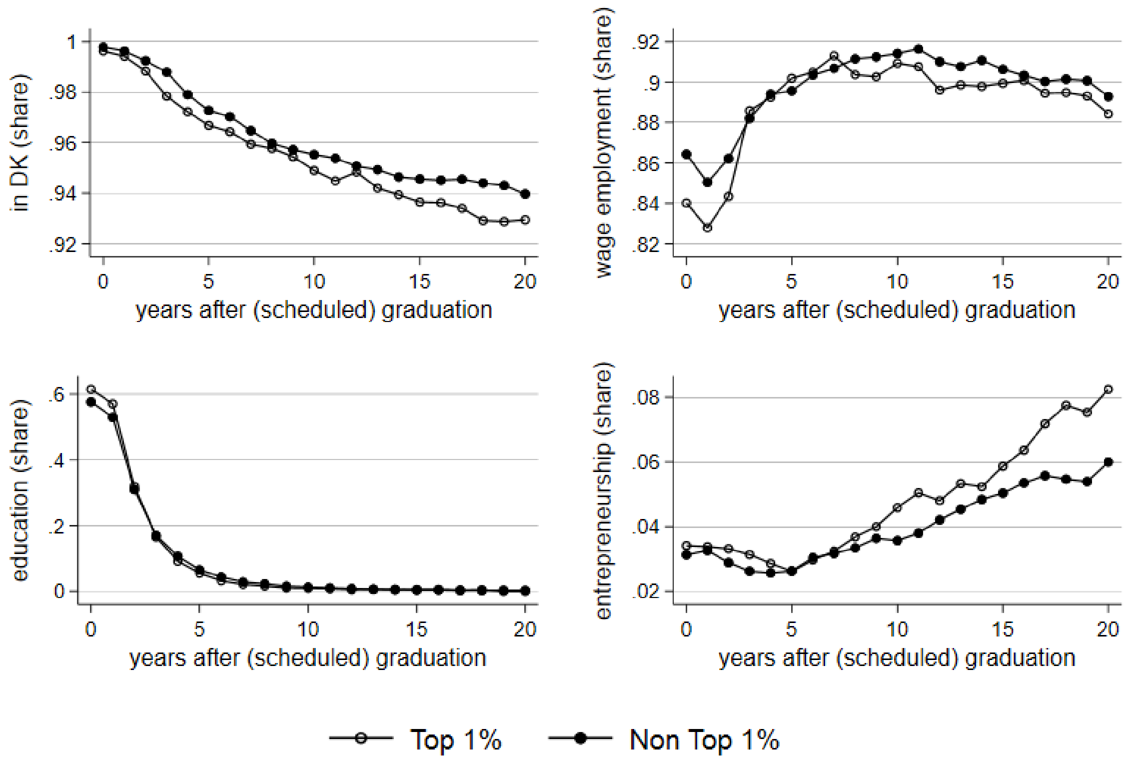

FIGURE A.4
Sample Selection Dynamics: Top 1% vs Non Top 1% Students

Note: The figure shows the share of students remaining in the sample by year since scheduled graduation, separately for students with fathers in the top 1% and others (with at least one parent in the tax records). The year after scheduled graduation is defined as the matriculation year plus 4. 'In DK' indicates having a tax record in Denmark in a given year. Sample attrition reflects emigration or death. Indicators for wage employment, education, and entrepreneurship are defined for students who are tax residents in Denmark. The wage employment sample is used in the career outcomes analysis and follows restrictions detailed in Sec. 2.2. Education and entrepreneurship indicators are based on the primary source of income.

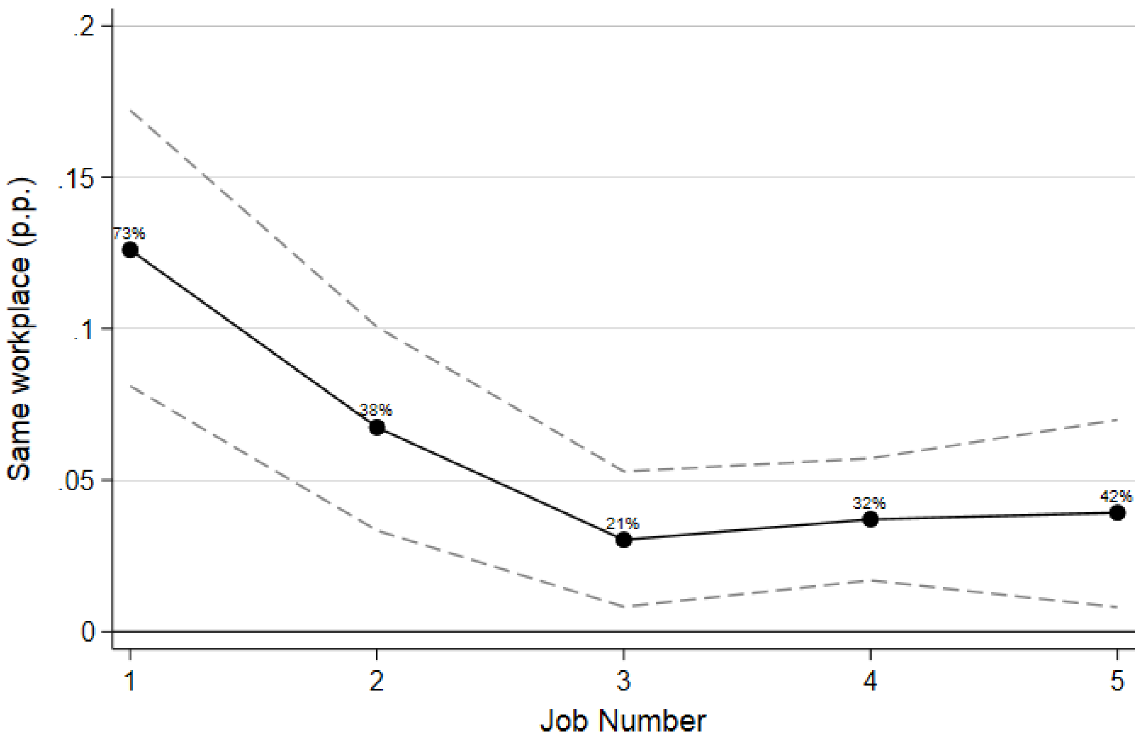

FIGURE A.5
Same Workplace: Timing of the Effect, by Job Order

Notes: The coefficients are from the linear probability model specified in Eq. 1 for directed dyads, featuring treatment interacted with a *j*'s job number. The job number is defined as the number of firms that a student worked at after the scheduled graduation (3 years after the matriculation year). Relative effects, shown as percentages, are displayed on the graph. The 5% confidence intervals refer to point estimates and are established through wild cluster bootstrap at the matriculation cohort level (9999 replications).

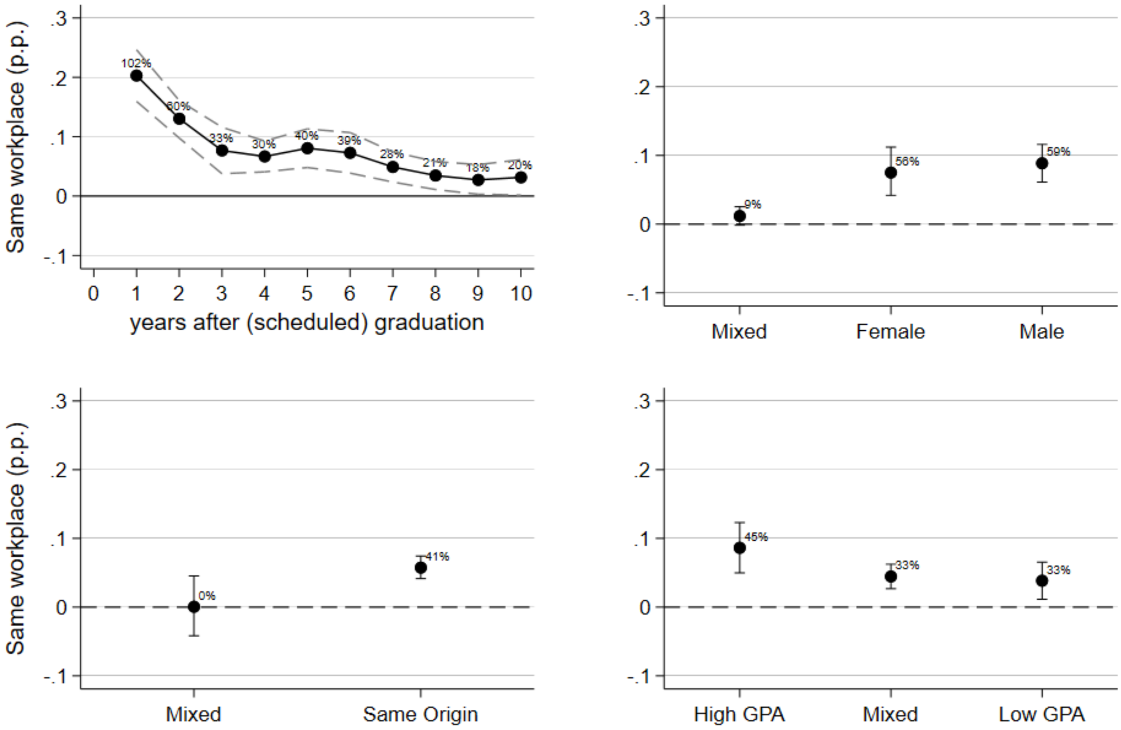

FIGURE A.6

Same Workplace: by Years Since Graduation, Country of Origin, and High School GPA

Note: The coefficients are from the linear probability model specified in Eq. 1, featuring treatment interacted with years subsequent to potential graduation and with the following group indicators: three gender groups—individual female and individual male, both females, and both males; two country of origin groups—different countries and same countries of origin; three groups by high school GPA—both students with above median within a respective CBS cohort, one higher and one lower, both students with below-average GPA. The year of potential graduation is calculated as the matriculation year plus 3. Relative effects, shown as percentages, are displayed on the graph. The 5% confidence intervals refer to point estimates and are established through wild cluster bootstrap at the matriculation cohort level (9999 replications).

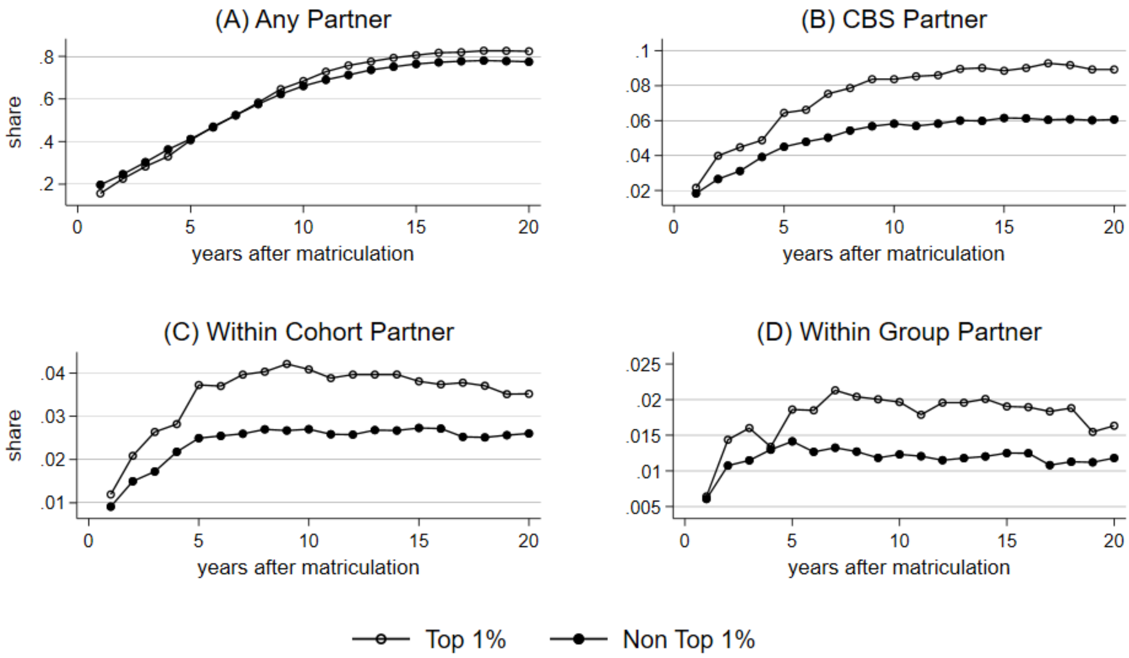

FIGURE A.7
Couple Formation, by Years Since Matriculation

Note: The figure plots the share of students in the sample with a registered partner by years since matriculation, separately for top 1% and non-top 1% students. Registered partners include married couples, couples cohabiting with a common child, and cohabiting couples without children. The latter are defined as two opposite-sex individuals registered at the same address, with an age difference below 15 years, no common child, and no close family relationship according to the population register. Panel A shows the share with any registered partner. Panel B shows the share with a partner from our sample, including cross-cohort couples. Panel C shows the share with a partner from the same cohort, either from the same or a different tutorial group. Panel D shows the share with a partner from the same initial tutorial group.

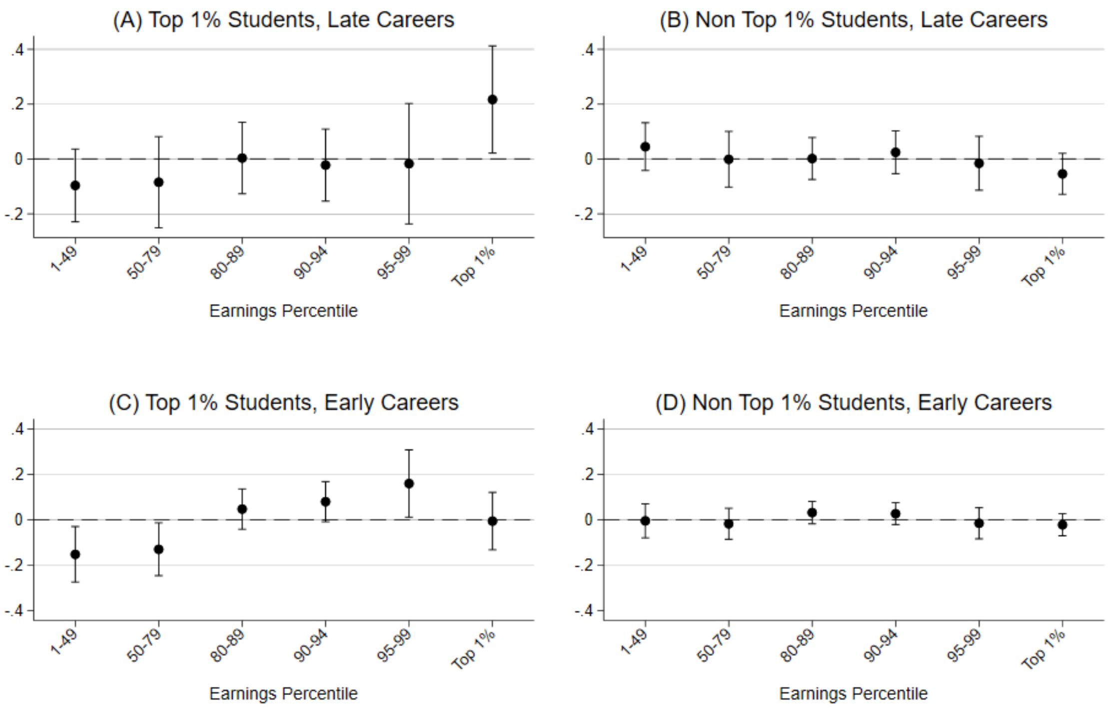

FIGURE A.8
Effects of Top 1% Peers across Earnings Distribution

Notes: This table presents estimates derived from the model as specified in Eq.2 with probability of being in a certain earnings rank bracket. Earnings ranks is defined by earnings distribution within a given birth cohort. "Early Careers" refers to students in the first 10 years after (potential) graduation. "Late Careers" to the years after. The sample consists solely of students with available income information for at least one parent. Father's income in the 5 years prior matriculation is used to define father's income rank. The same set of students is used to compute the peer variables. All regressions are run separately by own top 1% status. Standard errors are clustered at the peer group level.

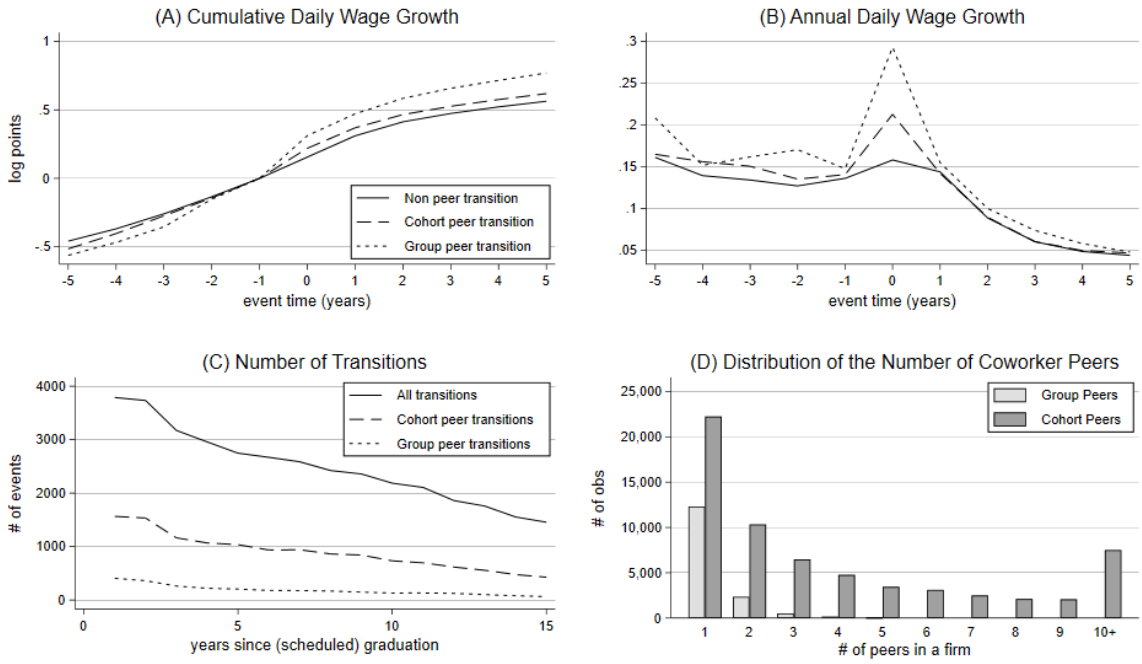

FIGURE A.9
Peer Transitions

Notes: Panel (A): real daily wage growth relative to the year before transition. Panel (B): annual real daily wage growth around the transition year. Both panels compare transitions to group peers, to cohort peers and non-peer transitions. Panel (C) plots number of transitions by year since since graduation defined as the 4th year after matriculation. Panel (D): the y-axis represents the number of observations in the CBS career panel, while the x-axis denotes the number of peers (from the same group or cohort) employed at the same firm as a specific student. The histogram excludes observations where there are no peers at a particular firm.

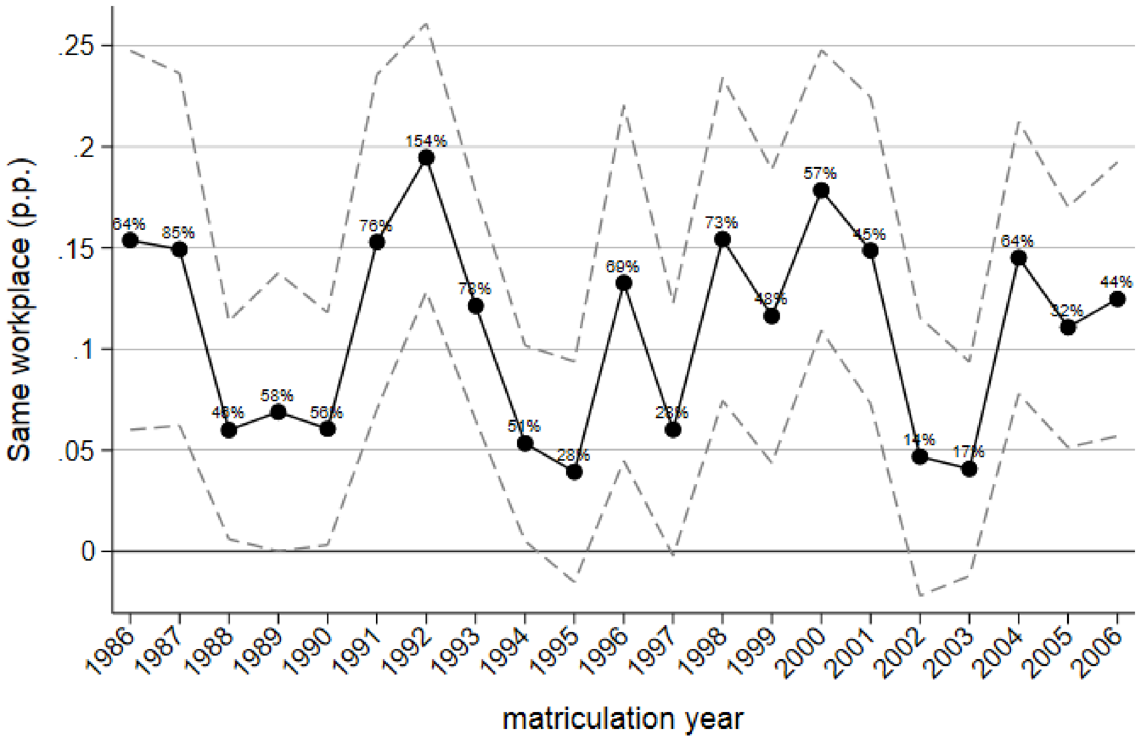

FIGURE A.10
Same Workplace: Timing of the Effect, by Matriculation Cohort

Notes: The coefficients are from the linear probability model specified in Eq. 1, featuring treatment interacted with a matriculation cohort. The sample is restricted to the first 5 years after the scheduled graduation year. Relative effects, shown as percentages, are displayed on the graph. The 5% confidence intervals refer to point estimates and are established through two-way clustering on the student level.

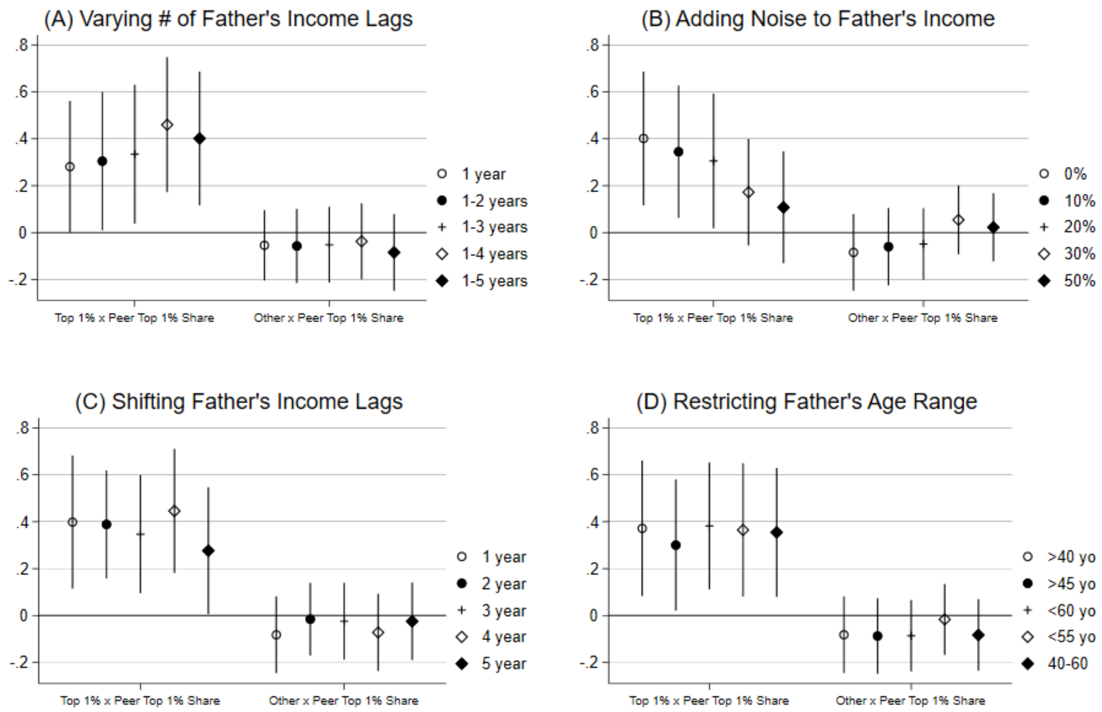

FIGURE A.11
Robustness to Alternative Top 1% Peer Group Shares

Notes: This table presents estimates derived from the model as specified in Eq.2 with alternative definitions of the father's top 1% status. Panel (A) varies the number of years used to calculate father's income between 5 years (baseline) and 1 year prior to matriculation. Panel (B) uses a multiplicative noise model, where the true level of income is multiplied by a random variable $\epsilon \sim \mathcal{N}(0, \sigma^2)$. We vary σ between 0 (baseline) and 0.5. Panel (C) shifts the time when the father's income is defined from 1 to 5 years before matriculation. Panel (D) restricts the sample by fathers' age. The sample consists solely of students with available income information for at least one parent. The same set of students is used to compute the peer variables. All regressions are run separately by own top 1% status. Standard errors are clustered at the peer group level.