Peers, Heirs, and Careers:
Labor Market Effects of Alumni Networks

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Abstract

Do ties among privileged university peers help preserve their economic status? Linking random group assignments at a Danish business school and administrative career data, we show students align career paths more closely with group peers than others in their cohort, particularly through shared workplaces. Comparisons of job transitions to firms with group versus cohort peers point to career benefits from alumni networks. Affluent students show a much higher "excess" propensity to work together and benefit more from joining firms with peers. In turn, they derive significant career advantages from exposure to wealthier university peers, while less privileged students do not.

Key words: Social connections, Peer effects, Elite univeristy, Intergenerational 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 realm of career progression. Being a part of a network of professionally successful people is often considered to be an integral part of the value of an education program. Business schools - whose programs are often characterized as stepping stones into high-paying careers - typically emphasize the importance of alumni peer ties.[1] Typically, programs that propel graduates to the upper echelons of the income ladder often enroll a disproportionate number of students from high-income families (Chetty et al., 2020). While, at the same time, previous research has demonstrated that returns to such education programs tend to favour students from wealthy backgrounds (Zimmerman, 2019). These facts prompt the question regarding the role of alumni ties in shaping individual careers and the reproduction of economic elites. Specifically, do social connections to individuals from affluent backgrounds assist students from less privileged upbringings in achieving career success, or do these connections primarily benefit individuals who share a similar social standing?

Answering this question poses significant empirical challenges, requiring a research design fulfilling several restrictive requirements. Firstly, information on the education program should be appended with detailed records on individual careers. Then, investigating the effects of social interactions is notorious for its many challenges (see, for example, Manski (1993), Angrist (2014) and Sacerdote (2014)). Most importantly, the formation of social ties between students should be independent of other factors that affect future careers. Given that it is difficult to find a setting that simultaneously satisfies both conditions, researchers often face a trade-off between the credibility of randomization and the observability of detailed career dynamics.

Our paper benefits from a unique research setting characterized by a combination of robust randomization and extensive data both - on students' careers and family backgrounds. Specifically, we exploit a policy that randomly assigned students to peer groups in a Business Economics program at Copenhagen Business School (from now on abbreviated as CBS) that reaches as far back as 1986. Business Economics at CBS is a large business education program known for producing graduates who often rank among the highest earners in the country. Notably, CBS has traditionally exhibited an overrepresentation of students from affluent families. Secondly, we are able to merge these records with Danish linked employer-employee data, providing in-depth insights into the individual career trajectories of the students. Moreover, the availability of population-wide registers enables us to link students with their parents, granting us access to parental income levels and employment histories.

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

The study resulted in three key findings. Firstly, we identify the causal impact of peers on individual career trajectories, evidenced by the tendency to select industries, occupations, and employers that align closer with their tutorial group peers than cohort peers. This pattern can be attributed to former students working together in the same workplaces, which partially overlap with their parents' previous employment. Secondly, we observe that job transitions to former group peers (peer-to-peer transitions) are associated with career gains, indicating that the phenomenon of "working together" goes beyond the non-monetary benefits of interacting with friends and improves access to superior job opportunities through social connections. Notably, while most students have parents in the upper tiers of the income distribution, the effect of being allocated to the same tutorial group, as well as the career advantages derived from joining a firm with a group peer, are particularly pronounced for students from the most affluent families. Lastly, using the linear-in-means model we show that students with fathers in the top 1% of the national income distribution experience significant long run career advantages when assigned to a tutorial group with a higher proportion of students from similarly privileged backgrounds. This supports the hypothesis that primarily students from well-off backgrounds benefit from interacting with one another, while those from less privileged social backgrounds are excluded from such advantages.

The analysis follows in three steps. First, to identify the causal effect of peers on choices after graduation, we use a dyadic approach. We analyze pairs of students within the same matriculation cohort. Pairs randomly assigned to the same peer group (referred to as group peers) are compared with pairs of students from the same cohort but assigned to different peer groups (referred to as cohort peers). We find that group peers tend to have more similar careers than cohort peers. There is an "excessive" tendency for group peers to work in the same occupations and industries and have a higher likelihood of being employed by the same firm. Tendency to share industries and employers can be attributed to students working together at shared workplaces. Moreover, peers are not significantly more likely to have similar careers while working at different offices. The workplace effect is relatively stronger, as evidenced by a pair of group peers being over 37% more likely to work in the same workplace compared to a pair of cohort peers (less than 3% for industries and occupations, and 17% for employers). We did not find evidence that peer similarities in educational choices stand behind the strong effect on career overlaps. When considered together, these findings suggest that explanations other than active "networking" among former students[2] (such as common education group-level shocks like TAs, human capital, or career preference peer effects acting through the classroom environment) are less likely to be the primary factors contributing to the observed career similarities.

[2]Active "networking" here includes a broad set of interactions like job referrals, sharing information about job opportunities or coordinating job moves.

Social ties formed during university have persistent effects that gradually diminish over time. Immediately after graduation, group peers are twice as likely to work together compared to cohort peers, which decreases to around 20% after ten years. The effect is significantly stronger when both students come from the wealthiest families (as measured by a father's income in the top 1% of the national income distribution), with a fourfold difference compared to students without such background (94% versus 24%). These pronounced differences fade when using a less strict definition of a "rich family". Student pairs with fathers between 90th and 99th percentiles do not experience stronger networking effects than students with fathers below 90th percentile. Moreover, the workplace effect is characterized by homophily across several dimensions, notably in terms of gender and country of origin.

We then explore whether working alongside peers yields career advantages beyond non-pecuniary job benefits. If the opposite were true, we would expect workers *ceteris paribus* to willingly accept lower-paid positions to collaborate with their tutorial group peers. To address this question, we employ an event-study methodology, comparing instances of job transitions to firms where one or more *group peers* are already employed with transitions to firms where one or more *cohort peers* are present. Given the absence of systematic *ex ante* differences between firms where group peers and cohort peers work, any disparities in transitions are indicative of the impact of social connections. Our findings indicate that transitioning to a firm with a group peer is associated with higher wages, better jobs and more stable employment. Importantly, the career returns are particularly concentrated among students from the top 1%.

Finally, considering the stronger peer ties observed among students from affluent families, we investigate whether they also derive greater benefits when their peer groups consist of more students from similar backgrounds. We employ an approach based on the linear-in-means model, comparing students from the same cohort who are randomly assigned to groups with different shares of top 1% peers. Students with fathers in the top 1% experience significant career advancements when studying in groups with a higher share of peers from similar backgrounds. They enjoy higher earnings, wages, incomes, and secure employment at higher-paying segments of the economy. Our estimates imply, for instance, that for a top 1% student having one more "elite" peer increases wages by around 1.4%. In contrast, we find no significant effects for students from less privileged backgrounds. Moreover, we do not observe any evidence of the share of top 1% peers significantly affecting educational outcomes for any of the analyzed groups.

Our findings make contributions to various research areas. A significant body of literature examines the labor market effects of education networks.[3] Marmaros and

[3]Even though our study focuses only on alumni ties, who you know appears to be important in different social contexts: neighbourhoods (Bayer et al. (2008); Hellerstein et al. (2011); Schmutte (2015),

Sacerdote (2002) explore how Dartmouth College seniors utilize fraternity/sorority connections to secure their first jobs using self-reported data on networking. Zhu (2022) identifies referral networks among graduates from community colleges in Arkansas. Hacamo and Kleiner (2021) focus on the managerial market, investigating how firms leverage social connections gained by their employees through MBA programs to attract talent. Zimmerman (2019) demonstrates, through cross-cohort variation, that graduates from the same cohorts of elite Chilean universities are disproportionately more likely to manage the same firms together. Similarly, Kramarz and Thesmar (2013) show that graduates from elite French universities exhibit a higher propensity to hire board members from the same colleges when serving as CEOs. More generally, our study contributes to two strands of literature. First, it adds to the broader body of research examining the role of peers in educational settings in shaping labor market outcomes (e.g., Black et al. (2013), Bjerge and Skibsted (2016), Anelli and Peri (2017), Bertoni et al. (2020), Feld and Zölitz (2022)). Second, it complements studies leveraging peer group randomizations in business school settings to identify postgraduation outcomes and interactions (Lerner and Malmendier (2013), Shue (2013)), with a specific focus on the role of business school alumni in shaping labor market careers.

This paper distinguishes itself from previous studies in several important ways. Firstly, the context in which we study social ties differs significantly. We concentrate on an education program where social connections potentially play a crucial role, with graduates often occupying high-paying corporate positions (unlike Zhu (2022)). However, not all student careers reach the top executive level (as in Kramarz and Thesmar (2013) or Shue (2013)). Additionally, unlike MBA students (as explored by Hacamo and Kleiner (2021))), our sample consists of Bachelor students who lack relevant connections through prior employers. Secondly, our empirical strategy relies on the assignment of peer exposure, which is long-lasting (compared to course-level group assignments as in Feld and Zölitz (2022) or Zhu (2022)) and utilizes explicit conditional randomization (instead of, for example, within-school and across-cohort variation (Zimmerman, 2019) or job displacements (Eliason et al., 2023)). These elements enable us to identify the causal effect of alumni ties on career development in the corporate sector and, thus illuminate a critical mechanism through which business education influences labor market outcomes. Secondly, in contrast to much of the existing research, our study specifically examines the interaction between students' socioeconomic status and the returns to alumni connections, aligning it closely with Michelman et al. (2022).[4] While our study shares similar conceptual results, it oper-

Tan (2022)), former coworkers (Cingano and Rosolia (2012), Glitz and Vejlin (2021), Hensvik and Skans (2016), Saygin et al. (2019)), family members (Kramarz and Skans, 2014) and ethnic groups (Edin et al. (2003), Damm (2009), Beaman (2011), Dustmann et al. (2016)).

[4] Cattan et al. (2023) uncover a comparable pattern studying education decisions, demonstrating that exposure to elite peers in Norwegian high schools leads to a higher likelihood of enrollment in elite

ates within a vastly different historical context - a contemporary business school in Scandinavia versus Harvard University nearly a century ago. The significance of our research lies not only in the confirmation of the pattern in a different context but also in its remarkable manifestation within the context of a Scandinavian welfare state, such as Denmark. Given the redistributive institutions, low inequality, and substantial social mobility characterizing Denmark, one might anticipate a diminished presence of such patterns; however, our findings challenge this expectation, highlighting the influence of social connections among elite academic peers even in this egalitarian setting. Additionally, we leverage detailed administrative records concerning students' careers and family backgrounds available in this context.

Our study contributes to the broad field of intergenerational mobility research, specifically at the intersection of two lines of inquiry: the role of education in shaping intergenerational mobility (Dale and Krueger (2002), Zimmerman (2019), Chetty et al. (2020)) and the effect of parental networks on labor market outcomes (Corak and Piraino (2011), Kramarz and Skans (2014)). Our findings support the notion that unequal returns to alumni networks may account for lower returns to elite business degrees among students from less privileged families (Zimmerman, 2019). Furthermore, while existing research primarily focuses on the direct effects of parental networks on labor market outcomes, we provide novel evidence of interactions between academic and parental connections, highlighting the role of peers' parental networks in facilitating employment opportunities for students at the early stages of their careers.

The remaining sections of this paper are structured as follows. The next section provides an overview of the institutional context of the Business Economics Program at CBS, 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 investigates the career implications of peer-to-peer transitions, while Section 5 adopts a linear-in-means approach to discern varying returns to elite peers. The paper concludes with a final section.

2 Data and Institutional Background

2.1 Business Economics at CBS 1986-2006

Copenhagen Business School is a large public institution located in the capital city of Denmark. Our study focuses on CBS's largest study program, a three-year degree in Business Economics. During the period under investigation, a degree in Business Economics from CBS held equivalency to a Bachelor's degree in the United States, allowing for admission into Master's programs. The majority of graduates from the

degree programs. The effect is substantially stronger for students from high SES backgrounds.

program pursued further education in either a Master of Science in Economics and Business Administration or a Master of Science in Business Economics and Auditing at CBS. Similar to other Danish study programs, the Business Economics program was tuition-free, and students were eligible for government-funded stipends.

During the sample period, the Business Economics program at CBS admitted around 600 students annually. The application and admission process was managed through a centralized system responsible for all higher education applications in Denmark. Admission to the program required a Danish high school degree or an equivalent qualification. The institutional features of the Business Economics program are well suited for studying social connections. Importantly, incoming students were assigned to peer groups comprising approximately 35 students.[5] These peer groups serve as the primary unit for organizing the study process, and the allocation takes place prior to the start of the first semester. The assignment of peer groups is based solely on the available information to the CBS administration, which is the social security number. From the social security number three criteria can be generated: gender, age and if the student is a Danish citizen.[6] As a result, the peer group assignment was conditionally as good as random, given the available information.

To ensure a credible identification strategy, our analysis relies solely on the initial peer group assignment and does not account for any subsequent changes in group composition. There were limited circumstances in which the composition of peer groups might have been altered, primarily driven by resource allocation considerations. In cases of substantial dropout rates, peer groups were occasionally merged. It is worth noting that student-initiated movements between groups were exceedingly rare, with the assigned group change being virtually impossible in most instances. Exceptions were only permitted under specific circumstances, such as scheduled medical treatment, and required a valid cause.

The Business Economics program primarily consisted of mandatory courses in the three main subjects; national economics, business economics, and academic tools, such as statistics. Courses are organized as a combination of tutorial sessions within peer groups and lectures for the whole cohort of students.[7] Throughout the program, students were expected to have tutorial sessions together within their peer groups,

[5]Tutorial groups are significantly smaller than business group sections previously studied in the literature (Lerner and Malmendier, 2013).

[6]However, it is important to note that while CBS administration primarily utilized these variables for controlling group assignments throughout our study period, the specific aims of these assignments could vary across different cohorts. For instance, efforts may have been made to achieve balanced peer groups in terms of gender and foreign citizenship, or to allocate older students to specific groups.

[7]For earlier cohorts in our sample, the distinction between tutorial sessions and lectures may be less clear. The first year of study focused exclusively on classroom teaching within peer groups, while some courses in the second and third year incorporated lectures. In more recent cohorts, a combination of lectures and tutorial sessions within peer groups was implemented from the start.

except for some elective courses in the final year. Therefore, students within the same tutorial group have the same set of TAs across mandatory courses. It is crucial to note, however, that teaching was standardized across all peer groups, with the same curriculum and assignments. All students across the peer groups faced the same examination, which was graded on the cohort level, and had the same requirements.

The administration of the program placed significant importance on fostering intensive interactions and a positive atmosphere within peer groups. As stated in the study guidelines, for instance in 1986, it was emphasized that "the group is your fixed point of reference throughout the study." Moreover, students were encouraged to form smaller reading groups within their peer groups.[8] Consequently, crucially for our empirical approach, interactions among group peers were more substantial compared to those among cohort peers.

2.2 Data Sources and Sample Selection

For this study, we employ a combination of administrative data from Copenhagen Business School and administrative data sourced from Statistics Denmark.

Our study relies on official records maintained by the CBS administration, including students enrolled in the Business Economics program from 1986 to 2006.[9] This provides us with a sample comprising 21 complete cohorts of Business Economics students. The dataset obtained from CBS includes details such as matriculation and exmatriculation dates, reasons for exmatriculation, high school GPA, high school track, citizenship, gender, age, and notably, information regarding the initial peer group assignment made by the CBS administration. Throughout our analysis, we retain student observations, irrespective of their graduation status.

We link CBS data with Danish register data. Firstly, we gain access to comprehensive background and demographic information regarding Danish residents, including age, gender, marital status, place of birth, place of residence, educational qualifications and taxable income. Importantly, we can also establish links between individuals and their parents. Secondly, we utilize detailed labor market data encompassing all firms and workers in Denmark. By combining these sources of information, we generate variables for students within our sample and their parents. Additionally, we obtain insights into the employment characteristics of individuals outside our CBS sample, allowing us to characterize the job placements of students after graduation.

[8] Reading groups were established during the fall of the first year and typically consisted of 3-5 students. These groups served as platforms for collaborative problem-solving, discussions on the syllabus, note exchange, and other related activities.

[9] Our study is the first to utilize this dataset for analysis. However, a previous study employed a subset of this dataset on tutorial group composition at CBS to investigate Master's program choices (Bjerge and Skibsted, 2016).

The primary data source for this study is the Danish matched employer-employee data. We utilize the annual cross-section of jobs (representing all primary jobs in the last week of November) for the period 1980-2021. The data contains labor market outcomes (employment, occupation, industry, wages and job spell duration) and identifiers of firms and workplaces. Firms are identified using the tax identity of the employer, and throughout the paper, we use the terms "firm" and "employer" interchangeably. Workplaces, on the other hand, correspond to physical locations where employees work, such as offices or plants, and it is possible for a single firm to have multiple workplaces (but not vice versa). All occupations are defined on the 4-digits level of the ISCO classification[10], and industries are defined on the 4-digits level of the NACE code[11]. We limit our sample to observations with identified workplaces. In case of multiple jobs per worker in a given year, we choose the employment spell with the highest annual earnings. To study the post-CBS educational trajectories of students in our sample, we utilize administrative data on students Bachelor's and Master's programs in Denmark.

This study has certain sample restrictions that are important to note. Firstly, our data does not include information on student careers outside of Denmark. This means that international students who leave Denmark after their studies or Danish students who pursue careers abroad are not covered in our analysis. Likewise, we are not able to identify the parents of international students. Consequently, our analyses involving parental income only include observations for students with available income data for at least one parent. Secondly, our focus is specifically on labor market networks, so we exclude observations that fall outside of wage employment. As a result, observations of workers in non-employment and self-employment are excluded from our analysis. Later we discuss patterns of sample selection and assess whether these sample restrictions potentially threaten our empirical strategy's validity.

2.3 Summary Statistics

[Table 1 about here.]

Table 1 presents summary statistics for our sample of 12,365 students. Approximately two thirds are male, with an average starting age above 21 years. Foreign citizens constitute a small portion (4%) of the student body. On average, high school GPA is only 0.1 standard deviation above their graduating cohort on the academic track.[12] The program dropout rate is 33%. Each student has an average of around 35

[10]DISCO is the Danish version of the international standard classification of occupations (ISCO). When studying occupational similarities, we limit our sample to years with available occupational data (1994-2016) and consider only observations with non-imputed occupational codes.

[11]The first 4 digits of Danish industrial classification (DB) corresponds to the EU classification of industries (NACE)

[12]Owing to data constraints, we standardize high school GPAs using the distribution of all graduates from the academic high school track for the respective year.

group peers and nearly 600 cohort peers. It is worth noting that many students come from affluent backgrounds. More than two thirds have fathers in top 10% and approximately 22% of fathers belong to the top 1% of the income distribution. Students with a father in the top 1% exhibit significantly higher high school GPAs and demonstrate a markedly lower dropout rate compared to their counterparts without parents in this income group. A minor fraction of students (about 4.5%) lacks parental income data, primarily consisting of foreign citizens.[13] [14]

In many contexts, the elite character of a university program - reflected in the high SES of its students - is often conflated with their high academic ability. Although parental income and high school GPA are generally positively correlated across Danish university programs (see Appendix Fig. A.1), the Business Economics program at CBS exhibits both - a pronounced overrepresentation of students from the top 1% families and relative underrepresentation of students with high GPAs.

[Figure 1 about here.]

Fig. 1 presents career outcomes by years since graduation, stratified by whether students have fathers in the top 1% of earners. Real annual earnings grow throughout the 20 years post-graduation at a decreasing rate, with the fastest growth occurring during the first five years as students transition to full-time employment. Initially, students with top 1% fathers show no earnings advantage, but their earnings grow faster over time, leading to a gap exceeding 15% in the second decade after graduation. Over time, more students enter the top 1% of their birth cohort's income distribution. By ten years post-graduation, 7% of students without top 1% fathers achieve this status, compared to 15% of those with top 1% fathers. Both groups advance in their careers by switching to higher-paying firms at similar rates, particularly early on. However, top 1% students are more likely to work at top 10% firms by daily wages and to advance faster within firms to top 10% positions. Career trajectories often intersect with peers: about one-third of students share a workplace with a peer within five years of graduation. Although this share decreases over time, it remains above 25% after 15 years, with top 1% students consistently more likely to work with peers. Appendix Fig. A.2 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

[13]Missing parental income data is attributed to students whose both parents are non-residents in Denmark the year before the student's matriculation.

[14]76% of fathers in our sample are wage employed the year before their child' matriculation. Top 1% fathers are more likely to be at a managerial top paying position at a larger higher paying firm.

growth among top 1% students.

3 Career Similarities & Networks

3.1 Identifying "Excess" Peer Similarities

To identify the effect of social interactions among university peers on their careers choices, we follow a dyadic approach and construct unique pairs of students (i,j) within each matriculation cohort $c(i,j)$.[15] The following empirical specification is implemented:

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

where F_{ijt} is an indicator variable for the event of "working together" for a pair of students i and j in year t. I_{ij} is an indicator of whether students i and j were assigned to the same peer group at the time of matriculation. The parameter of interest is β. Our identification strategy relies on comparing pairs of students within the same matriculation cohort who share the same combination of characteristics used by the administration to form tutorial groups—namely, age at matriculation, gender, Danish citizenship, and the calendar year. Consequently, X_{ijt} represents a fixed effect that takes a unique value for each combination of these variables. For example, consider a pair of students observed in 2015, who started the program in 2000. One student is a 22-year-old male Danish citizen at the time of matriculation, while the other is a 24-year-old female non-Danish citizen. To study cross-sectional outcomes (similarities in education outcomes) we apply similar model without t subscript.[16]

Our approach to using peer group assignment for identifying the effect of social interactions relies on two assumptions. Firstly, due to the assignment policy, any given pair of students within a given cohort has an equal likelihood of ending up in the same group as any other pair, conditional on the known set of stratification variables. Thus, random assignment helps resolve the "selection problem" (Manski, 1993). Below, we provide evidence supporting the exogeneity of group assignment. The second assumption is that, conditional on the "true" intensity of social interactions, the peer group assignment does not provide any additional information for predicting F_{ijt}. In other words, we assume that the group assignment lacks any useful information about the factors influencing students' careers, beyond the compositions of their peer groups.

The presence of group-level common shocks, which affect all peers within a group but are not a direct result of peer interactions, poses a potential threat to our identi-

[15] In our primary analysis, we consider undirected dyads, denoting (i,j) as equivalent to (j,i). However, in some supplementary analyses, we also employ directed dyads, where (i,j) differs from (j,i).

[16] Bjerge and Skibsted (2016) studies master program choices at CBS using an analogous method.

fication. One such example is the shared exposure to the same teachers (TAs) among students within the same peer group. While we cannot entirely rule out common shocks, a combination of the specific institutional setting and the observed results suggests that they are not a major concern. Although students in the same peer group have the same teachers for mandatory courses, the highly standardized nature of the teaching process across tutorial groups implies limited variation in teaching content.[17] Additionally, we believe that the effect of teachers is unlikely to be consistent with the overall pattern of our findings. As we will discuss later, the most substantial career similarity effects are concentrated at the workplace level, and general career choice similarity can be largely explained by the granular job choice level. This pattern contradicts any explanation that relies on group-level shocks influencing general career trajectories.

Relying on a within-cohort group assignment represents an improvement over studies employing between-cohort comparisons. First of all, controlled peer group assignment offers a more credible solution to the selection issue than relying on between cohort variation in student composition. Moreover, comparing students within the same matriculation cohort makes our analysis less vulnerable to common shocks. For instance, variation in teaching across peer groups within a cohort, driven by differences across tutorial instructors, is expected to be much smaller than variation across different cohorts. In general, the study conditions are much more comparable for students within the same cohort than for those from two different cohorts. Students starting their studies in different years might face varying labor market conditions, but this issue does not arise in our setting.

There are three crucial points to consider when interpreting our parameter of interest, β. First, our method allows us to identify the effect of group peers on career outcomes that is "in excess" of the influence of cohort peers. Although, the actual level of interaction between a pair of students, is not expected to be zero for cohort peers, we anticipate that group peers interact more frequently and have a greater influence on each other's outcomes on average. A positive and statistically significant estimate of β would provide evidence of more intensive interactions with peer group peers than with cohort peers. However, it should be noted that this estimate only provides a lower bound for the total effect of group peers, as it does not capture the case where students interact exclusively within their peer groups. Second, since we do not observe the actual network of social interactions between students, our estimation effectively measures an intention-to-treat effect. It is likely that not all students interact with the same intensity and quality with all their group peers. Additionally, one-third of students drop out,

[17] Furthermore, previous research indicates that the role of tutorial instructors in a standardized teaching environment has an insignificant impact on students' future academic outcomes (Feld et al., 2020).

and some groups are merged, leading to changes in group composition. We do not condition our estimates on graduation or staying in the same group throughout the study period. Therefore, β captures the effect of being initially assigned to the same peer group. Lastly, both F_{ijt} and I_{ij} are indicator variables, making β a percentage point difference between frequencies. Understanding the magnitude of these differences in the dyadic setting can be challenging. To provide more insight, we also calculate the effect as a percentage relative to a baseline measure of similarity. For instance, if F_{ijt} equals 1 for a pair of students (i, j) who share the same occupation in a given year, then a pair of students from the same peer group is β percentage points more likely to be observed with the same occupation than a pair from the same cohort but different groups. To better understand the magnitude, we divide β by the baseline frequency for students from the same cohort but different groups.

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.[18] Additionally, in the 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

The conditional random assignment of students to group peers is a crucial aspect of our study as it helps us identify the influence of social interactions on career dynamics. Without random assignment, if students have the ability to choose their peers, a selection problem arises (Manski, 1993). In such a scenario, observed similarities in labor market outcomes between peers could be driven by initial unobservable similarities between students, leading to biased estimates. 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 social interactions rather than pre-existing differences among students.

[Table 2 about here.]

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

[18] 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 (Aronow et al., 2015).

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.

Table 2 presents the results of the balancing test. 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.[19] 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.

3.3 Results

3.3.1 Main Results

[Table 3 about here.]

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 outcome variables, we observe that pairs of group peers are more likely to share common career paths compared to pairs of cohort peers. As previously discussed, point estimates measured in percentage points may not provide an intuitive understanding of the magnitude and relative importance of the effects. To address this, we present the effects in percent relative to a baseline, which we define as the propensity of cohort peers to be observed working together in the same "cell". The recalculated relative effects reveal significant differences. Specifically, a pair of students is approximately 3% more likely to work in the same industry and occupation if they were initially assigned to the same peer group. However, the effect becomes much more substantial when considering less aggregated labor market "cells". Being allocated to the same peer group leads to a 17% higher probability

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

of working at the same firm and a 37% increase in the probability of working at the same workplace after graduation.[20]

The pronounced concentration of the peer effect at the most granular level, the workplace, challenges the idea that peer interactions are confined to the period before graduation or are driven solely by shared academic experiences. Classroom human capital spillovers, co-formation of career preferences, and the influence of educators are more likely to manifest in broader career trajectories, as indicated by industry and occupation choices. The robust peer effect observed at the workplace level aligns with the existence of active alumni networks. These networks can serve as valuable sources of job-related information, allowing graduates to access information about job openings and secure referrals to potential employers. Such interactions are anticipated to result in increased similarities in firm and workplace selections among group peers.

To better understand the driving forces behind peer similarities in career outcomes, we investigate whether the workplace effect underlies the observed effects on occupation, industry, and firm choices. To do this, we redefine "working together" as an event where students share the same occupation, industry, or firm, but not the same workplace.[21] As shown in Panel B, after excluding workplace similarities, the effects of working in the same industry and firm lose statistical significance, suggesting that these similarities are largely driven by workplace interactions. However, the effect on occupational choices remains significant. This finding supports the notion that peer similarities in career outcomes are primarily driven by interactions at the most granular level - the workplace. We interpret these results as suggestive evidence that students use their networks after graduation, and that post-graduation interactions in the labor market play a crucial role in shaping the career trajectories of former academic peers.

Hypothetically, the higher likelihood of a pair of group peers working at the same workplace than a similar pair of cohort peers could be attributed to one of two reasons - either group peers are more likely to join the same workplaces, or the job matches at workplaces with group peers happen to be more stable. If this pattern is driven by the former reason, it could be due to either simultaneous coordinated moves of both students or the tendency to join incumbent peers (possibly due to referrals or other reasons). To explore these potential mechanisms, we conducted a directed dyad analysis using Eq. 1 and present the results in Appendix Table A.1. Remarkably, even when

[20]The contrast between the point estimates and the recalculated relative effects arises because the treatment leads to nearly the same increase in frequencies of "working together" events in percentage points, but the baseline probability of being observed at the same workplace is much lower than the baseline probability of being observed in the same industry.

[21]Note, that this exercise is different from conditioning on students not working at the same workplace (dropping these observations). The probability of working together in the same industry (occupation or firm) can be expressed as a sum of probabilities of two mutually exclusive events - working together in the same industry and at the same workplace and working together in the same industry and different workplaces. Here we use the latter as an outcome variable.

considering only new matches, the effect remains statistically significant. Specifically, when student i leaves a firm, she exhibits a disproportionately higher likelihood of joining a workplace with a group peer rather than a cohort peer. Moreover, this effect is observed due to both coordinated moves of two students and instances where a student joins an incumbent peer at the workplace.

[Table 4 about here.]

Students in our sample generally lack relevant labor market experience and are therefore unlikely to refer their peers to previous employers immediately after graduation (unlike, for example, MBA students (Hacamo and Kleiner, 2021)). However, for young workers, connections to their parents' employers may play a significant role (Kramarz and Skans, 2014). Our findings demonstrate that these strong ties can also benefit their peers. we focus on a special subset of workplaces - those connected with students' parents. These are workplaces where one of the parents worked a year before matriculation. As shown in Table 4, the workplace effect in the first 5 years after graduation is partially driven by this subset of workplaces. It is not only that students assigned to the same peer group are more likely to work together, but they are also more likely to work together at the workplace where one of the students' parents worked. It's important to note that parental workplaces are specifically defined as those where either parent was employed a year before the student's enrollment. Consequently, any discovery regarding the reduced significance of these workplaces is, to some extent, a result of the way the definition operates. This finding suggests that one of the reasons why peers might be important is that they provide access to their parental networks, which can be influential in shaping career opportunities.

3.3.2 Networks and Elite Homophily

[Figure 2 about here.]

As previously emphasized, a student from our sample not only achieves a career within the upper echelons of the labor market but also likely originates from financially well-off families. Findings from earlier studies, such as Zimmerman (2019), suggested that post-graduation, students from privileged backgrounds tend to establish robust labor market networks. This phenomenon could explain the unequal distribution of returns from "elite" education programs among students of varying social origins. In essence, students hailing from affluent backgrounds may accumulate greater social capital during their studies, potentially contributing to their higher educational returns.

To investigate this question, we analyze how being assigned to the same peer group affects the probability of student pairs working at the same workplace, classifying students based on their father's disposable income ranks (Fig. 2). We compare two definitions of "top" status: having a father in the top 1% versus having a father between the 90th and 99th percentiles. Student pairs are divided into three groups: (i) both students have fathers in the top group, (ii) only one student has a father in the top group, and (iii) neither student has a father in the top group. Our findings indicate that the networking effect is much stronger for students with the most affluent family backgrounds. Among students with fathers in the top 1%, the effect is four times larger compared to pairs where neither student has a father in the top 1%. Interestingly, when we exclude students with top 1% fathers and adopt the less restrictive definition of "rich" families—having a father in the 90th to 99th percentiles—the pattern disappears. Under this definition, the effect for student pairs from top families is indistinguishable from that of pairs with fathers in the bottom 90%. This suggests that the strongest networking effects are specific to students from the wealthiest families.

3.3.3 Timing and Heterogeneity

[Figure 3 about here.]

It is natural to assume that the intensity of post-graduation interaction with former academic peers decreases over time. Fig. 3 illustrates the timing of "excess" workplace similarities. The effect is strongest a few years after scheduled graduation and diminishes over time.[22] At the beginning of their careers, a randomly chosen pair of group peers is almost twice as likely to share a workplace compared to a randomly chosen pair of cohort peers. However, ten years after scheduled graduation, this effect decreases to 20%. Similar patterns emerge when we consider job numbers instead of years (see Appendix Fig. A.3). The peer effect is most pronounced for the first job after (scheduled) graduation and gradually decreases over subsequent jobs, although it remains statistically significant even for the 5th job. This indicates that the persistence of the effect over the years is not solely explained by the long-lasting impact of the post-graduation first employment. Furthermore, we observe no discernible trend of the effect across matriculation cohorts (see Appendix Fig. A.4), suggesting that the changing composition of cohorts over the years is not responsible for the observed pattern.

[Figure 4 about here.]

Fig. 4 further investigates heterogeneous effects by gender, country of origin, age, and high school GPA. Social connections tend to form more intensely between individuals that are more similar. Specifically, we divide same-gender pairs of students into pairs of male students and pairs of female students. The peer effect for both types of same-gender dyads is significantly higher than for mixed dyads, where the effect is not significantly different from zero. However, we do not observe a statistically significant difference in the effects across same-gender student pairs. Male and female students

[22] Similar pattern was documented by Eliason et al. (2023) for high school graduates.

appear equally likely to utilize their same-gender social connections in shaping their career outcomes.

Another dimension of potentially important heterogeneity is the country of origin. We investigate whether the effect for student pairs of the same origin differs from pairs of different origins. However, due to the limited number of non-Danish citizens in our sample, we cannot investigate Danish dyads separately from other students sharing a country of origin.[23] As a result, we compare the magnitude of the effect for pairs of students with the same country of origin (including Danes and non-Danes) to pairs of students with different origins. We find a significant effect for pairs of the same origin, while the effect for mixed origin pairs is imprecisely estimated. However, we interpret the fact that the effect for same-origin dyads is significantly higher as indicative of country-of-origin homophily.

To explore potential heterogeneity based on age at the time of matriculation, we define "same age" as students who are within one year of each other. We then examine the effect of being assigned to the same peer group for students by age difference. We do not observe statistically significant difference between effect for same age and different age pairs ($P(H_0 : \beta^{same} = \beta^{diff}) = .37$).

To investigate potential heterogeneity based on academic performance, we define high GPA students as those with GPAs exceeding the median within the respective CBS cohort. We then classify pairs of students into three categories: pairs with both students having high GPAs, mixed pairs with only one student having a high GPA, and low pairs where neither student has a high GPA. Upon examining the results, we observe a tendency towards higher excess similarity among pairs with higher GPA. The effect for pairs where both students have high GPAs is significantly higher when both have low GPA ($P(H_0 : \beta^{high} = \beta^{low}) = .02$). This suggests that the influence of peer interactions on career choices is more pronounced among high achieving students who share similar academic backgrounds.

3.3.4 Selection out of Sample and Education Choices

The evidence from Table 2 supports the assumption of (conditionally) random assignment of students to peer groups. However, it is important to acknowledge that not all students are observed in our career sample every year, resulting in missing data for certain student pairs. This missing data can be attributed to two main factors.[24]

[23]The construction of dyadic observations from a given group of students results in a much smaller share of these dyads compared to their share in the population. For example, if there are n students of a given type in a cohort of size N, the share of this type in the cohort is $\frac{n}{N}$, but the share of dyads constructed from students of this type will be $\frac{n(n-1)}{N(N-1)}$.

[24]Figure A.2 in the Appendix summarizes sample selection patterns for students in our dataset over 20 years post-graduation, with separate trends for top 1% and non-top 1% students.

First, some students may leave Denmark, either temporarily or permanently, which is particularly relevant for international students. Second, even students who remain in Denmark might experience periods of non-employment, self-employment, or further education, leading to gaps in the data. The presence of missing observations introduces potential challenges to our empirical strategy. For example, if peer group assignment influences students' decisions to leave the sample—potentially due to peer effects on migration—and this decision is correlated with their initial propensity to make similar career choices, our estimates could be biased.

[Table 5 about here.]

Table 5 investigates this issue directly for the whole sample as well as for subsamples split by top 1% status. Specifically, we test whether being assigned to the same peer group makes students more likely to be observed in our career data and both registered as tax residents in Denmark in a given year. Additionally, we examine whether peer group assignment affects the probability of being in our career sample or continuing in education, conditional on being in Denmark. We find a small but statistically significant effect at the 10% level for the latter outcome, driven primarily by the first two years after (potential) graduation, when the majority of students continue their studies. Overall, we conclude that sample selection issues do not pose a threat to the interpretation of our results.

[Table 6 about here.]

One channel through which social interactions might affect career trajectories is via speicific educational decisions made prior to starting a career. For instance, if peers exhibit similar drop-out behavior, it could lead to comparable career choices, even without further interactions in the labor market after graduation.[25] To explore this possibility, Table 6 examines the effects of peer group assignment on similarities in educational choices at both the Bachelor's and Master's levels. We do not observe any significant effects for these choices, except for the choice of Master's program. Being assigned to the same peer group leads to a small but statistically significant effect, at the 5% level, on the probability of graduating from the same Master's program.[26]

Table A.2 in the Appendix examines to what extent the choice of a Master's program might mediate the effect of peer groups on career similarities. Pairs of students who graduate from the same Master's program are significantly more likely to make similar subsequent career choices, conditional on the same set of controls as in Eq. 1. This observed tendency among group peers could reflect two factors: the real causal effect

[25]It is worth noting that even in such a scenario, it remains challenging to explain how similarities in educational choices translate into career similarities at the workplace level, as educational choices are arguably more likely to influence occupation and/or industry selection.

[26]It is worth noting that there is very limited variation in Master's degree choices within our sample. Among those who pursue a Master's degree, the overwhelming majority enroll in one of the two programs available at the time at CBS.

of the degree choice and the selection of similar students (e.g., in terms of career aspirations or grades) into the same Master's programs. For the purpose of this empirical exercise, we assume that this "excess" tendency represents the upper bound of the real causal effect. We impute the probability of working together based on a model similar to Eq. 1 with an indicator for graduating from the same Master's program as a treatment variable. Then, by rerunning the same regressions as in Table 3, but using the imputed probabilities as the outcome variables, we demonstrate that even under this upper-bound assumption, the observed effect remains very small (for example, the workplace effect is less than 1% of its original magnitude). Therefore, this channel is unlikely to explain our main results regarding career similarities among business school peers.

4 Wage Effects of Peer-to-Peer Transitions

4.1 Empirical Strategy

In the previous section, we presented evidence indicating that interactions among students influence their decisions to work together in their post-graduation careers. However, the underlying drivers of this tendency remain ambiguous. It is uncertain whether this tendency is primarily motivated by the career benefits derived from such peer interactions or if it is driven solely by the intrinsic utility derived from working with former university peers. In this section, our objective is to distinguish between these two scenarios by examining whether transitions to jobs at firms where group peers work (peer-to-peer transitions) are associated with career improvements or penalties. If CBS graduates simply derive utility from working with peers, we expect these job-to-job transitions to be linked to worse outcomes, as ceteris paribus students would be willing to sacrifice their earnings to work with their friends. On the other hand, if former business school peers share information about job openings or provide job referrals (shifting job offer distribution), we anticipate observing economic benefits when individuals join firms where their peers work.

A key challenge in identifying benefits associated with transitions to peers lies in selecting an appropriate comparison group. Even without peer interaction, voluntary job-to-job transitions may be associated with wage increases. Thus, peer-to-peer transitions must be compared to job transitions as well. However, if former CBS students tend to work at firms that, on average, offer higher pay, then transitions to these firms will mechanically lead to higher wage increases. To address these concerns, we employ an event-study approach where we compare job transitions for (observable similar) CBS graduates who move to a job with an incumbent group peer to workers who transition to a job with an incumbent cohort peer.[27] Assuming there is no systematic difference between firms where group peers and cohort peers work *ex ante*, then if joining a group peer leads to superior labor market outcomes, we interpret the latter effect as likely being a result of the social connections. This effect can arise from various sources, including referral premiums, a higher job arrival rate, a superior job offer distribution, and/or productivity gains.[28]

We implement a stacked event study approach. A panel window around each transition event e is constructed, focusing exclusively on transitions, where a student i joins a firm where another student from the same cohort is already present at time t. Specifically, for all transition events, we create a panel in years τ relative to the transition event, comprising 5 years before the transition ("leads") and 5 years after the transition ("lags"). As a result, the same student i may appear multiple times in the same year t.

Our approach is formalized in the following regression framework:

$$y_{eit} = \mu_e + \lambda_{\tau t} + \sum_{\substack{-5 \leq \tau \leq 5 \\ \tau \neq -1}} \gamma^{\tau} GroupPeer_e + \beta X_{et} + \epsilon_{eit}, \tag{2}$$

where y_{eit} represents the outcome variable for individual i in year t. $GroupPeer_e$ is a treatment indicator for when a transition event e is to a firm with at least one group peer. μ_e denotes event fixed effects that absorb cross-individual variation in outcomes (within the event time window). $\lambda_{\tau t}$ captures calendar-by-event time effects, which limit the comparison to observations at the same year t for transitions that happened in the same year $t - \tau$. Vector X_{it} includes second-degree age and year since matriculation polynomials that are interacted with gender and Danish citizenship indicators. γ^{τ} represents our parameters of interest. These are the event study coefficients that reflect how changes over time for workers in a treatment group differ from changes over time in the control group. Under the parallel trend assumption, treatment lags ($\tau \geq 0$) coefficients capture the excess benefit of joining a group peer relative to transitions of joining a cohort peer. Absent anticipation, treatment leads should be statistically indistinguishable from 0, and γ^{-1} is an omitted reference category. For some of the empirical exercises, the treatment effects of interest are the short-run effect γ^{0} and the long-run effects $\gamma^{\tau \in [1;5]}$. We cluster standard errors at the individual level.

In contrast to the previous and subsequent sections, the analysis in this section relies, to a lesser extent, on the random assignment of students into peer groups. Two

[27] Fig. A.5 shows variation in number of group and cohort peers per firm.

[28] It is important to note that there are several alternative interpretations that we cannot rule out here. It is possible that workers have higher reservation wages when they accept job offers from a place where someone they know works. This behavior could be rationalized by placing a higher weight on socially closer individuals in interpersonal comparisons. Also, working with peers can lead to higher productivity which is reflected in a wage premium (Bandiera et al., 2010).

critical assumptions underlie our interpretation of the results. Firstly, the aforementioned parallel trend assumption excludes shocks that are correlated with peer-to-peer transitions. Although we cannot directly test the validity of this assumption, we perform the conventional pre-trend test. Secondly, the treatment definition allows for the possibility that the peer transition effect might reflect factors other than peer interactions. For instance, while we demonstrated in the previous section that, thanks to the conditionally random nature of the group assignment, pre-matriculation group peers are not more similar to cohort peers, we do observe that as a result of group assignment, they tend to have more similar careers after graduation, leading to the accumulation of similar skills. Consequently, having similar previous employment histories might make workers better matches for jobs at firms where group peers work compared to firms with cohort peers. Prospective employers may be aware of this fact and make offers based on previous employment. Importantly, this effect is mechanical and independent of social interactions. To ensure that this is not driving the observed results, we also perform robustness checks to ensure that the effect holds when we restrict the comparison to former students transitioning *from* the same jobs.[29]

Our empirical strategy differs from the approaches commonly employed in the literature that study the effects of referrals on labor market outcomes. Some previous studies handled the selection problem of workers who are hired through referrals by using linked employer-employee data and employing both worker and firm fixed effects (e.g., Dustmann et al. (2016), Hensvik and Skans (2016) and Zhu (2022)). In our baseline specification we do not include firm fixed effect as our strategy relies on comparing transitions to cohort peers and group peers. Nevertheless, as a robustness check, we show that the results are robust to various controls for destination jobs (including destination firm fixed effects). At the same time, the difference between these approached potentially corresponds to the difference in the estimand. From the perspective of an employer, a referral wage premium (or penalty) represents a wage differential between otherwise similar workers that arises solely from the hiring channel of the worker. However, from the worker's perspective, this differential is not the only source of benefits from social connections (referrals or information sharing about job openings). Benefits from alumni networks could manifest themselves through a shifting of the job offer distribution, providing individuals with access to higher-paying jobs.

4.2 Results

[Figure 5 about here.]

In Fig. 5, we present the event study results using the specification in Eq. 2. The

[29] Note that conditioning on the previous job also makes a less demanding version of a parallel trend assumption - potential outcomes should evolve similarly *conditional* on previous job. Hence, the same robustness check should be informative on the validity of the original parallel trend assumption as well.

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 benefits compared 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.[30]

[Table 7 about here.]

Table 7 presents the effect on various labor market outcomes alongside log daily wages. As depicted in Figure 5, we observe a positive wage benefit for transitions to joining a group peer. In the year of the transition, the wage benefit is around 4,6%, and it declines to 3,6% in the following years. Moreover, our analysis indicates that peer-to-peer transitions not only result in higher wages but also lead to more stable job matches. Workers are less likely to leave a firm if there was a group peer among coworkers at the time of the transition. This may suggest that peers reduce uncertainty about worker skills, employer demands, and/or worker job preferences, thereby leading to longer job tenure. We find a significant negative effect of on the probability of leaving a job that increases over time. Additionally, these jobs tend to be are in higher-paying firms, industries and occupations, providing further support for the hypothesis that peers facilitate access to superior job prospects. Overall, our findings emphasize the beneficial impact of peer-to-peer transitions on both wages and job stability in the post-graduation careers of CBS graduates.

Furthermore, Appendix Table A.3 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.

[30] Even though we do not condition on staying in the same firm, this pattern aligns with findings in the referrals literature (e.g., Dustmann et al. (2016)).

[Figure 6 about here.]

Fig. 6 presents the heterogeneous returns to joining a group peer on daily wages. Previously, we observed that alumni networks exhibit strong gender homophily, but without noticeable differences in networking effects by gender. However, if male students perform better in the labor market, gender homophily could potentially benefit them more. We do not, however, observe any significant differences in the effects of joining a group peer by gender ($P(H_0 : \gamma^{male} = \gamma^{female}) = .38$). At the same time, the effect is more pronounced among workers with higher ability, as measured by having a high school GPA above the median within a given CBS matriculation cohort ($P(H_0 : \gamma^{high} = \gamma^{low}) = .01$). This result aligns broadly with previous empirical findings that social connections provide employers with valuable information about unobserved skills (Hensvik and Skans, 2016). The returns to joining a group peer are observed primarily for younger workers in the early stages of their careers, defined as being below the median age at transition (33 years) in our panel ($P(H_0 : \gamma^{early} = \gamma^{late}) = .01$). This finding echoes earlier observations that the benefits of networks are strongest immediately after graduation and dissipate over time. 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 effect for this group is significantly higher than for students with fathers in the top 10% ($P(H_0 : \gamma^{top1\%} = \gamma^{top10\%}) = .01$), whereas for the latter group, the effect is not significant. This finding suggests that students from affluent families not only engage in more networking but also derive greater benefits from their networks.

5 Career Effect of Exposure to Top 1% Peers

5.1 Empirical Strategy

Previous sections have highlighted that students from more affluent backgrounds tend to form stronger networks and benefit more from following each other in their careers. This raises the question of whether students from wealthy families generally gain in the long run from being exposed to each other during their studies. In this section, we again utilize the fact that, conditional on the few known individual characteristics, the composition of one's peer group and, hence, the share of students coming from the top 1% of families within a given matriculation cohort is as good as random. We investigate if the causal effect of being assigned to a group with more peers coming from rich families on the career outcomes and whether it is significantly higher for students who also come from affluent families.

In this section, we use a standard linear-in-means specification widely applied in the educational peer effects literature (Sacerdote, 2001). We apply this regression framework separately for two groups - students with fathers in the top 1% of the income distribution (H) and other students (L):

$$y_{it} = \alpha^G \times \overline{Top1\%}_{-ig} + \lambda^G_{ict} + \epsilon_{it}, \tag{3}$$

where $G \in \{H; L\}$.[31] The variable y_{it} represents one of the career outcomes of interest. λ^G_{cit} denotes a fixed effect that takes a unique value for each combination of age at matriculation, gender, Danish citizenship, matriculation cohort, and calendar year. The regression is run separately by top 1% status, so the parameters α^G represent the group-specific causal effects of interest. This specification ensures that the comparison of career outcomes occurs within the same calendar year for students from the same matriculation cohort who were similar in terms of group assignment characteristics but were initially assigned to peer groups with different shares of top 1% peers. $\overline{Top1\%}_{-ig}$ is a leave-one-out share of top 1% students in a peer group. Figure 7 plots raw and residualized identifying variation in the variable. Standard errors are clustered at the peer group level.

[Figure 7 about here.]

Note that the top 1% status of a father is measured before matriculation. By using this predetermined status of peers, we can circumvent the common shock problem and the reflection problem (Manski, 1993). Moreover, when combined with the (conditional) random assignment of students to peer groups, it allows for a causal interpretation of α^G. To further support the reliability of group randomization (in addition to the findings presented in Table 2), we conduct linear-in-means type balancing tests.

5.2 Results

[Figure 8 about here.]

Applying the framework in Eq.3, Figure8 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.[32] 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 of more than 4%. To express the coefficient magnitude in terms of the absolute number of top 1% peers, we divide the coefficient by the average group size of 35. 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

[31]Here we restrict our sample to students for which we observe parental income (see Table 1).

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

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 our earlier findings (Table 2), using predicted annual earnings instead of real earnings provides additional evidence supporting the validity of our research design (Panels C and D). The residualized elite peer share is not related to predetermined earnings, regardless of elite background. Table A.4 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. 3.

[Table 8 about here.]

Table 8 paints a broader picture of uneven career effects of elite peers by student's own family background. For a group of students with top 1% fathers, a 10-percentage-point increase in the share of top 1% students within a peer group yields almost a 5% increase in real daily wages (1.4% for each top 1% peer) and a .72-rank rise in disposable income distribution (.21 for for each top 1% peer). These effects correspond to higher-paying jobs in industry and occupation rankings. At the same time the group of students without fathers in the top 1% do not experience such effects (all of the effects are statistically different between the group on the 5% level).

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. For instance, students from affluent families might study more efficiently when surrounded by similar students. 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 Table A.5 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.[33]

To enhance the robustness of our interpretations, we assessed whether the treatment—assignment to peer groups with a higher share of top 1% peers—is associated with selection bias in the career sample. If students with lower earnings potential were more likely to drop out of the sample when assigned to peer groups with a higher proportion of top 1% peers, our estimates could be biased upwards. However, as shown in Appendix Table A.6, our findings do not indicate such a bias. We observe no discernible impact of the treatment on the likelihood of being included in the career sample, nor on the probability of being registered as a Danish resident. Similarly, we find no evidence of an effect on being engaged in the career sample or in education, conditional on Danish residency.

Lastly, we explore sensitivity of our result to the definition of a treatment variable.

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

We check whether the observed career effects are predominantly influenced by peers with top 1% fathers, in contrast to peers with top 1% mothers (who are less represented in our sample) and peers with either top 10% mothers or fathers (more prevalent in our dataset). The findings presented in the Appendix Table A.7 indicate that the impact is associated with the upper echelon of the peers' background distribution, specifically in relation peers with top 1% fathers. We also assess how potential measurement error in the share of top 1% peers might affect the robustness of our results. Figure A.6 shows that in full allignment with the hypothesis of random group assignment adding measurement error to the underlying father's disposable income variable leading to increasing attenuation of the effect.[34]

6 Conclusion

What is the impact of social connections among business school peers on individual career trajectories and economic mobility towards top jobs? Do alumni networks predominantly benefit students from similar affluent backgrounds, or do they open doors to career success for individuals from less privileged upbringings? To address these questions, we leveraged a unique research setting at Copenhagen Business School, where students (many of whom come from affluent family backgrounds) were randomly assigned to tutorial groups for many years. This robust randomization allowed us to establish causal relationships between peers and career outcomes. Moreover, our comprehensive data included both - extensive career and family background information from Danish administrative sources. Our study revealed significant career similarities among former group peers, surpassing those observed among cohort peers. These "excessive" tendencies to share common occupations, industries, and employers were explained by the fact that peers are more likely to work together at the same workplace. We found that these effects were particularly pronounced for students from affluent families. Further investigation into job transitions showed that students benefit from their alumni networks, gaining access to higher-paying jobs. Comparing transitions to firms with group peers versus cohort peers, we observed significant wage increases for those joining firms where group peers were employed. We show that former students from the top 1% families also benefit the most from working with their business school peers. Furthermore, using the linear-in-means model, we showed that students with fathers in the top 1% experienced significant career gains when assigned to tutorial groups with a higher proportion of peers from similarly privileged backgrounds. In

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

contrast, we found no significant effects for students from less privileged backgrounds. This suggests that the concentration of students from rich families in the program clearly benefits students coming from the same background.

In conclusion, our study underscores the significant impact of social connections among university peers on shaping career returns to a business education degree. These connections are instrumental in fostering career advancements for students coming from privileged backgrounds. Our findings point to the potential perpetuation of inequality through alumni networks, posing barriers to upward mobility for individuals from less affluent backgrounds. It suggests that merely providing access to education paths leading to top jobs for students without privileged family backgrounds may not be sufficient, as social interactions among students shape the returns to education. Further research is warranted to explore interventions that can enhance the effectiveness of these programs for underprivileged students.

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Tables

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 disposable 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.

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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: Parental Workplaces

	Parental (1-5y)	Not Parental (1-5y)	Parental (6+y)	Not Parental (6+y)
Same group	0.0076***	0.1040***	-0.0002	0.0416***
	(0.0017)	(0.0076)	(0.0004)	(0.0038)
P-value	0.009	0.000	0.819	0.000
Effect (in %)	95.92	49.91	-8.65	31.05
Baseline	0.008	0.208	0.002	0.134
Observations	9,941,721	9,941,721	24,793,489	24,793,489

Notes: This table presents estimates from the linear probability model specified in Eq. 1. Columns correspond to different outcome variables and periods. Sample is restricted to dyads with at least one defined parental workplace. Parental (1-5y): same workplace, where at least one parent worked the year before matriculation, observations within 5 years after potential graduation. Not Parental (1-5y): same workplace, where no parent worked the year before matriculation, observations within 5 years after potential graduation. Parental (6+y): same workplace, where at least one parent worked the year before matriculation, observations after 5 years post-potential graduation. Not Parental (6+y): same workplace, where no parent worked before matriculation, observations after 5 years post-potential graduation. 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 5
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 6
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 7
The Effect of Peer-to-Peer Transitions

	Log Daily Wage	Firm Change	Firm Pay Rank	Ind. Pay Rank	Occ. Pay Rank
1 Year After	0.046***	-0.007***	1.280***	2.922***	1.198**
	(0.015)	(0.010)	(0.456)	(0.647)	(0.564)
2-5 Years After	0.036***	-0.016**	0.174	1.334**	0.208
	(0.013)	(0.007)	(0.454)	(0.624)	(0.564)
R-squared	0.652	0.380	0.514	0.577	0.596
Observations	122,728	109,690	123,783	124,918	115,282

Notes: This table presents estimates derived from the model specified in Eq. 2, with "lag" periods aggregated into the year of transition and the first to fifth full years post-transition. All regressions incorporate transition event fixed effects, calendar by event year fixed effects, second-degree age and years since matriculation polynomials fully interacted with female and Danish citizenship indicators. Firm change is an indicator for being at a different firm the next year. Firm pay rank is derived from the population's individual daily wage distribution for a given year. Industry and occupation pay ranks are defined at the 4-digit classification level, utilizing the population's individual daily wage distribution for a given year. The lowest rank is 1 and the highest is 100. The standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

TABLE 8
The Effect of Top 1% Peers on Career Outcomes

	Log Daily Wages	Income Rank	Ind. Pay Rank	Occ. Pay Rank
Other x Peer Top 1% Share	-0.041	-0.128	-0.272	3.433
	(0.073)	(1.731)	(2.639)	(3.738)
Top 1% x Peer Top 1% Share	0.487***	7.234***	16.150***	20.670***
	(0.138)	(2.521)	(4.648)	(5.886)
P$(H_0 : \alpha^H = \alpha^L)$	0.001	0.015	0.003	0.011
Observations	218,402	220,937	190,827	220,862

Notes: This table presents estimates derived from the model as specified in Eq. 3. 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 income rank variable is defined based on the population's disposable income distribution for a given year. Industry and occupation pay ranks are established at the 4-digit classification level, employing the population's individual daily wage distribution for a specific year. Rank values range from 1 (lowest) to 100 (highest). 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.

Figures

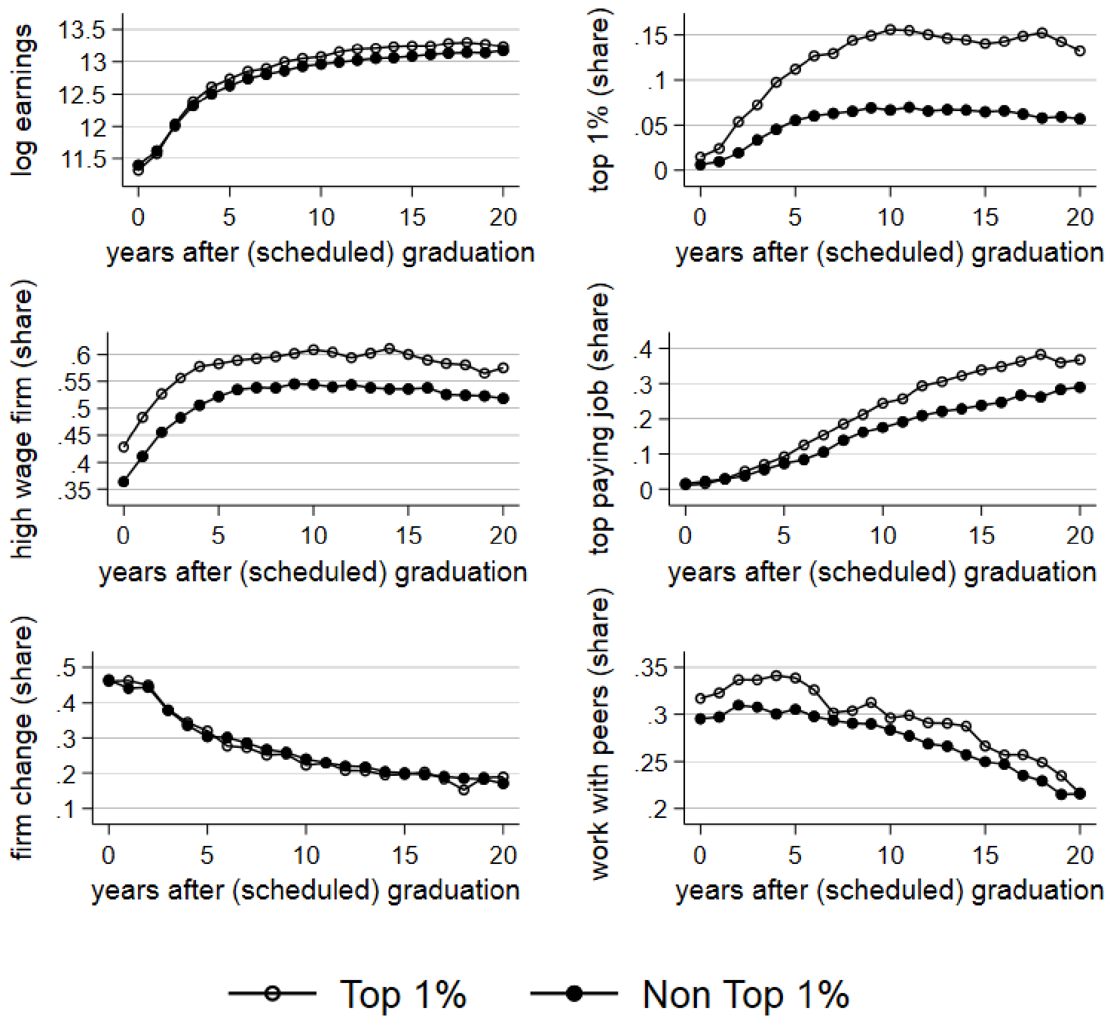

FIGURE 1
Career Dynamics: Top 1% vs Non Top 1% Students

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). The year after scheduled graduation is defined as the matriculation year plus 3. Earnings are measured in 2015 Danish kroner. A former student's top 1% status is based on disposable income percentiles in a given year for their birth cohort. A high-wage firm is defined as one in the top 10% by average daily wages. A student is in a top-paying job if they are among the 10% employees within a firm by daily wages (only for firms with 10 or more employees). The firm change indicator equals 1 if a student changed firms since the previous year. Working with peers is defined as being employed at the same workplace in a given year as a former cohort peer.

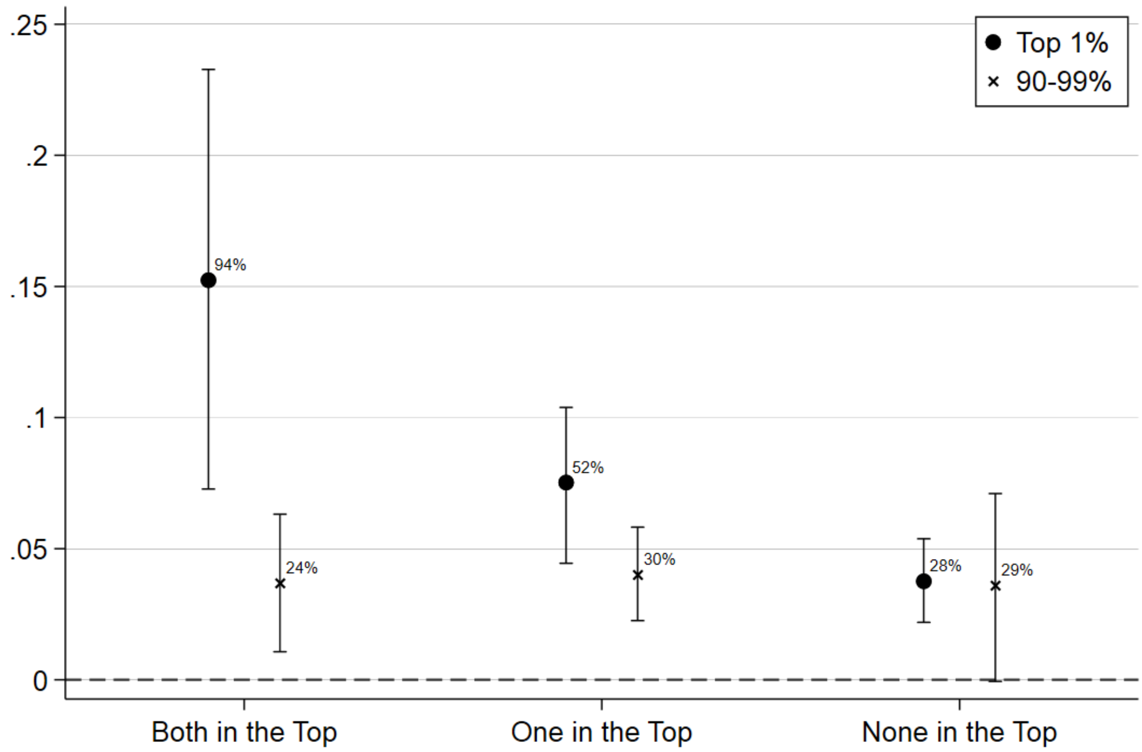

FIGURE 2
Same Workplace: by Father's Income Rank

Note: The coefficients are derived from the linear probability model specified in Eq. 1, incorporating treatment interaction with the father's income group indicators. In the first model ("Top 1%"), the categories "Both in Top" include both students having fathers in the top 1%, "One in the Top" refers to one student having a father in the top 1%, and "None in the Top" means neither student has a father in the top 1%. In the second model ("90-99%"), all top 1% students are dropped from the sample, and "the Top" is defined as students with fathers between the 90th and 99th percentiles. Fathers' income ranks are defined relative to the national disposable income distribution averaged across the five years prior to students' matriculation. 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).

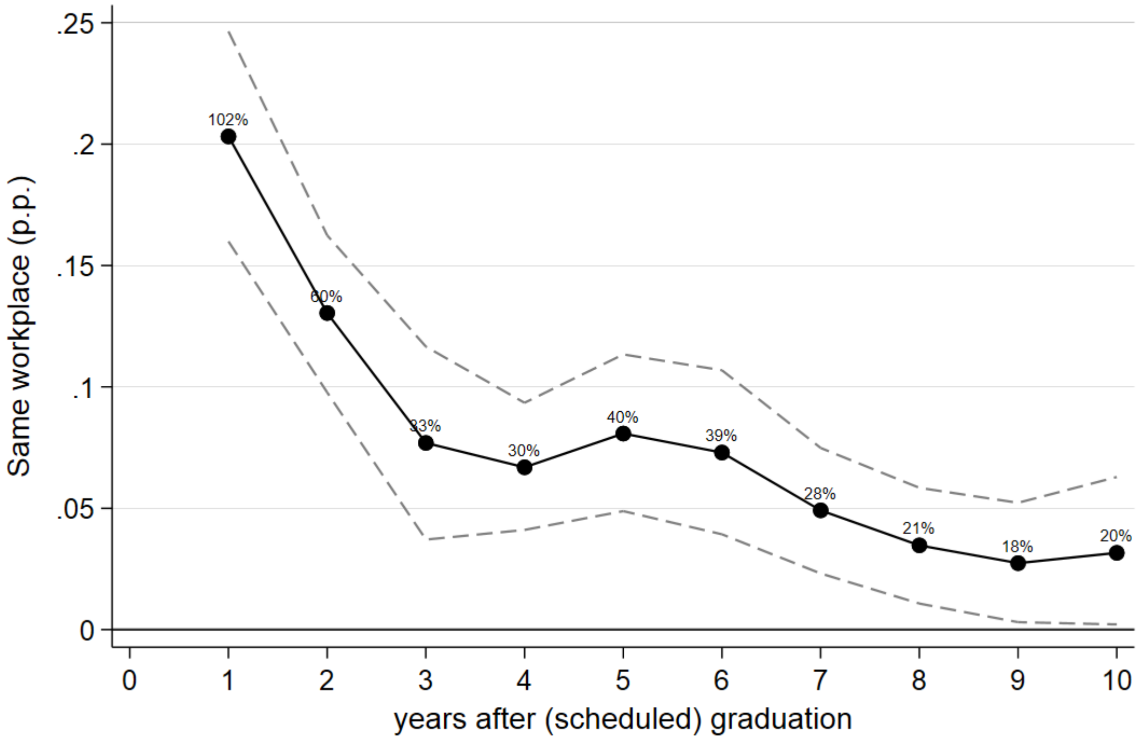

FIGURE 3
Same Workplace: Timing of the Effect

Notes: The coefficients are from the linear probability model specified in Eq. 1, featuring treatment interacted with years subsequent to potential graduation. 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).

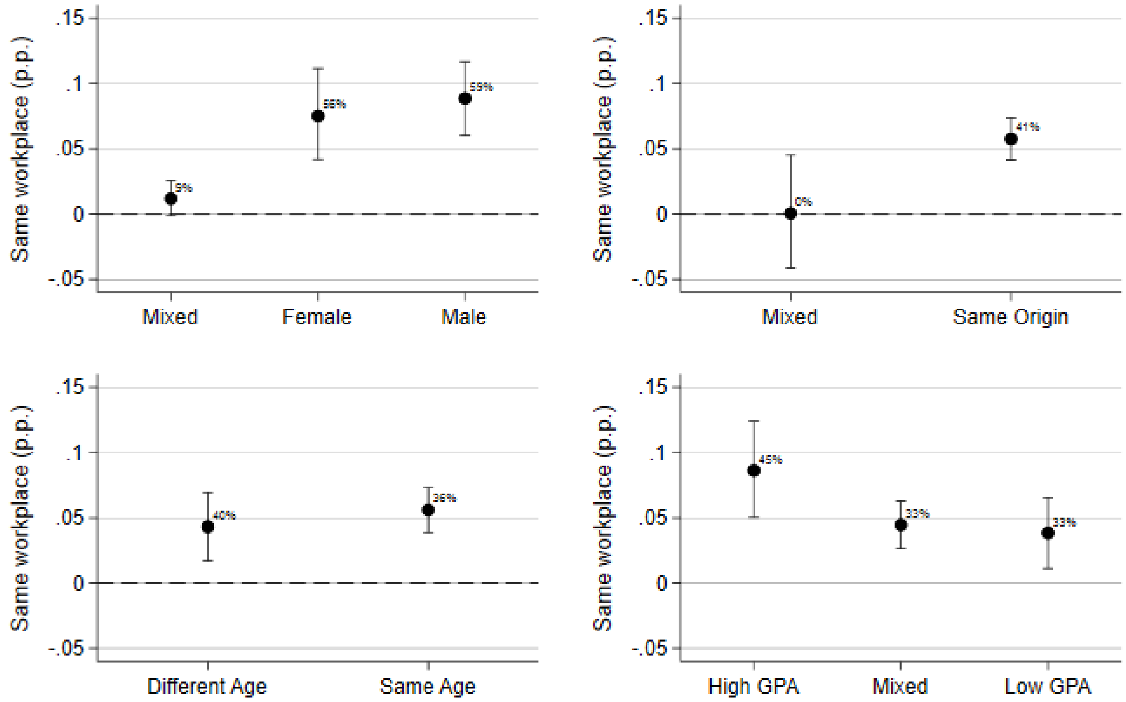

FIGURE 4

Same Workplace: by Gender, Country of Origin, Age and High School GPA

Note: The coefficients are derived from the linear probability model specified in Eq. 1, incorporating treatment interaction 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; two age groups—students with an age difference of more than 1 year and those with less or equal to one year; 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. 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).

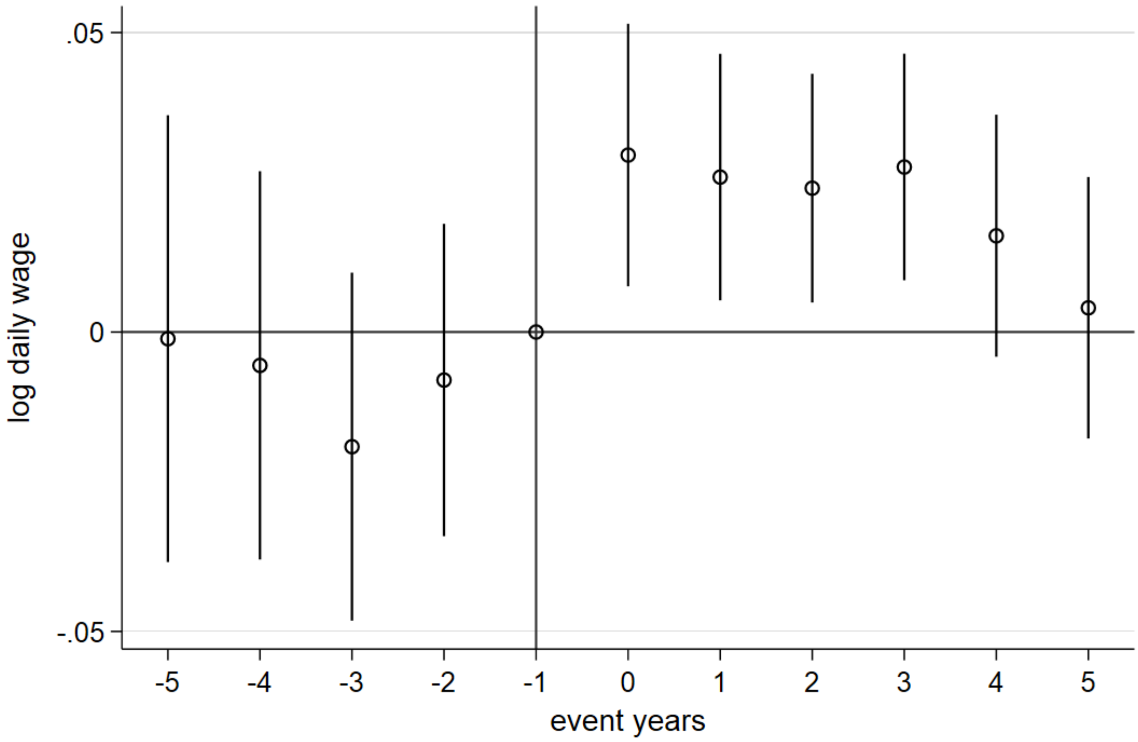

FIGURE 5
The Daily Wage Effect of Peer-to-Peer Transitions

Notes: Coefficients from the event-study in Eq. 2 - the effect of joining a group peer on log daily real wages by a year relative to the transition. The standard errors are clustered at the individual level. The vertical lines indicate 5% confidence intervals.

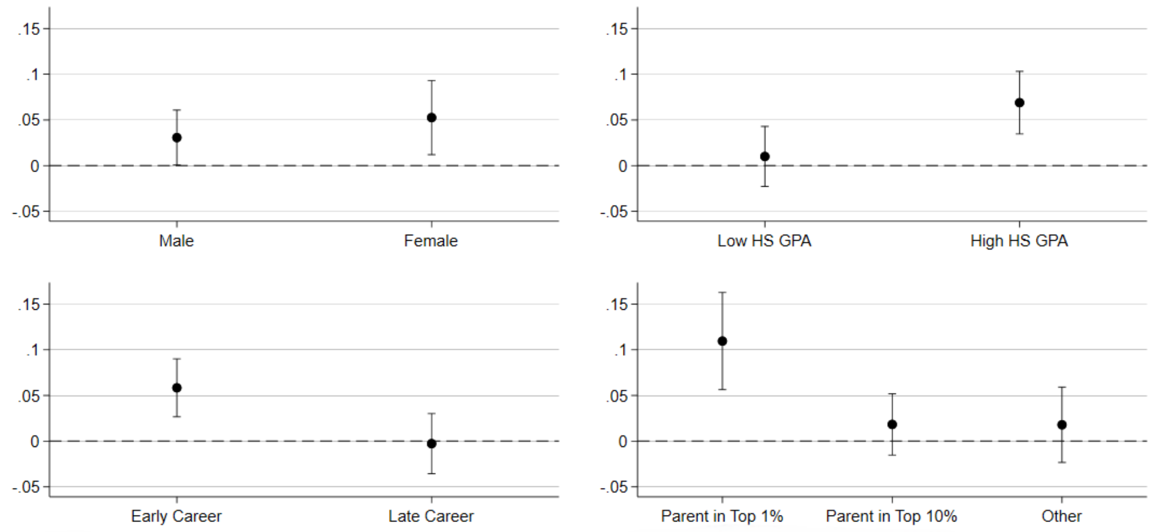

FIGURE 6
The Wage Effect of Peer-to-Peer Transitions, by Group

Notes: The figure displays coefficients extracted from the model described in Eq. 2, separately for various subgroups. The outcome variable is the logarithm of daily real wages (2015 Danish krona). We focus on the year of the job transition, aiming to identify short-term effects. The dataset is divided based on gender, high-school GPA (median split within a matriculation cohort), age (split at the median age of 33), and father's income group (categorized as top 1%, top 10%, or neither). The standard errors are clustered at the individual level. The vertical lines indicate 5% confidence intervals.

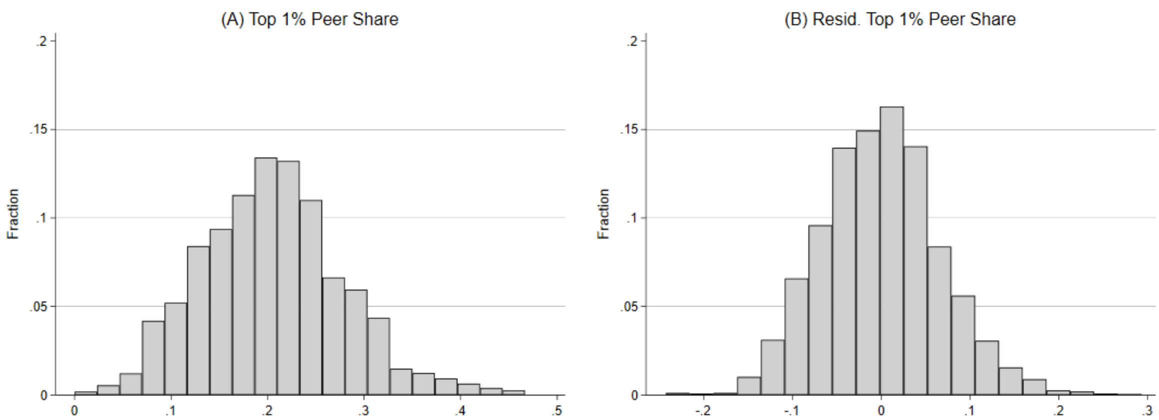

FIGURE 7
Top 1% Peer Group Shares

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 shares residualized peer shares used as indetifying variation in Eq.3.

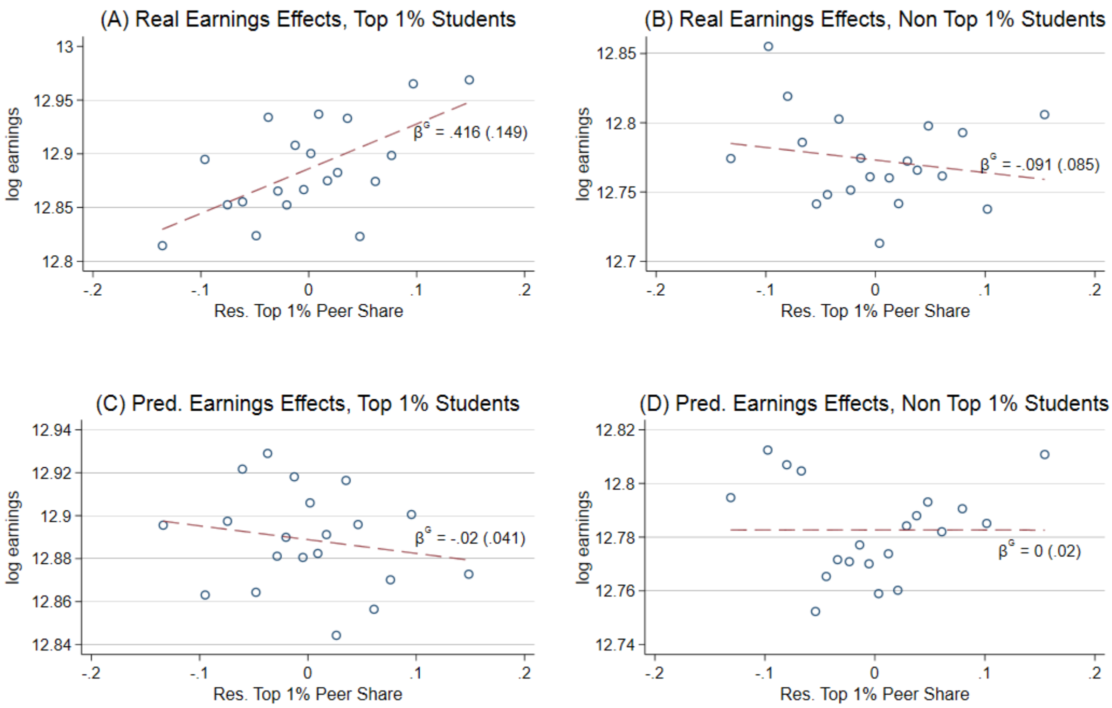

FIGURE 8

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

Notes: The figure presents estimates derived from the model specified in Eq. 3. 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^G_{cit} from Eq. 3, second-degree polynomials of standardized high school GPA, average disposable income, and parents' 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 controls for citizenship, high school track, municipality of residence, and fixed effects for the number of gap years before graduation. 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^G_{cit}. Standard errors are clustered at the peer group level.

Appendix Tables

TABLE A.1
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.2
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.3
The Effect of Peer-to-Peer Transitions, Robustness

Panel A: Alternative Treated and Control Groups				
	Baseline	First Time Treated	Never Treated Controls	Both
1 Year After	0.046***	0.048***	0.050***	0.051***
	(0.015)	(0.016)	(0.015)	(0.017)
2-5 Years After	0.036***	0.050***	0.041***	0.054***
	(0.013)	(0.014)	(0.014)	(0.015)
R-squared	0.652	0.650	0.648	0.646
Observations	122,728	116,978	101,942	96,192

Panel B: Destination Job Controls				
	Occupation	Industry	Firm	Workplace
1 Year After	0.052***	0.031**	0.036**	0.048**
	(0.015)	(0.015)	(0.018)	(0.019)
2-5 Years After	0.047***	0.042***	0.043***	0.040**
	(0.014)	(0.014)	(0.017)	(0.018)
R-squared	0.712	0.713	0.747	0.779
Observations	106,847	113,877	101,166	76,632

Panel C: Origin Job Controls				
	Occupation	Industry	Firm	Workplace
1 Year After	0.041***	0.037**	0.050**	0.050**
	(0.015)	(0.016)	(0.021)	(0.021)
2-5 Years After	0.042***	0.029**	0.034*	0.034*
	(0.014)	(0.014)	(0.019)	(0.019)
R-squared	0.716	0.724	0.779	0.779
Observations	100,937	106,797	57,895	57,895

Notes: This table presents estimates derived from the model specified in Eq. 2, with "lag" periods aggregated into the year of transition and the first to fifth full years post-transition. All regressions incorporate transition event fixed effects, calendar by event year fixed effects, second-degree age and years since matriculation polynomials fully interacted with female and Danish citizenship indicators. After the baseline estimates, Panel A presents results where the treatment group is restricted to only the first instances of treatment ("First Time Treated"), the control group to students who are never treated ("Never Treated Controls"), and a combination of both ("Both"). Panel B adds the destination job interaction to the calendar by event year fixed effect, and Panel C adds the origin job interaction to the calendar by event year fixed effect. Columns define jobs on the level of a 4-digit occupation, a 4-digit industry, a firm, and a workplace. The standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

TABLE A.4
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. 3 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' disposable 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.5
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. 3 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.6
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. 3. 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.7
The Effect of Top 1% Peers on Daily Wages, Alternative Treatment Definitions

	(1)	(2)	(3)	(4)
Other x Peer Top 1% Share (fathers)	-0.036			
	(0.074)			
Top 1% x Peer Top 1% Share (fathers)	0.411***			
	(0.137)			
Other x Peer Top 1% Share (mothers)		-0.051		
		(0.169)		
Top 1% x Peer Top 1% Share (mothers)		-0.346		
		(0.840)		
Other x Peer Top 10% Share (fathers)			-0.023	
			(0.055)	
Top 10% x Peer Top 10% Share (fathers)			0.027	
			(0.055)	
Other x Peer Top 10% Share (mothers)				0.033
				(0.060)
Top 10% x Peer Top 10% Share (mothers)				0.131*
				(0.072)
Observations	169,203	169,203	169,203	169,203

Notes: This table presents estimates derived from the model as specified in Eq. 3 with alternative definitions of the treatment variable. 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. Log real daily wages are used as an outcome variable in all columns. Column (1) presents a baseline estimate from Table 8. Column (2) adopts mothers' status for defining their own Top 1% standing and computing the share of Top 1% peers. Column (3) groups fathers based on belonging to the top 10% rather than the top 1%. Column (4) utilizes mothers' status for the definition of their own Top 10% standing and to compute the share of Top 10% peers. Across all group definitions, parental disposable income is averaged across all available observations 5 years prior to 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.

Appendix Figures

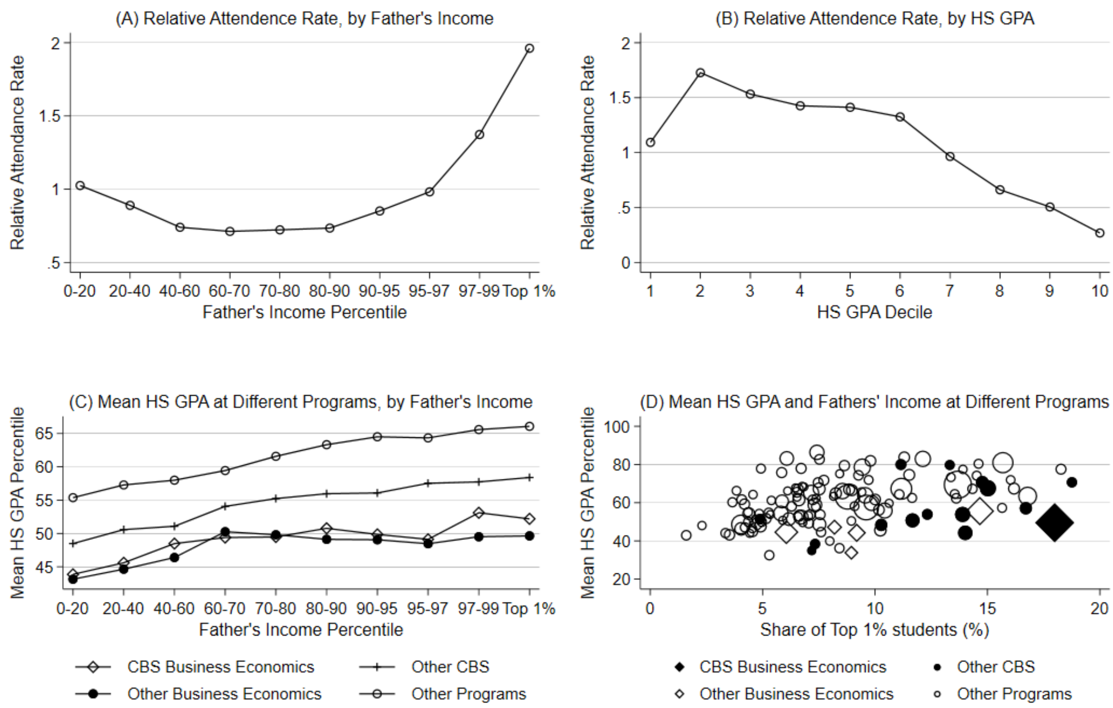

FIGURE A.1
Father's Income and High School GPA: CBS Business Economics vs Other Programs

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. 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. 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.

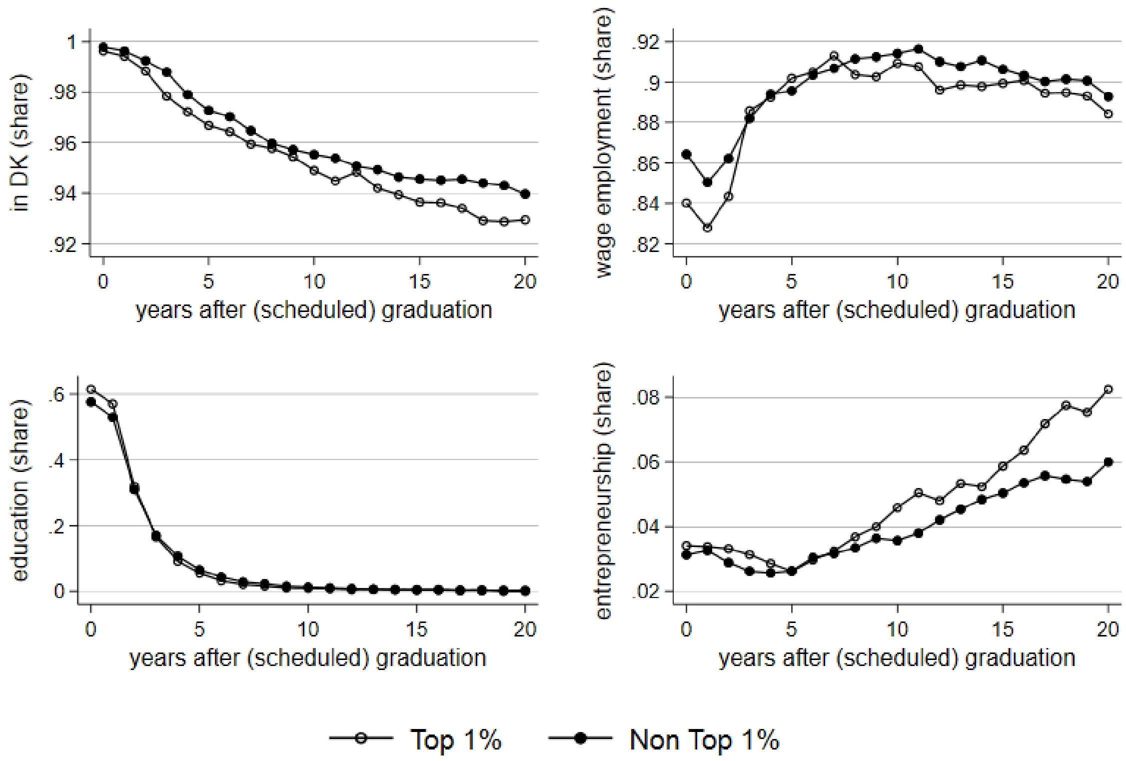

FIGURE A.2
Sample Selection Dynamics: Top 1% vs Non Top 1% Students

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 3. '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.

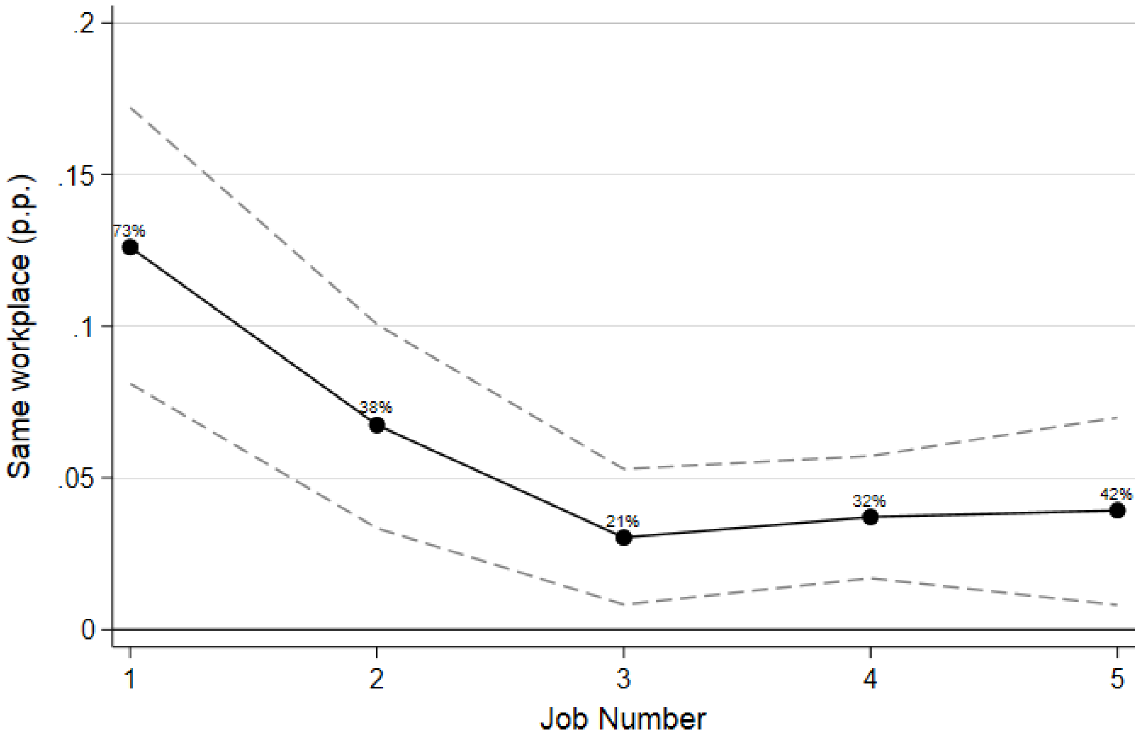

FIGURE A.3
Same Workplace: Timing of the Effect, by Job Order

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).

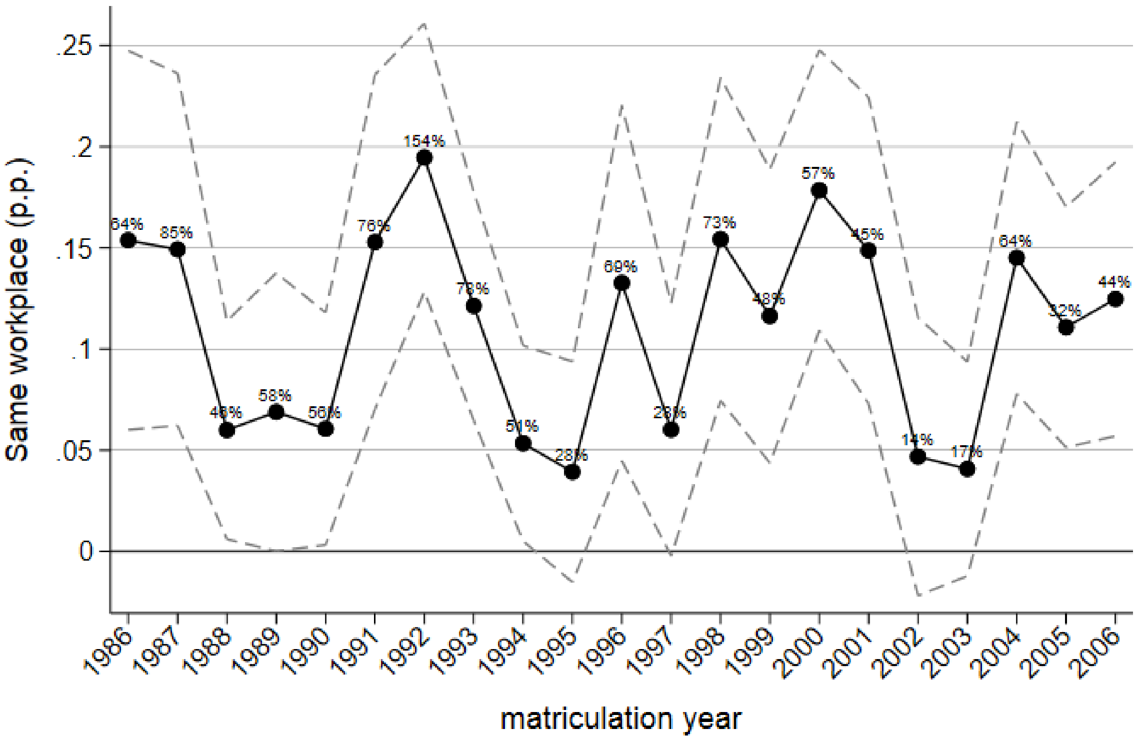

FIGURE A.4
Same Workplace: Timing of the Effect, by Matriculation Cohort

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.

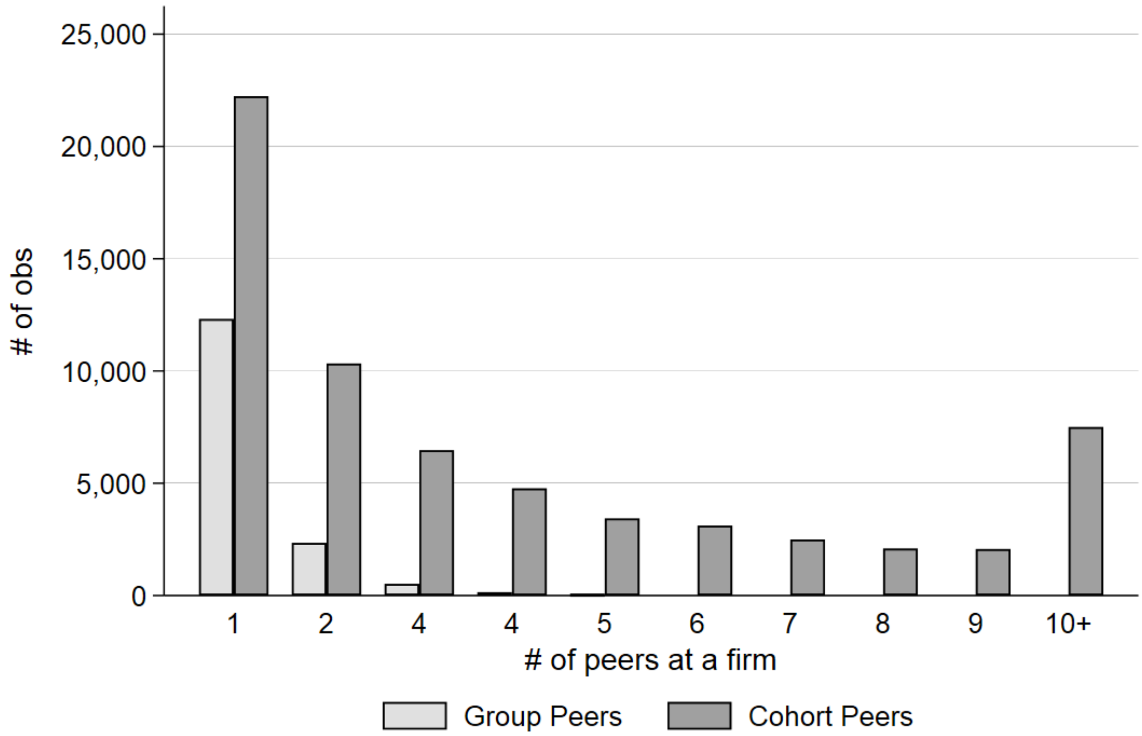

FIGURE A.5
Number of Group and Cohort Peers in a Firm

Notes: 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.

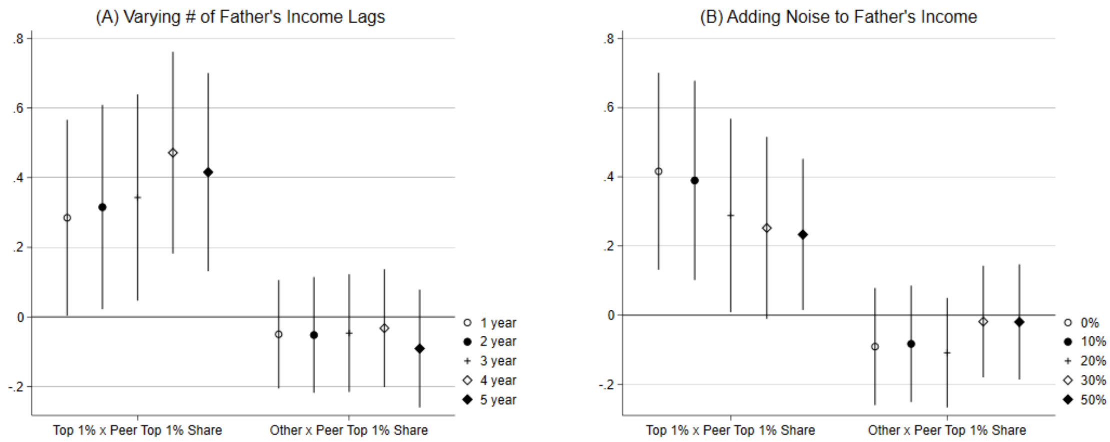

FIGURE A.6
Top 1% Peer Group Shares

Notes: This table presents estimates derived from the model as specified in Eq.3 with alternative definitions of the treatment variable artificially varying the level of "noise". Panel (A) varies the number of years used to calculate father's disposable income between 5 years (baseline) and 1 year prior to matriculation. Panel (B) uses a multiplicative noise model, where the true level of disposable income is multiplied by a random variable $\epsilon \sim \mathcal{N}(0, \sigma^2)$. We vary σ between 0 (baseline) and 0.5. 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.