PEERS, HEIRS AND CAREERS: LABOR MARKET EFFECTS OF ALUMNI NETWORKS*

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August 24, 2023

Abstract

How do social connections among business school peers contribute to career success and economic mobility to top jobs? Using long-standing records of student random assignment to tutorial groups at Copenhagen Business School, merged with comprehensive career data from Danish registers, we observe students sharing career similarities with former group peers, surpassing those within the same cohort. The "excessive" tendency to share occupations, industries, and secure em-

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We would like to thank Alex Bryson, Tor Eriksson, Jan Feld, Stefanie Fischer, Hedvig Horvath, Francis Kramarz, Oskar Nordström Skans, Rune Majlund Vejlin, and Herdis Steingrimsdottir. Gorshkov acknowledges funding from the ERC advanced grant FIRMNET (project ID 741467). Sandoy thanks the Novo Nordisk Foundation grant number NNF16OC0021056 for support on this project. This paper also benefited from seminar participants at Aarhus, CBS, SOFI, UCL and Uppsala. We also thank CBS for providing the data. A special thanks to Annette J. Hansen, Louise W. Jensen, Anne-Sophie Kvisgaard, Tine B Poulsen, and Pernille Brandt who assisted in acquiring the data, understanding the structure, and with information on the allocation of students to peer groups. Finally, we would like to thank Marie Skibsted for sharing her valuable experience from her own work on peer effects at CBS.

ployment with the same employers is explained by peers working together at the same workplace. This effect is partially driven by a propensity to work at firms connected to peers' parents. Comparison of job transitions to firms with group peers versus cohort peers suggests that students benefit from their alumni network, accessing higher-paying jobs. The business school has historically attracted students from the Danish economic elite. Students who already share a privileged background have a higher "excess" propensity to work together and gain more from joining firms with their peers. Moreover, these students derive significant career benefits from being assigned to peers from rich families, while no similar effects are observed for students from less affluent upbringings. This suggests that complementarity between elite family backgrounds and the effect of connections potentially perpetuates inequality and impedes upward mobility. *JEL classification:* I24, I26, J62.

I 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 on elite business education programs tend to favour students from wealthy backgrounds (Zimmerman, 2019). These facts prompt the question regarding the role of alumni ties among business school graduates 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

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

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 business 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 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 Danish 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. The availability of population-wide registers enables us to link students with their parents, granting us access to parental income levels and employment histories.

Three key findings are yielded by our study. Firstly, we identify the causal impact of peers on individual career trajectories, evidenced by students' inclination to select industries, occupations, and employers that align closer with their tutorial group peers than cohort peers. This tendency 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 advancements, indicating that the phenomenon of "working together" goes beyond the non-monetary benefits of interacting with friends and signifies access to superior job opportunities through social connections. Lastly, we demonstrate that the influence 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 affluent families. Analysis using the linear-in-means model reveals that students with fathers in the top 1% of the national income distribution experience significant career advantages when assigned to a tutorial group with a higher proportion of students from similarly privileged backgrounds. This supports the hypothesis that primarily business school 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 unfolds in three steps. First, to identify the causal effect of peers on choices after graduation, a dyadic approach is employed. Pairs of students 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. These effects can be attributed to students working together at shared workplaces. Peers are not significantly more likely to have similar careers (in terms of industries, occupations and employers) while working at different offices. The workplace effect is relatively stronger, as evidenced by a pair of group peers being over 40% more likely to work in the same workplace during the first ten years after graduation compared to a pair of cohort peers (3-4% for industries and occupations, and 20% for employers). Importantly, no evidence is found to support peer similarities in educational choices as a potential mediator of the observed career similarity effect. 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.

Social ties formed during university exhibit 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. In the years immediately after graduation, peers are also "excessively" more likely to work in workplaces linked to a student's parents. This hints that the peer's parental networks contribute to part of the career-start effect. Moreover, the workplace effect is characterized by homophily across several dimensions, notably in terms of gender, country of origin, and age similarity. 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 in the year before matriculation), with a fourfold difference compared to students without such background (111% versus 27%). These pronounced differences fade when using a less strict definition of a "rich family" based on paternal income in the top 10% of the national income distribution.

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 and more stable employment. These returns are particularly pronounced in the early stages of one's career, for high-ability students and for students from affluent backgrounds.

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

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 varying proportions of students from families in the top 1% of the Danish income distribution. Our analysis reveals that 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 wages and incomes, and secure employment at higher-paying segments of the economy. 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 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 the broader literature examining the role of peers in educational settings for determining labor market outcomes (e.g., Black et al. (2013), Bjerge 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), 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)).

Skibsted (2016), Anelli and Peri (2017) and Feld and Zölitz (2022)).

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 operates 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 enduring influence of social connections among elite academic peers even in this egalitarian

[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 degree programs. However, the effect is substantially stronger for students from high SES backgrounds.

setting. Additionally, we capitalize on 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 subsequent section provides an overview of the institutional context of the Business Economics Program at CBS, outlines the data sources employed, and presents descriptive statistics. In Section III, we employ a dyadic regression framework to examine network effects by exploring "excess" career similarities. Section IV investigates the career implications of peer-to-peer transitions, while Section V adopts a linear-in-means approach to discern varying returns to elite peers. The paper concludes with a final section.

II Data and Institutional Background

II.A 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 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 necessitated 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. 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.[5] 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

[5]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.

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.[6] Throughout the program, students were expected to have tutorial sessions together within their peer groups, except for some elective courses in the final year. It is crucial to note 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.[7] Consequently, crucially for our empirical approach, interactions among group peers were more substantial compared to those among cohort peers.

II.B Data Sources and Sample Selection

For this study, we employ a combination of administrative data from Copenhagen Business School and Danish administrative register 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.[8] 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

[6]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.

[7]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.

[8]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).

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 enhance the CBS data by integrating it 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.

The primary data source for this study is the Danish matched employer-employee data (IDA). We utilize the annual cross-section of jobs (representing all primary jobs in the last week of November) for the period 1990-2016.[9] The data contains labor market outcomes (employment, occupation, industry, wages and job spell duration) and identifiers of firms and workplaces. We limit our sample to the employment spell in which each student had the highest earnings within a given year. 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 DISCO classification[10], and industries are defined on the 4-digits level of the Danish industrial classification (DB).[11] To study the post-CBS

[9]In addition, for the years 2008-2016, we have access to the monthly employer-employee register (BFL), which we use to construct variables for the job spells from IDA in this time-period.

[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 DB corresponds to the EU classification of industries (NACE)

educational trajectories of students in our sample, we utilize administrative data on individual education spells in Denmark (KOTRE).

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. We define wage employment as having non-zero wages and a non-missing employer identifier in a given year. As a result, observations of workers in non-employment and self-employment are excluded from our analysis. We assess whether these sample restrictions potentially threaten our empirical strategy's validity.

[Table I about here.]

II.C Summary Statistics

Table I presents summary statistics for our sample of 12,517 students. A majority (approximately two-thirds) are male, with an average starting age above 21 years. Foreign citizens constitute a small portion (5%) of the student body. On average, students exhibit slightly higher high school GPAs compared to their graduating cohort in the academic track.[12] A significant portion (75%) of students have prior work experience in Denmark before commencing the program, while the program's dropout rate stands at 33%. Each student has an average of around 35 group peers and nearly 600 cohort peers.It is worth noting that many students come from affluent backgrounds, with approximately 20% of fathers belonging to the top 1% of the income distribution, and a similar proportion of mothers falling within the top 10%. Notably, students with

[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.

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 bracket. A minor fraction of students (about 5%) lacks parental income data, primarily consisting of foreign citizens.[13]

In comparison to other university programs (see appendix table A.1), the Business Economics program at CBS is bigger, displays lower female representation (typical for Business Economics programs in general) and notably, a significantly larger percentage of students with fathers placed in the top 1% income bracket (double that of non-CBS Business Economics programs). However, in terms of academic selectivity, the program's admission criteria are not overly stringent, with its students' high school GPAs generally falling below those of other university-level programs, yet remaining significantly higher than those of non-CBS Business Economics programs.

[Table II about here.]

Table II presents summary statistics for career outcomes in our panel, both overall and separately for years 5, 10, 15, and 20 after matriculation. Year 5 after matriculation is the expected first year in the labor market for a student with the most common educational path finished in the stipulated time (3 years Business Economics degree with a subsequent 2-year master's degree). In the first five years after matriculation, students typically start their careers at around the 55th percentile of the income rank, rapidly progressing to the 84th percentile, and ultimately reaching the 90th percentile by the 20th year. Notably, almost 70% of students find themselves in the top 10% of the income distribution, with approximately 13% reaching the top 1% after 20 years. Over time, around 2% of students move into management positions five years after matriculation, and this figure rises to 18% after 20 years. At the outset of their careers, students tend to be more concentrated in the same workplaces, firms, and industries, with concentration gradually decreasing over time, reflecting more diverse career trajectories (as measured by the Hirschman-Herfindal index). Graduates of the program exhibit exceptional career outcomes, surpassing not only the average

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

university graduate but also outperforming non-CBS Business Economics graduates and graduates from other CBS programs (see appendix table A.2).

To what extent do students' career paths intersect with each other? Table III provides insights into the prevalence of students sharing the same labor market "cell" with their program, cohort, and group peers.[14] Remarkably, it is not uncommon for students to cross paths with their peers in their career trajectories. The broader the labor market "cell," the more widespread the phenomenon of "working together" becomes. For instance, 35% of students have worked at the same firm as one of their group peers at some point during their careers, while an even higher percentage (79%) have worked in the same industry as their group peers. Additionally, since there are more cohort peers than group peers (groups are nested), it is more frequent for students to work alongside their cohort peers. Specifically, 20% of students shared a workplace with their group peer, while nearly 65% of students shared a workplace with a cohort peer.[15]

[Table III about here.]

[14] In this table cohort peers are also program peers and group peers are both - program and cohort peers.

[15] While there are instances where more than ten cohort peers work at the same firm (as shown in appendix Fig. A.1), the majority of cases when at least one cohort peer is present among coworkers, are characterized by having only one cohort peer at the same firm.

III Career Similarities & Networks

III.A Identifying "Excess" Peer Similarities

To identify the effect of social interactions among university peers on their careers after graduation, we follow a dyadic approach and construct unique pairs of students (i,j) within each matriculation cohort $c(i,j)$.[16] To illustrate the identification challenge and the mechanics of dyadic regressions, let's consider the likelihood of a student pair (i,j) working in the same job k (can be industry, occupation, firm, or workplace) at time t. However, observing a pair of students in the same job doesn't necessarily indicate peer interactions. Students from the same cohort may already have similar abilities and career aspirations before starting their studies, leading to comparable career paths. Additionally, yearly variations in program content might result in students from the same cohort having more similar skill sets after graduation. Starting careers under similar macroeconomic conditions can also contribute to similarities in career paths. Therefore, the propensity of students to be observed at the same job could be expressed as

$$T_{ijkt} = \lambda_{c(i,j)kt} + \alpha_k P_{ij} + u_{ijkt},$$

where T_{ijkt} is the propensity of a pair (i,j) to work in the same job k at time t, $\lambda_{c(i,j)kt}$ reflects all factors that shape similarities in career choices of students from the same matriculation cohort $c(i,j)$ and P_{ij} measures the intensity of social interactions between two students.

Adding up the propensities for all (mutually exclusive) jobs $k \in K$:

$$T_{ijt} = \lambda_{c(i,j)t} + \alpha P_{ij} + u_{ijt}, \tag{1}$$

[16] In our primary analysis, we consider undirected dyads, denoting (i,j) as equivalent to (j,i). Thus, stating that individuals i and j work together is synonymous with stating that individuals j and i work together. However, in some supplementary analyses, we also employ directed dyads, where (i,j) differs from (j,i). This is particularly relevant when examining new matches, distinguishing between the event of student i joining student j and the event of student j joining student i.

where α represents a theoretical parameter of interest. Estimation of Eq. 1 faces several major challenges. First, the intensity of social interactions P_{ij} is usually unobserved. Second, as students might choose to interact with someone who is *ex ante* more similar to themselves (a phenomenon which is often referred to as network homophily), it is likely that P_{ij} and u_{ijt} are correlated and the parameter of interest α would be unidentified. In addressing these challenges, we employ the peer group assignment as a proxy for the intensity of social interactions between students. Leveraging the random allocation of students to their peer groups and the varying intensity of interactions between group peers and cohort peers within the Business Economics program, we estimate the effect of peer interactions on career outcomes.

Our approach to using peer group assignment for identifying the effect of social interactions relies on two assumptions. Firstly, due to the random assignment, 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 T_{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 identification. 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. Moreover, relying on a within-cohort group assignment represents an improvement over studies employing between-cohort comparisons. Controlled peer group assignment offers a more credible solution to the selection issue and 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 matriculation cohorts. In general, the study conditions are much more comparable for students within the same cohort than for those from two different cohorts. Assuming that neither of these assumptions is violated, any "excess" similarities in careers between group peers compared to cohort peers can be attributed to the causal effect of excess interaction within peer groups.

We formalize the intuition in the following empirical specification:

$$F_{ijt} = \lambda_{c(i,j)t} + \beta I_{ij} + \gamma X_{ij} + \epsilon_{ijt}, \tag{2}$$

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. $\lambda_{c(i,j)t}$ represents a set of cohort/year/year-after-graduation fixed effects.[18] X_{ij} is a vector of dyadic covariates based on students' gender, age, and status as a Danish citizen at the time of matricula-

[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).

[18]The number of years after graduation refers to the number of years after "scheduled" graduation, which includes the duration of the program (3 years) plus the number of years after matriculation. Considering this predetermined concept helps address the endogeneity of graduation timing.

tion. To construct the gender variable for each pair of students, we identify whether both students are male or female or differ in gender. Similarly, we define a variable to specify whether both students are Danish citizens, both are foreign citizens, or there is a mix. For age, we control for the absolute age difference within a student pair and the age sum of the pair using a method described by Fafchamps and Gubert (2007) for undirected dyads.[19] The parameter of interest, β, captures the causal effect we want to estimate. To study similarities in education outcomes using cross-section of student-pairs we apply similar model without t subscript.[20] To account for the clustering of data at the cohort level (randomization level), we cluster all dyadic regressions on the cohort level. To address potential inference issues arising from a small number of clusters, we implement a wild cluster bootstrap.

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 P_{ij}, 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, and some groups are merged, leading to changes in group composition. We do not condition our estimates on graduation or staying in the same

[19]When dealing with directed dyads, constructing control variables follows a distinct approach. Here, we encounter four combinations of gender and citizenship indicators (as male-male, female-female, male-female, and female-male). Additionally, we replace the absolute age difference with a simple age difference in our control variables.

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

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.

III.B 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.

We implement a version of the balancing test based on Eq. 2, where future career states are replaced with a set of predetermined variables:

$$F_{ij} = \lambda_{c(i,j)} + \beta I_{ij} + \gamma X_{ij} + \epsilon_{ij}, \tag{3}$$

where I_{ij} is an indicator of being assigned to the same peer group at the time of matriculation and $\lambda_{c(i,j)}$ are matriculation cohort fixed effects. F_{ij}, in this case, might be both an indicator variable reflecting that students belong to the same category or an absolute difference between values of some predetermined variables for a pair of students (i, j).

[Table IV about here.]

To assess the balance between group peers and cohort peers, we conduct a balancing test using variables measured before matriculation. These variables include the distance in standardized high school GPA, indicators for being in the same high school track, being born or being registered before matriculation in the same municipality, distance in father's and mother's number of years of education, indicators for having mothers or fathers in the top 1% of the income distribution, being employed at the same workplace or industry at any point before matriculation, and having parents employed at the same industry or workplace the year before matriculation. Table IV presents the results of the balancing test, and we observe that none of the variables appears to be unbalanced at the 10% significance level. The municipality of residence is close to the 10% threshold. However, this should be considered in the context of a number of tests performed. Overall, we find these findings supportive of the validity of our identification strategy.

[Table V about here.]

III.C Results

III.C.A Main Results

We start by estimating the "excess" similarities formalized in Eq. 2. Table V 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". Although the point estimates appear similar for all outcomes, the recalculated relative effects reveal significant differences. Specifically, a pair of students is approximately 3-4% 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 20% higher probability of working at the same firm and a 40% increase in the probability of working at the same workplace after graduation.[21]

The pronounced concentration of the peer effect at the most granular level, the workplace, challenges the prevailing notion 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. However, an alternative interpretation emerges from our findings. We propose that the robust peer effect observed at the workplace level aligns with the existence of active alumni networks. These networks can serve as invaluable sources of job-related information, enabling graduates to access insights 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.

[Table VI about here.]

To better understand the driving forces behind peer similarities in career outcomes, we investigate whether the workplace effect influences 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.[22] As shown in Table VI, after excluding workplace similarities, the effects

[21]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. This pattern is also reflected in Table III.

[22]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

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 at the 10% level. 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 continue to form 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.

[Table VII about here.]

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. 2 and present the results in appendix Table A.3. Remarkably, even when 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.

Furthermore, 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 VII, 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

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.

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.

III.C.B Timing and Heterogeneity

It is natural to assume that the intensity of post-graduation interaction with former academic peers decreases over time. Fig. I illustrates the timing of "excess" workplace similarities. For firms the observed pattern is qualitatively similar (see appendix Fig. A.2). The effect is strongest a few years after scheduled graduation and diminishes over time.[23] 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 I about here.]

[Figure II about here.]

Social connections tend to form more intensely between individuals that are more similar. We observe striking evidence of homophily among gender lines (Fig. II). To

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

investigate this phenomenon further, we adopt the same empirical approach as before but separately examine the timing of the effect for pairs of students of the same gender and pairs of students of different genders. The observed pattern of peer "excess" similarity is primarily driven by pairs of students of the same gender. For different gender pairs, the effect is significant only during the first two years after graduation and becomes statistically insignificant afterwards. In contrast, for same-gender pairs, the effect remains substantial, reaching as high as 137% the year after graduation and remaining at 50% even ten years after graduation. These findings shed light on the significant influence of gender homophily in shaping career outcomes among former academic peers.

[Figure III about here.]

Fig. III further investigates heterogeneous effects by gender, country of origin, age, and high school GPA. 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 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.[24] 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. Interestingly, we find a significant effect for pairs of the

[24]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 $\frac{n(n-1)}{N(N-1)}$.

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 observe a higher effect for students with similar age, the difference in the coefficients is statistically significant at the 5% level.

To investigate potential heterogeneity based on academic performance, we define high GPA students as those with GPAs exceeding the CBS average. 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 of students when both students have high GPAs. The difference between the effects for high GPA and low GPA pairs is significant on the 10% level. This suggests that the influence of peer interactions on career choices is more pronounced among high achieving students who share similar academic backgrounds.

[Figure IV about here.]

As previously emphasized, a student from our sample not only achieves a career within the upper echelons of the Danish 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 elucidate 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 academic tenure, potentially contributing to their higher educational returns.

To investigate this question, we analyze the effects of being assigned to the same peer group on the probability of working at the same workplace for specific dyads, classified by their father's disposable income ranks in the year before matriculation (Fig. IV). We identify "rich" dyads as pairs of students where both have fathers in the top 1% (top 10%). We then compare these effects to pairs where only one student has a father in the top 1% (top 10%), and to the remaining pairs of students where neither student is in that group. Our findings indicate that among students with parents in the top 1%, the effects are four times as large as compared to students where neither has a father in the top 1%. Interestingly, when we use the less restrictive definition of a "rich" family - a father in the top 10% - the pattern is attenuated, and there are no statistically significant differences in effects. This suggests that the strongest similarities in workplace choices tend to emerge among students coming from the wealthiest families.

III.C.C Robustness Checks

[Table VIII about here.]

[Table IX about here.]

The evidence from Table IV supports the assumption of (conditionally) random assignment of students to peer groups. However, it's important to acknowledge that not all students are observed in our career sample every year, leading to some missing years for certain student pairs. The reasons for this missing data can be attributed to two main factors. Firstly, some students may leave Denmark temporarily or permanently, which is particularly relevant for international students. Secondly, even students who remain in Denmark might experience periods of non-employment, self-employment, or engage in further education. The presence of missing observations introduces potential challenges and biases in our estimates. For instance, if the peer group assignment process influences students' decisions to leave the sample, possibly due to peer effects in migration, and if this decision is associated with their initial propensity to make similar career choices, our estimates could be biased.

To assess the potential bias in our estimates due to sample selection, we conduct a test to estimate the effect of being assigned to the same peer group on being both observed in the wage employment sample in a given year. We use the same specification as in the baseline Eq. 2. The results of this test are presented in the first two columns of TableVIII. The first column includes all available observations in the data, while the second column focuses on the first five years after the planned graduation from the program, where we observed the most substantial effects in our main results. Our findings indicate no significant difference between group peers and cohort peers in terms of being missing from the sample. This result holds for the overall sample and the first five years after graduation. To further investigate the potential impact of group assignment on different stages of the selection process, we examine whether it affects the likelihood of being observed as Danish residents ("Population Sample") and the likelihood of being observed in our career sample, conditional on being both observed among Danish residents ("Employment Sample"). However, neither of these tests identifies significant correlations with the group assignment. Based on these analyses, we can conclude that sample selection is unlikely to cause bias in our estimates, and our results remain robust to potential biases stemming from missing observations in the data.

While investigating the effects of peer interactions on careers, it is essential to consider that similarities in career choices may also stem from peer influences on educational decisions prior to starting their careers. For instance, if peers exhibit similar drop-out behavior, it might lead to similar job choices even without further post-graduation interactions in the labor market.[25] To explore this possibility, we conduct regression analyses (see Table IX) to examine the similarities in educational choices on both Bachelor's and Master's levels, including graduation from the CBS Business Economics program, any Business Economics program, any Bachelor's program, and switching to a different program. However, our findings do not indicate any significant effects of peer group assignment on educational choices. Therefore, this channel is unlikely to explain our main results concerning career similarities among business

[25]It is worth noting that even in such a scenario, it would be challenging to explain how similarities in educational choices drive career similarities at the workplace level, as, perhaps, educational choices are more likely to impact occupation and/or industry choices.

school peers.

To ensure the robustness of our results, we also investigate whether using the linear probability model in our baseline specification (Table V) may lead to misleading outcomes due to the highly uneven distribution of the outcome variable. To address this concern, we repeat our baseline analysis using a logit specification (see appendix Table A.4). Remarkably, the coefficients in the logit model exhibit the same relative order and magnitude as those in our baseline specification. The strongest effects are once again observed at the most disaggregated level. Specifically, being assigned to the same peer group increases the probability for a pair to work together at the same workplace by 32%. The average marginal effects from the logit specification are consistent with the coefficients in the linear probability model. These findings demonstrate the robustness of our results, as the logit model confirms the patterns observed in our baseline analysis.

IV Wage Effects of Peer-to-Peer Transitions

IV.A 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. 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.[26]

The Fig. V illustrates the intuition behind our empirical strategy - contrasting the wage trajectory for a worker joining a group peer ("Group peer transition") with a wage trajectory for a worker joining any cohort peer (including a group peer) ("Cohort peer transition"). Since the graph depicts unconditional raw means, the level differences are not easily interpretable. However, the dynamics tell a clear story - before the transition, trajectories move in parallel, but after the transition, a student who has joined a group peer experiences higher wage growth than one who joined a cohort peer.

For the stacked event study, we construct a panel window around each transition event e, 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

[26]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).

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").[27]

Our approach is formalized in the following regression framework:

$$y_{eit} = \mu_e + \lambda_{\tau t} + \sum_{\substack{-5 \leq h \leq 5 \\ h \neq -1}} \gamma^{\tau} GroupPeer_e + \beta X_{et} + \epsilon_{eit}, \tag{4}$$

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 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 inter-

[27] As a result, the same student i may appear multiple times in the same year t.

actions. 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.[28]

Our empirical strategy differs from the approaches commonly employed in the literature that study the effects of referrals on labor market outcomes. Some studies rely on detailed personnel records and compare workers hired through referrals to non-referred workers within a given firm (for example, Burks et al. (2015) and Brown et al. (2016)). Another branch of research deals with 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)). 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. Therefore, even though we show that our main findings in this section are robust to the inclusion of destination job controls, we do not control for them in our baseline specification.

[28]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.

IV.B Results

In Fig. VI, we present the event study results using the specification in Eq. 4. The figure displays the estimated coefficients on the leads and lags of treatment. We find no statistically significant differences in pretrends between the treatment and control groups, while the treatment lags are significant and positive. This indicates that joining a firm with a group peer is associated with 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.[29]

Table X presents the effect on log daily wages. As depicted in Figure VI, we observe a positive wage benefit for transitions to joining a group peer. In the year of the transition, the wage benefit is around 6%, and it gradually declines to 4% in the following years, remaining statistically significant. 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. While there is no immediate effect on job turnover in the year of the transition, we find a significant negative effect of 1.6 p.p. on the probability of leaving a job in the first to fifth year following the transition. Additionally, these jobs tend to be at substantially larger firms and are in higher-paying 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.5 demonstrates that the wage effect remains generally robust when including destination job and origin job fixed effects at various levels of granularity.

[Figure V about here.]

[29] 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 VI about here.]

Fig. VII presents the heterogeneous returns to joining a group peer on daily wages. Generally, the strength of the wage effect is higher for subgroups with a higher excess tendency of working together, as explored in the previous section. Both male and female workers benefit from joining a group peer, with no significant difference in magnitude between the two groups. However, the effect is more concentrated among workers of higher ability, as measured by a high school GPA above the average among CBS students. We observe the largest returns to joining a group peer for younger workers in the early career stages (defined as being below the median age of 33 in our panel). Most notably, the wage benefit is concentrated among workers from the most wealthy backgrounds, particularly those with a father in the top 1% of the Danish income distribution. This finding indicates that students from affluent families not only network more but also benefit more from networking.

[Table X about here.]

[Figure VII about here.]

V Career Effect of Exposure to Top 1% Peers

V.A 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. Therefore, we investigate if the causal effect of being assigned to a group with more peers coming from rich Danish families is significantly higher for students who also come from affluent families.

In this section, we employ the standard linear-in-means specification used 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):

$$y_{it} = \alpha^G \times \overline{Top1\%}_{-ig} + \beta^G X_{it} + \lambda_c^G + \psi_t^G + \epsilon_{it}, \tag{5}$$

where $G \in \{H; L\}$.[30] The variable y_{it} represents one of the career outcomes we are interested in. The vector X_{it} includes indicators for Danish citizenship and gender, as well as age and years since matriculation polynomials. The terms λ_c^G and ψ_t^G denote cohort and year fixed effects, respectively. The parameters α^G are the group-specific causal effects of interest, reflecting the effect of the share of top 1% peers on labor market outcomes. Standard errors are clustered at the matriculation cohort level and wild cluster bootstrap is applied.

It is crucial to 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 IV), we conduct linear-in-means type balancing tests.

V.B Results

Applying the framework in Eq. 5, we present the results in Table XI, depicting the career effects of business school peers from affluent family backgrounds. The findings confirm the hypothesis of asymmetric impacts on long-term career success based on peers' social standing. While statistically significant effects are not observed for former CBS students without affluent family status, those originating from top 1% families

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

experience substantial benefits from studying alongside similar peers. A 10-percentage-point increase in the share of top 1% students within a peer group (approximately half the average) yields a 3% increase in real daily wages and a .75-rank rise in income status. These effects correspond to higher-paying jobs in industry and occupation rankings.

As shown in Table I, 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 XII 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 (except for the positive effect on the GPA of non-top 1% students, which is significant at the 10% level).

To enhance the robustness of our interpretations, we perform several supplementary tests. Firstly, as shown in appendix Table A.6, we conduct a linear-in-means type balancing test by replacing career outcomes with various predetermined individual-level characteristics in Eq. 5. Consistent with our earlier findings (Table IV), the results fail to provide evidence contradicting the notion that peer groups are not sorted based on student-level background variables for both groups of students. Secondly, 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. As discussed earlier, we lack data on outcomes for students who leave Denmark or are not engaged in wage employment (e.g., those who are unemployed or self-employed). 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 indicated in appendix Table A.7, our findings do not support such a bias. We observe no discernible impact on the likelihood of being included in the career sample or the career sample within the initial 5 years after graduation (a period when some students might still be pursuing education), and no influence on being registered as a Danish resident or engaging in wage employment, given residency.

Lastly, we explored 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 appendix Table A.8 indicate that the most substantial impact is associated with the upper echelon of the peers' background distribution, particularly peers with top 1% fathers.

[Table XI about here.]

[Table XII about here.]

VI 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 dataset, which included extensive career and family background information from Danish linked employer-employee data, provided us with in-depth insights into the individual career paths of the students. 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 often worked 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 revealed that students with fathers in the top 1% of the national income distribution experienced significant career advancements when assigned to tutorial groups with a higher proportion of peers from similarly privileged backgrounds. In 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 business school peers on individual career trajectories. These connections are instrumental in facilitating access to higher-paying jobs and fostering career advancements, especially for students hailing from privileged backgrounds. Our findings bring to light 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 I
Student Characteristics, by Father's Income Rank

	Total	Top 1%	Non Top 1%	Missing
Female	0.34	0.33	0.35	0.38
	(0.48)	(0.47)	(0.48)	(0.48)
Danish citizen	0.95	0.99	0.99	0.34
	(0.21)	(0.07)	(0.11)	(0.47)
Age at matriculation	21.44	20.96	21.41	23.33
	(2.34)	(1.62)	(2.15)	(4.68)
High School GPA	0.09	0.18	0.06	.
	(0.82)	(0.78)	(0.83)	.
Dropout	0.33	0.26	0.34	0.48
	(0.47)	(0.44)	(0.47)	(0.50)
Any work experience in DK	0.75	0.75	0.78	0.16
	(0.43)	(0.43)	(0.42)	(0.37)
Father's education (in years)	13.63	14.84	13.32	.
	(2.78)	(2.35)	(2.80)	.
Mother's education (in years)	12.89	13.62	12.73	.
	(2.71)	(2.42)	(2.74)	.
Father in top 1%	0.20	1.00	0.00	.
	(0.40)	(0.00)	(0.00)	.
Mother in top 1%	0.02	0.04	0.02	.
	(0.14)	(0.20)	(0.12)	.
Father in top 10%	0.63	1.00	0.53	.
	(0.48)	(0.00)	(0.50)	.
Mother in top 10%	0.22	0.27	0.20	.
	(0.41)	(0.44)	(0.40)	.
Group size	35.86	35.38	35.88	37.12
	(5.93)	(5.86)	(5.92)	(6.00)
Cohort size	599.56	602.49	599.27	594.27
	(47.22)	(47.20)	(47.59)	(41.23)
Observations	12,517	2,204	9,618	695

Notes: Descriptive statistics for CBS sample students based on fathers' income groups. Rows represent variables, columns - parental 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 in the year prior to matriculation; 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 II
Career Outcomes, by Year Since Matriculation

	Total	Year 5	Year 10	Year 15	Year 20
Income rank	81.04	55.06	84.24	88.91	90.11
	(20.25)	(18.52)	(16.05)	(13.96)	(13.75)
Top 10%	0.47	0.04	0.48	0.64	0.69
	(0.50)	(0.20)	(0.50)	(0.48)	(0.46)
Top 1%	0.07	0.00	0.03	0.09	0.13
	(0.25)	(0.04)	(0.16)	(0.28)	(0.34)
Log earnings	12.69	11.65	12.78	13.00	13.11
	(0.95)	(0.92)	(0.71)	(0.72)	(0.79)
Log daily wage	7.01	6.13	7.06	7.26	7.37
	(0.78)	(0.82)	(0.54)	(0.58)	(0.65)
Manager	0.10	0.03	0.05	0.11	0.18
	(0.30)	(0.16)	(0.22)	(0.31)	(0.38)
Firm size (FTE)	394.01	336.34	409.20	427.89	392.12
	(767.63)	(708.34)	(765.04)	(805.34)	(743.90)
HHI, workplaces	0.37	0.47	0.39	0.34	0.30
	(0.09)	(0.08)	(0.06)	(0.04)	(0.03)
HHI, firms	0.54	0.65	0.57	0.51	0.45
	(0.12)	(0.09)	(0.13)	(0.09)	(0.04)
HHI, industries	2.10	2.56	2.37	1.91	1.61
	(0.58)	(0.73)	(0.37)	(0.26)	(0.15)
Observations	174,557	10,086	10,291	7,832	5,492

Notes: This table presents descriptive statistics for career outcomes across a panel of employed students from the CBS sample (Total) and at four time points: years 5, 10, 15, and 20 after matriculation. The values in the cells represent the mean of each variable, with standard deviations indicated in parentheses. The variables Income rank, Top 10%, and Top 1% are determined based on the Danish disposable income distribution for the corresponding years. Earnings and daily wage variables are measured in terms of the log of 2015 DKK. The classification for managers is based on the occupation code. The HHI (Herfindahl-Hirschman Index) measures the concentration of students within the cohort working in specific workplaces, firms, and industries.

TABLE III
Career Similarities

	Group Peers	Cohort Peers	Program Peers
Same workplace	0.204	0.645	0.883
	(0.0552)	(0.293)	(0.614)
Same firm	0.348	0.797	0.943
	(0.0993)	(0.414)	(0.715)
Same industry	0.786	0.990	0.999
	(0.339)	(0.870)	(0.987)
Same occupation	0.856	0.985	0.998
	(0.454)	(0.899)	(0.988)

Notes: The table displays the percentages of students in the CBS sample who have ever worked together with their peers and the corresponding percentages of observations within the career panel (in parentheses). Columns categorize peers as students from the same peer group (Group Peers), the same matriculation cohort (Cohort Peers), and students from the CBS sample (Program Peers). Rows define "working together" based on shared workplace, firm, industry, and occupation, with industry and occupation classifications specified at the 4-digit level.

TABLE IV
Dyadic Balancing Test

	HS GPA	HS Track	Place of Birth	Municipality of Residence
Same group	-0.00176	0.156	0.0459	0.127
	(0.00184)	(0.121)	(0.0407)	(0.0749)
P-value	0.352	0.223	0.282	0.104
Baseline	0.872	60.40	2.817	4.090
Observations	3,463,268	3,680,910	3,643,689	3,332,099
	Mother's Education	Father's Education	Mother in Top 1%	Father in Top 1%
Same group	0.00682	-0.00538	0.00542	-0.00724
	(0.00542)	(0.00545)	(0.00527)	(0.0340)
P-value	0.224	0.331	0.339	0.835
Baseline	2.927	2.997	0.0411	3.940
Observations	2,990,758	2,716,390	3,224,432	2,999,602
	Prior Industries	Prior Workplaces	Parental Industries	Parental Workplaces
Same group	0.0679	0.0265	-0.00182	-0.00470
	(0.0582)	(0.0172)	(0.0771)	(0.00905)
P-value	0.254	0.113	0.983	0.610
Baseline	7.843	0.139	4.225	0.0987
Observations	2,970,013	2,435,049	2,663,119	2,403,770

Notes: The table provides regression coefficients resulting from the balancing test specified in Eq. 3 for predetermined student similarities. Standard errors, shown in parentheses, are clustered at the cohort level. Variables used to measure similarities include: difference in standardized high school GPA, indicator for the same high school track, same place of birth (municipaliy for born in Denmark and country for the rest), same municipality of residence, difference in years of education of mothers, difference in years of education of fathers, indicator if both mothers are in the top 1%, indicator if both fathers are in the top 1%, students worked at the same industry prior to matriculation, same workplace prior to matriculation, parents worked at the same industry and the same workplace prior to matriculation. Baseline values and coefficients when the outcome is an indicator variable are scaled by 100 to represent percentage points. P-values reflect coefficients' significance, determined by wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE V

Career Similarities: Baseline Regressions

	Same Industry	Same Occupation	Same Firm	Same Workplace
Same group	0.0724***	0.0970***	0.0674***	0.0608***
	(0.0251)	(0.0305)	(0.0117)	(0.00844)
P-value	0.00870	0.00440	0	0
Effect (in %)	3.834	3.317	20.35	41.01
Baseline	1.889	2.926	0.331	0.148
R-squared	0.00191	0.00240	0.000404	0.000475
Observations	42,864,255	23,133,124	42,864,255	37,422,346

Notes: This table presents estimates from the linear probability model specified in Eq. 2. 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. Sample restricted to the first 10 years after the scheduled 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 VI

Career Similarities: Net of Workplace Effects

	Same Industry	Same Occupation	Same Firm
Same Group	0.00429	0.0532*	0.00524
	(0.0225)	(0.0298)	(0.00560)
P-value	0.852	0.0841	0.363
Effect (in %)	0.239	1.868	2.672
Baseline	1.799	2.850	0.196
R-squared	0.00188	0.00225	0.000206
Observations	37,422,346	20,761,129	37,422,346

Notes: This table presents estimates from the linear probability model specified in Eq. 2. 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. Sample restricted to the first 10 years after the scheduled graduation. The outcome variables are defined as indicator variables for instances where individuals work within the same industry, occupation, or firm, but not within the same workplace. 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 VII
Career Similarities: Parental Workplaces

	Parental (1-5y)	Not Parental (1-5y)	Parental (6-10y)	Not Parental (6-10y)
Same group	0.00735**	0.108***	-0.000542	0.0508***
	(0.00326)	(0.0109)	(0.000811)	(0.00895)
P-value	0.0209	0	0.524	0
Effect (in %)	89.75	50.45	-24.51	38.01
Baseline	0.00819	0.214	0.00221	0.134
R-squared	0.0000990	0.000453	0.0000310	0.000346
Observations	9,245,693	9,245,693	19,792,366	19,792,366

Notes: This table presents estimates from the linear probability model specified in Eq. 2. 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 VIII
Career Similarities: Sample Selection

	Sample (All)	Sample (5 years)	Population Sample (All)	Population Sample (5 years)	Employment Sample (All)	Employment Sample (5 years)
Same group	0.0165	-0.00280	0.00940	0.0195	0.000983	0.0373
	(0.0430)	(0.0490)	(0.0179)	(0.0191)	(0.0419)	(0.0350)
P-value	0.712	0.959	0.608	0.328	0.985	0.298
Baseline	66.85	67.97	40.70	70.27	72.06	69.34
R-squared	0.0619	0.0649	0.854	0.863	0.0213	0.0229
Observations	63,615,468	18,404,550	63,615,468	18,404,550	25,786,678	12,874,238

Notes: The table presents the outcomes of the sample selection tests utilizing the baseline specification of Eq. 2. The outcome variables consist of indicator variables, taking the value one if both students are identified in our career sample during a specific year ("Sample"), within the Danish resident population for a given year ("Population Sample"), or within the employment sample, given both are observed as Danish residents during a particular year ("Employment Sample"). For each outcome variable, both all available observations and solely the first 5 years post-(scheduled) graduation from the program are used. 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 IX

Career Similarities: Educational Choices

	Graduate from CBS HA	Graduate from any HA	Graduate from any Bachelor's	Switch to other program
Same group	0.0561	0.0558	0.114	0.0205
	(0.0886)	(0.0898)	(0.0777)	(0.0125)
P-value	0.535	0.543	0.164	0.120
Effect (in %)	0.115	0.114	0.208	2.987
Baseline	48.70	49.02	54.72	0.686
R-squared	0.0485	0.0493	0.0588	0.00276
Observations	6,796,936	6,796,936	6,796,936	6,796,936

	Master's start	Master's graduate	Master's program	Master's institution
Same group	0.0482	-0.0117	0.285	0.0905
	(0.108)	(0.0682)	(0.205)	(0.0683)
P-value	0.667	0.864	0.182	0.197
Effect (in %)	0.0910	-0.0373	0.488	0.113
Baseline	53.02	31.24	58.34	80.27
R-squared	0.0651	0.0643	0.00970	0.0223
Observations	6,796,936	6,796,936	3,605,866	3,605,866

Notes: The table presents estimates derived from the linear probability model as specified in Eq. 3 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 X
The Effect of Peer-to-Peer Transitions

	Log Daily Wage	Firm Change	Firm Size	Ind. Pay Rank	Occ. Pay Rank
Year of Transition	0.0612***	-0.00941	136.0***	3.946***	1.826***
	(0.0134)	(0.0101)	(22.61)	(0.679)	(0.557)
1-5 Years After	0.0395***	-0.0164**	94.60***	2.178***	0.595
	(0.0135)	(0.00734)	(21.75)	(0.667)	(0.582)
R-squared	0.665	0.387	0.612	0.564	0.601
Observations	123,043	109,582	114,707	125,401	115,718

Notes: This table presents estimates derived from the model specified in Eq. 4, 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 size is a number of employees. 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 XI
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	-0.988	0.916	0.047
	(0.085)	(1.770)	(3.487)	(3.602)
Top 1% x Peer Top 1% Share	0.301**	7.491***	8.403*	15.467***
	(0.120)	(1.832)	(4.110)	(5.517)
R-squared	0.324	0.335	0.110	0.025
Observations	169,170	171,801	149,245	171,743

Notes: This table presents estimates derived from the model as specified in Eq. 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 top 1% share. All observations available within 3 years after matriculation for the student sample are utilized. 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 include matriculation cohort and year fixed effects, second-degree polynomials of age and years since matriculation, gender and Danish citizenship dummies. Standard errors are clustered at the matriculation cohort level. P-values are calculated via wild cluster bootstrap (9999 replications). *** p<0.01, ** p<0.05, * p<0.1.

TABLE XII
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	0.379*	0.030	0.040	-0.005
	(0.216)	(0.071)	(0.073)	(0.084)
Top 1% x Peer Top 1% Share	-0.015	0.107	0.121	0.074
	(0.245)	(0.125)	(0.098)	(0.148)
R-squared	0.257	0.046	0.054	0.061
Observations	7,617	11,309	11,309	11,309

Notes: This table presents estimates derived from the model as specified in Eq. 5 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 include matriculation cohort fixed effects, second-degree polynomials of age, gender and Danish citizenship dummies. Standard errors are clustered at the matriculation cohort level. P-values are calculated via wild cluster bootstrap (9999 replications). *** p<0.01, ** p<0.05, * p<0.1.

Figures

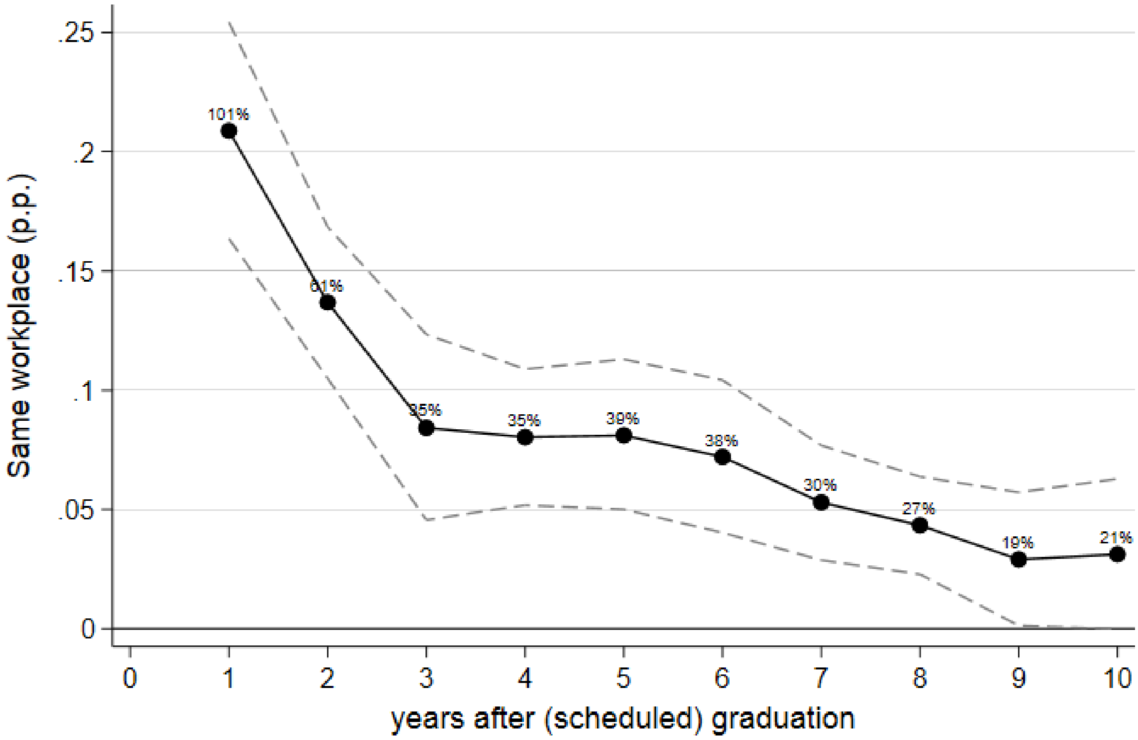

FIGURE I
Same Workplace: Timing of the Effect

Notes: The coefficients are from the linear probability model specified in Eq. 2, 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).

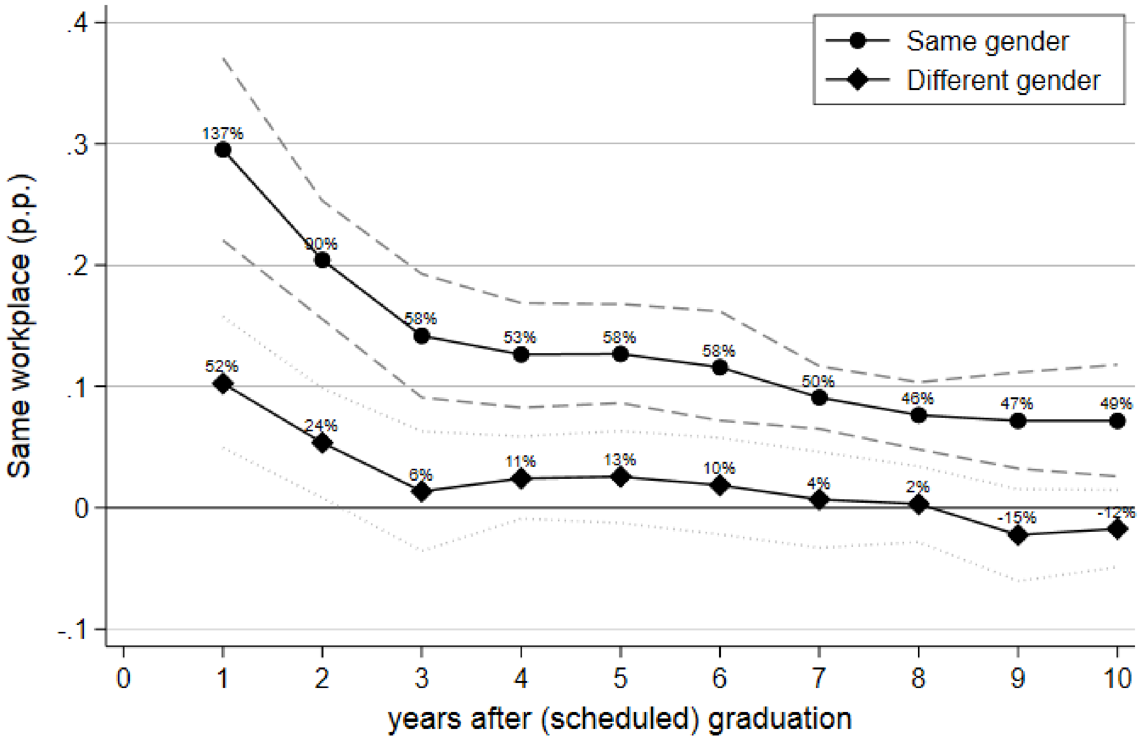

FIGURE II

Same Workplace: Timing of the Effect, by Gender

Note: The coefficients are from the linear probability model specified in Eq. 2, featuring treatment interacted with years subsequent to potential graduation by the same gender indicator. 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).

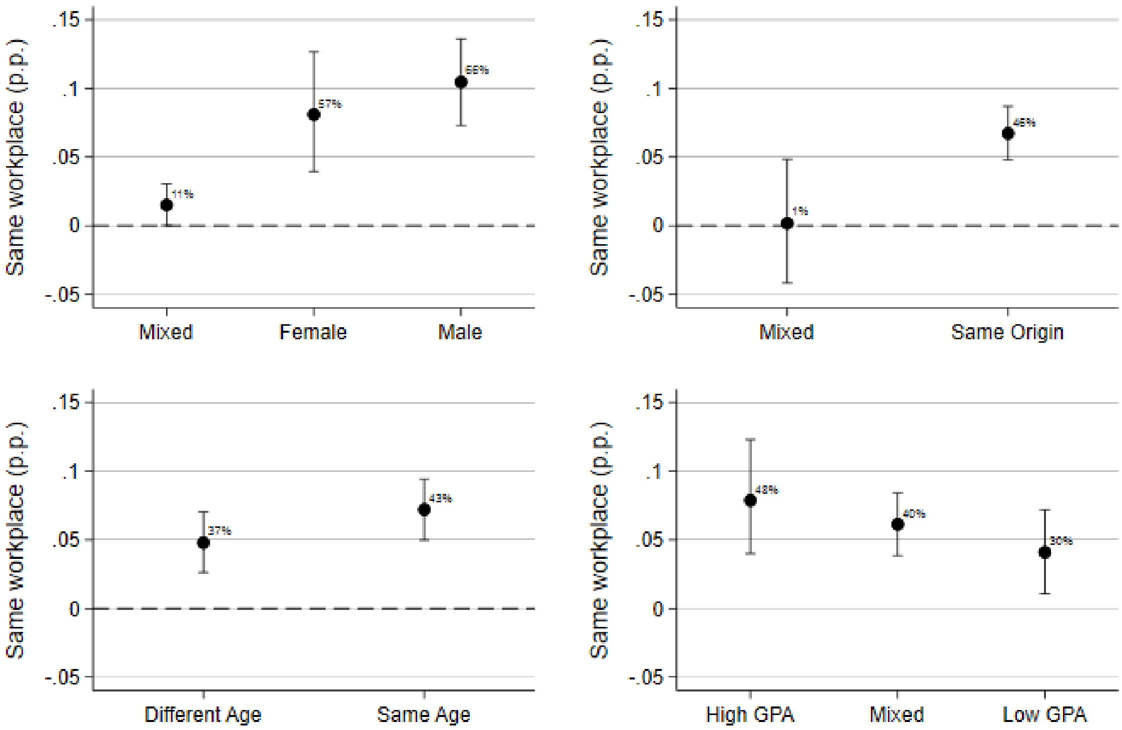

FIGURE III

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. 2, 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 CBS sample average GPA, 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).

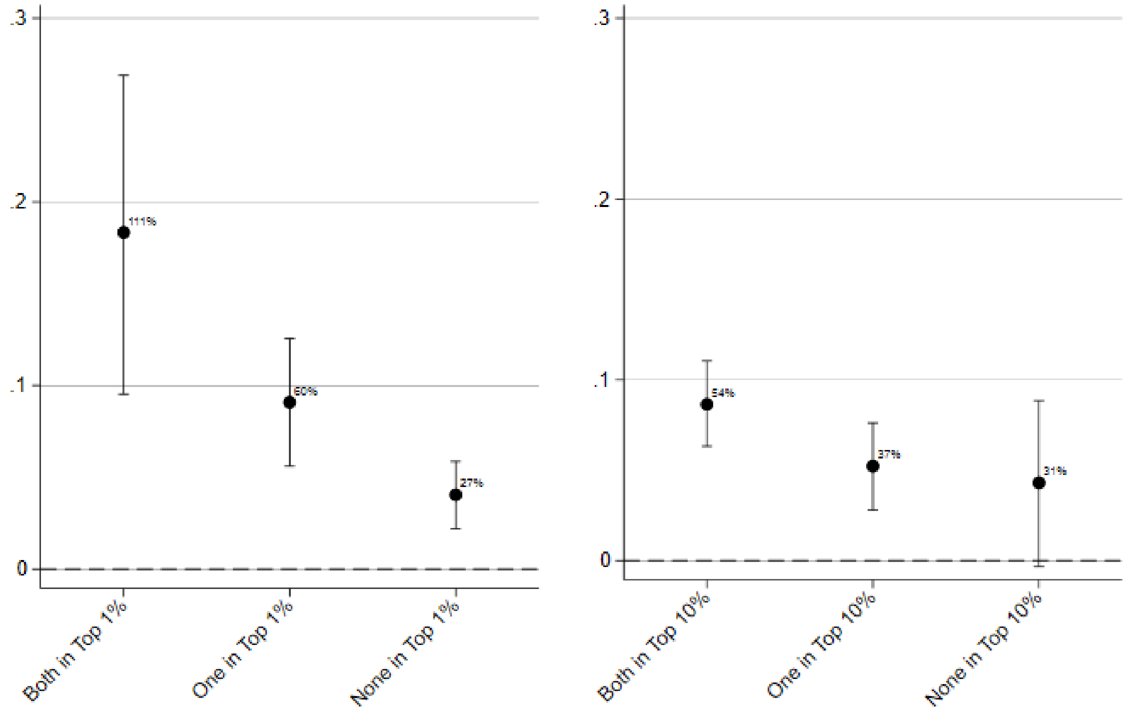

FIGURE IV
Same Workplace: by Father's Income Rank

Note: The coefficients are derived from the linear probability model specified in Eq. 2, incorporating treatment interaction with the father's income group indicators. In the first figure, the categories include both students having fathers in the top 1%, one student having a father in the top 1%, and neither student having a father in the top 1%. In the second figure - both students having fathers in the top 10%, one student having a father in the top 10%, and neither student having a father in the top 10%. Fathers' income ranks are defined relative to the national disposable income distribution one year prior to students' matriculation. 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).

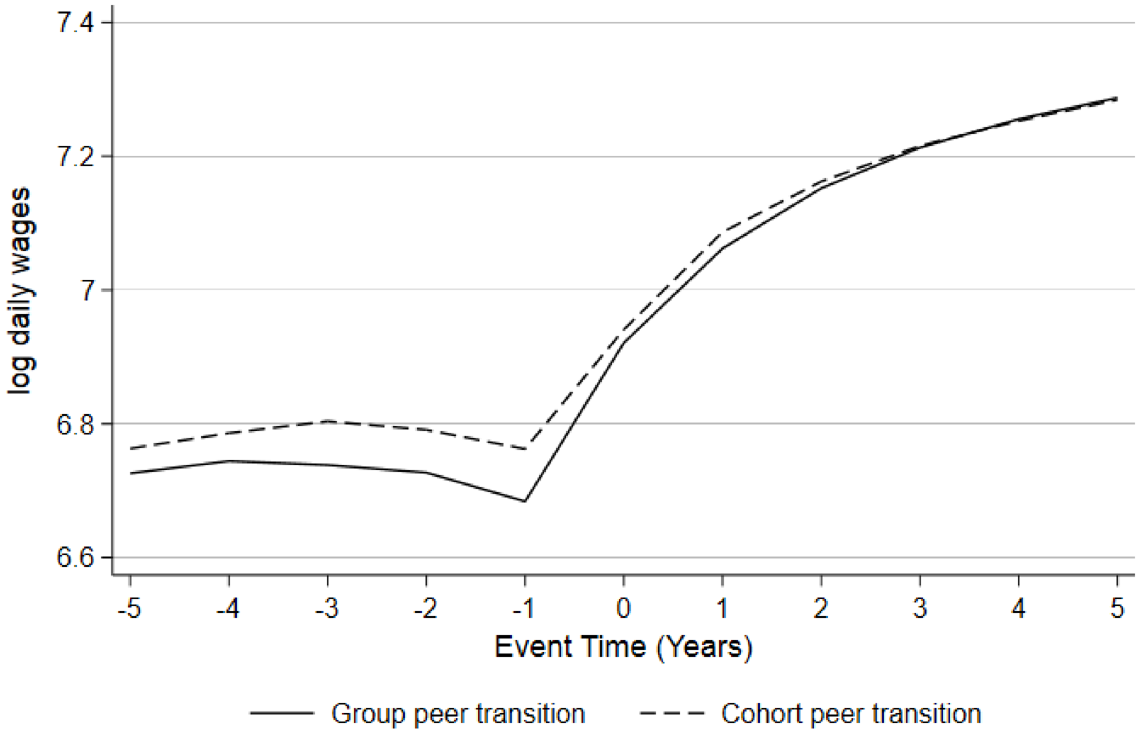

FIGURE V
Wage Dynamics, by Transition Type

Notes: The y-axis represents the average log daily wages, while the x-axis illustrates the number of years relative to the job transition event. An event time of 0 corresponds to the year of transition. The "cohort peer transition" line portrays dynamics for students who join a firm where a member of their cohort is employed. The "group peer transition" line illustrates dynamics for students who join a firm where a member of their peer group is employed.

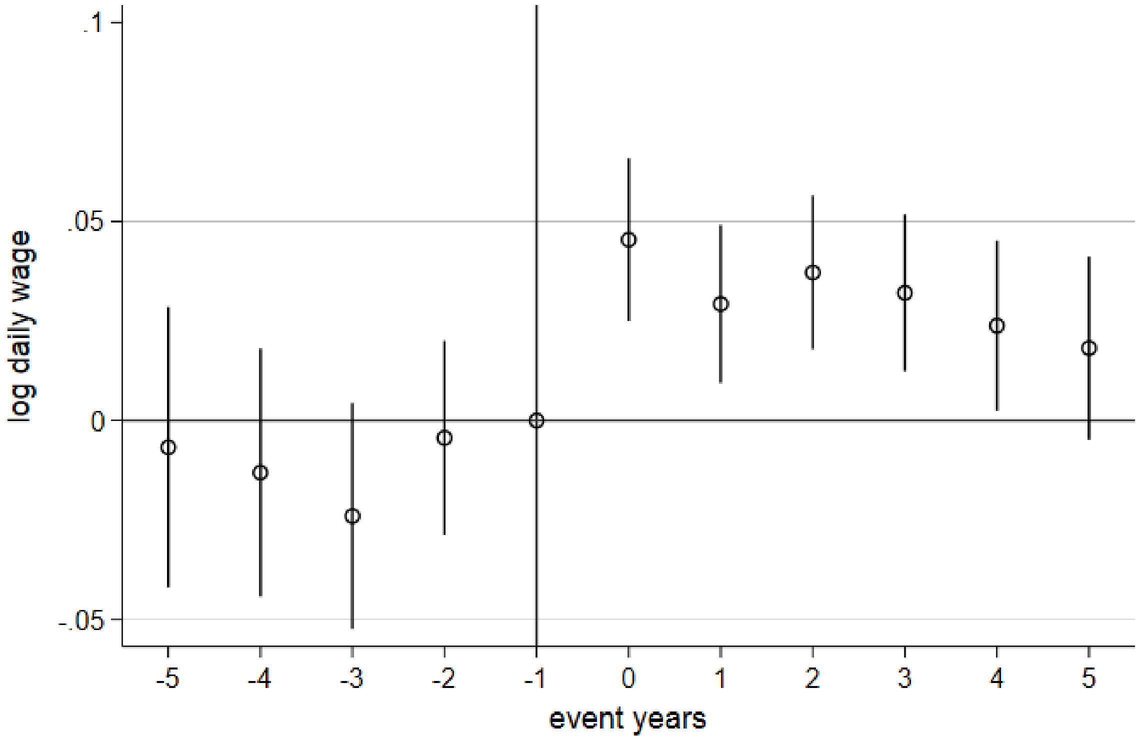

FIGURE VI
The Daily Wage Effect of Peer-to-Peer Transitions

Notes: Coefficients from the event-study in Eq. 4 - 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.

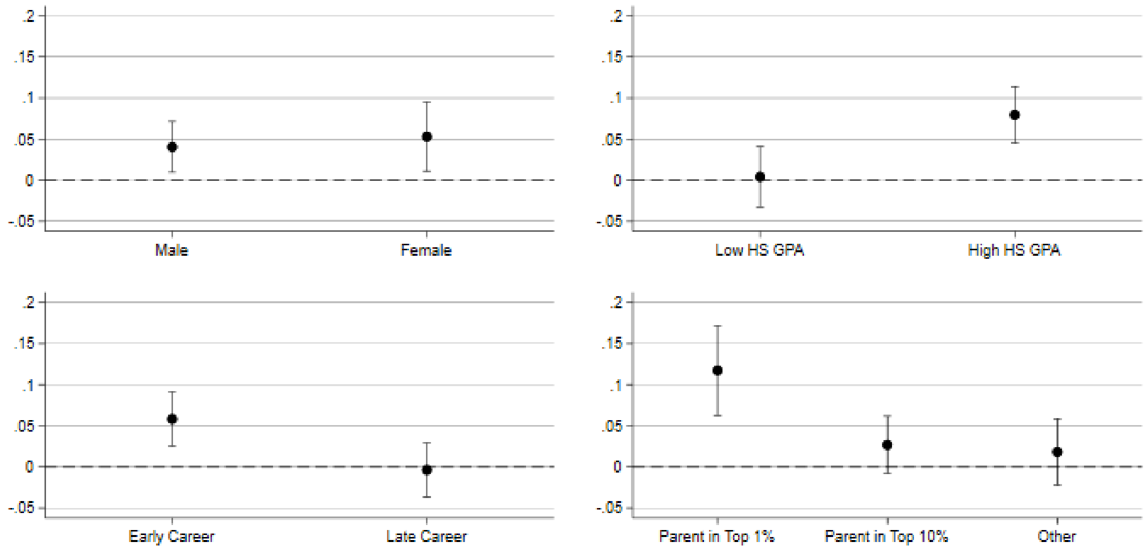

FIGURE VII
The Wage Effect of Peer-to-Peer Transitions, by Group

Notes: The figure displays coefficients extracted from the model described in Eq. 4, separately for various subgroups. The measured outcome is the logarithm of daily real wages. The focus is on the period before and after the job transition, aiming to identify short-term effects. The dataset is divided based on gender, high-school GPA (below and above the CBS average), age (split at the median age of 33), and father's income group (categorized as top 1%, top 10%, or neither, as determined by the national disposable income distribution the year before matriculation).The standard errors are clustered at the individual level. The vertical lines indicate 5% confidence intervals.

Appendix Tables

TABLE A.1
Descriptive Statistics: Background Variables, All Programs

	CBS HA	Other CBS	Other HA	Other Programs
Female	0.34	0.58	0.37	0.54
	(0.48)	(0.49)	(0.48)	(0.50)
Foreign-born	0.09	0.12	0.07	0.08
	(0.29)	(0.33)	(0.25)	(0.27)
Age	21.44	22.19	21.83	23.56
	(2.34)	(3.46)	(3.31)	(5.89)
Mother's education	12.89	13.07	12.09	13.20
	(2.71)	(2.69)	(2.88)	(2.84)
Father's income rank	85.11	84.10	83.08	82.74
	(22.32)	(19.61)	(18.71)	(19.44)
Father in top 1%	0.20	0.14	0.11	0.10
	(0.40)	(0.34)	(0.31)	(0.30)
High School GPA	0.09	0.23	-0.21	0.46
	(0.82)	(0.89)	(0.85)	(0.94)
Dropout	0.33	0.39	0.40	0.38
	(0.47)	(0.49)	(0.49)	(0.49)
Cohort Size	599.56	118.04	222.79	167.70
	(47.22)	(58.74)	(142.72)	(173.57)
Observations	12,517	21,613	21,858	195,486

Notes: The table presents descriptive statistics for individual background characteristics of students in the CBS sample and various groups of Bachelor students. Rows indicate the variables, while columns represent the student groups. The values in each cell indicate variable means and standard deviations (in parentheses). Column labels correspond to the following categories: CBS HA - CBS Business Economics student sample; Other CBS - CBS Bachelor students not in the Business Economics program; Other HA - Business Economics Bachelor students outside of CBS; Other Programs - Bachelor students outside of CBS and not in Business Economics program. High School GPA is standardized based on the GPA distribution for all high school graduates from the academic high school track in the respective graduation year. All groups are confined to matriculation cohorts from 1986 to 2006. Descriptive statistics for students outside the CBS sample are reweighted to match the distribution of students across matriculation cohorts as observed in the CBS sample.

TABLE A.2
Descriptive Statistics: Career Panel, All Programs

	CBS HA	Other CBS	Other HA	Other Programs
Income rank	81.01	73.62	78.25	70.62
	(20.25)	(21.61)	(19.63)	(21.92)
Top 10%	0.47	0.29	0.36	0.22
	(0.50)	(0.45)	(0.48)	(0.42)
Top 1%	0.07	0.02	0.03	0.01
	(0.25)	(0.15)	(0.18)	(0.10)
Log earnings	12.69	12.37	12.57	12.20
	(0.95)	(1.05)	(0.94)	(1.11)
Log daily wage	7.01	6.75	6.89	6.61
	(0.78)	(0.80)	(0.73)	(0.84)
Manager	0.10	0.05	0.08	0.03
	(0.30)	(0.22)	(0.28)	(0.17)
Firm size (FTE)	392.14	405.36	343.91	429.73
	(764.80)	(806.63)	(749.08)	(992.81)
Observations	178,619	242,571	316,243	2,131,024

Notes: The table presents descriptive statistics for career outcomes of students in the CBS career sample and various groups of Bachelor students. Rows indicate the variables, while columns represent the student groups. The values in each cell indicate variable means and standard deviations (in parentheses). Column labels correspond to the following categories: CBS HA - CBS Business Economics student sample; Other CBS - CBS Bachelor students not in the Business Economics program; Other HA - Business Economics Bachelor students outside of CBS; Other Programs - Bachelor students outside of CBS and not in Business Economics program. All groups are confined to matriculation cohorts from 1986 to 2006. Descriptive statistics for students outside the CBS sample are reweighted to match the distribution of students across matriculation cohorts as observed in the CBS sample. Samples include all students in wage employment from the first year after potential graduation, where potential graduation is defined from the program duration. The variables Income rank, Top 10%, and Top 1% are determined based on the Danish disposable income distribution for the corresponding years. Earnings and daily wage variables are measured in terms of the log of 2015 DKK. The classification for managers is based on the occupation code.

TABLE A.3
Career Similarities: New matches

	Baseline	Only New Matches	Joining an Incumbent	Simultaneous Move
Same group	0.0799***	0.0696***	0.0267***	0.0429***
	(0.00917)	(0.00877)	(0.00451)	(0.00692)
P-value	0	0	0	0
Effect (in %)	41.72	44.69	34.67	54.48
Baseline	0.192	0.156	0.0770	0.0788
R-squared	0.000444	0.000416	0.000223	0.000411
Observations	39,829,082	13,537,564	13,537,564	13,537,564

Notes: This table presents estimates from the linear probability model specified in Eq. 2 for directed dyads. Columns in the table correspond to regression results for distinct subsamples and outcome variables. "Baseline": all observations and an indicator for working together in the same workplace. "Only New Matches": only years when student i joins a new workplace and incorporates an indicator for working together in the same workplace. "Joining an Incumbent": only years when student i joins a new workplace and includes an indicator for joining a workplace where student j is an incumbent. "Simultaneous Move": only years when student i joins a new workplace and features an indicator for both students i and j joining a new workplace simultaneously. Standard errors, enclosed in parentheses, are clustered at the cohort level. Baseline values and coefficients have been magnified by a factor of 100 to represent percentage points. The reported p-values indicate the significance of coefficients, determined using wild cluster bootstrap at the matriculation cohort level with 9999 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

TABLE A.4
Career Similarities: Logit Specification

	Same Industry	Same Occupation	Same Firm	Same Workplace
Same group	3.564***	3.194***	17.89***	31.96***
	(1.283)	(0.962)	(2.861)	(3.896)
P-value	0	0	0	0
AME	0.0662	0.0907	0.0598	0.0486
Baseline	1.889	2.926	0.331	0.148
Pseudo R-sq	0.0101	0.00917	0.00842	0.0204
Observations	42,864,255	23,133,124	42,864,255	37,422,346

Notes: This table shows estimates from the logit specification of a baseline regression (Table V). Occupations and industries are categorized at the 4-digit level. Observations for occupations are available solely within the 1994-2016 period, and only non-imputed values are utilized. The outcome variables are defined as indicator variables for instances where individuals work within the same industry, occupation, or firm, but not within the same workplace. Average marginal effects and baselines are multiplied by 100 to reflect percentage points. P-values calculated using score cluster bootstrap (9999 replications) on matriculation cohort level are in parenthesis (Kline and Santos, 2012). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE A.5
The Effect of Peer-to-Peer Transitions, Robustness

Panel A: Destination Job Controls				
	Occupation	Industry	Firm	Workplace
Year of Transition	0.0589***	0.0389***	0.0490***	0.0544***
	(0.0140)	(0.0140)	(0.0155)	(0.0173)
1-5 Years After	0.0443***	0.0396***	0.0437***	0.0415**
	(0.0143)	(0.0142)	(0.0161)	(0.0181)
R-squared	0.719	0.722	0.755	0.788
Observations	108,244	115,140	103,200	75,153

Panel B: Origin Job Controls				
	Occupation	Industry	Firm	Workplace
Year of Transition	0.0494***	0.0550***	0.0396**	0.0396**
	(0.0139)	(0.0147)	(0.0187)	(0.0187)
1-5 Years After	0.0375***	0.0336**	0.0163	0.0163
	(0.0138)	(0.0142)	(0.0191)	(0.0191)
R-squared	0.724	0.736	0.789	0.789
Observations	102,830	109,907	56,015	56,015

Notes: This table presents estimates derived from the model specified in Eq. 4, 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. Panel A adds the destination job interaction to the calendar by event year fixed effect and Panel B 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.6

The Linear-in-Means Balancing Test

	HS GPA	Academic HS Track	Business HS Track	Gap Years
Other x Peer Top 1% Share	0.128	-0.018	0.010	-0.126
	(0.138)	(0.054)	(0.051)	(0.202)
Top 1% x Peer Top 1% Share	0.194	0.062	0.022	0.134
	(0.202)	(0.145)	(0.120)	(0.286)
Observations	11,575	11,822	11,822	11,268
R-squared	0.174	0.090	0.051	0.680

	Work Experience	Copenhagen	Father's Education	Mother's Education
Other x Peer Top 1% Share	0.000	0.030	0.788	-0.025
	(0.061)	(0.056)	(0.493)	(0.391)
Top 1% x Peer Top 1% Share	0.063	-0.021	0.566	0.149
	(0.124)	(0.081)	(0.721)	(0.483)
Observations	11,822	11,822	10,656	11,175
R-squared	0.061	0.064	0.059	0.053

Notes: This table presents the results of a balancing test derived from the model as specified in Eq. 5 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. 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. Work experience: an indicator for having any prior work experience in Denmark before matriculation. Copenhagen: an indicator of being a resident of Copenhagen the year 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 include matriculation cohort fixed effects, second-degree polynomials of age, gender and Danish citizenship dummies. Standard errors are clustered at the matriculation cohort level. P-values are calculated via wild cluster bootstrap (9999 replications). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE A.7
The Effect of Top 1% Peers, Sample Selection

	Sample (All)	Sample (5 years)	Population	Employment
Other x Peer Top 1% Share	-0.013	0.024	-0.024	0.007
	(0.033)	(0.035)	(0.026)	(0.020)
Top 1% x Peer Top 1% Share	-0.073	0.020	-0.049	-0.033
	(0.082)	(0.058)	(0.045)	(0.061)
R-squared	0.015	0.014	0.010	0.010
Observations	202,642	59,110	202,642	194,782

Notes: The table presents estimates derived from the sample selection test, based on the model outlined in Eq. 5. 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. The outcome variables are indicator variables. Sample (All): a student identified in the career sample during a specific year using all available years. Sample (5 years): the same but only within the first 5 years following scheduled graduation. Population: equals 1 if a student is identified within the Danish resident population for a given year. Employment: equals 1 if a student is identified within the employment sample using only observations for Danish residents. All regressions include matriculation cohort and year fixed effects, second-degree polynomials of age and years since matriculation, gender and Danish citizenship dummies. Standard errors are clustered at the matriculation cohort level. P-values are calculated via wild cluster bootstrap (9999 replications). *** p<0.01, ** p<0.05, * p<0.1..

TABLE A.8
The Effect of Top 1% Peers on Daily Wages, Alternative Treatment Definitions

	(1)	(2)	(3)	(4)
Other x Peer Top 1% Share (fathers)	-0.041			
	(0.085)			
Top 1% x Peer Top 1% Share (fathers)	0.301**			
	(0.120)			
Other x Peer Top 1% Share (mothers)		0.129		
		(0.198)		
Top 1% x Peer Top 1% Share (mothers)		-0.451		
		(1.930)		
Other x Peer Top 10% Share (fathers)			-0.016	
			(0.101)	
Top 10% x Peer Top 10% Share (fathers)			-0.100	
			(0.081)	
Other x Peer Top 10% Share (mothers)				0.024
				(0.073)
Top 10% x Peer Top 10% Share (mothers)				-0.029
				(0.087)
R-squared	0.324	0.321	0.322	0.321
Observations	169,170	169,170	169,170	169,170

Notes: This table presents estimates derived from the model as specified in Eq. 5 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. All observations available within 3 years after matriculation for the student sample are utilized. Log real daily wages are used as an outcome variable in all columns. Column (1) presents a baseline estimate from Table XI. 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, the national distribution of disposable income from the year prior to matriculation is applied. All regressions include matriculation cohort and year fixed effects, second-degree polynomials of age and years since matriculation, gender and Danish citizenship dummies. Standard errors are clustered at the matriculation cohort level. P-values are calculated via wild cluster bootstrap (9999 replications). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figures

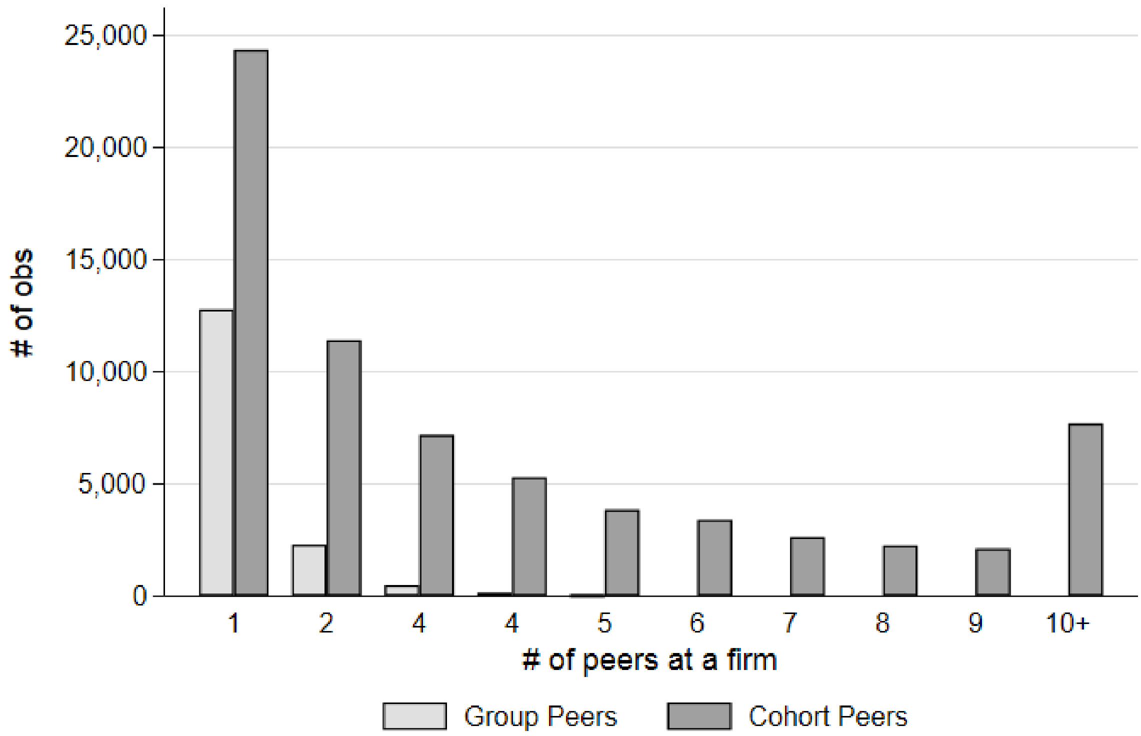

FIGURE A.1
Number of Group and Cohort Peers in a Firm

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.

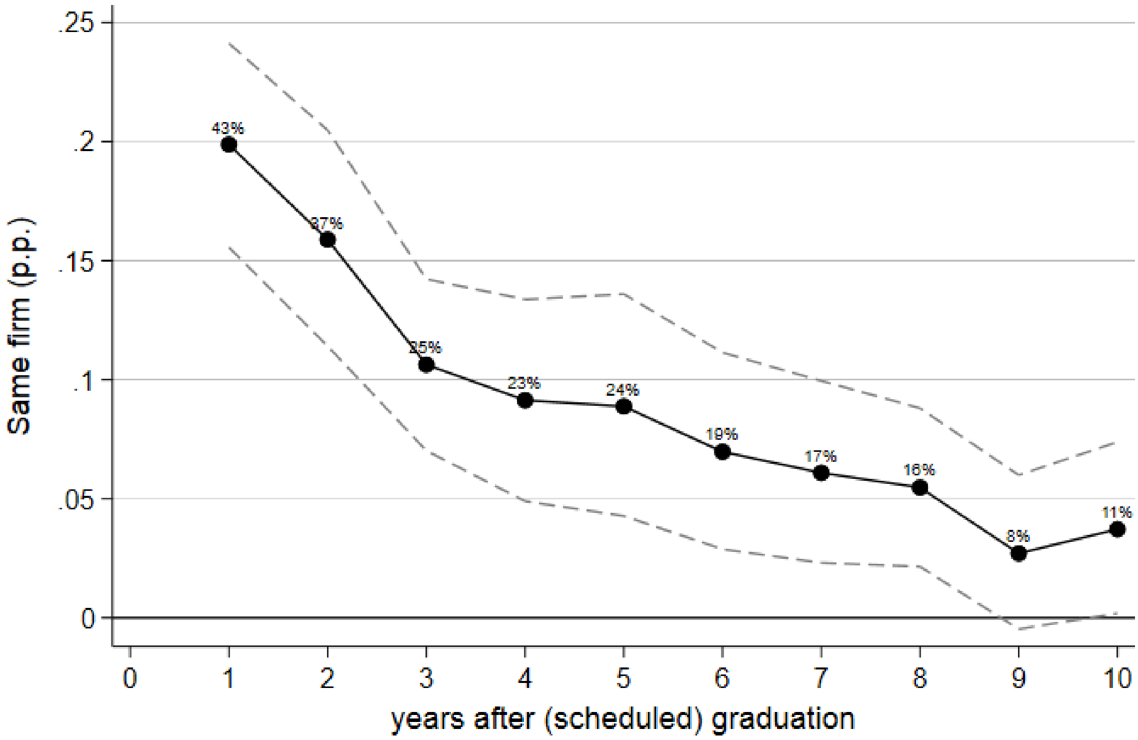

FIGURE A.2
Same Firm: Timing of the Effect

Notes: The coefficients are from the linear probability model specified in Eq. 2, 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).

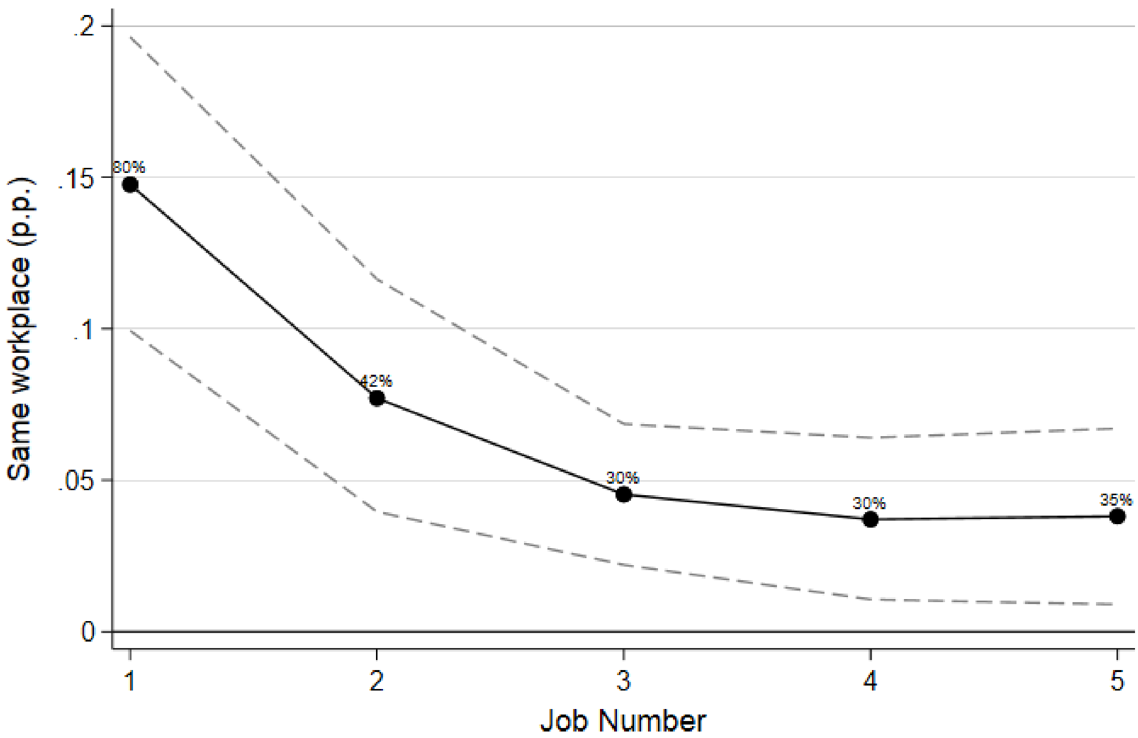

FIGURE A.3
Same Workplace: Timing of the Effect, by Job Order

Notes: The coefficients are from the linear probability model specified in Eq. 2 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).

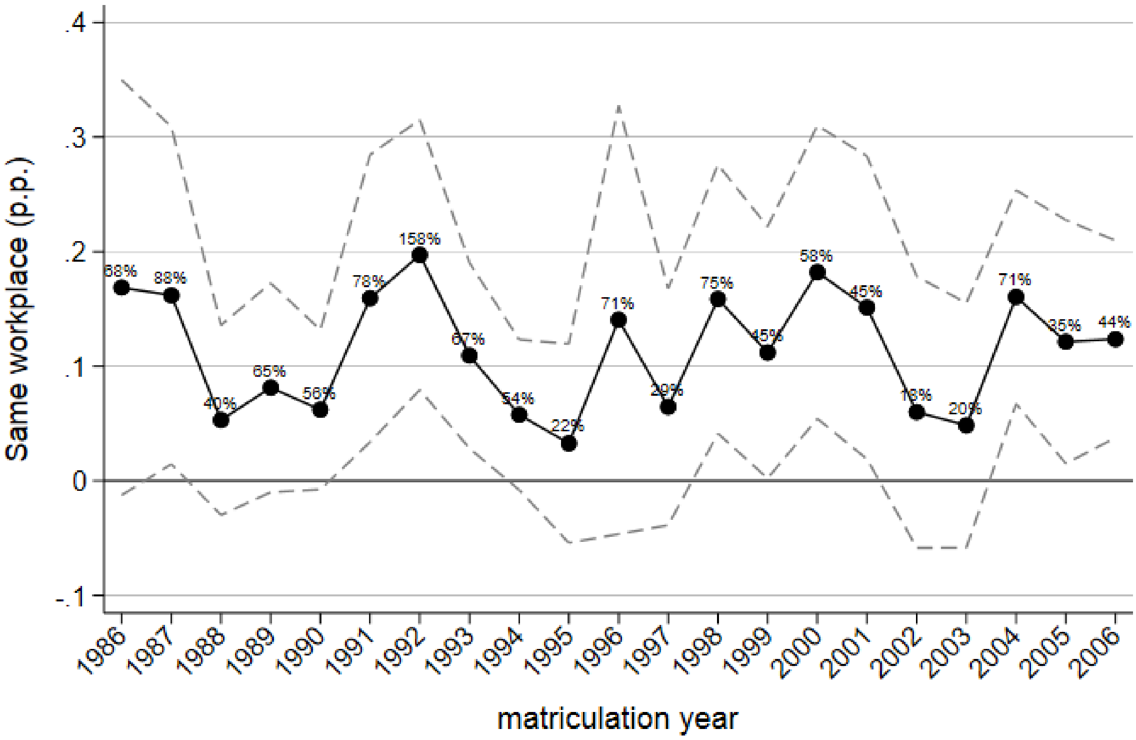

FIGURE A.4
Same Workplace: Timing of the Effect, by Matriculation Cohort

Notes: The coefficients are from the linear probability model specified in Eq. 2, 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 wild cluster bootstrap at the matriculation cohort level (9999 replications).