Peers and Careers: Labour Market Effects of Alumni Networks

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Abstract

Are social connections formed among university peers important in shaping their future careers? We answer this question by using records from the long-lasting random assignment of Business Economics students to tutorial groups at Copenhagen Business School from 1986 to 2006, which we merge to detailed labour market information from Danish registers. We find that students, randomly assigned to the same tutorial group, tend to have more similar careers than students from the same cohort, but a different tutorial group: they tend to work in the same occupations and industries and are more likely to be hired by the same employer. The strongest "excess" similarities of tutorial group peers over cohort peers are observed at the most disaggregated level, the workplace. This effect is strong, persistent (although decreasing over time), characterized by homophily and pronounced the most for students from the wealthiest family backgrounds. By comparing the transitions of students to workplaces with incumbent group peers to workplaces with incumbent cohort peers, we find that students benefit from their alumni network by gaining access to more stable and higher-paying jobs.

Keywords: social networks, peer effects, education, labour market outcomes, alumni.
JEL classification: D71, I23, J62.

1 Introduction

It is a widely held belief that "knowing the right people" can have a transformative effect on one's career. Joining 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] Nevertheless, despite the broad academic interest in educational peer effects and general recognition of their importance (see Sacerdote (2014) for a review), still very little is known about the role of peers in shaping career outcomes. Moreover, the interpretation of potential peer effects on careers is intrinsically ambiguous. Interactions of educational peers could be limited to their shared period of studies, but still manifest in persistent labour market effects. On the other hand, peer interactions might extend into their professional lives, where peers from university form an alumni network, which acts as a source of information on jobs and career opportunities.

In this paper, we ask if and how social interactions in higher education affect student's career choices and if the social ties from university persist into labour market interactions of former academic peers. Investigating the effects of social interactions is notorious for its many challenges (see, for example, Manski (1993) and Angrist (2014)). The ideal research design would need to fulfil several restrictive requirements. Most importantly, in order to avoid ex-post similarities between a pair of students reflecting only sorting on ex-ante unobserved similarities, the social ties between students needed to be randomly assigned and independent of other factors that affect future careers. A second hurdle for investigating the effects of social interactions in higher education on student's careers is that data on labour market outcomes of students needs to be available. 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 observability of detailed career dynamics.

The setting in our paper is characterized by both: arguably credible randomization and rich data on students' careers. First, we exploit a policy that randomly assigned students to tutorial 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, and its graduates are typically among the country's top earners. Second, we can merge these records to Danish register data, which offer rich and detailed information on workers and firms in Denmark. Therefore, we can observe the individual career dynamics of students.

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

To identify the effect of peers on choices after graduation, we use a dyadic approach and compare pairs of students that are randomly assigned to the same tutorial group (group peers) to pairs of students from the same cohort but who are assigned to a different tutorial group (cohort peers). We find that group peers tend to have more similar careers than cohort peers. Group peers "excessively" tend to work in the same occupations and industries and are more likely to be hired by the same firm. The effects are strongest at the most disaggregated level - the workplace. During their first ten years after graduation, a pair of group peers is more than 40% more likely to be working in the same workplace than a pair of cohort peers. These workplace effects are much stronger than effects on occupation, industry and firm choice. Moreover, they seem to be the driving force underlying group peers' career similarities. Conditional on not working at the same workplace, group peers are not significantly more likely to hold jobs in the same firms or industries. Moreover, we find no evidence of peer similarities in educational choices that might mediate the effect, which is consistent with post-graduation network interactions between students. While our identification strategy addresses the selection problem, our empirical design does not rule out that results are driven by common group-level shocks. Nevertheless, we conclude that overall pattern of our findings suggest that the "excess" career similarities between group peers (in comparison to cohort peers) is most likely indicative of social interactions with academic peers being impactful for students careers rather than just reflecting common shocks from the shared academic environment.

The social ties formed in university persist over time, but slowly fade out. Right after graduation, group peers are twice as likely to work together compared to cohort peers; ten years after scheduled graduation, the effect is attenuated to around 20%. This pattern is characterized by notable gender and country-of-origin homophily, with effects being more pronounced for peers with the same gender or country-of-origin and insignificant for "dissimilar" peers. Moreover, the most substantial effects are observed for students with a higher initial ability (measured by high school GPA) and for students coming from the richest families (as measured by a paternal income in top 1% of the national income distribution in the year before matriculation).

This evidence suggests that students form active alumni networks after graduation and use them to gather information on job opportunities. Nevertheless, is joining a group peer's workplace also beneficial? To answer this question, we compare events where a student joins a workplace with one or more incumbent *tutorial group peers* to events where a student joins a workplace with one or more incumbent *cohort peers*. We find that such transitions are associated with getting higher wages and more stable jobs in better-paying firms, industries and occupations. In alignment with the results from the first part of the analysis, these returns are more pronounced at earlier career stages and for high-ability students from wealthy families.

This paper aims to contribute to the large and growing literature on peer effects in education. A prominent branch of research exploits experimental settings with various types of random assignments in educational context (within dormitories, courses or cohorts), but mostly focuses on academic outcomes (see, for example, work by Sacerdote (2001); Zimmerman (2003); Lyle (2007); Carrell et al. (2009) and Feld and Zölitz (2017)). A handful of studies attempts to establish a link between academic peers and long-run outcomes without explicit randomization by using across cohort variation (as in Hoxby (2000)). For example, Black et al. (2013) investigate how variation in socioeconomic composition of cohorts in ninth grade in Norway affects students' various long-run outcomes (including labour market outcomes). Anelli and Peri (2017) and Brenøe and Zölitz (2020) study the effects of class gender composition in Italian and Danish high schools, both with a focus on college major choices and income.

This project adds to a much smaller set of studies that combines both - explicit randomization of students into peer groups and a focus on labour market outcomes after graduation. For example, using random assignment to sections in Harvard Business School, Lerner and Malmendier (2013) investigate how peers affect the decision to become an entrepreneur, and Shue (2013) explores the role of peer interaction between top managers in corporate decision making. Jones and Kofoed (2020) study how peers affect occupational preferences in the context of a military academy. The most related to the setting in our study is a paper by Feld and Zölitz (2018), which uses within-course randomization into classes in a Dutch business school to investigate the effects of peer GPA on course choices and evaluates labour market outcomes based on survey-based measures of career success and work satisfaction.

Our study's contribution to the literature on long-run peer effects is twofold. First, in contrast to the common interpretation of academic peer influences as being confined to the shared period of studies, we emphasize the role of contemporaneous alumni interactions in shaping students' careers by employing a labour market network perspective. Second, access to detailed administrative labour market data allows us to analyze in detail how academic peers affect each others' career choices at the industry, occupational, firm and workplace level.

This paper further contributes to a wide range of studies exploring the importance of social connections in the labour market. Who you know appears to be important in different social contexts: neighborhoods (Bayer et al. (2008); Hellerstein et al. (2011); Schmutte (2015)), former coworkers (Cingano and Rosolia (2012); Glitz (2017); Caldwell and Harmon (2018); Glitz and Vejlin (2019), Saygin et al. (2019)), family members (Kramarz and Skans, 2014) and ethnic groups (Edin et al. (2003), Damm (2009), Dustmann et al. (2016)). Several studies attempt to assess the importance of labour market networks among former students. Hacamo and Kleiner (2017) focus on the managerial market and investigate how firms use social connections that their employees gain through MBA programs to attract talent. Zhu (2018) identifies referral networks among graduates from community colleges in Arkansas.

Lastly, this study contributes by investigating a specific context in which peers are considered to be of particular importance for educational returns: business schools (e.g., Lerner and Malmendier (2013) and Shue (2013)). Besides identifying significant social interactions among former students and providing evidence of sizable benefits from such interactions, we point at significant disparities between students in the size of these effects. In accordance with (Zimmerman, 2019), we provide evidence that business education programs particularly benefit the accumulation of social capital among students from advantaged backgrounds. As a consequence, mobility into top incomes for initially disadvantaged students might be hindered by disproportionate network formation.

The rest of the paper is organized as follows. The next section discusses the institutional background of the Business Economics Program at CBS, data sources, and provides descriptive statistics. In Section 3, our strategy to identify peer effects as "excess similarities" is outlined, and the main results are presented. Section 4 investigates benefits from alumni interactions. The last section concludes.

2 Data and Institutional Background

2.1 Business Economics at CBS 1986-2006

Copenhagen Business School is a large public Business School in Denmark's capital, Copenhagen. In this paper, we focus on CBS's largest study program, which is the three-year-long Business Economics program. The completion of this program leads to a Bachelor of Science (B.Sc.) in Business Economics. In our period of interest 1986 - 2006, CBS enrolled around 600-700 students in the program each year. As other university programs in Denmark, the Business Economics program at CBS is free of charge.

The institutional features of the Business Economics program at CBS make it well suited for studying peer effects. First of all, incoming students are (conditionally) randomly assigned to tutorial groups. The tutorial groups are assigned before the start of the first semester, consist of around 35 students, who stay together for the full duration of the program. Students are allocated to tutorial groups by the CBS administration based on three criteria: gender, age and Danish citizenship. The CBS administration aimed at balanced sections based on gender and foreign citizenship, while clustering older students in specific groups. Throughout the study, we will refer to students who are initially assigned to the same tutorial group as ”group peers”, while we will refer to students from the same matriculation cohort as ”cohort peers”.

The Business Economics program primarily consists of mandatory courses, which combine lectures and tutorial classes and cover traditional topics including principles of micro- and macroeconomics, accounting, marketing, finance and quantitative methods. Lectures are usually taught by a senior faculty member, who introduces the course material and take place in large auditoriums to fit a full cohort of students. In the first years of the program, every lecture is accompanied by a weekly exercise session within the tutorial group. The assignments to be solved and the curriculum are the same across tutorial groups. Intensive interaction within the tutorial groups is a crucial feature of learning in Danish universities. Tutorial group peers usually jointly prepare problem sets for tutorial sections, which are then presented and discussed by the students. Within tutorial groups, students are encouraged to form smaller reading groups. The study regulation for Business Economics at CBS from 1986 states that ”the tutorial group is your fixed point of reference throughout your studies. In most teams, reading groups are formed during the fall of the first year. Reading groups [...] in general consist of 3-5 other students with whom you solve assignments, discuss the syllabus, exchange notes, etc. [...].” Teaching in tutorial groups is meant to be highly standardized across different groups. Students in all tutorial groups face the same exam that is graded on the cohort level. There are no specializations *per se*, but students are offered electives during the last year of their studies.

Group peers spend more time in the classroom with each other than with other ”regular” cohort peers. The first semester is 12 weeks long and consists of 4 mandatory courses. Each of them on average consists of 2 lecture hours and 2 hours of tutorials per week.[2] A typical course has 11 weeks of lectures and 10 weeks of tutorials. Therefore, during the first year, a student spends about 16 hours per week in the same room with peers from the tutorial group and 8 hours with both group and cohort peers. Note that, since there is no attendance checking, students are not forced to attend all classes. However, the general perception is that the attendance of tutorials is very high. The difference in the ”intensity” of interaction between group peers and cohort peers is a key ingredient of our identification strategy.

We use the information on initial tutorial group composition, which is meant to be stable for all mandatory courses, there are exceptions under which the composition of peer groups might deviate from the initial assignment. First, the composition of tutorial

[2] The following numbers are taken from Skibsted (2016) and based on a cohort which has matriculated in 2015, which is outside our sample. However, according to the study administration, the number of teaching hours has not changed substantially.

groups might change due to students dropping-out from the program. Around one-third of study spells in our sample do not end with students graduating. Furthermore, in case of a significant drop-out rate, tutorial groups are occasionally merged. Under other circumstances, it is difficult for students to change their tutorial group actively. A successful application for a group change would require a just cause (e.g., overlap between tutorials and a scheduled medical treatment) and finding another student willing to "exchange" groups. Moreover, with few exceptions, applications for group changes are possible only after the first year of studies. We discuss the potential implications of these processes for identification below.

2.2 Data Sources and Sample Selection

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

Our information about tutorial group composition stems from official records by the CBS administration for students who were enrolled between 1986 and 2006, and therefore our sample consists of 21 full cohorts of Business Economics students. The data provided by CBS contains information on the students' matriculation and exmatriculation dates, exmatriculation reasons, high school GPA, high school degree, citizenship, gender, age and most notably information on the initial tutorial group assignment by the CBS administration. Throughout our analysis, we keep all student observations regardless of their graduation status.

We augment the internal CBS data with Danish register data. In particular, we receive extensive information on the background and demographic information about Danish residents (as age, gender, marital status, place of birth, current place of residence, education) and most importantly annual labour market information on the universe of firms and workers in Denmark from 1980 to 2016. Additionally, we use the register data to get information on individuals outside the CBS sample to characterize jobs (workplaces, firms, occupations, industries) where students from our sample work after graduation.

The primary register-based data source for this study is the Danish matched employer-employee data (IDA). The data contains labour market outcomes (employment, occupation, industry, wages) and identifiers of firms and workplaces. To identify firms, we use the tax identity of the employer. Throughout the paper, we will use the terms "firm" and "employer" interchangeably. The definition of a workplace is not directly tied to an employer but corresponds to a physical location, namely the location where employees work (e.g. their office or a plant). All occupations are defined on the 4-digit level of the

DISCO classification[3], and industries - on the 6-digit level of the DB classification. We use the annual cross-section of jobs from IDA for the entire period of study (1980-2016). For the years 2008-2016, we have access to the monthly employer-employee register (BFL), which we use to construct variables for labour market outcomes in this time-period (wages, hours and days of employment, industry and occupation codes). To study the educational trajectories of students in our sample, we use administrative data on individual education spells in Denmark (KOTRE).

Since this study focuses on studying students' labour market careers, certain sample restrictions apply. First, we do not observe student careers outside Denmark. For example, international students leaving Denmark after their studies or Danish students' careers abroad are not covered by our data. Second, as our study is focused on labour market networks, we do not consider observations outside of wage employment, which we define through the existence of an employment observation with non-zero wages and a non-missing employer identifier for a given year. As a consequence, we exclude observations of workers in both non-employment and self-employment. Below, we check if the sample restriction presents a threat to the empirical strategy.

2.3 Estimation Sample

The initial dataset obtained through CBS contains 77,164 student by peer-group-by-academic-semester observations of 15,441 individual students and spans the entering cohorts from 1984-2006. We can merge around 98% of students in the CBS data to the list of students enrolled at the CBS Business Economics program according to the official education registers by Statistics Denmark.[4]

Several students are observed to repeat specific semesters. Since the group assignment procedure for students repeating the first semester is different, we keep only their first spell in the estimation data. In the last step, we exclude the cohorts of 1984 and 1985, as the group change procedure at that time was different.

For the main analysis, we construct a career panel that includes labour market information on the first 15 years after scheduled graduation (and hence 4-19 years after matriculation).

[3]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.

[4]In the case of very early program dropout, students are not registered to start an educational degree program in the education register.

2.4 Summary Statistics

Table 1 shows summary statistics for the background characteristics of male and female students. There are 12,518 students in our sample. Almost two-thirds of the students in our sample are male. On average, the students in our sample are older than 21 when they start their first spell at CBS. Few students (3.5%) are foreign citizens. High school GPA is measured in standard deviations from the distribution of a student's high school graduation cohort. On average, students have slightly higher grades than their high school cohort, and the female students in our sample have significantly higher high school grades than male students. An average student has one gap year after high school graduation and before the program start. Three-quarters of students start with having at least some work experience in Denmark. On average, they are from remarkably wealthy backgrounds: an average student's father is ranked in the 87th income percentile of the entire Danish income distribution. Strikingly, around 20% of fathers locate in the top 1% of the income distribution, while 20% of mothers are placed in the top 10%. For an average student, we observe around 35 tutorial group peers and 600 cohort peers.

Table 2 presents the ten most prominent occupations and industries for students in our sample. Students work in a wide array of white-collar occupations (finance, sales, administrative, managerial) and industries (finance, consultancy). Jobs in finance are dominating both in terms of industries and occupations.

How common is it for students from the same program/cohort/group[5] to share the same occupation, industry, employer or workplace? Table 3 presents how many students share a labour market "cell" with a group, cohort and program peer in a given year. Most importantly, it is not a rare event that workers in our sample share common career states with their peers. Non-surprisingly, the "broader" the labour market "cell", the more widespread is "working together". For example, 36% of group peers ever work in the same firm at a given point of time in their careers, while twice as many (73%) work at least once in the same industry as their group peers. Secondly, as there are more cohort peers than group peers (groups are nested), it is much more common to be observed working with your cohort peer. 21% of students shared a workplace with their group peer, while almost 68% of students shared a workplace with a cohort peer.

Table 4 shows summary statistics for career outcomes in the career panel. Despite striking gender differences, both male and female students in our sample spend their careers at the top of the national income distribution. More than half of all observations are in the top 10% and a considerable share in top 1% of the income distribution in Denmark in a given year.[6] More than 10% of spells in the career panel correspond to

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

[6]This observation is particularly remarkable given the fact that the sample includes early career observa-

a student working in a management position and 2.3% to spells in top management positions. Importantly, students tend to work in large firms.

3 Career Similarities & Networks

3.1 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 start with constructing all unique pairs of students (i, j) within each matriculation cohort $c(i, j)$.[7] In this study, we aim to identify the effect of interaction with group peers "in excess" of cohort peers. To illustrate the identification problem and the mechanics of dyadic regressions, let us consider the propensity of a pair of students (i, j) to work at the same job k at time t.[8] Observing a pair of students in the same job is not indicative of peer interactions *per se*. Students from the same cohort are likely to have similar abilities and career goals even before they start their studies, which in consequence should lead to similar careers. Moreover, yearly variation in program contents might lead students from the same cohort to have more similar skill sets *ex post*. In the end, starting careers under similar (macro-) economic conditions could itself drive similarities in career paths between students. Therefore, the propensity of students to be observed at the same job could be expressed as

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

where F_{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 $k \in K$:

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

where α represents a theoretical parameter of interest. Estimation of Eq. 1 faces two 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.

tions and observations of students who dropped out of the program.

[7] We construct pairs as undirected dyads - (i, j) is equivalent to (j, i).

[8] Here, a "job" could be interpreted as an industry, occupation, firm or a workplace.

We use the tutorial group assignment as a proxy for the intensity in social interactions between students. To estimate the effect of peer interactions on career outcomes, we leverage both the fact that students are randomly allocated to their tutorial groups, and that the structure of the Business Economics program creates variation in time spent with group peers vs cohort peers.

Our approach of using tutorial group assignment to identify the effect of social interactions, therefore, relies on two assumptions. First, due to the random assignment, a given pair is equally likely to end up in the same group as any other pair of students within a given cohort. The random assignment solves the "selection problem" (Manski, 1993).[9] We provide evidence in favour of exogeneity of group assignment below. A second assumption is that - conditional on knowing the "true" intensity of social interactions - the tutorial group assignment is redundant for predicting F_{ijt}. In other words, we assume that the group assignment does not contain any useful information about the determinants of students' careers besides the compositions of their peer groups. The existence of group-level common shocks - factors that affect all group peers but are not the result of peer interactions *per se* - would violate this assumption. We assume that the uniformity of curriculum for a given course across the tutorial groups provides that the condition is likely to be satisfied. Besides group peers (the effect of which we are interested in), students in the same tutorial group are exposed to the same teacher assistants (TAs). Even though being assigned to the same tutorial group effectively means having the same TAs for mandatory courses, the supporting role of TAs and highly standardized character of tutorial teaching process suggests that it is unlikely that they would have a strong and persistent effect on students' workplace choice. As we discuss below, we believe that the effect of TAs is unlikely to be consistent with the overall pattern of our findings. Previous research on the role of TAs suggest that they are unconnected with students' future academic outcomes (Feld et al., 2020). Moreover, we believe that using within cohort peer group randomization is an improvement over studies using between cohort comparisons. We expect that study conditions are much more similar for students from the same cohort than for students from two different cohorts. Provided that both assumptions are not violated, any "excess" similarities in careers between group peers over cohort peers can be attributed to the excess interaction within tutorial groups.

The intuition is formalized in the following simple 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 an event of "working together" for a pair of

[9] Note that, since students were assigned randomly *conditionally* on gender, age and Danish citizenship, we always compare pairs of students conditioning on these covariates.

students i and j in the year t; I_{ij} is an indicator of being assigned to the same tutorial group at the time of matriculation; $\lambda_{c(i,j)t}$ is a set of cohort/year/year-after-graduation fixed effects;[10] X_{ij} is a vector of dyadic covariates based on students' gender, age and status as a Danish citizen at the time of matriculation.[11] The causal coefficient of interest is β. In all dyadic regressions, we cluster on the level of randomization, the cohort. To address a (potential) threat to conducting inference due to a small number of clusters, we implement a wild cluster bootstrap.

Three important points need to be made concerning the interpretation of our parameter of interest, β. First, under the discussed assumptions, the proposed method allows us to identify not the total effect of group peers, but the effect "in excess" of the influence of cohort peers. P_{ij}, the actual level of interaction of a pair of students, should not be expected to be zero for cohort peers. Nevertheless, although cohort peers might be influential in shaping labour market outcomes as well, we assume that group peers interact more and are, on average, more influential. A positive and statistically significant estimate of β would provide evidence for more intensive interactions with tutorial group peers than with cohort peers. However, excluding the extreme case where students interact only within tutorial groups, this estimate is only a lower bound for the total effect of group peers. Second, since we do not observe the real network of social interactions between students, we effectively estimate an intention-to-treat effect. Not only is it realistic to assume that students interact between groups, but it is also likely to be true that not all students interact with the same intensity and quality with all of their group peers. Moreover, some students drop-out and some students change groups. In none of our estimates do we condition on graduation or staying in the same group throughout the studies. Therefore, β measures the effect of being initially assigned to the same tutorial group. Last, since both, F_{ijt} and I_{ij}, are indicator variables, β is a percentage point difference between frequencies. Given that the magnitude of these differences is hardly intuitive in the dyadic setting, we also calculate the effect in percents relative to a baseline measure of similarity. For example, if F_{ijt} equals to 1 for a pair of students (i, j) having the same occupation at year t, then a pair of students from the same tutorial group is β percentage points more likely

[10]Hereafter, by the number of years after graduation, we mean the number of years after "scheduled" graduation - hence the number of years after matriculation plus the duration of the program (3 years). Therefore, the number of years after graduation becomes a predetermined, "mechanical" concept in our setting, which helps us to address the endogeneity of graduation timing.

[11]For each pair of students, a gender variable is defined specifying whether both students are male, female or differ in gender. Similarly, we define a variable specifying whether both students at the time of matriculation are Danish citizens, foreign citizens or mixed. Since age is a continuous variable, we follow a method described by Fafchamps and Gubert (2007) for undirected dyads and calculate the absolute age difference within a student pair, the age sum and the squares of both terms.

to be observed with the same occupation than a pair from the same cohort but different groups. To get a better understanding of the magnitude, we divide β by the baseline frequency for students from the same cohort but different groups.

3.2 Evaluation of the Empirical Strategy

The conditional random assignment of students to tutorial group peers is crucial for the identification of the influence of social interactions on career dynamics. In the absence of random assignment, if students can choose their peers, a selection problem arises (Manski, 1993). In this case, observed peer similarities in labour market outcomes are driven by initial similarities between students that are unobservable to the econometrician. As our identification strategy crucially depends on the conditional random allocation of students to tutorial groups, we demonstrate the balancedness of our sample by showing that group peers are not initially more similar than cohort peers.

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 tutorial 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 absolute difference between values of some predetermined variables for a pair of students (i, j).

For the balancing test, we use 12 characteristics measured at the time of matriculation - educational background (high school GPA, high school track, non-high school degree finished), municipality of residence, family background (mother's and father's number of years of education, mother's and father's disposable income rank) and previous employment histories (previous workplaces, firms, occupations and industries). As it is shown in Table 5, none of the variables appear to be unbalanced on the 5% level. Concerning the municipality of residence before matriculation, test indicates marginally significant on the 10% level sorting, which could be due to the number of tests performed. Therefore, we conclude that the sample is balanced on predetermined similarities between group and cohort peers.

3.3 Results

3.3.1 Main Results

We start with estimating the "excess" similarities formalized in Eq. 2. Table 6 explores the "excess" similarities between group peers in their choice of industry, occupation, firm and workplace. For all four outcome variables, we find that pairs of group peers are more likely to "intersect" in their career paths than pairs of cohort peers.

Since, as it was discussed above, point estimates measured in percentage points do not help to gain an intuition for both magnitude and relative importance of the effects, we also report the effect in percents relative to a baseline, which we define as the propensity of cohort peers to be observed working together at the same "cell". The observed similarity of point estimates for all outcomes hides large differences in effects measured in comparison to the baseline. A pair of students is around 4% more likely to work in the same industry and the same occupation if they were initially assigned to the same tutorial group. However, the effect is much larger when less aggregated labour market "cells" are considered. For a pair of students, being allocated to the same tutorial group leads to a 23% higher probability to work at the same firm and an increase by 40% in the probability to work at the same workplace after the graduation.[12]

So, we see that the effect of peers is concentrated at the most disaggregated level - the workplace. One might find this to be at odds with both, the idea that peer interactions are restricted to the period before graduation only and common academic environment as a major factor behind the excess similarities. Human capital spillovers at the classroom level, co-formation of career preferences and influences of TAs are naturally expected to affect general career trajectories which would be proxied by industry and occupation choices.[13] On the other hand, active alumni networks might act as a source of job-related information, from which, for example, graduates gain knowledge about job-openings or refer each other to employers. This type of interaction is expected to go in line with the strong firm and workplace similarities.

To further corroborate this reasoning, we ask whether the workplace effect drives the effects on the occupation-, industry- and even firm-level. For instance, group peers may be more likely to choose similar industries just because they are, in the first place, more likely to work at the same workplaces. To answer this question, we redefine "working

[12]The contrast between the point estimates and the recalculated relative effects is caused by the fact that even though the treatment leads to almost the same increase in frequencies of "working together" events in percentage points, the baseline probability of being observed at the same workplace is much lower than the baseline probability of being observed at the same industry. The latter pattern is also reflected in Table 3.

[13]Otherwise, classroom interaction between students would need to affect human capital along firm-specific/workplace-specific lines and/or influence preferences towards specific firms/workplaces.

together" as an event of "working together (at the same firm, or in the same occupation or industry), but not at the same workplace". As it is evident from Table 7, the effects of working in the same industry and firm lose their statistical significance after the exclusion of workplace similarities. Only for occupational choices, the effect stays significant on 10% level. We conclude that peer similarities in career outcomes are driven by interactions on the most desegregated level - the workplace. Furthermore, we interpret this result as suggestive evidence that students form post-graduation networks, and that post-graduation interaction in the labour market is a key driver of career similarities of former academic peers.

3.3.2 Timing and Heterogeneity

Does the intensity of post-graduation interaction of former academic peers decrease over time? Figure 1 illustrates the timing of "excess" firm and workplace similarities. In both cases, the effect is strongest a few years after scheduled graduation and diminishes over time.[14] For working for the same firm, the magnitude of the effect decreases more than twofold over time: from above 40% to less than 20%. At the start of their careers, a randomly chosen pair of group peers is almost twice as likely to share a workplace as a randomly chosen pair of cohort peers. Ten years after scheduled graduation, this effect decreases by four times but is still economically significant - 22%.

Social connections tend to form more intensely between individuals that are more similar (e.g., Eliason et al. (2019)). This phenomenon is usually referred to as network homophily. In the context of social networks among potential top-earners, an increased propensity to interact with similar students might potentially exacerbate inequality and hamper social mobility for less advantaged groups. Therefore, it is of interest to investigate whether the strong peer effects in workplace choices are more pronounced for more similar students.

We observe the most striking evidence of homophily among gender lines (Figure 2). We apply the same empirical approach as before, but explore the timing of the effect separately for pairs of students of the same gender and pairs of students of a different gender. The observed pattern of peer "excess" similarity is mostly driven by pairs of students of the same gender. For different gender pairs, the effect is significant only for the first two years after graduation and not significantly different from zero afterwards. On the other hand, for same-gender pairs, the effect is as high as 134% the year after graduation and still 45% ten years after graduation.

Figure 3 further investigates heterogeneous effects by gender, country of origin, age and high school GPA. First, we divide same-gender pairs of students into pairs of male

[14] This pattern is broadly consistent with findings in other social contexts (Eliason et al., 2019).

students and pairs of female students. The effect for both types of same-gender dyads is significantly higher than for mixed dyads (for which the effect is not significantly different from zero). Although the point estimate for male pairs is slightly higher, we cannot statistically distinguish the effect for female dyads from the effect on male dyads. Another dimension of (potentially) important heterogeneity is country of origin. We investigate if the effect for the student pairs of the same origin is different from the pairs of different origin. Only a few students in our sample are not Danish citizens and (by design) the dyad data construction drives the relative share of non-Danish dyads down. Hence, we cannot investigate Danish dyads separately from dyads of immigrants from the same source country.[15] Consequently, 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 a different origin. As we see, the effect for same-origin pairs is significantly higher (on the 5%-level) than for different origin pairs, while the latter is not significantly different from zero. This finding suggests that the social interactions in our sample are characterized not only by gender homophily but also by country-of-origin homophily.

Furthermore, we look at heterogeneity by age at the time of matriculation and high school GPA. Since both variables are continuous, we implement a different approach by interacting the treatment with the absolute difference of matriculation age (GPA) and the sum of matriculation age (GPA) for a student pair.[16] These two types of interactions are meant to reflect the different characteristics of the effect. The difference-interaction-term is aimed to capture the homophily of the effect - namely if students of a similar age and with similar grades tend to interact more. On the other hand, the sum-interaction-term captures if the effect is different students for older students and students with higher GPA. As we see, neither the age difference nor the age sum interaction appears to be statistically significant. The only significant interaction for these variables is GPA-sum. For a pair of students with one standard deviation higher sum of high school GPA, the effect of being assigned to one peer group is almost twice as high. This finding suggests that more able students are more likely to make use of alumni networks.

As we have already highlighted, not only does the typical student from our sample end up having a career at the top segment of the Danish labour market but also most of the students stem from affluent families. In a related study, Zimmerman (2019) suggests that a higher tendency of students from a wealthy background to form active labour market

[15] The share of dyadic observations constructed from a given group of students is much smaller than the share of these students in the population. If there are n students of a given type in a cohort of size N, the share of this type in a cohort is $\frac{n}{N}$, but the share of dyads constructed from students of these type will be $\frac{n(n-1)}{N(N-1)}$.

[16] For the main effects to be comparable to the baseline estimate in Table 6 we standardize the absolute differences and sums within our sample.

networks after graduation might explain why the returns to "elite" education programs are unequally distributed between students with different social backgrounds. In short, educational returns might be higher for students from a wealthy background because they accumulate more social capital.

To address this question, we contrast the effects of being assigned to the same peer group on the probability of working at the same workplace for specific dyads which we classify by the fathers' disposable income rank in the year before matriculation (Figure 4). We perform three comparisons, where we are defining "rich" dyads as pairs of students where both have fathers in the top 1%, top 10% and top 25% of the disposable income distribution in Denmark and contrast these effect to the remaining pairs of students (hence including the pairs where one of the students has a father in top income group). Even though the classifications based on fathers in the top 10% and top 25% lead to higher point estimates for the "rich" dyads, the difference with other dyads is not statistically significant. However, looking at students with fathers in top 1% shows that effects for this group of students is more than twice as large (as compared to all other students) and the difference is statistically significant at the 5% level.[17] Therefore, we conclude that, indeed, the strongest networks tend to emerge among the students coming from the richest families.

3.3.3 Robustness Checks

In this section, we address several threats to our interpretation of the results. If peer interaction in class leads students to either work abroad or to be self-employed, our estimates might be biased. We, therefore, check if selection out of our sample might invalidate our results. Next, we ask if subsequent education choices mediate the observed career similarities. The observed similarities of peers in careers would then not be the result of labour market interaction, but at least partially a direct result of similar education choices. Last, we investigate if the linearity imposed by our baseline specification - a linear probability model - leads to the quantitatively and qualitatively deceptive results.

Even though the evidence presented in Table 5 supports the assumption of (conditionally) random assignment of students to peer groups in our sample, not all of the students are observed at each year in our career sample. Therefore, for some student pairs, some dyad-years are missing. There are different reasons why a student is not observed in our sample in a given year. First, some students might leave Denmark temporarily

[17]Looking at pairs of students with fathers at the very top of disposable income distribution leads to a problem similar that we face with non-Danish students. Even though one-fifth of all students have fathers in that group, a much lower fraction of resulting dyads has both students in that group (around 4%). Consequently, at the very top, our estimates are noisier.

or permanently (this is particularly relevant for international students). Second, even students who stay in Denmark might be out of wage employment (e.g., non-employed or self-employed). If the peer group assignment process affects students' decision to leave the sample, for example, through peer effects in migration, and that decision is correlated with the initial propensity of students to make similar career choices, then missing dyads might cause our estimates to be biased.

To check whether sample selection could potentially cause bias in our estimates, we 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 for the baseline Eq. 2). The first two columns of Table 8 present results of the test using, first, all available observations in the data and, second, only the first five years after planned graduation from the program (which is where we observe the most substantial effects for our main results). As we see, group peers are not significantly more likely to be both missing from the sample in comparison to cohort peers (neither overall nor in the first five years). We further investigate if group assignment affects any of the stages of the selection process: both being observed as Danish residents ("Population Sample") and both being observed in our career sample *conditional* on being both observed among Danish residents ("Employment Sample"). Neither of the tests identifies significant correlations with the group assignment. We, therefore, conclude that sample selection is unlikely to cause bias to our estimates.

Although in this study, we investigate the effects of peer interactions on careers, similarities in career choices may arise because peers affect each other's educational choices that precede their career start. For example, if peers tend to have similar drop-out behaviour, this might result in similar jobs even without further post-graduation interaction in the labour market.[18] Table 9 shows regression results for the specification of Eq. 3 to similarities in educational choices on a Bachelor's level (graduation from the CBS Business Economics program, graduation from any Business Economics program, graduation from any Bachelor's program, switch to a different program) and a Master's level (start of any Master's program, graduate from any Master's program, start the same Master's program, start any Master's program at the same institution). We do not observe any significant effects of peer group assignment on educational choices. Hence, this channel is unlikely to explain our main findings.

Last, we check if using the linear probability model in our baseline specification (Table 6) provides misleading results. Given that the outcome variable is distributed

[18]Note that, even in this case, it would be hard to explain how similarities in educational choices are driving career similarities at the workplace level. Similar educational choices would likely cause similarities at the level of occupation and/or industry choice.

highly unevenly, the linear model might provide a bad approximation. We repeat our baseline analysis using a logit specification. As we can see, the coefficients in Table 10 have the same relative order and the same order of magnitude as the coefficients in our baseline specification - the strongest effects are again observed at the most disaggregated level. According to this specification, being assigned to the same group increases the probability for a pair to work together at the same workplace by 31.6%. The average marginal effects from the logit specification mirror the coefficients in the linear probability model.

4 The Effect of Working Together

4.1 Empirical Strategy

In the previous section, we have shown that peer interaction leads to a tendency to share the same workplace with group peers. However, does joining a group peer also improve labour market outcomes? Does post-graduation interaction with peers benefit workers' careers?

A key challenge for identifying if joining the firm of an incumbent group peer is beneficial is the choice of a proper comparison group. For many reasons, comparing outcomes of "joiners" to outcomes of "non-joiners" does not help to answer the question at hand. First of all, workers joining their peers might ex-ante be different from workers not joining.[19] Second, even without social interactions, (voluntary) job-to-job transitions tend to be associated with wage increases. Moreover, employers in our sample are far from being average. Therefore, even by comparing the effect of joining a group peer's workplace to job-to-job transitions events does not disentangle the effect of "working together" from the effect of working at a specific type of workplace. Finally, workers who decide to join their peers may be subject to unobserved shocks, which would affect their careers even in the counterfactual scenario of not joining a group peer's workplace.

To address these concerns, we employ a Difference-in-Difference type strategy[20] and compare career trajectories of workers who join a workplace with one or more incumbent group peers with workers who join a workplace with one or more cohort peers. Using the event of joining cohort peers as a counterfactual resembles the "excess" similarity strategy discussed above. Since real social interactions are unobserved and may be active across

[19] Relying on previous research (e.g., Kramarz and Skans (2014)), one might expect workers who find jobs through their social connections to be negatively selected.

[20] In particular, the identifying variation that we use stems from a Difference-in-Difference type comparison. To accommodate statistical power concerns, resulting from the necessity to identify a set of fixed effects and controls, we use all information about student careers in our sample, not only on the events of joining a cohort or a group peer.

cohort peers as well, the interpretation of the difference between joining a group peer and cohort peer depends on the assumption that the former is more likely to be the result of "networking" among former peers than the latter.

We use within-worker variation (individual fixed effects) to account for systematic differences between workers hired through social connections and "formal" channels. The comparison of group peer "joiners" to cohort peer "joiners" addresses the identification threat from the firm composition. As group peers for one student are cohort peers for another, there should be no ex-ante systematic difference in the general characteristics and "quality" of firms between establishments where group peers or cohort peers work. Although, this does not mean that it is necessarily the case *ex-post*. Firms, where workers join their former group peers, might on average be better or worse than firms at which they join their cohort peers. Such an ex-post difference would point towards the fact that alumni networks are used to get access to specific jobs. Even though the existence of unobserved shocks (correlated with the decision to join a peer's workplace) is inherently untestable in our setting, we provide evidence in favour of common trends between students who join an incumbent peer-group member and students who join an incumbent cohort member below.

Our empirical strategy differs from the approaches that are commonly employed in the literature, which studies the effects of referrals on labour market outcomes. Some studies rely on detailed personnel records and compare workers hired through referrals to non-referred workers (for example, Brown et al. (2016) and Burks et al. (2015)). 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) and Zhu (2018)). From the point 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 job search networks (referrals or information sharing about job openings). Workers could benefit from getting access to jobs in higher-paying firms (or occupations). Controlling for firm-fixed effects suppresses these additional benefits of labour market networks for the worker. As we aim to capture the full picture of potential benefits (or costs) of labour market networks, we do not control for firm-fixed effects.

Our approach is formalized in the following regression framework:

$$y_{ijt} = \lambda_t + \mu_i + \alpha * CohortInc_{ijt} + \beta * GroupInc_{ijt} + \gamma X_{it} + \epsilon_{ict}, \tag{4}$$

where y_{ict} is the outcome variable of interest for individual i at firm j in year t; λ_t is a year fixed effect; μ_i is an individual fixed effect; $CohortInc_{ijt}$ is an indicator which equals to 1 if at the time where worker i joins firm j there was at least one cohort peer (from the same or different group) working at firm j; $GroupInc_{ijt}$ - is an indicator which equals to 1 if at the time when worker i joins firm j there was at least one group peer working at firm j; X_{it} are age and experience third degree polynomials.

Note that β is the parameter of interest, which is identified through a comparison between events of joining a group peer versus a cohort peer, and captures the benefits (or costs) of finding a job through the alumni network. On the other hand, α does not have a similar interpretation, as it compares transitions to cohort peers to all other observations (including individuals not changing a firm).

4.2 Evaluation of the Empirical Strategy

First of all, the validity of the proposed empirical strategy depends on a parallel trend assumption. The potential outcomes of students who join a group peer should be similar to those who join a cohort peer. To provide suggestive evidence in favour of this assumption, we look for differences in the pre-trends between the treatment and control groups. If students who join their former cohort peers as a comparison group for students who join their former group peers show similar career dynamics before the event of the job change, we interpret this as suggestive evidence that the assumption of parallel trends in potential outcomes holds.[21]

Figure 5 shows differences in dynamics between students who join a cohort peer and those who join a group peer for both earnings and daily wages. Most importantly, both groups show very similar wage dynamics before the job changing event.[22] However, after the event of a job change, workers joining their former group peers experience faster wage growth. Still, this gap is almost closed the year after the job change. Hence, we conclude that the treatment group is not selected on the pre-trend (e.g., initially faster-growing wages).

Another crucial component of our empirical approach is an assumption about the absence of *ex-ante* differences between group peers and cohort peers. For example, if there is no systematic difference between jobs where group peers and cohort peers work, but joining a group peer leads to superior labour market outcomes, we interpret the latter effect as a result of tighter social connections to group peers. The effect might stem from various sources - referral premiums, a higher job arrival rate, a superior job offer distribution, productivity gains.[23] Table 11 supports the assumption that there is no

[21] Note that the same students could be potentially in treatment and control groups in their careers.

[22] As the graphs represent raw means, the difference in levels between the two groups is not informative.

[23] Alternative interpretation that we are not able to rule out here is that workers have higher reservation wages when they accept job offers from a place where someone works whom they know. Such behaviour

ex-ante difference between jobs where group peers and cohort peers work.

4.3 Results

In this section, we present and discuss our empirical results on whether it is beneficial to use alumni networks to find a new job.

4.3.1 Main Results

We estimate the effect of joining a firm with an incumbent group peer vs an incumbent cohort peer using the framework formalized in Eq. 4. We consider multiple labour market outcomes. Besides daily wages, we also investigate the effects on job turnover behaviour. If peers provide information about jobs that match skills and/or preferences of workers better, we expect workers who join their group peers to stay at the workplace longer. Moreover, access to better-paying jobs might constitute a part of the benefits that workers get from alumni networks. To further investigate this point, we construct pay ranks of firms and occupations. We check if workers tend to join their group peers at better-paying workplaces/occupations/industries.[24]

As Table 12 suggests, joining a workplace with an incumbent group peer results in 5.8% higher daily wages than joining a workplace with an incumbent cohort peer. The probability of leaving the new employer is 5.9 percentage points lower, which means that job matches formed in a treatment group tend to be significantly more stable. Not all the return to alumni networks to find jobs, therefore, corresponds to within-firm "referral premiums", as joining a group peer is associated with joining better-paying firms, industries and occupations.

4.3.2 Heterogeneity

Table 13 investigates heterogeneous returns to joining a group peer in terms of daily wages. We interact the treatment variable with various predetermined characteristics of the worker. The results largely mirror the heterogeneity results from Section 3.3.2. Students experience the largest returns of joining a group peer (over joining a cohort peer) at early stages in their careers. There are no statistically significant gender differences. Students with a higher ability (measured by high school GPA) and students from the

could be rationalized by a higher weight of socially closer individuals in interpersonal comparisons.

[24]The pay rank is defined as the rank of a firm fixed effect (occupation fixed effect) in a given year from a daily wage regression on the entire Danish working population (1985-2016) with individual fixed effects, year fixed effects and age polynomials. Industry fixed effects are calculated as employment weighted averages of firm fixed effects. The lowest rank is 1 and the highest is 100.

wealthy background (measured by a father in the top 1% of income distribution in the year before matriculation) further experience more substantial wage effects of joining a ”network” job.[25]

5 Conclusion

This paper studies how social connections between fellow students shape their career outcomes. Our empirical strategy relies on the tutorial group randomization in a large business education program in Denmark, many of whose graduates later are part of the country's top earners. Using extensive Danish labour market register data, we follow the individual careers of former academic peers. To identify the effects of peer interactions on career trajectories, we compare propensities to choose similar jobs between pairs of students from the same tutorial groups and pairs of students from the same cohorts but a different tutorial group. We find that the professional lives of former group peers tend to be significantly more similar. The effect underlying these pattern is an ”excess” tendency of the former group peers to be employed at the same workplaces. The evidence suggests that ”excess” similarities between group peers are more likely to be driven by contemporaneous alumni interactions than by persistence of influences from the study period. Our analysis reveals that alumni networks persist for years after graduation, although their importance tends to decrease. The effect of social interactions with peers on career choices exhibits strong homophily by gender and origin. Importantly, students of higher ability and who stem from wealthy family background experience the strongest effects of peers on careers.

Moreover, we ask if students following group peers experience significant career improvements. To answer this question, we implement a strategy based on comparing two types of job-to-job transitions - joining a firm with a former tutorial group peer and joining a firm with a former cohort peer. We show that students that join their group peers benefit from higher wage growth and gain access to more stable and better-paying jobs. Again, these effects are most pronounced for high-ability students coming from affluent families at the earlier stages of their careers.

In this paper, we provide evidence for the significant benefits of alumni networks for students at the start of their careers. Universities who want to maximize their student's educational returns are therefore well advised to foster alumni interactions through alumni events. Furthermore, our results indicate that fostering alumni networks raises equity concerns.

[25] Here stronger effects might include 1) higher returns to social interactions and 2) a higher probability that a given job-to-job transition was caused by some sort of social interaction or 3) both.

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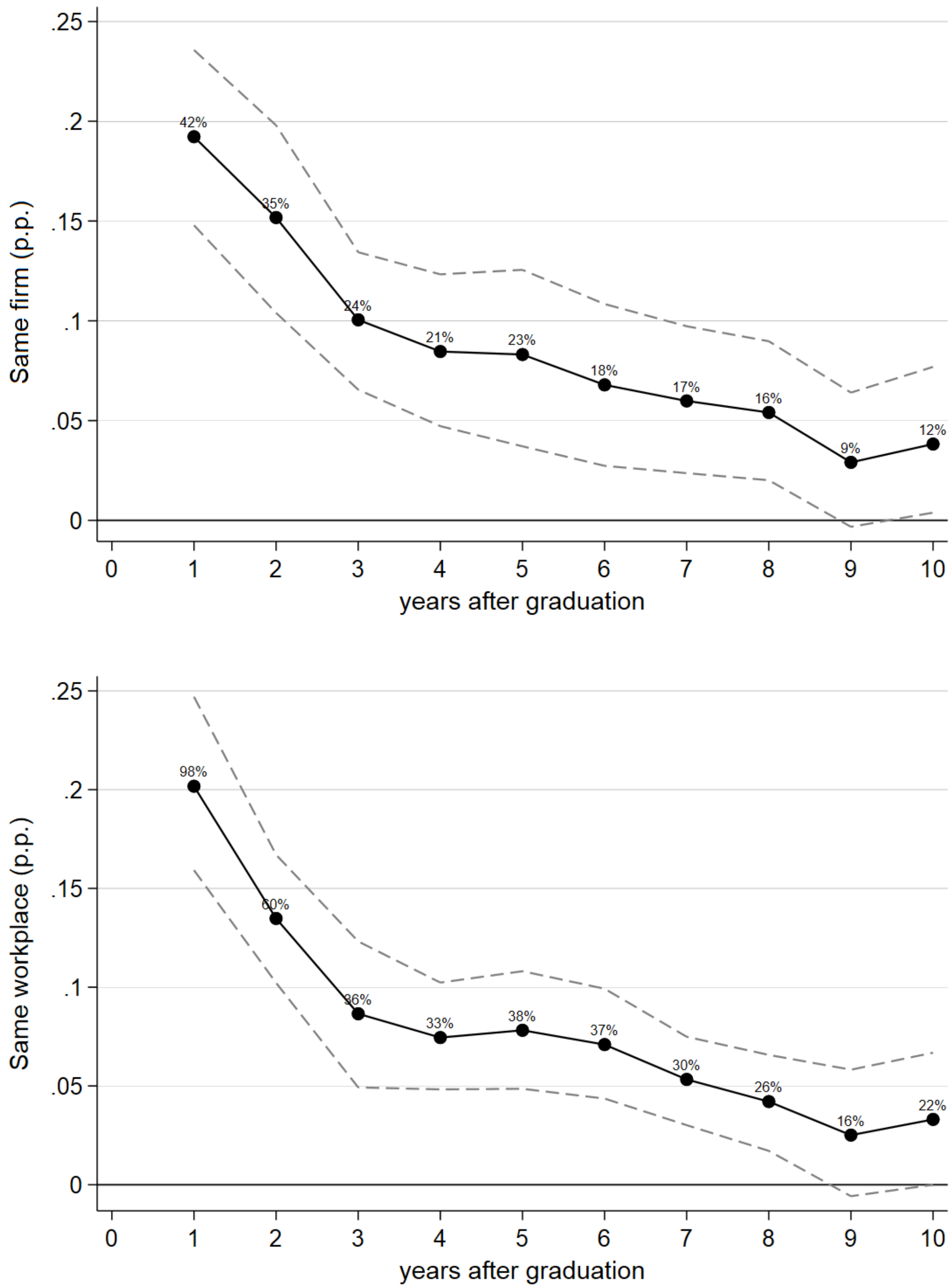

Figure 1: Same Firm and Same Workplace: Timing of the Effect

Notes: The y-axis represents the effect of being assigned to the same group in percentage points. The relative effects (in %) are represented on a graph. 5% confidence intervals refer to point estimates. Standard errors are calculated by wild cluster bootstrap at the matriculation cohort level (5000 replications).

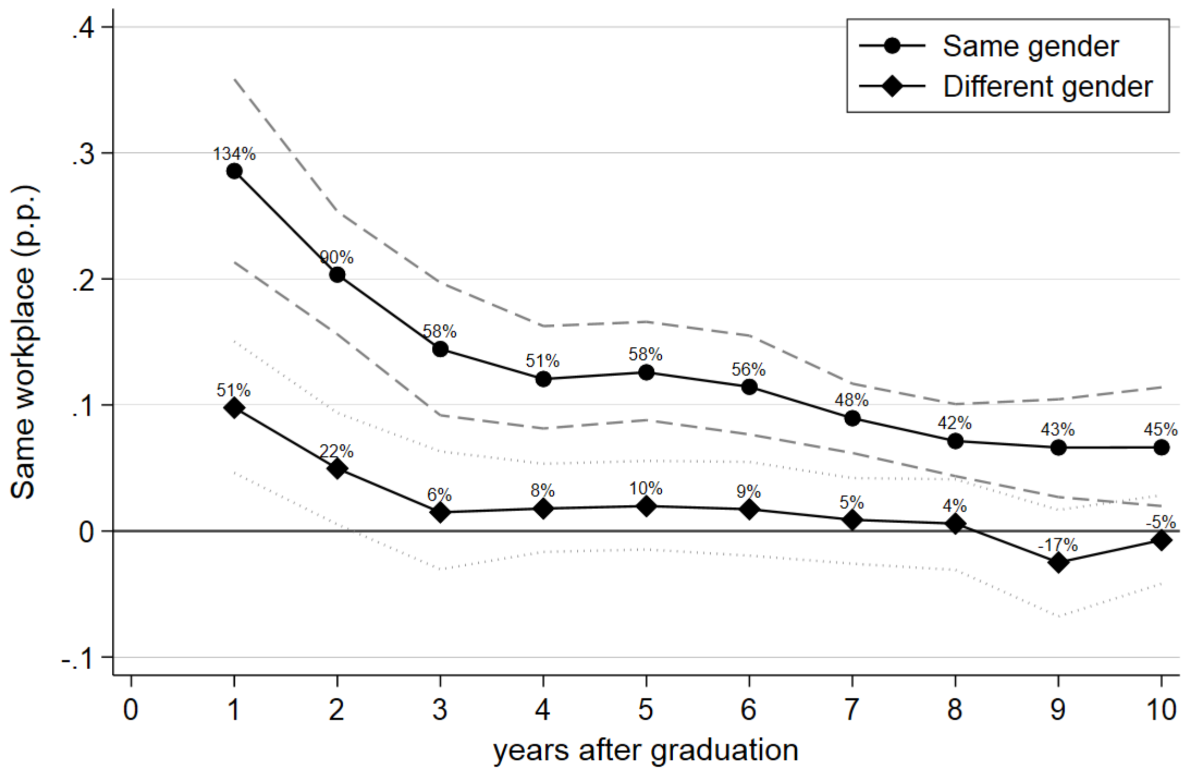

Figure 2: Same Workplace: Timing of the Effect, by Gender

Note: The y-axis represents the effect of being assigned to the same group in percentage points. The relative effects (in %) are represented on a graph. 5% confidence intervals refer to point estimates. Standard errors are calculated by wild cluster bootstrap at the matriculation cohort level (5000 replications).

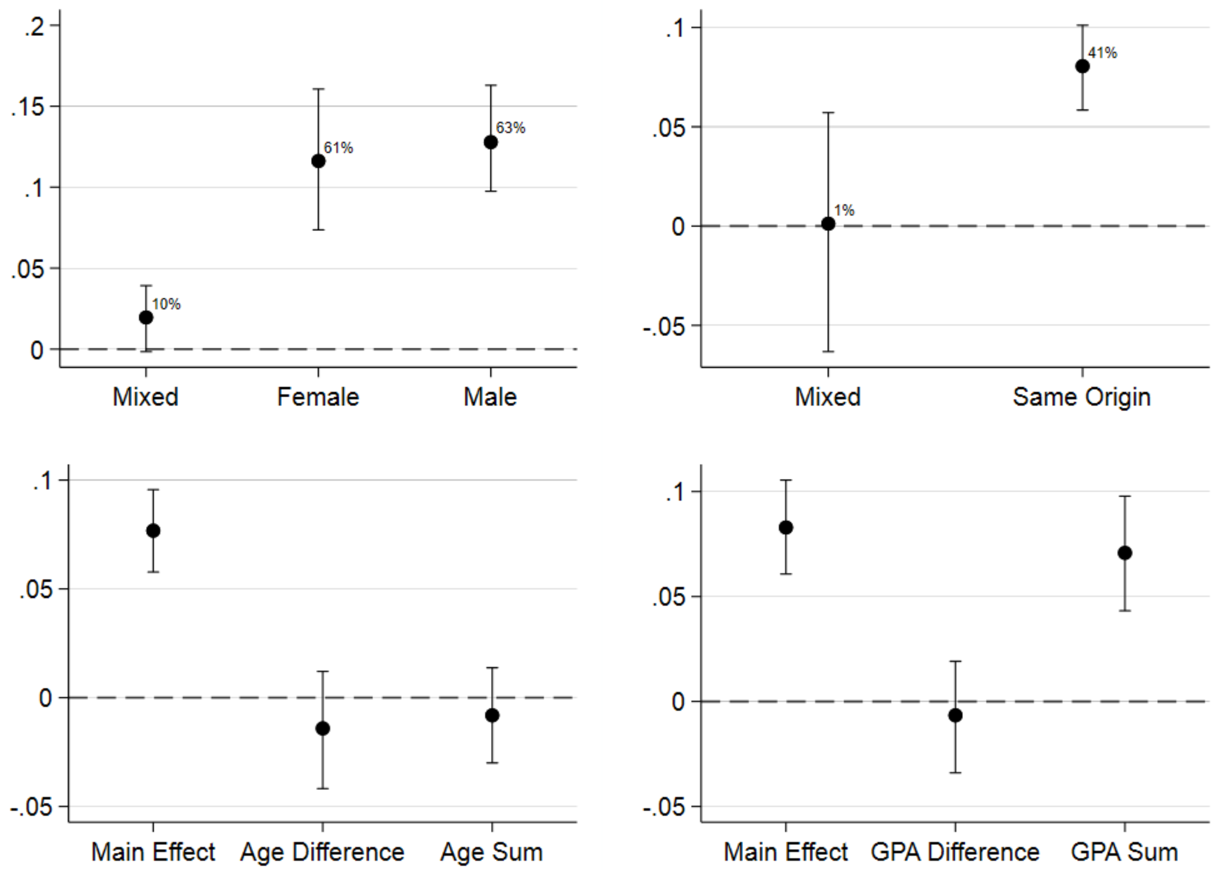

Figure 3: Same Workplace: by Gender, Country of Origin, Age and High School GPA

Note: The y-axis represents the effect of being assigned to the same group in percentage points. The relative effects (in %) are represented on a graph. 5% confidence intervals refer to point estimates. Standard errors are calculated by wild cluster bootstrap at the matriculation cohort level (5000 replications).

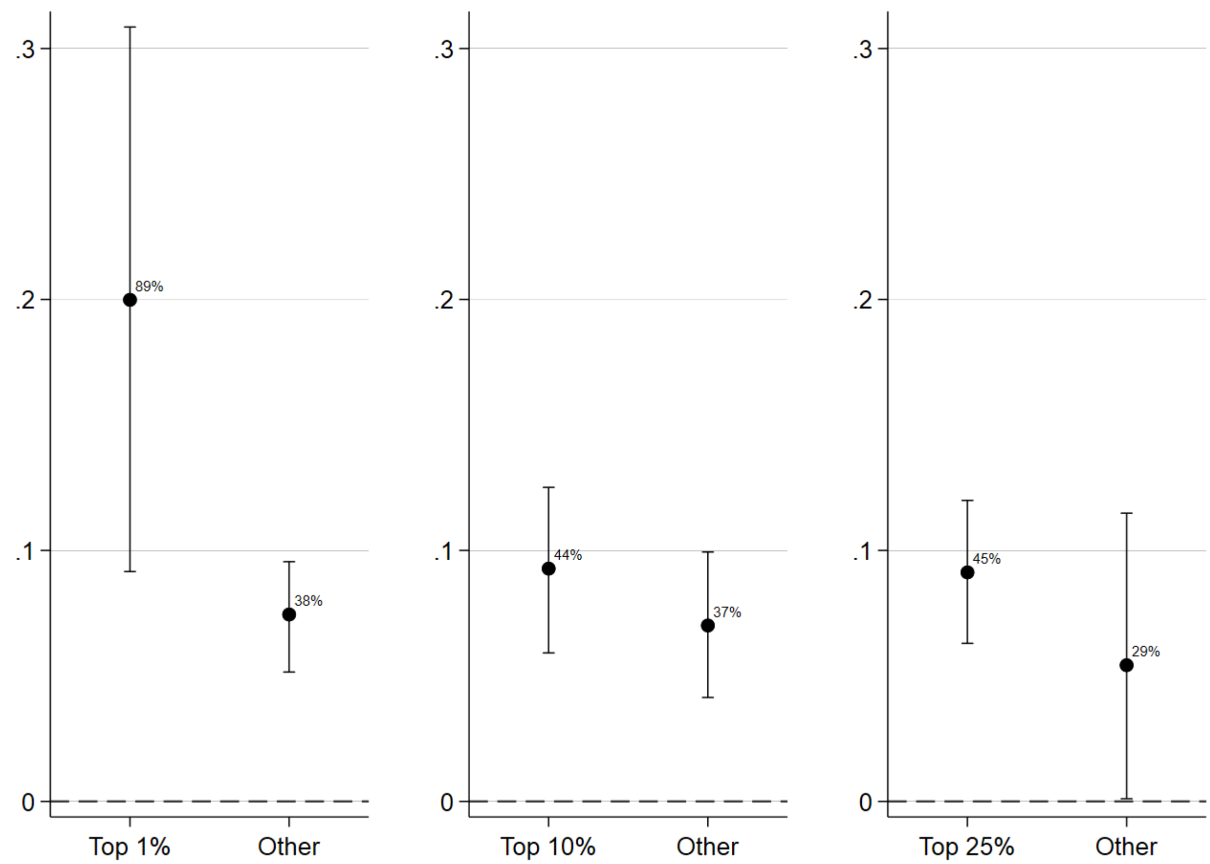

Figure 4: Same Workplace: by Father's Income Rank

Note: The y-axis represents the effect of being assigned to the same group in percentage points. The relative effects (in %) are represented on a graph. 5% confidence intervals refer to point estimates. Standard errors are calculated by wild cluster bootstrap at the matriculation cohort level (5000 replications).

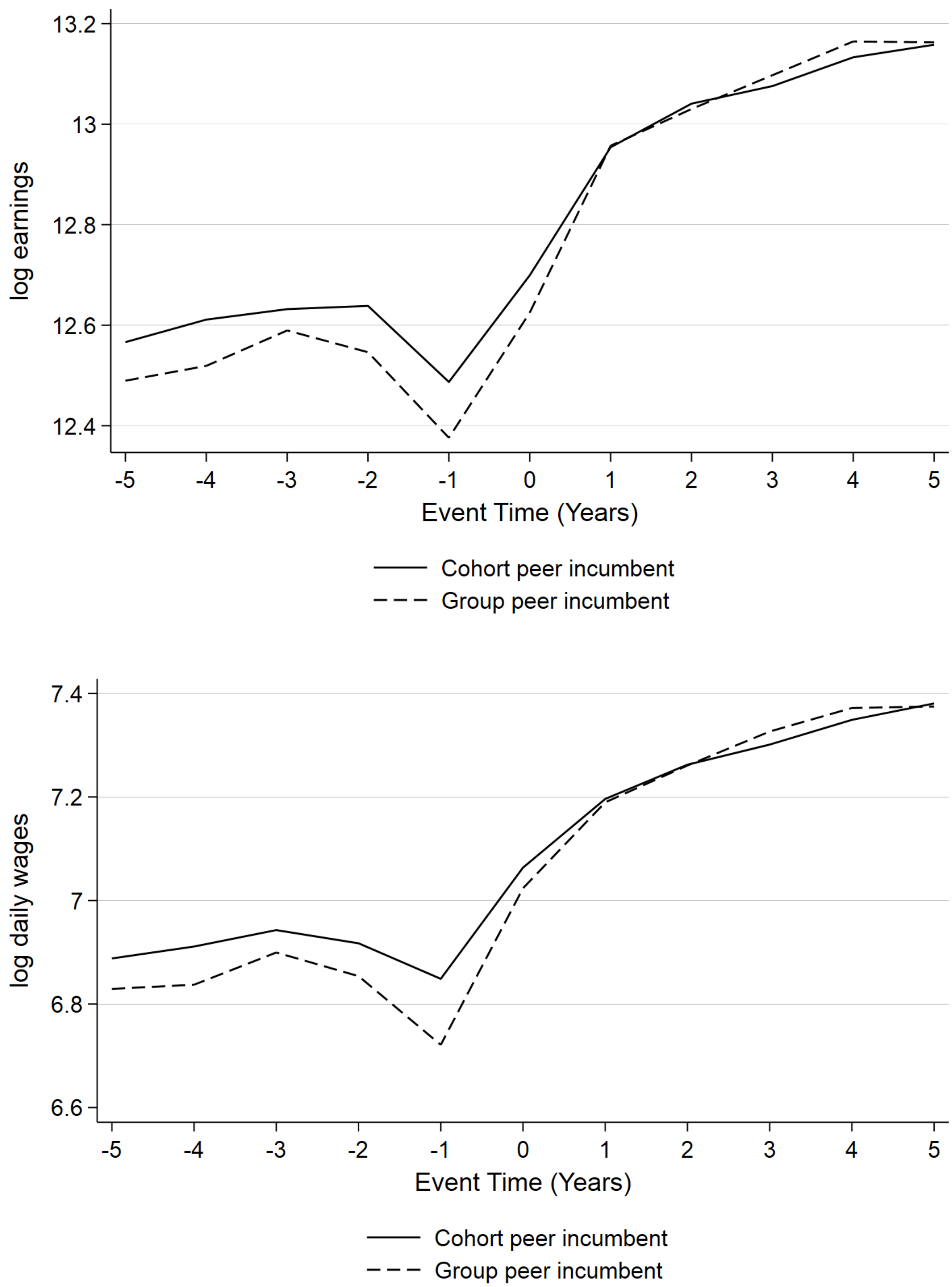

Figure 5: Wage Dynamics: Group Peer Joiner vs Cohort Peer Joiner

Notes: The y-axis represents real log annual/daily wages, the x-axis - number of years relative to the job-change event. ”Cohort peer incumbent” line depicts dynamics for students joining a firm where someone from their cohort works, ”Group peer incumbent” line - dynamics for students joining a firm where someone from their peer group works. Event time 0 corresponds to a year when a student joins a firm.

List of Tables

Table 1: Descriptive Statistics: Background Variables

	Male	Female	Total
Danish	0.966	0.962	0.965
	(0.181)	(0.191)	(0.185)
Age	21.47	21.40	21.45
	(2.291)	(2.394)	(2.327)
HS GPA	0.0931	0.223	0.138
	(0.792)	(0.718)	(0.770)
Gap years	0.988	0.958	0.978
	(1.346)	(1.354)	(1.349)
Work experience	0.746	0.745	0.746
	(0.435)	(0.436)	(0.435)
Father's education	13.65	13.58	13.62
	(2.779)	(2.806)	(2.788)
Mother's education	13.01	12.67	12.89
	(2.687)	(2.742)	(2.711)
Father's income rank	86.63	87.12	86.80
	(19.03)	(18.57)	(18.88)
Mother's income rank	70.90	70.45	70.75
	(22.52)	(22.52)	(22.52)
Father in top 1%	0.206	0.187	0.200
	(0.404)	(0.390)	(0.400)
Mother in top 10%	0.212	0.212	0.212
	(0.409)	(0.409)	(0.409)
Group size	36.11	35.35	35.85
	(5.793)	(5.989)	(5.872)
Cohort size	598.9	602.2	600.1
	(50.92)	(48.88)	(50.25)
Observations	8,218	4,300	12,518

Notes: This table shows mean values for individual background characteristics of students in the sample. Standard deviations are in parenthesis. Parental income is measured the year before student's matriculation.

Table 2: Descriptive Statistics: Top 10 Most Common Occupations & Industries

Occupation	Share	Industry	Share
Finance Professionals	14.12	Monetary intermediation	7.494
Financial and Mathematical Associate Professionals	6.937	Computer programming, consultancy and related	5.882
Legal and Related Associate Professionals	5.084	Accounting, bookkeeping, auditing, tax consultancy	4.962
Sales, Marketing and Public Relations Professionals	5.048	Public Administration, the economic and social policy	4.081
General Office Clerks	4.985	Wholesale of household goods	3.988
Sales and Purchasing Agents and Brokers	4.501	Advertising	3.397
Administration Professionals	4.414	Management consultancy activities	3.265
Software and Applications Developers and Analysts	3.710	Other financial service activities	2.498
Sales, Marketing and Development Managers	3.566	Secondary education	2.277
Business Services and Administration Managers	3.474	Pharmaceuticals	2.162
Total	55.84	Total	40.00

Notes: The table shows industry and occupation shares. Occupations are defined on the 3-digit level using the DISCO-08 classification. Industries are defined on the 3-digit level using the DB07 classification. Shares are calculated on all observations of careers in our sample for the years 2008-2016.

Table 3: Descriptive Statistics: Career Similarities

	Group Peers	Cohort Peers	Program Peers
Same workplace	0.210	0.677	0.911
	(0.0451)	(0.275)	(0.640)
Same firm	0.363	0.817	0.956
	(0.0849)	(0.398)	(0.733)
Same industry	0.731	0.985	0.999
	(0.279)	(0.810)	(0.986)
Same occupation	0.871	0.988	0.999
	(0.417)	(0.896)	(0.992)

Notes: The table reports share of students who have ever worked together with one of their group/cohort/program peers. Corresponding share of student-years in parenthesis. Industries are defined at the 6-digit level, occupations - at the 4-digit level.

Table 4: Descriptive Statistics: Career Panel

	Male	Female	Total
Income rank	85.81	83.20	84.87
	(17.69)	(16.41)	(17.28)
Top 10%	0.582	0.441	0.531
	(0.493)	(0.496)	(0.499)
Top 1%	0.102	0.0281	0.0751
	(0.302)	(0.165)	(0.263)
Log earnings	12.93	12.71	12.85
	(0.865)	(0.781)	(0.842)
Log daily wage	5.718	5.548	5.657
	(0.525)	(0.390)	(0.487)
Manager	0.127	0.0737	0.107
	(0.333)	(0.261)	(0.310)
Top manager	0.0305	0.0101	0.0229
	(0.172)	(0.1000)	(0.150)
Firm size (FTE)	377.4	444.5	401.4
	(667.7)	(908.4)	(763.4)
Observations	91,880	51,691	143,571

Notes: This table shows mean values for career outcomes in a panel of all available observations for employed students (from the first year after potential graduation). Standard deviations are in parenthesis.

Table 5: Dyadic Balancing Test

	GPA	HS Track	Non-HS Degree	Municipality
Same group	0.000529	0.0159	-0.00169	0.110*
	(0.766)	(0.905)	(0.317)	(0.0900)
Observations	3,308,941	3,166,697	3,749,507	3,749,507
	Mother's Education	Father's Education	Mother's Income	Father's Income
Same group	0.00382	-0.00266	-0.0329	-0.0335
	(0.422)	(0.615)	(0.444)	(0.550)
Observations	3,075,643	2,795,315	3,318,792	3,088,727
	Workplace	Firm	Industry	Occupation
Same group	0.0178	0.0236	0.00770	0.0391
	(0.122)	(0.426)	(0.869)	(0.165)
Observations	3,749,507	3,749,507	3,749,507	3,749,507

Notes: The table reports the balancing test as specified in Eq. 3 for the following variables measuring predetermined similarities between students: the difference in high school GPA, the same high school track, both students with non-high school degree, the same municipality of residence, the difference in years of education of mothers, the difference in years of education of fathers, the difference in disposable income ranks of mothers, the difference in disposable income ranks of fathers, worked at the same workplace, firm, industry and occupation five years before matriculation. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Career Similarities: Baseline Regressions

	Same Industry	Same Occupation	Same Firm	Same Workplace
Same group	0.0858***	0.136***	0.0877***	0.0779***
	(0.00660)	(0.000800)	(0)	(0)
Effect(in %)	3.999	3.967	23.09	40.16
Baseline	2.146	3.430	0.380	0.194
R-squared	0.00188	0.00110	0.000356	0.000446
Observations	20,707,850	12,201,751	25,073,724	20,419,748

Notes: This table shows estimates of the linear probability model as specified in Eq. 2. Coefficients and baselines are multiplied by 100 to reflect percentage points. P-values calculated using wild cluster bootstrap (5000 replications) on matriculation cohort level are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Career Similarities: Net of Workplace Effects

	Same Industry	Same Occupation	Same Firm
Same group	0.00936	0.0791*	0.00976
	(0.722)	(0.0504)	(0.249)
Effect(in %)	0.474	2.345	4.598
Baseline	1.974	3.373	0.212
R-squared	0.00161	0.000967	0.000163
Observations	20,417,993	10,278,441	20,419,748

Notes: This table shows estimates of the linear probability model as specified in Eq. 2. Coefficients and baselines are multiplied by 100 to reflect percentage points. P-values calculated using wild cluster bootstrap (5000 replications) on matriculation cohort level are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 8: 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	0.0447	0.0293	0.0330	0.00759	0.0215	0.0260
	(0.268)	(0.497)	(0.322)	(0.689)	(0.542)	(0.494)
Baseline	67.22	68.55	89.19	95.74	75.35	71.58
R-squared	0.0566	0.0565	0.106	0.0978	0.0250	0.0371
Observations	64,845,094	18,747,535	64,845,094	18,747,535	57,850,264	17,948,399

Notes: The table reports results of the sample selection tests (baseline specification of Eq. 2 is used). Outcome variables are indicator variables equal to one if both students are observed in our career sample in a given year ("Sample"), in population of Danish residents in a given year ("Population Sample") or in the career sample conditional on both being observed as Danish residents in a given year ("Employment Sample"). For each outcome variable we use both all available observations and only first 5 years after (scheduled) graduation from the program. Coefficients and baselines are multiplied by 100 to represent percentage points. P-values in parenthesis are calculated by wild cluster bootstrap on matriculation cohort level (5000 replications). *** p<0.01, ** p<0.05, * p<0.1

Table 9: Career Similarities: Educational Choices

	Graduate from CBS HA	Graduate from any HA	Graduate from any Bachelor's	Switch to other program
Same group	0.0187	0.0185	0.0755	0.0170
	(0.831)	(0.844)	(0.383)	(0.192)
Effect(in %)	0.0382	0.0375	0.137	2.411
Baseline	49.01	49.33	55.08	0.703
R-squared	0.0481	0.0488	0.0582	0.00272
Observations	3,428,983	3,428,983	3,428,983	3,428,983

	Master's start	Master's graduate	Master's program	Master's institution
Same group	0.0418	0.00417	0.238	0.0484
	(0.688)	(0.952)	(0.193)	(0.494)
Effect(in %)	0.0783	0.0133	0.409	0.0602
Baseline	53.36	31.39	58.24	80.26
R-squared	0.0615	0.0629	0.00908	0.0217
Observations	3,428,983	3,428,983	1,830,807	1,830,807

Notes: This table shows estimates 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, start any Master's program at the same institution. Coefficients and baselines are multiplied by 100 to reflect percentage points. P-values calculated using wild cluster bootstrap (5000 replications) on matriculation cohort level are in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: Career Similarities: Logit Specification

	Same Industry	Same Occupation	Same Firm	Same Workplace
Same group	3.549**	3.889***	20.25***	31.55***
	(0.0212)	(0.000800)	(0)	(0)
AME	0.0747	0.129	0.0777	0.0627
Baseline	2.146	3.430	0.380	0.194
Pseudo R-sq	0.00945	0.00377	0.00678	0.0163
Observations	20,707,850	12,201,751	25,073,724	20,419,748

Notes: This table shows estimates from the logit specification of a baseline regression (Table 6). Average marginal effects and baselines are multiplied by 100 to reflect percentage points. P-values calculated using score cluster bootstrap (5000 replications) on matriculation cohort level are in parenthesis (Kline and Santos, 2012). *** p<0.01, ** p<0.05, * p<0.1

Table 11: Group Peers vs Cohort Peers

	Joiner	Non-Joiner
Cohort Wage	6.886	7.004
	(0.493)	(0.449)
Group Wage	6.885	7.004
	(0.513)	(0.466)
Cohort Occ. Pay	62.19	63.86
	(8.318)	(7.724)
Group Occ. Pay	62.19	63.86
	(9.632)	(9.144)
Cohort Industry Pay	58.01	58.62
	(4.406)	(3.884)
Group Industry Pay	57.97	58.61
	(7.681)	(7.169)
Cohort Firm Pay	64.19	64.55
	(4.666)	(4.095)
Group Firm Pay	64.14	64.55
	(7.908)	(7.344)
Cohort Firm Size	384.3	395.5
	(59.63)	(58.23)
Group Firm Size	382.6	395.9
	(158.2)	(161.4)
Observations	3,348	147,049

Notes: This table shows mean values and standard deviations for labour market outcomes of students' group and cohort peers - for students who join their peer's firm and all students in the sample. Occupation, industry and firm pay ranks are constructed from the occupation, industry and firm fixed effects from the multi-way fixed daily wage regressions with individual fixed effects, year fixed effects and age polynomials on the whole population of Danish workers (1985-2016).

Table 12: The Effect of Working Together: Baseline Regressions

	Log Daily Wage	Firm Change	Occupation Pay Rank	Industry Pay Rank	Firm Pay Rank
Group Peer	0.0578***	-0.0591***	2.836***	3.135***	3.861***
	(0.0150)	(0.0126)	(0.721)	(0.906)	(0.843)
Cohort Peer	0.0791***	-0.0677***	2.806***	5.108***	6.436***
	(0.00667)	(0.00607)	(0.347)	(0.471)	(0.471)
R-squared	0.617	0.146	0.502	0.606	0.605
Observations	170,725	139,998	150,114	143,794	143,794

Notes: This table shows estimates from the model as specified in Eq. 4. The standard errors are cluster at the peer group level. Occupation/Firm/Industry Pay Ranks are based on corresponding fixed effects from regressions on the whole Danish working population with individual fixed effects, year fixed effects and age polynomials. *** p<0.01, ** p<0.05, * p<0.1

Table 13: The Effect of Working Together: Heterogeneous Effects

	(1)	(2)	(3)	(4)
Group Peer	0.0561***	0.0580***	0.0446***	0.0475***
	(0.0159)	(0.0156)	(0.0166)	(0.0161)
Group Peer×YSM		-0.00821***		-0.00830***
		(0.00228)		(0.00235)
Group Peer×Female		-0.00626		-0.0274
		(0.0273)		(0.0287)
Group Peer×High GPA			0.0587**	0.0770***
			(0.0249)	(0.0246)
Group Peer×Father Top 1%			0.0672**	0.0851***
			(0.0293)	(0.0294)
Cohort Peer	0.0773***	0.0769***	0.0782***	0.0776***
	(0.00727)	(0.00729)	(0.00777)	(0.00780)
R-squared	0.636	0.636	0.638	0.638
Observations	159,536	159,536	134,082	134,082

Notes: In all columns log daily wage is a dependent variable. Group Peer variable is interacted with the number of years after graduation, a female dummy, a dummy for high school GPA above cohort median and a dummy for having a father in Top 1% of Danish disposable income distribution year before matriculation. Variables in interaction terms were demeaned. The standard errors are cluster at the peer group level. *** p<0.01, ** p<0.05, * p<0.1