Beyond the “Old Boys’ Network”: Social Networks and Job Finding at Community Colleges[*]

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Abstract

Research and popular discourse on social networks in higher education have focused on elite universities, even though most students do not attend such institutions. This paper sheds novel light on how classmate networks at community colleges influence job finding. Using data from a state community college system linked to matched employer-employee records, I exploit quasi-random variation in class section enrollment within courses to detect network effects. I find students are significantly more likely to get a job at a classmate’s firm later on than at the firm of a peer enrolled in a different section of the same course.

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Social networks play an important role in the job search process (Ioannides and Datcher Loury, 2004; Topa, 2011). Researchers estimate that at least 50 percent of new jobs are found through informal channels, rather than through formal job search methods. (Granovetter, 1974; Topa, 2011). Social networks in college are potentially important in influencing not only early job finding but also career trajectory and lifetime earnings. Since people typically invest in higher education during the earlier stages of working life, job outcomes out of college are especially critical, and poor job prospects out of college have persistent negative career effects (Kahn, 2010; Oreopoulos, von Wachter, and Heisz, 2012).

Social networks in higher education are often conceptualized as an elite school phenomenon—as evoked by the phrase the "old boys' network"—in which well-connected individuals provide exclusive access to opportunities for one another. However, this framing excludes most college students who, by construction, attend a "non-elite" institution. Community colleges alone educate 42 percent of the college student population in the United States (Ma and Baum, 2016). These students on average come from very different backgrounds and go into different types of jobs than peers from elite schools, and they are likely to form and leverage social networks in different ways. How do social networks operate in the absence of the "old boys"?

This paper provides novel insight on how classmate networks at community colleges, also known as two-year colleges, affect job finding. Specifically, I use administrative data from community colleges in Arkansas to i) identify the causal effects of taking a class with a peer on getting a job at a firm where the peer works and ii) assess which factors influence network formation in this setting. To do so, I link student transcripts that contain detailed information on the courses students took from 2004 to 2012 with quarterly matched employer-employee records collected from Arkansas Unemployment Insurance data.

A key challenge to identification is disentangling network effects from other factors correlated with course selection that may also affect job finding. If classmates are more likely to end up working together than non-classmates, it is unclear a priori if this relationship is due to a network effect or due to unobservable factors that affect both students' class enrollment decisions and job finding. For example, students who are interested in health professions may be more likely to work at the same hospital later due to shared professional interests, and these interests may also lead them to sign up for the same courses that are geared towards these jobs. Alternatively, some programs or fields at a school may have reputations or connections with certain firms, creating a pipeline effect to the firm for students in those courses. Both of these channels would show an increased propensity for students to end up working with classmates compared to peers who were not classmates, even in the absence of any network effects.

To identify the causal effects of classmate networks, I exploit quasi-random variation in class section enrollment within courses. A course consists of one or more sections that are taught to discrete sets of students but that offer the same content and amount of credit. I examine whether an individual is more likely to get a job at a firm where a classmate works than at the firm of a peer enrolled in a different section of the same course.[1] I restrict my analysis to the first semester a pair shares any courses since subsequent enrollment patterns may be influenced by network effects from social interactions in prior courses. I define getting a job at a firm where a peer works as an instance in which a) the student and their peer are employed at the same firm and b) the peer started working at the firm first. This could include instances where the peer joined the employer during college, as well as new employment relationships the peer forms post-college. I interpret an increased propensity to get a job at a classmate's firm relative to a course peer's firm as indication that students use classmate

[1] I refer to a student's classmates in this setting as peers from the same section of a course, as these are the peers with whom a student shares a classroom setting.

networks to find jobs. Results indicate classmates at community colleges play a significant role in helping each other find jobs. Taking a class with a peer increases the propensity for a student to get a job at a firm where their peer works within three years of their first shared course by 3.8 percent.

Classroom networks likely represent a key channel through which two-year college students interact, especially since these campuses are largely non-residential and many students work during college, potentially limiting time for interaction outside of the classroom. However, it is likely some peer interactions also take place outside of the classroom setting. Overall, I interpret my results on the role of classroom networks as a lower bound for the *total* effect of peer networks on subsequent employment in community colleges.

The key identifying assumption underlying this design is that while selection into courses may be correlated with factors affecting job finding, enrollment in a given section within a course does not suffer from the same issue. In this setting, students are not randomly assigned to sections of a course and are able to register for sections themselves, subject to scheduling and capacity constraints. While it is likely that students are somewhat constrained in their choices due to commitments such as other classes or work, there remains the concern that students may be sorting into sections of a course non-randomly in ways that affect their propensity to get a job at their classmates' firms. To test the validity of the identifying assumption, I conduct several robustness checks. First, conditional on course enrollment, I find minimal sorting into sections of a course on observable characteristics. Furthermore, the degree of residual sorting on observables does not predict a higher propensity for a student to get a job at a classmate's firm. Next, I estimate results on subsamples of courses in which various sorting concerns are less likely to be an issue. Third, I augment the main specification with individual fixed effects, which controls for sorting on certain types of unobservable characteristics, to the degree that individuals who tend to sort on these characteristics also tend to end up in the same section of a course. Finally, I present evidence that results are not being driven by students signing up for sections with pre-existing network contacts.

Additionally, I find heterogeneity in network effects by gender, course performance, course type, and college location. First, results indicate that women are significantly more likely to obtain jobs through classmate networks than men, and this difference is driven by gender differences in the propensity to obtain jobs through female classmates. Men and women do not differ in their overall propensity to obtain jobs through male classmate networks. However, women are significantly more likely to find jobs through female peers than men are. Second, I find no significant overall difference in the propensity for students to get a job through a classmate by academic performance. However, I do find positive assortative matching by course performance—that is, high-performing students are significantly more likely to get jobs through high-performing peers, and low-performing students are more likely to get jobs through low-performing peers. Third, I find that classmate networks play a more important role for job finding in vocational courses than in liberal arts (i.e., general study) courses. Finally, I find that students attending community colleges in more rural areas are significantly more likely to make use of classmate networks in job finding than counterparts in urban areas.

This study relates to a large body of work on peer effects in education and contributes to growing research identifying job networks in a variety of social settings.[2] Prior research on education networks has overwhelmingly focused on the role of networks at elite institutions. Kramarz and Thesmar (2013) show that French CEOs from elite colleges are more likely to hire board members from the same institution, and Zimmerman (2019) finds similar results in management hiring patterns

[2]Residential neighbors: Topa (2001), Bayer, Ross, and Topa (2008), and Hellerstein, McInerney, and Neumark (2011). Former coworkers: Cingano and Rosolia (2012) and Hensvik and Skans (2016). Ethnic groups: Dustmann, Glitz, Schönberg, and Brücker (2016) and Beaman (2012). Family members: Kramarz and Skans (2014).

for graduates from the same degree cohort in elite Chilean schools. Marmaros and Sacerdote (2002) find that students at Dartmouth College use alumni and members of their fraternity or sorority for job finding after graduation. Michelman, Price, and Zimmerman (2022) study exclusive campus clubs at Harvard University in the early 1900s. They find high socioeconomic returns to joining exclusive campus clubs, whose membership is dominated by students from prestigious private high schools. These studies provide important information to our understanding of social networks in higher education and reinforce the importance of the "old boys' network" in helping people at elite schools get jobs.

However, findings in these studies may not be applicable to the broader student population. Although much of the rhetoric and research on education networks focuses on elite settings, these schools educate a very small fraction of the population. Most students do not attend highly selective institutions with peers from privileged and connected backgrounds. Given that students who attend community colleges typically come from different social settings and work in a different part of the labor market than their elite college peers, it is unclear whether we should expect social networks to play the same role in job finding at these institutions. Moreover, empirical strategies used to identify network effects at elite institutions are often not applicable in non-elite settings. For example, Zimmerman (2019) tracks social networks through year- and degree program-specific cohorts, but students from community colleges pursue a diverse array of education tracks and are thus not in well-defined cohorts. Kramarz and Thesmar (2013) focus on social networks in the boardroom using data from the Paris stock exchange, tracing networks through shared university and civil service backgrounds. However, unlike peers from elite colleges, community college students are not well-represented in high-profile positions that are readily tractable using public data. Marmaros and Sacerdote (2002) and Michelman et al. (2022) measure network relationships through campus residence and club membership, but community college students typically do not live on campus or participate in Greek life or other exclusive campus clubs. These aspects of two-year colleges pose challenges to using cohorts, roommates, club membership, or public corporate data for identification that prior studies have done.

This study contributes to our understanding of the role of social networks in community colleges. To my knowledge, this paper provides the first empirical examination of the importance of classmate connections for job finding at community colleges and uses a novel source of variation to identify networks at "non-elite" institutions. Two-year college students differ from four-year counterparts in a number of ways in terms of both student composition and subsequent labor market outcomes, which could lead to peer networks playing a very different role in this setting. Two-year college students are more likely to be nontraditional students compared to four-year counterparts: they are significantly older on average when entering college, less likely to be financial dependents on parents, and more likely to have a job while in school (Ma and Baum, 2016). Furthermore, Census data indicate young two-year college graduates are significantly less likely to move states or move to different areas within a state compared to young four-year college graduates, indicating ties to local labor markets may be especially important for these students.[3]

1 Data and Empirical Setting

[3]I measure migration patterns using the 2000 Census 5% sample IPUMS, comparing migration propensity from five years ago for two-year college graduates and four-year college graduates who are between the ages of 22 and 32 (Ruggles, Flood, Goeken, Schouweiler, and Sobek, 2022).

1.1 Data

Data for this paper come from the Arkansas Department of Higher Education. This dataset includes enrollment records for all students who attended a public college in Arkansas between academic years 2004 and 2012.[4] For each student, I observe information on background characteristics including gender, part-time or full-time attendance status, high school information, and college degree attainment. Transcript records also provide information on the classes students take, instructor information, and credits earned for each class.

I link students to matched employer-employee labor market records using data collected from Arkansas Unemployment Insurance records.[5] These records contain quarterly observations on all individuals working for Arkansas from 2001-2011, minus uncovered sectors.[6] The panel allows me to track students over time through a firm establishment identifier number, as well as a six-digit industry code of the firm.[7] If a student works multiple jobs in a quarter, the records report the job with the highest earnings in the quarter. If an individual does not appear in the labor market files in a given quarter, I am unable to disentangle whether she is unemployed, employed outside of Arkansas, or employed in an uncovered sector.

This study focuses on two-year college students. Arkansas has a total of 22 public two-year colleges, and during this time period, 44 percent of students enrolled in a public college in Arkansas attend a two-year school, which is similar to the nation-wide share of college students enrolled in community colleges, 42 percent (Ma and Baum 2016). I do not include four-year college students in this analysis because the research design is uniquely suited for a community college setting for

[4] Academic year 2004 denotes semesters Fall 2003-Spring 2004 and so on for subsequent academic years.

[5] The information in these data are comparable to that found in the Longitudinal Employer-Household Dynamics (LEHD) dataset, although I only observe records for individuals working in Arkansas.

[6] Individuals working in uncovered sectors in this data line constitute a very small portion of employees in the state. These include self-employed individuals and federal government employees.

[7] Industries are classified using North American Industry Classification System (NAICS) codes.

multiple reasons. First, I focus on classmate networks in this study, and classmates represent a relatively more prominent source of student interaction at two-year colleges. Four-year colleges often have a much stronger presence of potentially confounding sources of student networking such as residential dorms, Greek life, and on-campus clubs and organizations. Second, two-year college students are more likely to search for jobs in a local labor market, making them more tractable in my data set, as well as potentially increasing the salience of peer networks. Third, two-year colleges have smaller classes on average than four-year colleges and typically do not involve large lecture classes: mean class size for two-year colleges in this dataset is 17 students, and fewer than five percent of classes contain more than 30 students. This is important for the analysis because different sections of a course take place in separate classrooms in this setting, as opposed to representing discussion sections within a larger lecture class.

Table 1 describes the composition of students in the sample. From 2004-2012, I observe over 900,000 student-by-semester observations. Approximately 64 percent of students in the two-year college system are female, similar to national trends.[8] Slightly over half of students enroll as part-time, and 62 percent of students are employed in a given term. The average student in the sample is 26 years old, with a median age of 23, and 95 percent of students are classified as in-state.[9] Approximately one quarter of enrollees are transfer students, and students take an average of 3.16 classes a semester.

[8]In 2012, 57 percent of two-year college students and 56 percent of four-year college students nation-wide were female (USDOE, 2017).

[9]Nationally, the average age of two-year college students is 28 years (Kolesnikova and Shimek (2008)).

1.2 Class Structure

This section describes the structure of classes in this setting. Figure 1 shows a diagram of how classes are organized. Within a school and semester, there are multiple courses offered on different topics, and within courses there are different class sections. A course consists of one or more sections that are taught to discrete sets of students but offer the same content and amount of credit. Course registration for a semester begins in the prior semester, and some schools ask students to meet with an advisor before registration to ensure students are fulfilling requirements to meet their academic goals. Students sign up for classes themselves and are able to choose the section of a course in which they enroll, subject to space availability and compatibility with the student's schedule. I refer to a student's classmates in this setting as peers from the same section of a course, since these are the peers with whom a student shares a classroom setting.

The empirical strategy exploits variation in section enrollment within courses to identify network effects. Accordingly, I make two sample restrictions: First, I limit the sample to students in courses containing multiple sections. Second, I restrict the sample to sections in a course taught by the same instructor to address the concern that instructors may affect job finding outcomes.[10] Approximately 42 percent of classes remain after these two restrictions are imposed.

Table 2 shows the characteristics of class sections in the sample. The mean GPA of students in a section is 2.62 (out of a four-point scale), and 57 percent of sections are taught by female instructors.[11] On average, there are 17 students per section. Sections with one student are dropped from the sample since students in these classes do not have classmates with whom they can network ($\leq$1

[10]For example, if an instructor knows employers at a certain firm and helps her students get jobs there, we would see in the data that students in a section taught by this instructor are more likely to work at this firm. However, this would not be due to a peer network effect.

[11]Note: GPA data are inconsistently reported across sections, and I only observe GPA for 66 percent of section observations.

percent of student-class observations). Figure A1 in the appendix shows the distribution of students per section. The modal class section has 21-25 students, and fewer than five percent of sections have more than 30 students. Table 2 also provides information on the subject classification of courses in the sample, using 2010 Classification of Instructor Program (CIP) codes. The most common subjects are Basic Skills and Remedial Education, Social Sciences, English, Business, Management, and Marketing, and Biology.

To understand how the analysis sample compares with the full sample of students and classes, I include descriptive tables comparing the two samples in the appendix. Table A1 juxtaposes characteristics of students in the restricted sample with characteristics in the original sample. Students in the analysis sample are very similar to the full sample of students both in terms of observable characteristics and the number of classes taken per semester. Table A2 compares characteristics of class sections in the analysis sample with the full sample. Sections in the analysis sample have a similar class size composition, mean GPA, and instructor gender composition as the full sample. The 14 most common subjects in the analysis sample make up 90 percent of the courses in the sample, and these same courses make up 86 percent of the courses in the full sample. Overall, Table A1 and A2 indicate the analysis sample is fairly representative of students and classes in Arkansas two-year colleges.

The final sample consists of 75,833 sections nested in 31,590 courses. In this sample, each course contains multiple sections, and each section in a course shares the same instructor. Figure 2 shows the distribution of sections per course. Approximately 68 percent of (instructor-specific) courses contain two classes, and less than one percent of courses contain more than five sections. Table 3 provides an overview of job finding outcomes with course peers and section peers in a semester within three years after the semester. Approximately 11 percent of students work at a firm where a peer from the same course (but different section) was incumbent within three years of a semester. In contrast, 10 percent of students obtain a job at a firm where a peer from the same section works during this time period. Notably, while students are similar in their propensity to get a job at a firm where a course peer compared to a section peer works, students on average have significantly more course peers in a given semester than they do section peers because some courses in the sample have more than two sections.[12] Students have an average of 68 course peers in a semester and only 43 section peers. On average, 2.15 percent of course peers are incumbent at a firm where a student gets hired sometime within three years of their first shared course. This number is 2.27 percent for section peers, a 6 percent increase from that of course peers.

1.3 Network Analysis: Data Set-up

To detect the use of classmate networks in job finding, I examine whether taking a class section with a peer increases the propensity for a student to get a job at the firm where their peer works. From the student-semester-level observations in the raw data, I construct a dyadic data set consisting of students, i, matched with peers, j. I define a student's peers as the set of students with whom a student shares at least one course. For each dyadic observation, I observe whether $\{i, j\}$ were former classmates for each course shared (i.e., whether they were in the same section), as well as whether i ends up working in a firm where j was incumbent at time of hire. All individuals are included in the sample, regardless of whether they graduate or start a new job. If one or both individuals in the pair do not appear in the unemployment insurance data, then mechanically i will never appear to work in a firm where j is incumbent in these situations.[13]

[12] For example, suppose a course has three sections and each section has the same number of students. In this situation, a student will have approximately twice as many course peers as she does section peers.

[13] To the extent that individuals who do not appear in the unemployment data are unemployed, this will not affect the interpretation of estimates. To the extent that individuals who do not appear in the unemployment data are working outside of Arkansas or working in uncovered sectors, I will potentially undercount the number of instances in which a

I restrict the pair-level analysis of network effects to focus on the first semester a pair shares courses since subsequent enrollment patterns may be influenced by network effects from social interactions in prior sections. Figure 3 shows the distribution of the number of courses a pair of students share in their first semester of sharing at least one course. Most student pairs, over 90 percent, only share one course. Note that matched pair observations are not interchangeable since order of hire matters: $\{i, j\} \neq \{j, i\}$, highlighting the inherent asymmetrical nature of network relationships in this context.

To give a descriptive sense of the role of classmates in job finding, Figure 4 displays the probability that x years after their first course together, an individual works at a firm where her peer was incumbent at time of hire. The graph compares the propensity of i to get a job at a firm where j is incumbent for pairs $\{i, j\}$ who were peers in the same section of a course, compared to pairs who were in different sections of a course. If $\{i, j\}$ enrolled in multiple shared courses, I categorize the pair as former classmates if they enrolled in the same section for at least one of the shared courses. The x-axis denotes the number of years since i and j first shared a course, and the y-axis denotes the probability that i works at a firm where j was working at the time of i's hire. I observe pairs up to six years after their first course together in the data.

For all years of analysis, Figure 4 indicates a higher proportion of students start working at a given peer's firm when the pair shared at least one section together, compared with pairs with no shared sections. A two-sample test of proportions shows that these values are significantly different at a 95 percent confidence level for each year. The gaps between the lines suggest that the probability of working at a peer's firm is slightly higher if the peer was a former classmate, compared to if the

student is working at a firm where their peer is incumbent. Intuitively, this is because the analysis counts all instances of pairs who work out of state as not working at the same firm, regardless of whether they do or not. However, if pairs who are in the same section are more likely to be working at the same firm than pairs who are not in the same section of a course, this will lead to an underestimate of true network effects. Unfortunately, the data do not allow me to distinguish unemployed individuals from those who are employed out of state or employed in uncovered sectors.

peer was a former course-mate who was not in the same section as the student.

While patterns in Figure 4 are suggestive that classroom networks may play a role in job finding, the values in this graph should not be interpreted as causal for a couple of reasons. First, this figure does not control for what course(s) the pair enrolls in. The propensity for a pair of students in each course to end up that the same firm, independent of network effects, may vary across courses. For example, suppose a given location has only one healthcare firm (a hospital), which hires all healthcare workers in the area, and there are 10 accounting firms in this same area that hire business professionals. If students who take health classes tend to go on to work in health professions and students who take business classes are more likely to go into business professions, this means students in health classes are mechanically going to be more likely to sort into the same firm than students in business classes, absent any social network effects. Given that the number of students per section and number of sections per course are not evenly distributed across sections and courses, respectively, this graph cannot disentangle network effects from baseline differences across courses in job finding at the same firm. Second, Figure 4 does not allow for effects to vary based on the number of courses and/or classes within these courses that students share. This obscures the effect of a marginal increase in an additional shared section on the propensity for a student to get a job at a firm where their peer works. In the next section, I describe the empirical strategy, which addresses these limitations of the descriptive analysis to provide causal estimates of the effect of sharing an additional class with a peer.

2 Empirical Strategy

To identify the effects of classmate networks on job finding, I estimate a pair-level regression. This approach is most similar to that of Bayer et al. (2008), which assesses the probability for two individuals to work at the same location based on residential proximity. I estimate the following model:

$$F_{ijc} = \rho_c + \gamma N_{ij} + \epsilon_{ijc} \tag{1}$$

where i and j represent two individuals who take a bundle of courses, c, together in a given semester. The outcome of interest, F_{ijc}, is an indicator variable equaling one if i works at a firm where j was incumbent when i was hired, and zero otherwise. In my analysis, I estimate Equation 1 for different time frames after the pair's last course together (whether i works at a firm where j was incumbent at time of hire one year after the course, two years after the course, three years after the course, etc.). To capture the effect of classroom interactions, N_{ij} measures the number of sections i and j shared from the bundle of courses c they took together in their first semester sharing courses.[14] Courses are by definition year- and semester-specific, so course bundle fixed effects control for any time-specific shocks or trends.[15] The coefficient of interest, γ, captures the effect of taking an additional section together on the propensity for i to work at a firm where j is

[14] To illustrate, suppose i and j take one course c together and the time frame of interest for the outcome is two years after course c. An observation in which $\{N_{ij} = 1, F_{ijc} = 1\}$ indicates i and j are in the same section of course c and that two years after course c, i works at a firm where j was incumbent at the time i was hired. F_{ijc} only takes a value of one conditional on i obtaining the job in a period after course c occurred, so F_{ijc} takes a value of zero if i was already working at the firm before course c, regardless of whether j was incumbent at time of hire. An observation in which $\{N_{ij} = 1, F_{ijc} = 0\}$ indicates i and j are in the same section of course c and that two years after course c, i is not working at a firm where j was incumbent at the time i was hired. This could encompass a number of situations: i is working at a different firm, i is not employed, or i did not change jobs after course c. $\{N_{ij} = 0, F_{ijc} = 1\}$ indicates i and j were in different sections of course c and that two years after course c, i is working at a firm where j was incumbent at the time i was hired. $\{N_{ij} = 0, F_{ijc} = 0\}$ indicates i and j were in different sections of course c and that two years after course c, i does not work at a firm where j was incumbent at the time i was hired.

[15] In other words, for two sections to be part of the same course, they must have the same course title and occur in the same year and term in the same school.

incumbent. A positive estimated value of γ would indicate classmates serve as a channel through which students are finding jobs.

One concern is that the correlation between the number of classes i and j take together and the propensity for i to get a job at a firm where j works may not reflect the causal effect of classroom interactions since students do not choose their courses randomly in this setting. Thus, other factors correlated with course selection could also affect job finding outcomes. First, since students select courses non-randomly, it may be that unobserved factors affecting course choice also affect the firms at which students seek employment. Second, some jobs may have certain degree or accreditation requirements, and consequently firms hiring in those jobs may disproportionately draw from courses associated with those degrees or programs. Finally, if certain programs or tracks of study have strong reputations or connections with certain firms, this could lead to a pipeline hiring effect in the associated courses. To control for these other factors correlated with course enrollment, I include a course bundle fixed effect, ρ_c, which represents the complete set of instructor-specific courses a pair takes together during their first semester sharing courses. This fixed effect restricts the comparisons of observation $\{i, j\}$ to the sets of pairs that took the same bundle of courses together in that semester as i and j. The course bundle fixed effect captures the baseline propensity for a student to start working at a firm where a peer with whom she shares a set of courses c works, independent of classroom interaction effects. Intuitively, identification comes from comparing the number of shared sections for a pair $\{i, j\}$ with the number of shared sections for other pairs of individuals who took the same set of courses together. Since most pairs only share one course in a semester, ρ_c acts as a single course fixed effect for most observations.

The key identifying assumption in this strategy is that while individuals may sort non-randomly into the courses they choose to take, they do not sort into sections within a course in a way that also affects subsequent firm placement. This assumption is motivated by a few considerations. First, within a course, each section has the same course title and course description and offers the same number and type of credits, which eliminates much of the heterogeneity that drives student selection across courses. Furthermore, I have restricted analysis to variation in section enrollment within courses taught by the same instructor. This accounts for selection into certain instructors, as well as instructor-specific job placement effects. Second, as indicated in Table 1, 62 percent of students hold jobs while in school and students take on average 3.16 classes per semester. Thus, even if students would like to select into sections, work or other academic obligations may place constraints on how much choice they have. Finally, since one limitation of the data is that I do not observe the time of day or day of week of a section in the data, I further augment Equation 1 by controlling for the attendance status (part-time vs. full-time) of both i and j. This accounts for the possibility that part-time students may be more constrained to courses that take place at certain times or days of the week due to work obligations.

Potential Threats to Identification

A causal interpretation of Equation 1 requires that students do not sort into sections within a course in ways that affect their propensity to end up working at the same firm. While the previous paragraph provides some intuitive motivation for why this may hold, it is important to formally test this assumption since students can select their sections, subject to scheduling and capacity constraints. Specifically, I assess two types of sorting: systematic and pairwise. First, students may sort systematically into different sections of a course along unobservable characteristics. If these unobservable characteristics also affect firm choice, this could upwardly bias estimates of network effects. Second, students may enroll in the same sections based on pre-existing friendships. If friends tend to share unobservable characteristics that make it more likely they will end up at the same firm, independent of network effects, this would also upwardly bias network estimates. Since the nature of these two types of sorting are distinct from each other, they require different assessments to test for them.

In Section 3.2, I conduct a number of empirical tests on the plausibility of the identification strategy against potential threats. First, I perform multiple tests to assess the concern of sorting on unobservables. I start by analyzing the correlation on observable characteristics between students and peers in their section, conditioning on overall course demographics. While this evaluation does not explicitly provide information on the amount of sorting on unobservables, Altonji, Elder, and Taber (2005) show the amount selection on observables to be proportionally informative of the amount of selection on unobservables. Minimal amounts of selection on observables within sections of a course group would provide support that results are not being driven by student sorting. I then conduct two direct tests on the role of sorting on observables to see if heterogeneity in observable characteristics across sections can explain a statistically significant portion of the results. Next, I provide further testing for sorting concerns by estimating network effects on subsamples of courses in which various sorting concerns are less likely to be an issue. This includes subsamples that display the least amount of within-course student sorting on observable school and course characteristics, a sample including only courses in which students did not have a choice over the instructor teaching in a given semester, and eliminating the restriction of isolating my analysis to sections within courses taught by the same instructor. Finally, I estimate an augmented version of Equation 1 using individual fixed effects, which addresses sorting on certain types of unobservables. Specifically, if some individuals tend to get jobs at the same firm with those they take a course with and these individuals sort themselves into certain sections, Equation 1 would over-state the true classmate network effect.

Second, I assess the concern that students may be sorting into classes with pre-existing network contacts in a way that significantly affects the outcome of interest. This type of sorting focuses on students selecting into class with one other person, or perhaps a small number of other people, so the tests above, which are aimed at assessing broad patterns of differential student composition across entire sections, may not pick up these smaller clusters of sorting. To address this concern, I proxy for pre-existing network contacts using geographic proximity between a pair and information on whether they worked together previously and assess whether sorting into sections with these contacts is drives findings.

3 Results

3.1 Main Results

Figure 5 displays estimation results from Equation 1. Coefficients are estimated separately for each year after the first semester in which a pair shares their first course. The x-axis denotes years after a $\{i, j\}$ share their first course. The outcome of interest is an indicator for whether x years after their first course together, individual i works at a firm where individual j was working at the time of i's hire. I observe individuals up to six years after their first shared course. Coefficient estimates have been multiplied by 100 to represent percentage points, and estimates include controls for the attendance status (part-time or full-time) of i and j. Following Cameron, Gelbach, and Miller (2011), standard errors are multi-way clustered by student i, student j, and course bundle, to account for correlation in the residuals across student pairs that occur with dyadic analysis. Estimates are displayed with 95 percent confidence intervals, and numbers above point estimates represent the percent increase of the effect from the baseline propensity for an individual to work at the firm of a peer who took the same set of courses. Baseline values of working together are calculated from the sample of student pairs who share at least one course but no sections with each other.

Results indicate taking an additional section with a peer significantly increases the propensity to work at a firm where the peer was incumbent at time of hire for each of the first six years after the first semester of sharing courses. For example, taking an additional section together increases the propensity for an individual to work at a firm where her peer is incumbent three years after the course by 0.0131 percentage points. This represents a 5.1 percent increase in the baseline propensity for a student to work at a firm where a peer who took the same courses works, which is 0.259 percent. These results are presented in table form with information on baseline values and sample sizes in Table A3 of the appendix.

Since estimates are positive and statistically significant for all years of analysis, I append multiple years in Table 4 for subsequent analyses. Specifically, I look at the effects of taking a section with a peer on the propensity of getting a job at a firm where the peer is incumbent sometime within the first three years after the first course taken together. This outcome is an indicator variable that takes a value of one if the individual gets a job at a firm where their peer is incumbent in the first, second, and/or third year after their first course together and zero otherwise. I choose this specification because while I observe students for up to six years in the data, I face a tradeoff between increasing the number of years I follow a pair and sample size, since I cannot observe later cohorts of pairs for as many years. I aggregate outcomes over three years to balance observing student outcomes over more years and losing sample size.

Table 4 presents results on the aggregate effects of classmate networks on job finding within three years of sharing a course. The outcome of interest is an indicator variable taking a value one if student i gets a job at the firm where peer j works sometime within three years of the first shared course and taking a value of zero otherwise. As before, coefficient estimates measure the effect of taking an additional section with a peer on the outcome, holding constant the bundle of courses the pair shares. Column (1) shows estimation results using a linear specification in number of sections, analogous to the specification in Equation 1. Column (2) incorporates a quadratic term in number of sections shared to capture non-linearities in sections shared on job finding.

Coefficient estimates are statistically significant and indicate taking an additional section with a peer has a positive effect on getting a job at a firm where a student's peer works within three years after the pair's first course together. The linear specification in column (1) indicates taking additional sections with a peer increases the propensity that the student gets a job at a firm where their peer works within three years of their last course together by 0.024 percentage points. The quadratic specification in column (2) finds a statistically significant estimate of 0.025 percentage points on the number of sections shared on the outcome of interest. The estimated coefficient on the quadratic term of number of sections shared is noisily estimated and not statistically significant. Given these results, I cannot reject the hypothesis that the effect of a one-unit change in number of sections shared is linear. One reason for the lack of precision in coefficient estimates of the quadratic term may be that the vast majority of pairs (over 90 percent) only share one course together, as shown in Figure 3. The baseline propensity for an individual to get at a job at a firm where a peer who took the same set of courses works is 0.634 percent. Thus, taking an additional section with a peer increases the propensity for a student to start working at the peer's firm within three years by 3.8 percent from the baseline.[16]

[16]To place this finding in the context of the literature, Kramarz and Skans (2014) find that having a parent at a firm increases the propensity that a college student will get a job at the firm by 12 percent, relative to their classmates without parents at the firm. Zimmerman (2019) finds that being in the same degree program cohort as a college peer in Chile increases the probability that a pair of students end up in leadership roles at the same firm by 126 percent. In Chile, students are admitted into a specific degree program for college and progress through college with their cohort. To put this in context for comparing estimates, the Zimmerman (2019) estimates encapsulate the cumulative effects of taking multiple classes together and extracurricular cohort networking effects throughout college, while findings in this paper isolate the effect of taking one class together. In a working paper, Fischer, Gorshkov, Sandoy, and Walldorf (2021) use a similar empirical design to this paper to examine the effects of being assigned to the same tutorial section as a peer in an elite Danish business school setting. During their first ten years after graduation, a pair of individuals in the same

While estimated effects of classroom exposure on job finding at a peer's firm are small in magnitude, they constitute a significant increase from the baseline probability of getting a job at a firm with an incumbent peer since baseline values are also very small. The reason for small baseline values is a function of the pair-level structure of the data: Each student has many classmates on average, who are spread out over many firms. Mechanically, this constrains the mean probability that for any given student pair observation, the student ends up working at her peer's firm. Thus, overall, findings indicate classmate networks play an economically and statistically significant role in helping students at two-year colleges find jobs.

Another way to think about the magnitude of estimates is to consider the overall effects of classroom networks on the propensity to get a job at a firm with at least one classmate. As mentioned above, taking a class with a given peer increases the propensity for a student to get a job at a firm where that specific peer works within three years of their first course together by 0.024 percentage points. However, students typically have several classmates in a class and take multiple courses in a semester. Specifically, in this setting, students take an average of 3.16 courses per semester and average class size is 17 students (meaning each student has an average of 16 peers in a class). This translates into an estimated network effect of a 1.32 percentage point increase in the propensity for a given student to get a job at a firm with at least one classmate over two years of college.[17] As shown in Table 3, at baseline, 11 percent of students work at a firm where a peer was incumbent at time of hire sometime within three years after a course. This means peer networks increase the propensity of

group is slightly more than 40 percent to be working in the same place than a pair of individuals in the same cohort but not the same tutorial group. Students are assigned to a tutorial group before the start of their first semester and stay with this group for the duration of their time in the three-year program, so these effects represent the cumulative effect of interactions over this period. In contrast, in my setting, I find that taking a single class together with a peer increases the propensity of getting a job at the peer's workplace within three years by 3.8 percent. This is a much smaller effect but also reflects the fact that I am capturing the effect of an interaction in a single class rather than the effect of group meetings that regularly take place over the course of three years.

[17]To derive these estimates, I perform a back of the envelope calculation in which, following Bayer et al. (2008), I assume the likelihood of working with each peer is an independent event. Thus, the estimate translates into a reported $.0132 = (1 - .00634)^{202.24} - [1 - (.00634 + .000241)]^{202.24}$.

a student working at a firm where at least one classmate was incumbent at time of hire by 12 percent.

3.2 Robustness

As detailed in Section 2, identification of network effects in this setting relies on the assumption that students do not sort into sections within a course in ways that affect their propensity to end up working at the same firm. I conduct a number of robustness checks to test the plausibility of this assumption, and this section provides an overview of these tests. A detailed discussion of the implementation and results of the robustness tests are in Appendix B.

A. Examining Sorting on Observables

To assess whether student sorting on unobservable characteristics into sections within a course influences estimated results, I first analyze the degree of correlation on observable characteristics between students and peers in their section, conditioning on overall course composition. While this evaluation does not explicitly provide information on the amount of sorting on unobservables, the degree of selection on observables is informative of the amount of selection on unobservables. Following Bayer et al. (2008), for each section in the sample, I randomly select one student and look at the degree of correlation on observable characteristics between the individual and peers in the same section, both conditional and unconditional on course fixed effects.

As expected, unconditional regressions indicate a significant degree of sorting across courses, as indicated by a high degree of correlation in observable characteristics between an individual and peers in her section. Reassuringly, these correlations are much smaller after conditioning for course fixed effects, although correlation values are still not identical to zero. Thus, I next analyze whether remaining sorting into sections along observable characteristics after conditioning for course fixed effects would predict a significantly higher probability of an individual getting a job at a firm with an incumbent same-section peer. The goal of this exercise is to understand whether differences in student composition on observables across sections explain any of the network effects measured in the main results. To do so, I extend Equation 1 to allow for heterogeneity along pair-level covariates to assess whether differences in observable characteristics across sections courses would lead to a higher predicted propensity for an individual to start working at the same firm as a same-section peer, compared to a different-section peer from the same course.

The results of this robustness test find that the predicted propensities for a student to get a job at the firm where their peer works based on sorting patterns along observable characteristics are very similar to those found in the main estimation, indicating results are not being driven by sorting on observable characteristics. However, it should be noted that the set of available controls is limited in important ways. While I observe several characteristics of students, I do not observe in the data why a pair of students sign up for the same section of a course and have limited information on other relational characteristics between a pair of students. Thus, selection on observables may be less informative about selection on unobservables in this setting, where outcomes are defined in terms of relationships between pairs of individuals. Thus, I next discuss alternative methods for exploring the robustness of my estimates.

B. Estimation Using Subsamples

I provide further testing for the robustness of results to sorting concerns by estimating network effects on alternative samples of courses in which various sorting concerns are less likely to be an issue. First, I restrict to subsamples that display the least amount of within-course student sorting on observable school and course characteristics, respectively. Second, I perform a robustness check in which I eliminate the restriction of isolating my analysis to sections within courses taught by the same instructor. Third, I perform a robustness check restricting my analysis to courses in which students did not have a choice over the instructor teaching in a given semester.

For the first robustness check, I run two analyses, one on the 50 percent of schools and one on the 50 percent of course subjects that display the lowest sorting across sections within a course. Since these sample restrictions significantly change the composition of the courses analyzed, I do not expect results to be identical to main results. However, if estimates still suggest the presence of network effects in courses that experience the least amount of sorting into sections on observables, this provides further support to the ability of the research design to isolate the role of social interactions in the job finding process.

In the second robustness check, I re-estimate my results after eliminating the restriction of looking at sections within courses taught by the same instructor. In the preferred specification, I restrict my analysis to within-instructor variation in section enrollment due to concerns that students may be selecting into certain instructors within a course on dimensions that are correlated with subsequent job outcomes, or that there may be instructor-specific effects influencing students' job outcomes. However, one limitation of this restriction is that this could exacerbate selection issues induced by scheduling constraints by ruling out students who take the same course at the same time of day since the same instructor cannot be in two places at once. Since I do not observe the time of day or day of week that a section occurs, as a robustness check, I estimate results on a sample without within-instructor restrictions, which will include pairs of students who are enrolled in sections of a course that occur contemporaneously.

The above robustness check assesses one form of sorting induced by scheduling constraints. Next, I implement another robustness check to address an alternative form of sorting that could bias results. Following the approach implemented in Fairlie, Hoffmann, and Oreopoulos (2014), I estimate Equation 1 using the sample of courses in which students do not have a choice over instructors in a given semester. This restriction addresses concerns that results are biased by student sorting into certain instructors, which could be a potential concern even when restricting my analysis to comparisons of section enrollment within courses taught by the same instructor.[18]

I find positive and significant estimated network under each of the above alternative sample specifications. These findings provide further support that results are not driven by sorting into sections within a course.

C. Specification with Individual Fixed Effects

One concern with the analyses focused on assessing sorting on observable characteristics is that they cannot speak directly to sorting on unobservable characteristics. To assess sorting on unobservable characteristics directly, I next extend Equation 1 to include individual fixed effects for each member of the pair. Since students appear multiple times in the data, the inclusion of individual fixed effects can be used to test for one type sorting on unobservables. Specifically, this analysis accounts for sorting in situations where (i) certain types of workers are more likely to work with those in the same section for unobserved reasons, and (ii) these workers sort themselves into similar sections within a course. For example, suppose some students are more likely to work at firms with their peers because they prefer flexible work hours, and firms that offer flexible work hours attract a lot of students. If these students tend to sort into the same sections, perhaps if they sort into sections held

[18]For example, suppose some students are only able to take a certain course in the morning due to scheduling constraints while other students are only able to take the course in the afternoon. Instructor B teaches a section of the course in the morning and Instructor A teaches a section of the course in both the morning and the afternoon. If students who are more similar to each other (and thus tend to sort into the same firms) tend to sort themselves differentially by instructors conditional on their preferred course timing, this would make it more likely that students in the morning section of Instructor A end up at the same firm as students in the afternoon section of Instructor A, even in the absence of network effects.

at less conventional times of day, Equation 1 would over-estimate the effects of classmate networks on job finding. The inclusion of individual fixed effects controls for this concern by capturing any inherent differences across individuals to work with peers with fixed effects. Results estimated using individual fixed effects produce very similar estimates to the original specification, providing further reassurance that results are not being driven by sorting on unobservable characteristics.

D. Examining the Role of Sorting on Pre-Existing Relationships

The previous robustness checks test for concerns regarding students sorting into sections along unobserved characteristics. In the final set of robustness checks, I test for a different type of sorting, namely whether students sort into sections with pre-existing friends in a manner that affects the outcome of interest. This behavior will bias estimates if friends have correlated characteristics that make it more likely they will end up at the same firm, independent of classroom interactions. If certain pairs of students are more likely to sign up in sections with friends, this effect would not necessarily be picked up by the previous robustness tests of sorting along unobservables if sets of friends sort together, but not systematically, along unobserved traits across sections of a course. Since I do not observe directly whether a pair of students share a pre-existing relationship, I proxy for this information by looking at geographic proximity between a pair and whether they worked together previously. Results of this analysis indicate results are not being driven by students sorting into sections within a course with peers based on pre-existing relationships.

3.3 Heterogeneity

In interpreting the main results, this study remains agnostic on the channel through which taking a class with a peer increases the propensity for a student to get a job at the peer's firm. Former classmates may help students obtain jobs through a variety of channels. First, classmates could be providing information to a student that lowers search costs, such as information on job openings or advice on how to interview successfully. Alternatively, classmates could help in providing a referral or recommendation for a student to employers. Third, a classmate could provide information on firm-specific match quality, which could generate an increased propensity for classmates to work at a firm if students tend to be similar with people they socialize with along relevant firm-specific match characteristics. Finally, it may be the case that students derive utility from working with their friends, holding all other aspects of the job and job search process constant.

These potential mechanisms highlight a number of ways through which classmates are potentially helpful in the job search process. They also suggest that not all classmates may be equally helpful. Next, I look at how network effects vary across different contexts. Specifically, I assess differences in effects of classmate networks in job finding by gender, academic course performance, course type, and school location. All results in this section look at the aggregate outcome of whether i gets a job at peer j's firm within three years of the pair's first shared course.

Gender

Table 5 examines network effects across different gender pairings of students i and j. The variable "Female" is an indicator for whether i is female, and "Peer Female" is an indicator for whether j is female. Column (1) looks at the overall differences in propensity for men and women to use classmate networks in job finding. I include the interaction of an indicator variable for female with the course bundle fixed effect to account for the possibility that women sort into courses with different baseline rates of job finding at peers' firms than men. This interaction also subsumes gender fixed effects. Results indicate that while both and women use classmate networks to find jobs, women are significantly more likely to find a job through a classmate. For men, sharing an additional section with a peer increases the propensity of getting a job at a firm where their peer is incumbent by 0.012 percentage points. For women, this effect is 0.021 percentage points larger than it is for men, indicating that taking a class with a peer increases the propensity for women to get a job at the peer's firm by almost twice the amount it does for men.

Columns (2) and (3) examine at the role of the gender of the student's peer, j, on networking. Column (2) restricts the sample to male students, and column (3) restricts the sample to female students. Taking a section with a male peer increases the propensity for both men and women to get a job at a firm where their peer is incumbent by similar amounts, 0.019 percentage points for men and 0.022 percentage points for women. However, men are significantly less likely to get a job through a female classmate, while women are more likely to do so. For men, taking an additional section with a female peer leads to a 0.016 percentage point lower propensity of getting a job at the peer's firm relative to taking a section with a male peer, indicating men rarely find jobs through female classmates. In contrast, for women, taking an additional section with a female peer leads to a 0.015 percentage point higher propensity of getting a job at the peer's firm relative to taking a section with a male peer.

The heterogeneity in network effects by gender in this setting is especially interesting in comparison to prior studies. Assortative matching, or homophily, in network formation has been explored theoretically in both economics and sociology (McPherson, Smith-Lovin, and Cook, 2001; Montgomery, 1991), and empirical research largely finds evidence of this as well. In terms of empirical work on gender and networks, Brown, Setren, and Topa (2016) study referrals at a large US corporation and find that the majority of referrals occur between individuals of the same gender. Marmaros and Sacerdote (2002) look at differences in the propensity for men and women to use various channels of networking, such as through fraternities and sororities, relatives, and professors. They find that men are significantly more likely to obtain a job through fraternity connections than women are to get jobs through sorority connections. Bayer et al. (2008) examine assortative matching in network formation by gender-marital status and find stronger interactions for married males compared to other gender-marital status groups. Kramarz and Skans (2014) assess parental networks in helping children find jobs after college. They find that father-son links are more important for obtaining a job at the parent's firm compared to father-daughter links. Mother-daughter links are less important than father-son links as well.

Consistent with prior findings, this study also finds evidence of homophily in network formation with men being relatively more likely to find jobs through male peers than female peers and women being relatively more likely to find jobs through female peers than male peers. Differing from prior studies, I find that job finding network effects are larger for women than men overall. Interestingly, these differences in network effects between men and women are driven by differences in the propensity for men and women to find jobs through female peers; men and women do not significantly differ in their propensity to obtain jobs through male peers. These differences could be driven by a number of mechanisms, including men being less likely to seek out jobs from female peers, women being less willing to refer a male peer to their employer, men being less likely to listen to advice from female peers on job application advice, or women being less likely to provide helpful advice on job search to male peers. While I am unable to disentangle these mechanisms in the context of this paper, these findings highlight how the role of gender in network formation can vary greatly across social contexts.

Course Performance

Table 6 assesses whether the propensity for students to use peers for job finding differs based on whether the student is academically high-performing or low-performing. The variable "Grade" denotes the z-score of the grade student i received in the section, and "Peer Grade" denotes the z-score of the grade for her peer j. I only observe grade information for about 66% of sections in the sample, so as a starting point, column (1) re-estimates the main specification from equation 1 on the sample of observations for which I observe a section grade for both i and j. I find that for this sample, taking an additional section with a peer increases the propensity for the individual to get a job at a firm where their peer works within three years of their first course together by .027 percentage points. This is very similar to the results when estimating this specification on the full sample in Table 4.

Next, column (2) assesses how the propensity for a student to get a job through their peer varies by the student's performance in their class section. I find no evidence of significant differences in overall network effects based on student i's grade in the class. Column (3) examines how the propensity for a student to get a job through her peer varies by the peer's performance in the class. I also find no evidence of significant differences in overall network effects based on student j's grade in the class. Column (4) looks at the interaction of student and peer performance on classmate network effects. Estimates of the interaction effect of student grade and peer grade are positive and statistically significant, suggesting students are relatively more likely to get jobs through peers who are of a similar academic caliber to themselves. In other words, I find evidence of assortative matching along academic performance in networking outcomes. Multiple channels could drive these effects. It may be that students display assortative matching in their socialization patterns with each other. Alternatively, it may be that students tend to gravitate towards firms along dimensions that are correlated with academic performance, so peers of the same academic caliber are more likely to have jobs that are a better fit.

Course Type

Community colleges offer a mix of vocational courses and general study, or liberal arts, courses. Table 7 looks at whether the propensity for students to use peers for job finding differs across course types. Table A4 in the appendix shows the breakdown of courses into vocational or liberal arts by course subject. I do not observe course subject information for a small number of courses in the sample, so as a starting point, column (1) re-estimates the main specification from equation 1 on the sample of observations for which I observe course subject information. I find that for this sample, taking an additional section with a peer increases the propensity for the individual to get a job at a firm where their peer is incumbent within three years of their first course together by .024 percentage points. This is very similar to the effect I find when estimating this specification on the full sample in Table 4.

Next, column (2) assesses whether there are differences in the propensity for a student to find a job through her peer in vocational compared to liberal arts classes. I find that for a pair of students in liberal arts classes, taking an additional section with a peer increases the propensity that the student will get a job at a firm where their peer is incumbent within three years of their last shared course by 0.019 percentage points. This effect is 0.015 percentage points larger for a pair of students in vocational courses. These findings indicate classmate networks play a bigger role in job finding for students in vocational classes compared to general interest courses, at least in the near future. One potential explanation of this may be that students in liberal arts classes are on average more focused on transferring to a four-year college than on finding a job. Another possibility is that students who take vocational courses tend to be more similar in their career interests and thus more useful to each other in the job search process.

School Location

Table 8 examines heterogeneity in classmate networks effects across urban and rural areas of the state. I first classify schools as being in metropolitan statistical areas, micro statistical areas, or neither, based on the Office of Management and Budget 2013 county-level classifications. Table A5 in the appendix shows the classification of individual schools.

Column (1) of table 8 augments the main specification in Equation 1 with interactions of number of sections shared with whether the pair attends a school located in a micropolitan or metropolitan area. The omitted category is schools that are not located in either type of region, i.e., schools in more rural counties. Results indicate the effect of taking an additional section with a peer for students in more rural counties is significantly higher than for students in schools located in micropolitan or metropolitan counties. Column (2), the preferred specification, includes interaction terms of number of sections with school size and school instructional classification as well.[19] Results in column (2) show similar patterns to those in column (1). At schools that are not in metropolitan or micropolitan areas, taking an additional section with a peer increases the propensity that a student will get a job at a firm where their peer is incumbent within three years of their first shared course by 0.088 percentage points. The effect of taking an additional section together on network effects is 0.038 percentage points lower at schools in micropolitan areas and 0.047 percentage points lower at schools in metropolitan areas. These findings in part reflect that base levels of co-working are higher in more rural areas, but these differences persist even in comparisons relative to baseline values: For

[19] Instructional classifications are obtained from The Carnegie Classification of Institutions of Higher Education, and community colleges in this setting fall into one of three categories: High Transfer, High Vocational and Technical, and Mixed Transfer/Vocational and Technical.

individuals in rural areas, taking a section with a peer increases the propensity of getting a job at the peer's firm by 12.4 percent of the baseline propensity for individuals in these areas to get jobs with peers. In contrast, these effects are 7.6 percent and 7.2 percent for individuals in micropolitan and metropolitan areas, respectively. These results suggest that classmate networks play an especially important role in the job finding process for community college students in rural areas. One reason for this may be that the labor market is thicker in urban areas, making social networks a relatively less important channel in those areas. Alternatively, it may be that the types of jobs that rely heavily on networking are disproportionately concentrated in rural areas. Finally, it may be that students in rural areas are less geographically mobile after college, making it more likely that they are searching for jobs in a local labor market where more classmates are working.

4 Conclusion and Discussion

Prior studies on social networks in college have predominantly focused on elite, four-year college settings. However, these elite institutions educate only a small fraction of students, and the majority of college students attend a "non-elite" institution. This study sheds novel insight on the role of social networks in job finding at community colleges, which educate over 40 percent of the college population in the US. On average, these students come from very different socioeconomic backgrounds and go into different types of jobs compared to peers at elite schools, so it is not obvious a priori if findings on the role of social networks at elite schools will be applicable to these settings. Furthermore, due to differences in institutional setting, existing methods used to measure networks at elite schools are typically not applicable to a community college setting, requiring the innovation of new methods for examining social networks in education.

Using transcript data from Arkansas community colleges linked to employer-employee records, this study investigates the effect on enrolling in a class section with a peer on the propensity or a student to later get a job at a firm where their peer works. To address the concern that students sort into sections in ways that may affect job finding outcomes, I examine whether a student is more likely to get a job at a firm where a classmate works, as opposed to a peer enrolled contemporaneously in a different section of the same course.

Results suggest classmate networks play a significant role in helping community college students find jobs. Taking a section with a peer increases the probability of a student getting a job at her peer's firm within three years of their first shared course by 3.8 percent. I conduct several robustness checks to address concerns that results are driven by students sorting non-randomly into sections along unobservable characteristics that would also affect the outcome of interest and find that results are robust to these checks.

Overall, my findings indicate social networks play an important role in job finding beyond the realm of highly selective, elite institutions. Specifically, I find that classmates play an important and nuanced role in job finding at community colleges. Since this paper focuses on network formation in class sections, results likely represent a lower bound for the total effect of peer networks on job finding outcomes in community colleges, as students likely also interact with each other outside of the classroom setting. From a policy perspective, one implication of these results is that it may be beneficial for community colleges to put resources toward encouraging and facilitating more opportunities for students to form networks, such as clubs and organizations, alumni groups, social spaces, etc. These findings also have implications for the evaluation of the returns to college. Ignoring the role of social networks yields an underestimate the true returns to attending college.

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Tables

Table 1: Student Characteristics

Female (%)	63.78
Part-time (%)	52.61
Employed (%)	62.30
In-State Student (%)	94.92
Transfer (%)	24.43
Age	26.23
	(8.77)
Number of Courses	3.16
	(1.70)
N	944,003

Arkansas two-year college students, 2004-2012. Observations denote student-semester units. Table displays means (standard deviations in parentheses). Age is imputed from year of high school graduation, assuming students are 18 at the end of high school. Median age is 23.

Table 2: Class Section Characteristics

Section Size	17.37
	(8.20)
Mean GPA	2.62
	(0.96)
Female Instructor (%)	56.81
Course Subject (%)	
Basic Skills and Remedial	19.2
Social Sciences	14.2
English	12.7
Business, Management, and Marketing	9.4
Biology	5.6
Mathematics and Statistics	5.1
Computer and Information Sciences	4.5
Health Professions	4.1
Visual and Performing Arts	4.0
Humanities	3.2
Physical Sciences	2.2
Communication, Journalism	2.1
Mechanic and Repair	1.9
Engineering	1.7
Other	10.1
N	75,833

Arkansas two-year college classes, 2004-2012. Units of observations are class sections. Table displays means (standard deviations in parentheses). Sections with one student have been dropped from the sample. Course subjects are categorized using 2010 Classification of Instructor Program (CIP) codes. Note: GPA data are inconsistently reported across sections, and I only observe GPA for 66 percent of the full sample of section observations.

Table 3: Labor Market Peer Characteristics (Within Three Years of Course)

Number of course peers	67.74
	(72.09)
Number of section peers	43.12
	(30.64)
Works with course peer	0.11
Works with section peer	0.10
Fraction of course peers at firm	0.0215
	(0.1379)
Fraction section peers at firm	0.0227
	(0.1329)
N	416,786

Arkansas two-year college students, 2004-2008. Observations denote student-semester units. Table displays means (standard deviations in parentheses). Job finding outcomes are calculated within the time frame of three years after the current semester. Job finding outcomes only include individuals enrolled from 2004-2008 since I do observe labor market data after 2011.

Table 4: Aggregate Effects of Sharing Additional Class Sections on Probability of Working at Peer's Firm

	(1)	(2)
Baseline	0.634	0.634
Number of Sections	0.0241***	0.0250***
	(0.00298)	(0.00384)
(Number of Sections)^2		-0.000323
		(0.000650)
Course Bundle FE	X	X
N	44,619,121	44,619,121

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors are multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes.

Table 5: Student Gender: Effects of Additional Section on Prob. of Working at Peer's Firm

	All	Male	Female
	(1)	(2)	(3)
Number of Sections	0.012***	0.019***	0.022***
	(0.004)	(0.005)	(0.006)
Number of Sections×Female	0.021***		
	(0.005)		
Number of Sections×Peer Female		-0.016**	0.015**
		(0.006)	(0.007)
Course Bundle FE	X	X	X
Female×Cse Bundle FE	X		
N	43,416,171	14,676,591	28,739,580

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. "Female" is an indicator for whether i is female, and "Peer Female" is an indicator for whether j is female. All specifications include corresponding student i and j gender controls that are un-interacted with number of shared sections.

Table 6: Course Performance: Effects of Additional Section on Prob. of Working at Peer's Firm

	(1)	(2)	(3)	(4)
Number of Sections	0.027***	0.027***	0.027***	0.028***
	(0.004)	(0.004)	(0.004)	(0.004)
Num. Sections×Grade		-0.001		-0.000
		(0.004)		(0.004)
Num. Sections×Peer Grade			0.005	0.004
			(0.003)	(0.003)
Num. Sections×Grade×Peer Grade				0.008**
				(0.004)
Course Bundle FE	X	X	X	X
N	20,068,676	20,068,676	20,068,676	20,068,676

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes."Grade" is the z-score of student i's grade, and "Peer Grade" is the z-score of student j's grade. All specifications include corresponding student i and j grade controls that are un-interacted with number of shared sections.

Table 7: Course Type: Effects of Additional Section on Prob. of Working at Peer's Firm

	(1)	(2)
Number of Sections	0.024***	0.019***
	(0.003)	(0.003)
Number of Sections×Vocational Course		0.015**
		(0.007)
Course Bundle FE	X	X
N	43,809,004	43,809,004

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Courses that are not vocational are classified as "liberal arts". Table A4 in the appendix contains a detailed classification of courses by vocational status.

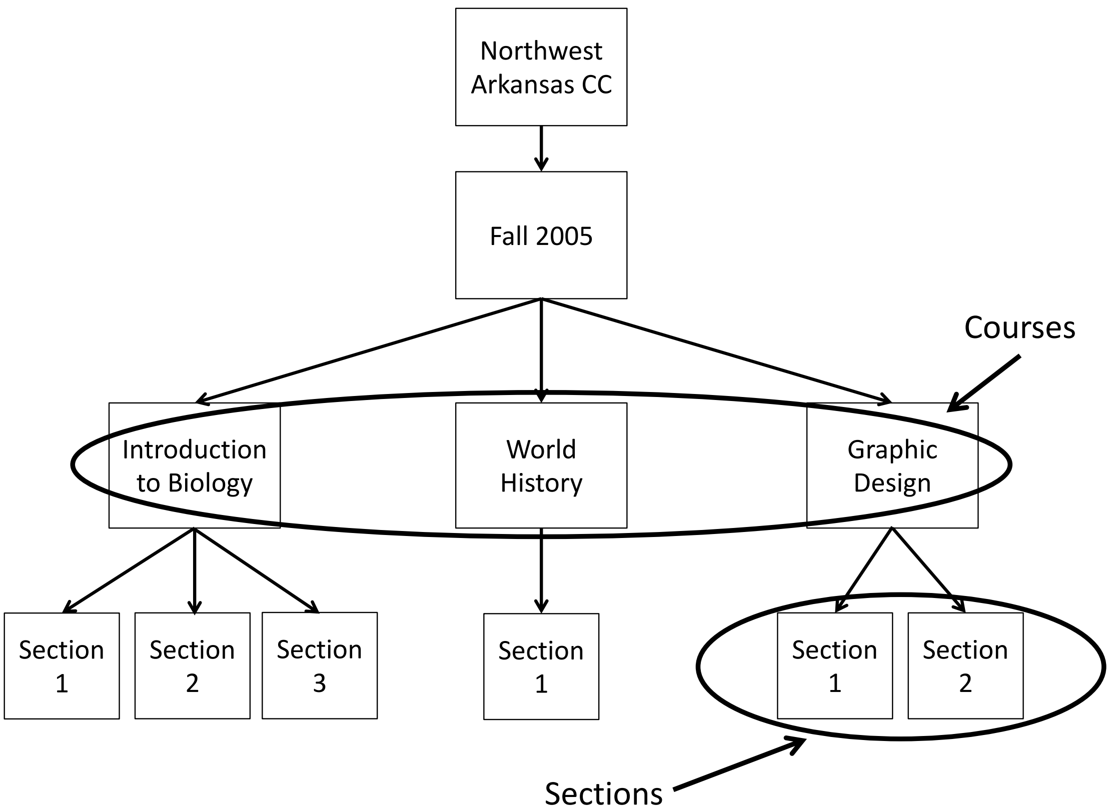
Table 8: Urbanicity: Effects of Additional Section on Prob. of Working at Peer's Firm

	(1)	(2)
Number of Sections	0.062***	0.088***
	(0.008)	(0.024)
Num. Sections×Micropolitan	-0.041***	-0.038***
	(0.009)	(0.009)
Num. Sections×Metropolitan	-0.046***	-0.047***
	(0.009)	(0.012)
Num Sections×School Enrollment		-0.000
		(0.000)
Num Sections×High Vocational+Technical		-0.025
		(0.021)
Num Sections×Mixed Transfer/Voc.+Tech.		-0.016
		(0.017)
Course Bundle FE	X	X
N	39,849,845	39,849,845

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. The baseline propensities for individuals to get a job at a firm where their peer works within three years of their first shared course across location types are: metropolitan=0.57 percentage points, micropolitan=0.66 percentage points, neither=0.71 percentage points

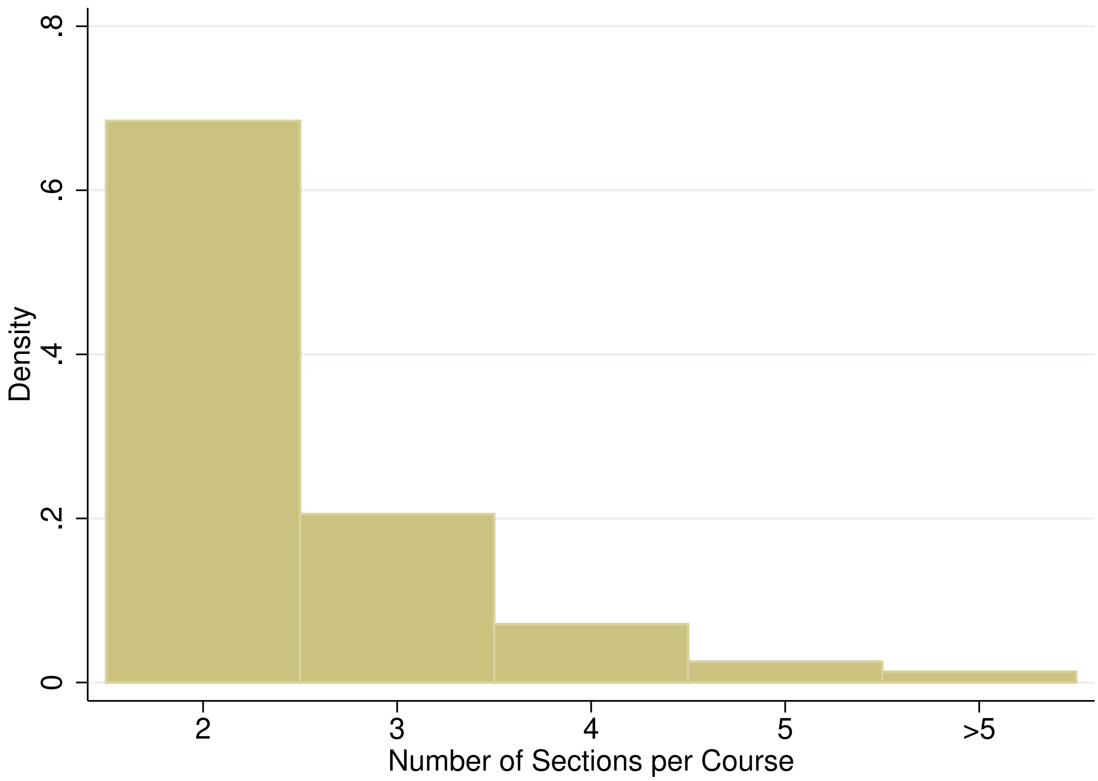
Figures

Figure 1: Structure of Classes



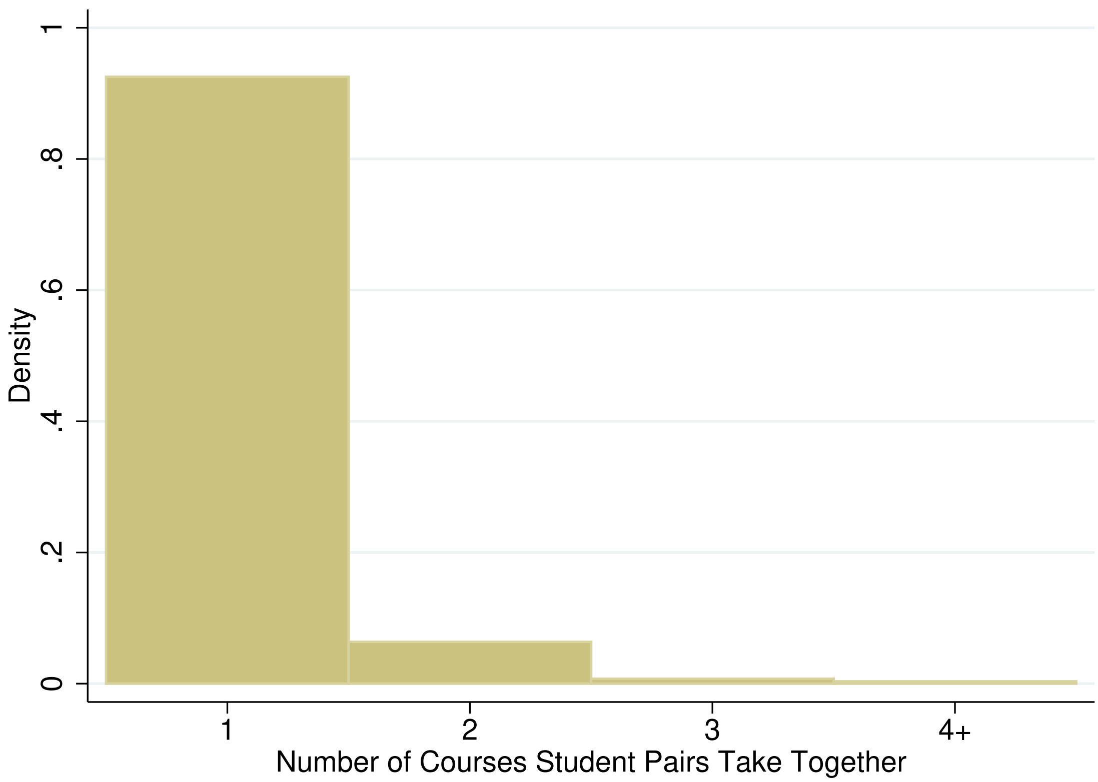
Note: Figure displays a simplified example of a school that offers three courses in a semester. Each course consists of a set of one or more sections that are taught to discrete sets of students but offer the same content and amount of credit.

Figure 2: Distribution of Sections per Course



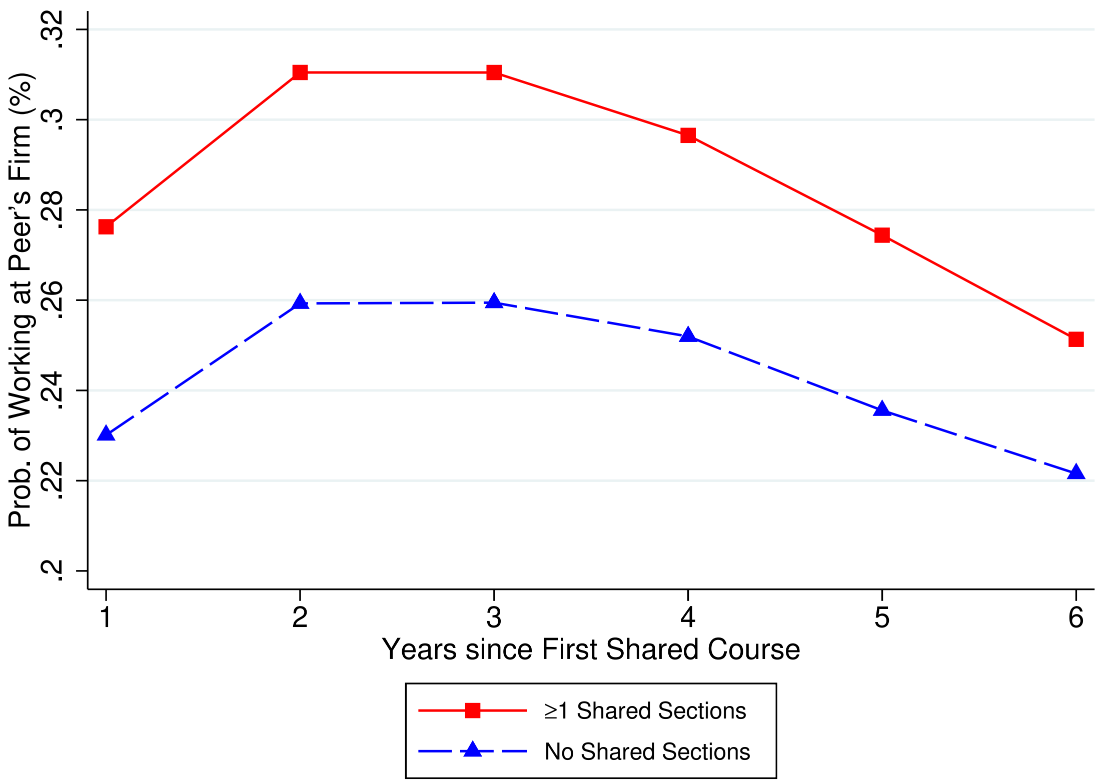
Note: Histogram shows the number of sections in each course of the final analytic sample of 31,590 courses.

Figure 3: Distribution of Number of Courses Student Pairs Share



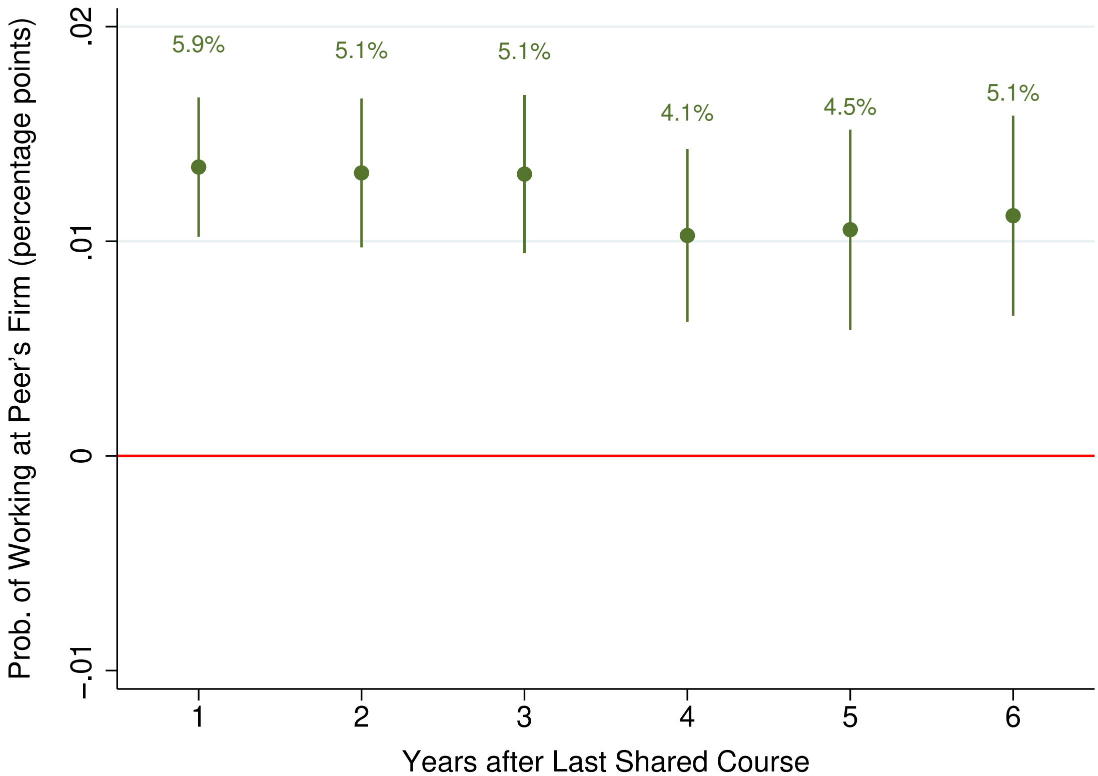
Note: Histogram displays number of courses a pair of students share in the first semester that they share at least one course together.

Figure 4: Probability of Working at Peer's Firm



Note: The x-axis displays number of years since the pair's first shared course together and the y-axis displays the propensity for an individual i to be working at a firm where peer j was incumbent at time of hire. These propensities are calculated separately for pairs who shared at least one section in the first semester they shared a course together compared to pairs who were exclusively in different sections of a shared course.

Figure 5: Effects of Sharing Additional Class Sections on Probability of Working at Peer's Firm

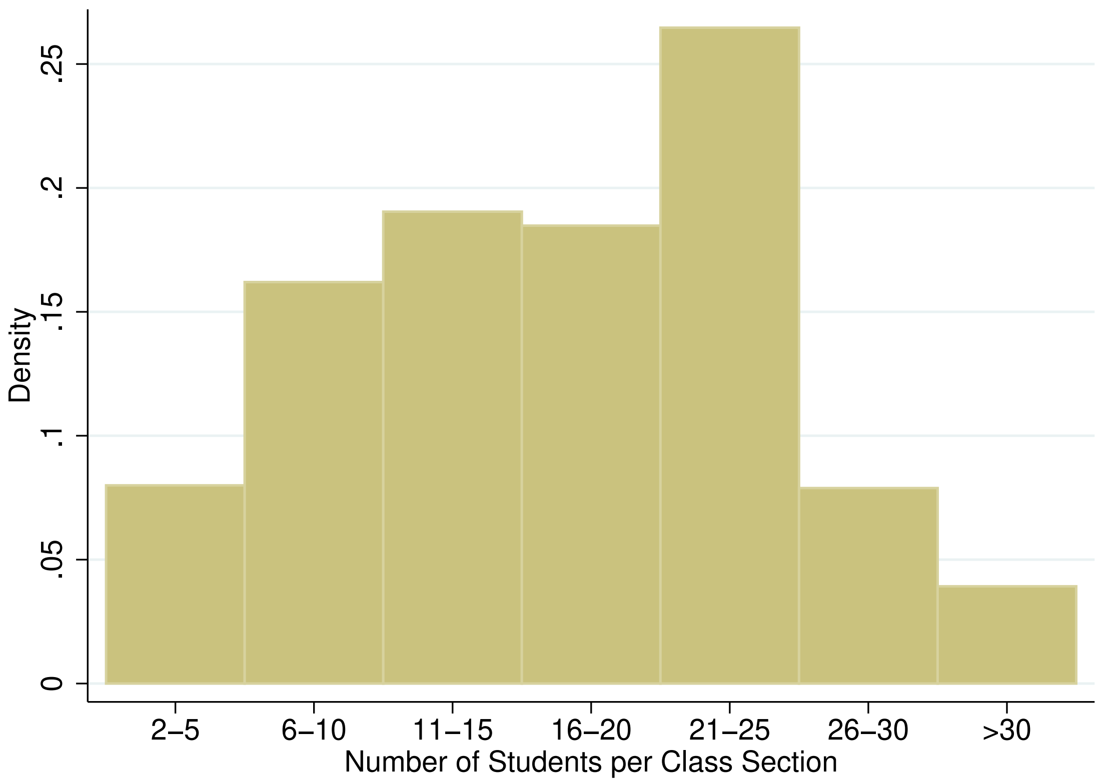


Note: Outcome is whether student i is working in a job where student j was incumbent at time of hire, x years after their first shared course. Coefficient estimates have been multiplied by 100 to reflect percentage changes, and standard errors are clustered by course bundle. Estimates are displayed with 95 percent confidence intervals, and standard errors are multi-way clustered by course bundle, student i, and student j. Numbers above point estimates represent the percent increase of the effect from baseline propensity for an individual to work at the firm of a peer who took the same set of courses. Results are displayed in table form with information on baseline values and sample sizes in Table A3 of the appendix. Estimates include controls for the attendance status (part-time or full-time) of i and j.

Appendix

A Additional Tables and Figures

Figure A1: Distribution of Students per Section



Note: Class sections with one student are dropped from the sample.

Table A1: Student Characteristics: Analysis Sample vs. All Students

	Analysis Sample (1)	All Students (2)
Female (%)	63.37	63.78
Part-time (%)	54.30	52.61
Employed (%)	61.42	62.30
In-State (%)	93.90	94.92
Transfer Student (%)	24.86	24.43
Age	26.87	26.23
	(9.07)	(8.77)
Number of Sections	3.09	3.16
	(1.82)	(1.70)
N	422,022	944,003

Arkansas two-year college students, 2004-2012. Observations denote student-semester units. Table displays means (standard deviations in parentheses). Age is imputed from year of high school graduation, assuming students are 18 at the end of high school. Column (1) represents observations that appear in the analysis sample for this paper. Column (2) represents observations of all students from Table 1.

Table A2: Class Section Characteristics: Analysis Sample vs. All Classes

	Analysis Sample (1)	All Classes (2)
Class Section Size	17.37	15.74
	(8.20)	(8.42)
Mean GPA	2.62	2.80
	(0.96)	(0.95)
Female Instructor (%)	57	55
Class Subject (%)		
Basic Skills and Remedial	19.2	14.3
Social Sciences	14.2	12.4
English	12.7	9.8
Business, Management, and Marketing	9.4	11.4
Biology	5.6	4.5
Mathematics and Statistics	5.1	4.6
Computer and Information Sciences	4.5	4.2
Health Professions	4.1	9.0
Visual and Performing Arts	4.0	4.2
Humanities	3.2	2.9
Physical Sciences	2.2	2.4
Communication, Journalism	2.1	1.2
Mechanic and Repair	1.9	2.8
Engineering	1.7	2.6
Other	10.1	13.7
Observations	75,833	181,413

Arkansas two-year college class sections, 2004-2012. Units of observations are class sections. Table displays means (standard deviations in parentheses). Sections with one student have been dropped from the sample. Course subjects are categorized using 2010 Classification of Instructor Program (CIP) codes. Column (1) represents class sections that appear in the analysis sample for this paper from Table 2. Column (2) represents observations of all class sections.

Table A3: Effects of Sharing Additional Class Section on Probability of Finding Job at Peer's Firm (Year-by-year Breakdown)

	+1yr	+2yrs	+3yrs	+4yrs	+5yrs	+6yrs
Baseline	0.230	0.259	0.259	0.252	0.236	0.222
Number of Sections	0.0135***	0.0132***	0.0131***	0.0103***	0.0105***	0.0112***
	(0.00166)	(0.00177)	(0.00188)	(0.00205)	(0.00238)	(0.00238)
Course Bundle FE	X	X	X	X	X	X
N	59,192,230	52,294,258	43,748,074	34,621,106	26,337,746	18,624,800

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student starts working at firm where peer is incumbent x years after their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes.

Table A4: Course Subject Classification

Liberal Arts	Social Sciences
	Basic Skills
	English Language and Literature
	Biological and Biomedical Sciences
	Mathematics and Statistics
	Computer and Information Sciences
	Visual and Performing Arts
	Humanities
	Physical Sciences
	Liberal Arts and sciences
	Multi/Interdisciplinary Studies
Vocational	Business, Management, and Marketing
	Health Professions
	Homeland Security, Law Enforcement, and Service
	Mechanic and Repair Technologies
	Engineering Technologies/Technicians
	Education
	Family and Consumer Sciences
	Parks, Recreation, Leisure, and Fitness
	Communication and Journalism
	Leisure and Recreation Activities
	Precision Production
	Personal and. Culinary Services
	Transportation and Materials Moving
	Personal Awareness and Self-Improvement
	Legal Professions
	Agriculture
	Library Science
	Construction Trades
	Communications Technologies/Technicians
	Natural Resources and Conservation
	Public Administration and Social Services
	Interpersonal and Social Skills
	Military Technollogies
	Military Science, Leadership, and Operations
	Science Technologies/Technicians

Classification of course subjects into liberal arts and vocational categories. Course subjects are classified using 2010 Classification of Instructional Program (CIP) codes.

Table A5: School Location Classification

Metropolitan	National Park Community College Northwest Arkansas Community College Pulaski Technical College Mid-South Community College
Micropolitan	East Arkansas Community Colege Arkansas State University–Mountain Home Univ. of Ark. Community College at Batesville Southern Arkansas University Tech. North Arkansas College Arkansas State University–Beebe Arkansas Northeastern College College of the Ouachitas
Neither	Rich Mountain Community College Univ. of Ark. Community College at Morrilton Cossatot Community College Ozarka College Black River Technical College Univ. of Ark. Community College at Hope

Schools are classified based on the county in which the school is located. Classification are based on 2013 definitions of statistical areas from the Office of Management and Budget (OMB).

B Robustness Checks

A. Examining Sorting on Observables

I first assess the degree of correlation on observable characteristics between students and peers in their section, conditioning on overall course composition. While this evaluation does not explicitly provide information on the amount of sorting on unobservables, the degree of selection on observables is informative of the amount of selection on unobservables (Altonji et al., 2005). To implement this test, for each section in the sample, I randomly select one student. Next, for each observable characteristic of interest in the data, I calculate the mean value of the characteristic of the non-selected students in the section, in order to construct a measure of average section characteristics. I sample only one individual per section in order to ensure that estimates are not biased by the fact that each individual would significantly affect average group characteristics if multiple individuals per section were sampled.

Table B1 shows correlations along observable characteristics. Column 1 shows unconditional correlations, and column 2 shows correlations conditional on course fixed effects. To obtain values in column 2, I first regress both individual and section measures separately on course fixed effects and then report the correlation between the residuals. These findings isolate the portion of the overall correlation to sorting patterns across sections within a course. As expected, results in column 1 show a significant degree of sorting across courses. Reassuringly, column 2 shows that with the addition course fixed effects, correlation on observable characteristics falls considerably. For example, the correlation between whether an individual is female and the proportion of peers in her section who are female is .344 overall, but this value falls to .068 with the addition of course fixed effects. On average, the addition of course fixed effects reduces estimated correlations by 76 percent for observable characteristics, and the average correlation between individuals and peers across characteristics is .049.

Table B1: Correlation between Individual and Average Characteristics of Peers

	Unconditional (1)	Conditional on Course (2)
Female	0.344	0.068
Employed	0.331	0.072
In-State	0.282	0.028
Transfer Student	0.340	0.065
Age 18-21	0.230	0.066
Age 22-26	0.056	0.021
Age 27+	0.078	0.026

Values shown represent correlations between individual characteristics and corresponding mean group characteristics of other individuals in the same section. Column 1 shows unconditional correlations, and the column 2 shows correlations conditional on course fixed effects.

Although the correlations of characteristics in Table B1 are much smaller after conditioning for course fixed effects, these values are not identical to zero. Thus, I next assess whether remaining sorting into sections along observables after conditioning for course fixed effects would predict a significantly higher probability of an individual getting a job at a firm with an incumbent same-section peer. The intuition behind this exercise is to understand whether differences in student composition on observable characterstics across sections explain any of the network effects measured in the main results. To do so, I extend Equation 1 to allow for heterogeneity along pair-level covariates:

$$F_{ijc} = \rho_c + \beta_1' X_{ij} + (\gamma + \beta_2' X_{ij}) N_{ij} + \epsilon_{ijc} \tag{2}$$

where X_{ij} represents a vector of pair-level covariates describing i and j for each characteristic listed in Table B1. Covariates of i and j are interacted with one another for each characteristic.[20] In

[20]For example, with regard to gender, X_{ij} includes controls for $Female_i \times Female_j$, $Female_i \times Male_j$, and

this setting, $\beta_1'X$ measures how the baseline propensity for an individual to start working at a firm with a course peer with whom they share no sections varies based on observable characteristics for each attribute of i and j. I first estimate the coefficients in Equation 2. Next, I use these parameter estimates to test whether the correlation on observable characteristics across sections of a course would lead to a significantly higher predicted probability for a student to work at a firm where a peer with whom they shared a section works, compared to peers from the same course with no shared sections.[21] To do so, I take the parameter estimates $\hat{\beta}_1$, which were derived using the full sample, and compare the average value of $\hat{\beta}_1'X$ for pairs who were not in the same section with the average value of $\hat{\beta}_1'X$ for pairs who were in the same section. This comparison indicates whether differences in correlation on observable characteristics across sections of a course would lead to a higher predicted propensity for an individual to start working at the same firm as a same-section peer, as opposed to a different-section peer from the same course. If the estimated value of $\hat{\beta}_1'X$ is much higher for same-section pairs, this would suggest that sorting into sections on observables may play a significant role in driving the finding that taking a section with a peer leads to a significant increase in the propensity to get a job at the firm where the peer works.

I estimate Equation 2 separately for each year τ after the pair's first course together, $\tau \in \{1, 2, ..., 6\}$. Table B2 presents predicted propensities for a student to get a job at the firm where their peer works τ years after a course, for pairs not in the same section with pairs who were in the same section. Results indicate that for all years of analysis, the estimated values of $\hat{\beta}_1$ predict that the propensity to work at a firm where a peer is incumbent is very similar for pairs who shared a section and those who did not. For example, three years after the course, the predicted probability that an individual works

$Male_i \times Female_j$ (with $Male_i \times Male_j$ as the omitted category).

[21] I restrict this analysis to pairs who took one course together, which represents over 90 percent of pairs, in order to isolate predicted propensities due to variation in shared sections for pairs who shared the same number of courses.

at a firm where a peer she shared a section with was incumbent at time of hire and the probability of working at a firm where a peer she didn't share a section with was incumbent are both .290 percent. Findings in Table B2 provide support that the small remaining amount of sorting on observables within courses do not drive the increased propensity for an individual to get a job at a firm where a peer from the same section works compared to a firm where a peer from a different section of the same course works.

Table B2: Counterfactual Predicted Propensities

τ	Different Section	Same Section
+1 year	0.241	0.240
+2 years	0.282	0.282
+3 years	0.290	0.290
+4 years	0.290	0.290
+5 years	0.271	0.271
+6 years	0.259	0.259

Numbers in table represent the predicted propensity for an individual i to work at a firm where their peer j is incumbent, τ years after their first shared course. Estimates are reported as percentages.

B. Estimation Using Subsamples

For the first subsample robustness check, I run two analyses, one on the 50 percent of schools and one on the 50 percent of course subjects that display the lowest sorting across sections within a course. To do so, for each school (or course subject) in the sample, I analyze the correlation between individual and average characteristics of peers in her section, conditional on course, as in column (2) of Table B1. I then take the average correlation across all observable characteristics. I select the 50% of schools (or course subjects) with the lowest average correlations to use for the restricted subsample.

Next, I re-estimate Equation 1 using these two subsamples and assess whether results finding significant classmate network effects hold. Since these sample restrictions substantially change the composition of the courses analyzed, I do not expect results to be identical to main results in Figure 5 and Table 4. However, I find significant estimated network effects in courses that experience the least amount of sorting into sections on observables, this provides further support to the ability of the research design to isolate the role of social interactions in the job finding process.

Figures B1 and B2 display results of this analysis for the subsample of schools and course subjects with the lowest degrees of sorting based on observables, respectively. Results indicate that for all years of analysis, the estimated effects of taking an additional section with a peer on the propensity for a student to work in a job where their peer was incumbent at time of hire are positive and statistically significant. Moreover, they are similar in magnitude to the estimates from the main sample in Table A3, although estimates in Figures B1 and B2 have wider confidence bands due to decreased sample size. Results are presented in table form with baseline values in Tables B3 and B4. These tables also include aggregate outcome results over the first three years after a pair's first shared course.

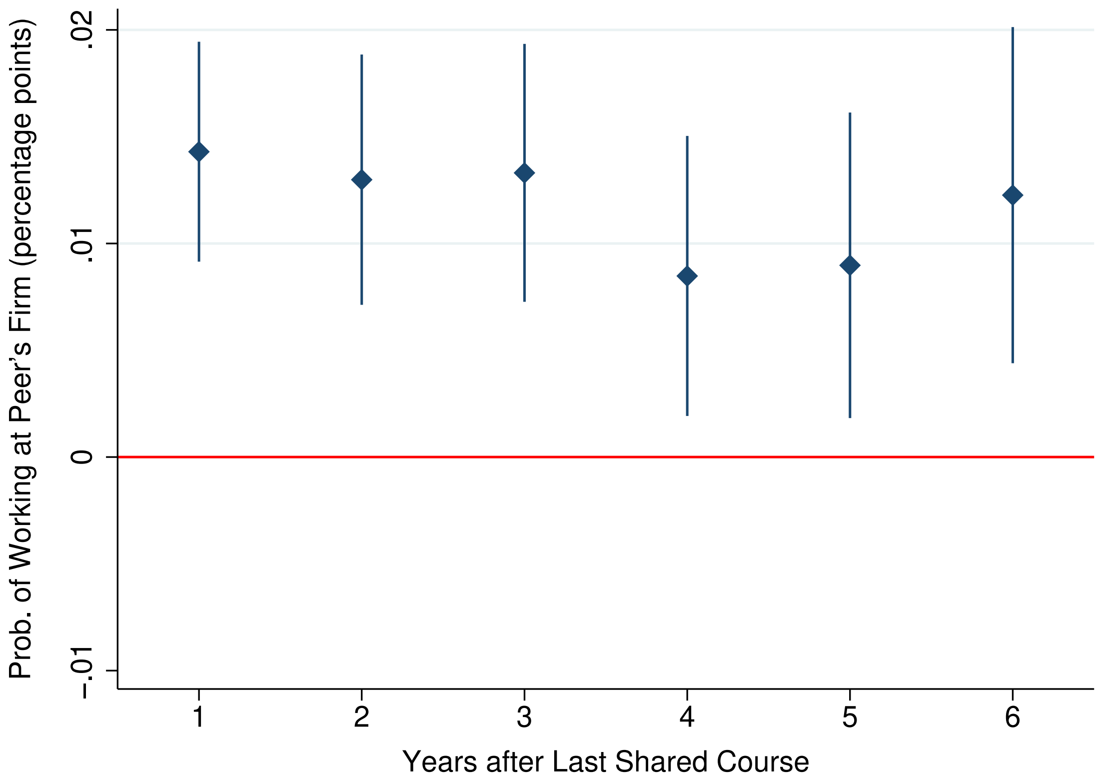
Table B3: Effects of Sharing Additional Class Section, Subsample 50% Schools with Lowest Sorting

	(1) Aggregate	(2) +1yr	(3) +2yrs	(4) +3yrs	(4) +4yrs	(6) +5yrs	(7) +6yrs
Baseline	0.573	0.208	0.242	0.240	0.241	0.232	0.219
Number of Sections	0.029***	0.014***	0.013***	0.013***	0.008**	0.009**	0.012***
	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Course Bundle FE	X	X	X	X	X	X	X
N	21,930,602	28,212,662	25,719,556	21,912,068	17,427,152	13,384,298	9,497,526

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Outcome variable in column (1) is whether student starts working at firm where peer is incumbent within three years of their first shared course, and columns (2)-(7) show year-by-year estimates.

Next, I re-estimate my results after eliminating the restriction of isolating my analysis to sections within courses taught by the same instructor. In the preferred specification, I restrict my analysis

Figure B1: Effects of Sharing Additional Class Section: Subsample of 50% of Schools with Lowest Amount of Sorting on Observables



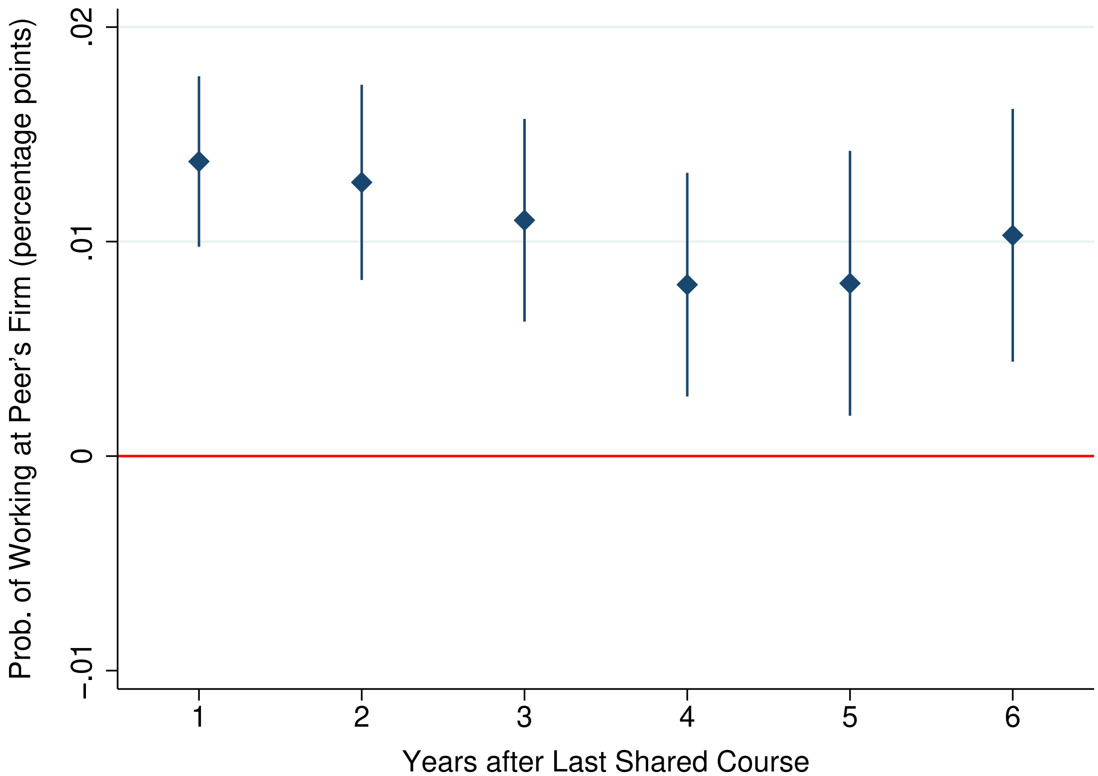
Note: Outcome is whether student i is working in a job where student j was incumbent at time of hire, x years after their first shared course. Coefficient estimates have been multiplied by 100 to reflect percentage changes. Estimates are displayed with 95 percent confidence intervals, and standard errors are multi-way clustered by course bundle, student i, and student j. Results are displayed in table format with information on baseline value and sample sizes in Table B3. Estimates include controls for the attendance status (part-time or full-time) of i and j

Table B4: Effects of Sharing Additional Class Section, Subsample 50% Course Subjects with Lowest Sorting

	(1) Aggregate	(2) +1yr	(3) +2yrs	(4) +3yrs	(4) +4yrs	(6) +5yrs	(7) +6yrs
Baseline	0.663	0.236	0.277	0.282	0.275	0.257	0.243
Number of Sections	0.020***	0.014***	0.013***	0.011***	0.008***	0.008**	0.010***
	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Course Bundle FE	X	X	X	X	X	X	X
N	23,447,710	31,367,690	27,789,966	23,426,624	18,672,868	14,344,494	10,243,348

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Outcome variable in column (1) is whether student starts working at firm where peer is incumbent within three years of their first shared course, and columns (2)-(7) show year-by-year estimates.

Figure B2: Effects of Sharing Additional Class Section: Subsample of 50% of Course Subjects with Lowest Amount of Sorting on Observables



Note: Outcome is whether student i is working in a job where student j was incumbent at time of hire, x years after their first shared course. Coefficient estimates have been multiplied by 100 to reflect percentage changes. Estimates are displayed with 95 percent confidence intervals, and standard errors are multi-way clustered by course bundle, student i, and student j. Results are displayed in table format with information on baseline value and sample sizes in Table B4. Estimates include controls for the attendance status (part-time or full-time) of i and j

to within-instructor variation in section enrollment due to concerns that students may be selecting into certain instructors within a course on dimensions that are correlated with subsequent job outcomes, or that there may be instructor-specific effects influencing students' job outcomes. However, one limitation of this restriction is that this could exacerbate selection issues induced by scheduling constraints by ruling out students who take the same course at the same time of day since the same instructor cannot be in two places at once. Since I do not observe the time of day or day of week that a section occurs, as a robustness check, I estimate results on a sample without within-instructor restrictions, which will include pairs of students who are enrolled in sections of a course that occur contemporaneously.

Table B5 displays results of this analysis. Results are estimated on a 25 percent random sample of pairs for computational ease since removing within-instructor restrictions significantly increases the sample size for the pair-level analysis. Using this sample, I find that sharing a class with a peer actually leads to significantly larger effects. In the linear specification, I find that each additional shared section increases the propensity that a student will get a job at her peer's firm within three years of their first shared course by 0.061 percentage points. This represents an 11.1 percent increase from the baseline propensity of getting a job at a peer's firm for this sample of 0.547 percent. Unfortunately, I am unable to separate what portion of these effects are due to students sorting into instructors or instructor-specific effects compared to removal of selection concerns induced by scheduling constraints. However, these results provide some reassurance that allowing for comparisons of students in sections occurring simultaneously does not reduce my estimated effects.

Table B5: Aggregate Effects of Sharing Additional Class Sections on Probability of Working at Peer's Firm (All instructors)

	(1)	(2)
Baseline	0.547	0.547
Number of Sections	0.0606***	0.0771***
	(0.00549)	(0.00547)
(Number of Sections)^2		-0.00618***
		(0.00136)
Course Bundle FE	X	X
N	39,640,828	39,640,828

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Sample includes courses for all instructors, not just instructors who taught multiple sections of a course, and observations are a 25% random sample of all matched pairs.

The above robustness check assesses one form of sorting that is induced by scheduling constraints. Another potential concern is sorting into instructors that could bias estimates even using a within-instructor approach due to scheduling constraints. I address this concern in the next robustness check. Following the approach implemented in Fairlie et al. (2014), I estimate Equation 1 using the sample of courses in which students do not have a choice over instructors in a given semester. This restriction addresses concerns that results are biased by student sorting into certain instructors, which could be a potential concern even when restricting my analysis to comparisons of section enrollment within courses taught by the same instructor. For example, suppose some students are only able to take a certain course in the morning due to scheduling constraints while other students are only able to take the course in the afternoon. Instructor B teaches a section of the course in the morning and Instructor A teaches a section of the course in both the morning and the afternoon. If students who are more similar to each other (and thus tend to sort into the same firms) tend to sort themselves differentially by instructors conditional on their preferred course timing, this would make it more likely that students in the morning section of Instructor A end up at the same firm as students in the afternoon section of Instructor A, even in the absence of network effects.

Results of this estimation are displayed in Table B6. Reassuringly, I find that sharing a class with a peer has a positive and significant effect on job finding. Specifically, each additional shared section increases the propensity that a student will get a job at her peer's firm within three years of their first shared course by 0.041 percentage points. This represents a 5.0 percent increase from the baseline propensity of getting a job at a peer's firm for this sample of 0.819 percent. This is actually a larger effect than the main estimation in Table 4 using the full sample, which finds a 3.8 percent increase.

C. Specification with Individual Fixed Effects

To analyze student sorting unobservable characteristics unobservables directly, I extend Equation

Table B6: Aggregate Effects of Sharing Additional Class Sections on Probability of Working at Peer's Firm (No Instructor Choice)

	(1)	(2)
Baseline	0.819	0.819
Number of Sections	0.0406***	0.0709***
	(0.00677)	(0.0105)
(Number of Sections)^2		-0.00462***
		(0.00109)
Course Bundle FE	X	X
N	6,899,822	6,899,822

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Outcome: whether student i gets a job at the firm where peer j works within three years of their first shared course. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Sample is restricted to courses for which students did not have a choice over instructors.

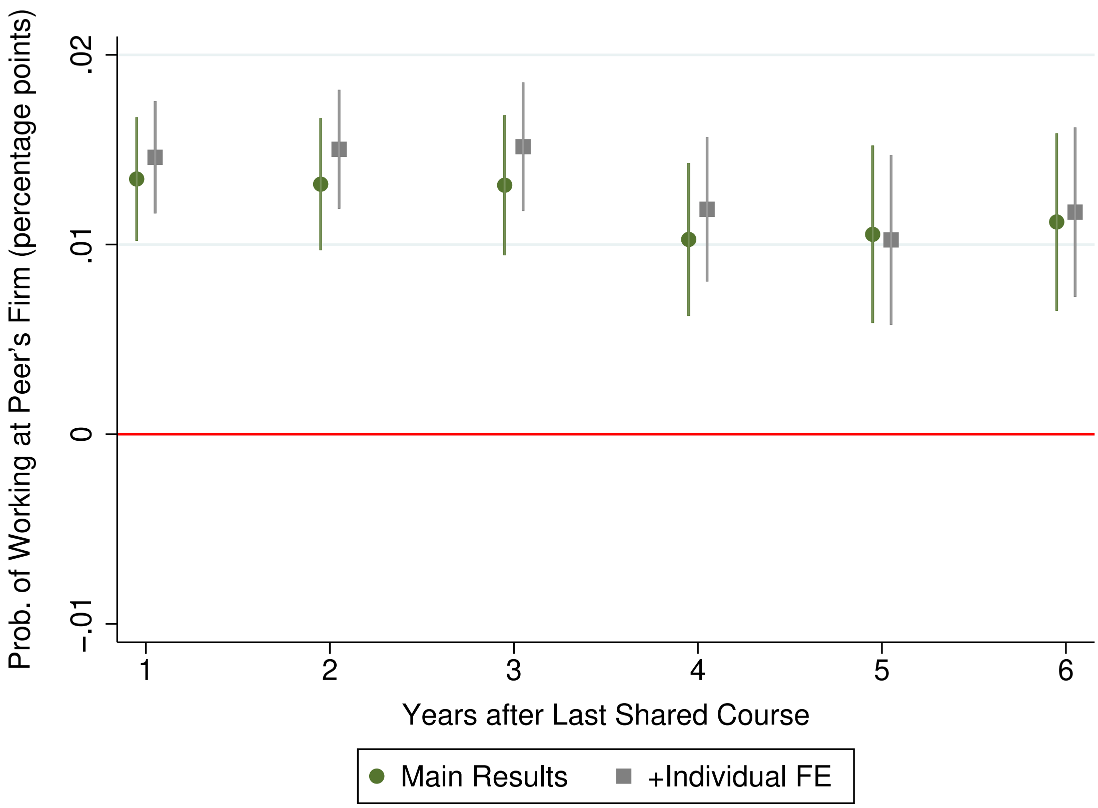
1 to include individual fixed effects for each member of the pair:

$$F_{ijc} = \tilde{\rho}_c + \tilde{\gamma} N_{ij} + \lambda_i + \lambda_j + \tilde{\epsilon}_{ijc} \tag{3}$$

where λ_i and λ_j represent individual fixed effects for i and j, respectively. Since students appear multiple times in the data, the inclusion of individual fixed effects can be used to test for one type sorting on unobservables. Specifically, this analysis accounts for sorting in situations where (i) certain types of workers are more likely to work with those in the same section for unobserved reasons, and (ii) these workers sort themselves into similar sections within a course. Equation 3 controls for this concern by capturing any inherent differences across individuals to work with peers with fixed effects.

Figure B3 shows estimated coefficients on the number of shared sections for Equation 3, compared to estimated coefficients for the primary specification in Equation 1. Results indicate that estimates including fixed effects produce very similar estimates to the original specification on the effect of taking an additional section together on the propensity for a student to work at a firm with an incumbent peer for any of the years of analysis. For five out of the first six years after the first shared course, the specification with individual fixed effects estimates network effects that are actually slightly *higher* than estimates from the main specification, although estimates with and without individual fixed effects are not significantly different for any years. All results are reported with 95% confidence bars, and individual fixed effects results are reported numerically in Table B7.

Figure B3: Effects of Sharing Additional Class Section: Adding Individual Fixed Effects



Note: Outcome is whether student i is working in a job where student j was incumbent at time of hire, x years after their first shared course. Coefficient estimates have been multiplied by 100 to reflect percentage changes. Estimates are displayed with 95 percent confidence intervals, and standard errors are multi-way clustered by course bundle, student i, and student j. Estimates include controls for the attendance status (part-time or full-time) of i and j. Results estimated using the augmented specification that includes individual fixed effects, Equation 3, are displayed in the appendix in Table B7.

Table B7: Effects of Sharing Additional Class Section, Specification with Individual Fixed Effects

	(1) Aggregate	(2) +1yr	(3) +2yrs	(4) +3yrs	(4) +4yrs	(6) +5yrs	(7) +6yrs
Baseline	0.634	0.230	0.259	0.259	0.252	0.236	0.222
Number of Sections	0.029***	0.015***	0.015***	0.015***	0.012***	0.010***	0.012***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Course Bundle FE	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X
N	44,611,609	59,944,330	53,061,894	44,576,624	35,423,320	27,099,464	19,348,024

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Outcome variable in column (1) is whether student starts working at firm where peer is incumbent within three years of their first shared course, and columns (2)-(7) show year-by-year estimates.

D. Examining Role of Sorting on Pre-Existing Relationships

This section assesses whether students sort into sections with pre-existing friends in a way that significantly affects the outcome of interest. Since I do not observe directly whether a pair of students share a pre-existing relationship, I proxy for this information by looking at geographic proximity between a pair and whether they worked together previously. I measure residential proximity of a pair by calculating the travel distance between their high schools of attendance, and I measure prior coworker status using an indicator for whether the pair worked at the same place at any time in the three years prior to their first course together. This proxy is created on the intuition that pairs who live closer together or who were prior coworkers are more likely to have established social ties before taking a course together. These metrics are motivated by prior research showing both residential proximity and former coworkers serve as significant channels for networking.[22]

To test whether sorting on pre-existing relationships drives outcomes, I first analyze whether students do in fact exhibit a higher propensity to enroll in the same section with peers who they

[22]See Topa (2001), Bayer et al. (2008), Hellerstein et al. (2011), Schmutte (2015), Cingano and Rosolia (2012), Hensvik and Skans (2016), and Glitz (2017).

worked with previously or with peers from a closer geographic proximity:

$$N_{ijc} = \varphi_c + \phi_1 Prior\_Cowork_{ij} + \phi_2 Dist_{ij} + \nu_{ijc} \tag{4}$$

where $Prior\_Cowork_{ij}$ is an indicator variable for that takes on a value of one if i and j worked at the same firm in the three years prior to taking their first course together and zero otherwise. The variable $Dist_{ij}$ measures the travel distance in miles between the pair's high schools. The outcome of interest, N_{ijc}, which acts as the independent variable in Equation 1, represents the number of sections i and j share, and φ_c is a course bundle fixed effect. The coefficients ϕ_1 and ϕ_2 measure whether pairs who worked together previously or live closer enroll in more sections together, conditional on the courses in which they enroll.

Table B8 shows estimation results for Equation 4. Column (1) displays unconditional results without course bundle fixed effects, and column (2) includes course bundle fixed effects. In column (1), estimates indicate a sizable positive correlation for the propensity of prior coworkers to enroll in a class section together. The coefficient on travel distance is positive, indicating pairs who are further away from each other are more likely to enroll in a section together. The inclusion of course bundle fixed effects reduces the magnitude of coefficient estimates on previous coworkers considerably, and the coefficient on travel distance becomes negative. Results from column (2) indicate pairs who were prior coworkers take approximately .034 more sections together, compared to pairs who did not work together before taking courses. An increase in the travel distance between pairs by one standard deviation decreases the number of sections individuals share by .006 sections. These findings suggest students display an increased propensity to enroll in sections with those with whom they were connected to previously, as measured by working together and geographic proximity.

Table B8: Effects of Prior Coworkers and Travel Distance on Number of Class Sections Together

	(1)	(2)
Previous Coworker	0.102***	0.034***
	(0.006)	(0.001)
Travel Distance	0.004**	-0.006***
	(0.002)	(0.001)
Course FE		X
N	39,432,852	39,432,852

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. "Travel Distance" captures the distance between the high school attended by student i and student j is measured using z-scores. "Previous Coworker" is an indicator variable that takes a value of one if i and j worked at the same firm in the three years leading up to their first shared course and zero otherwise. Outcome variable is number of sections pair i and j share.

Since students display an increased propensity to enroll in sections with peers they worked with or lived closer to, a concern to identification is that unobservable traits that lead people to sign up for the same section may also affect their propensity to work at the same firm, independent of network effects. In this situation, the coefficient estimate of γ in Equation 1 may overestimate the true effect of taking an additional section with a peer on the propensity of getting a job at the firm where a peer works.[23] To assess this concern, I augment Equation 1 to include controls for prior coworkers and travel distance:

$$F_{ijc} = \check{\rho}_c + \check{\gamma} N_{ij} + \phi_1 Prior\_Cowork_{ij} + \phi_2 Dist_{ij} + \check{\epsilon}_{ijc} \tag{5}$$

where F_{ijc} is an indicator variable taking a value of one if i works at a firm where j was working when i was hired, and zero otherwise The course bundle fixed effect, $\check{\rho}_c$, controls for sort-

[23]It is possible that friends are more likely to sign up for the same section and also more likely to end up at the same firm, both through network effects. However, it is also possible that individuals tend to sign up for sections with friends, and friends share unobserved characteristics that make it more likely for them to end up at the same firm, even absent any network effects. The latter scenario is the one I am concerned about.

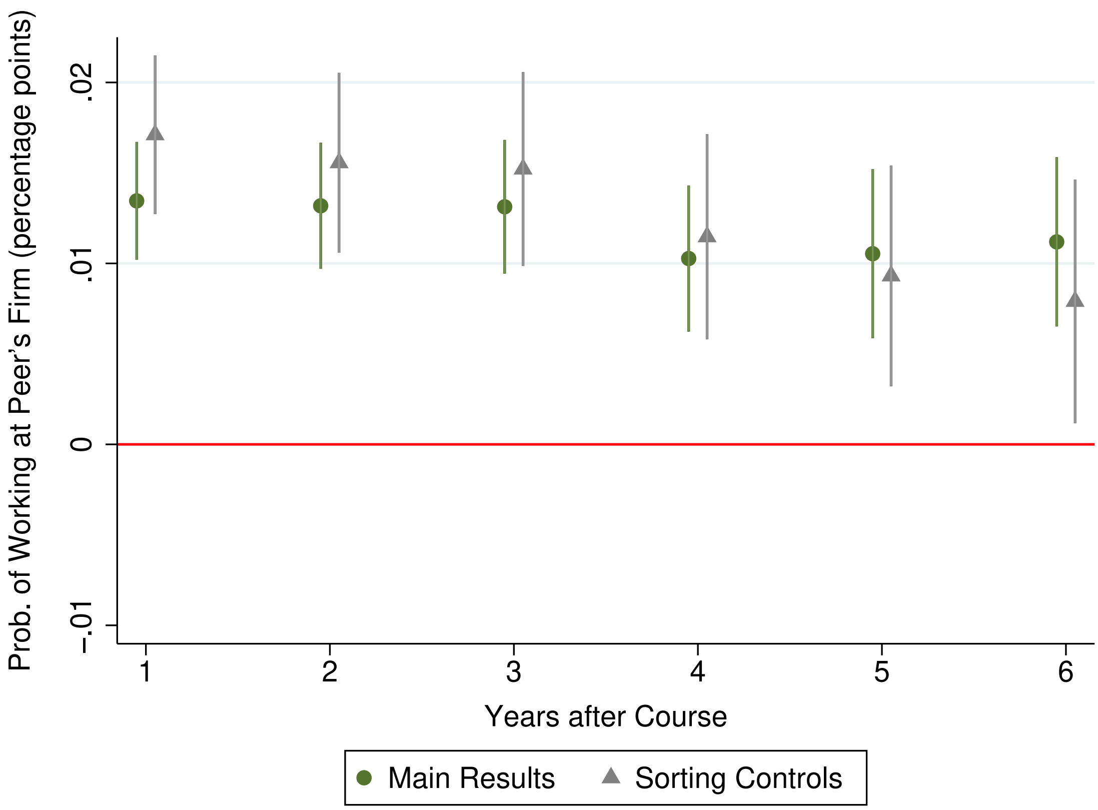
ing into courses, and N_{ijc} measures the number of sections i and j take together. I add variables $Prior\_Cowork_{ij}$ and $Dist_{ij}$ to account for the role sorting into sections along pre-existing relationships plays in affecting subsequent job finding outcomes. If estimates of $\tilde{\gamma}$ do not differ significantly from estimates of γ in Equation 1, this provides reassurance that sorting into sections within a course group based on pre-existing relationships as captured by former coworkers and peers in closer geographical proximity does not drive results.[24]

Figure B4 plots estimation results of Equation 5, in addition to original estimation results from Equation 1. All estimates are displayed with 95 percent confidence bars. Results indicate that estimates including controls for travel distance (as measured by high school location) and whether a pair were prior coworkers produce very similar estimates to the original specification on the effect of taking an additional section together on the propensity for a student to work at a firm with an incumbent peer for any of the years of analysis. For four out of the first six years after the last course, the specification with prior relationship controls estimates network effects that are actually slightly *higher* than estimates from the main specification. Estimates are reported numerically in Table B9, indicating results are not being driven by students sorting into sections within a course with peers based on pre-existing relationships.[25]

[24]Since students typically enroll in college either out of high school or after working for some time, geographic proximity based on high school and former workplace contacts intuitively should capture a significant portion of an individual's relationships. Furthermore, many other places where people form relationships will also be geographically correlated (e.g., family, places of worship, social groups). However, I cannot rule out the possibility that students systematically form pre-existing relationships in ways that are not captured by residential proximity or prior work history.

[25]As an additional robustness check, I conduct an event study analysis looking at the evolution of the probability for pairs of students to work together over time for students who are and are not in the same section of a course. This approach allows me to include individual fixed effects, which further addresses concerns about student sorting along pre-existing relationships. As expected, I find a sizable jump in the propensity for a student to get a job at a firm where her peer works immediately following a shared section, compared to a student whose peer enrolled in a different section of a course.

Figure B4: Effects of Sharing Additional Class Section: Controlling for Pre-existing Relationships



Note: Outcome is whether student i is working in a job where student j was incumbent at time of hire, x years after their first shared course. Coefficient estimates have been multiplied by 100 to reflect percentage changes, and standard errors are clustered by course bundle. Estimates are displayed with 95 percent confidence intervals, and standard errors are multi-way clustered by course bundle, student i, and student j. Estimation results from the main specification estimated using Equation 1 are displayed numerically in the appendix in Table A3. Results estimated using the augmented specification that includes sorting controls for proxies for pre-existing relationships, Equation 5, are displayed in Table B9.

Table B9: Effects of Sharing Additional Class Section, Controlling for Pre-existing Relationships

	(1)	(2)	(3)	(4)	(4)	(6)
	+1yr	+2yrs	+3yrs	+4yrs	+5yrs	+6yrs
Baseline	0.230	0.259	0.259	0.252	0.236	0.222
Number of Sections	0.0171***	0.0156***	0.0152***	0.0115***	0.00931***	0.00790**
	(0.00223)	(0.00253)	(0.00273)	(0.00289)	(0.00311)	(0.00343)
Course Bundle FE	X	X	X	X	X	X
N	31,807,940	28,225,720	23,883,746	19,208,472	14,862,692	10,723,794

*** p<0.01, ** p<0.05, * p<0.1. Observations represent matched pairs of students with peers, $\{i, j\}$. Standard errors multi-way clustered by course bundle, student i, and student j. Baseline and coefficient estimates are multiplied by 100, so baselines represent percent values and coefficient estimates represent percentage changes. Outcome variable in column (1) is whether student starts working at firm where peer is incumbent within three years of their first shared course, and columns (2)-(7) show year-by-year estimates. All specifications include controls for the travel distance between the high schools that student i and j attended, as well as a control for whether i and j worked at the same firm in the three years leading up to their first shared course.