The Effects of College Desegregation on Academic Achievement and Students' Social Interactions: Evidence from Turnstile Data

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Abstract

How does desegregation of elite schools impact academic achievement? Does desegregation affect students' interactions with different types of peers within their school? In this paper, I study a natural experiment at an elite university in Colombia where the number of low-SES students tripled as a result of the introduction of the financial aid program *Ser Pilo Paga*. The average increase in the percentage of low-SES peers had a null impact on high-SES students' academic performance. I shed light on the mechanisms behind this outcome by studying changes in social interactions using data on students' comovements across campus captured by turnstiles located at all entrances. Desegregation led to increased connections between high- and low-SES students, although with some persistent bias in favor of interactions among high-SES students. I find that at least half of the increase in interactions between these groups is explained by interactions with high-achieving low-SES students, but I find no evidence of a preference on the extensive margin for interactions with low-income students who are high achievers.

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1 Introduction

Segregation of students by socioeconomic status, race, or ethnicity is a pervasive issue in education. At the postsecondary level, policymakers have implemented financial aid and affirmative action programs that foster access to selective institutions for low-income and underrepresented groups. These policies are the targets of considerable debate, with affirmative action rules for college admission subject to ongoing challenges in the U.S. court system and discussions about the relevance and ramifications of fostering a diverse college environment continuously proliferating.[1]

Understanding the consequences of policies aiming to foster the access of low-income and potentially lower-achieving students to elite universities is critical. On one hand, these policies may exacerbate achievement gaps within institutions, particularly if the benefited students struggle to perform as well as their classmates. This could lead to potentially negative peer effects on the performance of students from incumbent demographic groups at these institutions (Arcidiacono, Lovenheim and Zhu, 2015). On the other hand, desegregation could diversify social interactions, which is a desirable outcome, especially if we account for the positive impacts that exposure to diversity has on privileged students (Rao, 2019; Boisjoly et al., 2006; Londoño-Vélez, 2022; Corno, La Ferrara and Burns, 2022). Thus, in this paper, I examine what the consequences of college desegregation are for academic achievement and whether desegregation can diversify students' social interactions.

To answer these questions, I use a natural experiment at a large elite college in Colombia that experienced a sharp and unexpected increase in the enrollment of students with a low socioeconomic background after the introduction of a nationwide financial aid program known as *Ser Pilo Paga* (SPP). To measure social interactions, I assemble a novel database of over a hundred million records of student movements across campus col-

[1]The outcome of the case *Students for Fair Admissions vs. Harvard and UNC Chapel Hill*, currently under consideration by U.S. Supreme Court, could lead to the overturning of *Grutter v. Bollinger*, a landmark decision that allowed colleges to consider race in their admissions for affirmative action purposes.

lected by the turnstiles guarding all entrances. I develop a measure to identify which students socialize with one another based on how commonly I observed them entering and exiting campus buildings together, and I validate it against a survey where students listed their friends and acquaintances. I combine these data with student-level records on course enrollment and academic achievement and persistence. I find that socioeconomic desegregation significantly increased interactions between students from different socioeconomic backgrounds, with no adverse effects on the achievement of the relatively better-off students traditionally attending this elite university.

In October 2014, the Colombian government launched *Ser Pilo Paga*, a policy that targeted students from the lowest socioeconomic backgrounds with outstanding academic achievement to promote their attendance of high-quality universities in the country. The program consisted of a loan covering 100 percent of tuition, to be forgiven upon degree completion, plus a small stipend for living expenses. SPP induced an influx of students of low socioeconomic backgrounds into high-quality private universities in the country, effectively closing the socioeconomic enrollment gap among high achievers (Londoño-Velez, Rodriguez and Sanchez, 2020). The rollout of the program was fairly quick, with the first cohort of students benefiting from SPP enrolling in January 2015, barely three months after its announcement. This timely implementation meant that recipient universities and the students of higher socioeconomic backgrounds traditionally attending these institutions had little to no time to adjust their admissions and application strategies to accommodate their preferences for peers of certain backgrounds. Critical to this fact is that, in Colombia, college applications are submitted to a college and major bundle (*program*), with admitted students enrolling directly into their program of application. In this context, as a result of SPP, the number of students admitted to the university that I study sharply increased, with the number of students from lower socioeconomic backgrounds tripling while the number and characteristics of students from other groups remained unchanged.

To estimate the effects of this episode of college desegregation, I leverage the plausibly random variation in the amount of exposure to students of low socioeconomic status (SES) within each program and across entry cohorts. Specifically, I implement a difference-in-differences approach that compares high-SES students enrolling right before and after the rollout of SPP (the 2014 vs. spring 2015 cohorts). A key identification assumption is that high-SES students' program choices did not change in response to the changes in composition of the student body in terms of the share of low-SES peers. To support this assumption, I conduct multiple pretrend and placebo tests and find no evidence of changes in the characteristics of high-SES students enrolling before and after the SPP rollout or in response to changes in the socioeconomic composition of their peer group. To facilitate interpretation of the estimated effects, I developed discrete treatment definitions for when the share of low-SES students is above the maximum share observed in previous cohorts and show evidence documenting the robustness of the research design when I use these discrete treatment alternatives.

I start by analyzing the changes in academic achievement among *elite university* students that came with the introduction of SPP. More low-SES students meant an increase in the achievement gap of this group relative to their high-SES peers. This was mainly driven by a drop in the performance of the low-SES students enrolling in the SPP cohort. Before SPP, the cumulative GPA and credits attempted by the third term of both high- and low-SES students remained around 3.8 and 50 credits, respectively. In the SPP entry cohort, however, low-SES students' GPA dropped to 3.65 and the credits attempted to 46 credits, while there were no changes among high-SES students. Notwithstanding, the increased exposure to these somewhat underachieving students did not affect the outcomes of high-SES students. I show this by using the difference-in-differences design to estimate the causal effect of increased exposure to low-SES students on high-SES students' achievement, finding no effect across GPA, credits attempted, or dropout and degree completion probability.

A potential explanation for the lack of effects on the achievement of high-SES students is a lack of interactions between high- and low-SES peers. Such a result would be consistent with the finding in prior literature that a lack of peer effects in performance is explained by a lack of social interactions between high- and low-achieving students that leaves high achievers' performance unaffected while negatively impacting the performance of low achievers (Carrell, Sacerdote and West, 2013). In this regard, students of different socioeconomic status may not interact much with each other due to a preference for the company of others of similar backgrounds (a phenomenon known as "homophily"). Thus, the second part of my empirical analysis examines how social interactions between low- and high-SES students changed with the increased presence of low-SES students in the university.

I measure social interactions using data on students' comovements across the *elite university* captured by turnstiles located at the 18 campus entrances. These data are available for the period from fall 2016 to spring 2019, and I use them to measure comovements among students in the same program and entry cohorts after six and seven semesters of their enrollment. I define a pair of students as linked if they passed through the turnstiles in the same direction (entering or exiting a building) within a time window of three seconds or less at least twice in a term. Appendix A describes the process by which I validated and arrived at this definition and addresses potential measurement error limitations. Descriptive statistics show that high-SES students had an average of 5.2 links in their program and cohort before the implementation of SPP, with approximately a quarter of the links being with a low-SES peer. On the other hand, low-SES students had on average five links, and 0.35 of those were with other low-SES peers. With the onset of SPP, high-SES students increased their total links to 5.6 and had four times more links with low-SES peers, whereas low-SES students stayed at five links but had six times more links with other low-SES peers. I next proceed to estimate how these changes in exposure to low-SES peers affected the number of interactions across socioeconomic groups.

Interactions between low- and high-SES students significantly increased with the onset of SPP, with high-SES students substituting links among themselves with links with low-SES peers, particularly among the groups with the largest shares of exposure. I estimate an increase in the probability of a link between the two groups of 14 percentage points at the average percent of low-SES peers, equivalent to an increase of 0.63 low-SES links. These effects are larger the greater is the share of low-SES peers. In groups with shares of low-SES peers above the median of that in the pre-SPP period, the probability of a link between a low- and a high-SES student increased by 14 percentage points and the number of links by one, while the links among high-SES students declined by 6 percentage points, equivalent to 1.2 links. These results suggest that interactions across socioeconomic groups did diversify with the college's desegregation despite the lack of impact on the academic performance of students from the demographic groups traditionally concentrated at this elite institution.

Importantly, I do find evidence suggesting that a bias for links within the same SES group persists, suggesting some lingering segregation among students. First, estimations on the percentage of low-SES links indicate that high-SES students' responses to the changes in peer group composition are not monotonic. Specifically, a one-percentage-point increase in the share of low-SES peers translates to a 0.7-point increase in the percentage of high-SES links with that group. This suggests an unrealized 30 percent reduction in segregation of social interactions that proved resistant to the increased exposure to low-SES peers. To further assess this issue, I compute a program–cohort measure of friendship bias following Chetty et al. (2022). My results suggest that, while the variation in friendship bias was reduced, many of the programs among the 2015 cohort did exhibit some bias in favor of links in their own SES group. I do not find evidence suggesting this bias correlates with the percentage of low-SES students in the group.

My last set of results bridges the findings of positive impacts on the diversity of social interactions and of the lack of effects on student academic achievement. The increased interactions between high- and low-SES students may be explained by similarities in academic achievement being stronger than the preferences for interactions with peers of the same SES background. To test this conjecture, I identify low-SES students with an academic performance equal to or above the average performance of high-SES students and estimate the effect that exposure to low-SES peers had on the number of interactions with these low-SES but very high-achieving students. Approximately half of the increase in interactions between low- and high-SES students is explained by interactions with very high-achieving low-SES peers; this figure increases slightly when I use first-term performance measures—which are more likely to be observed among peers—such as GPA or credits attempted. However, at the extensive margin, the probability of interactions with high-achieving students remains the same, again with a small increase when I use measures of first-term achievement. These effects suggest that, while some preferences for interactions with high achievers are present, the increased diversity in interactions is only partially explained by homophilic preferences for links with high-achieving students.

This study makes four contributions to the literature. First, my paper contributes to the research examining the consequences of college desegregation for academic achievement. Other scholars have examined the effects of exposure to minority students on white and Asian students' performance, finding somewhat conflicting results. Namely, Arcidiacono and Vigdor (2010) use quasi-random variation in the share of minority students across entry cohorts at selective U.S. colleges, finding negative effects, and Bleemer (2021a) examines the impact of resegregation (i.e., the ending of an affirmative action policy in California) on white and Asian students' performance, finding no effects. My findings contrast with those of Arcidiacono and Vigdor (2010) in that I show that increases in the exposure to underrepresented students at elite schools have no effect on privileged students' performance. My results are also complementary to those of Bleemer (2021a) in showing that the opposite of a resegregation policy—namely, a program inducing desegregation through financial aid targeting low-income students—has no impact on privileged students' achievement, either, and with findings from Lau (2022) and Corno, La Ferrara and Burns (2022) showing that exposure to racial minorities in college has no adverse effects on achievement and can even improve the outcomes of traditionally underserved students. Also, my findings align with previous evidence from K–12 settings of no effect of desegregation policies on the academic achievement of students from the demographic groups traditionally attending these institutions (Angrist and Lang, 2004; Dobbie and Fryer, 2014) and with findings from Lau (2022) and Corno, La Ferrara and Burns (2022).[2]

Second, this paper contributes to the literature examining how students' social interactions change with the onset of financial policies fostering desegregation. While prior research has consistently found positive impacts on the college attainment of students benefiting from financial aid and affirmative action programs for underrepresented groups (Bleemer, 2021b; Chetty et al., 2020; Londoño-Velez, Rodriguez and Sanchez, 2020; Mello, 2022), I provide novel evidence on how social interactions change under a desegregation policy in light of its lack of impacts on achievement. Findings from Michelman, Price and Zimmerman (2020) and Zimmerman (2019) show that low-income and minority students tend not to take part in privileged students' social clubs even if they share the same college environment, which may explain the somewhat slower or lacking social mobility among low-income students attending elite institutions. My measure extends the definition of social interaction by capturing dynamics outside those in clubs and classrooms, showing that high- and low-SES students do connect at the outset of desegregation, which may have other positive ramifications for the social mobility of low-income students and for prosocial behaviors among wealthy ones (Rao, 2019; Boisjoly et al., 2006; Londoño-Vélez, 2022).[3]

[2]Angrist and Lang (2004) study the effect of a desegregation program in Boston on the academic achievement of students from the groups traditionally attending the receiving schools, finding no significant impact; a similar study by Dobbie and Fryer (2014) focuses on students eligible to attend schools with high-achieving peers and finds no impacts on the achievement of either group.

[3]My work is closely aligned with that of Londoño-Vélez (2022), who studies the effect of socioeconomic diversity at an elite college in Colombia on students' preferences for redistribution. In this work, Londoño-

Third, this paper connects to the literature examining diversity in school settings and its effects on segregation in social networks. This research has examined the process under which friendships form in college settings and has relied on proxies of social interactions such as email exchanges (Marmaros and Sacerdote, 2006) or Facebook friendships (Baker, Mayer and Puller, 2011). My study provides a finer measure of effects on social interactions by capturing the effects of desegregation at the intensive and extensive margins of interactions with low-income students. Similarly, the evidence coincides in indicating that peers' proximity and race are determinants of friendship formation: namely, students assigned to the same dorm are more likely to be connected, but the chances of connecting are higher for same-race students.[4] My study uses a different dimension of proximity, namely, being in the same major and entry cohort. My findings indicate that proximity in major and cohort groups is determinant for student interactions. A related substream of research has focused on measuring overall segregation in social interaction and on studying how policies can reduce within-group segregation in K–12 settings, finding no association between whom students interact with and academic achievement (Echenique, Fryer and Kaufman, 2006) and nonlinear responses of interactions to scenarios in which minorities are reallocated across schools (Mele, 2020).[5] My findings show consistently positive impacts of exposure on the diversity of interactions at the program level and show that changes in interactions through changes in exposure do not impact academic achievement.

Vélez finds positive impacts of exposure on wealthy students' preferences—a result that seems to be related to more interactions with low-income peers. My work validates this latter finding while pointing out that the change in social interactions is relatively small.

[4]Marmaros and Sacerdote (2006) examine how people form social networks with their peers. They use student email exchange data and find that first-year students form friendships with students in physical proximity to them and are more likely to form friendships with peers of the same race. Baker, Mayer and Puller (2011) use data from Facebook and random dorm assignment at one college and find that exposure to students of different races in dorms leads to more diverse friendships.

[5]Echenique, Fryer and Kaufman (2006) measure within-school segregation as the extent to which students interact socially with other students of the same race. Mele (2017) develops a structural model of friendship formation among students, and Mele (2020) uses it to simulate reallocation programs across schools and examine its impacts on within-school friendship formation. His findings suggest that policies that reallocate students by parental income have less impact on racial segregation within schools than those that reallocate on the basis of race.

Last, this paper contributes to the research examining the the role of social networks in academic achievement and peer effects. In their work, Carrell, Sacerdote and West (2013) design a peer effects experiment aiming to optimize the assignment of high-achieving students with the aim of boosting the performance of low-achieving ones. Their results indicate no effects on the achievement of high achievers and negative effects on that of low achievers, a result that the authors document is driven by segregation in social interactions between the two groups. Complementing Carrell, Sacerdote and West's (2013) results, I find a lack of peer effects on the achievement of high-SES students—the large majority of whom are high performers—but a positive effect on interactions with low-SES students—the group with lower achievement on average. I do not find evidence suggesting that social interactions between high- and low-SES students are merely interactions among high performers. These results also complement those from Zárate (Forthcoming), who finds heterogenous results by gender with respect to how exposure to peers with different social characteristics can affect academic achievement.

2 Background and Setting

In this paper, I examine the effect of a socioeconomic desegregation policy on students' academic achievement and social interactions. Specifically, I study the case of a large private university located in Bogotá, Colombia (from now on, *Elite University*[6]), which in 2015 experienced a large and unexpected increase in the number of low-income students enrolled while the enrollment of relatively wealthy students remained constant. The increase was driven by *Ser Pilo Paga* (SPP)—a forgivable loan program for high-achieving low-income students who wished to attend a high-quality university. Importantly, the increase in low-income students' enrollment varied across the thirty-one programs offered at Elite University. My research design focuses on relatively wealthy students and com-

[6]I do not provide the real name of the university I study for confidentiality reasons.

pares students from the entry cohorts before and after the SPP rollout (2014 vs. 2015). I use the change in the number of low-SES students across programs and cohorts as the treatment. In this section, I explain the context of SPP and Elite University, where the natural experiment took place.

Higher education in Colombia is strongly segregated. By 2014, the gap in gross post-secondary enrollment between low-income and wealthy youth was 51 percentage points (Arias Ortiz, Elacqua and Gonzalez-Velosa 2017). Among those enrolled in bachelor's degrees, high-ability low-income students are much less likely to be enrolled at a private university than their wealthy counterparts (Carranza and Ferreyra 2019). This can be explained by the high tuition rates of private universities relative to average salaries in the country and the limited financial aid options available for low-income students. SPP aimed to address this segregation by providing low-income students a loan that covered tuition plus a small allowance for attending a high-quality accredited institution.[7] The loan was forgiven conditional on completion of the degree. Eligibility for SPP required students to be classified as poor under the government's index of household wealth and to have scored in the top decile of the national high-school exit exam SABER 11.[8] SPP awarded loans for new cohorts of students between 2015 and 2018, benefiting about 40,000 students nationwide. Previous research has found that SPP increased diversity at top private universities by shifting the basis of selection more toward ability instead of income (Londoño-Velez, Rodriguez and Sanchez, 2020). As depicted in Figure 3, of all the institutions eligible for the program, Elite University had the largest change in the percentage of low-SES students enrolled, with over 500 new low-SES students in the 2015

[7] High-quality accreditation is granted to higher education institutions by the National Council of Accreditation. It is granted after a detailed review from a panel formed by the institution, the academic community, and the council. By 2014, the year of the first round of SPP, 32 universities in Colombia had high-quality accreditation.

[8] The index of household wealth is known as SISBEN, and it is based on the census survey targeted to household previously screened as potentially poor. Londoño-Velez, Rodriguez and Sanchez (2020) provide more details about how SISBEN was used to screen SPP-eligible students. SABER 11 is required for all students in the country who are about to complete their high-school education. The exam is held twice a year, following the two academic calendars of schools in the country: January–November and August–June.

entry cohort, which tripled the share of this group relative to its 2014 enrollment share.

The timing of the SPP rollout and the admission rules at Elite University set the conditions of my research design. First, admissions to Elite University are open for each year's spring and fall terms and are determined by the applicant's score in the SABER 11 standardized test. Students must apply to a major and entry cohort for which admission officers predetermine a specific SABER 11 weighting formula and cutoff score.[9] Second, SPP was broadly unanticipated among students and higher education institutions. SPP was launched in October 2014, and only students who had taken that October's test were SPP-eligible. Candidates had to apply for enrollment in the following spring (2015), for which 10,000 forgivable loans were offered. Thus, students from the demographic groups that traditionally apply to Elite University had very little time to change their application portfolio, and university officers could not adjust the admission criteria to limit the influx of admitted and eventually enrolled students. As a result, the number of high-SES students enrolled in 2015 remained similar to that in 2014, but the number of low-SES students increased significantly.

Figure 4 depicts the first-term enrollment trends by socioeconomic status (SES) at Elite University. Between 2012 and 2014, less than 150 first-term students came from low-SES backgrounds. Once the first cohort of SPP beneficiaries enrolled, the number of low-SES students tripled to 541, while the number of students from other socioeconomic backgrounds remained almost the same. Figure 5 compares the number of low-SES students across programs in the entry cohorts before and after SPP. The gray- and blue-lined bars depict the number of low-SES students in the cohorts right before SPP (i.e., 2014-1 or spring 2014 and 2014-2 or fall 2014), whereas the gray-filled bars depict the number of low-SES students in the first cohort of SPP (i.e., 2015-1 or spring 2015). The variation in the number of low–SES students is important. Majors such as business and music expe-

[9]Unlike in the U.S., higher education applicants in Colombia must apply to both a major program and a college. SABER 11 is composed of five modules, which are given different weights depending on the major of application. For example, for admission to engineering majors, the quantitative reasoning module is assigned a higher weight than the social sciences module.

rienced virtually no change in the number of low-SES students, while others such as civil engineering and psychology experienced a notable increase.

Table 7 examines the relationship between the number of low- and high-SES students by program and cohort. Column (1) displays the unconditional correlation, and Column (2) controls for average program–cohort student characteristics. Both indicate that increases in the number of low-SES students are positively associated with the presence of more high-SES students in the program and cohort, suggesting that traditionally large programs enrolled more of the incoming low-SES students. Once program fixed effects are included in Column (3), the correlation between the number of low- and high-SES students is not statistically different from zero. The size of the estimated correlation also becomes much smaller in magnitude. I add entry cohort fixed effects in Column (4) to address shocks common to all programs in a given entry cohort and find no changes in the relationship. These results suggest that the positive correlation between the numbers of low- and high-SES students is a feature of certain programs and did not change with the increase in the number of low-SES students brought by SPP. Moreover, I do not find any evidence suggesting that the influx of low-SES students crowded out high-SES students from certain programs.

The influx of low-SES students did not crowd out other students but did lead to busier—albeit not over capacity—classrooms. Figure 6 provides descriptive statistics of the courses taken by first-term students from the 2012 to 2016 entry cohorts. The figure describes the average number of sections (equivalent to classrooms) available per course, the average number of seats available per section, and the ratio of students enrolled (all and low-SES students) per seats in a section. In 2015, classroom occupation peaked but remained below 100 percent (i.e., 84 percent on average), suggesting that classrooms, on average, did not have crowding issues that could have hampered learning. These findings also mean that, in the year of SPP implementation, the university did not create more sections per course or increase the number of seats available per classroom. In the case of

Elite University, I do not find significant increases in the number of sections per course or seats per section in the 2016 cohort, either, suggesting that the university already had the capacity to accommodate the extra students by 2015.

In the next section, I describe the data used in this research, its construction, and the final sample used in my research design. I also provide descriptive statistics for both low- and high-SES students, documenting the changes in their characteristics in the 2015 cohort and the significant achievement gap between the two groups in this cohort.

3 Data

The data for this paper come from two sources: administrative records from Elite University, and detailed records from the turnstiles located at each of the 18 access points to the Elite University campus.

Elite University administrative records. I use records from all students enrolled at Elite University between 2012 and 2018, which contain student–course-level data on student characteristics (i.e., gender, age, mother's education, high-school ID), SABER 11 (from here on SB11) standardized test scores, SPP recipient status, selected major, entry cohort and term of enrollment. For each semester, I observe each of the courses in which the student is enrolled and his or her course GPA. More importantly, I observe the student's household social stratum indicator. This indicator has six categories, which are used to provide homes with utility bill subsidies. In addition, it is widely known in the country as a proxy of social status. I use the household social stratum at the time of college application to classify students in two socioeconomic (SES) groups: high SES (students living in a household classified between strata three and six) and low SES (students from strata one and two). The students benefiting from SPP mostly fall in the low-SES category. As depicted in Figure 4, the majority of students at Elite University are classified as high SES.

Turnstile records. I use records on student movements on the Elite University campus to identify students' social interactions. The Elite University campus is guarded by turnstiles located at 18 entrances to main buildings and campus areas. To enter or exit through any of these entrances, students and university staff must swipe their university ID. Security officers at Elite University provided me individual-level records of university ID swipes at the turnstiles from February 1, 2016, to November 1, 2019. These records include the student ID number, entrance, action (IN or OUT of campus), and date, hour, minute and second of the swipe. Figure 2 in Appendix A displays a heat map of the average frequency of student ID swipes at three of the busiest campus entrances by 20-minutes block. Yellow and blue cells indicate peak and off-peak hours, respectively. The figure documents the constant flow of students across the campus entrances throughout the day, with peak hours at times of class changes and during lunch hours.

I define a pair of students as linked when their IDs are swiped at a turnstile in a time window of three seconds or less at the same entrance and in the same direction (either entering or exiting campus) and when I observe the same pair of IDs comoving at least twice in a semester. Appendix A describes the data validation process for this definition. The appendix also discusses alternative definitions, which I use as robustness tests. Appendix B displays results using alternative definitions of the turnstile interaction.

Sample. My analytic sample consists of all first-term students in the entry cohorts before and after the SPP rollout (i.e., fall and spring 2014 and spring 2015). I search for their interactions during the 6th and 7th calendar semesters after their first term of enrollment and among students in the same entry cohort and major. For example, I match students in the spring 2014 entry cohort with their interactions as captured by the turnstiles during fall 2016 and the spring 2017. I merge administrative records and pairwise student interaction data using the student ID number, which is available in both data sources. My final sample consists of 4,027 students across 31 majors and three entry cohorts. This sample captures the universe of students enrolled in these majors and cohorts, except for two programs (government and directed studies) that started after SPP and that I therefore exclude from my study.

Student characteristics. Table 6 provides descriptive statistics of high- and low-SES students in the pre- and post-SPP entry cohorts (i.e., 2014 vs. spring 2015). The table includes results of the t test of mean differences between low- and high-SES students.

I start by comparing the characteristics between the 2014 and 2015 cohorts of high-SES students. Both groups are similar in their observed characteristics, including gender shares, age, and mother's education level. Approximately a quarter of them graduated from high schools outside Bogotá, suggesting that they are internal migrants. The 2015 cohort of high-SES students has fewer numbers of high-school peers in the cohort (11.54 vs. 8.81 students) and slightly higher SB11 test scores (0.00 vs. 0.05 standard deviations). Both the 2014 and 2015 cohorts have similar ID swipes in the turnstiles (1,340.19 vs. 1,311.73) and similar numbers of turnstile-elicited links (5.21 vs. 5.62). In Table 4, I further document the similarities between the 2014 and 2015 cohorts and show that the high-SES student characteristics do not significantly change with the changes in the share of low-SES peers.

Low-SES and high-SES students differ significantly in both the 2014 and the 2015 cohorts, with the differences between the two widening among the 2015 students. In 2014, low-SES students were more likely to have a mother with no college degree than their high-SES peers (24 vs. 8 percent), to be internal migrants (35 vs. 23 percent), and to have a scholarship or loan (37 vs. 7 percent). Low-SES students also had fewer high-school peers in their cohort than their high-SES peers (3.16 vs. 11.54 peers) and had lower SB11 test scores (-0.10 vs. 0.00 standard deviations from the mean). These differences widened in the 2015 cohort. On average, 40 percent of 2015 low-SES students had a mother with no college degree, versus 11 percent of their high-SES peers, 57 percent were internal migrants relative to 24 percent of high-SES students, and 87 percent had either SPP or other forms of loans or scholarships relative to 16 percent of high-SES students. Both low- and high-SES students had fewer high-school peers, although the gap between the two groups persisted in favor of high-SES students (8.81 vs. 1.96 high-school peers). In terms of academic achievement, the gap in SB11 test scores between low- and high-SES students also widened in 2015 to 0.21 standard deviations, relative to the 0.05 standard deviations among the 2014 students. I will examine this divergence and its implications in more detail in the following sections.

Table 6 shows that high-SES students increased their number of links with low-SES peers to an average of one (from 0.24 in 2014). Their links also became more dispersed in terms of the difference in age and SB11 test scores in 2015 but remained very similar in other characteristics such as gender or the share of links with high-school peers. The differences between high- and low-SES link characteristics also remain fairly similar between the two cohorts, except for the SB11 test scores, as high-SES had a greater distance in test scores from their links than low-SES students (0.81 in 2015 vs. 0.69 in 2014). Importantly, while low-SES students had similar numbers of turnstile-elicited links in the 2014 and 2015 cohorts (approximately 4.96), the 2015 low-SES students had significantly fewer ID swipes at the turnstiles than the high-SES students (1,099.8 vs. 1,311.73 swipes). These differences in ID swipes have implications for the identification of the effects on social interactions, which I will examine in the empirical strategy section and in detail in Appendix A.

Student achievement and gaps between low- and high-SES students. I characterize the differences in academic achievement between the high- and low-SES students in Figures 7 and 8. For these figures, I take advantage of the administrative data availability and plot the trends in academic achievement across the entry cohorts enrolling since 2012. For each cohort, I plot the average achievement outcome among high- and low-SES students and include the estimated 95 percent confidence intervals based on clustered standard errors at the major and entry cohort levels. The red line separates the entry cohorts before the start of SPP (left side) and the cohorts entering during SPP (right side). Figure 7 displays performance indicators, mainly cumulative GPA and total credits attempted, whereas Figure 8 describes persistence (dropout rates and graduation). I label a student a dropout if he or she does not show up as enrolled for two consecutive terms after the fifth term of college. Similarly, I label a student as graduating if he or she completed the degree in eight terms or fewer.[10]

The cohort of high- and low-SES students who enrolled at Elite University at the onset of SPP (i.e., in the 2015 entry cohort) exhibits significant achievement gaps, particularly in GPA and cumulative credits attempted, with low-SES students having on average a lower cumulative GPA and fewer attempted credits than their high-SES peers. For example, the GPA of pre-SPP cohorts is relatively constant and close to 3.85 for both high- and low-SES students. For the SPP cohort, however, the GPA of low-SES students drops to 3.75 in the first term of college and to 3.6 by the third term, while the GPA of high-SES students remains the same. Regarding the cumulative number of credits attempted, the pre-SPP cohorts of high- and low-SES students attempted on average 50 and 48 credits by the third term, respectively. However, in the SPP cohort, low-SES students on average attempted 45.7 credits, while high-SES students continued to attempt on average 50 credits. A course at Elite University usually bears three credits. This means that low-SES students enrolling in 2015 had attempted on average one fewer class than their high-SES peers by the third term of college and had a cumulative GPA 0.25 points lower. Nevertheless, the differences in achievement do not pair with differences in dropout or graduation rates, suggesting that the lower achievement of low-SES students did not translate into diminished persistence.[11]

In the next section, I examine the implications of the changes in student composition for the academic achievement of high-SES students and then examine the role of the changes in social interactions between high- and low-SES students.

[10] At Elite University, this is considered an on-time graduation for all degrees except medicine.

[11] Importantly, rates of graduation in fewer than eight terms are very small at Elite University across all groups, as many students tend to take extra semesters to complete minor degrees or double major in other programs. Low-income students benefiting from SPP and other financial aid programs tend to be constrained in that they are not financed for terms beyond those scheduled for their major curriculum, which explains their slightly higher likelihood of graduation.

4 Identification Strategy: Effects on Academic Achievement

I start by examining the effects of increased exposure to low-SES peers on the achievement of students from the high-SES demographic groups who traditionally attend Elite University. To do so, I use a difference-in-differences approach that exploits the variation in the share of low-SES peers within programs and across entry cohorts before and in the first cohort with SPP students (2014 vs. 2015-1). As documented in the "Background" and "Data" sections, SPP increased the percentage of low-SES students at the university. Thus, high-SES students across different programs faced an increased share of low-SES peers in the student body, which could potentially affect the academic achievement of the former.

$$Outcome_i^{mc} = \beta_l R_{mc}^l + \mathbf{X_i'} B + \beta_m + \beta_c + \varepsilon_{imc} \tag{1}$$

Equation 1 describes the econometric model. $Outcome_i^{mc}$ represents the academic outcome of student i enrolled in major m and entry cohort c. R_{mc}^l represents the percentage of student i's peers who are of low SES, and $\mathbf{X_i}$ is a matrix of female, mother with no college education, and intermediate SES indicators, standardized high-school exit exam (SB11) scores, and age in years at the start of college. β_m and β_c capture major and entry cohort fixed effects, which absorb unobserved variation common to majors and entry cohorts, respectively. Finally, ε_{imc} represents robust standard errors clustered at the program and cohort level. I estimate Equation 1 using ordinary least squares (OLS). The estimated effect β_l captures the average treatment effect on the treated (ATT) of increased exposure to low-SES peers on student achievement. Figure 5 describes the variation exploited for causal identification. The percentage of low-SES students relative to that in 2014 increased at different rates across programs. Table 7 shows that the increased number of low-SES peers did not crowd out wealthy students.

Unbiased identification of β_l requires that, in the absence of the treatment, the outcomes for the treated and control groups would have exhibited the same trends. To test this parallel trends assumption, I estimate the placebo effects of the share of low-SES students on high-SES student achievement using data from the 2012 and 2013 entry cohorts. The results are displayed in Table 8. I do not find any evidence suggesting that changes in the percentage of low-SES students were associated with student outcomes in the period before SPP. Violations of this assumption would also be implied by nonrandom allocation of high-SES students across majors and entry cohorts driven by the changes brought by SPP. Specifically, at the outset of SPP, high-SES students could have self-selected in programs and entry cohorts due to their preferences regarding the proximity of low-SES students; this would be reflected by changes in high-SES students' characteristics. I test this by estimating Equation 1, without the matrix of student characteristics, for observed student sociodemographics. I display the results in Table 9. I find no evidence that the characteristics of wealthy students changed in response to the changes in the share of low-SES peers.

One concern with this design is that low-SES students could have sorted into certain programs for reasons correlated with changes in the outcomes of high-SES students but unobserved to the researcher.[12] As depicted in Figure 3, the increase in the share of low-SES students does not necessarily follow their distribution in the pre-SPP cohorts, suggesting differences in program preferences among students in the 2015 cohort. I assess this by estimating alternative analyses using as treatment a discrete variable for when the percentage of low-SES students in the program is above a threshold that is at least the maximum of that percentage in the previous terms. Specifically, I define indicators for when the share is over the 50th or 75th percentile of the distribution in 2015-1. This is equivalent to low-SES percentages of over 24 and 36 percent, respectively. I show that pretreatment trends in the outcomes and observed characteristics hold parallel under this

[12] For example, differences in the unobserved characteristics of low-SES peers could yield different peer effects, or differences in the unobserved characteristics of high-SES peers who continue to choose programs disproportionately attracting low-SES students even after the change in composition could also impact high-SES students' outcomes.

definition in Figures 9 to 12. This discrete treatment setup resembles a nonstaggered design with the treatment status shifting for some programs in the 2015-1 period, thus offering an alternative estimate of the ATT (de Chaisemartin et al., 2022; Roth et al., 2022).

5 Results on the Effects of Desegregation on Achievement

Table 10 displays the estimated effect of increased exposure to low-SES peers on wealthier students' academic achievement and persistence. Panel A displays OLS estimates of β_l for all the outcomes discussed in Figures 7 and 8. In Panel A, the estimated effects on cumulative GPA by the first, third, and sixth terms and on dropout and graduation are imprecise and statistically not different from zero. The point estimates for the number of credits attempted by the first and third terms are positive and statistically significant. However, the magnitude of the effects is small. The share of low-SES students increased by 18 percentage points on average from 2014 to 2015. This yields an increase in the number of credits taken by the first term of 0.27 credits and of 0.52 credits by the third term. The average course at Elite University bears three credits, which suggests a small effect on courses taken. Overall, I do not find conclusive evidence that exposure to low-SES peers impacted the academic performance of high-SES students.

To assess the robustness of my estimates, Panels B and C display results based on the discrete definition of treatment discussed in the previous section. The estimated effects for all outcomes continue to be imprecise and statistically not different from zero. Importantly, the positive effects on credits attempted found with the continuous treatment disappear, suggesting that those results are not robust. As an extra test, Panel D displays estimates that use the percentage of SPP students instead of the percentage of low-SES ones, showing no difference in findings.[13] Overall, these results speak to prior literature finding no effects of desegregation on traditionally privileged students' GPA in K–12 (An-

[13]Unlike the number of low-SES peers, the number of SPP peers was zero in 2014 and took positive values in 2015.

grist and Lang, 2004; Corno, La Ferrara and Burns, 2022) and higher education settings (Bleemer, 2021b).

The peer effects literature would suggest that increased exposure to low-achieving students should negatively impact the performance of high achievers (Arcidiacono and Vigdor, 2010; Epple and Romano, 2011). This should be the case in this setting, given that the incoming low-SES students had, on average, lower performance than their wealthy peers (see Figure 7). A possible hypothesis explaining the lack of effects is that segregation between high- and low-SES students persisted within program–cohorts. This would be consistent with findings from Carrell, Sacerdote and West (2013), which suggest that assigning low-achieving students to high-achieving classrooms can lead to segregation between the two groups. To test whether segregation between SES groups explains the lack of peer effects on achievement, I estimate the effects of exposure to low-SES students on the social interactions of high-SES students.

6 Estimating the Effects of Desegregation on Social Interactions

To estimate the effect of the increased exposure to low-SES students on high-SES students' social interactions, I use Equation 1 and include as controls the number of peers from the same high school in the student cohort, as this is likely to confound the social interactions that high-SES students have with other socioeconomic groups. Social interactions are defined on the basis of turnstile-elicited links, as discussed in the "Data" section and in Appendix A. That is, a pair of students is linked when I observe them swiping their student IDs at the same entrance and going in the same direction within a window of three seconds or less and at least twice in a semester. Appendix B shows the results based on the alternative definition of the turnstile-elicited interactions.

The turnstile-elicited measure of social interactions brings another source of bias in this difference-in-differences setup. Specifically, there is potential measurement error in the turnstile-elicited interactions, as there is a risk of capturing random comovements of low- and high-SES students across the turnstiles that could be falsely attributed to be effects of exposure to low-SES students. If turnstile-elicited interactions partially capture social interactions, then they need to be on average representative of true interactions and cannot be biased by potential random noise. Moreover, the rates of false positives and false negatives (i.e., the likelihoods of defining a pair of students as linked when in fact they are not and of defining a pair as not linked when in fact they are, respectively) cannot be determined by the exposure to low-income students R^l_{mc}. My definition of students' social links accounts for these possibilities and aims to minimize the false positive and negative rates. Specifically, I use survey-elicited secondary data on social interactions among one major–cohort group at Elite University to obtain estimates of the rates of false positives and false negatives under alternative definitions of turnstile-elicited links.[14]

Appendix A provides details on the secondary data and the computation framework and procedures that I use to assess measurement error. My results indicate that the turnstile-elicited links suffer a relatively high false negative rate of approximately 60 percent but a false positive rate below 10 percent (see Table 1). To assess the extent to which such measurement error can diminish the quality of the turnstile-elicited interaction data, I compare the average characteristics of the turnstile-elicited links with those from the survey-elicited links and those obtained under a simulated scenario of turnstile-elicited links formed at random. The turnstile-elicited links compare well with the survey-elicited links, despite the high false negative rate. More importantly, the characteristics of the turnstile-elicited links are statistically the same as those of the survey-elicited links but different from those that we would observe if the interactions identified as links were in

[14]The survey was conducted online between December 7, 2017, and January 5, 2018, and elicited the network information of 110 economics undergraduate students from the 2017 fall cohort. The survey was conducted with Qualtrics. Students who completed the survey received a free lunch voucher for a recognized chain restaurant in the campus area. Cárdenas et al. (2022) provide a detailed description of the survey.

fact purely random (see Figure 1). I rationalize the measurement error in a 2x2 difference-in-differences framework that follows Goodman-Bacon (2019) and Cunningham (2021). If the measurement error is associated with exposure to low-income students in ways unobserved by the researcher, then the observed average treatment effect on the treated (ATT) may differ from the true ATT. I proxy the measurement error with the number of ID swipes at the turnstiles and the number of courses taken with turnstile-elicited links. I do not find evidence of the change in the percentage of low-SES peers affecting any of these measures (see Table 3). In summary, I do not find evidence that measurement error in my turnstile-elicited interaction data biases my estimates on the impacts of exposure.

6.1 Results

I characterize social interactions as follows. First, I measure the effect of increased exposure to low-SES peers on a student's probability of having at least one link with a low-SES student in his or her group. Second, I estimate the effect on the number of low-SES links formed by high-SES students. Third, I measure the effect on the composition of the high-SES students' friendships, which I define as the percentage of links with low-SES peers. The first and second measures can be interpreted as extensive and intensive margin effects. The third describes how much a student's connections diversify in response to desegregation in his or her group.

Table 11 displays the results of estimating Equation 1. Panel I displays the estimated effects on the probability of interactions with high- and low-SES peers, Panel II displays the estimated impacts on the number of interactions, and Panel III displays the impacts on the percentage of links with low-SES students. Panel A shows continuous estimates following Equation 1, whereas Panels B and C display estimates based on the discrete treatment definitions.

Exposure to low-SES peers significantly changed the social interactions of high-SES students. Focusing on Panel A of Table 11, I find that the average increase in the percentage of low-income peers (18 percentage points) had significantly positive effects at the extensive and intensive margins of interactions between high- and low-SES students. The average increase in the low-SES share led to a 14.4-percentage-point higher probability of a link between a high- and low-SES student and an average increase in the number of links with low-SES students of 0.67 links. I also find evidence of a reduction in interactions among high-SES students of 0.63 links (significant at a 90 percent confidence level).

The effects of exposure on social interactions are larger the greater the share of low-SES peers in the group. Looking at segments B and C of Panels I and II in Table 11, I find that the effect on the probability of a link between high- and low-SES peers increased by 11 percentage points when the share of low-SES peers in the group was over 24 percent and by 14 percentage points when low-SES peers represented more than 36 percent of the group. This implies an increase in the number of links with low-SES peers of 0.63 and 0.94, respectively. My estimates also indicate that links among high-SES students significantly decreased at the higher tail of the distribution of low-SES shares. When the percentage of low-SES peers was over 36 percent, the probability of a link among high-SES students decreased by 6 percentage points and 1.2 links. This suggests that when the shares of low-SES peers in the group were large, high-SES students increased their links with low-SES peers, likely sacrificing some of their links with other high-SES peers. While the net result on the total number of links suggests fewer friends overall (-0.298), I do not find any evidence of statistically significant changes in the overall number of links.

The last panel of Table 11 displays the estimates of how sensitive high-SES students friendships were to changes in the socioeconomic composition of their group. Specifically, I estimate the effect of changes in the share of low-SES peers on the percentage of high-SES students' friends who are of low SES. If high-SES students experienced merely compositional responses to the changes of peers in their group, the estimated effect would be one: a one-percentage-point increase in low-SES peers would translate to a one-percentage-point increase in the share of low-SES friends. Estimates over one would suggest that high-SES students were even more welcoming of low-SES among their friends, and estimates below one would suggest some reluctance. My estimates indicate the latter. For every additional percentage point of the low-SES peer share, the percentage of links with low-SES students increases by only three-quarters of that (0.75 percentage points), suggesting some aversion among high-SES students to changing their friend group in the same ratio as their peer group changed.

To further assess the aversion to forming links with low-SES students, I follow Chetty et al. (2022) and apply their concept of friendship bias.[15] In the context of this paper, friendship bias is the tendency of high-SES students to befriend low-SES peers at lower rates than high-SES peers. It is mathematically defined as one minus the percentage of low-SES friends over the share of low-SES peers in the program and entry cohort. Values close to one suggest a high friendship bias that favors links with other high-SES peers, whereas values close to zero suggest no friendship bias. Similarly, values below zero suggest a bias that favors links with low-SES students beyond their representation in the group.

The friendship bias analysis suggests that in the 2015-1 cohort, there is a pattern of favoring links within the same SES group, regardless of the share of students in the program and cohort. Figure 13 plots the estimated friendship bias for programs in the 2014 and 2015-1 cohorts relative to the percentage of low-SES peers in each group. These results suggest a large variation in friendship bias in the cohorts enrolling before SPP and no relation with the percentage of low-SES students in the group. The red dots, which plot the estimated friendship bias in the 2015-1 entry cohort, suggest that the variation in the bias diminished, with no visible relationship with the percentage of low-SES students. The estimated friendship bias does seem to be consistently over one, with only a few exceptions. Coupling these results with those in Panel III of Table 11, I conclude that

[15] Chetty et al. (2022) define friendship bias as "the tendency for people with low SES to befriend people with high SES at lower rates even conditional on exposure".

interactions between low- and high-SES students did increase but that the response was not proportional to the new shares of low-SES peers in the group and a bias for friendships among high-SES peers persisted.

These results complement previous findings in the literature examining diversity in social interactions. Scholars have found positive impacts of increased diversity in schools using measures of interaction intensity captured by email exchanges (Marmaros and Sacerdote, 2006) and survey questions about the willingness to interact with racially and ethnically diverse groups (Boisjoly et al., 2006; Rao, 2019). My results provide a finer disaggregation of the effects by distinguishing the impacts on the probability and the number of interactions with peers in the same group. My findings also complement those of Mayer and Puller (2008) and Baker, Mayer and Puller (2011), who examine the changes in the composition of friendships with a measure of interactions bounded to peers in the same college group.[16] My findings add to this literature by showing that diversity in group composition increases interactions among students from different socioeconomic groups at both the intensive and extensive margins, despite the persistent bias toward friendships in the same group.

7 Role of Academic Achievement in the Diversity of Social Interactions

In this section, I examine how my results are explained by the relationship between academic achievement and interactions between different socioeconomic groups. Previous research (Arcidiacono and Vigdor, 2010; Epple and Romano, 2011) has found that exposure to low-achieving peers has a negative impact on student performance. Work by

[16] Mayer and Puller (2008) and Baker, Mayer and Puller (2011) use data from Facebook to study whether students' friendships on that social media platform become more diverse when the students are exposed to diverse peers in their dorms. The authors argue that the effects on the diversity of friendships are small. In contrast to my measure of social interactions, their measure of social networks is not bounded to peers from college.

Carrell, Sacerdote and West (2013) suggests that such exposure effects are shaped by the social interactions among the students. Specifically, a lack of interaction between students of different achievement levels predicts a lack of peer effects within a group. In this paper, low- and high-SES students enrolling after SPP show significant performance gaps, with low-SES students underperforming their high-SES peers. I do not find decreases in high-SES students' performance with exposure to more low-SES peers, but I do find that the two groups are socially interacting.

Could the increased interactions between low- and high-SES students be driven by interactions with high-achieving low-SES students? While low-SES students are on average lower performers than their high-SES counterparts, there is important variation in the distribution of scores among the two groups. In fact, in the 2015 cohort, 27 percent of the low-SES students had SABER 11 test scores equal to or above the average scores of their high-SES peers. I use this fact to identify high-achieving low-SES students and measure how much of the changes in social interactions was driven by interactions with students of this type. I use three measures of performance: SABER 11 test scores, first-term GPA, and credits attempted in the first term. I flag a low-SES student as a high achiever if his or her performance is equal to or above the average performance of the high-SES students in the program and cohort. GPA or credits attempted run the risk of being endogenous, as these measures could be the result of interactions that happened early in the first term. The SABER 11 exam is taken before college enrollment, which makes it independent of first-term social interactions, but it might be harder for the students to observe.

Table 12 displays the results. Overall, my estimates suggest that the increased interactions between low- and high-SES students are partially driven by links formed with high-achieving low-SES students. The estimated effect on the number of links with low-SES high-achieving students (Panel II) is 0.287 links when the share of low-SES peers is over 24 percent and when the achievement measure is SABER 11 test scores. This suggests that approximately 46 percent of the increase in low-SES links estimated in Table 11 can be explained by links with high-achieving students; this figure increases to 90 percent when I use as measure of achievement the number of credits attempted in the first term. The estimated probability of interaction with high-achieving students remains similar to that for all low-SES peers, with a higher estimated probability when I define high achievers by their credits attempted. These results suggest that while, on the extensive margin, the precollege achievement of low-SES students does not change the probability of interactions with high-SES peers, approximately half of the links formed between high- and low-SES students are links with high-achieving low-SES ones. These effects are more pronounced when I use achievement measures potentially observed by students such as the number of credits that students attempted in their first term, although these measures are subject to endogeneity, as credits attempted may also be the result of links formed early in the semester.

8 Conclusion

In this paper, I study how changes in the socioeconomic composition of students at an elite university impacted students' academic achievement, and in doing so, I shed light on how social interactions diversify and how the result is explained by students' performance. I exploit variation in the share of low-SES students driven by *Ser Pilo Paga*, a financial aid program in Colombia targeting high-achieving low-income students. To measure social interaction, I leverage records on turnstiles located across the 18 college campus entrances and develop a measured based on students' comovements across campus.

I summarize the findings from this paper in three points. First, the increased exposure to low-SES peers had no effect on the academic achievement of high-SES students, as measured by their cumulative GPA, number of credits attempted, or on-time graduation. Second, the increased exposure to low-SES peers led to more interactions between high- and low-SES students, albeit with some persistent bias for interactions among high-SES students. Third, at least half of the increase in interactions with low-SES peers is explained by interactions with high-achieving low-SES peers (that is, students with a performance equal to or above the average of that of their wealthy peers in the same major and entry cohort), but I do not find evidence that, at the extensive margin, the probability of interacting with this type of student is different from that of interacting with any low-SES peer.

These findings provide evidence of how socioeconomic desegregation of elite colleges can impact students within the institution. Similarly to Angrist and Lang (2004) and Bleemer (2021a), I show that there are no adverse impacts on the achievement of students from the privileged demographic groups traditionally attending these institutions. Moreover, I show that the lack of peer effects is not explained by segregation between wealthy and low-income students within the groups. Coupled with the findings from Londoño-Velez, Rodriguez and Sanchez (2020) and Londoño-Vélez et al. (2023) that SPP effectively increased higher education access and completion among the low-income students that it aimed to benefit, my findings contribute to the evidence on the positive effects of affirmative action and financial aid targeted to low-income students, showing that this policy did not harm the achievement of students from the demographic groups that traditionally concentrate in elite institutions while it did diversify their interactions.

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Appendix A: Turnstile-Elicited Interaction Data and Validation

Validation of student links definition. I define a time window and frequency thresholds by comparing the turnstile-elicited and survey-elicited links among first-term undergraduate economics students in the fall 2017 cohort. The survey was conducted online between December 7, 2017, and January 5, 2018, and elicited the network information of 110 economics students from the fall 2017 cohort. The survey was conducted with Qualtrics. Students who completed the survey received a free lunch voucher for a recognized chain restaurant in the campus area. Cárdenas et al. (2022) provide a detailed description of the survey.[17] The survey inquired about two types of links: friendships and acquaintances. Table 1 shows the results of the comparison. The time windows tested in Table 1 were selected based on in-person observations of different entrances. The observations of campus entrances were conducted between August 26 and 30, 2019. Because there are multiple turnstiles at each entrance, students walking together can essentially swipe their IDs simultaneously using different scanners, hence the choice of short time windows. I select the time window and the frequency criterion by minimizing the sum of the type II and type I measurement errors—that is, the number of unmatched survey-elicited links over the total number of survey links and the number of unmatched turnstile-elicited links over the total number of survey links. For the purposes of this test, I assume that the true numbers of links to which the type I and II errors refer are those captured by the survey.

To illustrate how to interpret the results in Table 1, I ask the reader to focus on the time window of three seconds and the survey-reported acquaintance links. The numbers in bold indicate the combinations of time windows and frequencies that minimize the sum of type I and II errors for each type of link. Thus, the frequency with which I should

[17] I am very grateful to Professor Tomás Rodríguez-Barraquer for providing access to these data.

observe two student IDs swiped on a turnstile entrance such that it resembles an acquaintance link should be a minimum of twice in the semester. Under this rule, the likelihood of type I error or a false positive—i.e., the likelihood of defining a pair of students as linked when, according to the survey, they are not—is 11 percent. Conversely, the likelihood of type II error or a false negative—i.e., the likelihood of not identifying a pair of students as acquaintances when, according to the survey, they are—is 62 percent. While a five-second window and a frequency of three comovements in the term would yield a lower sum of errors, it would do so by leaving one student from the 110 in the sample without turnstile-based link information—an omission that I want to avoid. Notice that the acquaintance criterion has a lower threshold in terms of frequency of comovements in the semester than the friendship criterion. I choose to use the acquaintance instead of the friendship criterion because it allows me to identify social interactions that students did not identify as friendships in the first term of college but that may eventually evolve into such.

The results in Table 1 indicate that, under the baseline definition, it is highly likely that the turnstile-elicited links capture links similar to those reported in the survey. However, an important share of the survey-reported links may not be captured by the turnstiles. This could be an issue to the extent that those that I do capture are not representative of the survey-elicited links. To assess this, I compare whether the turnstile-elicited links plausibly reflect the survey-elicited network characteristics. The results are displayed in Figure 1. The goal of this exercise is to estimate how far from random are the characteristics of the turnstile-elicited links and how close the average characteristics of the links are to those of the survey-elicited links. The computation proceeds as follows: I use the acquaintances derived by minimizing the criteria from Table 1 for each of the time windows and randomly assign the number of turnstile-elicited links derived under those criteria to the 110 students in the sample. Then, I compute the average of the following network individual attributes: age difference, number of courses that the students take together, GPA difference, degree or number of links, and local clustering. I conduct this procedure 1,000 times and plot the distribution of the characteristics. I include the average value that I observe for the turnstile- and survey-elicited links with the 95 percent confidence interval. I find statistically significant support indicating that the turnstile-elicited network characteristics closely resemble those of the friendship and acquaintances networks elicited by the survey and are not the result of random link formation.

The validity of the turnstile-elicited interaction data could be susceptible to the hours of the day during which comovements are captured. Comovements captured around lunch hours could be more susceptible to false negatives, whereas comovements captured at other times may be less susceptible to false positives. I test the extent to which this is an issue by replicating the comparison with the survey-elicited interactions from Table 1 but for comovements happening around lunchtime hours (from 11:40 am to 2:20 pm) with comovements at other times. The results are displayed in Table 2. For simplicity, I focus on acquaintance links and on the two- and three-second windows. As expected, comovements captured during lunchtime are more susceptible to false negatives than comovements captured outside lunch hours. Similarly, comovements captured at lunchtime are less susceptible to false positives than those captured at other times. However, the sum of error rates is much higher for either of these time categories individually than that obtained when I use all times pooled together, as presented in Table 1. These results suggest that searching for comovements at any time of the day is more reliable in terms of reducing measurement error than focusing on comovements happening at specific times of the day.

Table 1: Comparison of survey– and turnstile–elicited links

Time Window	A. two seconds					B. Three seconds					C. Five seconds				
Frequency	One	Two	Three	Four	Five	One	Two	Three	Four	Five	One	Two	Three	Four	Five
1. Turnstiles–Elicited															
No. Of dyads	868	368	235	180	148	1,209	509	314	251	198	1,906	898	552	401	315
No. of students	110	110	108	107	105	110	110	109	108	107	110	110	109	108	108
2. Survey–Elicited															
I. Students are Friends															
Dyads			505					505					505		
Matched	342	256	201	165	140	389	305	248	215	179	433	368	337	295	263
False Negatives (Type II)	0.32	0.49	0.60	0.67	0.72	0.23	0.40	0.51	0.57	0.65	0.14	0.27	0.33	0.42	0.48
False Positives (Type I)	1.04	0.22	0.07	0.03	0.02	1.62	0.40	0.13	0.07	0.04	2.92	1.05	0.43	0.21	0.10
Sum of Errors	1.36	0.71	**0.67**	0.70	0.74	1.85	0.80	**0.64**	0.65	0.68	3.06	1.32	0.76	0.63	**0.58**
II. Students are Acquaintances															
Dyads			1,033					1,033					1,033		
Matched	497	311	219	174	144	606	391	284	235	191	734	537	425	348	293
False Negatives (Type II)	0.52	0.70	0.79	0.83	0.86	0.41	0.62	0.73	0.77	0.82	0.29	0.48	0.59	0.66	0.72
False Positives (Type I)	0.36	0.06	0.02	0.01	0.00	0.58	0.11	0.03	0.02	0.01	1.13	0.35	0.12	0.05	0.02
Sum of Errors	0.88	**0.75**	0.80	0.84	0.86	1.00	**0.74**	0.75	0.79	0.82	1.42	0.83	**0.71**	0.71	0.74

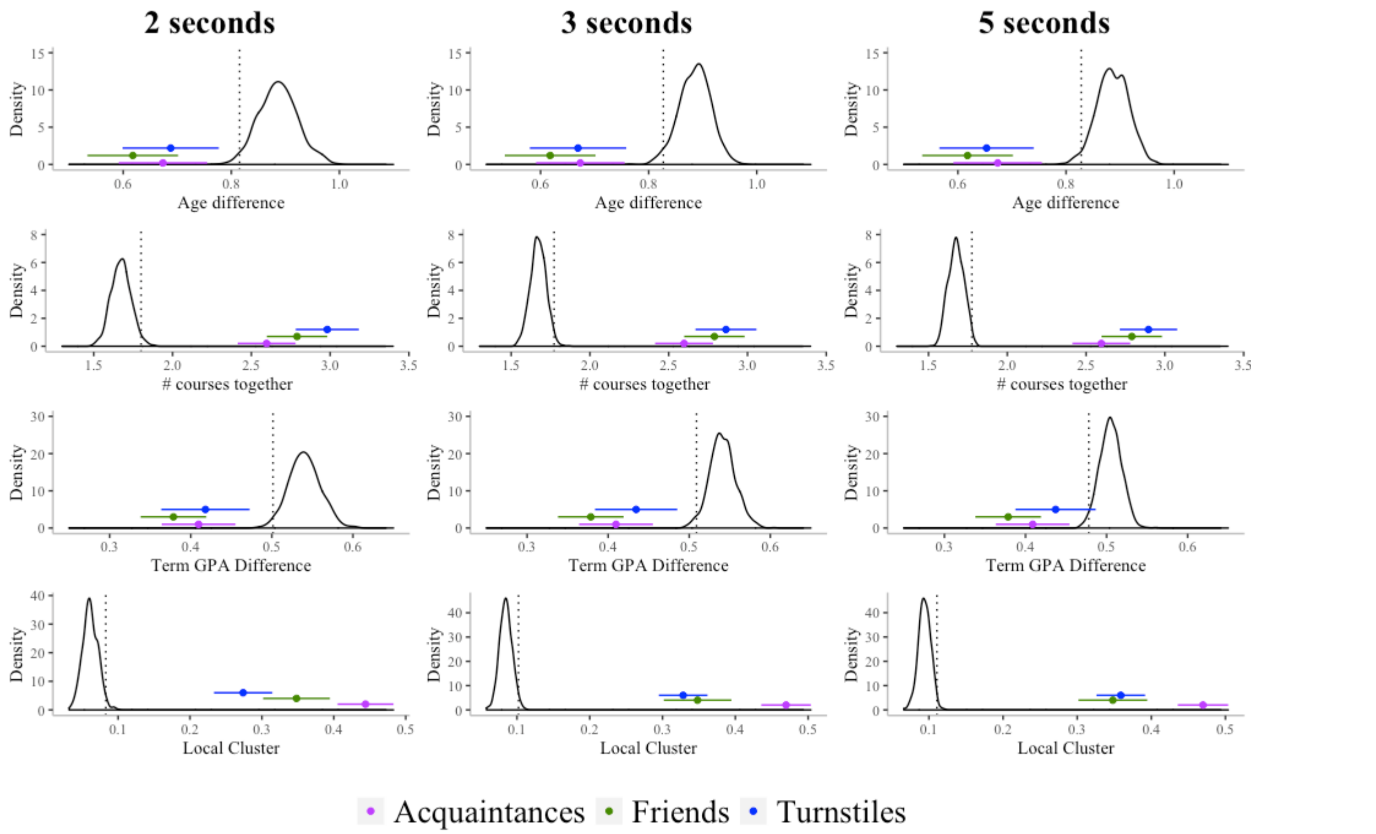
Note: N students = 110. Number of links possible (N*(N-1))/2 = 5995. Survey sample consist of economics undergrads from the August 2017 cohort; 113 students surveyed. One student did not report information, and two do not appear enrolled as of 2017-2. The survey asked each student who among the 113 students was an acquaintance and, among those, who was considered a friend. The type II error rate is the share of links in the survey that were not found in the turnstile-based links. The type I error is the links in the turnstile data that were not matched with links in the survey over the total links in the survey.

Table 2: Comparison of survey- and turnstile-elicited links during and outside lunch hours

Time window	A. Two seconds						B. Three seconds					
Type	11:40 am to 2:20 pm			Other times			11:40 am to 2:20 pm			Other times		
Frequency	One	Two	Three	One	Two	Three	One	Two	Three	One	Two	Three
1. Turnstiles												
No. Of dyads	397	159	100	654	272	172	554	213	135	893	376	233
No. of students	110	109	103	110	110	105	110	109	106	110	110	106
2. Students are Acquaintances												
Dyads	*1,033*						*1,033*					
Matched	255	143	93	411	236	162	321	180	123	494	308	214
False Negatives (Type II)	0.75	0.86	0.91	0.60	0.77	0.84	0.69	0.83	0.88	0.52	0.70	0.79
False Positives (Type I)	0.14	0.02	0.01	0.24	0.03	0.01	0.23	0.03	0.01	0.39	0.07	0.02
Sum of Errors	0.89	**0.88**	0.92	0.84	**0.81**	0.85	0.91	**0.86**	0.89	0.91	**0.77**	0.81

Note: N students = 110. Number of links possible (N*(N-1))/2 = 5995. Survey sample consist of economics undergrads from the August 2017 cohort; 113 students surveyed. One student did not report information, and two do not appear enrolled as of 2017-2. The survey asked each student who among the 113 students was an acquaintance and, among those, who was considered a friend. The type II error rate is the share of links in the survey that were not found in the turnstile-based links. The type I error is the links in the turnstile data that were not matched with the links in the survey over the total links in the survey.

Figure 1: Comparison with randomly generated distribution



Note: Turnstile-elicited links matched with the survey links are randomly assigned in 1,000 draws among 110 students, forming all possible 5,595 dyads. The 95 percent confidence intervals are presented. Matches for a 2-second time window with 2 comovements: 368 links. Matches for 3-second time window with 2 comovements: 368 links. Matches for a 5-second time window with 3 comovements: 552 links. The dotted vertical lines indicate the 95 percent confidence points.

Measurement error in difference-in-difference framework. To build understanding of the role of measurement error in the social interaction data, I build on a potential outcomes framework in a 2x2 difference-in-difference research design as coined by Goodman-Bacon (2019) and discussed by Cunningham (2021). Define t as a treated group (i.e., a group with a large R^l_{mc}) and u as an untreated group:

$$\hat{\alpha}_P^{2x2} = (E[L_t|Post] - E[L_t|Pre]) - (E[L_u|Post] - E[L_u|Pre]) \tag{2}$$

In Equation 2, the estimated $\hat{\alpha_P}^{2x2}$ is written as the difference between the expected post- and pretreatment values of the outcome L for the treated group t $(E[L_t|Post] - E[L_t|Pre])$ minus the difference between the expected post- and pretreatment values of the outcome L for the untreated group u $(E[L_u|Post] - E[L_u|Pre])$. Equation 2 can be rewritten in potential outcome terms. Define L^0 as the potential outcome had no treatment been assigned and L^1 as the potential outcome had the treatment been assigned. Hence, the estimated $\hat{\alpha_P}^{2x2}$ can be rewritten as:

$$\begin{aligned}\hat{\alpha_P}^{2x2} = {} & \underbrace{E[L_t^1|Post] - E[L_t^0|Post]}_{\text{ATT}} \\ & + \underbrace{\underbrace{(E[L_t^0|Post] - E[L_t^0|Pre])}_{\text{Treatment counterfactual}} + (E[L_u^0|Post] - E[L_u^0|Pre])}_{\text{nonparallel trend bias==0}}\end{aligned} \tag{3}$$

Equation 3 implies $\hat{\alpha_P}^{2x2}$ is composed of the average treatment effect on the treated (ATT), which is the difference between the expected values of the outcome L in the post-treatment period for the treated group t had the treated group received and not received the treatment, and the nonparallel trend bias. The nonparallel trend bias is the difference in the potential outcomes for the treated and untreated group had no treatment been assigned to either group. I showed in Section 4 that there is no evidence of nonparallel trend bias. However, if the measurement error is associated with the treatment in ways unobserved by the researcher, then the estimated ATT based on the observed outcome may differ from the true ATT that I aim to estimate.

To fix ideas, define the number of links that I aim to measure as $L^{true} = L^{obs} - L^{F(+)} + L^{F(-)}$. That is, true links can be defined as the number of observed links L^{obs} minus the turnstile-elicited links that are false positives $L^{F(+)}$ plus the number of true links not captured by the turnstile-elicited measure $L^{F(-)}$, i.e., the false negatives. Then, the ATT that I aim to estimate is:

$$ATT^{estimated} = E[L_t^{1,True}|Post] - E[L_t^{0,True}|Post] \tag{4}$$

Replacing $L_t^{1,True}$ and $L_t^{0,True}$ with their equivalents based on observed L and doing some rearrangement of terms, I obtain:

$$\begin{aligned} ATT^{estimated} &= E[L_t^{1,obs} - L_t^{1,F(+)} + L_t^{1,F(-)}|Post] - E[L_t^{0,obs} - L_t^{0,F(+)} + L_t^{0,F(-)}|Post] \\ &= \underbrace{E[L_k^{1,obs}|Post] - E[L_k^{0,obs}|Post]}_{\text{Observed ATT}} + \\ &\underbrace{E[L_t^{1,F(-)} - L_t^{1,F(+)}|Post] - E[L_t^{0,F(-)} - L_t^{0,F(+)}|Post]}_{\text{Measurement Error Bias}} \end{aligned} \tag{5}$$

Thus, the estimated ATT can be rewritten as the ATT based on the observed outcome L^{obs} plus a measurement error bias, which can be described as the ATT on $L^{F(-)}$ minus the ATT on $L^{F(+)}$:

$$ATT^{estimated} = ATT^{obs} + \underbrace{E[L_t^{1,F(-)} - L_t^{0,F(-)}|Post]}_{\text{ATT on F(-)}} - \underbrace{E[L_t^{1,F(+)} - L_t^{0,F(+)}|Post]}_{\text{ATT on F(+)}} \tag{6}$$

Equation 6 implies that if the treatment has no impact on $L^{F(-)}$ or $L^{F(+)}$ among the treated, then $ATT^{estimated} = ATT^{obs}$. In what follows, I discuss and test this implication in the context of my research design.

Ideally, I would have data on the measurement error variables $L^{F(-)}$ and $L^{F(+)}$ across different majors and cohorts such that I could use variation in the treatment R^l_{mc} to assess its effects. Since I do not have data of this nature, I rely on proxy variables that can help me assess the extent to which the treatment R^l_{mc} may lead to measurement error in the turnstile-elicited interaction data. I use two variables to assess measurement error: first, the total number of ID swipes at the turnstiles for each student, and second, the number of courses with turnstile-elicited links. I measure both proxies for the same terms for which I measure interactions (i.e., the sixth and seventh terms after first enrollment).

Intuitively, if the treatment leads to more ID swipes at the turnstiles, the chances of capturing false positives $L^{F(+)}$ in the treated group increases. Similarly, if the treatment leads to fewer ID swipes, the chances of missing true links $L^{F(-)}$ in the treated group increases. Likewise, if treatment N_{Pmc} is associated with a higher number of classes taken, the turnstile-elicited link data may be more susceptible to including false positives $L^{F(+)}$. Classes in the sixth and seventh terms may be more diverse due to the treatment, but the social interactions captured may be the product of chance. That is, wealthy students could be attending courses with low-SES peers and coinciding in comovements at the entrances without this implying true social interaction.

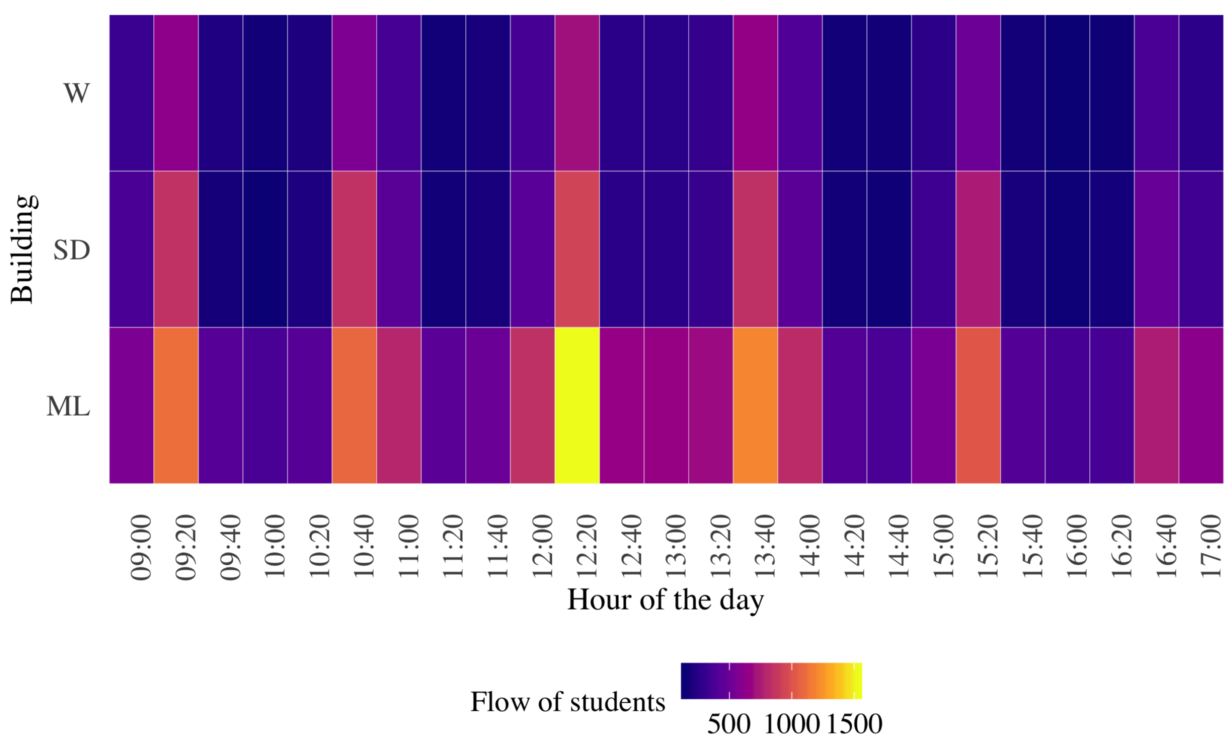
Table 3 displays the results of regressing R^{l}_{mc} on the measurement error proxies under each time window considered. The estimation follows the same structure as that of Equation 1 but uses the proxy variables on the left-hand side. I do not find statistically significant evidence of a change in the number of ID swipes at the turnstiles or in the number of courses with turnstile-elicited links due to exposure to low-income peers. Coupling these findings with the previous results, I conclude that there is no evidence to claim that measurement error biases the estimated effects of exposure to low-income peers on student interactions or academic achievement.

Table 3: ATT on Measurement Error Proxies

	ID Swipes	No. of Courses in the semester interacting with peers in: Two Seconds	Three Seconds	Five Seconds
	(1)	(2)	(3)	(4)
Percentage of Low–Income Peers	-416.225	0.060	-0.186	-0.092
	(269.122)	(0.390)	(0.428)	(0.396)
Pre-treatment Statistics for the Outcomes				
Mean	1340.192	1.091	1.118	1.135
Standard Deviation	1017.184	1.367	1.399	1.414
No. of Students	5,278	5,278	5,278	5,278
No. of Major-Cohort groups	124	124	124	124

Note: Results from estimating Equation 1 using measurement error proxies on the left-hand side. "ID swipes" is the total number of ID swipes of each student, either to enter or exit campus, in the sixth and seventh terms after first enrollment. "No. of courses with peers interacted" is the total number of courses that the student took with the peers whom I identify as turnstile-elicited links. All estimations include fixed effects by major and entry cohort and the covariates described for Equation 1. All standard errors are clustered at the major–cohort level.

Figure 2: Flow of students at selected entrances – Term and hour according to turnstiles



Note: Average number of swipes by day, entrance and 20-minute bloc. Swipes include building entries and exits. Only weekdays during the official academic calendar are included in the data.

Appendix B: Robustness and Validity Checks

Table 4: Effects of increased exposure to low-SES peers on students' social interactions

(turnstile–elicited interactions based on a two-second window)

	I. Probability of a Link with a:		II. Number of Links with:			III. % of Links with:
	(1) Low–SES	(2) High–SES	(3) Low–SES	(4) High-SES	(5) Any Student	(6) Low–SES
A. Continuous Treatment						
Percentage of Low–SES Peers	0.007***	–0.001	0.026***	–0.029*	–0.003	0.719***
	(0.001)	(0.001)	(0.003)	(0.016)	(0.015)	(0.080)
Mean Increase (18.0 points)	0.126	–0.018	0.468	–0.522	–0.054	12.942
B. 50th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 24%]	0.097***	–0.028	0.420***	–0.592	–0.172	13.841***
	(0.035)	(0.035)	(0.106)	(0.437)	(0.415)	(2.632)
C. 75th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 36%]	0.111**	–0.037	0.609***	–1.005*	–0.396	17.179***
	(0.044)	(0.048)	(0.111)	(0.555)	(0.540)	(3.886)
Outcomes Pre-treatment Statistics						
Mean	0.157	0.736	0.193	3.892	4.085	4.330
Standard Deviation	0.364	0.441	0.494	4.019	4.213	11.511
No. of Students	4,027	4,027	4,027	4,027	4,027	3,011
No. of Major-Cohort groups	93	93	93	93	93	90

Note: A socially interacting pair of students is defined when their IDs are swiped at a turnstile at the same entrance and in the same direction within two seconds or less and at least twice during a semester. Panel I outcomes are indicators equal to one when the student has interacted with at least one low- or high-SES peer. Panel II uses the number of peers whom the student has interacted with, and Panel III uses the percentage of links that are low SES. Panel A displays the results of estimating Equation 1. Panels B and C display the results based on a dummy variable for programs with low-SES student shares above the median and the 75th percentile, respectively. All regressions control for a female indicator, age in years at the time of entry, SB11 standardized test scores, mother without a college degree, socioeconomic stratum two or three (i.e., intermediate SES), receipt of an SPP loan, and number of high-school peers enrolled in the same cohort as the student. All standard errors are clustered at the program–cohort level. *** p <0.01, ** p <0.05, *, * p<0.1.

Table 5: Effects of increased exposure to low-SES peers on students' social interactions

(turnstile-elicited interactions based on a five-second window)

	I. Probability of a Link with a:		II. Number of Links with:			III. % of Links with:
	(1) Low–SES	(2) High–SES	(3) Low–SES	(4) High-SES	(5) Any Student	(6) Low–SES
A. Continuous Treatment						
Percentage of Low–SES Peers	0.008***	–0.002	0.039***	–0.036**	0.003	0.752***
	(0.001)	(0.001)	(0.004)	(0.018)	(0.019)	(0.056)
Mean Increase (18.0 points)	0.144	–0.036	0.702	–0.648	0.054	13.536
B. 50th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 24%]	0.110***	–0.034	0.717***	–0.856*	–0.139	15.086***
	(0.040)	(0.028)	(0.167)	(0.477)	(0.498)	(2.318)
C. 75th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 36%]	0.141***	–0.069**	1.042***	–1.287**	–0.245	18.707***
	(0.049)	(0.033)	(0.163)	(0.583)	(0.655)	(3.057)
Outcomes Pre-treatment Statistics						
Mean	0.180	0.752	0.231	4.771	5.002	4.386
Standard Deviation	0.384	0.432	0.548	4.816	5.040	11.145
No. of Students	4,027	4,027	4,027	4,027	4,027	3,079
No. of Major-Cohort groups	93	93	93	93	93	91

Note: A socially interacting pair of students is defined when their IDs are swiped at a turnstile at the same entrance and in the same direction within five seconds or less and at least three times during a semester. Panel I outcomes are indicators equal to one when the student has interacted with at least one low- or high-SES peer. Panel II uses the number of peers whom the student has interacted with, and Panel III uses the percentage of links that are low SES. Panel A displays the results of estimating Equation 1. Panels B and C display the results based on a dummy variable for programs with low-SES student shares above the median and the 75th percentile, respectively. All regressions control for a female indicator, age in years at the time of entry, SB11 standardized test scores, mother without a college degree, socioeconomic stratum two or three (i.e., intermediate SES), receipt of an SPP loan, and number of high-school peers enrolled in the same cohort as the student. All standard errors are clustered at the program–cohort level. *** p <0.01, ** p <0.05, *, * p<0.1.

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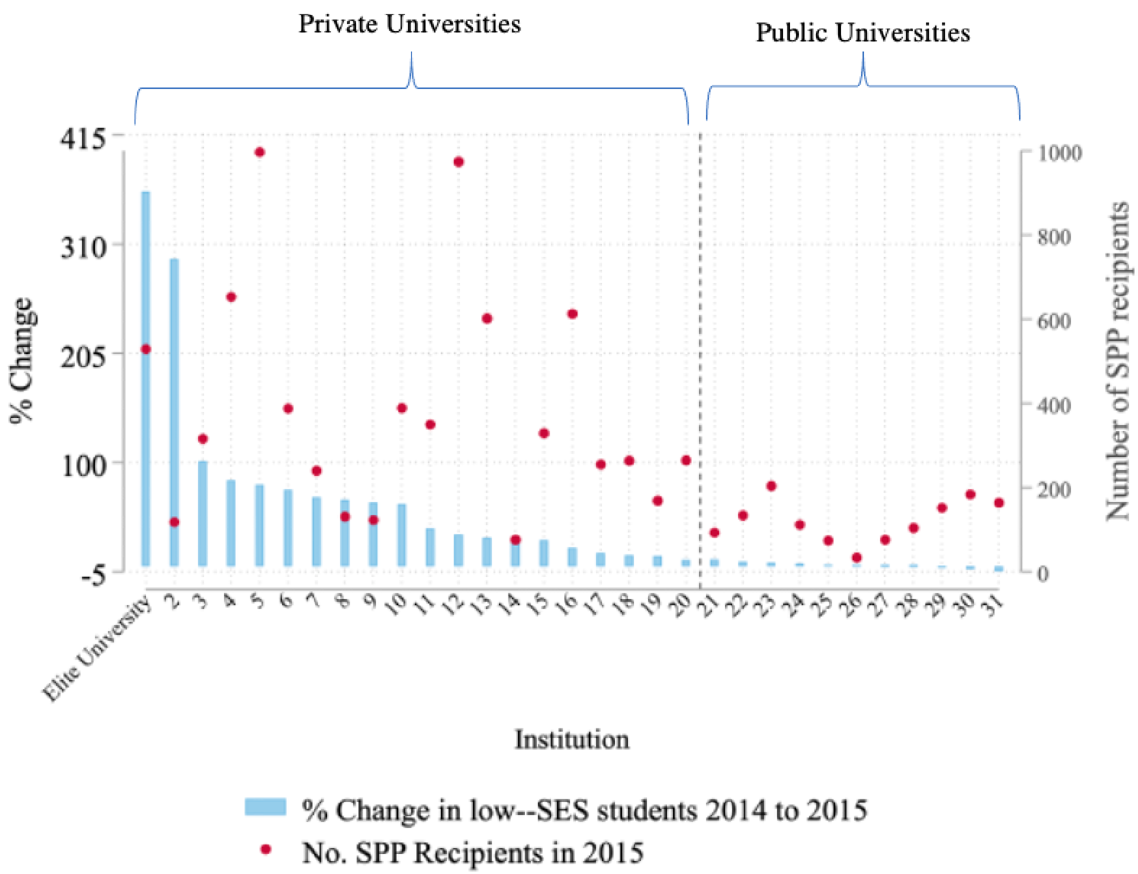
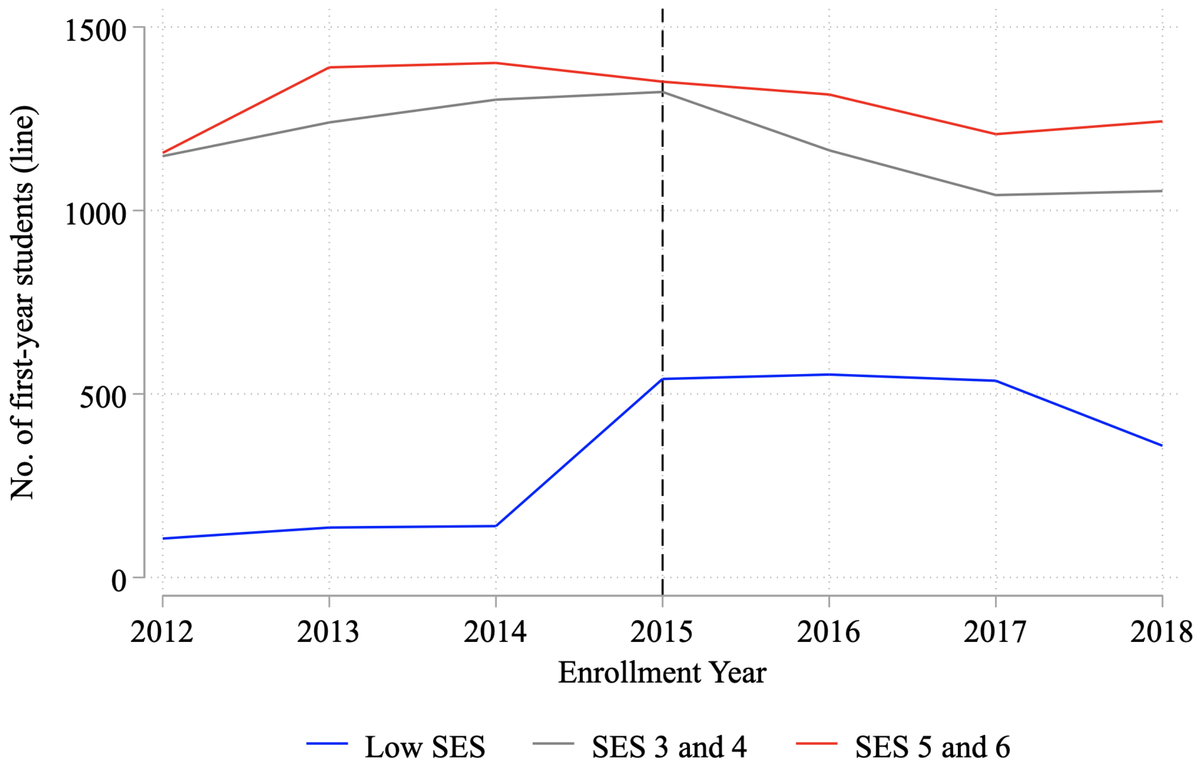
Figures

Figure 3: Change in the percentage of low-SES students across SPP-eligible universities



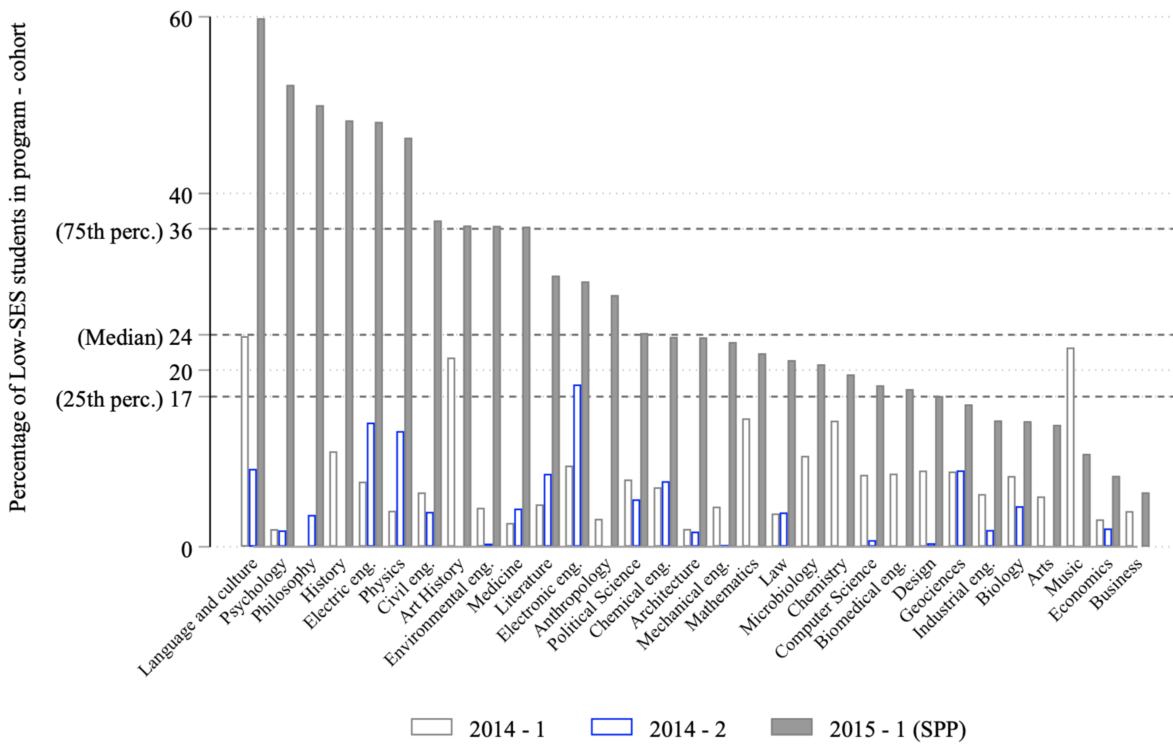
Note: This figure displays the percentage change in the number of low-SES students enrolling between the 2014 and 2015 entry cohorts at each of the SPP-eligible universities in Colombia. Calculations are based on publicly available data from the National Ministry of Education.

Figure 4: Number of first-term students by SES



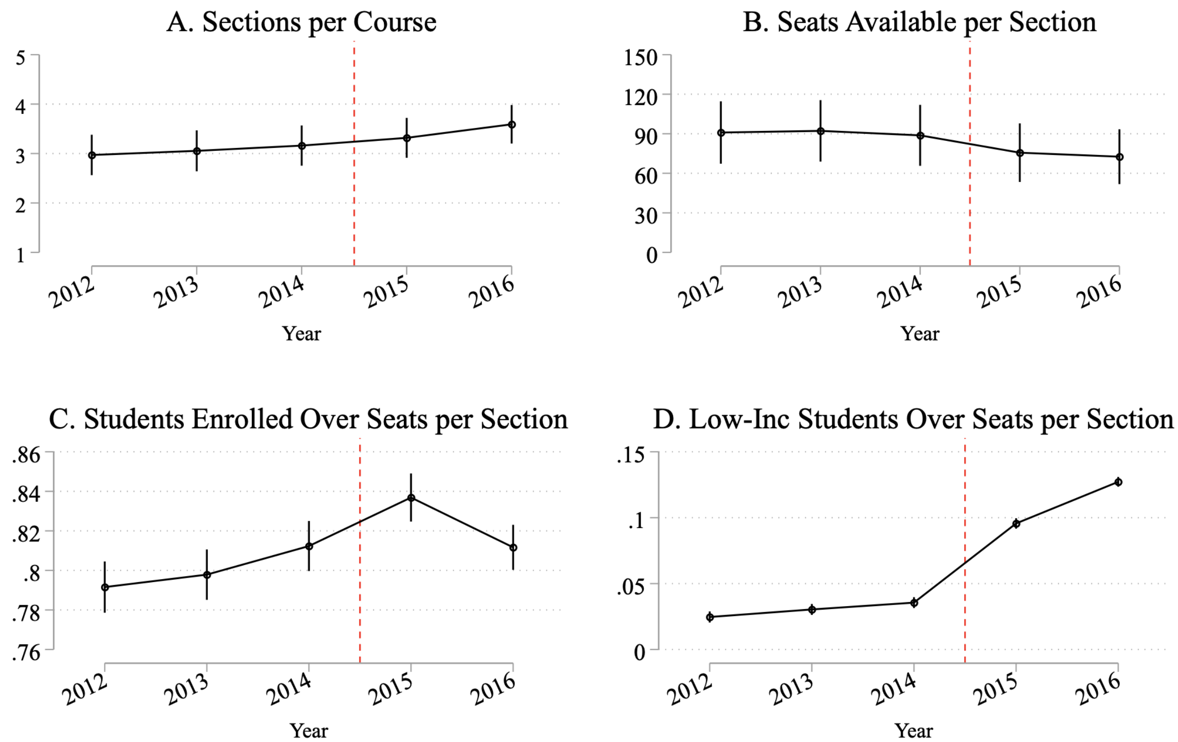
Note: This figure displays the total number of first-term students by SES group. Students are classified into three SES groups based on their housing stratum indicator. High-SES students are those from socioeconomic strata three to six, while low-SES students are those from socioeconomic strata one and two. I included both spring and fall enrollments per year. The dotted vertical line marks the start of SPP.

Figure 5: Percentage of low–SES students by major and before and after SPP



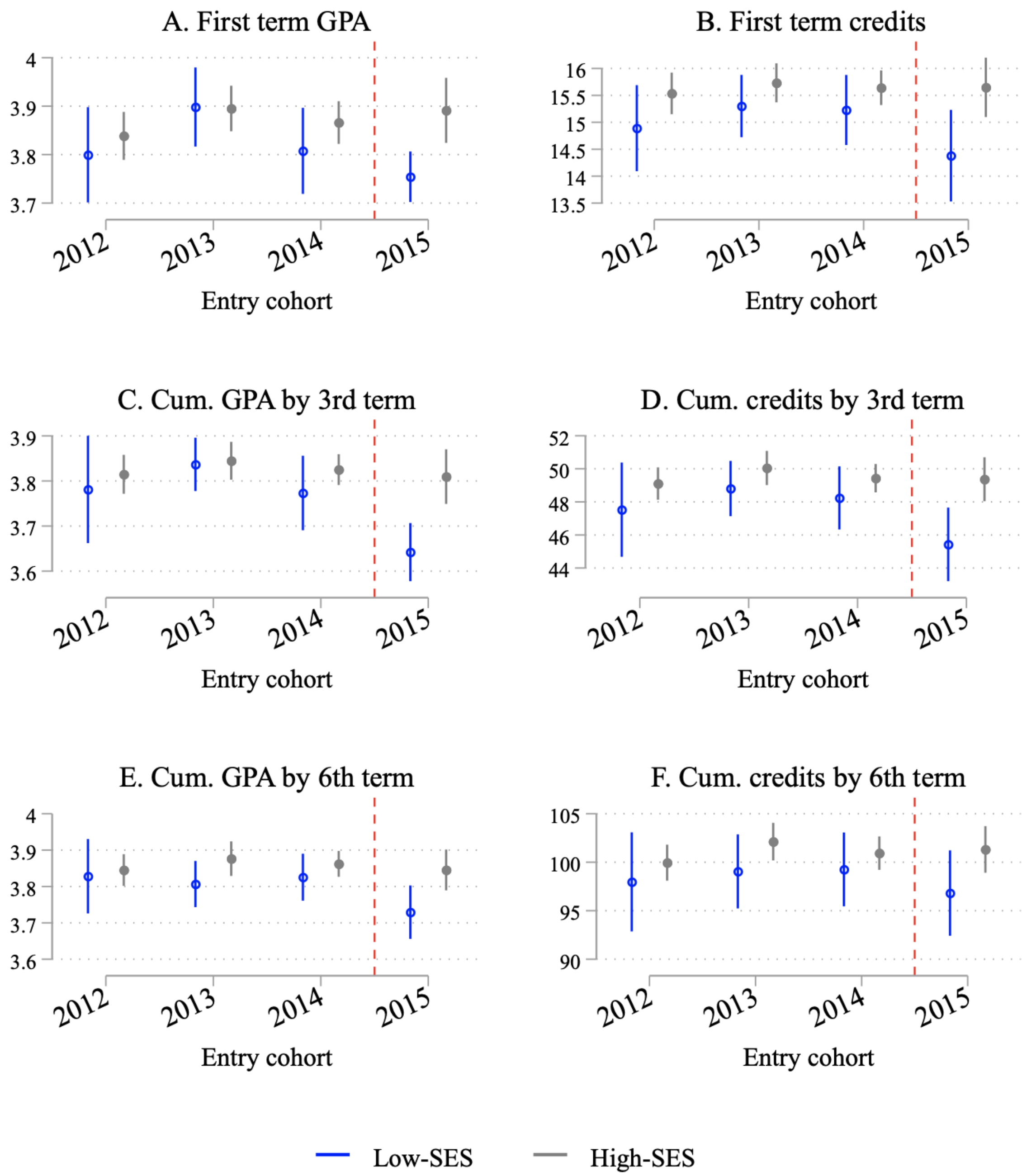
Note: This figure displays the percentage of low-income students by program and entry cohort period. The dotted horizontal lines indicate the 25th, 50th (median), and 75th percentile values of the distribution of low-SES students during the SPP period, 2015-1.

Figure 6: Composition of courses taken by first-term students in each entry cohort



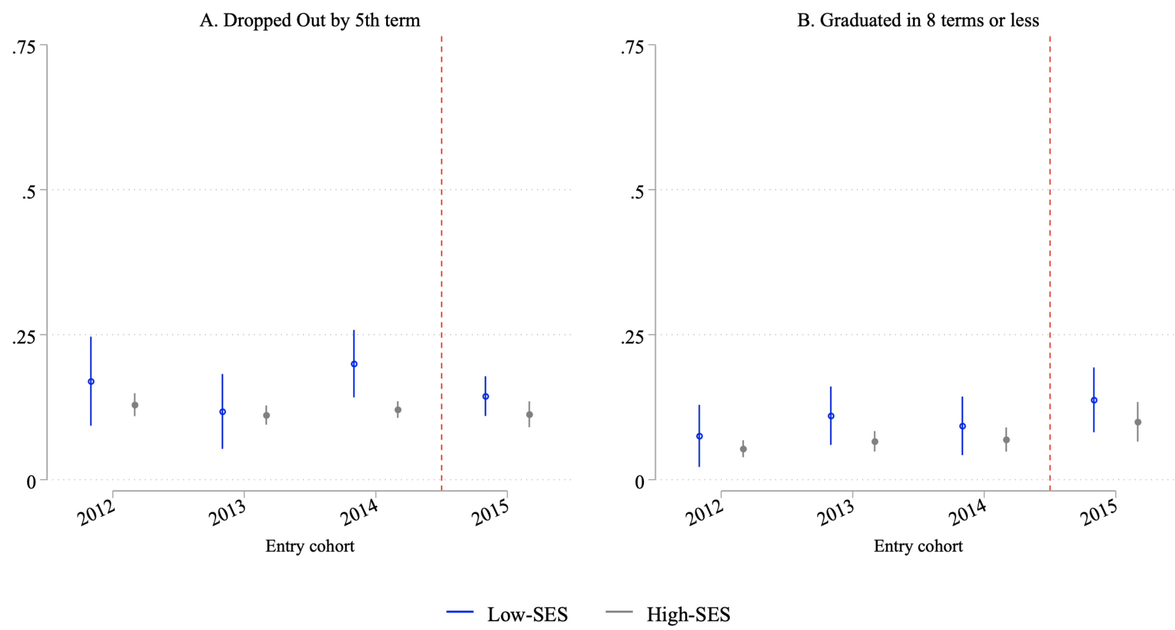
Note: This figure illustrates the composition of courses and classrooms attended by first-term students in each entry cohort. Panel A shows the average number of sections per course, Panel B shows the average number of available spots or seats per section, Panel C shows the ratio of enrolled students to the total number of available seats, and Panel D shows the ratio of low-SES students enrolled to the total number of available seats. In each panel, every point represents results from an OLS regression with no constant and dummies by entry year; 95 percent confidence intervals are plotted as a vertical line on each point. The data were previously aggregated at the section–course–term level. The dotted red line separates the cohorts that enrolled before the start of SPP (2014 and earlier) from those that enrolled during SPP (2015 onward).

Figure 7: Achievement gaps between high- and low-SES students by entry cohort



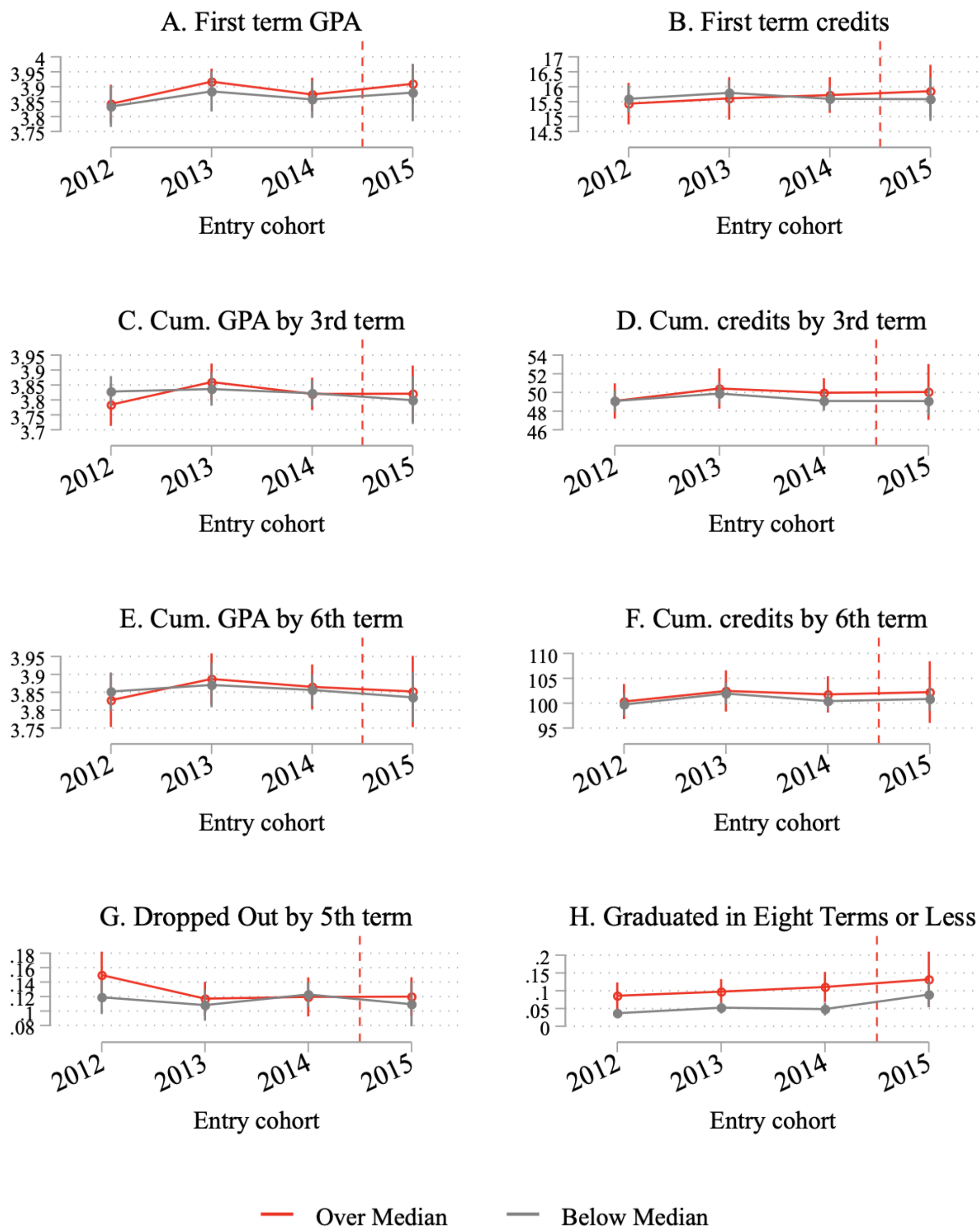
Note: These graphs display the point estimates of a cohort dummy variable from an OLS regression with no intercept, where the dependent variable is the student's GPA, ranging from one to five, with five being the highest grade, or the number of credits attempted. The 95 percent confidence intervals are shown as vertical lines on each dot and are based on clustered standard errors at the program–cohort level. Each yearly entry cohort includes the spring and fall cohorts of the respective calendar year. The dotted red line separates the cohorts that enrolled before the start of SPP (2014 and before) from the first SPP cohort, 2015-1.

Figure 8: Persistence gap between high and low-SES students by entry cohort



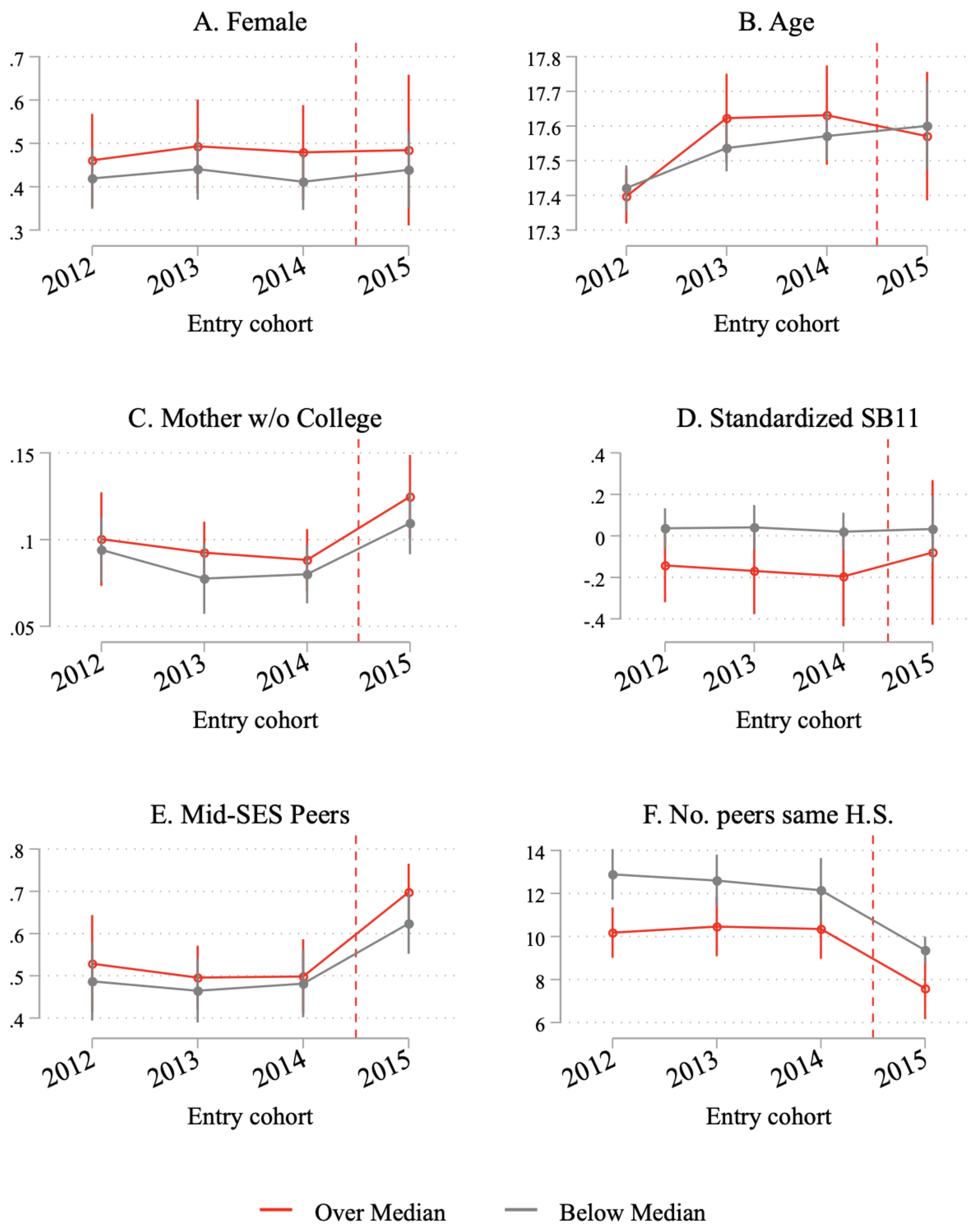
Note: These graphs display the point estimates of a cohort dummy variable from an OLS regression with no intercept, where the dependent variable is an indicator equal to one if the student dropped out by the 5th term or graduated in fewer than eight terms. The 95 percent confidence intervals are shown as vertical lines on each dot and are based on clustered standard errors at the program–cohort level. Each yearly entry cohort includes the spring and fall cohorts of the respective calendar year. The dotted red line separates the cohorts that enrolled before the start of SPP (2014 and before) from the first SPP cohort, 2015-1.

Figure 9: Pretreatment trends in observed outcomes – Programs with above- and below-median shares of low-SES peers in 2015



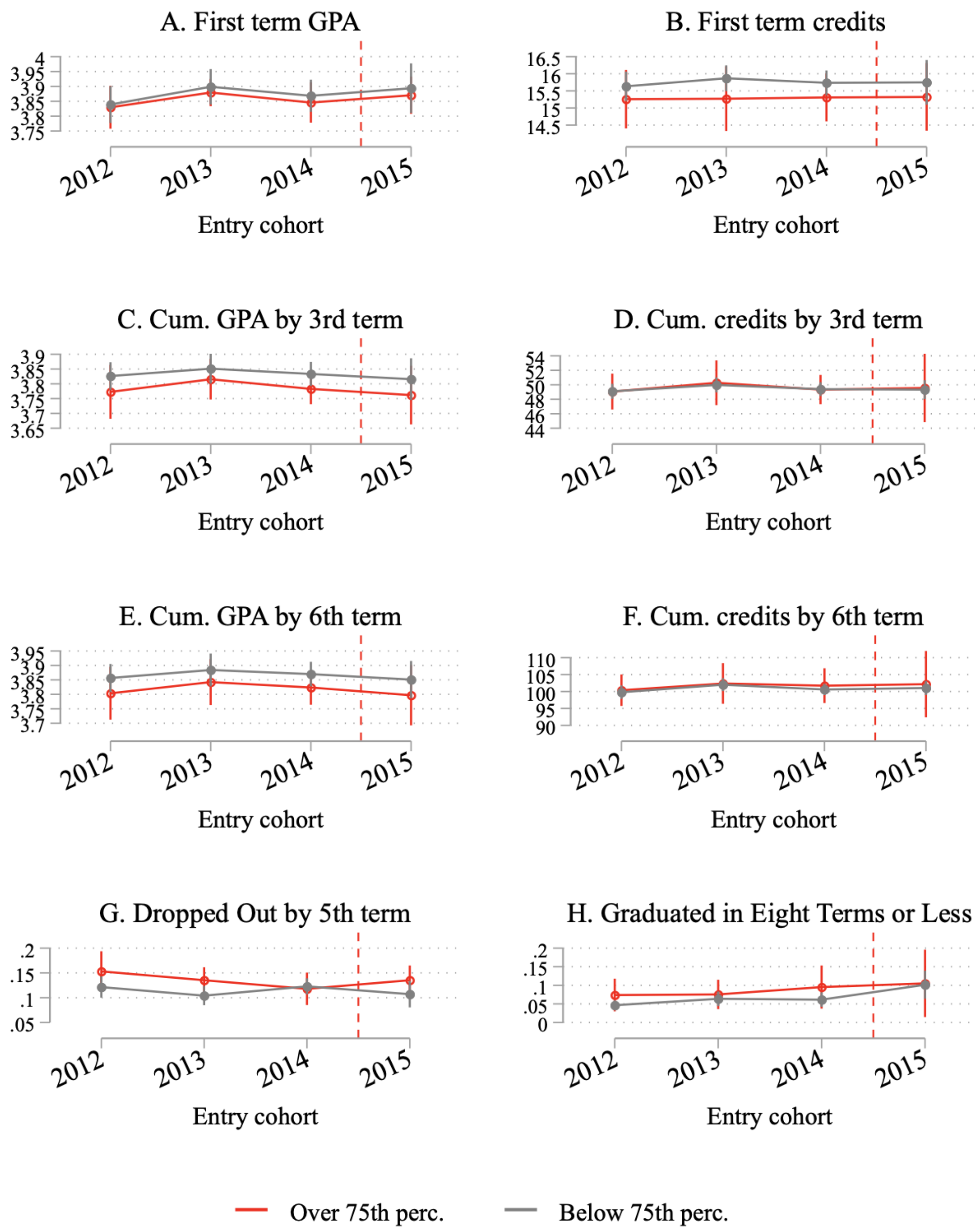
Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student outcomes as the dependent variable. Programs with a low-SES student share above the median value during the SPP period are classified as "Over Median" and others as "Below Median." The 95 percent confidence intervals use clustered standard errors at the program–cohort level. Yearly entry cohorts include both spring and fall, with a red line dividing pre-SPP and post-SPP (2015-1) cohorts.

Figure 10: Pretreatment trends in observed student sociodemographics – Programs with above- and below-median shares of low-SES peers in 2015



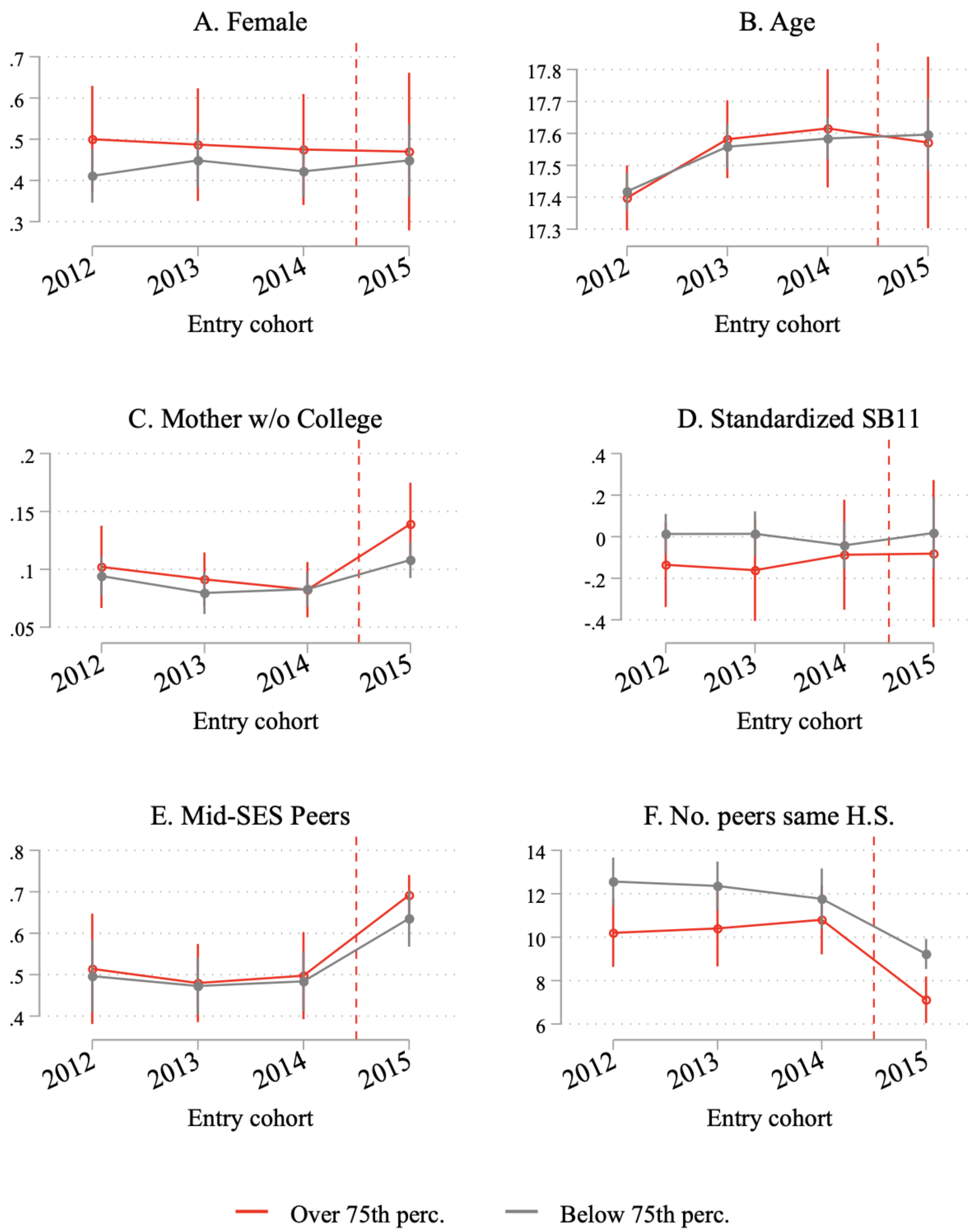
Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student characteristics as the dependent variable. Programs with a low-SES student share above the median value during the SPP period are classified as "Over Median" and others as "Below Median." The 95 percent confidence intervals use clustered standard errors at the program–cohort level. Yearly entry cohorts include both spring and fall, with a red line dividing pre-SPP and post-SPP (2015-1) cohorts.

Figure 11: Pretreatment trends in observed outcomes – Programs with above– and below–75th percentile shares of low-SES peers in 2015



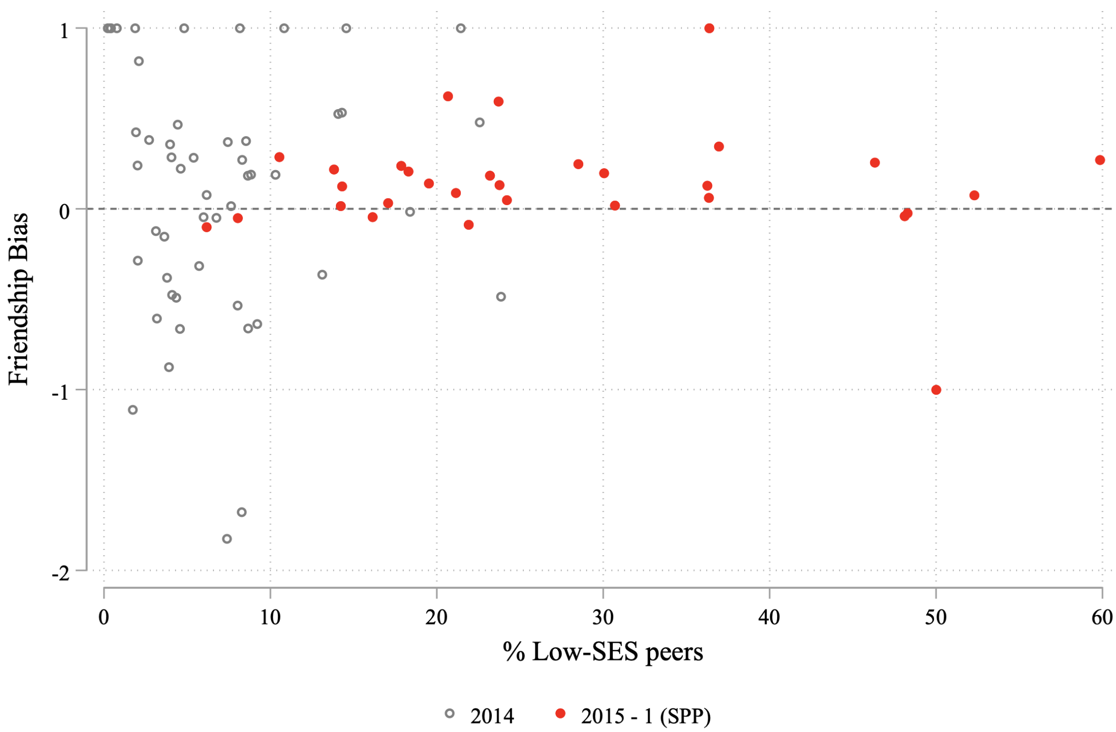
Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student outcomes as the dependent variable. Programs with a low-SES student share above the 75th percentile during the SPP period are classified as "Over 75th perc." and others as "Below 75th perc." The 95 percent confidence intervals use clustered standard errors at the program–cohort level. Yearly entry cohorts include both spring and fall, with a red line dividing pre-SPP and post-SPP (2015-1) cohorts.

Figure 12: Pretreatment trends in observed student sociodemographics – Programs with above– and below–75th percentile shares of low-SES peers in 2015



Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student characteristics as the dependent variable. Programs with a low-SES student share above the 75th percentile during the SPP period are classified as "Over 75th perc." and others as "Below 75th perc." The 95 percent confidence intervals use clustered standard errors at the program–cohort level. Yearly entry cohorts include both spring and fall, with a red line dividing pre-SPP and post-SPP (2015-1) cohorts.

Figure 13: Estimated friendship bias among programs and entry cohorts before and after SPP



Note: Graph displays the average friendship bias for low-SES friendships among high-SES students. Friendship bias follows the definition in Chetty et al. (2022) and is calculated as one minus the average percentage of low-SES links over the percentage of low-SES peers in the program and entry cohort. Thus, values close to one indicate a bias for friendships with students from the same SES background, and values below zero suggest a bias in favor of friendships with students from low-SES backgrounds. Values of zero suggest no bias, as the percentage of friendships with low-SES peers would equal the size of their presence in the program–cohort.

Table 6: Descriptive statistics

	2014 entry cohort			2015 entry cohort		
	High–SES	Low–SES		High–SES	Low–SES	
	Mean	Mean	t–test	Mean	Mean	t–test
Peers composition						
Number of links	5.21	4.94	0.66	5.62	4.98	2.17
Low–income Links	0.24	0.35	1.80	1.04	1.95	5.29
Student Characteristics						
Female	0.43	0.34	2.15	0.45	0.41	0.91
Age	17.59	17.24	3.94	17.59	17.13	10.54
Mother with no college degree	0.08	0.24	5.74	0.11	0.40	14.14
SB11 standardized test score	0.00	–0.10	1.17	0.05	–0.16	2.90
SPP recipient	0.00	0.00	N.A.	0.09	0.84	39.09
Other Scholarship or Loan	0.07	0.37	6.95	0.07	0.03	3.36
Internal inmigrant	0.23	0.35	2.67	0.24	0.57	8.55
No. of High School Peers in the cohort	11.54	3.16	12.53	8.81	1.96	18.72
ID Swipes in the 6th and 7th terms	1340.19	1349.79	0.11	1311.73	1099.80	3.91
Links' Characteristics						
Age Difference	0.60	0.66	0.70	0.66	0.67	0.15
Share of friends from the same gender	0.50	0.50	0.06	0.50	0.48	0.88
Courses taken together in first term	1.49	1.36	1.00	1.35	1.41	0.39
SB11 Difference	0.73	0.76	0.62	0.81	0.69	3.10
Share of friends from the same high school	0.04	0.01	5.28	0.03	0.01	7.17
Number of Students	2,669	139		1,358	463	
Number of Majors	31	31		31	31	

Note: This table shows the descriptive statistics for the sample of students described in Section 3. The 2015 entry cohort includes only the spring term (2015-1). High-SES students are those from household strata three to six, and low-SES students are from household strata one and two. The t test corresponds to the hypothesis that the difference in means between high- and low-SES students is equal to zero. The t tests are based on clustered standard errors at the program level.

Table 7: Correlation between numbers of high- and low-SES students in a program and entry cohort

	(1)	(2)	(3)	(4)
No. of low-SES peers in program–cohort	1.195***	1.654***	−0.232	−0.201
	(0.325)	(0.421)	(0.171)	(0.166)
Average of student characteristics		x	x	x
Major fixed effects			x	x
Entry cohort fixed effects				x
No. of program–cohort groups	93	93	93	93

Note: This table displays OLS estimates correlating the number of high- and low-SES students in a program and entry cohort between 2014 and 2015-1. The number of high-SES students is the dependent variable, and the number of low-SES students is the explanatory variable. Each observation in the data corresponds to one program and entry cohort. The average of student characteristics in a major–cohort group included are the share female, average age in years at entry, share of students whose mothers have no college education, average SB11 standardized test scores, share of students who are from SES 2 and 3, and share of SPP students. *** $p < 0.01$, ** $p < 0.05$, *, * $p < 0.1$.

Table 8: Placebo impact of exposure to desegregation on academic achievement and persistence – 2012 and 2013 cohorts

	(1) 1st term credits	(2) 1st term GPA	(3) 3rd term credits	(4) 3rd term GPA	(5) 6th term credits	(6) 6th term GPA	(7) Dropout by 5th term	(8) Graduation On Time
A. Continuous Treatment								
Percentage of Low–SES Peers	0.011	0.000	0.052	0.003*	−0.073	0.001	0.002	0.001
	(0.013)	(0.002)	(0.038)	(0.002)	(0.086)	(0.002)	(0.001)	(0.001)
Outcomes Statistics (2014 Cohort)								
Mean	15.540	3.837	49.088	3.813	99.928	3.844	0.129	0.053
Standard Deviation	3.079	0.476	8.580	0.386	16.818	0.342	0.336	0.225
No. of Students	3,569	3,563	3,253	3,253	2,981	2,981	3,569	3,569
No. of Major-Cohort groups	93	93	93	93	93	93	93	93

Note: This table displays placebo estimates of the effect of exposure to different percentages of low-SES peers on high-SES students' academic outcomes. Results from estimating Equation 1 on students enrolling in the 2012 and 2013 entry cohorts. All regressions control for a female indicator, age in years at the time of entry, SB11 standardized test scores, mother without a college degree, socioeconomic stratum two or three (i.e., intermediate SES), receipt of an SPP loan, and number of high-school peers enrolled in the same cohort as the student. All standard errors are clustered at the program–cohort level. *** p <0.01, ** p <0.05, *, * p<0.1.

Table 9: Estimates of relationship between share of low-SES peers and high-SES student characteristics

	(1) Female	(2) Age	(3) Mother w/o college	(4) Test Scores	(5) Mid–SES	(6) H.S. Peers
A. Continuous Treatment						
Percentage of Low–SES Peers	–0.001	–0.001	0.000	0.001	0.001	0.016
	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.029)
Outcomes Pre-treatment Statistics						
Mean	0.434	17.591	0.083	–0.052	0.487	11.544
Standard Deviation	0.496	0.912	0.276	0.955	0.500	11.711
No. of Students	4,027	4,027	4,027	4,027	4,027	4,027
No. of Major-Cohort groups	93	93	93	93	93	93

Note: This table displays estimates of the effect of exposure to different shares of low-SES peers on high-SES students' observed characteristics from Equation 1 and on students enrolling between 2014 and 2015-1. All standard errors are clustered at the program–cohort level. *** $p < 0.01$, ** $p < 0.05$, *, * $p < 0.1$.

Table 10: Effects of increased exposure to low-SES peers on academic outcomes

	(1) 1st term credits	(2) 1st term GPA	(3) 3rd term credits	(4) 3rd term GPA	(5) 6th term credits	(6) 6th term GPA	(7) Dropout by 5th term	(8) Graduation On Time
A. Continuous Treatment								
Percentage of Low–SES Peers	0.015***	0.001	0.029*	0.001	0.020	0.001	0.001	0.000
	(0.005)	(0.001)	(0.017)	(0.001)	(0.035)	(0.001)	(0.001)	(0.001)
B. 50th Percentile								
$\mathbb{I}$[% of Low–SES Peers > 24%]	0.063	–0.010	–0.030	0.015	0.226	0.015	0.022	–0.021
	(0.141)	(0.030)	(0.448)	(0.022)	(0.747)	(0.020)	(0.018)	(0.028)
C. 75th Percentile								
$\mathbb{I}$[% of Low–SES Peers > 36%]	0.071	0.011	0.505	0.017	0.541	0.025	0.037*	–0.035
	(0.177)	(0.030)	(0.610)	(0.027)	(0.828)	(0.025)	(0.019)	(0.038)
D. Percentage of SPP								
Percentage of SPP	0.006	0.000	0.021	0.001	0.024	0.000	0.002**	–0.001
	(0.006)	(0.001)	(0.017)	(0.001)	(0.033)	(0.001)	(0.001)	(0.001)
Outcomes Pre-treatment Statistics								
Mean	15.637	3.863	49.386	3.822	100.865	3.859	0.122	0.069
Standard Deviation	2.949	0.449	8.496	0.378	16.223	0.344	0.327	0.254
No. of Students	4,027	4,024	3,730	3,730	3,407	3,407	4,027	4,027
No. of Major-Cohort groups	93	93	93	93	93	93	93	93

Note: Panel A displays the results of estimating Equation 1. Panels B and C display the results based on a dummy variable for programs with low-SES student shares above the median and the 75th percentile, respectively. Panel D displays the results based on the percentage of SPP recipients in the program. All regressions control for a female indicator, age in years at the time of entry, SB11 standardized test scores, mother without a college degree, socioeconomic stratum two or three (i.e., intermediate SES), receipt of an SPP loan, and number of high-school peers enrolled in the same cohort as the student. All standard errors are clustered at the program–cohort level. *** p <0.01, ** p <0.05, *, * p<0.1.

Table 11: Effects of increased exposure to low-SES peers on students' social interactions

	I. Probability of a Link with a:		II. Number of Links with:			III. % of Links with:
	(1) Low–SES	(2) High–SES	(3) Low–SES	(4) High-SES	(5) Any Student	(6) Low–SES
A. Continuous Treatment						
Percentage of Low–SES Peers	0.008***	–0.002	0.037***	–0.035*	0.002	0.750***
	(0.001)	(0.001)	(0.003)	(0.018)	(0.018)	(0.066)
Mean Increase (18.0 points)	0.144	–0.036	0.666	–0.630	0.036	13.500
B. 50th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 24%]	0.109***	–0.034	0.630***	–0.690	–0.060	13.783***
	(0.041)	(0.027)	(0.149)	(0.494)	(0.483)	(2.544)
C. 75th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 36%]	0.143***	–0.058*	0.940***	–1.239**	–0.298	17.715***
	(0.048)	(0.033)	(0.128)	(0.611)	(0.636)	(3.428)
Outcomes Pre-treatment Statistics						
Mean	0.188	0.770	0.239	4.973	5.212	4.404
Standard Deviation	0.391	0.421	0.558	4.922	5.154	11.337
No. of Students	4,027	4,027	4,027	4,027	4,027	3,141
No. of Major-Cohort groups	93	93	93	93	93	91

Note: A socially interacting pair of students is defined when their IDs are swiped at a turnstile at the same entrance and in the same direction within three seconds or less and at least twice during a semester. Panel I outcomes are indicators equal to one when the student has interacted with at least one low- or high-SES peer. Panel II uses the number of peers whom the student has interacted with, and Panel III uses the percentage of links that are low SES. Panel A displays the results of estimating Equation 1. Panels B and C display results based on a dummy variable for programs with low-SES student shares above the median and the 75th percentile, respectively. All regressions control for a female indicator, age in years at the time of entry, SB11 standardized test scores, mother without a college degree, socioeconomic stratum two or three (i.e., intermediate SES), receipt of an SPP loan, and number of high-school peers enrolled in the same cohort as the student. All standard errors are clustered at the program–cohort level. *** p <0.01, ** p <0.05, *, * p<0.1.

Table 12: Effects of increased exposure to low-SES peers on students' interactions with high-achieving low-SES students

	I. Probability of a Low–Income Link High Achiever by:			II. Number of Low–Income Links High Achiever by:		
	(1) Saber 11	(2) GPA	(3) Credits Attempted	(4) Saber 11	(5) GPA	(6) Credits Attempted
A. Continuous Treatment						
Percentage of Low–SES Peers	0.007***	0.007***	0.009***	0.016***	0.020***	0.028***
	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)
Mean Increase (18.0 p.p.)	0.126	0.126	0.162	0.288	0.360	0.504
B. 50th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 24%]	0.102**	0.068	0.179***	0.287***	0.237*	0.570***
	(0.046)	(0.049)	(0.055)	(0.085)	(0.122)	(0.120)
C. 75th Percentile						
$\mathbb{I}$[% of Low–SES Peers > 36%]	0.167***	0.185***	0.212***	0.452***	0.567***	0.724***
	(0.051)	(0.043)	(0.062)	(0.101)	(0.083)	(0.145)
Outcomes Pre-treatment Statistics						
Mean	0.092	0.125	0.115	0.103	0.151	0.133
Standard Deviation	0.289	0.331	0.320	0.337	0.437	0.393
No. of Students	4,027	4,027	4,027	4,027	4,027	4,027
No. of Major-Cohort groups	93	93	93	93	93	93

Note: A socially interacting pair of students is defined when their IDs are swiped at a turnstile at the same entrance and in the same direction within three seconds or less and at least twice during a semester. Panel I outcomes are indicators equal to one when the student has interacted with at least one low-SES student with performance above his or her high-SES peers' mean on SABER 11 ((1) and (4)), first-term GPA ((2) and (5)) or first-term credits attempted ((3) and (6)). Panel A displays the results of estimating Equation 1. Panels B and C display the results based on a dummy variable for programs with low-SES student shares above the median and the 75th percentile, respectively. All regressions control for a female indicator, age in years at the time of entry, SB11 standardized test scores, mother without a college degree, socioeconomic stratum two or three (i.e., intermediate SES), receipt of an SPP loan, and number of high-school peers enrolled in the same cohort as the student. All standard errors are clustered at the program–cohort level. *** p <0.01, ** p <0.05, *, * p<0.1.