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

Tatiana Velasco*

March 15, 2023

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Abstract

How does the desegregation of elite schools impact academic achievement? And 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 null impacts on wealthy students' academic performance. I shed light on the mechanisms behind this lack of peer effects by studying changes in social interactions using data on students' co–movements across campus captured by turnstiles located at all entrances. Desegregation led to increased connections between wealthy and low–SES students. However, some bias in favor of interactions among wealthy students persisted. Moreover, at least half of the increase in interactions between these groups is explained by interactions of wealthy students with low–SES but high–achieving students. These results suggest students diversify their interactions primarily among students with similar academic achievement levels.
JEL Codes: I210, I240, I250

*Teachers College, Columbia University. e-mail: tv2225@tc.columbia.edu. I am grateful to the National Academy of Education / Spencer Foundation Dissertation Fellowship for providing funding for this project. All errors are my own.

1 Introduction

The segregation of students by socio–economic status, race, or ethnicity is a pervasive issue in education. At the post–secondary level, policymakers have implemented financial aid and affirmative action programs that foster access to selective institutions for low–income and underrepresented groups. However, these policies may exacerbate achievement gaps within institutions, particularly if benefited students struggle to perform as well as their classmates, which could lead to potentially negative peer effects (Arcidiacono, Lovenheim and Zhu, 2015). Moreover, researchers have found these changes in achievement composition may lead to segregation in social interactions between high– and low–performing students within a group (Carrell, Sacerdote and West, 2013). This is an undesirable result, if we account for the positive impacts that exposure to diversity has on privileged students (Rao, 2019; Boisjoly et al., 2006; Londoño-Vélez, 2022). In this paper I ask, what are the consequences of college desegregation for academic achievement? Can desegregation diversify students' social interactions?

To answer this, I use a natural experiment at a large elite college in Colombia that experienced a sharp and unexpected increase in the enrollment of low–income students, caused by 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 students' movements across campus as recorded by turnstiles guarding all campus 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 the increased exposure to low–income students significantly increases the interactions between wealthy and low–income peers, with no adverse effects on the achievement of the wealthy students.

In October of 2014, the Colombian government launched *Ser Pilo Paga*, a policy that targeted low–income students with outstanding academic achievement to promote their attendance to high–quality universities in the country. The program consisted of a loan that covered 100 percent of the tuition plus a small stipend for living expenses. The loan was forgiven upon completion of the degree. *Ser Pilo Paga* induced an influx in the number of low–income students enrolled at high–quality private universities in the country, closing the socio–economic enrollment gap among high achievers (Londoño-Velez, Rodriguez and Sanchez, 2020). The first cohort of students benefiting from SPP enrolled in January of 2015, barely three months after the announcement of the program. The short timing meant universities and wealthy students had little to no time to adjust their application and admission criteria in ways driven by their preferences for low–income peers. In Colombia, college applications are submitted to a college and major bundle, with admitted students enrolling directly into their major of application. This setting provides the conditions to plausibly measure the effect of increased exposure to low–income peers on college students' academic achievement and social interactions. Thus, the number of students admitted to the university I study increased, with the number of relatively wealthy students remaining stable while the number of low–income students tripled.

My empirical analysis leverages the plausibly random variation in the percentage of students from a low socioeconomic background (from now on, low–SES) within each program and across entry cohorts to which students from relatively high socioeconomic backgrounds (from now on, high–SES) were exposed to. Specifically, I implement a difference–in–differences approach that compares high–SES students enrolling right before SPP and in the first cohort of the program (2014 vs. 2015). I focus on the 2015 cohort as these students are the least likely to display selection issues threatening my research design. I conduct multiple test pre-trends and placebo test to show high–SES students in these cohorts do not exhibit evidence of self-selection that is explained by changes in the socioeconomic composition of their peers. Following recent advancements in the difference–in–differences literature (de Chaisemartin et al., 2022; Roth et al., 2022), I develop discrete treatment definitions alternative to the continuous percentage of low–SES students and test the robustness of my results.

My findings are summarized as follows: first, I do not find evidence suggesting that increasing the percentage of low–SES peers in the program-cohort led to changes in the academic achievement of high–SES students. I do find some positive yet modest impacts of exposure on the number of credits attempted by the first and third terms of college (terms are equivalent to academic semesters), which did not persist to the sixth term. However, these results did not hold when testing for discrete treatment alternatives and were very small in magnitude (less than one credit on average when a course bears at least three credits). These findings contrast with the significant gap in academic performance between high– and low–SES students. Peer effects theory would suggest that if students are exposed to peers of lower academic performance, then their own performance should be affected in some form.

A lack of interactions between SES groups of students could explain the lack of impact of desegregation on academic achievement. Students from different socioeconomic status (SES) groups may not interact much with each other due to a preference for interacting with others who are similar to themselves in terms of socioeconomic background (a phenomenon known as ”homophily”). But, if there are more low-SES peers in a student’s group, more interactions between high– and low–SES peers should occur simply due to their increased presence. The second part of my empirical analysis examines whether changes in the percentage of low–SES peers in the high–SES student group changed the diversity of their social interactions.

Thus, my second set of results shows that high–SES students diversified their social interactions when exposed to larger shares of low–SES peers. Specifically, I examine the effect of increased exposure to low-SES peers on turnstile-elicited interactions between high- and low-SES students. I define a pair of students as linked if they pass 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 arrived at this definition and discusses its potential limitations in terms of measurement error. Descriptive statistics reveal that high-SES students had an average of 5.2 links in their program and cohort before the implementation of SPP. Of those links, only an average of 0.24 were with low-SES peers, while nearly five were with other wealthy peers. However, the average 18 percentage point increase in low-SES peers induced by SPP resulted in a 0.67 increase in links between high- and low-SES students, and it increased the probability of their interaction by 14 percentage points.

Increased exposure to low-SES peers also led to significant reductions in links among high-SES students. The average increase of 18 points decreased the likelihood of a link with other high-SES peers by 3.6 percentage points and the number of links by 0.63. There were no changes in the total number of links. This suggests that high-SES students increased their links with low-SES peers while decreasing their links with other high-SES peers, indicating a friendship substitution effect.

Even though friendships with low–SES peers increased among high–SES students, I find evidence suggesting a bias for links among the same SES group persist. First, estimations on the percentage of links which are low–income indicate high–SES students’ responses to changes in peers composition are not monotonic. A one percentage point increase in low–SES peers translates into a 0.7 points increase in the percentage of high–SES links who are low–SES. 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-biased was reduced, many of the programs among the 2015 cohort do exhibit some bias in favor of links in their own SES group.

My last set of results bridges the positive impacts on the diversity of social interactions with the lack of effects on student academic achievement. Specifically, I examine if high–SES students have preferences for matching with others of similar or superior academic achievement that offset differences in socioeconomic backgrounds. To test for this, I identify low–SES students with an academic performance equal to or above the average performance of high–SES students in their major and entry cohort group and estimate the effect that exposure to low–SES peers have on the number of interactions with the low–SES very high–achieving students. I find that almost half of the increased number of interactions with low–SES students are with students whose academic performance –as measured by their high school exit exam scores and first-term GPA and credits attempted, is at least equal to the average performance of wealthy students. That is 0.13 of the 0.29 increase in links between wealthy and low–income students with low–income students whose performance is above the average performance of the wealthy students in the major and entry cohort. These results as suggestive of homophily in social interactions based on academic achievement, which offsets some of the homophily based on socioeconomic status. These findings complement results by Baker, Mayer and Puller (2011); Mayer and Puller (2008); Sacerdote (2001) and Mele (2020); Jackson et al. (2023) who find academic achievement significantly explains social network formation.

This study makes three contributions to the literature. First, my paper documents the causal impacts of desegregation on the academic achievement of privileged students at elite colleges. Other scholars have examined the effects of exposure to minorities 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 re–segregation (i.e., ending an affirmative action policy in California), on White and Asian students performance finding no effects. My findings contrast with Arcidiacono and Vigdor (2010) by showing that increases in the exposure to underrepresented students at elite schools have no effect on the privileged students' performance and, if anything, can lead to modest improvements in early outcomes. My results are also complementary to those of Bleemer (2021a) by showing that the opposite –inducing desegregation through financial aid policies targeted to the low–income, has no impact on the achievement of privileged students either. Moreover, my findings align with previous evidence from K–12 settings which find no effect of desegregation policies on the academic achievement of students traditionally attending these institutions (Angrist and Lang, 2004; Dobbie and Fryer, 2014), and with findings from Lau (2022); Corno, La Ferrara and Burns (2022)[1].

[1] Angrist and Lang (2004) studied the effect of a desegregation program in Boston on the academic achievement of the students 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.

Second, my study shows how students' social interactions at elite colleges change at the outset of financial aid and affirmative action policies fostering desegregation. While prior research has consistently found positive impacts on the college attainment of underrepresented students benefiting from financial aid and affirmative action programs (Bleemer, 2021b; Chetty et al., 2020; Londoño-Velez, Rodriguez and Sanchez, 2020; Mello, forthcoming), I provide novel evidence on how social interactions change under desegregation policies. Findings of Michelman, Price and Zimmerman (2020) and Zimmerman (2019) indicate low–income and minority students tend to not make part of 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 findings show social interactions between wealthy and low–income students do form in the outset of desegregation, which may have other positive ramifications in social mobility of low–income students and on pro–social behaviors of the wealthy ones (Rao, 2019; Boisjoly et al., 2006; Londoño-Vélez, 2022).[2]

Third, this paper also connects to the literature examining diversity in schooling 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 margin of interactions with the low–income. Similarly, evidence coincides in that peers' proximity and peers' race are determinants of friendship formation. Namely, students assigned to the same dorm are more likely to be connected, but the chances are higher for same–race students.[3] My study uses a different dimension of proximity which is being in the same major and entry–cohort. My findings indicate that proximity through majors and cohorts group is determinant for students interactions. A related sub–stream 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 who students interact with and academic achievement (Echenique,

[2]My work is closely aligned to that of Londoño-Vélez (2022), who studied the effect of socio-economic diversity at an elite college in Colombia on students' redistribute preferences. In this work, Londoño-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 the latter finding while pointing out that the change in social interactions is relatively small.

[3]Marmaros and Sacerdote (2006) examine how people form social networks with their peers. They use emails exchange data from students and find that first-year students form friendships with students in the proximity 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 finds exposure to different races via dorms leads to more diverse friendships.

Fryer and Kaufman, 2006), and finding non–linear responses in interactions to scenarios of minorities reallocation across schools (Mele, 2020).[4] My findings show consistently positive impacts on the diversity of interactions at the major–cohort level of exposure and show that changes in interactions through changes in exposure do not lead to impacts on academic achievement.

2 Background and Setting

In this paper, I examine the effect of a socio–economic 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*[5]), which in 2015 experienced a large and unexpected increase in the number of low–income students enrolled while keeping the enrollment of relatively wealthy students 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 increased enrollment of low–income students varied across the thirty one degree majors offered at Elite University. My research design focuses on relatively wealthy students and compares students from the entry cohorts before and after SPP (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 the 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.[6] The

[4](Echenique, Fryer and Kaufman, 2006) measure within–school segregation as the extent to which students interact socially with other students from the same race. Mele (2017) develops a structural model of friendship formation among students, and Mele (2020) use 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.

[5]This is a made-up name. I do not provide the real name of the university I study for confidentiality reasons.

[6]The high-quality accreditation is granted to higher education institutions by the National Council of

loan was forgiven conditional on completion of the degree. Eligibility to SPP required that students were classified as poor under the governments' index of household wealth, and scored in the top ten percentile of the national high school exit exam SABER 11.[7] SPP awarded loans for new cohorts of students between 2015 and 2018 benefiting about 40,000 students nationwide. Previous research has found SPP increased diversity at top private universities by making the selection mechanism based more on ability than on income (Londoño-Velez, Rodriguez and Sanchez, 2020). As depicted in Figure 1, 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 low–SES new students in the 2015 entry cohort that tripled their share of this group relative to the 2014 enrollment.

The timing of SPP and the admission rules at Elite University set the conditions of the natural experiment I exploit in my research design. First, admissions to Elite University are open for each year's Spring and Fall term and are determined by the applicant's score in the SABER 11 standardized test. Students must apply to a major[8] and entry cohort for which admission officers had predetermined a specific SABER 11 weighting formula[9] and cutoff score. Second, SPP was widely unexpected by students and higher education institutions. SPP was launched in October of 2014 and only students who had taken that October's test was eligible. Candidates had to apply for enrollment in the following Spring of 2015, for which 10,000 forgivable loans were offered. Thus, students who traditionally applied 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 from 2014, but the number of low-SES students increased significantly.

Figure 2 depicts the first–term enrollment trends by socio–economic status (SES) at Elite University. Between 2012 and 2014, less than 150 first–term students came from

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.

[7]The household's index of 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 a requirement for all students in the country who are about to complete their high school education. The exam is applied twice a year, following the two academic calendar of schools in the country: January – November and August – June.

[8]As opposed to the U.S., applicants to higher education must apply to a program for majoring in as well to as a college.

[9]The SABER 11 is made of five modules which are given different weights depending on the major of application. For example, for admission to engineering majors, quantitative reasoning is assigned a higher weight than the social sciences module

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 socio–economic backgrounds remained almost the same. Figure 3 compares the number of low–SES students across programs in the entry cohorts before and after SPP. Gray and blue lined bars depict the number of low–SES students in the cohorts right before SPP (i.e., 2014-1 or Spring and 2014-2 or Fall), whereas gray-filled bars depict the number of low–SES students in the first cohort of SPP (i.e., 2015-1 or Spring of 2015). The variation in the number of low–SES students is important. Majors such as Business and Music experienced virtually no change in the number of low–SES students, while others like Civil Engineering or Psychology experienced a notable increase.

To examine whether SPP led to the crowding out of high–SES students I examine the correlation between the number low–SES and high–SES students in Table 2 The unconditional correlation suggests increases in the number of low–income students are positively associated with more wealthy students in the major and cohort. Once program fixed effects are included, the correlation between the number of low–SES and high–SES students is no longer statistically significant. The size of the estimated correlation also becomes much smaller in magnitude. This is consistent with having program traditionally large programs enrolling more low–income students, a feature that is well captured by the major fixed effect. Including entry cohort fixed effects that address shocks in sizes common to all programs does not change the relationship, suggesting differences in sizes are mostly driven by program characteristics.

The influx in the number of low–SES students led to busier classrooms. However, on average, classrooms did not go over capacity during SPP, and the number of sections offered per course as well as the number of seats available per section remained constant. Figure 4 provides descriptive statistics of the courses taken by first–term students from 2012 to the 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 section. In 2015, classroom occupation peaked but remained below 100 percent (i.e., 84 percent on average), suggesting classrooms on average did not have crowing issues that could have hampered learning.

3 Data

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

Elite University campus.

Elite University administrative records. I use records from all students enrolled at Elite University between 2012 and 2018 which contained student-course level data on student characteristics (i.e. sex, 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 their course GPA. More importantly, I observe the student's household social strata indicator. This indicator has six categories that are used to provide homes with subsidies in utility bills. Plus, it is also widely known in the country as a proxy of social status. I use the household social strata at the time of college application to classify students in two socio-economic status (SES): high–SES – which are students living in a household classified between strata three and six, and low–SES students who are from strata one and two. Students benefiting from SPP mostly fall in the low–SES category. As depicted in Figure 2, the majority of students at Elite University are classified as high–SES.

Turnstile records. I use records on student access and exits to Elite University campus to identify students' social interactions. Elite University campus is guarded by turnstiles located at the 18 entrances to main buildings and campus areas. In order 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 on the turnstiles from February 1st, 2016 to November 1st, 2019. These records include student ID number, entrance, action (IN or OUT of campus), and the date, hour, minute and second of the swipe. Figure 13 in Appendix A displays a heat–map of the average frequency of student ID swipes at three of the busiest entrances to campus by 20 minutes blocks. Yellow cells 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 change as well as 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, in the same entrance and direction (either entering or exiting campus), and when I observed the same pair of IDs co-moving 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.

Sample. My analytic sample consists of all the first–term students in the entry cohorts before and after SPP (i.e. Fall and Spring of 2014 and Spring of 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 entry cohort of Spring of 2014 with their interactions as captured by the turnstiles during the Fall of 2016 and the Spring of 2017. I merge administrative records and pair-wise–level students' interactions 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 the Directed Studies) which started after SPP.

Student characteristics. Table 1 provides descriptive statistics of high– and low–SES students in the pre– and post–SPP entry cohorts (i.e., 2014 vs. Spring of 2015). The table includes the t–test of mean differences between low–income and wealthy students. In 2015, 84 percent of low–income students at Elite University were SPP recipients. About half of high–SES students are from household strata three and four (akin middle income group). In both cohorts, high–SES students have a larger share of females, are slightly older and with mothers more educated than the low–SES students in their entry year. Also, high–SES students have higher SB11 test scores than low–SES ones, and the gap increases and becomes statistically significant for the 2015 cohort. The gap in SB11 test scores between high– and low–SES students was 0.10 standard deviations in 2014, but increased to 0.35 standard deviations in 2015.

high–SES students have on average more links than low–SES peers with others in their major and entry cohort (5.21 vs. 4.94 links in 2014), and the difference becomes statistically significant in the 2015 cohort (5.62 vs. 4.98 links). Before SPP, the number of links with other low–SES students is statistically the same among wealthy and low–income students (0.24 vs. 0.35, respectively). But, in 2015, high–SES students have significantly fewer links with other low–SES peers (1.04 vs. 1.95, respectively). Importantly, high–SES students have on average more peers from high school enrolling at Elite University in their same cohort than low–SES students in both pre– and post–SPP cohorts (11.54 vs. 3.17 in 2014 and 8.81 and 1.96 in 2015). There are no statistically significant differences between the number of ID swipes at the turnstiles of high– and low–SES students in 2014, but the difference does become significant for 2015 students (1311 swipes vs. 1099 swipes). The latter may have implications for estimating the unbiased effects on social interactions between high– and low–SES students, which I will assess in the following sections.

Table 1 also describes the average characteristics of the links of both high–SES and low–SES students. Links of high– and low–SES students were statistically the same among the students in the 2014 cohort, except for the share of links from the same high school, which is larger among wealthy students (0.04 vs. 0.01). For the 2015 cohort, high–SES students exhibit a larger SB11 test score difference with their links (0.89 vs. 0.69), suggesting high–SES students form links with students of more varied academic performances than their low–SES counterparts.

Student achievement and gaps between low– and high–SES students: I characterize the differences in academic achievement between high– and low–SES students in Figures 5 and 6. 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 wealthy and low–income students, and include the estimated 95 percent confidence interval based on clustered standard errors at the major and entry cohort level. The red line separates the entry cohorts before the start of SPP (left side) and the cohorts entering during SPP (right side). Figure 5 displays performance indicators, mainly cumulative GPA and total credits attempted, whereas Figure 6 describes persistence (dropout rates and graduation). I label a student as a dropout if they do not show up as enrolled for two consecutive terms after their fifth term of college. Similarly, I label a student as graduating if they completed their degree in eight terms or less. At Elite university, this is considered as graduation on time for all their degrees except medicine.

The cohort of high– and low–SES students that enrolled Elite University at the outset of SPP (i.e., the 2015 entry cohort) exhibit significant achievement gaps, particularly in their GPA and cumulative credits attempted, with low–SES students having on average 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. But for the SPP cohort, the GPA of low–SES students drops to 3.75 in their first term of college and to 3.6 by their third term of college, 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 have attempted, on average 50 and 48 credits by the third term, respectively. But in the SPP cohort, low–SES students have 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 at least one class less than their high–SES peers by the third term of college, and with a cumulative GPA that is 0.25 lower. Nevertheless, the differences in achievement did not pair with differences in dropout or graduation rates, suggesting the relatively low achievement of low–SES students did not translate into diminished persistence.[10]

[10]Importantly, graduation rates in less than eight terms are very small at Elite University across all groups as many students tend to take extra semesters to course minor degrees or to double major with other degrees. low–income students benefiting from SPP and other financial aid programs tend to be constrained in that they do not get financed for terms beyond those scheduled in their major curriculum, which

4 Identification Strategy: Effects on Academic Achievement

I start by examining the effects of increased exposure to low–SES peers on the achievement of high–SES students who traditionally attended 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 versus 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, which could potentially affect their academic achievement.

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

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 low–SES peers of student i, $\mathbf{X_i}$ is a matrix of female, a mother with no college education, and middle–SES indicators, as well as standardized high school exit exams (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 to 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. The estimated effect β_l captures the Average Treatment Effect (ATT) of increased exposure to low–SES peers on student achievement. Figure 3 describes the variation exploited for causal identification. Relative to 2014, the percentage of low–SES students increased at different rates across programs. Table 2 shows the increased number of peers did not crow out of wealthy students.

Unbiased identification of β_l requires that, in the absence of the treatment, the outcomes exhibit the same trends for the treated and control groups (i.e., the parallel trends assumptions). To test for this, I estimate the placebo effects of the percentage of low-SES students on high-SES achievement using data from the 2012 and 2013 entry cohorts. Results are displayed in Table 3. I do not find any evidence suggesting that changes in the percentage of low-SES students were associated with student outcomes in the periods before SPP. Moreover, violations of this assumption would imply non-random 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 for low-SES students, which would

explains their slightly higher likelihood of graduation.

reflect changes in high-SES student characteristics. I test this by estimating Equation 1, without the matrix of student characteristics, on observed student sociodemographics. I display these results in Table 4. I do not find any evidence suggesting that the characteristics of wealthy students changed in response to the changes in the percentage of low-SES peers.

As additional robustness checks, I define alternative discrete treatment variables equal to one when the percentage of low–SES peers in the program is over the 50th or the 75th percentile of the distribution in 2015-1. This is equivalent to shares of low–SES over 24 and 36 percent, respectively. I show that pre-treatment trends in the outcomes and observed characteristics hold parallel under this definition in Figures 7 to 10. This discrete treatment set-up resembles a non-staggered treatment design with the treatment status shifting for some programs in the SPP 2015-1 period, thus offering an alternative non-biased estimate of the ATT (de Chaisemartin et al., 2022; Roth et al., 2022).

5 Results on the Effects of Desegregation on Achievement

Table 5 displays the estimated effect of the increased exposure to low–SES peers on relatively wealthy students' academic achievement and persistence. Panel A displays OLS estimates of β_l for all the outcomes discussed in Figures 5 and 6. Focusing on 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. Conversely, 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 in 18 percentage points on average from 2014 to 2015. This implies 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 marginal effect on courses taken.

Panels B and C display results using 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 disappeared, suggesting those results are not robust. Thus, I do not find conclusive evidence that exposure to low–SES peers impacted the academic performance of high–SES students. 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[11]. Overall, my findings speak to prior literature find-

[11] As opposed to low–SES peers, SPP peers were zero in 2014 and took positive values in 2015

ing no effects of desegregation on traditionally privileged students' GPA in K–12 settings (Angrist and Lang, 2004; Corno, La Ferrara and Burns, 2022) and higher education ones (Bleemer, 2021b).

The peer effects literature would suggest 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 have, on average, lower performance than their wealthy peers (see Figure 5). A possible hypothesis explaining the lack of effects is that segregation between high– and low–SES students persists within the program–cohort. This would be consistent with findings from Carrell, Sacerdote and West (2013), which suggest assigning low–achieving students to relatively high–achieving classrooms can lead to segregation between these two groups. To test if 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

Next, I estimate the effect of the increased exposure to low–SES students on high–SES students' social interactions. To do so, I use equation 1 and include as controls the number of peers that come from the same high school in the student cohort, as this is likely to confound the social interactions high–SES students have with other socio-economic groups. Social interactions are defined using 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 ID at the same entrance and going in the same direction in a window of three seconds or less, and at least twice in a semester.

The turnstile–elicited measure of social interactions brings another source of bias in this difference–in–differences set up. Specifically, there is a potential measurement error in the turnstile–elicited interactions, as there is a risk of capturing random co-movements of low– and high–SES students across the turnstiles that could be falsely attributed as effects of exposure to low–SES students. If turnstile–elicited interactions partially capture true social interactions, then those need to be on average representative of true interactions and cannot be biased due to the potential random noise in the measurement error. Moreover, the rate of false–positives and false–negatives (i.e., the likelihood of defining a pair of students as linked when in fact, they are not; and the likelihood 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 rate of false–positives and negatives. Specifically, I use secondary data on the survey–elicited social interactions among one major–cohort group at Elite University to obtain estimates of the rates of false–positives and false–negatives under alternative turnstile–elicited links definitions.[12]

Appendix A provides details on the secondary data and the computation framework and procedures I use to assess measurement error. My results indicate the turnstile–elicited links suffer a relatively large rate of false–negatives of approximately 60 percent but a rate of false–positives below 10 percent (see Table 8). To assess the extent to which measurement error can diminish the quality of the turnstile–elicited interactions, 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. Turnstile–elicited links compare well with survey–elicited links, albeit the large false–negatives rate. More importantly, the characteristics of turnstile–elicited links are statistically the same as those from the survey links but different from those that would be obtained if links were obtained purely at random (see Figure 12). I rationalize measurement error in a difference–in–differences 2x2 framework that follows Goodmann–Bacon (2019) and Cunninghan (2021). If the measurement error is associated with the exposure to low–income students in ways unobserved by the researcher, then the observed Average Treatment on the Treated (ATT) effect may differ from the true ATT. I proxy measurement error with the number of ID swipes on 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 10). In summary, I do not find evidence that measurement error in turnstile–elicited interactions bias 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 the probability of having at least a link with a low–SES peer in their group. Second, I estimate the effect on the number of low–SES links formed by each high–SES student. Third, I measure the effect on the composition of the high–

[12]The survey was conducted online between December 7, 2017, and January 5, 2018, and elicited the network among 110 Economics undergraduate students from the 2017 fall cohort. The survey was conducted using 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.

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 the desegregation in their group.

Table 6 displays the results of estimating Equation 1 on wealthy students' interactions with their peers. Panel I. displays the estimated effects on the probability of interactions with wealthy and low–income 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. Panel A shows continuous estimates following Equation 1, whereas Panels B and C display estimates using discrete treatment definitions.

Exposure to low–SES peers significantly impacted the diversity of interactions among high-SES students. Focusing on Panel A of Table 6, 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 margin of interactions between high– and low–SES students. The average increase in low–SES led to 14.4 percentage points more 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. I also find a significant reduction in the interactions among high–SES students of 3.6 percentage points and 0.63 fewer links. I do not find effects on the total number of links, suggesting increased shares of low–SES peers led to substituting high–SES links with low–SES links instead of expanding the total number of friends. The direction and significance of these results hold when using the discrete treatment definition from panels B and C.

Results in Panel III of Table 6 suggest that some preference for links among the same SES group prevailed even after SPP. Specifically, the response to the increased share of low–SES peers was not monotonic, and for every additional percentage point, the percentage of links with low–SES students increased only by three-quarters of that (0.75 percentage points). To further assess this, I measure friendship bias as defined by Chetty et al. (2022). In the context of this paper, friendship bias can be defined as 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 closer to zero, suggest no friendship bias. Similarly, values below zero suggest a bias that favors links with low–SES beyond their representation in the group.

In Figure 11, I plot 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 little relation with the percentage of low–SES students in the group. Red dots, which plot the estimated friendship bias on the 2015-1 entry cohort, suggest the variation in the bias reduced, with no visible relationship with the percentage of low–SES students. The estimated friendship bias does seem to be consistently over one, with just a few exceptions, suggesting a pattern of favoring links among the same SES group, regardless of the share of students in the program and cohort. Coupled with the results on Panel III of Table 6, I conclude that interactions between low– and high–SES students did increase, but 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. Namely, scholars have found positive impacts of increased diversity in schools using measures of intensity in interactions captured by email exchanges (Marmaros and Sacerdote, 2006), and survey questions about willingness to interact with racial and ethnically diverse groups (Boisjoly et al., 2006; Rao, 2019). My results provide a finer desegregation of the effects by distinguishing the impacts on the probability and the number of interactions with peers in the same group. These results also complement those by Mayer and Puller (2008) and Baker, Mayer and Puller (2011) by examining the changes in the composition of friendships with a measure of interactions bounded to peers in the same college group.[13]

7 The role of academic achievement in the diversity of social interactions

Previous research (Arcidiacono and Vigdor, 2010; Epple and Romano, 2011) suggest exposure to low–achieving peers should have a negative impact on performance. Lack of it would be suggestive of segregation between high– and low–achievers preventing the effects. At Elite University, SPP increased the exposure to low–SES peers who were also, on average, low–achievers relative to the students traditionally attending this institution. However, there is variation in the distribution of scores among low–income students enrolling during SPP. In fact, 27 percent of the low–income students enrolling during 2015 had a SB11 test score that was equal or above the average of the test scores of their wealthy

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

peers in the major and entry–cohort.

To examine the role of the academic achievement of low–SES students, I estimate the effect of exposure to low–SES students on the links with low–SES peers who are also high achievers in the group. Table 7 displays the results. Similar to Table 6, I compute the effects of the changes in exposure to low–SES students on the number and probability of a link with a low–income student who is also a high achiever in terms of: SB11 test scores, first-term GPA, and total credits attempted in the first term. I consider a low–SES student to be high achiever if their performance in the achievement variable is above the average of that of the wealthy students in their major and entry cohort. Importantly, SB11 test scores are measured before enrollment to college and therefore are not susceptible to unobserved within groups peer effects. However, performance during the first term of college measured by GPA and credits attempted may be the result of unobserved effects on the low–SES students. Nevertheless, the latter may be easier to observe by wealthy students than SB11 test scores, and so the results are interpreted as suggesting that preferences for interactions may be influenced by the academic performance early during college.

Focusing on Panel II of Table 7, I find that the average increase in exposure to low–SES students of 9.5 percentage points led to 0.13 more links with low–SES students with above average SB11 test scores, 0.17 more links with low–income students with above average first term GPA, and 0.24 more links with low–SES students with above average credits attempted in the first term. Relative to the initially estimated effect of exposure on links with any low–SES student of 0.3, the different measures of achievement explain 43 percent, 57 percent and 80 percent of the estimated effect described in Table 6, respectively. Arguably, that 80 percent of the links with the low–SES can be explained by the number of credits attempted, suggesting more exposure through more hours of class of low–SES peers is a likely channel explaining the increase in diversity.

Similarly, Panel I of Table 7 displays the impacts on the probability of interaction with the low–SES by the different achievement measures. Wealthy students exposed to an increase of low–SES peers of 9.5 percentage points are 0.06 points more likely to interact with a low–SES student with above-average SB11 achievement, seven percentage points more likely to interact with a low–SES student with above-average first-term GPA, and nine percentage points more likely to interact with a low–SES students with above average number credits attempted in the first–term of college. Relative to the initial estimated impact of exposure on the likelihood of interaction with any low–SES peer, I find that high achievement in terms of SB11 and GPA among low–SES students explain 75 percent of the effect on the probability of interaction with any low–SES student. As with the intensive margin effects, almost 100 percent of the probability of interactions with a low–income peer is captured by interactions with low–income students attempting more credits in the first–term than the average wealthy student. Coupled with the findings from Panel B, these results suggest interactions with low–income very–high–achieving peers are a likely driver of integration, also explaining the lack of negative impacts on academic achievement.

8 Conclusions

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 relatively wealthy students, as measured by their cumulative GPA, number of credits attempted, and graduation on time measures. Second, the increased exposure to low–SES peers led to more interactions between wealthy and low–SES students. Third, at least half of the increase in interactions with the 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). Moreover, wealthy students who are also high–achieving according to their first–term performance are significantly more likely to interact with low–income students.

These findings provide evidence of how socioeconomic desegregation of elite colleges can impact students within the institution. Similar to Angrist and Lang (2004) and Bleemer (2021a), I show there are no adverse impacts on the achievement of traditionally privileged students attending these institutions. Moreover, I show that the lack of peer effects is not explained by the segregation between wealthy and low–income students within the groups. In fact, I find that desegregation can be largely explained by students matching with others of similar academic achievement. These findings complement previous results from Carrell, Sacerdote and West (2013), who find groups with large achievement gaps can tend to segregate, likely explaining the lack of peer effects.

Lastly, my findings are promising about potentially positive impacts on long–term outcomes. Previous studies that have relied on group membership to measure social interactions find that membership to elite social groups has significant positive impacts on employment and labor market outcomes (Zimmerman, 2019; Michelman, Price and Zimmerman, 2020; Marmaros and Sacerdote, 2002). The low–income students connecting with wealthy peers should see an improvement in their long–term outcomes. Future work should address this hypothesis, as it is a key channel to understanding how desegregation of higher education can foster social mobility for low-income and underrepresented students.

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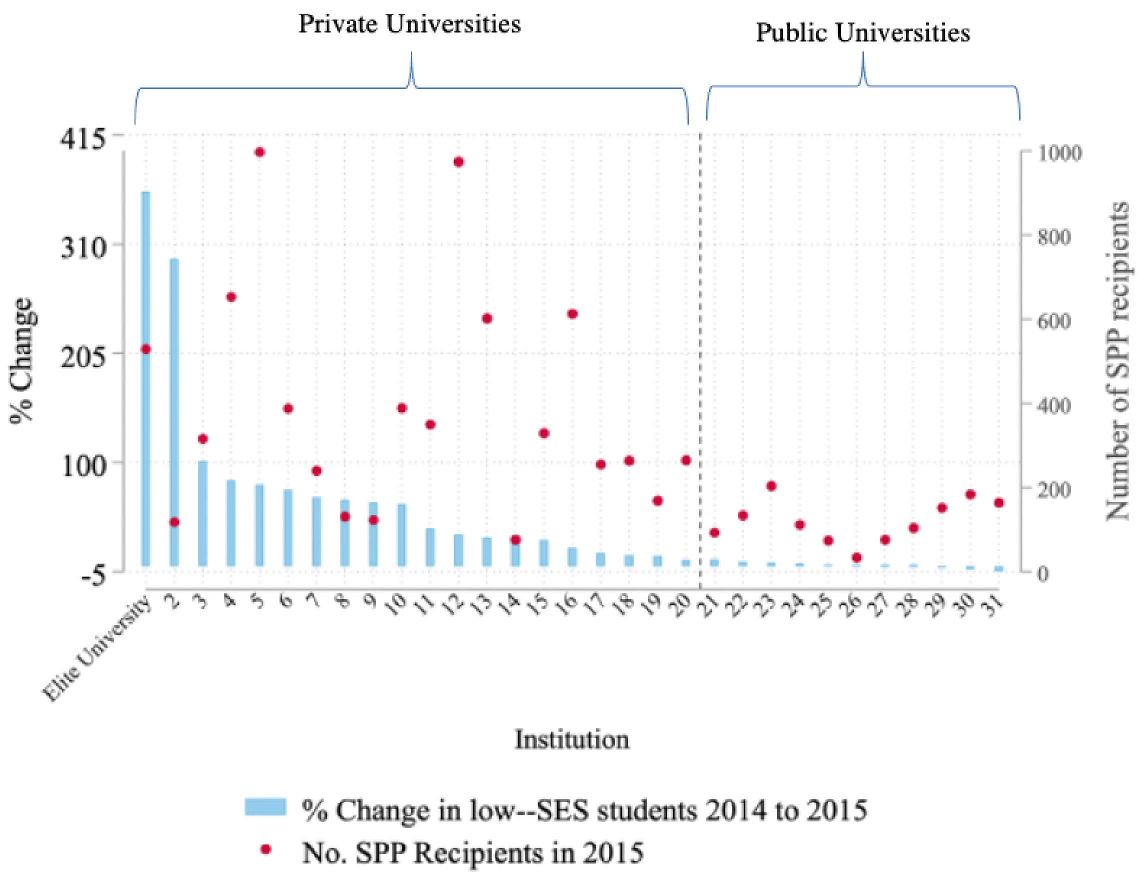
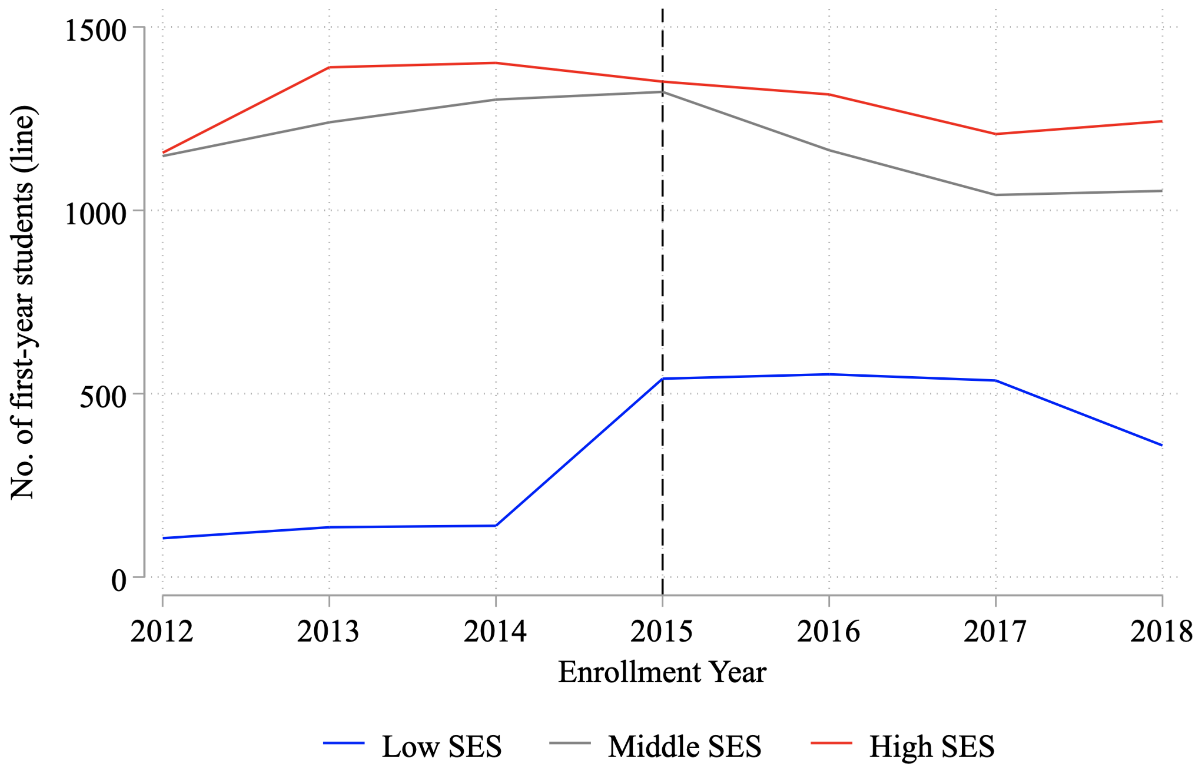
Figures

Figure 1: Change in the percentage of low–SES students across SPP–Eligible Universities



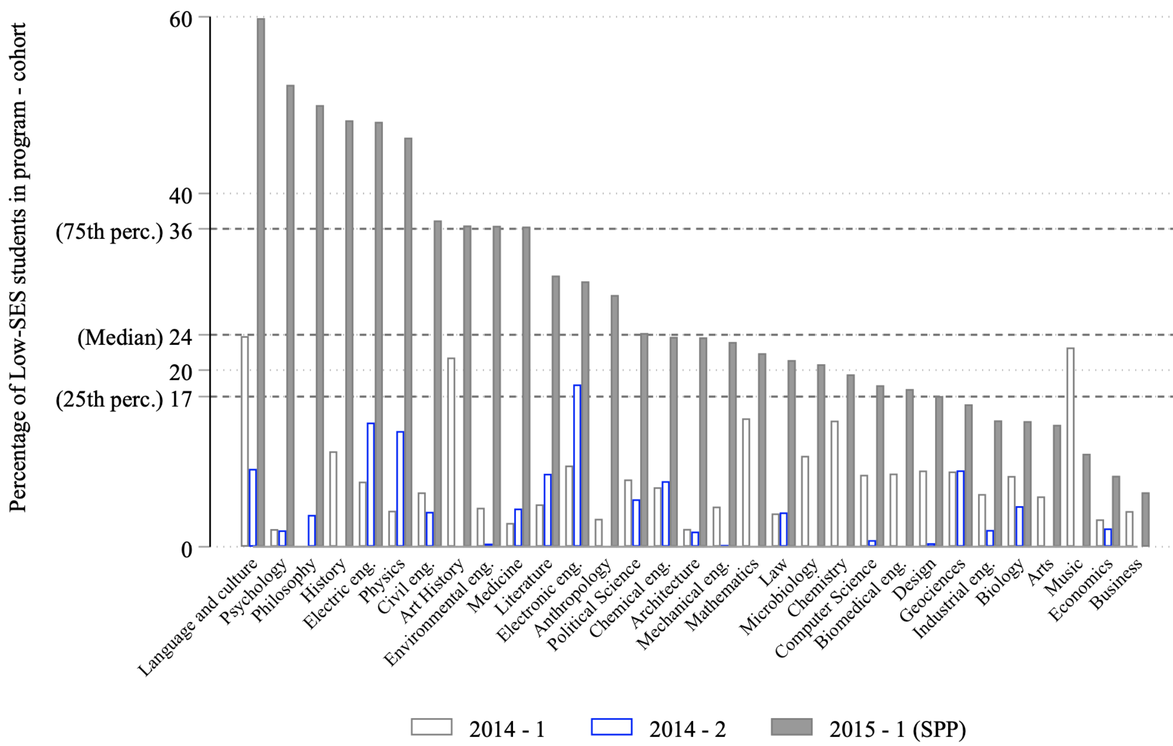
Note: This figure displays the percentage change in the number of low–SES students enrolling between 2014 and 2015 entry cohorts at each of the SPP–Eligible universities in Colombia. Calculations are based on publicly available data from the National Ministry of Education.

Figure 2: Number of first term students by socio–economic status (SES)



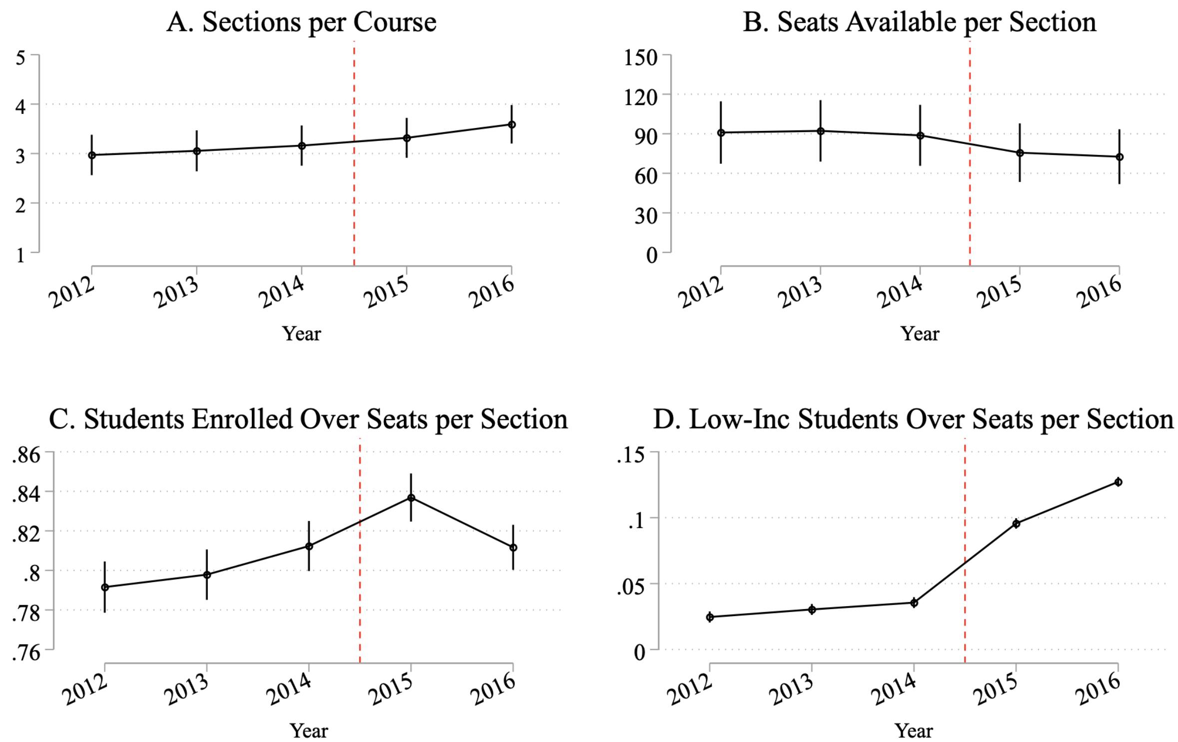
Note: This figure displays the total number of first-term students by socioeconomic (SES) group. Students are classified into three SES groups based on their housing strata indicator. High–SES students are those from socioeconomic strata five to six, while low–SES students are those from socioeconomic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

Figure 3: Percentage of low–SES students by major and before and after SPP



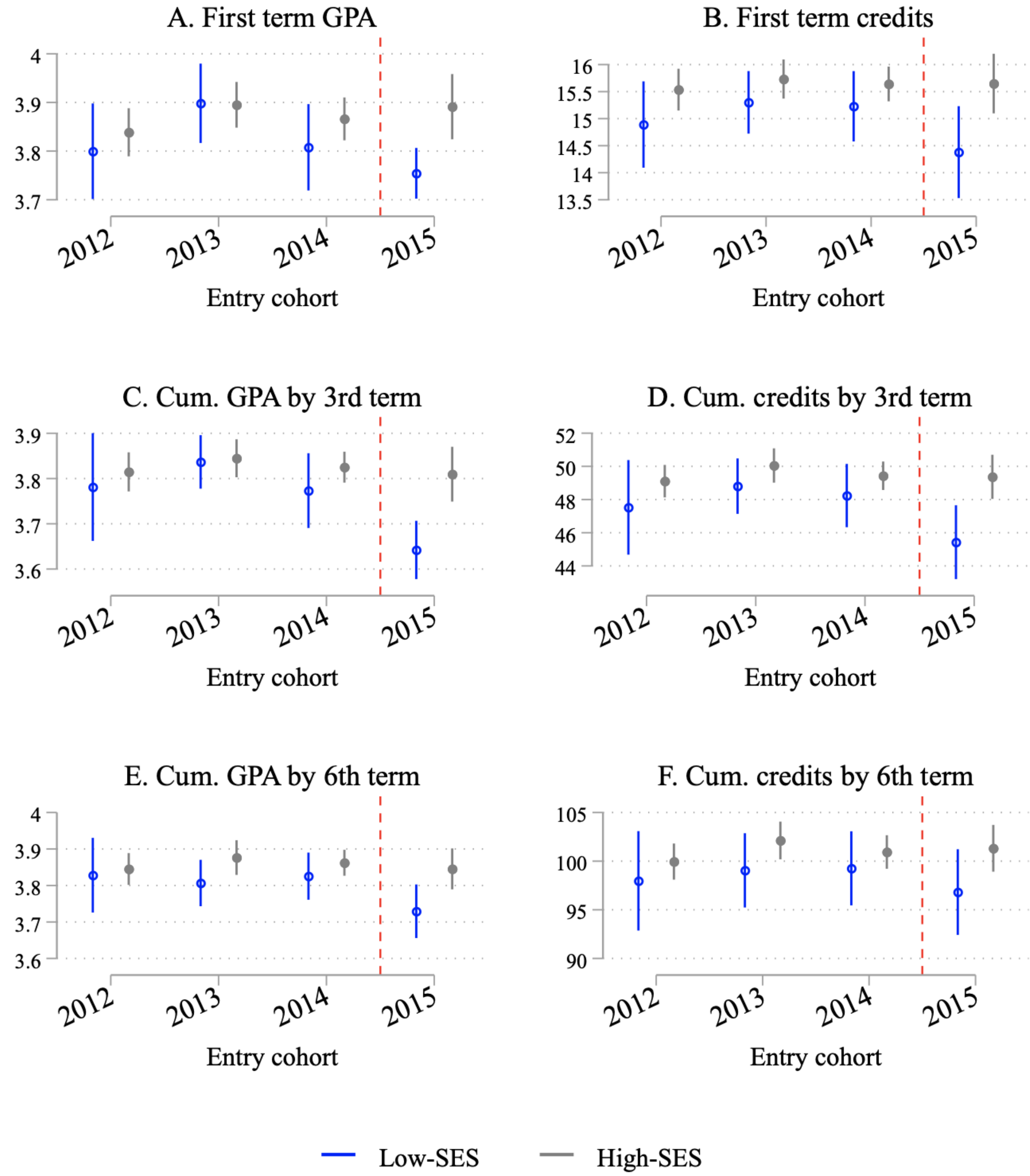
Note: This figure displays the percentage of low-income students by program and entry cohort period. The dotted horizontal lines indicate the 25th, 50th (median), and 75th percentile values of the distribution of low-SES students during the SPP period, 2015-1.

Figure 4: Composition of Courses Taken by First–Term Students at Each Entry Cohort



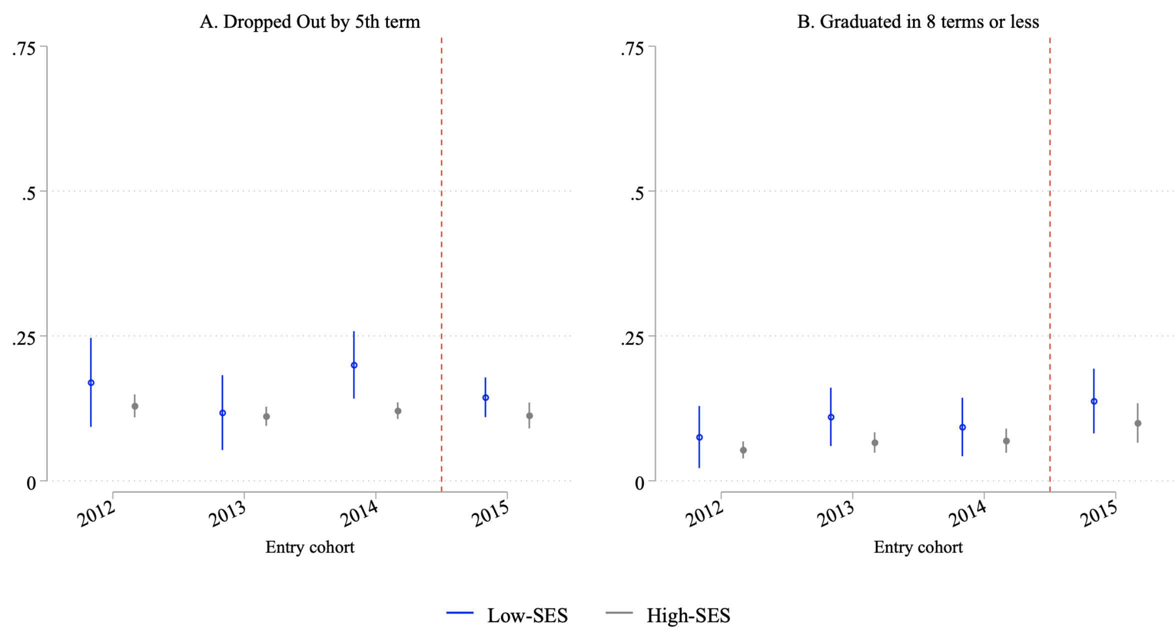
Note: This figure illustrates the composition of courses and classrooms attended by first-term students in each entry cohort. Panel A shows the average number of sections per course, Panel B shows the average number of available spots or seats per section, Panel C shows the ratio of enrolled students 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, and 95% confidence intervals are plotted as a vertical line on each point. The data was 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 onwards).

Figure 5: Achievement Gaps Between High– and Low–SES Students by Entry Cohort



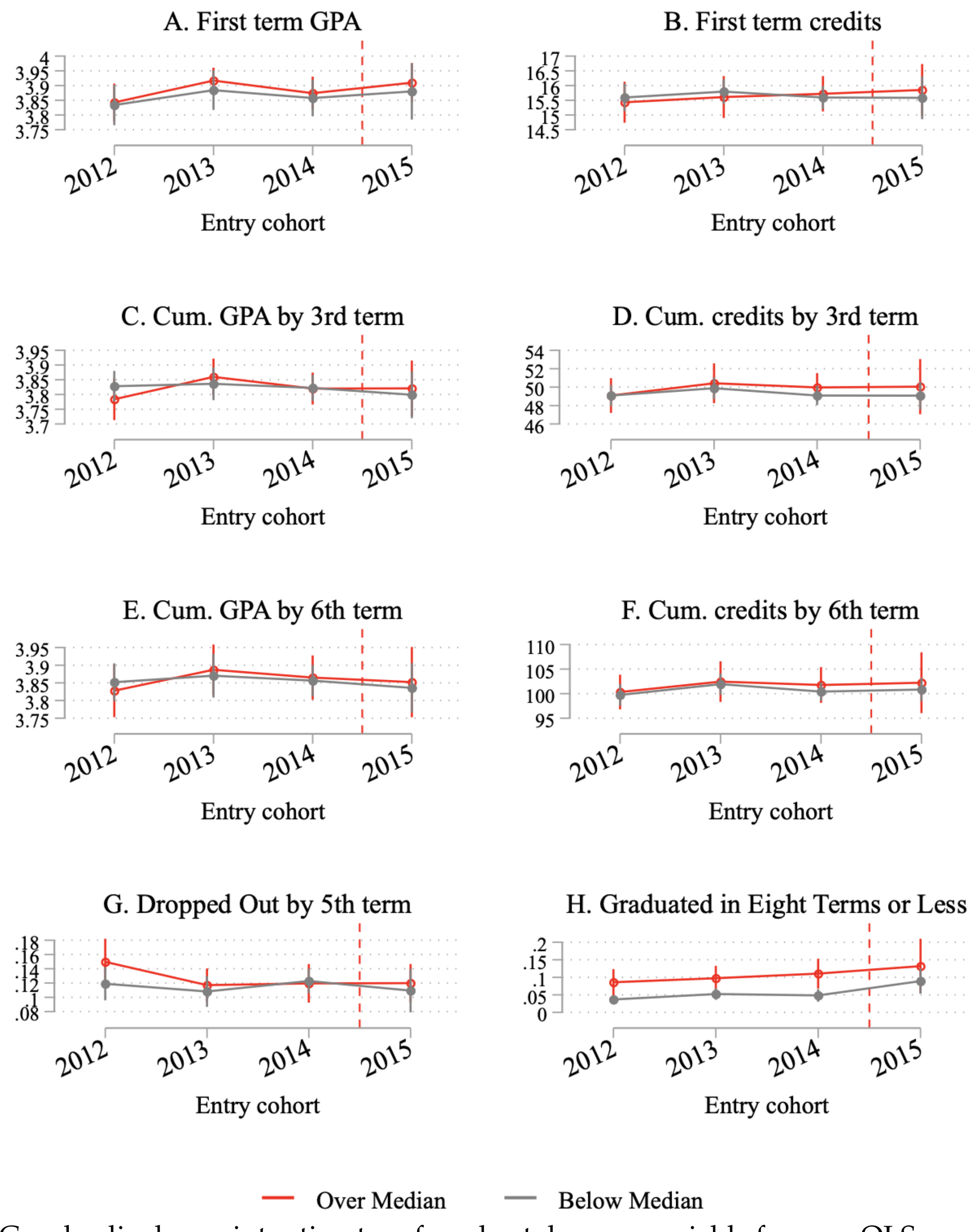
Note: These graphs display the point estimates of a cohort dummy variable from an OLS regression with no intercept, where the dependent variable is the students' GPA, ranging from one to five, with five being the highest grade, or the number of credits attempted. The 95% confidence intervals are shown 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 6: Persistence Gap Between High and Low–SES Students by Entry Cohort



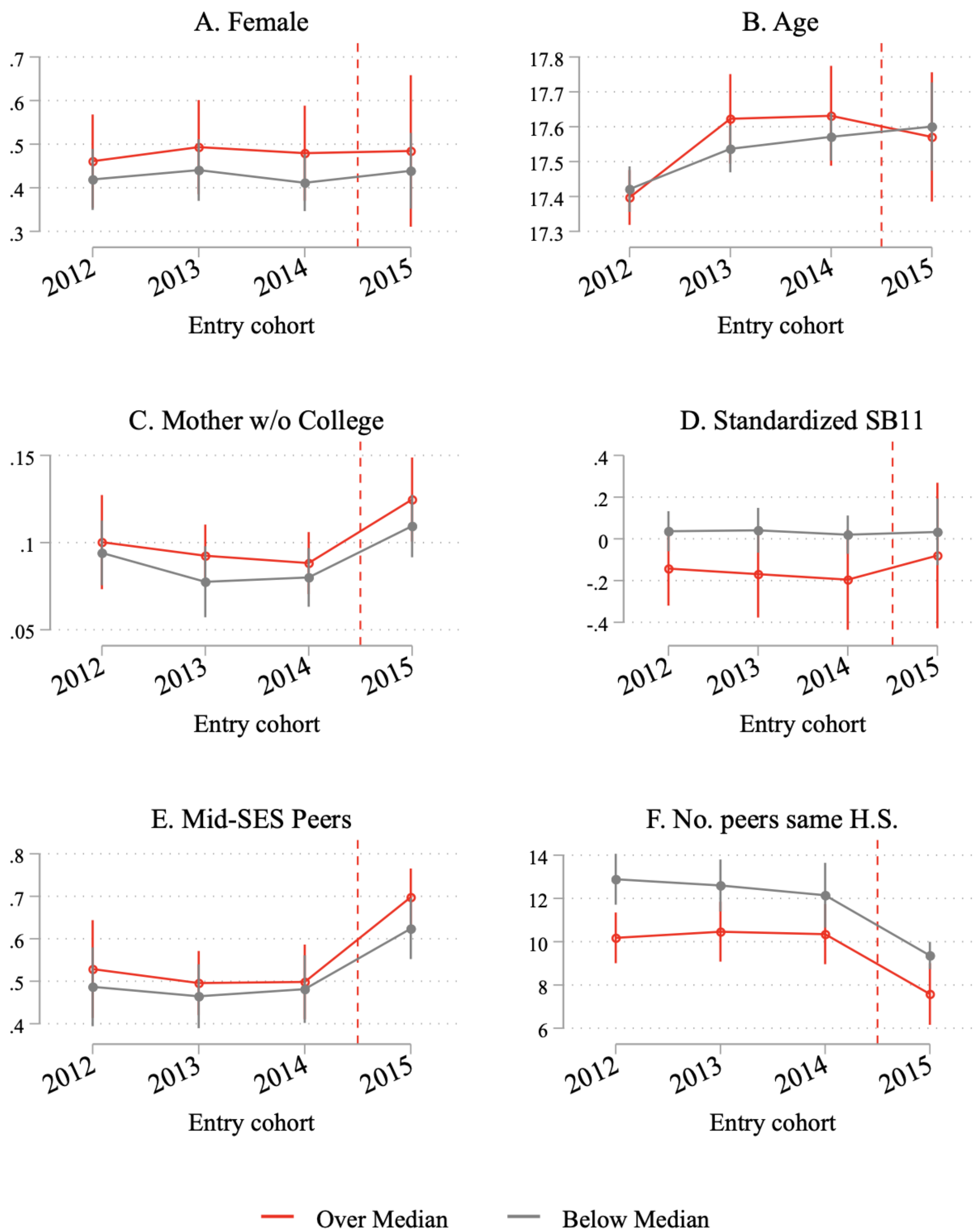
Note: These graphs display the point estimates of a cohort dummy variable from an OLS regression with no intercept, where the dependent variable is an indicator equal to one if the student dropped out by the 5th term, o graduated in less than eight terms. The 95% confidence intervals are shown as vertical 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 7: Pre–treatment Trends on Observed Outcomes - Programs Over and Below the Median Share of Low–SES Peers in 2015



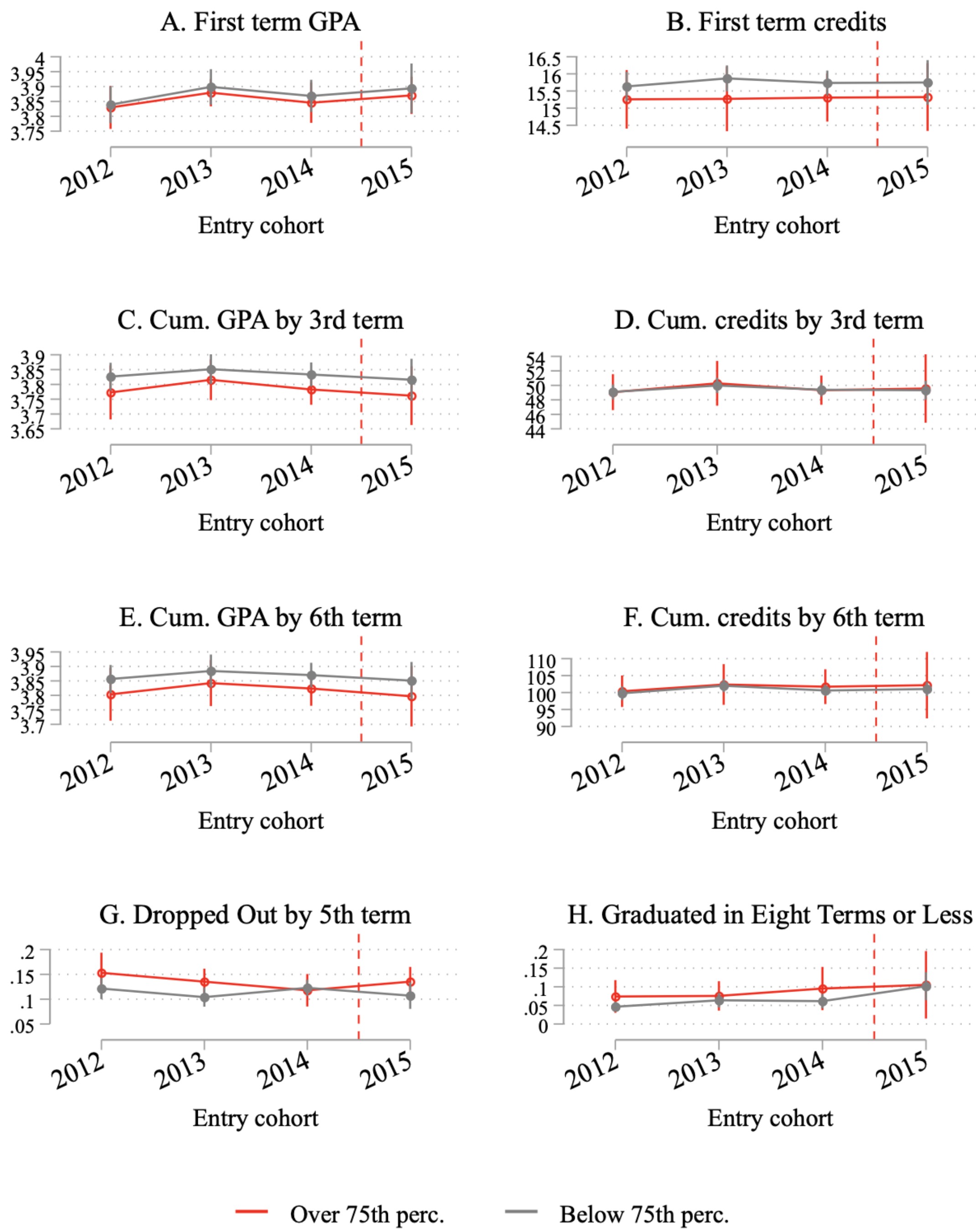
Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student outcomes as the dependent variable. Programs with a low-SES student share above the median value during the SPP period are classified as "Over Median" and others as "Below Median." 95%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

Figure 8: Pre-treatment Trends on Observed Student Socio-Demographics - Programs Over and Below the Median Share of Low–SES Peers in 2015



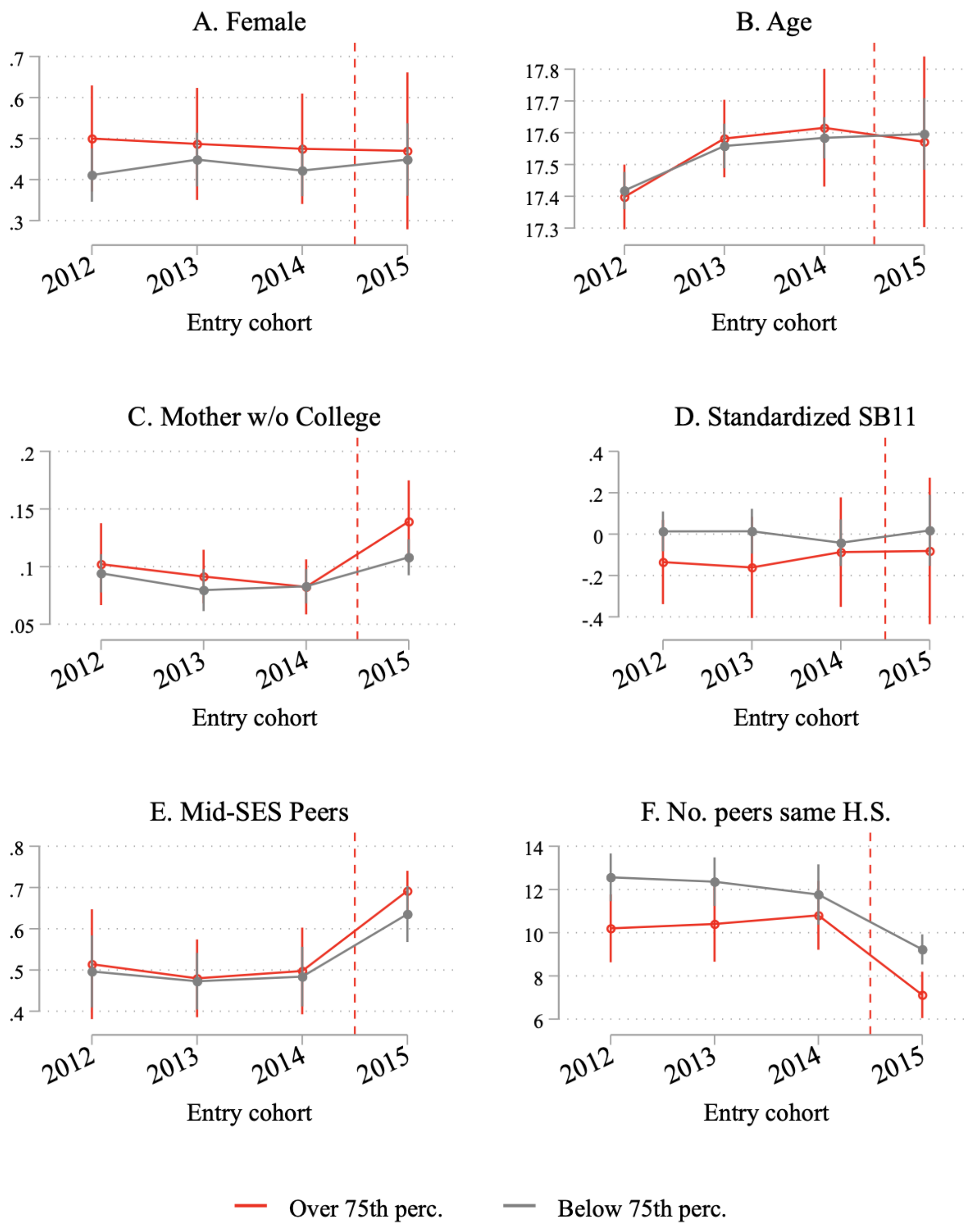
Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student characteristics as the dependent variable. Programs with a low-SES student share above the median value during the SPP period are classified as "Over Median" and others as "Below Median."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

Figure 9: Pre–treatment Trends on Observed Outcomes - Programs Over and Below the 75th Percentile of the 2015 Shares of Low–SES Peers



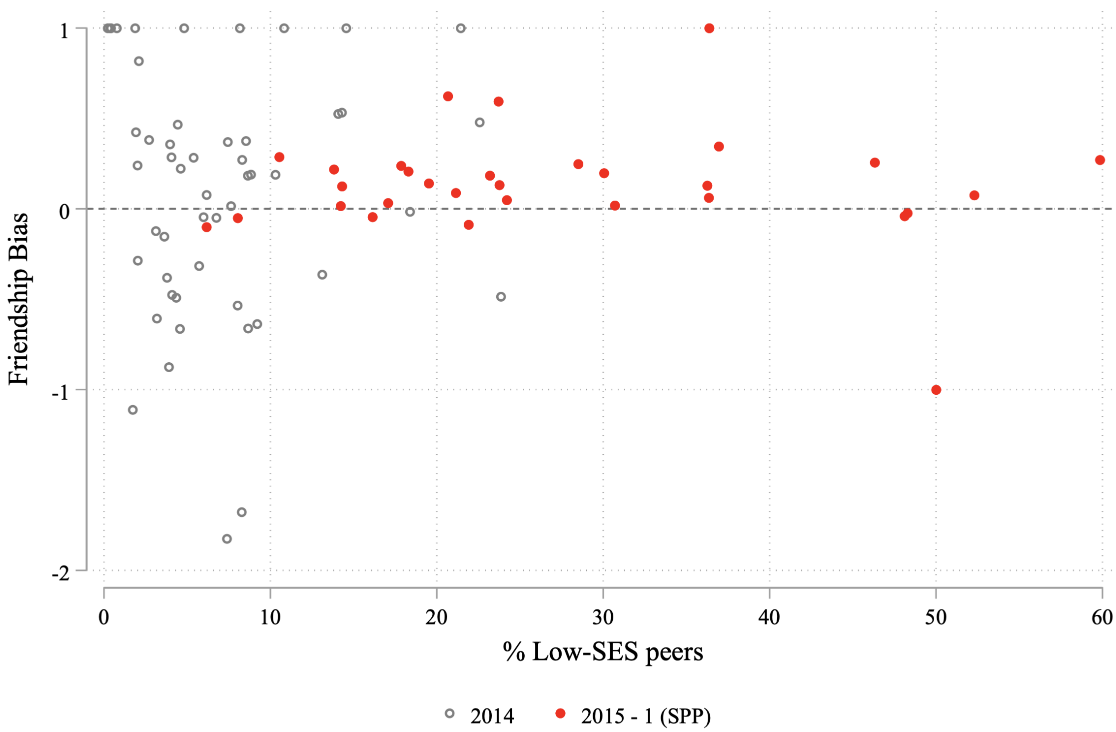
Note: Graphs display point estimates of a cohort dummy variable from an OLS regression with no intercept using student outcomes as the dependent variable. Programs with a low-SES student share above the 75th percentile during the SPP period are classified as "Over 75th perc." and others as "Below 75th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

Figure 10: Pre–treatment Trends on Observed Student Socio-Demographics - Programs Over and Below the 75th Percentile of the 2015 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." 95% confidence intervals use clustered standard errors at the program-cohort level. Yearly entry cohorts include both Spring and Fall, with a red line dividing cohorts pre- and post-SPP, 2015–1.

Figure 11: Estimated Friendship Bias among the 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 of 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 1: 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 only includes the spring term or 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 values test 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 2: Correlation Between the 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	1.117	1.747	-0.179	-0.222
	(0.319)	(0.408)	(0.196)	(0.212)
Average of student characteristics		x	x	x
Major Fixed Effects			x	x
Entry Cohort Fixed Effects				x
No. of program–cohort groups	124	124	124	124

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. The number of high–SES students is the dependent variable and the number of low–SES 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: share of females, average age in years at entry, share of students' whose mothers' does not have college education, SB11 standardized test scores, share of students who are middle–SES, and share of students who are SPP.

Table 3: 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	0.008	–0.001	0.015	0.001	–0.075	0.000	0.001	0.001
	(0.012)	(0.002)	(0.041)	(0.002)	(0.076)	(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	4,892	4,884	4,481	4,481	4,144	4,144	4,892	4,892
No. of Major-Cohort groups	124	124	124	124	123	123	124	124

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 strata two or three (i.e., middle SES), and receiving an SPP loan, and the number of high school peers enrolled in the same cohort of the student. All standard errors are clustered at the program-cohort level.

Table 4: Estimates of the Relationship Between the Percentage 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 percentages of low–SES peers on high–SES students' observed characteristics using Equation 1 and on students enrolling between 2014 and 2015-1. All standard errors are clustered at the program-cohort level.

Table 5: The 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 results using a dummy variable for programs with low–SES student shares above the median and the 75th percentile, respectively. Panel D displays results using 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 strata two or three (i.e., middle SES), and receiving an SPP loan, and the number of high school peers enrolled in the same cohort of the student. All standard errors are clustered at the program-cohort level.

Table 6: The 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. Continous 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 in the same entrance and 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 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 using 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 strata two or three (i.e., middle SES), and receiving an SPP loan, as well as the number of high school peers enrolled in the same cohort as the student. All standard errors are clustered at the program-cohort level.

Table 7: The 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 in the same entrance and 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 with performance above their high–SES peers mean in either Saber 11 ((1) and (4)), or 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 results using 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 strata two or three (i.e., middle SES), and receiving an SPP loan, as well as the number of high school peers enrolled in the same cohort as the student. All standard errors are clustered at the program-cohort level.

A Appendix: Turnstile–Elicited Interactions Data and Validation

Validation of student links definition. I define a time window and frequency thresholds by comparing turnstile-elicited with survey-elicited links among first-term undergraduate students of Economics from the fall of 2017 cohort. The survey was conducted online between December 7, 2017, and January 5, 2018, and elicited the network among 110 economics students from the 2017 fall cohort. The survey was conducted using Qualtrics. Students who completed the survey received a free lunch voucher for a recognized chain restaurant of the campus area. Cárdenas et al. (2022) provide a detail description of the survey.[14]. The survey inquired about two types of links: friendships and acquaintances. Table 8 shows the results of the comparison. The time windows tested in Table 8 were selected based on in-person observations to different entrances. The observations of entrances to campus were conducted between August 26th and 30th of 2019. Because there are multiple turnstiles at each entrance, students walking together can essentially swipe their IDs simultaneously using different scanners, thus the short time–windows. I select a time–window and a 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 the true number 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 8, I ask the reader to focus on the time window of three seconds and the acquaintances survey 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 observe two student IDs swiped on a turnstile entrance so that it resembles an acquaintances link should be minimum twice in the semester. Under that rule, the likelihood of Type I error or false positives - 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 a Type II error or 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-seconds and three times in the term criteria would yield a lower sum of errors, it would do so by leaving one student from the 110 in the sample without turnstile-based links information –an omission I want to avoid. Notice that the acquaintances criteria has a lower threshold in terms of the frequency of the co-movements in the semester than the friendship criteria. I chose to use

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

the acquaintances instead of the friendship criteria 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 as such.

The results in Table 8 indicate that under the baseline definition, it is highly likely that the turnstiles-elicited links capture survey-like links. However, an important share of survey links may not be captured by the turnstiles. This is an issue to the extent that those I do capture are not representative of the survey-elicited links. To assess this, I compare whether turnstile-elicited links plausibly reflect survey-elicited network characteristics. Results are displayed in Figure 12. The goal of this exercise is to estimate how far from random are the turnstile-elicited links' characteristics, 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 minimizing criteria from Table 8 for each of the time windows and randomly assign the number of turnstile-elicited links under that criteria to the 110 students in the sample. Then, I compute the average of the following network individual attributes: age difference, number of courses students are taking together, GPA difference, degree or number of links, and local clustering. I conduct this procedure 1000 times and plot the distribution of the characteristics. I include the average value I observe for the turnstile- and survey-elicited links with its 95 percent confidence interval. I find statistically significant support indicating turnstile-elicited network characteristics resemble closely those of the friendship and acquaintances networks elicited by the survey, and are not the result of random links formation.

The validity of the turnstiles–elicited interactions could be susceptible to the hours of the day during which co–movements are captured. Co–movements captured around lunch hours could be more susceptible to false negatives, whereas co–movements 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 8 but for co–movements happening around lunch–time hours (from 11:40 am to 2:20 pm) with co–movements at other times. The results are displayed in Table 9. For simplicity, I focus on Acquaintances links and on the two and three–seconds windows. As expected, co–movements captured during lunch–time are more susceptible to false negatives than co–movements captures off lunch–time. Similarly, co–movements captured at lunch–time are less susceptible to false positives than those captured at other times. However, the sum of error rates is much higher at either times than that obtained when using all times pooled together as presented in Table 8. These results suggest searching for co–movements at any time of the day is more reliable in terms of reducing measurement error than to focus on co–movements happening at specific times of the day.

Table 8: Survey– and Turnstile–elicited links comparison

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

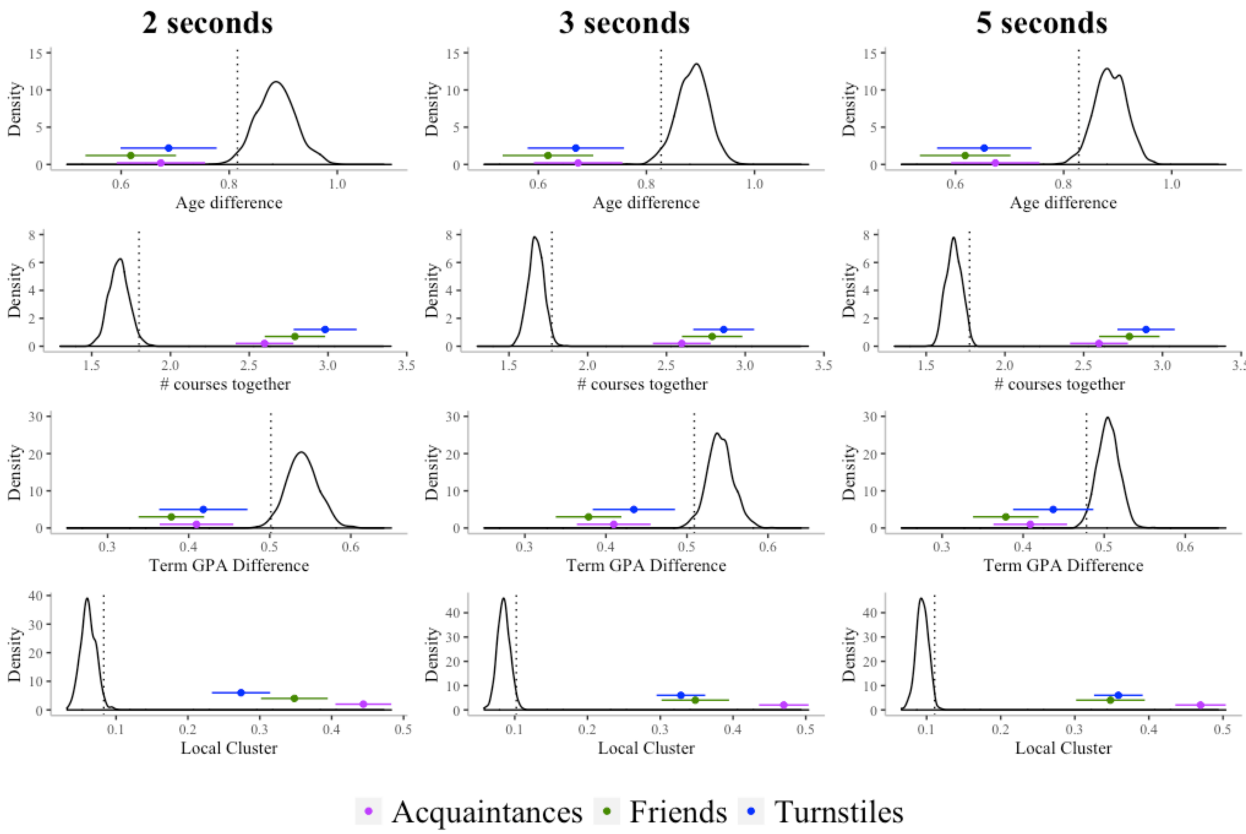
Note: N students = 110. Number of links possible (N*(N-1))/2 = 5995. Survey sample consist of economics undergrads from the August 2017 cohort. 113 students surveyed. One student did not report information and two do not show enrolled as of 2017-2. The survey asked each student who among the 113 students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

Table 9: Survey– and Turnstile–Elicited Links Comparison During and Off Lunch Time

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 show enrolled as of 2017-2. The survey asked each student who among the 113 students were an Acquaintance, and among those, who was considered a friend. Type II error rate is the share of links in survey that were not found in turnstiles-based links. Type I error is the links in turnstiles that were not matched with the links in survey, over the total links in survey.

Figure 12: Comparison against randomly generated distribution



Note: Turnstile-elicited links matched with the survey are randomly assigned in 1000 draws among 110 students forming all possible 5595 dyads. Confidence intervals of 95% confidence are presented. Matches for 2 seconds - 2 times window: 368 links. Matches for 3 seconds - 2 times window: 368 links. Matches for 5 seconds - 3 times window: 552 links. The dotted vertical line points the 95% confidence point.

Measurement Error in Difference–in–Difference Framework: To build understanding about the role of measurement error in social interactions, 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:

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

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

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

Equation 3 implies $\hat{\alpha_P}^{2x2}$ is made of the Average Treatment Effect on the Treated (ATT), which is the difference between the expected values of the outcome L on the post–treatment period and on the treated group t had the treated group received and not received the treatment, plus the non-parallel trend bias. The non–parallel trend bias is the difference in the potential outcomes for the treated and untreated group had no treatment be assigned to any group. I showed in Section 4 that there is no evidence of non–parallel trends bias. But, 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 which I aim to estimate.

To fix ideas, define the number of links I aim to measure $L^{true} = L^{obs} - L^{F(+)} + L^{F(-)}$. That is, true links can be defined as the number of observed links L^{obs} minus the turnstile–elicited links which are false positives $L^{F(+)}$, plus the number of true links which were not captured by the turnstile–elicited measure $L^{F(-)}$ i.e., the false negatives. Then, the ATT 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 $L_t^{0,True}$ for their equivalent based on observed L, and doing some re-arraignment of terms I get:

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

Thus, the estimated ATT can be re-written 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 ATT on the $L^{F(+)}$:

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

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 measurement error variables $L^{F(-)}$ and $L^{F(+)}$ across different majors and cohorts, in such a way that I can use variation in the treatment R^l_{mc} to assess its effects. Since I do not have data of that 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 turnstile–elicited interactions. I use two variables to assess measurement error. First, the total number of ID swipes at the turnstiles for each student. Second, I use the number of courses with turnstile–elicited links. I measure both proxies for the same terms I measure interactions (i.e., sixth and seventh terms after first enrollment).

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

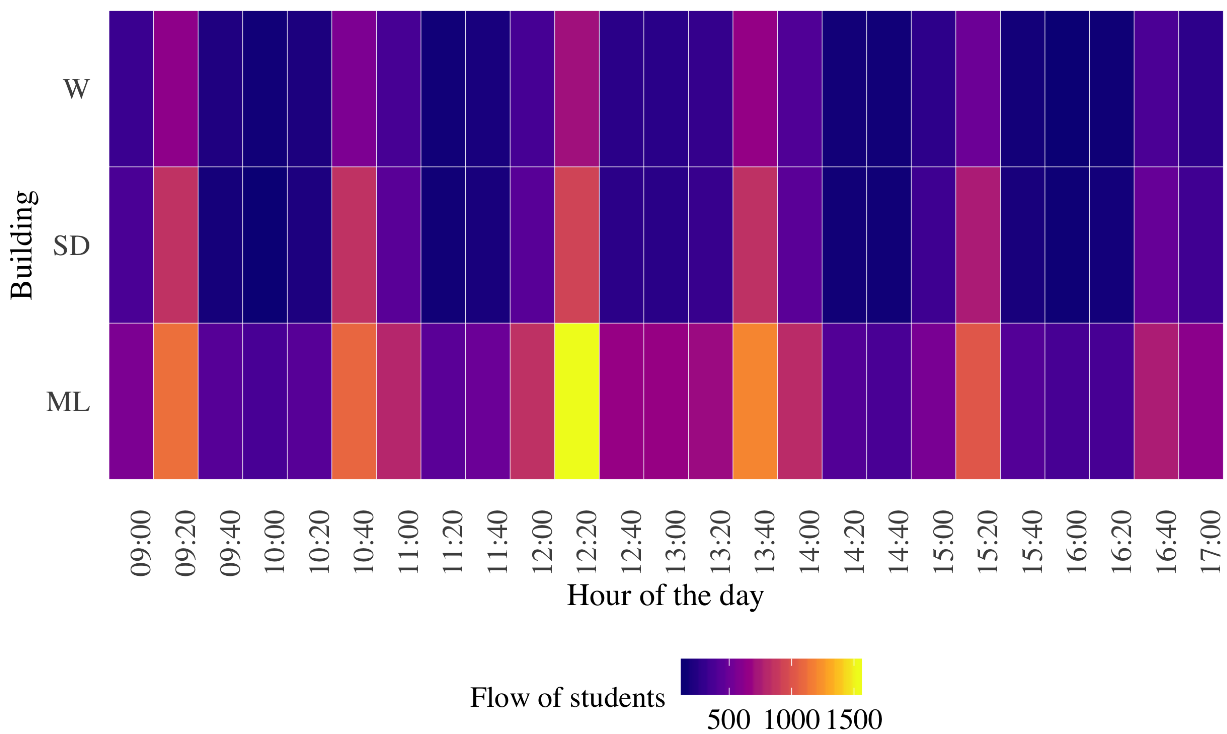
Table 10 displays the results of regressing R^l_{mc} on the measurement error proxies, under each time–window considered. The estimation follows the same structure as that of Equation 1 but using the proxy variables in the left–hand side. I do not find statistically significant evidence of a change in 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. Coupled with the previous results, I conclude there is no evidence to claim measurement error biases the estimated effects of exposure to low–income peers on students interactions or academic achievement.

Table 10: ATT on Measurement Error Proxies

		No. of Courses in the semester interacting with peers in:		
	ID Swipes	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 in the left–hand side. "ID swipes" is the total number of ID swipes of each students, 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 the student took with the peers I defined as a turnstile–elicited link. All estimations include fixed effects by major and entry cohort as well as the covariates described for Equation 1. All standard errors are clustered at the major-cohort level.

Figure 13: Flow of students at selected entrances - term and hour according to turnstiles



Note: Average number of swipes per day, entrance and 20 minutes blocks. Swipes include INs and OUT of the building. Only weekdays during the official academic calendar are included in the data.

B Appendix: Robustness and Validity Checks