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

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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–income students tripled as a result of the introduction of the financial aid program *Ser Pilo Paga*. The average increase in the percentage of low–income peers –9.5 percentage points– had modest to 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–income students. At least half of the increase in interactions between wealthy and low–income students, however, is explained by interactions of wealthy students with low–income but high–achieving students. These results suggest students diversify their interactions primarily among students with similar academic achievement levels.

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). 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 cost 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). This paper focuses on one large private university, where the number of low–income students enrolled tripled in the Spring of 2015 due to SPP,

while leaving the number of wealthy students enrolled unaffected. Because of the short timing of the policy, universities and wealthy students had no time to adjust their application and admission criteria in ways driven by their preferences for low–income peers. As opposed to the U.S., in Colombia, individuals apply to a college and a major in that college, and students enroll directly in their major of application. Thus, the policy induced plausibly random variation in the share of low–income peers across majors and entry cohorts, which I use to capture wealthy students' exposure to low–income peers.

My empirical analysis leverages the plausibly random variation in the percentage of low–income peers within each major and entry cohort to capture the effects of exposure to low–income peers on students' achievement and social interactions. My analytic sample focuses on the entry cohorts right before and after the introduction of SPP (2014 vs. 2015). I conduct tests demonstrating my research design is robust to potential identification threats such as self–selection or crowding out of students from certain majors and entry cohorts, differences in exposure to low–income students due to courses compositions, and measurement error in the turnstile–elicited social interaction outcomes.

I start by discussing the findings on the effects of exposure to desegregation on academic achievement and persistence in college. I find significantly positive yet modests impact 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. Moreover, I do not find impacts on GPA. The average increase in the percentage of low–income peers by major–cohort induced by SPP was 9.5 percentage points, which led to 0.02 and 0.1 more credits attempted by wealthy students by the first and third term, respectively. Both of these are equivalent to 0.04 standard deviations relative to the pre–SPP distribution. These effects are very small, considering one single course at Elite University averages three credits, and students are required to take about five courses each term. Regarding college persistence, I find a significantly positive but small impact on the probability of dropout of 0.2 percentage points – equivalent to 0.06 standard deviations relative to the pre–SPP dropout rate distribution. This impact does not match up with changes in the probability of graduation in eight terms (graduation on time), where there is no effect. Plus, I do not find evidence of differential impacts for students exposed to larger shares of low–income peers – i.e., at the top 25 percent of the distribution (equivalent to > 30 percent of low–income peers), or evidence suggesting adverse peer effects due to the lower average achievement of low–income students.

My findings on the effects of exposure to desegregation indicate modest to null impacts on achievement and persistence. Such results could be explained by a lack of interactions between wealthy and low–income students. Wealthy and low–income students

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may be avoiding interactions with each other if they prefer to keep their interactions with others similar to themselves in their academic performance and socio–economic levels (a.k.a. *homophily*). However, exposure to larger shares of low–income peers should lead to more interactions with them by virtue of their increased presence in the group. Moreover, wealthy students may anticipate positive impacts on pro–social preferences and behaviors from diversifying their social interactions (Rao, 2019; Boisjoly et al., 2006). The second part of my empirical analysis examines whether changes in exposure to the low–income changed the diversity of social interactions.

I examine the effect that the increased exposure to low–income peers had on the number of turnstile–elicited links between wealthy and low–income students and on the probability of a wealthy student forming a link with a low–income peer. A pair of students is linked if they co–move through the turnstiles in a time–window of three seconds or less, in the same direction (entering or exiting a building), and at least twice in a term.[1] Wealthy students have on average 5.2 links in their major and cohort and before SPP, only an average of 0.24 of those links were with low–income peers, while almost 5 were with other wealthy peers. Overall, the probability of a link with a low–income was 19 percentage points. The 9.5 percentage points increase in low–income peers led by SPP, increased the links between wealthy and low–income peers by 0.29 and increased the probability of a wealthy student having any link with a low–income by seven percentage points. This means the number of links with the low–income more than doubled (120 percent increase), while the probability of having any link with a low–income increased in 42 percent. Notably, the increased exposure to low–income peers led to marginal reduction in the number and probability of links between wealthy students (0.08 and 0.005 standard deviations, respectively), suggesting wealthy students networks expanded by including links with the low–income. Also, I examine the effect of exposure to low–income peers on the overall composition of wealthy students' social connections. The 9.5 percentage points increase in the percentage of low–income peers increased the percentage of links with the low–income in 6.8 percentage points, representing an 150 percent increase in the share of links who are low–income.

Importantly, I find large and significantly positive effects on the number and the probability of links with the low–income for students exposed to shares of low–income peers at the top 25 percent of the distribution, suggesting the positive impacts of exposure on the diversity of interactions between wealthy and low–income students persisted among the groups with the most considerable changes in the composition of students.

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[1]Appendix A describes the process through which I arrived to this definition and its limitations in terms of measurement error.

Results to this point indicate wealthy students have significantly more connections with low–income students, without having that affecting their academic achievement. One hypothesis explaining this is that wealthy students form links with low–income students whose academic performance matches or exceeds the performance of the wealthy students in the group. This would explain the positive impacts on the diversity of social interactions and a lack of negative effects on achievement. To test for this, I identify low–income students with an academic performance equal or above the average of the performance of wealthy students in the major and entry cohort, and estimate the effect that exposure to more low–income peers has on the number of interactions with the low–income high–achieving students. I find that about half on the increased number in interactions with low–income 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, 0.15 of the 0.29 increase in links between wealthy and low–income students is with low–income students whose performance is above the average performance of the wealthy students in the major and entry cohort. These results suggest wealthy students are more likely to form links with low–income peers, if the latter are high achievers among the wealthy.

Moreover, I find suggestive evidence that wealthy students with above average academic achievement are also more likely to form connections with high–achieving low–income peers. This is true when looking at the academic achievement during the first–term of college, but not when looking at the achievement pre–college as measured by standardized test scores. Because exposure and first–term achievement are measured simultaneously, these results should be taken with caution. Nevertheless, the lack of impact on academic achievement in the first–term suggest they are highly informative. Thus, I interpret these results as suggestive of homophily in social interactions based on academic achievement, which offsets some of the homophily based on socio–economic status. These findings complement results by Baker, Mayer and Puller (2011); Mayer and Puller (2008); Sacerdote (2001) and Mele (2020) who find academic achievement significantly explains social network formation. It is also consistent with theoretical formulations by Currarini, Jackson and Pin (2009) and Christakis et al. (2010) indicating students have types and have type–dependent benefits from links.

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 stu-

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dents across entry cohorts at selective U.S. colleges finding negative effects, and Bleemer (2021*a*) 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 (2021*a*) 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).[2]

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 un-derrepresented students benefiting from financial aid and affirmative action programs (Bleemer, 2020; Chetty et al., 2020; Londoño-Velez, Rodriguez and Sanchez, 2020; Mello, 2021), 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, 2020).[3]

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 so-

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[2]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.

[3]My work is closely aligned to that of Londoño-Vélez (2020), 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.

cial 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.[4] 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, Fryer and Kaufman, 2006), and finding non–linear responses in interactions to scenarios of minorities reallocation across schools (Mele, 2020).[5] 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*[6]), 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. Im-

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[4]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.

[5](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.

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

portantly, the increased enrollment of low–income students varied across the thirty-one degree majors offered at Elite University. In my research design, I focus on relatively wealthy students and compare students from the entry cohorts before and after SPP (2014 vs. 2015). I use the change in the number of low–SES students across majors 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 socio–economic background. High–quality private universities have exceedingly expensive tuition rates relative to average salaries in the country; supply of public university seats is stagnant; and financial aid is scant (Marta Ferreyra et al. 2017). These factors led students to sort across colleges by socio-economic status (Camacho, Messina and Uribe, 2017). SPP aimed to combat this segregation by providing low-income students a loan that covered tuition plus a small allowance for attending a high-quality accredited institution.[7] The loan was forgiven conditional on completion of the degree. Eligibility 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.[8] 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).

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 the Spring and Fall term of each year and are determined by the applicant's score in the SABER 11 standardized test. Students must apply to a major[9] and entry cohort for which admission officers had pre–determined a specific SABER 11 weighting formula[10] and cutoff score. Second, SPP was widely unexpected by students and higher

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

[8]The 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.

[9]As opposed to the U.S., applicants to higher education must apply to a major for degree as well to as a college.

[10]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 a assigned a

education institutions. SPP was launched in October of 2014 and only students who had taken that October's test were 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 middle– and high–income students enrolled in 2015 remained similar to that from 2014, but the number of low-income students increased significantly.

Figure 1 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 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 2 compares the number of low–SES students across majors, in the entry cohorts before and after SPP. Red bars depict the number of low–income students in the cohort right before SPP (i.e., 2014), whereas blue bars depict the number of low–income students in the first cohort of SPP (i.e., 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 crowding out of wealthy students by new incoming low–income peers, I plot in the secondary axis of Figure 2 the percentage change in the number of wealthy students (i.e., Middle– and High–SES) in 2015, relative to the 2014 cohort, and for each major. If the increase in the number of low–income students across majors had led to crowing out of the wealthy students traditionally attending these programs, then the percentage change in the number of wealthy students should decrease as the number of low–income students increases. Similarly, majors with virtually no change in the number of low–income students should show no percentage change in the number of wealthy students. However, neither of those is the case. Visual inspection of Figure 2 shows that the percentage change in the number of wealthy students enrolled in each major from 2014 to 2015 is not associated with the increase in the number of low–income students. Table 14 from Appendix C shows the correlation between the number of low–income and the number of relatively wealthy students by major and entry cohort. The unconditional correlation suggest increases in the number of low–income students are positively associated with more wealthy students in the major and cohort. Once major fixed effects are included, the correlation between the number of low–income students

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higher weight than the social sciences module

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and the number of wealthy students per major is no longer statistically significant. The size of the estimated correlation also becomes much smaller in magnitude. This is consistent with large majors enrolling more low–income students, a featured that is well captured by the major fixed effect.

The influx in the number of low–income students led to busier classrooms. However, on average, classrooms did not go overcapacity during SPP, and the number of sections offered per course as well as the number of seats available per section remained constant. Figure 3 provides descriptive statistics of the courses taken by first–term students from the 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–income students) per seats in the section. In 2015, classroom occupation peaks, but remains below 100 percent (i.e., 84 percent on average). [11]

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 which 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 to classify students in three socio-economic status (SES): middle- and high-SES – which I will refer to as relatively wealthy students, and low–income students. low–income are students from strata one and two, middle-SES are students from strata three and four, and high-SES are students from strata five and six. Students benefiting from SPP mostly fall in the low–income category. As depicted in Figure 1, the majority of students at Elite University are classified as high- and middle-

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[11]Importantly, students may be self–selecting in different courses than in the pre–SPP periods due to the influx in low–income students. Bias issues associated with these threat to identification will be discussed in Section **??**

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 10 in Appendix A displays a heatmap 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.

Sample. My analytic sample consist of all the first–term students in the entry cohorts before and after SPP (i.e. Fall and Spring of 2014 and 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 pairwise–level students' interactions data using the student ID number which is available in both data sources. My final sample consist of 5,955 students across 31 majors and four 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 wealthy and low–income students in the pre– and post–SPP entry cohorts (i.e., 2014 vs. 2015). I divide the sample between relatively wealthy students – that is, middle–SES and high–SES students, and low–income students, before and after the implementation of SPP (i.e., 2014 vs. 2015). The table includes the mean differences between low–income and wealthy students. In 2015, 81 percent of low–income students at Elite University were SPP recipients. About half of wealthy students are middle–SES students. In both cohorts, wealthy students are more likely to be females, are slightly older and with mothers more edu-

cated than low–income students. Also, wealthy students have higher SB11 test scores than low–income ones, and the gap increases and becomes statistically significant for the 2015 cohort. The gap in SB11 test scores between wealthy and low–income students was 0.10 standard deviations in 2014, but increased to 0.26 standard deviations in 2015.

Wealthy students have on average more links than low–income peers with others in their major and entry cohort (5.21 vs. 4.94 links in 2014), and the difference becomes statistically significant among the 2015 cohort (5.53 vs. 4.60 links). Before SPP, the number of links with other low–income students is statistically the same among wealthy and low–income students (0.24 vs. 0.35, respectively). But, in 2015, wealthy students have significantly fewer links with other low–income than their low–income peers (0.59 vs. 1.73, respectively). Importantly, wealthy students have on average more peers from high school enrolling at Elite University in their same cohort than low–income students in both pre– and post–SPP cohorts (11.54 vs. 3.17 in the 2014 and 11.73 and 1.98 in the 2015). There are no statistically significant differences between the number of ID swipes at the turnstiles of wealthy and low–income students for either the 2014 or the 2015 entry cohorts.

Table 1 also describes the average characteristics of the links of both wealthy and low–income students. The characteristics of the links of wealthy and low–income 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). But in the 2015 cohort, wealthy students exhibit a larger share of same–gender links than low–income students (0.51 vs. 0.45 links), and have a larger SB11 test score difference with their links (0.79 vs. 0.67). The latter suggest wealthy students enrolling in 2015 exhibit less diversity in their social interactions in terms of certain demographics, but more in terms of pre–college academic achievement, relative to the low–income peers.

Students' academic achievement: I characterize the differences in academic achievement between wealthy and low–income students in Figure 4 and Figure 5. For these figures, I take advantage of the administrative data availability and plot the trends in academic achievement across the entry cohorts starting with 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 4 displays performance indicators, mainly cumulative GPA and total credits attempted, whereas Figure 5 describes persistence (dropout rates and graduation). I label a student as a dropout if they do not show up as enrolled during two consecutive terms after their fifth term of college. Similarly, I label a student as graduated 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 wealthy and low–income 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–income students having on average lower cumulative GPA and fewer attempted credits than their wealthy peers. For example, the GPA of pre–SPP cohorts is relatively constant and close to 3.85 for both wealthy and low–income students. But for the SPP cohort, the GPA of low–income 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 wealthy students remains the same. Regarding the cumulative number of credits attempted the pre–SPP cohorts of wealthy and low–income students have attempted, on average 50 and 48 credits by the third term, respectively. But in the SPP cohort, low–income students have on average attempted 45.7 credits while wealthy students continued to attempt on average 50 credits. A course at Elite University usually bears three credits. This means that low–income students enrolling in 2015 had attempted on average at least one class less than their wealthy students peers by the third term of college, and with a cumulative GPA that is 0.25 below that of their wealthy peers. Nevertheless, the differences in achievement did not paired with differences in dropout or graduation rates, suggesting the relatively low achievement of low–income students did not translate in diminished persistance.[12]

4   Identification Strategy

In this paper, I use a difference–in–differences strategy to examine the impact of increased exposure to low–income students on academic achievement and social interactions. In particular, I focus on the impacts on wealthy students, and I exploit the variations in the percentage of low–income students enrolling in each cohort and across the different majors at Elite University. Specifically, I estimate:

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

In equation 1, $Outcome_i^{mc}$ represents either the academic achievement outcome of a rel-

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[12]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 degree. 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 explains their slightly higher likelihood of graduation.

atively wealthy student i enrolled in major m and entry cohort c or the number of links of the student with other low–income peers. R^l_{mc} is the percentage of low–income students in student i major and entry cohort i.e., $R^l_{mc} = \frac{N^l_{mc}}{N_{mc}} * 100$, where N^l_{mc} is the number of low–income students and N_{mc} is the total number of students enrolled in student i major m and entry cohort c. Figure 2 describes the variation exploited for causal identification. Relative to 2014, the percentage of low–income students R^l_{mc} increased for the 2015 entry cohorts at different rates across majors, without leading to crowding out of wealthy students. Equation 1 includes controls for student i characteristics that can determine the relation between the exposure to low–income students, and achievement and social interactions. Specifically, $\mathbf{X_i}$ is a matrix of female, mother with no college education, and middle–SES indicators, as well as standardized Saber 11 (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 major and entry cohort levels. I estimate Equation 1 using Ordinary Least Squares.

Identification of β_l relies on three assumptions: in the absence of the treatment, the outcomes exhibit the same trends for the treated and control groups (i.e., the parallel trends assumptions), there is no self-selection in exposure to low–income students within major-cohorts groups of students (i.e., unobserved exposure effects), and the measurement error in the turnstile–elicited links do not contain non–random variation confounding the effect of exposure to the low–income (i.e., measurement error in turnstile–elicited interactions). Table 10 summarizes the identification challenges and the exercises I conducted to assess them. The table refers to the Figures and Appendix sections in which I provide details of the analyses conducted.

The parallel–trends assumption implies that any differences in the outcomes of wealthy students across cohorts and within majors can be attributed only to changes in exposure to low–income peers, thus ensuring causality. Violations to this assumption imply there is non–random allocation of wealthy students across majors and entry cohorts driven by the change in the percentage of low–income peers. Specifically, at the outset of SPP, wealthy students could have self–selected in majors and entry cohorts due to their preferences for low–income peers. Alternatively, the influx in low–income students could have crowed out wealthy ones. To test for this, I examine the trends in the outcomes and student characteristics using an event study.[13] Results are displayed in Figure 6 for student

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[13]Namely, I estimate the following equation for both outcomes and student characteristics:

$$Y_{imc} = \sum_{c=2012}^{c=C} \mu_{lC} R^l_{m,c=C} + \mathbf{X'_i} M + \mu_m + \mu_c + \varepsilon_{imc}$$

characteristics, and in Figure 7 and 8 for student academic achievement and persistence indicators. The estimated effects are the same across all the pre– and post–SPP entry cohorts. Thus, the results suggest that the allocation of wealthy students across majors and cohorts did not change, and its relation with the percentage of low–income peers in the group did not change with SPP. Coupled with the tight timing of the policy, which gave Elite University no time to adjust admission criteria in ways that would crow out students based on their eligibility for admission (see Figure 2 and Figure 3), I conclude there is no evidence indicating the parallel–trends assumption is violated in this context.

The second bias concern deals with measurement error in the turnstile–elicited interactions and the risk of capturing random co–movements across the turnstiles, thus falsely depicting the effects of exposure to low–income on students' social interactions. 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.[14]

Appendix A provides details on the secondary data and the computation framework and procedures I use to assess measurement error. I find that turnstile–elicited links suffer a relatively large rate of false–negatives of approximately 60 percent, but a rate of false–positives below the 10 percent (see Table 11). To assess the extent to which measurement error can diminish the quality of the turnstile–elicited interactions, I compare the average characteristics of turnstile–elicited links with those from survey–elicited links and with 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

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[14]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. (2019) provide a detail description of the survey. I am very grateful to Professor Tomás Rodríguez-Barraquer for providing me access to these data.

statistically the same as those from the survey links, but different to those that would be obtained if links were obtained purely at random (see Figure 9). I rationalize measurement error in a difference–in–difference 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 researchers, 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–income peers affecting any of these measures (see Table 13). In summary, I do not find evidence that measurement error in turnstile–elicited interactions bias my estimates on the impacts of exposure.

Another potential source of bias deals with unobserved exposure to low–income peers due to the composition of first-term courses taken by each wealthy student in a major and cohort. Exposure to low–income students operates not only through majors and cohorts but also in the classroom. Several of the courses offered to first–term students are open to multiple majors, which may lead to variations in exposure to low–income peers not accounted for by my initial difference–in–differences research design. For example, students at all the Engineering, Economics, and Business programs must take Differential Calculus in their first term. As a result, students in Business majors –which had virtually no change in the number of low–income students enrolled, may take Calculus with low–income peers from other majors, thus being exposed to low–income students in ways not accounted for by the entry cohort and major variation captured by R_{mc}^{l}. If students within the same major and entry cohort deferentially self–select to take specific first–term courses based on preferences for low–income peers, the estimated β_l could be biased even after accounting for unobserved variation common to majors and cohorts. I test for the presence of this bias by exploiting the within major–cohort variation in the number of low–income students at each of the wealthy students' first term courses. To ensure exogeneity, I instrument the number of low–income peers by its predicted allocation across courses based on courses enrollment data from 2012 and 2013. Appendix B describes the methodological details and results from this test. Overall, I do not find evidence that student selection of first–term courses is biasing my Difference–in–Differences estimates.

5 Results

5.1 Effects of desegregation on achievement

Table 2 displays the estimated effect of the increased exposure to low–income peers on relatively wealthy students' academic achievement and persistence. Panel A displays OLS estimates of β_l for all the outcomes discussed in Figures 4 and 5. The size of the estimated effect on wealthy students' achievement is modest and not statistically different to zero for GPA and graduation outcomes. The point estimates for the number of credits attempted by first and third terms and for dropout probability by the 5th term are positive and statistically significant, but the size of the point estimate is small relative to the standard deviation of the variables. Namely, I estimate 0.02 additional credits attempted by the first term and 0.3 credits by the third term (0.04 standard deviations), and an increase in dropout probability 0.2 percentage points (0.06 standard deviations) when evaluated at the mean change $\Delta mu(R_{mc}^l)$=9.5 percentage points. Importantly, the estimated effect on dropout does not pair with impacts on graduation probability, which are estimated to be zero. These results suggest the increased exposure to low–income peers has if anything a positive yet small impact on the academic achievement of wealthy students, albeit a small negative effect on persistence which has not translated on impacts on graduation.

Prior literature has found non–linear effects of peers on academic achievement (Garlick, 2018; Zimmerman, 2003). Hence, Panel B of Table 2 estimates the effect of exposure when the increased percentage is at the top 25th percentile of the distribution (i.e., using as treatment variable an indicator equal to one when the percentage of low–income peers exceeds 30 percent). I do not find evidence of non–linear effects, which suggest even students in groups with the largest shares of peers are not affected in their academic achievement.

In summary, I do not find evidence that exposure to low–income peers has negative impacts on the academic achievement of wealthy students and if anything, I find suggestive evidence of a positive impacts on the number of credits attempted, albeit a small yet significant impact on academic persistence as measured by dropout probability by the 5th term of college. Importantly, the measures of persistence are relatively premature, as by the time of the data collection only few students had started to graduated (for example, graduation in 8 terms –which is considered graduation on time, was only at the 7 percent among wealthy students). Plus, my measure of dropout considers only college–level dropout as I am unable to see whether these students are enrolled at a different university, meaning transfers to other universities cannot be ruled out. Overall, my findings speak to prior literature finding no effects of desegregation on receiving students

achievement in K–12 settings(Angrist and Lang, 2004), contrast with findings from (Arcidiacono and Vigdor, 2010) on peer effects from minorities to White and Asian students on higher education settings, and complement the findings from Bleemer (2021b), who finds re–segregation has no impact on White and Asian students performance at selective universities in California.

Exposure to low–achievers could impact the performance of wealthy students, even after accounting for the percentage of low–income students in the group. For example, low–achievers could also exhibit disruptive behaviors impacting wealthy students (Carrell, Hoekstra and Kuka, 2018), and they could reduce the incentives of wealthy students to exert effort. Figure 4 showed low–income students brought by SPP have a significantly lower academic achievement than their wealthy peers. Moreover, Table 1 showed incoming low–income students had on average a lower performance than wealthy students even before enrolling in college, as shown by their average SB11 score. To test this, I expand Equation 1 by including average measures of achievement of low–income peers, namely their average achievement outcome and their average SABER 11 test score. Specifically, I estimate:

$$Y_i^{mc} = \eta_{l2} R_{mc}^l + \eta_{S\tilde{B}11} S\tilde{B}11_{mc} + \mathbf{X_i'} H_2 + \eta_{2m} + \eta_{2c} + u_{2imc} \tag{2}$$

$$Y_i^{mc} = \eta_{l1} R_{mc}^l + \eta_{\tilde{Y}} \tilde{Y}_{mc} + \mathbf{X_i'} H_1 + \eta_{1m} + \eta_{1c} + u_{1imc} \tag{3}$$

As coined in the peer effects and Linear–in–Means–Models literature (Manski, 1993; Moffitt, 2001), Equation 2 captures the exogenous effect of low–income prior achievement on wealthy students performance, and Equation 3 captures the endogenous effect of low–income performance on wealthy students' performance –that is, the relation between the outcome of low–income peers with those of wealthy peers. Specifically, $S\tilde{B}11_{mc} = \frac{\sum_{j\neq i}^{N}[SB11_j|W_j=0]}{N_{mc}^l}$, and $\tilde{Y}_{mc} = \frac{\sum_{j\neq i}^{N}[Y_j|W_j=0]}{N_{mc}^l}$ where $W_j = 0$ if the peer j in student i major m and entry cohort c is low–income, and N_{mc}^l is the number of low–income students in student i major and entry cohort. Estimates of $\eta_{S\tilde{B}11}$ and $\eta_{\tilde{Y}}$ capture whether low–income students' SB11 test scores and academic achievement explain wealthy students' performance.

Table 3 displays the results of this analyses. Panel A displays results from estimating Equation 2. The estimates of $\eta_{S\tilde{B}11}$ are different from zero in the case of first term credits and for sixth term GPA, suggesting low–income peers' SB11 is positively associated with the number of credits attempted in the first term by wealthy students. However, the sign of the estimate inverts to negative in the sixth term. Panel B displays the results from 3. With the exception of first term credits attempted, the estimated $\eta_{\tilde{Y}}$ is not different to

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zero for achievement and persistence outcomes. Overall, these results suggest the performance of low–income students has some relation with the number of credits attempted in the first term by wealthy students, but the effect disappears by subsequent terms. Specifically, SB11 test scores are positively associated with more credits attempted in the first term by wealthy students, by more credits attempted in the first term by low–income students has a negative association with those of wealthy students.

5.2 Effects on Social Interactions

The peer effects literature would suggest increased exposure to low–achieving students 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–income students have on average a lower performance than their wealthy peers (see Figure 4). A possible hypothesis explaining the lack of effects is that segregation between wealthy and low–income students persist within major–cohort groups. In fact, findings from Carrell, Sacerdote and West (2013) suggest assignment of low–achieving students to relatively high–achieving groups can lead to segregation between the two groups. To test if segregation between low–income and wealthy students explains the lack of peer effects on achievement, I proceed to estimate the effects of exposure to low–income students on the diversity of social interactions of wealthy students.

I characterize social interactions as follows. First, I measure the effect of increased exposure to low–income peers on the probability of having at least a link with a low–income peer in their group. Second, I estimate the effect on the number of low–income links formed by each wealthy student. Third, I measure the effect on the friendships' composition of wealthy students, which I define as the percentage of links with low–income peers. The first and second measure can be interpreted as extensive and intensive margin effects, respectively. The third describes how much the connections of a student diversify in response to the desegregation in their group.

Table 4 displays the results of estimating Equation 1 on wealthy students' interactions with their peers. Panel A displays the estimated effects on the probability of interactions with wealthy and low–income peers, panel B displays the estimated impacts on the number of interactions, and panel C displays the impacts on the percentage of links with the low–income. Panel 1 shows linear estimates following Equation 1, whereas Panel 2 shows non–linear estimates that compute the effects when exposure to low–income peers is at the top 25th percent of the distribution in 2015 (i.e., use an indicator equal to one when the percentage of low–income peers exceeds 30 percent).

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Exposure to low–income peers had a positive impact on the diversity of interactions of wealthy peers. Focusing on Panel 1 of Table 4, I find that the average increase in the percentage of low–income peers (9.51 percentage points) had significantly positive effects at the extensive and intensive margin of interactions between wealthy and low–income students sharing a major–cohort group. That is, the average increase in the percentage of low–income peers increased the probability of having a low–income link in 0.08 points and increased the number of links with low–income peers on an average of 0.3 links. Relative to the probability and number of links among the pre–SPP cohorts, these changes represent an increase in the probability of 40 percent and an increase in the number of interactions of 120 percent. As a result, the 9.5 percentage point increase in exposure to low–income peers translates into a 6.8 percentage point increase in the percentage of links that wealthy students have with other low–income peers.

Importantly, the probability and the number of links with other wealthy peers show a significantly small decrease. The average increase in the percentage of low–income peers decreased the probability and the number of links with other wealthy peers in 0.02 points and in 0.41 links, respectively. These represent a decrease of 0.04 and 0.08 standard deviations, relative to the pre–SPP distribution, and a decrease of 2 and 8 percent relative to the pre–SPP mean.

The effects of exposure to low–income peers on the diversity of social interactions are significantly larger at the top 25th percentile of the distribution. Students who were exposed to shares of low–income peers over the 30 percent have 0.68 more low–income links, have an increase in the probability of having at least one low–income link of 1.2 points, and have an increase in the percentage of low–income links of 14.57 points. Hence, the positive effects on diversity of social interactions persist even for those students experiencing the largest exposure to low–income peers.

In summary, exposure to low–income students has significant and positive impacts on the diversity of social interactions of wealthy students, at both the extensive and intensive margin. 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.[15]

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

Previous research (Carrell, Fullerton and West, 2009; 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–income peers who were also on average low–achievers relative to the traditionally privileged students 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 peers in the major and entry–cohort.

To examine the role of the academic achievement of low–income students, I estimate the effect of exposure to low–income students on the links with low–income peers who are also high achievers in the group. Table 5 displays the results. Similar to Table 4, I compute the effects of the changes in exposure to low–income 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–income 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–income 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 B of Table 5, I find that the average increase in exposure to low–income students of 9.5 percentage points led to 0.12 more links with low–income students with above average SB11 test scores, 0.15 more links with low–income students

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[15]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 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.

with above average first term GPA, and 0.20 more links with low–income students with above average credits attempted in the first term. Relative to the initial estimated effect of exposure on links with any low–income student of 0.3, the different measures of achievement explain 41 percent, 52 percent and 68 percent of the estimated effect described in Table 4, respectively. Arguably, that 68 percent of the links with the low–income can be explained by the number of credits attempted suggest more exposure through more hours of class of low–income peers, is a likely channel explaining the increase in diversity.

Similarly, Panel A of Table 5 displays the impacts on the probability of interaction with the low–income by the different achievement measures. Wealthy students exposed to an increase of low–income peers of 9.5 percentage points are 0.06 points more likely to interact with a low–income student with above average SB11 achievement, 0.06 points more likely to interact with a low–income student with above average first term GPA, and 0.08 points more likely to interact with a low–income 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–income peer, I find that high achievement in terms of SB11 and GPA among low–income students explains 75 percent of the effect on the probability of interaction with any low–income student. As with the intensive margin effects, almost a 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, explaining also the lack of negative impacts on academic achievement.

Above average academic achievement among wealthy students may also contribute to explain the increased diversity in social interactions. This would be consistent with homophily in social interactions based on academic achievement. Scholars who have analyzed homophily in educational settings find that while race and gender are strong determinants of social interactions, academic achievement is also a significant driver of network formation (Baker, Mayer and Puller, 2011; Mayer and Puller, 2008; Sacerdote, 2001; Mele, 2020).

To test for this, I build on the previous analysis and study differential effects on exposure to low–income students among wealthy students whose academic achievement is equal or above the average of that of their other wealthy peers. Because I am interested in looking for patterns of homophily based on achievement, I consider as outcomes the links with low–income students who also have an achievement equal or above the average of their wealthy peers. Specifically, I build on Equation 1 by including an indicator variable

for achievement, which I interact with the treatment variable R_{mc}^l.[16]

Results are displayed in Table 6. I focus on above average academic achievement as measured by SB11 test scores of both wealthy and low–income students. Since SB11 is measured pre–college, these differential impacts are not likely to be endogenous to the exposure to low–income peers. The results in Table 6 indicate high–achieving wealthy students are not differentially more likely to interact with low–income high–achieving peers when exposed to larger shares of low–income students.

Results from Table 5 suggest first–term achievement of low–income students is particularly important in driving diverse social interactions. To better understand whether the same would apply to wealthy students, I reproduce the results from Table 6, but using first–term GPA and credits attempted as a measure of achievement for wealthy students. One important caveat of these results is that first–term achievement is measured with the exposure to low–income peers. Thus, the simultaneity of exposure and achievement complicates the interpretation of these estimates, albeit the fact that results from Table 2 indicate there is no effect of exposure on wealthy students first–term achievement.

Estimates of the effects of exposure on the diversity of interactions among wealthy and low–income students who are high–achievers in their first–term are displayed in Table 7. Wealthy students with above average first–term GPA are more likely to increase their interactions with other low–income peers with high first–term GPA, when exposed to more low–income peers. The same is true when measuring achievement by first–term credits attempted. Relative to the results in Table 6, I find that the impact on interactions with low–income high–achieving peers is significantly larger among wealthy students who are also high achievers.

The results from Tables 6 and 7 suggest partial homophily based on academic achievement as driver of diversity of social interactions. It is partial in that it is observed when looking at first–term achievement, but not when looking at pre–college measures like standardized test scores. These results are suggestive of homophily based on academic achievement offsetting segregation based on socio–economic status. This behavior would be consistent with models where students have types and see type–dependent benefits from links (Currarini, Jackson and Pin, 2009), and with models where social networks are

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[16]Specifically, I estimate:

$$L_{imc}^{w=0,HighAchiv} = \alpha_d(R_{mc}^l * HighAchiever_i) + \alpha_l R_{mc}^l + \alpha_p HighAchiever_i + \mathbf{X_i'}A + \alpha_m + \alpha_c + \varepsilon_{imc}$$

Where $HighAchiever_i = 1$ if student i achievement is equal or above the average of their wealthy peers and equals zero otherwise. Similarly, $L_{imc}^{w=0,HighAchiv}$ represents the number of interactions with low–income students $w = 0$ whose achievement is equal or above the average of the wealthy peers in the same major and entry cohort mc

formed strategically with others of similar traits to maximize individual utility (Christakis et al., 2010; Jackson, 2008; Graham, 2015). However, these results would suggest achievement is either needed to be observed in the first–term by both low–income and wealthy students, or that it is driven by more hours of exposure through more credits attempted. In the next section, I address some of this potential endogeneity through courses exposure as a robustness check.

7 Robustness Checks

Exposure to low–income students operates not only through majors and cohorts but also in the classroom. Several of the courses offered to first–term students are open to multiple majors and offer multiple sections, which may lead to variations in exposure to low–income peers not accounted for by my initial difference–in–difference research design. For example, students at all the Engineering, Economics, and Business programs are required to take Differential Calculus in their first term. As a result, students in Business majors –which had virtually no change in the number of low–income students enrolled, may take Calculus with low–income peers from other majors, thus being exposed to low–income students in ways not accounted for by the entry cohort and major variation captured by Equation 1. Since this is the case for several other mandatory first term courses, the aggregated number of low–income peers a student may have in all their first–term courses may be much different than that captured by the major–cohort exposure utilized in the difference–in–difference strategy described so far.

To test for this source of bias, I exploit changes in the number of low–income peers wealthy students have throughout their first term courses and within majors and cohorts. In contrast to the difference–in–difference approach discussed in Section 4, this design does not rely on the quasi–random allocation of low–income students across majors and entry–cohorts, but on the change in low–income peers across all courses taken by a student and conditional on within major and entry cohort common shocks. To ensure the causal identification of the effects of individual–level exposure, I instrument the exposure to low–income peers across courses with its predicted number based on historical data on courses composition. I use the instrument to capture the exogenous variation in the size of exposure to low–income peers in first–term courses, had the distribution of low–income students not changed with the outset of SPP. This design allows me to assess the importance of individual level variations in exposure to low–income peers in explaining achievement and social interactions.

Equation 4 describes the specification I use to capture the effects of courses-based ex-

posure to low–income peers. Y_i^{mc} represent the outcome of the relatively wealthy student i in a major and entry cohort group mc. The estimand of interest is ρ_l, which captures the response on the outcome to changes in the number of low–income peers IN_{imc}^l. Appendix B provides details on the calculation of this variable. Exposure to low–income peers may be driven by unobserved non–random aspects which are common to students in the same major and entry–cohort and which are related to academic achievement and may lead to differences in student friendships (number of courses required in the first term, size of courses, number of elective courses required, etc.). Thus, I include a major plus entry–cohort fixed effect ρ_{mc} which absorbs non-observed variation common to students within each of these groups.

$$Y_i^{mc} = \rho_l IN_{imc}^l + \rho_N N_{imc}^s + \rho_s S_{imc} + \mathbf{X_i'}P + \rho_{mc} + \upsilon_{imc} \qquad (4)$$

Importantly, SPP changed the number of low–income peers by increasing the total number of students enrolled. To account for the change in the number of peers, I control for the total number of course peers across all courses the student took in their first term N_{imc}^s. This variable is computed like IN_{imc}^l, but without restricting the count by student socio–economic background (see Appendix B). Exposure to peers is also relative to the number of classes each student takes at each term. Hence, I include as a control the number of courses taking in the first term of enrollment by student i (S_{imc}). Thus, random variation comes only through the number of low–income peers across courses and not through the change in class or cohort size. This approach is similar to that in Angrist and Lang (2004) and rationalizes the endogenous variation through the number of low–income peers and conditional on the total number of classmates. I also control for student characteristics represented by the $\mathbf{X_i}$ and discussed for Equation 1. Lastly, υ_{imc} represented the error term which is clustered at the major and entry–cohort level. Since overall courses selection in the first term may be driven by non-random unobserved preferences for peers, particularly in the SPP outset, I instrument N_{imc}^s with the predicted number of course–level peers. Appendix B provides the details on the computation of the instrument.

Figure 11 describes the change in the distribution of the index among relatively wealthy students in the entry cohorts before and after SPP. The values of the index change dramatically from the pre– to the post–SPP cohorts. The values of the index are all below 100 for the 2014 cohorts, and 75 percent of students do not have more than 34 low–income peers in their first term courses. In 2015, 25 percent of students have at least 148 low–income peers in their first term courses, with some students having over 300 low–income peers. Notwithstanding, the variation in the index should be considered in light of the identifi-

cation strategy, which exploits the variation taking place within majors and entry cohorts.

Identification of the effect of exposure is challenged by students selecting in courses in ways associated with their preferences for low–income peers and achievement or socializing preferences, and not accounted by the research design. For example, wealthy students may change the first–term courses selected based on unobserved preferences for low–income peers. Additionally, SPP students in the 2015 entry cohort were more likely to enroll in their courses late in the spring semester, as their overall enrollment process in the University was delayed due to the tight timing of the program, and relative to the traditional timing of wealthy students.

Since individual course–level exposure can be associated with unobserved non–random variation in the assignment of students to courses with the outset of SPP, I instrument it with the predicted allocation of low–income students to first term courses based on historical information from 2012 and 2013 course–level enrollment. Ultimately, I will test the effects of first–term course–level exposure to low–income peers on wealthy students achievement and social interactions using an instrumental variables approach that allows me to isolate exogenous variation in courses' socio–economic composition, in a Two Stage Least Square Fixed–Effects setting.

The results are displayed in Tables 9 for academic achievement, and on Table 8 for the number of students' interactions. The results of the instrumental variables analysis indicate variation in exposure to low–income peers due to the composition of courses taken does not affect the academic achievement or the number of interactions with low–income students. Following Angrist and Pischke (2009), these tables also include the F–test of Excluded Instruments which is above 20 across all specifications, thus rejecting the null hypothesis of a weak instrument.

8   Conclusions

In 2014, the Colombian government launched *Ser Pilo Paga* a financial aid program targeting low–income high–achieving students that provided a forgivable loan covering tuition plus a stipend for living expenses, so long as the eligible students attended high-quality selective universities in the country. The program triggered a sharp influx in the number of low–income students enrolling at elite institutions in 2015. I study the case of one Elite University in Colombia where the enrollment of low–income students tripled with SPP, while maintaining the number of wealthy students enrolled constant. Importantly, the policy induced a significant academic achievement gap between low–income and relatively wealthy students. In this paper, I exploit the a quasi–random variation in

the percentage of low–income students at each major and entry cohort in a difference–in–differences design to answer: can exposure to desegregation at an elite college lead to more diverse social interactions without harming the achievement of students traditionally attending the institution?

I summarize the findings from this paper in three points. First, the increased exposure to low–income 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–income peers led to more interactions between wealthy and low–income students. The average increase in the percentage of low–income peers of 9.5 percentage points, led to significantly large positive impacts at both the probability (42 percent increase), and the number (120 percent increase) of links with low–income students. These effects are not susceptible to non–linearities and hold to measurement error and omitted variable bias robustness checks. Third, at least half of the increase in interactions with the low–income peers is explained by interactions with high–achieving low–income peers (that is, students with a performance equal 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 socio–economic desegregation of elite colleges can impact students within the institution. Similar to Angrist and Lang (2004) 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 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. This findings complements previous results from Carrell, Sacerdote and West (2013), who find groups with large achievements 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 significantly 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 understand how desegregation of higher education can foster social mobility for the low–income and underrepresented students.

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Tables

Table 1: Descriptive Statistics of Students' Characteristics

	2014 entry cohort				2015 entry cohort			
	Wealthy	Low–income			Wealthy	Low–income		
	Mean	Mean	Difference		Mean	Mean	Difference	
Peers composition								
Prop. Of middle–SES	0.49	0.00	-0.49	***	0.50	0.00	-0.50	***
No. Of links	5.21	4.94	-0.27		5.53	4.60	-0.93	**
No. of low SES links	0.24	0.35	0.11	*	0.59	1.73	1.14	***
Student characteristics								
Female	0.43	0.35	-0.09	**	0.46	0.41	-0.06	
Age	17.59	17.24	-0.35	***	17.59	17.14	-0.45	***
Mother with no college degree	0.08	0.24	0.16	***	0.09	0.40	0.32	***
SB11 standardized test score	0.00	-0.09	-0.10		0.04	-0.22	-0.26	***
SPP recipient	0.00	0.00	0.00		0.05	0.81	0.76	***
Other scholarship recipient	0.07	0.37	0.30	***	0.07	0.05	-0.02	
Immigrant according to H.S.	0.23	0.35	0.12	**	0.23	0.56	0.33	***
No. Of peers from H.S. in same cohort	11.54	3.17	-8.38	***	11.73	1.98	-9.75	***
Turnstiles characteristics								
Tot. ID swipes in turnstiles in 6th and 7th terms	1340.19	1349.79	9.60		1239.93	1162.33	-77.61	
Links' characteristics								
Age Difference	0.60	0.65	0.06		0.63	0.63	0.00	
Same Gender	0.50	0.51	0.00		0.51	0.45	-0.05	**
Ave. No. Of courses w/links	1.49	1.37	-0.13		1.51	1.31	-0.20	
SB11 difference	0.73	0.76	0.03		0.79	0.67	-0.12	**
Share of friends from same high school	0.04	0.01	-0.03	***	0.04	0.00	-0.04	***
No. Of majors	31	31			31	31		
No. Of students	2669	139			2609	538		
Individuals without links with their major-cohort	600	40			541	151		

Note: This table displays descriptive statistics of the sample of students described in Section 3. Wealthy students comprise middle– and high–SES students. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. P-values are based on t–test of the hypothesis that the difference in means between wealthy and low–income students is equal to zero. Standard errors where clustered at the major level.

Table 2: The Impact of Exposure to Desegregation on Academic Achievement and Persistence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1st term credits	1st term GPA	3rd term cum. Credits	3rd term cum. GPA	6th term cum. Credits	6th term cum. GPA	Dropout by 5th term	Graduated on time
A. OLS								
R^l_{mc}: *%tage of low–income peers*	0.011*	0.001	0.034**	0.001	0.031	0.001	0.002**	-0.000
	(0.006)	(0.001)	(0.017)	(0.001)	(0.031)	(0.001)	(0.001)	(0.001)
B. Non-linear Effects								
$\mathbb{I}$[% of low–income peers $> 30\%$]	0.063	-0.021	0.277	0.002	-0.065	-0.004	0.028	-0.034
	(0.174)	(0.033)	(0.571)	(0.026)	(0.815)	(0.022)	(0.020)	(0.029)
mu(Outcome)	15.64	3.863	49.39	3.822	100.9	3.859	0.122	0.0693
sd(Outcome)	2.949	0.449	8.496	0.378	16.22	0.344	0.327	0.254
Treatment distribution in 2015								
Δ mu(R^l_{mc})				9.51 p. p.				
sd(R^l_{mc})				12.98				
No. Students	5,278	5,274	4,895	4,895	4,507	4,507	5,278	5,278

Note: This table displays the estimates from Equation 1 in Panel A., and the non–linear estimates in Panel B. $\mathbb{I}$[percentage of low–income peers $> 30\%$] is an indicator function equal to one if the share of low–income peers in the major and cohort is greater than 30% i.e., at the top 25th percent of the distribution in the 2015 – Spring entry cohort. All estimations control for female indicators, age in year at the time of entry, SB11 standardized test scores, mothers' highest education is high school indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by imputing missing using the median value by major–cohort and by including an indicator equal to 1 if any of the covariates contains a missing value for the student i. Δ mu(R^l_{mc}) is the average increase in the percentage of low–income students from 2014 to 2015. All standard errors are clustered at the major-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Endogenous and exogenous effects of low–income peers on wealthy students' achievement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1st term credits	1st term GPA	3rd term cum. Credits	3rd term cum. GPA	6th term cum. Credits	6th term cum. GPA	Dropout by 5th term	Graduated on time
A. Exogenous SB11								
R^l_{mc}: % of low–income peers	0.014**	0.001	0.034**	0.001	0.031	0.000	0.002**	-0.000
	(0.005)	(0.001)	(0.017)	(0.001)	(0.032)	(0.001)	(0.001)	(0.001)
$S\tilde{B}11_{mc}$	1.262***	-0.006	0.204	-0.085	-0.058	-0.119**	-0.018	0.019
	(0.439)	(0.052)	(1.027)	(0.057)	(2.415)	(0.053)	(0.046)	(0.044)
B. Endogenous Achievement								
R^l_{mc}: % of low–income peers	0.013**	0.001	0.033*	0.001	0.030	0.001	0.002**	-0.000
	(0.006)	(0.001)	(0.017)	(0.001)	(0.032)	(0.001)	(0.001)	(0.001)
$\tilde{Y}_{mc}$	-0.015*	-0.003	0.004	0.005	0.003	0.006	0.032	0.021
	(0.009)	(0.005)	(0.007)	(0.005)	(0.008)	(0.004)	(0.045)	(0.028)
mu(Outcome)	15.64	3.866	49.43	3.825	100.9	3.862	0.121	0.0692
sd(Outcome)	2.939	0.448	8.469	0.377	16.19	0.344	0.326	0.254
No. Students	5,278	5,274	4,895	4,895	4,507	4,507	5,278	5,278

Note: This table displays the estimates from Endogenous Peer Effects (Eq. 3) and Exogenous SB11 Effects (Eq: 2). N_{Pmc} is the proportion of low–income students in the major and entry cohort as treatment. $S\tilde{B}11_{mc} = \frac{\sum_{j\neq i}^{N}[SB11_j|W_j=0]}{N^l_{mc}}$, and $\tilde{Y}_{mc} = \frac{\sum_{j\neq i}^{N}[Y_j|W_j=0]}{N^l_{mc}}$ where $W_j = 0$ if the peer j in student i major m and entry cohort c is low–income, and N^l_{mc} is the number of low–income students in student i major and entry cohort. All estimations control for female indicators, age in year at the time of entry, SB11 standardized test scores, mothers' highest education is high school indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by imputing missing using the median value by major–cohort and by including an indicator equal to 1 if any of the covariates contains a missing value for the student i. Δ mu(R^l_{mc}) is the average increase in the percentage of low–income students from 2014 to 2015. All standard errors are clustered at the major-cohort level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4: The Impact of Exposure to Desegregation on The Links of Wealthy Students

	A.Probability of a Link with:		B. Number of Links with:			C.% of Links with:
	(1)	(2)	(3)	(4)	(5)	(6)
	Wealthy	Low Income	Any	Wealthy	Low Income	Low Income
I. OLS						
R^l_{mc}: % of low–income peers	-0.002**	0.008***	-0.011	-0.043***	0.031***	0.714***
	(0.001)	(0.001)	(0.016)	(0.016)	(0.004)	(0.060)
At the mean increase (9.51 p.p.)	**-0.019**	**0.076**		**-0.409**	**0.295**	**6.790**
II. Non-linear Effects						
$\mathbb{I}$[% of low–income peers > 30%]	-0.043	0.117***	0.017	-0.667	0.684***	14.566***
	(0.028)	(0.038)	(0.495)	(0.507)	(0.131)	(2.915)
Pre-treatment statistics						
mu(No. of friends)	0.770	0.188	5.212	4.973	0.239	4.404
sd(No. of friends)	0.421	0.391	5.154	4.922	0.558	11.34
No. Students	5,278	5,278	5,278	5,278	5,278	4,137

Note: This table displays the estimates from Equation 1 in Panel I., and the non–linear estimates in Panel II. Outcomes are based on a turnstile–elicited links based on time–windows of 3 seconds and at least two co–movements in a term. *% of low–income peers* is calculated at the major–cohort levels. $\mathbb{I}[\%$ of low–income peers $> 30\%]$ is an indicator function equal to one if the share of low–income peers in the major and cohort is greater than 30% i.e., at the top 25th percentile of the distribution in the 2015 – Spring entry cohort. All estimations control for female indicators, age in year at the time of entry, SB11 standardized test score, mothers' has no college education indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by imputing missing values using the median value by major–cohort and by including an indicator equal to 1 if any of the covariates contains a missing value for the student *i*. 9.51 is the average increase in the percentage of low–income students from 2014 to 2015. All standard errors are clustered at the major-cohort level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 5: The Impact of Exposure on Links with the High–Performing Low–Income Students

	A. Number of Links with High Performers By:			B. Probability of a Link with High Performers By:		
	(1) SB11	(2) 1st Term GPA	(3) 1st Term Credits	(4) SB11	(5) 1st Term GPA	(6) 1st Term Credits
OLS						
R^l_{mc}: % of low–income peers	0.013***	0.016***	0.021***	0.006***	0.006***	0.008***
	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)
Pre-treatment statistics of the outcomes						
Mean	0.0892	0.135	0.118	0.0798	0.111	0.102
Standard Deviation	0.317	0.419	0.375	0.271	0.315	0.303
Δ mu(R^l_{mc})			9.51 p. p.			
No. Students	5,278	5,278	5,278	5,278	5,278	5,278

Note: This table displays the estimates from Equation 1. Low–Income students are labeled as high achievers, when their performance is above the average of that of the wealthy students in their major and entry cohort. *% of low–income peers* is calculated at the major–cohort levels. All estimations control for female indicators, age in year at the time of entry, SB11 standardized test score, mothers' has no college education indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by imputing missing values using the median value by major–cohort and by including an indicator equal to 1 if any of the covariates contains a missing value for the student *i*. Δ mu(R^l_{mc}) is the average increase in the percentage of low–income students from 2014 to 2015. All standard errors are clustered at the major-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of Exposure to Low–Income Students on Social Interactions Between High–Achievers Measured by Pre–College Test Scores

	High Achievers according to SB11 scores	
	(1)	(2)
	A. Probability of a Link	B. Number of Links
$R^l_{mc} * HighAchiever_i$	-0.000	-0.001
	(0.001)	(0.002)
R^l_{mc}	0.007***	0.015***
	(0.002)	(0.003)
$HighAchiever_i$	0.006	0.002
	(0.019)	(0.023)
No. Students	5,278	5,278

Note: This table displays the estimates from

$$L^{w=0,HighAchiv}_{imc} = \alpha_d(R^l_{mc} * HighAchiever_i) + \alpha_l R^l_{mc} + \alpha_p HighAchiever_i + \mathbf{X'_i}A + \alpha_m + \alpha_c + \varepsilon_{imc}$$

Where $HighAchiever_i = 1$ if student i achievement is equal or above the average of their wealthy peers and equals zero otherwise. Achievement is measured using SB11 standardized test scores. $L^{w=0,HighAchiv}_{imc}$ represents the number of interactions with low–income students $w = 0$ whose achievement is equal or above the average of the wealthy peers in the same major and entry cohort mc. In Panel A, this outcome is replaced by a indicator variable equal to one when $L^{w=0,HighAchiv}_{imc} > 0$. All estimations control for a female indicator, age in year at the time of entry, SB11 standardized test score, mothers' has no college education indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by imputing missing values using the median value by major–cohort and by including an indicator equal to 1 if any of the covariates contains a missing value for the student i. The average increase in the percentage of low–income students R^l_{mc} from 2014 to 2015 is 9.5 p.p. All standard errors are clustered at the major-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Exposure to Low–Income Students and Social Interactions Between High–Achievers Measured by First–Term Performance

	A. Probability of a Link with a Low-Income High Achievers By:		B. Number of Links with Low-Income High Achievers By:	
	(1)	(2)	(3)	(4)
	First Term GPA	First Term Credits Attempted	First Term GPA	First Term Credits Attempted
$R^l_{mc} * HighAchiever_i$	0.005***	0.002**	0.014***	0.007***
	(0.002)	(0.001)	(0.004)	(0.003)
R^l_{mc}	0.004***	0.008***	0.011***	0.020***
	(0.001)	(0.001)	(0.003)	(0.003)
$HighAchiever_i$	0.024	0.044***	0.009	0.030
	(0.015)	(0.014)	(0.028)	(0.020)
No. Students	5,278	5,278	5,278	5,278

Note: This table displays the estimates from

$$L^{w=0,HighAchiv}_{imc} = \alpha_d(R^l_{mc} * HighAchiever_i) + \alpha_l R^l_{mc} + \alpha_p HighAchiever_i + \mathbf{X'_i}A + \alpha_m + \alpha_c + \varepsilon_{imc}$$

Where $HighAchiever_i = 1$ if student i achievement is equal or above the average of their wealthy peers and equals zero otherwise. Achievement is measured by the first term. $L^{w=0,HighAchiv}_{imc}$ represents the number of interactions with low–income students $w = 0$ whose achievement is equal or above the average of the wealthy peers in the same major and entry cohort mc. In Panel A, this outcome is replaced by a indicator variable equal to one when $L^{w=0,HighAchiv}_{imc} > 0$. All estimations control for a female indicator, age in year at the time of entry, SB11 standardized test score, mothers' has no college education indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by imputing missing values using the median value by major–cohort and by including an indicator equal to 1 if any of the covariates contains a missing value for the student i. The average increase in the percentage of low–income students R^l_{mc} from 2014 to 2015 is 9.5 p.p. All standard errors are clustered at the major-cohort level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 8: Effect of Courses–level Exposure to low–income Peers on Wealthy Students' Friendships

	2 - seconds window			3 - seconds window			5 - seconds window		
	(1) Any	(2) Wealthy	(3) Low SES	(4) Any	(5) Wealthy	(6) Low SES	(7) Any	(8) Wealthy	(9) Low SES
2SLS									
IN_{imc}	0.001	-0.002	0.003	0.007	-0.001	0.008	0.014	0.003	0.011
	(0.018)	(0.015)	(0.006)	(0.024)	(0.019)	(0.008)	(0.025)	(0.020)	(0.008)
First Stage									
Predicted IN_{imc}	0.460***			0.460***			0.460***		
	(0.102)			(0.102)			(0.102)		
F-test excluded instruments	20.28	20.28	20.28	20.28	20.28	20.28	20.28	20.28	20.28
Reduced Form									
Predicted IN_{imc}	0.001	-0.001	0.002	0.003	-0.000	0.004	0.006	0.001	0.005
	(0.009)	(0.007)	(0.003)	(0.011)	(0.009)	(0.004)	(0.012)	(0.009)	(0.004)
OLS									
IN_{imc}	-0.000	-0.002	0.002	-0.000	-0.003	0.003**	0.001	-0.003	0.003*
	(0.003)	(0.002)	(0.001)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.002)
No. Students	5,278	5,278	5,278	5,278	5,278	5,278	5,278	5,278	5,278

Note: This table displays the estimates from Equation 4. All estimates include fixed effects by major–cohort. IN_{imc} is a measure of individual–level exposure to low–income peers in the first term of college based on the courses students take in that term. The computation of IN_{imc} and the predicted IN_{imc} used as instrument is described in Appendix B. All standard errors are clustered at the major-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effect of Courses-Level Exposure to low–income Peers on Wealthy Students' Achievement

	(1)	(2)	(3)	(4)	(5)	(6)
	6th term cum. Credits	6th term cum. GPA	7th term cum. Credits	7th term cum. GPA	Graduated in 8 term	Graduated in 9 terms
2SLS						
IN_{imc}	0.023	-0.001	-0.003	-0.001	-0.002	0.001
	(0.071)	(0.001)	(0.079)	(0.001)	(0.002)	(0.002)
First Stage						
Predicted IN_{imc}	0.511***	0.511***	0.533***	0.533***	0.460***	0.574***
	(0.110)	(0.110)	(0.112)	(0.112)	(0.102)	(0.102)
F-test excluded instruments	21.74	21.74	22.52	22.52	20.28	31.92
Reduced Form						
Predicted IN_{imc}	0.012	-0.001	-0.002	-0.001	-0.001	0.000
	(0.036)	(0.001)	(0.043)	(0.001)	(0.001)	(0.001)
OLS						
IN_{imc}	-0.033***	-0.001**	-0.043***	-0.001***	-0.000	-0.000
	(0.012)	(0.000)	(0.013)	(0.000)	(0.000)	(0.000)
No. Students	4,507	4,507	4,447	4,447	5,278	4,027

Note: This table displays the estimates from Equation 4. All estimates include fixed effects by major–cohort. IN_{imc} is a measure of individual–level exposure to low–income peers in the first term of college based on the courses students take in that term. The computation of IN_{imc} and the predicted IN_{imc} used as instrument is described in Appendix B. All standard errors are clustered at the major-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Identification Challenges in the Difference–in–Difference Research Design

Type of Bias	Source	Assessment	Conclusion
Non–parallel trends bias	**Selection of majors and entry cohorts changes with SPP**	Event study on outcomes and covariates (see Figures 7, 8, and 6)	No evidence of bias.
	Wealthy students are crowed out in the admission process by the low-income students recipients of SPP.	Total number of seats to high-income students did not change (see Figure 2)	There is no correlation between the increased number of low–income students per program and cohort and the change in the number of wealthy students
Unobserved exposure effects	**Exposure to low–SES operates through courses, and many courses can be taken by students from multiple majors.**	Index capturing exposure to low–SES peers at the course level. I embed the index in a within major-cohort specification and use IV to ensure exogeneity (see Appendix B).	Course-level exposure does not distinctly affect outcomes for students within the same major and cohort
Measurement Error on the social interactions outcomes	**Turnstile-elicited interactions are the result of random co–movements**	Use data from a survey on social interactions among students from one major and entry cohort (see Appendix A - Tables 11 and 12).	The rate of false positives (i.e., turnstile–elicited interactions not captured by the survey) is below 10 percent
	If false-negatives are non-random, there is bias in the turnstile-elicited links	I use simulations to estimate how far from random are the turnstile-elicited links' characteristics, and how close the average characteristics of the turnstile–elicited links are to those of the survey-elicited links (see Appendix A - Figure 9).	Turnstile–elicited interactions capture links characteristics, albeit the number of interactions missing
	Rate of false positives and negatives across majors and cohorts could be impacted by exposure to low–income peers	Use proxies of measurement error to test for the effects of exposure using the Dif–in–Dif design (See Appendix A - Table 13).	I do not find evidence of an effect of exposure on measurement error rates.

Note: This table describes the identification challenges of the Difference–in–Difference research design discussed in Equation 1.
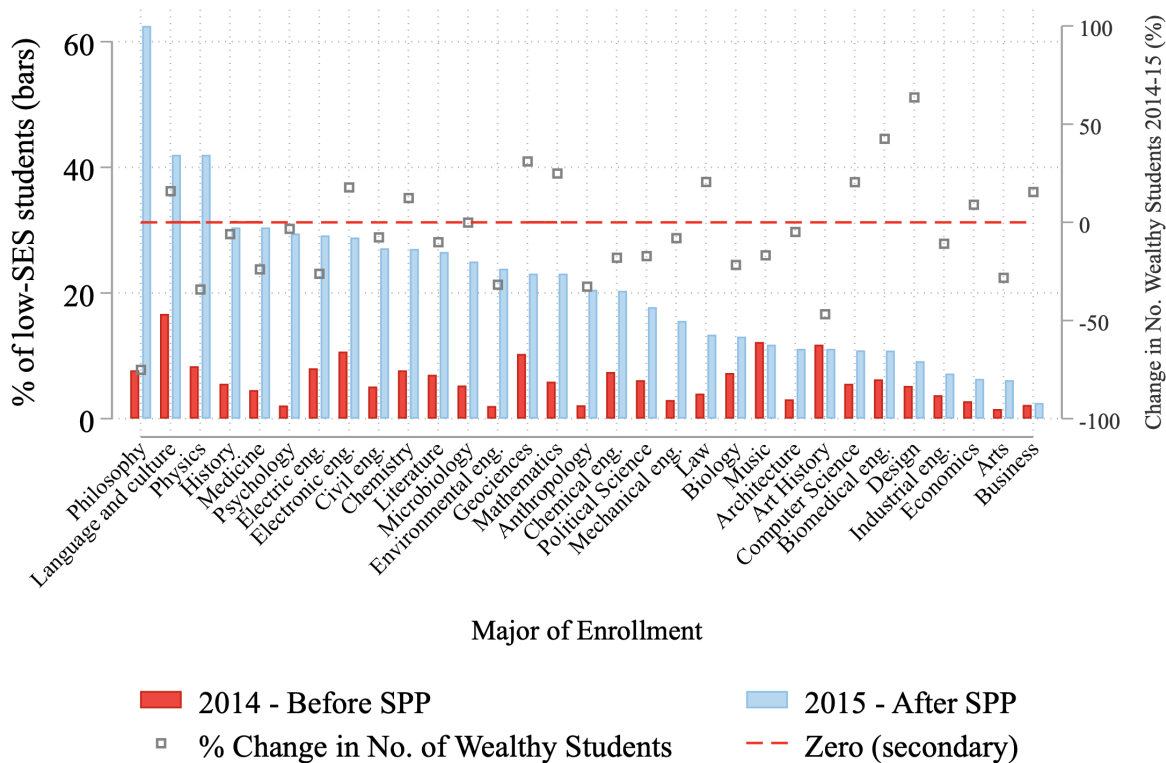
Figures

Figure 1: Number of first term students by socio–economic status (SES)
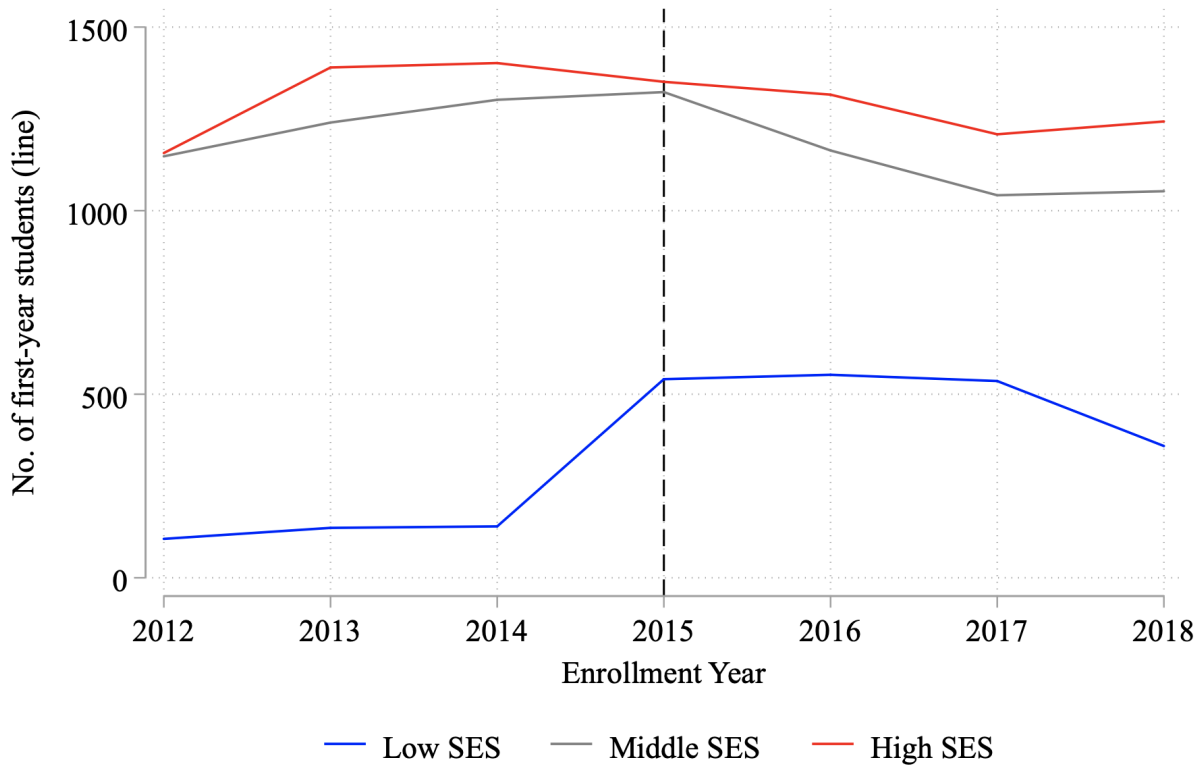

Note: This figure displays the total number of first–term students by so-cio–economic (SES) background. Students are classified in three SES groups ac-cording to their house strata indicator. Wealthy students are those from socio–economic strata three to six, whereas low–income students are those from socio–economic strata one and two. I add both spring and fall enrollment per year. The dotted vertical line marks the start of SPP.

Figure 2: Percentage of low–SES students by major and before and after SPP

Note: This figure displays the percentage of low–income students per major and entry cohort year. The secondary axis displays the percentage change in the number of wealthy students enrolled in the 2015 entry year relative to the 2014 year. Figures are calculated per year, by adding the total number of new students enrolled in each major and entry term (i.e., fall and spring).

Figure 3: Composition of Courses Taken by First–Term Students at Each Entry Cohort
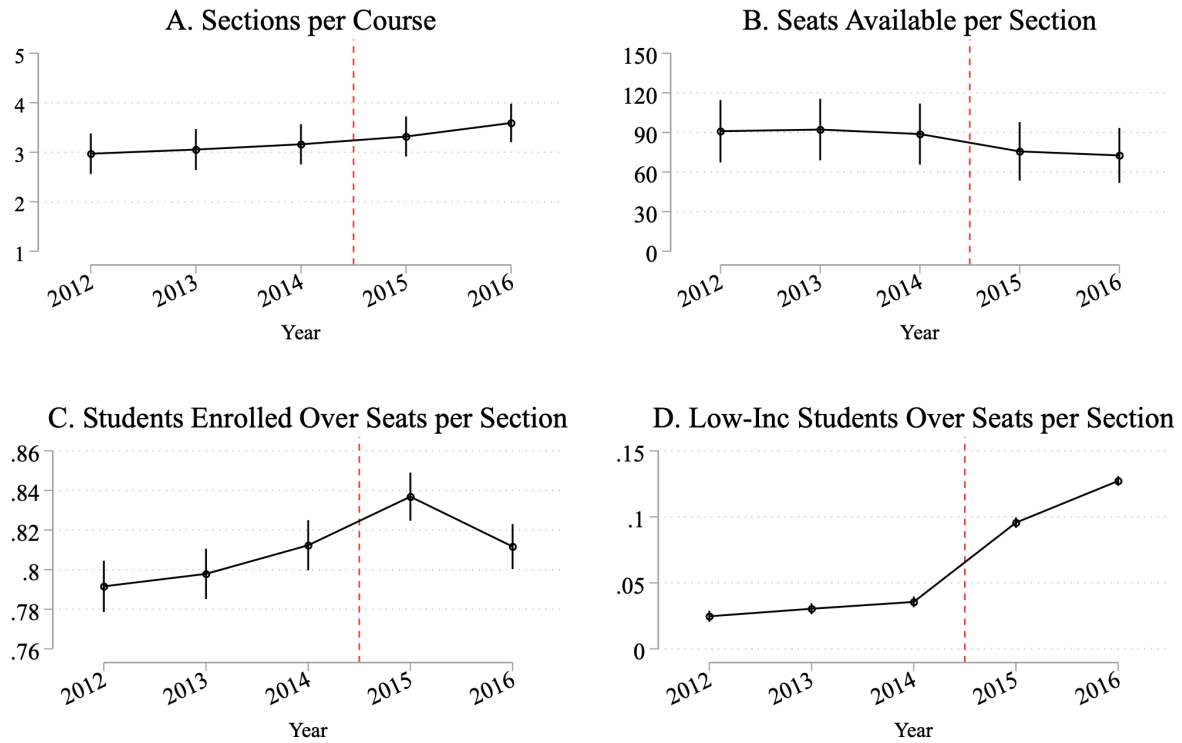

Note: This figure describes the courses and classrooms attended by all first–term students at each entry cohort. Panel A describes the average number of sections per course, Panel B the average number of spots or seats available per section, Panel C the ratio of students enrolled over the total number of seats available, and panel D the ratio of low–income students enrolled over the total number of seats available. In each panel, each point plots results from an OLS regression with no constant and dummies by entry year. 95% confidence intervals are plotted as a vertical line on each dot. The data was previously aggregated at the section–course–term level. The dotted red line separates the cohorts enrolling before the start of SPP (2014 en before) from the cohorts enrolling during SPP (2015 onward).

Figure 4: Average Cumulative GPA and Credits Attempted by Entry Cohort and Income group
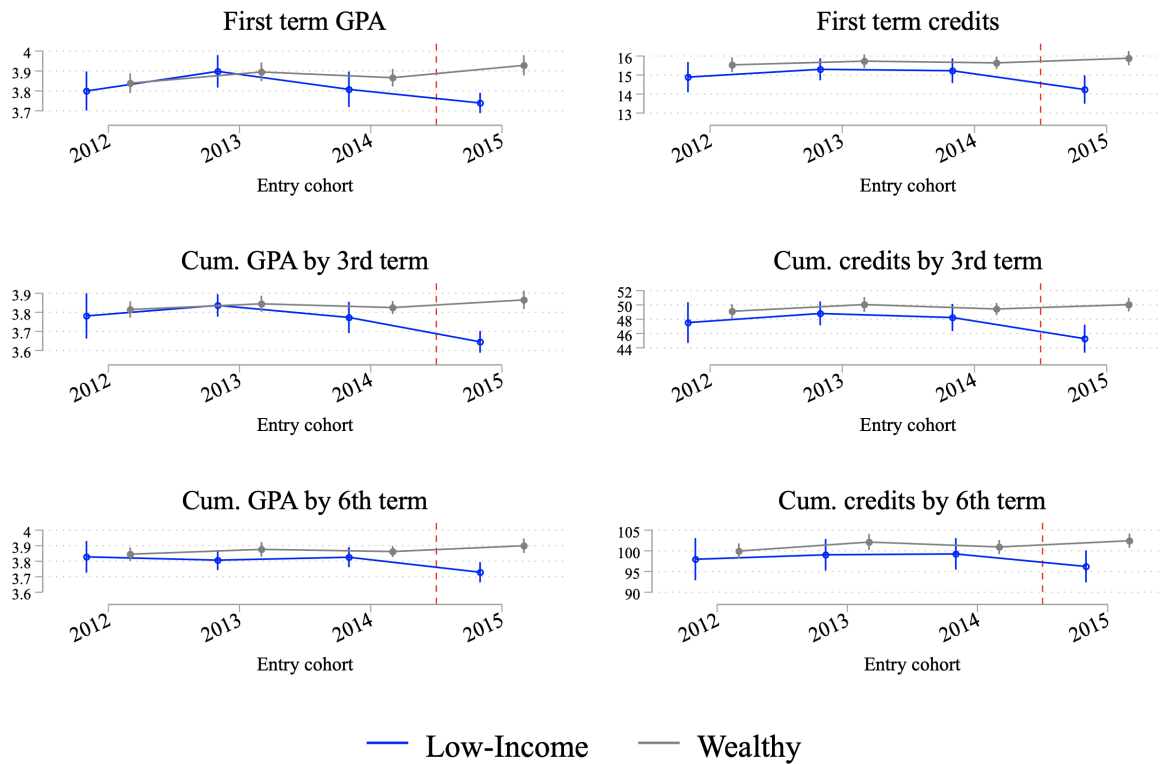

Note: These graphs display the point estimates of a cohort dummy from an OLS regression with no intercept where the dependent variable is the outcome of the student. GPAs range from one to five with five as the highest grade. 95% confidence intervals are plotted as a vertical line on each dot and based on clustered standard errors at the major-cohort level. Each year entry cohort includes the entry cohorts of Spring and Fall of the respective calendar year. Wealthy students are those from socio–economic strata three to six, whereas low–income students are those from socio–economic strata one and two. The dotted red line separates the cohorts enrolling before the start of SPP (2014 en before) from the cohorts enrolling during SPP (2015 onward).

Figure 5: Academic Persistence by Entry Cohort and Income group
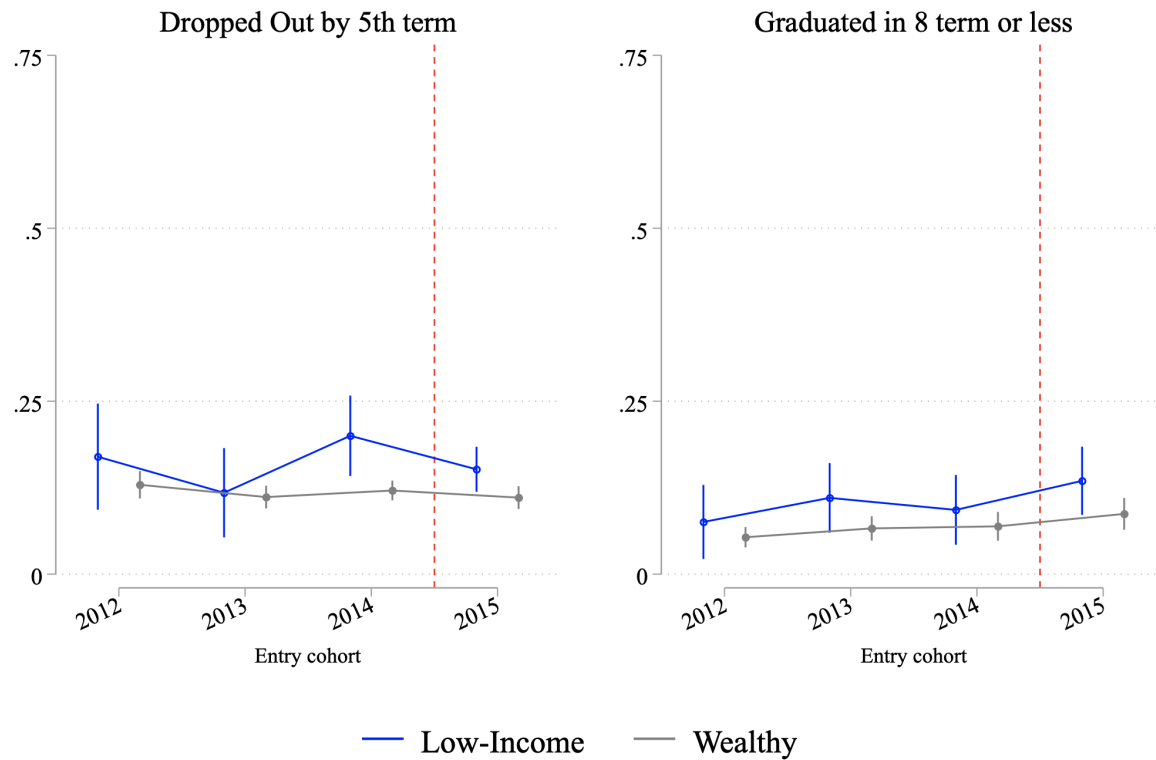
Note: These graphs display the point estimates of a cohort dummy from an OLS regression with no intercept where the dependent variable is the outcome of the student. 95% confidence intervals are plotted as a vertical line on each dot and based on clustered standard errors at the major-cohort level. Each year entry cohort includes the entry cohorts of Spring and Fall of the respective calendar year. Wealthy students are those from socio–economic strata three to six, whereas low–income students are those from socio–economic strata one and two. The dotted red line separates the cohorts enrolling before the start of SPP (2014 en before) from the cohorts enrolling during SPP (2015 onward).

Figure 6: Variation in Students' Characteristics within Cohorts and Across majors
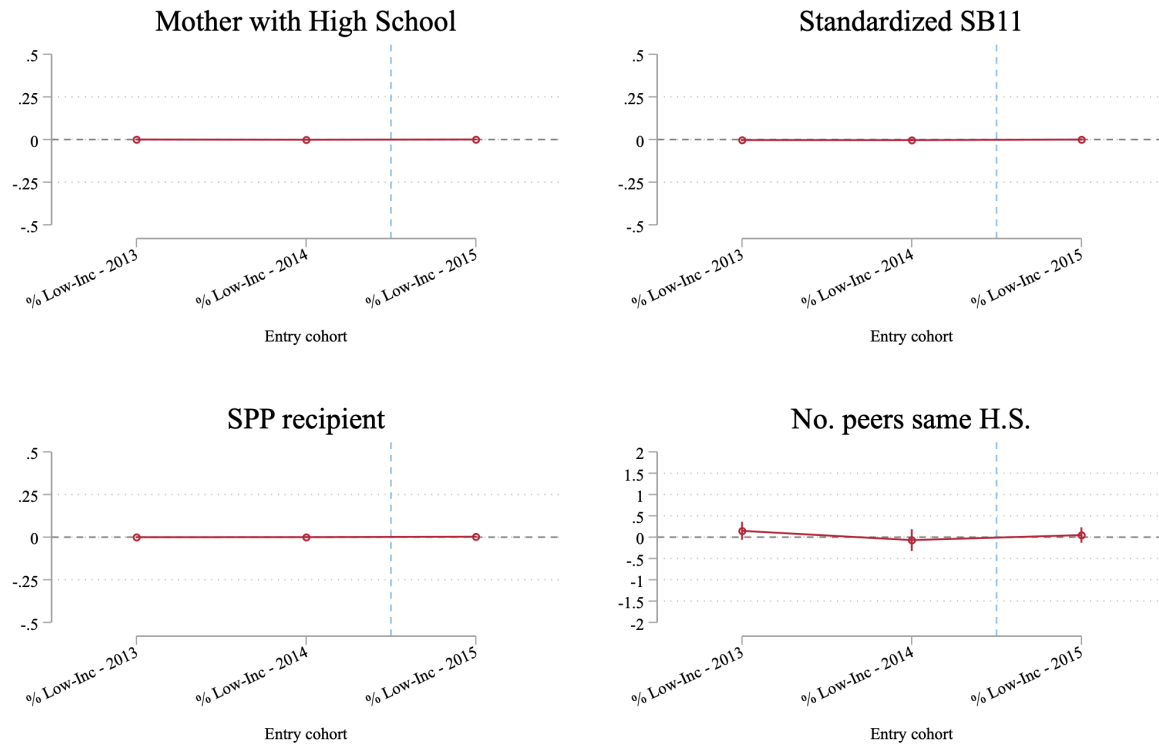
Note: Point estimates of a cohort dummy interacted with the percentage of low–income students within a major and cohort from an OLS regression where the dependent variable is the characteristic of the students controlling for major of enrollment dummies. Each year entry cohort includes the entry cohorts of Spring and Fall of the respective calendar year. The dotted red line separates the cohorts enrolling before the start of SPP (2014 en before) from the cohorts enrolling during SPP (2015 onward). Cluster standard errors at the major-cohort level. 95% confidence intervals.

Figure 7: Variation in Students' Achievement within Cohorts and Across Majors
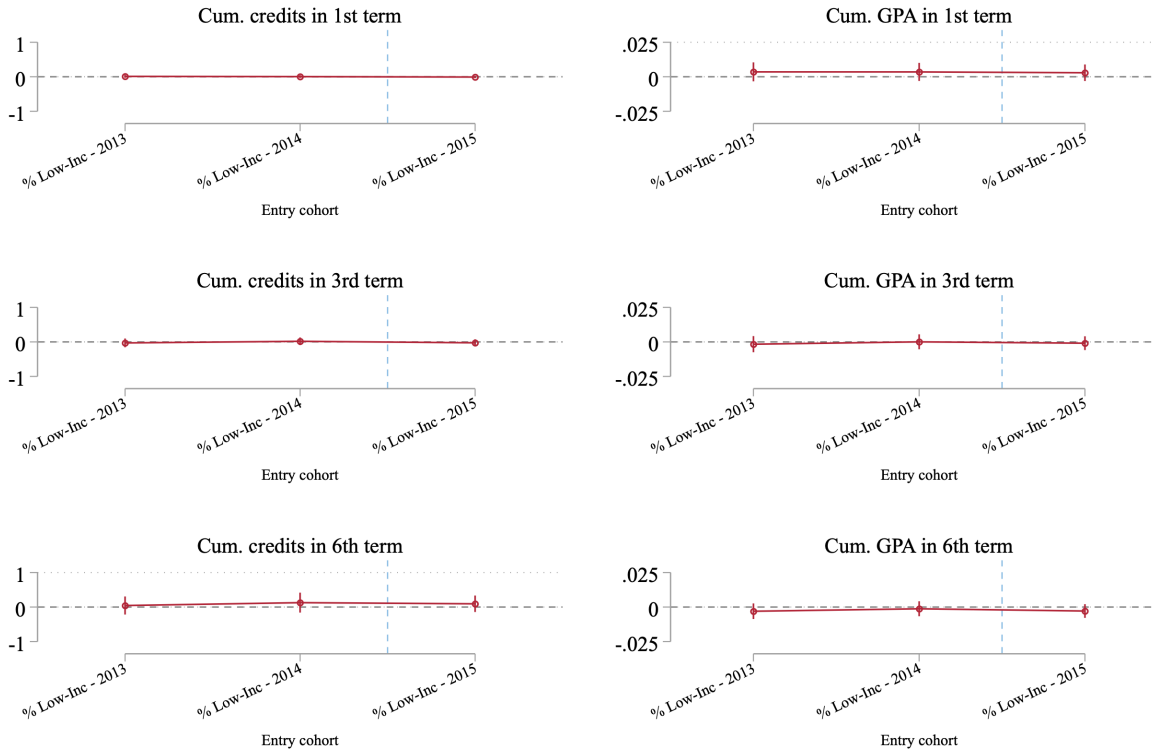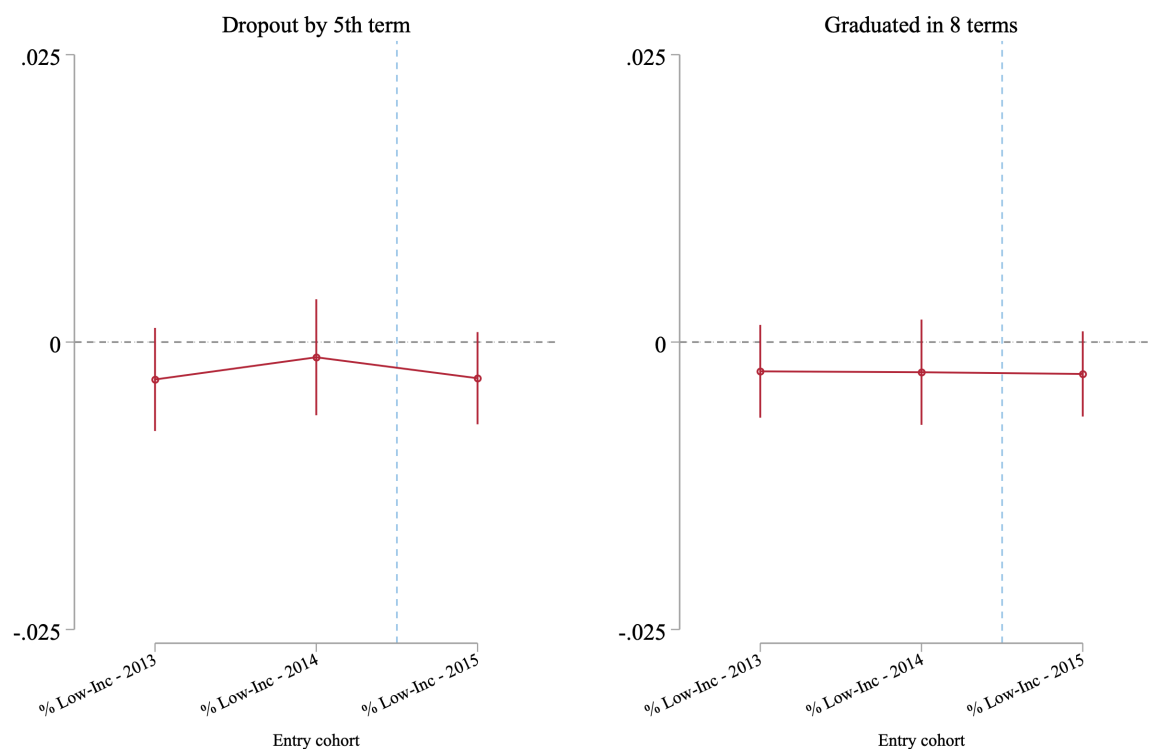
Note: Point estimates of a cohort dummy interacted with the percentage of low–income students within a major and cohort from an OLS regression where the dependent variable is the outcome of the student controlling for major of enrollment dummies. Estimations include the controls listed in Equation 1. Each year entry cohort includes the entry cohorts of Spring and Fall of the respective calendar year. The dotted red line separates the cohorts enrolling before the start of SPP (2014 en before) from the cohorts enrolling during SPP (2015 onward). Cluster standard errors at the major-cohort level. 95% confidence intervals.

Figure 8: Variation in Students' Persistence within Cohorts and Across Majors

Dropout by 5th term

Graduated in 8 terms

Note: Point estimates of a cohort dummy interacted with the percentage of low–income students within a major and cohort from an OLS regression where the dependent variable is the outcome of the students controlling for major of enrollment dummies. Estimations include the controls listed in Equation 1. Each year entry cohort includes the entry cohorts of Spring and Fall of the respective calendar year. The dotted red line separates the cohorts enrolling before the start of SPP (2014 en before) from the cohorts enrolling during SPP (2015 onward). Cluster standard errors at the major-cohort level. 95% confidence intervals.

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. (2019) provide a detail description of the survey.[17]. The survey inquired about two types of links: friendships and acquaintances. Table 11 shows the results of the comparison. The time windows tested in Table 11 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 11, 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 crite-

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

ria. I chose to use 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 11 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 9. 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 11 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 11 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 12. 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 11. 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 11: 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															
No. Of dyads	868	368	235	180	148	1209	509	314	251	198	1906	898	552	401	315
No. of students	110	110	108	107	105	110	110	109	108	107	110	110	109	108	108
2. Are friends															
Dyads			*505*					*505*					*505*		
Survey & Turnstiles															
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	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**
3. Acquaintances															
Dyads			*1033*					*1033*					*1033*		
Survey & Turnstiles															
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	*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 12: 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. Acquaintances												
Dyads			1033						1033			
Survey & Turnstiles												
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	*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 9: Comparison against randomly generated distribution
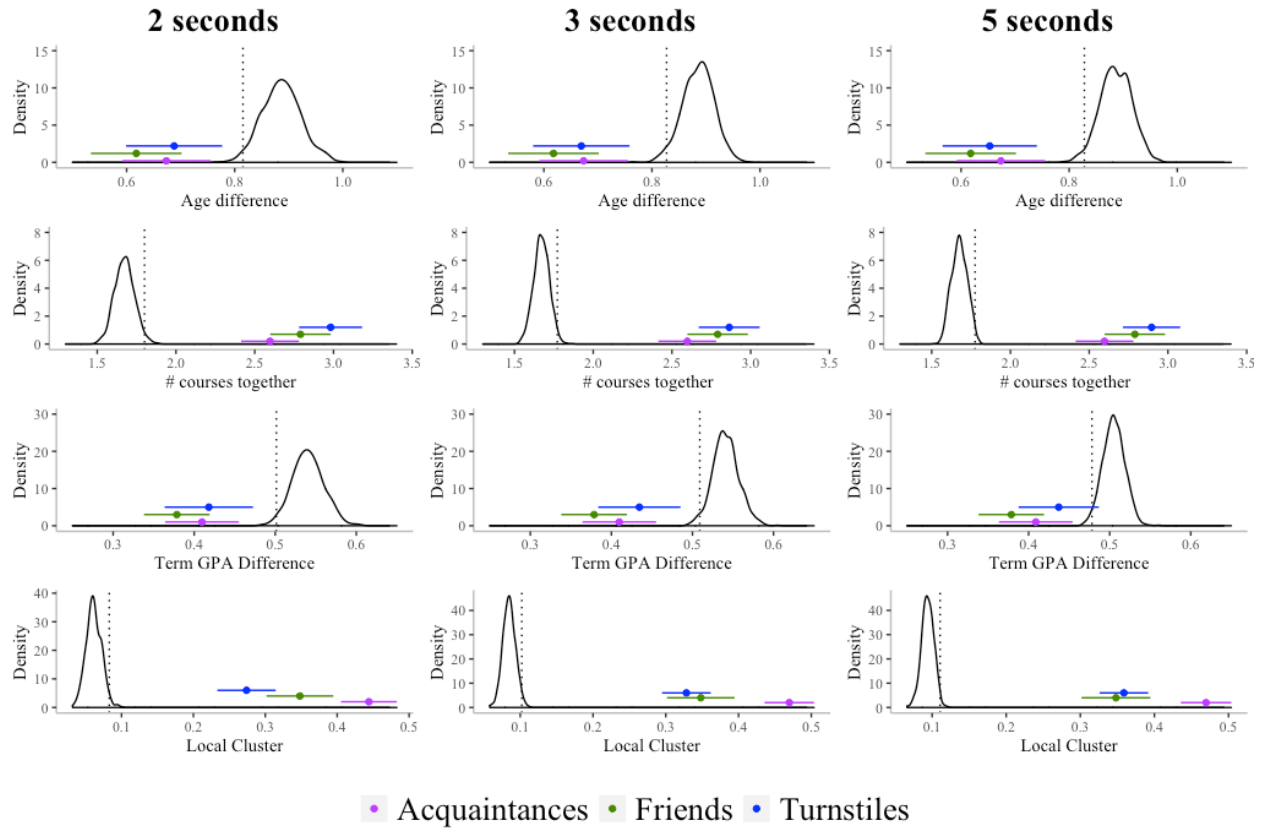
Note: Turnstile-elicited links matched with the survey are randomly assigned in 1000 draws among 110 students forming all possible 5595 dyads. Confidence intervals of 95% confidence are presented. Matches for 2 seconds - 2 times window: 368 links. Matches for 3 seconds - 2 times window: 368 links. Matches 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_{mc}^l), and u as an untreated group:

$$\hat{\alpha_P}^{2x2} = (E[L_t|Post] - E[L_t|Pre]) - (E[L_u|Post] - E[L_u|Pre]) \tag{5}$$

In Equation 5, the estimated $\hat{\alpha_P}^{2x2}$ 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 5 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:

$$\hat{\alpha_P}^{2x2} = \underbrace{E[L_t^1|Post] - E[L_t^0|Post]}_{\text{ATT}}$$
$$+ \underbrace{(E[L_t^0|Post] - E[L_t^0|Pre])}_{\text{Treatment counterfactual}} \underbrace{+ (E[L_u^0|Post] - E[L_u^0|Pre])}_{\text{non-parallel trend bias==0}} \tag{6}$$

Equation 6 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:

$$ATT^{estimated} = E[L_t^{1,True}|Post] - E[L_t^{0,True}|Post] \qquad (7)$$

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:

$$
\begin{aligned}
ATT^{estimated} &= E[L_t^{1,obs} - L_t^{1,F(+)} + L_t^{1,F(-)}|Post] - E[L_t^{0,obs} - L_t^{0,F(+)} + L_t^{0,F(-)}|Post] \\
&= \underbrace{E[L_k^{1,obs}|Post] - E[L_k^{0,obs}|Post]}_{\text{Observed ATT}} + \\
&\quad \underbrace{E[L_t^{1,F(-)} - L_t^{1,F(+)}|Post] - E[L_t^{0,F(-)} - L_t^{0,F(+)}|Post]}_{\text{Measurement Error Bias}}
\end{aligned} \qquad (8)
$$

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 $L^{F(+)}$:

$$ATT^{estimated} = ATT^{obs} + \underbrace{E[L_t^{1,F(-)} - L_t^{0,F(-)}|Post]}_{\text{ATT on F(-)}} - \underbrace{E[L_t^{1,F(+)} - L_t^{0,F(+)}|Post]}_{\text{ATT on F(+)}} \qquad (9)$$

Equation 9 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_{mc}^l 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_{mc}^l 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 interac-

tions 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 13 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 13: ATT on Measurement Error Proxies

| | ID swipes | No. of courses with peers interacted | | |
| | | 2 seconds | 3 seconds | 5 seconds |
	(1)	(2)	(3)	(4)
Percentage of low–income peers	-4.162	0.003	-0.001	0.000
	(2.621)	(0.007)	(0.007)	(0.007)
Mu(Outcome)	1340	2.006	2.059	2.084
SD(Outcome)	1017	2.466	2.515	2.538

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 10: Flow of students at selected entrances - term and hour according to turnstiles
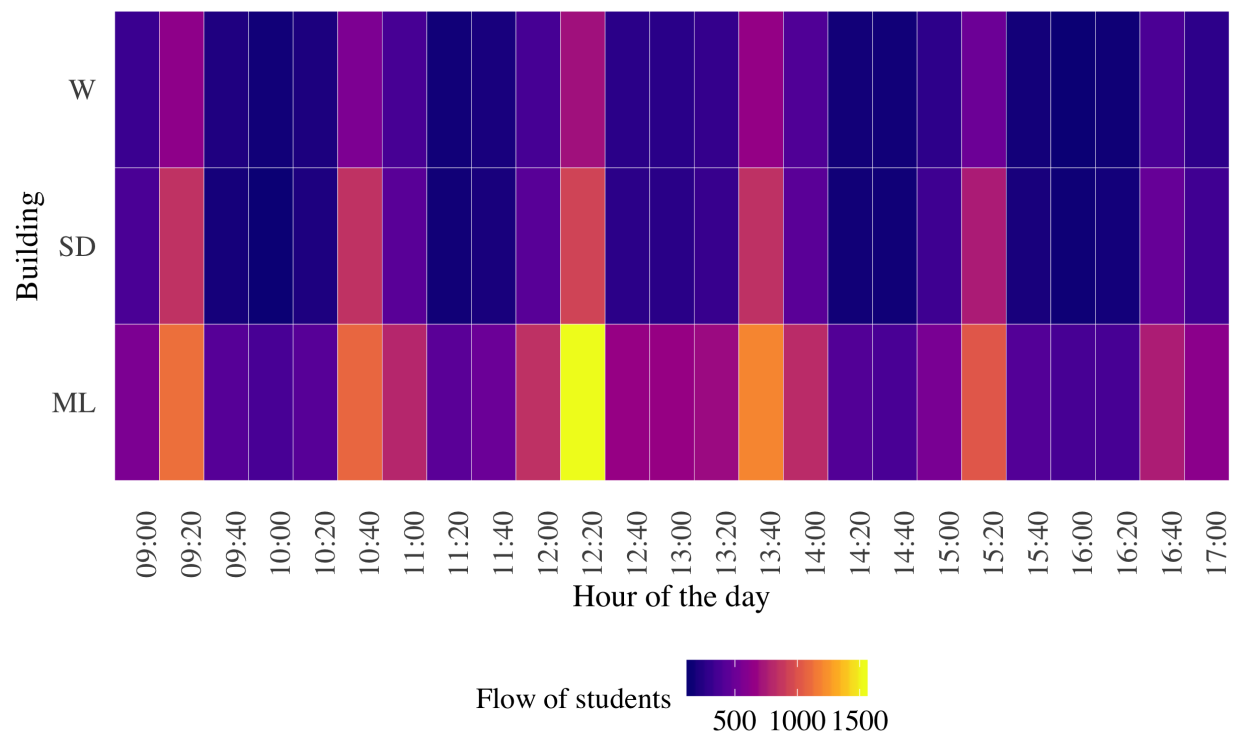
Note: Average number of swipes per day, entrance and 20 minutes blocks. Swipes include INs and OUT of the building. Only weekdays during the official academic calendar are included in the data.

B   Exposure to low–income in first–term courses

Computation of a Predicted Course–Level Exposure to low–income Peers: I use University data on course–level enrollment from 2012 and 2013 to estimate a distribution of low–income students across courses and to predict the number of low–income students in each course of the 2014 and 2015 cohort, had the distribution of low–income students not changed due to the outset of SPP. I computed the predicted distribution of low–income students as follows:

1. I keep the enrollment information for all courses offered between 2012 and 2013, one year before the start of SPP in 2015.

2. I keep the information only for students in their first–term of enrollment. I do this because I care about the allocation of incoming students across courses, and not about the allocation of other continuing students.

3. For each course –including all its sections and terms offered, I count the number of first–term low–income students enrolled.

4. I calculate the percentage distribution of first–term low–income students across all courses. I do this by dividing the number of low–income students in each course over the sum of low–income students across all courses. Doing so ensures that the sum of the percentage distribution of low–income students across courses adds to one.

5. Using the course–level data on first–term courses of the 2014 and 2015 entry cohorts, I match the distribution of low–income students per course computed in step 4. using the course ID information.

6. I predict the allocation of first–term students on those courses by multiplying the number of low–income students in each entry cohort with the estimated percentage distribution of the course.

7. For each student, I add the predicted allocation of low–income peers in their first term courses. This gives the predicted index of exposure to low–income peers in the first term courses, had the allocation of low–income students not changed with SPP.

Figure 11: Course-Level Exposure Index to low–income Peers in the First Term of Enroll-ment

excludes outside values
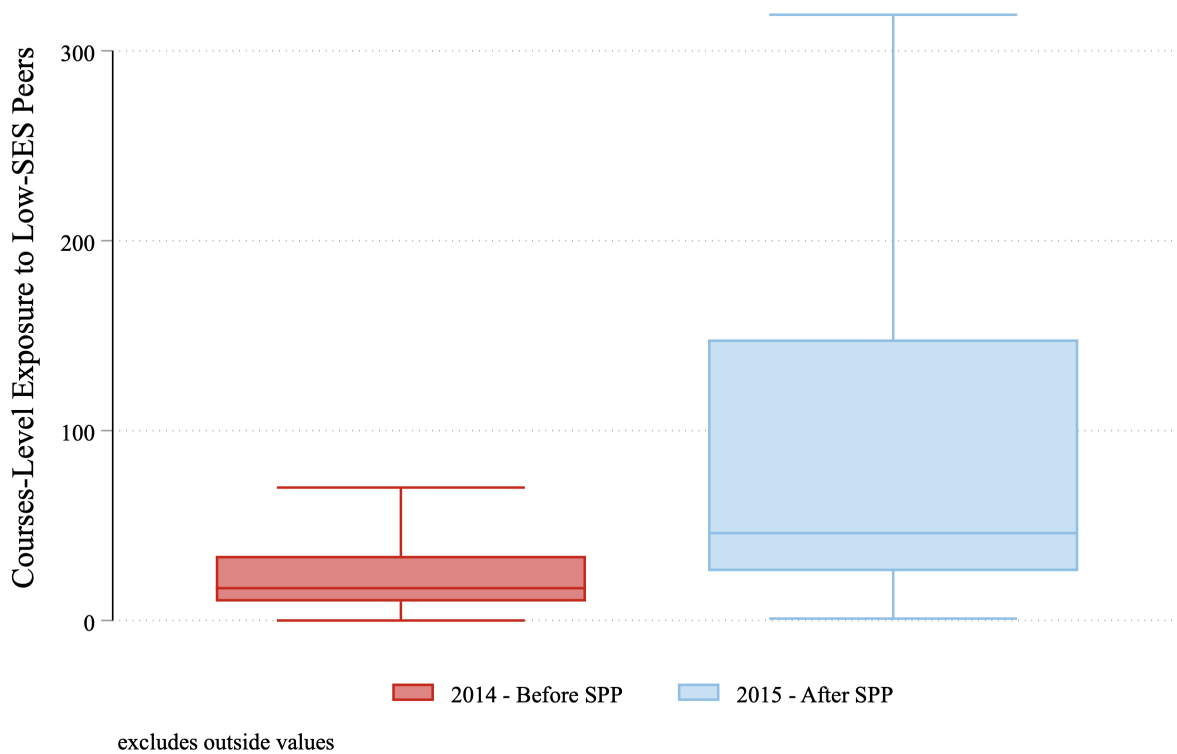
Note: This figure plots the distribution of the course–level exposure to low–income peers in the first term index. The plot follows the standard display of 75th per-centile, median and 25th percentile references.

C   Appendix: Additional Robustness Tests and Estimations

Table 14: Correlation Between the Numbers of Wealthy and Low–Income Students in a Major and Entry Cohort

	(1)	(2)	(3)	(4)
No. of low-income peers in major–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 major–cohort groups	124	124	124	124

Note: This table displays OLS estimates correlating the number of wealthy and low–income students in a major and entry cohort. The number of wealthy students is the dependent variable and the number of low–income is the explanatory variable. Each observation in the data corresponds to one major 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' highest education level is high school, SB11 standardized test scores, share of students who are middle–SES, and share of students who are SPP. Missing values for SB11 are imputed and a control capturing the share of students with missing values in SB11 is included as well. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: The Impact of Exposure to Desegregation on the Links of Wealthy Students - 2 seconds windows

	A. Number of Links			B. Probability of a Link with		C. Proportion of Links
	(1)	(2)	(3)	(3)	(4)	(5)
L_imc with:	Any	Wealthy	Low Income	Wealthy	Low Income	Low Income
OLS						
% of low–income peers	-0.013	-0.036***	0.023***	-0.003**	0.007***	0.707***
	(0.013)	(0.013)	(0.003)	(0.001)	(0.001)	(0.070)
B. Non-linear Effects						
$\mathbb{I}$[% of low–income peers > 30%]	-0.097	-0.568	0.470***	-0.042	0.111***	15.659***
	(0.412)	(0.440)	(0.100)	(0.037)	(0.032)	(3.045)
Pre-treatment statistics of the Outcomes						
mean	4.085	3.892	0.193	0.736	0.157	4.330
standard deviation	4.213	4.019	0.494	0.441	0.364	11.51
Treatment distribution in 2015						
Δ mu(R^l_{mc})			9.51 p. p.			
sd(R^l_{mc})			12.98			
No. Students	5,278	5,278	5,278	5,278	5,278	3,979

Note: This table displays the estimates from Equation 1 in Panel A., and the non–linear estimates in Panel B. Outcomes are based on a turnstile–elicited links based on time–windows of 2 seconds and at least two co–movements in a term. *% of low–income peers* is calculated at the major–cohort levels. $\mathbb{I}$[% of low–income peers > 30%] is an indicator function equal to one if the share of low–income peers in the major and cohort is greater than 30% i.e., over the 75th percentile of the distribution in the 2015 – Spring entry cohort. All estimations control for female indicators, age in year at the time of entry, SB11 standardized test score, mothers' has no college education indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by including an indicator equal to 1 if any of the covariates contains a missing value for the student *i*. All standard errors are clustered at the major-cohort level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 16: The Impact of Exposure to Desegregation on the Links of Wealthy Students - 5 Seconds windows

	A. Number of Links			B. Probability of a Link with		C. Proportion of Links
	(1)	(2)	(3)	(3)	(4)	(5)
L_imc with:	Any	Wealthy	Low Income	Wealthy	Low Income	Low Income
OLS						
% of low–income peers	-0.010	-0.043***	0.033***	-0.002**	0.008***	0.706***
	(0.017)	(0.016)	(0.004)	(0.001)	(0.001)	(0.050)
B. Non-linear Effects						
$\mathbb{I}$[% of low–income peers > 30%]	-0.001	-0.775	0.774***	-0.045	0.118***	15.699***
	(0.515)	(0.497)	(0.144)	(0.029)	(0.038)	(2.610)
Pre-treatment statistics						
mu(Outcome)	5.002	4.771	0.231	0.752	0.180	4.386
sd(Outcome)	5.040	4.816	0.548	0.432	0.384	11.14
Treatment distribution in 2015						
Δ mu(R^l_{mc})			9.51 p. p.			
sd(R^l_{mc})			12.98			
No. Students	5,278	5,278	5,278	5,278	5,278	4,063

Note: This table displays the estimates from Equation 1 in Panel A., and the non–linear estimates in Panel B. Outcomes are based on a turnstile–elicited links based on time–windows of 5 seconds and at least two co–movements in a term. *% of low–income peers* is calculated at the major–cohort levels. $\mathbb{I}$[% of low–income peers > 30%] is an indicator function equal to one if the share of low–income peers in the major and cohort is greater than 30% i.e., over the 75th percentile of the distribution in the 2015 – Spring entry cohort. All estimations control for female indicators, age in year at the time of entry, SB11 standardized test score, mothers' has no college education indicator, indicator of middle–SES background according to the social strata indicator, and dummies for whether the student got an SPP loan. I address missing values by including an indicator equal to 1 if any of the covariates contains a missing value for the student *i*. All standard errors are clustered at the major-cohort level. * p < 0.10, ** p < 0.05, *** p < 0.01.