Foreign Students in College and the Supply of STEM Graduates

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Abstract

Do foreign students affect the likelihood domestic students obtain a STEM degree and occupation? Using administrative student records from a U.S. university, we exploit idiosyncratic variation in the share of foreign classmates in introductory math classes and find that foreign classmates displace domestic students from STEM majors and occupations. However, displaced students gravitate towards high earning Social Science majors, so that their expected earnings are not penalized. We explore several mechanisms. Results indicate that displacement is concentrated in classes where foreign classmates possess weak English language ability, suggesting that diminished in-class communication and social interactions might play an important role.

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1 Introduction

Encouraging Science, Technology, Engineering, and Mathematics (STEM) education has been a long-standing goal in the United States, as STEM workers are key drivers of innovation and growth (Griliches, 1992; Jones, 1995; Kerr and Lincoln, 2010; Peri, Shih and Sparber, 2015). Higher education has been an area of particular concern, as recent decades have seen reductions in the share of degrees awarded in STEM fields, and substantial problems with retention – over 50% of intended STEM majors end up switching to non-STEM fields or dropping out (Chen, 2013). Sourcing STEM skills from abroad is one way to assuage concerns over inadequate domestic supply of STEM skills.[1] Large-scale immigration into the workforce has been accompanied by a growing presence of foreign students in higher education as family reunification and less restrictive student visa policies provide pathways for youth immigration.

This paper examines whether the growing presence of foreign students in higher education affects the likelihood that U.S. domestic students complete STEM degrees and eventually work in STEM jobs. We study this in the classroom setting, drawing on administrative student-level records from a large U.S. research university over the 2000-2012 academic years. We focus our analysis on domestic U.S. citizens who attend introductory math courses – often considered an initial gateway for STEM majors – during their first college term. We then explore whether the share of introductory math classmates that are foreign affects the likelihood of graduating with a STEM degree and working in a STEM occupation.

We classify students as foreign if they do not possess U.S. citizenship.[2] The period we analyze precedes the large surge in temporary student visa holders, which began in the late-2000s.[3] As such, our sample of foreign students primarily consists of non-citizen permanent residents (87% of all foreign students), and virtually all are in-state residents – 88% of foreign students are state residents compared with 97% of domestic students.[4] Importantly, permanent residents are likely

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[1] Programs like the H-1B and OPT visa explicitly aim to select STEM workers.

[2] Domestic U.S. citizens include also a minority of individuals who are born abroad: 7.7% of all U.S. citizens with some college in the cohorts of interest are born abroad and naturalized, according to own calculation using American Community Survey data. However, for the purposes of informing how STEM human capital flows into the labor market, U.S. citizenship is the most relevant margin. Naturalized citizens are more likely to be assimilated to U.S.-born students and immigration programs place large restrictions on the entry of non-citizens into the U.S. labor market.

[3] Data from the Institute of International Education Open Doors reports indicate that international undergraduate enrollments in the US grew by 73% from 2010-2020. The institution we study sustained largescale growth in international undergraduates from 2% to over 10% of enrollment over the same decade.

[4] Among foreign students, the distribution of permanent residents and student visa holders is roughly similar when separately examining each ethnic/racial group. For each ethnic/racial group (i.e. asian, white, and minority (black and latino)) about 88%-90% are permanent residents, while 11-13% are visa holders. We note that literature has documented that much of the later growth in student visa holders since the late 2000s has been from Asian,

to be more assimilated than student visa holders, and the incentives to major in STEM likely differ between the two groups. Additionally, US immigration policy prevents most student visa holders from remaining after graduation — currently only 10% of F-1 student visa holders transition to an H-1B visa, which provides temporary work authorization in the United States (Bound et al., 2021).

Our results show that foreign classmates reduce the likelihood that American students graduate with a STEM major and eventually work in a STEM occupation. A 1 standard deviation (4.4 p.p.) increase in the share of introductory math classmates that are foreign reduces the probability of graduating with a STEM degree and also working in a STEM occupation by 4.7 percentage points, or 10% of the mean STEM graduation rate of 48%. Applying our estimates to an average sized class indicates that 10 additional foreign classmates displace 6.7 domestic students from STEM degrees/occupations. Local linear regression analysis suggests that the displacement might be more pronounced among domestic students that possess a weak comparative advantage in STEM fields relative to non-STEM fields (rather than an absolute STEM disadvantage). These students then increase their propensity of majoring/working in Social Science majors/occupations that have equally high earning potential when compared to the STEM fields they leave. There is therefore no detectable aggregate impact on the expected earnings of domestic students. Our results imply that the total number of STEM graduates is slightly reduced by about 2% as the increased inflow of foreign students into STEM majors does not entirely offset the displacement of domestic students.[5]

To identify our effects we leverage idiosyncratic variation in the foreign student share within introductory math courses taught by the same instructor over time. This is a similar approach, albeit stricter, to other studies using variation in peer composition within school-grade pairs (e.g., Hoxby, 2000; Carrell and Hoekstra, 2010; Bifulco, Fletcher and Ross, 2011; Anelli and Peri, 2019; Carrell, Hoekstra and Kuka, 2018). Our identification leverages variation in exposure to foreign classmates, while holding fixed all other classroom factors, such as the instructor, course material, and other peer characteristics. To achieve this, we control for course-by-professor fixed effects and course-by-term effects in our primary specifications. With this specification, the bulk of our identifying variation is from across-cohorts rather than an instructor teaching multiple sections of the same course in a term, in order to account for potentially endogenous curriculum revisions, changes

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and particularly Chinese students (Khanna et al., 2020; Bound et al., 2021). With respect to state residency, the breakdowns are similar across racial groups–roughly 96-97% of domestic students are state residents, while about 86-87% of foreign students are state residents. One notable exception is foreign white students, who exhibit a slightly higher proportion as state residents at 93%.

[5] Roughly 43% of students graduate with a STEM major. At the average introductory math class size of 217 students, this yields about 93 eventual STEM graduates. Our estimates indicate 10 additional foreign students (a 1 sd increase) displaces 6.7 of domestic students from graduating in STEM. Since roughly 50% of foreign students complete STEM majors, 5 of those 10 will graduate in STEM. On net, there is a loss of 1.7 STEM majors (i.e., $6.7 - 5 = 1.7$), which represents about 1.8% of the 93 STEM graduates from the average class.

in course demand, and other course-specific trends. We also account for concomitant contextual effects in the classroom by including controls for peer ability, race, and gender composition. Our preferred specification further saturates the model with controls for class size, and individual-level background characteristics.

Our setting and granular data help us overcome various methodological challenges when estimating peer spillovers in the classroom. Foreign citizenship has the benefit of being a characteristic that cannot be altered by one's classmates, allowing us to identify peer effects without bias due to reflection. Focusing on students during their first-college term helps reduce the scope for selection bias as new students have less information about the registration process, instructors, and/or their classmates. Highly detailed registration actions of each student allow us to measure the class foreign share *prior* to the first day of instruction, to limit endogenous sorting after students observe their classmates.

We further provide formal tests that demonstrate our within course-professor variation is truly idiosyncratic. Balancing tests rule out selection of both domestic and foreign students on an array of observable background characteristics, including race, gender, and ability measures. We also show that the residual variation in the share of foreign peers after partialling out course-by-professor and fixed course-by-term effects is distributed as a randomly generated normal distribution and is uncorrelated with a large array of class characteristics such as class schedule, average student ability and the concentration of nationalities among the foreign students. Our results are robust to controlling for foreign student exposure in other courses, accounting for time of day, and potential endogenous foreign student information networks. As introductory math classes have high enrollment caps that never bind in our setting, our estimates are not attributable to mechanical crowd-out, whereby the entry of foreign students prevents domestic students from registering for the class.

Why do foreign classmates encourage domestic students to pursue non-STEM majors? We probe several candidate explanations: (1) lower introductory math grades due to direct competition, (2) fewer positive spillovers from communication, (3) lower individual ranking in STEM and (4) discriminatory preferences. While the empirical context does not allow us to identify any particular channel with precision, we provide suggestive evidence about which potential channels are plausibly relevant and deserve future research.

First, while we do not find any overall effect of foreign peers on the grade earned in the introductory math class, heteregenous analysis shows a direct negative effect on performance of white male domestic students that is consistent with their higher propensity to choose non-STEM majors as a response to higher share of foreign peers in the class. This suggests that direct competition in introductory math classes may lead to lower grades for white males, thereby displacing them from

STEM majors.

Second, displacement from STEM appears concentrated in classes where foreign peers have particularly low English proficiency. Studies have shown foreign college students to have weaker English proficiency and engage less in communicative activities during class (e.g. Horwitz, Horwitz and Cope, 1986; Erisman and Looney, 2007; Rodriguez and Cruz, 2009; Stebleton, Huesman Jr and Kuzhabekova, 2010; Stebleton, 2011; Yamamoto and Li, 2011). Social interactions and effective communication, such as asking clarifying questions, have been linked to success in schooling and labor market outcomes (Borjas, 2000; Carrell and Hoekstra, 2010; Deming, 2017; Carrell, Hoekstra and Kuka, 2018). In a less communicative classroom environment, there are fewer positive externalities from social interactions and instructors may alter the pace or style of instruction to accommodate non-native speakers. While we are not able to measure the actual level of interaction within the classrooms, we find this to be a likely mechanism and hope future research will explore more in this direction.

Third, class ranking is shown in the literature to impact students' performance and choices (Elsner and Isphording, 2017; Murphy and Weinhardt, 2018). In our context, peers can alter the relative ranking/comparative advantage of individuals within a class or labor market. If this constitutes an updated local signal of ability, individuals might respond by specializing in different human capital and labor market choices (Peri and Sparber, 2009, 2011; Cicala, Fryer and Spenkuch, 2017). Our analysis shows that domestic students in classes with a higher share of foreign peers indeed rank systematically lower in terms of STEM comparative advantage within the class, even after controlling for their true university-wide comparative advantage in STEM. This phenomenon is driven by the fact that foreign students have on average a much stronger STEM comparative advantage. Despite the effect on within-class rank, our analysis does not find differential impacts of foreign peers between domestic students sustaining a larger or smaller fall in STEM ability ranking in the introductory math class.

Finally, displacement from STEM may be a result of simple distaste for taking classes alongside foreign students. While we cannot directly test for distaste, we do not find any systematic relationship between the initial foreign share in one's introductory math class and their exposure to foreign classmates in future courses and terms.

Our work contributes to three distinct lines of inquiry. First, our analysis speaks to the impacts of immigration on education within host countries. This study is the first to link exposure to foreign classmates in college classrooms to eventual completion of particular fields of study. Existing studies on foreign peer impacts have solely focused on primary and secondary education, often in settings outside the United States.[6] Those focusing on higher education have generally examined

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[6] For example, Gould, Lavy and Daniele Paserman (2009); Diette and Oyelere (2012); Brunello and Rocco (2013);

extensive margin outcomes, and we complement new efforts towards elucidating intensive-margin educational outcomes (e.g., Betts and Fairlie, 2003; Cascio and Lewis, 2012; Orrenius and Zavodny, 2015; Chevalier, Isphording and Lisauskaite, 2019).[7] Our inquiry is similar in spirit to Borjas and Doran (2012), who find that the inflow Soviet mathematicians after the collapse of the Soviet Union had detrimental impacts on the careers of American math professors, reducing their publication rates and displacing them to lower ranked universities or out of academia altogether. We demonstrate that domestic individuals may experience less detrimental impacts from foreign peers when exposure occurs earlier in the life cycle, where changes in field-of-study are less costly.

Second, we highlight the importance of peers on human capital investment decisions. Recent work has brought new attention to the importance of major choice, showing that the return to high paying majors rivals the high school-college wage gap (Altonji, Blom and Meghir, 2012), exceeds the return to attending selective institutions (Arcidiacono, Aucejo and Hotz, 2016; Kirkeboen, Leuven and Mogstad, 2016), and has been widening over time (Altonji, Kahn and Speer, 2014). This paper highlights that peer composition can have a large effect on investments in particular fields of study.

Finally, our analysis on labor market outcomes demonstrate how early shocks in education can persist well into the labor market, and carry implications for how immigration of young students may affect the aggregate supply of STEM skills. A rough back-of-envelope calculation indicates that the total number of STEM graduates may fall slightly, as increases in foreign students only partially offset the decreases in domestic STEM graduates. Applying our estimate to the average class size indicates that 10 additional foreign students would reduce the number of domestic STEM graduates by 6.7. Of those 10 foreign students, about 5 will go on to complete STEM degrees. As such the total supply of STEM graduates shrinks by about 2, representing roughly 2% of the total number of STEM graduates expected from the average class. We note, however, that a smaller STEM supply may not be detrimental towards innovation. Hunt and Gauthier-Loiselle (2010) shows that foreign STEM workers patent at almost double the rate of native workers. Kerr and Lincoln (2010) show that skilled immigrants increase innovation within firms without crowding-out, and possibly even crowding-in, innovation from natives. Hence, it is not entirely clear that the slight reduction in total STEM graduates would necessitate less innovation in the long-run. Furthermore, as the skilled immigrant population in the US expands there may be important long-run effects on the STEM supply, if the children of skilled immigrants become increasingly important in our colleges and universities and in the STEM workforce.

Geay, McNally and Telhaj (2013); Ohinata and Van Ours (2013); Diette and Oyelere (2014); Ballatore, Fort and Ichino (2015); Conger (2015); Ohinata and Van Ours (2016); Figlio and Özek (2017); Frattini and Meschi (2019)

[7] Papers on U.S. higher education have focused on international students and enrollment or graduation (Betts, 1998; Hoxby, 1998; Borjas, 2004; Jackson, 2015; Hunt, 2017; Machin and Murphy, 2017; Shih, 2017).

The implications of our findings towards broader policies surrounding foreign enrollment will vary depending on context. Our focus on introductory math courses has broad scope as the subject matter and general way of teaching calculus based courses are fairly similar across higher education institutions. However, U.S. higher education institutions are quite diverse on a wide array of attributes, which need to be considered when assessing the impact of foreign students. The cost of switching majors, course enrollment caps, or the relative skill sets of foreign and domestic students are a few such attributes.

We proceed by describing the institutional setting and our data in section 2. Section 3 details our empirical framework, clarifies our identifying variation, and demonstrates it is consistent with truly random variation in foreign class shares. Section 4 discusses how we overcome the challenges of causal identification in our setting, and provides various tests for selection on observables. Results and robustness checks are presented in section 5. Section 6 describes and tests various mechanisms underlying our main findings. Section 7 concludes.

2 Data

This paper uses administrative data from a selective U.S. public university which follows a trimester system, with three terms per academic year. The university consistently ranks in the top 50 public universities in U.S. New and World Report rankings. Our data contain students' academic records for each term from academic years 2000/01–2011/12. Records contain the class registration activity of students which we use to reconstruct the rosters of each class. For each class we observe the course title, instructor, and term offered. Available student background measures include SAT scores, high school GPA, race, gender, U.S. citizenship status, and nationality. Student level outcomes include date of graduation, major at graduation, declared major (term by term), cumulative GPA, and grades in each course.

Enrollment at our institution is quite large, with undergraduate students comprising roughly 80% of the total student body. The student body is highly diversified with current enrollment figures around 28% Asian, 25% White, 22% Hispanic, and 15% International. Nearly 60% of students receive financial support. The institution is regarded as highly selective, with average SAT scores of incoming students above the national average.

The university provides a wide number of fields of study. Students can earn bachelor's degrees in over 100 different majors, with STEM fields (e.g. Biology, Chemistry, Mechanical Engineering) comprising half of the top 20 most popular majors.[8] Students may enter undeclared, but are required to formally declare a major before completing two full-time years of course work. Switch-

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[8] For the full list of majors classified as STEM see Appendix Table A1

ing majors requires obtaining approval from an advisor in the major they wish to leave and from an advisor in the major they wish to join. Approximately 50-60% of students graduate within 4 years and 80-85% graduate within in 6 years.

Generally, students register for courses in the prior term. First-term freshmen register for classes before they actually begin college. The university offers in-person onboarding, prior to the start of the first semester, where students can meet with academic counselors and advisors to help them schedule their first semester. For non-freshmen, registration occurs over the course of a few days, and registration time slots are randomly assigned within level (i.e. sophomores, juniors, seniors etc), with more senior levels receiving earlier registration priority.

Our focus is on introductory math classes, which never exhibit a binding cap. Introductory classes are large and occur in lecture halls. Instead of splitting each class into several discussion sections for extra tutoring, the math department centralizes tutoring for all introductory courses. Hence, there are no discussion sections. Instead, the department offers a tutoring office that is open during the day for all students to seek assistance.

In what follows, we provide more detail about the institution we study, specify the introductory math courses we focus on, and then describe the students and outcomes in our sample.

2.1 Introductory Math Courses

We focus on domestic students taking introductory, calculus-based, math courses during their first term of university attendance.[9] This choice is motivated by the fact that these courses have long been viewed as gateways to STEM degrees (Steen, 1988) and that all STEM fields require early and satisfactory completion of an introductory math course to progress in the major. Indeed, these introductory math courses are by far the most frequently enrolled among all STEM introductory courses in the university under analysis. Approximately 70% of all students in our data take an introductory math course at some point during their undergraduate studies, with over 40% of domestic students enrolling during their first term.[10] Within U.S. higher education, introductory math courses generally cover uniform subject material – Calculus – thereby limiting the scope for potential issues arising from differences in subject matter breadth and depth, while also enhancing the external validity of our findings. These courses have very high enrollment caps (999 students) that never bind in our institutional setting – enrollment never exceeds 40% of the cap. This implies

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[9] These students enroll in these courses before showing up on campus. Their enrollment decision is therefore hardly influenced by environmental factors and is shown to be exogenous to the class share of foreign peers as we show in section 3.

[10] For reference, the second most frequently enrolled STEM introductory courses are those of the Chemistry department, which have an overall enrollment that is only 2/5 of that for introductory math courses, followed by computer science introductory courses, with an enrollment equivalent to only 1/12 of that for introductory math courses.

that students cannot be mechanically crowded-out of classes because of high demand.

Table 1 lists the introductory math courses in our primary sample. For each course we also provide the total number of domestic first-term freshmen, the total number of domestic students, the total number of foreign students, the average percent foreign across classes, the average class size, and the total number of classes. We consider a course to be introductory if first-term freshmen can enroll, if it satisfies the university-level quantitative course General Education requirement, and if it is a prerequisite for at least one STEM major. Introductory math classes mainly cover calculus topics and have an average class size of 217 students. While we include high achieving students, who take more advanced courses (e.g. Calculus III) in their first term, the basic Calculus I course comprises the majority of the domestic first-term freshmen and a large number of classes in our data. While our primary analysis leverages variation in all of these courses, we also show our results are unchanged when using only Calculus I. First-term domestic students make up 65% of domestic students, and 54% (16,828 of a total of 31,032 students) of total enrollment, indicating these are in fact introductory-level courses.

2.2 Foreign Class Share

We measure exposure to foreign students at the class level, and individuals are identified as foreign if they are not U.S. citizens. The class is a natural unit where interactions might occur as students attend lectures together, and are evaluated jointly by the professor using the same exams and assignments.[11] Moreover, for most of these courses there is no separate discussion section to which students are assigned, as the Math department instead offers a centralized tutoring office. We measure exposure to all foreign students in one's introductory math class by calculating the share of one's classmates that are foreign. To reduce endogenous selection, we leverage detailed registration records to measure the foreign class share on the day prior to the first day of instruction. As such, our foreign share is measured *before* students are physically present to observe their classmates, meet the professor, or examine the syllabus.

Table 1 shows the average class foreign share is 12.3%, and is slightly lower/higher in more basic/advanced courses. Figure 1 shows the overall variation in the foreign share across introductory math classes in our sample. While typically ranging between 8-15%, some classes have less than 5%, and a few have greater than 20%. To abstract from the many potential differences across courses (e.g. course rigor and material, student ability and preparedness) and across instructors (e.g. instructor pedagogy), we choose to exploit variation in foreign shares within courses taught by the same professor over time. We clarify this variation further in section 3 and discuss the

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[11] In rare instances when a single professor teaches more than one class of a course in a term, the classes are treated as distinct. In the data, this only occurs 6 times out of 179 different course-professor offerings.

necessary identifying assumptions of this approach in section 4.

2.3 Analytical Sample Descriptive Statistics

Our resulting analytical sample comprises domestic first-term students enrolled in an introductory math. We note that the sample only contains enrollment in fall terms since the first term for freshmen is always the fall term. Though our data continues through 2012, we restrict the sample to 2006 and prior, so that we can observe 6-year graduation outcomes for all students. This yields a sample of 16,828 domestic first-term freshmen enrolled in introductory math classes between fall of 2000 and fall of 2006.

Table 2 provides various summary statistics of students in the university we study. Columns 1 and 2 refer to all domestic and foreign students enrolled during the period under analysis (2000-2006). Column 3 describes our primary analysis sample of domestic first-term freshmen in introductory math courses. Column 4 displays statistics for foreign students in introductory math courses. While 56% of domestic students are female, only half of first-term domestic freshmen that enroll in introductory math courses are female. Asians, whom account for only 37% of all domestic students overall, are overrepresented in introductory math courses, comprising nearly half of all the first-term freshmen enrolled in introductory math courses. A similar pattern is observed for foreign students. Nearly 80% of all foreign students are Asian.[12]

Foreign students do not appear to be substantially different in terms of ability in the general student population. One exception is that foreign students exhibit substantially lower SAT verbal scores, reflecting their lower English ability. This difference in English ability is magnified when comparing domestic first-term freshmen and their foreign classmates in introductory math classes – SAT verbal scores of foreign classmates are almost a full standard deviation below domestic students. Though differences in SAT verbal are the most salient, domestic freshmen outperform their foreign classmates in introductory math courses on all measures of background ability.

Table 3 summarizes outcome measures for our sample of students. We focus on major at graduation as it is a definitive measure of skill acquisition. Students are classified into one of three broad groups – STEM degree, Social Science degree, or Arts & Humanities – based on their major at graduation (further details in Table A1). We measure graduation within 6 years and those that do not complete within 6 years are referred to as "dropouts", however a small number may actually take 7+ years to graduate.[13]

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[12] International students, those on a temporary student visa, account for 11% of our foreign peer population. Their small sample size limits our ability to statistically distinguish effects of this group from foreign students.

[13] For our early cohorts (2000, 2001, 2002) for which we can observe graduation outcomes for at least 11 years, we find that among students not graduating within 6 years, fewer than 6% go on to graduate within 11 years.

Panel A provides a summary of academic outcomes. Approximately 82% of entering domestic freshmen graduate within 6 years, whereas 18% dropout or take more than 6 years. While domestic students graduate with an average GPA of 3.05, their foreign classmates in introductory math courses perform slightly lower. Students who graduate take slightly more than 16 terms, or 5.33 years (3 terms per year) to complete their degree. Nearly half of all students attending introductory math courses earn a degree in a STEM field, with Social Science comprising less than a third. Only around 8% of students earn degrees in Arts & Humanities.

Panel B focuses on student's labor market outcomes which come from two additional sources. These data allow us to explore if there are persistent effects from peers beyond graduation. First is a measure of expected earnings from The Hamilton Project (Hershbein and Kearney, 2014) and estimated using American Community Surveys (ACS) data.[14] Every student in our data is assigned an earnings level based on the country average for their major for 1, 6, 15, and 30 years after graduation. These earnings are not student-specific (e.g., all Economics majors are assigned the same value) and thus represent a generic estimate of student expected labor market success after college conditional on their major of graduation.[15] Descriptive statics in Table 3 show that domestic freshmen attending introductory math courses have expected earnings along their career similar to those of their foreign classmates. This suggests that on average the domestic students in our analytical sample choose majors that deliver similar earning levels in expectations.

Our second measure is a student-specific STEM occupation indicator. In conjunction with university administrators, we systematically gathered data on individual student job descriptions via publicly available information on the internet and linked it to their student records. We match occupational information for 74% of students in our analytical sample. Occupational descriptions are then matched to Standard Occupational Classification (SOC) Codes using an algorithm based on the O'NET dictionary of occupation titles. We index each occupation as STEM or non-STEM using a classification provided by the Bureau of Labor Statistics.[16] Based on the matching, we estimate that 44% of domestic freshmen are working in STEM fields, while 51% of foreign students

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[14] Hershbein and Kearney (2014) use earnings data from the U.S. Census Bureau's American Community Surveys (ACS) between 2009 (the first wave for which college major was asked) and 2012. Earnings are defined as the sum of wages, salaries, and self-employment business income and refer to the year prior to survey.

[15] Expected earnings and earnings profiles calculated by Hershbein and Kearney (2014) rely on the ACS cross-section of individuals from many cohorts only partially overlapping with the cohorts in our analytical sample. Using these values as outcomes for our cohorts thus implies assuming a certain degree of persistence in the returns to college major across cohorts. Moreover, average earnings by college major from the Hamilton Project are representative of the entire U.S. population of college graduates. Relying on them for our sample requires assuming that labor market outcomes for graduates from the university under analysis do not deviate substantially from those of the average U.S. graduate. Given the characteristics of this University, this assumption is fairly reasonable.

[16] See `https://www.bls.gov/soc/Attachment_C_STEM.pdf`. In particular, we define STEM occupations as those in categories 1 (Life and Physical science, Engineering, Mathematics, and Information Technology) and 4 (Health). The full matching process is described in detail in Appendix C.

are. Based on each individual SOC code we link occupational-based expected earnings, calculated using ACS data as the average earnings of all college graduates born in the same cohorts as our students working in that occupation. Estimated earnings are very similar across the two groups.

These two measures have distinct advantages and shortcomings. First, both major and occupation outcomes reflect the interaction of student choices and various constraints (e.g. major grade requirements, occupation labor demand, etc.). Notably, the occupation outcome occurs later in the life-cycle. The measure of major-specific expected earnings has the advantage of not being subject to bias arising from inaccuracy in measuring students major at graduation. However, a disadvantage is that expected earnings measures are not student-specific. The occupation indicator measure does capture individual-level outcomes, however it is subject to potential inaccuracies due to the imperfections in matching. Hence, the occupation-specific expected earnings measure suffers from both inaccuracy and the loss of individual-level earnings information. Nonetheless, the fact that we estimate similar effects when using these two distinct measures of earnings helps to increase the reliability of our findings.

3 Empirical Methodology

We aim to identify the causal impact of foreign classmates on completing a STEM degree and working in a STEM occupation. Our empirical design is motivated by the ideal experiment, in which identical sets of students would experience random variation in foreign peers, while everything else about the class – such as the professor, course material, other peer traits, and class size – would remain the same. Lacking such a natural experiment, we leverage idiosyncratic variation in foreign class shares, within courses taught by the same professor over time. [17].

We estimate the impact of exposure to foreign students using a linear probability model:[18]

$$Y_{icpt} = \alpha + \beta \frac{F_{cpt}}{N_{cpt} - 1} + \sigma_{cp} + \sigma_{ct} + \gamma \overline{X}_{cpt} + \delta X_i + \varepsilon_{icpt} \tag{1}$$

Y_{icpt} represents an outcome for student i who attended an introductory math class, identified by the course c, professor p, and term t. Exposure to foreign students is measured as the share of

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[17] Many papers have utlized cross-cohort within-class variation to estimate educational peer effects (Hoxby, 2000; Hanushek et al., 2003; Vigdor and Nechyba, 2006; Carrell and Hoekstra, 2010; Bifulco, Fletcher and Ross, 2011; Anelli and Peri, 2019). By included instructor-specific fixed effects, rather than just course-specific ones, we are reducing potential endogenous sorting or student choices that may relate to heterogeneous teaching methods or instructor turnover.

[18] Results from logit and probit estimation (available on request) yield average marginal effects that are similar in size. However, studies (e.g. Greene, 2004) have cautioned against using logit or probit estimation with fixed effects as it can generate biased and inconsistent results.

individual i's classmates that are foreign, $\frac{F_{cpt}}{N_{cpt}-1}$, where F and N represent the number of foreign students and the total number of students registered in the class on the day prior to the first day of instruction, respectively. We standardize the foreign share in our sample so that our primary coefficient of interest, β, can be interpreted as the impact of a 1 standard deviation increase in foreign share on outcome Y.

To leverage variation within course-professor we control for course-by-professor fixed effects (σ_{cp}), which account for fixed differences – such as teaching style, course difficulty, or workload – that might give rise to endogenous student selection across course-professor pairs. We also account for course-by-term indicators to absorb confounding time-varying course-level factors, such as curriculum revisions or growing student demand for particular courses.

We also control for other class characteristics ($\overline{X}_{cpt}$) to account for common classroom shocks. Class-level controls include peer ability measures – average peer SAT Math, SAT verbal, and high school GPA – and average peer race and gender composition. We also show results are robust to controlling for class size, as prior studies have found important interactions between foreign student inflows and class size (Ballatore, Fort and Ichino, 2015). We also add individual-level controls: race, gender, SAT verbal and SAT math scores, and high school GPA. Finally, ε_{icpt} is a mean-zero error term. We cluster standard errors at the professor level.

Before discussing the identification challenges of our empirical strategy, we provide a visual representation of the nature and magnitude of our variation. Figure 2 displays the class foreign share over time for 10 randomly sampled course-professor pairs. Connected points facilitate visual tracking of the foreign class share, within the same course-professor, over time. For example, points A, B, C, and D refer to distinct classes of the same course (e.g. Calculus I) taught by the same professor (e.g. Jane Doe), over different terms. The Calculus I class taught by Jane Doe in Fall 2000 (point A) has nearly double the foreign share than the one taught by Jane Doe in Fall 2001 (point B). Our empirical design draws comparisons in the outcomes of domestic first-time freshmen students enrolled in class A against those in class B. Students across these two classes took the same course (Calculus I), with the same professor (Jane Doe), but were exposed to very different levels of foreign classmates by virtue of entering the university and enrolling in introductory math in different terms. We argue that the difference in the foreign class share in A and B is driven by idiosyncratic fluctuations, and that students in the two classes are comparable.

In Figure 3, we provide visual evidence about the magnitude of our identifying variation. We first plot our within course-variation in panel (a), which displays a histogram of the foreign class share, after partialling out course-by-professor and course-by-term fixed effects. The residual variation in foreign share is still substantial and ranges between -0.6 percentage points and +0.6 percentage points relative to the within course-professor mean. In an average sized class this is equivalent to ± 4 students on average and a maximum of ± 12 students.

We then visually compare our within course-professor variation against random variation. For each class in our sample we randomly draw its foreign share from a normal distribution, using the mean and standard deviation of our observed within course-professor residuals. The histogram of randomly drawn foreign shares is displayed in Figure 3(b).

Intuitively, the randomly drawn foreign shares should be normally distributed. If our within course-professor variation is compatible with random variation, it should also be normally distributed. Figure 3(c) formally tests whether our within course-professor variation is normally distributed. The Shapiro-Wilk test for normality fails to reject the null hypothesis that the variation is normally distributed (p-value=1). For consistency, Figure 3(d) provides the same test of normality for the randomly drawn residuals.[19] These tests show that our within course-professor variation is consistent with the magnitude we would expect from random variation. In Figures 3(a) and (b) we also report the average and maximum variance in foreign students in an average sized class. The magnitude of the within course-professor variation is nearly identical to that of random variation. Hence, this check helps assure that our variation is idiosyncratic in nature and comparable in magnitude with a random normal.

4 Identification challenges

To establish a causal relationship between foreign classmates and the outcomes of domestic first-term freshmen, we first demonstrate that our within course-professor variation is robust to common challenges in estimating peer effects: reflection, selection, and common shocks (Manski, 1993; Moffitt, 2000; Sacerdote, 2011). We discuss in greater detail how our institutional setting, data and identification strategy provide unique advantages to overcome each of these issues. We also perform tests for selection on observables and class-level common shocks to assess the scope for bias in estimation.

4.1 Reflection

Our approach addresses issues of reflection that occur when explanatory peer measures can potentially be influenced by individuals. This is usually problematic when the peer measures is the average outcome of one's peers. However, we examine a peer background trait – citizenship –

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[19] In appendix figure A1 we also perform this test when using less demanding fixed effects. Specifically, when we use course, professor, and term fixed effects, the same test rejects that this level of variation is normally distributed (p=0.06). When forcing variation to come from within course-professor, by including course-by-professor fixed effects (and also term fixed effects), the variation begins to resemble a normal distribution.

that is measured before students meet their classmates. Thus, it is highly unlikely that domestic students could reasonably affect the citizenship status of their foreign classmates before they even physically enter the classroom.[20]

Because we do not include peer outcomes, $\bar{Y}_{-icpt}$, in our specification, this also means that our model estimates a combination of the endogenous and exogenous peer effects (Manski, 1993; Carrell, Sacerdote and West, 2013). While this is a limitation common to most peer effect studies due to the challenge of finding a credible source of identification to disentangle the two, mechanisms driving peer effects are often blends of these two channels anyways, so we do not feel that estimating a combination of the two channels detracts from the model.

4.2 Selection

Selection of students into classes that is related to the foreign class composition would bias our estimates. We take several precautions to help limit selection in our identification. First, we focus on first-term freshmen whom have little prior experience, or knowledge about professor reputation, course detail, and class composition at the time of registration.[21] To reinforce this we leverage detailed registration records to reconstruct the roster of each class one day prior to the first day of instruction. The foreign class share, and all other class-level variables, are therefore measured *before* students ever physically attend class to observe their classmates, meet the professor, and receive an introduction to the course.

Second, while students undoubtedly sort across courses and/or professors, our identifying variation renders endogenous selection within course-professor quite challenging for first-term freshmen. Recall that our design compares the foreign share of a class offered in a fall term to the same class (course-professor) offered in future fall terms (e.g. comparing points A, B, C, and D in Figure 2). Endogenous sorting within course-professor for first-term freshmen would only arise by either delaying (e.g., start college next year instead of this year) or accelerating enrollment (e.g., start college this year instead of next year). This is further complicated by the fact that instructor course assignments are decided during the prior term. Hence, for example, class offerings for Fall 2002 are decided and published in Spring 2002. Students deciding whether to enroll in Jane Doe's Calculus I class in Fall 2001, have little information about whether Jane Doe will teach Calculus I in Fall 2002 or after – often times the instructors themselves may not know future teaching assignments.

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[20] Additionally, because domestic and foreign students are mutually exclusive groups, our analysis does not suffer from more recent concerns of mechanical negative bias (e.g. Guryan, Kroft and Notowidigdo, 2009; Fafchamps and Caeyers, 2016).

[21] Enrollment of freshmen for first-term courses is done online even before students are physically present on campus.

While sorting within course-professor is quite difficult for first-term freshmen, it might be more feasible for non-freshmen students. Selection by non-freshmen within course-professor has the potential to endogenously alter other peer classroom characteristics. For example, domestic sophomores and juniors with poor math ability might delay enrollment within course-professor if they perceive greater competition from foreign students prior to the first day of instruction. Similar to endogenous information networks in immigrant labor markets (Munshi, 2003; Cadena, 2013), foreign students could leverage immigrant networks to select classes within course-professor that might provide them with an unobservable advantage over domestic students.

To this end we provide various checks to examine the extent of selection. First, we formally test for selection on observables using balancing tests. Additionally, in section 5.1 we also explore the potential role played by immigrant networks by controlling for an Herfindahl index measuring concentration of origin countries among the foreign peers. To test for selection on observables, we examine whether the class foreign share is systematically correlated with observable background characteristics of enrolled students. Importantly, we test for selection, not only among first-term freshmen, but also among other domestic and foreign non-freshmen in the class, as endogenous selection by any group could change the classroom composition. Finding little evidence of selection on observables helps increase confidence regarding selection on unobservables (Oster, 2019).

Specifically, we estimate the following regression model:

$$X_i = \alpha + \delta \frac{F_{cpt}}{N_{cpt} - 1} + \sigma_{cp} + \sigma_{ct} + \epsilon_{icpt} \tag{2}$$

The dependent variable in Equation 2 represents measures of individual background characteristics of student i (X_i). Including course-by-professor and course-by-term fixed effects allows us to examine whether selection occurs within course-professor pairs. Since foreign student information networks might operate differently from domestic students, we separately examine each group.

The results of these tests are displayed in Table 4a. Each column corresponds to a different individual background characteristic (X_i).[22] Panel I performs these exogeneity tests for all domestic students, while Panel II performs the tests for all foreign students. None of the estimates in Panel I, and only one estimate in Panel II, are statistically distinguishable from zero at any meaningful level of confidence – consistent with what would be expected under multiple hypothesis testing. The estimate in column 8 of Panel II indicates a one standard deviation increase in foreign classmates is associated with a 13.45 point lower SAT Verbal score for foreign students. Despite being statistically significant, the magnitude of this effect is exceedingly small (one-tenth of a standard

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[22] The sample of 25,701 include both the 16,828 domestic first-term freshmen, and other domestic students (i.e. non-first-term freshmen, sophomores, juniors, and seniors) enrolled in the introductory math courses.

deviation), and unlikely to make a meaningful difference in the classroom environment. Nonetheless, to limit the scope of potential selection bias, we include these background characteristics as controls when we analyze effects on STEM graduation.

4.3 Common Shocks

Causal identification in our setting also requires that there are no other unobserved factors that also vary within courses taught by the same professor, and are endogenously related to the class foreign share. To assess potential bias from common class-level shocks, we examine whether any of our observable class-level traits are systematically correlated with the class foreign share, within courses taught by the same professor. We collapse our data to the class-level and formally test for correlations using the following specification:

$$\overline{X}_{cpt} = \alpha + \psi \frac{F_{cpt}}{N_{cpt}} + \sigma_{cp} + \sigma_{ct} + \epsilon_{cpt} \tag{3}$$

Equation 3 regresses class characteristic X on the class foreign-share ($\frac{F}{N}$). We control for course-by-professor and course-by-term effects so that we focus on variation within course-professor, over time. As before, we standardize the foreign share in the sample so that the coefficient can be interpreted as the impact of a 1 standard deviation increase in the class foreign share.

The results of this exercise are displayed in Table 4b. Different class characteristics are examined across the columns. Columns (1)-(6) show no systematic correlation between the class foreign share, and class gender, race, or ability composition – for brevity we only report the class average composite admission score, which is a weighted combination of SAT Math, SAT Verbal, and High School GPA.

Columns (7) and (8) further show no significant correlation between class size or the time of day in which the class is taught.[23] We note that all introductory math classes are taught on a Monday-Wednesday-Friday schedule, so there is no variation in day of week. Despite not being statistically significant at conventional levels, we do acknowledge that the magnitude of the coefficient in column (7) is quite large – a 1 standard deviation increase in foreign share is associated with an increase in class size of 41 student, almost 25% of the mean. This is not surprising, however, as introductory math courses are in practice uncapped and foreign students have been shown to affect class size in other contexts (Ballatore, Fort and Ichino, 2015). Hence, our preferred specification will include class size as a control.

Finally, columns (9) and (10) examine two different measures of the foreign class composition:

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[23] All introductory math classes are 50 minutes long. There are a maximum of 11 possible class times throughout the day, with the earliest beginning at 8am and the latest at 6pm.

the foreign non-freshmen share, and a Herfindahl index based on foreign student nationality. Because related work has shown that immigrant information networks can lead to endogenous sorting across labor markets (Cadena, 2013), similar information networks might operate among foreign students, and particularly among foreign non-freshmen whom have greater ability to sort within course-professor. As such, systematic sorting of many upper class foreign students would also materialize in changes in the foreign share. Nonetheless, column (9) shows no systematic correlation between the class foreign share and the class non-freshmen foreign share. As an alternative check, we calculate a Herfindahl index based on foreign student's reported nationality.[24] Information networks may likely operate more strongly among students from the same nationality. In this case, systematic sorting of foreign students relying on nationality-specific information networks might result in stronger/weaker concentrations of students from the same nationality. The results in column (10), however, show no correlation between the class foreign share and the nationality-based Herfindahl index.

The lack of a systematic correlation between the class foreign share and observable class-level characteristics helps bolster our confidence that remaining unobservable class-level shocks are likely to be pseudo-random in nature. To further assess the potential for common shocks, we demonstrate in section 5 that our main findings are robust to controlling for these characteristics.

5 Results

We now proceed to our main results on the effect of foreign classmates in introductory math courses in Table 5. We first compare our preferred within course-professor variation (column (4)) to alternative types of variation, in columns (1)-(3). These comparisons help elucidate the trade-off between statistical power and exogeneity, and highlight that our preferred identifying variation is demanding in terms of exogeneity while retaining sufficient power in estimation. In columns (1)-(4) we control for the most basic contextual effects – peer ability, gender, and racial composition. We then interpret our main findings, show stability of within course-professor estimates to further controls in columns (5) and (6), and discuss magnitudes.

Table 5 shows estimates of the impact of foreign students on graduating with a STEM major using the basic framework in equation 1. The outcome variable is an indicator equal to 1 if the student graduated with a STEM major within 6 years from enrollment, and 0 otherwise. The explanatory variable of interest is the class foreign share, standardized within our sample. To facilitate comparisons we report the total number of student observations, and also the number of

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[24] The Herfindahl index is the sum of squared nationality shares. Students report over 100 different nationalities in our data.

student observations, classes, and instructors that actually contribute to identification and are not absorbed by fixed effects. To simplify exposition, we compare variation for effects for domestic first-term freshmen in Panel A. The bottom panel of the table reports the fixed effects and controls used in each column.

Column (1) only includes one-way fixed effects: course fixed-effects, professor fixed-effects, and term indicators. This approach is much less restrictive, leverages more variation in the data (e.g. across course-professor, across course-term), and provides more statistical power. Identification comes from 80% (13,498 of 16,828) of the total number of domestic first-term freshmen, which represent 129 out of a total of 179 classes, which are taught by 43 instructors. Larger statistical power, however, comes with the trade-off of more endogenous variation, as students are highly likely to sort across course-professor. The estimate still indicates a negative and significant relationship between foreign peers and the likelihood of obtaining a STEM degree, yet the coefficient (-0.023) is 60% smaller than that of column (4) (-0.049), indicating that such endogenous sorting may be a significant concern.

Column (2) includes course-professor fixed effects and term dummies, thereby using identifying variation from 55% of students (9,241 of 16,828), enrolled in 74 of 179 classes, and taught by 25 instructors. Compared to column (1), this approach reduces the scope for endogenous sorting across course-professor and also accounts for aggregate shocks/trends. However, this specification fails to account for factors varying at the course-by-term level. These may include factors such as curriculum revisions or changing course demand that may bias results. In instances when a given course is only taught by one (or a small number) of professors over time, these course-by-term factors may likely confound variation within course-professor. The column (2) estimate (-0.28) is still 43% smaller than our preferred coefficient in column (4), indicating that failure to account for such course-by-term factors may bias results.

Column (3) considers an extremely restrictive variation that includes course-by-professor-by-term fixed effects. Identification in this specification relies on professors teaching multiple sections of the same course in the same term.[25] While this perhaps is most equivalent to the ideal natural experiment – students take the same course with the same professor, in the same term, but experience differing levels of foreign classmates – there is a severe lack of statistical power as it is extremely uncommon for professors to teach multiple classes of the same course in a term. Identification comes from only 12 classes, comprising 6% of students in our sample. While we also estimate a

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[25] We note that course-by-professor-by-term fixed effects is conceptually the same as a model that includes the two-way fixed effects: course-by-professor, course-by-term, and professor-by-term fixed effects. Coefficient estimates are identical but standard errors are slightly different. The identifying variation is restricted to only come from professors who teach multiple sections of the same course in a given term. Standard errors are slightly larger when using two-way fixed effects due to the larger number of fixed effects used in estimation.

negative coefficient, standard errors grow tremendously.

We now turn to our preferred within course-professor variation in column (4), which includes course-by-professor and course-by-term fixed effects. Our preferred within course-professor variation (columns (4)-(6)) is identified from 52 of a total of 179 introductory math courses, accounting for 42% of domestic first-term freshmen (7,079 of 16,828) in our sample. While identifying variation is reduced compared to alternatives in columns (1)-(3), we believe our preferred within course-professor variation is advantageous as it is demanding on exogeneity while retaining sufficient statistical power. Further, our tests for exogeneity in section 3 increase our confidence that within course-professor variation is truly idiosyncratic.

We now focus on estimates using within course-professor variation in columns (4)-(6), interpret our coefficient estimates, and discuss magnitudes.[26] Column (5) adds in class size as a control, and column (6) further includes individual-level control variables. The coefficient estimates in Panel A are stable and indicate that foreign classmates are negatively associated with the likelihood that domestic first-time freshmen eventually complete a STEM major. All estimates are statistically significant at the 1% level. Our preferred estimate comes from the fully-saturated specification in column (6), which we utilize for all ensuing analysis. Results indicate a 1 standard deviation rise in the foreign class share reduces the probability of graduating with a STEM major by 4.7 percentage points. The coefficient is roughly 10% of the mean STEM graduation rate of 48%.

By way of comparison, the magnitude of our estimate is equal in size to 3/4ths of the White-Black STEM gap and 1/3rd of the STEM gap across genders.[27] We can also size our estimates by calculating the number of students displaced for a class that has all characteristics fixed at the means in our sample – 10 additional foreign students would displace 6.7 domestic freshmen, out of a total of 67 domestic freshmen STEM majors, from completing STEM degrees.[28]

Panel B considers the impacts of foreign peer exposure on foreign students. Results in columns (4)-(6) are not statistically significant, and coefficients do not appear to be stable. This evidence suggests that foreign students do not systematically respond to increased exposure to foreign class-

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[26] In appendix Table A2 we report the coefficient estimates for all control variables.

[27] Data from the National Science Foundation show that the share of bachelors' degrees earned by White students that were in STEM fields was roughly 17% in 2011. The same share for Black students was 11%. The male STEM graduation rate in 2011 was 25% compared with only 11% for females. Hence, the White-Black STEM gap is around 6 percentage points, while the STEM gap between males and females is 14 percentage points. See (`https://www.nsf.gov/statistics/seind14/index.cfm/chapter-2/c2s2.htm#s2`).

[28] The mean size of introductory math classes is approximately 217 students. If this course had the average foreign share (12.3%) and the average share of domestic first-term freshmen (approximately 65%), it would comprise roughly 26 foreign students and 141 domestic freshmen. Given that domestic freshman graduate in STEM at the mean rate of 48%, we would expect 67 STEM graduates from this group. A one standard deviation increase in foreign classmates amounts to roughly 10 additional foreign students. Recall our effect is 10% of the mean graduation rate. Multiplying 0.10 times 67 (the number of domestic students expected to graduate in STEM) yields 6.7 domestic students displaced from STEM.

mates. Thus, the displacement we observe for domestic freshmen is not offset by an increased likelihood of foreign students persisting in STEM.

5.1 Robustness Checks

Table 6 provides a series of robustness checks against various potential confounds in our analysis. All estimates are based off our preferred specification from column (6) of Table 5, which includes course-by-professor and course-by-term fixed effects, and controls for peer ability, race, and gender composition, class size, and individual controls. We reprint the estimate from this specification in column (1) for reference.

Column (2) ensures our foreign peer impacts are identified from exposure in introductory math courses. Specifically, we add a control for the share of foreign classmates in all other classes taken by domestic freshmen in their first-term. The results are virtually unchanged. This indicates that the transmission of foreign peer impacts on STEM major choice occurs indeed within introductory math classes, as opposed to in other courses.

We reemphasize that student sorting is quite difficult at our level of variation. To sort within course-professor, students would generally have to delay enrollment in a class to a future term. For first-term freshmen, this would require delaying college. For non-freshmen students, this could be more feasible. We provide various checks against sorting in columns (3) and (4). Column (3) examines whether there is potential sorting within course-professor based on scheduling preferences by controlling for an indicator of whether the class is offered in the AM or PM – we note there are a small number of classes for which the time of day was not available in our records, and so the sample size is slightly smaller.[29] Column (4) limits the sample to only Calculus I courses, where the large majority of students are first-term freshmen (see Table 1, and thus where endogenous sorting of non-freshmen students may pose less of a threat. Results from both of these checks remain robust and statistically significant, though column (4) loses some precision due to the 40% reduction in sample size.

Finally column (5) and (6) examine whether the presence of foreign student information networks biases our estimates. Column (5) replaces the explanatory variable of interest i.e. (i.e. the

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[29] With respect to sorting on time of day, we first note that all introductory math classes are taught on a Monday-Wednesday-Friday schedule throughout the entire period analysis, so there is no sorting based on day of week. There are a maximum of 11 potential time slots, as classes are offered in 50 minute intervals from 8AM to 7PM. However, in our sample there is very little variation in time of day within course-professor – that is, professors that teach the same course repeatedly tend to keep the time slot consistent. On average professors teach in 1.9 different slots, that is most of them teach in at most 2 different time slots and some keep the same exact slot. Considering this limited time slot variation within professor, it is hardly possible to identify specific time-slot effects separately from professors fixed effects. In column (3), we therefore control for a dummy variable indicating whether the course is offered in the AM or PM

overall foreign peer share) with the share of peers that are foreign first-term freshmen. As first-term freshmen are entirely new to the university, they may be less able to leverage information networks. While the estimated magnitude is similar to our baseline, using only foreign first-term freshmen reduces our foreign share variation. As a result, estimates become more imprecise. Column (6) controls for the nationality-based Herfindahl index, described in section 3, to account for the presence of foreign student information networks. Results remain robust and significant.

5.2 Non-STEM Degrees and Expected Earnings

Having established the robustness of our results on STEM graduation to various concerns, we now examine where domestic students that were displaced from STEM ended up. Table 7 uses our preferred specification from column (6) of Table 5 and examines alternative outcomes. Column (1) reprints our main effect on STEM graduation, for reference. Columns (2) and (3) examine the likelihood of completing a Social Science or Arts & Humanities degree, respectively. Column (4) examines the likelihood of dropping out. The decline in graduating with a STEM major is primarily offset by increases in graduating with a Social Science major. A one standard deviation increase in foreign classmates is associated with a 4.1 percentage point increase in the likelihood of graduating with a Social Science major. There also is a small positive impact on graduating in Arts & Humanities, while there is no discernible impact on dropping out.

Since STEM graduates earn more on average than non-STEM graduates[30], a decline in the probability of STEM graduation might be expected to negatively impact earnings of the domestic students. However, the aggregation of outcomes into three groups (STEM, Social Science, Arts & Humanities) may mask heterogeneity within STEM and non-STEM majors, and potentially important margins of adjustment. For example, displacement from a high earning STEM major to a low earning Social Science major carries far different implications than displacement from a low earning STEM major to a high earning Social Science major.

Lacking data on actual earnings, we link each student's major at graduation (we observe 151 different majors of graduation in our data) to measures of the expected earnings for that major, and use log earnings as the outcome variable.[31] Major-specific expected earnings provide an alternative way to measure of the relevant qualities and characteristics of each major, and may reveal intricacies not detectable when splitting by subject-matter into STEM, Social Science, and Arts &

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[30] In our data expected earnings of STEM graduates 11-15 years after graduation are 22% higher than those of non-STEM graduates.

[31] These measures are provided by the Hamilton Project (Hershbein and Kearney, 2014) and estimated using American Community Surveys data. Data include estimates for initial earnings, earnings at 6, 11-15, and 26-30 years after graduation. Dropouts are assigned the average earnings of students with some college who did not complete a degree. More details about these data are reported in the Data section.

Humanities. While expected earnings may be useful, we also caution that average earnings presumably masks substantial heterogeneity within college major, and acknowledge that this limits our ability to characterize marginal students.

Results are shown in columns 5-8 of Table 7. Estimates are positive, possibly suggesting that foreign classmates may induce domestic students to choose non-STEM majors with higher expected earnings, but small and very imprecisely estimated. Standard confidence intervals do allow us to rule out large negative impacts on expected earnings, and hence it is not the case that domestic students are systematically displaced from STEM majors with very high expected earnings to non-STEM majors with low expected earnings.[32]

5.3 Impacts on STEM Occupation

Graduation in a STEM degree is a strong correlate for entry into STEM occupations, as fewer than 9% of all individuals with a college degree in a non-STEM field report working in a STEM occupation.[33] Displacement from STEM majors would naturally be expected to also reduce the probability of working in a STEM occupation, but not for certain. For example, if every student displaced from a STEM major would have counterfactually worked in a non-STEM job, we should find no effect on the likelihood of working in a STEM occupation. At the other extreme, if every student displaced from a STEM major would have counterfactually worked in a STEM job, the effects of working in a STEM occupation and graduating with a STEM major should be identical. Hence, conditional on there being displacement from STEM majors, understanding to what extent foreign classmates have long run impacts on occupational choice still requires empirical investigation.

We utilize individual data on actual occupations of students, and estimate our baseline specification, replacing the outcome with indicator variables for working a STEM or non-STEM occupation. Because we are unable to link occupational data to all students, we first ensure that the likelihood of finding an occupation link is not endogenously related to the foreign classmates exposure. This check is performed in column 1 of Table 8, where the outcome is an indicator variable equal to 1 if occupational records were matched to the student and 0 if no match was found. The results assure that the sample of students containing occupational information is not endogenously selected.

We examine whether foreign classmates affect the likelihood of working in a STEM occupa-

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[32] Appendix B presents a graphical analysis of the major counterfactual dynamics underlying the null effect of foreign share on expected earnings.

[33] Authors' tabulations from individuals age 30 and under, reporting both college major and occupation in the 2009-2016 ACS.

tion after college in column 2. Results indicate that a 1 standard deviation rise in the foreign class share lowers the probability of working in a STEM occupation by 2.6 percentage points. The estimate is statistically significant and is 6% of the mean probability of working in a stem occupation (43%). The results indicate that the impact of foreign classmates has implications for educational attainment (STEM major) and STEM career paths. We caution, however, that our matching process is imperfect, and that having a binary dependent variable necessarily indicates the presence of non-classical measurement error. In order for measurement error to entirely explain our findings, however, would require that the difference in mean covariates of false positives and false negatives, weighted by their probabilities in the sample, be quite large (Meyer and Mittag, 2017).

Similar to our exercise using expected earnings for each major, we utilize occupational-specific earnings to better characterize the nature of displacement from STEM occupations.[34] Columns 3, 4 and 5 of Table 8 use the log of occupation-specific average individual income, family income and average wage, respectively, as outcomes. Coefficient estimates are negative, imprecisely estimated, and very small in size. These results indicate that domestic students are not displaced into significantly lower paying non-STEM occupations – they appear to be choosing occupations that have very similar earning power relative to the STEM occupations from which they are displaced.

In sum, we find that foreign peers reduce the likelihood domestic students complete STEM degrees. Domestic students respond by switching to Social Sciences. While these effects result in persistent long-run reductions in the likelihood of working in STEM occupations, they do not appear to have detrimental impacts on expected earnings. We now examine heterogeneity in effects to better characterize marginal students and inform our investigation of potential underlying mechanisms.

5.4 Baseline Ability

To further understand STEM displacement, we assess whether effects differ by baseline STEM ability, as marginal students might be those with relatively low baseline STEM ability. For each domestic student, we construct measures of both absolute ability and comparative ability in STEM. Individuals with high/low STEM ability are those with high/low SAT math scores relative to the average SAT math score of their cohort. Individuals with high/low comparative ability in STEM are those who have a relative SAT Math score (i.e. SAT math score relative to the average SAT math score or their cohort) that is higher/lower than their relative SAT Verbal score. To uncover heterogeneity, we use local linear regression to estimate the effects on STEM graduation at each

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[34] Specifically, using the 2014-2016 American Community Surveys we calculate average earnings/income measures for college-educated native-born workers from the same birth-cohorts as those observed in our student data. We then match these earnings/income measures to students according to their observed occupation.

percentile of the absolute and comparative advantage measures.[35]

Figure 4 plots coefficients from our main specification and shows that students with low comparative advantage in STEM (low percentiles) experience relatively stronger displacement. The bottom third of students have an average coefficient of -0.07, while for the top third it is -0.02. However, a parametric test for heterogeneous effects by tertiles does not show evidence of a statistically significant difference. Hence, we view this as suggestive evidence that students with weaker comparative advantage in STEM fields (higher comparative advantage in non-STEM) might be those most at risk of displacement.

The bottom panel of Figure 4 presents local linear regression estimates to see if effects differ based on a measure of absolute advantage.[36] There is little difference in the effect for domestic students with high and low absolute STEM ability. All point estimates are contained within the confidence interval for all others. Using this measure, we cannot reject that students with differing absolute STEM ability are equally displaced from STEM.

5.5 Race and Gender

Table 9 explores heterogeneity across different types of domestic students. Each estimate represents a separate regression using our preferred specification. Research on the gender gap in STEM education has uncovered various factors, such as confidence and role-models, as important for the retention of female students (e.g. Gneezy, Niederle and Rustichini, 2003; Niederle and Vesterlund, 2007; Carrell, Page and West, 2010). We assess whether foreign classmates may more strongly affect domestic females relative to males. The first two columns show that females are not strongly impacted by foreign classmates. Instead, the reduction in STEM primarily comes from domestic male students.

In columns 3-5, we stratify on domestic students' race/ethnicity. Similar to the gender gap in STEM, the minority gap in STEM has also received much academic attention. Our results show that foreign peers have negative impacts on the likelihood of graduating with a STEM major for all race groups, though for minorities (Blacks and Latinos), estimates are imprecise. An interest-

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[35] To construct our measure of comparative advantage, we separately standardize students SAT math and verbal scores at the cohort level to have mean 0 and standard deviation of 1. Then, students are ranked based on the difference in their standardized math and verbal test scores. Local linear regressions of Equation 1 are estimated at every percentile using a one-standard deviation bandwidth and Epanechnikov kernel weighting. 95% confidence intervals are constructed from 250 bootstrapped repetitions, sampled at the class (i.e. math lecture) level.

[36] To measure absolute advantage, we estimate the ex-ante likelihood that a student will graduate with a STEM major. We regress STEM graduation on all background characteristics (gender, race, SAT, etc.) and year fixed effects. We then use the regression coefficients to predict each student's likelihood of graduating with a STEM major. Our measure is relatively simple, but represents the type of prediction policymakers or education administrators may use when trying to determine what factors lead to STEM persistence.

ing insight is that minorities are more likely to stay in school and graduate, which leads positive and significant effects on expected earnings, both in the short and long run. We note this result is consistent with Arcidiacono, Aucejo and Hotz (2016), who find that minority students at highly ranked institutions may be more likely to have graduated with a STEM degree if they instead attended less-competitive, lower ranked institutions. In our setting, foreign peers in the classroom appear to indirectly induce minority students to stay in school by choosing less competitive majors. Whites and Asians are strongly displaced from STEM majors, similar to Borjas (2004), and gravitate towards Social Science. In contrast to minorities, Asian students also are more likely to dropout, which in turn results in significant negative impacts on expected earnings.

5.6 Initial Major Choice

To better characterize the nature of our effects, we assess whether results are more consistent with STEM majors being displaced out of STEM, or non-STEM/undeclared majors being less likely to switch into STEM fields. Columns (6) and (7) of Table 9 examines differences in effects between students who declared a STEM major by the end of the first term and those who did not (i.e. either declaring a non-STEM major or remaining undeclared). We note that our data only contain major at the end of the first term of enrollment, and not before. While we stratify on the major reported by the end of the first term, we also caution that this is in itself an outcome. Nonetheless, there does not appear to be an apparent difference between students that declare STEM early, and those who do not – both groups experience sizable displacement from STEM majors and into Social Sciences.

6 Exploring Mechanisms

Why do foreign classmates lead to lower STEM completion among domestic students? We hypothesize four mechanisms. First, displacement might be the result of competition which lowers grades, and hence leads students to abandon STEM majors. This might happen in classes with "curved" grading if foreign students perform relatively better than domestic students in these courses. Given that declaring a STEM major requires obtaining grades in introductory math courses above a certain threshold, if marginal domestic students obtain lower scores as a result of more competition when the share of foreign students in those classes is higher, they might be systematically displaced from STEM majors.

Second, changes in the communicative environment within classrooms following the entry of many non-fluent English speakers may reduce the scope for knowledge spillovers that arise from questions asked during lecture, or from peer-to-peer interaction. Alternatively, instructors may respond by altering the delivery of the course, thereby affecting student's relative learning and or enjoyment.

A third hypothesis is that foreign classmates in introductory math classes may provide students with a local assessment of their relative ability. As the introductory math class is often the first STEM class that students take, they may perceive their relative ability in that class as a signal of their ranking among all STEM majors. As foreign students have a comparative advantage in STEM relative to non-STEM fields, their presence may lead domestic students to update their perceptions of how their own comparative advantage in STEM ranks among other students.

The fourth mechanism we explore is simple distaste. If domestic students do not enjoy the presence of foreign students and/or update their beliefs about the presence of foreign workers in STEM occupations based on the foreign share observed in the introductory math courses, they may seek alternate classes or majors by means of avoidance.

6.1 Competition in Introductory Math Courses

We examine whether domestic students are displaced from STEM majors because of competition from foreign peers. This could manifest in several ways. Domestic students might have a higher likelihood of dropping the course, or may receive lower grades if they remain enrolled.

Panel A of Table 10 examines whether foreign peers impact the likelihood of withdrawing from the course. Positive effects would indicate that students select out of math very soon after meeting their classmates. Overall, results indicate that females and white students are induced to drop the course. Results for other groups are not statistically significant. We note that while withdrawal could signal competition, it could also be due to other reasons, such as preferences against studying with foreign peers. We return to this discussion later when we formally examine preferences in section 6.4.[37]

Panel B examines the impact on grades, which we standardize within the class to have mean of zero and standard deviation 1, conditional on remaining in the class. While the overall results in column (1) do not reveal grade effects, column (4) shows significant negative impacts on grades for white students – a 1 standard deviation increase in foreign peers reduces grades by a tenth of a standard deviation. Panel C examines a different measure of academic performance – the likelihood of receiving a grade above the median. While overall grade effects are useful to study, they may mask marginal changes. In particular, one margin of adjustment that would be relevant in our setting would be receiving the minimum grade sufficient for progression in the STEM major. Because majors vary in grade cutoff scores required for progression, we use above and below

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[37] In specifications not shown, we also separately examined immediate withdraws (one week or less into a course) and late withdraws (likely after receiving graded work) and found no significant effects.

median as a rough indicator of one's ability to progress. Interestingly results show white students are also less likely to score above the median. As such displacement of white students from STEM majors may be driven by lower grades, and in particular, reducing grades below the sufficient threshold for progression in the major.

We also examine other shorter run outcomes in panels D and E. Specifically, in panel D we examine whether students ever made a switch out of a STEM major, conditional to declaring STEM early before or during their first term of university attendance. The results helps us better characterize that the marginal student was one who otherwise would have majored in STEM, but switched out of STEM due to foreign peer exposure in their introductory math class. This contrasts with non-STEM majors who would have switched into STEM majors. Panel E also examines whether foreign peers affect the time to form their final major decision, measured in academic terms. The idea is to track whether foreign peers' displacement delays domestic students' major choice. This would be consistent with domestic students requiring time to decide the alternative major once they decide to abandon their STEM option. Results do not show a strong pattern of effects on time to declare the final major. For white students, the coefficient is marginally significant, but rather small in magnitude – a 1 standard deviation increase in foreign peers increases the time to declare a major by a third of a term, which is roughly one month.

6.2 English Communication

Descriptive studies and surveys about foreign student integration in U.S. education have emphasized their lower levels of English proficiency (Erisman and Looney, 2007), and subsequent reticence and hesitance to communicate within classroom settings (e.g. Horwitz, Horwitz and Cope, 1986; Rodriguez and Cruz, 2009; Stebleton, Huesman Jr and Kuzhabekova, 2010; Stebleton, 2011; Yamamoto and Li, 2011).[38] Lower levels of communication may reduce positive externalities arising from peer-to-peer or peer-to-instructor interaction. Lower English language ability may lead instructors to alter the pace or style of instruction, or substitute time away from helping domestic students towards helping foreign students (Diette and Oyelere, 2012; Geay, McNally and Telhaj, 2013).

To empirically assess this concern, we examine whether effects are driven by foreign classmates with low levels of English proficiency. Primarily, we categorize foreign students as having relatively "low" or "high" proficiency based on whether their SAT verbal score falls below or above the median score of all foreign students in their cohort. We then repeat regressions of equation 1, splitting the overall foreign share in the class into the shares with high and low fluency.

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[38] An extensive report on foreign individuals in higher education (Erisman and Looney, 2007) found that 66% of foreign students indicated English was not their primary language.

The results from this exercise are reported in panel A of Table 11. The displacement from STEM is larger for domestic students that experience increases in foreign classmates with low fluency. A one standard deviation rise in the share of low fluency classmates reduces the likelihood of completing STEM majors by 5.3 percentage points. An equivalent increase in classmates with high fluency has no significant effect. classmates with low fluency displace domestic students primarily towards Social Science, but partially also towards Arts and Humanities. In columns 5 and 6, we test whether the effects on grades that were statistically detectable for the whites in Table 10 are amplified when interacted with the language mechanism. While the effect of a higher share of low fluency classmates on the probability of getting a grade above the median is marginally not significant (p-value of 0.10), its magnitude is larger than for the effect of the overall share of foreigners. Overall, this constitutes weak evidence for a direct role of reduced classroom communication in affecting domestic students' performance in introductory math courses. Therefore, the effect of the language mechanism on major preferences is likely taking place through other potential channels, for instance the overall introductory math course enjoyment/experience.

In the appendix, we explore this communication/interaction channel further by exploring whether the impact of foreign classmates with low English fluency is exacerbated/limited by the English proficiency of instructors. In particular, native English speaking professors might be more equipped to alter the pace of instruction to compensate for lower levels of classroom communication. Foreign professors with less English fluency may reduce peer-to-instructor interaction even further. With the important caveat that domestic and foreign students might endogenously select instructors, Table A3 shows results where the shares of foreign classmates with high and low English fluency are interacted with indicators for whether the instructor is a native English speaker. These estimates show that in courses with a native English speaker instructor the heterogeneous effect of foreign peers' English proficiency is neutralized, although the displacement effect remains on average. This suggests native English speaker instructors might indeed be able to compensate for lower levels of communication in the classroom. To the contrary, foreign instructors appear to polarize and magnify heterogeneous effect of foreign peers' English proficiency on domestic students' major choice, with a large and precise displacement effect in classes with a high share of low proficient foreign peers and a positive effect in classes where the foreign peers can better communicate with classmates and the instructor.

Overall, these results constitute suggestive evidence that foreign classmates with lower levels of English ability might drive the displacement from STEM. While we cannot pin down the reason for why this is the case, our working hypothesis is that the communicative environment within classrooms is altered when the foreign peers have low English proficiency. Reduced communication within classrooms may result in diminished social interactions and/or missed peer-to-peer and peer-to-instructor exchanges, that are essential components of effective learning and of an enjoyable social experience. While our analysis offers first-hand evidence of the potential role of communication within the class, further research is required to provide more rigorous evidence of the linguistic dynamics of foreign peers in the classroom.

6.3 Relative Ranking in STEM

The movement of domestic students away from STEM fields may be a response to a signal that alters one's perceived relative ability ranking in STEM. Related literature has shown that rankings matter substantially for educational choices and outcomes (Cicala, Fryer and Spenkuch, 2017; Elsner and Isphording, 2017; Murphy and Weinhardt, 2018). In response to foreign peers, that may change relative STEM rankings within a classroom, individuals may switch to fields of study or occupations that are less quantitative in nature, and more communication-intensive, in accordance with the theory of comparative advantage (Peri and Sparber, 2009, 2011). In our context, domestic students may perceive that their comparative advantage in STEM fields falls with more foreign classmates, and respond accordingly by switching to non-STEM majors.

We use SAT Math and Verbal scores to proxy for individual ability in STEM and non-STEM fields, respectively, as they have been shown to predict STEM and non-STEM major choice (Turner and Bowen, 1999)[39] To measure the ability of each individual in STEM and how they rank relative to their classmates, we utilize a traditional approach aimed at identifying individual comparative advantage (Sattinger, 1975). We define individual's ability in STEM *relative* to their cohort, by calculating the distance in standard deviations of the individual's SAT Math score from the average SAT Math score of their cohort (which is standardized to 0). Our measure of comparative advantage in STEM is then the difference between an individual's relative ability in STEM and non-STEM. We refer to this as cohort-level comparative advantage. The summary statistics presented in Table 2 indicate that foreign students possess a comparative advantage in STEM fields. Their relative SAT Math to Verbal score is higher than that of domestic freshmen.[40]

We then also construct measures of comparative advantage within the individual's introductory math class, by comparing individual ability relative to the class SAT averages rather than the cohort averages, which we refer to as class-level STEM comparative advantage (CSCA).[41] This

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[39] Turner and Bowen (1999) documented that SAT verbal scores are associated with a higher likelihood of majoring in non-STEM, and in particular Humanities fields, especially when SAT math scores are low. They also show a similar association for SAT math scores – higher SAT math scores are correlated with a greater likelihood of majoring in STEM.

[40] The comparative advantage of foreign students in STEM is unlikely to be institution specific – foreign college educated individuals in the labor market are highly over-represented in STEM fields and STEM majors (Gambino and Gryn, 2011; Peri, Shih and Sparber, 2015).

[41] Our measure is similar to the measure of the degree of misinformation of ranking from Murphy and Weinhardt

allows us to first measure whether exposure to foreign classmates in introductory math classes actually provides a different signal of an individual's ranking in STEM in the classroom relative to their actual ranking in the cohort. Column 1 of Table 12 performs this check. We utilize our baseline specification, and replace the dependent variable with the measure of an individual's CSCA. Additionally, we also control for the cohort-level comparative advantage, so that regressions are identified from individuals with the same cohort-level comparative advantage but different exposure to foreign classmates. Results indicate that foreign students drive down the average CSCA ranking of domestic students relative to their position in the cohort.[42]

Column 2 offers descriptive evidence that the CSCA is on average positively correlated with the probability of graduating in STEM. Column 3 combines these two pieces of evidence to test whether the interaction of a higher share of foreign peers with a lower CSCA drives STEM displacement effects – i.e. are students who face a higher share of foreign peers and have a low CSCA, holding their cohort STEM comparative advantage constant, more likely to be displaced? The interaction coefficients of column 3 indicate that the displacement effect of foreign peers is not heterogenous across different CSCA quartiles. Hence, we conclude that mechanical effects of foreign peers on the class comparative advantage in STEM of domestic students are unlikely to be an operative mechanism. This finding is also consistent with Murphy and Weinhardt (2018) who find that local (classroom) ranking signals/information are generally less likely to be important when optimizing future effort and other educational decisions.

6.4 Social Preferences

A final reason for displacement may be due to preferences over peers in the classroom. Distaste for studying alongside foreign classmates would manifest in domestic students avoiding them in future courses. We replace the dependent variable in equation 1 with the share of foreign classmates in all classes taken in following terms. We perform this analysis for up to 9 terms (i.e. 3 academic years, where 1 academic year consists of 3 terms) since many students graduate or drop out of the sample after 3 years.

The results of this exercise are shown in Figure 5. Point estimates are indicated by the dots and 95% confidence intervals are provided for reference. The vertical axis measures the effect of a 1 standard deviation increase in the share of foreign classmates in intro courses on the share of foreign classmates in all classes in future terms. Results in panel (a) indicate no overall pattern of

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(2018), whereby the classroom ranking is a local measure that may not reflect one's ability in the cohort.

[42] This specification still holds individual and peers' SAT math and SAT verbal constant. This means that domestic students with same ability in courses with similar overall average ability can have very different within-class comparative advantage standings according to the foreign share in the course.

avoidance of foreign students.

In panel (b) we assess whether rigid course sequences for STEM majors may constrain their ability to avoid foreign peers. We therefore focus on the share of foreign classmates in all non-STEM classes for domestic students who declared a STEM major during the first term of university attendance. STEM majors may find it easier to select out of classes with many foreign peers when they are non-STEM, and likely elective courses. Panel (b) shows that domestic STEM majors show no systematic avoidance of foreign students in future non-STEM courses.

To complete the analysis, we provide an analogous figure in panel (c), which focuses on the future foreign share in STEM classes chosen by domestic students who did not declare a STEM major during their first term of university attendance. In this case, there is a negative effect of foreign peer composition in introductory math courses on future courses' foreign share, which shows up from the second year of university attendance. While this evidence might reflect actual avoidance behavior, we cannot exclude alternative explanations. For instance, students displaced might develop an aversion for math-intensive courses and therefore choose easier-to-pass elective STEM courses, which happen to be less attended by foreign peers.

7 Conclusion

Disinterest in STEM education has generated concern over whether the U.S. will have sufficient numbers of STEM workers. At the same time, globalization has increased the number of foreign students in higher education institutions. This paper explores whether the presence of foreign students in college affect the likelihood that domestic students obtain STEM degrees and eventually work in STEM occupations.

Using administrative records from a large U.S. research university, we find that higher exposure to foreign classmates in the first-term introductory math course reduces the likelihood that domestic students eventually complete a STEM degree and pursue a STEM career. Displaced domestic students adjust by moving to Social Science majors. Displacement does not appear to substantially harm the earnings of domestic students, as they gravitate towards Social Science majors/occupations with equally high earning power.

We follow the peer effect literature exploiting the natural variation in cohort composition across time within schools (e.g., Hoxby, 2000; Carrell and Hoekstra, 2010; Anelli and Peri, 2019) and focus on within course-professor groups to estimate the causal impact of foreign classmates. We argue, with empirical support from exogeneity tests, that there is idiosyncratic cohort-to-cohort variation in the number of foreign peers enrolling introductory math courses conditional on course and instructor. Our results are identified from idiosyncratic variation in foreign peers within courses taught by the same professor over time. This leaves open a narrow potential channel of endogenous student sorting on unobservables across years and we encourage future studies to use fully randomized assignment to courses.

We test several potential channels spanning from direct competition effects to within-class communication, signal updating and discriminatory preferences. We find suggestive empirical evidence that changes to the communicative environment within the classroom might play the main role in generating displacement. Foreign students with low levels of English proficiency may be less likely to engage in communication in the class, leading to fewer productive peer-to-peer and instructor-to-peer interactions. Corroborating analysis finds that foreign students who possess weak English language skills appear to have much stronger impacts than those who are fluent in English.

If the role of the communication channel is confirmed by further research, our study generate implications for interventions aimed at preventing attrition from STEM majors. Interventions that improve or facilitate interaction and communication of foreign students (e.g. compulsory attendance of pre-college English courses) may help improve peer-to-peer learning and instructor-to-peer interaction. Alternatively, distributing foreign students with very low English fluency more homogenously across courses and avoiding their concentration in courses taught by foreign instructors, might reduce the negative impact on the overall class communicative environment.

Though this study was performed on a single university, our findings carry implications for aggregate welfare. Increasing numbers of foreign students – who have unconditionally higher propensity to graduate in STEM majors – are unlikely to increase the future STEM labor supply, as domestic students are displaced to non-STEM fields. Moreover, given that a portion of foreign STEM students are likely to return to their country after graduation – for instance because of the cap on H-1B visas – the U.S. aggregate supply of STEM workers might actually decrease. Despite the lack of growth in the STEM workforce, however, there may be efficiency gains, as displaced students are those comparatively weak in STEM fields, and hence are being induced to move to fields in which they are comparatively stronger.

In the face of increasing globalization, understanding the impacts of foreign students in college remains an important undertaking. This paper is the first to explore whether foreign students affect college major and career occupational choices. Future research that further explores the mechanisms underlying such impacts would be of great value for education administrators and policymakers alike.

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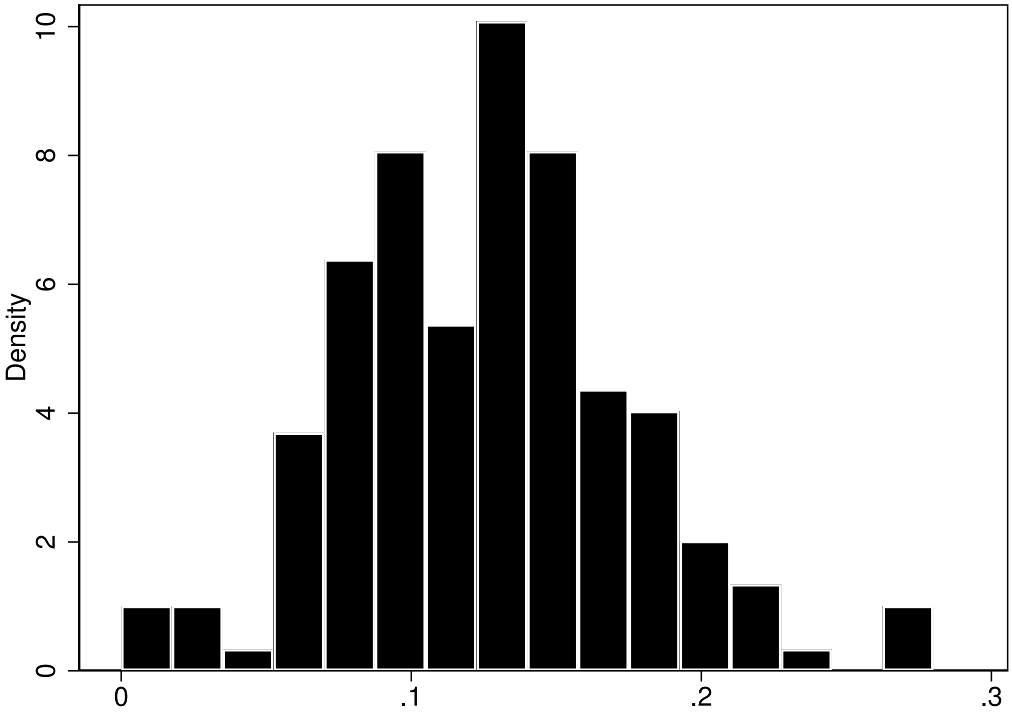
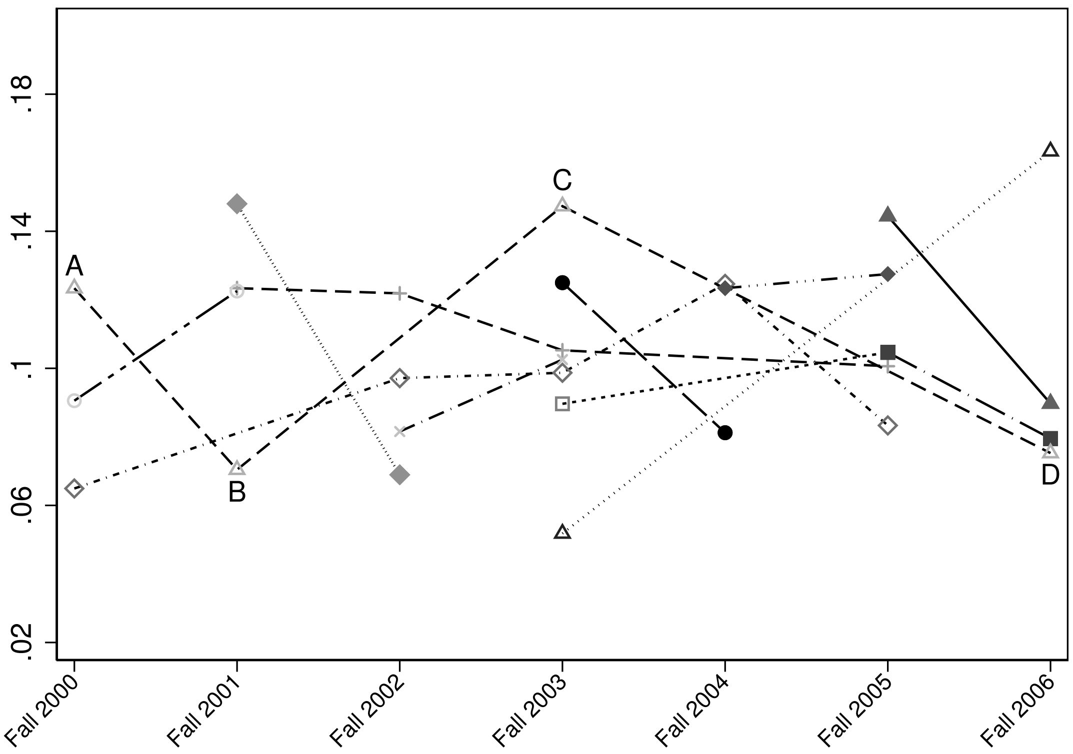
Figures & Tables

Figure 1: Variation in foreign share in introductory math courses



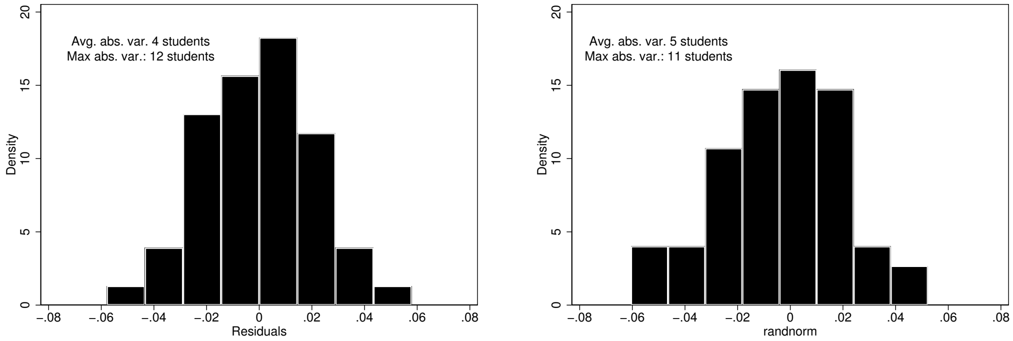
Note: Figure displays histogram of the foreign share across introductory math classes in our sample. Introductory math classes are defined by unique course, professor, and term combinations.

Figure 2: Foreign Share variation within course-professor over time



Note: Figure provides a visual illustration of our within-course professor variation for 10 randomly sampled course-professor pairs. Each point represents the foreign class share for a given introductory math class. Classes of the same course, taught by the same professor are connected with lines. Terms are displayed on the horizontal axis and share of foreign students in the class on the vertical axis.

Figure 3: Within Course-Professor Variation vs. Random Variation

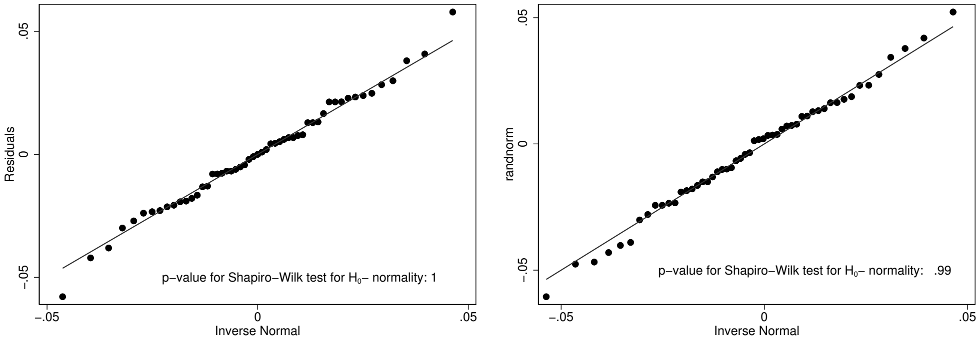


(a) Within Course-Professor Variation

(b) Randomly Generated Foreign Share Variation

(c) Normality Test of Within Course-Professor Variation

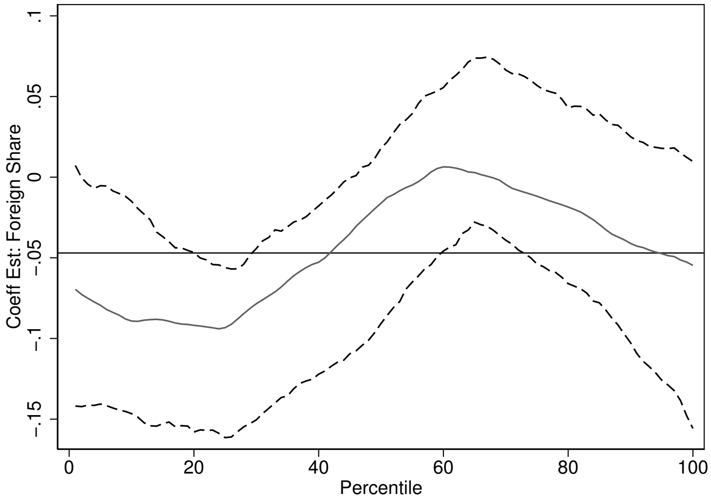
(d) Normality Test of Random Variation



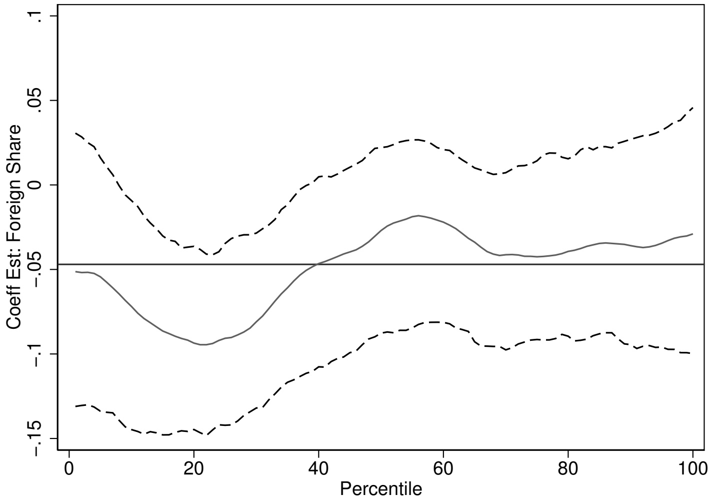
Note: Figures compare our within course-professor variation in foreign shares to randomly generated foreign shares. Panel (a) plots a histogram of our within course-variation in foreign class shares after partialling out course-by-professor and course-by-term fixed effects. Panel (b) plots a histogram of foreign class share variation after randomly drawing foreign shares from a normal distribution, using the mean and standard deviation of our observed within course-professor residuals. Intuitively, the randomly drawn foreign shares should be normally distributed. If our within course-professor variation is compatible with random variation, it should also be normally distributed. Panel (c) formally tests whether our within course-professor variation is normally distributed using the Shapiro-Wilk test for normality. Panel (d) provides the same test for the randomly drawn residuals.

Figure 4: Local Linear Regression Results

Comparative Advantage



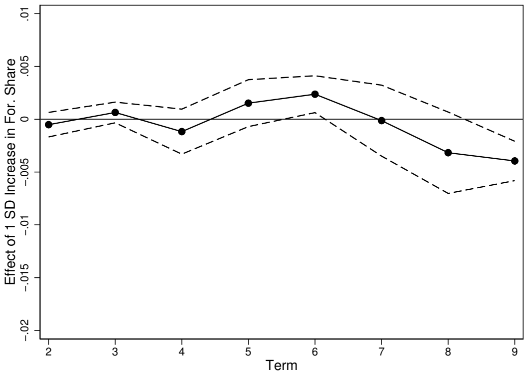
Absolute Advantage

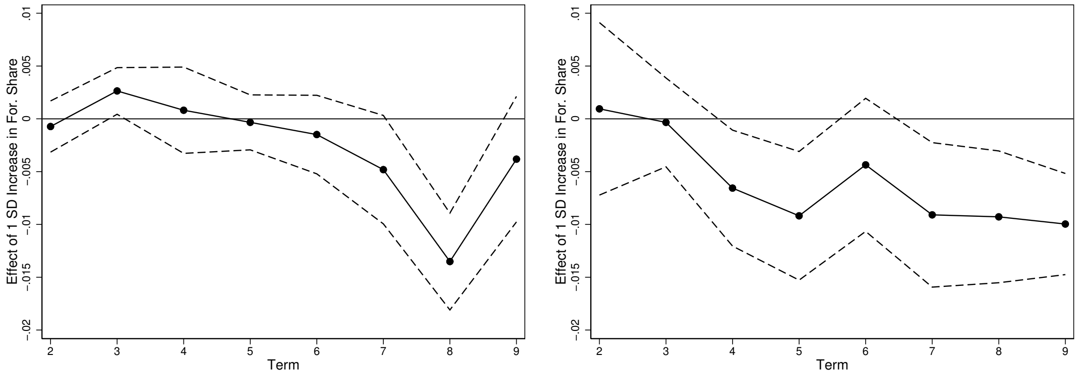


Note: Results show coefficient estimates from local linear regressions of Equation 1 with STEM graduation as the outcome. Domestic first-term freshmen in our core sample are ranked from 1 to 16,828 based on a measure of comparative advantage (top) and absolute advantage (bottom). Lower percentile represents lower inclination towards STEM. Each graph plots 99 estimates from local linear regression centered at each percentile using Epanechnikov kernel weighting. Confidence intervals (dashed lines) derived from 5th and 95th percentile of 250 bootstrapped estimations, resampled at the course level. See text for details on calculation of comparative and absolute advantage. Red line shows mean effect from Column 6 of Table 5

Figure 5: Effect of Intro Math Foreign Share on Foreign Share in Future Courses

(a) All Courses (All Majors)




(b) Non-STEM Courses (STEM Majors)

(c) STEM Courses (Non-STEM Majors)

Note: Results show coefficient estimates from regressions of Equation 1 with the outcome being the share of foreign classmates in all classes in each term after the first. Figure (a) shows effect on future exposure in all classes. Figure (b) limits the sample to students reporting a STEM major at the end of their first-term, and shows the effects on future exposure in Non-STEM courses only. Figure (c) limits the sample to students reporting a Non-STEM major at the end of their first-term, and shows the effects on future exposure in STEM courses only. 95% confidence intervals are provided for reference. We show results up to 12 terms out, which represents 4 years, as many students graduate and leave the sample after 4 years.

Table 1: List of Math Courses

	Domestic First-Term Freshmen	Total Domestic Students	Total Foreign Students	Avg. Percent Foreign	Avg. Class Size	Total Classes
Precalculus	1,838	2,307	246	9.5	289	15
				(2.7)	(96)	
Calculus I	7,031	8,851	1,046	10.3	246	52
				(2.9)	(84)	
Calculus I (Advanced)	4,965	5,377	990	15.4	207	35
				(3.7)	(55)	
Calculus II	392	2,379	300	10.0	198	18
				(3.7)	(61)	
Calculus II (Advanced)	922	1,553	351	17.5	151	14
				(4.2)	(33)	
Calculus III	54	1,376	190	10.6	210	11
				(3.6)	(64)	
Calculus III (Advanced)	299	1,516	316	15.2	155	15
				(4.3)	(42)	
Calculus IV (Advanced)	40	1,016	199	13.8	142	12
				(5.7)	(46)	
Total/Average	16,828	25,701	3,810	12.3	217	179
				(4.4)	(79)	

Note: Table displays the list of introductory mathematics courses offered by the university under analysis. Advanced courses cover similar material to non-advanced ones, but with greater depth. Sample includes 16,828 freshmen domestic students enrolling in introductory math courses in their first term of college attendance. Standard deviations in parentheses where applicable.

Table 2: Background Summary Statistics

	All Students		Intro Math Sample	
	(1) Domestic	(2) Foreign	(3) Domestic First-Term Freshmen	(4) Foreign Classmates
Female	0.56	0.53	0.50	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
White	0.47	0.16	0.41	0.12
	(0.48)	(0.33)	(0.48)	(0.30)
Asian	0.37	0.71	0.48	0.77
	(0.46)	(0.43)	(0.49)	(0.40)
Minority	0.15	0.13	0.10	0.11
	(0.35)	(0.32)	(0.29)	(0.30)
Black	0.03	0.02	0.02	0.02
	(0.17)	(0.13)	(0.13)	(0.13)
Latino	0.12	0.11	0.08	0.09
	(0.31)	(0.29)	(0.27)	(0.28)
HS GPA	3.70	3.70	3.76	3.72
	(0.30)	(0.26)	(0.33)	(0.30)
SAT Math	599.45	599.62	629.36	617.90
	(74.99)	(76.40)	(71.91)	(87.14)
SAT Verbal	562.90	510.62	573.84	491.14
	(79.63)	(90.42)	(84.49)	(104.15)
SAT	1160.18	1105.63	1200.35	1099.65
	(136.34)	(138.31)	(135.62)	(162.88)
Composite Adm. Score	7394.68	7429.27	7510.24	7390.66
	(758.58)	(715.93)	(831.34)	(911.75)
Obs	45,293	7,165	16,828	3,810

Note: Means for enrolled students from fall 2000-fall 2006. Standard deviations in parentheses. Column 3 refers to our analysis sample of 16,828 first-term domestic freshmen. Composite Admission Score is calculated by the admissions office using a weighted sum of various background ability and traits, which includes some measures available in our data and also other ability measures that are not available.

Table 3: Outcome Summary Statistics for Introductory Math Sample

	(1) Domestic First-Term Freshmen	(2) Foreign Classmates
Panel A: Academic Outcomes:		
Graduate	0.82	0.78
	(0.38)	(0.42)
Dropout	0.18	0.22
	(0.38)	(0.42)
Time to Degree (terms)	16.41	16.37
	(2.48)	(3.10)
Graduation GPA	3.05	2.96
	(0.44)	(0.45)
Graduate STEM	0.48	0.44
	(0.50)	(0.50)
Graduate SS	0.27	0.27
	(0.44)	(0.44)
Graduate AH	0.08	0.06
	(0.26)	(0.24)
Panel B: Labor Market Outcomes:		
Exp. Earn. at Graduation	23,231	23,768
	(13,793)	(15,462)
Exp. Earn. 6yrs after Grad.	38,552	38,765
	(20,617)	(22,968)
Exp. Earn. 15yrs after Grad.	49,729	50,154
	(26,642)	(29,832)
Exp. Earn. 30yrs after Grad.	52,760	52,905
	(30,907)	(34,071)
Occ. Based Personal Income	63,081	62,695
	(21,990)	(21,672)
Occ. Based Family Income	140,766	145,298
	(89,897)	(98,488)
Frac. w/ Occupation Linked	0.74	0.66
	(0.44)	(0.47)
Frac. w/ STEM Occupation	0.44	0.51
	(0.50)	(0.50)
Obs	16,828	3,810

Note: Table reports summary statistics for various outcome measures. Column 1 refers to our analysis sample of 16,828 domestic first-term freshmen. Column 2 refers to the foreign classmates of domestic freshmen enrolling in introductory math courses in their first term of college attendance. All earnings figures reported in thousands. Exp. Earn. refers to expected earnings based on a student's major according to data from the Hamilton Project. Occupation-based measures come from observation-specific occupational matching described in Appendix C.

Table 4a: Exogeneity Checks: Selection on Observables

	(1) Female	(2) White	(3) Asian	(4) Latino	(5) Black	(6) Minority	(7) SAT Math	(8) SAT Verbal	(9) High School GPA	(10) Composite Admission Score
Panel I: Domestic Students:										
Foreign Share	-0.01	-0.01	0.02	-0.00	-0.00	-0.01	1.95	-1.20	0.01	22.15
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(1.78)	(2.10)	(0.01)	(15.96)
Mean(Y)	0.51	0.42	0.46	0.10	0.02	0.12	618.93	568.08	3.74	7,443.55
Obs.	25,701	25,701	25,701	25,701	25,701	25,701	25,701	25,701	25,701	25,701
R-sq	0.11	0.02	0.02	0.01	0.01	0.02	0.15	0.03	0.03	0.29
Panel II: Foreign Students:										
Foreign Share	-0.00	-0.01	0.01	0.01	-0.01	0.00	2.98	-13.45***	-0.02	-46.58
	(0.04)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(4.64)	(4.09)	(0.02)	(30.78)
Mean(Y)	0.51	0.12	0.78	0.09	0.02	0.10	619.00	490.02	3.72	7,439.49
Obs.	3,810	3,810	3,810	3,810	3,810	3,810	3,810	3,810	3,810	3,810
R-sq	0.12	0.05	0.06	0.08	0.04	0.07	0.19	0.05	0.05	0.40

Note: The table displays estimates from equation 2. Regressions include controls for course-by-term and course-by-professor fixed effects. Outcome variables across the columns are individual background characteristics. Panel A reports results for all domestic students in introductory math courses. Panel B reports results for all foreign students in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 4b: Exogeneity Checks: Class-Level Correlations

	(1) Female Share	(2) White Share	(3) Asian Share	(4) Latino Share	(5) Black Share	(6) Avg. Composite Admission Score	(7) Class Size	(8) Time of Day	(9) Non-Freshmen Foreign Share	(10) Nationality Herfindahl Index
Foreign Share	-0.01	-0.03	0.03	0.00	-0.00	17.54	41.45	-0.37	0.01	-0.09
	(0.02)	(0.02)	(0.02)	(0.01)	(0.00)	(34.45)	(25.90)	(0.74)	(0.01)	(0.11)
Mean(Y)	0.47	0.38	0.49	0.10	0.02	7,432.52	173.36	5.26	0.05	0.08
Obs.	179	179	179	179	179	179	179	173	179	179
R-sq	0.99	0.94	0.96	0.88	0.90	0.99	0.94	0.94	0.99	0.77

Note: The table displays estimates from equation 3. Regressions include controls for course-by-term and course-by-professor fixed effects. Data is collapsed to the class-level and outcome variables across the columns are class average background characteristics. The foreign share is standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 5: Effects on STEM Graduation

	Alternative Variation			Within Course-Professor Variation		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Domestic First-Term Freshmen						
Foreign Share	-0.023***	-0.028**	-0.071	-0.049***	-0.050***	-0.047***
	(0.007)	(0.012)	(0.121)	(0.014)	(0.015)	(0.014)
Mean(Y)	0.48	0.48	0.48	0.48	0.48	0.48
R-sq	0.05	0.05	0.06	0.05	0.05	0.10
Obs.	16,828	16,828	16,828	16,828	16,828	16,828
Identifying Obs. (Students)	13,498	9,241	1,132	7,079	7,079	7,079
Identifying Classes	129	74	12	52	52	52
Identifying Instructors	43	25	5	18	18	18
Panel B: Foreign Students:						
Foreign Share	0.009	0.040	1.608***	-0.002	0.023	-0.020
	(0.013)	(0.030)	(0.566)	(0.035)	(0.039)	(0.024)
Mean(Y)	0.45	0.45	0.45	0.45	0.45	0.45
R-sq	0.08	0.09	0.11	0.09	0.09	0.15
Obs.	3,810	3,810	3,810	3,810	3,810	3,810
Identifying Obs. (Students)	2,880	1,698	236	1,164	1,164	1,164
Identifying Classes	129	74	12	52	52	52
Identifying Instructors	43	25	5	18	18	18
Fixed effects:						
Course FE	x					
Prof FE	x					
Term FE	x	x				
CourseXTerm FE				x	x	x
CourseXProf FE		x		x	x	x
CourseXProfXTerm FE			x			
Controls:						
Peer Ability	x	x	x	x	x	x
Peer Chars	x	x	x	x	x	x
Class Size					x	x
Ind. Controls						x

Note: The table displays estimates from equation 1. Regressions include controls for course-by-term and course-by-professor fixed effects. Panel A reports results for all domestic first-term freshmen in introductory math courses. Panel B reports results for all foreign students in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Peer ability includes average standardized SAT Math, SAT Verbal, and high school GPA of peers. Peer Characteristics include share of students from each race and share of females. Individual controls include a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA. Standard errors in parentheses are clustered by professor. In Appendix Table A2 we replicate Panel A, columns 4, 5 and 6 and show coefficients for all the included controls. Significance levels: *0.10, ** 0.05, ***0.01.

Table 6: Robustness Checks for Domestic First-Term Freshmen

	(1) Baseline	(2) Control for Foreign Share in Other Courses	(3) Control for AM/PM Dummy	(4) Calculus I Only	(5) Use 1st-Term Freshmen Foreign Share	(6) Control for Nationality Herfindahl Index
Foreign Share	-0.047***	-0.048***	-0.033***	-0.037*		-0.039**
	(0.014)	(0.014)	(0.010)	(0.019)		(0.016)
1st-Term Freshmen Foreign Share					-0.037	
					(0.029)	
Mean(Y)	0.48	0.48	0.48	0.42	0.48	0.48
Obs.	16,828	16,828	16,636	7,031	16,828	16,828
R-sq	0.10	0.11	0.10	0.07	0.10	0.10

Note: The table displays estimates from equation 1. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 7: Effects on Graduation Outcomes and Expected Earnings

	(1) Grad STEM	(2) Grad SS	(3) Grad AH	(4) Dropout	(5) Earn 0	(6) Earn 6	(7) Earn 11-15	(8) Earn 26-30
Foreign Share	-0.047***	0.041**	0.0100*	-0.0058	0.036	0.031	0.031	0.028
	(0.014)	(0.016)	(0.0056)	(0.012)	(0.089)	(0.087)	(0.086)	(0.083)
Mean(Y)	0.48	0.27	0.076	0.18	8.83	9.39	9.65	9.72
Obs.	16,828	16,828	16,828	16,828	16,828	16,828	16,828	16,511
R-sq	0.10	0.06	0.03	0.06	0.06	0.06	0.06	0.06

Note: The table displays estimates from equation 1. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Expected earning in columns 5-8 have been assigned to each student based on their graduation major. Earnings estimates come from calculations done by the Brookings' Hamilton Project and refer to median earnings calculated on U.S. Census Bureau's American Community Survey data at different years after college graduation. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 8: Effects on Stem Careers and Occupation-Based Expected Earnings

	(1) Matched=1	(2) Stem Occ=1	(3) Ind Inc	(4) Fam Inc	(5) Wage
Foreign Share	0.0034	-0.026***	-0.0042	-0.012	-0.0060
	(0.0097)	(0.0076)	(0.0098)	(0.012)	(0.0095)
Mean(Y)	0.75	0.43	11.0	11.8	10.9
Obs.	16,828	12,482	12,482	12,482	12,482
R-sq	0.41	0.03	0.02	0.01	0.02

Note: The table displays estimates from equation 1. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Expected income and earnings in columns 3-5 have been assigned to each student based on their observed occupation title. Expected earnings and income are estimated for each SOC occupation code using most recent American Community Survey data on a sample that mimics the characteristics of individuals in our administrative data. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 9: Effects on Different Domestic Groups

	(1) Female	(2) Male	(3) White	(4) Asian	(5) Minority	(6) First Major STEM	(7) First Major Not STEM
Grad STEM	-0.02	-0.07***	-0.04**	-0.09***	-0.01	-0.04***	-0.04***
	(0.02)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)
Grad SS	-0.00	0.09***	0.03	0.06***	0.02	0.03***	0.05*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.03)
Grad AH	0.02**	-0.00	0.01	0.01*	0.05***	0.01*	0.01
	(0.01)	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)	(0.03)
No Grad	0.00	-0.02	-0.01	0.02***	-0.06*	-0.00	-0.02
	(0.01)	(0.02)	(0.02)	(0.01)	(0.04)	(0.01)	(0.02)
Earn 0	-0.02	0.12	0.07	-0.17**	0.50*	0.02	0.12
	(0.06)	(0.16)	(0.12)	(0.07)	(0.28)	(0.09)	(0.14)
Earn 6	-0.02	0.10	0.06	-0.17***	0.49*	0.02	0.10
	(0.06)	(0.16)	(0.12)	(0.06)	(0.28)	(0.09)	(0.14)
Earn 11-15	-0.02	0.10	0.06	-0.17***	0.49*	0.02	0.10
	(0.06)	(0.15)	(0.12)	(0.06)	(0.28)	(0.09)	(0.14)
Earn 26-30	-0.03	0.10	0.06	-0.16***	0.46*	0.02	0.09
	(0.06)	(0.15)	(0.12)	(0.06)	(0.25)	(0.09)	(0.13)
Obs.	8,155	8,356	6,343	7,533	1,585	12,419	4,092
R-sq	0.07	0.07	0.05	0.07	0.17	0.07	0.08

Note: The table displays estimates from equation 1. Outcomes for different subgroups are reported across the columns. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 10: Effects on Performance in Introductory Math Courses

	(1) All	(2) Female	(3) Male	(4) White	(5) Asian	(6) Minority
Panel A: Drop Course						
Foreign Share	0.02	0.04**	-0.01	0.03*	0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.05)
Mean(Y)	0.11	0.12	0.11	0.09	0.12	0.17
Obs.	16,828	8,353	8,475	6,482	7,666	1,606
R-sq	0.05	0.06	0.07	0.05	0.08	0.14
Panel B: Std. Grade						
Foreign Share	-0.01	0.00	-0.02	-0.10***	0.03*	0.16
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.10)
Mean(Y)	0.17	0.21	0.12	0.21	0.19	-0.10
Obs.	14,799	7,284	7,515	5,865	6,671	1,328
R-sq	0.22	0.24	0.21	0.24	0.21	0.29
Panel C: Top 50%						
Foreign Share	-0.02	-0.02	-0.02	-0.08***	0.01	0.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Mean(Y)	0.67	0.70	0.65	0.69	0.68	0.57
Obs.	14,799	7,284	7,515	5,865	6,671	1,328
R-sq	0.17	0.19	0.17	0.19	0.17	0.27
Panel D: Switch Out STEM						
Foreign Share	0.04***	-0.00	0.07***	0.06***	0.05***	0.04
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.04)
Mean(Y)	0.42	0.42	0.42	0.41	0.40	0.54
Obs.	12,536	5,889	6,647	4,854	5,673	1,226
R-sq	0.10	0.11	0.11	0.10	0.11	0.22
Panel E: Time to Final Major Declaration						
Foreign Share	0.10	-0.15	0.32*	0.23	0.22	0.14
	(0.12)	(0.11)	(0.19)	(0.18)	(0.17)	(0.38)
Mean(Y)	5.05	5.24	4.86	4.89	5.18	5.00
Obs.	16,828	8,353	8,475	6,482	7,666	1,606
R-sq	0.04	0.04	0.05	0.05	0.06	0.09

Note: The table displays estimates from equation 1. Outcomes for different subgroups are reported across the columns. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. Different outcomes are reported in the panels. Panel A examines whether an individual withdrew from the class. Panel B examines the standardized grade earned, conditional on staying in the class. Panel C examines the likelihood of earning a grade above the median. Panel D examines whether individuals ever made a switch from STEM to non-STEM. Panel E examines the time, in terms, to declare one's final major. The foreign share is standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 11: Testing the Communication Mechanism - Foreign Classmates' Fluency

	(1) Grad STEM	(2) Grad SS	(3) Grad AH	(4) Dropout	(5) Std. Grade	(6) Above 50th Pctile
Foreign Sh. Low Fluency	-0.053**	0.060***	0.016***	-0.023	-0.008	-0.032
	(0.023)	(0.018)	(0.006)	(0.015)	(0.025)	(0.020)
Foreign Sh. High Fluency	-0.006	-0.008	-0.003	0.015	-0.000	-0.011
	(0.023)	(0.030)	(0.005)	(0.015)	(0.028)	(0.027)
Mean(Y)	0.48	0.27	0.08	0.18	0.17	0.60
Obs.	16,828	16,828	16,828	16,828	14,799	16,828
R-sq	0.10	0.06	0.03	0.06	0.22	0.17

Note: The table displays estimates from equation 1, where the foreign share is separately calculated for those with above median SAT verbal scores (i.e. "High Fluency") and those with below median SAT verbal scores (i.e., "Low Fluency"). Different outcomes are reported across the columns. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. The foreign shares are standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, ** 0.05, ***0.01.

Table 12: Comparative Advantage Mechanism

	(1) Class STEM Comp. Adv. (CSCA)	(2) Grad STEM	(3) Grad STEM
Foreign Share	-0.063***		-0.039***
	(0.023)		(0.013)
CSCA Quartile=2		0.026*	0.015
		(0.014)	(0.015)
CSCA Quartile=3		0.037*	0.021
		(0.019)	(0.020)
CSCA Quartile=4		0.055**	0.026
		(0.026)	(0.027)
CSCA Quartile=2 × Foreign Share			-0.002
			(0.011)
CSCA Quartile=3 × Foreign Share			-0.013
			(0.011)
CSCA Quartile=4 × Foreign Share			-0.017
			(0.013)
Mean(Y)	-0.02	0.48	0.48
Obs.	16,828	16,820	16,828
R-sq	0.99	0.10	0.11
Controls:			
HS GPA	x	x	x
SAT Math and Verbal		x	x
STEM Cohort Comp Adv	x	x	x
Peer Chars.	x	x	x
Class Size	x	x	x
Ind. Controls	x	x	x

Note: The table displays estimates from equation 1. In column 1 the dependent variable is a measure of within-class comparative advantage in STEM. In columns 2 and 3 we replicate our main specification separately for students who had a drop in the 2nd, 3rd, and 4th quartiles of the within-class comparative STEM advantage measure (with respect to her/his own university-level comparative advantage), with the 1st quartile being the omitted category. Regressions include controls for course-by-term and course-by-professor fixed effects, peer ability (i.e., average standardized SAT Math, SAT Verbal, and high school GPA of classmates), peer characteristics (i.e., share of students from each race and share of females), class size, and individual controls (i.e., a female indicator, race dummies, SAT Math and Verbal scores, and high school GPA). Results are for domestic first-term freshmen in introductory math courses. The foreign share is standardized to have mean 0 and standard deviation 1. Standard errors in parentheses are clustered by professor. Significance levels: *0.10, **0.05 ***0.01.