# A community college instructor like me: Race and ethnicity interactions in the classroom[1]

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**Abstract:**

Detailed administrative data from a large and diverse community college are used to examine whether academic performance depends on whether students are the same race or ethnicity as their instructors. To identify racial interactions and address many threats to internal validity we estimate models that include both student and classroom fixed effects. Given the large sample sizes and computational complexity of the 2-way fixed effects model we rely on numerical algorithms that exploit the particular structure of the model's normal equations. Although we find no evidence of endogenous sorting, we further limit potential biases from sorting by focusing on students with restricted course enrollment options due to low registration priorities, and on courses with no within-term or within-year racial variation in instructors. We find that the performance gap in terms of class dropout and pass rates between white and underrepresented minority students falls by roughly half when taught by an underrepresented minority instructor. The racial performance gap in grades is also lower with minority instructors. Results from detailed racial interactions indicate that African-American students perform particularly better when taught by African-American instructors.

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## 1. Introduction

The achievement gap between historically underrepresented minority students and non-minority students is one of the most persistent and vexing problems of the educational system in the United States.[2] African-American, Latino and Native-American students have substantially lower test scores, grades, high school completion rates, college attendance rates, and college graduation rates than non-minority students.[3] Recent research by Fryer and Levitt (2006) and Fryer (2011) documents that, for African Americans, achievement gaps start to appear in elementary school and persist throughout primary and secondary education. In addition, the empirical evidence presented by Arcidiacono et al. (2011) suggests that similar gaps exist at highly selective post-secondary institutions. Ultimately these gaps translate into substantially lower completion rates for African-Americans and Latinos compared to non-minorities. A major concern is that, in spite of substantial publicity and some affirmative action, the gap has not shrunk over the last two decades, which contrasts sharply with trends in other educational disparities such as the gender gap.[4] Such persistent disparities in educational attainment may have major implications for income and wealth inequality across racial and ethnic groups.[5] It is therefore imperative to study the sources of the minority achievement gap and to evaluate the effectiveness of potential policy interventions.

A common, though hotly debated, policy prescription is to expand the representation of minority instructors at all levels of the educational system. Indeed, there is a general lack of minority teachers, especially at the post-secondary level: only 9.6 percent of all full-time instructional faculty at U.S. colleges are black, Latino or Native American, while these groups

[2] In the following we use "underrepresented minority" and "minority" interchangeably. This group includes African-Americans, Hispanics, and Native Americans, which is the common definition used for "underrepresented minority" in California public higher education.
[3] See U.S. Department of Education (2010).
[4] See e.g. Fryer and Levitt (2006).
[5] Such arguments are made in e.g. Altonji and Blank (1999), Card (1999), and Jencks and Phillips (1998).

comprise one-third of the college-age population and an even higher percentage of children.[6] As argued by many social scientists, this imposes severe limits on the availability of role models, increases the likelihood of "stereotype threats" and discrimination against minority students, and restricts exposure to instructors with similar cultures and languages.

In this paper we offer the first systematic empirical study of minority interactions between students and instructors at the post-secondary education level. More specifically, we test whether minority students experience significant achievement gains from being taught by a minority professor. We also investigate if student-instructor interactions in the classroom exist on finer levels of racial or ethnic identity. These questions are examined using a novel and unique administrative dataset with detailed demographic information on instructors as well as students from a large and ethnically diverse community college located in the San Francisco Bay Area. Our study is also the first to shift the focus from selective 4-year colleges to the community college system. The lack of previous research using data from community colleges is somewhat surprising given that they enroll nearly half of all students attending public universities. Since community colleges, in addition to providing workforce training, serve as an important gateway to 4-year colleges, they can be seen as a crucial part of the post-secondary educational system in the United States. In fact, in some states, including California, nearly half of all students attending a 4-year college previously attended a community college.[7] With recent calls for major expansions in enrollments and provision of 4-year transfer courses, one can expect that community colleges will gain further importance.[8] Policy interventions targeting community colleges are therefore likely to have major effects on the educational system as a whole.

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[6] See U.S. Department of Education (2010).

[7] See U.S. Department of Education (2010); CCCCO (2009); Sengupta and Jepsen (2006).

[8] For example, President Obama has proposed an unprecedented funding increase for community colleges that aims to boost graduates by 5 million students by 2020. In California, transfers from community colleges to the California State University (CSU) system are projected to increase by 25 percent over the next decade (California Postsecondary Education Commission 2010).

The richness of our data allows us to address many challenges faced in estimating student-instructor interactions at the post-secondary level. By matching any student enrolled over a five-year period to their instructors in every course, we can flexibly model such interactions and overcome many of the major threats to the internal validity of our estimates. To further increase internal validity, we exploit a number of institutional features at our community college to generate samples of students in which the incidence of endogenous sorting of students to instructors is minimized. Another unique feature of our data is that we can observe whether a course is vocational or academic, and whether it is transferable to the UC and CSU systems, allowing us to explore the external validity of our results.

It is well known that random assignment of students to classes does not occur at community colleges or 4-year universities outside of the military post-secondary educational system.[9] We therefore employ several novel empirical strategies to rule out the possibility that the estimates are driven by omitted variable biases, to explore the external validity of our results, and to investigate the channels through which our estimated reduced-form effects operate. Our basic empirical approach is built on a regression model in which the parameter of interest is the ***differential effect*** between minority and non-minority students of being assigned to a minority-instructor in the same class. This answers the question of whether the minority achievement gap is smaller in classes that are taught by minority instructors and is answered affirmatively if minority students experience gains relative to non-minority students from being taught by minority instructors. The focus on estimation of these interaction effects from panel data such as ours permits tremendous flexibility in the types of specifications one can estimate. In particular, the explanatory variable of interest varies both within student and within classroom, allowing us to estimate models that simultaneously include student and classroom fixed effects. This eliminates biases coming from student specific differences common across courses and classroom

[9] Random assignment takes place at the U.S. Air Force Academy that provides undergraduate education for officers in the U.S. Air Force.

specific differences common across classmates.[10] Including classroom fixed effects leads to standardizing grade outcomes, since we are only using within-classroom differences among students who complete the same assignments, take the same exams, and are subject to the same grading policies. Furthermore, our two-way fixed effects specification with individual and class fixed effects controls for the possibility that minority and non-minority students enroll in courses or subjects with more lenient grading policies. Given the sample size – we observe over 30,000 students in nearly 21,000 classes – estimation of this model by conventional algorithms is computationally infeasible. To address this problem, we conduct the first application of an algorithm that has been applied to the estimation of firm and worker fixed effects with large administrative data to the estimation of student and teacher fixed effects.[11]

While our empirical model addresses many of the potential threats to internal validity, we cannot directly control for differential sorting across minority student groups that may arise if, for example, highly motivated minority students systematically sort into minority-taught classes while highly motivated non-minority students do not. We argue that with an appropriate set of observable variables that is highly correlated with unobserved student abilities, such as a student's past academic performance, the hypothesis of differential sorting is testable. Implementation of such a test using a rich set of observables does not uncover any evidence of differential sorting. Even so, we explore the robustness of our results by constructing subsamples in which any remaining concerns regarding biases generated by endogenous student sorting are mitigated. First, we take advantage of the registration priority system at the community college and focus on students with limited class enrollment choices. Given the intense competition for classes created by negligible tuition, absence of admissions requirements, and desirable location, this system is strictly enforced. As a consequence, students with the lowest registration priority

[10] Here and in the following we use the term "class" or "classroom" to refer to a particular offering or section of a course with a specific instructor during some term, such as "Principle of Microeconomics: ECON-100". Hence, a "class" or "classroom" is uniquely defined by course title, section, and term.
[11] See for example Abowd, Kramarz, and Margolis (1999) and Abowd, Creecy, and Kramarz (2002).

status have severely restricted class enrollment choices and are thus arguably exogenously assigned. Second, we restrict the variation in instructor race and ethnicity across classes in the same term or academic year used to identify our parameter of interest. More specifically, by estimating our model from a sample of courses in which students have no choice over instructor's race within a term or even academic year we can rule out the possibility of sorting within that term or year by construction.

We find that the minority achievement gap is smaller in classes taken with minority instructors for several course outcome measures. While underrepresented minority students are overall more likely to drop a course, less likely to pass a course, and less likely to have a grade of at least a B, these gaps decrease by 2.9 percentage points, 2.8 percentage points, and 3.2 percentage points respectively when assigned to an underrepresented minority instructor. These effects are large, representing roughly half of the achievement gap between non-minority and minority students in the data. We also find them to be equally large when focusing on vocational courses or academic courses, and when restricting the sample to courses that are transferable or non-transferable to universities in the UC- and CSU-systems (providing some evidence on the external validity of our results). Results from detailed racial interactions indicate that African-American students experience particularly large gains from being taught by instructors of the same race. We also find evidence allowing us to isolate racial and ethnic interaction effects due to role-model effects rather than instructor-driven discrimination effects.

Our paper is related to a number of studies, most notably Dee (2004, 2005, 2007) and Ehrenberg, Goldhaber and Brewer (1995), that use data from the elementary and 8$^{th}$ grade educational levels to estimate race and ethnicity interactions between students and teachers. They find some evidence of positive student-teacher interactions by race and gender. Our paper is also related to a small, but growing literature that focuses on gender interactions between students and instructors at the post-secondary level. Similar to our work, these studies rely increasingly on high-quality administrative student panel data that can be matched to instructor-level data. They tend to conclude that female students perform relatively better when matched to female instructors (e.g. Bettinger and Long 2005; Hoffmann and Oreopoulos 2009).[12] A recent study by Carrell, Page, and West (2010), which takes advantage of the random assignment of students to classrooms at the U.S. Air Force Academy, also finds that female students perform better in math and science courses with female instructors. None of these previous studies, however, examine the impact of instructor's minority status, race, or ethnicity on student outcomes on the post-secondary education level, due to not being able to obtain race information on instructors and the lack of underrepresented minority faculty at more selective colleges.[13] This might be an important omission in the literature, however, because the effects of minority faculty on minority students may be larger due to the sizeable racial achievement gap and similarities in culture, language and economic backgrounds. In addition, measures of racial inequality in education, income and other outcomes have not decreased over the last two decades, in sharp contrast to corresponding measures of gender inequality.

The rest of the paper proceeds as follows: Section 2 starts by providing some institutional background, and then describes and summarizes the data. In the next section we introduce our econometric framework. Section 4 presents evidence on student sorting and the main results on racial interactions in educational outcomes. The final section concludes.

## 2. Data

### *2.1 Institutional Background*

Our analysis is based on administrative data from De Anza College, a large community college that is part of the California Community College system and is located in the San

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[12] A larger literature studies gender interactions at the primary or secondary school level. The findings are generally mixed (see for example, Nixon and Robinson 1999, Ehrenberg, Goldhaber, and Brewer 1995, Dee 2007, Holmlund and Sund 2005, Carrington and Tymms 2005, 2007, Lahelma 2000, and Lavy and Schlosser 2007).

[13] For example, the U.S. Air Force Academy has roughly 35 disadvantaged minority instructors making it difficult to study racial and ethnic interactions.

Francisco Bay Area.[14] De Anza College has an average total enrolment of 22,000 students per year. It has a larger share of minority students than the nationally representative community college, reflecting the diversity of Northern California. The College is on the quarter system, and the majority of classes are restricted to 50 or fewer students. The tuition at De Anza College is $17 per unit (roughly $850 per year in tuition and fees) with a large percentage of students receiving fee waivers because of financial need. Similar to all community colleges in California it has open enrolment – anyone with a high school diploma or equivalent is automatically admitted.

**Registration Priority System**

Open enrolment, very low tuition costs, mandated small class sizes, and its location in the San Francisco Bay Area create intense competition for courses at De Anza College. Because of the general excess demand for courses, the College has established a strictly enforced registration priority system which determines the day on which students are allowed to register over an eight-day period. Registration priority is determined by whether the student is new, returning or continuing, the number of cumulative units earned at De Anza College, and enrollment in special programs.[15] It does not depend on past academic performance. Incoming students and students who have taken a break away from the college have the lowest priority status. Priority status improves for continuing students by cumulative unit blocks. For example, continuing students with less than 11 cumulative units register on day 7 and continuing students with 11-29.5 cumulative units register on Day 6.

[14] The California Community College system is the largest higher educational system in the United States. It includes 110 colleges and educates 2.9 million students per year.

[15] We remove students enrolled in special and often minority-student focused programs, such as SLAM, STARS, and SSRC. These students receive special registration priority status even if they are new or returning students.

A student's registration priority has a large impact on his or her choice of classes.[16] Analysing detailed wait list data for all courses, we find substantially higher probabilities of being placed on wait lists among low-registration priority students compared to students with higher registration priorities suggesting similar patterns for not being able to enrol in desired courses. Conversations with college administrators also revealed that students with a low ranking on course-priority lists have severely limited choices in instructors. As a consequence, for a particular course that has multiple class offerings in a quarter, these students have little control over to which instructor they are matched. We corroborate this anecdotal evidence by performing numerous tests for non-random sorting, all of which reject the hypothesis that students systematically sort into classes taught by an instructor who shares their race or ethnicity. We thus focus our analysis on a sample of students who have the lowest possible standing on class enrolment priority lists. To explore the robustness of our results and to exploit the large size of the full dataset, we also show results for regressions estimated on the unrestricted sample.

### *2.2 Data Set and Summary Statistics*

Our data record class grades, course credits, course dropout behaviour, and detailed demographic characteristics for all students registered at and all classes offered by De Anza College over a five-year period (between the Fall quarter of 2002 and the Spring quarter of 2007). We are able to match these data to detailed data on demographic characteristics of instructors such as race, ethnicity, age, and gender. To our knowledge, this is the first dataset that matches detailed information about instructors' race to student class outcomes on the post-secondary education level. We also observe students' registration priority at the beginning of each quarter and an indicator variable for whether they are an entering student. One further major advantage of

[16] In personal conversations with college administrators we have learned that students often register for classes as soon as they are allowed to through the system because of the intense competition for courses.

this dataset is that it allows us to match students to classes that students enrolled in before their first day of the term, regardless of whether they completed the class or not.

We exclude recreational courses, such as cooking, sports and photography, orientation courses, and summer courses from our analysis. The remaining sample is referred to as the "Unrestricted Sample" below. In our main analysis we also exclude courses that have an average enrolment per session of less than 15 students to minimize computation without losing identification power. To remove concerns about local community residents taking classes for recreational purposes and to focus on the general college-age population, we include only students who are at most 35 years old in the main sample. However, in one specification we explore age-heterogeneity of our estimated parameters of interest and thus remove this age restriction.

Roughly 6 percent of courses offered at De Anza College are video-delivered or online courses, which would be a concern for the analysis if students enrolled in these types of courses never see or interact with the instructor. However, these courses are video delivered through online video-streaming, local cable TV, or DVD, are combined with compulsory weekly tutorials, and require participation in an orientation session that is presented by the instructor. Hence, if role model effects are driving our results, inclusion of these courses should not significantly alter our results. We therefore include video-delivered courses in the main sample and test the robustness of the estimates to dropping them in a later section.

We consider four outcome variables: an indicator for whether a student drops out of a class at any time during the term, an indicator variable for whether the class was passed, a numerical grade variable, and an indicator variable for whether the course grade was a B or higher.[17] The last variable is interesting because a GPA equivalent to a letter grade of a B is commonly used as a threshold for qualification for admission to the University of California. In

[17] Students have to drop a class within three weeks to avoid paying for the class and within four weeks to avoid getting a record of a grade.

the regression analysis below we standardize the numerical grade variable to have zero mean and unit variance within each course.

The first panel of Table 1 (Unrestricted Sample) provides summary statistics of interest for the sample before dropping small courses, small departments, and students who are older than 35 years.[18] This sample consists of 506,280 student-course observations. Only 2.4 percent of the student-class observations are for small courses, and 1.2 percent for courses from a small academic department. 9.2 percent of the observations are for older students. The median age of the students who are at most 35 years old is 21.5 years. Because students who are between 21.5 and 35 years old comprise less than 50 percent of the student-class observations, it is clear that the youngest students take more classes on average. Dropping small courses, small departments, and students over age 35 from the sample leaves us with 446,239 student-class observations (our main sample). Ten percent of students are entering students, and 29 percent have low registration priority status when they enrolled in classes in the data. In terms of types of courses in the main sample, we find that only 3 percent of student/class observations are in language courses and 6 percent are in video-delivered classes. We exclude these later in a sensitivity analysis.

A further advantage of our data is that we can identify whether courses are used towards vocational degrees and whether they are transferable to University of California or California State University campuses. On the student-class level, 26 percent are vocational courses, and 70 percent are for courses that are transferable to 4-year California public universities. The latter reflects the reputation of De Anza College of being a more academically oriented community college.

We find that 26 percent of observations are for students who take a course in a quarter-year in which it is taught by only one instructor, even if multiple sections are offered. In this case, student selection on the minority status of the instructor is ruled out by construction. A less severe

[18] We document these statistics since we present the robustness of our results with respect to our main sample restrictions in tables 4 and 5. It is important to keep in mind that we exclude recreational courses, orientation courses, and summer courses from all our samples and sub-samples.

sample restriction with a similar effect is to include course-term or course-year combinations for which different sections are taught by different instructors, all of which share a particular minority status. As shown in the table, sixty-one percent of student/class observations have no variation in underrepresented minority status within quarters and 52 percent of student/class observations have no variation in underrepresented minority status within academic-years.

Panel B of Table 1 shows differences in average student outcomes across students of different races and ethnicities. Sample sizes vary by outcome. Since course grades are available only for students who finish a class while we can observe all classes a student enrolled in at the beginning of the quarter, the sample size is the largest for the "Dropped Course" outcome. Furthermore, some courses only assign a passed/failed outcome, so that the sample size for the variable "Passed Course" is larger than the sample sizes for the two remaining measures based on letter grades.

The fact that we observe grade outcomes only for those who do not drop a class generates a sample selection problem. We address this issue by computing non-parametric bounds below. However, given that students who are induced to not drop a class because it is taught by an instructor of the same minority group are likely to be from the lower end of the ability distribution, we interpret our point estimates as lower bounds for the true minority interactions and focus our analysis on them. This is essentially a monotonicity assumption, and it is testable if one has a variable that is highly correlated with unobserved student abilities. We use prior academic performance to conduct such a test and find no evidence against this hypothesis.

There are important differences in student outcomes across groups. White and Asian students have the highest average outcomes. Hispanics, African-American, and Native American, Pacific Islander and other non-white students are more likely to drop classes, are less likely to pass classes, receive lower average grades, and are less likely to receive a good grade (B or higher). For most outcomes, these differences are large and statistically significant, documenting that the largest differences in academic outcomes take place along the underrepresented minority-non-underrepresented minority margin rather than along less aggregated measures of differences in race and ethnicity. Aggregating up these statistics for the underrepresented minority group (not shown in the table) yields a dropout rate of 26 percent. The average grade is 2.9 (where 4.0 is equivalent to an A), and 66 percent of classes taken by students for letter grades receive a grade of B or higher. Of all underrepresented minority students who finish classes, the total pass rate is 88 percent. These classes include non-letter grades (pass/no pass) as well as letter grades.

Panel C of table 1 displays the racial composition of the student body. There are 31,961 students in the panel. White students comprise 28 percent of all students and Asians comprise 51 percent of students. Hispanic students represent the largest underrepresented minority group with 14 percent of all students. African-American students comprise 4 percent of students and Native American, Pacific Islanders, and other non-white students comprise 3 percent of students. Underrepresented minorities comprise 21 percent of the total student body. Half of all students are female.

The racial distribution of the 942 instructors in the sample (reported in Panel D) differs substantially from the student distribution. Nearly 70 percent of instructors are white. In contrast, only 14 percent of instructors are Asian and 6 percent of instructors are Hispanic. Interestingly, the percentage of African-American instructors and Native American, Pacific Islander and other non-white instructors are slightly higher than their representation in the student body. The lack of minority instructors at De Anza College does not differ from the national pattern for all colleges. Roughly 10 percent of all college instructors are from underrepresented minority groups (U.S. Department of Education 2010). At De Anza College, 16 percent of instructors are from underrepresented minority groups. The lack of minority instructors is perhaps even more surprising given the diversity of the workforce in the San Francisco Bay Area.

## 3. Statistical Methodology

### *3.1 Basic Econometric Model*

We now turn to the description of the econometric models for the student outcome variables, $y_{ijkst}$, such as course grades or course dropout behaviour. We index students by $i$, instructors by $j$, courses by $k$, sections by $s$, and term (i.e. quarter) by $t$. Let $min\_stud_i$ and $min\_inst_j$ be indicator variables that are equal to one if student $i$ and instructor $j$ belong to an underrepresented minority group, respectively, and let $X_{ijkst}$ and $u_{ijkst}$ be vectors of observable and unobservable variables affecting outcomes. To test whether minority students gain from being taught by a minority instructor, a natural starting point is to estimate the regression:

(1) $$y_{ijkst} = \alpha_0 + \alpha_1 * min\_inst_j + X'_{ijkst}\beta + u_{ijkst}.$$

for a sample of minority students only, where $\alpha_1$, is the coefficient of interest. However, this specification does not allow us to estimate whether non-minority students gain from being taught by a minority instructor as well. It is therefore useful to specify an empirical model that is estimated on the full sample. If role-model effects or stereotype threat are important determinants of the minority achievement gap, we expect that the returns of being assigned to a minority instructor differ between minority and non-minority students. In particular, if minority students gain from the interaction with minority instructors relative to non-minority students, these returns are likely to be larger for minorities. We are thus primarily interested in the student-instructor interaction effect, $\alpha_3$, from the regression:[19]

(2) $$\begin{aligned} y_{ijkst} &= \alpha_0 + \alpha_1 * min\_inst_j + \alpha_2 * min\_stud_i \\ &+ \alpha_3 * min\_inst_j * min\_stud_i + X'_{ijkst}\beta + u_{ijkst}. \end{aligned}$$

[19] Interaction effects between students' and instructor's demographic variables have also been the primary parameters of interests in recent papers about gender interactions, such as Hoffmann and Oreopoulos (2009) and Carrell, Page and West (2010).

The parameter of interest is $\alpha_3$ and determines the difference in the minority-instructor effect between minority and non-minority students. It thus measures the extent to which minority ***gaps*** in the outcome variables depend on whether the students are assigned to a minority or a non-minority instructor. As we will see in a moment, the focus on estimation of these interaction effects from panel data permits tremendous flexibility in the type of specification one can estimate and is a crucial part of our empirical strategy.

The parameter, $\alpha_3$, is consistently estimated if $\mathrm{cov}(u_{ijkst}; interact_{ij}) = 0$, where $interact_{ij}$ = $min\_inst_j * min\_stud_i$. Correlations between the interaction term and the unobserved component, however, may be caused by several factors, which have posed difficult challenges for the existing literature on student-professor (or student-teacher) interactions. These factors include (i) the possibility that students enrolled in minority-taught classes differ systematically from students enrolled in non-minority taught classes, possibly due to sorting; (ii) the possibility that grading policies vary by instructor's race; (iii) the possibility that minority students sort into programs in which the fraction of minority instructors is particularly high and that differ in difficulty from other programs; (iv) non-standardized testing across classes within courses; and (v) differential sorting of the type where minority students select on unobserved ability into minority-taught courses while non-minority students do not. Given our data and our focus on the estimation of interaction effects, we can address potential concerns (i) to (iv), but we cannot directly control for differential sorting (v). We will first discuss our empirical strategy for ruling out inconsistencies in our estimates due to any of the points (i) to (iv). We then describe our test for differential sorting and discuss several novel approaches used to mitigate any remaining concerns about omitted variable and sorting biases.

To address the first concern, it is important to note that the minority-interaction varies within individual since students of any racial and ethnic background may take classes that are taught by instructors belonging to different minority groups. Hence, we include individual fixed effects $\gamma_i$

in our regressions, controlling for ***absolute*** sorting that takes place if students taking classes from minority instructors are systematically different from those who do not, irrespective of their minority background. Likewise, we can address the second concern by introducing instructor fixed effects, $\lambda_j$, that eliminate potential threats to the validity of our results due to minority students taking courses from instructors who have systematically different grading policies from other instructors. Similarly, minority students may be drawn to courses in which minority instructors are relatively overrepresented and for which the grade distribution is significantly different from other courses. We address this concern by including minority-student specific course fixed effects, denoted by $\delta_k * min\_stud_i$. Both of the latter two issues are specific examples of classroom level shocks (i.e. factors that are unobserved by the econometrician, that vary on the classroom level, and that affect student performance). It is therefore essential to only compare academic performances of minority and non-minority students who enrol in the same class and are therefore subject to the same class-level shocks, such as an instructor's teaching performance or philosophy, the time of day, or external disruptions. As noted above, a "classroom" or "class" is defined by the specific offering of a course during a term (e.g. ECON-001-01 during Fall 2006) so that it is uniquely defined by the combination of the indices $k$, $s$, and $t$. In our main specification we include classroom fixed effects, $\phi_{kst}$, yielding the following econometric model:

$$\text{(3)} \qquad \begin{aligned} y_{ijkst} = {} & \alpha_0 + \alpha_1 * min\_inst_j + \alpha_2 * min\_stud_i + \alpha_3 * min\_inst_j * min\_stud_i \\ & + \gamma_i + \lambda_j + \delta_k * min\_stud_i + \phi_{kst} + u_{ijkst}. \end{aligned}$$

As specified, however, this model is clearly not identified since $min\_inst_j$ and teacher fixed effects $\lambda_j$ are perfectly multi-collinear, and so are $min\_stud_i$ and student fixed effects $\gamma_i$. Furthermore, since a student can enrol only in one section per course, and since each class is taught by exactly one instructor, neither minority-status specific course fixed effects nor instructor fixed effects can be separately identified from classroom fixed effects. We therefore estimate the following 2-way fixed effects model, where we replace the combination of the indices $k$, $s$, $t$ by a classroom index $c$ and where we index the minority-instructor dummy by $c$ rather than $j$:

(4) $$y_{ic} = \alpha_3 * min\_stud_i * min\_inst_c + \gamma_i + \phi_c + u_{ic}.$$

As discussed above, this specification implicitly controls for the possibility that minority and non-minority students may differ systematically in their choices of subjects or courses which have different grading standards. It is the focus on the interaction term that allows us to identify individual and classroom fixed effects. The latter comes from the fact that each class may be taken by minority and non-minority students, generating variation in the minority interaction within classroom. We should highlight that the inclusion of classroom fixed effects avoids the need to rely on data with standardized testing procedures across classrooms since within the same classroom students are taking exactly the same tests. Unless instructors discriminate against certain groups of students, consciously or not, students within a class are also subject to identical grading criteria. Otherwise our parameter of interest, $\alpha_3$, may be estimated to be significantly different from zero, reflecting the possibility that student-instructor interactions may exist because instructors react to students rather than vice versa. This however may take place even if tests are standardized and if students are randomly assigned to instructors. It is thus not a matter of omitted variable bias, but a matter of interpreting the reduced-form coefficient $\alpha_3$ correctly. However, for counterfactual policy analysis it is crucial to be able to differentiate between instructor- and student-driven interaction effects. In our empirical section we will therefore conduct several tests to investigate which of these hypotheses are more likely to explain our results.

Model (4) is our primary specification and helps identify minority students' relative gain of being assigned to a minority instructor. As noted above, we follow the official definition and include African-Americans, Hispanics, Native Americans, and Pacific Islanders in the underrepresented minority group. As shown in panel B of table 1, the largest achievement gaps can be observed between these aggregated groups, while within-group differences are mostly statistically insignificant. We therefore focus on student-instructor interactions between these two aggregated groups, allowing us to summarize student-instructor interactions in one parameter, similar to the recent literature on gender interactions in the classroom. One may hypothesize, however, that role model effects operate along less aggregated definitions of racial and ethnic groups. We also test whether students indeed gain from having an instructor of the same detailed race or ethnicity. We thus also estimate models in which we include a full set of indicator variables for the four main ethnic groups in the sample – Whites, African-Americans, Hispanics, and Asians. In this case there are 16 racial interactions, 9 of which are identified in the 2-way fixed effects model that includes classroom and student fixed effects.

While our specification addresses many of the potential threats to internal validity, we cannot directly control for differential sorting across minority student groups that may arise due to correlations between the unobserved component $u_{ic}$ and the interaction term. Such correlations exist if for example highly motivated minority students systematically sort into minority-taught classes, while highly motivated non-minority students systematically sort into non-minority-taught classes. In this case the following inequality will apply:

$$\begin{aligned} (5) \qquad & E[u_{ic} \mid min\_stud_i = 1, min\_inst_c = 1] - E[u_{ic} \mid min\_stud_i = 0, min\_inst_c = 1] \\ & \neq E[u_{ic} \mid min\_stud_i = 1, min\_inst_c = 0] - E[u_{ic} \mid min\_stud_i = 0, min\_inst_c = 0] \cdot \end{aligned}$$

The differences on each side of the inequality are “minority gaps” in unobserved components. The inequality can be replaced by an equality only if these gaps do not depend on the minority status of the instructor, which is the case if there are minority gaps that persist across all classes, independent of instructor characteristics. This type of gap is implicitly controlled for in our empirical model through the inclusion of individual fixed effects and the estimation of what is essentially a difference-in-difference.

The hypothesis of differential sorting is testable if one has access to some measurable characteristics, $x_{ic}$, that are highly correlated with $u_{ic}$. Consider minority-specific classroom ***averages*** of $x_{ic}$, denoted $\overline{X_{mc}}$, where $m \in \{0,1\}$ is an index equal to one if the average is computed for minority-students and zero if it is computed for non-minority students. Since a classroom is associated with exactly one instructor minority status, these averages are the empirical counterparts of the conditional expectations in (5). We can then test for differential sorting by estimating a difference-in-difference model:

$$(6) \qquad \overline{X_{mc}} = \delta_1 * min\_inst_c + \delta_2 * \mathrm{I}_m + \delta_3 * min\_inst_c * \mathrm{I}_m + \nu_{mc} .$$

where $\mathrm{I}_m$ is a dummy variable equal to one if $m = 1$ and zero otherwise, and $\delta_3$ is an empirical estimate of the difference-in-difference in equation (5), with the observable measure, $x_{ic}$, replacing the unobserved component, $u_{ic}$. Hence, $\delta_3$ quantifies the extent to which minority gaps in an observable variable, $x_{ic}$, vary across classes that are taught by instructors of different minority groups. Clearly, an estimate of $\delta_3$ is only helpful in testing for differential sorting if $x_{ic}$ is strongly related to $u_{ic}$. Given the richness of our data we are able to use several variables, such as past academic performance, age and gender, as measureable characteristics to estimate a large set of "sorting regressions" such as (6).

By including classroom fixed effects we implicitly control for systematic differences in subject or course choices and associated grading differences between minority and non-minority students. Differential sorting thus is an issue if it takes place across class offerings of a course, which may happen if there is unrestricted student choice of classes and multiple sections offered for the same course in the same term. To address these remaining concerns we estimate specifications in which the sample of students and courses is chosen to minimize the possibility of differential sorting across classes. We first focus on a sample of students who have the lowest registration priority status, and thus have limited choice in classes. We also estimate model (5) using samples that rule out variation in instructors' minority status across classes within course-term or even course-year. In this case, sorting across classes within course and term or year is ruled out by construction of the sample.

### *3.2 Student Outcome Variables*

We estimate our model for five different student outcome variables, four of which are straightforward to compute: a dummy variable for whether a student drops the course at some time during the academic term, a dummy variable for whether a student passes the course conditional on finishing it, a course grade variable that is normalized to have mean zero and unit standard deviation within a course, and a dummy variable for whether the student has a grade above a B-.[20] All of these outcomes relate to a student's academic achievement in a particular course. One may also be interested in whether minority interactions are relevant for a student's future curriculum. We therefore generate a fifth outcome variable that records whether a student takes another course in the same subject in the next quarter a student is enrolled at the college.[21] This variable cannot be directly "manipulated" by the instructor and measures a "motivation" effect for the student.[22] More specifically, it answers the question of whether students are more likely to enrol in a same-subject course if she was taught by an instructor who shares her minority status, possibly due to a role-model effect that encourages her to do so. To avoid aggregation to the student-subject-time level and therefore to be able to use the same regression model as used

[20] The counterfactual we have in mind when estimating the models below is: How different would the grade outcome of a student in a particular course be if she was assigned to an instructor of a different minority status than her actual instructor? We thus normalize the grade variable at the course level.
[21] We do not examine the probability of taking a same-subject course in any following quarters, rather than the quarter directly afterward, because we cannot rule out that the results from such a specification would be driven by effects that accumulate over time.
[22] Another possibility is to use subsequent grades in same-subject courses to directly measure a skill effect, following Hoffmann and Oreopoulos (2009) and Carrell, Page and West (2010) in their analysis of gender interactions. However, in community colleges, predetermined curricula that force students to take a specified sequence of same-subject courses are uncommon. Furthermore, students who are low on the registration priority list in one quarter may move up in the ranking in the subsequent quarter.

for other outcomes, we use observations only for which the student has taken exactly one course in a quarter in a certain subject. This sample restriction is not particularly restrictive as it drops only approximately 22 percent of student-class observations from the sample, reflecting the fact that community colleges do not impose strict rules on the curricula of students.

### *3.3 Estimation of Two-Way Fixed Effect Model*

Estimation of two-way fixed effects models with unbalanced panel data becomes computationally infeasible with large data sets. With more than 30,000 students and over 20,000 classrooms in our data, model parameters cannot be estimated directly by OLS. Since our data set is a non-balanced panel, conventional within transformations are not possible, either. We thus rely on recent advances in the estimation of firm-and worker fixed effects from administrative data. The computational algorithms used to estimate two-way fixed effects models with high-dimensional sets of dummy variables generally rely on the fact that each individual only contributes to the identification of a subset of the fixed effects.[23] In our example, each student only contributes to the identification of the classrooms she or he visits at one point. This implies that normal equations involve block-diagonal ("sparse") matrices whose inversion is much less difficult than the inversion of non-sparse matrices. In practice, one performs a within-transformation in a first step to eliminate individual fixed effects, and then solves the remaining normal equations using matrix-inversion schemes that exploit the block-diagonal structure of the remaining matrices.[24] All standard errors are clustered at the classroom-level.[25]

[23] The seminal paper in this literature is Abowd, Kramarz and Margolis (1999). Refinements have been developed by Abowd, Creezy and Kramarz (2002) and Andrews et al (2008). Cornelissen (2008) has written a Stata-routine based on these algorithms.

[24] The literature estimating firm-and worker fixed effects also utilizes the fact that many workers never change firms, thus not contributing to identification of any of the firm fixed effects. This can further increase the speed of computation. In our example, we cannot apply this method since nearly all students take more than one class in the data and thus contribute to the identification of at least some classroom fixed effects.

[25] We have also experimented with clustering standard errors at the classroom-minority level instead. As expected, this improves the precision of our estimates slightly.

### *3.4 Bounds*

Estimation of the econometric models for grade outcomes is possible only for the sample of students who complete the course. The propensity to finish a course might be affected by the variable of interest – the minority-status interactions between students and instructors within classrooms - as well. This creates a potential sample selection problem, formally described by the following set of equations:

(7) $$grade_{ijc} = \alpha_1^{grade} * min\_stud_i * min\_inst_j + \gamma_i^{grade} + \phi_c^{grade} + u_{ijc}^{grade}$$

(8) $$dropped_{ijc} = \alpha_1^{dropped} * min\_stud_i * min\_inst_j + \gamma_i^{dropped} + \phi_c^{dropped} + u_{ijc}^{dropped}$$

(9) $$grade_{ijc} = (1 - dropped_{ijc}) * grade^*_{ijc}.$$

Equations (7) and (8) replicate equation (4) for the grade-outcome and the dropout-variable, while equation (4) accounts for the potential selection bias. OLS-estimates of the parameter of interest, $\alpha_1^{grade}$, is biased conditionally on individual fixed effects if $\alpha_1^{dropped}$ is significantly different from zero. Correcting for sample selection is difficult in our case since any variable affecting dropout behavior arguably also affects potential grades. Without exclusion restrictions, identification in a standard Heckman-selection model is solely based on the non-linearity of the correction term. Furthermore, with the inclusion of classroom- and student fixed effects, estimates from reduced-form Probit equations required for a Heckit-procedure are biased. We thus estimate non-parametric bounds of $\alpha_1^{grade}$ following Lee (2010).[26] In general, OLS-estimates are biased downward if minority students are less likely to drop the course when the instructor belongs to the minority group as well, and if the marginal students induced to stay come from the left tail of the grade distribution. It is biased upward if the marginal students come from the right tail of the grade distribution. We can therefore estimate an upper (lower) bound of $\alpha_1^{grade}$ when

[26] See also Krueger and Whitmore (2002) and Hoffmann and Oreopoulos (2009) for a related application.

applying OLS to a sample without the ($\alpha_1^{dropped}$ *100)-percent worst (best) minority students in classes taught by a minority instructor.

We therefore apply the following procedure: In the first step we estimate equation (1) for the dropout-variable. This provides us with an estimate of $\alpha_1^{dropped}$, the "minority gap" in dropout behavior when the class is taught by a minority instructor. We then calculate the ($\alpha_1^{dropped}$ *100) percentile ($\left(1-\alpha_1^{dropped}\right)$*100 percentile) of the minority-student grade distribution for every class taught by a minority instructor and drop all minority students with a final grade lower (higher) than this percentile. Since we are focusing on selection due to the ***relative*** difference from having a minority instructor between minority and non-minority students, we do not need to trim marginal non-minority students. In the second step we use this restricted sample to estimate the same equation as in the first step, but with final grade replacing the dropout variable as the outcome. We also perform this algorithm by running the dropout-regressions course-by-course, therefore providing us with course-specific estimates of $\alpha_1^{dropped}$. As Lee (2010) shows, this procedure yields the tightest bounds on the parameter of interest if the outcome variable is continuous. We thus compute the bounds only for the grade variable, which is our only continuous outcome variable, while leaving the results for the discrete outcome "Passed Course" uncorrected.[27]

We interpret these bounds results as a robustness check rather than as the main part of our analysis. By the logic of role-model effects it is reasonable to assume that it is the lower-achieving minority students rather than the best students who are at the margin of dropping a class and who are induced not to do so because they share the minority status with their instructor. We can test this assumption by using a variable that is highly correlated with

[27] Strictly speaking, this variable is not continuous, either. For our application, this can be problematic because the grade distribution has mass-points at the lower and upper tail. Hence, if we trim the distribution at the x%-percentile, we might drop more than x% of the student/grade observations. We solve this problem by randomly drawing from the student/grade observations clustered at the mass-points in such a way that we are trimming exactly x% of the distribution.

unobserved student traits. Prior academic achievement, which is measured as the student's GPA prior to enrollment in each class, is used for this variable.[28] We then estimate a version of model (4) for the course dropout variable that allows for an interaction between the minority interaction and prior GPA. This is effectively a triple interaction and allows the minority effect on course dropout behavior to vary across students with different prior GPA's. Our monotonicity-assumption is violated if the minority-interaction is stronger for those with a higher prior GPA. As presented below in Section 4.4, we do not find evidence of a stronger minority interaction for those with a higher prior GPA suggesting that we can interpret our uncorrected estimates as lower bounds of minority-interactions.

## 4. Results

### *4.1 Evidence against Sorting*

A potential threat to the validity of our estimated minority interaction effects arises from the possibility that students sort into classes in a systematic way. If for example high-ability minority students are more likely than non-minority students to sort into classes taught by minority instructors, our results might be biased upwards. Although we will use several strategies to rule out the possibility that our results are being driven by this type of student behaviour, we first investigate whether there is evidence of non-random sorting by estimating equation (6) for various variables observed in the data that are likely to be correlated with the unobserved ability term. To implement this test for differential sorting we calculate ***minority-specific*** classroom averages of these variables and regress them on a dummy variable that is equal to one if the observation is associated with the minority student group, a dummy that is equal to one if the class is taught by a minority instructor, and the interaction between these two variables. The interaction coefficient measures the extent to which the minority-gap in the outcomes varies

[28] To increase statistical power and to be able to utilize observations for the first year-quarter a student is enrolled at the college, we also include the grades obtained in all other courses taken in the same year-quarter to calculate prior GPA.

across classes taught by minority and non-minority instructors and is thus an estimate of differential sorting. Estimates for the interaction term, $\delta_3$, for several different student background variables are presented in Table 2, panels A to D. For each of these variables we investigate the robustness of the results with respect to the regression specification, the sample, and the type of variation in instructor minority status across different class offerings of a course used to identify the parameter of interest. Standard errors are clustered at the course-term-minority level.

We use the following four outcome variables, corresponding to the variable $\overline{X_{mc}}$ in equation (6): Student age in panel A, student gender in panel B, the cumulated number of courses taken before enrolment in the current class in panel C, and the cumulated GPA prior to enrolment in panel D. As past GPA and present GPA are highly correlated, we view the last variable as a particularly good measure of a potential unobserved student component that might be related to differential selection. In particular, if the minority-non-minority gap of accumulated GPA prior to enrolment in the current course is different in classes that are taught by minority instructors, our assumption of no differential sorting is most likely violated. Column 1 shows results for equation (6) that includes minority-specific course and term (year-quarter) fixed effects. Allowing fixed effects to vary across minority groups allows for course- and term-specific minority gaps. The sorting-parameter of interest is therefore identified from within-course variation, and apart from an adjustment for general time effects it utilizes some variation across classes taught in different terms. In column 2 we modify the regression specification by including a set of minority-specific course-term fixed effects instead, thus only allowing for variation in instructor minority status across different class offerings of a course in the same year-quarter. This isolates the level of variation differential sorting is most likely to operate along: Different classes of the same course offered in a particular term may attract different populations of minority and non-minority instructors. In columns 3 and 4 we repeat the exercise for a sample that drops course-time combinations that have variation in instructor minority status across classes. As a consequence, the sorting parameter is identified from variation across different classes of the same course, but taught in different terms. By construction of the resulting sample, differential sorting can only arise if students systematically delay enrolling in a particular course to be matched to an instructor of the same minority group. In the last column we further restrict the sample to rule out any course-specific variation in instructor minority status within an academic year, so that the relationship between instructor and student characteristics for a given course is measured across years.

The results present an exceptionally clear story: There is no evidence for differential sorting. Perusing the panels of the table clarifies that the insignificance of our sorting parameter is not driven by the imprecision of our estimates. Rather, point estimates fluctuate wildly as we explore the robustness of our estimates, indicating that we cannot detect any systematic or robust sorting patterns in the data. Furthermore, our conclusions do not depend on the subsample, the set of fixed effects, the restrictions on the variation in instructor minority status across classes of the same course, or the outcome used as a measure in the sorting regressions. Most importantly, minority gaps in accumulated GPA prior to course enrolment - a variable that is most likely to be highly correlated with unobserved student traits – do not depend on instructor race. In other words, we do not find evidence that high ability minority students are more likely to take minority-taught classes compared with high ability non-minority students. We interpret this as strong evidence in favour of our working hypothesis of no differential sorting.

### *4.2 Main Results*

Estimates of the minority interactions between students and instructors for all four outcome variables using the full sample and a sub-sample of students who are low on the registration priority list are shown in table 3. We also explore the sensitivity of results with respect to the set of fixed effects included in the econometric models. As we move along the columns, we increasingly restrict the variation used to identify our parameter of interest. Results from our preferred specification described in equation (4) which includes both student and classroom fixed effects are displayed in column (6) of the table. The other specifications considered in the table include minority-specific time fixed effects and a set of student and instructor controls (column 1), a specification that adds minority-specific course fixed effects (column 2), a specification with minority-specific course-time fixed effects (column 3), and specifications with student or classroom fixed effects (columns 4 and 5, respectively).

We highlight three main results: First, there is a significant minority interaction effect on student dropout behaviour that is robust with respect to the sample used and the set of fixed effects included. Our main estimates indicate a reduction of the minority gap in course dropout behaviour when taught by a minority instructor by 2 to 3 percentage points.

Second, when using the remaining four outcome variables, minority interaction effects are robust with respect to the set of fixed effects only when relying on the sample of low-priority students (below we bound these effects by whether minority instructors cause better or worse performing minority students to stay). This is our preferred sample since students included in it are severely restricted in their choice of course and instructor. For this group of students, the minority gap in the probability of passing a course decreases by up to 4 percentage points, a sizable effect. Furthermore, we estimate a robust reduction of the minority gap in grades of 5 percent of a standard deviation. However, for this outcome the standard errors are too high to yield a definite conclusion. This reflects the trade-off between restricting the sample to students who are low on the registration priority list, which lessens concerns about sorting, and using the full sample, which provides more precise estimates. We therefore report estimates from both samples throughout. We do not find a robust minority-interaction effect on the probability of having a good grade, indicating that the grade adjustment takes place mostly at the lower end of the grade distribution. However, the minority gap in the probability of continuing a subject in the following quarter is significantly affected by the minority status of the instructor. This may be interpreted as evidence for a "role model effect", motivating students to continue in a particular subject if they were taught by an instructor sharing their minority identity earlier in their academic career at the college.

Third, when including both, student and classroom fixed effects, minority-interaction effects are statistically significant for all outcome variables, and they are robust with respect to the sample of students utilized in the estimation. One possibility for the large impact of including classroom fixed effects on the estimates is that instructors who are assigned to different sections of the same course apply different grading and evaluation procedures. Inclusion of classroom fixed effects acts as a way of "standardizing" tests and classroom conditions across the student observations used to identify the interaction effect. Our results suggest that it stabilizes our estimates considerably, although we estimate an additional set of over 20,000 parameters.

Overall, estimates from the full and low-registration priority samples indicate strong positive effects of having a minority instructor on dropout behaviour and course performance among minority students. The lack of sensitivity of estimates to the sample of low-registration priority students who have limited instructor choice provides further evidence that is consistent with the lack of racial sorting across course offerings noted above.

### *4.3 Robustness Checks*

In table 4 we report regression estimates for our preferred model (1) using several subsets of our data to explore the robustness of our estimates. The first two rows show results from a regression in which the minority interaction effect is allowed to vary by student gender. There is no evidence that these effects are gender specific.

Although we have presented robust evidence against differential sorting in table 2, and even though we use an identification strategy that minimizes the possibility of selection biases in our estimates in non-experimental data, one may still be concerned that unobserved differences in student traits between minority and non-minority students vary across classes based on the minority-status of the instructor. We thus experiment with three specifications that restrict the variation in instructor minority status within course-time and across classrooms. In the first of these specifications we drop course-time combinations with different instructors teaching different sections. Hence, we only keep courses that are taught by the same instructor in a term, no matter how many sections of the same course are offered simultaneously. In the second specification we allow different instructors to be observed in a year-quarter, but we drop observations for which some sections are taught by minority instructors and others by non-minority instructors. Identification of minority student-instructor interactions therefore comes only from across quarter variation in instructor ethnicity or race. In the third of this set of regressions we further restrict the sample to exclude variation in instructor minority status within an academic year for a given course. In this case, students would have to postpone taking a course for an entire academic year to satisfy a potential racial preference in their instructor, which may be very difficult given the required sequencing of courses within subjects and two-year enrolment goals. Other than for the first specification applied to a sample of low-priority students, we obtain substantial, robust and often significant estimates of the minority interactions for all outcomes other than the propensity to enrol in a same-subject course in the subsequent term. Insignificance of estimates is largely driven by an increase of standard errors, which is to be expected since we are using significantly smaller samples.

Robustness of our estimates with respect to exclusion of language courses or video-delivered courses is investigated in the next two sets of results. Interactions between students and instructors may be different in these types of courses and non-representative of normal effects. Our results are robust to either sample restriction. Importantly, and as expected, excluding video-delivered online courses strengthens our results somewhat, but most point estimates do not change significantly.[29]

[29] As discussed in section 2, it is debatable whether to include the video-delivered courses at all. The description of these courses indicates that there is significant student-instructor interaction for these

In the last three sets of results we investigate whether our findings may be driven by particular institutional features of community colleges. This can be interpreted as an exercise addressing the question of whether our results can be extrapolated to 4-year colleges. A first potential concern about external validity comes from the fact that students who return to the college after a leave of absence are automatically assigned the lowest possible registration priority status. These may be students with an "unstable" academic career who periodically enroll in courses at De Anza College - a sub-group of students that may be particular to the community college setting. We therefore estimate our preferred regression model for a sample of students who enrol at the College for the first time and thus also automatically qualify for the lowest standing on the registration priority list.[30] This yields point estimates that are nearly identical to those obtained from a sample of all low registration priority students, suggesting that our results are not driven by more senior students who are frequently leaving and returning to the college.[31] The smaller sample size, however, leads to insignificance of our estimates.

To explore whether racial and ethnic interactions differ depending on whether courses are academically or vocationally oriented, we exploit information in our data about whether each course counts towards a vocational degree and whether it can be transferred to the University of California and California State University systems. Due to the academic orientation of De Anza College, the sample fraction of academic (or non-vocational) courses is 74 percent and the sample fraction of transferable courses is 70 percent as documented in table 1. We find that vocational courses have significantly smaller minority interaction effects with respect to students' propensity

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courses, motivating our choice of not dropping them from our main sample. However, we have re-estimated all of our specifications for samples that exclude video-delivered courses. Consistent with the evidence presented here, this does not significantly affect our results. Hence, combining any of the sample restrictions applied in this table with excluding video-delivered courses does not alter our conclusions. Results are available upon request.

[30] Whether a student enrolls for the first time is information that is included in the original administrative data. This is a major advantage since it avoids the need to generate such a variable, e.g. from assuming that the first time a student is observed in the data is the first time the student is enrolled at De Anza College. As a consequence, this variable is **not** left-censored.

[31] Additional evidence is provided by the finding of roughly similar results using the full sample of students as displayed in Table 3.

to drop a course. For the other outcomes, there are no significant differences in these effects between vocational and non-vocational courses. However, the effects estimated for the non-vocational - and thus predominantly academic – courses are stronger than those found in our main specifications in table 3 (and in some cases significantly so). Similarly, we find some evidence of a stronger minority effect with respect to the dropout variable in courses that are transferable to the UC and CSU systems, but no evidence for any differences when using the other outcomes. At the same time, the point estimates are not significantly different from those found in table 3. Taken together, these results suggest that minority interaction effects exist in both more and less academically oriented courses, with some evidence that they are stronger in the former.

### *4.4 Bounds analysis of interaction effects on grades*

Table 5 displays lower and upper bounds of the minority interaction effects when using standardized grade outcomes as the dependent variable. We compute these bounds following the procedure described in Section 3.4 and interpret them as a robustness exercise. When using the full sample, estimates are bounded by 3.9 percent and 7.9 percent of a standard deviation in the course grade. The estimated lower and upper bounds are all statistically significant at conventional levels. When using the sample of low-priority students instead, the sample sizes decrease and the bounds widen. They are given by 2.9 percent and 9.3 percent of a standard deviation in the course grade. Standard errors increase by a factor 2.5, but the upper bounds are statistically significant. Taken together, these results provide further evidence of a robust and quite substantial minority interaction effect on grades, in addition to a substantial effect on the probability of dropping a class.

As argued above, we interpret our uncorrected estimates as representing a lower bound of minority interactions, since those who are at the margin of dropping a class and who are induced not to do so because they share the minority status with their instructor are more likely to be from the lower part of the student ability distribution. This monotonicity assumption can be tested by estimating a version of model (4) for the course dropout variable that allows for an interaction between the minority interaction and prior GPA. It is violated if the minority-interaction is stronger for those with a higher prior GPA. The estimated minority-interactions are -0.023 (s.e. 0.015) and -0.037 (s.e. 0.025) for the full sample and the sample of low registration priority students, respectively, while the corresponding triple-interactions with prior GPA are 0.0007 (s.e. 0.005) and 0.004 (s.e. 0.009) respectively. Since the minority effects are estimated to be negative, their positive interactions with prior GPA thus are in accordance with our hypothesis. However, these estimates are not significant, suggesting that differential dropout behavior does not depend systematically on a student's academic abilities.

For a more flexible specification, we also compute the distribution of prior GPA in the sample and estimate the dropout effect using model (1) separately for those student-class observations for which prior GPA is below the 25% percentile or above the 75% percentile. The estimated minority effect on course dropout behavior for the sample of low registration priority students is -0.032 (s.e. 0.026) for the former and -0.019 (s.e. 0.044) for the latter, clarifying that, if anything, those at the margin of dropping a course are more likely to come from the lower end of the ability distribution.[32]

### *4.5 Are Students really reacting to Instructors?*

Do our estimated minority interaction effects reflect minority students gaining from being assigned to minority instructors? Only in this case would a counterfactual policy that increases the representation of minority instructors have a skill or human capital effect on minority students. In this section we investigate two alternative behavioral mechanisms that can potentially generate significant estimates of minority interactions between students and instructors: instructor-based discrimination – consciously or unconsciously – and peer effects within the

[32] The corresponding estimates for the full sample are -0.031 (s.e. 0.012) and 0.004 (s.e. 0.015).

class, working through high registration priority students who sort systematically on the minority status of the instructor.[33]

We start by presenting several pieces of evidence that point toward students adjusting their academic behaviour to race of the instructor rather than instructors adjusting to the race of the student.[34] First, we have documented significant, robust, and sizable minority effects with respect to the course dropout behaviour of students. In particular, the minority gap in this outcome variable decreases by 2 to 3 percentage points if the class is taught by a minority instructor. The decision to drop out of the class is made entirely by the student and must be made in the first 4 weeks of a term, well before final grades are assigned by instructors. In other words, since an instructor at the college level does not play an active role in determining whether a student drops a course before taking a final exam, the significant dropout effect found in our data reflects a behavioural adjustment on the student side.

Similarly, we have investigated whether there are minority-interactions with respect to the probability of a student taking another course in the same subject in the following quarter.[35] This variable cannot be directly "manipulated" by the instructor and measures a "motivation" effect on the side of the student, possibly due to role-model effects. More specifically, it answers the question of whether students are more likely to enrol in a same-subject course if she was taught by an instructor who shares her race. We have documented significant and robust effects in table 3. While some sensitivity is found in the robustness analysis for this outcome in table 4, it is primarily driven by a loss of statistical power rather than sensitivity in point estimates. Generally,

[33] There may be skill (rather than pure grade-deflation) effects of instructor discrimination if the discriminated student group perceives or anticipates it and becomes discouraged.

[34] Whether students react to some characteristics of the instructor, or vice versa, has been a long-standing question in the literature on gender-interactions in the classroom (see for example Hoffmann and Oreopoulos 2009, and Carrell, Page and West 2011). It cannot be resolved by relying on experimental evidence since instructor discrimination may exist in a setting where students are randomly assigned to instructors. For a definite test, one needs a setting where final grades are based entirely on exams that are not hand-written, are single-blinded, and do not provide any information about a students' demographic characteristics such as race and gender.

[35] We do not examine the probability of taking a same-subject course in any following quarters, rather than the quarter directly afterward, because we cannot rule out that the results from such a specification would be driven by effects that accumulate over time.

these results seem to corroborate our hypothesis that minority interactions are, at least to some extent, driven by the student reacting to the instructor.

Our last piece of evidence builds on the logic that race or ethnicity based discrimination should not depend on student age. If for example non-minority instructors systematically discriminated against minority students, a minority interaction should not vary by a student's age. We thus estimate a version of our model that allows minority effects to depend on student age. To rule out the significance of the resulting triple interactions to be driven by varying sample sizes of our age groups, we generate a dummy variable that is equal to one if a student is above the median age of 21.5 years in our working sample. Since we are estimating the heterogeneity of results with respect to age in this specification, we also include students who are older than 35 years and who were dropped from the main sample. We generate a second dummy variable that is equal to one if a student is older than 35 and interact both age-group dummies with our minority effects. The results are shown in the first 3 rows of table 6. The results are quite striking: Minority interaction effects are by far the biggest for students who are younger than the median aged student. In fact, with one exception where an interaction effect is significant at the 10 percent level, there are no significant effects for older students. Hence, our results are largely driven by the students in our sample who are younger than 21. While these results are inconsistent with discrimination affecting all students of a certain minority or non-minority group irrespective of age, they might be explained by young students being more susceptible to role-model effects.

While there is evidence that our estimated minority effects are not entirely driven by discrimination on the instructor side, they may be generated by peer effects that take place in the classroom. Such peer effects could exist if high-ability minority students who do ***not*** have a low standing on course enrollment priority lists were more likely to be enrolled in classes taught by minority instructors than high-ability non-minority students, and if these additional high-ability students positively affect the academic achievement of only the low registration priority minority-students. However, results from the sorting analysis as shown in table 2 do not support the hypothesis that high registration priority students (denoted in the table by "Continuing Students, Not Low Registration Priority") systematically sort across classrooms. Hence, there is no evidence for the assumption that low registration priority students are subject to different peers depending on the minority status of the instructor.

Although our sorting analysis utilizes past academic achievement which is likely to be highly correlated with unobserved skills, it is worthwhile to employ a second test for peer effects. In a first step we calculate for each class in the sample the fraction of minority students within the group of those who do ***not*** have a low standing on registration priority lists. We next create two dummy variables, one of which is equal to one if the fraction is below the 25%-quantile of its distribution, and one of which is equal to one if the fraction is above the 75%-quantile of its distribution. To compute these shares, we use the respective numbers of students at the beginning of the term when using "dropout" as an outcome variable and the corresponding numbers at the end of the term for the other outcome variables. We then allow the minority effect to vary across classes that have a low, high, and intermediate share of minority students in the sub-population of high registration priority students. Results are shown in the second part of table 6. The precision of our estimates decreases considerably. However, the estimated minority effects for the baseline group – classes with an intermediate share of minority students among those who do not have a low standing on registration priority lists – are very close to those found from our main specification in table 3. However, the interaction terms are insignificant in all but one specification, and often have the opposite sign of what is predicted by peer effects models. Taken together, we conclude that our results are unlikely to be driven by differential peer effects across classrooms.

### *4.6 Race and Ethnicity Interactions*

In table 7 we break down our minority interaction estimates by different ethnicities and race. We focus on four major racial/ethnic groups, whites, African-Americans, Hispanics, and

Asians. While student fixed effects absorb the interaction for one of the student groups – in our case “whites” - the classroom fixed effects absorb the interaction for one of the instructor groups – again “whites”. Thus, only 9 of the 16 race and ethnicity interactions are identified and all estimated interaction effects are relative to outcomes for white students with alternative instructor types. We present the P-value from F-tests for two hypotheses of major interest, namely for the presence of an own-race interaction and for the presence of any race interaction. We find strong and robust evidence for own-race interactions. The positive interaction estimates are not overly sensitive to whether we use the full sample or limit the sample to low-registration priority students. We find positive interactions for all major racial groups with African-American students experiencing particularly large and robust relative gains from being taught by a same-race instructor. This is particularly noteworthy given that African-American students and instructors account for only 4 percent and 6 percent of the sample, respectively. It is noteworthy, however, that Hispanic students also experience positive interactions with Hispanic instructors.

## 5. Conclusion

Using a unique administrative dataset that matches student course outcomes to instructor's race, we estimate for the first time the importance of racial interactions between instructors and students at the college level. The estimation of two-way fixed effect models for a very large number of both students and classrooms over five years addresses most concerns about potential biases in estimating racial interactions. Remaining concerns about the internal validity of our estimates are addressed by taking advantage of the severely restricted class enrollment options among low-registration priority students at a very popular and class-rationed community college, and by restricting the variation in instructor minority status across classes within term or year. We find that minority students perform relatively better in classes when instructors are of the same race or ethnicity. African-Americans, Hispanics, and Native Americans are 2.9 percentage points more likely to pass classes with instructors of similar background and 2.8

percentage points more likely to pass classes with underrepresented instructors. These effects represent roughly half of the total gaps in classroom outcomes between white and underrepresented minority students at the college. The effects are particularly large for African-Americans. The class dropout rate relative to whites is 6 percentage points lower for Black students when taught by a Black instructor. Conditional on completing the class, the relative fraction attaining a B-average or greater is 13 percentage points higher.

We estimate relative grade score effects ranging from 4 to 8 percent of a standard deviation from being assigned an instructor of similar minority status. Taken together with the large class dropout interaction effects, these impacts are notably larger than those found for gender interactions between students and instructors at all levels of schooling. They are likely due to students behaving differently based on minority status of instructors rather than the other way around. We find dropout effects before receiving a grade, effects for younger students but not for older students, and effects on subsequent course choices – all of which provide evidence that minority students react to minority instructors instead of the reverse.

Our results suggest that the academic achievement gap between white and underrepresented minority college students would decrease by hiring more underrepresented minority instructors. However, the desirability of this policy is complicated by the finding that students appear to react positively when matched to instructors of a similar race or ethnicity but negatively when not. Hiring more instructors of one type may also lead to greater student sorting and changes to classroom composition, which may also impact academic achievement. A more detailed understanding of heterogeneous effects from instructor assignment, therefore, is needed before drawing recommendations for improving overall outcomes. The topic is ripe for further research, especially in light of the recent debates and legislative changes over affirmative action.

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## TABLE 1 - DESCRIPTIVE STATISTICS

### *PANEL A: Student-Class Level*

| | Mean | Std. Dev. | *Total Number of Observations* |
|---|---|---|---|
| ***Unrestricted Sample*** | | | |
| Small Course | 0.024 | 0.02 | |
| Small Department | 0.012 | 0.01 | |
| Student younger than 21.5 years | 0.536 | 0.25 | *506,280* |
| Student between 21.5 and 35 years | 0.372 | 0.23 | |
| Students older than 35 years | 0.092 | 0.08 | |
| ***Main Sample*** | | | |
| Age of Student | 22.2 | 4.14 | *446,205* |
| Entering Student | 0.10 | 0.30 | |
| Low Registration Priority Student | 0.29 | 0.46 | |
| Language Course | 0.03 | 0.16 | |
| Video-Delivered Course | 0.06 | 0.24 | |
| Vocational Course | 0.26 | 0.44 | |
| Course transferable to UC or CSU Systems | 0.70 | 0.46 | *446,239* |
| Course is taught by one instructor within quarter | 0.26 | 0.44 | |
| Course has no variation in instructor underrepresented-minority status within quarter | 0.61 | 0.24 | |
| Course has no variation in instructor underrepresented-minority status within academic Year | 0.52 | 0.25 | |
| Age of instructor | 52.52 | 10.8 | *446,235* |
| Instructor teaches part-time | 0.41 | 0.49 | *446,239* |

### *PANEL B: Student Outcome -, Student-Class Level, by Race/Ethnicity, Main Sample*

| | **Student Race/Ethnicity** | | | | |
|---|---|---|---|---|---|
| | *White* | *Asian* | *Hispanic* | *African American* | *Nat. American, Pacific Isl., other non-White* |
| Dropped Course | 0.24 | 0.26 | 0.28 | 0.30 | 0.28 |
| ***Total Nr of Obs: 444,239*** | *(0.43)* | *(0.44)* | *(0.45)* | *(0.46)* | *(0.45)* |
| Passed Course | 0.89 | 0.89 | 0.84 | 0.82 | 0.86 |
| ***Total Nr of Obs: 319,641*** | *(0.31)* | *(0.32)* | *(0.37)* | *(0.39)* | *(0.35)* |
| Grade | 2.90 | 2.91 | 2.58 | 2.51 | 2.71 |
| ***Total Nr of Obs: 277,889*** | *(1.14)* | *(1.14)* | *(1.19)* | *(1.21)* | *(1.19)* |
| Good Grade (B or higher) | 0.68 | 0.68 | 0.57 | 0.53 | 0.61 |
| ***Total Nr of Obs: 277,889*** | *(0.47)* | *(0.47)* | *(0.50)* | *(0.50)* | *(0.49)* |

### *PANEL C: Student Level*

| | Mean | S.D. | *Sample Size* |
|---|---|---|---|
| ***Main Sample*** | | | |
| Female Student | 0.50 | 0.50 | *31,894* |
| Underrepresented Minority Student | 0.21 | 0.41 | |
| White Student | 0.28 | 0.20 | |
| Asian Student | 0.51 | 0.25 | *31,961* |
| Hispanic Student | 0.14 | 0.12 | |
| African-American Student | 0.04 | 0.04 | |
| Native American, Pacific Islanders, Other non-White Student | 0.03 | 0.03 | |

### *PANEL D: Instructor Level*

| | Mean | S.D. | *Sample Size* |
|---|---|---|---|
| ***Main Sample*** | | | |
| Female Instructor | 0.49 | 0.50 | |
| Underrepresented Minority Instructor | 0.16 | 0.36 | |
| White Instructor | 0.70 | 0.21 | |
| Asian Instructor | 0.14 | 0.12 | *942* |
| Hispanic Instructor | 0.06 | 0.06 | |
| African-American Instructor | 0.06 | 0.05 | |
| Native American, Pacific Islanders, Other non-White Instructor | 0.04 | 0.03 | |

**NOTES**: Courses are defined to be "small" if their average enrollment per session falls below the 2-percentile of the course enrollment distribution. Departments are defined to be "small" if the total number of students in the sample associated with a department is smaller than the 1-percentile of the departmental size distribution. In our main analysis we focus on students who are at most 35 years old. The median age of the resulting sub-sample is 21.5 years. This motivates the choice of student-age groups listed in Panel A of this table. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites.

## TABLE 2 - SORTING REGRESSIONS

| | SAMPLE RESTRICTIONS: IS INSTRUCTOR RACE/ETHNITICY ALLOWED TO VARY ACROSS CLASSES OF THE SAME COURSE? | | | | |
|---|---|---|---|---|---|
| | *YES* | | *NOT WITHIN, BUT ACROSS TERMS* | | *ONLY ACROSS ACADEMIC YEARS* |
| ***PANEL A: OUTCOME - STUDENT AGE*** | | | | | |
| All Students | -0.004 | 0.046 | -0.205 | 0.091 | -0.093 |
| | (0.079) | (0.112) | (0.168) | (0.302) | (0.374) |
| All Low Registration Priority Students | 0.052 | 0.083 | -0.419 | 0.541 | -0.814 |
| | (0.123) | (0.174) | (0.328) | (0.651) | (0.685) |
| Entering Students (==> Low Registration Priority) | 0.061 | 0.037 | 0.266 | 2.058 | 0.101 |
| | (0.161) | (0.233) | (0.690) | (1.801) | (1.189) |
| Continuing Students, Low Registration Priority | -0.042 | -0.050 | -1.067 ** | -1.849 * | -0.828 |
| | (0.160) | (0.214) | (0.475) | (1.093) | (1.001) |
| Continuing Students, Not Low Registration Priority | -0.032 | 0.011 | -0.099 | -0.069 | 0.162 |
| | (0.082) | (0.118) | (0.195) | (0.373) | (0.399) |
| ***FIXED EFFECTS*** (BY UNDERREPRESENTED MINORITY STATUS): | | | | | |
| Course and Year | No | No | Yes | No | Yes |
| Course and Year-Quarter | Yes | No | No | No | No |
| Course-Year | No | No | No | Yes | No |
| Course-Year-Quarter | No | Yes | No | No | No |

**NOTES:** This table displays results from regressions of the minority-specific average student age in a classroom on an indicator equal to one if the average is associated with minority students, an indicator if the class is taught by a minority instructor, the interaction between these two variables, and a set of fixed effects. We only report the coefficient on the interaction term, to be interpreted as the extent to which minority students sort into classrooms taught by minority instructors. Each cell is associated with a different regression. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. Rows are defined by the subsample of students we consider. Low registration priority students are those students who have the lowest standing on class enrolment lists. Columns explore sensitivity of results with respect to different sets of fixed effects and different sources of variation used to identify the parameters. When using courses without variation in instructor underrepresented minority status in the same academic year, only one of the specifications can be estimated. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered on the level of the fixed effect.

| | SAMPLE RESTRICTIONS: IS INSTRUCTOR RACE/ETHNITICY ALLOWED TO VARY ACROSS CLASSES OF THE SAME COURSE? | | | | |
|---|---|---|---|---|---|
| | ***YES*** | | ***NOT WITHIN, BUT ACROSS TERMS*** | | ***ONLY ACROSS ACADEMIC YEARS*** |
| ***PANEL B: OUTCOME - STUDENT GENDER*** | | | | | |
| All Students | 0.009 | 0.014 | -0.003 | 0.015 | 0.018 |
| | (0.008) | (0.011) | (0.019) | (0.032) | (0.048) |
| All Low Registration Priority Students | 0.018 | 0.013 | -0.008 | 0.010 | 0.020 |
| | (0.011) | (0.017) | (0.031) | (0.052) | (0.066) |
| Entering Students (==> Low Registration Priority) | 0.006 | -0.012 | 0.066 | -0.127 | 0.209 |
| | (0.022) | (0.034) | (0.061) | (0.152) | (0.129) |
| Continuing Students, Low Registration Priority | 0.026 | 0.024 | -0.041 | -0.091 | 0.041 |
| | (0.018) | (0.026) | (0.050) | (0.095) | (0.126) |
| Continuing Students, Not Low Registration Priority | 0.006 | 0.012 | 0.002 | 0.019 | 0.008 |
| | (0.009) | (0.013) | (0.023) | (0.040) | (0.061) |
| ***FIXED EFFECTS*** (BY MINORITY STATUS): | | | | | |
| Course and Year | No | No | Yes | No | Yes |
| Course and Year-Quarter | Yes | No | No | No | No |
| Course-Year | No | No | No | Yes | No |
| Course-Year-Quarter | No | Yes | No | No | No |

**NOTES:** This table displays results from regressions of the minority-specific fraction of female students in a classroom on an indicator equal to one if the group fraction is associated with minority students, an indicator if the class is taught by a minority instructor, the interaction between these two variables, and a set of fixed effects. We only report the coefficient on the interaction term, to be interpreted as the extent to which minority students sort into classrooms taught by minority instructors. Each cell is associated with a different regression. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. Rows are defined by the subsample of students we consider. Low registration priority students are those students who have the lowest standing on class enrolment lists. Columns explore sensitivity of results with respect to different sets of fixed effects and different sources of variation used to identify the parameters. When using courses without variation in instructor underrepresented minority status in the same academic year, only one of the specifications can be estimated. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered on the level of the fixed effect.

| | SAMPLE RESTRICTIONS: IS INSTRUCTOR RACE/ETHNITICY ALLOWED TO VARY ACROSS CLASSES OF THE SAME COURSE? | | | | |
|---|---|---|---|---|---|
| | ***YES*** | | ***NOT WITHIN, BUT ACROSS TERMS*** | | ***ONLY ACROSS ACADEMIC YEARS*** |
| ***PANEL C: OUTCOME - CUMULATED COURSES PRIOR TO ENROLMENT*** | | | | | |
| All Students | -0.016<br>(0.094) | 0.077<br>(0.126) | -0.156<br>(0.306) | -0.012<br>(0.512) | -0.281<br>(0.600) |
| All Low Registration Priority Students | -0.126<br>(0.080) | -0.073<br>(0.101) | -0.118<br>(0.270) | -0.682<br>(0.511) | 0.724<br>(0.601) |
| Entering Students (==> Low Registration Priority) | -0.025<br>(0.057) | -0.070<br>(0.081) | 0.035<br>(0.268) | 0.129<br>(0.511) | -0.245<br>(0.337) |
| Continuing Students, Low Registration Priority | 0.014<br>(0.055) | -0.024<br>(0.076) | 0.364 **<br>(0.187) | 0.367<br>(0.443) | 0.147<br>(0.394) |
| Continuing Students, Not Low Registration Priority | -0.073<br>(0.093) | 0.034<br>(0.122) | -0.136<br>(0.327) | 0.203<br>(0.589) | -0.812<br>(0.636) |
| ***FIXED EFFECTS*** (BY MINORITY STATUS): | | | | | |
| Course and Year | No | No | Yes | No | Yes |
| Course and Year-Quarter | Yes | No | No | No | No |
| Course-Year | No | No | No | Yes | No |
| Course-Year-Quarter | No | Yes | No | No | No |

**NOTES:** This table displays results from regressions of the minority-specific average number of courses taken prior to enrolment in a classroom on an indicator equal to one if the group average is associated with minority students, an indicator if the class is taught by a minority instructor, the interaction between these two variables, and a set of fixed effects. We only report the coefficient on the interaction term, to be interpreted as the extent to which minority students sort into classrooms taught by minority instructors. Each cell is associated with a different regression. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. Rows are defined by the subsample of students we consider. Low registration priority students are those students who have the lowest standing on class enrolment lists. Columns explore sensitivity of results with respect to different sets of fixed effects and different sources of variation used to identify the parameters. When using courses without variation in instructor underrepresented minority status in the same academic year, only one of the specifications can be estimated. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered on the level of the fixed effect.

| | SAMPLE RESTRICTIONS: IS INSTRUCTOR RACE/ETHNITICY ALLOWED TO VARY ACROSS CLASSES OF THE SAME COURSE? | | | | |
|---|---|---|---|---|---|
| | ***YES*** | | ***NOT WITHIN, BUT ACROSS TERMS*** | | ***ONLY ACROSS ACADEMIC YEARS*** |
| ***PANEL D: OUTCOME - GPA PRIOR TO ENROLMENT*** | | | | | |
| All Students | 0.013<br>(0.015) | 0.017<br>(0.020) | -0.015<br>(0.037) | 0.030<br>(0.061) | -0.042<br>(0.089) |
| All Low Registration Priority Students | 0.025<br>(0.030) | 0.026<br>(0.042) | 0.000<br>(0.080) | 0.071<br>(0.142) | 0.017<br>(0.155) |
| Entering Students (==> Low Registration Priority) | 0.008<br>(0.067) | -0.003<br>(0.106) | 0.201<br>(0.217) | 0.586<br>(0.498) | 0.084<br>(0.526) |
| Continuing Students, Low Registration Priority | 0.039<br>(0.051) | 0.062<br>(0.073) | -0.072<br>(0.138) | -0.213<br>(0.342) | 0.116<br>(0.202) |
| Continuing Students, Not Low Registration Priority | 0.007<br>(0.015) | 0.013<br>(0.021) | -0.036<br>(0.037) | 0.015<br>(0.059) | -0.088<br>(0.101) |
| ***FIXED EFFECTS*** (BY MINORITY STATUS): | | | | | |
| Course and Year | No | No | Yes | No | Yes |
| Course and Year-Quarter | Yes | No | No | No | No |
| Course-Year | No | No | No | Yes | No |
| Course-Year-Quarter | No | Yes | No | No | No |

**NOTES:** This table displays results from regressions of the minority-specific average cumulated GPA prior to enrolment in a classroom on an indicator equal to one if the group average is associated with minority students, an indicator if the class is taught by a minority instructor, the interaction between these two variables, and a set of fixed effects. We only report the coefficient on the interaction term, to be interpreted as the extent to which minority students sort into classrooms taught by minority instructors. Each cell is associated with a different regression. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. Rows are defined by the subsample of students we consider. Low registration priority students are those students who have the lowest standing on class enrolment lists. Columns explore sensitivity of results with respect to different sets of fixed effects and different sources of variation used to identify the parameters. When using courses without variation in instructor minority status in the same academic year, only one of the specifications can be estimated. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered on the level of the fixed effect.

**TABLE 3 - ESTIMATED ROLE OF INSTRUCTOR MINORITY STATUS FOR STUDENT OUTCOMES**

| | (1) | | (2) | | (3) | | (4) | | (5) | | (6) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ***OUTCOME: STUDENT DROPPED COURSE*** | | | | | | | | | | | | |
| ***Number of Observations:*** | ***444,239*** | | | | | | | | | | | |
| All Students | -0.004 | | -0.020 | *** | -0.021 | *** | -0.018 | *** | -0.013 | *** | -0.020 | *** |
| | (0.006) | | (0.006) | | (0.007) | | (0.006) | | (0.005) | | (0.005) | |
| All Low Registration Priority Students | -0.006 | | -0.025 | *** | -0.030 | *** | -0.021 | * | -0.020 | ** | -0.029 | *** |
| | (0.009) | | (0.010) | | (0.011) | | (0.011) | | (0.010) | | (0.011) | |
| ***OUTCOME: STUDENT PASSED COURSE, CONDITIONAL ON FINISHING THE COURSE*** | | | | | | | | | | | | |
| ***Number of Observations:*** | ***319,641*** | | | | | | | | | | | |
| All Students | 0.002 | | 0.003 | | 0.001 | | 0.004 | | 0.003 | | 0.012 | *** |
| | (0.005) | | (0.006) | | (0.007) | | (0.006) | | (0.005) | | (0.005) | |
| All Low Registration Priority Students | 0.018 | * | 0.036 | *** | 0.040 | *** | 0.037 | *** | 0.014 | | 0.028 | ** |
| | (0.010) | | (0.011) | | (0.013) | | (0.012) | | (0.010) | | (0.012) | |
| ***OUTCOME: STANDARDIZED STUDENT COURSE GRADE, CONDITIONAL ON FINISHING THE COURSE*** | | | | | | | | | | | | |
| ***Number of Observations:*** | ***277,889*** | | | | | | | | | | | |
| All Students | 0.013 | | -0.018 | | -0.007 | | -0.006 | | 0.010 | | 0.054 | *** |
| | (0.017) | | (0.019) | | (0.020) | | (0.016) | | (0.015) | | (0.013) | |
| All Low Registration Priority Students | 0.052 | * | 0.056 | * | 0.056 | | 0.023 | | 0.017 | | 0.050 | |
| | (0.030) | | (0.033) | | (0.037) | | (0.034) | | (0.031) | | (0.033) | |
| ***OUTCOME: GOOD GRADE (B OR HIGHER), CONDITIONAL ON FINISHING THE COURSE*** | | | | | | | | | | | | |
| ***Number of Observations:*** | ***277,889*** | | | | | | | | | | | |
| All Students | -0.002 | | -0.007 | | -0.005 | | 0.004 | | 0.006 | | 0.024 | *** |
| | (0.008) | | (0.009) | | (0.009) | | (0.008) | | (0.007) | | (0.006) | |
| All Low Registration Priority Students | 0.000 | | 0.006 | | 0.000 | | 0.015 | | 0.003 | | 0.032 | ** |
| | (0.014) | | (0.015) | | (0.017) | | (0.016) | | (0.014) | | (0.016) | |
| ***OUTCOME: STUDENT ENROLS IN A SAME-SUBJECT COURSE IN THE SUBSEQUENT TERM*** | | | | | | | | | | | | |
| ***Number of Observations:*** | ***261,736*** | | | | | | | | | | | |
| All Students | 0.022 | *** | 0.020 | *** | 0.017 | ** | 0.015 | ** | 0.007 | | 0.011 | * |
| | (0.008) | | (0.007) | | (0.007) | | (0.008) | | (0.006) | | (0.006) | |
| All Low Registration Priority Students | 0.014 | | 0.033 | *** | 0.026 | ** | 0.018 | | 0.022 | * | 0.031 | ** |
| | (0.015) | | (0.012) | | (0.013) | | (0.015) | | (0.012) | | (0.016) | |
| ***FIXED EFFECTS:*** | | | | | | | | | | | | |
| Year-Quarter-Minority | **Yes** | | **Yes** | | No | | No | | No | | No | |
| Course-Minority | No | | **Yes** | | No | | **Yes** | | No | | No | |
| Course-Minority-Year-Quarter | No | | No | | **Yes** | | No | | No | | No | |
| Student | No | | No | | No | | **Yes** | | No | | **Yes** | |
| Classroom | No | | No | | No | | No | | **Yes** | | **Yes** | |
| ***CONTROLS:*** | | | | | | | | | | | | |
| Instructor Controls | **Yes** | | **Yes** | | **Yes** | | **Yes** | | No | | No | |
| Student Controls | **Yes** | | **Yes** | | **Yes** | | No | | **Yes** | | No | |

**NOTES:** This table displays results from our main outcome regressions. We report the coefficient of the interaction between student's and instructor's underrepresented minority status. Each cell is associated with a different regression. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. We consider 4 student outcomes: In panel A an indicator variation equal to one if the student drops the course; in panel B an indicator variable equal to one if the student passes the course; in panel C the student's **standardized** course grades; in panel D an indicator variable equal to one if the student has a grade of at least B. We explore the sensitivity with respect to the regression specification, i.e. the set of fixed effects and controls included in the regressions. Student controls include age and gender; instructor controls include age, gender, and a part-time indicator. We also compute the regression coefficients for a sample of all students and a sample of students with a low standing on class enrollment lists. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered by classroom.

**TABLE 4 - ESTIMATED ROLE OF INSTRUCTOR MINORITY STATUS FOR STUDENT OUTCOMES: ROBUSTNESS**

| | ALL STUDENTS | | | | | LOW REGISTRATION PRIORITY STUDENTS | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Dropped Course | Passed Course | Grade (Standar-dized) | Good Grade (B or higher) | Takes Same-Subject Course Subsequently | Dropped Course | Passed Course | Grade (Standar-dized) | Good Grade (B or higher) | Takes Same-Subject Course Subsequently |
| ***Male vs. Female Students*** | | | | | | | | | | |
| Minority Interaction | -0.021 *** | 0.012 * | 0.029 * | 0.021 ** | 0.004 | -0.019 | 0.038 ** | 0.021 | 0.031 | 0.019 |
| | (0.007) | (0.007) | (0.018) | (0.009) | (0.009) | (0.015) | (0.018) | (0.047) | (0.023) | (0.023) |
| Minority Interaction****Female Students*** | 0.002 | -0.001 | 0.044 ** | 0.005 | 0.012 | -0.019 | -0.019 | 0.054 | 0.003 | 0.021 |
| | (0.009) | (0.009) | (0.024) | (0.012) | (0.011) | (0.019) | (0.022) | (0.060) | (0.029) | (0.028) |
| ***Course-Quarters that are taught by one Instructor*** | | | | | | | | | | |
| Minority Interaction | 0.001 | 0.008 | 0.035 | 0.022 | 0.005 | 0.048 | -0.024 | -0.250 | -0.025 | -0.088 |
| | (0.012) | (0.010) | (0.039) | (0.018) | (0.026) | (0.035) | (0.046) | (0.226) | (0.087) | (0.214) |
| ***Course-Quarters without Variation in Instructor Underrepresented Minority Status*** | | | | | | | | | | |
| Minority Interaction | -0.014 | 0.023 *** | 0.097 *** | 0.045 *** | 0.003 | -0.010 | 0.041 | 0.073 | 0.042 | 0.056 |
| | (0.010) | (0.008) | (0.028) | (0.013) | (0.015) | (0.024) | (0.028) | (0.111) | (0.048) | (0.055) |
| ***Course-Years without Variation in Instructor Underrepresented Minority Status*** | | | | | | | | | | |
| Minority Interaction | -0.021 * | 0.012 | 0.065 * | 0.042 *** | 0.001 | -0.007 | 0.059 | 0.089 | 0.067 | -0.029 |
| | (0.013) | (0.010) | (0.036) | (0.016) | (0.019) | (0.033) | (0.041) | (0.175) | (0.074) | (0.075) |
| ***Excluding Language Courses*** | | | | | | | | | | |
| Minority Interaction | -0.018 *** | 0.008 | 0.039 *** | 0.019 *** | 0.014 ** | -0.027 ** | 0.022 * | 0.021 | 0.025 | 0.029 * |
| | (0.005) | (0.005) | (0.013) | (0.006) | (0.006) | (0.011) | (0.013) | (0.034) | (0.017) | (0.017) |
| ***Excluding Video-Delivered Courses*** | | | | | | | | | | |
| Minority Interaction | -0.015 *** | 0.012 *** | 0.052 *** | 0.025 *** | 0.011 * | -0.024 ** | 0.030 ** | 0.055 * | 0.033 ** | 0.024 |
| | (0.005) | (0.005) | (0.013) | (0.006) | (0.006) | (0.011) | (0.013) | (0.034) | (0.017) | (0.017) |
| ***Entering Students (==> Low Registration Priority)*** | | | | | | | | | | |
| Minority Interaction | - | - | - | - | | -0.027 | 0.029 | 0.050 | 0.038 | 0.025 |
| | | | | | | (0.025) | (0.029) | (0.090) | (0.048) | (0.048) |
| ***Vocational vs. Non-Vocational Courses*** | | | | | | | | | | |
| Minority Interaction | -0.025 *** | 0.011 * | 0.055 *** | 0.021 *** | 0.009 | -0.034 *** | 0.031 ** | 0.071 ** | 0.041 ** | 0.023 |
| | (0.006) | (0.006) | (0.014) | (0.007) | (0.007) | (0.012) | (0.014) | (0.036) | (0.018) | (0.017) |
| Minority Interaction****Vocational Course*** | 0.025 ** | 0.006 | -0.001 | 0.013 | -0.003 | 0.044 * | -0.020 | -0.138 | -0.060 | 0.064 |
| | (0.011) | (0.010) | (0.031) | (0.015) | (0.019) | (0.026) | (0.028) | (0.094) | (0.041) | (0.052) |
| ***Courses that are Transferable to UC and CSU Systems*** | | | | | | | | | | |
| Minority Interaction | -0.004 | 0.015 ** | 0.026 | 0.023 ** | 0.011 | -0.017 | 0.038 ** | 0.057 | 0.046 * | 0.041 |
| | (0.008) | (0.007) | (0.023) | (0.011) | (0.013) | (0.016) | (0.018) | (0.060) | (0.028) | (0.028) |
| Minority Interaction****Transferable Course*** | -0.025 *** | -0.005 | 0.038 | 0.001 | 0.000 | -0.020 | -0.017 | -0.010 | -0.019 | -0.015 |
| | (0.010) | (0.009) | (0.027) | (0.013) | (0.014) | (0.021) | (0.023) | (0.068) | (0.033) | (0.033) |
| ***FIXED EFFECTS:*** | | | | | | | | | | |
| Student | | | **Yes** | | | | | **Yes** | | |
| Classroom | | | **Yes** | | | | | **Yes** | | |

**NOTES:** This table explores the heterogeneity of our results across different student groups and types of courses considered. We report the coefficient of the interaction between student's and instructor's underrepresented minority status - referred to as "Minority Interaction". In cases where we allow minority effects to vary across student groups we also report the interaction between the main variable of interest and indicator variables that are equal to one if a student belongs to a certain subgroup. We only report results for our preferred specification, including student and classroom fixed effects. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. We consider 4 student outcomes: an indicator variation equal to one if the student drops the course; an indicator variable equal to one if the student passes the course; standardized course grades; and an indicator variable equal to one if the student has a grade of at least B. We also compute the regression coefficients for a sample of all students and a sample of students with a low standing on class enrollment lists. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered by classroom.

**TABLE 5 - UPPER AND LOWER BOUNDS FOR ESTIMATED ROLE OF INSTRUCTOR MINORITY STATUS FOR STUDENT GRADE**

| | TRUNCATION BY OVERALL DROPOUT BEHAVIOUR | | TRUNCATION BY COURSE-SPECIFIC DROPOUT BEHAVIOUR | |
|---|---|---|---|---|
| | **All Students** | **Low Registration Priority Students** | **All Students** | **Low Registration Priority Students** |
| Lower Bound | 0.039 *** | 0.029 | 0.042 *** | 0.033 |
| | (0.013) | (0.034) | (0.013) | (0.033) |
| Uncorrected Estimate | 0.054 *** | 0.050 | 0.054 *** | 0.050 |
| | (0.013) | (0.033) | (0.013) | (0.033) |
| Upper Bound | 0.079 *** | 0.093 *** | 0.072 *** | 0.063 * |
| | (0.013) | (0.033) | (0.013) | (0.034) |
| Student FE | Yes | | Yes | |
| Classroom FE | Yes | | Yes | |

**NOTES:** This table shows uncorrected and sample-selection corrected estimates for the minority interaction when grade is used as the outcome variable. We first estimate the minority interaction in dropout regressions (not shown in table). The estimate provides us with the x-percentage difference of the propensity to drop the course between minority and non-minority students when the class is taught by a minority instructor. We then calculate the x-percent and (100-x)-percent quantiles of the minority grade distribution in classes taught by minority instructors. To compute the upper bound on the interaction we drop minority students with grades below the x-percent quantile. To compute the lower bound we drop the students with grades above the (100-x) quantile. We report the coefficient of the minority interaction with standardized grade as outcome variable. We compute the regression coefficients for a sample of all students and a sample of low registration priority students. The first two columns report results when the trimming procedure relies on estimate of the minority interaction in dropout regressions that use the full sample; the last two columns report results when the trimming procedure relies on an estimate of the minority interaction in dropout regressions we run for each course separately; in the latter case we need to replace student fixed effects by student controls to achieve identification. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered by classroom.

**TABLE 6 - ESTIMATED ROLE OF INSTRUCTOR MINORITY STATUS FOR STUDENT OUTCOMES: ARE STUDENTS REALLY REACTING TO THE INSTRUCTOR?**

| | *ALL STUDENTS* | | | | | *LOW REGISTRATION PRIORITY STUDENTS* | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Dropped Course** | **Passed Course** | **Grade (Standar-dized)** | **Good Grade (B or higher)** | **Takes Same-Subject Course Subsequently** | **Dropped Course** | **Passed Course** | **Grade (Standar-dized)** | **Good Grade (B or higher)** | **Takes Same-Subject Course Subsequently** |
| ***Different Age Groups of Students*** | | | | | | | | | | |
| Minority Interaction****Student younger than 21.5 years*** | -0.018 *** | 0.007 | 0.039 ** | 0.017 ** | 0.010 | -0.028 ** | 0.039 *** | 0.077 * | 0.042 ** | 0.030 |
| | (0.007) | (0.007) | (0.016) | (0.008) | (0.008) | (0.013) | (0.016) | (0.041) | (0.020) | (0.019) |
| Minority Interaction****Student between 21.5 and 35 years*** | -0.001 | 0.011 | 0.038 | 0.015 | -0.003 | 0.011 | -0.022 | -0.067 | -0.023 | -0.007 |
| | (0.009) | (0.009) | (0.023) | (0.011) | (0.011) | (0.020) | (0.022) | (0.070) | (0.033) | (0.031) |
| Minority Interaction****Student older than 35 years*** | -0.017 | -0.005 | -0.050 | -0.020 | 0.009 | -0.033 | -0.061 * | -0.125 | -0.046 | 0.009 |
| | (0.016) | (0.013) | (0.044) | (0.020) | (0.026) | (0.034) | (0.036) | (0.135) | (0.057) | (0.070) |
| ***Different Class-Level Fractions of Minority Students who do not have a low standing on Course Enrolment Lists*** | | | | | | | | | | |
| Minority Interaction****Fraction smaller than 25%*** | - | - | - | | - | -0.002 | 0.001 | -0.094 | -0.029 | 0.029 |
| | | | | | | (0.030) | (0.031) | (0.095) | (0.048) | (0.023) |
| Minority Interaction****Fraction between 25% and 75%*** | - | - | - | | - | -0.045 *** | 0.016 | 0.054 | 0.036 | 0.041 |
| | | | | | | (0.016) | (0.017) | (0.047) | (0.024) | (0.050) |
| Minority Interaction****Fraction larger than 75%*** | - | - | - | | - | 0.038 * | 0.024 | 0.002 | -0.005 | -0.008 |
| | | | | | | (0.022) | (0.025) | (0.065) | (0.032) | (0.031) |
| ***FIXED EFFECTS:*** | | | | | | | | | | |
| Student | | | **Yes** | | | | | **Yes** | | |
| Classroom | | | **Yes** | | | | | **Yes** | | |

**NOTES:** This table investigates if our estimated minority effects are really driven by students reacting to the instructor. The two alternative hypotheses we consider are (a) instructor-driven discrimination and (b) peer effects. We report the coefficient of the interaction between student's and instructor's underrepresented minority status - referred to as "Minority Interaction". We also report the interaction between the main variable of interest and indicator variables that are equal to one if a student belongs to a certain subgroup. We only report results for our preferred specification, including student and classroom fixed effects. A classroom is defined by a section of a course offering during a particular academic year-quarter. Students and instructors belong to the group of "Underrepresented Minorities" if their race/ethnicity is reported to be Hispanic, African-American, or Native American, Pacific Islanders, or other non-Whites. We consider 4 student outcomes: an indicator variation equal to one if the student drops the course; an indicator variable equal to one if the student passes the course; standardized course grades; and an indicator variable equal to one if the student has a grade of at least B. We also compute the regression coefficients for a sample of all students and a sample of students with a low standing on class enrollment lists. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered by classroom.

**TABLE 7 - ESTIMATED ROLE OF INSTRUCTOR RACE/ETHNICITY FOR STUDENT OUTCOMES, USING A SAMPLE WITH FOUR RACE/ETHNICITY-GROUPS ONLY**

| | All Students | | | | All Low Registration Priority Students | | | |
|---|---|---|---|---|---|---|---|---|
| | *Instructor Race/Ethnicity* | | | | *Instructor Race/Ethnicity* | | | |
| | *White* | *African-American* | *Hispanic* | *Asian* | *White* | *African-American* | *Hispanic* | *Asian* |
| ***OUTCOME:STUDENT DROPPED COURSE*** | | | | | | | | |
| ***Number of Observations:*** | | ***418,283*** | | | | ***122,887*** | | |
| ***Student Race/Ethnicity*** | | | | | | | | |
| *White* | NOT IDENTIFIED | | | | NOT IDENTIFIED | | | |
| *African-American* | | -0.078 *** | -0.018 | 0.011 | | -0.083 *** | -0.018 | 0.092 *** |
| | | (0.016) | (0.017) | (0.015) | | (0.034) | (0.038) | (0.030) |
| *Hispanic* | NOT IDENTIFIED | -0.019 * | -0.025 *** | 0.022 *** | NOT IDENTIFIED | -0.007 | -0.042 * | 0.050 *** |
| | | (0.011) | (0.010) | (0.009) | | (0.023) | (0.022) | (0.019) |
| *Asian* | | -0.016 ** | -0.011 | -0.014 ** | | 0.008 | -0.003 | -0.003 |
| | | (0.008) | (0.008) | (0.006) | | (0.017) | (0.018) | (0.014) |
| ***F-test: Own-Race/Ethnicity Effect (P-value)*** | | | ***0.000*** | | | | ***0.023*** | |
| ***F-test: Race/Ethnicity-Effect (P-value)*** | | | ***0.000*** | | | | ***0.000*** | |
| ***OUTCOME: STUDENT PASSED COURSE, CONDITIONAL ON FINISHING THE COURSE*** | | | | | | | | |
| ***Number of Observations:*** | | ***300,503*** | | | | ***89,031*** | | |
| *White* | NOT IDENTIFIED | | | | NOT IDENTIFIED | | | |
| *African-American* | | 0.067 *** | -0.013 | -0.009 | | 0.094 *** | 0.038 | -0.010 |
| | | (0.015) | (0.016) | (0.014) | | (0.034) | (0.046) | (0.032) |
| *Hispanic* | NOT IDENTIFIED | 0.020 ** | 0.009 | -0.026 *** | NOT IDENTIFIED | 0.066 *** | 0.023 | -0.008 |
| | | (0.010) | (0.009) | (0.008) | | (0.025) | (0.025) | (0.020) |
| *Asian* | | 0.007 | 0.000 | 0.004 | | 0.010 | 0.017 | 0.015 |
| | | (0.007) | (0.006) | (0.005) | | (0.018) | (0.017) | (0.014) |
| ***F-test: Own-Race/Ethnicity Effect (P-value)*** | | | ***0.000*** | | | | ***0.025*** | |
| ***F-test: Race/Ethnicity-Effect (P-value)*** | | | ***0.000*** | | | | ***0.041*** | |
| ***OUTCOME: STANDARDIZED STUDENT COURSE GRADE, CONDITIONAL ON FINISHING THE COURSE*** | | | | | | | | |
| ***Number of Observations:*** | | ***260,466*** | | | | ***70,871*** | | |
| *White* | NOT IDENTIFIED | | | | NOT IDENTIFIED | | | |
| *African-American* | | 0.190 *** | 0.015 | 0.012 | | 0.157 | 0.068 | 0.045 |
| | | (0.040) | (0.046) | (0.033) | | (0.107) | (0.154) | (0.085) |
| *Hispanic* | NOT IDENTIFIED | 0.071 *** | 0.096 *** | -0.026 | NOT IDENTIFIED | 0.105 | 0.089 | -0.040 |
| | | (0.027) | (0.027) | (0.020) | | (0.068) | (0.075) | (0.057) |
| *Asian* | | 0.054 *** | 0.011 | 0.049 *** | | 0.067 | 0.074 | 0.021 |
| | | (0.020) | (0.019) | (0.014) | | (0.054) | (0.052) | (0.040) |
| ***F-test: Own-Race/Ethnicity Effect (P-value)*** | | | ***0.000*** | | | | ***0.291*** | |
| ***F-test: Race/Ethnicity-Effect (P-value)*** | | | ***0.000*** | | | | ***0.587*** | |

***OUTCOME: GOOD GRADE (B OR HIGHER), CONDITIONAL ON FINISHING THE COURSE***

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| ***Number of Observations:*** | | ***260,707*** | | | | ***70,925*** | | |
| *White* | | NOT IDENTIFIED | | | | NOT IDENTIFIED | | |
| *African-American* | | 0.090 *** | 0.025 | 0.007 | | 0.129 *** | 0.044 | 0.025 |
| | | (0.020) | (0.021) | (0.017) | | (0.047) | (0.072) | (0.042) |
| *Hispanic* | NOT IDENTIFIED | 0.029 ** | 0.039 *** | 0.001 | NOT IDENTIFIED | 0.063 * | 0.013 | -0.010 |
| | | (0.014) | (0.013) | (0.011) | | (0.033) | (0.037) | (0.029) |
| *Asian* | | 0.009 | 0.006 | 0.028 *** | | 0.035 | 0.003 | 0.006 |
| | | (0.010) | (0.009) | (0.007) | | (0.026) | (0.025) | (0.020) |
| ***F-test: Own-Race/Ethnicity Effect (P-value)*** | | | ***0.000*** | | | | ***0.051*** | |
| ***F-test: Race/Ethnicity-Effect (P-value)*** | | | ***0.000*** | | | | ***0.366*** | |

***OUTCOME: STUDENT ENROLS IN A SAME-SUBJECT COURSE IN THE SUBSEQUENT TERM***

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| ***Number of Observations:*** | | | | | | | | |
| *White* | | NOT IDENTIFIED | | | | NOT IDENTIFIED | | |
| *African-American* | | 0.019 | 0.018 | -0.009 | | 0.058 | 0.054 | -0.059 |
| | | (0.020) | (0.020) | (0.017) | | (0.048) | (0.063) | (0.043) |
| *Hispanic* | NOT IDENTIFIED | 0.008 | -0.006 | -0.006 | NOT IDENTIFIED | 0.012 | 0.051 | 0.006 |
| | | (0.012) | (0.013) | (0.011) | | (0.034) | (0.037) | (0.029) |
| *Asian* | | 0.002 | -0.008 | -0.002 | | 0.018 | 0.007 | 0.031 |
| | | (0.009) | (0.010) | (0.007) | | (0.026) | (0.029) | (0.021) |
| ***F-test: Own-Race/Ethnicity Effect (P-value)*** | | | ***0.734*** | | | | ***0.160*** | |
| ***F-test: Race/Ethnicity-Effect (P-value)*** | | | ***0.903*** | | | | ***0.353*** | |

| ***Fixed Effects:*** | | |
|---|---|---|
| Student FE | Yes | Yes |
| Classroom FE | Yes | Yes |

**NOTES:** This table displays results from outcome regressions in which we allow for interactions between all observed student and instructor races/ethnicities. We report the full set of 9 identified interactions for each regression. Same-Race/Ethnicity interactions are shown in red. We only show results for our preferred specification that includes student and classroom fixed effects. P-values for a F-test of the existence of same-race/ethnicity interactions and for the existence of any race/ethnicity-interactions are also listed. A classroom is defined by a section of a course offering during a particular academic year-quarter. We consider 4 student outcomes: an indicator variation equal to one if the student drops the course; an indicator variable equal to one if the student passes the course;standardized course grades; and an indicator variable equal to one if the student has a grade of at least B. We also compute the regression coefficients for a sample of all students and a sample of students with a low standing on class enrollment lists. *** Significant on 1%-level; ** Significant on 5%-level; * Significant on 10%-level. Standard errors are clustered by classroom.