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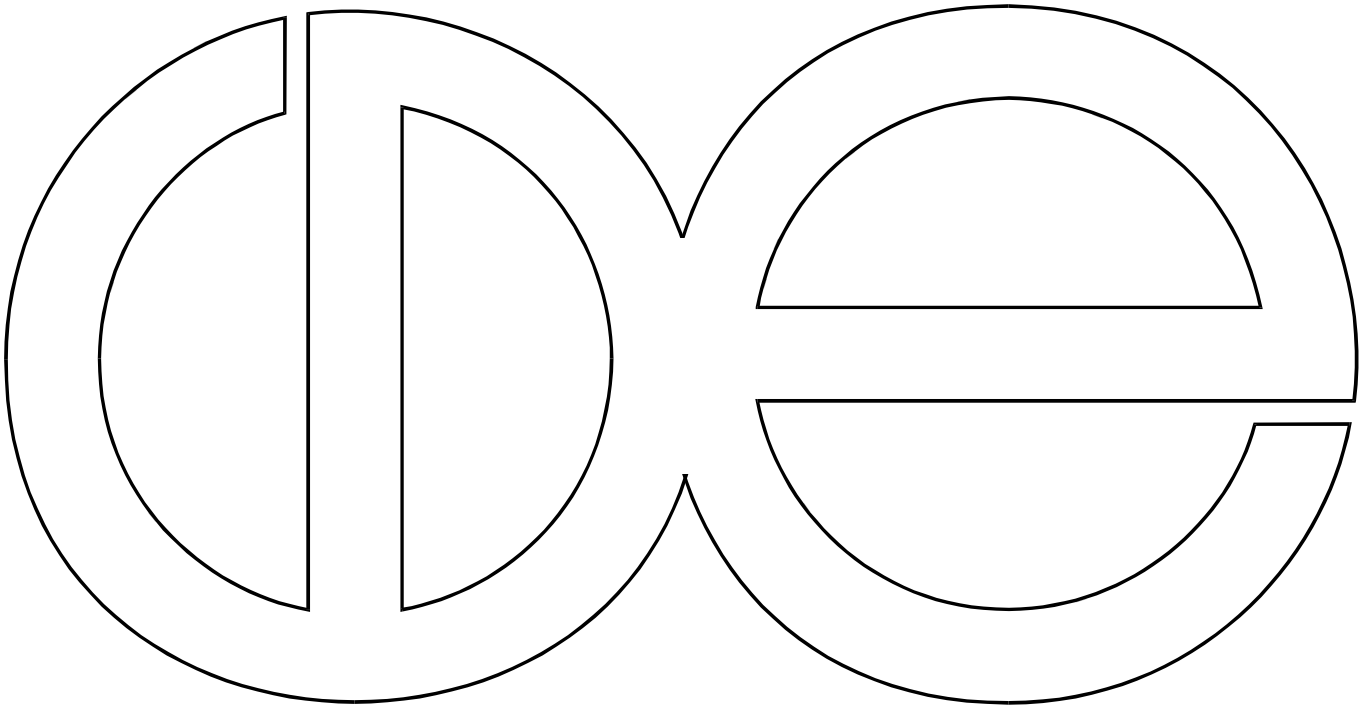

*MISMATCH AND THE PATERNALISTIC JUSTIFICATION FOR SELECTIVE COLLEGE ADMISSIONS**

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Although some scholars report that all students are better served by attending more prestigious postsecondary institutions, others have argued that students are better off attending colleges where they are about average in terms of academic ability and suffer worse outcomes if they attend a school "out of their league" at which they are "overmatched." The latter argument is most frequently deployed as a paternalistic justification for ending affirmative action. We take advantage of a natural admissions experiment at the University of California to test the effect of being overmatched for students on the margin of admission to elite universities. Consistent with the mismatch hypothesis, we find that students accumulate more credits when they attend a less demanding institution. However, students do not earn higher grades and are no more or less likely to drop out of a school where they are overmatched, and are less likely to drop out than they would have been had they attended a less demanding institution.

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MISMATCH AND THE PATERNALISTIC JUSTIFICATION FOR SELECTIVE COLLEGE ADMISSIONS

The legal battle over race-based affirmative action once again engaged the Supreme Court in 2013 via the *Fisher v. University of Texas* case. One of the Petitioner's central claims is that the pernicious effects of mismatch lead race-conscious admissions policies to harm minority students and "engender such high costs that they cannot be constitutionally justified" (American Educational Research Association 2012). However, how we identify mismatch, and how we conceive of its potential influence on both student outcomes and on the goals of public flagship institutions, remain open questions. This paper offers important new evidence on the issue and extends existing work in multiple ways. First, our empirical identification of mismatch takes advantage of a rare policy event that affords us exogenous variation in the college match process. Second, we employ a unique administrative dataset from a large system of selective public flagship institutions. Finally, we offer a different conceptual framing for why public institutions may be (or should be) concerned with mismatch. The potential benefits, or harms, of the selection process on student outcomes have important implications for students, their families, and the taxpayers who subsidize public universities.

Although the logic of the mismatch argument is colorblind, we have not been able to find an instance in which the mismatch argument has been deployed by advocates out of concern for White or Asian students.[1] Nonetheless, we argue that exclusion based on a mismatch argument poses a question that extends beyond the issue of affirmative action: Who should enjoy the privilege of attending the nation's most elite colleges and universities? While the question may

[1] In an empirical study, though, Massey and Mooney (2007) did consider the effect of academic mismatch on legacy and athlete admits to elite schools. They found that mismatch at the institutional level exerts a very small positive effect on the conditional odds of departing from college by the junior year for athletes and legacies but not for minority students.

be largely of academic interest in the case of private institutions, at the nation's premiere public flagship universities it is of both academic and public policy interest.

Public colleges and universities were created under the Morrill Act of 1862 to serve the citizens of their state, with the explicit goal of retaining a share of the educated population of students they produce in their vicinity (Brown 1995). As the most elite publicly supported institutions, flagships are important sites of knowledge and status production, charged with educating the most promising young high school graduates in the state they serve and feeding the state economy upon which they depend (Bowen, Chingos, and McPherson 2009). Elite public universities occupy a unique space: they are public resources available only to selective segments of the population. Over the years they have tried to balance their competing roles of exclusion (elite) and inclusion (public) by offering various tuition subsidies to economically disadvantaged students, implementing race-based affirmative action policies, and trying to draw students more evenly from across the state's public high schools.

Today, state flagship universities, like other public services, are under tremendous economic pressure and must walk a fine line, with some drawing an increasing share of their students from other states to increase tuition revenue while at the same time responding to the charge to educate their own populace (Geiger 2007). This influx of out-of-state students may complicate efforts of state flagships to satisfy their dual mission of serving those best prepared to succeed in college and those who constitute the eligible student-aged population in the state. In addition, race-based affirmative action practices in admission at selective public flagships have been under great legal and political scrutiny, with public institutions scaling back their affirmative action programs over time (Grodsky and Kalogrides 2008).

College Selectivity and the Mismatch Hypothesis

Prior research suggests that students have good reasons to want to attend elite private and public universities. More selective institutions appear to have a higher payoff in terms of persistence to degree completion (Alon and Tienda 2005; Bowen, Chingos, and McPherson 2009; Small and Winship 2007), graduate or professional school attendance (Mullen, Goyette, and Soares 2003), and earnings later in life (Black and Smith 2006; Hoekstra 2009; Long 2008). Although empirical work on the contribution of college selectivity to later earnings is mixed (Brand and Halaby 2006; Dale and Krueger 2002; Dale and Krueger 2011), students and parents certainly seem to think that school quality matters. Between 2002 and 2008 Princeton and Stanford averaged nine applications per student admitted. Among the most elite public universities, competition was less extreme but still formidable; the Berkeley and Los Angeles campuses of the University of California (UC) had the lowest acceptance rates of all public universities during this period, averaging about four applications per student admitted.[2]

The most important criteria for selection into competitive public and private colleges and universities are measures of prior academic achievement. These include a student's high school academic grade point average (GPA), scores on college entrance exams, and the rigor of the courses completed during high school. These criteria are motivated by two rationales. First, many believe that more prestigious colleges and universities make greater demands on their students than less competitive institutions. Students whose secondary school record place them substantially below their college peers are said to be "overmatched" and therefore at greater risk of academic failure or attrition than they would be at a less demanding college or university.

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[2] Based on the National Center for Education Statistics' Integrated Postsecondary Education Data System for 2002, 2004, 2006, and 2008.

According to this logic, it is in the student's own interest to attend a college at which she is about average, or at least not too far below average (Manski and Wise 1983). We refer to this viewpoint as the *paternalistic justification* for exclusion.

The second rationale for selecting students based on measures of prior academic achievement is rooted in the principle of fair competition. Writing a half-century ago about different types of social mobility, Turner (1960) defined contest mobility as a "system in which elite status is the prize in an open contest and is taken by the aspirants' own efforts" (856). Today, admission to an elite college or university can be thought of in the same way as a contest in which few of the competitors can achieve victory. A transparent and seemingly fair way to adjudicate among the winners and losers is to compare them on prior academic achievement. Those who have achieved the highest levels of success in high school are deemed most worthy of attending an elite postsecondary institution. We refer to this viewpoint as the *contest justification* for exclusion. Given well-documented inequities in school resources (including teacher and peer quality), as well as resources located in students' neighborhoods and families, the contest justification is deeply problematic. In fact, policies such as race-based or income-based affirmative action, or percent plans that provide some preference to the top achievers of all high schools, are rooted in the notion that the contest may not be so fair. In this paper we offer an empirical test of the paternalistic justification for exclusion but return to the contest justification in our discussion and conclusion.

The Paternalistic Justification and the Overmatch Hypothesis

The paternalistic justification gives rise to the overmatch hypothesis, which asserts that students are harmed by attending colleges and universities at which their level of prior academic achievement is substantially below the mean (Light and Strayer 2000; Sander and Taylor Jr. 2012; Thernstrom and Thernstrom 1997). This justification for exclusion is frequently employed

by critics of race-based affirmative action who posit that beneficiaries of affirmative action are actually hurt by the policies; they are mismatched with the demands of the university and ultimately pay a penalty through academic failure, dropout, or weaker employment opportunities down the road (Clegg and Thompson 2012; Thernstrom and Thernstrom 1997; Will 2011). For example, Clegg wrote: "among the victims of admission preferences for blacks and Latinos are … blacks and Latinos. That is, by mismatching students and institutions, students are set up for failure." More recently, in their book *Mismatch*, Sander and Taylor (2012) suggest that the unintended consequences of racial preferences, which they argue systematically put minority students in academic environments where they feel overwhelmed, is greater academic failure and stigma on the part of minority students.

The empirical manifestation of mismatch, however, is unclear. Some past research suggests, or at least has assumed, that each increment in prior academic achievement is associated with an identical change in the outcome (performance, credit attainment, probability of graduation, etc.). For example, Massey and Mooney (2007) and Fischer and Massey (2007) operationalized mismatch as the difference between individual students' SAT scores and the mean SAT scores for the college they attend. This implies that mismatch is a matter of magnitude—at some perhaps arbitrary location on the distribution of prior achievement (centered on mean prior achievement of other students attending a particular college), students reach a point beyond which they are over- or undermatched. We suggest instead that over- or undermatch must be a function of nonlinearities in the relationship between past achievement relative to college peers and postsecondary outcomes. Otherwise the concept of mismatch has little analytic value as it reflects nothing qualitatively different about the prior achievement-outcome relationship for the matched than it does for the mismatched. Are less academically prepared students ill served by attending elite colleges and universities? Despite the importance of the question, past empirical

research on this topic is surprisingly sparse. This is in large measure due to the challenges inherent in using observational data to understand the consequences of different types of postsecondary enrollment for students' academic outcomes. Descriptive statistics show a clear advantage in the probability of completing a degree for students who attend more selective colleges and universities. For example, Hess et al. (2009) reported that the average 6-year graduation rate in 2007 of institutions classified as "most competitive" by *Barron's College Guide* was 87.8%, compared to 48.6% for "competitive" colleges and 34.7% for "noncompetitive" colleges (p. 9). Hess et al. (2009) noted that these statistics mask substantial within-group heterogeneity in graduation rates, a point consistent with Bowen and Bok's work documenting variation in the probability of graduating from one's initial college or university for students attending one of the 36 selective colleges and universities included in the College and Beyond database (Bowen and Bok 1998) and the 21 public flagship universities represented in Bowen, Chingos, and McPherson's more recent work (Bowen, Chingos, and McPherson 2009).

Moving beyond purely descriptive estimates, some authors have conditioned parametrically or non-parametrically on key measures of prior academic achievement to address the overmatch hypothesis. For example, Bowen and Bok compared graduation rates within 100 point bands on matriculates' combined SAT scores to show that graduation rates increase across levels of college selectivity for both African American and White students (Bowen and Bok 1998). Using multiple regression to adjust for other differences among students, Bowen, Chingos, and McPherson (2009) showed that attending a more selective college increases the probability that a student will complete her degree regardless of her high school GPA. Similarly, Massey and Mooney (2007) found that individual mismatched students (measured as the difference between an individual's SAT score and the institution's average SAT scores) do not have higher probabilities of dropping out, although in some cases they may pay a penalty in lower grades.

Several other rigorous studies have investigated college selectivity and the mismatch hypothesis, attending to the inherent selection problem in various ways. Alon and Tienda (2005) utilized three large-scale surveys to assess the impact of institutional selectivity for students from different racial and ethnic backgrounds. They pursued three strategies to account for the fact that selectivity of the institution is endogenous to characteristics of students that contribute to their subsequent success in college. Two of these strategies are based on adjusting for the propensity to attend a selective college, estimated as a function of parental education and income, SAT scores and high school grades, type of high school, and other characteristics known to influence college entrance. In one version of the propensity adjustment they include the propensity as a covariate in a model predicting baccalaureate completion within 6 years of initial entry; in another version they employ nearest neighbor matching based on the same propensity score and compare mean graduation rates for students attending selective and nonselective (or less selective) institutions. Finally, in an effort to adjust for unobservable differences among students who do and do not attend (more) selective institutions, Alon and Tienda estimated a bivariate probit for selective college attendance and baccalaureate completion. Across the different estimation strategies their results suggest a positive effect of institutional selectivity on students' odds of graduation.

Alon and Tienda provided compelling evidence for the independent effects of institutional selectivity on baccalaureate completion, consistent with other work on the topic (Bowen, Chingos, and McPherson 2009; Massey and Mooney 2007; Small and Winship 2007). Yet, if one defines mismatch as we do, then this implies a nonlinearity in the effects of academic preparation and other attributes on outcomes, a situation in which the *effect* of the difference between individual student achievement and school mean achievement on key outcomes increases in magnitude as those differences get larger.

Alon and Tienda explicitly rejected the "narrow interpretation" of mismatch that we favor (footnote 3, p. 310), opting instead to use race and ethnicity as proxies for mismatch. While we recognize Alon and Tienda's contribution to the analysis of the effects of affirmative action and institutional selectivity more broadly, in this paper we employ a more explicit test of mismatch on both observed and unobserved attributes of students.

Light and Strayer's (2000) investigation of the effect of student match and college selectivity on graduation rates conforms more closely to our definition of mismatch. To address the selection problems in estimating the effect of college enrollment decisions on college success, they jointly estimated a multinomial probit model of college attendance at various selectivity levels and the probability of graduation. Their strategy allowed them to assess the causal effect of both college selectivity and mismatch on degree completion through a set of interactions between observed ability and selectivity levels. However, Light and Strayer had to approximate a matching criterion based on students' observed AFQT quartile, allowing the error terms to be correlated across the different points in time—attendance and graduation—and across different college selectivity levels. Contrary to Alon and Tienda (2005), Light and Strayer (2000) found that students have a higher probability of graduation if their observed ability levels match their college destination.

Testing the Mismatch Hypothesis at the University of California

In this paper we revisit the mismatch hypothesis, taking advantage of a unique natural experiment that occurred at UC in 2004. At that time the UC was comprised of eight campuses, three that we consider highly selective (Berkeley, Los Angeles, and San Diego) and five that we

consider moderately selective (Davis, Irvine, Riverside, Santa Barbara, and Santa Cruz).[3] Of the three campuses we consider highly selective, *Barron's* ranked two as "most competitive" in 2004 and the other (San Diego) as "highly competitive" (Schmitt 2009). The average acceptance rate of our highly selective campuses was 30% in that year, compared to 59% for our moderately selective campuses.[4]

Until recently, the UC sought to admit all students it regarded as "UC eligible," though not necessarily to the campus of their choice. To be UC eligible students must earn a minimum grade-point average on a specified set of high school courses (known as a-g courses) and exceed threshold scores on standardized admissions tests (SAT or ACT) (Douglass 1999). In 2004, however, as a result of state budget cuts, several thousand students eligible to attend UC were denied immediate admission to the University. Instead, these students were offered admission through the Guaranteed Transfer Option (GTO) program, guaranteeing them future admission to a *specific campus* conditional on successfully completing lower-division requirements at a California Community College. The highly selective campuses in particular were urged by the Office of the President to reconsider students that they initially rejected and they complied, offering GTO admission to 2,300 students. When the state budget was restored that summer, GTO students were offered immediate admission (or admission in the next term) to the campus promised to them under GTO. Figure A1 in the appendix displays the application path to obtaining an offer of regular admission or admission via the GTO program in 2004.[5]

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[3] The first cohort to enter UCs ninth campus, Merced, started in 2005.

[4] Authors' calculation based on UC Office of the President. 2010. "Final summary of freshman applications, admissions and enrollment, fall 1989–2009." (http://www.ucop.edu/news/factsheets/flowfrc_09.pdf).

[5] All campuses participated in the GTO program. However, two campuses, Davis and Santa Cruz, offered GTO students regular admission early in summer 2004 as space became available. Other campuses made

This unusual process resulted in the eventual admission of the GTO group to several highly selective UC institutions and presents a "natural experiment" in which the most selective campuses admitted applicants they originally rejected. These students are the most marginal admits—marginal on both observed attributes (as we demonstrate below) and attributes unobserved to the analyst.

These unobserved attributes can have real effects on admission to the UC. Starting in 2002, for example, the Berkeley campus formalized a process of "comprehensive review" under which "students who overcame obstacles to achievement got special consideration" (Hout 2005: p. 4). Hout found that students who undertook challenging courses of study were rewarded by admissions personnel for their efforts. It is therefore possible that students who *appear* mismatched based on the academic achievement criteria we observe may actually have other attributes unobserved by us that compensate for relative shortcomings on grades and test scores.

Exploiting this highly unusual admissions experiment, we asked what outcomes we would expect of the GTO students had they not been admitted to these competitive schools but instead attended less competitive UC campuses. The unique identification of these marginal admits freed us from relying on the standard parametric assumptions and statistical corrections for unobserved factors that lead students to choose schools and schools to choose students. On the student side, we observed a rich set of academic and socioeconomic characteristics, as well as the full UC application set, while on the institution side we had a natural experiment in admissions under which we could observe which matriculating students were least desirable from the perspective of the institution. The admissions game is tricky to observe; admissions officers at competitive

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GTO offers to both applicants and non-applicants, resulting in much lower yields. Since our interest is in overmatching we focus solely on GTO students who were admitted by selective campuses.

institutions are looking for "fit," weighing institutional goals against student capabilities (Steinberg 2002; Stevens 2009; Toor 2001). It is difficult for analysts to know or observe all the criteria schools consider, and nearly impossible to know the weights they attach to these criteria in choosing whom to admit. Our natural experiment allowed us to bypass this difficulty of parameterizing mismatch and simply assess mismatch as the differences in outcomes for those admitted *ex post* (below the standard admit bar) and those admitted under the standard admissions procedure (above the bar).

Data

Under a data sharing agreement with the University of California Office of the President, we have access to the census of students who applied to UC in 2004. Data files include detailed information about students from their admission and financial aid application files (academic background measures, individual demographic characteristics, family income, secondary school identifiers, and whether the student was a regular admit, GTO admit, or non-admit), and the postsecondary enrollment outcomes by term of students who matriculated at a UC campus (including number of credits attempted, number of credits earned, course information, GPA, and declaration of major).

Our data span the fall 2004 term through spring 2008, when students would be in their 4th year of study. Given that Berkeley is on the semester system and other UCs in our sample are on the quarter system, these 4 years represent a different number of total terms depending on the institution. In addition, two campuses admit a portion of their freshman class to start in the second term, (spring semester for Berkeley, and winter quarter for San Diego), so some GTO students had a one-term delay in their enrollment. A majority of these students, however, enrolled at a community college for that term and transferred those credits in.

We were interested in how mismatch might influence three main outcomes: (1) cumulative GPA by spring 2008; (2) dropout by spring 2008; and (3) cumulative credits earned after the first term of enrollment by spring 2008. We assumed that students who earned enough credits to graduate by spring 2008, and were no longer in the data, had actually graduated.[6] We preferred cumulative credits earned following the first semester of enrollment rather than including the first semester of enrollment to condition out differences in the number of AP and dual enrollment credits students may have transferred into the UC.

We considered students to be mismatched if they were admitted to a competitive campus through the GTO program (Berkeley, Los Angeles, and San Diego). In addition to GTO status (our proxy for mismatch), we adjusted for a variety of other student characteristics in our analyses, including race/ethnicity, gender, socioeconomic status, and academic achievement, as well as information about students' application choice set and major.

In order to evaluate the mismatch hypothesis we asked what outcomes we would expect of the GTO group had they not attended these competitive schools. To answer this question we compared GTO students at the three highly selective, or "elite" UC campuses with two reference groups of more closely matched students: (1) students at these institutions who were admitted via the traditional admissions process (i.e., most competitive UC applicants); and (2) students more observationally similar to the GTO admits who attended less selective, or "non-elite" UCs (i.e., institutions where the GTO students would be more closely matched). Figure 1 illustrates our comparison groups of interest.

<<INSERT FIGURE 1 ABOUT HERE>>

[6] To test the sensitivity of our results to this assumption we also estimated models in which we censored those who left after earning sufficient credits to graduate. Results, available from the second author on request, are substantively identical to those we present here.

Figures 2 and 3 compare distributions of students' "academic index scores" by GTO status. The academic index score, used by UC campuses to make admissions decisions, is a linear combination of UC-weighted high school GPA and SAT scores.[7] Figure 2 compares the academic index scores for students attending the three elite UC institutions by GTO status. The median academic index score for GTO students attending elite UCs (plotted by the solid line) is equivalent to the 7th percentile of the traditionally admitted group at these same institutions. Alternatively, the median academic index score for non-GTO admits is the same as that earned by GTO admits at the 98th percentile. The typical GTO admit (and even the relatively high-achieving GTO admit) is severely overmatched to traditionally admitted students at these elite institutions. The index score distribution of GTO students aligns more closely with that of students attending the six non-elite UCs, as shown in Figure 3. Even in this comparison, however, GTOs come out a bit behind. The median score for GTO students attending elite universities aligns with the 37th percentile of non-GTO students attending less competitive universities. Alternatively, the typical non-GTO student attending a less competitive university earns an academic index score equivalent to the 68th percentile of the elite GTO distribution. While GTO students are not so far from the typical non-GTO student at less competitive campuses, they are less prepared to succeed at even these universities than traditionally admitted students.

Analytic Strategy

Following the logic of a case control study, we compared GTO students to students in each of the two reference groups based on observed characteristics, including academic achievement. As an additional check on student self-selection we followed Dale and Krueger (2002; 2011) in

[7] The academic index is equal to High School Weighted GPA*1000 + Average of all SAT scores available (SATI–Verbal, SATI–Math, SAT II–Math, and SAT II–Writing).

adjusting for the observed set of institutions to which students submitted an application. Dale and Krueger argue that the application set reflects intuitions by students (or "self-revelation" in their terms) about their own academic potential; students who apply to more selective colleges and universities do so because they believe they can succeed in such environments (2002). While we would have preferred to adjust for the entire application set, we only observed the UC campuses to which students applied. Thus we only partially observed the "self-revelatory" dimension of postsecondary preparedness.

Our basic model specification is as follows:

$$Y_i = \alpha_{0,rg1} + \alpha_1(GTO\ Admit_i) + \beta X_i + \varepsilon_i \quad (1)$$

(Reference Group 1 (*rg1*)—traditional admits at elite UC campuses)

$$Y_i = \alpha_{0,rg2} + \alpha_1(GTO\ Admit_i) + \beta X_i + \varepsilon_i \quad (2)$$

(Reference Group 2 (*rg2*)—traditional admits at non-elite UC campuses)

where Y_i is cumulative GPA, credits accumulated or persistence by spring 2008 for student *i*. The parameter of interest is α_1, which represents the conditional difference in our outcomes between mismatched students at elite UC campuses (GTOs) and two different reference groups: traditional admits at elite UC campuses ($\alpha_{0,rg1}$) and traditional admits at non-elite campuses ($\alpha_{0,rg2}$). X_i is a vector of student-level covariates including measures of academic achievement, a set of race/ethnicity indicator variables, gender, logged family income, parental education, and, in the case of models of credit accumulation, number of terms the student has been enrolled; and ε_i represents unobserved predictors of Y_i for student *i* assumed to be orthogonal to other predictors. To investigate GPA and credit accumulation we fit OLS models, and to explore the binary outcome of persistence by spring 2008, we fit a logistic function, coding those who

graduate or are still enrolled as 0.[8] Our measures of academic achievement included a student's UC-weighted high school GPA, SAT I math and verbal scores, and SAT II math and writing scores. We used constant substitution to deal with missing SAT scores.[9] Some model specifications included a vector of fixed effects for a student's application choice set and major. Finally, we adjusted equations (1) and (2) to include race/ethnicity interactions in order to test whether the effects of mismatch for students attending elite UC campuses varied across racial/ethnic groups.

Not all students who received a GTO offer from a campus decided to take it. Of the 2,306 students who received a GTO offer from Berkeley, Los Angeles, or San Diego, only 491 (21%) accepted the offer. GTO offers were made late in the admissions cycle, with offers for Berkeley GTO students going out in June.[10] In order to accept the offer many students would have had to break commitments they had already made to attend other colleges or universities, likely sacrificing their enrollment deposits in the process. This effectively raises the monetary and psychic costs of attending the institution that made the GTO offer.

How different are those who accepted the GTO "treatment" from those who declined the GTO offer? Appendix Table A1 details the differences in means on academic and demographic background characteristics for GTO compliers and non-compliers based on their application information. Students who accepted the GTO offer (compliers) were statistically indistinguishable from those who declined (non-compliers) in mean high school GPA, SAT verbal scores, parental education, and median income. Males were slightly more likely than

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[8] We do not directly observe graduation, but instead assume observations who leave after they have accrued enough credits to exceed the credits required by their university to graduate have actually graduated.

[9] Less than 1% of all students were missing scores on the SAT.

[10] Sam Agronow, UC Office of the President, personal communication, 2/17/09.

females, and Asian students substantially more likely than White students, to accept the GTO offer. The average SAT math scores of non-compliers were slightly lower (15 points) than the average math scores of compliers. To account for these observable group differences we adjusted for significant predictors of compliance in our models. We revisit the implications of self-selection into GTO compliance in the discussion section.

Results

We summarize the differences in means on the outcome measures and predictors for GTO students and the comparison groups (non-GTO students at elite and non-elite UCs) in Table 1.[11] The descriptive results are largely consistent with the mismatch hypothesis. GTO students earn college GPAs that average four-tenths of a point lower than those earned by traditionally admitted elite students, while GTO students earn GPAs that are slightly less than a quarter of a point lower than those earned by traditionally admitted students at non-elite UC campuses. GTO students also accrue fewer terms of enrollment at UC and fewer total credits than non-GTO students attending either elite or non-elite campuses. They are five percentage points more likely to leave their initial university without earning enough credits to graduate than traditionally admitted students attending elite universities; however, they are seven percentage points *less likely* to depart than students at non-elite campuses. Turning to students' background characteristics, GTO students have lower high school GPAs and SAT II scores than either regularly admitted elite students or non-elite students. Their SAT I scores are significantly lower than those of regularly admitted elite students but not significantly distinguishable from students attending non-elite UCs. The mean logged family income of GTO students is one percent higher

[11] With the exception of SAT I verbal and math scores, GTO students are statistically distinguishable from other students on all measures.

than the means of other groups in our study; GTO students are less likely to be the first in their family to attend college than non-elite students and regularly admitted elite students and more likely than non-elite students to have a parent who completed college. Finally, GTO students are more likely to be African American than their traditionally admitted counterparts at elite UCs.

<<INSERT TABLE 1 ABOUT HERE>>

Comparing GTO students to traditionally admitted students

Table 2 presents a summary of the results from four different models for each outcome consistent with the first comparison of interest—comparing GTO students to traditional admits at elite UC institutions (equation (1)).[12] The first model for each dependent variable parameterizes the baseline mismatch effect discussed above conditional only on the number of terms students were enrolled, since many elite GTO students started in the spring rather than the fall term; this adjustment attenuates expected differences in GPA (by 0.05 points) and credits earned (by about 17 credits). Consistent with previous literature, once we add measures of prior academic achievement and social origins we account for virtually all of the mismatch penalty. Note that conditioning on these background controls is *not* equivalent to measuring mismatch; for example, academic achievement predicts variation in outcomes regardless of whether one is well matched or not. In fact, our models assume uniform increments to achievement for any unit increase in the predictors; we have not parameterized a mismatch "tipping point." Our key indicator of mismatch here is GTO status. GTO students at elite UCs pay, on average, a 0.35 point penalty in their GPA when compared to traditionally admitted students at elite institutions, conditional on the number of terms enrolled. Net of differences in students' academic and demographic backgrounds, traditionally admitted students attending elite institutions have

[12] Full model estimates are available from the authors upon request.

expected GPAs that are nearly identical to those of GTO students. Adding controls for application patterns (the "Application" column) and college major (the "Major" column) does not alter these differences. In sum, GTO students earn GPAs that are substantively and statistically more or less identical on average to those earned by comparable regularly admitted elite students.

Credits earned. Turning next to credits earned (the second panel of Table 2), we see that by the spring of their 4th year of college, GTO students at elite UCs accumulated about 6.5 fewer credits than traditionally admitted students at elite institutions on average (±1 credits) conditional on the number of terms enrolled.[13] This is a little less than half of a term disadvantage. Adjusting for differences in student academic achievement and other background characteristics eliminates the gap in credits accumulated, a finding that persists when controlling for both application patterns and college major.

Observed and conditional differences in attrition. The final panel of Table 2 shows observed and conditional differences in attrition from the initial college by spring 2008 (Year 4) in the probability metric based on average marginal effects from a logistic regression. Recall that we only know who leaves their initial institution; we do not know how many of these students graduated, dropped out of college, transferred to another college or university, or will return to their initial university to earn their degree at a later time. We assume that those who completed enough credits to have earned a degree by the time they left the university have in fact graduated.

The raw mismatch effect reveals that GTO students are slightly more likely to leave elite institutions without accruing the credits to earn a degree than their peers admitted via the traditional path (a 4.7 percentage point difference). Controlling for prior academic achievement

[13] Recall that most GTO students who started at Berkeley started in spring 2005 rather than fall 2004.

and background characteristics essentially eliminates this modest difference in the probability of persistence. Net of academic preparation and social background, marginal and traditional admits are about equally (un)likely to leave an elite university prior to earning enough credits to graduate.[14]

<<INSERT TABLE 2 ABOUT HERE>>

Comparing GTO students to their peers at non-elite institutions

Table 3 presents a parallel summary of Table 2 but for our second comparison of interest—GTO students relative to their counterparts at non-elite institutions (equation (2)). Again, the first model for each dependent variable parameterizes the baseline mismatch effect, conditional on number of terms of enrollment, while subsequent models account for differences in students' academic and demographic background characteristics, application patterns, and major. Conditional only on terms of enrollment, it appears that mismatched students earn GPAs that average about a tenth of a point lower than those they might have earned had they attended a non-elite UC campus. Net of differences in students' academic and demographic backgrounds, traditionally admitted students attending non-elite institutions have expected GPAs that are slightly more than a twentieth of a point higher than those of GTO students. Additional controls for application sets and choice of major account for about half of the remaining (already trivial) GTO disadvantage, resulting in a non-significant GPA penalty of about 3% of a point.

Credits earned. Turning to credits earned (the second panel of Table 3), we see that by the spring of their 4th year of college mismatched GTO students accumulate about 3.8 fewer credits than traditionally admitted students attending non-elite institutions (±2.6 credits), conditional on

[14] These results are robust to the inclusion of campus fixed effects, adjusting for possible systematic differences among the three elite public institutions—Berkeley, Los Angeles, and San Diego.

the number of terms they are enrolled. This is about a one course disadvantage. Adjusting for differences in student academic achievement and other background characteristics reduces this already small gap in accumulated credits slightly. In sum, when compared to regularly admitted students at non-elite UCs, the credit accumulation gap is about 3.5 credits (roughly one course).

Attrition. The last panel of Table 3 details differences in attrition from the initial college by spring 2008 (Year 4) based on average marginal effects from a logistic regression. Here, the raw mismatch effect reveals that GTO students are about seven percentage points *less likely* to drop out than their regularly admitted peers attending non-elite UC campuses conditional on terms of enrollment. Controlling for prior academic achievement and background characteristics and application patterns actually exacerbates this gap slightly to 8.1% and 7.2% respectively, but conditional on college major reduces the expected persistence difference to a 6.8 percentage point advantage for GTO students. Across a variety of specifications, GTO students appear to be more likely to persist at elite universities than they would have been at non-elite universities.

<<INSERT TABLE 3 ABOUT HERE>>

Subgroup differences in the effects of mismatch

The literature on mismatch has an important focus on underrepresented students in higher education. In fact, the paternalistic justification for exclusion is rarely invoked in the interest of White students. The dominant exchange around mismatch in much of the academic literature, and more recently in the popular press as a result of the *Fisher v. University of Texas* case before the Supreme Court (e.g., Will 2011), is focused on underrepresented students in higher education. Thus, we explored the possibility that mismatch imparts a particular disadvantage to those most likely to benefit from race-based affirmative action: African American and Latino students. We conducted additional analyses to explore the valence and magnitude of interactions between GTO status and race/ethnicity for each of the respective comparison groups—traditional

admits at elite UC institutions and traditional admits at non-elite UC institutions. Consistent with previous literature we did not find that minority groups are more harmed by mismatch than their White or Asian counterparts (Alon and Tienda 2005; Bowen, Chingos, and McPherson 2009); see Table 4. Thus, we conclude that if the paternalistic justification is invoked, it must be invoked equally for all students.

<<INSERT TABLE 4 ABOUT HERE>>

Discussion and Conclusion

UC, like all public higher education systems, seeks to strike a balance between providing opportunities to as many students as possible and rationing those limited opportunities based on merit. At a minimum, universities like those in the UC system strive to provide postsecondary opportunities to those likely to benefit from them as evidenced mostly, but not entirely, by their prior academic performance. Beyond the fact that the number of spots available at elite institutions is limited, universities have at least two motivations for rationing access to competitive campuses.

First, despite the pronounced disinvestment of states in their public postsecondary systems over the past decade, public higher education remains heavily subsidized by the public. In the 2007–08 fiscal year, approximately 27% of Berkeley's 1.9 billion dollars in revenue (about half a billion dollars) came directly from the state.[15] Campuses owe it to the state to try to invest that money wisely, in students likely to succeed. Second, in addition to their fiduciary responsibility to taxpayers, campuses are under pressure to increase graduation rates and reduce time to degree.

[15] Authors' calculations from Indicator 12.1 of University of California. 2010. "2009 Accountability Report." University of California Office of the President.

If they admit students who take longer to complete, or fail to complete entirely, this may reflect poorly on the institution.

Beyond these motivations, however, is the paternalistic justification for excluding students from elite schools. The empirical corollary of the paternalistic justification for exclusion is the mismatch hypothesis, which claims that students are more likely to successfully complete a program of study if their academic skills are close to the mean of the skills of those students who enroll in their college or university.

In this paper we operationalize "collegiate success" in three ways: GPA, credit accumulation, and persistence in the university 4 years from starting. Using a unique natural experiment in the admissions practices of three elite, highly selective UC campuses, we identified mismatched students as those not initially admitted but promised a spot after the UC Office of the President intervened. We showed that these students have academic profiles that are weaker than the academic profiles of the substantial majority of regularly admitted students to the elite universities, and more similar to (but still somewhat lower than) students admitted to other less competitive UC campuses. We used the regular admit pools to consider two comparison groups: (1) the better prepared students admitted in the first round by the elite UCs, and (2) students observationally similar to the GTOs attending institutions potentially better suited to our focal students—universities at which they would not be (as severely) mismatched.

Descriptively, the mismatch hypothesis appears to be at least partially true. Mismatched students attending elite schools earn lower average grades, are slightly slower to accrue credits, and are more likely to leave without a degree than regularly admitted students attending elite schools. Not surprisingly, much of the observed disadvantage GTO students face compared with their traditionally admitted classmates at elite schools is mediated by differences in academic achievement, social background, application patterns, and major. However, we argue that these

background differences are not the same as mismatch; even "matched" students vary in their academic qualifications in ways that predict variation in postsecondary outcomes. Net of measured academic and social background, the average GPA of GTO students is only about 0.01 points lower than that of traditionally admitted elite students. Mismatched students accrue about seven fewer credits over their first 4 years of college than their regularly admitted elite peers, the equivalent of one to two courses, but, again, prior academic preparation and social background account for this modest difference. Perhaps most importantly, mismatched students attending an elite UC campus are no more likely to leave in their first 4 years prior to earning a degree than are regularly admitted students net of background characteristics.

The empirical strategy we employed in this paper is different from those used in earlier tests of mismatch. At UC we were able to distinguish observationally between students who did and did not make the initial cut for entry into elite universities. This allowed us to avoid relying on parametric assumptions about correlated disturbances to identify unobserved components of self-selection (Alon and Tienda 2005; Light and Strayer 2000) as well as issues of measurement that invoke proxies like race to represent mismatch (Alon and Tienda 2005).

While the GTO experiment is an improvement over past (observational) studies for identifying mismatch effects, the extent of this improvement may be limited by the fact that a minority of students offered the opportunity to participate in GTO actually accepted the offer. We show in Appendix A1 that differences between participants and nonparticipants on important observables are generally modest in magnitude and in many instances fail to attain statistical significance. However, it could still be the case that relevant unobservables such as grit (Duckworth and Quinn 2009) or desire to earn a degree from a high-status institution distinguish participants from nonparticipants. If these unobservables predispose participants to be more successful in college than nonparticipants then our results may provide an overly restrictive test

of mismatch. However, this will only be true to the extent that unobservables are orthogonal to the controls we applied in this study, controls that include prior academic achievement, social background, and students' own assessments of their interest and capacity to succeed at competitive colleges as evidenced by their UC application patterns. We believe that the bias in our estimates is likely modest.

In this study we broadened the focus of mismatch beyond persistence to include grades and credit accumulation. We found that mismatch has no reliable or substantively notable bearing on grades, rates of credit accumulation, or persistence. Given the benefits that accrue to those who earn degrees from elite institutions, we reject the paternalistic justification for exclusion. Denying opportunities to students on the basis of a mismatch, at least within the rather substantial range of student background attributes we observe, is *not* clearly in the best interests of excluded students. We are particularly struck by the lack of evidence for differential effects of mismatch across racial/ethnic groups. Given the profound concerns raised by conservative critics of affirmative action for the welfare of mismatched students of color, and *only* those mismatched students who are students of color, we expected the weight of mismatch to be disproportionately borne by such students. In addition to the absence of differential effects, we note that, of those who accepted the GTO offer, 30% were White and 43% were Asian. It is unclear to us why those who advance a color-blind agenda would fail to protect mismatched White and Asian students from the adverse outcomes they erroneously believe mismatched students endure.

Absent the paternalistic justification of exclusion, institutions must rely exclusively on the contest justification. This puts the focus back squarely where it belongs, on the terms of the contest and the context in which the contest is joined. While there is certainly no shortage of empirical work on the contest justification, we hope that researchers and policy makers will remain diligent in their efforts to interrogate and illuminate social inequalities in the

opportunities to learn that clearly undermine the legitimacy of the contest. In our view, the path forward is not in shutting the door on students under the guise of protecting their interests, it is in providing students the opportunities they need to compete in a way that legitimates the contest justification for exclusion.

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Tables & Figures

Table 1

Descriptive Statistics Comparing GTO to Non-GTO Students at Elite Campuses and Non-elite Campuses

		Elite	
	GTO	**non-GTO**	**Non-elite**
Cumulative GPA	2.65	3.05	2.88
Terms enrolled	9.6	10.8	11.4
Total units earned minus cumulative units first term	111.6	134.3	141.4
Was not present in final term*	0.13	0.08	0.20
Predictors			
Secondary school achievement			
UC weighted HS GPA	3.57	4.01	3.66
SAT I-Math	590	662	596
SAT I- Verbal	556	623	562
SAT II- Math	566	661	588
SAT II- Writing	547	639	568
Social background			
Log of parent income	11.07	10.96	10.97
Parent education			
Less than high school	0.04	0.08	0.10
High school	0.09	0.10	0.10
Some college	0.18	0.15	0.19
College graduate	0.68	0.67	0.61
Female	0.55	0.56	0.55
Race/ethnicity			
Asian	0.44	0.47	0.38
Black	0.05	0.03	0.03
Hispanic	0.12	0.12	0.17
International	0.04	0.04	0.04
White	0.34	0.34	0.37
N	491	12,136	17,417

* Excludes those who left with enough credits to earn a degree

Table 2

Nested Regression Models Predicting Differences in GPA, Credit Accumulation, and 4-Year Persistence Rates between GTO Students and Traditional Admits at Elite UCs

	Mismatch	Academic achievement and social origins	Application patterns	Major
GPA (n=12,627)				
GTO	-0.354***	0.003	0.003	0.022
	(0.024)	(0.023)	(0.023)	(0.023)
r^2	0.080	0.282	0.284	0.310
Credits (n=12,640)				
GTO	-6.510***	0.039	-0.109	-0.218
	(0.996)	(1.024)	(1.039)	(1.045)
r^2	0.765	0.784	0.784	0.789
Dropout (n=12,487)				
GTO	0.047***	0.001	-0.005	-0.011
	(0.010)	(0.011)	(0.011)	(0.011)

Note: All models are conditioned on number of terms enrolled.

Academic achievement and social origins models include controls for high school GPA, SAT-Math & SAT-Verbal, parent income, sex, race/ethnicity, and parent education. The application pattern model adds a control for which UC campuses the student applied to. The major model adds a control for the major the student had in the last quarter she was present in our data.

Table 3

Nested Regression Models Predicting Differences in GPA, Credit Accumulation, and 4-Year Persistence Rates between GTO Students at Elite UCs and Traditional Admits at Non-elite UCs.

	Mismatch	Academic achievement and social origins	Application patterns	Major
GPA (n=17,908)				
GTO	-0.087***	-0.059**	-0.027	-0.026
	(0.034)	(0.030)	(0.033)	(0.034)
r^2	0.116	0.326	0.329	0.356
Credits (n=17,948)				
GTO	-3.806***	-3.502***	-2.017	-3.503**
	(1.326)	(1.288)	(1.442)	(1.517)
r^2	0.813	0.825	0.825	0.830
Dropout (n=17,842)				
GTO	-0.070***	-0.081***	-0.072***	-0.068***
	(0.021)	(0.020)	(0.024)	(0.025)

Note: All models are conditioned on number of terms enrolled.

Academic achievement and social origins models include controls for high school GPA, SAT-Math & SAT-Verbal, parent income, sex, race/ethnicity, and parent education. The application pattern model adds a control for which UC campuses the student applied to. The major model adds a control for the major the student had in the last quarter she was present in our data.

Table 4

Regression Models Predicting Racial/Ethnic Differences in GPA, Credit Accumulation, and 4-Year Persistence Rates, between GTO Students and (a) Traditional Admits at Non-elite UCs and (b) Traditional Admits at Elite UCs.

	Credits		**GPA**		**Dropout***	
	non-elite	**elite**	**non-elite**	**elite**	**non-elite**	**elite**
GTO elite (White)	-0.029	-0.143	-0.038	0.007	**-0.102**	**-0.037**
	(2.077)	(1.727)	(0.047)	(0.038)	**(0.030)**	**(0.017)**
Black*GTO	-7.244	0.598	0.045	0.070	0.042	0.022
	(5.127)	(4.843)	(0.117)	(0.107)	(0.073)	(0.042)
Hispanic*GTO	-0.327	3.652	0.112	0.083	0.045	0.023
	(3.469)	(3.244)	(0.080)	(0.072)	(0.054)	(0.033)

Coefficients in **bold** are statistically significant at the $p<0.05$ level.

Based on full models—including sex, race/ethnicity, parental education, logged family income, terms of enrollment, high school GPA, and scores on the SAT reasoning test and SAT II (math and writing).

* Average marginal effects.

Figure 1: Mismatch Comparison Groups

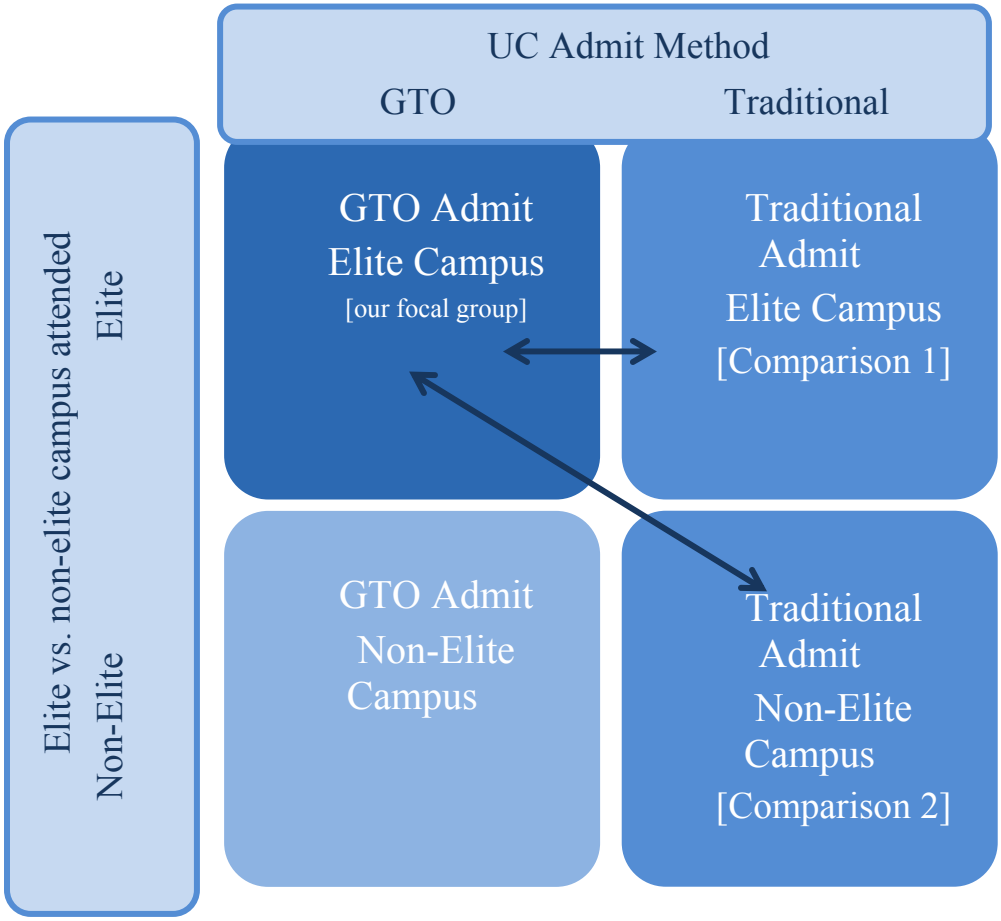


Figure 2

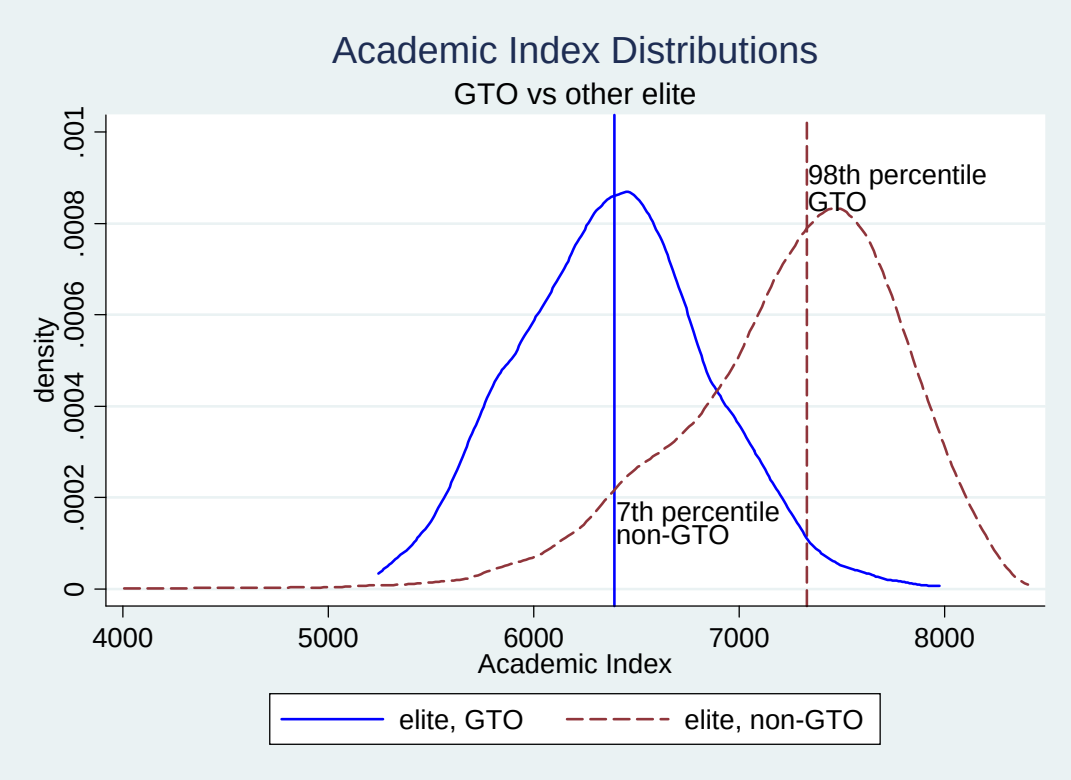
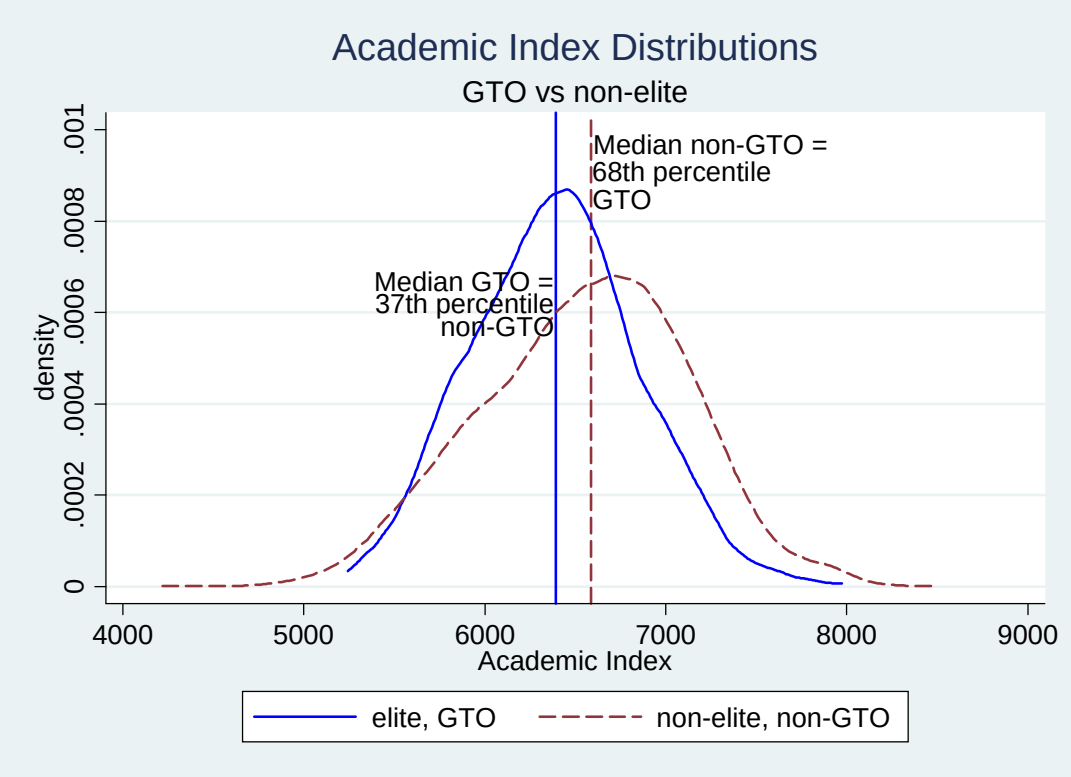


Figure 3



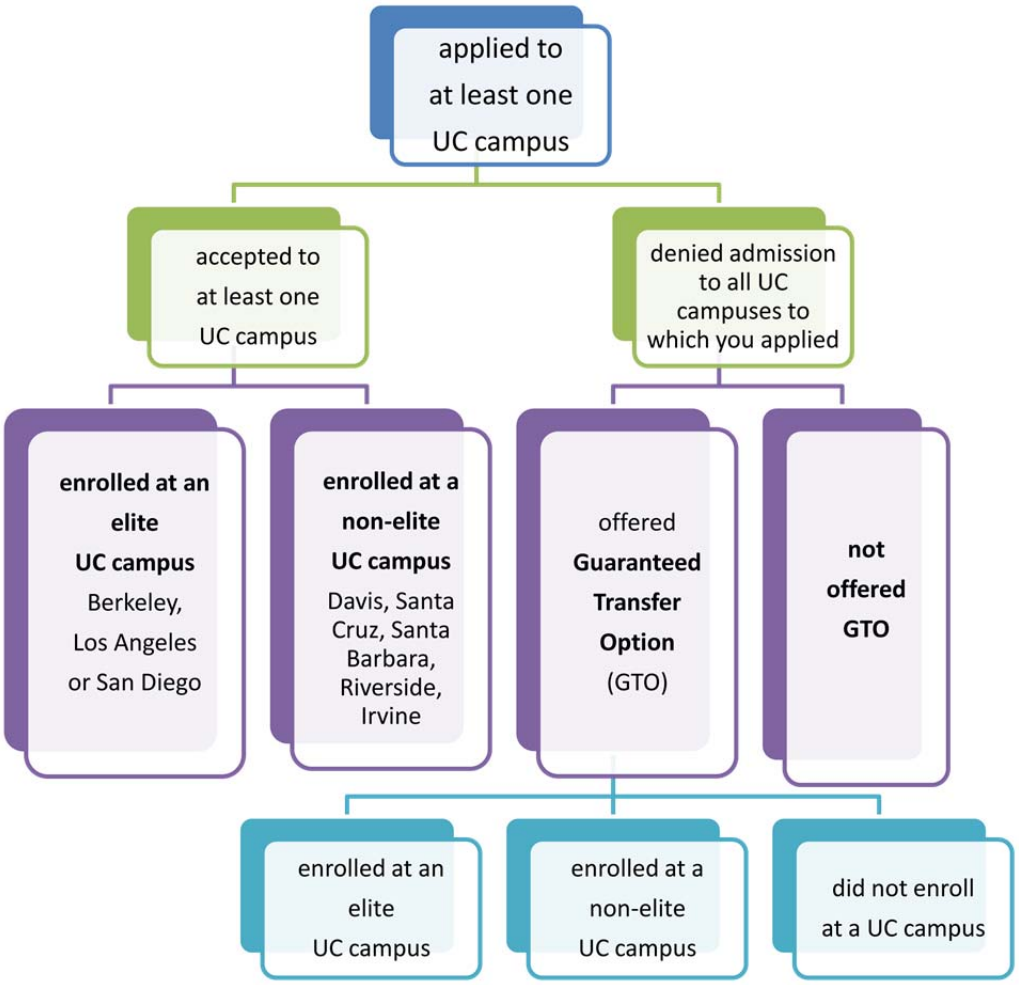
Appendix

Table A1

Differences between Students Who Use Their GTO Offer & Students Who Did Not Use Their GTO Offer

	Used GTO offer(n=484)	Did not use GTO offer(n=2,974)
High school GPA	3.56	3.57
SAT verbal	555	552
	590	575
SAT math		
Median income	65,000	75,000
	12.9%	87.1%
Female		
	15.6%	84.4%
Male		
	21.7%	78.3%
Asian		
	10.7%	89.3%
Black		
	9.1%	90.9%
Hispanic		
	11.2%	88.8%
White		
	14.2%	85.8%
Other		
Parent education		
< high school	11.9%	88.1%
high school	15.1%	84.9%
some college/AA	12.5%	87.6%
baccalaureate/+	14.5%	85.5%

Figure A1: GTO Admission Path



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