Breaking Barriers to Elite Education: Evidence from Sciences Po's Affirmative Action Policy*

Nagui Bechichi
Paris School of Economics

Aya Jibet
New York University

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Abstract

This paper examines whether affirmative action in elite higher education can expand access for disadvantaged students without generating mismatch effects or efficiency losses. We study Sciences Pos Conventions Éducation Prioritaire (CEP) programthe first affirmative action initiative implemented by a French Grande Écolewhich reserves seats for students from high schools in disadvantaged areas. Leveraging the quasi-random assignment of oral examiners with varying leniency levels, we implement a judge design instrumental variable strategy to estimate the causal effect of admission on students academic trajectories and predicted labor market outcomes. Using new linked administrative data combining Sciences Pos admission records with national education databases, we find no evidence of mismatch: CEP students admitted through the program are as likely to complete their degrees as comparable non-admitted applicants. In contrast, the gains from admission are larger for CEP students than for regular applicants, reflected in higher access to selective Masters programs and improved predicted early-career earnings. Within Sciences Po, early performance gaps between CEP and non-CEP students narrow over time, consistent with the effect of institutional support and adaptation mechanisms. Taken together, our results show that the CEP policy expanded access to elite education without reducing overall efficiency. Affirmative action beneficiaries not only succeeded once admitted, but also exhibited higher marginal returns to admission, implying that broadening access can enhancerather than compromisethe efficiency of elite higher education.

*Bechichi: Paris School of Economics, nagui.bechichi@gmail.com. Jibet: New York University, aj2384@nyu.edu.
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1 Introduction

Affirmative action in elite higher education aims to promote equity, but whether it can do so without sacrificing efficiency remains an open question. This tension underlies a long-standing debate in higher education policy, with proponents emphasizing its role in addressing structural barriers and opponents raising concerns about potential inefficiencies and unintended consequences. A central argument in favor of affirmative action is that it expands access for underrepresented groups, fostering greater socioeconomic mobility (Bowen and Bok (1998); Chetty et al. (2020)). There is extensive evidence that the elimination of affirmative action policies leads to declines in minority enrollment at selective institutions, often pushing affected students into lower-tier universities with lower graduation rates and weaker labor market returns (Backes, 2012a; Bleemer, 2022a). Advocates further argue that exposure to more selective academic environments generates positive peer effects, improving long-term outcomes.

A competing perspective posits that affirmative action can generate mismatch effects, whereby students admitted through preferential policies may struggle academically if placed in institutions where they are relatively less prepared (Sander (2004a); Arcidiacono et al. (2016a)). Empirical findings on mismatch effects are mixed, with some studies documenting negative effects on persistence and graduation rates, particularly in STEM fields, while others suggest that most students admitted through affirmative action perform at levels similar to their peers over time, particularly when institutional support structures are in place (Dillon and Smith (2020a); Mountjoy et al. (2020)).

A useful way to frame these debates is through a welfare-maximization perspective. Affirmative action can be modeled as an allocation problem in which policymakers maximize a social welfare function subject to equity constraints in access to selective institutions. Within this framework, mismatch arises when targeted students are placed in environments where the expected returns to their academic ability fall below those they would obtain in less selective settings. Efficiency loss refers to an aggregate reduction in total achievement or human-capital output if redistributing seats lowers the average productivity of all admitted students. Institutions can mitigate such losses through compensatory investmentssuch as preparatory workshops, mentoring, or tutoringthat raise the post-admission performance of less-prepared students. In this paper, we use this framework to test whether affirmative action at Sciences Po generates these welfare trade-offs, examining both mismatch and aggregate efficiency effects, as well as the role of institutional mechanisms in mediating them. In the context of the CEP, these mechanisms include preparatory workshops, summer bootcamps, and mentoring programs aimed at improving academic readiness and subsequent performance.

Although most empirical evidence on this trade-off comes from the United States, the French context provides a particularly revealing setting to assess whether affirmative action can enhance overall welfare by expanding access without creating mismatch or efficiency losses. Unlike the U.S. and other Anglo-Saxon countries, affirmative action has historically been absent from higher education policy in France. The countrys legal framework prohibits race-based affirmative action, and the prevailing republican model emphasizes universalist principles that reject explicit group-based preferences (Duru-Bellat (2015a)). Against this backdrop, Sciences Po was the first elite higher education institution in France to implement a form of affirmative action through the CEP program. By explicitly targeting high school students in disadvantaged areas, Sciences Po departed from the traditional selection model used by Frances *Grandes Écoles* and introduced a policy aimed at addressing social inequalities in access to higher education.

The CEP program at Sciences Po operates as an indirect affirmative action mechanism, similar to policies such as Californias Eligibility in the Local Context (ELC) and Texass Top Ten Percent Law (Horn et al. (2003)). These programs provide admission guaranties to high-achieving students within each high school rather than using race or socioeconomic status as explicit criteria. Using existing geographic and socioeconomic segregation, such policies increase diversity without directly considering individual background characteristics. The CEP program follows this logic, granting admissions advantages to students from designated high schools while maintaining a formally merit-based selection process through its alternative admissions pathway.

This paper contributes to the debate by providing empirical evidence on the effectiveness of such indirect affirmative action policies in a European setting. Using quasi-random variation in CEP admission decisions, we assess whether the program mitigates barriers to elite education and whether concerns related to mismatch hold in this context.

The case of Sciences Po provides a great opportunity to examine these welfare trade-offs directly, as the CEP program reshapes access to one of Frances most selective and socially exclusive institutions while maintaining strict academic standards.

Like other *Grandes Écoles* such as HEC, École Polytechnique, and the École nationale d'administration (ENA), Sciences Po has historically served as a key gateway to Frances political, administrative, and business elites, , concentrating students from the most advantaged social backgrounds and thereby reinforcing educational and social stratification (see, e.g., Duru-Bellat, 2015b; Albouy and Wagner, 2020). Before 2001, admission was primarily based on a competitive written entrance exam (*concours*), which strongly favored students from elite Parisian high schools or those able to afford expensive private preparatory programs. Despite low tuition fees, access to these schools was shaped by longstanding inequalities in students preparation, information, and social networks.

Empirical evidence confirms the strong social selectivity of this system: more than 80% of students admitted through the *concours* came from high-SES families, and roughly 40% attended private high schools (see Table 1, columns 1–2). In contrast, students from working-class, immigrant, or rural backgrounds were almost entirely absentnot necessarily because of lower ability, but because they were filtered out much earlier in the system, having been less exposed to the specific academic style required for the *concours*.

Before 2001, admission to Sciences Po was primarily based on a competitive written entrance exam (*concours*), which favored students with access to targeted preparation — usually those in elite Parisian high schools or having access to *expensive private prep programs*. Students from low-income backgrounds, immigrant families or rural areas were almost entirely absent, not necessarily because of ability, but because they were filtered out much earlier in the system. The *concours* required mastering a very specific academic style, often unrelated to what was taught in under-resourced high schools.

In 2001, Sciences Po launched the *Conventions Éducation Prioritaire* (CEP) program to broaden access without violating the legal ban on race based selection. Instead, the CEP relies on territorial targeting: Sciences Po formed bilateral agreements with high schools located in *Reseaux déducation prioritaire* (REP) or those with a high share of low-SES students. Students in these partner high schools can then apply through a dedicated track that does not require them to pass the written exams. Instead, applicants only submit an application including their high school transcripts and anticipated baccalaureat grades, and if selected by their teachers, prepare a press review on current events and attend a structured oral interview with a jury of three examiners at Sciences Po. Only a small number of CEP applicants are admitted, and the process remains competitive.

Over time, the program has expanded significantly. The program now includes nearly 200 high schools and in recent years CEP students represent 10–15% of the incoming undergraduate class. Moreover, following the 2021 reform of Sciences Pos admissions, which eliminated the *written concours* for all applicants, the CEP was further integrated into the mainstream admission process, although with separate admission thresholds and continued support structures.

One of the concerns often raised – both within and outside Sciences Po –is whether students admitted through the CEP track are academically prepared to succeed in such a demanding environment (see Sciences Po, 2018). The *mismatch hypothesis* is especially relevant here for several reasons. First, CEP applicants typically come from high schools with very little or no history of sending students to Sciences Po or other elite institutions. Their GPA and *baccalauréat* scores are often significantly lower than those of students admitted through the regular path.

Second, the academic environment at Sciences Po is much less flexible than many U.S. colleges. Students follow a core curriculum with few electives, and many courses are required regardless of background or interest. Final exams are centralized and graded by committees on a relatively harsh scale — there is little to no grade inflation, and in many cases, Sciences Po grades more severely than public universities. There is limited room for strategic course selection or GPA management. If CEP students were truly mismatched, these effects should show up clearly in their academic trajectories and longer-term outcomes, including access to selective master's programs or time to degree.

Third, this concern is not just theoretical. Internally, there has been ongoing debate within Sciences Po about whether the CEP is admitting students who are not "ready" for the institution, and whether this may ultimately do more harm than good. Outside the institution — especially among some alumni — the CEP has also raised questions about merit and legitimacy. In a system that heavily values the *concours* as the "fairest" form of selection, some critics view CEP students as not having "earned" their place in the same way as others. This became even more visible after 2021, when the *concours* was removed for everyone and the oral exam became the only selection step — with different admission thresholds maintained for CEP and regular applicants.

All of this makes Sciences Po and the CEP program a highly relevant context to test whether affirmative action leads to mismatch effects or efficiency losses, or whether students admitted through these alternative pathways can succeed once given access to the same institutional resources and additional adjustment mechanisms.

This paper investigates whether affirmative action in elite higher education can expand access for disadvantaged students without generating mismatch effects or efficiency losses. We study the indirect affirmative action implemented through the CEP program at Sciences Po and examine its impact on access to and success within a highly selective higher education institution. The empirical analysis is structured around three sets of questions, each corresponding to a central debate in the literature and to a specific set of comparisons in the data.

The first set of questions concerns the potential presence of mismatch effects. We ask whether students admitted through the CEP track were academically prepared for the demands of Sciences Po or whether they would have been better served by enrolling in a different type of institution. In this context, mismatch is defined as a situation in which the policy places students into an institution where they are overmatched relative to their preparation, leading to worse academic performance than comparable non-admitted applicants. To test this, we compare students who were admitted through CEP with CEP candidates who were deemed admissible, but were not ultimately offered admission. We also conduct the same comparison within the regular admission track. These analyzes rely on two sources of quasi-experimental variation: a regression discontinuity design leveraging admission score thresholds (available in the post-2020 period), and a judge fixed-effects design exploiting the random assignment of applicants to oral examiners during the interview stage.

The second dimension of the analysis explores how CEP students perform compared to their peers admitted through the regular track, and how all CEP applicants (admitted or not) compare to students from the regular admission pool who were not selected. These comparisons allow us to assess whether CEP students face greater academic challenges once enrolled, and whether their academic and post-graduate trajectories differ from those of students with similar or stronger prior academic records. In particular, comparing CEP-admitted students with regular-admitted students allows us to explore potential differences in retention, repetition, and access to selective masters programs. Comparisons between non-admitted CEP and non-admitted regular applicants help us examine whether the two groups face different opportunity structures outside of Sciences Po.

The third set of questions focuses on the efficiency and allocation implications of the CEP policy. Because CEP admission involves reserved seats, it can lead to the exclusion of some regular-track applicants who would otherwise have been admitted to a system without affirmative action. We therefore ask whether the marginal CEP admit benefits more from attending Sciences Po than the marginal regular-track applicant who was excluded due to the reallocation of seats. This speaks directly to a common critique of affirmative action policiesthat they may come at the cost of admitting higher-achieving students from the general pool. To answer this, we compare the results for the "winners" of the CEP (students admitted through the CEP) with those of regular-track applicants who just missed the cutoff and might have been admitted in the absence of reserved seats.

Across all three sets of questions, we examine both short-term academic outcomessuch as repetition in the first-year, dropout, and undergraduate graduationand longer-term indicators, including enrollment and completion of selective masters programs, as well as early labor market indicators when available. Together, these comparisons allow us to assess not only whether CEP increases access for underrepresented students, but also whether it does so in a way that supports success and maintains efficiency within the admissions process.

Related Literature. We contribute to three strands of literature. First, we add new evidence to the debate on the effectiveness of affirmative action in elite higher education, focusing on a context where such policies were newly introduced rather than abolished. Although most research studies the consequences of banning race-based affirmative action in the United States (Backes, 2012a; Hinrichs, 2012a; Bleemer, 2022a), we examine the introduction of a territorially targeted, race-neutral program in Francea country where group-based preferences are legally prohibited and where selective universities have historically been highly socially exclusive. This setting allows us to identify the causal effects of expanding access to elite education in the absence of prior affirmative action policies. As direct race-based affirmative action has faced increasing political and judicial challenges in many U.S. states, indirect affirmative action policies, such as top percent plans, have gained traction (Bleemer, 2021; Kapor et al., 2020). The CEP program provides a great opportunity to study such policies in a centralized, exam-driven admissions system outside the United States.

Second, we contribute to the literature on the returns to selective college attendance by implementing a novel judge-design instrumental variable strategy that exploits the random assignment of oral examiners. This approach generates exogenous variation in admission decisions, allowing us to identify the causal effect of admission on academic and predicted labor market outcomes. A key strength of our setting is that Frances centralized admissions platform enables us to observe each students actual counterfactual enrollment choicesomething rarely available in studies on elite institutions access.

Third, we contribute to the literature on the mismatch hypothesis, which examines whether students admitted through affirmative action are academically overmatched (Arcidiacono et al., 2016a; Dillon and Smith, 2020a; Mountjoy et al., 2020). We combine two complementary identification strategies: a regression discontinuity design based on post-2020 admission thresholds, and a judge leniency IV design based on oral interview assignment, which together provide a comprehensive view of the effects of affirmative action beyond score-based thresholds. Evaluating the mismatch hypothesis in the French context is particularly relevant because Sciences Po offers a fixed curriculum, centralized grading, and limited room for course selection, making any mismatch effects potentially more visible in academic outcomes. Finally, we explore whether institutional supports built into the CEP programsuch as preparatory workshops and guidancecan mitigate mismatch effects. These features aim to improve student readiness and confidence before entry, but are rarely evaluated in existing studies. This allows us to not only test whether mismatch occurs, but also whether specific interventions reduce academic gaps and support successful integration.

Together, these contributions provide new evidence on the equityefficiency tradeoff in elite higher education, showing that territorially targeted affirmative action can broaden access without generating mismatch or efficiency losses.

The rest of the article is organized as follows. Section 2 describes the institutional background and data. Section 3 presents the empirical strategy, including the construction of the instrumental variable and identification tests. Section 4 reports the main results and examines their robustness to alternative specifications. In Section 5, we explore heterogeneity in the estimated effects across student and high school characteristics. Section **??** discusses the policy implications of the findings. Finally, Section 7 concludes.

2 Institutional Background and Data

2.1 The French Higher Education system

French Higher Education Institutions The French higher education system is structured around a dual model, with universities on one side and a highly selective track composed of Grandes Écoles on the other. While universities are open to all students who pass the Baccalauréat exam, Grandes Écoles including Sciences Po, HEC, and École Polytechnique maintain selective admissions procedures and historically serve as gateways to the countrys elite. These institutions concentrate a disproportionate share of students from privileged backgrounds, despite Frances low tuition fees and ostensibly egalitarian access to education.

Access to a *Grande École* typically requires not only strong academic performance but also success in competitive entrance exams. Many students prepare for these through two-year *classes préparatoires* (CPGE), which are highly selective and disproportionately enroll students from advantaged backgrounds. Admission to CPGE depends on teacher recommendations and academic records, mechanisms that reinforce early educational stratification (see Albouy and Wagner, 2020; Caille and Lemaire, 2021).

Although tuition fees in France are low compared to many other countries, and access to higher education is formally open to all Baccalauréat holders, stark social inequalities strong social reproduction mechanisms persist in the allocation of students across institutions. Students from upper-class families are overrepresented in selective tracks, while those from disadvantaged backgrounds are more likely to attend universities with fewer resources, lower graduation rates, and weaker labor market outcomes. Though nearly all students with an academic Baccalauréat pursue higher education, stark inequalities remain in the type of institutions they access and the support they receive once enrolled.

College Admission in France Until 2017, admission to most higher-education programs in France was coordinated through the centralized platform *Admission Post-Bac* (APB). Candidates could submit up to 24 wishes (with at most 12 in a given program type, e.g., *CPGE*, *BTS*, *DUT/IUT*) and rank them. Under APB, *selective* programs (notably *CPGE*, *BTS*, and *DUT/IUT*) evaluated files and returned *rank-ordered lists* of candidates to the platform. By contrast, a large part of bachelor programs in public universities were "non-selective". These programs did not rank candidates: seats were mechanically allocated using capacity constraints and administrative priority rules (e.g., local sector priority, series/track compatibility), with tie-breaking as needed. A centralized, multi-round deferred-acceptance mechanism then assigned each student to the highest-ranked choice where a seat was available; students could hold only one offer at a time.

In 2018, APB was replaced by *Parcoursup*. While the same iterative matching logic is retained, the reform fundamentally changed both how institutions and applicants make decisions. All *institutions now rank candidates* across the system using criteria they define internally. Selection committees may take into account continuous assessment (highschool transcripts), anticipated *Baccalauréat* results (*épreuves anticipées*), teacher evaluations, and contextual information such as the applicants high school or place of residence, with the precise weighting left to each program. On the applicant side, students no longer pre-rank their choices. They may receive several offers simultaneously and must progressively accept or decline them by specific deadlines until only one offer is held.

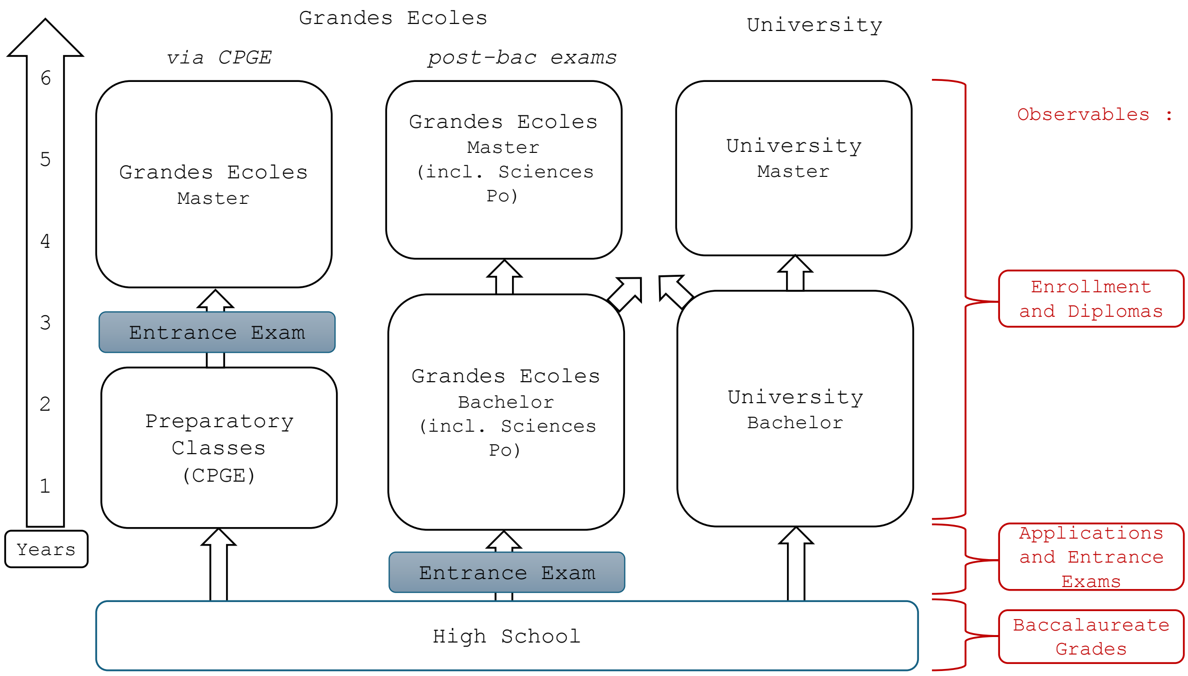

Figure 1: French higher education pathways for academic-track high school graduates. The diagram distinguishes traditional Grandes Écoles accessed after two years of preparatory classes (CPGE) from post-baccalauréat entrance Grandes Écoles, which include Sciences Po.

2.2 Sciences Po and the CEP Affirmative Action Policy

2.2.1 Sciences Po: Historical Significance and Institutional Context

Sciences Po, formally known as the Institut d'Études Politiques de Paris, occupies a prominent position among France's *Grandes Écoles*, with a primary focus on social sciences. Since its establishment in 1872, Sciences Po has served as a critical institution in the formation of France's political, administrative, and business elites. Its alumni network includes prominent figures across sectors, notably encompassing all Presidents of the French Fifth Republic since 1958. The institution's historical mandate to train the nation's leadership has cemented its reputation as a gateway to power and influence.

Historically, Sciences Po's admissions process relied on a highly selective and centralized competition based on written and oral *concours*. Until 2020[1], the entry into Sciences Po followed the traditional *Procédure par examen*. In this framework, candidates completed the *concours écrits*, comprising three rigorous written examinations, typically taken during the senior year of high school. Applicants who achieved a sufficiently high score were deemed eligible to proceed to the oral interview, which further evaluated their suitability for admission. This multistage process, while academically robust, systematically favored students from privileged socioeconomic backgrounds.

Empirical evidence underscores the homogeneity of Sciences Po's student body during the traditional admissions era. More than 70% of the candidates for *concours* and more than 80% of the admitted originated from households with high socioeconomic status. These applicants disproportionately attended a small subset of high schools, primarily private institutions and select public schools located in affluent urban areas. These patterns reflect a broader phenomenon observed in France's elite higher education system, where access to *Grandes Écoles* is closely linked to socioeconomic privilege, perpetuated by the exclusivity of preparatory classes (*classes préparatoires*) and competitive entrance examinations.

2.2.2 The CEP Affirmative Action Program: Addressing Structural Inequalities

Recognizing the socioeconomic barriers inherent in its traditional admissions process, Sciences Po implemented the *Conventions Éducation Prioritaire* (CEP) program in 2001. This initiative, the first affirmative action policy of its kind among Frances *Grandes Écoles*, aimed to diversify the institution's student body by creating alternative pathways to admission for students from underprivileged backgrounds. Specifically, the CEP program targeted high schools located in *Zones d'Éducation Prioritaire* (ZEP), which

[1] In 2020, Sciences Po integrated the Parcoursup platform, France's national higher education application system, as part of a broader reform of its admissions process. This reform eliminated written exams and introduced a selection mechanism based on high school grades, personal essays, and an oral interview.

are characterized by significant socioeconomic challenges and a high proportion of students from low-income families.

The CEP pathway diverged significantly from the traditional admissions process. Instead of the written *concours*, applicants from partner high schools were evaluated based on their academic performance, teacher recommendations, and extracurricular activities. Candidates deemed eligible through these criteria advanced to the oral interview, where their motivation, academic potential, and suitability for Sciences Po were assessed. This structure not only reduced the reliance on standardized written examinations but also acknowledged the broader context of socio-economic inequalities faced by applicants.

The program has expanded steadily since its inception. In its first year, Sciences Po partnered with seven high schools and admitted 17 students through the CEP pathway. Over the next two decades, the initiative grew both in scale and impact. By 2020, the program encompassed 106 partner high schools and accounted for approximately 170 admissions, representing 10% of the entering class. The scope of the CEP program increased further following the 2021 reform of Sciences Po's admissions process, which integrated the program more fully into the institutions overall strategy for fostering diversity and inclusion. In 2021, 60 additional high schools joined the program, followed by another 32 partnerships in 2022. This expansion underscores the institutions commitment to promoting equity in access to elite higher education.

Despite these advances, the CEP program remains a unique case among Frances *Grandes Écoles*. Its innovative approach highlights the potential for targeted policies to address structural barriers to educational mobility. The program also provides a valuable case study for understanding how affirmative action policies can reshape access to elite education while maintaining academic standards.

2.3 Data

Our analysis combines rich administrative datasets from the French Ministry of Education with internal application and enrollment records from Sciences Po. This section describes the structure of our data sources and how they are linked, as well as the construction of the sample used in our empirical analysis.

Administrative Data for College application and H.E trajectories to improve ASAP

We draw on several datasets maintained by the Ministère de lÉducation nationale, which collectively provide a comprehensive picture of students' academic backgrounds, application choices, and higher education trajectories. Our primary source is the OCEAN database, which covers all students who register for the Baccalauréat examFrances national high school leaving examination. This database includes detailed information on students high school of origin (identified via the UAI code), academic track (general, technological, or vocational), chosen specialization, and demographic characteristics such as gender, date of birth, and nationality. Critically, OCEAN also includes granular information on academic performance, including both final Baccalauréat grades and épreuves anticipées, or early exam scores from Première. These early results are particularly important, as they are visible to admissions officers and often heavily weighted in application reviews.

To follow students after high school, we rely on two additional Ministry databases. The first is the SISE (Système d'information sur le suivi de l'étudiant), which records students enrollment status by year and institution type (e.g., university, CPGE, IUT, BTS, or private programs). The second is the Base Scolarité, which provides complementary information on student progression and program type, particularly for cohorts enrolled in universities and CPGE. These data allow us to observe not only whether students continued into higher education, but also the type and selectivity of the institutions they entered. While these datasets do not contain precise graduation dates for all institutions, they provide reliable information on year-by-year enrollment and persistence, which we use to construct outcome variables such as first-year repetition, access to selective masters programs, and sustained enrollment in the higher education system.

Together, these Ministry sources allow us to construct rich longitudinal trajectories, from the final years of high school through to several years into postsecondary education, for all students in our sampleregardless of whether they were admitted to Sciences Po. They are critical for identifying both baseline characteristics and counterfactual enrollment outcomes.

Application Data from Sciences Po to improve ASAP

In addition to national data sources, we use three sets of internal administrative records provided by Sciences Po: one covering all applicants to the undergraduate program, another tracking academic outcomes for enrolled students, and a third drawn from alumni surveys administered after graduation.

The first set includes all individuals who applied through either the regular admission track (BAC0) or the Conventions Éducation Prioritaire (CEP) track between 2013 and 2020. For each applicant, we observe detailed demographic and academic background information, including gender, date of birth, Baccalauréat track, exam scores (including épreuves anticipées), and the high school attended. The application file also includes teacher evaluations and letters of recommendation. In the regular track, candidates were required to sit for three written entrance exams, and their scores on each component are recorded. After this initial file-based screening, admissible applicants in both tracks were invited to an oral interview. For each of these candidates, we observe the interview panel composition (two or three examiners), examiner-level oral scores, and the final admission decision. These administrative records are essential for constructing our instrumental variable based on examiner leniencymeasured as the leave-one-out average oral score an examiner assigned to other applicants in the same cohort.

The second set of records covers students who ultimately enrolled at Sciences Po. These include semester-level academic outcomes such as course enrollment and grades, major choice, re-enrollment, and graduation. We also observe whether students received financial aid and whether they accessed Sciences Pos graduate programs. Because the curriculum is standardized across undergraduate campuses, we can consistently track academic trajectories and outcomes across students and cohorts.

Finally, we supplement these administrative data with information from Sciences Pos alumni surveys, which are administered several years after graduation. These surveys provide insights into post-graduate education, employment trajectories, income, and perceptions of academic and professional preparedness. While response rates are not universal and the surveys are only available for selected cohorts, they offer valuable long-term outcome measures that complement the administrative tracking of educational progress.

Talk about the statistical matching to pair Sciences Po applicants to administrative data.

Sample of analysis

	General H.S Students	BAC0 qualified		CEP qualified	
Nb of students	2,370,372	10,309		3,428	
Nb of unique high schools	2,906	2,144		109	
Qualification rate		0.27		0.59	
		Refused	Accepted	Refused	Accepted
Nb of students		4,705	5,604	2,344	1,084
Nb of unique high schools		1,133	1,180	108	105
Matched students with admin. data	0.98	0.99	0.99	1.00	1.00
Students characteristics					
Women	0.57	0.66	0.60	0.64	0.61
Need-based Grant	0.16	0.04	0.05	0.32	0.31
Social Index (p25)	98	148	148	81	88
Social Index (p50)	129	156	156	117	125
Social Index (p75)	155	165	165	154	154
Social Index (mean)	125	152	152	115	119
H.S. option: ES	0.32	0.51	0.54	0.57	0.53
H.S. option: L	0.15	0.10	0.09	0.15	0.14
H.S. option: S	0.53	0.39	0.36	0.27	0.33
H.S. exam grade (p25)	11.1	16.6	17.1	12.2	14.3
H.S. exam grade (p50)	12.6	17.6	18.1	14.0	16.0
H.S. exam grade (p75)	14.6	18.5	18.9	15.5	17.3
H.S. exam grade (mean)	13.0	17.5	17.9	13.9	15.7
H.S characteristics					
Private High School	0.24	0.41	0.41	0.01	0.01
Paris Suburbs	0.16	0.19	0.19	0.40	0.41
Paris	0.04	0.15	0.15	0.01	0.02
Distance to Sciences Po (p50)		204	202	46	49
Applications in alternative H.E programs					
First choice: CPGE	0.10	0.60	0.61	0.28	0.38
First choice: L1	0.64	0.12	0.10	0.40	0.29
First choice: BTS	0.05	0.00	0.00	0.01	0.01
First choice: DUT	0.10	0.00	0.00	0.05	0.03
First choice: Other sel. pgms	0.11	0.28	0.28	0.25	0.30

Table 1: Summary Statistics of the Sample

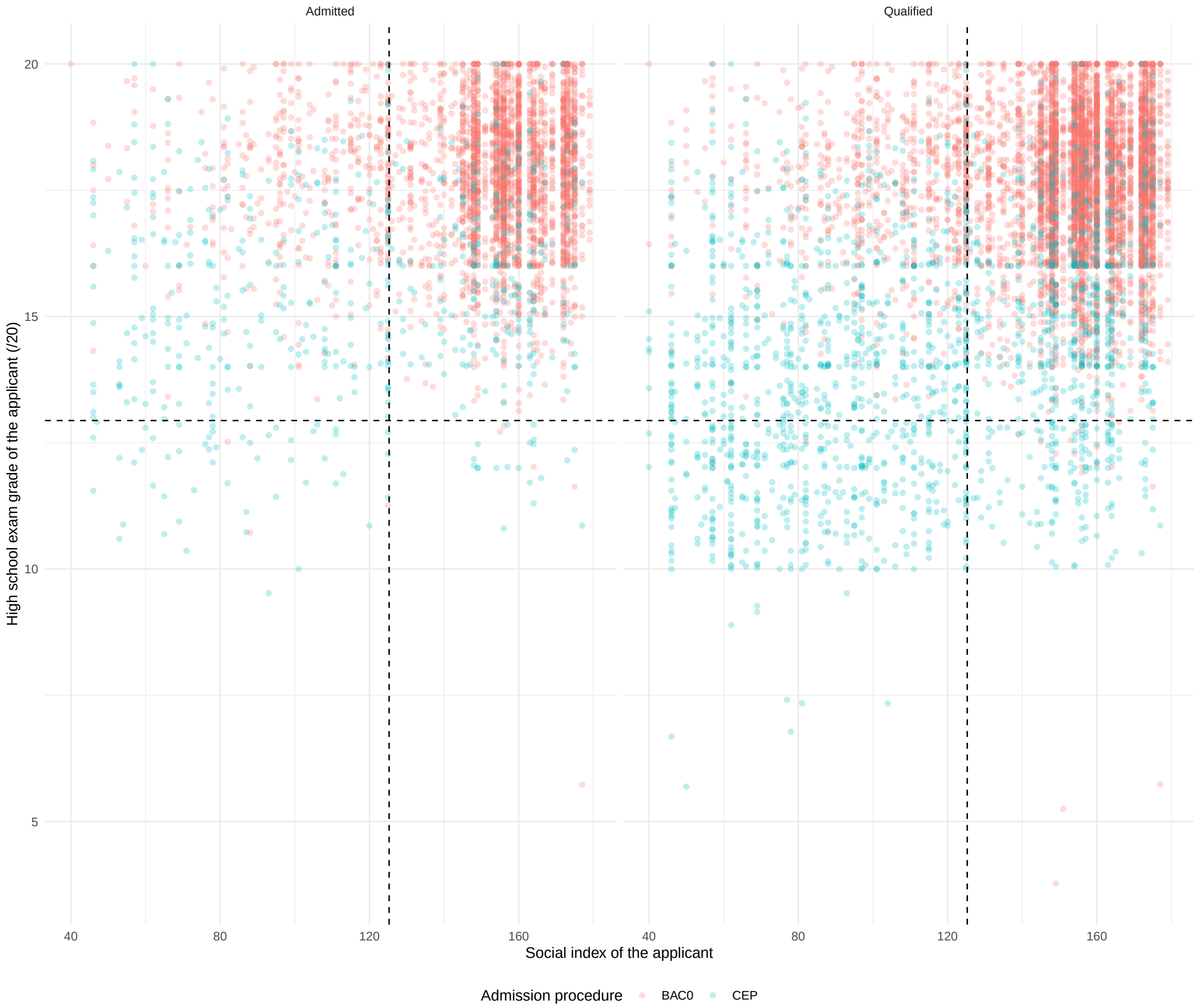

Figure 2: Distribution of academic and social characteristics of applicants qualified for Sciences Po

3 Empirical Strategy

3.1 Admission to Sciences Po and Selection Problem

Even in post-reform admissions (2020 and later), where thresholds (X_1, X_2, C_2) are observable, controlling for individual characteristics alone does not fully resolve selection bias. Unobserved factors that simultaneously influence both admission and outcomes may confound the causal relationship. A common approach in the literature is to use a regression discontinuity design (RDD) to estimate a Local Average Treatment Effect (LATE) at the cutoff. This strategy exploits the discontinuity in admission at C_2 to identify the treatment effect for candidates whose scores are near the threshold. In this study, we apply the RDD approach to the post-2020 data, where the admission process follows a structured cutoff-based system. Figures 3 and 4 illustrates the discontinuity in program quality and social index at the admission cutoff in the post-reform context. Figure 5 illustrate the discontinuity in grade repetition at the admission cutoff.

However, an RDD approach has several limitations when applied to Sciences Pos pre-reform admissions. First, the relevant scores (X_1, X_2) and threshold (C_2) are unobserved, making it infeasible to apply a sharp RDD framework across the full sample. Second, RDD identifies a treatment effect only for candidates at the margin of admission, meaning it estimates a LATE for students with very similar academic abilities around the cutoff. This limits its external validity when considering a broader applicant pool with greater heterogeneity in academic preparation. To address these challenges, we implement an instrumental variable (IV) strategy leveraging variation in judge leniency.

3.2 Judge-Leniency Design and Instrument Construction

3.2.1 Theoretical Justification

The scores X_1, X_2, and the admission cutoff are not observed in the pre-reform period. Instead of relying on proximity to a cutoff for X_1 and X_2, we take advantage of the fact that different judges apply distinct grading functions, which effectively transform X_1 and X_2 into an admission decision. The random assignment of candidates to oral examiners creates an exogenous shifter of admission probabilities, which serves as the basis for our instrumental variable strategy.

To overcome the limitations of RDD in the pre-reform context, we exploit the random assignment of candidates to oral examiners as a source of exogenous variation in admission probabilities. Judges differ systematically in their leniency, effectively transforming candidates' scores (X_1, X_2) into admission decisions. By constructing an instrument based on judge leniency, we isolate variation in admission probabilities that is unrelated to candidate characteristics, enabling us to estimate the causal effect of admission.

Unlike RDD, which focuses on students with similar academic abilities around the threshold, the judge leniency IV captures a different LATE. It estimates the effect of admission for students whose probabilities of acceptance vary due to examiner grading tendencies rather than their underlying qualifications alone. This distinction is critical, as it allows us to study how admission impacts students across a wider distribution of academic abilities.

Notably, to the best of our knowledge, judge leniency IV has not been applied in the education context. Prior applications have primarily been in legal and medical settings (e.g., assessing the impact of judicial sentencing or physician treatment choices). This study is among the first to implement this strategy in a higher education admissions setting, providing a novel contribution to the literature on selection bias and affirmative action.

The different measures of judge leniency and their construction are discussed below and illustrated in Appendix Figure 6. The random assignment of candidates to judges ensures that leniency-driven variation is exogenous. Finally, assumptions of monotonicity and random assignment are validated empirically, as discussed in Section 3.5 3.4.

3.2.2 Instrument : leniency of jury members

To isolate exogenous variation in admission probabilities, we exploit the random assignment of applicants to examiners and construct an instrumental variable based on examiners leniency. This approach builds on prior work using judge leniency as an instrument in the context of legal decisions (Aizer and Doyle Jr, 2015; Dobbie et al., 2018), but its application to education remains limited. The leniency measure serves as an exogenous shifter of the probability of admission, ensuring that variation in admission outcomes is driven by differences in examiner grading behavior rather than candidate characteristics. We define two measures of leniency: (i) **unconditional leniency**, which captures overall examiner admission tendencies, and (ii) **conditional leniency**, which accounts for candidate observables and isolates deviations from predicted admission probabilities.

Unconditional Leniency (JIVE) Unconditional leniency is defined as the examiner's propensity to admit candidates, calculated using a leave-one-out approach to avoid endogeneity.

Indeed, a naïve measure of examiner leniency is the average admission rate per examiner:

$$\text{Leniency}_j = \frac{\sum_k \mathbb{1}(\text{Admitted}_{kj} = 1)}{N_j},$$

where $\mathbb{1}(\text{Admitted}_{kj} = 1)$ is an indicator function that equals 1 if examiner j admits candidate k, and N_j is the total number of applicants evaluated by examiner j. However, this introduces a mechanical correlation between the instrument and the error term in the second stage.

To mitigate this, we employ the Jackknife Instrumental Variable Estimator (JIVE), which removes an individual's own case from the calculation. For examiner j, leniency is measured as the proportion of applicants they evaluate (excluding the candidate under consideration) who are admitted. This approach captures systematic differences in the tendencies of examiners to admit candidates, independent of the characteristics of the individual being evaluated.

Unconditional leniency for examiner j is calculated as:

$$\text{Unconditional Leniency}_{j,-i} = \frac{\sum_{k \neq i} \mathbb{1}(\text{Admitted}_{kj} = 1)}{N_j - 1},$$

where $\mathbb{1}(\text{Admitted}_{kj} = 1)$ is an indicator function that equals 1 if examiner j admits candidate k, and N_j is the total number of applicants evaluated by examiner j. The leave-one-out formulation ensures that the admission decision for candidate k does not directly influence the leniency measure.

For a candidate i evaluated by three examiners, j_1, j_2, and j_3, the average unconditional leniency across the assigned examiners is computed as:

$$\text{Unconditional Average Leniency}_i = \frac{1}{3} \sum_{j \in \{j_1, j_2, j_3\}} \frac{\sum_{k \neq i} \mathbb{1}(\text{Admitted}_{kj} = 1)}{N_j - 1}.$$

This measure provides a summary of the overall admission tendencies of the examiners assigned to candidate i.

Clustered JIVE For cases where candidates are examined in batches (e.g., morning/afternoon sessions), we use the Clustered JIVE (CJIVE) estimator:

$$\text{CJIVE Leniency}_{j,-C_i} = \frac{\sum_{k \neq C_i,} \mathbb{1}(\text{Admitted}_{kj} = 1)}{N_j - N_C},$$

where N_C is the number of candidates in session C_i, ensuring that session-wide effects do not bias leniency estimation.

Conditional Leniency While unconditional leniency provides a useful first-stage instrument, it may still be correlated with unobservable candidate characteristics if certain examiners systematically evaluate stronger or weaker applicants. To address this concern, we construct a measure of conditional leniency by adjusting for the predicted probability of admission based on observable candidate characteristics.

Using a regression model, we estimate the admission probability for candidate k as:

$$\Pr(\text{Admitted}_{kj} = 1) = \beta_0 + \beta_1 X_{kj} + \epsilon_{kj},$$

where X_{kj} represents the vector of observable characteristics for candidate k (e.g., prior academic performance, type of baccalaureate, anticipated test scores). The predicted leniency for examiner j is then calculated as the average predicted admission probability for all candidates they evaluate, excluding candidate i:

$$\text{Predicted Leniency}_j = \frac{1}{N_j - 1} \sum_{k \neq i} \Pr(\text{Admitted}_{kj} = 1).$$

Conditional leniency for examiner j is defined as the residual between the actual and predicted leniency:

$$\text{Conditional Leniency}_j = \text{Actual Leniency}_j - \text{Predicted Leniency}_j.$$

For a candidate i, the conditional average leniency across their assigned examiners is:

$$\text{Conditional Avg Leniency}_i = \frac{1}{3} \sum_{j \in \{j_1, j_2, j_3\}} \left(\frac{\sum_{k \neq i} \mathbb{1}(\text{Admitted}_{kj} = 1)}{N_j - 1} - \frac{\sum_{k \neq i} \Pr(\text{Admitted}_{kj} = 1)}{N_j - 1} \right).$$

This adjustment accounts for observable characteristics, ensuring that differences in examiner leniency reflect true deviations in admission behavior rather than imbalances in candidate quality.

We further refine this approach by estimating residualized leniency, where we regress examiner leniency on applicant characteristics and use the residual as our final instrument. This approach accounts for any potential sorting mechanisms or non-random assignment issues that may bias our estimates.

Specification variants. We construct two versions of the conditional leniency instrument. The baseline model (Regular Admission only) uses dossier information available for regular admission candidates (written exam average, A+ distinction, and cohort fixed effects). To ensure comparability across admission tracks, our main specification is the anticipated baccalaureate exam model, which relies on the type of baccalauréat and anticipated subject grades, with year fixed effects, estimated separately for Regular Admission and CEP. For Regular Admission students, the two specifications yield very similar predicted admission probabilities, indicating that results are not sensitive to this choice of observables.

For each candidate, conditional leniency is defined as the leave-one-out difference between an examiners actual and predicted admission rates. We then aggregate across examiners in two ways: a simple average (treating each examiner equally) and a judgment-weighted average (weighting examiners by the number of candidates they evaluated). In addition, we also record the minimum and maximum examiner leniency among the jury members assigned to a candidate; these are displayed in Figure 6. Finally, all leniency measures are standardized within procedure x cohort to mean zero and unit variance.

Validity of the Instrument: Relevance, Exogeneity, Monotonicity, and Exclusion Restriction For judge leniency to serve as a valid instrument, it must satisfy four key conditions: (i) **relevance** examiner leniency must strongly predict admission probability; (ii) **exogeneity** leniency must not be correlated with unobserved candidate characteristics that affect outcomes; (iii) **monotonicity** a more lenient examiner should weakly increase the probability of admission for all candidates, ensuring that no candidate is less likely to be admitted under a more lenient examiner; and (iv) **exclusion restriction** examiner leniency should affect student outcomes only through its impact on admission and not through any other channel.

To establish relevance, we estimate a first-stage regression of admission on judge leniency:

$$\text{Admission}_i = \alpha + \beta \text{Leniency}_i + \gamma X_i + \epsilon_i, \tag{1}$$

where X_i includes demographic and academic controls. We obtain a strong first-stage relationship (F-stat > 10), which confirms that leniency significantly influences admission.

The use of judge leniency as an instrument allows us to estimate a LATE that differs from traditional RDD approaches. While RDD identifies treatment effects for candidates at the margin of admission, the IV approach captures the impact of admission for a broader population, including candidates whose admission status is affected by examiner grading tendencies.

3.3 First Stage: Instrument Relevance

We begin by assessing the relevance of our instrumental variable, which captures the average leniency of the examiners assigned to each applicant. Examiner leniency is computed as a leave-one-out average of the grades that each examiner gave to other applicants in the same cohort. We construct two versions of this measure: (i) an *unconditional* leniency score, defined as the examiners overall propensity to admit candidates, and (ii) a *conditional* leniency score, which adjusts for predicted oral performance based on students observable characteristics (baccalauréat grades, anticipated test scores, and application year).

The following first-stage regression is estimated in the pooled sample:

$$\text{Admitted}_i = \alpha + \gamma \, \text{Leniency}_i + \delta \, \text{CEP}_i + \lambda \, (\text{Leniency}_i \times \text{CEP}_i) + X_i' \beta + \varepsilon_i, \qquad (2)$$

where Admitted_i is an indicator for admission to Sciences Po, Leniency_i is either the unconditional or conditional leniency z-score, CEP_i is an indicator for applicants in the CEP track, and X_i includes controls for parental social index (IPS), average baccalauréat score, and application-year fixed effects.

Table 2 and Table 3 report the first-stage results using the *unconditional* leniency measure separately for regular and CEP applicants, while Table 4 and Table 5 present the corresponding regressions using the *unconditional* and *conditional* leniency measure. Across specifications, examiner leniency is strongly and significantly associated with admission. In the pooled regression with conditional leniency, we obtain an F-statistic of 243.7 on the instrumentwell above the conventional threshold of 10 proposed by Staiger and Stock (1994). These results confirm that examiner leniency is a strong predictor of admission decisions in both tracks.

The interaction term between leniency and CEP status is negative and statistically significant ($p < 0.01$), indicating that examiner discretion plays a somewhat smaller role within the affirmative action track. This pattern likely reflects differences in evaluation procedures or the use of contextual criteria in the CEP process, but the instrument remains highly predictive of admission in both groups.

To validate instrument exogeneity, we test whether examiner leniency is correlated with applicants pre-application academic characteristics. Specifically, we regress examiner leniency on baccalauréat grades and find no evidence of correlation. This result is consistent across specifications. Appendix Figure 8 shows that the average baccalauréat score remains flat across values of the leniency measure, while Appendix Figure 7 demonstrates that the distribution of leniency scores is highly similar between CEP and regular applicants, ruling out differences in instrument support across tracks.

Taken together, these results provide strong evidence that examiner leniency satisfies the *relevance* and *exogeneity* conditions required for a valid instrument for admission to Sciences Po.

3.4 Random Allocation of Judges

To validate the use of judge leniency as an instrument for admission, we assess whether applicants were randomly assigned to oral examiners. The core identifying assumption behind our instrumental variable strategy is that, conditional on cohort, applicants are as-good-as-randomly assigned to judges. If this assumption holds, there should be no systematic relationship between applicant characteristics and the identity of the examiners.

To test this, we follow the approach introduced by Frandsen et al. (2023), which evaluates whether judge identity explains any variation in pre-treatment characteristics. For each observable variable X, we compute the share of its variance that is explained by examiner fixed effects within cohort, that is, the R^2 from a regression of X on judge identifiers. We then simulate a null distribution of R^2 values that would be observed under random assignment by randomly re-assigning applicants to judges within cohort and recalculating the R^2 100 times. Comparing the observed R^2 to this placebo distribution allows us to evaluate whether judge assignment deviates from randomness in a statistically meaningful way.

We implement this procedure separately for the regular admission track (BAC0) and the affirmative action track (CEP). The set of characteristics we examine includes both academic variables, such as the final baccalaureate grade, the anticipated baccalaureat exam grades, and socio-demographic indicators, including type of baccalauréat, gender, scholarship status, and a parental socio-economic index (IPS).

The results are presented in Appendix Figure 10, which plots, for each variable and admission track, the simulated distribution of R^2 values under random assignment (grey density), the 95th percentile of the placebo distribution (red line), and the actual R^2 observed in the data (black line). Across nearly all variables and both admission tracks, the observed R^2 lies well within the bounds of the simulated distribution. This indicates that judge assignment is not systematically related to any observable applicant characteristic.

The only partial exception is the plot for the parental socioeconomc status in the CEP track. In that case, the observed R^2 appears to lie just above the 95th percentile of the null distribution. However, the difference is marginal and likely reflects statistical noise rather than substantive sorting.

Overall, the evidence supports the validity of the random assignment assumption. Judge composition is orthogonal to applicants' academic and socio-demographic background in both tracks, which reinforces the credibility of our identification strategy.

The variation in admission outcomes induced by differences in examiner leniency can thus be interpreted as exogenous.

Furthermore, our main instrument is based on *conditional leniency*, which captures residual variation in judge behavior after conditioning on observable applicant characteristics. This approach further mitigates concerns about imperfect randomization, as any predictable differences in student composition across judges are already absorbed in the first-stage residualization. The combination of the placebo tests and the construction of the instrument reinforces the validity of our identification strategy.

3.5 Monotonicity

Theoretical Justification The monotonicity assumption is essential for interpreting instrumental variable (IV) estimates as causal effects. It requires that the instrument here, judge leniency shifts the probability of treatment (admission) in a consistent direction. Specifically, if a given applicant would have been admitted by a strict judge, they should not be rejected by a more lenient one. In other words, more lenient judges must be at least as likely to admit any applicant as stricter judges. This implies a coherent ranking of judges by leniency that holds across all applicants.

In our setting, this assumption ensures that we can identify a well-defined group of compliers: applicants whose admission outcome depends on which judge they are randomly assigned to. If monotonicity fails,for example, because some judges are strict with certain types of applicants and lenient with others, then the instrument may induce inconsistent shifts in treatment, and the IV estimate may no longer reflect a meaningful causal effect.

Tests and Results To assess whether this assumption holds in our setting, we follow the approach laid out in Frandsen et al. (2023) and run several diagnostics.

First, we estimate a flexible regression of the average admission rate (by judge) on judge leniency using B-splines. The goal is to check whether outcomes can be explained smoothly by the instrument. If monotonicity holds, there should be no large discontinuities or judge-specific outliers. As shown in Figure 11, the relationship is smooth and increasing. The corresponding regression (Table 7) confirms a good fit.

Second, we run a simple linear regression of average admission on leniency to check whether the slope is too steep, that is, whether the variation in outcomes exceeds what could plausibly be explained by variation in admission. This is known as the slope test. In our case, the slope (Table 6) is well below 1 and within bounds, consistent with monotonicity.

We also divide judges into leniency quintiles and plot the average admission rate in each bin (Figure 14). The trend is monotonic: stricter judges admit fewer candidates. The corresponding table confirms the visual result. Finally, we plot admission rates against judge leniency separately for different groups by gender, Bac honors, IPS quartile, and Baccalauréat track. The relationships are all in the expected direction. While some groups have fewer observations in the lower tail, especially among stricter judges, the patterns are broadly consistent across subgroups (Figure 13).

Overall, the diagnostics suggest that the monotonicity assumption holds reasonably well in this context.

3.6 Complier Identification and Profiling

A key step in interpreting our instrumental-variables estimates is clarifying who the compliers are (i.e., the applicants whose admission outcome is shifted by examiner leniency). Our IV strategy identifies a Local Average Treatment Effect (LATE), which is the causal impact of admission on those applicants at the margin of being admitted depending on whether they face a lenient or strict examiner (**?**).

To characterize these students, we follow **?**, who develop a nonparametric method to profile compliers and noncompliers by combining the logic of principal stratification with observed pretreatment covariates. Because their approach is designed for a binary instrument, we adapt it to our setting with a continuous instrument (examiner leniency) by discretizing the leniency score at several thresholds (median split, top tercile, top quartile, and above the bottom quartile). For each threshold, we compute the first-stage F-statistic, and we select the threshold with the strongest first stage. This binary split distinguishes applicants seen by stricter vs. more lenient examiners, creating an instrument that allows identification of complier shares and profiles.

For each group, we estimate the shares of the three principal strata implied by the monotonicity assumption: always-takers (admitted regardless of examiner), never-takers (rejected regardless), and compliers (marginal applicants admitted only by lenient examiners). Bootstrap methods provide confidence intervals for both complier shares and complier means of covariates.

The strongest threshold for Regular admission is the median split, while for CEP it is the top tercile split. The resulting estimates indicate that compliers represent about 9 - 10% of Regular Admission applicants, alongside 42% of alwaystakers and 48% of nevertakers. For CEP, the share of compliers is smaller, about 7% of applicants, with more students falling in the alwaystaker category.

Turning to complier profiles, our analysis suggests that Compliers' parental SES (IPS) lies between that of always-takers and nevertakers, clustering in the middle of the distribution (Figure **??**). On both the baccalauréat grade and the average of anticipated exam grades, compliers score between the stronger always-takers and weaker never-takers (Figure **??**).

Turning to complier profiles, our analysis suggests that compliers' parental SES (IPS) is consistently between that of always-takers and never-takers (Figure **??**). Alway-takers tend to come from higher-SES families, while nevertakers cluster at the lower end of the distribution. By contrast, compliers concentrate in the middle, suggesting that examiner leniency shifts admission decisions primarily for candidates from neither the most advantaged nor the most disadvantaged backgrounds among the students of their track.

Regarding academic preparation, in both the baccalaureate grade and the average of anticipated exam grades, compliers fall between the stronger always-takers and weaker never-takers (Figure **??**). In Regular Admission, compliers average around $12 - 13/20$ on the baccalauréat, compared to higher means $(14 - 15/20)$ for alwaystakers and lower means $(11/20)$ for nevertakers. This confirms that leniency matters at the academic "margin": examiners rarely swing outcomes for the strongest or weakest candidates, but instead for those whose records are intermediate. Finally, in terms of baccalaureate type, compliers are more likely to hold a general baccalaureate (S, ES, L) than a technological one. Within the general tracks, compliers are disproportionately from the scientific (S) and economics/social sciences (ES) tracks, and less frequently from the literary (L) track. Always-takers are especially concentrated in the scientific track, particularly with "Très Bien honors", while never-takers are more likely to come from the literary or technological tracks (Figure 15).

For completeness, we also considered an alternative approach, the Marginal Treatment Effect (MTE) framework (Heckman et al., 2006a; Frölich and Melly, 2017a) which is suited to continuous instruments. This method characterizes treatment effects along the entire distribution of the latent propensity to be admitted, allowing for a continuous description of marginal students. However, implementing MTE requires stronger parametric assumptions and is less transparent to communicate. We therefore present the Marbach-Hangartner profiling as our main approach, while the MTE framework provides a useful robustness check and conceptual complement.

4 Main Results

4.1 Mismatch effect ? CEP applicants benefit the most from admission to Sciences Po

We estimate two-stage least squares models using examiner leniency as an instrument for admission to Sciences Po. Throughout, we report effects separately for the Regular Admission and CEP tracks and then in pooled specifications with CEP interactions to understand whether admission impacts CEP and non CEP students differently.

4.1.1 Short term outcomes

Enrollment and first-year academic progress (extensive margin)
Enrollment into higher education is near-universal for both admitted and non-admitted candidates in both tracks (Regular Admission and CEP), and admission to Sciences Po does not meaningfully change the probability of enrolling somewhere. This pattern is clear in the descriptive trajectories (Figure 18). First-year success (no grade repetition) improves for Regular applicants when admitted by about 0.10 percentage points, while the corresponding coefficient for CEP applicants is small and imprecise. In the pooled model, Sciences Po's admission main effect is positive (approx +0.10), and the CEP x Admitted interaction is not significant, indicating no robust penalty for CEP admission (Table 13).

On-time bachelor completion (3 years)

Regular-track students see a large increase in on-time bachelor completion when admitted (+0.45 p.p.), whereas the CEP-track coefficient is near zero and imprecise. In pooled regressions, the admitted main effect is +0.440.45 with a negative CEP interaction (0.49 to 0.53), which implies a sizable boost for Regular admission students and no detectable change for CEP students, which aligns with the track-specific estimates (Table 16).

A natural explanation for the large effect among Regular applicants, and the near-zero effect among CEP applicants is the counterfactual pathway when not admitted. A substantial share of qualified-but-not-admitted Regular admission applicants enrolls in CPGE rather than a university bachelor track, so they are not on a bachelor-degree pathway at all; admission to Sciences Po therefore moves them from a non-bachelor track into a bachelor program, mechanically boosting three-year BA completion. By contrast, non-admitted CEP applicants predominantly enter general University Bachelor programs, so admission does not change their likelihood of being on a bachelor path to begin with, yielding a small net effect on three-year completion. This pattern is visible in the program-type breakdown (Figure 17) and consistent with the structure of French higher-education pathways in which CPGE is a preparatory route that does not award a bachelors degree (Figure 1).

Program quality at entry (intensive margin)

On the intensive margin, the firstalmost mechanicaleffect is that admission to Sciences Po produces a significant jump in the quality of the next program students enter. Using a program-quality index constructed from the average baccalaureate grade of entrants in each destination program, admission to Sciences Po raises this index for both Regular admission applicants and CEP applicants. In pooled specifications, the $CEP \times Admitted$ interaction is positive and significant, confirming a larger quality upgrade for CEP applicants than for Regular admission applicants (Table **??**). This differential is what one would expect given that Regular admission applicants have better outside options.

4.1.2 Long term outcomes

Masters completion within six years

We detect no statistically significant effect of admission on masters degree completion within six years in either track or in the pooled model. Regular-track estimates are small and imprecise (+0.07), CEP-track estimates are likewise imprecise, and in the pooled regression the cep x admitetd interaction is not significant (Table 17).

Selectivity of masters programs attended

While overall masters completion is unaffected, admission shifts students into more selective masters programs.

We find that Admission to Sciences Po raises the probability of attending a Top-30 Grande École master by about +0.230.25 percentage points. for Regular admission applicants and +0.380.39 percentage points for CEP applicants; pooled regressions show a positive (though imprecise) CEP x Admitted interaction, consistent with larger gains for CEP (Table 22).

Rank-based measures tell the same story. Admission substantially increases the chance of attending a top-decile (D1) master (+0.270.29 percentage points for Regular Admission students; +0.700.73 p.p. for CEP students), while shifting students away from lower ranked programs . In pooled models, the CEP Œ Admitted interaction is positive for top-decile (D1) master (+0.410.43 percentage points), and negative for mid-tier outcomes like Q2 (0.39 to 0.40 percentage points), confirming disproportionate upward reallocation for CEP applicants that get admitted into Sciences Po (Table 23-34 ; Figure 20- 22).

Bottom line

In the short run, admission boosts program quality without harming enrollment or early progress, and in the long run, it reallocates students, , especially CEP students.into more selective masters programs, with no reduction in master completion. The policy thus improves access on the intensive margin and shows no systematic evidence of mismatch.

4.2 Efficiency and the EquityEfficiency Trade-Off

The final part of our analysis evaluates whether the CEP policy altered the efficiency of seat allocation at Sciences Po. While mismatch focuses on affirmative action students individual welfare, efficiency concerns aggregate welfare: whether redistributing seats toward CEP students reduced or enhanced the overall returns to admission. In our framework, efficiency loss would occur if the marginal CEP admit gained less from admission than the marginal regular-track applicant displaced by the policy. Conversely, efficiency gain arises if affirmative action reallocates access to students who benefit more from the opportunity to attend an elite institution.

To estimate these differential effects, we pool CEP and regular-track applicants and estimate an instrumental variable model of the form:

$$Y_{it} = \beta_1 \text{Admitted}_i + \beta_2(\text{Admitted}_i \times \text{CEP}_i) + \beta_3 \text{CEP}_i + X_i'\gamma + \delta_t + \epsilon_i, \tag{3}$$

where Y_{it} denotes an academic or post-graduate outcome, Admitted_i is an indicator for admission at Sciences Po, and CEP_i identifies students applying through the affirmative-action track. The coefficient β_1 captures the causal effect of admission for regular-track students, while β_2 measures the additional (differential) effect of admission for CEP applicants. The sum $\beta_1 + \beta_2$ thus represents the total effect of admission for CEP students. Admission is instrumented using the conditional examiner leniency measure described in Section 3.

Extensive margin. Table **??** and Table **??** present the estimated effects of admission on degree progression and completion. Across all specifications, the differential admission effect (β_2) is statistically insignificant, implying that admission has comparable effects on persistence and graduation for CEP and regular students. This absence of a negative interaction coefficient rules out efficiency losses through lower completion rates among affirmative-action admits.

Extensive margin. Table 13, 19, present the estimated effects of admission on degree progression and completion. Across all specifications, the differential admission effect (β_2) is statistically insignificant, implying that admission has comparable effects on persistence and graduation for CEP and regular students. This absence of a negative interaction coefficient rules out efficiency losses through lower completion rates among affirmative-action admits.

Intensive Margin and long term gains. We next examine longer-term measures of educational and early-career success. Table 25, 28 31 34 reports the estimated effects of admission on Master's program selectivity and predicted earnings. The estimated β_2 coefficients are positive and significant for top-decile (D1) master (+0.410.43 percentage points), and negative for mid-tier outcomes like Q2 (0.39 to 0.40 percentage points), confirming disproportionate upward reallocation for CEP applicants that get admitted into Sciences Po

Moreover, predicted early-career earnings computed using survey administrative data on masters wage returns rise by 4.3% for CEP admits, whereas effects for regular admits are closer to 2.5%.

Interpretation. Taken together, the results indicate that the CEP program expanded access to elite education without sacrificing efficiency. While admission improves academic and career outcomes for all students, the gains are substantially larger for CEP applicants, suggesting that redistributing seats toward disadvantaged candidates *increased* the overall returns to admission. In welfare terms, the CEP policy enhances equity and efficiency simultaneously: targeted students succeed once admitted and derive greater benefits from elite education, showing that affirmative action can raise aggregate welfare when coupled with institutional support.

4.3 Adjustment and Academic Integration: What Happens Inside Sciences Po

The absence of mismatch among CEP admits suggests that once admitted, these students are able to adapt and succeed in a highly demanding environment. In this section, we examine what happens after admission and the mechanisms that may explain the convergence of outcomes between CEP and regular students. We focus on three dimensions of academic integration: (i) preparatory exposure in partner high schools through mandatory workshops, (ii) a pre-matriculation summer bootcamp introduced in 2016, and (iii) a first-semester mentoring program implemented on the Paris campus. These initiatives illustrate how Sciences Po coupled its affirmative action policy with targeted institutional support to ensure academic success.

Partner-School Workshops. As part of the CEP partnership, participating high schools are required to organize preparatory workshops for prospective applicants during the 11^{th} and 12^{th} grades. These workshops constitute the first level of support in the CEP pipeline. Until 2018, schools enjoyed substantial autonomy in their organization: some emphasized *culture générale* to address informational and cultural gaps, while others focused on analytical skills, oral expression, or introductions to history and political science. Attendance is mandatory for students intending to apply through the CEP pathway, ensuring that all candidates receive some degree of preparation before their interview at Sciences Po.

Since workshops were required for all CEP applicants and were not standardized prior to 2018, their effects cannot be separately identified from overall CEP participation.

Summer Bootcamp: Design and Empirical Strategy. In 2016, Sciences Po introduced a one-week *summer bootcamp* designed to help incoming CEP students transition to university-level expectations. The program, held in person until 2020, provided free travel, accommodation, and instruction from Sciences Po faculty. Its objectives were twofold: to familiarize students with academic norms (lectures, essays, debates) and to build confidence in an unfamiliar environment. Participation was initially capped at approximately 50 students per year. Priority for admission was granted to applicants who were *boursiers du lycée* (high-school scholarship holders),[2] but the number of eligible students never exceeded the programs capacity. As a result, some non-*boursiers du lycée* were also invited to fill remaining seats. In 2020, the bootcamp moved online and was opened to all incoming students, eliminating capacity constraints.

Because we observe individual participation, we estimate the causal effect of attendance using a fuzzy instrumental variables (2SLS) design. During 2016–2019, priority eligibility increased the likelihood of participation but did not perfectly determine it, providing a natural source of quasi-random variation. We use *boursier du lycée* status as an instrument for bootcamp attendance, restricting the sample to CEP admits in 2016–2019 to maintain a consistent policy regime. The first and second stages are given by:

$$\text{Bootcamp}_i = \pi_1 \,\text{BoursierLycee}_i + X_i'\pi_2 + \tau_y + \varepsilon_i, \tag{4}$$

$$Y_i = \beta^{BC} \,\widehat{\text{Bootcamp}}_i + X_i'\gamma + \tau_y + u_i, \tag{5}$$

where $\text{BoursierLycee}_i = 1$ for CEP admits who held a high-school scholarship in cohorts 2016–2019, τ_y are cohort fixed effects, and X_i includes baseline covariates (gender, baccalauréat type and honors, anticipated grades, and campus). Standard errors are clustered at the cohort level. Under standard assumptions, β^{BC} identifies the local average treatment effect (LATE) for students whose bootcamp participation was influenced by priority eligibility.

As complementary evidence, we estimate cohort-level intent-to-treat (ITT) effects of bootcamp *availability* by comparing pre- and post-introduction cohorts (2012–2015

[2]The *bourse nationale de lycée* is a means-tested national grant for high-school students from low-income families, providing an annual allowance to cover schooling costs. It serves as an official low-income indicator, roughly comparable to eligibility for free or reduced-price lunch in the United States.

vs. 2016–2019):

$$Y_{icy} = \alpha + \theta\,\mathbf{1}\{y \geq 2016\} + \lambda_c + \tau_y + X_i'\gamma + \varepsilon_{icy}, \tag{6}$$

where λ_c are campus fixed effects. We also document the 2020 shift to a remote, open-access format in an event-study specification centered on $y_0 = 2020$ to examine levels and pre-trends. We do not use post-2020 availability as an instrument for participation, since the near-universal access introduced at that point removed meaningful variation in treatment intensity.

First-stage estimates confirm that *boursier du lycée* status is a strong predictor of bootcamp participation between 2016 and 2020, supporting its validity as an instrument. The 2SLS results suggest that participation in the bootcamp increases the likelihood of successfully completing the first year (i.e., not repeating) and has modest effects on GPA. These findings are consistent with the program helping students better navigate the transition to university by clarifying academic expectations, reducing stress, and strengthening early academic confidence.

Mentoring and Tutoring: Causal Effect of Participation. We also observe whether a CEP student used the first-semester mentoring/tutoring program. Prior to 2020, mentoring was available only on the Paris campus; after 2020, it expanded to all campuses. We use program *availability* as an instrument for individual participation, exploiting campus-by-time variation. Restricting to CEP admits, the first and second stages are:

$$\text{Mentoring}_i = \kappa_1 \left(\text{Paris}_c \times \text{Pre2020}_y\right) + X_i'\kappa_2 + \lambda_c + \tau_y + \nu_i, \tag{7}$$

$$Y_i = \beta^M \, \widehat{\text{Mentoring}}_i + X_i'\gamma + \lambda_c + \tau_y + e_i, \tag{8}$$

where $(\text{Paris}_c \times \text{Pre2020}_y)$ indicates availability of mentoring before the 2020 expansion, λ_c and τ_y are campus and cohort fixed effects, and X_i includes baseline covariates. Standard errors are clustered at the campus level. This 2SLS design identifies a LATE for CEP students whose mentoring take-up responds to availability (compliers).

As a robustness check, we estimate a triple-difference ITT model contrasting CEP and regular students across Paris and non-Paris campuses before and after 2020:

$$Y_{icty} = \psi \left(\text{CEP}_i \times \text{Paris}_c \times \text{Pre2020}_y\right) + \text{all lower-order terms} + \lambda_c + \tau_y + X_i'\gamma + \varepsilon_{icty}, \tag{9}$$

which nets out campus- and time-specific shocks common to both tracks.

Academic Convergence. Taken together, these findings suggest that CEP students experience substantial academic convergence with their peers once inside Sciences Po. Initial performance gaps narrow over time. The combination of preparatory workshops, pre-entry orientation, and first-semester mentoring appears to have played a central role in facilitating this adjustment.

Interpretation. These institutional mechanisms help explain why we find no evidence of mismatch and no efficiency loss associated with CEP admission. The design of the CEP programpairing expanded access with sustained academic supportallowed students from underrepresented backgrounds to adapt quickly to Sciences Pos demanding curriculum. Although identifying the causal effects of each support measure is challenging, the timing of their introduction and the consistent convergence of outcomes across cohorts suggest that targeted adjustment mechanisms can effectively mitigate initial differences in preparation. In the broader context of the equityefficiency trade-off, these results highlight the importance of institutional design: affirmative action policies are most effective when access is coupled with complementary supports that foster academic integration and long-run success.

4.4 Robustness

We next assess the robustness of our main findings to alternative instrumental variables, specification choices, and sample definitions. This section verifies that the estimated effects of admission are not driven by a particular construction of the examiner leniency instrument, by omitted covariates, or by cohort-specific shocks.

4.4.1 Alternative Instruments

Our preferred specification uses the *average conditional leniency* of the oral examiners assigned to each candidate as the instrument for admission. This measure captures the mean deviation of each examiner's observed admission rate (conditional on observable student characteristics) from the institutional average, and it provides a smooth and unbiased proxy for the random component of examiner generosity.

To confirm that the results are not sensitive to the choice of instrument, we construct two alternative measures of examiner leniency described earlier in Section 3 and illustrated in Figure 6.

- **Minimum leniency:** the leniency of the most stringent examiner in the jury (the examiner with the lowest predicted admission rate among those assigned to the applicant).

- **Maximum leniency:** the leniency of the most generous examiner in the jury (the examiner with the highest predicted admission rate).

Both measures are highly correlated with our main instrument, ensuring that they provide independent robustness tests. For each specification, we re-estimate the IV model replacing the baseline instrument with these alternative ones.

Across all outcomesadmission, first-year GPA, graduation, and master's enrollmentthe estimated coefficients remain stable in both magnitude and significance. The effects obtained using the minimum and maximum leniency instruments fall well within the 95% confidence interval of the baseline average-instrument estimate. These findings indicate that our main results are not driven by the specific aggregation method used to construct the leniency measure, but rather by the exogenous variation in examiner generosity arising from random assignment.

4.4.2 Robustness to Controls and Specification Choices

Our baseline IV estimates are reported without additional controls beyond cohort fixed effects, in order to isolate the variation induced by examiner leniency without conditioning on covariates potentially correlated with treatment assignment. To assess sensitivity, we progressively add controls and fixed effects.

Table **??** summarizes results from four specifications: (1) the baseline IV with no controls; (2) IV including cohort fixed effects; (3) IV adding student-level covariates (anticipated baccalauréat grades, type, and honors); and (4) IV including both student controls and campus fixed effects. Across all outcomes, coefficients remain stable in sign and magnitude. These results confirm that our estimates are not sensitive to the inclusion of additional covariates or to alternative model specifications, supporting the validity of the instrument and the robustness of our main findings.

4.4.3 Cohort and Track-Specific Stability

Finally, we verify that our results are not driven by specific admission regimes or cohorts. We re-estimate the main IV model separately for pre-reform cohorts (2012–2015), post-bootcamp cohorts (2016–2019), and cohorts after the 2020 reform that eliminated the written exam and coincided with the COVID-19 shock. We also estimate the model separately for the regular (BAC0) and CEP tracks.

The estimated admission effects remain positive and statistically significant across all periods and tracks, with overlapping confidence intervals. These results confirm that the estimated effects of admission are stable across institutional contexts and not driven by a single cohort or selection regime.

5 Heterogeneity and aspirations controls

Having established the overall effects of CEP admission on access and subsequent outcomes, we next examine whether these effects differ across student subgroups. Understanding heterogeneity is central to interpreting the welfare implications of affirmative action: if gains are concentrated among disadvantaged or underrepresented students, the policy may simultaneously enhance equity and efficiency. Conversely, if effects are uniform across groups, this would suggest that the CEP policy primarily expands access without redistributing opportunity.

5.1 By Student Characteristics: SES, Gender, and Baccalauréat Track

We begin by investigating heterogeneity along three dimensions of student background that are central to the affirmative action debate: socioeconomic status (SES), gender, and academic specialization in high school. Socioeconomic heterogeneity is assessed using two complementary indicators: parental socioeconomic index (*IPS*) and eligibility for a high-school scholarship (*boursier du lycée*). Gender heterogeneity is captured by a female dummy. Academic background is proxied by the type of *baccalauréat* obtained (scientific, economic/social, literary, or other tracks).

Formally, we estimate specifications of the form:

$$Y_i = \beta_1 \widehat{\text{Admitted}}_i + \beta_2 \widehat{\text{Admitted}}_i \times G_i + X_i'\gamma + \tau_y + \lambda_c + \varepsilon_i, \tag{10}$$

where G_i denotes the relevant subgroup indicator (low-SES, female, or non-scientific *baccalauréat*). The coefficient β_2 captures the effect of differential treatment for that subgroup. All models are estimated using the same judge leniency instrument described earlier, with the full baseline control set (student background and pre-admission aspiration variables) included in both stages.

5.2 Aspirations and Outside-Option Controls.

A key component of the control set captures students pre-admission aspirations and outside options, derived from data on applications submitted through Frances centralized higher-education platform. Because the structure of this platform changed over timefrom the *Admission Post-Bac* (APB) system before 2018 to the *Parcoursup* system thereafterthe available information differs across cohorts. We therefore construct two sets of aspiration controls, ensuring comparability within each period.

Pre-2018 (APB) cohorts. Under APB, applicants were required to rank their preferences. We exploit this information to construct a rich set of variables summarizing students ambition and feasible outside options: the total number of applications (*nb_voeux*), capturing the breadth of search; the type of the first-ranked program (*typeForm_v1*), capturing stated aspiration; the type of the highest feasible offer (*typeForm_highest_feasible*), identifying the most selective program for which admission was possible; the preference rank of that offer (*min_rk_feasible*), indicating proximity to the students top choice; and a quantitative indicator of outside-option selectivity (*qual-*

ity_form_adm_apb), based on the programs admission cutoff. Together, these variables provide a detailed view of students ambitions and realistic opportunities before the oral examination.

Post-2018 (Parcoursup) cohorts. Beginning in 2018, the *Admission Post-Bac* system was replaced by *Parcoursup*, which no longer collects ranked preferences and instead operates a continuous matching process. Although ranking data are unavailable, the platform still records the types of programs to which each student applied and, in most cases, the final admission outcomes. We therefore construct a set of type-based indicators summarizing applicants aspirations and outside options: the total number of applications (*nb_voeux*);indicators for whether the student applied to major program types (*candi_CPGE*, *candi_L1*, *candi_DUT*); and corresponding indicators for whether they received at least one admission offer (*adm_CPGE*, *adm_L1*, *adm_DUT*).

All aspiration variables are measured prior to examiner assignment and are therefore unaffected by the treatment. Because the structure and content of these controls differ across platforms, we pool all cohorts but include platform fixed effects and allow the coefficients on aspiration and outside-option variables to differ by platform type (APB vs. Parcoursup). This approach preserves statistical power while accommodating differences in available information across periods. In practice, we estimate all specifications with the full set of student covariates, cohort and campus fixed effects, and platform-specific aspiration controls included in both stages of the IV. As a robustness check, we verify that results are similar when the model is estimated separately for pre-2018 (APB) and post-2018 (Parcoursup) cohorts, confirming that the main treatment effects are stable across platforms.

6 Policy Discussion

6.1 Implications for Elite Admissions in France

The CEP experiment provides direct lessons for the future of selective admissions in France. By targeting high schools located in disadvantaged areas, the program substantially increased access for students from low-income and underrepresented backgrounds without compromising academic success.

These results are particularly relevant in the context of ongoing debates on *égalité des chances* and the design of the *Parcoursup* system, where concerns about transparency and fairness in selection persist. The CEP model demonstrates that meaningful progress on equity requires not only differentiated access criteria but also sustained institutional support. CEP students benefited from preparatory workshops, mentoring, and academic follow-up that allowed them to converge with regular-track students after admission. A purely quota-based expansion of access would likely have been less successful. A policy mix that combines selective admissions with structured support mechanisms can thus be scaled across other *Grandes Écoles* and elite programs seeking to diversify while preserving standards.

6.2 Lessons for Indirect Affirmative Action Globally

Beyond France, the CEP offers valuable lessons for countries operating within legal or political constraints on group-based affirmative action. Frances universalist framework, which prohibits race- or income-based quotas. Territorial or school-based targeting, as used in the CEP, represents a politically acceptable and administratively feasible alternative. It aligns institutional incentives with broader social objectives while avoiding explicit identity-based classifications.

The experience of Sciences Po highlights two design principles with broader relevance. First, affirmative action is most effective when it targets institutional opportunity gaps, such as unequal access to information, guidance, and networks, rather than focusing solely on individual characteristics. Second, successful inclusion requires complementary academic investments: CEP students strong outcomes reflect not only admissions reform but also post-admission support through bridging programs, mentoring, and monitoring. These lessons are particularly relevant for selective systems seeking to combine social diversity with high academic standards under legal or political constraints.

6.3 Trade-Offs Between Equity and Efficiency in Design

The CEP results directly challenge the presumed trade-off between equity and efficiency in selective education. Expanding access through affirmative action increased diversity and equity while also improving aggregate efficiency: CEP students exhibit larger marginal returns to admissionboth in masters program quality and predicted earningsthan regular-track applicants. This finding suggests that reallocating seats toward disadvantaged but high-potential students can raise total welfare.

The conditions under which affirmative action enhances efficiency are consistent with modern micro-welfare theory (**???**). When treatment effects are heterogeneous, policies that target individuals with the highest marginal returnssuch as academically strong but underrepresented studentscan increase both fairness and productivity. The CEPs success reflects this combination of precise targeting, sustained institutional support, and transparent selection procedures.

More broadly, the French case shows that when affirmative action is well-designed and complemented by academic investment, it can simultaneously promote fairness and aggregate efficiency.

7 Conclusion

This paper examined the causal effects of Sciences Pos affirmative action policy on access to and outcomes within elite higher education. Using examiner leniency as a quasi-random source of variation in admission, we estimated the returns to being admitted through both the regular and CEP tracks. Across outcomes, we find no evidence of mismatch.

If anything, the results suggest that low-SES and CEP applicants benefit slightly more from admission, consistent with higher marginal returns to elite access among underrepresented groups. These findings imply that the CEP policy increased both equity and efficiency by expanding opportunity without lowering performance standards. Taken together, the evidence supports the view that carefully designed affirmative action can reduce social inequality in access to elite education while enhancing overall welfare.

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A Appendix Figures

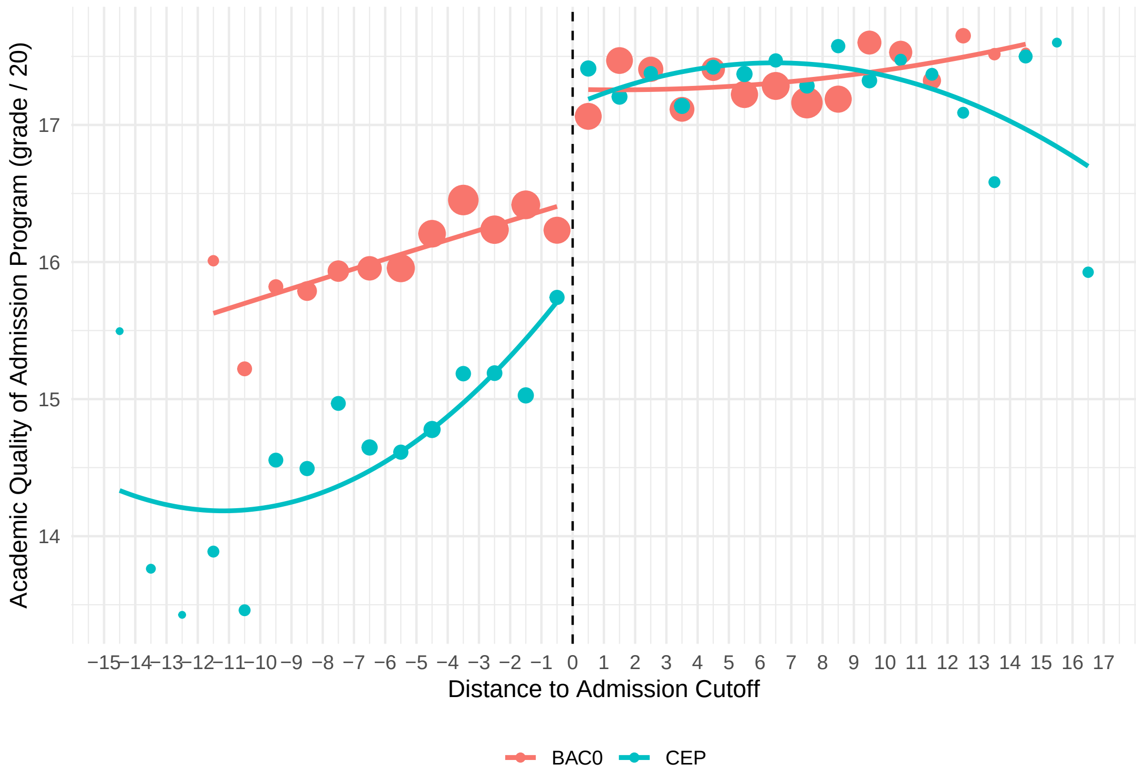


Figure 3: Quality of the admission program at the admission cutoff (post-reform).

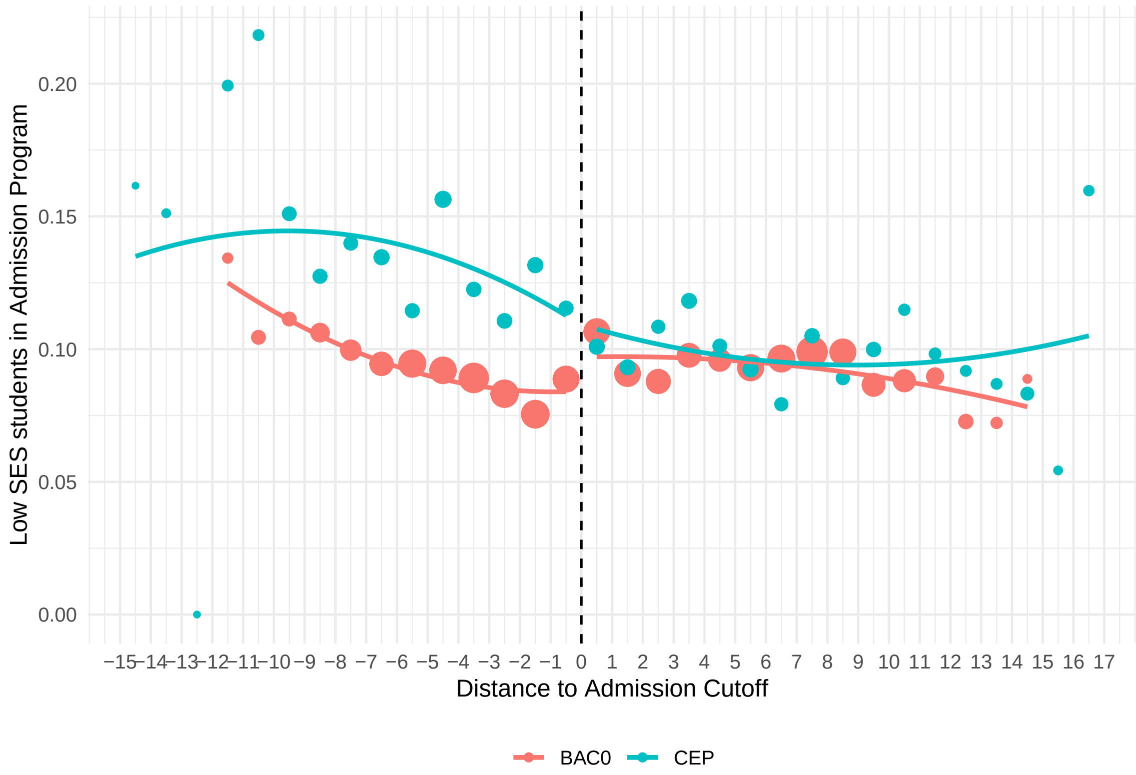


Figure 4: Social Index of the admission program at the admission cutoff (post-reform).

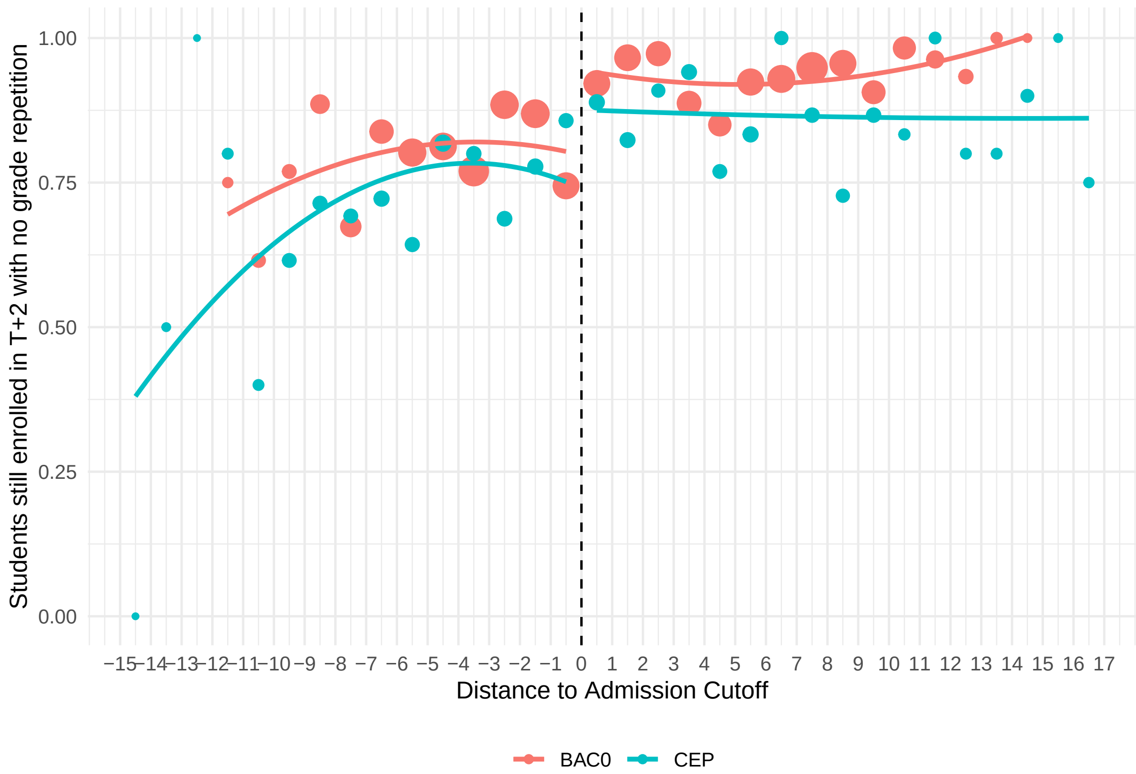
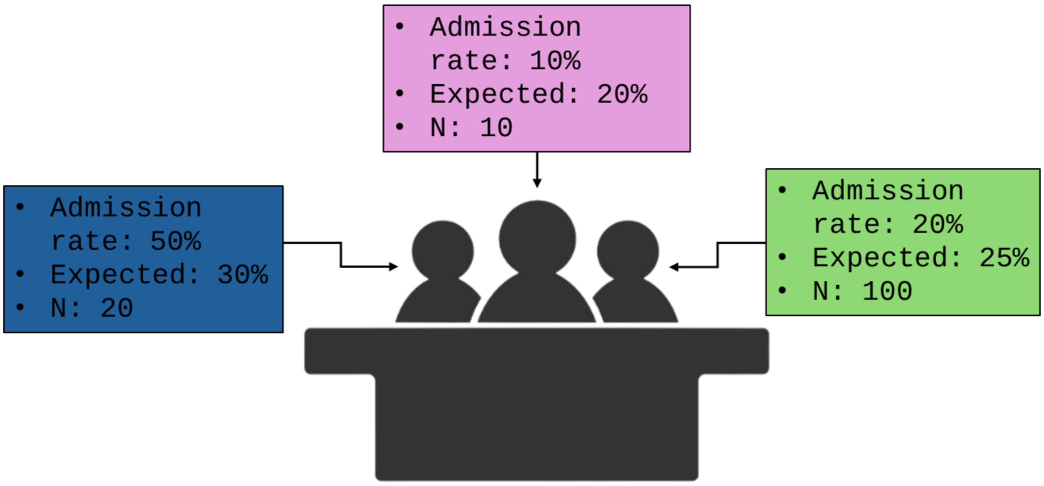


Figure 5: Students still enrolled in t+2 with no grade repetition).



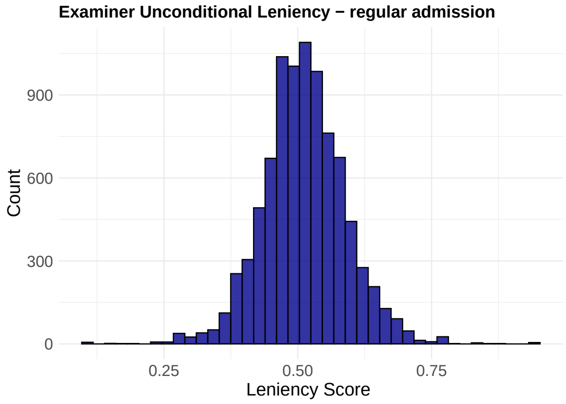
$$\textbf{\texttt{Conditional Leniency}} = \frac{1}{130}[20 * (.5 - .3) + 10 * (.1 - .2) + 100 * (.2 - .25)] \approx -.015$$

$$\textbf{\texttt{Absolute Leniency}} = \frac{1}{130}[20 * .5 + 10 * .1 + 100 * .2] \approx +.24$$

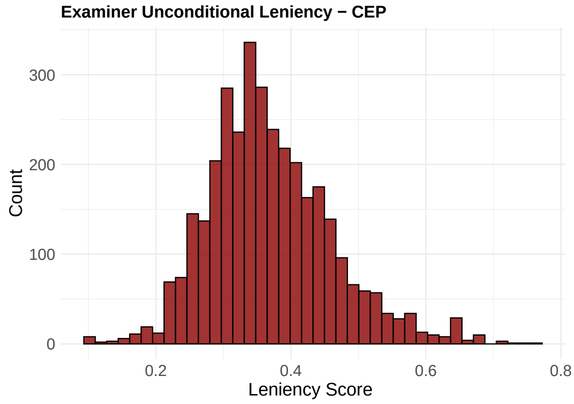
$$\textbf{\texttt{Min cond. Leniency}} = \min[(.5 - .3), (.1 - .2), (.2 - .25)] = -.10$$

$$\textbf{\texttt{Max cond. Leniency}} = \mathrm{m}ax[(.5 - .3), (.1 - .2), (.2 - .25)] = +.2$$

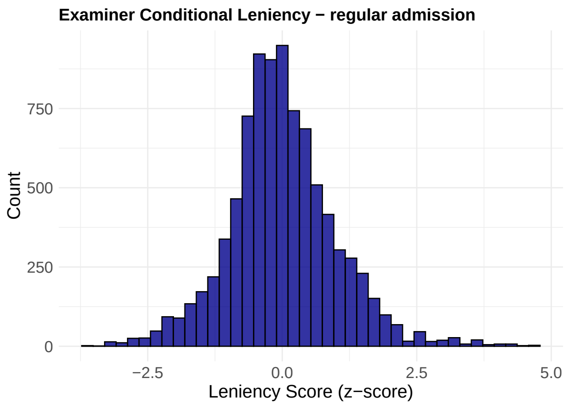
Figure 6: Illustration of judges' leniency.



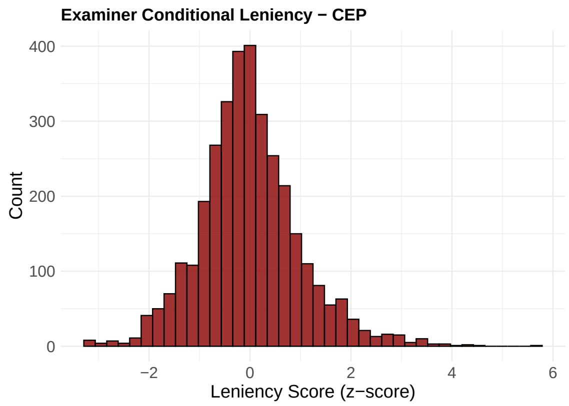
(a) Unconditional Leniency (Regular Admission)



(b) Unconditional Leniency (CEP)



(c) Conditional Leniency (Regular Admission)



(d) Conditional Leniency (CEP)

Figure 7: Distribution of Examiner Leniency by Admission Track. Each panel shows the leave-one-out leniency measure (unconditional or conditional) separately for regular (BAC0) and CEP applicants. The distributions are centered around zero and exhibit comparable support across tracks.

	Dependent variable:	
	Admission Probability	
	No Controls	Student + HS
	(1)	(2)
Unconditional Examiner Leniency (z-core)	0.077***	0.079***
	(0.005)	(0.005)
Instrument F-stat (leniency)	210.65	228.28
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.024	0.060
Adjusted R^2	0.024	0.055
Residual Std. Error	0.493 (df = 8685)	0.485 (df = 8641)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 2: First-Stage Regressions: Examiner Unconditional Leniency and Admission (Regular Admission)

	Dependent variable:	
	Admission Probabiilty	
	No Controls	Student + HS
	(1)	(2)
Unconditional Examiner Leniency (z-core)	0.032***	0.034***
	(0.008)	(0.007)
Instrument F-stat (leniency)	15.47	21.64
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,334	3,334
R^2	0.007	0.184
Adjusted R^2	0.004	0.174
Residual Std. Error	0.464 (df = 3326)	0.423 (df = 3295)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 3: First-Stage Regressions: Examiner Unconditional Leniency and Admission (CEP)

	Dependent variable:	
	Admission Probability	
	No Controls	Student + HS
	(1)	(2)
Conditional Examiner Leniency (z-core)	0.080***	0.083***
	(0.005)	(0.005)
Instrument F-stat (leniency)	227.93	252.25
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.026	0.063
Adjusted R^2	0.025	0.057
Residual Std. Error	0.492 (df = 8685)	0.484 (df = 8641)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 4: First-Stage Regressions: Examiner Conditional Leniency and Admission (Regular Admission)

	Dependent variable:	
	Admission Probability	
	No Controls	Student + HS
	(1)	(2)
Conditional Examiner Leniency (z-core)	0.036***	0.038***
	(0.008)	(0.007)
Instrument F-stat (leniency)	19.9	26.69
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.008	0.183
Adjusted R^2	0.006	0.174
Residual Std. Error	0.465 (df = 3288)	0.424 (df = 3258)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 5: First-Stage Regressions: Examiner Conditional Leniency and Admission (CEP)

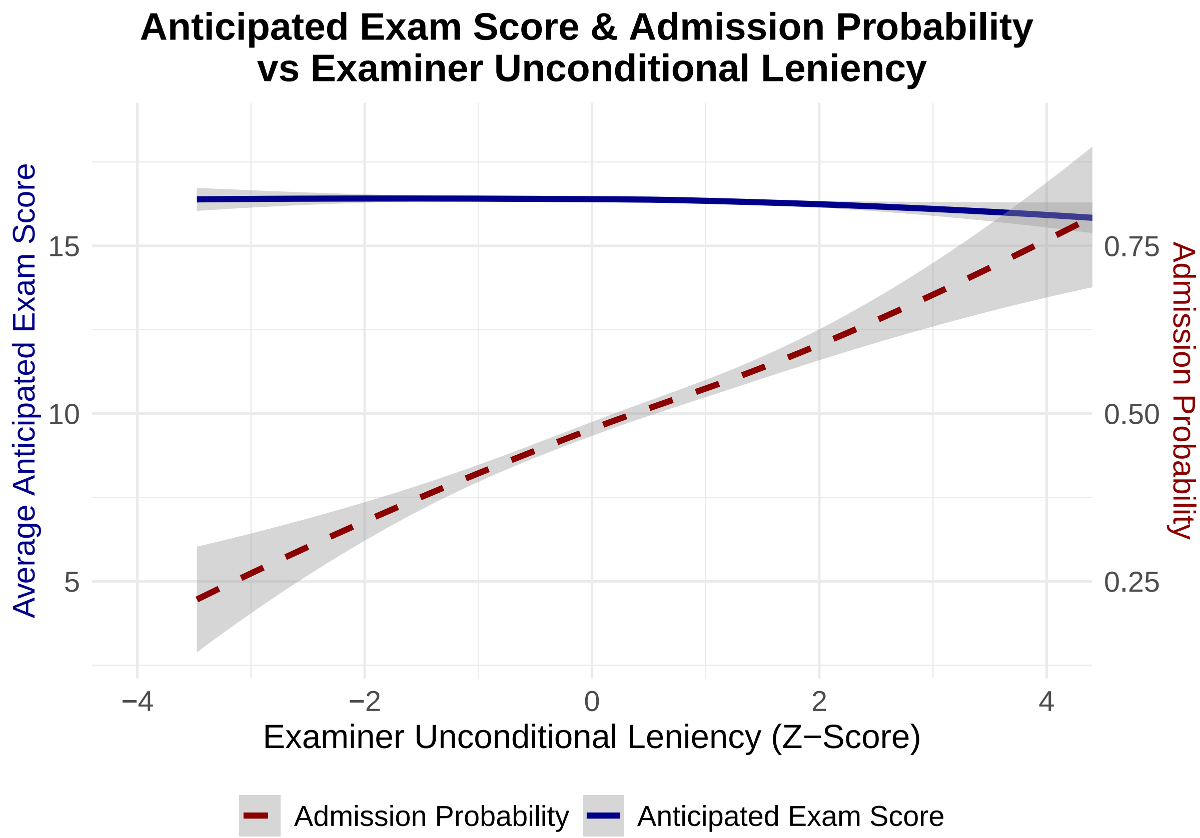


Figure 8: Baccalauréat Grade and Admission Probability vs. Examiner Leniency. This figure shows the relationship between examiner unconditional leniency and (i) anticipated baccalauréat grades and (ii) probability of admission. Each point represents an average within a bin of examiner leniency. The relationship with student ability is essentially flat, suggesting no systematic correlation between student ability and examiner leniency.

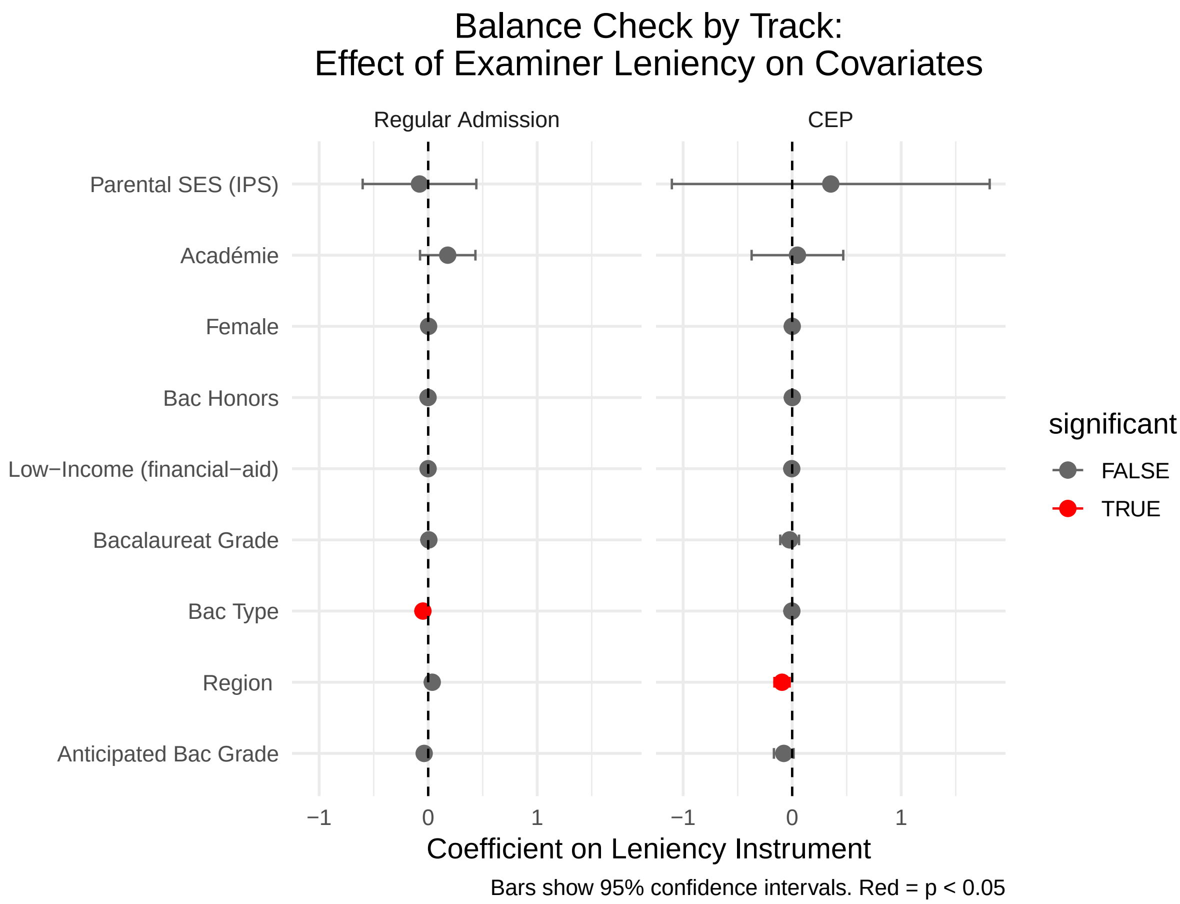
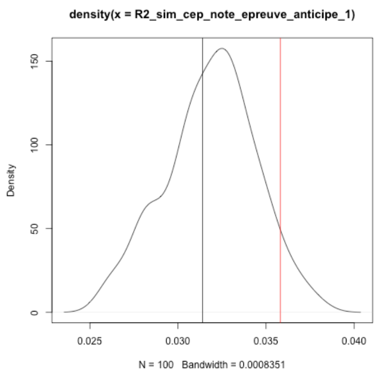
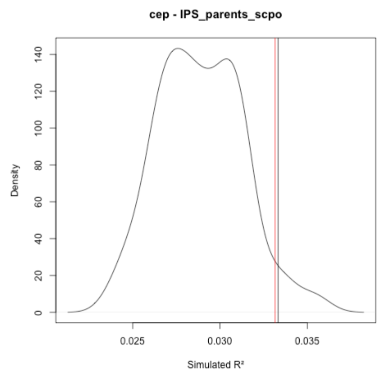


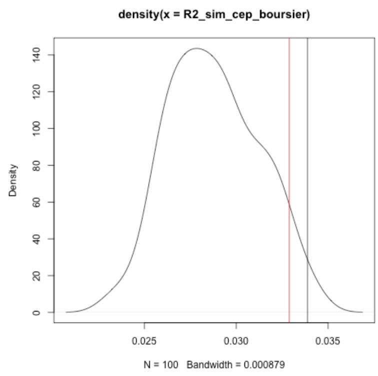
Figure 9: Balance Check by Track : Effect of Examiner Leniency on Covariates. This figure presents balance checks to assess the plausibility of quasi-random assignment of applicants to more or less lenient oral examiners. Each point represents the estimated coefficient from a regression of a baseline characteristic on the leniency instrument (examiner leniency), run separately for Regular Admission and CEP applicants. Bars indicate 95% confidence intervals. Red markers denote statistically significant associations ($p < 0.05$). The absence of systematic relationships across most covariates supports the assumption that examiner leniency is as good as randomly assigned with respect to observable student characteristics.



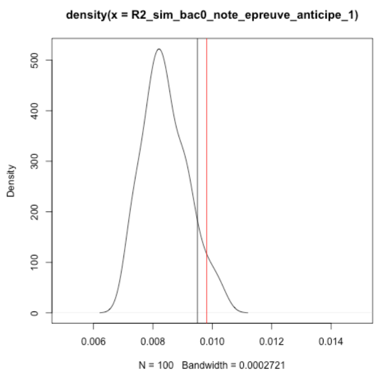
(a) Anticipated Baccalaureate Grad$e(CEP)$



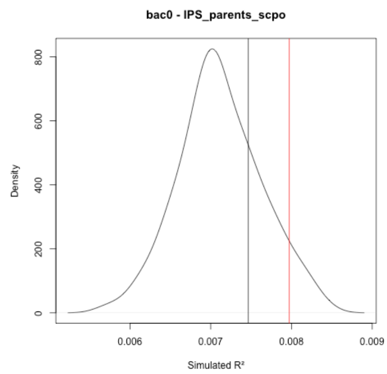
(b) Parental SES Index (CEP)



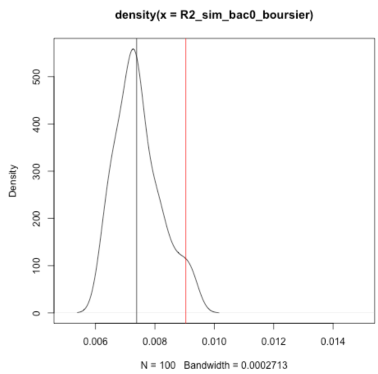
(c) Scholarship Status (CEP)



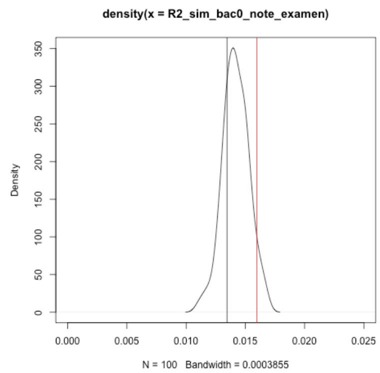
(d) Anticipated Baccalaureate Grade (Regular Admission)



(e) Parental SES Index (Regular Admission)



(f) Scholarship Status (Regular Admission)



(g) Written Exam Grade (Regular Admission)

Figure 10: Tests for Random Assignment of Judges Across Applicants. Each plot shows the simulated distribution of R^2 under random assignment of judges (black line: observed R^2; red line: 95% threshold from simulations).

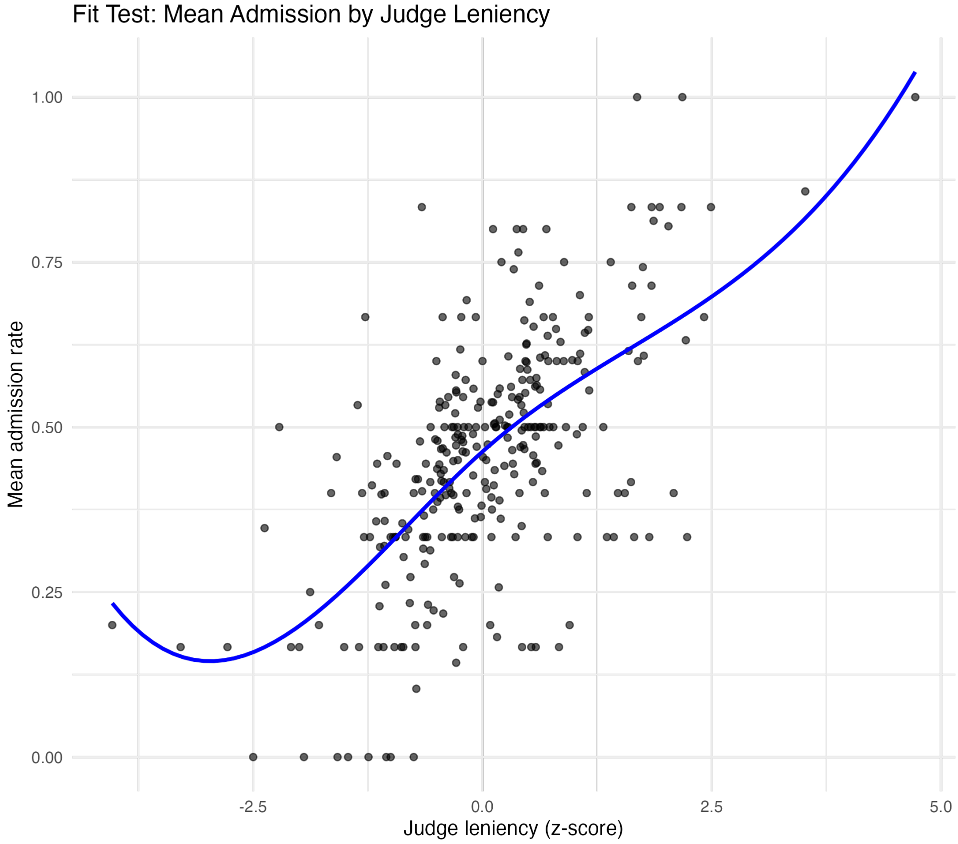


Figure 11: Flexible fit test.

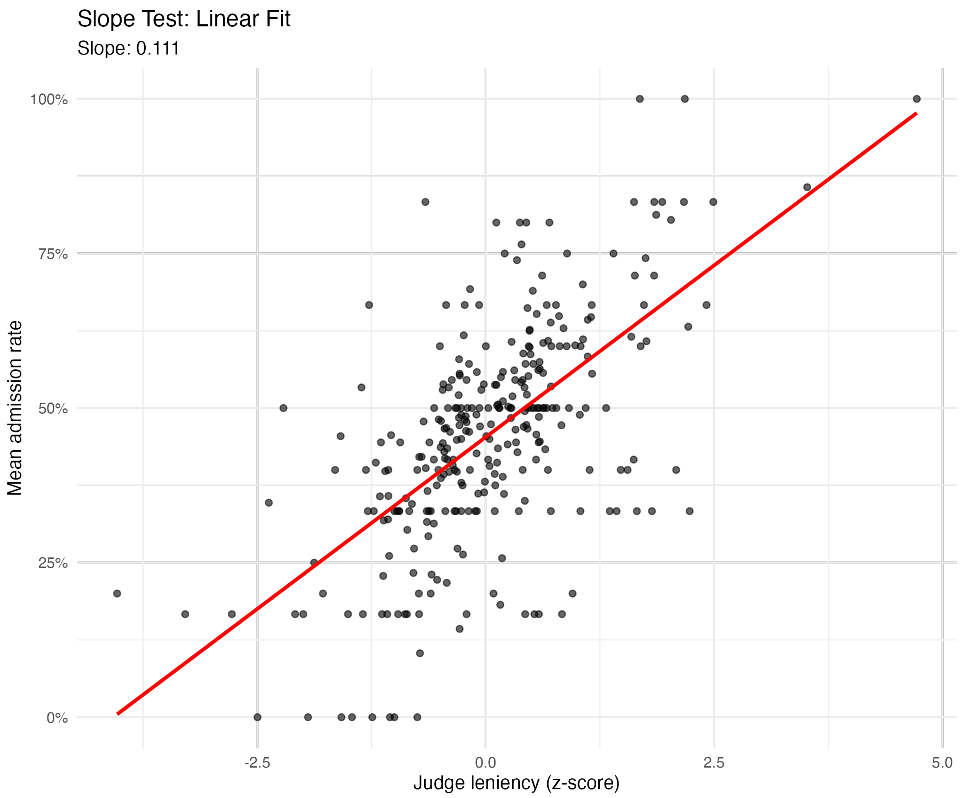


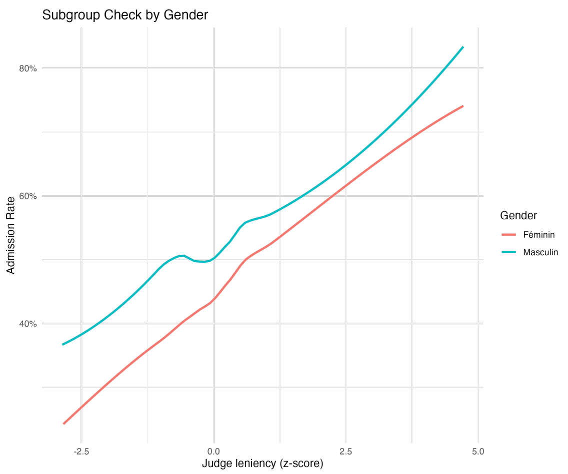
Figure 12: Bounded Slope test.

	Dependent variable:
	mean_admission
mean_leniency	0.111^{***}
	(0.008)
Constant	0.453^{***}
	(0.008)
Observations	306
R^2	0.387
Adjusted R^2	0.385
Residual Std. Error	0.142 (df = 304)
F Statistic	191.689^{***} (df = 1; 304)
Note:	*p<0.1; **p<0.05; ***p<0.01

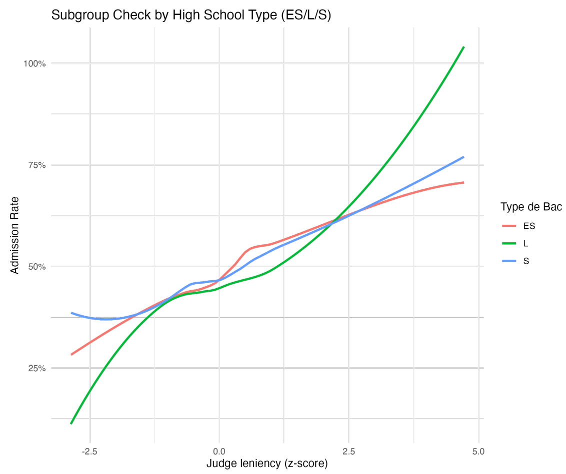
Table 6: Slope Test: Linear regression of mean admission on judge leniency

	Dependent variable:
	mean_admission
bs(mean_leniency, df = 4)1	−0.238
	(0.169)
bs(mean_leniency, df = 4)2	0.415***
	(0.126)
bs(mean_leniency, df = 4)3	0.434***
	(0.167)
bs(mean_leniency, df = 4)4	0.805***
	(0.176)
Constant	0.233*
	(0.122)
Observations	306
R^2	0.398
Adjusted R^2	0.390
Residual Std. Error	0.142 (df = 301)
F Statistic	49.701*** (df = 4; 301)
Note:	*p<0.1; **p<0.05; ***p<0.01

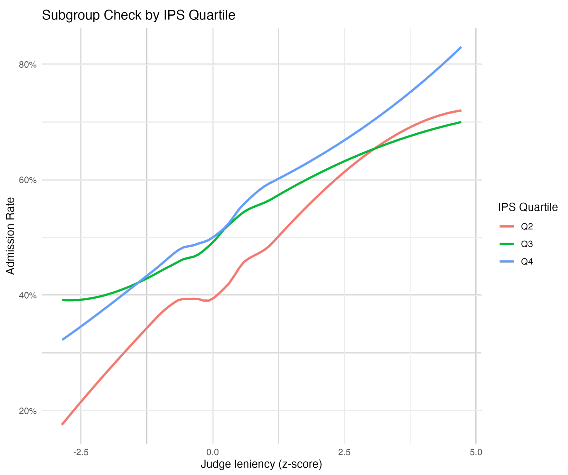
Table 7: Fit Test: B-spline regression of mean admission on judge leniency



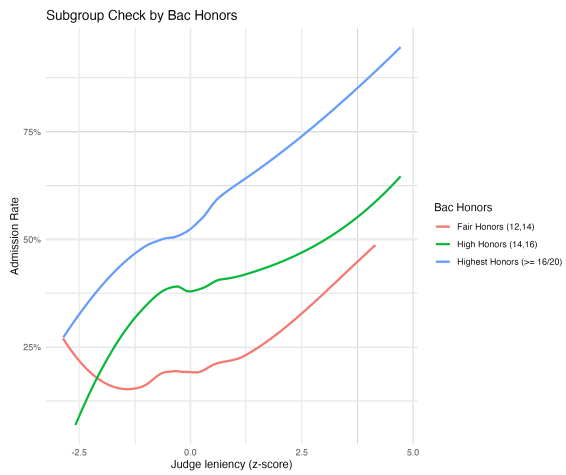
(a) Subgroup check - gender.



(b) Subgroup check - Baccalaureat major



(c) Subgroup check - Socioeconomic status



(d) Subgroup check - Baccalaureat Honors

Figure 13: Subgroup Monotonicity Checks

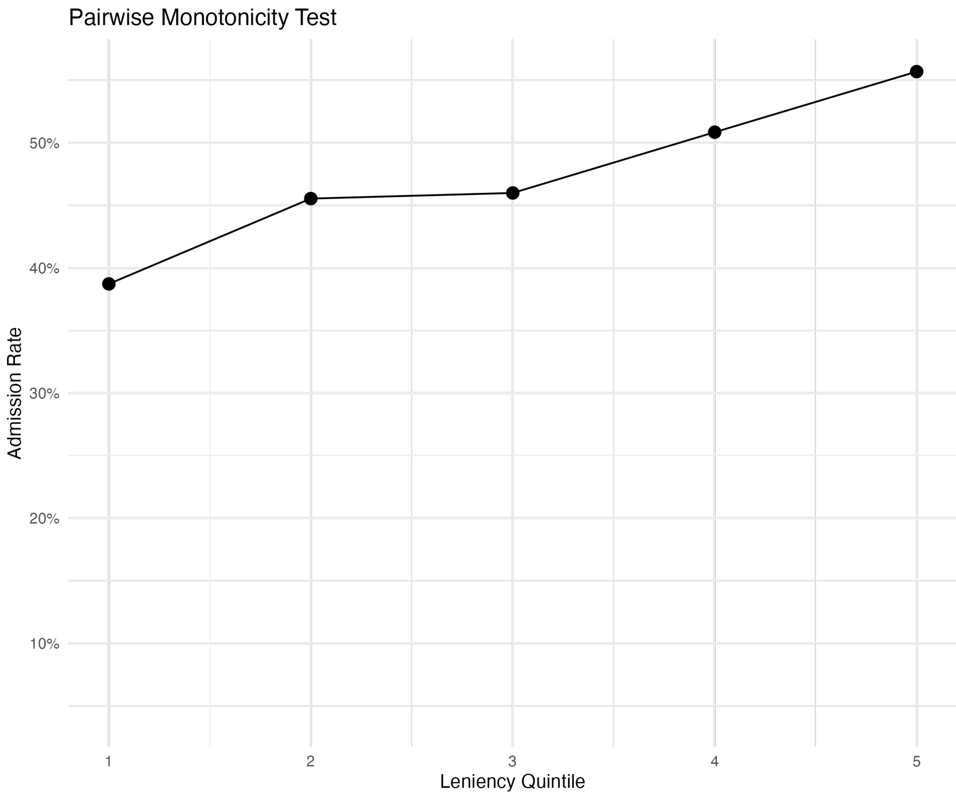


Figure 14: Pairwise monotonicity

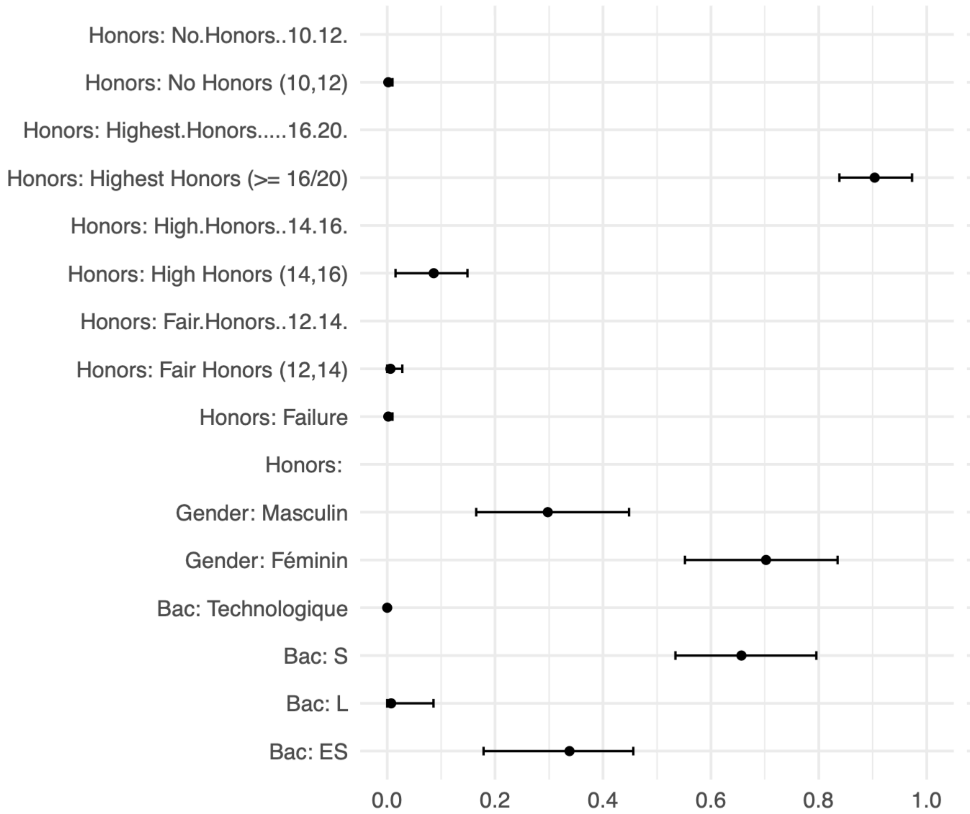


Figure 15: Compliers Carecteristics - gender, academic tracks, baccalaureat honors

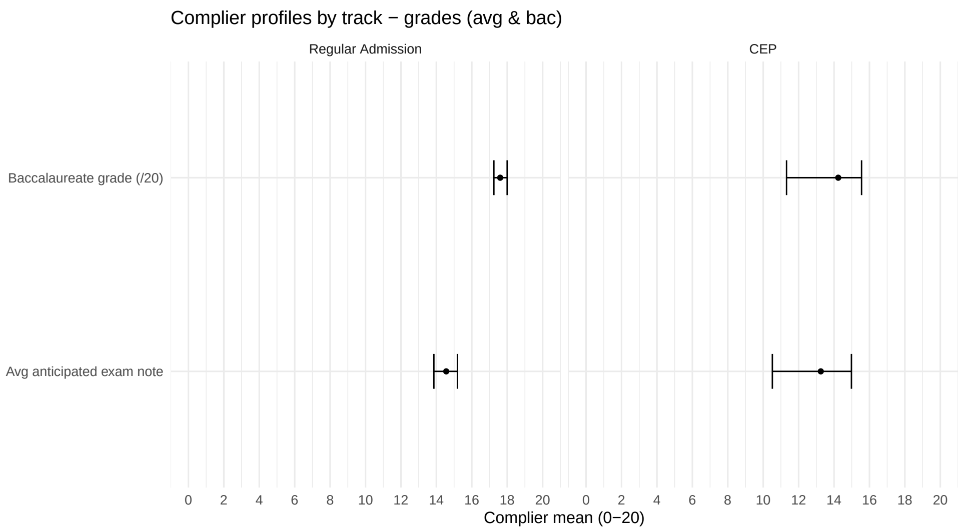


Figure 16: Compliers profile -baccalaureat grades

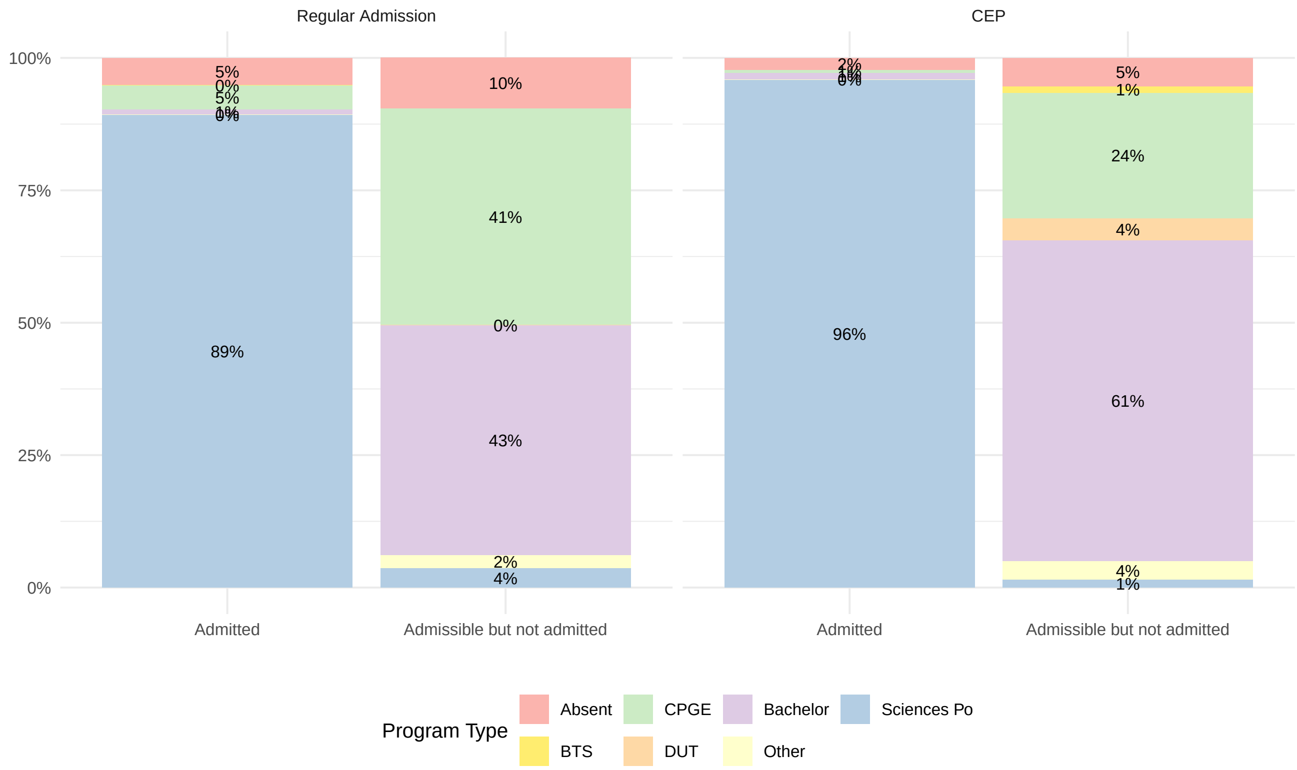
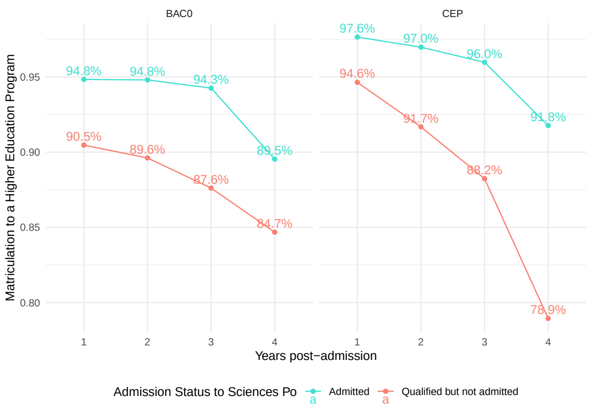
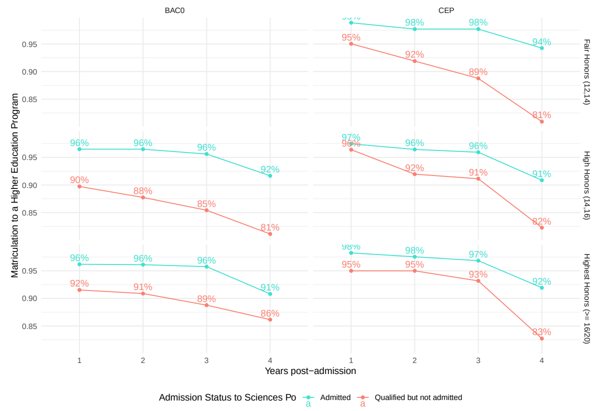


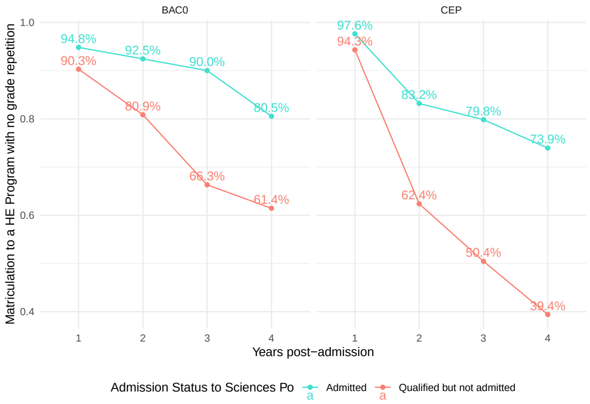
Figure 17: Students' Enrollment Program Type



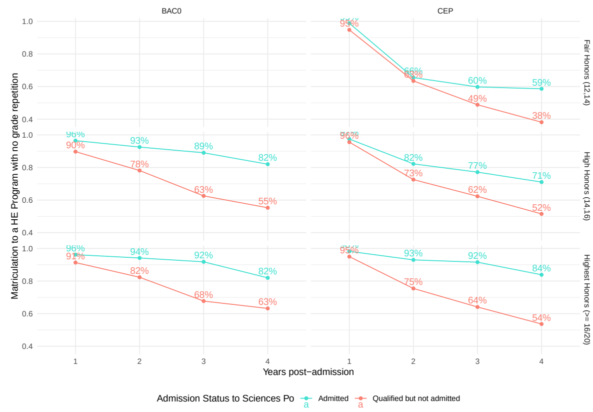
(a)Enrollment in a Higher Education Program



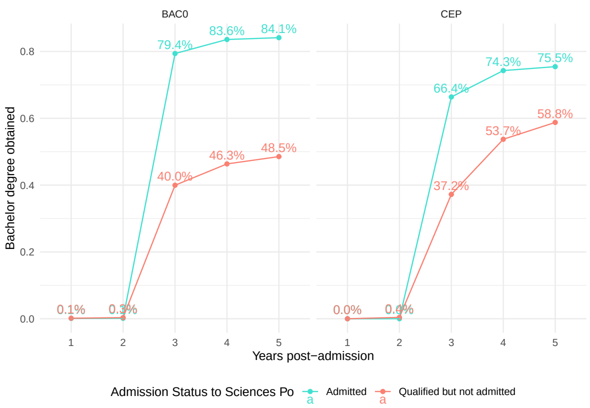
(b) Enrollment in Higher Education by Bac Honors



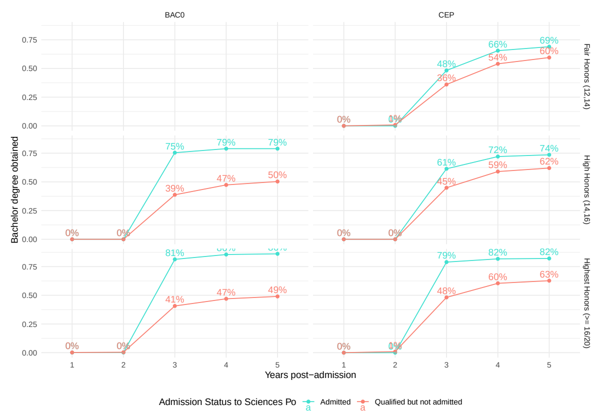
(c) No Grade Repetition



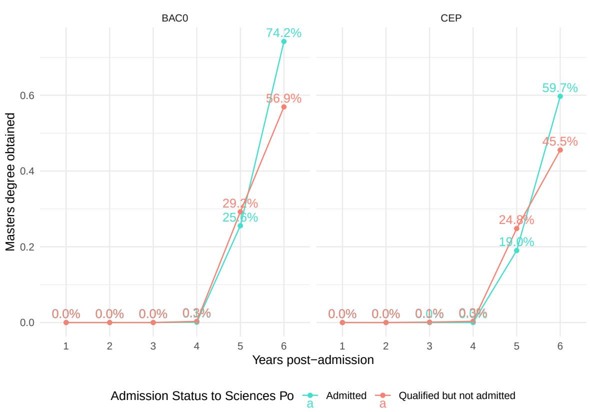
(d) No Grade Repetition by Bac Honors



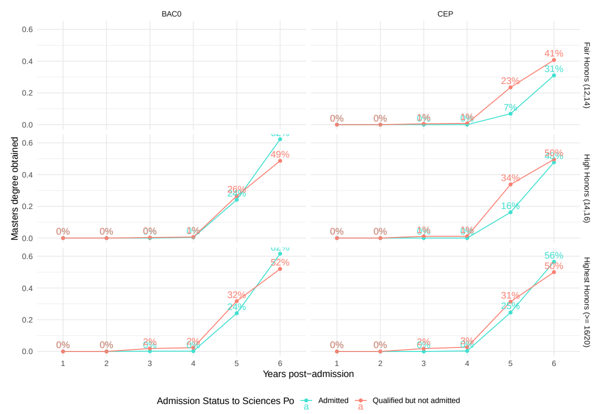
(e) Bachelor Graduation



(f) Bachelor Graduation by Bac Honors



(g) Bachelor Graduation



(h) Bachelor Graduation by Bac Honors

Figure 18: Descriptive Outcomes for Admitted and Non-Admitted Students

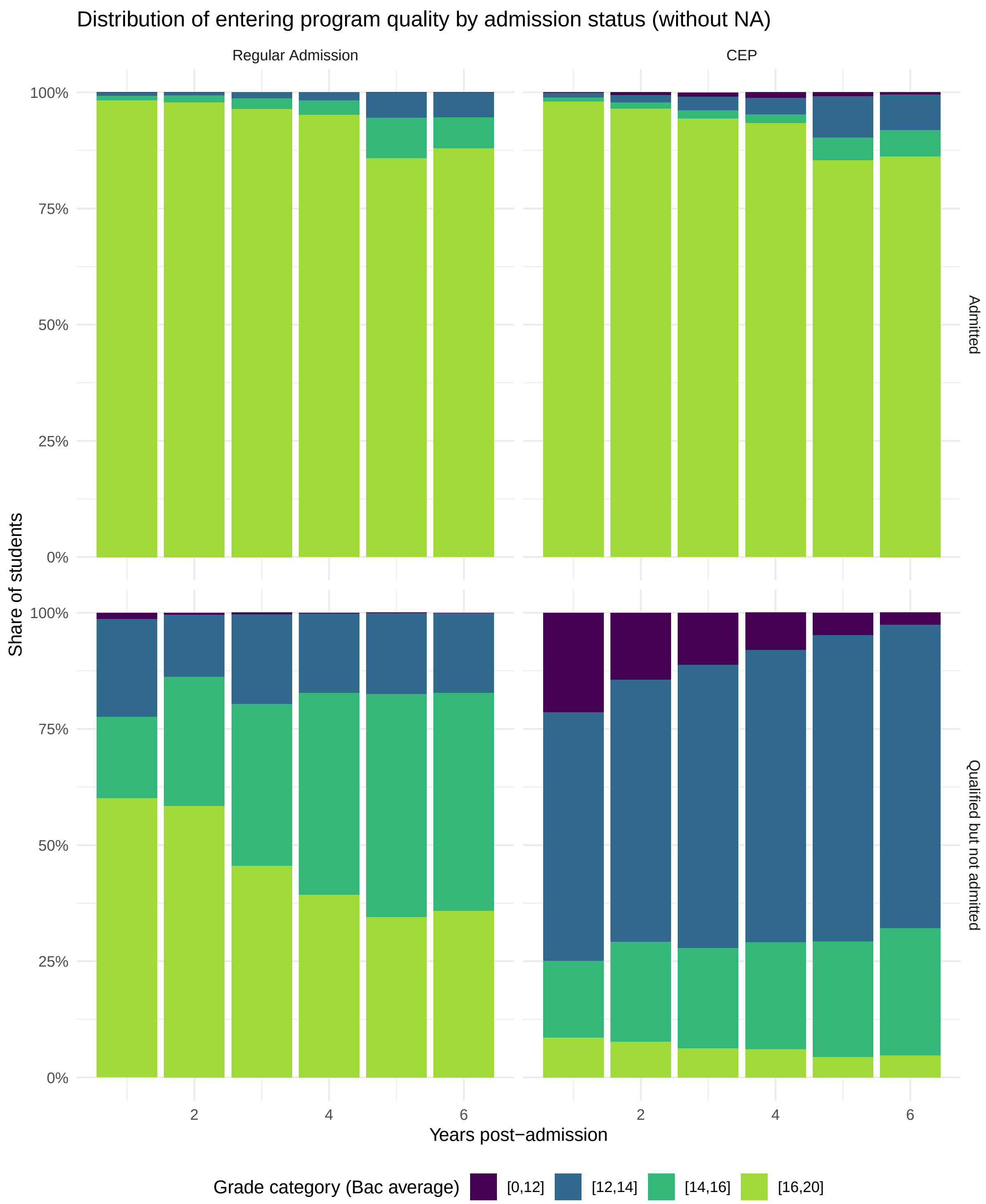


Figure 19: Students' Program Quality over time - including unenrolled students

	Program Quality	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	2.655***	2.644***
	(0.241)	(0.228)
Constant	14.673***	11.521***
	(0.139)	(0.354)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.373	0.403
Adjusted R^2	0.372	0.399
Residual Std. Error	1.790 (df = 8685)	1.751 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 8: College Quality for Regular Admission Students

	Program Quality	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	4.298***	4.189***
	(0.660)	(0.565)
Constant	12.920***	10.552***
	(0.225)	(0.788)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.688	0.748
Adjusted R^2	0.687	0.745
Residual Std. Error	1.370 (df = 3288)	1.237 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 9: College Quality for CEP Students

	Program Quality	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	2.655***	2.641***
	(0.227)	(0.213)
CEP dummy	−1.600***	−0.932**
	(0.290)	(0.376)
Admitted x CEP dummy	1.620*	1.515*
	(0.844)	(0.803)
Constant	14.638***	11.019***
	(0.129)	(0.688)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.511	0.545
Adjusted R^2	0.510	0.543
Residual Std. Error	1.686 (df = 11978)	1.628 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 10: College Quality

	Completed First Year Successfully	
	No Controls	Student + HS
	(1)	(2)
Admitted to Sciences Po	0.106**	0.103**
	(0.042)	(0.041)
Constant	0.829***	0.576***
	(0.024)	(0.049)
Year Fixed Effects	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.036	0.045
Adjusted R^2	0.035	0.044
Residual Std. Error	0.315 (df = 8,685)	0.314 (df = 8,679)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 11: First year success / No grade repetition - Regular Admission students

	Completed First Year Successfully	
	No Controls	Student + HS
	(1)	(2)
Admitted to Sciences Po	−0.062	0.001
	(0.216)	(0.179)
Constant	0.718***	−0.101
	(0.073)	(0.129)
Year Fixed Effects	Yes	Yes
Student & HS controls	No	Yes
Observations	3,297	3,297
R^2	−0.019	0.120
Adjusted R^2	−0.021	0.116
Residual Std. Error	0.447 (df = 3,289)	0.416 (df = 3,283)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 12: First year success / No grade repetition - CEP students

	Completed First Year Successfully	
	No Controls	Student + HS
	(1)	(2)
Admitted to Sciences Po	0.106**	0.098**
	(0.048)	(0.046)
CEP dummy	−0.078	0.054
	(0.061)	(0.075)
Admitted × CEP dummy	−0.175	−0.129
	(0.179)	(0.166)
Constant	0.821	0.171
	(0.027)	(0.105)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,989	11,989
R^2	0.041	0.089
Adjusted R^2	0.040	0.088
Residual Std. Error	0.357 (df = 11,979)	0.348 (df = 11,973)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 13: First year success / No grade repetition

	Bachelor Degree in 3 years	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.445***	0.464***
	(0.060)	(0.058)
Constant	0.413***	0.498***
	(0.035)	(0.089)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.171	0.187
Adjusted R^2	0.171	0.182
Residual Std. Error	0.446 (df = 8685)	0.443 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 14: Bachelor Graduation in 3 years - Reguar Admission students

	Bachelor Degree in 3 years	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	−0.037	−0.039
	(0.243)	(0.219)
Constant	0.525***	−0.685**
	(0.083)	(0.305)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	−0.013	0.089
Adjusted R^2	−0.016	0.079
Residual Std. Error	0.504 (df = 3288)	0.480 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 15: Bachelor Graduation in 3 years - CEP students

	Bachelor Degree after 3 years	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.444***	0.453***
	(0.062)	(0.060)
CEP dummy	0.125	0.229**
	(0.079)	(0.106)
Admitted x CEP dummy	−0.486**	−0.529**
	(0.231)	(0.226)
Constant	0.411***	−0.204
	(0.035)	(0.193)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.128	0.150
Adjusted R^2	0.127	0.146
Residual Std. Error	0.463 (df = 11978)	0.457 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 16: Bachelor Graduation in 3 years

	Master Degree in 6 years	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.071	0.071
	(0.057)	(0.054)
Constant	0.644***	0.554***
	(0.033)	(0.085)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.291	0.302
Adjusted R^2	0.291	0.298
Residual Std. Error	0.421 (df = 8685)	0.419 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 17: Masters Graduation in 6 years - Regular Admission students

	Master Degree in 6 years	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	−0.184	−0.239
	(0.204)	(0.185)
Constant	0.596***	−0.235
	(0.069)	(0.258)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.174	0.245
Adjusted R^2	0.172	0.236
Residual Std. Error	0.423 (df = 3288)	0.406 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 18: Masters Graduation in 6 years - CEP students

	Master Degree after 6 years	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.071	0.064
	(0.057)	(0.055)
CEP dummy	−0.014	0.114
	(0.073)	(0.097)
Admitted x CEP dummy	−0.266	−0.339
	(0.212)	(0.207)
Constant	0.637***	0.010
	(0.032)	(0.177)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.276	0.289
Adjusted R^2	0.275	0.286
Residual Std. Error	0.423 (df = 11978)	0.420 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 19: Masters Graduation in 6 years

	Attending a top 30 Grande Ecole for Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.249***	0.229***
	(0.062)	(0.060)
Constant	0.255***	−0.081
	(0.036)	(0.093)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.093	0.110
Adjusted R^2	0.093	0.105
Residual Std. Error	0.463 (df = 8685)	0.460 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 20: Master in a top-30 GE - Regular Admission students

	Attending a top 30 Grande Ecole for Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.379***	0.393***
	(0.146)	(0.138)
Constant	0.069	−0.129
	(0.050)	(0.192)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.357	0.366
Adjusted R^2	0.355	0.359
Residual Std. Error	0.302 (df = 3288)	0.301 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 21: Master in a top-30 GE - CEP students

	Attending a top 30 Grande Ecole for Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.249***	0.234***
	(0.057)	(0.055)
CEP dummy	−0.169**	−0.107
	(0.073)	(0.098)
Admitted x CEP dummy	0.134	0.137
	(0.213)	(0.209)
Constant	0.251***	−0.003
	(0.033)	(0.179)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.177	0.188
Adjusted R^2	0.176	0.184
Residual Std. Error	0.425 (df = 11978)	0.423 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 22: Master in a top-30 GE

	Attending a D1 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.294***	0.273***
	(0.051)	(0.048)
Constant	0.622***	0.004
	(0.030)	(0.075)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.133	0.182
Adjusted R^2	0.133	0.177
Residual Std. Error	0.381 (df = 8685)	0.372 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 23: Master in the top Decile - Regular Admission students

	Attending a D1 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.705***	0.734***
	(0.158)	(0.144)
Constant	0.164***	−0.482**
	(0.054)	(0.201)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.543	0.583
Adjusted R^2	0.542	0.578
Residual Std. Error	0.328 (df = 3288)	0.315 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 24: Master in the top Decile - CEP students

	Attending a D1 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.294***	0.272***
	(0.049)	(0.047)
CEP dummy	−0.477***	−0.335***
	(0.063)	(0.083)
Admitted x CEP dummy	0.410**	0.433**
	(0.184)	(0.176)
Constant	0.627***	−0.061
	(0.028)	(0.151)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.385	0.422
Adjusted R^2	0.385	0.420
Residual Std. Error	0.368 (df = 11978)	0.357 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 25: Master in the top Decile - CEP students

	Attending a D2 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	-0.383^{***}	-0.365^{***}
	(0.047)	(0.045)
Constant	0.389^{***}	0.292^{***}
	(0.027)	(0.071)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.097	0.116
Adjusted R^2	0.096	0.110
Residual Std. Error	0.353 (df = 8685)	0.350 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 26: Master in the second Decile - Regular Admission students

	Attending a D2 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	-0.058	-0.061
	(0.155)	(0.145)
Constant	0.106^{**}	-0.026
	(0.053)	(0.203)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.029	0.052
Adjusted R^2	0.027	0.042
Residual Std. Error	0.321 (df = 3288)	0.318 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 27: Master in the second Decile - CEP students

	Attending a D2 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	-0.383^{***}	-0.368^{***}
	(0.046)	(0.045)
CEP dummy	-0.227^{***}	-0.187^{**}
	(0.059)	(0.079)
Admitted x CEP dummy	0.322^{*}	0.281^{*}
	(0.172)	(0.169)
Constant	0.376^{***}	0.157
	(0.026)	(0.144)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.083	0.102
Adjusted R^2	0.083	0.098
Residual Std. Error	0.344 (df = 11978)	0.342 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 28: Master in the second Decile - CEP students

	Attending a Q1 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.055	0.048
	(0.043)	(0.041)
Constant	0.846***	0.352***
	(0.025)	(0.063)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.027	0.087
Adjusted R^2	0.026	0.082
Residual Std. Error	0.321 (df = 8685)	0.312 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 29: Master in the top Quartile - Regular Admission students

	Attending a Q1 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.542***	0.558***
	(0.199)	(0.178)
Constant	0.290***	−0.434*
	(0.068)	(0.249)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.320	0.397
Adjusted R^2	0.318	0.391
Residual Std. Error	0.413 (df = 3288)	0.390 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 30: Master in the top Quartile - CEP students

	Attending a Q1 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	0.055	0.048
	(0.047)	(0.044)
CEP dummy	-0.516^{***}	-0.353^{***}
	(0.060)	(0.078)
Admitted x CEP dummy	0.484^{***}	0.458^{***}
	(0.175)	(0.166)
Constant	0.837^{***}	0.012
	(0.027)	(0.142)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.296	0.349
Adjusted R^2	0.296	0.346
Residual Std. Error	0.349 (df = 11978)	0.336 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 31: Master in the top Quartile - CEP students

	Attending a Q2 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	-0.097^{**}	-0.095^{**}
	(0.042)	(0.040)
Constant	0.161^{***}	0.518^{***}
	(0.024)	(0.062)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	8,692	8,692
R^2	0.054	0.073
Adjusted R^2	0.053	0.068
Residual Std. Error	0.310 (df = 8685)	0.307 (df = 8642)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 32: Master in the second Quartile - Regular Admission students

	Attending a Q2 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	-0.482^{**}	-0.529^{***}
	(0.200)	(0.189)
Constant	0.349^{***}	-0.183
	(0.068)	(0.264)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	3,296	3,296
R^2	0.081	0.087
Adjusted R^2	0.079	0.077
Residual Std. Error	0.414 (df = 3288)	0.415 (df = 3259)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 33: Master in the second Quartile - CEP students

	Attending a Q2 Master	
	No Controls	Student + HS
	(1)	(2)
Admitted to Siences Po	-0.097^{**}	-0.102^{**}
	(0.046)	(0.045)
CEP dummy	0.247^{***}	0.258^{***}
	(0.059)	(0.079)
Admitted x CEP dummy	-0.386^{**}	-0.403^{**}
	(0.171)	(0.168)
Constant	0.147^{***}	0.095
	(0.026)	(0.144)
Year Fixed Effect	Yes	Yes
Student & HS controls	No	Yes
Observations	11,988	11,988
R^2	0.089	0.096
Adjusted R^2	0.089	0.092
Residual Std. Error	0.342 (df = 11978)	0.341 (df = 11934)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 34: Master in the second Quartile - CEP students

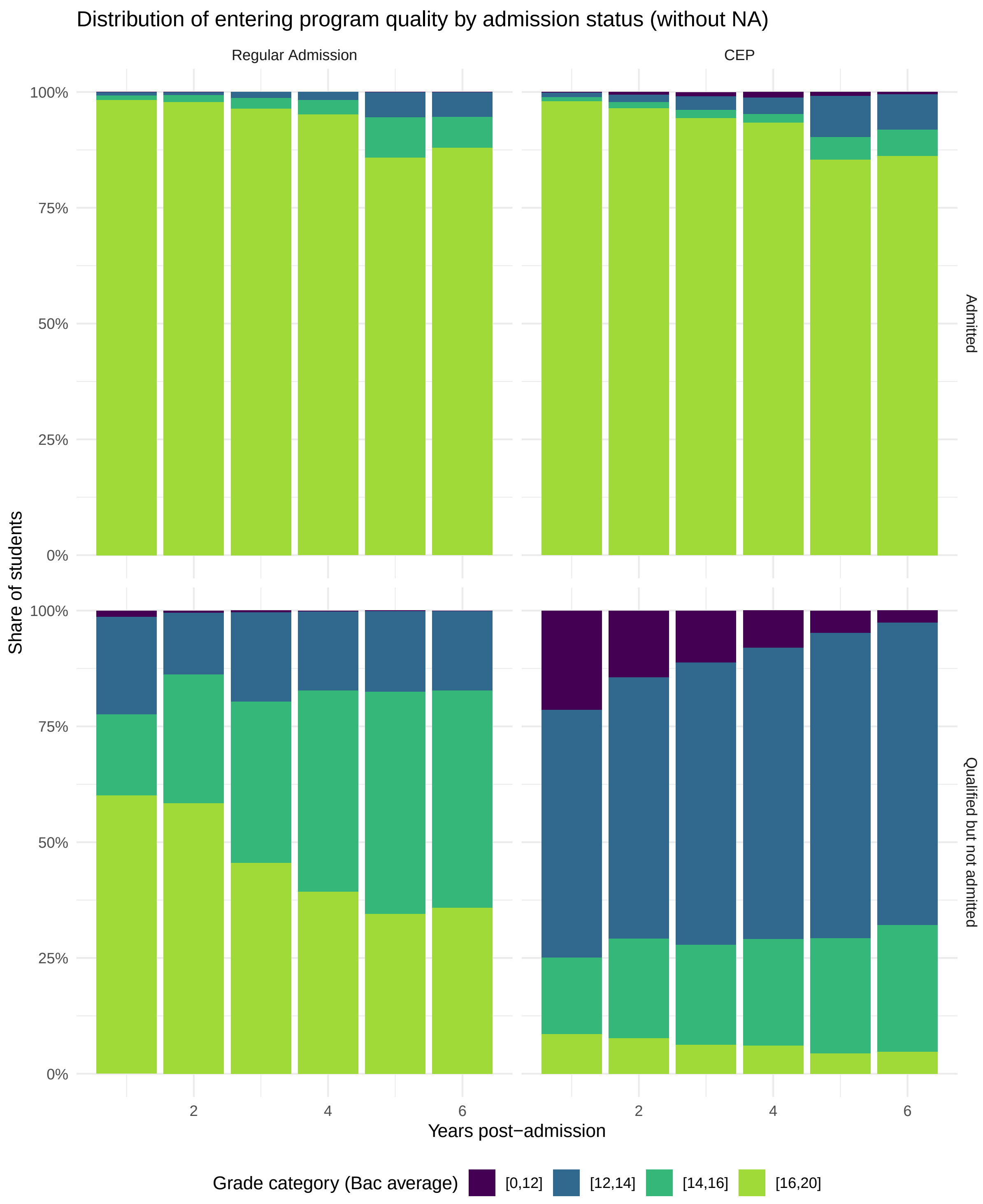


Figure 20: Students' Program Quality over time

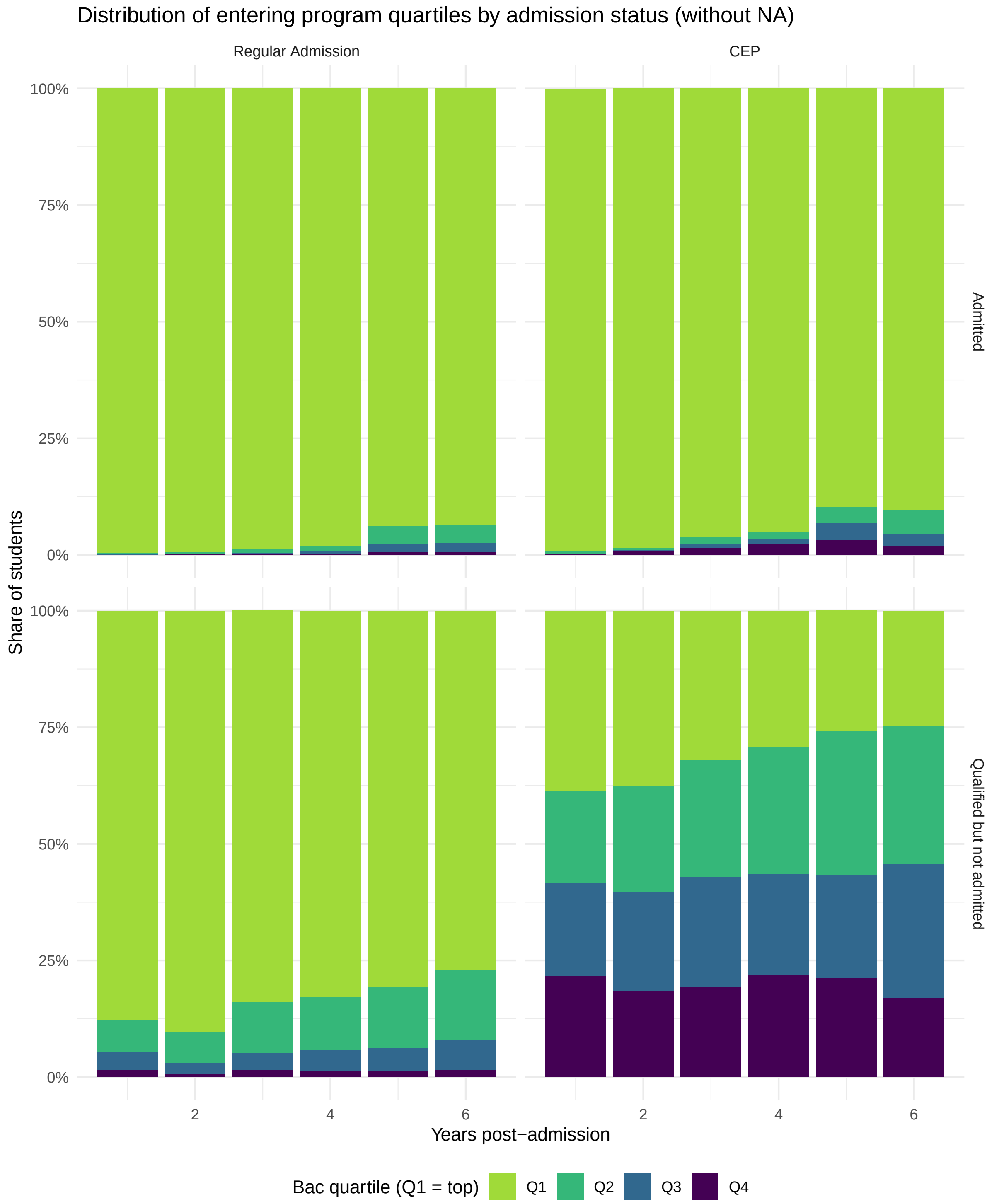


Figure 21: Students' Program Quality over time - quartiles

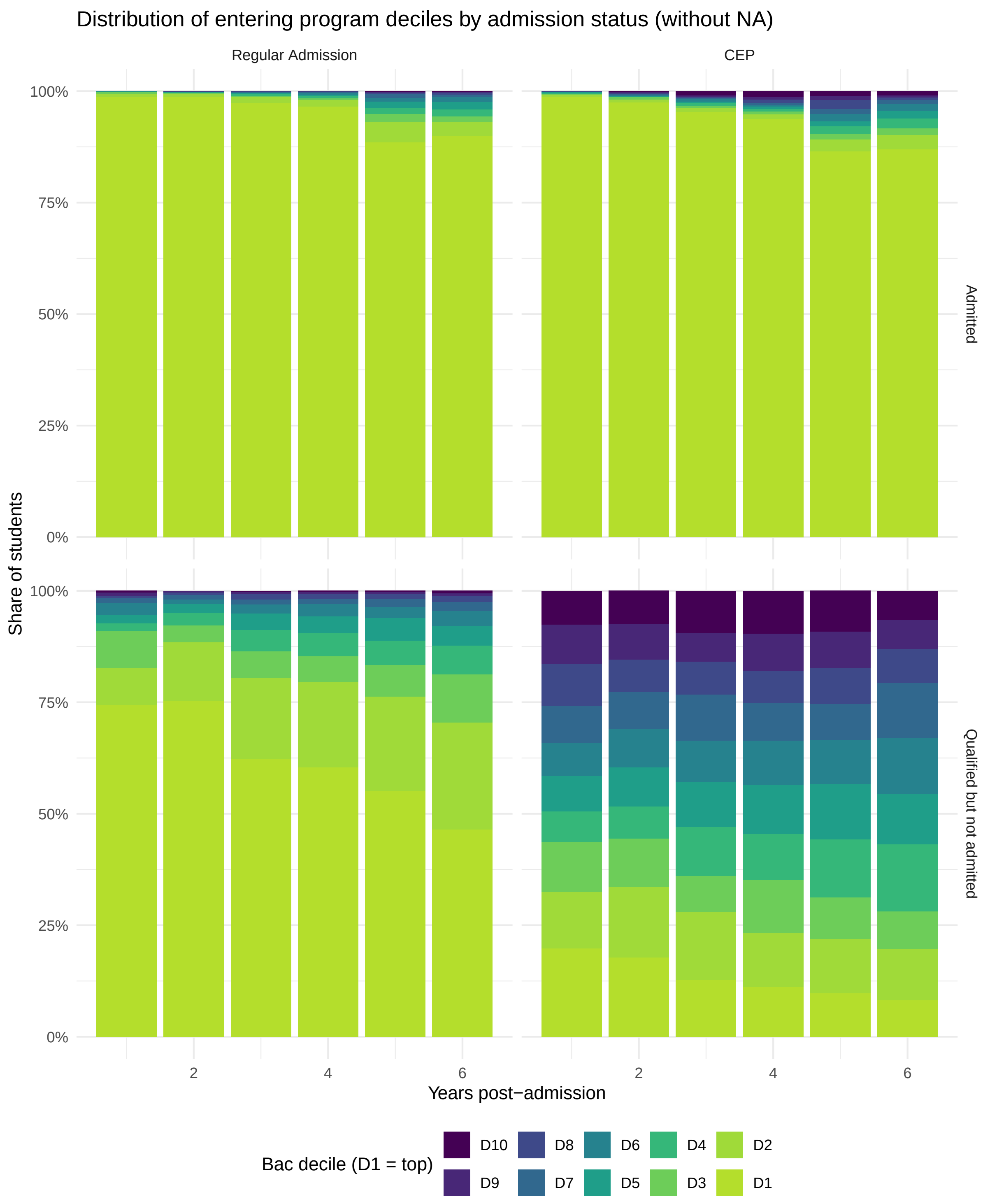


Figure 22: Students' Program Quality over time - deciles

	Dependent variable:		
	Attending a D1 Master		
	(1)	(2)	(3)
Admitted (IV)	0.050	−0.090	0.132
	(0.069)	(0.127)	(0.115)
Admitted x CEP	0.416*		
	(0.227)		
Admitted x Low SES		0.244*	
		(0.141)	
Admitted x Female			−0.087
			(0.137)
Admitted x Non-S Bac	0.116	0.235	0.249
	(0.228)	(0.219)	(0.225)
Observations	12,005	12,005	12,005
R^2	0.179	0.183	0.185
Adjusted R^2	0.175	0.178	0.181
Residual Std. Error	0.450 (df = 11937)	0.449 (df = 11936)	0.448 (df = 11937)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 35: Heterogeneity Analysis - Master in the top decile