Aiming higher: Spillover effects of affirmative action in higher education *

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Abstract

Institution-specific affirmative action programs aim to reduce gaps in educational attainment across social groups. However, there is little evidence on whether institution-specific programs increase overall minority enrollment, or whether students who enroll at one sponsoring institution simply substitute away from other universities of equal quality. I exploit the roll-out of a large affirmative-action program to measure the overall effect of institution-specific affirmative action on educational outcomes for low-income students. Using administrative data on applications and enrollment, I estimate the spillover effects of a program sponsored by an elite French college. For each student that enrolled at the sponsoring institution as a result of the program, two additional students enrolled at another elite college. These spillovers are driven by increases in students' applications to selective colleges. I rule out fixed costs in college applications as an explanation for this phenomenon. Instead, I suggest that students hold incorrect beliefs about their academic ability and chances of admission at selective universities, and that the affirmative action program corrected these beliefs upward.

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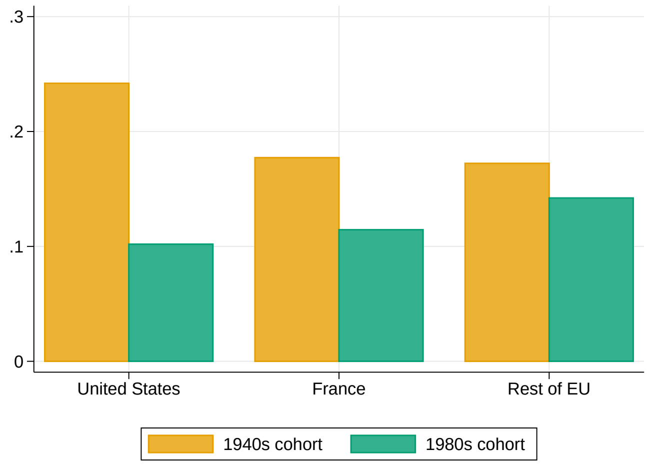
1 Introduction

Gaps in educational attainment across social groups represent a significant barrier to intergenerational mobility. These gaps are persistent and growing; in both Europe and the U.S., the probability of rising from the bottom half of parental educational attainment to the top quartile of educational attainment is lower for cohorts of men born in 1980 than for those born in 1940 (Figure 1). To promote social mobility, many universities employ affirmative action programs that offer preferential access to students from under-represented groups.

Figure 1: Probability that boys born in a family in the bottom half of the education distribution move to the highest quartile



Notes: This figure plots the average probability that boys born in a family in the bottom half of the education distribution make it to the highest quartile, for cohorts born in 1940 and in 1980 for the U.S., France and the rest of the European Union, excluding Malta. On average, that probability has decreased from 17.5% for boys born in the 1940s to 13.9% for boys born in the 1980s.
Source: GDIM. 2018. Global Database on Intergenerational Mobility.

In the United States, preferential admission criteria for minority students is not a federally mandated policy, but an institution-specific practice (Arcidiacono, Aucejo and Hotz, 2016). Critics of affirmative action have expressed concerns that such programs lead under-prepared students to enroll at elite colleges (Sander, 2004, 2005), reinforce stereotypes, or reduce the efforts of under-represened minorities (Bodoh-Creed and Hickman, 2018; Coate and Loury, 1993). While increasing evidence suggests that affirmative action does not worsen minority students' educational outcomes (See Arcidiacono, Aucejo and Hotz (2016) for a review) or decrease effort (Antonovics and Backes, 2014; Caldwell, 2010; Furstenberg, 2010), it remains challenging to identify the overall effect of affirmative action on minority student outcomes. Because most programs are institution-specific, increased enrollment of the target group at the participating institution may be offset by lower enrollment at non-participating institutions, leading to an ambiguous net effect. Alternatively, affirmative action could attract students who would otherwise not have enrolled at a selective college. In this case, affirmative action programs could also affect the beneficiaries' peers by increasing their exposure to elite institutions, potentially leading to a sizeable multiplier effect on the college outcomes of under-represented groups.[1] Therefore, measuring spillover effects of institution-specific affirmative action on minority students' enrollment at non-sponsoring institutions is key to assessing the efficacy of such programs.

However, estimating these spillovers requires enrollment data for both participating and non-participating institutions. Furthermore, affirmative action programs are not randomly assigned and it is often impossible to identify a proper control group if all students in the targeted group are granted preferential access. Due to these data restrictions and methodological concerns, there is a gap in the literature on how institution-specific affirmative action programs affect the overall enrollment outcomes of disadvantaged students.

My paper evaluates the spillover effects of the first affirmative action program to be implemented in France. The program was introduced by an elite French university, Sciences Po, and offers preferential access to students from select disadvantaged high schools without affecting their chances of admission at other colleges. The program's objective was to increase the enrollment of low-income students at the sponsoring institution; however, only the subset of low-income students enrolled in partner high schools could benefit from the program.

Just like the U.S., France has a clearly identifiable set of elite institutions, known as "the Grandes Ecoles". In 2018, 30 of the CEOs of the 40 largest French firms were graduates of the Grand Ecoles (Recto versoi, 2018).[2] In addition, college fees are much lower in France than in most high income countries.[3] One might predict that such low tuition fees would lead to a lower socio-economic gap in college enrollment. However, large socio-economic inequalities in higher education persist throughout France, and elite colleges have done little to increase economic diversity in enrollment.

In fact, the program I study in this paper remains the only one in France that has introduced special admission criteria for under-represented groups. Sciences Po implemented its affirmative active program by signing bilateral agreements with high schools that have a large proportion of students with low-SES students.[4] Admission to Sciences Po typically requires taking a very competitive entrance exam that lasts several days. Alternatively, students in partner high schools can earn admission through an alternative process, in which they prepare a press review of current events with their teachers and defend it at

[1] Indeed, low-income students who lack contact with graduates from elite colleges often exclusively apply to colleges which under-match their academic achievement, sometimes paying higher tuition fees than they would have at a better-matched institution (Hoxby and Avery, 2012). This suggests that, without enough exposure to elite institutions, students do not have accurate information regarding college admissions or hold wrong beliefs over their own academic abilities.

[2] Among the ten which were not, five were foreigners who completed their college education abroad.

[3] The maximum tuition fee charged by a public institution at a Bachelor level is less than 1,000 USD in France, in contrast the average tuition fee is 9,000 USD in the US or 12,000 USD in the UK (OECD, 2019).

[4] I define students' socio-economic status (SES) based on their parents' reported professional occupation category. Following the literature (Guyon and Huillery, 2016), I classify an occupation which typically requires five years of higher education as high-SES and other occupations as low-SES. Children in low-SES households have parents who never worked, are manual laborers, low-skilled white collars, craftmen or storekeepers or who work in intermediate occupations.

Sciences Po. Although French law prohibits targeting specific ethnic groups for college admission, partner high schools are located in municipalities with large proportions of immigrants, and one of of the program's unofficial objectives was to improve the higher education outcomes of under-represented minorities. Sciences Po is a high-profile university in France (all French Presidents since 1981 have been Sciences Po graduates), and this program has been widely publicized as the first attempt to introduce affirmative action in the country.

To study the impact of this program, I utilize four restricted administrative datasets from the French Ministry of Higher Education in Paris. My data covers the universe of high school students in France, including their demographic characteristics, the grades they obtained in a standardized exam, the college applications they completed on a centralized website, and the college they eventually enrolled at. My core sample covers eight years of higher education enrollment (1,443,204 students) and five years of college applications (787,736 students).

I start by documenting large socioeconomic gaps in college enrollment and college applications, reflecting the same patterns that have been identified in other European countries (Boneva and Rauh, 2017; Checchi and Leonardi, 2013) and the US(Dillon and Smith, 2017; Hoxby and Avery, 2012). In my sample, conditional on having the same test scores, students graduating from a low-SES school[5] are 5.2 pp. less likely to enroll at a selective college than students graduating from a high-SES school. This gap persists up to five years after entering college, suggesting that drop-out rates are similar across social groups. Furthermore, I observe that the gap is driven by differences in application rates to selective colleges; students graduating from a low-SES high school are on average 10pp. less likely to have applied to a selective college than students in the bottom quartile, conditional on test scores. Conditional on applying to a selective college, there is no significant difference in college enrollment across social groups. Thus, socioeconomic inequities in college outcomes among academically comparable peers are driven by student application patterns, not discriminatory recruitment practices on the part of institutions. Hoxby and Avery (2012) have documented that the majority of high-achieving low-income students in the US do not apply to selective colleges, even when they would be eligible for generous financial aid. It is remarkable that even in a setting such as France, where tuition is relatively low, these gaps in application behavior remain sizeable.

I then turn to assess the effect of Science Po's affirmative action program on students in low-SES schools. I do this by matching students in treated high schools to students with comparable demographic and academic characteristics in untreated high schools that have similar pre-event higher education outcomes. I then use a difference-in-differences approach to compare higher education outcomes before and after the affirmative action program across treatment and control schools.

My results show that when a high school signs an agreement with Sciences Po, its stu-

[5] I define low-SES schools as school which have a proportion of low-SES students in the top quartile of the distribution and high-SES schools as schools from the bottom quartile.

dents enroll disproportionately more at Sciences Po. However, the program does not simply shift students away from other selective colleges; this institution-specific affirmative action program *increases* overall enrollment at top universities. In fact, for each additional student enrolling at Sciences Po as a result of the affirmative action program, two additional students enroll at another selective college. On average, the affirmative action program leads to a 1.1pp. increase in enrollment at selective colleges other than Sciences Po, equivalent to 9% of the baseline value. I provide evidence that spillover effects are driven by an increase in applications to selective colleges, rather than by an increase in college preparedness; students in treated high schools apply on average to 0.45 additional selective colleges compared to students in untreated high schools, a 25% increase. A common concern in the affirmative action literature is "over-matching" students to colleges above their academic ability (Arcidiacono and Spenner, 2011; Arcidiacono, Aucejo and Hotz, 2014; Barnes, 2007; Loury and Garman, 1995; Sander, 2004, 2005). I find no evidence of over-matching in my setting: five years after graduating from high school, students in treated high schools are still more likely to be enrolled at a selective college than if they had attended an high school with no agreement with Sciences Po.

There could be several economic reasons for these positive spillover effects. Economies of scale may reduce the marginal cost of applications to other colleges once students have applied to Sciences Po. However, Science Po's affirmative action program recruits students through a specialized process (the presentation of a press review) that is starkly different from the requirements of other colleges: it is unlikely that the skills that the students acquire through that process are transferable. The affirmative action program could also have increased the returns to high school graduation (Diagne and Wasmer, n.d.) and increased effort. However, I do not find evidence of improved test scores or college preparedness in treated high schools.

Instead, I argue that the affirmative action program modified low-SES students' beliefs about the likelihood of admission in top universities. Growing evidence suggests that incorrect beliefs about the returns to a college application may be driving the social gap in college attendance (Boneva and Rauh, 2017), and that correcting those beliefs increases application and enrollment rates for under-reprented groups (Dynarski et al., 2018; Hoxby and Turner, 2013; Jensen, 2010). In the French context, I show that the enrollment gap between students from high and low-SES schools stems largely from the low-SES group's lower application rate, and that Science Po's affirmative action program reduces this gap.

To illustrate the process of updating beliefs, I build a simple model of college application where low-SES students under-estimate their academic ability and therefore the probability that a college application will be successful. This leads students who are not in the top 20% of the grade distribution to apply to fewer colleges than optimal and high-achievers to apply to more. When one college introduces affirmative action, applications to all other selective colleges should decrease, unless exposure to the program corrects the students' beliefs about admission probability upward. This could be the case if the program signaled that students from low-SES schools are sought after at selective colleges and that they have the academic ability to succeed there. Such a shift in beliefs has opposite effects for high and non-high-achievers: it further reduces high-achievers' applications to selective colleges but increases the number of applications sent by non-high-achievers. In line with these theoretical predictions, I find that non-high-achievers, i.e. students who are not in the top 20% of the grade distribution, benefit disproportionately more from the treatment.

This paper relates to several literatures. First, it adds to the increasing evidence that social gaps in college enrollment remain large in higher-income countries (Bailey and Dynarski, 2011) and are driven by group differences in college applications (Hoxby and Turner, 2013). In the US, policy recommendations to bridge the income gap in college applications include increasing financial aid[6] and moving towards a centralized application system, making college applications universal.[7] Both of those policies have already been implemented in France: all students use a universal application system, and tuition fees are very low. Yet we still observe a large social gap in *which* colleges students apply to.

Second, this paper speaks to a very active literature assessing the effectiveness of interventions to boost low-income students' enrollment at specific selective colleges.[8] College admissions counselling (Avery, 2018; Castleman and Goodman, 2018), increased financial aid(Andrews, Imberman and Lovenheim, 2016) and nudges encouraging high-achieving low-SES students to apply to selective institutions have all been shown to boost applications and enrollment rates (Dynarski et al., 2018; Hoxby and Turner, 2013). However, only a few papers measure the spillover effects of these interventions. Dynarski et al. (2018) do not find evidence that an informational packet which encouraged high-achieving low-SES students to apply to a selective institution and promised them free tuition had any spillover effects on non-treated peers, despite observing large treatment effects on the targeted students. The reason why spillover effects are larger in my setting, while the direct treatment effects are more modest, may be that the intervention evaluated in Dynarski et al. (2018)'s study is not as easily observed by peers as the affirmative action program developed by Sciences Po, which is advertised school-wide. Furthermore, the large majority of policy evaluations in this context focus on high-achievers, as measured by high school test scores. High-achievers may not be the most relevant subgroup to look at, as they represent a minority of all low-income students and the relative social gap in college enrollment is narrower among students with higher test scores. I also contribute to this literature by observing lasting effects of the affirmative action program, beyond application and enrollment.[9] I observe that five years after high school,[10] students from treated high schools are still more likely to be enrolled at a selective college.

[6]Several studies have found that lowering the costs, actual or perceived, faced by students significantly increased college enrollment (Abraham and Clark, 2006; Deming and Dynarski, 2009; Dynarski, 2003; Dynarski and Scott-Clayton, 2013; Kane, 2007; Page and Scott-Clayton, 2016).

[7]Such a system has recently been developed in California (Smith, 2019).

[8]See Page and Scott-Clayton (2016) for an in-depth review of policies aiming at bridging the social gap in college access within the United States.

[9]As noted by Page and Scott-Clayton (2016), there is much more evidence about the impact of such policies on college applications and enrollment than college completion.

[10]Degrees from Grandes Ecoles are delivered after five years of higher education.

In spite of the vast amount of empirical and theoretical work surrounding affirmative action (e.g. see Arcidiacono, Aucejo and Hotz (2016) and Page and Scott-Clayton (2016) for a review), empirical evidence of its spillover effects remains scarce and is mostly drawn from instances where affirmative action was halted rather than introduced. Those cases primarily displaced students away from institutions that used to have affirmative action policies, rather than affecting overall college enrollment rates and completion (e.g. Arcidiacono, Aucejo and Hotz (2014); Hinrichs (2012)).[11] These results suggest that affirmative action simply incentivizes students to substitute from one college to another. However, in instances where affirmative action has been banned, disadvantaged students may have had exposure to peers from previous cohorts who benefited from affirmative action and enrolled at selective colleges. This peer effect may offset the effect of an affirmative action ban and encourage disadvantaged students to continue applying to selective institutions. The effects of new introducing affirmative action programs and banning existing ones may therefore not be symmetrical. I provide novel evidence that the introduction of affirmative action at one selective institution can have positive spillover effects on enrollment at selective colleges with no affirmative action program in place.

Finally, this study contributes to the literature that documents that beliefs about pecuniary (Attanasio and Kaufmann, 2014; Dominitz and Manski, 1996; Jensen, 2010; Page and Scott-Clayton, 2016) and non-pecuniary (Attanasio and Kaufmann, 2017; Belfield and Shaw, 2017; Boneva and Rauh, 2017) returns or costs of education contribute to human capital investments. In particular, Boneva and Rauh (2017) show that students whose parents did not attend university systematically differ in their beliefs about the returns to university enrollment, and that these differences in beliefs explain substantial portion of the social gap in university applications and attendance in the UK. I build on these insights to show how affirmative action programs could correct low-SES students' beliefs about admission probability upward and have positive spillover effects.

The rest of this paper is structured as follows. I start by describing the structure of the French higher education system and the affirmative action program in section 2. Section 3 provides a brief review of the relevant literature on social differences in college outcomes and the policies that have been successful in increasing low-income students' enrollment. Drawing on that literature, I then present a simple model of college application and affirmative action in the presence of distorted beliefs in section 4. Section 5 introduces the administrative data I use and describes detailed evidence of the social gap in college application and enrollment. I present my identification strategy in section 6 and my results in section 7. Section 8 concludes.

[11] Similarly, Daugherty, Martorel and McFarlin (2014) finds that the introduction of the Texas Ten Percent Plan increased the enrollment of eligible students at the flagship institution at the expense of private college enrollment, leaving the overall quality of the college attended unaffected.

2 Institutional setting: Higher education in France

Grandes Ecoles and Universities: I start my analysis by describing the higher education opportunities offered to students graduating from an academic high school in France. This is already a selected set of students: they represent about 38% of an age cohort. Almost 100% of those graduating from an academic high school pursue higher education (Ministère de l'Education Nationale et de la Jeunesse, 2019). They have access to different options: they can apply to enroll at a university, a short technical program or selective colleges.

Figure 2 displays the structure of higher education in France, abstracting from technical programs. The most prestigious college degrees are granted by "Grandes Ecoles", such as business, engineering or political sciences schools. Three quarters of the CEOs of the largest French companies listed in the stock market have graduated from Grandes Ecoles. Those colleges select students through competitive entrance exams that takes place two years after the end of high school. During the two years prior, the majority of students ready to try their luck at a Grande Ecole attend a preparatory school. Those are intensive courses that are publicly funded and prepare students to the Grandes Ecoles' entrance exams. If a student is admitted at a Grande Ecole, she studies there for three academic years before graduating. Preparatory schools are allowed to grant university credits and students who fail the Grandes Ecoles entrance exams can usually transfer to the university system. Some Grandes Ecoles hold entrance exams during the last year of high school. Students recruited through that system receive a degree after five academic years. Note that preparatory schools are also selective and admit students based on their high school grades and their teachers' assessment.

In addition, a majority of students enroll at "Universities". Universities are not selective: until 2018, all high school graduates were guaranteed admission. Universities grant a bachelor's degree after three academic years and students have the possibility to study for two extra years to obtain a Master degree. In the remainder of this paper, I will be refering to Grandes Ecoles or preparatory schools as "selective colleges".

Table 1 summarizes some of the key differences between Universities and preparatory schools. Students enrolled at a preparatory school immediately after high school are slightly more likely to be high-achievers and much more likely to be from high-SES families, but receive almost 50% more public resources. Finally, those enrolling at a preparatory school immediately after high school are also more likely to have obtained at least a Master degree and less likely to have dropped out from higher education six years later.

The application process: Applications to the majority of higher education programs in France take place through a centralized website, "Admission Post-Bac" (APB), which matches students' applications and the offers they receive according to a deferred acceptance algorithm (Gale and Shapley, 1962). The boxes above the timeline in figure 3 show the application steps for the academic year 2015-2016. The application process has remained stable throughout the years in my sample.

Students enrolled in public high schools are automatically registered on that website. By March, the candidates have to select all the programs they want to apply to. Candidates can file at most 24 applications, including 12 of the same "type" (i.e. preparatory schools). Some programs have signed cooperation agreements that allow students to apply to several of them through a single APB application, so the number of actual applications that students file can actually exceed 24.

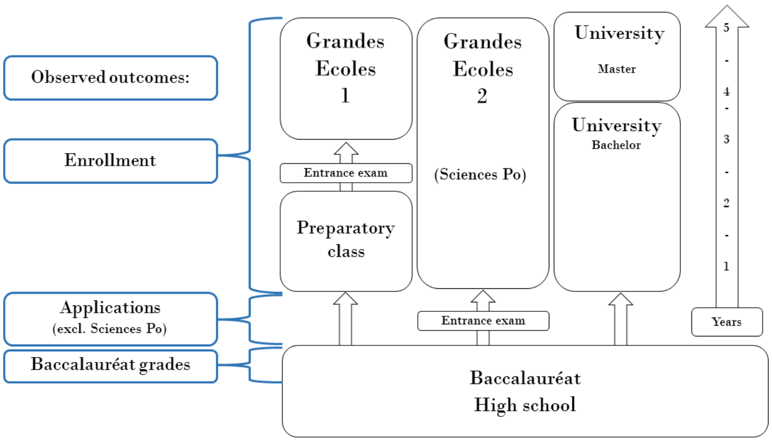
High school teachers enter the students' high school grades online and comment on their academic potential. Candidates who applied to Grandes Ecoles take exams in April and May. The pecuniary and non-pecuniary costs of an application to most programs other than Grandes Ecoles is therefore negligible. In contrast, applications to Grandes Ecoles come at a cost: exam fees alone range between 60 and 120 Euros (Darmon, 2017) even though students eligible to means-tested scholarships are exempted from them.

Students have until the end of May to rank their applications according to their preferences and start receiving offers at the beginning of June. They receive offers over three rounds. In each round, they receive an offer from the program that they ranked the highest among all those that accepted them. Each time, the student can decide to accept or reject the offer, leave the application process or wait for a better offer.

The Baccalauréat, that marks the end of high school, is taken at the end of June. Students have to obtain a grade of ten out of twenty to be allowed to graduate high school. The baccalauréat is a uniform exam, which provides a comparable measure of all the students' academic ability. However, most of the higher education decisions are made before the baccalauréat results are known.

Sciences Po is one of the only French colleges that has not joined the centralized application system, so that it can hold its admission process according to its own timetable. I therefore cannot observe which students have applied to Sciences Po.

Figure 2: Higher Education in France



Notes: This flow chart represents the higher education options available to students that graduate from an academic high school in France. It excludes short, vocational trainings that are attended by about 18.5% of the students. Source: Ministère de l'Education Nationale et de la Jeunesse (2019). The blue boxes indicate the outcomes I observe in this paper.

Table 1: Higher Education in France

	University	Preparatory school (CPGE)
2014 High School graduates enrolling	62.2%	12.4%
2014 High-achieving high-school graduates enrolling	37%	40%
High-SES among students, 2014	30%	49.5%
Obtained at least a Master degree 6 years later (2008 Cohort)	41.35%	66%
No degree, no further studies (2008 cohort)	13.825%	8%
Public spending, per student and per year (2015 Euros)	10,580	15,050

Notes: Characteristics of students who enroll at a University or a preparatory school in the year immediately following their high school graduation. High-achievers have obtained a grade of at least 14 out of 20 at the baccalauréat (top 23% of those graduating). *Source*: DEPP-RESR 2017.

Sciences Po is one of the most prestigious Grandes Ecoles: all of the French Presidents since 1981 have been Sciences Po graduates. Students enroll at Sciences Po immediately after high school and are selected on the basis of their high school grades, three written exams and one oral exam. In 2016, 10,145 candidates took the entrance exam and 1,872 of them were admitted, for an 18.45% pass rate (SciencesPo, 2016). This process, however, discourages applications from low-SES students and the student intake lacks social diversity. In 2010, 78% of candidates at the regular entrance exam were High-SES, as were 70% of those admitted through that process (Tiberj, 2011)[12].

In 2001, Sciences Po started a large affirmative action program, known as the Conventions d'Education Prioritaire (CEP). It was developed on the basis of bilateral agreements signed with public high schools with a large proportion of low-SES students. Those agreements provide for the admission of students through a parallel procedure. All students enrolled in partner high schools are eligible to apply to the CEP program, independently of their own socio-economic background. The boxes located below the timeline in figure 3 illustrate the application process. First, interested students apply to the program and are initially selected by their high-school teachers. Second, for several months, together with their teachers, the students prepare a press review on current events. They then present this press review and are interviewed at Sciences Po (Savary, 2004). This procedure remains highly competitive: only 17% of those selected by their teachers in the first stage gain admission. Importantly, by the time students have to complete their applications to other colleges, they do not know if they are admitted at Sciences Po.

The program is generally considered to be a success. A previous study found that low-SES students recruited through it take no more time to find a job than other graduates and have higher median salary (Tiberj, 2011).

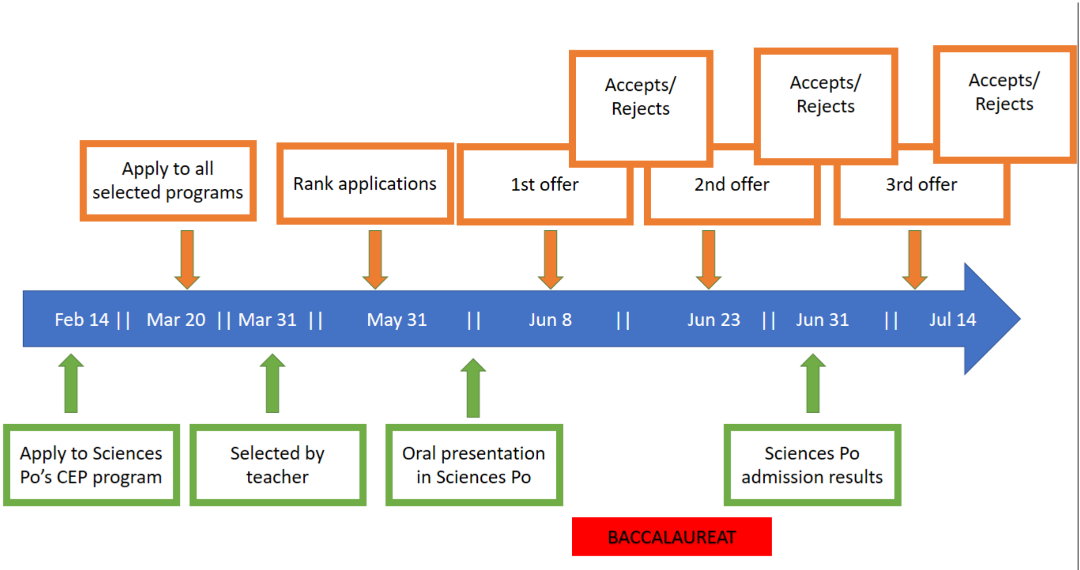
It also had a positive impact on social diversity in Sciences Po. Figures 4a shows that between 2009 and 2016, only slightly more than 20% of students who enrolled at Sciences Po from high schools without the affirmative action program were low-SES. However, low-SES students represented between 60 and 80% of students recruited through the affirmative action program. Figure 4b further shows that Sciences Po enrolls more students who classify as "very low-SES", i.e. children of unemployed, unskilled blue-collar or farm workers, through partner high schools: in 2014, those students represents 22% of students who enrolled at Sciences Po through partner high schools and 6.3% of other incoming students.

While almost 100% of low and high-SES students enrolling at Sciences Po through non-partner high schools are high-achievers (as defined by their baccalauréat grades), figure 4c shows that incoming students from partner high schools are far less likely to be high-achievers. Interestingly, however, the share of high-achievers among students recruited through the affirmative action program is higher than in the general population[13], proving that the affirmative action process remains selective.

[12]Sciences Po also admits a small fraction of students through other procedures: for instance, students who have obtained exceptionnally high grades at the baccalauréat can be exempted from taking the exam

[13]Only 23% of students graduating from public high schools are considered high-achievers.

Figure 3: Calendar of applications

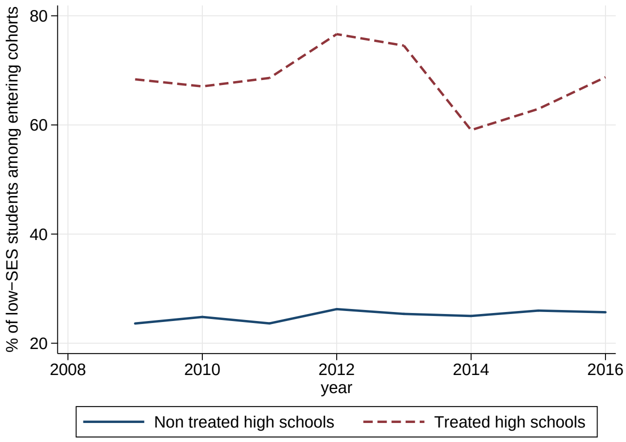


Notes: This shows the timing of applications to higher education for the academic year 2015-2016, but the timeline is similar for all the years in my sample. The boxes located above the timeline represent the calendar of applications through APB, the centralized college application website. The boxes located below the timeline represent the process of Sciences Po's affirmative action program.

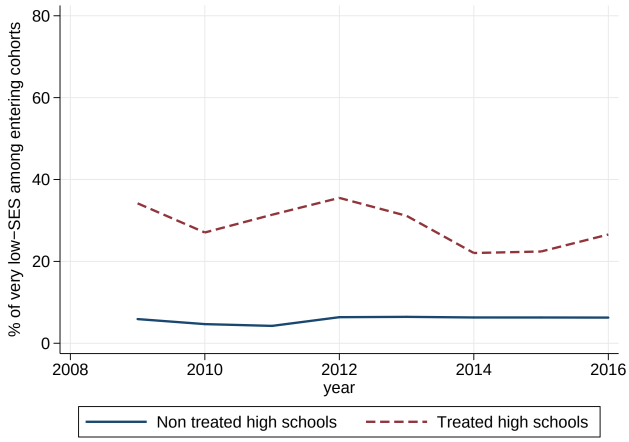
Finally, one cannot talk about affirmative action without mentioning its costs in terms of the students displaced away from Sciences Po to make space for the program beneficiaries. While I cannot say much about counter-factuals, figure 4d shows that the absolute number of students enrolling at Sciences Po through non-partner high schools has remained stable- around 500 per year - between 2009 and 2016 as the number of students entering through the affirmative action program doubled from 48 in 2009 to 105 in 2016. In addition, figures 4a and 4b show that the share of low-SES and very-low-SES students recruited through the regular procedure has also remained stable over that period. Those patterns suggest that the affirmative action program was not introduced at the expense of low-SES students in non-partner high schools.

Figure 4: Recruitment at Sciences Po: demographic profile of first-year incoming cohorts

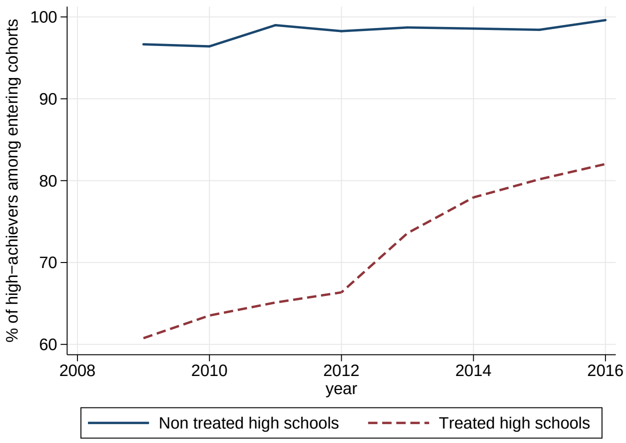
(a) Percentage of low-SES students



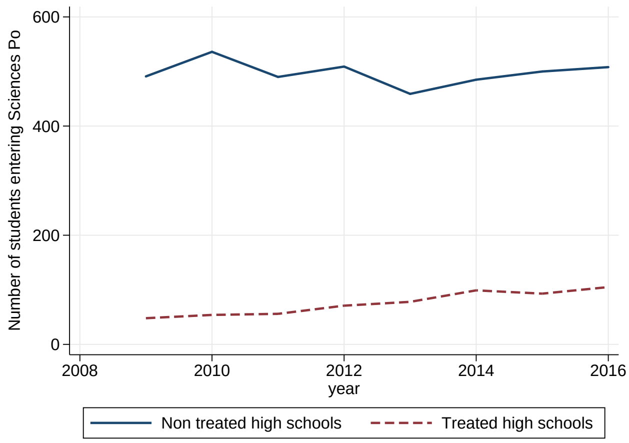
(b) Percentage of very low-SES students



(c) Percentage high-achievers



(d) Number of students enrolling



Notes: Those four figures present the profile of students entering Sciences Po immediately after high school, when graduating from high schools which are not part of the affirmative action program (solid line) and from high schools which are part of the affirmative action program (dashed line). Low-SES students are considered low-SES if none of their parents has a profession requiring at least five years of higher education studies. Both parents of very low-SES students are either unemployed, unskilled blue-collar or farm workers or pensioners who have retired from unskilled blue-collar or farmer jobs. Figure 4c represents the percentage of high-achievers among incoming cohorts. High-achievers are students who obtain a score of at least 14/20 in the standardized test taken at the end of high school. They represent about 23% of students graduating from a public high school in metropolitan France. Figure 4d shows the absolute number of students entering Sciences Po.

Sources: MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité.

Which high schools enter the affirmative action program? To be eligible to the program, high schools must fulfill one of three criteria: being labelled "Education Prioritaire" by the Ministry of Education (which identifies high schools located in vulnerable areas), having at least 43% of very low-SES students or having at least 60% of students who attended a "Education Prioritaire" middle school (Savary, 2004). After 2013, high schools located in isolated rural areas also became eligible.

High school principals apply to be part of the program and, once a year, Sciences Po's board of directors reviews the applications and approves the signature of agreements with high schools that meet the eligibility criteria. The treatment effectively starts in the following academic year. Seven high schools signed a cooperation agreement in 2001 and by 2016, 106 high schools were part of the program.

Table 2 shows the distribution of high schools according to the year they joined the affirmative action program. As I will describe in section 5, I have access to data on higher education enrollment between 2009 and 2016 and on college applications in 2012 and 2014-2017. In the enrollment sample, 1,294 high schools are never treated, 49 high schools are always treated and 31 get treated in different years. This last group represents 1.9% of all students in my higher education sample.

Table 3 displays the characteristics of high schools in 2009, hence at the beginning of my sample, according to the effective date at which high schools joined the treatment. First of all, it shows that there are large differences between treated and never treated high schools: in general treated high schools have a larger proportion of low-SES and foreign students, are located in municipalities with higher unemployment rates and a higher proportion of immigrants. Even though treated high schools seem to have worse academic achievement than never treated high schools, those differences are not statistically significant, including among low-SES students.

In terms of higher education outcomes, high schools which were already part of the affirmative action program in 2009 placed 1% of their students in Sciences Po, while "Never Treated" high schools placed 0.23% of their students there. Schools which were not yet treated did not place a single student to Sciences Po that year. 14% of students from "Never treated schools" enrolled at another selective college. For the schools which were already treated in 2009, the fraction of students enrolled at selective colleges is much lower at 8.8%. Table 3 also shows that there are large differences between treated high schools depending on the timing of the treatment. For instance, high schools that joined the program in 2014 have fewer low-SES students and are located closer to Paris than high schools which joined at other time periods. Similarly, schools which joined in 2013 and 2015 seem to have historically done better at placing students at selective colleges.

Table 2: Treatment timing

Treatment start year	Number of high schools	Proportion of all students	
		Enrollment data	Application data
Never treated	1,294	94.74%	93.3%
Treated before 2009	49	3.3%	4.3 %
2010	9	0.66%	0.83%
2011	9	0.48%	0.63%
2012	3	0.18%	0.21%
2013	5	0.40%	0.36%
2014	3	0.095%	0.13%
2015	1	0.064%	0.08%
2016	1	0.064%	0.10%

Notes: This shows the share of students enrolled in treated and never treated high school, disaggregated by the year in which their high school effectively joined the affirmative action program. The data on college enrollment spans the years 2009-2016 and the data on college applications the years 2012 and 2014-2017. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité.

Table 3: High school characteristics in 2009 and treatment timing

Variable	(1) Never Treated	(2) Treated before 2009	(3) 2010	(4) 2011	(5) 2012	(6) 2013	(7) 2014	(8) 2015	(9) 2016
Distance from Paris	315.869	84.721	99.110	89.112	121.419	485.858	54.615	690.900	618.889
(km)	(212.628)	(108.133)	(83.13)	(67.587)	(130.635)	(155.997)	(63.060)	(0.000)	(0.000)
Proportion of Immigrants	11.587	26.158	17.5	16.103	18.075	12.128	8.915	18.750	31.682
	(7.017)	(12.585)	(11.88)	(9.502)	(8.437)	(7.237)	(3.628)	(0.000)	(0.000)
Unemployment Rate	13.530	17.230	18.25	15.111	11.827	21.770	10.032	21.258	29.543
	(3.963)	(4.160)	(4.50)	(5.429)	(0.930)	(6.507)	(1.675)	(0.000)	(0.000)
Foreign Students (%)	2.035	5.220	3.04	4.631	3.686	2.553	0.688	0.806	5.600
	(2.638)	(3.964)	(1.71)	(3.991)	(2.757)	(2.846)	(0.377)	(0.000)	(0.000)
Low-SES Students (%)	61.035	77.497	76.88	74.392	81.713	72.469	45.914	70.968	92.000
	(14.897)	(9.222)	(8.39)	(7.950)	(5.643)	(6.977)	(14.560)	(0.000)	(0.000)
Female Students (%)	57.018	60.047	59.906	56.772	56.025	57.381	53.103	53.226	56.000
	(8.444)	(7.538)	(7.051)	(7.120)	(2.329)	(4.427)	(2.578)	(0.000)	(0.000)
Enrolled at Sciences Po (%)	0.232	1.012	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(3.300)	(8.384)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Enrolled at a selective college (%)	14.331	8.840	8.64	9.162	8.768	12.863	11.111	18.182	6.557
	(25.006)	(21.659)	(3.52)	(24.030)	(22.003)	(25.858)	(19.335)	(28.180)	(22.877)
High Schools	1,294	49	9	9	3	5	3	1	1
Students (in 2009)	201,255	7,835	1,018	1,157	422	443	288	121	122

Notes: This shows the characteristics of treated and never treated high schools in 2009, disaggregated by the year in which they effectively joined the affirmative action program. "Unemployment Rate" or "Proportion of Immigrants" refers to the value of those variables in the municipality in which the high school is located. The data on college enrollment spans the years 2009-2016 and the data on college applications the years 2012 and 2014-2017. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité.

3 Policies to bridge socio-economic gaps in higher education

Low-SES students are more likely to undermatch than their high-SES peers- that is, to attend a college with average test scores much below their own. As documented by Chetty et al. (2017), under-matching persists in spite of children from different backgrounds having similar earning outcomes conditional on the college attended. Concerningly, undermatching is negatively associated with bachelor completion rates (Howell and Pender, 2016), college satisfaction (Muskens and Borghans, 2019) and labor market outcomes (Ovink, 2018). Using two nationally-representative datasets from the U.S., Smith and Howell (2013) find that 41% of students undermatch and that undermatching is more likely among low-SES students. Roderick and Nagaoka (2011) and Bowen and McPherson (2009) find similar patterns among Chicago Public Schools and North Carolina students respectively. Even though empirical evidence from Europe is scarcer, recent research suggest that undermatching is a problem of similar magnitude there. Using a large-scale dataset from the Netherlands, Muskens and Borghans (2019) document that 13% of students are undermatched. In England, Campbell and Wyness (2019) develop a continuous measure of matching and find that low-SES students' match-index is 0.3-0.4 s.d. lower than that of comparable high-SES students.

There is growing evidence that low-income students' application behavior is one of the driving forces behind undermatching. Using a representative national dataset in the U.S., Dillon and Smith (2017) finds that the vast majority of undermatching students had not applied to any college that matched their academic achievement and that only 6% of them had applied to a matched institution and been rejected.

Social differences in college-student mismatches could be driven by the fact that more selective colleges are unaffordable for low-income students. Providing financial aid helps increasing low-income students' college entry and persistence.[14] It can also increase the quality of the institution attended. Londoño-Vélez and Rodríguez (Forthcoming) study the impact of a large-scale program offering financial aid to low-income high-achievers attending high-quality colleges in Colombia. This program bridged the social gap in college enrollment for high-achieving students. Interestingly, college supply increased in response to an heightened demand which also benefited the enrollment of non-eligible students.

However, generous financial aid is not always sufficient to bridge the social gap in college applications. Hoxby and Avery (2012) describe that high-achieving, low-income students in the US do not apply selective universities in spite of being eligible to very generous financial aid and enroll at less selective institutions at a higher cost.

If differences in returns to college or the cost of college do not fully account for the social gap in college application behavior, an alternative explanation could be that low-SES students have distorted beliefs over returns of college. Beliefs over the pecuniary and non-pecuniary returns and costs of schooling influence human capital investments (Attanasio

[14] See Deming and Dynarski (2009) and Dynarski and Scott-Clayton (2013) for a review of the evidence of the effectiveness of financial aid programs.

and Kaufmann, 2014, 2017; Belfield and Shaw, 2017; Dominitz and Manski, 1996; Page and Scott-Clayton, 2016). Boneva and Rauh (2017) document that those beliefs systematically differ between low and high-SES students and could explain a large part of the social gap in university enrollment. Hurwitz and Smith (2018) show that information on the earnings of college graduates increased the number of applications received by colleges with higher graduate earnings. However, the impact is almost exclusively driven by high-income students and well-resourced high schools. Correcting students' beliefs is possible though. Jensen (2010) documents that providing correct information about returns to secondary education increases the number of school years completed. In Canada, Oreopoulos and Dunn (2013) found that after watching a very short informational video about returns of higher education, students in high schools with low college enrollment rates reported expecting higher returns to college and being more likely to enroll at a post secondary institution three weeks later. However, the study did not measure whether these higher aspirations translated into higher enrollment rates.[15]

Low-SES students could also have wrong beliefs regarding their own academic ability, leading them to apply to the wrong set of colleges. Arcidiacono et al. (2016) provides a model in which high school students have imperfect information about their college and labor market ability and estimate that removing informational frictions would increase college graduation rates through increased sorting on ability. Light-touch informational interventions targeting high-achieving low-SES students have shown promising results. Hoxby and Turner (2013) find that providing high-achieving, low-income students with customized information about college costs and the application process and an application fee waivers substantially increased college application and enrollment rates. However, a larger replication exercise involving 785,000 students failed to find any significant effect of a similar information intervention (Gurantz et al., 2019), which suggests that the success of information interventions are highly design and context-dependent. Dynarski et al. (2018) sent high-achieving low-income students and influential adults (parents and school principals) packages encouraging them to apply to the University of Michigan and promising free tuition upon acceptance. The intervention was succesful in doubling the rates at which the students applied and enrolled at the sponsoring institution. Interestingly, the authors look at the impact of their intervention on enrollment at other selective colleges and rule out that their intervention had negative spillover effects.

Providing intensive counselling to college-applicants significantly increases enrollment at selective colleges (Avery, 2018) and particularly in programs flagged by the counselling (Castleman and Goodman, 2018). Carrell and Sacerdote (2013) find that mentoring by undergraduate students coupled with cash incentives boosted the college application and enrollment rate of students in New Hampshire. Bettinger et al. (2012) also finds that providing low-income families with financial aid information and assistance to complete

[15] Page and Scott-Clayton (2016) provides a review of informational and behavioral interventions and affirmative action policies targeting low-income students and their impact on a wide range of outcome, including the type of college attended.

financial aid applications increased financial aid forms submissions and subsequent enrollment rates.

There exists therefore a large body of evidence indicating that low-SES students face considerable search frictions in the college market. Interventions which alleviate those frictions by providing accurate information about returns or costs of college or the student's admission probability have been successful in improving low-SES students' college outcomes. There are three caveats to the existing body of work though. First of all, very few evaluations of institution-specific interventions allow researchers to assess their spillover effects and rule out that students substituted away from institutions of equal quality. Second,there is a large amount of evidence of the effectiveness of information interventions on the enrollment/application margin, but much less work focuses on drop-out and graduation rates. Third, the brunt of the existing evidence focuses on the behavior of high-achievers, as measured by their high school academic achievements. Understanding how those policies affect non-high-achievers, which represent the large majority of low-SES students, is important to be able to assess their effectiveness to promote social mobility on a larger scale.

If inaccurate information or beliefs generate a college achievement gap, eliminating admission uncertainty has the potential to reduce it. The "top percent" policies in place in states such as Texas guarantee admission at selective universities for the top decile of students within each high school. However, the introduction of a "top 10%" policy in Texas has not increased the number of high schools that funneled students to the state's flagship universities but reaching out to underrepresented high schools increased the probability that their graduates would take advantage of the program (Cortes and Klasik, 2019).

In theory, the effect of affirmative action on students' college application should also depend on the amount of information the students possess before the introduction of affirmative action. If students possess perfect information about their academic ability and college quality, affirmative action should lead to increased enrollment at the sponsoring institutions, only on the condition that those represent the best match for the students. Evidence from affirmative action bans in Texas and California indicate that the application patterns of minority students changed little after the ban (Antonovics and Backes, 2013; Card and Krueger, 2005) and that the change was the most pronounced for students which were unlikely to gain admission under the new conditions (Yagan, 2016). However, the introduction of affirmative action may have a different impact on applications than its ban. On one hand, it could, for instance, signal to students that they would be a good fit for a more selective institution and encourage students to apply more broadly. On the other hand, it could leads students to substitute into the sponsoring-institution at the detriment of other colleges of equal quality. My analysis is the first to analyze such spillover effects from the introduction of an institution-specific affirmative action program.

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4 Theoretical framework

Drawing on the insight that students may have wrong beliefs over their academic abilities, I build a simple model of college application in the presence of affirmative action and imperfect beliefs.

Baseline model: The baseline model builds on Card and Krueger (2004) and Howell (2010)'s models of college application and accounts for the uncertainty of the application process. At the end of high school, a student of ability $a \in [0,1]$ can choose between enrolling at a non-selective college or applying to one or several of J selective colleges. All applications happen simultaneously. I assume that a student's probability of getting admitted at a selective college conditional on applying is proportional to her ability: $p(a) = \lambda a$, with $\lambda \in (0,1]$. All selective colleges yield the same return $U > 0$ and the cost of applying to j selective colleges is given by $c(j)$, which is increasing in the number of applications: $c'(j) > 0$. For simplicity, I normalize the cost of applying and the utility derived from attending a non-selective college to zero.

Let v be a $J \times 1$ binary vector, such that $v_k = 1$ if the student applies to the k^{th} college. Her application strategy will be given by the choice set $C_v = \{k | v_k = 1\}$, that contains j elements: the number of selective colleges the student applies to. As applications to selective colleges all yield the same expected utility and are equally costly, a student's expected utility from a given choice set S_v depends solely on j:

$$\mathbb{E}(U(C_v)) = \mathbb{E}(U(j)) = U(1-(1-p(a))^j) - c(j) = U(1-(1-\lambda a)^j) - c(j) \qquad (1)$$

For simplicity, let's assume that the marginal cost of applications is constant, such that $c(j) = cj$. The optimal number of selective colleges the student should apply to will be given by: [16]

$$j^* = Max\{\frac{\log\left(-\frac{c}{U\log(1-\lambda a)}\right)}{\log(1-\lambda a)}, 0\} \qquad (2)$$

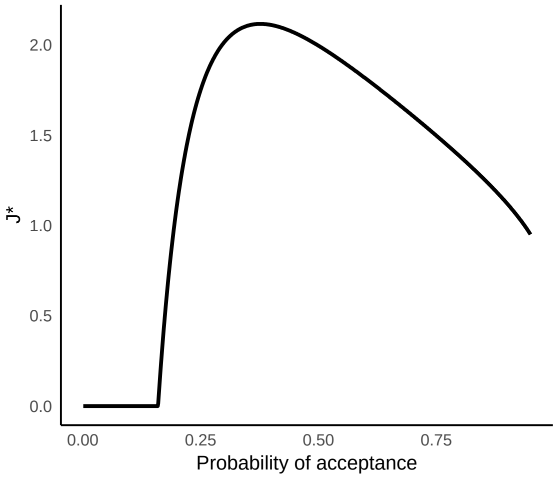
Figure 5 plots the theoretical number of optimal applications depending on the student's ability. Only students above a certain ability threshold start applying to selective colleges: given that an application is costly, the probability of admission needs to be high enough for a student to fill in one application. The number of applications to selective colleges

[16] Setting the first derivative of $\mathbb{E}(U(j))$ equal to zero yields:

$$\begin{aligned} Ulog(1-\lambda a)(1-\lambda a)^{j^*} &= c \\ \Leftrightarrow (1-\lambda a)^{j^*} &= \frac{c}{-Ulog(1-\lambda a)} \\ \Leftrightarrow j^* log(1-\lambda a) &= log(\frac{c}{-Ulog(1-\lambda a)}) \\ \Leftrightarrow j^* &= \frac{\log\left(-\frac{c}{U\log(1-\lambda a)}\right)}{\log(1-\lambda a)} \end{aligned}$$

then increases with the student's ability up to a certain point. After a certain ability threshold, students start applying to fewer selective colleges as applications are costly and the probability that one application is succesful is high.

Figure 5: Number of college applications as a function of acceptance probability
Baseline case



Notes: Figure 5 plots the optimal number of total applications to selective colleges that a student should file depending on her probability of admission: $p(a) = \lambda a$. On the x-axis is the student's probability of getting accepted into a selective colleges conditional on applying. Attending a selective college yields pay-offs that are constant across selective colleges and academic ability (normalized to 11.5), there is no fixed cost and constant marginal costs to an additional application (normalized to 2).

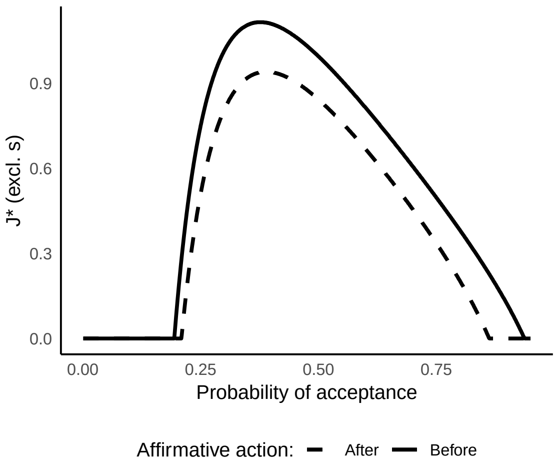
Introducing affirmative action What happens when one college s introduces affirmative action which increases $p_s(a) = \lambda_s a$, a student's probability of admission to s at all levels of academic achievements? The probability of enrolling in s conditional on applying becomes larger than the conditional probability of enrolling at other colleges: $\lambda_s > \lambda$. This implies that if a student decides to submit an application to a selective college, she will start by applying to s as it is now the application that yields the highest expected utility.

Let j_{-s} be the number of applications to colleges other than s and let v_s be equal to one if college s is in the student choice set, 0 otherwise. I can rewrite the student's expected utility as:

$$\mathbb{E}(U(C_v)) = \mathbb{E}(U(j_{-s}, v_s)) = \mathbb{1}\{v_s = 1\}(U(\lambda_s a + (1-\lambda_s a)(1-(1-\lambda a)^{j_{-s}}) - c(j_{-s}+1)) \quad (3)$$

Conditional on applying to college s, the optimal number of applications to selective colleges other than s will be given by:

Figure 6: Number of college applications as a function of acceptance probability
Spillover effects of affirmative action



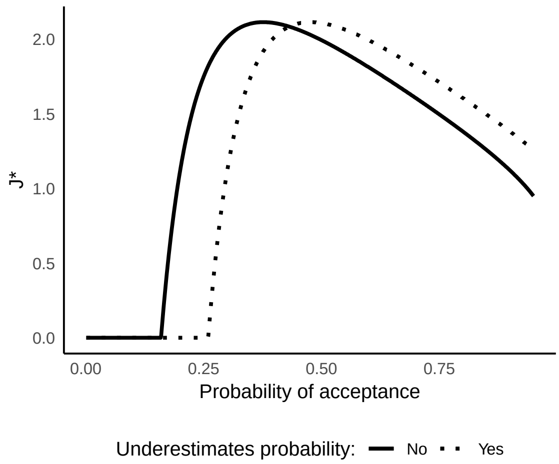
Notes: This figure shows the spillover effects of affirmative action and plots the theoretical optimal number of applications to selective colleges other than college s, which has introduced affirmative action. The solid line plots the optimal number of applications to selective colleges other than s before the introduction of the affirmative action program, when the admission probability at s is low. The dashed line when the admission probability is higher after the affirmative action program is introduced. On the x-axis is the student's probability of getting accepted into a selective colleges conditional on applying. Attending a selective college yields pay-offs that are constant across selective colleges and academic ability (normalized to 11.5), there is no fixed cost and constant marginal costs to an additional application (normalized to 2).

$$j^*_{-s} = Max\{\frac{\log\left(-\frac{c}{U(1-\lambda_s a)\log(1-\lambda a)}\right)}{\log(1-\lambda a)}, 0\} \tag{4}$$

When college s increases the probability that a student will get admitted, that student should substitute away from other selective colleges and apply to fewer of them, as the expected return of each extra application decreases. Figure 6 plots the optimal number of applications to selective colleges other than s (on the y-axis), according to the student's conditional probability of admission (on the x-axis), which is increasing in the student's ability. The solid curve plots this for a low probability of admission in s and the dashed curve for a high probability of admission. When λ_s increases, the optimal number of applications to other colleges should therefore go down for all students.

Beliefs So far, the model has assumed that the students hold correct beliefs about their academic ability and hence their probability of admission and their returns to attending a selective college. However, one common explanation for the social gap in application behavior is that low-income students lack information regarding higher education and under-estimate their chances of gaining admittance or graduating from a selective college.

Figure 7: Number of college applications as a function of acceptance probability
Introducing beliefs



Notes: This plots how under-estimating one's academic ability shifts the optimal application curve. The solid line plots the number of applications to selective colleges of students who hold corrects beliefs about their academic ability and the dotted line that of students who under-estimate their academic ability. On the x-axis is the student's probability of getting accepted into a selective colleges conditional on applying. Attending a selective college yields pay-offs that are constant across selective colleges and academic ability (normalized to 11.5), there is no fixed cost and constant marginal costs to an additional application (normalized to 2).

Specific interventions informing high-achieving, low-income students about their chances of admission have been able to increase students' applications to selective colleges (Dynarski et al., 2018; Hoxby and Turner, 2013). Assuming that students have imperfect information about their college academic ability has been included in previous models of college/labor market choice (Arcidiacono et al., 2016) but not to predict college application behavior.

I now extend the baseline model and allow the students to hold incorrect beliefs, $\tilde{p}(a)$, about their admission probability, such that $\tilde{p}(a) < p(a)$. Figure 7 overlays the optimal number of applications response curves of students who are facing the same parameters, except for the fact that one type of students, represented by the solid line, hold correct beliefs regarding their probability of acceptance and the second type of students, represented by the dotted line, systematically under-estimate their probability of admission. The impact of those incorrect beliefs on the number of selective colleges the student applies to depends on her academic achievement. Non-high-achievers apply to fewer selective colleges than what would be optimal had they held correct beliefs, but high-achievers apply to more selective colleges than they would otherwise have.

If I allow low-SES students to hold incorrect beliefs regarding their probability of admission, the spillover effects of the affirmative action program will also depend on whether it affects those beliefs and in which direction. In addition to increasing the probability of being admitted to s, can the affirmative action program push students' beliefs upward? In that case, the introduction of the affirmative action will not only shift the optimal response curve downwards, it will also lead students to re-evaluate their probability of admission to selective colleges. The additional effect of this change in beliefs will depend on academic achievement.

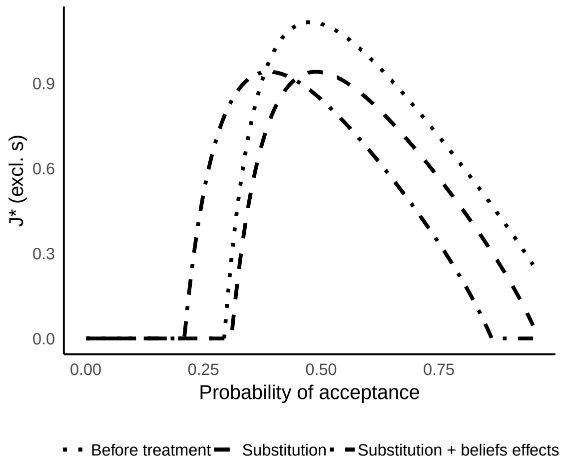
I model what happens when allowing the affirmative action program to shift beliefs in figure 8. As before, the dotted line represents the optimal number of applications to selective colleges before the introduction of the affirmative action program when students systematically underestimate their admission probability. The affirmative action program gets introduced and lowers $1-\lambda_s a$. This lowers the returns to applications to other selective colleges and the optimal response curve shifts downward, to the dashed line. However, the program offers an opportunity for students' to learn about their true probability of admission, which corrects their beliefs upwards. This shifts the optimal response curve to the left. The dot-dash line represents the combined effects of the increase in λ_s and the corrected beliefs on the students' college application strategies. The spillover effects can actually be positive providing the change in beliefs is large enough.[17]

Therefore, without a change in beliefs, the introduction of the affirmative action program will lead to a decrease in applications to other selective colleges for all students. However, the affirmative action program boosting low-income students' beliefs in their probability of admission to selective colleges will have an additional impact: low-achieving students will apply to more selective colleges and high-achieving students to even less. Which of those effects dominate is an empirical question that will be the object of the rest of that paper.[18]

[17] Note that those effects are indistinguishable from the case in which students initially hold incorrect beliefs regarding the utility of attending college and in which the affirmative action program corrects those beliefs upward.

[18] The model extends to a setting in which returns to college attendance is an increasing function of academic ability. Note that the predictions of the model would be similar if I introduced fixed costs to college applications and if the affirmative action program did not affect students' beliefs.

Figure 8: Number of college applications as a function of acceptance probability
Spillover effects of affirmative action with an effect on beliefs



Notes: This figure overlays the substitution and beliefs effect of the affirmative action program on students' applications to other selective colleges. The dotted line represents the optimal number of applications to selective colleges for students who systematically under-estimate their academic ability and their probability of admission to selective colleges before the introduction of the program by s, hence before an increase in λ_s. The dot-dash line represents the optimal number of applications once λ_s increases, if it does not correct students' beliefs upwards. The long-dash line represents the optimal number of applications after the introduction of the affirmative action program if the program corrects the beliefs upwards. It represents the combined effects of the increase in λ_s and the corrected beliefs. On the x-axis is the student's probability of getting accepted into a selective colleges conditional on applying. Attending a selective college yields pay-offs that are constant across selective colleges and academic ability (normalized to 11.5), there is no fixed cost and constant marginal costs to an additional application (normalized to 2).

5 Data and descriptive evidence

In this section, I describe the data I am using throughout my analysis to measure students' higher education outcomes and describe the gap in college achievement that divides students graduating from a low-SES school to students graduating from a high-SES high school.

5.1 Data

To understand how affirmative action affects students' higher education outcomes, I obtained restricted access to four sources of administrative data that cover the universe of high school students, their grades at the Baccalauréat, their college applications and which college they are enrolled at, up to five years after high school. Using an anonymized student identifier, I merge those databases at the student level. It is, to my knowledge, the first time those four databases are brought together.

Figure 9 shows the structure of my data. The OCEAN database, managed by the French Ministry of Education, provides the list of students who registered to take the Baccalauréat. I use this database to identify students enrolled in the final year of high school between 2009 and 2017. This data provides me information about all students' demographic characteristics — their age, gender, nationality and the occupation of one parent — and their academic achievement: their high school major and the grade they obtained in the Baccalauréat. Two sessions are held for the Baccalauréat: in June and September. If a student receives a grade between 8 and 10 in the June session, she is eligible to retake some exams in September.[19] I use only the grade obtained in the June exam as a measure of the student's academic achievement.

In addition, I obtain data on college enrollment by combining two databases: the Base Scolarité managed by the French Ministry of Education, which contains information on enrollment at Universities and preparatory schools and the base SISE managed by the French Ministry of Higher Education, Research and Innovation which contains enrollment on Grandes Ecoles. Those record the list of students that are enrolled at a given college as of January of a given academic year. For those colleges, this data is complete for students graduating high school between 2009 and 2016. Students appear in those databases until they leave college. While I cannot see whether or not students graduate from college, I can observe for early cohorts whether students are still enrolled in college five years after high school graduation, which is usually the year of college graduation for Grandes Ecoles. Note that, however, other types of higher education programs — such as short vocational trainings — do not appear in those databases: if a student does not appear in the enrollment data I cannot rule out that they are pursuing another form of higher education.

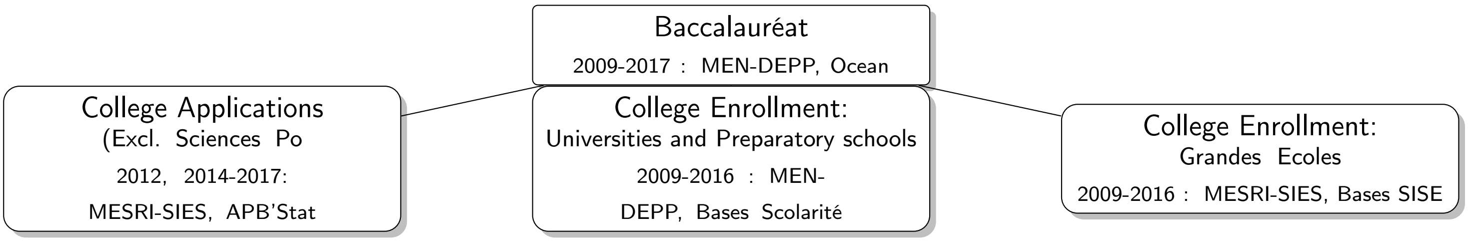
Finally, my data on college applications comes directly from the "Admission Post-Bac" (APB) website. This gives me the list and types of programs the students have applied to and their preference ranking. All students enrolled in a French high school automatically appear in this database and the dataset contains the universe of students enrolled in a French high school. I have access to five years of data: 2012 and 2014-2017. Note that the number of colleges that have joined the APB website has increased over time: from 3,078 in 2012 (and 10,629 unique programs) to 3,548 in 2017. Importantly, the short vocational trainings that may not appear in my higher education data exist in the application data. That said, the overlap between my application and my higher education database is large but not perfect: I can find information on enrollment at more than 97% of the colleges available in the application data.

I have to note here that Sciences Po is an exception. It did not join the APB website to be able to hold admissions according to its own calendar. I therefore cannot observe who applied to Sciences Po, but I can see who enrolled there.

Sample selection I restrict my analysis to students enrolled at a public high school in metropolitan France, which represents about 83% of students in the last year of high school. Further, I restrict my sample to students who receive a grade of ten out of 20 in the June session of the Baccalauréat, and are therefore allowed to enroll in higher education.

[19] 78% of students in my sample receive a passing grade —10/20— in the June session. Obtaining a passing grade is required to graduate high school and enrolling in higher education.

Figure 9: Data structure



Notes: This figure represents the structure of my data: I merge the college enrollment and application databases to the list of students registered to take the Baccalauréat exam.

Socio-Economic Status I define students' SES based on their parents' reported professional occupation category. The OCEAN data contains information about only one parent's occupation, which reduces the precision of this classification. Following the literature (Guyon and Huillery, 2016), I classify students whose parent's reported occupation is high-skilled — typically requiring five years of higher education— as high-SES and students for which the reported parental occupation is intermediate to lower skilled as low-SES. This includes parents who never worked, are manual laborers, low-skilled workers such as craftmen or storekeepers, or those who work in intermediate occupations. Based on this classification, low-SES students represent 54.8% of my sample. In the application data, I can observe eligibility for a means-based scholarship, which is significantly and positively but modestly correlated with being low-SES. [20] Given this modest correlation and the fact that only one parent's occupation is reported, I only refer to individual students' low-SES status in heterogeneity analyses. Instead, I average students' SES at the high school level, which is the level at which the treatment occurs. At the school level, measurement error in students' SES should be less of a concern.

I define high-SES schools as those which have an average proportion of low-SES students in the first quartile of the distribution over the period of study, and low-SES schools as schools which have an average proportion of low-SES students in the fourth quartile of the distribution. I distinguish between students attending "low-SES" and "high-SES" schools to describe how higher education outcomes vary according to one's socio-economic status in the next subsection.

5.2 Descriptive evidence

Social gap in college attendance. Figure 10a presents the non-parametric relationship between baccalauréat grades and the probability of enrolling at a selective college in the academic year following high school graduation. I estimate this relationship through a restricted cubic spline regression with 5 knots, separately for low and high-SES schools. At all levels of accademic achievement, students graduating from a high-SES school are more likely to enroll at a selective college than comparable peers in a low-SES school. Those differences are present for enrollment at both Grandes Ecoles and preparatory schools (see

[20] Correlation coefficient: 0.3549, p<0.0001.

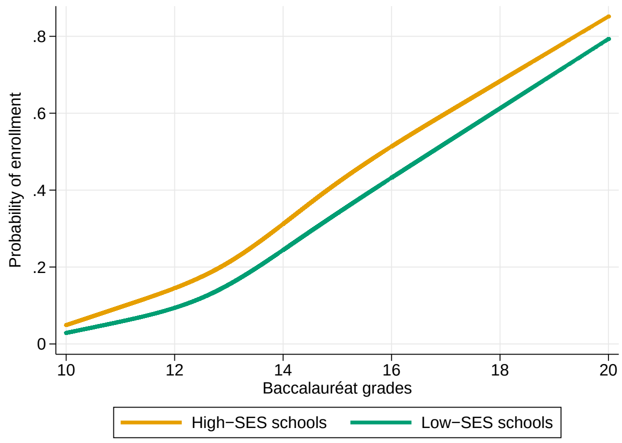
figure 11) However, that gap shrinks to the point of almost disappearing when the sample is restricted to students having applied to at least one selective college. This is what I plot in figure 10b.[21]

In appendix A, I formally assess the magnitude of the social gap in selective college enrollment. In column (1) of table A.1, I find that students graduating from low-SES schools are 11.52pp. less likely than students from high-SES schools to be enrolled at a selective college. Controlling for differences in academic achievement as measured by baccalauréat test scores brings this gap down to a statistically significant 5.2pp. This is a difference of large magnitude as 20% of students in this sample are enrolled at a selective college. Controlling for gender, nationality, high school major or year fixed effects does not significantly affect the magnitude of this gap. In table A.2, I restrict my sample to students who have submitted at least one application to selective colleges. After controlling for academic achievement, the social gap in college enrollment disappears: the results in column (2) suggest that students graduating from low-SES schools would be 0.1pp. more likely to enroll at a selective college conditional on applying, a negligible and statistically insignificant difference. Those results are consistent with the hypothesis that the application behavior of low-SES students is one of the key elements of social gap in college attendance.

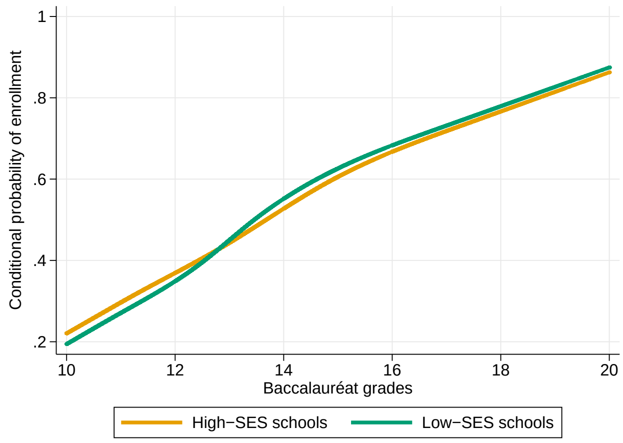
[21] Those dynamics could be driven by the fact that only high-SES students graduating from those high schools apply to a selective college. In appendix F, I find similar patterns based on the students' reported socio-economic status rather than on their schools: a large high-low SES gap in the unconditional probability of enrollment at a selective colleges that shrinks when I restrict the sample to students having applied to a selective college.

Figure 10: Probability of enrollment into a selective college and baccalauréat grades.

(a) Unconditional



(b) Conditional on having applied to one selective college



Notes: This plots the non-parametric relationship between baccalauréat grades and the probability of being enrolled at a selective college. This non-parametric relationship is obtained through a restricted cubic spline regression with 5 knots. Figure 10a plots the unconditional probability of being enrolled at a selective college. In figure 10b, I plot the probability of being enrolled at a selective college conditional on having applied to at least one selective college. The sample is restricted to students having obtained a passing grade at the baccalauréat. The baccalauréat is a uniform exam taken at the end of high school. With a few exceptions, students must obtain a grade of 10/20 to be allowed to graduate and enroll at a higher education institution. "High-SES high schools" are high schools that have a proportion of low-SES students which is, on average over my study period, in the first quartile of the sample. "Low-SES high schools" are high schools that have a proportion of low-SES students which is, on average over my study period, in the fourth quartile of the sample. Low-SES students are considered low-SES if none of their parents has a profession requiring at least five years of higher education studies. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité. 2012, 2014-2016.

Social gap in college applications. I therefore turn to studying differences in college application behaviors in table A.3. Without conditioning on test scores, students from low-SES schools are 17.28pp. less likely than students from high-SES schools to have applied to at least one selective college. After controlling for test scores, the social gap remains large, at 10.16pp., or 26% of the mean.

According to the simple baseline model of college application behavior, two variables should determine a student's number of applications to selective colleges: the cost of one application and the student's ability. Indeed, academic ability is positively correlated to probability of admission. In figure 11, I plot the relationship between academic ability and academic achievement —using baccalauréat grades as a proxy for ability— for students in low-SES and high-SES schools. To account for the role of application costs in my model, I distinguish between applications to Grandes Ecoles, which are costly, and applications to Preparatory schools, which have a very low cost. Two facts stand out from those figures. First, the number of costly applications filled by students from high-SES schools is hump-shaped, as predicted by the model: high-achievers apply to fewer Grandes Ecoles than non-high-achievers. Second, at almost all levels of academic achievement, low-SES school students apply to selective colleges at a lower rate than their peers, even for preparatory schools where applications have a very low cost. The lone exception is very high-achievers from low-SES schools that apply to more Grandes Ecoles than their peers from high-SES schools. This pattern would be consistent with the observations of the model (see figure 7), where the hump-shaped application profile means that high-achievers that underestimate their admission probability send out more applications.

Those patterns are in line with the existing evidence from other high-income countries ruling out that social differences are driven by discriminatory selection on the college side and are, instead, the product of the application behavior of low-SES students (Dillon and Smith, 2017).

Was the affirmative action program introduced by Sciences Po succesful in moving the behavior of students attending low-SES schools closer to that of their high-SES peers? To answer that question, it is first necessary to understand what move schools to join the affirmative action program.

6 Identification

6.1 Potential outcomes framework

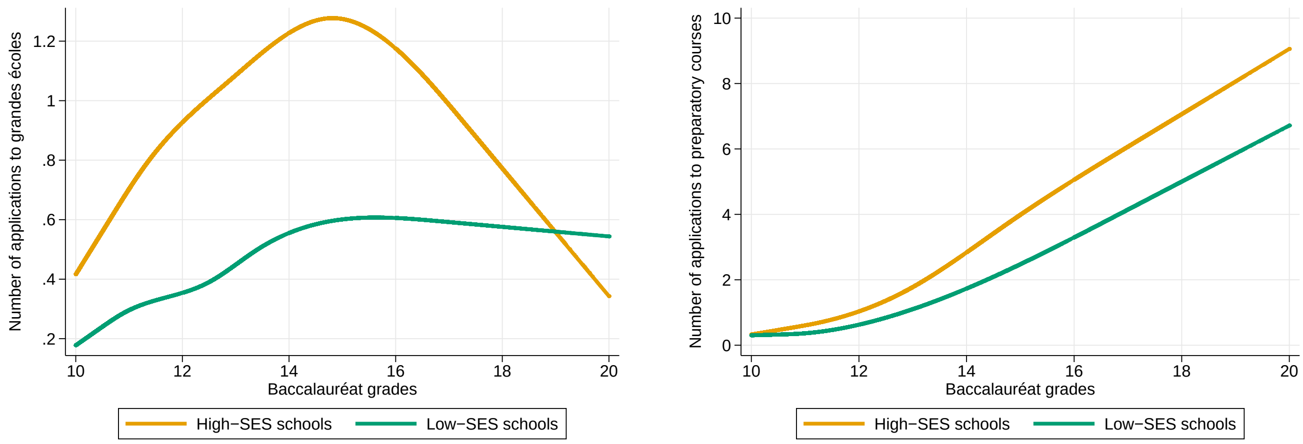
In this subsection, I introduce the notation that I will use throughout the rest of the paper. It follows closely the notation used by Callaway and Sant'Anna (2019) and takes into account the fact that high schools get treated in different years.

Let t denote a particular time period with $t = 1, ..., T$ and D_t be an indicator variable equal to one if a high school is treated in period t and 0 otherwise. In addition there are N high schools indexed $h \in \{0, 1..., N\}$. I further introduce a series of dummy variables:

Figure 11: Applications to Grandes Ecoles and preparatory schools

(a) Applications to Grandes Ecoles (Application cost>ϵ)

(b) Applications to preparatory schools (Application cost<ϵ)



Notes: Figures 11a and 11b present the non-parametric relationship between the number of a student's applications to Grandes Ecoles and preparatory schools separately as a function of her baccalauréat grades. The cost of one Grande Ecole application is high: Grandes Ecoles require students to take an exam during the year of the baccalauréat, which can involve traveling long distances. Most of them require students who are ineligible to means-tested scholarships to pay an exam fee. The cost of one application to a preparatory school is very low once students have created an account on APB: their teachers fill in the students' grades and their comments. The students then only need to select which preparatory school they want to apply to.

The sample is restricted to students having obtained a passing grade at the baccalauréat. The baccalauréat is a uniform exam taken at the end of high school. With a few exceptions, students must obtain a grade of 10/20 to be allowed to graduate and enroll at a higher education institution. "High-SES high schools" are high schools that have a proportion of low-SES students which is, on average over my study period, in the first quartile of the sample. "Low-SES high schools" are high schools that have a proportion of low-SES students which is, on average over my study period, in the fourth quartile of the sample. Low-SES students are considered low-SES if none of their parents has a profession requiring at least five years of higher education studies. Source: DEPP-SIES, 2012, 2014-2016. Source: APB'Stat and DEPP/SIES data. 2012, 2014-2016.

C, which is equal to 1 if the high school never enters the treatment and G_g, which is equal to 1 if a high school enters the treatment at $t = g$. Let $y_{iht}(0)$ denote the higher education outcome for a student i attending a non-participating high school h at time t, and $y_{iht}(1)$ the outcome of a student attending a participating high school.

For students attending high schools in a particular treatment cohort, the average treatment effect at t is given by:

$$ATT(g,t) = \mathbb{E}[y_{iht}(1) - y_{iht}(0)|X, G_g = 1] \tag{5}$$

where X is a vector of control variables.

However, in each period, I only observe one outcome per student, which is: $y_{iht} = y_{iht}(1)D_t + y_{iht}(0)(1 - D_t)$. Under the assumption of parallel trends in baseline outcomes, $ATT(g,t)$ can be recovered by difference-in-differences:

$$ATT(g,t) = [\mathbb{E}(y_{iht}|X, G_g = 1) - \mathbb{E}(y_{iht}|X, C = 1)] - [\mathbb{E}(y_{ihg-1}|X, G_g = 1) - \mathbb{E}(y_{ihg-1}|X, C = 1)] \tag{6}$$

6.2 Testing for pre-trends

Parallel trends in baseline outcomes are required for difference-in-differences estimates to yield causally interpretable results. To test whether this assumption is met, I start by plotting the path of college enrollment around the event time. For each treatment year, I create a separate sample consisting of high schools that get treated in that year and high schools that are never treated. I then center those samples around the time of treatment and stack them. Figure 12 depicts the path of the mean of several variables of interest around the time of treatment.

Figure 12a shows that prior to the treatment, treated and untreated schools have similar and very low rates of enrollment at Sciences Po. It does not appear that students from treated high schools started to enroll at Sciences Po at a higher rate before the agreement was in place.

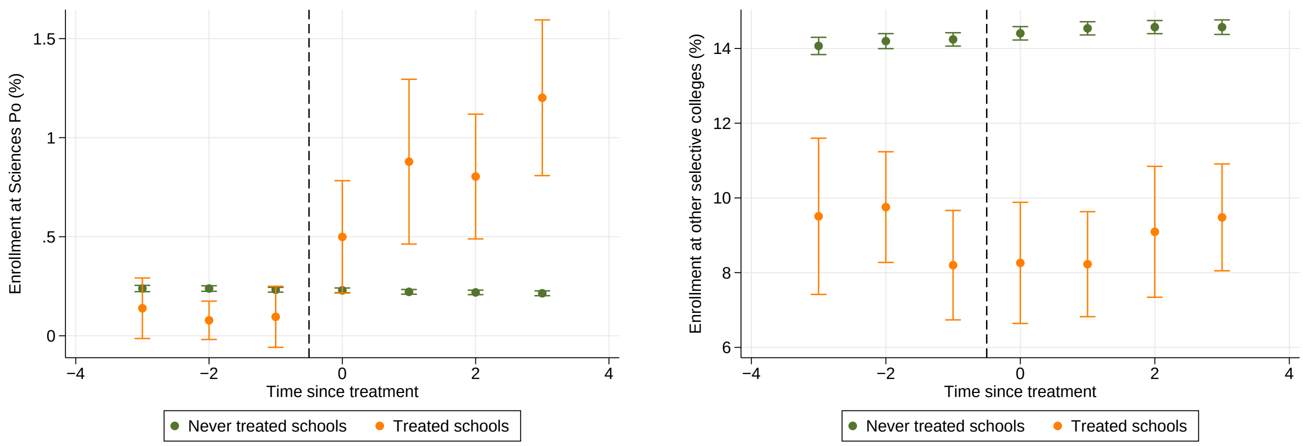
However, the path of enrollment at selective colleges other than Sciences Po, plotted in figure 12b, shows that enrollment rates decreased in treated schools before the start of the affirmative action program and stabilized after the start of the program. Those patterns are reminiscent of an Ashenfelter dip (Ashenfelter, 1978) and could lead to erroneous causal interpretations of the impact of the affirmative action program if not taken into account. In addition, they may indicate that high schools' decision to join the program could be driven by a decrease in the amount of graduates that enroll in selective colleges, encouraging high school principals to seek closer collaboration with Sciences Po.

Therefore, conditional on being treated, the timing of the treatment is not random across high schools, violating the assumption of parallel trends in baseline outcomes.

Figure 12: Path of variables of interest around the event time

(a) Students attending Sciences Po (%)

(b) Students attending selective colleges (excl. Sciences Po) (%)



Notes: Those figures show how the average value of different variables in the group of treated and never treated high schools evolves around the time of treatment. The event starts when the high school has an affirmative action agreement in place with Sciences Po. The agreement is signed in period $t = -1$ and is effective at $t = 0$. The unemployment rate and the proportion of immigrants are measured at the level of the municipality in which the high school is located. The sample is all public high schools located in continental France and the data ranges from 2009 to 2016. The unit of observation is a high school. The sample is restricted to never treated high schools or high schools who enter the treatment between 2010 and 2014. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité.

6.3 Matching on pre-event trends

To address the dip in selective college enrollment that precedes high schools joining the affirmative action program, I use a matching strategy to identify a set of control students who attended high schools with similar pre-event outcomes. Conditioning on only one pre-treatment outcome can bias the DiD estimator and recent theoretical work, backed by simulations, recommends matching on a larger number of pre-treatment outcomes to reduce the bias of the DiD estimator (Chabé-Ferret, 2017).

Within each high school, I aggregate students in cells based on their gender, their SES category, baccalauréat grade bins, whether or not they are French and their high school majors.[22] I then proceed to build a separate sample of control cells for each treatment wave, excluding high schools which were treated before 2009 and for which I cannot observe pre-treatment characteristics. I also exclude cohorts where only one high school gets treated in a given year.[23] The same cell can be used as a control for several waves of treatment.

The matching procedure involves matching cells exactly on gender, a dummy for French nationality, academic track and baccalauréat grade bin. Cells for whom there is no exact match for each of these categories are dropped. For each treated cell, I then identify the nearest-neighbour without replacement among similar cells, based on the Mahalanobis (1936) distance between cell i and cell j:

$$D_{ij} = (X_i - X_j)'\Sigma^{-1}(X_i - X_j),$$

where Σ^{-1} is the inverse of the sample variance-covariance matrix of the covariates X in the total sample.

I obtain 8,036 matched year $\times$ cells in the enrollment data, covering 35,486 students and 3,660 year $\times$ cells in the application data, representing 17,306 students. Table 4 shows that the final matched sample is balanced in terms of most variables used for matching. However, some small differences persist between the control and the treatment group. For instance, treated cells have a slightly lower proportion of low-SES students at baseline, slightly lower grades at the baccalauréat and have applied to slightly fewer selective colleges. Reassuringly, those differences are very small in magnitude: treated school have 0.55pp. extra low-SES students than matched control school, which represents less than 1% of the proportion of low-SES students in control schools. I explain in the following subsection how I adjust my estimates to correct for those imbalances.
I also check for balance across values of the matching variables in pre-event periods I have not used for matching. My matched sample is balanced on attendance in terms of college enrollment, but my sample remains slightly imbalanced with respect to the proportion of low-SES students and baccalauréat grades. To account for this, I check that controlling for high school composition or the baccalauréat grades does not affect my estimates much. Finally, my sample is balanced regarding the pre-event values of variables I have not used

[22] In the academic track, French high schools propose three majors: Sciences, Economics and literature
[23] I include them in robustness checks. Excluding them does not affect my results

for matching.

As a robustness check, I vary the set of variables used for matching and estimate the treatment effect for my main outcome variables in this new matched sample. The distribution of those alternative treatment effects is in appendix B. I find that the estimated treatment effects do not vary much when excluding or adding variables to the set of matching variables.

Table 4: Balance table: matched sample- nearest neighbor matching

Variable	(1) Treated	(2) Control	(3) Difference		(4) Treated	(5) Control	(6) Difference
	Variables and periods used for matching:				Pre-event value of variables not used for matching		
Female	0.600	0.619	-0.000	Enrolled at other, 1 year after high school	0.102	0.104	0.001
	(0.490)	(0.486)	(0.000)		(0.203)	(0.200)	(0.006)
Foreigner	0.004	0.004	0.000	Enrolled at other, 2 years after high school	0.091	0.094	-0.002
	(0.064)	(0.060)	(0.000)		(0.183)	(0.182)	(0.006)
Low$_{S}ES$	0.803	0.787	0.000	Enrolled at other, 3 years after high school	0.089	0.092	-0.001
	(0.398)	(0.409)	(0.000)		(0.181)	(0.177)	(0.006)
Sciences Po, (g-3) to (g-1)	0.001	0.001	0.000	Enrolled at other, 4 years after high school	0.090	0.093	-0.002
	(0.018)	(0.018)	(0.001)		(0.184)	(0.178)	(0.006)
Other colleges, (g-3) to (g-1)	12.837	12.578	0.720	Enrolled at Sciences Po, 1 year after high school	0.000	0.002	-0.000
	(22.322)	(21.596)	(0.580)		(0.011)	(0.019)	(0.001)
Applications to selective colleges, (g-2) to (g-1)	2.135	2.472	0.159*	Enrolled at Sciences Po, 2 years after high school	0.000	0.002	-0.000
	(3.499)	(3.616)	(0.094)		(0.011)	(0.019)	(0.001)
Applied to selective colleges, (g-2) to (g-1)	0.375	0.407	0.004	Enrolled at Sciences Po, 3 years after high school	0.000	0.002	-0.000
	(0.350)	(0.361)	(0.008)		(0.011)	(0.016)	(0.000)
Low-SES (%), (g-1)	74.301	72.533	1.768***	Enrolled at Sciences Po, 4 years after high school	0.000	0.001	-0.001*
	(9.817)	(11.203)	(0.595)		(0.000)	(0.016)	(0.001)
Baccalauréat, (g-1)	11.937	12.083	-0.146	At least one application	1.000	1.000	0.000
	(1.668)	(1.714)	(0.095)		(0.000)	(0.000)	(0.000)
	Matching variables, other periods						
Selective college ranked first	0.379	0.407	-0.012				
	(0.344)	(0.351)	(0.027)				
Sciences Po	0.000	0.003	-0.001	Enrolled at selective college when ranked first	0.489	0.433	0.071
	(0.000)	(0.017)	(0.001)		(0.364)	(0.416)	(0.072)
Other colleges	15.385	13.006	0.770	Enrolled at first wish	0.336	0.346	0.013
	(23.022)	(20.396)	(2.045)		(0.290)	(0.295)	(0.042)
Low-SES (%)	70.204	67.109	1.962***				
	(12.791)	(12.603)	(0.359)				
Baccalauréat	12.025	12.115	-0.066***				
	(1.683)	(1.711)	(0.016)				
Applied to selective colleges	0.358	0.364	0.075**				
	(0.315)	(0.369)	(0.028)				
Applications to selective colleges	0.965	0.844	0.285				
	(0.897)	(0.782)	(0.227)				
Unique cells × year in enrollment data	4,018	4,018	8,036	Unique cells × year in application data	1,830	1,830	3,660
Unique student × year in enrollment data	16,994	18,492	35,486	Unique student × year in application data	8,276	9,030	17,306

Notes: The balance table shows summary statistics for the matched sample of students. Student observations are aggregated to a SES/gender/major/baccalauréat achievement/foreigner cell and matched. The treated column refers to students attending high schools that will enter the affirmative action program at g. The control column refers to students attending high schools that will not enter the affirmative action program at any point in my sample. The sample is matched exactly on the following characteristics: a dummy for female, a dummy for low-SES, a dummy for foreigner, high school majors and a rough measure of baccalauréat grade. Each treated observation is then matched to its nearest neighbour, without replacement, based on the Mahadonis distance for the following baseline variables: the percentage of low-SES students in the high school at $t = -1$ (10 bins), the proportion of students who attended a selective college other than Sciences Po in the three years preceding the treatment, the proportion of students who applied to a selective college, the number of applications to selective colleges and the proportion of students who enrolled at Sciences Po the three years before the treatment and the average baccalauréat grade at $g - 1$ (5 bins each). If fewer years of data are available, the matching on the educational outcome variables is restricted to two, or one year, of data. Those summary statistics are weighted by cell size.

6.4 Bias-adjusted estimates

Matching on several continuous variables may introduce a bias in the nearest-neighbor estimator: as explained above, there remain pre-event differences between the treatment and the control cells. I therefore implement a bias-correction procedure proposed by Abadie and Imbens (2011) to address that issue.[24] This method consists in estimating the effect of the matching variables on the outcome variables, separately in the treatment and control group, and using those estimates to adjust for differences in covariate values within matched

[24] This approach has been implemented in a similar setting of matching on pre-event outcome variables by several recent studies, including Jardim (2018); Jensenius (2015); Malmendier and Tate (2009)

pairs. This guarantees that my results are not driven by matches with large differences between control and treatment cells.

6.5 Causal inference

The nearest-neighbour matching estimator without replacement allows the derivation of valid standard error estimates that take into account the matching step. As recommended by Abadie and Spiess (2016), I use a non-parametric block bootstrap that re-samples matched-pairs of treatment and control cells. I produce 1,000 clustered bootstrap samples to produce confidence intervals for each point estimates I present in this analysis.

To further demonstrate that the effects I measure are unlikely to have been caused by chance, I supplement my main results with permutation-based inference. I start by randomly assigning a placebo treatment status in my sample, respecting cohort size in the original sample. I then go through all the different steps in my analyses from matching to aggregating the average treatment effects and estimate the placebo treatment effect. I repeat that procedure 1,000 times.

6.6 Estimating equation: Average treatment effect on the treated

After having matched students and bias-adjusted the outcome variables, I estimate the average treatment effect on the treated for each cohort, G_g, and each treated period, t , by estimating $\beta_{g,t}$ in the following equation:

$$\Delta^t y_{img,t} = \beta_{g,t}\mathbb{1}\{G_g = 1\} + \lambda_m + \epsilon_{img,t} \tag{7}$$

where λ_m are matched groups fixed effects, $\epsilon_{img,t}$ bootstrapped standard errors, $\mathbb{1}\{G_g = 1\}$ a dummy variable equal to 1 if the observation belongs to the cohort that enters the program at time g, 0 if it is in the control group. $\Delta^t y_{img,t} = y_{img,t} - y_{img,g-1}$ is the change in college outcomes between time $g-1$ and time t.

I must note that, as shown in section 2, high school baseline characteristics vary accross treatment cohorts: there is therefore no reason to expect that the effect of the treatment would be homogenous across cohorts. A booming body of literature has shown that when the treatment timing varies across units, a traditional two-way fixed effects estimator produces results which are difficult to interpret. Indeed, it is a weighted average of all possible pairwise DiD estimators in the data. The weights assigned to a specific estimator are proportional to group size and the variance of the treatment dummy and can be negative (de Chaisemartin and D'Haultfeuille, 2019; Goodman-Bacon, 2018). The two-way fixed effects estimator is particularly problematic in the presence of heterogeneous effects across cohorts or treatment length (Abraham and Sun, 2019; Borusyak and Jaravel, 2017; Callaway and Sant'Anna, 2019; de Chaisemartin and D'Haultfeuille, 2019).

To overcome that challenge, after having estimated each $\hat{\beta}_{g,t}$ separately, I aggregate them into a summary measure of the average treatment effect, explicitely specifying how each estimate is weighted as recommended in Callaway and Sant'Anna (2019). In what follows, I present three summary estimates, using different sets of weights that all account for different dynamics of the treatment.

I start with a *Simple Weighted Average* that average all $\hat{\beta}_{g,t}$ according to the amount of students in each cell. This Simple Weighted Average will give more weight to groups treated first and therefore overestimate the average effect of the treatment if high schools with the highest potential gains enter the affirmative action treatment first. To account for this possibility, I also account for a *Selective Treatment Timing* estimate that first averages $\hat{\beta}_{g,t}$ within each cohort and then aggregates those averages according to the size of each treatment cohort at baseline. Given the heterogeneity that prevails across high schools depending on the time at which they join the affirmative action program, this is my preferred estimate. Finally, the Simple Weighted Average will over-weight initial treatment periods. If the effect of the treatments grows over time, a Simple Weighted Average will underestimate it. I therefore also use a *Dynamic Treatment Effect* estimate that first averages $\hat{\beta}_{g,t}$ within each length of exposure, weighted by the size of each cohort and is then averaged over different lengths.

One potential threat to identification could be that an unobserved change motivated a school principal to sign an agreement with Sciences Po while separately affecting the students' higher education outcomes. Note that at $g-1$, the high school has already applied to be part of the affirmative action program but the program has not yet started. Therefore, this specification accounts for the unobserved factors that lead the school to enter the affirmative action program in addition to controlling for time-invariant unobservable high school characteristics.

After having controlled for time-invariant high school characteristics, the effect of the program can be identified under two identification assumptions. The first one is that of parallel trends in outcomes between students in the control and treatment group in the absence of the affirmative action program. The second is that comparable students attending high schools in which previous comparable students have faced common transitory shocks are equally likely to attend a high school which will join the affirmative action program. This requires the absence of an unobserved confounder that would have an impact on students' propensity to attend a high school which is part of the program and that are related to outcomes. I am particularly concerned that being part of the program increases a high school's attractiveness, changing the student body composition as a result and in particular increasing the proportion of high-SES students.

However, in table 5, I am able to rule out that high-SES students enroll disproportionately more in high schools that are part of the program. If anything, the proportion of low-SES students increases slightly after the introduction of the program, by 0.306pp., which is statistically significant but very small in comparison to the mean in the control group: in those high schools, 70.73% of students are low-SES. Therefore, the increase in the proportion of low-SES students after the introduction of the affirmative action treatment represents only 0.43% of the mean in the control group and is unlikely to drive my results. In addition, in Panel B, I do not find any evidence that the affirmative action program affected the gender composition of the treated high schools.

Table 5: Changes to high school composition

	(1)	(2)	(3)
	Panel A: Δy: Low-SES students (%)		
Selective Treatment Timing	0.934***	0.306**	0.500*
95% C.I.	[0.202 1.84]	[0.023 1.150]	[-00.04 1.74]
Mean y in control group	70.73	70.73	70.73
	Panel B: Δy: Female students (%)		
Selective Treatment Timing	-0.191	-0.084	0.181
95% C.I.	[-9.23 0.206]	[-2.53 0.204]	[-0.98 0.57]
Mean y in control group	57.19	57.19	57.19
Bias-adjusted	No	Yes	Yes
Control variables	No	No	Yes
N	5,626	5,626	5,626

Notes: This shows the average treatment effect on the treated of the affirmative action program on a high school composition. In Panel A, the outcome variable is the proportion of low-SES students in a high school, in Panel B, it is the proportion of female students in a high school. Column (1) shows the baseline estimates. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the local unemployment rate and proportion of immigrants in the municipality. Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. I present here a *Selective Treatment Timing* estimate, computed by averaging ATT per cohort first and then across cohorts. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

7 Results

7.1 Enrollment at Sciences Po

I begin my analysis by focusing on the affirmative action program's primary objective: increasing the enrollment at Sciences Po of students stemming from disadvantaged high schools.

Figure 13a presents the direct dynamic effects of the treatment on enrollment at Sciences Po, without controlling for any covariate. There is no change in enrollment at Sciences Po before the start of the program. The first year of the treatment, enrollment at Sciences Po increases little, by 0.33pp. The effect of the treatment gets larger in subsequent years and oscillates between 0.45pp. and 0.74 pp..

Table 6 reports the estimated impacts of the treatment on enrollment at Sciences Po, averaged with different sets of weights as described above. A *Simple Weighted Average* of all $\hat{\beta}_{g,t}$ finds that the affirmative action program increased enrollment at Sciences Po by 0.535 pp. The *Selective Treatment Timing* estimate accounts for the fact that the treatment timing was not randomly allocated across treated high schools and reduces the weight allocated to the first cohorts of treated high schools. This modestly reduces the size of the treatment effect to 0.492pp., which is not statistically different from the simple average estimate. Similarly, the *Dynamic Treatment Effect* estimate accounts for the fact that the impact of the treatment may vary depending on the length of exposure to the treatment and ensures that all treated periods are weighted equally. This modestly, and insignificantly, increases the size of the estimated treatment effect.

I also report bias-adjusted estimates in column (2). Bias-adjustment does not affect the magnitude of my estimates much, suggesting that the bias arising from differences in matching covariates across matches is very small. In column (3), I control for changes in baccalauréat grades, in high school composition and in economic conditions in the municipality: my estimated effects remain robust to the inclusion of those controls.

All specifications indicate that the affirmative action program has significantly increased the enrollment of students from treated high schools in Sciences Po by about 0.5pp. This is a large effect: on average, in the control group, only 0.115% of students enrolled at Sciences Po. The affirmative action program therefore quintupled the enrollment of students from treated high schools at Sciences Po.

My preferred specification is bias-adjusted and is the Selective Treatment Timing estimate as it accounts for the fact that high schools entering the treatment in different years may stand to gain differently from the treatment and weights each cohort's average treatment effect equally. In appendix E, I look at the profile of students who benefited the most from the affirmative action program. Panel A of table E.2 looks at the impact of the affirmative action program per gender. In my preferred specification, I estimate that the affirmative action program increased boys' enrollment at Sciences Po by 0.368pp. or 4.71 times boys' average enrollment in the control group. Girls have seen a larger increase in enrollment at Sciences Po: their enrollment has increased by 0.568 pp., which represents 4.11 times the value of that variable in the control group. The difference between boys and girls is not statistically significant.

Given that all students in the treated high schools are eligible to take part in the affirmative action program, a concern would be that the relatively better-off students would monopolise the program. Panel A of table E.4 shows the impact of the program on enrollment at Sciences Po separately according to the student's self-reported SES.

It does appear that, in absolute value, high-SES students have benefited from the program the most: for them, enrollment at Sciences Po increased by 0.975 pp., while the enrollment of low-SES students only increased by 0.315pp. The difference between both groups of students is statistically significant. In relative terms, the picture is a bit more nuanced. On average in the control group, only 0.033% of low-SES students enroll at Sciences Po. The affirmative action program therefore lead to an almost ten-fold increase in enrollment at Sciences Po for low-SES students.

Table 6: Δy: Students' enrollment at Sciences Po (%): treatment effect

	(1)	(2)	(3)
Simple Weighted Average	0.535***	0.513***	0.534***
95% C.I	[0.373 0.719]	[0.387 0.736]	[0.353 0.747]
Selective Treatment Timing	0.492***	0.462***	0.513***
95% C.I	[0.343 0.691]	[0.357 0.697]	[0.336 0.734]
Dynamic Treatment Effect	0.550***	0.531 ***	0.533***
95% C.I	[0.379 0.738]	[0.396 0.743]	[0.359 0.737]
Bias-adjusted	No	Yes	Yes
Control variables	No	No	Yes
Mean y in control group	0.115	0.115	0.115
N	5,626	5,626	5,626

Notes: This shows the average treatment effect on the treated of the affirmative action program on students' enrollment at Sciences Po. The outcome is the percentage of students within each cell who was enrolled at Sciences Po in the academic year following high school graduation. Column (1) shows the baseline estimates. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the average baccalauréat grade, in the proportion of low-SES female students in the high school and in the local unemployment rate and proportion of immigrants in the municipality. Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. *Simple Weighted Average*: averages all ATT, weighted by cohort size. *Selective Treatment Timing*: averages ATT per cohort first and then across cohorts. *Dynamic treatment effect*: averages ATT per length of exposure first and then across length. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

7.2 Spillover on enrollment at other selective colleges

The affirmative action treatment has been succesful in increasing the amount of students from partner high schools who enroll at Sciences Po. My model predicts that a purely mechanical response to the introduction of the affirmative action program would be a decrease in application and enrollment at other selective colleges. However, the affirmative action program could have an additional effect on the students' beliefs about their admission probability, encouraging non-high-achievers to apply and enroll more at selective colleges. In this subsection, I study whether the increase in enrollment at Sciences Po happened at the expense of enrollment at other selective colleges.

I start by verifying that the matching strategy has been successful in eliminating pretrends in figure 13b, which shows presents the dynamic effects of the treatment on enrollment at other selective colleges. Each dynamic treatment effect is very imprecisely estimated, but it appears that the treatment increased enrollment at selective colleges other than Sciences Po, the spillover effects get particularly strong three years after the start of the treatment.

The average impact of the affirmative action program on enrollment outside of Sciences Po is reported in table 7. Those results are surprising and suggest that the increase in enrollment at Sciences Po did not happen at the expense of enrollment at other colleges. In column (1), I present my summary estimates without adjusting for differences in matching covariates or controlling for other variables. This basic selective treatment timing estimate shows that the affirmative action program increased enrollment at other selective colleges by 1.09pp. This estimate is statistically significant at the 95% level. The estimate does not vary if I change the weights used to average all average treatment effects. In column (2), the outcome variable is bias-adjusted for imbalances in matching covariates. This has almost no effect on the estimated treatment effect. My favorite specification, the bias-adjusted selective treatment timing estimate, shows that the affirmative action program increased enrollment at selective colleges other than Sciences Po by 1.1pp. That effect is economically significant: it represents a 9% increase relative to the average value of the outcome variable in the control group. It is also twice the size of the direct effect of the affirmative action program.

In column (3), I control for observables. This slightly decreases the precision of my estimates, which are only statistically significant at the 10% level, but increases the magnitude of the selective treatment timing estimate. After controlling for observables, I find that the affirmative action treatment increased enrollment at selective colleges other than Sciences Po by 1.21 pp. or 10.5% of the mean value of enrollment in the control group.

Which group experiences the largest spillover effects? In Panel B of table E.2, I study whether the magnitude of the spillover effects differ depending on students' gender. Even though the differences between males and females are not statistically significant, it appears that male students experience the largest increase in enrollment at colleges other than Sciences Po. The bias-adjusted estimates indicate that the enrollment of male students at other selective colleges increase by 1.71pp., while the enrollment of female students increase by a meagre 0.29pp.. In addition to social differences in college enrollment, there are large gender differences: in the control group, 17.13% of boys enroll at a selective college, while 8.1% of girls do. Therefore, a boy in low-SES schools is more likely to be the marginal student whose college enrollment reacts to the affirmative action program. In addition, note that there is a large gender imbalance in academic high school. On average, 57% of students in control high schools are female. This indicates that low-SES boys are more likely to be discouraged from pursuing education in academic high schools early on. Therefore the boys who are enrolled in the last year of an academic high school may be more likely to be motivated, on average, to pursue an higher education in a selective college.

In Panel B of table E.4, I show that the spillover effects of the affirmative action program seems to be twice as large for low-SES students than high-SES students, even though the difference between low and high-SES students is not statistically significant. The effect of the treatment on enrollment at selective colleges other than Sciences Po for high-SES students is 0.686 pp., this represents 3.5% of the control group mean. For low-

SES students, the treatment lead to a 1.492 increase in enrollment at selective colleges: that effect is large in magnitude as it represents 16% of the control group mean.

In table E.1, I separate the average treatment effects depending on the year in which the high school joined the affirmative action program. The treatment had heterogenous effects across high schools. High schools that joined the affirmative action program earlier seem to have benefited more from the treatment than late-joiners. However, in spite of large heterogeneity in terms of treatment spillovers, no clear pattern emerges in terms of treatment timing. The group of high schools that joined the affirmative action program in 2014 has experienced the largest spillover effects. Those high schools are relatively better-off: they have a lower proportion of low-SES students and are located in areas with lower unemployment rates. However, high schools which joined in 2012 have the second largest spillovers in spite of being located in more underpriviledged areas.

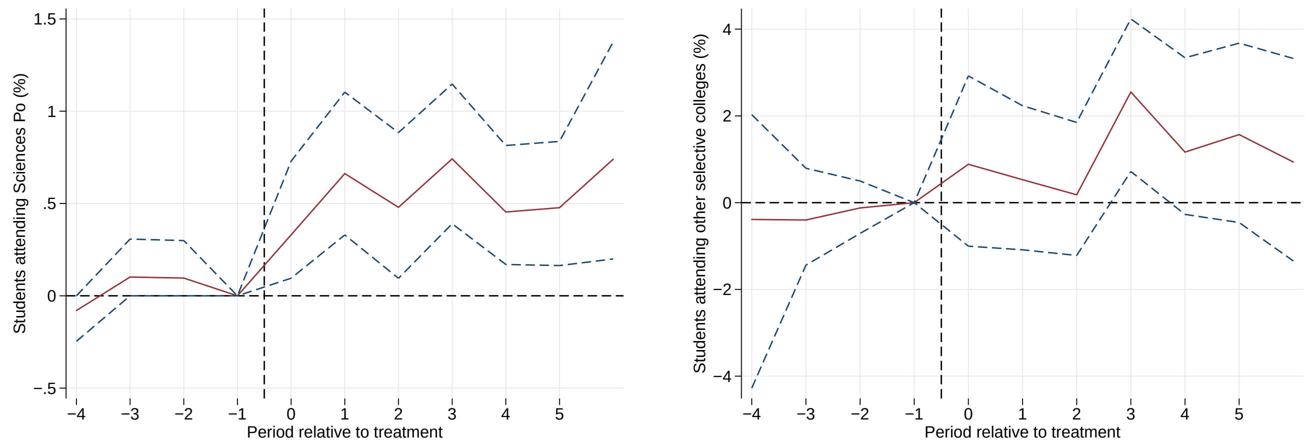
Table 7: Δy:Students' enrollment at other selective colleges (%): treatment effect

	(1)	(2)	(3)
Simple Weighted Average	1.09**	1.05**	0.99*
95% C.I	[0.195 1.93]	[0.18 2.001]	[-0.12 2.00]
Selective timing	1.09**	1.10**	1.21*
95% C.I	[0.134 2.001]	[0.191 2.05]	[-0.07 2.22]
Dynamic	1.12**	1.07**	0.95*
95% C.I	[0.234 2.012]	[0.182 2.004]	[-0.10 2.07]
Bias-adjusted	No	Yes	Yes
Control variables	No	No	Yes
Mean y in control group	11.56	11.56	11.56
N	5,626	5,626	5,626

Notes: This shows the average treatment effect on the treated of the affirmative action program on students' enrollment at selective colleges other than Sciences Po. The outcome is the percentage of students within each cell who was enrolled at Sciences Po in the academic year following high school graduation. Column (1) shows the baseline estimates. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the proportion of low-SES and of female students in the high school and in the local unemployment rate and proportion of immigrants in the municipality. Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. *Simple Weighted Average*: averages all ATT, weighted by cohort size. *Selective Treatment Timing*: averages ATT per cohort first and then across cohorts. *Dynamic treatment effect*: averages ATT per length of exposure first and then across length. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

Figure 13: Semi-parametric estimates: dynamic effects of the treatment

(a) Enrollment at Sciences Po (%) (b) Enrollment at selective colleges other than Sciences Po (%)



Notes: This figure plots the raw estimates of the average treatment effect on the treated of the affirmative action treatment program on students' higher education outcomes. In figure 13a the outcome is the percentage of students within each cell who was enrolled at Sciences Po in the academic year following high school graduation and in figure 13b, the outcome variable is the percentage of students within each each who was enrolled at a selective college other than Sciences Po in the academic year following high school graduation. Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. The red line represents the estimates of the treatment effect. Those estimates are computed by first estimating the average treatment effect for each cohort and each length of exposure to the treatment and then averaging them over length of exposure, weighted by the size of each treatment cohort. The dashed lines represent the 90% confidence intervals. Time (in year) relative to the year of the start of the affirmative action program runs on the horizontal axis. The confidence intervals are computed using block bootstrapped standard errors.

7.3 Robustness of the enrollment effects

In figure B.1a, I plot the distribution of the treatment effect on enrollment in Sciences Po, estimated after varying the set of matching variables. There is little dispersion among those estimates. In figure B.1b, I show that my estimates of the spillover effects are robust to matching on different sets of variables. The majority of those regressions estimate an increase in enrollment at other selective colleges after the introduction of the affirmative action treatment ranging between 0.8 and 1.2pp.. The most conservative specification reports that the affirmative action program increased enrollment at other selective colleges by 0.6pp., which remains as large as its impact on enrollment at Sciences Po. This lower bound arises when I do not match high schools on the baseline proportion of low-SES students, and may be driven by the differences in student body composition between treated and untreated high schools.

In my main specification, my sample is restricted to students who had obtained a passing grade in the baccalauréat. This may lead to an underestimation of the treatment effect if, as a result of lower effort, some students in treated schools would have received a lower grade in the absence of the affirmative action treatment. In addition, I use cells based on academic and demographic characteristics, whose composition may be affected by the treatment. In appendix D, I estimate how sensitive my main results are to those choices. I first extend my sample to include students who received a grade lower than 10 in the baccalauréat. Second, I create student cells based on students' demographic characteristics only. This decreases the precision of my bias-adjusted estimates but the point estimates are the same as in my main specification.

In addition, in columns (5)-(7) of table E.1, I extend my analysis to high schools that entered the program after 2014. I exclude them from my main analysis as it makes little sense to run a regression when only one high school entering the treatment in each of those cohort year. In column (7), I reproduce my main analysis with the full sample of treated high schools. My estimated treatment effects on enrollment at Sciences Po and at other selective colleges are robust to this extension, but lose in precision.

In a last robustness check, I execute a permutation test in which I measure the effects of 1,000 placebo treatments. For each placebo treatment, I start by randomly assigning a placebo treatment status in the full sample, respecting the original treatment cohort size. I then go through all the different steps in my analyses from matching to aggregating the average treatment effects and estimate the treatment effect of the placebo treatment. The distribution of those placebo treatment effects are displayed in figure 14. For my preferred specification, the bias-adjusted *Selective Treatment Timing* estimate, the effect of the "true" treatment on enrollment at Sciences Po is larger than that of 99.9% of the placebo treatments. The spillover effects of the "true" treatment is larger than the effects of 94% of the placebo treatments. The effects of the "true" treatment I measure are therefore unlikely to be due to chance.

7.4 Persistence

A common concern of the affirmative action literature is that affirmative action programs may lead to "over-matching" students to schools that are above their academic ability.[25] In table 8, I show that students remain more likely to be enrolled at a selective college several years after graduating from high school. In column (1), I reproduce my estimate of the treatment on enrollment at Sciences Po and other selective colleges immediately after high school for cohorts that I can observe up to five years after graduation.

In column (1), I show that the treatment effects on enrollment at Sciences Po and other selective colleges immediately after high school are similar in this subsample to the effects in the full sample. In terms of enrollment at Sciences Po, the treatment effects are 0.522pp. in this subsample and 0.462pp. in the full sample, and in terms of enrollment at other selective colleges 1.53 and 1.10pp. respectively. In column (2), I report the effect of the affirmative action program on enrollment in the third year of higher education. This is a pivotal year for students enrolled at a preparatory school as it is the year in which they take the Grandes Ecoles entrance exam. Students who fail those exams can stay in a

[25] See Arcidiacono and Spenner (2011) for a review of the mismatch literature.

Table 8: Persisting effects of the treatment: enrollment at selective colleges several years after high school graduation

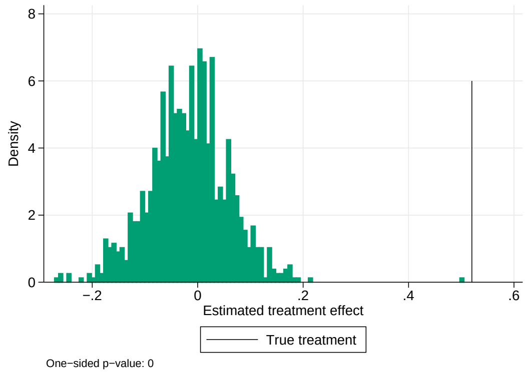
	(1)	(2)	(3)
	Panel A: Δy: Enrolled at a Sciences Po X years after high school (%)		
	1st year	3rd year	5th year
Selective Treatment Timing	0.522 ***	0.427***	0.458***
95% C.I	[-0.546 0.286]	[0.258 0.604]	[0.146 0.849]
Mean y in control group	0.023	0.121	0.089
N	2,685	2,685	2,685
	Panel B: Δy: Enrolled at another selective college X years after high school (%)		
	1st year	3rd year	5th year
Selective Treatment Timing	1.153**	1.214	1.701
95% C.I	[0.131 2.616]	[-0.642 3.061]	[-0.48 4.23]
Mean y in control group	10.64	8.93	9.00
Bias-adjusted	Yes	Yes	Yes
Control variables	No	No	No
N	2,685	2,685	2,685

Notes: This shows the average treatment effect on the treated of the affirmative action program on students' enrollment Sciences Po and other selective colleges one, three and five years after high school. All those estimates are bias-adjusted and are computed accounting for Selective Treatment Timing. The sample is restricted to treatment groups that I can observe up to 5 years after high school to guarantee that the composition of the sample does not change. Those are high schools that enter the treatment between 2010 and 2012. Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

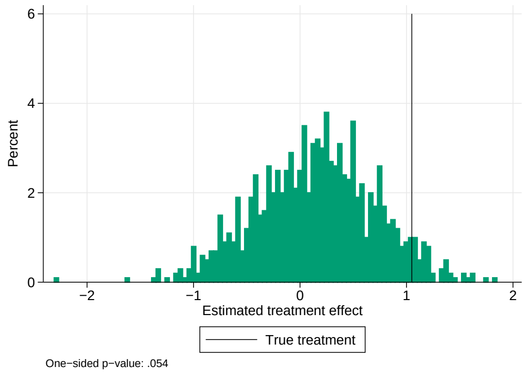
preparatory school for an extra year or drop out and join the university system. As a result, the proportion of students enrolled at a selective college three years after high school drops slightly overall. However, the magnitude of the estimated treatment effects on enrollment at Sciences Po and other selective colleges remain stable, even though the spillover effects are no longer statistically significant. In column (3), I report the estimated effects of the treatment on enrollment in the fifth year of higher education, which would typically be the last year before graduation from Sciences Po and other Grandes Ecoles. I do find that the magnitude of the estimated coefficients remain the same. I do not therefore find any evidence that the affirmative action treatment lead to students to over-match.

Figure 14: Permutation test: treatment re-assignment

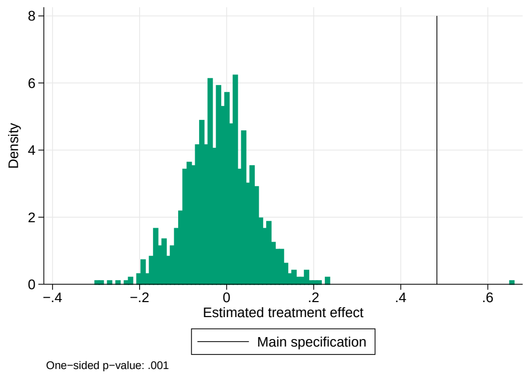
(a) Impact of the treatment on enrollment at Sciences Po (%)
Simple Weighted Average



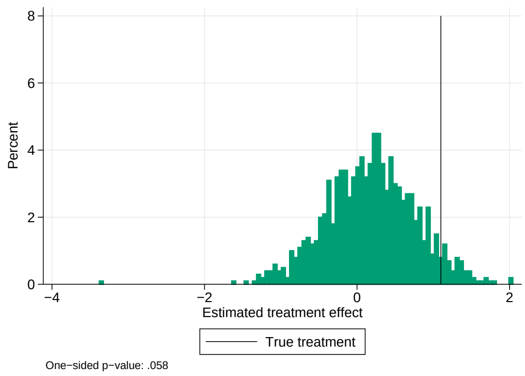
(b) Impact of the treatment on enrollment at other selective colleges (%)
Simple Weighted Average



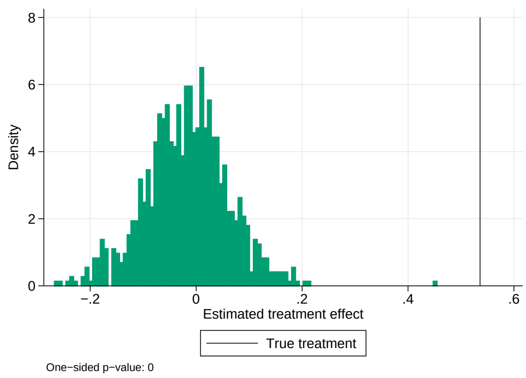
(c) Impact of the treatment on enrollment at Sciences Po (%)
Selective Treatment Timing



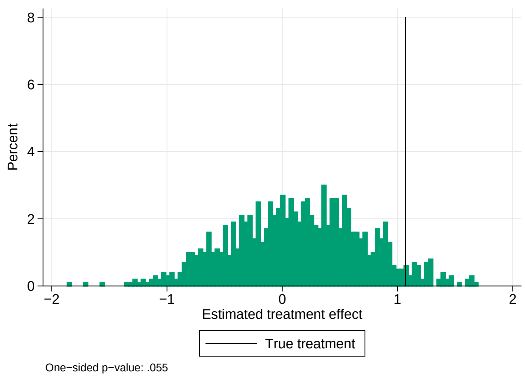
(d) Impact of the treatment on enrollment at other selective colleges (%)
Selective Treatment Timing



(e) Impact of the treatment on enrollment at Sciences Po (%)
Dynamic Treatment Effect



(f) Impact of the treatment on enrollment at other selective colleges (%)
Dynamic Treatment Effect



Notes: Those figures plot the distribution of the average treatment effect estimated after "randomly" reassigning treatment status, respecting the sample size accross cohorts in the original sample. I then go through all the different steps in my analyses from matching to aggregating the average treatment effects and estimate the placebo treatment effect. The estimates presented here are bias-adjusted for the matching step. I repeat that procedure 1,000 times. Average treatment effects are first computed separately for each treated cohort and each treated period. *Simple Weighted Average*: averages all ATT, weighted by cohort size. *Selective Treatment Timing*: averages ATT per cohort first and then across cohorts. *Dynamic treatment effect*: averages ATT per length of exposure first and then across length. The dotted line shows the estimated effect of the true treatment.

7.5 Applications and admission probability

The positive impact of the treatment on enrollment at selective colleges can be driven by three potential channels: 1) a student in a treated high school is more likely to apply to a selective college, 2) conditional on applying, a student in a treated high school is more likely to receive an offer from a selective college, 3) conditional on receiving an offer, a student in a treated high school is now more likely to enroll at a selective college.

I start by looking at the impact of the treatment on applications to selective colleges, presented in table 9. Note that, given that I have information on applications for a smaller number of years, those effects are only identified for high schools that entered the treatment in 2012 and 2013.

In Panel A is the probability that a student applied to at least one selective college. In the control group, this represents 32.36% of students. On that margin, the estimated treatment effect is very large: the unadjusted "Selective Treatment Timing" estimate is that the affirmative action program increased the probability of a student applying to a selective college other than Sciences Po by 9.8pp.. This represents a 30% increase as a proportion of the mean value of the outcome variable in the control group. This estimate is sensitive to controlling for observables though. Bias-adjustement increases the estimated effect size to 13.8pp., while controlling for local labor market conditions, baccalauréat grades and student body composition brings it down to 9.03pp..

I now turn to the extensive margin in Panel B. The average student in the control group sends applications to 1.69 selective colleges. My Selective Treatment Timing estimates, without bias-adjustment, suggest that students send 0.337 additional applications to selective colleges after their high school gets treated, but this estimate is not statistically significant. Bias-adjustment brings the impact of the treatment on the number of applications to selective colleges to a statistically significant 0.452 additional applications to selective colleges, which is more than 20% of the average value of that variable in the control group. This estimate is robust to using different weights and controlling for observables.

To provide further evidence that the treatment's impact on enrollment is driven by its effect on applications, I look at whether the same subgroups of students display the strongest increases in applications and enrollment in appendix E. Panel C of table E.2 shows that the impact of the treatment on the number of applications sent to selective colleges is also concentrated among male students. In my favorite specification, in column (2), I find that the affirmative action treatment increases the number of applications sent by male students by 1.24, which is a 50% increase with respect to the control group mean. Among females, the affirmative action treatment increased the number of applications by 0.086, which is not economically meaningful. The difference between genders is statistically significant and fits the patterns observed for the response of enrollment to selective colleges.[26]

[26] In Panel C of table E.4, I study whether the impact of the treatment on applications varies depending on

In figure B.1c, I show the distribution of the estimated treatment effect on the number of applications when varying the set of matching variables. Again, the estimated coefficient remains quite robust to changes in the matching procedure. The estimated coefficient varies quite a lot with the set of matching variables— an imprecision driven by the small sample size— but all specifications suggest that students apply to more selective colleges after their high school enter the affirmative action program.

Finally, in table E.1, I look at whether the impact of the treatment on the number of applications sent to selective colleges vary depending on the year in which schools enter the treatment. I find that the impact of the treatment varies quite a lot across cohorts, and is the largest among late-comers. Unsurprinsigly, I find that cohorts that have the largest increase in applications to selective colleges are the same cohorts that have the largest spillover effects. Including the cohorts that get treated last doubles the size of the estimated treatment effect, but it becomes noisier and statistically insignificant.

My data does not allow me to observe whether students are more likely to enroll at a selective college after receiving an offer, so I have to test channels 2 and 3 together: conditional on applying, are students more likely to enroll at a selective college? Of course, the composition of students applying to selective colleges has changed, as I have discussed above. The overall impact of the affirmative action treatment on students' probability of admission would therefore reflect both changes in college preparedness of applicants and changes in the process of selecting into applying. Panels C and D of table 9 show the estimated impact of the treatment on the probability that a student enrolls at the college she ranked first. For simplicity, I only report the Selective Treatment Timing average. In Panel C, this probability is computed for all students and in Panel D for students who ranked a selective college as their first application.

Overall, the affirmative action treatment did not increase the probability that a student will enroll at her first choice, when all applications are pooled. The estimated treatment effect is very close to zero in magnitude and statistically insignificant, even when bias-adjusting or controlling for observables.

However, the estimated treatment impact on the conditional probability of admission at the college ranked first, when the college ranked first is a selective college, is larger. The baseline specification, unadjusted for bias, estimate that the affirmative action program increased the conditional probability that a student enrolls at a selective college by 16.4pp. However, this estimate is statistically insignificant and very sensitive to controlling for observables. Bias-adjustment suggests that the affirmative action program increased the probability of admission at a selective college by 0.231pp., which remains statistically insignificant. However, controlling for changes in labor market conditions or high-school level variables divides the estimated treatment effect by four. The estimate in column (3) suggests that the conditional probability of enrolling at a selective college increased by 5.2 pp. This increase represents 12.5% of the mean in the control group. Note that all those

the students' SES, but differences between SES are not statistically significant and they vary qualitatively across specifications.

estimated coefficients are insignificant and imprecisely estimated.

Taken together—with a grain of salt due to their imprecision—those estimates do not lend support to the hypothesis that the affirmative action program moved poorly prepared students to apply to selective colleges more. If anything, the affirmative action program may have moved better-prepared students to apply more to selective colleges or encouraged them to put in more effort in their applications.

Table 9: Applications to selective colleges : treatment effect

	(1)	(2)	(3)
	Panel A: Δy: Applied to a selective college (%)		
Simple Weighted Average	10.4***	14.8***	9.03***
	[7.2 17.9]	[8.8 22.1]	[2.209 16.1]
Selective timing	9.8***	13.8 ***	9.03 ***
	[7.2 18.1]	[8.9 22.3]	[2.210 16.2]
Dynamic	11.1***	14.7***	8.52***
	[5.0 18.0]	[7.5 20.9]	[1.912 16.32]
Mean y in control group	32.36	32.36	32.36
N	3,428	3,428	3,428

Panel B: Δy: Number of applications to selective colleges

Simple Weighted Average	0.377	0.452***	0.409*
	[-0.241 0.402]	[0.091 0.930]	[-0.058 1.304]
Selective timing	0.337	0.452**	0.409*
	[-0.205 0.420]	[0.091 0.930]	[-0.058 1.304]
Dynamic	0.451	0.542**	0.504 *
	[-0.132 0.453]	[0.090 0.965]	[-0.058 1.304]
Mean y in control group	1.69	1.69	1.69
N	3,428	3,428	3,428

Panel C: Δy: Probability of enrolling at first wish, all colleges

Selective timing	-0.020	0.019	0.002
95% C.I.	[-0.169 0.143]	[-0.166 0.248]	[-0.228 0.237]
Mean y in control group	0.267	0.267	0.267
N	366	366	366

Panel D: Δy: Probability of enrolling at first wish, selective college

Selective timing	0.164	0.231	0.052
	[0-.127 0.521]	[-0.100 0.513]	[-1.825 1.104]
Mean y in control group	0.416	0.416	0.416
Bias-adjusted	No	Yes	Yes
Control variables	No	No	Yes
N	236	236	236

Notes: This table presents the estimated effect of being enrolled at a high school taking part in the affirmative action program on students' applications to selective colleges. In Panel A, the outcome variable is a dummy equal to 1 if the student applied to at least one selective college, 0 otherwise. In Panel B, the outcome variable is the number of applications to selective colleges. In Panel C, the outcome variable is the probability of enrolling at the college the student had ranked first in the academic year following high school graduation. In Panel D, the outcome variable is the probability of enrolling at the college the student had ranked first in the academic year following high school graduation, when the college ranked first is a selective college. Column (1) shows the baseline estimates. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the local unemployment rate and proportion of immigrants in the municipality. Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. I present here a *Selective Treatment Timing* estimate, computed by averaging ATT per cohort first and then across cohorts.

The application data is only available for students graduating high schools in 2012 and between 2014 and 2017. The enrollment data covers schools from 2009 to 2016. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

7.6 Channels

According to the baseline model of section 4, the introduction of the affirmative action program should lead to a *reduction* in the number of applications to selective colleges other than Sciences Po. Two assumptions are required for this prediction to hold: that students had correct information about their academic ability and the world of higher education, and the absence of fixed costs (or decreasing marginal costs) in college applications. The violation of one of those two assumptions can explain the positive spillover effects of Sciences Po's program on applications to other selective colleges.

First, the affirmative action program may have provided students with additional information about selective colleges. In particular, students may have initially held incorrect beliefs over their academic ability, leading them to under-estimate the probability that an application to a selective college is succesful. The affirmative action program could have corrected low-SES students' beliefs upward through exposure to an elite college's efforts to recruit students of a similar background. Those beliefs could also increase through peer effects: students get more exposed to similar students from previous cohorts enrolling to elite colleges. This is consistent with the path of spillovers displayed in figure 13b: the spillover effects get really large three years after the start of the treatment. The belief channel is in line with the literature documenting that the social gap in application behavior could be driven by differences in beliefs over returns to college attendance (Boneva and Rauh, 2017).

The model makes an additional critical prediction: in addition to the negative mechanical effect of the program on applications to other selective colleges, increasing students' beliefs about their academic achievement should have an additional negative effect on high-achievers' applications to selective colleges and a positive effect on non-high-achievers' applications. I test whether the impact of the affirmative action program has different effects depending on the students' level of academic achievement.

Table 10 reports the effect of the treatment on my main outcome variables of interest, disaggregated per academic achievement. All the specifications in table 10 report the "Selective Treatment Timing" bias-adjusted treatment effects. In order not to lose too much power, I divide students in my sample between "Non-high-achievers", who passed the Baccalauréat but obtained an average grade lower than 14/20 and "High-achievers" who passed the Baccalauréat with a grade larger that 14/20.

Panel A shows that the direct effect of the treatment is the strongest for high-achievers. The probability that they enroll at Sciences Po increases by almost 2pp. after the affirmative action treatment starts. In the control group, only 0.657% of high-achievers enroll at Sciences Po, so the probability that a high-achieving student enrolls at Sciences Po quadruples. In absolute terms, the increase is more modest for non-high-achievers: the probability that they enroll at Sciences Po only increases by 0.295 pp., but only 0.006% of their peers in the control group enroll at Sciences Po. As a proportion of the control mean, the increase in the probability of enrollment at Sciences Po increased a lot for non-high-achievers as well. The estimated direct effects of the treatment are statistically significant for both high and non-high-achievers.

In line with the model's prediction, I find in Panel B that the positive spillover effects of the treatment on higher education outcomes is concentrated among non-high-achievers. Their probability of enrolling at a selective college increases by a statistically significant 1.325pp., which represents a 18.5% increase with respect to the control group mean. The spillover effects for high-achievers are, however, negative and non-significant. The estimate suggests that the probability that a high-achieving students enrolls at a selective college decreases by 3pp., or 9% of the control mean. Given that high-achievers's probability of enrolling at Sciences Po increased by 2pp. in response to the affirmative action treatment, this suggests that the affirmative action program shifted high-achieving students from other selective colleges to Sciences Po, but had no economically or statistically significant impact on overall enrollment at selective colleges (minus 1pp., 3% of the control mean).

Finally, in Panel C, I show that the impact of the treatment on applications is also concentrated among non-high-achievers. On average, they submit 0.689 additional applications to selective colleges after the introduction of the program. In the control group, non-high-achievers only send 1.07 applications to selective colleges on average. Therefore, the affirmative action program increased the number of applications sent by non-high-achievers by more than 64%. However, it had a negative impact on high-achievers. The affirmative action treatment leads to 0.892 fewer applications being sent by high-achievers, which represents about 20% of the mean of their peers in the control group. Note, however, that this estimated treatment effect is statistically insignificant.[27]

[27] In table E.3, I check that the heterogenous effects of the treatment per level of academic achievement are not a mere artefact of gender heterogeneity. None of the interaction terms are precisely estimated, but those results suggest that the spillover effects of the treatment on enrollment are the strongest for non-high-achieving boys. The spillover effects on applications is the strongest for non-high-achieving boys followed by non-high-achieving girls.

Table 10: Heterogenous effects of the treatment, per academic achievement

	(1)	(2)
	Panel A: Δy: Enrollment at Sciences Po (%)	
	Non-high-achievers	High-achievers
Selective Treatment Timing	0.295***	1.999***
95% C.I	[0.188 0.430]	[0.820 3.014]
(Weighted) Mean y in control group	0.006%	0.657%
N	4,252	1,374
	Panel B: Δy: Enrollment at other selective colleges (%)	
	Non-high-achievers	High-achievers
Selective Treatment Timing	1.325***	-3.002
95% C.I	[0.496 2.350]	[-5.035 2.851]
(Weighted) Mean y in control	7.16%	33.41%
N	4,252	1,374
	Panel C: Δy: Number of applications to selective colleges	
	Non-high-achievers	High-achievers
Selective Treatment Timing	0.689 ***	-0.892
95% C.I	[0.37 1.26]	[-3.95 2.94]
(Weighted) Mean y in control	1.07	4.54
Bias-adjusted	Yes	Yes
Control variables	No	No
N	2,602	826

Notes: This table presents the effect of taking part in the affirmative action program on students' enrollment at Sciences Po (in Panel A), at other selective colleges (in Panel B) and on the number of applications to selective colleges (in Panel C). In column (1), the sample is restricted to students having achieved a grade ranging between 10 and 14 out of 20 at the Baccalauréat et in column (2) to students having achieved a grade higher than 14.

All those estimates are bias-adjusted and are computed accounting for Selective Treatment Timing.

Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

An alternative explanation for those spillover effects is that there could be fixed costs in applications to selective colleges: once the student has applied to Sciences Po, it gets easier or cheaper to apply to other selective colleges. This is unlikely to drive those results.

However, applications to Sciences Po are done through a separate system than applications to other selective colleges. Applications to other selective colleges are done through a unique online platform and the cost of an application varies depending on the type of selective college the student is applying to. If the student is applying to a preparatory school, the cost of one application is very low from the student's perspective: for most of them, she only needs to provide her high school grades and her teacher's comments, both of which are filled in online by the high school. Having applied to Sciences Po does not affect this application cost for the student. In addition, other "Grandes Ecoles" recruit students based on a series of competitive exams, often written and oral. Those entrance exams take place around the same time as Sciences Po, sometimes in other parts of France. They also select students on different skills than the press review that is required by the affirmative action program. One could argue that, from the student's perspective, there is a direct competition between the time required to prepare for Sciences Po and to prepare for those other entrance exams.

The heterogenous effects of the treatment per academic achievement are therefore consistent with the hypothesis that the affirmative action treatment increased students' beliefs in their academic achievement, encouraging non-high-achievers to apply to more selective colleges.[28] Note that "non-high-achievers" in this study are students enrolled at an academic high school— which represents about 38% of an age cohort— and who passed the Baccalauréat, all of them are eligible to attend college. Therefore, even "non-high-achievers" are positively selected from the population and ensuring that they are matched to a college that fits their academic ability should be of interest to policy-makers.

8 Conclusion

Persistent socio-economic gaps in higher education hamper intergenerational mobility in higher income countries. A large part of that gap is driven by the fact that low-income students do not apply to selective colleges at the same rate as high-income students. Affirmative action is a popular, and controversial, policy to promote the enrollment of students from under-represented groups in better colleges. In spite of the active debate surrounding its effectiveness, the literature has not focused on its spillover effects on under-represented groups' applications to selective colleges without affirmative action.

In this paper, I study the spillover effects of a large and visible affirmative action program targeting students in low-income schools in France. I find that for each low-

[28] The affirmative action program may have simply provided visibility to selective colleges and Grandes Ecoles. In other words, low-income students may have initially be simply unaware of the existence of selective colleges and the affirmative action expanded their choice set. However, if the affirmative action program had simply provided information about the existence of selective colleges to all low-SES students, there is no reason why the spillover effects should not be uniform accross all levels of academic achievement.

income student who enrolls at the sponsoring institution because of the affirmative action program, two additional students from the targeted schools enroll at selective colleges with no affirmative action policy. Those spillovers are driven by the fact that low-income students apply more to selective colleges, rather than by a change in the recruitment policies of other selective colleges. My findings contribute to an active literature that shows that, even though large socio-economic gaps persist in higher education, it is possible to bring the application behavior of low-SES students closer to that of their high-SES peers (Dynarski et al., 2018; Hoxby and Turner, 2013).

I provide evidence that it is consistent with the interpretation that the affirmative action program corrected students' beliefs about their academic achievement upward. Indeed, the spillover effects of the affirmative action treatment are concentrated among non-high-achievers, which is the subgroup for which the largest effects would be predicted in that scenario.

My findings have implications for the design of policies aiming to boost low-SES students' enrollment at selective colleges. The large spillover effects are particularly striking given that the absolute number of students that directly benefit from the affirmative action program by enrolling at the sponsoring college is low. Sciences Po's program stands out in that it was very visible and publicized within treated high schools and all students, irrespective of their academic achievement, were allowed to enroll in the workshops to prepare for its admission procedure. This sends a strong signal to students in partner schools with little prior exposure to elite colleges that students like them are sought after. Indeed, students are particularly reactive to outreach actions by elite institutions in their high schools (see Cortes and Klasik (2019)). In addition, to substantially narrow the social gap in college applications and enrollment, expanding the focus of college opportunity expansion policies beyond a small set of high-achieving students may be warranted.

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Appendices

A Social gap in selective colleges enrollment.

Table A.1: Social differences in selective college enrollment

	(1)	(2)	(3)	(4)
	Enrolled at a selective college (%)			
Low-SES school	-11.52***	-5.198***	-4.790***	-4.668***
	(0.642)	(0.490)	(0.447)	(0.446)
Baccalauréat grade		8.063***	7.746***	7.822***
		(0.118)	(0.113)	(0.114)
Female			-10.40***	-10.35***
			(0.215)	(0.214)
Foreigner			-4.303***	-3.962***
			(0.893)	(0.890)
High school major fixed effect	No	No	Yes	Yes
Year fixed effect	No	No	No	Yes
Mean	19.99	19.99	19.99	19.99
N	104028	104028	104028	104028

Notes: OLS regression estimates of the percentage of students graduating from a given high school that enroll at a selective college the year after graduation. $Low - SESschool$ is a dummy variable equal to one if the student is graduating from a high school which has a proportion of low-SES students at the highest quartile of the school distribution and 0 if the high school is at the lowest quartile of the school distribution. Standard errors clustered at the high school level in parenthesis. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité. 2012, 2014-2016. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table A.2: Social differences in college enrollment among students who applied to at least one selective college

	(1)	(2)	(3)	(4)
	Enrolled at a selective college (%)			
Low-SES school	-7.988***	0.0989	-0.154	-0.0638
	(0.764)	(0.615)	(0.614)	(0.614)
Baccalauréat grade		7.373***	7.409***	7.478***
		(0.125)	(0.123)	(0.123)
Female			-8.752***	-8.694***
			(0.349)	(0.348)
Foreigner			-7.039***	-6.809***
			(1.546)	(1.539)
High school major fixed effect	No	No	Yes	Yes
Year fixed effect	No	No	No	Yes
Mean	43.49	43.49	43.49	43.49
N	66841	59659	59659	59659

Notes: OLS regression estimates of the percentage of students graduating from a given high school that enroll at a selective college the year after graduation. The sample is restricted to students who have applied to at least one selective college. $Low - SESschool$ is a dummy variable equal to one if the student is graduating from a high school which has a proportion of low-SES students at the highest quartile of the school distribution and 0 if the high school is at the lowest quartile of the school distribution. Standard errors clustered at the high school level in parenthesis. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité. 2012, 2014-2016. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table A.3: Social differences in the probability of applying to a selective college

	(1)	(2)	(3)	(4)
	Applied to at least one selective college (%)			
Low-SES school	-17.28***	-10.16***	-9.543***	-9.414***
	(0.956)	(0.825)	(0.750)	(0.750)
Baccalauréat grade		8.972***	8.275***	8.360***
		(0.104)	(0.0942)	(0.0943)
Female			-12.65***	-12.58***
			(0.241)	(0.240)
Foreigner			-2.945***	-2.555**
			(1.082)	(1.081)
High school major fixed effect	No	No	Yes	Yes
Year fixed effect	No	No	No	Yes
Mean	38.49	38.49	38.49	38.49
N	104028	104028	104028	104028

Notes: OLS regression estimates of the percentage of students graduating from a given high school that apply to at least one selective college. $Low - SESschool$ is a dummy variable equal to one if the student is graduating from a high school which has a proportion of low-SES students at the highest quartile of the school distribution and 0 if the high school is at the lowest quartile of the school distribution. Standard errors clustered at the high school level in parenthesis. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table A.4: Persistence in social differences in selective college enrollment

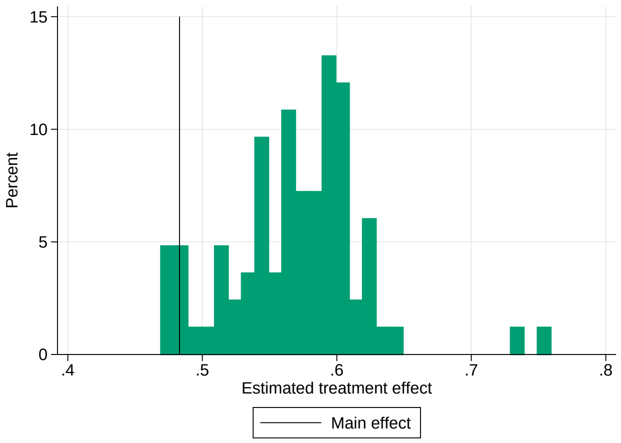
	Enrolled at a selective college X years after high school (%)				
	1st year	2nd year	3rd year	4th year	5th year
Low-SES schools	-3.162***	-3.637***	-3.848***	-4.362***	-4.430***
	(0.353)	(0.327)	(0.282)	(0.289)	(0.293)
Test scores	5.666***	5.266***	4.301***	4.156***	4.056***
	(0.0985)	(0.100)	(0.0844)	(0.0815)	(0.0756)
Mean	15.77%	14.28%	12.58%	12.53%	12.57%
N	187646	187646	187646	187646	187646

Notes: OLS regression estimates of the percentage of students graduating from a given high school that are enrolled at a selective college up to five years after graduation. $Low - SESschool$ is a dummy variable equal to one if the student is graduating from a high school which has a proportion of low-SES students at the highest quartile of the school distribution and 0 if the high school is at the lowest quartile of the school distribution. Standard errors clustered at the high school level in parenthesis. *Sources:* MESRI-SIES: APB'Stat, Bases SISE. MEN-DEPP: Océan, Base Scolarité. 2009-2013. * $p < 0.1$, ** $p < .05$, *** $p < .01$

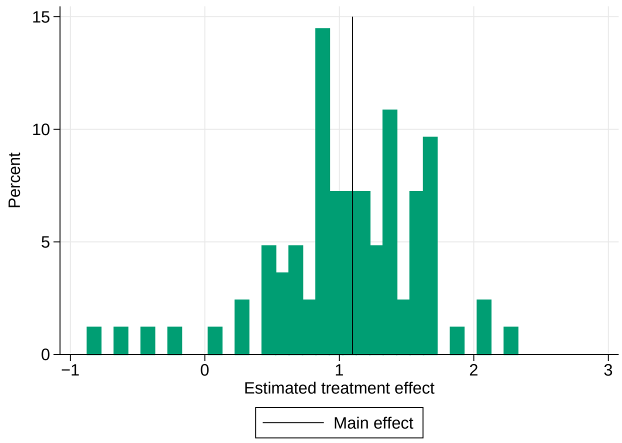
B Robustness check: varying the set of matching variables

Figure B.1: Robustness check: varying the set of matching variables

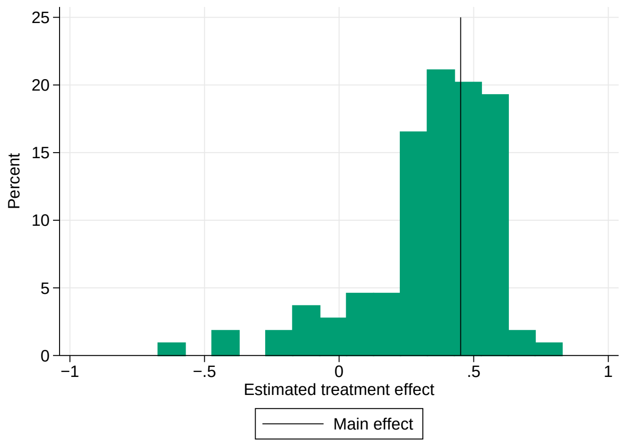
(a) Impact of the treatment on enrollment at Sciences Po (%)



(b) Impact of the treatment on enrollment at other selective colleges (%)



(c) Impact of the treatment on the number of applications

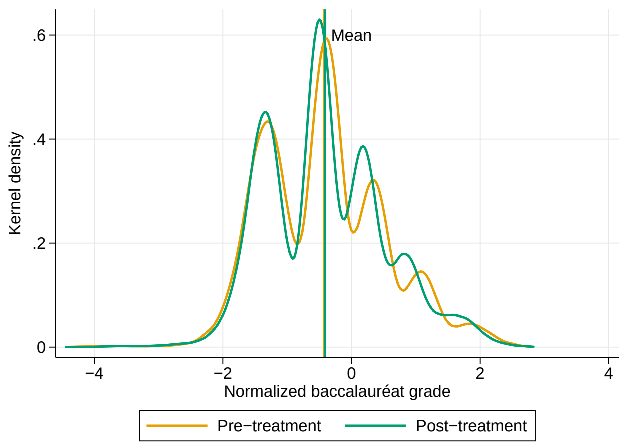


Notes: Those figures plot the distribution of the average treatment effect estimated after varying the set of matching variables in 85 different regressions. Each data point above is the estimated "Selective Treatment Timing" treatment effect, bias-adjusted, computed after matching student cells on a set of variables that excludes or adds variables to the set used in my main specification. The dotted line shows the estimated treatment effect in my main specification.

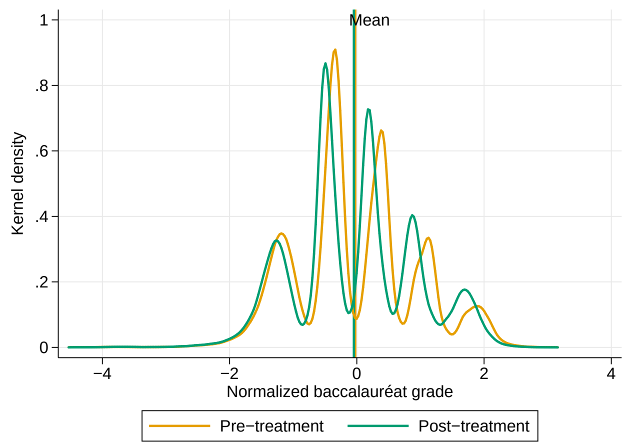
C Did the affirmative action program induce the students to put in more effort?

Figure C.1: Distribution of baccalauréat test-scores before and after the introduction of the affirmative action treatment.

(a) All students - Ever treated high schools



(b) All students - Never treated high schools



Notes: Distribution of normalized baccalauréat grades in high schools before and after the introduction of the affirmative action treatment. The left panel focuses on high schools which are not treated in 2009 and will enter the affirmative action program after 2009. The right panel focuses on high schools which are never treated. Source: DEPP Data from 2009 to 2016.

Table C.1: Impact of the treatment on the distribution of baccalauréat grades

	(1)	(2)	(3)	(4)	(5)
	Δy: Proportion of students obtaining a given baccalauréat grade				
			Panel A: Baseline estimates		
	< 10	10-12	12-14	14-16	16-20
Treatment effect	-1.637***	-0.018	2.299	0.151	-0.796**
95% C.I.	[-5.648 -1.229]	[-9.620 0.409]	[-0.602 2.587]	[-1.833 0.388]	[-2.394 -0.622]
			Panel B: Bias-adjusted estimates		
	< 10	10-12	12-14	14-16	16-20
Treatment effect	-3.123**	1.951	2.388**	-0.067	-1.150 **
95% C.I.	[-2.937 -0.678]	[-0.668 0.631]	[1.815 2.755]	[-0.532 0.619]	[-1.428 -1.123]
			Panel C: Bias-adjusted estimates, additionals controls		
	< 10	10-12	12-14	14-16	16-20
Treatment effect	-3.156**	1.431	2.634**	0.155	-1.064**
95% C.I.	[-2.235 -0.246]	[-0.467 0.214]	[1.010 3.214]	[-0.654 0.789]	[-1.321 -1.003]
Mean in control group	22.78%	31.93%	24.42%	13.67%	7.20%
N	5,626	5,626	5,626	5,626	5,626

Notes: This shows the Selective Treatment Timing estimates of the treatment effect of the affirmative action program on the share of students in each high school that obtain a given baccalauréat grade in the June session of the baccalauréat. In Panels (B) and (C), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (C), the estimates control for changes in the average baccalauréat grade, in the proportion of low-SES female students in the high school and in the local unemployment rate and proportion of immigrants in the municipality. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by size. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

D Robustness check: cells based on demographic characteristics only.

In the appendix C, I showed that the distribution of baccalauréat grades faced some changes after the introduction of the affirmative action program. In particular, the percentage of students who got a grade lower than 10 in the first session of the baccalauréat fell as did the number of high-achievers. A higher proportion of students obtained grades in the middle of the grade distribution.

In my main specification, my sample is restricted to students who had obtained a passing grade in the baccalauréat. This may lead to an underestimation of the treatment effect if, as a result of lower effort, some students in treated schools would have received a lower grade in the absence of the affirmative action treatment. In addition, I use cells based on academic and demographic characteristics, whose composition may be affected by the treatment. In this appendix, I estimate how sensitve my main results are to those choices. I first extend my sample to include students who received a grade lower than 10 in the baccalauréat. Second, I create student cells based on students' demographic characteristics only. Finally, I exclude students that identify as high-SES due to concerns that those students may have chosen to attend treated high schools to benefit from the treatment.

Table D.1: Treatment effect: cells based on demographic characteristics only

	(1)	(2)
	Panel A: Δy: Impact on enrollment at Sciences Po (%)	
Selective Treatment Timing	0.717***	0.867
95% C.I.	[0.539 0.895]	[-1.149 4.993]
N	1,336	1,336
Mean in control group	0.0189	0.0189
	Panel B: Δy: Impact on enrollment at other selective colleges (%)	
Selective Treatment Timing	0.129	1.186
95% C.I.	[-0.863 1.061]	[-10.26 25.27]
N	1,336	1,336
Mean in control group	7.566	7.566
	Panel C: Δy: Number of applications to selective colleges	
Selective Treatment Timing	0.474	0.707
95% C.I.	[-25.84 30.04]	[-0.807 2.10]
N	650	650
Mean in control group	1.115	1.115
Bias-adjusted	No	Yes

Notes: This shows the Selective Treatment Timing estimates of the treatment effect of the affirmative action program. In column (2) , the outcome variable was bias-adjusted for differences in matching covariates within matches. Each observation in this model is a student cell based on student demographic characteristics only, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. The estimates presented here are the *Selective Treatment Timing* estimates. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. The sample includes all students, not only those who received a passing grade in the June session of the Baccalauréat. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

E Heterogeneity

Table E.1: Treatment effect per cohort

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Δy: Enrollment at Sciences Po (%)				
Treatment cohort	2010	2011	2012	2013	2014	2015	2016	2010-2016
Selective Treatment Timing	0.665***	0.434***	0.396***	0.593**	-0.064	0.000	0.000	0.462***
95% C.I	[0.40 0.96]	[0.18 0.73]	[0.00 0.97]	[0.10 1.21]	[-0.19 0]	.	.	[0.30 0.66]
				Panel B: Δy: Enrollment at other selective colleges (%)				
Treatment cohort	2010	2011	2012	2013	2014	2015	2016	2010-2016
Selective Treatment Timing	0.705	2.344**	0.600	-0.435	3.556**	11.09	-0.589	1.127
95% C.I	[-0.49 1.96]	[0.41 4.28]	[-1.59 2.63]	[-3.69 2.27]	[0.30 8.01]	[-33.11 120.48]	[-21.09 22.11]	[-0.86 4.59]
				Panel C: Δy: Number of applications to selective colleges				
Treatment cohort	2010	2011	2012	2013	2014	2015	2016	2010-2016
Selective Treatment Timing	-	-	-	0.319	0.815*	1.576	1.842	0.932
95% C.I	-	-	-	[-0.261 1.385]	[-.0827 1.756]	[-0.083 11.467]	[-6.775 7.651]	[-0.107 1.399]
Proportion of Immigrants in municipality	17.5	16.103	18.075	12.128	8.915	18.750	31.682	
Unemployment Rate in municipality	18.25	15.111	11.827	21.77	10.032	21.258	29.543	
Low-SES Students (%)	76.88	74.392	81.713	72.469	45.914	70.968	92.000	
Enrolled at a selective college (%)	8.64	9.162	8.768	12.863	11.111	18.182	6.557	

Notes: This shows the average treatment effect on the treated of the affirmative action program separately for cohorts of high schools that enter the treatment in a given year. The outcome is the percentage of students within each cell who was enrolled at the relevant college in the academic year following high school graduation. Those are simple estimates, non-bias-adjusted. Each observation in those regressions is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

Table E.2: Heterogeneity per gender

	(1)	(2)	(3)
	Panel A: Δy: Enrollment at Sciences Po (%)		
Treated	0.412***	0.368***	0.330***
95% C.I.	[0.107 0.661]	[0.107 0.661]	[0.034 0.731]
Treated × Female	0.193	0.200	0.263
95% C.I.	[-0.156 0.596]	[-0.156 0.596]	[-0.126 0.723]
Mean female in control group	0.138	Mean male in control group	0.078
	Panel B: Δy: Enrollment at other selective colleges (%)		
Treated	1.155	1.710*	2.491**
95% C.I.	[-1.86 3.21]	[-0.18 3.09]	[0.422 4.61]
Treated × Female	-0.503	-1.423	-0.969
95% C.I.	[-2.81 2.86]	[-2.97 1.26]	[-4.50 1.46]
Mean female in control group	8.097	Mean male in control group	17.131
	Panel C: Δy: Number of applications to selective colleges		
Treated	1.343 **	1.244**	1.461*
95% C.I.	[0.141 2.221]	[0.279 2.060]	[-0.089 2.329]
Treated × Female	-1.191*	-1.158 **	-1.381*
95% C.I.	[-2.188 0.031]	[-1.949 -0.076]	[-2.269 0.254]
Mean female in control group	1.26	Mean male in control group	2.39
Bias-adjusted	No	Yes	Yes
With controls	No	No	Yes

Notes: This shows the Selective Treatment Timing estimates of the treatment effect of the affirmative action program. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the average baccalauréat grade, in the proportion of low-SES female students in the high school and in the local unemployment rate and proportion of immigrants in the municipality.

Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

Table E.3: Heterogeneity per gender and academic achievement

	(1)	(2)	(3)
	Panel A: Δy: Enrollment at Sciences Po (%)		
Treatment	0.770	0.746	0.50
95% C.I.	[-0.618 3.271]	[-0.51 3.72]	[-0.81 2.34]
× Female	1.224	1.241	1.468
95% C.I.	[-2.26 2.136]	[-3.07 4.87]	[-2.65 5.66]
times Non-high-achiever	-0.373	-0.395	-0.202
95% C.I.	[-3.095 0.646]	[-5.42 0.69]	[-2.65 1.23]
× Female *times* Non-high-achiever	-1.237	-1.252	-1.436
95% C.I.	[-2.362 2.279]	[-4.97 3.09]	[-5.23 3.21]
	Panel B: Δy: Enrollment at other selective colleges (%)		
Treatment	-6.869	-6.531	-5.529
95% C.I.	[-14.81 6.76]	[-15.08 3.61]	[-9.50 2.24]
× Female	8.370	8.375	9.872
95% C.I.	[-11.44 17.50]	[-3.23 20.41]	[-1.25 18.65]
× Non-high-achiever	9.175	9.380	9.487
95% C.I.	[-5.57 17.62]	[-2.14 15.69]	[-1.63 16.48]
× Female × Non-high-achiever	-9.913	-10.987	-12.772
95% C.I	[-16.80 5.12]	[-20.50 3.23]	[-21.65 1.05]
	Panel C: Δy: Number of applications to selective colleges		
Treatment	-0.549	-0.716	-0.361
95% C.I.	[-3.824 4.096]	[-3.386 2.734]	[-4.466 6.718]
× Female	-0.121	0.502	-0.088
95% C.I.	[-5.514 3.938	[-4.232 3.294]	[-8.096 4.685]
× Non-high-achiever	2.174	2.263	2.113
95% C.I.	[-1.312 5.491]	[-1.213 4.924]	[-4.905 6.820]
× Female × Non-high-achiever	-1.265	-1.942	-1.526
95% C.I.	[-5.481 4.011]	[-4.662 2.889]	[-6.726 6.355]
Bias-adjusted	No	Yes	Yes
With controls	No	No	Yes

Notes: This shows the Selective Treatment Timing estimates of the treatment effect of the affirmative action program. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the average baccalauréat grade, in the proportion of low-SES female students in the high school and in the local unemployment rate and proportion of immigrants in the municipality. Non-high-achievers are considered as such if they have obtained a grade between 10 and 14 at the baccalauréat. High-achievers have received a grade higher than 14.

Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

Table E.4: Heterogeneity per self-reported SES

	(1)	(2)	(3)
		Panel A: Δy: Enrollment at Sciences Po (%)	
Treated	1.066***	0.975***	0.893***
95% C.I.	[0.223 1.621]	[0.201 1.750]	[0.326 1.645]
Treated × low-SES	-0.660**	-0.598*	-0.496*
95% C.I.	[-1.491 -0.006]	[-1.351 0.023]	[-1.435 0.023]
Mean low-SES in control group	0.033	Mean high-SES in control group	0.391
		Panel B: Δy: Enrollment at other selective colleges (%)	
Treated	0.411 ***	0.686***	0.800**
95% C.I.	[-0.86 4.25]	[0.54 8.34]	[0.12 4.23]
Treated × low-SES	0.690	0.806	1.101
95% C.I.	[-2.73 2.15]	[-4.80 1.92]	[-5.23 2.12]
Mean low-SES in control group	9.265	Mean high-SES in control group	19.226
		Panel C: Δy: Number of applications to selective colleges	
Treated	0.900	0.445*	0.646
95% C.I.	[-0.392 2.396]	[-0.187 2.215]	[-0.518 2.855]
Treated × low-SES	-0.346	0.101	-0.116
95% C.I.	[-2.094 1.041]	[-1.874 0.726]	[-2.341 1.362]
Mean low-SES in control group	1.236	Mean high-SES in control group	3.228
Bias-adjusted	No	Yes	Yes
With controls	No	No	Yes

Notes: This shows the Selective Treatment Timing estimates of the treatment effect of the affirmative action program. In columns (2) and (3), the outcome variable was bias-adjusted for differences in matching covariates within matches. In column (3), the estimates control for changes in the average baccalauréat grade, in the proportion of low-SES female students in the high school and in the local unemployment rate and proportion of immigrants in the municipality.

Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

Table E.5: Heterogeneity per high school major

	(1)	(2)	(3)
	Panel A: Δy: Enrollment at Sciences Po (%)		
	Sciences	Economics	Humanities
Treated	0.460***	0.594***	0.648***
95% C.I.	[0.189 0.694]	[0.183 1.02]	[0.0069 3.11]
Mean in control group	0.036	0.048	0.000
N	2,828	1,827	1,069
	Panel B: Δy: Enrollment at other selective colleges (%)		
	Sciences	Economics	Humanities
Treated	1.896*	0.256	1.547
95% C.I.	[-0.0699 8.972]	[-4.997 2.641]	[-2.788 5.059]
Mean in control group	20.49	4.85	3.62
N	2,828	1,827	1,069
	Panel C: Δy: Number of applications to selective colleges		
	Sciences	Economics	Humanities
Treated	0.606	0.649	0.276
95% C.I.	[-0.41 1.46]	[-0.15 3.42]	[-0.59 1.91]
Mean in control group	4.37	0.95	0.22
N	1,759	1,145	621
Bias-adjusted	Yes	Yes	Yes
With controls	No	No	No

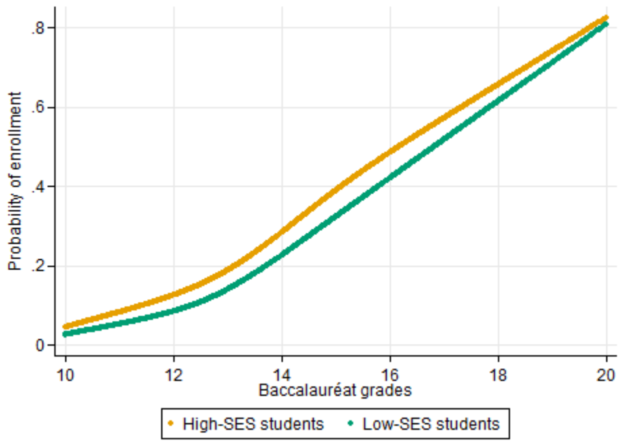
Notes: This shows the Selective Treatment Timing estimates of the treatment effect of the affirmative action program. All the estimates are bias-adjusted for differences in matching covariates within matches. In each column, the estimates are restricted to the corresponding high school major.

Each observation in this model is a student cell based on student demographic and academic characteristics, within each high school. Each treated observation is matched to a control observation according to the methodology explained in section 6.3. Each observation is weighted by cell size. Average treatment effects are first computed separately for each treated cohort and each treated period. Then, I average those ATT per cohort. The confidence intervals are computed using block bootstrapped standard errors. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

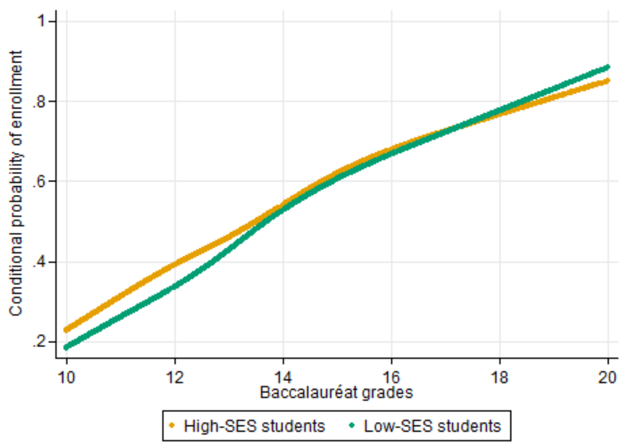
F Documenting the social gap in academic achievement based on self-reported SES

Figure F.1: Probability of enrollment into a selective college and baccalauréat grades.

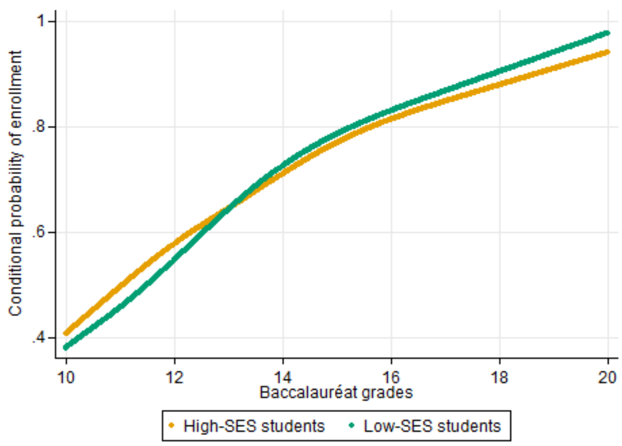
(a) Unconditional



(b) Conditional on having applied to one selective college



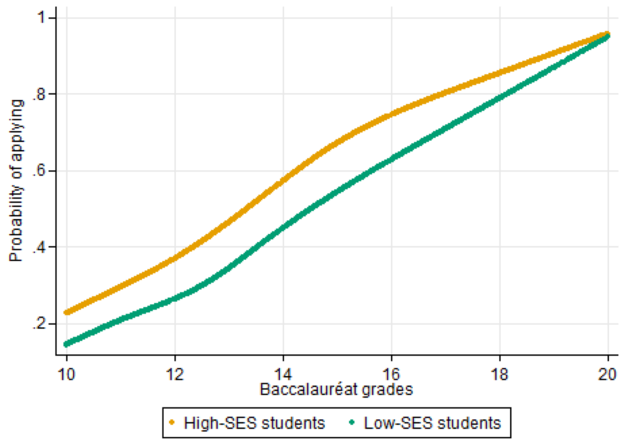
(c) Conditional on having ranked a selective college first



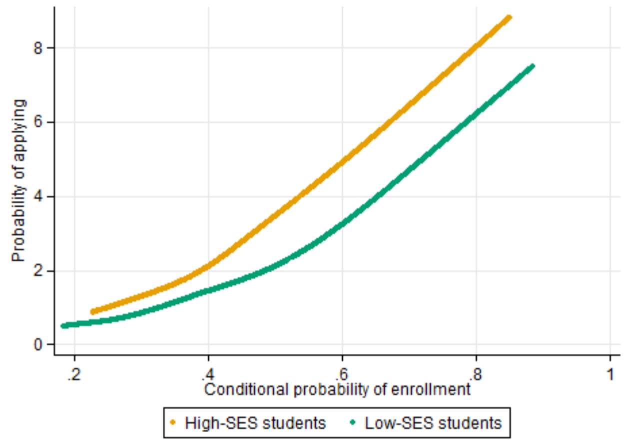
Notes: This plots the non-parametric relationship between baccalauréat grades and the probability of being enrolled at a selective college. This non-parametric relationship is obtained through a restricted cubic spline regression with 5 knots. Figure F.1a plots the unconditional probability of being enrolled at a selective college. In figure F.1b, I plot the probability of being enrolled at a selective college conditional on having applied to at least one selective college. Figure F.1c plots the probability of being enrolled at a selective college conditional on having ranked an application to a selective college first. The sample is further restricted to students having obtained a passing grade at the baccalauréat. The baccalauréat is a uniform exam taken at the end of high school. With a few exceptions, students must obtain a grade of 10/20 to be allowed to graduate and enroll at a higher education institution. Low-SES students are considered low-SES if their reported parental occupation is a profession requiring at least five years of higher education studies. Source: DEPP-SIES, 2012, 2014-2016. Source: APB'Stat and DEPP/SIES data. 2012, 2014-2016.

Figure F.2: Relationship between probability of having applied to at least one selective college and:

(a) Test scores



(b) Conditional probability of enrolment

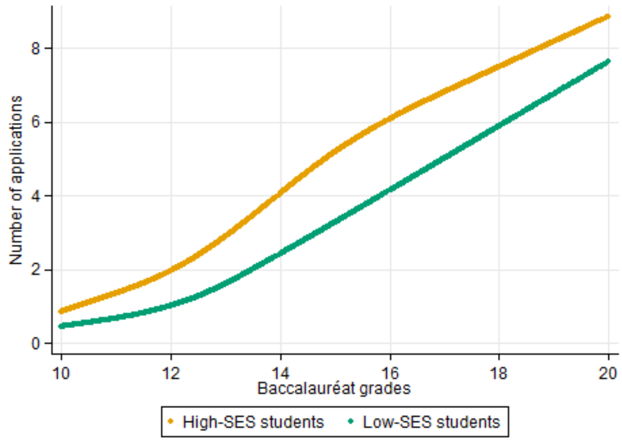


Notes: This plots the non-parametric relationship between the probability that a student has applied to at least one selective college and her academic achievement. This non-parametric relationship is obtained through a restricted cubic spline regression with 5 knots. Figure F.2a plots the unconditional probability of having applied to at least one selective college as function of the student's baccalauréat grade. In figure F.2b,the x-axis is the probability of enrolling at a selective college conditional on having applied to at least one. This probability was predicted non-parametrically through a restricted cubic spline regression as described in figure F.1b.

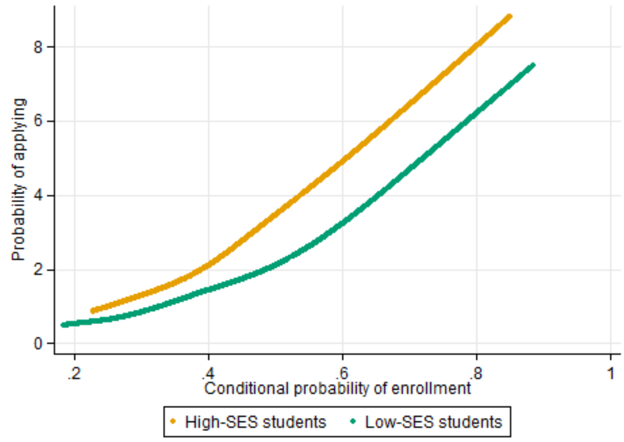
The sample is restricted to students having obtained a passing grade at the baccalauréat. The baccalauréat is a uniform exam taken at the end of high school. With a few exceptions, students must obtain a grade of 10/20 to be allowed to graduate and enroll at a higher education institution. Low-SES students are considered low-SES if none of their parents has a profession requiring at least five years of higher education studies. Source: DEPP-SIES, 2012, 2014-2016. Source: APB'Stat and DEPP/SIES data. 2012, 2014-2016.

Figure F.3: Relationship between the number of applications to selective colleges and:

(a) Test scores



(b) Probability of enrollment conditional on having applied to at least one selective college

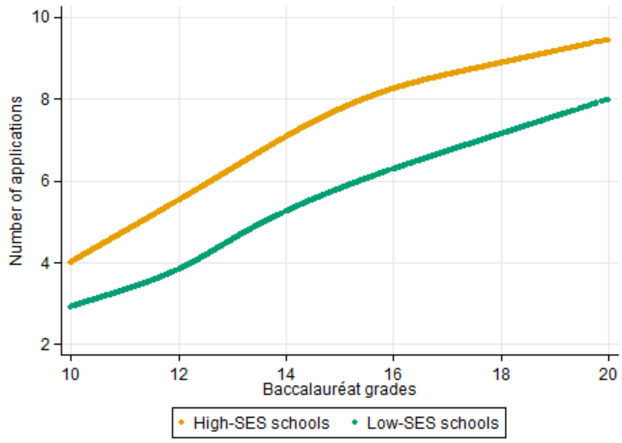


Notes: This plots the non-parametric relationship between the number of applications a student has filed to selective colleges and her academic achievement. This non-parametric relationship is obtained through a restricted cubic spline regression with 5 knots. In figure F.3a, the x-axis is the student's baccalauréat grade. In figure F.3b, the x-axis is the probability of enrolling at a selective college conditional on having applied to at least one. This probability was predicted non-parametrically through a restricted cubic spline regression as described in figure F.1b.

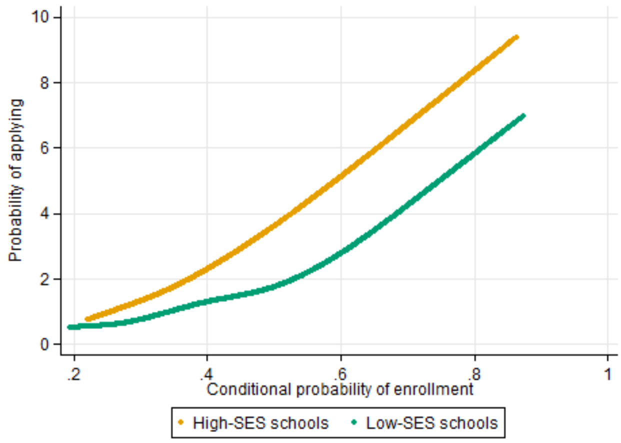
The sample is restricted to students having obtained a passing grade at the baccalauréat. The baccalauréat is a uniform exam taken at the end of high school. With a few exceptions, students must obtain a grade of 10/20 to be allowed to graduate and enroll at a higher education institution. Low-SES students are considered low-SES if none of their parents has a profession requiring at least five years of higher education studies. Source: DEPP-SIES, 2012, 2014-2016. Source: APB'Stat and DEPP/SIES data. 2012, 2014-2016.

Figure F.4: Relationship between the number of applications to selective colleges conditional on submitting at least one and:

(a) Test scores



(b) Probability of enrollment conditional on having applied to at least one selective college



Notes: This plots the non-parametric relationship between the number of applications a student has filed to selective colleges conditional on having applied to at least one and her academic achievement. This non-parametric relationship is obtained through a restricted cubic spline regression with 5 knots. In figure F.3a, the x-axis is the student's baccalauréat grade. In figure F.3b, the x-axis is the probability of enrolling at a selective college conditional on having applied to at least one. This probability was predicted non-parametrically through a restricted cubic spline regression as described in figure F.1b.

The sample is restricted to students having obtained a passing grade at the baccalauréat. The baccalauréat is a uniform exam taken at the end of high school. With a few exceptions, students must obtain a grade of 10/20 to be allowed to graduate and enroll at a higher education institution. Low-SES students are considered low-SES if none of their parents has a profession requiring at least five years of higher education studies. Source: DEPP-SIES, 2012, 2014-2016. Source: APB'Stat and DEPP/SIES data. 2012, 2014-2016.