Wanna Play? Get an A: The Effect of Academic Eligibility Requirements for High School Athletics Participation on Students' Academic Outcomes

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Abstract

This thesis examines the effect of academic eligibility requirements for high school athletics participation on the academic outcomes of public high school students. Specifically, I assess the effects of a 2013 policy which created more stringent academic eligibility requirements for participation in the Public School Athletic League (PSAL) of New York City. My investigation uses a difference-in-difference model to compare the changes in rates of attendance, suspension, and graduation in high schools that were in the PSAL, to those of schools that were not in the PSAL and thus unaffected by the policy. I find that overall, the policy had no effect on suspension rates, but, on average, increased attendance rates by 1.8 percentage points, or just over 3 school days, and increased graduation rates by 3.6 percentage points.

Heterogeneity analyses suggest that the gains in attendance and graduation rates were largely driven by significant improvements, both statistically and economically, for predominantly White schools and schools with predominantly non-low English proficiency students. Conversely, the policy had a relatively neutral effect on the attendance rates of schools serving greater shares of non-White or low English proficiency students, and a negative effect on the graduation rates of schools of the latter kind. Economic literature reveals that, on average, predominantly non-White schools spend disproportionately less per pupil than majority White schools. Thus, this heterogeneity in treatment effect suggests that while academic eligibility requirements may be successful at encouraging stronger academic performance by students overall, these improvements could be occurring at the expense of exacerbating achievement gaps between students at better funded schools and students at less funded schools.

JEL Classification: I24, I29, I21

Keywords: High School Athletics; Academic Eligibility Requirements; Academic Achievement Gaps; Education

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I. Introduction

Academic incentive schemes play a critical role in nearly every classroom. Ranging from casual pizza parties to official cash transfers, school teachers and administrations often rely on incentives to encourage stronger academic performance by their students. Incentive schemes generally come in one of two forms. Positive incentives reward students for good behavior. These incentives can vary in formality, including everything from a symbolic gold sticker on a wall to more tangible awards, prizes, and even cash transfers. On the other hand, negative incentives are punishments or repercussions for students who fail to meet an academic standard. These incentives include consequences as simple as losing certain extracurricular privileges, but can be as severe as academic suspension, academic probation, or being held back a year in school.

In theory, both kinds of incentive schemes are intended to improve student outcomes, either by rewarding students upon improved performance or by serving as a wake-up call upon poor performance. However, research has demonstrated that some incentive schemes tend to generate heterogeneous effects which ultimately exacerbate achievement gaps. In particular, many incentives disproportionately support those most likely to succeed while discouraging those most at risk of failing, further increasing the gap between high and low achieving students. The literature investigating the effects of positive incentives, most often studied in the form of cash transfers, has shown that formal positive incentives of this kind frequently demonstrate the largest and most significant effects on students at the top of the achievement distribution. These same transfers, however, often have little to no effect on relatively low-achieving students (Angrist and Lavy 2009; Fryer 2011; Bettinger 2010). Theoretical models of incentive schemes suggest that this result is likely due to the fact that students at the top of the achievement distribution see a large gain in utility for a relatively smaller marginal cost, compared to their peers at the bottom of the distribution (Vidal-Fernandez 2011). However, if positive incentives, or awards for achievement, are encouraging those at the top of the achievement distribution to succeed, could negative incentives, or consequences for failure, be affecting improvements for those students at the bottom?

The current literature which seeks to answer this question is quite extensive, but predominantly focuses on the effects of one specific kind of negative incentive: grade retention. There is a consensus in this literature that grade retention disproportionately harms students most at risk of succeeding (Jacob and Lefgren 2009; Cockx, Picchio, and Baert 2019; Battistin and Schizzerotto 2019). This result is largely due to the fact that low-achieving students are most likely to suffer the consequences of these incentives and rather than serving as a wakeup call, these incentives instead may be further discouraging students from applying themselves and performing well in school.

The heterogeneity in these findings on the effects of academic incentives have severe implications and demand urgent consideration in the context of educational inequality. If incentives intended to improve the outcomes of students are actually benefiting high-achieving students at the expense of their low-achieving peers, then these incentives will only further widen any gaps in achievement. Thus, it is critical that the impacts of academic incentive schemes be assessed particularly when being implemented in locations where achievement gaps are already pervasive.

New York City (NYC), home to the largest public school system in the country, has one of the most severe gaps in high school academic achievement.[1] My paper investigates an academic incentive in the form of academic eligibility requirements for high school athletics participation, which were enacted during the 2013-2014 school year and impacted student-athletes in the Public School Athletic League (PSAL) of NYC. These eligibility requirements behave as a negative academic incentive in that they threaten removal from athletic participation upon insufficient academic performance.

It is theoretically ambiguous as to what kind of an impact stricter eligibility requirements should have on student-athletes. On one hand, it is possible that the marginal student-athlete whose academic performance is right below the cusp of the requirements would react to the policy with increased effort in school in order to maintain eligibility. As the existing literature suggests, this reaction could largely be due to the relatively small marginal cost of improved performance compared to the large gain in utility from being able to continue playing a sport. Thus, the policy would yield positive effects on their academic performance. However, it is entirely possible that for a student whose performance is well below the minimum, this policy would discourage them by creating seemingly unattainable standards for their academic performance. Furthermore, these students would simultaneously be affected by a sudden removal from athletics participation, which could also negatively affect their academic performance (Heckman, Stixrud, and Urzua 2006; West et al. 2016). There is currently only one economist who has rigorously evaluated the causal impacts of

[1]A 2020 report written by brightbeam, a non-profit network of education activists, uses publicly available data from school districts on school achievement and identifies a 49-point achievement gap between Latino and White students in Math. In reading, they find a 33-point gap between Black and White students, and a 31-point gap between Latino and White students, placing NYC towards the top of their educational inequality rankings of cities across the nation.

high school athletics eligibility requirements on academic outcomes, Vidal Fernández (2011), and I discuss her findings more thoroughly in Section II.

My investigation uses data published by the New York State Education Department (NYSED) for the school years from 2004-2005 to 2018-2019. I employ a difference-in-differences strategy that compares the changes in PSAL member schools' average attendance, suspension, and graduation rates to those of NYC public schools which did not participate in the PSAL and were thus unaffected by the policy. I find that the policy had a significant positive effect on attendance rates, improving attendance by 1.8 percentage points or just slightly over 3 school days. The policy's positive effect on graduation rates, though statistically insignificant, yielded improvements of 3.6 percentage points, a finding which is consistent with that of Vidal Fernández (2011). Lastly, the policy had an insignificant, zero effect on share of students suspended.

Heterogeneity tests reveal that the positive effects on attendance and graduation rates were driven largely by disproportionately strong, positive impacts on predominantly White schools as well as schools composed of predominantly non-low English proficiency (LEP) students. In particular, the former schools, on average, saw improvements of 4.4 percentage points, or 8 school days, in attendance rates and 14.8 percentage points in graduation rates. The latter schools saw improvements of 2.2 percentage points in attendance rates and 13 percentage points in graduation rates. Of note, event study regressions suggest that these graduation rate results could, in part, be due to pre-trends in these subsamples of schools. However, the lack of trending in the few years leading up to the policy suggests that the academic eligibility requirements likely may have driven some of the increases in graduation rates as well. On the other hand, non-White schools saw no impact on attendance rates and a one percentage point decrease in graduation rates, though both statistically insignificant. Similarly, schools with predominantly LEP students saw no impact on attendance rates and a 6.8 percentage point decrease in graduation rates, though again, both statistically insignificant. Economic literature suggests that schools with larger shares of non-White or low-income students tend to have less access to funding and lower levels of expenditure-per-pupil compared to predominantly White schools (Rothbart 2020; Sosina and Weathers 2019; Weiss 2020). Thus, academic eligibility requirements for sports participation appear to disproportionately encourage students at better funded schools while having no effect, and in some instances an even negative effect, on the academic outcomes of students in less funded schools. As such, it is critical that policies looking to either implement or raise the standards of academic eligibility requirements for sports participation consider how these changes may exacerbate achievement gaps, particularly if differences in academic achievement are already substantial.

In Section II, I summarize the existing literature on the effects of academic incentives and discuss the only existing formal economic study that investigates the effects of high school academic eligibility requirements for athletics specifically, written by Vidal-Fernández (2011). In Section III, I provide background on the PSAL and the 2013 policy which increased the standards of academic eligibility for athletics participation within the league. In Section IV, I describe my data and provide sample summary statistics and in Section V, I detail my difference-in-difference methodology. Sections VI and VII summarize and discuss results respectively.

II. Previous Literature

Formal academic incentive schemes have been implemented in classrooms for over two centuries. New York City schools in particular are recorded as having used academic incentives as early as 1820, when schools first carried out a system which gave financial rewards for students' strong academic performance (Bettinger 2010). Since then, formal incentives have become increasingly prevalent in all kinds of classrooms. Consequently, the economic literature that studies their effect on student achievement is extensive and thorough. This literature can largely be divided into two categories: that which studies the effects of positive academic incentives and that which studies the effects of negative academic incentives. There is a consensus in the literature that both types of incentives often disproportionately affect different kinds of students and consequently, could be driving gaps in achievement. As such, a majority of the existing literature seeks to identify and assess any forms of effect heterogeneity.

The literature which studies positive academic incentives largely focuses on the effects of cash incentives on academic achievement. Cash incentives work as an academic incentive in that students are rewarded with payment upon satisfactory academic performance. Studies have investigated the effects of this incentive on a wide range of student ages. Many have observed that cash incentives rarely affect low-achieving students but have significant positive effects on the academic performance of high-achieving students. Affected performance indicators encompass a myriad of outcomes ranging from elementary school test scores to college matriculation rates (Angrist and Lavy 2009; Fryer 2011; Bettinger 2010). These results largely agree with one another and suggest that high-achieving students benefit the most from cash transfers.

On the other hand, more relevant to this study, is the literature which studies the effects of negative academic incentives. These investigations largely focus on the effects of grade retention. Grade retention is when a student is held back in school to repeat a year of education as a result of failure to achieve a certain academic standard. Economists have studied the impact that grade retention has throughout a student's educational career. Most conclude that this negative incentive tends to have detrimental long term impacts, which often disproportionately harm those students most at-risk of succeeding (Cockx, Picchio, and Baert 2019; Battistin and Schizzerotto 2019; Tafreschi and Thiemann 2016; Manacorda 2012). Investigating a sample most similar to mine, Jacob and Lefgren (2009) study the impact of grade retention on junior high school students in Chicago. They find that on average, students who are held back at an older age, when they have less time to catch up to their peers before high school, are eight percentage points more likely to drop out of high school relative to their same-aged peers. Furthermore, the sample of eighth graders most negatively impacted by grade retention were low-achieving African American and Black girls who had failed both their end-of-year exams. Thus, Jacob and Lefgren conclude that grade retention most negatively affects students who are already least likely to succeed.

Evidently, while the economic literature on academic incentives is rich in depth, it is deficient in breadth and focuses only on a handful of different academic incentives. Particularly lacking is the literature on the effects of academic eligibility requirements for extracurricular participation, of which, a majority is non-economic and studies college-aged student-athletes. The one economic paper that does focus on eligibility requirements for high school students is written by Vidal-Fernández (2011). Vidal-Fernández uses data from the National Longitudinal Youth Survey of 1979 in a linear probability model. She exploits cross-state variation in the number of passed courses required for athletic eligibility to predict the probability of a student graduating high school. Since she is unable to introduce any other forms of variation in her cross-sectional analysis, relative to a difference-in-difference model, her strategy is less capable of controlling for unobservable, confounding variables which may be correlated with treatment. Nonetheless, her findings are robust and suggest that a one course increase in the minimum requirement significantly increases the probability of graduation by two percentage points in high-school male athletes. With regards to heterogeneity, Vidal-Fernández finds that conditional on having taken the Armed Forces Qualification Test (a widely used proxy for academic ability), Black students are more likely to graduate while Hispanic students are less likely to graduate in response to the same one course increase in eligibility requirements. She finds no effect on girls but concludes that this is likely due to the lack of sports participation by girls during this time.

Vidal-Fernández's paper provides interesting insights into the positive effect that course-based eligibility requirements have on male, high-school-aged student-athletes. However, she is only able to investigate one kind of eligibility requirement and one academic outcome. Furthermore, her sample is comprised of significantly fewer Black and Hispanic students than White students. Thus, while Vidal-Fernández's paper produces valuable insights, her identification strategy could be strengthened, and her findings are only able to elucidate part of the larger picture. My paper's increased scope hopes to investigate the effects of more stringent academic requirements on a more diverse sample of students. Furthermore, my ability to observe variation across both schools and time allows for a more precise and reliable identification strategy. Lastly, by focusing on students in New York City, which is home to both the largest and one of the most unequal public education systems, my paper hopes to have critical implications for policies intended to mitigate high school academic achievement gaps.

III. PSAL and Policy Background

The PSAL is the oldest and largest athletic league in America (Waggoner 2016). Founded in 1903, its purpose was to provide a more proper and legitimate city-wide athletic league for “average” athletes. Kicking off its first tournament with just over 1,000 participants and two sports (Football and Track & Field), the league quickly gained traction amongst students and schools and expanded ferociously, vanquishing all other major NYC athletic leagues within five years. Today, the PSAL offers programming in 25 different varsity sports for more than 45,000 student-athletes from over 400 participating schools.

In 2012, the PSAL Board of Directors and key stakeholders, including members of the United Federation of Teachers (UFT), came together to identify a way in which the PSAL could use its authority to help improve the academic outcomes of public high school students. This collaboration was largely in response to grievances expressed by both parties with regards to the academic performance of these students. Amongst those involved with the PSAL, many lamented the fact that star athletes were ending their sports careers after high school due to their inability to graduate and gain eligibility to continue playing their sport in college with the NCAA. Amongst those members of the UFT, there was a pressing desire to ameliorate the academic standings of NYC public school students, particularly in the midst of Mayor Bloomberg’s aggressive education campaign. Thus, in January of 2013, the New PSAL Eligibility Requirements Memorandum was published with the stated goal to “increase graduation rates and [the] academic performance of students participating in the PSAL". The policy went into effect in September of 2013, at the start of the 2013-2014 school year.

Table 1 outlines the PSAL eligibility requirements before and after the policy. Before 2013, the eligibility requirements for PSAL athletic participation were relatively benign. Each semester, students had to pass a minimum of four credits and physical education for a total of eight credits for the school year, and achieve a minimum attendance rate of 80% at the end of each marking period.

TABLE 1 - PSAL Eligibility Requirements Updates

	Old Rule	New Academic Requirements
Academic Revisions	Student must pass four credit bearing subjects and physical education.	Student must pass five credit bearing subjects and physical education. 3 out of the 5 classes must be major subjects. CTE class may not be counted as majors. A senior programmed for 4 or 5 classes who fails one class, in his/her senior year, will be eligible for PSAL so long as the failed class is not required for graduation.
	Student must accumulate a minimum of eight credits for the two semesters prior to the eligibility period not counting PE.	Student must accumulate ten credits for the two semesters prior to the eligibility period not counting PE. (Effective February 1, 2014)
	There is NO current requirement for GPA	Student must achieve a passing GPA at the time of eligibility evaluation.*
Attendance Revision	Student must achieve a minimum of 80% attendance at the end of each marking period.	Student must achieve a minimum of 90% attendance at the end of each marking period.
Transfer Revision	9th grade students who transfer are not automatically eligible for one year.	9th grade students who are granted a transfer will be deemed eligible.

*The athletic director may submit a Request for an Eligibility Review of a Student-Athlete when extenuating circumstances exist. The principal must approve this request.

Source: Chief Executive Eric Goldstein, "2013 PSAL Final Memo," January 29, 2013.

The new PSAL eligibility requirements made five total revisions to the initial eligibility policy set forth by the league. Four of these revisions pertained to the academic performance of students. As outlined in Table 1, the performance-related changes included increases to the minimum number and the kinds of credits accumulated and passed, the creation of a minimum GPA requirement, and an increase in the minimum average attendance rate. In particular, each semester students were now expected to pass an additional 5^{th} credit and have 3 of their 5 credits be from major subjects. Thus, over the course of the school year, students were expected to have completed and passed a total of 10 credits, excluding physical education. Furthermore, a minimum GPA of passing, which requires students to pass all classes they enroll in, was instated to begin holding students accountable by their academic performance. Lastly, the minimum average attendance rate was increased from 80% to 90%. Any student who fails to meet all the requirements is deemed ineligible from PSAL participation until the next semester's eligibility evaluation, contingent on meeting the minimum requirements.

IV. Data

IVa. New York State Education Department: Report Card Database

A majority of the data used in this study are from the Report Card Database published by the New York State Education Department (NYSED). The Report Card database is a comprehensive report published annually that describes New York State public education at the school-, district-, county-, and state-level. The data provided by this report is comprehensive in both depth and breadth, providing my investigation with ample control and outcome variables. In particular, for my control variables, I use the following data on school and student demographics: fraction of students that are White, Hispanic, Black, Asian, as well as fraction on Free or Reduced Lunch status (FRL), of low English proficiency (LEP), and the student-to-teacher ratio.[2]

For my outcome variables, the Report Card database provides information on the attendance rate, number of suspensions, and number of graduates per school per year. I transform the data for the number of suspensions and number of graduates in order for the variables to take on values between 0 and 1. Specifically, for the former, I divide the number of suspensions by the total number of students enrolled in the school that year in order to create a share of students suspended. Recall, the numerator of my share of students suspended variable is the number of suspensions rather than the number of students suspended. Thus, while I refer to this variable as the share of students suspended, it actually describes the total number of suspensions which occurred relative to the student body size, regardless of how many students comprise those suspensions. However, it still allows us to better understand the frequency with which suspensions occur in a school in a given year.

To create the graduation rate, I divide the number of graduates by the number of students enrolled in the 12th grade. There are multiple sources throughout the Report Card database which provide information on the number of students enrolled in the 12th grade. However, these sources are often inconsistent with one another. So, in order to maximize the number of observations with a reasonable value for graduation rate, namely a graduation rate

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[2] The NYSED began reporting and publishing data differently following the 2016-2017 school year. As such, the data for these years come from slightly different sources, though still from the larger database of the NYSED. The data for my outcome variables and fraction FRL students for these years come from the Student Educator Database (STUDED). The STUDED is reported alongside the Report Card database and highlights all the information previously published in the Report Card database related to the student body and staff. Furthermore, neither the Report Card database nor the STUDED publishes enrollment by race or low English proficiency status following the 2016-2017 school year. Therefore, this data is collected from the Information and Report Services (IRS) page of the NYSED website for the two last years of my sample.

with a value between 0 and 1, I accept the largest of the numbers reported as the true value for 12th grade enrollment. It is also important to clarify that this calculation for graduation rate reflects the number of students enrolled in the 12th grade that year who graduate. It does not reflect a graduation rate relative to the total number of students who had enrolled four years prior. Thus, students who drop out before the 12th grade are neglected from this calculation.

IVb. NYCDOE PSAL Report

While the PSAL provides most public schools with access to their league, not every school gets its own team. Therefore, it is necessary to obtain a list of both all PSAL programs as well as the schools which comprise each program. The PSAL does not have this data publicly available. However, since funding is obtained through the city government, the PSAL is required to submit a budget report to the New York City Council breaking down their requested funding by program and school. This report includes a list of all PSAL member schools' names and ID codes, which together allow for more accurate matching to the data in the Report Card database. The earliest such publicly available report is from FY2017. Although this report was published 3-4 years after the policy year, PSAL Executive Director Donald Douglas was able to confirm that while teams within programs were added and removed between 2014 and 2017, the list of participating schools and programs themselves remained unchanged. Thus, this report allows me to accurately distinguish the treated schools, those which participated in the PSAL, from the control schools, those which did not participate in the PSAL, during the policy years.

IVc. Data Restrictions and Sample Summary Statistics

The full sample which my investigation uses is comprised of all NYC public and charter schools that enrolled high school students during the 2012-2013 school year. Specifically, my dataset includes 7,028 observations from 549 unique schools across 15 years, from the 2004-2005 school year to the 2018-2019 school year.[3] Roughly 15% of this sample are non-PSAL schools while roughly 85% are PSAL schools. However, I focus my investigation on the sample of schools that were open at least five years before the policy and for the entirety of the post-policy years, through 2019. The reason for this is twofold. First, during the years shortly before and after the policy, many new schools opened, and many failing schools were shut down. Thus, in order to minimize the effects of any confounding variables which may be linked to either the early years or the final years of a school, I exclude these schools from my main sample of interest. Second, by restricting my sample to those schools open for at least five years before the policy and for the entirety of the post-policy years, I maximize the data I have for each school and can more accurately evaluate pretends and long run policy impacts.

[3] Suspension data is unavailable for the 2016-2017 school year and attendance data is unavailable for the 2017-2018 data. However, I keep these years in the sample since data for all other outcome variables are still available for these observations.

TABLE 2 - NYC Public High Schools Sample Summary Statistics

		Pre-Policy 2005-2013		Post-Policy 2014-2019		
	Full Sample	Non-PSAL Schools	PSAL Schools	Non-PSAL Schools	PSAL Schools	Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)
School Demographics						
Fraction White	0.088	0.052	0.092	0.064	0.087	-0.017
Fraction Hisp	0.424	0.423	0.415	0.447	0.435	-0.004
Fraction Black	0.378	0.430	0.387	0.407	0.359	-0.005
Fraction Asian, Native Hawaiian, Pacific Islander	0.092	0.050	0.091	0.058	0.100	0.001
Fraction FRL Status	0.730	0.699	0.703	0.790	0.765	-0.030
Fraction Low English Proficiency	0.126	0.078	0.133	0.091	0.125	-0.021
Student-Teacher Ratio	14.6	12.3	14.9	11.4	14.6	0.667
Short-term Academic Outcomes						
Attendance rate	0.862	0.833	0.861	0.834	0.869	0.007
Share of Students Suspended	0.049	0.073	0.064	0.039	0.022	-0.008
Long-term Academic Outcomes						
Graduation rate	0.874	0.861	0.887	0.787	0.865	0.053
Number of Observations	5,687	209	3,051	183	2,244	
Number of Schools	406	31	375	31	375	
Number of Public Schools	376	20	356	20	356	
Number of Charter Schools	30	11	19	11	19	

NOTES: Attendance rate data excludes 2017, Suspension data excludes 2018.

TABLE 3- NYC Public High Schools Summary Statistics, Robustness Check's Full Sample

		Pre-Policy 2005-2013		Post-Policy 2014-2019		
	Full Sample	Non-PSAL Schools	PSAL Schools	Non-PSAL Schools	PSAL Schools	Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)
School Demographics						
Fraction White	0.080	0.032	0.089	0.049	0.082	-0.023
Fraction Hisp	0.428	0.420	0.419	0.443	0.440	-0.002
Fraction Black	0.388	0.483	0.387	0.445	0.364	0.015
Fraction Asian, Native Hawaiian, Pacific Islander	0.084	0.036	0.089	0.041	0.093	-0.001
Fraction FRL Status	0.736	0.726	0.704	0.791	0.769	0.000
Fraction Low English Proficiency	0.129	0.111	0.136	0.094	0.129	0.010
Student-Teacher Ratio	14.2	13.4	14.8	10.8	14.2	1.960
Short-term Academic Outcomes						
Attendance rate	0.855	0.805	0.858	0.826	0.864	-0.015
Share of Students Suspended	0.053	0.091	0.067	0.049	0.026	0.000
Long-term Academic Outcomes						
Graduation rate	0.864	0.826	0.884	0.805	0.856	-0.007
Number of Observations	7,028	535	3,364	344	2,785	
Number of Schools	549	77	451	68	467	
Number of Public Schools	494	58	423	48	432	
Number of Charter Schools	55	19	28	20	35	

NOTES: Attendance rate data exclude 2017, Suspension data exclude 2018.

As described in Table 2, the restricted sample that my investigation focuses on is comprised of 5,687 observations from 406 unique schools. 375 of these schools are PSAL member schools whereas 31 are non-PSAL schools. PSAL and non-PSAL schools in my sample tend to differ slightly with regards to the racial breakdown of enrolled students. In particular, PSAL schools on average enroll more White and Asian students and fewer Black students. This is likely due to the fact that the PSAL historically included only large public high schools and excluded smaller, underfunded public schools which tended to enroll more Black students (Garcia-Rosen 2013).

Table 3 outlines the summary statistics for the full sample of schools, which I use throughout my analysis as a robustness check. These summary statistics confirm that my main sample of interest largely resembles the full sample of schools in terms of racial breakdown, fraction LEP and FRL, and student-to-teacher ratio.

V. Difference-in-Difference Methodology

The main estimating equation which my investigation uses to isolate the effect of the PSAL eligibility requirements on academic outcomes is the following:

(1)

$$y_{ij} = b_0 + b_1(Treat)_{ij} + \Delta_{ij} + \theta_i + \pi_j + \varepsilon_{ij}$$

Here, y_{ij} is the attendance rate, share of students suspended, or graduation rate for school *i* in year *j*. $Treat_{ij}$ is a binary variable equal to 1 if a school participates in the PSAL and the observation is for the 2013-2014 school year or later. Δ_{ij} represents a set of school characteristics for school *i* in year *j,* which serve as my covariates. These controls include the fraction of students who are White, Black, Asian, Hispanic, of Free or Reduced Lunch status

(FRL), or of low English proficiency (LEP). Also included is the student-to-teacher ratio. θ_i represents school fixed effects and π_j represents year fixed effects. The primary coefficient of interest is b_1, which isolates the change in academic outcomes attributable to the effect of the PSAL policy. Standard errors are clustered at the school level to account for possible correlation of the error term ε_{ij} across years within schools. Regressions are weighted by school enrollment levels during the year before the policy. I include weights by enrollment in order to achieve more precise results by correcting for heteroskedasticity; since enrollment levels range tremendously across public schools, variability is likely unequal across each outcome variable. Furthermore, considering the wide variation in school size, weighting by enrollment allows the estimates to reflect the effects of the policy for the average student in my sample. Without weighting by enrollment, the treatment effect would be driven disproportionately by students at smaller schools.

In addition, I estimate an event study model to examine pre-trends between my control schools, or non-PSAL schools, and my treated schools, or PSAL member schools. My event studies use the following equation:

(2)

$$y_{ij} = b_0 + \Sigma f_{ij}(PSAL)_i * \pi_j + \Delta_{ij} + \theta_i + \pi_j + \varepsilon_{ij}$$

This equation modifies Equation 1 by interacting $PSAL_i$, a binary variable equal to one if a school belongs to the PSAL, with year fixed effects. This interaction term allows me to isolate the changes in academic outcomes attributable to the policy by year. Recall, the policy was designed in January of 2013 for induction in the 2013-2014 school year. Since my dataset begins with the 2004-2005 school year, the interaction of $PSAL_i$ with year fixed effects provides information on the pre-trends for the nine years prior to the policy.

The key identifying assumption that my investigation makes is that the PSAL and non-PSAL schools' respective attendance, suspension, and graduation rates would trend similarly over time in the absence of the PSAL policy. This allows me to attribute any difference in their outcomes to the policy. One possible threat to this assumption would be if students systematically switched into or out of PSAL schools during the policy years. One instance in which this could occur is if low-achieving students who want to be eligible for athletic participation without having to improve their academic outcomes switch into non-PSAL schools in order to continue being able to participate in athletics. However, the summary statistics described in Tables 2 and 3 demonstrate that there were no significant changes in racial or socioeconomic composition for either sample over time. Furthermore, more practically speaking, it is unlikely that any student would go so far as to switch high schools just to be able to participate in athletics. This is particularly true given the fact that the PSAL is the most competitive and legitimate athletic league in the city. As such, any academically low-achieving athlete who is serious about their sport is more likely to be incentivized to improve their academic outcomes to continue to participate in the PSAL, than to switch high schools and participate in a less renowned league.

Another possible threat to the validity of this identifying assumption is if there are any other concurrent policies present that differentially affect either PSAL or non-PSAL schools. Such a concurrent policy would threaten my methodology because it would render ambiguous whether differences in changes in outcomes are actually attributable to the PSAL policy versus the other simultaneous policy. Fortunately, for the most part, there is no way to distinguish a PSAL school from a non-PSAL school from a policy standpoint, since every interested public or charter school in NYC is fully capable of PSAL membership.

Furthermore, education related policies in NYC tend to be enacted on a city-wide basis for each type of school (public, charter, specialized, etc.), if not all schools simultaneously. Of note, a larger share of non-PSAL schools tend to be charter schools. However, since, each charter school is relatively independent from the rest, very rarely are there education-related policies which affect all charter schools at the same time, and which would thus affect non-PSAL schools in a more significant way than PSAL schools. To my knowledge, there were no such policies enacted during the years of my sample.

VI. Main Results

VIa. Event Study

Recall that my investigation focuses only on those schools that were open at least five years before the policy and six years following, through the 2018-2019 school year. To verify my key identification assumption and confirm the necessary pre-trends, I begin my investigation by running the event study in Equation 2 for each of my outcome variables. My findings are illustrated in Figure 1.

The outcome variable of most concern is the share of students suspended. Figure 1a appears to reveal a steady, downwards sloping trend from 2007 to 2012. This downwards trend implies that between 2007 and 2012, PSAL schools were suspending fewer and fewer students relative to non-PSAL schools. However, for the first two years in my sample, 2005-2006, and the last three years before the policy, 2011-2013, the trend seems flatter, providing less evidence of a pre-trend. Regardless, given these possible pre-trends, the suspension rate effects of the policy should be interpreted somewhat cautiously. Graduation rates present a more promising figure. While Figure 1b reveals a slight upwards trend during the pre-policy years, there is sufficient noise and large enough confidence intervals to suggest this pattern is a nonissue. Lastly, Figure 1c illustrates the results of the event study for my attendance rate outcome. This figure clearly reveals no evidence of any pre-trends in attendance rates during the pre-policy years.

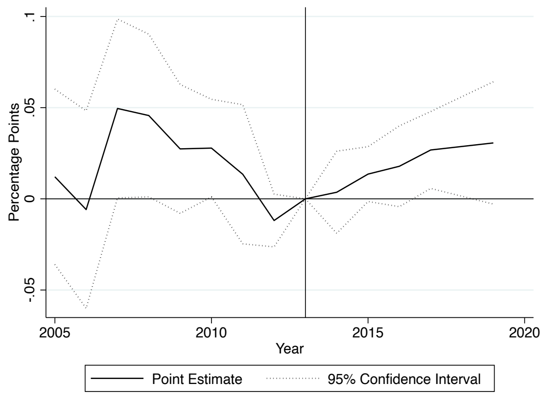

Figure 1a. Share of Students Suspended

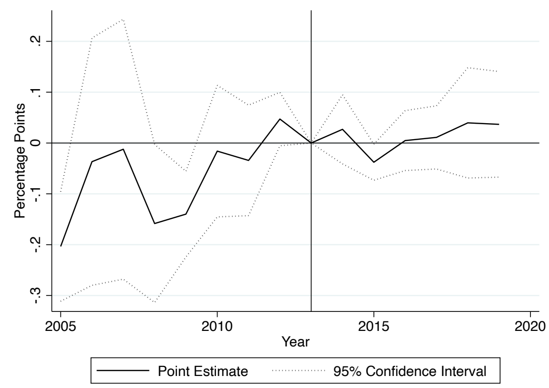

Figure 1b. Graduation Rates

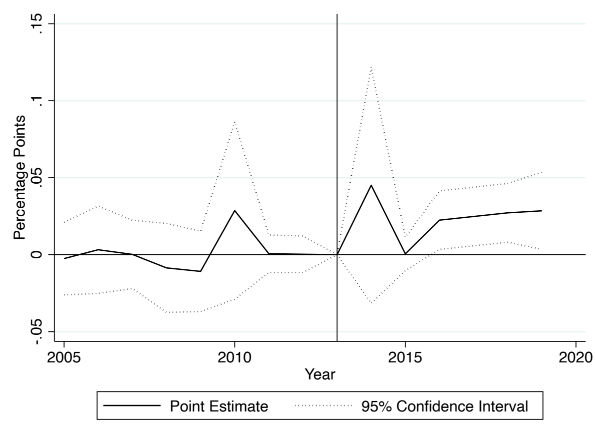

Figure 1c. Attendance Rates

Figure 1: Event Study Graphs for Each Outcome Variable

Notes: These figures show trends in graduation rates, attendance rates, and share of students suspended nine years prior and six years after the implementation of the 2013 PSAL policy. Solid lines are point estimates, and dashed lines are 95% confidence intervals. Data for attendance rates exclude the 2016-2017 school year. Data for share of students suspended exclude the 2017-2018 school year.

Given the lack of pre-trends confirmed from the event studies, I can comfortably move forward with my analyses, and particularly those for the attendance and graduation rate outcomes. My results will discuss all three outcome variables. However, I will focus my analysis on the attendance rate and graduation rate effects of the policy. This is because, in addition to the lack of pre-trends for these outcomes, their relative frequency makes them both more relevant as well as more likely to be affected by the policy. Conversely, since the share of students suspended is fairly small in magnitude, with a sample mean of 4.9 percent, it is less likely that the policy will have a direct impact on this outcome that is large enough to be detected with statistical precision.

VIb. Attendance Rates

I begin my investigation by studying the overall effect of the policy on the attendance rates of the PSAL schools. Column 3 of Table 4 demonstrates that, after controlling for my covariates and including school weights by enrollment, the policy significantly increased attendance rates by 1.8 percentage points. Column 4 provides a robustness check in which I include the full sample of schools, without any restrictions, in my regression. My point estimate remains robust and even gains significance to the 0.01 level. This 1.8 percentage point increase translates to a 3.21 school day increase in attendance. While this impact is positive, it seems small in magnitude relative to the full 180-school day year. However, further investigation suggests that this ostensibly mild effect masks significant heterogeneity across schools of different racial compositions and fractions of LEP students. In particular, running my regressions for predominantly non-White and predominantly White schools, I find that the impact of the PSAL policy differed significantly between these subgroups.

TABLE 4 - Difference-in-Difference Estimation for Effect on Attendance Rates

	Difference-in-Difference	Add Covariates	Covariates and Weight by Enrollment	Full Sample Robustness Check
	(1)	(2)	(3)	(4)
After X PSAL	0.007	0.012	0.018**	0.019***
	(0.011)	(0.013)	(0.009)	(0.006)
After	0.001			
	(0.011)			
PSAL	0.028			
	(0.007)			
School Fixed Effects	N	Y	Y	Y
Year Fixed Effects	N	Y	Y	Y
Covariates	N	Y	Y	Y
Weight by Enrollment	N	N	Y	Y
Adjusted R^2	0.01	0.57	0.81	0.82
Number of Observations	5,257	5,257	5,227	6,411

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Attendance rate data exclude the 2016-2017 school year. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

White or non-White status for a school is determined based off of that school's fraction of White students enrolled during the year before the policy, relative to the median percent of White students in a school in the sample during that same year. Schools with more than the median percent of White students are considered White, whereas schools with below the median percent of White students are considered non-White.

The coefficient in Column 2 of Table 5 suggests that for schools predominantly composed of White students, the policy significantly increased attendance rates by nearly 4.4 percentage points, or roughly 8 school days. This is the equivalent of more than 1.5 additional weeks of school. Conversely, for schools composed of predominantly non-White students, the policy appears to have had an insignificant, zero effect on attendance rates. Columns 3 and 4 demonstrate that these point estimates are largely robust to including the full sample; the 3.3 percentage point increase identified in my robustness check for White schools is the equivalent of a 6 day increase in school attendance, which is still over an additional week of schooling.

TABLE 5 - Attendance Rate Estimations for non-White/White Schools, Including Robustness Check

	Restricted Sample		Full Sample	
	Non-White	White	Non-White	White
	(1)	(2)	(3)	(4)
After X PSAL	0.000	0.044**	0.009	0.032**
	(0.006)	(0.022)	(0.006)	(0.014)
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Weight by Enrollment	Y	Y	Y	Y
Adjusted R^2	0.39	0.69	0.77	0.85
Number of Observations	1,281	1,623	3,098	3,313

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Attendance rate data exclude the 2016-2017 school year. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

One possible concern regarding the dramatic difference in treatment effect between non-White and White schools is that perhaps the overall lack of differential pre-trends actually masks heterogeneity in pre-trends. In other words, the extreme heterogeneity present in the treatment effect could be due to significantly different pre-trends and not due to the treatment itself. To explore this concern, I run the event study in Equation 2 for only predominantly White and only predominantly non-White schools respectively. The results of these event studies, illustrated in Figure 2, reveal that pre-trends are not a concern for either sample.

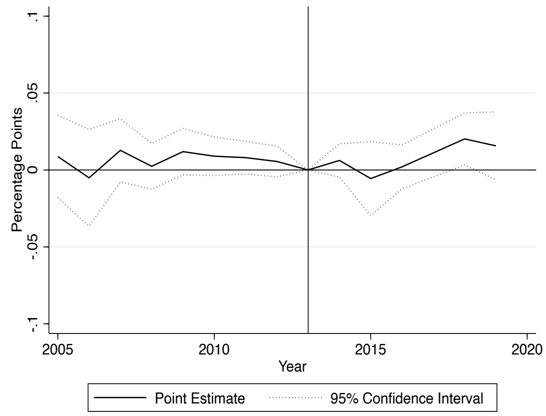

Figure 2a. Event Study for non-White Schools

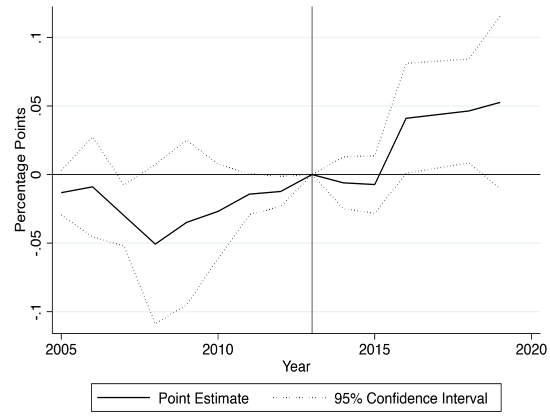

Figure 2b. Event Study for White Schools

Figure 2: Event Study Graphs for Attendance Rates in non-White/White Schools

Notes: These figures show trends in attendance rates nine years prior and six years after the implementation of the 2013 PSAL policy. Solid lines are point estimates, and dashed lines are 95% confidence intervals. Data exclude the 2016-2017 school year.

Given the stark differences in treatment effect on attendance rates by racial composition, I proceed to run a second heterogeneity test comparing schools by relative fraction of LEP students. LEP and non-LEP schools, similarly to White and non-White schools, are classified based off the percent of LEP students at the school during the year before the policy, relative to the median percent of LEP students in a school in the sample for that year. Given that LEP students tend to be not only students of color but also low-income, I expect the heterogeneity tests to yield similar, if not more dramatic, effects.

TABLE 6 - Attendance Rate Estimations for LEP/non-LEP Schools, Including Robustness Check

	Restricted Sample		Full Sample	
	LEP	Non-LEP	LEP	Non-LEP
	(1)	(2)	(3)	(4)
After X PSAL	0.007	0.022*	0.016***	0.020**
	(0.006)	(0.014)	(0.005)	(0.010)
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Weight by Enrollment	Y	Y	Y	Y
Adjusted R^2	0.81	0.76	0.82	0.78
Number of Observations	2,458	2,769	3,161	3,250

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Attendance rate data exclude the 2016-2017 school year. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

Table 6 suggests that non-LEP schools benefited significantly more than LEP schools from the policy. Specifically, the policy increased attendance rates in non-LEP schools by 2.2 percentage points, or roughly 4 days. Conversely, the policy increased attendance rates at LEP schools by only .7 percentage points, or 1 school day. The result that non-LEP schools benefited more from the policy is robust to including the full sample, although the gap in the gains between non-LEP and LEP schools shrinks to 0.4 percentage points, or ¾ a school day. To confirm that these effects are driven by the policy and not any masked pre-trends, I again run my event study from Equation 2 for only predominantly LEP and predominantly non-LEP schools respectively. The results of these event studies are illustrated in Figure 3, and suggest that there are no concerns of pre-trends for these subgroups, implying that the attendance rate effects are in fact consequences of the policy.

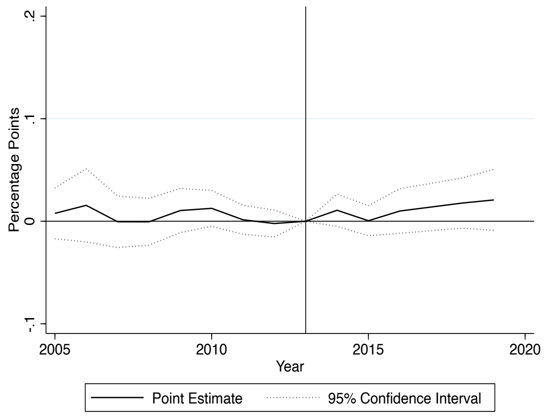

Figure 3a. Event Study for LEP Schools

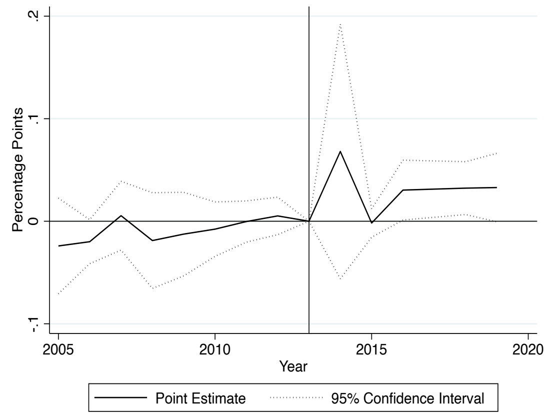

Figure 3b. Event Study for non-LEP Schools

Figure 3: Event Study Graphs for Attendance Rates in LEP/non-LEP Schools

Notes: These figures show trends in attendance rates nine years prior and six years after the implementation of the 2013 PSAL policy. Solid lines are point estimates, and dashed lines are 95% confidence intervals. Data exclude the 2016-2017 school year.

VIc. Graduation Rates

I begin my second analysis by studying the overall effect of the policy on the graduation rates of the PSAL schools. Column 3 of Table 7 reveals that the policy appeared to have a positive but statistically insignificant effect on graduation rates. Specifically, on average, the policy increased graduation rates by 3.6 percentage points in PSAL schools. These results are somewhat robust to including the full sample; column 4 summarizes a smaller but still positive effect of the policy on PSAL schools, increasing graduation rates by 1.2 percentage points.

While statistically insignificant, this result is fairly consistent with the findings of Vidal-Fernández (2011). Recall that the PSAL policy included a clause which increased the number of required passed courses by one, from four to five. Vidal-Fernández (2011) identifies that a one course increase in course-based eligibility requirements results in a 2 percentage point increase in graduation rates. Thus, our findings seem to largely agree with one another.

TABLE 7 - Difference-in-Difference Estimation for Effect on Graduation Rates

	Difference-in-Difference	Add Covariates	Covariates and Weight by Enrollment	Full Sample Robustness Check
	(1)	(2)	(3)	(4)
After X PSAL	0.053**	0.100**	0.036	0.012
	(0.02)	(0.045)	(0.043)	(0.021)
After	-0.075***			
	(0.022)			
PSAL	0.026			
School Fixed Effects	N	Y	Y	Y
Year Fixed Effects	N	Y	Y	Y
Covariates	N	Y	Y	Y
Weight by Enrollment	N	N	Y	Y
Adjusted R^2	0.01	0.36	0.43	0.43
Number of Observations	4,808	4,808	4,784	5,758

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL, LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

Before analyzing my heterogeneity tests across White versus non-White schools and LEP versus non-LEP schools, I run the event study from Equation 2 for each subgroup to confirm a lack of pre-tends. The results of these event studies are illustrated in Figure 4.

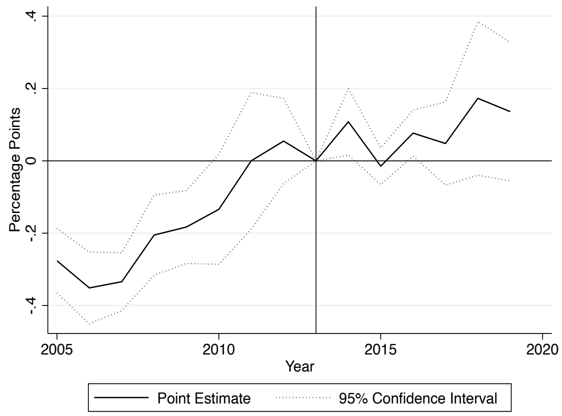

Figure 4a. Event Study for White Schools

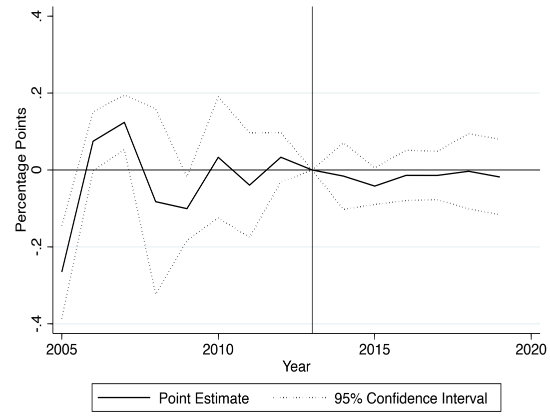

Figure 4b. Event Study for non-White Schools

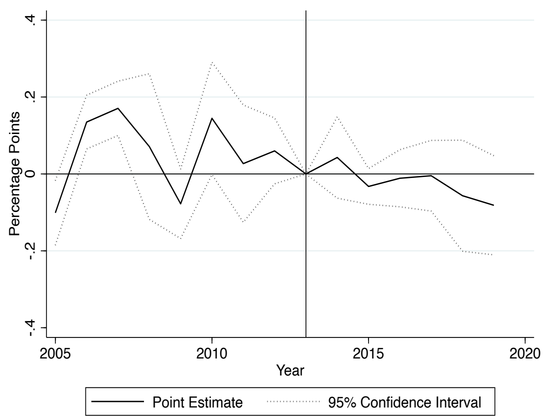

Figure 4c. Event Study for LEP Schools

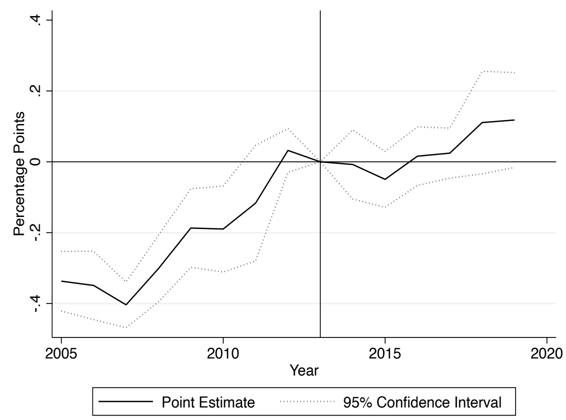

Figure 4d. Event Study for non-LEP Schools

Figure 4: Event Study Graphs of Graduation Rates for Subsamples

Notes: These figures show trends in graduation rates nine years prior and six years after the implementation of the 2013 PSAL policy. Solid lines are point estimates, and dashed lines are 95% confidence intervals.

Figures 4b and 4c reveal minimal pre-trends for predominantly non-White schools and predominantly LEP schools respectively. However, Figures 4a and 4d show more concerning pre-trends. The upwards slopes in both figures imply that for several years in the middle of the pre-policy years, treated White schools and treated non-LEP schools were experiencing increasing graduation rates relative to their corresponding control schools. Of note, for the last few years before the policy these trends seem to taper off, suggesting that the PSAL and non-PSAL schools were trending relatively similarly approaching the year of the policy. However, due to these pre-trends, the results of the regressions examining the effects of the policy on the graduation rates of predominantly White and predominantly non-LEP schools should be taken most suggestively.

TABLE 8 - Graduation Rate Estimations for non-White/White Schools, Including Robustness Check

	Restricted Sample		Full Sample	
	Non-White	White	Non-White	White
	(1)	(2)	(3)	(4)
After X PSAL	-0.010	0.148**	-0.005	0.052*
	(0.040)	(0.059)	(0.024)	(0.030)
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Weight by Enrollment	Y	Y	Y	Y
Adjusted R^2	0.35	0.48	0.35	0.46
Number of Observations	2,118	2,666	2,679	3,079

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL, LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

Table 8 summarizes the policy effects on graduation rates for non-White and White schools and reveals stark heterogeneity in treatment effect. The regressions suggest that on average, the policy yielded a significant 14.8 percentage point increase in the graduation rates of White schools, but a 1 percentage point decrease in the graduation rates of non-White schools. These results remain robust, although less dramatic, to including the full sample of schools; on average, White schools see a significant 5.2 percentage point increase while non-White schools experience a 0.5 percentage point decrease in graduation rates. Since Figure 4a revealed pre-trends for White schools in the pre-policy years, part of this large effect is likely due to the increasing graduation rates of predominantly White PSAL schools compared to predominantly White non-PSAL schools prior to the policy. However, since the pre-trends between these schools appear to taper off in the years right before the policy, it is fair to assume that the policy likely played some role in driving the large increase in graduation rates for White schools.

TABLE 9 - Graduation Rate Estimations for LEP/non-LEP Schools, Including Robustness Check

	Restricted Sample		Full Sample	
	LEP	Non-LEP	LEP	Non-LEP
	(1)	(2)	(3)	(4)
After X PSAL	-0.068	0.130***	-0.014	0.065*
	(0.047)	(0.049)	(0.022)	(0.038)
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Weight by Enrollment	Y	Y	Y	Y
Adjusted R^2	0.50	0.41	0.48	0.41
Number of Observations	2,211	2,573	2,828	2,930

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL, LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

Table 9 summarizes the results of my heterogeneity tests for graduation rates across LEP and non-LEP schools. Column 1 reveals that on average, LEP schools saw a statistically insignificant 6.8 percentage point decrease in graduation rates. Conversely, my results suggest that non-LEP schools experienced a strongly significant 13 percentage point increase in graduation rates. These results are less dramatic but remain robust to regressing with the full sample; full sample effects include a 1.4 percentage point decrease and a significant 6.5 percentage point increase for LEP and non-LEP schools' graduation rates respectively. Again, the pre-trends revealed in Figure 4d suggest that in part, some of the large positive effect on non-LEP schools' graduation rates is due to the increasing pre-trends during the pre-policy years. However, similarly to the trending of White PSAL and non-PSAL schools, the pre-trends in Figure 4d appear to diminish in the couple of years approaching the policy. As such, it not unreasonable to assume that the policy played some role in increasing the graduation rates of predominantly non-LEP PSAL schools.

VId. Share of Students Suspended

The last regressions I run are for the share of students suspended outcome variable. Recall, this variable revealed a slight downwards pre-trend in the event study figure. Thus, conclusions drawn from this analysis should be considered suggestive only.

TABLE 10 - Difference-in-Difference Estimation for Effect on Share of Students Suspended

	Difference-in-Difference	Add Covariates	Covariates and Weight by Enrollment	Full Sample Robustness Check
	(1)	(2)	(3)	(4)
After X PSAL	-0.007	0.100**	0.001	0.012
	(0.007)	(0.045)	(0.013)	(0.021)
After	-0.034***			
	(0.007)			
PSAL	-0.008			
	(0.005)			
School Fixed Effects	N	Y	Y	Y
Year Fixed Effects	N	Y	Y	Y
Covariates	N	Y	Y	Y
Weight by Enrollment	N	N	Y	Y
Adjusted R^2	0.10	0.46	0.49	0.50
Number of Observations	5,256	5,256	5,246	6,434

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Suspensions data excludes the 2017-2018 school year. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

Column 3 of Table 10 reveals that overall, the policy had a negligible effect on the share of students suspended. Before performing my heterogeneity tests, I run my event study from Equation 2 to confirm a lack of pre-trends for my subsamples. The results of these event studies are illustrated in Figure 5.

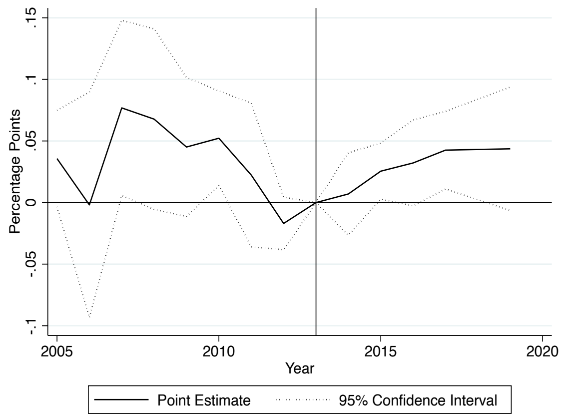

Figure 5a. Event Study for non-White Schools

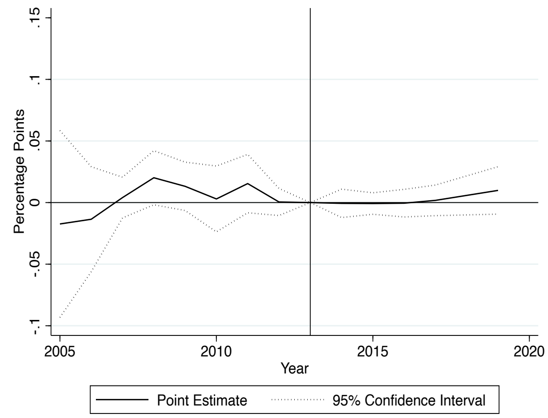

Figure 5b. Event Study for White Schools

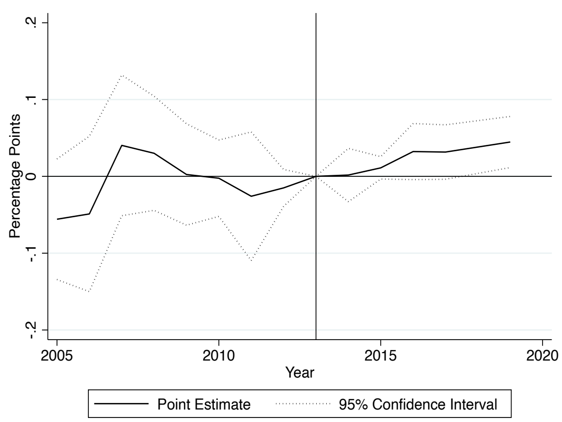

Figure 5c. Event Study for LEP Schools

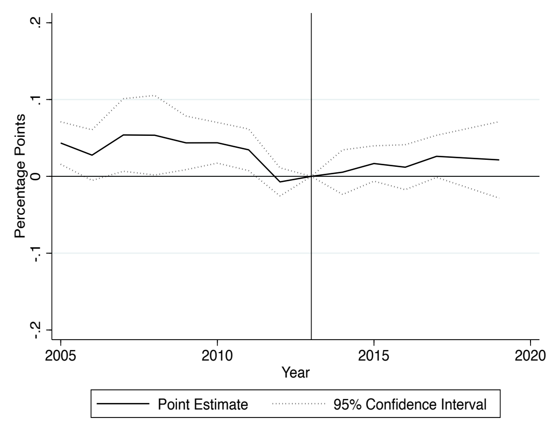

Figure 5d. Event Study for non-LEP Schools

Figure 5: Event Study Graphs of Share of Students Suspended for Subsamples

Notes: These figures show trends in the share of students suspended nine years prior and six years after the implementation of the 2013 PSAL policy. Solid lines are point estimates, and dashed lines are 95% confidence intervals. Data exclude the 2017-2018 school year.

Unlike the regression for the overall sample, the event studies for my subsamples mostly show no indication of pre-trends. In particular, Figures 5b, 5c and 5d reveal minimal trending for the White, LEP, and non-LEP schools respectively. Figure 5a reveals that on average, non-White PSAL schools were suspending fewer students relative to non-White non-PSAL schools during the pre-policy years. However, this trending seems to disappear during the few years just before the policy, slightly mitigating our pre-trend concerns. Thus, while we can comfortably interpret the results for the impact of the policy on the share of students suspended in most our subgroups, the policy's effect on non-White schools should only be taken as suggestive.

TABLE 11 - Suspension Rate Estimations for non-White/White Schools, Including Robustness Check

	Restricted Sample		Full Sample	
	Non-White	White	Non-White	White
	(1)	(2)	(3)	(4)
After X PSAL	0.001	-0.004	0.013	0.005
	(0.014)	(0.009)	(0.011)	(0.008)
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Weight by Enrollment	Y	Y	Y	Y
Adjusted R^2	0.43	0.50	0.45	0.50
Number of Observations	2,431	2,815	3,110	3,324

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Suspensions data excludes the 2017-2018 school year. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

Table 11 summarizes the results for my heterogeneity tests for the share of students suspended across non-White and White schools, and shows a similar, near zero, insignificant effect on both subgroups of schools. This zero effect remains robust to including the full sample. Thus, the policy appears to have made no effect on the share of students suspended in these schools.

On the other hand, heterogeneity tests across LEP and non-LEP schools reveal slightly more interesting results. The findings summarized in Table 12 demonstrate that on average, LEP schools saw a 2.7 percentage point increase in the share of students suspended, while non-LEP schools saw a significant 1.2 percentage point decrease in the share of students suspended. Results from the robustness check show a similar pattern in that LEP schools still seem to experience larger increases in the share of students suspended due to the policy. However, the effect on non-LEP schools is no longer negative nor is it significant, while the effect on LEP schools gains significance at the 0.05 level.

TABLE 12 - Suspension Rate Estimations for LEP/Non-LEP Schools, Including Robustness Check

	Restricted Sample		Full Sample	
	LEP	Non-LEP	LEP	Non-LEP
	(1)	(2)	(3)	(4)
After X PSAL	0.027	-0.012*	0.024**	0.004
	(0.019)	(0.007)	(0.012)	(0.008)
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Weight by Enrollment	Y	Y	Y	Y
Adjusted R^2	0.49	0.51	0.49	0.51
Number of Observations	2,472	2,774	3,177	3,257

NOTES: Each observation is at the school-year level. Each column presents results from a separate regression. Standard errors, listed in parentheses, are clustered at the school level. Suspensions data excludes the 2017-2018 school year. Covariates include fraction of students who are White, Black, Hispanic, Asian, FRL LEP, and the Student-to-Teacher ratio.
* p<0.1, ** p<0.05, *** p<0.01

VII. Discussion

My investigation suggests that overall, the policy had a positive impact on PSAL member schools. However, the policy clearly differentially benefitted predominantly White and predominantly non-LEP schools, while yielding no effect, and sometimes an even negative effect, on predominantly non-White and predominantly LEP schools. This heterogeneity between LEP and non-LEP schools specifically is consistent with the findings of work by Fryer (2011) who investigates the impact that cash incentives have on academic outcomes. Fryer finds that schools with predominantly non-LEP students saw the largest increases in quiz scores when implementing cash incentives.

Some of the most dramatic heterogeneity in effects from my investigation include a significant 4.4 percentage point, or 8 school day, increase in attendance rates for White schools and a zero, although insignificant, effect on attendance rates at non-White schools. Furthermore, my study suggests that the policy resulted in large increases in the graduation rates of White schools and of non-LEP schools. Meanwhile, non-White schools saw a slightly negative, likely zero, effect on graduation rates and LEP schools saw a 6.8 percentage point decrease in graduation rates, though both results were statistically insignificant. Since predominantly White schools and predominantly non-LEP schools appear to have experienced pre-trends in the middle of the pre-policy years, these graduation results should be taken as suggestive only. Nonetheless, their magnitude relative to the zero and negative impacts on non-White and LEP schools respectively support the finding that the policy had heterogeneous effects on graduation rates across these kinds of schools.

One potential reason for this heterogeneity by racial and LEP composition could be due to differences in school funding. A subset of the economic literature which studies disparities in school funding identifies the role that the racial composition of these schools plays in predicting their access to funding (Rothbart 2020; Sosina and Weathers 2019; Weiss 2020). In particular, the literature finds that schools composed of more non-White students on average receive less funding than those schools composed of predominantly White students. This disparity in funding is even more exaggerated between schools of predominantly LEP students and non-LEP students, given that LEP students tend to be both students of color and from low-income households. While there is no publicly available data on funding at the school-level, the NYCDOE does publish district-level data on school based expenditure-per-pupil for each of the 32 general education school districts in NYC. Data from the 2012-2013 school year, the year right before the policy, suggest that the five districts with the highest levels of spending-per-pupil on average had smaller shares of LEP students than the five districts with the lowest levels of spending-per-pupil, by 4 percentage points. Thus, the data supports the finding that predominantly LEP schools in NYC tend to have less access to funding than predominantly non-LEP schools.

School funding is relevant because it is a key indicator of a school's ability to provide high quantity and quality inputs such as small class sizes, better teacher quality, and effective classroom resources. Particularly, as students face increased academic pressure (as generated by the more severe academic eligibility requirements), small classes provide them with more attention from teachers, higher quality teachers may provide them with better support, and better classroom resources may enrich and help facilitate more effective learning. The formal economic literature on the effects of such inputs finds conflicting evidence regarding their impact on student achievement (see Hyman 2017, Krueger 2002, and Hanushek 2003). However, with the proper incentives in place, these inputs can behave as key drivers of improvement in academic outcomes (Hanushek 2003). As such, in the context of the academic incentive of athletics eligibility, it is clear how these resources may play a critical role in helping students improve their academic outcomes. Thus, higher levels of school funding can likely shed light on why the predominantly White and predominantly non-LEP schools were capable of driving such extreme improvements in academic outcomes following the policy; with more access to funding, these schools were better equipped to facilitate and promote improvements in academic achievement amongst their students. Conversely, the neutral, and in some instances negative, effect of the policy on predominantly non-White and predominantly LEP schools can possibly be attributed to removal from athletic participation. Without the funding to provide ample education inputs, these schools were likely less capable of supporting their students in the face of academic pressure. So, when confronted with more stringent academic eligibility requirements, not only were these students unable to achieve as strong improvements, but as a result, many were likely also made ineligible from athletics participation.

Consequently, one possible explanation for the negative effects seen amongst the less-resourced schools is that student-athletes at these schools were more likely to be forced to resign from sports. This removal from athletic participation meant that these students were no longer receiving the positive effects of sports participation. Furthermore, these students subsequently faced a reduced benefit of attending school. The economic literature on the impact of athletics participation reveals conflicting evidence regarding sports participation's effect on academic outcomes (Heckman, Stixrud, and Urzua 2006; West et al. 2016; Miller et al. 2005; Eccles and Barber 2001). However, this literature is largely focused on trying to account for the endogeneity issue of selection into athletics. In the case of the PSAL policy, regardless of whether or not certain kinds of students select into sports participation, removal from athletics has a clear impact on their academic lifestyle; by no longer participating in formal athletic programming, students can lose both a structure to their day as well as a larger sense of responsibility and accountability to a team. Furthermore, without the ability to participate in school-sanctioned athletics, students have less incentive to meet the baseline eligibility requirements that existed before the policy, or even attend school altogether. Those students who do select into athletics may experience this phenomenon to a lesser degree. However, for the students whose decision to join a team was exogeneous to personality traits associated with work-ethic, the negative impact of removal from athletics participation would likely be stronger.

The results of my investigation seem to suggest that removal from athletics participation did play a role in deteriorating student outcomes, particularly with regards to graduation rates. It is beyond the scope of this paper to identify by which mechanisms removal from athletics participation caused this decrease in graduation rates. However, the intuition presented above should help to provide a potential explanation for why this result is plausible.

VIII. Conclusion

The implementation of academic eligibility requirements for high school athletics participation is a common strategy used by high schools throughout the country to encourage improved academic performance by student-athletes. However, despite being intended to incentivize stronger academic performance, these requirements can sometimes discourage students by setting the bar too high, and even exclude some from being able to participate in athletics altogether. This paper aims to examine the effect that an increase in standards in academic eligibility requirements for athletic participation had on the academic outcomes of public high school students in NYC.

My results suggest that overall, the policy had no effect on the share of students suspended but improved the attendance rates and graduation rates of treated PSAL schools. However, these improvements were disproportionately driven by predominantly White schools and predominantly non-LEP schools, which saw gains as large as 8 school days in attendance rates and significant increases in graduation rates. On the other hand, non-White schools and LEP schools saw little to no improvements in attendance rates, and even negative impacts on graduation rates, with LEP schools seeing graduation rates drop by 6.8 percentage points, though statistically insignificantly, following the policy.

My findings have two key implications. First, the severe heterogeneity in treatment effect has critical consequences in the context of academic achievement gaps. It seems as though eligibility requirements intended to improve academic performance are largely successful overall. However, if these requirements are benefiting schools that are better funded while having no effect, or even harming, less funded schools, then policies that implement such requirements or enforce more strict requirements could be further driving a wedge between the academic achievement levels of the students at these respective schools. Second, recall that students who did not meet the eligibility requirements were no longer allowed to participate in formal athletic programming. The year before the policy, 62% of schools in my sample had an average attendance rate below the soon-to-be updated minimum outlined in the eligibility requirements. Furthermore, 22% of schools were a full ten percentage points below this impending minimum. This implies that a significant number of schools were likely simultaneously being affected through their removal from athletics participation. Since this was the only other explicit outcome of the policy, we can likely attribute some of the negative impacts of the policy on this removal from athletic participation. As such, my investigation seems to indirectly provide evidence on the important role high school athletic participation has on maintaining academic performance, and more specifically, the negative impact of removal from athletics participation. From a policy standpoint, this finding suggests that schools should avoid any diversions from preexisting athletic program funding, which may decrease the availability of sports programming for students. Furthermore, any policies which may interfere with a student's ability to participate in sports should be thoughtfully deliberated before being implemented.

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