# ABSTRACT

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| Title of Dissertation: | THE CAUSES AND CONSEQUENCES OF BETWEEN-SCHOOL SEGREGATION: EVIDENCE FROM THE U.S. AND CHILE |
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Between-school segregation poses significant challenges for students' short- and long-term success as well as for societal cohesion. Diversifying schools with regard to students' background characteristics is theorized by scholars from multiple disciplinary perspectives as an important social and policy endeavor. However, it can be an uphill battle due to several barriers, including residential segregation, inequitable distribution of school options, social closure dynamics, information asymmetries, and others. In my dissertation, I conduct applied quantitative analyses that speak to the potential benefits of and policy avenues toward more diverse school contexts. While the two quantitative chapters use different data sources and explore different questions, (i) they align broadly under the umbrella of the literature on between-school segregation and school diversity; (ii) they both are interdisciplinary analyses, bringing together knowledge from sociology and economics; and (iii) they both employ causal frameworks, exploiting exogenous variation derived from cross-sectional policy shocks and longitudinal demographic changes. To begin the dissertation, in Chapter 1, I review the theoretical and empirical literature on school-based segregation and diversity, arguing that key components of the theory have not yet been fully tested empirically. Chapter 2 investigates the

effects of school-based segregation and—on the flipside—school-based student diversity on short-term academic outcomes. I document changes in school racial and ethnic composition over time using statewide data from Maryland. Then, I assess the causal effects of these changes on short-term test scores and suspensions, using a value-added and a student-school fixed-effects approach. I find that, while most students benefit from being enrolled with a larger proportion of students of color, the association is non-linear and heterogenous by the student's race-ethnicity. The findings suggest that policy discussions around school diversity need to consider how potential shifts in peer composition may dissimilarly impact different students and institutional dynamics in more/less diverse schools at baseline. Chapter 3 examines the effects of a policy in Chile that prohibits selective admissions and introduces a centralized admissions lottery as a possible policy solution to between-school segregation in school choice contexts. Relying on publicly available administrative data, I exploit rollouts across regions and municipalities for identification in a difference-in-differences and event study framework. I find that prohibiting selective admissions reduces between-school socioeconomic segregation, mitigating the stratifying effects of school choice. The findings suggest that specific regulations are central for school choice systems to achieve their theorized goal of expanding schooling alternatives by breaking the link between neighborhood and segregative district boundaries. By drawing upon the findings of both papers, the dissertation highlights the normative and empirical complexities that the analysis of school segregation and diversity entails for policy and research aimed at attaining a more equitable distribution of educational opportunities.

# THE CAUSES AND CONSEQUENCES OF BETWEEN-SCHOOL SEGREGATION: EVIDENCE FROM THE U.S. AND CHILE

by

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# Dedication

I dedicate this dissertation to my wife and best friend, Johanna, whose love, company, and support have made it possible for me to get here. To our daughters, Aurora and Elena, who have grounded this journey, bringing love, warmth, and laughter. To my parents, Verónica and José, who loved me first and taught me to be persistent and generous. And to our families in Chile, who have nurtured and encouraged us over the years.

In memory of my grandparents: Silvia & Eleno, Odette & Hernán.

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# Table of Contents

# Introduction

Multiple factors including family preferences and local policy decisions result in substantial between-school segregation based on race, ethnicity, and income (Reardon & Owens, 2014). In turn, differences in school demographic composition can affect schools' and students' access to resources and exposure to social networks, leading to substantial inequities in educational opportunity and outcomes (Domina, Penner, & Penner, 2017; Reardon, Grewal, Kalogrides, & Greenberg, 2012). This dissertation combines multiple data sources, disciplinary perspectives, and empirical strategies to better understand the *consequences* of school-based segregation, its main *causes*, and *policy options* for offsetting it. Ultimately, findings from this dissertation aim at informing policy decisions and implementation that seek at reversing the course and consequences of school-based segregation.[1]

With regard to the *consequences* of between-school segregation (see Chapter 1, section 3), research shows that school-based segregation is problematic in many regards, including the extent to which students are exposed to a diverse group of peers. School peer composition, in terms of students' background characteristics, has been described as one of the main mechanisms explaining the association between segregation and key outcomes, as it directly affects the quality of educational resources in school, the political and economic power of the social networks in which students participate, and the expectations that frame teacher-student interactions (Card & Rothstein, 2007; Harris, 2010; Posey-Maddox, 2014). To date, a large

[1] As I discuss in Chapter 1, section 2, the dissertation relies on three main concepts: 1) Between-school segregation: the uneven distribution of students from different backgrounds across schools within a specific geographic or administrative territory, affecting the likelihood of their potential interactions; 2) School diversity: often used as the opposite of school segregation, school diversity refers to the degree of representation of specific social groups in schools within a territory, against a explicit or implicit standard; 3) School composition: descriptive term focused on the numeric representation of specific social groups in schools within a territory.

number of studies in the peer effects literature have documented that lack of peer diversity can negatively affect minoritized students' short-term educational outcomes (Van Ewijk & Sleegers, 2010). However, this literature has three important limitations. First, much of this work is correlational in nature (Harris, 2010; Moffitt, 2001). Studies often cannot identify and disentangle the effect of peers from other confounding variables, including the school students attend and the neighborhood they live in (Manski, 1993). Second, most studies use a linear definition of peers and exploit marginal differences (i.e., percent of minoritized students within a school) rather than considering non-linear and starker differences in peer composition, which may be more appropriate when thinking about policy implications (e.g., Epple & Romano, 2001). Finally, the peer-effects literature often identifies peers as the source of the causal effects, overlooking the impact of resources, school dynamics, or other factors that are intrinsic to peer composition.

Drawing on longitudinal education and workforce data from the Maryland Longitudinal Data System (MLDS) Center, and relying on a value-added approach and plausibly random variation in students' exposure to diverse peers over time, my first study (Chapter 2) contributes to this literature and asks*: What is the effect of racial-ethnic diversity in schools on short-term educational outcomes (i.e., test scores and suspensions)? How does this relationship changes depending on the operationalization of peer characteristics and school diversity?*

Taking into account the negative effects of school-based segregation on individuals and society, scholars and policymakers have considered several possible solutions, such as court-ordered desegregation and busing programs (Logan, Zhang, & Oakley, 2017; Lutz, 2005; Reber, 2005). Over the past several decades, some scholars and policymakers have argued that school choice is a policy alternative that may reduce school-based segregation by breaking the link between school, neighborhood, and segregative district boundaries (e.g., Chubb and Moe, 1990). In practice, however, school choice has increased between-school segregation where implemented (e.g., Bifulco & Ladd, 2007; Burguess, McConnell, Propper, & Wilson, 2004; Denice, DeArmond, & Carr, 2020; Frankenberg, Siegel-Hawley, & Wang, 2011; Glazerman, 1998; Garcia, 2008; Holme, Frankenberg, Diem, & Welton, 2013; Hsieh & Urquiola, 2006). I complement the work on the consequences of increased school diversity by further exploring the causes and possible *policy alternatives* for mitigating the segregative effects of school choice.

Scholars have theorized varied social and policy factors that can result in segregated schools in school choice contexts, including neighborhood segregation, demographic changes, non-random school admissions, and family preferences (see Chapter 1, section 4). However, rarely has empirical work been able to separate out the *unique* effect of each. The second paper (Chapter 3) in my dissertation contributes to this end by examining and teasing out the effect of non-random admission procedures (e.g., *cream-skimming*[2] and fee-charging) from other determinants. To do so, I exploit the recent implementation of centralized school admissions lotteries in a school choice setting as a natural experiment to answer the question: *What is the effect of using a centralized, lottery-based admissions system to assign students to schools on between-school segregation?* Specifically, I take advantage of a recent policy change in Chile that eliminates two common selective admissions practices—i.e., cream-skimming by ability or other background characteristics and charging tuition fees to families—and creates a centralized admission system. Relying on publicly available administrative data and the rollout of this policy, I estimate the causal effect of prohibiting selective admissions and implementing centralized lottery-based admission systems on between-school socioeconomic segregation.

[2] Cream-skimming is the practice by which higher-quality schools enroll and retain higher-ability and/or more priviledged students (Böhlmark, Holmlund & Lindahl, 2016; Epple & Romano, 2008)

Broadly, the first study shows that most students benefit from being enrolled with a larger proportion of students of color, that the association is non-linear and heterogenous by the student's race-ethnicity, and that the strong correlation between peer composition and resources directly influences students' outcomes. In turn, the second study demonstrates that prohibiting selective admissions reduces between-school socioeconomic segregation, mitigating the stratifying effects of school choice, especially in contexts of greater between-school competition. Together, these papers can inform ongoing discussions regarding school assignment policies aimed at a more equitable distribution of educational opportunities.

# Chapter 1. Literature Review: Interdisciplinary Perspectives on Between-School Segregation and School Diversity

## 1. Introduction

Educational systems are built on a fundamental paradox. On the one hand, contemporary societies have regarded education as the great equalizer, an instrument to reduce inequality, increase individual opportunities, and promote social welfare (Grubb & Lazerson, 2004). On the other, educational systems play a central stratifying role in these societies (Massey, 2007). School institutions divide and sort people into social and economic categories, reproducing rather than mitigating extant inequalities (Bowles & Gintis, 1976). As Domina, Penner, and Penner (2017) summarize, "schools are egalitarian institutions that produce social inequality" (p. 312).

One of the main reasons for this paradox, described in the theoretical literature, is driven by the sorting of students into schools when enrolling in the education system (Kerckhoff, 1995). This form of sorting is problematic for at least two reasons. First, it does not occur at random. Students end up in a determined school, with specific characteristics and peers, not by chance. Rather, sorting mechanisms systematically overlap with race, ethnicity, socioeconomic background, or immigration status (Domina et al., 2017). As scholars have documented, systematic non-random sorting of students into schools within a certain territory is the consequence of several determinants (Fiel, 2015). Historically, the most explicit and evident determinant is *de jure* segregation, that is, a legal system that forces the non-random sorting of students of different backgrounds into schools, such as the legal segregation of schools by race-ethnicity in the U.S. (Ryan, 2010). However, even in the absence of legally enforced mechanisms to sort students into schools, private choices (e.g., parental preferences, opportunity hoarding, social closure dynamics), and public policies (e.g., school district boundaries, privatization) have continuously supported the systematic separation of students based on their background, promoting *de facto* segregation (Frankenberg & Taylor, 2018).

Second, student sorting into different schools has direct consequences on students' opportunities. The systematic association of student characteristics to schools with varying levels of quality results in differential student exposure to educational opportunities, including pedagogical support and learning resources (Duncan & Murnane, 2011; Reardon, 2011; Van Ewijk & Sleegers, 2010). Specifically, researchers have extensively documented the association between social, economic, or racial imbalances in school composition and differential access to scarce educational resources, including teachers and social networks (Alexander, Entwisle, & Olson, 2007; Boyd, Goldhaber, Lankford, & Wyckoff, 2007; Buchmann, DiPrete, & McDaniel, 2008; Clotfelter, Ladd, Vigdor, & Wheeler, 2006; Harris, Rutledge, Ingle, & Thompson, 2006). The unequal distribution of opportunities incentivizes dominant groups to secure access to those scarce resources, denying access to non-dominant groups to these schools and their opportunities (Fiel, 2015). Importantly, as I discuss below, the association between composition and educational opportunities has important short- and long-term effects on students and society.

In the remainder of this conceptual framework, I review the theoretical and empirical literature on school-based segregation and school diversity, ultimately arguing that key components of the theory have not yet been fully tested empirically. This gap is driven, in part, by data challenges and limitations, as well as the fact that theory and empirical tests require interdisciplinary perspectives that bridge—in particular—across sociology and economics. For example, the reproduction of inequality is a longstanding sociological phenomenon, but one that has been explored more recently in the context of market-based reforms that often are the focus of economics. Further, scholars from different disciplinary traditions have hypothesized various policy solutions aimed at breaking the reproduction cycle; but, ultimately, understanding the effects of these solutions generally requires causal inference tools, often wielded by economists.

Therefore, this chapter aims at building an interdisciplinary perspective and argument—bridging across sociology and economics, in particular—on the causes, consequences, and potential policy alternatives to school-based segregation. Because of the interdisciplinary work, I first unpack and define key terminology, arguing that school segregation, school composition, and school diversity are interrelated but distinct concepts. Then I review the key empirical literature on the causes and consequences of and potential solutions for school-based segregation, to identify areas of consensus and gaps. This work leads me to outline specific directions for research to fill the gaps, which serve as motivation for the two empirical studies presented in chapters 2 and 3 of this dissertation.

## 2. Key Terminology: School Diversity, Composition, and Segregation

The concepts of school diversity, composition, and segregation broadly refer to how social groups are represented in specific units or organizations under analysis. In my dissertation, the units of interest are schools, and the groups of interest are students of different races, ethnicities, and socioeconomic statuses. In this context, the academic literature has often relied on the concept of *segregation* to describe the configuration of specific units in terms of the presence of individuals of certain social groups.

As Allen and Vignoles (2007) highlight, "at a general level, segregation is the degree to which two or more groups are separated from each other" (p. 645). More specifically, social scientists define *segregation* as the unequal distribution of individuals from two or more social groups across organizational units, geographical areas, or a combination of both, such that these differences in distribution affect the likelihood of interaction between members of these social groups (James & Taeuber, 1985). Paraphrasing this definition, I conceptualize *between-school segregation* as the uneven distribution of students from different social, economic, linguistic, or racial-ethnic backgrounds across schools within a specific geographic or administrative territory (e.g., districts), affecting the likelihood of their potential interactions.

The definition above combines two fundamental and interrelated dimensions of segregation: *distribution* and *interaction*. The interaction of individuals of different groups is only possible when individuals within a territory are well distributed across units (e.g., neighborhoods, schools). However, segregation depends also on the number of individuals of different groups in a given territory. Therefore, even in the presence of a perfect distribution of individuals (i.e., complete desegregation or proportional distribution of groups across units), interaction may not be possible or sufficient in the absence of enough individuals of different groups in a specific territory. In other words, from an *interaction* perspective, the *absolute* number or percentage of individuals of different groups present in a certain unit is central to the conceptualization of segregation. On the other hand, from a *distribution* lens, at the core of the conceptualization of segregation is the *relative* proportion or share of individuals of different groups that are represented in each unit, conditional on their total share in the territory.[3]

Most researchers agree that, depending on the research purpose and the intended uses of the segregation analysis, both relative (i.e., focused on distribution) and absolute (i.e., focused on interaction) definitions of *segregation* may be useful to be examined (Monarrez, Kisida, & Chingos, 2022; Reardon & Owens, 2014). For example, in the education context, the distribution

[3] This distinction between distribution (relative) vs. interaction (absolute) has important implications for the measurement of segregation. See, for example, Duncan & Duncan (1955), Gorard (2009), James & Taeuber (1985), and White (1986).

of students across schools within a territory is more likely to be directly affected by policy interventions than the absolute size of the racial, ethnic, or socioeconomic groups. In this case, relative definitions that center on the distribution of students, usually labeled as dispersion or evenness measures, may be more appropriate to use. On the other hand, the composition of schools may have direct consequences on the students' educational experiences. Then, even when the size of different social groups of students within a district may not be immediately affected by local or federal policies, characterizing, usually through exposure or isolation measures, how these groups of individuals across schools are represented in absolute terms could be relevant for the analysis of educational opportunities.

The concepts of *composition* and *diversity*, on the other hand, refer exclusively to an absolute definition of segregation, centered on the potential interaction between individuals of different social groups in a certain unit or organization, based on the actual number of individuals of these different social groups in these units (James & Taeuber, 1985). While some authors use composition and diversity as synonyms (see, for example, White, 1986), in my dissertation, I use the term *school composition* descriptively to characterize how social groups are represented in specific schools, and the term *school diversity* in a normative manner, to indicate the degree of representation of specific social groups in schools within a territory, against an explicit or implicit standard. As such, while the term *composition* may be used to describe both segregated and non-segregated schools, the term *diversity* is always used to characterize non-segregated schools, at least from an absolute interaction definition. Therefore, it is common for this literature, especially empirical research, to use *school diversity* and *school segregation* as opposites in a continuum.

Finally, I also distinguish between *diversity* and *integration.* While the concept of diversity focuses on "the [quantifiable] presence of people from different backgrounds" (Coughlan, 2018, p. 350), the term integration in the education context often refers to "organizational strategies and practices that promote meaningful social and academic interactions among [diverse] students who differ in their experiences, views, and traits" (Tienda, 2013, p. 467). The studies that form part of my dissertation cannot determine the extent to which integration or inclusion is achieved in schools with varying quantifiable presence of students with different backgrounds. As such, I do not use the terms integration or inclusion.

As described before, one of the key features of the proposed dissertation is that it explores the *consequences* and *causes* of between-school segregation and school diversity. In the next sections, I revise the empirical literature on these topics.

## 3. The Consequences of School-Based Segregation and the Benefits of School Diversity

Research reveals that increases in school income, racial or ethnic segregation are associated with rising educational inequality, deteriorated long-term outcomes such as crime and health, and increased incidences of racial and social discrimination (Altonji & Mansfield, 2011; Billings, Deming, & Rockoff, 2014; Goosby & Walsemann, 2012; Guryan, 2004; Jencks, 1973; Johnson, 2011; Mattia, 2021; Mayer, 2002; Owens, 2018; Rao, 2019; Reardon, 2011; Sampson, 2008). Meanwhile, desirable outcomes such as prosocial behavior, civic engagement, and social cohesion are diminished as school segregation expands (Mickelson & Nkomo, 2012; Orfield & Lee, 2005; Putnam, 2004).

In turn, a wide body of research suggests that increased diversity in schools based on race, ethnicity, and native language is essential for improving the outcomes of minority and minoritized populations (Reardon & Owens, 2014). This section summarizes the available evidence on the social and individual benefits of school diversity.

### *3.1. Evidence on the Social Benefits of School Diversity*

While most research focuses on documenting individual-level benefits of school diversity, many argue that school diversity has value in itself, based on the role that schools play in creating public goods (Labaree, 1997) and the social externalities derived from diversity (Mickelson & Nkomo, 2012; Oder, 2005).

Researchers have documented the social benefits of school diversity in three main areas. First, school diversity has been associated with increased social cohesion. As proposed by Chan and coauthors (2006), social cohesion can be defined as "a state of affairs concerning both the vertical and the horizontal interactions among members of society as characterized by a set of attitudes and norms that includes trust, a sense of belonging and the willingness to participate and help, as well as their behavioral manifestations" (p. 290). In order to increase trust, sense of belonging, and willingness to participate among individuals, several scholars have identified schools as playing a central role in modern democracies (see, for example, Putnam, 2004; Green, Preston, & Janmaat, 2006). As Braddock and Del Carmen (2010) empirically show, early racial isolation in schools is associated with diminishing levels of social cohesion among young adults in multigroup societies. Using data from the National Longitudinal Survey of Freshmen, the authors document that participant first-year students who enrolled in racially isolated K-12 schools (i.e., schools with limited racial-ethnic diversity composition) were more likely to feel socially distant from peers of other race-ethnic backgrounds as young adults than their counterparts that were enrolled in more diverse schools. At the same time, students in socially or racially isolated schools were more likely to prefer having same-race neighbors and same-race classmates.

Second, as scholars have further documented, these attitudes towards peers translate into the type of interactions and behaviors toward minoritized peers that white or socioeconomically advantaged students enact. For example, Merlino and colleagues (2019) document that white students are more likely to have more meaningful relationships with Black individuals as adults if they had larger shares of same-gender Black peers at school. Similarly, while not directly in a K-12 environment but in the U.S. Force Academy, Carrell and coauthors (2019) find that college diversity is causally linked to an increment in the likelihood of white students choosing Black roommates and having more non-same-race interactions in the future. Further, Rao (2019), taking advantage of a natural experiment in Indian schools where a policy forced elite schools to offer places to poor students, concludes that rich students with poor classmates are less likely to discriminate against poor peers, more likely to socialize with them, and more likely to exhibit prosocial, generous, and egalitarian behaviors.

Finally, a smaller body of literature has attempted to understand the association between school diversity and political beliefs, with conflicting results. While some studies show that having more minoritized peers is associated with less likelihood of identifying as politically conservative (Billings, Chyn, & Haggag, 2020; Mattia, 2021) and more likely to support affirmative action (Boisjoly, Duncan, Kremer, Levy, & Eccles, 2006), others have documented the opposite effects. For example, Chin (2020) compares the political beliefs of white adults who were differentially exposed to racial desegregation in the U.S., concluding that exposure to desegregated schools increased conservatism and decreased support for affirmative action. However, as the author discusses, these contradictory results may be explained not by diverse schools but by varying political opposition to court-mandated desegregation in different communities.

### *3.2. Evidence on the Individual Benefits of School Diversity*

On top of the already discussed effects on social attitudes, scholars have documented other short-term nonacademic outcomes associated with school diversity. There is a broad literature that focuses on the effects on individuals' psychological and physical health and disruptive, violent, or criminal behaviors. For example, pioneering work by Clark and Clark (1939, 1950) and Wertham (1952) documented the effects of school racial isolation on the well-being of Black students in the U.S. More recently, an observational study by Juvonen, Kogachi, and Graham (2018) in California shows a significant association between school-level racial-ethnic diversity and improvements in students' sense of vulnerability. In this study, students in more diverse schools report feeling safer in school, being less victimized, and feeling less lonely. Likewise, Schachner and coauthors (2016) document better well-being and fewer psychological and behavioral problems of immigrant students enrolling in more diverse and pluralistic schools in Germany. Scholars also have documented an association between school diversity and individual violent or disorderly behavior at school, though findings have been mixed. While some authors document a negative correlation racial or socioeconomic isolation and episodes of violent behavior at school (e.g., Eitle & Eitle, 2003; Akiba, LeTendre, Baker, & Goesling, 2002) or between racial isolation and juvenile arrests (e.g., Eitle & Eitle, 2010), others report a positive correlation between racial isolation and disorderly behavior at school (e.g., Stretesky & Hogan, 2005). Importantly, as Eitle and Eitle (2010) suggest, the positive or negative association seems to be mediated by the contextual inequality in which schools operate: as inequality increases, within-school conflicts and tensions in more diverse schools also increase.

A related body of research aligned to the human capital literature has focused on the academic and economic outcomes derived from school-based diversity versus segregation. My first dissertation paper contributes to this body of research directly. Equity-based research has raised concerns about significant achievement gaps between racial, ethnic, and language-minority populations relative to white, native English speakers (Fryer & Levitt, 2004; Jencks & Phillips, 2011; Reardon & Galindo, 2009). As research suggests, these differences in achievement may be partially explained by the association between school diversity and short-term educational achievement. With a few exceptions (e.g., Rivkin, 2000), many peer-reviewed articles and international reports have documented a higher probability of increased test scores, grades, high school graduation, and educational aspirations when students enroll in racially, ethnically, or socioeconomically diverse schools (e.g., Benito, Alegre, & González-Balletbó, 2014; Hattie, 2002; Mickelson & Nkomo, 2012, Orfield, 2001; Ryabov & Van Hook, 2007; Schofield, 1991, 1995; Sykes & Kuyper, 2013; Teltemann & Schunck, 2016; Wilkinson et al., 2000). As Van Ewijk and Sleegers (2010) conclude from a meta-analysis of 30 studies, while the specific association varies depending on how the composition is measured and the models are estimated, school socioeconomic diversity is associated with an average weighted effect size of 0.32 SD on student achievement.

Likewise, literature on school diversity's long-term academic and economic effects has suggested a positive association. For example, Altonji and Mansfield (2011), using longitudinal data from three different U.S. surveys, document a relationship between school stratification, college enrollment, and adult wages. Similarly, Goldsmith (2011) shows that students in minority-concentrated schools are more likely to attain less education in the long run. As Orfield and Lee (2005) and Wells (1995) summarize, this literature consistently reports increments in individuals' education and occupational attainment associated with increased school socioeconomic and racial diversity.

However, most studies documenting short- and long-term academic and economic consequences of school diversity are either descriptive or correlational in nature, producing estimates that are potentially biased (Harris, 2010; Moffitt, 2001). As I discussed before, bias comes mainly from the non-random sorting of families and students into neighborhoods, schools, and classrooms (Epple & Romano, 2011; Manski, 1993; Moffitt, 2001).

The smaller body of literature that reports credible causal estimates of academic and economic short- and long-term effects of school diversity can be organized into two blocks, depending on the source of variation the articles exploit. On the one hand, several studies use U.S. court-mandated desegregation rulings or their reversals. In these cases, researchers use the timing of the rulings as a source of exogenous variation (Reardon & Owens, 2014). For example, Guryan (2004) reported reduced high school dropout rates for Black students after court-mandated desegregation. Likewise, Johnson (2011), relying on the timing of the desegregation plans over three decades and sibling differences, documents an increment in educational and occupational attainments, college enrollment, college quality, and adult earnings as a result of desegregation. Conversely, Billings, Deming, and Rockoff (2014) provide evidence of the negative effects on educational attainment after the ending of busing and consequent resegregation of schools in Charlotte-Mecklenburg, and Lutz (2011) record increments of Black students' dropout rates following the dismissal of desegregation court-orders in the South.

The research question that the studies above aim to answer is focused on the causal effect of (de)segregation on different outcomes. Segregation in those studies refers not directly to the actual composition of schools and classrooms but to amendments to *de jure* segregation policies.

In other words, the focus of those articles is primarily the policy change itself. In the absence of these policy differences, scholars have focused on changes in school composition, relying on other sources of arguably exogenous variation. Some studies have used neighborhood lotteries (e.g., Sanbonmatsu, Kling, Duncan, & Brooks-Gunn, 2006; Schwartz, 2010). Others have relied on within-school year-to-year random variation (e.g., Bifulco, Fletcher, & Ross, 2011; Hanushek, Kain, & Rivkin, 2009; Hoxby, 2000). Finally, a third group has relied on students' reassignments due to school consolidation or resegregation plans (e.g., Hoxby & Weingarth, 2005). Results of these studies are mixed, with reports of positive, negative, and non-significant effects of diversity and peer composition on students' academic outcomes.

## 4. The Causes of School-Based Segregation and Policy Alternatives

Along with demographic changes (Mickelson, 2014; Owens, 2020), neighborhood segregation has been documented as one of the main determinants of between-school segregation by race, ethnicity, or income in different contexts (Caetano & Macartney, 2020; Denton, 1995). This association is explained both by parents' preference for enrolling their children in schools close to their homes (Burgess, Greaves, Vignoles, & Wilson, 2015) and by the unequal drawing of school attendance boundaries (Monarrez & Chien, 2021). As such, policy solutions for offsetting school-based segregation generally have focused on providing students and families with opportunities to enroll in schools outside of their own neighborhoods.

In recent decades, a primary policy tool aimed at offsetting school-based segregation resulting from neighborhood segregation is school choice. School choice advocates have argued that a system that allows families to cross school neighborhood or district lines might increase educational opportunities for disadvantaged students and reduce between-school segregation (Finn, 1990; Moe, 2001). However, with a few exceptions (Enami, 2017; Gorard, Fitz, & Taylor, 2001; Taylor & Gorard, 2001), most research coincides with the fact that, after controlling by residential segregation (Lindbom, 2010; Owens, 2020) or population shifts (Baum-Snow & Lutz, 2011), school choice has increased between-school segregation (Bifulco & Ladd, 2007; Burguess, McConnell, Propper, & Wilson, 2004; Denice, DeArmond, & Carr, 2020; Frankenberg, Siegel-Hawley, & Wang, 2011; Glazerman, 1998; Garcia, 2008; Holme, Frankenberg, Diem, & Welton, 2013; Hsieh & Urquiola, 2006; Phillips, Larsen, & Hausman, 2015; Yang Hansen & Gustafsson, 2016). As Roda and Wells (2013) summarize, unless school choice systems are intentionally designed to desegregate by race or socioeconomic status, they will likely maintain or increase between-school segregation.

Student placement in a school choice system should be, in theory, solely a function of parental preferences, available school seats, and contextual conditions such as distance to those seats (Chubb & Moe, 1990). However, in practice, parental choices become restricted in many choice settings by the direct or indirect action of schools, their communities, and policies in place. In this context, the association between school choice and increased segregation can be explained by the interaction between the demand for seats and the supply of those seats, and how that relationship is mediated by policies and institutions (Saporito, 2003).

### *4.1. Demand-Side Determinants of School Segregation in Choice Systems*

Researchers have highlighted several contributing factors to between-school segregation in a school choice setting regarding the demand for seats. First, in many school choice systems, not all families participate (Carlson, 2014; Henig, 1995; Hill, 2016). For example, Holme and Richards (2009) show that the likelihood of taking advantage of inter-district choice in Denver increases with the student income. Likewise, in a study that investigates prekindergarten application patterns in Boston, Shapiro and colleagues (2019) document systematic differences in terms of race-ethnicity, socioeconomic background, and language at home between appliers and non-appliers, with a higher proportion of minoritized, low income, and dual-language families in the latter group. Similar results have been found in different school choice systems, including in Rio de Janeiro and Santiago de Chile (Alves, Elacqua, Koslinki, Martinez, Santos, & Urbina, 2015), as well as in the Netherlands (Lutz, 1996).

Second, scholars document self-segregation across minoritized and non-minoritized groups as an essential source of between-school stratification among parents participating in the choice system. Self-segregation, that is, the practice by which families sort and isolate themselves into segregated schools (Bifulco & Ladd, 2007), has been associated with parental ideological (i.e., religious or pedagogical), practical (e.g., distance), and school composition (i.e., race, ethnicity, or socioeconomic characteristics) preferences (Denessen, Driessena, & Sleegers, 2005). According to Bifulco and coauthors (2009), self-segregation is the result of two interrelated behaviors: *outgroup avoidance*, that is, individuals' decisions of placement that are motivated by escaping other individuals that they identify as a threat (Billingham & Hunt, 2016; Iceland and Sharp, 2013), and *neutral ethnocentrism*, that is, individuals' decisions of placement that seek to interact with people that these individuals identify as of their own group (Clark, 1992; Krysan, 2002).

Understanding to what extent parental preferences are explicitly motivated by the racial, ethnic, or socioeconomic composition of schools is difficult. School preferences are complex (Ellison & Aloe, 2019) and stratified by various social and cultural markers (Gallego & Hernando, 2008; Hastings, Kane, & Staiger, 2005; Jacobs, 2013). Further, in the absence of good information, parents may use these social markers as proxies of program quality (Rowe & Lubienski, 2017). Also, social desirability may affect parental responses to surveys in which they are asked to state choice preferences (Schneider, Teske, & Marschall, 2000). For these reasons, some studies have relied on comparing stated—i.e., what parents declare as desired school characteristics—and revealed preferences—i.e., the actual characteristics of their choice set (Frankenberg, Kotok, Schafft, & Mann, 2017). For example, Elacqua and coauthors (2006) show that parents in Chile declare that, when choosing a school, school quality was at the center of their preferences and that school composition was not a relevant feature. However, on average, their choice set had significant variance in terms of quality but little in terms of students' demographic characteristics (op. cit). Furthermore, Ladd and Turaeva (2020) show that, even after controlling by distance, academic performance, services offered, and school mission, revealed preferences of charter school switchers in North Carolina lead to increased racial isolation of these students.

Social closure and opportunity hoarding dynamics may further explain parental preferences that result in self-isolation. These two concepts—i.e., social closure and opportunity hoarding—refer to a similar idea: in a context of scarce resources and competition for status, communities that have the social, cultural, or economic means isolate themselves and restrict access to others in order to maintain their position of privilege (Fiel, 2013; Hanselman & Fiel, 2017). As Fiel (2015) argues, "closure confers advantages on insiders by granting them access to resources and opportunities possessed by the group, such as esteem, access to desired social positions, or material resources" (p. 128). Importantly, this dynamic is particularly relevant in contexts of greater competition between schools (Fiel, 2015). As such, closure and hoarding are especially relevant in school choice contexts. According to Sattin-Baja and Roda (2020), school choice systems allow advantaged parents to exercise their privilege. Among other mechanisms, privileged parents game priority systems (e.g., gentrification in contexts of in-boundary priority), take advantage of schools that receive "gifted" or "specialized" labels (e.g., paying for test preparation), and invest time in navigating the choice system itself (e.g., hiring school choice consultants).

The mechanisms described by Sattin-Baja and Roda (2020) as pathways by which parents exercise their privilege in choice systems connect with the last demand-side determinant of segregation: differential access to information. Clear and accessible information about school alternatives induces choosing higher-performing schools in the first place (Elacqua & Fabrega, 2004; Hasitngs & Weinstein, 2008; Koning & Van der Wiel, 2013) or moving to better quality schools later in the school trajectory (Rich & Jennings, 2015). On the other hand, reliable and transparent information on the processes, steps, and priorities of the choice system allows parents to take advantage of the potential advantages of the system (Candipan, 2020). Unfortunately, the quality and availability of both pieces of information are stratified by socioeconomic status (Corcoran, Jennings, Cohodes, & Sattin-Baja, 2018), impacting the quality of decisions that families make and the overall stratification of the education system.

### *4.2. Supply-Side Determinants of School Segregation in Choice Systems*

Between-school segregation in choice systems is also a result of the actions of suppliers, namely, schools. In theory, competition between schools to enroll students in a choice system should lead to greater efficiency and equality by improving instruction and better aligning schools and families (Chubb & Moe, 2011; Fuller, Gawlik, Gonzales, & Park, 2003). However, depending on the incentives that schools face in specific contexts, a choice system may induce schools not to increase productivity but to enroll more advantaged students, which are less costly to work with, resulting in the social stratification of schools (Hsieh & Urquiola, 2006).

Specifically, researchers have identified two main mechanisms that school organizations can use to target relatively more advantaged populations which ends up increasing segregation in school choice contexts. [4] First, location. As Burdick-Will, Keels, & Schuble (2013) show, school openings and closings in a choice context are not done at random, potentially leading to segregation. Considering that home-to-school distance—due to safety, transportation, and time constraints—is one of the main factors determining parental school choices (Jacobs, 2013) and that some choice systems have an in-boundary preference built-in (Sattin-Bajaj & Roda, 2020), school location connects back student sorting to neighborhood characteristics, reinforcing the association between residence and school segregation.

Second, selectively admitting students or *cream-skimming*. Cream-skimming has been done by school organizations either by implementing admission tests (Contreras, Sepúlveda, & Bustos, 2010; MacLeod & Urquiola, 2015) or by conditioning admission based on family characteristics' that correlate with wealth, such as marital status (Carrasco, Gutierrez, & Flores, 2017; Karsten, Ledoux, Roeleveld, Felix, & Elshof, 2003). Selective admissions have also operated through fee-charging—either via vouchers or low-fee private schools (Bonal & Bellei, 2018), directly stratifying families based on their socioeconomic status.

## 5. Directions for Research

My dissertation—which includes two journal-article style studies—builds on the literature described above and aims to fill several concrete gaps. In this section, I summarize the gaps and provide an overview of how the studies aim to fill those gaps.

[4] A recent book by Mommandi and Welner (2021) identify different practices that charter schools in choice contexts implement to shape their enrollment, including marketing strategies, parental volunteering requests, dissuading applicants, or selectively pushing out students after enrollment, among others.

The first paper in my dissertation seeks to fill three critical gaps in the quantitative literature on school diversity and peer effects. First, as described before, while there is a large body of literature examining the relationship between peer composition and students' own outcomes, much of this work is correlational in nature (Harris, 2010; Moffitt, 2001). Studies often cannot identify and disentangle the effect of peers from other confounding variables, including the school students attend and the neighborhood they live in (Manski, 1993).

Second, most studies use a linear definition of peers and exploit marginal differences (i.e., percent of minoritized students within a school) rather than considering non-linear and starker differences in peer composition, which may be more appropriate when thinking about policy implications. As Sacerdote (2011) points out, mixed evidence on human capital peer effects literature is likely associated with what kind of peer effects (e.g., ability, socioeconomic status, etc.) are measured and how peer composition is operationalized. The first paper in my dissertation aims to contribute to the discussion on the operationalization of school diversity and peer effects. Specifically, following Sacerdote's (2011) typology, this paper will explore five models of peer effects: *linear in means* (i.e., the effect of the average of peers' background characteristics based on race, ethnicity, and native language); *invidious comparison* (i.e., the effect of being exposed to a majority of students with a different background); *boutique/tracking* (i.e., the effect of being surrounded by others like themselves); *focus* (i.e., the effect of a homogeneous context); and *rainbow* (i.e., the effect of a heterogeneous context).

Finally, the peer-effects literature often identifies peers as the source of the causal effects. Disentangling the unique effect of peers from the influence of resources, school dynamics, or other factors that are endogenous to peer composition is conceptually and methodologically difficult (Sacerdote, 2011).

The second paper focuses on selective admissions in school choice contexts. Further, it explores the effects of prohibiting these selective admission practices and introducing centralized admission lotteries as a policy tool to mitigate between-school segregation. Attending overall inequities in the assignment of students to seats, and, in particular, cream-skimming and non-random admission concerns in school choice systems, a growing number of countries (e.g., Chile, Ecuador, Sweden) and school districts in the U.S. (e.g., New Orleans, Charlotte-Mecklenburg, New York, Boston, Washington DC) have introduced systemwide admissions lotteries. Several researchers have relied on these lotteries to estimate the effects of schools or groups of schools (e.g., charters) on students' academic outcomes, using these lotteries as an instrument to reduce selection bias concerns (see, for example, Abdulkadiroğlu et al., 2011; Deming et al., 2014; Hastings et al., 2009; Stasz et al., 2007). However, as Monarrez and Chien (2021) point out, there are no evaluations that focus on the effects of prohibiting selective admissions in the first place or that exploit variation in the timing of introduction of these centralized seats' assignment mechanisms to systematically assess the general equilibrium effects of their implementation on between-school segregation. The second paper of my dissertation will contribute to this gap. As such, it will contribute to the broad literature on the causes and determinants of between-school segregation. Relatedly, this paper will contribute to the smaller body of literature that focuses on the unintended consequences of market-based reforms and the introduction of school choice and charter schools, where the implementation of centralized lotteries as a policy alternative should have a particularly stronger impact due to competition between schools (Epple & Romano, 1998, 2008).

# Chapter 2. School Diversity, Academic Outcomes, and the Operationalization of Race-Ethnicity Peer Effects in Education

## 1. Introduction

Over the last several decades, researchers and policymakers have been acutely aware of changing demographics in the United States, primarily driven by changes in fertility rates as well as international and local migration trends, resulting in changes in school composition throughout the country (Cillufo & Cohn, 2018; Devine, 2017; Frey, 2018; Orfield, Frankenberg, & Lee, 2003). In turn, contemporary researchers continue to wrestle with diverse—sometimes divergent—perspectives regarding the relationship between school diversity and educational opportunity, as well as the implications of these relationships for policymaking aimed at reducing inequality.

In particular, some authors argue for the potential benefits of increased school diversity if this translates into a more equitable distribution of educational resources found predominantly in white or affluent schools (Harris et al., 2006; Herring, 2009). This notion is based on the fact that isolation of students of color in segregated schools has been historically associated with lower per-pupil spending, higher pupil-teacher ratios, worse school infrastructure, and less access to instructional resources (Condron & Roscigno, 2003; Darling-Hammond, 2013; Mickelson et al., 2013). In recent years, progressive funding formulas have attempted to break this association between segregation and access to resources (Gamoran & An, 2016). However, in many settings the association between school composition and access to resources persists (e.g., Lagos, Galindo, & Zhao, 2021), and there are some signs that compensatory funding attempts are losing ground (Bischoff & Owens, 2019). For some, increasing school diversity is a potential way to secure equitable funding, which in turn may positively impact educational and social outcomes (Orfield, 2001; Orfield & Lee, 2005; Wells, 1995).

In contrast, a smaller body of research argues that homogeneous schools and classrooms may better target students' educational needs. As Mickelson and Nkomo (2012) summarize, some studies show that minoritized students' academic outcomes benefit from homogeneous learning environments by increasing their educational aspirations, self-efficacy, and self-esteem (e.g., Frost, 2007; Massey, 2006; Oates, 2004). In addition, some scholars have documented that students of color have less access to rigorous courses in diverse schools, as compared to their peers in schools where students of color are in the majority (e.g., Diette, 2012). At the same time, other researchers have documented that early and persistent racial discipline and exclusion gaps (Gopalan & Nelson, 2019; Mendez and Knoff, 2003; Skiba et al., 2002; Wallace et al., 2008) may be exacerbated in integrated or racially diverse school spaces (e.g., Freeman & Steidl, 2016), negatively impacting academic outcomes of students of color (Pearman et al., 2019). As Hailey (2022) discusses, one reason that minoritized families may choose to enroll their children in demographically homogenous environments is to avoid racial discrimination in academic and disciplinary practices. This perspective also highlights the benefits that students may experience when they share racial or ethnic-specific forms of language, social support, or views on adult authority with teachers, peers, and the larger school community (Borman & Pyne, 2016; Carter, 2016).

However, most of this literature is correlational and cannot establish a causal link between school diversity and academic outcomes (Harris, 2010; Moffitt, 2001). There are multiple reasons for this gap, related to data availability, measurement, and the overall difficulty of establishing causal links without random assignment (Angrist, 2014; Paloyo, 2020). With regarding to causal modeling, there is a small subset of studies that exploits the random assignment of students to schools or classrooms (e.g., Duflo, Dupas, & Kremer, 2015), court-mandated desegregation shocks (e.g., Guryan, 2004; Johnson, 2011), or other policy interventions such as school consolidations (e.g., Hoxby & Weingarth, 2005) to make causal claims regarding the impact of peer composition. Random assignment generally is considered the gold standard or causal inference in the social sciences. At the same time, these sorts of studies are relatively rare given the widespread phenomenon discussed in the first chapter on non-random sorting of students into schools. There also is discussion in the empirical peer effects literature that random assignment studies may provide peer effects estimates biased towards zero because the random assignment process is meant to ensure balanced peer composition across schools or classrooms (Feld & Zölitz, 2017).

A second limitation of the empirical literature relates to the operationalization of peer effects. Most of this literature uses a linear-in-means approach to operationalize peer composition and evaluate the potential impact of changes to this composition. Methodologically, this approach assumes that peer effects are incremental and that any shift in peer composition will have the same impact on students no matter the baseline. However, this perspective does not align entirely with the theoretical literature, which describes different theories on how different sets of students may benefit (or not) from distinct student compositions. Consider three types of schools: one type that is homogenous in terms of demographics and where an individual student is in the majority; another type that is homogenous but where a given student is not in the majority; and a third type that is diverse insomuch as the demographics match those of the state (or other areas). As discussed before, scholars that focus on the importance of cultural relevance or non-discrimination highlight the potential benefits of being enrolled in homogeneous schools where the individual student is in the majority (e.g., Carter, 2016). On the other hand, scholars that focus on how exposure to white-affluent peers determines access to resources may focus primarily on the advantages of an increased proportion of white-affluent students, even if that means being enrolled in homogeneous schools where the individual student is not in the majority (e.g., Harris et al., 2006).

In turn, Sacerdote (2011) proposes that non-linear relationships may be a better way to conceptualize, operationalize, and evaluate the effects of peer composition. The author (op. cit.), after describing a traditional *linear in means* approach (i.e., the effect of the average of peers' background characteristics based on race, ethnicity, and native language), suggests different non-linear models of peer effects, including *boutique/tracking* (i.e., the impact of being surrounded by others like themselves); *rainbow* (i.e., the effect of a heterogeneous context); *invidious comparison* (i.e., the effect of being exposed to a majority of students with a different background); and *focus* (i.e., the effect of a homogeneous context).

While the proposal for examining non-linear relationships has existed for over a decade, the empirical literature is slim. To my knowledge, my study is the one of the firsts to examine this topic in the context of race-ethnicity peer effects.[5] A likely reason for this is that examining non-linearities in a causal inference framework is challenging. Given the challenges of random assignment described above, most causally-oriented analyses of peer effects by combining panel data with a rich set of fixed effects for schools, students, and cohorts, among others (e.g., Hanushek, Kain, & Rivkin, 2009). These studies exploit changes in peer composition within schools or within students over time and across cohorts. However, this approach naturally

[5] Several studies have evaluated non-linearities in the context of ability peer effects. See for example Burke & Saas (2013), Duncan et al. (2005), Sacerdote (2001), Sund (2009), Tincani (2017). Hoxby (2000) and Hoxby & Weingarth (2005) examine heterogeneous peer effects based on race-ethnicity but does not model race-ethnicity peer composition non-linearly.

focuses on marginal differences in school or class composition, rather than contrasting large differences in peer composition (such as the three scenarios described above) that are more policy relevant (Epple & Romano, 2011).

The third limitation of this literature, which this study aims to address, is that it often identifies peers as the source of the causal effects, suggesting an association between peer composition and differences in school climate or learning environments (Harris, 2010), or between peer composition and differences in social capital and access to dominant cultural forms (Borman & Dowling, 2010; Feldman, 2019; Goldsmith, 2011). However, disentangling the unique effect of peers from the influence of resources, school dynamics, or other factors that are endogenous to peer composition is conceptually and methodologically difficult (Sacerdote, 2011). Furthermore, trying to isolate the unique contribution of peers may not be the right or more productive approach. As many scholars have documented, peer composition is directly correlated with resources (e.g., Reardon & Owens, 2014). This strong association does not occur by chance but as the result of systematic inequality in the education system (Domina et al., 2017; Ladson-Billings, 2006). In that context, a potential approach is to directly explore the association between peers and resources and examine the different ways in which this association affects the outcomes of interest. Specifically, I explore two possible resources where I have access to state-wide data: (i) the proportion of a school's teaching staff from different racial/ethnic backgrounds, given that access to same-race/ethnicity teachers is widely viewed as a resource to support academic and social outcomes (Redding, 2019); and (ii) per-pupil expenditures, which historically only have been available at the district level but recently have become available at the school level in Maryland (Edunomics Lab, n.d.).

To fill these gaps in the literature, I take advantage of recent changes in demographic composition and the availability of longitudinal data in the state of Maryland to ask: What is the relationship between racial-ethnic diversity in schools and short-term educational outcomes (i.e., test scores, suspensions)? How does this relationship changes depending on the operationalization of peer characteristics and school diversity?

Relying on demographic variation in Maryland and on a value-added methodological approach, I estimate the effects of school peer composition for the three largest populations of students (i.e., Black, Hispanic, and white). I rely both on a continuous (i.e., percentage of students of color) and a categorical (i.e., quintiles based on the distribution of students of color) measure of peer composition. The value-added approach allows me to examine the relationship between peer composition and student outcomes *across schools* that differ substantially in their student bodies, rather than focusing on marginal differences only.

I document positive and statistically significant effects on end-of-year math and ELA test scores for Black and white students enrolling in schools with a higher proportion of students of color (non-white), using the percentage of students of color as a continuous measure. I confirm this association by estimating a model with a categorical variable of school composition. Compared to being enrolled in a diverse school, representative of the state population, Black and white students benefit from being enrolled in schools with a substantially larger number of students of color. For Hispanic students, using the continuous measure yields non-significant results. When turning into the categorical variable, however, I document positive and statistically significant effects on end-of-year math and ELA test scores for Hispanic students enrolled in schools with either a larger proportion of white students, on the one hand, or with a larger proportion of students of color, on the other, as compared to being enrolled in a diverse/representative school.

Further, in terms of the likelihood of being ever suspended, I show that Black, Hispanic, and white students benefit from being enrolled in schools with a larger concentration of students of color. Both using a continuous and a categorical measure of school diversity, the likelihood of being ever suspended statistically significantly declines when the proportion of students of color increases. All the results summarized above are robust to a specification using a fixed-effects approach instead of a value-added model.

For the three groups of students and for the different outcomes, the association is non-linear. First, the association not always goes in the same direction. This is especially true in the case of Hispanic (at both ends of the distribution of students of color) and white students' test scores (at the top of the distribution of students of color). Second, observable starker differences in composition are associated with effects that are different from the ones that one would predict plugging estimated coefficients into the production function equation.

Finally, in terms of the association between peers and resources, exploratory analyses confirm, first, a strong correlation between peer composition and distribution of resources, and, second, that outcomes are sensitive to the inclusion/exclusion of these resources.

The remainder of this paper is organized as follows. The next section presents the data, sample, measures, and empirical strategy. In the third section, I provide results and discuss robustness checks and potential mechanisms. The last section discusses the implications of these findings. The conceptual framework for the full dissertation—which includes the framework for this paper—is outlined in Chapter 1.

## 2. Methods

### *2.1. Context and Data*

Like in other states (Maxwell, 2014), demographic shifts in Maryland have escalated since the turn of the 21st century such that public schools in the state now serve a majority-minority population. In 1990, less than 40% of the public-school student population in Maryland was non-white (Maryland State Department of Education, 2018), compared to more than 50% in 2010 and more than 60% in 2019. The non-white population is comprised primarily of Black and Hispanic students (see Table 1). Over the last decade, however, the relative participation of Black students decreased (from 35.8% in 2008 to 33% in 2019), while the participation of Hispanic students increased (from 10.2% in 2008 to 18.4% in 2019).

This paper draws on data from the Maryland Longitudinal Data System (MLDS) Center. This dataset contains information on all students, staff, and schools in the state of Maryland. I focus on a census of K-12 students enrolled in public and charter schools between the academic years 2007-08 and 2018-19. Estimation of peer effects centers on the three larger race-ethnicity groups of students in the state, namely, Black, Hispanic, and white.

### *2.2. Measures*

To evaluate short-term effects, I focused on three outcomes: end-of-year math test scores, end-of-year ELA test scores, and a dummy indicator for being ever suspended in a year. Table 2 presents descriptive characteristics of the student sample, including summary statistics of the outcomes of interest.

The key independent variable is the proportion of students of different race-ethnicities (as measured by the school counts of students of each race-ethnicity divided by school total enrollment) in a school/year. Relying on these proportions, I defined two measures of school diversity. In line with most of the literature on race-ethnicity peer effects, which focuses on a linear-in-means approach, the first measure is the continuous proportion of students of color (i.e., non-white). However, as discussed before, a linear-in-means approach assumes that the estimated effects are independent of the baseline distribution. It also makes predictions of potentially significant changes in school diversity based on small year-to-year variations.

To avoid these restrictions, the second independent variable that I used in this project is a categorical variable that divides the sample into five groups, depending on the discrete school composition of students each year. Specifically, I divided the sample of schools into quintiles using the distribution of students of color. Table 3 presents the average composition of schools in each of these quintile groups.

As Table 3 shows, each quintile represents a distinct type of school composition, in a continuum that goes from a large concentration of white students (quintile 1) to a large concentration of students of color (quintile 5), with quintile 3 closely matching the overall representation of students in the state.

As described before, Sacerdote (2011) proposes different models of peer effects: *linear in means* (i.e., the effect of the average of peers' background characteristics based on race, ethnicity, and native language); *boutique/tracking* (i.e., the effect of being surrounded by others like themselves); *rainbow* (i.e., the effect of a heterogeneous context); *invidious comparison* (i.e., the effect of being exposed to a majority of students with a different background); and *focus* (i.e., the effect of a homogeneous context). Based on this taxonomy, quintiles 1 and 2 can be described as a *comparison* group in the case of students of color (*boutique* in the case of white students); quintile 3 as a *rainbow* group; and quintiles 4 and 5 as a *boutique* group for students of color (*comparison* in the case of white students).

There are two main advantages of using quintiles obtained from the percentage of minority students rather than quintiles obtained from the distribution of Black, Hispanic, and white students separately. First, relying on quintiles obtained from the distribution of minority allows for a direct comparison between Black, Hispanic, and white students. [6] Second, quintile 3 closely resembles the overall distribution of students in the state, as shown in Table 3. Therefore, in estimating the effects of changes in peer composition I can use that median quintile as the group of reference and interpret the coefficients in a continuum that goes from schools with a large enrollment of white students to schools with a large enrollment of students of color. I also can test for differences in effects for students attending demographically homogenous schools where they are in the majority versus homogenous schools where they are in the majority. Overall, this approach allows me not only to focus on starker differences in composition but also to formally test for non-linearities.[7]

Table 4 summarizes the percentage of students in each school quintile (overall and by race-ethnicity). On average, 18.7% of students enroll in quintile 1 schools, 21.1% in quintile 2, 22.5% in quintile 3, 20.6% in quintile 4, and 17.2% in quintile 5.[8] However, enrollment in these different types of schools is stratified by race-ethnicity. While the majority of Black students enroll in schools with a larger concentration of students of color (30.4% in quintile 4 and 39% in

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[6] In turn, if using quintiles based on the distribution of each racial-ethnic group, quintiles would not be comparable. I included as an appendix table the distribution of students that I obtain from creating quintiles using the distribution of Black and Hispanic students separately (see Table A1). As the table shows, quintiles obtained using this procedure are not directly comparable either between them neither to the demographic composition of the state. For example, while the average school in Maryland enrolls 14% Hispanic students, and a quintile 3 school, using the percentage minority distribution, 14.2% Hispanic students, a quintile 3 school using the distribution of Hispanic students enrolls 7.4% Hispanic students. In turn, a quintile 3 school using the distribution of Black students enrolls 22.4% of Hispanic students.

[7] An alternative approach to testo for a non-linear association between race-ethnicity peer composition and outcomes would be to use a quadratic and a cubic functional form. I come back to this approach in the robustness checks section. I focus on the quintiles analysis because it is both easily interpretable and less prompt to be driven by outliers.

[8] The reason why these proportions are not all 20% is because I defined quintiles at the school level whereas these proportions are at the student level. At the school level, each quintile accounts for 20% of schools.

quintile 5 schools), the majority of white students enroll in schools with a larger concentration of white students (38.8% in quintile 1 and 32.8% in quintile 2 schools). Enrollment of Hispanic students, although skewed towards schools with a larger concentration of students of color, seems to be distributed more evenly than Black and white students across different types of schools, with larger concentrations in quintiles 3 and 4 (22.8% and 37.4%, respectively).

### *2.3. Empirical Strategy*

Peers are not randomly assigned to students. Students sort into schools based on family preferences (and available resources) on where to live, and then they sort into classrooms within schools based also on family preferences and teacher and school leader decisions (Manski, 1993). These factors determine the peers with whom an individual student goes to school and interacts with inside the classroom.

To my knowledge, no experiment has been conducted that randomly assigns peer composition for the specific purpose of estimating peer effects. Some analyses exploit the random assignment of students to classes to analyze the effects of peers (e.g., Nye, Hedges, & Konstantopoulos, 2000; Duflo, Dupas, & Kremer, 2015), though in these cases, as I discussed before, the random assignment process is meant to ensure balanced peer composition across classrooms and schools, biasing results toward zero. Other analyses exploit school enrollment lotteries (e.g., Angrist & Lang, 2004) or school consolidations (e.g., Hoxby & Weingarth, 2005) as instruments and natural experiments, but policy shocks that can be used for this purpose are not always available.

Instead, the most common strategy in the peer effects literature is to pair panel data with fixed effects for student or school fixed effects (e.g., Ammermueller & Pischke, 2009; Carrell & Hoekstra, 2010; Vigdor & Nechyba, 2007). These models exploit within-school or within-student differences in peer composition across years or across cohorts to make a causal claim regarding the association between peer composition and certain outcomes (Sacerdote, 2011). This strategy works well when peer composition is measured continuously, using specific percentages or proportions of students of specific characteristics. However, given that the theory suggests that it is stark differences in peer composition that may matter most, this approach can be limited. School or student fixed effect strategies exploit marginal changes in peer composition across cohorts. Technically, one could include a set of dummy variables characterizing schools that differ substantially in their composition, as I propose in this paper. Practically speaking, though, this strategy exploits very small variation across cohorts for schools that switch from one group to the other. In other words, all of the estimating variation will come from instances where a school or student switches from one side of a discrete bin to another (e.g., upper end of third quintile to lower end of the fourth quintile).

Because I instead am interesting in large differences in peer composition across schools, I specify the following value-added model that also is thought to support causal conclusions but without use of school or student fixed effects. The model allows me to compare changes in outcomes over time for students with similar background characteristics but attending very different types of schools:

$$Y_{igst} = \beta PC_{st} + \alpha f(S_{it-1}) + \zeta f(\overline{Sch}_{it-1}) + \pi X_{it} + \varphi Z_{it}^{s} + \theta T_{it}^{s} + \nu_g + \vartheta_t + \varepsilon_{isgt}. \quad (1)$$

where $Y_{ist}$ is the end-of-year outcome of interest—that is, math test scores, ELA test scores, and being ever suspended—for student *i* in grade *g*, in school *s,* and in year *t*. The full set of controls includes a function of students' prior academic achievement, suspensions, and absences, $S_{it-1}$, which make the model a value-added one because I examine changes in student outcomes over time; a function of prior school average test scores, suspensions, and absences,

$\overline{Sch}_{it-1}$; student demographic characteristics, $X_{it}$, including gender, free and reduced-price lunch eligibility, English Language Learner (ELL) status, and special education status; school level controls, $Z_{it}^{s}$, including total enrollment and counts of students receiving free and reduced-price meals; grade and academic year fixed-effects ($\nu_g$ and $\vartheta_t$, respectively), and an error term, $\varepsilon_{isgt}$. I also control for the proportion of teachers of different racial-ethnic backgrounds in school, $T_{it}^{s}$, because demographic characteristics of teachers are correlated with demographic characteristics of students, and because teachers of similar racial-ethnic background might be seen as an important resource for students. I go into more depth into this discussion below, when exploring mechanisms. The coefficient of interest is $\beta$, which measures peer composition, $PC_{st}$, in school *s* and year *t*, either with a continuous or a categorical measure. I estimate this model separately for Black, Hispanic, and white students.

In theory, the model should account for the factors by which students sort into schools; for example, prior literature shows how prior achievement is associated with a number of factors such as parental education or family income that also can drive school enrollment decisions. The downside is that I cannot know for sure whether I have accounted fully for omitted variables bias. Therefore, in a robustness test, I demonstrate that this specification produces similar results to a student or student-school fixed-effects approach, when peer composition is measured continuously and, so, when it is reasonable to include student or school fixed effects.

## 3. Results

### *3.1. Main Results*

In Table 5, I present the results of varying school peer composition on test scores and suspensions for Black, Hispanic, and white students. Panel A focuses on the continuous measure of peer composition (i.e., percentage of students of color), and models control for prior-year student and school average test scores, absences, and suspensions, as well as by student and school covariates. I find that a one percent increase in the percentage of minority students in school is associated with an increase in math and ELA test scores, both for Black and white students. On average, as the percentage of minority students in the school increase by one percent, end-of-year math test scores increase by 0.0007 SD and 0.0009 SD for Black and white students, respectively, while end-of-year ELA test scores increase by 0.0011 SD and 0.0013 SD for the same groups of students. These point estimates are small but should be interpreted relative to average within-school or within-student changes in the percent/proportion of students of color over time. For Hispanic students, on the other hand, the association is negative but not statistically significantly different from zero. Regarding being ever suspended in a year, panel A also shows that a one percent increase in the percentage of minority students is associated with a reduction in suspensions for Black (-0.07 percentage points [pp]), Hispanic (-0.05 pp), and white students (-0.03 pp).

Panel B presents the estimated results of the same value-added model but using the school quintiles categorical variable instead. In all models, I excluded from the estimation quintile 3, so the reported results can be interpreted as the difference between enrolling in a school with a representative demographic composition (i.e., a rainbow model of peer effects that is captured by students enrolling in schools in this third quintile) versus enrolling in a school with a large white students population (i.e., that is captured by students enrolling in schools in the first or second quintile and that conceptually refers to comparison model of peer effect in the case of Black and Hispanic students and to a boutique/tracking model of peer effect for white students) or a school with a large students' of color population (i.e., that is captured by students enrolling in schools in the fourth or fifth quintile and that conceptually refers to a boutique/tracking model of peer effect for Black and Hispanic students, and a comparison model of peer effect for white students). In the next paragraphs, I focus, first, on end-of-year test scores and then on suspensions.

For Black students, being enrolled in schools with a larger concentration of students of color than the one found in representative/rainbow schools (approximately 60% of students of color, as reported in Table 3), is associated with an increase in end-of-year math and ELA test scores. After controlling by prior year student and school average test scores, absences, and suspensions, as well as by student and school covariates, Black students enrolled in schools with approximately 87% (quintile 4) and 98% (quintile 5) of students of color exhibit end-of-year math test scores 0.0196 SD and 0.0298 SD larger than Black students enrolled in schools with a more representative school composition (see Table 5, panel B, columns 1 and 2). I observe a similar pattern for ELA test scores. On average, Black students enrolled in boutique/tracking schools, where the majority of students are also students of color, exhibit ELA test scores 0.0418 SD (quintile 4) and 0.0316 SD (quintile 5) larger than Black students enrolled in quintile 3 schools. Interestingly, both for math and ELA test scores being enrolled in schools with a larger concentration of white students (quintiles 1 and 2), as compared to being enrolled in schools with a representative composition of students, is associated with a decrease in math and ELA test scores. However, the association is statistically different from zero only for the latter, with an estimated decrease of 0.0168 SD (quintile 2) and 0.0182 SD (quintile 1) in end-of-year ELA scores (see Table 5, panel B, columns 1 and 2).

I document a similar pattern for white students. As compared to being enrolled in a rainbow/representative school (quintile 3), being enrolled in quintile 1 and quintile 2 schools, which in the case of white students are considered boutique/tracking schools, is associated with a decrease of 0.0335 SD and 0.0251 SD, respectively, in math test scores, as well as with a decrease of 0.0535 SD and 0.0274 SD, respectively, in ELA test scores, on average. In turn, being enrolled in quintile 4 schools, where the proportion of white students is only 12.6%, is associated with an increase in end-of-year math and ELA test scores of 0.0399 SD and 0.0481 SD, respectively (see Table 5, panel B, columns 7 and 8). It is worth noticing, however, that the proportion of white students enrolling in quintile 4 (and 5) schools is small. As reported in Table 4, less than 7% of white students enroll in schools with the largest concentrations of students of color, raising concerns about unaccounted unobservable factors that may explain sorting into these schools.

As described, the models above compare schools with a large concentration of white students (quintiles 1-2) and with a large concentration of students of color (quintiles 4-5) to schools with a representative population of students (quintile 3). To further confirm that differences between schools with even starker differences in composition exist I obtained *p-values* for the difference in coefficients between quintiles 1 and 5. I present these p-values in Table 5, panel C. Both for Black and white students, we confirm that the coefficients in quintiles 1 and 5 are statistically different from each other in test scores.

Overall, for Black and white students, the linear and non-linear approaches to the operationalization of the effect of peers on test scores yield consistent patterns. The continuous (percentage) and the categorical (quintiles) approaches show that, for Black and white students, end-of-year math and ELA test scores increase as the relative enrollment of students of color increases. There is, however, evidence of one non-linear pattern: as compared to being enrolled in representative quintile 3 schools, white students enrolled in quintile 5 schools (i.e., homogeneous schools where white students are the minority) exhibit test scores that are not statistically different (and in the case of ELA, smaller) from that reference point.

In the case of Hispanic students, the pattern is different. As compared to being enrolled in quintile 3 schools, end-of-year math and ELA test scores of Hispanic students increase when they are enrolled in schools with either a larger concentration of white students (quintiles 1 and 2) or a larger concentration of students of color (quintiles 3 and 4). On average, I observe statistically significant increments in test scores that range from 0.0184 SD to 0.0634 SD (see Table 5, panel B, columns 4 and 5) for Hispanic students enrolled in quintile 1-2 or 4-5 schools. I further confirm this pattern by obtaining not significant p-values of the difference between quintile 1 and quintile 5 coefficients (see panel C). Overall, results for Hispanic students using the quintiles approach differ from the null results presented in the linear approach (see panel A) but also signal a clear non-linear association between peer composition and test scores.

Turning into being ever suspended, the linear and non-linear approaches yield a similar pattern for all students. As compared to being enrolled in quintile 3 schools, being a Black, Hispanic, or white student enrolled in schools with a larger concentration of white students (i.e., quintile 1 and 2 schools) is associated with an increment of the likelihood of being suspended. On average, for Black students, this increment is about 1.02 pp (quintile 1) and 1.29 pp (quintile 2), for Hispanic students is about 1.73 pp (quintile 1) and 1.06 pp (quintile 2), and for white students is about 1.43 pp (quintile 1) and 1.25 pp (quintile 2). In turn, as compared to being enrolled in quintile 3 schools, the likelihood of being suspended decreases when students are enrolled in schools with a larger concentration of students of color (see Table 5, panel B, columns 3, 6, and 9). On average, for Black students, this decline is about 1.98 pp (quintile 4) and 3.45 pp (quintile 5), for Hispanic students is about 1.46 pp (quintile 4) and 1.39 pp (quintile

5), and for white students is 0.74 pp (quintile 4) and 1.84 pp (quintile 5). In all cases, quintile 1 and quintile 5 coefficients are statistically different from each other (see panel C).

### *3.2. Robustness Checks*

I conducted two robustness checks of the main results presented above. First, on the value-added model specification. Second, on the non-linear approach.

***Value-added versus fixed effects specification.*** The key identifying assumption of my analyses is that, conditional on the set of covariates included in the model, students who attend schools with very different demographic makeup are "equal in expectation". The broad set of covariates I include in my value-added models—including, most importantly, a prior measure of the outcome—are likely to meet this assumption (Koedel & Rockoff, 2015). However, I cannot test that assumption directly because potential confounders and omitted variables are, by definition, unobserved.

Therefore, I instead checked the robustness of the results presented before by comparing the value-added estimates to estimates obtained by relying on a fixed-effects approach. As described earlier, much of the empirical literature on peer effects relies on a fixed-effects strategy to identify the impact of changes in composition on the outcomes of interest. This approach assumes that, even if families sort into schools based on average composition, their classroom's, cohort's, or school's specific race/ethnic, socioeconomic, or linguistic composition is exogenous to that decision (Bifulco, Fletcher, & Ross, 2011; Hoxby, 2000). However, school- or student-fixed effects strategies only make sense when using a continuous measure of peer composition.

For the sake of parsimony, I conducted this robustness test using a sample that pools across Black, Hispanic, and white students, rather than disaggregating by race/ethnicity.

Specifically, I estimated the same value-added model as the ones presented for each race-ethnicity group, a student fixed-effects model, and a student-school fixed-effects model. I included prior student and school average test scores, absences, and suspensions for the value-added model, only prior school average measures for the student fixed-effects approach, and no prior student or school average measures for the student-school fixed-effects model. In the student-school fixed effects models, I excluded grade or year fixed effects because including all sets of fixed effects leads to a fully saturated model. Therefore, I also estimated the value-added and the student fixed-effects models with and without grade and year fixed effects. I present the results in Table 6.

Results are robust to these different specifications. As Table 6 documents, value-added estimates are robust to a fixed-effects approach. In all cases, the direction of the coefficient is the same: positive for end-of-year math and ELA test scores (see panels A and B) and negative for being ever suspended (see panel C). Estimates are also relatively stable to the inclusion/exclusion of grade and year fixed effects (see column 1 vs. column 2 and column 3 vs. column 4). As compared to the value-added and the student-school fixed-effect approaches, estimated coefficients are larger when including only student fixed-effects. In turn, coefficients obtained by estimating a value-added and a student-school fixed-effects model are close in magnitude (see columns 2 and 5). In all cases, value-added estimates are more conservative than the ones obtained with a fixed-effects approach.

***Quintiles versus quadratic/cubic approaches to non-linearity.*** As I mentioned before, I relied on the quintiles analysis as the preferred approach to non-linearity because it is both easily interpretable and less prompt to be driven by outliers. In this section, I present evidence of an alternative approach to non-linearity based on varying the functional form of the percentage minority continuous measure. Specifically, I estimated the model in equation (1) adding a quadratic and a cubic term of the percentage minority composition measure. I present the results in figures 1A, 1B, and 1C. In general, the figures confirm the patterns presented in Table 5. Consistent with the coefficients presented in Table 5, panel A, for all students a linear increase in the percentage of minority students is associated with a linear increase in test scores and a linear decrease in the likelihood of being suspended. Turning to the quadratic and cubic terms, a non-linear increase in test scores and a non-linear decrease in the likelihood of being suspended are apparent in the figures close to the 60% minority cutoff—which corresponds to the percentage of minority students in quintile 3—for all students. However, the figures also show a non-linear association at the bottom of the distribution, which may be due to the presence of outliers. This potential influence by outliers needs to be further explored.

### *3.3. Peers and Resources*

Thus far I demonstrated that peer composition is associated with short-term academic outcomes. Black and white students show increases in end-of-year math and ELA test scores when enrolled in schools with larger concentration of students of color, as compared to being enrolled in schools with a composition representative of the state population. Compared to the same representative group of schools, Hispanic students, on the other hand, show increases in test scores when enrolled in schools with larger concentration of either white or students of color. Finally, Black, Hispanic, and white students' likelihood of being suspended decreases when enrolled in schools with a larger proportion of students of color, as compared to schools with a representative composition.

Scholars have argued that one of the potential explanations for the effects of school racial-ethnic composition on academic outcomes is access to more and better school resources

(Condron & Roscigno, 2003; Darling-Hammond, 2013; Mickelson et al., 2013; Sosina & Weathers, 2019). In other words, the observed effects may simply capture the fact that peer composition and access to resources are directly linked, driving simultaneously the outcomes.

In this study, I explored the association between outcomes, peer composition, and two resources: (i) access to same-race/ethnicity teachers, (ii) and per-pupil school funding. While there are many other types of resources—monetary and non-monetary—that could be associated with the observed effects, these are two that have theoretical warrant and also are visible in the data. Demographic characteristics of teachers often are available in large administrative datasets, and I have this information for all years of data included in my analyses. Therefore, I control for racial-ethnic composition of teachers in all models described above. Below, I assess whether or not results change when I exclude this information. For per-pupil expenditures, historically data only has been available at the district level, which necessarily masks differences in expenditures across schools within the same district. That said, recently, this information has become available at the school level due to new federal regulation (i.e., Every Student Succeeds Act of 2015). Because I only have this data for one school year, I only include/exclude this information from my models in the robustness tests shown below.

***Student-teacher race-ethnicity matching.*** While we know that, in the U.S., student diversity is much greater than teacher diversity (Census Bureau, 2022; US Department of Education, 2016; Pew Research Center, 2021), the probability of student-teacher race-ethnicity matching increases at the school level if the concentration of students of a determined race-ethnicity group is larger (Spiegelman, 2020). At the same time, studies have documented clear benefits of race-ethnicity matching for improving a range of academic outcomes, including test score performance (Dee, 2004; Egalite et al., 2015), suspensions and expulsions (Lindsay &

Hart, 2017), absences (Holt & Gershenson, 2019), academic expectations (Gershenson, Holt, & Papageorge, 2016), and longer-run outcomes in college (Gershenson et al., 2018). Here, I explore the association between peer composition and student-teacher race-ethnicity matching, as well as their association with the estimated outcomes.

First, I document how the percentage of same-race teachers increases/decreases when the school peer composition diversity also increases/decreases. To investigate this, for each race-ethnicity group in which I focus here (i.e., Black, Hispanic, white), I estimated an OLS regression of the percentage of teachers of their same race-ethnicity as a function of school diversity, as measured by the categorical quintiles' variable. In all cases, I use quintile 3 as the group of reference. As Table 7 shows (column 1), as compared to being enrolled in a school with a representative school composition, for Black students enrolled in schools with a larger proportion of students of color, the percentage of Black teachers in their schools is, on average, 17.13 pp (quintile 4) and 43.93 pp (quintile 5) higher. In turn, for Black students enrolled in schools with a larger proportion of white students, the percentage of Black teachers in their schools is, on average, 5.58 pp (quintile 2) and 8.6 pp (quintile 1) lower, as compared to representative schools. I document a pattern in the same direction for Hispanic students. However, in their case, the increases and decreases are much smaller, because the overall share of Hispanic teachers in Maryland is quite small (approximately 2.28%, see Table A2). As compared to being enrolled in a school with a representative school composition, for Hispanic students enrolled in schools with a larger proportion of students of color, the percentage of Hispanic teachers in their schools is, on average, only 1.81 pp (quintile 4) higher. For Hispanic students, on the other hand, enrolled in schools with a larger proportion of white students, the percentage of Hispanic teachers in their schools is, on average, 1.13 pp (quintile 2) and 2.14 pp (quintile 1) lower (see Table 7, column 2).

Finally, for white students enrolled in schools with a larger proportion of students of color, the percentage of white teachers in their schools is, on average, 15.19 pp (quintile 4) and 47.93 pp (quintile 5) lower, as compared to being enrolled in representative schools. In contrast, for white students enrolled in schools with a larger proportion of white students, the percentage of white teachers in their schools is, on average, 6.61 pp (quintile 2) and 11.3 pp (quintile 1) lower, as compared to being enrolled in representative schools.

The results presented in Table 5 control for the percentage of teachers of different race-ethnicity groups. Table 8 shows the results of the same value-added models but excluding these teacher percentages as covariates. Overall, the estimated effects of school diversity, measured continuously (panel A) or with the quintile categorical variable (panel B), are similar in direction to the ones presented in Table 5. The magnitude of the coefficients when excluding teacher race-ethnicity percentages are smaller in the case of end-of-year math test scores for Black and Hispanic students, and larger in all other cases (i.e., end-of-year math scores for white students, and end-of-year ELA test scores and ever suspended for Black, Hispanic, and white students). This confirms the notion that part of the effects of peer composition on academic outcomes is associated with the teacher-staff composition that results from having a more or less diverse school environment. At the same time, the differences in estimates are not large.

***Per-pupil school funding.*** The second association that I explore in this study is between peer composition and per-pupil school funding. While for some time scholars argued on whether funding matters at all or at least for whom (Jackson, 2018),[9] recent evidence has demonstrated

[9] See, for example, the Hanushek (1989, 1991, 1994) versus Hedges, Laine, & Greenwald (1994a, 1994b) debate.

that access to increased funding explains differences in educational achievement (Burtless, 2011; Hedges et al., 2016; Jackson et al., 2016; Martorell et al., 2016). Further, scholars have documented that, in absence of state redistributive policies and federal assistance, increased racial isolation in schools is likely associated with unequal distribution of funding (Owens, 2018; Reardon & Owens, 2014; Weathers & Sosina, 2019). Using novel school-level funding data collected by the Edunomics Lab at Georgetown University,[10] I explored descriptive differences in funding availability by school diversity and how the estimated effects of peer diversity change when including per-pupil school funding. Historically, only school districts have been required to report per-student funding and expenditures, which average funding across all students in a district and so mask substantially between-school variability. However, spurred in part by the federal Every Student Succeeds Act of 2015, Maryland and other states have started to require schools that report per-pupil funding and expenditure information (Darling-Hammond et al., 2016; Edunomics Lab, n.d.). Because this push is quite new, school-level data in Maryland only is available for the 2018-19 school cohort.

Table 9 presents a descriptive analysis of school-level funding by school diversity, as measured by the school quintile categorical variable. Columns 1-3 present raw school funding amounts and columns 4-6 present per-pupil school funding. As column 3 shows, after including state and federal funding, in the academic year 2018-19, representative schools (i.e., quintile 3) are the ones with the greatest dollar amount of funding, followed by schools with a larger concentration of students of color (i.e., quintiles 4 and 5 schools), and then by schools with a larger concentration of white students (i.e., quintiles 1 and 2 schools). However, the pattern is different when we consider the per-pupil funding amount. As column 6 shows, after including

[10] For background information on the lab and on the data collection process, go to https://edunomicslab.org.

state and federal funding into the calculations, in the academic year 2018-19, per-pupil funding is the least in schools with the largest concentration of white students (i.e., quintile 1 schools, with a total of $13,284.50 per-pupil funding) and is the greatest in schools with the largest concentration of students of color (i.e., quintile 5 schools, with a total of $15,408.49 per-pupil funding).

This pattern in funding seems to contradict the idea that schools with a larger concentration of students of color are under-resourced relative to schools with a high concentration of white students. However, there are a few caveats. First, per-pupil funding is only higher in schools with a larger concentration of students of color after accounting for state and federal redistributive funding. As Chingos and Blagg (2017a) show, local district-level funding in the state of Maryland is regressive (i.e., because of the tax base, funding at the local level is inversely associated with poverty rates), a feature that is only reversed once poverty rates and other demographic characteristics are weighted and corrected by state and federal funding formulas. Second, progressive funding is associated with the proportion of students that require special education or ELL services, as well as with the proportion of students that qualify for free and reduced-price meals (Chingos & Blagg, 2017b). In the study sample, schools with a larger concentration of students of color also concentrate a larger proportion of special education, ELL, and free and reduced-price meals students (see Table A3). Finally, these patterns reflect only per-pupil funding on a year-to-year basis. However, they do not reflect other expenses such as capital campaigns or parent/school-level fundraising, which can lead to differences in the amount and quality of resources and available infrastructure, and which have been documented as a source of inequality in expenditure (Nelson, & Gazley, 2014; Posey-Maddox, 2013).

Using the sample of 2018-19 observations, I estimated the model in equation 1 but in this case including a variable that summarizes per-pupil school total funding. Results are presented in Table 10. As compared to the estimates presented in Table 5, once per-pupil school funding is added to the estimation, two patterns emerge.

First, when considering effects on test scores, in a few cases there is a change in the sign of the estimated coefficient, resulting on a significant coefficient that signals an association going in the opposite direction as the one documented in Table 5. In the case of the models that use a continuous measure of peer composition, the estimated association between the end-of-year ELA test score and composition is now negative for white students (see Table 10, panel A, column 8). Using the categorical variable of peer diversity, on the other hand, the sign of the estimated coefficient also changes for Black and white students in the case of math and ELA test scores in quintiles with the largest concentration of white students. Specifically, I now observe a positive and significant coefficient for students in quintile 1 schools in math and ELA test scores in the case of Black students and only ELA test scores in the case of white students (see Table 10, panel B, columns 1-2, 8).

For other relationships between peer composition and test scores, most of the significant coefficients that I show and discuss in Table 5 are in this case smaller in magnitude and, in most cases, not statistically significantly different from zero once controlling for funding. Specifically, as compared to being enrolled in rainbow/representative schools, I no longer document an increase in math or test scores for Black, Hispanic, and white students enrolled in schools with a larger concentration of students of color (quintiles 4 and 5 for Black and white students, only quintile 4 for Hispanic students), and I document only a marginally significant association for Hispanic students enrolled in quintile 5 schools (see Table 10, panel B, columns 1-2, 4-5, 7-8).

Overall, these results indicate that, once per-pupil school funding is included in the estimation, a larger concentration of students of color is not directly associated with an increase in test scores, as compared to a diverse/representative school. In other words, the correlation between peer composition and per-pupil spending affects the association with test scores.

Second, when considering effects on suspensions, including school-level funding largely leaves estimated relationships unaltered. After controlling by per-pupil funding, I still document clear benefits in terms of suspensions for Black, Hispanic, and white students of either enrolling in schools with a larger concentration of students of color or avoiding schools with a larger concentration of white students (see Table 10, panels A and B, columns 3, 6, 9). These contrasting findings suggest potential different channels in which the association between peer composition and spending work in affecting results. However, more years of spending data will be required to confirm this pattern over time.

## 4. Discussion

In this study, I examined the association between racial and ethnic peer composition in schools in Maryland and short-term academic outcomes, namely, math and ELA test scores and suspensions. Overall, I document a heterogeneous association between peer composition and outcomes by race-ethnicity of the student. Black students' test scores increase as the proportion of students of color increases, and their likelihood of being suspended decreases as the proportion of students of color increases. I observe a similar pattern in test scores and suspensions for white students, with one important difference: while enrolling in schools with a larger concentration of students of color, as compared to schools with a more balanced representation, is associated with increments in test scores for white students, enrolling in schools with the largest concentration of students of color is not, as compared to the same reference group. Finally, for Hispanic students, the evidence for suspensions goes in the same direction as for Black and white students (i.e., the likelihood of being suspended decreases as the proportion of students of color increases), but test scores increase in association with increments of both the proportion of white, on one side, and the proportion of students of color, on the other side, as compared to a balanced/representative composition.

I aimed at contributing to the literature on peer composition by operationalizing school race-ethnicity diversity using a continuous and categorical measure and exploring the distinct patterns of association of these two measures with short-term academic outcomes. What do we learn by using both operationalizations of school composition about our understanding of peer effects? First, while I use the proportion of students of color as the basis for calculating the continuous and the categorical independent variable, in the categorical measure each quintile has an observable distribution of different populations over the years. As I presented in Table 3, each quintile in the categorical approach has a particular distribution of Black, Hispanic, Asian, and other race-ethnicity students, which goes beyond the distribution of white students in these quintiles. For example, while the percentage of Black students increases continuously, and the percentage of white students decreases continuously, from quintile 1 to quintile 5, that is different in the case of Hispanic or Asian students, for example. For these two populations their representation first increases and then, in quintile 4 for Asian students and quintile 5 for Hispanic students, decreases. Accounting by the actual distribution of students in different schools gives us a better sense of how to interpret and extrapolate these results.

Second, while in most cases the continuous and categorical measures point in the same direction, marginal estimates obtained by predicting coefficients at different points of the distribution, holding everything else constant, yield in most cases estimates that are different from the ones that we obtain in the categorical approach. For example, based on the composition of the different quintiles reported in Table 3 and the coefficients reported in Table 5, we observe that Black students enrolling in a quintile 2 school, with an average of 36.5% of students of color, score in end-of-year math 0.0087 SD lower, on average, than students in quintile 3 schools, with 60.6% of students of color. However, the marginal predicted difference using the continuous measure of composition is twice as big (0.018 SD). Other predictions of marginal estimates result in larger or smaller effects, as compared to the ones obtained with the categorical approach. These differences in prediction highlight the idea that policy implications solely derived from marginal changes in peer composition may not hold in reality, or that linear predictions may be affected by different dynamics at certain points of the distribution.

Finally, in some cases, predictions based solely on a continuous measure of peer composition may lead to wrong conclusions. This is particularly true in the case of Hispanic students and end-of-year test scores. Based on the estimates obtained using the continuous measure we would predict no association between the percentage of students of color and math and ELA test scores. This is not the case when we analyze the categorical measure of peer composition and the effects reported for these students of enrolling in schools with a larger proportion of white or students of color, as compared to a more heterogenous composition (see Table 5, panel B, columns 4 and 5). At the same time, based on the continuous measure of peer composition, for white students we would predict a linear association between the percentage of students of color and test scores at all different points. However, as I show in Table 5 (panel B, columns 7 and 8), we see a significant increment in test scores from quintile 3 to 4, but not from quintile 4 to 5. This points to the presence of non-linearities that need to be considered in making predictions or deriving policy implications.

In this study, I also explored the association between peer composition and resources. I document, first, a direct correlation between peer composition and the likelihood of teacher-student race matching: for all students, this probability increases when they are enrolled in schools with a larger proportion of students like themselves. Differences in this likelihood are especially large for Black and white students. Second, I document a direct correlation between peer composition and per-pupil spending: after accounting for state and federal progressive formulas, per-pupil spending increases as the proportion of students of color increases. Further, I explored how the direct association between peer composition and resources may jointly explain differences in test scores and suspensions.

Taken together, the results and patterns presented in this study highlight some of the complexities of the discussions around school diversity, especially in the context of changing demographics. As compared to being enrolled in schools with a large concentration of white students, most students, regardless of their race-ethnicity, seem to benefit from being enrolled in schools with an increased representation of students of color. On the other hand, homogenous learning environments can be beneficial for some students of color, if they are in the majority. At the same time, these empirical results cannot be separated from their association with staff representation and distribution of resources, or from potential discrimination in academic and disciplinary practices that students of color face in homogeneous schools where they are in the minority. The policy debate around school diversity needs to take into consideration these findings, as school composition and racial-ethnic representation affect not only social cohesion and the development of democratic values, but also the experiences, well-being, and potential achievement of students.

## 5. Tables

Table 1. Changes in School Composition 2008-2019

| | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Number of schools | 1323 | 1334 | 1328 | 1330 | 1339 | 1337 | 1336 | 1334 | 1334 | 1322 | 1319 | 1317 |
| Number of students | 820,539 | 818,645 | 820,443 | 823,052 | 824,978 | 830,331 | 837,212 | 844,118 | 852,351 | 861,446 | 865,486 | 869,411 |
| Asian | 0.053 | 0.055 | 0.056 | 0.058 | 0.060 | 0.061 | 0.062 | 0.063 | 0.064 | 0.065 | 0.067 | 0.067 |
| Black | 0.358 | 0.355 | 0.351 | 0.348 | 0.345 | 0.342 | 0.340 | 0.338 | 0.336 | 0.333 | 0.332 | 0.330 |
| Hispanic | 0.102 | 0.108 | 0.113 | 0.118 | 0.125 | 0.132 | 0.141 | 0.150 | 0.158 | 0.169 | 0.175 | 0.184 |
| Other | 0.036 | 0.040 | 0.044 | 0.045 | 0.047 | 0.048 | 0.049 | 0.050 | 0.050 | 0.050 | 0.051 | 0.051 |
| White | 0.452 | 0.442 | 0.436 | 0.430 | 0.424 | 0.417 | 0.409 | 0.400 | 0.391 | 0.382 | 0.375 | 0.367 |

Table 2. Sample Means

| | Mean |
|---|---|
| Panel A: Student characteristics | |
| Asian | 0.064 |
| Black | 0.341 |
| Hispanic | 0.148 |
| Other | 0.045 |
| White | 0.402 |
| Female | 0.490 |
| ELL | 0.130 |
| Special education | 0.180 |
| Panel B: Short-term outcomes | |
| Math | -0.008 *(0.993)* |
| ELA | 0.003 *(0.998)* |
| Days Absent | 10.358 *(12.322)* |
| Ever suspended | 0.063 *(0.243)* |
| Observations | |
| Student-year | 10,068,012 |
| Student | 1,908,456 |
| Schools | 1412 |

Notes: Panel B includes mean and standard deviation in parentheses.

Table 3. School Composition by Quintile

| | Overall | Quintile 1<br>*Largely white students* | Quintile 2 | Quintile 3 | Quintile 4 | Quintile 5<br>*Largely students of color* |
|---|---|---|---|---|---|---|
| Asian | 0.061 | 0.020 | 0.071 | 0.112 | 0.069 | 0.016 |
| Black | 0.342 | 0.044 | 0.149 | 0.288 | 0.507 | 0.777 |
| Hispanic | 0.140 | 0.042 | 0.082 | 0.142 | 0.255 | 0.178 |
| Other | 0.047 | 0.041 | 0.062 | 0.064 | 0.043 | 0.016 |
| White | 0.410 | 0.852 | 0.635 | 0.394 | 0.126 | 0.013 |

Table 4. Distribution of Students across Quintile Schools

| | All | Black | Hispanic | White |
|---|---|---|---|---|
| Quintile 1: *Largely white students* | 0.187 | 0.024 | 0.057 | 0.388 |
| Quintile 2 | 0.211 | 0.092 | 0.124 | 0.328 |
| Quintile 3 | 0.225 | 0.189 | 0.228 | 0.216 |
| Quintile 4 | 0.206 | 0.304 | 0.374 | 0.063 |
| Quintile 5: *Largely students of color* | 0.172 | 0.390 | 0.218 | 0.006 |

Table 5. Short-Term Race-Ethnicity Peer Effects

| | Black | | | Hispanic | | | White | | |
|---|---|---|---|---|---|---|---|---|---|
| | Math | ELA | Suspended | Math | ELA | Suspended | Math | ELA | Suspended |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| <u>Panel A: Continuous</u> | | | | | | | | | |
| % minority | 0.0007*** | 0.0011*** | -0.0007*** | -0.0002 | -0.0001 | -0.0005*** | 0.0009*** | 0.0013*** | -0.0003*** |
| | (0.0002) | (0.0001) | (0.0001) | (0.0002) | (0.0002) | (0.0000) | (0.0002) | (0.0002) | (0.0000) |
| <u>Panel B: Categorical</u> | | | | | | | | | |
| Quintile 1: *Largely white students* | -0.0003 | -0.0182* | 0.0102+ | 0.0540*** | 0.0301*** | 0.0173*** | -0.0335*** | -0.0535*** | 0.0143*** |
| | (0.0101) | (0.0090) | (0.0054) | (0.0104) | (0.0089) | (0.0028) | (0.0078) | (0.0086) | (0.0019) |
| Quintile 2 | -0.0087 | -0.0168* | 0.0129** | 0.0198* | 0.0184* | 0.0106*** | -0.0251*** | -0.0274*** | 0.0125*** |
| | (0.0077) | (0.0070) | (0.0042) | (0.0100) | (0.0088) | (0.0025) | (0.0072) | (0.0071) | (0.0019) |
| Quintile 4 | 0.0196* | 0.0418*** | -0.0198*** | 0.0236* | 0.0317*** | -0.0146*** | 0.0399*** | 0.0481*** | -0.0074** |
| | (0.0090) | (0.0069) | (0.0045) | (0.0113) | (0.0088) | (0.0026) | (0.0101) | (0.0099) | (0.0023) |
| Quintile 5: *Largely students of color* | 0.0298* | 0.0316*** | -0.0345*** | 0.0634** | 0.0556*** | -0.0139** | 0.0182 | -0.0130 | -0.0184*** |
| | (0.0122) | (0.0095) | (0.0060) | (0.0199) | (0.0136) | (0.0043) | (0.0188) | (0.0188) | (0.0051) |
| <u>Panel C: Q1 vs Q5</u> | | | | | | | | | |
| *p-values* | 0.066 | 0.000 | 0.000 | 0.705 | 0.1139 | 0.000 | 0.017 | 0.072 | 0.000 |
| *Student/year observations* | *1592632* | *1583330* | *2232952* | *615058* | *601707* | *874876* | *1669654* | *1771151* | *2633260* |

Notes: Estimates are the result of regressing the end-of-year outcome of interest (end-of-year math and ELA test scores; being ever suspended) on a continuous (Panel A) or a categorical (Panel B) measure of school diversity, prior year students' outcomes and prior year school averages in these outcomes. Regressions of each outcome include all prior year students and school measures. All models include student (gender, ELL status, special education status, free-reduced price lunch status) and school (total enrollment, free-reduced price lunch counts, proportion of teachers of different race-ethnicities) covariates. All models include grade and academic year fixed effects. Robust standard errors, clustered at the school level, in parentheses.
*+ p < 0.1; *p<0.05; **p<0.01; ***p<0.001*

Table 6. Robustness Check – Value Added vs. Fixed Effects Approach

| | Value added | | Student FE | | Student and school FE |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Panel A: Math | 0.0001 | 0.0004+ | 0.0032*** | 0.0045*** | 0.0005*** |
| | (0.0001) | (0.0002) | (0.0002) | (0.0003) | (0.0001) |
| *Observations* | *4318586* | *4318586* | *4908088* | *4908088* | *5096111* |
| Panel B: ELA | 0.0003** | 0.0005*** | 0.0011*** | 0.0014*** | 0.0011*** |
| | (0.0001) | (0.0001) | (0.0002) | (0.0002) | (0.0000) |
| *Observations* | *4422903* | *4422903* | *5010077* | *5010077* | *5010077* |
| Panel C: Suspended | -0.0000 | -0.0002** | -0.0007*** | -0.0007*** | -0.0007*** |
| | (0.0000) | (0.0000) | (0.0000) | (0.0001) | (0.0000) |
| *Observations* | *6421532* | *6421532* | *9126091* | *9126091* | *9790788* |
| Grade and year fixed effects | Y | N | Y | N | N |
| Student lagged | Y | Y | N | N | N |
| School lagged | Y | Y | Y | Y | N |

Notes: Estimates are the result of regressing the end-of-year outcome of interest in each panel on a continuous measure of school diversity. Models in columns (1) and (2) use a value-added approach; models in columns (3) and (4) a student fixed effects approach; and column (5) a student and school fixed effects approach. All models include student (gender, ELL status, special education status, free-reduced price lunch status) and school (total enrollment, free-reduced price lunch counts, proportion of teachers of different race-ethnicities) covariates. Robust standard errors, clustered at the school level, in parentheses
*+ p < 0.1; *p<0.05; **p<0.01; ***p<0.001*

Table 7. Association between School Composition and Same-Race Teachers

| | Black<br>(1) | Hispanic<br>(2) | White<br>(3) |
|---|---|---|---|
| Quintile 1: *Largely white students* | -8.6040*** | -2.1398*** | 11.2989*** |
| | (0.8241) | (0.3315) | (0.5348) |
| Quintile 2 | -5.5772*** | -1.1277*** | 6.6072*** |
| | (0.6325) | (0.3294) | (0.5405) |
| Quintile 4 | 17.1318*** | 1.8084*** | -15.1874*** |
| | (1.2582) | (0.4031) | (1.0557) |
| Quintile 5: *Largely students of color* | 43.9331*** | 0.6648 | -47.9311*** |
| | (1.2945) | (0.4817) | (1.3367) |
| *Student/year observations* | *3444017* | *1411596* | *4126673* |

Notes: Estimates are the result of regressing the percentage of teachers of the same race-ethnicity as the student on a categorical measure of school diversity, controlling by school covariates (total enrollment, free-reduced price lunch counts). All models include academic year fixed effects. Robust standard errors, clustered at the school level, in parentheses.

*+ p < 0.1; *p<0.05; **p<0.01; ***p<0.001*

Table 8. Short-Term Race-Ethnicity Peer Effects Excluding Teacher Proportions

| | Black | | | Hispanic | | | White | | |
|---|---|---|---|---|---|---|---|---|---|
| | Math | ELA | Suspended | Math | ELA | Suspended | Math | ELA | Suspended |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Panel A: Continuous | | | | | | | | | |
| % minority | 0.0001 | 0.0013*** | -0.0012*** | -0.0001 | 0.0006*** | -0.0007*** | 0.0009*** | 0.0017*** | -0.0004*** |
| | (0.0001) | (0.0001) | (0.0001) | (0.0002) | (0.0002) | (0.0000) | (0.0001) | (0.0001) | (0.0000) |
| Panel B: Categorical | | | | | | | | | |
| Quintile 1: *Largely white students* | 0.0086 | -0.0246** | 0.0194*** | 0.0526*** | 0.0173* | 0.0218*** | -0.0364*** | -0.0733*** | 0.0176*** |
| | (0.0098) | (0.0087) | (0.0055) | (0.0095) | (0.0086) | (0.0028) | (0.0069) | (0.0074) | (0.0016) |
| Quintile 2 | -0.0032 | -0.0203** | 0.0184*** | 0.0191* | 0.0113 | 0.0132*** | -0.0269*** | -0.0387*** | 0.0145*** |
| | (0.0075) | (0.0068) | (0.0044) | (0.0097) | (0.0088) | (0.0026) | (0.0068) | (0.0070) | (0.0018) |
| Quintile 4 | 0.0042 | 0.0509*** | -0.0316*** | 0.0164+ | 0.0435*** | -0.0199*** | 0.0396*** | 0.0650*** | -0.0109*** |
| | (0.0080) | (0.0065) | (0.0041) | (0.0097) | (0.0083) | (0.0026) | (0.0087) | (0.0094) | (0.0020) |
| Quintile 5: *Largely students of color* | -0.0044 | 0.0494*** | -0.0544*** | 0.0408** | 0.0826*** | -0.0262*** | 0.0100 | 0.0289** | -0.0271*** |
| | (0.0082) | (0.0067) | (0.0042) | (0.0136) | (0.0109) | (0.0034) | (0.0126) | (0.0112) | (0.0035) |
| *Student/year observations* | *1592632* | *1583330* | *2232952* | *615058* | *601707* | *874876* | *1669677* | *1771174* | *2633283* |

Notes: Estimates are the result of regressing the end-of-year outcome of interest (end-of-year math and ELA test scores; being ever suspended) on a continuous (Panel A) or a categorical (Panel B) measure of school diversity, prior year students' outcomes and prior year school averages in these outcomes. Regressions of each outcome include all prior year students and school measures. All models include student (gender, ELL status, special education status, free-reduced price lunch status) and school (total enrollment, free-reduced price lunch counts) covariates. All models include grade and academic year fixed effects. Robust standard errors, clustered at the school level, in parentheses.
*+ p < 0.1; *p<0.05; **p<0.01; ***p<0.001*

Table 9. School Resources by Quintile

| | Total state and local | Total federal | Total | Per-pupil state and local | Per-pupil federal | Per-pupil total |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Quintile 1: *Largely white students* | 10234883.00 | 436906.66 | 10671816.00 | 12685.51 | 598.95 | 13284.50 |
| _ | (5374870.1) | (219907.18) | (5525106) | (1541.708) | (347.635) | (1726.702) |
| Quintile 2 | 12135552.00 | 531666.31 | 12667225.00 | 13013.60 | 613.36 | 13626.96 |
| | (7039566) | (318433.45) | (7266283) | (1834.463) | (374.255) | (1995.782) |
| Quintile 3 | 13758187.00 | 672331.21 | 14430472.00 | 13750.13 | 751.75 | 14501.87 |
| | (7765596.1) | (404512.46) | (8000538.6) | (1674.481) | (509.065) | (1778.528) |
| Quintile 4 | 13064372.00 | 727573.01 | 13791905.00 | 14411.74 | 888.54 | 15300.23 |
| | (7479365.4) | (455885.13) | (7754781.8) | (2295.853) | (634.871) | (2462.152) |
| Quintile 5: *Largely students of color* | 12295850.00 | 868479.09 | 13164369.00 | 14184.32 | 1224.13 | 15408.49 |
| | (7612981.8) | (556747.16) | (7806862) | (2227.486) | (914.636) | (2394.411) |

Notes: All values in dollars. Standard deviation in parentheses. Source: Edunomics Lab, Georgetown University, https://edunomicslab.org/nerds/

Table 10. Short-Term Race-Ethnicity Peer Effects Controlling by School Resources

| | Black | | | Hispanic | | | White | | |
|---|---|---|---|---|---|---|---|---|---|
| | Math | ELA | Suspended | Math | ELA | Suspended | Math | ELA | Suspended |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| <u>Panel A: Continuous</u> | | | | | | | | | |
| % minority | 0.0004 | -0.0000 | -0.0005*** | -0.0003 | -0.0004 | -0.0004*** | -0.0002 | -0.0007* | -0.0002* |
| | (0.0003) | (0.0003) | (0.0001) | (0.0003) | (0.0004) | (0.0001) | (0.0003) | (0.0003) | (0.0001) |
| <u>Panel B: Categorical</u> | | | | | | | | | |
| Quintile 1: *Largely white students* | 0.0355* | 0.0653*** | 0.0189* | 0.0616** | 0.0769*** | 0.0120* | 0.0064 | 0.0356* | 0.0088** |
| | (0.0176) | (0.0176) | (0.0078) | (0.0196) | (0.0204) | (0.0051) | (0.0142) | (0.0163) | (0.0032) |
| Quintile 2 | -0.0166 | 0.0014 | 0.0174** | 0.0020 | 0.0294+ | 0.0080+ | -0.0200+ | 0.0059 | 0.0102*** |
| | (0.0119) | (0.0134) | (0.0064) | (0.0158) | (0.0169) | (0.0048) | (0.0118) | (0.0140) | (0.0029) |
| Quintile 4 | 0.0151 | 0.0108 | -0.0048 | 0.0151 | 0.0282 | -0.0102** | -0.0091 | 0.0126 | -0.0011 |
| | (0.0135) | (0.0143) | (0.0059) | (0.0137) | (0.0183) | (0.0039) | (0.0177) | (0.0181) | (0.0032) |
| Quintile 5: *Largely students of color* | 0.0241 | 0.0087 | -0.0168+ | 0.0424+ | 0.0436+ | -0.0098+ | -0.0024 | -0.0102 | -0.0085 |
| | (0.0177) | (0.0185) | (0.0089) | (0.0247) | (0.0265) | (0.0057) | (0.0327) | (0.0360) | (0.0079) |
| *Student/year observations* | *147090* | *148986* | *204196* | *78601* | *77840* | *109953* | *142456* | *152026* | *224096* |

Notes: Estimates are the result of regressing the end-of-year outcome of interest (end-of-year math and ELA test scores; total absences; being ever suspended) on a continuous (Panel A) or a categorical (Panel B) measure of school diversity, prior year students' outcomes and prior year school averages in these outcomes. Regressions of each outcome include all prior year students and school measures. All models include student (gender, ELL status, special education status, free-reduced price lunch status) and school (total enrollment, free-reduced price lunch counts, proportion of teachers of different race-ethnicities) covariates. All models include grade fixed effects. Robust standard errors, clustered at the school level, in parentheses.
*+ p < 0.1; *p<0.05; **p<0.01; ***p<0.001*

Figure 1A. Percent Minority and Outcomes – Black Students

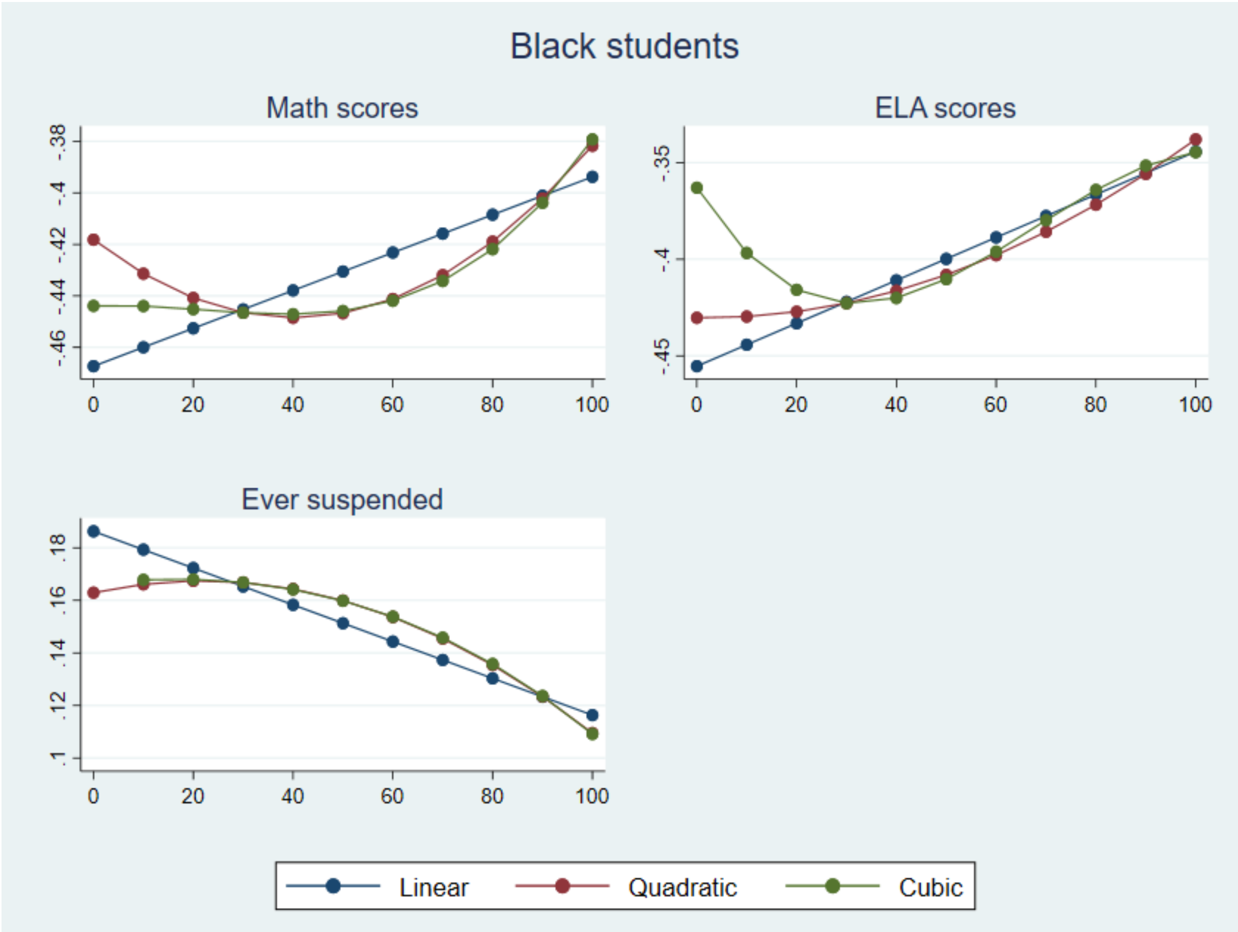


Figure 1B. Percent Minority and Outcomes – Hispanic Students

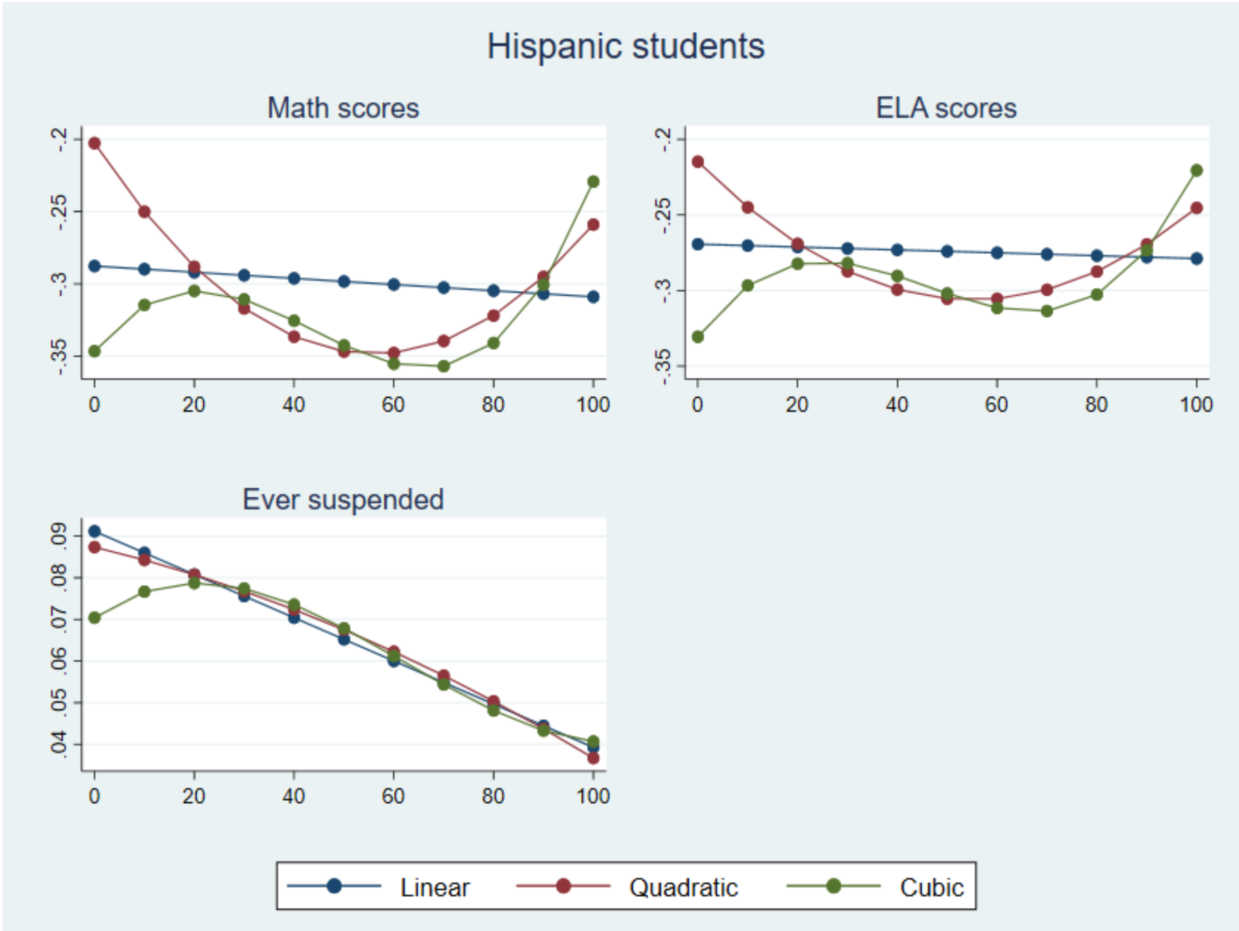
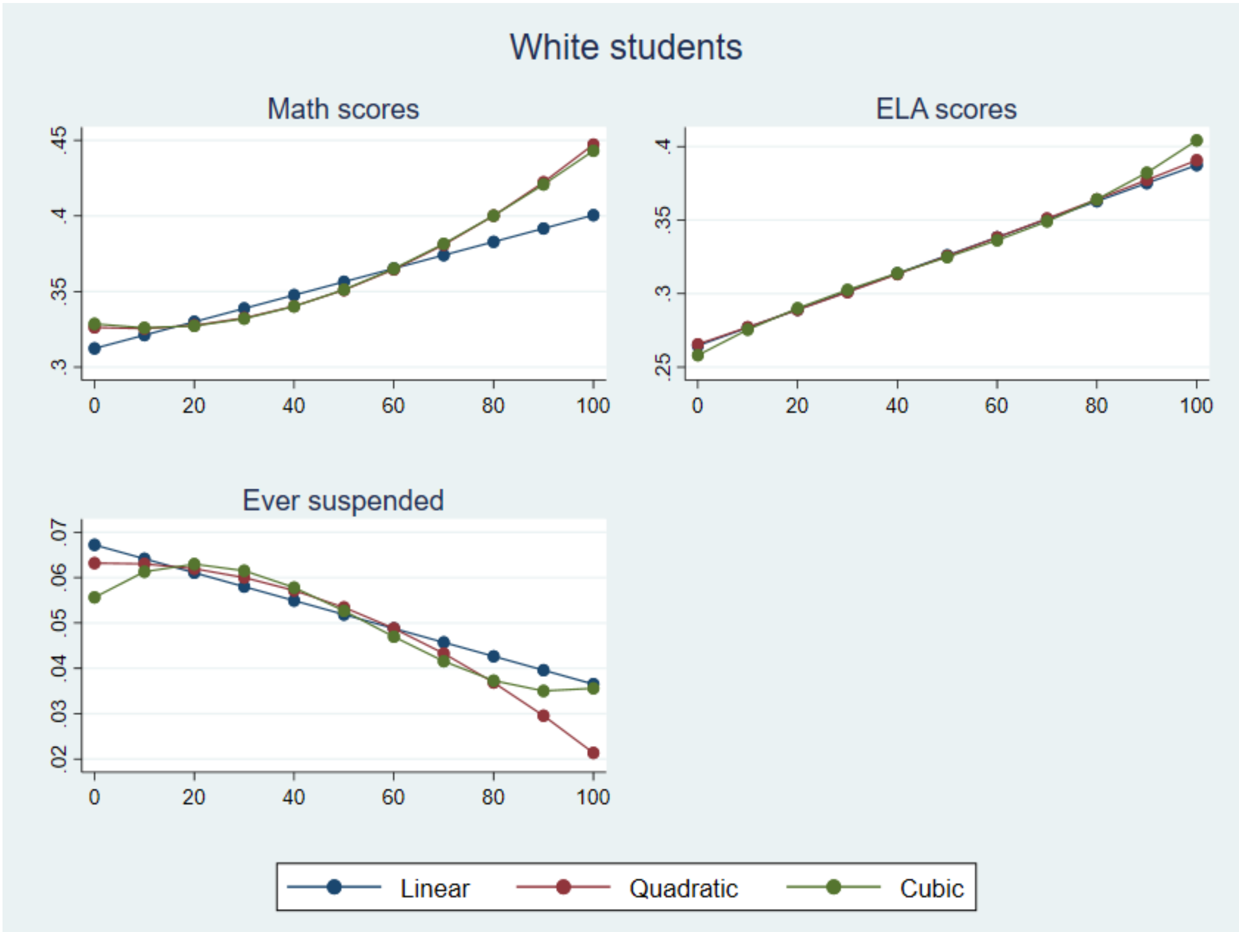


Figure 1C. Percent Minority and Outcomes – White Students



## 6. Appendices

Table A1. School Composition by Black and Hispanic Quintiles

| | Quintile 1 | Quintile 2 | Quintile 3 | Quintile 4 | Quintile 5 |
|---|---|---|---|---|---|
| | *Largely white* | | | | *Largely Black/Hispanic* |
| Panel A: Black Quintiles | | | | | |
| Asian | 0.053 | 0.095 | 0.077 | 0.053 | 0.013 |
| Black | 0.035 | 0.114 | 0.255 | 0.511 | 0.890 |
| Hispanic | 0.060 | 0.144 | 0.224 | 0.182 | 0.053 |
| Other | 0.043 | 0.055 | 0.057 | 0.051 | 0.019 |
| White | 0.810 | 0.592 | 0.387 | 0.203 | 0.025 |
| Panel B: Hispanic Quintiles | | | | | |
| Asian | 0.028 | 0.049 | 0.063 | 0.078 | 0.076 |
| Black | 0.497 | 0.275 | 0.309 | 0.359 | 0.318 |
| Hispanic | 0.019 | 0.045 | 0.074 | 0.131 | 0.387 |
| Other | 0.027 | 0.044 | 0.058 | 0.061 | 0.039 |
| White | 0.428 | 0.587 | 0.495 | 0.370 | 0.180 |

Table A2. School Level Teachers' Proportions by Race-Ethnicity

| Variable | N | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| % Asian | 15,945 | 3.438729 | 4.971231 | 0 | 46.67 |
| % Black | 15,945 | 17.81925 | 21.38532 | 0 | 100 |
| % Hispanic | 15,945 | 2.281018 | 3.593039 | 0 | 79.17 |
| % Other | 15,945 | 2.929024 | 3.896149 | 0 | 54.55 |
| % White | 15,945 | 73.53198 | 25.71923 | 0 | 100 |

Table A3. Students' Characteristics by School Quintile

| | ELL | | Special Education | | Farms | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| All | 0.071 | 0.257 | 0.138 | 0.345 | 0.413 | 0.492 |
| Quintile 1 | 0.007 | 0.086 | 0.137 | 0.344 | 0.212 | 0.409 |
| Quintile 2 | 0.031 | 0.172 | 0.136 | 0.343 | 0.273 | 0.446 |
| Quintile 3 | 0.069 | 0.253 | 0.137 | 0.344 | 0.376 | 0.484 |
| Quintile 4 | 0.140 | 0.347 | 0.133 | 0.339 | 0.540 | 0.498 |
| Quintile 5 | 0.112 | 0.316 | 0.148 | 0.355 | 0.702 | 0.457 |

# Chapter 3. Does Prohibiting Selective Admissions Mitigate the Segregative Effects of School Choice? Evidence from a Policy Change in Chile

## 1. Introduction

A theorized goal of school choice systems is to expand educational alternatives to students by breaking the link between where students live and where they go to schools (Rasell & Rothstein, 1993). At the same time, there is a widespread concern among scholars and policymakers that—while school choice policies might improve educational outcomes of underrepresented students (Figlio, Hart, & Karbownik, 2020)—this may be done at the cost of having a more segregated school system (Bifulco & Ladd, 2007; Henig, 1996; Marcotte & Dalane, 2019; Renzulli & Evans, 2005). By definition, school choice promotes self-selection (Denessen, Driessena, & Sleegers, 2005) that can enable opportunity hoarding, group avoidance, and social closure dynamics (Candipan, 2020; Fiel, 2013, 2015). Schools in many school choice systems implement non-random or cream-skimming admission processes (i.e., test scores, interviews with parents), segregating students based on particular abilities and other background characteristics (Epple & Romano, 1998). Further, the amount and quality of information available to parents differ among families of different backgrounds (Hastings & Weinstein, 2008). Public and publicly subsidized private schools (outside of the U.S.) also can charge fees and tuition to families in some school choice systems, segregating students based on their income status and their capacity or motivation to pay (Böhlmark, Holmlund, & Lindahl, 2016; Elacqua, 2012).

Understanding the processes and mechanisms that influence between-school segregation in school choice contexts merit rigorous study. Several authors have descriptively documented an association between school choice and school segregation (e.g., García, 2008). Some authors also have been able to identify the causal effects of establishing a school choice system on the prevalence of between-school segregation, documenting a positive association between school choice and segregation (e.g., Monarrez, Kisida, & Chingos, 2022; Söderström & Uusitalo, 2010). However, due to the concurrent nature of self-selection, social closure dynamics, information limitations, fee-charging, and non-random admission procedures in a school choice system, most studies have not been able to identify the partial contribution of each of these mechanisms on the level of between-school segregation in a given school choice setting. We need robust evidence on the specific policy options that can help ensure that school choice systems work as intended.

In this paper, I directly test the effect of two of these five mechanisms—i.e., fees and selective admissions—on between-school socioeconomic segregation by leveraging a recent policy shock in Chile called the Inclusion Law that includes components aimed at offsetting each, but where each was rolled out separately. Several decades before implementation of the Inclusion Law, in 1981, Chile adopted a universal school choice system. According to several researchers, selective admissions allowed in this setting are responsible for the high levels of between-school socioeconomic segregation that the school system exhibits (e.g., Valenzuela, Bellei & De Los Rios, 2013). In order to reduce school segregation, in 2015, the Chilean Congress passed the Inclusion Law, which introduced two primary changes in school regulation. First, it prohibits all schools receiving public funds, both private and municipal, from charging tuition to families. Second, it prohibits non-random admission by schools, introducing a centralized lottery-based deferred admission system.

In this context, I ask two research questions: 1) What is the effect of making schools free, instead of allowing schools to charge tuition to students and their families, on between-school socioeconomic segregation? 2) What is the effect of using a centralized, lottery-based admissions system to assign students to schools, instead of allowing each school to select students non-randomly, on between-school socioeconomic segregation? The staggered rollout of the policy that introduces these features is the basis for estimating the causal effects of each of these policy components on between-school segregation through difference-in-differences, two-way fixed effects, and event study analyses. I examine the effect of the policy and its two components on changes in between-school segregation, as measured by the dissimilarity index, within a municipality.[11] I focus on changes at the municipality level because this is arguably the relevant subdivision at which the set of school choices for families varies (or not) due to the policy. While the municipality in which a family lives does not bound their school options, between 80% and 85% of the students go to a school in the same municipality in which they live (Centro de Estudios MINEDUC, 2020a). Therefore, as I will explain in detail below, even if the free component is primarily implemented at the school level, and the admission component at the region level, the sorting of students into schools depends on the set of choices available to them, which is exogenously varying for families at the municipality level as a result of the policy.

Results for the first years since the introduction and implementation of the policy show that prohibiting selective admissions significantly reduced between-school socioeconomic segregation in Chile. On average, introducing the free policy component reduced segregation by approximately 2.3 to 2.6 percentage points, while introducing the admission component reduced it by 0.8 to 1.8 percentage points. These results are robust to different sample restrictions, treatment definitions, and alternative measurements of the outcome. Further, heterogeneity analyses suggest that consistent with the policy implementation, the effects of the free policy

[11] A municipality is the smallest administrative subdivision in Chile. Large cities such as Santiago or Concepción contain many municipalities.

component are larger when all grade levels are considered. In comparison, the effects of the admission component are larger for students in entry-level grades (e.g., PK, K, 1, 7, and 9); by definition, the admission law should affect the students at the point of entry into a school, rather than students already enrolled in a school. Heterogeneity analyses also suggest that as competition between schools increases within a municipality, the effects of both policy components become stronger, as we would expect in school choice contexts.

The results presented in this paper confirm that the rules that govern school enrollment in a school choice system are central to understanding the association between such a system and between-school segregation. The estimated effects of the policy implemented in Chile demonstrate that prohibiting school selective admissions (i.e., cream-skimming and charging tuition fees), and, in turn, allowing families to exercise their preferences, may reduce, at least partially, socioeconomic segregation between schools in school choice contexts.

The remainder of this paper is organized as follows. The following two sections introduce contextual information about the Chilean education system and the Inclusion Law. The fourth section presents the data, sample, and measures, and the fifth section discusses the empirical strategy. In the sixth section, I provide results and discuss threats to identifying causal effects. The last section discusses the implications of these findings. The conceptual framework for the full dissertation—which includes the framework for this paper—is outlined in Chapter 1.

## 2. Background on the Chilean Education System

The Chilean education system is characterized by the decentralization of public schools, the availability of public funding to private schools, and a nationwide voucher and school choice system (Elacqua, 2012; McEwan, Urquiola & Vegas, 2008). The system enrolls approximately 3.4 million students, distributed in nearly 12,000 schools. Almost 40% of the students attend schools in Santiago, Chile's capital. The country is split into 16 regions, which are further subdivided into 56 provinces and 346 municipalities. Municipalities are the structure of local government and also run local public-school systems. Importantly for this paper, I focus on

There are three types of school providers: municipal schools (public schools administered by the respective municipalities); publicly subsidized private schools (schools administered by private not-for-profit or for-profit organizations that receive a public subsidy per student of the same amount as municipal schools); and non-subsidized private schools (schools administered by private not-for-profit or for-profit organizations that do not receive public subsidies). While municipal schools enrolled almost 60% of students in the country in 1990, due to shifting parental preferences these schools enrolled approximately 37% of students in 2015. On the other hand, publicly subsidized private schools had increased their participation in the education market from an enrollment of 33% of the students in 1990 to 55% of the enrollment in 2015 (see Figure 1). In terms of the schools' socioeconomic composition, the differences are substantial: students at the private non-subsidized schools come primarily from high-income homes; 52% of students at the publicly subsidized private schools come from middle-class homes; and 76% of students at the municipal schools come from low-income homes (OECD, 2013).

In this context, school socioeconomic segregation in Chile has been largely documented. According to Valenzuela, Bellei & De Los Rios (2013), students in the top and bottom 30% in terms of income distribution tend to enroll in schools with estimated average segregation of 0.5 points in the dissimilarity index.[12] The authors document differences between types of school and grade levels. Students are more segregated in primary schools (as compared to secondary

[12] The dissimilarity index ranges between 0 and 1, where 0 implies total integration and 1 implies total segregation. According to Glaeser and Vigdor (2001), values in the dissimilarity index above 0.6 are considered hyper segregation.

schools) and in private subsidized and non-subsidized schools (as compared to public schools). Other researchers have reported similar estimates (see, for example, by Elacqua, 2012; Mizala & Torche, 2012; Mizala & Urquiola, 2013; Hsieh & Urquiola, 2006).

Over the years, several policy reforms attempted by the central government have focused on reducing inequality in the education system. Besides the Inclusion Law (2015), which is the focus of this paper and which I discuss in detail in the next section, two other policy reforms could have influenced segregation trends in the country (Gutierrez & Carrasco, 2021). In 2009, a reform to the voucher system was introduced—the *Subvención Escolar Preferencial (SEP)* program, which increased the voucher for socioeconomically vulnerable students. The main objective of this policy was to reduce investment disparities between vulnerable and non-vulnerable students. The policy also attempted to reduce school socioeconomic segregation by incentivizing schools to receive vulnerable students and mandating schools to exempt vulnerable students from paying fees. However, as Murnane and others (2017) document, the policy did not produce changes in between-school segregation by income. In 2010, a new *General Law of Education* was passed. One of the components of this law was to limit selective admissions, prohibiting schools from implementing any admission process for students entering grades 1 through 6. However, researchers have argued that enforcement of this mandate was weak (e.g., Contreras, Sepúlveda, & Bustos, 2010; Carrasco, Gutierrez, & Flores, 2017; Santos & Elacqua, 2016), limiting its potential impact on segregation trends.

## 3. The Inclusion Law

The Inclusion Law was first introduced to the public on May 19, 2014, was passed on May 29, 2015, and started to be gradually implemented on January 1, 2016, to "make progress in ending the structural inequalities of the education system so that the State of Chile guarantees the right to quality education for all its population" (BCN, 2015, p. 8). The law prohibits subsidized schools from charging tuition or fees to their students to achieve this objective. It also mandates creating a centralized admission system that enforces the prohibition of screening and selectively admitting students into schools.[13] In this paper, I focus on the effects of these two policy changes on between-school socioeconomic segregation during the first four years of implementation through 2019.[14]

### *3.1. Making Subsidized Schools Free*

In 2015, 96% of municipal schools and 33% of the private subsidized schools were free (Centro de Estudios MINEDUC, 2020). The policy's aim was that, by 2022-2023, all subsidized PK-12 schools would be free. To accomplish this objective, the first regulation introduced by this policy was to mandate subsidized schools to freeze their tuition amount to the value they were charging in August 2015. Also, if schools were not charging any tuition in 2015, they could not start charging tuition in 2016. Second, this policy introduced a new subsidy (*aporte gratuidad*) that schools start receiving once they eliminate parental fees. Schools that were free before the policy also receive this subsidy. Schools were allowed to opt-in to this subsidy once they eliminated their fees but were forced into it once the subsidy was equal to or larger than the fees that schools were charging in August 2016. According to this policy reform, all schools would be

[13] Until 2015, approximately 40% of the schools in the Chilean education system were legally constituted as for-profit organizations. Between 2016 and 2018, the law mandates that all schools constituted as for-profit schools follow legal procedures to become non-profit foundations or corporations. The law also mandates publicly subsidized private schools to legally constitute as non-for-profit organizations. While transforming for-profit schools into non-profit schools may affect the education market, the probable effects on segregation seem less clear.

[14] While 2020 data is available, I decided not to include it because of potential distortions in enrollment patterns due to COVID. For example, school dropouts sharply increased during the pandemic, disproportionally affecting low-income students (Centro de Estudios MINEDUC, 2020b).

free once the subsidy is equal to or larger than the fees that subsidized schools with the highest tuition fees were charging before the policy change.[15]

Interestingly, the exact year-to-year increment of the voucher, which effectively defines which schools become free, depends on the available governmental budget and cannot be manipulated by the schools. Therefore, it can be argued that when a specific school is scheduled to become free depends on an exogenous source of variation.

### *3.2. Implementing a Centralized Admission System*

The centralized admission component of this policy (*Sistema de Admisión Escolar, SAE)* started operating for the 2017 admission to entry grades—i.e., grades PK, K, 1, 7, and 9—in one out of the 16 administrative regions in the country. In the 2018 admission cycle, the system advanced to all grades in that first region and entry grades in four additional regions. In 2019, it reached all grades in those new four regions and to entry grades in five more regions. In the 2020 admission cycle, the system was extended to all grades in the 2019 new regions and to the entry levels of the Metropolitan Region of Santiago, where the capital is. By 2021, the system was operating in all regions and levels. This implementation in stages, from smaller to bigger regions, creates variation that can be used to analyze the policy's effects.

The centralized admission system works as follows. Parents and guardians who want to enroll their children in a school in a participating region need to list and rank their school preferences on a website. Parents are assigned to their first preference unless that school has more applications than seats. If the school is oversubscribed, schools must follow specific procedures to create randomized lists of potentially admitted students. Schools send these lists to

[15] For example, the amount of the subsidy in 2016 was CLP$5,809 per student/month. All schools that in 2015 were charging less than CLP$5,809 per month to families started receiving this new subsidy in 2016 and were mandated to become free. All schools charging more than CLP$5,809 in 2015 did not receive the subsidy and were allowed to keep charging tuition. Figure A1 graphically summarizes this process.

the Ministry of Education. A deferred acceptance algorithm, which maximizes the chances of each family enrolling the student in one of their top preferences, is run at the central government level. Once the lottery results are informed to families, families can enroll in their assigned school or wait for a second process. Appendix Figure 1 graphically summarizes this process.[16]

### *3.3. Treated Municipalities: First Four Years of Implementation*

Based on the policy's rollout, municipalities have been affected over the first four years of implementation (2016-2019) by neither component, the admission component, the free component, or both policy components. There are never affected by the policy, scheduled to be treated, and actively treated municipalities for each component.

For the free component of the policy, I define municipalities as being never affected by the policy as those where 70% or more schools were free before the policy. I identified all other municipalities as scheduled to be treated. Municipalities became actively treated once 85% or more of the schools became free between 2016 and 2019. [17] In identifying schools within a municipality as being actively treated by the free component, I considered schools as turning free once they were scheduled to do so, not if they opted into the policy.

Regarding the admission component, identifying a municipality as never affected by the policy, scheduled to be treated, and actively treated by the admission system depends on its location. Municipalities in the Metropolitan region ($n$ = 52) are the only ones that were never treated between 2016 and 2019.[18] All other municipalities ($n$ = 294) are scheduled to be treated.

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[16] The centralized admission system considers some priority groups: former students and students with parents working or siblings at the school. It also allows for quotas for vulnerable students, students with special education needs, and students that demonstrate high academic performance. For a detailed account of this system, see Correa and others (2019).
[17] Both cutoff points (70% or more, 85% or more) are arbitrary. To check how robust the results are to different cutoff points I ran estimations of the effects of the policy using alternative definitions of the free-treatment component at the municipality level. I present these estimations in the results section.
[18] Municipalities in the Metropolitan Region started implementing the admission system in 2020.

These municipalities turned to actively treated once the region they are located in became part of the admission component.

Table 1 summarizes the number and percentage of municipalities actively treated by either, both, or neither of the policy components between 2016 and 2019.

### *3.4. Previous Research*

To date, research available on this new policy has focused on characterizing how school actors and families interact with these changes (Canales, Guajardo, Orellana, Bellei, & Contreras, 2020; Carrasco, Oyarzún, Bonilla, Honey, & Díaz, 2019; Sillard, Garay, & Troncoso, 2018), how preferences are distributed among families (Eyzaguirre, Hernando, & Blanco, 2018; Eyzaguirre, Hernando, Razmilic, Blanco, Figueroa, Tagle, & Icaran, 2019a, 2019b), how the policy may reduce income inequalities in school enrollment (Carrasco & Honey, 2019), and the limited effect of the policy in increasing low-income students' access to selective schools (Honey & Carrasco, 2022). Only the work by Kutscher, Nath, and Urzua (2020) has attempted to assess the effect of this policy on between-school segregation among schools.

In their paper, the authors estimate that, on average, the introduction of the centralized admission system had no statistically significant effects on between-school segregation (Kutscher et al., 2020). Furthermore, they document increases in segregation in specific municipalities, conditional on pre-existing levels of residential segregation and characteristics of the education market. However, this work has three important limitations. First, it relies on self-reported socioeconomic data collected in the context of the administration of the national standardized test, which underrepresents low-performing students (Cuesta, Gonzalez, & Larroulet, 2020). According to Kutscher and coauthors, approximately 80% of their sample have valid background information. Second, it focuses on students transitioning to secondary education instead of entering primary education. While many students switch schools in Chile at the end of primary education, not all need to do it. According to the authors, approximately half of the students used the centralized admission system in regions where it was operating. The other half stayed in the schools in which they were previously enrolled. Finally, the authors focus on the centralized admission component of the policy, omitting the free component from their estimations. As Table 1 shows, the free component affected almost 10% of schools by year four of implementing the law. Also, it started impacting schools and municipalities before the admission component was in place.

## 4. Data, Sample, and Measures

### *4.1. Data*

This project relies on administrative and publicly available data. All datasets described below are census data (i.e., they consider all schools and students in the school system).

***Students General Information System (SIGE).*** This dataset contains the main characteristics of the students, classrooms, and schools. At the student level, it identifies which classroom and school each student is enrolled in. At the school level, it provides a set of characteristics such as the region, province, city, and municipality in which the school is located, whether the school is municipal, publicly subsidized private, or private, and whether the school is urban or rural, among others. Each student and school have a unique identification number that allows merging this dataset with others. This dataset is available from 2002 to 2019.

***Vulnerable Students Dataset (SEP).*** This dataset contains all the students identified as vulnerable in socioeconomic terms based on different sources of information (e.g., social security, health, income). Among other uses, this information is used to assign resources to schools, depending not only on the total number of vulnerable students in the school but also on the proportion of the school's total students they represent. This dataset can be merged with others using the individual or the school identification number. SEP data is available from 2008 to 2019.

***Co-Payment Dataset (FICOM).*** This dataset contains information on tuition and fees schools charge to parents, both by year and month. It is available from 2004 to 2019. Combined with administrative information on the increments in the voucher, it allows determining which schools will become free between 2016 and 2019, based on their initial copayment level.

***Educational Admission System (SAE).*** This dataset contains information on the centralized admission system. Among other information, it captures the number of applicants per available seat. It can be merged with the other datasets using the school identification number. This dataset is available from 2017 to 2019.

***System for Measuring Education Quality (SIMCE).*** Finally, to include school and municipality quality measures as controls, I use school-level results in the national standardized test available yearly for $4^{th}$ grade in the SIMCE dataset. Data from SIMCE can be merged with other datasets using the school identification number. This data is available from 2002 to 2019.

### *4.2. Sample*

In all analyses, the sample includes students enrolled in grades PK through $8^{th}$ in primary schools between 2009 and 2019. I focused on these grade levels and years because the SEP data, which allows identifying vulnerable students and calculating between-school socioeconomic segregation indices, is only available after 2009 and only for students in PK to $8^{th}$ grade (pre-primary through middle school). Further, because of the focus of the policy, I focused on traditional primary schools, excluding special education schools, secondary schools that offer $7^{th}$ and $8^{th}$ grade, preschools that offer pre-primary levels (e.g., preschools), and adult schools.

Finally, because I am interested in municipalities in which families have more than one school option to choose from, I excluded municipalities with less than five schools. These are rural municipalities, representing approximately 8% of the municipalities and accounting for less than 1% of the schools. Table 2 presents descriptive statistics for the sample before (2009-2015) and after (2016-2019) the policy implementation.[19]

### *4.3. Socioeconomic Indicator and Outcome Measure*

This paper focuses on the distribution of students of different socioeconomic backgrounds between schools within a municipality. To identify a student's socioeconomic status, I used a publicly available administrative indicator that classifies students as vulnerable or not vulnerable in socioeconomic terms. This is an indicator at the student/year level, and it may change over time if the socioeconomic status of the student and their family changes. As such, there is a potential concern of endogeneity that needs to be examined. Because of its focus, the policy may affect not only the distribution of students of different socioeconomic characteristics between schools but also the socioeconomic status of students. For example, if a student was paying tuition before the policy and their school became free because of the policy, that change potentially affected their socioeconomic status. Based on my calculations, 74.2% of students that were enrolled before and did not experience the policy change, as well as 80.4% that enrolled in the school system after the policy was already in place, had a stable socioeconomic indicator. This number goes down to 55.4% when I consider students enrolled when the policy was

[19] Table 2 presents descriptive statistics of the sample as they show in the Chilean education system, before and after the policy. However, I adjusted this sample in a few ways for analysis purposes. First, some municipalities were administratively divided or merged in the period under analysis. I fixed the municipalities to the ones that existed in 2015 and used that municipality to identify schools' municipalities and regions. Second, some schools changed their free status, one way or the other, before the policy change. Because their free status in 2015 determined their treatment status by the free treatment component, I used their 2015 free status as their status for the pre-policy years. Similarly, private schools may opt-in or out of the public subsidy. Because private non-subsidized schools are not affected by the free or the admission treatment, their treatment status depends on whether they received public funding or not in 2015. I use their 2015 status to determine their treatment eligibility.

implemented. In other words, students who experienced the policy implementation are less likely to have a stable socioeconomic indicator than students enrolled before or after.

To avoid this potential source of endogeneity, in creating the outcome measure of between-school segregation, instead of relying on the original student/year indicator, I used a stable socioeconomic indicator per student, namely, the first socioeconomic status available for each student in the panel data. As I discuss later, I checked the robustness of the results to two alternative indicators of socioeconomic status, i.e., the administrative original student/year indicator and the one closest to the policy.

To determine changes in between-school socioeconomic segregation I rely on the dissimilarity index as the outcome measure. This index is one of the most common measures used in the between-school segregation literature in the presence of a two-group category (in this case, socioeconomically vulnerable vs. socioeconomically not vulnerable). Formally, the index is calculated as follows:

$$D_m = \frac{1}{2}\sum_{i=1}^{S}\left|\frac{nv_i}{NV} - \frac{v_i}{V}\right| \tag{1}$$

where NV and V represent the total number of not vulnerable and vulnerable students, respectively, in municipality $m$. The total number of schools in municipality $m$ is defined by S, and the number of students in school $i$ for group NV and V are $nv_i$ and $v_i$, respectively. The dissimilarity index $D$ ranges from zero to one and it is interpreted as the proportion of students that should be transferred from one school to another to achieve an equal distribution of students of these two characteristics in a specific territory (James and Taeuber, 1985; Reardon and Firebaugh, 2002).

## 5. Empirical Strategy

Based on the different steps in which the policy was introduced to the public and finally implemented, I considered different shocks or entry points to the policy. The law's actual implementation was preceded by two years of intense debate in Congress and public discussions among school administrators, school communities, and families (Canales et al., 2020). The presentation and discussion of the bill in 2014 included details on the staggered implementation of each policy component. The final approval in 2015, on the other hand, froze tuitions and fees charged by schools, effectively introducing an element of the free component from the beginning. Therefore, in this paper, I anticipate and model different entry points to the policy and estimate the effects of two pre-implementation stages: the discussion and the approval stages, using two discrete cut-off points. In this context, I estimated, first, the following difference-in-difference model:

$$\begin{aligned} Y_{ijt} = {} & \alpha + \zeta Post_t + \delta Free_i + \theta Admission_i + \vartheta(Free_i * Post_t) \\ & + \beta\left(Admission_{ij} * Post_t\right) + \partial_i + \gamma_t + \tau' M_{it} + \varepsilon_{it} \end{aligned} \tag{2}$$

where $Y_{it}$ is the outcome of interest (i.e., dissimilarity index) in municipality *i* and year *t;* $\alpha$ is a constant; $\zeta$ is a dichotomic indicator of time with value of 1 on year 2014 (introduction and debate of the bill) or on year 2015 (final approval and promulgation); $\delta$ and $\theta$ are dichotomic indicators of municipalities *i* are scheduled to be treated with the free and/or the admission policy components, respectively; $\partial_i$ is a group fixed effect, that is municipality *i;* $\gamma_t$ is a time (year) fixed effects; $M_{ijt}$ is a vector of characteristics of municipality *i* in a given year *t*, such as the number of schools, proportion of private schools, proportion of free schools, proportion of female students, the proportion of rural schools, the proportion of students that live in the same municipality as the school they are enrolled in, and the mean and standard deviation of school test results in the municipality; and $\varepsilon_{it}$ is an error term. The coefficients of interest are $\vartheta$ and $\beta$, which capture the effect of the free and the admission component, respectively, among scheduled to be treated municipalities post the two points of entry to the policy. Robust standard errors are clustered at the municipality level.

To model the actual implementation and to more directly estimate the effect of the two components of the policy on changes in between-school socioeconomic segregation at the municipality level, I estimated the following two-way fixed effect (TWFE) model:

$$Y_{it} = \alpha + \beta Free_{it} + \theta Admission_{it} + \partial_i + \gamma_t + \vartheta' M_{it} + \varepsilon_{it} \tag{3}$$

where $Y_{it}$ is the outcome of interest (i.e., dissimilarity index) in municipality *i* and year *t;* $\alpha$ is a constant; $\delta$ and are dichotomic indicators of municipalities *i* are scheduled to be treated with the free and/or the admission policy components, respectively; $\beta$ and $\theta$ are dichotomic indicators of municipalities that are actively treated in a given year by the free and admission component, respectively; $\partial_i$ is a group fixed effect, that is municipality *i;* $\gamma_t$ is a time (year) fixed effects; $M_{it}$ is a vector of characteristics of municipality *i* in a given year *t*, such as the number of schools, proportion of private schools, proportion of free schools, proportion of female students, the proportion of rural schools, the proportion of students that live in the same municipality as the school they are enrolled in, and the mean and standard deviation of school test results in the municipality; and $\varepsilon_{it}$ is an error term. The coefficients of interest are $\beta$ and $\theta$, which capture the effect of the free and the admission component, respectively, among scheduled to be treated municipalities once the treatment becomes actively implemented in the municipality. I clustered robust standard errors at the municipality level. As I discuss in the next section, I ran a series of robustness and heterogeneity analyses using this approach.

Finally, following recent developments in the difference-in-difference literature (see, for example, Callaway & Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021), I additionally conduct an event study for different treatment cohorts. Specifically, I estimate the following model:

$$Y_{ijt} = \sum_{p=-n}^{n} 1(Free = Free_i^* + l)\vartheta p + 1(Admission = Admission_i^* + l)\beta p + \theta' M_{it} + \rho_i + \gamma_t + \varepsilon_{it} \quad (4)$$

where $Y_{ijt}$ is the outcome of interest (i.e., dissimilarity index) in municipality *i* and year *t;* controlling for $M_{ijt}$, which is a vector of characteristics of municipality *i* in a given year *t*; with group fixed effect ($\partial_i$), that is municipality $i,$ and time (year) fixed effects ($\gamma_t$). The terms $Free_i^*$ and $Admission_i^*$ identify the year of implementation of the policy component in municipality *i*, such that $1(Policy\ Component = Policy\ Component_i^* + l)$ creates indicators for the years before and after the component implementation. The coefficient $\vartheta p$ and $\beta p$ capture the effect of the free and admission components, respectively, on segregation *p* years before or after the policy component.

Importantly, in equations 2-4 above, I always include both policy components in the estimations of each policy component effects. In other words, when estimating the effect on segregation of the free component, I control by the admission component, and vice versa. This is relevant, first, because the timing of implementation of each policy component was not the same in each municipality (see Table 1). Furthermore, if estimations were done separately for each treatment component, coefficients would be likely biased downwards. This would likely happen because the control group on those cases will consider municipalities treated by the other policy component, where segregation was also impacted.

## 6. Results

### *6.1. Average Treatment Effects*

Table 3 presents the effect of each policy component on changes in between-school socioeconomic segregation, as measured by the dissimilarity index, at the municipality level.[20] Columns 1 and 2 present estimates from the difference-in-difference approach using the post-debate and the post-promulgation indicators, respectively, as entry points to the policy. On average, municipalities scheduled to be treated by the free treatment component, compared to those not scheduled to be treated, exhibit an estimated decrease in between-school segregation of 2.45 and 2.61 percentage points in the years after the debate of the bill and the promulgation of the law, respectively. Further, municipalities scheduled to be treated by the admission component show an estimated decrease in between-school segregation of 1.86 and 1.75 percentage points in the years after each entry point to the policy, on average.

Column 3 focuses on the TWFE models and presents the estimated causal effect of each policy component on scheduled to be treated municipalities after they become actively treated, as compared to not treated municipalities, both scheduled and not scheduled. On average, municipalities scheduled to be treated by the free treatment component show reductions in between-school socioeconomic segregation, as measured by the dissimilarity index, of 2.29 percentage points after they become actively treated (i.e., at least 85% of the school in the municipality become free). On the other hand, municipalities scheduled to be treated by the admission component exhibit reductions of the dissimilarity index of 0.83 percentage points once the admission component kicks in (i.e., all schools in the municipality participate in the

[20] Appendix Table 2 replicates this table using two additional segregation measures, i.e., the information theory segregation index, and the Gini index.

centralized admission system).[21] As Table 3 shows, estimates using the difference-in-difference and the TWFE approaches are remarkable consistent.

### *6.2. Heterogeneous Treatment Effects*

Considering the implementation process of the policy, I would expect to see variation in the treatment effect associated with which grade levels and which component of the treatment are included in the analysis. Once a school becomes free due to this policy change, all grade levels within that school are free of tuition for all families. Therefore, within municipalities that are defined as treated by the free component in this analysis, all grade levels in at least 85% of the schools in the municipality became free of tuition for families once this component of the policy kicks in. Therefore, I would expect to see larger effects when all grade levels are considered in the analysis. This grade level coverage does not occur with the admission component. As described before, during the first year of implementation of the admission system, only entry-level grades in affected regions participate in the admission system. Admission to non-entry grade levels becomes part of the centralized admission system only starting in the second year of implementing the policy in each municipality. This characteristic of the implementation of the admission system, on top of a potential learning process at the community level on how to navigate the system itself, would likely translates into larger effects of the admission component when I restrict the analysis to entry-level grades.

Tables 4 confirms the hypothesis above. In this table, I present the effect of each policy component on between-school segregation, restricting the sample to entry-level grades (i.e., PK,

[21] Importantly, as I described in the empirical strategy section, all the estimated effects of the free and the admission policy component on segregation control for the presence of the other policy component. Otherwise, estimators will be likely biased downwards. I confirm this hypothesis by replicating models in Table 3 but estimating regressions that include only one policy component at a time. I present the results in Appendix Table 1. As expected, results go in the same direction and are all statistically different from zero, but coefficients are smaller in magnitude.

K, 1st grade, and 7th grade) that should experience the biggest effect from the admissions component. I observe, first, a negative but statistically not different from zero coefficient for the free treatment component when the sample is restricted to entry-level grades. As predicted, these estimates are smaller in magnitude than when all grade levels are included in the analysis (Table 3, column 3). Regarding the admission treatment component, I document an estimated significant reduction of 1.23 percentage points in between-school segregation in municipalities actively treated by the centralized admission component, on average. Compared to the coefficient considering all grade levels (Table 3, column 3), the estimated effect is approximately 50% larger when I focus on entry grade levels.

The second approach to heterogeneous effects of the policy focuses on between-school competition within a municipality. Assuming that, before the policy, schools facing greater competition could have incentives to non-randomly select students (Epple & Romano, 1998), eliminating barriers to parental preferences in a school choice system should have larger effects where competition between schools is greater. Before the policy passed, schools were allowed to select their students by charging tuition or defining arbitrary characteristics that they would expect to see in their applicants. This practice meant that different schools effectively targeted different populations, ultimately limiting parental options. After the policy change, schools are not allowed to select their students in any way. This prohibition increases school options for parents and forces schools within a certain territory to enroll students from a common, non-stratified pool.

To test this hypothesis, I conducted a series of heterogeneity analyses in which I restrict the sample of analysis to municipalities with varying levels of competition. I use several proxies of competition to assess the effect of the policy. First, similar to Gallego (2013), I use the share of private schools within a municipality as a proxy. Considering that most non-free schools before the policy were private and that publicly subsidized private schools were more likely to adopt cream-skimming mechanisms (Carrasco et al., 2017), municipalities with a larger share of private schools should experience increased competition after the policy change. Second, I use the proportion of urban schools within a municipality as another competition proxy. In this case, the larger the share of urban schools in a municipality means that schools are likely concentrated in a somewhat shared geographic area. Therefore, schools in municipalities with a larger proportion of urban schools probably face more competition than those isolated in rural areas.

Finally, I also use the proportion of students who live in the same municipality where the school they are enrolled is located as another proxy. I interpret this parameter as a measure of municipality (not school) isolation. As I mentioned before, school options for students in Chile are not bounded to the municipality in which they live. A larger proportion of students staying within municipality lines potentially means that the municipality is away from other municipalities, so traveling to the closer municipality is not feasible. It may also mean that the school market in which students and schools are located better aligns with parental preferences for any reason, such that options outside the municipality are less attractive. If more students go to a school within the municipality, schools in those municipalities are directly competing for the same students.

For each of the three measures of competition, I divided the municipalities into deciles in terms of the distribution of each variable. I then compare municipalities in the top 80 (percentile 20th or higher) and the top 50 (percentile 50th or higher).[22] Table 5 summarizes the result of this

[22] Correlation between decile groups that result from using these three competition measures shows that the correlation between the proxy same-municipality, on the one hand, and private and urban, on the other, is negative (-0.244 and -0.166, respectively). In turn, the correlation between private and urban is positive (0.655). This

analysis. In almost all cases, the magnitude of the effect is larger when I restrict the sample to municipalities in which schools face greater competition (i.e., municipalities in the top 50 of the distribution, as compared to municipalities in the top 80, and both compared to all municipalities in the panel data). Coefficients are also larger when all grades are included in the analysis than when only entry-level grades are considered. Estimated coefficients are, in almost all cases, statistically different from zero for the free treatment component. For the admission component, only the coefficients for the proportion of students who live in the same municipality where they are enrolled are statistically different from zero and larger than those presented in tables 3 and 4. Between-school socioeconomic segregation decreases between 0.86 and 1.96 percentage points in municipalities in or above the 80th percentile of the distribution of this variable, with larger coefficients when I restrict the sample to entry-level grades.

### *6.3. Robustness Checks*

I conducted a series of robustness checks to evaluate whether the results presented in Tables 3 and 4 hold if I introduce specific changes in the sample, measurement of the outcome, or the definition of the free treatment component. As the appendix tables show, the TWFE results presented in Table 3 (column 3) and Table 4 are robust to all these different checks.

In terms of the sample, I compared the results in tables 3 and 4 to specifications using a balanced sample of municipalities that consistently show across all years in the panel data. Because I limited the sample of municipalities to those having at least five schools in a given year, if the number of schools within a municipality went below five from one year to the next one, then that municipality is not used in the estimations for that year. Therefore, by limiting the

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correlation indicates that, while there is some overlap between municipalities, these different competition proxies capture and compare, at least partially, different municipalities.

analysis to municipalities that consistently appear in the data, I effectively check that results are robust to the exclusion of the smallest municipalities. Coefficients using this balanced sample of municipalities are slightly larger (see Appendix Table 2).

Next, I consider how the measurement of the outcome measure affects results. I start first with the student-level vulnerability indicator, which I then use to create a measure of between-school segregation. As previously discussed, the original indicator of socioeconomic vulnerability may be endogenous to the implementation of the policy, which justified using an alternative indicator. Here, I estimated the TWFE models using a between-school segregation outcome based on the original student/year socioeconomic indicator and the socioeconomic indicator closest to the year before the implementation of the policy. In both cases, the results are similar to the ones presented in tables 3 and 4 (see Appendix Table 3).

Concerning the between-school segregation index, while the dissimilarity index is the most common measure of segregation when one care about evenness in the distribution of two groups across units, some authors suggest using the information-theory H index or the Gini index instead (see, for example, Clotfelter, 1998; Yun & Reardon, 2002). Therefore, I estimated the effects of the policy using these alternative indices. While the coefficients vary slightly, in all cases, the direction and magnitude of the effect are comparable to the estimates presented in tables 3 and 4 (see Appendix Table 4).

Finally, I estimated TWFE models using alternative definitions of the free treatment component (see Appendix Table 5). As previously discussed, in defining scheduled to be treated and actively treated municipalities, I relied on arbitrary cutoffs. To check if the results are robust to different cutoff points, I estimated the TWFE models using alternative arbitrary cutoff points. Except for the effect of the free treatment component in entry-level grades (results in three out of four specifications change sign but are still not statistically different from zero), all the other models using alternative definitions of the free treatment component are similar to the ones presented in tables 3 and 4, both in terms of direction and magnitude.

### ***6.4. Identification Threats***

Estimating causal effects using a difference-in-difference approach is based on two primary assumptions (Murnane & Willett, 2010). First, the intervention is unrelated to the outcome at baseline. Second, treatment and control groups have parallel trends in the outcome of interest before the policy. In the context of this paper, evaluating these assumptions is particularly important in reference to the difference-in-difference estimations presented before (see Table 3, columns 1 and 2).

I ran a falsification test to assess the first assumption and investigate concerns around selection into the treatment. I regressed the outcome of interest (i.e., the dissimilarity index) on a dichotomic variable that results from the interaction between a treatment indicator and a placebo post-period. In terms of the treatment markers, I first estimated the models with an indicator of a municipality as eligible or scheduled to be treated by each treatment component. I then estimated the models with an indicator of being actively treated in the post-implementation period. Regarding the placebo post-indicator, I set it up in 2013, before any of the potential policy entry points were in place. In theory, we would expect no changes in the outcome measure in municipalities scheduled to be treated or actively treated before the policy was implemented.

In terms of the admission component, Table 6 (see panel B) confirms this hypothesis. Controlling by municipality level characteristics and municipality and year fixed effects, we observe no changes in the outcome measure when the post placebo indicator is introduced. These results hold when all or entry-level grades are included in the sample, and when both eligible and treated municipalities are considered.

Regarding the free treatment component, eligible municipalities show an increment in between-school socioeconomic segregation in the year before the policy started being debated (see Table 6, panel A, columns 1 and 3). While in other contexts this will raise concerns regarding potential selection into the treatment, in this case, these concerns are attenuated by the results presented for actively treated municipalities (see Table 6, panel A, columns 2 and 4). Actively treated municipalities, which are the ones pseudo-randomized into the treatment by exogeneous year increments of the voucher, do not exhibit significant changes in the outcome of interest before the policy was implemented.

Regarding the parallel trends assumption, Figure 2 compares the trends of the outcome measure before the policy change for municipalities scheduled and not scheduled to be treated with each component of the policy. As this figure shows, pre-trends of the outcome measure seem to follow parallel trends before the policy.

Turning into TWFE estimations, identification threats require a detailed discussion. Traditionally, quasi-experimental research designs that rely on a difference-in-difference approximation are built around a 2x2 framework, with a clear pre-post timeline and distinct treatment-control groups (Angrist & Pischke, 2009; Bertrand, Duflo, & Mullainathan, 2004; Murnane & Willett, 2010). However, policy interventions are often introduced in stages, such that treated units adopt them in different moments, blurring the pre-post cut-off point. In these situations, Athey and Imbens (2018) argue that if treatment adoption is done at random, a traditional difference-in-difference approach still provides an unbiased average estimator of the effect of the treatment. Nevertheless, some authors have proposed to use a slightly different approach in these cases, in which the estimation procedure accounts for the staggered nature of the treatment. In these staggered difference-in-difference approaches, the pre-post indicator is specific to the treated unit, such that, out of a group of eligible or scheduled to be treated units, the post indicator of treatment turns into one once the treatment kicks in for that unit (see, for example, Conti et al., 2020; Field, 2007).

In practice, these staggered approaches use TWFE models to estimate the effects of the policy under analysis. These models rely on the assumption that controlling for both time-specific and group-specific characteristics or shocks provides an unbiased estimator of the effect of the policy (see, for example, Anttila-Hughes, Fernald, Gertler, Krause, & Wydick, 2018). While popular,[23] scholars recently have raised concerns regarding the estimator that result from these models. Treatment effects in these cases represent a weighted average of several 2x2 comparisons. This feature of TWFE models raises two specific concerns. First, average effects may result from comparing never treated and treated units, between treated units at different points of implementation, or within treated units at different implementation points. If the latter represents a large proportion of the estimated average effect, then the TWFE coefficient may be biased. Second, because the timing of implementation varies across treated units, then testing the parallel trends assumption needs to be done differently (Callaway & Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021).

Regarding concerns around which comparisons are the estimated weighted average effect accounting for, I follow Goodman-Bacon's (2021) approach to decompose the weights of the estimator, using the bacondecomp command in Stata (Goodman-Bacon, Goldring, & Nichols, 2019). Table 7 and Figure 3 summarize the results of this decomposition. In line with the

[23] De Chaisemartin and d'Haultfoeuille (2020) estimate that almost 20% of all papers published in a top economic journal between 2010 and 2012 use this approach.

estimated weighted average, the coefficients are negative in all but one case. The only exception is for the free treatment component when entry-level grades are considered. Regarding the weights of the different comparisons, approximately 96% of the estimated treatment effect of the free policy component comes from comparisons between treated and never-treated municipalities. In the case of the admission component, estimated treatment effects are coming equally from comparisons between early treated municipalities against late treated municipalities (approximately 47-49%, depending on the grade level) as well as between treated and never treated municipalities (approximately 49-50%, depending on the grade level). The within-component of the decomposition, which compares late-treated municipalities to early-treated, accounts for a small proportion of the weighted average (approximately 2-3%). According to Goodman-Bacon (2021), this is the comparison we expect to avoid in a TWFE estimation. It summarizes variation that comes from the controls evolving differently across units with the same treatment indicator.

Recent literature suggests exploring pre-trends with an event study to test for parallel trends in a staggered context (Callaway & Sant'Anna, 2021). Table 8 presents the results of this approach.[24] While we would expect to see only significant coefficients on or after the implementation (i.e., starting in year 0 since the event), when all grade levels are considered, we start seeing negative and significant coefficients one year before the implementation of the free treatment component and two years before the admission treatment component. Based on the results presented in Table 3, this is not entirely surprising. I hypothesized that the policy likely had an impact before its implementation and estimated its effects using two cut-offs before the

[24] Table 8 shows three post-event periods, i.e., Year 0, Year 1, and Year 2. In practice, the free treatment component was in place for four periods. However, as I documented in Table 1, only one municipality was treated in 2016 and had four post-implementation periods. Therefore, in modeling the event study, I grouped that municipality with municipalities treated in 2017 and had three years of implementation.

components were implemented. The debate around this new regulation and its implementation was public and heated; tuition fees were frozen before the promulgation, and some schools opted-in the free treatment component before they were mandated to do so. Moreover, the discussion around the centralized admission system, which was created with this law to enforce existing regulations on the prohibition of cream-skimming and non-selective admissions in subsidized schools, likely alerted schools and parents that regulation was already in place. Therefore, while concerns around pre-trends may suggest selection into the treatment, in this case, I argue that these pre-trends are capturing different shocks introduced by the policy at different points in time, which also yield to similar point estimates of the effect of the policy.

To check whether the estimated effects hold once an alternative estimator of the event study is implemented, I estimated and plotted the results of an event study, with group and time fixed effects, using the imputation approach developed by Borusyak, Jaravel, and Spiess (2022). This estimation procedure uses never treated and not yet-treated observations to estimate a model for non-treated potential outcomes. It then imputes these potential outcomes to treated observations, estimates effects for each treated observation, and summarizes them as an average treatment effect. Figure 4 presents the results of this specification. Using this robust estimator, I confirm negative and statistically significant treatment effects after the implementation of each policy. In the case of the free treatment component, effects are different from zero in years 0 and 1 (not in year 2), while in the case of the admission component, these effects are different from zero in years 1 and 2 (not in year 0).

## 7. Discussion

Literature on school choice argues that between-school segregation is one of the major unintended consequences of introducing such systems (Bifulco & Ladd, 2007; Kotok et al., 2017; Monarrez, Kisida, & Chingos, 2022). In most education systems in which school choice has been introduced, between-school segregation has actually *increased* (e.g., Coughlan, 2018; Denice, DeArmond, & Carr, 2020; Frankenberg, Siegel-Hawley, & Wang, 2011; Glazerman, 1998; Garcia, 2008; Holme, Frankenberg, Diem, & Welton, 2013), rather than decreased as its proponents initially theorized (e.g., Moe, 2001).

In light of these troubling patterns, researchers have identified demand (e.g., self-selection, information constraints, opportunity hoarding) and supply (e.g., location, selective admissions) determinants of between-school segregation in school choice settings, with the goal of identifying policy solutions. However, rarely has empirical work been able to disentangle the effect of these determinants on the prevalence and magnitude of segregation. In this paper, I took advantage of a recent policy change in Chile that aims to eliminate two specific supply-side factors—namely, cream-skimming by ability and other characteristics and charging tuition fees to families—to assess their effect on between-school socioeconomic segregation, holding constant other determinants. In theory, if selective admission procedures increase between-school segregation, then eliminating these practices should reduce segregation levels.

This paper confirms the hypothesis above. On average, prohibiting selective admissions reduced between-school socioeconomic segregation by approximately 0.8 to 2.6 percentage points. These results are robust to different specifications of the sample, the measures, and the treatment definitions. Also, in line with the implementation procedures, effects are larger for the free treatment component when all grade levels are considered and larger for the admission component when entry-level grades are included. Furthermore, in line with theory, these effects are slightly larger as competition between schools—as measured by the municipality's proportion of private schools, the municipality's proportion of urban schools, and the municipality's proportion of students that go to schools in the same municipality in which they live—increases.

The estimated effects seem to capture the causal effect on school segregation by prohibiting selective admissions in the Chilean school choice system. For the most part, the assumptions for a difference-in-difference and an event study approach hold. While I document anticipatory effects of the actual implementation of the policy (i.e., post-debate and post-promulgation), I argue that these effects are explained by components of the policy that were introduced before its final implementation and not by municipalities self-selecting into the treatment.

Whether the magnitude of the estimated effects is substantively significant is open to discussion. There are at least three ways in which this discussion can be approached. First, by interpreting the magnitude of these effects in reference to the existing levels of between-school segregation in the country. On average, between-school socioeconomic segregation, as measured by the index of dissimilarity, was 0.276 between 2009 and 2019. This means that approximately 27.6% of students in this period needed to be relocated to a different school to achieve an even distribution of vulnerable and non-vulnerable students across schools. As I reported before, the policy reduced between-school socioeconomic segregation by approximately 0.8 and 2.6 percentage points. If we consider an average effect of 2 percentage points, that means that, after the policy implementation, 2% fewer students need to be transferred from one school to another in treated municipalities. If we consider the average 27.6% that needed to be relocated during this period, this effect is equivalent to a 7.25% reduction in the percentage of students that need to be relocated after the policy.

A second approach to this discussion is to compare this policy to other interventions aimed at reducing between-school socioeconomic segregation. In the case of Chile, the most recent policy that aimed at this objective, at least indirectly, was the *Subvención Escolar Preferencial (SEP)* program (Gutierrez & Carrasco, 2021). According to different authors, reductions in socioeconomic segregation associated with this policy are close to zero and only in some cases statistically significant (Allende et al., 2018; Dominguez, 2014; Valenzuela et al., 2013). The effects of the policy under analysis in this paper are all different from zero and not entirely neglectable.

A third and final approach is to check trends in school-based segregation. According to different scholars, trends in between-school socioeconomic segregation in industrialized and developing countries, including Chile, tend to persist over time, even after economic and policy shocks (e.g., Bonal & Bellei, 2020; Gutierrez, Jerrim, & Torres, 2020). On the other hand, if we consider trends in socioeconomic segregation in the U.S., Rusk (2002), in describing a period in which income segregation between schools increased, documents an increment of 2 percentage points in the dissimilarity index between 1989 and 1999. In that sense, statistically significant reductions derived from implementing the prohibition of non-random admission procedures seem to be relevant and similar in magnitude to increments described in other contexts as significant.

Considering these approaches together, the documented effects seem to be substantively significant. At the same time, these results indicate that school location, neighborhood segregation, and self-selection, among other determinants, are still important mechanisms explaining between-school segregation in the country.

These results suggest some policy implications. The first and most important is that regulations in school choice systems matter. This study shows that the documented stratifying consequences of school choice in different contexts may be partially mitigated with a better set of rules that breaks non-random and selective admission procedures. Examining the extent to which schools are allowed to discriminate against or in favor of determined students and proposing regulations to prohibit or limit those mechanisms may effectively reduce between-school segregation in choice contexts.

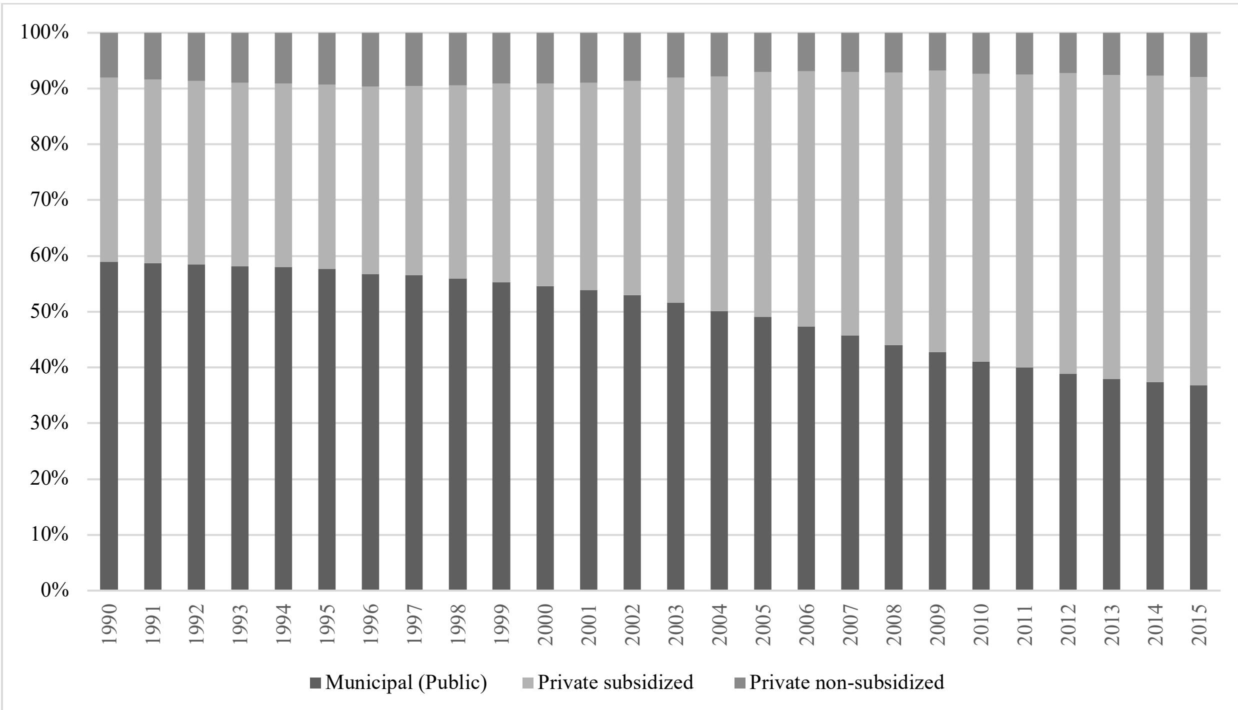
Should we expect these patterns to hold in other contexts? One of the particularities of the Chilean education system is that families do not have any priority to enroll in specific schools based on their place of residence. That is not the case in other education systems, including the United States. As Monarrez and Chien (2021) show, in all but one of the largest one hundred districts in the country, place of residence and school boundaries play a role in school assignments, including in districts in which school choice has been implemented. As such, prohibiting selective admission via lotteries or other randomized mechanisms may have a smaller or null effect in the U.S. because parents can self-select into attendance zones of oversubscribed schools and get priority in the school lottery. In fact, several authors have documented an association between gentrification and school choice, especially in districts in which lotteries are in place (e.g., Billings, Brunner, & Ross, 2018; Jordan & Gallagher, 2015; Makris, 2018; Pearman & Swain, 2017). This built-in residential priority could decrease the likelihood of detecting reductions in between-school segregation after prohibiting selective admissions. Replicating the study presented here in contexts in which attendance zones still have a role in school assignments may be an interesting potential extension of this work.

Two additional policy interventions may complement the prohibition of selective admissions to further mitigate the segregative effects of school choice. Researchers have documented persisting stratified family preferences in the Chilean context under the new centralized admission system (Eyzaguirre et al., 2019a). Families with higher income and more educational credentials tend to apply to more and more demanded schools than families with less education and income. As the authors document, this is only partially associated with spatial segregation that affects school options. In turn, they suggest that it may be driven by information asymmetries (Eyzaguirre et al., 2019b). Similar to what has been recommended in other school choice contexts, information interventions may increase the effectiveness of the policy (see, for example, Hasting & Weinstein, 2008).

On the other hand, exploring *controlled choice* alternatives may be another complementary route. Controlled choice systems work in the same way as non-controlled systems, allowing parents to choose schools of their preference. However, in controlled choice systems, the final distribution of students of different characteristics is also intentioned (Frankenberg, 2017). To achieve desegregation, education systems that implement centralized admissions have explored alternatives such as eliminating in-boundary or siblings priorities; introducing racial, ethnic, or socioeconomic priorities; or incentivizing diversity quotas (see, for example, Carlson, Bell, Lenard, Cowen, McEachin, 2020; Ehlers, Hafalir, Yenmez, & Yildirim, 2014). Future research may explore these nudges.

## 8. Tables & Figures

Figure 1. Student Enrollment by Type of School, 1990-2015




Source: Prepared by the author using data from Centro de Estudios MINEDUC

Table 1. Implementation

| | Year 1 [2016] | | Year 2 [2017] | | Year 3 [2018] | | Year 4 [2019] | |
|---|---|---|---|---|---|---|---|---|
| | N | % | N | % | N | % | N | % |
| Neither | 313 | 99.68 | 311 | 99.04 | 228 | 72.61 | 43 | 13.69 |
| Only free | 1 | 0.32 | 1 | 0.32 | 4 | 1.27 | 9 | 2.87 |
| Only admission | 0 | 0 | 1 | 0.32 | 81 | 25.8 | 259 | 82.48 |
| Both | 0 | 0 | 1 | 0.32 | 1 | 0.32 | 3 | 0.96 |

Note: Schools and municipalities are considered to be scheduled to be treated by the admission component if they are located in a region in which the centralized admission system started to operate. They become actively treated once the admission system reaches their region. Schools are considered to be scheduled to be treated by the free component if they receive public funding and were not free before the policy. They are considered to be actively treated once the school becomes free. Municipalities are considered to be scheduled to be treated by the free component if in 2015, before the policy change, the percentage of free schools within the municipality was equal or below to 70%. Municipalities are considered to be actively treated if they were scheduled to be treated and, after the policy, the percentage of free schools in that municipality is equal or above 85%.

Table 2. Descriptive Statistics

| | Before [2009-2015] | | After [2016-2019] | |
|---|---|---|---|---|
| | Mean | SD | Mean | SD |
| Regions | 15 | | 15 | |
| Municipalities | 315 | | 316 | |
| Total schools | 8,492 | 178.86 | 8,018 | 77.90 |
| Proportion municipal | 0.549 | 0.498 | 0.535 | 0.499 |
| Proportion subsidized | 0.402 | 0.490 | 0.407 | 0.491 |
| Proportion private | 0.049 | 0.216 | 0.058 | 0.234 |
| Proportion rural | 0.444 | 0.497 | 0.415 | 0.493 |
| Proportion free | 0.734 | 0.442 | 0.800 | 0.400 |
| Total students | 2,257,975 | 37,425.14 | 2,272,115 | 28,934.83 |
| Proportion municipal | 0.407 | 0.021 | 0.371 | 0.001 |
| Proportion subsidized | 0.518 | 0.017 | 0.543 | 0.001 |
| Proportion private | 0.077 | 0.004 | 0.087 | 0.001 |
| Proportion female | 0.491 | 0.001 | 0.490 | 0.001 |
| Proportion vulnerable | 0.434 | 0.079 | 0.485 | 0.004 |
| Dissimilarity index | 0.262 | 0.105 | 0.302 | 0.120 |

Notes: In any given year, municipalities with less than 5 schools, which represent 8% of the municipalities and account for less than 1% of the schools, are not considered in the sample.

Table 3. Municipality Average Effects

| | Post | | |
|---|---|---|---|
| | Introduction and Debate (DID) | Final Approval and Promulgation (DID) | Implementation (TWFE) |
| | (1) | (2) | (3) |
| Free | -0.0245** | -0.0261*** | -0.0229*** |
| | (0.00754) | (0.00620) | (0.00533) |
| Admission | -0.0186** | -0.0175** | -0.00837* |
| | (0.00647) | (0.00585) | (0.00329) |
| *Municipality/year observations* | *3448* | *3448* | *3448* |

Notes: Estimates in columns (1) and (2) are the result of a difference-in-difference model. In both cases, I regress the outcome of interest, the dissimilarity index, by an interaction term between a post-event indicator (1=post; 0 otherwise) and the indicator for a municipality scheduled to be treated by the free component, as well as the post-event indicator and the admission component (1=scheduled to be treated; 0 otherwise). Estimates in column (3) are the result of a TWFE regression of the outcome of interest on the active treatment indicator of each component (1=actively treated; 0 otherwise). All models include group (municipality) and time (year) fixed effects and control variables (number of schools in the municipality, average and standard deviation of test results in the municipality, proportion of private schools, proportion of free schools, proportion of females, proportion of rural schools, and proportion of students that live in the same municipality as their school). Robust standard errors, in parentheses, are clustered at the municipality level.
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table 4. Municipality Effects – Entry Level

| | Post Implementation |
|---|---|
| | (1) |
| Free | -0.0101 |
| | (0.00724) |
| Admission | -0.0123* |
| | (0.00487) |
| *Municipality/year observations* | *3398* |

Notes: Estimates are the result of a TWFE regression of the outcome of interest on the active treatment indicator of each policy component (1=actively treated; 0 otherwise). The model includes group (municipality) and time (year) fixed effects and control variables. Robust standard errors, in parentheses, are clustered at the municipality level.
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table 5. Municipality Effects - Heterogeneity by Intra Municipality Competition

| | Proportion private | | Proportion urban | | Proportion same municipality | |
|---|---|---|---|---|---|---|
| | Top 80 | Top 50 | Top 80 | Top 50 | Top 80 | Top 50 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: All levels | | | | | | |
| Free | -0.0252*** | -0.0263*** | -0.0245*** | -0.0259*** | -0.0262* | -0.0252* |
| | (0.00567) | (0.00580) | (0.00588) | (0.00562) | (0.0104) | (0.0102) |
| Admission | -0.00511 | -0.00579 | -0.00345 | -0.00613 | -0.00857* | -0.0127* |
| | (0.00349) | (0.00399) | (0.00342) | (0.00382) | (0.00399) | (0.00520) |
| *Municipality/year observations* | *2758* | *1717* | *2747* | *1724* | *2758* | *1724* |
| Panel B: Entry levels | | | | | | |
| Free | -0.0152+ | -0.0215** | -0.0156* | -0.0212** | -0.00969 | 0.00241 |
| | (0.00786) | (0.00782) | (0.00774) | (0.00799) | (0.0129) | (0.0185) |
| Admission | -0.00558 | -0.00728 | -0.00691 | -0.00469 | -0.0123* | -0.0196* |
| | (0.00438) | (0.00507) | (0.00438) | (0.00427) | (0.00614) | (0.00948) |
| *Municipality/year observations* | *2702* | *1619* | *2701* | *1618* | *2718* | *1699* |

Notes: Two-way fixed effects coefficients in all models are the result of a regression of the outcome of interest, the dissimilarity index, on the active treatment indicator of the free component and the admission component. All models include group (municipality) and time (year) fixed effects and control variables. Panel A reports estimates for all levels. Panel B includes only entry levels. Columns 1, 3, and 5 include only communes above the 20th percentile in the distribution of communes in terms of the proportion of private schools, the proportion of urban schools, and the proportion of same commune students, respectively. Columns 2, 4, and 6 include only communes above the 50th percentile in the same variables. Robust standard errors, in parentheses, are clustered at the commune level.
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table 6. Falsification Test

| | Grade Levels | | | |
| --- | --- | --- | --- | --- |
| | All | | Entry | |
| | Scheduled | Treated | Scheduled | Treated |
| | (1) | (2) | (3) | (4) |
| <u>Panel A: Free</u> | | | | |
| Placebo Post | 0.0163*** | -0.00430 | 0.0241*** | -0.0101 |
| | (0.00478) | (0.00615) | (0.00572) | (0.00955) |
| <u>Panel B: Admission</u> | | | | |
| Placebo Post | 0.00381 | -0.00738 | 0.00289 | -0.0110 |
| | (0.00641) | (0.00562) | (0.00682) | (0.00671) |
| *Municipality/year observations* | *1566* | *1566* | *1547* | *1547* |

Notes: Coefficients are the result of a difference-in-difference regression of the outcome of interest on an interaction variable of the treatment indicator, which is 1 in the case of municipalities scheduled to be treated or actively treated in the post-period, and a placebo post indicator that is 0 between the years 2009 and 2012 and turns to 1 in 2013, before any of the policy entry points policy. The outcome of interest is the dissimilarity index. The model includes municipality control variables, and group (municipality) and time (year) fixed effects. Columns (1) and (2) present results with all and entry grade levels, respectively. Robust standard errors, in parentheses, are clustered at the municipality level.
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Figure 2. Parallel Trends Before Period

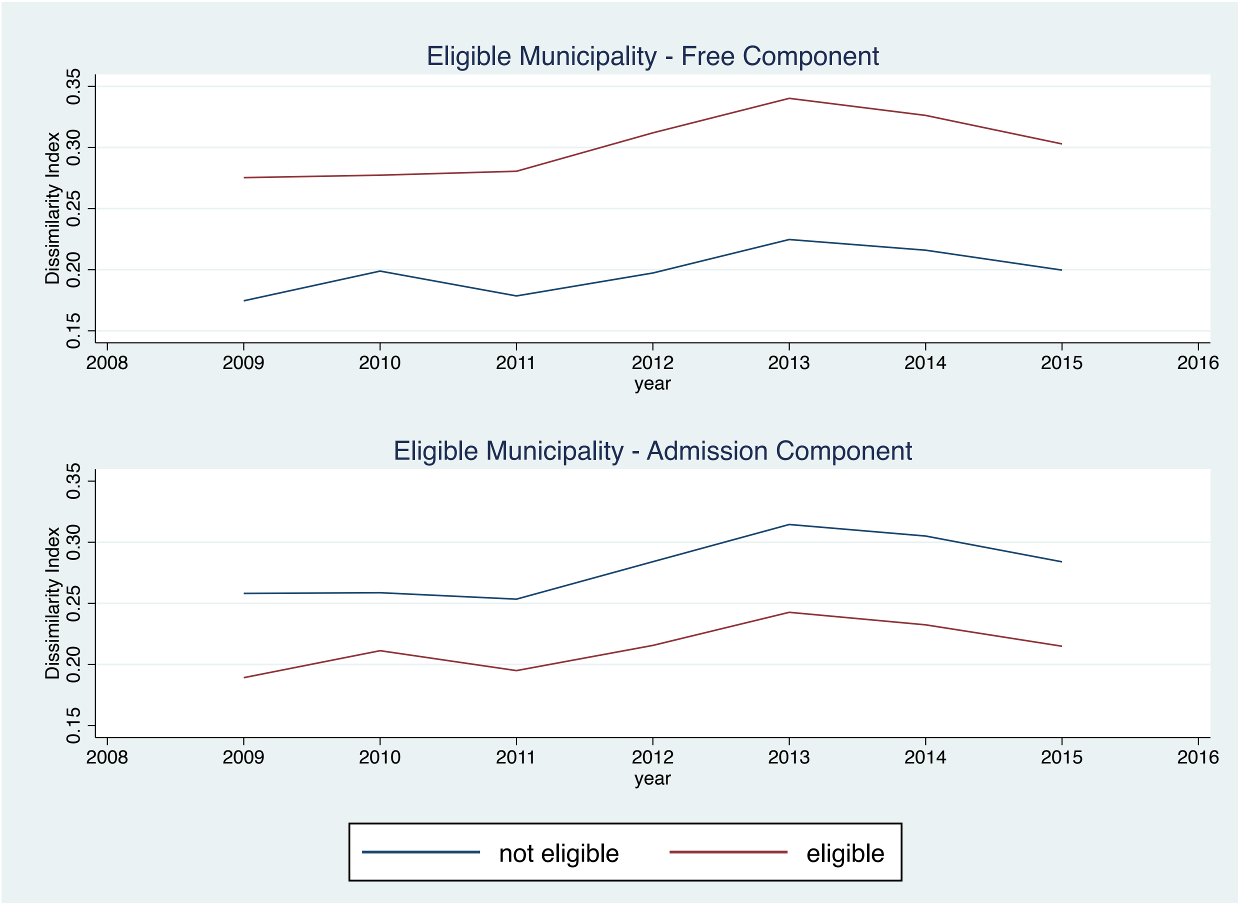


Table 7. Decomposition of Weighted Average Effects

| | Beta<br>(1) | Weight<br>(2) |
|---|---|---|
| <u>Panel A: All levels</u> | | |
| Free ($\beta$= -.023;  $p$ = 0.000) | | |
| Timing groups | -0.016 | 0.012 |
| Never versus timing | -0.018 | 0.962 |
| Within | -0.186 | 0.027 |
| Admission ($\beta$ = -.009; $p$ = 0.010) | | |
| Timing groups | -0.005 | 0.488 |
| Never versus timing | -0.110 | 0.488 |
| Within | -0.049 | 0.024 |
| <u>Panel B: Entry levels</u> | | |
| Free ($\beta$= -.013;  $p$ = 0.077) | | |
| Timing groups | 0.015 | 0.012 |
| Never versus timing | -0.005 | 0.960 |
| Within | -0.282 | 0.028 |
| Admission ($\beta$ = -.013; $p$ = 0.008) | | |
| Timing groups | -0.004 | 0.473 |
| Never versus timing | -0.024 | 0.497 |
| Within | -0.019 | 0.030 |

Notes: Decomposition estimated with bacondecomp Stata module (Goodman-Bacon, Goldring, & Nichols, 2019)

Figure 3. Decomposition of Weighted Average Effects

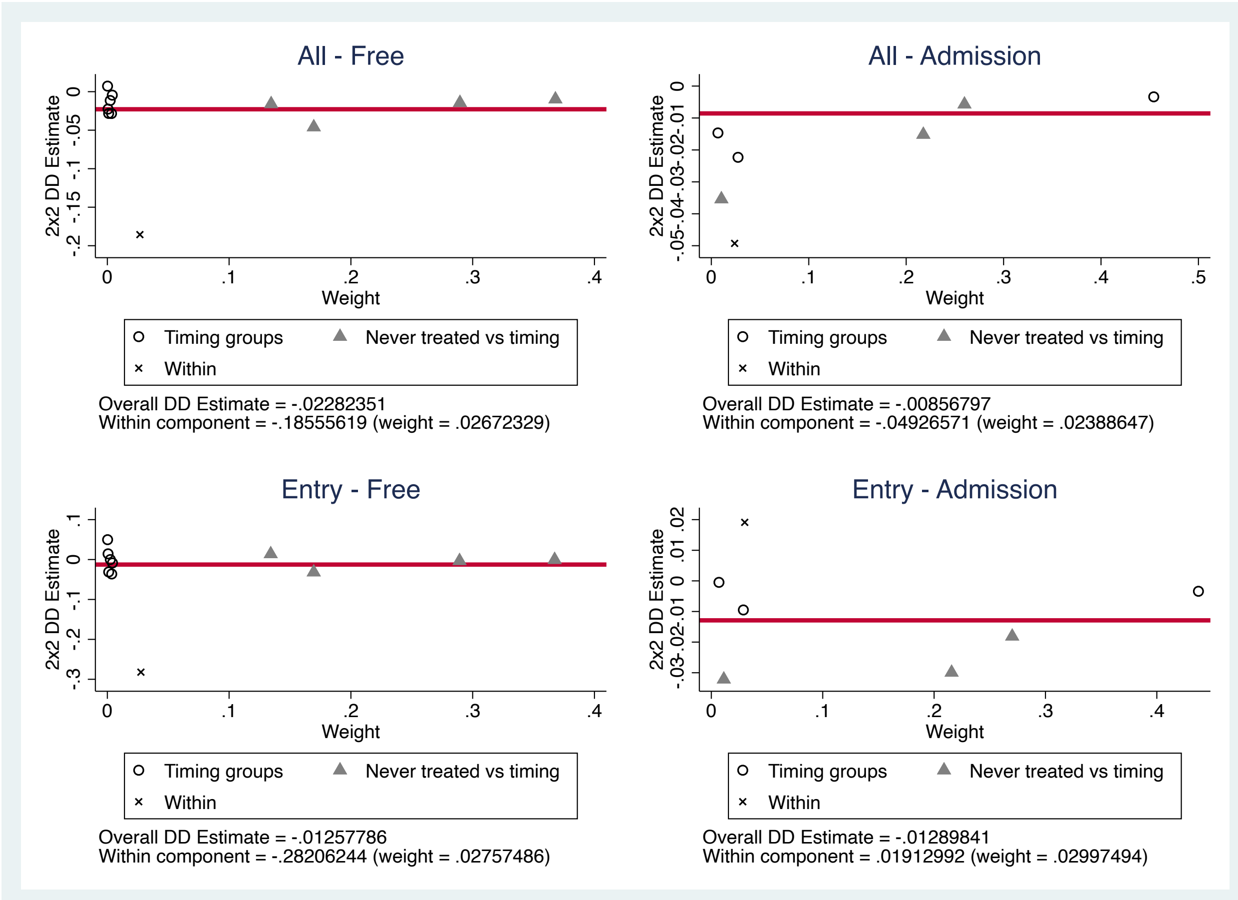


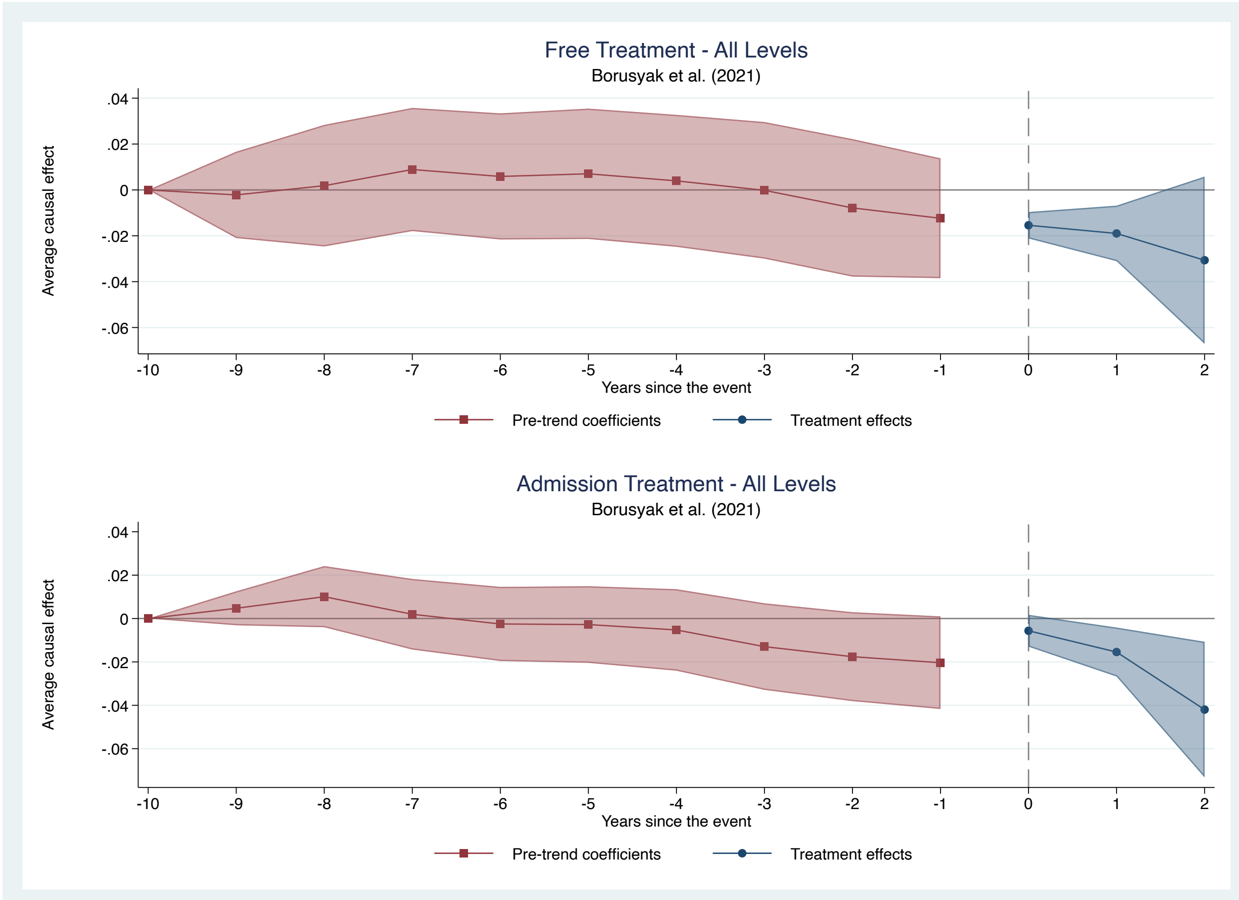
Table 8. Municipality Effects – Event Study

| | All Levels | | Entry Levels | |
|---|---|---|---|---|
| | Free | Admission | Free | Admission |
| | (1) | (2) | (3) | (4) |
| Year 2 since event | -0.0375* | -0.0496*** | -0.0180 | -0.0396* |
| | (0.0178) | (0.0141) | (0.0271) | (0.0181) |
| Year 1 since event | -0.0365* | -0.0333** | -0.0227 | -0.0393** |
| | (0.0144) | (0.0117) | (0.0206) | (0.0152) |
| Year 0 since event | -0.0349** | -0.0232* | -0.0182 | -0.0329** |
| | (0.0131) | (0.0103) | (0.0182) | (0.0124) |
| Year -1 since event | -0.0299* | -0.0218* | -0.0138 | -0.0306* |
| | (0.0127) | (0.00984) | (0.0177) | (0.0120) |
| Year -2 since event | -0.0230 | -0.0196* | -0.00582 | -0.0271* |
| | (0.0142) | (0.00957) | (0.0164) | (0.0114) |
| Year -3 since event | -0.0124 | -0.0148 | -0.00771 | -0.0240* |
| | (0.0137) | (0.00925) | (0.0193) | (0.0112) |
| Year -4 since event | -0.00594 | -0.00670 | -0.00961 | -0.00931 |
| | (0.0127) | (0.00847) | (0.0203) | (0.0107) |
| Year -5 since event | 0.00238 | -0.00375 | 0.0123 | 0.00403 |
| | (0.0121) | (0.00776) | (0.0192) | (0.0107) |
| Year -6 since event | -0.000319 | -0.00357 | -0.00313 | 0.00108 |
| | (0.0113) | (0.00720) | (0.0166) | (0.00955) |
| Year -7 since event | 0.00837 | 0.000689 | 0.00881 | 0.000371 |
| | (0.0100) | (0.00636) | (0.0164) | (0.00845) |
| Year -8 since event | 0.00106 | 0.00756 | 0.00912 | 0.00304 |
| | (0.00898) | (0.00479) | (0.0142) | (0.00646) |
| *Municipality/year observations* | *3448* | *3448* | *3398* | *3398* |

Notes: Coefficients in all models are the result of a regression of the outcome of interest, the dissimilarity index, on the active treatment indicator of the free component and the admission component n years before (leads) or after (lags) the implementation of the treatment. All models include group (municipality) and time (year) fixed effects and control variables. Considering that only one municipality was treated with the free treatment component 3 years after the event, year 2 of the free treatment combines municipalities 2 and 3 years post-implementation. Robust standard errors, in parentheses, are clustered at the municipality level.

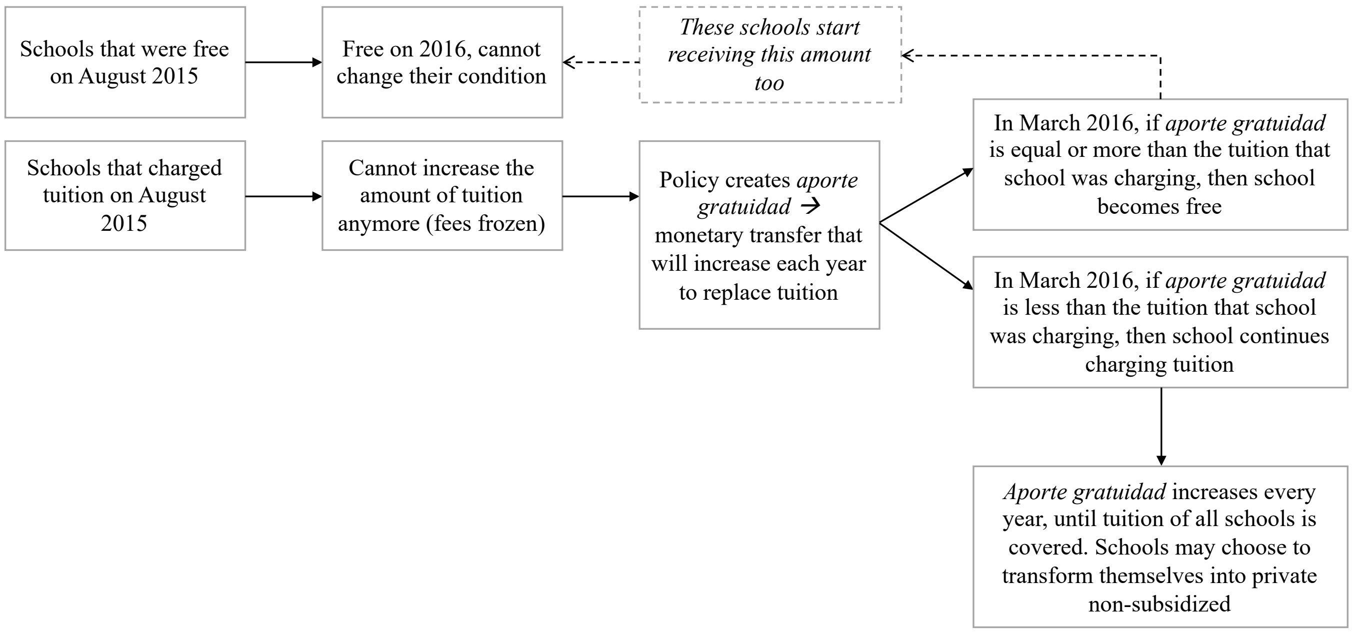
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Figure 4. Event Study using DID Imputation



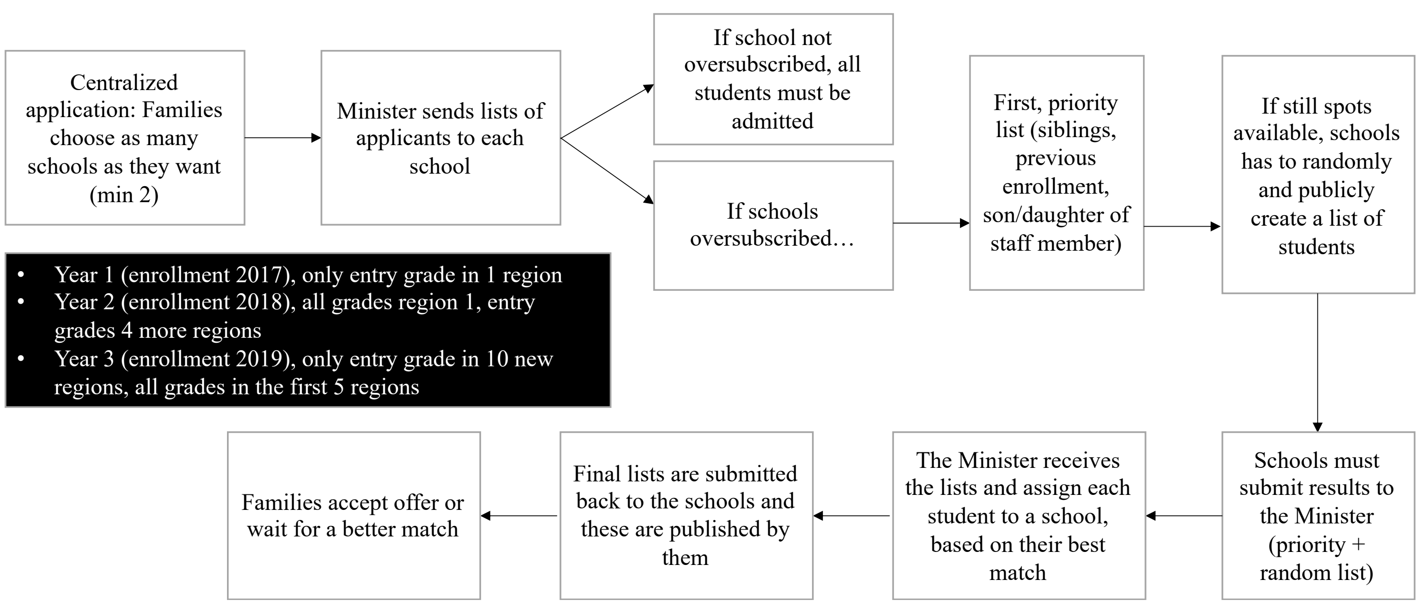
# 9. Appendices

Figure A1. Implementation Process of the Free Component of the Policy




Source: Prepared by the author

Figure A2. Admission to Schools Under the Centralized Admission System




Source: Prepared by the author

Table A1. TWFE Separate Treatment Estimations

| | Post | | | | | |
|---|---|---|---|---|---|---|
| | Introduction and Debate | | Final Approval and Promulgation | | Implementation | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Free | -0.0135* | | -0.0156** | | -0.0198** | |
| | (0.00643) | | (0.00512) | | (0.00601) | |
| Admission | | -0.0147* | | -0.0134* | | -0.00697* |
| | | (0.00594) | | (0.00541) | | (0.00326) |
| *Municipality/year observations* | *3448* | *3448* | *3448* | *3448* | *3448* | *3448* |

Notes: Estimates in columns (1) and (2) are the result of a difference-in-difference model. In both cases, I regress the outcome of interest, the dissimilarity index, by an interaction term between a post-event indicator (1=post; 0 otherwise) and the indicator for a municipality scheduled to be treated by the free component and/or the admission component (1=scheduled to be treated; 0 otherwise). Estimates in column (3) are the result of a TWFE regression of the outcome of interest on the active treatment indicator of either or both components (1=actively treated; 0 otherwise). All models include group (municipality) and time (year) fixed effects and control variables (number of schools in the municipality, average and standard deviation of test results in the municipality, proportion of private schools, proportion of free schools, proportion of females, proportion of rural schools, and proportion of students that live in the same municipality as their school). Robust standard errors, in parentheses, are clustered at the municipality level.

*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table A2. Robustness – Balanced Sample of Municipalities

| | Grade Levels | |
|---|---|---|
| | All | Entry |
| | (1) | (2) |
| Free | -0.0228*** | -0.0126+ |
| | (0.00534) | (0.00712) |
| Admission | -0.00857** | -0.0129** |
| | (0.00331) | (0.00486) |
| *Municipality/year observations* | *3432* | *3300* |

Notes: Two-way fixed effects coefficients in all models are the result of a regression of the outcome of interest, the dissimilarity index, on the active treatment indicator of the free component and the admission component. Both models include group (municipality) and time (year) fixed effects and control variables (number of schools in the municipality, average and standard deviation of test results in the municipality, proportion of private schools, proportion of free schools, proportion of females, proportion of rural schools, and proportion of students that live in the same municipality as their school). Columns (1) and (2) include all and entry grade levels, respectively. Sample is restricted to municipalities that have observations every year, between 2009 and 2019. Robust standard errors, in parentheses, are clustered at the municipality level.
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table A3. Robustness – Alternative Socioeconomic Indicator

| | All levels | | Entry levels | |
|---|---|---|---|---|
| | Original | Close to Policy | Original | Close to Policy |
| | (1) | (2) | (3) | (4) |
| Free | -0.0142+ | -0.0125* | -0.00957 | -0.00967 |
| | (0.00847) | (0.00491) | (0.0116) | (0.0111) |
| Admission | -0.00878+ | -0.0113*** | -0.0137* | -0.0105* |
| | (0.00447) | (0.00338) | (0.00565) | (0.00447) |
| *Municipality/year observations* | *3448* | *3448* | *3398* | *3398* |

Notes: Two-way fixed effects coefficients in all models are the result of a regression of the outcome of interest, the dissimilarity index, on the active treatment indicator of the free component and the admission component. Both models include group (municipality) and time (year) fixed effects and control variables (number of schools in the municipality, average and standard deviation of test results in the municipality, proportion of private schools, proportion of free schools, proportion of females, proportion of rural schools, and proportion of students that live in the same municipality as their school). Columns (1) and (2) include all grade levels; columns (3) and (4) only entry levels. Robust standard errors, in parentheses, are clustered at the municipality level. Columns (1) and (3) use the original indicator of student vulnerability; columns (2) and (4) fix that indicator to the student/year observation to the closest year before the implementation of the policy (i.e., 2015).

*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table A4. Robustness – Alternative Segregation Indices

| | All levels | | Entry levels | |
|---|---|---|---|---|
| | H index | Gini index | H index | Gini index |
| | (1) | (2) | (3) | (4) |
| Free | -0.0115** | -0.00754 | -0.00801 | -0.00120 |
| | (0.00402) | (0.0104) | (0.00549) | (0.0151) |
| Admission | -0.00994** | -0.00832+ | -0.0130*** | -0.0168** |
| | (0.00300) | (0.00494) | (0.00356) | (0.00647) |
| *Municipality/year observations* | *3448* | *3448* | *3398* | *3398* |

Notes: Two-way fixed effects coefficients in all models are the result of a regression of the outcome of interest, the dissimilarity index, on the active treatment indicator of the free component and the admission component. Both models include group (municipality) and time (year) fixed effects and control variables (number of schools in the municipality, average and standard deviation of test results in the municipality, proportion of private schools, proportion of free schools, proportion of females, proportion of rural schools, and proportion of students that live in the same municipality as their school). Columns (1) and (2) include all grade levels; columns (3) and (4) only entry levels. Robust standard errors, in parentheses, are clustered at the municipality level. Columns (1) and (3) use the original identificator of student vulnerability; columns (2) and (4) fix that indicator to the student/year observation to the closest year before the implementation of the policy (i.e., 2015)

*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

Table A5. Robustness – Alternative Specifications of Free Treatment

| | Below 25th percentile - Above mean | Below 25th percentile - Above 25th percentile | Below 70% - Above 70% | Below 70% - Above 80% |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| <u>Panel A: All Levels</u> | | | | |
| Free | -0.0174*** | -0.0147** | -0.0147** | -0.0159*** |
| | (0.00356) | (0.00509) | (0.00509) | (0.00444) |
| Admission | -0.00799* | -0.00805* | -0.00805* | -0.00822* |
| | (0.00334) | (0.00330) | (0.00330) | (0.00333) |
| *Municipality/year observations* | *3448* | *3448* | *3448* | *3448* |
| <u>Panel B: Entry Levels</u> | | | | |
| Free | -0.0133 | 0.000542 | 0.000542 | 0.00157 |
| | (0.00807) | (0.00601) | (0.00601) | (0.00619) |
| Admission | -0.0125* | -0.0116* | -0.0116* | -0.0115* |
| | (0.00494) | (0.00488) | (0.00488) | (0.00490) |
| *Municipality/year observations* | *3398* | *3398* | *3398* | *3398* |

Notes: Two-way fixed effects coefficients in all models are the result of a regression of the outcome of interest, the dissimilarity index, on the active treatment indicator of the free component and the admission component. All models include group (municipality) and time (year) fixed effects and control variables (number of schools in the municipality, average and standard deviation of test results in the municipality, proportion of private schools, proportion of free schools, proportion of females, proportion of rural schools, and proportion of students that live in the same municipality as their school). Robust standard errors, in parentheses, are clustered at the municipality level. Each column uses an alternative way to identify municipalities scheduled to be treated and considered to be actively treated by the free component after the bill was passed. Panels A and B include all and entry levels, respectively.
*+p<0.1, *p<0.05, **p<0.01, ***p<0.001*

# Conclusion

The non-random sorting of students into schools that results in between-school segregation by race, ethnicity, or socioeconomic status has been a source of inequality in multiple education systems worldwide, including the U.S. and Chile. Several factors determine how students sort into schools, stratifying their access to educational opportunities that ultimately impact their achievement and well-being. In this dissertation, I explored two specific dimensions of this complex and multifaceted phenomenon, one in association with the literature on the consequences of school-based segregation; one with the literature on its causes.

Literature on the consequences of school-based segregation has discussed the negative effects of racial or socioeconomic segregation. In turn, it has predicted educational and social benefits of school diversity, especially for minoritized students. Within this literature, economists have attempted to identify the unique contribution of peers in explaining these benefits. However, the economic literature on peer effects has traditionally approached this problem by relying on marginal year-to-year differences in peer composition. This approach, while methodologically sound, ignores the potential effects that may be observed when the groups under comparison present starker differences.

In my first study, I documented benefits in test scores and suspensions for all students, especially Black and white, of being enrolled in schools with a larger proportion of students of color. However, going back to the discussion in the previous paragraph, I specifically explored how our understanding of race-ethnicity peer effects may be enriched by directly analyzing schools with starker differences in race-ethnicity peer composition. I find that, while such an approach confirms some of the patterns that we observe by exploiting marginal year-to-year variation, it also paints a more complete picture: depending on the students' own race-ethnicity, the association between peer composition and academic outcomes in some cases is linear, and in some cases is non-linear. In other words, some students experience continuous benefits of being exposed to a more diverse school context, and some may lose those benefits at certain points of the distribution. At the same time, not only do I empirically confirm a correlation between peer racial composition and the distribution of resources across schools, but I also show how the estimated outcomes are sensitive to this correlation. Taken together, while these results broadly support policies aimed at diversifying schools by race-ethnicity, they also highlight the need to include in those policy efforts discussions on directly related topics, such as local funding, staff diversity and representation, and institutional dynamics that result in academic and disciplinary discrimination against specific groups of students, among others.

On the other hand, literature on the causes of between-school segregation has identified multiple determinants of segregation over the years, including changing demographics, residential segregation, family preferences, opportunity hoarding, and selective school admissions. However, due to the concurrent nature of these factors in the education system, their specific contribution to producing or aggravating school-based segregation has been elusive to prove. Moreover, some policymakers have questioned to what extent some of these determinants play a significant role in segregating schools. School choice advocates, for example, have argued that the increase in school segregation in school choice systems results from residential segregation and family preferences rather than selective school admissions.

In my second study, I show that prohibiting selective admissions and enforcing this prohibition reduces between-school socioeconomic segregation, mitigating the segregative effects of school choice. Thus, I demonstrate that, while selective admissions are not the sole factor in producing school segregation, they play a significant role in creating and maintaining between-school segregation in school choice contexts. Moreover, I show that one of the specific features of school choice systems, that is, the promotion of competition between schools to enroll students, is one of the drivers of selective school admissions that results in segregation. In education systems such as the U.S., where school choice seems to be expanding rather than contracting, limiting selective admissions and finding alternatives to school competition are policy strategies that need to be discussed to mitigate school-based segregation.

In sum, between-school segregation attempts against the equalizer potential of education and a more integrated society. In most education settings, diversifying schools by race, ethnicity, and socioeconomic status is a policy imperative. This dissertation contributed with evidence on some of the nuances that this policy discussion requires to effectively distribute educational opportunities and promote social cohesion.

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