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The impact of increasing school resources on peer victimization: Evidence from targeted funding on low-income families in Chile

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While a large body of literature has examined the impact of school spending on academic outcomes, far less is known about its effect on students' socioemotional development and school experiences. This study contributes to narrowing this gap by evaluating the impact of a nationwide school finance reform in Chile on peer victimization in high schools. The reform significantly increased school revenues by providing targeted funding for low-income students, with some school budgets growing by up to 60%. Using a panel of approximately 600 high schools from 2006 to 2018, I followed two main empirical strategies to causally evaluate the impact of this policy. First, Difference-in-Differences models designed to identify intertemporal effects and to evaluate continuous treatments show that the reform's impact grew over time, with significant reductions in peer victimization emerging after four years of exposure and concentrating in the 2016–2018 period when funding increases were largest. Second, to isolate the causal effect of funding, I used a two-stage least squares model, instrumenting for per-pupil revenues with plausibly exogenous variation from the reform's funding formula: the interaction between a school's pre-reform share of low-income students and the years since the policy's onset. The results indicate that a 10% increase in per-student revenue leads to a reduction in overall peer victimization of 7% to 10% from baseline levels. These reductions are more consistent for physical, verbal, and social aggression and less robust for online victimization. The effects on peer victimization are stronger in coeducational schools and those with a higher proportion of male students. Overall, these findings demonstrate that targeted educational investments matter not only to improve academic achievement but also the socioemotional well-being of students.

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The impact of increasing school resources on peer victimization: Evidence from targeted funding on low-income families in Chile*

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Abstract

While a large body of literature has examined the impact of school spending on academic outcomes, far less is known about its effect on students' socioemotional development and school experiences. This study contributes to narrowing this gap by evaluating the impact of a nationwide school finance reform in Chile on peer victimization in high schools. The reform significantly increased school revenues by providing targeted funding for low-income students, with some school budgets growing by up to 60%. Using a panel of approximately 600 high schools from 2006 to 2018, I followed two main empirical strategies to causally evaluate the impact of this policy. First, Difference-in-Differences models designed to identify intertemporal effects and to evaluate continuous treatments show that the reform's impact grew over time, with significant reductions in peer victimization emerging after four years of exposure and concentrating in the 2016–2018 period when funding increases were largest. Second, to isolate the causal effect of funding, I used a two-stage least squares model, instrumenting for per-pupil revenues with plausibly exogenous variation from the reform's funding formula: the interaction between a school's pre-reform share of low-income students and the years since the policy's onset. The results indicate that a 10% increase in per-student revenue leads to a reduction in overall peer victimization of 7% to 10% from baseline levels. These reductions are more consistent for physical, verbal, and social aggression and less robust for online victimization. The effects on peer victimization are stronger in coeducational schools and those with a higher proportion of male students. Overall, these findings demonstrate that targeted educational investments matter not only to improve academic achievement but also the socioemotional well-being of students.

Keywords: School finance reform; School accountability; School autonomy; School competition; Instrumental Variables

JEL Classification: I22, J24, J28, C36, J1

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1. Introduction

After the Coleman et al. (1966) report concluded that family background was the primary determinant of student outcomes, researchers and policymakers have intensely debated the costs, merits, and effectiveness of increasing school spending. This debate has centered on whether additional resources improve educational attainment and academic achievement – as key determinants of future labor market success and economic competitiveness – while giving less attention to how increasing funding may affect youth's socioemotional development and mental health.

Two recent meta-analyses synthesize the causal evidence from the United States on the impact of public school spending on educational outcomes. They report that a sustained 10% annual increase in per-pupil spending over four years leads to approximately 20 percentage points reduction in high school dropout rates, 3 percentage points increase in high school graduation rates, 8 percentage points increase in the probability of attending college, and test score gains of approximately 0.05 standard deviations (Handel and Hanushek, 2024; Jackson and Mackevicius, 2023).

While these positive effects on educational outcomes are encouraging, far less is known about the impact of school spending on student relationships, classroom climate, and broader developmental outcomes. To my knowledge, no previous study has causally examined how increases in school funding affect peer interactions and social dynamics in the classroom – factors critical to socioemotional and cognitive skill development, mental health, long-term well-being and labor market outcomes, above and beyond academic training and performance (Jackson, Porter, Easton, Blanchard, and Kiguel, 2020; Sarzosa and Urzúa, 2021).

To address this gap, this study evaluates the impact of a major funding reform on peer victimization in Chilean high schools – a key predictor of well-being, mental health and educational outcomes across the lifespan (Arseneault, 2017; Evans-Lacko et al., 2017; Takizawa, Maughan, and Arseneault, 2014). Specifically, I examined how an annual 10% increase in school revenues per student affected four types of peer victimization (physical, verbal, social, and online) among tenth-grade students. Furthermore, I analyzed whether these effects varied by the intensity of victimization and schools' characteristics, including proportion of boys, average student age, vocational versus academic orientation, private versus public management, coeducational versus single-sex enrollment.

Leveraging plausibly exogenous variation induced by Chilean legislation that increased monetary transfers from the central government to schools based on the number of low-income students enrolled, this study provides causal evidence on the effects of a nationwide school finance reform. As a landmark educational policy in the Latin American and Caribbean context, the Chilean reform significantly increased government expenditure, translating into school budget increases of up to 60% in less than a decade (Bertoni et al., 2018; Jaimovich, Elacqua, Silva, Schwartz, and Román, 2022). Consistent with evidence from the US, a growing body of causal studies has documented positive impacts of this reform on learning and educational outcomes at the elementary level, yet little is known about its impacts on non-educational outcomes at the high school level (e.g., Carrasco, Pérez, and Núñez, 2015; Hofflinger and von Hippel, 2020; Martínez, 2023; Murnane, Waldman, Willett, Bos, and Vegas, 2017; Romaguera and Gallegos, 2010).

Using Chilean administrative data, I built a panel comprising approximately 600 high schools offering grades 9-12, observed between 2006 and 2018. I linked school revenue data –

from central and local governments, as well as family contributions – to data from the national testing system, which includes information on educational quality and contextual factors affecting learning. Using these data, and under the assumption that the timing of the funding reform was exogenous, I implemented a difference-in-differences (DiD) model to estimate cumulative effects of the reform based on varying exposure durations to increased school funding. Results from this approach demonstrate that impacts grew over time, consistent with a progressive increase in funding after the policy was adopted. Additionally, this method shows that there were no significant pre-treatment differences in revenues, school violence, or academic outcomes across schools with differing funding intensities.

Next, I implemented a second DiD model specifically designed for continuous treatment settings. Employing the framework developed by Callaway, Goodman-Bacon, and Sant'Anna (2024a), I used a nonparametric estimator to recover flexible dose-response functions that characterize how peer victimization outcomes vary with pretreatment social vulnerability, as a proxy of (expected) funding intensity. This analysis reveals a progressive reduction in peer victimization among schools in the lower half of socioeconomic vulnerability, with the impact stabilizing at higher levels of vulnerability. These findings suggest that a similar effect could have been achieved by increasing funding to the level provided to the school at the median of the social vulnerability distribution, highlighting the complexity of choosing adequate levels for funding.

These DiD methods capture the overall impact of the reform, including the effect of institutional changes accompanying the increase in revenues such as the introduction of accountability measures targeted at low-performing schools. To isolate the causal effect of increased revenues, I captured plausibly exogenous changes in school revenues attributable to the reform by using a two-stage least squares (2SLS) model where per-pupil revenues is the endogenous treatment variable. Following a strategy similar to those in Jackson, Johnson, and Persico (2016) and Goldhaber and Falken (2024), I instrumented post-reform revenues using the interaction between a school's pre-reform share of low-income students and the number of years since the policy onset. This design leverages student cohort and school-level variation to predict likely exogenous funding intensity, which in turn is used to estimate the effect of increased revenues on peer victimization. Accounting for potential confounders – such as accountability and baseline funding levels in elementary education – this model estimates the impact of a 10% increase in school revenues on peer victimization.

The results indicate that a 10% increase in school revenues per student leads to a reduction in peer victimization between 7% to 10% relative to baseline levels, a magnitude about half of that reported in previous meta-analyses and studies reporting the impact of randomized control trial evaluations of anti-bullying programs (Fraguas et al., 2021; Gaffney, Farrington, and Ttofi, 2019; Gaffney, Ttofi, and Farrington, 2021). These improvements are observed across all types of victimization, though the estimates are more robust for verbal and social forms of aggression than for physical and online incidents. The reductions in aggression are detected for both moderate and more severe victimization experiences and are concentrated in the period 2016–2018, when funding increased two to three times compared to 2013–2015. Moreover, heterogeneity exploration shows stronger effects in schools with a higher proportion of boys and in coeducational settings.

This study contributes to the growing literature focused on the effects of school finance reforms by demonstrating that increased public funding enhances not only academic outcomes but also students' social experiences and school climate. By focusing on peer victimization – a crucial predictor of socioemotional development and mental health – this work extends prior research that has relied on plausibly exogenous variation in school funding to examine academic outcomes alone. Additionally, its focus on a developing country and on predominantly vocationally oriented high schools without elementary grades provides novel insights into how targeted educational investments can promote a broader range of youth outcomes in diverse school settings.

2. Relevance and theoretical mechanisms linking resources to peer victimization

School peer victimization has long-lasting costs for individuals and societies. Decades-long studies have shown adverse impacts on individuals' mental health, well-being, and human capital accumulation as well as overall economic competitiveness (Ammermueller, 2012; Arseneault, 2017; Brimblecombe et al., 2018; Brown and Taylor, 2008; Evans-Lacko et al., 2017; Gorman, Harmon, Mendolia, Staneva, and Walker, 2021; Takizawa et al., 2014). Moreover, these negative consequences are likely to extend to children uninvolved in aggressive relationships with peers via classroom disruptions, which force changes in teachers' pedagogical practices and management interventions.(Carrell, Hoekstra, and Kuka, 2018; Epple and Romano, 2011). Consequently, multiple cost-benefit analyses show large financial benefits from interventions aiming to improve peer relationships at schools (Agee and Crocker, 2016; Huitsing, Barends, and Lokkerbol, 2020; Persson and Svensson, 2013).

One promising yet relatively unexplored mechanism to improve student relationships is increasing public funding. Indeed, Chilean policymakers encouraged schools to allocate part of the funding increase evaluated in this study to promoting safe environments that would enable better peer relationships and learning (see section 3.1). There are several potential mechanisms through which increased resources may lead to improved social environments. First, resources could be invested in evidence-based antibullying and socioemotional learning programs designed to increase peers' empathy, develop students' interpersonal skills, and improve school climate (Arias Ortiz, Hincapie, and Paredes, 2020; Durlak, Weissberg, Dymnicki, Taylor, and Schellinger, 2011; Gaffney et al., 2021; Valenzuela et al., 2022). Second, the finance reform allowed for increased hiring of counselors, psychologists, and social workers. This staff is likely to expand prevention, supervision, and targeted supports for at-risk students as well as effectively improve students' school environments, academic outcomes, and mental health (Bhatt et al., 2023; Carrell and Hoekstra, 2014; López, Cárdenas, and González, 2021; Martínez et al., 2024; Mulhern, 2023; Reback, 2010). Third, additional funding could be used to hire more teachers, reducing the student-teacher ratio. This would likely improve working conditions, planning time, classroom management, teacher retention – all factors previously linked to improvements in social and educational outcomes (Nguyen, Anglum, and Crouch, 2023; Ramberg, Brolin Låftman, Åkerstedt, and Modin, 2020; Simon and Moore-Johnson, 2015). Lastly, investments in principals' capacity, professional development, improving facilities, and strengthening community ties are other potential mechanisms supported by previous research (Hanushek et al., 2024; Huang, Espelage, Polanin, and Hong, 2019; Lafortune and Schönholzer, 2022; Park, Goodman, Hurwitz, and Smith, 2020; Younan et al., 2018). These mechanisms are not mutually exclusive and may operate simultaneously, reinforcing one another.

3. Institutional background

The foundational structure of the Chilean school system was established in the early 1980s based on centralized funding, decentralized management, and school choice. This model introduced a flat per-student voucher system through which the central government transferred funding to schools based on student enrollment and attendance, regardless of students' socioeconomic background. Public schools, managed by municipal governments, and private subsidized schools, competed for enrollments under this common voucher structure. Local governments were allowed to top up funding, which led to disparities in educational spending as municipalities with a greater fiscal capacity contributed more. Beginning in 1994, schools were also permitted to charge additional tuition fees, which further widened the resource gap between schools serving affluent versus low-income families (Santos and Elacqua, 2016).

For more than two decades, this system remained largely unchanged despite growing concerns over its regressive structure and its inability to reduce persistent socioeconomic inequalities in educational opportunities (Hsieh and Urquiola, 2006). By the mid-2000s, widespread public dissatisfaction – especially among secondary school students – culminated in a national protest movement known as the "Penguin Revolution," which called attention to the unequal conditions of the Chilean education system and demanded a more equitable resource distribution (Bellei and Vanni, 2015).

In response, the Chilean government introduced the Preferential School Subsidy Law (*Ley de Subvención Escolar Preferencial*, SEP) in 2008. This reform aimed to address the structural inequities of the existing flat voucher model by introducing a targeted per-student subsidy for socially vulnerable students (referred to as "priority students" in the SEP law), defined primarily by household income and other indicators of social disadvantage. The SEP program initially provided an additional 50% of the base voucher for each "priority student" enrolled in eligible schools. The value of this subsidy was increased by 20% in 2011 (law 20,501), and again by another 20% in 2016 (law 20,845).[1]

The SEP transfer was composed of two main components. First, a per-pupil grant that allocated additional funding for each priority student. Second, a concentration grant that scaled with the proportion of low-income students enrolled in the school – schools with greater concentrations of disadvantaged students received a higher supplement. For example, schools with 15-30% of their students classified as priority received an approximate 5% concentration bonus, while those with 60% or more received up to 13%.

Initially, the SEP program only covered grades pre-kindergarten through fourth grade, with one additional grade added each year until reaching eighth grade in 2012. However, the scope was later expanded through a series of legislative reforms. In 2011, Law 20,501 introduced an extension of the SEP coverage to secondary education (grades 9 through 12), to be phased in beginning in 2014. To prevent a funding gap for students entering ninth grade in 2013, a subsequent law passed in 2012 (law 20,637) ensured uninterrupted SEP benefits for students progressing from eighth to ninth grade. By 2016, SEP had expanded to cover all compulsory grade levels from pre-kindergarten to twelfth grade.

In addition to increasing public funding, the SEP law introduced a series of institutional reforms aimed at promoting an efficient use of resources. These reforms included mandates for schools to develop and implement school improvement plans, new accountability mechanisms

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[1] Starting in 2016, the Chilean government introduced two additional policies that further increased public funding for schools participating in the SEP program (law 20,845). First, the *Aporte por Gratuidad* (free tuition contribution) provided additional public funding to all schools not charging family fees. This incentive aimed to lower financial barriers for low-income families and grew from approximately 12.5% of the base voucher in 2016 to nearly 20% by 2018. Second, the government introduced a supplementary subsidy for preferential students – students below the 80th percentile of the national social vulnerability index who did not meet the stricter criteria for priority student status. The subsidy for preferential students was half the amount allocated for priority students under the SEP program.

tied to student performance, differentiated levels of autonomy based on school performance, and incentives that heightened inter-school competition – particularly for the enrollment of low-income students. Together, these policy changes sought to ensure that the additional resources granted through SEP translated into meaningful improvements in educational quality and equity. The following subsection describes each of these reforms and the mechanisms through which they could influence school practices, student relationships, and learning environments.

3.1. SEP potential mechanism of improvement and the empirical evidence

While this article focuses on estimating the effects of increased funding through SEP on peer victimization in Chilean high schools, it is important to note that the law introduced several complementary reforms that may also have shaped school environments. Most existing empirical studies focus on test score improvements and elementary grades, leaving open questions about how these mechanisms operate in secondary education and whether they influence student relationships. This section describes the main components of the SEP reform – including increased funding, accountability, competition, and school autonomy – and draws on the literature to identify potential channels through which each might affect student outcomes.

More resources and planning

The primary channel through which SEP could change educational and social outcomes is the substantial increase in funding – a central aim of this article. Depending on the share of priority enrolled, SEP transfers could increase a school's budget by up to 60% relative to the base voucher. Based on rules defined by the laws involving SEP, **Table 1** shows how school budgets would increase according to the proportion of priority students. Schools with just 10% of priority students would see an estimated 5.4% budget increase, while schools serving an entirely low-income population could see increases exceeding 60% – with 54% corresponding to the per-student subsidy and the other 8% to the concentration grant.

Table 1: Simulation of SEP budget increase, by school socioeconomic vulnerability

Proportion of low-income students	School budget increase 2012-2018	Increase due to	
		Priority student	Concentration
0%	0.000%	-	-
10%	5.397%	5.397%	0.000%
25%	14.269%	13.492%	0.777%
50%	30.550%	26.985%	3.565%
75%	46.512%	40.477%	6.035%
90%	55.814%	48.572%	7.242%
100%	62.016%	53.969%	8.047%

Note: Simulations are based on subsidy values defined by laws 20,550 (enacted 2011) 20,637 (2012), and 20,845 (2016) for a high school offering an academic track and half day schedules.

In parallel with this financial boost, SEP required schools to sign an agreement with the Ministry of Education in which they committed to designing and implementing a four-year school improvement plan. These plans had to include a diagnostic review of teaching and management practices and outline actions to improve school performance in four key areas: (1) curriculum organization, (2) school leadership, (3) social relationships, and (4) resource management.

This model of school improvement planning – common in the United States education systems – relies on schools' ability to assess their own needs, define priorities, identify specific and effective actions, and execute targeted reforms efficiently (Fernandez, 2011). Conceptually, these plans are expected to develop relevant information and accountability systems, foster a culture of continuous improvement, strengthen collective learning among staff, and align school practices with strategic-attainable goals (Morrison, 2018).

However, the evidence on their effectiveness remains mixed (e.g., Caputo and Rastelli, 2014; Carnoy, Gove, Loeb, Marshall, and Socias, 2008; Fernandez, 2011; Huber and Conway, 2015; Lockheed, Harris, and Jayasundera, 2010; VanGronigen and Meyers, 2022). Some research highlights their potential to improve collaboration and professional development (e.g., Escobar, 2019; Van der Voort and Wood, 2014), while others caution that rigid templates and bureaucratic demands can lead to symbolic compliance rather than substantive change (e.g., Levine and Leibert, 1987; Meyers and VanGronigen, 2021; Mintrop and MacLellan, 2002; Mintrop, MacLellan, and Quintero, 2001).

Although the improvement plans were not explicitly designed to reduce peer victimization, their emphasis on improving school relationships could plausibly influence peer environments. Moreover, the increase in resources also enabled schools to hire additional teaching and ancillary staff, such as school psychologists and social workers, who are often critical actors in shaping school climate, addressing interpersonal conflict, and support students' learning (e.g., Mulhern, 2023). As shown in **Table 2**, average revenue per student grew by nearly 50% between 2012 and 2018, and the average number of teachers and ancillary staff per school also rose substantially. These staffing increases may represent a key mechanism through which additional resources contributed to improved student experience.

Other mechanisms

Accompanying the additional funding, SEP introduced institutional changes aimed at incentivizing the efficient use of resources, including accountability measures, autonomy provisions, and competitive pressures. I explicitly control for these additional mechanisms when isolating the effects of additional funding on peer victimization, though not when evaluating the overall effects of SEP. Below, I briefly describe these components and outline how each may conceptually influence peer relationships, with a more detailed discussion in **Section A.1** in the Appendix.

First, SEP established a novel test-based accountability system, which classified schools into three performance categories based primarily on students' standardized test scores in mathematics, language, and sciences. Schools were categorized as autonomous (high-performing), emerging (average-performing), and in-recovery (low-performing). Low-performing schools faced significant consequences, including potential funding loss if they failed to improve within four years. Although this accountability framework was legislated in 2008, high schools were formally integrated into this system in 2017, following updates introduced by Law 20,529 passed in 2011 (*Sistema de Aseguramiento de la Calidad*, SAC) and a new system to rank schools approved in 2014. While accountability systems primarily aim to enhance academic performance, they may inadvertently increase student stress and anxiety, negatively impacting peer relationships, or alternatively, foster better discipline and stronger student support structures, thereby potentially reducing peer victimization (Heissel, Adam, Doleac, Figlio, and Meer, 2021; Holbein and Ladd, 2017).

Second, SEP provided greater autonomy to high-performing schools, granting them more discretion in using public funds and exempting them from submitting annual improvement plans and evaluations. This autonomy allowed schools greater flexibility to address specific local needs, potentially enhancing their ability to manage resources effectively (Jackson, 2023; Valenzuela, Villarroel, and Villalobos, 2013). The SAC law later reinforced this autonomy, designating high-performing schools as technical support providers for lower-performing institutions starting in 2016–2017.

Lastly, SEP aimed to stimulate inter-school competition through two mechanisms. It required participating schools to waive tuition fees for low-income students, thereby increasing potential enrollment from economically vulnerable families. Additionally, it increased subsidies for enrolling priority students, enhancing schools' financial incentives to attract low-income students. Although competitive pressures might drive schools to enhance their quality to retain and attract students, research from Chile also suggests that intensified competition could inadvertently harm socioemotional outcomes and peer dynamics by increasing stress, academic pressure, and negative interpersonal behaviors among students (Gajardo and Grau, 2019).

4. Data and measures

This study draws on multiple administrative and survey data sources to construct a panel of Chilean high schools observed between 2006 and 2018. Key information includes annual school-level revenues and enrollment records, as well as student reports on peer relationships and academic performance from the national SIMCE assessment. SIMCE data cover the full population of tenth-grade students, with assessments conducted biennially from 2006 to 2012 and annually from 2012 to 2018.

4.1. Outcome variables: Exposure to peer victimization

The primary outcomes capture student-reported exposure to peer victimization, measured through SIMCE's student questionnaires. Since 2012, the survey has included four questions asking how often, over the past school year, classmates mistreated the respondent in the following ways: (1) Physically (i.e., hitting you or damaging belongings); (2) Verbally (i.e., insulting, mocking, or threatening you); (3) Socially (i.e., isolating, speaking ill, or humiliating you in front of others); (4) Electronically (i.e., online threats, or harassment). Students responded using a five-point scale (1=Never, 2=A few times per year, 3=Several times per month, 4=Several times per week, 5=Every day).[2]

Following common practice in the literature (e.g., Solberg and Olweus, 2003), I dichotomized each experience, assigning a value of one for experiences occurring at least several times per month and zero otherwise. I also created a binary variable equal to one if at least one of these four dichotomized variables were equal to one and zero otherwise. To assess differences by intensity, I created alternative binary variables, assigning a value of one for experiences occurring every day and zero otherwise.

4.2. Endogenous variable: School revenues

School revenue data were collected in Chilean pesos by the Ministry of Education and the National Municipal Information System (or *Sistema Nacional de Información Municipal*, SINIM). The Ministry of Education collects and reports monetary transfers from the central government and family fees, while SINIM provides information on school revenue from municipal (local government) sources.

To construct a consistent school-level measure, I summed revenues from all three sources and divided by enrollment at the start of the academic year to obtain per-student revenues. These values are then corrected for inflation and converted into 2017 US dollars using Purchasing Power Parity, based on the Consumer Price Index from the International Monetary Fund and comparative price data from the World Bank's International Comparison Program. Finally, to reduce the influence of outliers and measurement error, per-student revenue values below the 1st

[2] While these measures capture distinct forms of peer victimization, each is based on a single survey item. As such, internal consistency measures (e.g., Cronbach's alpha) cannot be computed, which limits the ability to assess reliability across items.

percentile and above the 99^{th} percentile are bottom-coded and top-coded at those percentile values, respectively.

4.3. Treatment dose and instrumental variable: Priority students before SEP

To measure schools' pretreatment social vulnerability, I constructed a proxy for the share of "priority students" using data from the SIMCE caregiver questionnaires administered to tenth-grade students. Following the SEP law's eligibility criteria, I defined a student as priority if any of the following applied: (a) the student's caregivers did not complete high school, (b) their household income was below percentile 20 of the income distribution, or (c) the student was enrolled in the free-of-fees bracket of the National Health Insurance Fund (known as FONASA A). I pooled observations from the 2006 and 2008 assessments, when these variables are available, and computed the school-level average across available years to obtain a stable pretreatment measure.

4.4. Additional variables

To account for potential confounders and test the robustness of the main results, I constructed additional variables capturing relevant student and school characteristics.[3]

Caregivers' schooling years correspond to the mother's number of years of formal education, as reported by the primary caregiver in the SIMCE contextual questionnaire.

Eighth grade school revenues were calculated using the same method as the main revenue variable. To link this information to tenth-grade students, I used student-level data containing the school ID for both eighth- and tenth-grade schools. I then merged these records

[3] Other variables were created to provide more context of the Chilean setting or to conduct supporting analyses to check the robustness of the main findings. These variables are described in **Section A.2** in the Appendix.

with revenue data at the school level, adding the eighth-grade revenue next to the tenth-grade revenue information.

School accountability is measured based on the criteria defined in the 2008 SEP law. According to its second transitory article, a school is classified as *in-recovery* if in at least two out of the three most recent SIMCE assessments: (1) their SIMCE score was below 220, and (2) fewer than 20% of students score above 250 points. Based on this definition, I created a binary variable equal to one for schools meeting both conditions in at least two of the following tenth-grade assessments: 2006, 2008, and 2010.

School autonomy is also measured based on the criteria established in the 2008 SEP law. According to its first transitory article, a school qualifies as autonomous if, in at least two out of the three most recent SIMCE assessments, it meets the following conditions relative to other schools in the same socioeconomic group (out of five possible groups): (1) its average score exceeds the group median; (2) the proportion of students scoring above 250 points exceeds the group median; and (3) the proportion scoring above 300 points also exceeds the group median. While the Ministry of Education publicly identified autonomous elementary schools, it did not do so for high schools. To address this, I applied the same official criteria using SIMCE assessments from 2006, 2008, and 2010 for tenth-grade students to construct a binary autonomy indicator.

School competition from fee-charging schools captures the degree of local competition from fee-charging schools. It is defined as the fraction of total student enrollment at these schools in 2007 within a 10-kilometer radius around each school in the sample. Each school's competitive market is thus specific to its location. The 10-kilometer threshold aims to reflect the typical set of schooling options available to families, based on evidence that the average distance between a tenth grader's home and school in Chile is approximately 5 kilometers (Chumacero, Gómez, and Paredes, 2011).

School competition for enrolling priority students measures the degree of competition among schools to enroll low-income students. Specifically, I calculated the proportion of priority students across each school's local market. As with the previous variable, school markets were defined using a 10-kilometer radius around each school to reflect the typical geographic range of school choice. This measure captures the local density of vulnerable students, which may influence school incentives under the SEP program.

Table 2 summarizes the evolution of the main variables used for the main analysis. The final sample includes over 7,000 school-year observations from more than 600 schools, each serving an average of approximately 600 students per year. These high schools only serve grades 9 through 12, which compared to PK-12, K-12, and 1-12 high schools are more likely to be publicly managed (58% vs. 16%), have a vocational orientation (75% vs. 23%), less educated parents (10 vs. 12 schooling years) and lower math test scores (-0.51 standard deviations).

Between 2012 and 2018, exposure to any form of peer victimization affected roughly 36% to 39% of tenth-grade students at these high schools, with verbal and social victimization being the most common types. During the same period, average school revenues per student increased by nearly 50%, alongside steady growth in teaching and ancillary staff. From the descriptive statistics alone, the relationship between school revenues and peer victimization remains unclear. The following sections present the empirical strategies used to estimate the causal impact of the SEP program, and the effect of increasing revenues, on students' social outcomes.

Table 2: Summary statistics, averages by school-year

	High schools, 10th grade				
	2010	2012	2014	2016	2018
	(1)	(2)	(3)	(4)	(5)
Number of schools	663	653	633	618	601
Number of students, averaged	672.014	603.100	578.913	565.173	583.742
Outcomes					
Physical, PV	.	0.114	0.130	0.101	0.108
Verbal, PV	.	0.284	0.302	0.285	0.296
Social, PV	.	0.220	0.231	0.226	0.222
Online, PV	.	0.115	0.113	0.100	0.101
Any of the four types, PV	.	0.358	0.392	0.375	0.393
Schooling inputs					
Revenue per student	$ 2,488	$ 2,466	$ 2,899	$ 3,480	$ 3,710
N teachers per school	31.63	31.03	32.49	34.20	35.77
N ancillary staff per school	18.03	19.19	21.30	24.17	27.59
Socioeconomic context					
Low-income students before SEP (%)	0.62	0.62	0.62	0.62	0.62
Poverty municipality	0.19	0.18	0.17	0.16	0.15
Potential confounders					
Non-SEP confounders					
Mothers' schooling	9.80	10.00	10.22	10.30	10.66
Elementary school revenues	$ 1,751	$ 2,333	$ 2,595	$ 3,152	$ 3,543
SEP pro-efficiency policies					
Accountability pressure	0.17	0.16	0.16	0.16	0.16
Autonomy to use funds	0.41	0.42	0.43	0.44	0.44
Competition from schools charging fees	0.41	0.41	0.41	0.41	0.41
Competition for priority students	0.54	0.54	0.54	0.55	0.55

PV: Peer victimization

5. Impact of the SEP reform

I began the analysis by evaluating the overall impact of the SEP policy, considering the combined influence of all its components – namely increased school revenues, improvement planning, accountability pressures, autonomy provisions, and heightened inter-school competition. To accomplish this, I applied three DiD methodologies that accommodate varying intensities of policy exposure or "treatment doses."

I started by using a simple DiD model to estimate how peer victimization changes after SEP between schools serving higher income families (i.e., control group) and four groups of schools varying on their expected SEP benefit (i.e., four treated groups). I separately estimated these differences aiming to illustrate in simple terms how the identification strategy leverages school's socioeconomic status and the timing of SEP to determine exposure intensity.

Next, I implemented the DiD estimator of intertemporal effects proposed by de Chaisemartin and D'Haultfoeuille (2024), designed to estimate impacts of multivalued treatments at different points in time since treatment onset. This approach yields event-study coefficients capturing cumulative policy effects over the duration of exposure. Finally, for the third approach, I employed the DiD framework developed by Callaway et al. (2024a), which explicitly models continuous treatment variation to estimate dose-response relationships in scenarios where all units are treated simultaneously but with varying intensities.

All three methodologies rely on standard DiD assumptions: (1) the absence of anticipation effects prior to treatment, and (2) parallel trends in outcomes between control and treated groups in the absence of treatment. Meeting these assumptions supports causal interpretations across all methods; however, the latter two estimators are preferred since they also address limitations that have been recently associated with traditional two-way fixed effects (TWFE) models, which involve potential biases and interpretability challenges when there is substantial heterogeneity in treatment effects (e.g., de Chaisemartin and D'Haultfoeuille, 2023).

5.1. Separate DiD models

I first assessed SEP's effects on peer victimization using a common DiD approach among applied researchers, the dynamic TWFE. This model includes time fixed effects (ϕ_t), school fixed effects (α_i), and a set of interaction terms between each year in the sample with an indicator variable for treatment status ($\sum_{\substack{r\neq 0 \\ -\underline{T}\leq r\leq \overline{T}}} 1[R_{it} = r]$):

$$Y_{i,t} = \alpha_i + \phi_t + \sum_{\substack{r\neq 0 \\ -\underline{T}\leq r\leq \overline{T}}} 1[R_{it} = r] \cdot \beta_r + \epsilon_i \tag{1}$$

The dynamic TWFE estimates treatment effects for each post-treatment year, yielding causal estimands when there is heterogeneity only in time. However, if there is heterogeneity across treated schools, the coefficients β_r from specification (1) may become difficult to interpret due to potential "forbidden comparisons," which involve negative weight on the treatment effect r periods after treatment for some schools or positive weight on treatment effects at lags of r (Roth, Sant'Anna, Bilinski, and Poe, 2022).

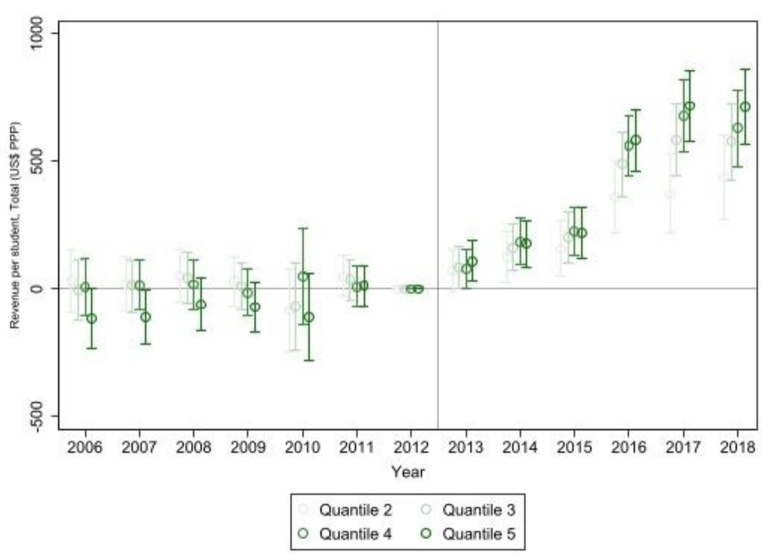
To reduce the likelihood of these "forbidden comparisons," I separately implemented this model for four more homogeneous groups of treated schools. Specifically, I categorized schools into five groups based on their anticipated benefits from SEP prior to its implementation by using the quintiles of the percentage of low-income students before SEP – employing this measure as a proxy for expected revenue gains. Schools in the lowest quintile (least vulnerable) serve as the control group, while schools in the remaining four quintiles correspond to groups of increasingly higher treatment intensity. Using pretreatment values to define these groups helps to avoid biases that could arise if schools changed their socioeconomic composition in response to SEP.

While data limitations preclude a direct inspection of pretreatment trends for control and treated groups of schools in peer victimization outcomes, I visually examined these pretreatment differences in per-student revenues. Implementing the dynamic TWFE model separately for revenues and peer victimization reveals a similar trajectory post SEP, with larger increases in revenues and larger decreases in peer victimization observed between 2016-2018, across treated groups. Furthermore, there are no significant differences in revenue per student during the pretreatment years (see **Figure 1**).

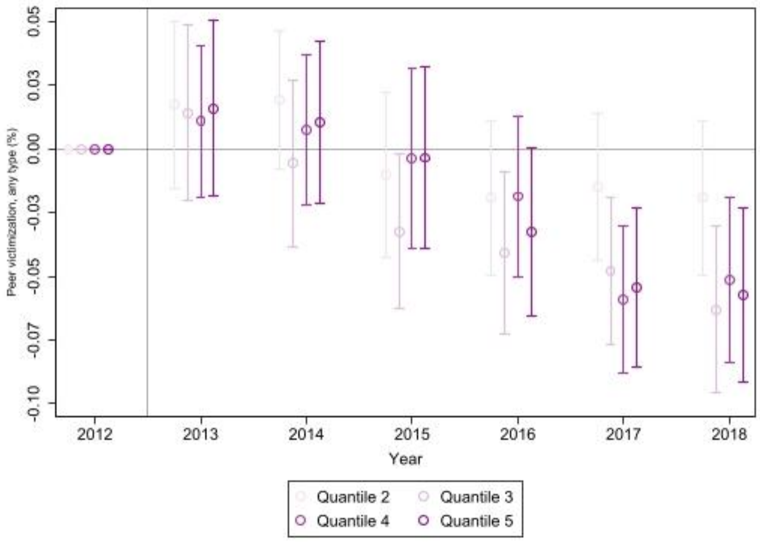
This initial exercise suggests that SEP reduced peer victimization by an average of around 2 percentage points [pp] between 2013 and 2018. This reduction is stronger among schools in quantiles 3 through 5 of social vulnerability and after 2016, when the revenues were more than 500 dollars per student higher relative to baseline (or around a 20% increase). While separating schools into four groups of increasing treatment intensity reduces the concern that treatment effects across schools might yield non causal estimates, this approach does not fully rule out within-quantile variations. To further address this challenge, I employed two additional DiD models that were designed to enhance interpretability of treatment effects and avoid the "forbidden comparisons" in the TWFE model.

Figure 1: The impact of SEP on revenues and peer victimization, separate DiD models

Panel A: The impact of SEP on school revenues



Panel B: The impact of SEP on any type of peer victimization



5.2. DiD model of intertemporal treatment effects

The DiD estimator proposed by de Chaisemartin and D'Haultfoeuille (2024) estimates treatment effects by comparing outcome changes over time between groups of schools with the same initial treatment intensity but different trajectories afterward. At baseline (before the policy), treated schools (or “switchers”) share an identical treatment status with control schools (or “non-switcher”). After the implementation of the policy, treated schools experience varying increases in treatment intensity, whereas control schools remain at their baseline intensity level.

This method estimates treatment effects for each year following the policy introduction as the average difference between the actual outcomes observed among treated schools and the outcomes that would have occurred had their treatment remained unchanged since the baseline year. The counterfactual outcomes are approximated using schools that maintained their initial treatment intensity up to each period the treatment effects are estimated. This approach results in estimates including the effect of lagged treatment up to each year. Formally, the DiD estimator comparing schools with different treatment intensities over time is defined as follows:

$$DID_{g.l} = \left[Y_{g,F_g-1+l} - Y_{g,F_g-1}\right] - \frac{1}{N^g_{F_g-1+l}} \cdot \sum_{g':D_{g',1'}=D_{g,1},F_{g'}>F_g-1+l} \left(Y_{g',F_g-1+l} - Y_{g',F_g-1}\right) \quad (2)$$

Where $DID_{g.l}$ represents the DiD estimate for group g after l periods since treatment initiation. The term Y_{g,F_g-1+l} denotes the average outcome for schools in group g at period $F_g - 1 + l$, with $F_g - 1$ indicating the last period before the initiation of treatment (2012 in the SEP setting). Thus, the first component of (2) into brackets captures the observed change in outcomes for treated schools from the pretreatment period (or baseline) to the period l (2013 through 2018 in this study). The second component of the formula provides the counterfactual comparison by calculating the average outcome change among the control group of schools (g') that shared the same initial treatment intensity as the treated group g, but remained at that baseline intensity at the follow up period $F_{g'} > F_g - 1 + l$. The number of schools in this comparison group is denoted by $N^g_{F_g-1+l}$.

Under the assumptions of no anticipation (i.e., there are no effects in periods before F_g) and parallel trends (i.e., in the absence of SEP, outcome trajectories for switchers and non-switchers would have followed the same path), the DiD estimate isolates the causal effect by subtracting the outcome trajectory of these comparable, non-switching schools from that of the schools whose treatment intensity increased.

To obtain summary estimates, the authors proposed three estimators: (1) non-normalized effects by year defined as the average effect of having been treated rather than untreated for l periods, across all groups reaching l treatment periods at or before the last period where there is still at least one untreated group; (2) normalized effects by year corresponding to the weighted average of the effects of groups' current and $l - 1$ first treatment lags on their outcome; and (3) an overall average effect of having been treated for $t - F_g + 1$ periods (from 2013 to 2018 in this study) across all treated (g, t) cells.

As before, I used schools in the lowest quintile as the control group and schools in the other four quintiles as groups of increasingly higher treatment intensity. The main results using school revenues and exposure to peer victimization as outcomes are presented in **Figure 2** (Panel A presents the effects on revenues and Panel B on any type of victimization) and **Figure A.1** and **Table A.1** in the Appendix (revenues and all forms of peer victimization). Regarding revenues, these results demonstrate that SEP significantly increased school resources, with these effects growing over time from approximately 72 dollars per student after one year to over 610 dollars per student after six years of exposure, which corresponds to approximately to a 25% increase.

For peer victimization, significant reductions emerge consistently after four years of exposure. By the sixth year of SEP, reductions in peer victimization are evident across all types: physical (-1.9 pp), verbal (-3.9 pp), social (-4.3 pp), online (-1.8 pp), and any type of victimization (-4.9 pp). These cumulative effects correspond to a reduction between 14% to 19% of the baseline values. While initial effects after one or two years are mostly negligible, consistent reductions become clear starting at year four, aligning with the substantial increases observed in school funding. Supporting a causal interpretation of these results and the parallel trends assumption, the results from placebo tests indicate no pre-treatment differences for school revenues.

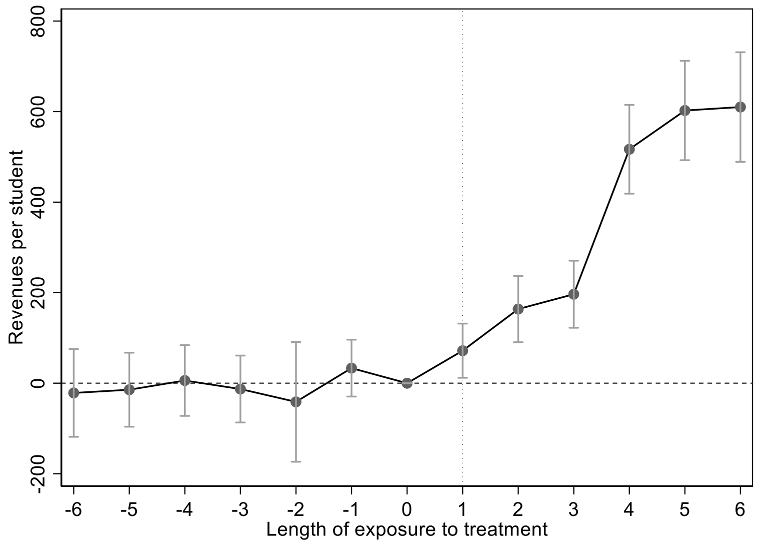
The lack of pretreatment data for the peer victimization outcomes precludes testing the no anticipation and parallel trends assumptions for these variables. To provide further support for the causal interpretation of results, I conducted placebo tests using related outcomes that were available prior to SEP. Specifically, I examined three school violence variables reflecting behaviors observed by students but not directly experienced – teasing, threats, and physical (collected since 2010) – and three variables related to academic achievement – math test scores (available since 2006), college attendance one year after expected high school graduation, and college attendance after two years of expected graduation (available since 2007).

For the school violence variables, results must be interpreted cautiously due to changes in response options in the questionnaire over time. Nevertheless, I find no pretreatment differences for teasing and threats at school, and negative SEP impacts emerged by year six. In contrast, the placebo test suggests pre-treatment differences for physical violence; however, the estimates also show that the SEP program had not a significant impact on this variable after six years (see **Figure A.2** and **Table A.2** in the Appendix).

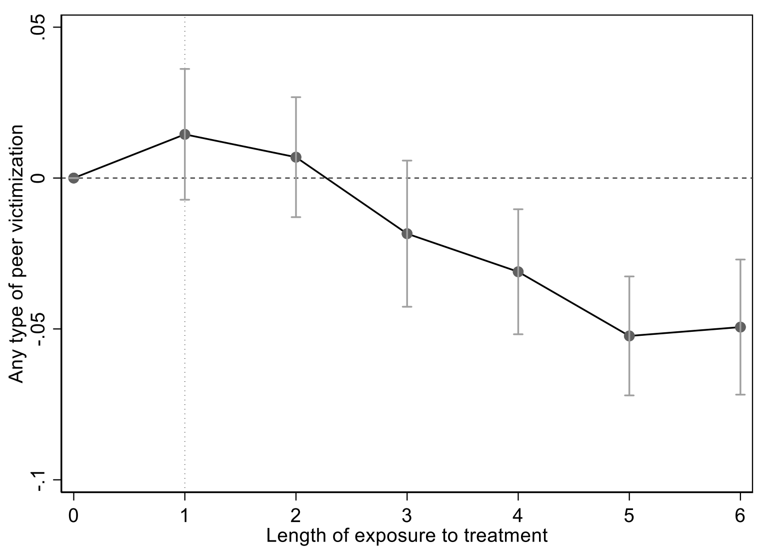
Regarding academic outcomes, the placebo tests suggest no pretreatment differences for any of the three variables, and the results show a positive effect for math test scores by year 6, and an increasingly positive effect for college attendance from year two onwards (see **Figure A.2** and **Table A.2** in the supplement). Although academic impacts are not the primary focus of this study, it is noteworthy that these findings align closely with recent causal evidence from the United States indicating that a 10% increase in school spending per student leads to test score improvements of between 0.05 and 0.07 standard deviations and increases college attendance by approximately 3.9 percentage points (Handel and Hanushek, 2024; Jackson and Mackevicius, 2023).

Figure 2: The impact of SEP on revenues and peer victimization, intertemporal effects

Panel A: The impact of SEP on school revenues



Panel B: The impact of SEP on any type of peer victimization



Lastly, I assessed the robustness of SEP impacts on peer victimization outcomes by including two potential confounders: average per-student revenues in the elementary schools attended by tenth graders, and mothers' years of schooling. The first covariate is a proxy for changes over time in public investments in elementary schools, which also received SEP funding since 2008. The second covariate accounts for long-term socioeconomic trends linked to poverty reduction between 1990 and 2018. While SEP estimates remained statistically significant after controlling for these covariates, their magnitude slightly decreased across outcomes. For example, by year six the SEP effect on school revenues per student decreased from 610 to 588 dollars, and the reduction in exposure to any type of peer victimization went from -0.049 to -0.041 (see **Table A.3** in the Appendix).[4]

5.3. DiD with a continuous treatment

An alternative DiD model allowing for a fully continuous treatment was recently proposed by Callaway et al. (2024a). In their framework, outcome changes between two periods are predicted by a flexible vector transforming continuous treatment doses into smooth curves, which are used to capture the impact of every possible dose on the outcome change. Formally, these authors define a general specification of the form:

$$\Delta Y_i = \sum_{k=1}^{K} \psi_k(D)\beta_k + \varepsilon_i \tag{3}$$

Where $\psi_k(d)$ is a K-dimensional vector of flexible transformations of the dose D, β_k a vector of finite dimensional parameters, and ε_i an idiosyncratic error term. In practice, this specification is commonly implemented using cubic B-spline basis functions, which allow for

[4] In addition to the cumulative (non-normalized) effects discussed above, I also estimated normalized effects, which reflect the policy impacts in each individual year while controlling for the effects of prior years (**Table A.4** in the Appendix), and the average total effect, summarizing policy impacts across the entire post-treatment period (Panel B of **Table A.1**, **Table A.3**, **Table A.4**, and **Table A.5** in the Appendix). These additional analyses also revealed statistically significant reductions on average in peer victimization emerging consistently after year four, though, and as expected, the magnitude of these effects was smaller than the cumulative (non-normalized) estimates.

smooth and flexible estimation of potentially nonlinear dose-response relationships (Callaway, Goodman-Bacon, and Sant'Anna, 2024b). To implement this method in my analysis, I specified cubic B-splines with knots at the quartiles of the observed treatment distribution. This choice captures sufficient nonlinearity while avoiding excessive complexity or overfitting. Using this flexible approach, I estimated smooth dose-response curves that show the estimated effect of each possible level of treatment intensity on changes in peer victimization outcomes.

Consistent with the previous analysis, I evaluated SEP impacts on schools in the top four quantiles of pretreatment socioeconomic vulnerability, using schools in the lowest quantile as the control group. The pretreatment period was defined as 2012, with three distinct post-treatment periods analyzed: the full period (2013–2018), an initial period with lower revenue increases (2013–2015), and a subsequent period characterized by higher revenue increases (2016–2018). The treatment dose was measured by the percentage of low-income students prior to SEP implementation. Under assumptions of no anticipation effects and parallel trends, this approach provides causal treatment effect estimates for the targeted schools.

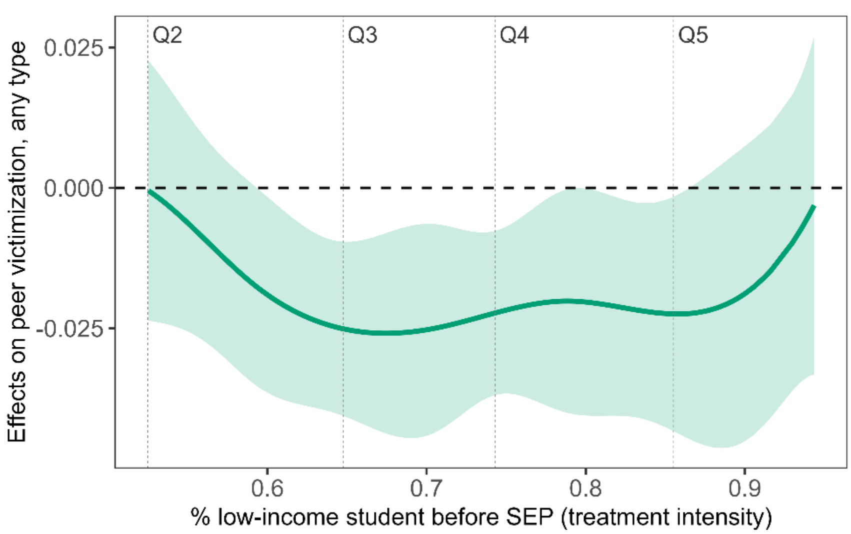
Focusing specifically on variations in treatment intensity, I examined changes over time for exposure to any type of peer victimization. The results indicate an overall SEP impact of approximately -1.8 percentage points (95% confidence interval [CI] = -0.026; -0.010 pp), becoming statistically significant for schools with approximately 60% pretreatment low-income students, corresponding roughly to the 50th percentile of vulnerability. These findings suggest that SEP effectively reduced peer victimization among the most vulnerable half of high schools (see Panel A in **Figure 3**).

Mirroring findings from the de Chaisemartin and D'Haultfoeuille (2024) model, this approach indicated that SEP had no discernible effect during the initial period (2013-2015;

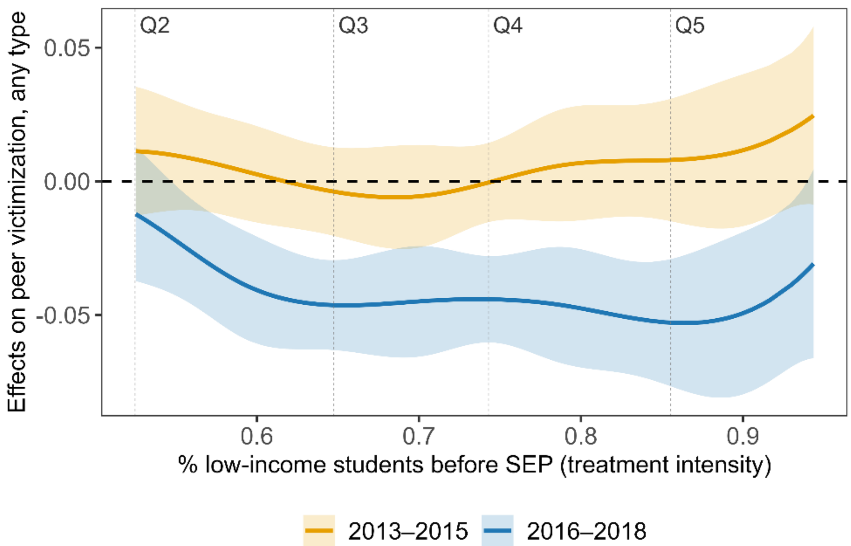
average effect = 0.004; CI = -0.005; 0.012), while significant reductions occurred during 2016–2018 (average effect = -0.039; CI = -0.048; -0.030). Interestingly, the SEP effects during 2016-2018 decreased between 50% to 60% of pretreatment low-income students and maintained stable at approximately -5 percentage points afterwards (see Panel B in Figure 3). These results suggest that while the revenue increases between 2013 and 2015 were insufficient to achieve meaningful impacts, the larger funding increments between 2016 and 2018 could have achieved similar results even if allocated at slightly lower levels.

Figure 3: The impact of SEP on any type of peer victimization, continuous treatment

Panel A: The impact of SEP on any type of peer victimization, years 2013-2018



Panel B: The impact of SEP on any type of peer victimization, periods 2013-2015 and 2016-2018



The following section addresses the specific impact of revenue increases attributable to SEP, leveraging variations in school vulnerability, the program's rollout, and subsequent subsidy increments. Additionally, the analysis controls for potential confounders linked to other SEP components and distinct characteristics of the Chilean school system.

6. Impact of increased revenues

In this section, I aim to provide causal estimates of the impact of increasing school revenues on peer victimization. The correlation between revenues per student and peer victimization can be confounded by factors such as family fees paid by higher-income families or demographic shifts arising from school recruiting responses to SEP. To address these potential biases, I isolated exogenous variation in resources induced by the changes in the funding formula introduced by SEP, which increased funding based on schools' enrollment of low-income students. Furthermore, SEP's phased rollout included progressively one grade per year, resulting in more modest revenue increases between 2013-2015 as grades 9 through 11 joined the program. In 2016, with the inclusion of the entire 9-12 cycle and a 20% increase in the SEP subsidy value, there was a marked rise in revenues, which subsequently stabilized.

The change in funding rules introduced by SEP created a natural experiment in which some schools experienced little budget changes, while others saw substantial increases. Leveraging this natural experiment allows causal estimation of additional resource effects by isolating revenue variations due exclusively to SEP. The analysis thus compares schools exposed to lower funding increases with those exposed to significantly higher SEP-induced resources.

I captured the variation in revenues attributable to SEP through a variable I called *dosage*. This variable combines variation from schools' socioeconomic composition, the program rollout schedule, and the changes in the SEP subsidy. This dosage served as a key predictor of revenues per student in the first stage of a Two-Stage Least-Square (2SLS) model. To mitigate potential endogeneity bias arising if schools changed their social composition to attract more SEP resources, I used pretreatment measures of school socioeconomic vulnerability.

Following previous applied research that utilized a similar model (e.g., Biasi, 2023; Goldhaber and Falken, 2024; Jackson et al., 2016), I employed a 2SLS regression model where per pupil revenues is the endogenous treatment of interest. Specifically, I estimated the following system of equations:

$$\bar{R}_{st} = \beta_1 \cdot (L_{st} \cdot SES_{s06-08}) + \beta_2 \cdot Cov_{st} + \alpha_s + \alpha_t + \mathcal{E}_{st} \quad (4)$$

$$Y_{st} = \delta \cdot \left(\widehat{\bar{R}_{st}}\right) + \Phi \cdot Cov_{st} + \theta_s + \theta_t + \xi_{cs} \quad (5)$$

Where Y_{st} represents peer victimization outcomes for school s and year t. The endogenous treatment variable, $\bar{R}_{st}$, is the average revenues in high school s and year t, divided by 250, which approximately corresponds to the 10% of revenues per student in 2012. I instrument revenues by the interaction of two components: L_{st}, capturing the rollout and subsidy increases (years since SEP initiation capped at four), and SES_{s06-08}, the pretreatment fraction of low-income students in school s between years 2006 and 2008 (years when available information allowed me to create a proxy for *priority* students).

To isolate the revenue effect from other confounding factors, I included a vector of covariates, Cov_{st}, comprising maternal schooling years, elementary school revenues per student, and interactions between a post-SEP indicator and SEP-driven policy changes (i.e., accountability, autonomy, and inter-school competition from schools charging family fees, and from schools seeking to attract low-income students). Additionally, to account for differences across schools, I included school fixed effects α_s and θ_s in the first and second stages, respectively. Similarly, I also added year fixed effects α_t and θ_t in the first and second stages to control for shocks affecting all high schools in any given year. Lastly, $\mathcal{E}_{cs}$ and ξ_{cs} are random error terms. Robust standard errors are clustered at the school level.

The coefficient δ from specification (5) captures the effect of the SEP-induced increase in revenues on peer victimization by comparing high dosage schools to low dosage schools. A negative estimate for δ indicates that schools exposed to longer and larger funding increases have lower peer victimization prevalence than unexposed schools. Specifically, it reports the impact of a 250 dollars per student-year, or a 10% increase in yearly revenues. This estimate represents a local average treatment effect relevant for those schools more likely to benefit from and participate in SEP (i.e., those with higher pre-reform share of low-income students). One key requirement to interpret δ as the causal effect of school revenues on peer victimization is that the timing of the SEP law – conditional on the covariates in vector Cov_{st} and school- and time-fixed effects – is exogenous to changes in peer victimization.

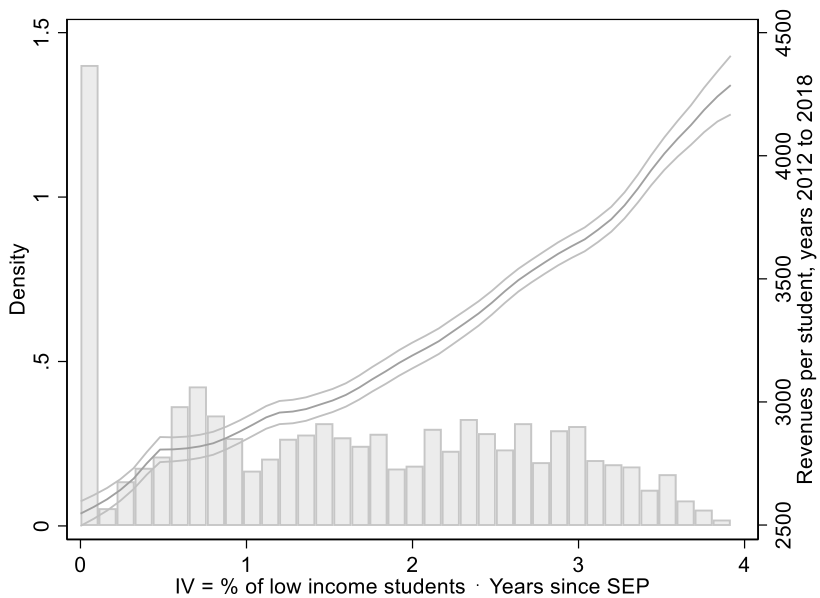
An additional crucial requirement for a causal interpretation of δ is that the instrument is relevant and valid, issues discussed in the next subsection.

6.1. Assessing the instrument

The minimum conditions for a causal interpretation of δ in (4) is that the instrument meets three core assumptions: relevance, exogeneity, and exclusion. A relevant instrument is significantly and substantially associated with the treatment. The exogeneity assumption requires that the instrument is not associated with any unmeasured factors that are associated with the treatment and outcome, and exclusion requires that the instrument affects the outcome only through its association with the treatment. In the presence of heterogeneous effects across schools, monotonicity must also be assumed. That is, schools with higher social vulnerability and exposed for longer to SEP indeed experienced larger increases in revenues.

The relevance of the instrument can be empirically tested by examining the strength of the association between the instrument and treatment captured by the estimate of β_1 in the first stage specification (4). Figure 4 provides a graphical representation of this relationship, including a histogram in the background that shows the distribution of the instrument (i.e., $L_{st} \cdot SES_{s06-08}$ in specification (4)). The line in the graph is a flexible approximation for the β_1 estimate in the first stage equation, plotting estimates form a local linear regression. This regression shows that the treatment dosage increases monotonically as the instrument increases. For example, a zero value for the instrument (i.e., before SEP implementation) is associated with a revenue of approximately 2,500 dollars per student, whereas an instrument value closer to four relates with revenues above 4,000 dollars per student (i.e., four years after SEP implementation in a school serving a high proportion of low-income students). More specifically, the estimates from the first stage revealed that the estimate of β_1 is a strong predictor of school revenues with a Kleibergen-Paap Wald F-statistic of the first stage around 140 (see **Table A.6** in the Appendix).

Figure 4: First stage graph of instrument on school revenues, years 2012 to 2018



Note: the variable years since SEP is capped at 4

Exogeneity implies that the instrument must be uncorrelated with variables that could affect the peer victimization outcomes, conditional on the set of covariates included in specifications (4) and (5). Given that the instrument changes from zero in pretreatment years to positive values in the posttreatment period (depending on the predicted dosage), it resembles an event study or DiD framework with continuous treatment. In these approaches, the exogeneity or conditional independence assumption is met if the timing of the SEP implementation is not confounded by other events occurring around the same time. The placebo test presented above showing no pre-treatment differences in revenues and other outcomes between lower- vs higher-intensity treated schools supports the claim that the timing of SEP was exogenous.

An alternative, indirect test of exogeneity implemented in applied work is to explore the impact of adding a large set of covariates to the first stage regression (e.g., Bhuller, Dahl, Løken, and Mogstad, 2020). If the conditional independence assumption holds, the inclusion of pre-determined variables should not significantly change the estimates, as they should be uncorrelated with the instrument. Given that I included school fixed effects in the first stage, I implemented this indirect test by adding a set of covariates measured in 2012 (before SEP) and interacting each of them (i.e., private management, vocational orientation, coeducation school, larger size, fraction of boys, and average students' age) with an indicator variable of the post SEP period. The inclusion of these covariates does not change appreciably the estimate of β_1 in the first stage specification (4) with both coefficients indicating that an increase of the instrument in one-unit is associated with an increase in revenues between 305 to 310 per student (see **Table A.7** in the Appendix).

Exclusion is a more restrictive assumption than exogeneity. Exogeneity (or conditional independence) is sufficient for a causal interpretation for the reduced form or the overall effects of SEP including all its components. To interpret the 2SLS estimates as the causal effects of revenues on peer victimization, the exclusion restriction must be met. That is, the instrument predicting treatment dosage should affect peer victimization only through its impact on revenues, and not directly through any other channel or mechanism. While there is no formal test that provides evidence of the validity of this assumption, applied researchers typically augment their preferred specification to show whether the main effects vary when they control for other potential mechanisms (e.g., Bhuller et al., 2020). I followed this strategy, and after presenting the main findings, I provide empirical evidence that the impacts do not change significantly when controlling for spending per student on school meals, lengthening of the school day, or expectations to attend college (see **Table A.12** in the Appendix).[5]

Monotonicity is required when effects are heterogeneous across schools. This assumption implies that all schools respond to the instrument in the same direction. In the 2SLS model of this chapter, it means that higher values of the instrument will cause an increase in school revenues, not a decrease. If monotonicity holds, then the 2SLS estimand can be interpreted as the effect of revenues on peer victimization among those schools that decided to participate in SEP due to the expected benefits captured by the instrument.

One testable implication of monotonicity is that the first stage estimates should be non-negative for any subsample. I implemented this test for eight different school types: public, private, academic orientation, vocational orientation, coeducational, single-sex students, smaller than the median size, and larger than the median size. For all these subsamples, the first stage estimates are large, positive, and statistically different from zero, providing support to the monotonicity assumption (see **Table A.8** in the Appendix).

[5] In the period under analysis, there are no institutional changes affecting school meals or the length of the school day. Regarding college attendance, a targeted increase in the national budget in 2016, made tuition free for the most vulnerable half of students applying to 30 universities (out of 58). In 2017, this benefit was institutionalized under law 21,091 and extended to six professional institutes (out of 33) and six technical institutions in higher education (out of 46). In 2018, the program expanded again to 49 higher education institutions (out of 139). If low-income students attending tenth grade were more academically motivated to obtain this benefit, their behavior towards peers could have improved, hence part of the effects estimated in this chapter could be attributable to the free-tuition program. However, including tenth grade students' expectation to attend college as an additional covariate does not change the estimated coefficients, suggesting the free-tuition program did not affect peer victimization among tenth grade students.

6.2. Main results

The 2SLS estimates presented in Table 3 indicate that increased SEP revenues significantly reduced peer victimization across all victimization types. Specifically, a 10% increase in per-pupil revenues lead to statistically significant reductions in physical (-1.0 pp), verbal (-2.0 pp), social (-2.1 pp), online (-0.8 pp), and victimization of any type (-2.7 pp). These effects represent reductions ranging from 7% to 10% of their baseline prevalence and are substantially larger than those obtained using an OLS model that estimates the association between revenues and peer victimization. The differences between the 2SLS and OLS estimates suggest that endogeneity may be biasing the OLS estimates. For example, other sources of funding variation, such as family-paid fees, may confound the OLS estimates as they relate to both higher school resources and lower peer victimization. Similarly, unobserved school management quality may also bias the OLS estimates if it leads to both a positive school climate and a greater ability to attract funding from various sources.

Table 3: Impact of SEP revenues on peer victimization

	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
OLS	-0.001	-0.002	-0.001	-0.002**	-0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2SLS	-0.010***	-0.020***	-0.021***	-0.008***	-0.027***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
Outcome average in 2012	0.115	0.283	0.221	0.116	0.360
Number of observations	3,694	3,694	3,694	3,694	3,694
Number of schools	537	537	537	537	537
Number of years	7	7	7	7	7

Note: Each cell reports results from independent models. All estimates include school fixed effects, time fixed effects, mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.
$*$ $p < 0.1$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Supplementary analyses indicate that these results are robust to different specifications. The main effects remain significant even when excluding the covariates, including interactions between the instrument and each of the SEP pro-efficiency interventions, or when progressively adding them to control for maternal education, elementary school resources, and SEP-induced school policy changes (see **Table A.9**, **Table A.10**, and **Table A.11** in the Appendix, respectively). Additionally, when including potential alternative mechanisms beyond revenues (i.e., per pupil spending on school meals, lengthening of the school day, and expectations to attend college; see footnote 5 for details), the estimated effects largely remain stable, strengthening the credibility of the exclusion restriction (see **Table A.12** in the Appendix).

Furthermore, the SEP-driven revenue increases significantly reduce intense experiences of verbal (-0.6 pp), and social (-0.7 pp) victimization, though they have no detectable impact on physical and online forms of aggression (see **Table A.13** in the Appendix). The effects on verbal and social victimization represent a reduction of 7% and 11% of the baseline prevalence, respectively, which are similar to the reduction of less intense exposure. The specificity of these findings suggests that reducing forms of victimization with very low baseline prevalence (3%–4%, as with intense physical and online aggression) might be more challenging than addressing more common types, such as intense verbal (8%) and social victimization (7%).[6]

[6] To explore whether SEP-related changes in teaching and non-teaching staff could explain these results, I estimated the impact of the reform on two outcomes: the student-to-teacher ratio and the student-to-ancillary staff ratio (see Appendix **Section A.2** and Appendix **Table A.14**, **Figure A.3**, and **Table A.15** for results). The analysis revealed significant pre-treatment trend differences for the student-to-teacher ratio, invalidating a causal interpretation for this input. For the student-to-ancillary staff ratio, however, the parallel trends assumption was met. The results show a post-SEP reduction in this ratio; a 10% increase in per-pupil revenue led to a 6% to 14% decrease from baseline values. While the magnitude of the results is sensitive to the inclusion of covariates, the findings suggest that an increase in support staff, such as school psychologists, constitutes a plausible mechanism for the impact of SEP revenues on peer victimization, which is consistent with recent evidence (e.g., Mulhern, 2023).

6.3. Specification checks

The primary findings rely on a 2SLS model incorporating TWFE for schools and years and controlling for several potential confounders. Recent research has scrutinized these modeling choices, highlighting that TWFE models or including covariates in a 2SLS model can bias the estimates in the presence of heterogeneous treatment effects or can complicate their typical interpretation as local average treatment effects relevant to complying schools that decided to participate in SEP due to its benefits (e.g., Blandhol, Bonney, Mogstad, and Torgovitsky, 2025; de Chaisemartin and D'Haultfoeuille, 2023).

Concerning treatment-effect heterogeneity, de Chaisemartin and D'Haultfoeuille (2023) and Callaway et al. (2024a) show that in complicated designs involving non-binary treatments, a TWFE model requires homogeneous treatment effects across units (i.e., schools) and over time (i.e., years) to avoid biases. For instance, biases could emerge when treatment effects of the same dosage vary across dose groups.

One strategy to address these concerns is to model the heterogeneity of treatment effects across schools and years, an approach proposed by Wooldridge (2021; 2023) in the extended TWFE model. Following this work, I added interaction terms between the revenues (and instrument) with (i) each of the top four school quantiles of pretreatment social vulnerability; (ii) each year between 2013 and 2018; and (iii) the combination of these groups and years. Results from these analyses continue to show significant and consistent reductions in physical (between -0.009 pp to -0.021 pp) and social peer (-0.042 pp to -0.018 pp) victimization, with less robust effects on verbal victimization (around -0.030 pp to -0.014 pp). The impact of revenues on online victimization is less robust and the coefficient closer to zero (-0.012 to 0.001; all estimates are presented in **Table A.16** in the Appendix).

Regarding the inclusion of covariates, Blandhol et al. (2025) and Mogstad and Torgovitsky (2024) demonstrate that while the inclusion of covariates typically strengthens the justification of exogeneity, linearly controlling for covariates might introduce biases if there are substantial differences between the conditional mean of the instrument given the covariates and the linear regression of the instrument onto the covariates. These authors recommend using data-driven machine learning techniques to select a flexible functional form of covariates and report these results along with the linear estimates from the 2SLS.

Following their work, I employed a Double Debiased Machine Learning (DDML) estimator for the partially linear instrumental variable (PLIV) specification outlined by Chernozhukov et al. (2018) and Ahrens et al. (2025), using the *ddml* package in Stata (Ahrens, Hansen, Schaffer, and Wiemann, 2024). The DDML approach involves fitting three expectation functions conditional on covariates – for the outcome, revenue, and instrument – using a combination of OLS and random-forest learners in fivefold cross-fitting procedure. I replicated this procedure five times, applying it to three model specifications: (i) the preferred specification used to estimate the main SEP effects reported in **Table 3**; (ii) the specification including additional covariates to test the exogeneity assumption (**Table A.7** in the Appendix); and (iii) the specification including alternative mechanisms to test the exclusion restriction (**Table A.12** in the Appendix).

The DDML estimates closely align with the original 2SLS results. However, the DDML point estimates for physical (2SLS = -0.10; DDML = -0.24) verbal (2SLS = -0.020; DDML = -0.029), and online victimization (2SLS = -0.008; DDML = 0.002) have a larger magnitude, while estimates for social victimization (2SLS = -0.021; DDML = -0.014) were somewhat smaller. These results demonstrate that the SEP revenues lead to a reduction on physical, verbal, and social forms of aggression; however, it is important to note that the DDML PLIV estimates are less precise than 2SLS with standard errors between two to four times larger (**Table A.17** in the Appendix).

6.4. Heterogeneous effects by school characteristics

Finally, I explored whether SEP revenue impacts on peer victimization vary by ten school characteristics measured before the implementation of the program: accountability pressure, autonomy in the use of SEP resources, competition from schools charging family fees, inter-school competition for low-income students, fraction of boys, average students' age, vocational orientation, private management, coeducational status, and enrollment size smaller than the median.

Two characteristics consistently moderate SEP's effectiveness: the proportion of boys and coeducational settings. SEP revenue impacts on peer victimization are significantly stronger in schools with higher proportions of boys and in coeducational schools (see **Table A.18** in the Appendix). These results suggest that the SEP-induced resource increases were particularly effective in improving social relationships within coeducational schools that had higher shares of male students.

7. Discussion

Promoting positive peer relationships at schools as a mechanism to improve students' socioemotional development, academic achievement, and long-term outcomes has become a critical goal for school and public health policy. School finance reforms designed to provide more funding to high-need schools are a promising, yet relatively unexplored, channel to achieve this objective.

In this study, I evaluated the impacts of Chile's nationwide SEP reform – which increased school budgets by up to 60% over six years based on student socioeconomic vulnerability – on peer victimization in high schools. Implementing DiD and 2SLS models, I found that SEP revenues consistently reduced physical, verbal, and social aggression, with less robust impacts for online victimization. By the sixth year of implementation, the cumulative effect of the program resulted in reductions of approximately 14% to 19% relative to baseline prevalence. On average, a 10% increase in per-student revenues between 2013 and 2018 translated into reductions in peer victimization between 7% to 10% from baseline, an effect size approximately half of that reported for the impact of interventions designed to improve school climate and peer relationships (Fraguas et al., 2021; Gaffney et al., 2019; Gaffney et al., 2021). These impacts extend to intense experiences of verbal and social victimization, but not for physical and online victimization. Overall, these benefits are notable given that prior evidence indicates that the impact of school-based interventions aiming to change students' behaviors tend to decrease over time with weaker effects for adolescents ages 13-17, compared to younger youth ages 9–12 (Yeager, Dahl, and Dweck, 2018; Yeager, Fong, Lee, and Espelage, 2015).

The positive impacts of SEP are concentrated in 2016-2018, a period characterized by a substantially larger funding increase compared to 2013-2015. DiD models using schools in the lowest quantile of social vulnerability as the control group show that while per pupil revenues rose by about 8% by 2015, they increased by nearly 21% by 2016 and 25% by 2018. The absence of meaningful effects in the earlier period suggests that an 8% increase in school revenues per student might have been insufficient to generate significant improvements in school inputs and outcomes. Conversely, the similar impact level for schools in the top half of the vulnerability distribution suggests that increases beyond the resources given to schools around the median of the vulnerability distribution (around 18% increase in school revenues per student) may have been more generous than necessary in the sense that the same impact could have been achieved with fewer resources. While relevant to the context and research questions of this study, an increase in the 15-20% range for school revenues per student should not be taken as a rule of thumb for making budget decisions. Rather than applying uniform funding thresholds, expert-driven, context-specific *cost out studies* are essential to define adequate budget allocations for achieving desired educational outcomes (Baker, Di Carlo, and Weber, 2022; Downes and Stiefel, 2015; Duncombe, Nguyen-Hoang, and Yinger, 2015).

While the benefits of increasing school funding are observed across forms of peer victimization, they emerge more consistently for social and verbal forms of aggression. The less consistent effects on physical and online victimization may relate to the nature of these negative interactions and how they are identified and addressed at schools. For instance, online victimization and cyber bullying are more costly to monitor for the school staff, who typically report in qualitative studies they perceive these aggression forms as more complex to detect and intervene on effectively (Cunningham, 2016; Lechner, Crăciun, and Scheithauer, 2023).

Additionally, physical and online types of peer violence are characterized by lower occurrence and higher persistence, which could be another factor complicating school responses. Qualitative and ethnographic research has identified two broad categories of bullying: (i) temporary exposure lasting weeks to months and (ii) more distressing and persistent exposure lasting years (Thornberg, 2018). The difficulty in managing persistent cases is illustrated by a serious cyberbullying case occurring in Canada between 2009 and 2012 that led to the *Protecting*

Canadians from Online Crime Act. In this case, the target of bullying was persistently harassed through the internet by multiple unrelated individuals as she transferred from one school to another multiple times (Espelage and Hong, 2017; Proctor, 2022).

While the effectiveness of school-based intervention may be limited in these extreme cases, the impact of SEP funds is observed across peer victimization types and different levels of intensity, especially at coeducational schools and those with a higher proportion of boys. The stronger impact in these settings may result from the combination of higher baseline peer victimization rates at schools with more boys, positive peer effects in the interaction between boys and girls favoring boys, and more proactive behavior to disrupt victimization among female students. Previous studies robustly show that girls are more cooperative with antibullying interventions, exhibit greater empathy, and are more likely to notice and disrupt bullying events compared to boys (Jenkins and Nickerson, 2019; Tamm and Tulviste, 2015).

Despite its strengths, including the use of causal methods linking increased funding to reduced victimization, this study has certain limitations. First, only 600 high schools out of over 2,000 started benefitting from SEP in 2013, which limits the external validity of findings both within Chile and across other education systems. Second, the data availability for the peer victimization variables during pre-treatment years was limited to only one year. More and earlier availability for the outcome data would have allowed me to conduct more direct tests to validate the empirical strategy rather than using data of revenues and other related variables. Third, comparable data across schools on their specific input needs are not available, precluding an examination of the main challenges perceived by the school community and the identification of budget constraints faced by managers that would better inform the adequacy of the extra funds. Lastly, the analyses are limited to evaluating the impact of increasing revenues, without examining specific actions and interventions schools undertook to utilize these funds or implementation challenges. Future research should address these limitations and examine specific school-level mechanisms and resource allocations that most effectively promote healthier peer relationships among different groups of students with distinct educational and social needs. A better understanding is needed of how higher investments are transformed into better student outcomes, under varying incentive structures and multiplicity of student needs. This could help to improve policies around defining adequate levels of funding across vulnerability levels and the types of incentives that should be attached to funding increases, taking into account heterogeneity in students' developmental needs.

Appendix

Section A.1: Other institutional changes introduced by SEP

This section expands on additional institutional components of the SEP reform that may have influenced school climate and peer relationships, specifically accountability measures, autonomy provisions, and competitive pressures. These mechanisms were quantified and included as covariates in the 2SLS model aiming to isolate the impact of revenues on peer victimization.

Accountability

The SEP program introduced a novel test-based accountability system that classified schools into three performance categories: *autonomous* (high-performing), *emerging* (average-performing), and *in-recovery* (low-performing, i.e., schools that haven't met minimum academic standards). These classifications were based primarily on standardized test scores in fourth-grade mathematics, language, and science. Although the three categories were established in the 2008 law, schools serving four grade students were formally ranked as in-recovery starting in 2012, and high schools serving 9-12 were incorporated into the system in 2017.[7]

Schools identified as *in-recovery* faced high-stakes consequences. If they fail to improve within three years, the Ministry of Education would notify families and facilitate transfers to other schools. After four years, these schools would lose their public funding, forcing them to cease operating. In addition, *in-recovery* schools were required to co-develop their improvement plans with external consultants and ministry officials, reducing their discretion over how SEP funds could be used.

International meta-analyses report that accountability systems lead to improvements in math test scores of around 0.06 standard deviations, smaller or null effects for language arts, null

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[7] The original 2008 SEP legislation did not include high schools. Laws 20,501 (2011) and 20,637 (2012) extended the program to cover secondary education. In addition, law 20,529 (2011), known as *Sistema de Aseguramiento de la Calidad* (SAC), required the government to revise the methodology to identify low-performing schools, ensuring that all *in-recovery* schools would be ranked as low-performing under the new method. The new system to rank schools was approved in 2014 and started operating in 2016 for elementary schools and in 2017 for high schools. The new system kept the threat of closing low-performing schools if they didn't improve after four years; however. Under the new methodology, around 340 high schools were identified as low-performing schools. Given the uncertainty during the first years of SEP about how schools were ranked, I used the three original categories of performance (that is, autonomous, emerging, and in-recovery) for analysis and defined pretreatment years as 2006, 2008, and 2010.

or positive effects on attendance and graduation, and no consistent effects on college enrollment or disciplinary incidents (Redding and Nguyen, 2020; Schueler, Asher, Larned, Mehrotra, and Pollard, 2022). The evidence on students' socioemotional outcomes is more limited and less encouraging. For example, Whitney and Candelaria (2017) found that accountability increased academic anxiety without improving behavior or emotional outcomes. Holbein and Ladd (2017) reported that school-based accountability raised suspension rates, fights, disciplinary infractions, and engagement in risky behaviors, with most pronounced effects among marginalized students and those with lower academic performance.

These findings echo broader concerns that accountability pressure can increase student stress, particularly among those with hypo- or hyper-reactive stress response systems – often students from disadvantaged backgrounds or those exposed to adversity (Heissel et al., 2021; Rudolph, Skymba, Modi, Davis, and Sze, 2022). Such stress can contribute to academic disengagement, heightened anxiety, and deteriorating peer relationships – highlighting the potential unintended social costs of accountability systems narrowly focused on academic outcomes.

Nonetheless, proponents argue that accountability frameworks can generate positive organizational change when paired with support, resources, and capacity-building efforts. In practice, these systems often enable governments to identify schools in need and direct technical assistance, staff training, or additional resources (Dee, 2020; Lee and Reeves, 2012; Strunk and McEachin, 2014). For instance, No Child Left Behind in the US led to increases in per-pupil spending, teacher compensation, and staff qualifications (Dee, Jacob, and Schwartz, 2013). In Chile, evidence suggests that SEP's accountability measures led schools to restructure teaching staff and invest in teacher training (Elacqua, Hincapie, and Martínez, 2024; Elacqua, Jaimovich, and Román, 2019) , with academic gains in previously under-regulated private schools (Murnane et al., 2017).

Although SEP's accountability framework primarily aimed to improve academic performance, its implications for school climate and peer dynamics remain understudied. On the one hand, it may have promoted better discipline, student support systems, and classroom organization. On the other, it may have overburdened school staff with administrative responsibilities, placed excessive academic pressure on students, and diverted attention away from socioemotional development.

Autonomy

Autonomous schools under SEP were granted greater flexibility in using public funds, based on the assumption that these schools had already institutionalized effective practices. Unlike *emerging* or *in-recovery* schools, *autonomous* schools were not required to develop formal improvement plans or undergo annual evaluations. Instead, they were assessed by the Ministry of Education once every four years and allowed to allocate 100% of their SEP funds at their own discretion, without earmarking half of them for specific planning activities as required for *emerging* schools.[8]

The impact of school autonomy on student outcomes has received less empirical attention than other components of SEP. A recent meta-analysis by Jackson (2023) reviewed eight quasi-experimental studies comprising 28 estimates and found an average effect of 0.0245 standard deviations on academic performance (p-value = 0.19). These results suggest that autonomy may yield gains or losses depending on the school context and leadership capacity.

In theory, greater flexibility in the use of funds can empower school leaders to tailor resource allocation to local needs, especially in schools serving students with more diverse backgrounds or more complex challenges (e.g., Acemoglu, Aghion, Lelarge, Van Reenen, and Zilibotti, 2007). In addition, autonomy may foster innovation, improve morale, and reduce staff turnover when principals have strong managerial skills and discretion to adapt programming (Jackson, 2023). However, more autonomy can also backfire when leadership lacks managerial capacity, or feels isolated, unsupported, or overburdened with administrative tasks (e.g., Travlos, 2020).

In Chile, the role of autonomy in shaping school outcomes under SEP remains relatively unexplored. In one of the few studies on this topic, Valenzuela et al. (2013) used a nearest-

[8] The SAC law (Law 20,529) required that all autonomous schools be classified as high-performing under the updated methodology introduced in 2016–2017. These schools retained key benefits such as reduced oversight, fewer reporting requirements, and an additional provision: the ability to serve as technical support providers for lower-performing schools. Under the new methodology, more than 400 high schools were classified as high performing. Given the uncertainty about classification rules during the first years after SEP was implemented, I used the original three performance categories (*autonomous*, *emerging*, *in-recovery*) and defined pretreatment years as 2006, 2008, and 2010.

neighbor matching design and found that greater autonomy in fund use was associated with stronger academic outcomes. While not focused on social dynamics, these findings suggest that flexibility in managing resources might help schools respond more effectively to local priorities, including those related to student relationships and climate.

Competitive pressures

SEP also introduced inter-school competition through two mechanisms. First, the policy required schools participating in SEP to waive family fees for low-income students, reducing financial barriers to access and encouraging families to switch to schools in better-resourced environments. As a result, the share of elementary schools offering free tuition to SEP-eligible students rose from 52% in 2007 to 71% in 2010 (Navarro-Palau, 2017). However, evidence suggests that actual school switching remained limited and was concentrated among relatively higher-income families within the eligible population (Aguirre, 2022; Navarro-Palau, 2017). Many low-income families continued to choose schools with similar academic performance and socioeconomic composition, indicating that informational barriers, residential segregation, or school preferences may have constrained competitive effects.

Second, SEP increased the financial attractiveness of enrolling low-income students. Because schools received larger per-student subsidies for priority students, the expected revenue from these students rose while their marginal costs remained relatively unchanged. Using a structural model of school choice, Neilson (2021) concludes that the combination of making more schools available to low-income students and the extra benefits from enrolling them reduced school market power and created incentives to improve service quality.

More broadly, international evidence supports the notion that competition can improve academic outcomes. A meta-analytic review of 92 studies estimated a positive effect of school competition on academic achievement of 0.06 standard deviation (Jabbar et al., 2022). These studies operationalized school competition using variation introduced by the opening, presence, or reach of private operators, as well as measures of concentration, diversity, and number of participants in each school market, enrollment in competing schools weighted by proximity, perceived threat of losing enrollment, and actual loss of enrollment to other nearby schools (Figlio and Hart, 2014; Jackson, 2012; Misra, Grimes, and Rogers, 2012).

In the Chilean context, a study by Gajardo and Grau (2019) suggests that competition may lead schools to prioritize academic outcomes at the expense of other dimensions of student development. These authors found that a one standard deviation increase in local school competition driven by the presence of private subsidized schools was associated with a 0.06 standard deviation gain in test scores, but also with declines in student self-esteem, academic motivation, civic participation, and perceptions of school climate, ranging between 0.02 to 0.12 standard deviations. These findings suggest that while competitive pressure may lead schools to improve test scores, it can also strain socioemotional outcomes and peer relationships – particularly if competition fosters stress, exclusion, or increased academic pressure.

Taken together, the SEP reform introduced not only substantial increases in school funding but also a range of complementary policies that may have shaped the school environment in ways beyond the intended academic improvements. To empirically evaluate the reform's impact on peer victimization, the analysis relies on rich administrative data linking school finances, student assessments, and contextual information. The next section describes the data sources, variables, and measurement strategies used to assess trends in peer victimization and explore how changes in public funding may have shaped student relationships across Chilean high schools.

Section A.2: Extra variables used for context and robustness checks

This section describes additional variables used to provide contextual information about SEP, high schools, and students, and to support robustness checks of the main results. While not central to the primary analyses, these measures offer complementary insights into student experiences, resource allocation, and school characteristics during the study period.

School violence variables were derived from SIMCE's student questionnaires. Since 2010, the survey has included three items asking how often, over the past year, students observed the following at their school: (1) physical fights (e.g., pushing, kicking, punching); (2) verbal aggression (e.g., insults, teasing, swearing); and (3) threats or harassment between classmates. Response formats varied over time: a 3-point scale in 2010 (1=Always or almost always, 2=Sometimes, 3=Never or Almost never), a 5-point scale from 2012 to 2016 (1=Never, 2=A few times per year, 3=Several times per month, 4=Several times per week, 5=Every day), and and a 4-point scale from 2017 to 2018 (1= Never or Almost never, 2=A few times, 3=Many times, 4=Always or almost always). To improve comparability over time, I recoded all responses to a 1–3 scale and reversed the order for 2010.[9] I used the school-level average of each item as outcome variables and the missing 2011 assessment data were imputed using linear interpolation at the school level. Given differences in response formats and their broad framing, these measures were interpreted with caution.

Test scores were obtained from SIMCE assessments that evaluate students' mastery of the national math curriculum. These scores follow a normal distribution, standardized to have a mean of 250 and a standard deviation of 50 in the first year of implementation. For tenth-grade students, the first SIMCE math assessment was administered in 1998. I focused on the 2006-2018 period, for which school revenue data is available. Missing 2007, 2009, and 2011 assessments data were imputed using linear interpolation at the school level.

To include students not taking the test but attending the school, I imputed missing scores using student-level GPA data. Specifically, I estimated a linear regression model including GPA

[9] To harmonize the response categories across years, I recoded them into a common 3-point scale as follows: For 2010, the original responses were mapped to 1=Always or almost always, 2=Sometimes, 3=Never or Almost never. For 2012-2016, the 5point scale was recoded into 1=Never or a few times per year, 2=Several times per month, 3=Several times per week or Every day. For 2017-2018, the 4-point scale was recoded into 1= Never or Almost never, 2=A few times or many times, 3= Always or almost always.

and school fixed effects to predict math scores for students with missing data. Predicted values below the minimum observed test score were replaced with that minimum, and values above the maximum were similarly capped. For ease of interpretation in the analyses, I standardized these imputed test scores to have an average of 0 and a standard deviation of 1.

College attendance were measured by two binary variables: (1) the proportion of 9th-grade students enrolled in each school who attend tertiary education one year after the expected high school graduation, and (2) the proportion of 9th-grade students enrolled in each school who attend tertiary education two years after the expected high school graduation. For example, for the cohort graduating high school in 2008 (that is, they were in 12th grade in 2008), the college attendance after one year since their expected graduation is calculated by dividing the number of students attending tertiary education in 2009 (that is, a university, professional institute, general training center, with the latter two being equivalent to community colleges), by the number of enrolled students at their school in 2005, when this cohort was in 9th grade.

College-going expectations are reported by students in the SIMCE questionnaires by responding to a question with six possible responses (1=will not finish high school, 2=will complete high school [academic track], 3=will complete high school [vocational track], 4=will obtain an associate degree, 5=will obtain a bachelor degree, 6=will obtain a graduate degree). I dichotomized this variable taking the value of one if the expectation is to obtain a bachelor or graduate degree and zero otherwise.

Teacher-to-student ratio was calculated by dividing the number of classroom teachers by the number of enrolled students in each school. Both figures were obtained from annual censuses conducted by the Ministry of Education, which record the school assignments of all teachers and students across the country.

Ancillary staff-to-student ratio was computed by dividing the number of ancillary staff by the number of enrolled students in each school. Ancillary staff data, available since 2007, were obtained from the annual census of school education assistants (e.g., school counselors, social workers, administrative assistants, other support personnel). Student enrollment data were drawn from the Ministry of Education's annual student census, available since 2002. Both sources include school identifiers, allowing for consistent school-level aggregation.

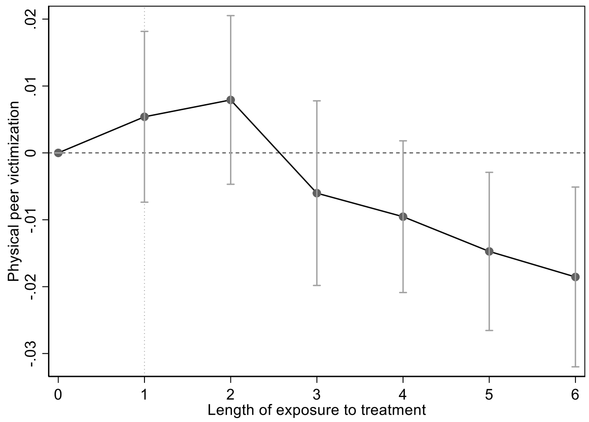
Municipal poverty rates were constructed using the National Socioeconomic Characterization Survey (CASEN), a nationally representative household survey collected biennially between 1990 and 2000, every three years from 2000 to 2015, and once more in 2017. For each municipality, I calculated the average headcount poverty rate, based on household income, over the 15 years preceding each SIMCE assessment of tenth-grade students, who are typically 15 years old. To ensure data consistency, the measure was restricted to 246 municipalities that participated in at least 9 of the 12 survey waves conducted between 1992 and 2017. Missing values were imputed using linear interpolation at the municipality level.

School meals program data were not available at the school level and were therefore excluded from the main analysis. However, to indirectly test the exclusion restriction, I constructed an approximate measure of school-level meal funding. I used data on annual budget allocations for the national high school meals program, defined at the beginning of each fiscal year, and assigned these funds to schools proportionally based on the number of student beneficiaries reported by the implementing agency (*Junta Nacional de Auxilio Escolar y Becas*, JUNAEB). To calculate a per-student allocation, I then divided the estimated school-level budget by total student enrollment, using administrative data from the Ministry of Education.

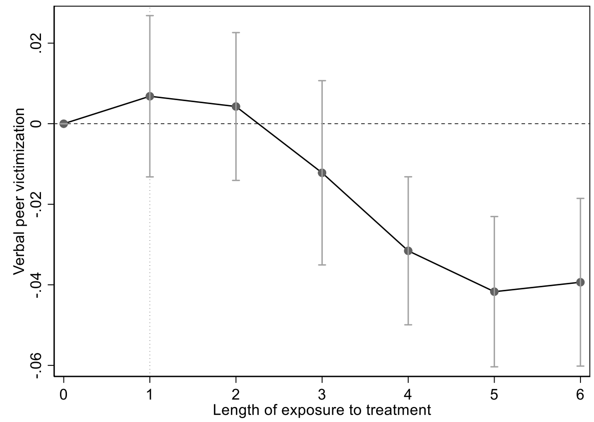
School day length was obtained from administrative records maintained by the Ministry of Education. I constructed a binary indicator equal to one for the year in which each school extended the instructional day for tenth-grade students, and zero otherwise. This variable captures the staggered rollout of full-day schooling across the school system.

Figure A.1: SEP effects on different forms of peer victimization

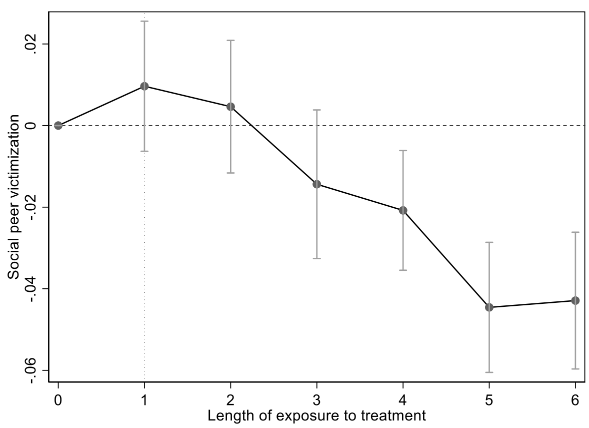
Panel A: Physical peer victimization



Panel B: Verbal peer victimization



Panel C: Social peer victimization



Panel D: Online peer victimization

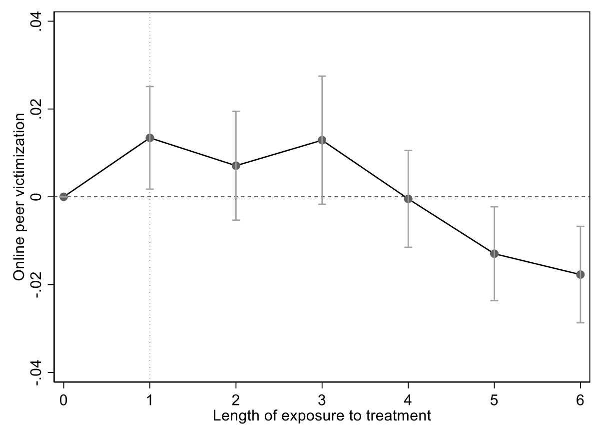


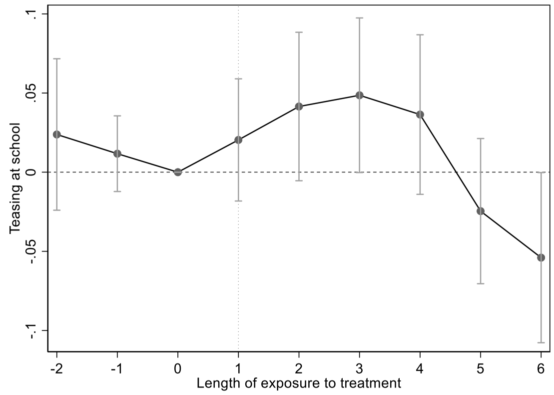
Table A.1: SEP effects on peer victimization

	Dependent variable					
	Revenues	Physical, PV	Verbal, PV	Social, PV	Online, PV	Any type, PV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cumulative effect						
Length of exposure to SEP						
One year	71.785**	0.005	0.007	0.010	0.013**	0.014
	(30.544)	(0.007)	(0.010)	(0.008)	(0.006)	(0.011)
Two years	163.732***	0.008	0.004	0.005	0.007	0.007
	(37.378)	(0.006)	(0.009)	(0.008)	(0.006)	(0.010)
Three years	196.504***	-0.006	-0.012	-0.014	0.013*	-0.018
	(37.787)	(0.007)	(0.012)	(0.009)	(0.007)	(0.012)
Four years	516.772***	-0.010*	-0.032***	-0.021***	-0.000	-0.031***
	(50.051)	(0.006)	(0.009)	(0.007)	(0.006)	(0.011)
Five years	602.398***	-0.015**	-0.042***	-0.045***	-0.013**	-0.052***
	(56.005)	(0.006)	(0.010)	(0.008)	(0.005)	(0.010)
Six years	610.053***	-0.019***	-0.039***	-0.043***	-0.018***	-0.049***
	(61.754)	(0.007)	(0.011)	(0.009)	(0.006)	(0.011)
Panel B: Overall average effect						
Average total effect	138.599***	-0.002	-0.008**	-0.007***	0.000	-0.009***
	(14.802)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
p-value placebo	0.443	-	-	-	-	-
Outcome average in 2012	2466	0.114	0.284	0.220	0.115	0.358
Number of observations	3285	3366	3366	3366	3366	3366
Number of schools	631	648	648	648	648	648
Number of years	13	7	7	7	7	7

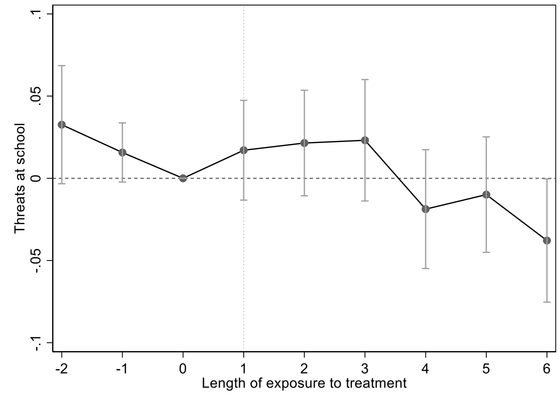
PV: peer victimization
Note: Each column within panels is an independent model. Robust standard errors clustered at the school level.
∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Figure A.2: SEP effects on other outcomes

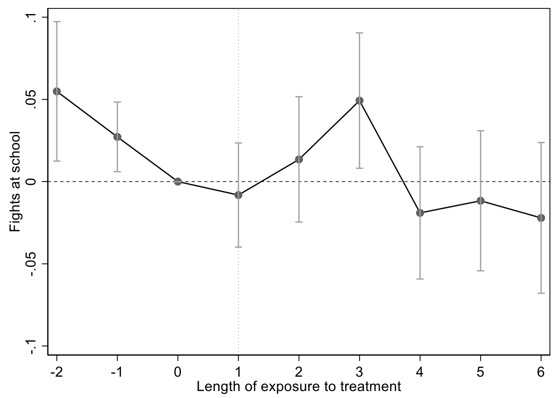
Panel A: Teasing at school



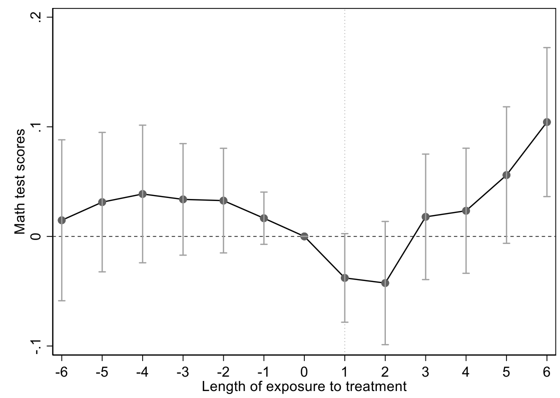
Panel B: Threats at school



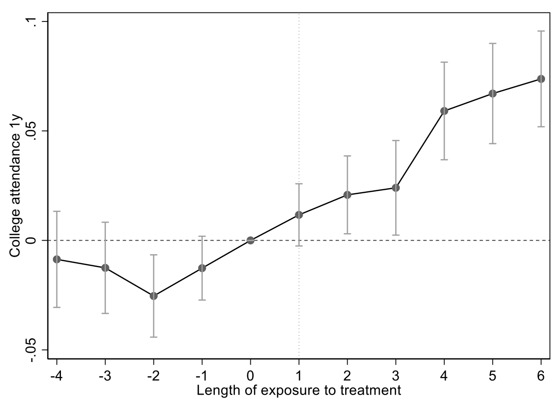
Panel C: Fights at school



Panel D: Math test score



Panel E: College attendance, 1-year



Panel F: College attendance, 2-years

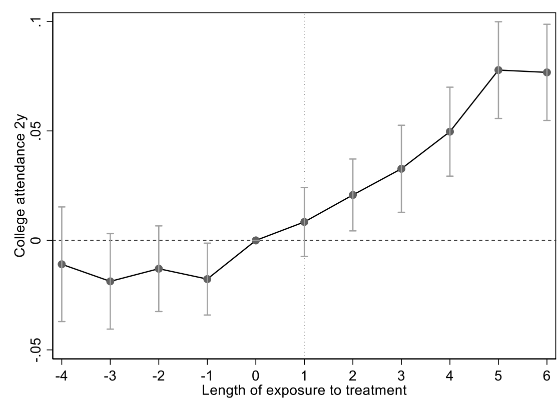


Table A.2: SEP effects on other outcomes

	Teasing in classroom	Threats in classroom	Fights in classroom	Math test scores	College going 1y	College going 2y
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cumulative effect						
Length of exposure to SEP						
One year	0.020	0.017	-0.008	-0.038*	0.012	0.008
	(0.020)	(0.015)	(0.016)	(0.021)	(0.007)	(0.008)
Two years	0.041*	0.021	0.013	-0.043	0.021**	0.021**
	(0.024)	(0.016)	(0.019)	(0.029)	(0.009)	(0.008)
Three years	0.049*	0.023	0.049**	0.018	0.024**	0.033***
	(0.025)	(0.019)	(0.021)	(0.029)	(0.011)	(0.010)
Four years	0.036	-0.019	-0.019	0.023	0.059***	0.050***
	(0.026)	(0.018)	(0.021)	(0.029)	(0.011)	(0.010)
Five years	-0.025	-0.010	-0.012	0.056*	0.067***	0.078***
	(0.023)	(0.018)	(0.022)	(0.032)	(0.012)	(0.011)
Six years	-0.054**	-0.038**	-0.022	0.104***	0.074***	0.077***
	(0.027)	(0.019)	(0.023)	(0.035)	(0.011)	(0.011)
Panel B: Overall average effect						
Average total effect	0.005	-0.000	0.000	0.008	0.017***	0.018***
	(0.007)	(0.006)	(0.006)	(0.010)	(0.003)	(0.003)
p-value placebo pretreatment test	0.585	0.170	0.0376	0.674	0.0585	0.282
Outcome average in 2012	2.043	1.450	1.519	-0.231	0.245	0.345
Number of observations (total effect)	3618	3618	3618	3614	3627	3627
Number of schools	664	664	664	685	676	676
Number of years	9	9	9	13	12	12

Note: Each column within panels is an independent model. Robust standard errors clustered at the school level.
∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Table A.3: SEP effects on peer victimization, including potential confounders

	Dependent variable					
	Revenues	Physical, PV	Verbal, PV	Social, PV	Online, PV	Any type, PV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cumulative effect						
Length of exposure to SEP						
One year	62.186**	0.005	0.009	0.012	0.013**	0.016
	(30.214)	(0.006)	(0.010)	(0.008)	(0.006)	(0.011)
Two years	161.904***	0.007	0.004	0.006	0.007	0.007
	(37.694)	(0.006)	(0.010)	(0.008)	(0.006)	(0.010)
Three years	190.231***	-0.008	-0.011	-0.011	0.012*	-0.017
	(37.829)	(0.007)	(0.012)	(0.009)	(0.007)	(0.013)
Four years	492.937***	-0.013**	-0.026***	-0.013*	-0.002	-0.026**
	(48.948)	(0.006)	(0.010)	(0.008)	(0.006)	(0.011)
Five years	593.162***	-0.018***	-0.035***	-0.036***	-0.015***	-0.047***
	(55.623)	(0.006)	(0.010)	(0.008)	(0.005)	(0.010)
Six years	588.218***	-0.023***	-0.030***	-0.031***	-0.020***	-0.041***
	(61.369)	(0.007)	(0.011)	(0.009)	(0.006)	(0.012)
Panel B: Overall average effect						
Average total effect	133.943***	-0.003*	-0.006*	-0.005**	-0.000	-0.007**
	(14.782)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
p-value placebo	0.660	-	-	-	-	-
Outcome average in 2012	2466	0.114	0.284	0.220	0.115	0.358
Number of observations	3,262	3,364	3,364	3,364	3,364	3,364
Number of schools	631	648	648	648	648	648
Number of years	13	7	7	7	7	7

Note: Each column within panels is an independent model. Robust standard errors clustered at the school level.
$*$ p < 0.10; $**$ p < 0.05; $***$ p < 0.01.

Table A.4: SEP effects on peer victimization, normalized effects

	Dependent variable					
	Revenues	Physical, PV	Verbal, PV	Social, PV	Online, PV	Any type, PV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cumulative effect						
Length of exposure to SEP						
One year	27.937**	0.002	0.003	0.004	0.005**	0.006
	(11.887)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)
Two years	31.956***	0.002	0.001	0.001	0.001	0.001
	(7.295)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Three years	25.672***	-0.001	-0.002	-0.002	0.002*	-0.002
	(4.937)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Four years	50.397***	-0.001*	-0.003***	-0.002***	-0.000	-0.003***
	(4.881)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Five years	46.987***	-0.001**	-0.003***	-0.004***	-0.001**	-0.004***
	(4.368)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Six years	39.525***	-0.001***	-0.003***	-0.003***	-0.001***	-0.003***
	(4.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Panel B: Overall average effect						
Average total effect	133.943***	-0.003*	-0.006*	-0.005**	-0.000	-0.007**
	(14.782)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
p-value placebo	0.660	-	-	-	-	-
Outcome average in 2012	2466	0.114	0.284	0.220	0.115	0.358
Number of observations	3,262	3,364	3,364	3,364	3,364	3,364
Number of schools	631	648	648	648	648	648
Number of years	13	7	7	7	7	7

Note: Each column within panels is an independent model. Robust standard errors clustered at the school level.
$*$ p < 0.10; $**$ p < 0.05; $***$ p < 0.01.

Table A.5: SEP effects on peer victimization including potential confounders, normalized effects

	Dependent variable					
	Revenues	Physical PV	Verbal PV	Social PV	Online PV	Any type of PV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cumulative effect						
Length of exposure to SEP						
One year	24.187**	0.002	0.004	0.005	0.005**	0.006
	(11.751)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)
Two years	31.664***	0.002	0.001	0.001	0.001	0.001
	(7.372)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Three years	24.813***	-0.001	-0.001	-0.001	0.002*	-0.002
	(4.934)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Four years	48.086***	-0.001**	-0.003***	-0.001*	-0.000	-0.003**
	(4.775)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Five years	46.213***	-0.001***	-0.003***	-0.003***	-0.001***	-0.004***
	(4.334)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Six years	38.191***	-0.002***	-0.002***	-0.002***	-0.001***	-0.003***
	(3.985)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Panel B: Overall average effect						
Average total effect	133.943***	-0.003*	-0.006*	-0.005**	-0.000	-0.007**
	(14.782)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
p-value placebo	0.660	-	-	-	-	-
Outcome average in 2012	2466	0.114	0.284	0.220	0.115	0.358
Number of observations	3,262	3,364	3,364	3,364	3,364	3,364
Number of schools	631	648	648	648	648	648
Number of years	13	7	7	7	7	7

PV: Peer victimization

Note: Each column within panels is an independent model. Robust standard errors clustered at the school level.

∗ $p < 0.1$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Table A.6: First stage estimates

	Dependent variable: Revenues per student							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment dose x Years since SEP	309.920***	314.125***	292.223***	306.403***	310.509***	316.348***	322.557***	306.071***
	(24.885)	(24.638)	(25.702)	(24.986)	(24.855)	(24.933)	(26.823)	(27.766)
Mother's schooling years		22.030*						26.480**
		(12.851)						(12.960)
Elementary school revenues			0.126**					0.154***
			(0.054)					(0.054)
Accountability x Post SEP				45.741				58.513
				(44.150)				(48.195)
Autonomy x Post SEP					32.066			51.609
					(30.004)			(34.101)
Competition from family fee schools x Post SEP						67.092		12.767
						(52.139)		(67.853)
Competition for low-income students x Post SEP							-157.537	-224.445*
							(105.110)	(135.910)
F-stat. (instrument)	182.4	191.9	152	176.9	183.6	189.2	170	143.4
Outcome average in 2012	2464	2464	2470	2464	2464	2462	2462	2461
Number of observations	3,867	3,745	3,850	3,867	3,867	3,812	3,812	3,691
Number of schools	578	548	574	578	578	568	568	538
Number of years	7	7	7	7	7	7	7	7

Note: Each column is an independent model. All estimates include school fixed effects and robust standard errors clustered at the school level.
$*$ $p < 0.10$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Table A.7: Testing for conditional independence

	Dependent variable: Revenues per student	
	(1)	(2)
Treatment dose x Years since SEP	306.071***	308.650***
	(27.766)	(28.475)
Mother's schooling years	26.480**	25.268*
	(12.960)	(12.937)
Elementary school revenues	0.154***	0.150***
	(0.054)	(0.052)
Accountability x Post SEP	58.513	11.453
	(48.195)	(46.487)
Autonomy x Post SEP	51.609	26.322
	(34.101)	(31.058)
Competition from family fee schools x Post SEP	12.767	31.101
	(67.853)	(65.900)
Competition for low-income students x Post SEP	-224.445*	-301.278**
	(135.910)	(133.000)
Private school x Post SEP		-215.306***
		(35.327)
Vocational school x Post SEP		-169.719***
		(35.590)
Coeducational school x Post SEP		10.819
		(49.187)
Larger schools x Post SEP		4.177
		(31.358)
Fraction of boys in school x Post SEP		15.476
		(67.997)
Students' age x Post SEP		65.264
		(61.853)
F-stat. (instrument)	143.4	138.7
Outcome average in 2012	2461	2461
Number of observations	3,691	3,685
Number of schools	538	538
Number of years	7	7

Note: Each column is an independent model. All estimates include school fixed effects and robust standard errors clustered at the school level.
∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Table A.8: Testing for monotonicity

	Dependent variable: Revenues per student							
School sub-sample:	Public (58%) (1)	Private (42%) (2)	Non-vocational (25%) (3)	Vocational (75%) (4)	COED (92%) (5)	Single-sex (8%) (6)	Smaller (50%) (7)	Larger (50%) (8)
Treatment dose x Years since SEP	169.381***	265.755***	356.483***	380.400***	296.153***	435.719***	304.445***	308.746***
	(33.198)	(42.967)	(47.260)	(47.846)	(28.252)	(116.514)	(36.646)	(42.110)
F-stat. (instrument)	30.59	45.59	66.99	74.72	129.7	16.63	81.76	63.28
Outcome average in 2012	2379	2612	2357	2500	2465	2408	2466	2456
Number of observations	2,435	1,256	1,009	2,682	3,413	278	1,797	1,894
Number of schools	348	190	147	391	498	40	268	270
Number of years	7	7	7	7	7	7	7	7

Note: Each cell is an independent model. All estimates include school fixed effects, time fixed effects, mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.
$*$ $p < 0.10$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Table A.9: OLS and 2SLS effects of SEP revenues on peer victimization, no control variables

	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
OLS	-0.001	-0.003*	-0.002*	-0.002**	-0.003
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2SLS	-0.009***	-0.020***	-0.020***	-0.007***	-0.024***
	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)
Outcome average in 2012	0.115	0.285	0.223	0.116	0.361
Number of observations	3,887	3,887	3,887	3,887	3,887
Number of schools	587	587	587	587	587
Number of years	7	7	7	7	7

Note: Each cell reports results from independent models. All estimates include school fixed effects and time fixed effects. Robust standard errors clustered at the school level.
∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Table A.10: OLS and 2SLS effects of SEP revenues on peer victimization, including interactions between the instrument and SEP pro-efficiency interventions

	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
2SLS (preferred specification)	-0.010*** (0.003)	-0.020*** (0.005)	-0.021*** (0.004)	-0.008*** (0.003)	-0.027*** (0.005)
2SLS (heterogeneity of pro-efficiency SEP measures)	-0.013*** (0.004)	-0.026*** (0.007)	-0.028*** (0.006)	-0.007* (0.004)	-0.038*** (0.008)
Outcome average in 2012	0.115	0.285	0.223	0.116	0.361
Number of observations	3,887	3,887	3,887	3,887	3,887
Number of schools	587	587	587	587	587
Number of years	7	7	7	7	7

Note: Each cell reports results from independent models. The second row presents estimates from models including interactions between the instrument and each of the post SEP variables measuring accountability, autonomy, competition from school charging fees, and competition from low-income students. All estimates include school fixed effects and time fixed effects. Robust standard errors clustered at the school level.
$*$ $p < 0.10$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Table A.11: 2SLS effects of SEP revenues on peer victimization, by covariate

	Dependent variable: Peer victimization, any type							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revenues	-0.024***	-0.025***	-0.025***	-0.023***	-0.024***	-0.024***	-0.027***	-0.027***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Mother's schooling years		0.002						0.002
		(0.005)						(0.005)
Elementary school revenues			0.000					0.000
			(0.000)					(0.000)
Accountability x Post SEP				-0.018				-0.008
				(0.012)				(0.013)
Autonomy x Post SEP					0.022***			0.015*
					(0.007)			(0.008)
Competition from family fee schools x Post SEP						-0.004		0.024
						(0.013)		(0.016)
Competition for low-income students x Post SEP							0.069***	0.075**
							(0.026)	(0.033)
Outcome average in 2012	0.360	0.359	0.360	0.360	0.360	0.360	0.360	0.359
Number of observations	3,771	3,734	3,771	3,771	3,771	3,718	3,718	3,681
Number of schools	562	540	562	562	562	552	552	530
Number of years	7	7	7	7	7	7	7	7

Note: Each column reports results from independent models. All estimates include school fixed effects and time fixed effects. Robust standard errors clustered at the school level.
$* \ p < 0.10$; $** \ p < 0.05$; $*** \ p < 0.01$.

Table A.12: 2SLS effects of SEP revenues on peer victimization, including other mechanisms

	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
2SLS (preferred specification)	-0.010***	-0.020***	-0.021***	-0.008***	-0.027***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
2SLS (controlling for other mechanisms)	-0.009***	-0.019***	-0.022***	-0.008***	-0.027***
	(0.003)	(0.005)	(0.005)	(0.003)	(0.006)
Outcome average in 2012	0.115	0.285	0.223	0.116	0.361
Number of observations	3,887	3,887	3,887	3,887	3,887
Number of schools	587	587	587	587	587
Number of years	7	7	7	7	7

Note: Each cell is an independent model. Additional mechanisms tested include spending per student on school meals, extended school day, and expectations to attend college. Robust standard errors clustered at school level.
∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Table A.13: OLS and 2SLS effects of SEP revenues on intense experiences of peer victimization

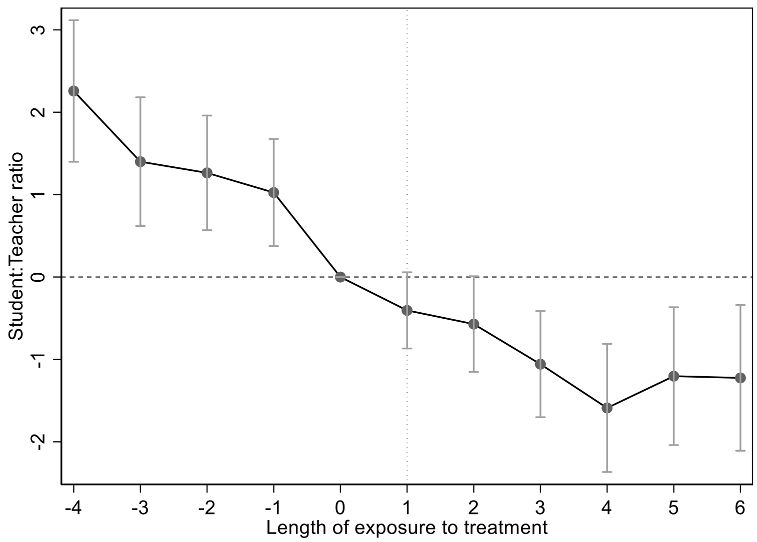
	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
OLS	0.000	-0.001	-0.001	-0.001	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
2SLS	-0.000	-0.006**	-0.007***	-0.001	-0.011***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
Outcome average in 2012	0.0269	0.0844	0.0710	0.0398	0.118
Number of observations	3,694	3,694	3,694	3,694	3,694
Number of schools	537	537	537	537	537
Number of years	7	7	7	7	7

Note: Each cell reports results from independent models. All estimates include school fixed effects, time fixed effects, mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.

∗ $p < 0.1$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Figure A.3: SEP effects on teaching and non-teaching staff, DiD intertemporal effects model

Panel A: The impact of SEP on student:teacher ratio



Panel B: The impact of SEP on student:ancillary staff ratio

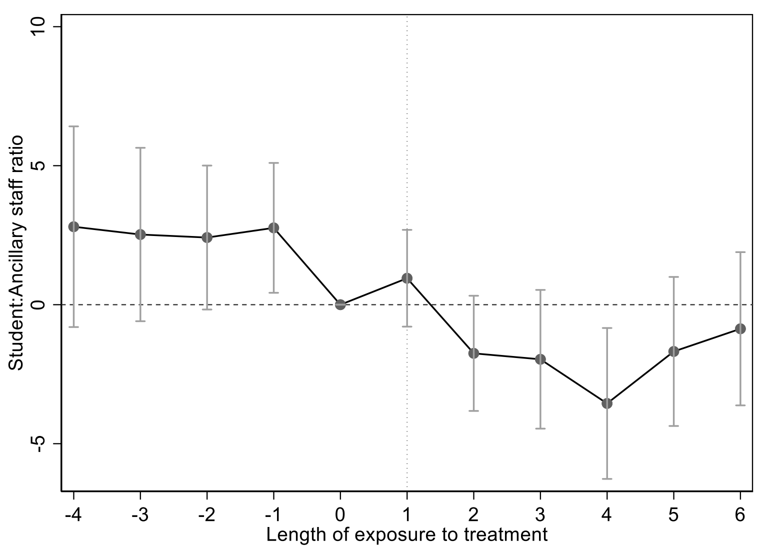


Table A.14: SEP effects on teaching and non-teaching staff, DiD intertemporal effects model

	Non-normalized effects				Normalized effects			
	No covariates		Covariates		No covariates		Covariates	
	STR	SAR	STR	SAR	STR	SAR	STR	SAR
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A: Cumulative effect								
Length of exposure to SEP								
One year	-0.405*	0.951	-0.442*	0.855	-0.162*	0.382	-0.177*	0.343
	(0.236)	(0.888)	(0.232)	(0.888)	(0.095)	(0.356)	(0.093)	(0.356)
Two years	-0.571*	-1.750*	-0.565*	-1.905*	-0.115*	-0.352*	-0.114*	-0.384*
	(0.296)	(1.057)	(0.296)	(1.061)	(0.060)	(0.213)	(0.060)	(0.214)
Three years	-1.057***	-1.964	-1.058***	-2.174*	-0.142***	-0.265	-0.142***	-0.293*
	(0.328)	(1.273)	(0.328)	(1.277)	(0.044)	(0.172)	(0.044)	(0.172)
Four years	-1.589***	-3.553**	-1.596***	-3.952***	-0.160***	-0.358**	-0.161***	-0.398***
	(0.397)	(1.385)	(0.398)	(1.393)	(0.040)	(0.139)	(0.040)	(0.140)
Five years	-1.203***	-1.682	-1.153***	-2.355*	-0.097***	-0.135	-0.093***	-0.190*
	(0.427)	(1.368)	(0.426)	(1.364)	(0.034)	(0.110)	(0.034)	(0.110)
Six years	-1.224***	-0.866	-1.165***	-1.633	-0.082***	-0.058	-0.078***	-0.110
	(0.451)	(1.406)	(0.450)	(1.403)	(0.030)	(0.094)	(0.030)	(0.094)
Panel B: Overall average effect								
Average total effect	-0.403***	-0.590	-0.399***	-0.742*	-0.403***	-0.590	-0.399***	-0.742*
	(0.118)	(0.415)	(0.118)	(0.415)	(0.118)	(0.415)	(0.118)	(0.415)
p-value placebo pretreatment test	<0.001	0.215	<0.001	0.197	<0.001	0.215	<0.001	0.197
Outcome average in 2012	18.70	33.54	18.70	33.54	18.70	33.54	18.70	33.54
Number of observations (total effect)	3632	3626	3610	3603	3632	3626	3610	3603
Number of schools	675	675	675	675	675	675	675	675
Number of years	11	11	11	11	11	11	11	11

STR: Student:teacher ratio; SAT: Student:ancillary staff ratio

Note: Each column within panels is an independent model. Robust standard errors clustered at the school level.

∗ p < 0.10; ∗∗ p < 0.05; ∗∗∗ p < 0.01.

Table A.15: OLS and 2SLS effects of SEP revenues on teaching and non-teaching staff

	No covariates		Covariates	
	STR	SAR	STR	SAR
	(1)	(2)	(3)	(4)
OLS	-0.179***	-0.571***	-0.126**	-0.543***
	(0.054)	(0.155)	(0.054)	(0.141)
2SLS	-1.244***	-2.051***	-1.408***	-4.597***
	(0.186)	(0.605)	(0.286)	(0.874)
Outcome average in 2012	18.45	33.42	18.60	33.62
Number of observations	6,381	6,286	6,099	6,009
Number of schools	619	618	591	590
Number of years	11	11	11	11

STR: Student:teacher ratio; SAR: Student:ancillary staff ratio

Note: Each cell reports results from independent models. All estimates include school fixed effects and time fixed effects. Columns (3) and (4) additionally control for mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.

$*$ $p < 0.1$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Table A.16: Treatment effects heterogeneity of the impact of SEP revenues on peer victimization

	Number of IVs	Type of peer victimization as outcome variable				
		Physical	Verbal	Social	Online	Any type
		(1)	(2)	(3)	(4)	(5)
2SLS, no TE heterogeneity	1	-0.010***	-0.020***	-0.021***	-0.008***	-0.027***
		(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
2SLS, TE heterogeneity by SES groups	5	-0.020*	-0.030	-0.040**	-0.012	-0.046*
		(0.012)	(0.019)	(0.020)	(0.010)	(0.024)
2SLS, TE heterogeneity by years	6	-0.009*	-0.014*	-0.018**	0.001	-0.023**
		(0.005)	(0.008)	(0.007)	(0.005)	(0.010)
2SLS, TE heterogeneity by SES groups and years	25	-0.021*	-0.030	-0.042*	-0.012	-0.048*
		(0.013)	(0.020)	(0.022)	(0.011)	(0.026)
Outcome average in 2012		0.115	0.283	0.221	0.116	0.360
Number of observations		3,694	3,694	3,694	3,694	3,694
Number of schools		537	537	537	537	537
Number of years		7	7	7	7	7

Note: Each cell reports results from independent models. All estimates include school fixed effects, time fixed effects, mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.

∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

Table A.17: 2SLS and DDML effects of SEP revenues on peer victimization

	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
Panel A: 2SLS models					
2SLS, main specification	-0.010***	-0.020***	-0.021***	-0.008***	-0.027***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
2SLS, exogeneity covariates	-0.011***	-0.022***	-0.022***	-0.010***	-0.028***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.006)
2SLS, exclusion covariates	-0.009***	-0.019***	-0.022***	-0.008***	-0.027***
	(0.003)	(0.005)	(0.005)	(0.003)	(0.006)
Panel B: DDML models					
DDML, main specification	-0.024**	-0.029*	-0.014	0.002	-0.017
	(0.012)	(0.017)	(0.011)	(0.008)	(0.014)
DDML, exogeneity covariates	-0.005	-0.008*	-0.010***	-0.004	-0.011**
	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
DDML, exclusion covariates	-0.028*	-0.030	-0.018	-0.004	-0.041*
	(0.016)	(0.019)	(0.014)	(0.010)	(0.021)
Outcome average in 2012	0.115	0.282	0.220	0.115	0.359
Number of observations	3,681	3,681	3,681	3,681	3,681
Number of schools	555	555	555	555	555
Number of years	7	7	7	7	7

Note: Each cell reports results from independent models. All estimates include school fixed effects, time fixed effects, mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.
$*$ $p < 0.10$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Table A.18: Heterogeneous impacts of SEP revenues on peer victimization, by school characteristics

	Type of peer victimization as outcome variable				
	Physical	Verbal	Social	Online	Any type
	(1)	(2)	(3)	(4)	(5)
Revenues x accountability	-0.000	0.002	0.002	0.002	0.003
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Revenues x autonomy	0.004***	0.003	0.003	-0.000	0.004*
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Revenues x competition (family fees)	-0.002	0.001	0.001	0.001	0.000
	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)
Revenues x competition (low-income)	0.004	0.004	0.006	-0.004	0.012
	(0.005)	(0.008)	(0.006)	(0.005)	(0.009)
Revenues x fraction of boys	-0.015***	-0.030***	-0.021***	-0.013***	-0.036***
	(0.003)	(0.005)	(0.004)	(0.002)	(0.005)
Revenues x students' age	-0.005*	-0.008*	-0.005	-0.003	-0.006
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
Revenues x vocational high school	-0.001	-0.003	-0.004**	0.000	-0.005*
	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)
Revenues x private high school	-0.002	-0.005	-0.007**	-0.003	-0.010**
	(0.003)	(0.004)	(0.003)	(0.002)	(0.005)
Revenues x coed high school	-0.005*	-0.013***	-0.005*	-0.001	-0.012***
	(0.003)	(0.005)	(0.003)	(0.002)	(0.004)
Revenues x smaller high school	0.001	0.001	-0.001	-0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Outcome average in 2012	0.115	0.282	0.220	0.115	0.359
Number of observations	3,681	3,681	3,681	3,681	3,681
Number of schools	555	555	555	555	555
Number of years	7	7	7	7	7

Note: Each cell reports results from independent models. All estimates are based on the baseline specification including school fixed effects, time fixed effects, mother's schooling years, elementary school revenues per student, and the following covariates interacted with the post SEP indicator: accountability, autonomy, competition from school charging fees, competition from low-income students. Robust standard errors clustered at the school level.
∗ $p < 0.10$; ∗∗ $p < 0.05$; ∗∗∗ $p < 0.01$.

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