

# Breaking Boundaries: Competitive Effects of Inter-District School Choice

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## Abstract

Expanding school choice can incentivize school districts to improve educational quality as they compete for enrollment. When students exercise that choice, however, changes in student composition and district resources may affect how well districts can educate students, leaving the overall effect uncertain. To study these tradeoffs, I exploit an Indiana property-tax reform that incidentally eliminated fees to attend public schools outside a student's residential district. Using statewide administrative data, I measure the extent to which each school district's enrollment is threatened by comparing its pre-reform achievement with that of neighboring districts. I find that districts facing greater enrollment pressure improve educational quality, narrowing pre-existing achievement gaps with neighboring districts by 5.3 percent overall and 8.4 percent in elementary grades. Gains are largest in smaller districts, where enrollment losses are harder to absorb. These findings suggest that expanding access to non-residential public schools can improve educational quality even for students who remain enrolled in their home district.

**JEL Classifications:** H75, I21, I28

**Keywords:** Inter-district school choice; Enrollment competition; Educational quality; Student mobility

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# I Introduction

Educational opportunities vary substantially across neighborhoods in the United States. Third through eighth grade students in one-sixth of school districts learn an additional two-thirds of a grade level in material, while those in another sixth fall behind by at least that amount, relative to the national average (Reardon, 2019). Inter-district school choice (IDSC) may expand access to higher-performing school districts by allowing students to attend schools outside the districts where they live.<sup>1</sup> Expanding access to IDSC may also affect students who do not transfer by increasing competition for enrollment. To prevent exits, low-performing districts may improve how effectively they educate their students. However, to the extent students select into choice, changing enrollment and resources may affect how well districts are able to teach their students.

In this paper, I study how enrollment competition impacts school districts' effectiveness. In 2010, Indiana's property-tax reform, House Enrolled Act 1001, incidentally eliminated fees households paid to enroll in a nonresidential school district. The reform sharply lowered the cost of transferring to another district, and IDSC participation grew steadily in the following years. Using comprehensive administrative records covering Indiana public-school students from 2006 to 2016, I construct district-year measures of enrollment, student composition, and academic achievement. These data are then used to calculate each district's value added (VA), which estimates how much districts are contributing to students' learning as measured by standardized test scores. I then study how enrollment and VA change across districts facing different enrollment-demand shocks after transfer fees are eliminated.

A simple model of competition is developed to show how removing barriers to IDSC participation may change districts' incentives to provide quality education. The model shows that when barriers to IDSC participation fall, districts must exert more effort to retain residential students but less effort to attract students from the neighboring school district.

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<sup>1</sup>In open-enrollment settings, there is little evidence students immediately benefit from attending a choice school (Cowen & Creed, 2017; Cullen et al., 2005), although Deming et al. (2014) find in the long run that elementary-grade choice users are more likely to enroll in college.

These opposing forces imply that districts may respond differently depending on how attractive they are relative to their neighboring alternatives.

Informed by my empirical model, along with the educational-demand literature that shows households strongly prefer nearby schools with higher-achieving students (Abdulkadiroğlu et al., 2020; J. Hastings et al., 2009; Reback, 2008; Rothstein, 2006), I measure each district’s exposure to IDSC enrollment threats using pre-policy achievement differences between itself and bordering districts. Bordering districts are defined as the relevant market because 92 percent of transfer students enroll in a school district bordering their own. Districts with higher-achieving neighbors face greater predicted enrollment threats after transfer tuition is eliminated, while those with lower-achieving neighbors are predicted to have net enrollment inflows. This measure is then used in the construction of a continuous difference-in-differences model. The model, when estimated, shows how much enrollment and VA change in school districts facing greater exposure as measured by the enrollment-threat exposure mechanism. I also use an event-study design to trace these changes over time. Both specifications control for average exposure among neighboring districts to account for spillovers between markets.<sup>2</sup>

Districts facing greater predicted enrollment threats experience enrollment declines but increase VA after transfer fees are eliminated. A one-standard-deviation increase in exposure reduces net enrollment by 2.06 percent of their legal enrollment, equivalent to losing an average of 50 students and roughly \$370,000 in state funding. A one-standard-deviation increase in exposure also raises VA by .0074, narrowing baseline achievement gaps with neighboring districts by 5.3 percent.<sup>3</sup> These gains are concentrated in elementary grades, where students are most likely to begin using IDSC, but are small and statistically insignificant in middle

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<sup>2</sup>Competitive pressure may also depend on the exposure faced by nearby districts. For example, neighboring districts facing greater predicted enrollment demand may reach capacity more quickly, limiting their ability to accept additional transfer students, thereby muting the enrollment threat faced by the focal district.

<sup>3</sup>VA is measured in test-score standard deviations. A VA increase of .0074 means students in this district are testing .0074 standard deviations above expectation after accounting for their previous test scores and socio-economic characteristics.

grades.<sup>4</sup>

Because districts facing greater enrollment threats also tend to serve larger shares of students in poverty or with disciplinary issues, they may not provide valid counterfactual trends for districts with lower exposure. However, event-study estimates show that VA was trending similarly across districts facing different enrollment pressure before transfer fees were eliminated. The event studies also show that districts facing greater enrollment threats increase VA immediately. Meanwhile, enrollment losses for these districts emerge gradually, suggesting districts are responding to enrollment threats proactively instead of responding when they lose students.

The continuous difference-in-differences model also assumes that any given achievement gap with neighboring districts generates the same competitive pressure regardless of a district's own achievement level. For example, it treats an average-achieving district surrounded by high-achieving neighbors as facing the same pressure as a low-achieving district surrounded by average-achieving neighbors. However, students may have weaker preferences for higher-achieving schools when their current district is already high-achieving.<sup>5</sup> I therefore estimate a separate specification that interacts the exposure measure with indicators for whether districts are low, middle, or high achieving in the pre-policy years. Competitive effects are then estimated within each tercile. In low- and middle-achieving districts, which tend to face enrollment losses on net, a one-standard-deviation increase in the exposure measure increases VA by .0119 and .0144, respectively. In contrast, the effect among high-achieving districts, which tend to gain students on net after IDSC is expanded, is small and both statistically and economically insignificant. These results then suggest achievement gaps are shrinking because threatened districts increase educational effectiveness in response to enrollment pressure, rather than higher-achieving districts decreasing effort because they

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<sup>4</sup>Although transfer enrollment is not continuously guaranteed, the modal Indiana transfer student remains in the same nonresidential district for four years.

<sup>5</sup>Researchers modeling household preferences for higher-achieving schools have historically assumed their preferences are concave in achievement (Epple & Romano, 1998; McMillan, 2004; Nechyba, 2000). Empirical evidence from Maroulis et al. (2019) also shows that students are more likely to use school choice to move from a low- to average-achieving school, rather than from an average- to high-achieving school.

are expecting enrollment gains.

VA results remain robust to modifying key features of the empirical design, including defining competitors by geographic distance rather than shared borders. Estimating a student-level model where the dependent variable is standardized test scores, rather than VA, also shows the findings are not sensitive to using a generated outcome. Finally, controlling for the time-varying effects of districts' pre-policy characteristics and resources gives evidence that compositional differences between more- and less-exposed districts do not threaten identification.

Even after transfer tuition is eliminated, remaining market conditions may shape whether districts respond to competition. I thus explore heterogeneous treatment effects by interacting the exposure measure with indicators for relevant pre-policy market characteristics. Two factors predict larger VA responses. Treatment effects are larger in geographically smaller districts, where shorter travel distances make neighboring districts more accessible. Stronger responses also emerge among districts with below-median pre-policy enrollment, where student losses are harder to absorb financially and operationally. Both patterns provide evidence that districts are actively improving educational effectiveness in response to competition, as VA gains are largest where enrollment threats are most consequential.

Although evidence consistently demonstrates competition is incentivizing districts facing greater enrollment threats to increasing VA, changes in student composition or district resources as IDSC participation expands could also contribute to the observed gains. However, I find little evidence for either channel. Students who participate in IDSC tend to be higher achieving and come from wealthier households, increasing the concentration of disadvantaged students in more-exposed districts. These compositional changes predict a small decline in VA, opposite in sign to the overall VA treatment effect. Greater exposure also produces no detectable changes in instructional spending per student, teacher salaries, or student-teacher ratios.

Finally, the data do not capture what internal practices districts may change to produce

immediate and persistent VA gains. I therefore draw on prior research to identify potential mechanisms and examine whether they can be proxied by other observable outcomes. I find no detectable changes in suspensions or expulsions, but modest increases in attendance over time may be indicative of districts creating more engaging school environments.

My paper directly contributes to the growing economics literature on school choice. A large body of work studies the direct effects of using intra- and inter-district choice on achievement (Bifulco et al., 2009; Choi & Hwang, 2017; Cowen & Creed, 2017; Cullen et al., 2005; J. S. Hastings & Weinstein, 2008) and long-run outcomes such as high-school graduation and college enrollment (Deming et al., 2014). I complement this work by studying how expanding access to IDSC affects the quality of education available to students who cannot or do not directly participate, broadening our understanding of how school-choice policies shape access to high-quality education overall.

A substantial literature studies public-school responses to enrollment competition induced by charter school entry (Bettinger, 2005; Gilraine et al., 2021; Imberman, 2011; Mumma, 2022; Ni, 2009; Winters, 2012) and private-school vouchers (Adger & Felegi, 2024; Figlio & Hart, 2014; Neilson et al., 2013), with mixed evidence on whether competition induces schools to raise educational effectiveness. IDSC differs from these settings in two important ways. First, IDSC can extend school-choice opportunities to families in rural communities, where charter and private schools tend not to locate.<sup>6</sup> IDSC is therefore one of the few forms of choice that create competition in these markets. Second, unlike charter and voucher programs, which primarily threaten public-school enrollment, IDSC creates competitive pressure both to retain resident students and to attract nonresident students. Districts may therefore respond differently depending on whether they are positioned to lose or gain students, making the overall effect on educational quality theoretically ambiguous.

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<sup>6</sup>In 2022–23, only 12 percent of charter schools were located in rural areas, compared with about 30 percent of traditional public schools (National Center for Education Statistics, 2024). Rural students were substantially less likely to attend private schools. Consistent with this pattern, Chingos and Blagg (2017) find that only 17 percent of rural families lived within ten miles of a charter school and document similarly limited geographic access to private schools.

A limited literature also examines competitive responses to IDSC expansion. Welsch and Zimmer (2012) find through Wisconsin’s statewide IDSC program that districts experiencing greater out-migration subsequently improve student achievement, while Lavy (2010) shows introducing public-school choice in Tel Aviv reduced dropout rates. I build on this work by measuring districts’ exposure to enrollment threats before students can respond. Because exposure is determined before IDSC is expanded rather than through realized student transfers, I can distinguish whether districts increase VA in response to anticipated enrollment threats or after realized losses.

## II Motivating Model

Removing barriers to IDSC participation may create competitive pressure on school districts by making schools in other districts more accessible to families. In this section, I adapt a model of private-school choice from McMillan (2004) to show how lowering IDSC transfer costs changes the effort districts must exert to retain residential students or attract students from competing districts. The model illustrates why opposing incentives can yield different quality responses depending on districts’ competitive positions.

### II.A Model Setup

Mirroring McMillan (2004), I consider a focal school district  $d$  where households differ only in income, with high-income households  $h$  earning  $y^h$  and low-income households  $l$  earning  $y^l < y^h$ . To abstract from the effects of student composition, households are otherwise identical: each has one child, and all children have the same ability. All households also share the same concave utility function  $U(c, A) = u(c) + g(A)$ , where  $c$  is consumption and  $A$  is the achievement level of the district their child attends.<sup>7</sup>

I then depart from McMillan (2004) by allowing households to choose between their

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<sup>7</sup>I assume  $u(c)$  and  $g(A)$  are each continuously differentiable, strictly increasing, and strictly concave, with  $u'(c) > 0$  and  $g'(A) > 0$ .

district of residence and a representative outside district. Residents of the outside district may also remain enrolled there or transfer into  $d$ , and these outside households have the same income and preferences as the corresponding household type in  $d$ . Households can enroll in their residential district for free, but incur a transfer cost  $v_t$  to attend a nonresidential district. This transfer cost includes tuition and other costs such as transportation.<sup>8</sup> Transfer tuition is eliminated between the model's two periods,  $t \in \{0, 1\}$ , reducing the total transfer cost so that  $0 < v_1 < v_0$ .

District  $d$  chooses effort  $e$  to maximize rents:<sup>9</sup>

$$R(e; v_t) \equiv [V - C(e)] E(e; v_t), \quad (1)$$

where  $V$  is the exogenous amount the district receives per student and  $C(e)$  is the generalized per-pupil cost of effort. Increasing effort therefore reduces the district's per-pupil markup but raises the quality  $Q(e)$  of education.<sup>10</sup> Quality directly influences district achievement:

$$A_d = Q(e) + \Gamma_d,$$

where  $\Gamma_d$  exogenously shifts achievement.<sup>11</sup> Increasing quality therefore makes district  $d$  more attractive and weakly increases enrollment  $E(e; v_t)$ , creating a benefit that the district trades off against the higher cost of effort. I assume the focal district takes the outside district's achievement  $A_{-d}$  as given when choosing effort to maximize total rents.<sup>12</sup>

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<sup>8</sup>I assume  $v_0 < y^l$ , ensuring positive consumption after transferring for both income types in both periods.

<sup>9</sup>Because public schools are not profit-making firms, it is not obvious why they should be modeled as rent maximizers. McMillan (2004) justifies this assumption by arguing that weak performance incentives allow schools to operate inefficiently and retain rents.

<sup>10</sup> $C(e)$  is strictly increasing and convex in effort, while  $Q(e)$  is strictly increasing and concave in effort.

<sup>11</sup>This exogenous component generates variation in achievement, and hence attractiveness, across the focal and outside districts while holding student ability fixed. Allowing student ability to vary instead would introduce compositional changes in enrollment, which I seek to abstract from.

<sup>12</sup>That is, I model the focal district's effort choice in partial equilibrium.

## II.B Quality Responses to Reducing Transfer Costs

Let  $l^R(v_t)$  and  $h^R(v_t)$  denote the minimum effort required for the focal district to retain low- and high-income resident households, respectively, and let  $l^I(v_t)$  and  $h^I(v_t)$  denote the minimum effort required to attract low- and high-income households from the representative outside district. Because a given transfer cost imposes a larger utility loss on low-income households, they are easier to retain when they reside in the focal district and harder to attract when they reside outside it. The effort thresholds therefore satisfy:<sup>13</sup>

$$l^R(v_t) < h^R(v_t) < h^I(v_t) < l^I(v_t).$$

Because enrollment is unchanged between any two adjacent thresholds and effort is costly, the focal district will optimally choose one of these four effort levels.<sup>14</sup>

Figure 1 illustrates how eliminating transfer tuition shifts these thresholds. When transfer costs fall from  $v_0$  to  $v_1$ , transferring to the outside district becomes more attractive to resident households. The focal district must therefore exert more effort to retain both household types, so:

$$l^R(v_1) > l^R(v_0) \quad \text{and} \quad h^R(v_1) > h^R(v_0).$$

At the same time, lower transfer costs make transferring into the focal district less costly for outside households, reducing the effort needed to attract either type:

$$h^I(v_1) < h^I(v_0) \quad \text{and} \quad l^I(v_1) < l^I(v_0).$$

Incentives to change effort depending on which students the focal district initially enrolled. At one extreme, consider a district just exerting enough effort to retain low-income resident households. Once transferring becomes less costly, the minimum effort required to

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<sup>13</sup>I formally derive the existence, ordering, and comparative statics of the effort thresholds in [Section D](#).

<sup>14</sup>I abstract from the possibility that the focal district shuts down and enrolls no students. [Section D](#) formally characterizes the focal district's optimal effort choice among these thresholds.

retain them rises. Because the district cannot shut down, it must increase effort from  $l^R(v_0)$  to  $l^R(v_1)$  to maintain their current enrollment (Figure 1).

At the other extreme, consider a district already exerting enough effort to attract both types of households from both their own and the outside districts. Lower transfer costs make outside students easier to attract, allowing the focal district to maintain the same enrollment with less effort. Therefore, it will continue to remain optimal to exert enough effort to maintain its current enrollment, meaning district initially choosing  $l^I(v_0)$  reduces effort and quality by choosing  $l^I(v_1)$ .<sup>15</sup>

Between these extremes, the response is ambiguous because lower transfer costs make resident students harder to retain while making outside students easier to attract. A district that previously exerted enough effort to retain all of its residents must now exert more effort to keep them, but it may instead accept some resident losses and reduce effort. Alternatively, the district may increase effort to attract outside households now that doing so is less costly. Likewise, a district already attracting some outside households may reduce effort and continue attracting them, or increase effort if attracting additional students becomes sufficiently valuable. Which response is optimal depends on the enrollment gains from changing effort relative to the additional effort required. The model therefore does not sign the overall effect of eliminating transfer tuition on district quality.<sup>16</sup>

The strength of these incentives also depends on the focal district's achievement compared with the outside district. When the focal district has lower achievement than the outside district, its residents face a stronger outside option and greater effort is required to retain them once transfer costs fall.<sup>17</sup> This result motivates my empirical approach of comparing pre-policy achievement differences between each district and its neighboring competition.

The model consequently motivates two empirical questions. First, does greater exposure

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<sup>15</sup>Section D formally establishes the signed effort responses for districts initially choosing  $l^R(v_0)$  and  $l^I(v_0)$ .

<sup>16</sup>Section D characterizes the possible post-policy choices at the two intermediate thresholds and shows why their effort responses are theoretically ambiguous.

<sup>17</sup>Section D formally derives how the household enrollment thresholds vary with achievement in the outside district.

to enrollment threats translate into larger enrollment losses after transfer tuition is eliminated? Second, how are districts’ educational effectiveness affected by enrollment threats when IDSC is expanded?

### III Inter-District School Choice in Indiana

Inter-district school choice (IDSC) allows students to enroll in schools outside their districts of legal residence. Indiana has supported some form of voluntary IDSC since the 1970s (“Commission on General Education v. Union Township School”, 1981), with districts retaining broad discretion over transfer admissions. Before the 2010 academic year, 15 percent of surveyed districts had policies against accepting transfer students, while others required applicants to meet achievement criteria and have a clean disciplinary history (Herrmann et al., 2009; A. S. Jackson, 2011). Thus, although IDSC was legally available, district admissions policies constrained families’ ability to use it.

Indiana school districts are financed through a combination of state and local dollars allocated across separate accounts. The largest account, the General Fund, covers districts’ day-to-day operating expenses, such as teacher and staff salaries and school supplies. While state funding follows students across district borders, local property-tax revenue remains with students’ districts of residence. Indiana law therefore allows nonresidential districts to charge students a fee, called transfer tuition, to replace this local revenue not following them. The fee was capped at the average amount of General Fund revenue per student financed through the local property-tax base (Michael et al., 2009). In the 2008 academic year (AY), transfer tuition ranged from \$2,000 to \$4,000, with the median district charging slightly over \$3,000 (A. S. Jackson, 2011).<sup>18</sup> Anecdotal evidence from A. S. Jackson (2011) suggests that transfer tuition represented a substantial financial barrier to IDSC participation for many households.

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<sup>18</sup>I refer to an academic year by the latter year. For example, the 2007–2008 school year is referred to as the 2008 AY.

Indiana eliminated transfer tuition as an unintended consequence of House Enrolled Act 1001 (HEA-1001). Passed in March 2008 amid growing financial hardship at the onset of the Great Recession, the bill capped property taxes at 1, 2, and 3 percent of assessed value for homestead, agricultural, and commercial/industrial real estate, respectively (Chu & Scafidi, 2019). To offset anticipated lost local property-tax revenue, the state assumed full responsibility for financing school districts' General Funds beginning in January 2009.<sup>19</sup> Because transfer tuition is tied to the share of General Fund revenue financed through local property taxes, the bill removed the statutory basis for charging it. Since households request transfers in the spring and summer of the prior academic year, the first transfer applications affected by the tuition change were for the 2010 AY. Figure 2 summarizes the timing of HEA-1001, state guidance, and the first transfer application cycle without transfer tuition.

The Indiana legislature did not anticipate HEA-1001 would eliminate transfer tuition (Chu & Scafidi, 2019). A September 2008 State Board of Education memo clarified that districts retained discretion over whether and how to accept transfer students (Herrmann et al., 2009). However, the 2009 AY had already begun and transfer decisions for were already made, leaving little opportunity for districts or households to immediately adjust. Furthermore, A. S. Jackson (2011) documents districts revising their transfer policies in October and November 2008 and continuing to adjust them through March 2009. The 2010 AY was therefore the first year where districts could implement any enrollment and instructional decisions fully understanding the tuition change.<sup>20</sup>

Consistent with this timing, Figure 3 shows that the percent of students using IDSC increased starting in 2010, continuing to rise steadily from 0.5% of public school students in 2009 to about 4% by the 2016 AY. Although eliminating transfer tuition removed a major financial barrier, access to IDSC still depended on whether districts accepted transfer

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<sup>19</sup>Consequently, district revenue per student from the state increased sharply in the 2010 AY, while local revenue per student fell (Figure A1).

<sup>20</sup>Research on school reform highlights the time needed to develop and implement district policies (Marsh et al., 2013). Quint (2006) similarly documents how a planning year helps schools prepare staff for organizational and instructional changes. These implementation demands likely constrained districts' ability to make substantial adjustments once the 2009 AY was underway.

students, how much capacity they had for transfer enrollment, admissions decisions, and households’ ability to provide transportation to non-residential districts.

Between the 2010 and 2013 AYs, state IDSC policy remained unchanged, with districts retaining discretion over whether to participate and which transfer students to accept. Beginning in the 2014 AY, however, House Bill 1381 (HB 1381) required participating districts to announce the number of available seats before each application cycle and use a lottery when applications exceeded capacity.<sup>21</sup> The bill did not require districts to participate or expand capacity. Households could still apply to any number of school districts and were not obligated to accept an offers of enrollment from school districts.<sup>22</sup> Except when required by a court order, districts were not obligated to provide transportation for transfer students.

## IV Data and Constructed Variables

### IV.A Data Source and Analytic Sample

Primary data are Indiana student-level administrative records from the 2006 to 2016 AYs. For each student, I observe the school they attend, district of residence, any use of accommodation and disability services, suspensions and expulsions, and Indiana Statewide Testing for Educational Progress (ISTEP) math and English language arts (ELA) scores. Because ISTEP scores are not directly comparable across grades and years, I standardize math and ELA scores within each grade-year. I also construct a composite score by summing each student’s raw math and ELA scores and standardizing the sum within grade-year. These records are supplemented with data from the National Center for Education Statistics (NCES) Common Core of Data (CCD). The Local Education Agency Universe Survey is used to construct student-teacher ratios and the School District Finance Survey to measure

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<sup>21</sup>I discuss transfer-student characteristics before and after the implementation of HB 1381, along with the implications for my main results, in [Section E](#).

<sup>22</sup>Households typically apply for transfer at the school-district level rather than to individual schools, listing school preferences when a district operates multiple schools at the relevant grade level. If the transfer request is accepted, the district then assigns the student to a school and notifies the household.

district expenditures and revenues.<sup>23</sup>

Any student-year observation is included in the sample if they are in grades K–8 attending a non-charter public school between the 2006 through 2016 AYs. While enrollment and attendance data are consistently reported for students in grades K–8, only 3–8 grades students take the ISTEP. To create a balanced district panel, I exclude any student in a district that do not report test-score data to the IDOE in every sample year.<sup>24</sup> These restrictions leave 4,843,182 student-year observations across 285 school districts.

Students are classified as transfers if they attend a school outside their district of legal residence on October 1<sup>st</sup>, the state’s official fall enrollment count date. [Table 1](#) compares residential and transfer students after the elimination of transfer tuition. Relative to residential students, transfers are less likely to qualify for free or reduced-price lunch (FRPL) or to be suspended or expelled (*Disciplined*). Among tested students, transfers also score approximately 0.075 standard deviations higher on the ISTEP. This observed selection is consistent with prior evidence that advantaged households are more likely to use public school choice (J. S. Hastings et al., [2005](#); Reback, [2008](#)), raising the possibility that choice users can affect districts’ educational effectiveness by changing the distribution of peers across schools.

Two district-level outcomes are constructed from the student-level data: the net transfer rate, which measures the percentage of a district’s legal enrollment gained or lost through net transfer flows, and school district value added, my measure of academic quality.<sup>25</sup> The resulting analytic sample consists of 3,135 district-year observations. The next two subsections describe how I construct each measure.

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<sup>23</sup>Because these district-level data cover grades outside the analytic student-level sample, they may be noisy proxies for K–8 financial resources. The financial data used in this paper cover the 2007–2016 AYs, though some variables are only observed starting in the 2008 AY.

<sup>24</sup>The excluded districts include North Central Park Community School Corporation, Union School Corporation, and Tri-Township Community School Corporation. These districts accounted for less than 1% of Indiana’s total enrollment in the 2009 AY.

<sup>25</sup>I construct outcomes at the district-year level because school districts, rather than individual schools, are the primary units competing for enrollment under IDSC. This aggregation matches the level at which the policy applies and enrollment-threat exposure is defined.

## IV.B Measuring Enrollment Shifts Due to Transfer Students

Following Welsch and Zimmer (2012), I measure the extent which net transfer flows affect school-district enrollment over time using the following equation:

$$\%NET_{dt} = \frac{\text{Incoming Transfers}_{dt} - \text{Outgoing Transfers}_{dt}}{\text{Legal Enrollment}_{dt}} \times 100 \quad (2)$$

Incoming transfers are students who reside outside district  $d$  but attend one of its schools, while outgoing transfers are residents of  $d$  who attend school in another district. Legal enrollment is the district’s residential student base, so %NET measures the percent change in a districts’ enrollment due to net-transfer students. Positive values indicate that incoming transfers exceed outgoing transfers, while negative values indicate the reverse. %NET is calculated for the full sample and separately using only elementary (K–5) and middle (6–8) grades, respectively.<sup>26</sup>

Figure 4 shows that net transfers left most districts’ enrollment unchanged before transfer tuition was eliminated. Afterwar, the interquartile range and tails of the %NET distribution widen, while the median remains near zero. By 2016, for districts in the top quartile net transfers increased enrollment between five to fifteen percent of their legal enrollment base, while those in the lowest quartile lost three to thirteen percent, reflecting growing enrollment redistribution across districts.

## IV.C Measuring School District Quality

To measure districts’ academic quality, I estimate their value added (VA) using students’ standardized test scores and socio-economic characteristics.<sup>27</sup> VA measures the extent to which students in district  $d$  in AY  $t$  are scoring on the ISTEP after accounting for their prior achievement and observed characteristics (Chingos et al., 2015).

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<sup>26</sup>Unless otherwise specified, reported values of %NET use the full sample.

<sup>27</sup>Studies of school-choice competition similarly use school VA to measure academic quality; see Adger and Felegi (2024), Campos and Kearns (2022), and Gilrairie et al. (2023).

Following Chetty et al. (2014) and Chingos et al. (2015), I generate VA estimates for each district-year by estimating the following OLS regression:

$$A_{idgt} = \alpha + \beta_{1g}A_{idg,t-1} + \beta_{2g}(A_{idg,t-1})^2 + X'_{it}\lambda + \theta_{dt} + \varepsilon_{idgt} \quad (3)$$

where  $A_{idgt}$  is the standardized achievement of student  $i$  in grade  $g$  attending a school in district  $d$  during AY  $t$ .  $X_{it}$  is a vector of the student’s characteristics.<sup>28</sup> The specification controls for lagged achievement and its square, allowing both relationships to vary by grade level. Because the model requires lagged test scores, I am limited to estimating VA using fourth to eighth grades students from the 2007 to 2016 AYs. VA is then the estimate of fixed-effect  $\theta_{dt}$ . Because  $\theta_{dt}$  captures all factors shared by students within a district-year, it should be interpreted as a reduced-form measure of district academic quality rather than a measure of teachers’ instructional effectiveness alone.<sup>29</sup>

VA is computed separately for math, ELA, and the composite score. I treat the composite score as my primary VA outcome, studying subject-specific VA when appropriate.<sup>30</sup> Figure 5 plots the kernel density of overall VA before and after the elimination of transfer tuition. Because test scores are standardized within each grade-year, this comparison describes changes in the distribution of districts’ relative performance within an AY rather than changes in Indiana’s absolute achievement over time. Both distributions remain approximately centered around zero, with greater variation in the post-policy VA estimates, particularly in the lower tail.

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<sup>28</sup>Student controls include sex, race, free or reduced-price lunch (FRPL) status, testing accommodations used, and if they have an individualized education plan.

<sup>29</sup>Section G shows that results are robust to an alternative VA specification that uses Bayesian shrinkage to account for differences in estimation precision across district-year cells (Chetty et al., 2014; Kane & Staiger, 2008).

<sup>30</sup>Lewis and Linzer (2005) show that treating value added as an observed outcome ignores its first-stage sampling uncertainty. If this error is correlated with IDSC competition measures, estimates of my primary model may be biased. I address this concern by estimating a student-level version of my primary dosage model, and find no evidence of bias from using the value-added measure as the primary outcome.

## V Methodology

### V.A Measuring Exposure to Enrollment Demand Shocks

Traditionally, the extent schools are exposed to enrollment pressure has been identified either through the presence or number of nearby competitors (Bifulco & Ladd, 2006; Figlio & Hart, 2014; Gilraine et al., 2021; Guul et al., 2021) or using geographic instruments that predict the number of choice options in a market (Hoxby, 2000; Ni, 2009; Winters, 2012). These measures capture the availability of nearby alternatives and are well suited to settings such as charter-school entry and private-school voucher expansion.

In the context of Indiana IDSC, however, eliminating transfer tuition may increase, decrease, or leave unchanged a district’s net enrollment demand. Proximity to competitors alone cannot characterize whether districts face increases or decreases in net-enrollment demand nor its intensity. Rather, this pressure depends on the relative attractiveness of nearby alternatives. I therefore construct an ex-ante exposure measure using achievement differences between each school district, which I refer to as the focal district, and its bordering school districts, which I call neighbors. Two observations motivate this approach.

First, parents value schools with high-achieving students, a proxy for educational quality (Abdulkadiroğlu et al., 2020; Burgess et al., 2015; Rothstein, 2006).<sup>31</sup> This is especially relevant in Indiana, where aggregate district test-score performance is made publicly available, but measures of academic quality like VA are not.<sup>32</sup> The data also indicate that, after transfer tuition is eliminated, students tend to transfer from districts with lower ex-ante achievement to those with higher achievement (Figure 6a), while selection based on districts’ ex-ante quality as measured by VA is less pronounced (Figure 6b).

Second, distance limits the set of districts that students are likely to consider transferring

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<sup>31</sup>By decomposing households’ education preferences, Abdulkadiroğlu et al. (2020) show that a strong preference for high-achieving schools exists even after conditioning on schools’ value added.

<sup>32</sup>Indiana began requiring public reporting of aggregate school-district performance data, including ISTEP test scores, with the annual performance report created by Public Law 34-1996 (Indiana General Assembly, 2006).

to (J. Hastings et al., 2009; Reback, 2008). After transfer tuition is eliminated, 92 percent of transfer students enroll in a district that borders their residential district, leading me to define a district’s relevant competitors as all bordering districts.<sup>33</sup>

Given these observations, I compute each district’s ex-ante exposure to enrollment threats using the following equation:

$$EXP_d = \sum_{h=1}^{H_d} \underbrace{(\overline{A}_h - \overline{A}_d)}_{\text{Pre-Policy Achievement Differences}} \cdot \underbrace{\frac{enroll_h}{\sum_{k=1}^{H_d} enroll_k}}_{\text{Pre-policy Enrollment Weights}} \quad (4)$$

where  $h$  indexes a neighbor of the focal district. Average pre-policy achievement for the focal and neighboring districts is given by  $\overline{A}_d$  and  $\overline{A}_h$ , respectively. These averages are computed using all composite standardized test scores from students enrolled in the respective district from AY 2007 through AY 2009.<sup>34</sup> Similarly,  $enroll_h$  is the number of students enrolled in district  $h$  over the same time frame.

Pre-policy achievement differences,  $\overline{A}_h - \overline{A}_d$ , are weighted by district  $h$ ’s share of pooled student-year enrollment among all of  $d$ ’s neighbors. This calculation places greater weight on larger districts, which have greater capacity either to accept incoming transfer students or to serve as a source of transfer enrollment. Because  $EXP_d$  is constructed entirely from pre-policy achievement and enrollment, it is not mechanically affected by post-policy changes in either measure.

Larger positive values of  $EXP_d$  indicate that the district is surrounded by relatively

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<sup>33</sup>Restricting competitors to neighboring districts may misrepresent educational markets if students can feasibly commute to schools in non-neighboring districts. As a robustness check, I construct alternative exposure measures that define competitors as non-residential districts with at least one elementary or middle school within three or six miles of the focal district’s borders. Results remain consistent with the primary exposure specification.

<sup>34</sup>Averaging achievement over multiple pre-policy years reduces the influence of temporary year-to-year fluctuations and better reflects persistent differences in district performance.

higher-achieving neighbors and therefore faces greater predicted enrollment losses after transfer tuition is eliminated. Similarly, negative values indicate predicted enrollment gains, while values nearer zero predict smaller enrollment shifts. For brevity, I refer to districts with positive values of  $EXP_d$  as *predicted-leaving districts* and those with negative values as *predicted-receiving districts*.

Table 2 reports summary statistics of three measures of  $EXP_d$ ; the measures are calculated either using all students in third to eighth grade, elementary-grade students (grades 3 to 5), or middle-grade students (grades 6 to 8).<sup>35</sup> A one-standard-deviation increase in exposure corresponds to a 0.21-standard-deviation increase in the enrollment-weighted achievement gap between the focal district and its neighbors.<sup>36</sup>

Figure 7 maps IDSC enrollment-threat exposure across Indiana school districts, with darker shading indicating greater exposure. Because exposure is defined relative to nearby districts, districts with greater predicted enrollment losses tend to be located near districts with greater predicted enrollment gains, while districts with similar exposure levels also tend to be geographically clustered.

Bordering districts vary substantially in geographic size, so adjacency may imperfectly capture how accessible neighboring alternatives are to students. Ideally, competition would be measured through the distance between a student’s assigned school and reasonably accessible alternative schools. However, the data identify only the school district a student would attend, not the student’s assigned school. Moreover, because transfers are administered primarily at the district level, meaning households’ abilities to target specific schools within districts are somewhat limited, it remains the natural unit at which to construct the exposure measure. In Section VI.D, I assess exposure measures that account for the distance between districts, showing my results remain robust to these specifications.

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<sup>35</sup>Unless otherwise stated, exposure refers to the measure calculated using all students in tested grades, three through eight.

<sup>36</sup>Figure 8 shows that standardized exposure ( $StExp_d$ ) is approximately normally distributed within each grade group.

## V.B Baseline Differences by Predicted Enrollment Pressure

Table 3 reports average district characteristics during pre-policy AYs, 2007 through 2009, for all districts and separately for predicted-leaving and predicted-receiving districts.<sup>37</sup> Compared with predicted-receiving districts, predicted-leaving districts enroll larger shares of students eligible for FRPL, receiving special education services, and that are suspended or expelled. Predicted-leaving district students score approximately 0.22 standard deviations lower on the ISTEP and have .03 lower VA, despite having higher revenues per student and lower student-teacher ratios.

My empirical design does not require balance on levels. Instead, I use a continuous difference-in-differences framework that compares changes in enrollment and VA across districts with different levels of pre-policy exposure. Identification therefore depends on whether districts with different realized exposure to enrollment threats are valid counterfactuals for each other on enrollment and VA trends.

## V.C Empirical Specification

I use the following continuous difference-in-differences model to estimate the effects of IDSC competition on school districts' enrollment and VA:<sup>38</sup>

$$Y_{dt} = \beta Post_t \cdot StExp_d + \lambda Post_t \cdot StNbr_d + \delta_d + \tau_t + \epsilon_{dt} \quad (5)$$

$Y_{dt}$  is an outcome for district  $d$  in AY  $t$ , and  $Post_t$  indicates AY 2010 onward, when students could first use IDSC without paying transfer tuition.  $StExp_d$  is the standardized exposure measure defined earlier, while  $StNbr_d$  captures the average exposure faced by neighboring districts. District and year fixed effects,  $\delta_d$  and  $\tau_t$ , account for time-invariant differences

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<sup>37</sup>I exclude AY 2006 from these baseline comparisons because constructing district value added requires lagged student test scores.

<sup>38</sup>The primary specification does not control for time-varying peer composition or district resources because both may be affected by enrollment shifts. Controlling for these potential mechanisms would therefore absorb part of the effect the model is intended to estimate.

across districts and common shocks within years. Because exposure is determined before the policy change,  $\beta$  is identified from changes in  $Y_{dt}$  across districts with different pre-policy exposure to enrollment competition. I calculate exposure using the same grade-level sample as the outcome whenever possible.<sup>39</sup>

The extent a district’s enrollment is threatened may depend not only on its own exposure to competition, but also on the exposure faced by neighboring districts. For example, neighbors facing stronger enrollment demand may reach capacity quicker, reducing threats faced by the focal district independently of its own exposure. This interaction across markets violates the stable unit treatment value assumption (SUTVA), since district  $d$ ’s outcome may depend on the treatment exposure of other districts.<sup>40</sup> I therefore control for  $StNbr_d$ , the standardized average exposure faced by neighboring districts, interacted with the post-treatment indicator.<sup>41</sup> Under this specification, SUTVA requires that average neighboring-district exposure captures all relevant spillover effects:

$$Y_{dt}(StExp_d, StExp_{-d}) = Y_{dt}(StExp_d, StNbr_d).$$

Interpreting  $\beta$  relative to a counterfactual in which transfer tuition remained in place would require assuming that the policy generated no statewide response and that districts predicted to experience little changes to enrollment demand did not respond to the policy. Seeing both assumptions as strong, I instead interpret estimates of  $\beta$  as a measure of the post-policy change in  $Y_{dt}$  associated with a one-standard-deviation increase in the enrollment-threat measure, conditional on neighboring districts’ average exposure.

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<sup>39</sup>For example, when studying middle-grade district value added, I calculate exposure using only middle-grade students. The exception is the elementary net enrollment outcome: exposure uses students in grades three through five, while net enrollment includes students in kindergarten through fifth grade because enrollment records, unlike test scores, are available for these earlier grades.

<sup>40</sup>In spatial settings, Clarke (2017) emphasizes that SUTVA may fail when treatment effects spill across nearby or economically connected units and proposes controlling for factors related to competitors’ market conditions.

<sup>41</sup>I first calculate each neighboring district’s IDSC enrollment-threat exposure using Equation (4), excluding district  $d$  from the calculation. I then average these measures to obtain  $Nbr_d$  and standardize it to place the average neighboring exposure on the same scale as  $StExp_d$ .

Because districts' markets are interconnected, outcomes may also be correlated across districts competing for the same students. I therefore use network-robust standard errors following Colella et al. (2023), defining a district's network as all bordering districts. This allows for dependence across neighbors as well as serial correlation within districts over time.

To assess whether pre-policy trends differed across exposure levels and to examine post-policy dynamics, I also estimate an event-study specification:

$$Y_{dt} = \sum_{l=-3, l \neq -1}^6 \psi_l(StExp_d \cdot \mathbf{1}\{t - 2010 = l\}) + \sum_{l=-3, l \neq -1}^6 \theta_l(StNbr_d \cdot \mathbf{1}\{t - 2010 = l\}) + \delta_d + \tau_t + \epsilon_{dt} \quad (6)$$

where AY 2009 is the omitted reference year. The coefficient  $\psi_l$  captures the differential change in  $Y_{dt}$  in AY  $2010 + l$ , relative to AY 2009, associated with a one-standard-deviation increase in predicted exposure to enrollment pressure.

For  $\beta$  to be causally identified, the strong parallel trends (SPT) assumption must hold, meaning districts with realized exposure  $e$  must be valid counterfactuals on trends for districts with other exposure levels (Callaway et al., 2024):

$$\mathbb{E}[Y_{post}(StExp) - Y_{pre}(0)] = \mathbb{E}[Y_{post}(StExp) - Y_{pre}(0) | StExp = e]$$

This assumption may be violated because more-exposed districts differ systematically from less-exposed districts in ways that can affect educational effectiveness (Table 3). To weaken this assumption, I estimate a *tercile-adjusted* specification that allows treatment effects to vary across districts' with different ex-ante achievement terciles.<sup>42</sup>

$$Y_{dpt} = \alpha_d + \tau_{pt} + \sum_{a=1}^3 \beta_a (Post_t \cdot StExp_d \cdot \mathbf{1}\{p = a\}) + \nu Post_t \cdot StNbr_d + \varepsilon_{dpt} \quad (7)$$

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<sup>42</sup>This approach follows from C. K. Jackson et al. (2016), who allow the effects of school finance reforms to vary flexibly across districts' baseline spending quartiles.

Here  $p$  denotes ex-ante achievement tercile, and coefficient  $\beta_a$  identifies the exposure effect within tercile  $a$ .<sup>43</sup> Year fixed effects are allowed to vary within achievement terciles as well. This specification weakens the baseline SPT assumption by only requiring districts with different exposure levels if they are within the same achievement tercile.

The primary model also makes the assumption that same achievement gap across districts generates the same level of enrollment pressure, even conditional on their ex-ante achievement. However, students may have stronger preferences to move from a low- to an average-achieving district than from an average- to a high-achieving district.<sup>44</sup> The achievement-tercile also weakens this assumption by only comparing districts with similar ex-ante achievement. Furthermore, because linearity violations can bias  $\beta$  even if SPT holds (Callaway et al., 2024), in Section F I estimate a non-parametric specification that binarizes treatment, treating predicted-leaving districts as the treatment group and predicted-receiving districts as the control group. I also use Callaway et al. (2024) to estimate the average causal response function, which I do not use as my primary specification due to power limitations<sup>45</sup>

## VI Results

### VI.A Exposure and Realized Enrollment Shifts

The extent IDSC’s can pressure districts to improve educational quality depends in part on how extensively families use the program and which districts they choose to attend. Figure 6 illustrates this scale by tracing post-policy transfer student-years across districts’ ex-ante achievement terciles. More than half of outgoing transfer student-years originate

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<sup>43</sup>Figure A2 shows exposure distributions by ex-ante achievement tercile, illustrating the correlation between exposure and baseline achievement.

<sup>44</sup>Existing evidence suggests that households may be more responsive to poor performance at their current school than to high performance at alternative schools. For example, Maroulis et al. (2019) finds that the push of low school performance is stronger than the pull of high performance, particularly among low-achieving students.

<sup>45</sup>I later show that point estimates from both the discretized and average-causal-response specifications are comparable to my main results.

in the lowest-achieving districts, including 26,531 student-years flowing to districts in the highest-achieving tercile. Overall, these flows produce net enrollment losses for the lowest-achieving tercile and net gains for the highest.

I then study how these transfers affect districts’ enrollment bases. [Table 4](#) reports effects of IDSC enrollment-threat exposure on  $\%NET$ , the percent change in enrollment due to net transfer students within a district. Column (1) presents the primary specification (Equation (5)), while column (2) allows exposure effects to vary across ex-ante achievement terciles (Equation (7)). Parentheses report network-robust standard errors, while brackets report each group’s average post-policy net transfer enrollment as a percentage of legal enrollment. These estimates capture realized enrollment shifts net of competitive responses, since improvements in VA may induce students considering transferring to remain in their residential districts.

The primary specification shows that exposure to enrollment threats translates into meaningful enrollment losses: a one-standard-deviation increase in exposure reduces net transfer enrollment by 2.06 percent of districts’ legal enrollment base, statistically significant at the one-percent level. Estimated losses are larger among low- and middle-achieving districts (2.38 and 3.48 percent, respectively) than among high-achieving districts (1.55 percent), although these differences are not statistically significant. Although the differences are not statistically significant, these point estimates are consistent with households having stronger demand to switch districts when their current district is already higher-achieving. Neighboring exposure does not significantly predict net transfers, providing little evidence of spillover effects across districts.

Enrollment-threat exposure has similar effects on elementary- and middle-grade enrollment, with estimated losses of 2.08 and 1.73 percent of legal enrollment, respectively ([Table B2](#)). The slightly larger elementary-grade estimate is consistent with students being more likely to begin IDSC in elementary school, particularly kindergarten, where anecdotal evidence from Harris and Larsen (2015) suggests transition costs are lowest ([Table B3](#)).

Because state funding follows students, enrollment losses reduce districts’ financial resources. For the average Indiana district, the estimated 2.06 percent decline in K–8 enrollment corresponds to approximately 50 fewer students and \$370,000 less in annual state funding.<sup>46</sup> This represents approximately 0.8% of average district revenues in AY 2010 (Cornman, 2013). These financial losses may strengthen districts’ incentives to improve educational quality to retain and attract students, while simultaneously reducing the resources available to make those improvements.

The event-study estimates show that enrollment losses from greater exposure grow steadily over time (Figure 9). Pre-policy coefficients are close to zero, while post-policy coefficients become increasingly negative, with the estimated effect of a one-standard-deviation increase in exposure approaching a 4-percentage-point loss by AY 2016.<sup>47</sup> These losses may accumulate as families learn about IDSC and receiving districts expand their capacity to enroll transfer students (A. S. Jackson, 2011).

Taken together, these results show that enrollment-threat exposure translates into meaningful enrollment losses, creating incentives for districts to retain students. The next section examines how this competitive pressure and the resulting enrollment shifts affect educational quality, as measured by VA.

## VI.B Exposure and School District Value-Added

Despite being negatively impacted by enrollment losses, greater exposure to enrollment threats improves districts’ educational effectiveness. Table 5 shows that a one-standard-deviation increase in enrollment-threat exposure raises districts’ VA by 0.0074, statistically significant at the five-percent level. The math estimate of 0.0087 is slightly larger than the ELA estimate of 0.0076. As with net transfer enrollment, neighboring exposure does not significantly predict VA.

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<sup>46</sup>The student count is derived from average base enrollment reported in Table 3. Indiana provided approximately \$7,100 per student in the 2010 AY (Figure A1(a)).

<sup>47</sup>Separate event studies for elementary- and middle-grade enrollment mirror Figure 9 and are shown in Figure A3.

To understand which districts are driving these improvements I turn to the tercile-adjusted specification. Estimated VA gains are larger among low- and middle-achieving districts, at 0.0119 and 0.0144, respectively, compared with 0.0024 among high-achieving districts, although only the low-achieving estimate is statistically significant at the five-percent level. Combined with the earlier finding that lower-achieving districts experience greater net losses, this pattern suggests that positive VA responses are driven by districts improving their educational effectiveness to retain current students, rather than by predicted receiving districts seeking to attract additional enrollment.<sup>48</sup>

Because districts facing greater enrollment threats also tend to have lower initial achievement, their VA improvements may also narrow existing achievement gaps. To gauge the magnitude of this effect, I compare districts at the 75th and 25th percentiles of the enrollment-threat exposure mechanism. Before the policy, these districts differed in achievement by 0.174 test-score standard deviations, with the more exposed districts performing worse.<sup>49</sup> These districts differ in enrollment-threat exposure by 1.24 standard deviations, which, when multiplied by the estimated VA effect of 0.0074, implies a 0.0092 larger VA gain for the more exposed districts. This improvement is equivalent to 5.3 percent of their initial achievement gap.<sup>50</sup>

Estimates from other education policies provide useful benchmarks for assessing the economic significance of the implied 5.3% reduction in achievement gaps. Card and Payne (2002) estimate that school-spending equalization following court rulings narrowed the SAT-score gap between children of highly and poorly educated parents by approximately 5%, nearly identical to the implied reduction from IDSC competition. Other interventions produce estimates of similar or larger magnitudes. Gardner et al. (2009) project that universal

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<sup>48</sup>The motivating model also predicted districts anticipating enrollment gains would reduce effort, which is costly in the model. However, the near-zero VA effect among high-achieving districts provides little evidence of this response, suggesting that improvements among threatened districts drive the overall positive effect.

<sup>49</sup>Figure A6 plots the relationship between exposure and ex-ante achievement. Mean pre-policy achievement is 0.147 test-score standard deviations above the mean at the 25<sup>th</sup> percentile of exposure and 0.027 below it at the 75<sup>th</sup> percentile.

<sup>50</sup>The calculation is  $(0.0074 \cdot 1.24)/0.174 \approx 0.053$ , using the linear treatment-response relationship in Equation (5) over the interquartile range of exposure.

after-school program participation could reduce racial achievement gaps by 2–5% among children below the poverty line and 4–12% when participation extends to children below 200% of the poverty line. Similarly, Walters (2018) project that charter-school expansion in Boston could reduce racial achievement gaps by 5% and the math-score gap between Boston and the rest of Massachusetts by 10%.<sup>51</sup>

The few studies of open-enrollment competition provide the closest institutional comparisons. Welsch and Zimmer (2012) find that Wisconsin districts experiencing greater student out-migration subsequently improve academic performance. This pattern is consistent with my finding of larger VA responses among districts facing greater enrollment-loss threats, although their categorical achievement outcomes prevent direct comparisons of effect sizes. In a different setting, Campos and Kearns (2022) find that Los Angeles’s Zones of Choice program raises ELA achievement by 0.16 standard deviations by its sixth year, largely through improvements in school value added. Although substantially larger than my estimate, their effect captures the longer-run impact of an expansion of within-district choice, rather than responses to enrollment-threat exposure spurred by removing barriers to IDSC.

Evidence from voucher and charter-school competition literatures provides additional context for the magnitude of the IDSC VA effect. Adger and Felegi (2024) offer a close comparison in the Indiana setting, finding that schools within five miles of a private school participating in the state’s voucher program increase VA by 0.023, an effect the authors describe as modest. Although larger than my 0.0074 estimate, their effect compares schools with and without a nearby voucher competitor, rather than differences along a continuous exposure measure. Evidence from charter-school competition is unclear. Some studies find positive effects: Gilraine et al. (2021) estimate math-score gains of 0.02–0.05 standard deviations following charter-school entry, while Hoxby (2003) finds fourth-grade reading and math gains of 0.13 and 0.16 standard deviations, respectively, when charter enrollment reaches at least 6% of district enrollment. Meanwhile, other studies find null or negative effects of char-

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<sup>51</sup>These studies concern different populations, outcomes, and interventions, so I interpret their estimates as magnitude benchmarks.

ter competition (Imberman, 2011; Mumma, 2022; Ni, 2009; Winters, 2012). In contrast, my estimates suggest a modestly positive response to IDSC competition, although differences in outcomes and measures of competition limit direct comparisons of effect sizes.

The pooled VA estimate, however, masks substantially greater VA responses in elementary grades. Table 6 shows that a one-standard-deviation increase in exposure raises elementary-grade VA by 0.0156, while the middle-grade estimate is just 0.0023 and statistically insignificant. Within elementary grades, the math estimate of 0.021 exceeds the ELA estimate of 0.0131, although both are statistically significant at the one percent level. Applying the same achievement-gap benchmark exercise from earlier, the elementary-grade VA effect translates to an 8.4 achievement gap reduction.<sup>52</sup> The concentration of first-time transfers in elementary grades, especially kindergarten (Table B3), offers one possible explanation for these larger VA gains. Because transfer students remain in their non-residential districts for more than four years on average, districts may have stronger incentives to improve educational quality at the grades when families first decide whether to transfer.

I next use event-study estimates to examine how VA responses evolve relative to enrollment losses. Figure 10 shows pre-policy coefficients close to zero, followed by an immediate and sustained increase in VA after transfer tuition is eliminated. By contrast, enrollment losses grow gradually over time (Figure 9). This difference in timing is consistent with districts improving educational quality in anticipation of enrollment losses, rather than gains arising entirely from compositional changes after transfers occur. Nevertheless, the elementary-grade VA response continues to grow in magnitude over time, reaching 0.023 by AY 2016 (Figure A4), leaving open the possibility that changes in peer composition or resources also contribute to the VA gains. I investigate these possibilities in the robustness and mechanisms analyses.

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<sup>52</sup>The interquartile difference in unstandardized elementary-grade exposure is 0.297, and its standard deviation is approximately 0.21. The implied differential value-added gain is therefore  $0.0156 \cdot (0.297/0.21) \approx 0.0221$ . Mean ex-ante achievement for districts at the 25<sup>th</sup> and 75<sup>th</sup> percentiles of the exposure distribution is 0.153 and  $-0.109$ , respectively, yielding an initial gap of 0.262. The benchmark is thus  $0.0221/0.262 \approx 0.084$ .

## VI.C Heterogeneous Effects of Enrollment Competition

The average VA effect may mask differences in how districts respond to enrollment threats. Even after transfer tuition is eliminated, the extent districts' respond to enrollment competition may depend on how readily families can exercise choice or how districts internalize enrollment threats. To examine these conditions, I interact  $Post_t \cdot StExp_d$  with  $GRP_d$ , an indicator for districts belonging to a specified subgroup, to the primary specification:

$$Y_{dt} = \beta_1 Post_t \cdot StExp_d + \beta_2 Post_t \times StExp_d \times GRP_d + \lambda Post_t \cdot StNbr_d + \delta_d + \tau_t + \epsilon_{dt}. \quad (8)$$

The coefficient  $\beta_2$  captures differences in exposure effects between subgroups. Subgroup characteristics are measured before tuition elimination or are time-invariant, while all other elements follow the primary specification.<sup>53</sup>

Because non-residential districts are not required to provide transportation, travel remains an important barrier to using IDSC. Districts may therefore face greater enrollment threats when families can drive to neighboring schools, even they face the same level the exposure mechanism. To examine this factor, Panel A of [Table 7](#) splits districts at the median land area, using geographic size as a proxy for travel costs. The VA response to a one-standard-deviation increase in exposure is 0.0044 larger among districts with below-median land area, although the difference is statistically insignificant. Among elementary grades, where the overall VA gains are concentrated, the difference increases to 0.0187 and is statistically significant at the five-percent level ([Table B6](#)).<sup>54</sup> These patterns are consistent with stronger competitive responses when transportation costs are lower, although districts' land area may proxy for access to other choice options, such as charter and private schools.

Hess et al. (2001) argue that small districts are especially vulnerable to enrollment shocks because competitors can capture a larger share of their enrollment. Panel B therefore splits

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<sup>53</sup>Kernel-density plots show similar exposure distributions across each subgroup split, with the exception of rural and urban or suburban districts ([Figure A5](#)).

<sup>54</sup>Middle-grade differences are small and statistically insignificant. These results are available upon request.

districts at the median pre-policy enrollment. The VA response to a one-standard-deviation increase in exposure is 0.0078 larger among smaller districts, although the difference is statistically insignificant. Among elementary grades, however, this difference increases to 0.0269 and is statistically significant at the five-percent level. These stronger responses may reflect not only smaller districts' inability to absorb enrollment losses, but also more limited within-district schooling options, which may make families looking for alternative schools more likely to use IDSC.

IDSC competition may be particularly salient in rural districts, where charter and private school options are less prevalent. However, longer travel distances and fewer nearby public-school alternatives may also limit their participation in IDSC, making the expected difference in competitive responses ambiguous. To examine these competing forces, I classify districts at least ten miles from an NCES CCD-defined urbanized area as rural. Panel C reports a larger VA point estimate for rural districts, but the difference is statistically insignificant even at the elementary-grade level. While inconclusive, the absence of a detectable difference between rural and nonrural districts suggests that the earlier land-area and enrollment results may not be driven solely by differences in access to other school-choice options.

The heterogeneity results provide further evidence consistent with districts responding to competitive incentives, particularly in elementary grades. However, they do not establish whether changes in student composition or resources also contribute to these VA effects, a question I examine in [Section VI.E](#).

## VI.D Robustness

In this subsection I highlight that my results are robust to concerns about model assumptions and alternative exposure definitions and not driven by a few districts.

***Sensitivity to Empirical Model Assumptions:*** As previously discussed, the treatment setting raises concerns about SUTVA and strong parallel trends. I assess the sensitivity

of my results to plausible violations of these assumptions.

In the primary specification, I control for neighboring districts’ average IDSC enrollment-threat exposure to address potential SUTVA violations. Although this measure does not significantly predict enrollment or value-added changes, it may obscure exposure variation across neighbors and miss relevant spillovers.<sup>55</sup> I therefore re-estimate the primary specification with minimum and maximum neighboring exposure alongside average exposure. All three measures are standardized to facilitate interpretation. The value-added effects of own exposure remain statistically significant and similar in magnitude to the primary estimates (Table B7). The coefficients on all three neighboring exposure measures are small and statistically insignificant, providing no detectable evidence of spillovers under these exposure mappings.

Even within achievement terciles, pre-policy differences in student composition and resources could generate differential post-policy trends, making districts unsuitable counterfactuals for one another. Although pre-policy coefficients are close to zero and statistically insignificant (Figure 10), I allow outcome trends to vary with average pre-policy district characteristics,  $\mathbf{X}_d^{pre}$ .<sup>56</sup>

$$Y_{dt} = \beta Post_t \cdot StExp_d + \lambda Post_t \cdot StNbr_d + \sum_{l=-3}^6 \psi_l(\mathbf{X}_d^{pre} \cdot \mathbf{1}\{t - 2010 = l\}) + \delta_d + \tau_t + \epsilon_{dt} \quad (9)$$

I make the same adjustment to the achievement-tercile specification. The value-added estimates remain similar to the primary results (Table B8), reducing concerns that differential trends associated with baseline characteristics drive the results.

I also apply the sensitivity analysis developed by Rambachan and Roth (2023) to assess

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<sup>55</sup>A district surrounded by both receiving and leaving districts may have average neighboring exposure close to zero even if their spillover effects do not offset one another.

<sup>56</sup>Composition controls include the percentages of students who are Black, White, male, qualify for FRPL, are classified as special education, or receive accommodations on exams. Revenue controls include total revenue per student, as well as federal, state, and local revenue per student. Each variable is averaged within districts over the pre-policy 2007 to 2009 AYs.

how large a violation of parallel trends the estimates are robust to. Under their relative-magnitude restriction,  $\bar{M} = 1$  permits post-policy deviations from parallel trends to be as large as the largest observed pre-policy deviation. The overall value-added estimates are not robust to any parallel-trends violation (Figure A7). However, this aggregate result masks substantial differences by subject: the math estimate remains robust up to  $\bar{M} = 1$ , whereas the ELA estimate is not robust for any  $\bar{M} > 0$ . Regardless, some concerns about sensitivity to parallel-trends violations remain.

Concurrent education reforms could threaten strong parallel trends if they disproportionately affected districts with greater IDSC enrollment-threat exposure. Indiana introduced the Teacher Evaluations and Licensing Act and the A–F school grading system in the 2011 AY, both targeting lower-achieving districts.<sup>57</sup> Two features reduce this concern. First, the exposure-related increase in value added appears in the 2010 AY, before either reform, and event-study estimates show no discernible change in 2011 or 2012 (Figure 10). Second, achievement-tercile-by-year fixed effects absorb policy shocks shared by districts within the same ex-ante achievement group.

The Indiana Choice Scholarship Program (ICSP), a private-school voucher program introduced in 2012 and expanded in 2014, presents another concurrent-policy concern. Adger and Felegi (2024) find modest quality gains among public schools exposed to private-school competition beginning in the 2014 AY, with larger gains at schools with higher FRPL shares. The estimated IDSC effect could therefore partly reflect ICSP competitive pressure. However, the exposure-related increase in value added appears in the 2010 AY, with no discernible change after 2014 (Figure 10). Moreover, leaving and receiving districts are exposed to similar counts of private schools (Table 3), and estimates remain similar when outcome trends are allowed to vary with baseline FRPL shares (Equation (9)). Together, these patterns

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<sup>57</sup>The Teacher Evaluations and Licensing Act required annual teacher evaluations incorporating student achievement and growth. The A–F grading system assigned schools letter grades based on academic performance, with repeated failing grades exposing schools to state intervention. Neither policy has been found to consistently raise achievement among targeted schools (Murphy & Cole, 2017; Swartzentruber & Kaizar, 2022).

make ICSP unlikely to explain the estimated IDSC effect.

***Alternative Measures of Enrollment Pressure:*** The primary exposure measure makes three choices: it uses the average achievement difference within a market, defines the relevant market as neighboring districts, and weights focal-neighbor achievement differences by pre-policy enrollment. I test whether the value-added results are sensitive to these specifications.

Because households need only one sufficiently attractive alternative to leave their residential district, I use the maximum achievement difference to capture pressure from the strongest nearby competitor. I also use the median achievement difference to reduce the influence of unusually large gaps and capture pressure from a typical neighbor. After standardizing both measures, greater exposure continues to predict statistically significant but modest increases in value added (Table B9).<sup>58</sup> The positive value-added response therefore does not depend on using average achievement gaps, although separate standardization limits comparisons of coefficient magnitudes.

Second, I test whether defining markets by neighboring districts excludes accessible non-bordering alternatives. Evidence from other school-choice settings suggests that financial and non-pecuniary costs limit families' consideration to schools within three to six miles (Dustan & Ngo, 2018; J. Hastings et al., 2009).<sup>59</sup> I therefore recalculate exposure after expanding district boundaries by three or six miles, treating another district as a competitor if any of its elementary or middle schools falls within the expanded boundary.<sup>60</sup> As the boundary expands from three to six miles, the value-added estimates decline in magnitude (Table B10). This pattern is consistent with distance limiting transfer options, as broader markets include more distant competitors that few families may consider. Together with the finding that 92% of post-policy transfer students enroll in bordering districts, this pattern

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<sup>58</sup>The maximum- and median-difference measures have correlations of 0.728 and 0.899 with the original exposure measure, respectively.

<sup>59</sup>These studies examine Mexico City and the Charlotte-Mecklenburg School District, respectively.

<sup>60</sup>This approach follows Figlio and Hart (2014) and Bifulco and Ladd (2006), who measure competitive exposure using proximity to private or charter schools.

supports using neighboring districts to define the relevant educational market.

Finally, shared borders may not capture accessibility when neighboring districts' schools are far from the focal district. For each district-neighbor pair, I calculate the distance between the two closest elementary schools as a proxy for minimum travel distance. I focus on elementary schools because first-time IDSC participation and estimated quality responses are concentrated in elementary grades. I replace pre-policy enrollment weights with inverse-distance weights, emphasizing achievement differences involving nearby competitors. Under this measure, a one-standard-deviation increase in IDSC enrollment-threat exposure raises value added by 0.0116 (Table B11). The larger estimate supports robustness to emphasizing nearby competitors, but separate standardization and different weights prevent attributing the increase to accessibility alone. I nevertheless retain enrollment weights because the distance between the closest elementary schools may not reflect the typical household's travel distance.<sup>61</sup>

Taken together, these checks show that the positive value-added response is not unique to the primary exposure measure. The results are also consistent with tuition removal creating stronger competitive pressure where higher-achieving alternatives are accessible.

***Results Are Not Driven by Any Single District or County:*** To assess whether the results are driven by unusually large responses in particular locations, I re-estimate the model iteratively excluding one district and then all districts within one county. Treatment effects remain consistent across district exclusions (Figure A8) and county exclusions (Table B12), indicating that no single district or county drives the estimated average response.

***Transfer Students Do Not Directly Explain Results:*** Measured value added could change mechanically if transfer students are systematically easier or harder to educate than students who remain. I re-estimate the primary specification after excluding all transfer

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<sup>61</sup>I do not observe students' home addresses. Moreover, households apply to districts rather than individual schools, so calculating expected travel distance would also require knowing which school a household expects to attend if accepted.

students from the value-added calculation. The estimated effect remains similar (Table B13), indicating that transfer students’ own outcomes do not drive the result. This analysis does not rule out selective changes in which residential students remain enrolled or indirect effects through peer composition, district resources, or school practices.

## VI.E Mechanisms

The positive relationship between IDSC enrollment-threat exposure and value added could reflect districts increasing effort or changing operating practices in response to enrollment competition. However, student selection may alter peer composition, while enrollment shifts may change district resources because funding follows students. I first examine these channels and find little evidence that observed composition or measured resources account for the value-added gains. I then examine attendance and disciplinary outcomes as observable margins of district responses to enrollment competition.

***Student Composition and School District Resource Channels:*** Research on K12 peer effects finds that high-achieving and higher-income peers can improve educational attainment and future earnings, whereas larger shares of male or disruptive peers can reduce learning (Carrell et al., 2018; Duflo et al., 2011; Einiö, 2026; Lavy & Schlosser, 2011). Because IDSC participants tend to be higher achieving and from higher-income households (Table 1), their departure could worsen peer environments in districts with greater enrollment losses. Such selection would therefore tend to offset the positive value-added effect in more exposed districts.

I estimate the primary model using student composition measures as outcomes. After tuition elimination, a one-standard-deviation increase in exposure raises the minority share by 0.6 percentage points, or 4% relative to its post-policy mean of 14.94%, and the FRPL share by 0.82 percentage points, or approximately 2% relative to its post-policy mean of 46.24%. Both estimates are statistically significant (Table 8). The higher FRPL share is

consistent with higher-income students being more likely to leave, while the higher minority share does not itself imply a deterioration in peer quality. These changes are modest, and their contribution to the positive value-added effect remains unclear.

To assess whether observed composition changes could explain the value-added gains, I adapt an exercise from Adger and Felegi (2024). I first regress district value added in AY 2007 on district characteristics:

$$VA_{d,2007} = \sigma X_d^{2007} + \epsilon_d \quad (10)$$

where  $X_d^{2007}$  contains district-level characteristics analogous to the student-level controls in the value-added model. Using the estimated coefficients and contemporaneous characteristics, I predict value added in subsequent years and use it as the outcome in the primary dosage model. Predicted value added declines by 0.0016 per unit of exposure (Table B14), about one-fifth the magnitude of the positive main estimate. If the pre-policy relationship remains stable, observed composition changes would therefore modestly offset the value-added gains.

Enrollment losses through IDSC reduce district revenue and could lower instructional spending or teacher compensation, potentially reducing educational attainment (C. K. Jackson et al., 2016). Conversely, declining enrollment could increase remaining students' access to resources fixed in the short term, including textbooks, technology, and attention from teachers or principals (Biasi et al., 2025; Fredriksson et al., 2013). I examine these possibilities using district spending and resource measures as outcomes in the primary dosage model (Table 9).<sup>62</sup> I find no detectable changes in total or instructional spending per student, teacher pay, or student-teacher ratios among more exposed districts. The teacher-pay estimate implies an increase of \$906 per unit of exposure, approximately 1.5% of the average salary of \$58,977, but is imprecise and statistically insignificant. Stable per-pupil spending

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<sup>62</sup>The NCES Common Core of Data report teacher employment and spending at the district level, preventing separate analyses by grade level. Available spending measures are limited, and student-teacher ratios do not capture all fixed resources.

and staffing ratios can coexist with enrollment and total revenue losses, so these results do not imply an absence of fiscal pressure. They provide little evidence that measured resources explain the value-added gains, although unobserved resource changes remain possible.

The timing also weighs against these channels explaining the immediate value-added gains. Value added rises immediately after tuition elimination and remains elevated (Figure 10), while IDSC participation grows gradually (Figure 3). Moreover, observed student composition (Figure A9) and district resources (Figure A10) do not sharply diverge between predicted-receiving and predicted-leaving districts from AY 2009 to AY 2010.

I re-estimate the primary and achievement-tercile models controlling for the observed composition and resource measures examined above. The value-added estimates remain stable across these specifications (Table B15). Because these variables may themselves respond to IDSC enrollment competition, this exercise is not intended as a causal decomposition. Rather, it reinforces the conclusion that observed changes in student composition and measured resources do not account for the main value-added results.

The predicted composition result, absence of detectable resource changes, and timing of the value-added response provide little evidence that observed composition or measured resources explain the positive value-added effect, particularly its immediate appearance.

***Attendance and Discipline as Potential Channels:*** District practices can shape how effectively resources are used and student engagement, with some changes impacting quality immediately (Fryer, 2014).<sup>63</sup> Changes to teacher collaboration, parent communication, and discipline policies can also improve outcomes (Craig & Martin, 2026; Papay et al., 2020; Rogers & Feller, 2018). However, these practices are generally unobservable in the IDOE data, preventing me from identifying which practices more exposed districts adopted.

The IDOE data do allow me to examine attendance and disciplinary outcomes, two ob-

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<sup>63</sup>Fryer (2014) applied practices associated with high-performing charter schools to traditional Houston public schools in a school-level randomized experiment. These practices included increased instructional time, high-dosage tutoring, and “culture camps” designed to establish behaviors conducive to academic success. Math scores in treated schools increased by .19 standard deviations in the first year.

servable margins that could contribute to the value-added response. Greater attendance increases students' exposure to instruction, whereas disciplinary outcomes reflect both student behavior and how districts respond to it. Both margins may respond to district practices but could also reflect student selection, although the preceding analysis reduces concerns about selection on observed characteristics. I therefore estimate the primary model using measures of attendance and discipline as outcomes (Table 10).

A one-standard-deviation increase in IDSC enrollment-threat exposure is associated with a 0.48-day increase in average annual attendance. Applying estimates from Aucejo and Romano (2016) implies gains of approximately 0.0007 standard deviations in math and 0.0006 in ELA.<sup>64</sup> These gains equal approximately 3% and 5% of the corresponding elementary-grade value-added estimates of 0.021 and 0.0131. Because attendance rises gradually (Figure A11) while value added increases immediately, increased attendance may contribute modestly to later gains but is unlikely to explain most of the effect. However, the attendance gains may be reflective that efforts to increase educational quality translate to greater student engagement with their schools.

After transfer tuition is eliminated, greater IDSC enrollment-threat exposure is not associated with detectable changes in suspension or expulsion rates, average suspension length, or the average number of suspensions per student. These results do not establish that disciplinary practices remain unchanged because policy changes could be offset by changes in student composition, but they provide no evidence that reduced exclusionary discipline explains the value-added response.

Taken together, attendance could explain only a small share of the value-added gains, and disciplinary outcomes show no detectable changes. Although the data do not identify the underlying practices, larger gains where household transfer costs are lower and enrollment losses are more costly for districts remain consistent with responses to enrollment

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<sup>64</sup>Using variation in school days before state testing in North Carolina, Aucejo and Romano (2016) estimate that an additional day of school raises math and reading scores by 0.0015 and 0.0013 standard deviations, respectively.

competition.

## VII Conclusion

This paper examines how expanding access to inter-district school choice (IDSC) affects public school districts' educational effectiveness. Exploiting Indiana's elimination of transfer tuition in AY 2010, I measure districts' exposure to enrollment threats using pre-policy achievement differences with neighboring districts. A one-standard-deviation increase in exposure reduces net transfer enrollment by 2.06 percentage points of legal enrollment and raises district VA by 0.0074 test-score standard deviations, with larger gains of 0.0156 in elementary grades. These improvements are equivalent to 5.3% and 8.4% of overall and elementary-grade pre-policy achievement gaps, respectively.

The timing and heterogeneity of VA responses are consistent with districts improving educational quality in response to competitive pressure, while changes in student composition and measured district resources do not appear to explain these gains. However, the available administrative data do not identify which practices districts change in response to enrollment threats. Future research should examine these practices and whether districts target their efforts toward students most likely to transfer.

These findings demonstrate that expanding IDSC access can improve educational quality even for students who remain in their residential districts. Unlike charter-school entry or private-school vouchers, IDSC introduces competition among existing public school districts rather than through new providers or private-school subsidies. This may be particularly relevant to improving educational access in rural communities, where charter and private school options are less prevalent. However, the benefits of IDSC competition may be concentrated among districts facing greater enrollment threats, rather than extending to all districts. Given the limited evidence on competitive responses to IDSC, further research is needed to establish whether similar effects emerge in other policy settings.

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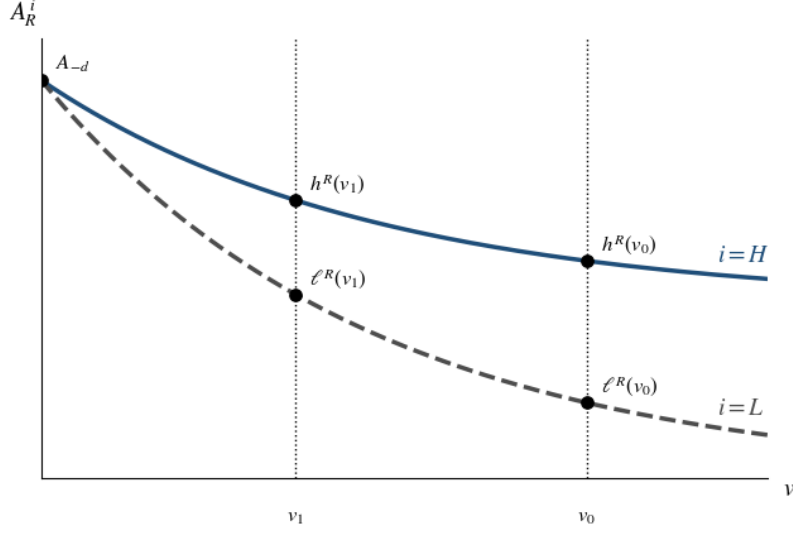
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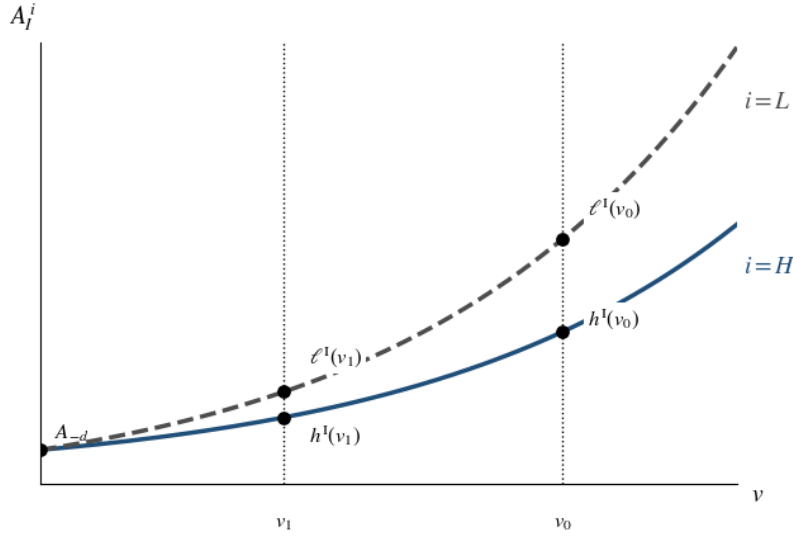
## VIII Figures

Figure 1: Motivating Model - Relationship Between Transfer Costs and Effort Thresholds

(a) To Maintain Residential Students



(b) To Attract Non-Residential Students



**Notes:** Panels (a) and (b) plot the focal-district achievement required to retain resident households and attract nonresident households, respectively (Section II). Solid and dashed curves denote high- and low-type households, respectively. Each marked point's height gives the achievement cutoff, while its label gives the corresponding effort threshold implied by  $Q_d(e_d)$ . The vertical dotted lines mark transfer costs before ( $v_0$ ) and after ( $v_1$ ) transfer tuition is eliminated, with  $v_1 < v_0$ . When  $v = 0$ , households face no transfer costs and are indifferent between districts with equal achievement, so all curves share the intercept  $A_{-d}$ .

Figure 2: Timeline of HEA-1001 and the Elimination of Transfer Tuition

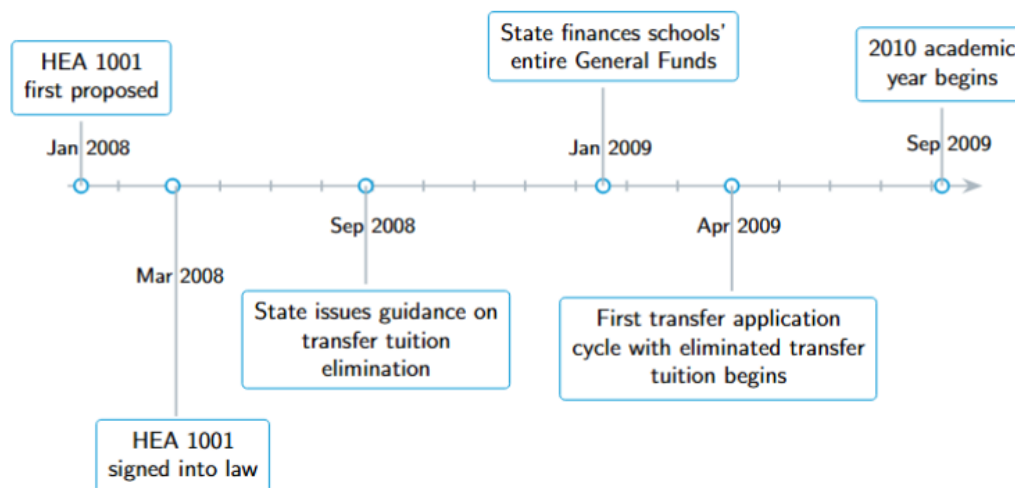
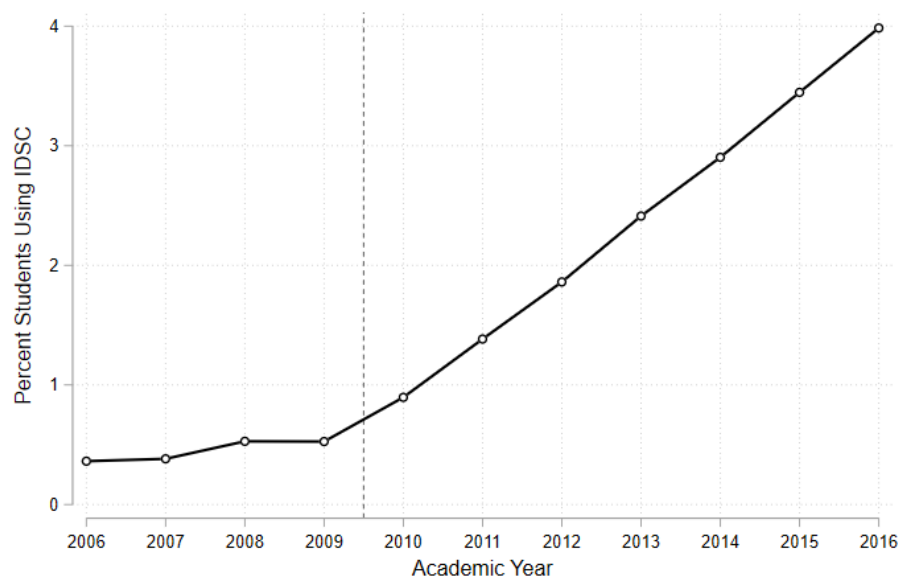
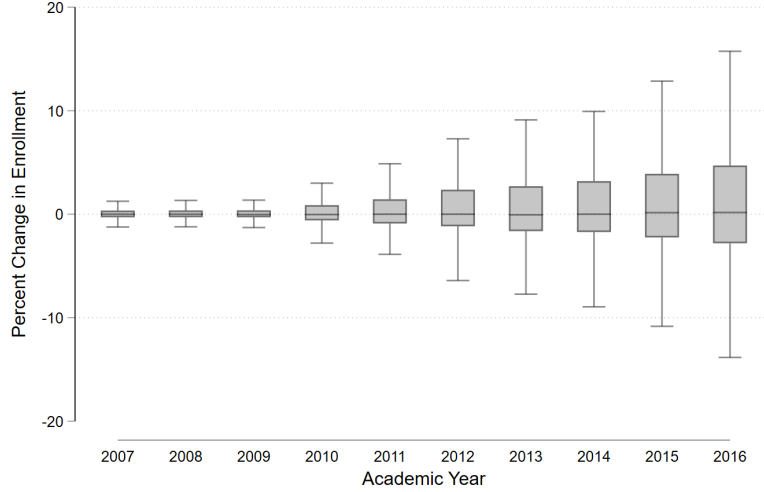


Figure 3: K-8 Student Utilization of Inter-District School Choice (IDSC)



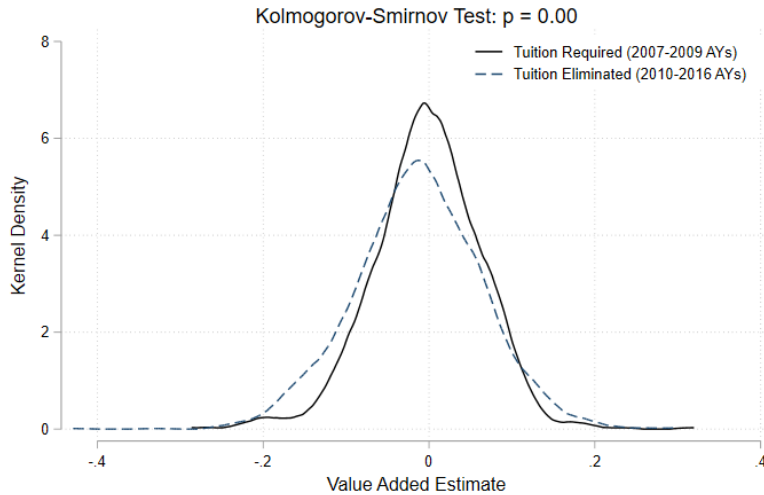
**Notes:** This figure reports the percentage of K-8 students enrolled in a school outside their district of legal residence in each academic year. The sample excludes students attending private or charter schools. The dashed vertical line marks the elimination of transfer tuition beginning in the 2010 academic year. Enrollment and district-of-residence data come from the IDOE-CREO dataset.

Figure 4: Distribution of District Enrollment Changes Due to Net Transfer Students



**Notes:** This figure shows the distribution of percentage enrollment changes due to net transfers across districts in each academic year. The measure equals incoming minus outgoing transfer students, divided by the number of K–8 students legally residing in the district and multiplied by 100. For presentation, the plots omit observations above  $Q_3 + 1.5(IQR)$  or below  $Q_1 - 1.5(IQR)$ , where  $Q_1$  and  $Q_3$  are the first and third quartiles and  $IQR = Q_3 - Q_1$ . This excludes 8–15 districts per academic year. Enrollment and district-of-residence data come from the IDOE-CREO dataset.

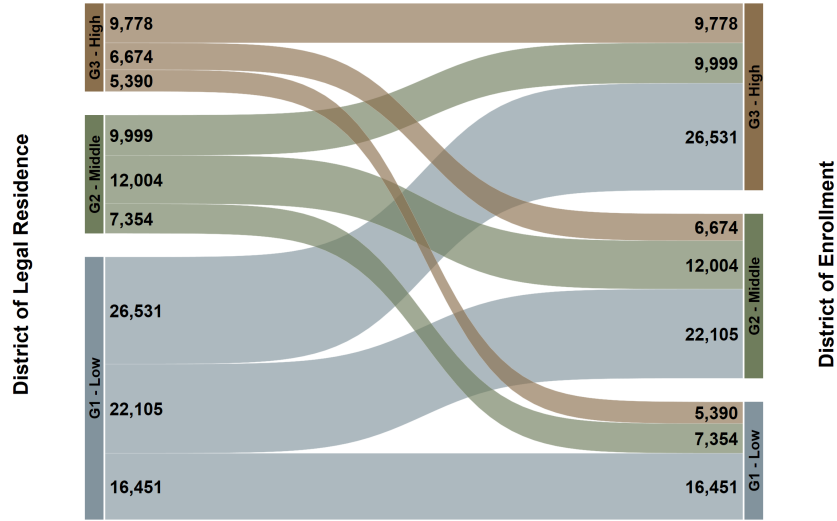
Figure 5: Distribution of District Value Added Before and After Elimination of Transfer Tuition



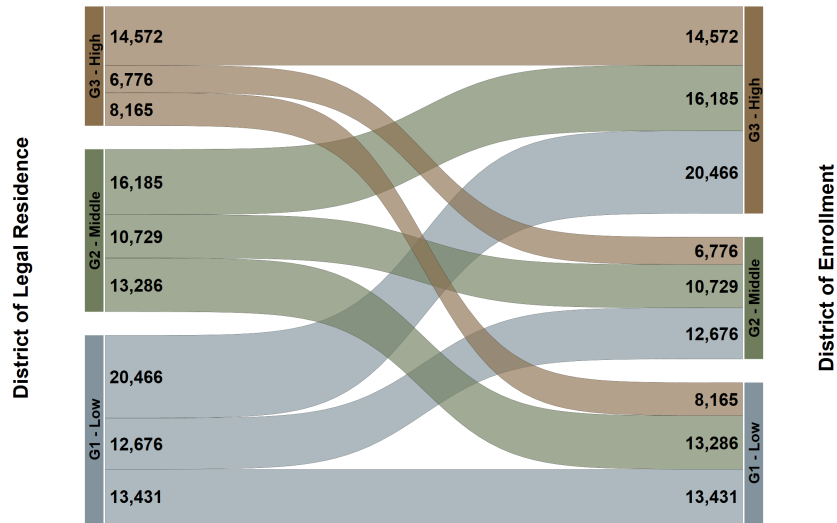
**Notes:** This figure plots kernel densities of district-year value-added estimates before (2007–2009 AYs) and after (2010–2016 AYs) transfer tuition was eliminated. Value added is estimated as described in [Section IV.C](#), using test scores and student characteristics from the IDOE-CREO dataset. The reported  $p$ -value is from a two-sample Kolmogorov–Smirnov test of equality between the two distributions.

Figure 6: Student Transfer Flows After Transfer-Tuition Elimination

(a) By Ex-Ante Achievement Terciles

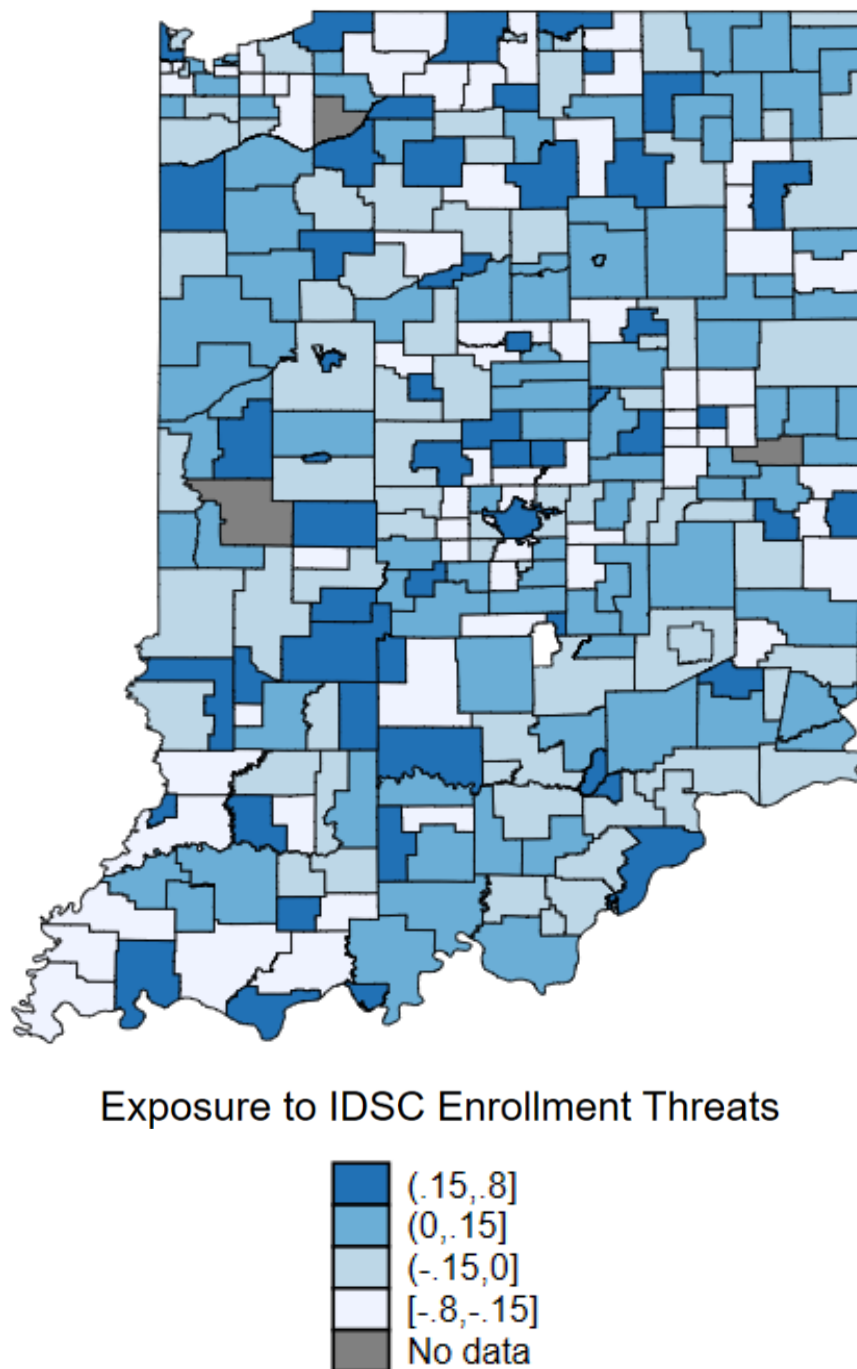


(b) By Ex-Ante Value-Added Terciles



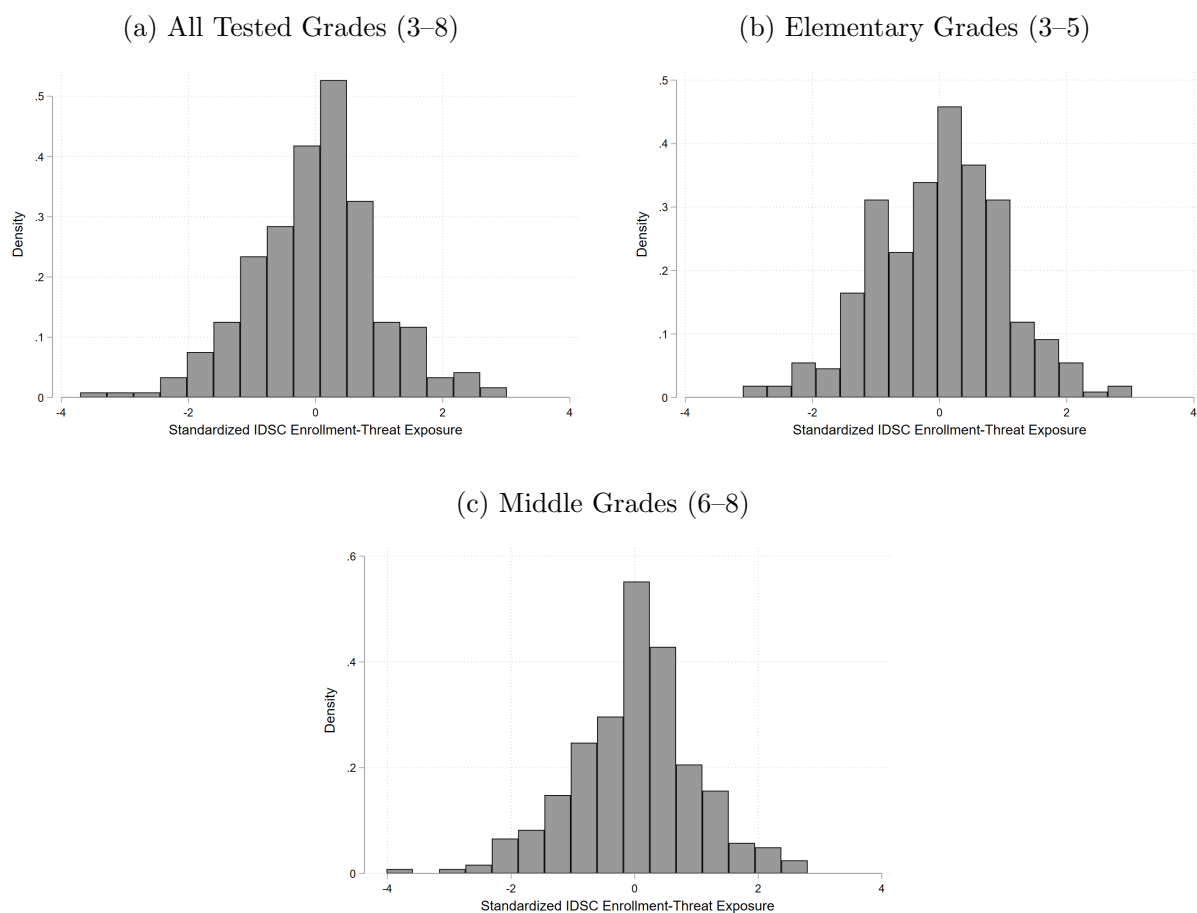
**Notes:** This figure reports student-year KG to 8<sup>th</sup> grade transfer counts for the 2010–2016 AYs. Flows connect students' districts of legal residence (left) to their districts of enrollment (right). Panels (a) and (b) group districts by ex-ante achievement and value-added terciles, respectively. G1, G2, and G3 denote the lowest, middle, and highest terciles. District value added is estimated as described in [Section IV.C](#). Enrollment, district-of-residence, test-score, and student-characteristic data come from the IDOE-CREO dataset.

Figure 7: IDSC Enrollment-Threat Exposure Across Indiana School Districts



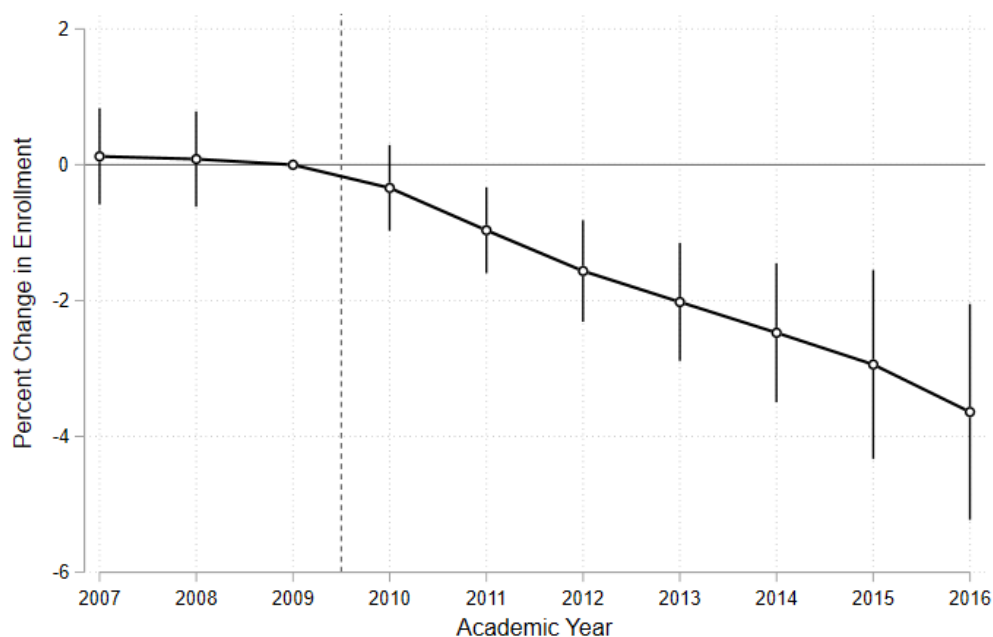
**Notes:** Exposure is constructed using achievement and enrollment data from the 2007–2009 AYs. Darker shading indicates greater exposure, reflecting relatively higher-achieving neighboring districts. The construction and standardization of exposure are described in Section ?? . Gray districts are excluded from the analytic sample because they do not report ISTEP scores in every sample year. Achievement and enrollment data come from the IDOE-CREO dataset.

Figure 8: Distribution of Standardized IDSC Enrollment-Threat Exposure by Grade Group



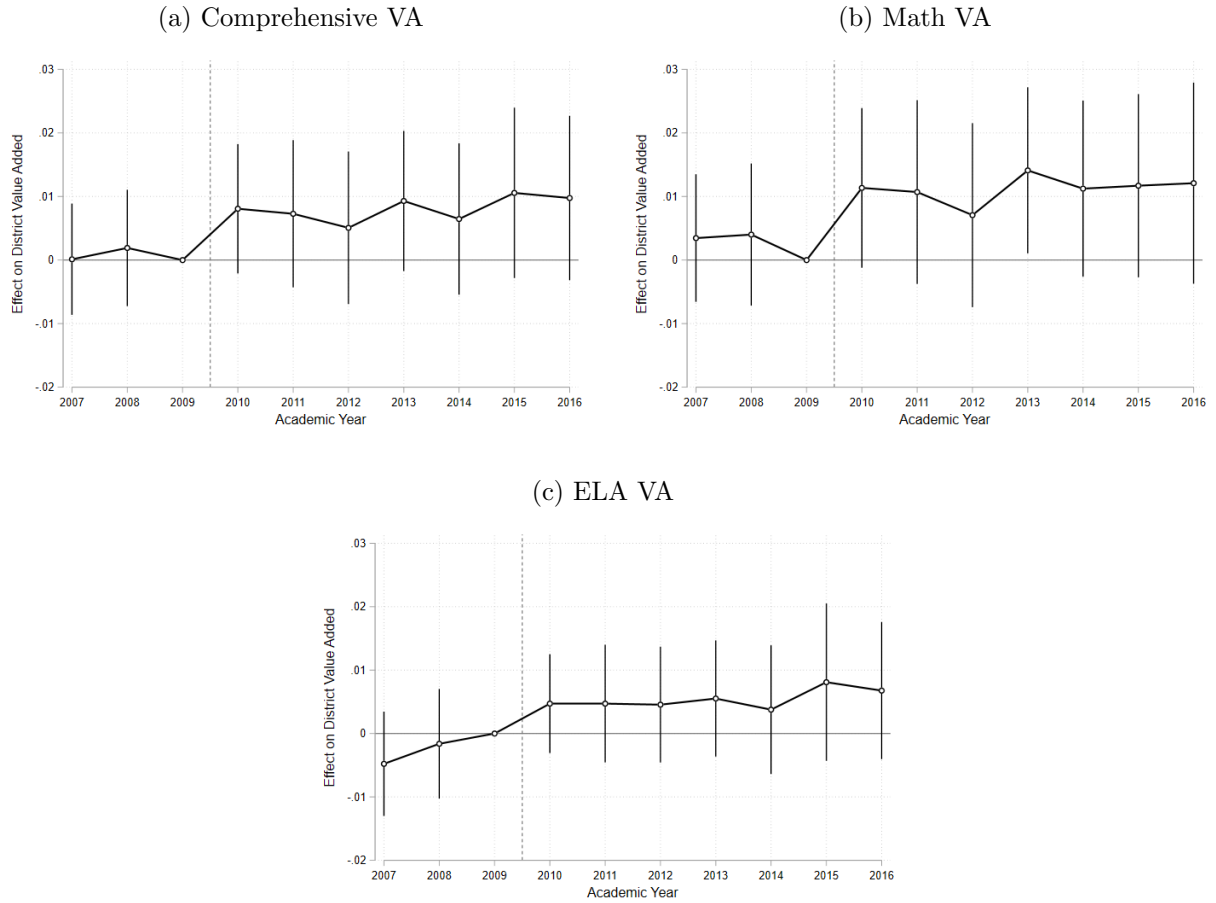
**Notes:** Each observation is a school district. Panels (a), (b), and (c) use exposure constructed from students in grades 3–8, 3–5, and 6–8, respectively. Exposure is standardized separately within each panel. Positive values indicate stronger predicted enrollment losses, while negative values indicate predicted enrollment gains. Section ?? describes exposure construction, and [Table 2](#) reports summary statistics for the unstandardized measures. Achievement and enrollment data come from the IDOE-CREO dataset.

Figure 9: Event-Study Estimates of IDSC Enrollment-Threat Exposure and Enrollment Shifts Due to Net Transfers



**Notes:** This figure plots event-study estimates from Equation 6, with incoming minus outgoing transfers as a percentage of legal enrollment (%NET) as the outcome. Each coefficient measures the differential change relative to the omitted 2009 AY, in percentage points per one-standard-deviation increase in exposure. The dashed vertical line marks transfer-tuition elimination beginning in the 2010 AY. Regressions include district and year fixed effects and control for average neighboring-district exposure. Vertical bars show 95% confidence intervals based on network-robust standard errors following Colella et al. (2023). Section ?? describes exposure construction. Enrollment and achievement data come from the IDOE-CREO dataset.

Figure 10: Event-Study Estimates of IDSC Enrollment-Threat Exposure and District Value Added



**Notes:** This figure plots event-study estimates from Equation 6. Exposure and value added are constructed using Equations 4 and 3, respectively. Each coefficient measures the differential change in value added relative to the omitted 2009 AY, in test-score standard deviations per one-standard-deviation increase in exposure. Regressions include district and year fixed effects and control for average neighboring-district exposure. Vertical bars show 95% confidence intervals based on network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

## IX Tables

Table 1: Transfer and Residential Student Characteristics Post Transfer Tuition Elimination

|                           | (1)<br>Resident<br>Students | (2)<br>Transfer<br>Students | (3)<br>Difference<br>(2)-(1) | (4)<br>Standardized<br>Difference |
|---------------------------|-----------------------------|-----------------------------|------------------------------|-----------------------------------|
| Male                      | 51.44                       | 50.89                       | -0.55**<br>(0.26)            | -0.01                             |
| Minority                  | 27.02                       | 16.84                       | -10.18***<br>(0.23)          | -0.25                             |
| FRPL                      | 48.93                       | 40.96                       | -7.97***<br>(0.29)           | -0.16                             |
| Disciplined               | 7.46                        | 5.68                        | -1.78***<br>(0.08)           | -0.07                             |
| Standardized Test Score   | 0.01                        | 0.08                        | 0.08***<br>(0.01)            | 0.08                              |
| Student-Year Observations | 4,723,946                   | 116,286                     | 4,840,232                    |                                   |

**Notes:** Columns (1) and (2) report average characteristics of resident and transfer public-school students, respectively, after transfer tuition was eliminated (AYs 2010–2016). Column (3) reports differences (transfer minus resident), with standard errors clustered by student in parentheses. Column (4) reports standardized differences, using resident students as the reference group. Observations are student-years, so a student’s classification as resident or transfer may vary across years depending on the district attended. FRPL denotes the percentage of students qualifying for free or reduced-price lunch. Student characteristics, enrollment, and district-of-residence data come from the IDOE-CREO dataset. Standardized test scores are reported for 3,018,600 3<sup>rd</sup> to 8<sup>th</sup> grade student-years over the post-treatment years, of which there are 2,952,976 residential student-years and 65,624 transfer student-years.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 2: IDSC Enrollment-Threat Exposure Distribution

| <i>Grade Levels Used<br/>to Compute Exposure</i> | (1)<br>Mean | (2)<br>Standard Deviation | (3)<br>P25 | (4)<br>Median | (5)<br>P75 |
|--------------------------------------------------|-------------|---------------------------|------------|---------------|------------|
| All — Grades Three to Eight                      | -0.011      | 0.211                     | -0.138     | 0.005         | 0.111      |
| Elementary — Grades Three to Five                | -0.005      | 0.211                     | -0.159     | 0.011         | 0.138      |
| Middle — Grades Six to Eight                     | -0.004      | 0.229                     | -0.139     | 0.016         | 0.121      |
| District Count                                   | 285         | 285                       | 285        | 285           | 285        |

**Notes:** This table reports summary statistics for unstandardized IDSC enrollment-threat exposure. Exposure equals the enrollment-weighted average of neighboring districts' ex-ante overall test-score achievement minus the focal district's achievement (Section ??). Positive values indicate stronger predicted enrollment losses, while negative values indicate predicted enrollment gains. Each observation is a school district, and exposure is fixed over time. Achievement and enrollment data come from the 2007–2009 AYs of the IDOE-CREO dataset.

Table 3: Pre-Policy District Characteristics by IDSC  
Enrollment-Threat Exposure

|                                                               | (1)<br>All<br>Districts | (2)<br>Leaving<br>Districts (EXP $\geq$ 0) | (3)<br>Receiving<br>Districts (EXP $\leq$ 0) | (4)<br>Difference<br>(2)-(3) | (5)<br>Percent<br>Difference |
|---------------------------------------------------------------|-------------------------|--------------------------------------------|----------------------------------------------|------------------------------|------------------------------|
| <i>Panel A: Student Composition and Performance Variables</i> |                         |                                            |                                              |                              |                              |
| Male                                                          | 51.421<br>(1.460)       | 51.611<br>(1.300)                          | 51.225<br>(1.590)                            | 0.386**<br>(0.172)           | 1%                           |
| White                                                         | 86.846<br>(16.292)      | 86.045<br>(18.081)                         | 87.675<br>(14.223)                           | -1.630<br>(1.931)            | -2%                          |
| FRPL                                                          | 37.304<br>(14.717)      | 42.217<br>(13.965)                         | 32.216<br>(13.757)                           | 10.001***<br>(1.643)         | 31%                          |
| Disciplined                                                   | 7.692<br>(4.618)        | 8.534<br>(4.826)                           | 6.819<br>(4.237)                             | 1.715***<br>(0.539)          | 25%                          |
| Special Education                                             | 15.331<br>(3.293)       | 16.058<br>(3.209)                          | 14.578<br>(3.220)                            | 1.480***<br>(0.381)          | 10%                          |
| Standardized Test Score                                       | 0.031<br>(0.207)        | -0.077<br>(0.164)                          | 0.142<br>(0.187)                             | -0.219***<br>(0.021)         | NA                           |
| Test-Score Value Added                                        | -0.002<br>(0.046)       | -0.019<br>(0.041)                          | 0.015<br>(0.046)                             | -0.034***<br>(0.005)         | NA                           |
| <i>Panel B: Resource Variables</i>                            |                         |                                            |                                              |                              |                              |
| Revenue Per Student                                           | 10,617<br>(1,351)       | 10,727<br>(1,303)                          | 10,504<br>(1,395)                            | 223<br>(160)                 | 2%                           |
| Student Teacher Ratio                                         | 17<br>(2)               | 17<br>(2)                                  | 18<br>(2)                                    | -1***<br>(0)                 | -5%                          |
| Enrolled Students                                             | 2,222<br>(2,740)        | 2,175<br>(3,099)                           | 2,270<br>(2,321)                             | -95<br>(325)                 | -4%                          |
| <i>Panel C: Market-Related Variables</i>                      |                         |                                            |                                              |                              |                              |
| Private & Charter Schools                                     | 2.51<br>(5.18)          | 2.49<br>(5.73)                             | 2.54<br>(4.56)                               | -0.05<br>(0.62)              | -2%                          |
| Square Mileage                                                | 125.49<br>(92.03)       | 128.77<br>(91.78)                          | 122.13<br>(92.49)                            | 6.64<br>(10.92)              | 5%                           |
| Neighboring Districts                                         | 5.46<br>(1.85)          | 5.35<br>(1.81)                             | 5.56<br>(1.89)                               | -0.21<br>(0.22)              | -4%                          |
| District Observations                                         | 285                     | 145                                        | 140                                          | 285                          | 285                          |

**Notes:** This table reports district-level means for student composition, resources, and market characteristics in the 2007–2009 AYs. Columns (2) and (3) report leaving districts (positive exposure, predicted enrollment losses) and receiving districts (negative exposure, predicted enrollment gains), respectively. Exposure construction is described in Section ???. Standard deviations appear in parentheses in columns (1)–(3). Column (4) reports differences between leaving and receiving districts, with standard errors in parentheses. Column (5) expresses these differences as percentages of receiving-district means. FRPL denotes the percentage of students qualifying for free or reduced-price lunch. Student and market characteristics come from the IDOE-CREO dataset; revenue and student-teacher ratios come from the NCES Common Core of Data fiscal and non-fiscal datasets, respectively.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 4: Effect of IDSC Enrollment-Threat Exposure on Enrollment Shifts Due to Net Transfers

|                                            | Pct. Enrollment Change<br>Due to Net Transfers |                               |
|--------------------------------------------|------------------------------------------------|-------------------------------|
|                                            | (1)                                            | (2)                           |
| <b>Model Exposure Variables</b>            |                                                |                               |
| $Post_t \cdot StExp_d$                     | -2.06***<br>(0.41)<br>[1.13]                   | -                             |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                              | -2.38***<br>(0.41)<br>[-1.45] |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                              | -3.48***<br>(1.12)<br>[1.34]  |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                              | -1.55**<br>(0.70)<br>[3.51]   |
| $Post_t \cdot StNbr_d$                     | -0.38<br>(0.26)                                | -0.37<br>(0.26)               |
| <b>District-Year Count</b>                 | 2850                                           | 2850                          |
| District Fixed Effects                     | <i>Yes</i>                                     | <i>Yes</i>                    |
| Year Fixed Effects                         | <i>Yes</i>                                     | <i>No</i>                     |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                                      | <i>Yes</i>                    |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7). The outcome, %NET, equals net transfer enrollment as a percentage of legal enrollment. The district-year sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. Column (1) reports the pooled effect of IDSC enrollment-threat exposure, while column (2) allows effects to vary by districts’ ex-ante achievement terciles. Coefficients measure differential post-policy enrollment changes in percentage points per one-standard-deviation increase in exposure. Both specifications include district fixed effects and control for average neighboring-district exposure. Column (1) includes year fixed effects, while column (2) includes achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each district group’s post-policy mean %NET. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 5: Effect of IDSC Enrollment-Threat Exposure on District Value Added

|                                            | District<br>Value Added          |                                    | District Math<br>Value Added     |                                   | District ELA<br>Value Added       |                                    |
|--------------------------------------------|----------------------------------|------------------------------------|----------------------------------|-----------------------------------|-----------------------------------|------------------------------------|
|                                            | (1)                              | (2)                                | (3)                              | (4)                               | (5)                               | (6)                                |
| <b>Model Exposure Variables</b>            |                                  |                                    |                                  |                                   |                                   |                                    |
| $Post_t \cdot StExp_d$                     | 0.0074**<br>(0.0031)<br>[0.0306] | -                                  | 0.0087**<br>(0.0040)<br>[0.0292] | -                                 | 0.0076***<br>(0.0023)<br>[0.0278] | -                                  |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                | 0.0119***<br>(0.0041)<br>[-0.1781] | -                                | 0.0118**<br>(0.0053)<br>[-0.1763] | -                                 | 0.0085***<br>(0.0033)<br>[-0.1673] |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                | 0.0144*<br>(0.0078)<br>[0.0267]    | -                                | 0.0015<br>(0.0139)<br>[0.0292]    | -                                 | 0.0191***<br>(0.0073)<br>[0.0200]  |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                | 0.0024<br>(0.0060)<br>[0.2431]     | -                                | 0.0081<br>(0.0069)<br>[0.2348]    | -                                 | 0.0045<br>(0.0043)<br>[0.2306]     |
| $Post_t \cdot StNbr_d$                     | -0.0019<br>(0.0033)              | -0.0023<br>(0.0034)                | -0.0006<br>(0.0042)              | -0.0011<br>(0.0041)               | -0.0023<br>(0.0027)               | -0.0022<br>(0.0027)                |
| <b>District-Year Count</b>                 | 2850                             | 2850                               | 2850                             | 2850                              | 2850                              | 2850                               |
| District Fixed Effects                     | <i>Yes</i>                       | <i>Yes</i>                         | <i>Yes</i>                       | <i>Yes</i>                        | <i>Yes</i>                        | <i>Yes</i>                         |
| Year Fixed Effects                         | <i>Yes</i>                       | <i>No</i>                          | <i>Yes</i>                       | <i>No</i>                         | <i>Yes</i>                        | <i>No</i>                          |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                        | <i>Yes</i>                         | <i>No</i>                        | <i>Yes</i>                        | <i>No</i>                         | <i>Yes</i>                         |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7). The sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. Columns (1)–(2), (3)–(4), and (5)–(6) report estimates for overall, math, and ELA district value added, respectively. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in exposure. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each district group’s average post-policy test-score achievement. Value added is calculated using Equation (3), and exposure construction is described in Section ???. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 6: IDSC Enrollment-Threat Exposure on District Value Added by Grade Level

|                                 | <u>All Tested Grades (4-8)</u> |                      |                       | <u>Elementary Grades (4-5)</u> |                       |                       | <u>Middle Grades (6-8)</u> |                    |                    |
|---------------------------------|--------------------------------|----------------------|-----------------------|--------------------------------|-----------------------|-----------------------|----------------------------|--------------------|--------------------|
|                                 | Overall                        | Math                 | ELA                   | Overall                        | Math                  | ELA                   | Overall                    | Math               | ELA                |
|                                 | (1)                            | (2)                  | (3)                   | (4)                            | (5)                   | (6)                   | (7)                        | (8)                | (9)                |
| <b>Model Exposure Variables</b> |                                |                      |                       |                                |                       |                       |                            |                    |                    |
| $Post_t \cdot StExp_d$          | 0.0074**<br>(0.0031)           | 0.0087**<br>(0.0040) | 0.0076***<br>(0.0023) | 0.0156***<br>(0.0044)          | 0.0210***<br>(0.0059) | 0.0131***<br>(0.0033) | 0.0023<br>(0.0054)         | 0.0007<br>(0.0068) | 0.0048<br>(0.0040) |
| $Post_t \cdot StNbr_d$          | -0.0019<br>(0.0033)            | -0.0006<br>(0.0042)  | -0.0023<br>(0.0027)   | -0.0010<br>(0.0045)            | 0.0054<br>(0.0060)    | -0.0070**<br>(0.0032) | 0.0026<br>(0.0046)         | 0.0021<br>(0.0061) | 0.0022<br>(0.0036) |
| <b>District-Year Count</b>      | 2850                           | 2850                 | 2850                  | 2850                           | 2850                  | 2850                  | 2850                       | 2850               | 2850               |

**Notes:** This table reports estimates from the primary continuous difference-in-differences specification in Equation (5). Columns (1)–(3), (4)–(6), and (7)–(9) report estimates when exposure is constructed from all tested, elementary, and middle grade students, respectively. Within each grade group, columns report overall, math, and ELA value added. The sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. All specifications include district and year fixed effects and control for average neighboring-district exposure. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in IDSC enrollment-threat exposure. Parentheses report network-robust standard errors following Colella et al. (2023). Value added is calculated using Equation (3). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

Table 7: Heterogeneous Effects of IDSC Enrollment-Threat Exposure on District Value Added

|                                             | (1)<br>District<br>Value-Added | (2)<br>District Math<br>Value-Added | (3)<br>District ELA<br>Value-Added |
|---------------------------------------------|--------------------------------|-------------------------------------|------------------------------------|
| <i>Panel A: Below Median Square Mileage</i> |                                |                                     |                                    |
| $Post_t \cdot StExp_d$                      | 0.0046<br>(0.0040)             | 0.0041<br>(0.0053)                  | 0.0056<br>(0.0034)                 |
| $Post_t \cdot StExp_d \cdot GRP_d$          | 0.0044<br>(0.0058)             | 0.0073<br>(0.0076)                  | 0.0031<br>(0.0043)                 |
| $Post_t \cdot StNbr_d$                      | -0.0017<br>(0.0033)            | -0.0002<br>(0.0042)                 | -0.0021<br>(0.0027)                |
| <i>Panel B: Below Median Enrollment</i>     |                                |                                     |                                    |
| $Post_t \cdot StExp_d$                      | 0.0050<br>(0.0033)             | 0.0063<br>(0.0038)                  | 0.0034<br>(0.0025)                 |
| $Post_t \cdot StExp_d \cdot GRP_d$          | 0.0078<br>(0.0074)             | 0.0079<br>(0.0103)                  | 0.0134**<br>(0.0054)               |
| $Post_t \cdot StNbr_d$                      | -0.0020<br>(0.0034)            | -0.0007<br>(0.0043)                 | -0.0024<br>(0.0027)                |
| <i>Panel C: Rural School District</i>       |                                |                                     |                                    |
| $Post_t \cdot StExp_d$                      | 0.0057<br>(0.0040)             | 0.0057<br>(0.0051)                  | 0.0057**<br>(0.0026)               |
| $Post_t \cdot StExp_d \cdot GRP_d$          | 0.0044<br>(0.0061)             | 0.0074<br>(0.0080)                  | 0.0046<br>(0.0048)                 |
| $Post_t \cdot StNbr_d$                      | -0.0021<br>(0.0033)            | -0.0009<br>(0.0042)                 | -0.0025<br>(0.0027)                |
| <b>District-Year Count</b>                  | 2850                           | 2850                                | 2850                               |

**Notes:** This table reports estimates from the heterogeneous-effects specification in Equation (8).  $GRP_d$  equals one for districts with below-median acreage in Panel A, below-median enrollment in Panel B, and rural districts in Panel C. Rural districts are located at least ten miles from an urbanized area, as defined by the NCES CCD. The coefficient on  $Post_t \cdot StExp_d$  reports the exposure effect for districts with  $GRP_d = 0$  (the omitted group). The coefficient on  $Post_t \cdot StExp_d \cdot GRP_d$  reports the difference in effects between  $GRP_d = 1$  districts and the omitted group. The total effect for  $GRP_d = 1$  districts is the sum of these two coefficients. Columns (1)–(3) report effects on overall, math, and ELA district value added, respectively. Effects are measured in test-score standard deviations per one-standard-deviation increase in IDSC enrollment-threat exposure during the post-policy period. All specifications include district and year fixed effects and control for average neighboring-district exposure. Parentheses report network-robust standard errors following Colella et al. (2023). Value added is calculated using Equation (3). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 8: Effect of IDSC Enrollment Competition on District Student Composition

|                                 | Percent Male<br>(1)       | Percent Minority<br>(2)      | Percent FRPL<br>(3)          | Percent Special Ed.<br>(4) |
|---------------------------------|---------------------------|------------------------------|------------------------------|----------------------------|
| <b>Model Exposure Variables</b> |                           |                              |                              |                            |
| $Post_t \cdot StExp_d$          | 0.02<br>(0.09)<br>[51.41] | 0.60***<br>(0.19)<br>[14.94] | 0.82***<br>(0.26)<br>[46.24] | -0.14<br>(0.15)<br>[13.13] |
| $Post_t \cdot StNbr_d$          | 0.14*<br>(0.08)           | 0.09<br>(0.18)               | -0.10<br>(0.27)              | -0.00<br>(0.15)            |
| <b>District-Year Count</b>      | 2850                      | 2850                         | 2850                         | 2850                       |

**Notes:** This table reports continuous difference-in-differences estimates for district student composition from Equation (5). The sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. Columns (1)–(4) report the percentages of K–8 students who are male, non-White, eligible for free or reduced-price lunch, and receiving special education services, respectively. Coefficients measure differential post-policy changes in percentage points per one-standard-deviation increase in IDSC enrollment-threat exposure. All specifications include district and year fixed effects and control for average neighboring-district exposure. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each outcome’s post-policy mean across all districts. Enrollment and student-composition data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 9: Effect of IDSC Enrollment Competition on District Spending and Resources

|                                 | Student-Teacher<br>Ratio<br>(1) | Total Expenditure<br>Per Student<br>(2) | Classroom Spending<br>Per Student<br>(3) | Teacher<br>Salary<br>(4)              |
|---------------------------------|---------------------------------|-----------------------------------------|------------------------------------------|---------------------------------------|
| <b>Model Exposure Variables</b> |                                 |                                         |                                          |                                       |
| $Post_t \cdot StExp_d$          | 0.1485<br>(0.1523)<br>[17.36]   | 43.4395<br>(59.4930)<br>[10,743.18]     | 2.2891<br>(24.0275)<br>[5,287.38]        | 906.0330<br>(574.3910)<br>[58,977.00] |
| $Post_t \cdot StNbr_d$          | -0.1414<br>(0.1043)             | -4.8486<br>(68.1840)                    | 4.0803<br>(27.1821)                      | -310.0647<br>(356.7674)               |
| <b>District-Year Count</b>      | 2562                            | 2850                                    | 2850                                     | 2562                                  |

**Notes:** This table reports continuous difference-in-differences estimates for district spending and resources from Equation (5). The sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. Columns (1)–(4) report student-teacher ratios, total expenditure per student, classroom instructional spending per student, and teacher salaries, respectively. Coefficients measure differential post-policy changes per one-standard-deviation increase in IDSC enrollment-threat exposure, expressed in students per teacher in column (1) and dollars in columns (2)–(4). All specifications include district and year fixed effects and control for average neighboring-district exposure. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each outcome’s pre-policy mean across all districts. Enrollment data come from the IDOE-CREO dataset, while expenditure data come from the NCES CCD financial database.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table 10: Effect of IDSC Enrollment Competition on Attendance and Disciplinary Rates

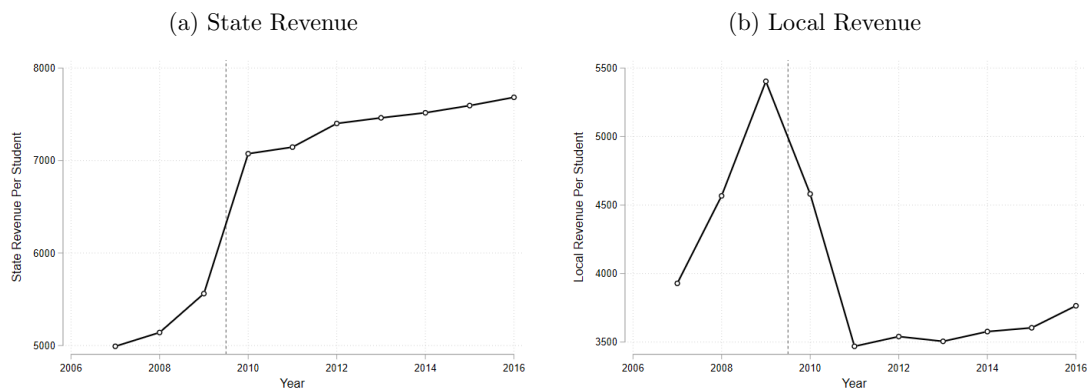
|                                 | Days<br>Attended<br>(1)           | Excused<br>Absences<br>(2)    | Percent Chronically<br>Absent<br>(3) | Percent<br>Disciplined<br>(4) | Suspension<br>Length<br>(5)   | Suspensions Per<br>Student<br>(6) |
|---------------------------------|-----------------------------------|-------------------------------|--------------------------------------|-------------------------------|-------------------------------|-----------------------------------|
| <b>Model Exposure Variables</b> |                                   |                               |                                      |                               |                               |                                   |
| $Post_t \cdot StExp_d$          | 0.4805***<br>(0.1019)<br>[170.40] | -0.0860<br>(0.0551)<br>[4.26] | -0.1610<br>(0.1177)<br>[7.95]        | -0.0449<br>(0.2075)<br>[6.51] | -0.0750<br>(0.1656)<br>[5.79] | 0.0033<br>(0.0321)<br>[1.95]      |
| $Post_t \cdot StNbr_d$          | 0.2324**<br>(0.1122)              | 0.0495<br>(0.0746)            | -0.0290<br>(0.1104)                  | -0.1060<br>(0.1545)           | -0.1301<br>(0.2248)           | -0.0856**<br>(0.0384)             |
| <b>District-Year Count</b>      | 2850                              | 2850                          | 2850                                 | 2850                          | 2846                          | 2846                              |

**Notes:** This table reports continuous difference-in-differences estimates for attendance and disciplinary outcomes from Equation (5). The sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. Columns (1)–(3) report attendance outcomes, while columns (4)–(6) report disciplinary outcomes. Coefficients measure differential post-policy changes in each outcome’s units per one-standard-deviation increase in IDSC enrollment-threat exposure. All specifications include district and year fixed effects and control for average neighboring-district exposure. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each outcome’s pre-policy mean across all districts. Enrollment, attendance, and disciplinary incident data come from the IDOE-CREO dataset.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

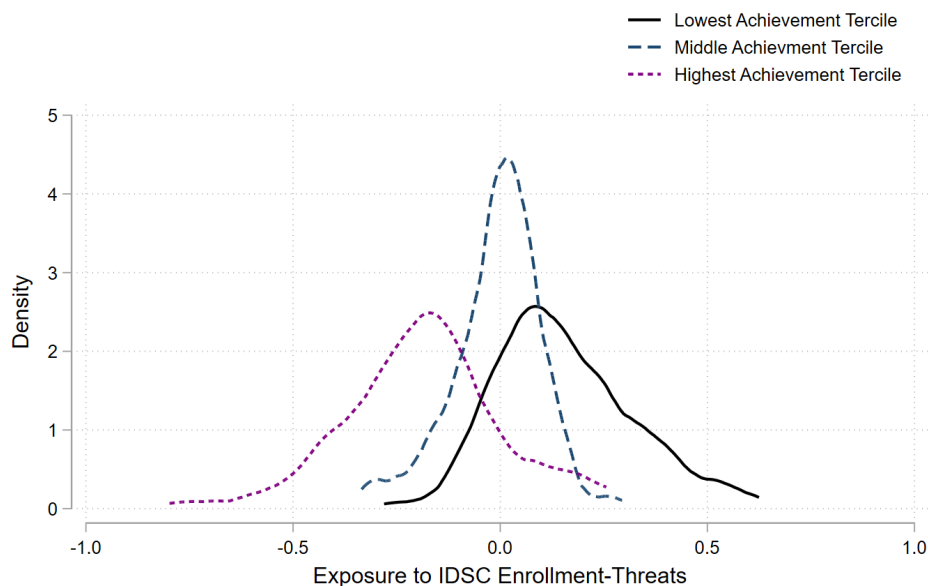
## A Appendix Figures

Figure A1: Indiana State and Local Revenue Per Student



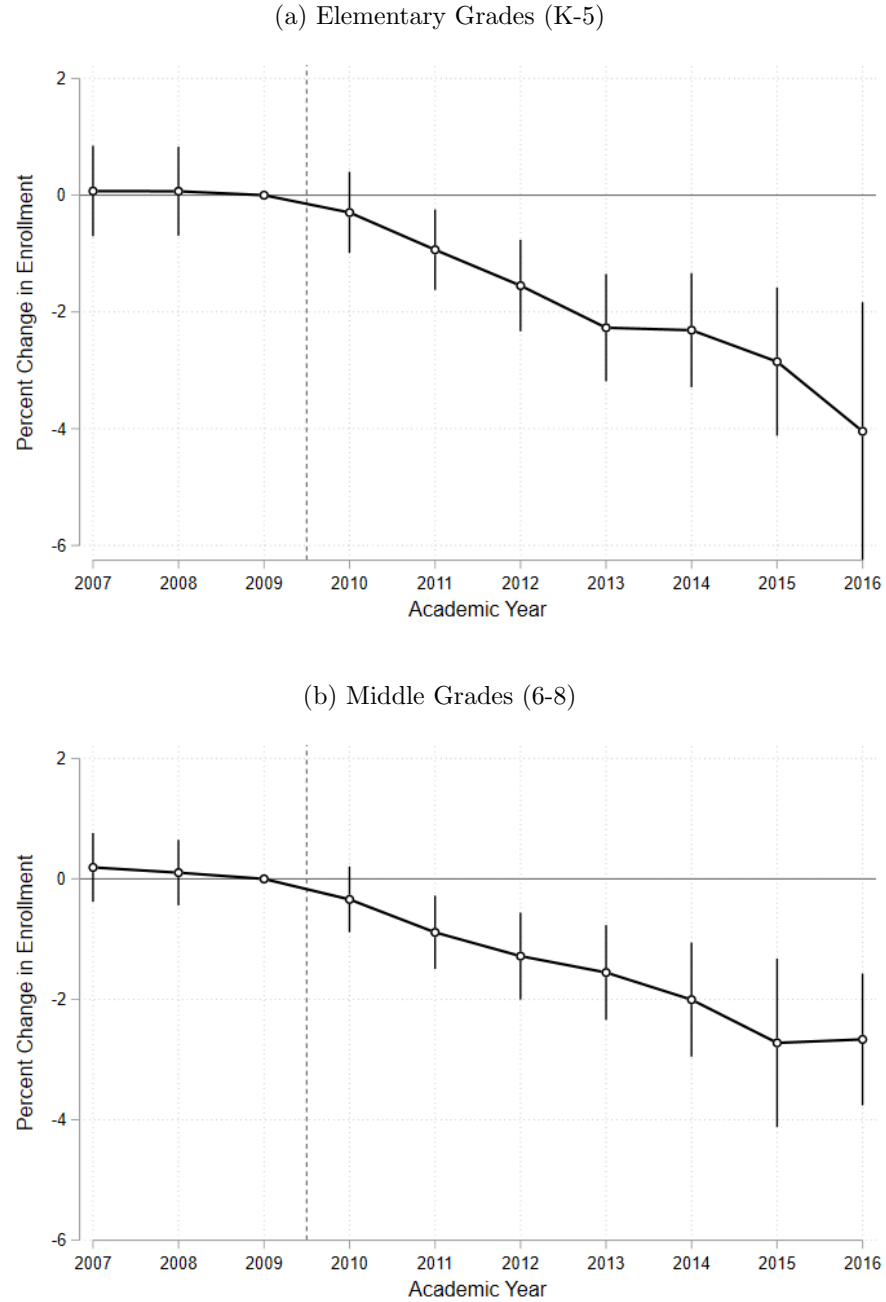
**Notes:** I report state and local revenues per student over the studied time period in panels (a) and (b), respectively. Revenue data comes from the NCES CCD.

Figure A2: Distribution of IDSC Enrollment-Threat Exposure by Ex-Ante Achievement Tercile



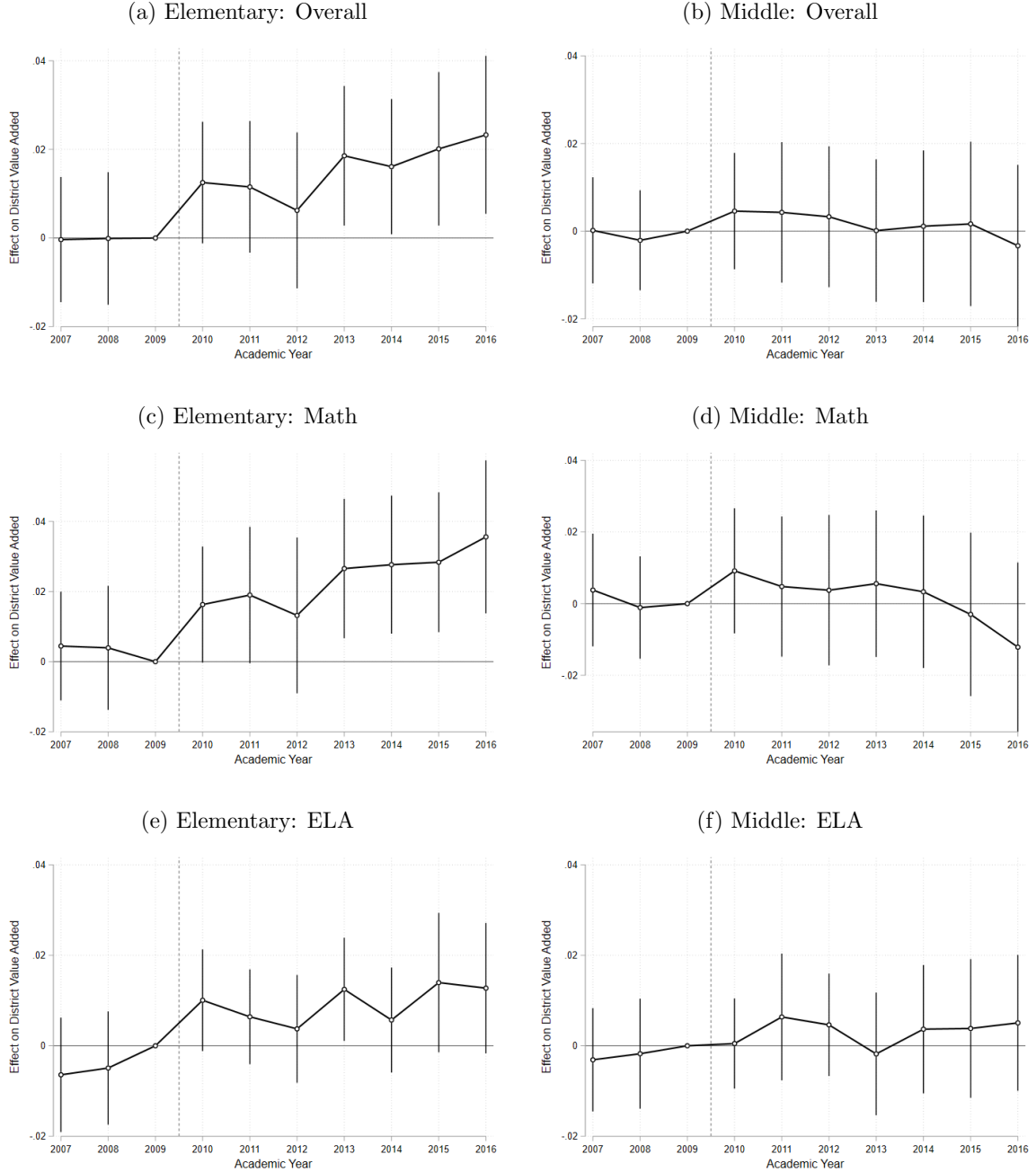
**Notes:** This figure plots kernel densities of unstandardized IDSC enrollment-threat exposure by districts' ex-ante achievement terciles. Each observation is a school district. Exposure and achievement terciles are constructed using K–8 enrollment and test-score achievement in grades 3–8 from the 2007–2009 AYs of the IDOE-CREO dataset. Positive exposure indicates stronger predicted enrollment losses, while negative exposure indicates predicted enrollment gains. Exposure construction is described in Section ??.

Figure A3: Event-Study Estimates of IDSC Enrollment-Threat Exposure on Enrollment by Grade Level



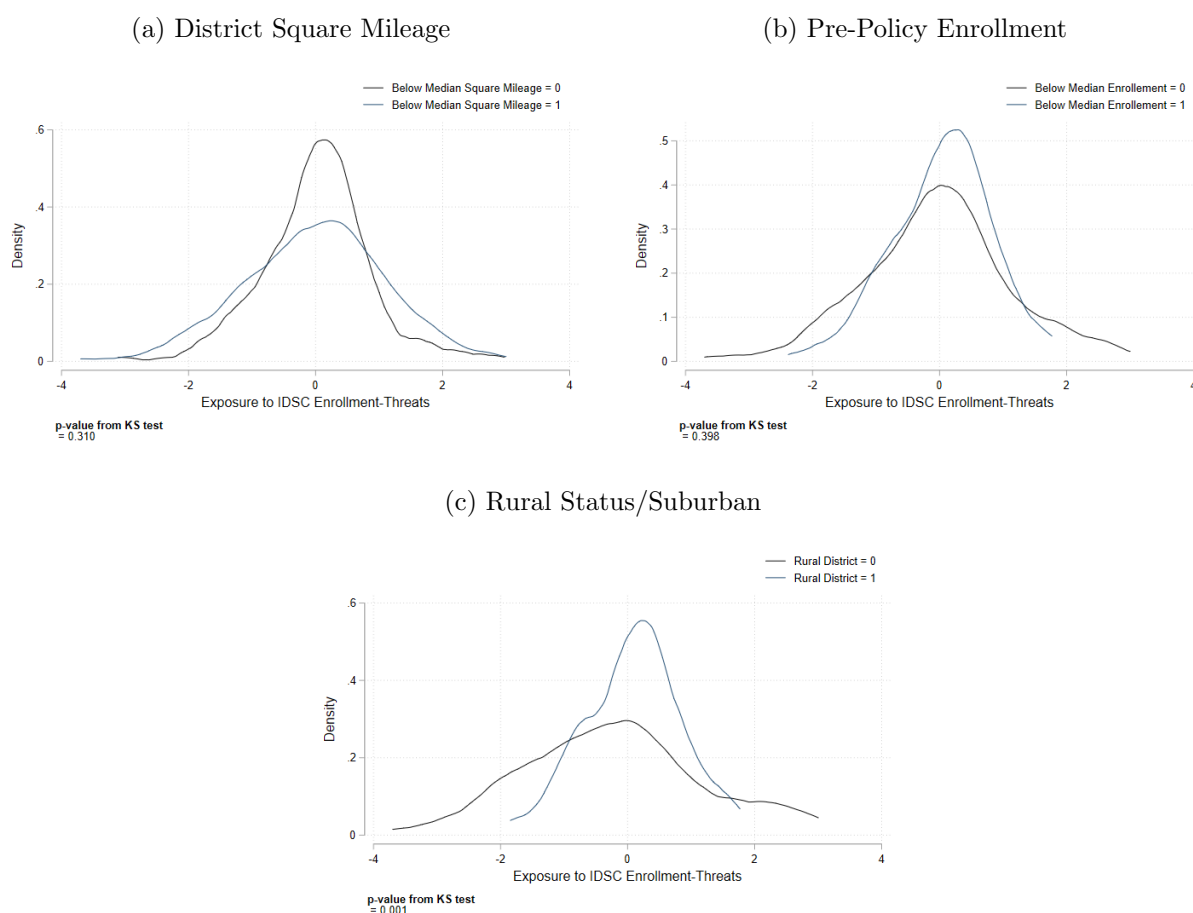
**Notes:** This figure plots event-study estimates from Equation 6 for incoming minus outgoing transfers as a percentage of legal enrollment (%NET). Panel (a) measures enrollment using K-5 students and constructs exposure using students in grades 3-5. Panel (b) uses students in grades 6-8 to construct both measures. Each coefficient measures the differential (%NET) change relative to the omitted 2009 AY per one-standard-deviation increase in exposure. The dashed vertical line marks transfer-tuition elimination beginning in the 2010 AY. Regressions include district and year fixed effects and control for average neighboring-district exposure. Vertical bars show 95% confidence intervals based on network-robust standard errors following Colella et al. (2023).

Figure A4: Event-Study Estimates of Effect of IDSC Enrollment-Threat Exposure on District Value Added by Grade Level



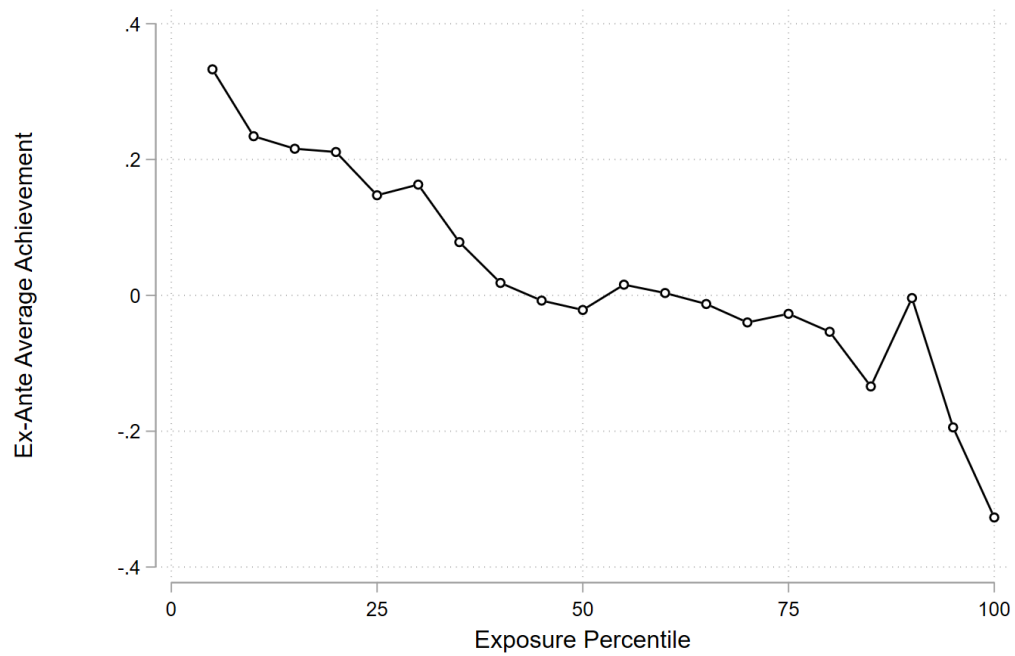
**Notes:** This figure plots event-study estimates for district value added from Equation 6. The left and right columns report elementary- and middle-grade estimates, respectively. Rows report overall, math, and ELA value added from top to bottom. IDSC enrollment-threat exposure is constructed separately by grade group using Equation 4 and standardized within each group. Each coefficient measures the differential change in value added relative to the omitted 2009 AY per one-standard-deviation increase in exposure. The dashed vertical line marks transfer-tuition elimination beginning in the 2010 AY. Regressions include district and year fixed effects and control for average neighboring-district exposure. Vertical bars show 95% confidence intervals based on network-robust standard errors following Colella et al. (2023). Value added is calculated using Equation (3). Achievement and enrollment data come from the IDOE-CREO dataset.

# Figure A5: IDSC Enrollment-Threat Exposure Distributions by District Characteristics



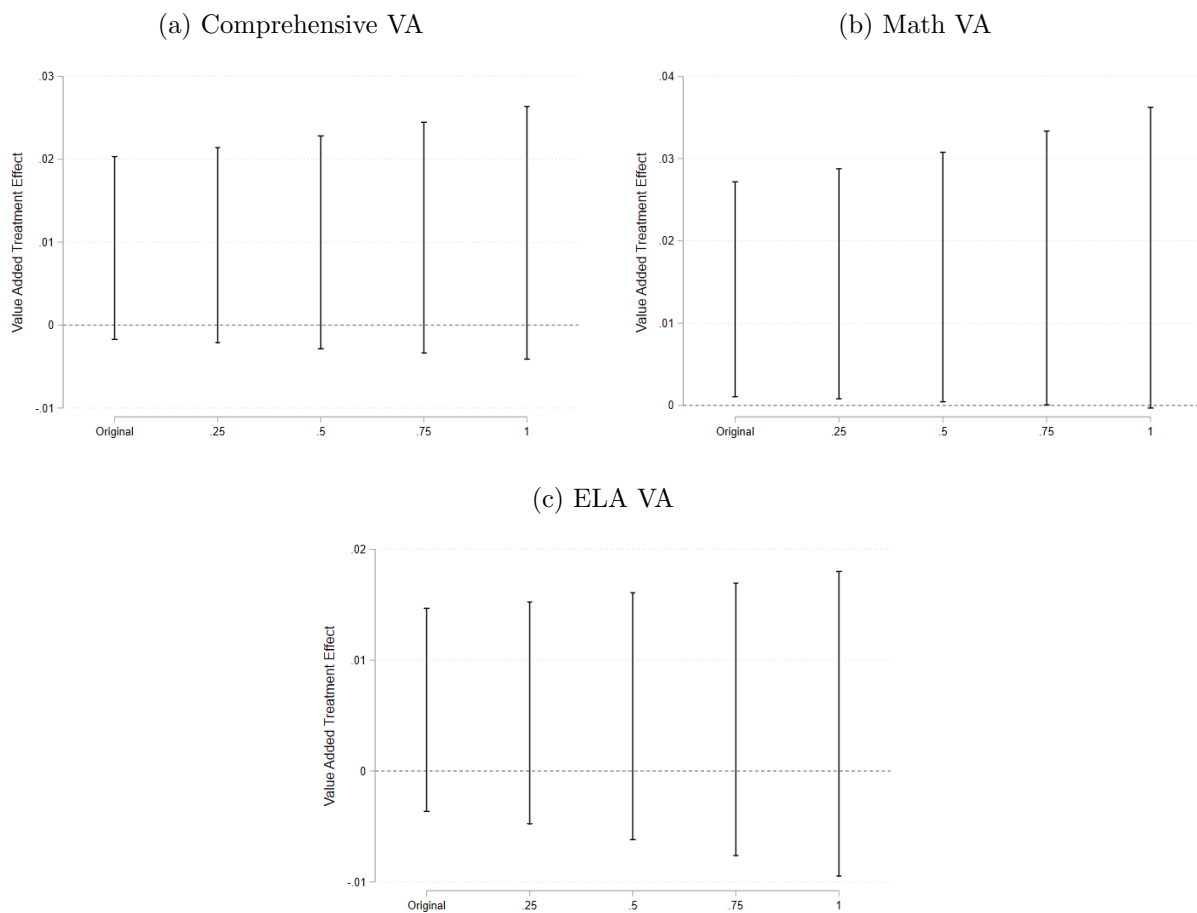
**Notes:** This figure plots IDSC enrollment-threat exposure distributions across district characteristics. Panel (a) splits districts by above- and below-median land area, panel (b) by above- and below-median pre-policy enrollment, and panel (c) by rural versus urban/suburban status. Each panel reports the p-value from a Kolmogorov-Smirnov test of equality of the two exposure distributions. Exposure and subgroup classifications are constructed using the IDOE-CREO dataset.

Figure A6: Relationship Between Districts' Exposure to IDSC Enrollment-Threats and Ex-Ante Achievement



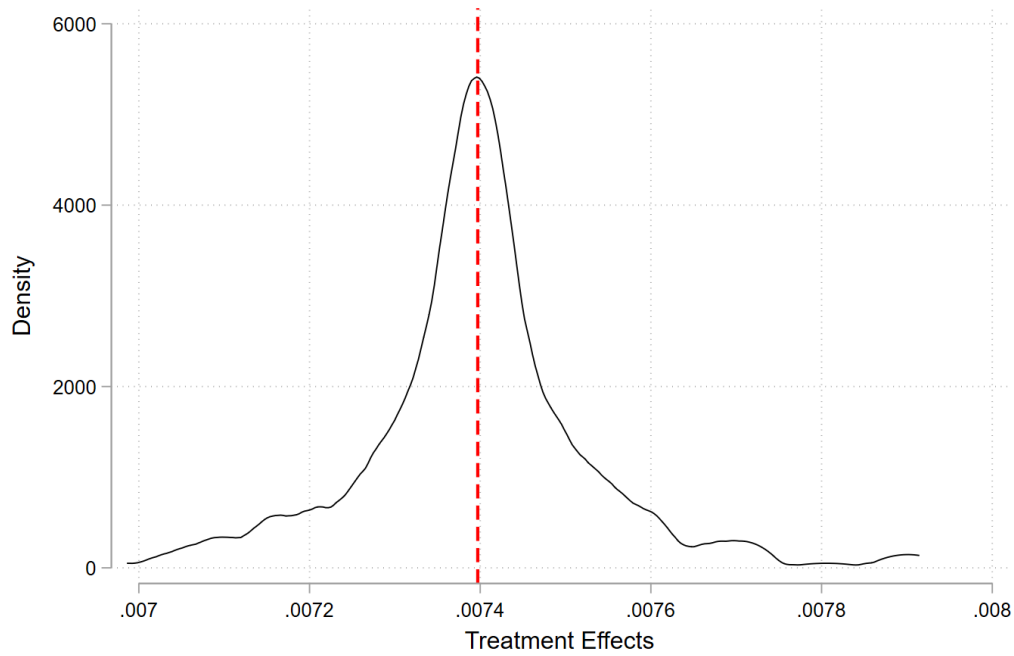
**Notes:** This figure plots school districts' average ex-ante achievement by exposure ventiles. Achievement is measured using standardized comprehensive ISTEP scores, where the exposure mechanism is calculated using students in all grades.

Figure A7: Sensitivity of District Value-Added Estimates to Parallel-Trends Violations



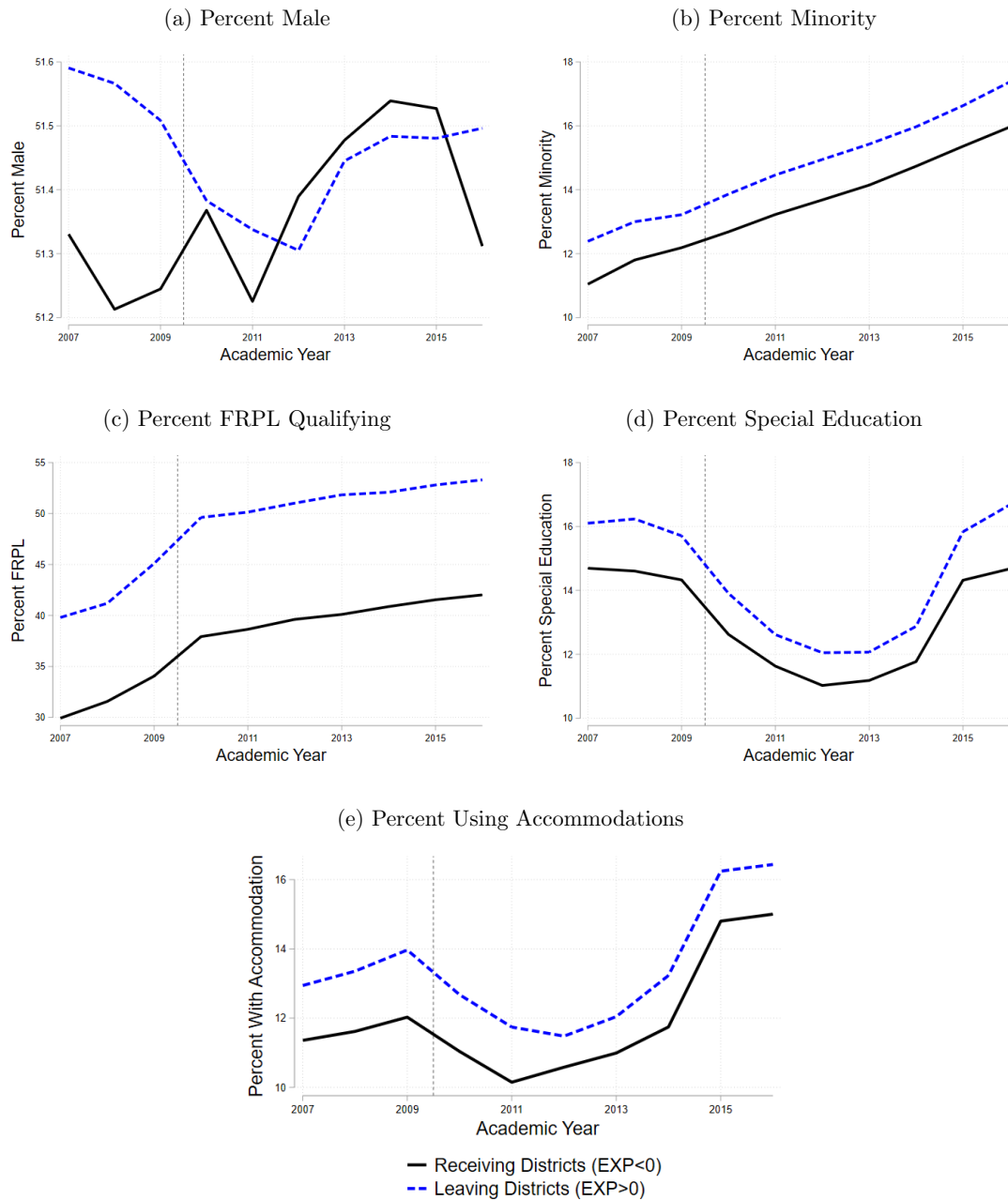
**Notes:** This figure assesses the sensitivity of district value-added estimates to violations of parallel trends using the method of Rambachan and Roth (2023). Panels (a), (b), and (c) report results for overall, math, and ELA value added, respectively. The model includes district and year fixed effects and controls for average neighboring-district exposure. IDSC enrollment-threat exposure is constructed using Equation 4, and value added is calculated using Equation (3). Achievement and enrollment data come from the IDOE-CREO dataset.

Figure A8: Distribution of Leave-One-District-Out Estimates - Outcome of Overall Value Added



**Notes:** This figure plots the kernel density of 285 estimates from the primary continuous difference-in-differences specification in Equation (5), each obtained after excluding a different school district. Each coefficient measures the differential post-policy change in overall district value added, in test-score standard deviations per one-standard-deviation increase in IDSC enrollment-threat exposure. The dashed vertical line marks the full-sample estimate. All specifications include district and year fixed effects and control for average neighboring-district exposure. IDSC enrollment-threat exposure is constructed using Equation 4, and value added is calculated using Equation (3). Enrollment and achievement data come from the IDOE-CREO dataset.

Figure A9: Student Composition Trends by Exposure to IDSC Enrollment Competition



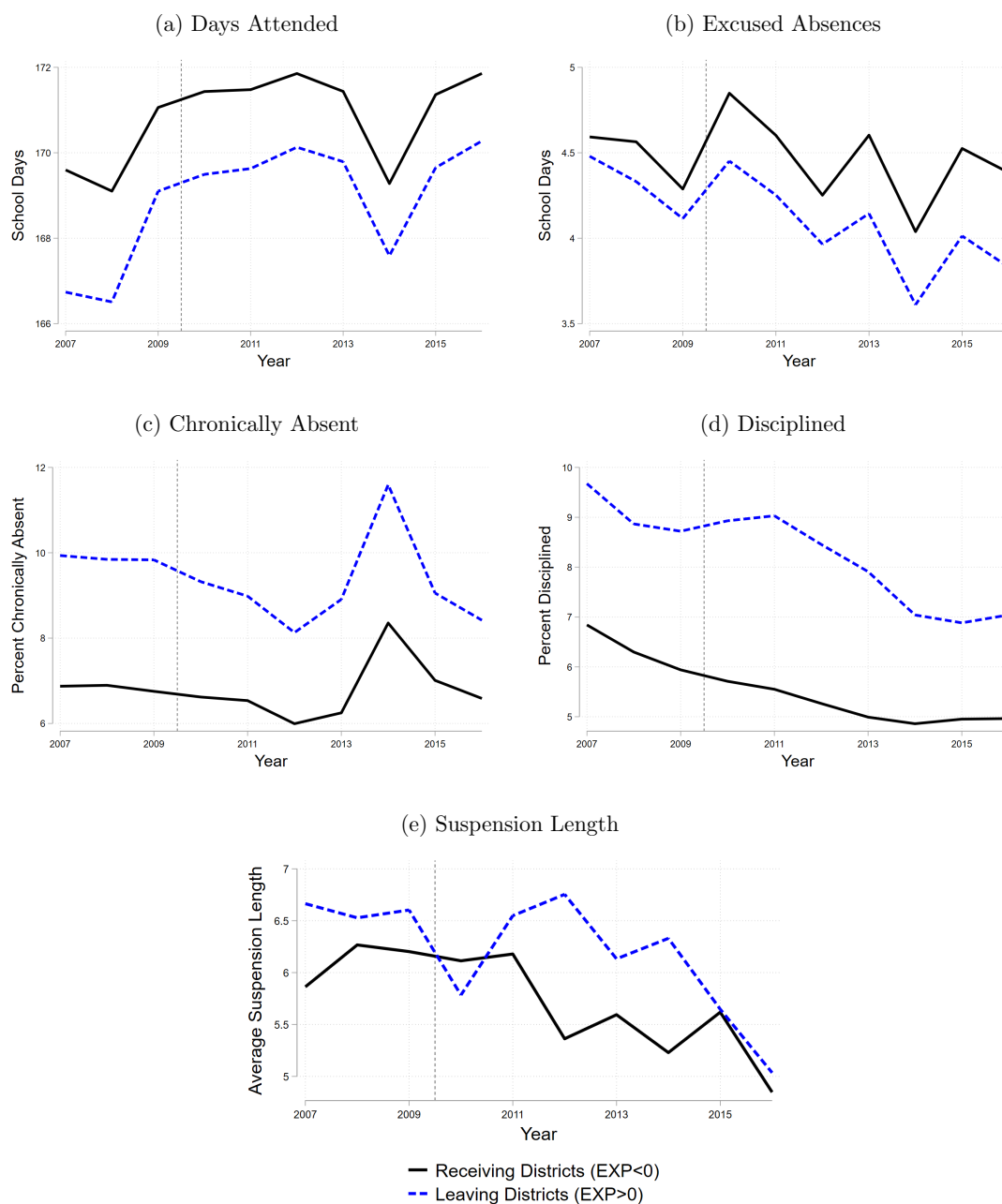
**Notes:** This figure plots student composition trends in the 2007–2016 AYs for receiving districts (negative IDSC enrollment-threat exposure) and leaving districts (positive exposure). Panels (a)–(e) report the percentages of K–8 students who are male, non-White, eligible for free or reduced-price lunch, receiving special education services, and using accommodations, respectively. The dashed line marks transfer-tuition elimination beginning in the 2010 AY. Exposure construction is described in Section ???. Student-characteristic, enrollment, and achievement data come from the IDOE-CREO dataset.

Figure A10: Resource and Spending Trends by Exposure to IDSC Enrollment Competition



**Notes:** This figure plots district spending and resource trends in the 2008–2016 AYs for receiving districts (negative IDSC enrollment-threat exposure) and leaving districts (positive exposure). Panels (a)–(e) report average spending per student, classroom spending per student, teacher salary, faculty salary, and student-teacher ratios, respectively. The dashed vertical line marks transfer-tuition elimination beginning in the 2010 AY. Exposure construction is described in Section ???. Enrollment and achievement data come from the IDOE-CREO dataset. Expenditure and resource data come from the NCES Common Core of Data and include spending and resources for students in grades 9–12.

# Figure A11: Student Behavior Trends by Exposure to IDSC Enrollment Competition



**Notes:** This figure plots district attendance and discipline trends in the 2007–2016 AYs for receiving districts (negative IDSC enrollment-threat exposure) and leaving districts (positive exposure). Panels (a)–(c) report average days attended, average excused absences, and the percentage of students chronically absent, respectively. Panels (d) and (e) report the percentage of students suspended or expelled in an academic year and average suspension length, respectively. The dashed vertical line marks transfer-tuition elimination beginning in the 2010 AY. Exposure construction is described in Section ???. Enrollment and achievement data come from the IDOE-CREO dataset.

## B Appendix Tables

Table B1: Percent Enrollment Change Due to Transfer Students  
Distribution in Post-Policy Years

| <i>Population Used to<br/>Compute Ex-Post %NET</i> | (1)<br>Mean | (2)<br>Standard Deviation | (3)<br>P25 | (4)<br>Median | (5)<br>P75 |
|----------------------------------------------------|-------------|---------------------------|------------|---------------|------------|
| All — Grades KG to Eight                           | 1.134       | 6.583                     | -1.381     | 0.000         | 2.399      |
| Elementary — Grades KG to Five                     | 1.149       | 7.733                     | -1.536     | 0.000         | 2.454      |
| Middle — Grades Six to Eight                       | 1.035       | 6.057                     | -1.275     | 0.000         | 2.275      |
| District Count                                     | 285         | 285                       | 285        | 285           | 285        |

**Notes:** This table reports summary statistics for %NET during the post-policy period (2010–2016 AYs). %NET equals incoming transfer students minus outgoing resident students, divided by the number of students legally residing in the district and multiplied by 100. Rows report statistics for all students in the analytic sample (K–8), elementary grades (K–5), and middle grades (6–8), respectively. The final row reports the number of unique school districts. Enrollment data come from the IDOE-CREO dataset.

Table B2: Effect of IDSC Enrollment-Threat Exposure on Percent Enrollment Shifts Due to Net Transfers by Grade Level

|                                            | Elementary Grades<br>Pct. Enrollment Change<br>Due to Net Transfers |                               | Middle Grades<br>Pct. Enrollment Change<br>Due to Net Transfers |                               |
|--------------------------------------------|---------------------------------------------------------------------|-------------------------------|-----------------------------------------------------------------|-------------------------------|
|                                            | (1)                                                                 | (2)                           | (3)                                                             | (4)                           |
| <b>Model Exposure Variables</b>            |                                                                     |                               |                                                                 |                               |
| $Post_t \cdot StExp_d$                     | -2.08***<br>(0.44)<br>[1.15]                                        | -                             | -1.73***<br>(0.36)<br>[1.03]                                    | -                             |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                                                   | -2.15***<br>(0.43)<br>[-1.50] | -                                                               | -2.75***<br>(0.54)<br>[-1.26] |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                                                   | -2.06**<br>(0.81)<br>[1.44]   | -                                                               | -1.88*<br>(0.98)<br>[1.31]    |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                                                   | -2.03**<br>(0.87)<br>[3.50]   | -                                                               | -0.92*<br>(0.49)<br>[3.05]    |
| $Post_t \cdot StNbr_d$                     | -0.44<br>(0.29)                                                     | -0.44<br>(0.30)               | -0.10<br>(0.27)                                                 | -0.02<br>(0.28)               |
| <b>District-Year Count</b>                 | 2850                                                                | 2850                          | 2850                                                            | 2850                          |
| District Fixed Effects                     | <i>Yes</i>                                                          | <i>Yes</i>                    | <i>Yes</i>                                                      | <i>Yes</i>                    |
| Year Fixed Effects                         | <i>Yes</i>                                                          | <i>No</i>                     | <i>Yes</i>                                                      | <i>No</i>                     |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                                                           | <i>Yes</i>                    | <i>No</i>                                                       | <i>Yes</i>                    |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7). The outcome, %NET, equals net transfer enrollment as a percentage of legal enrollment. The district-year sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. In columns (1)–(2), %NET is constructed using K–5 students, while exposure is constructed using students in grades 3–5. In columns (3)–(4), both measures are constructed using students in grades 6–8. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. Coefficients measure differential post-policy enrollment changes in percentage points per one-standard-deviation increase in exposure. Both specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each district group’s post-policy mean %NET. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B3: Grade Level at First Observed IDSC Participation

|                                                  | (1)<br>Count of<br>Transfer Students | (2)<br>Percent of<br>Transfer Students |
|--------------------------------------------------|--------------------------------------|----------------------------------------|
| <i>Panel A: Pre Tuition Removal (2006-2009)</i>  |                                      |                                        |
| KG                                               | 1,200                                | 14.7%                                  |
| 1st to 5th                                       | 3,496                                | 42.7%                                  |
| 6th to 8th                                       | 3,494                                | 42.7%                                  |
| <b>All Grades</b>                                | 8,190                                | 100%                                   |
| <i>Panel B: Post Tuition Removal (2010-2016)</i> |                                      |                                        |
| KG                                               | 15,418                               | 29.4%                                  |
| 1st to 5th                                       | 22,252                               | 42.4%                                  |
| 6th to 8th                                       | 14,806                               | 28.2%                                  |
| <b>All Grades</b>                                | 52,476                               | 100%                                   |

**Notes:** This table reports student counts and percentages by grade span at first observed IDSC participation. Each transfer student is counted once. Panels A and B report first observed participation before (2006–2009 AYs) and after (2010–2016 AYs) transfer-tuition elimination, respectively. Percentages are calculated within each panel. Enrollment data come from the IDOE-CREO dataset.

Table B4: Effect of IDSC Enrollment-Threat Exposure on District Elementary-Grade Value Added

|                                            | District<br>Value Added           |                                    | District Math<br>Value Added      |                                   | District ELA<br>Value Added       |                                    |
|--------------------------------------------|-----------------------------------|------------------------------------|-----------------------------------|-----------------------------------|-----------------------------------|------------------------------------|
|                                            | (1)                               | (2)                                | (3)                               | (4)                               | (5)                               | (6)                                |
| <b>Model Exposure Variables</b>            |                                   |                                    |                                   |                                   |                                   |                                    |
| $Post_t \cdot StExp_d$                     | 0.0156***<br>(0.0044)<br>[0.0289] | -                                  | 0.0210***<br>(0.0059)<br>[0.0251] | -                                 | 0.0131***<br>(0.0033)<br>[0.0291] | -                                  |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                 | 0.0210***<br>(0.0066)<br>[-0.1734] | -                                 | 0.0218**<br>(0.0097)<br>[-0.1704] | -                                 | 0.0190***<br>(0.0051)<br>[-0.1689] |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                 | 0.0158<br>(0.0160)<br>[0.0231]     | -                                 | 0.0138<br>(0.0180)<br>[0.0233]    | -                                 | 0.0232***<br>(0.0090)<br>[0.0240]  |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                 | 0.0109<br>(0.0079)<br>[0.2371]     | -                                 | 0.0224**<br>(0.0109)<br>[0.2223]  | -                                 | 0.0059<br>(0.0059)<br>[0.2322]     |
| $Post_t \cdot StNbr_d$                     | -0.0010<br>(0.0045)               | -0.0013<br>(0.0046)                | 0.0054<br>(0.0060)                | 0.0054<br>(0.0061)                | -0.0070**<br>(0.0032)             | -0.0077**<br>(0.0033)              |
| <b>District-Year Count</b>                 | 2850                              | 2850                               | 2850                              | 2850                              | 2850                              | 2850                               |
| District Fixed Effects                     | <i>Yes</i>                        | <i>Yes</i>                         | <i>Yes</i>                        | <i>Yes</i>                        | <i>Yes</i>                        | <i>Yes</i>                         |
| Year Fixed Effects                         | <i>Yes</i>                        | <i>No</i>                          | <i>Yes</i>                        | <i>No</i>                         | <i>Yes</i>                        | <i>No</i>                          |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                         | <i>Yes</i>                         | <i>No</i>                         | <i>Yes</i>                        | <i>No</i>                         | <i>Yes</i>                         |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), using value added and exposure constructed from elementary-grade students. Columns (1)–(2), (3)–(4), and (5)–(6) report estimates for overall, math, and ELA district value added, respectively. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in exposure. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each district group’s average post-policy test-score achievement. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B5: IDSC Enrollment Competition on District Middle-Grade Value Added

|                                            | <u>District Value Added</u>    |                                  | <u>District Math Value Added</u> |                                  | <u>District ELA Value Added</u> |                                 |
|--------------------------------------------|--------------------------------|----------------------------------|----------------------------------|----------------------------------|---------------------------------|---------------------------------|
|                                            | (1)                            | (2)                              | (3)                              | (4)                              | (5)                             | (6)                             |
| <b>Model Exposure Variables</b>            |                                |                                  |                                  |                                  |                                 |                                 |
| $Post_t \cdot StExp_d$                     | 0.0023<br>(0.0054)<br>[0.0324] | -                                | 0.0007<br>(0.0068)<br>[0.0335]   | -                                | 0.0048<br>(0.0040)<br>[0.0266]  | -                               |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                              | -0.0008<br>(0.0063)<br>[-0.1963] | -                                | -0.0062<br>(0.0084)<br>[-0.2010] | -                               | 0.0045<br>(0.0055)<br>[-0.1835] |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                              | -0.0086<br>(0.0131)<br>[0.0303]  | -                                | -0.0115<br>(0.0233)<br>[0.0345]  | -                               | -0.0038<br>(0.0100)<br>[0.0235] |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                              | 0.0062<br>(0.0092)<br>[0.2630]   | -                                | 0.0092<br>(0.0111)<br>[0.2668]   | -                               | 0.0068<br>(0.0067)<br>[0.2399]  |
| $Post_t \cdot StNbr_d$                     | 0.0026<br>(0.0046)             | 0.0028<br>(0.0047)               | 0.0021<br>(0.0061)               | 0.0028<br>(0.0063)               | 0.0022<br>(0.0036)              | 0.0023<br>(0.0037)              |
| <b>District-Year Count</b>                 | 2850                           | 2850                             | 2850                             | 2850                             | 2850                            | 2850                            |
| District Fixed Effects                     | <i>Yes</i>                     | <i>Yes</i>                       | <i>Yes</i>                       | <i>Yes</i>                       | <i>Yes</i>                      | <i>Yes</i>                      |
| Year Fixed Effects                         | <i>Yes</i>                     | <i>No</i>                        | <i>Yes</i>                       | <i>No</i>                        | <i>Yes</i>                      | <i>No</i>                       |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                      | <i>Yes</i>                       | <i>No</i>                        | <i>Yes</i>                       | <i>No</i>                       | <i>Yes</i>                      |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), using value added and exposure constructed from middle-grade students. Columns (1)–(2), (3)–(4), and (5)–(6) report estimates for overall, math, and ELA district value added, respectively. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in exposure. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each district group’s average post-policy test-score achievement. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B6: Heterogeneous Effects of IDSC Enrollment-Threat Exposure on Elementary-Grade District Value Added

|                                             | (1)<br>District<br>Value-Added | (2)<br>District Math<br>Value-Added | (3)<br>District ELA<br>Value-Added |
|---------------------------------------------|--------------------------------|-------------------------------------|------------------------------------|
| <i>Panel A: Below Median Square Mileage</i> |                                |                                     |                                    |
| $Post_t \cdot StExp_d$                      | 0.0047<br>(0.0069)             | 0.0071<br>(0.0097)                  | 0.0054<br>(0.0047)                 |
| $Post_t \cdot StExp_d \cdot GRP_d$          | 0.0187**<br>(0.0087)           | 0.0238*<br>(0.0122)                 | 0.0131**<br>(0.0060)               |
| $Post_t \cdot StNbr_d$                      | -0.0005<br>(0.0045)            | 0.0060<br>(0.0059)                  | -0.0067**<br>(0.0032)              |
| <i>Panel B: Below Median Enrollment</i>     |                                |                                     |                                    |
| $Post_t \cdot StExp_d$                      | 0.0068<br>(0.0046)             | 0.0107*<br>(0.0062)                 | 0.0046<br>(0.0031)                 |
| $Post_t \cdot StExp_d \cdot GRP_d$          | 0.0269***<br>(0.0100)          | 0.0314**<br>(0.0140)                | 0.0260***<br>(0.0070)              |
| $Post_t \cdot StNbr_d$                      | -0.0019<br>(0.0046)            | 0.0043<br>(0.0061)                  | -0.0079**<br>(0.0032)              |
| <i>Panel C: Rural School District</i>       |                                |                                     |                                    |
| $Post_t \cdot StExp_d$                      | 0.0129***<br>(0.0048)          | 0.0173**<br>(0.0069)                | 0.0093***<br>(0.0032)              |
| $Post_t \cdot StExp_d \cdot GRP_d$          | 0.0059<br>(0.0085)             | 0.0082<br>(0.0118)                  | 0.0083<br>(0.0064)                 |
| $Post_t \cdot StNbr_d$                      | -0.0012<br>(0.0044)            | 0.0051<br>(0.0058)                  | -0.0074**<br>(0.0032)              |
| <b>District-Year Count</b>                  | 2850                           | 2850                                | 2850                               |

**Notes:** This table reports continuous difference-in-differences estimates from Equation (8), using exposure and value added constructed from elementary-grade students.  $GRP_d$  equals one for districts with below-median acreage in Panel A, below-median enrollment in Panel B, and rural districts in Panel C. Rural districts are located at least ten miles from an urbanized area, as defined by the NCES CCD. The coefficient on  $Post_t \cdot StExp_d$  reports the exposure effect for districts with  $GRP_d = 0$  (the omitted group). The coefficient on  $Post_t \cdot StExp_d \cdot GRP_d$  reports the difference in effects between  $GRP_d = 1$  districts and the omitted group. The total effect for  $GRP_d = 1$  districts is the sum of these two coefficients. Columns (1)–(3) report effects on overall, math, and ELA district value added, respectively. Effects are measured in test-score standard deviations per one-standard-deviation increase in IDSC enrollment-threat exposure during the post-policy period. All specifications include district and year fixed effects and control for average neighboring-district exposure. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B7: Effect of IDSC Enrollment-Threat Exposure on District Value Added with Additional Neighboring District Exposure Controls

|                                 | Comprehensive Value Added<br>(1) | Math Value Added<br>(2) | ELA Value Added<br>(3) |
|---------------------------------|----------------------------------|-------------------------|------------------------|
| <b>Model Exposure Variables</b> |                                  |                         |                        |
| $Post_t \cdot StExp_d$          | 0.0077**<br>(0.0036)             | 0.0105**<br>(0.0047)    | 0.0065***<br>(0.0025)  |
| $Post_t \cdot StNbr_d$          | -0.0028<br>(0.0033)              | -0.0020<br>(0.0043)     | -0.0023<br>(0.0026)    |
| $Post_t \cdot StNbrMax_d$       | -0.0048<br>(0.0415)              | 0.0371<br>(0.0539)      | -0.0395<br>(0.0343)    |
| $Post_t \cdot StNbrMin_d$       | -0.0327<br>(0.0296)              | -0.0496<br>(0.0389)     | -0.0075<br>(0.0248)    |
| <b>District-Year Count</b>      | 2850                             | 2850                    | 2850                   |

**Notes:** This table reports estimates from the primary continuous difference-in-differences specification in Equation (5), adding controls for maximum and minimum neighboring-district exposure.  $StNbr_d$ ,  $StNbrMax_d$ , and  $StNbrMin_d$  denote average, maximum, and minimum IDSC enrollment-threat exposure among neighboring districts, respectively. The maximum and minimum measures are standardized separately, and all three neighboring-exposure measures are interacted with the post-policy indicator. The coefficient on  $Post_t \cdot StExp_d$  measures differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in a district's own exposure. Columns (1)–(3) report effects on comprehensive, math, and ELA district value added, respectively. All specifications include district and year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B8: Effect of IDSC Enrollment-Threat Exposure on District Value Added with Differential Trends by Pre-Policy District Characteristics

|                                            | <u>No Controls</u>   |                       | <u>Revenue Controls</u> |                      | <u>Composition Controls</u> |                      | <u>All Controls</u> |                     |
|--------------------------------------------|----------------------|-----------------------|-------------------------|----------------------|-----------------------------|----------------------|---------------------|---------------------|
|                                            | (1)                  | (2)                   | (3)                     | (4)                  | (5)                         | (6)                  | (7)                 | (8)                 |
| <b>Model Exposure Variables</b>            |                      |                       |                         |                      |                             |                      |                     |                     |
| $Post_t \cdot StExp_d$                     | 0.0074**<br>(0.0031) | -                     | 0.0074*<br>(0.0039)     | -                    | 0.0068*<br>(0.0036)         | -                    | 0.0066*<br>(0.0039) | -                   |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                    | 0.0119***<br>(0.0041) | -                       | 0.0105**<br>(0.0051) | -                           | 0.0112**<br>(0.0048) | -                   | 0.0100*<br>(0.0052) |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                    | 0.0144*<br>(0.0078)   | -                       | 0.0148*<br>(0.0076)  | -                           | 0.0122<br>(0.0078)   | -                   | 0.0130*<br>(0.0077) |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                    | 0.0024<br>(0.0060)    | -                       | 0.0024<br>(0.0061)   | -                           | 0.0022<br>(0.0063)   | -                   | 0.0017<br>(0.0061)  |
| $Post_t \cdot StNbr_d$                     | -0.0019<br>(0.0033)  | -0.0023<br>(0.0034)   | -0.0018<br>(0.0046)     | -0.0013<br>(0.0047)  | -0.0021<br>(0.0033)         | -0.0024<br>(0.0034)  | -0.0014<br>(0.0047) | -0.0009<br>(0.0048) |
| <b>District-Year Count</b>                 | 2850                 | 2850                  | 2850                    | 2850                 | 2850                        | 2850                 | 2850                | 2850                |
| District Fixed Effects                     | <i>Yes</i>           | <i>Yes</i>            | <i>Yes</i>              | <i>Yes</i>           | <i>Yes</i>                  | <i>Yes</i>           | <i>Yes</i>          | <i>Yes</i>          |
| Year Fixed Effects                         | <i>Yes</i>           | <i>No</i>             | <i>Yes</i>              | <i>No</i>            | <i>Yes</i>                  | <i>No</i>            | <i>Yes</i>          | <i>No</i>           |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>            | <i>Yes</i>            | <i>No</i>               | <i>Yes</i>           | <i>No</i>                   | <i>Yes</i>           | <i>No</i>           | <i>Yes</i>          |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), with comprehensive district value added as the outcome. Additional controls interact pre-policy district revenue and student-composition measures with year indicators, allowing outcome trends to vary with these characteristics. Columns (1)–(2) include no additional trend controls, columns (3)–(4) include revenue controls, columns (5)–(6) include student-composition controls, and columns (7)–(8) include both sets. The control variables are listed in Section VI.D. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment, achievement, and student-composition data come from the IDOE-CREO dataset, while revenue data come from the NCES CCD.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B9: Effect of IDSC Enrollment-Threat Exposure on District Value Added Using Alternative Achievement-Difference Summaries

|                                            | Using Average<br>Achievement Differences |                       | Using Largest<br>Achievement Difference |                      | Using Median<br>Achievement Difference |                       |
|--------------------------------------------|------------------------------------------|-----------------------|-----------------------------------------|----------------------|----------------------------------------|-----------------------|
|                                            | (1)                                      | (2)                   | (3)                                     | (4)                  | (5)                                    | (6)                   |
| <b>Model Exposure Variables</b>            |                                          |                       |                                         |                      |                                        |                       |
| $Post_t \cdot StExp_d$                     | 0.0074**<br>(0.0031)                     | -                     | 0.0087**<br>(0.0035)                    | -                    | 0.0118***<br>(0.0031)                  | -                     |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                        | 0.0119***<br>(0.0041) | -                                       | 0.0100**<br>(0.0047) | -                                      | 0.0102**<br>(0.0045)  |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                        | 0.0144*<br>(0.0078)   | -                                       | 0.0039<br>(0.0084)   | -                                      | 0.0343***<br>(0.0103) |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                        | 0.0024<br>(0.0060)    | -                                       | 0.0093<br>(0.0066)   | -                                      | 0.0105*<br>(0.0058)   |
| $Post_t \cdot StNbr_d$                     | -0.0019<br>(0.0033)                      | -0.0023<br>(0.0034)   | 0.0033<br>(0.0034)                      | 0.0033<br>(0.0033)   | 0.0032<br>(0.0033)                     | 0.0036<br>(0.0033)    |
| <b>District-Year Count</b>                 | 2850                                     | 2850                  | 2850                                    | 2850                 | 2850                                   | 2850                  |
| District Fixed Effects                     | <i>Yes</i>                               | <i>Yes</i>            | <i>Yes</i>                              | <i>Yes</i>           | <i>Yes</i>                             | <i>Yes</i>            |
| Year Fixed Effects                         | <i>Yes</i>                               | <i>No</i>             | <i>Yes</i>                              | <i>No</i>            | <i>Yes</i>                             | <i>No</i>             |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                                | <i>Yes</i>            | <i>No</i>                               | <i>Yes</i>           | <i>No</i>                              | <i>Yes</i>            |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), with comprehensive district value added as the outcome. Columns (1)–(2) use the enrollment-weighted average of neighboring districts’ achievement minus the focal district’s achievement (Equation (4)). Columns (3)–(4) and (5)–(6) use the maximum and median differences, respectively. Each exposure measure is standardized separately. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in the corresponding exposure measure. Odd-numbered columns report pooled effects, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. All specifications include district fixed effects and control for average neighboring-district exposure, recalculated using the corresponding exposure measure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B10: Effect of IDSC Enrollment-Threat Exposure on District Value Added Using Alternative Definitions of Neighboring Districts

|                                            | Bordering District<br>Exposure |                       | 3 Mile Border<br>Extension Exposure |                      | 6 Mile Border<br>Extension Exposure |                      |
|--------------------------------------------|--------------------------------|-----------------------|-------------------------------------|----------------------|-------------------------------------|----------------------|
|                                            | (1)                            | (2)                   | (3)                                 | (4)                  | (5)                                 | (6)                  |
| <b>Model Exposure Variables</b>            |                                |                       |                                     |                      |                                     |                      |
| $Post_t \cdot StExp_d$                     | 0.0074**<br>(0.0031)           | -                     | 0.0077*<br>(0.0040)                 | -                    | 0.0055**<br>(0.0024)                | -                    |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                              | 0.0119***<br>(0.0041) | -                                   | 0.0118**<br>(0.0060) | -                                   | 0.0050<br>(0.0041)   |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                              | 0.0144*<br>(0.0078)   | -                                   | 0.0030<br>(0.0107)   | -                                   | 0.0195**<br>(0.0089) |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                              | 0.0024<br>(0.0060)    | -                                   | 0.0055<br>(0.0072)   | -                                   | 0.0042<br>(0.0041)   |
| $Post_t \cdot StNbr_d$                     | -0.0019<br>(0.0033)            | -0.0023<br>(0.0034)   | -0.0042<br>(0.0042)                 | -0.0035<br>(0.0042)  | -0.0025<br>(0.0027)                 | -0.0026<br>(0.0028)  |
| <b>District-Year Count</b>                 | 2850                           | 2850                  | 2850                                | 2850                 | 2850                                | 2850                 |
| District Fixed Effects                     | <i>Yes</i>                     | <i>Yes</i>            | <i>Yes</i>                          | <i>Yes</i>           | <i>Yes</i>                          | <i>Yes</i>           |
| Year Fixed Effects                         | <i>Yes</i>                     | <i>No</i>             | <i>Yes</i>                          | <i>No</i>            | <i>Yes</i>                          | <i>No</i>            |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                      | <i>Yes</i>            | <i>No</i>                           | <i>Yes</i>           | <i>No</i>                           | <i>Yes</i>           |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), with comprehensive district value added as the outcome. Columns (1)–(2) define neighbors as districts sharing a border with the focal district. Columns (3)–(4) and (5)–(6) extend the focal district’s boundary outward by 3 and 6 miles, respectively, and include districts with at least one elementary or middle school within the expanded boundary. Exposure measures otherwise follow Equation (4) and are standardized separately. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in the corresponding exposure measure. Odd-numbered columns report pooled effects, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. All specifications include district fixed effects and control for average neighboring-district exposure under the corresponding geographic definition. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B11: Effect of IDSC Enrollment-Threat Exposure on District Value Added Using Alternative Exposure-Weighting Schemes

|                                            | Using Enrollment<br>Weighted Exposure |                       | Using Distance<br>Weighted Exposure |                       | Using Equally<br>Weighted Exposure |                       |
|--------------------------------------------|---------------------------------------|-----------------------|-------------------------------------|-----------------------|------------------------------------|-----------------------|
|                                            | (1)                                   | (2)                   | (3)                                 | (4)                   | (5)                                | (6)                   |
| <b>Model Exposure Variables</b>            |                                       |                       |                                     |                       |                                    |                       |
| $Post_t \cdot StExp_d$                     | 0.0074**<br>(0.0031)                  | -                     | 0.0116***<br>(0.0030)               | -                     | 0.0110***<br>(0.0031)              | -                     |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                     | 0.0119***<br>(0.0041) | -                                   | 0.0110***<br>(0.0041) | -                                  | 0.0101**<br>(0.0042)  |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                     | 0.0144*<br>(0.0078)   | -                                   | 0.0091<br>(0.0102)    | -                                  | 0.0359***<br>(0.0095) |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                     | 0.0024<br>(0.0060)    | -                                   | 0.0128**<br>(0.0056)  | -                                  | 0.0087<br>(0.0061)    |
| $Post_t \cdot StNbr_d$                     | -0.0019<br>(0.0033)                   | -0.0023<br>(0.0034)   | 0.0049<br>(0.0035)                  | 0.0047<br>(0.0034)    | 0.0032<br>(0.0035)                 | 0.0037<br>(0.0034)    |
| <b>District-Year Count</b>                 | 2850                                  | 2850                  | 2850                                | 2850                  | 2850                               | 2850                  |
| District Fixed Effects                     | <i>Yes</i>                            | <i>Yes</i>            | <i>Yes</i>                          | <i>Yes</i>            | <i>Yes</i>                         | <i>Yes</i>            |
| Year Fixed Effects                         | <i>Yes</i>                            | <i>No</i>             | <i>Yes</i>                          | <i>No</i>             | <i>Yes</i>                         | <i>No</i>             |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                             | <i>Yes</i>            | <i>No</i>                           | <i>Yes</i>            | <i>No</i>                          | <i>Yes</i>            |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), with comprehensive district value added as the outcome. Columns (1)–(2) use the exposure measure in Equation (4), weighting neighbors by their pre-policy enrollment. Columns (3)–(4) assign greater weight to district-neighbor pairs with shorter distances between their closest elementary schools. Columns (5)–(6) weight neighbors equally. Each exposure measure is standardized separately. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in the corresponding exposure measure. Odd-numbered columns report pooled effects, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. All specifications include district fixed effects and control for average neighboring-district exposure, constructed using the corresponding weighting scheme. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B12: County Leave-One-Out Estimates of the Effect of IDSC Enrollment-Threat Exposure on District Value Added

| County Dropped | Estimate | Standard Err. | Observations | County Dropped | Estimate | Standard Err. | Observations |
|----------------|----------|---------------|--------------|----------------|----------|---------------|--------------|
| ADAMS          | 0.007**  | (0.003)       | 2,820        | LAWRENCE       | 0.008**  | (0.003)       | 2,830        |
| ALLEN          | 0.007**  | (0.003)       | 2,810        | MADISON        | 0.008**  | (0.003)       | 2,800        |
| BARTHOLOMEW    | 0.007**  | (0.003)       | 2,830        | MARION         | 0.009*** | (0.003)       | 2,740        |
| BENTON         | 0.007**  | (0.003)       | 2,840        | MARSHALL       | 0.008**  | (0.003)       | 2,800        |
| BLACKFORD      | 0.007**  | (0.003)       | 2,840        | MARTIN         | 0.007**  | (0.003)       | 2,830        |
| BOONE          | 0.007**  | (0.003)       | 2,820        | MIAMI          | 0.007**  | (0.003)       | 2,810        |
| BROWN          | 0.007**  | (0.003)       | 2,840        | MONROE         | 0.007**  | (0.003)       | 2,830        |
| CARROLL        | 0.007**  | (0.003)       | 2,830        | MONTGOMERY     | 0.007**  | (0.003)       | 2,820        |
| CASS           | 0.008**  | (0.003)       | 2,820        | MORGAN         | 0.007**  | (0.003)       | 2,810        |
| CLARK          | 0.007**  | (0.003)       | 2,820        | NEWTON         | 0.008**  | (0.003)       | 2,830        |
| CLAY           | 0.007**  | (0.003)       | 2,840        | NOBLE          | 0.008**  | (0.003)       | 2,820        |
| CLINTON        | 0.008**  | (0.003)       | 2,810        | OHIO           | 0.007**  | (0.003)       | 2,840        |
| CRAWFORD       | 0.007**  | (0.003)       | 2,840        | ORANGE         | 0.007**  | (0.003)       | 2,820        |
| DAVIESS        | 0.008**  | (0.003)       | 2,820        | OWEN           | 0.007**  | (0.003)       | 2,840        |
| DE KALB        | 0.007**  | (0.003)       | 2,820        | PARKE          | 0.008**  | (0.003)       | 2,840        |
| DEARBORN       | 0.007**  | (0.003)       | 2,820        | PERRY          | 0.007**  | (0.003)       | 2,830        |
| DECATUR        | 0.007**  | (0.003)       | 2,830        | PIKE           | 0.007**  | (0.003)       | 2,840        |
| DELAWARE       | 0.008**  | (0.003)       | 2,780        | PORTER         | 0.007**  | (0.003)       | 2,780        |
| DUBOIS         | 0.008**  | (0.003)       | 2,810        | POSEY          | 0.008*** | (0.003)       | 2,830        |
| ELKHART        | 0.007**  | (0.003)       | 2,780        | PULASKI        | 0.007**  | (0.003)       | 2,830        |
| FAYETTE        | 0.007**  | (0.003)       | 2,840        | PUTNAM         | 0.007**  | (0.003)       | 2,810        |
| FLOYD          | 0.007**  | (0.003)       | 2,840        | RANDOLPH       | 0.007**  | (0.003)       | 2,810        |
| FOUNTAIN       | 0.007**  | (0.003)       | 2,820        | RIPLEY         | 0.007**  | (0.003)       | 2,810        |
| FRANKLIN       | 0.007**  | (0.003)       | 2,840        | RUSH           | 0.007**  | (0.003)       | 2,840        |
| FULTON         | 0.007**  | (0.003)       | 2,830        | SCOTT          | 0.007**  | (0.003)       | 2,830        |
| GIBSON         | 0.008**  | (0.003)       | 2,820        | SHELBY         | 0.007**  | (0.003)       | 2,810        |
| GRANT          | 0.007**  | (0.003)       | 2,810        | SPENCER        | 0.007**  | (0.003)       | 2,830        |
| GREENE         | 0.006**  | (0.003)       | 2,800        | ST. JOSEPH     | 0.007**  | (0.003)       | 2,800        |
| HAMILTON       | 0.007**  | (0.003)       | 2,790        | STARKE         | 0.008**  | (0.003)       | 2,820        |
| HANCOCK        | 0.007**  | (0.003)       | 2,810        | STEUBEN        | 0.008**  | (0.003)       | 2,820        |
| HARRISON       | 0.007**  | (0.003)       | 2,820        | SULLIVAN       | 0.008**  | (0.003)       | 2,830        |
| HENDRICKS      | 0.008*** | (0.003)       | 2,790        | SWITZERLAND    | 0.007**  | (0.003)       | 2,840        |
| HENRY          | 0.007**  | (0.003)       | 2,800        | TIPPECANOE     | 0.006**  | (0.003)       | 2,820        |
| HOWARD         | 0.007**  | (0.003)       | 2,800        | TIPTON         | 0.007**  | (0.003)       | 2,830        |
| HUNTINGTON     | 0.007**  | (0.003)       | 2,840        | UNION          | 0.007**  | (0.003)       | 2,840        |
| JACKSON        | 0.007**  | (0.003)       | 2,810        | VANDEBURGH     | 0.007**  | (0.003)       | 2,840        |
| JASPER         | 0.007**  | (0.003)       | 2,830        | VERMILLION     | 0.007**  | (0.003)       | 2,830        |
| JAY            | 0.007**  | (0.003)       | 2,840        | VIGO           | 0.007**  | (0.003)       | 2,840        |
| JEFFERSON      | 0.007**  | (0.003)       | 2,830        | WABASH         | 0.008**  | (0.003)       | 2,820        |
| JENNINGS       | 0.007**  | (0.003)       | 2,840        | WARREN         | 0.007**  | (0.003)       | 2,840        |
| JOHNSON        | 0.007**  | (0.003)       | 2,790        | WARRICK        | 0.008**  | (0.003)       | 2,840        |
| KNOX           | 0.007**  | (0.003)       | 2,820        | WASHINGTON     | 0.007**  | (0.003)       | 2,820        |
| KOSCIUSKO      | 0.008**  | (0.003)       | 2,810        | WAYNE          | 0.007**  | (0.003)       | 2,800        |
| LA PORTE       | 0.008**  | (0.003)       | 2,800        | WELLS          | 0.008**  | (0.003)       | 2,820        |
| LAGRANGE       | 0.007**  | (0.003)       | 2,820        | WHITE          | 0.007**  | (0.003)       | 2,810        |
| LAKE           | 0.008**  | (0.003)       | 2,690        | WHITLEY        | 0.007**  | (0.003)       | 2,830        |

**Notes:** This table reports estimates for comprehensive district value added from the primary continuous difference-in-differences specification in Equation (5). Each estimate excludes all school districts located in the indicated county. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in IDSC enrollment-threat exposure. Adjacent columns report standard errors and the number of district-year observations remaining after each exclusion. All specifications include district and year fixed effects and control for average neighboring-district exposure. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B13: IDSC Enrollment-Threat Exposure on District Value Added Calculated Using Non-Transfer Students

|                                            | District<br>Value Added |                       | District Math<br>Value Added |                      | District ELA<br>Value Added |                       |
|--------------------------------------------|-------------------------|-----------------------|------------------------------|----------------------|-----------------------------|-----------------------|
|                                            | (1)                     | (2)                   | (3)                          | (4)                  | (5)                         | (6)                   |
| <b>Model Exposure Variables</b>            |                         |                       |                              |                      |                             |                       |
| $Post_t \cdot StExp_d$                     | 0.0076**<br>(0.0031)    | -                     | 0.0088**<br>(0.0040)         | -                    | 0.0080***<br>(0.0023)       | -                     |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                       | 0.0123***<br>(0.0041) | -                            | 0.0121**<br>(0.0053) | -                           | 0.0090***<br>(0.0033) |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                       | 0.0149*<br>(0.0076)   | -                            | 0.0020<br>(0.0139)   | -                           | 0.0195***<br>(0.0072) |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                       | 0.0025<br>(0.0059)    | -                            | 0.0079<br>(0.0068)   | -                           | 0.0048<br>(0.0043)    |
| $Post_t \cdot StNbr_d$                     | -0.0021<br>(0.0033)     | -0.0025<br>(0.0034)   | -0.0008<br>(0.0042)          | -0.0012<br>(0.0041)  | -0.0025<br>(0.0027)         | -0.0024<br>(0.0027)   |
| <b>District-Year Count</b>                 | 2850                    | 2850                  | 2850                         | 2850                 | 2850                        | 2850                  |
| District Fixed Effects                     | <i>Yes</i>              | <i>Yes</i>            | <i>Yes</i>                   | <i>Yes</i>           | <i>Yes</i>                  | <i>Yes</i>            |
| Year Fixed Effects                         | <i>Yes</i>              | <i>No</i>             | <i>Yes</i>                   | <i>No</i>            | <i>Yes</i>                  | <i>No</i>             |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>               | <i>Yes</i>            | <i>No</i>                    | <i>Yes</i>           | <i>No</i>                   | <i>Yes</i>            |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), with district value added calculated after excluding all transfer students. Exposure construction and the district-year estimation sample are unchanged. Columns (1)–(2), (3)–(4), and (5)–(6) report effects on comprehensive, math, and ELA value added, respectively. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B14: Effect of IDSC Enrollment Competition on District Predicted Value Added

|                                            | District<br>Value Added |                        | District Math<br>Value Added |                       | District ELA<br>Value Added |                        |
|--------------------------------------------|-------------------------|------------------------|------------------------------|-----------------------|-----------------------------|------------------------|
|                                            | (1)                     | (2)                    | (3)                          | (4)                   | (5)                         | (6)                    |
| <b>Model Exposure Variables</b>            |                         |                        |                              |                       |                             |                        |
| $Post_t \cdot StExp_d$                     | -0.0016***<br>(0.0006)  | -                      | -0.0022***<br>(0.0008)       | -                     | -0.0022***<br>(0.0008)      | -                      |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                       | 0.0001<br>(0.0007)     | -                            | -0.0004<br>(0.0008)   | -                           | -0.0000<br>(0.0012)    |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                       | -0.0049<br>(0.0037)    | -                            | -0.0023*<br>(0.0013)  | -                           | -0.0056<br>(0.0035)    |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                       | -0.0023***<br>(0.0008) | -                            | -0.0037**<br>(0.0015) | -                           | -0.0033***<br>(0.0011) |
| $Post_t \cdot StNbr_d$                     | 0.0013***<br>(0.0005)   | 0.0012***<br>(0.0004)  | 0.0014**<br>(0.0006)         | 0.0013**<br>(0.0006)  | 0.0016**<br>(0.0007)        | 0.0013**<br>(0.0007)   |
| <b>District-Year Count</b>                 | 2850                    | 2850                   | 2850                         | 2850                  | 2850                        | 2850                   |
| District Fixed Effects                     | <i>Yes</i>              | <i>Yes</i>             | <i>Yes</i>                   | <i>Yes</i>            | <i>Yes</i>                  | <i>Yes</i>             |
| Year Fixed Effects                         | <i>Yes</i>              | <i>No</i>              | <i>Yes</i>                   | <i>No</i>             | <i>Yes</i>                  | <i>No</i>              |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>               | <i>Yes</i>             | <i>No</i>                    | <i>Yes</i>            | <i>No</i>                   | <i>Yes</i>             |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7). The outcomes are district value-added measures predicted from changes in student composition using the model estimated in the 2007 AY, described in Section VI.E. Columns (1)–(2), (3)–(4), and (5)–(6) report estimates for predicted overall, math, and ELA district value added, respectively. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. Exposure coefficients measure differential post-policy changes in predicted value added, in test-score standard deviations per one-standard-deviation increase in exposure. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment, achievement, and student-characteristic data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

Table B15: IDSC Enrollment Competition on District Value Added with Time-Varying Composition and Revenue Controls

|                                            | <u>No Controls</u>   |                       | <u>Revenue Controls</u> |                       | <u>Composition Controls</u> |                       | <u>All Controls</u>  |                       |
|--------------------------------------------|----------------------|-----------------------|-------------------------|-----------------------|-----------------------------|-----------------------|----------------------|-----------------------|
|                                            | (1)                  | (2)                   | (3)                     | (4)                   | (5)                         | (6)                   | (7)                  | (8)                   |
| <b>Model Exposure Variables</b>            |                      |                       |                         |                       |                             |                       |                      |                       |
| $Post_t \cdot StExp_d$                     | 0.0074**<br>(0.0031) | -                     | 0.0080***<br>(0.0031)   | -                     | 0.0073**<br>(0.0031)        | -                     | 0.0080**<br>(0.0031) | -                     |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                    | 0.0119***<br>(0.0041) | -                       | 0.0136***<br>(0.0043) | -                           | 0.0115***<br>(0.0041) | -                    | 0.0132***<br>(0.0044) |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                    | 0.0144*<br>(0.0078)   | -                       | 0.0156**<br>(0.0078)  | -                           | 0.0134*<br>(0.0078)   | -                    | 0.0145*<br>(0.0078)   |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                    | 0.0024<br>(0.0060)    | -                       | 0.0021<br>(0.0057)    | -                           | 0.0028<br>(0.0060)    | -                    | 0.0024<br>(0.0058)    |
| $Post_t \cdot StNbr_d$                     | -0.0019<br>(0.0033)  | -0.0023<br>(0.0034)   | -0.0021<br>(0.0034)     | -0.0024<br>(0.0035)   | -0.0021<br>(0.0033)         | -0.0025<br>(0.0033)   | -0.0022<br>(0.0034)  | -0.0025<br>(0.0035)   |
| <b>District-Year Count</b>                 | 2850                 | 2850                  | 2850                    | 2850                  | 2850                        | 2850                  | 2850                 | 2850                  |
| District Fixed Effects                     | <i>Yes</i>           | <i>Yes</i>            | <i>Yes</i>              | <i>Yes</i>            | <i>Yes</i>                  | <i>Yes</i>            | <i>Yes</i>           | <i>Yes</i>            |
| Year Fixed Effects                         | <i>Yes</i>           | <i>No</i>             | <i>Yes</i>              | <i>No</i>             | <i>Yes</i>                  | <i>No</i>             | <i>Yes</i>           | <i>No</i>             |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>            | <i>Yes</i>            | <i>No</i>               | <i>Yes</i>            | <i>No</i>                   | <i>Yes</i>            | <i>No</i>            | <i>Yes</i>            |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7), with comprehensive district value added as the outcome. Columns (1)–(2) include no additional controls. Columns (3)–(4) add time-varying revenue per student, columns (5)–(6) add time-varying student-composition measures, and columns (7)–(8) include both sets. The control variables are listed in Section VI.D. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by districts’ ex-ante achievement terciles. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Enrollment, achievement, and student-composition data come from the IDOE-CREO dataset, while revenue data come from the NCES CCD.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

## C Motivating Model Derivations

## D Motivating Model Derivations

This appendix provides the formal derivations underlying the motivating model. I first derive the achievement and effort thresholds governing household enrollment decisions, then characterize the focal district's optimal effort choice and the effect of eliminating transfer tuition.

### D.A Household Enrollment Thresholds

For the derivations below, normalize the mass of households residing in the focal district to one and let  $x_d$  denote the share of high-income households. Let  $m_{ld}$  and  $m_{hd}$  denote the masses of low- and high-income households, respectively, residing in the outside district. For household type  $i \in \{l, h\}$ , define the consumption-utility loss from transferring as

$$\Delta u_i(v_t) \equiv u(y^i) - u(y^i - v_t) > 0.$$

Because  $u(c)$  is strictly concave and  $y^l < y^h$ , a given transfer cost imposes a larger utility loss on low-income households. To see this, for fixed  $v_t > 0$ ,

$$\frac{\partial}{\partial y} [u(y) - u(y - v_t)] = u'(y) - u'(y - v_t) < 0.$$

Therefore,

$$\Delta u_l(v_t) > \Delta u_h(v_t) > 0. \tag{11}$$

I assume that the focal district can generate achievement over a sufficiently broad range that the household indifference thresholds defined below exist in both periods. Formally, suppose the focal district can generate every achievement level in an interval  $[\underline{A}_d, \bar{A}_d]$  satis-

fying

$$g(\underline{A}_d) \leq g(A_{-d}) - \Delta u_i(v_t) \quad \text{and} \quad g(A_{-d}) + \Delta u_i(v_t) \leq g(\overline{A}_d)$$

for every  $i \in \{l, h\}$  and  $t \in \{0, 1\}$ , where  $A_{-d}$  denotes achievement in the outside district. Continuity of  $g$  ensures existence of each indifference threshold, while strict monotonicity ensures uniqueness.

For a resident household of type  $i$ , let  $A_R^i(v_t, A_{-d})$  denote the focal-district achievement level that makes the household indifferent between remaining in the focal district and transferring to the outside district:

$$U(y^i, A_R^i(v_t, A_{-d})) = U(y^i - v_t, A_{-d}).$$

Using additive separability,

$$g(A_R^i(v_t, A_{-d})) = g(A_{-d}) - \Delta u_i(v_t). \quad (12)$$

A resident household remains in the focal district whenever  $A_d \geq A_R^i(v_t, A_{-d})$ .

Because  $u$  and  $g$  are continuously differentiable and  $g'(A) > 0$ , the implicit function theorem gives

$$\frac{\partial A_R^i(v_t, A_{-d})}{\partial v_t} = -\frac{u'(y^i - v_t)}{g'(A_R^i(v_t, A_{-d}))} < 0. \quad (13)$$

Thus, lowering the transfer cost raises the focal-district achievement necessary to retain either type of resident household:

$$A_R^i(v_1, A_{-d}) > A_R^i(v_0, A_{-d}), \quad v_1 < v_0. \quad (14)$$

For a household of type  $i$  residing in the outside district, let  $A_I^i(v_t, A_{-d})$  denote the focal-district achievement level that makes the household indifferent between remaining in the

outside district and transferring to the focal district:

$$U(y^i - v_t, A_I^i(v_t, A_{-d})) = U(y^i, A_{-d}).$$

Equivalently,

$$g(A_I^i(v_t, A_{-d})) = g(A_{-d}) + \Delta u_i(v_t). \quad (15)$$

The household transfers into the focal district whenever  $A_d \geq A_I^i(v_t, A_{-d})$ . Again applying the implicit function theorem,

$$\frac{\partial A_I^i(v_t, A_{-d})}{\partial v_t} = \frac{u'(y^i - v_t)}{g'(A_I^i(v_t, A_{-d}))} > 0. \quad (16)$$

Thus, lowering the transfer cost reduces the focal-district achievement necessary to attract either type of household from the outside district:

$$A_I^i(v_1, A_{-d}) < A_I^i(v_0, A_{-d}). \quad (17)$$

These thresholds also establish how the attractiveness of the outside district affects enrollment pressure. Differentiating Equations (12) and (15) with respect to  $A_{-d}$  gives

$$\frac{\partial A_R^i}{\partial A_{-d}} = \frac{g'(A_{-d})}{g'(A_R^i)} > 0 \quad \text{and} \quad \frac{\partial A_I^i}{\partial A_{-d}} = \frac{g'(A_{-d})}{g'(A_I^i)} > 0.$$

Higher achievement in the outside district therefore raises the achievement the focal district must generate both to retain its residents and to attract households from the outside district.

## D.B Effort Thresholds and Enrollment

Observed achievement in the focal district satisfies

$$A_d = Q(e) + \Gamma_d.$$

Because  $Q(e)$  is strictly increasing, each achievement threshold maps uniquely into an effort threshold. The minimum effort levels necessary to retain low- and high-income residents are

$$l^R(v_t) = Q^{-1} (A_R^l(v_t, A_{-d}) - \Gamma_d) , \quad (18)$$

$$h^R(v_t) = Q^{-1} (A_R^h(v_t, A_{-d}) - \Gamma_d) . \quad (19)$$

Similarly, the minimum effort levels necessary to attract low- and high-income households from the outside district are

$$l^I(v_t) = Q^{-1} (A_I^l(v_t, A_{-d}) - \Gamma_d) , \quad (20)$$

$$h^I(v_t) = Q^{-1} (A_I^h(v_t, A_{-d}) - \Gamma_d) . \quad (21)$$

I assume that each achievement threshold net of  $\Gamma_d$  lies within the range of  $Q(e)$ .

Because  $Q^{-1}$  is strictly increasing, the comparative statics for achievement carry directly into effort. Eliminating transfer tuition therefore raises both retention thresholds,

$$l^R(v_1) > l^R(v_0), \quad h^R(v_1) > h^R(v_0), \quad (22)$$

and lowers both attraction thresholds,

$$h^I(v_1) < h^I(v_0), \quad l^I(v_1) < l^I(v_0). \quad (23)$$

The model also implies an ordering of the four effort thresholds. From Equation (11) and the household indifference conditions,

$$\begin{aligned} g(A_R^l(v_t, A_{-d})) &< g(A_R^h(v_t, A_{-d})) < g(A_{-d}) \\ &< g(A_I^h(v_t, A_{-d})) < g(A_I^l(v_t, A_{-d})). \end{aligned}$$

Because  $g$  is strictly increasing,

$$A_R^l(v_t, A_{-d}) < A_R^h(v_t, A_{-d}) < A_I^h(v_t, A_{-d}) < A_I^l(v_t, A_{-d}).$$

Applying the strictly increasing function  $Q^{-1}$  gives

$$l^R(v_t) < h^R(v_t) < h^I(v_t) < l^I(v_t). \quad (24)$$

Low-income residents are therefore easier to retain than high-income residents because transferring imposes a larger utility loss on them. Conversely, high-income households in the outside district are easier to attract because they bear a smaller utility loss from transferring.

At effort  $e$ , focal-district enrollment is

$$\begin{aligned} E(e; v_t) = & \underbrace{(1 - x_d)\mathbf{1}\{e \geq l^R(v_t)\} + x_d\mathbf{1}\{e \geq h^R(v_t)\}}_{\text{Resident Enrollment}} \\ & + \underbrace{m_{hd}\mathbf{1}\{e \geq h^I(v_t)\} + m_{ld}\mathbf{1}\{e \geq l^I(v_t)\}}_{\text{Enrollment from Outside District}}. \end{aligned} \quad (25)$$

## D.C The Focal District's Optimal Effort

The focal district chooses effort to maximize

$$R(e; v_t) = [V - C(e)]E(e; v_t).$$

Enrollment in Equation (25) is constant between adjacent effort thresholds, while  $C(e)$  is strictly increasing. Conditional on enrolling the same set of households, the focal district therefore prefers the lowest feasible effort level. Abstracting from shutdown, optimal effort must belong to

$$\mathcal{T}(v_t) = \{l^R(v_t), h^R(v_t), h^I(v_t), l^I(v_t)\}. \quad (26)$$

The district's choice among these thresholds can be characterized by comparing any two

candidate effort levels  $e_a < e_b$ . Let  $E_a$  and  $E_b$  denote enrollment at the two thresholds and define  $\Delta E = E_b - E_a > 0$ . The district prefers  $e_b$  whenever

$$[V - C(e_b)]E_b \geq [V - C(e_a)]E_a.$$

Using  $E_b = E_a + \Delta E$  and rearranging gives

$$[V - C(e_b)]\Delta E \geq [C(e_b) - C(e_a)]E_a. \quad (27)$$

The left-hand side is the net rent generated by the additional students obtained by increasing effort from  $e_a$  to  $e_b$ . The right-hand side is the additional effort cost incurred across the students already enrolled at  $e_a$ . Higher effort is therefore more attractive when the enrollment gain is larger, the net rent generated by additional students is greater, or the incremental effort necessary to cross the next enrollment threshold is smaller.

## D.D Quality Responses to Eliminating Transfer Tuition

Equations (22) and (23) show that eliminating transfer tuition moves retention and attraction thresholds in opposite directions. I next establish the implications for optimal effort at each possible initial threshold.

**Lowest retention threshold.** Suppose the focal district initially chooses  $l^R(v_0)$ . It therefore retains low-income residents but does not exert enough effort to retain any other household group. Because shutdown is excluded, post-policy effort must be at least  $l^R(v_1)$ . From Equation (22),

$$e^1 \geq l^R(v_1) > l^R(v_0) = e^0.$$

Optimal effort must therefore increase. Because  $Q(e)$  is strictly increasing, educational quality also increases.

**Highest attraction threshold.** Now suppose the focal district initially chooses  $l^I(v_0)$  and therefore attracts both types of households from the outside district in addition to retaining its residents. Lower transfer costs reduce the effort required to attract both outside household types.

It remains to establish that the focal district continues to prefer the highest attraction threshold after transfer costs fall. First consider the incremental effort required to move from attracting high-income outside households to attracting both types. Define

$$D_u(v_t) = \Delta u_l(v_t) - \Delta u_h(v_t) > 0.$$

Its derivative is

$$D'_u(v_t) = u'(y^l - v_t) - u'(y^h - v_t) > 0,$$

where the inequality follows from strict concavity of  $u$  and  $y^l < y^h$ . Thus, as transfer costs fall, both  $\Delta u_h(v_t)$  and the difference  $\Delta u_l(v_t) - \Delta u_h(v_t)$  decrease.

Because  $g$  is increasing and concave,  $g^{-1}$  is increasing and convex. It follows from Equation (15) that the achievement gap

$$A_I^l(v_t, A_{-d}) - A_I^h(v_t, A_{-d})$$

shrinks as  $v_t$  falls. Because  $Q$  is also increasing and concave,  $Q^{-1}$  is increasing and convex, so the corresponding effort gap

$$l^I(v_t) - h^I(v_t)$$

also shrinks. Since  $C(e)$  is increasing and convex, the incremental cost

$$C(l^I(v_t)) - C(h^I(v_t))$$

therefore falls when transfer costs fall.

At the same time,  $l^I(v_1) < l^I(v_0)$  raises the net per-pupil rent  $V - C(l^I(v_t))$ . The left-hand side of Equation (27) therefore increases relative to the incremental cost of attracting low-income households from the outside district. Because choosing  $l^I(v_0)$  was optimal before the policy, attracting these households remains profitable after the policy.

The comparison with the two retention thresholds also becomes more favorable:  $l^I(v_t)$  falls while  $l^R(v_t)$  and  $h^R(v_t)$  rise, reducing the incremental effort needed to attract outside households relative to retaining only residents. Thus,

$$e^1 = l^I(v_1) < l^I(v_0) = e^0.$$

Optimal effort and educational quality therefore decrease.

**Intermediate retention threshold.** Suppose instead that the focal district initially chooses  $h^R(v_0)$ , retaining both types of resident households but attracting no households from the outside district. Maintaining the same resident enrollment after transfer costs fall requires

$$h^R(v_1) > h^R(v_0),$$

which creates an incentive to increase effort.

The district need not maintain that enrollment, however. It may instead choose  $l^R(v_1)$  and allow high-income residents to transfer to the outside district. The model imposes no ordering between  $l^R(v_1)$  and the initial effort level  $h^R(v_0)$ , so this adjustment can imply a reduction in effort. At the same time, the decline in  $h^I(v_t)$  makes attracting high-income households from the outside district less costly and may make moving to an attraction threshold profitable.

Whether the focal district increases or decreases effort therefore depends on the post-policy threshold locations and the enrollment gains and incremental effort costs in Equation (27). The response is not signed.

**Intermediate attraction threshold.** Finally, suppose the focal district initially chooses  $h^I(v_0)$ , retaining both resident types and attracting high-income households from the outside district. Because

$$h^I(v_1) < h^I(v_0),$$

the district can continue serving the same groups of households with less effort, creating an incentive to reduce quality.

However, the effort required to attract low-income households from the outside district also falls. The decline in  $l^I(v_t)$  may make the additional enrollment obtained by attracting these households sufficiently valuable to justify moving to the higher attraction threshold. The model imposes no ordering between  $l^I(v_1)$  and the district's initial effort  $h^I(v_0)$ , and Equation (27) determines whether the additional enrollment justifies the required effort.

Optimal effort can therefore rise or fall for a district initially operating at this intermediate margin.

Taken together, the model signs the response at the two extreme enrollment positions but not at the two intermediate positions. Districts initially exerting only enough effort to retain some resident enrollment must increase quality when transfer costs fall, while districts already attracting all household types reduce quality. For districts between these extremes, eliminating transfer tuition changes both the incentive to defend resident enrollment and the cost of attracting additional households, leaving the optimal quality response theoretically ambiguous.

## E House Bill 1381’s Impact on Transfer Student Selection

Eliminating transfer tuition reduced the financial cost of IDSC, but it did not initially guarantee students access to a non-residential district. Districts retained control over how many transfer students to accept and which applications to approve. This discretion matters for interpreting the paper’s results because selective admissions could have affected both the enrollment competition created by HEA-1001 and the composition of students moving across districts. Because HB-1381 restricted this discretion partway through the post-policy period, this section examines how the law changed access to IDSC and whether the resulting compositional changes affect the interpretation of the paper’s main results.

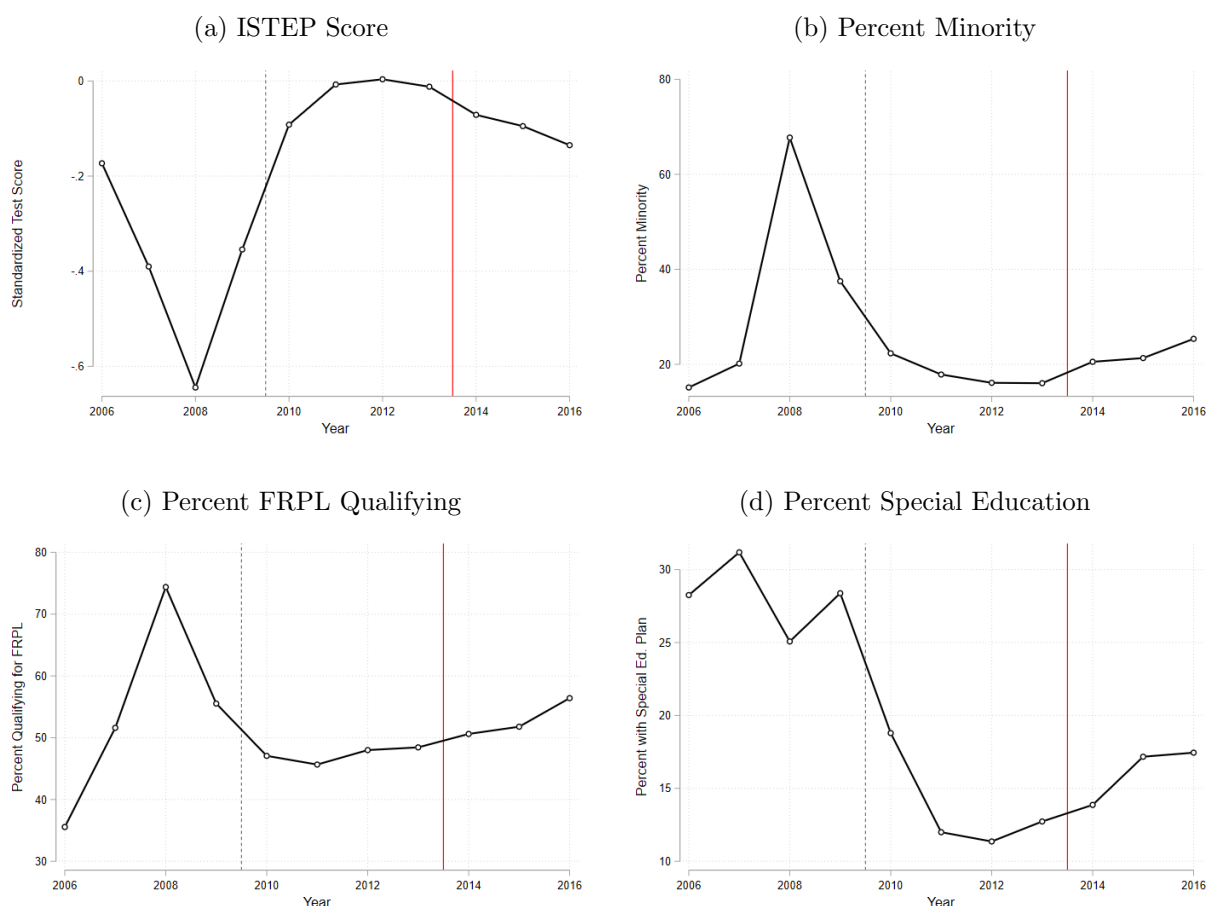
Although districts could not select applicants based on demographic characteristics, they could deny applications based on low achievement, suspension history, and special education needs (Herrmann et al., 2009). After transfer tuition was eliminated in the 2010 AY, first-time transfer students were substantially higher-achieving and less likely to use free or reduced-price lunch than first-time transfer students before the elimination (Figure D1).

Indiana policymakers argued that allowing districts to select which transfer students to accept constituted discrimination that prevented certain students from utilizing IDSC. House Bill 1381 (HB-1381), signed into law in May 2013, addressed these concerns by requiring districts to announce the number of available seats for non-residential students and hold a lottery when applications exceeded those seats (Indiana General Assembly, 2013). Districts could deny applications only from students with substantial disciplinary histories. Following HB-1381, first-time transfer students were lower-achieving, more likely to be minorities, and more likely to have special education plans, consistent with demand for IDSC among students whose participation had previously been constrained by district admissions policies (Table D1).

As discussed in Section VI.D, these compositional changes could affect district academic

quality through peer selection and thereby differentially affect districts according to their IDSC enrollment-threat exposure. However, I find no evidence that peer selection explains the paper’s main results. Value-added estimates remain similar when I exclude all transfer students from the sample (Table B13) and when I control for school districts’ student composition (Table B15).

Figure D1: Characteristics of First Time Transfer Students Over Time



**Notes:** This figure plots trends related to first-time transfer student characteristics. Dashed and solid vertical lines indicate the elimination of transfer tuition and passage of House Bill 1381, respectively. Student characteristic data come from the IDOE-CREO dataset.

Table D1: Characteristics of First-Time Transfer Students Before and After HB-1381

|                         | (1)<br>Pre HB-1381 Characteristics<br>(AY 2010–2013) | (2)<br>Post HB-1381 Characteristics<br>(AY 2014–2016) | (3)<br>Difference<br>(2)-(1) |
|-------------------------|------------------------------------------------------|-------------------------------------------------------|------------------------------|
| Standardized Test Score | -0.021                                               | -0.103                                                | -0.083***<br>(0.013)         |
| Minority                | 17.584                                               | 22.640                                                | 5.056***<br>(0.512)          |
| FRPL                    | 47.401                                               | 53.214                                                | 5.813***<br>(0.640)          |
| Disciplined             | 6.729                                                | 6.357                                                 | -0.372*<br>(0.217)           |
| Has Disability Plan     | 1.443                                                | 2.272                                                 | 0.829***<br>(0.172)          |
| Special Education       | 13.227                                               | 16.329                                                | 3.102***<br>(0.454)          |
| Student Count           | 11,605                                               | 12,726                                                | 24,331                       |

**Notes:** This table reports characteristics of first-time transfer students before (Column 1) and after (Column 2) House Bill 1381 was passed in Indiana. All characteristics are reported as percentages, with the exception of standardized test scores. Column (3) reports differences in observed characteristics. Student characteristic data come from the IDOE-CREO dataset. FRPL denotes the percentage of students qualifying for free or reduced-price lunch.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

## F Robustness to Linear-Treatment-Effect Specification

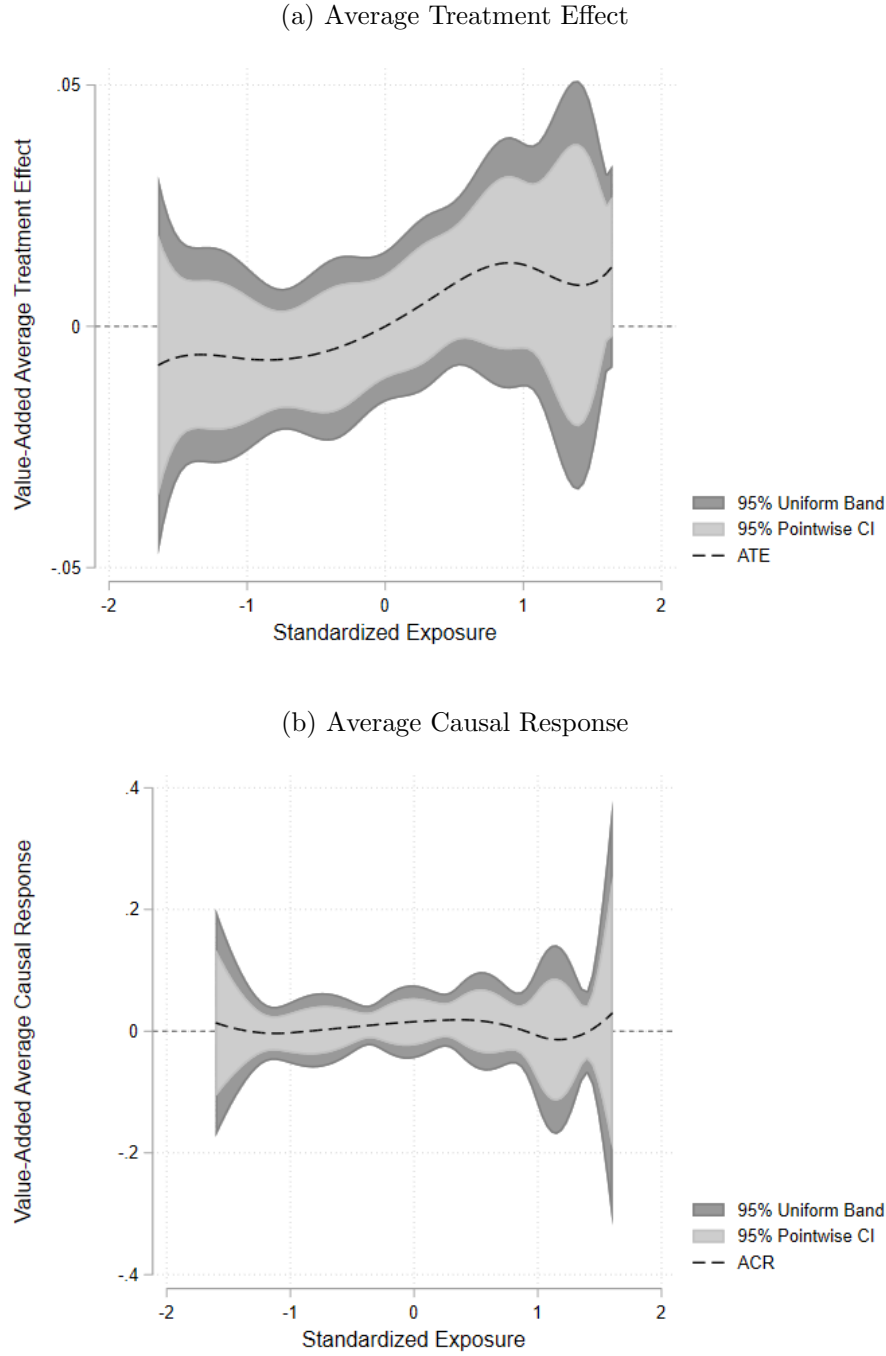
Even under continuous parallel trends, Callaway et al. (2024) show that TWFE need not recover the population-average marginal effect when effects are nonlinear because its weights on marginal effects generally differ from the exposure distribution. Their nonparametric alternative estimates the average treatment effect (ATE) schedule, which, under strong parallel trends, gives the population-average effect of each exposure level relative to zero exposure. The estimator uses cubic B-splines, selecting the number of terms from the data to balance approximation bias from restrictive functional forms against sampling variance from greater flexibility.

Figure E1 presents the nonparametric estimates. The ATE schedule (Panel a) generally indicates lower value added at negative exposure and higher value added at positive exposure relative to districts with approximately zero exposure. Its derivative, the average causal response (ACR) schedule (Panel b), measures the marginal effect of exposure and is broadly consistent with my small, positive baseline estimate. Neither schedule differs significantly from zero using pointwise confidence intervals or uniform confidence bands at the 95% confidence level. Their estimator’s data requirements limit precision in my district-level sample: although compatible with linearity, the schedules are too imprecise to distinguish it from meaningful nonlinear alternatives.

I also re-estimate my primary and event-study specifications (Equations (5) and (6)) using an indicator for positive exposure in place of the continuous exposure measure. This specification measures differential post-policy changes in value added between districts with positive and nonpositive exposure, relaxing the constant marginal effect restriction and removing the additional leverage associated with extreme exposure magnitudes. Because discretization changes the estimand, I compare the direction and patterns of effects across grades and over time rather than coefficient magnitudes. The overall estimate remains significant at the 10 percent level and all three elementary-grade estimates at the 1 percent level, while middle-grade estimates are statistically insignificant (Table E1). The event stud-

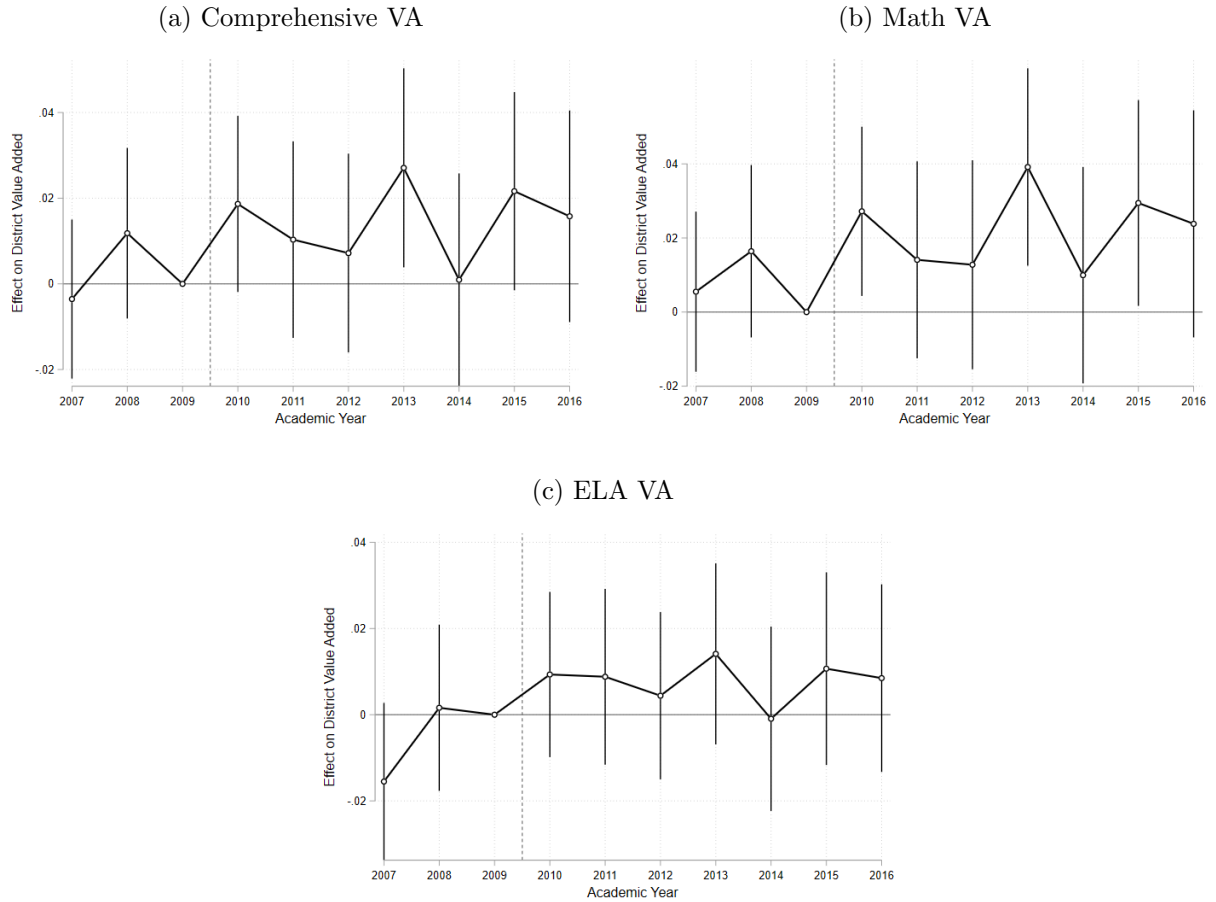
ies ([Figure E2](#)) generally show positive estimates from 2010 through 2016, consistent with an immediate and sustained response, although estimates fluctuate and are often imprecise. These findings suggest that the positive value-added response, particularly in elementary grades, does not depend solely on linearity, but the short pre-policy period limits assessment of parallel trends.

Figure E1: Non-Parametric Estimation of Average Treatment Effect and Average Causal Response Functions



**Notes:** This figure plots nonparametric estimates of the average treatment effect (ATE) and average causal response (ACR) schedules for comprehensive value added across all tested grades, following Callaway et al. (2024). Panel (a) reports effects relative to districts with standardized exposure within 0.02 of zero. Panel (b) reports the derivative of the ATE schedule with respect to standardized exposure. ATEs are measured in test-score standard deviations; ACRs measure the marginal effect per unit of standardized exposure. Estimation uses cubic B-splines with a data-selected number of terms and controls for continuous average neighboring-district exposure. Light and dark shaded regions show 95% pointwise confidence intervals and 95% uniform confidence bands, respectively, with inference clustered by district. The exposure distribution is winsorized at the 5<sup>th</sup> and 95<sup>th</sup> percentiles.

Figure E2: Event-Study Estimates of Discretized IDSC Enrollment-Threat Exposure and District Value Added



**Notes:** This figure plots event-study estimates from Equation 6, replacing continuous exposure with an indicator equal to one for positive exposure and zero otherwise. Panels (a)–(c) report comprehensive, math, and ELA value added, respectively, for all tested grades. Each coefficient measures the differential change in value added between districts with positive and nonpositive exposure relative to the omitted 2009 AY, in test-score standard deviations. Regressions include district and year fixed effects and control for continuous average neighboring-district exposure. Vertical bars show 95% confidence intervals based on network-robust standard errors following Colella et al. (2023).

Table E1: Discretized IDSC Enrollment-Threat Exposure on District Value Added by Grade Level

|                                 | All Tested Grades (4-8) |                     |                      | Elementary Grades (4-5) |                       |                       | Middle Grades (6-8) |                    |                    |
|---------------------------------|-------------------------|---------------------|----------------------|-------------------------|-----------------------|-----------------------|---------------------|--------------------|--------------------|
|                                 | Overall                 | Math                | ELA                  | Overall                 | Math                  | ELA                   | Overall             | Math               | ELA                |
|                                 | (1)                     | (2)                 | (3)                  | (4)                     | (5)                   | (6)                   | (7)                 | (8)                | (9)                |
| <b>Model Exposure Variables</b> |                         |                     |                      |                         |                       |                       |                     |                    |                    |
| $Post_t \cdot StExp_d$          | 0.0118*<br>(0.0062)     | 0.0150*<br>(0.0079) | 0.0125**<br>(0.0052) | 0.0264***<br>(0.0098)   | 0.0351***<br>(0.0133) | 0.0219***<br>(0.0073) | -0.0004<br>(0.0086) | 0.0017<br>(0.0113) | 0.0005<br>(0.0075) |
| $Post_t \cdot StNbr_d$          | -0.0015<br>(0.0033)     | -0.0001<br>(0.0041) | -0.0018<br>(0.0026)  | -0.0000<br>(0.0045)     | 0.0068<br>(0.0059)    | -0.0062*<br>(0.0033)  | 0.0031<br>(0.0046)  | 0.0021<br>(0.0060) | 0.0031<br>(0.0036) |
| <b>District-Year Count</b>      | 2850                    | 2850                | 2850                 | 2850                    | 2850                  | 2850                  | 2850                | 2850               | 2850               |

**Notes:** This table reports estimates from Equation (5), replacing continuous exposure with an indicator equal to one for positive exposure and zero otherwise. Columns (1)–(3), (4)–(6), and (7)–(9) use exposure and value added constructed for all tested, elementary, and middle grades, respectively. Within each grade group, columns report overall, math, and ELA value added. The sample covers the 2007–2016 AYs, with the post-policy period beginning in the 2010 AY. All specifications include district and year fixed effects and control for continuous average neighboring-district exposure. The coefficient on the interaction between the binary exposure indicator and the post-policy indicator measures the differential post-policy change in value added between districts with positive and nonpositive exposure, in test-score standard deviations. Parentheses report network-robust standard errors following Colella et al. (2023).

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.

## G Robustness to Value Added Specification

Chetty et al. (2014) identify two potential sources of bias in traditional value-added estimation. First, value added may drift over time, making effectiveness in nearby years more informative about current quality than effectiveness in distant years and potentially reducing the accuracy of estimates that assume fixed value added. They address this concern by allowing value added to vary over time and constructing forecasts that weight years by their informativeness about current quality. Second, they address sampling error, particularly in estimates based on few students, using Bayesian shrinkage, which shrinks noisier value-added estimates toward the mean according to their signal-to-noise ratio.

Both concerns are unlikely to be consequential in my setting. Unlike forecasts of current teacher quality based on performance in other years, my estimator uses contemporaneous student achievement to estimate separate district-year effects, allowing district quality to vary freely across years. Moreover, district-year estimates are generally based on substantially more students than teacher-level estimates, reducing the sampling variation that motivates Bayesian shrinkage. Aggregation across many classrooms and teachers may also smooth idiosyncratic year-to-year fluctuations in individual teacher effectiveness, further reducing the importance of the high-frequency drift documented in teacher-level value added.

Nevertheless, I re-estimate my primary model using value-added estimates constructed following the methodology in Chetty et al. (2014), limiting lagged outcomes to three years. Table F1 presents these results. The estimate is .0066, significant at the five percent level and similar to the primary estimate of .0074. The neighbor-exposure estimate remains statistically insignificant, while the effects for low- and middle-achieving districts are statistically significant and continue to drive the overall result. Together with the considerations above, these findings provide no evidence that my primary value-added estimation procedure biases the main results.

Table F1: IDSC Enrollment Competition and District Value Added: Adjusting for Drift and Bayesian Shrinkage

|                                            | District<br>Value Added          |                                    | District Math<br>Value Added     |                                   | District ELA<br>Value Added       |                                    |
|--------------------------------------------|----------------------------------|------------------------------------|----------------------------------|-----------------------------------|-----------------------------------|------------------------------------|
|                                            | (1)                              | (2)                                | (3)                              | (4)                               | (5)                               | (6)                                |
| <b>Model Exposure Variables</b>            |                                  |                                    |                                  |                                   |                                   |                                    |
| $Post_t \cdot StExp_d$                     | 0.0066**<br>(0.0027)<br>[0.0306] | -                                  | 0.0078**<br>(0.0035)<br>[0.0292] | -                                 | 0.0062***<br>(0.0019)<br>[0.0278] | -                                  |
| $Post_t \cdot StExp_d \cdot LowAchieve_d$  | -                                | 0.0108***<br>(0.0033)<br>[-0.1781] | -                                | 0.0106**<br>(0.0043)<br>[-0.1763] | -                                 | 0.0074***<br>(0.0027)<br>[-0.1673] |
| $Post_t \cdot StExp_d \cdot MidAchieve_d$  | -                                | 0.0122*<br>(0.0072)<br>[0.0267]    | -                                | 0.0015<br>(0.0125)<br>[0.0292]    | -                                 | 0.0150**<br>(0.0058)<br>[0.0200]   |
| $Post_t \cdot StExp_d \cdot HighAchieve_d$ | -                                | 0.0022<br>(0.0053)<br>[0.2431]     | -                                | 0.0071<br>(0.0061)<br>[0.2348]    | -                                 | 0.0035<br>(0.0035)<br>[0.2306]     |
| $Post_t \cdot StNbr_d$                     | -0.0026<br>(0.0030)              | -0.0030<br>(0.0030)                | -0.0015<br>(0.0038)              | -0.0019<br>(0.0037)               | -0.0029<br>(0.0022)               | -0.0029<br>(0.0022)                |
| <b>District-Year Count</b>                 | 2850                             | 2850                               | 2850                             | 2850                              | 2850                              | 2850                               |
| District Fixed Effects                     | <i>Yes</i>                       | <i>Yes</i>                         | <i>Yes</i>                       | <i>Yes</i>                        | <i>Yes</i>                        | <i>Yes</i>                         |
| Year Fixed Effects                         | <i>Yes</i>                       | <i>No</i>                          | <i>Yes</i>                       | <i>No</i>                         | <i>Yes</i>                        | <i>No</i>                          |
| Achievement-Tercile by Year Fixed Effects  | <i>No</i>                        | <i>Yes</i>                         | <i>No</i>                        | <i>Yes</i>                        | <i>No</i>                         | <i>Yes</i>                         |

**Notes:** This table reports continuous difference-in-differences estimates from Equations (5) and (7). The sample spans the 2007–2016 AYs, with treatment beginning in the 2010 AY. Columns (1)–(2), (3)–(4), and (5)–(6) report estimates for overall, math, and ELA district value added, respectively. Odd-numbered columns report pooled effects of IDSC enrollment-threat exposure, while even-numbered columns allow effects to vary by ex-ante achievement tercile. Coefficients measure differential post-policy changes in value added, in test-score standard deviations per one-standard-deviation increase in exposure. All specifications include district fixed effects and control for average neighboring-district exposure. Odd-numbered columns include year fixed effects, while even-numbered columns include achievement-tercile-by-year fixed effects. Parentheses report network-robust standard errors following Colella et al. (2023). Square brackets report each group’s mean post-policy test-score achievement. Value added is constructed following Chetty et al. (2014), allowing for drift over time and applying Bayesian shrinkage, with lagged outcomes limited to three years. Enrollment and achievement data come from the IDOE-CREO dataset.

\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.10.