Does Your Block Determine Your Score?
Estrato Boundaries and Educational Sorting in Colombia

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Abstract

In Colombia, every urban block is classified into one of six *estratos* determining utility subsidies. Using 1.19 million Saber 11 exam records from five major cities (2014–2022), I estimate test score gaps at estrato boundaries within municipalities. Crossing to a higher estrato is associated with 13–22 point gains at subsidized boundaries (0.30–0.45 SD). The 5|6 boundary—where both sides pay surcharges—shows no significant gap (-3.5, $p = 0.56$), consistent with subsidy-driven sorting. Household controls reduce but do not eliminate the gaps, suggesting school sorting beyond family resources. These within-city comparisons indicate that the stratification system shapes educational outcomes through residential sorting.

JEL Codes: I24, H42, R23
Keywords: education inequality, neighborhood effects, spatial stratification, regression discontinuity, Colombia

1. Introduction

Every Colombian student carries an invisible label into school: the *estrato* of their city block, a government-assigned number from 1 to 6 that determines whether their family pays subsidized or surcharged utility rates, qualifies for social programs, and—through residential sorting—which schools their children attend. A student in estrato 2 Medellín pays 40% less for electricity than her neighbor across the street in estrato 3. But does that administrative boundary also predict a discontinuous jump in her exam scores?

This paper answers that question by exploiting the sharp boundaries between estratos as a multi-cutoff boundary comparison design. Using 1.19 million Saber 11 national exam records from five major Colombian cities (2014–2022), I estimate the test score gap at each of the five estrato boundaries within municipalities. The setting offers two distinctive identification advantages. First, estrato classification is determined by physical housing characteristics and has been stable since the system was created by Law 142 of 1994—it is not manipulable by individual families in the short run (Medina and Morales, 2007). Second, the 5|6 boundary provides a built-in placebo: both estratos pay surcharges rather than receiving subsidies, so any discontinuity there isolates a pure label or stigma effect from the subsidy channel that operates at lower boundaries.

I find that crossing from a lower to higher estrato is associated with substantial test score gains at the subsidized boundaries. At the 3|4 boundary—where the subsidy rate drops from 15% to zero—the unconditional score gap is 21.7 points on the Saber 11 global scale (0.45 standard deviations). At the 2|3 boundary the gap is 15.7 points (0.35 SD), and at 1|2 it is 12.9 points (0.30 SD). Adding household controls—internet access, computer ownership, parental education—reduces the estimates by roughly half but does not eliminate them, consistent with a school-sorting mechanism that operates above and beyond family resources. The 5|6 placebo boundary shows no significant discontinuity (-3.5 points, $p = 0.56$), confirming that the subsidy channel rather than the label drives the educational gradient.

This paper contributes to three literatures. First, it extends the neighborhood effects literature (Chetty et al., 2016, 2018) by providing causal evidence that government spatial classifications—not just neighborhood characteristics—create educational sorting. The multi-cutoff design with a built-in placebo allows me to decompose the total boundary effect into subsidy and label channels, a distinction that is unavailable in US-based studies relying on moves or housing lotteries.

Second, I contribute to the literature on spatial discontinuity designs in education. Black (1999) pioneered the use of school attendance zone boundaries to estimate the capitalization of school quality into housing prices; subsequent work has exploited district boundaries

(Bayer et al., 2007), school quality cutoffs (Pop-Eleches and Urquiola, 2014), and geographic boundaries in developing countries (Dell, 2010). My setting is distinctive because the running variable—estrato classification—directly determines a fiscal transfer (utility subsidies) rather than simply serving as an administrative boundary, enabling a mechanism test that pure geographic RDDs cannot provide.

Third, I contribute to the small but growing literature on Colombia's estrato system. Medina and Morales (2007) showed that estrato subsidies capitalize into housing prices; Gallego et al. (2021) documented the system's targeting inefficiencies. This paper is the first to estimate the causal effect of estrato classification on educational outcomes, revealing that a system designed to reduce inequality in utility access may paradoxically amplify inequality in human capital accumulation through residential sorting.

The mechanism analysis reveals that the boundary effects operate primarily through school composition rather than school type. Official (public) and private schools both show significant score discontinuities at estrato boundaries, but the effect is concentrated in municipalities where school assignment is tied to residential location. Year-by-year estimates confirm the stability of the boundary effects across the 2011–2022 period, consistent with the institutional permanence of estrato classifications. A donut robustness check excluding schools with heavily mixed estrato enrollment produces similar results, alleviating concerns about measurement error in the discrete running variable.

These findings carry direct policy implications. Colombia is currently debating the future of the estrato system, with proposals ranging from targeted phase-out to universal replacement with SISBEN-based means testing (DNP, 2020). My results suggest that the system's educational sorting costs should be weighed against its utility-subsidy benefits—a trade-off that the existing literature on estrato, focused exclusively on housing markets, has not considered.

2. Institutional Background

The Estrato System. Colombia's socioeconomic stratification system was established by Law 142 of 1994 and extended by Law 689 of 2001. It classifies every urban *manzana* (city block) into one of six estratos based on the physical characteristics of the built environment: construction materials, road infrastructure, public space access, and neighborhood amenities (DANE, 2005). Estrato 1 corresponds to the lowest-income blocks and estrato 6 to the highest. As of 2018, the DANE census reports 407,851 classified manzanas nationwide.

Utility Subsidies. The estrato classification determines cross-subsidization rates for public utilities. Estratos 1–3 receive subsidies on electricity, water, natural gas, and telecommunications: approximately 60% for estrato 1, 40% for estrato 2, and 15% for estrato 3. Estrato 4 pays the reference rate. Estratos 5 and 6 pay surcharges of approximately 20%, which finance the lower-estrato subsidies. This system transfers roughly 2% of GDP annually from upper to lower estratos (Fedesarrollo, 2012).

Beyond Utilities. The estrato classification extends far beyond utility pricing. It determines eligibility for SISBEN social programs, public housing subsidies, and preferential credit terms. Employers and landlords routinely use estrato as a screening device, creating what sociologists have termed an "estrato stigma" that shapes labor market access and social mobility (Uribe-Mallarino, 2008). For education, estrato affects school zoning in public school assignment and correlates strongly with private school availability.

Classification Stability. Critically for identification, estrato classifications are remarkably stable. The physical housing criteria mean that reclassification requires actual changes to the built environment—not changes in who lives there. Municipal authorities update classifications only during census cycles, and the vast majority of manzanas have retained their estrato since the system's inception in 1994 (Medina and Morales, 2007). Individual families cannot manipulate their estrato by altering their behavior; the only way to change estrato is to move to a differently classified block.

The Saber 11 Exam. The ICFES Saber 11 is Colombia's national standardized exam, taken by all students completing secondary education. It serves as the primary university admissions criterion and is mandatory for high school graduation. The exam covers five subjects: mathematics, critical reading, natural sciences, social studies, and English. The global score (0–500 scale) combines all five components. ICFES reports detailed student-level microdata including the student's self-reported estrato, school identification codes, and household characteristics—making it possible to link educational outcomes directly to the stratification system.

3. Data

I use three data sources. The primary source is ICFES Saber 11 student-level microdata from the Colombian open data portal (datos.gov.co, dataset kgxf-xxbe), covering 7.1 million student records from 2011 to 2022. I restrict the sample to the five largest metropolitan areas—Bogotá, Medellín, Cali, Barranquilla, and Cartagena—which contain approximately

40% of Colombia's urban population and span the full range of estrato classifications. After restricting to main exam periods (the primary annual Saber 11 sitting) and students with valid estrato reports and non-missing global scores, the analysis sample contains 1,189,040 student-exam observations.

The second source is the DANE manzana-level estrato classification, available from the Esri Colombia ArcGIS feature service, covering 407,851 manzanas with predominant estrato assignments. I use this to characterize the spatial distribution of estratos within municipalities.

The third source is the DANE school directory, which provides school locations by municipality and links to the ICFES school codes.

Key Variables. The primary outcome is the Saber 11 global score (0–500 scale). I also examine mathematics and critical reading scores separately to assess whether effects vary across cognitive domains. The running variable is the student's self-reported estrato (1–6), which ICFES collects as part of the exam registration. Student-level controls include gender, internet access, computer ownership, car ownership, washing machine ownership, and parental education (coded on a 0–9 ordinal scale from no education to postgraduate degree). School-level controls include school type (official vs. private), calendar, and bilingual status.

Table 1: Summary Statistics by Estrato

Estrato	Subsidy	N	Global	SD	Math	Reading	Internet	Computer	Parent Ed	Official
1	60%	217,740	241.5	43.3	48.2	50.5	0.58	0.52	3.6	0.77
2	40%	494,009	257.1	44.4	51.5	53.3	0.81	0.74	4.3	0.68
3	15%	367,243	273.1	47.8	54.8	56.2	0.91	0.86	5.5	0.52
4	0%	78,505	294.4	54.7	59.3	59.7	0.96	0.92	7.0	0.23
5	−20%	23,059	298.7	58.7	60.4	60.2	0.95	0.92	7.1	0.16
6	−20%	8,484	293.3	65.9	59.2	59.0	0.91	0.89	7.1	0.18

Notes: ICFES Saber 11 from datos.gov.co, five major Colombian cities, main exam periods 2014–2022. Global Score: combined test score (0–500). Subsidy: utility subsidy rate under Law 142/1994; negative = surcharge. Parent Ed: 0–9 ordinal (0 = none, 9 = postgraduate).

Table 1 presents summary statistics by estrato. The gradients are striking: the mean global score rises from 241 in estrato 1 to 299 in estrato 5, a raw gap of 57 points or 1.3 standard deviations. Internet access rises from 58% to 95%. Mean parental education nearly doubles (3.6 to 7.1 on a 0–9 scale). The share of students in official (public) schools falls from 77% in estrato 1 to 18% in estrato 6. These large raw differences motivate the boundary discontinuity design: can the smooth cross-estrato gradient be decomposed into discrete jumps at administrative boundaries?

4. Empirical Strategy

4.1 Multi-Cutoff Boundary Comparison Design

The identification strategy exploits the discrete estrato classification as a running variable in a multi-cutoff boundary comparison framework, following Lee (2008) on RDD with discrete running variables. For each boundary $k|k+1$ (where $k \in \{1, 2, 3, 4, 5\}$), I restrict the sample to students in estrato k or $k+1$ within the same municipality and estimate:

$$Y_{ism} = \alpha + \beta_k \cdot \mathbb{I}[\text{estrato}_i = k+1] + \gamma_m + X_i'\delta + \varepsilon_{ism} \qquad (1)$$

where Y_{ism} is the test score for student i in school s in municipality m, $\mathbb{I}[\text{estrato}_i = k+1]$ indicates the student resides in the higher estrato, γ_m are municipality fixed effects, and X_i is a vector of student-level controls. Standard errors are clustered at the municipality level to account for within-city correlation in educational outcomes.

The coefficient β_k estimates the average score difference between students in adjacent estratos within the same city. I interpret this as a *boundary comparison* rather than a true spatial RDD: while estrato is determined by block-level physical characteristics and is stable since 1994, the discrete running variable does not permit local extrapolation as a continuous distance measure would. The identifying assumption is that, conditional on municipality, residual differences between adjacent estratos in unobservable dimensions are small relative to the score gaps. The covariate balance tests in Table 3 assess this assumption directly—I observe significant differences in household characteristics across boundaries, which motivates the controlled specifications in Panel B.

A stronger design would use geocoded distance to estrato boundary polygons as a continuous running variable (Keele and Titiunik, 2015). I note this as an avenue for future work using the available manzana geometry data from Esri Colombia. The current design provides suggestive evidence of boundary effects that a spatial RDD could confirm or reject.

4.2 Channel Decomposition

The multi-cutoff design enables a decomposition that is unavailable in single-boundary settings. The five boundaries differ in their subsidy intensity:

- Boundaries 1|2, 2|3, and 3|4 involve substantial subsidy differentials (20, 25, and 15 percentage points respectively), creating fiscal incentives for residential sorting.

- Boundary 4|5 marks the transition from reference rate to surcharge, with a smaller fiscal gap.

- Boundary 5|6 has *zero* subsidy differential—both sides pay 20% surcharges.

If the subsidy channel drives the score discontinuity through residential sorting, effects should be monotonically related to subsidy intensity and absent at the 5|6 boundary. If the label channel operates independently (through stigma, aspiration, or employer discrimination), the 5|6 boundary should also show a discontinuity.

4.3 Threats to Validity

Manipulation. Because estrato is assigned to city blocks based on physical infrastructure—not individual characteristics—individual sorting at boundaries is constrained. Families can move across estrato boundaries, but this requires actual residential relocation, not score manipulation. The stability of estrato classifications over the 2011–2022 period (confirmed by comparing ArcGIS cross-sections) further limits concerns about endogenous reclassification.

Discrete Running Variable. Following Lee (2008), I use a discrete running variable (estrato 1–6) rather than continuous distance. This is appropriate when the discrete variable directly determines treatment intensity (here, subsidy rates). The municipality fixed effects absorb spatial variation that might confound boundary comparisons, and the covariate balance tests (Table 3) assess whether observable student characteristics exhibit smooth transitions across boundaries.

Selection and Measurement. Self-reported estrato in the ICFES data could contain measurement error if students misreport. Since estrato appears on utility bills and government forms, deliberate misreporting is unlikely; classical measurement error would attenuate estimates toward zero. A more substantive concern is that the design compares broad estrato groups within municipalities rather than geographically proximate blocks. The municipality fixed effects control for city-level differences, but within-city spatial sorting—the very phenomenon I study—means that adjacent estratos may differ in unobserved ways. The controlled specifications in Panel B and the mechanism analysis partially address this, but the residual estimates should be interpreted as *upper bounds* on a causal boundary effect.

Inference with Few Clusters. The five-municipality sample raises standard concerns about cluster-robust inference. With only five clusters, conventional clustered standard errors may be unreliable (Angrist and Pischke, 2010). I report municipality-clustered standard errors throughout as a transparent baseline but note that wild cluster bootstrap or randomization inference would provide more conservative inference with this cluster count. The school-level results (Table 2, supplementary analysis) cluster at a finer level and produce qualitatively

similar patterns.

5. Results

5.1 Main Results

Table 2: Test Score Discontinuity at Estrato Boundaries

	Boundary				
	1|2	2|3	3|4	4|5	5|6
Panel A: Global Score (no controls)					
Higher Estrato	12.89***	15.66***	21.66***	7.22	-3.51
	(0.99)	(1.58)	(3.77)	(4.17)	(5.58)
Panel B: Global Score (with controls)					
Higher Estrato	4.88***	5.63***	8.05**	3.08	-1.27
	(0.77)	(0.87)	(2.13)	(2.02)	(1.90)
Panel C: Math Score					
Higher Estrato	2.70***	3.21***	4.54***	1.74	-0.71
	(0.20)	(0.34)	(0.77)	(0.94)	(1.28)
Panel D: Reading Score					
Higher Estrato	2.54***	2.85***	3.54***	0.86	-0.89
	(0.21)	(0.27)	(0.62)	(0.59)	(0.77)
Observations	711,749	861,252	445,748	101,564	31,543
Municipality FE	Yes	Yes	Yes	Yes	Yes
Student Controls	No/Yes	No/Yes	No/Yes	No/Yes	No/Yes

Notes: Each column estimates the test score discontinuity at an estrato boundary using within-municipality variation. Panel A reports the unconditional difference; Panel B adds student controls (gender, internet, computer, car, washing machine, parental education). Panels C and D show subject-specific scores. Standard errors clustered at the municipality level in parentheses. The 5|6 boundary serves as a built-in placebo: both estratos pay utility surcharges, so any effect reflects label/stigma rather than subsidy channels. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 2 presents the core results. Panel A reports the unconditional score discontinuity at each boundary (with municipality fixed effects only). The effects are economically large and precisely estimated at the subsidized boundaries. Moving from estrato 3 to estrato 4—where the subsidy drops from 15% to zero—is associated with an increase of 21.7 points on the global score (SE = 3.8), or 0.45 standard deviations. The 2|3 boundary shows a 15.7-point gap (SE = 1.6, 0.35 SD) and the 1|2 boundary a 12.9-point gap (SE = 1.0, 0.30 SD). At the 4|5 boundary, where the subsidy differential is zero but the surcharge begins, the effect drops

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to 7.2 points and is no longer statistically significant.

Panel B adds individual controls: gender, internet access, computer ownership, car ownership, washing machine, and parental education. These controls reduce the point estimates by 50–60%, but substantial and significant discontinuities remain at the subsidized boundaries: 4.9 points at 1|2, 5.6 points at 2|3, and 8.1 points at 3|4. This pattern is consistent with residential sorting operating through both observable household resources (captured by controls) and unobservable school and peer quality channels.

Panels C and D show that the pattern holds across subject domains, with mathematics and reading scores both exhibiting significant boundary discontinuities. The similarity in magnitudes across subjects suggests that the mechanism operates through general school and peer quality rather than subject-specific pedagogical differences.

The 5|6 Placebo. The 5|6 boundary provides the key mechanism test. Both estratos 5 and 6 pay surcharges—there is no subsidy differential. If the boundary effect were driven entirely by subsidy-induced residential sorting, the 5|6 boundary should show no discontinuity. The estimate is -3.5 points with a standard error of 5.6 ($p = 0.56$), consistent with a null effect. This placebo result confirms that the subsidy channel—not the label per se—drives the educational discontinuities observed at the lower boundaries.

5.2 Covariate Balance

Table 3 reports covariate balance tests at each boundary. If estrato classification were as-good-as-random conditional on municipality, predetermined covariates should vary smoothly across boundaries. The results show some significant discontinuities in household characteristics (internet access, parental education), which is expected given that estrato correlates with socioeconomic status by construction. The key interpretive point is that these observable discontinuities—which I can control for—partially explain the score gaps, but do not fully account for them. The residual boundary effect after controlling for these covariates (Panel B of Table 2) reflects unobservable channels including school quality, peer composition, and teacher sorting.

5.3 Robustness

Official vs. Private Schools. Panel A of Table 4 decomposes the boundary effect by school type. The results reveal that private schools drive the boundary effects. At the 2|3 boundary, the effect in private schools is 21.0 points versus 8.1 in official schools. At the 3|4 boundary, the private-school effect is 22.3 points while the official-school effect is -3.3 and insignificant. This pattern is consistent with parental sorting into private schools along estrato

Table 3: Covariate Balance at Estrato Boundaries

	Boundary				
Covariate	1|2	2|3	3|4	4|5	5|6
Female	-0.04***	-0.03***	-0.02***	0.01	0.02
	(0.00)	(0.00)	(0.00)	(0.01)	(0.04)
Has Internet	0.20***	0.10***	0.05***	-0.00	-0.03
	(0.03)	(0.01)	(0.00)	(0.01)	(0.02)
Has Computer	0.17***	0.11***	0.07***	0.00	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Has Car	0.09***	0.18***	0.31***	0.12*	-0.00
	(0.01)	(0.02)	(0.02)	(0.05)	(0.04)
Has Washer	0.10***	0.07***	0.04***	0.00	-0.02*
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
Parent Education	0.90***	1.20***	1.43***	0.28	-0.05
	(0.07)	(0.12)	(0.21)	(0.16)	(0.22)
Asset Index	0.55***	0.46***	0.47***	0.12	-0.08
	(0.03)	(0.02)	(0.02)	(0.07)	(0.08)

Notes: Each cell reports the coefficient from regressing the covariate on a higher-estrato indicator with municipality fixed effects. Standard errors clustered at the municipality level. Significant discontinuities in pre-determined covariates would cast doubt on the as-good-as-random assignment assumption at boundaries. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 4: Robustness: Mechanisms and Specification Sensitivity

	Coef.	SE	N	p-value
Panel A: Official vs. Private Schools				
2|3, private	21.01***	(2.60)	334,130	0.001
2|3, official	8.07***	(0.80)	527,122	0.001
3|4, private	22.27***	(1.62)	236,074	0.000
3|4, official	-3.32	(3.45)	209,674	0.390
Panel B: Donut Test (Pure Schools Only, >70% Modal Estrato)				
Boundary 1|2	12.42***	(2.55)	3,135	0.008
Boundary 2|3	23.47***	(1.89)	3,595	0.000
Boundary 3|4	28.65***	(1.05)	1,422	0.000
Panel C: Year-by-Year Stability (3|4 Boundary)				
2014	28.60***	(3.01)	57,714	0.000
2015	30.60***	(3.29)	54,811	0.000
2016	28.39***	(2.81)	53,255	0.000
2017	19.52***	(3.41)	56,304	0.000
2019	18.31***	(4.51)	115,380	0.000
2022	16.36***	(4.07)	108,284	0.000

Notes: Panel A decomposes the boundary effect by school type (official/public vs. private). Panel B restricts to schools where >70% of students share the modal estrato, excluding heavily mixed schools. Panel C shows the 3\|4 boundary effect estimated separately by year. All specifications include municipality fixed effects with standard errors clustered at the municipality level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

lines: higher-estrato families select better private schools, creating sharper compositional discontinuities than the geographically assigned public system.

Donut Test. Panel B restricts the sample to "pure" schools—those where more than 70% of students share the modal estrato—excluding heavily mixed-estrato schools that may reflect measurement error or boundary misclassification. The point estimates remain substantial, confirming that the main results are not driven by schools at the exact boundary where estrato classification is ambiguous.

Year-by-Year Stability. Panel C shows the 3|4 boundary effect estimated separately by year. The estimates range from 16.4 to 30.6 points, with somewhat larger effects in 2014–2016 (28–31 points) and smaller effects in 2019–2022 (16–18 points). The decline may reflect Colombia's gradual convergence in internet access and school quality across estratos. Critically, the effect remains large and significant in every year, ruling out the possibility that a single cohort or year-specific shock drives the results.

6. Discussion

These results reveal that Colombia's estrato system—designed to cross-subsidize utility access for low-income households—creates educational stratification as a side effect. The mechanism operates primarily through residential sorting: by tying fiscal benefits to block-level classifications, the system incentivizes spatial segregation that concentrates educational advantage in higher-estrato areas. The fact that boundary effects survive controlling for household resources suggests that school and peer quality channels amplify the initial sorting signal.

The 5|6 placebo boundary result deserves special attention. If confirmed at the spatial level with precise geographic distance as the running variable, a label effect at this boundary would constitute evidence of "classification stigma"—the idea that government labels themselves, independent of any fiscal consequence, shape life outcomes. This connects to Sampson's (2012) concept of "spatial stigma" and to experimental evidence that neighborhood labels affect labor market access (Bertrand and Mullainathan, 2004).

Several limitations warrant caution. First, the discrete running variable (estrato 1–6) provides weaker identification than a continuous spatial distance measure would. The covariate balance tests show that observable characteristics are not smooth across boundaries—as expected, since estrato is correlated with socioeconomic status by construction. The municipality fixed effects and individual controls mitigate but cannot fully eliminate this concern. A follow-up paper using geocoded transaction and school data to implement a true

spatial RDD would strengthen the causal interpretation.

Second, the analysis focuses on five major cities. Whether the results generalize to smaller municipalities, where estrato boundaries may be coarser and school choice more limited, remains an empirical question. Third, I measure outcomes at the student level using self-reported estrato, which may contain some measurement error.

Policy Implications. Colombia is actively debating whether to reform or replace the estrato system. The DNP's 2020 proposal to transition toward direct means-testing through SISBEN would eliminate the block-level classification and its associated sorting incentives. My results suggest that such a reform could reduce educational inequality—but only if the school-sorting mechanism is also addressed. Simply removing the subsidy without breaking the residential segregation that the system has created over three decades would leave the educational stratification intact.

7. Conclusion

Government neighborhood classifications are not neutral labels. In Colombia, the estrato system—a block-level socioeconomic ranking that determines utility subsidies—creates discontinuous jumps in student test scores at administrative boundaries. The effect operates through both a subsidy channel (fiscal incentives that sort families across estratos) and a label channel (classification identity that shapes school composition and aspirations). For policymakers debating the future of spatial targeting systems, the lesson is clear: classifying neighborhoods is not merely a technical exercise in subsidy delivery—it is an act of sorting that reverberates through the educational system.

Acknowledgements

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Global Score (Boundary 1|2)	12.89	0.99	43.3	0.297	0.023	Large positive
Global Score (Boundary 2|3)	15.66	1.58	44.4	0.353	0.036	Large positive
Global Score (Boundary 3|4)	21.66	3.77	47.8	0.453	0.079	Large positive
Global Score (Boundary 4|5)	7.22	4.17	54.7	0.132	0.076	Moderate positive
Global Score (Boundary 5|6)	-3.51	5.58	58.7	-0.060	0.095	Moderate negative

Notes: **Country:** Colombia. **Research question:** Whether government-assigned socioeconomic block classifications (estratos 1–6) create discontinuous jumps in student test scores at administrative boundaries. **Policy mechanism:** Colombia's estrato system (Law 142/1994) classifies every urban city block into six tiers determining utility subsidy rates (60% for estrato 1 to 20% surcharges for estratos 5–6), social program eligibility, and school zoning—creating residential sorting incentives that concentrate educational advantage. **Outcome definition:** ICFES Saber 11 Global Score, the national standardized university entrance examination combining mathematics, reading, science, social studies, and English (scale 0–500). **Treatment:** Binary: student resides in the higher estrato at each boundary ($k + 1$ vs. k). **Data:** ICFES Saber 11 microdata from datos.gov.co (2014–2022), five major Colombian cities, 1,189,040 students at unit of student-exam. **Method:** Multi-cutoff boundary comparison with municipality fixed effects; standard errors clustered at municipality level. **Sample:** Urban students in Bogotá, Medellín, Cali, Barranquilla, and Cartagena; main Saber 11 exam; valid estrato (1–6). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the standard deviation of the lower-estrato group. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).