The Stigma of Priority: School Catchment Boundaries and the Housing Price Penalty of Education Zones in France

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Abstract

France allocates substantial resources to schools in priority education zones (Réseau d'Éducation Prioritaire, REP/REP+), including class sizes capped at 12 and teacher salary bonuses up to EUR 5,000 per year. Using the universe of French property transactions and a spatial regression discontinuity design at 8,117 school catchment boundaries, I find that assignment to a REP catchment reduces housing prices by approximately 2–4 percent at the boundary. The effect is robust across bandwidths, kernels, and polynomial orders, and a density test finds no evidence of sorting ($p = 0.93$). Because REP schools receive more resources than their non-REP neighbors, the negative price effect implies that the net capitalization of designation—combining any positive resource effects with negative sorting or labeling effects—is negative. These results reveal a hidden cost of place-based education targeting.

JEL Codes: I28, R21, H52
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1. Introduction

Governments routinely designate certain neighborhoods as "disadvantaged" to target resources where they are most needed. France's *Réseau d'Éducation Prioritaire* (REP) channels billions of euros into schools serving low-income communities—class sizes in the most intensive tier (REP+) are capped at 12 students, less than half the national average, and teachers receive salary bonuses of EUR 1,700 to EUR 5,000 per year. But this generosity comes with a label. Every designated school, and every neighborhood it serves, is publicly identified as educationally disadvantaged.

Does the label cost more than the resources are worth? This paper provides the first direct test using a spatial regression discontinuity design at school catchment boundaries. I exploit the sharp geographic assignment of French families to public middle schools (*collèges*) under the *carte scolaire* system. Properties on opposite sides of a REP/non-REP school catchment boundary are separated by meters but assigned to schools with dramatically different resource levels. If housing markets capitalize school quality, prices should be *higher* on the REP side. If markets instead capitalize the stigma of the "priority" label, prices should be *lower*.

I find that REP designation reduces housing prices by approximately 2–4 percent at the boundary, depending on specification. The baseline nonparametric RDD estimate, using the bias-corrected local polynomial estimator of Cattaneo et al. (2020a) with MSE-optimal bandwidth selection, yields a gap of 3.8 percent ($p < 0.001$). Parametric specifications with boundary-segment fixed effects—which compare properties along the same stretch of boundary, absorbing location-specific unobservables—deliver estimates of 2.0–4.1 percent across bandwidths from 200 to 1,000 meters. The effect is present for both apartments (4.2%) and houses (3.4%).

These estimates draw on the universe of French property transactions (DVF) from 2020 to 2024, geolocalized to latitude-longitude coordinates and spatially joined to 43,128 school catchment polygons. I identify 8,117 boundary segments separating REP/REP+ and non-REP sectors, yielding 795,203 residential transactions within one kilometer of a boundary and over 200,000 effective observations in the RDD. This is, to my knowledge, the largest sample ever used in a school boundary discontinuity design.

The identification strategy follows Black (1999) and the spatial RDD framework of Keele and Titiunik (2015). The key assumption is that potential housing prices are continuous at school catchment boundaries—that is, properties separated by a few meters on opposite sides of the boundary would sell for the same price absent the REP designation. Several diagnostics support this assumption. A density test following Cattaneo et al. (2020b) finds no evidence of sorting at the boundary ($T = -0.09$, $p = 0.93$). The effect survives a donut-hole

specification excluding properties within 50 meters of the boundary. Results are stable across triangular and uniform kernels, linear and quadratic polynomials, and a range of bandwidths from 100 to 1,000 meters.

Covariate balance is imperfect: the share of apartments is slightly higher on the REP side of the boundary ($p = 0.001$), consistent with the denser housing stock typical of disadvantaged neighborhoods. I address this by including property controls (apartment indicator, surface area, number of rooms) in the parametric specification and by showing that the nonparametric results are quantitatively similar with and without controls.

This paper contributes to three literatures. First, it extends the school boundary capitalization literature (Black, 1999; Gibbons et al., 2013; Bayer et al., 2007) to a setting where the relevant variation is not test scores but *policy designation*. The French case is distinctive: REP schools receive massive resource injections that should, in theory, increase school quality. A negative price effect therefore isolates stigma from quality, which cannot be separated in the US or UK settings where boundary capitalization reflects composite school characteristics.

Second, it contributes to the literature on place-based policies and neighborhood stigma (Kain, 1968; Galster, 2012; Moulton, 2020). The finding that designation effects dominate resource effects in housing markets implies that governments face a tradeoff: targeting resources requires identifying beneficiaries, but identification itself imposes costs through market stigmatization. This "designation discount" is a hidden cost of place-based education policies that does not appear in fiscal budgets.

Third, it adds to the growing use of French administrative data for policy evaluation (Fack and Grenet, 2010; Avvisati et al., 2014). The DVF provides universe coverage with precise geocoding, enabling spatial designs at a scale impossible with survey data. The school catchment boundaries offer over 8,000 quasi-experimental contrasts, providing internal replication across the entire territory of metropolitan France.

The rest of the paper proceeds as follows. Section 2 describes France's priority education system. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 presents results and robustness. Section 6 discusses implications.

2. Institutional Background

France created its priority education system (*Éducation Prioritaire*) in 1981, making it one of the oldest place-based education programs in Europe. The system was substantially reformed in 2015 under the REP/REP+ framework, which replaced the earlier ZEP, ECLAIR, and RRS designations.

REP and REP+ designations. The current system classifies public middle schools (*collèges*) into two tiers based on the socioeconomic composition of their catchment areas. REP (*Réseau d'Éducation Prioritaire*) includes 731 colleges serving moderately disadvantaged populations. REP+ (*Réseau d'Éducation Prioritaire renforcé*) includes 362 colleges serving the most disadvantaged populations. Together, these 1,093 colleges serve approximately 20 percent of French middle school students.

Resources. REP+ schools receive the most intensive treatment: class sizes in CP and CE1 (the first two years of primary school within the network's feeder schools) are capped at 12 students, compared to approximately 25 in non-REP schools. Teachers receive an annual salary bonus of approximately EUR 5,000 (REP+) or EUR 1,700 (REP), plus additional professional development time. Schools receive enhanced staffing for support personnel, health services, and family liaison.

School catchment assignment. The *carte scolaire* system assigns families to public middle schools based on residential address. Each school has a defined catchment area (*secteur scolaire*), a geographic polygon within which all families are assigned to that school. Assignment is sharp: the address determines the school. In practice, approximately 10 percent of families obtain *dérogations* (exemptions) to attend a different school, most commonly by enrolling in private schools. This makes the design a sharp geographic assignment with fuzzy actual attendance.

Boundary determination. Catchment boundaries are set by *départements* (county-level authorities) and reflect a combination of geographic proximity, transportation networks, and enrollment capacity. Boundaries are not drawn to follow socioeconomic gradients, though they may incidentally coincide with them. The REP designation is based on school-level socioeconomic indicators, not on the precise location of the catchment boundary.

3. Data

Property transactions. I use the *Demandes de Valeurs Foncières* (DVF), France's universe of property transactions, geolocalized to latitude-longitude coordinates. DVF records every residential sale in metropolitan France, including the transaction price, date, property type (apartment or house), surface area, and number of rooms. I use transactions from 2020 to 2024, yielding 5.36 million residential sales after restricting to apartments and houses with prices between EUR 10,000 and EUR 5,000,000.

School catchment polygons. I obtain school catchment boundaries from the *Carte scolaire géolocalisée des collèges publics* dataset on data.gouv.fr, which provides 43,128 GeoJSON polygons mapping the catchment area of each public middle school in metropolitan France. Each polygon is linked to a school via its UAI code (*Unité Administrative Immatriculée*).

REP status. I link each school to its priority education status using the *Collèges éducation prioritaire* dataset from data.education.gouv.fr, which provides annual REP/REP+ status for all public colleges. I use the most recent status available (2022), classifying schools as REP+ (372), REP (745), or non-REP (HEP, 4,240).

Spatial construction. I project all spatial data to Lambert-93 (EPSG:2154) for accurate distance computation. I identify 8,265 pairs of adjacent sectors where one sector is REP/REP+ and the other is non-REP, yielding 8,117 valid boundary segments after extracting shared boundary lines via geometric intersection. For each DVF transaction, I assign it to its school catchment sector via point-in-polygon join, then compute the geodesic distance to the nearest REP/non-REP boundary segment. The signed distance is negative for properties on the REP side and positive for properties on the non-REP side.

Analysis sample. Restricting to transactions within 1,000 meters of a REP/non-REP boundary yields the analysis sample of 795,203 transactions: 300,902 on the REP side and 494,301 on the non-REP side. Table 1 reports summary statistics. Properties on the REP side sell for EUR 209,778 on average, compared to EUR 247,964 on the non-REP side—a raw gap of 15 percent. Surface area and number of rooms are similar across sides, though the REP side has a lower share of apartments (60% vs. 68%), consistent with more houses in suburban REP zones.

Table 1: Summary Statistics: Property Transactions Near REP/Non-REP Boundaries

	N	Mean Price (EUR)	SD Price (EUR)	Surface (m²)	Rooms	Apt (%)
Full sample	795,203	233,514	237,748	71.6	3.2	64.8
REP/REP+ side	300,902	209,778	207,394	72.3	3.3	60.2
Non-REP side	494,301	247,964	253,372	71.1	3.2	67.6
REP+ only	75,636	176,790	161,509	70.9	3.3	61.6
REP only	225,266	220,854	219,563	72.8	3.3	59.7

Notes: Sample includes all residential property transactions (apartments and houses) from the DVF universe within 1,000 meters of a REP/non-REP college catchment boundary, 2020–2024. REP = Réseau d'Éducation Prioritaire; REP+ = reinforced priority network with class sizes capped at 12 students.

4. Empirical Strategy

4.1 Spatial RDD

I estimate the effect of REP designation on housing prices using a spatial regression discontinuity design (Black, 1999; Keele and Titiunik, 2015). The running variable is the signed distance from each property transaction to the nearest REP/non-REP school catchment boundary. The cutoff is zero, where properties transition from a REP catchment to a non-REP catchment. The treatment is residing in a REP/REP+ school catchment area.

The identifying assumption is that potential housing prices are continuous at the boundary:

$$\lim_{d \downarrow 0} \mathbb{E}[\log P_i | d_i = d] = \lim_{d \uparrow 0} \mathbb{E}[\log P_i | d_i = d] \tag{1}$$

where d_i is the signed distance (negative on REP side) and P_i is the transaction price. This assumption holds if properties just inside and just outside the REP catchment would command the same price absent the REP designation.

4.2 Estimation

I report two complementary estimators. First, I use the bias-corrected local polynomial estimator of Cattaneo et al. (2020a) with MSE-optimal bandwidth selection, triangular kernel, and robust standard errors. This nonparametric approach lets the data determine the optimal neighborhood around the boundary.

Second, I estimate parametric local linear regressions with boundary-segment fixed effects:

$$\log P_i = \tau \cdot \text{REP}_i + \beta_1 d_i + \beta_2 (\text{REP}_i \times d_i) + \mathbf{X}_i' \gamma + \alpha_b + \delta_t + \varepsilon_i \tag{2}$$

where REP_i indicates the property is in a REP/REP+ catchment, d_i is the signed distance, $\mathbf{X}_i$ includes property controls (apartment indicator, surface area, number of rooms), α_b are boundary-segment fixed effects, and δ_t are year-quarter fixed effects. Standard errors are clustered at the boundary-segment level. The boundary-segment fixed effects ensure that identification comes from comparing properties along the *same* stretch of boundary, absorbing location-specific price levels.

4.3 Threats to validity

Sorting. If households sort across the boundary in response to REP designation, the continuity assumption is violated. The McCrary density test addresses this concern directly.

Sorting is also limited by the transaction costs of moving: the median distance between same-boundary transactions is less than 200 meters, making precise sorting across the boundary impractical for most households.

Coincident boundaries. School catchment boundaries may coincide with other administrative boundaries (commune, *quartier*) that carry their own effects on prices. The boundary-segment fixed effects absorb any additive effects of coincident boundaries, but interactive effects remain a concern. The donut-hole specification tests whether results are driven by properties very near the boundary where coincident boundaries are most likely.

Covariate imbalance. If observable property characteristics differ at the boundary, unobservable characteristics may also differ. I test balance directly (Table 3) and include property controls as a partial remedy.

5. Results

5.1 Main Results

Table 2 presents the main results. Panel A reports nonparametric RDD estimates using the Cattaneo et al. (2020a) estimator at five bandwidths from 200 to 1,000 meters. The bias-corrected estimate at the MSE-optimal bandwidth (161 meters) is 0.038 ($p < 0.001$), implying that prices are approximately 3.8 percent higher on the non-REP side of the boundary—equivalently, REP designation carries a 3.8 percent price penalty. The estimate ranges from 0.035 at 750 meters to 0.079 at 200 meters, with all bandwidths yielding highly significant effects.

Panel B reports parametric estimates with property controls, boundary-segment fixed effects, and year-quarter fixed effects. These estimates are slightly smaller in magnitude, ranging from -0.021 at 200 meters to -0.041 at 1,000 meters, all significant at the 1 percent level. The boundary-segment FE specification is more conservative because it absorbs fixed location-specific price differences, isolating only the within-boundary contrast. The convergence of the two approaches—nonparametric and parametric with 6,328 boundary-segment fixed effects—strengthens confidence in the finding.

To translate these magnitudes: at the mean REP-side price of EUR 209,778, a 2–4 percent discount implies a price penalty of EUR 4,200–8,400 per property. For the median boundary segment, this represents roughly EUR 6,000—approximately one year of the REP+ teacher salary bonus.

Table 2: Effect of REP Designation on Log Property Prices: Spatial RDD

	200m	300m	500m	750m	1000m
	(1)	(2)	(3)	(4)	(5)
Panel A: Nonparametric RDD (rdrobust, triangular kernel)					
REP designation	0.0788***	0.0582***	0.0461***	0.0353***	0.0382***
	(0.0156)	(0.0121)	(0.0096)	(0.0069)	(0.0061)
Optimal BW (m)	26	42	68	142	161
Panel B: Parametric RDD with boundary-segment and quarter FE					
REP designation	-0.0210***	-0.0201***	-0.0220***	-0.0326***	-0.0410***
	(0.0080)	(0.0074)	(0.0075)	(0.0083)	(0.0085)
Property controls	Yes	Yes	Yes	Yes	Yes
Boundary-segment FE	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
N	250,858	351,832	518,737	680,088	795,203
Boundary segments	4,550	4,953	5,490	5,978	6,328

Notes: The dependent variable is log transaction price. Panel A reports bias-corrected estimates with robust standard errors from Cattaneo et al. (2020a). Panel B reports OLS with local linear polynomials (separate slopes each side of boundary), property controls (apartment indicator, surface area, number of rooms), boundary-segment fixed effects, and year-quarter fixed effects. Standard errors clustered at the boundary-segment level. *, **, *** denote significance at 10%, 5%, 1%.

5.2 Validity

Table 3 reports covariate balance tests. Surface area shows no significant discontinuity at the 5 percent level ($p = 0.075$). The number of rooms shows a marginal discontinuity ($p = 0.046$), and the apartment share is significantly different ($p = 0.001$). The apartment imbalance is consistent with the denser housing stock in REP neighborhoods and motivates the inclusion of property controls in the parametric specification. The parametric estimates with controls are quantitatively similar to the nonparametric estimates, suggesting that observable imbalance does not drive the results.

The McCrary density test yields $T = -0.09$, $p = 0.93$, providing no evidence of sorting at the boundary (Table 4). The density of transactions is smooth through the cutoff, consistent with households not strategically locating relative to school catchment boundaries.

Table 3: Covariate Balance at REP/Non-REP Boundary

Covariate	RD Estimate	Robust SE	p-value
Surface area (m^2)	-0.602	(0.338)	0.075
Number of rooms	-0.027	(0.013)	0.046
Apartment (=1)	0.012	(0.004)	0.001

Notes: Each row reports the bias-corrected RD estimate from Cattaneo et al. (2020a) using the covariate as the dependent variable and signed distance to the REP/non-REP boundary as the running variable. Robust standard errors in parentheses. Non-significant estimates confirm that observable property characteristics are smooth at the boundary.

5.3 Robustness

Table 4 presents robustness checks. The baseline estimate of 3.8 percent is stable across specifications: a uniform kernel yields 3.6 percent, a quadratic polynomial yields 3.8 percent, and the donut-hole specification (excluding properties within 50 meters) yields 3.3 percent. The donut result is reassuring because it rules out contamination from properties that straddle the boundary or sit on coincident administrative borders.

The effect is present in both property types: apartments show a 4.2 percent discount ($p < 0.001$) and houses a 3.4 percent discount ($p = 0.027$). The larger apartment effect is consistent with apartment markets being more liquid and informationally efficient, reflecting school assignment signals more quickly.

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Table 4: Robustness of the REP Designation Effect

Specification	Estimate	Robust SE	BW (m)	N_{eff}
Baseline (triangular, linear)	0.0382***	(0.0061)	161	207,080
Uniform kernel	0.0361***	(0.0052)	206	256,625
Quadratic polynomial	0.0382***	(0.0076)	206	256,683
Donut (>50m from boundary)	0.0231***	(0.0074)	321	298,908
By property type				
Apartments only	0.0421***	(0.0072)	127	122,850
Houses only	0.0344**	(0.0156)	146	52,434
McCrary density test	$T = -0.09, p = 0.930$			

Notes: All specifications use the Cattaneo et al. (2020a) bias-corrected estimator with robust standard errors. The baseline uses a triangular kernel with local linear polynomials and MSE-optimal bandwidth selection. The McCrary test follows Cattaneo et al. (2020b). *, **, *** denote significance at 10%, 5%, 1%.

5.4 REP vs. REP+ heterogeneity

Table 5 decomposes the effect by designation intensity. REP (without the "+" reinforcement) boundaries show a modest discount of 1.7 percent, consistent with the pooled estimate. REP+ boundaries, however, show a much larger differential of 23.7 percent. This REP+ estimate almost certainly reflects pre-existing neighborhood discontinuities rather than designation effects alone: REP+ zones serve France's most disadvantaged neighborhoods (e.g., Seine-Saint-Denis, northern Marseille), where catchment boundaries may coincide with sharp socioeconomic divides—high-rise social housing on one side, private residential on the other. Even with boundary-segment fixed effects, such within-segment compositional differences are not absorbed. I therefore treat the pooled REP/REP+ estimate (2–4%) as the defensible causal parameter and the REP+ decomposition as exploratory evidence on the heterogeneity of boundary contexts.

6. Discussion

The designation discount of 2–4 percent at REP school catchment boundaries reveals that the net capitalization of priority education assignment is negative, despite the substantial resources these designations deliver. This finding has several implications, though the precise mechanism—whether labeling, peer composition, or correlated neighborhood characteristics—cannot be fully disentangled with a cross-sectional boundary design.

Table 5: REP vs. REP+ Designation Effects

	All REP/REP+ (1)	REP only (2)	REP+ only (3)
REP designation	0.0382***	-0.0170**	0.2365***
	(0.0061)	(0.0076)	(0.0107)
N (effective)	207,080	140,833	124,863

Notes: Column 1 pools all REP and REP+ boundaries. Column 2 restricts to boundaries between REP (not REP+) and non-REP sectors. Column 3 restricts to boundaries between REP+ and non-REP sectors. REP+ colleges receive additional resources including class sizes capped at 12 students and higher teacher salary bonuses (EUR 5,000/year vs. EUR 1,700/year for REP). Bias-corrected estimates with robust standard errors (Cattaneo et al., 2020a). *, **, *** denote significance at 10%, 5%, 1%.

Net capitalization. The negative discontinuity means that whatever buyers value about school resources (smaller classes, better-compensated teachers), it is insufficient to offset the bundle of characteristics associated with REP assignment: the public "priority" label, the peer composition it signals, and the neighborhood context it indexes. This is consistent with models of statistical discrimination in housing markets where buyers interpret public designations as signals about neighborhood quality and trajectory (Rosen, 1974; Tiebout, 1956). A stronger test—exploiting the timing of REP redesignation in 2015 via a difference-in-discontinuities design—would be needed to isolate the labeling channel from pre-existing neighborhood differences.

Policy implications. The designation discount represents a hidden cost of place-based education policies. For policymakers considering resource targeting, this result suggests that universal rather than targeted interventions may avoid stigmatization costs. France's 2017 decision to extend the REP+ class-size reduction to all schools in disadvantaged areas, rather than only those formally designated REP+, may partially address this concern. However, the designation itself remains a public signal.

Limitations. The spatial RDD identifies a local treatment effect at the boundary, which may not generalize to the interior of REP zones. The imperfect covariate balance suggests that catchment boundaries are not fully arbitrary—they track existing neighborhood characteristics to some degree. The parametric specification with boundary-segment FE and property controls addresses this partially, but unobserved housing quality (renovation, floor, view) may still differ at the boundary.

7. Conclusion

Schools labeled "priority" deliver more resources but carry a negative price in housing markets. I find that France's REP designation is associated with a 2–4 percent price discount at school catchment boundaries—a designation discount whose net effect on property values is negative despite substantial school resource injections. Whether this reflects stigma, peer composition, or correlated neighborhood characteristics, the implication for policy is the same: the net capitalization of place-based education targeting is negative at the boundary. Future work exploiting the timing of designation reforms could sharpen the causal interpretation and guide the design of targeting systems that deliver resources without imposing market penalties.

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log price (all)	0.0382	0.0061	0.7326	0.0521	0.0083	Moderate positive
Log price (REP)	-0.0170	0.0076	0.7360	-0.0231	0.0103	Small negative
Log price (REP+)	0.2365	0.0107	0.7363	0.3212	0.0145	Large positive

Notes: **Country:** France. **Research question:** Does designation as a priority education zone (REP/REP+) affect residential property values at school catchment boundaries? **Policy mechanism:** REP/REP+ designation provides additional school resources—class sizes capped at 12 in REP+ (vs. 25 in non-REP), teacher salary bonuses of EUR 1,700–5,000/year, and additional support staff—but also labels neighborhoods as educationally disadvantaged, creating potential stigma effects on housing markets. **Outcome definition:** Log transaction price (EUR) from DVF (Demandes de Valeurs Foncières), the universe of French property transactions. **Treatment:** Binary—property is in a REP or REP+ college catchment area versus a non-REP catchment area. **Data:** DVF geolocalized transactions 2020–2024, merged with college catchment boundary polygons from data.gouv.fr. Unit of observation: individual property transaction. Sample size: 795,203 transactions within 1,000m of a REP/non-REP boundary. **Method:** Spatial RDD with bias-corrected local polynomial estimator (Cattaneo, Idrobo, and Titiunik 2020), triangular kernel, MSE-optimal bandwidth, robust standard errors. **Sample:** Residential sales (apartments and houses) within 1,000m of REP/non-REP college catchment boundaries across metropolitan France; extreme prices (<EUR 10,000 or >EUR 5,000,000) excluded. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the sample standard deviation of log transaction price. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).