

# Place-Based Policies: A Path to Opportunity or a Mark of Stigma for Targeted Neighborhoods?

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**Abstract**

In France, place-based policies channel public subsidies to low-income urban zones labeled "priority" neighborhoods. Yet, such policies may backfire by stigmatizing the targeted areas. This paper exploits a quasi-experimental discontinuity created by a reform that redrew the subsidy map using a local poverty cutoff to evaluate impacts on school outcomes, population sorting across labeled and unlabeled neighborhoods, and urban segregation. Using a difference-in-differences design, we show that public middle schools in newly designated neighborhoods experienced a persistent decline in enrollment relative to schools in unlabeled areas just above the cutoff. This territorial stigma, driven by school avoidance across all socioeconomic groups, redirected pupils to public schools in unlabeled areas and, among wealthier families, to private schools. Policy designation also depressed housing prices and triggered reverse gentrification, while educational support proved insufficient to offset rising urban segregation. Crucially, the stigma persisted even after neighborhoods lost their subsidized status, leading to sustained declines in academic achievement due to both reputational damage and subsidy withdrawal. (Keywords: School enrollment, Educational achievement, Urban segregation, Spatial sorting, Housing price.) (JEL: I24, I28, R23, R58)

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## 1. Introduction

For more than 50 years, policy makers have relied on place-based subsidies to reduce urban segregation and improve living conditions in low-income neighborhoods. In France, these programs target about 1,300 designated “priority” neighborhoods, home to 5.4 million residents (8% of the population). A central component of these geographically-tied interventions is education: initiatives designed to expand opportunities for disadvantaged children through tutoring, psychological support, cultural and sports activities, and personalized healthcare, all aimed at improving educational outcomes.

However, despite substantial subsidies, concerns have grown that unintended effects may undermine these place-based policies. This paper examines one such effect: territorial stigmatization (Wacquant et al., 2014), which has received little attention in economics. In France, “priority” neighborhoods are often negatively portrayed in the media (Magat et al., 2018; ONPV, 2022), most infamously when a Fox News journalist labeled them “no-go zones” in 2015.<sup>1</sup> Local officials and school leaders also report that middle- and upper-class families often avoid schools in these neighborhoods, raising concerns about reinforced segregation and reduced social mobility (Dieusaert, 2018; Ville & Banlieue, 2016).

This paper examines the unintended consequences of place-based policies in France, with a focus on their effects on school enrollment, neighborhood composition, and urban segregation. Place-based policies can influence educational outcomes and urban dynamics through three channels. First, they can improve education outcomes by directing additional resources to disadvantaged students enrolled in targeted neighborhoods. Second, they can simultaneously depress school reputations by reinforcing negative perceptions of the areas receiving public subsidies. Third, family-sorting responses and neighborhood compositional shifts resulting from this territorial stigma can reshape peer environments, with further implications for student achievement. The net effect is theoretically ambiguous, as potential gains may be offset by complex general equilibrium effects induced by place-based redistribution (Gaubert et al., 2025), a tension that this paper empirically investigates.

Exploiting a quasi-natural experiment created by a reform that revised policy eligibility based on a sharp and non-manipulable poverty threshold, we provide causal evidence on how gaining or losing policy designation influences both educational outcomes and local residential dynamics. Our main finding is that “priority” designation generates significant stigma. Public middle schools serving labeled neighborhoods experienced an immediate

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1. See, among others: Washington Post or Fox News.

and persistent decline in enrollment relative to comparable schools serving areas just above the eligibility threshold. This decline, which persisted up to five years after policy designation, reflects both a “flight-to-quality,” with low-income families relocating their children to public schools outside the targeted areas, and a “rich-flight,” with more affluent households either moving away or switching to private schooling. We further document marked housing price depreciation and patterns of “reverse gentrification” following designation, indicating that the policy unintentionally reinforced, rather than mitigated, urban segregation. Importantly, reputational damage appears highly persistent: we find little evidence of stigma reversal even after neighborhoods lose their policy status.

Our study speaks to several strands of the economics literature. The first examines how additional educational resources affect students’ outcomes. Extensive US evidence shows sizable gains in test scores, attainment, and earnings, particularly for disadvantaged students (Card and Payne, 2002; Jackson et al., 2015; Jackson and Mackevicius, 2024), although the magnitude depends on implementation (Handel and Hanushek, 2023). European findings are more mixed: positive effects in the UK (Machin et al., 2010; Gibbons et al., 2017) contrast with null results in Romania (Munteanu, 2024) and France (Bénabou et al., 2009; Befy and Davezies, 2013). This paper diverges from this existing literature because, unlike school-targeted resource programs, French place-based educational initiatives allocate funds to municipalities rather than schools, with benefits flowing directly to resident children.

Second, our study contributes to the literature on information, reputation, and school choices. By labeling neighborhoods, place-based policies provide salient signals about socioeconomic disadvantage, which parents can interpret as markers of school quality. This mechanism aligns with evidence showing that family residential and school choices respond strongly to social composition and perceived reputation (Rothstein, 2006; Burgess et al., 2015), with downstream effects on segregation and housing markets (Black, 1999; Fack and Grenet, 2010; Gibbons et al., 2013a; Schwartz et al., 2014; Collins and Kaplan, 2017).

Third, we contribute to research on peer and neighborhood effects. While the influence of peers on academic trajectories is well documented (Epple and Romano, 2011; Sacerdote, 2011), the role of immediate neighbors has received less attention (Barrios-Fernandez, 2023; Garrouste and Hémet, 2024; Zenou, 2025). Evidence from France (Goux and Maurin, 2007; Behaghel et al., 2017), the UK (Gibbons et al., 2013b), and the US (Angrist and Lang, 2004; Abdulkadiroğlu et al., 2014; Dobbie and Fryer, 2014; Angrist et al., 2023) underscores that peer composition shapes both academic and non-cognitive outcomes, albeit in heterogeneous ways. Our findings show that policy labeling influences children’s educational trajectories not only through peer composition or resource allocation, but also by shaping perceptions

of neighborhood amenities, particularly school quality, paralleling how the public disclosure of school performance metrics guides parental school choices (Friesen et al., 2012; Koning and van der Wiel, 2013).

We also extend the broader literature on place-based policies, which has primarily focused on enterprise zones and labor market outcomes (Neumark and Simpson, 2015; Bartik, 2020; Corinth and Feldman, 2024). Existing studies find limited effects on residents' employment (Freedman, 2015; Reynolds and Rohlin, 2015), with benefits often dissipating over time or accruing to newcomers (Gobillon et al., 2012; Charnoz, 2018), with adverse consequences for incumbent residents through job displacement (Briant et al., 2015; Mayer et al., 2017) or house price inflation (Freedman, 2013; von Ehrlich and Seidel, 2018; Kitchens and Wallace, 2022). By contrast, evidence on human capital effects is sparse and largely indirect, often relying on child mobility experiments such as Moving to Opportunity in the U.S. (Chetty et al., 2016; Chetty and Hendren, 2018a,b; Chyn, 2018; Chyn and Katz, 2021), refugee resettlement in Israël or Sweden (Gould et al., 2004, 2011; Åslund et al., 2011) or school closures in France Guyon (2022).<sup>2</sup> The only evaluation of a French geographically targeted educational initiative found minimal effects on educational achievement and even declines in students' self-esteem (Bressoux et al., 2016). Our study expands this evidence base by providing the first large-scale evaluation of French place-based education programs and documenting a powerful stigmatizing mechanism.

Finally, we contribute to the emerging literature on territorial stigmatization, which shows that labeling-and sometimes treating-neighborhoods can adversely affect children's outcomes (Fishback et al., 2023; Aaronson et al., 2021, 2023; Domínguez et al., 2023; Govind et al., 2024), generate labor market discrimination (Bunel et al., 2016), depress housing prices (Koster and van Ommeren, 2022; Andersson et al., 2026; Yamagishi and Sato, 2025), and reduce economic interactions (Besbris et al., 2014). Our contribution is twofold. First, our quasi-experimental setting, combining a two-way fixed-effects difference-in-differences strategy with a spatial discontinuity design, allows us to disentangle policy and labeling effects from residential sorting, a persistent empirical challenge (Cutler and Glaeser, 1997; Sharkey, 2016; Caetano and Macartney, 2021). This is especially salient in France, where school assignments are tied to residential catchment areas, causing segregation across neighborhoods and schools to reinforce one another (Monarrez, 2023; Boutchenik et al., 2020). Second, we show that labeling shapes perceptions of school quality and induces persistent parental sorting across both schools (public vs private) and neighborhoods (labeled vs unlabeled), ultimately reinforcing the segregation these policies were intended to mitigate. Notably,

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2. An exception is Baum-Snow et al. (2019), which uses labor demand shocks across U.S. tracts to study children who could not relocate.

we show that stigma persists even after neighborhoods lose labeling, leading to sustained declines in academic achievement driven by both reputational damage and the withdrawal of policy resources.

The remainder of the paper is organized as follows. Section 2 describes the institutional context and our quasi-experimental setting. Section 3 presents the empirical framework and data. Section 4 reports main effects on school enrollment, while Section 5 discusses robustness. Section 6 explores heterogeneous effects, and Section 7 analyzes neighborhood dynamics and segregation. Section 8 concludes.

## 2. The French institutional background

Over the past decades, social stratification across French urban neighborhoods has deepened, with an increasing share of areas concentrating either very affluent or very disadvantaged residents (Gerardin and Pramill, 2023). In response, a series of place-based policies have been implemented to target the most deprived neighborhoods. These initiatives, jointly administered by the State and municipalities, aim to improve living conditions, labor market integration, and educational outcomes in targeted areas. Since their inception in the late 1970s, they have relied on zoning systems that allocate public support based on neighborhood-level socioeconomic indicators. This section provides a brief overview of the institutional context and the 2014 reform that redefined policy boundaries using an income-based criterion.

### 2.1. *Place-based urban policies in France before 2014*

In France, the most comprehensive urban policy targeting low-income neighborhoods began in 1996 with the launch of the *Pacte de Relance pour la Ville* (Urban Stimulus Package). This program introduced a three-tier zoning system that classified urban neighborhoods by increasing levels of deprivation: 751 Tier-1 zones, 416 Tier-2 zones, and 100 Tier-3 zones. Tier-1 *Zones Urbaines Sensibles* (ZUS, Urban Sensitive Zones) were identified for their deteriorated housing and low job-to-resident ratios. Tier-2 *Zones de Redynamisation Urbaine* (ZRU, Urban Revitalization Zones) form a subset of Tier 1, and were selected among the lowest ranked Tier-1 areas according to a multidimensional deprivation index, and designated as enterprise zones. Finally, Tier-3 *Zones Franches Urbaines* (ZFU, Urban Tax-Free Zones) were chosen among the most deprived Tier-2 areas and granted broader tax exemptions.<sup>3</sup> Each tier corresponded to progressively more generous incentives for firms operating or locating within these areas. In Tier-1 zones,

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3. In a few cases, Tier-3 areas were extended beyond Tier 2 boundaries to include adjacent vacant land, which explains that the two Tiers are not always perfectly nested.

fiscal support was modest and discretionary: local authorities could offer limited tax relief to micro-entrepreneurs and social housing providers, but these measures were neither automatic nor extensive. By contrast, Tier-2 and Tier-3 zones benefited from automatic rebates on payroll, corporate, business, and property taxes, determined solely by firm location. These incentives were substantially larger and longer-lasting in Tier-3 areas, where firms also qualified for further wage credits conditional on hiring local residents (see Table B1 for a detailed overview of such incentive schemes).<sup>4</sup>

In 2006, the program was expanded through the *Contrats Urbains de Cohésion Sociale* (CUCS, Urban Social Cohesion Contracts), which incorporated all Tier-1 to Tier-3 zones and added 1,750 Tier-0 neighborhoods facing multidimensional social challenges. These contracts formalized cooperation between central and local governments, bringing the total number of subsidized neighborhoods to nearly 2,500, and committed them to coordinated actions aimed at improving residents' living conditions.

By then, all four tiers of zones were eligible for educational programs designed to promote equal access to quality schooling for children and teenagers in targeted areas. The cornerstone initiative, the *Programme de Réussite Éducative* (Educational Success Program, hereafter PRE), provides individualized support to disadvantaged resident pupils, addressing their academic, psychological, and social needs to improve educational outcomes. Assistance includes tutoring and homework help, psychological counseling, healthcare interventions (e.g., screening for untreated illnesses, vision problems, mental health issues, and learning disorders), and extracurricular activities (cultural, sports, or leisure programs) fostering social integration.<sup>5</sup> Recognizing the importance of parental involvement, the PRE also promotes family engagement through parenting workshops, counseling, and resources aimed at strengthening parents' role in their children's education.

The local educational community plays a pivotal role in implementation. Teachers and principals identify students showing early signs of disengagement, exclusion, academic difficulty, or behavioral issues, and refer them to appropriate programs.<sup>6</sup> However, schools act only as intermediaries: they do not receive funds directly. Policy resources are managed by local authorities and allocated to multidisciplinary teams, including educators, counselors,

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4. Tier benefits are substitutive rather than additive, that is, Tier 2 benefits replace those of Tier 1 where the two overlap, and Tier 3 benefits replace those of Tier 2.

5. The PRE has an annual budget of about €100 million and supported roughly 122,000 students at its peak in 2010. It is funded mainly by the ANCT (70%), with the remainder financed by municipalities and inter-municipal bodies. See Demangeclaude (2018) and <http://observatoire-reussite-educative.fr/dispositifs/dossier-PRE/>.

6. Other initiatives include *École Ouverte*, which keeps middle schools open during vacations and weekends to provide educational, cultural, sports, and leisure activities promoting social inclusion and violence prevention. For a comprehensive list, see <http://observatoire-reussite-educative.fr/dispositifs/>.

social workers, health professionals, and psychologists, responsible for after-school initiatives. As a result, these programs primarily target resident children, while their schools play a facilitative but non-financial role.

The place-based educational programs examined in this paper therefore differ fundamentally from conventional school-based policies, which typically focus on class-size reductions, staff recruitment, teacher training and bonuses, or pedagogical reforms implemented within schools. France also operates school-based programs, managed by the Ministry of Education, that allocate additional resources to schools enrolling large shares of disadvantaged students (e.g., pupils from low-income families, public scholarship recipients, or students who have repeated a grade).<sup>7</sup> Because disadvantaged pupils are highly concentrated geographically, place-based and school-based policies often overlap, although their beneficiaries do not necessarily coincide.<sup>8</sup> As school-based programs can induce sorting, where relatively less disadvantaged families opt out of targeted schools (Davezies and Garrouste, 2020), our empirical strategy includes school fixed effects and an annual indicator of school-based funding, to ensure that the estimated impacts of place-based educational programs are not confounded with school-based interventions.

## 2.2. *The 2014 Reform of Urban Zoning (Lamy Law)*

In 2012, the Court of Auditors, the French institution responsible for overseeing public fund management, sharply criticized the spread of subsidies over too many urban neighborhoods (Cour des Comptes, 2012). In response, the 2014 Lamy Law for Cities and Urban Cohesion streamlined the existing four-tier zoning system into a single category of approximately 1,300 Priority Neighborhoods (*Quartiers Prioritaires de la politique de la Ville*, QPV hereafter).<sup>9</sup>

The Lamy Law determined geographical eligibility solely on the basis of residents' taxable income: urban neighborhoods with a median income below 60% of a reference income qualified for treatment, while others were excluded. The reference income was a weighted average of national and local urban unit

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7. The main initiatives are the Reinforced Priority Education networks, known as REP+ (schools facing severe socio-academic disadvantages) and REP (schools with moderate difficulties).

8. Among the 126,000 students attending a middle school in REP+ (resp. REP), approximately 65% (resp. 28%) live in neighborhoods also targeted by place-based policies, compared with only 4% for students outside these networks (Dieusaert, 2017).

9. *Loi n°2014-173 du 21 février 2014 de programmation pour la ville et la cohésion urbaine.*

medians,<sup>10</sup> limiting potential manipulation by local authorities. This reform thus generated a transparent, income-based rule for policy inclusion.

Technically, eligibility was determined on a 200-meter square grid by computing the median taxable income of residents within each cell using 2011 data, with contiguous low-income cells aggregated into neighborhoods meeting minimum population thresholds. This mechanical, data-driven procedure, entirely outside the control of local actors, underpins our identification strategy. The reform substantially reshaped the geography of policy intervention, as illustrated by Figure 1 in the Paris region, which accounts for roughly 20% of newly eligible neighborhoods: many previously treated areas were removed (e.g., polka-dotted zones in the right panel of Figure 1), while previously ineligible areas entered the program (e.g., hatched zones in the left panel). As Table 1 shows, similar reshuffling occurred nationwide.

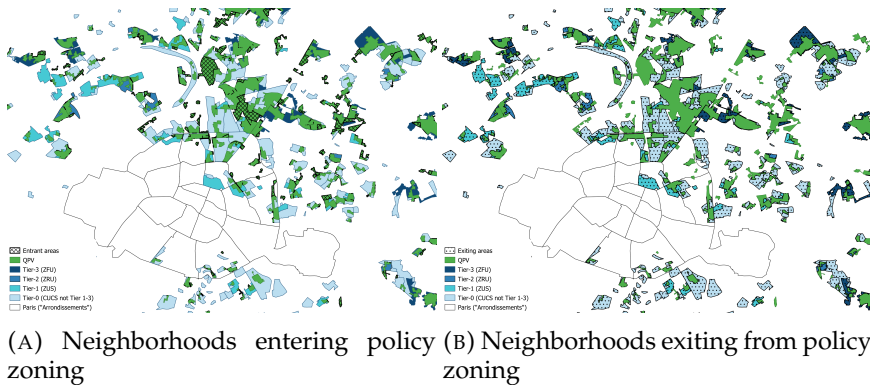


FIGURE 1. Policy areas before and after the 2014 reform in the Paris region

Sources: Authors based on shapefiles from the ANCT (2010c, 2018a,b,c) and the Direction de l’Urbanisme - Ville de Paris (2013).

Notes: The boundaries of Parisian *arrondissements* are outlined in black. Hatched areas represent neighborhoods entering the program, while polka-dotted zones indicate areas disqualified after the reform.

Crucially, the Lamy reform did not change school catchment boundaries, so schools entered or exited treatment solely as a result of neighborhood reclassification (see Figure 2, which illustrates this entry and exit in the Paris region). We exploit this quasi-exogenous reshuffling across France to identify

10. In France, an urban unit is a municipality (or a group of adjacent municipalities) with over 2,000 inhabitants forming a single unbroken built area. For urban units between 10,000 and 5 million inhabitants, the reference income per consumption unit,  $I_R$ , was defined as follows:  $I_R = 0.7 \times I_{FR} + 0.3 \times I_{UU}$ , with  $I_{FR}$  the national median income per consumption unit and  $I_{UU}$  its equivalent for the relevant urban unit. For urban units over 5 million inhabitants (i.e., Paris), greater weight was assigned to the local median income, as it exceeded the national median (€22,048 versus €19,218 in 2011):  $I_R = 0.3 \times I_{FR} + 0.7 \times I_{UU}$ .

TABLE 1. Neighborhoods affected by the 2014 reform (mainland France)

Treated Zones	Before 2014	Intersecting with a QPV after 2014
Tier-0 (CUCS not Tiers 1–3)	1,596	894
Tier-1 (ZUS)	717	635
Tier-2 (ZRU)	396	374
Tier-3 (ZFU)	93	93
Priority neighborhoods (QPV)	0	1,296

*Sources:* Authors based on shapefiles from the ANCT (2010c, 2018a,b,c) (excluding French overseas).

the causal effects of neighborhood entry and exit on school enrollment, neighborhood composition, and educational outcomes. Importantly, most subsidy schemes remained largely unchanged by the reform: educational programs stayed consistent, while enterprise tax benefits were only slightly modified in terms of generosity and duration (see Table B1 in Appendix B). Transitional arrangements allowed firms in phased-out zones to retain benefits for several years, mitigating immediate local labor market economic shocks that could confound our analysis (see Section 7.4 for a more detailed robustness check).

Following the enactment of the Lamy Law on February 21, 2014, the government launched a nationwide communication campaign, including press conferences and an online address-search tool that allowed residents to verify their neighborhood’s eligibility.<sup>11</sup> These measures provided substantial transparency and informational impact, giving parents several months to adjust their school choices before the 2014–2015 school year.

11. Figure A1 in Appendix A provides a screenshot illustrating this tool (see <https://sig.ville.gouv.fr/>).

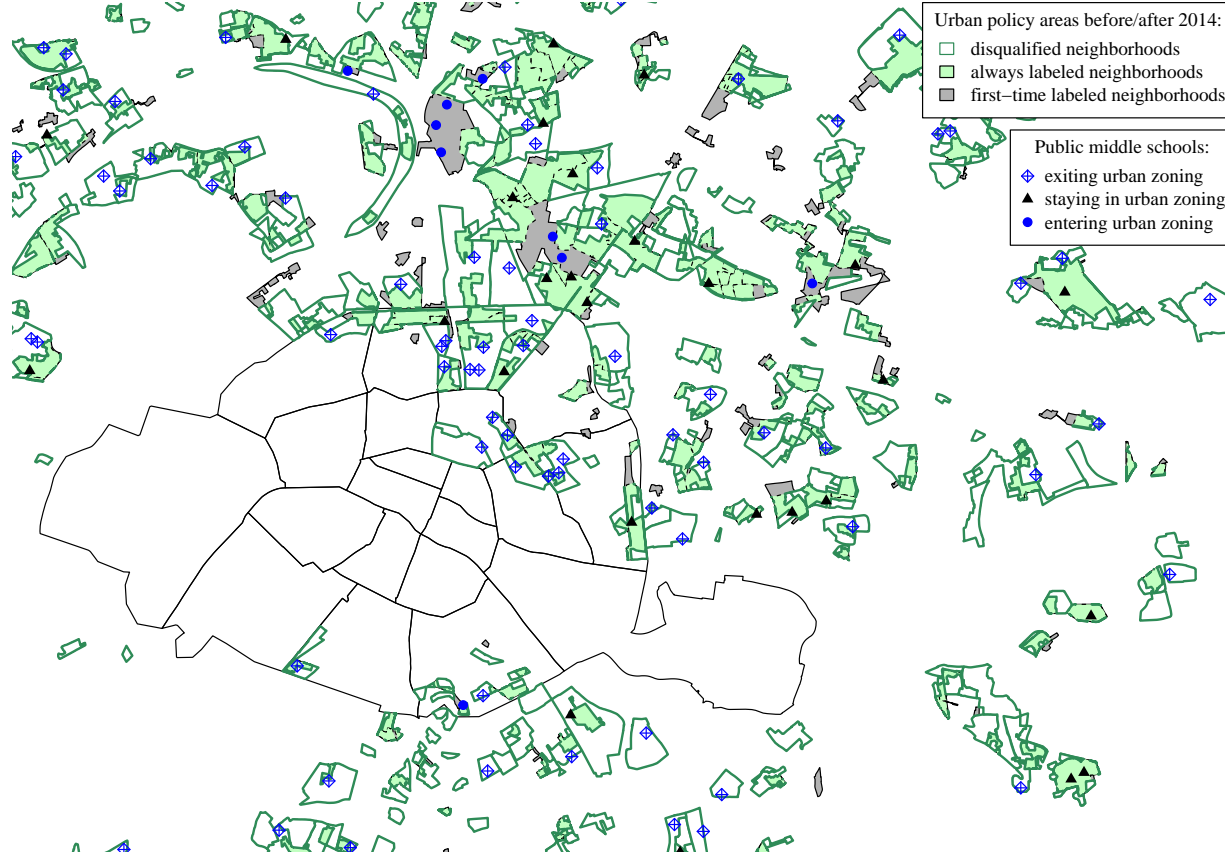


FIGURE 2. The 2014 policy-reform implementation in the Paris region

Sources: Authors based on *Annuaire de l'éducation* (DNE-MEN, 2019), and shapefiles provided by the ANCT (2010c, 2018a,b,c) and the Direction de l'Urbanisme - Ville de Paris (2013).

Notes: The boundaries of Parisian *arrondissements* are shown in black. Hollow green (resp. filled gray) polygons represent neighborhoods that "exited" (resp. "entered") policy zoning, while filled green polygons indicate those that have had and continue to have policy coverage. Hollow blue diamonds (solid blue circles) represent middle schools that "exited" (resp. "entered") policy coverage, and solid black triangles represent middle schools that have had and continue to have policy coverage.

### 2.3. *Pupil assignment to middle schools in France*

In France, education is compulsory from ages 6 to 16, comprising five years of primary school, four years of lower secondary education in middle schools, and three years of upper secondary education in high schools. Middle school marks a key transition for students, as they move from a single-class, single-teacher environment to multiple teachers and subjects. Because lower secondary education strongly shapes future vocational and academic trajectories in high schools, the choice of middle school at the start of lower secondary education is particularly important for families. Accordingly, we focus primarily on pupils entering 6<sup>th</sup> grade (ages 11–12) over the 2010–2019 school years, which encompass the implementation of the 2014 Lamy reform.

Public middle school assignment is primarily determined by residential catchment areas. Most pupils attend their default school, although parents may request enrollment elsewhere if capacity allows. Exceptions are granted for students with disabilities, merit- or need-based scholarships, specific medical requirements, siblings already attending the requested school, or specialized curricula (e.g., music, sport, foreign languages). Admission decisions for out-of-catchment students are made by the Academy rector, not schools.<sup>12</sup> Families may also opt for private schools, which are mostly publicly funded and follow the national curriculum, with optional religious instruction. Private schools charge relatively modest fees compared to other countries, making them affordable for many families: about 20% of lower secondary students attend private schools (see Table C1 in Appendix C).

The middle school choice process begins in March, when elementary headmasters inform parents of their assigned default school. Families may accept this assignment, request a transfer to another public school, or opt for a private school. The Academy director decides on transfer requests by mid-June, with final public school assignments communicated by the end of the month.

## 3. Empirical framework and data

Our primary objective is to identify the causal effect of place-based policies on student enrollment at the start of lower secondary education, which serves as a measure of demand and revealed preferences for neighborhood attributes. Since the influence of these policies on school choices is likely confounded by residential sorting, we leverage the discontinuity generated

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12. An Academy is a regional administrative division of the Ministry of National Education, and each of the 30 French academies is headed by a rector, who oversees schools, staff, and resource allocation.

by the Lamy reform, combining a local difference-in-differences design with two-way fixed-effects panel techniques to account for selection into treatment.

### 3.1. Empirical strategy

As illustrated in Figure 2 for the Paris region, we classify public middle schools into four distinct groups: (i) schools outside policy areas pre-reform but in policy areas post-reform ("entrant" schools), (ii) schools in policy areas pre-reform but not post-reform ("exiting" schools), (iii) schools in policy areas pre- and post-reform, and (iv) schools that remained outside the scope of the policy throughout the period. Under the assumption that school "entry" or "exit" is exogenous to families' school preferences, conditional on school-specific characteristics, we leverage the boundary changes induced by the reform to isolate and estimate the causal impact of place-based policies on school outcomes.

For pupil  $i$  assigned to catchment-area school  $d$  in school-year  $t$ , let  $Y_{idt}$  denote in turn a dummy for being enrolled at their catchment-area school, at another public school, and at a private school. Our two treatment variables are defined as follows:  $T_{d(i)}^{entry}$  is a dummy variable for the pupil's district school  $d$  being in a neighborhood that switched in the program post-reform, and  $T_{d(i)}^{exit}$  a dummy variable for  $d$  being in a neighborhood that switched out. We then estimate the following two-way fixed effects linear-probability models:

$$Y_{idt} = \alpha_1 + \beta_1 T_{d(i)}^{entry} \times \mathbb{1}_{t \geq 2014} + X_{it} \gamma_1 + Z_{dt} \delta_1 + \mu_{d(i)} + \mu_t + \epsilon_{idt}, \quad (1)$$

$$Y_{idt} = \alpha_2 + \beta_2 T_{d(i)}^{exit} \times \mathbb{1}_{t \geq 2014} + X_{it} \gamma_2 + Z_{dt} \delta_2 + \mu_{d(i)} + \mu_t + \epsilon_{idt}, \quad (2)$$

where  $X_{it}$  is a vector of observed pupil characteristics,  $\mu_t$  a year fixed effect, and  $\epsilon_{idt}/\epsilon_{idt}$  the error terms. Although we control for key observables at the pupil level, these estimates may still be biased by unobserved factors such as school quality in the catchment area. We address this concern via the catchment-area school fixed effect  $\mu_d$  and a vector of time-varying characteristics observed for this school and its local environment,  $Z_{dt}$ . In particular,  $Z_{dt}$  includes a yearly indicator of whether school  $d$ 's perceive extra funds, which captures any change in resources that may come from confounding school-based policies.

The  $\beta_1$  (respectively  $\beta_2$ ) parameter provides the causal impact of the policy on pupils' enrollment in schools that switched in (or out) of policy coverage, relative to schools serving counterfactual neighborhoods. This estimation assumes that, in the absence of the reform, enrollment patterns in both types of neighborhoods would have followed the same trend. Therefore,  $\beta_1$  captures the effect of the policy on schools in neighborhoods that became eligible for the policy, while  $\beta_2$  reflects the impact on schools serving neighborhoods that lost eligibility.  $\beta_1$  and  $\beta_2$  can be either positive or negative. Parents in low-income neighborhoods may perceive the policy as providing additional resources to

help their children perform better academically. On the other hand, policy designation may inadvertently carry a negative stigma by signaling the socio-economic challenges faced by residents in labeled neighborhoods, prompting parents to reassess the quality of schools in the presence of informational frictions. As a result, the "net" average treatment effect on school enrollment is theoretically ambiguous. If  $\beta_1$  is negative for the catchment-area school choice, it suggests that, on average, the policy's potential benefits are outweighed by the negative effects of territorial stigmatization. Conversely, if families reassess school quality positively after neighborhood disqualification, and if this reassessment outweighs the loss of public subsidies, then  $\beta_2$  should be positive. In this case, the policy's disqualification may lead to an increase in the catchment-area school enrollment due to the perceived enhancement in school quality.

The "net" effect of the policy on school enrollment is also likely to be heterogeneous across families. If parents are imperfectly informed about school quality in their catchment areas, they may adjust their school preferences following policy designation. In this context, we anticipate two potential patterns: (i) High-SES families may react more strongly than low-SES families, as the costs of changing schools (e.g., transportation, additional costs or fees) are relatively lower for higher-income families. These families may also exhibit greater flexibility in exploring alternative schooling options, demonstrate heightened sensitivity to changes in perceived school quality, or place a lower value on the academic support offered by the program; (ii) Well-informed families, such as teachers or individuals with more knowledge of the educational system, may have different reactions compared to other parents. These families might have a more accurate understanding of the implications of policy designation and may make more informed decisions about their children's schooling options, potentially leading to different patterns of enrollment behavior. These differences in reactions imply that the overall effect of the policy on school enrollment could vary depending on the socio-economic status, occupation, and information level of the families involved.

To evaluate whether policy designation has persistent effects over our period of analysis (2010-2019) and to test for the common-trend assumption needed for our difference-in-differences framework, we also adopt a linear panel event-study design and estimate the two complementary specifications:

$$Y_{idt} = \sum_{k=2010}^{2019} \beta_{1k} T_{d(i)}^{entry} \times \mathbb{1}_{t=k} + X_{it}\sigma_1 + Z_{dt}\lambda_1 + \mu_{d(i)} + \mu_t + \epsilon_{idt}, \quad (3)$$

$$Y_{idt} = \sum_{k=2010}^{2019} \beta_{2k} T_{d(i)}^{exit} \times \mathbb{1}_{t=k} + X_{it}\sigma_2 + Z_{dt}\lambda_2 + \mu_{d(i)} + \mu_t + \epsilon_{idt}, \quad (4)$$

where coefficients  $\beta_{1k}$  and  $\beta_{2k}$  now measure the effects of school entry and exit in year  $k$ .

Note that, in practice, defining treatment is more complex than simply classifying schools within entrant or exiting neighborhoods, since the exact boundaries of school catchment areas are not observed in France. Some schools located just outside policy boundaries may nonetheless enroll pupils from treated neighborhoods (if their catchment areas extend into treated zones), effectively participating in the program and potentially generating spurious “entry” or “exit” cases. To address this issue, we adopt a fuzzy approach and expand formerly treated zones by a small buffer (incorporating census blocks expanding over past policy areas) to avoid creating false entrants.<sup>13</sup> As this conservative approach may attenuate the treatment effect, Section 5.4 shows less conservative estimates, restricting pre-reform treated schools to those strictly within past policy boundaries.

### 3.2. Counterfactual neighborhoods

To evaluate the impact of French place-based policies, we exploit the discontinuities arising from the Lamy reform, comparing neighborhoods that transitioned into and out of the program with those that did not change status post-reform. Our analysis primarily exploits the income discontinuity, assuming that neighborhoods on either side of the poverty threshold are comparable, except for the policy designation. For neighborhoods entering the program for the first time, our counterfactual neighborhoods are formed by census tracts intersecting squares lying just above the poverty cut-off (i.e., in the 60-70% range of the reference income) (Insee, 2018c; Quantin and Sala, 2018). This 10 percentage-point window is dictated by data constraints, as confidentiality concerns limit access to the complete square grid used for delineating QPVs, which would have enabled the implementation of more sophisticated propensity-score matching techniques. Nevertheless, this data limit is not particularly restrictive for us, as Quantin and Sala (2018) demonstrated that the median income and employment rates of these areas evolved similarly to QPVs from 2007 to 2012. We perform analogous tests on our datasets to confirm that they are similarly balanced in terms of school and pupil characteristics (see Figure D1 in Appendix D). Figure 3 provides an illustration of the areas impacted by the reform in the Paris region, along with the counterfactual neighborhoods used for comparison.

Moreover, we refine the control group to address spatial spillovers, as parents changing schools post-reform are likely to choose alternatives nearby to minimize home-school distances. Those spatial spillovers prevent us from

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13. As census tract boundaries may change over time, we construct blocks with a stable geography across years (Zargouni et al., 2026).

fully exploiting the geographical discontinuities created by the Lamy reform, since comparing enrollment in schools on either side of the boundary assumes that close schools are unaffected by treatment, a condition unlikely to hold in our setting. To mitigate this issue, we exclude schools located too close to policy boundaries from the counterfactual group, specifically those within the upper envelope of census blocks that overlap with formerly or newly treated neighborhoods. Additionally, we conduct robustness checks to show that excluding counterfactual schools that are too distant from treated schools does not significantly affect our baseline results (see Section 5.3).

We adopt an analogous approach for neighborhoods transitioning out of the program that we compare with QPV areas that were treated pre-reform, within a 10% window around the poverty cut-off. Control schools are those in formerly- and still-labeled areas with a median income just below (50% to 60% of) the reference income. They are compared to schools serving exiting neighborhoods not too far from the poverty cut-off (60% to 70% of the reference income) to have comparable treated and control groups. Comparative statistics are provided in Figure D2 in Appendix D, confirming that the control and treatment groups are also balanced along key dimensions.

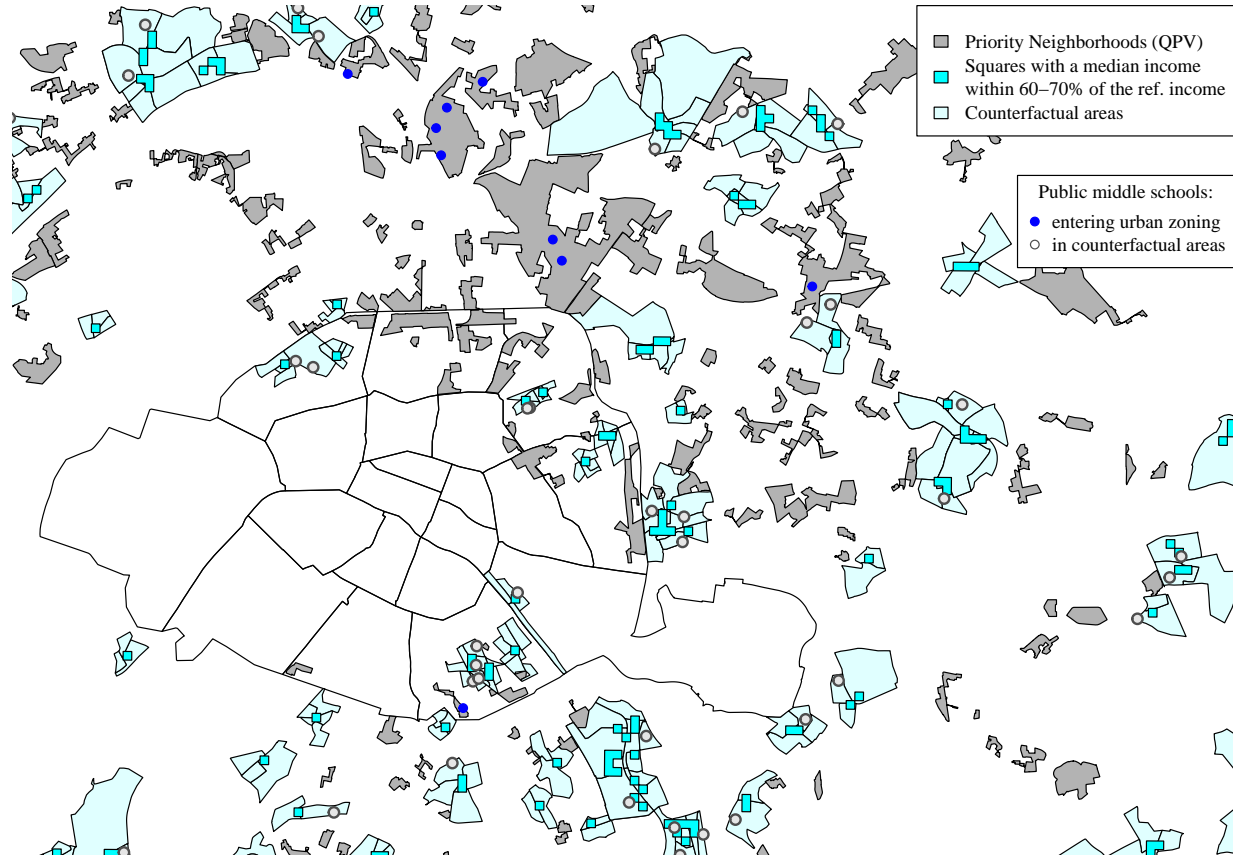


FIGURE 3. Treated vs Control middle schools in the Paris region

*Sources:* Authors based on *Annuaire de l'éducation* (DNE-MEN, 2019); Shapefiles from the ANCT (2010c, 2018a,b,c), the Direction de l'Urbanisme - Ville de Paris (2013), Quantin and Sala (2018), and Zargouni et al. (2026).

*Notes:* The dark grey areas denote the newly designated priority policy zones (QPV), while the blue dots mark the middle schools serving neighborhoods that switched into the program. The turquoise squares highlight poverty clusters with median incomes just above the poverty threshold (i.e., 60%-70% window of the reference income). The light-blue areas are the counterfactual census tracts, and the grey circles indicate the control middle schools located within those tracts.

### 3.3. Pupil data and descriptive statistics

We use exhaustive administrative data from multiple sources, described below from the lowest to the highest level of granularity. The core dataset is the *Base centrale Scolarité* (BCS hereafter) provided by the statistical service of the French Ministry of Education (DEPP-MEN, 2009-2019). The BCS consists of repeated cross-sections covering the universe of pupils enrolled in lower secondary education. We primarily focus on students entering middle school (6<sup>th</sup> grade) from 2010 to 2019, but we also use 8<sup>th</sup> grade cohorts as a placebo sample.

Additionally, we obtained restricted access to geo-coded data covering the entire population of pupils enrolled in lower secondary education in school years 2011, 2013, 2015, and 2017, which we use both for robustness checks and to measure the socioeconomic composition of neighborhoods (DEPP-MEN, 2011, 2013, 2015, and 2017)). Both datasets report each pupil's gender, age, and the occupation of the referent parent. They also identify the school attended in both the current and the previous academic year. For pupils entering lower secondary education, this allows us to observe both the middle school attended in 6<sup>th</sup> grade and the primary school attended in the previous year, which we use to infer catchment areas, whose precise delineations are unobserved in France. We define a pupil's district school as the public middle school geographically closest either to the pupil's primary school (using BCS data; see Figure F1 in Appendix F) or to the pupil's home address (using the geo-coded data).<sup>14</sup> We also observe whether each school is public or private, whether it receives additional resources under school-based policies in a given year, and its geographic coordinates, and whether it is open or closed in each year (DEPP-MEN, 2018; DNE-MEN, 2019). These features allow us to classify schools into four categories: newly treated, formerly (but no longer) treated, continuously treated, and never treated.

As public school avoidance strategies vary significantly across neighborhoods, especially in large metropolitan areas such as Paris where private schooling options are particularly numerous (Boutchenik, 2020), we compute the number of private schools within a 5 km radius (the sample median distance between the pupil's primary school and nearest private school) from the pupil's primary school or address, to control for such differences.

We also draw from the online application *Aide au Pilotage et à l'Auto-évaluation des Établissements* (APAE, DEPP-MEN, 2009-2020) and open data published by the French Ministry of Education (DNB, DEPP-MEN, 2009-2020), various indicators of middle school performance. These include the school average pass rates for progression from 6<sup>th</sup> to 7<sup>th</sup> grade, as well as exam pass rates at the end of 9<sup>th</sup> grade, when students take the *Diplôme National*

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14. Section 5 examines whether measurement error in this assignment affects our results.

*du Brevet* (DNB hereafter). The DNB exam comprises standardized tests in French, Mathematics, and History–Geography, supplemented by continuous assessment based on students' scores throughout 9<sup>th</sup> grade. Between the 2010 and 2019 school years, the average DNB pass rate across schools was approximately 84%, with schools located in neighborhoods targeted by the policy showing a pass rate of less than 80%, compared to over 86% for schools in non-targeted areas.

Armed with these data, we end up with a main sample of around 7.5 million 6<sup>th</sup> graders in 6,831 middle schools from 2010–2019. On average, over half (54%) of these students are enrolled at their catchment-area school, 24% at another public school, and 22% at a private school. Among this population, 570,320 students are treated (i.e., assigned to middle schools serving neighborhoods affected by the 2014 reform), and 418,517 form the control group (i.e., they are assigned to middle schools in counterfactual areas). Among the treated pupils, 26,022 are enrolled in neighborhoods receiving a policy label for the first time, and 544,298 in neighborhoods that saw their label removed (but still close to the eligibility threshold, i.e., within the 60%–70% range of the reference income). Further descriptive statistics appear in Table C1 in Appendix C.

We complement the pupil and school data with various Geographical Information Systems from the *Agence Nationale de la cohésion des territoires* (ANCT) providing the perimeters of neighborhoods eligible for the policy pre- and post-reform (i.e., former Tier-0 to Tier-3 zones and QPVs) (ANCT, 2010a,b,c, 2018a,b,c). Finally, Insee granted us access to confidential data on the median income and geographic location of census blocks lying just above the poverty eligibility threshold (i.e., within 60%–70% of the reference income) at the time of the Lamy reform implementation (Insee, 2018c; Quantin and Sala, 2018). We combine this information with publicly available data at the census-block and urban-unit levels to compute the poverty cut-offs used for policy designation and to construct our counterfactual neighborhoods (Insee, 2014c,d, 2013c, 2016c).

Figures D1 and D2 in Appendix D provide balancing tests across treated and counterfactual neighborhoods. Schools serving neighborhoods entering or exiting the policy are similar to counterfactual schools in most aspects. No significant differences in school resources (illustrated by the number of teachers) are observed between treated and counterfactual schools, confirming that schools do not receive financial support for participating in place-based educational programs. On average, students attending these schools are more socioeconomically disadvantaged than the overall student population. Our counterfactual groups of pupils are more comparable to the treated groups than to the overall population. Although some level differences remain, these will be fully washed out by catchment-area school fixed effects. As for trend differences, Section 4 presents falsification tests based on complementary event-study approaches.

#### 4. Average Treatment Effects

This section presents the average treatment effects from our local difference-in-differences models, exploiting the discontinuity design of the 2014 reform to increase the likelihood of parallel trends prior to the (re-)assignment of neighborhoods.

##### 4.1. Entry into policy zoning and pupil enrollment in middle schools

The results from the linear probability model assessing the impact of neighborhood labeling on school enrollment (Equation (1)) are presented in the top panel of Table 2.<sup>15</sup> Column (1) shows that, post-reform, public schools serving newly labeled neighborhoods experienced a significant 3.7 percentage point decline in enrollment relative to public schools serving comparable but never-labeled neighborhoods.<sup>16</sup> This pattern indicates that the policy designation prompted parents to avoid district schools situated in areas entering the program. Columns (2) and (3) further reveal that parents turned to other public schools (+3.9 pp) rather than private schools (where the coefficient is insignificant), although this average treatment effect masks substantial heterogeneity in avoidance behavior across socioeconomic groups of parents, studied in Section 6. Therefore, the general evidence suggests that the policy backfired, generating an unintended negative signal that harmed public schools serving neighborhoods newly incorporated into the program. While the policy's stated goal was to support pupils residing in these areas, its primary observable consequence was a marked "flight" out of their assigned public schools.

The top panel of Figure 4 depicts school enrollment over time (equation 3).<sup>17</sup> The pre-treatment coefficients are not significantly different from zero, supporting the parallel trend assumption required for our difference-in-differences set-up. Post-reform, the stigma effect associated with policy labeling shows up consistently. In the first year post-reform, the avoidance rate of public middle schools in newly designated neighborhoods increased by 4.1 pp relative to counterfactual areas (panel a). This penalty then peaked at 5 pp in the second and third years post-reform, before reducing slightly afterward. Given that the average 6<sup>th</sup>-grade cohort size was about

15. Results relying on a reduced set of controls (pupil characteristics only) appear in Table E1 of Appendix E and are highly similar.

16. Note that standard errors are clustered at the CA-school level. While clustering at the neighborhood level might appear more appropriate, the substantial overlap between former and new policy zones (see Figure 1) would require prioritizing overlapping zones and additional assumptions about the correlation structure. Clustering at the CA-school level is conceptually simpler, and since most treated areas contain only one public middle school, the two approaches yield very similar standard errors (see Table G1 in Appendix G).

17. The point estimates are presented in Table H1 of Appendix H.

TABLE 2. “Entry/Exit” into policy zoning and pupil enrollment

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.037*** (0.014)	0.039*** (0.014)	-0.002 (0.008)
$R^2$	0.173	0.125	0.187
No. obs	324,527	324,527	324,527
No. clusters	196	196	196
$T^{exit}$	0.006 (0.007)	-0.007 (0.007)	0.001 (0.005)
$R^2$	0.165	0.107	0.229
No. obs	684,448	684,448	684,448
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors (in parentheses) are clustered at the CA school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

196 students in targeted areas pre-reform, a 3.7 pp average decline post-designation translates to approximately seven fewer students per school yearly.

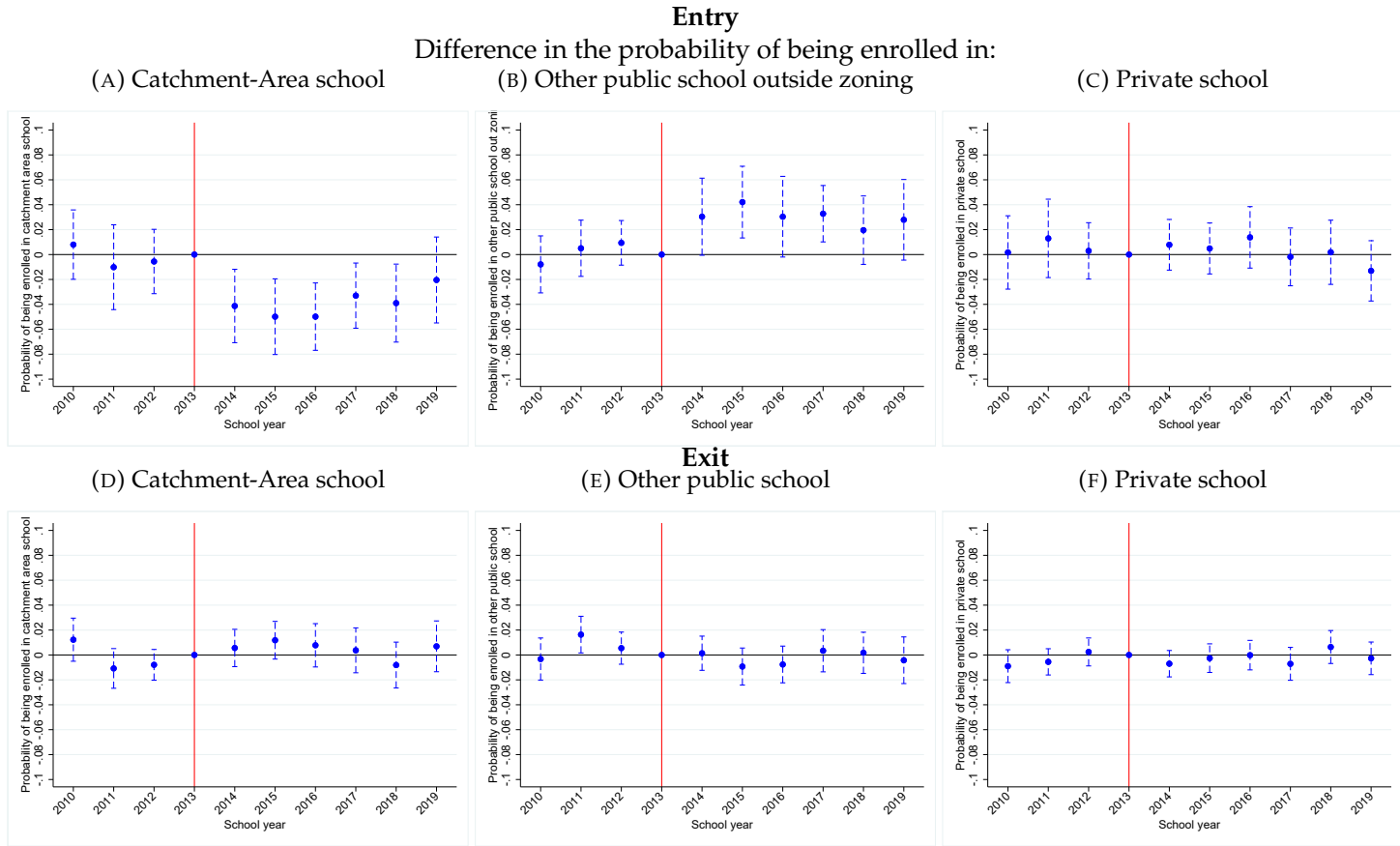


FIGURE 4. Zoning “Entry/Exit” and pupil enrollment - By year

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: The X-axis represents school years, with 2013 as the reference. The Y-axis shows the estimated  $\hat{\beta}_{1k}$  coefficients from equation (3) in the top panel and  $\hat{\beta}_{2k}$  coefficients from equation (4) in the bottom panel, with 95% confidence intervals. Standard errors are clustered at the CA-school level. Regressions include year and CA-school fixed effects, pupils’ characteristics (socioeconomic background, gender, and age), and school and catchment area time-varying controls (yearly indicator of extra funds from school-based policies, number of private schools within a 5 km radius of the pupil’s primary school).

This represents a substantial reduction, equivalent to the closure of an entire class over 2014-2019, as the stigma persists significantly 5 years post-labeling. Moreover, Figure 4 (panel b) and Table H1 of Appendix H show that policy designation led parents to shift to alternative public schools outside policy areas (see column 3), supposedly less disadvantaged.

The bottom panels of Table 2 and Figure 4 report the estimated coefficients from Equations (2) and (4). The estimates show no indication of stigma reversion once neighborhoods exit the policy scheme: post-reform enrollment gaps remain statistically indistinguishable from zero (see column 1 of Table H2 in Appendix H). The lack of any measurable rebound in enrollment following policy disqualification points to the presence of lock-in effects, whereby school-avoidance behaviors persist even after the policy has been revoked. This pattern suggests that the reputational consequences of policy labeling are particularly sticky and may prove difficult to undo, even once the underlying source of stigma has been removed.

#### *4.2. Pupil enrollment changes across policy rounds*

So far, we have compared first-time treated to never-treated areas, as well as no-longer treated to still-treated areas, on either side of the poverty threshold. However, we can also compare entrant neighborhoods to areas that have consistently been targeted by the policy both pre- and post-reform. Although this comparison is less causal than the one provided in Section 4.1, it provides insights into whether school choice behaviors were influenced differently across policy rounds. The top panels of Table 3 and Figure 5 provide the results of this comparison.<sup>18</sup>

We observe significantly higher levels of CA-school avoidance (3.4 pp) in newly-treated areas compared to formerly-treated ones, along with an increased likelihood of parents transferring their children to other schools within the public sector (+2 pp) or to private institutions (+1.3 pp). This suggests that the new policy has created a stronger territorial stigma than the previous rounds, possibly due to the public disclosure of QPV boundaries online. We estimate this "cost of transparency" to be approximately 6 fewer pupils per district school yearly post-designation. In this regard, the new policy influenced parental behaviors more than previous policy rounds, generating informational effects akin to the effects observed with the public dissemination of school performance metrics (Friesen et al., 2012; Koning and van der Wiel, 2013) or tax records (Perez-Truglia, 2020). Alternative ways of publicizing and disseminating policy-related information may help mitigate these unintended consequences in future policy rounds.

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18. The point estimates are provided in Table I1 of Appendix I.

Similarly, comparing areas that were de-labeled with those that were never labeled allows us to infer whether school enrollment actually reverts to its pre-designation level, aligning with the trends observed in never-treated neighborhoods. The bottom panels of Table 3 and Figure 5 present the results.<sup>19</sup> Post-reform, public schools in disqualified areas continued to experience heightened parental avoidance compared to never-treated areas, with enrollment shifting toward other public schools (+1.5 pp). Moreover, there is no evidence of convergence back to the enrollment levels observed in never-labeled neighborhoods, which provides further support to the stigma hysteresis underlined above.

TABLE 3. Pupil enrollment in first-time treated vs. formerly-treated neighborhoods (Top panel) and no-longer treated vs. never-treated neighborhoods (Bottom panel)

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
<i>T<sup>entry</sup></i>	-0.034** (0.013)	0.020* (0.011)	0.013* (0.008)
<i>R</i> <sup>2</sup>	0.178	0.106	0.221
No. obs	349,545	349,545	349,545
No. clusters	220	220	220
<i>T<sup>exit</sup></i>	-0.009 (0.006)	0.015** (0.006)	-0.006 (0.005)
<i>R</i> <sup>2</sup>	0.167	0.114	0.217
No. obs	4,996,293	4,996,293	4,996,293
No. clusters	3,525	3,525	3,525
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors in parentheses are clustered at the CA-school level. Pupils' characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil's primary school. For clarity, the intercept and the coefficients on these controls are not reported.

19. The point estimates are provided in Table I2 of Appendix I.

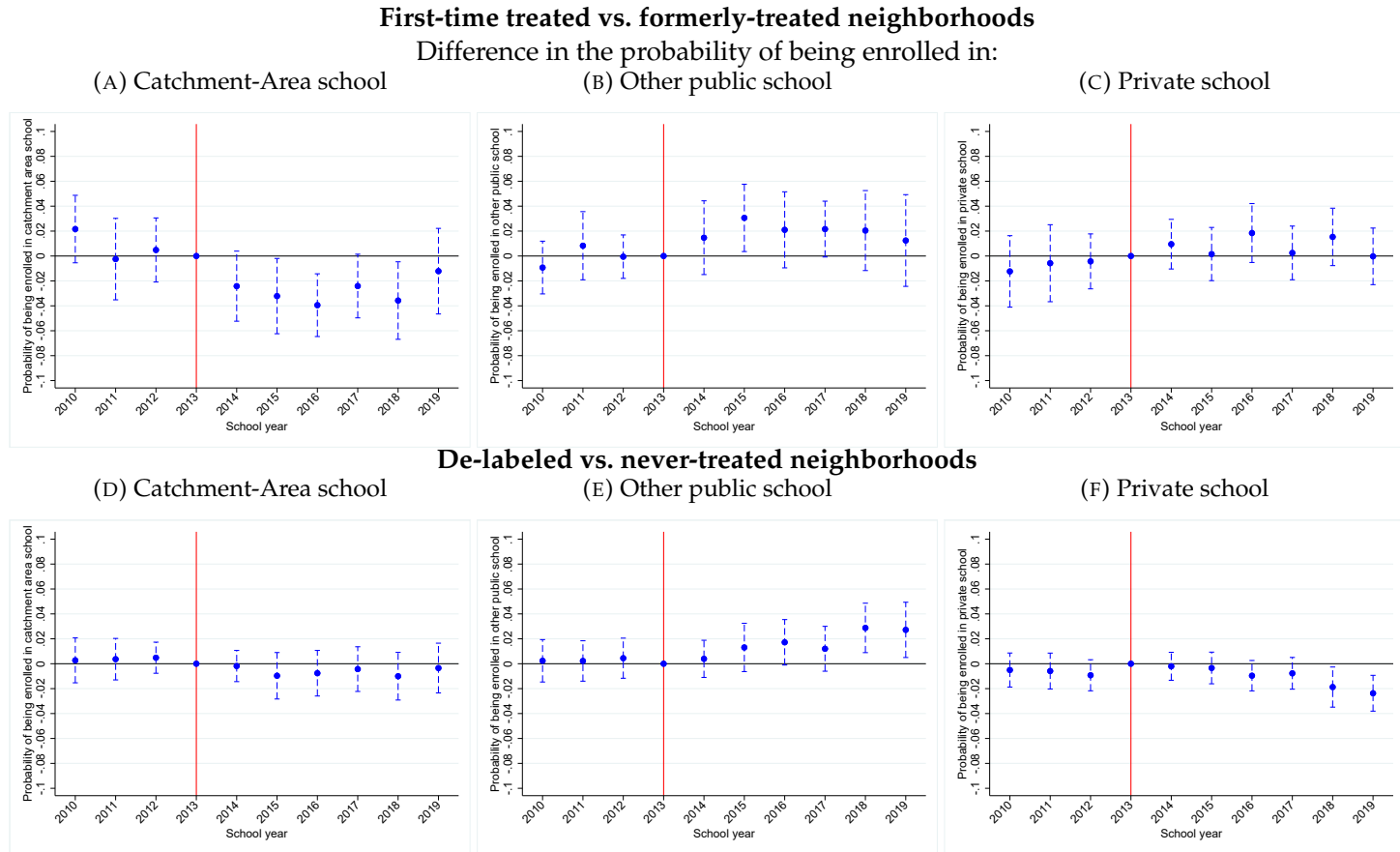


FIGURE 5. Pupil enrollment in first-time vs. formerly-treated / de-labeled vs. never-treated neighborhoods

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: The X-axis represents school years, with 2013 as the reference. The Y-axis shows the estimated difference between schools in first-time vs. formerly-treated neighborhoods in the top panel and between schools in de-labeled vs. never-treated neighborhoods in the bottom panel, with 95% confidence intervals. Standard errors are clustered at the CA-school level. Covariates include pupils' characteristics (socioeconomic background, gender, and age) and school and catchment area time-varying controls (yearly indicator of extra funds from school-based policies, number of private schools within a 5 km radius of the pupil's primary school).

## 5. Robustness checks

Before turning to heterogeneous enrollment responses and the underlying causal mechanisms, we first assess the robustness of the average treatment effects reported above. This section examines the sensitivity of our findings to variations in (i) the construction of school catchment areas, (ii) the modeling of school choice behavior, (iii) a spatial discontinuity design, and (iv) alternative definitions of treatment.

### 5.1. Construction of school catchment-areas

For now, given that the exact boundaries of catchment areas are unknown, we have assigned each pupil to the public middle school nearest to their primary school. However, this approach may lead to misassignments if the actual catchment-area school is not the closest one or if two public middle schools are equidistant from the primary school. Our point estimates could be biased if these errors are not random. However, as long as misclassification of catchment areas is exogenous, there should be no systematic estimation bias.

We address this issue by using geo-coded datasets that enable us to recover catchment areas based on the shortest distance between each pupil's home address and all public middle schools.<sup>20</sup> Unfortunately, as these data are available only in odd years from 2011 to 2017, we cannot replicate the event-study analyses provided above and conduct a thorough analysis of pre-trends. Nonetheless, as shown in Table 4, results appear similar to those based on pupils' primary schools: zoning entry significantly reduces the likelihood that parents choose their catchment-area school (compared to parents in counterfactual areas), leading to a 3.7 pp average decrease in district-school enrollment, which closely aligns with our baseline estimates. This definition of catchment areas confirms that parents primarily shift to other public schools (with a point estimate at +4.2 pp, similar to our baseline estimate) rather than the private sector (where the coefficient is still insignificant). Consistent with previous findings, zoning exit does not change the likelihood that parents enroll their children in disqualified areas relative to parents in counterfactual areas.

To further test whether assigning public schools to pupils on the basis of the distance to their primary school raises concerns, we check if some public schools end up with zero enrollment following our assignment rule. This is the case for 72 public middle schools out of 5,125, of which only 2 are in the "entry" sample and 6 in the "exit" sample. Excluding these observations, which presumably have the wrong catchment area, has almost no effect on

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20. Maugis and Touahir (2018) show that this procedure provides a fairly accurate approximation of catchment areas.

TABLE 4. Zoning “Entry/Exit” and pupil enrollment - Geo-coded data

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.037** (0.015)	0.042*** (0.012)	-0.004 (0.011)
$R^2$	0.111	0.097	0.137
No. obs	131,187	131,187	131,187
No. clusters	197	197	197
$T^{exit}$	0.002 (0.009)	-0.002 (0.010)	0.000 (0.005)
$R^2$	0.128	0.082	0.159
No. obs	276,829	276,829	276,829
No. clusters	421	421	421
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Fichiers géoréférencés des élèves du secondaire DEPP-MEN (2011, 2013, 2015, and 2017).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

the results: the point estimates are identical (to two or three decimal places) and the significance levels are unaffected.<sup>21</sup>

### 5.2. Multinomial analysis

In previous analyses, middle-school enrollment was modeled using separate linear probability regressions for three independent dichotomous outcomes: enrolling in the catchment-area school, another public school, or a private school. Given the potential limitations of this approach, we estimate a multinomial model in which parents choose among these three alternatives

21. These results are available upon request. Furthermore, Boutchenik (2020) identifies avoidance patterns similar to ours for three case studies based on district maps that could be recovered.

(see Appendix J for model details). The multinomial estimates, presented in Table J1, closely align with those from the linear probability models. The likelihood that parents enroll their children in another public school rather than the catchment-area school increases by 4.2 pp post-reform compared to counterfactual areas (column 1 of the top panel in Table J1). Furthermore, we observe no significant deviations to the private sector on average, nor enrollment rebounds in disqualified neighborhoods compared to counterfactual areas (bottom panel of Table J1).

### *5.3. Spatial discontinuity approach*

So far, our analysis has primarily leveraged the income discontinuity introduced by the Lamy reform, ensuring that neighborhoods on either side of the poverty threshold are comparable in all respects but policy designation. We have deliberately avoided fully exploiting spatial discontinuities arising from the reform (beyond identifying areas entering or exiting the policy), due to concerns over spatial spillovers. Given that parents changing schools post-reform are likely to opt for nearby alternatives to minimize home-school distances, close schools outside policy boundaries are exposed to proximity-induced spillovers. To mitigate this concern so far, we have excluded from our control group schools located in the upper envelope of census blocks overlapping with treated areas. However, schools that are located far from policy-targeted areas may also be less comparable, even though this concern is not evident given our balancing tests. To address this potential issue, we provide additional estimates that exclude schools beyond a 3 km buffer (the average separation between public middle schools in French municipalities) around policy boundaries from our counterfactual groups.

Table 5 displays the results from this distance-based approach. For areas entering into the program for the first time, we observe a slightly attenuated stigma effect compared to our baseline estimates (supporting the hypothesis that nearby schools benefit from positive spillovers triggered by parents switching to them). However, the point estimates remain statistically indistinguishable from our main results. Similarly, for disqualified areas, we do not detect any significant rebound effects, consistent with our baseline estimates. Overall, our main findings remain robust under this spatial-discontinuity approach.

### *5.4. Definition of treatment*

As discussed in Section 3.1, defining treatment is not straightforward in practice because the exact boundaries of school catchment areas are unknown. Schools located just outside policy borders may enroll pupils from treated neighborhoods if their catchment areas extend into the policy zone. Such cases can generate school-level treatment misclassification, depending on how

TABLE 5. Zoning “Entry/Exit” and pupils’ enrollment - Restricting to counterfactual areas less than 3km from treated areas

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.027** (0.012)	0.027*** (0.010)	0.000 (0.009)
$R^2$	0.170	0.099	0.192
No. obs	179,551	179,551	179,551
No. clusters	109	109	109
$T^{exit}$	0.006 (0.007)	-0.006 (0.007)	0.001 (0.004)
$R^2$	0.162	0.107	0.230
No. obs	672,348	672,348	672,348
No. clusters	426	426	426
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

“entry” and “exit” are defined. To limit false “entries,” which could bias our estimates, we have so far adopted a conservative “fuzzy” definition: schools situated in census blocks overlapping formerly treated neighborhoods are not considered entrants, as they likely served pupils who already benefited from the policy in previous rounds. This approach minimizes the risk of overstating treatment effects. We now complement this strategy by applying a sharper boundary-based definition of pre-reform treatment, which allows for potential false entrants and thus yields less conservative estimates. Table K1 in Appendix K shows that the results remain highly robust, with point estimates that are statistically indistinguishable from our baseline findings.

A second concern regarding treatment definition arises from potential heterogeneity in informational frictions across formerly treated tiers before 2014. Tier-1 to Tier-3 zones were relatively well known, notably because

employers could claim low-wage tax credits when hiring residents from these areas. By contrast, other Tier-0 zones were primarily known only to local authorities, which may have led residents to believe that these areas had not been labeled prior to the reform. Consequently, parents whose children had not previously benefited from educational programs may have perceived the policy as newly introduced in their neighborhood, even when the area had in fact been treated before. Such differences in perceived labeling could have induced parents in Tier-0 zones other than Tiers 1-3 to reassess school quality differently from those in Tier-1 to Tier-3 areas. To test the robustness of our results against this conjecture, we redefine entrant/exiting neighborhoods to include only Tier 1-3 areas that transitioned in and out of the policy, leaving aside other Tier-0 zones. Table K2 in Appendix K displays the related point estimates, which remain totally consistent with our baseline estimations.

All these robustness checks reinforce our central conclusion: the policy label stigmatizes public middle schools in designated neighborhoods, and this stigma persists even after policy disqualification. The inability to reverse parental beliefs about school quality post-delabeling suggests that the initial negative signal associated with policy labeling had a lasting impact, shaping school reputation and choices beyond the policy's active period.

## 6. Heterogeneous Treatment Effects

We now examine whether the policy reform has had heterogeneous effects on school choice, considering factors such as family background, informational frictions regarding school quality, differences in school quality, and other potential sources of variation, including pupil gender, availability of additional school-funding from other policies, and the distance to private school options nearby.

### 6.1. *Heterogeneity by Socioeconomic Status*

We begin by exploring whether families' responses to re-zoning differ by socioeconomic background. High-SES parents face fewer financial constraints, making them more inclined to opt for alternative schooling options, particularly private schools, in response to policy changes. Likewise, parents with older kids or specific occupations (e.g., teachers) may encounter fewer informational frictions due to their preexisting knowledge of school quality, potentially leading to distinct reactions compared to other parents. To examine this heterogeneity, we categorize parental occupations into three broad SES groups: High, Medium, and Low.<sup>22</sup> We then employ a triple-difference

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22. High SES include business managers, engineers, executives from the public and private sectors, independent/creative/intellectual professions, white-collar workers, teachers,

approach, interacting all explanatory variables from Equations (1) and (2) with parental socioeconomic status or occupation.

The top panel of Table 6 suggests that policy designation magnified school avoidance across all types of families. However, strategic behaviors vary by socioeconomic status. Higher-SES families were significantly more likely to enroll their children in private schools (+3.6 pp relative to unlabeled neighborhoods), while medium and low-SES parents were less inclined to do so (-3.7 and -5.1 pp respectively relative to high-SES families). In contrast, low-SES households exhibited a higher propensity to choose another public school (+4.7 pp). These findings suggest that policy designation triggered a "rich flight" to private schools, reinforcing school enrollment disparities and deepening social segregation between the public and private sectors. The bottom panel of Table 6 further indicates that lock-in effects make these patterns particularly persistent, as only low-SES families seem more prone to change their beliefs and turn back to district schools after designation loss (+2.5 pp compared to the wealthiest socioeconomic groups, see column 1).

Table L1 in Appendix L, which examines the specific response of teachers and professors to the reform, indicates that they were significantly less reactive than other parents. These groups likely had a more accurate pre-reform knowledge of school quality, being less affected by any new (whether positive or negative) information associated with policy changes. This finding highlights the role of imperfect information in shaping the territorial stigma induced by policy designation.

## 6.2. *Heterogeneity over secondary education*

To further examine the role of informational frictions, we conduct a placebo analysis following the approach of Boutchenik (2020). The idea is to test whether parents whose children were already enrolled in lower secondary school at the time of the reform adjusted their school choices afterward. If imperfect information at middle school entry is a key driver, because parents must choose a school without firsthand experience, then parents already familiar with their child's school should have little incentive to update their beliefs after the reform. Given that lower secondary education in France lasts four years (from 6<sup>th</sup> to 9<sup>th</sup> grade), we identify pupils entering the 8<sup>th</sup> grade as our placebo cohort. These pupils had already completed two years in lower secondary education, providing their parents with substantial knowledge about school quality. We then estimate the probability that parents transferred

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intermediate professions, technicians, clergy, and retired executives/intermediate professions. Medium SES includes farmers, artisans, shopkeepers, public or private employees, police officers or military personnel, and retired farmers/artisans/traders/managers. Low SES covers blue-collar, students, the unemployed/unoccupied, and missing parental occupation (about 4% of the sample).

TABLE 6. Zoning “Entry/Exit” and pupil enrollment - By SES

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.044* (0.024)	0.009 (0.017)	0.036* (0.020)
SES (ref.=High)			
Medium SES $\times T^{entry}$	0.011 (0.017)	0.025 (0.020)	-0.037* (0.019)
Low SES $\times T^{entry}$	0.005 (0.022)	0.047** (0.020)	-0.051** (0.023)
$R^2$	0.185	0.137	0.207
No. obs	324,527	324,527	324,527
No. clusters	196	196	196
$T^{exit}$	-0.009 (0.009)	0.011 (0.009)	-0.001 (0.009)
SES (ref.=High)			
Medium SES $\times T^{exit}$	0.011 (0.011)	-0.023** (0.010)	0.011 (0.011)
Low SES $\times T^{exit}$	0.025** (0.010)	-0.018* (0.011)	-0.006 (0.009)
$R^2$	0.180	0.128	0.254
No. obs	684,448	684,448	684,448
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

their child to another school between 7<sup>th</sup> and 8<sup>th</sup> grade in response to the reform.

The results, presented in Table 7, show no effect of the policy on 8<sup>th</sup>-grade enrollment. Parents of students already enrolled in lower secondary

TABLE 7. Zoning “Entry/Exit” and pupil in 8<sup>th</sup> grade (placebo cohort)

	Probability of being enrolled in:		
	Previous Public School	Other Public School	Private School
$T^{entry}$	-0.000 (0.005)	-0.001 (0.004)	0.001 (0.002)
$R^2$	0.010	0.009	0.006
No. obs	255,980	255,980	255,980
No. clusters	198	198	198
$T^{exit}$	-0.003 (0.003)	0.003 (0.002)	0.000 (0.001)
$R^2$	0.011	0.011	0.007
No. obs	497,360	497,360	497,360
No. clusters	437	437	437
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . Previous Public School refers to the middle school of enrollment in 7<sup>th</sup> grade. Standard errors, reported in parentheses, are clustered at the previous-public-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the previous school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s previous school. For clarity, the intercept and the coefficients on these controls are not reported.

education did not react to policy (de-)labeling, consistent with the idea that they faced far fewer informational frictions. This finding further strengthens our interpretation that imperfect information played a central role in shaping the territorial stigma triggered by policy designation. The average treatment effects identified in earlier sections are thus unlikely to be statistical artifacts; instead, they reflect strategic parental behavior at the point of entry into lower secondary education, when informational frictions about school quality are at their highest.

### 6.3. Other sources of heterogeneity

We now leverage pre-reform school-level average pass rates at the DNB exam, which marks the completion of lower secondary education, to assess whether

parents who changed their child's enrollment post-reform opted for schools of supposedly higher quality. While we acknowledge that stratifying the analysis by school performance may raise endogeneity concerns, investigating this heterogeneity with appropriate caution provides valuable insights into whether policy designation triggered a "flight-to-quality", encouraging parents to seek more information on school performance and opt for schools that outperform the catchment-area school in response to policy labeling.

Table M1 in Appendix M reports estimates in which all variables are interacted with the difference between the average DNB success rates of the enrolled school and the catchment-area school (when they differ post-reform), measured in 2013, prior to policy designation.

The interaction terms enable us to examine whether changes in enrollment decisions were influenced by the relative academic performance of schools pre-treatment (i.e. in 2013). The results indicate that a pre-reform performance gap disadvantaging the catchment-area school relative to another public middle school located outside a designated area is associated with a greater likelihood (+4.5 pp) of enrolling children in this alternative school (see column 2). This provides empirical support for a "flight-to-quality" hypothesis, suggesting that the reform prompted parents to compare the performance of public middle schools across labeled and unlabeled neighborhoods.

Table M2 in Appendix M further indicates that the probability of avoiding the catchment-area school rises even more (by an additional 3 pp) when the closest private school is located within a distance shorter than the median distance in our sample, and corroborates that this "flight-for-quality" is more accentuated within a highly competitive local environment.

Finally, we also examine heterogeneity across other dimensions. We find no robust evidence of differential effects based on gender (Table M3), or the receipt of extra funding from school-based policies (Table M4).

## 7. Neighborhood composition effects

To further explore the territorial stigma hypothesis, we extend our analysis by assessing the impact of place-based policies on local outcomes beyond school enrollment. We investigate the policy reform's impact on four key dimensions that may reinforce educational disparities through local socio-economic composition effects: changes in house prices, variations in the social composition of neighborhoods, school performance evolutions, and labor market dynamics. By examining these additional channels and mechanisms, we seek to offer a more comprehensive understanding of the broader consequences of policy designation or policy disqualification on the perception of neighborhood attributes, beyond parental beliefs on school quality and reputation.

### 7.1. Policy designation and housing prices

As housing prices serve as a powerful and quickly responsive indicator of location desirability, including perceived school quality (Fack and Grenet, 2010; Collins and Kaplan, 2017), we test whether policy designation or policy disqualification impacted the house price gradient across treated and counterfactual neighborhoods. To do so, we leverage a rich data source on house prices in France, the *Demande de Valeurs Foncières* (DV3F hereafter), which is compiled by the Cerema, a public organization associated with the French Ministry for Housing (DGFIP-DGALN-Cerema, 2010-2019). The dataset records all second-hand housing transactions from 2010 onward, and provides detailed information on properties sold, including both multi-family properties (apartments) and single-family homes (houses), and contains key variables such as the sale price, sale date, and dwelling characteristics (e.g., living area, construction period, number of rooms, and additional features like balconies, terraces, cellars, and garages). The geographic coordinates of the parcels sold allow us to assign each transaction to one of our four sets of neighborhoods.<sup>23</sup>

In line with our school enrollment analysis, we run the two following hedonic regressions:

$$P_{inmt} = \sum_{k=2010}^{2019} \beta_{1k} T_{n(i)}^{entry} \times \mathbb{1}_{t=k} + X_i \sigma_i + \mu_{n(i)} + \mu_m + \mu_t + \epsilon_{inmt}, \quad (5)$$

$$P_{inmt} = \sum_{k=2010}^{2019} \beta_{2k} T_{n(i)}^{exit} \times \mathbb{1}_{t=k} + X_i \sigma_i + \mu_{n(i)} + \mu_m + \mu_t + \epsilon_{inmt}, \quad (6)$$

where  $P_{inmt}$  is the price (in log) per square meter of surface of property  $i$ , sold in neighborhood  $n$ , month  $m$ , and year  $t$ , and  $X_i$  the vector of dwelling characteristics.<sup>24</sup> Coefficients  $\beta_{1k}$  and  $\beta_{2k}$  measure the log-point difference in property prices between entrant/exiting neighborhoods and their counterfactual analogs over year  $k$ . To address endogeneity issues, we include neighborhood fixed effects  $\mu_n$ , account for price seasonality with month fixed effects  $\mu_m$ , and capture housing market cycles with year fixed effects  $\mu_t$ . As the Lamy law was enacted on the 21<sup>st</sup> of February 2014, we start treatment from March 2014 and provide event studies and difference-in-differences estimates with years rolling from March to February.

Figure 6 presents our event studies for multi-family property sales, with detailed point estimates reported in Tables N1 and N2 in Appendix N, along

23. Note that the dataset does not include house transactions for three departments in northeastern France (the *Haut-Rhin* and *Bas-Rhin*, and the *Moselle*) due to specific notary regulations.

24. We follow the Insee methodology described in Cailly et al. (2019).

with corresponding estimates for single-family homes. Following the reform, newly designated neighborhoods experienced a significant, substantial, and persistent decline in house prices relative to counterfactual neighborhoods, reaching a relative depreciation of up to  $(\exp^{0.068} - 1) \times 100 = 7$  pp by 2019 (Table N1, column 1). This price discount is comparable to that documented by Andersson et al. (2026) for neighborhoods classified as “vulnerable” in Sweden. In contrast, consistent with patterns observed in school choice responses, we find no significant effect of policy disqualification on housing price gradients. Neighborhoods that lost their label did not experience any subsequent price appreciation compared to still-labeled areas, a finding aligned with Yamagishi and Sato (2025), who demonstrate that historically stigmatized areas in Japan continue to face persistent land price discounts.

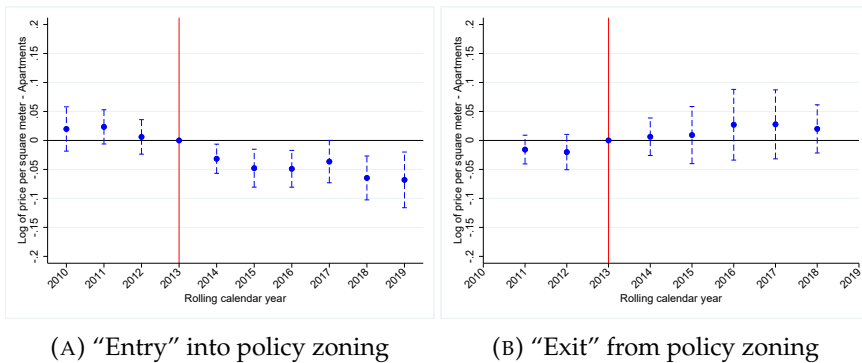


FIGURE 6. Zoning “Entry/Exit” and differences in multi-family property prices

Sources: DV3F 2025-1, DGFIP-DGALN-Cerema (2010-2019).

Notes: The X-axis displays rolling calendar years from March to February (March 2013 to February 2014 is the reference). The Y-axis displays the  $\hat{\beta}_{1k}$  and  $\hat{\beta}_{2k}$  drawn from estimating equation (5) and (6) respectively, with 95% confidence intervals. Standard errors are clustered at the neighborhood level. Regressions include property characteristics (property type, floor level, construction year, floor area, number of bathrooms, presence of a cellar, balcony or terrace, and garage), neighborhood fixed effects, month, and rolling years dummies. In sub-figure (b), the estimation includes third-order polynomial time trends to wash out pre-trends observed in the control group (see Figures N1 and N2 displayed in Appendix N). Two additional degrees of freedom are then needed, resulting in no point estimates for 2010 and 2019.

## 7.2. Social recomposition of neighborhoods

Given the substantial housing price discount observed in newly eligible areas compared to counterfactual neighborhoods, it is highly likely that these areas experienced changes in their social composition. Low-income families, who are more likely to live in social housing, have limited mobility relative to higher-income residents, thus reducing the likelihood of substantial “Tiebout

flight".<sup>25</sup> As such, we expect Low-SES families to have resorted across schools by requesting opt-out derogations rather than by relocating. The catchment-area rule, which imposes limitations on exceptions granted to parents based on student characteristics, has probably mitigated the observed decline in school enrollment post-reform. This suggests that the stigma estimated so far is likely underestimated, as the rule may have prevented more Low-SES pupils from avoiding their catchment-area schools, thus reducing the potential negative impact on school enrollment that could have otherwise been observed. However, if policy areas become more affordable, middle- and high-income families who can afford to relocate may be replaced by lower-SES households. This section examines shifts in the socioeconomic status of parents residing in treated and control areas to explore the dynamics of neighborhood sorting and the broader consequences of policy designation on urban segregation.

To investigate those socio-composition effects, we exploit the geo-coded pupil dataset already leveraged in Section 5 (available every odd year from 2011 to 2017), extending it to the full population of secondary education pupils (i.e., pupils from 6<sup>th</sup> to 9<sup>th</sup> grades). Given that this data source covers the universe of households with pupils of secondary education age in France, we expect to provide a comprehensive view of the key socio-composition trends at play in neighborhoods entering or exiting the policy scheme and their counterfactual areas. To accurately assess compositional changes within these neighborhoods, as for housing prices, we control for factors that may influence residential choices beyond policy designation through neighborhood and time-fixed effects.

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25. According to Sala (2018), 74% of QPV residents live in social housing, compared to 16% in other neighborhoods within the same urban units.

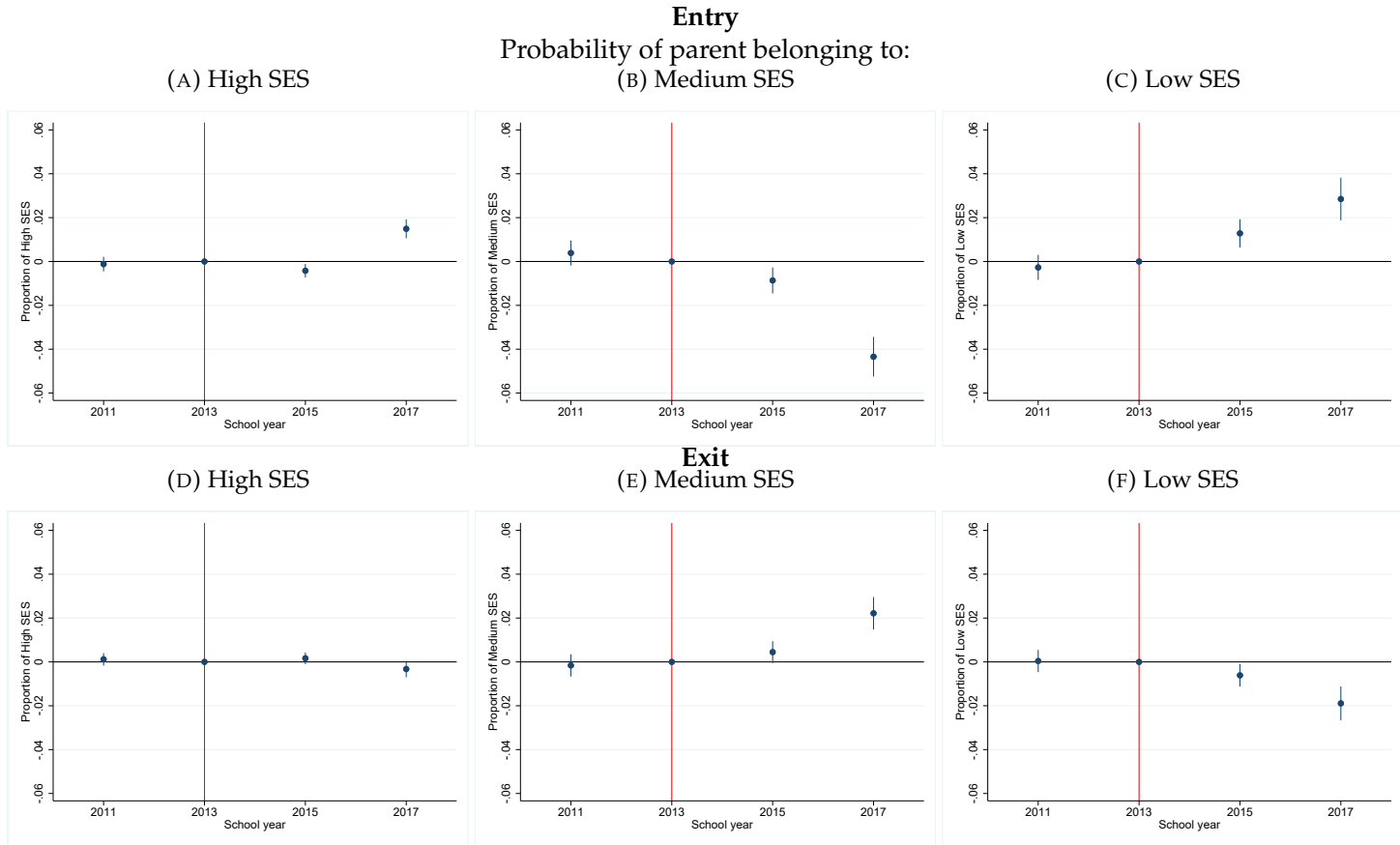


FIGURE 7. Zoning “Entry/Exit” and difference in social composition

Sources: Fichiers géoréférencés des élèves du secondaire, DEPP-MEN (2011, 2013, 2015, and 2017).

Notes: The X-axis displays school years, with 2013 as the reference. The Y-axis represents the estimated difference in the probability that the reference parent belongs to a given SES group. Estimates compare schools located in newly designated neighborhoods (top panel) and in neighborhoods that lost their designation (bottom panel) with their respective counterfactual counterparts. Regressions include neighborhood fixed effects. 95% confidence intervals are plotted with standard errors clustered at the neighborhood level.

Figure 7 presents our main findings, with point estimates reported in Tables O1 and O2 in Appendix O. We document significant shifts in neighborhood composition post-reform, both for areas entering and exiting the program. In newly designated neighborhoods, the share of Low-SES residents with children of secondary education age increased by 1.3 pp in the first year post-reform and by 2.8 pp in the third year, relative to counterfactual areas (panel c). Conversely, the proportion of Medium-SES residents declined by 0.9 pp in 2015 and by 4.3 pp in 2017 (panel b). While the share of High-SES residents initially fell slightly in 2015, it subsequently rose by 1.5 pp in 2017 relative to 2013 levels (panel a). These patterns suggest that policy designation contributed to a process of "reverse gentrification" characterized by the outmigration of the wealthiest residents and an influx of lower-income populations, consistent with the findings of Govind et al. (2024) for Denmark's publicized "ghettos".

Neighborhoods exiting the policy scheme also experienced shifts in their social composition, though these changes were less pronounced, echoing the hysteresis and path dependence observed in school enrollment. In disqualified neighborhoods, the share of Medium-SES residents with children of secondary school age increased by 0.4 pp in 2015 and 2.2 pp in 2017 relative to counterfactual areas (panel e), while the share of Low-SES residents declined by 0.6 pp in 2015 and 1.9 pp in 2017 (panel f). The proportion of High-SES residents showed only a negligible rise and subsequent decline in 2017 (panel d). Overall, neighborhoods that lost the policy label experienced a slower and less pronounced shift toward a more balanced population mix.

These asymmetric effects between newly labeled and disqualified neighborhoods provide further evidence supporting the territorial stigma hypothesis, underscoring its long-term role in signaling neighborhood attributes and acting as a coordination mechanism that influences residential mobility, re-sorting dynamics, and urban segregation.

### *7.3. Place-based policies and academic achievement*

We now turn to the core question of whether policy rezoning influenced pupil academic achievement across treated and counterfactual neighborhoods. On one hand, the provision of increased individualized support to pupils residing in policy-targeted areas is expected to enhance pupil achievement, whereas the withdrawal of such support in disqualified areas may negatively impact educational outcomes. On the other hand, the socio-compositional shifts triggered by the reform can also affect school performance through peer effects. In newly designated neighborhoods, academic outcomes may decline due to the concomitant departure of higher-achieving peers and the influx of lower-income pupils. Conversely, if such movements result in more homogeneous peer groups benefiting from enhanced educational support,

the new policy may yield positive effects on the academic achievement of disadvantaged pupils.

To assess which effects dominate, we estimate the overall impact of the policy reform on pupil performance using two measures of educational achievement observed at the school level.<sup>26</sup> The first measure is the pass rate from 6<sup>th</sup> to 7<sup>th</sup> grade, which provides a short-term indicator of 6<sup>th</sup> graders' academic success. The second measure is the average share of students passing the DNB exam, which offers a longer-term assessment of academic achievement at the end of lower secondary education.

Figure 8 presents an event-study analysis of the estimated gap in pass rates from 6<sup>th</sup> to 7<sup>th</sup> grade.<sup>27</sup> Although a very small and barely significant positive effect appears in the second year post-reform (panel a), there is no significant difference on average between schools in newly designated policy areas and their counterfactual counterparts (see Table P1, column 1, in Appendix P). In contrast, schools serving disqualified neighborhoods experienced a significant and sustained decline in their relative educational achievement ( $-1.25$  pp on average; see Table P1, column 2) over the post-reform period (panel b). Hence, in the short run, newly designated policy areas show only modest gains for 6<sup>th</sup> graders, while schools in disqualified neighborhoods experience a marked and persistent decline in academic achievement.

Figure 9 reports the estimates for our second measure of school performance, the average success rate at the DNB exam. Although these estimates are very imprecise, we again find no significant differences in academic achievement between newly designated neighborhoods and their counterfactual areas (panel a). In contrast, the loss of support following policy disqualification had detrimental consequences for students: even though the average post-reform effect is not statistically significant over the entire post-reform period, the deterioration is visible and statistically significant in several individual post-reform years (panel b).

Overall, there is no evidence of significant differences in academic achievement between newly designated neighborhoods and their counterfactual areas, neither shortly after students entered 6<sup>th</sup> grade nor by the time they completed 9<sup>th</sup> grade. By contrast, disqualified neighborhoods experienced a sustained deterioration in school performance over both the short and long term, suggesting that these areas struggled to attract higher-achieving pupils back, even after the initial stigma was lifted.

Importantly, these effects on academic achievement likely stem from two intertwined mechanisms: shifts in peer composition resulting from increased segregation across schools and neighborhoods, and the withdrawal

26. Recall that pupil datasets are not panelized; hence, we cannot track individual pupil performance over time.

27. See also Tables P1-P4 in Appendix P for detailed estimation results.

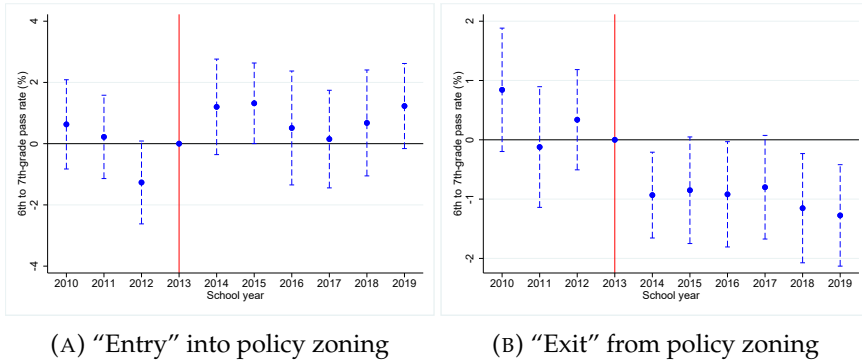


FIGURE 8. Zoning “Entry/Exit” and difference in school performance at the start of lower secondary education

Sources: APAE, DEPP-MEN (2010-2019).

Notes: The X-axis displays school years (with 2011 standing for the 2011–2012 academic year, and so on), taking 2013 as the reference year. The Y-axis reports the estimated gap in pass rates from 6<sup>th</sup> to 7<sup>th</sup> grade for schools located in newly labeled neighborhoods (panel a) and in disqualified neighborhoods (panel b), relative to their counterfactual counterparts. Estimations are carried out at the school level, with control for school and year fixed-effects, a dummy for school benefiting from extra funds from school-based policies, and the number of private schools within a 5 km radius of the pupil’s primary school. 95% confidence intervals are plotted with standard errors clustered at the school level.

of subsidies following policy disqualification, two channels that we cannot fully disentangle. However, the asymmetry observed between “Entry” and “Exit” effects helps to infer the underlying dynamics. The absence of strong gains in newly labeled neighborhoods, coupled with the persistent losses observed after disqualification, suggests that policy subsidies and additional resources may temporarily offset the negative peer effects associated with the stigma of being designated. In other words, educational incentives can initially mitigate the reputational costs attached to zoning labels. Yet over time, these compensatory effects tend to fade, while the social and symbolic dimensions of territorial stigma and their influence on residential and school choices endure. The persistence of such negative peer dynamics implies that stigma-related mechanisms are more durable than the short-term benefits generated by policy support, ultimately undermining the long-term academic gains intended for disadvantaged students in targeted areas. From a policy standpoint, this asymmetry points to a critical inefficiency that future program designs should seek to address.

#### 7.4. Labour markets and preferences for schools

Place-based policies in France combine education-related incentives with tax rebates for firms operating within designated zones. If these tax incentives

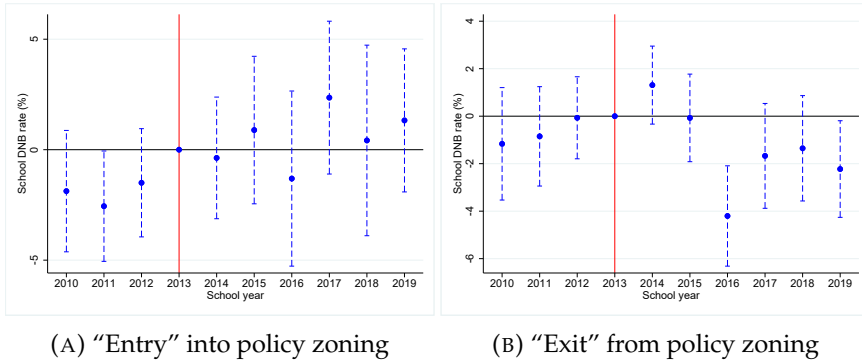


FIGURE 9. Zoning “Entry/Exit” and difference in school performance at the end of lower secondary education

Sources: DNB, DEPP-MEN (2010-2019).

Notes: The X-axis displays school years (with 2011 standing for the 2011-2012 academic year, and so on), taking 2013 as the reference year. The Y-axis reports the estimated gap in DNB pass rates for schools located in newly labeled neighborhoods (panel a) and in disqualified neighborhoods (panel b), relative to their counterfactual counterparts. Estimations are carried out at the school level, with control for school and year fixed-effects, a dummy for school benefiting from extra funds from school-based policies, and the number of private schools within a 5 km radius of the pupil’s primary school. 95% confidence intervals are plotted with standard errors clustered at the school level.

spur local job creation and earnings growth for the residents of targeted areas, they may confound our interpretation of changes in school preferences attributed to territorial stigma. In other words, parents might respond not to the label signal, but to improved household finances. For example, higher incomes in policy areas could enable families to afford private schooling, invest more in home study environments, or relocate to more affluent neighborhoods.

To address this concern, we draw on two comprehensive administrative datasets tracking firms, employment, and earnings over time. The *Sirene* register records all establishments active in France each January (Insee, 2010-2019), including their geographic coordinates (Insee, 2023). It distinguishes between new creations (Insee, 2010-2019) and relocations (Insee, 2010-2019), allowing us to determine whether firms were genuinely attracted to the new zoning or simply relocated from elsewhere. We complement these records with the *Déclarations Annuelles de Données Sociales* (BTS postes), an employer–employee database that details job spells, wages, and hours worked for all salaried employees working in the private sector (Insee, 2009-2020). Crucially, DADS data enable us to identify whether plant employees reside in the same municipality as their employer, providing the opportunity to approximate the evolution of the share of “residents” in the workforce of the designated or disqualified neighborhoods. Combining both sources enables us

to track the evolution of business activity and labor-market outcomes across our four types of neighborhoods: entrant, exiting, still treated, and never treated.

Following Mayer et al. (2017), we first estimate standard binary logit business location models:

$$P_{int} = \mathbb{1} \{ \beta \mathbb{1}_{t \geq 2015} + \mu_{l(n)} + \varepsilon_{int} \geq 0 \}, \quad (7)$$

where  $P_{int}$  denotes the probability that business  $i$  locates in neighborhood  $n$  entering (or exiting) the policy zone post-reform, and  $\mu_{l(n)}$  are local labour market fixed effects. The coefficient  $\beta$  captures the change in location propensity after 2015, when tax rebates came into effect in newly designated areas.<sup>28</sup>

Table 8 shows that the policy led to a small but significant increase in the likelihood of plant creation (+0.7 pp) and settlement (+0.3 pp) within newly eligible neighborhoods, relative to counterfactual areas. Strikingly, we detect no evidence of displacement effects, in contrast with earlier policy rounds. We also find no indication of business flight from areas losing label, suggesting that allowing firms located in phased-out zones to retain tax advantages for several years effectively mitigated any potential decline in the relative locational attractiveness of formerly-treated areas.

In line with the approaches adopted by Givord et al. (2013), Briant et al. (2015) and Givord et al. (2018), we next investigate whether the reform influenced broader local labor-market dynamics. To do so, we estimate the following specification:

$$\Delta L_{nt} = L_{nt} - L_{nt-1} = \sum_{k=2010}^{2019} \beta_{1/2k} T_n^{entry/exit} \times \mathbb{1}_{t=k} + \mu_n + \mu_t + \varepsilon_{nt}, \quad (8)$$

where  $L_{nt}$  denotes changes in the growth rate of business outcomes such as the (log of) total number of jobs, hours worked and earnings, as well as the median wage, and the share of resident employees in neighborhood  $n$ . The specification includes both neighborhood ( $\mu_n$ ) and year ( $\mu_t$ ) fixed effects.

We find no evidence that policy designation significantly affected the growth of employment, hours worked, total earnings, or median wages (Table 9). The share of resident employees, our proxy for the extent to which any gains accrue locally, exhibits only modest increases on average (+0.8 pp, see column 5 of Table 9), with effects emerging slowly, peaking two to three years after designation (+1.5 pp), and subsequently dissipating (see last column of Appendix Table Q1). The modest, gradual and short-lived

28. Given that tax rebates were targeted at Tier-2 and Tier-3 zones prior to the reform (see Table B1), we define treatment "entry/exit" accordingly.

TABLE 8. Zoning “Entry/Exit” and business location

	Probability of business:		
	Creation	Relocation	Location
	in newly labeled neighborhoods		
Post-reform	0.007** (0.003)	0.010 (0.008)	0.003* (0.002)
Pseudo $R^2$	0.150	0.177	0.191
No. obs	546,197	123,696	3,212,397
No. clusters	1,451	1,391	1,456
	in disqualified neighborhoods		
Post-reform	-0.002 (0.005)	0.001 (0.012)	-0.003 (0.004)
Pseudo $R^2$	0.133	0.200	0.180
No. obs	73,545	13,370	326,872
No. clusters	153	146	153
Local Labour Market FE	✓	✓	✓

*Sources:* Sirene historique géolocalisé, Insee-CASD (1973-2022), and Stocks/ Créations/ Transferts d'établissements, Insee-CASD (2010-2019).

*Notes:* \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . Reported coefficients are marginal effects from logit models. Business location is registered each year on January 1. As the Lamy law was passed in February 2014, the post-reform period starts in 2015. Standard errors in parentheses are clustered at the neighborhood level. For clarity, we do not list the intercept and the fixed effects estimates.

nature of these labor-market responses contrasts sharply with the immediate and pronounced re-sorting of families observed in school-enrollment data, suggesting that changes in school choices are unlikely to have been driven by evolving local labor-market conditions.

Taken together, these findings suggest that the reform's labor-market effects were limited in magnitude and slow to materialize for the local workforce. Although the policy attracted some new firms, these establishments neither hired substantially more residents nor significantly increased local pay. The data thus offer little support for the idea that the re-sorting of parents across schools could stem from a sudden improvement in residents' job opportunities or earnings. In other words, while the reform slightly enhanced the economic attractiveness of entrant neighborhoods, the scale and timing of these changes are inconsistent with the abrupt shifts in school enrollment documented earlier. Parental responses occurred too quickly and too sharply to have been driven by adjustments in residents'

earnings or labor-market conditions. Instead, the evidence points toward mechanisms rooted in spatial labeling and perceived neighborhood stigma: the policy reshaped territorial reputations rather than underlying economic fundamentals, leading families to reassess the quality of local schools and neighborhood amenities.

TABLE 9. Zoning “Entry/Exit” and local business dynamics

	Business outcomes:				
	Jobs	Hours	Earnings	Median wage	% Res. in workforce
<i>T<sup>entry</sup></i>	0.004 (0.006)	0.009 (0.008)	0.011 (0.008)	-0.003 (0.004)	0.008*** (0.002)
<i>R<sup>2</sup></i>	0.076	0.104	0.107	0.048	0.047
No. obs	15,686	15,686	15,686	15,686	15,686
No. clusters	1,607	1,607	1,607	1,607	1,607
<i>T<sup>exit</sup></i>	-0.003 (0.017)	-0.002 (0.018)	0.001 (0.019)	0.004 (0.008)	0.002 (0.005)
<i>R<sup>2</sup></i>	0.101	0.130	0.130	0.053	0.049
No. obs	1,818	1,818	1,818	1,818	1,818
No. clusters	185	185	185	185	185
Neighborhood FE	✓	✓	✓	✓	✓

Sources: DADS - BTS postes, Insee-CASD (2009-2020).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. Standard errors in parentheses are clustered at the neighborhood level. For clarity, we do not list the intercept and the fixed effects estimates.

## 8. Conclusion

Although place-based policies transfer substantial resources to low-income neighborhoods, the extent to which they provide disadvantaged residents with increased opportunities remains a topic of debate. While educational components of these policies may increase school enrollment in targeted areas if parents anticipate gains in their children’s academic outcomes, policy designation can also generate territorial stigma that undermines the perceived quality of local public schools. Such reputational effects may induce nontrivial spatial sorting responses, with the potential to reinforce patterns of urban segregation. This paper quantifies the net effect of these countervailing mechanisms in France over the period 2010–2019.

A central challenge in evaluating place-based policies lies in addressing selection into treatment, as neighborhoods are designated precisely due to their economic deprivation. To overcome this issue, we exploit a quasi-natural experiment stemming from a reform that redefined neighborhood eligibility using a non-manipulable local poverty threshold. Using a regression discontinuity design embedded within a spatial difference-in-differences framework with two-way fixed effects, we identify the causal impact of the policy on schooling outcomes, housing prices, and population sorting between labeled and unlabeled neighborhoods.

We find that, following designation, public middle schools in newly labeled neighborhoods experienced significant and persistent enrollment declines compared to counterfactual areas. The policy tarnished the reputation of these schools, benefiting private schools and higher-performing public schools elsewhere, and generating enduring sorting effects equivalent to the loss of an entire class per school in treated areas. These estimates likely represent a lower bound of the territorial stigma induced by policy designation, given that school sectorization in France restricts parental mobility. In contexts where parents have greater freedom to choose schools, the consequences could be even more severe. Moreover, areas losing their label exhibited little reversal of stigma, while policy disqualification was associated with a significant and lasting decline in academic achievement.

Importantly, resorting effects differ by family background: low-SES parents shifted children to public schools outside designated areas, while high-SES parents opted for private schools or relocated, generating both a “flight-to-quality” and a “rich-flight.” Newly designated neighborhoods also experienced significant and persistent declines in housing prices and a process of reverse gentrification, with a higher share of low-SES families and fewer medium-SES residents.

Our results highlight two key implications for place-based educational initiatives. First, enrollment declines, housing depreciation, and reverse gentrification demonstrate that territorial stigma can undermine intended policy benefits, highlighting the need for substantial additional educational resources to offset the territorial stigma induced by the policy label. Second, the persistent negative impact on academic achievement in schools serving disqualified neighborhoods makes clear that withdrawing support to disadvantaged pupils residing in low-income neighborhoods is certainly not an option. Instead, targeting support directly at individual pupils is likely more effective than neighborhood-based programs in addressing their educational needs.

## Appendix A: Internet information on the urban-policy coverage

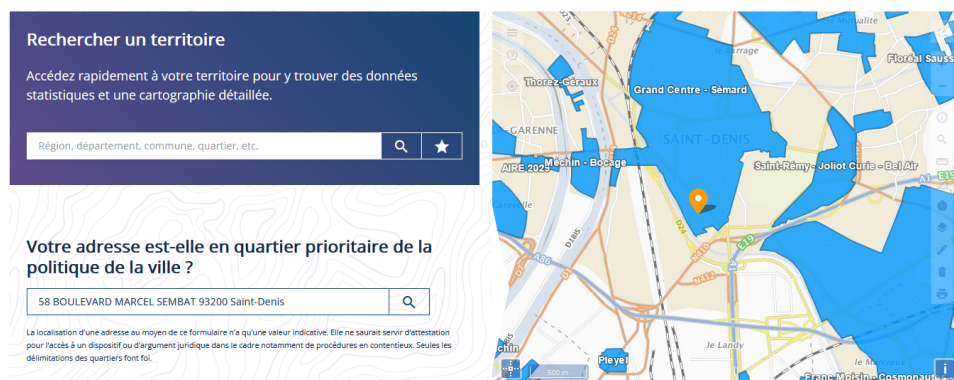


FIGURE A1. Internet information on the urban policy coverage

Source: Caption from the ANCT website (<https://sig.ville.gouv.fr/>).

Note: The address refers to a public middle school located in a QPV (shown in blue) located in the north of Paris.

## Appendix B: Policy changes associated with the Lamy reform

TABLE B1. Tier-2 and Tier-3 vs. QPV policies

Main Tax rebates	Tier-2	Tier-3	QPV
<b>Payroll tax on labor contracts &gt; 12 months</b>			
Plants	All	< 50 workers & 10 Mn sales*	No longer derogative**
Workers	New hires	All employees	No longer derogative**
Full exemption (wages ≤ 1.4 Smic)	1 year	5 years	No longer derogative**
Partial exemption (1.4 < wages ≤ 2 Smic)	No	Yes up to 9 years	No longer derogative**
Resident hiring condition	No	Yes (20% from Tier-3 before 2002, 33% from Tier-1 over 2002-2012, 50% from Tier-1 after 2012)	No longer derogative**
<b>Social security contributions (sickness and maternity)</b>			
Eligibility	No	Artisans & shopkeepers	No longer derogative**
Full exemption	0	100% over 5 years	0
Cap exemption	No	€25,157 in 2007	No
<b>Corporate tax (on profit)</b>			
Plants	New settlers	Already settled*	No
Full exemption	2 years	5 years	0
Partial exemption	up to 3 years	up to 9 years	0
Cap exemption	€225,000/3 years	€100,000/year (+5,000/hire from Tier-1)	No
<b>Local business Tax</b>			
Plants	< 150 workers	< 50 workers & 10 Mn sales*	New settlers < 150 workers Already settled < 50 workers & 10 Mn sales*
Full exemption	2 years	5 years	5 years
Partial exemption	up to 3 years	up to 9 years	up to 3 years
Cap exemption	€125,197 in 2006	€337,713 in 2006 €76,729 in 2014	New settlers €29,796 in 2022 Already settled €80,375 in 2022
<b>Tax for business properties built</b>			
Eligibility	No	Yes	Yes < 50 workers & 10 Mn sales*
Full exemption	0	100% over 5 years	100% over 5 years
<b>Transfer rights for business acquisition</b>			
Eligibility	Yes	Yes	No
Cap exemption	€107,000	€107,000	No

Notes: Smic stands for minimum wage. \*Some sectors are excluded from the exemption regime (liberal professions, real estate leasing, shipbuilding, car construction, manufacturing of artificial or synthetic textile fibers, tobacco sales, rail or road freight transport, agriculture, and fishing). Beneficiary firms cannot be owned more than 25% by a company with over 250 employees and an annual turnover exceeding €50 Mn or a total annual balance exceeding €43 Mn. \*\* In 2006, the 'Fillon' law expanded low-wage credits to the entire French territory. Therefore, after 2014, employers in QPV still benefit from payroll credits for low wages, but these rebates are no longer conditional on their location.

**Appendix C: Descriptive statistics**

TABLE C1. Description of the sample

	Freq.	%
Gender		
Girl	3,674,021	49
Boy	3,800,433	51
Socioeconomic status		
Very high SES	1,748,435	23
High SES	955,279	13
Medium SES	2,006,990	27
Low SES	2,459,656	33
Unknown	304,094	4
Age		
7-10	213,609	3
11-12	7,249,451	97
13-17	11,394	0
Middle school choice		
Catchment area	4,069,682	54
Other public	1,763,580	24
Private	1,641,192	22
Catchment-area schools		
In policy zoning	2,580,057	34.5
Entering policy zoning	26,022	0.3
Exiting policy zoning	2,230,512	29.8
Exiting policy zoning ( $0.6 < I_r \leq 0.7$ )	544,298	7.3
In entry-counterfactual areas	298,505	4.0
In exit-counterfactual areas	120,012	1.6
Total	7,474,454	100.0

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Appendix D: Balancing tests between control and treated schools

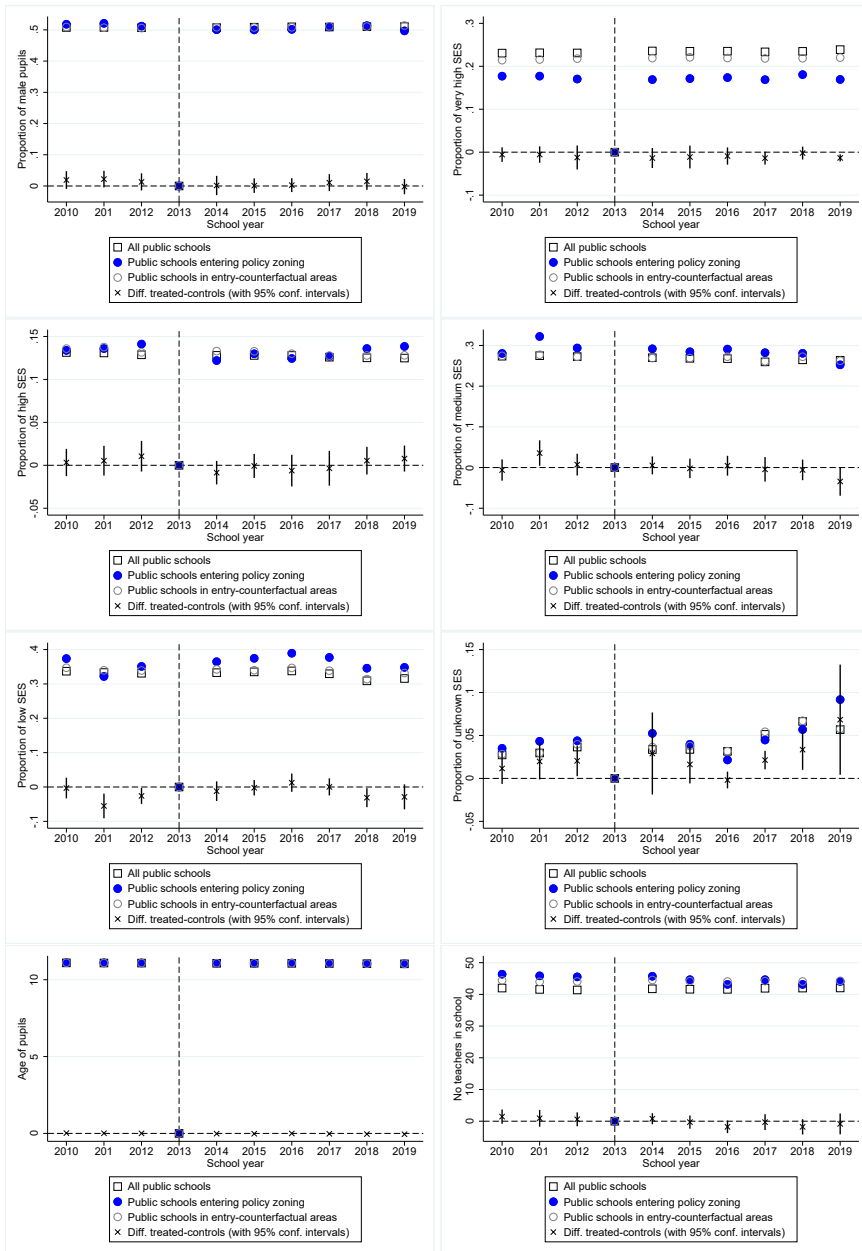


FIGURE D1. Schools transitioning into zoning vs. entry-counterfactual schools

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: Balancing tests for pupils entering 6<sup>th</sup> grade for the first time. Yearly proportions and differences between treated and counterfactual schools are computed with respect to 2013.

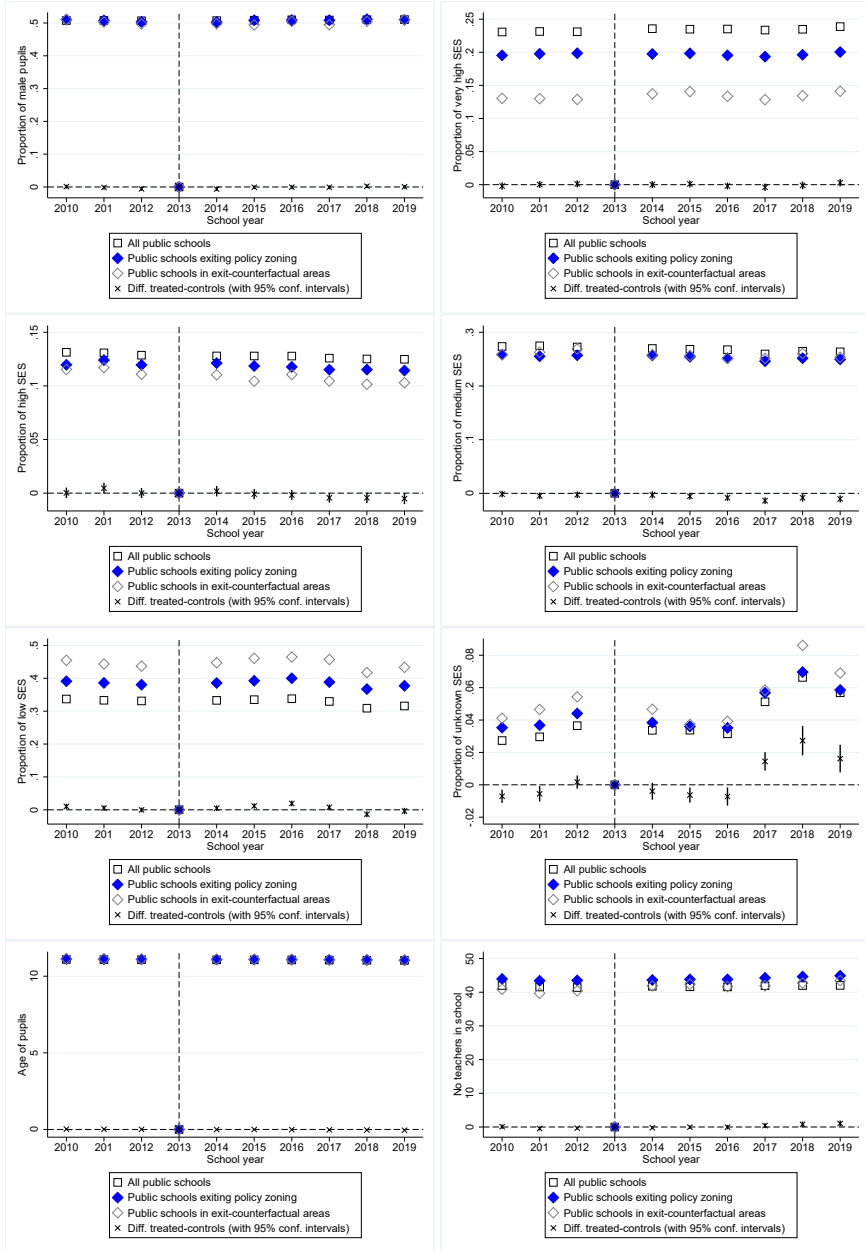


FIGURE D2. Schools transitioning out zoning vs. exit-counterfactual schools  
 Sources: Base centrale scolaire (BCS), DEPP-MEN (2009-2019).  
 Note: Balancing tests for pupils entering 6<sup>th</sup> grade for the first time. Yearly proportions and differences between treated and counterfactual schools are computed with respect to 2013.

**Appendix E: Complementary estimates on pupil enrollment**

TABLE E1. Zoning “Entry/Exit” and pupil enrollment

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
<i>T<sub>entry</sub></i>	-0.023** (0.011)	0.022** (0.009)	0.001 (0.008)
<i>R</i> <sup>2</sup>	0.165	0.111	0.186
No. obs	324,527	324,527	324,527
No. clusters	196	196	196
<i>T<sub>exit</sub></i>	0.005 (0.007)	-0.007 (0.007)	0.001 (0.004)
<i>R</i> <sup>2</sup>	0.162	0.107	0.228
No. obs	684,448	684,448	684,448
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors in parentheses are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. For clarity, the intercept and the coefficients on these controls are not reported.

**Appendix F: Catchment-areas based on pupils’ primary schools**

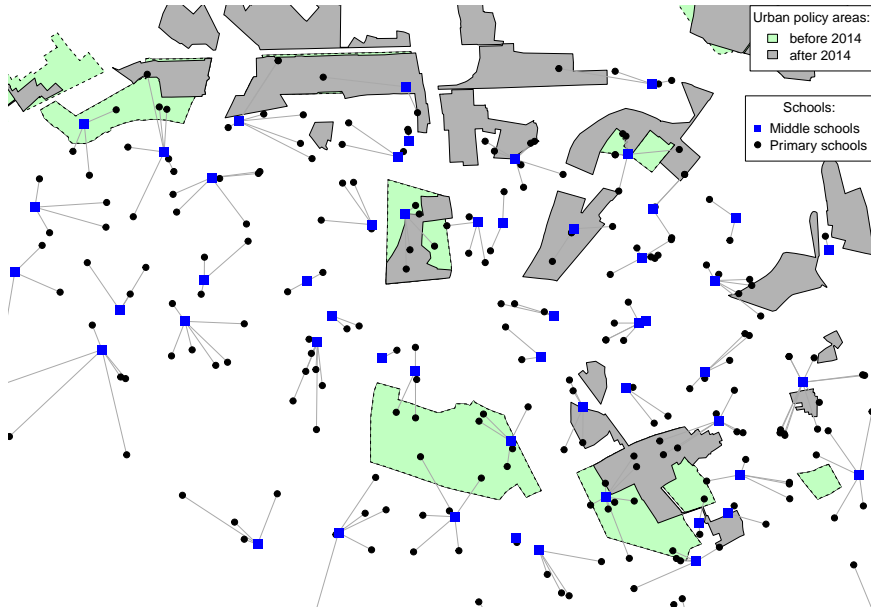


FIGURE F1. Catchment-areas based on each pupil’s primary school (e.g. Northern Paris area)

*Sources:* Authors based on Annuaire de l’éducation (DNE-MEN, 2019), Base centrale scolarité (BCS), DEPP-MEN (2009-2019), and shapefiles from the ANCT (2010c, 2018a,b,c).

*Notes:* Blue squares indicate public middle schools and black dots primary schools. Black segments connect each primary school to its nearest public middle school, which defines catchment-areas for all pupils previously enrolled in that primary school.

**Appendix G: Clustering at the neighborhood level**

TABLE G1. Zoning “Entry/Exit” and pupil enrollment - Clustering by neighborhood

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
<i>T<sup>entry</sup></i>	-0.037** (0.016)	0.039** (0.017)	-0.002 (0.008)
<i>R</i> <sup>2</sup>	0.173	0.125	0.187
No. obs	324,527	324,527	324,527
No. clusters	181	181	181
<i>T<sup>exit</sup></i>	0.006 (0.007)	-0.007 (0.007)	0.001 (0.005)
<i>R</i> <sup>2</sup>	0.165	0.107	0.229
No. obs	684,448	684,448	684,448
No. clusters	328	328	328
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors (in parentheses) are clustered at the neighborhood level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

**Appendix H: Rezoning and pupil enrollment by year**

TABLE H1. “Entry” into policy zoning and pupil enrollment - By year

	Probability of being enrolled in:			
	CA School	Other Public School		Private School
		In zoning	Out zoning	
$T^{entry} \times 2010$	0.008 (0.014)	-0.002 (0.006)	-0.008 (0.012)	0.002 (0.015)
$T^{entry} \times 2011$	-0.010 (0.017)	-0.008 (0.006)	0.005 (0.011)	0.013 (0.016)
$T^{entry} \times 2012$	-0.006 (0.013)	-0.007** (0.003)	0.009 (0.009)	0.003 (0.011)
(ref.=2013)				
$T^{entry} \times 2014$	-0.041*** (0.015)	0.003 (0.004)	0.030* (0.016)	0.008 (0.010)
$T^{entry} \times 2015$	-0.050*** (0.015)	0.003 (0.005)	0.042*** (0.015)	0.005 (0.010)
$T^{entry} \times 2016$	-0.050*** (0.014)	0.006 (0.007)	0.030* (0.016)	0.014 (0.013)
$T^{entry} \times 2017$	-0.033** (0.013)	0.002 (0.008)	0.033*** (0.011)	-0.002 (0.012)
$T^{entry} \times 2018$	-0.039** (0.016)	0.018** (0.009)	0.020 (0.014)	0.002 (0.013)
$T^{entry} \times 2019$	-0.020 (0.017)	0.006 (0.007)	0.028* (0.016)	-0.013 (0.012)
$R^2$	0.173	0.147	0.118	0.187
No. obs	324,527	324,527	324,527	324,527
No. clusters	196	196	196	196
Pupil’s characteristics	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

TABLE H2. “Exit” from policy zoning and pupil enrollment - By year

	Probability of being enrolled in:			
	CA School	Other Public School		Private School
		In zoning	Out zoning	
$T^{exit} \times 2010$	0.012 (0.009)	-0.006 (0.007)	0.002 (0.006)	-0.009 (0.007)
$T^{exit} \times 2011$	-0.011 (0.008)	0.007 (0.007)	0.009** (0.005)	-0.006 (0.005)
$T^{exit} \times 2012$	-0.008 (0.006)	0.001 (0.006)	0.004 (0.004)	0.002 (0.006)
(ref.=2013)				
$T^{exit} \times 2014$	0.006 (0.008)	-0.001 (0.006)	0.003 (0.004)	-0.007 (0.005)
$T^{exit} \times 2015$	0.012 (0.008)	-0.003 (0.007)	-0.006 (0.005)	-0.003 (0.006)
$T^{exit} \times 2016$	0.008 (0.009)	0.003 (0.007)	-0.010** (0.004)	-0.000 (0.006)
$T^{exit} \times 2017$	0.004 (0.009)	0.012 (0.009)	-0.009 (0.006)	-0.007 (0.007)
$T^{exit} \times 2018$	-0.008 (0.009)	0.004 (0.008)	-0.002 (0.006)	0.006 (0.007)
$T^{exit} \times 2019$	0.007 (0.010)	0.005 (0.008)	-0.009 (0.008)	-0.003 (0.007)
$R^2$	0.165	0.139	0.120	0.229
No. obs	684,448	684,448	684,448	684,448
No. clusters	436	436	436	436
Pupil’s characteristics	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

**Appendix I: Pupil enrollment changes over policy rounds**

TABLE I1. Pupil enrollment in first-time vs. formerly-treated neighborhoods - By year

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry} \times 2010$	0.022 (0.014)	-0.009 (0.011)	-0.012 (0.015)
$T^{entry} \times 2011$	-0.002 (0.017)	0.008 (0.014)	-0.006 (0.016)
$T^{entry} \times 2012$	0.005 (0.013)	-0.001 (0.009)	-0.004 (0.011)
(ref.=2013)			
$T^{entry} \times 2014$	-0.024* (0.014)	0.015 (0.015)	0.010 (0.010)
$T^{entry} \times 2015$	-0.032** (0.015)	0.031** (0.014)	0.002 (0.011)
$T^{entry} \times 2016$	-0.039*** (0.013)	0.021 (0.015)	0.018 (0.012)
$T^{entry} \times 2017$	-0.024* (0.013)	0.022* (0.011)	0.002 (0.011)
$T^{entry} \times 2018$	-0.036** (0.016)	0.020 (0.016)	0.015 (0.012)
$T^{entry} \times 2019$	-0.012 (0.017)	0.012 (0.019)	-0.000 (0.012)
$R^2$	0.178	0.106	0.221
No. obs	349,545	349,545	349,545
No. clusters	220	220	220
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils' characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil's primary school. For clarity, the intercept and the coefficients on these controls are not reported.

TABLE I2. Pupil enrollment in no-longer treated vs. never-treated areas - By year

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{exit} \times 2010$	0.003 (0.009)	0.002 (0.009)	-0.005 (0.007)
$T^{exit} \times 2011$	0.004 (0.008)	0.002 (0.008)	-0.006 (0.007)
$T^{exit} \times 2012$	0.005 (0.006)	0.004 (0.008)	-0.009 (0.006)
(ref.=2013)			
$T^{exit} \times 2014$	-0.002 (0.006)	0.004 (0.008)	-0.002 (0.006)
$T^{exit} \times 2015$	-0.010 (0.010)	0.013 (0.010)	-0.003 (0.006)
$T^{exit} \times 2016$	-0.008 (0.009)	0.017* (0.009)	-0.010 (0.006)
$T^{exit} \times 2017$	-0.004 (0.009)	0.012 (0.009)	-0.008 (0.006)
$T^{exit} \times 2018$	-0.010 (0.010)	0.029*** (0.010)	-0.019** (0.008)
$T^{exit} \times 2019$	-0.003 (0.010)	0.027** (0.011)	-0.024*** (0.007)
$R^2$	0.167	0.114	0.217
No. obs	4,996,293	4,996,293	4,996,293
No. clusters	3,525	3,525	3,525
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils' characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil's primary school. For clarity, the intercept and the coefficients on these controls are not reported.

**Appendix J: Multinomial logit estimation results**

Let  $U_{idt}^l$  denote the utility that the family of pupil  $i$ , assigned to catchment-area school  $d$  at time  $t$ , derives from selecting school  $l$ . We model school choice using a multinomial logit framework:

$$Y_{idt} = k \text{ if } U_{idt}^k > U_{idt}^l, \tag{J.1}$$

where the indirect utility function is specified as:

$$U_{idt}^k = \alpha^k + \beta^k T_d \times \mathbb{1}_{t \geq 2014} + X_{it} \gamma^k + Z_{dt} \delta^k + \mu_d^k + \mu_t^k + \eta_{idt}^k, \tag{J.2}$$

with  $k = \{1, 2, 3\}$  representing, respectively, the catchment-area school, another public school, or a private school.

TABLE J1. Multinomial logit - “Entry/Exit” into policy zoning and pupil enrollment

	Average marginal effects	
	Middle School Choice (ref. = CA-school) Other Public School	Private School
$T^{entry}$	0.042*** (0.014)	-0.003 (0.007)
Pseudo $R^2$	0.167	0.167
No. obs	324,527	324,527
No. clusters	196	196
$T^{exit}$	-0.007 (0.006)	0.001 (0.005)
Pseudo $R^2$	0.169	0.169
No. obs	684,448	684,448
No. clusters	436	436
Pupil’s characteristics	✓	✓
Time-varying controls	✓	✓
Year FE	✓	✓
School FE	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not listed.

**Appendix K: Other definitions of policy treatment**

TABLE K1. Zoning “Entry/Exit” and pupil enrollment - Sharp definition (policy boundaries)

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.029** (0.013)	0.033*** (0.012)	-0.004 (0.006)
$R^2$	0.176	0.134	0.191
No. obs	353,006	353,006	353,006
No. clusters	215	215	215
$T^{exit}$	0.010 (0.009)	-0.005 (0.008)	-0.005 (0.006)
$R^2$	0.173	0.099	0.215
No. obs	318,860	318,860	318,860
No. clusters	202	202	202
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

TABLE K2. Zoning "Entry/Exit" and pupil enrollment - Narrow definition (living aside treatment Tier-0 zones other than Tier-1/3)

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.024** (0.010)	0.025*** (0.009)	-0.000 (0.006)
$R^2$	0.174	0.122	0.190
No. obs	367,620	367,620	367,620
No. clusters	226	226	226
$T^{exit}$	0.005 (0.009)	-0.006 (0.009)	0.000 (0.006)
$R^2$	0.164	0.098	0.216
No. obs	339,270	339,270	339,270
No. clusters	220	220	220
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils' characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil's primary school. For clarity, the intercept and the coefficients on these controls are not reported.

**Appendix L: Heterogeneity across occupations**

TABLE L1. Zoning “Entry/Exit” and enrollment of teachers’ children

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.037*** (0.014)	0.043*** (0.014)	-0.005 (0.009)
SES (ref.=Non-Teachers)			
Teachers $\times T^{entry}$	0.056** (0.026)	-0.079*** (0.026)	0.023 (0.017)
$R^2$	0.160	0.125	0.154
No. obs	324,526	324,526	324,526
No. clusters	196	196	196
$T^{exit}$	0.006 (0.007)	-0.007 (0.007)	0.001 (0.005)
SES (ref.=Non-Teachers)			
Teachers $\times T^{exit}$	-0.014 (0.019)	0.037* (0.020)	-0.023 (0.018)
$R^2$	0.139	0.109	0.184
No. obs	684,447	684,447	684,447
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

## Appendix M: Other heterogeneity dimensions

TABLE M1. Zoning “Entry/Exit” and pupil enrollment - By school performance gaps

	Probability of being enrolled in:	
	Private School	Other Public School Outside Zoning
$T^{entry}$	-0.018 (0.018)	-0.025** (0.012)
DNB gap (ref. $\leq 0$ )		
DNB gap $> 0 \times T^{entry}$	-0.005 (0.019)	0.045*** (0.015)
$R^2$	0.487	0.434
No. obs	151,864	78,294
No. clusters	196	196
$T^{exit}$	0.004 (0.005)	-0.005 (0.009)
DNB gap (ref. $\leq 0$ )		
DNB gap $> 0 \times T^{exit}$	0.005 (0.009)	-0.007 (0.013)
$R^2$	0.470	0.611
No. obs	350,010	200,888
No. clusters	426	426
Pupil’s characteristics	✓	✓
Time-varying controls	✓	✓
Year FE	✓	✓
School FE	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019), and DNB, DEPP-MEN (2013).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Regressions are run on the sample of pupils who are not enroll at the CA-school. The reference (DNB gap  $\leq 0$ ) indicates that the CA-school had performed better than (or at least equally as well as) the enrolled school in 2013, based on its average DNB success rate. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

TABLE M2. Zoning “Entry/Exit” and pupil enrollment - By distance to the closest private school

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.014 (0.013)	0.027** (0.013)	-0.013 (0.013)
Distance private (ref.>Median) Below Median $\times T^{entry}$	-0.030* (0.017)	0.016 (0.023)	0.013 (0.018)
$R^2$	0.204	0.158	0.208
No. obs	324,527	324,527	324,527
No. clusters	196	196	196
$T^{exit}$	0.014 (0.011)	-0.010 (0.012)	-0.003 (0.009)
Distance private (ref.>Median) Below Median $\times T^{exit}$	-0.011 (0.013)	0.005 (0.014)	0.005 (0.010)
$R^2$	0.197	0.152	0.243
No. obs	684,448	684,448	684,448
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors in parentheses are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not listed.

TABLE M3. Zoning “Entry/Exit” and pupil enrollment - By gender

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.031** (0.014)	0.035** (0.014)	-0.004 (0.009)
Sex (ref.=Girl) Boy $\times T^{entry}$	-0.011 (0.012)	0.008 (0.008)	0.002 (0.010)
$R^2$	0.173	0.126	0.188
No. obs	324,527	324,527	324,527
No. clusters	196	196	196
$T^{exit}$	0.008 (0.007)	-0.010 (0.007)	0.002 (0.005)
Boy $\times T^{exit}$	-0.002 (0.006)	0.005 (0.006)	-0.003 (0.005)
$R^2$	0.166	0.108	0.230
No. obs	684,448	684,448	684,448
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. CA School refers to the Catchment-Area School. Standard errors, reported in parentheses, are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported.

TABLE M4. Zoning “Entry/Exit” and pupil enrollment - By school type

	Probability of being enrolled in:		
	CA School	Other Public School	Private School
$T^{entry}$	-0.022 (0.014)	0.025** (0.011)	-0.003 (0.011)
REP (ref.=No extra-funds) Extra-funds $\times T^{entry}$	-0.015 (0.040)	0.041 (0.036)	-0.027 (0.039)
$R^2$	0.175	0.126	0.190
No. obs	324,527	324,527	324,527
No. clusters	196	196	196
$T^{exit}$	0.010 (0.013)	0.004 (0.009)	-0.014 (0.009)
REP (ref.=No extra-funds) Extra-funds $\times T^{exit}$	-0.001 (0.016)	-0.011 (0.013)	0.012 (0.010)
$R^2$	0.167	0.110	0.230
No. obs	684,448	684,448	684,448
No. clusters	436	436	436
Pupil’s characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS), DEPP-MEN (2009-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . CA School refers to the Catchment-Area School. Standard errors in parentheses are clustered at the CA-school level. Pupils’ characteristics include socioeconomic background, gender, and age. Time-varying controls include a dummy for the CA school benefiting from extra funds from school-based policies and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not listed.

Appendix N: Housing prices analysis

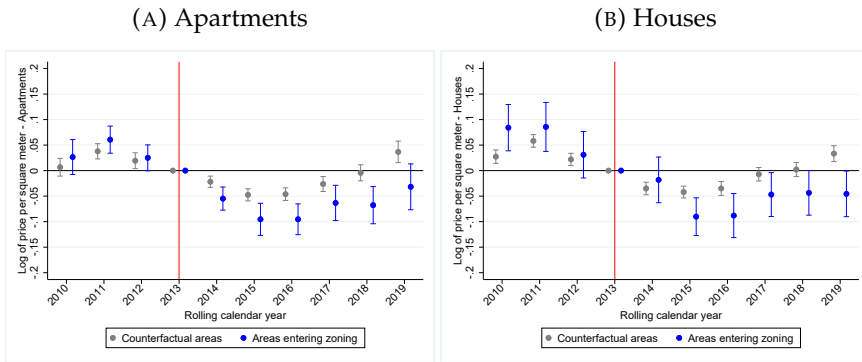


FIGURE N1. “Entry” into policy zoning and housing prices

Sources: DV3F 2025-1, DGFIP-DGALN-Cerema, 2010-2019.

Note: The X-axis displays rolling calendar years from March to February (March 2013 to February 2014 is the reference). The Y-axis displays the coefficients of year dummies, with 95% confidence intervals. Standard errors are clustered at the neighborhood level. Regressions include property characteristics (property type, floor level, construction year, floor or land area, number of bathrooms, presence of a cellar, balcony or terrace, and garage), neighborhood fixed effects, month and rolling years dummies.

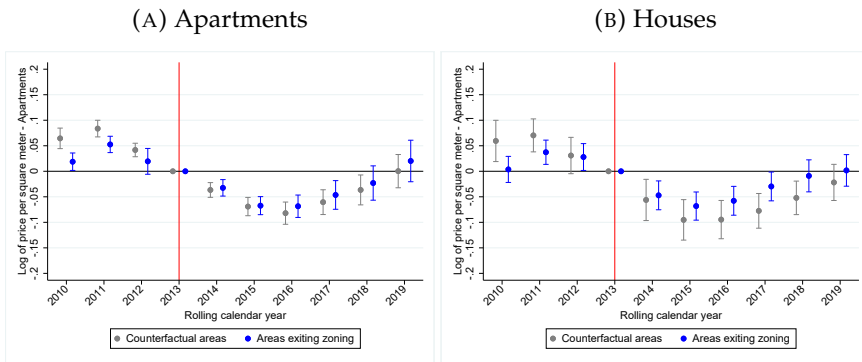


FIGURE N2. “Exit” from policy zoning and housing prices

Sources: DV3F 2025-1, DGFIP-DGALN-Cerema (2010-2019).

Notes: The X-axis displays rolling calendar years from March to February (March 2013 to February 2014 is the reference). The Y-axis displays the coefficients of year dummies, with 95% confidence intervals. Standard errors are clustered at the neighborhood level. Regressions include property characteristics (property type, floor level, construction year, floor or land area, number of bathrooms, presence of a cellar, balcony or terrace, and garage), neighborhood fixed effects, month, and rolling years dummies.

TABLE N1. "Entry" into policy zoning and housing prices

	Change in log prices per square meter	
	Multi-family properties	Single-family properties
$T^{entry} \times 2010$	0.020 (0.019)	0.056** (0.023)
$T^{entry} \times 2011$	0.023 (0.015)	0.023 (0.025)
$T^{entry} \times 2012$	0.006 (0.015)	0.009 (0.023)
(ref.=2013)		
$T^{entry} \times 2014$	-0.032** (0.013)	0.018 (0.023)
$T^{entry} \times 2015$	-0.048*** (0.017)	-0.047** (0.020)
$T^{entry} \times 2016$	-0.049*** (0.016)	-0.053** (0.023)
$T^{entry} \times 2017$	-0.036* (0.019)	-0.041* (0.023)
$T^{entry} \times 2018$	-0.065*** (0.019)	-0.040* (0.023)
$T^{entry} \times 2019$	-0.068*** (0.024)	-0.072*** (0.024)
$R^2$	0.755	0.657
No. obs	98,667	73,814
No. clusters	680	706
Property characteristics	✓	✓
Neighborhood FE	✓	✓
Month FE	✓	✓
Year FE	✓	✓

Sources: DV3F 2025-1, DGFIP-DGALN-Cerema (2010-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. Standard errors, in parentheses, are clustered at the neighborhood level. Regressions include property characteristics (property type, floor level, construction year, floor or land area, number of bathrooms, presence of a cellar, balcony or terrace, and garage), neighborhood fixed effects, month, and rolling years dummies. For clarity, the intercept and the coefficients on these controls are not reported.

TABLE N2. "Exit" from policy zoning and housing prices

	Change in log prices per square meter	
	Multi-family properties	Single-family properties
$T^{exit} \times 2011$	-0.016 (0.013)	0.017 (0.027)
$T^{exit} \times 2012$	-0.020 (0.015)	0.024 (0.025)
(ref.=2013)		
$T^{exit} \times 2014$	0.006 (0.016)	-0.017 (0.033)
$T^{exit} \times 2015$	0.009 (0.025)	-0.015 (0.048)
$T^{exit} \times 2016$	0.027 (0.031)	-0.022 (0.054)
$T^{exit} \times 2017$	0.028 (0.030)	-0.016 (0.056)
$T^{exit} \times 2018$	0.020 (0.021)	-0.011 (0.041)
$R^2$	0.811	0.692
No. obs	62,237	24,736
No. clusters	602	601
Property characteristics	✓	✓
Neighborhood FE	✓	✓
Month FE	✓	✓
Year FE	✓	✓
Group time-trends	✓	✓

Sources: DV3F 2025-1, DGFIP-DGALN-Cerema (2010-2019).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. Standard errors, in parentheses, are clustered at the neighborhood level. Regressions include property characteristics (property type, floor level, construction year, floor or land area, number of bathrooms, presence of a cellar, balcony or terrace, and garage), neighborhood fixed effects, month dummies, rolling years dummies, and third-order polynomial group time trends (two additional degrees of freedom are required, and therefore the effects for 2010 and 2019 are not estimated). For clarity, the intercept and the coefficients on these controls are not reported.

**Appendix O: Social recomposition of neighborhoods**

TABLE O1. "Entry" into policy zoning and social composition

	Proportion of secondary education pupils from:		
	High-SES	Medium-SES	Low-SES
$T^{entry} \times 2011$	-0.001 (0.002)	0.004 (0.003)	-0.003 (0.003)
(ref.=2013)			
$T^{entry} \times 2015$	-0.004*** (0.002)	-0.009*** (0.003)	0.013*** (0.003)
$T^{entry} \times 2017$	0.015*** (0.002)	-0.043*** (0.005)	0.028*** (0.005)
$R^2$	0.088	0.031	0.100
No. obs	913,292	913,292	913,292
No. clusters	1,212	1,212	1,212
Neighborhood FE	✓	✓	✓
Year FE	✓	✓	✓

Sources: Fichiers géoréférencés des élèves du secondaire, DEPP-MEN (2011, 2013, 2015, and 2017).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. Standard errors, in parentheses, are clustered at the neighborhood level.

TABLE O2. "Exit" from policy zoning and social composition

	Proportion of secondary education pupils from:		
	High-SES	Medium-SES	Low-SES
$T^{exit} \times 2011$	0.001 (0.001)	-0.002 (0.003)	0.000 (0.003)
(ref.=2013)			
$T^{exit} \times 2015$	0.002 (0.001)	0.004* (0.003)	-0.006** (0.003)
$T^{exit} \times 2017$	-0.003* (0.002)	0.022*** (0.004)	-0.019*** (0.004)
$R^2$	0.040	0.037	0.061
No. obs	1,136,477	1,136,477	1,136,477
No. clusters	1,031	1,031	1,031
Neighborhood FE	✓	✓	✓
Year FE	✓	✓	✓

Sources: Fichiers géoréférencés des élèves du secondaire, DEPP-MEN (2011, 2013, 2015, and 2017).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. Standard errors, in parentheses, are clustered at the neighborhood level.

## Appendix P: Academic achievement

TABLE P1. Zoning “Entry/Exit” and pass rate from 6th to 7th grade

	6th to 7th grade pass rate	
	Entry	Exit
<i>T</i>	0.952 (0.637)	-1.253*** (0.350)
$R^2$	0.369	0.422
No. obs	1,940	4,210
No. clusters	194	421

Sources: APAE, DEPP-MEN (2010-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . Coefficients are the estimated pass rate gaps (from 6<sup>th</sup> – to 7<sup>th</sup>-grade between schools in newly labeled (resp. de-labeled) neighborhoods and counterfactual schools. Estimations are carried out at the school level, with control for school and year fixed-effects, a dummy for the CA school benefiting from extra funds from school-based policies, and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported. Standard errors, in parentheses, are clustered at the school level.

TABLE P2. Zoning “Entry/Exit” and end-of-middle-school exam pass rate

	DNB pass rate	
	Entry	Exit
<i>T</i>	2.035 (1.467)	-0.848 (0.644)
$R^2$	0.499	0.510
No. obs	1,940	4,210
No. clusters	194	421

Sources: DNB, DEPP-MEN (2010-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . Coefficients are the estimated DNB pass rate gaps between schools in newly labeled (resp. de-labeled) neighborhoods and counterfactual schools. Estimations are carried out at the school level, with control for school and year fixed-effects, a dummy for the CA school benefiting from extra funds from school-based policies, and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported. Standard errors, in parentheses, are clustered at the school level.

TABLE P3. Zoning “Entry/Exit” and pass rate from 6th to 7th grade - By year

	6th to 7th grade pass rate	
	Entry	Exit
$T \times 2010$	0.630 (0.882)	0.843 (0.631)
$T \times 2011$	0.221 (0.823)	-0.122 (0.617)
$T \times 2012$	-1.267 (0.819)	0.339 (0.513)
(ref.=2013)		
$T \times 2014$	1.202 (0.944)	-0.933** (0.439)
$T \times 2015$	1.319* (0.796)	-0.851 (0.545)
$T \times 2016$	0.512 (1.126)	-0.919* (0.539)
$T \times 2017$	0.150 (0.964)	-0.800 (0.530)
$T \times 2018$	0.677 (1.047)	-1.153** (0.559)
$T \times 2019$	1.229 (0.841)	-1.275** (0.519)
$R^2$	0.372	0.423
No. obs	1,940	4,210
No. clusters	194	421

Source: APAE, DEPP-MEN (2010-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . Coefficients are the estimated differences in the pass rates from 6<sup>th</sup> to 7<sup>th</sup> grade for schools in newly labeled neighborhoods (resp. disqualified neighborhoods) compared to their counterfactual schools. Estimations are carried out at the school level, with control for school and year fixed-effects, a dummy for the CA school benefiting from extra funds from school-based policies, and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported. Standard errors, in parentheses, are clustered at the school level.

TABLE P4. Zoning “Entry/Exit” and end-of-middle-school exam pass rate - By year

	DNB pass rate	
	Entry	Exit
$T \times 2010$	-1.876 (1.662)	-1.164 (1.436)
$T \times 2011$	-2.557* (1.511)	-0.850 (1.269)
$T \times 2012$	-1.498 (1.482)	-0.068 (1.048)
(ref.=2013)		
$T \times 2014$	-0.372 (1.665)	1.309 (0.996)
$T \times 2015$	0.889 (2.020)	-0.073 (1.119)
$T \times 2016$	-1.307 (2.398)	-4.202*** (1.281)
$T \times 2017$	2.357 (2.093)	-1.672 (1.339)
$T \times 2018$	0.419 (2.609)	-1.352 (1.346)
$T \times 2019$	1.326 (1.959)	-2.226* (1.234)
$R^2$	0.501	0.514
No. obs	1,940	4,210
No. clusters	194	421

Sources: DNB, DEPP-MEN (2010-2019).

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ . Coefficients are the estimated differences in the DNB exam pass rates for schools in newly labeled neighborhoods (resp. disqualified neighborhoods) compared to their counterfactual schools. Estimations are carried out at the school level, with control for school and year fixed-effects, a dummy for the CA school benefiting from extra funds from school-based policies, and the number of private schools within a 5 km radius of the pupil’s primary school. For clarity, the intercept and the coefficients on these controls are not reported. Standard errors, in parentheses, are clustered at the school level.

## Appendix Q: Labour market outcomes

TABLE Q1. Zoning “Entry/Exit” and Local Business Dynamics - By year

	Business outcomes:				
	Jobs	Hours	Earnings	Median wage	% Res. in workforce
$T^{entry} \times 2010$	-0.026 (0.016)	-0.020 (0.018)	-0.027 (0.018)	0.004 (0.011)	-0.002 (0.005)
$T^{entry} \times 2011$	-0.013 (0.017)	-0.005 (0.018)	-0.003 (0.018)	0.007 (0.012)	0.000 (0.005)
$T^{entry} \times 2012$	-0.005 (0.015)	-0.006 (0.018)	-0.003 (0.019)	0.006 (0.012)	-0.002 (0.005)
$T^{entry} \times 2013$	-0.002 (0.015)	-0.002 (0.017)	-0.001 (0.017)	0.005 (0.014)	0.000 (0.006)
(ref.=2014)					
$T^{entry} \times 2015$	-0.017 (0.018)	0.003 (0.022)	0.007 (0.022)	0.020 (0.015)	-0.000 (0.006)
$T^{entry} \times 2016$	-0.020 (0.016)	-0.027 (0.018)	-0.024 (0.018)	-0.010 (0.012)	0.015*** (0.005)
$T^{entry} \times 2017$	-0.012 (0.016)	-0.001 (0.017)	0.007 (0.017)	0.002 (0.012)	0.015*** (0.006)
$T^{entry} \times 2018$	0.011 (0.016)	0.021 (0.020)	0.017 (0.020)	-0.004 (0.012)	0.001 (0.006)
$T^{entry} \times 2019$	0.013 (0.016)	0.018 (0.017)	0.013 (0.017)	-0.005 (0.012)	0.006 (0.006)
$R^2$	0.076	0.104	0.108	0.049	0.047
No. obs	15,686	15,686	15,686	15,686	15,686
No. clusters	1,607	1,607	1,607	1,607	1,607
$T^{exit} \times 2010$	0.050 (0.040)	-0.001 (0.029)	-0.005 (0.031)	-0.024 (0.027)	-0.021 (0.020)
$T^{exit} \times 2011$	0.074 (0.050)	0.008 (0.032)	0.000 (0.031)	-0.023 (0.025)	-0.012 (0.028)
$T^{exit} \times 2012$	-0.057 (0.074)	-0.041 (0.071)	-0.063 (0.074)	0.020 (0.031)	-0.003 (0.021)
$T^{exit} \times 2013$	0.047 (0.044)	0.010 (0.031)	0.021 (0.028)	-0.042 (0.028)	-0.004 (0.017)
(ref.=2014)					
$T^{exit} \times 2015$	0.046 (0.037)	0.026 (0.030)	0.033 (0.027)	-0.016 (0.026)	-0.004 (0.019)
$T^{exit} \times 2016$	0.050 (0.049)	0.020 (0.033)	0.021 (0.030)	0.028 (0.030)	-0.012 (0.021)
$T^{exit} \times 2017$	0.009 (0.042)	0.032 (0.028)	0.010 (0.026)	-0.008 (0.025)	-0.013 (0.020)
$T^{exit} \times 2018$	0.023 (0.040)	-0.031 (0.036)	-0.022 (0.035)	0.006 (0.028)	-0.025 (0.022)
$T^{exit} \times 2019$	-0.003 (0.042)	-0.061** (0.029)	-0.053* (0.027)	-0.042 (0.033)	0.009 (0.022)
$R^2$	0.109	0.135	0.137	0.060	0.053
No. obs	1,818	1,818	1,818	1,818	1,818
No. clusters	185	185	185	185	185
Neighborhood FE	✓	✓	✓	✓	✓

Sources: DADS - BTS postes, Insee-CASD (2009-2020).

Notes: \*\*\*p<0.01, \*\*p<0.05, \*p<0.10. The coefficients are drawn from estimating equation (8) (where the dependent variable is  $\Delta \log(Y_{nt} + 1)$ ). Standard errors in parentheses are clustered at the neighborhood level. For clarity, we do not list the intercept and the fixed effects estimates.

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