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Postal Address:
Institut d'Economia de Barcelona
Facultat d'Economia i Empresa
Universitat de Barcelona
C/ John M. Keynes, 1-11
(08034) Barcelona, Spain
Tel.: + 34 93 403 46 46
ieb@ub.edu
http://www.ieb.ub.edu

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PLACE-BASED POLICIES: OPPORTUNITY FOR DEPRIVED SCHOOLS OR ZONE-AND-SHAME EFFECT?*

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ABSTRACT: Even though place-based policies involve large transfers toward low-income neighborhoods, they may also produce territorial stigmatization. This paper appeals to the quasi-experimental discontinuity in a French reform that redrew the zoning map of subsidized neighborhoods on the basis of a sharp poverty cut-off to assess the effect of place-based policies on school enrollment into lower secondary education. Using a difference-in-differences approach, we find strong evidence of stigma from policy designation, as public middle schools in neighbourhoods below the policy cut-off, which qualified for place-based subsidies, saw a significant 3.5pp post-reform drop in pupil enrollment, compared to their counterfactual analogues in unlabeled areas lying just above the poverty threshold. This "zone-and-shame" effect is immediate but does not persist, as it is only found for the first pupil-entry cohort in middle schools immediately after the reform. We show that it was triggered by the behavioral reactions of parents from all socioeconomic backgrounds, who avoided public schools in policy areas and shifted to those in other areas or, only for richer parents, to private schools. We uncover, on the contrary, only weak evidence of stigma reversion after an area loses its designation, suggesting hysteresis in bad reputations.

JEL Codes: I24, I28, R23, R58
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Manon Garrouste
Univ. Lille, CNRS, IESEG School of Management
manon.garrouste@univ-lille.fr

Miren Lafourcade
Université Paris-Saclay (RITM), Universitat de Barcelona - IEB, Paris School of Economics and CEPR
miren.lafourcade@universite-paris-saclay.fr

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1 Introduction

Policy-makers worldwide have implemented place-based urban policies for over forty years to address socioeconomic disparities across neighborhoods, which are particularly marked in dense metropolitan areas (Neumark and Simpson, 2015). In Mainland France, these policies currently target 1,300 "priority" neighborhoods, or equivalently 8% of the French population (4.9 million people). As in many other countries,[1] one element of these policies is tax and payroll exemptions conditional on firm location in these priority areas. However, French urban policies do not only aim to attract business and create new employment prospects in deprived areas, they also involve policies to support education, culture, transport and health. Approximately €100 Mn additional funding per year is allocated to education in "priority" neighborhoods in France. These public subsidies, administered by municipalities, cover about 90,000 pupils each year, who benefit from personalized care or group programs to help them perform better at school.

Even though place-based policies funnel potentially-large transfers to low-income urban neighborhoods, there is a growing recognition that their effectiveness may be limited by unintentional detrimental effects. This paper focuses on one of these, commonly referred to by Sociologists as "territorial stigmatization" (Wacquant et al., 2014), but that is often overlooked by Economists. We in particular examine whether labeling neighborhoods in order to make them eligible for place-based subsidies affects spatial sorting and urban segregation, which we analyze through the lens of school choice. Urban policies targeted at low-income neighborhoods may improve school enrollment if parents expect benefits on their children's education. However, they may also stigmatize schools and affect their reputation if the policy designation conveys a negative image of the targeted neighborhoods. The effect of place-based urban policies on school choice is thus ambiguous, and whether the policy gains will be reversed by territorial stigma is ultimately an empirical question. We here address this issue, which has potentially substantial implications for social segregation at school and children's educational outcomes.

There is considerable empirical evidence that neighborhoods and school environments determine children's life-time opportunities. However, identifying the changes in opportunities arising from residential sorting and neighborhood or school attributes remains particularly challenging (Cutler and Glaeser, 1997; Sharkey, 2016). For social scientists primarily interested in education outcomes the issue is even more salient, given that pupils' assignment to public schools is most-often based on a catchment area system, as is the case in France. As parents are partially constrained by a legal map of school districts, residential sorting and social segregation at school reinforce each other, as families can strategically choose where to live taking into account the quality and reputation of schools, which capitalizes into housing prices (Bayer et al., 2007; Fack and Grenet, 2010; Collins and Kaplan, 2017).

We overcome this econometric challenge by exploiting the quasi-natural experiment provided by a French reform that redrew the map of urban neighborhoods eligible for place-

[1] For example, Federal Empowerment Zones or the UK Local Employment Growth Initiatives.

based subsidies on the basis of a sharp poverty cut-off that was not predictable nor manipulable by individuals. Some neighborhoods that were not previously targeted by the French urban policy now qualified, as they had median income below the poverty cut-off; equally, others that were previously treated were no longer eligible as their median income was above this threshold. As such, without any concomitant change in school catchment areas, some schools "entered" or "exited" policy treatment. This reform, which took place a few months before the start of the 2014-2015 school year, provides a unique opportunity for the causal identification of the impact of French urban policy on school choice. It allows us to tackle two econometric issues that are commonly associated with the evaluation of place-based policies. First, as policy (dis-)qualification targeted the most- (least-) deprived neighborhoods, which are more (less) likely to host pupils with difficult backgrounds and thus to be avoided by parents,[2] we use school fixed effects to control for selection into treatment. Doing so, we can control for key confounding factors such as residential sorting across neighborhoods. We also control for many pupil and family characteristics, as well as time-varying measures of school performance and environment. Second, we appeal to the discontinuity design of the reform to construct credible counterfactual schools. As the reform was based on a non-manipulable income cut-off, we can see whether schools in neighborhoods lying below (above) the poverty threshold witnessed significant changes in pupil enrollment after entering (exiting) zoning compared to schools in neighborhoods on the other side of the threshold.

We find strong evidence of stigma from policy designation, as public middle schools in labeled neighborhoods saw a significant 3.5pp post-reform drop in pupil enrollment, compared to their counterfactual analogues in non-designated areas lying just above the poverty threshold. This "zone-and-shame" effect is immediate but does not persist, as it is only found for the first pupil-entry cohort in middle schools immediately after the reform. We show that it was triggered by the behavioral reactions of parents from all socioeconomic backgrounds, who avoided public schools in policy areas and shifted to those in other areas or, to a lesser extent and only for richer parents, to private schools. We uncover, on the contrary, only weak evidence of stigma reversion after an area loses its designation, suggesting hysteresis in bad reputations.

In addition to these contributions, we add to the extensive literature showing that place-based policies have only few positive effects on residents' outcomes (Freedman, 2015; Freedman et al., 2021), especially in France (Malgouyres and Py, 2016; Lafourcade and Mayneris, 2017). We first expand the focus to education outcomes, which helps fill an overlooked, although policy-important, gap in this literature. Most papers evaluating place-based policies have focused on Enterprize Zones (hereafter EZ), as these are the most-widespread urban policy across the world. EZ aim to attract businesses to deprived neighborhoods via lower taxes conditional on firm location. However, their effectiveness in reviving low-income neighborhoods is ambiguous. EZ policies are generally successful in attracting economic activities that

[2]There is evidence in France that pupils living in deprived neighborhoods have poorer academic outcomes than other pupils on average (Baccaïni et al., 2014; Bressoux et al., 2016; ONPV, 2019; Cour des Comptes, 2020; Alivon, 2021).

were previously dormant due to insufficient expected profits, especially in zones benefiting from good transport connections (Briant et al., 2015). However, they also attract businesses that would have located elsewhere absent the program (Bondonio and Engberg, 2000; Hanson and Rohlin, 2013; Givord et al., 2013; Mayer et al., 2017; Einiö and Overman, 2020), or that quickly move away or go bankrupt after the tax-exemption period (Givord et al., 2018), thereby generating potentially-large windfall effects or negative externalities for untargeted neighborhoods. The local labor market effects of place-based policies depend on the adjustment of labor supply to firm location in the targeted area. If the new labor demand is met by the local labor force, EZs yield a substantial rise in local employment and lower unemployment (Ham et al., 2011; Busso et al., 2013; Freedman, 2013; Bartik, 2020), and all the more so that they are associated with local hiring requirements (Charnoz, 2018; Ehrlich and Overman, 2020). Otherwise, they do not reduce local unemployment much (Gobillon et al., 2012; Gibbons et al., 2021), and the composition effects triggered by the arrival of more-employable residents lie behind a large part of any such drop (Freedman, 2012; Charnoz, 2018; Chyn and Katz, 2021), suggesting the gentrification benefits of the policy are captured by an initially untargeted population (Reynolds and Rohlin, 2015; Freedman, 2015). There is also evidence that EZ policies capitalize into real estate markets (Hanson, 2009; Ehrlich and Seidel, 2018; Kitchens and Wallace, 2022), especially if the treated neighborhoods cannot quickly adjust their supply of commercial or residential property.

Much less is known about the causal impact of place-based urban policies on residents' education, as the most-prominent existing work considers spatial variations in neighborhood and school attributes via children's moves. For instance, Gould et al. (2004) and Gould et al. (2011) exploit variations in living conditions experienced by the arrival of Ethiopian and Yemenite communities brought to Israel and show that children who were placed in more-favorable schooling or urban environments experienced better education outcomes in adulthood. Äslund et al. (2011) build on a similar quasi-experimental refugee-placement policy in Sweden to show that child migrants who arrived at a young age in neighborhoods with a larger share of highly-educated adults from their own ethnicity perform better at school. The MTO experiment and housing demolition quasi-experiments in the U.S. also suggest that moving to a higher-income neighborhood prior to adolescence can yield dramatic improvements in educational outcomes (Chetty et al., 2016), and later economic opportunities (Chetty and Hendren, 2018a,b; Chyn, 2018). Baum-Snow et al. (2019) is one exception in this respect, as they appeal to quasi-random variation in skill-specific labor demand shocks that hit U.S. urban census tracts to analyze how neighborhood attributes affect the education outcomes of children who stay put. However, regardless of whether they focus on child movers or stayers, all of the previous contributions provide only indirect evidence that urban policies affect educational inequality, as they do not establish whether labeling and/or treating neighborhoods actually changes the educational outcomes of incumbent students.

Our work also contributes to a large body of evidence on the impact of place-based affirmative action aiming to increase the enrollment of children from low-income neighborhoods at better schools. For instance, Guyon (2022) finds that closing down a middle school located

in a deprived neighborhood and reallocating its students to other schools in the same city reduces the probability that the moved students, and especially the most-disadvantaged, drop out after middle school. On the contrary, Behaghel et al. (2017) show that moving disadvantaged adolescents to boarding schools only benefits the initially-strongest students, and only once they have adapted to their new school. In Abdulkadiroğlu et al. (2014) and Dobbie and Fryer (2014), attending a school with high-achieving peers has little impact on academic outcomes, while initially-weaker applicants from low-income neighborhoods may even suffer a short-term reduction in their well-being and self-esteem, due to their worse relative ranking in selective schools (Behaghel et al., 2017; Barrow et al., 2020).

This paper is also related to a small body of U.S. evidence showing the positive impacts of the extra resources provided to schools on students' exam scores (Card and Payne, 2002; Papke, 2005; Jackson et al., 2015) and educational qualifications and later earnings (Lafortune et al., 2018; Schmick and Shertzer, 2019). However, similar analyses in France and Romania yield less-optimistic results (Bénabou et al., 2009; Feigenberg et al., 2019; Benhenda and Grenet, 2020; Munteanu, 2022). A small number of papers have even concluded that compensatory education policies may be counterproductive, if schools invest their extra resources in less-efficient teaching methods (Leuven et al., 2007), or if families from different social backgrounds select into (or out of) the targeted schools (Beffy and Davezies, 2013; Davezies and Garrouste, 2020).

Last, our work is more-closely related to the burgeoning literature on the legacy of U.S. 'redlining' maps on the development of the urban neighborhoods that were classified as the most 'risky' for investment. For example, Aaronson et al. (2021) show that redlining has had a profound long-lasting influence on various local outcomes such as home-ownership rates, house values and rents, and racial segregation. In Aaronson et al. (2022) children living in the lowest-graded neighborhoods had significantly lower levels of educational attainment. Equally, some non-U.S. analyses have underlined that labeling neighborhoods may stigmatize their residents in various ways, by discriminating against them on the labor market (Petit et al., 2020), depreciating the value of housing (Koster and van Ommeren, 2022), reducing economic transactions in the neighborhood (Besbris et al., 2014), and having a detrimental effect on children's schooling (Domínguez et al., 2022).

We are different from such previous work in a number of ways, however. We first appeal to exhaustive longitudinal administrative data from multiple cohorts of students over a decade, which we combine with rich geo-coded information on middle schools, neighborhoods and urban policies, to investigate school-choice responses to neighborhood labeling and treatment. The second novelty comes from our econometric strategy that combines a discontinuity design with panel techniques to avoid confounding the impact of policy designation with neighborhood or school-composition effects. Last, in contrast to most previous contributions, we show that living in a treated neighborhood affects children not only through mechanisms involving school resources and peer networks, but also via significant changes in the perception of school quality. As such, policy designation influences parental beliefs and perceived educational returns in a very similar way to the public display of information on

school scores in the media (Friesen et al., 2012; Koning and van der Wiel, 2013).

The remainder of the paper is organized as follows. Section 2 introduces the institutional context and describes the reform we use to evaluate the causal impact of neighborhood labeling on school choice. Section 3 then presents the empirical framework and the data, and Section 4 outlines the average treatment effects. Section 5 checks the robustness of our point estimates, and Section 6 explores heterogeneous effects across various dimensions, including parental socioeconomic status and occupation. Last, Section 7 concludes.

2 The French institutional background

Spatial inequalities in French cities have risen dramatically over the past four decades, and their consequences in terms of segregation, exclusion, and juvenile delinquency and violence, have underlined the need for innovative political responses. French urban policy has primarily aimed to reduce the vulnerability of low-income neighborhoods. It is cross-ministerial and addresses multiple domains, including education and early childhood, employment preservation and job creation, housing rehabilitation and urban renewal, health, social cohesion, security and the prevention of delinquency. Policy consists in both the enhancement of ordinary-law policies in treated areas, and the use of specific measures such as tax rebates and additional public support for the local urban fabric and population.

From their inception in the late 1970's, place-based urban policies in France have been applied via a variety of zoning systems and eligibility rules for public subsidies within the spatial perimeters covered by the policy. In this section, we briefly document the history of the French urban zoning system up to the reform that we will analyze: the Lamy Law for cities and urban cohesion, which was passed in February 2014.

2.1 Place-based urban policies in France

The surge in repeated urban riots in the 1970's, 1980's and 1990's underlined the distress of the young urban unemployed, and prompted French politicians to launch a comprehensive set of measures entitled *Pacte de Relance pour la Ville* (Urban Stimulus Package) in 1996. As for other concomitant programs around the world,[3] this was originally designed to produce a substantial stimulus in deprived urban neighborhoods.

The first pillar of this program was based on a three-tier zoning system of deprived urban neighborhoods: the first tier, composed of 751 *Zones Urbaines Sensibles* or ZUS (Urban Sensitive Zones), was initially formed by urban neighborhoods with a derelict housing stock and a low ratio of jobs-to-residents. Of these, 416 *Zones de Redynamisation Urbaine* or ZRU (Urban Regeneration Zones) became Enterprize Zones (EZ afterwards). The ZRU were selected by ranking the ZUS on a multi-dimensional index of deprivation covering the size of the local population, the unemployment rate, the proportion of residents with no qualifications, the

[3]The most-notable examples include the Social City program in Germany, the Big City program in the Netherlands, the National Strategy for Neighborhood Renewal in Great Britain and the HOPE IV program in the US.

share of young residents, and the local tax base. The 44 most-deprived ZRU were declared *Zones Franches Urbaines* or ZFU (these would subsequently be known as the first-generation of ZFU).[4] Last, 66 additional ZFU were created in 2004 and 2006 (the second and third ZFU generations), from the stock of ZRU that were not already designated as ZFU.[5]

From the outset, the French EZ program was supplemented by an education program designed to fight the school failure and dropout of disadvantaged children living in ZUS. In particular, the *Programme de Réussite Educative* or PRE (Educational Success Program) provided an additional funding of approximately €100 Mn per year for child tutoring, child homework help, and early detection of child eyesight problems or learning difficulties.[6]

In 2007, the total annual cost of the EZ and PRE programs was over half a Billion Euros. French urban policy yet expanded to cover an additional 1,750 neighborhoods experiencing unemployment, violence or housing difficulties. Urban Social Cohesion Contracts (CUCS hereafter) were signed between the central and local authorities in charge of almost all of the neighborhoods treated by this urban policy (i.e. a total of about 2,500 zones),[7] committing them to concerted action to improve residents' daily lives. Almost all areas in the first pillar (741 of the 751 ZUS) could thus combine the benefits of the various zoning systems.

2.2 The 2014 reform to the French urban zoning system

The juxtaposition of these zoning systems forced French public authorities to combine a regulatory approach based on automatic qualification (ZRU/ZFU), with a contractual approach generating potential, but not automatic, benefits (other ZUS/CUCS), with the two approaches not necessarily being applied to the same urban areas. In 2012, the French Audit Court roundly criticized the dilution of public policy over urban zoning systems that were too dispersed and complex, and the low cost-benefit ratio of the policy (Cour des Comptes, 2012). In an effort to increase the cost-effectiveness and public understanding of the policy, and to harmonize the legal and contractual zoning systems, the then French Minister of Urban Affairs, François Lamy, undertook a complete overhaul of urban policy, which was announced and implemented in mid-2014.

To reduce the dilution of public resources, zoning systems that had accumulated over the past decades were replaced by a single tighter urban-zoning scheme in order to concentrate public support on approximately 1,300 neighborhoods in Mainland France (and another 214 neighborhoods in French overseas Departments and Polynesia) that were called "Priority" neighborhoods (*Quartiers Prioritaires* or QP thereafter). From 2014, a unique poverty criterion was used to identify those neighborhoods: median income below 60% of a reference income calculated as a weighted average of the nationwide and citywide median incomes per con-

[4] Firms locating in a ZRU or ZFU benefited from multiple tax credits and payroll exemptions, the generosity and longevity of which was much higher in ZFU, where local recruitment was encouraged via a local-hiring requirement for payroll-tax exemptions.

[5] The perimeter of the initial ZUS was sometimes expanded to include vacant land to attract more businesses, resulting in two urban zoning systems that were not perfectly nested.

[6] See Demangeclaude (2018) for a detailed presentation of the PRE, and Bressoux et al. (2016) for an evaluation of its impact on children's well-being and cognitive skills.

[7] Figure B1 in Appendix B illustrates these imperfectly-nested "Russian doll" zoning systems.

sumption unit.[8] Let I_{FR} denote the median income per consumption unit in mainland France and I_{UU} its counterpart in a given urban unit, neither of which are manipulable by local authorities.[9] The reference income I_R was then:

- For urban units between 10,000 and 5 million inhabitants: $I^R_{UU} = 0.7 \times I_{FR} + 0.3 \times I_{UU}$;
- For urban units over 5 million inhabitants:[10] $I^R_{UU} = 0.3 \times I_{FR} + 0.7 \times I_{UU}$.

The detection of poverty clusters was then based on a very disaggregated scan (200-meter squares) of France. Contiguous squares of more than 1,000 inhabitants below the poverty cut-off were amalgamated to form a single unbroken zone. QP boundaries generally followed the street map, and were sometimes adjusted marginally at the request of local authorities, as long as the boundary changes complied with the poverty cut-off. The resulting new zoning system is illustrated in Figure 1 for the Paris region (the QP are the dark grey areas, and the other green areas refer to the old zoning systems).

Public policy regarding these QP was set out in six-year State-City contracts with four main objectives: (i) increasing social cohesion (through non-for-profit organization subsidies or the construction of new social, cultural, transport, health and sport facilities); (ii) improving the residential living environment (through construction of new public housing, the rehabilitation of existing social housing, and public subsidies for private home-ownership); (iii) promoting economic development, employment and labor-market participation (through tax breaks or other positive-discrimination policies); (iv) fostering educational achievement and crime prevention (through extra-resources to local schools and/or young residents).

Importantly, the reform provided residents with the opportunity to actively engage in the underlying political process, as citizens' councils were set up to help develop the State-City contracts. Moreover, the reform was accompanied by the introduction of search engines to help individuals obtain precise information on the policy zoning (see Figure A1 in Appendix A). Figure 2 shows that the number of Google queries regarding the new urban policy peaked at the date of the reform's announcement, a few months before the 2014-2015 school year. This empowerment and information of residents, absent in previous urban policies, could have encouraged some parents to quickly adapt their behavior as a response to the reform.

It is also worth pointing that most (around 85%) of the newly-treated areas overlapped with previously-treated areas, most of which continued to be monitored or even treated after the reform, even though they might have not qualified for the new-policy program.[11]

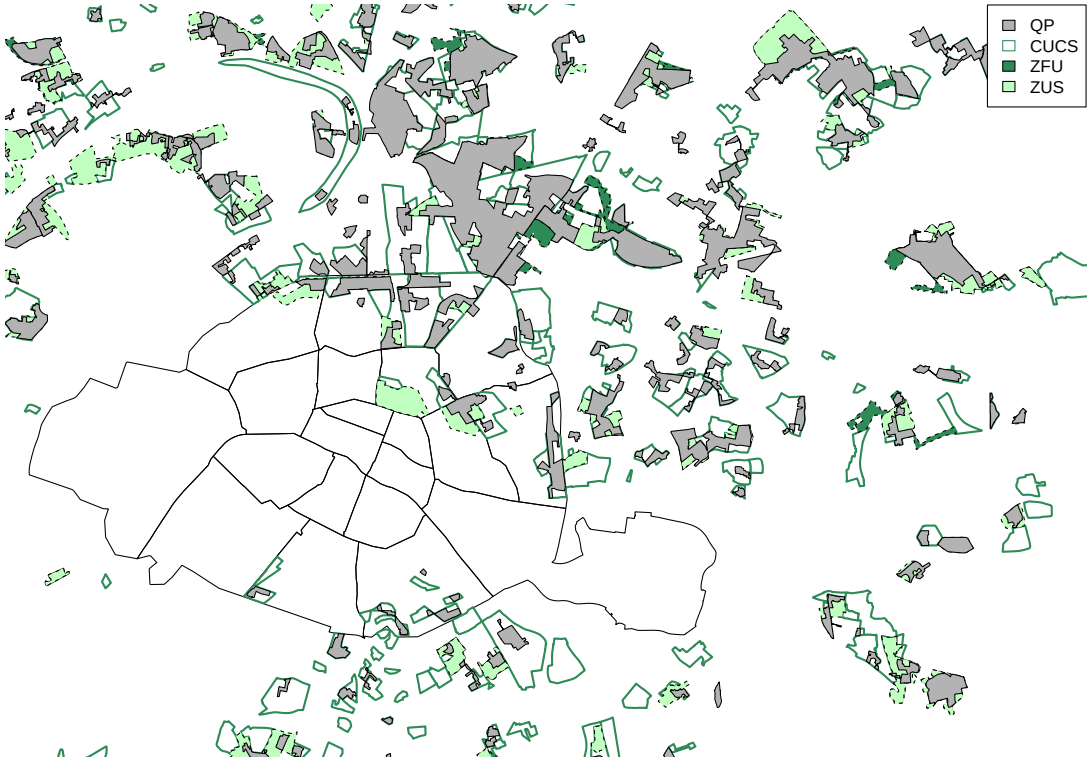
[8] The French Statistical Administration calculates consumption units as follows: the first adult in a household counts for 1, other adults (age 14 or over) for 0.5, and children under 14 for 0.3.

[9] In France, an urban unit is a municipality (or a group of adjacent municipalities) with over 2000 inhabitants forming a single unbroken spread of built-development (i.e. no buildings separated by more than 200 meters).

[10] The data used for this calculation (*Revenus fiscaux et sociaux localisés des ménages 2011*) come from the French National Institute for Economic Studies (Insee, herafter), and refer to gross incomes (i.e. before redistribution) in 2011. For urban units above 5 million inhabitants, i.e. Paris, more weight is put on the local median income, as it is significantly higher than the national median income (€22,048, against €19,218 in Mainland France).

[11] For instance, businesses were eligible for ZFU rebates up to 14 years after their installation, so that a firm that set up in a ZFU in 2013 would be eligible for ZFU subsidies until 2027.

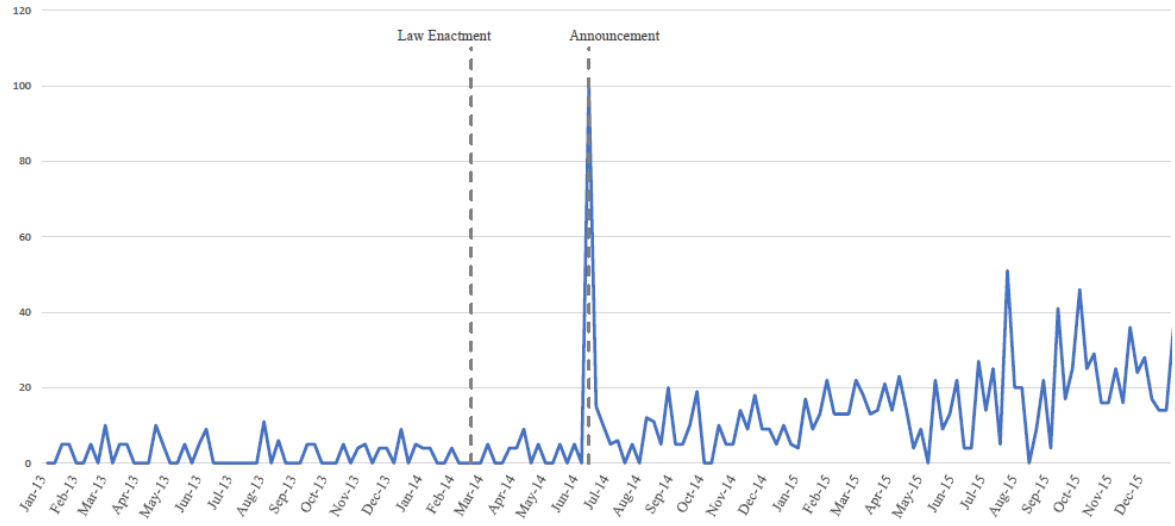
Figure 1 – Old and new urban zoning systems in the Paris area



Source: Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET).

Note: The boundaries of Parisian *arrondissements* appear in black. *New urban zoning*: Priority neighborhoods (QP) in dark grey. *Former urban zoning systems*: Urban Sensitive Zones (ZUS) in light green, Enterprize Zones (ZFU) in dark green, Urban Social Cohesion Contracts (CUCS) in empty green polygons.

Figure 2 – Google queries for *Quartier Prioritaire de la politique de la ville (QP)*



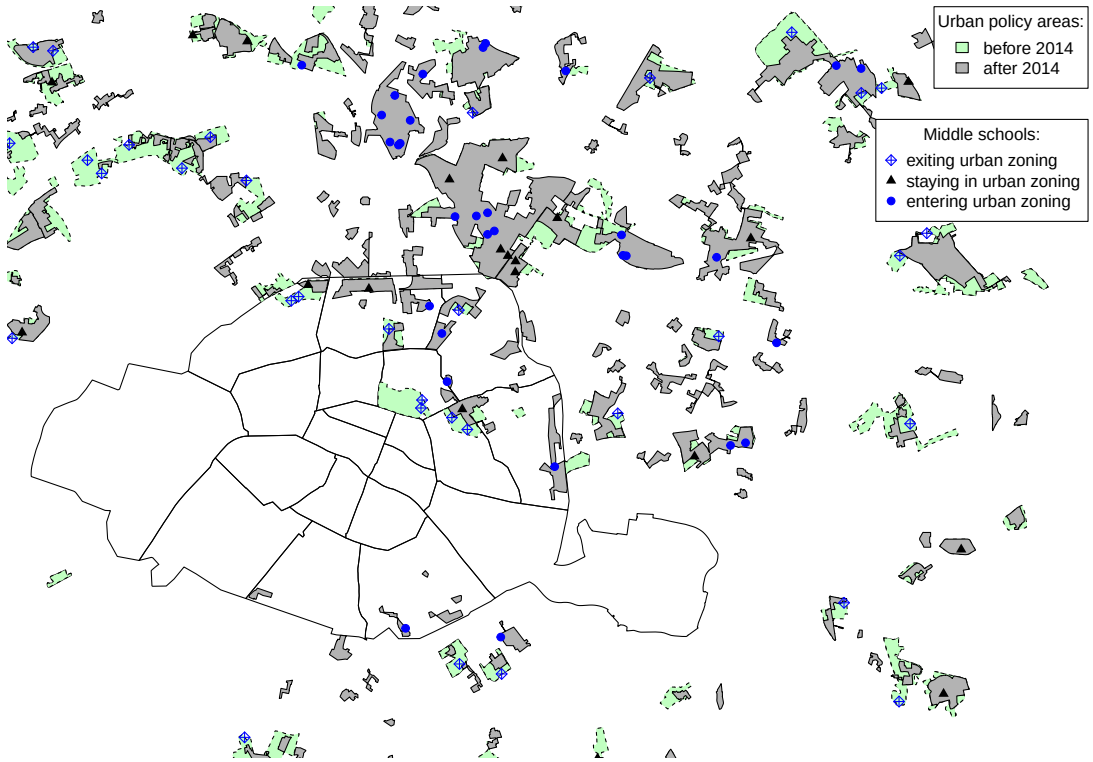
Source: Google.

This treatment hysteresis implies that the 2014 reform may have had a larger impact in incoming than outgoing neighborhoods, a conjecture for which we will find support in our empirical analysis below.

2.3 An illustration of the reshuffling of middle schools driven by the reform

The Lamy reform provides a unique opportunity to exploit boundary changes in urban zoning to estimate the impact of place-based policies on school choices and social segregation at school. Following the reform, some previously-treated neighborhoods now did not qualify for support as their median income was above 60% of the reference income, whereas some previously non-treated neighborhoods now became eligible as their median income was below this threshold. As a result, without any change in school catchment areas at the time of the reform, schools in the newly-treated neighborhoods "entered" into the policy zoning, whereas those in no-longer-treated neighborhoods "exited".

Figure 3 – The 2014 urban policy reform in the Paris region



Sources: Base centrale des établissements (DEPP - Ministère de l'Éducation), shapefiles from the French Ministry of Urban Affairs (ANCT-CGET).

Note: The boundaries of Parisian *arrondissements* appear in black. *New urban zoning*: Priority neighborhoods (QP) in dark grey. *Former urban zoning systems*: Urban Sensitive Zones (ZUS) and Urban tax-Free Zones (ZFU) in light green (for the sake of clarity, CUCS are not represented, but will be considered afterwards). Hollow blue diamonds (solid blue circles) represent middle schools that "exited" ("entered") policy coverage, and solid black triangles those that had and continue to have policy-coverage.

Figure 3 illustrates the reshuffling of middle schools brought by the Lamy reform in the Paris region, which includes many well-known deprived neighborhoods covered by French urban policy. But the reshuffling was also relatively-substantial in many other French cities. We will below exploit these spatio-temporal shifts all over France to quantify the causal effect of neighborhood designation and disqualification on school enrollment.

2.4 School-based compensatory education policies in France

This paper focuses on middle schools, and more specifically on pupils entering 6th grade for the first time (i.e. pupils aged about 11-12) over the 2010-2019 period. Education is compulsory in France for children aged 6 to 16, with five years of education in primary schools, followed by four years of lower secondary education in middle schools and then three years of higher secondary education in high schools. Middle-school choice is a key decision for families for at least two reasons. First, children change school between primary and lower secondary education, and the type of schooling changes from a single class with only one teacher in primary school to a number of classes with different teachers and class subjects in middle school. Second, lower secondary education has a substantial influence on pupils' education paths, as it conditions the choice between a vocational and an academic track later on, and all the more so that there is considerable social segregation across French middle schools (Boutchenik et al., 2020; Ly and Riegert, 2015).

As in many other countries, child allocation to public schools in France is based on catchment areas, in which pupils are assigned to one single public school according to their parents' address.[12] French public schools charge no tuition fees and have to accept all pupils, regardless of their family background, previous academic performance or special learning needs. Most pupils go to their catchment-area school, which is the default rule. However, parents can ask to enroll their child in another public school. As long as the maximum capacity of that school is not attained after accepting all of its default pupils, dispensations can be granted by academy inspectors, primarily to students with disabilities, with merit- or social-based scholarships, with specific medical needs, with a sibling already enrolled in the requested school, who wish to follow a particular curriculum in that school (music, sport or foreign-language tracks for instance), or who live very close to it. Families can also opt for the private sector, which is not subject to any boundary constraints. Most private schools are publicly-funded and follow the same national curriculum as public schools (except for religious instruction, as most private schools are Catholic). Private schools charge fees, which are low on average in France as compared to other countries, so that the private sector is affordable for many families. The share of pupils in private-sector lower secondary education is then relatively high in France (at over 20%, see Table 2 in Section 3.2 below).

Disadvantaged schools can also benefit from specific compensatory education programs (school-based policies) overseen by the French Ministry of education, and that are partly decoupled from the urban zoning system (place-based policies). The first compensatory scheme entitled *Zones d'éducation prioritaire* ("Priority education zones", or ZEP hereafter) was designed in 1981 to provide extra resources to 300 schools where social and academic disadvantages were the highest, corresponding to 10% of lower-secondary education students. This compensatory education program was originally meant to be temporary, but was substantially expanded throughout the 1980's and 1990's. In the 1990's, successive reforms expanded its coverage to additional schools, and created compensatory education "networks" by group-

[12] See Musset (2012) for a review of school-choice systems in OECD countries.

ing primary, middle, and high schools within catchment areas, to share pedagogical and financial resources.[13] In the 2000's two reforms reshaped the compensatory education scheme, and defined two school groups according to their levels of social and academic deprivation. The least-disadvantaged schools were labeled *Réseaux d'éducation prioritaire* (REP) and the most-disadvantaged REP+. At the end of our analysis period, the compensatory education program covered about 1,000 middle schools, i.e. 20% of the students enrolled in lower secondary education in France (DEPP, 2021).

One important aspect of the Lamy reform was that the urban policy and compensatory education schemes were nested through a new eligibility criterion for benefiting from the REP or REP+ compensatory programs, based on the share of pupils living in a QP.[14] In practice, this interdependence requires an estimation strategy that controls for whether the middle schools benefited also from compensatory education schemes, in order to identify the separate impacts of place-based and school-based policies.

3 Empirical framework and data

Our goal is to identify the causal effect of place-based policies on school enrollment. As the correlation between school choice and urban policy is likely confounded by residential or school sorting, we use the discontinuity design from the Lamy reform in a local difference-in-differences approach and panel techniques to control for selection into treatment. Let Y_{idt} denote in turn a dummy for pupil i from cohort t, being enrolled at her catchment-area school d, at another public school, and at a private school. Our treatment variable T_{dt} indicates whether school d is in an urban neighborhood treated by the policy at time t. The probability of being enrolled in the catchment-area school (resp. another public/private school) is then estimated via a linear-probability model as follows:

$$Y_{idt} = \alpha + \beta T_{dt} + X_{it}\gamma + Z_{dt}\delta + \mu_d + \mu_t + \eta_{idt}, \tag{1}$$

where X_{it} is a vector of observed pupil characteristics, μ_t a year fixed effect, and η_{idt} the error term. Although we control for key observables at the pupil level, these estimates may well 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 μ_d, and a vector of time-varying characteristics observed for this school and its local environment, Z_{dt}.

Our identification strategy relies not only on the pseudo-panel structure of the data but also on the discontinuity design from the Lamy reform, as we also exploit the fact that, post-reform, schools in neighborhoods with median incomes below (above) the reference income "entered" ("exited") the policy zoning, while observationally-equivalent schools in counterfactual neighborhoods with median incomes above (below) the reference income remained untreated (treated). As illustrated in Figure 3 for the Paris region, we define four types of

[13] The middle school is at the center of the "network".

[14] The other eligibility criteria include the share of pupils from low socioeconomic backgrounds, with public scholarships, and who have repeated a year when entering the 6th grade.

public middle schools: (i) those outside the old urban zoning pre-reform but inside the new urban zoning post-reform, (ii) those inside the old urban zoning pre-reform but outside the new urban zoning post-reform, (iii) those inside both urban zonings (pre- and post-reform), and (iv) those outside both urban zonings (pre- and post-reform). Assuming that school "entry" or "exit" is independent of neighboring families' preferences for schools once we control for school heterogeneity, we can use the boundary changes from the reform to recover the causal effect of urban policy zoning on school enrollment.

Let T_{dt}^{entry} and T_{dt}^{exit} be respectively dummy variables for the catchment-area school d being in a neighborhood that entered or exited the policy zoning in 2014. For schools located in neighborhoods that were not treated pre-reform, we estimate the linear-probability model:

$$Y_{idt} = \alpha_1 + \beta_1 T_{dt}^{entry} + X_{it}\gamma_1 + Z_{dt}\delta_1 + \mu_d + \mu_t + \epsilon_{idt}, \tag{2}$$

and for schools in neighborhoods that were already treated pre-reform, we estimate:

$$Y_{idt} = \alpha_2 + \beta_2 T_{dt}^{exit} + X_{it}\gamma_2 + Z_{dt}\delta_2 + \mu_d + \mu_t + \varepsilon_{idt}. \tag{3}$$

The β_1 (respectively β_2) parameter provides the causal impact of the urban policy on pupils' enrollment in schools located in newly (no-longer) treated neighborhoods, relative to observationally-equivalent schools in counterfactual neighborhoods, under the assumption that enrollment in both types of neighborhoods would have followed the same trend without the reform. β_1 and β_2 can be either positive or negative. Parents in low-income neighborhoods may expect the urban policy to provide additional resources that will help their children to perform better at school. On the contrary, policy-designation may convey a negative image of the labeled neighborhoods. The "net" average treatment effect on school enrollment is hence theoretically ambiguous. If β_1 is negative for the catchment-area school choice, then, on average, the benefits of the policy are more than offset by territorial stigmatization. By analogy, if families re-evaluate school quality upwards after neighborhood disqualification, and if this appraisal overcomes the loss of public subsidies, β_2 should be negative.

The "net" effect of the urban policy on school enrollment is also potentially heterogeneous across families. If parents were imperfectly informed about school quality in their catchment area, they may then readjust their school preferences. We thus may expect that (i) high-SES families react more than low-SES families, as changing school is less costly for them, and (ii) well-informed families (for instance, teachers) will react differently from other parents. The reform may also affect school enrollment differentially over time. If many parents fear that their catchment-area school will suffer from stigma, and that the reform will lead to avoidance by other parents, coordination mechanisms and self-fulfilling expectations may perpetuate the stigma over time. If parents simply re-evaluate school quality in light of the new zoning information and media buzz following the reform and its implementation, school choices may change only in the short-run.

3.1 Counterfactual neighborhoods

To evaluate the impact of this French urban policy on school choices, we could simply compare school enrollment in labeled and unlabeled neighborhoods pre- and post-reform. However, as the common-trend assumption may not hold for those two groups even with controls for school fixed effects and other time-specific confounders, we restrict the control group to a set of plausibly-good counterfactual neighborhoods. For incoming neighborhoods, we use the method in Quantin and Sala (2018). As illustrated in Figure 4 for the Paris region, we select all census tracts that intersect contiguous squares with a median income just above (i.e. 60 to 70% of) the reference income, and among these we exclude those intersecting a QP, so that the control units are not contaminated by policy spillovers. We end up with 216 counterfactual public middle schools scattered all over France (located, by construction, in unlabeled neighborhoods) that are very similar to public schools in newly-treated zones (see Figure C1 in Appendix C for the comparative statistics). We take an analogous approach for outgoing neighborhoods, with the control schools being those in all QP's formerly ZUS, ZFU or CUCS with a median income just below (50 to 60%) of the reference income (201 public middle schools). We compare it to the set of outgoing neighborhoods not too far from the poverty cut-off (60 to 70% of the reference income) to produce more-similar treated and control groups (see Figure C2 in Appendix C for the comparative statistics). Our estimation strategy then consists in the comparison of pupils' enrollment at schools located in incoming or outgoing neighborhoods (close to the poverty threshold) to their counterparts in neighborhoods just above or below the poverty cut-off, pre- and post-reform.

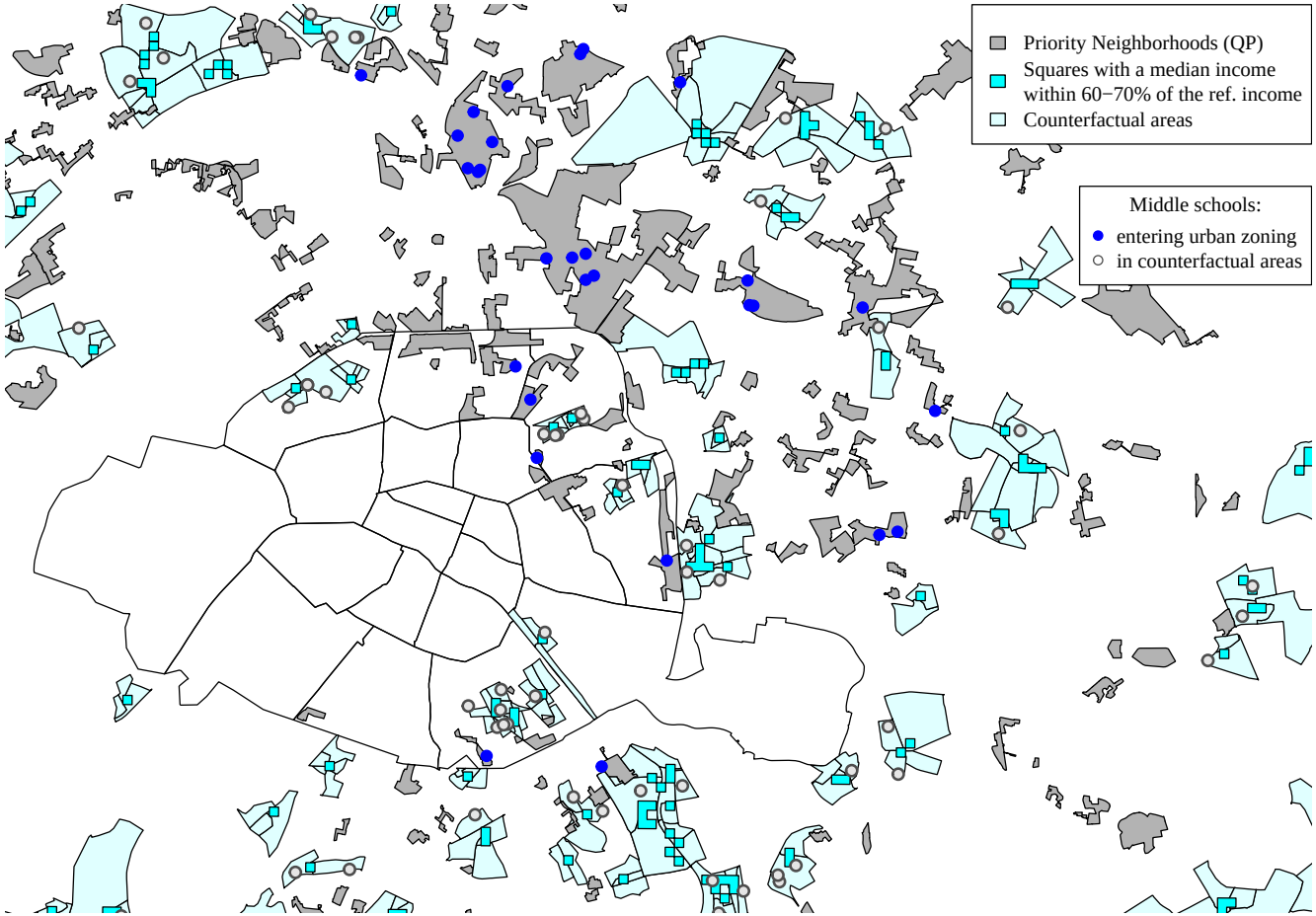
3.2 School data and descriptive statistics

We take exhaustive administrative data from various sources, described in turn from the lowest to the highest spatial granularity. We first use the *Bases centrales scolarité* (BCS hereafter) from the statistical service of the French Ministry of Education (DEPP-ADISP), which provide annual information on the universe of pupils enrolled in French schools from 2010 to 2019. We were also provided with restricted access to geo-coded micro-data on all pupils entering French lower secondary education (6^{th} grade) in September of 2011, 2013, 2015 and 2017, which we use only for robustness checks.[15] We have information of each pupil's gender, country of birth, age and the occupation of the reference parent, which we aggregate to five Socio-Economic Statuses (Very High, High, Medium, Low and Unknown SES).[16]

[15] Unfortunately, the home address of pupils was geo-coded only in odd years, so that we cannot identify school catchment areas on an annual basis from these data.

[16] Very High SES includes business managers, engineers, executives from the private and public sectors, independent/creative professions, white-collars, and teachers. High SES covers intermediate professions, technicians, clergy and retired executives/intermediate professions. Medium SES includes farmers, craftsmen, shopkeepers, public or private employees, police officers or military personnel, and retired farmers/craftsmen/traders/managers. Low SES covers blue-collars, students, and the unemployed/unoccupied. Last, SES is unknown for pupils with missing parental occupation. We do not drop this last category, which covers 4% of our sample, as this would reduce statistical power.

Figure 4 – Newly-treated and never-treated control schools in the Paris region

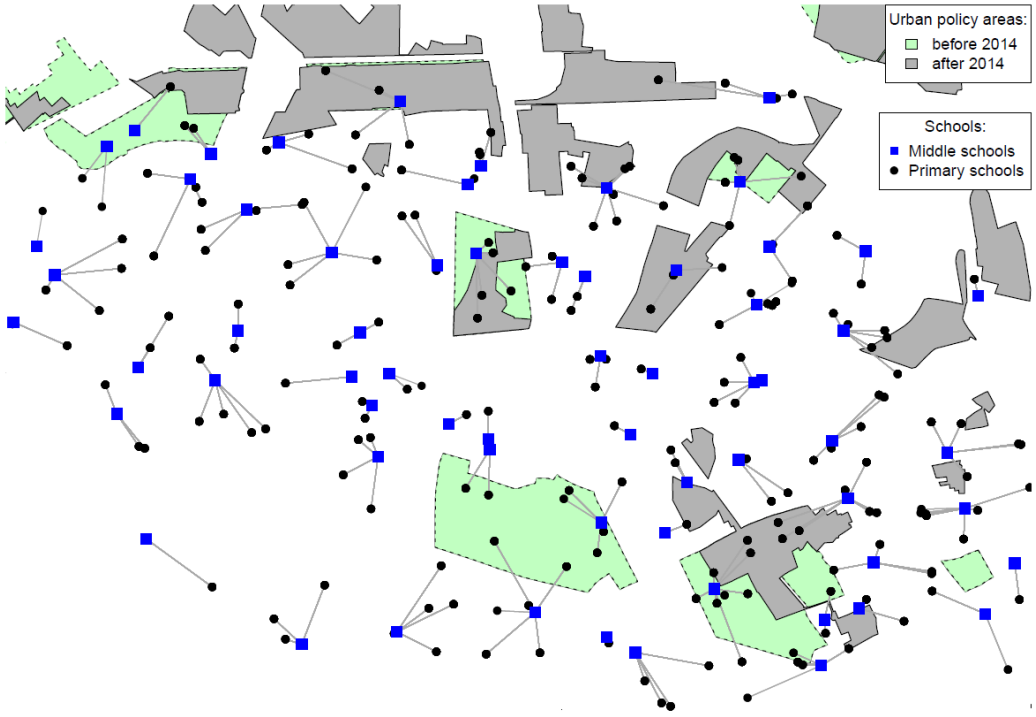


Source: Base Centrale des Établissements (DEPP - Ministère de l'Éducation), Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET), and authors' calculations based on Quantin and Sala (2018).

Notes: The dark-grey areas refer to the new policy zoning (QP), and the blue dots to middle schools "entering" this zoning. The turquoise blue squares are poverty clusters with a median income just above (60% to 70% of) the reference income. The light-blue areas are French census tracts intersecting these squares, and the grey circles indicate middle schools in those tracts.

BCS data allow us to identify the primary school of the pupil and her actual middle school of enrollment. Since we do not have a precise delineation of catchment areas over the whole of France, we define the catchment-area school as the public middle school closest to either the pupil's primary school (BCS data, see Figure 5), or the pupil's address (geo-coded data).[17] We also know whether these schools are private or public, and whether they benefit from a compensatory education program (such as REP or REP+). To take into account the schooling options that likely compete with the catchment-area school each year, we calculate various time-varying indicators such as the number of private schools within a given radius (2, 5 or 7km) from the pupil's primary school, or in the urban unit to which it belongs.[18]

Figure 5 – Catchment-areas based on each pupil's primary school in the Paris municipality



Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP.

Note: This illustration refers to the north-eastern part of the urban unit of Paris: blue squares represent public middle schools and black dots primary schools. Black segments link each primary school to its closest public middle school, defined as the catchment-area school of all pupils previously enrolled at this primary school.

For the public middle schools surveyed yearly in the online application *Aide au Pilotage et à l'Auto-évaluation des Établissements* (APAE), we also have the share of graduates of the *Brevet National des Collèges* exam (DNB hereafter) in each school.[19] There is however missing data on the DNB, which would be a problem if, for example, the most-deprived schools choose not to disclose information. We will therefore only use this information parsimoniously for robustness checks, to avoid additional selection issues.

[17] Section 5 provides robustness checks to ensure that our results do not reflect any measurement error from this assignment.

[18] These thresholds were chosen using clear criteria: 5km and 7km are the sample median and average distances between the pupil's primary school and their closest private middle school, and 2km is the average distance between the pupil's primary school and their closest public middle school.

[19] DNB is a French secondary education exam taken at the end of the 9th grade.

We complement the pupil and school data with various Geographical Information Systems from the *Agence nationale de la cohésion des territoires* (ANCT-CGET, French Ministry of Urban Affairs) providing the delineation of all the neighborhoods treated by urban policy pre- and post-reform (i.e. ZUS, ZFU, CUCS and QP). Last, the Insee gave us access to confidential data on the local median incomes used to construct the new urban zoning. We combine these with open data published at the Census Tract and Urban Unit levels to calculate the poverty cut-offs used for neighborhood policy designation, and to construct our set of counterfactual neighborhoods.

Table 1 – Description of the school sample

	Middle schools		Pupils	
	Freq.	%	Freq.	%
School type				
Public	5,139	75.2	5,832,386	78.0
Private	1,692	24.8	1,641,192	22.0
In urban zoning				
No	4,953	72.5	5,373,212	71.9
Yes	1,878	27.5	2,100,366	28.1
In compensatory education program				
No	5,838	85.5	6,422,096	85.9
Yes	993	14.5	1,051,482	14.1
Total	6,831	100.0	7,473,578	100.0
Catchment-area schools				
In urban zoning	1,479	28.9	2,291,369	30.7
Entering urban zoning	19	0.4	29,374	0.4
Exiting urban zoning	1,259	24.6	1,941,826	26.0
Exiting urban zoning ($0.6<I_r<0.7$)	450	72.3	686,137	71.2
In counterfactual areas (entry)	216	4.2	355,104	4.8
In counterfactual areas (exit)	201	3.9	320,169	4.3
Total	5,125	100.0	7,473,578	100.0

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP.

Combining all these datasets yields a sample of 6,831 middle schools in Mainland France (including Corsica), of which 1,878 (or approximately 28%) are located within the urban zoning system (see Table 1). Around 7.5 million 6^{th} graders attended these middle schools over the 2010-2019 period (see Table 2), with over half (54%) being enrolled at their catchment-area school, 24% at another public school, and 22% at a private school. Further descriptive statistics on the pupil and school samples appear in Tables 1 and 2.

Figures C1 and C2 in Appendix C provide school-composition comparisons in the treated and counterfactual neighborhoods. As expected, pupils assigned to schools in neighborhoods entering or exiting the urban policy zoning are more deprived than the overall pupil population. They come less often from High SES families, and are less often born French citizens. Our counterfactual groups of pupils are more comparable to the treated groups than the overall population, and even though some differences persist these will be entirely picked up by catchment-area school fixed effects. The only threat to our estimation strategy would then be

Table 2 – Description of the pupil sample

	Freq.	%
Gender		
Girl	3,673,594	49
Boy	3,799,984	51
Socioeconomic status		
Very High SES	1,748,272	23
High SES	955,174	13
Medium SES	2,006,649	27
Low SES	2,459,399	33
Unknown SES	304,084	4
Citizenship		
French	7,165,558	96
Other	308,020	4
Age		
7-10	213,575	3
11-12	7,248,610	97
13-17	11,393	0
Middle School Choice		
Catchment-Area School	4,069,682	54
Other Public School	1,762,704	24
Private School	1,641,192	22
Total	7,473,578	100

Source: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP.

non-parallel trends in the social composition of the treated and control schools pre-reform. To rule out the possibility that we wrongly attribute to the urban policy a factor that actually reflects pre-trends, Section 5 will provide a robustness check with treatment-group specific linear trends.

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 treatment (re-)assignment of neighborhoods.

4.1 Urban zoning entry and middle-school choices

The results from the linear-probability model used to assess the impact of neighborhood labeling on school choices (i.e. Equation (2)) appear in Table 3.

Unsurprisingly, and regardless of the changes in urban policy, pupils from High socioeconomic backgrounds are more likely to attend private schools than are those from a Medium socioeconomic background, while more-disadvantaged pupils are more likely to be enrolled in public schools, and among those schools in their catchment-area school.[20]

[20]Recall that the BCS data do not always provide information on parental occupation (this is missing for 4% of the sample), which is why Table 3 includes an "unknown" SES category. The point estimate for this category is

Table 3 – "Entry" into policy zoning and pupil enrollment

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.035**	0.041***	-0.006
	(0.015)	(0.015)	(0.008)
SES (ref.=Medium)			
Very High SES	-0.069***	-0.017***	0.086***
	(0.007)	(0.006)	(0.007)
High SES	-0.016***	-0.009**	0.025***
	(0.006)	(0.004)	(0.006)
Low SES	0.096***	0.023***	-0.120***
	(0.006)	(0.005)	(0.006)
Unknown SES	0.082***	0.041***	-0.123***
	(0.017)	(0.013)	(0.010)
Male	-0.011***	0.006***	0.005**
	(0.002)	(0.002)	(0.002)
French	-0.068***	-0.014	0.083***
	(0.010)	(0.010)	(0.009)
Age	0.011***	0.028***	-0.039***
	(0.004)	(0.003)	(0.004)
CA School in comp. educ. prog.	0.009	-0.005	-0.004
	(0.013)	(0.011)	(0.012)
No. Private Schools within 5km	0.061***	-0.075***	0.014**
	(0.013)	(0.013)	(0.007)
R^2	0.166	0.123	0.187
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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. For the sake of clarity, we do not list the constant.

The probability of going to a public school is higher for older pupils, who are more likely to be behind in their education. Conversely, the probability of going to a private school is higher for both French and male pupils. Greater private-schooling options in the catchment area raise the likelihood of parents opting for a private school, instead of another public school.[21]

By way of contrast, compensatory education policies have no discernible impact on school choice, suggesting that the extra funding provided to the most-deprived schools, beyond

very similar to that for Low SES. In Section 6, which considers heterogeneity by SES, we will combine these two categories, as well as Very-High and High SES, which also attract similar point estimates.

[21] As noted above in Section 3.2, we have calculated various time-varying indicators of the schooling options available in catchment areas. Even though significance changes slightly across indicators, our point estimates are remarkably stable across specifications. As our key findings continue to hold with alternative metrics, we hereafter stick with the number of private middle schools within a 5 km radius of the primary school, as this is the indicator with the greatest spatio-time variability.

urban policy subsidies, do not make them more attractive to parents, in line with the literature on priority education schemes in France, in particular Bénabou et al. (2009) and Davezies and Garrouste (2020).

As shown in column (1), after the 2014 reform, public schools in labeled neighborhoods experienced a significant 3.5 percentage-point drop in pupil enrollment, relative to observationally-equivalent public schools in similar (but unlabeled) counterfactual neighborhoods. As such, policy designation seems to have produced a negative image of the labeled neighborhoods, and so changed parents' perceptions of school quality. Columns (2) and (3) show that the parents who avoided their catchment-area school switched their children to other public schools (+4.1 pp), rather than to private schools (where the coefficient is insignificant).[22]

Table 4 displays the short- and medium-run effects of policy designation on school choice, allowing the treatment to vary over time.[23] The "zone-and-shame" effect triggered by neighborhood labeling is immediate, and reaches its peak in the school year immediately following the reform (-4 pp: see column 1). As the 6^{th}-grade cohorts consist of about 150 pupils per catchment area on average, a 4 pp drop in the enrollment probability corresponds to six fewer pupils per school in treated neighborhoods relative to their analogues in counterfactual unlabeled neighborhoods, immediately after the reform. However, the stigma disappears over time, as the point estimates are insignificant from 2015 onwards. The absence of any medium-run impact of neighborhood labeling suggests that the mechanisms behind school stigmatization are mostly informational and do not reflect parental coordination. It is worth noting that, even if the stigma is visible only in the first school-year after the reform, the penalty will endure, as lower secondary education lasts four years in France and very few students change middle school throughout this period. The additional sorting and exacerbation of school segregation from neighborhood labeling will therefore persist for some time.

Table 4 shows that neighborhood labeling spurred parents to shift to other public schools outside the policy zoning (+2.2 pp higher enrollment: see column 3), while other public schools in the urban zoning seem to be affected by the same lack of parental trust as the catchment-area school. This is consistent with parents using the stigmatizing information conveyed by the reform to re-gauge school quality quickly post-reform.

A standard way of testing the difference-in differences common-trend assumption is to see whether policy changes occurred pre-reform. Table 4 shows that the pre-reform "fake" treatment is never significantly different from zero before 2014, in line with the finding in Quantin and Sala (2018) of no significant difference in the median income and employment rates of incoming neighborhoods and their counterfactual areas over the 2007-2012 period.

[22]Note that we cluster standard errors at the catchment-area school level, even though treatment is at the neighborhood level, as most neighborhoods have only one public middle school. Clustering by neighborhood does not change the significance of our point estimates. Results available upon request.

[23]In the following, pupil and catchment-area characteristics attract similar estimated coefficients to those in Table 3, and we do not show or discuss these estimates.

Table 4 – "Entry" into policy zoning and pupil enrollment - Changes over time

	Probability to enroll at:			
	CA School	Other Public School		Private School
		In zoning	Out zoning	
$T^{entry-2011}$	-0.004	-0.006	0.007	0.004
	(0.014)	(0.006)	(0.012)	(0.007)
$T^{entry-2012}$	-0.006	0.012	-0.000	-0.006
	(0.013)	(0.009)	(0.010)	(0.011)
$T^{entry-2013}$	0.016	-0.001	-0.005	-0.010
	(0.015)	(0.007)	(0.007)	(0.010)
$T^{entry-2014}$	-0.040***	0.013	0.022**	0.005
	(0.013)	(0.011)	(0.010)	(0.010)
$T^{entry-2015}$	-0.007	-0.003	0.007	0.003
	(0.011)	(0.007)	(0.008)	(0.009)
$T^{entry-2016}$	-0.004	0.003	-0.008	0.009
	(0.013)	(0.008)	(0.011)	(0.008)
$T^{entry-2017}$	0.008	0.003	0.004	-0.015*
	(0.013)	(0.007)	(0.012)	(0.009)
$T^{entry-2018}$	-0.006	0.010	-0.008	0.004
	(0.015)	(0.011)	(0.013)	(0.012)
$T^{entry-2019}$	0.015	-0.001	-0.002	-0.012
	(0.014)	(0.008)	(0.011)	(0.009)
R^2	0.166	0.143	0.129	0.187
No. obs	384,478	384,478	384,478	384,478
No. clusters	235	235	235	235
Pupil's characteristics	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant, and the coefficients on these controls are not listed.

4.2 Exit from urban zoning and middle-school choices

We carry out a symmetric analysis of the causal impact of the urban policy on de-zoned areas. To ensure the similarity of our treated and control groups, we compare public school enrollment in census tracts with median income just above (60 to 70% of) the reference income (that are no longer covered by the urban policy) to public school enrollment in the QP that were formerly ZUS, ZFU or CUCS with median income just below (50 to 60% of) the reference income (that continue to be covered by urban policy post-reform). Tables 5 and 6 list the coefficients from the various specifications of Equation (3).

Table 5 – "Exit" from policy zoning and pupil enrollment

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{exit}	0.002	-0.012**	0.010***
	(0.005)	(0.005)	(0.003)
SES (ref.=Medium)			
Very High SES	-0.114***	-0.010*	0.124***
	(0.006)	(0.005)	(0.006)
High SES	-0.037***	0.002	0.034***
	(0.004)	(0.004)	(0.004)
Low SES	0.122***	0.003	-0.125***
	(0.005)	(0.004)	(0.004)
Unknown SES	0.096***	0.033***	-0.129***
	(0.010)	(0.009)	(0.007)
Male	-0.010***	0.008***	0.002*
	(0.001)	(0.001)	(0.001)
French	-0.068***	0.007	0.061***
	(0.006)	(0.006)	(0.004)
Age	0.027***	0.026***	-0.053***
	(0.002)	(0.002)	(0.002)
CA School in comp. educ. prog.	0.010	-0.009	-0.001
	(0.006)	(0.007)	(0.005)
No. Private Schools within 5km	0.029***	-0.027***	-0.002
	(0.006)	(0.005)	(0.004)
R^2	0.167	0.114	0.211
No. obs	954,666	954,666	954,666
No. clusters	616	616	616
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

We find symmetric, although less significant, reform impacts on public schools located in outgoing neighborhoods that are not too far above the poverty cut-off. Column (1) of Table 6 suggests that policy disqualification raised the probability that parents in de-zoned neighborhoods choose their catchment-area school by 1.1 pp immediately post-reform, but not at later dates, relative to parents in counterfactual still-treated neighborhoods.[24] Symmetrically to entry, the benefits for public schools in de-zoned neighborhoods come at the expense of public schools outside the policy zoning, which experienced a 1 pp drop in enrollment (column 3 of Table 6). This revival in the attraction of public schools in dis-labeled neighborhoods (at un-

[24] As above, the estimated coefficients on pupil and catchment areas characteristics are similar in all specifications. These are listed in Table 5, but not in subsequent Tables.

Table 6 – "Exit" from policy zoning and pupil enrollment - Changes over time

	Probability to enroll at:			
	CA School	Other Public School		Private School
		In zoning	Out zoning	
$T^{exit-2011}$	-0.004	0.009	-0.005	0.001
	(0.007)	(0.005)	(0.005)	(0.005)
$T^{exit-2012}$	0.011	-0.008	-0.009**	0.006
	(0.007)	(0.005)	(0.004)	(0.005)
$T^{exit-2013}$	-0.005	0.001	0.004	-0.000
	(0.006)	(0.005)	(0.004)	(0.005)
$T^{exit-2014}$	0.011*	-0.002	-0.010***	0.001
	(0.006)	(0.005)	(0.004)	(0.004)
$T^{exit-2015}$	-0.003	-0.001	0.003	0.001
	(0.005)	(0.005)	(0.004)	(0.004)
$T^{exit-2016}$	-0.009*	0.000	0.000	0.008*
	(0.005)	(0.004)	(0.004)	(0.004)
$T^{exit-2017}$	0.000	0.005	-0.004	-0.002
	(0.006)	(0.005)	(0.005)	(0.004)
$T^{exit-2018}$	-0.004	0.002	-0.004	0.006
	(0.006)	(0.005)	(0.005)	(0.004)
$T^{exit-2019}$	0.004	-0.001	0.003	-0.006
	(0.006)	(0.004)	(0.004)	(0.004)
R^2	0.167	0.167	0.140	0.211
No. obs	954,666	954,666	954,666	954,666
No. clusters	616	616	616	616
Pupil's characteristics	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

der two more pupils per cohort) nevertheless did not suffice to offset the penalty of previous neighborhood labeling. It was moreover rapidly counterbalanced by a backlash in the aftermath of the reform (-0.9 pp in 2016) to the benefit of the private sector (+0.8 pp), explaining the overall insignificant impact of neighborhood de-zoning on the catchment-area school choice (+0.2 pp: column 1 of Table 5), and its significantly positive average impact on private-school choice (+ 1 pp: see column 3 of Table 5).

These more-mixed results for areas exiting zoning are unsurprising in the light of the reform design described in Section 2. Areas only gradually exited from the place-based program, and had been treated long prior to de-zoning. The informational shock from the reform

was therefore probably insufficient to undo decades of public school stigmatization in areas disqualified from the urban policy. Hysteresis in parental beliefs may thus have prevented any drastic reassessment of school quality, despite the de-zoning.

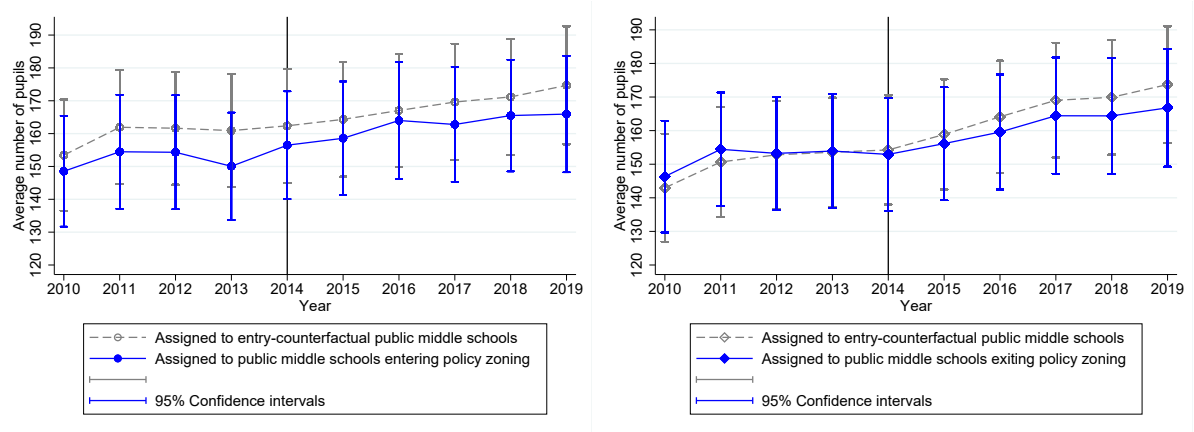
Table 6 also shows that stigma started to fall slightly pre-reform, as column (3) indicates parents switching to public schools outside the urban zoning in 2012 as well as in 2014, although the 2012 coefficient is less significant. The urban policy reform may then have exacerbated a pre-existing trend in untreated neighborhoods. As this potential pre-trend will likely blur the causal interpretation for de-zoned neighborhoods, Section 5 below provides some further robustness checks.

4.3 Sorting across schools or moving house?

We have not so far identified whether the policy effects reflect parental re-sorting across schools or across neighborhoods. As noted in Section 2, parents in France can bypass the legal map of catchment areas in three ways: moving house to be assigned to a "better" (or perceived as such) default public school, opting for a private school (in 22% of the cases in France, as shown in Table 1), or requesting a derogation to enroll their child outside their catchment area (24% of the cases in France, as shown in Table 1).

Most households in the neighborhoods targeted by the French urban policy live in social housing, and are thus far less mobile on average than households in better-off neighborhoods.[25] In addition, the eligibility criterion used for neighborhood selection could not have been predicted by parents, as there were no publicly-available local income data allowing them to calculate the poverty threshold. We therefore conjecture that parents were unlikely to plan a rapid house move either just before or after the reform's announcement, even though they might have been participated in citizens' councils involved in the reform process.

Figure 6 – Average number of pupils assigned to treated and counterfactual schools



Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

[25] According to Sala (2018), 74% of QP residents live in social housing, as against 16% in other neighborhoods in the same urban unit.

Figure 6 provides strong support for this conjecture. There are no significant differences over time in the average number of pupils assigned to public middle schools affected by the reform,[26] either for those entering the urban zoning (on the left-hand side) or those leaving it (on the right-hand side), and their control schools. As we find no evidence of "Tiebout flight" post-reform, we conclude that parents did not move house in response to the reform, and therefore re-sorted across schools rather than across neighborhoods, by asking for more (entry) or fewer (exit) opt-out derogations to not enroll their children in their catchment area.

As middle-school avoidance does not reflect moving or residential resorting, it is important to note that school stigma from neighborhood labeling was probably greatly reduced by the catchment-area rule, as derogations are granted only if the schools requested have sufficient capacity. If the number of requests exceeds the school's capacity, the exceptions are granted only in very specific cases, such as disabilities, low family-income, sibling reunion, unusual tracks, or medical care provided close to the requested school. Derogations for most other motives may therefore have been refused, especially given the school-capacity constraints that are likely in populated urban neighborhoods. This may well have considerably mitigated school stigmatization, with the 3.5 pp average drop in school enrollment therefore likely being a lower-bound estimate of the stigma incurred from neighborhood labeling.

5 Robustness checks

It is important to check whether the average treatment estimates above are robust to a number of checks. We here provide tests of the common-trend assumption that is required in our difference-in-differences setting. We then assess whether the findings are robust to (i) a change in the neighborhood-treatment definition, (ii) the inclusion of time-varying indicators of school quality, (iii) the way in which we identify school catchment areas, and (iv) the multinomial modeling of parental choice.

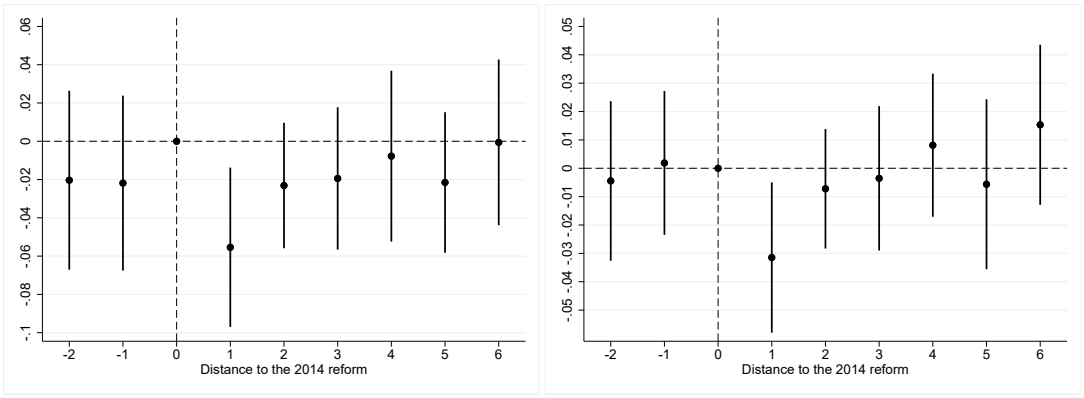
5.1 Pre-reform trend tests

Potential pre-trends are a key threat to causal interpretation. Despite school fixed effects, the parallel-trend assumption would be violated if schools in re-zoned areas had started to diverge pre-reform, as compared to the schools in counterfactual neighborhoods. To rule out wrongly attributing to the urban policy a factor that lies behind these pre-trends, which we cannot totally exclude for de-zoned areas (see Table 6), we add treatment-group specific linear trends to specifications (2) and (3). The results for urban-zoning entry in Figure 7 are not affected. This provides further support for our key result that public schools located in newly-zoned neighborhoods were stigmatized by policy designation, leading parents to switch to alternative public schools.

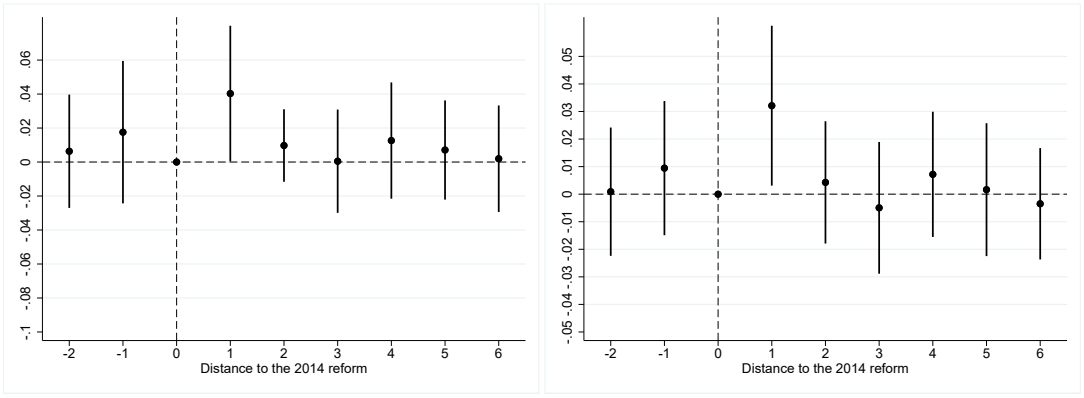
[26] In other words, the population within catchment areas is stable over time.

Figure 7 – Relative probabilities pre- and post-zoning - With (left) and without (right) treatment-group specific linear trends

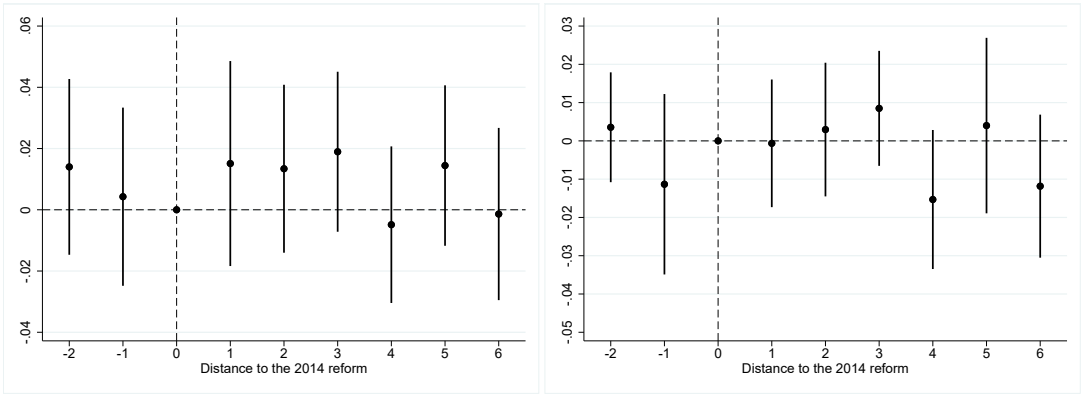
(a) Choosing the catchment-area school



(b) Choosing another public school



(c) Choosing a private school



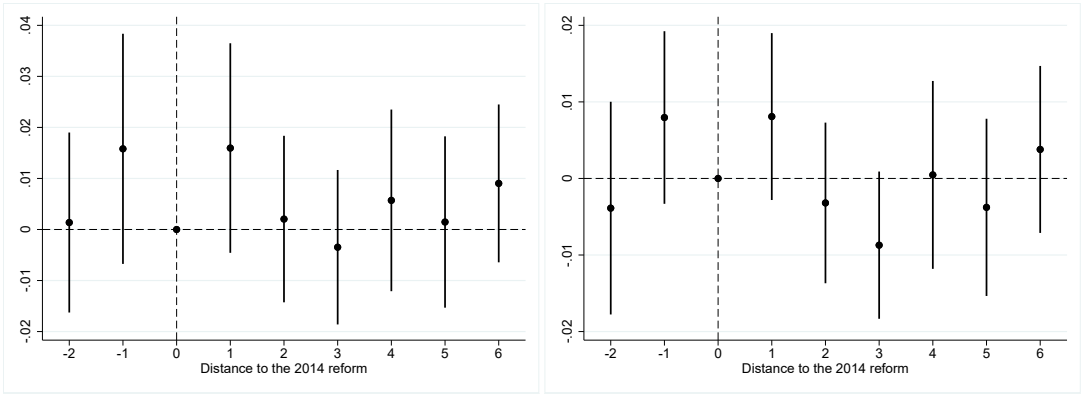
Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

Note: The X-axis represents the distance to the year of the reform: 0 corresponds to the 2013 school year, 1 to the 2014 school year, and so on. The Y-axis displays $\hat{\beta}_1$ drawn from estimating equation (2) with (left) and without (right) treatment-group specific linear trends.

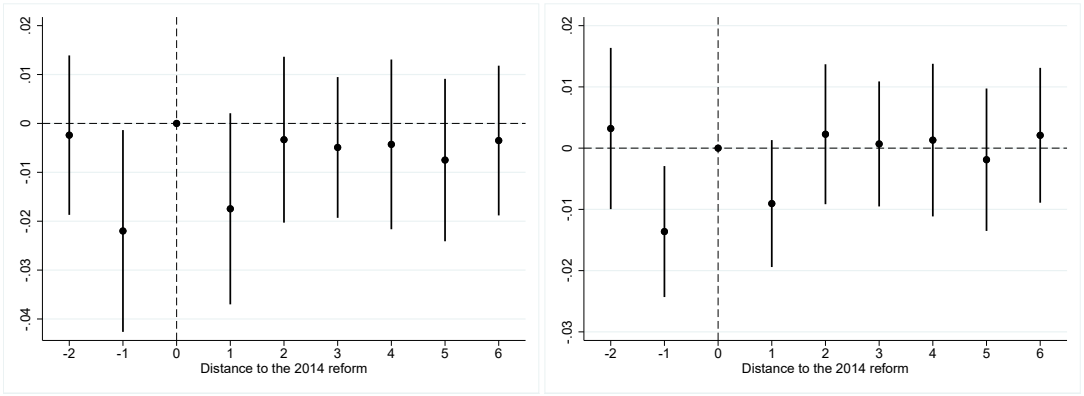
Figure 8 carries out the same exercise for de-zoned neighborhoods. The point estimates here are not or are only barely significant in 2012 with pre-reform linear trends. However, we cannot completely dismiss the existence of pre-trends that affect our conclusion of dis-labeling alone dissipating the stigma of previous zoning.

Figure 8 – Relative probabilities pre- and post- de-zoning - With (left) and without (right) treatment-group specific linear trends

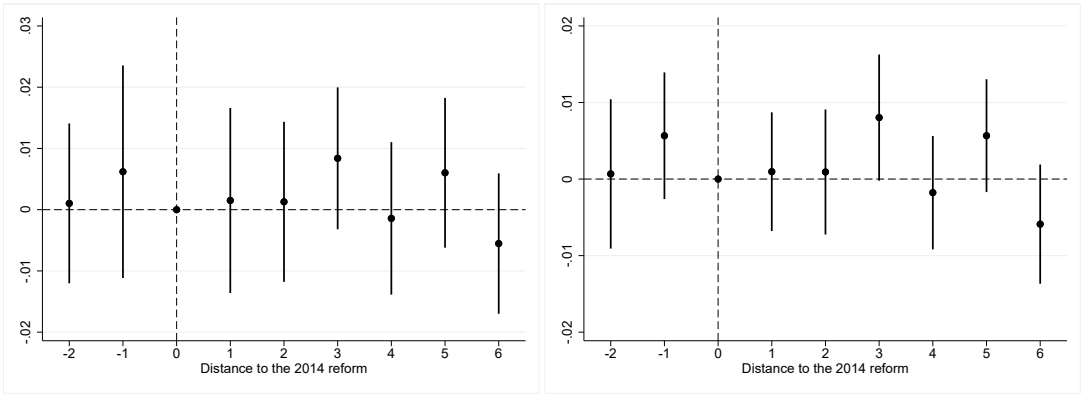
(a) Choosing the catchment-area school



(b) Choosing another public school



(c) Choosing a private school



Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

Note: The X-axis represents the distance to the year of the reform: 0 corresponds to the 2013 school year, 1 to the 2014 school year, and so on. The Y-axis displays $\hat{\beta}_2$ drawn from estimating equation (3) with (left) and without (right) treatment-group specific linear trends.

5.2 Post-reform placebo tests on 8th graders

We provide further support for our core result that neighborhood labeling had a causal impact on school choices via a placebo test inspired by Boutchenik et al. (2020). This test checks that parents who had already enrolled their children in lower secondary education at the time of the reform were unaffected by any re-zoning, as there is little reason to believe that parents would adjust their preferences once their children are enrolled in a 4-year program, as they are already informed about school quality.

Table 7 shows that 8th-grade enrollment was totally unaffected by the reform, with no parental re-sorting across middle schools over the course of lower secondary education. Our main average treatment effects are not then statistical artifacts but rather represent strategic parental behavior at the start of lower secondary education.

Table 7 – "Entry" into policy zoning and pupil enrollment in 8th grade

	Probability to enroll at:		
	Previous CA School	Other Public School	Private School
T^{entry}	0.009	-0.008	-0.000
	(0.010)	(0.009)	(0.004)
R^2	0.010	0.009	0.006
No. obs	303,977	303,977	303,977
No. clusters	237	237	237
T^{exit}	0.003	-0.004	0.001
	(0.003)	(0.003)	(0.001)
R^2	0.010	0.009	0.007
No. obs	687,380	687,380	687,380
No. clusters	619	619	619
Pupil's characteristics	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

5.3 Narrow definition of urban policy treatment

Prior to the 2014 reform, French urban policy combined a regulatory approach based on automatic support (ZRU/ZFU) with a contractual approach producing potential (but not automatic) credits (Other ZUS/CUCS). As such, some labeled neighborhoods may have received little or no additional public funding.

In addition, while many households were aware of the geographical perimeter of ZUS, ZRU and ZFU, as the related public subsidies could accrue directly to residents, CUCS boundaries were mostly known only by institutions and local authorities. As such, while most parents were generally well informed about whether the catchment-area school was part of a ZUS or a ZFU, many were unaware of the existence or perimeter of CUCS, so that the reform's effect on school perception or preferences could differ from those predicted under perfect information.

We thus check whether our results are robust to a narrower definition of urban-policy treatment, whereby only ZUS, ZRU and ZFU were treated before the reform. The resulting point estimates appear in Table 8.[27]

Table 8 – Rezoning and pupils' enrollment - Narrow treatment definition

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.021***	0.026***	-0.005
	(0.008)	(0.008)	(0.005)
R^2	0.170	0.126	0.192
No. obs	449,998	449,998	449,998
No. clusters	280	280	280
T^{exit}	0.014*	-0.017**	0.003
	(0.007)	(0.007)	(0.005)
R^2	0.178	0.102	0.218
No. obs	574,409	574,409	574,409
No. clusters	368	368	368
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends (exit)	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and coefficients on these controls are not listed.

There is no qualitative change, with the main difference being that the stigma from labeling is smaller with this narrow treatment definition. As shown in the top panel of Table 8, the probability that parents choose their catchment-area middle school is now 2.1 pp lower than in counterfactual neighborhoods (as against -3.5 pp in Table 3), and the probability of choosing another public school 2.6 pp higher (as against +4.1 pp in Table 3).

[27] Appendix D provides complementary yearly treatment effects (see Table D1 for entry and Table D2 for exit).

As with our previous treatment definition, re-sorting is immediate and mostly benefits public schools outside the policy scheme, which experience a +1.2 pp rise in enrollment compared to the control schools (see column 3 of Table D1 in Appendix D). All of these robustness tests thus confirm our key result that urban zoning stigmatized public middle schools in labeled neighborhoods, and exacerbated social segregation at school.

The point estimates for de-zoned neighborhoods are remarkably stable, both in terms of size and significance, to those with the broader definition of policy treatment. In the bottom panel of Table 8, de-zoning raised the probability that parents choose their catchment-area middle school by 1.4 pp compared to the control schools (as against +1.1 pp in Table 5), and reduced that of choosing another public school by 1.7 pp the year after the reform (as against 1 pp in Table 6). However, this stigma reversion becomes more diluted over time, as with the broader treatment definition (See Table D2 in Appendix D). Despite the treatment-group specific linear trends, there is still evidence that parallel trends might be violated (See Table D2 in Appendix D), and hence we should remain cautious in interpreting our stigma-reversion result as only reflecting de-zoning.

5.4 Controlling for changes in school quality

It might also be argued that catchment-area school fixed effects do not totally control for unobservables such as school quality, that may change over time. This section provides a robustness check that includes a time-varying control for school quality: the average share of DNB graduates at the end of the 9^{th} grade over the two years preceding the pupil's enrollment. As noted in Section 3.2, a word of caution is required here, as we do not observe the DNB success rates of all the middle schools in our sample, and in particular several schools affected by the 2014 reform. Selection issues may therefore play a role.[28]

Tables E1 and E2 in Appendix E show that our results are robust. Unsurprisingly, the probability of pupil enrollment at the catchment-area school rises with its past DNB performance, so that parents do react positively to indicators of good school quality in their catchment area: a greater past success rate reduces the likelihood that parents will opt for a public middle school other than the default catchment-area school, and in particular a private school. More importantly, we find very similar average treatment effects: the impact of policy designation on public-school choices remains very significant, with point estimates of similar size to those without time-varying school-quality control. Regarding de-zoning, the coefficients are also of similar size, although less significant due to the lower statistical power resulting from sample attrition.

[28]Furthermore, note that the inclusion of time-varying indicators of school quality, even in the form of lagged variables, likely raises dynamic endogeneity issues in fixed-effects panel estimation if the share of DNB graduates is correlated with families' unobserved preferences for schools. These estimations thereby only serve as robustness checks, and we exclude the share of DNB graduates from the covariates in the main specifications.

5.5 Public school assignment and catchment areas construction

Last, as we do not know the exact perimeter of catchment areas, we check whether our school assignment to pupils affects the results. For now, we allocated public middle schools via their shortest distance to each pupil's primary school. This could wrongly assign schools to some pupils if their catchment-area school is not necessarily the closest to the pupil's primary school, or if there are two equidistant public middle schools. If these errors are not random, our point estimates may be biased.

We tackle this issue by using geo-coded information from a separate dataset that allows us to recover catchment areas on the basis of the shortest distance between each pupil's home address and the set of all public middle schools.[29] Unfortunately, we cannot replicate all of our previous analyses, as these geo-coded datasets are available only every odd year from 2011 to 2017, and do not contain all of the individual covariates that appeared in the annual data. Nonetheless, as shown in Table 9, the results are qualitatively similar to those based on pupils' primary schools: zoning significantly reduces the probability that parents choose their catchment-area schools relative to counterfactual schools, with a stigma size slightly lower than that in the yearly data (-2.6 pp instead of -3.5), although the two coefficients are not significantly different from each other. We also find that parents shifted mostly to other public schools, but less so than in the yearly data (-2 pp instead of -4.1), instead of to private schools, (although the coefficient in column (3) is also positive at +0.6 pp). By way of contrast, de-zoning significantly increases the probability that parents enroll their children back at their catchment-area school (relative to counterfactual schools), with a stigma reversion even more significant and slightly larger than with the yearly data (+2.7 pp on average against +1.1 at best, just after the reform).

To further test whether assigning public schools to pupils on the basis of the distance to their primary school could present problems, we check whether 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 12 in the "exit" sample. Excluding these observations, which presumably have the wrong catchment area, has almost no effect on the results: the point estimates are identical (to two or three decimal places) and the significance levels are unaffected.[30]

5.6 Multinomial analysis

In the above, middle-school choices were modeled via separate linear-probability regressions of three dichotomous variables that were considered to be independent: choosing the catchment-area middle school, another public middle school, or a private school. As this may be restrictive, we now estimate a multinomial model in which parents choose one school out of the three alternatives.

[29] Maugis and Touahir (2018) show that this procedure yields a fairly good approximation of catchment areas.
[30] These results are available upon request.

Table 9 – Re-zoning and pupil enrollment - Geo-coded data

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.026***	0.020***	0.006
	(0.009)	(0.007)	(0.006)
R^2	0.110	0.083	0.135
No. obs	152,679	152,679	152,679
No. clusters	236	236	236
T^{exit}	0.027***	-0.019*	-0.008
	(0.010)	(0.012)	(0.006)
R^2	0.136	0.077	0.150
No. obs	391,673	391,673	391,673
No. clusters	607	607	607
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends (exit)	✓	✓	✓

Sources: Fichiers géoréférencés des élèves, 2011, 2013, 2015 and 2017, DEPP - Ministère de l'Éducation.

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, scholarship and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the share of private schools in the urban unit hosting the primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

Let U^l_{idt} denote utility that the family of pupil i, who is assigned to catchment-area school d at time t, derives from choosing school l. Our school choice model is then:

$$Y_{idt} = k \text{ if } U^k_{idt} > U^l_{idt}, \tag{4}$$

with

$$U^k_{idt} = \alpha^k + \beta^k T_{dt} + X_{it}\gamma^k + Z_{dt}\delta^k + \mu^k_d + \mu^k_t + \eta^k_{idt}, \tag{5}$$

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

Tables F1 and F2 in Appendix F list the multinomial point estimates, which are very similar to those from the linear-probability models. The probability that pupils be enrolled in a public school outside their catchment area is 32.2% higher in labeled neighborhoods than in entry-counterfactual neighborhoods post-reform (column 1 of Table F1). As the baseline probability of being enrolled at the catchment-area school is 54% for the counterfactual group (respectively 47% for the treated group), and the baseline probability to enroll at another public school 24% for the counterfactual group, this corresponds to a 3.6 pp increase in the probability to enroll at another public school,[31] which is comparable to the 4.1 point estimate in Table 3.

[31] Calculated as $0.47 \times 1.322 \times \frac{0.24}{0.54} = 0.276$, i.e 3.6 pp higher than 24%.

6 Heterogeneous Treatment Effects

This section raises the question of whether the reform have had differential effect on school choices by a number of characteristics: family background, pupil gender, and catchment-area school characteristics (such as compensatory education programs, or the vicinity of private schools that likely compete with public schools).

6.1 Heterogeneity by Socioeconomic Status

We first ask whether the responses to re-zoning are different in families from different socioeconomic backgrounds, as High-SES parents or parents in particular occupations (such as teachers) may have better access to information about school quality. We first re-code parental occupation into three broad categories: High, Medium and Low SES,[32] and estimate the most-conservative augmented versions of Equations (2) and (3) in a triple-difference approach, where all of our explanatory variables are interacted with occupation.

The top panel of Table 10 shows that zoning entry caused a "rich flight" to the private sector, as High-SES parents (the reference category in Table 10) increased their probability of private-school choice by 3.6 pp relative to High-SES parents living in counterfactual unlabeled neighborhoods, post-reform. While the second row of this panel shows that there is no difference between Medium- and High-SES parents (with all of the estimated coefficients being insignificant), in the third row Low- and High-SES parents do behave differently. In the top panel of Table 10, the probability of shifting to another public (private) school after entry into zoning is 4.8 pp (3.5 pp) larger (lower) for Low- than for High-SES parents. In the bottom panel, the probability to choose the catchment-area school following de-zoning is 2.1 pp higher for Low- than for High-SES parents, in line with the former re-adjusting their school-quality beliefs post-reform more than the latter.

Table 11, which isolates the reform's impact on teachers and professors, reveals that they reacted significantly less to policy designation than all of the other types of parents (see the top panel of Table 11). These two occupations were plausibly much more aware of the intrinsic quality of schools pre-reform, and were thus less reactive to any new (bad or good) signals conveyed by neighborhood labeling.

6.2 Heterogeneity in other dimensions

We also investigated heterogeneity across a number of other dimensions. First, pupil citizenship, as foreign residents may have more difficulty in understanding French and insufficient knowledge of the French institutional requirements to bypass their catchment-area school, or adapt quickly to the new information produced by the reform. Table G1 in Appendix G provides no clear indication of such heterogeneity for zoning entry, which has the same enrollment effect for French and foreign pupils. Zoning exit seems to have reduced the likelihood

[32] We aggregate Very High and High SES, as well as Low and Unknown SES, as Section 4 revealed similar school-choice patterns and point estimates in these combined categories.

Table 10 – Re-zoning and pupil enrollment by SES

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.036*	-0.000	0.036**
	(0.019)	(0.019)	(0.016)
SES (ref.=High)			
Medium SES $\times$ T^{entry}	-0.002	0.029	-0.027
	(0.016)	(0.020)	(0.018)
Low SES $\times$ T^{entry}	-0.013	0.048**	-0.035*
	(0.021)	(0.019)	(0.021)
R^2	0.180	0.136	0.207
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
T^{exit}	-0.003	-0.001	0.004
	(0.010)	(0.010)	(0.008)
SES (ref.=High)			
Medium SES $\times$ T^{exit}	0.010	-0.014	0.004
	(0.009)	(0.009)	(0.009)
Low SES $\times$ T^{exit}	0.021**	-0.010	-0.011
	(0.009)	(0.010)	(0.008)
R^2	0.186	0.133	0.237
No. obs	954,666	954,666	954,666
No. clusters	616	616	616
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

that parents of foreign pupils opt for a private school, whereas those of French pupils did adjust their public-school opt-out strategies. Tables G1, G2, G3 and G4 in Appendix G reveal no robust evidence of heterogeneity by gender (Table G2), by catchment-area school type i.e. with or without additional compensatory education resources (Table G3), or by distance to the catchment-area school (Table G4) regarding urban-zoning entry.[33]

[33]We also tested for heterogeneity by DNB gap between the catchment-area and the actual schools of enrollment, and did not find any significant differences either. These results are available upon request.

Table 11 – Re-zoning and the enrollment of teachers' children

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.041***	0.036**	0.005
	(0.013)	(0.016)	(0.011)
SES (ref.=Non-Teachers)			
Teachers $\times$ T^{entry}	0.059**	-0.061**	0.001
	(0.025)	(0.027)	(0.018)
R^2	0.153	0.123	0.155
No. obs	384,476	384,476	384,476
No. clusters	235	235	235
T^{exit}	0.011*	-0.010*	-0.001
	(0.006)	(0.006)	(0.004)
SES (ref.=Non-Teachers)			
Teachers $\times$ T^{exit}	-0.018	0.021	-0.003
	(0.014)	(0.017)	(0.014)
R^2	0.144	0.116	0.168
No. obs	954,660	954,660	954,660
No. clusters	616	616	616
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

Heterogeneity is more salient for urban-zoning exit, but it might be interpreted with caution, as we could not completely dismiss the existence of pre-trends in this case. Parents with a French pupil citizenship seem more affected by school bad reputations, as they favour relatively more private schools after de-zoning. Girls seem more likely to be enrolled back in their catchment after de-zoning than boys. And public middle schools more distant from their closest private school seem to experience larger enrollment rise post-reform than other public middle schools.

7 Conclusion

Even though place-based policies funnel large transfers toward low-income neighborhoods, the extent to which they provide disadvantaged residents with more opportunities is still a matter of debate. Urban-policy resources may improve school enrollment in low-income

neighborhoods if parents expect benefits on their children's education, but can also affect schools' reputations via a negative image from policy-designation. This paper estimates the net effect of these two opposing forces on middle-school choices in France over the 2010-2019 period.

The main challenge in evaluating place-based policies is selection into treatment, as neighborhoods qualify for public subsidies due to their deprivation. The naive comparison of labeled and unlabeled neighborhoods is thus likely to underestimate policy effectiveness. We overcome this challenge by appealing to the quasi-natural experiment provided by a 2014 policy reform that redrew the boundary of urban neighborhoods that were eligible for place-based subsidies, on the basis of a non-manipulable local poverty cut-off. We exploit this discontinuity design in a spatial difference-in-differences framework with school fixed effects to evaluate the causal impact of French urban policy on school choices. We focus in particular on school enrollment into lower secondary education, which is a key decision for French parents that affects pupils' education paths over a 4-year time span.

We show that public middle schools in neighborhoods that became zoned and benefited from placed-based subsidies witnessed a significant reduction in school enrollment after the reform. This "zone-and-shame" effect is triggered by responses from parents of all socioeconomic backgrounds, who switched to public schools outside the policy coverage. We also find evidence of a "rich flight" to the private sector, driven by the sorting of parents across schools but not across neighborhoods. The turn away from the public schools in zoned neighborhoods is large and immediate, but the school stigma dissipates after two years. Symmetrically, public schools in de-zoned neighborhoods saw enrollment rise, but only from disadvantaged families and only in the short-run. In our preferred specification, neighborhood labeling reduced the probability that a child be enrolled at a public school in a labeled neighborhood by 4 pp. This penalty, which is equivalent to 6 fewer pupils per school in zoned neighborhoods, is not offset by the 1.1 pp rise following de-zoning (corresponding to fewer than 2 pupils per school). Note, however, that, as many more schools "exited" than "entered" the urban policy scheme post-reform, the total number of pupils leaving zoned schools is about 10 times smaller than the number of pupils coming back to de-zoned neighborhoods. Our main estimates are robust to a number of different specifications, various placebo tests, an alternative treatment definition, and many other checks.

Our analysis suggests that place-based policies that rely on zoning may stigmatize the public schools in targeted neighborhoods and further accentuate social segregation via family sorting. Even though the policy stigma disappears quickly after the new information provided by neighborhood labeling, school-composition effects are expected to persist, as most pupils remain in the same middle school throughout lower secondary education in France. This composition effect may also be hard to undo, as only Low-SES families seem to re-adjust their school-quality beliefs following de-zoning. Moreover, our results reflect only the lower bound of area stigmatization from neighborhood labeling, which would have been larger without the school sectorization that dampened the rise in social segregation.

These results refer to the French context, but area stigma may well apply in other countries

where place-based policies rely on zoning. It remains to be seen to what extent the benefits from changes in school composition and resources offset this penalty later on, as the segregation triggered by the policy may produce more-homogeneous groups of peers at school, with more teaching resources at their disposal. We unfortunately cannot provide this cost-benefit analysis, as the confidential data provided so far do not allow us to follow pupils (and their test scores) over time, and hence to see whether the extra resources from urban policy helped them to perform better in their post-reform education. We hope to overcome this limitation in future work if this data becomes available for research purposes.

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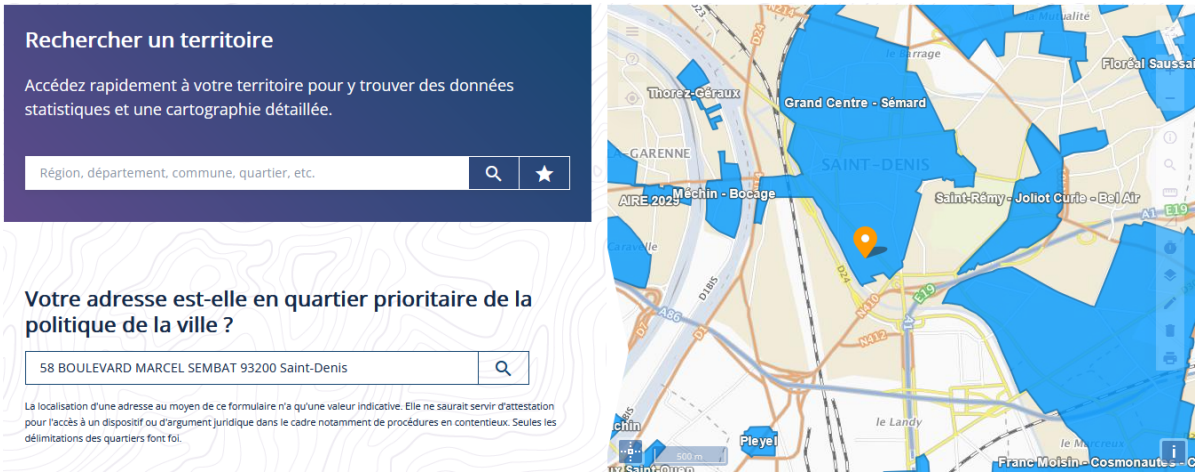
A Internet information on urban-policy coverage

Figure A1 – Internet information on the urban policy coverage

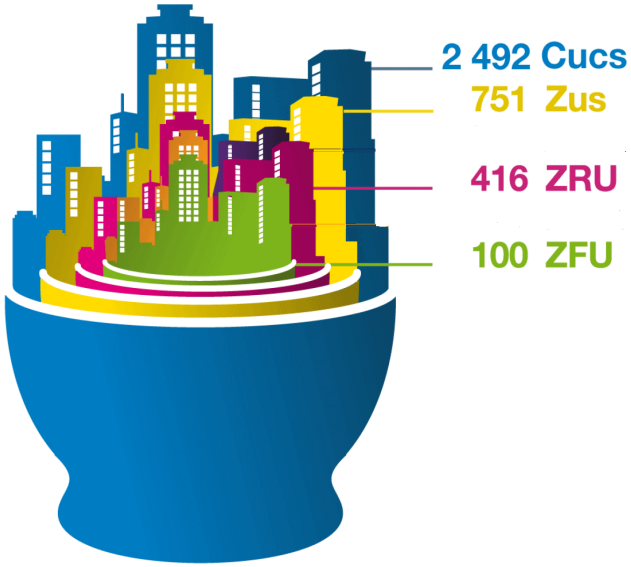


Source: French Ministry of Urban Affairs (https://sig.ville.gouv.fr/).

Note: The address refers to a middle school located in one of the QP's (shown in blue) in the Seine-Saint-Denis region.

B French urban-zoning systems before the 2014 reform

Figure B1 – French urban-zoning systems before 2014



Source: CGET

Note:

ZUS = *Zones urbaines sensibles* = Urban Sensitive Zones

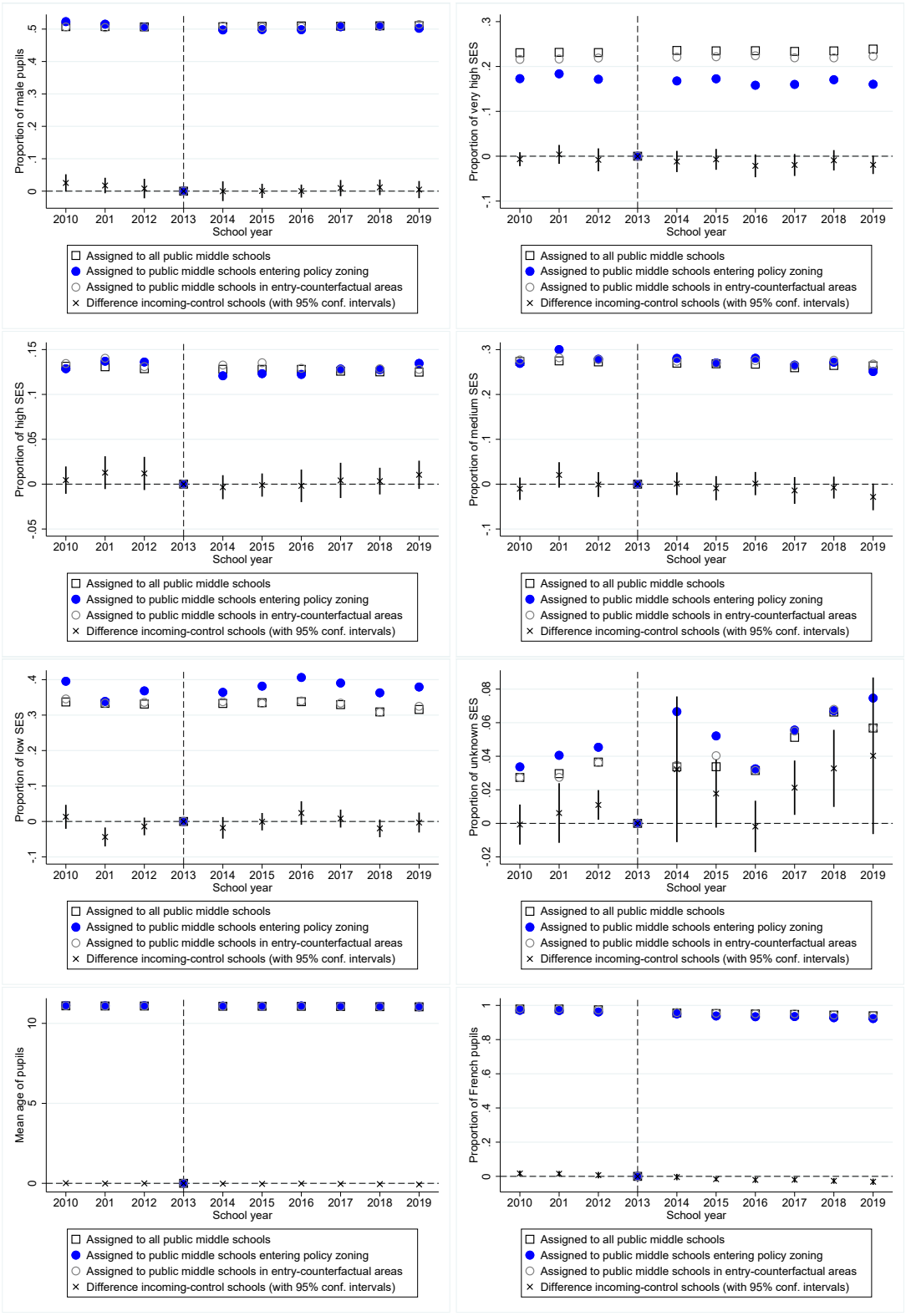
ZRU = *Zones de revitalisation urbaine* = Urban Regeneration Zones

ZFU = *Zones franches urbaines* = Enterprize Zones

CUCS = *Contrats urbains de cohésion sociale* = Urban Social Cohesion Contracts

C Balancing tests between control and treated schools

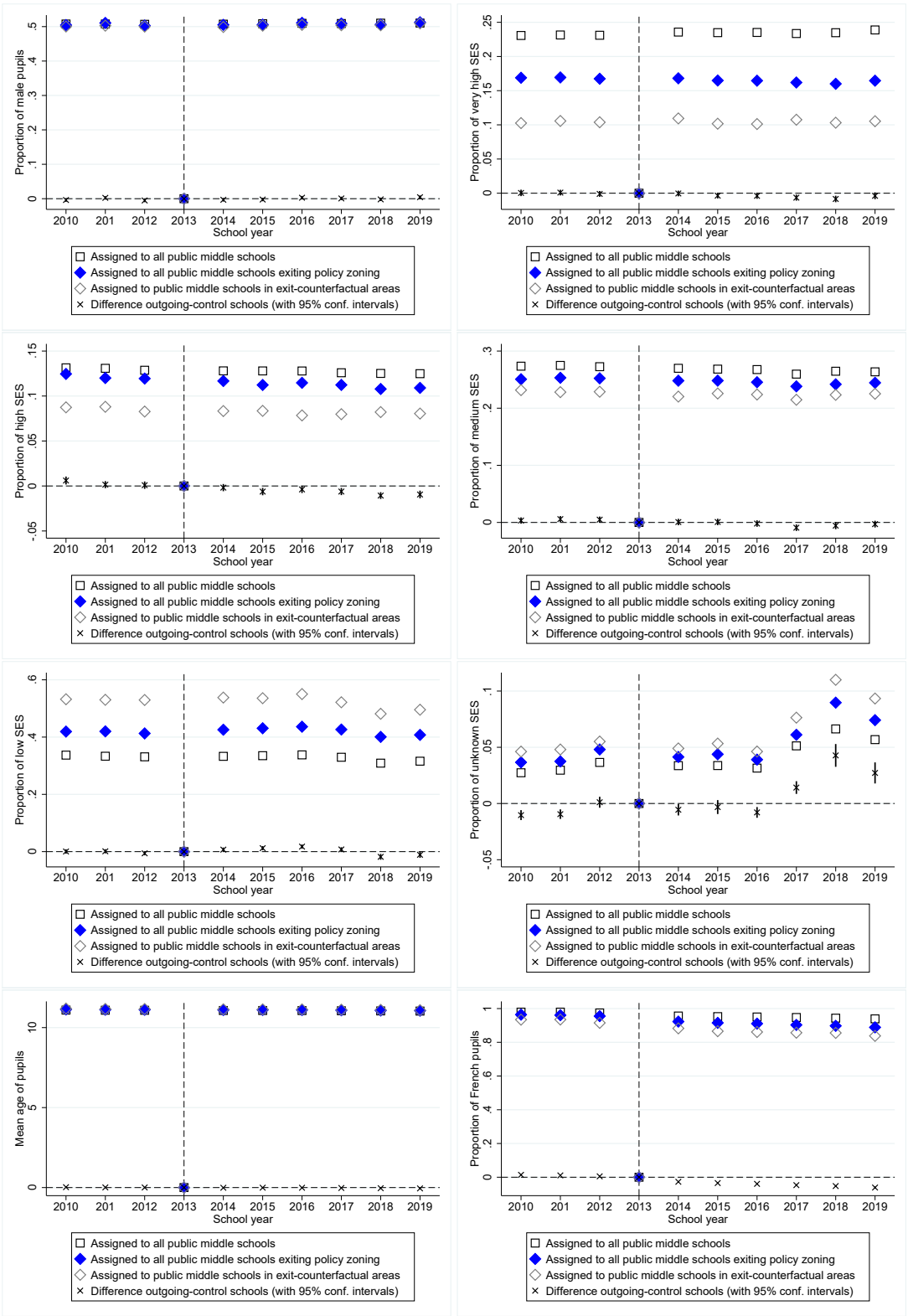
Figure C1 – Comparison of pupils assigned to incoming vs. entry-counterfactual schools



Note: Balancing tests for pupils entering 6^{th} grade for the first time.

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP.

Figure C2 – Comparison of pupils assigned to outgoing vs. exit-counterfactual schools



Note: Balancing tests for pupils entering 6^{th} grade for the first time.

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP.

D Narrow definition of neighborhood treatment

Table D1 – "Entry" into policy zoning and pupil enrollment - Narrow definition of neighborhood treatment

	Probability to enroll at:			
	CA School	Other Public School		Private School
		In zoning	Out zoning	
$T^{entry-2011}$	-0.002	0.002	-0.001	0.001
	(0.010)	(0.006)	(0.007)	(0.007)
$T^{entry-2012}$	-0.004	0.002	0.007	-0.005
	(0.009)	(0.007)	(0.007)	(0.007)
$T^{entry-2013}$	0.005	0.007	-0.006	-0.007
	(0.009)	(0.006)	(0.005)	(0.007)
$T^{entry-2014}$	-0.025***	0.007	0.012**	0.006
	(0.009)	(0.006)	(0.006)	(0.006)
$T^{entry-2015}$	0.001	-0.004	-0.001	0.004
	(0.008)	(0.006)	(0.006)	(0.006)
$T^{entry-2016}$	-0.003	0.005	0.003	-0.004
	(0.008)	(0.006)	(0.006)	(0.006)
$T^{entry-2017}$	0.008	-0.004	-0.003	-0.001
	(0.008)	(0.006)	(0.007)	(0.006)
$T^{entry-2018}$	0.001	0.011*	-0.002	-0.010
	(0.010)	(0.006)	(0.009)	(0.006)
$T^{entry-2019}$	-0.004	-0.002	-0.001	0.006
	(0.008)	(0.006)	(0.006)	(0.006)
R^2	0.170	0.188	0.132	0.192
No. obs	449,998	449,998	449,998	449,998
No. clusters	280	280	280	280
Pupil's characteristics	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

Table D2 – "Exit" from policy zoning and pupil enrollment - Narrow definition of neighborhood treatment

	Probability to enroll at:			
	CA School	Other Public School		Private School
		In zoning	Out zoning	
$T^{exit-2011}$	-0.006	0.012*	-0.007	0.001
	(0.008)	(0.006)	(0.006)	(0.006)
$T^{exit-2012}$	0.015*	-0.013**	-0.007	0.005
	(0.008)	(0.006)	(0.005)	(0.006)
$T^{exit-2013}$	-0.007	0.004	0.002	0.002
	(0.007)	(0.006)	(0.005)	(0.006)
$T^{exit-2014}$	0.013*	-0.005	-0.011**	0.004
	(0.007)	(0.007)	(0.004)	(0.005)
$T^{exit-2015}$	-0.001	0.000	0.003	-0.003
	(0.006)	(0.006)	(0.004)	(0.005)
$T^{exit-2016}$	-0.013**	0.003	0.003	0.006
	(0.006)	(0.005)	(0.005)	(0.005)
$T^{exit-2017}$	0.008	0.001	-0.006	-0.003
	(0.008)	(0.005)	(0.006)	(0.004)
$T^{exit-2018}$	-0.010	0.007	-0.004	0.008*
	(0.007)	(0.006)	(0.005)	(0.004)
$T^{exit-2019}$	0.005	0.000	0.005	-0.010**
	(0.007)	(0.005)	(0.004)	(0.005)
R^2	0.178	0.160	0.145	0.218
No. obs	574,409	574,409	574,409	574,409
No. clusters	368	368	368	368
Pupil's characteristics	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School FE	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

E Results with DNB past success rate as a control

Table E1 – "Entry" into policy zoning and pupil enrollment - Controlling for changes in school quality over time

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.033**	0.040**	-0.007
	(0.015)	(0.016)	(0.008)
SES (ref.=Medium)			
Very High SES	-0.069***	-0.017***	0.086***
	(0.007)	(0.006)	(0.007)
High SES	-0.016***	-0.009**	0.025***
	(0.006)	(0.004)	(0.006)
Low SES	0.096***	0.023***	-0.120***
	(0.006)	(0.005)	(0.006)
Unknown SES	0.082***	0.041***	-0.123***
	(0.017)	(0.013)	(0.010)
Male	-0.011***	0.006***	0.005**
	(0.002)	(0.002)	(0.002)
French	-0.068***	-0.014	0.083***
	(0.010)	(0.010)	(0.009)
Age	0.011***	0.028***	-0.039***
	(0.004)	(0.003)	(0.004)
CA School in comp. educ. prog.	0.009	-0.005	-0.004
	(0.012)	(0.011)	(0.012)
No. Private Schools within 5km	0.061***	-0.075***	0.014**
	(0.013)	(0.013)	(0.007)
Past DNB Rate in CA School	0.094**	-0.044	-0.051
	(0.037)	(0.034)	(0.031)
R^2	0.166	0.123	0.187
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Aide au Pilotage et à l'Auto-évaluation des Établissements (APAE); Local income data (Insee).

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. For the sake of clarity, we do not list the constant.

Table E2 – "Exit" from policy zoning and pupil enrollment - Controlling for changes in school quality over time

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{exit}	0.010	-0.010*	0.000
	(0.006)	(0.006)	(0.004)
SES (ref.=Medium)			
Very High SES	-0.115***	-0.010*	0.124***
	(0.006)	(0.005)	(0.006)
High SES	-0.036***	0.002	0.034***
	(0.004)	(0.004)	(0.004)
Low SES	0.122***	0.003	-0.125***
	(0.005)	(0.004)	(0.005)
Unknown SES	0.096***	0.032***	-0.129***
	(0.010)	(0.009)	(0.007)
Male	-0.010***	0.007***	0.002*
	(0.001)	(0.001)	(0.001)
French	-0.069***	0.007	0.061***
	(0.006)	(0.006)	(0.004)
Age	0.027***	0.026***	-0.053***
	(0.002)	(0.002)	(0.002)
CA School in comp. educ. prog.	0.009	-0.008	-0.001
	(0.006)	(0.007)	(0.005)
No. Private Schools within 5km	0.030***	-0.027***	-0.002
	(0.006)	(0.005)	(0.004)
Past DNB Rate in CA school	0.055***	-0.015	-0.040***
	(0.020)	(0.021)	(0.011)
R^2	0.168	0.114	0.211
No. obs	951,126	951,126	951,126
No. clusters	610	610	610
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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. For the sake of clarity, we do not list the constant.

F Multinomial logit estimation results

Table F1 – Multinomial logit - "Entry" into policy zoning and pupil enrollment

	Relative risk ratios	
	Middle school choice (ref = CA School)	
	Other Public School	Private School
T^{entry}	1.322***	1.053
	(0.139)	(0.061)
SES (ref.=Medium)		
Very High SES	1.114***	1.664***
	(0.045)	(0.066)
High SES	0.996	1.155***
	(0.027)	(0.040)
Low SES	0.907***	0.370***
	(0.027)	(0.018)
Unknown SES	0.990	0.377***
	(0.086)	(0.039)
Male	1.052***	1.054***
	(0.011)	(0.016)
French	1.120**	2.361***
	(0.059)	(0.163)
Age	1.100***	0.770***
	(0.022)	(0.022)
CA School in comp. educ. prog.	0.965	0.927
	(0.069)	(0.079)
No. Private Schools within 5km	0.594***	0.919
	(0.048)	(0.050)
Pseudo R^2	0.165	
No. obs	384,478	
No. clusters	235	
Year FE	✓	
School FE	✓	

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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. For the sake of clarity, we do not list the constant.

Table F2 – Multinomial logit - "Exit" from policy zoning and pupil enrollment

	Relative risk ratios	
	Middle school choice (ref = CA School)	
	Other Public School	Private School
T^{exit}	0.960	1.056*
	(0.028)	(0.031)
SES (ref.=Medium)		
Very High SES	1.386***	2.182***
	(0.045)	(0.083)
High SES	1.112***	1.270***
	(0.024)	(0.034)
Low SES	0.755***	0.300***
	(0.017)	(0.009)
Unknown SES	0.873***	0.310***
	(0.040)	(0.022)
Male	1.052***	1.038***
	(0.006)	(0.011)
French	1.192***	2.441***
	(0.036)	(0.103)
Age	1.013	0.617***
	(0.012)	(0.012)
CA School in comp. educ. prog.	0.942	0.966
	(0.035)	(0.034)
No. Private Schools within 5k	0.835***	0.899***
	(0.029)	(0.031)
Pseudo R^2	0.168	
No. obs	954,666	
No. clusters	616	
Year FE	✓	
School FE	✓	

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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. For the sake of clarity, we do not list the constant.

G Other heterogeneity dimensions

Table G1 – Re-zoning and pupil enrollment by citizenship

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.016	0.033	-0.016
	(0.030)	(0.032)	(0.030)
SES (ref.=Foreign)			
French $\times$ T^{entry}	-0.028	-0.000	0.028
	(0.026)	(0.029)	(0.031)
R^2	0.179	0.136	0.207
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
T^{exit}	0.007	0.014	-0.021***
	(0.010)	(0.010)	(0.006)
SES (ref.=Foreign)			
French $\times$ T^{exit}	0.005	-0.027***	0.023***
	(0.009)	(0.009)	(0.005)
R^2	0.186	0.133	0.236
No. obs	954,666	954,666	954,666
No. clusters	616	616	616
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

Table G2 – Re-zoning and pupil enrollment by gender

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.042***	0.027*	0.015
	(0.014)	(0.016)	(0.010)
Gender (ref.=Girl)			
Boy $\times$ T^{entry}	-0.001	0.010	-0.009
	(0.006)	(0.007)	(0.005)
R^2	0.179	0.136	0.207
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
T^{exit}	0.015**	-0.015**	-0.001
	(0.006)	(0.006)	(0.004)
Gender (ref.=Girl)			
Boy $\times$ T^{exit}	-0.008***	0.007***	0.000
	(0.002)	(0.002)	(0.002)
R^2	0.186	0.133	0.236
No. obs	954,666	954,666	954,666
No. clusters	616	616	616
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

Table G3 – Re-zoning and pupil enrollment by school type

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.027**	0.017	0.010
	(0.012)	(0.015)	(0.013)
CA School extra-funds $\times$ T^{entry}	-0.049	0.051	-0.002
	(0.036)	(0.040)	(0.015)
R^2	0.179	0.136	0.207
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
T^{exit}	0.005	-0.009	0.004
	(0.007)	(0.007)	(0.005)
CA school extra-funds $\times$ T^{exit}	0.011**	-0.004	-0.007**
	(0.006)	(0.005)	(0.004)
R^2	0.186	0.133	0.236
No. obs	954,666	954,666	954,666
No. clusters	616	616	616
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefits from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

Table G4 – Re-zoning and pupil enrollment by distance to the closest private school

	Probability to enroll at:		
	CA School	Other Public School	Private School
T^{entry}	-0.065**	0.025	0.040***
	(0.026)	(0.022)	(0.015)
Below median distance $\times$ T^{entry}	0.042	0.006	-0.048
	(0.048)	(0.046)	(0.032)
R^2	0.181	0.140	0.211
No. obs	384,478	384,478	384,478
No. clusters	235	235	235
T^{exit}	0.029***	-0.030***	0.001
	(0.010)	(0.010)	(0.006)
Below median distance $\times$ T^{exit}	-0.025**	0.027***	-0.002
	(0.011)	(0.009)	(0.006)
R^2	0.192	0.142	0.237
No. obs	954,666	954,666	954,666
No. clusters	616	616	616
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET); Local income data (Insee).

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, age and citizenship. Time-varying controls include a dummy for the CA school benefiting from a compensatory education program, and the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and the coefficients on these controls are not listed.

2019

2019/1, Mediavilla, M.; Mancebón, M. J.; Gómez-Sancho, J. M.; Pires Jiménez, L.: "Bilingual education and school choice: a case study of public secondary schools in the Spanish region of Madrid"
2019/2, Brutti, Z.; Montolio, D.: "Preventing criminal minds: early education access and adult offending behavior"
2019/3, Montalvo, J. G.; Piolatto, A.; Raya, J.: "Transaction-tax evasion in the housing market"
2019/4, Durán-Cabré, J.M.; Esteller-Moré, A.; Mas-Montserrat, M.: "Behavioural responses to the re)introduction of wealth taxes. Evidence from Spain"
2019/5, Garcia-López, M.A.; Jofre-Monseny, J.; Martínez Mazza, R.; Segú, M.: "Do short-term rental platforms affect housing markets? Evidence from Airbnb in Barcelona"
2019/6, Domínguez, M.; Montolio, D.: "Bolstering community ties as a means of reducing crime"
2019/7, García-Quevedo, J.; Massa-Camps, X.: "Why firms invest (or not) in energy efficiency? A review of the econometric evidence"
2019/8, Gómez-Fernández, N.; Mediavilla, M.: "What are the factors that influence the use of ICT in the classroom by teachers? Evidence from a census survey in Madrid"
2019/9, Arribas-Bel, D.; Garcia-López, M.A.; Viladecans-Marsal, E.: "The long-run redistributive power of the net wealth tax"
2019/10, Arribas-Bel, D.; Garcia-López, M.A.; Viladecans-Marsal, E.: "Building(s and) cities: delineating urban areas with a machine learning algorithm"
2019/11, Bordignon, M.; Gamalerio, M.; Slerca, E.; Turati, G.: "Stop invasion! The electoral tipping point in anti-immigrant voting"

2020

2020/01, Daniele, G.; Piolatto, A.; Sas, W.: "Does the winner take it all? Redistributive policies and political extremism"
2020/02, Sanz, C.; Solé-Ollé, A.; Sorribas-Navarro, P.: "Betrayed by the elites: how corruption amplifies the political effects of recessions"
2020/03, Farré, L.; Jofre-Monseny; J., Torrecillas, J.: "Commuting time and the gender gap in labor market participation"
2020/04, Romarri, A.: "Does the internet change attitudes towards immigrants? Evidence from Spain"
2020/05, Magontier, P.: "Does media coverage affect governments'preparation for natural disasters?"
2020/06, McDougal, T.L.; Montolio, D.; Brauer, J.: "Modeling the U.S. firearms market: the effects of civilian stocks, crime, legislation, and armed conflict"
2020/07, Veneri, P.; Comandon, A.; Garcia-López, M.A.; Daams, M.N.: "What do divided cities have in common? An international comparison of income segregation"
2020/08, Piolatto, A.: "'Information doesn't want to be free': informational shocks with anonymous online platforms"
2020/09, Marie, O.; Vall Castello, J.: "If sick-leave becomes more costly, will I go back to work? Could it be too soon?"
2020/10, Montolio, D.; Oliveira, C.: "Law incentives for juvenile recruiting by drug trafficking gangs: empirical evidence from Rio de Janeiro"
2020/11, Garcia-López, M.A.; Pasidis, I.; Viladecans-Marsal, E.: "Congestion in highways when tolls and railroads matter: evidence from European cities"
2020/12, Ferraresi, M.; Mazzanti, M.; Mazzarano, M.; Rizzo, L.; Secomandi, R.: "Political cycles and yardstick competition in the recycling of waste. evidence from Italian provinces"
2020/13, Beigelman, M.; Vall Castelló, J.: "COVID-19 and help-seeking behavior for intimate partner violence victims"
2020/14, Martínez-Mazza, R.: "Mom, Dad: I'm staying" initial labor market conditions, housing markets, and welfare"
2020/15, Agrawal, D.; Foremny, D.; Martínez-Toledano, C.: "*Paraísos fiscales*, wealth taxation, and mobility"
2020/16, Garcia-Pérez, J.I.; Serrano-Alarcón, M.; Vall Castelló, J.: "Long-term unemployment subsidies and middle-age disadvantaged workers' health"

2021

2021/01, Rusteholz, G.; Mediavilla, M.; Pires, L.: "Impact of bullying on academic performance. A case study for the community of Madrid"
2021/02, Amuedo-Dorantes, C.; Rivera-Garrido, N.; Vall Castelló, J.: "Reforming the provision of cross-border medical care evidence from Spain"

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2021/06, Asensio, J.; Matas, A.: "The impact of 'competition for the market'regulatory designs on intercity bus prices"
2021/07, Boffa, F.; Cavalcanti, F.; Piolatto, A.: "Ignorance is bliss: voter education and alignment in distributive politics"

2022

2022/01, Montolio, D.; Piolatto, A.; Salvadori, L.: "Financing public education when altruistic agents have retirement concerns"
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2022/07, Foremny, D.; Sorribas-Navarro, P.; Vall Castelló, J.: "Income insecurity and mental health in pandemic times"
2022/08, Garcia-López, M.A.; Viladecans-Marsal, E.: "The role of historic amenities in shaping cities"
2022/09, Cheshire, P. C., Hilber, C. A. L., Montebruno, P., Sanchis-Guarner R.: "(IN)convenient stores? What do policies pushing stores to town centres actually do?"
2022/10, Sanchis-Guarner R.: "Decomposing the impact of immigration on house prices"

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d'Economia
de Barcelona

UNIVERSITAT DE
BARCELONA

ieb@ub.edu
www.ieb.edu

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