Place-Based Policies: Opportunity for Deprived Schools or Zone-and-Shame Effect?*

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Abstract

Even though place-based policies channel large transfers toward low-income neighborhoods, the degree to which they provide disadvantaged residents with more opportunities is still a matter of debate, as they can also convey territorial stigmatization. This paper leverages the quasi-experimental discontinuity design provided by a French reform that redrew the map of urban neighborhoods eligible for public subsidies on the basis of a sharp poverty cut-off, to assess the effect of place-based policies on school enrollment into lower secondary education. Results show that district schools located in neighborhoods below the poverty cut-off, that were qualified to public subsidies, experienced a significant drop in pupils' attendance relative to public schools located in counterfactual neighborhoods just above the policy cut-off. This "zone-and-shame" effect is triggered by behavioral reactions of parents from all socioeconomic backgrounds, who have shifted to public schools located outside the policy zoning. Besides, we find partial evidence of a "rich flight" to private schools driven by neighborhood labeling. Conversely, public schools located in neighborhoods disqualified from the program regained pupils' attendance, but only temporarily and only from disadvantaged families.

JEL codes: I24, I28, R23, R58.
Keywords: School choices, Territorial stigmatization, Redlining, Urban segregation, Spatial sorting.

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1 Introduction

To mitigate socioeconomic disparities across neighborhoods, which are particularly marked in dense metropolitan areas, policy makers worldwide have been implementing place-based urban policies for over forty years (Neumark and Simpson, 2015). In Mainland France, these policies currently targets 1,300 "priority" neighborhoods or equivalently 8% of its population, i.e. 4.9 million people. The French enterprise zone program launched in 1996, and still in force today, covers approximately one third of these neighborhoods, and it proposes tax and payroll exemptions conditional to plant settlement in those neighborhoods. It is undoubtedly the most emblematic and long-lasting place-based urban policy in France, with an outstanding financial effort compared to similar experiences abroad.[1] However, French urban policies do not only aim at attracting businesses and creating new employment prospects into distressed neighborhoods, they also provide them with larger supports for education, culture, social, transport and health policies. Extra-resources to improve educational achievements in low-income urban neighborhoods amount to approximately €100 Mn per year, but they are more largely widespread across "priority" neighborhoods than business tax rebates.

Even though place-based policies channel large transfers toward low-income urban neighborhoods, there is a growing recognition that their effectiveness is challenged by unintentional adverse effects that can be detrimental to their residents. This paper focuses on one collateral damage commonly referred to as "territorial stigmatization" by sociologists (Wacquant et al., 2014), but that remains 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, that we study through the lens of school choices. Indeed, urban policy resources targeted at low-income districts can improve school attendance if parents expect positive feedback loops on their children's education. But they can as well stigmatize schools and alter their reputation if policy-designation conveys a negative image on the targeted neighborhoods. The effect of place-based urban policies on school choices is thereby not entirely straightforward. Whether the benefits of the policy will overcome the territorial stigma is ultimately an empirical question, that this paper aims to address, with potentially large implications regarding social segregation at school and children's educational achievements.

There is considerable empirical evidence that neighborhood and school environments determine children's life-time opportunities. However, distinguishing changes in opportunities arising from residential sorting and from neighborhood or school attributes remains particularly challenging (Cutler and Glaeser, 1997; Sharkey, 2016). For social scientists interested primarily in education outcomes, this issue is even more salient that pupils' assignment to public schools is most often based on a catchment area system, as in France. Since parents are partially constrained by a legal map of school districts, residential sorting and social segregation at school self-nurture, as families can strategically choose where to live, and do so

[1] At the peak of the program in 2007, enterprise zone subsidies amounted to approximately €500 Mn per year, or about €360 per resident, against less than $150 for federal Empowerment Zones and about £60 (less than €100) for the UK Local Employment Growth Initiatives.

by 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).

To overcome this econometric challenge, we exploit the quasi-natural experiment provided by a French reform that redrew the contour of urban neighborhoods eligible for place-based subsidies on the basis of a sharp poverty cut-off hardly predictable and manipulable by individuals. Some neighborhoods that were not previously targeted by the French urban policy were qualified because they had a median income below the poverty cut-off. Symmetrically, some neighborhoods that were previously treated by the urban policy were disqualified, because their median income was lying above the poverty threshold. Therefore, without any change in district school boundaries concomitant with the reform, some schools "entered" or "exited" the urban policy zoning. This reform, which occurred a few months before the start of the school year 2014-2015, provides a unique opportunity to exploit those spatio-temporal shifts to provide a causal identification of the impact of the French urban policy on school choices. It allows us to overcome two econometric issues commonly associated with the evaluation of such policies. First, we leverage the discontinuity design of the reform in a local difference-in-differences setting to control for selection into treatment, as (dis-)qualification was directed to the most (least) deprived neighborhoods, that are more (less) likely to host pupils with difficult backgrounds, and to be avoided by parents.[2] Since the reform was based on a non-manipulable income cut-off, we can test whether schools located in neighborhoods lying above and below the poverty threshold witnessed significant changes in their pupils' enrollment after rezoning. Second, we also control for key confounding factors such as pre-reform residential sorting across neighborhoods through school fixed-effects, many pupil and family observables, as well as time-varying indicators of school performances and school environment.

We find strong evidence of a spoiled reputation conveyed by policy designation, as public schools located in labeled neighborhoods experienced a significant drop of 3.5pp in their pupils' attendance post-reform, compared to their counterfactual analogues in unlabeled neighborhoods lying just above the poverty threshold. We show that this "zone-and-shame" effect was triggered by behavioral reactions from parents of all socioeconomic backgrounds, who black-marked public schools located in disparaged neighborhoods, and shifted to public schools located in unlabeled neighborhoods or, to a less extent and only for the wealthiest ones, to private schools. We document only weak signals of stigma reversion triggered by neighborhood dis-labeling, which suggests hysteresis effects associated with the blemish of policy designation.

Besides these contributions, our paper improves on the extensive literature showing that place-based policies have few positive effects on residents' outcomes (Freedman, 2015; Freedman et al., 2021), especially in France (Malgouyres and Py, 2016; Lafourcade and Mayneris,

[2]In France, there is evidence 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). They have a lower probability of repeating a grade compared to pupils living nearby -but outside- treated neighborhoods, but they have a higher probability of choosing a vocational rather than an academic track after secondary education (Alivon, 2021).

2017). First, we enlarge the focus on education outcomes, which fills an overlooked -though policy important- niche in this literature. Most papers evaluating place-based policies have indeed focused on Enterprise Zones (hereafter EZ), as they are the most widespread urban policy worldwide. EZ aims at attracting businesses into deprived neighborhoods through tax and payroll alleviation conditioned to plant settlements in those areas. Yet, their effectiveness in reviving low-income neighborhoods is ambivalent. EZ policies are generally successful in attracting economic activities that had previously been dormant due to insufficient expected profitability, especially in zones benefiting from good transport connections (Briant et al., 2015). But they also attract businesses that would have settled 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 delocate or go bankrupt after the tax exemption period (Givord et al., 2018), hereby generating potentially large windfall effects or negative spillovers on untargeted neighborhoods. As for place-based policy effects on local labor markets, they depend on how labor supply adjusts to plant settlements in the targeted zones. If new labor demand is met by the local labor force, EZ policies yield substantial increase (decrease) in jobs (unemployment) accruing to residents (Ham et al., 2011; Busso et al., 2013; Freedman, 2013; Bartik, 2020), even more so that they are associated with local hiring conditions (Charnoz, 2018; Ehrlich and Overman, 2020). Otherwise, they do not decrease much local unemployment (Gobillon et al., 2012; Gibbons et al., 2021), and composition effects triggered by the arrival of more employable residents drive a large share of this drop (Freedman, 2012; Charnoz, 2018; Chyn and Katz, 2021), suggesting gentrifying benefits of the policy captured by initially non-targeted populations (Reynolds and Rohlin, 2015; Freedman, 2015). Besides, there is evidence that EZ policies capitalize into real estate markets (Hanson, 2009; Ehrlich and Seidel, 2018; Kitchens and Wallace, 2022), especially if treated neighborhoods cannot adjust quickly their commercial or housing supply.

Much less is known about the causal impact of place-based urban policies on the education paths of residents, as most prominent existing studies leverage spatial variations in neighborhood and school attributes through children's moves. For instance, Gould et al. (2004) and Gould et al. (2011) exploit variations in living conditions experienced by Ethiopian and Yemenite repatriates to Israel, to show that children placed in more favorable schooling or urban environments experience better education outcomes in adulthood. Äslund et al. (2011) build on a similar refugee quasi-experiment placement policy in Sweden to provide evidence that child migrants arriving at a young age in a neighborhood with a larger share of highly-educated adults from their own ethnicity perform better at school. The MTO experiment and housing demolitions quasi-experiments in the U.S. also suggest that moving to a higher-income neighborhood prior to adolescence can yield dramatic improvements in educational achievements (Chetty et al., 2016), and economic opportunities later on (Chetty and Hendren, 2018a,b; Chyn, 2018). Baum-Snow et al. (2019) is one outstanding exception that analyzes how neighborhood attributes affect the education opportunities of sedentary children, by leveraging quasi-random variation in skill-specific labor demand shocks hitting U.S. census tracts. They show that children growing up in higher quality school districts have better

socioeconomic outcomes later on. However, regardless of whether they focus on movers or stayers, all previous papers are only indirect evidence that urban policies matter for educational gaps, as none of them examines whether labeling and treating neighborhoods actually changes the education path of incumbent students.

Our work is also related to a large body of evidence on the impact of place-based affirmative actions aiming at increasing the probability that students from low-income neighborhoods gain admission to 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 -especially the most disadvantaged ones- drop out after middle school. By way of contrast, Behaghel et al. (2017) show that moving disadvantaged adolescents to boarding schools benefit only to initially strongest students, and only once they have adapted to their new school. Abdulkadiroğlu et al. (2014) and Dobbie and Fryer (2014) show that attending a school with high-achieving peers has little impact on academic achievements, 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 worst relative ranking within selective schools (Behaghel et al., 2017; Barrow et al., 2020).

This paper also relates to a small body of U.S. evidence showing that there are positive impacts of extra-resources provided to schools on students' test-scores (Card and Payne, 2002; Papke, 2005; Jackson et al., 2015), or on educational attainments and earnings later on (Lafortune et al., 2018; Schmick and Shertzer, 2019). However, similar studies performed in the French context are much less optimistic (Bénabou et al., 2009; Feigenberg et al., 2019; Benhenda and Grenet, 2020). A few papers find that compensatory education policies may be even counterproductive, if schools invest their extra-resources in less efficient teaching techniques (Leuven et al., 2007), or if families from different social backgrounds select themselves into (or out of) beneficiary schools (Beffy and Davezies, 2013; Davezies and Garrouste, 2020).

Finally, our paper more closely relates to the burgeoning literature assessing the legacy of U.S. 'redlining' maps on the development of urban neighborhoods classified as the most 'hazardous' for investment. For instance, Aaronson et al. (2021) show that redlining had a profound and long-lasting influence on various local outcomes such as home ownership rates, house values or rents, and racial segregation. Aaronson et al. (2022) also find that children living in the lowest-graded neighborhoods had significantly lower levels of educational attainment. Likewise, a few non-U.S. studies underline that labeling urban neighborhoods for policy purposes may stigmatize their residents in various ways, by discriminating them on the labour market (Petit et al., 2020), by depreciating their housing values (Koster and van Ommeren, 2022), by reducing economic transactions in their residence neighborhood (Besbris et al., 2014), or by worsening adolescents' schooling paths there (Domínguez et al., 2022).

We depart from these previous papers in several ways, however. First, we leverage outstanding longitudinal exhaustive administrative data from multiple cohorts of students throughout a decade, that we combine with rich geo-coded information on middle schools schools, neighborhoods and urban policy delineation, to investigate school choice responses to neigh-

borhood labeling. The second key novelty comes from our econometric strategy that combines a discontinuity design with panel techniques to avoid compounding the impact of policy designation with neighborhood or school composition effects. Finally, in contrast with most previous studies, we show that residing in a treated neighborhood does not only affect children through mechanisms involving school resources and peer networks, but also through significant changes in the perception of school quality. As such, policy designation influences parental beliefs in a way very similar to publicly displaying information about school scores through the media (Friesen et al., 2012; Koning and van der Wiel, 2013).

The remainder of the paper is structured as follows. Section 2 introduces the institutional context and describes the reform we leverage to evaluate the causal impact of neighborhood labeling on school choices. Section 3 presents our empirical framework and the data. Section 4 outlines our average treatment effects. Section 5 checks for the robustness of our point estimates, while section 6 explores heterogeneous effects across various dimensions, among which parental socioeconomic status and occupations. Last, Section 7 concludes.

2 The French institutional background

In the past four decades, spatial inequalities have peaked within French cities and their dramatic consequences in terms of segregation, exclusion, and juvenile delinquency or violence, have constantly called for innovative political responses. The French urban policy aims primarily at reducing the vulnerability of low-income neighborhoods. It is cross-ministerial and covers multiple domains including education and early childhood, employment preservation and job creation, housing rehabilitation and urban renewal, health, social cohesion, security and prevention of delinquency. It consists both in the reinforcement of common law policies and social services, and in the mobilization of specific measures, such as tax credits and payroll exemptions or extra public support for the local urban fabric.

From their inception in the late 1970's, place-based urban policies in France have been lyign on multiple zoning systems, and on various eligibility rules for benefiting from 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 we leverage in this study, that was implemented by the Lamy's law for cities and urban cohesion, which was passed on February 2014.

2.1 Place-based urban policies in France

The first French urban zoning system, entitled *Habitat et Vie Sociale* (Housing and Social Life) was launched in the mid-1970's. Primarily designed to foster urban social cohesion, it had been met with little success in the context of rising unemployment driven by oil crises. The surge in repeated urban riots in the late 1970's, 1980's and 1990's brought to the forefront the distress of the idle suburban youth, and prompted French politicians to launch a comprehensive set of measures entitled *Pacte de Relance pour la Ville* (Urban Stimulus Package) in 1996.

Like other concomitant programs around the world,[3] it was originally designed to generate a big push in favor of the most distressed urban neighborhoods.

The first pillar of this program was based on a three-tier zoning system of deprived urban neighborhoods: the first-tier level, 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 job-to-resident ratio. Among them, 416 *Zones de Redynamisation Urbaine* or ZRU (Urban Revitalization Zones) became Enterprise Zones. Their selection stemmed from ranking ZUS according to a multi-dimensional index of deprivation combining local population size, unemployment rate, proportion of residents with no diploma, share of young residents and local tax base. Finally, the 44 ZRU that seemed the most underprivileged were declared *Zones Franches Urbaines* or ZFU (subsequently, these would be known as the first-generation of ZFU). Firms settling in a ZRU or a ZFU could benefit from multiple tax credits and payroll exemptions, but the generosity and longevity of these rebates was much higher in ZFU, where the recruitment of residents was also fostered through a local hiring condition for receiving payroll taxes exemptions. Finally, 66 further ZFU were created in 2004 and 2006 (second- and third-generations of ZFU), out of the stock of ZRU that had not already been designated ZFU.[4] By that time, the annual cost of the French urban policy program reached more than half a billion euros.

In 2007, the French urban policy expanded to cover 1,750 further neighborhoods experiencing unemployment, violence or housing difficulties. Urban Social Cohesion Contracts (CUCS hereafter) were signed between central and local authorities in charge of most of the neighborhoods treated by the urban policy (i.e. a total of about 2,500 zones),[5] hereby committing to implementing concerted actions to improve the daily life of their residents. The vast majority of neighborhoods covered by the first pillar (i.e. 741 over 751 ZUS) could therefore combine the benefits of various zoning systems.

2.2 The 2014 reform to the French urban zoning system

The juxtaposition of various zoning systems had forced French public authorities to combine a regulatory approach based on automatic advantages (ZRU/ZFU), with a contractual approach generating potential -but not automatic- credits (Other ZUS/CUCS), while the two approaches were not necessarily benefiting to the same urban neighborhoods. In 2012, the French Court of Audit roundly criticized the dilution of public action over too dispersed and complex urban zoning systems, and the low cost-benefit ratio of the policy (Cour des Comptes, 2012). In an effort to increase the cost-effectiveness and citizens' intelligibility of the policy, and to harmonise the legal and contractual zoning systems, the French Minister of Urban Affairs in function, Pascal Lamy, decided to undertake a complete overhaul of 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.

[4]Since political considerations came into play for such designation, the selection of second and third generations of ZFU was more exogenous. Moreover, the perimeter of initial ZUS was sometimes expanded to include vacant land plots to attract more enterprises, resulting in two urban zoning systems not perfectly nested.

[5]Figure 10 provided in Appendix B illustrates these imperfectly nested "Russian dolls" zoning systems.

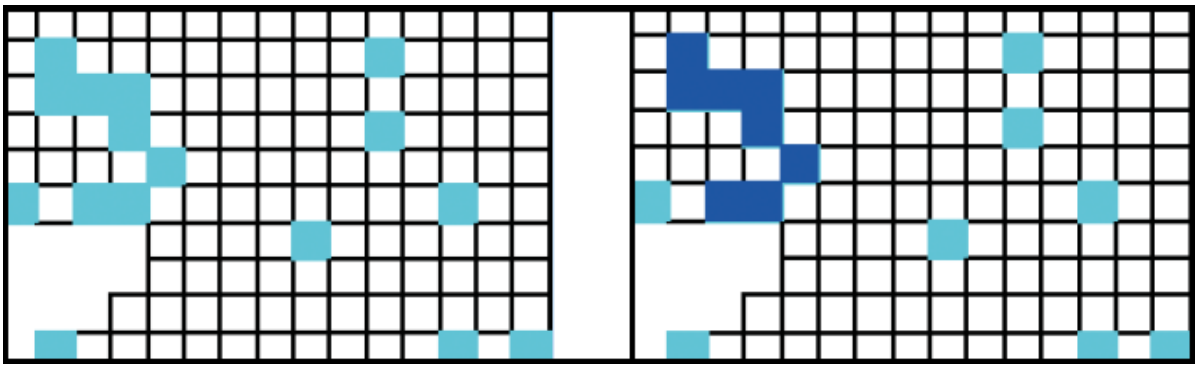
urban policy, which was announced and implemented in mid-2014.

To reduce the scattering of public resources, zoning systems stacked up for more than thirty years were replaced by a single and tighter urban zoning, so as to refocus public subsidies on approximately 1,300 neighbourhoods in Mainland France (and 214 further neighborhoods in French overseas departments and Polynesia), that were denominated "Priority" neighborhoods (*Quartiers Prioritaires* or QP thereafter). From 2014, a unique poverty criterion was required to identify those neighborhoods: a median income below 60% of a reference income computed as a weighted average of the nationwide and citywide median incomes per consumption unit.[6] Let denote I_{FR} the median income per consumption unit in mainland France and I_{UU}, the counterpart in a given urban unit, that are both hardly manipulable by local authorities.[7] The reference income I_R was defined as follows:

- 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:[8] $I^R_{UU} = 0.3 \times I_{FR} + 0.7 \times I_{UU}$.

The detection of poverty clusters was then based on a very fine scanning (200-meter squares) of the French territory, such as the one depicted in Figure 1 (Quantin and Sala, 2018). Once detected, contiguous squares of more than 1,000 inhabitants below the poverty cut-off were amalgamated to form a single unbroken spread zone, illustrated for the Paris region on Figure 2 (QP correspond to grey areas, other areas refer to the old zoning systems). QP boundaries were generally delineated to map the street design,[9] and their outline was sometimes adjusted marginally after their initial delineation, at the request of local authorities, as long as boundary changes were complying with the poverty cut-off.

Figure 1 – Selection of squares with a median income below 60% of the reference income



Source: Quantin and Sala (2018).

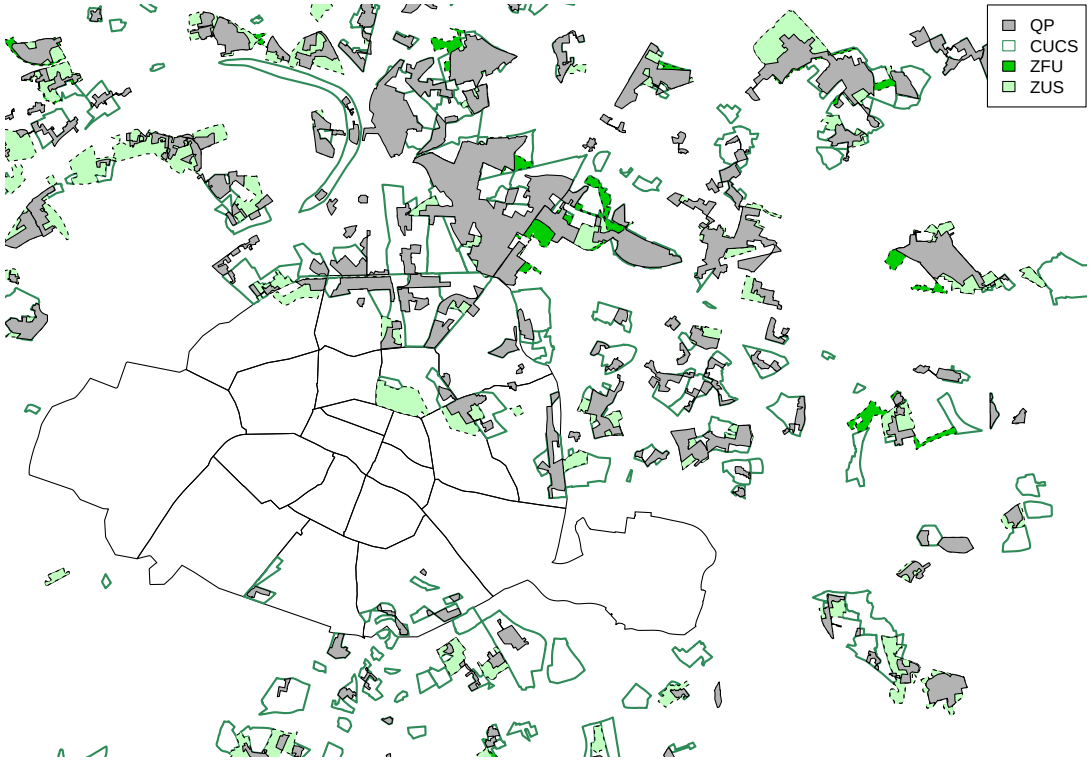
[6]The French Statistical administration computes consumption units as follows: the first adult in a household counts for 1, other relatives over 14 years for 0.5 and children under 14 for 0.3.

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

[8]The data used for such computation (*Revenus fiscaux et sociaux localisés des ménages 2011*) come from the French National Institute for Economic Studies (Insee, hereafter), and refer to declared incomes (i.e. before redistribution) in 2011. For urban units above 5 million inhabitants, i.e. Paris, the formula is reversed to put more weight on the local median income of the urban unit, which is significantly higher than the national median income (I_{FR} =€19,218 for Mainland France in 2011).

[9]Vacant land plots were excluded from their perimeter.

Figure 2 – Old and new urban zoning systems in the Paris area



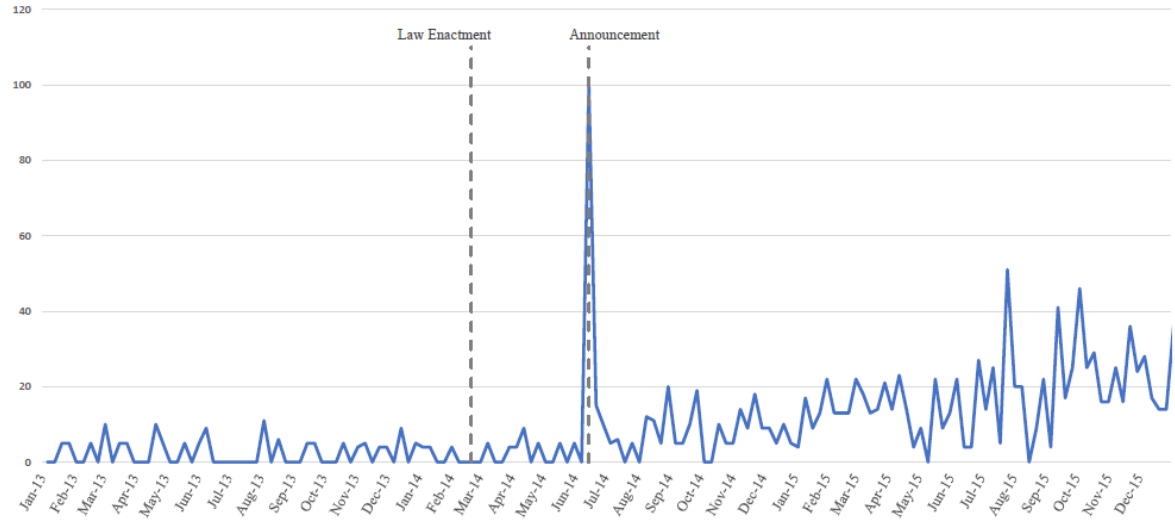
Source: Shapefiles from the French Ministry of Urban Affairs (ANCT-CGET).

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

Public intervention on those QP was formalised through State-City contracts of 6 years on the basis of three main objectives: (i) increasing social cohesion (through non-for-profit organization subsidies, or the construction of new social, cultural, transport and sport facilities); (ii) improving the living environment of residents (construction of new public condominiums, rehabilitation of existing social housing, public subsidies to private home-ownership); (iii) promoting economic development, employment and labor market participation (through tax breaks or other positive discrimination policies).

It is important to note that the reform also gave residents the means to actively engage in the underlying political process, as citizens' councils were set up to participate in the development of State-City contracts. Moreover, the reform came along with the development of search engines to help individuals find out precise information on the policy zoning (see Figure 9 in Appendix A). Figure 3 shows that the number of google queries on the new urban policy picked at the date of the reform announcement, that was made official a few months before the school year 2014-2015. The empowerment and information of residents is an important change relatively to the urban policy implemented in the past, as it might have yield (at least some) parents to adapt their behaviour quickly in response to the reform shock, according to their new beliefs, perceived educational returns, and preferences.

Figure 3 – Google queries for *Quartier Prioritaire de la politique de la ville (QP)*



Source: Google.

It is also crucial to emphasize that most (around 85%) of the neighborhoods covered by the new urban zoning overlap, at least partly, with formerly treated neighborhoods, the vast majority of which kept being "watched" or even treated by various former tax schemes after the reform, even though thay might have been disqualified from the urban policy program.[10] This treatment continuity implies that the 2014 reform may have had a larger impact on incoming than on outgoing neighborhoods/schools, a conjecture that will be corroborated by our empirical analysis below.

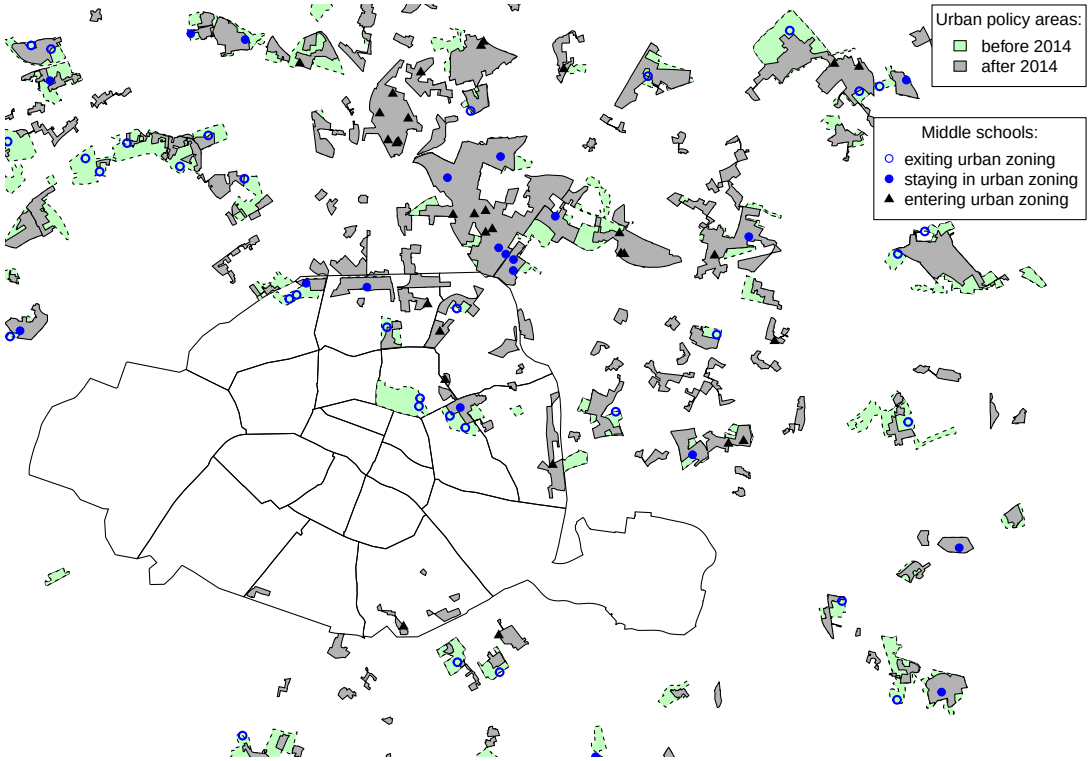
2.3 An illustration of the reshuffling of middle schools driven by the reform

The Lamy's reform provides a unique opportunity to exploit boundary changes in the urban zoning to estimate the impact of place-based policies on school choices and social segregation. Following the Lamy's reform, some neighborhoods that were previously treated were disqualified from the policy because their had a median income above 60% of the reference income, whereas some previously non-treated neighborhoods became eligible to the program because they had a median income below this threshold. As a result, without any changes in school district boundaries concomitant with the reform, district schools located within newly-treated neighborhoods "entered" into the policy zoning, whereas district schools located within no longer-treated neighborhoods "exited" out of the policy coverage.

Figure 4 gives an illustration of the reshuffling of middle schools triggered by the reform in the Paris region, that includes many notorious distressed neighborhoods covered by the French urban policy. Black polygons depict the boundaries of Parisian *arrondissements*, dashed light green polygons represent the ZUS and ZFU (neighborhoods treated before the reform), and dark grey polygons the QP (neighborhoods treated after the reform). Filled blue circles

[10] For instance, establishments located in a ZFU were eligible for tax credits up to 14 years after their installation, implying that a firm that settled in a ZFU in 2013 is eligible for public urban subsidies until 2027.

Figure 4 – The 2014 urban policy reform in the Paris region



Source: Base centrale des établissements (DEPP - Ministère de l'Éducation), urban zoning shapefiles (ANCT-CGET)

Note: Boundaries of Parisian *arrondissements* 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 they will be considered afterwards). Empty blue circles (respectively filled black triangles) represent middle schools that "exited" out of ("entered" into) the policy coverage, and filled blue circles middle schools that were and are still covered by the policy.

refer to public middle schools that "exited" the policy zoning, filled black triangles to public middle schools that "entered" the zoning, and empty blue circles to public middle schools that were and are still covered by the policy. As can be seen, the reform brought about a relatively large reshuffling of public schools in the Paris region, likewise in many other French cities. We exploit those spatio-temporal shifts to quantify the causal effect of re-zoning on pupils' enrollment and attendance in schools located in newly designated and disqualified neighborhoods.

2.4 School-based compensatory education policies in France

This paper puts the spotlight on middle schools, and more specifically on pupils entering 6th grade for the first time (i.e. pupils aged about 11-12) over the period 2010-2019. In France, education is compulsory 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 represents 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 unique class with only one teacher in primary school to several classes with different teachers and class subjects in middle school. Second, middle school choice is a crucial decision for many families because it usually deeply influences pupils' education paths, all the more so that social segregation across French middle schools is large (Boutchenik et al., 2020; Ly and Riegert, 2015). As in many other countries, allocation to public schools in France is based on a catchment area system, in which every pupil is assigned to a single public school according to the parents' address.[11] In France, public schools are tuition-free and must accept all pupils, regardless of their family background, previous academic performance or special learning needs. Therefore, most pupils go to their district school, which is the default rule. However, parents can request to enroll their kid into another public school outside their district area. As long as the maximum capacity of that school is not reached after accepting all pupils who attend it by default, dispensations can be granted by academy rectors on the basis of either special medical needs, scholarship grant, enrolled siblings, residence proximity, or specific curriculum (music or linguistic tracks for instance). Families can also opt for the private sector, which is not subject to any boundary system. 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 schools). Private schools charge fees, which are low on average in France compared to other countries, so that the private sector is relatively affordable for many families. For that reason, the share of pupils enrolled in the private sector throughout lower secondary education is relatively high in France (more than 20%, see Table 1 in section 3.2 below).

Disadvantaged schools can also benefit from specific compensatory education programs (school-based policies) ruled 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 school districts where social and academic disadvantages were the highest, or equivalently to 10% of lower secondary education students. This compensatory education program was originally meant to be temporary, but it was substantially expanded throughout the 1980's and 1990's. In the 1990's, successive reforms expanded its coverage to further schools, and created compensatory education "networks" by grouping primary, middle, and high schools within school districts, to mutualize pedagogical and financial resources.[12] In the 2000's two reforms reshaped the compensatory education scheme, and defined two school groups according to their level of social and academic distress. The least disadvantaged schools were labelled *Réseaux d'éducation prioritaire* (REP) and the most disadvantaged ones REP+. At the end of our period under study, the compensatory education program was covering 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's reform was that the urban policy and compensatory education schemes were nested through a new eligi-

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

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

bility criterion for benefiting from REP or REP+ compensatory schemes, based on the share of pupils living in a QP.[13] In practice, this inter-dependency calls for an estimation strategy that perfectly controls for whether middle schools benefit from compensatory education schemes, to isolate the urban policy impact from that of compensatory education programs.

3 Empirical framework and data

Our goal is to identify a causal effect of place-based policies on school enrollment. Because the correlation between school choice and urban policy is likely confounded by residential sorting or school sorting, we leverage the discontinuity design provided by the Lamy's reform in a local difference-in-differences framework, and we use panel techniques to control for district school heterogeneity. Let denote Y^s_{idt} a dummy indicating whether pupil i from cohort t, whose primary school falls within middle school district d, enrolls at middle-school-type s, which can be either the district school d, another public school, or a private school. Our treatment variable T_{dt} indicates whether the district school d falls into a urban neighborhood treated by the policy at time t. The probability to enroll at middle school type s is then modeled as follows:

$$Y^s_{idt} = \alpha^s + \beta^s T_{dt} + X_{it}\gamma^s + Z_{dt}\delta^s + \mu^s_d + \mu^s_t + \eta^s_{idt}, \tag{1}$$

where X_{it} (respectively Z_{dt}) is a vector of pupils' (resp. school district) observed characteristics, μ^s_t a year fixed effect, and η_{idt} the error term. Although we control for key observables at the pupil and school district levels, it is possible that unobserved factors such as school quality may still bias these estimates. To address this concern, we also include a district school fixed effect μ^s_d.

Our identification strategy does not rely only on the pseudo-panel structure of the data, but also on the discontinuity design provided by the Lamy's reform, as we also exploit the fact that, consequently to the reform, schools located in neighborhoods with a median income below (respectively above) the reference income "entered" (resp. "exited") the policy zoning, while observationally-equivalent schools located in counterfactual neighborhoods with a median income above (respectively below) the reference income remained untreated (resp. treated). As illustrated on Figure 4 for the Paris area, four types of public middle schools can be more precisely defined: (i) schools outside the old urban zoning systems (before the reform) and inside the new urban zoning (after the reform), (ii) schools inside the old urban zoning system (before the reform) and outside the new urban zoning (after the reform), (iii) schools inside both urban zoning systems (before and after the reform), and (iv) schools outside both urban zoning systems (before and after the reform). Assuming that school "entry" or "exit" is independent of neighboring families' preferences for schools once both school and neighborhood heterogeneity have been controlled for, we can use boundary changes caused

[13] The other eligibility criteria include the share of pupils from low socioeconomic backgrounds, the share of pupils with a public scholarship, and the share of pupils having repeated a grade upon entering 6th grade.

by the reform to recover the causal effect of the urban zoning on school enrollment (and also ideally on achievement gaps, though we cannot study this outcome as we do not have access to each individual pupil's test scores).

Let denote T_{dt}^{entry} (respectively T_{dt}^{exit}) a dummy which equals one if district school d is located in a neighborhood that entered (resp. exited) the policy zoning in 2014, and zero otherwise. For schools located in neighborhoods that were not already treated before the reform, we estimate the linear probability model:

$$Y_{idt}^{s} = \alpha_1^s + \beta_1^s T_{dt}^{entry} + X_{it}\gamma_1^s + Z_{dt}\delta_1^s + \mu_d^s + \mu_t^s + \epsilon_{idt}^s. \quad (2)$$

As for schools in neighborhoods that were already treated before the reform, we estimate:

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

Parameter β_1^s (respectively β_2^s) gives the causal impact of the urban policy on pupils' enrollment into schools located in newly (resp. no longer) treated neighborhoods, relatively to observationally-equivalent schools located in counterfactual neighborhoods, under the assumption that both types of neighborhoods would have followed the same path absent the policy reform. The β_1^s and β_2^s can be either positive or negative. Parents residing in low-income neighborhoods may expect that the urban policy will provide further resources helping their children to better perform at school. But policy-designation may also convey a negative image on the labeled neighborhoods. If parents were not perfectly informed about the quality of their district school, they may readjust their school preferences. Thereby, we could expect i) that high SES families react more than low SES parents, because changing school is less costly for them, and ii) that very well-informed families (for instance teachers) react differently from the other parents.

The "net" effect of the urban policy on school enrollment is thereby theoretically ambiguous, and potentially heterogeneous across families. If β_1^s is estimated negative, this suggests that, on average, the benefits of urban policy are more than offset by territorial stigmatization. Likewise, if families re-evaluate upwards school quality after neighborhood disqualification, and if this appraisal overcomes the reduction of public subsidies, β_2^s should be positive. Furthermore, the reform could impact school enrollment differently over time. If many parents fear that their district school will suffer from stigma effects and that the reform is likely to generate avoidance strategies from the other parents, coordination mechanisms and self-fulfilling expectations may interfere and perpetuate the stigma overtime. If parents simply re-gauge school quality in light of the information and the media buzz associated with the reform and its implementation, school choices may readjust only in the short-run.

3.1 Counterfactual neighborhoods

To evaluate the impact of the French urban policy on school choices, one could simply compare school attendance in labeled and unlabeled neighborhoods before and after the reform.

Yet, as the common trend assumption may not hold for those two groups, even once controlled for school fixed effects and other time-specific confounders, we restrict the control group to a set of plausibly good counterfactual neighborhoods.

Regarding incoming neighborhoods, we draw inspiration from Quantin and Sala (2018), and resort to the methodology used for delineating QP neighborhoods. As illustrated by Figure 5 for the Paris area, we select all census tracts intersecting contiguous squares with a median income just above (i.e. between 60 and 70% of) the reference income and, among those tracts, we exclude those intersecting a QP, so that control units may not be contaminated by policy spillovers. We end up with 216 counterfactual public middle schools scattered all over France located, by construction, in unlabeled neighborhoods very similar to treated zones (see Table 3 in section 3.2 for comparative statistics). As for outgoing neighborhoods, we proceed symmetrically and select as control units all QP's corresponding to former ZUS, ZFU or CUCS with a median income just below (i.e. between 50 and 60% of) the reference income. Though less comparable to the set of outgoing neighborhoods, we keep working with this control group, because restricting further the distance to the poverty cut-off would entail too few control units. However, we compare it to the set of outgoing neighborhoods not too far away from the poverty cut-off (i.e. between 60 and 70% of the reference income), to end up with more similar treated and control groups (see Table 4 in section 3.2 for comparative statistics).

Our estimation strategy consists then in comparing schools located in incoming or outgoing neighborhoods to their counterfactual schools, before and after the reform.

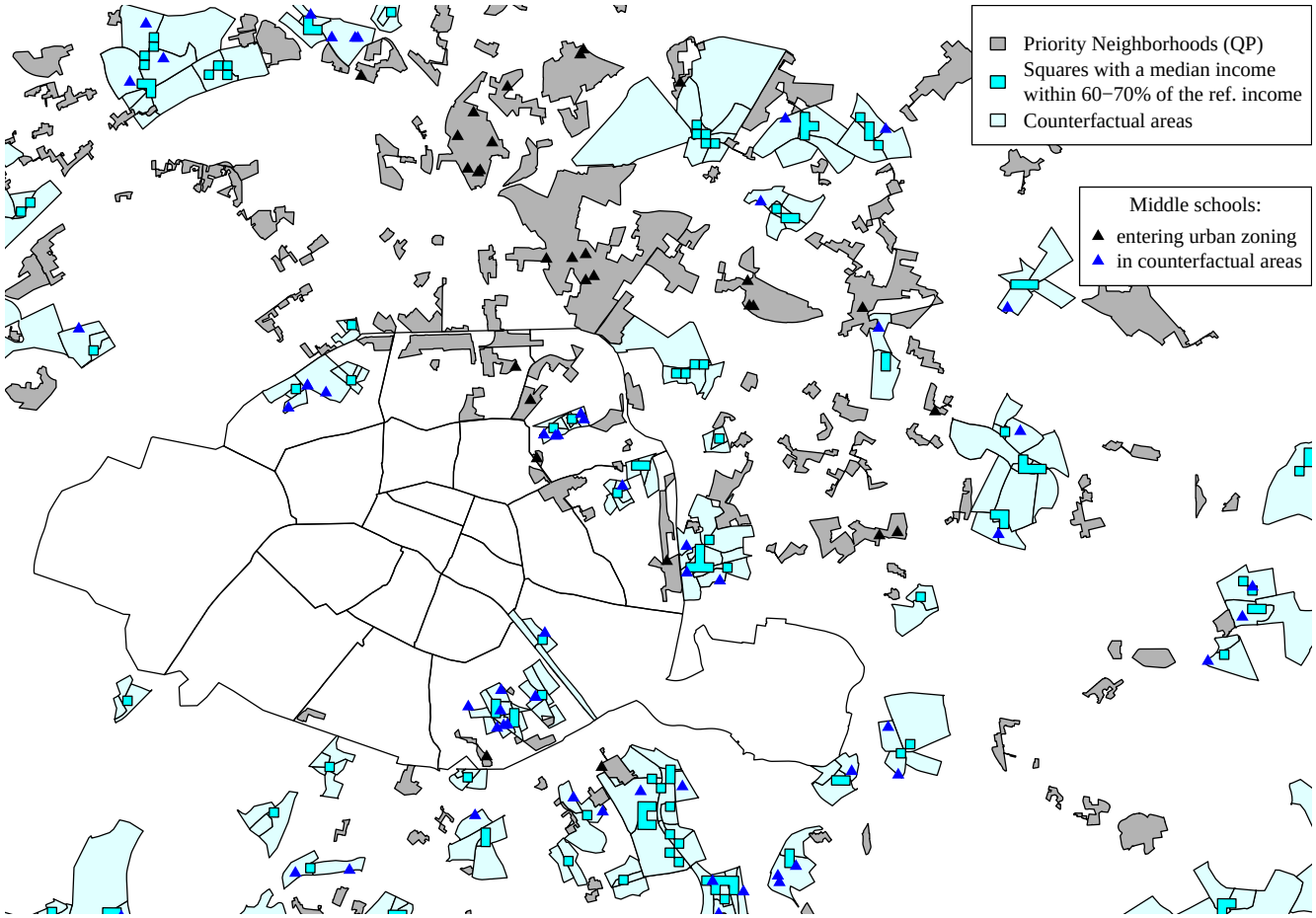
3.2 School data and descriptive statistics

We use exhaustive administrative data from several sources, that this section describes from the smallest to the largest spatial granularity. First, we mobilize the *Bases centrales scolarité* (BCS hereafter) from the statistical service of the French Ministry of education and youth (DEPP-ADISP), that provide information on the universe of pupils enrolled in French schools on a yearly basis, from 2010 to 2019. We were also given restricted access to geo-coded microdata on all pupils entering French lower secondary education (6^{th} grade) in September 2011, 2013, 2015 and 2017, that we use only for robustness checks.[14] For each pupil, we know the gender, country of birth, age and parental occupations, that we aggregate into five Socio-Economic Statuses (very high, high, medium, low and unknown SES).[15]

[14] Unfortunately, the home address of pupils has been geo-coded every even year only, so that we cannot reconstitute school catchment areas on an annual basis with those data sets.

[15] Very high SES include business managers, engineers, executives from both the private and public sectors, independent or creative professions, white-collars, professors and teachers. High SES include intermediate professions, technicians, clergy people and retired executives or intermediate professions. Medium SES include farmers, craftsmen, shopkeepers, public or private employees, police officers or military, and retired farmers/craftsmen/traders/managers. Low SES include skilled or unskilled blue-collars, students, unemployed and unoccupied people. Finally, unknown SES include all pupils for whom parental occupation is missing. We do not drop this category as it represents around 4% of our sample, which would entail a loss of statistical power for identification.

Figure 5 – Newly-treated and never-treated control schools in the Paris area



Source: Base Centrale des Établissements (DEPP - Ministère de l'Éducation), urban policy zoning (ANCT-CGET) and authors' calculation based on Quantin and Sala (2018).

Notes: Dark grey areas refer to the new policy zoning (QP), black triangles to middle schools "entering" this zoning. Blue squares are poverty squares with a median income just above (between 60% and 70% of) the reference income. Light blue areas are French census tracts (Iris) intersecting those squares, and blue triangles middle schools in those tracts.

BCS data allow us to identify the primary school of the pupil, her district middle school,[16] and her actual middle school of enrollment. We also know whether these schools are private or public, and whether they benefit from a compensatory education program (such as REP or REP+). As all schools are geo-coded, we can track school districts over time, and know more specifically if they have experienced school openings or closures over our studied period. To take into account schooling options likely to compete with the district school each year, we compute various time-varying indicators such as the number (or the share) of private schools located within a given radius of the pupil's primary school, or within its host urban unit.[17]

For public middle schools surveyed in the online application *Aide au Pilotage et à l'Auto-évaluation des Établissements* (or APAE),[18] we also have time-varying indicators of school performance and resources, such as the share of graduates at the *Brevet National des Collèges* exam (or DNB hereafter),[19] the gap between the DNB success rate of low SES pupils and the average DNB success rate in the school (or the DNB success rate of the whole academia), the number of teachers or different proxies of class size. The sample attrition triggered by these controls is very large, however. Obviously, such censoring is an issue if, for instance, the most disadvantaged schools choose not to disclose information on purpose. Therefore, we will use these controls with parsimony and only for robustness checks, to avoid to create more selection issues than we already have.

We supplement pupil and school data with various Geographical Information Systems coming from the *Agence nationale de la cohésion des territoires* (ANCT-CGET), and allowing for a precise delineation of all neighborhoods treated by the urban policy, before and after the reform (i.e. ZUS, ZFU, CUCS and QP). Finally, the Insee gave us access to confidential data on the local median incomes used to define the new urban zoning. We combine them with open data published at the Census Tract and Urban Unit levels to recover the poverty cut-offs on which were based neighborhood policy designation, and to build our set of counterfactual neighborhoods.

Armed with all those data sets, we end up with a sample of 6,831 middle schools located in Mainland France (Corsica included), among which 1,878 (or approximately 28%) located within the urban zoning system (see Table 1). Around 7.5 million 6^{th} graders attended those middle schools over 2010-2019 (see Table 2). On average over this period, more than half (54%) of the pupils were enrolled at their district public school, 24% at another public school, and 22% at a private school.[20]

[16] Since we do not have a precise delineation of school districts for the whole French territory, we define the district school as the public middle school closest to either the pupil's primary school (BCS data) or the pupil's address (geo-coded data). Section 5 provides robustness checks to ensure that our results are not driven by any possible measurement error incurred by this assignation.

[17] These thresholds have been chosen upon clear criteria: 5km (respectively 7km) is our sample median (resp. average) distance between the pupil's primary school and its closest private middle school and 2km, our sample average distance between the pupil's primary school and its closest public middle school.

[18] This application, which centralizes information on the personnel, resources and performance of many public middle schools, has been developed by the French Ministry of Education for managerial and reporting issues.

[19] DNB is a French certificate of secondary education passed at the end of 9^{th} grade.

[20] Further descriptive statistics on the pupils' and schools' samples are provided in Tables 1 and 2.

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
District 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
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.

Table 2 – Description of the pupils' 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	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		
District public	4,069,682	54
Other public	1,762,704	24
Private	1,641,192	22
Total	7,473,578	100

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

Tables 3 and 4 provide balancing tests of school composition in treated and counterfactual neighborhoods. As expected, pupils living in school districts entering or exiting the urban policy zoning are less advantaged than the overall population of pupils. They come less often from high SES families, and they are less often born French citizens. They are also more likely to be enrolled at middle schools which are not their district schools. Our counterfactual groups of pupils are more comparable to the treated groups than the overall population. Even though some differences persist, they will be entirely washed out by district school fixed effects. The only threat to our estimation strategy would then be non-parallel trends in the social composition of treated and control schools before the reform. To rule out the possibility that we wrongly attribute to the urban policy something that would actually be due to such pre-trends, Section 5 will provide a robustness check with treatment-group specific linear trends to capture the eventuality of pre-trends.

Table 3 – Pupils' sub-samples: incoming vs entry-counterfactual schools: Average 2010-2013

	All pupils		Living in school districts entering zoning		Living in counterfactual school districts	
	Freq.	%	Freq.	%	Freq.	%
Gender						
Girl	1,431,732	49	5,650	49	68,248	50
Boy	1,474,328	51	5,890	51	69,545	50
Socioeconomic status						
Very high SES	672,613	23	2,042	18	29,910	22
High SES	377,038	13	1,518	13	18,549	13
Medium SES	793,666	27	3,255	28	38,419	28
Low SES	967,876	33	4,280	37	46,503	34
Unknown	94,867	3	445	4	4,412	3
Citizenship						
Other	69,397	2	413	4	3,138	2
French	2,836,663	98	11,127	96	134,655	98
Age						
7-10	91,702	3	299	3	4,359	3
11-12	2,808,714	97	11,214	97	133,166	97
13-17	5,644	0	27	0	268	0
Middle school choice						
District public	1,596,226	55	5,419	47	74,423	54
Other public	678,375	23	3,036	26	33,328	24
Private	631,459	22	3,085	27	30,042	22
Total	2,906,060	100	11,540	100	137,793	100

Note: Entry-counterfactual schools = public middle schools in never-treated neighborhoods with a median income just above (i.e. between 60 and 70% of) the reference income. Treated schools = schools in QP not previously covered by the urban policy.

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

Table 4 – Pupils' sub-samples: outgoing vs exit-counterfactual schools: Average 2010-2013

	All pupils		Living in school districts exiting zoning		Living in counterfactual school districts	
	Freq.	%	Freq.	%	Freq.	%
Gender						
Girl	1,431,732	49	128,738	49	38,802	50
Boy	1,474,328	51	132,211	51	38,826	50
Socioeconomic status						
Very high SES	672,613	23	43,976	17	8,037	10
High SES	377,038	13	31,460	12	6,623	9
Medium SES	793,666	27	65,471	25	17,739	23
Low SES	967,876	33	108,987	42	41,287	53
Unknown	94,867	3	11,055	4	3,942	5
Citizenship						
Other	69,397	2	11,115	4	5,809	7
French	2,836,663	98	249,834	96	71,819	93
Age						
7-10	91,702	3	7,058	3	1,795	2
11-12	2,808,714	97	253,219	97	75,429	97
13-17	5,644	0	672	0	404	1
Middle school choice						
District public	1,596,226	55	129,538	50	32,256	42
Other public	678,375	23	78,300	30	30,474	39
Private	631,459	22	53,111	20	14,898	19
Total	2,906,060	100	260,949	100	77,628	100

Note: Exit-counterfactual schools = schools in former ZUS, ZFU or CUCS with a median income just below (i.e. between 50 and 60% of) the reference income, and that are still covered by the urban policy (also QP). Treated schools = schools in census tracts with a median income just above (i.e. between 60 and 70% of) the reference income, and that are no longer covered by the urban policy.

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

4 Average Treatment Effects

In this section, we present the average treatment effects drawn from our local difference-in-differences models, exploiting the discontinuity design of the 2014 reform to increase the likeliness of parallel trends prior to the treatment (re-)assignment of neighborhoods.

4.1 Urban zoning entry and middle school choices

The results of the linear probability model we use to assess the impact of neighborhood labeling on school choices -i.e. equation (2)- are displayed in Table 5.

Table 5 – "Entry" in policy zoning and pupils' enrollment

	Probability to enroll at:		
	District public	Other public	Private
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	0.082***	0.041***	-0.123***
	(0.017)	(0.013)	(0.010)
Male pupil	-0.011***	0.006***	0.005**
	(0.002)	(0.002)	(0.002)
French pupil	-0.068***	-0.014	0.083***
	(0.010)	(0.010)	(0.009)
Pupil's age	0.011***	0.028***	-0.039***
	(0.004)	(0.003)	(0.004)
District school in comp. educ. prog.	0.009	-0.005	-0.004
	(0.013)	(0.011)	(0.012)
No. of 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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. For the sake of clarity, we do not report the constant.

Unsurprisingly, and regardless of changes in the urban policy, pupils from high socioeconomic backgrounds are more likely to attend a private school than pupils from medium socioeconomic background, while more disadvantaged pupils are more likely to be enrolled in a public school, and among those schools, in their district school.[21] The probability to attend a public school is higher for older pupils, who are more likely to experience backwardness. Conversely, the probability to attend a private school is larger for both French and male pupils. An increase of private schooling options in the school district raises the likeliness that parents opt for a private school, instead of another public school.[22] By way of contrast, compensatory education policies have no discernible impact on school choices, which suggests that additional resources provided to the most disadvantaged schools do not make them more attractive for parents, which is in line with the literature studying priority education schemes in France, in particular Bénabou et al. (2009).

As shown by column (1), after the 2014 reform, district schools located in labeled neighborhoods experienced a significant 3.5 percentage points drop in pupils' attendance, relatively to observationally-equivalent public schools located in similar -but unlabeled- counterfactual neighborhoods. This suggests that policy designation spurred parents to forge a negative representation of labeled neighborhoods and thereby, changed their perception of district school quality there. As 6^{th}-grade cohorts are composed of about 150 pupils per district on average, a drop by 3.5 pp in the probability of choosing the district school is equivalent to a loss of almost 5 pupils per school located in treated neighborhoods relative to their analogues in counterfactual unlabeled neighborhoods. Columns (2) and (3) show that parents avoiding disparaged district schools switched to other public schools (+4.1 pp), rather than to private schools (insignificant coefficient).[23]

Table 6 displays the short-run and medium-run effects of policy designation on school choices, by allowing the treatment to vary over time.[24] Results indicate that the "zone-and-shame" effect is immediate and reaches its peak on the school year which immediately follows the reform. However, the stigma vanishes later on, as point estimates become insignificant from 2015. The absence of medium-run impact of the urban policy designation on school choices suggests that the mechanisms driving school stigmatization are mostly informational and do not transit through parental coordination. It is worth noting that, even if the stigma is visible only the first-year after the reform, this penalty is likely to be perennial, since lower

[21] Note that the BCS data do not always provide information on the occupation of parents (this information is missing for 4% of the sample), which is the reason why Table 5 includes a SES category "unknown". The point estimate associated with this category is very similar to that of low SES. In section 6, which will study heterogeneous effects across SES, we will thus aggregate these two categories, as well as very-high and high SES, as they also behave similarly.

[22] As already mentioned in section 3.2, we have computed various time-varying indicators of schooling options available in school districts. Even though significance slightly diverges across indicators, our point estimates are remarkably stable across specifications. As our key findings continue to hold with alternate metrics, in the afterwards, we will stick to the number of private middle schools within a 5 km radius of the primary school, as this is the indicator with the largest spatial-time variability.

[23] Note that we cluster standard errors at the school level because most school districts include one single public middle school only. However, clustering by neighborhood does not change our point estimates, the significance and magnitude of which remain remarkably similar (results are available upon request).

[24] In the following, pupil and school district characteristics attract similar estimated coefficients as in Table 5. As such, we will not show nor discuss these estimates below.

secondary education lasts four years in France, and very few students change middle school throughout this curriculum. The extra-sorting and exacerbation of school segregation triggered by neighborhood labeling is therefore expected to last for a while.

Table 6 – "Entry" in policy zoning and pupils' enrollment - Changes over time

	Probability to enroll at:		
	District public	Other public	Private
$T^{entry-treatment2014}$	-0.032**	0.037**	-0.005
	(0.014)	(0.014)	(0.009)
$T^{entry-treatment2015}$	-0.007	0.004	0.003
	(0.011)	(0.011)	(0.009)
$T^{entry-treatment2016}$	-0.004	-0.005	0.008
	(0.013)	(0.012)	(0.008)
$T^{entry-treatment2017}$	0.008	0.007	-0.015*
	(0.013)	(0.012)	(0.009)
$T^{entry-treatment2018}$	-0.006	0.002	0.004
	(0.015)	(0.012)	(0.012)
$T^{entry-treatment2019}$	0.015	-0.003	-0.012
	(0.014)	(0.010)	(0.009)
R^2	0.166	0.123	0.187
No. obs	384,478	384,478	384,478
No. clusters	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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Interestingly, Table 7 shows that neighborhood labeling spurred parents to favor mostly public schools located outside the policy zoning (+2 pp higher attendance). This is perfectly consistent with the conjecture that parents used all the information conveyed by the reform to re-gauge school quality very quickly after reform, in light of the spatial defamation triggered by neighborhood labeling, inside or outside their school district.

Table 7 – "Entry" in policy zoning and sorting across schools

	Probability to enroll at:			
	District public	Other public		Private
		In zoning	Out zoning	
$T^{entry-treatment2014}$	-0.032**	0.017*	0.020**	-0.005
	(0.014)	(0.010)	(0.008)	(0.009)
$T^{entry-treatment2015}$	-0.007	-0.003	0.007	0.003
	(0.011)	(0.007)	(0.008)	(0.009)
$T^{entry-treatment2016}$	-0.004	0.003	-0.008	0.008
	(0.013)	(0.008)	(0.011)	(0.008)
$T^{entry-treatment2017}$	0.008	0.003	0.004	-0.015*
	(0.013)	(0.007)	(0.012)	(0.009)
$T^{entry-treatment2018}$	-0.006	0.010	-0.008	0.004
	(0.015)	(0.011)	(0.013)	(0.012)
$T^{entry-treatment2019}$	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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

4.2 Urban zoning exit and middle school choices

We proceed symmetrically to estimate the causal impact of the urban policy on de-zoned areas. To ensure the similarity of our treated and control groups here, we do compare school enrollment at district schools located in census tracts with a median income just above (i.e. between 60 and 70% of) the reference income (that are no longer covered by the urban policy), with school enrollment at public schools located in former ZUS, ZFU or CUCS with a median income just below (i.e. between 50 and 60% of) the reference income (that are still covered by the urban policy after the reform). Results are depicted in Tables 8, 9 and 10, that report the

coefficients drawn from estimating various augmented specifications of equation (3).

Table 8 – "Exit" of policy zoning and pupils' enrollment

	Probability to enroll at:		
	District public	Other public	Private
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	0.096***	0.033***	-0.129***
	(0.010)	(0.009)	(0.007)
Male pupil	-0.010***	0.008***	0.002*
	(0.001)	(0.001)	(0.001)
French pupil	-0.068***	0.007	0.061***
	(0.006)	(0.006)	(0.004)
Pupil's age	0.027***	0.026***	-0.053***
	(0.002)	(0.002)	(0.002)
District school in comp. educ. prog.	0.010	-0.009	-0.001
	(0.006)	(0.007)	(0.005)
No. of 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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

We find symmetric -though less significant- reform impacts on public schools located in outgoing neighborhoods not too far above the poverty cut-off. Columns 1 of Tables 8 and 10 suggest that de-zoning raised the probability for parents residing in disqualified neighborhood to choose their district school by 1.1 pp immediately after the reform, relatively to parents residing in counterfactual still-treated neighborhoods.[25] However, this regain of attractiveness, that amounts to less than 2 pupils by cohort, is not sufficient to offset the penalty triggered by neighborhood labeling. These more mixed results for outgoing neighborhoods

[25] As previously, pupils and school district characteristics attract similar estimated coefficients throughout specification. As such, we show their estimates in Table 8, but not afterwards.

Table 9 – "Exit" of policy zoning and sorting across schools

	Probability to enroll at:		
	District public	Other public	Private
$T^{exit-treatment2014}$	0.011**	-0.015***	0.004
	(0.006)	(0.005)	(0.004)
$T^{exit-treatment2015}$	-0.003	0.002	0.001
	(0.005)	(0.006)	(0.004)
$T^{exit-treatment2016}$	-0.009*	0.001	0.008*
	(0.005)	(0.005)	(0.004)
$T^{exit-treatment2017}$	0.000	0.001	-0.002
	(0.006)	(0.006)	(0.004)
$T^{exit-treatment2018}$	-0.004	-0.002	0.006
	(0.006)	(0.006)	(0.004)
$T^{exit-treatment2019}$	0.004	0.002	-0.006
	(0.006)	(0.006)	(0.004)
R^2	0.167	0.114	0.211
No. obs	954,666	954,666	954,666
No. clusters	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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

are not so surprising in light of the reform design, however. As recalled in section 2, neighborhoods exited out of the policy scheme very gradually, and most of them had actually been treated for decades before disqualification. As a matter of consequence, hysteresis skewing parents' beliefs may have prevented them from reassessing drastically school quality, despite de-zoning.

Symmetrically to entrance, benefits accruing to district schools located in dis-labeled neighborhood occur at the expense of public schools outside the policy zoning, that witnessed a 1.2 pp drop in their pupils' attendance (see column 3 of Tables 9 and 10). However, those gains were rapidly offset by a backlash afterwards (-0.9 pp in 2016), that benefited to the private sector (-+0.8 pp), which explains the positive coefficient displayed on the first line of column (3) in Table 8. The impact of neighborhood disqualification is thereby ultimately insignificantly null on average. This result is not surprising in light of the reform design described in section 2, that planned only a gradual exit of formerly-treated neighborhoods from the policy coverage. The informational shock triggered by the reform was thereby probably not sufficient to alleviate more than forty years of territorial stigmatization carried out by public

Table 10 – "Exit" of policy zoning and pupils' enrollment - Changes over time

	Probability to enroll at:			
	District public	Other public		Private
		In zoning	Out zoning	
$T^{exit-treatment2014}$	0.011**	-0.003	-0.012***	0.004
	(0.006)	(0.004)	(0.003)	(0.004)
$T^{exit-treatment2015}$	-0.003	-0.001	0.003	0.001
	(0.005)	(0.005)	(0.004)	(0.004)
$T^{exit-treatment2016}$	-0.009*	0.000	0.000	0.008*
	(0.005)	(0.004)	(0.004)	(0.004)
$T^{exit-treatment2017}$	0.000	0.005	-0.004	-0.002
	(0.006)	(0.005)	(0.005)	(0.004)
$T^{exit-treatment2018}$	-0.004	0.002	-0.004	0.006
	(0.006)	(0.005)	(0.005)	(0.004)
$T^{exit-treatment2019}$	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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

schools located in disqualified neighborhoods.

4.3 Sorting across schools or residential moves?

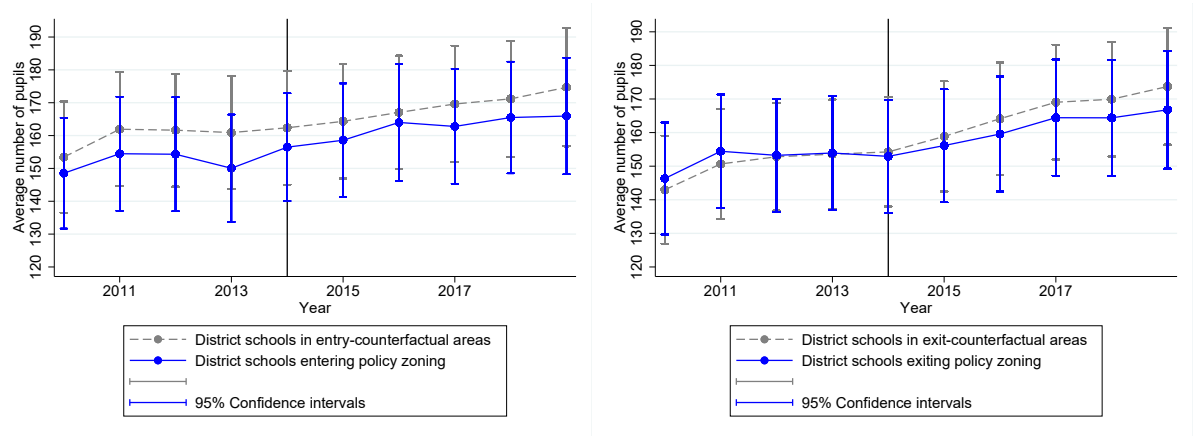
So far, we have not identified whether policy effects are driven by parental re-sorting across schools or across neighborhoods. As already mentioned in section 2, in France, parents can bypass the legal school district map in three ways: either by changing their residence to get assigned to a "better" -or perceived as such- default district school, or by opting for a private school (in 22% of the cases in France, as shown by Table 1), or by requesting a derogation to enroll their kid outside their district of residence (24% of the cases in France).

As a matter of fact, most households living in neighborhoods targeted by the French urban policy reside in social housing, and they are far less mobile on average than people residing in better-off neighborhoods (Briant et al., 2015).[26] Besides, the eligibility criterion upon which

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

was based the selection of neighborhoods was totally unpredictable for parents, as there were no publicly available local income data that could have allowed them to compute the poverty threshold. Therefore, we conjecture that parents were unlikely to plan a residential move quickly either just before or just after the reform announcement.

Figure 6 – Average number of pupils in district schools



Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Figure 6 provides strong credit to this conjecture. It displays no significant differences over time in the average number of pupils assigned to the district schools affected by the reform, either those entering the urban zoning (left figure), or those disqualified from the policy (right figure), and the control district schools. As we do not find any evidence of a "Tiebout flight" triggered by the reform, we can safely conclude that parents did not change their residence in reaction to the reform and re-sorted across schools rather than across neighborhoods, by asking for more (entry) or less (exit) opt-out derogations for not enrolling their children in their district school.

5 Robustness checks

Before proceeding any further, it is important to check whether our average treatment estimates are robust to several econometric robustness checks. This section provides two placebo tests allowing us to test the validity of the common trend assumption needed in our difference-and-differences setting. It then assesses whether policy impacts are robust (i) to a change in the treatment definition, (ii) to the inclusion of time-varying indicators of school quality, and (iii) to the way we identify school districts.

5.1 Pre-reform falsification tests

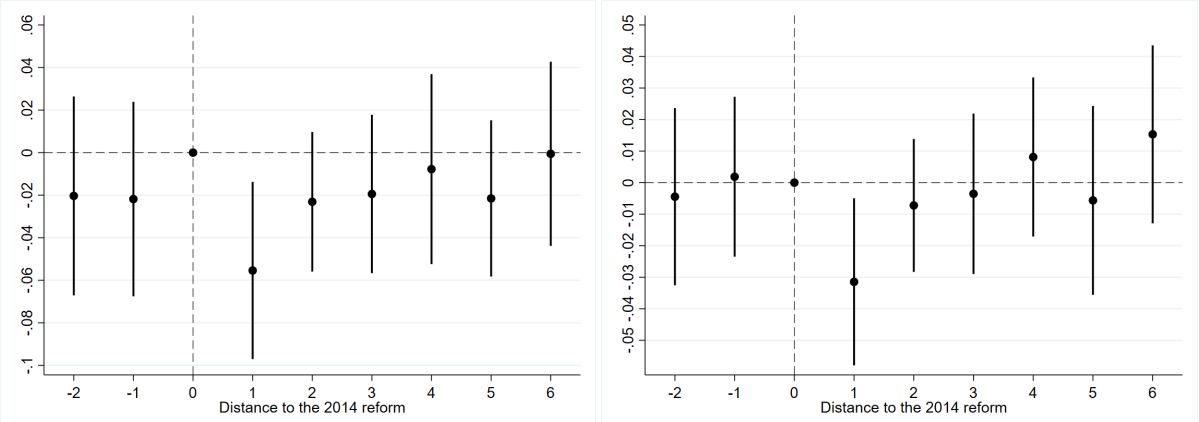
One key threat to causal interpretation is the potential existence of pre-trends. Admittedly, one might argue that, despite school fixed effects, parallel trend assumptions could be violated if schools located in qualified or disqualified neighborhoods started diverging before the reform, compared to schools located in their counterfactual neighborhoods. A common

diagnostic approach to validate the common trend assumption is to look at whether policy changes occurred before the policy implementation. We thus perform a placebo analysis in which we artificially set the reform date to either 2011, 2012 or 2013.

The top-panel of Table 11 provides the result of such falsification tests for neighborhoods entering the policy zoning. The placebo treatment for entry is never significantly different from zero, which is fully consistent with Quantin and Sala (2018), who have shown that there were no significant difference in the median income and employment rate of incoming neighborhoods and their counterfactual areas over 2007-2012. The bottom-panel of Table 11 reports similar falsification tests for outgoing neighborhoods. As a matter of fact, territorial stigmatization started to reduce slightly before the reform, as column (3) reveals significant parental switches both in 2012 and in 2014. The urban policy reform seems thereby to have exacerbated a pre-existing trend, and to have caused further pupils' transfers between the public and the private sectors.

To rule out the possibility that we wrongly attribute to the urban policy something that would actually be due to pre-trends, that we cannot totally preclude for disqualified neighborhoods, we include treatment-group specific linear trends into specifications (2) and (3). Results associated with urban zoning entry, which are displayed in the top panel of Table 12, stay fully consistent. The event studies provided on Figures 7 and 8 make us even more confident in our DiD setting for urban neighborhoods newly labeled to benefit from public subsidies, while providing further support to our key result that schools located in those neighborhoods were stigmatized later on by policy designation.

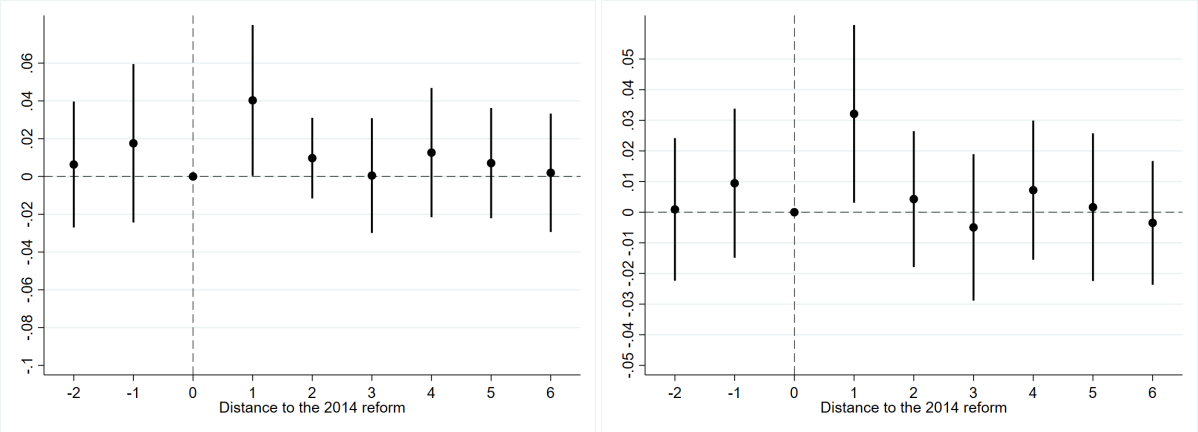
Figure 7 – Relative probability to choose the district school before and after policy designation - With (left) and without (right) treatment-group specific linear trends



Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

As for disqualified neighborhoods, to bottom panel of Table 12 displays point estimates that are now only barely significant in 2012 (at 10% levels only). That said, we cannot totally exclude the existence of pre-trends jeopardizing our conjecture that de-zoning alone may have contributed to dissipate the stigma.

Figure 8 – Relative probability to choose a public school other than the district school before and after policy designation - With (left) and without (right) treatment-group specific linear trends



Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

5.2 Post-reform placebo tests on 8th graders

To add further credit to our core result that neighborhood labeling had a causal impact on school choices, this section provides another placebo test inspired by Boutchenik et al. (2020). The purpose of this robustness check is to verify that parents who had already enrolled their children into lower secondary education at the time of the reform were unaffected by re-zoning, as there is much less reasons to believe that parents would adjust their preferences once they have engaged their children in a 4-years-long curriculum and, as a result of this, do already have information on school quality.

Tables 13 and 14 both show that enrollment into 8th grade was left totally unchanged by the reform, that did not produce any parental re-sorting across middle schools over the course of lower secondary education. This test definitely ensures that our average treatment effects are not pure statistical artefacts, but embody parental strategic resorting behaviors at the entry of lower secondary education.

5.3 Narrow definition of urban policy treatment

Before the 2014 reform, the French urban policy used to combine a regulatory approach based on automatic advantages (ZRU/ZFU), with a contractual approach generating potential -but not automatic- credits (Other ZUS/CUCS). As such, some neighborhoods were subject to discretionary interventions that might have resulted in low public subsidies in the end. Besides, whereas the geographical perimeter of ZUS, ZRU and ZFU was common knowledge for households because tax or pay-roll exemptions could accrue directly to residents, CUCS boundaries were known mostly by local authorities and institutional actors.

Therefore, while most parents were generally well informed about whether the district school assigned to their residence was part of a ZUS or a ZFU, many households were not aware of the existence nor of the perimeter of CUCS, which may have resulted in school

Table 11 – "Entry" in policy zoning and pupils' enrollment - Falsification test

	Probability to enroll at:			
	District public	Other public		Private
		In zoning	Out zoning	
$T^{entry-treatment2011}$	-0.004	-0.006	0.007	0.004
	(0.014)	(0.006)	(0.012)	(0.007)
$T^{entry-treatment2012}$	-0.006	0.012	-0.000	-0.006
	(0.013)	(0.009)	(0.010)	(0.011)
$T^{entry-treatment2013}$	0.016	-0.001	-0.005	-0.010
	(0.015)	(0.007)	(0.007)	(0.010)
$T^{entry-treatment2014}$	-0.043***	0.017	0.021***	0.005
	(0.013)	(0.012)	(0.008)	(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
$T^{exit-treatment2011}$	-0.004	0.009	-0.005	0.001
	(0.007)	(0.005)	(0.005)	(0.005)
$T^{exit-treatment2012}$	0.011	-0.008	-0.009**	0.006
	(0.007)	(0.005)	(0.004)	(0.005)
$T^{exit-treatment2013}$	-0.005	0.001	0.004	-0.000
	(0.006)	(0.005)	(0.004)	(0.005)
$T^{exit-treatment2014}$	0.002	0.001	-0.010**	0.007**
	(0.006)	(0.005)	(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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

perception or preference changes driven by the reform different from what we could expect under perfect information. To test this conjecture, we check whether our results are robust to a narrower definition of the urban policy treatment, whereby we consider that only ZUS, ZRU and ZFU were treated before the reform. Point estimates are displayed in Table 15.[27]

We obtain very similar results, the main difference being that the stigma attached to incoming neighborhoods is nearly twice smaller with this narrow definition of treatment. As

[27] Appendix C provides complementary results on yearly treatment effects and falsification tests (see Tables 22 and 23 for entry, as well as Tables 24 and 25 for exit).

Table 12 – Rezoning and pupils' enrollment - Treatment-group linear trends

	Probability to enroll at:			
	District public	Other public		Private
		In zoning	Out zoning	
$T^{entry-treatment2011}$	-0.006	-0.009	0.009	0.006
	(0.015)	(0.008)	(0.012)	(0.008)
$T^{entry-treatment2012}$	-0.007	0.009	0.002	-0.004
	(0.013)	(0.009)	(0.010)	(0.012)
$T^{entry-treatment2013}$	0.015	-0.004	-0.003	-0.008
	(0.015)	(0.007)	(0.007)	(0.010)
$T^{entry-treatment2014}$	-0.047***	0.006	0.029***	0.013
	(0.011)	(0.011)	(0.011)	(0.011)
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
$T^{exit-treatment2011}$	-0.001	0.007	-0.004	-0.001
	(0.007)	(0.006)	(0.005)	(0.005)
$T^{exit-treatment2012}$	0.013*	-0.009*	-0.008*	0.004
	(0.007)	(0.006)	(0.004)	(0.005)
$T^{exit-treatment2013}$	-0.003	-0.001	0.005	-0.002
	(0.006)	(0.005)	(0.004)	(0.005)
$T^{exit-treatment2014}$	0.011*	-0.005	-0.006	0.001
	(0.006)	(0.005)	(0.005)	(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	✓	✓	✓	✓
Group-Trends	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. For the sake of clarity, we do not report the intercept. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district school benefits from a compensatory education program, the number of private schools within a 5km radius of the pupil's primary school. For the sake of clarity, the constant and coefficients associated with these controls are not reported.

shown by top-panel of Table 15, the probability for parents to choose their district middle school is now 2.1 pp lower than in counterfactual neighborhoods (against -3.5 pp in Table 5), and the probability to choose another public school 2.6 pp higher (against +4.1 pp in Table 5). In line with our previous treatment definition, resorting is immediate and it benefits mostly to public schools outside the policy scheme, that experience a +1.7 pp increase in their attendance compared to control schools (see column 3 of Table 22 in Appendix C). Table 23 in Appendix C also pleads in favor of the common trend assumption. All of these robust-

Table 13 – "Entry" in policy zoning and pupils' enrollment in 8th grade

	Probability to enroll at:		
	Previous public	Other public	Private
T^{entry}	0.009	-0.008	-0.000
	(0.010)	(0.009)	(0.004)
SES (ref.=Medium)			
Very high SES	-0.001	-0.004***	0.005***
	(0.002)	(0.001)	(0.001)
High SES	0.005***	-0.004***	-0.000
	(0.002)	(0.001)	(0.001)
Low SES	0.007***	0.002	-0.009***
	(0.001)	(0.001)	(0.001)
Unknown	-0.040***	0.037***	0.003
	(0.005)	(0.005)	(0.002)
Male pupil	-0.005***	0.004***	0.001**
	(0.001)	(0.001)	(0.000)
French pupil	0.005	-0.011***	0.006***
	(0.003)	(0.003)	(0.001)
Pupil's age	-0.028***	0.022***	0.006***
	(0.001)	(0.001)	(0.001)
Previous school in comp. educ. prog.	0.006	-0.004	-0.002
	(0.005)	(0.003)	(0.002)
R^2	0.010	0.009	0.006
No. obs	303,977	303,977	303,977
No. clusters	237	237	237
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. For the sake of clarity, we do not report the constant.

ness checks add strong credit to our key result that urban zoning stigmatized public middle schools located in labeled neighborhoods, and exacerbated social segregation at school.

As for disqualified neighborhoods, our point estimates stay remarkably similar, both in terms of magnitude and significance, as with our broader definition of policy treatment. The bottom-panel Table 15 suggests that de-zoning raises the probability for parents to choose their district school by 1.4 pp compared to control schools (against +11 pp in Table 8), and decreases their relative probability to choose another public school by 1.7 pp. However, the de-stigmatization seems more diluted over time as with our first definition of treatment (See Table 24 in Appendix C). Despite treatment-group specific linear trends, we still find evidence that parallel trends might be violated (See Table 25 in Appendix C) and hence, that we have to stay cautious in not interpreting our result as a clear signal of stigma reversion caused by de-zoning.

Table 14 – "Exit" of policy zoning and pupils' enrollment in 8th grade

	Probability to enroll at:		
	Previous public	Other public	Private
T^{exit}	0.003	-0.004	0.001
	(0.003)	(0.003)	(0.001)
SES (ref.=Medium)			
Very high SES	-0.003**	-0.004***	0.007***
	(0.001)	(0.001)	(0.001)
High SES	0.002	-0.004***	0.002***
	(0.001)	(0.001)	(0.001)
Low SES	0.014***	-0.006***	-0.008***
	(0.001)	(0.001)	(0.000)
Unknown	-0.038***	0.034***	0.004***
	(0.003)	(0.003)	(0.001)
Male pupil	-0.007***	0.006***	0.001***
	(0.001)	(0.001)	(0.000)
French pupil	0.008***	-0.011***	0.003***
	(0.002)	(0.002)	(0.000)
Pupil's age	-0.023***	0.021***	0.002***
	(0.001)	(0.001)	(0.000)
Previous school in comp. educ. prog.	0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.001)
R^2	0.010	0.009	0.007
No. obs	687,380	687,380	687,380
No. clusters	619	619	619
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group-trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. For the sake of clarity, we do not report the constant.

5.4 Controlling for time-changes in school quality

It might also be argued that school fixed effects do not totally partial out unobservables such as school quality, that is time-dependent. Therefore, this section provides a robustness check whereby we include a time-varying control of school quality, the share of DNB graduates at the end of 9th grade, computed over the two years preceding the pupil's enrollment.[28] As mentioned in section 3.2, a word of caution is needed here, since we do not observe the DNB success rates of all middle schools included in our sample, in particular several schools

[28] We choose the DNB success rate because the sample attrition is lower with this variable than with other time-varying indicators such as the gap between the DNB success rate of low SES pupils and the average DNB success rate in the school (or the DNB success rate of the whole academia), the number of teachers or class size. Results based on those indicators are available upon request.

Table 15 – Rezoning and pupils' enrollment - Narrow definition of treatment

	Probability to enroll at:		
	District public	Other public	Private
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
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. For the sake of clarity, we do not report the constant.

affected by the 2014 reform. Therefore, selection issues might be at stake.[29]

Tables 16 and 17 show that our results stay nevertheless robust. Unsurprisingly, the probability to enroll pupils at the district school increases with its past DNB performance, which suggests that parents do react positively to good indicators of district school quality, which raises the likeliness that they do not opt-out for a school other than the default district school, in particular a private school. More importantly, we find very similar average treatment effects. In particular, the impact of policy designation on public school choices is still highly significant, with point estimates of similar magnitude as without time-varying controls of school quality.

5.5 District school assignation

Finally, as we do not observe the exact perimeter of school districts, we have to check whether our district school assignation to pupils does not condition the results. For now, we have allocated public schools on the basis of their shortest distance to each pupil's primary school. By doing so, we may wrongly assign some schools to some pupils, for instance, if the actual district school is not necessarily the closest to the pupil's primary school, or if there are two public middle schools equally-distant to the primary school. If such error is not random, this may bias our point estimates.

[29]Furthermore, it must be noted that including time-varying indicators of school quality, even in the form of lagged-variables, is likely to raise dynamic endogeneity issues with our fixed-effects panel setting, if the share of DNB graduates is correlated to families' unobserved preferences for schools. These estimations thereby only serve robustness purposes, and this explains why we have excluded the share of DNB graduates from our set of covariates in our main specifications.

Table 16 – "Entry" in policy zoning and pupils' enrollment - Controlling for time-changes in school quality

	Probability to enroll at:		
	District public	Other public	Private
T^{entry}	-0.034**	0.039***	-0.004
	(0.013)	(0.014)	(0.008)
SES (ref.=Medium)			
Very high SES	-0.064***	-0.015**	0.080***
	(0.007)	(0.006)	(0.007)
High SES	-0.013**	-0.008*	0.020***
	(0.006)	(0.004)	(0.006)
Low SES	0.097***	0.023***	-0.119***
	(0.006)	(0.005)	(0.006)
Unknown	0.078***	0.024*	-0.101***
	(0.019)	(0.014)	(0.013)
Male pupil	-0.011***	0.005**	0.007***
	(0.002)	(0.002)	(0.002)
French pupil	-0.062***	-0.013	0.075***
	(0.011)	(0.011)	(0.009)
Pupil's age	0.015***	0.024***	-0.039***
	(0.004)	(0.003)	(0.004)
District school in comp. educ. prog.	0.017	-0.009	-0.008
	(0.013)	(0.010)	(0.012)
No. of private schools within 5km	0.062***	-0.077***	0.016**
	(0.014)	(0.013)	(0.007)
Past av. DNB rate in district school	0.077**	-0.051	-0.026
	(0.038)	(0.032)	(0.031)
R^2	0.166	0.125	0.188
No. obs	304,151	304,151	304,151
No. clusters	235	235	235
Year FE	✓	✓	✓
School FE	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. For the sake of clarity, we do not report the constant.

To tackle this issue, we build on geo-coded data allowing us to recover school catchment areas on the basis of the shortest distance between each pupil's home address and the set of all opened public middle schools.[30] Unfortunately, due to data limitations, we cannot replicate our previous analysis, since these geo-coded data are available only every even year from 2011 to 2017, and preclude from computing the same exact covariates as with our yearly-based data. Nonetheless, as shown by Table 18, the results drawn from these data sets are qualitatively similar to what we find based on pupils' primary schools. We still do find that

[30] Absent a digitized contour of school districts, Maugis and Touahir (2018) show that this assignment procedure yields a fairly good approximation of pupils' catchment areas.

Table 17 – "Exit" of policy zoning and pupils' enrollment - Controlling for time-changes in school quality

	Probability to enroll at:		
	District public	Other public	Private
T^{exit}	0.010	-0.008	-0.002
	(0.006)	(0.006)	(0.004)
SES (ref.=Medium)			
Very high SES	-0.110***	-0.007	0.118***
	(0.006)	(0.005)	(0.006)
High SES	-0.034***	0.005	0.028***
	(0.005)	(0.004)	(0.004)
Low SES	0.124***	0.004	-0.127***
	(0.005)	(0.005)	(0.005)
Unknown	0.086***	0.026***	-0.112***
	(0.010)	(0.008)	(0.007)
Male pupil	-0.010***	0.007***	0.003**
	(0.001)	(0.001)	(0.001)
French pupil	-0.066***	0.008	0.057***
	(0.007)	(0.006)	(0.004)
Pupil's age	0.032***	0.021***	-0.053***
	(0.002)	(0.003)	(0.002)
District school in comp. educ. prog.	0.011*	-0.009	-0.002
	(0.006)	(0.007)	(0.005)
No. of private schools within 5km	0.031***	-0.029***	-0.002
	(0.006)	(0.006)	(0.004)
Past av. DNB rate in district school	0.046**	-0.019	-0.027**
	(0.020)	(0.020)	(0.011)
R^2	0.170	0.116	0.210
No. obs	747,430	747,430	747,430
No. clusters	608	608	608
Year FE	✓	✓	✓
School FE	✓	✓	✓
Group trends	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. For the sake of clarity, we do not report the constant.

policy designation reduced significantly the probability for parents to choose their district schools, relatively to other public schools, with a stigma magnitude slightly lower than with our yearly data (-2.6 pp instead of -3.5), though the two coefficients are not significantly different. We also find that parents shifted mostly to other public schools, but to a less extent than with our yearly-data (-2 pp instead of -4.1), rather than to private schools, though the coefficient reported in column (3) is also positive (+0.6 pp). As for disqualified neighborhoods, the coefficients sign the same as with our yearly-data, but estimates are insignificant.

To further test whether assigning district schools to pupils on the basis of distance to their

Table 18 – Rezoning and pupils' enrollment - Geo-coded data

	Probability to enroll at:		
	District public	Other public	Private
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}	0.002	-0.002	0.001
	(0.005)	(0.004)	(0.003)
R^2	0.129	0.081	0.149
No. obs	911,718	911,718	911,718
No. clusters	1,419	1,419	1,419
Pupil's characteristics	✓	✓	✓
Time-varying controls	✓	✓	✓
Year FE	✓	✓	✓
School FE	✓	✓	✓

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. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, scholarship and citizenship. Time-varying controls include a dummy indicating whether the district school benefits 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 coefficients associated with these controls are not reported

primary school could be an issue, we also check whether some public schools end up with zero enrollment after our assignation process. This concerns 72 public middle schools over a total of 5,125, among which only 2 schools in the "entry" sample, and 12 schools in the "exit" sample. When we exclude those schools, for which we presumably wrongly define the catchment area, results are barely unaffected: our point estimates stay strictly identical (to two or three decimal places), and significance levels are left unchanged.[31]

6 Heterogeneous Treatment Effects

In this section, we analyze the extent to which the reform had a heterogeneous impact on school choices across various dimensions. First, we check whether parental reactions differ by family background. We also study potential heterogeneity across pupils of different genders, school district characteristics likewise compensatory education programs or private schooling options likely to compete with the district school.

6.1 Heterogeneity across Socioeconomic Statuses

We first investigate whether responses to rezoning differ among families from different socioeconomic backgrounds, since high SES parents or parents with particular occupations (like

[31] Results are available upon request.

teachers) may have better access to information and school quality. To do so, we first re-aggregate parental occupations into three broad categories: High, medium and low SES.[32] We then estimate the most conservative augmented versions of equations (2) and (3) in a triple-difference set up, whereby all our explanatory variables are interacted with these categories.

The top panel of Table 19 provides evidence that zoning entry caused a "rich flight" to the private sector, since high SES parents (the reference in Table 19) experienced an increase in their probability to choose a private school by 3.6 pp relative to parents living in counterfactual unlabeled neighborhoods. While there is no sign of heterogeneous reactions across parents of medium and high SES, we do find evidence of heterogeneous reactions across parents of low and high SES. The top panel of Table 19 shows that the probability to shift to another public (respectively private) school after policy designation is 4.8 pp (resp. 3.5 pp) larger (resp. lower) for low SES parents than for high SES families. The bottom panel of Table 19 also shows that, after de-zoning, the probability to choose the district school is relatively larger (by 2.1 pp) for low SES parents than for high SES parents, which suggests that the former might have re-adjusted their beliefs on school quality after the reform, contrary to the latter.

Interestingly, Table 20, that isolates the impact of the reform on teachers and professors, reveals that they have reacted significantly less to policy designation than all other parents (see top panel of Table 20). These two specific occupations are plausibly much more aware of the intrinsic quality of schools. As such, it is coherent that they appear less sensitive to any new (bad or good) signal conveyed by neighborhood labeling.

6.2 Heterogeneity across other dimensions

We also investigate whether parents reacted differently across various other dimensions, such as pupils' characteristics (citizenship or gender), or school district environments (presence of compensatory education subsidies or distance to the closest private school).

Table 21 disentangles the school choices of parents on the basis of pupil citizenship, as foreign residents may have difficulties to understand French and thereby, may not have sufficient knowledge of the French institutional context to manage to bypass their default district school, or to adapt quickly to the informational changes conveyed by the reform. No clear sign of heterogeneity prevails among pupils of different citizenship: zoning entry does not affect French pupils' enrollment differently from foreign pupils' enrollment. As for zoning exit, even though it seems to have reduced the likeliness that the parents of foreign pupils opt for a private school, this was not the case for French pupils, who rather adjusted their public opt-out strategies.

[32] We aggregate very high and high SES, as well as low and unknown SES, as section 4 showed displayed similar school choice patterns and point estimates for these categories.

Table 19 – Re-zoning and pupils' enrollment by SES

	Probability to enroll at:		
	District public	Other public	Private
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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 20 – Re-zoning and the enrollment of teachers' children

	Probability to enroll at:		
	District public	Other public	Private
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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 21 – Re-zoning and pupils' enrollment by citizenship

	Probability to enroll at:		
	District public	Other public	Private
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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Tables 26, 27 and 28 provided in Appendix D show that we do not find heterogeneous responses across other dimensions such as gender (Table 26), compensatory education programs (Table 27) or distance to the closest private school (Table 28).

7 Conclusion

Even though place-based policies channel large transfers toward low-income neighborhoods, the degree to which they provide disadvantaged residents with more opportunities is still a matter of debate. Urban policy resources can improve school enrollment in low-income neighborhoods if parents expect positive feedback loops on their children's education. But they can as well stigmatize schools and alter their reputation if policy-designation conveys a negative image on the targeted neighborhoods. This paper estimates the net effect of these two opposite forces on school enrollment in France over the period 2010-2019.

In evaluating place-based policies, the main challenge to overcome is selection into treatment, as neighborhoods qualify for public subsidies because they are particularly deprived. Therefore, a naive comparison between labeled and unlabeled neighborhoods is likely to underestimate the policy effectiveness. To overcome this challenge, this paper leverages the quasi-natural experiment provided by a policy reform that redrew the boundary of urban neighborhoods eligible for place-based subsidies in 2014, 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 the French urban policy on school choices. We focus in particular on school enrollment into lower secondary education, a key decision for parents in France that conditions pupils' education paths over a 4-year time span.

We show that public middle schools located in neighborhoods labeled to benefit from the urban policy scheme witnessed a significant reduction in their school attendance following the reform. This "zone-and-shame" effect is triggered by responses from parents of all socioeconomic backgrounds, who switched to public schools located in unlabeled neighborhoods. We also find partial evidence of a 'rich flight' to the private sector. Discrimination against public schools located in disparaged neighborhoods is large and immediate, but school stigma is no longer detected after two years. Symmetrically, district schools located in disqualified neighborhoods regained pupils' attendance, but from disadvantaged families only. In our preferred specification, we find that neighborhood labeling decreases by approximately 3.5 pp the individual probability that a kid would be enrolled in a district school located in a labeled neighborhood. This penalty, which is equivalent to a loss of 5 pupils per school located in disparaged neighborhoods is not offset by the 1 pp increase associated with labeling loss, as the regained attendance amounts to less than 2 pupils by cohort. Our main estimates are robust to a number of different specifications, various placebo checks and alternative treatment definitions.

These results suggest that place-based urban policies relying on zoning systems may stigmatize public schools located in targeted neighborhoods, and further accentuate social segre-

gation through family extra-sorting. Even though the policy stigma disappears quickly after the informational shock provided by neighborhood labeling, school composition effects are expected to last, as most pupils do not change middle school throughout lower secondary education in France. Besides, this composition effect may be hard to rectify as only low-SES families seem to re-adjust their beliefs on school quality after de-zoning.

Obviously, our results hold in the French context, but such territorial stigma may very likely appear in any other country where place-based policies rely on a zoning system, which provides external validity to our study. It remains to analyze to what extent policy benefits later on could offset this penalty, as the segregation triggered by the policy might change the social mix at school and yield more homogeneous groups of pupils, with more teaching resources at their disposal. Unfortunately, we cannot provide such a cost-benefit analysis, because the confidential data provided to us so far do not allow to follow pupils (and their test scores) over time and hence, to test whether extra-resources associated to urban policy helped them perform better throughout their education curriculum post-reform. We hope to overcome this limit in a companion paper, if those data sets can be disclosed for research purposes.

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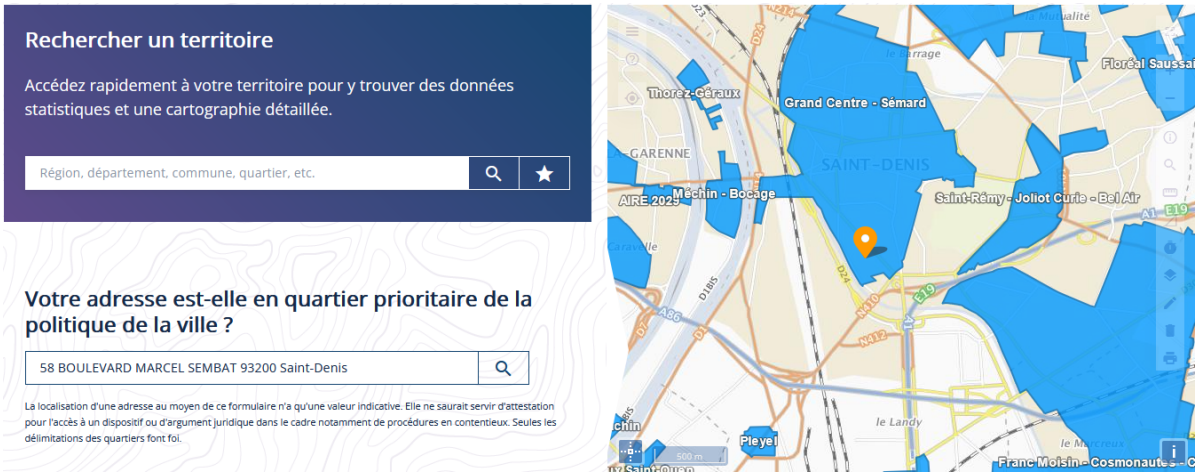
A Internet information on the urban policy coverage

Figure 9 – Internet information on the urban policy coverage

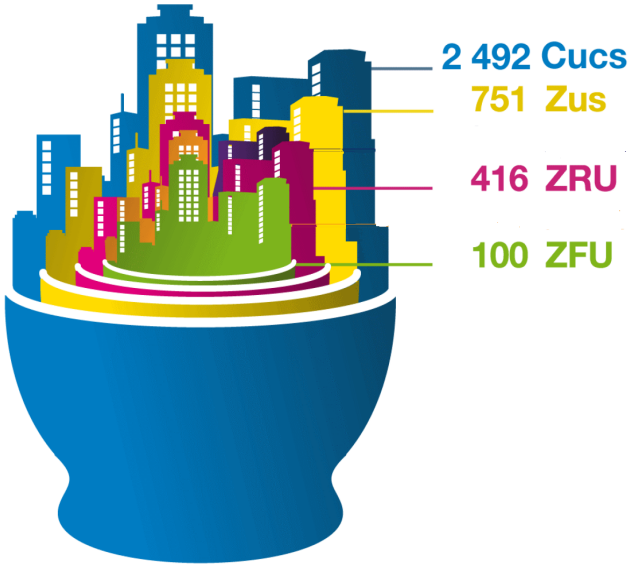


Source: French Ministry of Urban Affairs (https://sig.ville.gouv.fr/).

Note: The address refers to the Collège Pierre De Geyter, which is located in a QP of Seine-Saint-Denis (delineated in blue).

B French urban zoning systems before the 2014 reform

Figure 10 – French (or Russian?) urban zoning systems before 2014



Source: CGET

Note:

ZUS = *Zones urbaines sensibles* = Urban Sensitive Zones

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

ZFU = *Zones franches urbaines* = Enterprise Zones

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

C Narrow definition of neighborhood treatment

Table 22 – "Entry" in policy zoning and pupils' enrollment - Changes over time and narrow definition of neighborhood treatment

	Probability to enroll at:			
	District public	Other public		Private
		In zoning	Out zoning	
$T^{entry-treatment2014}$	-0.024***	0.009**	0.017**	-0.002
	(0.008)	(0.003)	(0.007)	(0.006)
$T^{entry-treatment2015}$	0.001	0.000	-0.005	0.004
	(0.008)	(0.004)	(0.008)	(0.006)
$T^{entry-treatment2016}$	-0.003	0.001	0.007	-0.004
	(0.008)	(0.004)	(0.009)	(0.006)
$T^{entry-treatment2017}$	0.008	-0.001	-0.005	-0.001
	(0.008)	(0.004)	(0.009)	(0.006)
$T^{entry-treatment2018}$	0.001	-0.001	0.010	-0.010
	(0.010)	(0.006)	(0.012)	(0.006)
$T^{entry-treatment2019}$	-0.004	-0.007	0.005	0.006
	(0.008)	(0.004)	(0.007)	(0.006)
R^2	0.170	0.157	0.124	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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 23 – "Entry" in policy zoning and pupils' enrollment - Falsification test and narrow definition of neighborhood treatment

	Probability to enroll at:			
	District public	Other public	Private	
$T^{entry-treatment2011}$	-0.002	0.001	0.001	
	(0.010)	(0.009)	(0.007)	
$T^{entry-treatment2012}$	-0.004	0.009	-0.005	
	(0.009)	(0.009)	(0.007)	
$T^{entry-treatment2013}$	0.005	0.002	-0.007	
	(0.009)	(0.008)	(0.007)	
$T^{entry-treatment2014}$	-0.023***	0.020**	0.003	
R^2	0.170	0.126	0.192	
No. obs	449,998	449,998	449,998	
No. clusters	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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 24 – "Exit" of policy zoning and pupils' enrollment - Changes over time and narrow definition of neighborhood treatment

	Probability to enroll at:			
	District public	Other public		Private
		In zoning	Out zoning	
$T^{exit-treatment2014}$	0.008	-0.001	-0.008	0.001
	(0.008)	(0.006)	(0.006)	(0.006)
$T^{exit-treatment2015}$	-0.003	-0.003	0.011*	-0.005
	(0.007)	(0.005)	(0.006)	(0.005)
$T^{exit-treatment2016}$	-0.014**	0.004	0.007	0.004
	(0.007)	(0.004)	(0.006)	(0.005)
$T^{exit-treatment2017}$	0.006	-0.001	0.001	-0.005
	(0.008)	(0.005)	(0.008)	(0.005)
$T^{exit-treatment2018}$	-0.012*	0.004	0.004	0.005
	(0.007)	(0.006)	(0.007)	(0.005)
$T^{exit-treatment2019}$	0.003	-0.001	0.010*	-0.013**
	(0.007)	(0.005)	(0.006)	(0.005)
R^2	0.178	0.191	0.126	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	✓	✓	✓	✓
Group-trends	✓	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 25 – "Exit" of policy zoning and pupils' enrollment - Placebo test and narrow definition of neighborhood treatment

	Probability to enroll at:		
	District public	Other public	Private
$T^{exit-treatment2011}$	-0.004	0.003	0.001
	(0.009)	(0.008)	(0.006)
$T^{exit-treatment2012}$	0.017**	-0.021***	0.004
	(0.008)	(0.008)	(0.006)
$T^{exit-treatment2013}$	-0.005	0.004	0.001
	(0.008)	(0.008)	(0.005)
$T^{exit-treatment2014}$	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	✓	✓	✓

Sources: Base centrale scolarité (BCS) - 2010-2019, DEPP - Ministère de l'Éducation, ADISP; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at the school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

D Other heterogeneity dimensions

Table 26 – Re-zoning and pupils' enrollment by gender

	Probability to enroll at:		
	District public	Other public	Private
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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 27 – Re-zoning and pupils' enrollment by school type

	Probability to enroll at:		
	District public	Other public	Private
T^{entry}	-0.027**	0.017	0.010
	(0.012)	(0.015)	(0.013)
Comp. educ. prog. $\times$ 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)
Comp. educ. prog. $\times$ 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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.

Table 28 – Re-zoning and pupils' enrollment across distance to closest private school

	Probability to enroll at:		
	District public	Other public	Private
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; Urban policy zoning (ANCT-CGET); Local income data (Insee).

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis are clustered at school level. Pupils' characteristics include socioeconomic background, gender, age and citizenship. Time-varying controls include a dummy indicating whether the district 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 coefficients associated with these controls are not reported.