



## Residence-based priorities and school choice<sup>☆</sup>

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### ABSTRACT

Many countries use centralized school choice procedures to assign pupils to schools. To address excess demand for a particular school, ties are broken according to priority points granted based on various criteria, such as proximity to the school. Using a unique reform undertaken in Madrid (Spain), we estimate the impact of abolishing residence-based priorities on families' school choices, the stated motivation for choosing a school, and the final school allocation. Utilizing several administrative datasets on school applications, we find that the reform increases families' out-of-district school assignments and assignments to schools further away from their home address. Parents of immigrant children did not change their application behavior in the first years of the reform but caught up with natives three years after its implementation. Children generally accessed slightly better-performing schools, particularly those from lower-educated backgrounds.

### 1. Introduction

In most educational systems, students are assigned to public schools (at least partially) based on their place of residence.<sup>1</sup> Over the past decades, two-thirds of OECD countries have implemented reforms to provide more school options to families by weakening residence-based assignment (Musset, 2012; OECD, 2019a). Between 2000 and 2015, the average share of OECD students whose school admission process was based on their home address decreased by six percentage points (OECD, 2019b); in several countries such as the US, Sweden,

and Japan, this share decreased by approximately 20 percentage points. In the academic literature and policy debate over school choice, one major concern is whether increasing the number of choices may have distributional impacts. While increasing school choice could allow families who live in deprived neighborhoods to opt out of their assigned school, it could backfire if nonnative and low-income families are less able to exercise choice.<sup>2</sup> In this paper, we investigate the impact of a unique reform abolishing residence-based priorities to choose a school on families' choices, the stated motivation for choosing a school, school assignment, and its distributional effects.

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<sup>1</sup> Urquiola (2005) highlights that "interdistrict or Tiebout choice is, and is likely to remain, the main form of school choice" in the US.

<sup>2</sup> Burgess, Greaves, Vignoles, and Wilson (2015) show a significant socioeconomic gradient in the attributes of accessible schools. Other strands of the literature analyze the relationship between school choice and school stratification (Billings, Deming, & Rockoff, 2014; Böhlmark, Holmlund, & Lindahl, 2016; Epplé, Romano, & Urquiola, 2017; Figlio, Hart, & Metzger, 2010; Lucas & Mbiti, 2012; MacLeod & Urquiola, 2015), school choice and attainment (Abdulkadiroğlu, Angrist, Dynarski, Kane, & Pathak, 2011; Cullen, Jacob, & Levitt, 2006; Deming, Hastings, Kane, & Staiger, 2014; Dobbie & Fryer, 2011; Rouse, 1998), characteristics of families who opt out of their assigned school (Hastings, Kane, & Staiger, 2005; Levin, 1998), and "tipping points" for schools (Böhlmark & Willén, 2020; Card, Mas, & Rothstein, 2008).

In a centralized school choice procedure, parents submit a list with their ranking of the available schools. If a particular school is overdemanded, ties are broken according to priority points granted based on various criteria, including proximity to the school (e.g., living near the school).<sup>3</sup> The context of this study is the primary school system in the region of Madrid (Spain), which uses a centralized school choice procedure based on a widely used student assignment mechanism, the so-called Boston mechanism (henceforth, BM).<sup>4</sup> We use comprehensive administrative data on the universe of applicants to the public primary school system in the region of Madrid over the period 2010–2016, along with detailed data on school supply, household socioeconomic characteristics, standardized test scores, and parental questionnaire. This dataset allows us to geolocalize the precise household residence address of the universe of applicants to the public school system.

We exploit a unique large-scale school choice reform to estimate the impact of abolishing residence-based priority points on families' choices and school allocation. Madrid has 21 school districts, with approximately 25 schools per district. Before 2013, in the case of school overdemand, Madrid placed a high priority on assigning students whose primary residence was located in the same school district, in a way that made it very difficult for a student to get assigned to a school outside their district. In 2013, this residence-based priority in assigning pupils to overdemanded schools was virtually abolished, and the previous 21 school districts were merged into a single school district zone. The reform implied integrating a large-scale educational market, in which families increased their feasible choice set from approximately 25 to 500 schools.

We use two complementary empirical approaches to identify the effect of this reform. First, we use an event-study approach to identify its impact on families' choices by comparing school choices made before with those undertaken immediately after the modification. Second, we use a difference-in-differences design, comparing parents who are closer to the school's district boundary (and presumably more impacted by the reform) to those farther away (and plausibly less sensitive to changes in school boundaries).

Our first empirical finding is that families have modified their school choices since the reform, leading to a three-percentage-point or 30% increase in the fraction of out-of-school district assignments and a 259-m or 22% increase in distance to the assigned school from residential addresses. Although families in higher quintiles of parental education have reacted more to the reform in absolute terms, the relative effects are similar across parental education quintiles.

We uncover strong heterogeneous effects by immigrant status. Families with Spanish-born children have been entirely responsible for the increase in out-of-district assignments, with no reaction from parents of immigrant children in the first two years of the reform. This result suggests an information gap since immigrant families started to react to the reform to a similar extent as native families three years after its implementation.

After the reform, most families accessed slightly better-performing schools, but children from the lowest educational quintile benefited the most. The average performance of the assigned school increased

<sup>3</sup> These school choice priorities in secondary education typically differ from those in preschool or primary education. Some countries, such as Sweden, or cities, such as Paris, incorporate student grade point average among the priority points granted in secondary education, while those in primary education tend to emphasize more sociodemographic indicators.

<sup>4</sup> This school choice allocation mechanism has been widely used in US school districts such as Boston, Cambridge, Denver, Minneapolis, and Seattle, as well as in other cities such as Beijing, Amsterdam, Denmark and Frankfurt. Evidence has found that the BM does not incentivize families to report their true preferences (Abdulkadiroglu & Sönmez, 2003). See (Agarwal & Somaini, 2018) for a description of the school choice allocation mechanisms used in different cities worldwide.

by 2.5% for students whose parents were in the lowest education quintile and by 0.1% for students whose parents were in the top quintile for parental education. We find evidence that parents from the most-educated families have decreased their applications to the top in-district schools to apply for higher-performing schools in different school districts after the reform; however, this change in the application behavior has not been directly translated into admission to better-performing schools. Even though parents in the lowest educational quintile have not been found to apply more often to the top-performing in-district school as a result of the reform, they have increased their applications to in-district schools that have an above-the-median-average performance, thereby increasing their final admission to better-performing schools.

To complement these results, we also show substantial differences in the families' stated reasons for choosing a school. The reform has increased the fraction of families choosing a school based on its characteristics yet reduced the fraction of families reporting that their child was assigned to a school they did not choose. However, this effect is only present for families with Spanish-born children. In contrast, parents of immigrant children have been unaffected, suggesting that natives may be more knowledgeable about the assignment system and involved in the school choice process. In addition, the reform was implemented in several municipalities (mainly those of medium size) in 2012 and in most larger municipalities in 2013. We exploit this phasing-in of the reform to analyze the effects on the other municipalities as a robustness check.

Our paper is most closely related to the stream of literature focusing on the effects of school choice reforms and families' behavioral responses. Few papers identify families' behavioral reactions regarding changes in school catchment areas. Calsamiglia and Güell (2018) exploit a change in the definition of residence-based priorities in Barcelona, where the set of schools to which families have high priority was reduced, showing that families readjust their school choices by excluding all schools at which they do not have high priority. This study shows evidence of high-income, seemingly unsophisticated parents who do not enroll in the assigned school but instead enroll in an outside school option. Bjerre-Nielsen and Gandil (2020) find that redrawing school attendance boundaries leads households to defy reassignments to schools with lower socioeconomic status, with a significant social gradient in defiance in Denmark.<sup>5</sup>

Our paper makes several contributions to the school choice literature. First, we use a sharp increase in the set of schools that families can choose from to learn about the effects of priorities on school choice. We provide evidence on the importance of residence-based priorities to determine school choice, the main reason for choosing a school, and school assignment. Second, we highlight that changes in school choice priorities can have important distributional impacts.

The remainder of the paper is organized as follows. Section 2 describes and contextualizes the school choice reform undertaken in Madrid. The data and summary statistics are detailed in Section 3. The empirical strategy is discussed in Section 4. The results are presented in Section 5. Section 6 discusses potential identification challenges, displays the results of the DID strategy, and exploits the phasing-in of the reform to analyze the effects on municipalities other than Madrid as a robustness check. Finally, Section 7 concludes the paper.

<sup>5</sup> Söderström and Uusitalo (2010) focus on an admission reform that changed admission criteria to be solely based on grades for access to upper secondary schools in Sweden, finding that segregation by ability has increased. Burgess and Briggs (2010) find that children from low-income families are less likely to secure places in good schools and that probability is unaffected by the degree of school choice. Kessel and Olme (2018) adopt a more structural approach to study how the design of priority structures for assigning pupils to schools impacts segregation in primary schools in a Swedish municipality.

## 2. Institutional background

**The Spanish Education System.** The Spanish education system consists of 10 years of compulsory education, which starts at age 6, including six years of primary school (up to age 12) and four years of lower secondary education (up to age 16). Although compulsory primary education starts at age 6, students are offered free universal access to the public education system from age 3 onward. Since most publicly funded schools offer preschool and primary education together, age 3 is typically the time when families enroll their children in primary school.<sup>6</sup>

Educational policies are jointly determined at the national, regional and municipal levels. The central government is responsible for establishing the organic laws, and the royal decrees that the regional governments are allowed to further develop as long as they do not contradict the organic laws.<sup>7</sup>

In Spain, the vast majority of schools are publicly funded. The publicly funded school system includes both public and semipublic schools. The government fully funds public schools, which are managed by civil servants and local school boards. Semipublic schools (*centros concertados*) are privately run but mostly financed through public funds. Concerning admissions, all schools in the public system (both public and semipublic) are expected to unconditionally accept all students assigned by the centralized school choice mechanism, provided that demand does not exceed supply.

**School Choice in Madrid.** In the region of Madrid, the majority of schools (approximately 85%) are part of the publicly funded system of schools. This system includes publicly managed schools (which enroll approximately 50% of all students) and semipublic schools (which cover approximately 35% of all students).

The school choice system is based on a centralized assignment mechanism that is used to allocate students to schools in the publicly funded system for preschool (starting at age 3), primary (age 6), lower secondary (age 12) and special education. More than 96 percent of the students in the region of Madrid attend preschool, and school choice decisions are made when the children are three years old (Anghel, Cabrales, & Carro, 2016). Students who are enrolled in preschool in a given school have full priority over every student applying to primary education (age 6). If all vacancies are filled at the age of 3 years and no student leaves the school, there would be no available slots for those who were not previously enrolled in that level in that cohort. As a result, changes in school after the age of 3 are not frequent, and the vast majority of families make their schooling decisions at this moment in their children's lives.

Before the school year starts in September (between the end of April and early May), every participating family is asked to submit a rank-ordered list of schools up to a total of fourteen. Applicants are allocated by a centralized and algorithm-based automatic allocation procedure so-called as the Boston mechanism (hereafter, BM) (Abdulkadiroglu & Sönmez, 2003), which works as follows. First, students are allocated to

their top-ranked school. For schools that are overdemanded, students are granted priority points (according to several criteria that depend on student characteristics and location of the household or parental job), providing them each with a rank number that assigns places to students until all available places are filled. Ties are broken conditional on priority bonus points obtained (see Table 1 and Table A 1). In the second step, students who are rejected from their first choice are proposed to their second-choice school in their rank-ordered list if there are seats available after the first step. If there are more applicants than available places, students are allocated in the same way as that described in the first step, and the mechanism continues until all students are either assigned a seat or rejected from all schools in their rank-ordered list. The final assignments are made public in June, and enrollment must take place at the end of June (for preprimary and primary education) or July to September (for lower secondary education).<sup>8</sup>

Until recently, the BM has been very influential in practice. US school districts that have used this mechanism include Boston, Cambridge, Denver, Minneapolis, and Seattle, among others, and cities such as Beijing, Amsterdam, Denmark, and Frankfurt have also implemented this approach. One of the special features of this assignment system is that the choice of the top-ranked school is highly important since the assignment in each round is final. Empirical evidence shows that under the BM, most assignments are resolved in the first round. In Madrid, approximately 88 percent of students are assigned to their top-ranked school (see Section 3), and this empirical fact is similar across different cities worldwide.<sup>9</sup>

**Reform of the priority criteria for school access in the city of Madrid.** In the case of overdemand at a specific school, students are assigned based on government-determined priority criterion, which grants points to students according to their characteristics and their residence. For school choice, the region of Madrid contains 179 municipalities, with the medium and large municipalities being subsequently divided into school choice catchment areas (*zonas de influencia*). In particular, the city of Madrid, the largest municipality and our central unit of analysis, is divided into 21 school districts that coincide with the catchment areas (Fig. 1).

Table 1 shows the score scale used in the city of Madrid over the period 2010–2016. Before the 2012/2013 school year, children living in (within the boundary of) the district of their top-ranked school received 4 points (2 points). Families also received additional points due to individual household characteristics such as family income, whether a family member had a disability or a sibling being enrolled in a top-ranked school. In addition, a specific point could be granted at the discretion of school principals, which must be based on objective criteria that were made public.

<sup>8</sup> A special feature of the system is that students' priority points that are used for tie-breaking at all stages are based on those obtained for the top-ranked school. Calsamiglia (2014) states that the main reason that the government uses this procedure is that it is computationally easier. Online Appendix Section H provides further details on the theoretical properties of the BM assignment mechanism.

<sup>9</sup> Abdulkadiroglu and Sönmez (2003) highlight that one of the major difficulties of the BM is the fact that it is not strategy-proof. A student may have a very high priority to enter school *s*, but if she does not list it as her top-ranked school, she loses her seat in favor of students who have listed *s* as their top choice. The BM provides incentives for families to misreport their preferences by ranking first those schools in which they have higher priorities to be admitted. See also (Abdulkadiroglu, Pathak, Roth, & Sonmez, 2006) for Boston; (Hastings, Kane, & Staiger, 2009) for Charlotte; (Calsamiglia & Güell, 2018) for Barcelona; (Lavy, 2010) for Tel Aviv; (De Haan, Gautier, Oosterbeek, & Van der Klaauw, 2015) for Amsterdam; (Lai, Sadoulet, and de Janvry (2009) and He (2017) for Beijing; and Hwang (2017) for Seoul. Another essential feature of the BM is that this mechanism tends to promote socioeconomic segregation across public schools (Calsamiglia, Martínez-Mora, & Miralles, 2021).

<sup>6</sup> Preschool education is entirely publicly funded from ages 3 to 6. This right is recognized in the OrganicLaw1/1990(LOGSE).

<sup>7</sup> The 1978 Spanish Constitution grants the right to education and the freedom to educate children. In terms of education policy, the second principle was translated into the 1985 education act (LODE), which explicitly regulated the degree of freedom that families have with regard to choosing their children's school (see OrganicLaw8/1985,LODE). In the following years, this was accompanied by a decentralization process (see the OrganicLaw9/1992: *Ley Orgánica de transferencia de competencias a Comunidades Autónomas que accedieron a la autonomía por la vía del artículo 143 de la Constitución*). Concerning the Spanish school choice policies in the years surrounding the reform, the national organic law in place at the time of the reform (LOE) established the general regulatory principles to be followed by the regional governments to determine the priority criteria of students in overdemanded schools (see OrganicLaw2/2006,LOE).

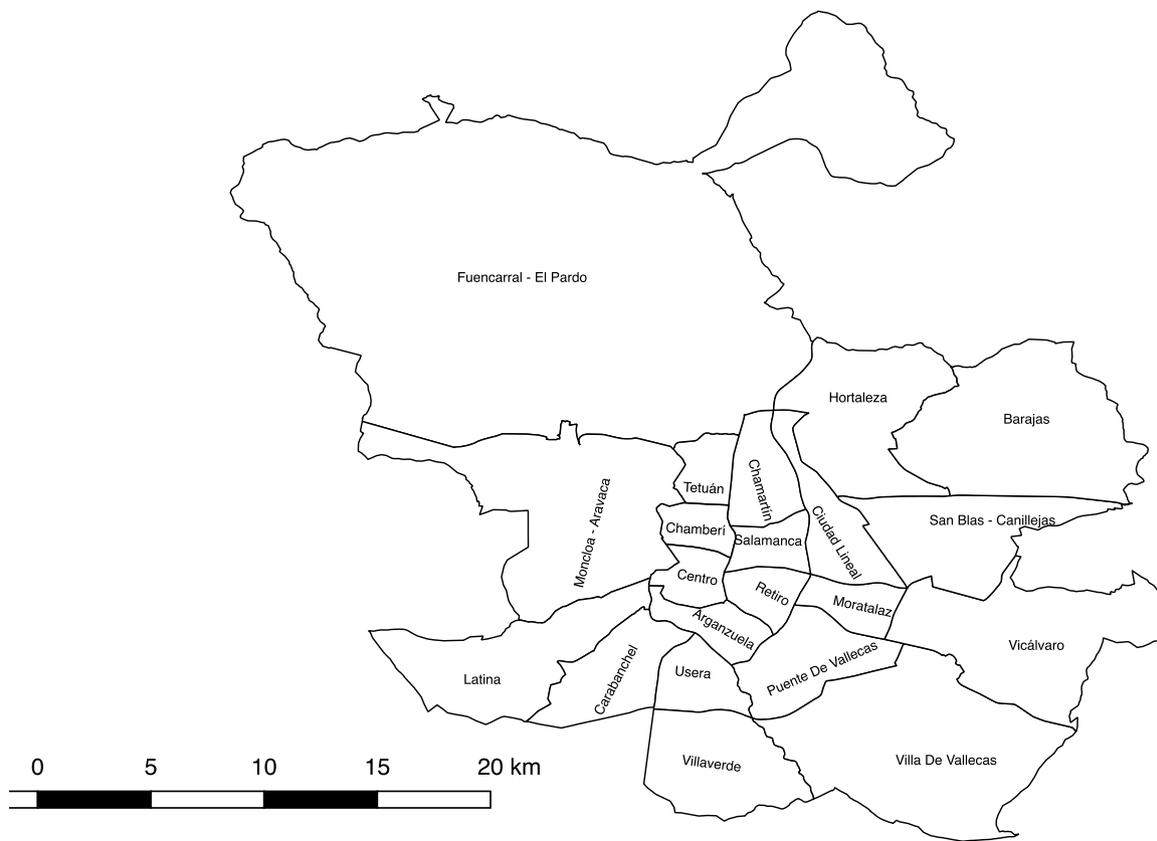


Fig. 1. School Districts in the City of Madrid. Notes: Own computation using shapefile data from the 21 school districts of the city of Madrid and Census Office data of Madrid.

**Table 1**  
Priority points in case of overdemand of schools in the city of Madrid.

| Bonus                    | Criteria   | Number of points |           |           |
|--------------------------|--|------------------|-----------|-----------|
|                          |  | Before 2012/2013 | 2012/2013 | 2013/2014 |
| Proximity to Madrid city | Family house or parents' work located in:<br>School district   | 4                | 4         |           |
|                          | Boundary school district                                       | 2                | 2         |           |
| Low income               | Family house or parents' work located in:<br>Same municipality |                  |           | 4         |
|                          | School district  |                  |           | 0.5       |
|                          | Region of Madrid   |                  |           | 2         |
| Siblings                 | Income <= IPREM  | 2                |           |           |
|                          | IPREM < Income <= 2IPREM                                       | 1                |           |           |
|                          | Minimum Insertion Subsidy                                      |                  | 2         | 2         |
| Disability               | First one 4 pts, and each additional 3 pts                     | 4                |           |           |
|                          | One or more  |                  | 8         | 10        |
| Large Family             | Parents, students or siblings                                  | 1.5              | 1.5       | 1.5       |
|                          | General  | 1.5              | 1.5       | 1.5       |
| Alumni family member     | Special  | 2.5              | 2.5       | 2.5       |
|                          | Family member is alumni student                                |                  | 1.5       | 1.5       |
| School discretionary     |  | 1                | 1         | 1         |

Notes: The changes beyond the proximity criteria were applied together across all medium and large municipalities. IPREM is the acronym in Spanish for the Multiple Effects Income Public Index, which was €7,455.14 during the period of study. The Minimum Insertion Subsidy (*Renta Mínima de Inscricin*) is a special provision granted for people with income lower than the IPREM. The number of recipients of this subsidy is much smaller (approximately 30,000 households in a region of more than 6 million population) than the number of families with a per capita household income under 100% of the IPREM (approximately 15% of the population). Families receive the same number of points if any parent/guardian works in the district of the top-ranked school. The school discretionary point is a point that schools have the freedom to assign based on “public and objective” criteria. Table 1 also applies to the rest of the municipalities regarding the individual characteristics.

In March 2012, the regional government announced a reform intended to strengthen the principle of school choice for households with children entering preprimary, primary and lower secondary schools.<sup>10</sup> The main goal of this government policy was to enhance free school choice. The reform modified the computation of pupils' priority points.

The changes in priority points and school districts were implemented in two consecutive years. In 2012/2013, the criteria to obtain bonus points granted to low-income families changed, and a new priority bonus was granted if a pupil's family member was an alumni student of their top-ranked school.<sup>11</sup> In 2013/2014, the proximity to the school criterion was relaxed, introducing an almost *de facto* single school choice zone for all the municipalities in the region. The weight of attending a school within the same district as the household residence declined from 4 to 0.5 points after the reform, representing an 87% reduction in the weight of the residence-based school priority. This reform implied moving from 21 school districts (with approximately 25 schools per district) to a virtually unified municipal school choice district with more than 500 schools in the city of Madrid.<sup>12</sup> **Implementation of the reform in other municipalities.** In the region of Madrid, the number of prereform school catchment areas was a function of the size of the municipality. The smallest 142 municipalities (with a population of less than 10,000/15,000 inhabitants) have always had a unique school catchment area, whereas larger municipalities had more than one. Due to capacity constraints, the abatement of residence-based priorities in medium-sized and large municipalities was conducted in two consecutive years. In 2012/2013, 22 municipalities, mostly of medium size (with populations between approximately 15,000 and 100,000 inhabitants), adopted the reform. In 2013/2014, the remaining 15 municipalities (mostly the larger ones, including the city of Madrid) adopted it.<sup>13</sup> We use this gradual implementation across municipalities to test the reaction of medium-size municipalities that implemented the single school choice district one year before the large-size cities as a robustness check (see Section 6).

### 3. Data and summary statistics

#### 3.1. Data

We use a combination of five administrative datasets that provide information on the universe of preschool and primary school applications of each household in the region of Madrid, the characteristics of the publicly funded schools in the region of Madrid, the education level of households at the census block level in the city of Madrid, standardized test scores at the school level, and a parental questionnaire regarding stated motivations to choose school. Data on student applications, schools, school test scores, and parental questionnaires are provided by the Education Ministry (*Consejería de Educación*) of the regional government of Madrid, and data on parental education were obtained from the Madrid Census. Information is available for every year from 2010/11 (2010 hereafter) to 2016/2017 (2016 hereafter).

<sup>10</sup> Order 2939/2012 of March 9 of the Regional Government of Madrid.

<sup>11</sup> Additionally, more points were granted to families with siblings enrolled in the school. Following (Calsamiglia & Güell, 2018), we consider this change irrelevant to the analysis, given that families' choice is previously conditioned by their choices for their older children. The tie-breaking criteria were also slightly modified, as seen in Table A 1 of the online Appendix. We provide several validity checks to show that the 2012 reform had little effect on our outcome variables (see the online Appendix, Section K).

<sup>12</sup> The regional government of Madrid updated the regulatory framework with a regional decree that regulated the single school choice for all the municipalities in the region (Decree 29/2013 (*Decreto del Consejo de Gobierno, de libertad de elección de centro escolar en la Comunidad de Madrid*)).

<sup>13</sup> Table A 2 in the Appendix provides a summary of the municipalities that joined the single-zone school choice system over time.

**Applications for preschool and primary education.** Our primary source of data for the analysis is a unique administrative database containing information on the universe of families who applied to a primary school in the region of Madrid. For each applicant, the dataset contains the top-ranked school from the rank-ordered list, the primary student information regarding family characteristics, the home address, the total priority points obtained based on such characteristics, and the final school assignment. Regarding family information, the data contain the precise geolocation of each pupil's home residence, which we link (with the help of geolocation software) to different geographical areas (districts, neighborhoods, and census blocks). In addition, the application contains information about the pupil's country of birth, which we use to construct a proxy for immigrant background status. We measure immigrant status through a dummy variable that takes the value of one when the child is not Spanish and zero otherwise.

**School database.** We use the universe of schools in the region of Madrid provided by the regional government. This database includes the precise geographic coordinates of each school, an indicator for the school type (public, semipublic or private) and whether the school offers bilingual education (schools with English and Spanish instruction).

**Household socioeconomic characteristics.** We use information from the Census Office of the city of Madrid, which provides the distribution of education levels of the population by census block on January 1, 2012, 2013, and 2014. The data are accessible for the three geographical levels of disaggregation, namely, districts, neighborhoods, and census blocks. We have access to information on the proportion of the population in each level of education at the census-block level. We use this to translate the corresponding level of education to an equivalent number of years of schooling, allowing us to compute the average number of years of schooling in each census block.<sup>14</sup> We assign to each family the corresponding value of the census block where the family resides. Hence, we proxy for parental education by the average years of schooling, dividing families by quintiles of parental education. This proxy for parental education is therefore affected by measurement error, which is discussed in the following subsection.

**School Standardized Test Scores.** To proxy for school quality, we use a standardized exam administered to all Grade-6 students in the region of Madrid between the 2004/2005 and 2014/2015 school years. The exam, known as the Essential Knowledge and Skills test (*CDI-Conocimientos y Destrezas Indispensables*), is designed for education policy measures and does not have any specific academic consequences for students. The goal of this test is to provide information for policy-makers, schools, and families about the school's average performance. The test focuses mostly on curriculum content knowledge in the areas of reading and mathematics.

**Parental Questionnaire on Stated Motivations for Choosing School.** To test the effect of the school choice reform on the parents'

<sup>14</sup> The most disaggregated units are the census blocks (*Sección Censal*), which are constructed for local, regional and national election purposes (assigning each census block to one voting center) and usually contain no more than 2500 individuals. Figure B.1 in the Appendix includes an example of a block of the district *Centro* of Madrid. The construction of this variable is detailed in the online Appendix (Section C). Ideally, we would use data from the 25–49 age group to obtain a more accurate proxy for parental education, as this is the most relevant level of education for parents with preschool and primary school children. Nevertheless, we do not use this measure in the primary analysis since the database that includes such disaggregation corresponds to the year 2017 onward and not to prereform years. We assign to each family the corresponding value of the census block in the following way: (i) families applying in 2010, 2011 or 2012 are assigned the values of January 1, 2012; (ii) families applying in 2013 are assigned the values of January 1, 2013; and (iii) families applying in 2014, 2015 and 2016 are assigned the values of January 1, 2014. The results are robust to changes in the computation of this variable and are available upon request.

main reason to choose school, we use the standardized exam administered to all Grade-3, Grade-6 and Grade-10 students in the region of Madrid in the 2016/2017 and 2018/2019 school years. The goal of this exam, known as the LOMCE test, is similar to that of the CDI. We use data from the parent questionnaire, in particular, a specific question in which parents are asked about the main reason for choosing a school. The question is as follows: “What is the main reason that you selected the school in which your son or daughter is enrolled?” (*Cul es la principal razn por la que ha elegido el colegio al que acude su hija o hijo?*). There are a total of ten possible answers, and parents can pick up to three answers. Some of the answers refer to whether the school is the closest from their family home, the schools reputation, the schools ideology or that the school was not chosen but rather assigned.

The population of interest for our main analysis consists of households located in the city of Madrid that apply for preschool for a child aged 3 years in Madrid. We restrict our sample to families that have no siblings in the top-ranked school, following Calsamiglia and Güell (2018). Families with older siblings enrolled at the school have different incentives and behavior from the rest of the applicants since their past choice conditions their present one. The number of bonus points that families obtain when applying to a school where a sibling is already enrolled is the highest. As a result, admission to those schools is almost automatically guaranteed. These families may, therefore, react differently to policy changes since they have distinct preferences and incentives, which could make their decisions more inelastic to changes in the bonus criteria.<sup>15</sup> To avoid bias and exploit the cleared school choice market in the first preschool year, we use families with 3-year-old children as our estimation sample.<sup>16</sup> The online Appendix (Section G) presents the restrictions that we use to construct the analysis sample.

### 3.2. Summary statistics of applicants

Table 2 presents the summary statistics of applicants from the period 2010–2016. The majority of applicants (approximately 70 percent) are applying to start preschool education when their children are at the age of 3 (our population of interest). Moreover, 86 percent of applicants are pupils with Spanish citizenship. This table presents the decreasing trend in the number of families with 3-year-old children applying to preschool over time, with a 6 percent reduction in 2013. Table D 1 presents the population census of 3-year-old children over the period under study, showing that the observed declines in the total population at that age are not specific to our estimation sample. We observe that the gradual decrease in the sample size is not correlated with parental education or immigrant status.<sup>17</sup> We see that applicants

<sup>15</sup> Pupils awarded places due to each of the two new priority bonuses created in the 2012 reform only account for a small share, i.e., between 3%–4% for the new low-income bonus and approximately 6% for the bonus for having alumni student relatives from the school. We do not drop these students from the main sample analysis since we do not have this information for the years before the reform. Thus, discarding these observations could lead to sample selection bias between the periods before and after the reform.

<sup>16</sup> Pupils who enter the system for the first time (at the age of 3) may have different preferences, priorities and behavior than pupils who seek to change their school later on in the system, making those groups difficult to compare. Pupils who enter the system after the age of 3 do not face the same set of feasible schools as pupils who enter the system at the age of 3, as they are only left with the available slots due to current students leaving the school or underdemanded schools. Additionally, these families may have different preferences for schools. In our empirical framework, we need to assume that the distribution of parents' preferences remains constant over time. This seems to be plausible at least for the specific years of the reform.

<sup>17</sup> The changes in sample size over time are driven by city demographics. According to official municipal data from the city of Madrid, there was a sharp decrease in the number of births in 2009 and 2010 (who were 3 in 2013) relative to 2008. Hence, the observed declines in the census are consistent with those observed in Table 2. In our empirical strategy, we account for these demographic changes by tracking the residential dynamics of the population.

have, on average, four schools within a radius of 500 m, and the closest school is located at approximately 230 m within the city of Madrid. The average distance to the closest district border is 630 m. In the rest of the municipalities, families have, on average, three schools in a radius of 500 m, and the closest school is located at approximately 330 m.

We find two limitations in the data. First, we do not have information concerning the location of the parents' workplace. Given that the reform reduces the importance of the district of both the household and workplace location, we need to assume that parental distance to the workplace does not change differentially during the years of the reform. We believe that this would be at most a limited concern. Changes in the location of the workplace are limited and challenging to exert by families, particularly over the years when the global economic crisis was particularly severe in Spain. Moreover, households typically choose where to locate their residence to take advantage of the supply of local public goods (Tiebout, 1956).

The second data limitation relates to the parental education variable, which may suffer from unobserved heterogeneity caused by measurement error. We use average values at the census-block level, which potentially reverts to the mean tails in each block. For example, in low- and middle-income census blocks, highly educated parents are identified with much lower education levels. If the reaction to the reform is higher in relative terms for highly educated parents, our measure of parental education may incur measurement error that is correlated with the effect of the reform. If that is the case, our estimates of changes in out-of-district assignment by parental education may suffer from negative bias, so that the “true” effects may be even larger after the reform for relatively highly educated families.<sup>18</sup>

## 4. Empirical strategy

### 4.1. Out-of-school district choice and assignment: Event study-first difference approach

The event-study approach identifies the effect of the reform on families' choices by comparing school choices made before with those undertaken immediately after the modification. First, we investigate whether parents sought admission at or were finally assigned to a school located in a different school district from the household's residence. Second, we analyze whether the commuting distance between the household and the top-ranked (and assigned) school was impacted by the reform.<sup>19</sup> The two variables provide different information. While out-of-district applications may be seen as an “extensive” margin measure of commuting distance to school, the average distance to top-ranked (and assigned) school may represent a combination of the “intensive” and “extensive” margins of commuting distance, as the distance traveled is composed of both within- and between-school district willingness to travel.

To estimate the effect of the changes in residence-based priorities on families' out-of-district choices, we estimate the following reduced-form specification:

$$D_{ibt} = \alpha + \delta_t + X'_i\beta + B'_{bt}\gamma + v_b + \epsilon_{ibt} \quad (1)$$

<sup>18</sup> It is unlikely that the sign of the bias would go in the opposite direction since, on average, highly educated families living in districts with high-quality schools do not prefer low-educated areas with low-performing schools.

<sup>19</sup> We use the Open Source Routing Machine (OSRM) routine, which returns the travel distance using the latitude and longitude coordinates of the household and the school. The command computes this distance based on a map; we use OpenStreetMap because it allows us to work offline with an unlimited request of distances to be computed and replicated (Huber & Rust, 2016). The database contains the UTM coordinates in the ED50 base. The OSRM command needs GPS coordinates and the ETRS89 base, so we use a geographical information system (GIS) to convert them into suitable coordinates.

**Table 2**  
Summary Statistics: School applicants in the region of Madrid over 2010–2016.

| Variable  | 2010              | 2011              | 2012              | 2013              | 2014              | 2015              | 2016              |
|---|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
| <b>A. Grades in City of Madrid</b>  |                   |                   |                   |                   |                   |                   |                   |
| Preschool age of 3  | 17,399<br>[70.48] | 17,239<br>[71.03] | 17,382<br>[67.50] | 16,357<br>[66.87] | 15,883<br>[67.50] | 15,521<br>[71.71] | 15,009<br>[70.62] |
| Preschool age of 4 & 5  | 2,919<br>[11.82]  | 2,785<br>[11.46]  | 3,441<br>[13.35]  | 3,289<br>[13.44]  | 3,013<br>[12.80]  | 2,374<br>[10.96]  | 2,395<br>[11.27]  |
| Grades 1–6  | 4,368<br>[17.69]  | 4,252<br>[17.50]  | 4,938<br>[19.16]  | 3,289<br>[19.70]  | 4,641<br>[19.71]  | 3,752<br>[17.33]  | 3,850<br>[18.11]  |
| <b>B. Students characteristics — preschool age of 3- City of Madrid</b>                         |                   |                   |                   |                   |                   |                   |                   |
| N immigrant   | 2,026             | 2,267             | 2,904             | 2,587             | 2,297             | 2,124             | 1,974             |
| N female  | 8,547             | 8,502             | 8,419             | 7,996             | 7,971             | 7,622             | 7,316             |
| Frac. quintile 1  | 0.198             | 0.198             | 0.197             | 0.197             | 0.197             | 0.196             | 0.196             |
| Frac. quintile 2  | 0.198             | 0.197             | 0.197             | 0.197             | 0.196             | 0.197             | 0.197             |
| Frac. quintile 3  | 0.197             | 0.197             | 0.198             | 0.198             | 0.197             | 0.196             | 0.197             |
| Frac. quintile 4  | 0.197             | 0.197             | 0.196             | 0.197             | 0.196             | 0.197             | 0.196             |
| Frac. quintile 5  | 0.197             | 0.197             | 0.197             | 0.197             | 0.196             | 0.196             | 0.196             |
| Distance to closest district border (m)   | 634.0             | 623.5             | 627.0             | 626.5             | 632.4             | 633.0             | 625.3             |
| Distance to closest school (m)  | 230.1             | 226.0             | 231.7             | 230.5             | 231.7             | 234.2             | 239.9             |
| Number of schools in a radius of 500 m  | 3.696             | 3.761             | 3.690             | 3.680             | 3.664             | 3.650             | 3.623             |
| Observations  | 17,399            | 17,239            | 17,382            | 16,357            | 15,883            | 15,521            | 15,009            |
| <b>C. Students characteristics — preschool age of 3- Region of Madrid (without Madrid city)</b> |                   |                   |                   |                   |                   |                   |                   |
| N immigrant   | 3,747             | 4,285             | 4,475             | 4,271             | 3,865             | 3,489             | 3,466             |
| N female  | 12,791            | 13,194            | 13,116            | 12,774            | 12,254            | 11,997            | 11,529            |
| Distance to closest district border (m)   | 1,454             | 1,439             | 1,436             | 1,431             | 1,438             | 1,416             | 1,431             |
| Distance to closest school (m)  | 323.5             | 319.1             | 333.0             | 330.1             | 331.2             | 336.6             | 342.1             |
| Number of schools in a radius of 500 m  | 2.792             | 2.788             | 2.674             | 2.654             | 2.677             | 2.597             | 2.578             |
| Observations  | 26,261            | 27,175            | 27,039            | 26,099            | 25,299            | 24,856            | 23,620            |

Notes: Each year corresponds to the year of application and the school year starting in September of that year. Data on bonus information for alumni family members and RMI are not available before 2012 given that the reform was implemented that year. Quintiles of parental education are defined at the census block level. Percentage of households applying to different grade levels are displayed in square brackets.

where  $D_{ibsd_t}$  is either a dummy that takes a value of 1 if pupil  $i$  living in census block  $b$  applies to school located in a different school district from where she resides in year  $t$  and 0 otherwise, or the travel distance from the residence of pupil  $i$  in census block  $b$  and school district to the school that was top-ranked in year  $t$  on her application form;  $\delta_t$  represents year fixed effects;  $\nu_b$  represents census block fixed effects;  $X'_i$  is a vector of household characteristics, which includes the number of schools within a radius of 500 m (i.e., a proxy for school supply) and the Euclidean distances (in meters) to the closest school district border and to the nearest school;  $B'_{bt}$  is a vector of time-variant census block characteristics, such as population density and average income; and  $\epsilon_{ibt}$  is the error term. The fixed effect of the year prior to the reform is excluded, such that  $\delta_t$  values are interpreted as the change in the dependent variable in year  $t$  with respect to the baseline year 2011. Therefore,  $\alpha$  captures the mean outcome variable in the year prior to the reform (2011/2012 academic year) for the excluded census block. We cluster the standard errors at the census-block level to account for the fact that willingness to commute may exhibit spatial and serial correlation within a given census block.<sup>20</sup>

#### 4.2. Parents' main reason for choosing a school

We also investigate whether the school choice reform affected the parents' stated motivation for choosing a school. To estimate the effect of the school choice reform, we perform a difference-in-difference (DID) approach that compares the answers of parents whose children took the test in Grade 3 in 2018/19 (they chose a school in 2013/14, i.e., the reform year) with those parents whose children took the test in 2016/17 (their parents chose a school in 2011/12, i.e., before the reform). We control for the answers of parents whose children took

<sup>20</sup> We also perform different specifications that include neighborhood or school district fixed effects, as well as observable characteristics at these two geographical units. The results are robust to all these specifications and provided in the online Appendix (Section K).

the test in Grades 6 and 10 for both years (thus, all chose a school before the school reform).<sup>21</sup> To estimate the effect of the changes in residence-based priorities on families main reason for choosing school, we estimate the following reduced-form specification:

$$Y_{it} = \alpha + \delta \text{Grade } 3_i * \text{Post2013}_i + \gamma \text{Grade } 3_i + \sigma \text{Post2013}_i + X'_i \beta + \epsilon_{it} \quad (2)$$

where  $Y_{it}$  is a dummy that takes a value of 1 if the parents of student  $i$  at year  $t$  include this particular answer on the main reason for choosing a school and zero otherwise; Grade 3<sub>*i*</sub> is a dummy that takes a value of 1 if student  $i$  is enrolled in Grade 3, and zero if student  $i$  is enrolled in Grade 6 or 10; Post2013<sub>*i*</sub> is a dummy variable equal to 1 when the year is 2018/19 and zero otherwise;  $X'_i$  is a vector of household characteristics; and  $\epsilon_{it}$  is the error term. The coefficient of interest is  $\delta$ , which provides the effect of the reform. We cluster the standard errors at the school level.

## 5. Results

### 5.1. Out-of-school district choice and assignment

The main shortcoming of the event-study approach is that we compare different cohorts, i.e., those unaffected by the reform with those affected by the reform; however, cohorts of applicants can be different, and there might be trends in the outcome variable that drive the results. Table I 1 tests for pretrends in all baseline outcomes. We find little evidence of pretrends over the two prereform years when implementing the event-study approach. None of the main four contrasts are significant at the 10% level. In addition, we also test for pretrends in every subsample that we use in the paper (see Table I 2

<sup>21</sup> We only keep parents whose children have been in the Spanish education system for five or more years and hence selected school at the time of the reform. We cannot use the LOMCE database for 2017/18 since unfortunately, it is not possible to identify a students school grade on the family questionnaire.

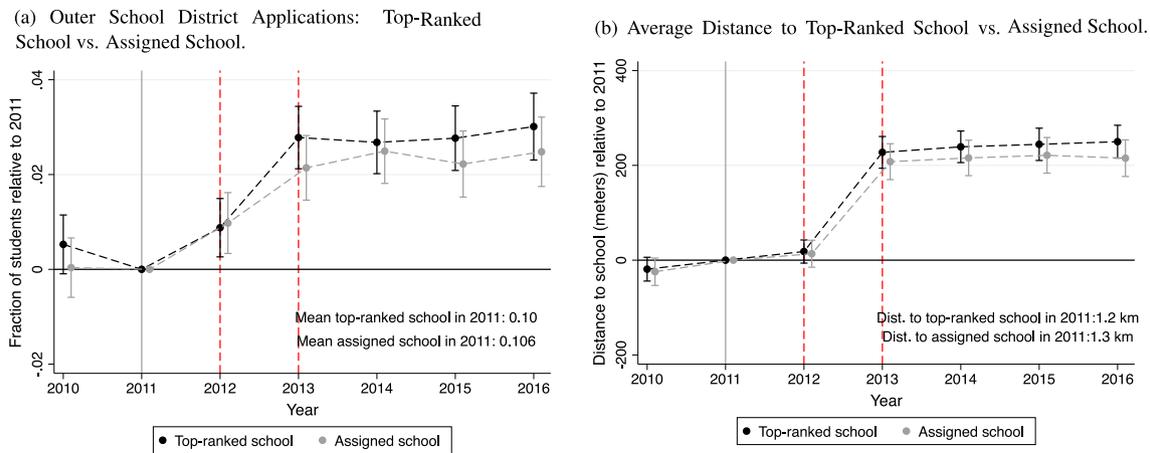


Fig. 2. Out-of-District Assignment and Average Distance to Assigned School.

Note: Black and gray dots represent point estimates. Horizontal lines depict confidence intervals at the 95% level of confidence. The red stripes reflect the two consecutive changes in the choice priority system in 2012 and 2013. The coefficients are normalized to zero in 2011.

and Table I 3). These analyses suggest little evidence of pretrends, at least over the two prereform years.

Panels A and B of Fig. 2 plot the point estimates and confidence intervals of year fixed effects in Eq. (1) for out-of-district top-ranked and assigned schools and average travel distance to the top-ranked and assigned school, respectively. The abolishment of residence-based priorities in 2013 (and, to a lesser extent, the 2012 reform) seems to be associated with a sharp increase in these two variables. Panel A of Fig. 2 shows a relatively low prereform level of the fraction of out-of-district applications (0.10), which may be explained by the fact that the BM induces a high level of residential sorting. The reform is associated with a further increase in the fraction of out-of-district applications of 3 percentage points (30% with respect to the baseline mean). The figure shows that the probability of top-ranking an out-of-district school is almost the same as being assigned to it since most families are assigned their top-ranked school under the BM. Panel B of Fig. 2 shows that the reform increased the average travel distance to top-ranked schools by 259 m on average (22% with respect to the baseline mean of 1.2 km). Overall, the sharp increase in the levels of the outcome variables followed by a constant evolution between 2013–2016 supports the hypothesis that the reform may be behind these results.

### 5.2. Out-of-school district choice and assignment: Heterogeneous effects by parental education and immigrant status

Panels A and B of Fig. 3 explore heterogeneous effects by parental education and immigrant status. Panel A shows that families with higher average parental education are more willing to travel and be assigned to schools in different districts than their counterparts. The reform is associated with an increase in the out-of-district assignments of 1, 3, and 5 percentage points for parental education quintiles one, three, and five, respectively (26, 31, and 31 percent with respect to the baseline mean, respectively). Interestingly, even though families with higher levels of education reacted more to the reform in absolute terms, the effects were somewhat similar across parental education quintiles in relative terms.

A different situation occurs when considering heterogeneous effects by immigrant status. Panel B of Fig. 3 shows that while families of Spanish children increased their out-of-district assignment from 2013 onward, parents of immigrant children did not react at all in the first years of the reform. This result supports the idea of potential information gaps across immigrant status groups since immigrants started to react to the reform (in the same magnitude as Spanish families) three years after its implementation. The dynamics suggest a learning process

over time for families with non-Spanish children, catching up to the absolute effect of the reform in the last two observed years (2015 and 2016).<sup>22</sup>

### 5.3. Performance of assigned school: Heterogeneous effects by parental education and immigrant status

After the reform, most families managed to access slightly better-performing schools, but children from the lowest educational quintile benefited the most. Fig. 4 shows the average school performance of the assigned school measured by the CDI standardized test before our period of study by parental education and immigrant status. Panel A shows that the children of parents in the top quintile of education are assigned to schools that perform approximately twenty percent higher in standardized tests than the schools to which the children of parents in the lowest educational level are assigned. The reform seems to have had heterogeneous effects. The average performance of the assigned school has increased much more for those students whose parents are at the lowest educational quintile in comparison to those whose parents are at the top. (0.12 versus 0.01 points out of 10, respectively). The average performance of the assigned school has increased by 2.5% for students whose parents are at the lowest education quintile and by 0.1% for those at the top of parental education.

Figure K.8 shows that the number of applications to the top performing quintile in-district school from the highest educated parents decreased due to the reform. Families from the top educational quintile are 26% less likely to choose a top-performing in-district school as their top-ranked school after the reform. The effect fades out after 2014, which coincides with the slight increase in the average performance of the assigned school documented in Fig. 4. Given that parents from the highest educated quintile are decreasing their applications to top-performing in-district schools, we test whether they are choosing higher-performing schools in different districts. Figure K.7 shows that this is indeed the case. After the reform, parents in the top quintile of education are choosing schools that have higher average performance. This evidence suggests that parents from educated families are decreasing their applications to the top in-district schools to apply for higher-performing schools in different school districts after the reform. The results from Fig. 4 indicate that this change in the application behavior is not translated into admission to better-performing schools.

The story for low-educated families seems to be different. Figure K.7 shows that parents in the bottom quintile of education are choosing

<sup>22</sup> We also identify heterogeneous effects by distance to the school district border. For further details, see the online Appendix (Section K).

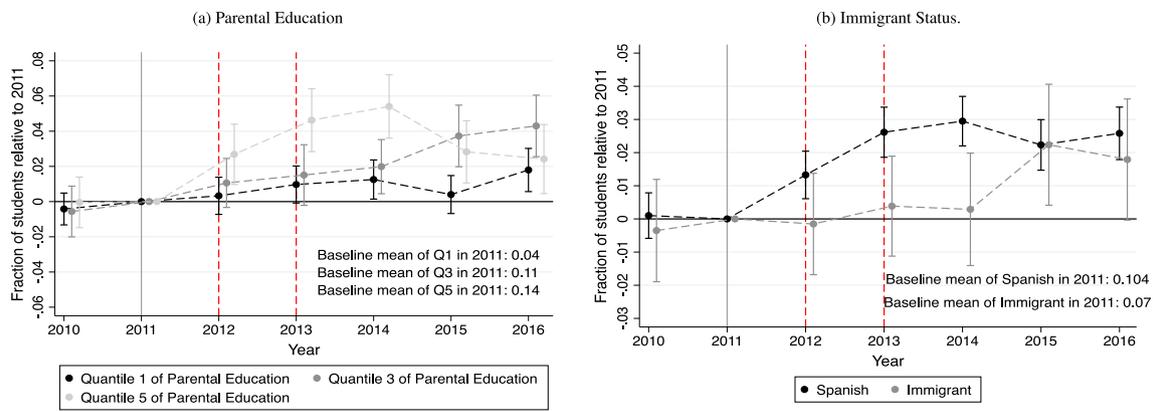


Fig. 3. Impact of the Reform on Out-Of-District Assignment by Parental Education and Immigrant Status.

Note: Black and gray dots represent point estimates. Horizontal lines depict confidence intervals at the 95% level of confidence. The red stripes reflect the two consecutive changes in the choice priority system in 2012 and 2013. The coefficients are normalized to zero in 2011. Quantile 1 and Quintile 5 refer to families in the lowest and highest quintiles of parental education, respectively.

schools that have higher average performance after the reform. These schools might be either in-district or out-of-district schools. Evidence points toward high-performing in-district schools. Even though parents in the lowest educational quintile are not applying more often to the top-performing in-district school (see Figure K.8), they are increasing their applications to in-district schools that have above median average performance. Figure K.9 shows that low-educated families are 11% more likely to choose an above-median performance in-district school as their top-ranked school after the reform compared to before the reform. We believe that evidence shows that low-educated families are assigned to better-performing schools after the reform due to an increase in their application to better-performing in-district schools and a decrease in their application to top-performing in-district schools by higher-educated families. This result might indicate that because of the perceived lower competition level for in-district high-performing schools, less-educated families started to rank higher-performing schools as their top choice, potentially thinking that their admission chances are higher after the reform.<sup>23</sup>

Panel B of Fig. 4 shows that children of Spanish parents are assigned to schools that perform approximately ten percent higher on standardized tests than the schools to which immigrants are assigned. The average performance of assigned schools increased more for Spanish than for immigrants in the first years after the reform. Nevertheless, the variable does level out, with no substantial differences displayed between them after two years.

#### 5.4. Effects on the main reason for choosing a school

Table J 1 shows the fraction of families' main reasons for choosing a school before the reform. The most popular reason for choosing a school was whether it was the closest to their household residence (with approximately 50 percent of families selecting this option), along with the school's reputation, being bilingual school, and a school offering seats for all grades. There are substantial differences in the stated motivation for choosing a school according to immigrant status. Columns (3) and (4) display the difference between families with Spanish and immigrant children. We observe that immigrants prefer schools closer to their homes and place less weight on school characteristics with respect to families with Spanish children. In addition, immigrants were approximately 40% more likely to be assigned to a school they did

<sup>23</sup> We find little evidence of parents trying to avoid any specific schools after the reform. Table K 8 shows the reform's effect on the out-of-district application by the performance of the closest school and by parental education and immigrant status.

not choose. Column (4) shows that these differences are statistically significant. Nine out of ten contrasts are significant at least at the 10% level, and we can reject the null hypothesis that the F-statistic is equal to zero (p value <0.01). These results are informative about the school characteristics that Spanish and immigrant families valued when choosing a school prior to the reform.

To investigate whether these stated motivations changed after the school choice reform, Table J 2 and Table J 3 display the main results of a DID strategy on the main reason for choosing a school, estimating Eq. (2). Families with Spanish children seem to drive the changes based entirely on the main reason for school choice after the reform, while parents of immigrant children do not seem to react.

Table J 2 seems to support the hypothesis that parents of immigrant children have not changed their main reason for choosing a school in response to the school choice reform. None of the twenty contrasts are significant at the 10% level, irrespective of controlling for individual family controls (see Panels A and B). This result is also consistent with immigrant families not reacting at all in the first years of the reform (Fig. 3).

Table J 3 shows that families with Spanish children have significantly changed their main reason for choosing a school for half of the outcome variables after the reform. The school choice reform has increased parents' choices regarding several school characteristics, such as whether the school offers a flexible timetable or complementary services. In addition, the school choice reform has reduced the fraction of families declaring that they were assigned to a school they did not choose but to which they were automatically assigned. We interpret these results as suggestive evidence of families being more involved in the process of selecting a school.<sup>24</sup>

Overall, there are two significant findings to extract from this section. First, on average, the reform seems to have increased households' distance traveled and out-of-district school assignments. Second, the effect has not been homogeneous across families with different parental education levels and immigrant status. In particular, students from the

<sup>24</sup> We do not find statistically significant effects on the probability that Spanish families name the "closest school" as one of their main reasons for choosing a school. Despite being the most popular reason for choosing a school, it seems to be an inelastic variable since 60% of them do not enumerate any other reason. Potentially, this translates into a reduced margin of improvement. In addition, parents can name up to three main reasons to choose a school, implying that they may still value the school's closeness to their household but exercise their choice by changing their selection in the other categories. Potentially, the decision to move to a school that is located further away is motivated to achieve "better" school inputs of the education production function, such as teachers, peers, or amenities.

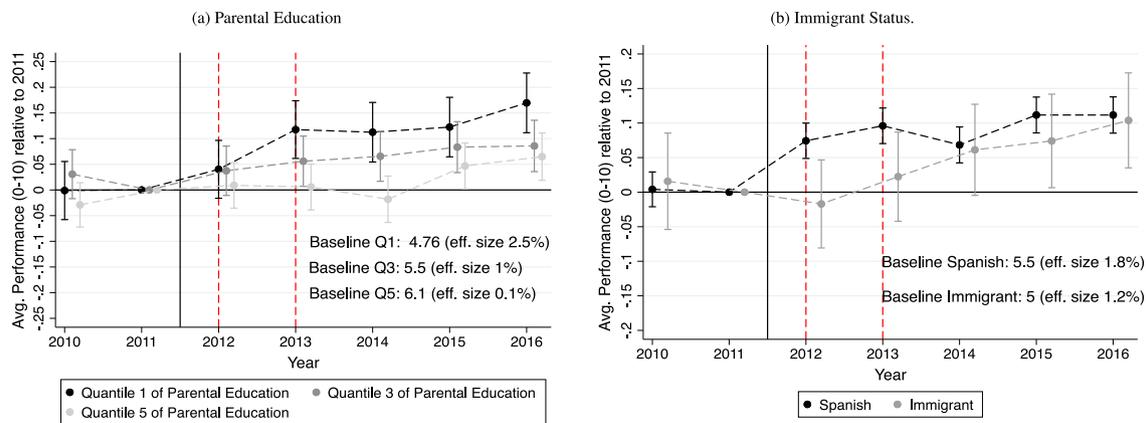


Fig. 4. Impact of the Reform on the Average Performance of Assigned Schools (0–10) by Parental Education and Immigrant Status.

Note: The outcome variable is the average school performance of the assigned school in the CDI test score a year before 2010. Black and gray dots represent point estimates. Horizontal lines depict confidence intervals at the 95% level of confidence. The red stripes reflect the two consecutive changes in the choice priority system in 2012 and 2013. The coefficients are normalized to zero in 2011. Quantile 1 and Quintile 5 refer to families in the lowest and highest quintiles of parental education, respectively.

most educated households and native students reacted the most to the reform in absolute terms. However, in relative terms, the effect size of the reform was relatively similar and hence neutral across parental education quintiles.

These results are consistent with previous evidence from similar reforms in other countries, such as Sweden (Böhlmark & Lindahl, 2007). These reforms tend to show that less advantaged and immigrant households tend to exert less choice than more socially advantaged and native families when choosing schools (which may reflect, for instance, differences in preferences or information). The heterogeneous effects of the school choice reform on parents' stated motivation for choosing a school seem to support this hypothesis.

Finally, the absolute magnitude of the change in terms of the total student population is modest, probably due to the weight of other critical contextual factors. Some examples are the incentives generated by the BM assignment model, the disutility of commuting, and the extent of the effect expected to be small due to the low-risk behavior in the top-ranked school induced by the BM.

## 6. Robustness checks

### 6.1. Identification threats

In this subsection, we identify several potential identification challenges for our main specification and discuss their implications for our results.

First, we cannot directly capture parents' full responses since we only observe the top-ranked school in the submitted rank-ordered list of schools.<sup>25</sup> Table G 2 shows that approximately 88 percent of the students in the current were assigned to the school they ranked first. Hence, despite not being able to capture the full extent of families' behavioral responses, the top-ranked school provides most of the information used to understand families' choices under the BM, with an almost one-to-one mapping between top-ranked and assigned schools. Note that the top-ranked school might not necessarily be the preferred school under the BM since this algorithm allocation procedure provides incentives for families to misreport their preferences (Abdulkadiroglu & Sönmez, 2003); hence, it is not possible to disentangle families' changes in preferences versus strategies.

<sup>25</sup> A unique characteristic of the BM is the fact that the first-ranked school is crucial for final allocation. Abdulkadiroglu, Che, and Yasuda (2011) highlight that one of the features of the BM is that it better accounts for the parents' cardinal preferences than other algorithms with more desirable ordinal properties (e.g., Gale Shapley or top trading cycles).

Another important threat to our identification is the potential residential sorting of families as a consequence of anticipation of the reform. The abolishment of residence-based priorities was a proposal in the electoral program of the political party that won the elections in the region of Madrid in May 2011.<sup>26</sup> The information on the 2012 (and 2013) priority changes was disclosed to the press in February 2012 and announced formally through an administrative order in March 2012. Applications are handled between April and May of the academic year, implying that families did not have much time from March to May of 2012 to change their decisions. However, they had approximately one year between the formal announcement of the reform and the 2013 round of applications, which could raise concerns about anticipation. Such effects would create a problem for identification if families reacted before the implementation of the reform changing their primary household residence.<sup>27</sup> A relaxation of the proximity priority criteria may provide incentives for families to change their residence to districts with lower housing prices (typically positively correlated with lower school performance) since living in the school district of the desired school is not as decisive as it was previously in terms of admission probabilities. In this case, a potential positive significant effect on out-of-district application may be correlated with this dispersion effect, leading to an upward bias in our estimates that would reflect an upper bound of the "true" effect. Table E 1 shows the main results of the DID strategy using housing prices as an outcome. We find that there is little evidence of pretrends in housing prices, with no statistically significant differences in trends of housing prices between the treatment and control groups. In addition, Table E 2 shows no evidence of differential changes in housing prices depending on school average performance associated with the policy reform. The previous evidence presented suggests little evidence supporting the hypothesis of prereform residential anticipation effects.

Finally, another threat to our identification is the difficulty of disentangling the isolated impact of each reform due to the consecutive years

<sup>26</sup> In the program, the winning party in the 2011 election (*Partido Popular*) included the following statement: *to establish full freedom of school choice, implementing a single zone for parents to bring their children to whichever school they want.*

<sup>27</sup> Hastings et al. (2009) find that the preference for highly ranked schools increases with proximity and that parents with higher incomes are more willing to commute farther away for such schools. This increases the demand for school districts with higher average public school performance, leading to increases in the housing prices of those districts. See also Black (1999), Fack and Grenet (2010), Gibbons, Machin, and Silva (2013), Imberman and Lovenheim (2016).

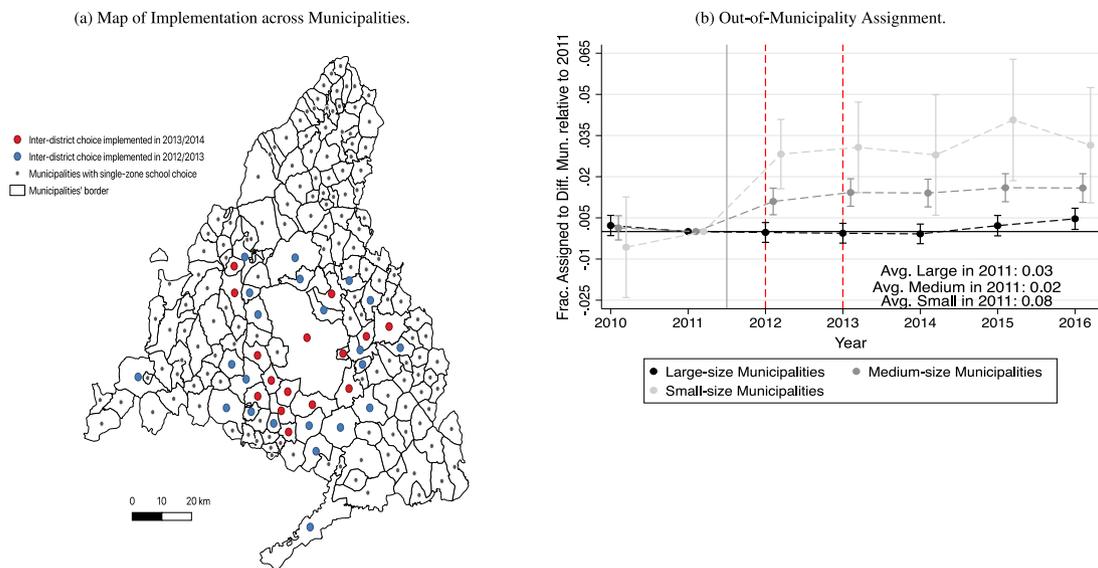


Fig. 5. The Effect of the School Choice Reform on Out-of-Municipality Assignment by Municipality Size.

Note: Panel A shows the progressive implementation of the reform across municipalities. Panel B shows the effects of the school choice reform on out-of-municipality assignment by municipality size. Black and gray dots represent point estimates. Horizontal lines depict confidence intervals at the 95% level of confidence. The red stripes reflect the two consecutive changes in the choice priority system in 2012 and 2013. The coefficients are normalized to zero in 2011.

of their implementation. As mentioned in Section 2, in 2012/2013, the criteria to obtain bonus points granted to low-income families changed, and a new priority bonus was granted if a pupils family member was an alumni student of the top-ranked school. In the online Appendix (Section K), we provide several checks to rule out that the 2012 reform is behind our results of interest. Table K 2 and Table K 3 test for the impact of both reforms using year fixed-effects and a dummy for post 2013 and a before–after 2013 dummy variable, respectively. Table K 2 shows that the 2012 reform has no effect on the average travel distance to top-ranked and assigned schools, while the 2013 reform displays strong and statistically significant effects at the 1% level. The 2012 reform has significant effects on out-of-district outcomes, with an effect size three times smaller than that of the 2013 reform. Table K 3 provides more evidence of the clearer and more significant effect of the 2013 residence-based priorities reform. In addition, Table K 1 tests for a joint effect of both reforms. The coefficients are smaller but still sizeable and significant at the 1 percent confidence level. Therefore, these additional validity checks show that the 2012 reform has little effect on our outcome variables and that when there are significant effects, its size is relatively small to explain the stronger and clearer results of the 2013 reform.

## 6.2. Difference-in-difference analysis

Table E 3 shows the main results of the DID strategy, estimating Equation 4. Columns (1) to (3) report three specifications for out-of-district assignment, while Columns (4) to (6) provide the same three specifications for distance to the assigned school. The effect of the reform is positive and statistically significant in all specifications. The effect size is an increase of 3.6 percentage points for out-of-district assignments and approximately 311 m for the distance to the assigned school, which is equivalent to a 70% increase for out-of-district assignments and 25% for the distance to the assigned school. Figure K.6 displays the event-study graphs for the DID design. The results are robust to alternative definitions of groups (see Table K 7) and to the use of year dummies instead of a dummy variable for Post2013, (see Table K 6).<sup>28</sup>

<sup>28</sup> Since the school boundaries are a much larger geographical unit than the center of school districts, the treatment group will be mechanically larger

## 6.3. Phasing-in of the reform across municipalities

As we described in Section 2, school choice reform was gradually implemented in other municipalities beyond the city of Madrid. The reform was implemented in several municipalities (mostly those of medium size) in 2012, whereas for most of the larger municipalities (including Madrid), it took place in 2013. Panel A of Fig. 5 shows a map of the phase-in of the reform in the entire set of municipalities in the region of Madrid. We exploit this phasing-in of the reform to test the reaction of medium-size municipalities that implemented the single school choice district one year before the large-size cities (i.e., in 2012).<sup>29</sup> We do not have access to geographical data on the catchment areas in municipalities other than the city of Madrid before the reform. Nevertheless, we measure the distance to the assigned school.<sup>30</sup> Given this limitation, we calculate the out-of-municipality assignment as a proxy for an extensive margin measure instead of computing out-of-district assignment.

Panel B of Fig. 5 shows that families living in medium and small municipalities increased the out-of-municipality assignment in the year where the reform was implemented, followed by a constant effect through the following years. An interesting result from this analysis is the spillover effects. As the educational markets at the municipal

than the control group. Table K 6 shows that the estimates are statistically significant on the distance to assigned school variable and qualitatively similar for three out of the four post reform years on the out-of-district assignment variable.

<sup>29</sup> Table A 2 in the online Appendix describes which municipalities adopted the reform in 2012 and 2013. The relationship between the labels of medium-sized and large municipalities is not a perfect 1:1 mapping to population size. For instance, in 2012/2013, Alcobendas and Las Rozas (with approximately 120,000 and 100,000 inhabitants, respectively) adopted the reform and are labeled medium-sized municipalities. In 2013/2014, municipalities such as Rivas-Vaciamadrid, Collado-Villalba, Boadilla del Monte, or Galapagar (with approximately 80,000, 60,000, 50,000, and 30,000 inhabitants, respectively) implemented the reform and are labeled large municipalities.

<sup>30</sup> In Madrid, the choice catchment areas coincide with the city districts, but other municipalities define catchment areas using other criteria. We do not employ data on catchment areas in municipalities other than Madrid, which implies that we cannot compute the out-of-school district assignment.

level are not independent, the relaxation of the intermunicipality bonus points generates interactions between municipalities. We show that small and medium size increase the out-of-municipality assignment from 2012 onward, while no effect is shown for large-size municipalities. These heterogeneous effects suggest that when the supply of feasible schools increases, families with a higher scarcity of school supply are those who seem to react the most and be willing to travel further away for schooling.

## 7. Conclusion

We use administrative data from the region of Madrid to analyze the impact of abolishing residence-based priorities in school assignment on families school choices, the stated motivation behind the decision of choosing a school, and school allocation. We find that abolishing residence-based priorities increases families' out-of-district school assignment and assignment to schools located farther away from their home address. Families with Spanish-born children are responsible for driving these increases for the first two years following the reform, after which families with immigrant children are shown to have caught up. Most families access slightly better-performing schools, particularly children of lower-educated parents.

Relaxing residence-based priorities may be an effective policy to help increase pupils' interdistrict school assignment; however, heterogeneous responses, for example, by immigrant parents, need to be taken into account. Immigrants may lack the network and information necessary to make an informed choice, or they may prefer to choose the nearest school for cultural reasons. Such informational gaps seem to have narrowed three years after the reform. Abolishing residence-based priorities seems to help families from less-educated backgrounds achieve better performing schools, potentially leading to better long-term outcomes.

We do not precisely claim that these effects directly translate into an increase in families' welfare through a better fit with their preferences since the BM does not incentivize families to report their true preferences. However, we provide evidence that families exercise more choice and are more involved in the process of choosing a school. This paper shows that even in a context that induces nonrisky behavior, in which low elasticity of response to changes in school priorities is expected, families can modify their school choices, thereby affecting the final school assignment.

## CRedit authorship contribution statement

**Lucas Gortázar:** Conceptualization, Data curation, Methodology, Formal analysis, Writing – original draft. **David Mayor:** Conceptualization, Data curation, Methodology. **José Montalbán:** Conceptualization, Data curation, Methodology, Formal analysis, Visualization, Writing – original draft, Writing – review & editing.

## Data availability

The authors do not have permission to share data.

## Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.econedurev.2023.102384>.

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