Economics
Working Papers

2020-03

Effects of Busing on Test Scores and the Wellbeing of Bilingual Pupils: Resources Matter

Anna Piil Damm, Helena Skyt Nielsen, Elena Mattana and Benedicte Rouland

DEPARTMENT OF ECONOMICS
AND BUSINESS ECONOMICS
AARHUS UNIVERSITY

Effects of Busing on Test Scores and the Wellbeing of Bilingual Pupils: Resources Matter*

Anna Piil Damm	Elena Mattana	Helena Skyt Nielsen	Benedicte Rouland
Aarhus University	Aarhus University	Aarhus University	University of Nantes
apd@econ.au.dk	emattana@econ.au.dk	hnielsen@econ.au.dk	benedicte.rouland@univ-nantes.fr

May 2020

Abstract: We exploit exogenous variation from a school desegregation policy to evaluate the impact of forced busing on bilingual school starters. The policy moved pupils from schools with many Danish as additional language (DAL) pupils and high per-pupil spending to schools with fewer DAL pupils but lower per-pupil spending. Assignment to busing may be regarded as exogenous conditional on three observed individual characteristics. In contrast to the literature on voluntary busing to promote racial school integration, we find that assignment to forced busing has a negative effect on the academic performance and wellbeing of DAL pupils. Our investigation of potential mechanisms shows that bused pupils attend schools with a lower budget per pupil and a lower overall budget for DAL pupils, have a lower enrollment rate in the after-school program in the assigned school, and are more likely to transfer to another public school (after regaining free school choice). Our results suggest that school resources can more than compensate for potential negative peer effects in schools with moderate levels of segregation.

Keywords: School Segregation; Integration; Immigration; Education; Peer Effects; School Resources

JEL codes: I21; I24; I28; J15; R23; R28

---

* We acknowledge financial support from TrygFonden and helpful comments from Peter Fredriksson, Mikkel Aagaard Houmark, Victor Ronda, Timo Hener, Andreas Bjerre-Nielsen and Miriam Genzowski. We are grateful to seminar participants at Hamburg University, Padova University, Statistics Norway, Aarhus University, and conference participants at UEA 2019, IWAEE 2018, IZA World Labour Conference 2018, ESPE 2018, EALE 2018, and Danish Economic Society 2020 for useful comments and suggestions. Finally, we thank Villiam Vellev and Camilla Hedegaard Hansen for research assistance and employees from Aarhus Municipality, Children & Youth, notably Lone Nielsen, for sharing their knowledge about the local institutions with us. Damm, Mattana, and Nielsen: Department of Economics and Business Economics and Trygfonden's Centre for Child Research, Aarhus University, Fuglesangs Allé 4, DK-8210 Aarhus V. Rouland: Department of Economics and Management, University of Nantes, Chemin de la Censive du Tertre, BP52231, 44322 Nantes Cedex 3, France. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

1. Introduction

The high recent inflows of immigrants and refugees in Western countries have created an urgent need for policies improving integration. School desegregation policies, such as busing, vouchers, or rezoning, have been an integral part of policymakers' strategies to improve the academic achievements of minority children by means of a heterogeneous socio-economic mix in school. This paper presents new empirical evidence on how school desegregation initiatives influence the academic achievement and wellbeing of the minority children directly impacted by the policies. We examine forced busing of Danish as additional language (DAL) pupils to schools with fewer DAL children but lower per-pupil spending and we show that school resources can compensate for potentially adverse peer effects.

There is a large literature investigating the effect of the US racial school desegregation policies that took place from the 1960s in the wake of Brown vs. Board of Education and the eventual disbandment of some of these policies, which started in the early 1990s (Cascio et al. 2008). This literature finds that school desegregation leads to higher academic achievement for the black minority students, whereas the effects for whites are mixed (Angrist and Lang 2004, Guryan 2004, Reber 2010, Lutz 2011, Billings et al. 2014, Bergman 2019, Johnson 2019). Recently, Bergman (2019) studies the effects of an ongoing voluntary inter-district school assignment program in Northern California on the academic achievements of black and Hispanic minority school starters. Outside of the US, few studies directly analyze the effects of school segregation (e.g. Rangvid 2010, Böhlmark et al. 2016), whereas many papers examine the effects of immigrant and/or ethnic minority peers in the classroom (Jensen and Würtz 2011, Geay et al. 2013, Ballatore et al. 2018).

We make several contributions to the literature. We are the first to study the effects of *forced* busing and thus avoid the common issue of positive selection into the programs (Cullen et al. 2006; Bergman 2019). Secondly, we are the first to study busing targeted at bilingual pupils. Thirdly, we are the first to be able to follow school entrants over time, which allows us to study a young population thought to be more receptive to school interventions (Cunha and Heckman 2007) and to characterize compliance with the policy over time. Finally, we observe detailed school spending. While US state grants given to school districts with more disadvantaged students reduce the inequality in per-pupil spending between school districts with high and low property values (Reber 2011, Jackson et al. 2016, Johnson 2019, Bergman 2019), in our Scandinavian context the local government school spending formula is such that the per-pupil spending in poor districts by far exceeds that of affluent districts. Therefore, we

study the effects of forced busing in the context of trade-off between exposure to native pupils and per-pupil spending.

We exploit the quasi-random assignment to schools of DAL pupils stemming from an ongoing school desegregation policy in Aarhus, Denmark. Every year, around 550 bilingual school starters (around 18% of all school starters) are exposed to mandatory language screening.[1] Pupils deemed to have limited Danish language proficiency (around two thirds) are assigned to either their local school, also referred to as the district school, or another school. Assignment is conditional on three characteristics which we observe in the administrative registers: special needs, siblings in district school, and distance to district school. We denote pupils assigned to another school as leaving the "sending school district" to attend school in the "receiving school district". The local authorities provide the pupils with free busing between home and the receiving school. We estimate the effect of busing on academic achievement and wellbeing, comparing bilingual pupils who are assigned to busing to those not assigned to busing while controlling for the determinants of school assignment. These causal estimates should be interpreted as intention-to-treat estimates, because children may delay school start or enroll in private school, and they may gain free school choice in later years.

Forced busing may affect the academic achievement of pupils through multiple channels. First, school resources are allocated in order to secure an even quality of education across school districts. Sending school districts collect more resources per pupil than receiving school districts due to their disadvantaged pupil body. While ample school resources do not necessarily imply better academic performance, they can be an important determinant of academic achievement if stable and used well.[2] In this case, ample resources combined with high shares of bilingual pupils in the sending schools may imply better teaching for bilingual pupils due to better school inputs combined with gains from specialization (e.g. higher overall budget for DAL support enabling the school to hire a trained DAL teacher). Second, receiving schools have a different peer composition than sending schools, both in terms of demographic characteristics and language proficiency. This has possibly conflicting effects: While bilingual pupils may benefit from having more classmates with native-level language proficiency, they might also be better off socially and academically in a classroom with more bilingual pupils

[1] Danish language screening is mandatory for school starters who speak another language than Danish at home. Such school starters are termed "bilingual" by Aarhus Municipality.

[2] See Hanushek (2006), Andersen and Mortensen (2010), Holmlund et al. (2010), Hægeland et al. (2012), Fredriksson et al. (2013) and Hyman (2017).

with whom they are more likely to form friendships due to shared common knowledge and everyday lives.[3] Third, busing may have almost mechanical detrimental effects on academic achievement due to the longer commute and a subsequent increase in school absence (Aucejo and Romano 2016). Finally, the policy implies the disruption of school life at school start and potentially later due to the possibility of returning to the local school. Hence, the effect of busing on academic achievement and wellbeing is ambiguous.

We find negative effects of forced busing on both academic achievement and wellbeing. First, we find that pupils who are assigned to busing have poorer test scores in math in grades 3 and 6 (0.17−0.18 of a standard deviation) and reading in grades 6 and 8 (0.13−0.25 of a standard deviation), with effects on reading being more pronounced among boys and effects on math being more pronounced among girls. Second, we show that forced busing negatively affects wellbeing in the first years of school; pupils assigned to busing report higher levels of distress in early grades (0.23 of a standard deviation).

Because busing implies moving to a district with lower resources and more language proficient peers, our results imply that school resources matter for the outcomes of bilingual pupils and that the negative effects of a lower per-pupil premium more than outweigh any potential positive peer effects from exposure to a higher share of native pupils. While Billings et al. (2014) argue that higher school resources can mitigate the negative effects of segregation, we show how school resources can more than compensate for them. In addition, forced busing leads to lower attendance in after-school programs in the assigned school. This casts doubts to the positive sign of peer effects: Bused pupils interact less with their class- and schoolmates and these interactions are more likely to be conflictual, as evidenced by higher distress.

The rest of the paper is organized as follows. Section 2 describes the institutional setting and background of the school desegregation policy in Aarhus Municipality. Section 3 presents our data, while Section 4 explains our empirical strategy. We present our results in Section 5. Section 6 discusses possible mechanisms, and Section 7 concludes, suggests avenues for future research and draws policy implications.

[3] For models of peer effects, see Sacerdote (2011) and Antecol et al. (2016). For examples of quasi-experimental studies, see Angrist and Lang (2004), Chin et al. (2013), Billings, Deming, and Rockoff (2014). For examples of Danish studies of peer effects of the academic performance of immigrant pupils, see Rangvid (2007, 2010) and Jensen and Rasmussen (2011).

2. Background of the school desegregation policy

2.1. Institutional background

At age four, the vast majority (98% in 2007[4]) of Danish children are enrolled in some form of subsidized public daycare.[5] For the children in our study, education was compulsory from the calendar year in which they turned six[6] until completing ninth grade. While pupils' parents can choose freely among public schools, given available slots at non-district schools, most choose the public school located in their school district (i.e. the district school).[7] Since 2005, municipalities are allowed to revoke free school choice from bilingual pupils requiring Danish language support. A bilingual child is one who does not speak Danish at home.[8] Aarhus Municipality was the first to implement a policy inspired by this law.

School starts with a one-year kindergarten class, henceforth referred to as grade zero, and ends with a compulsory school exit exam (around age 16). Compulsory education, as well as most post-compulsory education, is free of charge at public schools, whereas private schools charge tuition fees.[9] Pupils are divided into classes upon entering grade zero and typically remain in the same class until grade nine. While the maximum class size—regulated by national law—is 28 students, the average is 22 students, which is similar to other OECD countries (OECD 2016). In grade zero, pupils are taught by a grade teacher, whereas they are taught by subject-specific teachers from first to ninth grade.

Each public school offers an after-school program and youth clubs with activities guided by professionals and paraprofessionals. These services are available from grade zero to age 18 and usually located in the immediate vicinity of the school facilities. Attendance in after-school programs and youth clubs is high until ages 10–12, when children become more autonomous and opt out. The charges for these services are income-dependent but heavily subsidized.

---

[4] Source: statistikbanken.dk/PAS11 and /BRN9.
[5] A minimum of 67% of the expenses are covered by the local authorities (c.f. the Children's Act).
[6] Pre-2009 cohorts could opt out of pre-school programs, though, which became compulsory for the cohort starting school in 2009. Before 2009, average enrollment in the optional pre-school was 83% (2005 figures; UNI-C 2012).
[7] Each school district has one public school, referred to as the district school.
[8] Cf. article 5, Part 8 in the Public School Act, passed as Law no. 594 on the 24th of June 2005.
[9] The average annual tuition fee across the 18 private schools in Aarhus was USD 2,166 in 2015 (ranging from $891 to $3,745). Private schools receive a subsidy corresponding to 75% of public-school costs regardless of the ideological, religious, political, or ethnic motivation for their establishment, see eng.uvm.dk. Throughout the paper, we use the exchange rate 0.1485 DKK/USD (www.statistikbanken.dk/DNVALA for year 2016).

2.2. Aarhus Municipality busing policy

With a population of around 345,000, Aarhus is the second largest city in Denmark.[10] Danish cities in general and Aarhus in particular are characterized by a high residential concentration of immigrants and children of immigrants born in Denmark, hereafter referred to as "descendants," throughout its neighborhoods[11] and school districts. In 2005, the share of immigrants and descendants in Aarhus school districts ranged from 2–90%, the second-largest share being 62%, and has increased steadily since. Figure 1 shows the concentration of immigrants and descendants throughout Aarhus school districts in 2016. School districts with high shares of immigrants and descendants tend to be school districts with high shares of public housing and located around 5–6 kilometers from the city center.

[Figure 1. The share of immigrants and descendants across school districts in Aarhus, 2016]

The Aarhus Municipality policy to desegregate schools is aimed at reducing native flight from immigrant-dense schools to improve the academic performance of all bilingual pupils in public schools. A secondary goal of this policy is to obtain equal academic outcomes of bilingual pupils across school settings by means of compensatory resource allocation to schools with low shares of native pupils (Brøndum and Fliess 2009).

The policy consists of two main components: busing and school resources. Since August 2006, Aarhus has carried out a busing policy that involves bilingual school-starters, school-movers, and newcomers. All bilingual pupils about to enroll in an Aarhus school are required to take a language test and—if they test below a predetermined threshold—they lose their free school choice and the municipality assigns them to a school.

Every year, around 550 school starters (i.e. 18% of all school starters in the municipality) take the language test, which consists of three tasks designed to evaluate different aspects of a child's language skills. Specifically, the tasks test their vocabulary and level of language comprehension. The language test is administered by a person appointed by the municipality. While parents are allowed to be present while the test is administered, they are not allowed to talk. The test is subsequently scored by a central office in the administration of Aarhus Municipality to avoid manipulation by the adults present. Two thresholds separate three

[10] www.statistikbanken.dk/BY2.

[11] Calculating the residential concentration of immigrants and descendants from the ten largest source countries across residential neighborhoods (with on average 291 households) in Denmark over the 1986–2016 period, Damm, Hassani, and Schultz-Nielsen (2019a) find an average dissimilarity index of 45%, i.e. 55% of immigrants and descendants from these source countries should move to a different neighborhood in order to obtain the same distribution as the majority group.

categories of language proficiency. These thresholds have not changed over the years and depend on the age of the child (6-month increments).

Based on the test score, test-takers are assigned to one of three categories: F, S, and B. Pupils whose test scores exceed the threshold for having an adequate level of Danish language proficiency for that age maintain their free school choice (category F) and typically attend the district school. Pupils testing below this threshold get a school assignment (category S). These pupils are deemed to require language support, further broken down into low, medium, and high. They must attend the school they are assigned to[12] but can regain their free school choice by developing age-appropriate Danish language proficiency in later grades (as indicated by a later language assessment).[13,14] They also have the right to attend DAL classes after school. Finally, pupils whose test scores fall below the threshold for an even more substantial need for language support are referred to a basic DAL class (category B). For all children, private school is also an option. Annually, around 26% of pupils are assigned to category F, and 5% to category B. Around one-third of all category-S pupils are assigned to a school outside their school district, either due to a parental request for a vacant slot outside their district or assignment to forced busing.[15]

In particular, if the number of category-S pupils in grade zero in a school district exceeds 20%, the surplus is assigned to a receiving school outside the district and provided with free bus services between home and school.[16] In the remainder of the paper, we refer to pupils assigned to a receiving school district as "assigned to busing" and to the school district of residence as "sending school districts." The municipality selects the category-S pupils who are assigned to the district school while accounting for the following priority criteria: First priority goes to pupils with special needs or problems in the family. Second priority goes to pupils with siblings in the district school. Finally, the municipality has information about the age of older siblings in the district school and the distance from the district school of the pupils'

[12] Cat-S pupils can only request a slot in another public school if the share of Cat-S pupils in that grade is below 20%.
[13] The language proficiency of Cat-S pupils is assessed regularly using grade-appropriate assessment material.
[14] We do not use the duration until regaining free school choice as an outcome variable in our analysis, because Cat-S pupils referred to a receiving school have a stronger incentive to have their Danish language proficiency assessed in later grades in order to have the option to transfer to the district school.
[15] Since 2009, school referrals became less likely while the share of pupils granted free school choice increased (see Figure A1 in the Appendix). Possible explanations include compositional effects caused by more restrictive asylum and family-immigration laws in Denmark since 2002 and increased resources for DAL support to bilingual children in pre-school programs in Aarhus during our observation period. Moreover, one school, Ellekær School, has been exempt from the 20% rule since 2015.
[16] The municipality lists which schools outside the school district Cat-S pupils can be referred to, according to the school district they reside in. The school bus runs from the district school to the receiving schools.

residence, which they might take into account. Additionally, when deciding the exact school of assignment, the municipality also considers the assignment of other category-S pupils from the neighborhood of residence in order to gather them at one - or at least few - schools.

In 2006, in addition to the school assignment policy, Aarhus Municipality instituted two new types of schools: full-day schools and magnet schools. Two school districts with more than 40% immigrants and descendants were converted to full-day schools, i.e. public schools requiring pupils to attend school for the entire day (8 am to 4 pm), rather than 8 am to 2 pm, which is the norm in Denmark. Full-day schools do not follow the 20% rule for the busing policy and implement busing only on a voluntary basis, and for this reason we exclude them from our study. Four public schools located in districts with high shares of bilingual pupils were declared magnet schools and were allocated substantially higher funding per pupil with the purpose to attract and retain local pupils and improve school quality.[17]

[Figure 2. Map of sending and receiving school districts in Aarhus Municipality, 2007–16]

Figure 2 is a map of the school districts in Aarhus Municipality in 2016 and illustrates the three types of school districts: sending, receiving, and neither sending nor receiving. In 2016, there were 11 sending school districts, 23 receiving districts and 12 districts neither sending nor receiving. Comparison of Figure 2 with Figure 1 reveals that school districts with high concentrations of immigrants and descendants are sending school districts (except for the two school districts with the highest concentrations, which have full-day schools instead and are classified as neither sending nor receiving).

Aarhus Municipality allocates resources to regular classes in public schools on the basis of the number of pupils in each grade. The specific rate per pupil in a regular class depends on the grade because the legal requirements of minimum coursework are different for each grade. Resources target specific tasks, such as regular education, education for pupils with special needs, and physical school facilities. On top of that, schools receive resources for additional educational and pedagogical activities, such as support centers. Of these additional resources, 60% is allocated based on the number of pupils and the remaining 40% is allocated using a statistical model including three indicators of the adult population in each district: income, level of education, and employment status. Magnet schools receive further additional resources that are allocated based on their historical pupil share.

[17] Teaching in magnet schools is planned with special focus on interculturalism, targeted teaching, social skills, school-parent collaboration, music, and other creative subjects (Brøndum and Fliess 2009).

Using the school budgets for 2014 as an example, Figure 3 shows the budget per pupil in regular classes in Aarhus public schools. The average budget per pupil in regular classes increased by grade (1–3, 4–6, and 7–9) and school type (regular schools, regular schools with more than 20% DAL pupils and magnet schools). For example, the per-pupil budget was around $6,000 in grades 4–6 in regular schools. Magnet schools on average received a budget premium per pupil of 20–23%, depending on the grade.

[Figure 3. Budget per pupil in regular classes and budget premium for DAL pupils. 2014]

Schools receive additional resources to accommodate the needs and requirements of DAL pupils. The specific rule for resource allocation to different DAL activities follows a point system, where a point corresponds to a given rate (e.g. $779 in 2016). The school receives 0.75 points per DAL pupil plus additional points for language-tested DAL pupils in grades 0–3, according to their category of DAL support need (B, S, or F). Additionally, schools with more than 20% DAL pupils receive "task-specific resources" to facilitate school–parent cooperation. The total annual budget for "task-specific resources" is allocated between these schools based on the school's overall share of DAL pupils.

The last columns in Figure 3 illustrate the importance of the additional DAL funding. The DAL budget is sizable: It was on average $832 per pupil in schools with at least 20% DAL pupils and slightly higher in regular schools with fewer than 20% DAL pupils. Moreover, schools with more than 20% DAL pupils on average received an extra per-pupil premium of $356. Even though the municipality distributes resources to schools for specific purposes, the school principals have the autonomy to spend the budget as they see fit. See Appendix B for further details on the Aarhus Municipality guidelines for the allocation of school resources.

Relatedly, Aarhus Municipality closed the schools in the Frydenlund and Nordgaard school districts in 2008, which had very high shares of bilingual pupils. We drop pupils living in these two districts from the analysis. While pupils who were bused from those two schools in 2007 were unaffected by the ex-post school disruption generated by the closure, pupils who attended those schools were redistributed across other school districts. Specifically, pupils were redistributed to receiving schools or schools with characteristics similar to the receiving schools and an extra per-pupil budget of around $1,200 was allocated to the schools that took them in. In practice, bused pupils experienced ex-ante school disruption while their non-bused counterparts experienced ex-post school disruption but similar school characteristics. As a consequence, we lack a proper control group for pupils assigned to busing from these districts.

Furthermore, after the year of the test, compliance for pupils from these two districts who were assigned to the district school cannot be correctly defined.

3. Data

3.1. Data sources

Our micro data stems from five sources: national administrative registers, administrative registers and school budget data from Aarhus Municipality, national education data collected by public schools, an online database from the Ministry of Education, Children and Youth, and the neighborhood data set constructed by Damm, Hassani, and Schultz-Nielsen (2019b).[18]

The national administrative registers provide detailed information on the school district of residence, daycare attendance and individual demographic characteristics of children and their parents (e.g. age, country of origin, immigrant status, date of immigration, and marital and residence status). For parents, we also have information on education level, income, and employment status.

From the pupil register for Aarhus Municipality (2007–17) and the national administrative education register, we obtain detailed information about which school, grade, and class the pupils attend every year. From the after-school programs register for Aarhus Municipality (2007–15), we obtain information on the school at which the pupil attends after-school programs. The Aarhus language test register contains detailed information on all language tests administered between 2006 and 2017, including the test date, scores in each task, final overall score, and assignment to a school.

National education data collected by public schools includes data on national test scores, absentee rates, and wellbeing. The national test register (2010–19) contains information on the pupils' test scores on the national test in reading, math, English and science.[19] The school absence register (2011–19) has information on the number of days of absence during the school year and the total number of active school days by school year. The Danish wellbeing survey (2015–19) is an annual survey among the population of pupils in public schools and contains answers to a range of questions about their wellbeing in school.

[18] See Table A1 in the Appendix for detailed definitions and the data sources of all variables.
[19] Our data access to the national test register in 2019 for this study excludes access to test scores in English and science.

Our measures of characteristics of public schools in Aarhus stem from an online database maintained by the Ministry of Education, Children and Youth, available from 2012. The database includes the share of lessons with qualified staff by subject (e.g. language taught by a teacher who has specialized in languages) and grade, pupils per teacher, age composition of teachers, annual number of language (math) lessons, and school size (in terms of number of pupils). School budget information (overall and by sub-items) from Aarhus Municipality for 2014–16 allows us to calculate the per-pupil budget for pupils in regular classes across schools and the budget premium for DAL support for bilingual pupils in regular classes by category of language support need.

Finally, we obtain information about the individual's (micro-) neighborhood of residence from the data set constructed by Damm et al. (2019b), available from 1986 until 2016.

3.2. Sample selection and description

In Aarhus Municipality, 33,106 children start school between school years 2007/08 and 2016/17. Of these, 5,996 school starters have been screened in Aarhus between 2007 and 2016 and are alive in 2017. We exclude 156 pupils who were considered unfit for busing, as these pupils are all referred to the district school. We drop 17 children who took the language test after age 7.[20] Finally, we drop 141 pupils who are assigned to either private or special needs schools.

We focus on the remaining 3,403 category-S pupils. We further restrict the sample to include only school starters living in a school district that (i) does not have a full-day school (2,631 school starters) and (ii) is a sending school in the year of school start. This reduces our sample to 1,431 school starters. We exclude school starters whose parents expressed a school preference before assignment, because expressing a school preference may influence school assignment or compliance. Finally, we exclude school starters who lived in the Nordgaard and Frydenlund school districts in 2007 and a small number of school starters for whom we lack information on the neighborhood of residence at the time of language screening.[21] In the end, our sample consists of 954 school starters.[22]

[20] These children are older than the official school-start age and are likely to be either new immigrants or negatively selected.
[21] The families of these children moved to Aarhus Municipality during 2016 in the calendar year that the child turned 6. Since information about the individual's neighborhood of residence is only available until the beginning of 2016, their neighborhood of destination is unknown.
[22] See Table A2 in the Appendix for a description of each step in the sample selection procedure.

In Table 1, we report sample characteristics for the final sample and by school assignment status (assigned vs. not assigned to busing). According to Panel A, the majority of children are either immigrants (8%), or descendants (85%); 86% originate from non-Western countries, and almost half of them are of middle-Eastern origin. These children come from large families (the average number of siblings is over three in the year the child turns 4), and only 70% live in a nuclear family.[23]

We measure parental characteristics in the year the child turns 4 and report them in Panel B. Compared to fathers, mothers tend to be 5 years younger, less likely to be employed (26% vs. 50%), and more likely to be out of the labor force (64% vs. 36%). Overall, 44% of pupils have both parents not employed. The real disposable income of each parent is low relative to the school district average (see Table 2, Panel A). When compared to the distribution of disposable income of working-age immigrants in Aarhus Municipality, 16% of mothers and 27% of fathers are in the lowest quartile, while 19% of mothers and 16% of fathers are in the highest quartile. Parental education level, when known, is relatively low; 31% of mothers and 25% of fathers did not complete high school, while 20% of mothers and 18% of fathers have tertiary education.[24]

[Table 1. Sample characteristics: individuals and parents]

3.3 The school assignment policy

In Table 1, Panel C, we describe how the policy is applied in our sample of category-S pupils. First, according to their test score, school starters are divided into levels of language support need: 13% have strong need, 43% medium need, and 43% low need.

Approximately 50% of the pupils in the sample are assigned to busing. In accordance with the municipality determinants of school assignment, pupils assigned to the district school are more likely than pupils assigned to busing to have a sibling attending the district school (65% vs. 19%) and have a lower age difference to the youngest older sibling at the district school (3 vs. 5 years). We calculate distance to school by computing the distance from the neighborhood of residence to the district school and the school of assignment (both in kilometers). Pupils in our sample on average live 850 meters from the district school. Pupils assigned to busing must travel on average 7 km each way, which can take up to 25 minutes.

[23] In 2012, 84% of children aged 4 in Denmark lived in a nuclear family, defined as a household with two adults who are married, registered partners, or cohabiting (www.statistikbanken.dk/FAM111N).

[24] Individuals having no education information in the registers means that either they have not completed an education at all (in Denmark or in the country of origin) or they have not reported it in Statistics Denmark surveys.

Receiving schools distribute category-S school starters across classes. On average, pupils assigned to busing who enroll in grade zero in a public school are in class with two other category-S pupils, whereas category-S school starters assigned to the district school attend a class with four other category-S pupils.

[Table 2. Sample characteristics: school districts of residence and assignment]

The children in our sample reside in 10 school districts and are assigned to 43 different school districts, either the district school or a receiving school. In Table 2, we report average characteristics of the school districts by type across relevant years: sending district with a magnet school (3), sending district without a magnet school (7), and receiving districts (33).[25] On average, the share of immigrants and descendants is substantially higher in sending districts with magnet schools (50%) compared to sending districts without a magnet school (24%) and receiving districts (10%). Residents in the sending districts also have lower socio-economic status (SES): The average employment rate of adults is 61% (74%) in sending districts with (without) a magnet school compared to 84% in receiving districts. The average real annual disposable income of adults is $30,302 ($32,956) in sending districts with (without) a magnet school, below the national average of $38,852[26] and below the receiving districts average of $41,702.[27]

In general, school starters living in sending districts are less likely to enroll in the district school compared to school starters living in receiving school districts. This is even more pronounced for districts with magnet schools (34% compared to 58% in sending districts with non-magnet schools and 78% in receiving districts).

Sending schools have on average higher shares of category-S pupils (29% in magnet schools, 20% in non-magnet schools), whereas receiving schools have much lower shares (14%).[28] This is consistent with the 20% rule for school assignment. Across school types, the overall school share of DAL pupils is substantially higher than 20%, as it also includes

[25] Sending and receiving districts do not overlap over the years and none of the receiving districts has a magnet school.

[26] Average for population aged 25–54 in 2016.

[27] School starters in the sample are not evenly distributed across the school district, tending to live in areas with higher shares of immigrants and non-Western residents, lower employment and socio-economic status. An analysis of the micro-neighborhoods obtained from Damm, Hassani, and Schultz-Nielsen (2019b) reveals that children in the sample live in 35% of the neighborhoods contained in the school district. There is a lot of overlap between the neighborhoods where pupils assigned to busing and pupils assigned to the district school live and the characteristics of the residence neighborhoods are very similar across the two groups.

[28] These shares refer to the share of Cat-S pupils at the start of grade 0. For magnet schools, the average share exceeds 20%. In fact, it only exceeds 20% for the sending magnet school Ellekærskolen. This is because Ellekærskolen has been exempted from the 20% rule since the school year 2013/2014 and possibly also because of flight of Danish pupils between class formation and school start.

bilingual pupils with free school choice as well as bilingual pupils enrolled before the start of the policy or after grade zero.

Sending schools, in particular magnet schools, have fewer pupils than receiving schools and smaller class sizes: On average, magnet schools have 219 fewer pupils than receiving schools and a class size of 16 compared to 20 in receiving schools. Magnet schools have a substantially higher annual budget per pupil in regular classes. The average in years 2014–16 was $6,978 compared to $5,698 in regular sending schools and $5,514 in receiving schools. The total DAL budget of sending schools was on average about four times that of receiving schools. Furthermore, it implied a per-pupil budget premium for DAL pupils of 18% in magnet schools, 21% in non-magnet sending schools, and 16% in receiving schools. The higher per-pupil budget of magnet schools enables them to have fewer pupils per teacher than other sending schools (10 vs. 13), slightly older teachers (46 vs. 44), and more classes taught by qualified staff (79% vs. 75%). In receiving districts, schools have on average 10 pupils per teacher, teachers are on average 44 years old, and 75% of classes are taught by qualified staff.

Finally, we report class averages of the standardized national test scores (by grade and subject, mean 0, std. dev. 1) for the different types of district schools. Pupils in sending schools perform between one-third and two-thirds of a standard deviation worse than those in receiving schools, with pupils in magnet schools performing substantially worse.

3.4 Outcome variables

The outcomes we use to understand the effects of busing on children are: (i) national tests scores, (ii) answers to a wellbeing survey of all public-school pupils, and (iii) other outcomes, including school absentee rates and enrollment in after-school programs.

[Table 3. Sample characteristics: outcomes]

3.4.1 National tests

We use national test scores as measures of pupil achievement. Each spring since 2010, all public-school pupils are tested in reading, math, English and science. They take a reading test in grades 2, 4, 6, and 8, a math test in grades 3 and 6, an English test in grade 7, and natural science tests in grade 8. These national tests are designed to estimate the student's ability in three cognitive areas of each subject, and the algorithm alternates question testing in each of these three cognitive areas. For reading, the cognitive areas are language comprehension, decoding, and reading comprehension. For mathematics, the cognitive areas are numbers and

algebra, geometry, and applied mathematics. For English, the cognitive areas are reading comprehension, vocabulary, and language and linguistic usages. There are three separate tests in natural sciences: biology, geography, physics and chemistry. For biology, the cognitive areas are the living organism, living organisms' interactions, and applied biology. For geography, the cognitive areas are natural, cultural and applied geography. Finally, for physics and chemistry, the cognitive areas are energy, phenomena, substances and materials, and applications and perspectives.

The tests are IT-based, self-scoring, and adaptive: Instead of giving all pupils the same questions and summing the number of correct answers, an algorithm estimates an ability measure after each question and then finds a next question with a difficulty level that matches the current measure of the student's ability. Thus, the final ability estimates are not a function of the number of correct answers but rather a function of the difficulty level of the questions and the ability of the student.[29, 30]

While the tests are compulsory for all students enrolled in public schools, principals may exempt some students from the tests. From Table 3, we see that 91–98% of public-school students in our sample take the tests in reading and math in the relevant years, meaning that 2–9% of all students are exempt from the test. The share of test-takers in our sample is close to that for Aarhus public-school pupils, which is 95%. However, the share of test-takers in Aarhus is lower among immigrants (88%), low SES pupils (90%), pupils who did not take the test in past years (79%), and—conditional on taking the test—on achievement in past tests (96% vs. 98% for those who scored at the bottom vs. the top of their school in the past test). Moreover, although the share of pupils taking the test is higher in receiving schools than in sending schools, the opposite is true for bilingual pupils. Hence, we check whether assignment to busing affects whether the pupil takes the test or not.

To calculate the average student ability scores, we first standardize the ability measures in the population within year, grade, subject and cognitive area (mean 0, std. dev. 1); we then sum the standardized measures for the three cognitive areas in each subject; finally, we standardize the final measures in the population (mean 0, std. dev. 1). In Table 3, we see that the average test scores for individuals in our sample are well below the national mean, ranging

[29] For details on the national tests, see Beuchert and Nandrup (2018).

[30] The national tests are supposed to have a pedagogical purpose rather than an accountability purpose. Thus, the main purpose of the tests is to give feedback to teachers, students and parents regarding the individual child's ability level. The teacher can assist academically weak students or provide them with aids or breaks during tests. Unfortunately, information on assistance, aid or other provisions made for these students is unavailable to researchers.

from an average of –0.654 in the reading test across grades 2, 4, 6 and 8, to –0.344 in science in grade 8.

3.4.2 Wellbeing

To assess the wellbeing of the pupils in our sample, we use the Danish wellbeing survey, administered since 2015 to all public-school students. The survey is administered by a designated teacher during class between January and April of every year.[31] Pupils in grades 0–3 and 4–9 receive different surveys.[32]

Even considering that only public-school pupils take the survey, there is attrition in survey taking. Although questions can be read aloud if the students have difficulties reading them, the designated teachers can decide if students with special needs are able to take the survey or if they will be exempted (see Andersen et al. forthcoming). In Table 3, we see that 86% of our sample take the 0–3 survey and 83% take the 4–9 survey, which is the same as the overall participation rate in Aarhus public schools. Pupils assigned to busing are less likely to take the survey than their non-bused counterparts (83% and 79% vs. 88% and 87%). Hence, we check if assignment to busing affects whether the pupil takes the survey.

We focus on the survey answers by the younger pupils, as they are closer to the date of our treatment. We run an exploratory factor analysis and find that two factors explain most of the variation in the data. We run a confirmatory factor analysis keeping the survey questions with factor loadings above 0.5 and controlling for grade, year of the survey, age and sex of the child. We present the questions associated with the two factors and the corresponding factor loadings from this confirmatory analysis in Table 4. The first factor is associated with questions about happiness with the school/class/lessons and about how nice it is to be in class (both in relation to the physical classroom and the teachers). We call this first factor school satisfaction. The second factor is associated with questions assessing the level of distress or uneasiness of the child: loneliness, somatization through headaches or stomachaches, teasing by other children, and disruption in the classroom. We call this second factor distress. In Table 3, we see that, on average, pupils in grades 0–3 who are not assigned to busing report higher levels of school satisfaction and lower levels of distress than those assigned to busing.[33]

[31] While the teacher tells the students that the purpose of the survey is to improve the wellbeing of everyone at school, he/she stresses that their responses will not be shown to their parents, teachers or anybody else in the school. For details on the wellbeing survey, see Andersen et al. (forthcoming).

[32] Younger pupils respond to 20 questions, each with three possible answers, focusing on the happiness of the pupil with the school, teachers and classmates, and eventual social isolation. Older students respond to 40 questions with five possible answers, ranging from happiness in school to the student perception of their academic achievement.

[33] A full list of the survey questions (including an English translation) can be found in Table A3 in the Appendix.

[Table 4. Factor analysis: relevant wellbeing survey questions]

3.4.3 Additional outcomes

We also analyze the effect of busing on school absences and enrollment in after-school programs. Our measure of school absences is the share of days of absence during the school year over the total number of active school days, which on average is 200 days. In Table 3, we show that on average pupils in our sample are absent in 8% of active school days (approx. 15 days) in grade zero, and the average is similar across grades 0–4 (7%). Pupils assigned to busing tend to have more absenteeism than those not assigned to busing, especially in grade zero (around 10% vs. 6%).

Finally, public-school pupils may attend after-school programs either in the school they attend or in the school of their school district of residence (if different). The school buses leave the receiving schools to go back to the school district of residence both after the normal school hours and after the after-school activities, allowing children to choose. In Table 3, we show that 82% of grade-zero pupils enroll in an after-school program, while 77% attend the after-school program in the attended school. The enrollment rates are similar for grades 0–4.

4. Empirical strategy

4.1. Identification

Whether a pupil requiring language support is assigned to busing or not is exogenous, once we account for the three observed determinants: special needs, siblings in district school and distance to district school. We exclude all pupils marked in the registers as having had a special needs assessment because children with special needs are assigned to their district school. To account for the other observed characteristics, we condition on having siblings already enrolled in the district school, age difference with the youngest of those siblings, and distance to the district school. Finally, we condition on year-by-school district of residence because the probability of being assigned to busing depends on the number of category-S pupils and the overall number of grade-0 school starters in the district school in the relevant year.[34] Only as many pupils are bused as those who would bring the share of DAL pupils in the district school above 20%.

[34] Reduced (increased) native flight from the district school would increase (decrease) the probability of Cat-S pupils being assigned to the district school.

Let y_{igtr} be the outcome of interest in grade g. Let b_{itr} be a dummy that takes value 1 if child i, who resided in school district r at school start and who took the language test in year t, is assigned to busing, and 0 otherwise. Let Z_{itr} be the vector of known determinants of b_{itr}, and η_{tr} the year-by-school district fixed effects, and ε_{igtr} the error term. We include year-by-school district of residence fixed effects to account for within-school-district variation in the probability of treatment due to variation in the share of category-S pupils over time. Thus, we estimate the effect of busing in grade g by comparing outcomes of category-S pupils who are assigned versus not assigned to busing and lived in the same school district and took the language test in the same year.

Since we observe each outcome across several grades, we estimate a panel data model using pooled individual data across grades (years since language test) for each outcome. Our main specifications allow both the level and effects of assignment to busing to differ across grades. We estimate the effects of forced busing on wellbeing, school absentee rates and enrollment in after-school programs using this specification:

$$y_{igtr} = \alpha_g b_{itr} + \gamma_g + \beta Z_{itr} + \eta_{tr} + \varepsilon_{igtr} \tag{1}$$

where γ_g denotes grade fixed effects, and α_g is our parameter of interest, which allows the treatment effect to vary over grades. Given our identifying assumption that treatment is exogenous, conditional on the assignment mechanism, $\hat{\alpha}_g$ gives the intent-to-treat estimate of forced busing on the outcome by grade.

Our panel data model for estimation of the effects of assignment to busing on national test scores allows the level and effects of treatment to differ by grade and subject, extending Eq. (1) to this model:

$$y_{igstr} = \alpha_{gs} b_{istr} + \gamma_g + \sigma_s + \beta Z_{itr} + \eta_{tr} + \epsilon_{igstr}, \tag{2}$$

where σ_s denotes subject fixed effects, and α_{gs} is our parameter of main interest, which allows the treatment effect to vary over grades and subjects. ϵ_{igstr} is the error term. We interpret $\hat{\alpha}_{gs}$ as the grade-and-subject-specific intent-to-treat estimate of forced busing.

To increase the efficiency of $\hat{\alpha}_g$ and $\hat{\alpha}_{gs}$, we also report the results of a second specification in which we add a set of individual and parental controls to the main specification. Individual characteristics include the pupil's age on the day of the test and dummies for the assessed level of language support needed, gender, immigrant status (immigrant, descendant of immigrants or descendant of descendants of immigrants), area of origin (Africa, East Asia, Middle East or Europe, America, and Oceania), having ever attended daycare, number of siblings (capped at 7), living arrangements (child lives in a two-parent household), and parents

missing from the registers. Parental characteristics are recorded when the child is 4 years old and include dummies for immigration status (immigrant or descendant), highest achieved education (high school dropout, high school graduate, college graduate, or education not reported), employment status (employed, unemployed or out of the labor force), real disposable income (four quartiles), and age group (below 25, 25–29, 30–34, 35–39, over 40). We cluster standard errors at the family level, since treatment is dependent on having siblings in the district school.[35]

In order to test the validity of our estimation strategy, we investigate whether the known determinants of the treatment affect school assignment as expected and whether any other individual and family characteristics affect school assignment. The first column in Table 5 shows the results of a regression of being assigned to busing on the known determinants of assignment to busing and a full set of year-by-school district dummies that capture the time-invariant school district-year characteristics that affect the probability of being bused. In columns (2–4), we gradually add individual and parental characteristics. In all specifications, we report standard errors clustered at the family level.

Consistent with the priority criteria, having at least one sibling who attends the district school significantly decreases the probability of assignment to busing, while the age difference to the youngest sibling at the district school and distance to the district school significantly increase the probability of assignment to busing. Importantly, assignment to busing is neither affected by other individual characteristics (e.g. the category of language support need, age on the test day) nor parental characteristics (Table A4 in the Appendix). A joint F-test cannot reject that these additional controls do not predict the treatment; the p-value is 0.86.

[Table 5. Determinants of treatment]

Importantly, Table 5 shows that assignment does not depend on the category of language support need, which derives directly from the language test score.[36] Had the test score in the language test influenced the assignment decision, treatment would have been selected on a potential predictor of our main outcome. That could have raised two concerns about identification. First, that pupils in the treatment and control group also differ in terms of unobserved abilities, i.e. that parents of higher ability children could have manipulated the treatment decision. Second, that cat-S pupils who at the time of school assignment have an older sibling attending the district school were selected on ability, assuming that siblings'

[35] The standard errors are virtually unaffected by whether we cluster by person ID instead of family ID.
[36] This result holds if we use the total points on the language test, both total and split by tasks.

abilities are correlated. However, as shown in Table 5, the coefficients on the categories of language support need are very small and insignificant, confirming that these concerns are unfounded. [37] Moreover, in our effect evaluation we control for the potential determinants of treatment including language test year by school district of residence fixed effects (specification 1) and also report the results of a second specification in which we in addition control for the category of language support need and a wide range of parental background characteristics.

4.2. Policy compliance

While in grade zero category-S pupils can only attend the public school they are assigned to, they have other options available. In this section we show that policy compliance is incomplete, which implies that the results presented in Section 5 should be interpreted as intention-to-treat estimates of the effect of assignment to busing. We define compliance as being enrolled in the school to which the municipality assigned the pupil by the end of August of the relevant year.

In Figure 4, we show the raw compliance probabilities by treatment status (assigned to busing or to the district school) and years from the language test. Compliance is substantially higher for the non-treated: Pupils assigned to busing are less likely to attend the school to which they are assigned. In the year of the test, 75% of pupils assigned to busing attend the assigned school against 89% of pupils assigned to the district school. For both treated and non-treated, compliance is high in the year of the test, and decreases progressively over the years. By the sixth year after the test, compliance is down to 32% for pupils assigned to busing and 64% for pupils assigned to the district school. In the test year, non-compliance can be achieved in two main ways: delaying school entry or enrolling in private school.[38] In the years after the test, pupils can avoid school assignment by enrolling in a different school than the school of assignment, either private or – in specific cases – public. Category-S pupils can regain their free school choice if a later assessment of their Danish language proficiency shows that they have obtained an age-appropriate level of proficiency and then choose whether to stay in the

[37] Table A4 in the Appendix presents coefficients for all the covariates in Table 5. We run sensitivity checks on our identification strategy: (i) adding neighborhood characteristics as controls, (ii) allowing the effect of categories for level of language support to be different between the subgroup of children who do not have an older sibling who has been language tested (including first-borns) and the remaining subgroup of children. Check (i) shows that neighborhood characteristics do not affect assignment to schools. Check (ii) shows that category of language support did not influence assignment to treatment of children who have an older sibling who has been language tested.

[38] An additional 31 children in our sample attend a public school other than the assigned school: 20 of them move either outside of Aarhus (8 of them) or within Aarhus before school start (12), and attend a school in the district of destination.

assigned school or to transfer to another. Pupils moving to another school district or municipality can also transfer to a school in the new district. Treated pupils are more likely to transfer than pupils assigned to the district school. By year 6 after the language test, 15% of treated pupils have returned to the district school of residence in the year of the test, 17% are enrolled in a private school, and an additional 27% have transferred to another school than the district school. This includes pupils who moved to a new school district or municipality subsequent to the language test.[39] By year 6 after the language test, 43% of the treated pupils have regained free school choice and, of those, one third moved back to the district school, one third moved to another school, and another third stayed in the school of assignment.

[Figure 4. Compliance with the policy]

Figure 5 illustrates the extent to which assignment to busing causes non-compliance. In all six panels of Figure 5, we show the coefficient estimates from regressing the outcome on a dummy for assignment to busing for each school year since the year of the test until 6 years after the test, the known determinants of assignment to busing, and a full set of year–by–school district dummies to account for the different composition of students and availability of school choices between districts.

[Figure 5. Effect of assignment on compliance]

Figure 5.a shows the effect of assignment to busing on compliance. While the effect is zero in the year of the test, the effect becomes negative, significant, and increases numerically over time in the years after the test. This result suggests that pupils assigned to busing fight the policy by not attending the school of assignment.

In the rest of Figure 5, we present the results of our empirical specification on the different ways to achieve non-compliance. Figure 5.b shows the effect of assignment to busing on being enrolled in school by the end of August of the relevant year. We find that the children who are assigned to busing are 5 percentage points less likely to enroll in school in the year of the test.[40] This effect disappears the year after the test, indicating that these children delay school start.

[39] We analyze the characteristics of compliers by regressing a dummy for attending the assigned school over year-by-school district fixed effects and the covariates described in Section 4.1, separately by treatment and control status and year since the language test. The results (presented in Tables A5.a and A5.b in the Appendix) do not show any clear pattern other than around 20 percentage points higher compliance for control children with at least one sibling attending the district school in the year of the test.

[40] These children would either retake the test or have their test reassessed the year after, as discussed in Section 3.4.1.

Figures 5.c, 5.d, and 5.e show the effect of being assigned to busing on enrollment in private school, the district school, and another school, respectively, by the end of August conditional on enrollment in school. We find that being assigned to busing does not affect the decision to enroll in private school in the test year, conditional on school enrollment in that year. This is likely due to the fact that applications to private school are usually submitted before the language test and therefore would not depend on the test result. In order to control for this, we have excluded all children from our sample who are admitted to a private school before the language assessment, thereby signaling their pre-determined intention to not enroll in public school.[41] Moreover, Figure 5.c shows that, conditional on school enrollment in the year of the language test, assignment to busing does not affect the decision to attend private school in the years after the test.

Figure 5.d shows that, in the year of the test, being assigned to busing reduces the probability of attending the district school by more than 80 percentage points, although the percentage-point drop in the probability falls as the years pass. Six years after the test, the percentage-point drop in the probability of attending the district school due to an initial assignment to busing declines to 45. This indicates that a significant share of parents of children assigned to busing prefers the district school to the receiving school and, at some point after regaining free school choice, exercises their option to enroll their children in the district school.

Finally, Figure 5.e shows that being assigned to busing reduces the probability of attending another school that is neither the receiving nor the district school in the test year by 5 percentage points. Figure 5.f confirms this: being assigned to busing decreases the probability of transferring to a new school district or moving to another municipality in the test year by 7 percentage points. Moreover, Figure 5.e shows that being assigned to busing reduces the probability of attending another school that is neither the receiving nor the district school 3–5 years after the test.

Overall, Figure 5 confirms that being assigned to busing reduces compliance with the policy over time, both by increasing school delays and by increasing the probability that pupils return to the district school after regaining their free school choice.

[41] Because our outcomes are not collected for pupils in private schools, we restrict our analysis in Section 5 to public-school pupils. Figure 5.c confirms that this restriction does not bias our analysis.

5. Results

5.1. Academic achievement

In Table 6, we report the estimated effects of assignment to forced busing on the national test scores in reading, math, English and natural sciences by grade and conditional on taking the test (columns 3 and 4). Since the estimated effects on national test scores can only be given a causal interpretation if treatment does not affect the test taking probability, we report the effect of assignment to busing on taking the test in columns 1 and 2. The results of the main specification presented in Section 4.1 are shown in odd columns and the results with additional controls for individual and parental characteristics in even columns.

[Table 6: National test by grade and subject]

Overall, assignment to busing has a negative effect on test scores in reading and math between grades 2 and 8 with effects ranging between 0.02 and 0.25 of a standard deviation. Specifically, it significantly reduces test scores in math in grade 3 by around 0.17–0.18 of a standard deviation and reading in grade 8 by around 0.23–0.25 of a standard deviation. The remaining effects are of the same magnitude but imprecisely estimated.

While assignment to busing reduces the probability of taking the reading test in grades 2 and 4 by 5 and 3 percentage points, it does not affect test-taking in reading in grades 6 and 8, math, English and science. The effect on test-taking in reading in grades 2 and 4 could reduce the estimated effect on grade 2 and grade 4 test scores; we test this in Section 5.3.

[Table 7: National test by grade and subject. By gender, socioeconomic status and category of language level support]

It is often found that the impact of school resources is more important for certain groups, e.g. pupils low-SES backgrounds or boys. Therefore, we study if the effects of assignment to busing are different by sex, socio-economic status (SES), and language support need assessed before school start (see Table 7). We find that the overall picture described above holds: assignment to busing lowers math test scores in grade 3 and reading scores in grade 8 across gender, SES and language support need, although coefficients are estimated less precisely for some subgroups. In addition, some gender differences appear: In grade 6, assignment to busing significantly reduces the test score in reading for boys (only) and the test score in math for girls (only). In grade 7, assignment to busing significantly lowers the test score in English for girls (only). Finally, in grade 8, assignment to busing significantly reduces the test score in reading of girls. Table 7 also reveals some differences by parents' employment

status (SES): Although effects on test scores are never significantly different across SES, the point estimates are generally larger for pupils with low SES background. Furthermore, the detrimental effects of assignment to busing on taking the reading test in grades 2 and 4 are entirely driven by pupils with low SES background.

5.2 Wellbeing

Table 8 shows the effect of assignment to busing on school satisfaction and distress in school for pupils in grades 0 to 3, both overall (Panel A) and by grade (Panel B). In columns (1) and (2), we show the effect of assignment to busing on survey take-up without and with controls for individual and parental background characteristics.

While assignment to busing does not significantly reduce school satisfaction, it increases distress. The effects on distress are particularly strong in grades 0 and 3. This result implies that pupils assigned to busing are more likely to report feeling alone and upset, and to experience headaches and stomachaches while in school, so that their overall distress level is one quarter of a standard deviation lower than that of the pupils assigned to the district school. Increased distress may indicate social isolation in school and consequently affect academic achievement. When we turn to survey take-up, we find a negative effect on take-up in grade 1 but not overall.

In Table A6 in the Appendix, we show the effects of busing on wellbeing in school by sex, SES background and the level of language support need before school start. We find that assignment to busing increases distress, irrespective of gender, SES background, and the level of language support need. Moreover, we confirm that assignment to busing does not significantly affect school satisfaction, irrespective of gender and background.[42]

[Table 8. School Satisfaction and Distress]

5.3 Sensitivity checks

In this section, we discuss some robustness checks on our estimates. First, as we discuss in Section 5.1, while assignment to busing has a negative effect on test-taking in reading in grades 2 and 4, it has a negative effect on test scores in reading in later grades and on math (tested in grades 3 and 6). School principals have the option of exempting pupils for whom the test is not considered beneficial for their academic development. Consequently, there is persistent lower

[42] We report the estimated effects of assignment to busing on school satisfaction and distress in grades 4–9 in Table A7 in the Appendix. The effects are not significant at conventional significance levels.

test take-up among public-school pupils from low-SES families as well as immigrant pupils and special needs pupils (Andersen and Nielsen, forthcoming). Among those taking the test, pupils who scored at the bottom of the school in earlier grades are less likely to take it. Consistent with this pattern, our results in Table 7 show that assignment to busing only reduces test-taking in reading among pupils with low SES background. Hence, we believe that the most likely outcome for the non-takers would have been a below-average test score. In Table A8 in the Appendix, we show what the results of our main specification (Eq. 2) would have been if the non-takers had scored the fifth percentile of the distribution of test scores among the sample (columns 5 and 6). We also show the opposite case in which non-takers are assigned the 95th percentile instead (columns 7 and 8). We find that our main conclusions would not change if any of these two extreme cases were true. Similarly, as discussed in Section 5.2 assignment to busing increases distress but also has a negative effect on survey take-up in grade 1. As above, we compute bounds on our estimates and show them in Table A9 in the Appendix. We conclude that the effect on distress is robust to this test. Moreover, the overall effect of busing on school satisfaction, while non-significant, would still have a negative sign.

Because of the young age of the pupils in our sample, we do not observe test scores for all pupils in all grades. In Table A10 in the Appendix, we show the results of our main specification (Eq. 2) using a restricted sample of only the cohorts for which we observe all the national test results (language test in years 2007–10). The effects are at least as strong as for the full sample.[43]

6. Potential mechanisms

Bused pupils attend schools that are different in two main dimensions: peer composition and school resources. In Figure 6, we present the results of our empirical specification on a set of characteristics of the attended school over time. The four panels in Figure 6 show the impact of assignment to busing on the share of DAL pupils, the share of high-SES pupils, the per pupil budget, and the total DAL budget in the attended school over grades 0–6. These results tell us the extent to which forced busing affects the characteristics of the school that the pupils attend over time, which is useful knowledge for interpretation of the intention-to-treat estimates.

In grade zero, pupils assigned to busing attend schools with better majority language role models: The average share of DAL pupils is about 36 percentage points lower. On top of

[43] Our baseline results on the effects of assignment to busing on national test scores are also robust to excluding children who have an older sibling who has also been language tested. Results are available upon request.

that, the average share of employed parents of pupils in the attended school is 26 percentage points higher. School resources go in the opposite direction: On average, the per-pupil budget is $542 (around 14%) lower, and the total DAL budget is around $187,000 lower. Because of the incomplete compliance described above, by grade 6 these differences are about halved but still substantial. The higher resources are reflected in fewer pupils per teacher and better qualified and more experienced teachers (Table 2), which most likely has positive effects on academic achievement (Hanushek 2006, Hægeland et al. 2012, Holmlund et al. 2010, Fredriksson et al. 2013, Jackson et al. 2016, Hyman 2017). Overall, two potentially opposing effects of busing are at play: Lower school resources have a negative effect on academic achievement, whereas peers with a better command of the Danish language may counteract or reinforce this effect.

[Figure 6. The effect of busing on peer composition and resources of the attended school]

Our main results show negative effects of assignment to busing on test scores, which suggests that, in our context, the negative effect of lower resources is stronger than any potential positive effect of better language role models. Moreover, we find that assignment to busing increases distress and lowers enrollment in the after-school program in the assigned school, which casts doubt on the positive nature of peer effects for this population.

Recall that our investigation of possible heterogeneous effects of assignment to busing on academic achievement (Table 7) shows that children of parents who are not employed (low SES) are significantly more (negatively) affected than children with at least one employed parent (high SES). This result is in line with the literature on the importance of school resources for academic achievement, in particular for pupils from low-SES background (e.g. Holmlund et al. 2010, Jackson et al. 2016).

In the remainder of this section, we investigate and discuss the potential channels through which busing affects academic achievement and wellbeing in more detail.

6.1 School resources and gains from specialization

In order to further investigate the effect of school resources, we exploit the difference between magnet and non-magnet schools. As seen in Section 3.3, magnet schools have more DAL pupils and higher per-pupil premium. Figure 7 describes how pupil composition and per-pupil budget differ between pupils assigned versus not assigned to busing by type of sending school: magnet or non-magnet. Pupils living in a school district with a magnet school can either attend a school with higher resources per pupil and more DAL peers (the magnet school) or a school with

lower resources and fewer DAL peers if they are bused. For pupils living in a district without a magnet school, the difference between the sending and receiving schools is smaller, both in terms of resources and peers.

[Figure 7. The Impact of Busing on Peers and Resources of the Attended School. By Sending School District Type: Magnet versus Non-Magnet School.]

Table 9 shows the effects of assignment to busing on academic achievement, allowing for separate effects for pupils with different counterfactual (district school type): magnet school versus regular school. While the coefficients across school district types are not statistically different, we see that the detrimental impact on math test scores in grade 3 is driven by non-magnet schools, while the detrimental effect on reading scores in grade 8 is driven by magnet schools. However, the negative effects on test taking in the early grades are entirely driven by pupils assigned to busing from magnet school districts. Across grades 6-8, in absolute terms the point estimates of assignment to busing on reading, math and English for pupils living in a magnet-school district systematically exceed the point estimates for pupils living in a non-magnet school district by 0.1–0.3 of a standard deviation.

[Table 9. Effect of Assignment to Busing on National Test Score. By Grade, Subject and Type of Sending School District]

In a similar vein, Table 10 shows the effect of busing on school satisfaction and distress, allowing for separate effects for pupils with different counterfactual (district school type): magnet school versus regular school. The effect on distress persists for both types of schools; the coefficient estimate is higher for pupils who live in a regular school district than for pupils who live in a magnet school district but not significantly different. This result indicates that busing increases distress across school types, e.g. due to social isolation. It also suggests that the compensatory resources to magnet schools are insufficient to obtain the same level of wellbeing as at non-magnet schools. By contrast, for pupils assigned to busing from a magnet school district school satisfaction is one-fifth of a standard deviation lower, but imprecisely estimated. Rather than a sign of less happiness with teachers and classmates, the result may be due to the physical school facilities. For pupils living in magnet school districts, assignment to busing also reduces the probability of survey participation. Only if the most satisfied among the pupils assigned to busing are overrepresented among the non-respondents, can selection explain the lower school satisfaction among pupils bused from a magnet school district. For pupils living in a regular school district assignment to busing does not affect school satisfaction.

These results bolster our conclusion that school resources are important for the overall performance and indicate that school resources also influence school satisfaction.

[Table 10. Effect of Assignment to Busing on Wellbeing. By Grade, Subject and Type of Sending School District]

Having higher numbers of children with a specific need, such as DAL children, can lead to specialized teaching and economies of scale. Higher resources might amplify this effect: A survey experiment shows that teachers are less willing to accommodate pupils with non-Western origins if budgets are tight (Andersen and Guul, 2019). While sending schools have a higher total DAL budget than receiving schools because they have more DAL pupils, DAL budgets are generally considered well balanced by the school principals. Having more DAL pupils makes any intervention more cost effective, whereas having higher total DAL budgets allows for the implementation of multiple interventions, thereby achieving gains from specialization. For instance, just the average yearly salary of an extra teacher responsible for DAL teaching is $79,448, which would eat up the entire DAL budget of an average receiving school.[44]

More generally, receiving schools are mismatched to the needs and abilities of bilingual pupils. Furthermore, pupils assigned to busing are relatively lower in the test score distribution of their school (see Tables 2 and 3). An inferior rank position in the class may affect academic achievements by reducing the degree to which teachers teach to their level (Duflo et al. 2011) or by detrimental peer effects going through the individual's self-confidence, self-image, and academic aspirations (Sacerdote 2011; Antecol et al. 2016; Elsner and Isphording 2017). Consistent with the latter mechanism, we find that pupils assigned to busing experience higher distress.

6.2 Other potential mechanisms: social isolation and disruption

Another key channel through which busing changes the lives of children is through available social links. What really matters for peer effects to operate is the extent to which pupils interact socially with each other. It is well established that peers tend to sort according to the homophily principle; that is, social networks form within groups with similar abilities and demographic backgrounds (Carrell et al. 2012, Damm and Dustmann 2014). In particular, Gulløv (2010) and Jensen (2020) suggest that common language skills, common knowledge, and common everyday lives of children matter significantly when they choose friends. In other words,

[44] For details on budgets and priorities, see Aarhus Municipality (2019).

minority and majority children often self-segregate. Jensen and Vitus (forthcoming) report that children assigned to busing think of themselves as “guests” or “outsiders.” Thus, bilingual pupils attending receiving schools seem to form social networks with the other bused children and will be socially isolated from the other classmates. We find that assignment to busing increases pupil’s distress in school.

Another test of social isolation is attendance in after-school programs. If pupils attend the after-school program together with classmates, they are more likely to be integrated in the class. Bused pupils can decide to attend the after-school program of the school they attend or at their district school. We show the effect of assignment to busing on whether the pupils attend an after-school program (Figure 8a) and on attending an after-school program in the attended school (Figure 8b). While assignment to busing does not affect the probability of attending any after-school program, children assigned to busing are 13 and 10 percentage points less likely to attend the after-school program in the attended school in grades 0 and 1. This suggests that bused pupils interact less with their class- and schoolmates after school. Figures 8c and 8d show the effect of assignment to busing on whether pupils who attend an after-school program do so in the school to which they are assigned or the one in their district school, respectively. In grade zero, they are 13 percentage points less likely to attend the after-school program in the attended school. This negative pattern attenuates over the grades, and at the same time attendance in the after-school program in the district school increases. This suggests that a substantial share of bused children who attend after-school programs in early grades (Figure 8a) do so by going to their district school after-school program and, at some point, also switch to the district school for regular classes. The main take-away from Figure 8 is that we find that children assigned to busing are substantially less likely to attend the after-school program in the attended school in early grades, when after-school programs are more important in a child’s social life. These results, together with the increase in non-compliance with the policy over the grades, suggest detachment with the school of assignment. Bused pupils interact less with their class- and schoolmates and their interactions are more likely to be conflictual.

[Figure 8. Effect of Assignment to Busing on Enrollment in After-School Programs]

Moreover, busing can affect academic achievement through the act of having to take the bus every morning and evening. For example, pupils might miss the bus and consequently miss school, or they might suffer from extended time spent on the school bus. Figure 9 shows the effect of busing on the share of absent days over active school days by grade. Assignment to busing causes a small increase in school absences in grade zero, corresponding to about 2–

3 days of school. This is consistent with bused pupils missing the bus a few mornings in grade zero.[45] Clearly, such a small effect does not explain our main findings.

[Figure 9. The Effect of Assignment to Busing on School Absentee Rates: Share of Days of Absence over Active Days.]

Finally, bused children experience a higher level of disruption of their school lives than other children. Initially, they start school life with a set of peers who are completely different from their peers in daycare, who would otherwise be their natural primary interaction group. Another channel of disruption comes from the policy design: Category-S children can obtain free school choice by taking another language test. This can cause disruption in two ways: First, children who move back need to integrate in a new peer group in the district school (Beuchert et al. 2018; Chetty et al. 2016). Second, those who do not move back might lose their primary interaction groups if close peers obtain free school choice and move back to their district school (Jensen 2020). To rule out the effects being driven by disruption, we exclude non-compliers from the analysis. This analysis is only suggestive because returners are likely to be more resourceful, because regain of free school choice requires a level of age-appropriate Danish language proficiency. However, effects seem to be driven by compliers and not by returners (see Table A11). This suggests that the effect of social isolation at the receiving school dominates the disruption costs of transferring to the district school.

Summing up, our results suggest that busing increases distress across school types, at least in part due to social isolation and costs of disruption[46] and, though not statistically different, the impact of assignment to busing on academic achievement seems to be

[45] In an alternative specification, we add distance to the assigned school and an interaction term to the regressions reported in Tables 6 and 8. Because of our specification, we rely on variation in distance within district, which is limited due to the fact that pupils from one school district are bused to the same receiving schools. This addition strengthens the negative effect of assignment to busing on test scores. However, there is no additional effect of traveling longer distances to the assigned school. Time on the bus could affect wellbeing through social interaction with the other children on the bus. As long as children are more likely to interact with bilingual pupils with similar ethnic origins, the negative effect should be stronger if the pupil is part of the minority among the children on the bus. We find this not to be the case.

[46] An additional channel through which busing could affect academic performance and wellbeing of bused pupils is through effects on school-parent collaboration. We have tried to investigate this channel using parental school satisfaction surveys. However, in view of the low take-up rates (44% across grades 0–6) and a significantly lower take-up rate among parents of children assigned to busing from a magnet school (40%) than among parents of children assigned to a magnet school (56%), the answers of respondents are unlikely to be representative for parents of cat-S pupils in our sample. For cat-S pupils in our sample from a non-magnet school district, parents have similar survey take up rates across school assignment (49% among bused vs. 42 % among non-bused). Estimation of the effects of assignment to busing on parental satisfaction survey take-up across grades 0–6 yields a negative, but insignificant overall effect. Heterogeneity analyses show a significant and negative effect for parents of children living in a magnet school district and low-SES households. Results are available upon request.

systematically more detrimental along a range of dimensions, e.g. for pupils assigned to a school with substantially lower school resources and pupils with low SES background.

7. Conclusion

We use quasi-experimental variation from a school desegregation policy to evaluate the effects of the busing of bilingual school starters to a school outside of the school district. In particular, we examine how busing to schools with fewer children with Danish as additional language but lower school resources affect bilingual school starters requiring language support. We find that busing has negative effects on test scores and wellbeing. We discuss the importance of four different channels: school resources and gains from specialization, peer effects, the bus ride, and school disruption. We conclude that fewer resources outweigh the potential positive peer effects from better Danish-speaking peers; effects of commuting and disruption are of minor importance.

Our findings indicate that the current policy does not reach the stated goal of obtaining equal academic outcomes of bilingual pupils across school settings using a combination of a change in peer composition and compensatory school resource allocation to schools with a low share of native pupils. The policy assumes that there are positive peer effects from exposure to more native pupils, in which case it does not strike the right balance between school resources and peer composition. However, this assumption is questionable. In line with previous research, our results suggest that this relationship is complex. More research is required to shed light on this issue.

Previous research found positive effects of comparable programs in the US. Post-Brown versus Board of Education within-district desegregation plans led to increases in the resources of schools attended by black students, and disproportionally so, for school districts with larger black population shares (Cascio et al. 2008, Reber 2010, 2011). Reber (2010) shows how these districts experienced the highest increases in educational attainment while experiencing the lowest increases in exposure to whites. She concludes that the increase in school resources was more important than exposure to whites in the success of these school desegregation policies. Billings et al. (2014) also conclude that school resources can mitigate the racial and socio-economic inequalities caused by segregated schools. In comparison, we show that higher resources can more than compensate for the potential negative effects of segregation. Bergman (2019) finds positive effects on test scores in a context where Hispanic and black pupils can apply to attend a school in a more affluent school district, such as Palo

Alto and Menlo Park. Because peers and resources go hand in hand, this study is not informative about the potential trade-off between peers and resources.

More generally, our results indicate that policies that disproportionately allocate resources to disadvantaged groups might be more effective than policies aimed only at changing the peer groups in the classroom. However, the exact trade-off between input factors in the educational production function in the context of bilingual and/or minority pupils is unknown. Future research should investigate whether higher school budgets per se have a positive effect or whether returns on some school inputs are higher than others for this particular population.

A related question is whether bilingual pupils with weak native-language proficiency perform better under forced busing than in the absence of the policy. Our results do not lend themselves to answering this question. This is because, in the absence of forced busing, native flight from predominantly immigrant school districts may change the peer composition and other school inputs substantially. Thus, the answer would rely on out-of-sample prediction. One might compare the outcomes of cohorts of bilingual school starters pre- and post-forced busing policy. Due to severe data limitations (e.g. no information on host-country language proficiency of bilingual pupils prior to the policy) and simultaneous changes of policy instruments (e.g. school budgets and peer composition), however, this proves impossible.

While our results do not allow for such radical policy recommendations, they do indeed suggest that if the policy is retained in the future, receiving schools should be held accountable for their use of budgets for DAL pupils in order to improve the school outcomes of bilingual pupils. Furthermore, although an explicit cost–benefit analysis is not possible, it is worth noting that the annual transport cost paid by the municipality (i.e. cost of the free bus service) amounted to $3 million, which in the Danish context would be more than sufficient to double the total DAL budgets at all of the sending schools or hiring another 37 full-time full-year teachers and thus keeping the roughly 480 pupils with substantial need for DAL support at their eight district schools.[47]

[47] Using 2015 numbers. The 480 pupils with a substantial need for DAL support include 181 pupils who moved to the municipality after school start. Since 2015, the number of pupils with a substantial need for language support has steadily declined, decreasing the annual transport cost to around $2 million by 2019.

References

Aarhus Municipality (2019) Analysis of school budgets of schools in Aarhus. (In Danish: Analyse af folkeskolernes økonomi), Aarhus Kommune. https://aarhus.dk/media/26872/analyse-af-folkeskolernes-oekonomi-2019.pdf (accessed on January 15, 2020).

Andersen, Simon C. and Peter B. Mortensen (2010) Policy Stability and Organizational Performance: Is There a Relationship? *Journal of Public Administration Research and Theory* 20(1): 1-22.

Andersen, Simon C., Miriam Gensowski, Steven Ludeke and Oliver P. John (forthcoming) A Stable Relationship Between Personality and Academic Performance from Childhood through Adolescence. An original study and replication in hundred-thousand-person samples. Forthcoming in *Journal of Personality*.

Andersen, Simon C. and Thorbjørn S. Guul (2019) Reducing Minority Discrimination at the Front Line—Combined Survey and Field Experimental Evidence. *Journal of Public Administration Research and Theory* 29(3): 429-44.

Andersen, Simon C. and Helena Skyt Nielsen (forthcoming) Learning from Performance Information. Forthcoming in *Journal of Public Administration Research and Theory*.

Angrist, Joshua D. and Kevin Lang (2004) Does School Integration Generate Peer Effects? Evidence from Boston's Metco Program, *American Economic Review* 94(5): 1613-1634.

Antecol, Heather, Ozkan Eren, Serkan Ozbeklik (2016) Peer effects in disadvantaged primary schools evidence from a randomized experiment. *Journal of Human Resources* 51(1): 95-132.

Aucejo, Esteban M. and Teresa F. Romano (2016) Assessing the effect of school days and absences on test score performance. *Economics of Education Review* 55: 70-87.

Ballatore, Rosario M., Margherita Fort and Andrea Ichino (2018) Tower of Babel in the Classroom: Immigrants and Natives in Italian Schools. *Journal of Labor Economics* 36 (4): 885-921.

Bergman, Peter (2019) The Risks and Benefits of School Integration for Participating Students: Evidence from a Randomized Desegregation Program. Manuscript, Columbia University.

Beuchert, Louise V., Maria K. Humlum, Helena S. Nielsen and Nina Smith (2018) The Short-term Effects of School Consolidation on Student Achievement: Evidence of Disruption? *Economics of Education Review* 65: 31-47.

Beuchert, Louise V., and Anne Brink Nandrup (2018) The Danish National Tests at a Glance. *Danish Economic Journal* 1.

Billings, Stephen, David. J. Deming and Jonah Rockoff (2014) School Segregation, Educational Attainment and Crime: Evidence from the end of busing in Charlotte-Mecklenburg, *Quarterly Journal of Economics* 129(1): 435-476.

Böhlmark, Anders, Helena Holmlund and Mikael Lindahl (2016) Parental choice, neighbourhood segregation or cream skimming? An analysis of school segregation after a generalized choice reform. *Journal of Population Economics* 29: 1155-1190.

Brøndum and Fliess (2009) Evaluation of magnet and full-day school in Aarhus Municipality (In Danish: Evaluering af magnet- og heldagsskolerne i Århus Kommune), May 9, 2009.

Carrell, Scott E., Bruce I. Sacerdote and James E. West (2012) From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation. *Econometrica* 81(3): 855-882.

Cascio, Elizabeth, Norah Gordon, Ethan Lewis and Sarah Reber (2008) From Brown to busing. *Journal of Urban Economics* 64: 296-325.

Chetty, Raj, Nathaniel Hendren and Lawrence F. Katz (2016) The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment, *American Economic Review* 106(4): 855-902.

Chin, Aimee, Meltem Daysal and Scott Imberman (2013) Impact of Bilingual Education Programs on Limited English Proficient Students and Their Peers: Regression Discontinuity Evidence from Texas. *Journal of Public Economics* 107: 63-78.

Cullen, Julie B., Brian A. Jacob, and Steven Levitt (2006) The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica* 75(5): 1191-1230.

Cunha, Flavio and James J. Heckman (2007) The technology of skill formation. *American Economic Review* 97(2): 31-47.

Damm, Anna P. and Christian Dustmann, (2014) Does Growing Up in a High Crime Neighborhood affect Youth Criminal Behavior? *American Economic Review* 104(6): 1806-1832.

Damm, Anna P., Hassani, Ahmad and Marie L. Schultz-Nielsen (2019a) "Residential Segregation in Denmark during 1986-2016", in: Hassani, A. "Residential Segregation and Public Housing", PhD dissertation, Aarhus BSS, Aarhus University, Department of Economics and Business Economics, 2019, Ch. 2.

Damm, Anna P., Hassani, Ahmad and Marie L. Schultz-Nielsen (2019b) "Construction of Danish Residential Neighbourhoods for 1986-2016", in: Hassani, A. "Residential Segregation and Public Housing", PhD dissertation, Aarhus BSS, Aarhus University, Department of Economics and Business Economics, 2019, Ch. 1.

Duflo, Esther, Pascaline Dupas, and Michael Kremer (2011) Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya. *American Economic Review* 101(5): 1739–1774.

Elsner, Benjamin and Ingo E. Isphording (2017) A Big Fish in a Small Pond: Ability Rank and Human Capital Investment. *Journal of Labor Economics*: 787-828.

Fredriksson, Peter, Björn Öckert and Hessel Oosterbeek (2013) Long-term effects of class size. *Quarterly Journal of Economics* 128(1): 249-285.

Geay, Charlotte, Sandra McNally and Shqiponja Telhaj (2013) Non-native speakers of English in the Classroom: What are the effects on Pupil Performace? *Economic Journal* 123: F281-F307.

Gulløv, Eva (2010) *Integration eller segregering I børnehaven*, In: Mikkelsen, Peter (ed.). *Pædagogisk arbejde med tosprogede børn.* (Frederikshavn: Dafolo, 2010).

Guryan, Jonathan (2004) Desegregation and black dropout rates. *American Economic Review* 94(4): 919-943.

Hanushek, Eric (2006) School Resources, ch.14. In: Hanushek, E. and F. Welch *Handbook of the Economics of Education* vol. 2, North-Holland, pp. 865-908.

Holmlund, Helena, Sandra McNally and Martina Viarengo (2010), Does money matter for schools? *Economics of Education Review* 29: 1154-64.

Horowitz, Joel L. and Charles F. Manski (1998) Censoring of outcomes and regressors due to survey nonresponse: Identification and estimation using weights and imputations. *Journal of Econometrics* 84(1): 37-58.

Hyman, Joshua (2017) Does Money Matter in the Long Run? Effects of School Spending on Educational Attainment. *American Economic Journal: Economic Policy* 9(4): 256–280.

Hægeland, Torbjørn, Oddbjørn Raaum and Kjell G. Salvanes (2012) Pennies from heaven? Using exogenous tax variation to identify effects of school resources on pupil achievement. *Economics of Education Review* 12: 601-14.

Jackson, Kirabo, Rucker C. Johnson, Claudia Persico (2016). "The Effects of School Spending on Educational & Economic Outcomes: Evidence from School Finance Reforms". *Quarterly Journal of Economics* 131(1): 157-2018.

Jensen, Peter and Astrid Würtz Rasmussen (2011) The Effect of Immigrant Concentration in Schools on Native and Immigrant Children's Reading and Math Skills, *Economics of Education Review* 30(6): 1503-1515.

Jensen, Sidsel Vive (2020) Contact Theory and Integration Policy: A Study of Interethnic Interaction and Social Integration among Young Children in two Danish Desegregated Schools. Manuscript, VIVE.

Jensen, Sidsel Vive and Kathrine Vitus (forthcoming) Broken Interaction Rituals, Struggles for Membership, and Violence among Young Children in Two Danish Schools. Forthcoming in *Symbolic Interaction.*

Johnson, Rucker C. (2019) *Children of the Dream: Why School Integration Works.* New York, NY: Basic Books and Russell Sage Foundation Press.

Lutz, Byron (2011) The End of Court-Ordered Desegregation. *American Economic Journal: Economic Policy* 3(2): 130-168.

OECD. Education at a Glance 2016, OECD indicators. OECD Publishing, 2016.

Rangvid, Beatrice S. (2007) Living and Learning Separately? Ethnic Segregation of School Children in Copenhagen. *Urban Studies* 44(7): 1329– 1354.

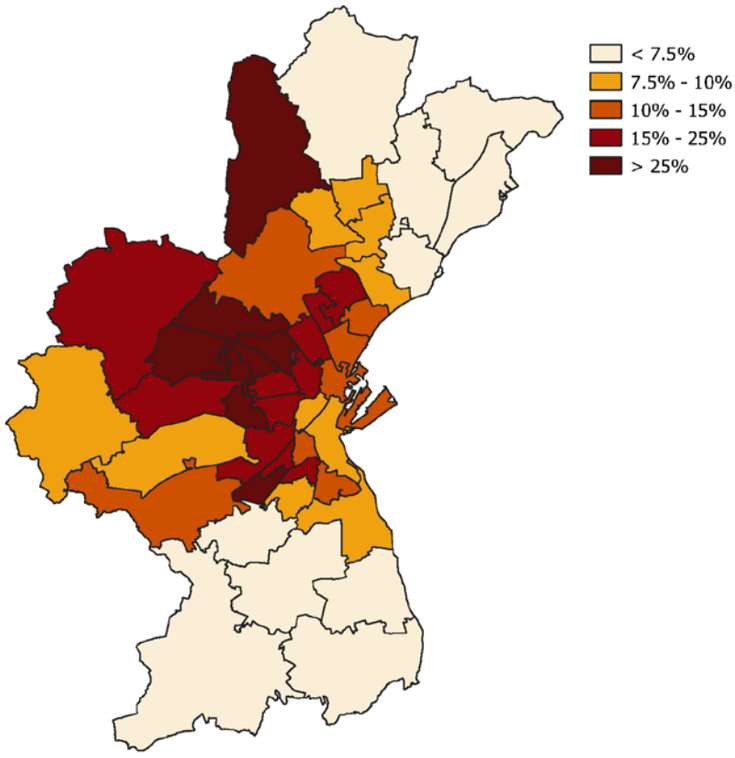
Rangvid, Beatrice S. (2010) School Choice, Universal Vouchers and Native Flight from Local Schools. *European Sociological Review* 26(3): 319–335

Reber, Sarah (2010) School Desegregation and Educational Attainment for Blacks. *Journal of Human Resources* 45(4): 893-914.

Reber, Sarah (2011) From Separate and Unequal to Integrated and Equal? School Desegregation and School Finance in Louisiana. *Review of Economics and Statistics* 93(2): 404-415.

Sacerdote, Bruce (2011) Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far? In (eds.) E. A. Hanushek, S. Machin and L. Woessmann *Handbook of the Economics of Education*, Elsevier, Chap. 4, 249-277.

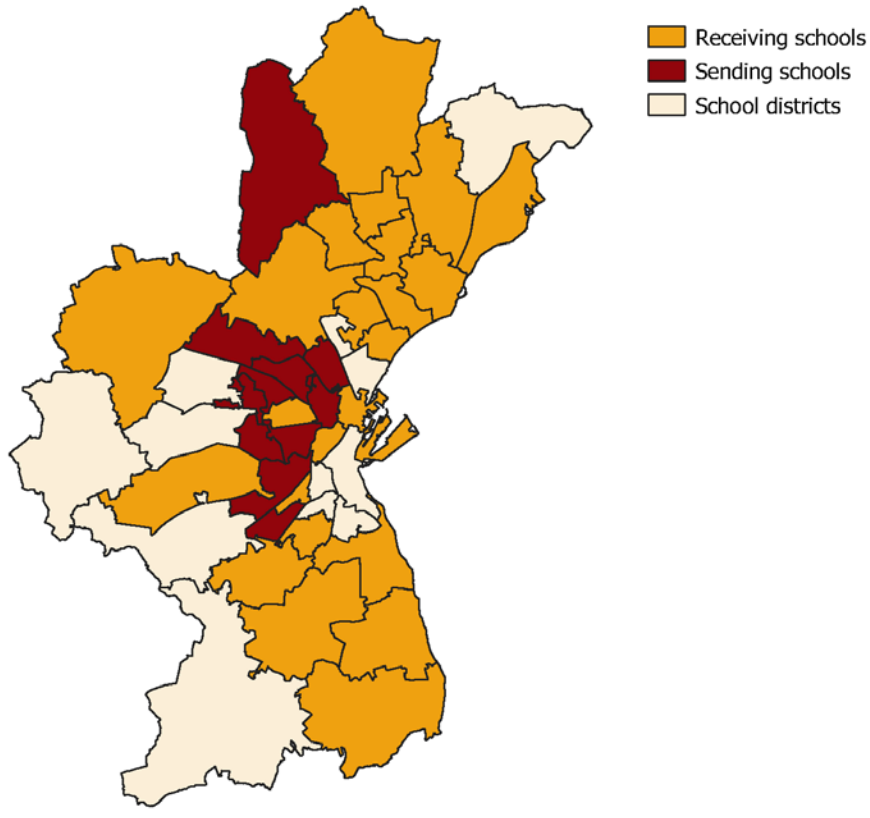
Figure 1. Share of Immigrants and Descendants across School Districts in Aarhus Municipality. 2016.



Source: Authors' own calculations using administrative register data from Statistics Denmark for 2016 and the shape file of school districts in Aarhus Municipality in 2016 (https://webkort.aarhuskommune.dk/spatialmap).

Note: Statistics Denmark's definition of immigrants and descendants.

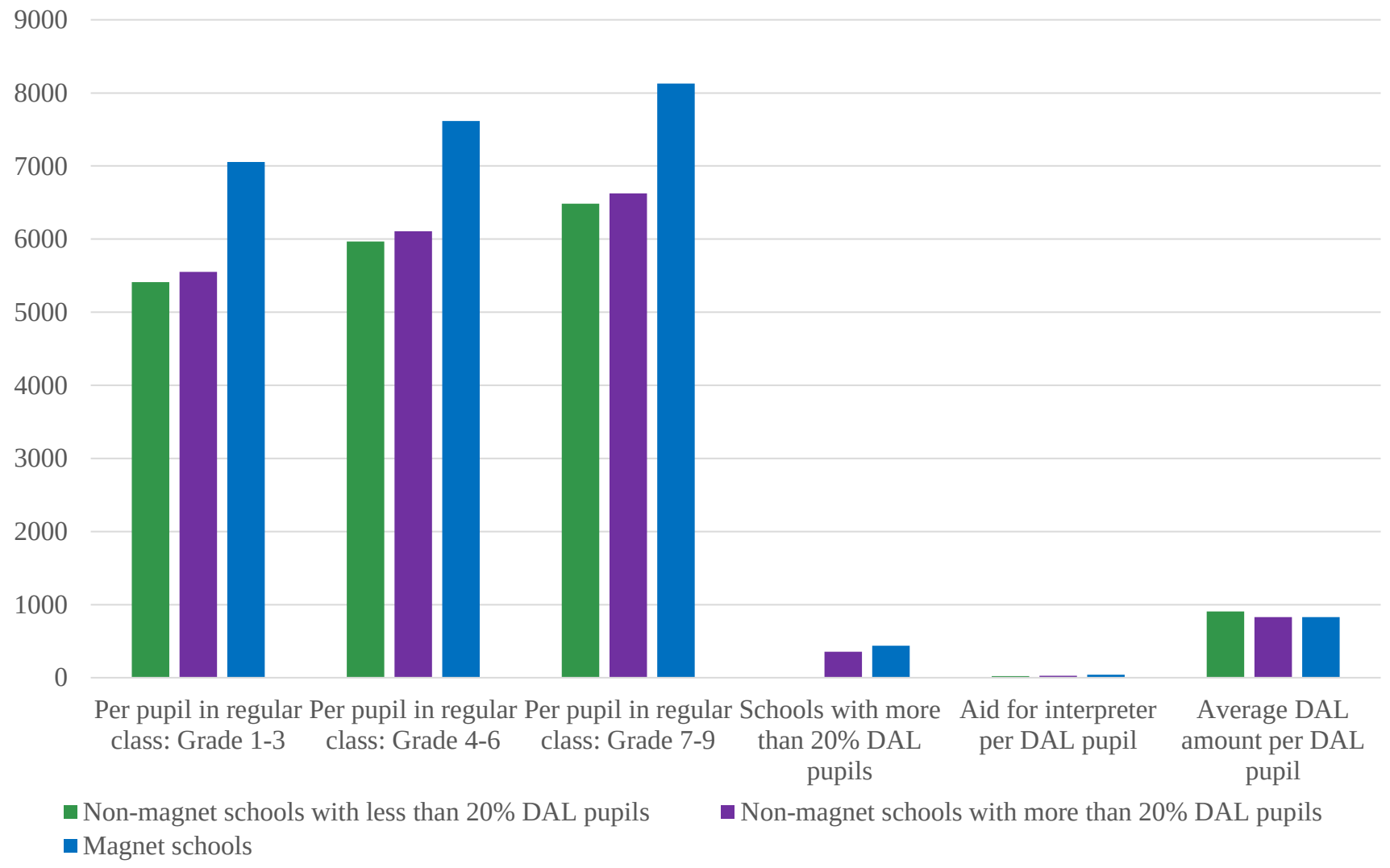
Figure 2. Sending and Receiving School Districts in Aarhus Municipality. 2007-2016.



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and the shape file of school districts in Aarhus Municipality in 2016 (https://webkort.aarhuskommune.dk/spatialmap).

Notes: Receiving schools (in at least one year between 2007 and 2016): Bavnehøj skole, Beder skole, Elev skole, Ellevangskolen, Elsted skole, Gammelgårdskolen, Hårup skole, Højvangskolen, Holme skole, Kolt skole (merged with Bavnehøj skole), Lisbjergskolen, Lystrup skole, Mårslet skole, Malling skole, N. J. Fjordsgades skole (Frederiksbjerg skole), Risskov skole, Sabro-Korsvejskolen, Samsøgades skole, Skåde skole, Skæring skole, Sølystskolen, Strandskolen, Tranbjerg skole, Viby skole, Virupskolen. Sending schools (in at least one year between 2007 and 2016) (M for magnet schools): Åby skole, Bakkegårdskolen, Ellekærskolen (M), Frydelundskolen (closed), Hasle skole (M), Katrinebjergskolen, Møllevangskolen, Nordgårdskolen (closed), Sødalskolen (M), Tilst skole, Vejlby skole (merged with Ellevangskolen), Vestergårdskolen.

Figure 3. Average Budgets per Pupil in Regular Classes (in USD) by Grade and Average Additional School Budgets for Danish-as-Additional-Language Pupils. 2014. By School Type.



Source: Authors' own calculations from allocated school budgets to public schools in Aarhus Municipality in 2014.

Note: The average budget per pupil in regular classes is calculated as the sum of the grade-specific rate per pupil in a regular class, the additional budget to guarantee minimum required budget for regular classes per pupil in regular classes in grade 0-10, the budget for social pedagogical support per pupil in regular classes in grades 1-10 and the budget for lunch scheme per pupil in grades 0-10. The average amount per pupil in regular classes in grades 1-3 in addition includes the budget for two teacher arrangement in grades 0-3 per pupil in grades 0-3. Schools with more than 20% DAL pupils receive an additional budget for "task-specific resources"; the amount per DAL pupil is shown in the column titled "Schools with more than 20% DAL pupils". All schools with DAL pupils receive a budget for "aid from interpreters"; the aid for interpreter per DAL pupil is shown in the column "Aid for interpreter". The average DAL amount per DAL pupil in regular classes is calculated by dividing the budget for DAL support to DAL pupils in regular classes by the number of DAL pupils in regular classes. Exchange rate DKK/USD 0.1485 (base year 2016).

Figure 4.a. Fraction of Pupils Assigned to Busing (Treatment Group) Attending Different Types of School. By Years Since the Language Test.

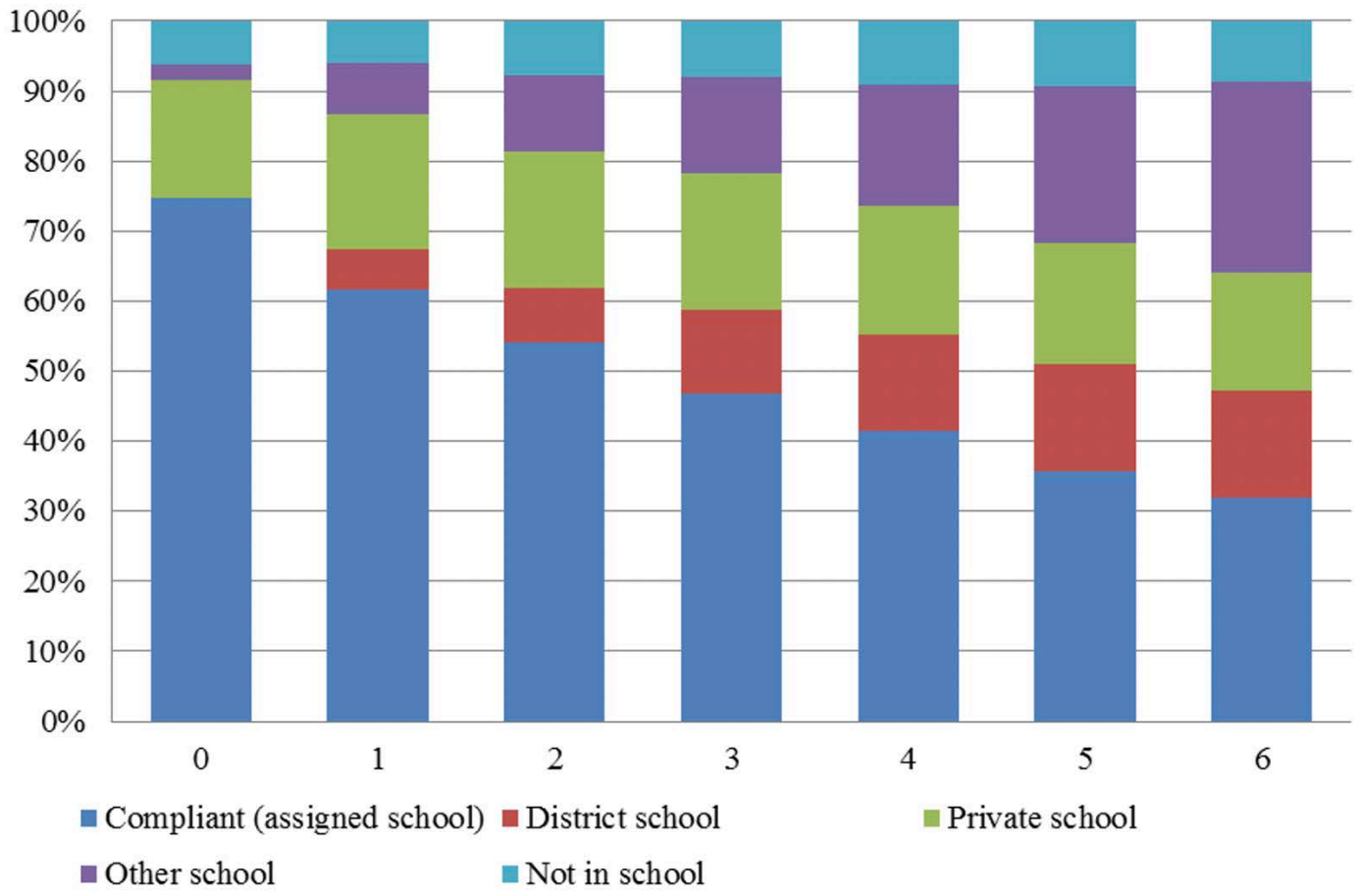
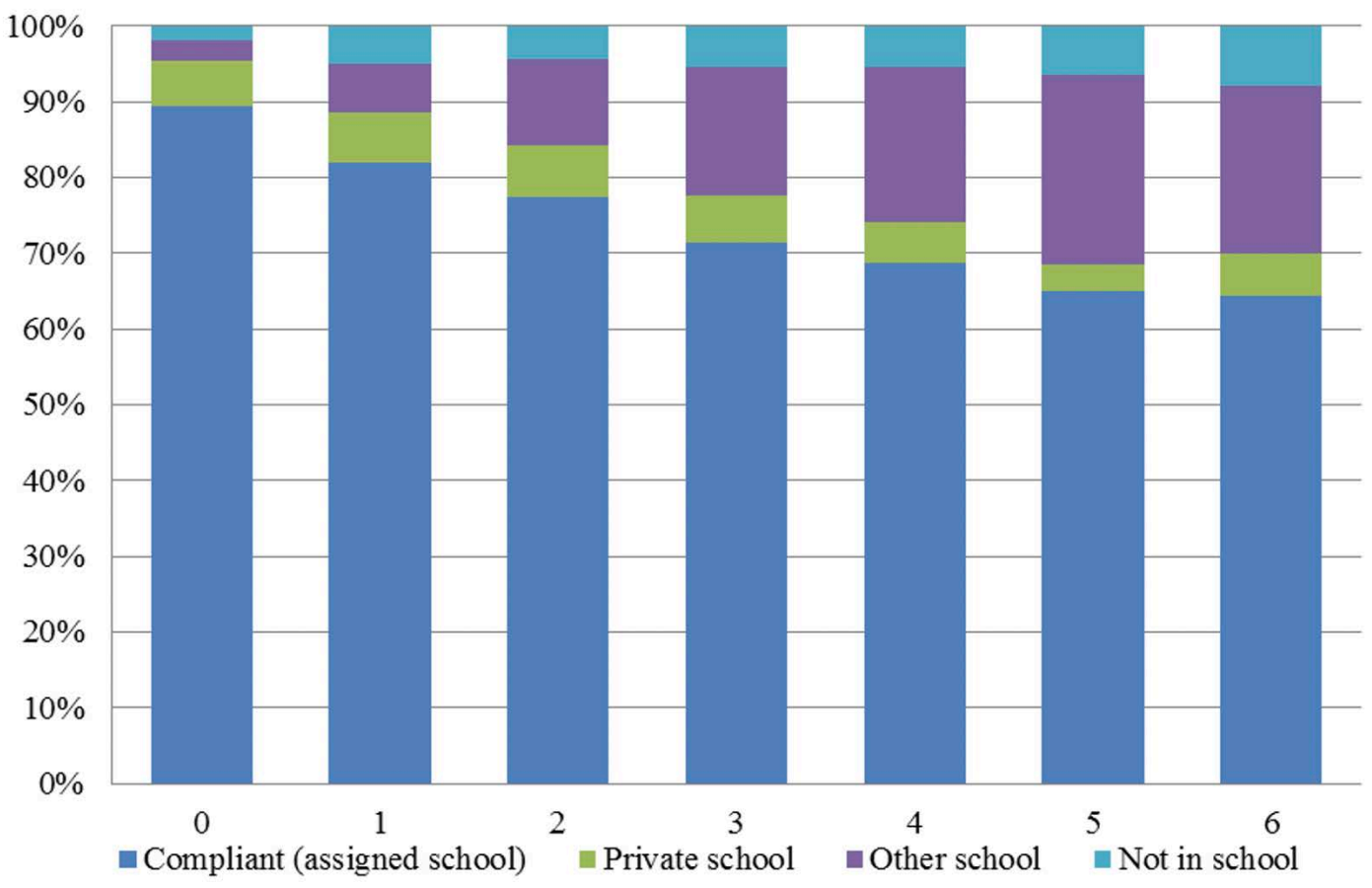


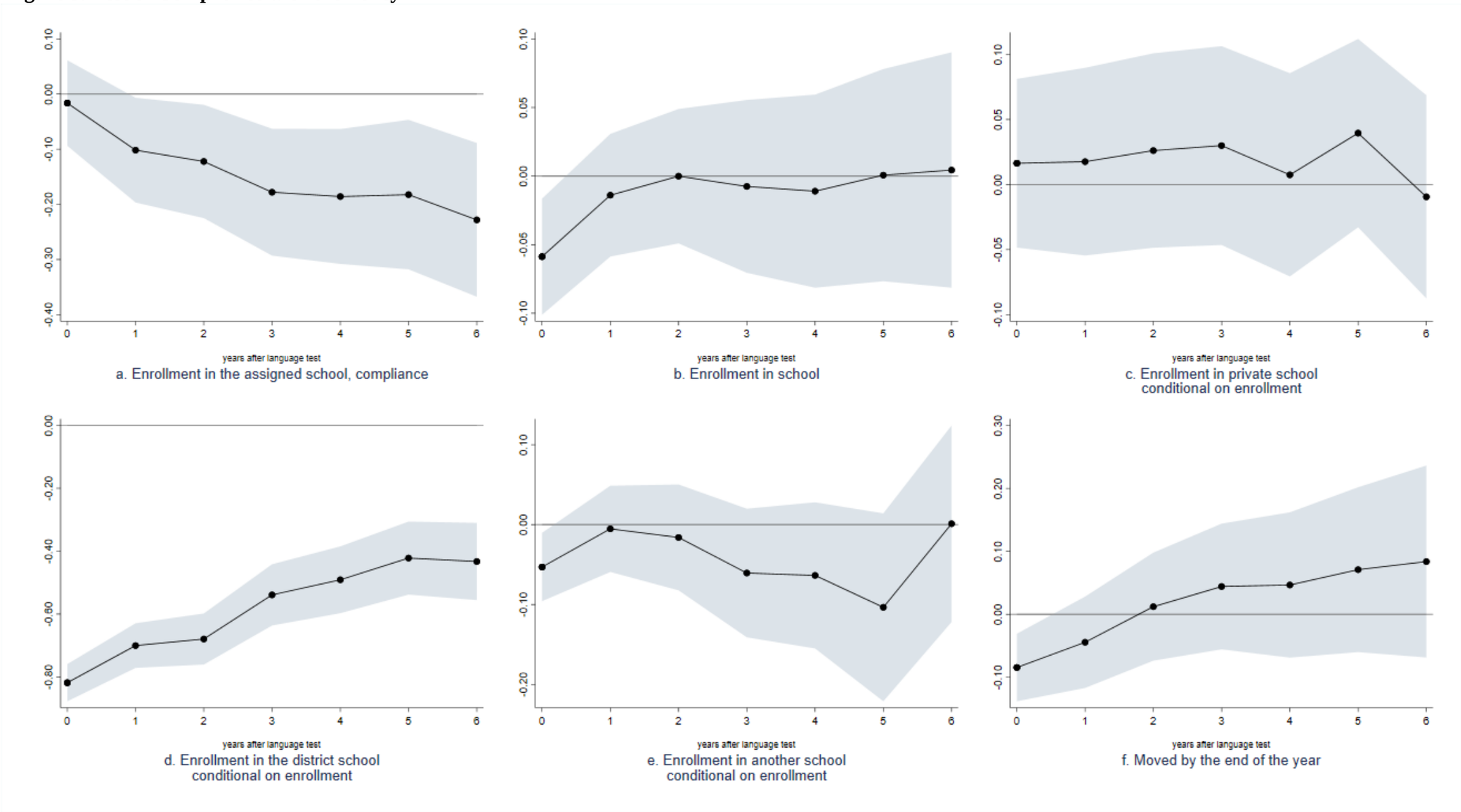
Figure 4.b. Fraction of Pupils Assigned to the District School (Control Group) Attending Different Types of School. By Years Since the Language Test.



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Enrollment in school in the end of August of the relevant year, for the year of the language test (year 0) until 6 years after.

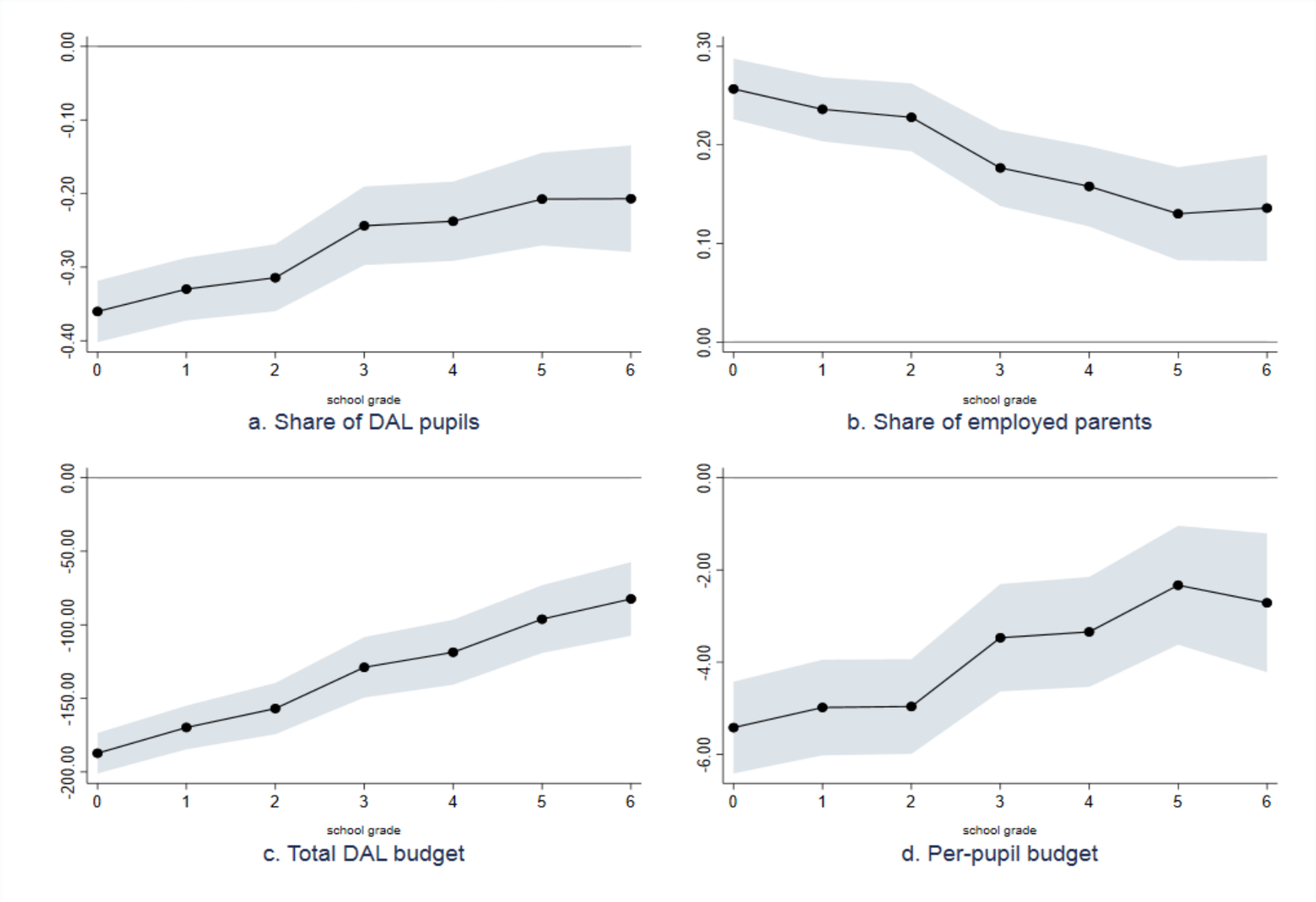
Figure 5. Test of Compliance with the Policy



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Coefficients and 95% confidence intervals of OLS regression of outcome on a dummy for being assigned to busing by year since the language test. Controls for the determinants of assignment to busing interacted with year since the language test and language test-year–by–school district of residence fixed effects. Robust standard errors clustered at the family level. Outcomes: a. enrollment in the assigned school, b. enrollment in school, c. enrollment in private school conditional on any enrollment, d. enrollment in the district school of residence in the year of the test conditional on any enrollment, e. enrollment in a school other than the assigned school and the district school of residence in the year of the test conditional on any enrollment, f. having moved school district and/or municipality by the end of the year.

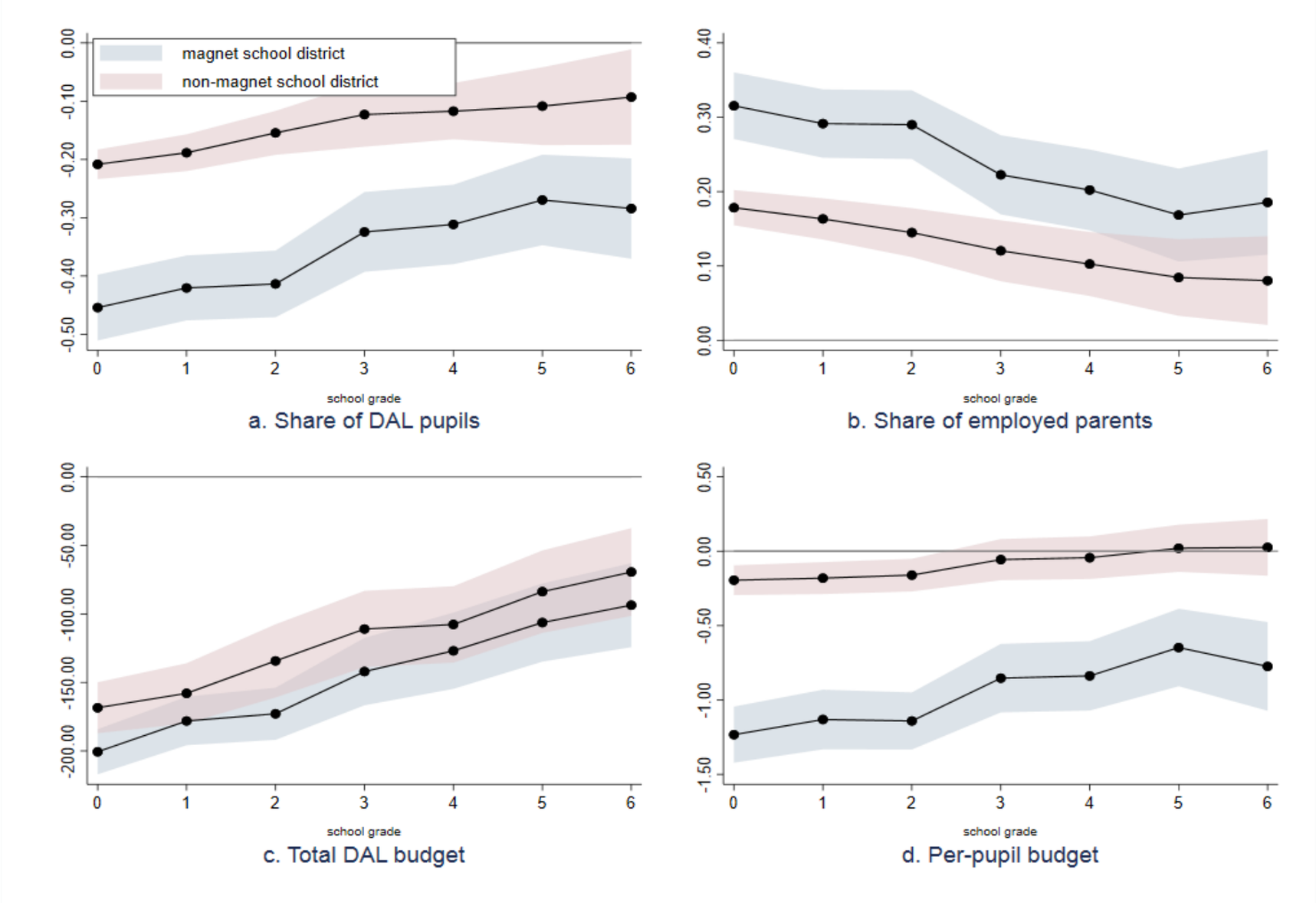
Figure 6. The Impact of Assignment to Busing on Peer Composition and Resources of the Attended School



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Coefficients and 95% confidence intervals of OLS regression of outcome on a dummy for being assigned to busing by year since the language test. Controls for the determinants of assignment to busing interacted with year since the language test and language test-year–by–school district of residence fixed effects. Robust standard errors clustered at the family level. Outcomes: a. share of Danish as Additional Language pupils in the assigned school, b. share of employed parents of pupils in the assigned school, d. total DAL budget in the assigned school in thousands of USD, d. per-pupil budget in the assigned school in thousands of USD.

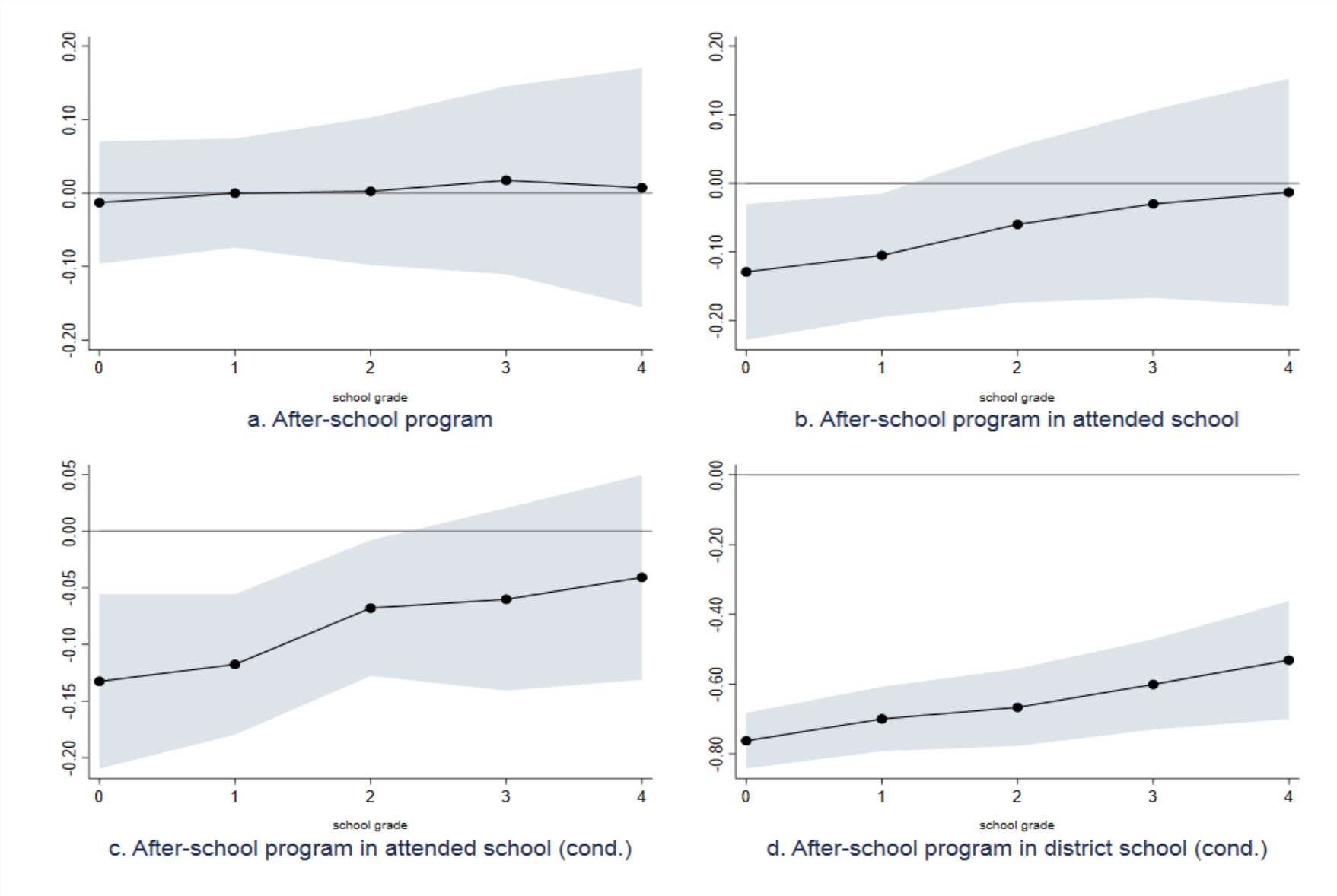
Figure 7. The Impact of Busing on Peers and Resources of the Attended School. By Sending School District Type: Magnet versus Non-Magnet School.



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). Coefficients and 95% confidence intervals of OLS regression of outcome on a dummy for being assigned to busing from a magnet school district (blue) and from a non-magnet school district (pink) by year since the language test. Controls for the determinants of busing interacted with year since the language test and language test-year–by–school district of residence fixed effects. Robust standard errors clustered at the family level. Outcomes: a. share of Danish as Additional Language pupils in the assigned school, b. share of employed parents of pupils in the assigned school, d. total DAL budget in the assigned school in thousands of USD, d. per-pupil budget in the assigned school in thousands of USD.

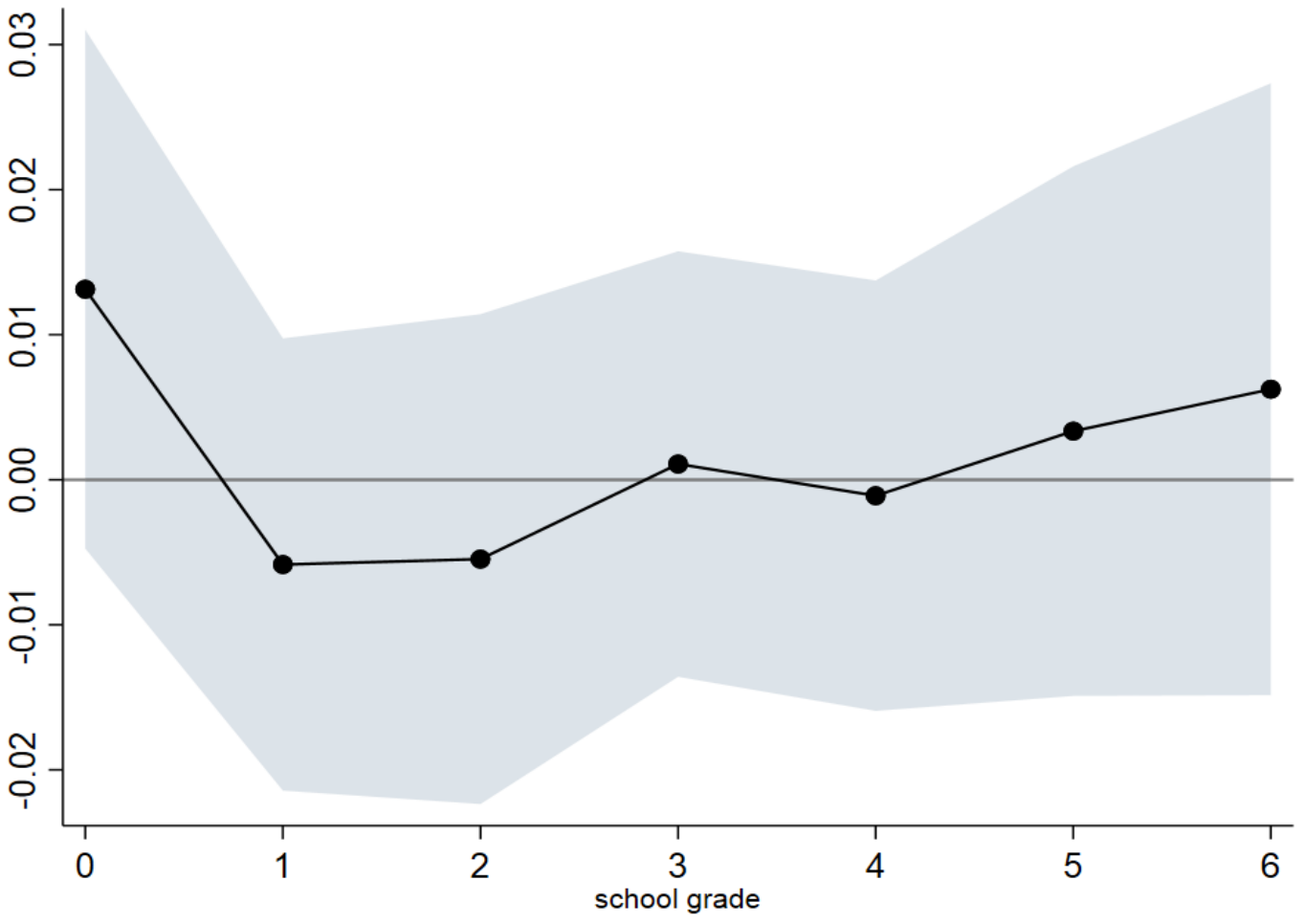
Figure 8. Effect of Assignment to Busing on Enrollment in After-School Programs



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Coefficients and 95% confidence intervals of OLS regression of outcome on a dummy for being assigned to busing by year since the language test. Controls for the determinants of assignment to busing interacted with year since the language test and language test-year–by–school district of residence fixed effects. Robust standard errors clustered at the family level. Outcomes: a. enrollment in any after-school program, b. enrollment in the after-school program of the attended school, c. enrollment in the after-school program of the attended school conditional on enrollment in any after-school program, d. enrollment in the after-school program of the district school conditional on enrollment in any after-school program.

Figure 9. The Effect of Assignment to Busing on School Absentee Rates: Share of Days of Absence over Active Days.



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Coefficients and 95% confidence intervals of OLS regression of the share of absences over the number of active days on a dummy for being assigned to busing by year since the language test. Robust standard errors clustered at the family level. Controls for the determinants of assignment to busing interacted with year since the language test and and language test-year–by–school district of residence fixed effects.

Table 1. Description of Sample of School Starters

	All		Assigned to busing		Assigned to district school	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Panel A - Individual characteristics						
Boy	0.526	0.500	0.540	0.499	0.512	0.500
Age on August 1st	6.163	0.363	6.176	0.372	6.149	0.354
Immigrant	0.078	0.268	0.055	0.228	0.100	0.300
Descendant	0.845	0.362	0.886	0.318	0.804	0.397
Non-Western origin or descent[2]	0.861	0.347	0.884	0.321	0.837	0.369
Africa	0.306	0.461	0.289	0.454	0.323	0.468
Middle East	0.447	0.497	0.515	0.500	0.379	0.486
East Asia	0.108	0.311	0.080	0.272	0.135	0.343
Number of siblings	3.280	2.077	3.310	1.997	3.250	2.155
Attended daycare	0.977	0.150	0.977	0.151	0.977	0.150
Living in two-parent household	0.705	0.456	0.675	0.469	0.735	0.442
Parents not employed (low SES)[1]	0.436	0.496	0.470	0.500	0.402	0.491
Observations	954		474		480	
Panel B - Parental characteristics[1]						
Mothers						
Age	32.294	6.004	31.527	6.072	33.066	5.841
Immigrant	0.925	0.263	0.946	0.226	0.905	0.294
Married	0.734	0.442	0.768	0.423	0.701	0.458
High school dropout	0.311	0.463	0.329	0.470	0.292	0.455
High school graduate	0.235	0.424	0.230	0.421	0.239	0.427
College graduate	0.195	0.397	0.191	0.394	0.199	0.400
No education reported	0.259	0.439	0.249	0.433	0.269	0.444
Employed (includes self-employed)	0.257	0.437	0.232	0.423	0.282	0.450
Unemployed	0.072	0.258	0.092	0.290	0.051	0.220
Out of the labor force	0.639	0.480	0.649	0.478	0.629	0.484
Real disposable income[3]	25,020	10,261	24,756	9,764	25,281	10,731
Real disposable income in first quartile[4]	0.160	0.367	0.168	0.374	0.153	0.360
Real disposable income in second quartile[4]	0.272	0.445	0.297	0.457	0.248	0.432
Real disposable income in third quartile[4]	0.353	0.478	0.335	0.473	0.371	0.484
Real disposable income in fourth quartile[4]	0.191	0.393	0.187	0.390	0.195	0.397
Observations	937		465		472	
Fathers						
Age	37.280	7.071	36.483	6.893	38.060	7.163
Immigrant	0.932	0.252	0.949	0.220	0.916	0.278
Married	0.760	0.427	0.774	0.419	0.747	0.435
High school dropout	0.248	0.432	0.277	0.448	0.219	0.414
High school graduate	0.294	0.456	0.304	0.460	0.284	0.451
College graduate	0.182	0.386	0.155	0.363	0.208	0.406
No education reported	0.277	0.448	0.264	0.441	0.290	0.454
Employed (includes self-employed)	0.504	0.500	0.472	0.500	0.535	0.499
Unemployed	0.090	0.286	0.106	0.309	0.074	0.261
Out of the labour force	0.360	0.480	0.375	0.485	0.346	0.476
Real disposable income[3]	23,229	12,423	22,057	12,890	24,373	11,851
Real disposable income in first quartile[4]	0.269	0.444	0.310	0.463	0.229	0.421
Real disposable income in second quartile[4]	0.327	0.470	0.330	0.471	0.325	0.469
Real disposable income in third quartile[4]	0.216	0.412	0.200	0.400	0.232	0.422
Real disposable income in fourth quartile[4]	0.159	0.366	0.133	0.340	0.184	0.388
Observations	913		451		462	

(continued)

Table 1. (continued)

Panel C: School assignment policy[5]						
Strong language support need	0.128	0.334	0.131	0.338	0.125	0.331
Medium language support need	0.436	0.496	0.456	0.499	0.417	0.494
Low language support need	0.436	0.496	0.414	0.493	0.458	0.499
Bused	0.497	0.500	1.000	0.000	0.000	0.000
Sibling attending the district school	0.422	0.494	0.192	0.394	0.650	0.477
Age difference with youngest sibling in district school	3.835	2.547	4.862	2.964	3.353	2.333
Sibling bused	0.202	0.402	0.213	0.410	0.192	0.394
Distance to district school (km)	0.853	0.654	0.852	0.518	0.855	0.766
Distance to assigned school (km)	3.835	3.716	6.859	3.011	0.855	0.766
Number of category-S pupils in class	4.106	2.109	3.437	1.981	4.677	2.049
Observations	954		474		480	

Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing, are less than seven years old when taking the language screening test, are referred to a regular public school, have a total test score "S", live in a regular school district (without a full-day school) with a sending school, who have not expressed desire for another school than the district school, who do not reside in Norgaards and Frydelund school districts in 2007, and who do not move to Aarhus between January and school start in 2016.

[1] Characteristics in the year the child turns 4.

[2] Origin or descent: not from Western Europe, Australia, New Zealand, Canada, USA.

[3] Real USD (base year 2016, exchange rate DKK/USD 0.1485).

[4] Distribution of real disposable income of the adult immigrant population (age 25 to 54) in Aarhus Municipality.

[5] Year of the language screening test.

Table 2. Characteristics of School Districts of Residence and Assignment

	Sending districts (magnet schools)		Sending districts (non-magnet schools)		Receiving districts (non-magnet schools)	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Districts						
Share of residents who are immigrants or descendants	0.496	0.089	0.240	0.067	0.103	0.144
Employment rate[1]	0.607	0.066	0.740	0.033	0.842	0.109
Share of residents with a tertiary education[1]	0.171	0.022	0.150	0.024	0.139	0.038
Avg. real disposable income[1,2]	30,302	1,512	32,956	2,542	41,701	8,711
Share of school starters who enroll in the district school	0.344	0.129	0.583	0.155	0.777	0.160
District Schools						
School size[3]	371	115	562	168	590	220
Class size[4]	16.213	3.125	18.773	2.670	20.484	2.958
Grade 0 class size[4]	14.805	6.363	18.542	4.122	21.553	9.103
Share of employed parents	0.401	0.139	0.601	0.083	0.751	0.156
Share of DAL pupils[5]	0.665	0.144	0.363	0.064	0.172	0.211
Share of category-S pupils in grade 0[3,6]	0.294	0.199	0.196	0.059	0.138	0.119
Average age of teachers[7]	46.194	1.982	43.899	1.334	43.683	2.289
Pupils per teacher[7]	9.531	1.846	12.641	1.695	9.447	1.933
Share of lessons with qualified staff[7]	0.792	0.106	0.754	0.115	0.751	0.130
Share of Danish lessons with qualified staff[7]	0.817	0.208	0.863	0.137	0.843	0.132
Share of math lessons with qualified staff[7]	0.863	0.177	0.770	0.223	0.766	0.196
Per-pupil budget [2,3,8,9]	6,978	851	5,698	318	5,514	335
Real total DAL budget (thousands)[2,9]	282.294	25.583	256.393	69.926	80.071	60.234
DAL per-pupil premium (%)[9]	18.055	1.340	20.959	1.873	16.413	2.181
Average class DNT score in the school, reading grade 2[10]	-0.509	0.940	-0.288	0.996	-0.113	0.989
Average class DNT score in the school, math grade 3[10]	-0.275	0.894	-0.254	0.985	-0.036	0.968
Average class DNT score in the school, reading grade 4[10]	-0.552	0.940	-0.295	0.987	-0.029	0.952
Average class DNT score in the school, reading grade 6[10]	-0.736	1.044	-0.393	0.988	-0.011	0.924
Average class DNT score in the school, math grade 6[10]	-0.548	0.882	-0.320	0.899	0.071	0.906
Average class DNT score in the school, English grade 7[10]	-0.263	0.955	-0.079	0.963	0.144	0.966
Average class DNT score in the school, reading grade 8[10]	-0.718	0.947	-0.141	0.929	0.075	0.878
Average class DNT score in the school, natural sciences grade 8[10,11]	-0.624	0.900	-0.149	0.848	0.093	0.854
Number of schools	3		7		33	

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Sample: School districts of residence and assignment of our sample of language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Averages across the relevant years and school districts.

[1] Adult population (age 25 to 54) in the school district.

[2] Real USD (base year 2016, exchange rate DKK/USD 0.1485).

[3] At the beginning of the school year (Aug 31), from the Municipality pupil's register.

[4] Years 2007-2015.

[5] Number of Danish-as-Additional Language (DAL) pupils reported by the Municipality.

[6] The share of S-pupils in grade 0 is above the policy threshold of 20% for sending magnet school Ellekærskolen, because Ellekærskolen has been exempted from the 20% rule since around 2016 and possibly also because of flight of Danish pupils between class formation and school start.

[7] Source: https://www.uddannelsesstatistik.dk. Average age of teachers in years 2007-2016, pupils per teachers in years 2010-2016, lessons with qualified staff in years 2012-2016.

[8] Per pupil budget for grades 1 to 3.

[9] Danish National Test score. Average of available years (2014-2016), conditional on the school being open in those years.

[10] Mean and standard deviation of the class-average DNT score, available for school years from 2009/2010 to 2018/2019.

[11] Science includes: biology, geography, physics and chemistry.

Table 3. Sample Characteristics: Outcomes.

	All		Assigned to busing		Assigned to district school	
	Mean (Std. dev.)	Obs.	Mean (Std. dev.)	Obs.	Mean (Std. dev.)	Obs.
National Tests [1]						
Reading, grades 2,4,6,8 (taker)	0.947	2080	0.930	1099	0.966	981
Reading, grade 2,4,6,8 (score)	-0.654	1970	-0.703	1022	-0.601	948
	(0.945)		(0.944)		(0.944)	
Math, grades 3,6 (taker)	0.956	1097	0.948	578	0.965	519
Math, grades 3,6 (score)	-0.559	1049	-0.651	548	-0.459	501
	(0.879)		(0.862)		(0.887)	
English, grade 7 (taker)	0.739	375	0.744	238	0.730	137
English, grade 7 (score)	-0.347	277	-0.398	177	-0.258	100
	(0.860)		(0.851)		(0.873)	
Natural sciences, grade 8 (taker)[2]	0.893	458	0.889	325	0.902	133
Natural sciences, grade 8 (score)	-0.344	409	-0.354	289	-0.319	120
	(1.007)		(1.024)		(0.967)	
Wellbeing [3]						
Survey taker (grades 0–3)	0.864	1,227	0.831	313	0.875	914
	(0.343)		(0.376)		(0.331)	
School satisfaction (grades 0–3)	0.122	1,060	0.041	260	0.148	800
	(0.992)		(1.025)		(0.980)	
Distress (grades 0–3)	0.070	1,060	0.192	260	0.030	800
	(1.054)		(1.049)		(1.053)	
Survey taker (grades 4–9)	0.825	2,064	0.793	1,171	0.867	893
	(0.380)		(0.405)		(0.340)	
School satisfaction (grades 4–9)	-0.028	1,703	0.016	929	-0.080	774
	(1.057)		(1.057)		(1.055)	
Distress (grades 4–9)	0.066	1,703	0.041	929	0.097	774
	(1.003)		(1.034)		(0.097)	
School Absentee rates [4]						
Share of absences over school days in grade 0	0.077	519	0.102	183	0.064	336
	(0.068)		(0.084)		(0.052)	
Share of absences over school days, grades 0–4	0.069	3,003	0.079	1,299	0.061	1,704
	(0.064)		(0.077)		(0.052)	
Enrollment in after-school programs [5]						
After-school in grade 0	0.823	679	0.801	351	0.848	328
After-school in the attended school in grade 0	0.766	679	0.695	351	0.841	328
After-school in the district school in grade 0	0.464	679	0.120	351	0.832	328
After-school in grades 0–4	0.822	2,386	0.813	1,365	0.833	1,021
After-school in the attended school in grades 0–4	0.777	2,386	0.740	1,365	0.828	1,021
After-school in the district school, grades 0–4	0.427	2,386	0.158	1,365	0.787	1,021

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Outcomes pooled across grades.

[1] Public school pupils enrolled in the relevant grade between 2010 and 2017, and who take the Danish National Test (score).

[2] Natural sciences cover biology, geography, physics and chemistry.

[3] Public school pupils enrolled in the relevant grade between 2014 and 2018.

[4] Public school pupils enrolled in the relevant grade between 2007 and 2015.

[5] Public school pupils enrolled in the relevant grade between 2011 and 2017.

Table 4. Factor Loadings on School Satisfaction and Distress

School Satisfaction	
Survey Question	Factor loading
Do you learn anything exciting in school?	1
Are your classrooms nice to be in?	0.948
Are lessons boring?	0.936
Are you happy with your school?	0.917
Are teachers good at helping you in school?	0.817
Are you happy with your class?	0.800
Are you happy with your teachers?	0.761
Distress	
Survey Question	Factor loading
Is there someone who teases you, so that you get upset?	1
Do you have stomachache, when you are in school?	0.935
Do you have headache, when you are in school?	0.925
Are you afraid that the other kids laugh at you in school?	0.883
Do you feel alone in school?	0.874
Is it difficult to hear what the teacher says during lessons?	0.739

Source: Danish Wellbeing Survey of all public school pupils in grades 0-3, waves 2015-2019.

Notes: Factor loadings from confirmatory factor analysis on the two most important factors in a exploratory factor analysis. We run the exploratory factor analysis using all 20 items in the questionnaire. We find 4 factors with eigenvalue above 1, of which only two explaining above 10% of the variance in the data. We run the confirmatory factor analysis of these two factors using only the items with factor loadings of .5 and above, and controls for year of the survey, grade, age, and sex.

Table 5. Test of the Identification Strategy

	Dependent variable: Assigned to busing			
	(1)	(2)	(3)	(4)
Explanatory variables:				
Sibling in the district school	-0.484***	-0.476***	-0.482***	-0.485***
	(0.036)	(0.038)	(0.039)	(0.039)
Age difference with youngest sibling in the district school	0.024***	0.022***	0.023***	0.023***
	(0.007)	(0.007)	(0.007)	(0.007)
Distance from neighborhood of residence to the district school	0.050***	0.050***	0.048**	0.048**
	(0.019)	(0.019)	(0.019)	(0.020)
Sibling bused		0.028	0.025	0.024
		(0.032)	(0.032)	(0.032)
Medium language support need		-0.004	-0.002	-0.002
		(0.033)	(0.034)	(0.034)
Low language support need		-0.007	-0.004	-0.004
		(0.033)	(0.034)	(0.034)
Age on language test day		0.021	0.021	0.023
		(0.029)	(0.029)	(0.029)
R^2	0.638	0.645	0.647	0.651
Observations	954	954	954	954
Controls:				
Year–by–school district fixed effects	YES	YES	YES	YES
Individual characteristics	NO	YES	YES	YES
Mother characteristics	NO	NO	YES	YES
Father characteristics	NO	NO	NO	YES
F-test joint insignificance for additional controls		0.765	0.701	0.779
P-value F-test		0.764	0.900	0.858

Source: Micro data from Danish National Tests (scores) linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). OLS of dummy for assignment to busing (sample avg 0.504) over the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school and language test-year–by–school district of residence fixed effects. Further specifications in adddition control for individual characteristics including dummies for having an older sibling bused, the assessed level of language support need, and the continuous age of the pupil on the day of the test (columns 2-4), mother (columns 3-4) and father characteristics (column 5). Fixed effects for 10 school districts of residence for each year in the period from 2007-2016. F-test on individual, mother and father characteristics, age, level of language support, and sibling bused. Individual characteristics of the child include gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39).

Table 6. Effect of Assignment to Busing on National Test Scores. By Grade and Subject.

	Dependent variable:			
	Test taker	Test taker	Standardized test score	Standardized test score
	(1)	(2)	(3)	(4)
Explanatory variables:				
Assigned to busing, reading test, grade 2	-0.057**	-0.055**	-0.089	-0.092
	(0.023)	(0.023)	(0.099)	(0.096)
Assigned to busing, math test, grade 3	-0.015	-0.013	-0.170*	-0.184*
	(0.021)	(0.021)	(0.101)	(0.097)
Assigned to busing, reading test, grade 4	-0.033*	-0.031	-0.019	-0.037
	(0.020)	(0.021)	(0.106)	(0.102)
Assigned to busing, reading test, grade 6	-0.001	-0.002	-0.114	-0.132
	(0.021)	(0.021)	(0.108)	(0.105)
Assigned to busing, math test, grade 6	-0.011	-0.011	-0.152	-0.172
	(0.020)	(0.020)	(0.113)	(0.109)
Assigned to busing, English test grade 7	0.032	0.031	-0.077	-0.082
	(0.045)	(0.045)	(0.133)	(0.125)
Assigned to busing, reading test, grade 8	-0.006	-0.008	-0.234*	-0.250*
	(0.027)	(0.028)	(0.133)	(0.131)
Assigned to busing, natural science tests, grade 8	0.016	0.012	-0.008	0.027
	(0.035)	(0.036)	(0.160)	(0.154)
R^2	0.108	0.127	0.093	0.170
Observations	4,010	4,010	3,705	3,705
Controls:				
Determinants of assignment	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES
Subject fixed effects	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Pooled dataset. Outcomes: Dummies for taking the test conditional on attending the relevant grade in a public school in a year where the test took place. Standardized test scores conditional on having taken the test. National tests in math (grades 3 and 6), reading (grades 2, 4, 6, 8), English (grade 7) and natural science (grade 8). The natural science tests cover geography, biology, physics and chemistry. OLS of the outcome on a dummy for assignment to busing interacted with grade and test subject. We control for the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the

Table 7. Effect on Assignment to Busing on National Test Scores. By Grade, Subject and subgroups: Gender, Socio-E

	Subgroup			Subgroup		
	Boys	Girls	Difference	High SES	Low SES	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dep. Var.: Test taker						
Explanatory variables:						
Assigned to busing, reading test, grade 2	-0.033	-0.083**	0.050	-0.027	-0.086***	0.059*
	(0.027)	(0.032)	[0.169]	(0.026)	(0.032)	[0.096]
Assigned to busing, math test, grade 3	-0.006	-0.022	0.016	0.008	-0.037	0.045
	(0.025)	(0.027)	[0.608]	(0.023)	(0.028)	[0.138]
Assigned to busing, reading test, grade 4	-0.031	-0.033	0.002	0.007	-0.073**	0.080**
	(0.024)	(0.029)	[0.935]	(0.021)	(0.031)	[0.017]
Assigned to busing, reading test, grade 6	0.020	-0.029	0.010	-0.011	0.008	0.019
	(0.023)	(0.029)	[0.105]	(0.026)	(0.024)	[0.463]
Assigned to busing, math test, grade 6	0.001	-0.026	0.027	-0.003	-0.022	0.019
	(0.023)	(0.027)	[0.359]	(0.023)	(0.026)	[0.513]
Assigned to busing, English test, grade 7	0.072	-0.022	0.094*	0.019	0.045	-0.026
	(0.050)	(0.057)	[0.096]	(0.053)	(0.053)	[0.628]
Assigned to busing, reading test, grade 8	-0.004	-0.010	0.006	-0.018	0.004	-0.022
	(0.032)	(0.032)	[0.864]	(0.033)	(0.030)	[0.487]
Assigned to busing, natrual science tests,	0.013	0.012	0.001	0.060	-0.040	0.100**
grade 8	(0.043)	(0.043)	[0.979]	(0.039)	(0.047)	[0.039]
R^2	0.129			0.132		
Observations	4,010			4,010		
Panel B: Dep. Var.: Standardized test score						
Explanatory variables:						
Assigned to busing, reading test, grade 2	-0.138	-0.025	-0.103	-0.032	-0.157	0.125
	(0.112)	(0.119)	[0.373]	(0.112)	(0.120)	[0.325]
Assigned to busing, math test, grade 3	-0.163	-0.208*	0.045	-0.132	-0.242**	0.110
	(0.115)	(0.119)	[0.725]	(0.115)	(0.122)	[0.409]
Assigned to busing, reading test, grade 4	-0.101	0.040	-0.141	0.039	-0.123	0.162
	(0.121)	(0.120)	[0.277]	(0.117)	(0.125)	[0.204]
Assigned to busing, reading test, grade 6	-0.205*	-0.031	-0.174	-0.091	-0.175	0.084
	(0.122)	(0.124)	[0.173]	(0.127)	(0.117)	[0.497]
Assigned to busing, math test, grade 6	-0.072	-0.295**	0.223*	-0.112	-0.238*	0.126
	(0.124)	(0.129)	[0.090]	(0.123)	(0.130)	[0.330]
Assigned to busing, English test, grade 7	0.042	-0.261*	0.301**	-0.002	-0.169	0.167
	(0.140)	(0.145)	[0.029]	(0.143)	(0.141)	[0.216]
Assigned to busing, reading test, grade 8	-0.177	-0.357**	0.180	-0.250*	-0.245	-0.005
	(0.154)	(0.153)	[0.271]	(0.144)	(0.168)	[0.976]
Assigned to busing, natrual science tests,	0.184	-0.217	0.401**	-0.070	0.137	-0.207
grade 8	(0.152)	(0.212)	[0.036]	(0.165)	(0.186)	[0.213]
R^2	0.177			0.173		
Observations	3,705			3,705		
Controls:						
Determinants of assignment	YES			YES		
Year–by–school district fixed effects	YES			YES		
Grade fixed effects	YES			YES		
Subject fixed effects	YES			YES		
Individual characteristics	YES			YES		
Mother characteristics	YES			YES		
Father characteristics	YES			YES		

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neigh
Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. P-value of test of equality between c
Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Outcomes
attending the relevant grade in a public school in a year where the test took place. Standardized test scores conditional on having taken the test. Na
(grades 2, 4, 6, 8), English (grade 7) and natural science (grade 8). The natural science tests cover geography, biology, physics and chemistry. OLS o
busing interacted with grade, test subject, and sex of the child (columns 1-3), socio-economic status of the child (high if at least one parent is employ
the child has low language support need or medium to high (columns 8-11). We control for the school assignment determinants: a dummy for hav
school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance
language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects, additional individual and family characterist
include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, in
area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings
in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration
school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real
the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

Table 8. Effect of Assignment to Busing on Wellbeing Survey Factors: School Satisfaction and Distress. Grades 0–3.

	Dependent variable:					
	Survey taker	Survey taker	School satisfaction	School satisfaction	Distress	Distress
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Explanatory variable:						
Assigned to busing	-0.062*	-0.065*	-0.090	-0.084	0.244*	0.233*
	(0.036)	(0.034)	(0.116)	(0.122)	(0.139)	(0.134)
R^2	0.064	0.105	0.086	0.123	0.110	0.169
Observations	1,227	1,227	1,060	1,060	1,060	1,060
Panel B						
Explanatory variables:						
Assigned to busing, grade 0	-0.019	-0.023	-0.085	-0.076	0.424*	0.404*
	(0.062)	(0.061)	(0.176)	(0.184)	(0.233)	(0.243)
Assigned to busing, grade 1	-0.219***	-0.217***	-0.095	-0.112	0.085	0.088
	(0.062)	(0.063)	(0.159)	(0.171)	(0.183)	(0.174)
Assigned to busing, grade 2	-0.006	-0.012	-0.007	0.005	-0.065	-0.068
	(0.051)	(0.048)	(0.170)	(0.174)	(0.170)	(0.168)
Assigned to busing, grade 3	-0.015	-0.019	-0.170	-0.154	0.537***	0.503***
	(0.050)	(0.050)	(0.165)	(0.168)	(0.175)	(0.171)
R^2	0.075	0.115	0.087	0.124	0.120	0.178
Observations	1,227	1,227	1,060	1,060	1,060	1,060
Controls:						
Determinants of assignment	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES
Grade FE	YES	YES	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES	NO	YES

Source: Micro data from Danish Wellbeing Surveys linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Outcomes: a dummy for having taken the well-being survey (1, 2) and standardized factors for school satisfaction (3, 4) and distress (5, 6). OLS of the outcome on a dummy for assignment to busing (Panel A), a dummy for assignment to busing interacted with the grade (Panel B) and the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. We further control for: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

Table 9. Effect of Assignment to Busing on National Test Score. By Grade, Subject and Type of Sending School District.

	Subgroup			Subgroup		
	Magnet school district	Non-magnet school district	Difference	Magnet school district	Non-magnet school district	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dep. Var.: Test taker						
Explanatory variables:						
Assigned to busing, reading test, grade 2	-0.085***	0.016	-0.101***	-0.079***	0.012	-0.091***
	(0.029)	(0.028)	[0.004]	(0.029)	(0.029)	[0.011]
Assigned to busing, math test, grade 3	-0.033	0.027	-0.060*	-0.026	0.022	-0.048*
	(0.025)	(0.027)	[0.060]	(0.026)	(0.028)	[0.139]
Assigned to busing, reading test, grade 4	-0.041*	-0.027	-0.014	-0.034	-0.032	-0.002
	(0.024)	(0.035)	[0.740]	(0.024)	(0.036)	[0.965]
Assigned to busing, reading test, grade 6	-0.011	0.012	-0.023	-0.006	0.007	-0.013
	(0.026)	(0.027)	[0.462]	(0.026)	(0.028)	[0.684]
Assigned to busing, math test, grade 6	-0.026	0.022	-0.048*	-0.022	0.017	-0.039
	(0.025)	(0.023)	[0.093]	(0.026)	(0.024)	[0.211]
Assigned to busing, English test, grade 7	0.058	-0.068	0.116*	0.062	-0.071	0.133*
	(0.049)	(0.068)	[0.059]	(0.049)	(0.068)	[0.050]
Assigned to busing, reading test, grade 8	-0.003	-0.034	0.031	0.001	-0.042	0.043
	(0.030)	(0.046)	[0.514]	(0.030)	(0.047)	[0.375]
Assigned to busing, natural science tests, grade 8	0.013	0.014	-0.001	0.016	0.000	0.016
	(0.042)	(0.041)	[0.986]	(0.042)	(0.042)	[0.740]
R^2	0.114			0.132		
Observations	4,010			4,010		
Panel B: Dep. Var.: Standardized test score						
Explanatory variables:						
Assigned to busing, reading test, grade 2	-0.097	-0.096	-0.001	-0.054	-0.162	0.108
	(0.128)	(0.135)	[0.995]	(0.125)	(0.125)	[0.490]
Assigned to busing, math test, grade 3	-0.147	-0.273**	0.126	-0.116	-0.345***	0.229
	(0.132)	(0.137)	[0.463]	(0.127)	(0.130)	[0.160]
Assigned to busing, reading test, grade 4	-0.013	-0.079	0.066	0.016	-0.158	0.174
	(0.137)	(0.135)	[0.695]	(0.130)	(0.130)	[0.278]
Assigned to busing, reading test, grade 6	-0.158	-0.024	-0.134	-0.130	-0.090	-0.040
	(0.141)	(0.131)	[0.421]	(0.136)	(0.128)	[0.797]
Assigned to busing, math test, grade 6	-0.212	-0.012	-0.200	-0.189	-0.075	-0.114
	(0.148)	(0.141)	[0.262]	(0.142)	(0.138)	[0.509]
Assigned to busing, English test, grade 7	-0.157	0.170	-0.327*	-0.113	0.096	-0.209
	(0.163)	(0.170)	[0.092]	(0.153)	(0.162)	[0.256]
Assigned to busing, reading test, grade 8	-0.296*	-0.059	-0.237	-0.269*	-0.121	-0.148
	(0.164)	(0.173)	[0.247]	(0.163)	(0.165)	[0.458]
Assigned to busing, natural science tests, grade 8	0.003	-0.101	0.104	0.083	-0.103	0.186
	(0.189)	(0.206)	[0.640]	(0.181)	(0.198)	[0.384]
R^2	0.095			0.173		
Observations	3,705			3,705		
Controls:						
Determinants of assignment	YES			YES		
Year–by–school district fixed effects	YES			YES		
Grade fixed effects	YES			YES		
Subject fixed effects	YES			YES		
Individual characteristics	NO			YES		
Mother characteristics	NO			YES		
Father characteristics	NO			YES		

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Outcomes: Dummies for taking the test conditional on attending the relevant grade in a public school in a year where the test took place. Standardized test scores conditional on having taken the test. National tests in math (grades 3 and 6), reading (grades 2, 4, 6, 8), English (grade 7) and natural science (grade 8). The natural science tests cover geography, biology, physics and chemistry. OLS of the outcome on a dummy for being bused interacted with grade, test subject, and a dummy for whether the child resides in a district with a magnet school. We control for the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. We further control for: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

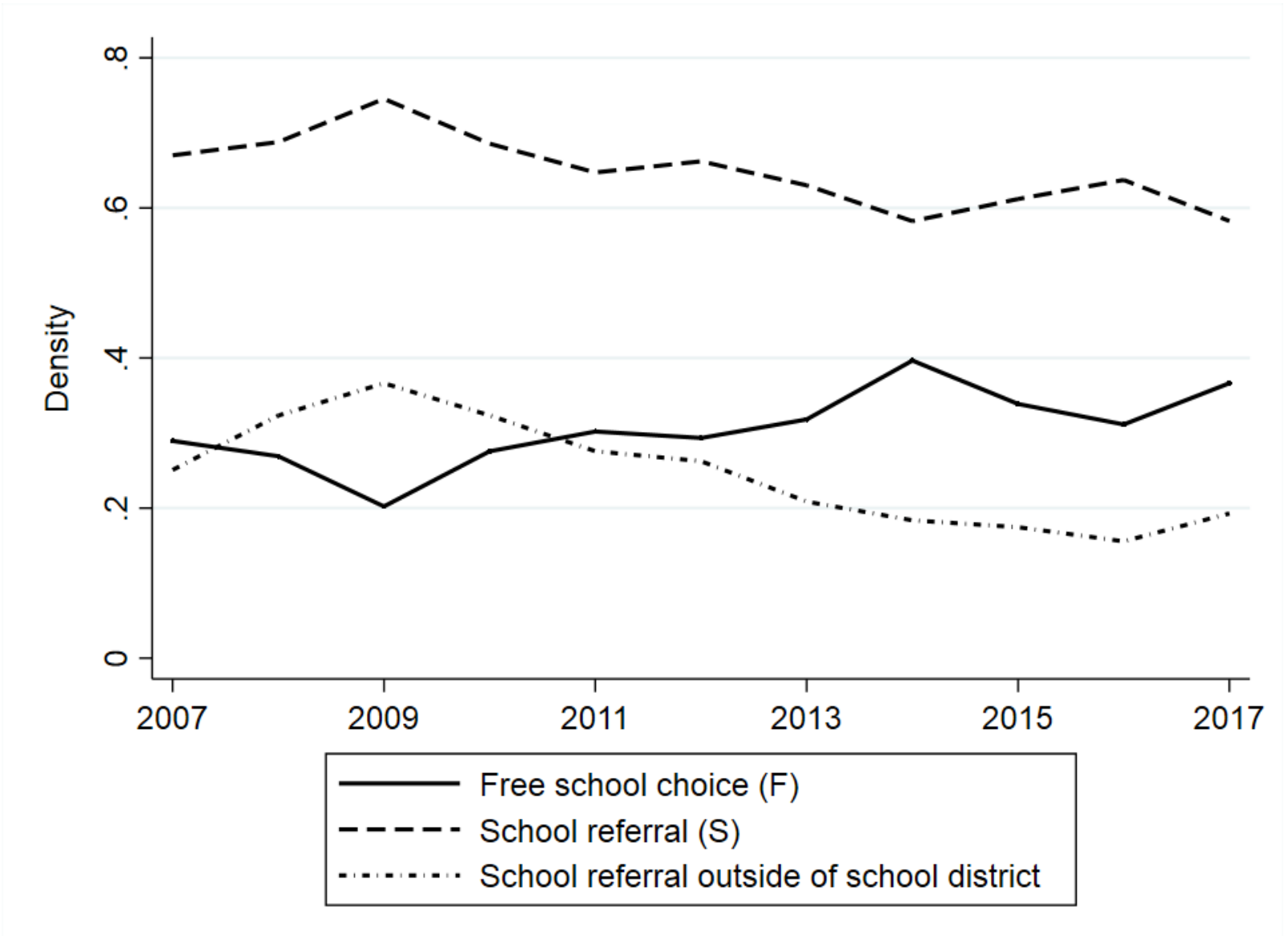
Table 10. Effect of Assignment to Busing on Wellbeing in Grades 0–3. Two Survey Factors: School Satisfaction and Distress. By Type of Sending School District.

	Dep. var.:					
	Survey taker	Survey taker	School satisfaction	School satisfaction	Distress	Distress
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
Assigned to busing, from a magnet school	-0.101**	-0.091**	-0.153	-0.192	0.174	0.170
	(0.045)	(0.040)	(0.139)	(0.142)	(0.154)	(0.158)
Assigned to busing, from a non-magnet school	0.016	-0.014	0.022	0.087	0.369	0.358*
	(0.044)	(0.049)	(0.181)	(0.195)	(0.226)	(0.208)
Difference: magnet - non-magnet	-0.117*	-0.077*	-0.175	-0.279	-0.195	-0.188
R^2	0.067	0.106	0.087	0.128	0.111	0.171
Observations	1,224	1,224	1,059	1,059	1,059	1,059
Controls:						
Determinants of assignment	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES	NO	YES

Source: Micro data from Danish Wellbeing Surveys linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1). Outcomes: a dummy for having taken the well-being survey (1, 2) and standardized factors for school satisfaction (3, 4) and distress (5, 6). OLS of the outcome on a dummy for being bused interacted with an indicator for residence in a magnet school district, and the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. We further control for: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 additionally controls for individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

Figure A1. Aarhus Municipality's Busing Policy during the Period 2007-2017



Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Note: Sample: Language screened school starters in Aarhus Municipality 2007-2017. Sample size: 5,664 school starters.

Table A1. Definitions and Data Sources of Variables

Variable	Definition	Data source
Panel A: Individual characteristics		
School starters:		
Boy	Dummy for the child being a boy.	Population register, Statistics Denmark (DST).
Age on August 1st	Continuous age on August 1st of the year of test.	Population register, Statistics Denmark (DST). Language test register, Aarhus Municipality.
Age on language test day	Continuous age on test date. Author's calculations: screening date - birthdate, divided by 365.25.	Population register, Statistics Denmark (DST). Language test register, Aarhus Municipality.
Immigrant	Dummy for the child being being born abroad from non-Danish parents.	Population register, Statistics Denmark (DST).
Descendant	Dummies for the child being born in Denmark of immigrant parents.	Population register, Statistics Denmark (DST).
Descent	Dummies for geographical descent of the child. Western countries are Europe, including former Soviet block, Australia, New Zealand, Canada, USA. Non-western countries are all the rest. When both parents are known, geographical descent is the mother's country of birth or of citizenship (if different). When the mother is not known, geographical descent is the country of birth or of citizenship (if different) of the child.	Population register, Statistics Denmark (DST).
Daycare	Dummy for having attended daycare at least once between ages 0 and 5	Population register and daycare register (1995-2014), Statistics Denmark (DST).
Number of siblings	Number of siblings (capped at 7)	Population register, Statistics Denmark (DST).
Living status	Dummies if the child lives in a household with a single parent or two parents. From family id.	Population register, Statistics Denmark (DST).

Table A1. Definitions and data sources of variables

Variable	Definition	Data source
Panel B: Mother and father characteristics		
Parents missing	Dummies for missing mother or father information in the population register.	Population register, Statistics Denmark (DST).
Age (continuous, class)	Age when the child is 4 years old. Dummy variables for whether the parent is in the following age slots: <25, 25-29, 30-34, 35-39, and >39.	Population register, Statistics Denmark (DST).
Immigrant	Dummy for being a first generation immigrant.	Population register, Statistics Denmark (DST).
Civil status	Dummies for civil status being unmarried, married, divorced, single (unmarried or divorced).	Population register, Statistics Denmark (DST).
Highest acquired education	Dummies for being a high school dropout, having graduated high school, having graduated from tertiary education, not having any education reported. Calculated when the tested child is 4 years old.	Education register, Statistics Denmark (DST).
Employment status	Dummies for being employed, unemployed, out of the labor force	Employment register, Statistics Denmark (DST).
Annual disposable income	Annual real disposable income, in USD.	Income register, Statistics Denmark (DST).
Annual disposable income, quartiles	Dummies for whether annual real disposable income is in the first to fourth quartiles of the income distribution of the Aarhus adult population of immigrant residents (age 25-54).	Income register, Statistics Denmark (DST).

Table A1. Definitions and data sources of variables

Variable	Definition	Data source
Panel C: School Assignment Policy (year of the test)		
Total score on language test	Dummies for total score category in the language test: need for reception class (category 0, M), strong language support need (category 1, S), medium language support need (category 2, S), low language support need (category 3, S), no significant language support need (category a, F).	Language test register, Aarhus Municipality.
Assignment to busing	Dummy for child being assigned to busing (treatment)	Language test register, Aarhus Municipality.
Sibling in the district school	Dummy for having at least one older sibling attending the district school in the year of the test.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Age difference with sibling in the district school	Age difference with the youngest older sibling attending the district school in the year of the test.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Distance to district school	Distance in km from the centroid of the micro-neighborhood of residence to the hectare cell position of the main entrance of the district school.	Population register, Statistics Denmark (DST). Language test register, Aarhus Municipality. Micro-neighborhoods data from Damm, Hassani and Schultz-Nielsen (2019b).
Distance to assigned school	Distance in km from the centroid of the micro-neighborhood of residence to the hectare cell position of the main entrance of the assigned school.	Population register, Statistics Denmark (DST). Language test register, Aarhus Municipality. Micro-neighborhoods data from Damm, Hassani and Schultz-Nielsen (2019b).
Sibling bused	Dummy for having at least one bused sibling at the time of the test	Population register, Statistics Denmark (DST). Language test register, Aarhus Municipality.
Attended school year of the test:		
Enrolled	Dummy for being enrolled in school on August 31st of the year of the test	Pupil register, Aarhus Municipality.
Private school	Dummy for being enrolled in private school on August 31st of the year of the test	Pupil register, Aarhus Municipality.
Attend the district school	Dummy for attending the district school in the *t* year after the test.	Population register, Statistics Denmark (DST).
Number of category-S pupils in class	Number of other category-S pupils attending the same class	Pupil register, Language test register, Aarhus Municipality.
School desire	Dummy for expressing a desire for a school different than the district school	Language test register, Aarhus Municipality.
Re-take or reassess the language test	Dummy for re-taking (or re-assessing) the language test the year after the first attempt.	Language test register, Aarhus Municipality.
First-born child	Dummy for being the first-born child in the family.	Population register, Statistics Denmark (DST).
Majority among tested children	Dummy for whether the child is the same broad descent (Africa, Middle East, East Asia) of the other children tested in her school district	Population register, Statistics Denmark (DST). Pupil and language test registers, Aarhus Municipality.
Socio-economic status	Own calculations: Low if both parents not employed, high if either parent is employed.	Population register, Employment register, Statistics Denmark (DST).

Table A1. Definitions and data sources of variables

Variable	Definition	Data source
Panel D: Outcome Variables		
National test taker	Dummies for taking the test in grades 2, 3, 4, 6, and 8. Conditional on attending public school.	DNT register (2010-2019), Statistics Denmark (DST). Pupil register, Aathus Municipality.
National test score	Standardized test score in reading (language comprehension, decoding, reading comprehension, grades 2, 4, 6, 8), math (numbers and algebra, geometry, applied mathematics, grades 3, 6, 8), English (grade 7), or science (biology, geology, physics and chemistry, grade 8). We first standardize the ability measures in the population within year, grade, subject, and cognitive area (mean 0, st. dev. 1); then we sum the standardized measures for the three cognitive areas in each subject and we standardize the final measures in the population (mean 0, st. dev. 1). See Beuchert and Nandrup (2018) for details.	DNT register (2010-2019), Statistics Denmark (DST). Pupil register, Aathus Municipality.
Wellbeing survey take up	Dummy for filling up the well-being survey conditional on being enrolled in a public school.	Danish Wellbeing Survey (DWS), Pupil register, Aarhus Municipality.
School satisfaction	School satisfaction measure from a exploratory+confirmatory factor analysis of the Danish Wellbeing Survey.	DWS, own calculations.
Distress	Distress measure from a exploratory+confirmatory factor analysis of the Danish Wellbeing Survey.	DWS, own calculations.
School absences	Share of absences over total active school days per grade in grades 0-4. Conditional on attending public school and on absence data having been recorded.	School absence register (academic years 2011-2019), Aarhus Municipality.
After-school attendance	Dummies for attending an after-school program per grade at any institution, at the attended school, or at the district school.	SFO register (2007-2015 Feb) and pupil register, Aarhus Municipality.

Table A1. Definitions and data sources of variables

Variable	Definition	Data source
Panel E: School district of residence and school characteristics		
Share of residents who are immigrants or descendants	Share of the population living in the school district with non-Danish origin or descent.	Population register and income register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Employment rate	Share of the adult population (age 25-54) living in the school district who is employed.	Population register and employment register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Share of residents with a tertiary education	Share of the adult population (age 25-54) living in the school district who has a tertiary degree.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Avg. real disposable income	Average annual real disposable income in USD, of the adult population (age 25-54) living in the school district.	Population register and employment register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.

Table A1. Definitions and data sources of variables

Variable	Definition	Data source
Panel F: School district of residence and school characteristics (cont.)		
Share of school starters who enroll in the district school	Share of all school starters living in the district who enrolls in the district school. School starters are defined as all children enrolling in school in the relevant year.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
School size	Number of pupils enrolled in all grades in the district school	Aarhus Municipality records
Class size	Average class size in the school (overall and only in grade 0) on August 1st of the relevant year.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Share of employed parents	Share of employed parents of pupils starting in the district school on August 1st of the relevant year.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Share of DAL pupils	Share of Danish as Additional Language pupils: share of tested pupils over all pupils enrolled in school on September 5 of the relevant year. It includes pupils who started school before the policy was introduced and as a consequence were not tested.	Aarhus Municipality records
Share of category-S pupils	Number of cathegory-S pupils enrolled in all grades in the district school on August 1st of the relevant year.	Population register, Statistics Denmark (DST). Pupil register and language test register, Aarhus Municipality.
Average age of teachers	Average age of teachers calculated from the age composition of teachers.	"Uddannelsesstatistik" (academic years 2007-2016), the Danish Ministry of Education. Own calculations.
Pupils per teacher	Calculated as number of pupil per full-time teacher (full-time equivalents are calculated for part-time teachers)	"Uddannelsesstatistik" (academic years 2010-2017), the Danish Ministry of Education.
Qualified staff	The share of lessons with qualified staff by subject (overall, danish, math) and grade (overall, 0-3).	"Uddannelsesstatistik" (academic years 2012-2017), the Danish Ministry of Education.
Number of Danish lessons	The annual number of math lessons across grades.	"Uddannelsesstatistik" (academic years 2010-2017), the Danish Ministry of Education.
Number of math lessons	The annual number of math lessons across grades.	"Uddannelsesstatistik" (academic years 2010-2017), the Danish Ministry of Education.
Per-pupil budget	Per pupil budget in real USD 2016.	2014-2016 school budgets, Aarhus Municipality
DAL per-pupil premium	Additional per pupil budget for DAL pupils, as percentage of the school per-pupil budget.	2014-2016 school budgets, Aarhus Municipality
Total DAL budget	Share of total school budget earmarked for development of bilingual pupils, in thousands of real USD 2016.	2014-2016 school budgets, Aarhus Municipality
Average class test score	Average class score in the National Test, by task and grade. Calculated as the average of per class averages.	DNT register (2010-2017), Statistics Denmark (DST). Pupil register, Aathus Municipality.
Std. dev. of class test score	Average class standard deviation of the score in the National Test, by task and grade. Calculated as the average of per class averages.	DNT register (2010-2017), Statistics Denmark (DST). Pupil register, Aathus Municipality.

Table A2. Sample Selection

	Sample selection criteria	N
1	School starters in Aarhus Municipality 2007/8-2016/17	33,106
2	Language screened school starters (remain alive in 2017)	5,506
3	who are considered fit for busing, i.e. no special needs	5,350
4	who are less than seven years old when taking the language screening test	5,333
5	who are referred to a regular public school [not private, special needs, or missing assignment]	5,192
6	who have a total test score “S”	3,403
7	whose district schools are not full-day schools since they follow another policy rule	2,631
8	whose district school is a sending school	1,431
9	who have no expressed preference for a school other than the district school	1,062
10	who do not live in Nordgaard or Frydenlund school districts in 2007	-
11	who do not move to Aarhus between January and school start in 2016	954

Table A3. Wellbeing Survey Questions for Pupils in Grades 0–3

	Original Question	English translation	:-(	:-|	:-)
1	Er du glad for din skole?	Are you happy with your school?	1. No	2. Yes, a little	3. Yes, a lot
2	Er du glad for din klasse?	Are you happy with your class?	1. No	2. Yes, a little	3. Yes, a lot
3	Føler du dig alene i skolen?	Do you feel alone in school?	1. Yes, often	2. Yes, sometimes	3. No
4	Kan du lide pauserne i skolen?	Do you like the breaks at school?	1. No	2. Yes, a little	3. Yes, a lot
5	Er du glad for dine lærere?	Are you happy with your teachers?	1. No	2. Yes, a little	3. Yes, a lot
6	Har du ondt i maven, når du er i skole?	Do you have stomachache, when you are in school?	1. Yes, often	2. Yes, sometimes	3. No
7	Har du ondt i hovedet, når du er i skole?	Do you have headache, when you are in school?	1. Yes, often	2. Yes, sometimes	3. No
8	Er du god til at løse dine problemer?	Are you good at solving your problems?	1. No	2. Yes, sometimes	3. Yes, most of the times
9	Kan du koncentrere dig i timerne?	Can you concentrate during lessons?	1. No	2. Yes, sometimes	3. Yes, most of the times
10	Er I gode til at hjælpe hinanden i klassen?	Are you good at helping each other in class?	1. No	2. Yes, a little	3. Yes, a lot
11	Tror du, at de andre børn i klassen kan lide dig?	Do you think that the other kids in class like you?	1. No	2. Yes, a few	3. Yes, most of them
12	Er lærerne gode til at hjælpe dig i skolen?	Are teachers good at helping you in school?	1. No	2. Yes, a little	3. Yes, a lot
13	Er der nogen, der driller dig, så du bliver ked af det?	Is there someone who teases you, so that you get upset?	1. Yes, often	2. Yes, sometimes	3. No
14	Er du bange for, at de andre børn griner ad dig i skolen?	Are you afraid that the other kids laugh at you in school?	1. Yes, a lot	2. Yes, a little	3. No
15	Er du med til at bestemme, hvad I skal lave i timerne?	Do you help decide what you do during lessons?	1. No	2. Yes, sometimes	3. Yes, often
16	Er timerne kedelige?	Are lessons boring?	1. Yes, often	2. Yes, sometimes	3. No
17	Lærer du noget spændende i skolen?	Do you learn anything exciting in school?	1. No	2. Yes, a little	3. Yes, a lot
18	Er det svært at høre, hvad læreren siger i timerne?	Is it difficult to hear what the teacher says during lessons?	1. Yes, often	2. Yes, sometimes	3. No
19	Er jeres klasselokale rart at være i?	Are your classrooms nice to be in?	1. Yes, a lot	2. Yes, a little	3. No
20	Er toiletterne på skolen rene?	Are toilets in school clean?	1. Yes, most of the times	2. Yes, sometimes	3. No

Source: Danish Wellbeing Survey questionnaire to public school pupils in grades 0–3, 2015-2018.

Table A4. Test of the Identification Strategy. Full Set of Covariates.

	Dependent variable: Assignment to busing							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Explanatory variables:								
Sibling in the district school	-0.484***	-0.483***	-0.476***	-0.482***	-0.485***	-0.487***	-0.478***	-0.461***
	(0.036)	(0.036)	(0.038)	(0.039)	(0.039)	(0.040)	(0.042)	(0.047)
Age difference with youngest sibling in the district school	0.024***	0.024***	0.022***	0.023***	0.023***	0.024***	0.023***	0.020***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Distance from the district school	0.050***	0.050***	0.050***	0.048**	0.048**	0.053**	0.048**	0.046**
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.021)	(0.019)	(0.020)
Medium language support need		0.004	-0.004	-0.002	-0.002	-0.005	0.041	0.040
		(0.032)	(0.033)	(0.034)	(0.034)	(0.034)	(0.052)	(0.054)
Low language support need		-0.000	-0.007	-0.004	-0.004	-0.008	0.091*	0.091*
		(0.032)	(0.033)	(0.034)	(0.034)	(0.034)	(0.049)	(0.052)
Strong language support need, first tested in the family							0.098	0.135*
							(0.060)	(0.069)
Medium language support need, first tested in the family							0.042	0.069
							(0.037)	(0.043)
Low language support need, first tested in the family							-0.059*	-0.030
							(0.035)	(0.042)
Age on language test day			0.021	0.021	0.023	0.024		0.019
			(0.029)	(0.029)	(0.029)	(0.029)		(0.029)
Sibling assigned to busing			0.028	0.025	0.024	0.021		0.048
			(0.032)	(0.032)	(0.032)	(0.032)		(0.039)
Male			0.019	0.019	0.016	0.015		0.018
			(0.022)	(0.022)	(0.022)	(0.022)		(0.022)
Neither immigrant nor descendant			-0.154**	-0.126*	-0.103	-0.101		-0.110
			(0.061)	(0.073)	(0.085)	(0.085)		(0.086)
Immigrant			-0.089**	-0.091*	-0.084*	-0.093*		-0.092*
			(0.043)	(0.048)	(0.048)	(0.049)		(0.049)
Origin or descent: Africa			-0.038	-0.042	-0.039	-0.042		-0.036
			(0.028)	(0.029)	(0.030)	(0.030)		(0.030)
Origin or descent: Western countries[1]			0.056	0.047	0.053	0.047		0.053
			(0.050)	(0.051)	(0.052)	(0.052)		(0.052)
Origin or descent: East Asia[2]			-0.031	-0.043	-0.043	-0.047		-0.041
			(0.037)	(0.038)	(0.039)	(0.039)		(0.039)
Attended daycare			0.041	0.038	0.044	0.039		0.053
			(0.050)	(0.052)	(0.052)	(0.053)		(0.051)
Number of siblings: 1			-0.034	-0.032	-0.036	-0.040		-0.037
			(0.066)	(0.066)	(0.066)	(0.066)		(0.067)
Number of siblings: 2			-0.022	-0.025	-0.021	-0.028		-0.020
			(0.066)	(0.066)	(0.067)	(0.066)		(0.067)
Number of siblings: 3			-0.025	-0.029	-0.029	-0.036		-0.031
			(0.068)	(0.068)	(0.069)	(0.068)		(0.070)
Number of siblings: 4			-0.018	-0.018	-0.017	-0.022		-0.019
			(0.072)	(0.074)	(0.074)	(0.074)		(0.075)
Number of siblings: 5			-0.028	-0.030	-0.027	-0.034		-0.024
			(0.075)	(0.077)	(0.078)	(0.078)		(0.078)
Number of siblings: 6			-0.071	-0.069	-0.073	-0.075		-0.074
			(0.075)	(0.077)	(0.079)	(0.079)		(0.078)
Number of siblings: 7 or more			-0.033	-0.040	-0.047	-0.052		-0.050
			(0.075)	(0.078)	(0.079)	(0.081)		(0.080)
No mother recorded in register datasets			0.049	0.050	0.039	0.028		0.030
			(0.084)	(0.097)	(0.096)	(0.097)		(0.096)
No father recorded in register datasets			0.009	0.002	0.023	0.025		0.017
			(0.053)	(0.053)	(0.071)	(0.072)		(0.070)
Living in two-parent household			-0.004	0.005	0.005	0.008		0.009
			(0.025)	(0.028)	(0.029)	(0.029)		(0.029)
Mother's age: under 25				0.036	0.053	0.056		0.049
				(0.053)	(0.058)	(0.058)		(0.057)
Mother's age: 25 to 29				0.026	0.035	0.038		0.028
				(0.044)	(0.049)	(0.050)		(0.049)

(continued)

Table A4. (continued)

Mother's age: 30 to 34	0.009	0.012	0.010	0.007
	(0.042)	(0.044)	(0.044)	(0.043)
Mother's age: 35 to 39	0.005	0.007	0.008	-0.000
	(0.041)	(0.041)	(0.042)	(0.041)
Mother: immigrant	0.023	0.037	0.033	0.026
	(0.060)	(0.060)	(0.060)	(0.062)
Mother: high school dropout	-0.004	-0.007	-0.009	-0.009
	(0.030)	(0.031)	(0.031)	(0.030)
Mother: education not reported	-0.034	-0.033	-0.027	-0.035
	(0.034)	(0.034)	(0.035)	(0.035)
Mother: completed university	-0.036	-0.038	-0.034	-0.037
	(0.033)	(0.034)	(0.033)	(0.033)
Mother: employed (includes self-employed)	0.005	0.017	0.021	0.013
	(0.027)	(0.028)	(0.028)	(0.028)
Mother: unemployed	0.030	0.036	0.037	0.036
	(0.043)	(0.043)	(0.043)	(0.042)
Mother's disposable income: second quartile	-0.048	-0.058	-0.058	-0.056
	(0.038)	(0.039)	(0.039)	(0.039)
Mother's disposable income: third quartile	-0.018	-0.029	-0.026	-0.018
	(0.037)	(0.038)	(0.038)	(0.038)
Mother's disposable income: fourth quartile	0.018	0.011	0.011	0.020
	(0.042)	(0.045)	(0.045)	(0.045)
Father's age: under 25		-0.021	-0.028	-0.019
		(0.075)	(0.074)	(0.076)
Father's age: 25 to 29		-0.025	-0.028	-0.023
		(0.047)	(0.047)	(0.047)
Father's age: 30 to 34		-0.011	-0.013	-0.009
		(0.038)	(0.038)	(0.038)
Father's age: 35 to 39		0.001	-0.003	0.005
		(0.031)	(0.031)	(0.031)
Father: immigrant		0.014	0.015	0.013
		(0.070)	(0.071)	(0.070)
Father: high school dropout		0.026	0.025	0.028
		(0.030)	(0.030)	(0.030)
Father: education not reported		-0.001	0.001	0.002
		(0.033)	(0.032)	(0.032)
Father: completed university		-0.012	-0.010	-0.007
		(0.032)	(0.033)	(0.032)
Father: employed (includes self-employed)		-0.034	-0.030	-0.033
		(0.027)	(0.028)	(0.027)
Father: unemployed		-0.051	-0.050	-0.049
		(0.039)	(0.040)	(0.039)
Father's disposable income: second quartile		0.021	0.027	0.020
		(0.030)	(0.030)	(0.030)
Father's disposable income: third quartile		0.036	0.037	0.034
		(0.035)	(0.035)	(0.035)
Father's disposable income: fourth quartile		-0.003	-0.000	0.000
		(0.040)	(0.041)	(0.040)
Neigborhood: share of immigrants			0.843*	
			(0.493)	
Neigborhood: share of employed adults			0.223	
			(0.287)	
Neigborhood: share of adults with tertiary education			-0.139	
			(0.478)	
Neigborhood: average real disposable income			-0.131	
			(7.217)	

(continued)

Table A4. (continued)

R^2	0.643	0.643	0.650	0.653	0.655	0.657	0.647	0.659
Observations	954	954	954	954	954	954	954	954
Controls:								
Determinants of assignment	YES	YES	YES	YES	YES	YES	YES	YES
Year–by–school district fixed	YES	YES	YES	YES	YES	YES	YES	YES
Individual characteristics	NO	NO	YES	YES	YES	YES	NO	YES
Mother characteristics	NO	NO	NO	YES	YES	YES	NO	YES
Father characteristics	NO	NO	NO	NO	YES	YES	NO	YES
Neighborhood characteristics	NO	NO	NO	NO	NO	YES	NO	NO
additional controls			0.765	0.701	0.779	0.791		0.914
P-value F-test			0.764	0.900	0.858	0.855		0.644

Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing, are less than seven years old when taking the language screening test, are referred to a regular public school, have a total test score "S", live in a regular school district (without a full-day school) with a sending school, who have not expressed desire for another school than the district school, who do not reside in Norgaards and Frydelund school districts in 2007, and who do not move to Aarhus between January and school start in 2016. OLS of dummy for being bused (sample avg 0.504) over the municipality assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school and language test-year–by–school district of residence fixed effects. Other controls include: individual characteristics including the assessed level of language support need (2-6,8), dummies for having an older sibling bused, and the continuous age of the pupil on the day of the test (3-6,8), mother (4-6,8) and father characteristics (5-6,8). We also add characteristics of neighborhoods of residence (6). In columns 7 and 8 the assessed level of language support need is separated for pupils who are the first in their family to be language tested. Fixed effects for 12 school districts of residence in each year of the period 2007-2016. F-test on individual, mother and father characteristics, age, level of language support, and sibling bused. Individual characteristics of the child include gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Neighborhood characteristics include: share of immigrants, share of employed adults (age 25-54), share of adults with tertiary education, average real disposable income (million USD).

[1] Western Europe (incl. former Soviet block), Australia, New Zealand, Canada, USA

[2] Excl. MiddleEast and former Soviet block

Table A5.a Determinants of Compliance with the Policy. Treatment Group (Assigned to Busing).

	Dependent variable: Enrollment in the assigned school						
	Grade						
	0	1	2	3	4	5	6
Explanatory variables:							
Sibling in the district school	0.008	0.126	0.162	-0.048	-0.011	-0.023	-0.003
	(0.100)	(0.113)	(0.117)	(0.114)	(0.112)	(0.114)	(0.112)
Age difference with youngest sibling in the district school	0.031*	-0.024	-0.011	0.008	0.008	0.015	0.011
	(0.017)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
Distance from the district school	-0.041	0.025	0.043	0.119*	0.099	0.127**	0.133**
	(0.056)	(0.063)	(0.065)	(0.063)	(0.063)	(0.063)	(0.062)
Age on language test day	-0.037	0.049	-0.021	-0.039	-0.002	0.008	-0.044
	(0.055)	(0.062)	(0.064)	(0.062)	(0.062)	(0.062)	(0.061)
Medium language support need	-0.055	-0.073	-0.064	-0.074	0.002	-0.004	0.067
	(0.069)	(0.078)	(0.080)	(0.079)	(0.080)	(0.081)	(0.080)
Low language support need	-0.015	-0.037	-0.036	-0.060	-0.006	0.005	0.047
	(0.071)	(0.080)	(0.082)	(0.081)	(0.083)	(0.085)	(0.085)
Male	0.081*	0.092*	0.079	0.034	0.015	0.083	0.064
	(0.045)	(0.051)	(0.052)	(0.051)	(0.051)	(0.052)	(0.052)
Immigrant	-0.220	-0.111	-0.029	0.040	0.073	0.169	0.160
	(0.200)	(0.226)	(0.233)	(0.227)	(0.231)	(0.233)	(0.230)
Neither immigrant nor descendant	0.178	0.104	0.164	0.119	0.082	0.087	0.162
	(0.114)	(0.129)	(0.132)	(0.131)	(0.130)	(0.132)	(0.135)
Origin or descent: Africa	0.064	-0.069	-0.088	-0.105*	-0.121*	-0.064	-0.077
	(0.055)	(0.062)	(0.064)	(0.063)	(0.064)	(0.065)	(0.064)
Origin or descent: Western countries[1]	0.130	0.012	-0.121	-0.248*	-0.211	-0.221	-0.264*
	(0.111)	(0.125)	(0.128)	(0.128)	(0.130)	(0.135)	(0.135)
Origin or descent: East Asia[2]	0.193**	0.102	-0.032	-0.057	-0.110	-0.083	-0.042
	(0.092)	(0.104)	(0.107)	(0.104)	(0.103)	(0.103)	(0.103)
Attended daycare	-0.028	0.008	0.200	0.156	0.109	0.053	-0.011
	(0.151)	(0.170)	(0.175)	(0.179)	(0.178)	(0.176)	(0.172)
Number of siblings: 1	0.156	0.038	0.063	0.127	0.132	0.011	-0.083
	(0.148)	(0.167)	(0.171)	(0.171)	(0.169)	(0.176)	(0.191)
Number of siblings: 2	0.206	0.131	0.122	0.204	0.148	0.005	-0.078
	(0.149)	(0.168)	(0.173)	(0.172)	(0.171)	(0.176)	(0.192)
Number of siblings: 3	0.233	0.191	0.162	0.221	0.191	0.084	-0.006
	(0.149)	(0.168)	(0.173)	(0.172)	(0.170)	(0.175)	(0.190)
Number of siblings: 4	0.255	0.086	0.094	0.200	0.155	0.065	-0.022
	(0.157)	(0.177)	(0.182)	(0.183)	(0.183)	(0.189)	(0.203)
Number of siblings: 5	0.163	0.113	0.078	0.153	0.118	0.016	-0.090
	(0.163)	(0.184)	(0.189)	(0.187)	(0.186)	(0.191)	(0.206)
Number of siblings: 6	0.160	0.160	0.143	0.166	-0.019	-0.140	-0.231
	(0.181)	(0.204)	(0.210)	(0.208)	(0.206)	(0.211)	(0.222)
Number of siblings: 7 or more	0.225	0.035	0.132	0.174	0.134	0.021	-0.077
	(0.170)	(0.192)	(0.198)	(0.197)	(0.197)	(0.202)	(0.217)
No mother recorded in register datasets	-0.016	0.379	0.311	0.206	0.197	0.168	0.365
	(0.224)	(0.253)	(0.260)	(0.263)	(0.269)	(0.267)	(0.270)
No father recorded in register datasets	0.037	0.087	0.159	0.188	0.345*	0.171	0.100
	(0.150)	(0.169)	(0.174)	(0.170)	(0.176)	(0.183)	(0.182)
Living in two-parent household	-0.084	-0.028	-0.042	0.038	0.015	0.029	0.038
	(0.058)	(0.065)	(0.067)	(0.066)	(0.067)	(0.067)	(0.067)
Mother's age: under 25	-0.124	0.008	-0.001	-0.103	-0.126	-0.148	-0.227*
	(0.113)	(0.128)	(0.131)	(0.129)	(0.129)	(0.130)	(0.131)
Mother's age: 25 to 29	-0.119	0.007	0.035	-0.140	-0.195*	-0.220**	-0.250**
	(0.094)	(0.106)	(0.109)	(0.107)	(0.107)	(0.107)	(0.107)
Mother's age: 30 to 34	-0.065	0.021	-0.009	-0.183*	-0.203**	-0.246**	-0.300***
	(0.087)	(0.098)	(0.101)	(0.099)	(0.099)	(0.100)	(0.102)
Mother's age: 35 to 39	-0.070	0.081	0.074	-0.029	-0.084	-0.129	-0.170*
	(0.087)	(0.098)	(0.101)	(0.098)	(0.100)	(0.100)	(0.101)
Mother: immigrant	-0.152	-0.121	-0.071	-0.054	-0.042	-0.022	-0.049
	(0.173)	(0.195)	(0.200)	(0.195)	(0.204)	(0.205)	(0.202)
Mother: high school dropout	0.054	0.048	0.039	-0.036	-0.028	-0.029	-0.029
	(0.061)	(0.069)	(0.071)	(0.069)	(0.069)	(0.070)	(0.069)
Mother: education not reported	0.053	-0.013	0.014	0.041	0.039	0.049	-0.003
	(0.075)	(0.085)	(0.087)	(0.086)	(0.087)	(0.087)	(0.087)
Mother: completed university	0.011	-0.000	-0.016	-0.077	-0.048	-0.057	-0.098
	(0.070)	(0.080)	(0.082)	(0.080)	(0.081)	(0.082)	(0.082)
Mother: employed (includes self-employed)	0.055	0.040	0.060	0.025	-0.020	0.046	0.005
	(0.060)	(0.068)	(0.070)	(0.069)	(0.069)	(0.069)	(0.070)

(continued)

Table A5.a. (continued)

Mother: unemployed	0.131*	0.262***	0.160*	0.185**	0.097	0.156*	0.066
	(0.079)	(0.089)	(0.091)	(0.089)	(0.093)	(0.094)	(0.093)
Mother's disposable income: second quartile	-0.008	-0.036	-0.010	-0.026	0.023	-0.034	0.034
	(0.073)	(0.082)	(0.085)	(0.082)	(0.084)	(0.085)	(0.087)
Mother's disposable income: third quartile	-0.042	0.013	0.033	-0.018	-0.033	-0.024	0.018
	(0.076)	(0.086)	(0.088)	(0.086)	(0.089)	(0.090)	(0.093)
Mother's disposable income: fourth quartile	-0.038	0.058	-0.039	0.059	0.082	0.054	0.110
	(0.091)	(0.102)	(0.105)	(0.102)	(0.103)	(0.105)	(0.109)
Father's age: under 25	-0.027	0.121	-0.038	-0.034	-0.009	0.095	0.119
	(0.143)	(0.161)	(0.166)	(0.160)	(0.160)	(0.164)	(0.167)
Father's age: 25 to 29	-0.018	0.038	0.029	0.012	0.058	0.032	0.051
	(0.087)	(0.098)	(0.101)	(0.099)	(0.098)	(0.098)	(0.097)
Father's age: 30 to 34	0.045	0.089	0.043	0.091	0.086	0.053	0.054
	(0.073)	(0.082)	(0.084)	(0.083)	(0.084)	(0.085)	(0.084)
Father's age: 35 to 39	0.028	0.003	0.034	0.100	0.027	0.008	0.054
	(0.062)	(0.070)	(0.072)	(0.071)	(0.071)	(0.072)	(0.072)
Father: immigrant	-0.054	0.047	0.010	-0.006	0.202	0.164	0.137
	(0.148)	(0.167)	(0.171)	(0.167)	(0.168)	(0.169)	(0.170)
Father: high school dropout	0.005	-0.088	-0.061	-0.084	-0.103	-0.088	-0.104
	(0.059)	(0.067)	(0.069)	(0.067)	(0.067)	(0.067)	(0.068)
Father: education not reported	0.048	-0.063	-0.056	-0.035	-0.010	-0.083	-0.083
	(0.064)	(0.072)	(0.074)	(0.073)	(0.074)	(0.075)	(0.075)
Father: completed university	0.037	0.090	0.059	0.075	-0.012	-0.048	-0.115
	(0.069)	(0.078)	(0.081)	(0.078)	(0.079)	(0.079)	(0.078)
Father: employed (includes self-employed)	-0.058	-0.134**	-0.166**	-0.155**	-0.156**	-0.154**	-0.187***
	(0.057)	(0.064)	(0.066)	(0.064)	(0.065)	(0.067)	(0.067)
Father: unemployed	0.026	-0.102	-0.118	-0.014	-0.059	-0.107	-0.055
	(0.081)	(0.091)	(0.094)	(0.091)	(0.091)	(0.093)	(0.092)
Father's disposable income: second quartile	-0.008	-0.009	-0.028	-0.012	-0.015	-0.041	-0.041
	(0.057)	(0.064)	(0.066)	(0.064)	(0.065)	(0.066)	(0.066)
Father's disposable income: third quartile	0.034	-0.056	-0.014	-0.100	-0.016	-0.037	-0.066
	(0.073)	(0.082)	(0.085)	(0.083)	(0.083)	(0.084)	(0.084)
Father's disposable income: fourth quartile	-0.033	-0.007	-0.043	-0.057	0.003	0.014	0.039
	(0.087)	(0.098)	(0.101)	(0.099)	(0.099)	(0.099)	(0.101)
R^2	0.244	0.225	0.220	0.273	0.278	0.262	0.287
Observations	474	474	474	458	437	414	389
Controls:							
Determinants of assignment	YES	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES	YES
Individual characteristics	YES	YES	YES	YES	YES	YES	YES
Mother characteristics	YES	YES	YES	YES	YES	YES	YES
Father characteristics	YES	YES	YES	YES	YES	YES	YES
F-test joint insignificance for additional controls	1.176	1.009	0.872	1.208	1.039	1.071	1.238
P-value F-test	0.208	0.461	0.712	0.174	0.409	0.357	0.149

Source: Danish National Tests linked with Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). OLS of dummy for being enrolled in the assigned school in the end of August of the relevant year over the municipality assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school and language test year–by–school district of residence fixed effects. Other controls include: individual characteristics, mother and father characteristics. Fixed effects for 10 school districts in each year from 2007-2016. F-test on individual, mother and father characteristics, age, level of language support, and sibling bused.

[1] Western Europe (incl. former Soviet block), Australia, New Zealand, Canada, USA

[2] Excl. MiddleEast and former Soviet block

Table A5.b Determinants of Compliance with the Policy. Control Group (Assigned to District School)

	Dependent variable: Enrollment in the assigned school						
	Grade						
	0	1	2	3	4	5	6
Explanatory variables:							
Sibling in the district school	0.228***	0.205***	0.145**	0.174**	0.148*	0.111	0.063
	(0.044)	(0.057)	(0.062)	(0.070)	(0.079)	(0.096)	(0.118)
Age difference with youngest sibling in the district school	-0.014*	-0.014	-0.005	0.002	0.005	0.005	0.008
	(0.008)	(0.011)	(0.011)	(0.013)	(0.015)	(0.017)	(0.019)
Distance from the district school	-0.027	-0.002	-0.006	0.004	0.067	0.103*	0.097
	(0.021)	(0.027)	(0.029)	(0.046)	(0.049)	(0.060)	(0.067)
Age on language test day	0.031	0.112**	0.167***	0.084	0.095	-0.014	0.105
	(0.042)	(0.054)	(0.058)	(0.065)	(0.072)	(0.087)	(0.102)
Medium language support need	0.131***	0.162***	0.171***	0.186**	0.162*	0.054	0.047
	(0.047)	(0.061)	(0.065)	(0.077)	(0.085)	(0.106)	(0.123)
Low language support need	0.163***	0.201***	0.195***	0.237***	0.202**	0.149	0.180
	(0.046)	(0.060)	(0.065)	(0.076)	(0.084)	(0.101)	(0.117)
Male	-0.099***	-0.075**	-0.052	-0.093**	-0.069	-0.060	0.025
	(0.029)	(0.038)	(0.041)	(0.046)	(0.053)	(0.062)	(0.071)
Immigrant	0.037	0.224	0.307**	0.509***	0.539**	0.558**	0.364
	(0.108)	(0.139)	(0.150)	(0.186)	(0.208)	(0.277)	(0.290)
Neither immigrant nor descendant	0.033	0.019	0.040	0.040	-0.045	-0.095	-0.106
	(0.061)	(0.079)	(0.085)	(0.101)	(0.115)	(0.146)	(0.158)
Origin or descent: Africa	-0.063	-0.042	-0.049	-0.036	-0.026	-0.046	0.104
	(0.042)	(0.054)	(0.058)	(0.064)	(0.073)	(0.089)	(0.104)
Origin or descent: Western countries[1]	-0.125*	-0.127	-0.244**	-0.477***	-0.526***	-0.636***	-0.354*
	(0.071)	(0.092)	(0.099)	(0.121)	(0.139)	(0.172)	(0.190)
Origin or descent: East Asia[2]	-0.100*	-0.005	0.043	0.032	0.037	-0.025	0.018
	(0.051)	(0.066)	(0.071)	(0.078)	(0.084)	(0.100)	(0.115)
Attended daycare	0.023	0.151	0.022	-0.246	-0.214	-0.551	-0.974*
	(0.106)	(0.137)	(0.147)	(0.178)	(0.231)	(0.393)	(0.559)
Number of siblings: 1	0.166*	0.121	0.138	0.119	-0.078	-0.309	0.067
	(0.097)	(0.126)	(0.135)	(0.191)	(0.220)	(0.366)	(0.375)
Number of siblings: 2	0.098	-0.014	-0.010	-0.042	-0.186	-0.364	0.023
	(0.097)	(0.126)	(0.135)	(0.188)	(0.218)	(0.358)	(0.361)
Number of siblings: 3	0.081	0.008	0.094	0.095	-0.150	-0.378	0.075
	(0.103)	(0.133)	(0.143)	(0.198)	(0.230)	(0.363)	(0.369)
Number of siblings: 4	0.084	0.010	0.097	0.063	-0.140	-0.397	0.040
	(0.107)	(0.138)	(0.149)	(0.200)	(0.234)	(0.370)	(0.377)
Number of siblings: 5	-0.020	-0.103	-0.027	-0.124	-0.322	-0.533	-0.216
	(0.117)	(0.151)	(0.162)	(0.212)	(0.244)	(0.389)	(0.397)
Number of siblings: 6	0.046	-0.017	-0.041	-0.089	-0.448*	-0.602	-0.150
	(0.115)	(0.148)	(0.159)	(0.208)	(0.237)	(0.376)	(0.384)
Number of siblings: 7 or more	0.066	-0.005	0.060	-0.011	-0.246	-0.338	0.077
	(0.113)	(0.146)	(0.157)	(0.210)	(0.245)	(0.379)	(0.398)
No mother recorded in register datasets	-0.186	0.009	0.167	0.250	0.299	0.357	0.676*
	(0.132)	(0.170)	(0.183)	(0.211)	(0.231)	(0.313)	(0.357)
No father recorded in register datasets	0.070	0.034	-0.011	0.106	0.159	-0.049	0.087
	(0.105)	(0.136)	(0.147)	(0.162)	(0.190)	(0.259)	(0.308)
Living in two-parent household	0.008	0.033	0.073	0.046	0.017	-0.021	0.006
	(0.036)	(0.046)	(0.049)	(0.055)	(0.062)	(0.075)	(0.086)
Mother's age: under 25	0.056	-0.035	-0.142	-0.033	-0.095	-0.159	-0.205
	(0.075)	(0.097)	(0.104)	(0.119)	(0.136)	(0.176)	(0.191)
Mother's age: 25 to 29	-0.013	-0.059	-0.071	-0.036	-0.045	-0.030	-0.026
	(0.062)	(0.080)	(0.086)	(0.094)	(0.104)	(0.124)	(0.135)
Mother's age: 30 to 34	-0.042	-0.051	-0.055	-0.019	-0.026	-0.129	-0.139
	(0.054)	(0.069)	(0.074)	(0.081)	(0.091)	(0.110)	(0.120)
Mother's age: 35 to 39	-0.003	-0.063	-0.025	-0.028	-0.046	-0.121	-0.105
	(0.048)	(0.062)	(0.067)	(0.073)	(0.084)	(0.102)	(0.112)
Mother: immigrant	-0.044	0.044	-0.069	0.061	0.140	0.210	0.378
	(0.078)	(0.101)	(0.109)	(0.135)	(0.151)	(0.237)	(0.253)
Mother: high school dropout	-0.049	-0.040	-0.090	-0.078	-0.122*	-0.108	-0.073
	(0.041)	(0.053)	(0.057)	(0.064)	(0.072)	(0.083)	(0.095)
Mother: education not reported	-0.031	-0.041	-0.114*	-0.145**	-0.125	-0.057	-0.020
	(0.045)	(0.058)	(0.063)	(0.073)	(0.082)	(0.094)	(0.109)
Mother: completed university	0.015	0.002	-0.085	-0.118*	-0.161**	-0.150	-0.265**
	(0.046)	(0.059)	(0.064)	(0.071)	(0.080)	(0.095)	(0.110)
Mother: employed (includes self-employed)	0.001	-0.020	0.026	0.012	-0.039	0.027	0.060
	(0.040)	(0.051)	(0.055)	(0.063)	(0.071)	(0.084)	(0.100)

(continued)

Table A5.b. (continued)

Mother: unemployed	-0.032	-0.075	-0.020	0.079	0.116	0.077	0.196
	(0.067)	(0.087)	(0.093)	(0.112)	(0.132)	(0.160)	(0.188)
Mother's disposable income: second quartile	0.001	0.026	0.039	-0.001	-0.057	-0.137	-0.144
	(0.049)	(0.064)	(0.068)	(0.080)	(0.090)	(0.111)	(0.126)
Mother's disposable income: third quartile	0.024	0.070	0.086	0.124	0.059	-0.050	0.003
	(0.047)	(0.060)	(0.065)	(0.076)	(0.086)	(0.108)	(0.120)
Mother's disposable income: fourth quartile	0.047	0.095	0.096	0.078	-0.025	-0.185	-0.110
	(0.056)	(0.072)	(0.077)	(0.089)	(0.098)	(0.122)	(0.135)
Father's age: under 25	0.075	-0.006	0.239*	0.217	0.170	0.229	0.138
	(0.104)	(0.134)	(0.145)	(0.163)	(0.185)	(0.222)	(0.240)
Father's age: 25 to 29	-0.134*	-0.070	-0.002	-0.027	-0.032	-0.214	-0.061
	(0.069)	(0.089)	(0.096)	(0.111)	(0.121)	(0.141)	(0.162)
Father's age: 30 to 34	-0.087*	-0.062	0.009	0.058	-0.004	-0.030	0.002
	(0.050)	(0.065)	(0.070)	(0.079)	(0.087)	(0.104)	(0.118)
Father's age: 35 to 39	-0.007	0.024	0.003	-0.012	-0.065	-0.058	-0.088
	(0.042)	(0.054)	(0.058)	(0.066)	(0.073)	(0.085)	(0.092)
Father: immigrant	-0.066	0.055	0.105	0.111	0.062	-0.058	0.013
	(0.090)	(0.116)	(0.126)	(0.159)	(0.174)	(0.213)	(0.236)
Father: high school dropout	0.020	0.063	0.067	0.117*	0.199***	0.188**	0.138
	(0.041)	(0.053)	(0.057)	(0.066)	(0.076)	(0.092)	(0.107)
Father: education not reported	-0.000	0.058	0.012	-0.012	0.012	-0.007	0.046
	(0.045)	(0.058)	(0.063)	(0.070)	(0.078)	(0.091)	(0.103)
Father: completed university	0.024	0.079	0.064	0.136**	0.179**	0.204**	0.271**
	(0.043)	(0.055)	(0.060)	(0.068)	(0.078)	(0.093)	(0.109)
Father: employed (includes self-employed)	-0.019	-0.049	-0.034	-0.011	0.029	-0.072	-0.037
	(0.039)	(0.051)	(0.054)	(0.061)	(0.067)	(0.083)	(0.094)
Father: unemployed	0.015	-0.040	0.068	0.143	0.129	0.091	-0.071
	(0.061)	(0.078)	(0.086)	(0.100)	(0.120)	(0.142)	(0.170)
Father's disposable income: second quartile	0.032	0.032	0.037	-0.015	0.039	0.114	0.147
	(0.041)	(0.053)	(0.057)	(0.064)	(0.072)	(0.086)	(0.096)
Father's disposable income: third quartile	0.102**	0.016	0.056	0.017	0.044	0.104	0.066
	(0.046)	(0.060)	(0.064)	(0.074)	(0.082)	(0.100)	(0.114)
Father's disposable income: fourth quartile	0.053	0.056	0.088	0.024	-0.036	-0.005	0.045
	(0.052)	(0.067)	(0.072)	(0.081)	(0.091)	(0.107)	(0.117)
R^2	0.353	0.310	0.308	0.398	0.433	0.467	0.545
Observations	480	480	478	410	348	275	219
Controls:							
Determinants of assignment	YES	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES	YES
Individual characteristics	YES	YES	YES	YES	YES	YES	YES
Mother characteristics	YES	YES	YES	YES	YES	YES	YES
Father characteristics	YES	YES	YES	YES	YES	YES	YES
F-test joint insignificance for	2.565	1.657	1.697	2.201	1.958	1.417	1.385
P-value F-test	0.000	0.006	0.004	0.000	0.001	0.054	0.075

Source: Danish National Tests linked with Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). OLS of dummy for being enrolled in the assigned school in the end of August of the relevant year over the municipality assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school and language test year–by–school district of residence fixed effects. Other controls include: individual characteristics, mother and father characteristics. Fixed effects for 10 school districts in each year from 2007-2016. F-test on individual, mother and father characteristics, age, level of language support, and sibling bused.

[1] Western Europe (incl. former Soviet block), Australia, New Zealand, Canada, USA

[2] Excl. MiddleEast and former Soviet block

Table A6. Effects of Assigment to Busing on Wellbeing in Grades 0–3. Two Survey Factors: School Satisfaction and Distress. By Sex, Socio-Economic Status, Origin/Descent, Language Support Need.

	Dependent variable:								
	Survey taker	Survey taker	Survey taker	School satisfaction	School satisfaction	School satisfaction	Distress	Distress	Distress
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Explanatory variables:									
Assigned to busing, boys	-0.091**			-0.172			0.300**		
	(0.038)			(0.150)			(0.148)		
Assigned to busing, girls	-0.031			0.029			0.147		
	(0.046)			(0.150)			(0.168)		
Assigned to busing, high SES		-0.086**			-0.185			0.278*	
		(0.041)			(0.151)			(0.156)	
Assigned to busing, low SES		-0.042			0.024			0.185	
		(0.043)			(0.142)			(0.158)	
Assigned to busing, low LSN			-0.049			-0.230			0.296*
			(0.042)			(0.159)			(0.159)
Assigned to busing, medium or high LSN			-0.075*			0.022			0.188
			(0.041)			(0.144)			(0.163)
Difference	-0.059	-0.044	0.026	-0.201	-0.208	-0.252	0.152	0.930	0.108
R^2	0.106	0.106	0.105	0.124	0.125	0.125	0.170	0.170	0.170
Observations	1,227	1,227	1,227	1,060	1,060	1,060	1,060	1,060	1,060
Controls:									
Determinants of assignment	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Individual characteristics	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mother characteristics	YES	YES	YES	YES	YES	YES	YES	YES	YES
Father characteristics	YES	YES	YES	YES	YES	YES	YES	YES	YES

Source: Danish Well-being Survey linked with Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). Outcomes: a dummy for having taken the well-being survey (1, 4) and standardized factors for school satisfaction (5, 8) and distress (9, 12). OLS of the outcome on a dummy for being bused interacted with: a) the sex of the child, b) socio-economic status measured as low if both parents are not employed and high otherwise, c) a dummy for whether the child has low or medium/high language support need. We control for school assignment determinants: dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects, individual and family characteristics. Individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016. The presented parameters are not significantly different from each other in any of the regressions.

Table A7. Effects of Assignment to Busing on Wellbeing in Grades 4–9. Two Survey Factors: School Satisfaction and Distress.

	Dependent variable:					
	Survey taker	Survey taker	School satisfaction	School satisfaction	Distress	Distress
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Explanatory variable:						
Assigned to busing	-0.029	-0.027	-0.039	-0.008	-0.046	-0.038
	(0.025)	(0.025)	(0.120)	(0.114)	(0.104)	(0.094)
R^2	0.104	0.126	0.087	0.151	0.081	0.129
Observations	2,064	2,064	1,703	1,703	1,703	1,703
Panel B						
Explanatory variables:						
Assigned to busing, grade 4	-0.052	-0.050	-0.166	-0.119	-0.093	-0.092
	(0.039)	(0.039)	(0.151)	(0.149)	(0.152)	(0.146)
Assigned to busing, grade 5	0.010	0.014	0.006	0.049	0.033	0.042
	(0.040)	(0.039)	(0.154)	(0.147)	(0.132)	(0.125)
Assigned to busing, grade 6	0.014	0.018	0.037	0.067	-0.010	-0.016
	(0.038)	(0.038)	(0.160)	(0.153)	(0.142)	(0.130)
Assigned to busing, grade 7	-0.075*	-0.076*	0.099	0.118	-0.006	-0.001
	(0.042)	(0.043)	(0.152)	(0.145)	(0.131)	(0.123)
Assigned to busing, grade 8	0.001	0.002	-0.113	-0.105	-0.157	-0.126
	(0.051)	(0.050)	(0.174)	(0.170)	(0.153)	(0.147)
Assigned to busing, grade 9	-0.112	-0.110	-0.292	-0.246	-0.158	-0.125
	(0.073)	(0.074)	(0.212)	(0.212)	(0.192)	(0.190)
R^2	0.107	0.129	0.090	0.154	0.083	0.130
Observations	2,064	2,064	1,703	1,703	1,703	1,703
Controls:						
Determinants of assignment	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES	NO	YES

Source: Danish Wellbeing Survey linked with Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). Outcomes: a dummy for having taken the well-being survey (1, 2) and standardized factors for school satisfaction (3, 4) and distress (5, 6). OLS of the outcome on a dummy for being bused (Panel A), a dummy for being bused interacted with the grade (Panel B) and the municipality assignment determinants. All specifications include the following individual controls: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

Table A8. Robustness Checks. Bounds on Estimated Effect on National Test Score. By Grade and Subject.

	Dependent variable:							
	Test taker	Test taker	Test score	Test score	Test score	Test score	Test score	Test score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Explanatory variables:								
Assigned to busing, reading test,	-0.057**	-0.055**	-0.089	-0.092	-0.182*	-0.183*	-0.003	-0.009
grade 2	(0.023)	(0.023)	(0.099)	(0.096)	(0.102)	(0.099)	(0.095)	(0.093)
Assigned to busing, math test,	-0.015	-0.013	-0.170*	-0.184*	-0.171*	-0.183*	-0.130	-0.146
grade 3	(0.021)	(0.021)	(0.101)	(0.097)	(0.103)	(0.099)	(0.097)	(0.093)
Assigned to busing, reading test,	-0.033*	-0.031	-0.019	-0.037	-0.064	-0.079	0.030	0.010
grade 4	(0.020)	(0.021)	(0.106)	(0.102)	(0.108)	(0.104)	(0.101)	(0.098)
Assigned to busing, reading test,	-0.001	-0.002	-0.114	-0.132	-0.097	-0.119	-0.100	-0.121
grade 6	(0.021)	(0.021)	(0.108)	(0.105)	(0.110)	(0.107)	(0.105)	(0.103)
Assigned to busing, math test,	-0.011	-0.011	-0.152	-0.172	-0.147	-0.169	-0.127	-0.147
grade 6	(0.020)	(0.020)	(0.113)	(0.109)	(0.113)	(0.109)	(0.109)	(0.106)
Assigned to busing, English test,	0.032	0.031	-0.077	-0.082	0.028	0.012	-0.070	-0.083
grade 7	(0.045)	(0.045)	(0.133)	(0.125)	(0.127)	(0.124)	(0.122)	(0.117)
Assigned to busing, reading test,	-0.006	-0.008	-0.234*	-0.250*	-0.212	-0.217*	-0.198	-0.199
grade 8	(0.027)	(0.028)	(0.133)	(0.131)	(0.131)	(0.130)	(0.131)	(0.129)
Assigned to busing, natural	0.016	0.012	-0.008	0.027	0.030	0.045	-0.023	0.005
science tests, grade 8	(0.035)	(0.036)	(0.160)	(0.154)	(0.154)	(0.148)	(0.152)	(0.148)
R^2	0.108	0.127	0.093	0.170	0.088	0.160	0.101	0.155
Observations	4,010	4,010	3,705	3,705	4,010	4,010	4,010	4,010
Non-takers	-	-	-	-	p5	p5	p95	p95
Controls:								
Determinants of assignment	YES	YES	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Subject fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES	NO	YES	NO	YES

Source: Danish National Tests linked with Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). Outcomes: Dummies for taking the test conditional on attending the relevant grade in a public school in a year where the test took place. Standardized test scores conditional on having taken the test. National tests in math (grades 3 and 6), reading (grades 2, 4, 6, 8), English (grade 7) and natural science (grade 8). The natural science tests cover geography, biology, physics and chemistry. OLS of the outcome on a dummy for being bused interacted with grade and test subject. We control for the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016. Columns 5-8 show bounds according to Horowitz and Manski (1998). In columns 5 and 6 non test takers are assigned the 5th percentile of the test score distribution in the sample, in columns 7 and 8 non test takers are assigned the 95th percentile.

Table A9. Robustness Check: Bounds on Estimated Effect on Wellbeing in Grades 0–3.

	Dependent variable:						
	Survey taker	School satisfaction	School satisfaction	School satisfaction	Distress	Distress	Distress
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Explanatory variable:							
Assigned to busing	-0.063*	-0.092	-0.187	-0.012	0.237*	0.096	0.299**
	(0.034)	(0.122)	(0.127)	(0.105)	(0.132)	(0.134)	(0.117)
R^2	0.107	0.126	0.107	0.114	0.170	0.130	0.148
Observations	1,227	1,060	1,227	1,227	1,060	1,227	1,227
Non takers	-	-	p5	p95	-	p5	p95
Controls:							
Determinants of assignment	YES	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES	YES	YES	YES
Individual characteristics	YES	YES	YES	YES	YES	YES	YES
Mother characteristics	YES	YES	YES	YES	YES	YES	YES
Father characteristics	YES	YES	YES	YES	YES	YES	YES

Source: Danish Wellbeing Survey linked with Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table A4). Outcomes: a dummy for having taken the well-being survey, and standardized factors for school satisfaction and distress. OLS of the outcome on a dummy for being bused, language test year–by–school district of residence fixed effects, and the municipality assignment determinants. These are a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects, individual and family characteristics. Individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force),, dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016. Columns 3-4 and 6-7 show bounds according to Horowitz and Manski (1998). In columns 3 and 6 non survey takers are assigned the 5th percentile of the test score distribution in the sample, in columns 4 and 7 non survey takers are assigned the 95th percentile.

Table A10. Effect of Assignment to Busing on National Test Score. By Grade and Subject. Balanced Sample: Test Cohorts 2007-2010.

	Dependent variable:			
	Test taker	Test taker	Test score	Test score
	(1)	(2)	(3)	(4)
Explanatory variables:				
Assigned to busing, reading test, grade 2	-0.041	-0.040	-0.001	-0.112
	(0.032)	(0.032)	(0.131)	(0.132)
Assigned to busing, math test, grade 3	-0.011	-0.010	-0.240*	-0.348**
	(0.028)	(0.028)	(0.144)	(0.139)
Assigned to busing, reading test, grade 4	-0.038	-0.035	-0.075	-0.175
	(0.025)	(0.026)	(0.144)	(0.145)
Assigned to busing, reading test, grade 6	-0.006	-0.006	-0.115	-0.202
	(0.026)	(0.026)	(0.146)	(0.145)
Assigned to busing, math test, grade 6	-0.010	-0.010	-0.114	-0.206
	(0.023)	(0.024)	(0.147)	(0.142)
Assigned to busing, English test, grade 7	-0.032	-0.035	-0.075	-0.149
	(0.036)	(0.036)	(0.147)	(0.138)
Assigned to busing, reading test, grade 8	-0.006	-0.008	-0.232	-0.319**
	(0.029)	(0.030)	(0.146)	(0.144)
Assigned to busing, natrual science tests, grade 8	0.018	0.006	-0.005	-0.075
	(0.036)	(0.038)	(0.172)	(0.167)
R^2	0.044	0.080	0.088	0.192
Observations	2,609	2,609	2,441	2,441
Controls:				
Determinants of assignment	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES
Subject fixed effects	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2010, who are eligible for forced busing (described in Notes to Table 1). Pooled dataset. Outcomes: Dummies for taking the test conditional on attending the relevant grade in a public school in a year where the test took place. Standardized test scores conditional on having taken the test. National tests in math (grades 3 and 6), reading (grades 2, 4, 6, 8), English (grade 7) and natural science (grade 8). The natural science tests cover geography, biology, physics and chemistry. The science tests include geography, biology, physics and chemistry. OLS of the outcome on a dummy for being bused interacted with grade and test subject. We also control for the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

Table A11. Effect of Assignment to Busing on National Test Score. By Grade and Subject. Compliers.

	Dependent variable:			
	Test taker	Test taker	Test score	Test score
	(1)	(2)	(3)	(4)
Explanatory variables:				
Assigned to busing, reading test, grade 2	-0.054**	-0.052**	-0.193*	-0.117
	(0.025)	(0.025)	(0.110)	(0.106)
Assigned to busing, math test, grade 3	-0.019	-0.016	-0.286**	-0.224**
	(0.024)	(0.024)	(0.111)	(0.108)
Assigned to busing, reading test, grade 4	-0.037	-0.032	-0.180	-0.105
	(0.025)	(0.026)	(0.122)	(0.114)
Assigned to busing, reading test, grade 6	0.022	0.028	-0.170	-0.103
	(0.023)	(0.023)	(0.127)	(0.125)
Assigned to busing, math test, grade 6	-0.001	0.005	-0.107	-0.040
	(0.023)	(0.024)	(0.136)	(0.130)
Assigned to busing, English test, grade 7	-0.030	-0.025	0.069	0.131
	(0.060)	(0.061)	(0.164)	(0.151)
Assigned to busing, reading test, grade 8	0.061**	0.066**	-0.092	-0.054
	(0.030)	(0.031)	(0.177)	(0.176)
Assigned to busing, natural science tests, grade 8	0.086	0.093*	-0.051	0.075
	(0.053)	(0.051)	(0.198)	(0.192)
R^2	0.115	0.133	0.131	0.223
Observations	2,590	2,590	2,431	2,431
Controls:				
Determinants of assignment	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES
Grade fixed effects	YES	YES	YES	YES
Subject fixed effects	YES	YES	YES	YES
Individual characteristics	NO	YES	NO	YES
Mother characteristics	NO	YES	NO	YES
Father characteristics	NO	YES	NO	YES

Source: Micro data from Danish National Tests linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1. Sample: Language screened school starters in Aarhus Municipality 2007-2016, who are eligible for forced busing (described in Notes to Table 1) and who attend the school they are assigned to at the time of the National test. Pooled dataset. Outcomes: Dummies for taking the test conditional on attending the relevant grade in a public school in a year where the test took place. Standardized test scores conditional on having taken the test. National tests in math (grades 3 and 6), reading (grades 2, 4, 6, 8), English (grade 7) and natural science (grade 8). The natural science tests cover geography, biology, physics and chemistry. OLS of the outcome on a dummy for being bused interacted with grade and test subject. We control for the school assignment determinants: a dummy for having at least one sibling attending the district school in the fall of the year of the test, age difference with the youngest sibling attending the district school, distance in km from the main entrance of the district school. Other controls include: language test-year–by–school district of residence fixed effects, grade fixed effects, subject fixed effects. Specification 2 controls for additional individual and family characteristics. The individual characteristics of the child include dummies for having a bused sibling, the assessed level of language support need, continuous age of the pupil on the day of the test, gender, immigration status (immigrant or descendant), area of origin (Africa, Europe, Americas and Oceania, East Asia or Middle East), dummy for daycare attendance, dummies for the number of siblings (capped at 7), living arrangement (child lives in a two-parent household), dummies for parents missing from the registers. Family characteristics include, for both mother and father: immigration status, education (high school dropout, high school graduate, tertiary degree or not reported), employment status (employed, unemployed or out of the labor force), dummies for quartiles of real disposable income, dummies for age group in the year of the test (below 25, 25-29, 30-34, 35-39, above 39). Fixed effects for 10 school districts in each year from 2007-2016.

Appendix B. Allocation of school resources

The first section briefly describes the main principles and components of the current resource allocation to the public schools in Aarhus Municipality.[1] The model has not seen major changes since January 1, 2009.[2] The second section presents the formulas used for calculating school budgets per pupil across schools and school budget premiums per Danish-as-additional language (DAL) pupil.

B1. The resource allocation to public schools in Aarhus Municipality

The school budget model includes the following components:

- Regular education
- Grade 0 classes
- Guarantee resources
- Teacher seniority
- Magnet schools
- Full-day schools
- Management and administration
- Physical school facilities
- DAL support[3]
- Pupils with special needs
- Social pedagogical support
- Other expenditures

Although Aarhus Municipality uses the above-mentioned budget items in their allocation of resources across schools, the school principals have the autonomy to spend the budget as they like. For example, money received as DAL support can be spent on regular education.

The number of pupils is used to distribute some of the budget items. These numbers are calculated each year on September 5. The school budgets follow the calendar year, meaning that the budget allocation for 2014 is based on the number of pupils on September 5, 2013. However, the budget for regular education and Grade 0 is adjusted accordingly when new numbers are available on September 5 of the budget year.

Regular education and grade 0 classes

[1] The school budget model will be reformed in August 2020.

[2] Disclaimer: The allocation of resources appears to look like formula funding, because it follows some clear rules, but the allocation may very well be subject to several cost reimbursements made by Children & Youth during the budget period. An example could be that some schools are more exposed to vandalism and therefore receive additional resources for building maintenance. Furthermore, some part of the allocation may still follow the historical allocation, e.g. the share of the total annual budget for magnet schools decided by the Municipal Council for each magnet schools.

[3] DAL support is required by the Danish Public School Law. Cf. Article 4 in "Bekendtgørelse af Lov om Folkeskolens undervisning i dansk som andetsprog nr. 1053 af 29/06/2016".

Overall, the resource allocation model makes use of two principles: allocation per class and allocation per pupil. While the schools primarily receive resources for grade 0 based on the number of classes, the schools primarily receive resources for grades 1–10 based on the number of pupils. Furthermore, the specific rate per pupil in a regular class depends on the grade, because the legal requirement regarding the minimum number of lessons differs by grade. Before 2014, there were two rates per pupil: one for grades 1–7 and another for grades 8–10. More categories have since been introduced. In 2017, the rates were as shown in Table B1.

Table B1. Rates per pupil in a regular class

Grade	Amount per pupil (USD)
0	4,099
1–3	5,016
4–6	5,616
7–9	6,149
10	5,156

Source: Aarhus Municipality (2017).
Note: For grade 0, the number is approximate because resources are distributed based on the number of classes. Exchange rate used is 0.1485 USD/DKK, www.statistikbanken.dk/DNVALA for year 2016.

The rates vary a bit between the schools to account for the varying seniority of teachers and thereby expenditures on salaries.

Resources to grade 0 are allocated based on the number of classes multiplied by the average salary for a grade 0 teacher (and in cases involving more than 22 pupils in a class, a small compensation per pupil in excess of 22).

Guarantee resources

Allocations based on a rate per pupil do not take into account the fact that having fewer pupils in a class results in a higher expenditure per child. Therefore, based on some rules, a school receives extra resources if the combination of the number of classes and pupils in a grade makes it difficult for a school to fulfill the minimum number of lessons required (or "guaranteed") by the Ministry of Education.

Magnet schools

While the total annual budget premium for magnet schools is decided each year by the City Council, the allocation across magnet schools is based on a historical allocation key, which has been unchanged since around 2010.

Full-day schools

Pupils in full-day schools have classes from 8am until 4 pm every school day. The financing of the regular education at the full-day schools differs from the other schools by being determined by the number of classes in grades 0–10.

Management and administration

The schools receive resources equivalent to the salaries of one school leader, one pedagogical leader, one administrative leader, and 0.4 administrative employees. They also receive a rate per pupil exceeding a total of 300 pupils.

Physical school facilities

Since 2014, the rules for allocation to physical school facilities have been as follows:

- An amount defined by school/geographical location
- A rate per square meter needing cleaning
- A rate per pupil
- A rate per pupil attending the after-school care program
- Compensation if the school has an indoor swimming pool, the amount depending on the swimming pool being small or large.
- Compensation for electricity costs (exact rules unknown).

DAL support[4]

Schools receive additional resources to accommodate the needs of DAL pupils. These funds are distributed based on the deservingness of the schools. For example, resources to the different activities for DAL pupils are based on the number of DAL pupils at the school.

Resource allocation for DAL activities follows a point system, where all DAL pupils are endowed with 0.75 points. Language-tested DAL pupils receive additional points in grades 0–3, depending on their language support need: Basic (B), Substantial support (S1, S2, S3), Age-appropriate language proficiency (F). The number of additional points are indicated in Table B2.

Table B2. Point system for budget allocation to language tested DAL pupils

DAL support need	Grade 0	Grade 1	Grade 2	Grade 3
No (F)	0.75+0.25	0.75	0.75	0.75
Low (S3)	0.75+1.25	0.75+0.25	0.75	0.75
Medium (S2	0.75+2.25	0.75+1.25	0.75+0.25	0.75
High (S1)	0.75+3.25	0.75+2.25	0.75+1.25	0.75+0.25

Source: E-mail from Lone Nielsen, Aarhus Municipality, Children & Youth, dated Sept. 6, 2019.

Each point corresponds to a given rate, which was USD 789 in 2014, USD 787 in 2015 and USD 779 in 2016 (E-mail from Lone Nielsen, Aarhus Municipality, Children & Youth, dated September 6, 2019).

[4] Resources for basic DAL classes are based on the number of basic DAL classes at the school. We disregard these costs in this description because pupils in basic DAL classes (category-B pupils) are not part of our impact evaluation.

In addition, schools with more than 20% DAL pupils receive resources to facilitate cooperation between the school and parents. The total annual budget for such activities is allocated between schools on the basis of the school's overall share of DAL pupils.

Special needs pupils

Each school has the financial responsibility for pupils attending special classes and not pupils referred to special schools or more specific treatment schools. Resources to special classes in the public schools are distributed as follows: 50% of the resources are allocated based on the number of pupils attending the school, and 50% are allocated based on characteristics of the school district: (i) income, (ii) education, (iii) employment, (iv) income replacing benefits, and (v) share of DAL pupils. The first four characteristics are computed for the entire adult population in the school district, whereas the fifth is computed for the pupils attending the district school.

Social pedagogical support

The schools receive resources for additional educational and pedagogical support. The resources are allocated as follows: 60% of the resources are allocated based on the number of pupils attending the school, and 40% are allocated based on three variables of the adult population in each district: Income, education, and employment.

Other expenditures

The schools receive some minor compensation for other expenditures, including teacher's aides in grades 0–3, lunch schemes, and IT-related expenses.

B2. School budget per pupil across public schools in Aarhus Municipality

We calculate the budgets per pupil in a regular class from the budget items that primarily vary by the number of pupils (variable costs); that is, ignoring the budget items that are primarily fixed (e.g. management and administration as well as physical school facilities).

School budget per pupil in regular class in grades 1–3 =

$$\begin{aligned}
&\text{Rate per pupil}_{\text{Grades 1–3}} \\
&+\frac{\text{Additional budget to guarantee minimum required budget for regular classes}}{\text{Number of pupils in regular classes}_{\text{Grades 0–10}}} \\
&+\frac{\text{Budget for social pedagogical support}}{\text{Number of pupils in regular classes}_{\text{Grades 1–10}}} \\
&+\frac{\text{Budget for two teacher arrangement}_{\text{Grades 0–3}}}{\text{Number of pupils in regular classes}_{\text{Grades 0–3}}} \\
&+\frac{\text{Budget for lunch scheme}}{\text{Number of pupils}_{\text{Grades 0–10}}}
\end{aligned}$$

School budget per pupil in regular class in grades 4–6 =

$$\text{Rate per pupil}_{\text{Grades 4–6}}$$
$$+\frac{\text{Additional budget to guarantee minimum required budget for regular classes}}{\text{number of pupils in regular classes}_{\text{Grades 0–10}}}$$
$$+\frac{\text{Budget for social pedagogical support}}{\text{Number of pupils in regular classes}_{\text{Grades 1–10}}}$$
$$+\frac{\text{Budget for lunch scheme}}{\text{Number of pupils}_{\text{Grades 0–10}}}$$

School budget per pupil in regular class in grades 7–9 =

$$\text{Rate per pupil}_{\text{Grades 7–9}}$$
$$+\frac{\text{Additional budget to guarantee minimum required budget for regular classes}}{\text{number of pupils in regular classes}_{\text{Grades 0–10}}}$$
$$+\frac{\text{Budget for social pedagogical support}}{\text{Number of pupils in regular classes}_{\text{Grades 1–10}}}$$
$$+\frac{\text{Budget for lunch scheme}}{\text{Number of pupils}_{\text{Grades 0–10}}}$$

Additional school budget for DAL pupils in schools with max. 20% DAL pupils =

$$\text{Budget for DAL support to DAL pupils in regular classes}$$
$$+\ \text{Budget for interpreters}$$

Additional school budget for DAL pupils in schools with at least 20% DAL pupils =

$$\text{Budget for DAL support to DAL pupils in regular classes}$$
$$+\text{Budget for interpreters}$$
$$+\text{Task-specific resources}$$

[Insert Figures B1.a–B1.c around here]

Using the 2014 allocated school budgets to public schools in Aarhus, Figure B1.a illustrates the budget per DAL pupil in regular classes in grades 1–3 for each category of language support needed for non-magnet schools with at least 20% DAL pupils. The budget per DAL pupil decreases strongly with the DAL support need until grade 3. In grade 3, only DAL pupils with the strongest level of support need receive a higher premium than DAL pupils in regular classes in general, who receive the 0.75-point base rate for "DAL support per DAL pupil in regular classes," which given the rate per point in 2014 amounts to USD 789, corresponding to a premium of 11%. As shown in

the figure, non-magnet schools with at least 20% DAL pupils receive an average premium of USD 356, whereas magnet schools (that all have more than 20% DAL pupils) receive an average premium of USD 435.

As illustrated in Figures B1.b and B1.c, the budget per DAL pupil in regular classes in grades 4–6 and grades 7–9, respectively, is lower than in grades 1–3 for non-magnet schools with at least 20% DAL pupils and identical for all categories of DAL pupils in regular classes. All DAL pupils in regular classes receive the 0.75-point base rate for "DAL support per DAL pupil in regular classes," which, given the rate/point in 2014 of USD 789 corresponds to a premium of 10% in grades 4–6 and 9% in grades 7–9.

All schools with DAL pupils receive an additional budget for interpreters; the amount per DAL pupil is modest, on average USD 24 in non-magnet schools and around USD 40 in magnet schools.

References

Aarhus Kommune, Børn og Unge (Aarhus Municipality, Children & Youth department). 2013. "Budgetter til skolerne – hvor kommer tallene fra?" Slides dated Jan. 28, 2013.

Aarhus Kommune, Børn og Unge, Økonomi og Administration, Budget og Regnskab (Aarhus Municipality, Children & Youth department, Finance and Administration, Budget and Accounts). 2015."Budgettildelingsmodel for skolerne". Dated Nov. 18, 2015 Sag: 19/034052-1.

Aarhus Kommune, Børn og Unge (Aarhus Municipality, Children & Youth department) and Århus Lærerforening (Aarhus Teacher Association). 2017. "Bliv klogere på din skoles Økonomi: Tallene i pjecen refererer til budget 2017". Dated Nov. 6, 2017.

Aarhus Kommune, Børn og Unge, Økonomi og Administration, Budget og Regnskab (Aarhus Municipality, Children & Youth department, Finance and Administration, Budget and Accounts). "Bilag 1". Dated Jan. 21, 2019. Sag: 19/001850-2.

Aarhus Kommune (Aarhus Municipality). 2019. "Analyse af folkeskolernes økonomi. Sammenfatning samt delanalyser". Dated June 21, 2019.

Figure B1.a Average Budget Items for Danish-as-Additional-Language Pupils in Regular Classes (USD). 2014. Grades 1-3. Non-Magnet School with at least 20% DAL Pupils.

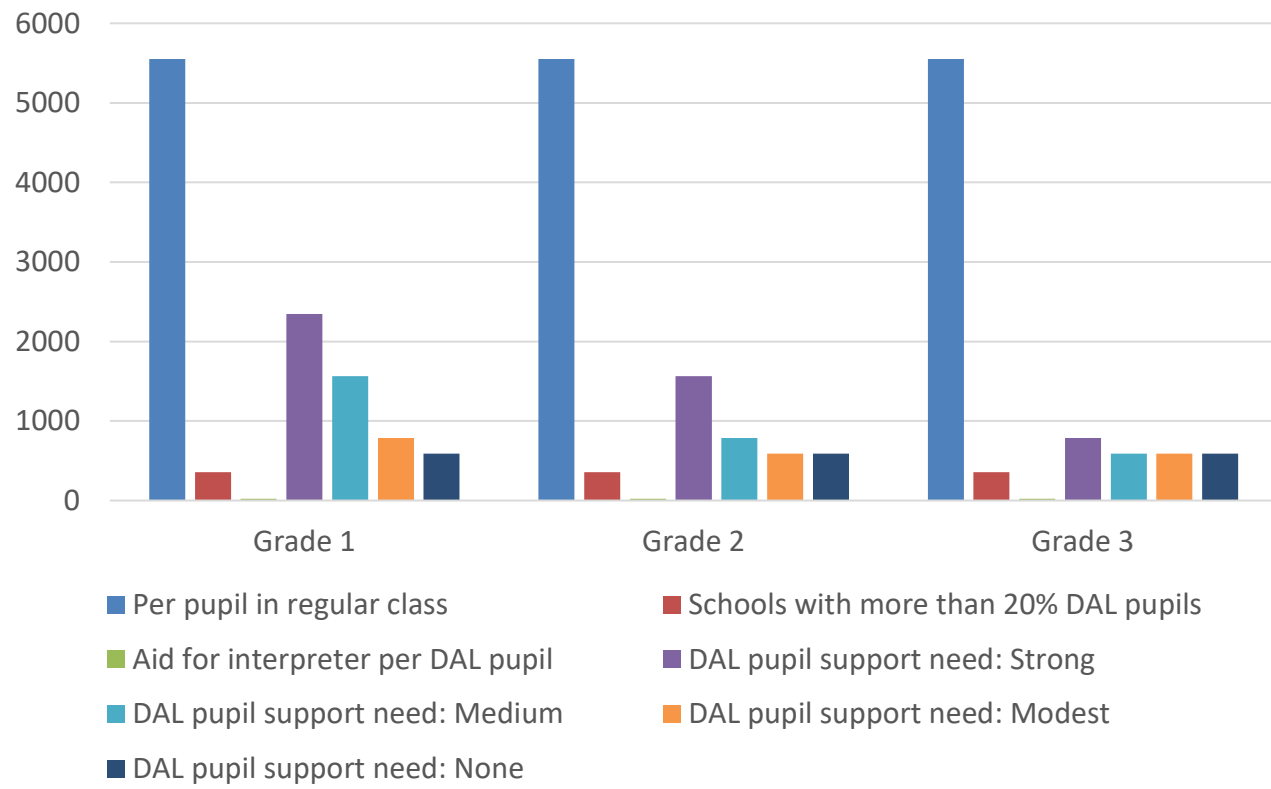


Figure B1.b Average Budget Items for Danish-as-Additional-Language Pupils in Regular Classes (USD). 2014. Grades 4-6. Non-Magnet School with at least 20% DAL Pupils.

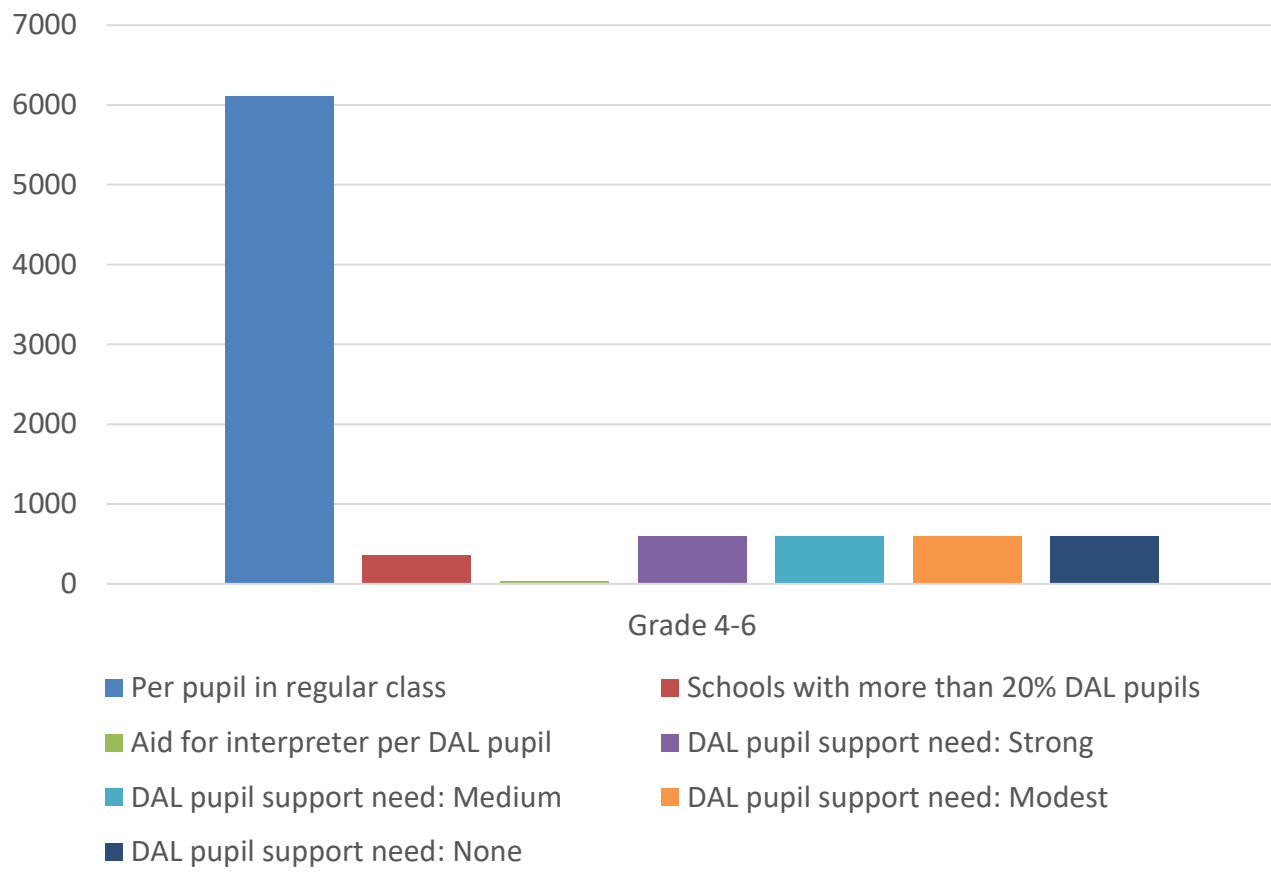
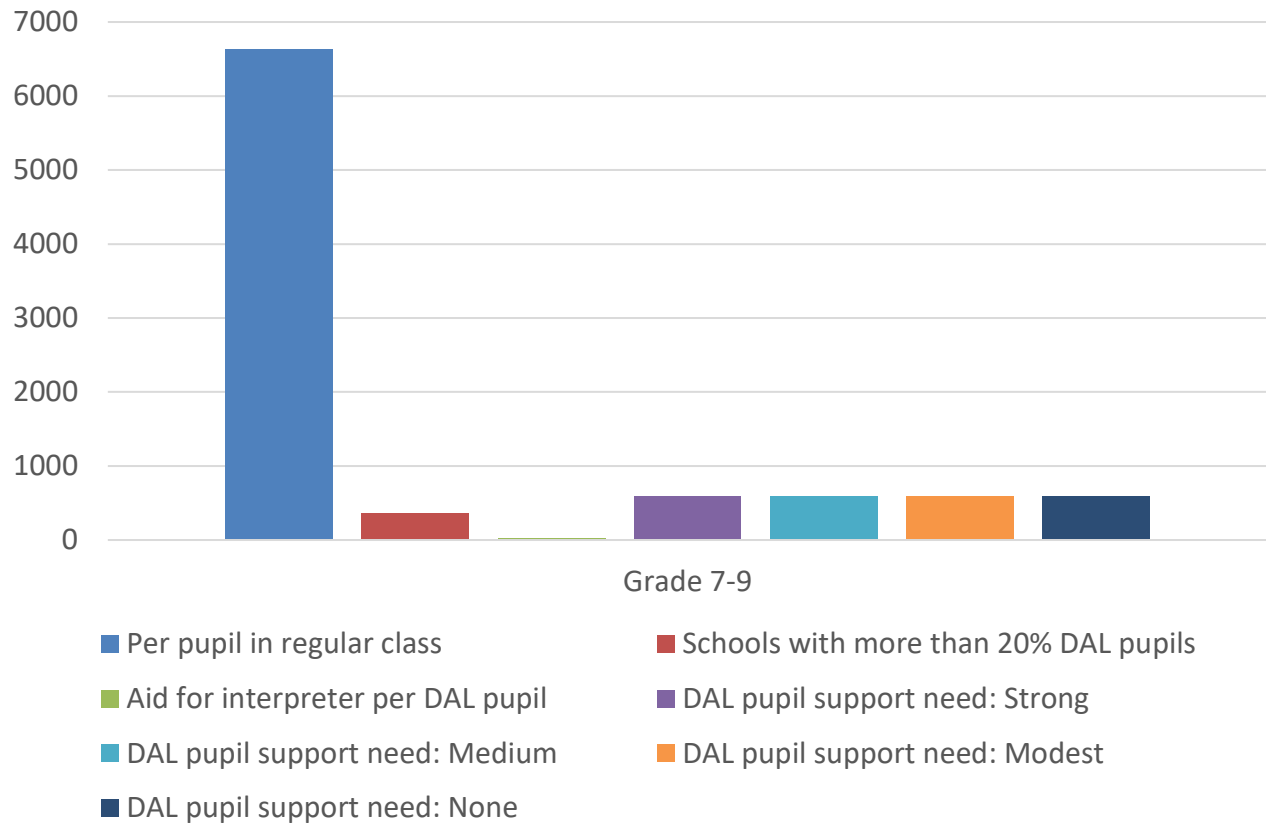


Figure B1.c Average Budget Items for Danish-as-Additional-Language Pupils in Regular Classes (USD). 2014. Grades 4-6. Non-Magnet School with at least 20% DAL Pupils.



Source: Authors' own calculations from allocated school budgets to public schools in Aarhus Municipality in 2014.
Note: The average budget per pupil in regular classes (column 1) is calculated as the sum of the grade-specific rate per pupil in a regular class, the additional budget to guarantee minimum required budget for regular classes per pupil in regular classes in grade 0-10, the budget for social pedagogical support per pupil in regular classes in grades 1-10 and the budget for lunch scheme per pupil in grades 0-10. The additional budget to schools with at least 20% Danish-as-additional-language (DAL) pupils (column 2) is calculated as the budget for "task-specific resources" divided by the number of DAL pupils. The budget for "aid for interpreter" (column 3) is calculated as the budget for "aid from interpreters" divided by the number of DAL pupils. The addtional budget for DAL support to each DAL pupil in regular classes for DAL pupils is shown in the last 4 columns for each category of DAL-pupils, depending on their DAL support need (according to the language screening test before school start). Exchange rate DKK/USD 0.1485 (base year: 2016).

Economics Working Papers

2019-01: Ran Sun Lyng and Jie Zhou: Household Portfolio Choice Before and After a House Purchase

2019-02: Mongoljin Batsaikhan, Mette Gørtz, John Kennes, Ran Sun Lyng, Daniel Monte and Norovsambuu Tumennasan: Daycare Choice and Ethnic Diversity: Evidence from a Randomized Survey

2019-03: Dan Anderberg, Jesper Bagger, V. Bhaskar and Tanya Wilson: Marriage Market Equilibrium, Qualifications, and Ability

2019-04: Carsten Andersen: Intergenerational Health Mobility: Evidence from Danish Registers

2019-05: Michael Koch, Ilya Manuylov and Marcel Smolka: Robots and Firms

2019-06: Martin Paldam: The Transition of Corruption - Institutions and dynamics

2019-07: Martin Paldam: Does system instability harm development? A comparative empirical study of the long run

2019-08: Marianne Simonsen, Lars Skipper, Niels Skipper and Peter Rønø Thingholm: Discontinuity in Care: Practice Closures among Primary Care Providers and Patient Health

2019-09: Leonidas Enrique de la Rosa and Nikolaj Kirkeby Niebuhr: Loss aversion and the zero-earnings discontinuity

2019-10: Emma von Essen, Marieke Huysentruyt and Topi Miettinen: Exploration in Teams and the Encouragement Effect: Theory and Evidence

2019-11: Erik Strøjer Madsen: From Local to Global Competitors on the Beer Market

2020-01: Nikolaj Kirkeby Niebuhr: Managerial Overconfidence and Self-Reported Success

2020-02: Tine L. Mundbjerg Eriksen, Amanda Gaulke, Niels Skipper and Jannet Svensson: The Impact of Childhood Health Shocks on Parental Labor Supply

2020-03: Anna Piil Damm, Helena Skyt Nielsen, Elena Mattana and Benedicte Rouland: Effects of Busing on Test Scores and the Wellbeing of Bilingual Pupils: Resources Matter