Academic Achievement and Wellbeing of Dual Language Learners: Evidence from a Busing Program*

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Abstract: We exploit exogenous variation from a school desegregation policy to investigate the determinants of academic achievement and wellbeing of dual language learners. The policy buses some school starters with low host-country language proficiency from schools with many dual language learners and high per-pupil spending to schools with fewer dual language learners but lower per-pupil spending. Assignment to busing is exogenous conditional on three observed individual characteristics, hence we compare bused and non-bused pupils conditional on these characteristics. We find that assignment to forced busing has a negative effect on the academic performance and wellbeing of dual language learners, which is at odds with findings for school desegregation in the US.

Keywords: School Segregation; Integration; Immigration; Education; School Environment; Second-language learner.

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

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1. Introduction

The high recent inflows of immigrants in high-income countries have created an urgent need for policies improving social and economic outcomes of immigrants and their children (Algan, Bisin and Manning 2012). In particular, the academic achievement of ethnic minority children lags behind that of natives (Dustmann, Machin and Schönberg 2010, Bratsberg, Raaum and Røed 2012, OECD 2019). Among the reasons advanced for such a gap are lack of host-country language proficiency, school quality, and school segregation stemming from residential segregation that in turn leads to ethnic minority children attending schools with a high number of other children with poor host-country language proficiency (Dustmann et al. 2010). In this paper, we ask which school environment is more conducive to better outcomes for children whose first language is not the host-country language, henceforth referred to as dual language learners.[1]

We exploit quasi-random assignment to schools of dual language learners with limited host-country language proficiency, following a school desegregation policy in Aarhus, Denmark.[2] Every year in Aarhus, around 550 school starters who do not speak Danish as the first language at home (around 18% of all school starters) are exposed to mandatory language screening. Pupils found to have limited Danish language proficiency (around two thirds) are assigned to either their local district school or another school. The local authorities provide pupils assigned to another school with free busing between home and the school. Assignment to schools is conditional on three characteristics which we observe in the administrative registers: special needs, siblings in the local district school, and distance to the district school. We denote pupils assigned to busing as leaving the "sending school district" to attend school in the "receiving school district". The school environments of sending and receiving school districts differ in terms of peers and resources. First, the pupil body in receiving schools consists of more native Danish speakers and pupils from higher socio-economic status families. The pupil body in sending schools instead consists of pupils more similar in both ethnic background and socio-economic status to the pupils participating in the policy. Second, school resources in Denmark are allocated in order to secure a minimum standard of education across

[1] We refer to Head Start for this definition: *Dual language learner* means a child who is acquiring two or more languages at the same time, or a child who is learning a second language while continuing to develop their first language. The term "dual language learner" may encompass or overlap substantially with other terms frequently used, such as bilingual, English language learner (ELL), Limited English Proficient (LEP), English learner, and children who speak a Language Other Than English (LOTE), see Head Start Policy & Regulation (link, accessed 11/01/21).

[2] Aarhus is the second largest city in Denmark. Aarhus Municipality has a population of around 345,000, see statbank.dk/BY2.

school districts. Therefore, sending school districts collect more resources per pupil than receiving school districts due to their disadvantaged pupil body.

We compare pupils who are assigned to busing to those not assigned to busing while controlling for the determinants of school assignment. We find negative effects of forced busing on both academic achievement and wellbeing. First, pupils who are assigned to busing have poorer test scores in both reading (one sixth of a standard deviation) and math (one quarter of a standard deviation). The gaps open up early and persist across grades. Second, pupils assigned to busing report higher levels of distress in early grades (0.23 of a standard deviation). In addition, we find that forced busing leads to lower attendance in after-school programs in the assigned school. This indicates that in early grades bused pupils interact less with their class- and schoolmates and these interactions are more likely to be conflictual, as suggested by their higher distress.

Busing implies attending a school with lower resources and more classmates who are native Danish speakers. Therefore, our results suggest that higher school resources promote academic outcomes of dual language learners. Moreover, ample resources combined with high shares of dual language learners in the sending schools may imply better teaching for dual language learners due to targeted school inputs and gains from specialization (e.g. higher overall budget for Danish as Additional Language (DAL) support enabling the school to hire a trained DAL teacher). However, busing also implies that pupils are removed from their local environment and placed in peer groups with different characteristics in terms of both cognitive (e.g. language) and non-cognitive skills (e.g. openness to others). Although dual language learners with limited Danish proficiency may benefit academically from having more classmates who are native Danish speakers, our results also suggest that dual language learners at sending schools experience faster social integration by being in a classroom with more dual language learners with whom they are more likely to form friendships due to shared common knowledge and everyday lives. Slower social integration of dual language learners at receiving schools constitutes another channel through which busing creates persistent gaps in academic achievement.

There are possibilities for non-compliance with the school assignment: Children may delay school start or enroll in private school, and they may gain free school choice in later years. Therefore, our causal estimates should be interpreted as intention-to-treat estimates. We find higher non-compliance with the assignment for pupils assigned to busing in the years after the test. We interpret this as additional evidence that bused pupils are less happy with their school of assignment. Additional findings suggest that the school disruption potentially

introduced by switching school mid-education does not drive our results. Finally, the nature of the policy implies that there is one more channel that could explain our estimates: 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). We find no evidence that this channel plays an important role for our findings.

Our study bridges two strands of literature concerned with school desegregation and schooling environment of dual language learners. School desegregation policies, such as busing, vouchers, or rezoning, are thought to improve academic achievement through influence on school quality and the ethnic and socio-economic mix of pupils. A large literature focuses on the effect of the US racial school desegregation policies and the eventual disbandment of some of these policies.[3] This literature indicates that school desegregation policies improve performance of minority pupils in high school and college as well as adult long-term outcomes such as earnings, health and crime (Angrist and Lang 2004, Guryan 2004, Johnson 2011, 2019, Billings, Deming and Rockoff 2014; Tuttle 2019). Desegregation policies have no or little effects on whites across these outcomes; instead exposure to more minority pupils makes white less (more) likely to register as a Republican (Democrat) (Billings, Chyn and Haggag (forthcoming), Kaplan, Spenkuch and Tuttle 2019). Among more recent contributions, Bergman (2019) studies the effects of an ongoing voluntary inter-district school assignment program in Northern California on the academic achievements of (primarily) Hispanic minority school starters. He finds that the program increases not only academic achievement but also, unintendedly, arrests and assignment to special education for minority pupils. We make several contributions to this literature. We are the first to evaluate busing outside the US, and moreover we study *forced* busing and thus avoid the common issue of positive selection into the programs (Cullen et al. 2006, Bergman 2019). Our paper stands out because while US papers produce findings that suggest that integration policies (desegregation/busing) can improve outcomes for disadvantaged groups, ours does not.

The second strand of literature we contribute to concerns the impact of the schooling environment on outcomes of dual language learners. This research has studied various contexts and results are mixed. Some studies have examined the relationship between concentration of dual language learners and school achievement. For example, Jensen and Rasmussen (2011)

[3] Outside the US context, a smaller literature has been concerned with school segregation as a consequence of universal vouchers (e.g. Rangvid 2010, Edmark et al. 2014, Böhlmark et al. 2016): effects are moderate unless the fraction of minority children exceeds roughly 35% (Rangvid, 2010). Relatedly, Bjerre-Nielsen and Gandil (2020) studies rezoning of school districts and finds that in areas with high levels of segregation, households almost completely offset the intended effects of rezoning.

document that PISA scores of dual language learners are negatively associated with immigrant concentration in Danish schools, while Cortes (2006) find no difference between achievement of pupils attending schools with above or below 25% immigrant pupils (based on the Children of Immigrants Longitudinal Study (CILS)). Other studies have looked at the impact of school quality. Gould et al. (2004) rely on quasi-random assignment of Ethiopian Jews to Israeli schools and find that a poor early schooling environment has negative effects on long-term education outcomes. Alan et al. (2020) evaluate a program improving social cohesion in schools that are part of Turkey's refugee placement program. They find the program reduces peer violence, victimization, social exclusion and ethnic segregation in the classroom.[4] We contribute to this literature by studying the effect of the overall school environment (peer composition as well as school resources) on academic outcomes of dual language learners.

Our paper exploits high quality register-based data, which allows for studies of school outcomes from the time of school entry onwards. This means we can study a young population thought to be more receptive to school interventions (Cunha and Heckman 2007) and characterize compliance with the policy over time. Furthermore, in contrast to existing studies of effects of busing we investigate the effects of forced busing. Finally, we observe detailed school spending. While US state grants given to school districts with more disadvantaged pupils 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), our Scandinavian context is vastly different. 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 outcomes of dual language learners in the context of a trade-off between exposure to native pupils and per-pupil spending.

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, Section 7 explores potential effects of the

[4] A few papers study more specifically how to best improve host country language acquisition of ethnic minority children. Some countries restrict language of instruction to the host country language in order to facilitate integration; one such an example is Denmark, who discarded mother-tongue education for pupils of non-European origin in 2001 (Salö et al. 2018). Fouka (2020) studies the ban of German as a language of instruction in US schools after WWI and finds no effect on academic outcomes but detrimental effects on social inclusion. Other countries provide bilingual education in order to ease access to education (Barrow and Markman-Pithers 2016, Behaghel et al. 2018). Chin et al. (2013) find no effects of bilingual education, while Valentino and Reardon (2015) find that beneficial effects arise in later grades.

policy on native flight, and Section 8 concludes, suggests avenues for future research and draws policy implications.

2. Background of the school desegregation policy

2.1.Institutional background

At age four, the vast majority (98% in 2007[5]) of Danish children are enrolled in some form of subsidized public daycare.[6] For the children in our study, education was compulsory from the calendar year in which they turned six[7] 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).[8] Since 2005, municipalities are allowed to revoke free school choice from pupils requiring Danish language support. 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 pupils, the average is 22 pupils, 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. Mother-tongue education is offered for pupils originating from European countries; typically, two lessons per week if at least twelve pupils sign up.

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 are 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

[5] Source: statbank.dk/PAS11 and /BRN9.
[6] A minimum of 67% of the expenses are covered by the local authorities (c.f. the Children's Act).
[7] Pre-2009 cohorts could opt out of pre-school programs, though, which became compulsory for the cohort starting school in 2009. Before 2009, average enrolment in the optional pre-school was 83% (2005 figures; UNI-C 2012).
[8] Each school district has one public school, referred to as the district school.
[9] Around 15% of the relevant cohorts in Aarhus attended private schools, see noegletal.dk. 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 (statbank.dk/DNVALA for year 2016).

autonomous and opt out. The charges for these services are income-dependent but heavily subsidized.

2.2. Aarhus Municipality busing policy

Danish cities in general and Aarhus in particular are characterized by a moderate level of residential concentration of immigrants and children of immigrants born in Denmark, hereafter referred to as "descendants," throughout its neighborhoods[10] and school districts. In 2005, the share of immigrants and descendants among families of children who turn 6 in that year (potential school starters) across Aarhus school districts ranged between 0–99%, the second-largest share being 82%, and has increased steadily since. Panel (a) of Figure 1 shows the concentration of immigrants and descendants among potential school starters and their families in 2016 across Aarhus school districts. 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. School Districts in Aarhus Municipality, 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 dual language learners in public schools. A secondary goal of this policy is to obtain equal academic outcomes of dual language learners 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, all dual language learners about to enroll in an Aarhus school are required to take a Danish 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.

Every year, around 550 school starters (i.e. 18% of all school starters in the municipality) take the language test. According to how they perform in the test, pupils are

[10] Calculating the residential concentration of immigrants and descendants from the ten largest source countries across residential neighborhoods (with on average 291 households) for each municipality in Denmark over the 1986–2016 period, Damm, Hassani, and Schultz-Nielsen (2019a) find an average dissimilarity index of 46%, i.e. 46% 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.

assigned to one of three categories of language proficiency, F, S, and B. There is a high test-score threshold separating categories F and S, and a low threshold separating categories S and B. The three categories have not changed over the years and depend on the age of the child (6-month increments). Pupils whose test scores exceed the high 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 the high threshold lose their free school choice and receive a school assignment for a regular class (category S), or if their test also fall below the low threshold, they are referred to a basic DAL class (category B).[11] For all children, private school is an option. Annually, around 26% of pupils are assigned to category F, and 5% to category B.

Note that, from here on, we refer to pupils whose first language is not Danish as dual language learners, to dual language learners with limited Danish proficiency as category-S pupils, and we use the acronym DAL (Danish as Additional Language) to indicate school activities targeted to dual language learners. In this paper, we focus on category-S pupils. According to their language test score, they are further divided into three levels of need for language support: 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 later language assessments).[13] They also have the right to attend DAL lessons after school. Around one-third of all category-S pupils are assigned to a school outside their school district, either due to parental request for a vacant slot outside their district or assignment to forced busing.[14]

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

[11] Basic DAL classes focus on language acquisition and are not exposed to the full age appropriate curriculum. Pupils in basic DAL classes are not integrated in regular classes as they are considered to have so limited Danish language proficiency that they cannot benefit from regular teaching. The classes are small and span three grades. They are located on 5-6 schools at a given point in time. In 2020/21 they are placed at one sending school and four receiving schools.

[12] Category-S pupils can only request a slot in another public school if the share of category-S pupils in that grade is below 20%.

[13] The language proficiency of category-S pupils is assessed regularly using grade-appropriate assessment material. We do not use time to regaining free school choice as an outcome variable in our analysis, because category-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.

[14] 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 has been exempted from the 20% rule since 2015.

with free bus services between home and school.[15] 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 gives priority to the following types of category-S pupils at the district school: First, pupils with special needs or problems in the family are assigned to the district school. Second, pupils with older siblings in the district school are assigned to the district school, starting with those with the youngest siblings in the district school. Finally, the municipality might use the distance between the district school and the pupils' residence as a final determinant. If the three characteristics were one-hundred percent strictly used and the continuous age and distance measured without error, the assignment determinants would be exhaustive, and there would be no lottery-type assignment.[16] However, these measures can be reported with different levels of precision or use different definitions; we do not observe the exact measures used by the municipality, but we construct them from the register data. We do indeed observe deviations from the stipulated assignment process, and we assume that any such deviation is as good as random, and most importantly, unforeseeable by parents. We show that this assumption holds in Section 4.

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 dual language

[15] The municipality lists which schools outside the school district category-S pupils can be referred to, according to the school district they reside in. When deciding the exact receiving school of assignment, the municipality also considers the assignment of other category-S pupils from the neighborhoods of residence in order to gather them at one – or at least few – schools. The school bus runs from the district school, with a few stops on the way, to the receiving schools in the mornings and back to the district schools in the afternoon, once after normal school hours and once after the after-school activities.

[16] If the assignment rule is taken at face value, students were simply ranked according to the three criteria and the 20% slots were filled with students ranked at the top. Let us illustrate by a simple example. A hypothetical grade zero cohort consists of 44 students among whom 12 are category-S pupils. Two of the category-S pupils have special needs, four have siblings in the district school, and six have no siblings in the district school. In order to keep the number of category-S pupils below 20%, four students must be assigned to busing. In this example, the four pupils with the longest distance from their home to the district school will be assigned to busing. If, for example, the number of students with siblings in the district school had been eight instead, the age difference between the pupil and the youngest among the older siblings in the district school would define which students would be assigned to busing.

learners 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]

Panel (b) in figure 1 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 10 sending school districts, 23 receiving districts and 12 districts neither sending nor receiving. Comparison of panel (b) with panel (a) 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).

Relatedly, in 2008, Aarhus Municipality closed two schools which had very high shares of dual language learners. We drop pupils living in these two districts from the analysis. While pupils who were bused from those two schools in 2006 and 2007 were unaffected by the closure, pupils who attended those schools were redistributed across other school districts. As a consequence, we lack a proper control group for pupils assigned to busing from these districts.[18]

2.3. Allocation of School Resources

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 of different coursework requirements. 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; the annual total additional resources to magnet schools is split between schools based on the school's historical share of magnet school pupils.

Using the school budgets for 2014 as an example, Figure 2 shows the budget per pupil in regular classes in Aarhus public schools. The average budget per pupil in regular classes

[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).
[18] 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.

increased by grade (1–3, 4–6, and 7–9) and school type (regular schools, regular schools with more than 20% DAL pupils, 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 2. Average Budgets per Pupil in Regular Classes (in USD) by Grade and Average Additional School Budgets for Dual Language Learners. 2014. By School Type.]

Schools receive additional resources to accommodate the needs and requirements of dual language learners. 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 dual language learner plus additional points for language-tested pupils in grades 0–3, according to their category of language support need (B, S, or F). Additionally, schools with more than 20% dual language learners 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 dual language learners.

The last columns in Figure 2 illustrate the importance of the additional DAL funding. The DAL budget is sizable: In 2014 it was on average $832 per pupil in schools with at least 20% dual language learners. Moreover, schools with more than 20% dual language learners 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 Online Appendix C for further details on the Aarhus Municipality guidelines for the allocation of school resources.

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).[19]

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

[19] See Table B1 in the Online Appendix for definitions and the data sources of all variables.

and residence status). For parents, we also have information on education level, income, and employment status.

The pupil registers for Aarhus Municipality (2007–17) and the national administrative education register provides detailed information about which school, grade, and class the pupils attend every year. The after-school programs register for Aarhus Municipality (2007–15), provides information about 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 natural science.[20] 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.[21]

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 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 dual language learners 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.

[20] Our data access to the national test register in 2019 for this study excludes access to test scores in English and natural science.
[21] See Table B2 in the Online Appendix for an illustration of data availability.

3.2. Sample selection and description

Around one sixth of all school starters in Aarhus Municipality are language screened before school start. Specifically, a total of 6,596 school starters have been screened between 2006 and 2016 and are alive in 2017. We focus on category-S pupils[22] who are eligible and fit for forced busing, are less than seven years old when taking the language screening test, and are referred to a regular public school, i.e. neither a private nor a special-needs school. We further restrict the sample to include only school starters living in school districts that (i) do not have a full-day school and (ii) have a sending school in the year of school start. Furthermore, 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 in 2006 and 2007 lived in the school districts that closed in 2008 and a small number for whom we lack information on the neighborhood of residence at the time of language screening.[23] In the end, our sample consists of 999 school starters.[24]

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 descendants of immigrants or children of descendants (92%); 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.[25]

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 20% of mothers and 17% of fathers are in the highest quartile. Parental education level, when known, is relatively low; 31% of mothers and

[22] Few pupils who are classified as category-S pupils despite having enough points in the language test to receive free school choice. We drop them because we do not know the reasons nor the direction of the misclassification.
[23] The families of these children moved to Aarhus Municipality during 2016 in the calendar year that the child turned 6. Since information about the individuals' neighborhoods of residence is only available until the beginning of 2016, their neighborhoods of destination are unknown.
[24] See Table A1 in the Appendix for a description of each step in the sample selection procedure.
[25] 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 (statbank.dk/FAM111N).

25% of fathers did not complete high school, while 20% of mothers and 18% of fathers have tertiary education.[26]

[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, category-S school starters are divided into levels of language support need: 13% have strong need, 43% medium need, and 44% low need.

Approximately 52% 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. 20%) and have a lower age difference to the youngest older sibling at the district school (3 and a half vs. 4 and a half 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 in the designated bus used for 0-3 graders. 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. Characteristics of School Districts of Residence and Assignment.]

The children in our sample reside in 10 school districts and are assigned to 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 (10), and receiving districts (35). On average, the share of immigrants and descendants among potential school starters, i.e. children who turn 6 during the year, and their families is substantially higher in sending districts (48%) compared to receiving districts (12%). Families of potential school starters in the sending districts also have lower socio-economic status (SES): The average employment rate of adults is 63% in sending districts compared to 85% in receiving districts, while the share of adults with a tertiary education (college or above) is 16% in sending districts compared to 13% in

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

receiving districts. The average real annual disposable income of adults is $33,957 in sending districts, below the national average of $38,852[27] and below the receiving districts average of $42,691.[28]

Across school districts, a substantial share of school starters enrolls in a private school or a non-district school and a small share postpones school start. On average, the enrollment rate of potential school starters is 63% for receiving school districts. As a result of busing and possibly also higher native flight, potential school starters living in sending districts are much less likely to enroll in the district school compared to school starters living in receiving school districts (40%).

Sending schools have on average higher shares of category-S pupils (23.5%) compared to receiving schools (14%). This is consistent with the 20% rule for school assignment.[29] Across school types, the overall school share of dual language learners is substantially higher than 20%, as it also includes dual language learners with free school choice as well as dual language learners enrolled before the start of the policy or after grade zero.

Sending schools have fewer pupils than receiving schools and smaller class sizes: On average, sending schools have 107 fewer pupils than receiving schools and average class size across grades of 20 compared to 26 in receiving schools. Sending schools have a substantially higher annual budget per pupil in regular classes. The average in years 2014–16 was $6,316 compared to $5,556 in receiving schools. The total DAL budget of sending schools was on average about three times that of receiving schools. Furthermore, it implied a per-pupil budget premium for dual language learners of 20% in sending schools, and 16% in receiving schools. The higher per-pupil budget of sending schools enables them to have slightly older teachers than receiving schools (45 vs. 44), and more classes taught by qualified staff (77% vs. 75%), but not fewer pupils per teacher (11.5 vs. 9.5).

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

[28] School starters in the sample are not evenly distributed across the school district, tending to live in areas with higher shares of immigrants and residents with 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. The characteristics of the residence neighborhoods of pupils assigned to busing and pupils assigned to the district school are very similar and the neighborhoods of the two groups overlap to a large degree.

[29] These shares refer to the share of category-S pupils at the start of grade 0. For magnet schools, the average share exceeds 20%. In fact, it only exceeds 20% for one magnet school that has been exempted from the 20% rule since the school year 2015. Flight of Danish pupils between class formation and school start might also have contributed to it.

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

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. We use all available observations of outcomes for the 999 school starters in our sample including repeated observations across several grades. [30]

[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 natural 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. 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 pupil'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 performance of the pupil.[31, 32]

To calculate the average pupil ability scores, we first standardize the ability measures in the population of test takers within year, grade, subject and cognitive area (mean 0, std. dev. 1); we then sum the standardized measures for each subject's cognitive areas; finally, we standardize the final measures in the population (mean 0, std. dev. 1). In Table 3, we report test taking rates among potential test takers and test scores. In reading, all public-school pupils are

[30] Because of the nature of the data, composed of different registers available for different years, not every pupil is observed in all grades for every outcome. We refer to Appendix Table A2 for the exact number of pupils observed in each grade for all outcomes.

[31] For details on the national tests and the cognitive areas for each subject, see Online Appendix Table B1 and Beuchert and Nandrup (2018).

[32] 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.

observed as potential test takers at least once and a maximum of four times. In math, we observe all public-school cohorts once or twice, except for the 2016 test cohort who is too young to be observed as test taker in math given our observation period. In English and natural sciences, roughly half of the public-school pupils are observed as test takers, because younger cohorts of school starters, i.e. cohorts starting in 2011 (2012) or later, do not grow into 7^{th} (8^{th}) grade during our observation period. When we pool all tests across subjects and grades in the empirical analysis, public-school pupils are observed a minimum of one and a maximum of ten times.

We see that the average test scores for individuals in our sample are well below the national mean, ranging from an average of –0.657 in the reading test across grades 2, 4, 6 and 8, to –0.362 in natural science in grade 8.

While the tests are compulsory for all pupils enrolled in public schools, principals may exempt some pupils from the tests. From Table 3, we see that 95% of public-school pupils in our sample take the tests in reading and math in the relevant years, meaning that 5% of all pupils are exempt from the test, which is in line with numbers for the universe of Aarhus public-school pupils. 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 dual language learners. In section 4.3, we report the effect of assignment to busing on whether the pupil takes the test or not.

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 pupils. The survey is administered by a designated teacher during class between January and April of every year.[33] Pupils in grades 0–3 and 4–9 receive different surveys.[34]

[33] 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. (2020).

[34] 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. Table B3 in the Online Appendix includes a full list of the survey questions (including an English translation).

We run an exploratory factor analysis on the grade 0-3 survey 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. We construct the same two factors for the grade 4-9 survey using corresponding questions. 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, whereas the opposite is the case for grades 4-9.[35]

[Table 4. Factor Loadings on School Satisfaction and Distress.]

Even considering that only public-school pupils take the survey, there is attrition in survey taking, on top of the missing data due to the fact that the data was only collected from 2015 onwards. Therefore, cohorts who start school in the years 2006 to 2011 (2015 and 2016) are not observed being enrolled in the relevant grades when the grade 0-3 (4-9) survey was collected; the remainder of pupils are observed between one and four/five times. Although questions can be read aloud if the pupils have difficulties reading them, the designated teachers can decide if pupils with special needs are able to take the survey or if they will be exempted (see Andersen et al. 2020). In Table 3, we see that 86% of potential test takers in our sample take the 0–3 survey and 82% 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%). In section 4.3, we report the effect of assignment to busing on whether the pupil takes the survey.

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 is the same across schools and around

[35] Factor loadings for the grade 4-9 survey can be found in Table B4.

200 days, depending on the year. 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 (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). 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

Given the assignment procedure described in Section 2, assignment to busing is exogenous once we account for the observed determinants: special needs, siblings in district school and distance to district school. First, we excluded from our sample pupils marked in the registers as having had a special needs assessment because all children with special needs are assigned to their district school. The other observed characteristics are having siblings already enrolled in the district school, age difference with the youngest of those siblings, and distance to the district school. We use age difference between siblings measured in days, and linear distance between the center of the micro-neighborhood of residence and the main entrance of the district school. These variables might imply measurement error, as we do not know the exact measures used by the municipality in the assignment. Finally, 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. Only as many pupils are bused as those who would bring the share of category-S pupils starting in grade 0 in the district school above 20%.

The assignment mechanism we consider is fairly simple. This allows us to directly control for the assignment determinants in our main specifications, without the need of reducing the dimensionality of the problem by using the probability of assignment as a proxy, as in Bergman (2019).[36] Ideally, we would control for the assignment determinants semi-parametrically. However, this approach is not feasible in our case due to our limited sample and the fact that the probability of treatment depends on the language test year-by-school-district share of pupils who need to be bused. Hence, we control for the assignment

[36] We run this alternative specification as a robustness check and our main results are unchanged. Results are available upon request.

determinants linearly and as a robustness check we control for non-linear effects of assignment determinants.

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 language test year-by-school district fixed effects, and ε_{igtr} the error term. We include language test 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 and across school districts. 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, 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:

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

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 intention-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 to differ by grade and subject and the treatment effects to differ by subject, extending Eq. (1) to this model:

$$y_{igstr} = \alpha_s b_{itr} + \gamma_g + \sigma_s + \beta Z_{itr} + \eta_{tr} + \epsilon_{igstr} \quad (2)$$

where σ_s denotes subject fixed effects, and α_s is our parameter of main interest, which allows the treatment effect to vary across subjects. ϵ_{igstr} is the error term. We interpret $\hat{\alpha}_s$ as the subject-specific intention-to-treat estimate of forced busing.

To increase the efficiency of $\hat{\alpha}_g$ and $\hat{\alpha}_s$, 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, a dummy for whether he child is an immigrant, area of origin (Africa; East Asia; Middle East; or Europe, Australia, New Zealand,

Canada and USA), 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 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.[37]

4.2. Test of the identification strategy

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 language test year-by-school district dummies that capture the time-varying school district characteristics that affect the probability of being bused. Hence, the first column of Table 5 can be thought of as the first-stage of our analysis. 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.

In our case, a traditional balancing test run by comparing sample means for the treated and the non-treated is not informative because the assignment is at the school district-year level. Instead, in columns (2–4) of Table 5 we check that, net of the assignment determinants, treatment is balanced by controlling for individual and parental characteristics in the first-stage regression. We show that 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. Moreover, a joint F-test cannot reject that these additional controls do not predict the treatment; the p-value is 0.91.

[Table 5. Test of the Identification Strategy.]

[37] The standard errors are virtually unaffected by whether we cluster by person ID instead of family ID.

Importantly, Table 5 shows that assignment does not depend on the category of language support need, which derives directly from the language test score.[38] 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. This could have raised two concerns about identification. First, that pupils in the treatment and control group also differ in terms of unobserved abilities that also affect our outcomes. Second, that older siblings attending the district school were selected on ability. This would imply that the assignment rule that gives priority in the district school to pupils with a sibling in the district school indirectly selects pupils on ability, assuming that siblings' abilities are correlated. A final source of selection into treatment might arise if parents of bused pupils with a younger child "at risk" of being bused act to have their older child re-assessed and moved back to the district school, in order to decrease the probability of being bused of the younger child. This concern is minimized by the fact that, out of all siblings who attend the district school, only 8.9% were initially assigned to busing and moved back to the district school. In Section 5.3 we show that our results are not driven by the sample with older siblings who were tested previously by restricting our sample to the first-tested in the family.[39,40]

We choose a parsimonious specification to control for the assignment determinants. One potential concern is that in doing so, we miss important non-linearities in the way covariates affect assignment to busing and outcomes. We test this concern in Appendix Table A4 by repeating columns 1 and 5 of Table 5 and adding interactions of our assignment variables. We start by adding interactions of distance to the district school with the dummy for having a sibling in the district school and age difference with the youngest sibling in the district school, then we add interactions with the school district dummies and with the test cohort dummies. While we find that distance to the district school matters significantly only for pupils without siblings in the district school, adding these interactions does not affect the R-squared nor the joint significance of the additional controls. The fully interacted model with interactions of all the assignment variables with school district and test cohort dummies allows the assignment criteria to vary for every school district and language test year. Columns 6 and 12

[38] This result holds if we use the total points on the language test, both total and split by tasks.

[39] There are 702 older siblings who attend the district school at the time their younger siblings in our sample take the test. Of them, 54% were tested themselves prior to school entry. Of all tested older siblings, 35% were assigned category F and 59% were assigned category S. The correlation between the language test score of the pupils in our sample and their older siblings in the district school is low (0.19).

[40] Table A3 in the Appendix presents coefficients for all the covariates in Table 5. Furthermore, we add an extra column with neighborhood characteristics as controls. Table A3 shows that neighborhood characteristics do not affect assignment to schools.

show that, controlling for fully interacted assignment determinants, treatment is balanced, providing further support of our identification strategy. We show how different specifications affect our results in Section 5.3.

4.3. Compliance

While in grade zero category-S pupils can attend only the public school they are assigned to, they have other options available. In this section we show that compliance with the assignment 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 3, 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 33% 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.[41] 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 shows that they have obtained an age-appropriate level of Danish language proficiency. Pupils moving to another school district or municipality can also transfer to a school in the new district.[42] Treated pupils are more likely to transfer than pupils assigned to the district school. Six years after the language test, 15% of treated pupils have returned to the district school of residence in the year of the test, 16% are enrolled in a private school, and an additional 27% have transferred to another school than the district school. School movers among treated pupils include pupils who had regained free school

[41] 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.

[42] While in grade zero compliance with the assignment is almost equivalent to compliance with the policy, in subsequent grades pupils can move schools while still complying with the policy by regaining free school choice or moving district of residence. Few pupils move from the school district between school assignment and school start.

choice[43] as well as pupils who moved to a new school district or municipality subsequent to the language test.[44]

[Figure 3. Fraction of Pupils Attending Different Types of Schools. By Treatment Status and Years Since the Language Test.]

Figure 4 illustrates the extent to which assignment to busing causes non-compliance. In all six panels of Figure 4, 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 pupils and availability of school choices between districts.

[Figure 4. Effect of Assignment to Busing on Compliance with the Policy.]

Panel (a) of Figure 4 shows the effect of assignment to busing on compliance with the assignment. 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 4, we present the results of our empirical specification on the different ways to achieve non-compliance. Panel (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 6 percentage points less likely to enroll in school in the year of the test. This effect disappears the year after the test, indicating that these children delay school start.

Panels (c), (d), and (e) of Figure 4 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. 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

[43] 6 years after the language test 43% of treated pupils had regained free school choice. 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.
[44] 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 B5.a and B5.b in the Online Appendix) do not show any clear pattern other than around 23 percentage points higher compliance for control children with at least one sibling attending the district school in the year of the test.

language assessment, thereby signaling their pre-determined intention not to enroll in public school.[45] Moreover, Panel (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.

Panel (d) shows that, in the year of the test, being assigned to busing reduces the probability of attending the district school by 81 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 has declined to 42. 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 the option to enroll their children in the district school. Notice that the effect in Panel (d) does not mirror the effect in Panel (a), because it combines the effect due to bused pupils leaving the assigned school to go to the district school and the effect due to non-bused pupils leaving the district school to go somewhere else.

Finally, Panel (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, due to pupils who move to another school district. Panel (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 8 percentage points. Moreover, Panel (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 4 confirms that being assigned to busing reduces compliance with the school assignment over time, both by increasing school delays and due to pupils returning to the district school after regaining their free school choice. We complete our analysis of the first stage by analyzing the effect of assignment to busing on the characteristics of the attended school. We defer this discussion to Section 6.[46]

[45] 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.

[46] Exploiting within-school variation in the share of category S-pupils school starters assigned to be bused to the receiving school over the period 2007-2017, and using the full sample of native (potential) school starters resident in a receiving school district (28,079 pupils), we also investigate the effects of the share of category-S pupils assigned to receiving schools on native flight from receiving schools, test-taking and test-scores of native pupils in receiving schools. Our results suggest that share of bused pupils did not affect local pupils in receiving school districts. Likely explanations include that higher immigrant school concentration does not induce local and native flight at modest levels of concentration (Rangvid 2010) and bused pupils do not affect academic achievement of native pupils due to compensatory funding. Results are available upon request.

4.4. Non-response

Our two main outcomes of interest result from either taking a test or completing a survey. In Table 3 we showed that there is non-response both in test taking and in survey taking. Hence, we check that there is no high and significant effect of assignment to busing on take-up.

We report the effect of assignment to busing on taking the national tests in the first two columns of Table 6. Across subjects, there is no significant effect of assignment to busing on national test taking. The only significant negative coefficient is for the reading test in grade 2. We report the effect of assignment to busing on survey take-up by grade in the first column of Appendix Table A5. We find a strong negative effect on take-up in grade 1 but not in the other grades. We investigate the sensitivity of our results to non-response in Section 5.3.

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 science conditional on taking the test. The result of the main specification presented in Section 4.1 is shown in column 3 and the result with additional controls for individual and parental characteristics in column 4.

[Table 6. Effect of Assignment to Busing on National Test Scores. By Subject.]

Assignment to busing significantly reduces test scores in math by around one quarter of a standard deviation and reading by around 0.14–0.17 of a standard deviation. There is an imprecisely estimated negative effect also on English test scores, of around 0.11-0.16 of a standard deviation. The effect for the natural science tests has a negative coefficient but it is small and imprecisely estimated. In Panel B, we show the effects split by grade and subject. For math, we find that the negative effects persist across grades. For reading, we find negative effects on test taking in early grades and negative effects on reading test scores in later grades.

[Table 7. Effect on Assignment to Busing on National Test Scores. By Subject and Subgroups: Gender, Socio-Economic Status (SES) and Language Support Level.]

It is often found that the impact of school resources is more important for certain groups, e.g. pupils from low-SES backgrounds or boys, possibly due to public investments crowding out parental investments (Fredriksson et al. 2016, Gensowski et al., 2020, Fort et al. 2020). 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 and reading scores across gender, SES and language support need, although coefficients are estimated less precisely for some subgroups. In addition, although not significantly different at the conventional levels, some gender differences appear: Assignment to busing significantly reduces the test score in reading for boys and the test score in math for girls. Moreover, assignment to busing significantly lowers the test score in English for girls (only). 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 and significant (in math and reading) for 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 (Panel A) and in grades 4 to 9 (Panel B), and both without and with controls for individual and parental background characteristics. While assignment to busing does not significantly reduce school satisfaction overall, it increases distress in the early years by approximately one quarter of a standard deviation. This result implies that pupils assigned to busing are more likely to report feeling alone, and to experience headaches and stomachaches while in school, so that their overall distress level is almost 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. The effect on distress disappears in the later grades, suggesting that the negative effects of busing on wellbeing in school are not long-lasting.[47]

In Table A5 in Appendix we show the effect of assignment to busing on school satisfaction and distress in school by grade for pupils in grades 0 to 3 (Panel A) and in grades 4 to 9 (Panel B). The effects on distress are particularly strong in grades 0 and 3.

[47] For grades 4-9, we also construct the four validated measures for conscientiousness, agreeableness, emotional stability and general wellbeing suggested by Andersen et al. (2020), and none of those measures are affected by being assigned to busing, which is consistent with our main results. As a further robustness check, we report the estimated effects of assignment to busing on the standardized responses to each survey question in Tables B6 and B7 in the Online Appendix. We find that overall the survey supports our result of negative effects of busing on the pupils' wellbeing in the early grades (effect are significantly negative for 4-6/20 questions). In particular, busing reduces the probability of feeling that they learn exciting things in schools, and increases the risk of often having a stomachache in school, and of having difficulty of hearing the teacher during lessons. The latter effect could be linked to either their lower language proficiency relative to other classmates or more noisy and crowded classrooms. In the later grades the effects disappear and bused pupils report to be happier with their class and to be making academic progress (effects are significantly positive for 3-4/40 questions). This could be evidence of them feeling that they are catching up after having experienced distress in the earlier grades.

In Table A6 in the Appendix, we show the effects of busing on wellbeing in school in grades 0 to 3 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.

[Table 8. Effect of Assignment to Busing on Wellbeing Survey Factors: School Satisfaction and Distress.]

5.3 Sensitivity checks

In this section, we discuss some robustness checks on our estimates. First, as we show in Table 3 and discuss in Section 4.4, while there is no significant effect of assignment to busing on test take-up, test take-up is not universal. School principals have the option of exempting pupils for whom the test is not considered beneficial for their development. Consequently, there is persistent lower test take-up among public-school pupils from low-SES families as well as immigrant pupils and special needs pupils (Andersen and Nielsen 2020). Among those taking the test, pupils who scored at the bottom of the school in earlier grades are less likely to take it, suggesting that the most likely outcome for the non-takers would have been a below-average test score. In Table A7 in the Appendix, we show what the results of our main specification (Eq. 2) would have been if the non-takers had scored in the fifth percentile (column 3) and the 95th percentile (column 4) of the distribution of test scores of individuals in the sample. 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 A8 in the Appendix. We conclude that the effect on distress in grades 0 to 3 is robust to this test. Moreover, the overall effect of busing on school satisfaction, while non-significant, would still have a negative sign.[48]

Because of the young age of the pupils in our sample, we do not observe test scores for all pupils in all grades. In column 1 of Table A9 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 2006–10). The effects are at least as strong as for the full sample. In column 2 of Table A9 in the Appendix, we show the results of our

[48] We bound our estimates using the Horowitz and Manski (1998) method. The alternative Lee (2009) bounding method is not available in our case because we cannot ensure monotonicity of non-response and because of the many covariates necessary for identification.

main specification (Eq. 2) using a restricted sample of only pupils who attend the school they are assigned to at the time of the national test. Again, our results are unchanged. In column 3 of Table A9 in the Appendix, we show that the results of our main specification (Eq. 2) are also robust to excluding children who have an older sibling who has also been language tested.

Finally, in columns 4 and 5 of Table A9 in the Appendix, we show that the effects on standardized test scores are robust to including interactions between the determinants of assignments, as we discussed in Section 4.2. In particular, our results are robust also to including the determinants of assignments fully interacted with test cohort by school district fixed effects, which fully controls for the fact that the probability of treatment depends on the year-by-school-district share of pupils who need to be bused. The pooled dataset and relatively large number of tests per pupil allows us to stay very close to the point estimates with all specifications, losing a bit of power in the specifications with full interactions with the school district and test cohort dummies. In Table A10 in the Appendix, we show how the effects on the wellbeing factors vary when we add progressively more interactions between the assignment determinants. Our results are robust up to including the fully interacted assignment model. However, the available dataset for the wellbeing surveys is substantially smaller than the one for the national tests and the fully interacted specifications includes many empty or almost empty cells.

6. School environment and potential mechanisms

Bused and non-bused pupils attend schools that are different in two main dimensions: peer composition and school resources. In Figure 5, we present the results of our empirical specification on a set of characteristics of the attended school over time. The four panels in Figure 5 show the impact of assignment to busing on the share of dual language learners, 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.

In grade zero, pupils assigned to busing attend schools with fewer dual language learners in the school (36 percentage points fewer) and a higher share of employed parents (26 percentage points higher). School resources go in the opposite direction: On average, the per-pupil budget is $784 (around 13%) lower, and the total DAL budget is around $185,000 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 smaller class size and better qualified and more experienced teachers (Table 2), which most likely have 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, Jackson 2020). Overall, two potentially opposing effects are at play: Lower school resources have a negative effect on academic achievement, whereas peers with systematically different characteristics in terms of cognitive (e.g. native language) or non-cognitive skills (e.g. openness to others) may counteract or reinforce this effect.

[Figure 5. The Impact of Assignment to Busing on Peer Composition and Resources of the Attended School.]

In the remainder of this section, we investigate and discuss the potential channels through which the different school environments experienced by bused and non-bused pupils affect academic achievement and wellbeing in more detail.

6.1 School resources and gains from specialization

Our main results show negative effects of assignment to busing on test scores relative to assignment to the district school. Dual language learners who attend schools with higher resources and more dual language learners perform better than those who attend schools with lower resources but more native Danish speakers. This suggests that, in our context, the negative effect of lower resources is stronger than any potential positive effect of better language role models. Recall that our investigation of possible heterogeneous effects 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).

Higher numbers of children with a specific need, such as dual language learners, 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 origin if budgets are tight (Andersen and Guul 2019). While sending schools have a higher total DAL budget than receiving schools because they have more dual language learners, DAL budgets are generally considered well balanced by the school principals. Having more dual language learners in the pupil body makes any intervention more cost effective, whereas having higher total DAL budgets allows for the implementation of multiple and targeted 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.[49]

More generally, receiving schools appear to be mismatched to the needs and abilities of dual language learners. 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 in early grades.

6.2 Peers and social isolation

As mentioned above, it is not obvious that attending a school with more native peers is beneficial for dual language learners. While peers with a better command of the Danish language might help with Danish skills, they might affect the dual language learners' self-confidence negatively and restrict their available strong social ties. In fact, 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, minority and majority children often self-segregate. Jensen and Vitus (2020) report that children assigned to busing think of themselves as "guests" or "outsiders." Thus, dual language learners attending receiving schools seem to form social networks with the other bused children and will be socially isolated from the other classmates. Consistently with Jensen and Vitus (2020), we find that assignment to busing increases pupil's distress in school in early grades. This effect on distress disappears in later grades. One possible explanation is it takes longer for bused pupils to adapt to the new environment and form social ties with their classmates.

Another test of social isolation is attendance of 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

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

at their district school. 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 effectively choose. We show the effect of assignment to busing on whether the pupils attend an after-school program (Figure 6 Panel (a)) and on attending an after-school program in the attended school (Figure 6 Panel (b)). While assignment to busing does not affect the probability of attending any after-school program, children assigned to busing are 11 and 9 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. The main take-away from Figure 6 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. Therefore, these results casts doubt on the positive nature of peer effects for this population of dual language learners.

[Figure 6. Effect of Assignment to Busing on Enrollment in After-School Programs.]

Switching to a special class after school enrollment can be regarded as extreme form of social exclusion. Therefore, we also test whether pupils assigned to busing are more likely to switch from a regular class to special education after school start. We find no effects of busing on such switch.[50] However, few pupils in our sample make such a switch, possibly because all pupils deemed unfit for busing attend the district school and are excluded from our sample.

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 (ex-ante disruption). 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 ex-post 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 a sub-analysis. It is only suggestive because

[50] Results are available upon request.

returners are likely to be more resourceful, as regaining free school choice requires an age-appropriate level of Danish language proficiency. Our results seem to be driven by compliers and not by returners (see Table A9, column 2). This suggests that the effect of social isolation at the receiving school, possibly due to ex-ante disruption, dominates the disruption costs of transferring to the district school.

6.3 Other potential mechanisms: bus ride and absenteeism

Finally, the school experience of bused and non-bused pupils differs in another, almost mechanical, aspect. 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 7 shows the effect of assignment to busing on the share of absent days over active school days by grade. Assignment to busing causes a small non-significant 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.[51] Clearly, such a small effect does not explain our main findings.[52]

[Figure 7. The Effect of Assignment to Busing on School Absentee Rates: Share of Days of Absence over Active Days.]

7. Conclusions

We use quasi-experimental variation from a school desegregation policy to evaluate how busing affects dual language learners who start school requiring language support. We find that dual language learners who are assigned to busing have lower academic achievement across grades and higher distress, although only in early grades, relative to those assigned to the

[51] 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.

[52] 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), the answers of respondents are unlikely to be representative for parents of category-S pupils in our sample. 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 low-SES parents. Results are available upon request.

district school, which are characterized by more school resources and fewer native peers. Our results suggest that school resources matter for the outcomes of dual language learners and that ample school resources combined with high shares of dual language learners enhance academic achievement of dual language learners who require language support at school start, e.g. due to gains from specialization. Although dual language learners with limited Danish proficiency may benefit academically from having more classmates who are native Danish speakers, the short-term effect on distress suggests that dual language learners experience faster social integration at sending schools as opposed to receiving schools because they have more classmates with similar cognitive (e.g. language) and non-cognitive skills (e.g. openness to others) as well as shared common knowledge and everyday lives, all of which facilitates friendship formation. Slower social integration of dual language learners at receiving schools constitutes another channel through which busing creates persistent gaps in academic achievement.

In general, our results suggest 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 dual language learners remains 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.

In the specific case of the Aarhus busing policy, our findings indicate that the current policy does not reach the stated goal of obtaining equal academic outcomes of dual language learners 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, the underlying assumption of positive effects of native peers is questionable and the social interaction effects are more complex.

A related question is whether dual language learners with weak host-country language proficiency perform better under forced busing than in the absence of the policy. Our results do not lend themselves to answering this question because busing affects the peer composition and other school inputs of both sending and receiving schools.[53] However, our results suggest

[53] One might compare the outcomes of cohorts of dual language school starters pre- and post-forced busing policy. Due to severe data limitations (e.g. no information on host-country language proficiency of dual language learners

that, if the policy is retained in the future, receiving schools should implement initiatives to increase social integration of dual language learners and be held accountable for their use of DAL budgets in order to improve the school outcomes of dual language learners. 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.[54]

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prior to the policy) and simultaneous changes of policy instruments (e.g. school budgets and peer composition), however, this proves impossible.

[54] 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.

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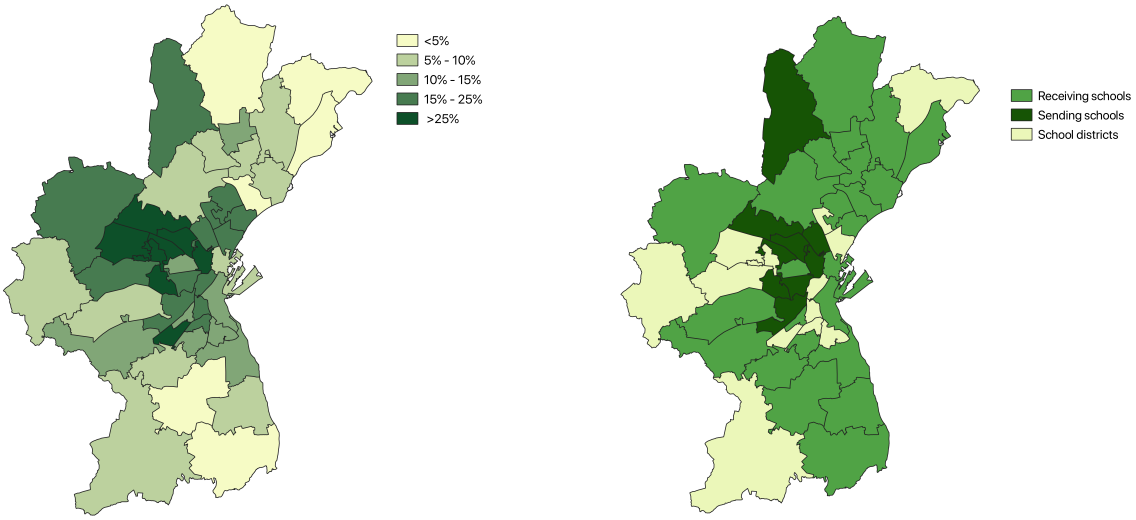
Figure 1: School Districts in Aarhus Municipality, 2016.



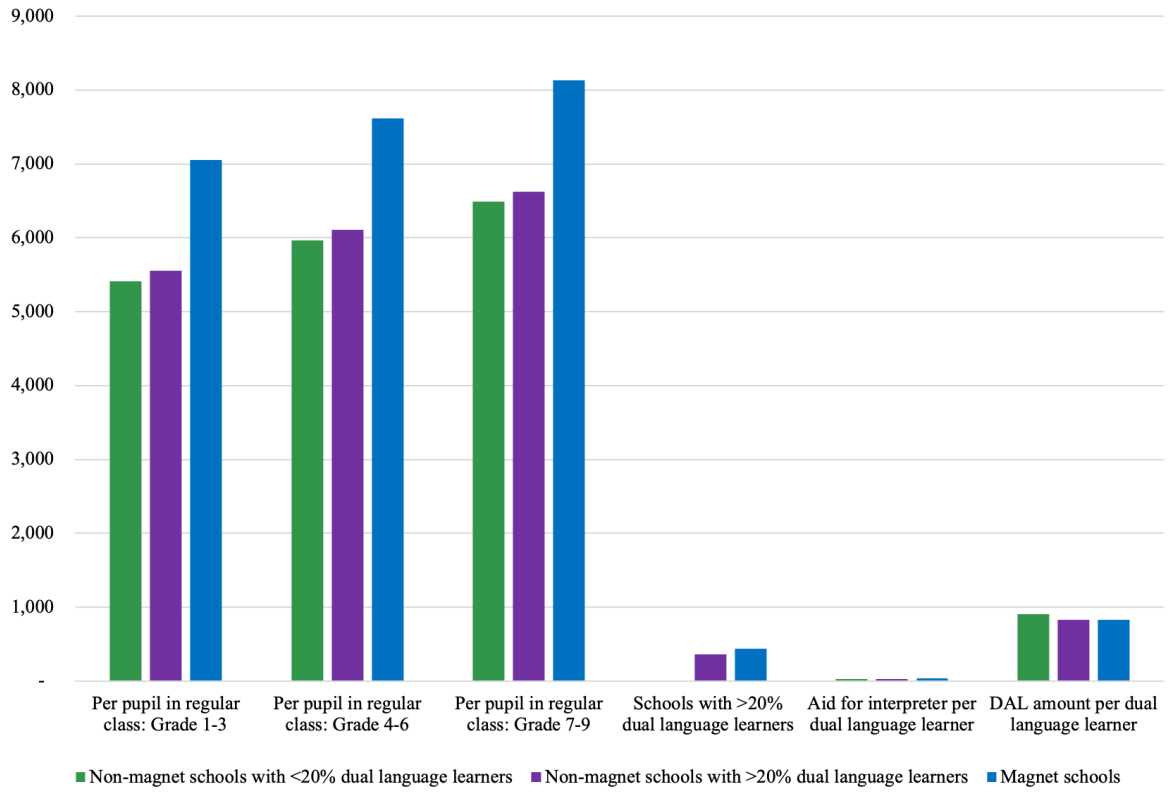
(a) Share of Immigrants and Descendants

(b) Sending and Receiving Districts

Source: Administrative register data from Statistics Denmark and Aarhus Municipality and the shape file of school districts in Aarhus Municipality in 2016 (up to date shape files at https://webkort.aarhuskommune.dk/spatialmap - Administration - Distrikter - Skoledistrikter).

Notes: Figure (a) Share of potential school starters, their parents and siblings living in the school district with non-Danish origin or descent. Potential school starters are children who turn 6 during the calendar year. Figure (b) Receiving schools (in at least one year between 2007 and 2016).

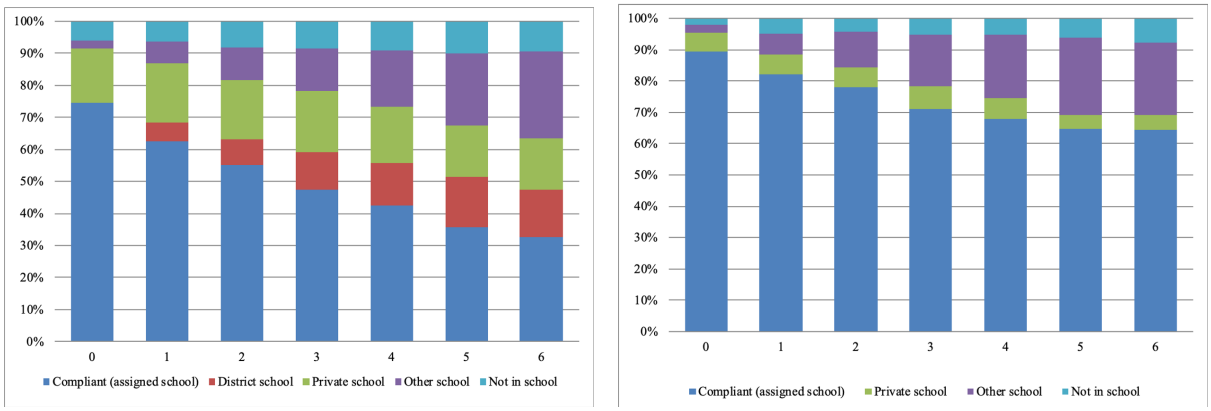
Figure 2: Average Budgets per Pupil in Regular Classes (in USD) by Grade and Average Additional School Budgets for DAL Activities. 2014. By School Type.



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

Notes: 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% dual language learners receive an additional budget for "task-specific resources"; the amount per dual language learner is shown in the column titled "Schools with more than 20% dual language learners". 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 dual language learner in regular classes is calculated by dividing the budget for DAL support to dual language learners in regular classes by the number of dual language learners in regular classes. Exchange rate DKK/USD 0.1485 (base year 2016).

Figure 3: Fraction of Pupils Attending Different Types of Schools. By Treatment Status and Years Since the Language Test.



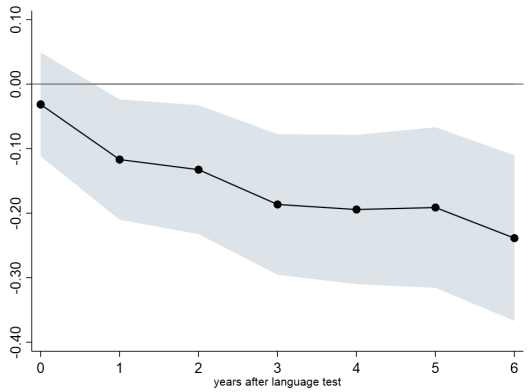
(a) Pupils Assigned to Busing (Treatment)

(b) Pupils Assigned to the District School (Control)

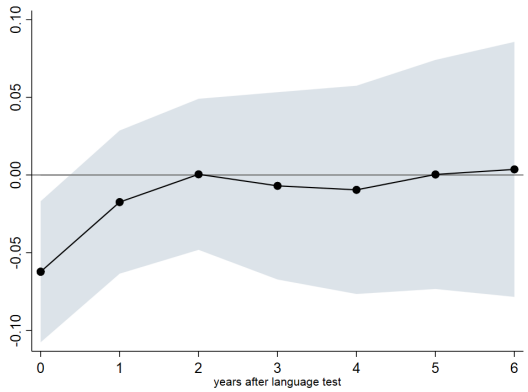
Source: Administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Refer to notes under Table 1 for sample description. 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. Data not available for the 2006 cohort in year 0.

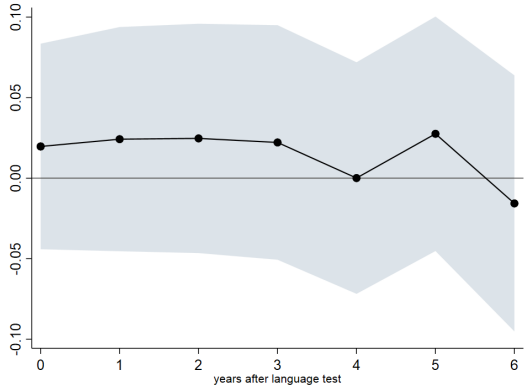
Figure 4: Effect of Assignment to Busing on Compliance with the Policy.



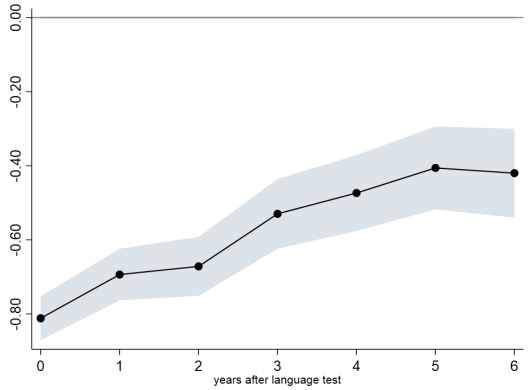
(a) Enrollment in the assigned school, compliance



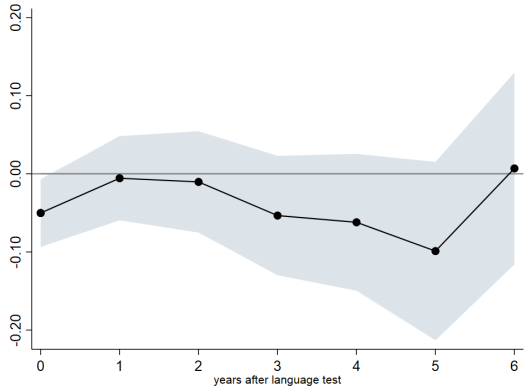
(b) Enrollment in school



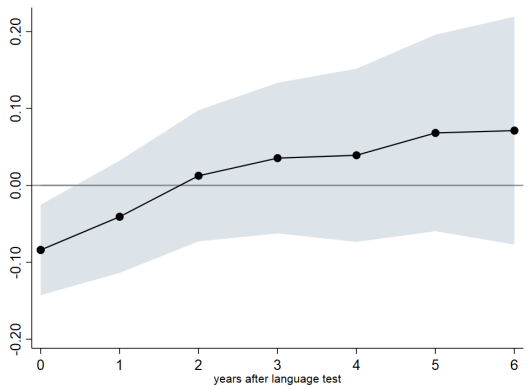
(c) Enrollment in private school, conditional on enrollment



(d) Enrollment in district school, conditional on enrollment



(e) Enrollment in another school, conditional on enrollment

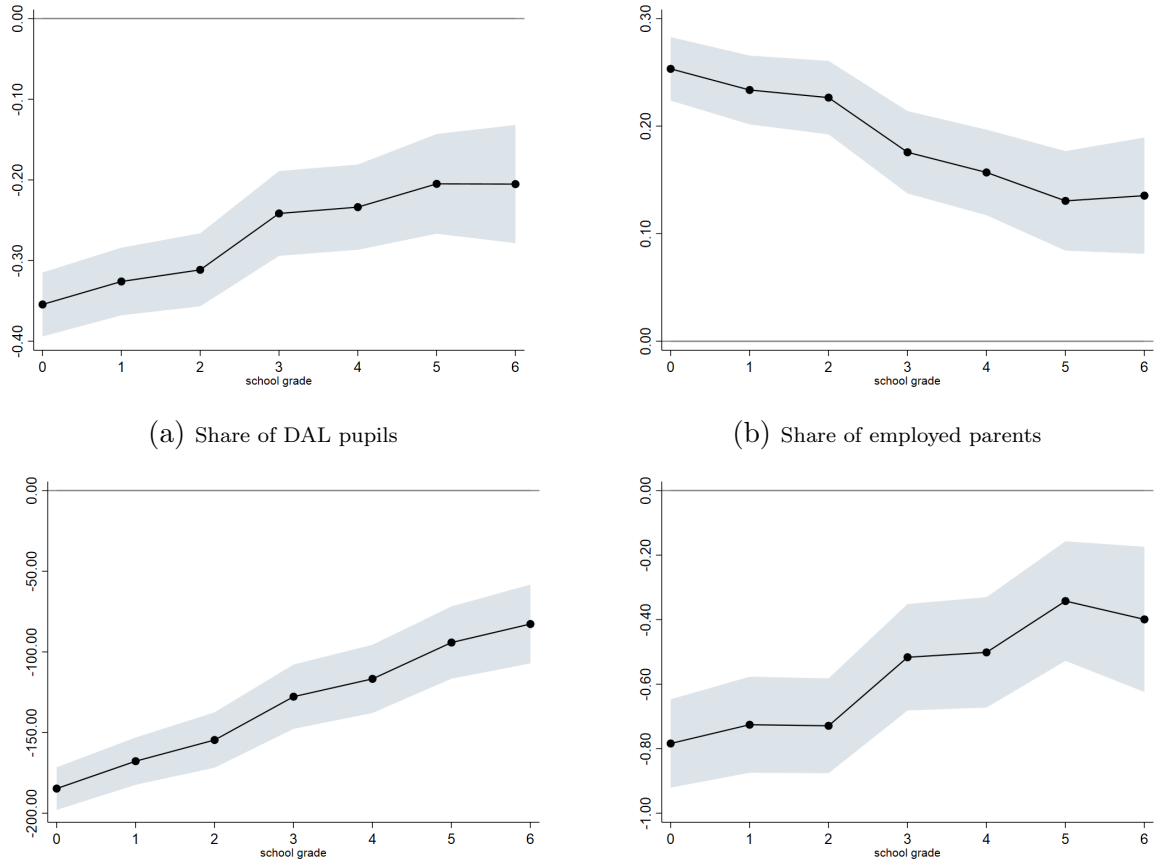


(f) Moved by the end of the year

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

Notes: Refer to notes under Table 1 for sample description. 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. Refer to notes under Table 5 for a list of the additional controls. 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 5: The Impact of Assignment to Busing on Peer Composition and Resources of the Attended School.



(a) Share of DAL pupils

(b) Share of employed parents

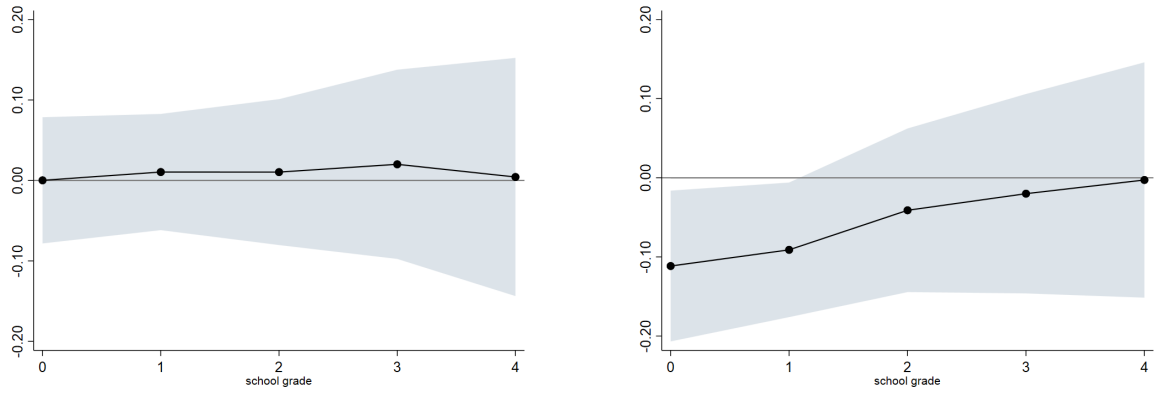
(c) Total DAL budget

(d) Per-pupil budget

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

Notes: Refer to notes under Table 1 for sample description. 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. Refer to notes under Table 5 for a list of the additional controls. 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 6: The Effect of Assignment to Busing on Enrollment in After-School Programs.



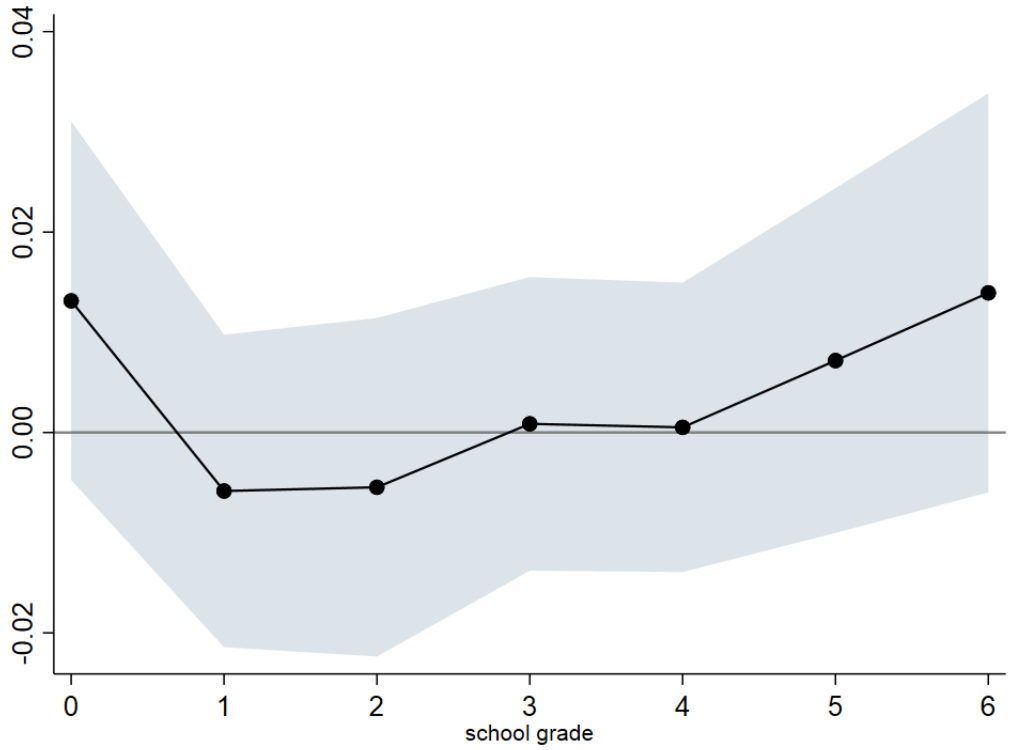
(a) After-school program

(b) After-school program in attended school

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

Notes: Refer to notes under Table 1 for sample description. 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. Refer to notes under Table 5 for a list of the additional controls. Robust standard errors clustered at the family level. Refer to notes under Table 5 for a list of the additional controls. Outcomes: a. enrollment in any after-school program, b. enrollment in the after-school program of the attended school.

Figure 7: 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).

Notes: Refer to notes under Table 1 for sample description. 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. Refer to notes under Table 5 for a list of the additional controls. 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. Refer to notes under Table 5 for a list of the additional controls.

Table 1: Sample characteristics: Individuals and Parents.

	All		Assigned to busing		Assigned to district school	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Panel A - Individual characteristics						
Boy	0.528	0.499	0.538	0.499	0.517	0.500
Age on August 1st	6.167	0.365	6.182	0.375	6.152	0.354
Immigrant	0.077	0.267	0.054	0.227	0.101	0.302
Origin or descent: Africa	0.304	0.460	0.289	0.454	0.320	0.467
Origin or descent: Middle East	0.447	0.497	0.513	0.500	0.378	0.485
Origin or descent: East Asia	0.111	0.314	0.085	0.280	0.138	0.346
Number of siblings	3.287	2.076	3.326	2.005	3.246	2.151
Attended daycare	0.977	0.150	0.977	0.151	0.977	0.149
Living in two-parent household	0.705	0.456	0.676	0.469	0.736	0.442
Parents not employed (low SES)[1]	0.440	0.497	0.478	0.500	0.401	0.491
Observations	999		515		484	
Panel B - Parental characteristics[1]						
Mothers						
Age	32.246	6.012	31.522	6.083	33.033	5.840
Married	0.740	0.439	0.777	0.417	0.702	0.458
High school dropout	0.312	0.463	0.328	0.470	0.294	0.456
High school graduate	0.232	0.422	0.225	0.418	0.239	0.427
College graduate	0.199	0.399	0.200	0.400	0.197	0.399
No education reported	0.258	0.438	0.247	0.432	0.269	0.444
Employed (includes self-employed)	0.255	0.436	0.231	0.422	0.279	0.449
Unemployed	0.072	0.259	0.093	0.291	0.050	0.219
Out of the labor force	0.643	0.479	0.652	0.477	0.632	0.483
Real disposable income[2]	24,994	10,170	24,741	9,650	25,263	10,699
Real disposable income in first quartile[3]	0.157	0.364	0.160	0.367	0.153	0.361
Real disposable income in second quartile[3]	0.271	0.445	0.294	0.456	0.246	0.431
Real disposable income in third quartile[3]	0.347	0.476	0.324	0.469	0.372	0.484
Real disposable income in fourth quartile[3]	0.203	0.402	0.209	0.407	0.195	0.397
Observations	982		506		476	
Fathers						
Age	37.163	7.072	36.363	6.901	38.009	7.160
Married	0.765	0.424	0.783	0.413	0.747	0.435
High school dropout	0.253	0.435	0.280	0.450	0.223	0.417
High school graduate	0.294	0.456	0.307	0.462	0.281	0.450
College graduate	0.183	0.387	0.159	0.366	0.208	0.406
No education reported	0.270	0.444	0.254	0.436	0.288	0.453
Employed (includes self-employed)	0.499	0.500	0.463	0.499	0.536	0.499
Unemployed	0.092	0.289	0.110	0.313	0.073	0.260
Out of the labour force	0.364	0.481	0.382	0.486	0.345	0.476
Real disposable income[2]	23,159	12,245	21,971	12,520	24,413	11,833
Real disposable income in first quartile[3]	0.267	0.443	0.305	0.461	0.227	0.420
Real disposable income in second quartile[3]	0.327	0.469	0.329	0.470	0.324	0.469
Real disposable income in third quartile[3]	0.212	0.409	0.195	0.397	0.230	0.421
Real disposable income in fourth quartile[3]	0.166	0.372	0.144	0.352	0.189	0.392
Observations	958		492		466	

(continued)

Table 1: Sample characteristics: Individuals and Parents. (continued)

	All		Assigned to busing		Assigned to district school	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Panel C: School assignment policy[4]						
Strong language support need	0.130	0.337	0.136	0.343	0.124	0.330
Medium language support need	0.429	0.495	0.443	0.497	0.415	0.493
Low language support need	0.440	0.497	0.421	0.494	0.461	0.499
Bused	0.516	0.500	1.000	0.000	0.000	0.000
Sibling attending the district school[5]	0.419	0.494	0.204	0.403	0.649	0.478
Age difference with youngest sibling in district school	3.809	2.528	4.659	2.894	3.525	2.330
Sibling bused	0.193	0.395	0.196	0.397	0.190	0.393
Distance to district school (km)	0.848	0.646	0.845	0.512	0.851	0.764
Distance to assigned school (km)	3.973	3.745	6.912	2.978	0.851	0.764
Number of category-S pupils in class	4.102	2.107	3.433	1.975	4.677	2.049
Observations	999		515		484	

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

Sample: Language screened school starters in Aarhus Municipality 2006-2016, 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 the school districts closed in 2008, and who do not move to Aarhus between January and school start in 2016.

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

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

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

[4] Year of the language screening test.

[5] For the 2006 test cohort, inferred from 2007 school registers.

Table 2: Characteristics of School Districts of Residence and Assignment.

	Sending districts		Receiving districts	
	Mean	Std. dev.	Mean	Std. dev.
Districts				
Share of potential school starters and their family members who are immigrant or descendant	0.484	0.173	0.120	0.173
Employment rate[1]	0.635	0.109	0.852	0.131
Share with a tertiary education[1]	0.165	0.046	0.135	0.051
Avg. real disposable income[1,2]	33,957	3,755	42,691	8.134
Share of potential school starters who enroll in the district school	0.406	0.180	0.632	0.215
District Schools				
School size[3]	480	175	587	213
Class size[4]	19.514	10.692	25.506	20.318
Grade 0 class size[4]	17.077	5.383	21.481	9.035
Share of employed parents	0.511	0.153	0.743	0.155
Share of dual language learners[5]	0.496	0.187	0.179	0.209
Share of category-S pupils in grade 0[3,6]	0.235	0.141	0.138	0.118
Average age of teachers[7]	45.157	2.296	43.745	2.295
Pupils per teacher[7]	11.475	2.296	9.497	1.992
Share of lessons with qualified staff[7]	0.771	0.111	0.749	0.128
Share of Danish lessons with qualified staff[7]	0.842	0.172	0.839	0.132
Share of math lessons with qualified staff[7]	0.813	0.205	0.762	0.194
Per-pupil budget[2,3,8,9]	6,316	908	5,556	384
Real total DAL budget (thousands)[2,9]	267.624	54.968	85.841	62.765
DAL per-pupil premium (%)[8,9]	19.578	2.215	16.401	2.156
Average DNT score, reading[10]	-0.475	0.969	-0.039	0.937
Average DNT score, math[10]	-0.339	0.922	0.009	0.939
Average DNT score, English[10]	-0.154	0.960	0.137	0.974
Average DNT score, natural sciences[10,11]	-0.349	0.870	0.091	0.850
Number of schools	10		35	

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: Refer to notes under Table 1 for sample description. Sample size 999 individuals. Averages across the relevant years and school districts.
[1] Parents and older siblings (age 25 to 54) of potential school starters in the school district. Potential school starters are children who turn 6 in the relevant year and reside 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 one sending school 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] Average of available years (2014-2016), conditional on the school being open in those years.
[10] Mean and standard deviation by school of the class-average DNT score, available for school years from 2009/2010 to 2018/2019.
[11] Natural sciences include: 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)	0.946	2,192	0.931	1,199	0.964	993
Reading, grade 2, 4 , 6, 8 (score)	-0.657	2,073	-0.712	1,116	-0.593	957
	(0.946)		(0.944)		(0.946)	
Math, grades 3, 6 (taker)	0.949	1,167	0.938	640	0.962	527
Math, grades 3, 6 (score)	-0.567	1,107	-0.666	600	-0.449	507
	(0.878)		(0.862)		(0.887)	
English, grade 7 (taker)	0.751	413	0.761	272	0.730	141
English, grade 7 (score)	-0.364	310	-0.424	207	-0.241	103
	(0.863)		(0.851)		(0.873)	
Natural sciences, grade 8 (taker)[2]	0.900	569	0.906	424	0.883	145
Natural sciences, grade 8 (score)	-0.362	512	-0.390	384	-0.276	128
	(0.973)		(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)	0.823	2,138	0.790	1,238	0.868	900
	(0.382)		(0.407)		(0.339)	
School satisfaction (grades 4–9)	-0.027	1,759	0.017	978	-0.081	781
	(1.050)		(1.047)		(1.052)	
Distress (grades 4–9)	0.064	1,759	0.037	978	0.098	781
	(1.000)		(1.026)		(0.963)	
School absentee rates[4]						
Share of absences over school days, 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	3,042	0.079	1,334	0.061	1,708
	(0.064)		(0.076)		(0.052)	
Enrollment in after-school programs[5]						
After-school in grade 0	0.824	682	0.802	354	0.848	328
After-school in attended school, grade 0	0.767	682	0.698	354	0.841	328
After-school, grades 0-4	0.823	2,401	0.815	1,380	0.833	1,021
After-school in attended school, grades 0–4	0.779	2,401	0.743	1,380	0.828	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: Refer to notes under Table 1 for sample description. Sample size 999 individuals. 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. We reverse the second factor to increase with the child's distress.

Table 5: Test of the Identification Strategy.

	Dependent variable: Assigned to busing			
	First stage	Balancing tests		
	(1)	(2)	(3)	(4)
Determinants of assignment:				
Sibling in district school	-0.466***	-0.460***	-0.467***	-0.467***
	(0.036)	(0.037)	(0.038)	(0.039)
Age difference with youngest sibling in district school	0.023***	0.022***	0.023***	0.022***
	(0.006)	(0.007)	(0.007)	(0.007)
Distance from neighborhood of residence to district school	0.051***	0.056***	0.053***	0.054***
	(0.019)	(0.019)	(0.019)	(0.020)
Additional controls:				
Sibling bused		0.026	0.024	0.023
		(0.032)	(0.032)	(0.032)
Medium language support need		-0.003	0.001	-0.001
		(0.032)	(0.033)	(0.032)
Low language support need		-0.010	-0.007	-0.009
		(0.032)	(0.033)	(0.033)
Age on language test day		0.013	0.019	0.015
		(0.027)	(0.028)	(0.028)
R^2	0.641	0.645	0.650	0.651
Observations	999	999	999	999
Controls:				
Year–by–school district fixed effects	YES	YES	YES	YES
Additional Controls:				
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		0.600	0.733	0.723
P-value F-test		0.915	0.865	0.911

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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Refer to notes under Table 1 for sample description. Sample size 999 individuals. First stage: OLS of dummy for assignment to busing (sample avg. 0.516) 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 (2-4) 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 (2-4), mother (3-4) and father characteristics (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, area of origin (Africa; Europe, Australia, New Zealand, Canada and USA; East Asia; 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: 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 Subject and Grade.

	Dependent variable:			
	Test taker		Standardized test score	
	(1)	(2)	(3)	(4)
Panel A:				
Explanatory variables:				
Assigned to busing, reading	-0.027	-0.029	-0.173*	-0.141
	(0.018)	(0.018)	(0.098)	(0.093)
Assigned to busing, math	-0.021	-0.022	-0.248**	-0.220**
	(0.020)	(0.018)	(0.104)	(0.096)
Assigned to busing, English	0.047	0.046	-0.167	-0.113
	(0.045)	(0.044)	(0.136)	(0.125)
Assigned to busing, natural science	0.054	0.048	-0.111	-0.001
	(0.040)	(0.037)	(0.160)	(0.145)
Observations	4,341	4,341	4,002	4,002
Panel B:				
Explanatory variables:				
Assigned to busing, reading test grade 2	-0.057**	-0.056**	-0.148	-0.119
	(0.024)	(0.023)	(0.101)	(0.097)
Assigned to busing, math test grade 3	-0.028	-0.028	-0.253**	-0.224**
	(0.022)	(0.022)	(0.107)	(0.099)
Assigned to busing, reading test grade 4	-0.033	-0.033	-0.105	-0.072
	(0.022)	(0.022)	(0.110)	(0.105)
Assigned to busing, reading test grade 6	0.000	-0.002	-0.208*	-0.179*
	(0.024)	(0.023)	(0.114)	(0.108)
Assigned to busing, math test grade 6	-0.012	-0.015	-0.233*	-0.207*
	(0.024)	(0.023)	(0.119)	(0.110)
Assigned to busing, English test grade 7	0.048	0.047	-0.171	-0.117
	(0.045)	(0.045)	(0.136)	(0.126)
Assigned to busing, reading test grade 8	0.008	0.008	-0.310**	-0.270**
	(0.030)	(0.030)	(0.134)	(0.130)
Assigned to busing, natural science grade 8	0.055	0.051	-0.114	-0.005
	(0.040)	(0.038)	(0.160)	(0.145)
Observations	4,341	4,341	4,002	4,002
Additional Controls	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: Refer to notes under Table 1 for sample description. Sample size 999 individuals. Pooled dataset. Outcomes: A dummy for having taken the national tests (cols 1 and 2) and the standardized test scores conditional on having taken the test (cols 3 and 4). 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. Panel A: OLS of the outcome on a dummy for assignment to busing interacted with the test subject (Panel A) and the grade (Panel B). 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. Refer to notes under Table 5 for a list of the additional controls.

Table 7: Effect on Assignment to Busing on National Test Scores. By Subject and subgroups: Sex, Socio-Economic Status (SES) and Language Support Need (LSN).

	Dependent variable: Standardized test score								
	Subgroup: Sex			Subgroup: SES			Subgroup: LSN		
	Boys	Girls	Diff.	High	Low	Diff.	Low	Med/high	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(4)	(5)	(6)
Explanatory variables:									
Assigned to busing, reading	-0.182*	-0.087	-0.095	-0.104	-0.182*	0.078	-0.144	-0.137	0.001
	(0.108)	(0.108)	[0.395]	(0.108)	(0.105)	[0.462]	(0.116)	(0.101)	[0.951]
Assigned to busing, math	-0.165	-0.285***	0.120	-0.151	-0.297***	0.146	-0.308**	-0.160	-0.148
	(0.112)	(0.110)	[0.291]	(0.112)	(0.109)	[0.188]	(0.119)	(0.105)	[0.198]
Assigned to busing, English	-0.025	-0.238*	0.213	-0.052	-0.178	0.126	-0.085	-0.132	0.046
	(0.143)	(0.142)	[0.118]	(0.142)	(0.139)	[0.331]	(0.147)	(0.138)	[0.731]
Assigned to busing, natural sciences	0.128	-0.195	0.322**	-0.086	0.082	-0.168	-0.058	0.047	-0.105
	(0.148)	(0.187)	[0.047]	(0.159)	(0.166)	[0.258]	(0.166)	(0.159)	[0.477]
Observations	4,002			4,002			4,002		
Additional controls	YES			YES			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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. P-value of test of equality between coefficients in squared parentheses. Refer to notes under Table 1 for sample description. Sample size 999 individuals. Pooled dataset. Outcome: 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 the test subject and sex of the child (columns 1-3), socio-economic status of the child (high if at least one parent is employed) (columns 4-7), and a dummy for whether the child has low language support need or medium to high (columns 8-11). 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. Refer to notes under Table 5 for a list of the additional controls.

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

	Dependent variable:			
	School satisfaction		Distress	
	(1)	(2)	(3)	(4)
Panel A: grade 0–3				
Explanatory variable:				
Assigned to busing	-0.090	-0.080	0.244*	0.239*
	(0.116)	(0.121)	(0.139)	(0.132)
Observations	1,060	1,060	1,060	1,060
Panel B: grade 4–9				
Explanatory variables:				
Assigned to busing	-0.028	-0.008	-0.035	-0.045
	(0.116)	(0.112)	(0.101)	(0.093)
Observations	1,759	1,759	1,759	1,759
Additional controls	NO	YES	NO	YES

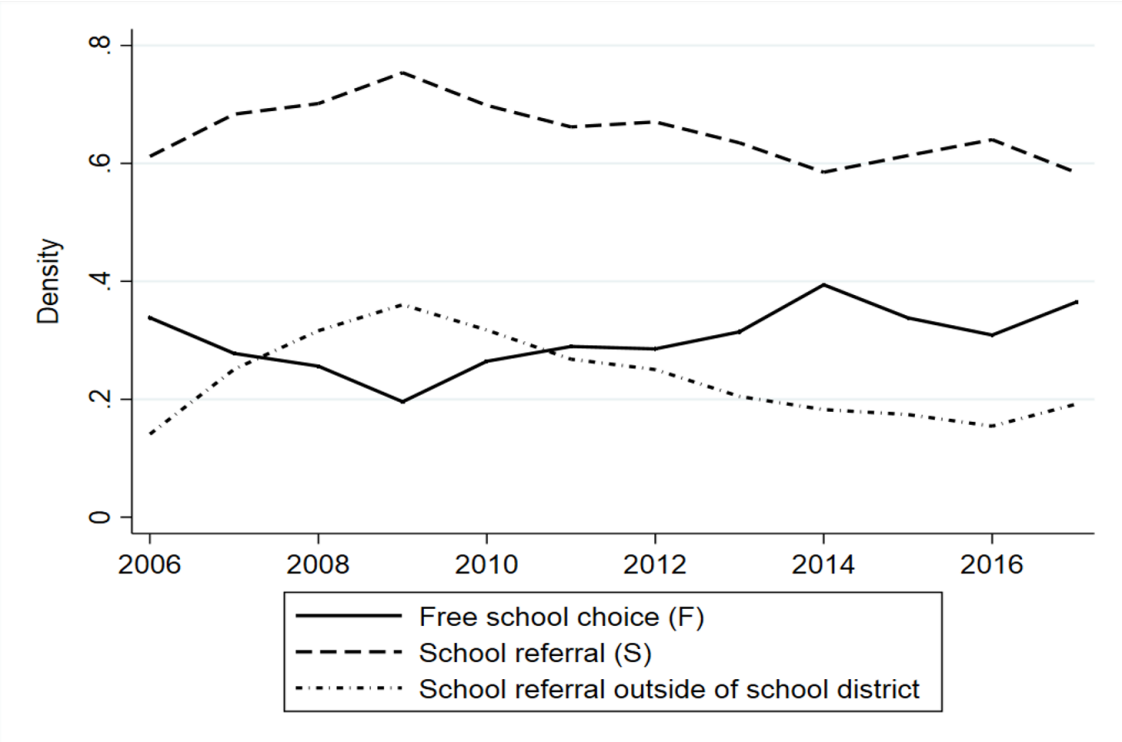
Source: Micro data from Danish Wellbeing Surveys linked with administrative register data from Statistics Denmark and Aarhus Municipality and neighborhood of residence register constructed by Damm et al. (2019a).

Notes: Robust standard errors in parentheses, clustered at the family level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Refer to notes under Table 1 for sample description. Sample size 999 individuals. Pooled dataset. Outcome: Standardized factors for school satisfaction (1, 2) and distress (3, 4) in grades 0-3 (Panel A) and grades 4-9 (Panel B). OLS of the outcome on a dummy for assignment to busing, 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 (cols. 2, 4) controls for additional individual and family characteristics. Refer to notes under Table 5 for a list of the additional controls.

Appendices

Appendix A. Extra Figures and Tables.

Figure A.1: Aarhus Municipality's Busing Policy during the Period 2006-2017



Source: Administrative register data from Aarhus Municipality.
Notes: Sample: Language screened school starters in Aarhus Municipality 2006-2017. Sample size: 6,596 school starters.

Table A.1: Sample Selection

	Sample selection criteria	*N*
1	Language screened school starters in 2006/7-2016/17 (remain alive in 2017)	6,596
2	who are considered fit for busing, i.e. no special needs	6,437
3	who are less than seven years old when taking the language screening test	6,418
4	who are referred to a regular public school (not private, special needs, or missing assignment)	6,261
5	who are classified as category S and have a total test score that implies category S	4,006
6	whose district schools are not full-day schools since they follow another policy rule	3,125
7	whose district school is a sending school	1,677
8	who have no expressed preference for a school other than the district school	1,272
9	who do not live in school districts closed in 2008	1,072
10	who do not move to Aarhus between January and school start in 2016	999

Table A.2: Number of Observation per Grade and Outcome.

grade	*in Education Registers*	Outcomes										
		Test taker	Test score, reading	Test score, math	Test score, English	Test score, physics and chemistry	Test score, geography	Test score, biology	Wellbeing surveys	Wellbeing surveys responses	Absenteeism	After-school program
0	999								230	204	519	682
1	999								292	254	610	563
2	999	746	697						350	304	665	470
3	916	689		649					355	298	639	388
4	832	636	601						378	332	609	298
5	741								395	344	521	213
6	658	956	459	458					402	348	456	114
7	578	413			310				377	310	407	43
8	482	901	316			233	138	141	331	267	324	0
9	370								255	158	252	0

Note: Number of pupils in the sample observed each grade in each dataset. Sample: Language screened school starters in Aarhus Municipality 2006-2016, 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 the school districts closed in 2008, and who do not move to Aarhus between January and school start in 2016.

Education Registers: Available until academic year 2018/2019. Details of data availability by cohort in Online Appendix Table B2. National Test Register: National tests administered from academic year 2009/2010, compulsory only for public school pupils. Available until 2018/2019 (math and reading) and 2017/2018 (English and Science). Biology and geography tests only compulsory until academic year 2016/2017. Wellbeing Surveys: Administered from 2015 to public school pupils in all grades. Existing waves cover academic years 2014/2015 to 2018/2019. School Absence Register: Data available from 2011 to 2019. After-school Register: Data available only from August 2007 to February 2015. We stop the analysis with grade 4 because for later grades we have few/no test cohorts and attendance declines sharply as children move to sport clubs.

Table A.3: Test of the Identification Strategy. Full Set of Covariates.

	Dependent variable: Assignment to busing					
	(1)	(2)	(3)	(4)	(5)	(6)
Determinants of assignment:						
Sibling in the district school	-0.466***	-0.466***	-0.460***	-0.464***	-0.467***	-0.466***
	(0.036)	(0.036)	(0.037)	(0.038)	(0.039)	(0.039)
Age difference with sibling in district school	0.023***	0.023***	0.022***	0.022***	0.022***	0.022***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Distance to district school	0.051***	0.051***	0.056***	0.054***	0.054***	0.056***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Additional controls:						
Medium language support need		0.002	-0.003	-0.001	-0.001	-0.002
		(0.031)	(0.032)	(0.033)	(0.032)	(0.032)
Low language support need		-0.004	-0.010	-0.008	-0.009	-0.010
		(0.031)	(0.032)	(0.033)	(0.033)	(0.033)
Age on language test day			0.013	0.014	0.015	0.015
			(0.027)	(0.028)	(0.028)	(0.028)
Sibling assigned to busing			0.026	0.024	0.023	0.023
			(0.032)	(0.032)	(0.032)	(0.032)
Male			0.011	0.010	0.009	0.009
			(0.021)	(0.022)	(0.022)	(0.022)
Immigrant			-0.061	-0.070	-0.070	-0.067
			(0.042)	(0.045)	(0.045)	(0.045)
Origin or descent: Africa			-0.042	-0.049*	-0.046	-0.047*
			(0.027)	(0.028)	(0.028)	(0.029)
Origin or descent: Western countries[1]			-0.049	-0.049	-0.041	-0.040
			(0.035)	(0.036)	(0.036)	(0.036)
Origin or descent: East Asia[2]			-0.054	-0.067*	-0.064	-0.064
			(0.039)	(0.040)	(0.040)	(0.040)
Attended daycare			0.044	0.046	0.056	0.053
			(0.047)	(0.048)	(0.047)	(0.047)
Number of siblings: 1			-0.073	-0.067	-0.064	-0.070
			(0.067)	(0.067)	(0.068)	(0.068)
Number of siblings: 2			-0.058	-0.056	-0.046	-0.052
			(0.066)	(0.067)	(0.068)	(0.068)
Number of siblings: 3			-0.065	-0.063	-0.058	-0.066
			(0.068)	(0.069)	(0.070)	(0.070)
Number of siblings: 4			-0.054	-0.048	-0.037	-0.042
			(0.072)	(0.074)	(0.075)	(0.075)
Number of siblings: 5			-0.074	-0.070	-0.058	-0.067
			(0.075)	(0.076)	(0.078)	(0.079)
Number of siblings: 6			-0.107	-0.098	-0.093	-0.097
			(0.075)	(0.077)	(0.079)	(0.079)
Number of siblings: 7 or more			-0.067	-0.067	-0.060	-0.068
			(0.075)	(0.077)	(0.079)	(0.080)
No mother recorded in register datasets			0.041	0.040	0.025	0.021
			(0.086)	(0.091)	(0.090)	(0.091)
No father recorded in register datasets			0.005	0.001	0.005	0.002
			(0.053)	(0.054)	(0.069)	(0.068)
Living in two-parent household			0.001	0.004	0.007	0.010
			(0.025)	(0.027)	(0.029)	(0.029)
Mother's age: under 25				0.022	0.035	0.033
				(0.052)	(0.056)	(0.056)
Mother's age: 25 to 29				0.021	0.029	0.029
				(0.043)	(0.048)	(0.048)
Mother's age: 30 to 34				0.009	0.009	0.008
				(0.041)	(0.043)	(0.043)
Mother's age: 35 to 39				0.005	0.005	0.006
				(0.040)	(0.041)	(0.041)
Mother: high school dropout				-0.002	-0.004	-0.004
				(0.030)	(0.030)	(0.030)
Mother: education not reported				-0.036	-0.036	-0.034
				(0.034)	(0.034)	(0.034)
Mother: completed university				-0.035	-0.036	-0.034
				(0.032)	(0.033)	(0.033)
Mother: employed (includes self-employed)				0.017	0.031	0.035
				(0.026)	(0.028)	(0.028)
Mother: unemployed				0.045	0.049	0.049
				(0.041)	(0.041)	(0.041)
Mother's disposable income: second quartile				-0.048	-0.058	-0.057
				(0.037)	(0.038)	(0.038)
Mother's disposable income: third quartile				-0.029	-0.042	-0.042
				(0.037)	(0.038)	(0.038)

(continued)

Table A.3: Test of the Identification Strategy. Full Set of Covariates. (continued)

Mother's disposable income: fourth quartile				0.011	0.000	0.000
				(0.041)	(0.043)	(0.043)
Father's age: under 25					-0.016	-0.015
					(0.061)	(0.061)
Father's age: 25 to 29					-0.032	-0.034
					(0.044)	(0.044)
Father's age: 30 to 34					-0.009	-0.010
					(0.037)	(0.037)
Father's age: 35 to 39					0.009	0.007
					(0.031)	(0.031)
Father: high school dropout					0.018	0.018
					(0.029)	(0.029)
Father: education not reported					-0.003	-0.001
					(0.031)	(0.031)
Father: completed university					-0.017	-0.012
					(0.032)	(0.032)
Father: employed (includes self-employed)					-0.028	-0.027
					(0.027)	(0.027)
Father: unemployed					-0.028	-0.027
					(0.037)	(0.037)
Father's disposable income: second quartile					0.012	0.012
					(0.029)	(0.029)
Father's disposable income: third quartile					0.026	0.028
					(0.035)	(0.035)
Father's disposable income: fourth quartile					-0.030	-0.026
					(0.039)	(0.040)
Neighborhood: share of immigrants and descendants						0.063
						(0.062)
Neighborhood: share of adults with tertiary education						0.122
						(0.139)
R^2	0.641	0.641	0.645	0.649	0.651	0.652
Observations	999	999	999	999	999	999
Controls:						
Determinants of assignment	YES	YES	YES	YES	YES	YES
Year–by–school district fixed effects	YES	YES	YES	YES	YES	YES
Additional controls:						
Individual characteristics	NO	NO	YES	YES	YES	YES
Mother characteristics	NO	NO	NO	YES	YES	YES
Father characteristics	NO	NO	NO	NO	YES	YES
Neighborhood characteristics	NO	NO	NO	NO	NO	YES
F-test joint insignificance of additional controls			0.600	0.652	0.723	0.774
P-value F-test			0.915	0.932	0.911	0.861

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 the school districts closed in 2008, and who do not move to Aarhus between January and school start in 2016. OLS of dummy for being bused (sample avg. 0.516) 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), dummies for having an older sibling bused, and the continuous age of the pupil on the day of the test (3-6), mother (4-6) and father characteristics (5-6). We also add characteristics of neighborhoods of residence (6). Fixed effects for 12 school districts of residence in each year of the period 2006-2016. 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, Australia, New Zealand, Canada and USA; East Asia; 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 potential school starters and their families who are immigrants or descendent and share of parents and older siblings of potential school starters (age 25-54) with tertiary education. Potential school starters are children who turn 6 in the relevant year and reside in the school district.

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

[2] Excl. Middle-East and former Soviet block

Table A.4: Test of the Identification Strategy. Alternative specifications.

	Dependent variable: Assignment to busing											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Assignment variables:												
Sibling in the district school	-0.466***	-0.440***	-0.442***				-0.467***	-0.429***	-0.435***			
	(0.036)	(0.047)	(0.051)				(0.039)	(0.050)	(0.055)			
Age difference with sibling in district school	0.023***	0.023***	0.024***				0.022***	0.022***	0.024**			
	(0.006)	(0.006)	(0.009)				(0.007)	(0.007)	(0.010)			
No sibling in the district school		0.064**	0.064**					0.072**	0.072**			
# Distance to district school		(0.029)	(0.029)					(0.030)	(0.030)			
Yes sibling in the district school		0.032	0.034					0.026	0.034			
# Distance to district school		(0.020)	(0.032)					(0.023)	(0.037)			
Distance to district school	0.051***						0.054***					
	(0.019)						(0.020)					
Age difference with sibling in district school			-0.001						-0.002			
# Distance to district school			(0.008)						(0.009)			
Observations	999	999	999	999	999	999	999	999	999	999	999	999
R^2	0.641	0.641	0.641	0.685	0.687	0.824	0.651	0.652	0.652	0.696	0.697	0.831
Assignment determinants specification	Baseline	Alternative 2	Alternative 2b	Full school district interaction	Full test-cohort interaction	Fully interacted	Baseline	Alternative 2	Alternative 2b	Full school district interaction	Full test-cohort interaction	Fully interacted
Additional controls	NO	NO	NO	NO	NO	NO	YES	YES	YES	YES	YES	YES
F-test joint significance for additional controls							0.723	0.748	0.747	0.750	0.677	0.563
P-value F-test							0.911	0.885	0.887	0.883	0.947	0.991

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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Refer to notes under Table A.3 for sample description. Sample size 999 individuals. Pooled dataset. OLS of dummy for being bused (sample avg. 0.516) over assignment determinants in 6 different specifications and additional controls (cols. 7-12). F-test on individual, mother and father characteristics, age, level of language support, and sibling bused. Additional controls include: individual characteristics including the assessed level of language support need, dummies for having an older sibling bused, and the continuous age of the pupil on the day of the test, mother and father characteristics. Individual characteristics of the child include gender, immigration status (immigrant or descendant), area of origin (Africa; Europe, Australia, New Zealand, Canada and USA; East Asia; 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).

The baseline specification (cols. 1, 7) includes 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. In alternative 2 (cols. 2, 8) we interact distance in km from the main entrance of the district school with the dummy for having at least one sibling attending the district school in the fall of the year of the test. In alternative 2b (cols. 3, 9) we add an interaction of distance with the age difference with the youngest sibling attending the district school. In columns 4 and 10 we interact all variables in alternative 2 with school district dummies. In columns 5 and 11 we interact all variables in alternative 2 with language test year dummies. In columns 6 and 12 we interact all variables in alternative 2 with language test-year–by–school district of residence dummies.

Table A.5: Effects of Assignment to Busing on Wellbeing by Grade. Two Survey Factors: School Satisfaction and Distress.

	Dependent variable:		
	Survey taker	School satisfaction	Distress
	(1)	(2)	(3)
Panel A: grade 0-3			
Explanatory variable:			
Assigned to busing, grade 0	-0.025	-0.075	0.414*
	(0.061)	(0.184)	(0.242)
Assigned to busing, grade 1	-0.220***	-0.100	0.098
	(0.062)	(0.170)	(0.173)
Assigned to busing, grade 2	-0.016	0.010	-0.064
	(0.048)	(0.173)	(0.165)
Assigned to busing, grade 3	-0.021	-0.153	0.510***
	(0.050)	(0.166)	(0.172)
Observations	1,227	1,060	1,060
Panel B: grade 4-9			
Explanatory variables:			
Assigned to busing, grade 4	-0.067*	-0.109	-0.090
	(0.040)	(0.147)	(0.145)
Assigned to busing, grade 5	-0.003	0.057	0.038
	(0.039)	(0.147)	(0.126)
Assigned to busing, grade 6	0.003	0.057	-0.025
	(0.038)	(0.152)	(0.130)
Assigned to busing, grade 7	-0.088**	0.116	-0.004
	(0.043)	(0.143)	(0.123)
Assigned to busing, grade 8	-0.038	-0.068	-0.128
	(0.049)	(0.162)	(0.137)
Assigned to busing, grade 9	-0.116	-0.271	-0.138
	(0.071)	(0.202)	(0.181)
Observations	2,138	1,759	1,759
Additional controls	YES	YES	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: Refer to notes under Table A.3 for sample description. Sample size 999 individuals. Pooled dataset. Outcomes: a dummy for whether the pupil completed the wellbeing survey, standardized factors for school satisfaction (1, 2) and distress (3, 4) in grades 0-3 (Panel A) and grades 4-9 (Panel B). OLS of the outcome on a dummy for being bused interacted with the grade 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. Additional controls for individual and family characteristics. Refer to notes under Table A.4 for a list of the additional controls.

Table A.6: Effects of Assignment to Busing on Wellbeing in Grades 0–3. Two Survey Factors: School Satisfaction and Distress. By Sex, Socio-Economic Status, Language Support Need.

	Dependent variable:					
	School satisfaction			Distress		
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
Assigned to busing, boys	-0.163			0.304**		
	(0.149)			(0.146)		
Assigned to busing, girls	0.026			0.156		
	(0.149)			(0.169)		
Assigned to busing, high SES		-0.181			0.288*	
		(0.151)			(0.156)	
Assigned to busing, low SES		0.027			0.187	
		(0.141)			(0.155)	
Assigned to busing, low LSN			-0.223			0.302*
			(0.157)			(0.159)
Assigned to busing, medium or high LSN			0.027			0.192
			(0.144)			(0.161)
Difference	-0.189	-0.209	-0.250	0.148	0.101	0.110
P-value	[0.282]	[0.199]	[0.160]	[0.387]	[0.533]	[0.553]
Observations	1,060	1,060	1,060	1,060	1,060	1,060
Additional controls	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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. P-value of test of equality between coefficients in squared parentheses. Refer to notes under Table A.3 for sample description. Sample size 999 individuals. Pooled dataset. Outcome: Standardized factors for school satisfaction (1, 3) and distress (4, 6). 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. Refer to notes under Table A.4 for a list of the additional controls. The presented parameters are not significantly different from each other in any of the regressions.

Table A.7: Robustness Checks. Bounds on Estimated Effect on National Test Score by Subject.

	Dependent variable:			
	Test taker	Standardized test score		
	(1)	(3)	(3)	(4)
Explanatory variables:				
Assigned to busing, reading	-0.027	-0.141	-0.173*	-0.091
	(0.018)	(0.093)	(0.093)	(0.087)
Assigned to busing, math	-0.021	-0.220**	-0.226**	-0.167*
	(0.019)	(0.096)	(0.096)	(0.090)
Assigned to busing, English	0.050	-0.001	0.070	-0.085
	(0.037)	(0.145)	(0.142)	(0.136)
Assigned to busing, natural science	0.046	-0.113	0.002	-0.135
	(0.045)	(0.125)	(0.125)	(0.117)
Observations	4,341	4,002	4,341	4,341
Non-takers	-	-	p5	p95
Additional controls	YES	YES	YES	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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Refer to notes under Table A.3 for sample description. Sample size 999 individuals. 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 the 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, controls for additional individual and family characteristics. Refer to notes under Table A.4 for a list of the additional controls. Columns 5-8 show bounds according to Horowitz and Manski (1998). In column 3 non test takers are assigned the 5th percentile of the test score distribution in the sample, in column 4 non test takers are assigned the 95th percentile.

Table A.8: Robustness Check: Bounds on Estimated Effect on Wellbeing.

	Dependent variable:						
	Survey taker	School satisfaction			Distress		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: grade 0–3							
Explanatory variable:							
Assigned to busing	-0.066*	-0.088	-0.189	-0.006	0.243*	0.085	0.297**
	(0.034)	(0.121)	(0.126)	(0.105)	(0.131)	(0.135)	(0.117)
Observations	1,227	1,060	1,227	1,227	1,060	1,227	1,227
Panel B: grade 4–9							
Explanatory variable:							
Assigned to busing	-0.046*	-0.008	-0.106	0.052	-0.045	-0.103	0.038
	(0.025)	(0.112)	(0.106)	(0.099)	(0.093)	(0.087)	(0.088)
Observations	2,138	1,759	2,138	2,138	1,759	2,138	2,138
Non takers	-	-	p5	p95	-	p5	p95
Additional controls	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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Refer to notes under Table A.3 for sample description. Sample size 999 individuals. Pooled dataset. Outcomes: a dummy for having taken the wellbeing survey, and standardized factors for school satisfaction and distress in grades 0-3 (Panel A) and grades 4-9 (Panel B). 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. Refer to notes under Table A.4 for a list of the additional controls. 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 A.9: Effect of Assignment to Busing on National Test Score by Subject. Robustness Checks.

	Dependent variable: Standardized test score				
	Balanced Sample: Test Cohorts 2006-2010 (1)	Sample: Compliers (2)	Sample: First Tested in the Family (3)	Full sample (4)	Full sample (5)
Explanatory variables:					
Assigned to busing, reading	-0.218*	-0.150	-0.068	-0.150	-0.145
	(0.122)	(0.103)	(0.116)	(0.093)	(0.124)
Assigned to busing, math	-0.300**	-0.226**	-0.270**	-0.228**	-0.226*
	(0.127)	(0.106)	(0.119)	(0.095)	(0.130)
Assigned to busing, English	-0.069	0.025	-0.028	-0.120	-0.094
	(0.151)	(0.184)	(0.169)	(0.125)	(0.153)
Assigned to busing, natural science	-0.168	0.060	0.056	-0.005	-0.032
	(0.136)	(0.151)	(0.174)	(0.144)	(0.172)
Observations	2,738	2,565	2,476	4,002	4,002
Assignment determinants specification	Baseline	Baseline	Baseline	Alternative 2	Fully interacted
Additional Controls	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: Column 1: Language screened school starters in Aarhus Municipality 2006-2010, who are eligible for forced busing (described in Notes to Table A.4). Column 2: Sample described in the notes of Table A.3 restricted to pupils who attend the school they are assigned to at the time of the National test. Column 3: Sample described in the notes of Table A.3 restricted to pupils who are the first to take the language test in their family. Columns 4 and 5: Sample described in the notes of Table A.3. Pooled dataset. Outcome: 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 the test subject. We control for the school assignment determinants according to the specifications described in Table A.4. Determinants include 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, additional individual and family characteristics. Refer to notes under Table A.3 for a list of the additional controls.

Table A.10: Effect of Assignment to Busing on Wellbeing Survey Factors: School Satisfaction and Distress. Robustness Checks.

	Dependent variable:							
	School satisfaction				Distress			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: grade 0-3								
Explanatory variable:								
Assigned to busing	-0.096	-0.003	-0.142	-0.146	0.250*	0.247*	0.259*	-0.027
	(0.122)	(0.127)	(0.135)	(0.171)	(0.132)	(0.145)	(0.134)	(0.173)
Observations	1,060	1,060	1,060	1,060	1,060	1,060	1,060	1,060
Panel B: grade 4-9								
Explanatory variables:								
Assigned to busing	0.016	-0.013	-0.039	0.022	-0.035	0.046	-0.066	-0.052
	(0.111)	(0.110)	(0.117)	(0.137)	(0.093)	(0.100)	(0.099)	(0.134)
Observations	1,759	1,759	1,759	1,759	1,759	1,759	1,759	1,759
Assignment determinants specification	Alternative 2	Full school district interaction	Full test cohort interaction	Fully interacted	Alternative 2	Full school district interaction	Full test cohort interaction	Fully interacted
Additional controls	YES	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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Refer to notes under Table A.3 for sample description. Pooled dataset. Outcome: Standardized factors for school satisfaction (1, 2) and distress (3, 4) in grades 0-3 (Panel A) and grades 4-9 (Panel B). OLS of the outcome on a dummy for assignment to busing. We control for the school assignment determinants according to the specifications described in Table A.4. Determinants include 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, additional individual and family characteristics. Refer to notes under Table A.3 for a list of the additional controls.

Online Appendix for Academic Achievement and Wellbeing of Dual Language Learners: Evidence from a Busing Program

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Table B.1: 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).
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).
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 B.1: Definitions and Data Sources of Variables. (continued)

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.
Panel E: School district of residence and school characteristics		
Share of potential school starters and their family members who are immigrants or descendants	Share of potential school starters, their parents and siblings living in the school district with non-Danish origin or descent. Potential school starters are children who turn 6 during the calendar year.	Population register and income register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Employment rate	Share of employed parents and older siblings (age 25 to 54) of potential school starters living in the school district.	Population register and employment register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Share with a tertiary education	Share of tertiary educated parents and older siblings (age 25 to 54) of potential school starters living in the school district.	Population register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Avg. real disposable income	Average annual real disposable income in USD, of parents and older siblings (age 25 to 54) of potential school starters living in the school district.	Population register and employment register, Statistics Denmark (DST). Pupil register, Aarhus Municipality.
Panel F: School district of residence and school characteristics		
Share of potential school starters who enroll in the district school	Share of all pupils of school starting age living in the district who enrolls in the district school. Pupils of school starting age are all children who turn 6 during the calendar 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 dual language learners	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 category-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, defined as the share of teachers at different ages.	"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 dual language learners, 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, Aarhus 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, Aarhus Municipality.

Table B.2: Availability of Education Data, by Test Cohort.

		Academic year												
		2006/2007	2007/2008	2008/2009	2009/2010	2010/2011	2011/2012	2012/2013	2013/2014	2014/2015	2015/2016	2016/2017	2017/2018	2018/2019
Cohort	2006	0	1	2	**3**	**4**	**5**	**6**	**7**	**8**	9			
	2007		0	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	9		
	2008			0	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	9	
	2009				0	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	9
	2010					0	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
	2011						0	1	**2**	**3**	**4**	**5**	**6**	**7**
	2012							0	1	**2**	**3**	**4**	**5**	**6**
	2013								0	1	**2**	**3**	**4**	**5**
	2014									0	1	**2**	**3**	**4**
	2015										0	1	**2**	**3**
	2016											0	1	**2**

Note: Cohorts indicate the year of school start in August and are equivalent to language test cohorts for pupils who do not delay school start. The table reports the predicted grade if the pupil follows a standard school career, i.e. starts in grade 0 in August in the year of the language test and is not retained or delayed. Bold letters indicate availability of national test data, whereas the light grey area indicate availability of the national wellbeing survey.

Table B.3: 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, sådu 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 påskolen 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 B.4: Wellbeing Survey Questions for Pupils in Grades 4–9.

	Original Question	English translation	1	2	3	4	5
1	Er du glad for din skole?	Are you happy with your school?	Never	Rarely	Once in a while	Often	Very often
2	Er du glad for din klasse?	Are you happy with your class?	Never	Rarely	Once in a while	Often	Very often
3	Jeg prøver at forståmine venner, når de er triste eller sure.	I try to understand my friends' feelings when they are sad or angry.	Never	Rarely	Once in a while	Often	Very often
4	Jeg er god til at arbejde sammen med andre.	I am good at collaborating with others.	Never	Rarely	Once in a while	Often	Very often
5	Jeg siger min mening, når jeg synes, at noget er uretfærdigt.	I speak my mind when I think something is unfair.	Never	Rarely	Once in a while	Often	Very often
6	Hvor tit kan du finde en løsning påproblemer, bare du prøver hårdt nok?	How often can you find a solution to problems if you just try hard enough?	Never	Rarely	Once in a while	Often	Very often
7	Hvor tit kan du klare det, du sætter dig for?	How often can you complete what you decide to do?	Never	Rarely	Once in a while	Often	Very often
8	Kan du koncentrere dig i timerne?	Can you concentrate during lessons?	Never	Rarely	Once in a while	Often	Very often
9	Føler du dig ensom?	Do you feel lonely?	Never	Rarely	Once in a while	Often	Very often
10	Hvor tit har du ondt i maven?	How often does your stomach hurt?	Very often	Often	Once in a while	Rarely	Never
11	Hvor tit har du ondt i hovedet?	How often does your head hurt?	Very often	Often	Once in a while	Rarely	Never
12	Er du bange for at blive til grin i skolen?	Are you afraid to be laughed at at school?	Always	Most of the times	Once in a while	Rarely	Never
13	Hvor ofte føler du dig tryg i skolen?	How often do you feel safe at school?	Always	Most of the times	Once in a while	Rarely	Never
14	Er du blevet mobbet i dette skoleår?	Have you been bullied this school year?	Very often	Often	Once in a while	Rarely	Never
15	Har du selv mobbet nogen i skolen i dette skoleår?	Have you bullied anyone this school year?	Very often	Often	Once in a while	Rarely	Never
16	Er du og dine klassekammerater med til at bestemme, hvad I skal arbejde med i klassen?	Do you and your classmates have any say in what you work on in class?	Never	Rarely	Once in a while	Often	Very often
17	Hvis jeg bliver forstyrret i undervisningen, kan jeg hurtigt koncentrere mig igen.	If get distracted during lessons, I can quickly concentrate again.	Never	Rarely	Once in a while	Often	Very often
18	Hvis der er larm i klassen, kan lærerne hurtigt fåskabt ro.	If there is noise in the classroom, theachers can quickly re-establish silence.	Never	Rarely	Once in a while	Often	Very often
19	Er undervisningen kedelig?	Are the lessons boring?	Very often	Often	Once in a while	Rarely	Never
20	Er undervisningen spændende?	Are the lessons exciting?	Never	Rarely	Once in a while	Often	Very often
21	Hvis jeg keder mig i undervisningen, kan jeg selv gøre noget for, at det bliver spændende.	If I am bored during the lessons, I can do something about it myself to make it exciting.	Never	Rarely	Once in a while	Often	Very often
22	Hvis noget er for svært for mig i undervisningen, kan jeg selv gøre noget for at komme videre.	If something is too difficult for me during class, I myself can do something to move on.	Never	Rarely	Once in a while	Often	Very often
23	Møder dine lærere præcist til undervisningen?	Do your teachers show up for classes on time?	Never	Rarely	Once in a while	Often	Very often
24	Er det let at høre, hvad læreren siger i timerne?	Is it easy to hear what the teachers say during lessons?	Never	Rarely	Once in a while	Often	Very often
25	Er det let at høre, hvad de andre elever siger i timerne?	Is it easy to hear what the other pupils say during lessons?	Never	Rarely	Once in a while	Often	Very often
26	Lykkes det for dig at lære dt, du gerne vil, i skolen?	Do you succeed in learning what you set out to in school?	Never	Rarely	Once in a while	Often	Very often
27	Hjælper dine lærere dig med at lære påmåder, som virker godt?	Do your teachers help you learn in ways that work?	Never	Rarely	Once in a while	Often	Very often
28	Hvad synes dine lærere om dine fremskridt i skolen?	What do your teachers think of your progress in school?	Below average	Average	Good	Really good	
29	Jeg klarer mig godt fagligt i skolen.	I am doing well academically in school.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
30	Jeg gør gode faglige fremskridt i skolen.	I am making good academic progress in school.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
31	Undervisningen giver mig lyst til at lære mere.	Lessons make me want to learn more.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
32	Lærerne er gode til at støtte mig og hjælpe mig i skolen, når jeg har brug for det.	The teachers are good at supporting and helping me at school when I need it.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
33	Jeg føler, at jeg hører til påmin skole.	I feel that I belong at my school.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
34	Jeg kan godt lide pauserne i skolen.	I like the breaks at school.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
35	De fleste af eleverne i min klasse er venlige og hjælpsomme.	Most of the pupils in my class are friendly and helpful.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
36	Andre elever accepterer mig, som jeg er.	Other pupils accept me for who I am.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
37	Lærerne sørger for, at elevernes ideer bliver brugt i undervisningen.	The teachers ensure that the pupils' ideas are being used in the lessons.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
38	Jeg synes godt om udeområderne påmin skole.	I like the surroundings outside my school/the schoolyard at my school.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
39	Jeg synes godt om undervisningslokalerne påskolen.	I like the classrooms at my school.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
40	Jeg synes, toiletterne påskolen er pæne og rene.	I think the toilets at my school are nice and clean.	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree

Source: Danish Wellbeing Survey questionnaire to public school pupils in grades 4-9, 2015-2018.

Table B.5: Factor Loadings on School Satisfaction and Distress, Grade 4–9 Survey.

School Satisfaction	
Survey Question	Factor loading
Are the lessons exciting?	1
Do your teachers help you learn in ways that work?	0.974
The teachers are good at supporting and helping me at school when I need it.	0.986
Are you happy with your school?	0.881
Are the lessons boring?	0.981
I like the classrooms at my school.	0.812
Are you happy with your class?	0.659
Distress	
Survey Question	Factor loading
I feel that I belong at my school.	1
Other pupils accept me for who I am.	0.992
How often do you feel safe at school?	0.873
Do you feel lonely?	0.856
Most of the pupils in my class are friendly and helpful.	0.852
Are you afraid to be laughed at at school?	0.813
Have you been bullied this school year?	0.584
How often does your stomach hurt?	0.601
How often does your head hurt?	0.612
Is it easy to hear what the teachers say during lessons?	0.447

Source: Danish Wellbeing Survey of all public school pupils in grades 4-9, waves 2015-2019.
Notes: Factor loadings based on exploratory and confirmatory factor analysis on the grade 0-3 survey. We run the confirmatory factor analysis of these two factors using the items with most similarity to the grade 0-3 survey and controls for year of the survey, grade, age, and sex. See Table B.4 for the coding of answer categories to each survey question.

Table B.6: 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	0.000	0.178*	0.161	-0.025	0.046	0.033	0.054
	(0.088)	(0.102)	(0.106)	(0.104)	(0.102)	(0.103)	(0.101)
Age difference with youngest sibling in district school	0.031**	-0.029	-0.010	0.006	0.003	0.008	0.004
	(0.015)	(0.018)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)
Distance from the district school	-0.038	0.028	0.026	0.106*	0.105*	0.119**	0.123**
	(0.052)	(0.060)	(0.062)	(0.061)	(0.060)	(0.060)	(0.060)
Age on language test day	-0.035	0.030	-0.018	-0.029	0.013	0.028	-0.020
	(0.050)	(0.058)	(0.060)	(0.058)	(0.058)	(0.058)	(0.057)
Medium language support need	-0.039	-0.073	-0.058	-0.047	0.022	-0.017	0.050
	(0.061)	(0.072)	(0.074)	(0.073)	(0.074)	(0.074)	(0.073)
Low language support need	-0.005	-0.035	-0.032	-0.030	0.010	0.008	0.037
	(0.063)	(0.074)	(0.076)	(0.075)	(0.077)	(0.078)	(0.078)
Male	0.078*	0.082*	0.058	0.011	-0.020	0.056	0.047
	(0.041)	(0.048)	(0.049)	(0.048)	(0.048)	(0.049)	(0.048)
Immigrant	0.225**	0.079	0.195*	0.121	0.099	0.086	0.171
	(0.096)	(0.112)	(0.116)	(0.116)	(0.114)	(0.116)	(0.120)
Origin or descent: Africa	0.065	-0.111*	-0.133**	-0.153***	-0.156***	-0.122**	-0.104*
	(0.050)	(0.058)	(0.060)	(0.059)	(0.060)	(0.060)	(0.060)
Origin or descent: Western countries[1]	0.070	-0.010	-0.126	-0.215**	-0.211**	-0.157*	-0.187**
	(0.070)	(0.082)	(0.085)	(0.085)	(0.085)	(0.088)	(0.088)
Origin or descent: East Asia[2]	0.180**	0.132	-0.003	-0.028	-0.060	-0.023	-0.005
	(0.081)	(0.094)	(0.097)	(0.094)	(0.094)	(0.093)	(0.092)
Attended daycare	-0.051	-0.014	0.238	0.145	0.063	0.013	-0.042
	(0.134)	(0.156)	(0.161)	(0.165)	(0.162)	(0.161)	(0.157)
Number of siblings: 1	0.159	0.071	0.080	0.157	0.173	0.046	-0.043
	(0.138)	(0.161)	(0.166)	(0.167)	(0.165)	(0.171)	(0.186)
Number of siblings: 2	0.185	0.150	0.131	0.233	0.168	0.023	-0.049
	(0.139)	(0.161)	(0.166)	(0.167)	(0.165)	(0.170)	(0.185)
Number of siblings: 3	0.219	0.214	0.159	0.216	0.194	0.115	0.015
	(0.140)	(0.163)	(0.168)	(0.168)	(0.166)	(0.171)	(0.184)
Number of siblings: 4	0.238	0.141	0.115	0.235	0.205	0.141	0.054
	(0.145)	(0.169)	(0.174)	(0.176)	(0.175)	(0.180)	(0.193)
Number of siblings: 5	0.158	0.149	0.070	0.159	0.138	0.064	-0.057
	(0.151)	(0.176)	(0.181)	(0.181)	(0.179)	(0.184)	(0.198)
Number of siblings: 6	0.156	0.228	0.162	0.194	0.036	-0.106	-0.179
	(0.167)	(0.194)	(0.200)	(0.199)	(0.197)	(0.202)	(0.213)
Number of siblings: 7 or more	0.196	0.102	0.152	0.182	0.152	0.074	-0.050
	(0.156)	(0.181)	(0.187)	(0.187)	(0.186)	(0.191)	(0.205)
No mother recorded in register datasets	0.097	0.500**	0.397*	0.286	0.265	0.229	0.450**
	(0.176)	(0.205)	(0.212)	(0.216)	(0.214)	(0.212)	(0.221)
No father recorded in register datasets	0.065	0.065	0.141	0.174	0.242	0.132	0.056
	(0.132)	(0.154)	(0.159)	(0.156)	(0.159)	(0.166)	(0.166)
Living in two-parent household	-0.066	-0.030	-0.035	0.032	0.028	0.063	0.065
	(0.052)	(0.060)	(0.062)	(0.061)	(0.062)	(0.062)	(0.061)
Mother's age: under 25	-0.108	-0.031	-0.029	-0.109	-0.114	-0.141	-0.220*
	(0.102)	(0.118)	(0.122)	(0.120)	(0.121)	(0.121)	(0.122)
Mother's age: 25 to 29	-0.101	0.009	0.031	-0.111	-0.156	-0.185*	-0.209**
	(0.086)	(0.100)	(0.104)	(0.101)	(0.102)	(0.102)	(0.102)
Mother's age: 30 to 34	-0.059	0.023	-0.007	-0.151	-0.187**	-0.236**	-0.268***
	(0.080)	(0.093)	(0.096)	(0.094)	(0.095)	(0.095)	(0.097)
Mother's age: 35 to 39	-0.069	0.070	0.087	0.008	-0.040	-0.109	-0.159*
	(0.079)	(0.092)	(0.095)	(0.093)	(0.094)	(0.095)	(0.095)
Mother: high school dropout	0.035	0.021	0.019	-0.045	-0.037	-0.047	-0.030
	(0.056)	(0.065)	(0.067)	(0.066)	(0.066)	(0.066)	(0.065)
Mother: education not reported	0.047	0.001	0.004	0.026	0.036	0.054	0.015
	(0.068)	(0.079)	(0.081)	(0.080)	(0.081)	(0.081)	(0.081)
Mother: completed university	-0.010	-0.025	-0.031	-0.098	-0.076	-0.067	-0.101
	(0.063)	(0.074)	(0.076)	(0.075)	(0.075)	(0.076)	(0.075)
Mother: employed (includes self-employed)	0.049	0.039	0.023	-0.007	-0.033	0.027	0.002
	(0.054)	(0.063)	(0.065)	(0.064)	(0.064)	(0.064)	(0.064)

(continued)

Table B.6: Determinants of Compliance with the Policy, Treatment Group (Assigned to Busing). (continued)

Mother: unemployed	0.125*	0.271***	0.170**	0.182**	0.076	0.152*	0.086
	(0.071)	(0.083)	(0.086)	(0.084)	(0.087)	(0.088)	(0.087)
Mother's disposable income: second quartile	-0.004	-0.054	-0.001	-0.019	0.021	-0.010	0.051
	(0.067)	(0.078)	(0.080)	(0.078)	(0.079)	(0.081)	(0.082)
Mother's disposable income: third quartile	-0.030	-0.027	0.041	-0.017	-0.041	-0.002	0.043
	(0.069)	(0.080)	(0.082)	(0.081)	(0.083)	(0.084)	(0.086)
Mother's disposable income: fourth quartile	-0.015	-0.011	-0.016	0.057	0.082	0.086	0.114
	(0.078)	(0.091)	(0.094)	(0.092)	(0.092)	(0.093)	(0.096)
Father's age: under 25	-0.015	0.102	-0.039	-0.016	-0.050	0.029	0.076
	(0.112)	(0.131)	(0.135)	(0.131)	(0.134)	(0.137)	(0.141)
Father's age: 25 to 29	-0.032	0.010	0.002	-0.013	0.048	0.026	0.071
	(0.077)	(0.090)	(0.092)	(0.091)	(0.090)	(0.090)	(0.089)
Father's age: 30 to 34	0.027	0.060	0.022	0.078	0.082	0.033	0.054
	(0.066)	(0.077)	(0.079)	(0.078)	(0.079)	(0.080)	(0.078)
Father's age: 35 to 39	0.027	-0.010	-0.007	0.063	0.007	-0.015	0.057
	(0.057)	(0.066)	(0.069)	(0.067)	(0.067)	(0.068)	(0.068)
Father: high school dropout	0.008	-0.110*	-0.080	-0.104	-0.118*	-0.102	-0.105*
	(0.054)	(0.062)	(0.064)	(0.063)	(0.063)	(0.063)	(0.064)
Father: education not reported	0.046	-0.069	-0.054	-0.045	-0.059	-0.134*	-0.118*
	(0.058)	(0.068)	(0.070)	(0.069)	(0.069)	(0.070)	(0.070)
Father: completed university	0.041	0.060	0.022	0.032	-0.037	-0.068	-0.125*
	(0.062)	(0.073)	(0.075)	(0.073)	(0.073)	(0.073)	(0.072)
Father: employed (includes self-employed)	-0.068	-0.123**	-0.175***	-0.164***	-0.174***	-0.175***	-0.202***
	(0.051)	(0.059)	(0.061)	(0.060)	(0.060)	(0.062)	(0.062)
Father: unemployed	0.016	-0.069	-0.063	0.036	-0.004	-0.043	-0.021
	(0.071)	(0.083)	(0.086)	(0.083)	(0.083)	(0.084)	(0.083)
Father's disposable income: second quartile	-0.013	-0.015	-0.028	-0.021	-0.014	-0.037	-0.055
	(0.051)	(0.060)	(0.061)	(0.060)	(0.060)	(0.061)	(0.061)
Father's disposable income: third quartile	0.029	-0.060	-0.002	-0.088	-0.037	-0.043	-0.083
	(0.066)	(0.077)	(0.080)	(0.078)	(0.078)	(0.079)	(0.079)
Father's disposable income: fourth quartile	-0.028	-0.053	-0.018	-0.041	0.007	0.029	0.020
	(0.075)	(0.088)	(0.091)	(0.089)	(0.089)	(0.089)	(0.090)
R^2	0.380	0.227	0.220	0.270	0.279	0.260	0.276
Observations	515	515	515	499	478	455	430
Additional controls	YES	YES	YES	YES	YES	YES	YES
F-test joint insignificance for additional controls	1.126	1.070	0.881	1.301	1.304	1.331	1.461
P-value F-test	0.271	0.356	0.694	0.0976	0.0964	0.0813	0.0324

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 2006-2016, 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 school districts closed in 2008, and who do not move to Aarhus between January and school start in 2016. Sample size 999 individuals. 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 2006-2016. 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 B.7: 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	0.230***	0.211***	0.152**	0.185***	0.164**	0.137	0.084
	(0.044)	(0.057)	(0.061)	(0.070)	(0.079)	(0.095)	(0.114)
Age difference with youngest sibling in district school	-0.014*	-0.015	-0.006	-0.002	-0.001	0.001	0.006
	(0.008)	(0.010)	(0.011)	(0.013)	(0.015)	(0.017)	(0.018)
Distance from the district school	-0.029	0.001	-0.000	-0.003	0.062	0.088	0.091
	(0.020)	(0.026)	(0.028)	(0.046)	(0.049)	(0.059)	(0.065)
Age on language test day	0.036	0.122**	0.189***	0.114*	0.115	0.011	0.126
	(0.041)	(0.053)	(0.057)	(0.065)	(0.072)	(0.086)	(0.100)
Medium language support need	0.131***	0.158***	0.164**	0.178**	0.159*	0.065	0.065
	(0.047)	(0.060)	(0.066)	(0.077)	(0.086)	(0.107)	(0.121)
Low language support need	0.164***	0.198***	0.190***	0.228***	0.191**	0.146	0.178
	(0.046)	(0.060)	(0.065)	(0.076)	(0.084)	(0.102)	(0.116)
Male	-0.098***	-0.077**	-0.057	-0.100**	-0.077	-0.062	0.033
	(0.029)	(0.038)	(0.041)	(0.046)	(0.052)	(0.062)	(0.069)
Immigrant	0.010	-0.037	-0.042	-0.065	-0.182*	-0.192	-0.153
	(0.055)	(0.071)	(0.077)	(0.092)	(0.105)	(0.136)	(0.144)
Origin or descent: Africa	-0.056	-0.038	-0.039	-0.032	-0.011	-0.023	0.126
	(0.041)	(0.053)	(0.058)	(0.064)	(0.073)	(0.089)	(0.103)
Origin or descent: Western countries[1]	-0.059	-0.019	-0.064	-0.222***	-0.233***	-0.273***	-0.206*
	(0.047)	(0.061)	(0.066)	(0.079)	(0.085)	(0.100)	(0.108)
Origin or descent: East Asia[2]	-0.093*	0.004	0.052	0.043	0.068	0.015	0.045
	(0.051)	(0.065)	(0.071)	(0.077)	(0.084)	(0.099)	(0.112)
Attended daycare	-0.006	0.149	-0.024	-0.264	-0.229	-0.526	-0.721
	(0.102)	(0.132)	(0.143)	(0.170)	(0.220)	(0.373)	(0.509)
Number of siblings: 1	0.173*	0.133	0.146	0.164	0.028	-0.389	0.031
	(0.097)	(0.125)	(0.135)	(0.189)	(0.213)	(0.367)	(0.370)
Number of siblings: 2	0.108	0.008	0.017	0.021	-0.062	-0.414	0.004
	(0.096)	(0.124)	(0.135)	(0.185)	(0.211)	(0.360)	(0.359)
Number of siblings: 3	0.090	0.036	0.124	0.175	-0.011	-0.419	0.058
	(0.102)	(0.131)	(0.142)	(0.194)	(0.222)	(0.366)	(0.367)
Number of siblings: 4	0.100	0.034	0.128	0.137	-0.012	-0.445	0.020
	(0.106)	(0.136)	(0.147)	(0.198)	(0.228)	(0.372)	(0.374)
Number of siblings: 5	-0.020	-0.095	-0.022	-0.067	-0.230	-0.646*	-0.281
	(0.115)	(0.149)	(0.161)	(0.209)	(0.238)	(0.390)	(0.393)
Number of siblings: 6	0.052	0.002	-0.020	-0.030	-0.349	-0.702*	-0.217
	(0.114)	(0.147)	(0.159)	(0.206)	(0.232)	(0.377)	(0.380)
Number of siblings: 7 or more	0.073	0.019	0.083	0.063	-0.108	-0.401	0.026
	(0.112)	(0.145)	(0.156)	(0.207)	(0.237)	(0.382)	(0.394)
No mother recorded in register datasets	-0.156	-0.001	0.232	0.254	0.242	0.205	0.355
	(0.124)	(0.160)	(0.173)	(0.194)	(0.207)	(0.246)	(0.277)
No father recorded in register datasets	0.094	0.027	-0.023	0.082	0.200	0.038	0.091
	(0.102)	(0.132)	(0.142)	(0.153)	(0.186)	(0.252)	(0.280)
Living in two-parent household	0.002	0.023	0.055	0.026	-0.010	-0.046	0.006
	(0.035)	(0.045)	(0.049)	(0.055)	(0.061)	(0.073)	(0.082)
Mother's age: under 25	0.064	-0.032	-0.111	-0.020	-0.090	-0.147	-0.198
	(0.074)	(0.095)	(0.103)	(0.117)	(0.136)	(0.176)	(0.188)
Mother's age: 25 to 29	-0.006	-0.057	-0.072	-0.036	-0.037	-0.023	-0.009
	(0.062)	(0.079)	(0.086)	(0.095)	(0.104)	(0.124)	(0.133)
Mother's age: 30 to 34	-0.044	-0.057	-0.059	-0.026	-0.040	-0.163	-0.158
	(0.053)	(0.069)	(0.074)	(0.081)	(0.092)	(0.111)	(0.119)
Mother's age: 35 to 39	-0.007	-0.068	-0.037	-0.040	-0.055	-0.137	-0.092
	(0.048)	(0.062)	(0.067)	(0.073)	(0.084)	(0.102)	(0.111)
Mother: high school dropout	-0.046	-0.031	-0.074	-0.067	-0.106	-0.092	-0.062
	(0.041)	(0.053)	(0.057)	(0.064)	(0.072)	(0.083)	(0.094)
Mother: education not reported	-0.022	-0.035	-0.083	-0.143**	-0.123	-0.057	-0.038
	(0.044)	(0.056)	(0.061)	(0.072)	(0.082)	(0.094)	(0.107)
Mother: completed university	0.019	0.011	-0.072	-0.103	-0.130	-0.115	-0.227**
	(0.045)	(0.059)	(0.064)	(0.071)	(0.080)	(0.094)	(0.108)
Mother: employed (includes self-employed)	0.002	-0.022	0.022	0.019	-0.037	0.002	0.039
	(0.040)	(0.051)	(0.055)	(0.063)	(0.071)	(0.084)	(0.098)

(continued)

Table B.7: Determinants of Compliance with the Policy, Control Group (Assigned to District School). (continued)

Mother: unemployed	-0.031	-0.087	-0.031	0.069	0.092	0.060	0.192
	(0.067)	(0.086)	(0.093)	(0.112)	(0.132)	(0.160)	(0.186)
Mother's disposable income: second quartile	0.003	0.042	0.055	0.029	-0.022	-0.101	-0.113
	(0.048)	(0.062)	(0.067)	(0.079)	(0.089)	(0.110)	(0.122)
Mother's disposable income: third quartile	0.024	0.081	0.100	0.136*	0.069	-0.029	0.028
	(0.046)	(0.059)	(0.064)	(0.075)	(0.085)	(0.106)	(0.116)
Mother's disposable income: fourth quartile	0.043	0.097	0.097	0.080	-0.035	-0.184	-0.105
	(0.055)	(0.071)	(0.076)	(0.087)	(0.097)	(0.119)	(0.130)
Father's age: under 25	0.099	-0.035	0.161	0.151	0.122	0.223	0.128
	(0.093)	(0.120)	(0.129)	(0.142)	(0.163)	(0.198)	(0.224)
Father's age: 25 to 29	-0.135**	-0.059	0.011	-0.008	-0.017	-0.167	-0.055
	(0.068)	(0.088)	(0.095)	(0.111)	(0.121)	(0.140)	(0.156)
Father's age: 30 to 34	-0.084*	-0.056	0.011	0.057	0.004	-0.018	0.026
	(0.050)	(0.064)	(0.070)	(0.079)	(0.087)	(0.103)	(0.114)
Father's age: 35 to 39	-0.009	0.024	0.003	-0.006	-0.068	-0.046	-0.086
	(0.041)	(0.053)	(0.058)	(0.065)	(0.073)	(0.085)	(0.091)
Father: high school dropout	0.018	0.063	0.067	0.118*	0.201***	0.183**	0.135
	(0.041)	(0.053)	(0.057)	(0.066)	(0.077)	(0.092)	(0.107)
Father: education not reported	0.010	0.056	0.010	-0.014	0.001	-0.010	0.046
	(0.044)	(0.057)	(0.061)	(0.069)	(0.077)	(0.091)	(0.101)
Father: completed university	0.021	0.077	0.056	0.127*	0.163**	0.176*	0.258**
	(0.043)	(0.055)	(0.060)	(0.069)	(0.078)	(0.093)	(0.108)
Father: employed (includes self-employed)	-0.026	-0.043	-0.032	-0.001	0.052	-0.044	-0.009
	(0.038)	(0.049)	(0.053)	(0.060)	(0.066)	(0.081)	(0.089)
Father: unemployed	0.012	-0.049	0.054	0.128	0.104	0.075	-0.073
	(0.060)	(0.078)	(0.086)	(0.099)	(0.120)	(0.143)	(0.168)
Father's disposable income: second quartile	0.029	0.031	0.029	-0.018	0.040	0.125	0.157
	(0.040)	(0.052)	(0.057)	(0.064)	(0.072)	(0.086)	(0.095)
Father's disposable income: third quartile	0.097**	0.012	0.051	0.011	0.025	0.086	0.051
	(0.046)	(0.059)	(0.064)	(0.074)	(0.083)	(0.101)	(0.112)
Father's disposable income: fourth quartile	0.055	0.061	0.092	0.041	-0.014	0.027	0.073
	(0.051)	(0.066)	(0.072)	(0.081)	(0.090)	(0.106)	(0.113)
R^2	0.391	0.306	0.295	0.384	0.418	0.451	0.543
Observations	484	484	48x	414	352	279	223
Additional controls	YES	YES	YES	YES	YES	YES	YES
F-test joint insignificance for additional controls	2.731	1.696	1.560	2.129	1.898	1.402	1.508
P-value F-test	8.93e-08	0.00446	0.0144	8.61e-05	0.00100	0.0609	0.0356

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Refer to notes under Table B.6 for sample description. Sample size 999 individuals. 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 2006-2016. 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 B.8: Effects of Assignment to Busing on Responses to Wellbeing Survey 0–3.

	Explanatory variable: Assigned to busing	
	(1)	(2)
Dependent variable:		
Do you learn anything exciting in school?	-0.205*	-0.221*
	(0.116)	(0.114)
Are your classrooms nice to be in?	-0.157	-0.095
	(0.116)	(0.116)
Are lessons boring?	0.038	0.069
	(0.124)	(0.124)
Are you happy with your school?	0.028	0.037
	(0.107)	(0.106)
Are teachers good at helping you in school?	-0.174*	-0.173
	(0.102)	(0.108)
Are you happy with your class?	0.095	0.144
	(0.109)	(0.111)
Are you happy with your teachers?	-0.102	-0.093
	(0.117)	(0.118)
Is there someone who teases you, so that you get upset?	0.050	0.079
	(0.147)	(0.138)
Do you have stomachache, when you are in school?	-0.254**	-0.271**
	(0.128)	(0.131)
Do you have headache, when you are in school?	-0.249*	-0.186
	(0.129)	(0.117)
Are you afraid that the other kids laugh at you in school?	-0.050	-0.098
	(0.113)	(0.108)
Do you feel alone in school?	-0.137	-0.161
	(0.138)	(0.131)
Is it difficult to hear what the teacher says during lessons?	-0.349***	-0.396***
	(0.122)	(0.125)
Do you like the breaks at school?	-0.108	-0.149
	(0.123)	(0.129)
Are you good at solving your problems?	-0.160	-0.091
	(0.122)	(0.125)
Can you concentrate during lessons?	-0.200*	-0.133
	(0.117)	(0.116)
Are you good at helping each other in class?	-0.038	-0.014
	(0.112)	(0.115)
Do you think that the other kids in class like you?	-0.191	-0.069
	(0.138)	(0.144)
Do you help decide what you do during lessons?	-0.187	-0.162
	(0.141)	(0.145)
Are toilets in school clean?	-0.003	0.069
	(0.100)	(0.101)
Observations[1]	1,060	1,060
Additional Controls	NO	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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Refer to notes under Table B.6 for sample description. Sample size 999 individuals. Outcome: Standardized responses to wellbeing survey questions. Every cell shows the coefficient of an OLS of the outcome on a dummy for being bused. 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, individual and family characteristics. Individual characteristics of the child include gender, immigration status (immigrant or descendant), area of origin (Africa; Europe, Australia, New Zealand, Canada and USA; East Asia; 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). See Table B.3 for the coding of answer categories to each survey question. Note that for every question the higher the coefficient, the happier the child.

[1] Maximum number of observations, most questions have missing responses.

Table B.9: Effects of Assignment to Busing on Responses to Wellbeing Survey 4–9.

	Explanatory variable: Assigned to busing	
	(1)	(2)
Dependent variable:		
Are you happy with your school?	0.049	0.050
	(0.124)	(0.122)
Are you happy with your class?	0.219*	0.221*
	(0.128)	(0.128)
I try to understand my friends' feelings when they are sad or angry.	0.028	-0.074
	(0.133)	(0.129)
I am good at collaborating with others.	0.023	-0.028
	(0.122)	(0.129)
I speak my mind when I think something is unfair.	-0.005	-0.033
	(0.131)	(0.126)
How often can you find a solution to problems if you just try hard enough?	-0.024	-0.004
	(0.128)	(0.129)
How often can you complete what you decide to do?	0.024	0.045
	(0.113)	(0.110)
Can you concentrate during lessons?	0.111	0.127
	(0.121)	(0.120)
Do you feel lonely? (reversed)	0.019	-0.002
	(0.112)	(0.115)
How often does your stomach hurt? (reversed)	0.059	0.036
	(0.128)	(0.121)
How often does your head hurt? (reversed)	0.065	0.049
	(0.116)	(0.116)
Are you afraid to be laughed at at school? (reversed)	-0.052	-0.074
	(0.125)	(0.126)
How often do you feel safe at school?	0.212	0.171
	(0.137)	(0.137)
Have you been bullied this school year? (reversed)	0.079	0.103
	(0.152)	(0.158)
Have you bullied anyone this school year? (reversed)	0.258	0.284
	(0.195)	(0.193)
Do you and your classmates have any say in what you work on in class?	0.170	0.210
	(0.132)	(0.135)
If get distracted during lessons, I can quickly concentrate again.	0.039	0.041
	(0.107)	(0.112)
If there is noise in the classroom, teachers can quickly re-establish silence.	0.082	0.130
	(0.125)	(0.126)
Are the lessons boring? (reversed)	0.089	0.092
	(0.137)	(0.134)
Are the lessons exciting?	0.081	0.088
	(0.135)	(0.135)
If I am bored during the lessons, I can do something about it myself to make it exciting.	0.091	0.082
	(0.122)	(0.125)
If something is too difficult for me during class, I myself can do something to move on.	0.061	0.065
	(0.110)	(0.113)
Do your teachers show up for classes on time?	0.021	0.051
	(0.121)	(0.121)
Is it easy to hear what the teachers say during lessons?	0.259**	0.239*
	(0.125)	(0.125)
Is it easy to hear what the other pupils say during lessons?	-0.022	-0.005
	(0.112)	(0.113)
Do you succeed in learning what you set out to in school?	0.027	0.068
	(0.122)	(0.125)
Do your teachers help you learn in ways that work?	0.149	0.161
	(0.122)	(0.121)
What do your teachers think of your progress in school?	0.077	0.114
	(0.124)	(0.125)
I am doing well academically in school.	0.064	0.063
	(0.119)	(0.121)

(continued)

Table B.9: Effects of Assignment to Busing on Responses to Wellbeing Survey 4–9. (continued)

	Explanatory variable: Assigned to busing	
	(1)	(2)
Dependent variable:		
I am making good academic progress in school.	0.250**	0.239**
	(0.111)	(0.110)
Lessons make me want to learn more.	-0.162	-0.124
	(0.120)	(0.116)
The teachers are good at supporting and helping me at school when I need it.	0.215*	0.204
	(0.119)	(0.128)
I feel that I belong at my school.	0.031	0.004
	(0.117)	(0.117)
I like the breaks at school.	0.116	0.043
	(0.113)	(0.123)
Most of the pupils in my class are friendly and helpful.	0.135	0.161
	(0.130)	(0.133)
Other pupils accept me for who I am.	0.008	0.009
	(0.108)	(0.111)
The teachers ensure that the pupils' ideas are being used in the lessons.	0.013	0.048
	(0.132)	(0.134)
I like the surroundings outside my school/the schoolyard at my school.	-0.011	0.018
	(0.100)	(0.106)
I like the classrooms at my school.	0.073	0.121
	(0.113)	(0.118)
I think the toilets at my school are nice and clean.	-0.029	-0.015
	(0.084)	(0.086)
Observations[1]	1,091	1,091
Additional Controls	NO	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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Refer to notes under Table B.6 for sample description. Sample size 999 individuals. Outcome: Standardized responses to wellbeing survey questions. Every cell shows the coefficient of an OLS of the outcome on a dummy for being bused. 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, individual and family characteristics. Refer to notes under Table B.8 for a list of the additional controls. See Table B.4 for the coding of answer categories to each survey question. Note that for every question the higher the coefficient, the happier the child.

[1] Maximum number of observations, most questions have missing responses.

Appendix C. 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.

C1. 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

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

[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".

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 C1.

Table C1. 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 C2.

Table C2. 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).

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

[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.

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.

C2. 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 =

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

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 C1.a–C1.c around here]

Using the 2014 allocated school budgets to public schools in Aarhus, Figure C1.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 C1.b and C1.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.

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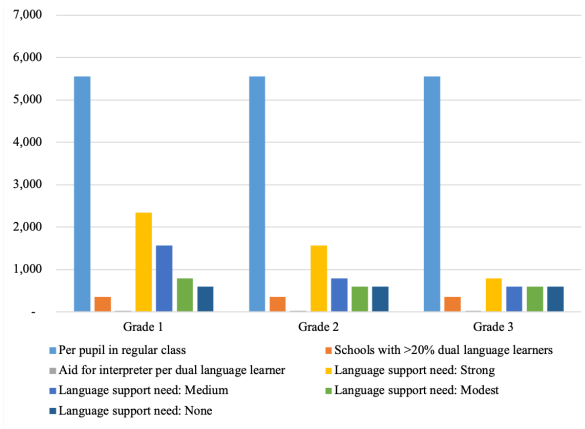
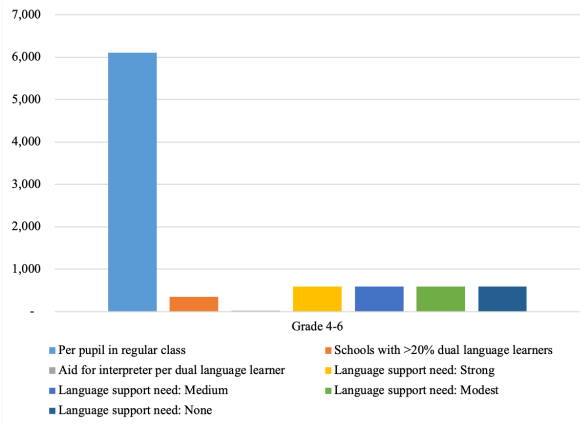
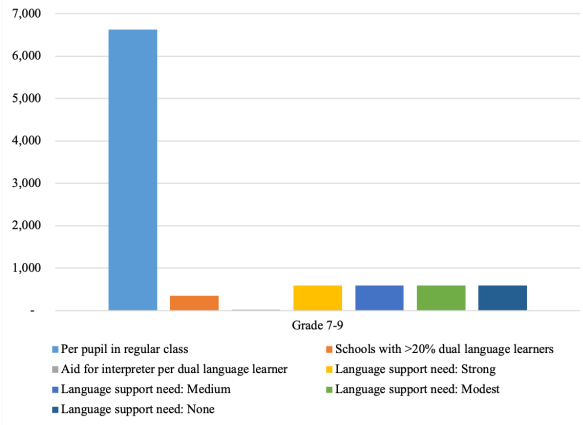
Figure C.1: Average Budget Items for Dual Language Learners in Regular Classes (USD). 2014. Non-magnet School with at Least 20% Dual Language Learners.



(a) Grades 1–3.



(b) Grades 4–6.



(c) Grades 7–9.

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

Notes: 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% dual language learners (column 2) is calculated as the budget for "task-specific resources" divided by the number of dual language learners. The budget for "aid for interpreter" (column 3) is calculated as the budget for "aid from interpreters" divided by the number of dual language learners. The additional budget for DAL support to each dual language learner in regular classes is shown in the last 4 columns for each category of dual language learners, depending on their language support need (according to the language screening test before school start). Exchange rate DKK/USD 0.1485 (base year: 2016).