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Who Benefits and Loses from Large Changes to Student Composition? Assessing Impacts of Lowering School Admissions Standards in Indonesia

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Abstract

We study the effects of an admission policy change that caused a massive shift in student composition in public and private junior secondary schools in Yogyakarta, Indonesia. In 2018, the primary criterion for admission into Yogyakarta's 16 preferred, free public schools (grades 7-9) changed from a grade 6 exam score ranking to a neighborhood-to-school distance ranking. This policy change resulted in a decline in average grade 6 scores in public schools by 0.4 standard deviations (s.d.) and a 0.4 s.d. increase in private schools. We assessed learning impacts caused by the changed student composition by comparing two otherwise similar cohorts of students admitted before and after the policy change. Average grade 8 test scores across math and Indonesian declined by 0.08 s.d. (not significant). To understand which students throughout the education system gained and lost in terms of learning, we simulated public school access under the 2018 policy and its predecessor for both cohorts. In public schools, teachers attempted to adapt lessons to lower-scoring students by changing teaching approaches and tracking students. These responses and/or exposure to different peers negatively affected learning for students predicted to have access to public schools under both policies (-0.13 s.d., significant at the 10 percent level) and aided students with predicted public school access under the new policy slightly (0.12 s.d., not significant). These results are in contrast to existing literature which finds little or no impact from shifts in student composition on incumbent students' learning. In private schools, we found no such adaptations and no effects on predicted incumbent students. However, students predicted to enter private schools under the new policy saw large negative effects (-0.24 s.d., significant), due to lower school quality and/or peer effects. Our results demonstrate that effects from high-performing, selective schools can be highly heterogenous and influenced by student composition.

JEL classification: H75, I2, O15

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1. Introduction

Altering selection criteria is one way to improve equity and fairness in selective school access. When education systems change admissions criteria and integrate students with different backgrounds or academic preparation into traditionally selective schools, student composition changes. Understanding the impacts of changing student composition is complex because large changes affect a wide range of students and schools. If seats at preferred schools are fixed, students will be displaced, and students who remain in preferred or less-preferred schools will be affected by teacher and peer responses to compositional changes. Throughout the education system, administrators, teachers, parents, and students may alter behaviors like teaching, classroom management, studying, and educational investments in response to changes to admissions criteria for even a subset of schools.

We study the impacts of an admissions policy change in public schools that had a massive effect on student composition in public and private schools in Yogyakarta, Indonesia. In 2018, the city adopted a policy called "zoning," to indicate residency-based rather than test-based admissions, for its 16 public junior secondary schools (grades 7-9). Zoning brought a large influx of lower-scoring students into high-performing, preferred public schools and displaced many higher-scoring students from public schools into lower-performing private schools. As a result of the policy, the share of students in public schools among those in the lowest quintile on the baseline grade 6 leaving exam rose from 14 to 59 percent, and the average grade 6 exam scores of public school students in grade 7 declined by 0.4 standard deviations (s.d.) in mathematics and in Indonesian. In private schools, we found the opposite – the equivalent metric increased by 0.4 (s.d.) in both subjects.

We examined which students throughout the education system gained and lost – in terms of learning – and the magnitude of these gains and losses. In addition, we studied the mechanisms for why impacts varied, such as differences in school quality and peer characteristics and behavior changes of teachers and students. We assessed impacts by comparing two cohorts of students who were admitted before and after the zoning policy. Both policies admitted students to public schools using a ranking on observable characteristics, primarily test scores or distance to school, allowing us to predict for which types of students public school access changed and remained the same. Using the 2017 and 2018 admissions criteria, grade 6 leaving exam data, students' residence, and students' background characteristics, we defined four groups: students whose simulated public school access would be the same under both policies ("always access" or "never access"), and students who would only have access to public schools under one of the policies ("gained access" and "lost access"). Because the cohort composition was similar under the two policies, the simulation allowed us to compare each student's learning between grade 6 and 8 to that of a group of students with similar characteristics under an alternate policy.

We found school enrollment by public school access group largely changed as predicted and that changes were substantial. Overall, 30 percent of public school students in the zoning cohort "gained" access while 29 percent of private school students "lost" access. For the "gained access" and "lost access" groups, we found a 47 to 60 percentage point difference in public school enrolment between policies. While public school enrolment did not differ between cohorts for the "always access" and "never access" students, the peers of these groups scored 0.36 s.d. lower and 0.28 s.d. higher on the grade 6 exam in the first zoning cohort than in the pre-zoning cohort, respectively.

Although the overall impact was small and negative (-0.08 s.d., not significant), we found that the compositional shifts caused by zoning had negative or neutral impacts on most students. Students who "lost access" to public schools experienced the greatest losses (0.24 s.d. averaged across math and

Indonesian, significant), which were greater than the gains for the students who "gained access" to public school, a modest and statistically insignificant improvement in learning (0.12 s.d.). The actual effect for the "gained access" students would have been 0.3 s.d higher if they learned as much in public schools as public school students did prior to the policy change. The "gained access" students likely learned less in public schools than previously enrolled students due to heterogeneous effects by initial student test scores. The "gained access" students had much lower initial scores than the "lost access" students (a difference of 1.4 s.d. on average).

Learning also declined for the "always access" group, i.e. those who remained in public schools and were exposed to lower-scoring peers, by 0.13 s.d. (significant, 10 percent level). We cannot know whether these declines are due to direct peer effects and/or teacher adaptations. However, we found that public schools attempted to serve the "gained access" students, perhaps at the expense of the "always access" group. The majority of public school teachers reported to have changed their teaching methods in some way after the policy change, with over a quarter having adjusted the difficulty level of their instruction. In turn, nearly a third fewer "always access" students found the instruction level difficult than their comparison group in the pre-zoning cohort. "Never access" students reported no difference in instruction level and did not see any change in learning, suggesting that public school teachers made more efforts to adapt than private school teachers. Hence, our results suggest that there were negative effects from exposure to lower-scoring peers but no positive effects from exposure to higher-scoring peers.

Our paper makes two important contributions to literature related to compositional changes and peer effects: (1) we examine student learning impacts in the context of a large compositional change; and (2) we are able to estimate impacts not just for those who gained entry to desired public schools, but also students who were displaced to lower-quality private schools and students exposed to a new peer group in public and private schools. A range of studies have looked at the impacts of selective schools on admitted students with different backgrounds or academic preparation (for instance, see Jackson et al., 2020; Oosterbeek et al., 2020; Angrist et al., 2019). It is less common to study the impacts of changes to student composition and/or the impacts of such changes on students who remain in or are displaced from selective schools. To our knowledge there is just one paper that looks at impacts on displaced students, Black et al., (2020), and this is at the tertiary level. The related, extensive peer effects literature mostly exploits small year-to-year variation in peer composition in linear-in-means models and student fixed-effects models (see Sacerdote, 2011, for an overview), which provides limited insight into impacts from larger-scale peer changes.

The research that analyzes peer changes focuses mostly on impacts on incumbent students in selective schools (the equivalent of "always access" in our paper), providing a limited picture of their effects. In this literature, impacts on incumbent students are generally null or negative. For example, Rao (2019) studied compositional changes resulting from a policy that required approximately 400 elite private schools in Delhi to reserve 20 percent of their seats for poor students. He found a 0.17 s.d. decline in English learning (no impact on math or Hindi) on incumbent students. Angrist and Lang (2004) found no impacts on local students from busing Black, lower-scoring students to mostly white, higher-performing schools in Boston suburbs. Imberman et al., (2012) examined the impacts of schools receiving Hurricane Katrina evacuees in Louisiana and Houston. They found on average no achievement impacts on nonevacuee peers in Houston other than small effects on students in the middle quartiles in elementary school and small impacts in Louisiana.

Two experimental studies estimated impacts of a change in student composition on incoming and incumbent students. Muralidharan and Sundararaman (2015) randomized an offer of vouchers for high-quality private schools for public school students at both the village and student levels in Andhra Pradesh. They found limited benefits to students who gained access to private schools and no effects on remaining students in private and public schools. Duflo et al. (2011) studied the effects of within-school tracking in 60 schools in western Kenya, splitting grade 1 classes into the top and bottom half of the baseline score distribution. This intervention substantially increased classroom homogeneity in terms of test scores, essentially reducing integration. The average effect of tracking was 0.14 s.d. in math and 0.20 in literacy, and it had a positive effect on students in all test score quantiles. While their estimates of peer effects are internally valid, responses like teacher or student behavior might not replicate from a research to real-world settings (Pop-Eleches and Urquiola, 2013). Private schools in Andhra Pradesh did not adapt teaching methods to new students, likely because the intervention only affected one cohort (Muralidharan and Sundararaman, 2015; Singh, 2015). In Duflo et al. (2011), researchers had some control over school inputs as they randomly allocated teachers to classrooms, a policy less likely outside of a research setting.

When comparing impacts from compositional changes, it is important to consider the share of students who changed schools along with the background and preparation of incoming students compared to incumbent students. All the papers mentioned above studied smaller overall changes in student composition and smaller incoming-incumbent test score gaps than our paper, which potentially explains why they found small or no impacts from changing peers. In Yogyakarta, the share of students who gained access to public schools occupied between 6 and 47 percent (average 30 percent) of public school seats and scored on average 1.5 s.d. lower in Indonesian and 1.7 s.d. in math than incumbent students. The share of evacuees in the Hurricane Kartina natural experiment was a maximum of 25 percent in Houston and 56 percent in Louisiana, although this was 3 percent on average in both locations. Evacuees scored between 0.1 and 1.1. s.d. lower than incumbent students in math and reading (Imberman et al., 2012). In western Kenya, there was an average difference of 1.6 s.d. in math and literacy between the two classes that were split based on ability (Duflo et al., 2011), but this was at the classroom not the school level.

Reflecting on policy implications, our study provides evidence that when a policy alters student composition within schools substantially, students, administrators, teachers, and parents are all affected. Research that considers system-wide impacts can serve to inform designers about how different schools affect different types of students and how key actors in the education system may react to new student composition and reallocation. The Yogyakarta policy harmed some students significantly more than anticipated and benefited others much less than anticipated. Both public and private schools were largely unsuccessful at managing the transition to a more heterogenous group of students, rendering school effects at least partly dependent on student selection in the brief 18 months over which we measured impacts. Understanding the mechanisms for changing learning for students across the learning distribution is an important contribution of studies that assess system-wide impacts.

The rest of the paper proceeds as follows. The next section describes the zoning policy and the context in which it took place. Section 3 describes our data and Section 4 presents descriptive results on how the policy changed student composition in public and private schools. Section 5 explains the value-added approach we use to estimate the impact of the change in student composition on learning. The impact results are presented in Section 6 and in Section 7 we conclude.

2. Yogyakarta context and the zoning policy

Approximately 420,000 people live in Yogyakarta, a city on the Indonesian island of Java with a reputation for academic reverence and excellence. Yogyakarta has a gross enrollment rate of over 100 in junior secondary schools which include grades 7 to 9. Nearly all residents attend school until grade 9 or higher as schooling has been compulsory through grade 9 throughout the country since 1994 (Pusat Data dan Teknologi Informasi Kemdikbud, 2019). Twenty-one thousand students attended Yogyakarta's 16 public and 41 private and junior secondary schools in the 2017-18 school year (PDSP-K, 2018). Yogyakarta's public schools are free and some of the most reputable in the country. Thirteen out of the 16 public junior secondary schools were among the 100 highest-scoring schools nationally on the 2019 grade 9 leaving exam;[1] one of the 16 schools was ranked first, and four schools were in the top 10 (Rahmawati, 2019). Yogyakarta's 15-year olds scored on average 414 points on the 2018 Programme for International Student Assessment (PISA) in reading and 430 points in mathematics. While not extraordinary, considering that the OECD average was 485, Yogyakarta's performance was still markedly higher than the all-Indonesia average of 371 in reading and 379 in mathematics (OECD, 2019).

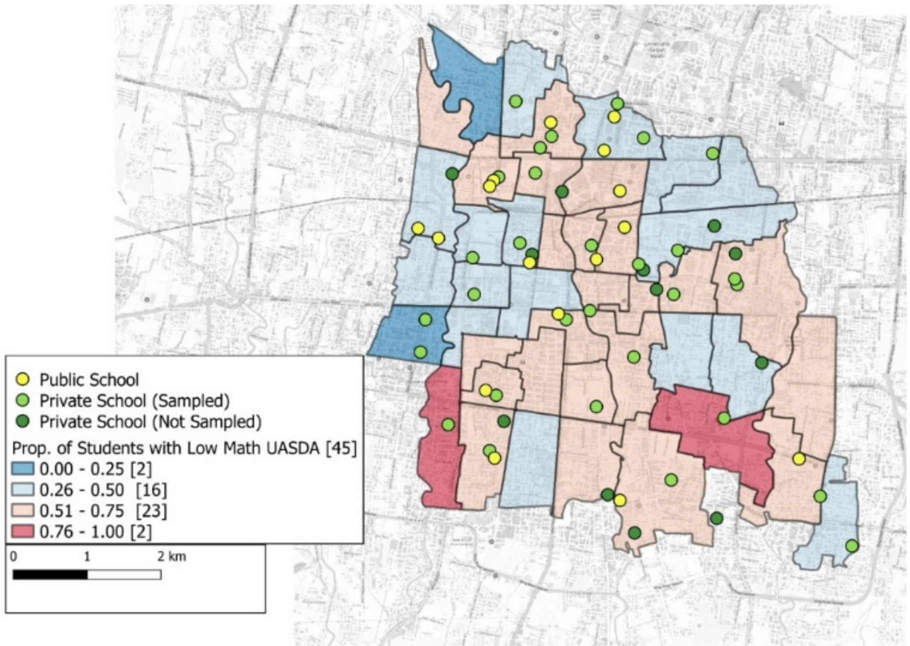
Public schools generally perform better than private schools in Indonesia (Newhouse and Beegle, 2006). In 2017, prior to the policy change we study in this paper, public schools in Yogyakarta scored on average 23 points higher (on a 100-point scale) than private schools in the city and surrounding areas on the grade 9 leaving exam, the primary measure parents and education administrators use to assess quality. Prior to the zoning policy, between grades 6 to 8, students in public school learned on average about 0.4 s.d. more than those in private school (Table A.1). It is not the case that all 16 public schools are higher-performing than all private schools (Figure 6.1), but there were only 11 private schools out of 41 that were higher-performing than the lowest-performing public school at this study's baseline. Public schools are also generally better resourced. Teachers in public schools have more years of experience and received on average more than double the salary of teachers in private schools in 2018 (Table A.11).

Public schools in Yogyakarta have the capacity to serve only 60 percent of students enrolled in junior secondary schools and are highly selective. All students apply to public schools as they are preferred over private schools. Public schools traditionally admitted students based on test scores, such that the average mathematics grade 6 leaving exam score among public junior secondary school students was 1.2 s.d. higher than that of private school students (Table 4.2). Income-eligible students who are not admitted to public schools can apply for a publicly-funded voucher that covers 60 to 100 percent of typical private school tuition. Private school students receive Rp 2,000,000 or approximately US$140 per semester. Any child in a household eligible for *Kartu Menuju Sejahtera* (KMS), a comprehensive poverty assistance program, is eligible for a voucher, i.e., there is no oversubscription.

Yogyakarta is not a geographically segregated city from an education perspective, in the sense that lower-testing students live near highly sought-after schools. Figure 2.1 shows the location of public (yellow dots) and private (green dots) schools throughout Yogyakarta. The background colors indicate the share of students scoring below the median on the grade 6 leaving exam in mathematics by *kelurahan*, i.e., the borders are kelurahan, an administrative unit equivalent to a village.[2] The grade 6 leaving exam is called the *Ujian Akhir Sekolah Daerah* or UASDA. Students who score lower on the UASDA are disbursed throughout the city, and over half of public schools are located in kelurahan with between 51 and 75 percent of students scoring below the median on the UASDA (light pink shading). While UASDA scores and KMS participation are somewhat correlated, this correlation is modest; the correlation coefficient is -0.18 for mathematics and -0.24 for Indonesian.[3]

[1] In total, 56,500 junior secondary schools across Indonesia took the 2019 grade 9 leaving exam.
[2] A *kelurahan* is an administrative unit in Indonesia between a *rukun warga* (RW, see footnote below) and a *kecamatan* or subdistrict. In Yogyakarta, there are 45 kelurahan and 14 kecamatan (Major Decree No. 335/2018).
[3] The UASDA and asset index correlation is 0.32. We describe the asset index in Section 3.

Figure 2.1 Proportion of students scoring below median on the mathematics UASDA by kelurahan in Yogyakarta



Source: UASDA 2017.

Note: Sampling details are in Section 3. We surveyed all public schools and a subset of private schools for this study. Brackets indicate number of kelurahan.

We study the effects of a change in public junior secondary admissions policy that was first introduced in 2018. Figure 2.2 depicts the percent of seats allocated to different admissions categories under three different policies: (1) pre-zoning (pre-2018), (2) zoning 1 (2018-2019 school year), and (3) zoning 2 (2019-2020 school year). Under the pre-zoning policy, all admissions categories used an UASDA ranking. For example, 55 percent of the junior secondary seats available in each school were reserved for students residing in Yogyakarta and allocated by ranking students' UASDA scores, which were an unweighted average of a student's mathematics, Indonesian, and science UASDA scores. Twenty-five percent of seats were reserved for students who participated in the KMS program; applicants were ranked by UASDA score among those who qualified.[4] This policy extended to students in Yogyakarta's suburbs, such that students from outside of Yogyakarta were eligible to apply to public junior secondary schools in the city; up to 20 percent of seats in a school could be, at the school's discretion, reserved for applicants outside of Yogyakarta, again using UASDA scores as the admission criterion.[5]

[4] KMS-qualifying students could also be admitted through the pure test-score ranking category, so the total share of KMS-eligible students in a school could exceed 25 percent.

[5] If schools weren't able to fill 20 percent of seats with students from outside of Yogyakarta, the remaining seats were allocated again using the test score ranking among Yogyakarta resident applicants. Each district in Indonesia sets its own UASDA, and tests are not comparable across districts. Students from within and outside of Yogyakarta were admitted using a test score ranking but with incomparable tests.

Figure 2.2 Allocation of seats within each school under the pre-zoning, first zoning, and second zoning policies

Percent of seats allocated based on:	PRE-ZONING	ZONING 1	ZONING 2
UASDA score (Yogyakarta residents)	55	15	40
UASDA score (non-Yogyakarta residents)	20	5	5
Poverty status (UASDA rank)	25	0	10
Proximity to school (Yogyakarta residents)	0	75	30
"**Special talents**" (UASDA rank)	0	0	10
Relocation (UASDA rank)	0	5	5

May 2018 **May 2019**

The public junior secondary admission system is choice-based; students rank their preference for up to 16 public junior secondary schools and are only considered for the schools they rank. Students are permitted to enter into multiple admissions categories since these are implemented sequentially. For example, before zoning, a student could enter the UASDA score category (which is first); and if not selected, enter the KMS-participant category. Different school rankings are permitted across categories, meaning that a student could place a school in a different preference rank for each category. Students within each selection category are allocated to schools using the deferred acceptance mechanism with the aim to optimally match students to schools based on student school preferences (for an explanation of this mechanism, see Roth (2008)).

Yogyakarta's policy of admitting junior secondary students by ranking UASDA scores was based on convention. Similar policies were implemented in cities throughout the country, and since generally admission policies are the responsibility of local government, prior to zoning there was no national Ministry of Education (MoE) guidance on admissions policies for public schools.[6] The MoE had for several years recognized that admitting students primarily based on the UASDA restricted tens of thousands of students nationally from accessing schools closest to their homes, importantly schools that parents preferred (Hadi, 2019). Despite districts having local autonomy over school admissions, in 2017 the MoE put forward Regulation No. 17 to encourage more equitable admissions criteria, calling the

[6] The name of the ministry that oversees public, non-religious education has changed twice during the timeframe of our study, and thus we use the generic term "Ministry of Education" or MoE to refer to this ministry. Its names have been Ministry of Education and Culture (2011-2021) and Ministry of Education, Culture, Research and Technology (2021-present).

policy "zoning" (Kemdikbud, 2017). The then Minister of Education Muhadjir Effendy stated, "[z]oning is one of the strategies to accelerate the distribution of quality education. Our target is not only equal access to educational services, but also equal distribution of education quality" (Kemdikbud, 2018). Under the policy, the MoE strongly suggested that local governments in 2017 to allocate a minimum of 90 percent of seats in public junior and secondary schools according to the distance from students' homes to the school rather than their test scores.[7,8]

MoE's argument that the zoning policy would be fairer was based on the fact that in tens of cities throughout Indonesia, many disadvantaged students lived near public schools yet could not attend them. Thus the policy didn't directly target students with lower incomes or lower test scores, but rather hoped they would benefit due to residence. This was especially relevant to cities like Jakarta, where poorer neighborhoods are more concentrated around desirable public schools than in Yogyakarta. As shown in Figures 2.1 and A.1, in Yogyakarta lower-testing and poorer students also live close to public schools. However, as shown in the "pre-zoning" column in Figure 2.2, the pre-zoning policy already reserved 25 percent of public school seats for the highest-scoring KMS participants, which could have been a more direct way of providing poorer students access to public schools. Zoning also had other targeting shortcomings. Naturally, students and schools are not distributed equitably throughout the city such that everyone has equal access to schools of equal quality. In Yogyakarta, some students were not accepted to any public school because they lived too far from any school. Moreover, location-based admissions policies have a history of *creating* inequities in other countries (Black, 1999). Thus, we evaluate the learning impacts of this policy considering the *stated* goal of fairness and expanding access, even if we as researchers might have recommended a different policy to achieve these goals.

Many education officials and parents in Yogyakarta resisted the zoning policy. District education leadership expected that zoning could provide greater access to public schools for lower-testing students who lived near public schools. However, they were still concerned that these changes would not adequately help traditionally excluded students, that some students would not have access to *any* public school because they lived in a part of the city without public schools, and that zoning would bring down average grade 9 leaving exam scores, hurting Yogyakarta's reputation as one of the highest-performing districts in the country. Many parents felt that their high-testing students would be unfairly excluded from public schools. Their resistance took the form of city government making numerous appeals throughout 2017 to the MoE for an exemption to implementing the zoning policy. In late 2018, Yogyakarta's mayor requested that officials from the local education agency meet with MoE lawyers to discuss their appeal (interview with local government officials, November 2018).

The appeals failed, and Yogyakarta reluctantly first implemented the zoning policy in the 2018-19 school year, prompting a significant change to its admissions policy for public schools, albeit a less extreme version of MoE's policy recommendation. The "zoning 1" column in Figure 2.2 shows how seats were allocated under the new policy. The most significant change was that now 75 percent of the public junior secondary seats were reserved for applicants who lived closest to the school, i.e., applicants were ranked

[7] While the central government issued this regulation, due to Indonesia's decentralized government, local districts still have a lot of autonomy over local policy and can disregard central government policy in some circumstances. MoE has threatened sanctions like withholding funding against districts that don't comply with zoning (Mustusilo, 2019).

[8] Primary schools have been subject to zoning regulations, meaning location-based admissions, since the early 2010s. Students are not assigned to schools. Caregivers can apply to any public primary school in their *kecamatan* or subdistrict. If schools are oversubscribed, students are admitted based on an age ranking, with older students given higher priority.

by distance from their neighborhood to a school with closest students ranking highest and admitted using this rank.[9] The zoning policy still reserved some seats for students with the highest UASDA scores, but this declined from 55 to 15 percent of seats at a school for Yogyakarta residents and from 20 to 5 percent of seats for residents outside Yogyakarta. This decline in seats from residents outside of Yogyakarta effectively also meant that public school seats expanded for city residents. The remaining 5 percent of seats were allocated to students who moved to Yogyakarta due to natural disasters in their place of origin or their parent's job assignment to Yogyakarta, who were admitted using the UASDA rank. While the first zoning policy gave greater priority to students living in Yogyakarta, there was still no guarantee that every Yogyakarta resident received a seat in a public junior secondary school.[10]

The third column "zoning 2" in Figure 2.2 indicates that the zoning policy was further revised. This happened after only one year of zoning due to parental pressure to allocate more seats based solely on test scores. Parents lobbied government representatives, some with the help of community-based organizations, and the local ombudsman, which had the potential to lead to a lawsuit against the city (interview with Ombudsman, August 2018). The city also threatened to file a lawsuit against the MoE. Ultimately, the city government sided with parents, again making the UASDA a critical component of admissions. The second zoning policy in 2019 was not a complete reversion to the pre-zoning policy, but increased the UASDA-based share of seats in a school to 40 percent, compared to 15 percent under the first zoning policy and 55 percent under pre-zoning. Under the second zoning policy, just 30 percent of seats were allocated according to proximity criteria, while this was 75 percent under the first zoning policy. Another 10 percent of seats were reserved for a new category of "talented students;" students in this category were designated by the district as having a special talent related to athletics, arts, or academics. These students were also admitted based on the UASDA score rank. Another 10 percent of seats were reserved for KMS-participating students and ranked by UASDA scores. The remaining 10 percent of seats were allocated to students from outside Yogyakarta and from relocated families, as was the case with the first zoning policy. The 2019 policy continues today.

3. Data sources

We use two main data sources to estimate the impact of the admissions policy changes and subsequent compositional changes on learning outcomes. First, we use the UASDA taken by all students who attended public and private primary schools in Yogyakarta in 2017, 2018, and 2019. The Indonesian school year generally runs from July to May. Children in grade 6 who took the UASDA in May 2017 were the last cohort to graduate from primary school before the zoning policy was enacted. The first zoning cohort took the UASDA in May 2018 and the second zoning cohort took the UASDA in May 2019. The UASDA includes three subjects: mathematics, science, and Indonesian, with 40 items per subject and scores ranging from 1 to 100. We analyzed results separately for mathematics and Indonesian, excluding science results for comparability as our second data source does not include a science exam.

UASDA items differed across years, so to correct for potential differences in test difficulty over time, we standardized the score within each cohort using the mean and standard deviation of the entire population

[9] Technically this is the distance from the *rukun warga* (RW) to the school, i.e., all students from the same RW are ranked equally from a distance perspective. An RW is a small neighborhood; there are 616 RW in Yogyakarta (Major Decree No. 335/2018). There are approximately on average 190 households per RW in Yogyakarta. Students applying to the same school from the same RW are ranked using their grade 6 leaving exam scores; if the scores are identical, then they are ranked according to age and then the time of application.

[10] In 2019, 7,345 students from within Yogyakarta applied to a seat in public schools and 3,455 received one. There were some RW from which zero students received slots in public junior secondary schools because they lived too far from any public school.

of students who took the test. Later cohorts scored significantly lower on the mathematics test and higher on the Indonesian test compared to the cohort one year before them.[11] Because the average mathematics score went down and the Indonesian score went up during our study period, we suspect that the tests were of varying difficulty (even if the local government aims to make all UASDA equally difficult) rather than the three subsequent cohorts of students having different underlying skills.[12] Note that primary school enrolment is universal in Yogyakarta, so we can rule out lower test-takers dropping out of grade 6 in some years and not others. Our standardization method improves comparability across cohorts, assuming the tests rank students similarly, even if the tests were of varying difficulty.

Another concern about UASDA comparability across cohorts could be that students exerted different levels of effort in different years, depending on their perceived chances of admission to desired schools given the admissions policy in that year. As mentioned above, the UASDA is in May. The first zoning policy was announced in June 2018, after the UASDA. Therefore, the pre-zoning and first zoning cohorts should have had equivalent perceptions of the UASDA's stakes. The second zoning policy was also announced after the May 2019 UASDA, in June 2019. Because the second zoning cohort knew about the zoning policy, they might have expected admission to be based on house-to-school distance, but they also could have heard about publicized parent demands that the zoning policy be reversed, so it is hard to assess how much effort they exerted on the UASDA. We do not find evidence for students exerting less effort on the UASDA test when they live close to a public school. Table A.2 shows that, conditional on student background characteristics to adjust for some segregation, students living further from public schools did not score significantly higher on the UASDA score in any cohort. If anything, these students scored lower than students living close to public schools. Therefore, we are not concerned about student motivation on the UASDA exam for our analysis.

The second learning outcomes source we used is the student learning assessment (SLA) (Rarasati et al., 2020) that we administered for this study in grades 7 and 8 in all 16 public junior secondary schools and 30 out of 41 private schools in Yogyakarta in 2019 and 2020.[13] The SLA measured mathematics and Indonesian skills. We used stratified sampling to create a sample representative of all private junior secondary schools in Yogyakarta.[14] Throughout the paper we applied sampling weights to correct for under-sampling of private school students. The weight for public school students is one while the weight for private school students is the inverse of the share of private school students in each cohort in our sample. Table 3.1 shows the testing data we used and the sample size for each zoning cohort. As we discuss further in Section 5, we analyzed the impact of the first zoning policy over 18 months using the 2017 and 2018 UASDA and the 2019 and 2020 grade 8 SLA; and the impact of the second zoning policy over 6 months using the 2018 and 2019 UASDA and the 2019 and 2020 grade 7 SLA.[15] We standardized

[11] The first zoning cohort scored on average 2 points lower on the mathematics test and 5 points higher on the Indonesian test than the pre-zoning cohort, and the second zoning cohort scored 9 points lower on the mathematics test and 1 point higher on the Indonesian test than the first zoning cohort.

[12] Grading was also done slightly differently between the pre-zoning and zoning 1 cohorts. Test scores were rounded in the pre-zoning cohort such that the math UASDA scores were reported in 2.5-point intervals and the Indonesian UASDA scores were reported in 2-point intervals. There was no rounding in the zoning 1 or zoning 2 cohorts. However, if we round the zoning 1 and 2 cohort scores as was done for the pre-zoning cohort, this does not explain the difference in mean scores between the cohorts.

[13] The 11 private schools that were not sampled had 704 students in the pre-zoning cohort (22 percent of private school students), 646 students in the first zoning cohort (23 percent of students), and 794 students in the second zoning cohort (25 percent of students).

[14] We stratified the schools using four geographical strata, two in the north and two in the south. Then we randomly sampled schools within each of the geographical strata.

[15] All testing was completed before schools closed due to the pandemic in March 2020.

the percent correct on the grade 8 SLA using the mean and standard deviation of the pre-zoning cohort, and we standardized the percent correct on the grade 7 SLA using the mean and standard deviation of the first zoning cohort.

In addition to testing data, we collected survey data from students, teachers, and school principals in 2019 and 2020. We administered a short survey to all students enrolled in public and sampled private schools about their school preferences, household assets, mother's education, and distance to their enrolled school.[16] We used the questions about assets to generate an asset index to examine impacts by students' asset levels as a proxy for wealth or income. Using principal component analysis, we converted the asset data into independent components; the component that explained the largest amount of variance of the original data was used as the asset index (Filmer and Pritchett, 2001).[17] We interviewed teachers to ask about their background characteristics like salary, years of experience, civil servant status, and tutoring activities; and teaching practice, such as how they might have adjusted lessons or teaching due to the policy changes. We interviewed principals about school facilities and school responses to the policy changes. In addition, we used administrative data from the Yogyakarta education agency on whether the student participated in the KMS program and their house location to study their chances of admission to public schools.

We do not have UASDA and SLA data for every student in the sampled schools. We have UASDA data for students who graduated from primary schools in Yogyakarta and SLA data for students who enrolled in our sampled junior secondary schools in Yogyakarta. These would be the same students in a closed system, but this is not the case in Yogyakarta as the education system is open to students from surrounding districts. Thus we have SLA data but no UASDA data for students who went to primary school outside of Yogyakarta and attended junior secondary school in Yogyakarta (around 1,500 students in each cohort, see the difference between columns 2 and 3 in Table 3.1); and we have UASDA data but no SLA data for students who attended primary school in Yogyakarta but attended junior secondary school outside of Yogyakarta. This explains the large difference of about 3000 students in each cohort between columns 1 and 3 in Table 3.1.[18]

The share of enrolled students for whom we have a UASDA score is higher in the zoning cohorts than in the pre-zoning cohort due to the fact that the share of public school seats for students from Yogyakarta increased from 80 to 95 percent under the first zoning policy (Figure 2.2). The share of students enrolled in the public schools for whom we have a UASDA score increased from 77 percent of pre-zoning students to 83 and 85 percent of public school students in the first and second zoning cohort, respectively (comparing columns 2 and 3 in Table 3.1). These percentages are lower than the share of seats allocated to Yogyakarta residents because there are students who live in Yogyakarta but graduated from primary schools outside Yogyakarta (including relocated students who make up 5 percent of seats under both zoning policies).

Our analysis sample is columns 4 and 5 in Table 3.1, which includes students enrolled in public and private junior secondary schools in Yogyakarta for whom we have UASDA and SLA data. This amounts

[16] We do not have data on which schools students applied to, how students ranked schools, or schools they were rejected from.
[17] The asset inventory included questions on whether the household owned these items: computer/laptop, mobile phone, tablet, car, motorcycle, game console, washing machine, air conditioning, refrigerator, TV, water heater; and whether a student had their own bedroom.
[18] Since enrollment in junior secondary schools is basically universal in Yogyakarta (Badan Pusat Statistik D.I. Yogyakarta, 2020), we attribute this difference to students from primary schools in Yogyakarta studying in junior secondary schools outside of Yogyakarta rather than drop out.

to 55, 51 and 56 percent of all UASDA takers in the pre-zoning, first zoning and second zoning cohorts, respectively (comparing columns 4 and 5 to column 1 in Table 3.1), and to 66, 65 and 70 percent of all enrolled students in the sampled schools in the pre-zoning, first zoning and second zoning cohorts, respectively (comparing columns 4 and 5 to column 2 in Table 3.1).

In Table A.4, we compare our analysis sample to students enrolled in the sampled schools for whom we do not have a UASDA score. As expected, most students for whom we do not have a UASDA score lived outside of Yogyakarta: 83 percent of the pre-zoning cohort and 74 percent of the first zoning cohort for whom we do not have a UASDA score lived outside the city. They lived about 2.6 km further from the public schools than the students in our sample, and about 1.6 times more students than in our sample indicated to have moved in grade 6, suggesting that some of them moved to Yogyakarta after taking the UASDA. Therefore, we interpret the results in this paper as being valid only for students from Yogyakarta. In Section 5.1 we provide balance tests for our analysis sample. In Section 6.5 we also perform a robustness check by including students without UASDA scores in our analysis and find similar results.

We find that attrition among enrolled students, i.e. the share of students who declined to take the SLA, is small and not systematic and not a concern for comparability. Attrition in the grade 8 SLA was 4 percent in the pre-zoning cohort and 9 percent in the first zoning cohort, while in the grade 7 SLA it was 4 percent in the first zoning cohort and 6 percent in the second zoning cohort (comparing columns 4 and 5 to column 3 in Table 3.1). Students who refused to participate in the SLA were similar in terms of their standardized UASDA score to students who participated (not shown). Attrition was somewhat larger in private schools than in public schools. We apply inverse probability weighting as a robustness check in section 6.5 to correct for this small difference.

Table 3.1 Number of test takers by test type and cohort

	(1) **All gr6 UASDA takers**	(2) **Enrolled in sample schools**	(3) **UASDA + enrolled in sample schools**	(4) **UASDA + enrolled in sample schools + took gr7 SLA**	(5) **UASDA + enrolled in sample schools + took gr8 SLA**
Pre-zoning	7139	5943	4080		3903
Public		3376	2601		2503
Private		2567	1479		1400
Zoning 1	7200	5590	4002	3834	3654
Public		3406	2821	2715	2592
Private		2184	1181	1119	1062
Zoning 2	7345	5874	4403	4130	
Public		3442	2911	2779	
Private		2432	1492	1351	

Note: The UASDA is administered in May while the 2019 SLA was administered in January and the 2020 SLA in February. Table includes students from all 16 public schools and 30 out of 41 private schools for whom we have a UASDA test score. The total number of students enrolled in the unsampled private schools is 704 students in the pre-zoning cohort, 646 students in the first zoning cohort, and 794 students in the second zoning cohort.

The central result we show in this paper is the impact of the first zoning policy compared to the pre-zoning policy because we have test score data for these cohorts for a longer timeframe. While we can compare the pre-zoning to the first zoning cohort after 18 months of enrolment, we can only compare the second zoning cohort to the first zoning cohort in grade 7. The six-month comparison is a small timeframe over which to expect learning impacts to materialize. Moreover, the change in admissions criteria from pre-zoning to the first zoning policy was much more substantial than the change from the first to the second zoning policy and thus we would expect to see a larger impact from this change. Another issue with comparability for the first and second policies is that the second zoning cohort might have exerted less effort on the UASDA because they thought they would be admitted to public junior secondary schools based on house-to-school distance, so it is more challenging to compare UASDA scores between these cohorts. We primarily utilize the second zoning impact results in Section 6.4 to support the argument that the impacts we find are causal by showing that impacts moved in opposite direction after the changes in student composition were mostly reversed.

4. Changes in student composition across schools as a result of the first zoning policy

We discussed in Section 2 that most public junior secondary schools are located in kelurahan with a higher proportion of students scoring below the median on the UASDA. Given the location of public junior secondary schools throughout Yogyakarta, it's not surprising that the student composition across public and private junior secondary schools changed substantially as a result of the zoning policy change. In Table 4.1, we show changes by UASDA and a continuous measure of assets. We group students in our sample into UASDA and asset index quintiles and demonstrate how much the share of students enrolled in public school in each UASDA or asset index quintile shifted because of the policy change. The total row confirms that public school enrolment in our sample increased by almost 7 percentage points due to the larger share of seats reserved for Yogyakarta residents under the first zoning policy.

The pre-zoning policy allocated 25 percent of seats to KMS participants who scored higher on the UASDA, so it is not surprising that we see much larger enrollment changes by UASDA than assets. The lower UASDA quintiles saw the greatest changes, with the bottom quintile increasing enrollment by 45 percentage points after the first zoning policy. The two top quintiles saw a decline, but only by approximately 12 percentage points, in part because the zoning policy still reserved 15 percent of seats for the highest-scoring students. The lower asset index quintiles benefitted from the policy, although their public school enrollment was high pre-zoning. The share of students enrolled in public school in the bottom asset index quintile rose from 61 to 74 percent; this increased by 18 percentage points for the second-lowest quintile. There was nearly no change in terms of public school enrollment for the top two asset index quintiles. This indicates that zoning clearly wasn't a heavily pro-poor policy, largely because, as mentioned above, the pre-zoning policy already benefitted some KMS participants.

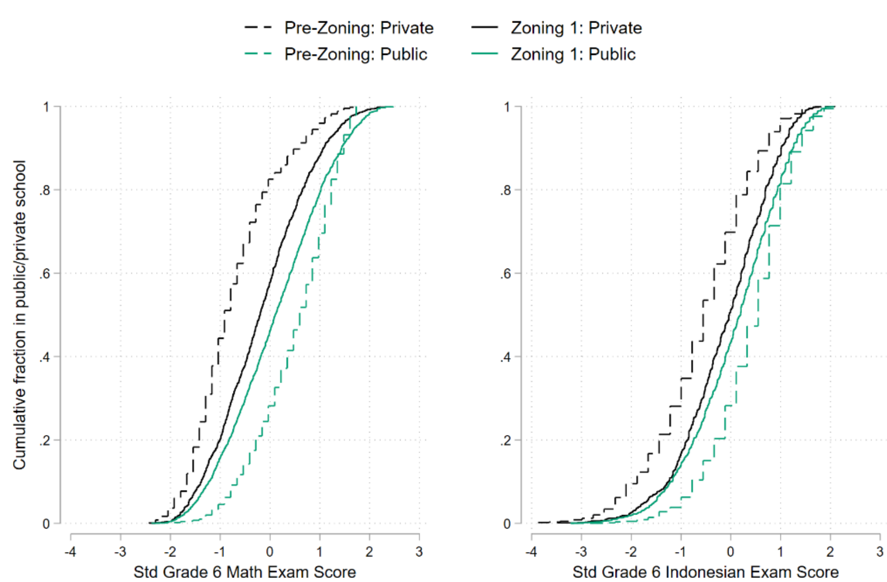
Table 4.1 Percent of sampled students enrolled in public school by UASDA and asset quintiles and policy

	Percent in public school			
	UASDA score		Asset index	
	Pre-zoning	Zoning 1	Pre-zoning	Zoning 1
Total	58.5	65.3	58.5	65.3
Quintile 1 (lowest)	13.9	58.5	61.0	74.1
Quintile 2	43.4	60.0	58.2	76.4
Quintile 3	69.1	63.1	61.7	67.1
Quintile 4	80.6	68.8	58.5	57.3
Quintile 5 (highest)	89.3	76.5	53.2	50.5

Note: Table includes UASDA graduates who enrolled in sampled junior secondary schools. This mechanically overestimates the share of students enrolled in public school as we sampled all 16 public schools but sampled 30 out of 41 private schools. This is corrected for using survey weights. We used the full population of UASDA takers to determine the quintile scores.

The distributions of UASDA scores among public and private schools became much more similar due to the first zoning policy as shown in Figure 4.1. Under the pre-zoning policy, less than 30 percent of students in public schools scored below the mean UASDA score for mathematics (which is zero due to the standardization), while 80 percent of students in the private schools did. After the zoning policy was implemented, about 50 percent of students in public schools scored below the mean, while 60 percent of students in the private schools did.

Figure 4.1 Distribution of UASDA scores by cohort, school type, and UASDA subject



Source: UASDA 2017 and 2018 and SLA 2019.

Note: The pre-zoning lines appear stepwise because pre-zoning, the math UASDA scores were reported in 2.5-point intervals and the Indonesian UASDA scores were reported in 2-point intervals (and no one scored above 90). Scores were not rounded in the zoning cohorts. As discussed in Section 3, we standardize within each cohort.

Examining differences in UASDA scores and student characteristics across public and private schools under each policy, we find that the change in mean UASDA was of similar size in public and private schools. The mean UASDA score[19] of enrolled students was 0.43 s.d. lower in the first zoning cohort than in the pre-zoning cohort in mathematics in public schools, and 0.45 s.d. higher in private schools. It was 0.36 s.d. lower in Indonesian in public schools and 0.37 s.d. higher in private schools (Table 4.2). In the pre-zoning cohort, the average difference in UASDA scores between public and private schools was 1.10 s.d. and 0.92 s.d. in math and Indonesian, respectively; after the first zoning policy it was 0.29 s.d. and 0.19 s.d., respectively. As a result, classrooms became more heterogeneous; the mean within-classroom standard deviation in UASDA math scores increased by 0.23 s.d. in public schools and 0.12 s.d. in private schools. The change in classroom heterogeneity in Indonesian was smaller and even negative in private schools. It increased by 0.09 s.d. in public schools but it declined by 0.08 s.d. in private schools.

Not surprisingly, we also find some changes in student composition between cohorts in terms of background characteristics, namely gender, KMS status, assets, and mother's education (Table 4.2). There was a small, but statistically significant change in the share of boys enrolled in private school because boys generally scored lower on the UASDA and thus had higher pre-zoning private school enrollment. In accordance with Table 4.1, the zoning policy slightly improved public school access for poorer students, defined as students who participated in the KMS program and students with lower asset index values. In turn, private schools admitted wealthier students after the first zoning policy. The share of public school students participating in the KMS program rose by 5 percentage points while this declined by 14 percentage points in private schools.

Looking at mother's education, a predictor of a child's attainment and school performance, there was nearly no change in the share of students in public school whose mothers completed tertiary education or higher, although this increased by 12 percentage points in private schools, consistent with the asset index results. Note that pre-zoning students enrolled in public and private schools were similar in terms of their wealth, despite the large differences in test scores. This can be explained by the modest correlation between test scores and wealth as mentioned in Section 2.

[19] Note that this score should be interpreted as a student's relative performance within their cohort because we standardized the UASDA score within each cohort. The standardized score shows the performance of the student in terms of standard deviations relative to the mean within their cohort. This should be considered when comparing the standardized UASDA score between cohorts.

Table 4.2 Student characteristics before and after the zoning policies, by public and private school

	Public			Private		
	Pre-zoning	Zoning 1	Difference	Pre-zoning	Zoning 1	Difference
Standardized UASDA score						
Math	0.53	0.11	-0.43***	-0.63	-0.18	0.45***
	(0.78)	(0.99)	[0.09]	(0.87)	(0.92)	[0.07]
Indonesian	0.44	0.08	-0.36***	-0.48	-0.11	0.37***
	(0.78)	(0.94)	[0.05]	(0.99)	(0.90)	[0.06]
Within classroom UASDA standard deviation						
Math	0.49	0.73	0.23***	0.60	0.72	0.12***
	(0.18)	(0.20)	[0.07]	(0.15)	(0.18)	[0.03]
Indonesian	0.61	0.70	0.09**	0.79	0.71	-0.08***
	(0.14)	(0.18)	[0.04]	(0.18)	(0.21)	[0.03]
Male						
	0.45	0.48	0.03*	0.55	0.50	-0.05**
	(0.50)	(0.50)	[0.01]	(0.50)	(0.50)	[0.02]
KMS participation						
	0.36	0.41	0.05	0.31	0.17	-0.14***
	(0.48)	(0.49)	[0.03]	(0.46)	(0.38)	[0.03]
Student asset index						
	-0.01	-0.12	-0.10*	0.09	0.26	0.18***
	(1.01)	(0.95)	[0.06]	(1.05)	(0.98)	[0.06]
Mother completed tertiary education						
	0.44	0.43	-0.01	0.49	0.61	0.12***
	(0.50)	(0.50)	[0.02]	(0.50)	(0.49)	[0.04]
Observations	2503	2592		1400	1062	

Note: Table includes students for whom we have a UASDA and an SLA score. The change in student composition in terms of gender, KMS participation, the asset index and mother's education looks similar when including students for whom we don't have a UASDA score (not shown). Standard deviations are in parentheses and standard errors between brackets. Observations are weighted with the inverse of the propensity to be in the first zoning cohort. The number of observations is slightly different for mother's education due to students not knowing their mother's education level (n=3,122 in the pre-zoning cohort and n=2,968 in the first zoning cohort). KMS participation and house location data is non-missing for 78.3 percent of the pre-zoning cohort, 67.3 percent of the first zoning cohort, and 71.3 percent of the second zoning cohort. Stars indicate the significance level of the difference with the mean of the zoning cohort as estimated using a t-test, corrected for clustering at the school level. ***p<0.01, **p<0.05, *p<0.1

5. Empirical strategy

To measure the average impact of the compositional changes on learning outcomes, we compared the mean SLA (grade 8 test) score between the pre-zoning and first zoning cohorts, conditional on students' UASDA scores and background characteristics. We estimated a value-added model as in Andrabi et al., (2011) Equation 1 for student i

$$Y_i^2 = \beta_0 + \beta_1 Z_i + \beta_2 Y_i^1 + \beta_3 X_i + \varepsilon_i \tag{1}$$

where Y^2 is the mathematics or Indonesian SLA score, and Y^1 is the standardized UASDA score in the relevant subject. Z is a dummy variable indicating the first zoning cohort. X is a vector of control variables for gender, age at the time of the UASDA exam, the asset index, and whether the mother completed tertiary education. X also includes kelurahan (neighborhood) indicators that capture any cohort-invariant characteristics of a student's kelurahan.[20] We interact each control variable with an indicator for missing values of that variable, such that all students with a non-missing SLA and UASDA scores are included in the model. ε is the residual. Standard errors are corrected for clustering at the school level.[21]

5.1 Identifying assumptions required for causal interpretation of the results

Our coefficient of interest is β_1, the difference in learning between the pre-zoning and first zoning cohorts for students from the same kelurahan and with the same UASDA rank within their cohort.[22] We can interpret this as the causal effect of the zoning policy on learning if learning between grade 6 and 8 as measured by the UASDA and SLA would have been the same across cohorts in absence of the policy change. This requires accepting two assumptions, namely (1) that there were no other policy changes that could have affected value-added and (2) that primary school graduates from Yogyakarta who enrolled in the sampled junior secondary schools were similar across cohorts.

The zoning policy change was singular among policy or administrative changes that took place in junior secondary schools in 2018 in Yogyakarta. There were no changes in public school-level funding since budgets are determined on a per-pupil basis, and there was nearly no change in the total number of seats in public schools.[23] There were also no changes to teaching staff in public schools, other than regular teacher retirement. In private schools, some schools laid off teachers due to lower enrollment. Without any changes to school resources between cohorts, we argue that a change in value-added between two subsequent student cohorts in absence of the zoning policy is negligible.

Table 5.1 shows that cohorts were similar in terms of their background characteristics (measured by KMS status, an asset index and mother's education), but they differed in terms of their UASDA scores. The mean average UASDA score of -0.01 for mathematics and of 0 for Indonesian in the zoning sample shows that this sample is representative for the full population of UASDA takers, since we standardized the UASDA score using the mean and standard deviation of this population. In the other cohort, relatively high-scoring students on the UASDA selected into the sample, which is not surprising given that the pre-zoning admission system admitted higher shares of students with higher UASDA scores into public schools, but these differences are insignificant.

[20] There are 49 kelurahan categories. Students with missing kelurahan data were coded as a separate category. Students living outside of Yogyakarta were given codes that correspond to their districts (Sleman or Bantul).

[21] We do not include school fixed effects. With school fixed effects, the coefficient β_1 would only measure the average change in school value-added (within-school changes) and the model would ignore the effect from the increase in the number of seats for students from Yogyakarta in public schools in the first zoning cohort. Yet, between-school differences are important for the latter effect. As we see in Table A.1, value-added of public schools is higher than that of private schools in all cohorts. So, moving more students into public schools would still increase overall learning levels.

[22] Note that for the interpretation of coefficient β_1, it does not matter if the UASDA and SLA are different, as long as the UASDA score captures the contribution of previous inputs and unobservable endowments (Singh, 2015; Andrabi et al., 2011). The coefficient for β_2 is 0.5 and 0.6 for Indonesian and mathematics, respectively, showing that the UASDA score is strongly correlated with the SLA score.

[23] The total number of students, including those from outside Yogyakarta, increased by only 30 students, from 3,376 to 3,406 between the pre-zoning and first zoning cohorts.

We corrected for the small differences in UASDA scores between cohorts by conditioning on this score in our value-added model. An important identifying assumption of value-added studies is that the lagged test score captures the contribution of previous inputs and unobservable endowments to correct for selection (Singh, 2015; Andrabi et al., 2011; Todd and Wolpin, 2003). Selection in other value-added studies is generally larger than in our study, because they measure differences in learning between public and private schools or between teachers where different types of students select into different schools and classrooms (for example Chetty et al., (2014) or Bau and Das (2020)). Due to a limited amount of selection between cohorts in terms of background characteristics, the identifying assumption is likely to be met in our case. Hence, we interpret differences in learning value-added between cohorts as being caused by the policy change. We also show that our results are robust against several other specifications in Section 6.5.

Table 5.1 Balance between cohorts

	Pre-zoning	Zoning 1	Difference
Mathematics standardized UASDA score			
	0.05	-0.01	-0.06
	(1.00)	(0.98)	[0.10]
Indonesian standardized UASDA score			
	0.06	-0.00	-0.06
	(0.98)	(0.95)	[0.07]
Male			
	0.49	0.49	0.00
	(0.50)	(0.50)	[0.01]
KMS participation			
	0.34	0.32	-0.02
	(0.47)	(0.47)	[0.02]
Student asset index			
	0.03	0.01	-0.02
	(1.03)	(0.99)	[0.04]
Mother completed tertiary education			
	0.46	0.49	0.03
	(0.50)	(0.50)	[0.02]
Observations	3903	3654	

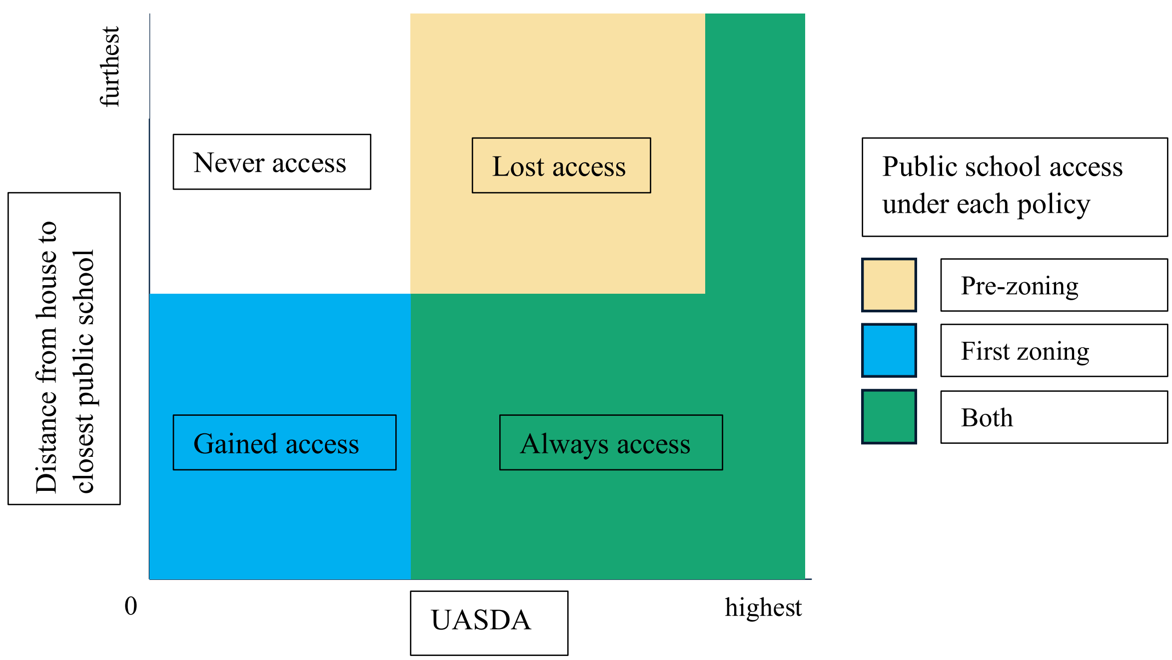
Note: Table includes data from students with non-missing UASDA and SLA scores. Numbers are corrected for under-sampling of private schools using sampling weights. Standard deviations are in parentheses and standard errors between brackets. Gender, mother's education, and household assets were reported by the students tested. We use tertiary education for mothers because such a high share of students' mothers had completed this education level. The number of observations is slightly different for mother's education due to students not knowing their mother's education level (n=3,122 in the pre-zoning cohort, n=2,968 in the first zoning cohort). KMS participation data is non-missing for 78.3 percent of the pre-zoning cohort and 67.3 percent of the first zoning cohort. We test the difference in means with a t-test, correcting standard errors for clustering at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

5.2 Determining who gains and who loses from the policy

An important contribution of our study is that we can categorize students by whether their public school access changed and remained the same. We simulated public school acceptance for each student under the pre-zoning and zoning admissions scenarios to define four groups: students whose simulated public school access would be the same under both policies ("always access" or "never access") and students who would only have access to public schools under one of the policies ("gained access" and "lost access"). We replicate the pre-zoning and zoning allocation of students to schools by ranking pre-zoning and zoning cohort students by their UASDA scores and "fill" public school seats reserved for Yogyakarta residents according to the percent of seats allocated to the various groups described in Figure 2.2 and using the deferred acceptance mechanism.

Figure 5.1 is a graphic depiction of this allocation process. For simplicity, the figure ignores admission by KMS status in the pre-zoning policy although we take KMS participation into account in the actual group identification. The yellow and green blocks represent students with UASDA scores that qualified them for public school, all of whom who had public school access under the pre-zoning policy and a small share of whom maintained this access under the first zoning policy (and remained green) as under zoning there were still 15 percent of students admitted using a UASDA ranking. We refer to this latter group as the "always access" group. Students in the yellow block only had access under the pre-zoning policy and are called the "lost access" group. Students in the blue block only had access under the first zoning policy, defined as "gained access," since they lived closest to public schools and had relatively lower UASDA scores. Students with relatively lower UASDA scores who lived farther from public schools are represented in the white the "never access" group. Figure A.2 shows a scatter plot of the result of our group identification by UASDA score and distance to the closest public school, which mimics Figure 5.1 for each cohort.

Figure 5.1 Schematic illustration of group allocation by public school access



To construct the groups, we had to make assumptions about school preferences. We assumed that all students preferred *any* public school over all private schools because all students apply to public schools. When allocating students based on their UASDA score, we assumed that the order of public school preferences does not matter; or said another way, we considered all 16 public schools as one big school to which we allocated the highest scoring students. When allocating students based on proximity, we assumed that students preferred the closest public school to their house because they had the highest probability of admission there.[24] Using this method we identified students who had the opportunity to enroll in a public school under each of the policies rather than students who actually enrolled. We do not attempt to predict actual enrolment because that requires data on actual school preferences under each policy. A student's preferences could vary depending on the chances of admission to a school and that could vary by policy scenario, so we cannot observe this.

In addition to the assumptions on school preferences, our sorting of students into the four groups relied on several more minor assumptions due to data limitations. First, we are missing location data for 9 percent of students in the first zoning cohort and 24 percent in the pre-zoning cohort.[25] These location data are missing for several schools entirely. If data were missing, we imputed with the location of their primary schools, assuming that these students live nearby their primary school. Public primary schools in Yogyakarta have allocated seats based on house-to-school proximity for decades, and the average distance from house to primary school among our sample students was 900 meters. We are missing primary school information for the zoning students who attended one private school, so we assumed that those students with missing data lived near this private school. Second, we are missing data on whether students participated in the KMS program for 23 percent of the pre-zoning cohort and 33 percent of the first zoning cohort. We assumed students with missing data with an above-mean asset index did not qualify for the KMS program. Among students we know participated in KMS, 82 percent had a below-mean asset index, while 38 percent of students who did not participate in KMS had a below-mean asset index.[26]

In Table 5.2 we show that the simulation performed well at predicting public school enrolment. The simulation results suggest that about 47 percent of students in each cohort had access to public schools under both policies ("always access"), 23 percent gained access, 19 percent lost access, and 11 percent had no public school access under either policy ("never access"). The "gained access" group is larger than the "lost access" group due to the increase in the share of seats for students from Yogyakarta. We find that actual public school enrolment among the "always access" group was around 86 percent for both cohorts. About 17 percent of "never access" students still enrolled in public schools.[27] For the "gained access" and "lost access" groups, we find a 47 to 60 percentage point difference in public school enrolment between policies. We also present characteristics of the groups in Table A.5. As expected, students who "gained access" under the zoning policy lived closer to public schools than the students who "lost access" and students who had access under the pre-zoning policy had higher UASDA scores than the "lost access" students. Under the first zoning policy, the "lost access" students lived about 1.5 km further

[24] Since we allocated the students living closest to each public school to that school, school preferences assumptions only mattered for students who live close to multiple schools. If they were in the "catchment area" of multiple schools, we needed to allocate them to one. This affected the simulated public school enrolment for 215 students in the zoning cohort and for 191 students in the pre-zoning cohort out of 2,322 seats allocated based on distance.
[25] We have location data for more students in the first zoning cohort than in the pre-zoning cohort because the house location is included in the admissions data for the first zoning cohort, but not for the pre-zoning cohort.
[26] Of the students with a non-missing KMS status, about a third are registered as poor (see Table 5.1). As students with missing KMS data have a higher average asset index than students with non-missing KMS data, this imputation only increases the share of students registered as poor to 36 percent. We also tried to predict KMS participation based on the asset index, mother's education, and kelurahan, but this prediction was less precise than imputation based on only the asset index. Of the students for whom we know they participated in the KMS program, we only predicted their KMS participation correctly for 53 percent.
[27] When students with access to public schools decide to enroll elsewhere, their seats open up to students who have lower UASDA scores or live further from the public schools. This explains why some students from the "never access" group could still enroll in public schools.

away from schools than the “gained access” students; the “lost access” students scored about 1.4 s.d. higher on the UASDA than “gained access” students.

Table 5.2 Actual public school enrolment by simulated public school access category and cohort

		Always access	Gained access	Lost access	Never access
Pre-zoning	% of total sample	45.1	24.1	19.4	11.4
	% in public school	85.3	25.5	71.6	16.4
Zoning 1	% of total sample	48.7	22.3	17.5	11.5
	% in public school	86.9	85.1	24.8	17.2

Note: Table includes students with non-missing UASDA and SLA scores, although the simulation is performed on all students with non-missing UASDA scores. Numbers are corrected for under-sampling of private schools using sampling weights.

To study the impact of changes to students’ educational environment on these groups, we estimated Equation 1 to compare learning outcomes between the pre-zoning and first zoning cohort within each group. The categorization could be seen as treatment allocation with imperfect take-up rates. The students we predicted would have access to any public school had the opportunity to go to public schools, but not everyone took that opportunity, for instance because they preferred certain private schools over the specific public schools to which they had access. Therefore, the estimators capture an intent-to-treat effect.

The similarity across background characteristics within these groups confirms that our predictors identified similar students in each cohort (see Table A.5). Within each group, the cohorts are similar in terms of gender and wealth. However, we find that the zoning cohort in the “always access” group scored 0.14 s.d. lower on the UASDA exam than the pre-zoning cohort in the same group, and the zoning cohort in the “lost access” and “never access” groups scored about 0.20 s.d. lower than the pre-zoning cohort. As previously described, by conditioning on the UASDA score in our value-added model, we correct for this selection. We also show that our results are similar when we apply inverse probability weighting as a robustness check, as explained in Section 6.5.

6. Impact results

In this section we first show results for changes to students’ educational environment (Section 6.1), then we show how these changes resulted in effects on learning outcomes (Section 6.2), and finally we explain differences in learning effects by exploring changes in teacher and student behavior (Section 6.3). To provide supportive evidence for the causal interpretation of our results, we also show that learning outcomes bounced back after the second zoning policy (Section 6.4) and that our results are robust to several other specifications (Section 6.5).

6.1 School quality, peer characteristics, and school distance among predicted public school access groups

We first studied the magnitude of the changes to students’ educational environment to better understand what the treatment entailed for each of the public access groups. Students who gained and lost access to public schools were exposed to different levels of school quality and to different peers after the zoning policy. In addition, students who didn’t change schools also experienced indirect effects of a new schooling environment through a change in peers or teacher behavior. We refer to these latter effects jointly as "peer effects" throughout this paper.[28] In addition, because it prioritized proximity to school, the zoning policy was expected to reduce travel time to schools, which could affect learning outcomes if the saved amount of time is substantial (Afoakwah and Koomson, 2021). We present the treatment effects on school quality, peer characteristics, and travel time to school for the full sample (column 1) and for each of the public school access groups (always, gained, lost, never; columns 2-5) in Table 6.1. We provide more details about variable construction below.

We define school quality as school value-added before the policy change, which is similar to methods used by Jackson et al. (2020) and Andrabi et al. (2020). We used the Ordinary Least Squares (OLS) value-added model as specified in Equation 2 for student *i* in school *s* in the pre-zoning cohort,

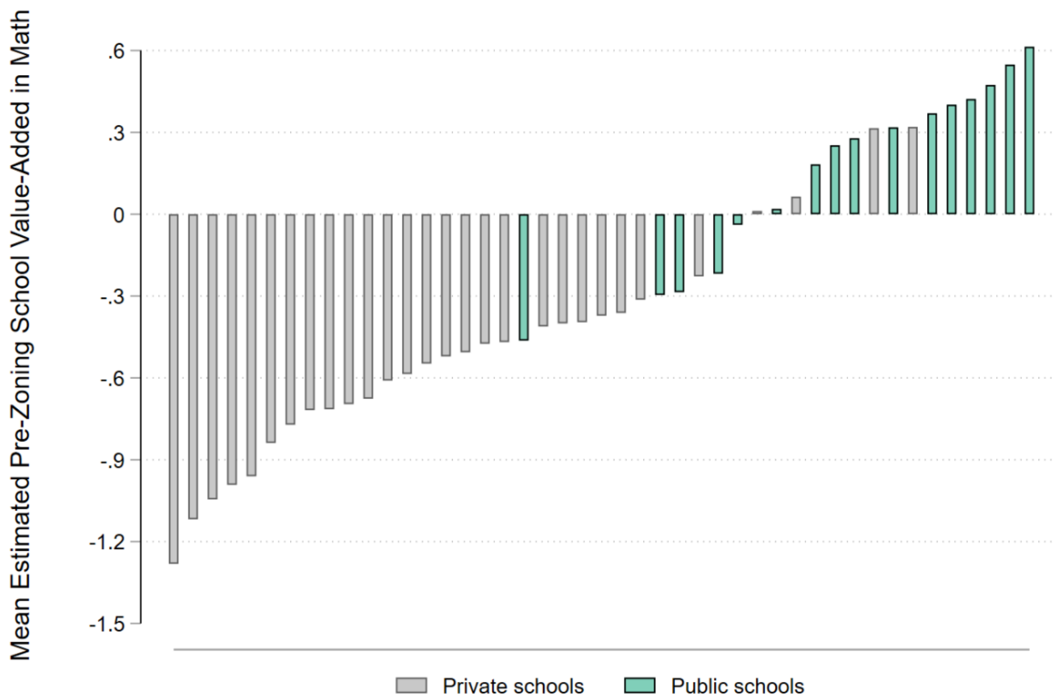
$$Y_{i,s}^2 = \alpha_1 Y_{i,s}^1 + \alpha_2 X_{i,s} + \gamma_s + \varepsilon_{i,s} \tag{2}$$

where γ_s are school indicators that capture the average school value-added in the pre-zoning (baseline) cohort. We estimated the model separately for math and Indonesian. In addition to school value-added, we also looked at school quality in terms of the average grade 9 exam scores of the schools, which is called the *Ujian Nasional* (UN).

The pre-zoning school value-added estimates for mathematics are presented in Figure 6.1.[29] The figure shows that there was large variation of school value-added within public and private schools, and that only 11 private schools had higher value-added than the public school with the lowest value-added. The results for Indonesian are similar, as shown in Figure A.3.

[28] This is the broad definition of peer effects, as in Sacerdote (2011). It includes direct externalities that spill over from peers' or peers' family background or current actions and indirect externalities through teachers who respond to a student's peers, for instance by changing how they teach.
[29] Note that the school value-added estimates basically show school mean grade 8 scores, conditional on the UASDA score and other student characteristics. These can be negative due to test score standardization.

Figure 6.1 Estimated pre-zoning school value-added in math by school type

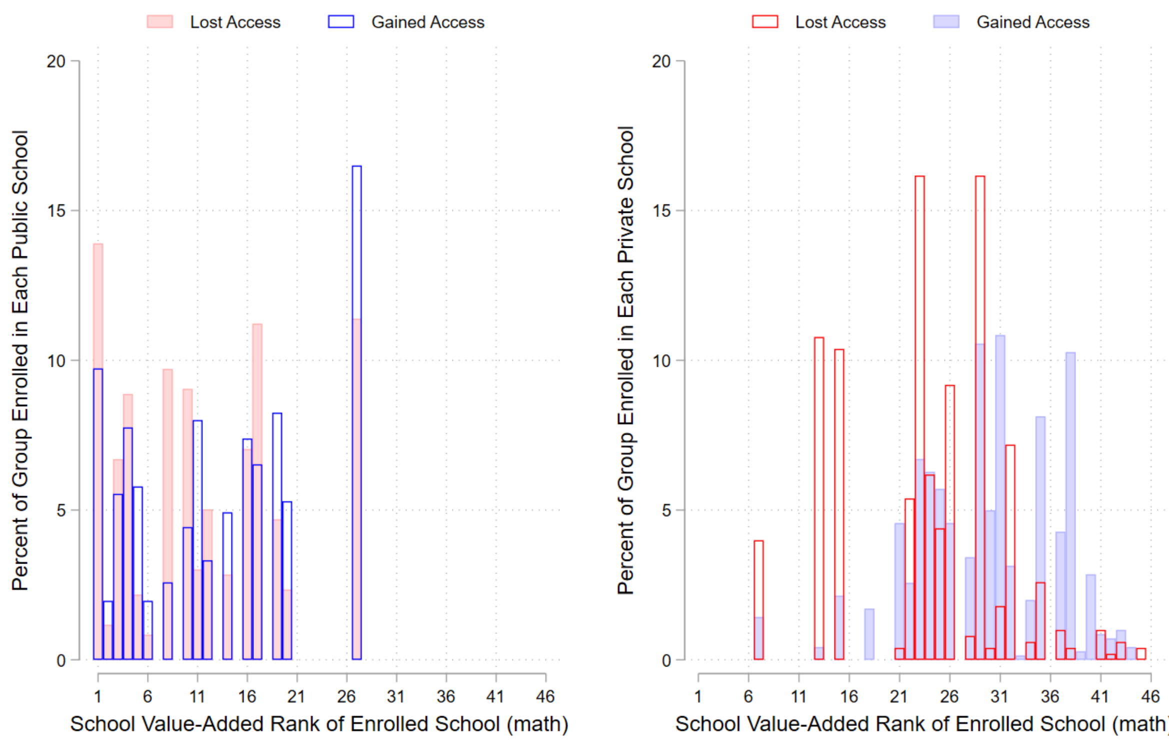


For the total sample, as shown in the first column of Table 6.1, we found that students enrolled in somewhat higher-quality schools after the zoning policy; on average 0.06 s.d. higher value-added in math and 0.08 s.d. higher value-added in Indonesian. The schools also scored 2.05 points higher on the grade 9 exam compared to a pre-zoning mean score of 70.15. As mentioned in Section 2, the zoning policy decreased the number of public school seats that could be allocated to students from outside of Yogyakarta from 20 to 5 percent, effectively increasing the number of public school seats for students in our sample. Because value-added was higher in public schools on average, it makes sense that we found that more seats in public schools had a positive effect on the average school quality in which students were enrolled.

As expected, we found no change in school quality for the "always access" and "never access" students since they did not "switch" schools, while we saw large changes in school quality for the "gained access" and "lost access" students. The public schools that the "gained access" students enrolled in produced on average 0.40 s.d. higher value-added in mathematics and 0.46 s.d. higher value-added in Indonesian than the private schools that their pre-zoning counterfactuals enrolled in. This equivalent figure was 0.28 s.d. lower value-added in mathematics and 0.24 s.d. lower value-added in Indonesian for the "lost access" group. These changes in school quality were also reflected in the school average grade 9 exam scores in 2018. The "gained access" students enrolled in schools that scored 12 points higher on this exam on average, while the "lost access" students enrolled in schools that scored 6 points lower on average.

The fact that the improvement in school quality for the "gained access" group is larger than the decline in school quality for the "lost access" group can be explained by differences in students' school choices. Figure 6.2 shows that students who lost access to public schools enrolled in relatively higher-ranked private schools than students who gained access to public schools did before the zoning policy, seven of which are ranked higher than the lowest ranked public school. On average, the "lost access" group enrolled in private schools with 0.22 s.d. higher value-added in math and 0.36 s.d. higher value-added in Indonesian than the private schools the "gained access" group enrolled in before the zoning policy. The figure also shows that students who gained access to public schools chose to enroll in relatively lower-ranked public schools in terms of school value-added, suggesting that this group could have experienced an even larger increase in school quality. Had these students enrolled in the closest public school to their home, then the difference in school quality between the pre-zoning and zoning cohorts would have been 0.09 s.d. higher in math and 0.03 s.d. higher in Indonesian than the coefficients in Table 6.1.

Figure 6.2 Enrolment in public (left) and private (right) schools by public school access group



Note: Figures only include "treatment compliers," i.e., students for whom we correctly simulated their public school enrolment. The "lost access" students enrolled in public schools pre-zoning (left) and enrolled in private schools after the zoning policy (right). The "gained access" students enrolled in private schools pre-zoning (right) and enrolled in public schools after the zoning policy (left). The figure looks similar for Indonesian and when using the grade 9 exam rank (not shown).

While public school enrolment only changed for the "gained access" and "lost access" groups, all groups were exposed to different peers after the zoning policy, as shown in Table 6.1. The "always access" and "lost access" groups enrolled in schools with 0.36 s.d. and 0.30 s.d. lower scoring peers in the first zoning cohort than in the pre-zoning cohort, respectively. The "gained access" and "never access" groups enrolled in schools with 0.50 and 0.28 s.d. higher scoring peers, respectively. In terms of peer wealth, the "always access" group was exposed to poorer peers (measured by the asset index), while the "lost access" and “never access” were exposed to wealthier peers. There was no change for the “gained access” students in terms of peer wealth, likely because the share of gained access students was so substantial. Table A.6 shows that the effects on peer wealth are similar when measured in terms of KMS participation and mother’s education.

However, the differences in peers at the classroom level are much smaller than at the school level, suggesting that schools started tracking, i.e., grouping students by test scores, in response to zoning. We found that lower-scoring students were grouped together in the public schools they gained access to, and higher scoring students were grouped together in private schools. As a result, the classroom peers of the “gained access” group only scored 0.19 s.d. higher in the first zoning cohort than in the pre-zoning cohort and those of the “lost access” group only 0.13 s.d. lower. For the “always access” group tracking limited the change in classroom peer group UASDA scores to 0.20 s.d. and to 0.11 s.d. for the “never access” group.

Travel time was largely unaffected by the policy changes because Yogyakarta is a relatively small city. The policy decreased students’ travel time to school by about 4 minutes; such a change is unlikely to affect learning outcomes through time savings that could be devoted to studying. The decline in travel time for the “always access” students suggests that they enrolled in public schools that were somewhat closer to their homes than their counterfactuals, and the decline for the “never access” students suggests that they enrolled in closer private schools. Since they still enrolled in schools of the same type (i.e. private or public), which is also reflected by the null effect on school quality, we do not expect this to affect their learning.

We also do not find a change in mobility in terms of students moving closer to their preferred public schools to potentially gain access to those schools. The share of students who moved in their last year of primary school remained similar between cohorts and, if anything, even declined. Since the first zoning policy was only announced about one month before school started it is highly unlikely that parents could change their address in time to benefit from the policy change. Moreover, to discourage relocating to gain admission to a desired school, the city government did not allow applicants to use addresses that had changed in the year prior to the zoning policy.

Table 6.1 Change in school and peer quality and travel time to school by predicted public school access

		Full sample	Always access	Gained access	Lost access	Never access
School quality						
School value-added - math	Zoning coefficient	0.06***	0.02	0.40***	-0.28***	0.02
		(0.02)	(0.03)	(0.08)	(0.09)	(0.02)
	Pre-zoning mean	-0.07	0.17	-0.40	0.07	-0.40
School value-added - Indonesian	Zoning coefficient	0.08***	0.01	0.46***	-0.24***	0.01
		(0.02)	(0.01)	(0.07)	(0.07)	(0.04)
	Pre-zoning mean	0.10	0.33	-0.24	0.27	-0.20
School average grade 9 UN score	Zoning coefficient	2.05***	0.71	12.14***	-6.24***	0.97
		(0.57)	(0.54)	(1.77)	(1.70)	(0.82)
	Pre-zoning mean	70.15	77.64	59.51	74.47	60.33
Peer characteristics – school						
Average standardized UASDA score	Zoning coefficient	-0.06	-0.36***	0.50***	-0.30***	0.28***
		(0.06)	(0.07)	(0.10)	(0.10)	(0.06)
	Pre-zoning mean	0.06	0.58	-0.68	0.34	-0.61
Average asset index	Zoning coefficient	-0.01	-0.11***	0.00	0.18***	0.09**
		(0.03)	(0.04)	(0.06)	(0.06)	(0.04)
	Pre-zoning mean	0.03	0.17	-0.23	0.09	-0.02
Peer characteristics – classroom						
Average standardized UASDA score	Zoning coefficient	-0.05	-0.20***	0.19*	-0.13*	0.11
		(0.04)	(0.06)	(0.11)	(0.08)	(0.07)
	Pre-zoning mean	0.06	0.62	-0.74	0.39	-0.68
Average asset index	Zoning coefficient	-0.01	-0.06*	-0.07	0.19***	0.07*
		(0.03)	(0.04)	(0.06)	(0.06)	(0.04)
	Pre-zoning mean	0.03	0.18	-0.26	0.12	-0.04
Distance to school						
Travel time to school (minutes)	Zoning coefficient	-3.78***	-3.18***	-5.65***	-3.71***	-2.05**
		(0.39)	(0.51)	(0.88)	(0.94)	(0.84)
	Pre-zoning mean	18.23	17.57	18.45	18.86	19.06
Moved in grade 6	Zoning coefficient	-0.02**	-0.01	-0.02*	-0.03*	-0.01
		(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	Pre-zoning mean	0.12	0.10	0.15	0.12	0.15
Observations			3509	1728	1383	855

Note: Standard errors in parentheses and corrected for clustering at the school level. Each model controls for standardized UASDA score in math and Indonesian, gender, age at the time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and sub-district (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. A school has 4 classrooms for grade 8 on average with 31 students in each. There are 6 classrooms for grade 8 on average in public schools and 3 classrooms in private schools. Peer characteristics indicators include the observed student but excluding the observed student does not make a difference to the results (not shown). Students for whom we do not have a UASDA score are included in the peer wealth measure. Note that the UASDA score is the average of the mathematics, Indonesian and science scores and the grade 9 UN score is the average of the mathematics, Indonesian, English and science scores in 2018. * $p < .10$, ** $p < .05$, *** $p < .01$

6.2 Impacts of the zoning policy on learning

The impact results on learning outcomes are presented in Table 6.2. The table shows results by subject and the average of math and Indonesian. Since results are similar between subjects, we mainly report the average result. Effects on test-score value-added for the whole sample declined by 0.08 s.d. (not significant). Yet, as shown above in Table 6.1, students went to schools of somewhat higher quality. This suggests that, on average, schools were hardly able to maintain the same learning production as they were prior to the policy change. The results also demonstrate that the UASDA score is a good predictor of the SLA score. The coefficient of the standardized UASDA score is around 0.6 s.d., which is comparably high; Andrabi et al. (2011) find test score persistence, or coefficients for lagged test scores, of between 0.2 and 0.5 s.d..

Table 6.2 also shows impact results for each of the public school access groups. The "lost access" group saw a 0.24 decline in learning, while the "gained access" group saw an insignificant increase in learning of 0.12 s.d.[30] The effect on the "lost access" group is equal to about one year of learning in public schools for the zoning cohort,[31] although this number should be interpreted carefully since it is of course affected by the policy change. Hence, students who shifted to better schools benefitted somewhat from and at least were not harmed by public school enrolment, at least in the short-run, while students who lost access to public schools were very disadvantaged by losing access to public schools. Yet, based on Table 6.1 we expected to find opposite effects, since the improvement in school quality was larger for the "gained access" group than for the "lost access" group. This expectation is consistent with previous studies that found that students whose default outside options are lower quality could benefit most from enrolment in selective schools (Angrist et al., 2019; Deming et al., 2014).

These heterogeneous effects of public school enrolment suggest that school effects depend on initial student abilities and wealth. The "gained access" and "lost access" students differed on multiple characteristics, as shown in Table A.5. The "gained access" group scored 1.4 s.d. lower on the UASDA exam than the "lost access" group and they had a 0.6 s.d. lower asset index. In addition, there was almost a 40 percentage point difference in the share of mothers who completed tertiary education between the "gained access" and "lost access" students. Hence, our findings suggest that higher scoring, wealthier students benefited more from public schools in terms of test scores. These findings are consistent with Newhouse and Beegle (2006), who found that highest scoring students benefited most from public school enrolment in Indonesia, while lower-scoring students were not harmed by public school enrolment. They are also consistent with studies that found that, even if a student chose the highest-quality school they would be accepted to, selective schools might not be able to produce the same learning for all students across the learning distribution (Bau, 2021; Cullen et al., 2006).[32]

[30] Note that these are Intent-to-Treat estimates. To get the Treatment-on-the-Treated (TOT) estimates, we can divide these coefficients with the change in public school access as reported in Table 5.2 (+59.6 percent in the "gained access" group, and -46.8 percent in the "lost access" group). The TOT would be 0.20 for the "gained access" students and 0.51 for the "lost access" students, showing an even larger difference in effects between the groups.
[31] Only zoning students were tested twice. Between grades 7 and 8, zoning students gained 0.23 s.d. in math and 0.29 s.d. in Indonesian on the SLA test in public schools and -0.06 s.d in math and 0.07 s.d. in Indonesian in private schools.
[32] This is sometimes referred to as mismatch – that the school isn't a good fit for students' experience or abilities. See Sander (2004) for seminal work of or Arcidiacono and Lovenheim (2016) for an overview of the literature.

We also see heterogeneous school effects by initial student ability and wealth when we compare the effect on school quality in terms of pre-zoning school value-added, presented in the school quality section in Table 6.1, and the actual impact results in Table 6.2. We interpret the effect on school quality in terms of pre-zoning school value-added as the expected learning improvement if schools had been able to produce the same learning value-added for the zoning students as for pre-zoning students, i.e. if school value-added remained constant across policies.[33] For the "gained access" students, the learning improvements under pre-zoning value-added would have been 0.40 in math and 0.46 s.d. in Indonesian, while the actual learning improvement was only 0.12 s.d. and insignificant in both subjects. The predicted and actual effects on learning were similar for the "lost access" students, indicating that private schools produced similar learning for the high-scoring "lost access" group as for the pre-zoning, lower-scoring students.

Our findings for the "always access" students demonstrate that public schools could not maintain their pre-zoning learning production even for high-scoring students. The impacts on the "always access" and "never access" groups reflect effects from staying in schools of similar quality while being exposed to a new peer group. For the "always access" group, we found no predicted change in learning since they wouldn't change schools, while they saw a 0.13 s.d. decline in learning. For the "never access" group, we find no benefit of the change in their peers in our impact results. This group saw a change of 0.04 s.d., although it is not significant.

Table 6.2 Impact of first zoning policy for the full sample and public school access group, by subject

	Full sample	Always access	Gained access	Lost access	Never access
Mathematics					
Zoning coefficient	-0.07	-0.13	0.12	-0.22*	0.00
	(0.06)	(0.08)	(0.09)	(0.12)	(0.06)
Std UASDA score	0.58***	0.50***	0.31***	0.48***	0.30***
	(0.02)	(0.05)	(0.03)	(0.06)	(0.07)
Indonesian					
Zoning coefficient	-0.08*	-0.15**	0.12	-0.30***	-0.10
	(0.05)	(0.06)	(0.10)	(0.10)	(0.10)
Std UASDA score	0.46***	0.30***	0.32***	0.22***	0.25***
	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)
Average score across subjects					
Zoning coefficient	-0.08	-0.13*	0.12	-0.24**	-0.04
	(0.06)	(0.08)	(0.09)	(0.11)	(0.07)
Std UASDA score	0.64***	0.55***	0.51***	0.56***	0.45***
	(0.02)	(0.04)	(0.04)	(0.06)	(0.06)
Observations	7475	3509	1728	1383	855

Note: Standard errors in parentheses and corrected for clustering at the school level. Each model controls for gender, age at the time of UASDA exam, an asset index, an indicator for the mother having completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

[33] Technically this interpretation also requires the coefficients for the UASDA score and student characteristics, as specified in Equation 2, to stay constant across policies.

In Table A.9 we present heterogeneous effects by gender, mother's education, and assets. We found a negative effect of about 0.1 s.d. for girls, students whose mother completed tertiary education, and students with an above median asset index in Indonesian but no effect in math. We found no effect on boys, students whose mother completed less than tertiary education, or students with a below median asset index, yet these are the students with initially lower UASDA scores and whose public school enrolment increased (by about 10 percentage points).

6.3 Explaining heterogeneous effects of public school enrolment using survey data

We also used student and teacher survey data discussed in Section 3 to help explain why school learning production varied by students' initial test scores and why "always access" students learned less in public schools under zoning. While we don't have a comprehensive picture of how students and school staff responded to changes in student composition, we have some data on these adaptations. We focused on teacher reports about changes to lesson difficulty which are consistent with student reports; a finding about tracking students in some schools, another kind of teacher adaptation; and a decline in household educational investments in tutoring. The results are presented in Tables 6.3 to 6.5.

Teachers would play a major role in Yogyakarta's response to the zoning policy. If they maintained pre-zoning lesson pace, the "gained access" students could struggle (Duflo et al., 2011). Or if their adaptations were tailored to lower-scoring students, the "always access" students could find the pace or content less challenging and not learn as much as they otherwise would have, even if they remained in the same school. An overwhelming majority of public school teachers (78 percent) reported they changed some kind of teaching or classroom management in response to the new student composition; this figure was 39 percent in private schools (Table 6.3). Similarly, approximately 28 percent of teachers in public schools and 10 percent of teachers in private schools reported they changed lesson difficulty specifically after the policy change. For example, one public school teacher stated, "We used to have high-performing students. Teachers can just give them lessons and assignments, and they could complete the assignment without any difficulties. But it is different right now. Teachers now have to make more preparations in terms of pedagogical skills as well as how to approach students" (January 2018). Likewise, another public school teacher stated, "It is challenging to teach zoning students. I need to work harder to make students understand the lessons. Before zoning, teaching was not that hard [but] after zoning, it is difficult to make students pay attention to their lessons, let alone get them to study" (January, 2021). At the same time, a private school teacher reported: "I only provided more exercise questions; there was no change from previous years." (January 2021). Public school teachers took action to adapt, even if these changes may not have been effective (yet) in markedly improving learning outcomes. It is plausible that a school system could take longer than 18 months to adjust to such a dramatic change in student composition.

In addition to self-reported behavior changes, we find evidence that schools or teachers implemented tracking, i.e. grouping students in classrooms by past performance, in this case by UASDA scores. Such grouping could help teachers buffer the impact of greater school heterogeneity by making classrooms more homogenous, although teachers would still likely have to make some adjustments if they were accustomed to teaching higher-performing students. Approximately 27 percent of grade 8 teachers who taught the zoning cohort (32 percent in public, 23 percent in private) reported their school implemented tracking in response to the zoning policy (Table 6.3). While tracking was implemented in a minority of schools, it happened enough to result in a smaller change in peers at the classroom level than at the school level after the zoning policy (Table 6.1).

Table 6.3 Changes in teaching methods after zoning policy, grade 8 teachers

	Public	Private	Difference
Changed teaching methods after the zoning policy			
	0.78	0.39	0.39***
	(0.42)	(0.49)	[0.08]
Changed difficulty level of tasks after zoning policy			
	0.28	0.10	0.18**
	(0.45)	(0.30)	[0.07]
School implemented tracking in response to the zoning policy			
	0.32	0.23	0.09
	(0.47)	(0.42)	[0.08]
Observations	69	84	

Note: Table includes grade 8 teachers. Standard deviations in parentheses and standard errors between brackets. We test the difference in means with a t-test, correcting standard errors for clustering at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

We suspect that tracking was mostly beneficial to higher-scoring students. Since tracking is an outcome of the zoning policy, we cannot directly estimate how it affected the magnitude of the learning impact of the zoning policy. Arguably, without tracking, the negative effect on the "always access" group would have likely been even larger. In tracked classrooms in public schools with the highest-scoring students, teachers could continue using the teaching methods they were familiar with. At the same time, even with tracking, public schools did not significantly improve learning outcomes for the "gained access" group compared to what they would have learned in private schools. Presumably, public school teachers struggled adapting their methods to low-scoring students. Tracking could have also limited benefits of exposure to higher-scoring peers for the "gained access" group, if these peers were able to have a positive effect on lower-scoring students.

Student reports are also consistent with the finding that public schools adapted lessons more than private schools (Table 6.4). Reported lesson difficulty of the "always access" and "lost access" students was 8 and 6 percentage point lower, respectively, in the zoning cohort than in their pre-zoning counterfactual, while it was 6 percentage points higher for the "gained access" zoning students on the same measure. There was no change for the "never access" group, corroborating teacher reports that private school teachers didn't adapt much.

In addition to some students struggling in a more challenging academic environment, we found that some students' college-going aspirations declined. When asked what the highest education level they aspired to complete was, the share of students in the "gained access" and "never access" group declined by almost 10 percentage points (Table 6.4). Possibly, exposure to higher performing peers in their schools demotivated these students from their low relatively lower performance in the classroom (Denning et al., 2020).

The only evidence we have for parental responses to sending their children to a new schooling environment is a change in investing in tutoring, the primary form of outside educational support in Indonesia. Nationally, in 2018, approximately 60 percent of junior secondary students utilized some form of out-of-school tutoring (Badan Pusat Statistik, 2018). In Yogyakarta, after-school group-based tutoring is common in public and private school and often organized by teachers on school premises.[34] Parents must pay for tutoring – there are no scholarships – and for school-based tutoring, parents generally pool

[34] Some students also took private tutoring classes outside the schools, but we did not find any effects of zoning on this type of tutoring. This suggests that there was no substitution into other forms of tutoring.

funds to pay a teacher such that there is no advertised price, although, at least prior to zoning, tutoring associated with public schools was more expensive than that associated with private schools. Forty-five percent of public school students and 38 percent of private school students in the pre-zoning cohort took tutoring classes.

We found that the share of students in school-based tutoring went down by 9 percentage points after the first zoning policy, with the largest decline in the "gained access" group, which experienced a decline by nearly a half in school-based tutoring (Table 6.4).[35] This suggests that if this group was struggling, the parental response was not tutoring. We didn't interview parents or ask students reasons for tutoring participation, so we cannot know why we see this decline. One potential explanation is that the perceived higher quality of public schools prompted parents to assume they no longer needed tutoring (Das et al., 2013; Pop-Eleches and Urquiola 2013). Another is that although schooling costs went down as families switched from private to public schools, school-based tutoring in public schools was just too expensive for some families because there are no scholarships for tutoring and school-based tutoring is generally slightly more expensive in public school than in private school; tutoring is also usually much lower cost in lower-quality private schools. The average asset index of the "gained access" group is about 0.5 s.d. lower than that of the "always access" group (Table A.5) suggesting that if cost was a factor in accessing tutoring, there was a large variation among public school students' in their ability to pay for it. Regardless of the reason for not taking tutoring classes, the low take-up of tutoring in the "gained access" group might have contributed to the limited impact from public school enrolment on their learning outcomes.

Table 6.4 Change in students' perceptions about schooling difficulty, aspirations, and after-school tutoring practices by predicted public school access

	Full sample	Always access	Gained access	Lost access	Never access
Finds instruction level is difficult					
Zoning coefficient	-0.03*	-0.08***	0.06**	-0.06**	-0.00
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Pre-zoning mean	0.26	0.26	0.27	0.25	0.26
Aspires to go to university					
Zoning coefficient	-0.04***	-0.03*	-0.09***	-0.02	-0.07***
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)
Pre-zoning mean	0.80	0.89	0.64	0.89	0.72
Takes tutoring at school					
Zoning coefficient	-0.09**	-0.07	-0.21***	-0.03	-0.08
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
Pre-zoning mean	0.42	0.41	0.46	0.43	0.37
Observations	7,471	3,507	1,724	1,386	854

Note: Standard errors in parentheses and corrected for clustering at the school level. Each model controls for gender, age at the time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and sub-district (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

[35] The results are similar for analysis by UASDA quintile. The lowest three quintiles saw a decline in school-based tutoring by between 15 and 21 percentage points, and there was no change for the two highest quintiles. In addition, we find no effect on other private additional tutoring classes (the decline in other tutoring classes for the lowest three quintiles is small and insignificant).

Student reports about a decline in tutoring are corroborated by teacher reports. The share of public school teachers reporting that they conducted tutoring over the school year declined by 35 percentage points (significant at the 1 percent level) after zoning, amounting to about 51 fewer minutes of tutoring per week (Table 6.5). Among private school teachers, tutoring went down by 14 percentage points or about 21 minutes per week (not significant).

Table 6.5 Changes in self-reported tutoring of teachers in public and private schools

	Public			**Private**		
	Pre-zoning	Zoning 1	Difference	Pre-zoning	Zoning 1	Difference
Tutoring outside teaching hours						
	0.70	0.35	-0.35***	0.63	0.49	-0.14
	(0.46)	(0.48)	[0.11]	(0.49)	(0.50)	[0.09]
Tutoring minutes per week						
	99.30	48.41	-50.89**	64.53	43.57	-20.96
	(114.48)	(91.79)	[20.79]	(80.08)	(58.04)	[12.52]
Observations	60	69	129	81	84	165

Note: Table includes grade 8 teachers. Standard deviations in parentheses and standard errors between brackets. We test the difference in means with a t-test, correcting standard errors for clustering at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

6.4 The second zoning policy

As discussed in Section 4, we conducted an impact analysis related to the second zoning policy primarily to support our claim that one can interpret impacts of the policy change as causal. While the six-month timeframe over which we have learning data is possibly too short a period to allow schools to readjust to the policy change, we can show how the student composition and initial learning changed as a result of the second policy. Because the second zoning policy partially reversed the first policy only a year after the first policy was enacted, reallocating more seats to higher scoring students again (Figure 2.2), we expected this partial reversal to result in opposite signs for the impact coefficients for the first and second zoning cohorts.

It was complicated to replicate the second zoning policy with four public school access groups for students who attended primary school in Yogyakarta because we cannot identify the “special talent” students (10 percent of public school seats) and because location data is missing for 23 percent of students, even after imputations with primary school location. Therefore, we show these results by UASDA quintile instead of the groups specified in Table 5.2. We estimated the same value-added model, as specified in Equation 1, to compare test score value added between the first zoning and the second zoning cohorts within each quintile. We present a balance test between the first and second zoning cohort in Table A.3. The cohorts were similar in terms of UASDA scores and wealth. We cannot directly compare the second zoning cohort to the pre-zoning cohort because the pre-zoning cohort was only tested in grade 8.

Before showing the impact results in Figure 6.3, we confirmed that public school enrolment indeed reversed with the second zoning policy. Table 6.6 shows the share of each UASDA quintile enrolled in public school by cohort. Recall, as shown in Table 4.1, that the highest-scoring UASDA quintile saw only a small 12 percentage point decline in public school enrolment, while the lowest-scoring quintile saw a 45 percentage point increase in public school enrolment after the first zoning policy. Table 6.6 shows that the highest-scoring UASDA quintile saw an increase in public school enrolment again with the second zoning policy, from 77 to 80 percent enrolled in public school, and the lowest-scoring UASDA quintile saw a 27 percentage point decrease, about half the size of the public school enrolment change between the pre-zoning and first zoning policy.

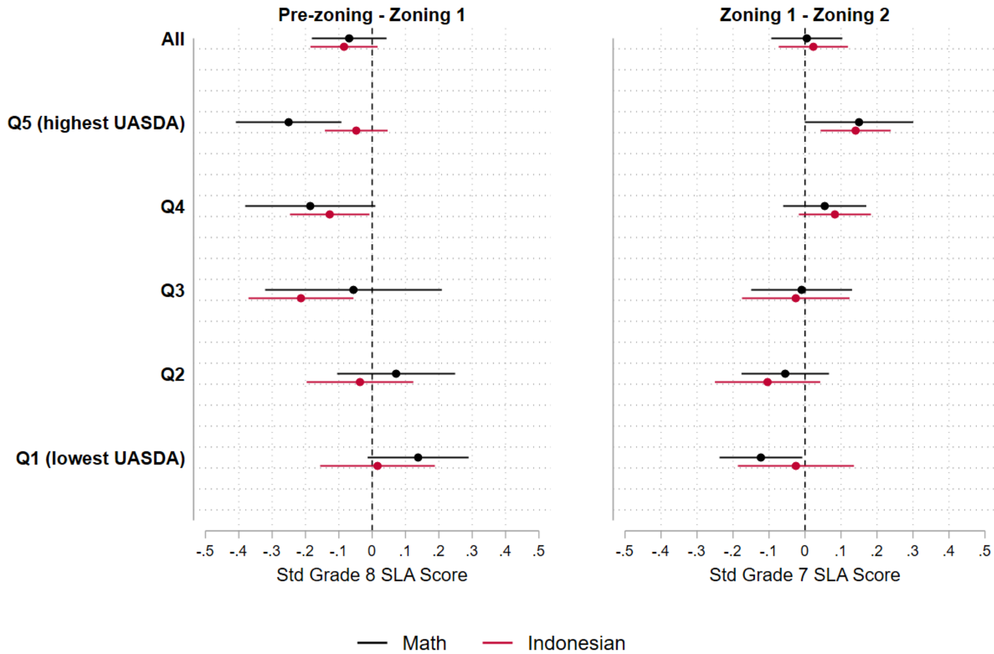
Table 6.6 Percent of students enrolled in public school by UASDA quintile and policy type

	Percent in public school		
	Pre-zoning	**Zoning 1**	**Zoning 2**
Total	58.5	65.3	60.8
Quintile 1 (lowest)	13.9	58.5	31.1
Quintile 2	43.4	60.0	50.6
Quintile 3	69.1	63.1	63.4
Quintile 4	80.6	68.8	76.2
Quintile 5 (highest)	89.3	76.5	80.2

Note: Table includes UASDA graduates who enrolled in sampled junior secondary schools. This mechanically overestimates the share of students enrolled in public school as all 16 public schools are sampled but 30 out of 41 private schools. Numbers are corrected for under-sampling of private schools using sampling weights. The Pre-zoning and 'Zoning 1' columns are the same as in Table 4.1.

In the right-hand panel of Figure 6.3, we show impact estimates for the second zoning policy, six months after the second policy was enacted. Comparing the two panels in Figure 6.3, we find a "bounce back" effect, even though the second zoning policy was not a complete reversion to the pre-zoning policy and the results for the second zoning policy are only after 6 months (compared to after 18 months for the first zoning policy). The negative impacts of the first zoning policy were mostly concentrated in the top three quintiles and more positive effects with the bottom two quintiles, which saw the largest increase in public school enrollment, especially in math. Yet, between the first and second zoning cohorts, grade 7 SLA scores for the top quintile increased by 0.15 s.d. in mathematics and grade 7 SLA scores in mathematics for the bottom quintile decreased with 0.12 s.d. These results demonstrate that the effects are driven by the different student allocation mechanisms.

Figure 6.3 Impact of the two zoning policies on test scores by UASDA quintile



Note: We cannot compare the three groups directly since, as shown in Table 3.1, the pre-zoning cohort was not tested in grade 7 and the second zoning cohort was not tested in grade 8. Figure presents the estimated coefficients and 95 percent confidence intervals for β_1 in Equation 1 (Section 5). Standard errors are corrected for clustering at the school level. Each model controls for gender, age at time of UASDA exam, asset index, an indicator for whether the child's mother completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights.

6.5 Robustness checks

Our results are robust against several different specifications. First, the impact results were similar without control variables, see Table A.7. This is not surprising, as we showed before that the pre-zoning and zoning cohorts were similar in terms of their background characteristics.

Second, the impact results were similar when we applied inverse probability weighting (IPW) (see Table A.8). As mentioned before and as shown in Table A.5, the zoning cohort scored lower on the UASDA exam than the pre-zoning cohort in the "always access," "lost access," and "never access" groups. As an alternative correction for this selection to conditioning on the UASDA score, we weighted the observations with the inverse of the estimated probability to be in the first zoning cohort based on background characteristics. We estimated the propensity to be in the first zoning cohort using a logit model that included the KMS participation, the standardized mean UASDA score, the kelurahan, the asset index, gender, age at the time of the UASDA exam, and whether the student's mother completed tertiary education. All variables were interacted with an indicator for a missing value, such that all students with a non-missing UASDA score were included in the model. We show in Table A.5 that the weighted means were no longer significantly different between cohorts.[36] Yet, as shown in Table A.8, the IPW correction

[36] After weighting the observations, the distance to the closest public school for the "always access" and "gained access" groups is still significantly different between the cohorts. Within these groups, students in the first zoning

made little difference to our impact results, confirming that conditioning on the UASDA score in our main model sufficiently captured the selection effect (explained in Section 5 and shown in Section 6).

We also estimated our model for all students who took the SLA but included an indicator for missing UASDA score in the model, effectively including students from outside Yogyakarta who enrolled in the junior secondary schools in Yogyakarta. Note that we can only estimate the average impact on these students, because we cannot determine their public school access under each policy without their UASDA score. The results are very similar to our main results, only now the average decline in learning between the pre-zoning and the zoning cohort was significant (Table A.10). The coefficients for the missing UASDA score indicator were zero in all models, demonstrating that this group of students is not different from the sample used in the analysis in terms of the SLA scores, conditional on background characteristics. The results suggest that the students who graduated from primary schools outside Yogyakarta but enrolled in junior secondary schools in Yogyakarta also experienced a decline in learning after the zoning policy.

Finally, our results are also robust against different assumptions about the data used to determine the public school access groups. They were similar when assuming that students preferred the highest ranking SMP instead of the closest SMP, and when assuming that students with missing location data lived far away instead of imputing using the location of their primary school (not shown).

7. Discussion and conclusion

We presented evidence of impacts of an admissions policy change that dramatically altered student composition, mainly in terms of average achievement scores, in public and private schools in Yogyakarta, Indonesia. The mean UASDA rank of students enrolled in public schools went down by about 0.4 s.d. in math and in Indonesian in the zoning cohort compared to the pre-zoning cohort; we saw the opposite effect in private schools (Table 4.2). The zoning policy expanded public school access for students previously excluded from the public school system, as the share of students who scored in the lowest quintile on the UASDA enrolled in public schools increased from 14 to 59 percent (Table 4.1).[37] Two important contributions of our paper are that (1) we examine effects on student learning when compositional changes are large; and (2) we are able to estimate impacts not just for those who gained entry to public schools, but also students who were displaced to lower-quality private schools and students exposed to a new peer group in public and private schools.

We found that while the overall impact of zoning was slightly negative – an insignificant decline of 0.08 s.d. averaged across Indonesian and math – some groups experienced large, negative impacts (Table 6.2). In public schools, compositional changes triggered behavioral responses (either by teachers or peers) that hindered learning for "always access" students, who saw an average 0.13 s.d. decline. This could be in part because, as shown in our survey results, teachers attempted to adjust their teaching strategy to accommodate "gained access" students. One public school teacher illustrated this shift: "Now, we at [school name], we have very moderate expectations. We do not aim to achieve high scores. Three years ago, we scored the highest in Yogyakarta. We even ranked high nationally. That was our target. But now we changed the target. Given the low score of our incoming students, we just would like to improve their

cohort lived somewhat closer to public schools on average than the pre-zoning cohort (about 400 meters). This could be a result of having to impute location data for the pre-zoning cohort. Since we rank students within their cohort, and cohorts are balanced within groups on other characteristics, we do not think this distance difference would affect our results.

[37] Because the changes in student composition in terms of assets were much smaller than changes in terms of test scores, our findings should be interpreted as impacts of school integration based on academic background.

performance. We no longer think about getting ranked among the highest scoring schools in the national examination" (January 2021). Some public schools started tracking students by ability to buffer impacts of the new classroom heterogeneity, but this response was not sufficient given the magnitude of the change in student composition and the fact that teachers received no preparation to confront this change.

In contrast, we found that teachers in private schools made little to no behavioral changes, and thus they did not adapt to an influx of higher-scoring students. This is evidenced by the fact that "lost access" students experienced the greatest harm from zoning. Their learning declined by an average 0.24 s.d., while the "never access" group experienced no change in learning, further supporting the premise that public schools kept business as usual.

Impacts for "gained access" students were positive (0.12 s.d. but insignificant) and much smaller than expected based on predictions using pre-zoning school value-added (0.4 s.d., significant). Perhaps expected gains weren't realized because public schools were not able to, at least in the short-run, well-serve the "gained access" students. This group seemed to have struggled, as there was a 22 percent difference in the share of students in this group who considered instruction levels difficult compared to their comparison group in private schools. Of course, difficulty could have been positive if it had resulted in improved learning; but students struggled with only little benefit. In addition, the "gained access" students did not take tutoring classes that seemed to be an important aspect of public schools: the share of these students who took tutoring was half the share of their comparison group in private schools and half the share of the "always access" students in public schools. This suggests that parents may have thought that access to more desired schools meant they needed to invest less in outside support for students; or that school-based tutoring at public schools was just too expensive for this group.

The somewhat somber results from the large change in student composition in Yogyakarta have two main implications for policies that seek to promote integration. First, one cannot assume constant school value-added for students of similar backgrounds when student composition changes, especially if the change is large. Recent studies in Pakistan aimed to predict how redistributing students across schools could improve learning outcomes when school value-added differs across student types but is unchanging with student composition (Andrabi et al., 2020; Bau, 2021). This assumption was based on a small share of students changing schools and on findings from other key studies that found small or no impact on incumbent students from integration (Rao, 2019; Muralidharan and Sundararaman, 2015; Imberman et al., 2012; Angrist and Lang, 2004). In contrast, we found negative impacts for incumbent students. This discrepancy could have been due to the fact that in Yogyakarta the share of incoming students to public schools was much larger and/or that the incoming-incumbent test score gap was larger than in other contexts. However, our study *did* show constant school value-added in private schools. There were no positive effects to lower-scoring peers from higher-scoring peers joining private schools. This finding emphasizes that constant school value-added is possible, but not a given and is context dependent.

Second, policy should consider that diversifying the student body in terms of test scores can have negative effects on students other than the primary target beneficiaries. In Yogyakarta, students who lost access to public school lost substantially more learning than the students who gained access to public schools gained. If a system seeks to promote fairness by allowing students with lower test scores access to high-quality public schools, and the only alternative is lower-quality private schools, those displaced to private schools will likely see a decline in learning. Therefore, it is important to consider (1) who the policy is supposed to benefit; and (2) how all actors in the system, especially teachers and students, might react to a new student composition and reallocation. A policy strategy that would more likely result in learning for all groups is improving the quality of schools more broadly and ensuring that receiving schools are a good fit for receiving students, rather than changing selection criteria for high-performing schools.

A final consideration for policy is that we caution against residency-based admissions policies as they could in the long run encourage greater residential segregation (Abdulkadiroğlu and Andersson, 2022). In the case of Yogyakarta, lower-scoring students gained access to preferred schools, but it is unclear how sustainable this would have been had zoning endured. While the UASDA-asset index correlation in Yogyakarta was not strong, it is in many other cities in Indonesia and throughout the world. Residency-based admissions usually leads to wealthier parents eventually moving closer to the most preferred schools to ensure enrolment for their children (Black, 1999). Although Yogyakarta reverted to largely test-based admissions in 2019, zoning remains a policy option for tens of other cities throughout Indonesia that are much more segregated by income and test scores than Yogyakarta. As local and national education policymakers consider supporting zoning, they should also consider how it could promote greater residential and school segregation, along with how it could affect learning.

In closing, we stress several caveats when interpreting our results. Our study timeframe was a brief 18 months, and schools might need more time to adjust to such a substantial change. In public schools, even though the estimates for the "gained access" group were insignificant, they were positive (0.12 s.d.) and the benefits could grow over time; and the negative effects for the "always access" group could decline, as teachers become more comfortable with the new student body. Teachers in Yogyakarta were not given any training or support before the zoning policy was enacted and began to ask for training throughout the school year. Finally, our study examined only one narrow primary outcome (test scores), while benefits of selective schools might materialize for other outcomes, such as non-cognitive skills, university enrolment, wages or other aspects of human capital (Jackson et al., 2020; Anstreicher et al., 2022). Future studies would benefit from considering a wider range of outcomes over a longer timeframe.

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APPENDIX TABLES AND FIGURES

Table A.1. Difference in value-added between public and private schools by cohort

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Grade 8 SLA Score				Grade 7 SLA Score			
	Pre-zoning		Zoning 1		Zoning 1		Zoning 2	
	Math	Indonesia n	Math	Indonesia n	Math	Indonesian	Math	Indonesia n
Public School	0.37***	0.47***	0.40***	0.45***	0.17	0.11	0.23***	0.30***
	(0.12)	(0.09)	(0.13)	(0.15)	(0.11)	(0.08)	(0.08)	(0.08)
Std UASDA	0.54***	0.34***	0.48***	0.43***	0.47***	0.51***	0.55***	0.50***
	(0.05)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Observations	3833	3858	3642	3650	3774	3825	4082	4096

Standard errors in parentheses and corrected for clustering at the school level. Each model controls for gender, age at time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.2. Correlation between UASDA score and distance to closest public school by cohort

	(1) Average UASDA score	(2) Math UASDA score	(3) Indonesian UASDA score
Distance to closest public school	0.02	0.02	-0.00
	(0.02)	(0.02)	(0.02)
Second zoning cohort	0.09	0.09	0.08
	(0.07)	(0.07)	(0.06)
First zoning cohort	-0.08	-0.06	-0.07
	(0.11)	(0.12)	(0.08)
Second zoning cohort # Distance to closest public school	-0.06	-0.08	-0.04
	(0.04)	(0.05)	(0.03)
First zoning cohort # Distance to closest public school	-0.05	-0.06	-0.03
	(0.04)	(0.05)	(0.04)
Observations	11517	11517	11517

Average UASDA score is the unweighted average score in math, Indonesian and science. Standard errors in parentheses and corrected for clustering at the school level. Each model controls for gender, age at time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.3. Balance between cohorts

	(1) Pre-zoning	(2) Zoning 1	(3) Zoning 2	(4) Diff c1-c2	(5) Diff c2-c3
Mathematics standardized UASDA score	0.05	-0.01	0.02	-0.06	0.02
	(1.00)	(0.98)	(0.97)	[0.10]	[0.07]
Indonesian standardized UASDA score	0.06	-0.00	0.04	-0.06	0.04
	(0.98)	(0.95)	(0.94)	[0.07]	[0.06]
Male	0.49	0.49	0.50	0.00	0.01
	(0.50)	(0.50)	(0.50)	[0.01]	[0.01]
KMS participation	0.34	0.32	0.34	-0.02	0.02
	(0.47)	(0.47)	(0.47)	[0.02]	[0.02]
Student asset index	0.03	0.01	0.01	-0.02	-0.00
	(1.03)	(0.99)	(1.00)	[0.04]	[0.03]
Mother completed tertiary education	0.46	0.49	0.47	0.02	-0.02
	(0.50)	(0.50)	(0.50)	[0.02]	[0.01]
Observations	3903	3967	4130		

Note: Table includes data from students with non-missing UASDA and SLA scores. Standard deviations are in parentheses and standard errors in brackets. Numbers are corrected for under-sampling of private schools using sampling weights. Gender, mother's education, and household assets were reported by the students tested. We use tertiary education for mothers because such a high share of students' mothers had completed this education level. The number of observations is slightly different for mother's education due to students not knowing their mother's education level (n=3,122 in the pre-zoning cohort, n=2,968 in the first zoning cohort, n=2,953 in the second zoning cohort). KMS participation data is non-missing for 78.3 percent of the pre-zoning cohort, 67.3 percent of the first zoning cohort, and 71.3 percent of the second zoning cohort. We test the difference in means with a t-test, correcting standard errors for clustering at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.4. Difference between students for whom we do and do not have a UASDA score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pre-zoning cohort			First zoning cohort			Second zoning cohort		
	Missing UASDA	Non-missing UASDA	Diff	Missing UASDA	Non-missing UASDA	Diff	Missing UASDA	Non-missing UASDA	Diff
Grade 8 SLA score - math	-0.10	-0.06	0.05	-0.32	-0.15	0.17**			
	(1.02)	(1.01)	(0.55)	(1.02)	(0.99)	(0.03)			
Grade 7 SLA score - math				-0.07	-0.01	0.06	-0.07	-0.01	0.07
				(1.02)	(1.00)	(0.39)	(1.12)	(1.03)	(0.50)
Grade 8 SLA score - Indonesian	-0.07	-0.04	0.02	-0.36	-0.15	0.21**			
	(1.03)	(1.01)	(0.74)	(1.13)	(1.00)	(0.03)			
Grade 7 SLA score - Indonesian				-0.09	-0.01	0.08	-0.03	0.03	0.06
				(1.08)	(1.00)	(0.46)	(1.09)	(1.02)	(0.49)
Public school	0.36	0.58	0.23***	0.32	0.65	0.33***	0.30	0.61	0.31***
	(0.48)	(0.49)	(0.00)	(0.47)	(0.48)	(0.00)	(0.46)	(0.49)	(0.00)
Male	0.52	0.49	-0.03	0.52	0.49	-0.03	0.51	0.50	-0.00
	(0.50)	(0.50)	(0.25)	(0.50)	(0.50)	(0.12)	(0.50)	(0.50)	(0.93)
Std asset index	0.27	-0.06	-0.33***	0.17	-0.08	-0.25***	0.18	-0.08	-0.26***
	(1.02)	(1.02)	(0.00)	(0.98)	(0.98)	(0.00)	(0.97)	(0.99)	(0.00)
KMS participant	0.25	0.34	0.09**	0.22	0.32	0.10***	0.23	0.34	0.11***
	(0.43)	(0.47)	(0.01)	(0.41)	(0.47)	(0.01)	(0.42)	(0.47)	(0.00)
Mother completed tertiary education	0.62	0.46	-0.16***	0.58	0.49	-0.09***	0.59	0.47	-0.12***
	(0.48)	(0.50)	(0.00)	(0.49)	(0.50)	(0.01)	(0.49)	(0.50)	(0.00)
Lives outside Yogyakarta	0.84	0.23	-0.61***	0.76	0.20	-0.56***	0.79	0.21	-0.59***
	(0.37)	(0.42)	(0.00)	(0.42)	(0.40)	(0.00)	(0.41)	(0.40)	(0.00)
Distance to closest public school (km)	4.03	1.28	-2.75***	3.70	0.85	-2.84***	4.01	1.06	-2.95***
	(4.06)	(1.61)	(0.00)	(4.48)	(1.58)	(0.00)	(4.39)	(1.65)	(0.00)
Minutes to school	22.13	18.21	-3.92***	17.86	14.58	-3.27***	18.94	15.63	-3.31***
	(16.16)	(13.24)	(0.00)	(13.14)	(10.68)	(0.00)	(14.37)	(12.59)	(0.00)
Moved in grade 6	0.19	0.12	-0.07***	0.18	0.11	-0.07***	0.15	0.09	-0.07***
	(0.40)	(0.33)	(0.00)	(0.38)	(0.31)	(0.00)	(0.36)	(0.28)	(0.00)
Observations	1,578	3,903	5,481	1,747	3,967	5,714	1,468	4,130	5,598

With UASDA we mean the UASDA exam in primary school in Yogyakarta. Standard deviations are in parentheses. We test the difference in means with a t-test where standard errors are correct for clustering at the school level. Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.5. Balance between cohorts within groups by public school access

	Always Access				Gained Access			
	PZ	Z1	Diff	IPW Diff	PZ	Z1	Diff	IPW Diff
Std UASDA (math, Indonesian and science)	0.78	0.65	-0.13**	-0.01	-1.02	-1.06	-0.05	0.01
	(0.57)	(0.65)	[0.06]	[0.06]	(0.59)	(0.47)	[0.06]	[0.06]
Std UASDA Math	0.76	0.61	-0.16**	-0.04	-0.99	-0.95	0.04	0.08
	(0.65)	(0.78)	[0.07]	[0.07]	(0.54)	(0.55)	[0.05]	[0.06]
Std UASDA Indonesian	0.63	0.58	-0.05	0.04	-0.80	-0.91	-0.11	-0.04
	(0.68)	(0.65)	[0.05]	[0.05]	(0.86)	(0.70)	[0.08]	[0.09]
Distance to closest public school	1.00	0.47	-0.53***	-0.42***	0.73	0.32	-0.41***	-0.35***
	(1.33)	(0.95)	[0.12]	[0.08]	(0.68)	(0.44)	[0.04]	[0.04]
Male	0.44	0.45	0.01	0.01	0.55	0.56	0.02	0.01
	(0.50)	(0.50)	[0.02]	[0.02]	(0.50)	(0.50)	[0.02]	[0.02]
KMS participant	0.31	0.32	0.01	-0.00	0.46	0.46	-0.00	0.02
	(0.46)	(0.47)	[0.05]	[0.04]	(0.50)	(0.50)	[0.04]	[0.04]
Std Asset Index	0.19	0.12	-0.07	-0.01	-0.37	-0.43	-0.06	0.01
	(0.99)	(0.96)	[0.05]	[0.05]	(1.03)	(0.93)	[0.11]	[0.15]
Mother completed tertiary education	0.53	0.55	0.02	-0.01	0.28	0.26	-0.02	-0.02
	(0.50)	(0.50)	[0.02]	[0.02]	(0.45)	(0.44)	[0.04]	[0.05]
Observations	1815	1903			1006	918		

	Lost Access				Never Access			
	PZ	Z1	Diff	IPW Diff	PZ	Z1	Diff	IPW Diff
Std UASDA (math, Indonesian and science)	0.54	0.32	-0.22***	-0.05	-0.88	-1.07	-0.19***	0.09
	(0.41)	(0.44)	[0.05]	[0.05]	(0.52)	(0.57)	[0.05]	[0.08]
Std UASDA Math	0.55	0.28	-0.27***	-0.08	-0.92	-0.97	-0.05	0.15**
	(0.52)	(0.62)	[0.06]	[0.06]	(0.53)	(0.58)	[0.05]	[0.06]
Std UASDA Indonesian	0.42	0.29	-0.13**	-0.01	-0.66	-0.95	-0.29***	-0.02
	(0.63)	(0.57)	[0.06]	[0.06]	(0.79)	(0.77)	[0.06]	[0.06]
Distance to closest public school	2.21	1.95	-0.27	-0.28	1.96	1.67	-0.30*	-0.03
	(2.28)	(2.49)	[0.23]	[0.24]	(1.79)	(1.91)	[0.15]	[0.16]
Male	0.46	0.44	-0.02	-0.02	0.60	0.60	-0.01	-0.00
	(0.50)	(0.50)	[0.02]	[0.03]	(0.49)	(0.49)	[0.02]	[0.04]
KMS participant	0.22	0.16	-0.06	0.00	0.35	0.31	-0.04	-0.00
	(0.41)	(0.37)	[0.04]	[0.04]	(0.48)	(0.46)	[0.05]	[0.06]
Std Asset Index	0.22	0.30	0.08	0.01	-0.01	-0.10	-0.09	0.01
	(0.92)	(0.90)	[0.09]	[0.09]	(1.00)	(1.09)	[0.11]	[0.10]
Mother completed tertiary education	0.56	0.65	0.09	-0.02	0.40	0.32	-0.08	0.01
	(0.50)	(0.48)	[0.05]	[0.05]	(0.49)	(0.47)	[0.04]	[0.03]
Observations	788	703			471	479		

Note: Table includes students with non-missing UASDA and SLA score. Weighted differences (IPW) are weighted with the inverse of the propensity to be in the first zoning cohort. Standard deviations are in parentheses and standard errors between brackets. Numbers are corrected for under-sampling of private schools using sampling weights. We test the difference in means with a t-test where standard errors are correct for clustering at the school level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.6. Effect on peer wealth in terms of KMS participation and mother's education

Outcome variable		(1) **Full sample**	(2) **Always access**	(3) **Gained access**	(4) **Lost access**	(5) **Never access**
Peer characteristics – School						
Share KMS participants	Zoning coefficient	-0.01	0.04*	-0.03	-0.07***	-0.07*
		(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
	Pre-zoning mean	0.32	0.28	0.41	0.27	0.33
Share whose mothers completed tertiary education	Zoning coefficient	0.02	-0.02	0.03	0.09***	0.03
		(0.01)	(0.02)	(0.03)	(0.02)	(0.04)
	Pre-zoning mean	0.48	0.55	0.36	0.51	0.44
Peer characteristics – Classroom						
Share KMS participants	Zoning coefficient	-0.02	0.03	-0.01	-0.08**	-0.07*
		(0.03)	(0.02)	(0.04)	(0.03)	(0.04)
	Pre-zoning mean	0.31	0.26	0.42	0.25	0.33
Share whose mothers completed tertiary education	Zoning coefficient	0.01	-0.00	-0.01	0.10***	-0.00
		(0.01)	(0.02)	(0.03)	(0.02)	(0.03)
	Pre-zoning mean	0.48	0.55	0.34	0.52	0.44
Observations		7475	3509	1728	1383	855

Note: Standard errors in parentheses and corrected for clustering at the school level. Each model controls for standardized UASDA score in math and Indonesian, gender, age at the time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and sub-district (and indicators for missing values in these controls). Students for whom we do not have a UASDA score are included in the peer wealth measures. Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.7. Effect by public school access without additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		Always access		Gained access		Lost access		Never access	
	Math	Indo	Math	Indo	Math	Indo	Math	Indo	Math	Indo
Zoning dummy	-0.06	-0.09	-0.14	-0.16**	0.13	0.14	-0.21	-0.30**	-0.02	-0.17*
	(0.06)	(0.06)	(0.09)	(0.07)	(0.10)	(0.12)	(0.13)	(0.12)	(0.07)	(0.09)
Standardized UASDA score	0.62***	0.52***	0.58***	0.41***	0.32***	0.39***	0.51***	0.28***	0.31***	0.32***
	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.06)	(0.04)	(0.07)	(0.05)
Observations	7517	7550	3527	3538	1739	1753	1392	1395	859	864

Note: 'Indo' stands for Indonesian. Standard errors in parentheses and corrected for clustering at the school level. Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.8. Effect by public school access with weighting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		Always access		Gained access		Lost access		Never access	
	Math	Indo	Math	Indo	Math	Indo	Math	Indo	Math	Indo
Zoning dummy	-0.05	-0.06	-0.10	-0.13**	0.14	0.15	-0.15	-0.25**	0.03	-0.05
	(0.05)	(0.05)	(0.09)	(0.06)	(0.09)	(0.11)	(0.13)	(0.12)	(0.06)	(0.14)
Standardized UASDA score	0.56***	0.46***	0.52***	0.29***	0.31***	0.31***	0.49***	0.19***	0.29***	0.30***
	(0.02)	(0.03)	(0.05)	(0.03)	(0.03)	(0.04)	(0.07)	(0.04)	(0.06)	(0.07)
Observations	7475	7508	3509	3520	1728	1742	1383	1386	855	860

Note: 'Indo' stands for Indonesian. Standard errors in parentheses and corrected for clustering at the school level. Observations are weighted with the inverse of the propensity to be in the first zoning cohort. Each model controls for gender, age at the time of UASDA exam, an asset index, an indicator for the mother having completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.9. Heterogeneous effects by baseline characteristics – full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Gender				Mother's Education				Asset Index			
	Male		Female		Tertiary		Below tertiary		Above median		Below median	
	Math	Indo	Math	Indo	Math	Indo	Math	Indo	Math	Indo	Math	Indo
Zoning	-0.04	-0.09	-0.09	-0.08*	-0.06	-0.10**	-0.07	-0.04	-0.08	-0.14***	-0.05	-0.03
	(0.07)	(0.07)	(0.06)	(0.04)	(0.06)	(0.05)	(0.07)	(0.06)	(0.06)	(0.05)	(0.07)	(0.06)
Observations	3594	3616	3881	3892	2732	2736	3071	3089	3792	3805	3683	3703
Mean UASDA score	-0.04	-0.10	0.18	0.24	0.44	0.45	-0.09	-0.09	0.31	0.32	-0.17	-0.18
Public school enrolment – Pre-zoning	0.54		0.63		0.60		0.64		0.57		0.61	
Public school enrolment – Zoning 1	0.64		0.66		0.59		0.75		0.57		0.74	

Note: 'Indo' stands for Indonesian. Standard errors in parentheses and corrected for clustering at the school level. Each model controls for gender, age at the time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.10. Differences between cohorts estimated on all enrolled students

	(1)	(2)	(3)	(4)
	Pre-zoning – Zoning 1		**Zoning 1 – Zoning 2**	
	Std Grade 8 SLA score		Std Grade 7 SLA score	
	Math	Indonesian	Math	Indonesian
Zoning	-0.12**	-0.15***	0.01	0.05
	(0.05)	(0.04)	(0.05)	(0.05)
Std UASDA Score	0.56***	0.43***	0.52***	0.51***
	(0.02)	(0.02)	(0.02)	(0.02)
Missing UASDA score	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	10557	10599	10604	10690

Note: Table includes all students who took the SLA test. Standard errors in parentheses and corrected for clustering at the school level. Each model controls for gender, age at the time of the UASDA exam, an asset index, an indicator for the mother having completed tertiary education and kelurahan (and indicators for missing values in these controls). Numbers are corrected for under-sampling of private schools using sampling weights. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.11. Teacher Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		All			**Public**			**Private**	
	PZ	Z1	Diff	PZ	Z1	Diff	PZ	Z1	Diff
Male	0.31	0.27	-0.04	0.32	0.20	-0.11***	0.31	0.33	0.02
	(0.46)	(0.45)	(0.03)	(0.47)	(0.41)	(0.04)	(0.46)	(0.47)	(0.04)
Age	41.72	41.91	0.19	47.47	45.46	-2.00	37.46	38.99	1.53
	(12.63)	(12.71)	(1.01)	(11.24)	(12.08)	(1.58)	(11.94)	(12.54)	(1.14)
University degree	0.98	0.99	0.01	1.00	1.00	0.00	0.96	0.99	0.03
	(0.14)	(0.08)	(0.01)	(0.00)	(0.00)	(0.00)	(0.19)	(0.11)	(0.02)
Certified Teacher	0.57	0.57	-0.01	0.83	0.77	-0.07	0.38	0.40	0.02
	(0.50)	(0.50)	(0.04)	(0.38)	(0.43)	(0.06)	(0.49)	(0.49)	(0.04)
Years of experience	16.98	17.35	0.37	22.13	20.75	-1.38	13.16	14.56	1.40
	(12.21)	(12.65)	(0.97)	(10.98)	(12.18)	(1.48)	(11.72)	(12.41)	(1.18)
Civil servant teacher	0.40	0.39	-0.01	0.78	0.72	-0.06	0.12	0.12	-0.00
	(0.49)	(0.49)	(0.03)	(0.42)	(0.45)	(0.06)	(0.33)	(0.33)	(0.03)
Permanent teacher	0.23	0.28	0.05	0.02	0.01	-0.00	0.40	0.50	0.10**
	(0.42)	(0.45)	(0.03)	(0.13)	(0.12)	(0.02)	(0.49)	(0.50)	(0.05)
Has NUPTK	0.66	0.71	0.05	0.83	0.83	-0.01	0.53	0.61	0.08
	(0.48)	(0.46)	(0.04)	(0.38)	(0.38)	(0.05)	(0.50)	(0.49)	(0.06)
Average monthly salary from teaching (IDR)	3,679,695	3,794,482	114,787	5,537,623	5,387,000	-150,622	2,326,389	2,486,341	159,952
	(2,978,714)	(3,028,119)	(214,887)	(2,839,592)	(2,922,715)	(397,607)	(2,273,851)	(2,440,015)	(212,492)
Observations	141	153	294	60	69	129	81	84	165

Figure A.1. Proportion of students who participated in KMS (poor) by kelurahan

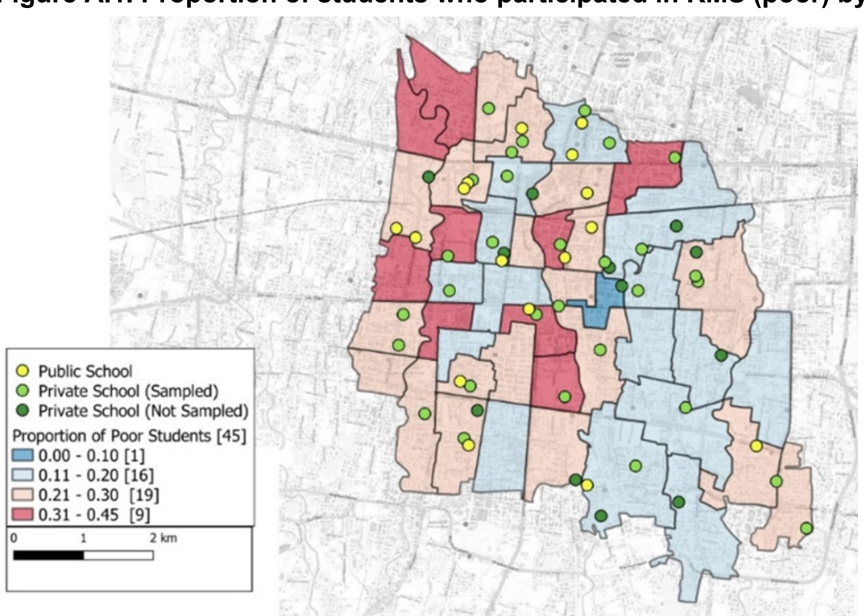
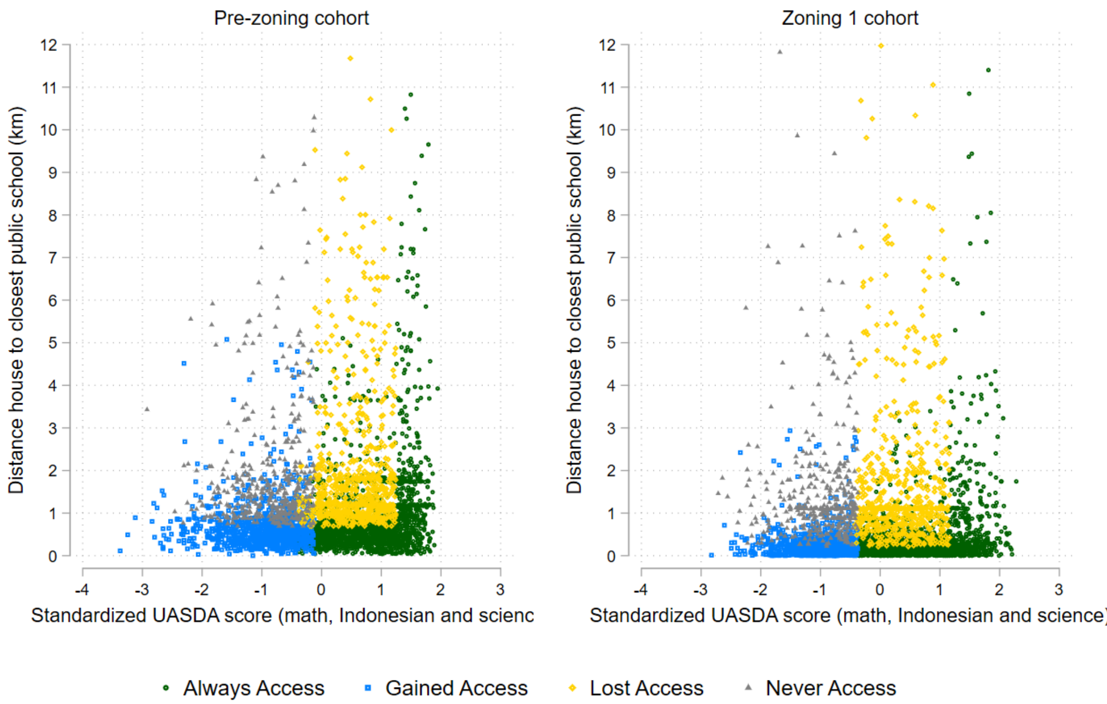


Figure A.2. Result of group simulation by UASDA score and distance to closest public school



Note: There is overlap in the distance to the closest public school between those who had public school access under the zoning policy (always and gained) and those who did not have that (lost and never). This is because some kids lived close to public schools with limited seats and where many students live, while others lived a bit further from a public school with more seats or less close-living students.

Figure A.3. Estimated pre-zoning school value-added in Indonesian by school type