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Moving opportunities: The impact of mixed-income public housing regenerations on student achievement

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ABSTRACT

I use mixed-income public housing regenerations in London as a natural experiment to identify how schools affect low-income students' educational achievement when affluent households flow into their neighborhood. I compare student achievement in schools in the same neighborhood located at different distances from a regeneration before and after its completion. I employ a grandfathering instrument for enrollment in treated schools to address potential endogenous mobility. Students exposed to regenerations have higher test scores at the end of primary school. I estimate that schools explain 65–81% of the overall achievement effects, which are mediated by changes in the student body's composition.

1. Introduction

Neighborhoods play a substantial role in shaping child development and adult outcomes, and children living in high-poverty areas are the most affected. Moving from a high-poverty to a lower-poverty neighborhood improves children's future outcomes, such as school dropout rates, college attendance, earnings, and employment (Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018; van Dijk, 2019; Laliberté, 2021). The timing of relocation is crucial, as neighborhoods seem to affect later life outcomes mainly through childhood exposure.

The specific driving forces of these effects are still unclear. Educational inputs during childhood, and school quality in particular, have a long-lasting impact (e.g., Heckman, 2006; Deming, 2009; Chetty et al., 2011). However, empirical evidence is scarce on the role of school inputs in explaining changes in later outcomes when children move to a different neighborhood. When a child's neighborhood changes, it is hard to identify what drives their long-term outcomes because multiple inputs, such as amenities and school quality, change at the same time.

I study how local schools affect the educational achievement of low-income students when their neighborhood changes as a result of an inflow of more affluent households, using public housing regenerations as a natural experiment. In London, between 2001 and 2014, 156 public housing buildings were regenerated to pave the way for new high-density mixed-income developments. Regenerations were seen as an opportunity to revitalize local communities rather than move residents away (Mayor of London, 2018) and yielded little displacement of local families. The buildings slated for regeneration, initially hosting about 25,200 individuals, became home to approximately 39,400. On average, 54% of the new housing units were sold on the private market and would typically appeal to more affluent households. Four years after a regeneration, the number of primary school-age children living on the regeneration's block increases by about 24%, with 76% of them belonging to more affluent families. My research design compares students attending schools primarily enrolling children living on the regeneration site to students in schools located in the same neighborhood but farther away from a regeneration, which therefore enroll students from a different catchment area. I use a grandfathering instrument (e.g., Abdulkadiroğlu et al., 2016) to adjust for endogenous mobility and estimate the impact of regenerations on students who were originally enrolled in local schools before their completion.

I find that children enrolled in schools closer to a regeneration before its completion have higher math and language test scores at the end of primary school. On average, achievement (in terms of value-added) increases by about 0.064σ and 0.049σ (2 and 1.5% of the control group average) in math and language, respectively. The increase in math test scores happens across the board—the share of students performing at the top (bottom) increases (decreases) by 2.1 (1.8) percentage points. Language gains are particularly large for low achievers; the probability of scoring at the bottom of the distribution decreases by 1.7 percentage points (about 17% of the control average), while the probability of scoring at the top is unaffected. Placebo estimates obtained by imputing the regeneration's completion before the actual date yield fairly precise zeros.

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I show that the schooling context is the main driver of these achievement gains. I disentangle the role of neighborhoods and schools by exploiting the fact that school attendance is not strictly residence-based and use my research design to separately estimate school- and residence-based effects of housing regenerations. Schools explain between 65% and 81% of the overall effects. Such figures suggest that the educational gains of being exposed to a better neighborhood are mainly explained by the different school quality experienced.

I end my analysis by showing that achievement effects are plausibly mediated by changes in the composition of the student body of local schools. School inputs, such as pupil-to-teacher ratios and funding, are unaffected by housing regenerations, and achievement gains do not differ across schools characterized by different levels of autonomy. Students in the last year of primary school (grade 7) in treated schools, however, are exposed to more students with an advantaged background and more high-ability students. Such changes are concentrated in earlier school grades. On average, the share of students enrolled in treated schools that have attained a grade at the expected level or above in math or language at the end of grade 3 increases by $0.16 - 0.21\sigma$ ($1.6 - 2.5\%$). I document that such compositional changes are likely to affect incumbent students via *cross-grade* peer group effects.

I contribute to the literature by studying the role of different inputs in explaining children's long-term gains from exposure to a better neighborhood. The estimates of school effects documented in this paper are slightly larger than those found by Laliberté (2021), who reports that 50% to 70% of neighborhood effects are due to differential access to (good) schools. The latter paper, however, relies on a movers design, whereas I compare over time 15 cohorts of students in schools located in the same neighborhood, but differentially exposed to affluent households.

My results also shed light on the effects of public housing regenerations on children who stay in the neighborhood, whereas existing works have focused on the outcomes of children displaced by public housing demolitions (Jacob, 2004; Chyn, 2018). I show that creating opportunities in deprived neighborhoods by regenerating public housing into mixed-income communities can raise the achievement of children who remain in the neighborhood, and thereby their future outcomes (Heckman, 2006; Chetty et al., 2016; Chetty and Hendren, 2018).

More broadly, this paper contributes to the literature studying the impact of neighborhood change on incumbent residents. I identify short-term causal achievement effects for incumbent children following a change in their neighborhood's composition due to housing policies, whereas existing work has focused on long-term outcomes driven by labor shocks (Baum-Snow et al., 2019) or gentrification effects for incumbent residents who endogenously decided to stay (Brummet and Reed, 2019). Gibbons et al. (2013b, 2017) study achievement effects for secondary school students in England driven by changes in turnover and quality of peers in adjacent school grades. In contrast, my paper examines primary school students and exploits quasi-experimental changes in broader neighborhood composition driven by public housing regenerations.

2. Institutional background and context

2.1. Public housing in London

Most public housing estates in London (public housing "projects" in the US) were built between 1950 and 1980 and are now owned and managed by Local Authorities (LAs) and housing associations (HAs).[1] In 1985 and 1988, two housing acts allowed LAs to transfer the management of their public housing stock to housing associations. The latter are non-profit organizations, and in 2003, they managed approximately one-third of the total public housing stock (about 5.2 million in housing units).

Any adult with a low income and recognized housing needs, who has lived for a certain number of years in the LA and not displayed anti-social behavior or accumulated rent arrears can apply for public housing. Once an individual meets these eligibility criteria, they join a waiting list and can apply for housing as properties become available. Priority is given to people with medical or welfare needs, those living in unsatisfactory conditions (e.g., overcrowding), and people experiencing homelessness. Public housing tenants are given a contract for either a fixed number of years (flexible tenancy) or their entire life (secure tenancy). Secure tenants can also become eligible to buy their houses through the so-called "Right To Buy" scheme and become homeowners.[2] In 2001, there were about 790,000 public housing units in London, providing affordable housing for about 26% of the 3 million households in the city. Of the 790,000 public housing units, about 530,000 were still managed by the LAs, whereas housing associations managed the rest.

Table 1 shows several descriptive characteristics for individuals (Panel A) and children (Panel B) living in London in private and public housing, using 2001 census data and the National Pupil Database (NPD). Individuals living in public housing tend to be from ethnic minorities, have a higher probability of having a low-skilled job and being unemployed, and have a lower probability of owning a car. Their children are more likely to be eligible for subsidized school lunches or special educational needs (SEN) support and have higher deprivation scores.[3]

2.2. Public housing regenerations

Since the responsibility for public housing estates ultimately rests on local councils and housing associations, they are also responsible for making regeneration decisions. Buildings should be prioritized for regeneration based on their level of unfitness (poor design and poor condition). It is often the case, however, that regeneration programs include demolishing existing premises and constructing new buildings to facilitate the sale of a substantial number of newly built housing units on the private market. This implies that, in practice, local councils and housing associations may have the incentive to prioritize buildings – or entire estates – located in more "profitable" neighborhoods.[4] Regenerations are often carried out with private developers through two approaches, the "inclusionary" and the "linkage". The inclusionary approach requires market-rate housing developers to include a (minimum) percentage of below-market-price public housing units as part of the new development. The linkage approach requires private developers either to make cash payments into a housing trust fund (which in turn finances below-market housing developments) or to develop below-market housing at other sites.

During the regeneration, tenants are moved to an alternative public or private accommodation, located either in the preferred area or in an

[1] LAs, or local councils, represent one of the local government units in England and are responsible for a range of services, such as education and housing. There are 32 Local Authorities in London and the City of London. In some parts of England, there is a further subdivision whereby some services (e.g., housing, waste collection) are devolved to lower-layer units known as non-metropolitan districts. This is never the case for London, so this distinction is irrelevant in this context.

[2] The "Right-to-buy" scheme helps public housing tenants buy their homes by benefiting from a consistent discount. House and apartment tenants can benefit from a 35% and 50% discount, respectively, after they have been public sector tenants for three years. After five years, the discount increases by 1% and 2%, respectively, up to a maximum of 70%.

[3] Characteristics of children living in public housing are proxied using children living in census blocks targeted by a regeneration program. However, their figures (Table 1, Panel B) are broadly consistent with those for the households obtained from the 2001 census.

[4] See, for example, https://www.theguardian.com/society/2017/oct/29/gentrification-pushing-out-the-poor-haringey-council-housing-battle-corbyn-labour, accessed on September 4th, 2023.

Table 1
Households living in public housing.

	Households living in:			Blocks:		
	Greater London (1)	Private housing (2)	Public housing (3)	Without a regeneration (4)	With a regeneration (5)	Final sample (6)
Panel A: Census (2001)						
Percent white	0.77	0.80	0.68	0.72	0.68	0.63
Percent black	0.11	0.07	0.21	0.11	0.15	0.16
Percent asian	0.08	0.11	0.07	0.12	0.11	0.14
Percent managers	0.44	0.49	0.21	0.44	0.40	0.35
Percent low skilled	0.20	0.17	0.33	0.20	0.22	0.25
Percent unemployed	0.05	0.03	0.14	0.05	0.07	0.09
Percent with no car	0.37	0.30	0.60	0.37	0.44	0.50
Percent with no qualification	0.26			0.26	0.29	0.32
Percent high qualified	0.32			0.32	0.29	0.27
Panel B: National Pupil Database (2002)						
Percent male	0.51			0.51	0.51	0.52
Percent white	0.58			0.58	0.53	0.48
Percent black	0.19			0.19	0.25	0.25
Percent asian	0.13			0.13	0.13	0.17
Percent native	0.68			0.68	0.65	0.59
Deprivation score	0.28			0.28	0.38	0.46
Percent eligible for subsidized lunch	0.25			0.25	0.33	0.41
Percent with SEN	0.17			0.17	0.20	0.20
Number of blocks	4765			4432	333	67

Notes: The table shows descriptive statistics for households in Greater London (column 1), private housing (column 2), public housing (column 3), blocks without a regeneration program (column 4), blocks with a regeneration program (column 5), and blocks of regenerations in the final sample (column 6). Private housing includes households in owned, privately rented, and rent-free accommodation; public housing includes households in accommodation provided by local councils or housing associations. Column (5) uses regenerations whose permission date is between 2003 and 2013. Panel A uses data from the 2001 Census for the population, whereas Panel B uses data from the National Pupil Database in 2002 for all children aged 4–11 enrolled in state-funded schools. Data on qualifications and children by type of tenancy are not available.

area that minimizes disruption to the household's working and schooling circumstances.[5] Individuals holding secure tenancy have the right to be offered a unit in the new premises, while house owners are offered a price for their unit. After permission is granted for the regeneration (the "announcement date"), it takes on average 11 months to start the regeneration and 24 months to complete it. These regenerations are often considered an opportunity to increase the housing stock, and therefore they frequently entail a sizeable increase in the number of houses provided on site. Among all buildings slated for regeneration collected, the existing premises contained 65 housing units on average, whereas the new development contained 102. This implies a more significant housing density, with an average net increase of about 37 housing units per building.

I categorize the new developments as either "mixed-income" (the main object of this study) or "non-mixed". The former buildings include at least one market-rate housing unit, whereas the latter only provide public housing units. Within mixed-income developments, the share of units sold or rented on the private market varies depending on the specific program, but it is around 54% on average. Private market units may appeal to affluent households who seek to relocate, which is confirmed by the analysis presented in Section 5. In some instances, large regeneration programs can include the provision of other amenities for the local area, such as new parks or playgrounds.[6]

2.3. Primary school provision in England

In England, children enter school when they are five years old (Reception year or grade 1). The first phase of primary school is Key Stage 1 (KS1) and lasts two years, at the end of which children are assessed in math, language, and science by their teachers. During this stage, teachers assign each student the "level" at which they are working in the three subjects. Students can be graded as working below the expected level (Level 1), at the expected level (Level 2), or above the expected level (Level 3). The second phase of primary school, lasting four years, is Key Stage 2 (KS2), with students ages eight to eleven. At the end of this stage, in grade 7, all students take national standardized tests in math and language. These tests are proctored locally in every school and marked externally. Students are assessed again as working below, at, or above the expected level (Level 3, 4, and 5, respectively) depending on the score attained in the standardized test. Every year, school performance averages are used to form school rankings made available to parents in the School Performance Tables.

A significant majority (about 95%) of primary-school-age children are enrolled in public tuition-free schools. The LA regulates school entry, and parents can rank up to six schools based on their preferences. In cases of oversubscription, priority goes to children with SEN or siblings at the school. However, applications are mostly ranked by the distance from the child's home to school.[7] Primary schools in England are small (the average grade enrollment in my sample is about 50), and the average catchment area in London is about 1 km. Catchment areas are not fixed and depend on the number of places and specific applicant pool each year. Although parents can apply to schools outside their LA of residence, in practice, this rarely happens, and about 96% of primary-school children attend a school in the same LA as their residence.

There are different school types in England, which have different degrees of autonomy. Community and Voluntary Controlled schools are managed mainly by the LA, which recruits teachers and staff and provides schools with most of the services they need to run their operations (e.g., back-office and accounting activities). Foundation and Voluntary Aided schools enjoy more autonomy from the LA's control, although the LA still plays a significant role in the governing body and

[5] Households who have to move also get priority when bidding for vacancies advertised by the council.

[6] See Appendix Figure A.1 for an example of a regeneration program carried out in West London.

[7] In some schools, such as faith or "foundation" schools, the local governing body has direct responsibility for student admissions and may prioritize students according to different criteria, such as faith. However, selection based on ability is ruled out by law.

has powers of oversight. In all cases, funding comes from the LA using money provided by the central government through general taxation. In addition, starting in the 2000s, "academies" began to appear. Their original aim was to improve student performance in weak schools by giving head teachers direct control over their schools (called Sponsor Led academies). In 2010, however, the program was expanded, and now every school can voluntarily decide to become an academy (called Converter academies). As of July 2019, approximately 5600 primary schools (about 34%) had obtained academy status. Almost all academies are existing schools that decided to gain academy status, not new schools.[8] They are free to set their admission criteria, but they must abide by the guidelines stated in the Admission Code and cannot select students based on ability. In practice, this implies that most schools do not change their admission criteria after becoming academies.

3. Data

I consider "mixed-income" regeneration programs in Greater London announced between 2003 and 2013. Since the public housing stock is managed at the local level by LAs and housing associations, a unified database that includes public housing buildings and their redevelopments does not exist. I have constructed a novel database with regeneration programs involving public housing buildings and linked it to several datasets. An overview of the different sources used is provided below.

Housing regenerations. I have constructed a database including all public housing regenerations in Greater London using administrative records from the London Development Database (LDD). The LDD contains all planning applications filed to the London planning authorities – the 33 London local councils – *completed* after 2006 (as of November 2017, it contained 60,845 records).[9] I first tracked all applications concerning residential regenerations that included at least one public housing unit in the new premises (618 records as of November 2017). I kept all planning applications that included a development, redevelopment, or demolition of existing premises and whose planning application was completed. Using this procedure, I collated an initial dataset with 376 residential regenerations, the earliest initiated in 1999.[10] All regenerations have been geolocated and linked to their census block, a small-level geography with a target population of about 800 households and an average size slightly larger than 0.25 square miles.[11]

Student-level data. This study employs administrative records from the NPD on primary school-age students in England from 2002 to 2016 (approximately 600,000 per year). Data include student test scores in math and language at the end of the primary school cycle (KS2 scores) and each student's teacher assessments at the end of grade 3 (KS1).[12] The dataset also includes detailed student demographics, such as gender, ethnicity, language spoken at home, eligibility for subsidized lunches, and SEN support, and each student's block of residence. Children are linked to regeneration programs through their block of residence.

School-level data. Data on school characteristics have been gathered from different sources. The NPD (2002–2016) contains information on school type and address, which is used to link schools to regenerations. The School Census (2006–2010) and School Workforce dataset (SWF, 2011–2016) contain information on teacher qualifications, teacher status (e.g., teaching assistants), the pupil-to-teacher ratio, and teacher absences. Finally, the Consistent Financial Reporting (CFR, 2006–2016) contains data on school funding broken down by funding category (e.g., learning resources, SEN funding, staff funding).

House prices. I use administrative records from the Land Registry on house transactions from 1998 to 2018. Every transaction records the date, price paid for the house, house type (detached, semi-detached, terraced, apartment), house age (newly built or old), and contract type (leasehold or freehold).

Census and crime data. I exploit census data at block level from the 2001 UK census. Block-level statistics include detailed information on population characteristics, such as ethnic composition, jobs, employment rate, education, and social status (e.g., car ownership, socio-economic class). Furthermore, I use crime data at block level from 2008 to 2016 that is publicly available from the London Metropolitan Police website. The latter dataset records the number of crimes broken down by category (e.g., burglary, theft, violence against the person).

4. Empirical strategy

The first empirical challenge arises from the location and timing of regeneration programs. As about 54% of the housing units created in the new buildings are sold on the private market, local councils may have an incentive to prioritize locations that can generate higher revenues. Although councils should prioritize estates based on conditions of unfitness – poor design and poor condition – in practice, they have often been accused of social cleansing and targeting buildings or estates in more profitable areas.[13]

I overcome this challenge by exploiting variation in children's educational outcomes within schools in the same neighborhood but at different distances from the regeneration program site before and after its completion. I use a Difference in Differences (DID) design that compares students enrolled in schools located close to a regeneration (the treatment ring) with students enrolled in schools located in the same neighborhood but farther away from the regeneration (the control ring). The treatment ring includes schools in the same LA as a

[8] Like US charter schools, academies are public, tuition-free schools funded by the government but autonomous in several aspects, such as staffing, the provision of services (e.g., Human Resources), curriculum, and educational approach (e.g., school philosophy). However, despite many similarities, there are some notable differences between English academies and US charter schools. The latter are often located in deprived areas and serve a large number of low-performing or ethnic minority students, while academies include a substantial number of high-achieving schools. Additionally, whereas almost all academies are represented by school takeovers, in the US, many charter schools are newly opened. Finally, while academies can only be nonprofit organizations, US charter schools can be for-profit.

[9] The LDD is publicly available and updated monthly; the latest version can be accessed at: https://data.london.gov.uk/dataset/planning-permissions-on-the-london-development-database–ldd-, accessed on September 4th, 2023.

[10] The word "demolition" is mentioned explicitly in 354 of the 376 planning applications. Of the remaining 22 regenerations, 19 mention the word "redevelopment".

[11] The census blocks considered in the analysis are Lower Layer Super Output Areas (LSOAs), a geographical layer developed by the Office for National Statistics (ONS) for census statistics reporting purposes. There are 4765 LSOAs in London and 32,482 in England, designed to fit the LAs' boundaries.

[12] Students are awarded a mark between 0 – 100 for math (0 – 110 from 2016 onwards) and 0 – 50 for language. Until 2012, the language exam carried 100 points, evenly split between a reading and a writing section. Since 2012, only the reading section has been maintained as part of the national standardized assessments; therefore, only this latter mark has been included as the language outcome. Moreover, the reporting of KS2 levels changed in 2016, and students were no longer awarded levels 3, 4, and 5 according to the test score achieved. I have retrieved the level that would have been awarded to every student for 2016 by inferring the level thresholds corresponding to the average test score distribution observed between 2002 and 2016.

[13] Public housing regenerations have received extensive media coverage by the main British newspapers, such as The Guardian. See, for instance: https://www.theguardian.com/lifeandstyle/2018/nov/21/urban-regeneration-scheme-mask-problems-communities; https://www.theguardian.com/society/2017/jul/21/the-real-cost-of-regeneration-social-housing-private-developers-pfi, accessed on September 4th, 2023.

Table 2
Descriptive statistics.

	All		Treatment		Control	
	mean (1)	S.D. (2)	mean (3)	S.D. (4)	mean (5)	S.D. (6)
Panel A. Student characteristics						
Native	0.56	0.50	0.50	0.50	0.60	0.49
White	0.43	0.50	0.36	0.48	0.49	0.50
Black	0.22	0.42	0.26	0.44	0.19	0.39
Asian	0.21	0.41	0.24	0.43	0.18	0.39
Male	0.50	0.50	0.50	0.50	0.50	0.50
On subsidized lunch	0.30	0.46	0.35	0.48	0.26	0.44
With special educational needs (SEN)	0.25	0.43	0.27	0.44	0.23	0.42
School enrollment	49.79	23.28	48.28	19.90	50.99	25.56
Panel B. Student achievement						
Above expected level at KS1 (math)	0.20	0.40	0.18	0.38	0.22	0.41
Below expected level at KS1 (math)	0.10	0.31	0.12	0.32	0.09	0.29
Above expected level at KS1 (language)	0.23	0.42	0.20	0.40	0.26	0.44
Below expected level at KS1 (language)	0.16	0.37	0.18	0.38	0.15	0.35
Math score	68.19	21.40	66.83	21.63	69.26	21.16
Language score	30.12	9.26	29.41	9.26	30.68	9.22
Above expected level at KS2 (math)	0.38	0.49	0.35	0.48	0.41	0.49
Below expected level at KS2 (math)	0.14	0.35	0.15	0.36	0.13	0.34
Above expected level at KS2 (language)	0.37	0.48	0.34	0.48	0.40	0.49
Below expected level at KS2 (language)	0.11	0.32	0.12	0.33	0.10	0.30
Number of schools	680		374		306	
Number of observations	439,243		193,338		245,905	

Notes: The table shows summary statistics for student characteristics (Panel A) and student achievement (Panel B) for the whole sample (columns 1–2), the treated group (columns 3–4), and the control group (columns 5–6).

housing regeneration and located within the $80th$ percentile of the student-school LA-specific distance distribution (1.3 km on average). The control ring has the same width as the treatment ring, it is similarly centered on the regenerations, and is drawn so that its inner boundary crosses – and includes – the nearest untreated school. The schools located within this ring and not exposed to any regeneration represent the control group. Figure A.2 illustrates this idea, and Table 2 shows summary statistics for the main sample (columns 1–2), the treatment (columns 3–4), and the control group (columns 5–6). This strategy holds any neighborhood input constant across treated and control schools (and students), making it possible to disentangle the effects of regenerations from broader neighborhood effects on students attending local schools.

Recent econometric literature highlighted several issues with Two-Way Fixed Effects (TWFE) estimators with varying treatment timing and heterogeneous treatment effects.[14] In this setting, schools are exposed to treatment in different years, and I cannot rule out some degree of heterogeneity in treatment effects. To deal with the pitfalls associated with TWFE estimation, I use a "stacked-by-event" design, building "placebo" events for control schools (see, e.g., Deshpande and Li, 2019; Cengiz et al., 2019). All buildings slated for regeneration located in the same census block are assumed to be part of the same housing regeneration program. In such cases, the "type" (mixed or non-mixed) and announcement year are defined based on the earliest regeneration(s) of the block. The total number of such housing regeneration programs is 333, with 203 non-mixed and 130 mixed income.[15] I begin by creating a separate dataset for each regeneration, which includes all students in treated schools and never-treated students enrolled in control schools. Then, I define the relative time to event in each dataset with respect to the year when the regeneration was announced. Finally, I stack all datasets into one. In this setup, a treated student can be exposed to multiple regenerations, and never-treated students can, in principle, serve as the control for different regenerations.[16] I estimate the following model:

$$Y_{ir} = \beta_1 D_{s(i),r} \cdot T_{t(i),r} + \beta_2 X_i + \gamma_{s(i),r} + \gamma_{b(i),r} + \gamma_{t(i),r} + \upsilon_{ir} \quad (1)$$

where Y_{ir} is the math or language test score, measured in grade 7, of student i exposed to regeneration r. $s(\cdot)$, $b(\cdot)$, and $t(\cdot)$ map student i to their school, year, and census block of residence. $D_{s(i),r}$ is the treatment indicator for students enrolled in school s exposed to regeneration r; $T_{t(i),r}$ is the "post" dummy and takes a value of 1 from 3 years after regeneration r's announcement year, when the new building is expected to be completed. β_1 is the main parameter of interest and identifies the effect of regenerations on the achievement of students attending local treated schools. X_i includes student characteristics—ethnicity, gender, subsidized lunch eligibility, and baseline KS1 test scores in math and language obtained before the regeneration's expected completion date (i.e., before the treatment starts). $\gamma_{s(i),r}$, $\gamma_{b(i),r}$ and $\gamma_{t(i),r}$ are school by regeneration, block by regeneration, and regeneration by year fixed effects (FEs), respectively. Regeneration by year FEs ($\gamma_{t(i),r}$) flexibly account for time patterns around *each* regeneration; regeneration by school ($\gamma_{s(i),r}$) and regeneration by block ($\gamma_{b(i),r}$) FEs control for time-invariant differences of schools and blocks. I cluster standard errors at school level to account for intra-school correlation.[17]

In this setting, another challenge arises from families' endogenous enrollment choices. Over time, families may decide to relocate their children across schools, possibly due to the regeneration programs themselves. On average, student mobility within the last phase of

[14] See De Chaisemartin and d'Haultfoeuille (2020), Baker et al. (2022), Borusyak et al. (Forthcoming), Callaway and Sant'Anna (2021), Goodman-Bacon (2021) and Sun and Abraham (2021).

[15] Table A.1 shows the number of housing regenerations by announcement year.

[16] In addition, I consider an alternative approach, whereby each school is uniquely assigned to the *earliest* regeneration it was exposed to (i.e., treatment is an absorbing state, see Sun and Abraham, 2021). In this setting, I also estimate treatment effects using the estimator proposed in Sun and Abraham (2021). The results from this analysis are presented in Appendix A.

[17] Figure C.1 shows standard errors obtained using different clustering definitions. I consider standard errors clustered on regenerations and Spatial Heteroskedasticity and Autocorrelation Consistent (HAC) standard errors (Conley, 1999; Hsiang, 2010).

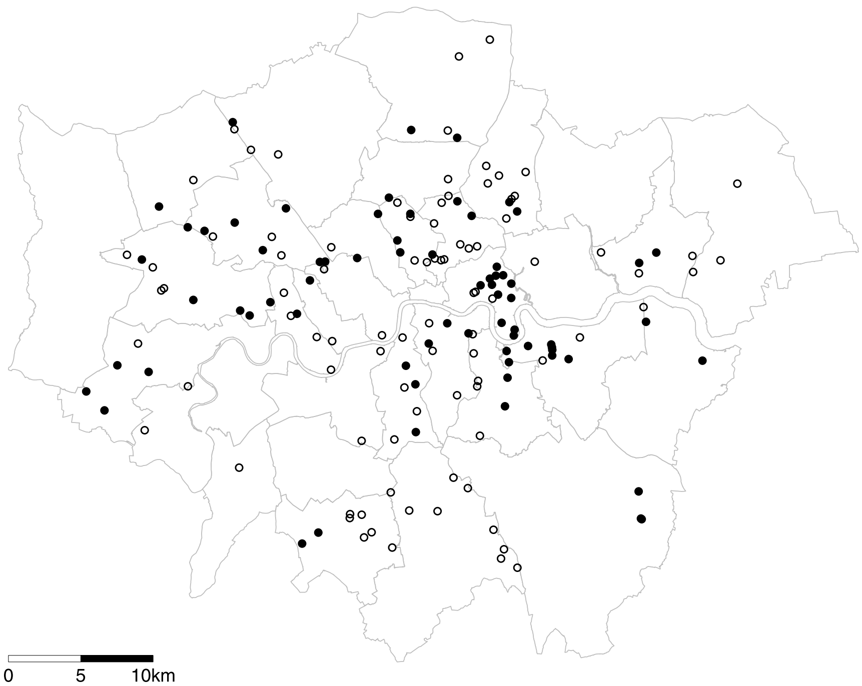


Fig. 1. Maps of regeneration programs. *Notes:* The figure plots the 156 public housing regeneration programs announced between 2003 and 2014 in Greater London and included in the final sample. Solid dots indicate mixed-income housing regeneration programs, and circles indicate non-mixed housing regenerations. The 33 London local authorities are marked by the gray outline.

primary school (KS2) is low in England. In addition, families living in regenerated buildings are likely to be relocated in the same neighborhood (see Section 2). Still, I use an Instrumental Variable (IV) strategy to account for the possibility of endogenous mobility across schools. I exploit the fact that once children are enrolled in their school, they have the right to retain their seats, regardless of whether they relocate or new students enter their school's neighborhood. This motivates a "grandfathering" instrument similar to the one in Abdulkadiroğlu et al. (2016). I instrument student enrollment in treated schools with a variable indicating whether the student was enrolled in that school before the regeneration's expected completion date. The underlying assumption is that enrollment decisions were not driven by expectations about the regeneration programs.[18] Since parents can hardly anticipate the expected completion date, enrollment after treatment of students who were already enrolled before completion can essentially be considered passive. 2SLS estimates are obtained by instrumenting the interaction $D_{s(i),r} \cdot T_{t(i),r}$ with $G_{s(i),r} \cdot T_{t(i),r}$, where $G_{s(i),r}$ is an indicator for students who were enrolled in a treated school before the expected completion date of regeneration r. Similar to previous works that have employed the grandfathering instrument, I restrict the sample to students who were enrolled in treated and control schools at the time of regeneration r's expected completion.[19]

In the main analysis, I consider public housing regenerations that explicitly target an existing public housing building (156), defined as having at least one housing unit whose provider is a public entity (LA or HA). I drop housing regenerations whose permission date was granted before 2003 (to allow for at least one year of pre-treatment) or after 2013 (to allow for at least three years after the announcement date). I focus on mixed-income regenerations. The final sample includes 67 mixed-income regenerations (680 schools and 439,243 observations).[20] Fig. 1 highlights housing regenerations included in the final sample, showing that they are geographically dispersed around London. Table A.1 (columns 5–6) shows the number of housing regenerations by announcement year in the final sample. Table 1 displays summary statistics for the population and children living in blocks with a regeneration (column 5) and with a mixed-income regeneration in the final sample (column 6), showing that households and children living in the affected blocks tend to be substantially more deprived than the London average along several dimensions.

5. The impact of regeneration programs on local residents

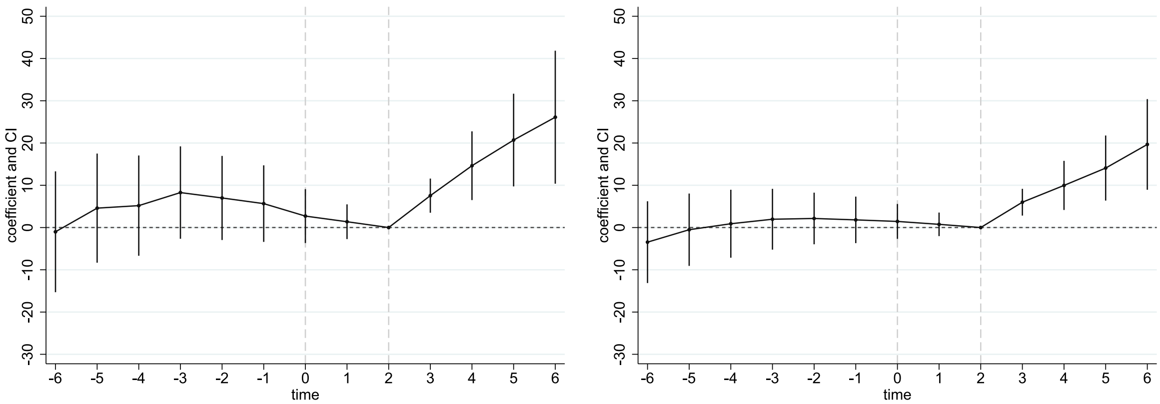
The empirical investigation begins by studying how regeneration programs affect the households living on the site. Since these programs include demolishing existing premises and constructing new buildings, they may generate substantial outflows and inflows of households and children. Consistent with the institutional provisions, however, the majority of children living in the regeneration block are relocated within the local area.[21]

[18] There is no certainty about each program's exact start and completion dates. In addition, there is uncertainty on which schools parents can target after the regeneration ends, as school catchments vary over time and depend on the degree of oversubscription and each year's specific applicant pool.

[19] For a given regeneration, the final sample includes all students in Grade 7 in the linked treated and control schools up to the year of expected completion *plus* all students in grades 3 – 6 in the (expected) completion year. In this setting, the school fixed effect is defined based on the *baseline* school where the student was enrolled at the time of a regeneration's completion.

[20] Appendix B replicates the main results for non-mixed regenerations (N = 79).

[21] As I do not observe children's postcodes, it is impossible to directly track the children living in the buildings slated for regeneration. Using the block of residence, I can track changes in residence across census blocks. Figure A.3 shows the share of children relocating to the local area and outside it. The share of children relocating within the local area increases around the announcement year ($t = 0$). If residents of the buildings slated for regeneration were not disproportionately relocated nearby – as per regulation – we would not observe any change in the trend. Conversely, if they were disproportionately relocated outside the neighborhood, one would expect to observe the opposite pattern; that is, the proportion of children relocated outside would increase.



Panel A. All children

Panel B. Children ineligible for subsidized lunches

Fig. 2. Effects of public housing regenerations on the number of children. *Notes:* The figure shows the total number of children (Panel A) and the number of children ineligible for subsidized lunches (Panel B) living on a regeneration site before and after its announcement. The two vertical lines mark the program's announcement year (time 0) and the year before program completion (time 2). Each Panel plots coefficients and the related 95% confidence interval (vertical spikes) obtained estimating Eq. (2). Standard errors are clustered on census blocks.

I characterize the change in the demographics of new residents following a housing regeneration by considering the following equation, which uses outcomes at block level:

$$Y_{brt} = \sum_k \pi_{1k} D_{br} \mathbb{1}(t - E_r = k) + \eta_{br} + \eta_{tr} + \kappa_{brt} \tag{2}$$

where Y_{brt} is the outcome for block b located around regeneration r at time t. D_{br} is the treatment dummy, taking a value of one for all blocks exposed to regeneration r and zero for all blocks in the control ring. The treatment and control rings are defined in Section 4. E_r denotes the year when the permission for regeneration r was announced, and the index k indicates periods of k years before or after a regeneration's announcement year. η_{br} and η_{tr} are regeneration by block and regeneration by year fixed effects.

The number of children ineligible for subsidized lunches increases following a public housing regeneration. Fig. 2 presents the estimates obtained using as outcomes the number of children (Panel A) and the number of children ineligible for subsidized school lunches (Panel B). Consistently with the regeneration programs' expected completion date, there is a sharp increase in primary school-age children three years after the announcement date, when the new buildings are expected to be completed. The number of children living in the regeneration block (Panel A) decreases around the announcement date – consistently with children relocating before the demolition – and increases by about 26 four years after the completion (an increase of about 7.5% of the average primary school roll in my sample). This increase in children is almost entirely driven by the increase in students who are ineligible for subsidized lunches (Panel B; about 20). This shows that, besides the increase in children, the new premises are targeted mainly by more affluent families.

I then explore how regenerations affect the surrounding area in terms of house prices by considering the following DID model:

$$Y_{hr} = \sum_k \theta_{1k} D_{p(h),r} \mathbb{1}(t - E_r = k) + \theta_2 W_h + \psi_{p(h),r} + \psi_{m(h),r} + \mu_{hr} \tag{3}$$

where Y_{hr} is log (deflated) price paid for the transaction of house h located around regeneration r; $m(\cdot)$ and $p(\cdot)$ map house transactions to postcodes and year-month of the transaction. $D_{p(h)}$ is the treatment dummy and takes value one (zero) for all houses transacted in the treatment (control) ring. E_r denotes the year when the permission for regeneration r was announced, and the index k indicates time periods k years before or after a regeneration's announcement year. W_h is a vector of house characteristics (house type, age, contract); $\psi_{p(h),r}$ and $\psi_{m(h),r}$ are regeneration by postcode and regeneration by year-month fixed effects, respectively. Standard errors are clustered at postcode level. The average increase in house prices after the completion year with respect to the control ring is about 1.3%. Fig. 3 shows that house prices start increasing after the completion year, suggesting that the housing market quickly incorporates the presence of the new development.[22]

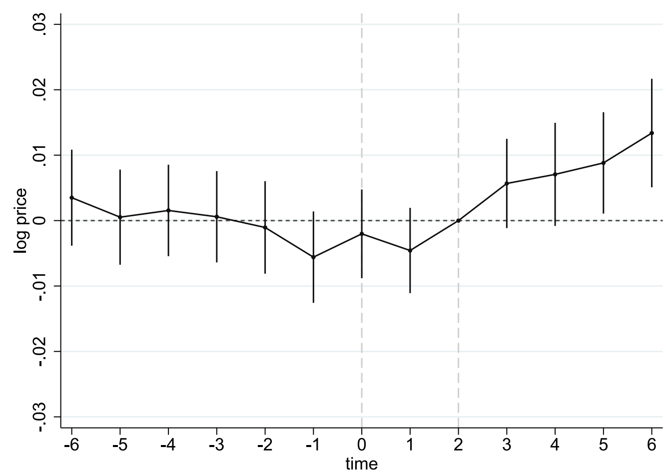
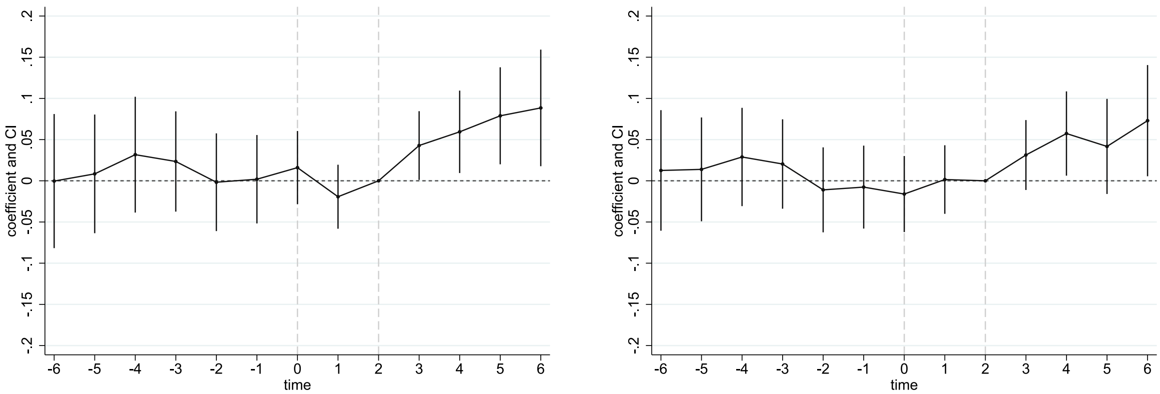


Fig. 3. Effects of public housing regenerations on house prices. *Notes:* The figure shows time-specific estimates of the (log) price paid for houses transacted within the treated area with respect to those transacted in the control ring (see Section 4). Coefficients and the related 95% confidence interval are obtained estimating Eq. (3). The two vertical lines mark the year the regeneration was announced ($time = 0$) and the year before the program was completed ($time = 2$).

[22] Other studies that have focused on different housing interventions have also generally found a positive impact on nearby land and house prices. Rossi-Hansberg et al. (2010) show that in neighborhoods targeted with urban residential revitalization programs, land prices increased by 2–5% per year after the program. Koster and Ommeren (2018) find that public housing renovations in the Netherlands increase surrounding house prices by about 3.5%. Diamond and McQuade (2019) document how affordable housing developments increased house prices nearby by 6.5% in disadvantaged neighborhoods across 15 US states. Blanco (2023) estimates that over the ten years following a public housing demolition in Chicago, house prices increase by about 10%.



Panel A. Math Panel B. Language

Fig. 4. Effects of public housing regenerations on student test scores: Event study. *Notes*: The figure shows event study estimates of the effect of housing regenerations on student achievement for math (Panel A) and language (Panel B) obtained estimating the Reduced-Form version of Eq. (1) and interacting $G_{s(i),r}$ with event time indicators. The vertical lines indicate 95% confidence intervals. The two vertical lines mark the announcement year ($time = 0$) and the year before completion ($time = 2$). Standard errors are clustered on schools. P-values of a test for joint significance of placebo estimates ($time < 2$) are 0.54 and 0.84 for math and language, respectively.

6. Main results

6.1. The impact of housing regenerations on student achievement

Test scores across treated and control students follow a similar pattern before a housing regeneration's completion and start diverging afterward. Fig. 4 plots event study estimates obtained by estimating the Reduced-Form version of Eq. (1) and interacting $G_{s(i),r}$ with event-time indicators. Placebo estimates before the actual announcement date lend validity to the suitability of the control group and the parallel trend assumption. For both math and language scores (Panels A and B, respectively), the estimates are fairly close to zero (and never statistically significant) before the actual treatment event and do not exhibit any trend.[23] A joint test for the significance of the coefficients before the expected completion date ($time = 2$) also rejects their statistical significance.[24]

Public housing regenerations drive an increase in student performance. Table 3 (Panel A) shows the main results obtained by estimating Eq. (1). OLS estimates would imply a $0.06 - 0.044\sigma$ increase in math and language test scores for students attending a school located close to a regeneration. Columns (2)–(3) and (5)–(6) present the results obtained after accounting for student selection across schools. First-stage estimates (columns (3) and (6)) can be interpreted as the share of students who were enrolled in a treated school before the treatment date and went on to take the end-of-primary school exams in the same school. Estimates imply that about 92% of students remain in a treated school, consistent with the fact that the number of students who experience short-term displacement is not expected to be large.[25]

After accounting for the endogenous selection of students across schools and consistent with first-stage estimates, regenerations still have a positive and sizeable effect on student achievement. Reduced-Form estimates imply an effect of 0.059σ and 0.045σ, and the corresponding 2SLS estimates imply an effect of 0.064σ to 0.049σ (about 1.4 and 0.45 points, or $2{-}1.5\%$ of the control group average) in math and language, respectively.[26] The second and third sets of results in the table use as outcomes indicators for scoring at the top and bottom of the test score distribution in math and language (above and below what would be expected at the end of primary school, Level 5 and 3, respectively). The estimates suggest that the increase in math test scores happens across the board – the share of students performing at the top (bottom) increases (decreases) by 2.1 (1.8) percentage points. The pattern for language gains is different, with no changes at the top of the distribution and a sizeable decrease in the number of students scoring at the bottom (1.7 percentage points). Appendix B replicates the main result for the sample of non-mixed regenerations – the regeneration programs including only public housing in the new buildings. Since this type of development only includes public housing in new buildings and therefore does not change the composition of families living on the site, we would not expect substantial effects on student achievement. The results confirm this hypothesis, showing that estimates of the impact of non-mixed regenerations on student achievement are either slightly negative or fairly well-estimated zeros.[27]

Students who were originally attending the regeneration's neighboring schools are of particular interest from a policy perspective. Schools considered in this context predominantly serve more deprived neighborhoods and may therefore be locked in a bad equilibrium where there are few incentives to improve student achievement (Hastings et al., 2010). More disadvantaged households may face barriers in exerting school choice, such as high house prices close to high-quality schools (see, for instance, Black, 1999; Machin, 2011; Gibbons et al., 2013a; Battistin and Neri, 2024).[28] In addition, a sizeable literature shows the importance of education early in life for children's future outcomes. Raising achievement during the early stages of a child's educational path can substantially improve medium- and long-term outcomes (Heckman, 2006); more-educated children are more likely

[23] In addition, I note that achievement effects are close to zero between $time = 0$ and $time = 2$, suggesting that potential disruption effects are limited. The existence of any disruption effects would imply that the estimated achievement effects would likely be underestimated.

[24] To corroborate the "stacked-by-event" design, Figure C.2 plots event study estimates obtained using the estimator proposed in Sun and Abraham (2021). The results obtained using this alternative approach are similar.

[25] Table A.2 presents the full set of first-stage estimates by event time.

[26] 21.5% of schools are exposed to more than one regeneration. Table A.3 adds to Eq. (1) an interaction between the treatment indicator and an indicator for schools exposed to more than one regeneration. Achievement effects for the latter are not statistically different from those documented in Table 3.

[27] Table B.1 presents descriptive statistics for the sample of non-mixed regenerations, and Table B.2 shows estimates of Eq. (1) for this sample. First-stage coefficients (Table B.2, columns 3 and 6) are essentially the same as those in Table 3, suggesting that selection concerns are not larger in this sample. Figure B.1 shows event study estimates for non-mixed regenerations, similar to Fig. 4.

[28] It is possible this is the reason school choice policies have been shown to have a quite limited impact on student achievement (Cullen et al., 2005; Hastings et al., 2010; Deming et al., 2014).

Table 3
Effects of public housing regenerations on student achievement.

	End-of-primary school achievement in:					
	Math			Language		
	OLS (1)	RF (2)	2SLS (3)	OLS (4)	RF (5)	2SLS (6)
Test scores	0.060	0.059	0.064	0.044	0.045	0.049
	(0.019)	(0.022)	(0.024)	(0.018)	(0.022)	(0.024)
	[0.001]	[0.008]	[0.008]	[0.017]	[0.039]	[0.039]
Above expected level	0.021	0.020	0.021	0.012	0.009	0.010
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)
	[0.006]	[0.024]	[0.024]	[0.115]	[0.288]	[0.288]
Below expected level	−0.017	−0.017	−0.018	−0.013	−0.015	−0.017
	(0.005)	(0.006)	(0.007)	(0.004)	(0.005)	(0.005)
	[0.001]	[0.008]	[0.008]	[0.001]	[0.001]	[0.001]
First Stage			0.917			0.917
			(0.003)			(0.003)
			[0.000]			[0.000]
Observations	439,243	439,243	439,243	438,580	438,580	438,580
Student characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Baseline scores	Yes	Yes	Yes	Yes	Yes	Yes
Regeneration by school FE	Yes	Yes	Yes	Yes	Yes	Yes
Regeneration by block FE	Yes	Yes	Yes	Yes	Yes	Yes
Regeneration by year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows OLS, Reduced-Form, and 2SLS estimates of the impact of mixed-income public housing regenerations on math (columns 1–3) and language (columns 4–6) scores at the end of primary school. Test scores in math and language are standardized by cohort to have zero mean and unit variance. All columns control for student characteristics (gender, ethnicity, subsidized lunch eligibility), baseline (KS1) scores in math, reading, and writing, and regeneration by school, child's block, and year fixed effects. Standard errors, shown in parentheses, are clustered on schools. P-values are shown in square brackets.

to enroll in college and have better health and labor market outcomes (Deming, 2009; Chetty et al., 2011; Campbell et al., 2014). These issues gain even greater relevance in deprived neighborhoods, where the lack of good schools implies that disadvantaged children may struggle to obtain a high-quality education.

The results presented here provide convincing evidence that mixed-income housing programs generate positive externalities for local children. This finding implies that, at least in the short-term, such programs can increase student achievement in more deprived neighborhoods. The benefits of housing regenerations for local students are often neglected in the public debate. In addition, the consequences of such short-term increases in student performance can potentially have long-lasting effects on local schools and neighborhoods. On the one hand, if marginal (possibly more affluent) families start moving into the area, attracted by the improved school quality, schools may experience a positive reinforcing mechanism leading to even larger increases in student achievement in the future (Battistin and Neri, 2024). On the other hand, a further inflow of new, more affluent households to the neighborhood, attracted by the improved school quality and coupled with the increase in house prices, might eventually lead to the displacement of local residents in the long term.

6.2. Heterogeneous effects

The results presented mask some heterogeneity across different subgroups of children. In this section, I study whether the effects found differ with respect to several student characteristics (e.g., gender), socio-economic status, baseline achievement, and local housing conditions.

Achievement effects from housing regenerations are not substantially different across subgroups of students. Fig. 5 summarizes the results. Achievement effects are slightly stronger for males, non-natives, and children ineligible for subsidized lunches. A mild gradient in student ability emerges, with higher achievement effects for low-ability students. While students living in low-crime areas seem to benefit slightly more in math, the same pattern does not hold for language test scores. However, these point estimates are never statistically different. In the bottom panel of Fig. 5, I stratify the sample to consider students living in blocks with a share of owners and renters higher than the sample median. Regeneration effects are similar for those living in "renter-prevalent" and "owner-prevalent" areas. This suggests that income effects – possibly due to the increase in housing values around the regenerations – are limited and unlikely to drive the results.

6.3. Summary of main robustness checks

I end this section by discussing several robustness checks for the paper's main results. Firstly, I replicate the main results, but I uniquely assign treated schools to housing regenerations. This approach considers an "absorbing treatment" (see Sun and Abraham, 2021), with schools' treatment timing defined based on the "earliest" regeneration they are exposed to (i.e., the one announced first). The results are similar to those discussed in Section 6.1. Table C.1 replicates Table 3 using this alternative approach, and Figure C.2 estimates the corresponding event study using the stacked-by-event design and the estimator proposed by Sun and Abraham (2021).

Secondly, I consider an alternative approach to grouping nearby buildings. Instead of assuming that buildings located within the same census block belong to the same estate (see Section 4), I consider "clusters" of buildings located nearby (within 400 m of one another). The results are presented in Table C.2 and are similar to those in Table 3.

The third set of checks is presented in Table C.3. Column (1) presents the main results (i.e., those in columns 3 and 6 of Table 3). First, I exclude large regenerations from the sample (the top 5%), which might be part of broader neighborhood programs and possibly include additional resources for new amenities, such as parks, schools, or business activities (column 2). The results are similar to those in Table 3. Then, in the treatment group, I keep only schools located within the 50th percentile of the student-school LA-specific distance distribution (column 3). These estimates, which consider schools closer to regenerations and therefore potentially more exposed to the changes

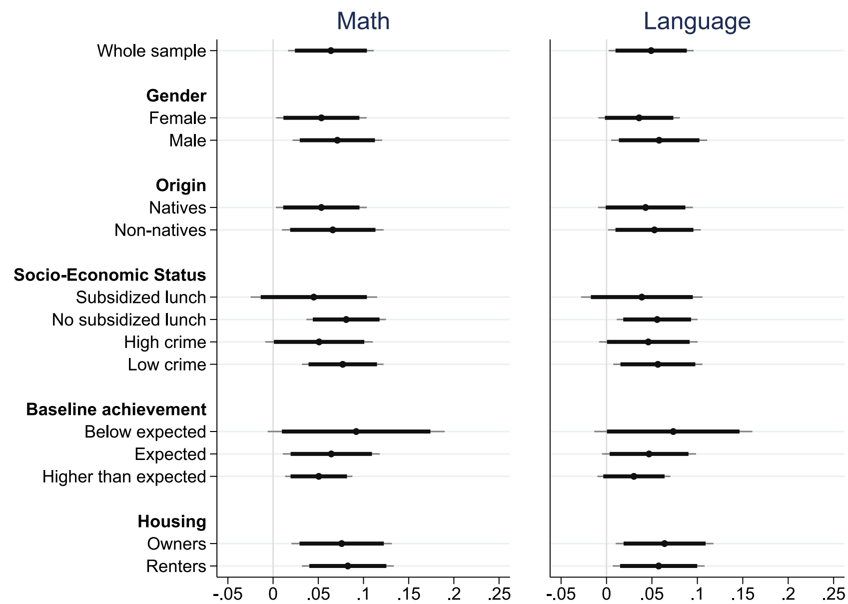


Fig. 5. Effects of public housing regenerations by subgroup. *Notes:* The figure shows 2SLS estimates of the effect of housing regenerations on student achievement and the related 95% and 90% confidence intervals (the gray and black thick bars, respectively) in math and language for different subsamples. Natives are students who speak English at home. High-crime and low-crime areas are defined as census blocks with a crime rate above and below the sample median, respectively. Baseline achievement is measured at KS1 by teacher assessments in the two subjects and individuates students achieving at Level 1 (below expected level), Level 2 (expected level), and Level 3 (higher than expected level). "Owners" and "renters" areas are defined as blocks with a share of owners and renters above the sample median.

brought about by them, are larger (though not statistically different) than those in Table 3. Considering test scores, the impact is 0.099σ and 0.061σ in math and language, respectively. Finally, I exclude from the control group schools between the 80th and 90th percentiles in the LA-specific school-student distance distribution (column 4). Again, the results are similar to those obtained using the main design.

Next, I consider two placebo tests. In the first, I recode control students as "treated" and compare their achievement with that of students enrolled in schools located farther away (for each regeneration, within twice as much the upper-bound distance of the control ring). In the second, I estimate Eq. (1) for the subsample of mixed-income housing regenerations that do not target a public housing building. Since these regenerations target buildings located in less-deprived neighborhoods (considering the same statistics as in Table 1, they are close to the London average), one would expect limited compositional changes and therefore limited effects. For both subsamples, the estimates obtained are either close to zero or slightly negative, differ substantially in magnitude from the estimates in Table 3, and are never statistically different from zero (see Table C.4 and Table C.5, respectively).

Finally, Figure C.1 shows standard errors from Table 3 obtained using alternative ways of standard error clustering. I cluster standard errors at regeneration level (instead of school level) and consider Spatial Heteroskedasticity and Autocorrelation Consistent (HAC) standard errors (see Conley, 1999; Hsiang, 2010). Compared to the main estimates, the alternative standard errors are generally smaller than those obtained by clustering on schools, leaving the main conclusions unchanged.

7. Neighborhood and school effects

I disentangle the relative roles of neighborhoods and schools by exploiting the fact that school attendance is not strictly residence-based; school attendance is determined by distance and not by fixed catchment areas. Hence, students living in the same area potentially attend different schools. Specifically, I add a variable to Eq. (1) that identifies whether a student is exposed to a regeneration (i.e., they are "treated") based on residence rather than school ("residence-based" treatment indicator). The results are presented in Table 4, which shows reduced-form estimates. Column (1) presents the estimate obtained with the main design (Table 3, column 2), which assigns treatment based on the school of attendance ("school-based" treatment indicator). This analysis shows that while the residence effects are somewhat positive (column 2, 0.042σ), once the school-based treatment indicator is added (column 3), the residence effects decrease (0.012σ, not statistically different from zero). In this setting, school effects can arise both through changes in the composition of peers, as well as through changes in other school inputs (e.g., pupil-to-teacher ratio, class size); I return to this issue in Section 8 below.

Schools potentially explain about 81% of the achievement effects documented in Section 6. The "interaction" effect of the program – school and residence exposure combined – on student test scores is 0.064σ for math test scores (column 3).[29] These estimates are slightly larger than those in column (2) of Table 3 and represent the combined effect of living and attending a school close to a regeneration. While the two estimates are not statistically different, taken at face value, they would imply that the overall effects may be larger for those living closer to the new buildings, suggesting that neighborhood peer effects from new residents also play a role. The relative magnitude of the two coefficients implies that school effects explain $0.052/0.064 = 81\%$ of the overall effects.

One can alternatively disentangle school and neighborhood effects by exploiting the fact that students may attend a treated school but live at different distances from a regeneration, thereby being more or less exposed to *neighborhood* peer effects due to their proximity to the new building in terms of residence. Column (4) uses this approach by interacting the school-based treatment indicator with two residence-based indicators, one that identifies students living in the treated area and one that identifies students residing outside the treated area (and, therefore, farther away). The estimated coefficients are 0.066σ and 0.043σ, respectively. The former group is exposed to school effects and proximity to the new building, while the latter group is only exposed to school effects. These estimates would imply that schools explain about $0.043/0.066 = 65\%$ of the overall effect.

[29] Table A.4 presents similar estimates for language.

Table 4
School- and residence-based treatment effects for math scores.

	End-of-primary school achievement: math test scores				
	(1)	(2)	(3)	(4)	(5)
School-based treatment	0.059 (0.022) [0.008]		0.052 (0.022) [0.018]		
Residence-based treatment		0.042 (0.016) [0.008]	0.012 (0.010) [0.260]		
School-based treatment, living far				0.043 (0.023) [0.060]	
School-based treatment, living close				0.066 (0.023) [0.004]	
Residence-based treatment, attending a treated school					0.052 (0.018) [0.004]
Residence-based treatment, attending a control school					−0.017 (0.015) [0.258]
Total effect			0.064 (0.023) [0.007]		
Observations	439,243	439,243	439,243	439,243	439,243
Student characteristics	Yes	Yes	Yes	Yes	Yes
Baseline scores	Yes	Yes	Yes	Yes	Yes
Regeneration by school FE	Yes	Yes	Yes	Yes	Yes
Regeneration by block FE	Yes	Yes	Yes	Yes	Yes
Regeneration by year FE	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the Reduced-Form effects of public housing regenerations on math test scores considering alternative treatment indicators. Column (1) shows the main results (Table 3, column 2; school-based treatment indicator). Column (2) uses a treatment indicator based on residence (residence-based). Treated students are the students in the main sample residing in a block close to a regeneration, defined using the same distance in the main design (see Section 4). Column (3) estimates a regression where both treatment variables are included. Column (4) shows results from a specification that interacts with the school-based treatment indicator of column (1) with indicators for students living far away from or close to a regeneration (see main text for details). Column (5) estimates a regression that includes the residence-based indicator interacted with (i) the school-based indicator and (ii) an indicator for attending a control school. The bottom of column (3) shows the linear combination of the school-based and residence-based indicators. All columns control for student characteristics (gender, ethnicity, subsidized lunch eligibility), baseline (KS1) scores in math, reading, and writing, and regeneration by school, child's block, and year fixed effects. Standard errors, shown in parentheses, are clustered on schools. P-values are shown in square brackets.

The main caveat of this approach is that schools within the treated group may themselves be "exposed" to different treatment effects depending on their distance to the regeneration. The results presented in Table C.3 seem to suggest that this might be the case (although the estimates in columns 1 and 3 of Table C.3 are generally not statistically different). If this is the case, the extent of the school effects would be underestimated, as students living farther away may be more likely to be enrolled in treated schools that are also farther away from the regeneration. Therefore, the 65% figure can be seen as a lower-bound estimate for the school effects. The range obtained (65–81%) is consistent – if anything, slightly larger – than the estimates in Laliberté (2021).

Finally, I document the results from a placebo test that again exploits the fact that students' residence does not mechanically determine the school of attendance. Column (5) estimates a version of Eq. (1) where the residence-based treatment indicator is interacted with two different school-based treatment indicators. The first interaction identifies students living in the treated area and attending a treated school. The second interaction identifies students living in the treated area but attending a control school. While the first coefficient is similar to the main school-based estimate (0.052 vs. 0.058σ), the second coefficient is slightly negative (-0.017σ) and not statistically different from zero. This suggests that treatment effects are driven by students' *attendance* of treated schools rather than residential proximity to the new buildings.

8. School inputs and peers

Since schools seem to explain the majority of the achievement effect, I end by focusing on the possible mechanisms within schools. The effects of housing regenerations do not differ across schools characterized by different levels of autonomy. Achievement effects are similar across non-autonomous and autonomous schools (Table A.5). Housing regenerations do not drive meaningful changes in school inputs, either. Schools' pupil-to-teacher ratios do not change, suggesting that class size is unaffected by housing regenerations. Similarly, schools' available funding and its allocation are unchanged (Table A.6).[30]

[30] While it is not possible, with the data at hand, to study changes in teacher quality, a change in teacher quality (as for other school inputs) is likely to come about more slowly, as teacher recruitment is lengthy and there is excess demand in primary education for urban contexts like that of Greater London (Arnaud and Dolton, 2005). This contrasts with the pattern observed in Fig. 4, which shows that test score gains start increasing in the years immediately following the regeneration's completion date.

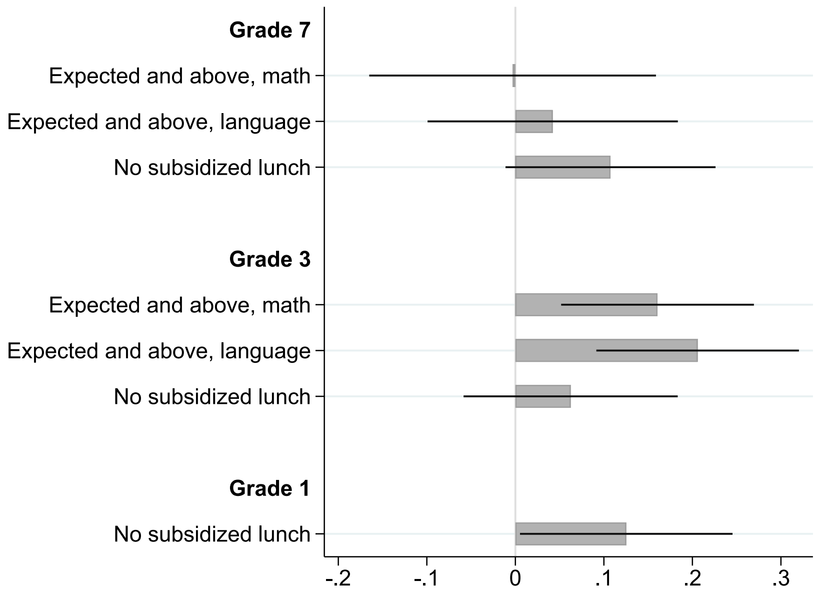


Fig. 6. Effects of housing regenerations on school composition. textitNotes: The figure shows the effect of public housing regenerations on student composition in grade 7 (top panel), grade 3 (middle panel), and grade 1 (bottom panel). I consider student ability, defined as the share of students achieving at the expected level and above in math and language, and student SES, defined as ineligibility for subsidized school lunches. The top panel considers students in the same grade (7) of the students considered in the main analysis (see, e.g., Table 3). The middle panel considers students in the same school but at the end of grade 3 (end of KS1). Outcomes are standardized by year to have zero mean and unit variance. Standard errors (horizontal spikes) are clustered on schools.

Treated schools experience slight changes in their student composition in grade 7. Fig. 6 shows the effect of public housing regenerations on student composition by school grade, considering ability (KS1 test scores) and SES (proxied with subsidized lunch eligibility). The top panel shows that public housing regenerations drive mild changes in student composition in the last school grade (grade 7), and only in terms of students' SES. The effect on student ability is negligible. In other words, students in grade 7 whose test scores are affected by housing regenerations (as shown in Section 6) are exposed to more students with a better SES (an increase of about 0.11σ, or 3.1% of the pre-treatment average, significant at 10%) but are not exposed to "better" students in terms of ability.

Schools' composition changes more substantially in earlier grades both in terms of student SES and ability. On average, the share of students enrolled in treated schools that have attained a grade at the expected level or above in math or language at the end of KS1 increases by $0.16–0.21\sigma$ (1.6 to 2.5% of the pre-treatment average; middle panel). Student composition also improves in grade 1, suggesting that incoming families with more advantaged backgrounds are enrolling their children in local schools (bottom panel). These results show that students in grade 7 are not exposed to more high-ability students within their *grade*, although they are within their *school*.[31]

Peer ability is related to improvements in student test scores. In a separate analysis, I relate student achievement to peer ability in different school grades. I follow the approach in Lavy et al. (2012) to study how the test scores of grade 7 students are affected by the share of high-ability peers in grade 7 and 3 (Table A.7).[32] Test scores of grade 7 students are positively affected by the share of high-ability peers in grade 3, over and beyond the share of high-ability peers in grade 7. I use these estimates to compute what would be the implied increase in grade 7 test scores due to peer effects from high-ability students in my setting. Using the estimate for math in Fig. 6 (0.16), the standard deviation of the share of students at the expected level and above at grade 3 (0.10 — see Table A.7's footnote), and the estimate in Table A.7, column (4), I obtain an implied increase in grade 7 test scores due to peer effects from high-ability students of about $0.16/0.10 \cdot 0.017\sigma = 0.027\sigma$. Since regenerations target more deprived neighborhoods, I also use the estimate for subsidized lunch-eligible students (Table A.7, column 5), and obtain $0.16/0.10 \cdot 0.025\sigma = 0.04\sigma$. These figures can be compared to the estimate in Table 4, column (3), which nets out neighborhood effects (0.052σ).

These back-of-the-envelope calculations suggest that *cross-grade* peer group effects are a plausible driver of the achievement effects documented in the paper. While they are not able to explain the effect entirely, it is worth noting that the measure of peer composition considered may not be able to fully capture peer group effects. At the same time, the approach in Lavy et al. (2012) controls for any student-level unobservable (e.g., family background) and potentially unobserved changes that may affect student outcomes and peer quality similarly across subjects. The estimates of this paper, on the other hand, compound all these changes. An improved composition of the student body may improve student achievement through interactions of incumbent students with high-ability students. In addition, school-level compositional changes signal underlying changes in parents' "quality" (as measured by SES). A potential channel could also therefore be represented by an increase in teachers' effort or parental participation (Altonji et al., 2015). Alternatively, a recent work by Campos and Kearns (2023) shows that families place substantial weight on schools' academic quality, and this weight provides schools with competition-induced incentives to improve their effectiveness.

[31] Figure A.4 presents the corresponding estimates for the residence-based treatment, i.e., it documents how student composition changes in census blocks around the regenerations. Consistent with the findings outlined in Table 4, residence-based compositional changes are small, slightly positive and not statistically different from zero (except for grade 1 students' SES), and always considerably smaller than the school-based composition changes documented in Fig. 6.

[32] This approach relies on *within-pupil* regressions that exploit differences in the baseline achievement of a student's peers across subjects (math and language). Such models relate subject-to-subject variation in test scores for the same students to subject-to-subject variation in their peers' ability. The same approach cannot be used to study the relationship between test scores and other peer characteristics (e.g., the share of students ineligible for subsidized lunches), since there is no cross-subject variation.

9. Conclusion

A growing literature suggests that neighborhoods extensively affect children's future outcomes through neighborhood exposure. However, the specific mechanism driving these effects is not yet well understood. In this paper, I study how an inflow of more affluent households to a neighborhood affects the short-term schooling outcomes of children already living in the neighborhood. I address this issue by exploiting a wave of public housing regenerations in London into mixed-income developments.

A DID analysis reveals that a positive externality of public housing regenerations is an increase in the quality – in terms of student achievement – of nearby primary schools. Exploiting an IV strategy, this study provides convincing evidence that this externality is not simply driven by a compositional effect; instead, students originally attending a school close to a regeneration before its completion enjoy substantial benefits in terms of educational achievement at the end of primary school.

My findings highlight that rather than generating residential segregation, in the short term, these programs can potentially drive positive externalities for local children in terms of educational achievement. In addition, creating opportunities directly in deprived neighborhoods without having to relocate families can be important in contexts characterized by low-income families' lack of mobility (Bergman et al., Forthcoming). However, public housing regenerations seem to also drive an increase in house prices over time, making the neighborhood potentially less affordable for poorer households in the medium run. The latter finding suggests that in the medium-to-long run, the areas surrounding the regenerations could be targeted by wealthier households, which could eventually drive incumbent residents out; this is supported by a growing body of literature showing that more affluent households tend to target areas with good schools.

Finally, the achievement effects documented are plausibly driven by changes in local schools' peer composition. An improved composition of the student body may improve student achievement through interactions of incumbent students with high-ability students. At the same time, an alternative and potentially complementary channel is represented by changes in parental demand and school competition. While such changes cannot be directly assessed within the available data, understanding their relative role (if any) in fostering student achievement within neighborhood programs is of paramount importance. Disentangling the relative role played by these factors is an interesting avenue for future research.

Declaration of competing interest

I hereby declare that I have no relevant or material financial interests that relate to the research described in this paper.

Data availability

The data that has been used is confidential.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2023.105053.

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