Educational Tracking, Residential Sorting, and Intergenerational Mobility

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Abstract

I examine how student allocation rules impact achievement of students of different ability and socio-economic background. When the assignment rule shifts from exam to district based, a model illustrates that income relative to ability becomes a stronger predictor of student achievement and higher income households sort towards the better school districts. Using evidence from South Korea, I find that the impact of father's education, relative to one's middle school grade, on college entrance exam score increases twofold under district assignment. The change in housing land price is 13 percentage points higher in the better school district when the regime shifts.

Keywords: Intergenerational mobility, Educational tracking, School districts, Residential sorting

JEL Codes: I20, I28, J62, R21

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I. Introduction

Student allocation rules impact the educational development of individuals. This paper compares two secondary school student allocation rules: an exam based system, where schools choose students based on entrance exam results, and a district based system, where residential location determines school choice. How does the shift in student allocation rules affect achievement of individuals of different ability and socio-economic background?

Secondary school admission rules vary extensively across countries, particularly in the degree of ability tracking between schools.[1] For instance, Canada, the UK, and the US provide comprehensive public education and allocate students based on school districts and residential location. On the other hand, China and Romania track students by administering high school entrance exams and allocating students based on exam results.[2] Whether secondary education should track students by prior achievement is contentious and has even led to instances where countries shift education policies. The UK, Scandinavian countries, and South Korea shifted from achievement based secondary school allocation to district based assignment during the 1960s and 1970s. More recently, some major Chinese cities have made the same transition for middle school admission.[3] The policy rationale for this drastic regime shift was that educational tracking perpetuates inequality and randomly allocating students within school districts would likely reduce inequality (Betts 2011, Lai et al. 2011, Kang et al. 2008).

This paper demonstrates that, contrary to the policy rationale, the shift from exam based tracking to district based assignment exacerbates inequality and reduces intergenerational mobility. Intergenerational mobility is reduced in the sense that the achievement of poor students who did well in middle school decreases with the regime shift. Specifically, when I consider a student whose father's education level is in the 15^{th} quantile and own middle school score is in the 80^{th} quantile, his or her performance on the college entrance exam under the district regime is lower by about 1/5 of the standard deviation of test scores than what he or she would have obtained under the exam regime. The notion of intergenerational mobility I use in this paper is more specific than what is generally used in the literature, the intergenerational correlation of income (Solon 1992, Kopczuk et al. 2010). I am interested in the intergenerational mobility of high ability students from poor households relative to that of low ability

[1] Though ability tracking within schools exists in various societies, this paper examines student allocation rules to schools. Hence, I focus on between school tracking. Slavin (1990) and Duflo et al. (2009) examine ability tracking within schools in the US and Kenya, respectively.

[2] Many countries have a system that lies within this spectrum. Austria and Germany channel students to different tracks of secondary education, which differ in the degree of academic and vocational training. In Kenya, top-tier public secondary schools admit students based on exam results while admission to less prestigious secondary schools is location based.

[3] For instance, Xiamen, Shanghai, Changsha, and Beijing have fully or partially abolished exam based entrance to middle schools following the extension of compulsory education to 9 years.

students from rich households. I am interested in these students because they are near the margin where policy can impact their outcomes, relative to low ability poor students or high ability rich students.

The intuition for why achievement of a high ability student from a poor household is lower under the district regime is as follows. Under the exam regime households compete in high school entrance exam scores to gain access to good schools. Under the district regime households compete in the housing market to live in the better school districts to gain access to good schools. In short, households compete in test scores under the exam regime and compete in housing prices under the district regime. Household income directly buys housing while student ability predominantly determines test scores. Hence, different sorting arise under each regime. High ability students are matched to the better schools under an exam regime whereas high income households are matched to the better schools under a district regime. As the nature of competition shifts from exams to housing prices, the high ability student from a poor household, who would have gained access to the good high school under the exam regime, will no longer be able to benefit from the high quality school and perform lower under district assignment. This paper formalizes this intuition and empirically substantiates the predictions.

I develop a stylized model where households value student achievement and school quality is heterogeneous. The main prediction of the model is that, in the reduced form estimation of student achievement on household income and student ability, the coefficient on household income relative to the coefficient on student ability will be larger under the district regime. This prediction arises because of the direction of bias in the coefficient estimates under each regime. Since school quality is unobserved and more strongly correlated with income under the district regime, the coefficient estimate on income is biased upward more under the district regime. A similar reasoning applies to the coefficient estimate on ability which is biased upward more under the exam regime. Another prediction of the model is that higher income households sort towards and increase housing prices in the better school districts when the regime shifts.

I test predictions of the model using the variation in the timing of the regime shift across cities in South Korea (hereafter Korea) during the 1970s. Utilizing the policy change in Korea has several advantages over the literature that examines the UK and Scandinavian countries. The regime shift in the UK was endogenous because the local education authorities determined whether or not and when to shift. In Korea, the military dictatorship centrally implemented the regime change on short notice across several cities over seven years. The regime shift in Sweden was accompanied by the extension of compulsory education and the unification of curriculums. The policy change in Korea centered on the student allocation rule, enabling a focused evaluation rather than an analysis of a package of reforms. Lastly, the exam regimes in the European countries were often tied to location. That is, students would be channeled into certain educational tracks but students would attend schools in their locality. The exam based regime in Korea was strictly individual school based with no notion of location. Anyone could apply to any school in the country.[4]

Using father's education level and one's middle school grade as the main proxies for household income and pre-high school ability, I find that district based assignment increases the impact of father's education, relative to one's middle school grade, on college entrance exam score. Results remain robust whether I examine within occupation groups, control for mother's education, focus on the upper quantiles, or use other outcome variables of achievement. Geographically, I find this effect to be stronger in Seoul and the larger cities where average school quality and education levels are higher. To directly test whether residential sorting, as hypothesized by the model, is the underlying reason for the reduction in intergenerational mobility under the district regime, I examine the change in housing land price pre and post regime change across newly established school district boundaries. I focus on Seoul for analytical tractability but also because of a unique event that occurred concurrently with the regime shift. The government relocated then South Korea's most prestigious high school from the city center to the city periphery, in order to reduce central city congestion. This event exogenously divorces school quality from neighborhood characteristics, which helps the identification of residential sorting. I find that the change in housing land price in the better school district increases by about 13 percentage points when the regime shifts. I further examine the change in the number of households and confirm that the increase in price is a demand response and not a supply shift. The empirical results confirm that district based assignment, relative to exam based tracking, generates residential sorting which segments schools by income and lowers achievement of high ability students from poor households, thus, decreasing intergenerational economic mobility.

These results have implications for researchers and policy makers. The literature comparing ability tracking and district based comprehensive education have shown various results. Using cross-country data, Hanushek and Woessman (2005) find that tracking increases educational inequality but Waldinger (2006) finds that the importance of family background does not increase with tracking. Meghir and Palme (2005) examine the Swedish reform to comprehensive education and find that educational attainment increases especially for students from low socio-economic status. On the other hand, Galindo-Rueda and Vignoles (2007) use the UK reform and find that exam based selection increases test scores of high ability students whereas district based assignment increases test scores of low ability wealthy students. The contradicting results between the Swedish and UK reforms could be due to the difference in the extent of household sorting when they move away from tracking across schools. Meghir and Palme (2005) do not find evidence of sorting in Sweden but Manning and Pischke (2006) and Maurin and

[4] It was not uncommon for high ability students from smaller cities or rural areas to live with relatives or board in small rooms if they gained admissions to prestigious high schools in the major cities.

McNally (2007) find that the UK reform to district based secondary education is associated with changes in student achievement even at primary education, which suggests that households may have selected into districts with the reform. Even in the US, where schools differ in the degree of tracking within schools, Figlio and Page (2002) show evidence consistent with higher income households sorting to schools that group students by achievement into different classrooms. These studies point to the relevance of sorting in understanding intergenerational economic mobility.

There is a large literature that studies sorting in various forms. A large literature structurally estimates sorting (Nechyba 2000, Epple and Pratt 1998, De Bartelome 1990) or empirically examines equilibrium outcomes (Bayer et al. 2007, Rothstein 2006, Urquiola 2005, Black 1999). However, there is little direct empirical evidence of residential sorting. Baum-Snow and Lutz (2011) use the change in desegregation laws and examine sorting by race in the US. Similarly, I use an education regime shift to empirically confirm residential sorting. Furthermore, I show that people's behavior to sort and price education through the housing market can undo the initial goals of policy and exacerbate the persistence of inequality.

This paper relates to a developing country context of South Korea in the 1970s. Most studies on ability tracking and comprehensive education are based on the US or Europeans countries. Duflo et al. (2008) examine how tracking within elementary school affects individual achievement and teacher incentives and Lucas and Mbiti (2011) examine the impact of better schools under tracking in Kenya. However, I believe this is the first paper that examines how student allocation rules to schools affect intergenerational mobility in a developing country context. Also, the exam based high school admission policy in Korea then is similar to what we see in China, Romania, Kenya, and Ghana today. As many developing countries achieve universal primary education, their governments are now focusing on extending compulsory education and reforming secondary schools (World Bank 2005). Understanding how different student allocation rules impact the educational opportunity of students of different ability and socio-economic status will be important for structuring secondary education policies in these countries.

The paper proceeds as follows. I present a simple model and predictions for the empirical work. There are two parts to the empirical analysis. The first part tests predictions on achievement and intergenerational mobility. The second part tests predictions on the underlying channels of residential sorting using housing land prices. I conclude by discussing external validity and policy implications.

II. Theoretical Examination

The model aims to understand how students characterized by household income and ability are matched to schools of different quality under the two education regimes and derive testable predictions. I introduce a model in the spirit of Epple and Romano (1998) but with simplifications where school quality is exogenous, characterized by the quality of teachers, facilities, etc. I abstract away from peer effects and tutoring for simple illustration but later explain that introducing peer effects or tutoring does not change the main predictions of the model. Throughout this section I describe or footnote how well the model's simple setting actually reflects the two education regimes in Korea during the 1970s.

II.A Model of Household Behavior and Test Score Production

Consider a city where households are randomly distributed over N neighborhoods and each neighborhood has one high school of quality θ. Quality is teaching, administrative, or facility aspects that affect student achievement and is exogenously given.[5] Quality varies and the ranking of high schools are known. All other amenities are the same across neighborhoods. Schools are centrally financed, i.e., there is no local taxation for school financing and there is no transportation costs.[6] Each neighborhood (or district) has a fixed number of houses and each household consumes one unit of housing and pays a housing cost of r. Under exam based tracking students take high school entrance exams and schools choose students based on exam results. Once the regime shifts to district assignment, neighborhoods become school districts and students attend the high school in the district where they live.

Each household has one adult and one child and is identified by (y,a), where y denotes household income and a is the child's ability. The household's utility function $U(\cdot)$ increases with numeraire consumption c and the educational achievement of the child t, and is continuous and twice differentiable in both variables. High school achievement, i.e, performance on the college entrance exam $t=t(a,\theta)$ is a continuous and increasing function of child's ability a and high school quality θ. Each household maximizes $U(c,\ t(a,\theta))$ subject to the budget constraint $c + r = y$, which returns the indirect utility function $V(r,\theta;y,a) = U(y-r,\ t(a,\theta))$. One fundamental property naturally stems from the above set up. The implicit function theorem implies:

PROPERTY 1. Increasing bid-rent: $$\left.\frac{dr}{d\theta}\right|_{V=\overline{V}} = -\frac{\partial V/\partial\theta}{\partial V/\partial r} = \frac{U_2}{U_1}\frac{\partial t}{\partial\theta} > 0\ .$$

Property 1 characterizes the household's indifference curve in the *(r, θ)* plane. The indifference curve slopes up in the *(r, θ)* plane and illustrates the natural feature that people are willing to pay higher housing prices for better schools. In addition, I assume two properties of the household's indifference curve in the *(r, θ)* plane.

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[5] Contextually, high schools that were established earlier (early 1900s or before) generally had the highest reputations with the better teachers and alumni support in each city.

[6] Most high school students then used public transportation at a student rate and there were no convenient alternatives such as school buses.

ASSUMPTION 1. Single-crossing in income (SCI): $\dfrac{\partial r_\theta}{\partial y} = \partial\left(\dfrac{\partial V / \partial \theta}{-\partial V / \partial r}\right)\Big/\partial y > 0$.

ASSUMPTION 2. Weak single-crossing in ability (WSCA): $\dfrac{\partial r_\theta}{\partial a} = \partial\left(\dfrac{\partial V / \partial \theta}{-\partial V / \partial r}\right)\Big/\partial a \geq 0$.

Single-crossing in income states that all else equal, higher income households are willing to pay more for school quality. Sufficient conditions on U for single-crossing in income is $U_{11} \leq 0$ and $U_{12} \geq 0$, with at least one having strict inequality. Weak single-crossing in ability implies, all else equal, households with higher ability students will not pay less for school quality. Both are general assumptions in the literature. Appendix Figure 1 depicts the household's indifference curve in the *(r, θ)* plane and its single crossing property.

I do not explicitly model the school's objective function but point out what is relevant for equilibrium. Under the exam regime, each high school administers an exam and admits students based on entrance exam results. Schools care about reputation, either for prestige or alumni support, and aim to obtain the brightest students. Hence, schools optimize by choosing the highest performing students it can admit. On the other hand, under district assignment schools simply admit those who reside in their districts. Furthermore, schools can not charge differential tuition and receive the same subsidy from the government. Hence, schools' choices are muted under the district regime.

DEFINITION OF AN EXAM EQUILIBRIUM. An allocation of students across schools such that each student attends one and only one school is an exam equilibrium if and only if:

(i) all households maximize $V(r,\theta;y,a)$

(ii) student allocation clears in all schools.

The fact that school quality is known and that schools desire higher achieving students gives the following proposition, which describes how students are allocated to schools under the exam regime.

PROPOSITION 1. Household allocation to school quality is stratified by performance on the high school entrance exam in an exam equilibrium. Students with higher scores are matched with the higher quality schools.

Proofs are in Appendix A and the main text will provide the intuitions. Since schools want the best students, households want the best school, and slots are limited in each school, a cut off score for each high school will arise. The cutoff increases with high school quality and only students above each high school's cutoff will be able to attend that high school. Hence, schools become segmented by high school entrance exam score.

DEFINITION OF A DISTRICT EQUILIBRIUM. An allocation of households across districts such that each household lives in one and only one district is a district equilibrium if and only if:

(i) all households choose their residence to maximize $V(r,\theta;y,a)$

(ii) the housing market clears in all districts.

Now there are school districts and schools can not choose students. The household's location choice determines the school its child will attend. Everyone wants to live in the better school district and is willing to pay additional housing price to access the better district. The additional price each household is willing to pay is determined by its endowments (y,a) and the quality of the school. I formalize this statement in the following.

LEMMA 1 (BOUNDARY INDIFFERENCE). Consider two districts J and J+1 in a district regime. Housing price in district J is r_j and school quality is such that $\theta_{j+1} > \theta_j$. Then there exists $\tilde{r}$, a housing price in district J+1, with $\tilde{r}(r_j, \theta_j, \theta_{j+1}; y, a) > r_j$ such that $V(r_j, \theta_j; y, a) = V(\tilde{r}, \theta_{j+1}; y, a)$.

Since households trade off school quality with higher housing price, households now compete amongst each other in terms of their willingness to pay for school quality to obtain a house in the better district, which leads to the following proposition.

PROPOSITION 2. Household allocation to school quality is stratified by willingness to pay for school quality in a district equilibrium.. Households who are willing to pay more for school quality are matched with the higher quality schools and housing prices are higher at the better quality school districts in a district equilibrium.

To provide intuition, I illustrate a case where there are two districts, H and L, with school quality $\theta_H > \theta_L$ and housing price r_L in L.[7] By Lemma 1, every household depending on its income and ability *(y,a)* has a willingness to pay for the higher school quality θ_H that renders it indifferent between living in the two districts. If a household can pay less than its willingness to pay for θ_H and live in district *H,* it is better off and will move to district H. Each household's willingness to pay for θ_H can be ordered and the household with a higher willingness to pay will overcut the next household's willingness to pay by epsilon and live in district *H*. Because of the fixed number of houses in district H and market clearing, there is a marginal household who will be able to live in the last available house in district H. Its willingness to pay determines the market housing price $\tilde{r}$ at district *H*. This marginal household's willingness to pay also determines the locus of households that are indifferent between the two districts in the (y, a) space. Any household above that locus sorts into district H and those below remain in district L. An equilibrium outcome unique to district assignment is the existence of this housing price premium and identifying this price premium will be evidence of residential sorting later in the empirical analysis.

[7] Recall that the setting assumed identical houses with no neighborhood amenities other than schools. Hence, houses in the same school district have the same housing cost.

II.B Graphical Illustration of Equilibrium Properties

I consider the following simple parametric model to graphically illustrate equilibrium properties under each regime.

(1) $$U = (y - r) \cdot t(a, \theta)$$

$$t(a, \theta) = a^{\beta} \theta^{\gamma}, \quad 0<\beta<1,\ 0<\gamma<1$$

There are five schools ($\theta_5 > ... > \theta_1$), each in a different neighborhood or school district. Under the exam regime households sort based on performance on the high school entrance exam, $t_m(a, \bar{\theta}) = a^{\beta_m} \bar{\theta}^{\gamma_m}$, $0<\beta_m<1$, $0<\gamma_m<1$. I make the simplification that middle school quality $\bar{\theta}$ is homogenous across schools to focus on the high school allocation rules. What is important is that the variation of middle school quality is lower than the variation of high school quality. The contextual basis for this assumption is the fact that the military dictatorship closed down elite middle schools to equalize middle school quality but left the elite high schools intact (KEDI 1998).[8] Since middle school quality $\bar{\theta}$ is homogenous, student ability *a* directly maps into high school entrance exam scores.

Under the district regime households sort based on the willingness to pay for school quality. The above parametric example satisfies single crossing in income ($\partial r_{\theta} / \partial y > 0$) and exhibits a zero ability elasticity of demand ($\partial r_{\theta} / \partial a = 0$), so sorting towards schools under the district regime will be driven by income only. In other words, the willingness to pay for school quality $\theta_{j+1} > \theta_j$ for household type (*y*,*a*) living in district *J* and paying r_j is

(2) $$\tilde{r} = r_j \left(\theta_j / \theta_{j+1}\right)^{\gamma} + y\left(1 - \left(\theta_j / \theta_{j+1}\right)^{\gamma}\right)$$

which is increasing in household income and is neutral to student ability. Figure 1A and 1B illustrate the equilibrium allocation of households to schools in the (*y*, *a*) space. In an exam equilibrium, households segment into schools by ability because ability directly maps entrance exam scores. In a district equilibrium, households stratify based on willingness to pay for school quality and since willingness to pay is directly mapped by income in (2), households perfectly segment into schools by income. Figures 1C and 1D illustrate student achievement $t(a, \theta)$ in the (*y*, *a*) space given the allocation of households to schools in Figures 1A and 1B. The iso-achievement lines represent the set of households that obtain the same achievement level. In an exam equilibrium, ability solely determines achievement. However, under

[8] I can not test this directly because I do not observe school quality. However, if I compare the correlation between father's education level and middle school score and the correlation between father's education and high school exit exam score under the district regime, the former is 0.20 whereas the latter is 0.31. If father's education is strongly correlated with both high school quality and middle school quality, then a lower correlation between father's education and middle school score provides indirect evidence that middle school quality varied less than high school quality.

a district equilibrium, income affects achievement because higher income gives access to better schools. Thus, the gradient of income, when achievement is mapped into the (y, a) space, will no longer be zero but positive once the regime shifts from exam to district based assignment.

II.C Predictions for the Empirical Analysis

The simple model provided the intuition on how households differentially match to schools and the subsequent results on achievement under the two regimes. The following lemma manifests the differential sorting under the initial set up.

> LEMMA 2. Consider the space of households and high schools where household (y,a) is matched with school quality θ. All else equal, correlations will be such that $\mathrm{Corr}(y\theta)_\mathrm{d} > \mathrm{Corr}(y\theta)_\mathrm{e}$ and $\mathrm{Corr}(a\theta)_\mathrm{d} < \mathrm{Corr}(a\theta)_\mathrm{e}$ where d denotes the district equilibrium and e denotes the exam equilibrium.

Given Lemma 2, I introduce one of the main predictions of the paper.

> PROPOSITION 3. Consider the estimation of student i's college exam score t_i on household income y_i and student ability a_i where high school quality θ_{is} is unobserved,
>
> $$t_i = c + \beta y_i + \gamma a_i + \varepsilon_i \,. \tag{3}$$
>
> All else equal, $\hat{\beta}_d > \hat{\beta}_e$ and $\hat{\gamma}_d < \hat{\gamma}_e$ where $\hat{\beta}_d$ and $\hat{\gamma}_d$ are coefficient estimates of (3) under the district regime and $\hat{\beta}_e$ and $\hat{\gamma}_e$ are coefficient estimates of (3) under the exam regime.

The inequality results in Proposition 3 are based on the omitted variable bias due to unobserved school quality and the inequality in the degree of correlations as specified in Lemma 2. The error term in (3) can be decomposed into school quality θ and a random shock component u that is orthogonal to all variables in the model ($\varepsilon = \theta + u$). Then the estimated coefficients in (3), abstracting away from the random shock component, will be estimated with bias as below:

$$\begin{bmatrix} \hat{\beta} \\ \hat{\gamma} \end{bmatrix} = \begin{bmatrix} \beta + \kappa(\sum a^2 \sum y\theta - \sum ya \sum a\theta) \\ \gamma + \kappa(\sum y^2 \sum a\theta - \sum ya \sum y\theta) \end{bmatrix} \text{ where } \kappa = \frac{1}{\sum y^2 \sum a^2 - (\sum ya)^2} \,.$$

I drop the subscripts for simplicity. Assuming that the marginal and joint distribution of income and ability do not change, i.e., $\sum y^2$, $\sum a^2$, and $\sum ya$ are all positive and constant, then κ is positive and constant as well. Households sort differently towards school quality θ based on income y and ability a under the two regimes, and $\sum y\theta$ and $\sum a\theta$ will differ between regimes with inequalities as specified in Lemma 2. As a result, $\hat{\beta}_d > \hat{\beta}_e$ and $\hat{\gamma}_d < \hat{\gamma}_e$. Next, I describe when coefficient estimates would differ in magnitude under district assignment.

PROPOSITION 4. Consider a district equilibrium where schools are segmented by income, *all else equal*,

a) if city A's distribution of school quality first order stochastically dominates that of city B then β_d will be larger in city A.
b) if city A's distribution of household income first order stochastically dominates that of city B then β_d will be larger in city A.

If city A's school quality first order stochastically dominates city B's school quality then for any household, the probability of being matched to a better school is higher in city A. Hence, for all households E($y\theta$) will be larger in city A, rendering β_d larger. In Proposition 4b) school quality and the number of districts are fixed and a similar intuition holds. For each school quality θ the income level matched with each school is higher and E($y\theta$) will be larger in city A. The implications of proposition 4 is that cities that have dominantly better schools or richer population will likely see a larger jump in the coefficient on income when the regime shifts to district assignment. The larger cities in Korea had the higher quality schools that households from other regions wished to send their children to. Also, the larger cities have high income jobs. I later test whether the coefficient estimates differ by city size in the empirical analysis. Lastly, the following proposition, which is simply a restatement of the model's assumption of the random distribution of households over space and Proposition 2, clarifies when residential sorting would occur in a city context.

PROPOSITION 5. Conditional on no pre-sorting, that is, the random distribution of households over neighborhoods, the shift from an exam equilibrium to a district equilibrium will increase the housing price in the better school districts.

If income were already geographically correlated with school quality, so that high income households were already living close to the better schools under the exam regime, we would see no sorting. Furthermore, if housing prices were already discretely higher in the high income neighborhoods we would see no increase in housing prices.[9] Cities where income is less correlated with the better school will see more sorting once the regime shifts to district assignment and larger increases in housing prices at the better school districts. Given that cities are formed over a long period of time and the places where affluent people congregate over generations often have historic and prestigious high schools, pre-sorting is not an unlikely scenario. To confirm the theoretical predictions of sorting in the data, I would either need to know the degree of pre-sorting in each city or have a unique experiment that generates variation in school quality not or weakly correlated with neighborhood income. I do not know the former but have

[9] Even with pre-sorting, housing prices could increase. If the marginal household's willingness to pay for higher school quality is larger than the pre-existing price gap between the districts, then we would still observe housing price increases when the regime shifts even if households already pre-sorted.

instances of the latter. The relocation of the top-tier high schools in Seoul exogenously generated changes in school district quality. I utilize the relocation of schools to identify residential sorting in the second part of the empirical analysis.[10]

II.D Implications of Including Peer Effects or Tutoring in the Model

The above model assumed away peer effects and tutoring. I heuristically explain why adding peer effects in the achievement production will not alter the main prediction. With peer effects in the model households desire to gain access to high quality schools and high ability peers. Under an exam equilibrium, the better quality schools have the higher ability students and hence, automatically the higher ability peer groups. Higher ability students, regardless of income level, congregate at the better quality schools and form better peer groups. Thus, the nature of sorting under the exam regime does not differ whether or not peer effects are included in the model. Under the district regime, wealthier households can buy high quality schools through the housing market. If households have no information about the ability of other students, sorting will only be on school quality. If households know the ability of all other students then high income households can buy school quality by moving to the high quality district and buy high ability peers by moving to the district with the highest peer group. This could result in multiple equilibriums depending on the assumptions on how school quality and peer quality enter the achievement production function.[11] However, what is clear is that relative to the exam equilibrium, where schools are stratified purely by ability, income plays a stronger role in one's achievement because income can buy both better school quality and better peers. The model does not aim to formalize the degree to which one's achievement can be attributed to school quality relative to peer quality. Moreover, the data cannot empirically differentiate the two as well. However, the model does imply that income becomes a stronger predictor of achievement under the district regime even when peer effects are considered.

Finally, the model can also be enriched so that income directly impacts one's achievement via tutoring. Adding tutoring x in achievement so that $t = t(x, a, \theta)$ will not change the main implication unless one makes the drastic assumption that ability plays only a minor role in one's achievement and that achievement is predominantly determined by tutoring. Under reasonable assumptions, it still maintains that the gradient of income, when achievement is mapped in the (y, a) space, is larger under the district regime. Appendix B illustrates this point with simulation results of a model with tutoring. Now I turn to the data to test the predictions of Propositions 3 to 5.

[10] Note that pre-sorting does not preclude the predictions of Proposition 3 that intergenerational mobility decreases with the regime shift. The differential matching of households towards school quality still differs between the two regimes, regardless of pre-sorting. Pre-sorting is a condition for housing price differentials to be arise with the shift.

[11] Even in a simple example with four households and two schools, equilibrium can be configured in different ways depending on the specification of achievement production.

III. Background and Data for the Empirical Analysis of Achievement

III.A The Education Regime Shift – High School Equalization Policy

Demand for education in Korea surged after the Japanese occupation ended in 1945, and the elementary school entrance rate, which continued to rise through the Korean War, reached 96% by 1959. The large pool of elementary school graduates combined with the limited number of secondary schools made admissions to secondary school, which had been determined by exams, more competitive. Though, middle school entrance became exam free in the late 1960s, high school entrance continued to be exam based. Students applied to schools of their choice, took exams offered at that school, and the school admitted students with the highest grades. This system naturally generated a "tracked" system of high schools. Excessive competition and tutoring among the wealthier middle school students became a social issue. The military government announced the High School Equalization Policy (HSEP) in 1973, with the goal of eliminating competitive high school entrance exams and equalizing the quality of high school education across schools.[12]

The High School Equalization Policy created high school districts and assigned students randomly to schools within the district. Since the number of high schools was limited, the high school entrance pool was regulated by a high school qualifying exam. Those who pass the test would be eligible for general high school education but the school one attends would be randomly assigned. The military government initiated the reform starting with the largest cities in 1974. Appendix Table 1 describes the roll out of the regime shift by region and year. Seoul and Busan first shifted to district assignment in 1974, then the three other large cities Daegu, Incheon, Gwangju in 1975, then the nine provincial capitals in 1979, and lastly various regional cities in 1980.

III.B Individual Level Data

The main data used in the empirical analysis of achievement comes from the Korea Labor and Income Panel Survey (KLIPS). KLIPS is an annual panel survey of around 5000 households and 11000 individuals. The survey is conducted annually and asks various questions on the labor market and income dynamics of households and individuals. The first wave was conducted in 1998 and this survey continues to be administered. In addition to detailed family background information, the advantage of KLIPS for my purpose is the supplemental education survey conducted during the 11th wave. This supplemental survey provides information on individual educational history including the name, city, and entrance year

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[12] The policy initially had three goals: to equalize students, teachers, and facility. Equalizing student quality was the easiest and least costly to implement. The government shifted the student allocation rule from exam to district based selection. This change mechanically equalized student mix, initially. The other components were not successfully implemented because of the high costs associated with teacher training and facility improvement, and limited government budget (KEDI, 1998).

of the college, high school, and middle school one attended. I utilize the high school location and enrollment year information to identify each individual's exposure to either the exam or district regime. Another valuable aspect of the education supplemental survey is that it asks one's achievement level in middle school. Specifically, it asks one's middle school math, Korean, and English grade. Grades are reported in a 1 to 5 scale. I standardize each grade, take the average, and rescale so that the mean is 50 with standard deviation 10.

For the outcome variable I construct achievement measures using the average test score of admitted students for the college one attends.[13] As many colleges administered their own admission tests on top of the national test, the college one attends provides a consistent measure of achievement especially given that the hierarchical ranking of colleges remained robust. The Data Appendix illustrates the ranking of colleges for 1976 and 1994 and describes how I construct the average college score measure. I also examine dummy outcome variables of whether one attends a top tier college.

Finally, I restrict my sample to the set of individuals who answer the education supplemental survey. Since my study examines the effect of secondary school policies on achievement, I further restrict to individuals who went to high school and provided answers to school location and entrance year, so I can identify which education regime he or she was in. Several cities that did not shift to district assignment in the 1970s later shifted after 2000. However, starting in the mid 1980s elite special purpose schools that administered their own competitive exams were being established and gradually became an influential part of the general education. Moreover, the policy changes that occurred later were endogenous in the sense that the local governments chose whether or not to shift.[14] Cohorts that entered high school before 1970 were born during the Korean War and are subject to selective survival or selective birth by income level. Also, the observation in the data drops considerably for pre 1970 cohorts. Therefore, I restrict my analysis to individuals who entered high school between 1970 and 1985.[15] Summary statistics for this sample is presented in Table 1 Panel A.

III.C Reduced Form Interpretation

The actual variables used to proxy income y, ability a, and achievement t determine the interpretation for the empirical work. For y, I use father's years of education. Since the outcome variable is college score, the straightforward interpretation of the empirical work will be the impact of father's

[13] KLIPS directly asks college entrance exam scores only for more recent cohorts.

[14] As of today over 70% of all high school students in Korea are under district assignment. Some cities still maintain exam based tracking.

[15] Also, the distinction between public and private high schools is not relevant for analysis. Private schools were subsidized by the government, did not have the autonomy to charge their own tuition or admit students, and operated in the same manner as public high schools. After 2000, "independent" private schools less constrained by the government emerged. However, this is outside the time scope of the paper's analysis.

education level on the child's college entrance outcome. Using father's years of education has several advantages over income measures. As Solon (1992) outlines, measuring permanent income, particularly of the parents, based on survey reports suffers from measurement error. On the other hand, father's years of education is relatively noise free and a good proxy for long-term income.[16] In addition to father's years of education, I use occupation group and mother's years of education as further robustness checks.

I proxy ability a using middle school score which has the advantage of capturing pre-high school ability unaffected by high school education or policies. Middle school score contains information of not only ability but also household background. Therefore, the empirical work asks how the effect of father's education β, in addition to what is already captured in the coefficient on middle school score γ, changes with the shift to district assignment.

The outcome variable t, the average test score of the admitted students of the college one attends represents the hierarchical ranking of colleges. Given that ranking of colleges has various long term impacts in society, it is an interesting variable of itself. Appendix Table 2 provides a simple but stark dimension of attending a better tertiary institution in Korea. A regression of the log of one's average income on the type of college one attends indicates that attending a top 3 college is associated with earnings that are about 153% higher than not attending any college and about 66% higher than attending a college outside the top 30 during the sample period. What the public perceives is the fact that better colleges imply better outcomes, regardless of whether it is due to value added, signaling, or selection.

Moreover, because of the institutional setting of college entrance exams, the average test score of the college one attends is a valid proxy for individual achievement. Students could apply to only one college and had to apply first and take the entrance exam for that school afterwards. Admission being strictly determined by test results, applicants with the higher scores would fill the admission pool until the last available slot.[17] The fact that one already had to tie one's hand to one school removes any factors that determine choice after performance on the exam. There could be preference factors in deciding which school to apply to before taking the exam (e.g., distance, friends, etc.). But if these variables do not affect achievement conditional on the variables in the model, estimated coefficients using average test score will not be biased. The main reason is because one's performance on the exam determines with probability one whether one attends that college or not. As Appendix C illustrates, this makes the conditional expectation of one's test score equal to the conditional expectation of the average test score of the college one attends.

[16] Kremer (1997) uses education level as the outcome to examine intergenerational mobility and inequality due to sorting.
[17] Those who fail would either apply to a low-tiered college at a later date or retake the exam the following year.

Lastly, around 60% of the students did not enter college during the sample period. Hence, the average college score variable is not observed for those students and is bottom coded. The bottom coding will not affect the estimation at the mean as long as the selection rule discussed above holds. That is, those who do not attend college are generally lower achieving students. Appendix Table 3 examines whether this is reasonable by comparing middle school score for those who do not enter college to those who go to college but one that is outside the top 30. Those who do not go to college on average have a significantly lower middle school score: 6 points lower when the overall standard deviation is 10 points. The standard error is very small at 0.037. This supports the assumption that those who do not go to college are low achieving students. In the empirical section, I also perform quantile regressions on the 70th and 90th quantiles. Since observations in the upper quantiles are less likely to be bottom coded, the upper quantile regressions provide additional robustness checks.

IV. Empirical Results of Achievement

IV.A Patterns of the Data and the Estimating Equation

I first examine patterns in the data. Figure 2 provides a first look at the evolution of intergenerational mobility by plotting the coefficients in the regression of one's college score on father's years of education by high school entrance year. Coefficients on cohorts that enter high school before the mid 1970s hover around 0.4 and then steadily increase reaching 0.8 for the late 1980s cohort. The coefficients imply that a year increase in father's education was associated with an increase of around 0.4 points in one's college entrance exam score but the association increases more than twofold for the later cohorts. What is noticeable is that this rise starts in the mid 1970s which is when the regime shift started. Figure 3 examines if this was the case for the cities that shift to district assignment by plotting the coefficients for a similar regression but by policy cohorts. The jump in coefficient for cohorts within the first few years of regime shift provides evidence that the increase in the impact of father's education is associated with the education regime change. I next turn to the regression framework to examine the significance of this pattern and test the impact of father's education relative to that of one's middle school score.

An important aspect for estimation will be to control for all other potential sources of endogeneity other than school quality θ. I include city, high school entrance year, and age fixed effects in the reduced form estimation. City fixed effects capture city specific variations in the education environment such as the administrative capacity of the city's education system. Also, it can capture city specific externalities or city specific peer effects that affect individual achievement. Year fixed effects capture overall time trends and age fixed effects allow for the fact that the high school entrance age varies in the data. With fixed effects included, the underlying achievement of individual *i* who went to high school in city *j* in year *m* at age *k* can be expressed as:

$$(4) \qquad t_{ijkm} = c + \beta\ y_{ijkm} + \gamma\ a_{ijkm} + Z_{ijkm}\pi + \mu_j + \eta_k + \lambda_m + (\theta_{is} + u_{ijkm})$$

where μ_j, η_k, λ_m each denote the city, age, and high school entrance year fixed effects. Z includes additional individual level variables: gender and whether the mother was the primary earner in the household. The error term is composed of the school quality component θ_{is} and a random shock component, so that $\varepsilon_{ijkm} = \theta_{is} + u_{ijkm}$. If equation (4) represents a reasonable approximation of the achievement production then the switching regression framework

$$(5) \qquad t_{ijkm} = c + \beta_1 y_{ijkm} + \gamma_1 a_{ijkm} + \beta_2 y_{ijkm} D_{jm} + \gamma_2 a_{ijkm} D_{jm} + \delta D_{jm} + Z_{ijkm}\pi + \mu_j + \eta_k + \lambda_m + \varepsilon_{ijkm}$$

where D_{jm} is an indicator of city *j* in year *m* being under district assignment, will return the set of reduced form coefficients $(\hat{\beta}, \hat{\gamma})$ that takes the differential sorting of each regime into account. My main interest, as implied by Proposition 3, is in whether $\hat{\beta}$ is larger under the district regime relative to the exam regime and vice versa for $\hat{\gamma}$.

However, rather than just comparing each coefficient between regimes, there are reasons to compare the ratio of coefficients for a more robust analysis in the empirical work. The actual dependent variable, test score, is fixed in range across all years but the variance of the right hand side variables, e.g., father's years of education, differed between regimes during the study period. [18] Then a simple comparison of coefficient estimates between the two regimes can render incorrect interpretation. For instance, a decrease in the variance of father's education would automatically return larger coefficient estimates because the range of the dependent variable is fixed. In other words, when the dependent variable is fixed, the change in one coefficient estimate over time captures not only a policy affect but also the change in the variability in the factors that determine achievement. Taking the ratio of estimates, $\hat{\beta}/\hat{\gamma}$, cancels out the bias generated because the variability in father's education changed overtime and enables a focused evaluation of the policy effect.

More formally, given that achievement is mapped into standardized scores, what one actually observes is relative achievement. That is, we observe $s = (t - \bar{t})/\sigma_t$ where $\bar{t}$ is the mean and σ_t is the standard deviation of true achievement. Rewriting notation so that, $s = \phi t + \eta$, what one is actually estimating when one uses test scores is a scaled factor of true achievement with some additive term. This is not a big concern when we perform cross-sectional estimation. However, the scaling becomes

[18] The variance of father's years of education for those who went to high school in the 1960s is 4.65 whereas those who went during the 1980s is 4.07 in my sample.

problematic when we compare coefficient estimates over time. Suppose that the scale factor ϕ varies between the exam and district regime. In the regression of standardized entrance exam score on father's education and one's middle school score:

$$s = \eta_e + \phi_e \beta_e y + \phi_e \gamma_e a + \varepsilon_e \qquad \text{under the exam regime}$$

$$s = \eta_d + \phi_d \beta_d y + \phi_d \gamma_d a + \varepsilon_d \qquad \text{under the district regime,}$$

what we actually estimate under each regime are coefficients biased because of the omitted variable bias multiplied by the scaling factor for each regime, i.e., $\phi_e \beta_e$, $\phi_e \gamma_e$, $\phi_d \beta_d$, and $\phi_d \gamma_d$. Suppose that the scaling factors are considerably different between the two regimes so that $\phi_d > \phi_e$. Then even though the omitted variable bias implies $\gamma_d < \gamma_e$, we would not know the direction of inequality between $\phi_d \gamma_d$ and $\phi_e \gamma_e$, the coefficients that we actually estimate. Taking the ratio of coefficient estimates $\hat{\beta} / \hat{\gamma}$ will cancel out the scaling bias and hence, will imply that $\hat{\beta}_d / \hat{\gamma}_d > \hat{\beta}_e / \hat{\gamma}_e$. A formal proof on the last point is provided in Appendix A. Comparing the ratio of coefficients between regimes not only provides a robust test on how the impact of father's education controlling for own middle school score changes in the empirical analysis, but also concisely captures the notion of intergenerational mobility that this paper is interested in.

IV.B Main results on intergenerational mobility

Table 2 column (1) provides estimates of the base regression as specified in equation (5) but restricting the constant term to be the same across regimes. Under the exam regime, an additional year of father's education is associated with a 0.26 point increase in college score. With the shift to district assignment the effect nearly doubles to a 0.47 point increase. With the regime shift, father's education becomes a stronger predictor of which college one enters even when one's middle school performance is controlled for. On the other hand, the coefficient on middle school score, though statistically not different, decreases from 0.38 to 0.36 with the regime shift. The changes in the direction of coefficients are precisely in the directions predicted by Proposition 3. The magnitudes in column (1) imply that under district assignment, children whose fathers graduated college on average would attend a college about 2 points higher than those whose fathers graduated only high school. This amounts to about 23% of the standard deviation and is equivalent to moving up from a 4 year college outside of Seoul to the more preferred 4 year colleges inside Seoul.

The bottom panel in Table 2 provides the empirical test of whether the coefficient on father's education relative to that on middle school score increases with the regime shift, i.e., I estimate the test statistics $t = \beta_d / \gamma_d - \beta_e / \gamma_e$ and report the p-value of the one sided hypothesis test $H_0 : \beta_d / \gamma_d = \beta_e / \gamma_e$ and $H_1 : \beta_d / \gamma_d > \beta_e / \gamma_e$ by testing t >0. The test statistic in column (1) is 0.63 and the one sided test is statistically significant at the 1% level, confirming the prediction that intergenerational mobility decreases with district assignment.

In column (2) I allow the constant term to change between regimes and include the district assignment dummy variable in the regression. Similar to column (1), the coefficient on father's years education is 0.23 under the exam regime and increases by 0.25 with the shift to district assignment. The coefficient on middle school score actually increases slightly 0.08 with the regime shift but is not statistically significant at the 5% level. This slight increase is accompanied by a strong negative coefficient estimate on the district assignment dummy variable. Since middle school score would also contain information of household income in addition to the student's ability, it is not surprising that we find a slight increase in the impact of middle school score with the regime shift. Nonetheless, the ratio test in the bottom panel shows a statistically significant reduction in intergenerational mobility as in column (1). To put column (2) results into context, consider a high ability student from a poor household: his father's year of education is one year (no formal education; about 15% of the sample) and his middle school score is 55 (about 80th percentile of the distribution). Based on estimates from column (1), his achievement under the district regime would be about 1.7 points lower than what he would have obtained under the exam regime. This accounts to about 20% of the standard deviation of college scores.

The model predicted that the coefficient estimates would change because of the differential sorting of households to school quality. If I could control for school quality then I would not expect the coefficient estimates to differ between regimes. I do not have measures of school quality in the data but can generate high school fixed effects and include them to the base specifications of columns (1) and (2).[19] As I am controlling for the fixed school quality aspects, I expect the change in coefficient estimates to be much weaker. Columns (3) and (4) report results based on all identifiable high schools and columns (5) and (6) report results on the sample of high schools where there are at least 5 observations. Consistent with the predictions, none of the interaction terms in columns (3) through (6) are significant and there is no statistically significant evidence that the ratio of coefficients increased post regime change. The base results in columns (1) and (2) and the high school fixed effects results confirm that the regime shift reduces intergenerational mobility and supports the idea that the differential selection towards unobserved school quality is the underlying reason.

IV.C Intergenerational Mobility by City Size

There could be several reasons why intergenerational mobility differs across cities in a district regime. Proposition 4 points to how the distribution of school quality or distribution of income can affect

[19] I manually went through the list of high schools to clean the matches and provide unique codes to each high school name. The sample drops from 2228 to 2160. This is because I can not identify some of the high school names or the high schools were outside of South Korea.

the magnitudes of the bias and hence the estimated coefficients. Cities that have higher quality schools, higher educated households, or more in-migration of higher educated households will likely have a larger coefficient estimate on father's education or a larger ratio estimate under the district regime. Since information on the distributions of school quality or education levels are not available, I test these predictions by estimating by city size.

Seoul and the large metropolitan areas had the prestigious high schools that households wanted to send their children to and the high wage occupations. The regime shifted in the bigger cities in 1974 and 1975 and then to the smaller cities in 1979 and 1980, which makes analysis by city size equivalent to analysis by years. I estimate equation (5) with Seoul in Table 3 columns (1) and (2), the other large cities that shift regimes in 1974 or 1975 in columns (3) and (4), and lastly the cities that shift regimes in 1979 or 1980 in columns (5) and (6). The coefficient estimates follow the same pattern as the base results in Table 2 but the coefficient estimates on the father's education and district assignment interaction term are less pronounced for the smaller cities. The ratio estimates present a consistent pattern. The ratio test statistics are positive and significant for Seoul and the large cities, whereas the test statistic for the smaller cities is smaller and insignificant. In other words, estimates of β_d/γ_d are greater in the larger cities implying lower intergenerational mobility for these cities.

To examine the timing of the impact, I estimate coefficients by city groups over time by the following equation which is the interaction term analog of the switching regression with the city groups.

$$
\begin{aligned}
t_{ijkm} = c &+ \sum_{l=1}^{7}(y_{ijkm}\cdot d_l)\beta_{1l} + \sum_{l=1}^{7}(a_{ijkm}\cdot d_l)\gamma_{1l} + \sum_{l=5}^{7}(y_{ijkm}\cdot d_l\cdot D_{jm})\beta_{2l} + \sum_{l=5}^{7}(a_{ijkm}\cdot d_l\cdot D_{jm})\gamma_{2l} \\
&+ \sum_{l=3}^{7}(y_{ijkm}\cdot d_l\cdot D_{jm}\cdot C_{jm})\beta_{3l} + \sum_{l=3}^{7}(a_{ijkm}\cdot d_l\cdot D_{jm}\cdot C_{jm})\gamma_{3l} + \mu_j + \eta_k + \lambda_m + \varepsilon_{ijkm}
\end{aligned} \tag{6}
$$

where d_l is a dummy that indicates whether individual i enters high school in the years grouped as in Figure 4, D_{jm} is the dummy for treated cities, and C_{jm} is a dummy for the 1974 or 1975 cities. Appendix Table 4 reports the coefficients and Figure 4 plots the ratio of the estimated coefficients. As in Table 3 the effects are stronger in the larger cities that shift regimes in 1974 or 1975. Also, the evolution diverges from the control group after 4 to 5 years into district assignment. For the 1979 and 1980 cities the effect seems to be more immediate but the divergence is smaller.

IV.D Controlling for Mother's Education and Father's Occupation

Table 2 shows that father's education becomes more important relative to middle school achievement in determining one's college score under district assignment. I further control for mother's education and father's occupation to indirectly test if these results are consistent with household income playing a role. In Table 4 Panel A, I include mother's education in addition to father's education for the set of households where the father is the primary earner.[20] Column (1) replicates the base specification for this subset. Column (2) includes the mother's education component. Under the exam regime, both father's education and mother's education are significant with mother's education being the stronger predictor. After the shift to district assignment the coefficient on father's education more than doubles but the coefficient on mother's education interacted with district assignment is virtually zero. Including only the mother's education component in column (3) returns the same pattern.

These results support the models prediction that household income plays a stronger role under the district regime if father's education proxies household income and mother's education proxies some other features of the household. Panel B additionally controls for the occupation group of the working father. I use the 10 occupation groups used in the Labor Statistics Annals. I examine the differential effects of father's education and mother's education within each occupation group. Higher educated fathers within an occupation category are likely to earn more and the results show the same patterns as in Panel A. High income households are pricing out low income households from access to good schools more so under the district regime.

IV.E Robustness Checks

Table 5 presents various robustness checks.[21] Columns (1) and (2) are quantile regression results at the 70^{th} and 90^{th} quantile. I examine the higher quantiles because the estimates in this range are less sensitive to the bottom coding of college scores. Less than 5% of the fitted values for the 90^{th} quantile are bottom coded. The estimates in columns (1) and (2) are consistent with Table 2 column (1). Results are not sensitive to the bottom coding of college scores. Column (3) uses whether one enters a top 20 college as the dependent variable. It shows that father's education relative to middle school score becomes a stronger predictor of whether one goes to a decent college in the district regime.

I next test whether results are sensitive to whether one attended a vocational high school or not. The regime shift concerned general high schools only and vocational high schools were considered as alternatives for low achieving students. However, given that vocational high school students also went to college (20% of vocational and 50% of general high school students attended college during the sample years), and often to the lower ranked colleges (college score of 50 compared to 56 for general high school students, conditional on going to college), excluding vocational high school students and simply focusing on general high school students would result in sample selection on the outcome variable. The quantile regression results in columns (1) and (2) could also serve as a sensitivity test to this issue because the

[20] I present the results from equation (5) in the main text and present results where I constrain the constant terms to be the same across regimes in Appendix Table 5. The two tables show similar patterns.
[21] Appendix Table 6 presents results from the specification where the constant terms is constrained to be the same across regimes.

upper conditional quantiles would most likely be comprised of general high school students only. In column (4), I include the high school type and its interaction with the year dummies. Given that the high school type is mostly determined by covariates in the model (father's education and one's middle school score), including the general high school dummy would not generate additional endogeneity. The results are similar to the base result and confirm that the main prediction is not sensitive to the high school type or the change in its composition over the years.

The next set of robustness implements counterfactual exercises using placebo policy years. I test if results are sensitive to policy years when I assume that all the cities that shift to district assignment shifted before or after when the policy actually took place. Columns (5) and (6) assume policy years of 1965 and 1969 respectively and column (7) assumes a policy year of 1985. For each specification I examine the window five years before and after the placebo policy year. None of the one sided hypothesis tests are significant, as well as, any of the coefficients on the interaction terms. These set of placebo tests show that the results are driven by the regime change in focus.

Some of the students that entered high school in 1970 or 1971 had to take exams to enter middle school. This should not affect results since those students were under the exam based regime for high school as well. Nonetheless, I exclude these students by focusing on the period after 1972. Results remain the same as reported in column (8). Lastly, I use the income reported over the multiple waves of KLIPS to generate CPI adjusted average income measures and see if the regime shift had significant effects on income. The coefficient on father's education becomes significant after the shift to district assignment and the ratio of coefficients are significantly larger under the district regime as well.

The empirical results up to now show that the transition from an exam to a district based system increases the impact of father's education on one's college score, controlling for middle school grade. The differential impact goes away once I control for the high school one attends. The model predicted that the different matching of household income and student ability to school quality between the two regimes would generate this differential impact. In the next sections, I provide more direct evidence on the implied channel of residential sorting: higher income households sorting to the better school districts.[22]

V. The Regime Shift in Seoul and Descriptive Evidence of Sorting

V.A Background on the Regime Shift in Seoul.

As discussed in Proposition 5, if high income households pre-sort near the high quality schools, residential sorting and the subsequent changes in housing land prices may not be identifiable when the

[22] Though the theoretical model did not have predictions on average achievement, I examined the reduced form average effect of the regime change. Though estimates are statistically not significant when high school fixed effects are included, the point estimates suggest an overall negative impact of the regime change. Appendix D elaborates more on the estimation and results.

regime shifts. A unique event that occurred in Seoul at the onset of regime change, i.e., the relocation of several prestigious high schools from the downtown area to the periphery, in order to reduce central city congestion, helps identify residential sorting in this section. In addition to this exogenous change in school quality, I focus on Seoul for a few other reasons. The results in the previous sections show that the increase in the impact of father's education is strongest in the larger cities. Also, Seoul is the dominantly larger city with a large enough number of neighborhood observations for empirical analysis.

When the regime shifted in Seoul in 1974, five school districts were created based on middle school districts. Figure 5 illustrates the five districts and the number of high schools in each district. In general, two to four middle school districts formed one high school district and students graduating from middle schools would be randomly allocated to one of the district's high schools. However, many high schools were disproportionately concentrated near downtown, and moreover, the top-tier schools were mostly in downtown. Those living far from the city center and attending middle schools in the outer districts would have a clear disadvantage if the downtown area formed its own separate high school district. Hence, the city drew high school district boundaries so that they extended out from the city center and created a coalition of high schools within a 4km radius circle in downtown called the Unified Central District (UCD). The UCD was drawn on top of the original five districts. Each student would be randomly matched to a high school from the pool of all schools in his or her own district and those in the UCD.[23] The slots for the UCD from each district were adjusted by population so that about 50% of students from each original district would be allocated to high schools in the UCD.[24] Since everyone had the similar probability of attending a school in the UCD, people would perceive district quality based on the high schools in their own districts.

Meanwhile, in October 1972, the Education Minister announced that Gyeonggi High School, the ranking one high school, would relocate from downtown to the outskirts of District 3. The announcement was initially met with strong disagreement by the alumni but eventually the relocation took place and the new campus opened in District 3 in 1976. As shown in Appendix Table 7 Gyeonggi High School was the unambiguously top school back then dominating entrance to Seoul National University (SNU) under the exam regime. More than 50% of its students would gain admission to SNU annually. High schools were often ranked by the number of students admitted to SNU. Hence, the relocation of Gyeonggi High School

[23] There was the option to include only the schools in one's district in the matching pool. For example, a girl attending middle school in District 1 could choose to include just the four girl high schools in District 1, or include all 20 girl or co-ed high schools in the UCD in addition to the 4 district schools in the pool where she would be randomly allocated to. Since schools in the UCD were generally better than schools in the outer five districts, most students chose to include the UCD. According to the Feb 14, 1974 article in Kyunghyang Shinmun, over 90% of students included the UCD in their application.

[24] The UCD became smaller over the years and was eventually abolished in 1980. This could explain the trends in Figure 4. The ratio of coefficients for the larger cities increases about four to five year after the regime shift. On the other hand, the smaller cities which did not have a UCD see immediate response from the regime shift.

and access to it without any exam was an attractable option to many households. Hweemun High School, one of the top-tier private high schools, finished construction of its new campus in 1978 in District 3 and sold its old campus in the city center to Hyundai Group. Seoul High School, a school second only to Gyeonggi High School, relocated to District 3 in 1980. Several other high schools followed suit. Appendix Table 7 lists top-tier high schools in Seoul and their location changes to District 3. What these relocations imply in terms of timing is that when the policy shifted from exam to district assignment in 1974, people already knew that the most prestigious high school and other top-tier high schools would relocate to District 3 in years soon to come. This exogenous relocation of high schools increased the expected school quality in District 3 and created the opportunity to attend the most prestigious high schools simply by living in District 3.[25]

Other than the perception of historical prestige, households did not have much information to assess the quality of school districts. Schools that do well would often put up billboards of the number of students accepted to prestigious colleges but in general there were no publically available objective measure of school quality. My own search for high school quality in the 1970s mostly yielded articles on how a number of prestigious high schools faired in sending their students to the top three or five universities. I did find one newspaper article that listed the college eligibility exam pass rate for each high school in Seoul for 1969 only, which was the first year that exam took place. I collected this school level information and linked it to the 1974 districts to generate district averages. I use this measure as an example that quantifies what the school relocations to District 3 implied. The Data Appendix describes the construction of the measure and the newspaper sources and the table in Figure 5 presents the average quality for each district.[26] Other than District 2 the outer districts had a much lower average pass rate than the UCD.[27] For district 3, I include Gyeonggi high school, which had a pass rate of 98%, in the calculation. The number in parentheses for District 3 indicates what the expected pass rate would have been had Gyeonggi high school not move there. The movement of this one school increases the average pass rate drastically from 0.4 to 0.6. The other high schools that later move to District 3, Seoul high school and Hweemun high school, had pass rates of 99% and 84%. Though the college pass rate measures tell only one part of the story, it confirms that the relocated schools, in addition to sending many students

[25] A case about sorting based on household risk aversion and the variance of school quality within a district could be made. However, during this period the people were more concerned about the quality difference between schools in the UCD and the outer districts rather than between schools within a district. As the table in Figure 6 indicates, there were 46 schools, many of them good, in the UCD and only 7 to 11 in the other districts, most of them of poor quality.
[26] For district i, I calculate 0.5(pass rate of UCD)+0.5(pass rate of district i), i=1,..,5.
[27] District 2's high average pass rate is driven by the then relatively new Annex High School to the College of Education of Seoul National University. Though its performance in terms of college pass rate was good, it was not particularly known for sending their students to the top universities.

to top universities, did well in sending students to any college.[28] I expect that households would have sorted towards these schools and as a result increased housing land prices in District 3 relative to the other districts when the regime changed.

V.B The Data and Descriptive Evidence of Residential Sorting at the District Level

I first descriptively compare how the population composition changed at the school district level using census data in Figure 6. The 1975 and 1980 census provides information on the number of college graduates by 5 year age groups for each administrative district. Each school district is comprised of two to four administrative districts. As Seoul was undergoing considerable population growth, several districts were split by 1980. Hence, I aggregate to the school district created in 1974 as in Figure 5. The black line in Figure 6 indicates the change in the percent of college educated for each age group between 1975 and 1980 for District 3. The share of college graduates increases steadily from the younger age group, peaks at the 40 to 44 age group and then continues to decline. The other four districts track each other with percentage change for the younger age group around zero and the older cohorts slightly above zero. What is stark in Figure 6 is how the college educated people in the age group with school aged children (ages 30 to 49) differentially sorted towards District 3. The difference between District 3 and the other districts become much smaller for those above 50 years old. Figure 6 indicates that higher income households with school aged children responded to the regime shift by moving towards the newly formed high quality school district.

Though Figure 6 is consistent with residential sorting, examining a 5 year span during periods of urban development in District 3 could raise concern that other factors potentially have played a role. For instance, a differential increase in jobs that particularly cater towards college educated people between the ages 30 to 49 in District 3, compounded with plentiful housing supply between 1975 and 1980, could also return patterns consistent with Figure 6. Thus, I now focus on the narrower time period of 1973 to 1975. Since detailed population data is not available for this time period, I use housing land price data from now on.

The data I use is the neighborhood-level housing land price appraisal data assessed by the Korea Appraisal Board. Appraisal is based on transactions of comparables and is believed to reflect true market value. Representative housing lands for high, medium, and low quality location in each neighborhood area were assessed annually and created in reports. I copied the reports on Seoul for 1971 to 1977 and entered the data manually for all odd number years for this analysis. I also generate a center point for each

[28] An important point for analysis is whether school quality remains the same once the regime shifts. Teacher quality, facility, and resources would not have changed drastically in the time frame examined. As shown in Appendix Table 7, Gyeonggi High School and Seoul High School ranked number one in terms of students sent to SNU among district based high schools in Seoul, even in 1980.

neighborhood and calculate the distance to district boundaries using GIS software and collect the number of households for each neighborhood for the same period. I match neighborhoods from each data set and obtain 662 observations. Table 1 Panel B provides summary statistics for these variables.

Table 6 reports the regression of the change in log housing land prices between 1973 and 1975 on district dummies and controlling for the location quality. The omitted district is District 1 in column (1). The difference in the price change is largest and significant for District 3 at 0.15. No other district reports a significant increase or drop. Column (2) omits all other districts other than District 3 and returns an estimate of 0.13 which is also significant. The population patterns in Figure 6 and the results in Table 6 indicate that residential sorting of high educated households predominantly occurred towards District 3 when the regime shifted and resulted in differential increases in housing land prices.

VI. Identification and Estimation of Residential Sorting

VI.A First Difference Estimation with a Regime Shift

If school quality additively enters the housing land price equation when the regime shifts then the first differenced regression in Table 6 would return precise estimates of the valuation of school quality. However, it may well be the case that the underlying hedonic framework for housing land price under each regime differs because school quality newly enters the hedonic equation under the district regime, altering the marginal valuation of the other variables in the model. Denoting the exam regime 1 and the district regime 2, the hedonic framework can be expressed as

$$P_{2ijd} = \alpha T_d + \beta_2 X_{2ijd} + \gamma_2 Z_{2jd} + \varepsilon_{2ijd}$$

$$P_{1ijd} = \beta_1 X_{1ijd} + \gamma_1 Z_{1jd} + \varepsilon_{1ijd}$$

where P_{ijd} is the price of housing land i in neighborhood j in district d. T_d represents the quality of school districts or the set of school district dummies as in Table 6. X_{ijd} represents the characteristics of the housing land and Z_{jd} represents neighborhood characteristics and district characteristics other than school quality. I allow the valuation of both X and Z i.e., β and γ, to differ between regimes. Taking first differences:

$$\text{(7)} \qquad \Delta P_{ijd} = \alpha T_d + \Delta(\beta X_{ijd}) + \Delta(\gamma Z_{jd}) + \varepsilon_{ijd}.$$

Note that the differenced variables can be decomposed so that $\Delta(\beta X_{ijd}) = \Delta\beta X_{2ijd} + \beta_1 \Delta X_{ijd}$ and $\Delta(\gamma Z_{jd}) = \Delta\gamma Z_{2jd} + \gamma_1 \Delta Z_{jd}$. Hence, even if one assumes that the land and neighborhood characteristics remain the same so that ΔX and ΔZ are zero, the marginal valuations in the equation may differ across regimes so that $\Delta\beta$ and $\Delta\gamma$ are not zero and the second and third terms in equation (7) do not disappear. Controlling for all the characteristic variables, i.e., $X, \Delta X, Z, \Delta Z$ would be ideal for estimating equation (7). However, in reality I do not observe most of these variables. Moreover, I am only interested in identifying

α. The strategy I employ is to control for the location of each neighborhood within the city and map the dependent variable using general functions. The underlying idea is that location abstractly captures information of land prices and by including functions that vary across space I allow the change in land prices to vary in a general way.[29]

There could be multiple ways to identify location in the two dimensional space. The method I use, which naturally stems from a monocentric city with boundaries that extend out from the city center, is to use distance from the city center and distance to the district boundaries to identify the location of each neighborhood.[30] Figure 7 illustrates the idea. Identification using distance from district boundaries requires assignment of sides. I denote the better school district along each boundary the positive side. This identification method is tied to each boundary. Hence, what I will be testing is whether I see a jump in the change in housing land prices across each boundary, especially for the Boundary 3 which borders the district receiving the good schools and has the larger discrepancy in average pass rate across borders.

In practice, I perform the following regression:

$$\Delta P_{i,(1973,1975)} = \tau_b \phi_{ib} D_{ib} + f(d_{ic}) + g_b(d_{ib}) + D_b * g'_b(d_{ib}) + Z_i + \phi_{ib} + \xi_i \,. \qquad (8)$$

$\Delta P_{i,(1973,1975)}$ is the change in log housing land price between 1973 and 1975. Each observation is matched to its nearest boundary and ϕ_{ib} represents the set of dummies which equal one if i's nearest boundary is b (=1,..,5) and zero otherwise. D_{ib} is an indicator equal to one if i is in the better school district along i's relevant boundary. d_{ib} is the distance from neighborhood i to its closest boundary b, and d_{ic} is the distance from i to the city center (center of the UCD). $f(d_{ic})$ is the polynomial that captures trends from the city center. $g_b(d_{ib})$ and $g'_b(d_{ib})$ are polynomials across each boundary. Note that the g functions differ for each boundary and on both sides of each boundary. I allow $f(d_{ic})$ to be a fifth order polynomial and $g(d_{ib})$'s to be linear or quadratic functions.[31] Z_i is the set of additional control variables: dummy variables indicating the location quality of the land (low and high, where medium is the omitted category), and the set of school district dummies interacted with the UCD dummy. I focus on the residential neighborhoods while flexibly controlling for the downtown area with these set of interacted dummies. The UCD overlaps with the central business district and depopulates throughout the 1970s as shown in Appendix Table 8. I am interested in whether τ_b is positive, especially for Boundary 3. I estimate the above framework on sub-

[29] The idea that location captures information of the dependent variable is not new. One of the earliest boundary discontinuity paper (Holmes 1998) examines how right-to-work laws affect business activity by comparing counties across state borders. In that paper location specific traits in manufacturing activity are captured through general functional forms that move along state boundaries. See Dell (2010) for a more recent application.

[30] Appendix Figure 3 visually illustrates how land price is related to location. For each district, each line represents a local linearized fit of the log of housing land prices in 1975 on the distance to the city center. There a strong spatial trend that maps the monocentric city model.

[31] Whether I use a 3rd, 4th, or 5th order polynomial for f does not change the empirical results. I opt for the more flexible form.

samples based on distance to the boundaries (1km to 4km), and fit different functional forms to obtain ranges of estimates in the empirical analysis.[32]

The identifying assumption is that no other factors that affect housing land price jump discretely across the boundaries between 1973 and 1975 and that there is no systematic relation between the residual and D_b once the spatial trends are accounted for in (8). Focusing on a narrow time period pre and post regime change helps control for other demand factors that could change relative to examining a longer time horizon. Another concern is the potential change in housing supply. Hence, I also test whether housing supply differentially shifts across boundaries by examining the number of households. Another relevant test is to see if there are any differential jumps across boundaries before the regime change. This is similar to testing the parallel trends assumption in difference in difference regressions. I also examine the change in housing land price between 1971 and 1973.

VI.B Empirical Results

I first graphically examine patterns across each boundary to see if there are any visually identifiable jumps as well as to choose the reasonable order of polynomial to fit across boundaries. Figure 8 plots the raw data for log housing land prices in levels for the years 1971, 1973, and 1975, and the change in log housing land price between 1973 and 1975 by distance to each boundary. I restrict the plot to residential neighborhoods outside the UCD. The right hand side of the boundary indicates the better school district and the solid lines are quadratic fits with the shaded region representing 95% confidence intervals. The solid circles are averages for the observations in each 1 km bin, e.g., 0.5 indicates observations between 0 and 1 km. There is an increasing trend in housing land prices over the years for all boundaries but the discrete jump in the change in housing land price is evident and significant for Boundary 3 only. Appendix Figure 4 plots the change in the log number of households between 1973 and 1975. Boundary 3 displays no jump. The figures for Boundary 3 are consistent with a price increase driven by a shift in demand. I next test whether the observed patterns hold and estimates are significant in the regression framework.

Table 7 reports the results for equation (8). Panel A reports the change in log housing land prices between 1973 and 1975. Column (1) present results for the 1km boundary sub-sample that compare levels across border. Coefficient estimates are positive and significant only for Boundary 3. Results in column (2) which uses 2.5 km boundary samples with linear trends, and column (3) which uses 4 km boundary samples with quadratic trends also indicate a positive and significant increase for Boundary 3 only. The estimates imply that the housing land price increase in District 3 was about 26 to 54 percentage points

[32] Focusing on sub-samples better fit polynomials around the boundaries without being subject to outliers farther from the boundary. The 4km sub-sample contains 90.2% of the observations.

higher than District 4 around Boundary 3.[33] Panel B examines the change in household numbers which serves as a proxy for the quantity of houses. The estimates for all boundaries in columns (1) through (3) are statistically indistinguishable from zero. The combination of results from Panel A and Panel B supports a demand driven change in housing prices.

As robustness checks, I examine the periods before the policy change. If the policy, not some differential trend across districts, generated the jump then we should see no significant increase in the change in prices before the policy change. Table 7 column (4) reports results using the quadratic trend specification of column (3). Between 1971 and 1973, no boundary exhibits a change in housing land price that is significantly different from zero, consistent with patterns illustrated in Figure 7. I also examine the 1975 to 1977 results and find increases along the better districts of Boundary 1 and 5. Panel B presents results on the change in the number of households and show no significant jump across all boundaries. In sum, the results in this section indicate that households sorted towards and increased housing land prices in District 3, the district that saw an exogenous increase in school district quality when the regime shifted to district assignment.

VII. Conclusion

This paper asks how a shift from a merit based system to a location based system affects the educational development of individuals of different ability and socio-economic background. The unusually drastic educational regime change that happened in South Korea during the 1970s provides the opportunity to test predictions based on a stylized model. I find that the shift from an exam to a district based allocation rule in secondary education increases the impact of socio-economic status, namely father's education level, on one's performance on the college entrance exam, controlling for middle school achievement. I also confirm that with the shift to district assignment higher income households sort to the better school districts, and as a result, raise prices and price out lower income households in the housing market. Empirical results also confirm that price increases are driven by household demand and not shifts in housing supply. In sum, district based assignment, relative to exam based tracking, segments schools by income, lowers achievement of high ability students from poor households.

I motivated in the beginning, that these results would be meaningful for developing countries in structuring their secondary education policies, because the period of this regime shift in Korea resembles conditions of many developing countries today. However, what is important for the results here to have implications to other settings is the institutional set up, particularly of the exam based tracking regime. The reason that intergenerational mobility was higher under the exam regime in Korea was because the

[33] To be more exact, an estimate of 0.5 where prices increase *x* % in District 4 would imply that the percentage point increase in prices in District 3 relative to District 4 is $(e^{0.5}-1)+e^{0.5}x$.

exam based allocation rule was as simple as in the model introduced in the paper: test scores were the only factor that determined access to quality education and households were not restricted by location. Exams have been used to gain access to higher social status in Korea for hundreds of years. A transparent exam based system unhindered by residential restriction, monetary contribution, or corruption would be critical for educational tracking to select talented students from poor households.

I introduced that the literature on the UK and the US find evidence consistent with residential sorting with the shift away from tracking. Furthermore, this paper finds that the reduction in intergenerational mobility in Korea was due to stark residential sorting. However, Meghir and Palme (2005) find no evidence of sorting in Sweden and the literature on Scandinavian countries finds that the movement away from tracking reduces inequality. Why would residential sorting occur in one context and not in another? One explanation for such difference may be due to the underlying variance in school quality and reputation. In Korea, there were prestigious high schools that were singled out from the rest of the high schools and households aspired and competed to send their kids to such schools. On the other hand, Finland's education policy aims to improve the achievement of all students without leaving struggling students behind.[34] The difference in the educational approach could provide an understanding of why the variance in educational quality could be larger in Korea versus the Scandinavian countries, and the different degree of household sorting when the education regimes shifted away from tracking.

Lastly, though this paper illustrates the segmentation of high schools by income, given that middle schools and primary schools are all part of the school district system, income segmentation would naturally arise over the whole spectrum of primary and secondary education, further solidifying the stratification of educational opportunities by income. Policy tends to focus on creating differential margins within the status quo institutional set up. As much as expanding school choice e.g., through vouchers or busing, within a district based system is an option, modified versions of a transparent merit based system could serve as alternatives to promote the educational development of high ability students from low socio-economic backgrounds.

[34] The OECD (2011) report provides detailed description of the education system of Finland. Though both Korea and Finland consistently ranks at the top of international standardized test scores, their educational approaches and performance in terms of reducing educational inequality are very different.

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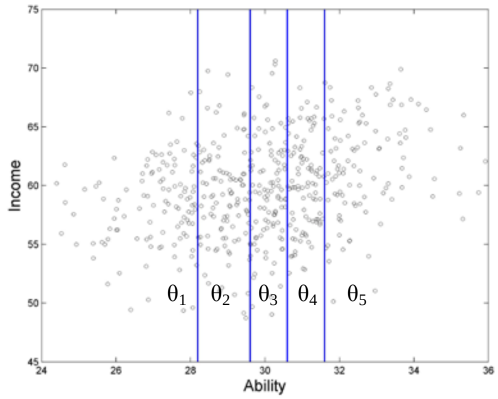
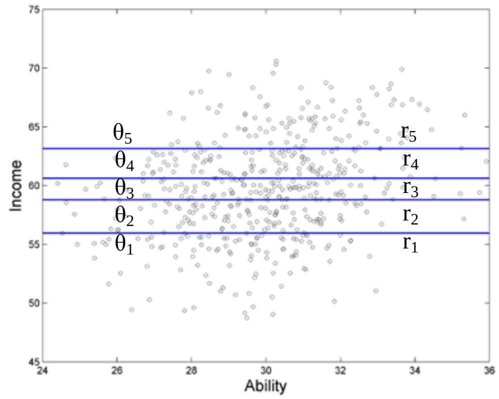
Figure 1. Equilibrium school allocation and achievement contour lines - Simulation results

I. Household to school allocation

A. Exam based selection



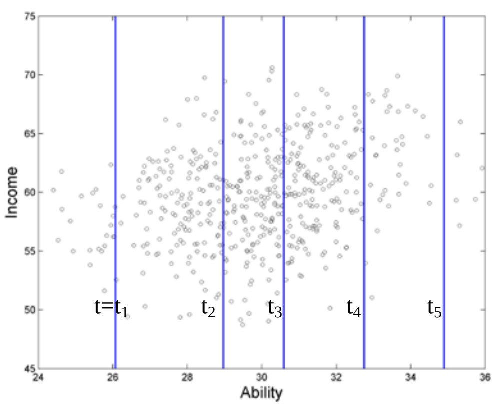
B. District assignment



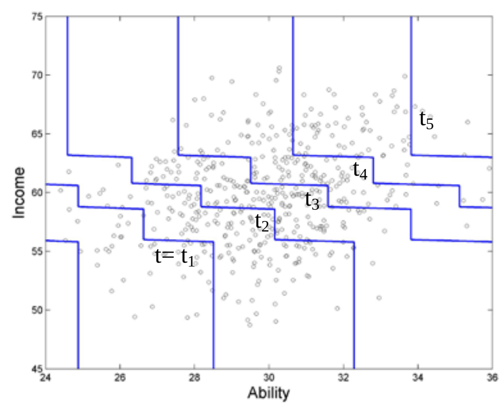
Notes: Each dot represents a household where the correlation between income and ability is 0.3. The solid line represents the stratification of households to schools. There is one school per neighborhood/district and each school is represented by school quality θ where $\theta_1 < \theta_2 < \theta_3 < \theta_4 < \theta_5$. Under exam based tracking all neighborhoods pay the same price for housing. Rent premium emerges under district assignment and $r_1 < r_2 < r_3 < r_4 < r_5$.

II. Achievement contour lines

C. Exam based selection

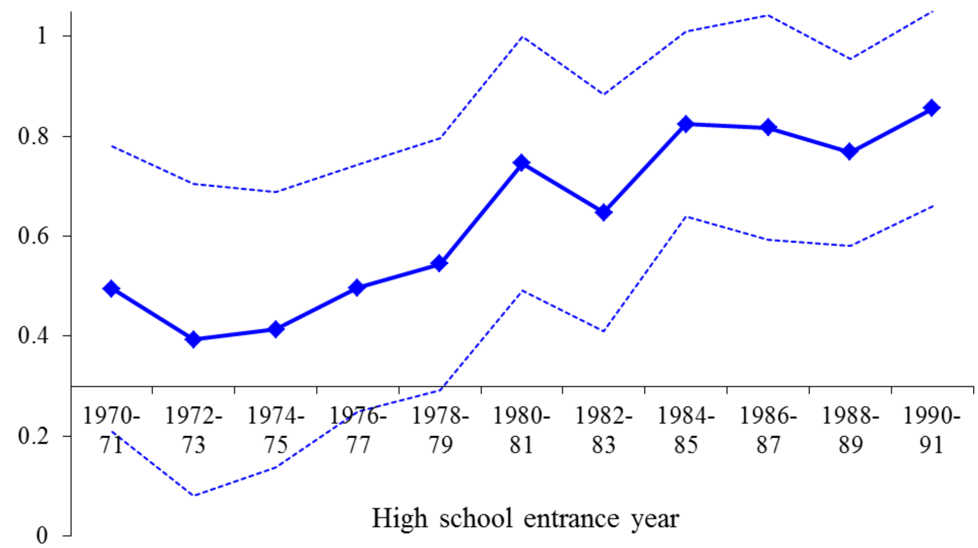


D. District assignment



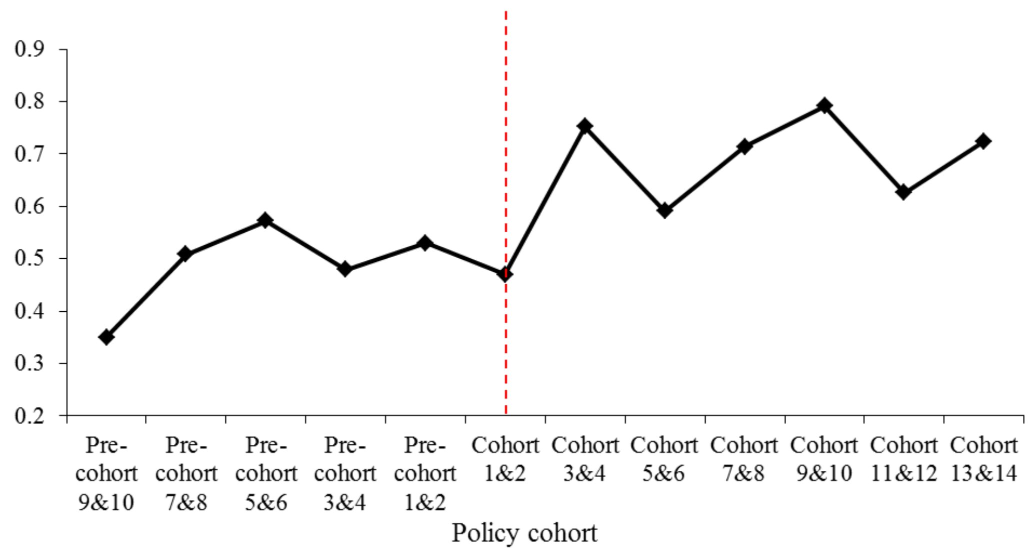
Notes: Each solid line represents the set of households that obtain the same achievement level on the college entrance exam. Achievement varies such that $t_1 < t_2 < t_3 < t_4 < t_5$. Under an exam equilibrium achievement increases with ability. Under a district equilibrium achievement increases with income and ability.

Figure 2. Evolution of intergenerational education mobility for all individuals – Relation between father's education and child's achievement



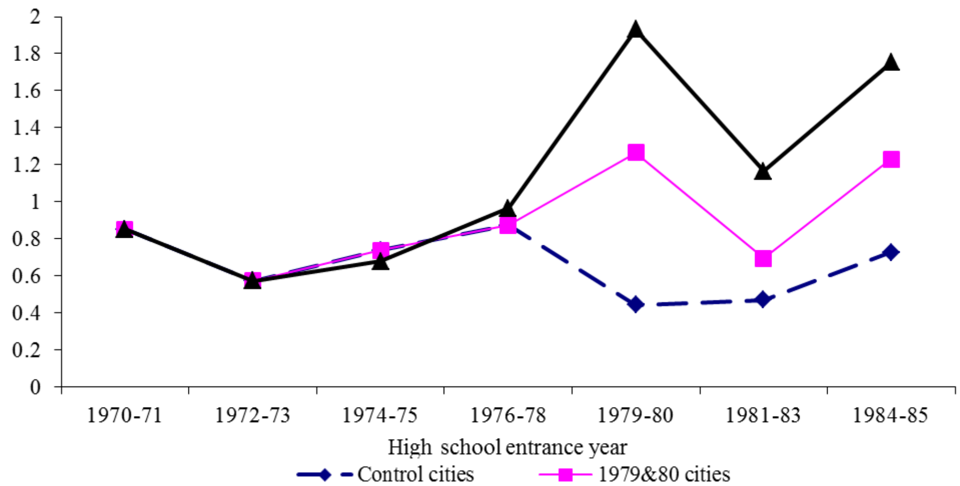
Notes: Above figure plots the coefficients β_K from the equation, $y_{ik} = \sum_k (fedu_{ik} \times D_{iK}) \times \beta_K + \mu_k + \varepsilon_{ik}$ where $fedu_{ik}$ is father's years of education for individual i who entered high school in year k, y_{ik} is achievement measured by the average entrance exam score based on one's college status. D_{iK} is a dummy variable equal to one if individual i entered high school in the two year period K as specified in the x axis, μ_k are birth year fixed effects. I use a nationally representative sample and pool all individuals who went to high school in the Korea Labor and Income Panel Survey (KLIPS) to generate the coefficients. The shift in high school admission policy from exam to district started in 1974 beginning with Seoul and then gradually rolled out to other cities until 1980. 95% confidence interval bands are in dashed lines.

Figure 3. Event study by cohorts for cities that shift to district assignment – Relation between father's education and child's achievement



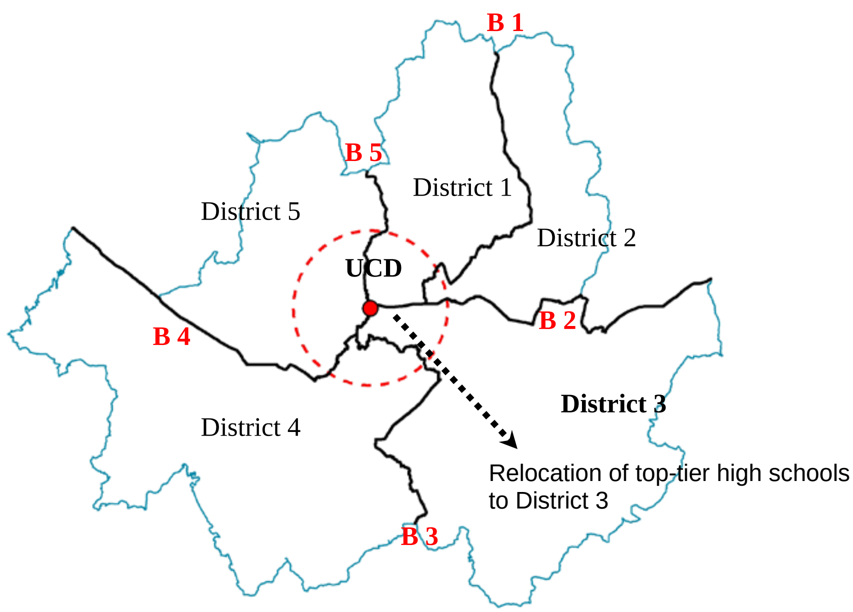
Notes: Black line plots the coefficients β_K from the equation, $y_{ik} = \sum_k (fedu_{ik} \times C_{iK}) \times \beta_K + \mu_k + \lambda_j + \varepsilon_{ik}$ where C_{iK} is a dummy variable equal to one if individual i was in the K^{th} policy cohort, where K groups two cohorts as in the x axis. μ_k is the set of birth year fixed effects and λ_j is the set of city fixed effects. $fedu_{ik}$ and y_{ik} is same as in Figure 1.

Figure 4. Evolution of $\hat{\beta}/\hat{\gamma}$ by city groups



Notes: Above figure plots the ratio of coefficient based on estimates from the equation $y_{ijkm} = c + \sum_{l=1}^{7}(x_{ijkm} \cdot d_l)\beta_{1l} + \sum_{l=1}^{7}(a_{ijkm} \cdot d_l)\gamma_{1l}$ $+ \sum_{l=5}^{7}(x_{ijkm} \cdot d_l \cdot D_{jm})\beta_{2l} + \sum_{l=5}^{7}(a_{ijkm} \cdot d_l \cdot D_{jm})\gamma_{2l} + \sum_{l=3}^{7}(x_{ijkm} \cdot d_l \cdot C_{jm})\beta_{3l} + \sum_{l=3}^{7}(a_{ijkm} \cdot d_l \cdot C_{jm})\gamma_{3l} + \mu_j + \eta_k + \lambda_m + \varepsilon_{ijkm}$ where d_l is a dummy that indicates whether individual i enters high school in the years grouped as in the x axis, D_{jm} is the dummy for all treated cities and C_{jm} is the dummy for the cities that shift in 1974 or 1975.

Figure 5. School districts and boundaries created in Seoul in 1974



	UCD	District 1	District 2	District 3	District 4	District 5
Number of high schools	46	8	7	7	10	11
Expected pass rate	0.75	0.36	0.78	0.6 (0.41)	0.47	0.28

Notes: The Unified Central District was formed as a 4km radius circle with the center at Gwanghwamun, indicated by the red dot. The number of general high schools and the average pass rate in the college eligibility exam for each district are listed in the table. District boundaries are denoted with the prefix B. Many top-tier schools relocated from the UCD to District 3 under the district regime. The relocation of the ranking one high school Gyeonggi high school was announced in 1972 with the new campus opening in 1976. Many other prestigious schools moved to District 3 in the following years. Appendix Table 7 describes the relocation of high schools. The average pass rate in District 3 takes the relocation of Gyeonggi high school into account. The number in paranthesis, 0.41, indicates what the pass rate at District 3 would have been had Gyeonggi high school not relocate.

Figure 6. The change in percent college educated between 1975 and 1980 by district and age group

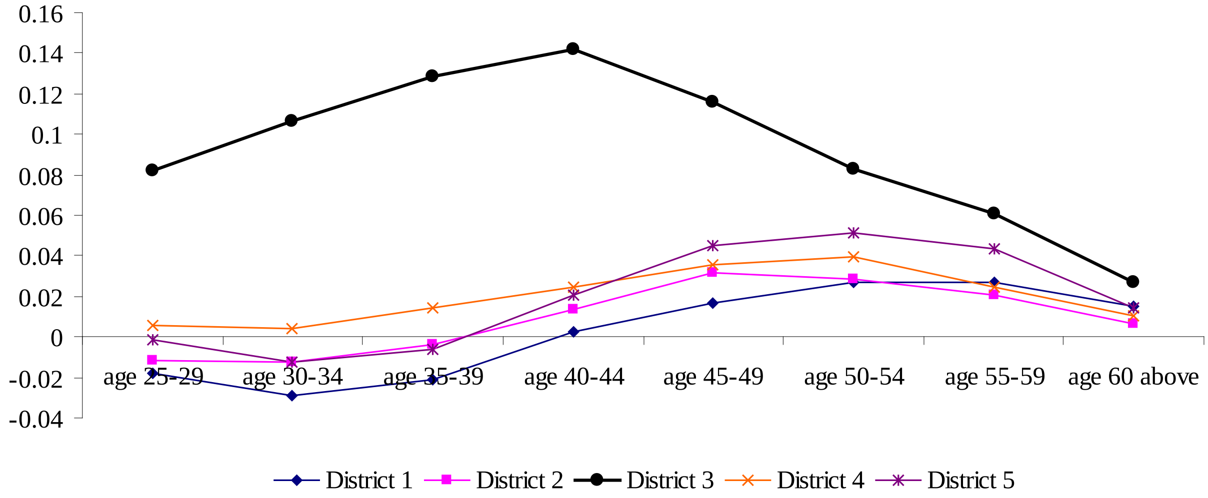
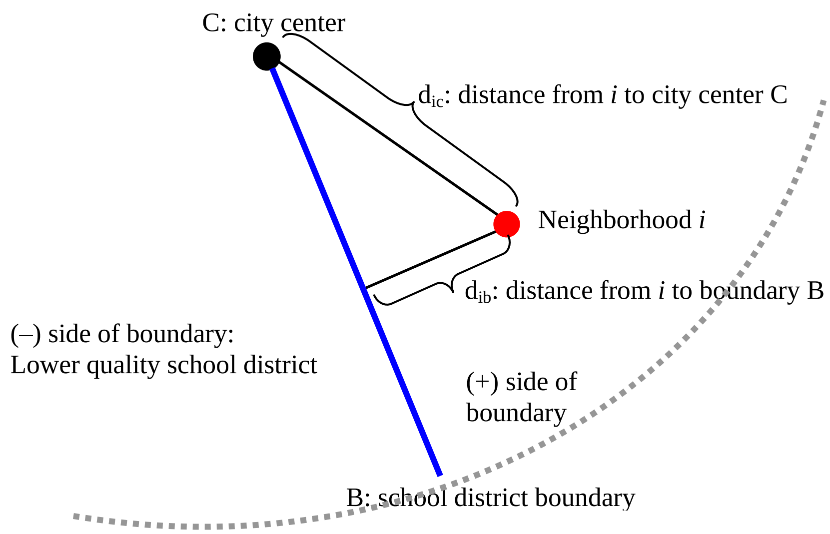
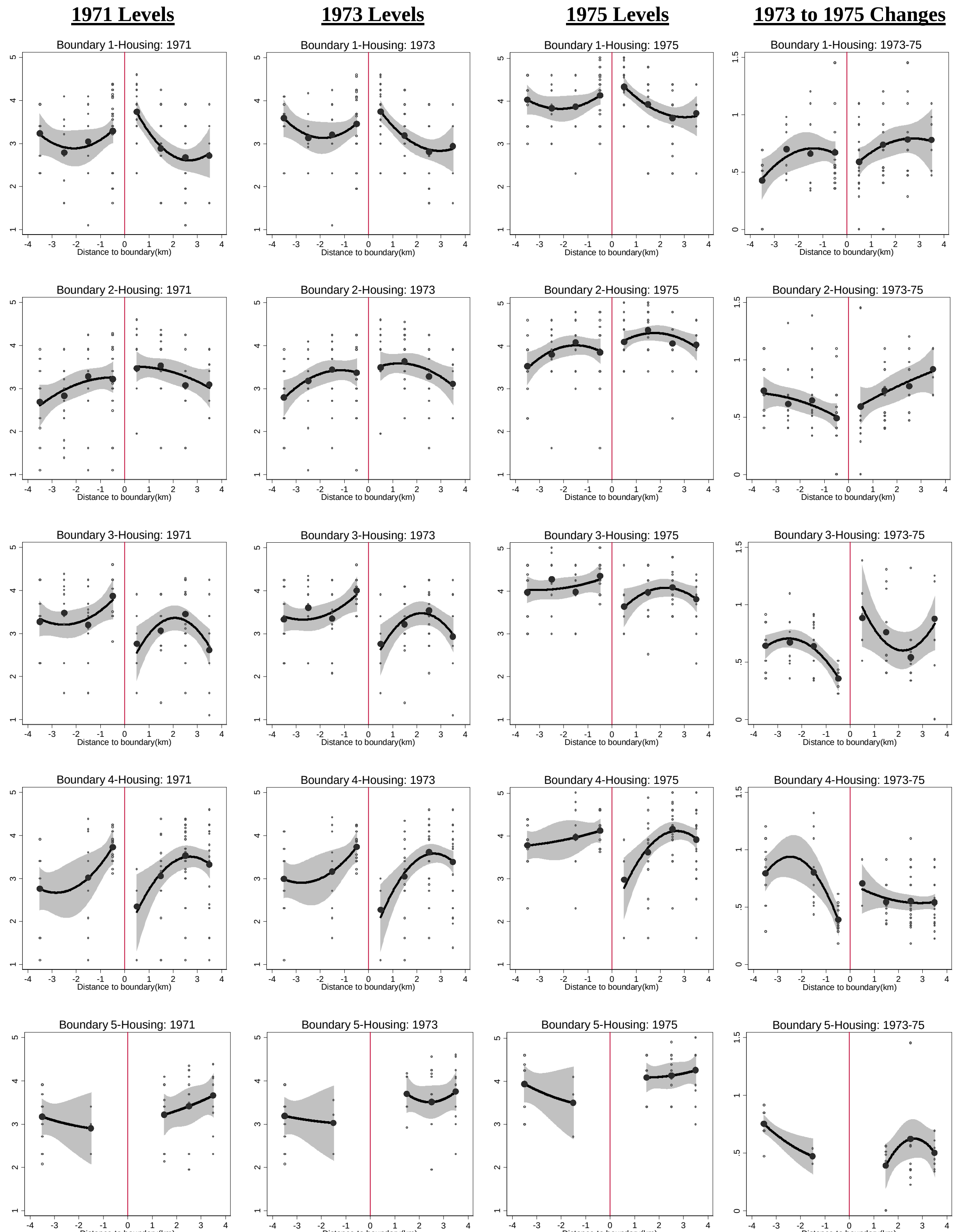


Figure 7. Identifying neighborhood location based on distance from city center and distance from a school district boundary



Notes: Distance from each neighborhood to its relevant boundary is assigned a positive or negative number. For each boundary the plus side is defined to be the side with the better school quality.

Figure 8. Trends in housing land price: 1971, 1973, 1975 levels in log prices, 1973-75 change in log prices



Notes: The open circles represent neighborhoods and the solid circles represent the mean value for neighborhoods within each integer bands. Solid lines are quadratic fits. The shaded areas represent 95% confidence interval bands.

Table 1. Summary Statistics

Variable	Mean	Std. Dev.
Panel A: Individual level data (2228 observations)		
College score	46.62	8.74
Father's year of education	7.53	4.19
Mother's year of education	4.87	3.44
Middle school grade	50.32	8.89
Received tutoring during high school	0.10	0.30
General high school student	0.67	0.47
Male	0.55	0.50
Mother was primary earner	0.07	0.25
Age when entering high school	16.00	0.57
Went to top 20 college	0.06	0.24
Under district regime	0.43	0.49
High school in group 1 (1974) cities	0.28	0.45
High school in group 2 (1975) cities	0.13	0.34
High school in group 3 (1979) cities	0.08	0.27
High school in group 4 (1980) cities	0.09	0.28
Panel B: Neighborhood level data (662 observations)		
Change in log housing land price (1971-1973)	0.105	0.234
Change in log housing land price (1973-1975)	0.586	0.340
Change in log housing land price (1975-1977)	0.287	0.283
Change in log number of households (1971-1973)	0.051	0.215
Change in log number of households (1973-1975)	0.138	0.223
Change in log number of households (1975-1977)	0.035	0.206
District average college eligibility pass rate	0.487	0.170
Distance to nearest boundary (m)	1860	1756
Within 1km to nearest boundary	0.394	0.489
Within 2.5km to nearest boundary	0.773	0.419
Within 4km to nearest boundary	0.899	0.302
Within Unified Central District	0.276	0.448

Notes: Data is based on KLIPS (Round 1-11) for Panel A and Korea Land Appraisal Annals (1971-1977) and Seoul Statistics Annal for Panel B.

Table 2. Main results on intergenerational mobility

Dependent variable: College score	Base result		High school fixed effects			
			All schools		Schools with at least 5 obs.	
	(1)	(2)	(3)	(4)	(5)	(6)
District assignment (D)		-6.410***		-5.355		-4.113
		(2.062)		(4.927)		(4.650)
Father's years of education	0.258***	0.230***	0.247*	0.224*	0.269**	0.244*
	(0.080)	(0.079)	(0.128)	(0.127)	(0.126)	(0.127)
Father's education*D	0.209***	0.253***	0.073	0.113	0.048	0.083
	(0.080)	(0.081)	(0.197)	(0.200)	(0.183)	(0.187)
Middle school score	0.382***	0.332***	0.358***	0.316***	0.378***	0.343***
	(0.031)	(0.033)	(0.049)	(0.058)	(0.045)	(0.056)
Middle school score*D	-0.025	0.081*	-0.013	0.075	-0.007	0.060
	(0.016)	(0.041)	(0.041)	(0.094)	(0.038)	(0.087)
Male	2.953***	2.927***	4.021***	3.998***	5.026***	4.973***
	(0.397)	(0.390)	(1.246)	(1.230)	(1.446)	(1.444)
Primary earner is mother	-1.801***	-1.784***	-1.039	-1.001	-0.207	-0.171
	(0.455)	(0.445)	(1.430)	(1.449)	(1.367)	(1.379)
City, Age, and HS year FEs	Y	Y	Y	Y	Y	Y
High school FEs			Y	Y	Y	Y
Observations	2,228	2,228	2,160	2,160	1,220	1,220
R-squared	0.34	0.34	0.676	0.677	0.573	0.574
Test-statistic: $t = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}} - \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}}$	0.633***	0.479**	0.236	0.154	0.143	0.101
	(0.244)	(0.233)	(0.639)	(0.627)	(0.552)	(0.548)
p-value of one sided hypothesis test $H_0: \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}} = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}}$, $H_1: \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}} < \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}}$	0.0052	0.0209	0.356	0.404	0.398	0.427

Notes: Standard errors are clustered by cities in column (1) and (2), and clustered by high schools in columns (3) through (6). ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 3. Results by city size

Dependent variable: College score	Seoul (1)	Seoul (2)	Other large cities (3)	Other large cities (4)	Smaller cities (5)	Smaller cities (6)
District assignment (D)		-10.719***		-7.896**		-5.793
		(2.461)		(3.651)		(3.505)
Father's years of education	0.276***	0.239***	0.156***	0.133**	0.177***	0.165***
	(0.101)	(0.088)	(0.059)	(0.060)	(0.056)	(0.055)
Father's education*D	0.202**	0.286***	0.334***	0.385***	0.191	0.218
	(0.083)	(0.088)	(0.111)	(0.109)	(0.138)	(0.137)
Middle school score	0.381***	0.331***	0.360***	0.320***	0.325***	0.305***
	(0.049)	(0.040)	(0.038)	(0.037)	(0.030)	(0.035)
Middle school score*D	-0.071***	0.090**	-0.014	0.112	-0.015	0.085
	(0.019)	(0.041)	(0.024)	(0.074)	(0.026)	(0.066)
Male	3.303***	3.213***	2.562***	2.514***	2.573***	2.571***
	(0.448)	(0.435)	(0.301)	(0.296)	(0.385)	(0.379)
Primary earner: Mother	-1.681***	-1.556***	-1.378**	-1.399**	-1.687**	-1.753**
	(0.599)	(0.574)	(0.542)	(0.548)	(0.749)	(0.748)
City, Age, and HS year dummies	Y	Y	Y	Y	Y	Y
Observations	1,390	1,390	1,393	1,393	1,305	1,305
R-squared	0.355	0.361	0.384	0.389	0.347	0.349
Test-statistic: $t = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}} - \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}}$	0.820**	0.525**	0.986***	0.782**	0.643	0.442
	(0.368)	(0.257)	(0.399)	(0.336)	(0.509)	(0.364)
p-value of one sided hypothesis test $H_0 : \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}} = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}}$, $H_1 : \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}} < \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}}$	0.014	0.021	0.007	0.011	0.105	0.113

Notes: Column (3) and (4) include cities that shifted in 1974& 75 which are the five largest cities other than Seoul. Columns (5) and (6) include the smaller cities that shifted in 1978& 80. Standard errors clustered by cities are in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 4. Additionally controlling for mother's education and occupation groups

Dependent variable:	A. Include Mother's Education			B. Control for Occupation Groups		
College score	(1)	(2)	(3)	(4)	(5)	(6)
District assignment (D)	-7.259***	-5.939***	-6.274***	-8.232***	-7.959***	-8.091***
	(2.125)	(2.060)	(2.003)	(2.108)	(2.318)	(2.315)
Father's years of education	0.231***	0.140**		0.067	-0.012	
	(0.077)	(0.067)		(0.090)	(0.071)	
Father's education*D	0.279***	0.163*		0.260***	0.175*	
	(0.081)	(0.083)		(0.094)	(0.094)	
Mother's years of education		0.279***	0.342***		0.229***	0.212***
		(0.076)	(0.087)		(0.075)	(0.076)
Mother's education*D		0.035	0.136		-0.008	0.083
		(0.152)	(0.154)		(0.128)	(0.138)
Middle school score	0.340***	0.325***	0.323***	0.311***	0.292***	0.286***
	(0.033)	(0.032)	(0.032)	(0.032)	(0.031)	(0.030)
Middle school score*D	0.094**	0.084*	0.104**	0.118***	0.124**	0.143***
	(0.043)	(0.043)	(0.042)	(0.043)	(0.048)	(0.044)
Male	3.038***	2.905***	2.894***	3.362***	3.111***	3.168***
	(0.386)	(0.370)	(0.373)	(0.417)	(0.378)	(0.368)
City, Age, and HS year dummies	Y	Y	Y	Y	Y	Y
Occupation group dummies				Y	Y	Y
Observations	2,078	1,748	1,799	1,842	1,563	1,608
R-squared	0.352	0.356	0.349	0.386	0.381	0.381
Test-statistic:						
$t = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}} - \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}}$	0.497**	0.311		0.546**	0.433*	
	(0.240)	(0.255)		(0.301)	(0.304)	
p-value of one sided test	0.020	0.113		0.036	0.078	

Notes: Sample is based on individuals where the father was the primary earner of the household. Standard errors clustered by cities. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 5. Robustness tests

	Quantile regressions		Linear	High	Placebo policy years			Post 1972	
	0.7 quantile	0.9 quantile	Probability	school type	1965	1969	1985	sample	
Dependent variable:	College score	College score	Top 20 college	College score	College score	College score	College score	College score	Ln(income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District assignment (D)	-12.242***	-4.945	-0.180***	-4.543**	-1.054	0.037	-4.864*	-5.462**	-0.237
	(2.975)	(4.463)	(0.061)	(2.060)	(3.503)	(2.855)	(2.736)	(2.425)	(0.148)
Father's years of education	0.226**	0.310***	0.001	0.173**	0.405***	0.436***	0.413***	0.235***	0.002
	(0.107)	(0.081)	(0.002)	(0.076)	(0.105)	(0.136)	(0.073)	(0.059)	(0.006)
Father's education*D	0.425***	0.283**	0.006***	0.196***	0.142	0.089	0.144*	0.255***	0.014*
	(0.122)	(0.139)	(0.001)	(0.069)	(0.151)	(0.233)	(0.080)	(0.070)	(0.007)
Middle school score	0.356***	0.507***	0.005***	0.306***	0.327***	0.371***	0.369***	0.333***	0.013***
	(0.053)	(0.050)	(0.001)	(0.032)	(0.048)	(0.065)	(0.032)	(0.037)	(0.002)
Middle school score*D	0.197***	0.066	0.003**	0.055	0.076	-0.022	0.064	0.084*	0.003
	(0.064)	(0.089)	(0.001)	(0.041)	(0.056)	(0.056)	(0.051)	(0.045)	(0.003)
Male	3.396***	3.774***	0.027**	3.002***	3.299***	2.963***	2.542***	2.890***	0.838***
	(0.549)	(0.754)	(0.011)	(0.374)	(0.834)	(0.698)	(0.248)	(0.379)	(0.039)
Primary earner: Mother	-1.564**	-1.697	-0.036***	-1.622***	-1.675	-1.489	-1.268**	-1.786***	-0.043
	(0.730)	(1.037)	(0.011)	(0.430)	(1.075)	(1.688)	(0.521)	(0.489)	(0.073)
General high school(GHS)				2.516**					
				(1.264)					
GHS*HS year dummies				Y					
City, Age, and HS year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,228	2,228	2,259	2,228	817	682	2,215	1,955	1,967
R-squared			0.131	0.389	0.395	0.412	0.351	0.347	0.366
Test-statistic:									
$t = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}} - \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}}$	0.543*	0.424*	0.663***	0.457**	0.118	0.328	0.168	0.469**	0.842**
	(0.374)	(0.291)	(0.256)	(0.234)	(0.434)	(0.720)	(0.268)	(0.221)	(0.507)
p-value of one sided test	0.074	0.073	0.005	0.026	0.393	0.325	0.266	0.018	0.049

Notes: Quantile regression results are based on Blaise Melly's Stata command quantreg which allows for clustering. Age and age squared are included in column (10). Standard errors are clustered by cities. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 6. District level change in log housing land prices between 1973 and 1975

Dependent variable: Change in log housing land prices	Difference in difference estimates: Relative to District 1 (1)	Relative to all districts other than District 3 (2)
District 2	0.0869	
	(0.0594)	
District 3	0.150**	0.130**
	(0.0693)	(0.0617)
District 4	-0.0192	
	(0.0431)	
District 5	0.0301	
	(0.0501)	
Location quality dummies	Y	Y
Observations	656	656
R squared	0.058	0.047

Notes: Standard errors are clustered at the neighborhood level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 7. Neighborhood level boundary sample estimates

Changes between	1973 and 1975	1973 and 1975	1973 and 1975	1971 and 1973	1975 and 1977
Fitted trend across school district boundaries	Levels	Linear trend	Quadratic trend	Quadratic trend	Quadratic trend
Distance from boundary	1km	2.5km	4km	4km	4km
	(1)	(2)	(3)	(4)	(5)
A. Change in log housing land price					
Boundary 1* Better district	-0.168	0.0313	-0.0439	-0.155	0.233*
	(0.134)	(0.0812)	(0.110)	(0.122)	(0.138)
Boundary 2* Better district	-0.0300	0.0361	-0.0748	-0.202	-0.0629
	(0.185)	(0.146)	(0.192)	(0.180)	(0.195)
Boundary 3* Better district	0.262**	0.505***	0.535**	-0.209	-0.198
	(0.123)	(0.166)	(0.260)	(0.167)	(0.187)
Boundary 4* Better district	-0.101	0.0755	0.100	0.0566	0.0438
	(0.137)	(0.101)	(0.115)	(0.0979)	(0.183)
Boundary 5* Better district	-0.0659	-0.0207	-0.309	0.285	0.687***
	(0.217)	(0.151)	(0.220)	(0.248)	(0.251)
Location quality dummies	Y	Y	Y	Y	Y
Observations	260	506	592	588	594
R-squared	0.501	0.441	0.487	0.276	0.342
B. Change in log number of households					
Boundary 1* Better district	0.0245	0.0544	0.0887	-0.0881	0.0289
	(0.175)	(0.0743)	(0.106)	(0.0724)	(0.0668)
Boundary 2* Better district	-0.0826	0.0363	0.109	-0.115	0.00922
	(0.224)	(0.165)	(0.217)	(0.174)	(0.167)
Boundary 3* Better district	-0.126	-0.320	-0.128	0.306	-0.295
	(0.175)	(0.199)	(0.342)	(0.200)	(0.335)
Boundary 4* Better district	-0.0808	0.0294	0.0568	0.0680	-0.0254
	(0.212)	(0.105)	(0.117)	(0.148)	(0.157)
Boundary 5* Better district	-0.0564	0.00498	0.240	-0.0212	-0.201
	(0.194)	(0.146)	(0.220)	(0.0786)	(0.166)
Observations	90	177	206	203	206
R-squared	0.251	0.326	0.505	0.288	0.253
C. Controls in Panels A and B					
5^{th} order polynomial in distance from city center	Y	Y	Y	Y	Y
Boundary dummies	Y	Y	Y	Y	Y
District*UCD dummies	Y	Y	Y	Y	Y

Notes: Functional forms are allowed to vary on each side of the boundary and for each boundary. Standard errors are clustered at the neighborhood level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

APPENDIX A: PROOFS OF PROPOSITIONS AND LEMMAS

PROOF OF PROPOSITION 1.

Suppose not so that there is a household with student *i* whose achievement on the high school entrance exam t^m_i is higher than the cutoff for district J , t^m_J , but is allocated to the lower quality school district J-1. By the continuity of achievement *t* in *a,* there exists a household with student *k* whose achievement is such that $t^m_J < t^m_k < t^m_i$ that lives in J. Since, school J has chosen student *i* , the lower achieving student, instead of student *k* it is not optimizing, contradicting the definition of an exam equilibrium.

PROOF OF LEMMA 1:

Consider a household endowed with (*y,a*) that lives in district J, pays rent r_j, and obtains utility *V*. If this household could live in district J+1 but still pay the same rent, r_j, then it would obtain *V'* higher then *V*. On the other hand, if this household lived in J+1 but was paying a very high rent, r_h, so that consumption reduces drastically, then its utility *V"* would be lower than the initial utility level *V*. Continuity of utility in housing price implies that for household type (*y,a*) living in district J+1 there is an *r* between r_j and r_h so that it obtains utility *V* and becomes indifferent between J and J+1.

PROOF OF PROPOSITION 2.

Suppose not, so that there is a household *i* living in district J while paying housing price r_i that is lower than the cutoff for district J, r_J. By continuity of household utility in *r*, this implies that there is another household *k* with a willingness to pay $r_k > r_i$ in district J-1, which contradicts a district equilibrium.

PROOF OF LEMMA 2.

The school quality each household is matched with is determined by the household's ranking in the decision rule, where higher ranking in the decision rule maps to better school quality. Under the district regime the decision rule is willingness to pay for school quality. The ranking in willingness to pay strictly increases with household income and weakly increases in student ability because of the assumptions of single-crossing in income and weak single-crossing in ability. Denote this ranking $R(y,a)_d$. Under the exam regime the decision rule is one's performance on the high school entrance exam which is determined only by ability, since middle school quality is assumed to be equal. Hence, ranking is an increasing function of only ability, denoted $R(a)_e$. Thus, by construction $\text{Corr}(y,R(y,a)_d) > \text{Corr}(y,R(a)_e)$. Since school quality θ is a one to one function with $R(\cdot)$, the ranking under each regime, it follows that $\text{Corr}(y, \theta)_d > \text{Corr}(y, \theta)_e$. Following the same argument it follows that that $\text{Corr}(a, \theta)_d < \text{Corr}(a, \theta)_e$.

PROOF OF PROPOSITION 3.

First note that Lemma 2 implies that $E(y\theta)_d > E(y\theta)_e$ and $E(a\theta)_d < E(a\theta)_e$ because E(*y*), E(θ), Var(*y*), and Var(θ) are constant by assumption. Simply applying this to the sample equivalent implies

$$\beta_d - \beta_e = \kappa[\sum a^2 (\sum (y\theta)_d - \sum (y\theta)_e) + \sum ya(\sum (a\theta)_e - \sum (a\theta)_d)] > 0 \text{ and}$$

$$\gamma_d - \gamma_e = \kappa[\sum y^2 (\sum (a\theta)_d - \sum (a\theta)_e) + \sum ya(\sum (y\theta)_e - \sum (y\theta)_d)] < 0 .$$

PROOF OF PROPOSITION 4.

First note that

$$E(y\theta)$$
$$= \iint y\theta\, dF(y)dG(\theta) = \iint (\theta_1 y \cdot 1_{[y_0,y_1]} + ... + \theta_N y \cdot 1_{[y_{N-1},y_N]})dF(y)dG(\theta)$$
$$= \int [\theta_1 \int_{y_0}^{y_1} ydF(y) + ... + \theta_N \int_{y_{N-1}}^{y_N} ydF(y)]dG(\theta) = \sum \int_{y_{i-1}}^{y_i} ydF(y) \cdot \theta_i \cdot \frac{n_i}{n}$$

The second equality is the feature of the district equilibrium, where school quality and income is perfectly sorted. The fourth equality comes from the fact that θ is a discrete distribution. N is the number of districts and n is the number the total population, n_i is the population in district i. Given the above the proof is as follows.

Suppose not. So that $E(y\theta)$ is lower in city A. Denote the distribution in city A as F_A and city B as F_B. Since F_A FOSD F_B, $E(h(\theta))$ is larger in A than B for any non decreasing function h in θ. Since the district equilibrium matches higher θ with higher y, $\int_{y_{i-1}}^{y_i} ydF(y)$ is non-decreasing in θ. Hence, $E(y\theta)$ must be larger in city A which is a contradiction.

PROOF OF THE STATEMENT ON THE RATIO INEQUALITY IN PAGE 18.

As discussed in the main text, the reduced form estimation of standardized test scores on income and ability can be expressed as

$$s_{it} = \beta\phi_t y_{it} + \gamma\phi_t a_{it} + \varepsilon_{it}$$

where $\varepsilon_{it} = \phi_t(\theta_{is} + u_{it})$, θ_{is} is school quality, and u_{it} is random noise. t indexes regime: e(exam) or d(district). Hence, the estimated coefficients scale β and γ by the set of regime specific factors ϕ_t. The estimated coefficients, abstracting away from the covariances with the random noise u, is

$$\begin{bmatrix} \hat{\beta}\phi_t \\ \hat{\gamma}\phi_t \end{bmatrix} = \begin{bmatrix} \beta\phi_t \\ \gamma\phi_t \end{bmatrix} + \begin{bmatrix} \sum y^2 & \sum ya \\ \sum ya & \sum a^2 \end{bmatrix}^{-1} \begin{bmatrix} \sum ya \\ \sum a\theta \end{bmatrix} \phi_t = \begin{bmatrix} \beta\phi_t + \kappa\phi_t(\sum a^2 \sum y\theta - \sum ya \sum a\theta) \\ \gamma\phi_t + \kappa\phi_t(\sum y^2 \sum a\theta - \sum ya \sum y\theta) \end{bmatrix}$$

where $\kappa = \dfrac{1}{\sum y^2 \sum a^2 - (\sum ya)^2}$.

Denote β_d and γ_d the coefficient estimates under the district regime, i.e., $\hat{\beta}\phi_d$ and $\hat{\gamma}\phi_d$, and β_e and γ_e the coefficient estimates under the exam regime, i.e., $\hat{\beta}\phi_e$ and $\hat{\gamma}\phi_e$. Then,

$$\frac{\beta_d}{\gamma_d} - \frac{\beta_e}{\gamma_e} = \frac{\hat{\beta}\phi_d}{\hat{\gamma}\phi_d} - \frac{\hat{\beta}\phi_e}{\hat{\gamma}\phi_e}$$
$$= \frac{[\sum y^2 \sum a^2 - (\sum ya)^2][\sum(y\theta)_d \sum(a\theta)_e - \sum(y\theta)_e \sum(a\theta)_d]}{\gamma_d \gamma_e}$$
$$+ \frac{(\beta+\gamma)\kappa[(\sum(a\theta)_e - \sum(a\theta)_d) + (\sum(y\theta)_d - \sum(y\theta)_e)]}{\gamma_d\gamma_e} > 0$$

The second equality is based on simply reordering terms using the estimated coefficients in the above matrix. The last inequality holds because (1) $\gamma_d\gamma_e > 0$ because estimates of the γ's are positive, i.e., ability positively increases achievement under both regimes, (2) $\sum y^2 \sum a^2 - (\sum ya)^2 > 0$ because of the Cauchy-Schwartz inequality, and (3) $\sum(y\theta)_d > \sum(y\theta)_e$ and $\sum(a\theta)_d < \sum(a\theta)_e$ follows from Lemma 2.

APPENDIX B. MODEL WITH TUTORING CHOICE

I illustrate here a model that allows an additional choice variable, tutoring, where tutoring x directly impacts test scores and the price of tutoring is p. The set up is

$$U = (y + \delta t(x,a,\theta) - r - px) \cdot t(x,a,\theta)$$

$$t(x,a,\theta) = (x+k)^{\alpha} a^{\beta} \theta^{\gamma} \text{ , } 0<\delta, 0<\alpha<1, 0<\beta<1, 0<\gamma<1$$

which satisfies both single crossing in income and ability. Note that I allow intergenerational contracting in the model, in the sense that households can borrow against child's achievement. This illustrates a general feature often observed in developing countries where grown children support the old parents. The model is not explicitly solvable, so I graphically illustrate the equilibrium properties by simulation. I draw 500 households from a joint normal distribution with a correlation of 0.3 in the (y, a) space and simulate equilibrium where there are five schools each comprising a neighborhood or school district.

Households solve:

$$\max_{x} (y + \delta(x+k)^{\alpha} a^{\beta} \theta^{\gamma} - r - px)(x+k)^{\alpha} a^{\beta} \theta^{\gamma}, \quad s.t.\ x \geq 0.$$

All households have the same base level of home input, k, and can choose the corner solution of no tutoring, x=0. The Kuhn-Tucker conditions give the general condition when households will decide to provide tutoring:

$$x^* > 0 \text{ if } y > pk - 2\alpha\delta k^{\alpha} a^{\beta} \theta^{\gamma} + r \text{ .}$$

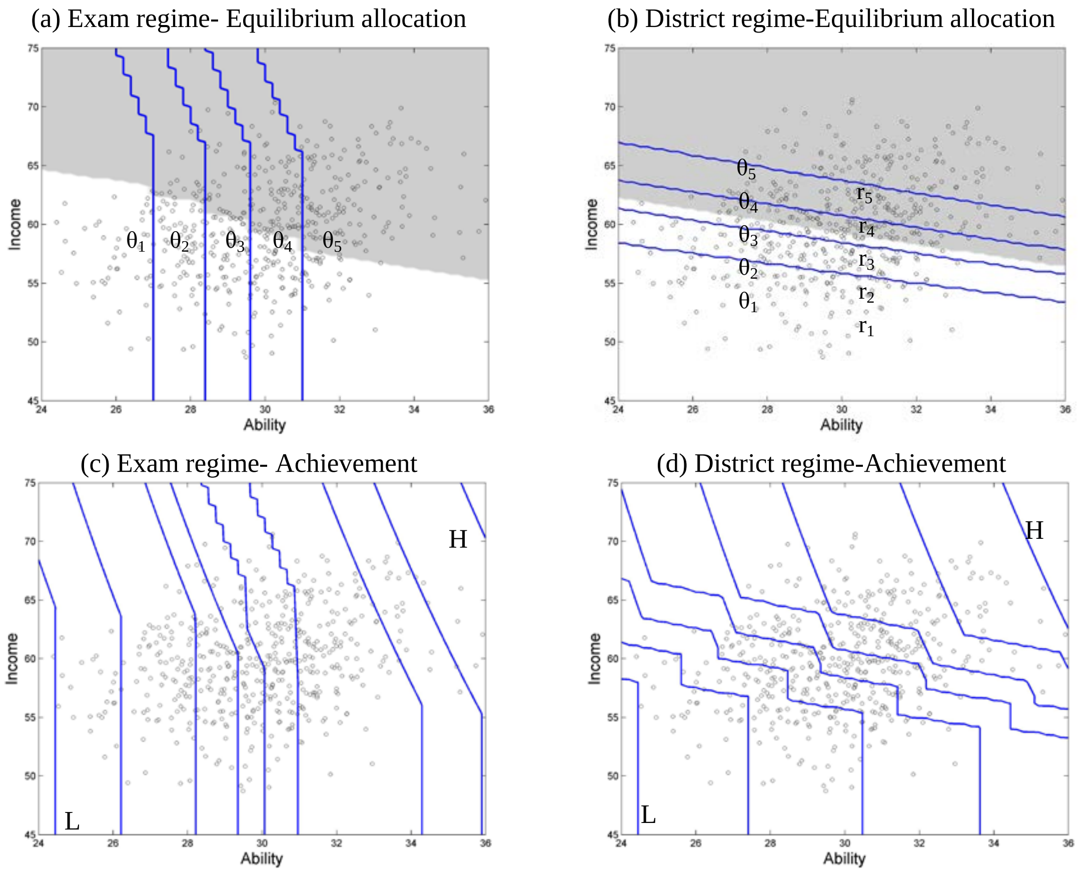
This implies that households with higher income y, ability a, or school quality θ will more likely choose to tutor. The underlying reason is the intergenerational contracting that makes consumption, both of the numeraire good c and tutoring x, increase with achievement. Given that income, ability, and school quality increase achievement, tutoring will also increase correspondingly. At an interior solution the first and second order properties along with the implicit function theorem, indicates that the amount of tutoring will increase monotonically with income, ability, and school quality in the above parametric model.

To solve the model, I set $\alpha = 0.3, \beta = 0.7, \gamma = 0.5, \delta_m = 0.1, \delta = 0.13, k = 10.5, p = 2$. School quality θ increases by 10% for each better school with the lowest starting at 10. Figures (a) and (b) depict how households are matched to school quality and their tutoring decisions under each regime. There is one school per neighborhood/district and each school is represented by school quality θ where $\theta_1 < \theta_2 < \theta_3 < \theta_4 < \theta_5$. Under tracking all neighborhoods pay the same price for housing. Rent premium emerges under the district regime and $r_1<r_2<r_3<r_4<r_5$. The solid lines in Figures (a) and (b) represent the stratification of households to schools. The shaded region indicates households that choose tutoring amount greater than zero. Given the above allocation of school quality and tutoring choice achievement will look as Figures (c) and (d).

Each solid line in Figures (c) and (d) represents an iso-achievement line, i.e., children of households in the same line will obtain the same achievement level. Achievement will be higher towards the upper right direction. Figure (c) depicts the achievement contour lines given the stratification and tutoring choice in Figure (a). The lines are steep in ability indicating that achievement is primarily determined by ability under an exam equilibrium.

Under district assignment the market clearing process in the 5 districts results in matching of households to school quality as in Figure (b). Tutoring choice is strongly determined by income as before. The impact of income on achievement is much more evident in Figure (d). Since, competition on housing price is mostly dominated by income, higher income and low ability households are able to benefit from higher school quality and thus obtain higher achievement. The contour lines are much steeper in income than before in the (y,a) space.

Any reasonable or even extreme parameter specifications still support that district equilibrium results in a higher income gradient than exam equilibrium. The underlying reason is because tutoring choice does not change much between the two regimes but the added component of the housing under a district regime is largely driven by income.



APPENDIX C: EQUALITY BETWEEN THE CONDITIONAL EXPECTATION OF INDIVIDUAL TEST SCORE AND THE CONDITIONAL EXPECTATION OF AVERAGE TEST SCORE OF THE COLLEGE

To illustrate this formally, denote t_c the average entrance exam score of students who enter college c, so that $t_c = E(t_i \mid s_{ic})$, where s_{ic}=1 represents the event that student i enters college c. This event is determined strictly by one's test score so s_{ic}=1 if $t_i \geq t_{c\min}$ and 0 otherwise. $t_{c\min}$ is the cutoff for college c, which is assumed to be determined exogenously of the model and depends on the applicant pool and number of slots available. Taking conditional expectations on the above definition of t_c,

$$
\begin{aligned}
& E(t_c \mid y_{is}, a_{is}, \theta_{is}) = E(E(t_{is} \mid s_{ic}) \mid y_{is}, a_{is}, \theta_{is}) \\
& = E(E(t_{is} \mid y_{is}, a_{is}, \theta_{is}, \beta y_{is} + \gamma a_{is} + \pi\theta_{is} + \varepsilon_{is} \geq t_{c\min}) \mid y_{is}, a_{is}, \theta_{is}) \\
& = E(t_{is} \mid y_{is}, a_{is}, \theta_{is})
\end{aligned}
$$

where the second equality is from the definition of the event of s_{ic}. The fact that the conditioning set inside is a subset of the conditioning set outside enables the use of the Law of Iterated Expectations and returns the final equality. Note that the decision whether or not to apply to college c is not relevant for the above property, since selection is purely determined by achievement which in turn is purely determined by variables included in the model. Under the assumption that the factors that affect the decision to apply to certain schools do not affect achievement directly, then estimating y_c on $y_{is}, a_{is}, \theta_{is}$ will return the same coefficient estimates as estimating y_{is} on $y_{is}, a_{is}, \theta_{is}$. Moreover, students constantly take mock college entrance exams during high school and receive information on her performance in the overall distribution. In addition, the above assumes risk neutrality and that differing degrees of risk aversion does not impact test performance.

APPENDIX D. AVERAGE EFFECTS

Though the theoretical model did not have predictions on average achievement, I examine the reduced form average effect of the regime change by estimating

$$t_{ijkm} = c + \beta y_{ijkm} + \gamma a_{ijkm} + \delta D_{jm} + Z_{ijkm}\pi + \theta_s + \mu_j + \eta_k + \lambda_m + \varepsilon_{ijkm},$$

which includes high school fixed effects θ_S but excludes the interaction terms that appear in previous tables. δ estimates how the remaining factors of test score production, i.e., factors not captured by father's education, own middle school grade, the high school and various fixed effects, impact college achievement on average. The most obvious factor is peer effects. Hence, one interpretation of δ could be the average peer effect in the district regime minus the average peer effect in the exam regime, or average peer effect when peers are predominantly segmented by income minus the average peer effect when peers are predominantly segmented by ability. As columns (1) and (2) below illustrate, the average effect of district assignment is -0.6 for the base sample and is -0.35 for the sub-sample of schools that have at least five observations. Both estimates are statistically indistinguishable from zero. I perform a couple of robustness checks by controlling for high school type in column (3) and examining log income as the outcome variable in column (4). δ is slightly negative and statistically indistinguishable from zero in these specifications as well. These estimates should not be interpreted as causal treatment effects but as reduced form outcomes that are inclusive of any impact sorting across districts or even cities have on average achievement. The results indicate that district assignment did not change achievement on average, with the consistent negative estimates pointing to potential negative effects.

Appendix D Table 1. Results on average achievement

Dependent variable:	College score	College score	College score	Ln(income)
	(1)	(2)	(3)	(4)
District assignment (D)	-0.600	-0.347	-0.511	-0.018
	(1.526)	(1.399)	(1.552)	(0.124)
Father's years of education	0.284***	0.295***	0.270**	0.004
	(0.105)	(0.102)	(0.111)	(0.009)
Middle school score	0.353***	0.375***	0.344***	0.016***
	(0.045)	(0.042)	(0.045)	(0.004)
Male	4.066***	5.059***	3.771***	0.932***
	(1.241)	(1.446)	(1.217)	(0.124)
Primary earner: Mother	-1.029	-0.202	-1.027	-0.050
	(1.435)	(1.366)	(1.418)	(0.166)
General high school(GHS)			-0.452	
			(4.016)	
GHS*HS year dummies			Y	
City, Age, and HS year FEs	Y	Y	Y	Y
High school FEs	Y	Y	Y	Y
Observations	2,160	1,220	2,160	1,907
R-squared	0.676	0.573	0.683	0.739

Notes: Age and age squared are included in column (4). Standard errors are clustered by high schools. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Appendix Figure 2 illustrates achievement trends for the treatment cities, cities that shift to district assignment, and the control cities, cities that maintain exam based selection. There is no evidence of a differential increase in achievement for the treatment cities relative to the control cities. To the contrary, achievement in the control cities seems to have improved more around 1980, the year all treatment cities shifted to the district regime. Various hypotheses could predict these results but more detailed data would be needed to test predictions. Some interpretations consistent with these finding are (1) sorting of high ability students from treatment to control cities in 1980; (2) no treatment effect with larger high school expansion in control cities than treatment cities around 1980; (3) a negative treatment effect with high school expansion in both groups around 1980. Nonetheless, Appendix Figure 2 does confirm the above table's results that district assignment does not increase achievement on average.

[ONLINE APPENDICES]

Appendix Table 1. Shift from exam based tracking to district assignment by city and year

Group	Policy year	City	City type
Group 1	1974	Seoul, Busan	Capital and 2nd largest city
Group 2	1975	Daegu, Inchon, Gwangju	Cities with population over 1,000,000 in 1975
Group 3	1979	Daejeon, Suwon, Masan, Jeonju, Jeju, Chongju, Chuncheon	Province capitals
Group 4	1980	Jinju, Changwon, Andong, Mokpo, Gunsan, Iksan, Wonju, Chonan	Other major regional cities
Group5 (Others)	No policy change	All other regions	

Appendix Table 2. Returns to colleges by rank groups

Dependent variable	Log (income)	
Top 3 college	0.931***	(0.090)
Top 4 to 7 college	0.874***	(0.114)
Top 8 to 10 college	0.805***	(0.111)
Top 11 to 30 college	0.610***	(0.062)
Any college below top 30	0.424***	(0.035)
Observations	2,133	
R-squared	0.12	

Notes: Omitted category is no college. Age and Age squared included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3. High school grade for non college goers compared to students who go to a college outside the top 30

Dependent variable	Middle school score	
Do not go to college	-6.0***	(0.037)
Observations	2,193	
R-squared	0.108	

Notes: Omitted category is students who go to a college outside the top 30. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4. Regression results of the specification in Figure 4

	Coefficient of Dummy(high school entrance year = N)*					
Dependent variable:	Father's education	Father's education* District assignment	Father's education* District assignment* Large (74&75) cities	Middle school score	ms score* District assignment	ms score* District assignment* Large (74&75) cities
College score	(1)	(2)	(3)	(4)	(5)	(6)
N=						
1970 or 1971	0.322			0.322		
	(0.238)			(0.238)		
1972 or 1973	0.215			0.215		
	(0.202)			(0.202)		
1974 or 1976	0.234*		-0.011	0.234*		0.011
	(0.125)		(0.193)	(0.125)		(0.049)
1977 or 1978	0.263***		0.026	0.263***		-0.002
	(0.093)		(0.135)	(0.093)		(0.022)
1979 or 1980	0.186	0.302	0.208	0.186	-0.033	-0.025
	(0.151)	(0.465)	(0.470)	(0.151)	(0.058)	(0.057)
1981 or 1983	0.205	0.094	0.163	0.205	-0.006	-0.034
	(0.156)	(0.267)	(0.266)	(0.156)	(0.045)	(0.052)
1984 or 1985	0.307	0.175	0.156	0.307	-0.029	-0.027
	(0.201)	(0.355)	(0.294)	(0.201)	(0.062)	(0.061)
Observations	2,228					
R-squared	0.342					

Notes: Above table reports result of the regression specified under Figure 4. The coefficient estimates are used to generate the evolution of the ratio of coefficients for the three groups of cities. Standard errors are clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively

Appendix Table 5 – Table results when constant terms are constrained to be equal across regimes

Dependent variable:	A. Include Mother's Education			B. Control for Occupation Groups		
College score	(1)	(2)	(3)	(4)	(5)	(6)
Father's years of education	0.265***	0.169**		0.105	0.029	
	(0.080)	(0.070)		(0.097)	(0.081)	
Father's education*D	0.228***	0.117		0.203**	0.113	
	(0.081)	(0.085)		(0.097)	(0.102)	
Mother's years of education		0.283***	0.361***		0.237***	0.239***
		(0.075)	(0.089)		(0.074)	(0.079)
Mother's education*D		0.036	0.111		-0.007	0.049
		(0.153)	(0.156)		(0.130)	(0.144)
Middle school score	0.396***	0.370***	0.372***	0.372***	0.351***	0.348***
	(0.032)	(0.030)	(0.030)	(0.030)	(0.029)	(0.030)
Middle school score*D	-0.026	-0.014	-0.004	-0.018	-0.007	0.003
	(0.016)	(0.020)	(0.017)	(0.017)	(0.021)	(0.016)
Male	3.080***	2.950***	2.946***	3.396***	3.151***	3.217***
	(0.394)	(0.375)	(0.381)	(0.422)	(0.385)	(0.379)
City, Age, and HS year dummies	Y	Y	Y	Y	Y	Y
Occupation group dummies				Y	Y	Y
Observations	2,078	1,748	1,799	1,842	1,563	1,608
R-squared	0.348	0.353	0.346	0.38	0.375	0.375
Test-statistic:						
$t = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}} - \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}}$	0.663***	0.345*		0.587**	0.332	
	(0.246)	(0.264)		(0.293)	(0.317)	
p-value of one sided test	0.004	0.097		0.023	0.148	

Notes: Sample is based on individuals where the father was the primary earner of the household. Standard errors clustered by cities. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Appendix Table 6. Robustness tests when constant terms are constrained to be equal across regimes

	Quantile regressions		Linear	High	Placebo policy years			Post 1972	
	0.7 quantile	0.9 quantile	Probability	school type	1965	1969	1985	sample	
Dependent variable:	College score	College score	Top 20 college	College score	College score	College score	College score	College score	Ln(income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Father's years of education	0.254**	0.359***	0.002	0.192**	0.408***	0.436***	0.433***	0.261***	0.003
	(0.106)	(0.084)	(0.002)	(0.080)	(0.104)	(0.134)	(0.070)	(0.060)	(0.006)
Father's education*D	0.420***	0.202	0.004***	0.163**	0.136	0.089	0.101	0.221***	0.012*
	(0.111)	(0.144)	(0.001)	(0.069)	(0.144)	(0.229)	(0.080)	(0.069)	(0.007)
Middle school score	0.416***	0.540***	0.006***	0.341***	0.336***	0.371***	0.400***	0.381***	0.014***
	(0.064)	(0.040)	(0.001)	(0.031)	(0.043)	(0.058)	(0.021)	(0.029)	(0.002)
Middle school score*D	-0.020	0.001	-0.000	-0.020	0.060*	-0.021	-0.012	-0.002	-0.000
	(0.024)	(0.029)	(0.000)	(0.017)	(0.033)	(0.032)	(0.019)	(0.019)	(0.001)
Male	3.201***	3.581***	0.028**	3.018***	3.311***	2.963***	2.548***	2.896***	0.839***
	(0.599)	(0.730)	(0.011)	(0.381)	(0.837)	(0.697)	(0.251)	(0.380)	(0.039)
Primary earner: Mother	-1.611**	-2.322**	-0.036***	-1.630***	-1.672	-1.488	-1.331**	-1.789***	-0.043
	(0.751)	(1.062)	(0.012)	(0.434)	(1.079)	(1.686)	(0.513)	(0.497)	(0.074)
General high school(GHS)				2.688**					
				(1.128)					
GHS*HS year dummies				Y					
City, Age, and HS year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,228	2,228	2,259	2,228	817	682	2,215	1,955	1,967
R-squared			0.127	0.387	0.395	0.412	0.349	0.345	0.366
Test-statistic:									
$t = \frac{\hat{\beta}_{district}}{\hat{\gamma}_{district}} - \frac{\hat{\beta}_{exam}}{\hat{\gamma}_{exam}}$	1.090***	0.371	0.747***	0.541**	0.159	0.326	0.292	0.585***	0.881**
	(0.417)	(0.307)	(0.292)	(0.233)	(0.451)	(0.725)	(0.255)	(0.213)	(0.506)
p-value of one sided test	0.005	0.113	0.006	0.011	0.362	0.363	0.128	0.003	0.042

Notes: Quantile regression results are based on Blaise Melly's Stata command quantreg which allows for clustering. Age and age squared are included in column (10). Standard errors are clustered by cities. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

AppendixTable 7. The relocation of top-tier high schools in Seoul

School type and name	1974 District	Present Location as of 1974 District	Year of Move	Admission to Seoul National University in 1972: Number of students admitted	Admission to Seoul National University in 1972: Rank nationwide	Admission to Seoul National University in 1980: Number of Students	Admission to Seoul National University in 1980: Rank among district based schools in Seoul
Public:							
Gyeonggi	UCD	District 3	1976	333	1	59	1
Seoul	UCD	District 3	1980	248	2	59	1
Gyeongbok	UCD	UCD	no move	212	3	n/a	n/a
Private:							
Joongang	UCD	UCD	no move	n/a	n/a	n/a	n/a
Baejae	UCD	District 3	1984	n/a	n/a	n/a	n/a
Hweemun	UCD	District 3	1978	n/a	n/a	34	8
Bosung	UCD	District 3	1989	n/a	n/a	43	5

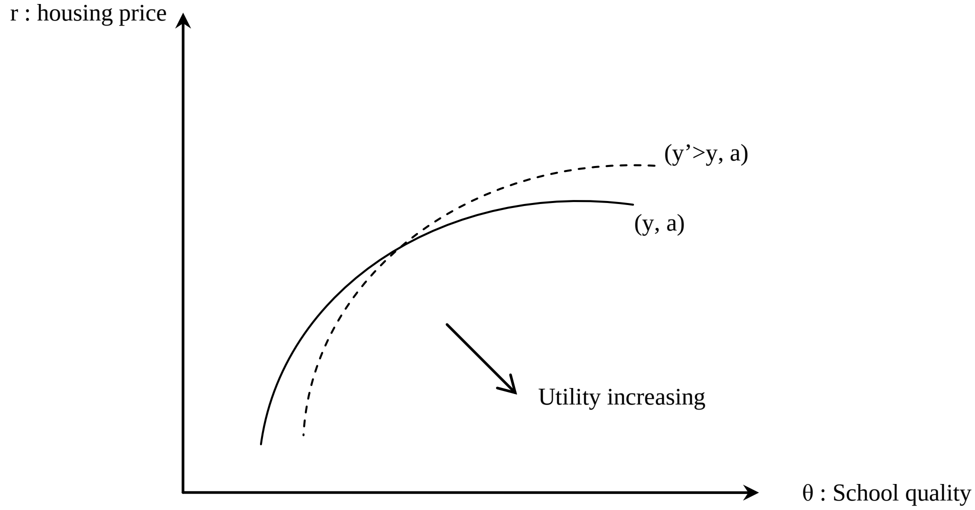
Sources: Donga Daily 1972.02.07 and 1980.1.29 accessed via Naver's Digital News Archive at dna.naver.com. Location information retrieved from each high school’s websites.

AppendixTable 8. Changes in population in the UCD

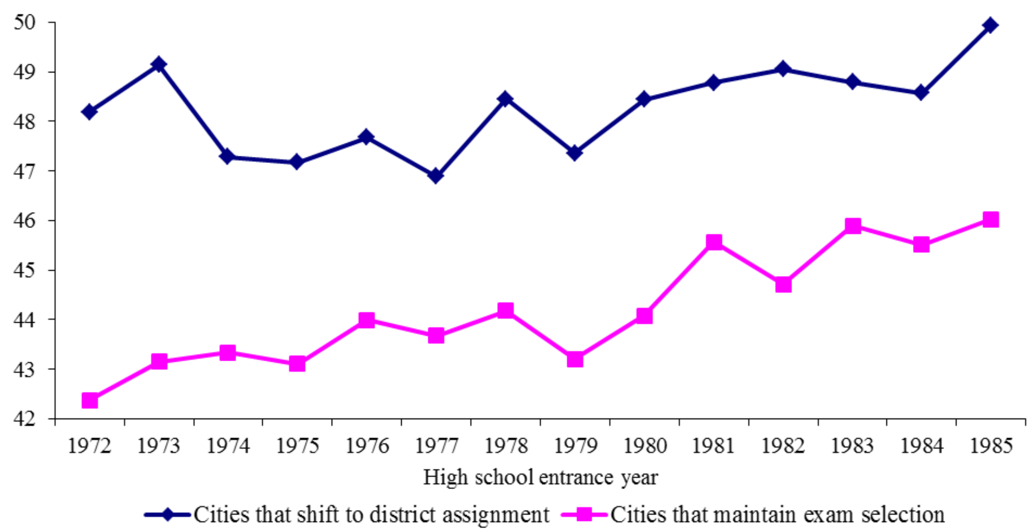
	Change in log population between 1973 and 1975	Change in log population between 1971 and 1977
District 1	0.0213	0.0779*
	(0.0250)	(0.0439)
District 2	0.0559***	0.126***
	(0.0205)	(0.0367)
District 3	0.212***	0.365***
	(0.0197)	(0.0351)
District 4	0.144***	0.313***
	(0.0169)	(0.0304)
District 5	0.121***	0.287***
	(0.0250)	(0.0439)
UCD*Dist 1	-0.0715*	-0.193**
	(0.0426)	(0.0750)
UCD*Dist 2	-0.0305	-0.124
	(0.0633)	(0.112)
UCD*Dist 3	-0.299***	-0.604***
	(0.0467)	(0.0824)
UCD*Dist 4	-0.0456	-0.214***
	(0.0385)	(0.0680)
UCD*Dist 5	-0.162***	-0.369***
	(0.0343)	(0.0603)
Observations	687	675
R-squared	0.260	0.305

Notes: Robust Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Figure 1. Illustration of single-crossing in income: Indifference curves in the *r-θ* space

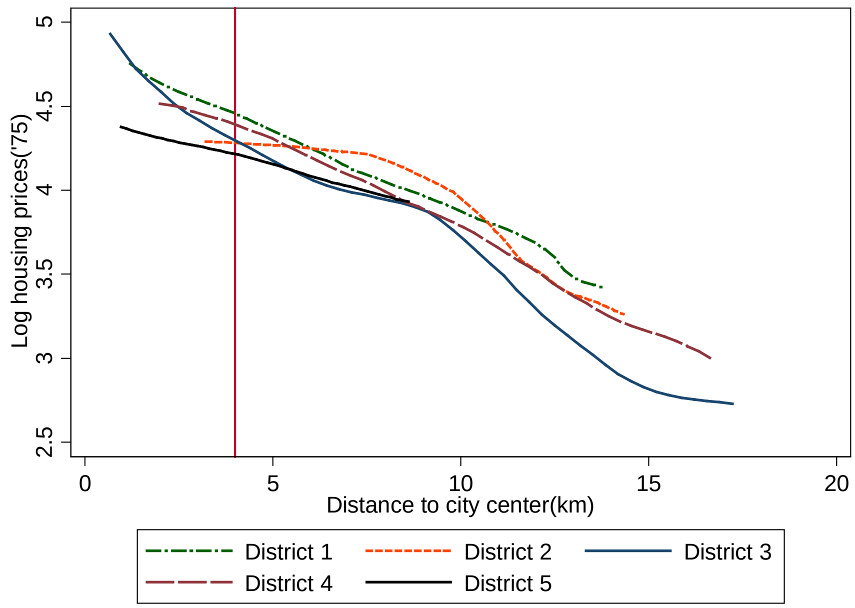


Appendix Figure 2. Trends in achievement for cities that shift to district assignment and cities that maintain exam based tracking



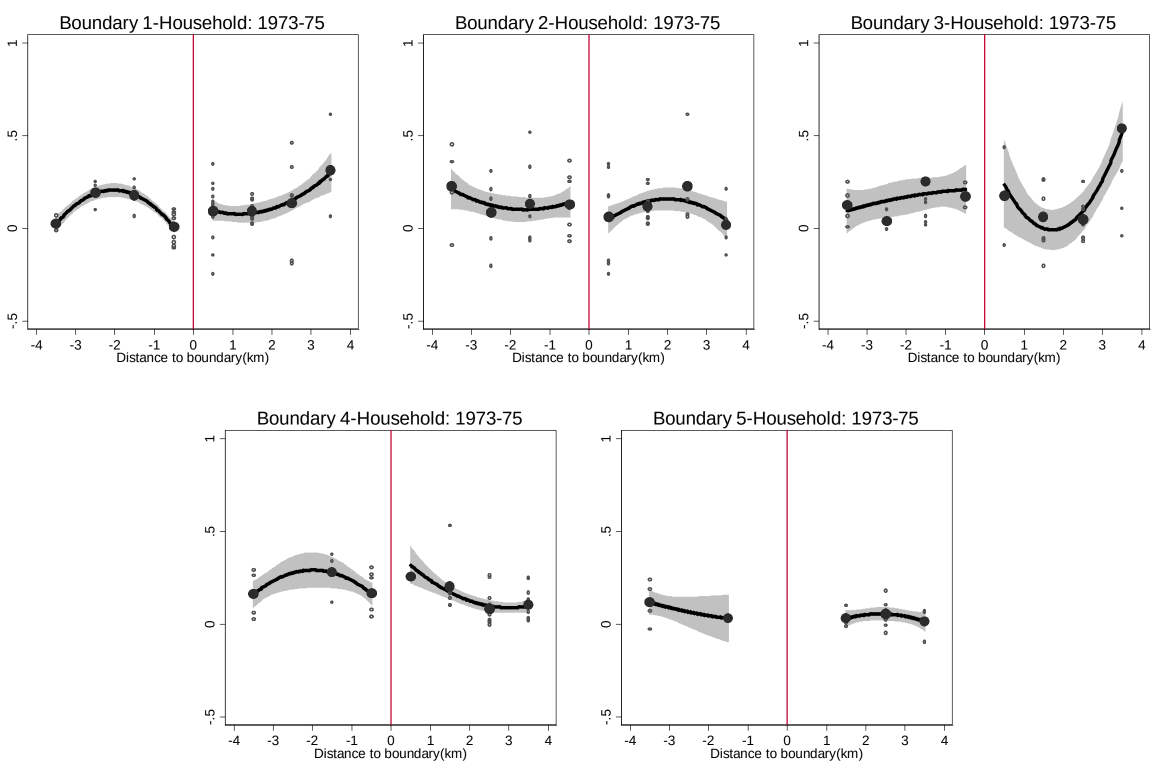
Notes: Above figure plots the average achievement for each high school entrance year for cities depending on whether it shifts regimes. By 1979 and 1980 all treatment cities shifted to district assignment.

Appendix Figure 3. Log housing land prices in 1975 by distance from the city center by district



Notes: Each line indicates a local linearized fit for each district using an Epanechnikov kernel with 2km bandwidths. The vertical red line identifies the 4km point, the boundary for the Unified Central District (UCD). There is an evident trend based on the distance to city center that follows a monocentric city model. The main point is that the spatial trend can be captured by distance.

Appendix Figure 4. Change in log number of households between 1973 and 1975 for each boundary



Notes: The open circles represent neighborhoods and the solid circles represent the mean values within each respective integer band. The solid lines are quadratic polynomial fits of the neighborhoods on each side of the boundary. The shaded areas represent 95% confidence interval bands.

DATA APPENDIX

The college score variable

I utilize two sources that report average test scores, one in 1976 and the other in 1994, to create the college score measure I use in the analysis. One source is an article in the daily newspaper Joongang-Ilbo which provides 1976 average test scores by college. The other is a college application reference book that provides average scores for 1994. The 1994 information is used to include schools that do not appear in the 1976 report, but the relative rankings of schools between the years do not change much. I normalize each test score to a 100 scale and then take the average of the two years to get a score for each college listed in the two sources. For schools not listed in the two sources, I categorize the college school (e.g., 4 year college in Seoul, regional 2 year technical college, etc.), and assign an average score based on the categorization and information in the two sources. I then assign each individual in KLIPS with his or her average college score. I standardize this score in the KLIPS sample and rescale so that the mean is 50 with standard deviation 10. Those who do not go to college are bottom coded at 41. Approximately 40% of the observations went to college during the sample years. The table below provides the distribution of the college's average test score before the rescaling based on the KLIPS sample.

Data Appendix Table 1. University scores used in the analysis

College	Score	College	Score	College	Score
Seoul National	80.93	Bukyung	66.08	Hansung	61.96
Yonsei	76.05	Kookmin	65.95	Gangwon	61.68
Korea	74.66	Sookmyung Women's	65.91	Donga	60.55
Sogang	73.17	Gwangun	65.66	Gyeongsung	60.06
Ewha Women's	72.85	Chungnam	65.60	Other 4yr college in Metropolis	60.00
Busan	72.63	Duksung Women's	65.56	Ulsan	59.13
Hanyang	72.28	Myungji	65.42	Wongwang	58.59
Korea Foreing	72.17	Inha	64.87	Sangjji	58.46
Seoul City	71.92	Seoul Women's	64.70	Silla	58.13
Sunkyunkwan	70.88	Sungshin Women's	64.67	Gyemyung	57.80
Korea Aerospace	69.91	Junbuk	64.36	Cheonggju	57.59
Joongang	69.49	Jeju	64.24	Gwandong	57.28
Gyeongbuk	69.38	Sejong	64.00	Other 4yr college	57.00
Kyunghee	68.63	Dongduk Women's	63.99	Chosun	56.82
Catholic	68.59	Chongshin	63.74	Mokwon	56.79
Dongkuk	67.75	Chungbuk	62.73	Baeje	55.80
Dankuk	67.29	Gyungsang	62.68	Daegu	55.41
Konkuk	67.03	Sangmyung	62.56	2yr Tech college in Seoul	50.00
Sungsil	66.97	Seogyung	62.54	2yr Tech college in Metropolis	47.00
Hongik	66.51	Youngnam	62.35	Other 2yr Tech college	44.00
Ajoo	66.47	Gyeonggi	62.34	No college	41.00
Junnam	66.40	Samyuk	62.22		
Gongju	66.12	Other 4yr college in Seoul	62.00		

School district quality

School district quality was created based on the January 17, 1969 edition of the daily newspaper, Kyeonghyang Shinmun. For each high school, the percentage of students who pass the college eligibility exam and the size of the test applicant are recorded. I match each high school to the school district as reported in the January 24, 1974 edition of Kyeonghyang Shinmun. I generate district quality measures by averaging each high school's pass rate weighted by the size of each high school's applicant pool. The below table provides the data used in the calculation. These newspaper articles have been made available through the historical archiving project by the Korean web portal, Naver. Archived newspapers are accessible free of charge at dna.naver.com

Data Appendix Table 2. College entrance exam pass rates by high schools in Seoul (1969)

Boy's High School				Girl's High School			
District	School name	Cohort size	College exam pass rate	District	School name	Cohort size	College exam pass rate
UCD	Gyeonggi	863	0.98	UCD	Gyronggi	557	1.00
	Gyeongdong	865	0.96		Gyesung	301	0.81
	Gyeongbok	901	0.98		Geumran	167	0.57
	Gyeongshin	408	0.67		Deoksung	445	0.31
	Gyunmyung	716	0.25		Baehwa	398	0.72
	Daeshin	514	0.36		Bosung	65	0.17
	Dongdae-busok	378	0.46		Seoul	211	0.41
	Dongbuk	313	0.61		Sudo	487	0.87
	Dongsung	367	0.91		Sookmyoung	530	0.99
	Baemyung	299	0.40		Soongeui	394	0.34
	Baemun	296	0.26		Shingwang	250	0.13
	Baeje	707	0.91		Ehwa	574	0.99
	Bosung	601	0.97		Edae-busok	69	0.87
	Seoul	920	0.99		Choongang	389	0.58
	Seongdong	779	0.84		Jinmyung	538	0.96
	Sungmun	489	0.61		Changduk	510	0.93
	Yangjung	645	0.89		Poongmun	342	0.54
	Yongsan	931	0.97		Jeongshin	445	0.86
	Edae-busok	80	0.80		Hyehwa	N/A	
	Inchang	554	0.46				
	Jangchoong	102	0.23				
	Joongdong	1193	0.76				
	Joongang	851	0.92				
	Hansung	446	0.52				
	Hongki	413	0.30				
	Hweemun	887	0.84				
District 1	Goryo	N/A		District 1	Seongshin	354	0.43
	Daeil	N/A			Hansung	281	0.33
	Seorabul	182	0.30				
	Shinil	N/A					
	Younghoon	N/A					
District 2	Kyunghee	409	0.79	District 2	Kyunghee	352	0.62
	Daegwang	553	0.89		Dongduk	462	0.63
	Seoul Sadae-busok	442	0.98		Seoul Sadae-busok	232	0.97
	Yongmun	120	0.18				
District 3	Sudo Sadae-busok	N/A		District 3	Muhak	436	0.78
	Youngdong	N/A			Hanyang	177	0.15
	Hanyoung	475	0.26		Sudo-busok	193	0.30
	Osan	239	0.37				
District 4	Gwanak	46		District 4	Seongshim	46	0.96
	Seongnam	576	0.64		Youngdeungpo	205	0.43
	Youngdeungpo	386	0.36		Joongdae-busok	78	0.33
	Janghoon	40	0.25		Sangmyung	475	0.41
	Joongdae-busok	136	0.32				
District 5	Gyeongsung	N/A		District 5	Dongmyung	284	0.16
	Daeseong	N/A			Myungji	18	0.33
	Mapo	327	0.33		Sunil	N/A	
	Myeongji	190	0.31		Yeil	N/A	
	Youido	N/A			Hongik	N/A	
	Choongam	N/A					