Exams, Districts, and Intergenerational Mobility: Evidence from South Korea

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Abstract

This paper examines how student assignment rules impact intergenerational mobility. High school admission had traditionally been exam based in South Korea. However, between 1974-80 the central government shifted several municipalities to a school district based admission system. I estimate the impact of this reform on intergenerational income elasticity. Results indicate that the shift increased intergenerational income elasticity from 0.21 to 0.32. I further find that selective sorting to reform cities by high income households was the underlying reason for the decrease in intergenerational mobility. Prior to the reform, a 10% increase in household income was associated with a 1.4% decrease in the probability of migrating, whereas, with the creation of school districts resulted in a 0.3% increase in the probability of migration. In sum, I find that the shift from a merit to a location based student assignment rule decreases intergenerational mobility and promotes selective migration by high income households.

Keywords: Intergenerational mobility, Exams, School districts, Migration

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I. Introduction

This paper examines how educational policy impacts intergenerational mobility. Specifically, I compare 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 from an exam to a district based system affect intergenerational income elasticity and through what channels?

Secondary school admission rules vary extensively across countries, particularly in the degree of ability tracking between schools. 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.[1] Whether secondary education should track students by prior achievement is contentious and has even led to instances where countries shift education policies. Several countries, including the UK, Sweden, Finland, and South Korea shifted away from an achievement based student allocation system during the 1960s and 1970s. More recently, some major Chinese cities have made similar transitions for middle school admission. One common motivation for such reforms was the concern that a merit based system likely perpetuates inequality and providing comprehensive education would lead to more equitable outcomes. (Betts 2011, Lai et al. 2011, Pekkarinen et al. 2009, Kang et al. 2008).

Economists have made considerable improvement in the measurement and understanding of intergenerational income elasticity and have started to examine the role educational policy plays in determining intergenerational mobility (see Black and Devereux 2011 for an overview).

[1] 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.

Among the literature that examines cross-country student assessment data, Hanushek and Woessman (2006) find that ability tracking exacerbates the impact of family background and test scores. However, Waldinger (2007) finds that the importance of family background does not increase with tracking using a difference in difference framework. Analysis using cross country achievement is consistently marred by concerns rising from unobservable determinants of achievement as well as the heterogeneity in educational policy across countries. The country specific literature improves on these points but nonetheless find various results. Pekkarinen et al. (2009) find that the Finnish school reform that transformed the selective education system to a comprehensive one reduced intergenerational income elasticity from 0.3 to 0.23. Similarly, 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 tracking increases test scores of high ability students whereas the district based system increases test scores of low ability wealthy students. Manning and Pischke (2006) suggest that households may have selected into districts with the UK reform. 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 household sorting in understanding intergenerational economic mobility. I contribute to this literature by examining the impact of the reform not only on intergenerational mobility but also on selective migration in South Korea (hereafter Korea).

The reform in Korea has several advantages for analysis. In the UK the local education authorities determined whether or not and when to implement the reform, which raises the concern of policy endogeneity. In Korea the military dictatorship centrally implemented the

regime change on short notice across several municipalities between 1974 and 1980. The reforms in Finland and Sweden were accompanied by the expansion 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. There are some notable differences as well. In the European countries, before the reform students were channeled into certain, e.g., academic versus vocational, tracks based on prior achievement and students usually attended schools in their locality. However, the exam based regime in Korea was strictly individual school based and was not tied to location. Anyone could apply to any school in the country and it was not uncommon for high achievement 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.

Using the variation in the timing of the regime shift across several municipalities, I find that the intergenerational income elasticity between parent and child increases from 0.21 to 0.32 after the regime shift. In other words, a 10% increase in the household income was associated with a 2% increase in the child's income under the exam system but increases to 3% under the district regime. I also find that the increase in intergenerational income elasticity is predominantly coming from students from higher income households.

Why would the shift from an exam to a district based assignment rule reduce intergenerational mobility? The municipalities that shifted to the district system often were the largest cities with many of the nation's prestigious high schools. If families desire better school quality then the district system would incentivize families to move or find ways to send their kids to a high school in these municipalities. Furthermore, higher income households would be more likely to support such move. Consistent with this hypothesized channel, I find evidence

consistent with selective migration by household income. The probability of a high school student in the reform city to have graduated from a middle school in a different city increases with income after the reform. Also, I find that the relationship between one's education quality and household income increases after the shift to district assignment. As richer families move or send their children to the reform cities, one's tertiary education quality becomes more strongly related to household income.

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 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, that of South Korea in the 1970s. Moreover, the exam based high school admission policies that we see in China, Romania, Kenya, and Ghana today are similar to that of Korea then. 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 intergenerational mobility will be important for structuring secondary education policies in these countries.

The paper proceeds as follows. In the following section I describe the shift from exam to district based assignment in Korea. Sections 3 and 4 explain the identification strategy and the data used for analysis. Section 5 presents the empirical results on intergenerational mobility, education quality, and selective migration. Section 6 concludes.

2. The shift from exam to district based assignment in South Korea

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 would apply to high schools of their choice, take exams offered by each individual high school, and each school would admit students with the highest scores. This system naturally generated a "tracked" system of high schools where there existed a perceived ranking of high schools based on prestige and how well each school did in sending students to top colleges. The most prestigious high schools were located in Seoul and the major regional cities. Under the exam system, excessive competition and tutoring among the wealthier middle school students was a recurring social issue and the military government announced in 1973 that individual high school entrance exams would be abolished and high school education would become normalized between schools. This reform was known as the High School Equalization Policy (HSEP).

The HSEP initially had three goals: to equalize student mix, teachers, and facility. Equalizing student mix was the least costly to implement: student allocation would be determined based on school districts and not on exams. The other components of the policy were not as successfully implemented because of the high costs associated with teacher training and facility improvement, and limited government budget (KEDI, 1998). Under the new district system, students would take a city wide eligibility exam and those above the cutoff would be allocated to a high school within their district by a lottery. High school districts were large with

Seoul initially starting off with 5 districts when there were over 80 schools. In smaller cities the whole city would become one high school district. Given the large size of high school districts, the shift initially succeeded in mixing student composition in high schools. The centralized shift to district assignment was implemented on a subset of cities. The HSEP started with the largest cities shifting in 1974 and then to the smaller cities. By 1980 when the central government initiated shift ended, 20 cities had transitioned to the district system. Table 1 provides the list of cities that shifted by year during this period.

Eventually in the 1990s, the central government allowed each city to determine which admission rule each city would pursue. Some cities that initially shifted to the district system reverted back to the exam system in the 1990s. Other cities newly shifted to the district system in the 2000s. As of today over 70% of all high school students in Korea are under district assignment. Also starting in the mid 1980s elite special purpose high schools that administered their own competitive exams were being established in cities that were otherwise district based. These new exam schools gradually became an influential part of the general education and the distinction between exam and district base admission becomes less clear. Therefore, I focus on the years before 1985 when the regime shift was centrally implemented by the government.[2]

3. Estimating the impact of the shift to district assignment on intergenerational mobility

I first estimate the impact of the shift to district assignment on intergenerational income elasticity. The empirical strategy relies on a difference in difference estimation that exploits the regional and temporal variation of the shift. In practice, I estimate

[2] The difference between public and private schools are not relevant for this period. Private schools were heavily regulated under the central government and operated in the same manner as public high schools. Private schools received the same government subsidy and did not have the autonomy to charge their own tuition or admit students. Many of the private schools were established by wealthy landlords as a means to maintain their estates during the land reforms enacted by the central government soon after the Japanese Occupation in 1945.

$$y_{ijk} = c + \beta_1 y_h + \beta_2 y_h D_{jk} + \gamma D_{jk} + Z_{ijk}\pi + \mu_j + \eta_k + \varepsilon_{ijk} \qquad (1)$$

where y_{ijk} is log own income for individual *i*, graduating from middle school in municipality *j*, belonging to cohort *k*, where cohort is defined by the year one graduates from middle school. y_h is the log household (parental) income for individual *i*, and D_{jk} is a dummy variable equal to 1 if cohort *k* in municipality *j* was under the district regime and 0 if under the exam regime. μ_j and η_k denote the municipality and cohort fixed effects. *Z* includes additional individual level controls: gender, whether the mother was the primary earner in the household, and the individual's middle school score. I also estimate versions of equation (1) that include log household income interacted with a time trend, and municipality specific time trends. The parameter β_1 measures the intergenerational income elasticity under the exam regime, β_2 measures the effect of the regime shift, and $\beta_1+\beta_2$ measures the intergenerational income elasticity under the district regime. The identifying assumption is that any change in intergenerational income elasticity unrelated to the shift is not systematically related to the timing of the regime shift in the different municipalities. In computing the standard errors I allow for arbitrary regional level spatial and temporal correlation in earnings by clustering at the regional level.

To understand the underlying channels of how the reform impacts intergenerational income elasticity, I first examine how the reform differentially impacts one's quality of education by household income. This amounts to using proxies for college and high school quality as the dependent variable in (1) and performing similar difference in difference estimation. The differential matching of households based on income to education quality would imply that there was selective household sorting after the regime shift in treated municipalities. I test how the reform impacted migration patterns of students who attended high school in the reform municipalities. As Table 1 indicates the reform areas were the major national and regional cities that often were the destination

for migrants seeking employment but also the places where many of the desired high schools under the exam regime were located. I examine how the probability of a high school student to have graduated from a middle school in a different city changed with the reform in relation to household income. In practice, I estimate

$$m_{ijk} = c + \beta_1 y_h + \beta_2 y_h D_{jk} + \gamma D_{jk} + Z_{ijk}\pi + \eta_k + \varepsilon_{ij} \quad (2)$$

where m is equal to 1 if individual *i* who attended a high school in municipality *j* graduated from a middle school in a municipality other than *j*, and zero otherwise. The base specification in (2) includes the other controls in (1) but excludes the municipality fixed effect and pools all intercity migration for estimation. I also estimate versions of (2) with municipality fixed effects and I use both linear probability models and probit models.

4. Data: variables and sample selection

My main data comes from the Korea Labor and Income Panel Survey (KLIPS), a nationally representative individual and household level labor market survey conducted by the Korea Labor Institute between 1998 and 2010. In addition to information on one's income over multiple years and parental education and occupation, KLIPS provides a supplemental education survey conducted during the 11th wave. The supplement provides information on individual educational history including the name, city, and entrance and graduation years of one's middle school, high school, and college. I use middle school location and graduating year information to identify each individual's exposure to either the exam or district regime.

Another useful aspect of the education supplement is that it asks one's achievement during middle school. Specifically, it asks one's middle school math, Korean, and English performance reported in a one to five scale. I standardize each score, take the average, and

rescale to mean zero and standard deviation one. I can use this as a pre-high school measure of achievement and include it as a control variable if it is not affected by the reform, which I show later in the empirical section. Hence, when I include the middle school score variable, I am asking how the high school reform impacted intergenerational mobility controlling for one's performance in middle school. Finally, I construct the average middle school performance of students by college and use it as a rough proxy for the quality of one's tertiary education. Those who do not enter college get a value equal to the average middle school performance of all individuals with no college. Similarly, I also generate a high school quality proxy but allow it to change based on the timing of the reform.

Own income is measured by averaging the reported annual pre-tax income during the working years between 1998 and 2010, where all income is converted to 2000 prices. Household parental income is estimated based on whether the household head was the father or mother, his or her respective years of education, and his or her occupation group. I use the large occupation group as classified in the survey. Since there is no information on parental income in KLIPS, I predict pre-tax household income based on another survey, the Household Income and Expenditure Survey, a quarterly administered survey that collects detailed expenditure and earnings data from rotating representative samples. Predicted parental earnings based on education, occupation or social class, are often used in the intergenerational income elasticity literature when direct measures are not available (Bjorklund and Jantti, 1997; Dearden, Machin, and Reed, 1997). I was able to find the Household Income and Expenditure Survey micro data with education and occupation information starting from 1985. I pool data from the 1985, 1987 and 1989 surveys, and restrict the sample so that the age of the household head was equal to or above 40 in 1985. This gives an approximate counterfactual set of parents who could have had

middle school students in my sample cohorts. The Appendix provides the summary statistics of this sample and the regression results used to predict household income for my main sample.

Lastly, to examine the impact of the high school admission reform on intergenerational income elasticity, I restrict my sample to those who reported income in KLIPS, responded to the supplemental survey, and graduated from middle school between 1970 and 1985. Cohorts that graduated middle school before 1970 were born during the Korean War and are subject to selective survival or birth by income level. Also, the observation in the data drops considerably for pre 1970 cohorts. I restrict to the 1985 and before cohorts due to the policy endogeneity concerns that I described before. The final sample size is 2,491 individuals. Table 2 presents the summary statistics of the main variables used in the analysis.

5. Results

Intergenerational income elasticity estimates vary widely between countries, cohorts, and methodology, but in general lie between 0.1 and 0.5 (Solon 2002, Black and Devereux 2011). I first examine the intergenerational income elasticity for all individuals in my sample. That is, I regress log own income on log household income for all cohorts graduating middle school between 1970 and 1985 and get an estimate of 0.287. Perrarinen et al. (2009) obtain an estimate of 0.277 for the Finnish birth cohorts born between 1960 and 1966, and Bjorklund and Jantti (1997) obtain an estimate of 0.28 for the Swedish cohorts aged 29 to 38 in 1990. The estimate for Korea during this period is comparable to that of the Scandinavian countries. However, I find that the intergenerational income elasticity estimates differ substantially across regions ranging from 0.36 in Seoul to 0.25 outside of Seoul. Also, the estimates seem to be increasing over time with an estimate of 0.28 for the earlier half of the sample (the 1970 to 1977 cohort) and 0.30 for the later cohorts. These regional and temporal differences in the elasticity estimates point to the

importance of using a difference in difference framework to estimate the impact of the shift to district assignment.

5.1. The effect of shifting from exam to district assignment on intergenerational income elasticity

Table 3 presents the estimates of the impact of the reform on intergenerational income elasticity. In column (1), I regress one's own average log earnings on the household's parental predicted log earnings, the district assignment dummy, and the interaction term without any additional control variables. The resulting intergenerational coefficient is 0.211 under the exam regime and increases by 0.127 to 0.338 in the district regime. However, this could reflect a nationwide upward trend or the regional variation in intergenerational income elasticity and not the reform per se. In column (2), I account for both of these factors by adding a full set of cohort and municipality fixed effects. The intergenerational income elasticity estimates are slightly smaller with 0.209 under the exam regime and 0.318 under the district regime. Both in columns (1) and (2) the district assignment is associated with a negative direct impact on one's own income. This is reflective of the non-linear impact of the regime shift whereby the increase in the intergenerational income elasticity is coming from the higher income households. I discuss the non-linear impacts of the reform in Table 4.

I am further interested in examining how the regime shift impacted intergenerational income elasticity controlling for one's performance in middle school. How do the estimates for intergenerational income elasticity change, when we are able to control for a measure that contains information about the student's ability? In column (3), I first check whether the shift had an impact on the measure of one's middle school performance I use. Household income strongly impacts one's middle school performance. A doubling of household income is

associated with a 0.5 standard deviation increase in one's middle school performance. However, this household income effect does not change with the regime shift nor the regime shift has any direct impact on one's middle school performance.

In column (4), I include the middle school score variable as one additional control to column (2). Including one's middle school score substantially reduces the intergenerational income elasticity under the exam regime. The coefficient estimate on household income drops by almost a half to 0.129 under the exam regime and the coefficient estimate on middle school score is large and statistically significant at 0.152. However, the impact of the shift on intergenerational income elasticity increases to 0.117 and is statistically significant at the 5% level. Even when we control for one's middle school performance, household income becomes much more important in determining one's own income under district assignment. In column (5), I include additional controls to confirm that the results I have found are not driven by some spurious pattern that coincides with the timing of the regime shift. I include the household income interacted with a time trend, and region specific time trends. The intergenerational income elasticity estimates for both the exam and district regime slightly increases and the overall patterns are identical to that of column (4). Region specific trends or time trends in intergenerational mobility cannot explain the findings.

These results are consistent with a model of selective matching whereby high income households are able to access better education under the district regime and earn higher earnings. Lee (2013) develops a closed city multi district model where households compete to gain access to the higher quality high school and one's education production is a function of school quality and own ability. In the model, 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 plays an important role in determining test scores. The implication is that income will play a stronger role in determining one's school quality under the district regime relative to the exam regime. I test later whether the results I find in Table 3 are consistent with selective matching towards educational quality as the underlying mechanism. However, I first examine whether the increase in intergenerational income elasticity is coming from a positive impact on students from rich households or a negative impact on students from poor households.

I evaluate this in Table 4 column (1) by estimating the impact of the shift by household income quartiles. Since I impose no linear constraints on the relationship between own income and household income the estimates of the shift are less restrictive than those reported in Table 3. First, under the exam regime only students from the richest quartile exhibit higher earnings than students from the poorest quartile. However, under the district regime students in the 2nd highest income quartile see a substantial increase in own earnings by 19%. The coefficient estimate on the highest income quartile is also large at 0.071 but is more noisy. Furthermore, there is evidence that earnings of students from the lowest income quartile may have decreased by 3% after the regime shift. This non-linear impact also explains the negative coefficient estimate on the district assignment dummy in Table 3. District assignment pulls down own income at lower levels of household income but increases at a faster rate with household income.

This strong positive impact of district assignment on students especially from households just above the median income is not surprising. The highest income families were able to use their resources and were less likely to be the marginal household impacted by the regime shift. On the other hand, households above the median income but not in the highest income quartile were likely impacted by the reform at the margin and could use their resources to buy the better schools and peers by moving or sending their child to a district school.

5.2. The effect of the reform on the quality of college education

Table 5 shows how the shift from exam to district assignment selectively impacted students' college quality by income and ultimately intergenerational mobility. In column (1) I simply add a proxy for one's college quality to the regression in Table 3 column (4). Recall that college quality was created by averaging the middle school score of all individuals that attended each college or no college. This is a rough measure but admission to college has always been exam based and the ranking of colleges have remained steady, providing a consistent measure to compare across cohorts (Lee 2012). Including this one variable drastically reduces the impact of the regime shift. Net of college quality, intergenerational income elasticity under the exam regime is reduced to 0.076 and is statistically significant only at the 10% level. The coefficient estimate on the interaction term also drops to around 0.07 but is no longer statistically significant. Unsurprisingly, college quality strongly impacts own income. I next test whether the regime shift selectively impacted college quality by household income. In column (2) I use college quality as the dependent variable in the base specification and find patterns identical to the intergenerational income elasticity results. The reform doubles the impact of household income on one's college quality.

How would a change in high school admission rule selectively impact one's college quality by household income? This would be possible, if higher income households move to districts with favorable factors for college entrance exams, e.g., higher school quality or better peer group. To test this, I first add high school fixed effects to column (2). The underlying idea is that high school fixed effects would capture school quality aspects like facility, teacher, principal

quality fixed during the sample years. If indeed there was selective sorting to high schools by income, I would expect the impact of the reform to diminish when I add high school fixed effects. As column (3) reports the coefficient estimate on the interaction term drops from 0.228 to 0.135 and is not statistically significant. In reality the reform would have likely changed aspects of high school quality. In column (4) I generate a proxy for high school quality but allow the measure to change pre and post shift by the average middle score by high school and regime status. This is a very rough measure since the number of observations at the high school level is very small and in many instances less than two. I include this proxy to column (2). Similarly, the impact of the interaction term decreases substantially and is no longer significant. Lastly, I use this proxy as the dependent variable in column (5). The evidence is weak but the signs are consistent with selective matching to the proxy measure by income under district assignment.

Table 4 column (2) examines the impact of district assignment on college quality by income quartiles. The improvement in college quality after the shift to district assignment is large and significant for the highest and 2nd highest income quartiles, a pattern that maps the income results in column (1). In column (3) I add the high school proxy variable. Consistent with the findings in Table 5, the large and significant impact of district assignment from the higher income quartiles dissipates with estimates no longer significant at the 5% level.

Table 5 illustrates that the significant increase in the relation between one's college quality and household income after the shift was in part due to the selective matching between high school quality and household income. However, there are likely many other factors that determine college quality that I do not control for in the regressions. Living in school districts or cities with more college educated households or after school cram schools could all impact one's desire for college and college quality. These could be the factors households were sorting

towards or benefited from after sorting towards reform cities. Such selective migration by income implies that high income families would have moved residences or sent their children to the reform cities when the regime shifted to district assignment.

5.3 The effect of the reform on selective migration

Migration is often empirically challenging to estimate due to limitations on residential location data. I also do not have residential location information. However, I am able to examine one dimension of migration, intercity migration, using information on middle school and high school location. I focus on the sample of individuals who attended a high school in any of the municipalities that made the transition to district assignment and create a dummy variable equal to one if that individual graduated from a middle school from a different municipality. I will define this incidence as "migration" from onward.

Among the 1,390 individuals that graduated from middle school between 1970 and 1985 and attended high school in an eventual district assignment city, 21 % migrated. The high level of migration may seem surprising but the exam regime was inherently prone to intercity migration. Furthermore, the 1970s and 80s were a period of high growth and urbanization in Korea. Recall there was no notion of districts under the exam regime and anyone could attend any high school in the country as long as he or she was accepted. This fact becomes more evident if we examine the share of individuals who migrate under each regime. Among those under the exam regime 25% had migrated, whereas, under the district regime 19% had migrated.

I perform a similar difference in difference in regression on this sample and examine whether the patterns of intercity migration changed by household income. Table 6 presents the result from the linear probability and probit models. In column (1), I run a simple regression of

migration on the district assignment dummy with the basic control variables and the cohort dummies. I initially do not include the region dummies and pool all intercity migrations for estimation. District assignment results in an 11% decrease in intercity migration. In column (2), I add household income and that interacted with the district assignment dummy. Poorer households were more likely to migrate. This would reflect the poor students from rural areas doing well on the high school entrance exam and moving to high schools in the urban area or the migration of poor families to the city seeking work. However, the coefficient estimate on the interaction term is positive. With the regime shift, students from relatively higher income households migrate to a high school in one of the reform cities. I add household income interacted with a time trend in column (3), and additionally the municipality fixed effect in column (4). I find a much stronger and statistically significant estimate on the interaction terms in both sepcifications. The estimates of the interaction term are larger than the main effect on household income. Under the exam regime a 10% increase in household income resulted in a 1.4% decrease in the probability of migrating whereas, after the district regime resulted in a 0.3% increase in the probability of migration. The probit estimates in columns (5) and (6) support the same results. Higher income increases the probability of migrating under district assignment. The coefficient of -1 on the district assignment dummy indicates that the average individual in the sample attending a high school in one of the reform cities would have had almost surely come from a different city under the exam system.

As the student allocation rule shifted from exam to district based assignment, intergenerational income elasticity increased in Korea. When the reform happened students from higher income households migrated to the larger district cities, which had the prestigious schools, and were able to benefit from the better school quality, ultimately resulting in better college

quality and higher earnings. The migration I can test with this data is limited to intercity migration but Lee (2013) finds evidence of migration across school districts within a city, whereby, residential land prices across school districts in Seoul increase differentially before and after the reform.

5.4. Robustness checks

Table 7 presents robustness checks that test the sensitivity of the results. In column (1) I use the predicted household head's salary instead of the predicted household total income for y_h. The intergenerational income elasticity estimate is slightly smaller than that in Table 3, but the relative magnitude of the estimate on the interaction term is similar and statistically significant. In column (2) I replicate the base intergenerational income elasticity regression on the migration sample used in Table 6, i.e., I only include the treated cities. I find results similar to the full sample with the interaction term large and significant at the 10% level with somewhat larger standard errors. In columns (3) and (4), I run the intergenerational income elasticity regression and migration probit regression on a sample that drops observations from Seoul which was the major destination city for migrants and also had many of the nation's most prestigious high schools. Coefficient estimates on the intergenerational income elasticity regressions in column (3) show similar magnitudes with slightly larger standard errors. In column (4) the interaction term is substantially stronger indicating that migration to cities other than Seoul were more highly related with household income.

The final 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 4 years before when the policy actually took place. The coefficient

estimate on the interaction terms in both columns (5) and (6) are indistinguishable from zero, which confirms that there is no treatment effect when I use the placebo policy years.

6. Conclusion

This paper finds that the shift from an exam to a district based admission system for high school increased intergenerational income elasticity in South Korea. I also find that students from higher income households were more likely to move to district cities and access better quality high school and college after the reform. A handful of research have examined the role of educational policy in determining intergenerational mobility. I find that educational policy coupled with selective migration of households by income can impact intergenerational mobility. In the case of Korea, district based admission relative to an exam one increased intergenerational income elasticity by about ten percentage points to 0.3.

However, the literature on the Scandinavian countries finds that the shift away from a selective to a comprehensive admission system reduces intergenerational income elasticity, and finds no evidence that selective migration by income is playing a role. One explanation for the difference in migration behavior between Korea and the Scandinavian countries may be the difference in policy implementation. In Korea the transition to school districts only occurred in the larger municipalities which often had high schools that were desired by families even before the reform. On the other hand, the reform in both Finland and Sweden was nationwide. Furthermore, the perceived variance in school quality and reputation could have been larger in Korea. Prestigious high schools were singled out and visible. Every year major newspapers would publish a list of well performing high schools and the number of students admitted to the nation's top colleges. Another explanation may be the difference in the educational approach

whereby the Scandinavian model promotes educational equality whereas the Korean model emphasizes competition. Recognizing why residential sorting is more prominent in one context versus another would help further our understanding of the underlying determinants of intergenerational mobility.

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Table 1. The shift from exam to district based assignment by city and year

Year of shift	City	City type
1974	Seoul, Busan	Metropolis (Population over 1,000,000 in 1975)
1975	Daegu, Inchon, Gwangju	
1979	Daejeon, Suwon, Masan, Jeonju, Jeju, Chongju, Chuncheon	Province capitals
1980	Jinju, Changwon, Andong, Mokpo, Gunsan, Iksan, Wonju, Chonan	Other major regional cities
	No shift in all other regions	

Notes: The central government allowed municipalities to choose its own admission system later in the 1990s. Some of the cities that initially shifted to the district system reverted back to the exam system in the 1990s. Other cities shifted to the district system in the 2000s. I focus on the period before 1985 when the shift was exogenously enforced by the central government.

Table 2. Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Log average income (self)	16.57	0.77	12.02	18.95
Log household income (parent)	16.05	0.32	15.31	17.14
Female	0.41	0.49	0	1
Primary earner: mother	0.07	0.26	0	1
Age in 2000	57.73	4.66	41	71
Middle school score	-0.08	0.99	-2.60	1.59
Under district assignment	0.32	0.47	0	1
Year graduated middle school	1978	4.52	1970	1985

Notes: Data comes from the Korea Labor Income Panel Survey, 1998-2010, and the above summary statistics are for the 2,941 observations used in the base sample that estimates intergenerational income elasticity. Own income is the average pre-tax income reported in all rounds of the survey in 2000 Korean Won. Household parental income is predicted based on the household head's years of education and occupation using data from the Household Income and Expenditure Surveys of 1985, 1987, and 1989. The Appendix provides the summary statistics of the observations used to predict parental income and the regression results.

Table 3. Main results on intergenerational income elasticity

Dependent variable	Log income	Log income	Middle school score	Log income	Log income
	(1)	(2)	(3)	(4)	(5)
District assignment	-1.960**	-1.669*	-0.079	-1.797*	-2.288**
	(0.859)	(0.938)	(1.771)	(0.855)	(0.786)
Household income	0.211***	0.209***	0.527***	0.129**	0.180**
	(0.048)	(0.043)	(0.073)	(0.046)	(0.064)
Household income*district assignment	0.127**	0.109*	0.000	0.117**	0.148***
	(0.053)	(0.059)	(0.110)	(0.054)	(0.049)
Middle school score				0.152***	0.153***
				(0.010)	(0.010)
Cohort dummies		Y	Y	Y	Y
Region dummies		Y	Y	Y	Y
Household income*time trend					Y
Region specific timetrends					Y
Observations	2,491	2,491	2,766	2,462	2,462
R-squared	0.017	0.307	0.079	0.341	0.345

Notes: All specifications include dummy variables indicating whether the individual was female and whether household head was the mother. Observations are for individuals who graduated from middle school between 1970 and 1985. Standard errors clustered at the municipality level are reported in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 4. Impact by household income quartiles

Dependent variable	Log income	College quality	College quality
	(1)	(2)	(3)
Household income 1st quartile			
Household income 2nd quartile	-0.005	-0.004	0.074
	(0.024)	(0.032)	(0.053)
Household income 3rd quartile	-0.030	0.007	0.096
	(0.035)	(0.042)	(0.064)
Household income 4th quartile	0.149***	0.224***	0.329***
	(0.043)	(0.048)	(0.061)
Household income 1st quartile *district assignment	-0.030	-0.023	-0.021
	(0.027)	(0.060)	(0.081)
Household income 2nd quartile *district assignment	0.065	0.054	0.007
	(0.041)	(0.068)	(0.089)
Household income 3rd quartile *district assignment	0.191***	0.236***	0.172*
	(0.038)	(0.077)	(0.099)
Household income 4th quartile *district assignment	0.071	0.221***	0.127*
	(0.061)	(0.062)	(0.074)
High school quality			0.038
			(0.025)
Cohort dummies	Y	Y	Y
Region dummies	Y	Y	Y
Observations	2,462	2,407	1,905
R-squared	0.343	0.292	0.350

Notes: The 1st income quartile indicates the lowest income quartile and the 4th income quartile indicates the highest income quartile. All specifications include middle school score and dummy variables indicating whether the individual was female and whether household head was the mother. Observations are for individuals who graduated from middle school between 1970 and 1985 and report income. Standard errors clustered at the municipality level are reported in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 5. Results on education quality

Dependent variable	Log income	College quality	College quality	College quality	High school quality
	(1)	(2)	(3)	(4)	(5)
District assignment	-1.080	-3.557***	-2.099	-1.967	-0.346
	(0.836)	(0.933)	(2.203)	(1.277)	(1.030)
Household income	0.076*	0.231***	0.204*	0.320***	0.038
	(0.043)	(0.053)	(0.110)	(0.068)	(0.054)
Household income*district assignment	0.070	0.228***	0.135	0.127	0.018
	(0.052)	(0.057)	(0.136)	(0.078)	(0.064)
College quality	0.201***				
	(0.018)				
High school quality				0.037	
				(0.024)	
Cohort dummies	Y	Y	Y	Y	Y
Region dummies	Y	Y	Y	Y	Y
High school dummy			Y		
Observations	2,462	2,407	1,905	1,905	1,905
R-squared	0.361	0.288	0.724	0.346	0.646

Notes: All specifications include middle school score and dummy variables indicating whether the individual was female and whether household head was the mother. Observations are for individuals who graduated from middle school between 1970 and 1985 and report income. Standard errors clustered at the municipality level are reported in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 6. Selective migration by household income

Dependent variable:	Migrate (High school and middle school were in a different city)					
	Linear probability model				Probit model	
	(1)	(2)	(3)	(4)	(5)	(6)
District assignment	-0.116***	-1.416	-2.866**	-2.814*	-1.000***	-1.000***
	(0.043)	(1.107)	(1.401)	(1.449)	(0.002)	(0.002)
Household income		-0.183***	-0.140**	-0.138**	-0.137*	-0.123
		(0.060)	(0.068)	(0.070)	(0.075)	(0.077)
Household income*district assignment		0.080	0.170**	0.173*	0.164	0.165
		(0.068)	(0.086)	(0.089)	(0.100)	(0.103)
Cohort dummies	Y	Y	Y	Y	Y	Y
Household income*time trend			Y	Y	Y	Y
High school city dummy				Y		Y
Observations	1,374	1,305	1,305	1,305	1,305	1,296
R-squared	0.018	0.027	0.028	0.114		

Notes: All specifications include middle school score and dummy variables indicating whether the individual was female and whether household head was the mother. Observations are for individuals who attended a high school in the reform cities between 1970 and 1985. Robust standard errors are reported in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

Table 7. Robustness checks

	Household head salary	Treatment region only	Without Seoul		Placebo year	
Dependent variable	Log income	Log income	Log income	Migrate (Probit)	Log income	Migrate (Probit)
	(1)	(2)	(3)	(4)	(5)	(6)
District assignment	-1.354*	-2.413	-2.209	-1.000***	0.993	-0.411
	(0.657)	(1.420)	(1.536)	(0.000)	(1.450)	(1.285)
Household income	0.099**	0.094	0.117*	-0.102	0.275***	-0.038
	(0.036)	(0.079)	(0.063)	(0.113)	(0.071)	(0.055)
Household income *district assignment	0.092**	0.157*	0.141	0.323**	-0.060	0.007
	(0.043)	(0.088)	(0.097)	(0.147)	(0.089)	(0.118)
Cohort dummies	Y	Y	Y	Y	Y	Y
Region dummies	Y	Y	Y		Y	
Observations	2,462	1,144	2,064	840	1,187	542
R-squared	0.341	0.313	0.350		0.325	

Notes: All specifications include middle school score and dummy variables indicating whether the individual was female and whether household head was the mother. Column (1) uses predicted household head salary instead of predicted parental income as household income. Column (2) restricts samples to individuals who attended high school in the reform cities between 1970 and 1985 as in Table 6. Columns (3) and (4) drop individuals that graduated from middle school in Seoul. Column (5) and (6) uses a counterfactual shift that occurs 4 years before the actual regime shift. Columns (4) and (6) report probit results. The income regressions cluster standard errors at the municipality level, which are reported in parentheses. The migration regressions use robust standard errors, which are reported in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.

APPENDIX: SUMMARY STATISTICS OF SAMPLE AND REGRESSIONS USED TO PREDICT HOUSEHOLD INCOME

Appendix Table 1. Summary statistics of sample used to predict parental income

Variable	Mean	Std. Dev.	Min	Max
Year of birth	1938	5.88	1911	1945
Age of household head	47.44	5.88	40	74
Household head: years of education	10.14	4.23	0	18
Household head: female	0.17	0.38	0	1
Log (household income)	16.25	0.63	12.97	18.71
Log (household head salary)	15.88	0.73	13.00	18.63
Occupaton group: professional	0.08	0.27	0	1
Occupaton group: administration	0.01	0.10	0	1
Occupaton group: government	0.06	0.23	0	1
Occupaton group: office work	0.13	0.34	0	1
Occupaton group: sales	0.05	0.22	0	1
Occupaton group: service	0.14	0.35	0	1
Occupaton group: production	0.48	0.50	0	1
Occupaton group: other	0.05	0.22	0	1

Notes: Data comes from the Household Income and Expenditure Surveys for 1985, 1987, and 1989. I pool data from the 1985, 1987 and 1989 surveys, and restrict the sample so that the age of the household head was equal to or above 40 in 1985. This gives an approximate counterfactual set of parents who could have had middle school students in my sample cohorts. The summary statistics are reported for the base 4,045 individuals used in the household income regressions.

Appendix Table 2. Regression predicting household income and household head salary

	(1)	(2)
	log (household income)	log (household head salary)
Household head: years of education	0.047***	0.063***
	(0.002)	(0.003)
Household head: female	-0.264***	-0.389***
	(0.023)	(0.023)
Professional	16.226***	15.813***
	(0.048)	(0.049)
Administrator	16.291***	15.842***
	(0.086)	(0.089)
Government	16.086***	15.687***
	(0.047)	(0.049)
Office work	16.005***	15.553***
	(0.040)	(0.041)
Sales	15.677***	15.068***
	(0.044)	(0.045)
Service	15.744***	15.151***
	(0.032)	(0.033)
Production	15.733***	15.194***
	(0.024)	(0.025)
Other	15.532***	15.008***
	(0.044)	(0.046)
Observations	4,045	4,038
R-squared	0.999	0.999

Notes: Household income includes all pre-tax annual parental income. Household head salary is pre-tax annual salary. Robust standards errors are reported in parentheses. ***, **, and * indicates significance at the 1%, 5%, and 10% level, respectively.