The Effects of Korea's High School Leveling Policy on School Choice and Labor Market Earnings

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Abstract

This paper analyzes the impact of a high school leveling policy on high school and college attendance and their later labor market earnings in South Korea. Since 1974, some cities have replaced the traditional high school assignment system, where students took an entrance exam to get accepted to high schools, with a lottery-based enrollment system within a school district. By using a new DiD estimation method for staggered policy adoptions, proposed by Callaway and Sant'Anna (2021), I revisit the policy impact on labor market earnings. I also investigate a potential mechanism through which the policy affected the labor market earnings: the high school tuition effect on students' high school choices. My estimation results show that the high school leveling policy increased tuition for public high schools and 4-year college attendance while there are heterogenous policy impacts on high school choices, other college outcomes, and labor market earnings across different groups of cities.

1 Introduction

In this paper, I examine the impact of a high school leveling policy on high school and college attendance and their later labor market earnings in South Korea. Since 1974, some cities have replaced the traditional high school assignment system, where students took an entrance exam to get accepted to high schools, with a lottery-based enrollment system within a school district. Proponents of the high school leveling policy argue that it helped reduce educational inequality by allocating educational resources more evenly across schools and helped prevent low-achieving students from falling behind (Strike, 1986). Opponents argue that it hurts high-achieving students because high-achieving students can no longer cluster and benefit from peer effects. (Lazear, 2001). Therefore, whether the high school leveling policy is beneficial to society is theoretically

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ambiguous, and requires empirical evaluation.

The high school leveling policy was first introduced in Seoul and Busan in 1974. Since then, other cities have adopted this policy at different times. While previous studies (Nam and Choi, 2004; Kang et al., 2007) analyze the policy effect by using difference-in-differences methods, they choose the treatment and comparison groups based on variables that could have been affected by the policy change. In addition, Nam and Choi (2004) use two-way fixed effects (TWFE) regressions to estimate the policy impact with the sample spanning from 1969 to 1986, but Goodman-Bacon (2021) points out that when regions receive treatments at different points in time, the standard two-way fixed effects (TWFE) regressions require the assumption of a time-invariant treatment effect to get an unbiased estimate, which is less likely in many cases. Therefore, in this paper, I estimate the impact of high school leveling in the 1970s with a new estimation method proposed by Callaway et al. (2021) and a more appropriate definition of the treatment and comparison groups.

It is highly likely that the high school leveling policy had heterogeneous effects across the treated cities and over time, creating bias in the standard two-way fixed effects regression specification. This is mainly due to differences in infrastructure and the quality of education across cities along with policy updates by the government. Larger cities were the first to adopt the high school leveling policy. It was first implemented in the two largest cities in South Korea, the next three largest cities comprised the second roll-out, and the other major cities were included in the third and fourth roll-outs. However, it was not necessarily true that bigger cities were more ready to be equipped with a uniformly good learning environment across high schools for a successful policy implementation.

One of the imminent problems for the policy was that many private schools had poor school facilities, but they were required to finance most of school improvement on their own, leaving some schools below the educational standard proposed by the Ministry of Education. Since there was wide variation in the proportion of high schools that were private across cities, ranging from 18.92% in Gangwon province to 80.14% in Seoul, it could have induced different policy effects across cities, through different readiness for private high school improvement. The leveling policy was also likely to create dynamic effects over time as the Korean government amended school operation rules based on the feedback and complaints from schools, parents, and students. Since cities with later adoption incorporated those updated rules upon their policy adoption, it would also have contributed to heterogeneous effects across cities.

My work contributes to the literature that studies the impact of the high school leveling policy in Korea on labor market earnings. First, I revisit the previous studies that analyze the effect of the high school leveling policy in the 1970s in Korea on the labor market earnings (Nam and Choi, 2004;Kang et al., 2007) while defining the treatment variable differently. Nam and Choi (2004) define the treatment(comparison) group as high school graduates whose high schools were in cities with(without) the leveling policy between 1974 and 1986. High school choice is, however, potentially another outcome variable that could be affected by the policy change. For example, in the absence of policy adoption, high-achieving students in rural areas, which did not adopt the leveling policy, might have chosen to attend a prestigious high school in a larger city. Once larger cities adopted the leveling policy, some might not have wanted to be mixed with low-achieving students and attended high schools in their hometowns. For this reason, instead of using high school location to define the treatment and comparison groups, I use students' residences when they were 14 years old, which was usually a year ahead of the high school entrance, to define the treatment and comparison groups.

Kang et al. (2007) define the treatment(comparison) group as general high school graduates (vocational high school graduates) in school districts with the policy. They carefully examine students' geographic mobility before and after the policy change to address student composition changes and limit the sample to the five cities that adopted the policy between 1974 and 1975. They argue that the policy only applied to general high schools, and thus vocational high schools were not treated by the policy change. To address the endogeneity of school type choice, they conduct an IV estimation by using whether the father was alive in the year she entered high school as an instrumental variable. However, school type choices were still likely to be affected because policy adoption made it easier for low-achieving students, who would have chosen a vocational high school due to their low middle school grades, to get into a general high school. Therefore, by defining the treatment status at the city level, regardless of the types of high schools, I attempt to measure the policy impact more accurately.

Second, the high school leveling policy not only changed the student mix but also increased high school tuition in the affected cities. The South Korean government recognized that every high school should have comparable facilities and teachers to ensure successful policy implementation. For this reason, the government required every high school to improve facilities ahead of the policy and fired/retrained unqualified teachers. However, such improvements in the educational environment came at the expense of higher tuition as the government failed to finance school improvement projects, allowing schools to raise tuition instead. While the previous studies failed to incorporate the policy impact on high school tuition, as a possible mechanism for school attendance and, thereby, labor market earnings, I directly estimate the leveling policy impact on high school tuition, which further supports my argument that neither high school locations nor high school types should be used to determine treatment status. I find that the high school leveling policy increased tuition for public general high schools and public vocational high schools by 12.79 percent and 51.35 percent, respectively, on average, while decreasing tuition for private high schools by 9.74 percent.

Third, I rigorously measure the impact of high school leveling on students' high school choices as a possible channel through which the leveling policy could affect labor market earnings. Policy adoption was accompanied by tuition increases, which were more rapid for public high schools to equalize tuition in all high school types. Therefore, the policy change could have negatively affected high school attendance. On top of that, due to such changes, fewer students could have chosen public vocational high schools as those schools had the steepest tuition increases. My main analysis finds that there was a negative policy impact on high school attendance but a positive impact on vocational high school attendance, on average, although these are not statistically significant at the aggregated level.

Lastly, I also examine how the leveling policy affected college outcomes since the policy impact on college outcomes is theoretically ambiguous in that the policy created a mix of students with different academic abilities while improving the overall academic environment through better facilities and teachers. I find that the policy raised the probability of going to any college and a 4-year college in the affected cities by 15.13 percentage points and 15.35 percentage points, respectively, on average. However, there was a slightly negative but statistically insignificant effect on getting into a prestigious college. The policy had a negative but statistically insignificant effect on hourly wages, which could indicate that the lower high school attendance with the higher vocational high school attendance had a larger negative impact on the hourly wages, on average.

My findings for college outcomes are comparable to Nam and Choi (2004) while those for hourly wages are somewhat different from Nam and Choi (2004) and Kang et al. (2007). Although the estimates from Nam and Choi (2004) are statistically insignificant, they find that the leveling policy increased the probability of going to a 4-year college and one of the top 18 colleges by 2.9 percentage points and 1.1 percentage points, respectively. On the other hand, the 15.35-percentage-point increase in 4-year college attendance that I find is statistically significant, and the magnitude is much larger. For hourly wages, I find that the policy decreased them by 6.91 percent, although the point estimate is not statistically significant. However, Nam and Choi (2004) estimate that the policy increased hourly wages by 13.5 percent, which is statistically significant, and Kang et al. (2007) find a statistically insignificant increase in the hourly wages by 16.4 percent.

2 Institutional Background

The Korean K-12 education system consists of six years of elementary school, three years of middle school, and three years of high school. When it comes to high schools, students could choose between vocational high schools and general high schools in the 1970s and 1980s. Vocational high schools are designed to train for industrial technicians, so most vocational high school graduates started working upon graduation. General high schools focus more on academic curriculum and many general high school graduates went to colleges.[1]With the increasing demand for higher education, getting into general high schools was more competitive than vocational high schools.

Until 1968, students took an entrance exam to get into middle school. It was important to get into a prestigious middle school for better (general) high school and college outcomes, which caused fierce competition among elementary school students. To alleviate such competition among children, along with increasing demand for middle school education, the Ministry of Education introduced a lottery system for middle school assignments in 1969. The new assignment system was rolled out nationwide, starting in Seoul in 1969, other major cities in 1970, and all the other regions in 1971. For successful policy implementation, the Korean government mainly focused on reducing gaps in school facilities and the quality of teachers between schools, as prestigious middle schools had better school facilities and teachers.

Although the newly adopted lottery system settled competition toward better middle schools, students were still in competition for prestigious high schools, as getting into a prestigious high school still raised the chance of getting into a prestigious college. Before 1974, high school applicants took an entrance exam, which was conducted at the school level. As concerns about a wide educational gap between high schools and fierce competition among middle school students came to the fore, the high school leveling policy was introduced and rolled out from 1974 to 1980 across big cities to address such concerns. It was first implemented in Seoul and Busan in 1974, then Daegu, Incheon, and Gwangju in 1975. It expanded to 15 additional cities between 1979 and 1980.

Table 1 and Figure 1 show the variation in policy adoption across cities. The initial plan was to start in the two biggest cities, Seoul and Busan, in 1974 and then expand to the other metropolises and the capital cities of provinces in 1975, and the rest of the cities in 1976 (Kang, Jeungmo (Kyunghyang Shinmun), 1974). However, the government postponed the policy expansion plan and introduced it in only three metropolises

[1] According to the 1973 Statistical Yearbook of Education, 44.4% of general high school graduates went to colleges while 12.2% of vocational high school graduates did.

in 1975 as the first implementation in Seoul and Busan faced issues of transferring from/to other regions and complaints from students and parents about unsatisfactory improvement in school facilities(Kyunghyang Shinmun, 1974). The policy implementation resumed in the seven capital cities of provinces in 1979, and the eight big cities in 1980. The Ministry of Education revised its plan to expand the policy across cities year by year and implement it in every city by 1986, but it had to put a hold on expansion again due to opposition from regional superintendents of education (Chosun Ilbo, 1980b).

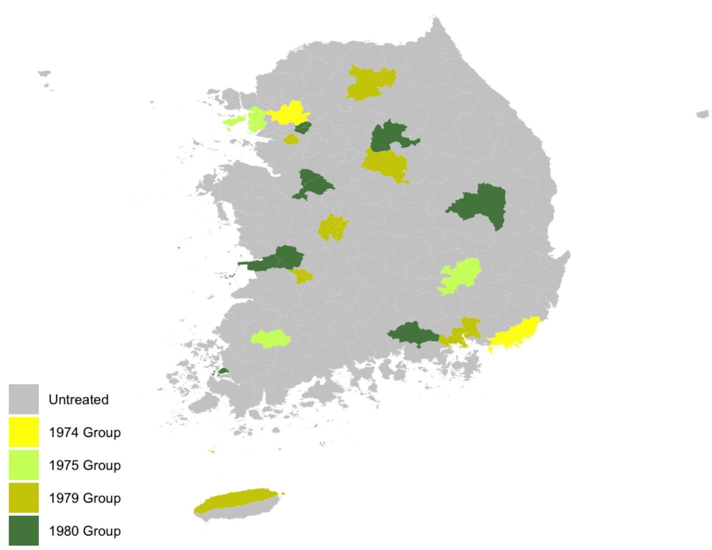
Table 1: Variation in Policy Adoption

Year	**City**
1974	Seoul, Busan
1975	Daegu(Gyeongbuk), Incheon(Gyeonggi), Gwangju(Jeonnam)
1979	Daejeon(Chungnam), Jeju, Suwon(Gyeonggi), Cheongju(Chungbuk), Jeonju(Jeonbuk) Masan(Gyungnam), Chuncheon(Gangwon)*
1980	Seongnam(Gyeonggi), Jinju(Gyungnam), Mokpo(Jeonnam)* Cheonan(Chungnam)*, Gunsan(Jeonbuk)*, Iksan(Jeonbuk)*, Wonju(Gangwon)* Andong(Gyeongbuk)*

Province is in parentheses. * Cities abolished the leveling policy between the late 1980s and early 1990s, which is after the end of my analysis sample.

Sources: Chosun Ilbo (1974a);Kyunghyang Shinmun (1974);Chosun Ilbo (1978); Chosun Ilbo (1980b)

Figure 1: Variation in Policy Across Regions



Sources: Chosun Ilbo (1974a);Kyunghyang Shinmun (1974);Chosun Ilbo (1978); Chosun Ilbo (1980b)

As for implementing the high school leveling policy, Seoul and Busan had a more strict geographic restriction on the high school application eligibility than the other cities did. Students who resided in Seoul or Busan could apply for high schools in their city, and students outside the cities were prevented from applying. Other cities that adopted the policy from 1975 onward accepted students outside the cities as long as they resided in the same province.

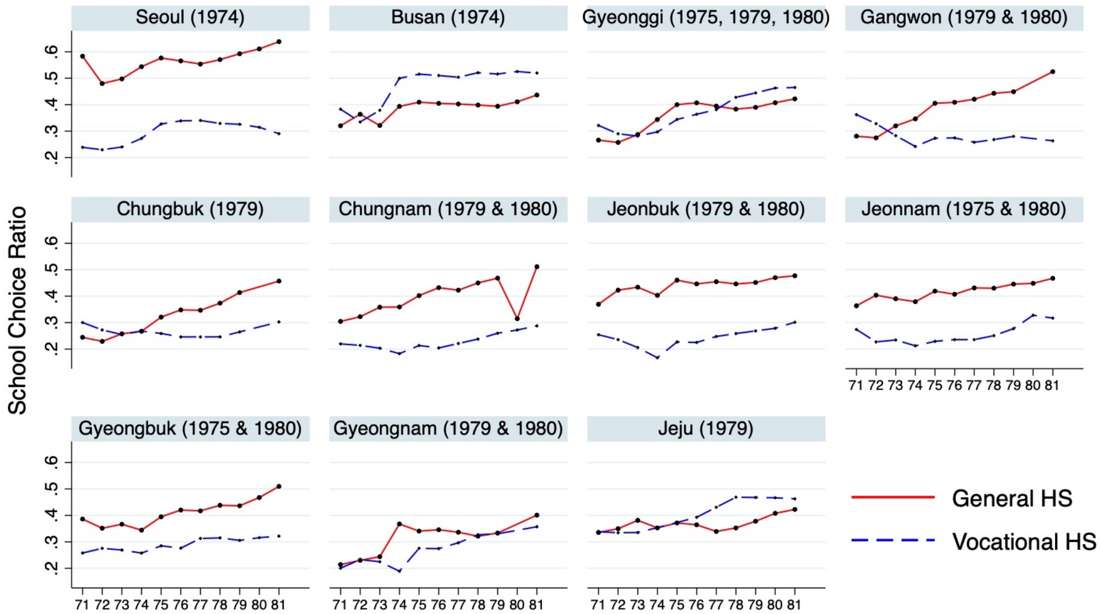
The geographic restriction in Seoul and Busan greatly alleviated competition for getting into high schools among students who lived in the two cities. Figure 2a shows that less than 50% of middle school graduates in Seoul went to general high schools in Seoul before 1974 while that proportion increased to 58% in 1975. Figure 2b shows that about 6% of middle school graduates in Gyeonggi province went to general high schools outside the province, but the proportion dropped to less than 1% since 1974. Given the geographical proximity of Gyeonggi province to Seoul, such change indicates that the high school leveling policy made it easier for students, particularly in Seoul and Busan, to go to high schools.

Although the other cities that adopted the policy from 1975 onward still accepted students outside the cities within the same province, they were also likely to experience decreased competition for high school entrance as students who did not want to be mixed with low-achieving peers would rather choose schools in an unaffected city within the same province. However, it is not possible to test this from available data as school choices were tracked at the province level, not at the city level.

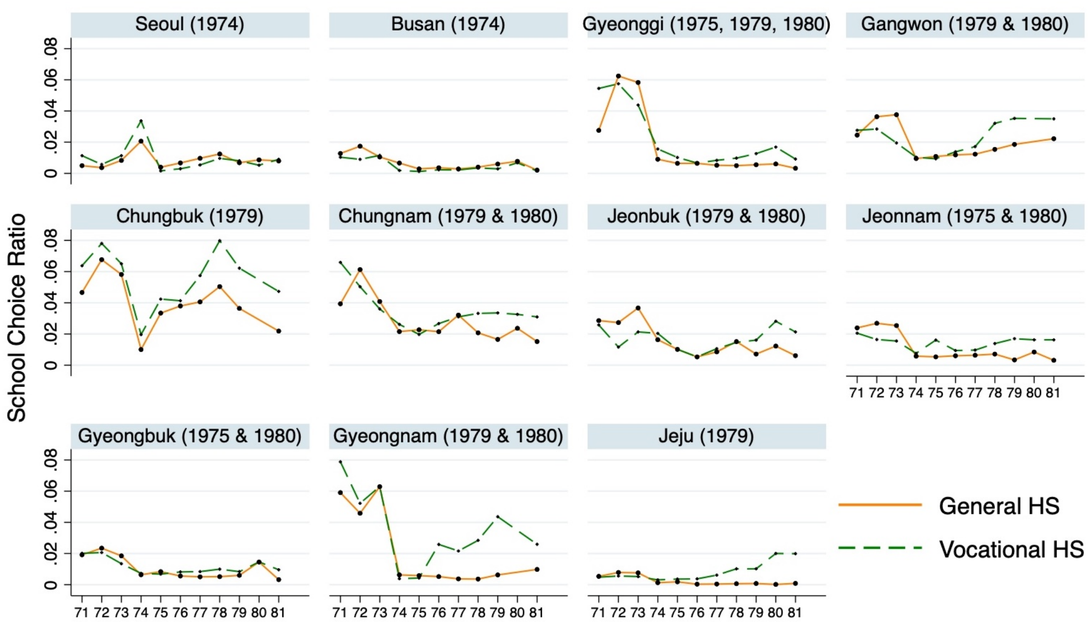
Despite the possible decrease in competition for general high school entrance in the affected cities, entrance into a general high school was still competitive, due to the higher demand for general high school education. Therefore, 3rd-grade middle school students took a unified high school entrance exam, and once they passed the cutoff, they were assigned to a general high school within their own school districts through a lottery system. If they applied for a vocational high school, they were still required to take the exam as the exam score was used to determine their admissions. In general, the cutoff scores for vocational high schools were lower than those for general high schools.

Figure 2: Trends in High School Choices by Province and School Type

(a) Within-Region School Choices



(b) Outside-Region School Choices



Notes: Information on school choices for Gangwon, Chungbuk, and Gyeongnam in 1980 is unavailable.
Sources: Statistical Yearbook of Education (1971;1972;1973;1974;1975;1976;1977;1978;1979;1981), Regional Statistical Yearbook of Education 1980 (Seoul; Busan; Gyeonggi; Gangwon; Chungbuk; Chungnam; Jeonbuk; Jeonnam; Gyeongbuk; Gyeongnam; Jeju)

To achieve policy success, it was necessary to reduce gaps in school facilities and the quality of teachers between prestigious and non-prestigious schools. The South Korean government required schools to invest in their facilities, fired or re-trained unqualified teachers, and moved some teachers from prestigious high schools to non-prestigious high schools (Jeong, 2013). As a result, high schools in cities with the policy had more uniform quality facilities and teachers over time than those without the policy. However, such improvement was at the expense of increased tuition fees because the Ministry of Education allowed high schools to use higher tuition to finance the investment. Figure 3 shows that tuition for public vocational/general high schools did not significantly differ across cities before the policy adoption. Upon policy adoption, however, the government raised public school tuition in affected cities to the level of private school tuition to finance school investment and to equalize tuition regardless of school types.[2]

To put such tuition increases in perspective, in 1973, monthly high school tuition in Seoul and Busan ranged from 1,080 Won for public vocational high schools to 2,320 Won for private high schools while the average monthly earnings in the mining and manufacturing industries were 22,708 Won (Mining and Manufacturing Survey, Mining and Manufacturing Survey). In 1974, monthly high school tuition in the two regions increased to 2,670 Won, regardless of high school types, while the average monthly earnings in the two relatively well-paying industries were 29,274 Won (Mining and Manufacturing Survey, Mining and Manufacturing Survey). Considering that the high school tuition in Seoul and Busin in 1974 was roughly 9% of the average monthly earnings in the mining and manufacturing industries, such tuition increases could have particularly been substantial for families with below the average earnings and/or multiple high school-age children.

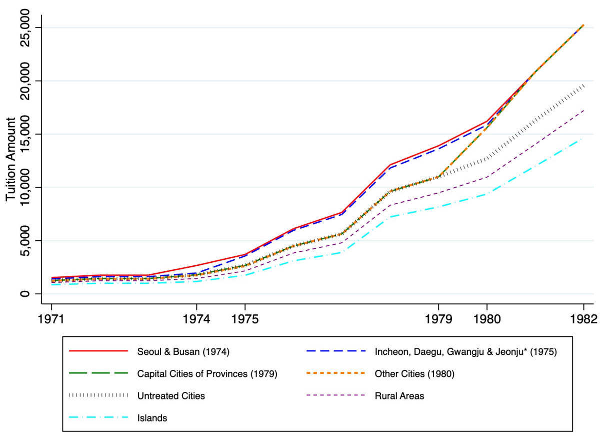
Although the high school leveling policy is expected to have increased high school entrance for middle school graduates in the affected cities, through the above-mentioned decrease in competition for general high school entrance, the tuition increases in the affected cities might have deterred some students from attending high school. In addition, the accompanying tuition increases were the most rapid for public vocational high schools as they had had the cheapest tuition before the policy adoption. Therefore, the steepest tuition increase and the ease of general high school entrance could cause more students to avoid vocational high schools in the affected cities.

While the government mainly focused on reducing gaps in school facilities and teachers between high

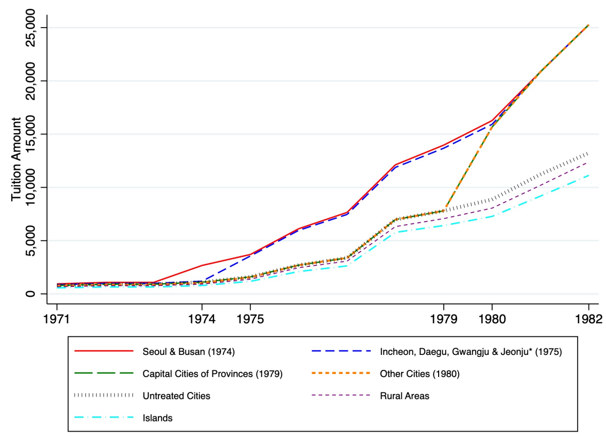
[2]Before 1974, tuition for private schools was more expensive than public schools. Although many private high schools were required to improve their poor facilities, they were not allowed to finance it through their tuition revenues, which increased their financial burden and thus opposition to the policy expansion in other cities.

Figure 3: Trends in Tuition by Province and School Type

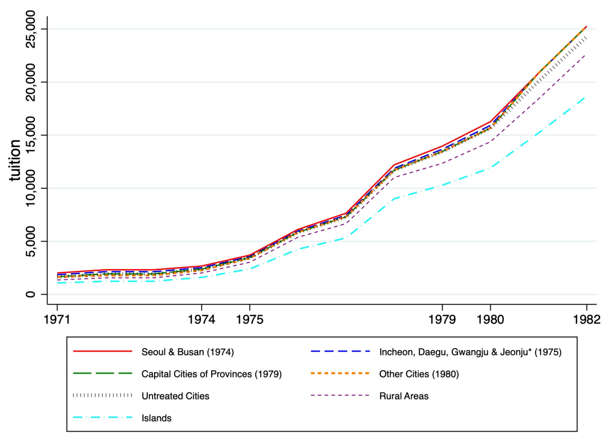
(a) Public General HS



(b) Public Vocational HS



(c) Private HS (General & Vocational)



Notes: Jeonju adopted the policy in 1979 while its tuition increased with the other three cities, Incheon, Daegu, and Gwangju, as they belonged to the same group according to the classification that the Ministry of Education used.
Sources: Dong-A Ilbo (1971);Maeil Economy (1972); Maeil Economy (1974);Chosun Ilbo (1974b); Dong-A Ilbo (1976);Kyunghyang Shinmun (1977a); Dong-A Ilbo (1978b);Kyunghyang Shinmun (1979); Chosun Ilbo (1980a);Kyunghyang Shinmun (1981); Kyunghyang Shinmun (1982)

schools, especially at the earlier stage of the new policy, another concern among teachers and parents was how to teach students with different academic abilities. Although the government officially forbade streaming students based on their grades, some schools did it informally by subjects, which was eventually allowed by the government later (Dong-A Ilbo, 1974; Kyunghyang Shinmun, 1977b; Dong-A Ilbo, 1978a). Therefore, if high school leveling worsened education effectiveness through heterogenous student composition within a class, the negative impact would have likely been mitigated over time.

3 Data

My empirical analysis draws on data from two main sources. First, I collect tuition information from digitized news articles published between 1970 and 1982. The Ministry of Education classified every administrative district in South Korea into six groups and determined tuition for public general/vocational and private high schools in each group. Whenever there was a change in tuition, newspapers announced it. I acquired relevant news articles through the Naver News Library website. The order of policy adoption is generally consistent with the order of the six groups, except that Jeonju adopted the policy in 1979 while the other three cities in the same group adopted it in 1975. In 1980, the Ministry of Education further distinguished groups of smaller cities as some cities in the group adopted the policy while others did not. Therefore, I construct the tuition dataset for eight groups from 1971 to 1982 and use the log-transformed tuition to estimate the policy impact on tuition.

I use the Korean Labor & Income Panel Study (KLIPS) for individual-level outcome variables. KLIPS is a longitudinal survey of the labor market and income activities of households and individuals residing in urban areas. It is an annual survey that started in 1998 and the most recent data available are from 2020. The initial sample size was 5,000 households when it began in 1998, but 1,415 households and 5,044 households were added in 2009 and 2018, respectively, due to sample attrition over time. The survey is conducted between April and September each year for every household member older than 15.

For analysis, I construct a sample of individuals who could have entered high schools between 1971 and 1982, from the dataset that spans from 1998 to 2020. I focus on the three outcome variables: high school entrance and type, college outcomes, and labor market earnings. First, the high school leveling policy definitely increased the probability of getting into a high school as students outside the affected cities, in principle, were not allowed to apply. However, some students might have been discouraged from going to a high school due to the increase in tuition, which was accompanied by the policy change. In addition, the policy adoption was likely to induce some low-achieving students, who would have chosen vocational high schools, to choose general high schools, due to the ease of competition for general high schools and the more rapid increase in vocational high school tuition. Therefore, I measure the policy impact on students attending high schools and choosing general or vocational schools by exploiting individuals' education history in KLIPS.

Second, I analyze the impact of the high school leveling policy on college outcomes: getting into any college including a 2-year college, getting into a four-year college, and getting into a top 50 prestigious college. High-achieving students prefer to go to a prestigious high school since they have more resources for college preparation, and the school culture and peer effect reinforce students' academic achievements. Therefore, the high school leveling policy could have undermined one's getting into a prestigious college. On the other hand, since many schools offered separate classes based on students' academic abilities, high school leveling might not have affected college outcomes in a significant way. On top of that, if the policy increased the overall education quality for high schools that were previously not recognized as prestigious, it could have at least improved one's getting into a college in general. When measuring the policy impact on getting into prestigious colleges, I define prestigious colleges as the top 50 colleges, following Ko (2011).

Lastly, I examine the policy impact on labor market earnings as students under such policy experienced an overall increase in school facilities and teachers, which in turn could positively affect their education and thus labor market outcomes in their later lives. Unlike the other outcome variables, labor market earnings are observed multiple times in the dataset, ranging from 1998 to 2020. Therefore, I first construct one's hourly wages by dividing the average monthly earnings by one's usual hours worked each year, then regress the log of the hourly wages on a dummy variable for sex, year-fixed effects, fixed effects for the current residence at the province level, tenure, and the squared term of tenure, to obtain the wage residuals. Then I compute one's average wage residual and use the log-transformed wage residuals as the outcome variable for labor market earnings. Individuals without at least one year of positive labor market earnings between 1998 and 2020 are dropped from the labor market earnings analysis.

4 Methodology

4.1 Differential-Timing Treatment Effect Proposed by Callaway and Sant'Anna (2021)

Goodman-Bacon (2021) finds that the treatment effect identified from a Two-Way Fixed Effects (TWFE) linear regression specification,

$$Y_{it} = \alpha_i + \gamma_t + \beta^{DD} D_{it} + X_{it}\delta + \varepsilon_{it}, \tag{1}$$

where i and t index unit and time, respectively, could be biased when there is variation in treatment timing across units and the treatment effect is heterogeneous over time. The TWFE estimate, $\hat{\beta}^{DD}$, is a weighted-average of $\hat{\beta}$ coefficients that are estimated from the canonical 2×2 DD estimators using combinations of early-treated, later-treated, and never-treated groups. The problems are that TWFE uses already-treated units as comparison groups for later-treated units, and that the weighting is affected by the number of units in a group and group treatment variance.

To address the above problems, Callaway et al. (2021) propose robust DD estimators that do not use already-treated units as comparison groups. The Callaway et al. (2021) estimator is non-parametrically point-identified by using either outcome regression, inverse probability weighting, or doubly robust estimands that combine both. While an outcome regression or an inverse probability weighting are only consistent when either the regression model or the propensity score are correctly specified, a doubly robust estimator is consistent when either of the two conditions is correctly specified. Therefore, I use a doubly robust estimator with not-yet-treated and never-treated units as the comparison group, which is of the following general form:

$$ATT(g,t) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}}{E\left[\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}\right]} \right) (Y_t - Y_{g-1} - m_{g,t}(X)) \right], \tag{2}$$

where D_t is a binary variable equal to one if a unit is treated in period t and equal to zero otherwise, G_g is a binary variable that is equal to one if a unit is first treated in period g and equal to zero otherwise, and $p_{g,t}(X) = P(G_g = 1|X, G_g + (1 - D_t)(1 - G_g) = 1)$, denotes the generalized propensity score that indicates the probability of being first treated at time g, conditional on pre-treatment covariates X and the treatment status by time t. The population outcome regression for the not-yet-treated group by time t is $m_{g,t}(X) = E[Y_t - Y_{g-1}|X, D_t = 0, G_g = 0]$, and it captures the mean of the outcome change for each $X = x$ for the comparison group. For asymptotically valid inference, it uses a multiplier bootstrap procedures.

4.2 Identification Strategy

I identify the impact of the high school leveling policy on high school attendance and labor market earnings by using a difference-in-differences estimation with staggered policy adoption, which was proposed by Callaway et al. (2021). I first identify the group-time average treatment effect for the treated (ATT), and aggregate them to form overall policy impacts. I define five groups based on the year when the policy was first implemented: 1974, 1975, 1979, 1980, and untreated as of 1980. The time component of my group-time ATTs is defined as the years of the high school entrance. While my sample consists of individuals who went to a high school between 1971 and 1982, the Callaway et al. (2021) estimator requires only the base year as the pre-treatment period when calculating the group-time ATTs. Therefore, I estimate nine ATTs for the 1974 group, eight ATTs for the 1975 group, four ATTs for the 1979 group, and three ATTs for the 1980 group.

Since the high school leveling policy was implemented at the metropolis/city level, I assign the treatment status based on the region one resided when she was 14 years old, which is usually a year ahead of the high school entrance, and use not-yet-treated groups and untreated groups as the comparison group to estimate the group-time ATTs. When it comes to defining the treatment status at the metropolis/city level, there is a concern that families might have changed their residence when students were 14 or younger, as a response to the policy change. Such migration response could be particularly an issue for Seoul and Busan as students in other cities were prevented from applying for high schools in Seoul and Busan, which had better high school quality overall. For example, Kyunghyang Shinmun (1975) reveals that more than 10,000 middle/high school students transferred to schools in Seoul every year after the policy change.

On the other hand, bias from migration response is not likely to be problematic for other metropolises and cities that adopted the policy from 1975 onward because students in unaffected cities were still allowed to apply for high schools in affected cities within the same province. To deal with the possible migration response, I conduct a robustness check where I assign the treatment status based on one's birthplace at the metropolis/province level and see if there is a significant difference in the policy impact for Seoul and Busan (the 1974 group) between the use of residence at age 14 and birthplace.

Although my identification strategy could potentially have some bias, it still seems more appropriate than other identification strategies used in the previous studies. Nam and Choi (2004) define the treatment status based on the location of the high school one attended, but that is potentially another outcome variable that could be affected by the policy change. On the other hand, Kang et al. (2007) define treatment status based on high school types such that general high school graduates are the treated group and vocational high school graduates are the comparison group. However, it would be a valid identification strategy only if every student in the treated cities had chosen her school type, either general or vocational, regardless of the policy change. However, given the ease of competition for general high school entrance and the more rapid increase in tuition for vocational high schools, the choice of school type was likely to be affected by the policy change.

Table 2 shows that the distribution of covariates that are likely to affect the outcome variables of interest differs across regions. Moreover, the policy was not adopted randomly across regions, rather, it was adopted in bigger cities earlier. Therefore, my identifying assumption is that conditioning on the pre-treatment covariates, changes in outcome variables relative to the base year for the treated group would have been, on average, the same as those for the not-yet-treated groups. For the identifying assumption, I control for dummy variables for sex, whether her family was financially poor/on average at 14, whether her father was the main breadwinner at 14, and whether her father was a high school graduate/college graduate.

While I rely on the conditional parallel trends to estimate the policy impacts on individual-level outcome variables, I assume unconditional parallel trends when estimating the policy impact on tuition because tuition was determined at the regional group level by the Ministry of Education, and that makes the possibility of other covariates differently affecting tuition less likely.

5 Results

The estimation results for high school tuition show that the high school leveling policy increased tuition for public high schools while decreasing tuition for private high schools. To be specific, Figure 4 shows that tuition for public general high schools increased especially in the earlier period of the policy implementation. Figure 5 illustrates that tuition for public vocational high schools increased the most after the policy change, and unlike tuition for public general high schools, tuition increases were persistent. Figure 6 shows that tuition for private high schools decreased in the earlier cohorts after the policy adoption, while it increased slightly in the later cohorts. Table 3 shows that aggregated policy impacts on tuition increases were strongest for public vocational high schools and the 1980 group.

The estimation results for school outcomes and labor market earnings indicate that there are heterogenous policy impacts across groups and over time. Figure 7 shows that the high school leveling policy did not significantly affect overall high school attendance outcomes in the treated cities, except that there was a positive impact for the 1974 cohort of the 1974 group, a negative impact for the 1980 and 1982 cohorts of the 1975 group, a negative impact for the 1979, 1981, and 1982 cohorts of the 1979 group. Figure 8 reflects a positive impact on attending vocational high schools for the 1974 cohort of the 1974 group, a negative impact for the 1978 and 1982 cohorts of the 1975 group, and a positive impact for the 1979, 1980, and 1981 cohort of the 1979 group.

Figure 9 shows that the policy increased college attendance substantially for the 1974 group, while generally decreasing it for the 1979 group. Figure 10 shows that the policy had positive impacts on attending 4-year colleges for the 1974 and 1975 groups while having negative impacts for the 1979 group and no impact for the 1980 group. The negative policy impact for the 1979 group is consistent with the negative impact on high school attendance and the positive impact on vocational high school attendance as college entrance requires a high school graduation and vocational high school graduates usually start working after graduation. However, Figure 11 shows that high school leveling did not have a statistically significant impact on prestigious college entrance for any group, except for a negative impact for the 1980 cohort of the 1975 group, which is consistent with the decrease in high school attendance in that cohort.

Figure 12 shows that high school leveling had a negative impact on average hourly wages for some cohorts of the 1974 and 1979 groups while having a positive impact for some cohorts of the 1975 group. Comparing heterogenous impacts on vocational high school attendance and overall college attendance by groups over time implies that choosing general high schools and going to any college were likely to increase the average hourly wages for the 1976 and 1978 cohorts of the 1975 group.

Table 4 summarizes the group-time ATTs. Overall, the high school leveling policy did not have a statistically significant impact on high school attendance, vocational high school attendance, prestigious college attendance, and average hourly wages. However, it increased students' all college and 4-year college attendance in the treated cities by 15.13 percentage points and 15.35 percentage points, respectively. The group-specific aggregated ATTs show that high school leveling was the most beneficial for the 1975 group as it increased college outcomes and average hourly wages while it was the least beneficial for the 1979 group as it decreased high school and college attendance.

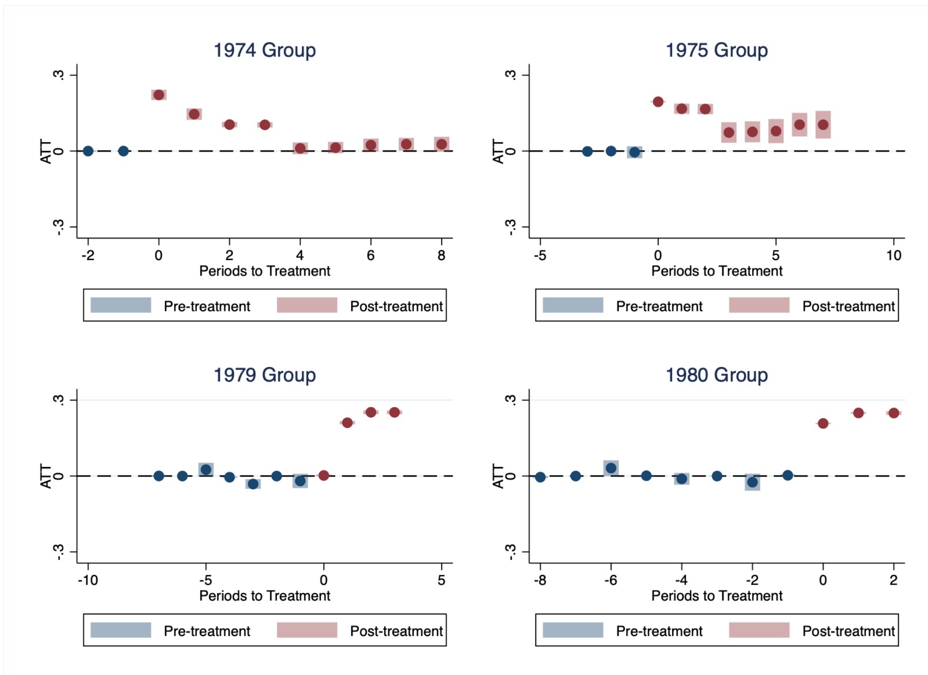
Table 2: Summary Statistics

A. Treated vs Untreated	Treated Cities	Untreated Cities	Diff.	P-val on Diff.
Female	0.4977	0.5578	0.0602	0.0257
Financially poor at age 14	0.4096	0.5395	0.1299	0.0000
Financially just okay at age 14	0.4404	0.3648	-0.0756	0.0072
Mother as main breadwinner at age 14	0.0902	0.0596	0.0143	0.0323
Father had high school education	0.1319	0.0371	-0.0948	0.0000
Father had college education	0.1128	0.0289	-0.0835	0.0000
# of Obs (Individuals)	531	498		
# of Obs (Cities)	20	122		

B. First-Treated vs Later-Treated	1974 Group	1975 Group	Diff.	P-val on Diff.
Female	0.4537	0.4888	-0.0350	0.4499
Financially poor at age 14	0.3569	0.3864	-0.0295	0.5659
Financially just okay at age 14	0.4498	0.4848	-0.0350	0.5096
Mother as main breadwinner at age 14	0.1015	0.0751	0.0264	0.3337
Father had high school education	0.1640	0.1228	0.0412	0.2239
Father had college education	0.1514	0.0994	0.0520	0.1072
	1974 Group	1979 Group	Diff.	P-val on Diff.
Female	0.4537	0.6081	-0.1544	0.0161
Financially poor at age 14	0.3569	0.5077	-0.1508	0.0250
Financially just okay at age 14	0.4498	0.3692	0.0806	0.2407
Mother as main breadwinner at age 14	0.1015	0.0959	0.0056	0.8850
Father had high school education	0.1640	0.0959	0.0681	0.1437
Father had college education	0.1514	0.0411	0.1103	0.0116
	1974 Group	1980 Group	Diff.	P-val on Diff.
Female	0.4537	0.6481	-0.1944	0.0079
Financially poor at age 14	0.3569	0.6111	-0.2542	0.0005
Financially just okay at age 14	0.4498	0.3704	0.0794	0.2842
Mother as main breadwinner at age 14	0.1015	0.0600	0.0415	0.3546
Father had high school education	0.1640	0.0189	0.1452	0.0051
Father had college education	0.1514	0.0189	0.1326	0.0083
	1974 Group	1975 Group	1979 Group	1980 Group
# of Obs (Individuals)	258	135	60	45
# of Obs (Cities)	2	3	7	8

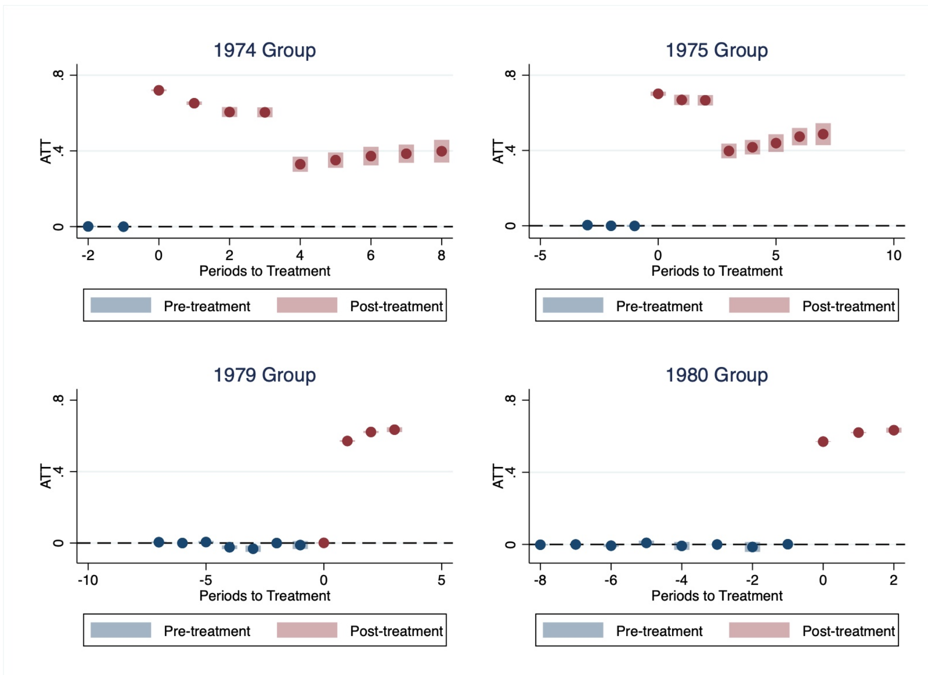
Notes: Panel A provides summary statistics for individuals who could potentially have entered high schools between 1971 and 1973 in cities that adopted the high school leveling policy between 1974 and 1980 (treated) and other areas that did not adopt the policy as of 1980 (untreated). Panel B provides summary statistics by cities that adopted the policy.

Figure 4: High School Leveling Impact on Tuition for Public General High Schools



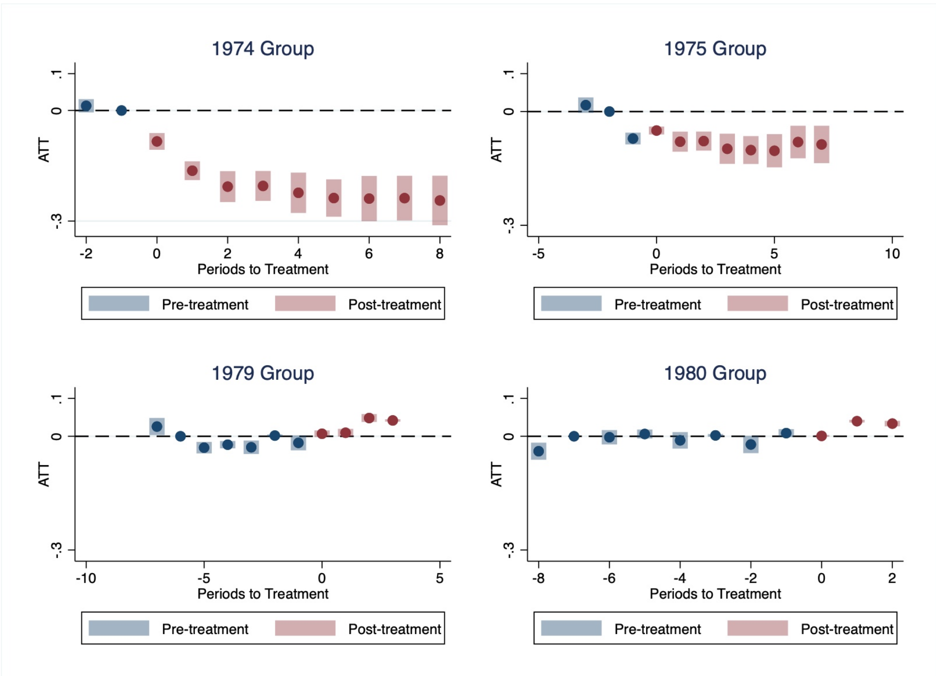
Notes: Standard errors are clustered at the regional group level. Regression adjustment is used for the outcome model. Tuition is log-transformed.

Figure 5: High School Leveling Impact on Tuition for Public Vocational High Schools



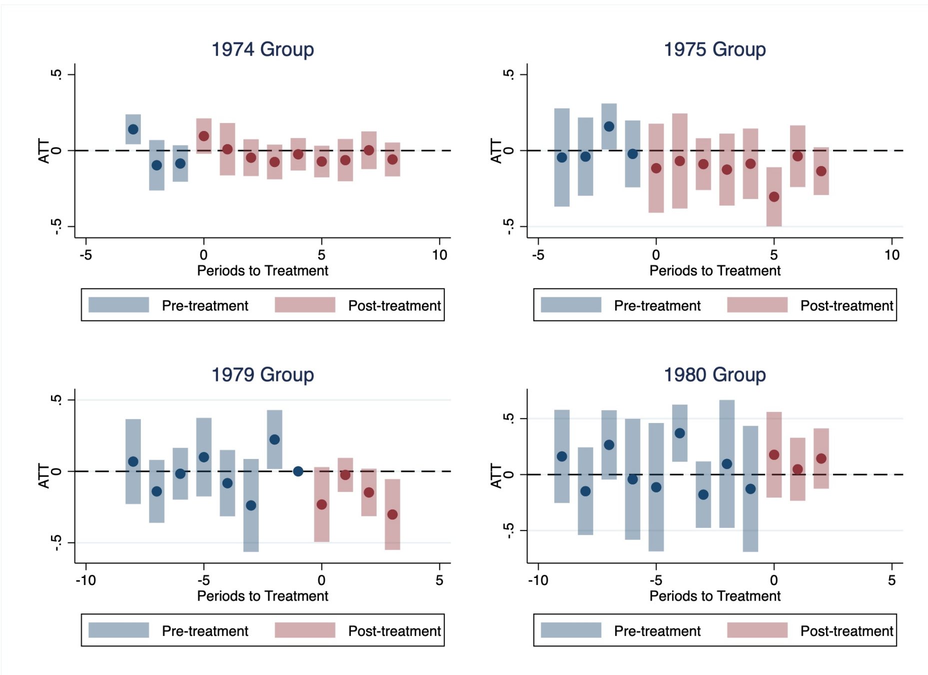
Notes: Standard errors are clustered at the regional group level. Regression adjustment is used for the outcome model. Tuition is log-transformed.

Figure 6: High School Leveling Impact on Tuition for Private High Schools



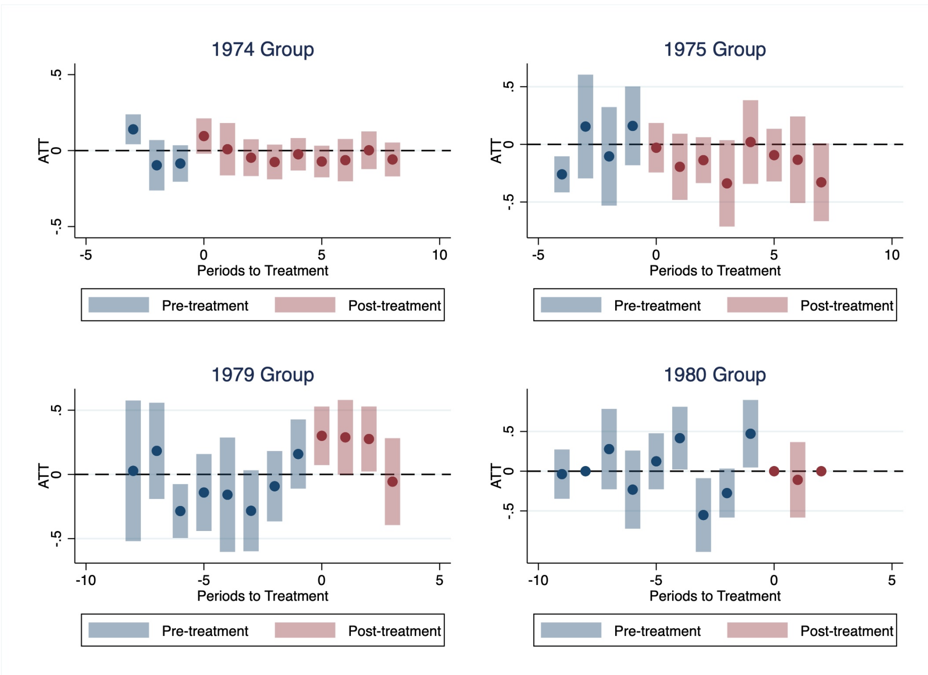
Notes: Standard errors are clustered at the regional group level. Regression adjustment is used for the outcome model. Tuition is log-transformed.

Figure 7: High School Leveling Impact on High School Attendance



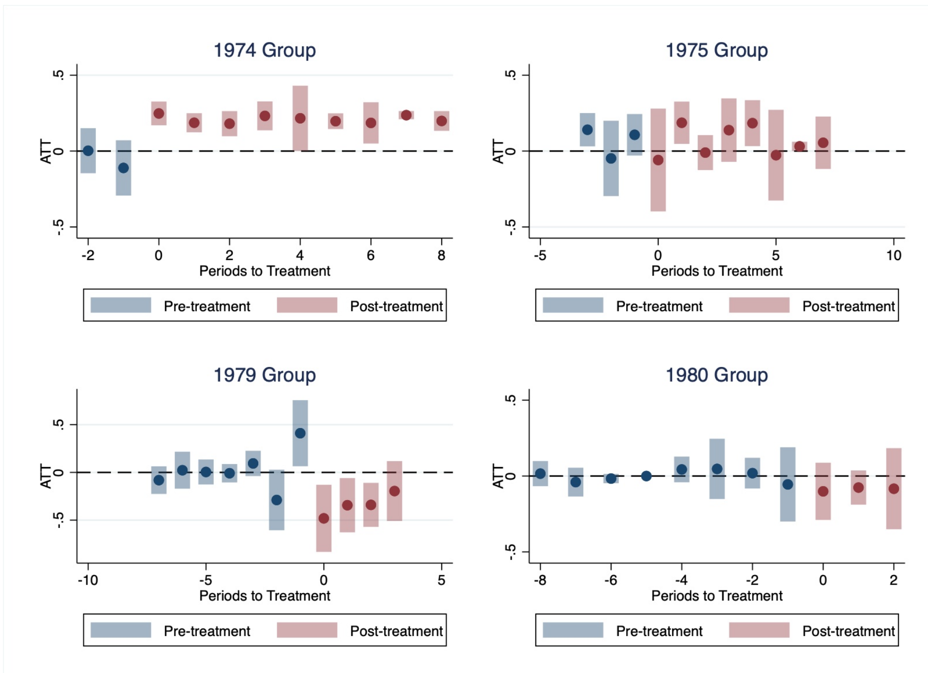
Notes: Standard errors are clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model.

Figure 8: High School Leveling Impact on Vocational High School Attendance



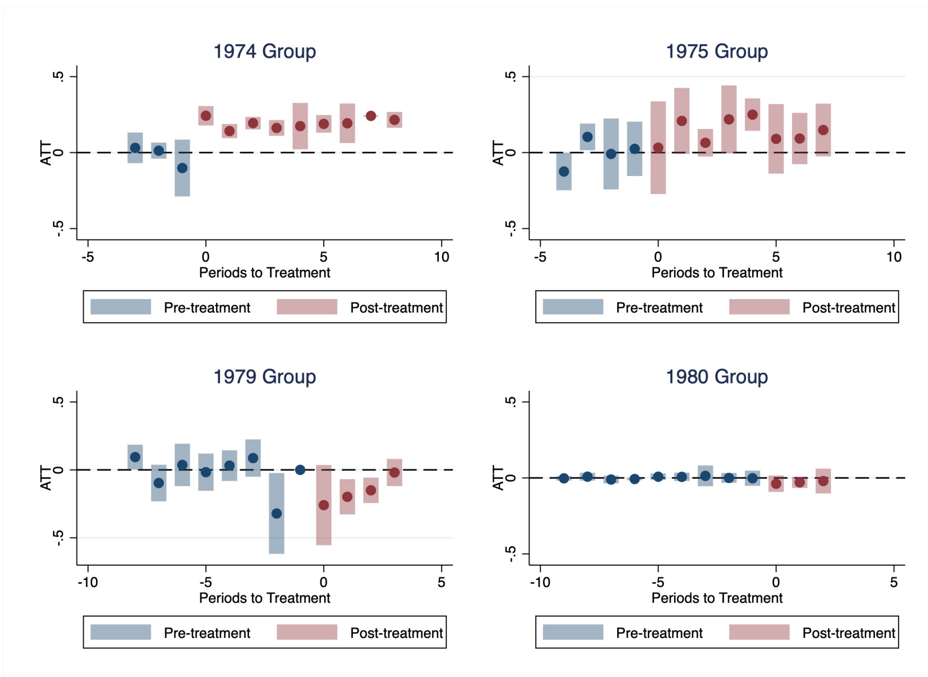
Notes: Standard errors are clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model.

Figure 9: High School Leveling Impact on Overall College Attendance



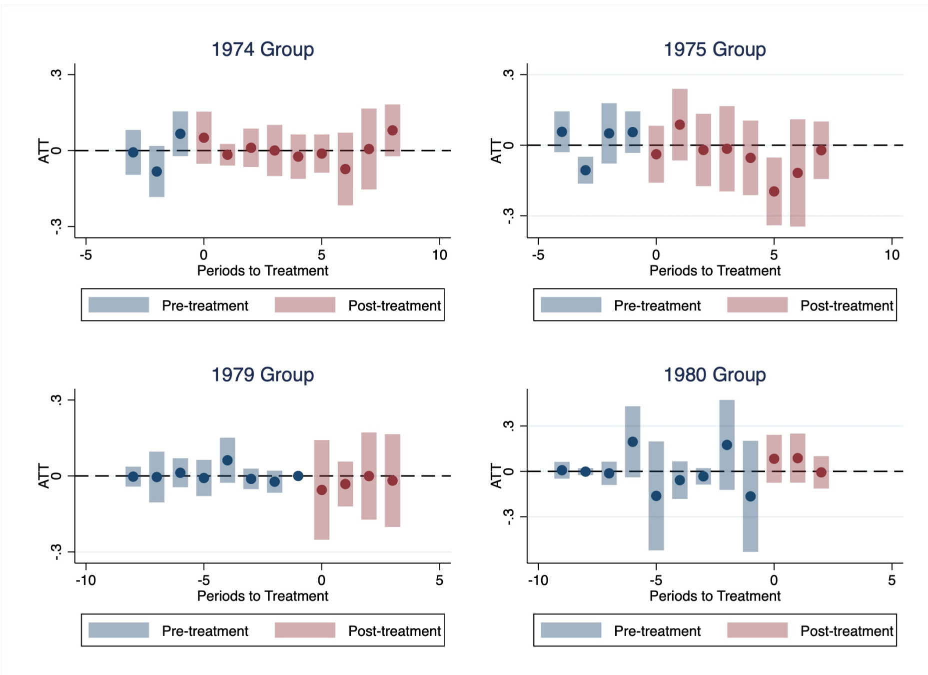
Notes: Standard errors are clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model.

Figure 10: High School Leveling Impact on 4-Year College Attendance



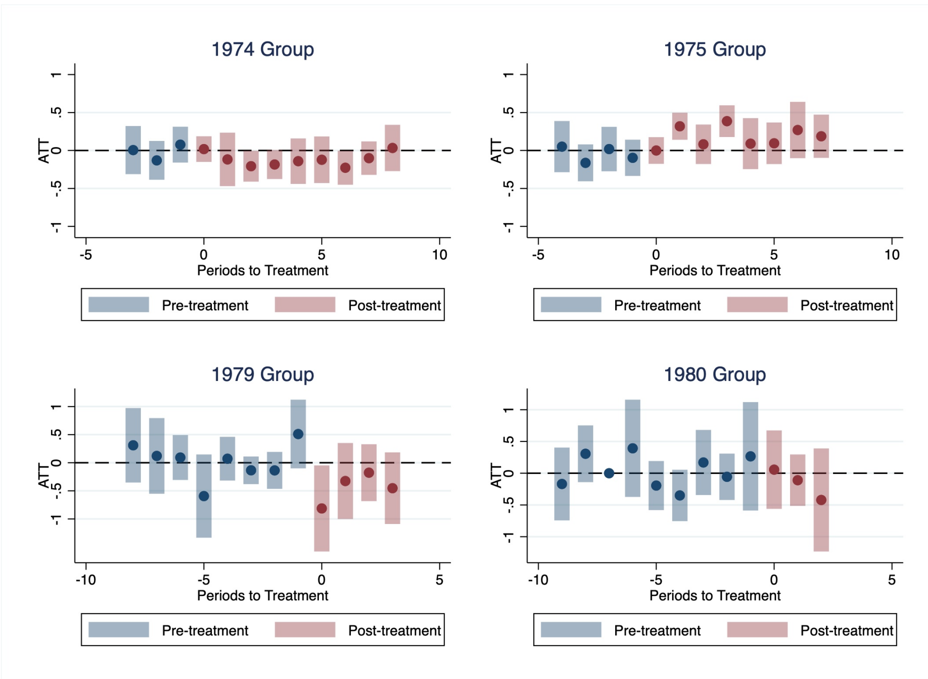
Notes: Standard errors are clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model.

Figure 11: High School Leveling Impact on Top 50 College Attendance



Notes: Standard errors are clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model.

Figure 12: High School Leveling Impact on Average Hourly Wages



Notes: Standard errors are clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model. Hourly wages are log-transformed. For wage analysis, individuals without positive wages in any of years from 1998 to 2020 are excluded.

Table 3: Aggregated Group-Time ATTs for Tuition

	Tuition for Public General HS		Tuition for Public Vocational HS		Tuition for Private HS	
	Mean	ATT	Mean	ATT	Mean	ATT
All Groups	1,492	0.1279***	920	0.5135***	1,962	-0.0974*
	(171)	(0.0264)	(101)	(0.0210)	(230)	(0.0509)
1974 Group	1,683	0.0755***	1,033	0.4910***	2,047	-0.2045***
	(133)	(0.0068)	(80)	(0.0162)	(162)	(0.0224)
1975 Group	1,550	0.1207***	947	0.5314***	1,843	-0.0847***
	(121)	(0.0166)	(75)	(0.0176)	(150)	(0.0165)
1979 Group	1,387	0.1793***	870	0.4572***	1,843	0.0265***
	(110)	(0.0027)	(69)	(0.0025)	(150)	(0.0041)
1980 Group	1,350	0.2354***	833	0.6081***	1,740	0.0248***
	(104)	(0.0015)	(64)	(0.0025)	(87)	(0.0017)
Obs	96		96		96	

Notes: For each outcome variable, the mean column reports the mean value before 1974, with the standard deviation in parentheses. The ATT columns report estimated ATTs with standard errors in parentheses clustered at the regional group level. Regression adjustment is used for the outcome model. Tuition is log-transformed.

Table 4: Aggregated Group-Time ATTs

	HS Attendance		Vocational HS		Any College		4-Year College		Top 50 College		Hourly Wages	
	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT
All Groups	0.6610	-0.0518	0.3837	0.0767	0.6285	0.1513***	0.6285	0.1535***	0.0341	-0.0087	1.0222	-0.0691
	(0.4739)	(0.0845)	(0.4872)	(0.0401)	(0.4218)	(0.0379)	(0.3694)	(0.0378)	(0.1818)	(0.0246)	(0.8488)	(0.0893)
1974 Group	0.7767	-0.0271	0.4106	0.1471	0.2713	0.1920***	0.1899	0.1948***	0.0465	0.0023	1.0752	-0.1227
	(0.4175)	(0.0422)	(0.4936)	(0.1077)	(0.4455)	(0.0247)	(0.3930)	(0.0239)	(0.2110)	(0.0292)	(0.9498)	(0.0976)
1975 Group	0.6887	-0.1213	0.2899	-0.1577	0.2667	0.1351**	0.1704	0.1379**	0.0370	-0.0476	0.8927	0.1566**
	(0.4652)	(0.0971)	(0.4570)	(0.1327)	(0.4439)	(0.0674)	(0.3774)	(0.0610)	(0.1896)	(0.0602)	(0.7043)	(0.0761)
1979 Group	0.5357	-0.1639***	0.5357	0.2067**	0.1000	-0.1527**	0.1000	-0.1536**	0.0000	-0.0244	1.1604	-0.3735
	(0.5032)	(0.0562)	(0.5079)	(0.0821)	(0.3025)	(0.0664)	(0.3025)	(0.0661)	(0.0000)	(0.0657)	(0.8247)	(0.2784)
1980 Group	0.2222	0.1174	0.2000	-0.1087	0.0667	-0.0184	0.0667	-0.0292	0.0000	0.0570	0.8740	-0.1905
	(0.4204)	(0.1339)	(0.4216)	(0.2424)	(0.2523)	(0.0267)	(0.2523)	(0.0275)	(0.0000)	(0.0619)	(0.5480)	(0.2622)
Obs	943	3,868	487	2,606	1,029	3,688	1,029	3,688	1,029	3,688	330	2,026

Notes: For each outcome variable, the mean column reports the mean value before 1974, with the standard deviation in parentheses. The ATT columns report estimated ATTs with standard errors in parentheses clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model. Hourly wages are log-transformed. For wage analysis, individuals without positive wages in any of years from 1998 to 2020 are excluded.

6 Robustness Checks

As a robustness check, I conduct another set of regressions where I limit untreated cities to those most similar to the treated cities based on characteristics in 1972 and 1973.[3]I use k-means clustering based on the control variables and choose 18 cities from 122 untreated cities. Table 5 shows generally similar results to 4. However, pre-trend tests indicate that vocational high school attendance has a different pre-trend.

If families changed their residence when students were 14 or younger as a policy response, that will create bias as the treatment status is affected by such endogenous migration response. To deal with the possible migration response between unaffected and affected cities, particularly from smaller cities to Seoul and Busan, I estimate the policy impact again by defining treatment status based on one's birthplace. This is because KLIPS provides retrospective residence information on birthplace and residence at age 14, and individuals in my sample were all born before the first policy roll-out in 1974.

While the information on residence at age 14 is available at the city level, the information on the birthplace is only available at the metropolise/province level. With this information, I can only distinguish the metropolises that adopted the policy in 1974 and 1975. Therefore, I estimate the policy impact for the 1974 and 1975 groups by using the sample from 1971 to 1978 with the 1979, 1980, and untreated groups as the comparison group.

Table 6 shows that the signs of the aggregated effects remain unchanged. The policy impacts on all college and 4-year college attendance are statistically significant in the main analysis, but they lose statistical significance here. In addition, unlike the main analysis, the policy impacts on vocational high school and prestigious college attendance are now statistically significant. The notable differences from the main analysis are in the college outcomes and hourly wages for the 1975 group. First, the main analysis finds that the probability of going to all college and 4-year colleges increases by 13.51 and 13.79 percentage points, respectively, for the 1975 group. But in this robustness exercise, the policy impacts on the same outcome variables are estimated to be negative. Second, the wage effect for the 1975 group becomes negative while that is positive in the main analysis.

When it comes to defining the treatment status at the metropolis/city level, there is a concern that estimates are potentially contaminated. This is mainly because some high-achieving students in smaller

[3]For Seoul, the most appropriate comparison group is be Gyeonggi and Incheon, based on geographic proximity. When estimating the policy impacts for Seoul, by using the sample restricted to Seoul, Gyeonggi, and Incheon (Gyeonggi and Incheon belonged to the same province until June 1981), I find that there was an increase in top college attendance by 5.53 percentage points, which is statistically significant at 1% level. However, this estimate has potentially larger contamination, as many students from Gyeonggi and Incheon went to high schools in Seoul before the policy change.

cities or rural areas used to attend prestigious high schools in bigger cities before 1974. In this case, students who lived in untreated regions were actually affected by the leveling policy in other cities, but I assign them to the comparison group. However, the number of prestigious high schools was very small at the time.[4] Even if the majority of the top achievers from smaller cities or rural areas would have been affected by the policy change, contamination is likely to be small.

To further investigate the potential contamination issue, I run a set of regressions while excluding the never-treated group from the comparison group. In general, students in the untreated regions went to high schools in treated cities because those cities provided better education. Given that the order of the leveling policy adoption is consistent with the overall education quality, students in the eventually treated cities would have had less incentive to migrate to the other city, compared to those in untreated regions. Kang et al. (2007) also shows that cities that belong to either the 1974 or 1975 group were experienced positive net migration. Therefore, it is expected that potential contamination would be minimized if those who lived in untreated regions were excluded from the sample.

Table 7 presents the policy impacts estimated from the sample from 1971 to 1978 with the eventually-treated groups as the comparison group. The aggregated estimates for the college outcomes change little while the aggregated estimate for the vocational high school attendance is larger in magnitude and more statistically significant than in the main analysis. However, the policy impacts on college outcomes for the 1975 sample lose statistical significance while the wage effect becomes larger in magnitude. Overall, these results show that my analysis is robust to the potential contamination issue.

[4] For example, ten high schools in Seoul were recognized as prestigious among 94 high schools. According to the Statistical Yearbook of Education for 1973, the nationwide high school freshmen quota in 1973 was 160,090. If I assume that the quota for each prestigious school in Seoul was 500 as such information is not available, only the top 3 percent of students could get into those schools.

Table 5: Aggregated Group-Time ATTs (Selective Untreated Cities)

	HS Attendance		Vocational HS		Any College		4-Year College		Top 50 College		Hourly Wages	
	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT
All Groups	0.6610	0.0191	0.3837	-0.0194	0.6285	0.1332**	0.6285	0.1373**	0.0341	0.0282	1.0222	0.0477
	(0,4739)	(0.0409)	(0.4872)	(0.1101)	(0.4218)	(0.0592)	(0.3964)	(0.0612)	(0.1818)	(0.0451)	(0.8488)	(0.0855)
1974 Group	0.7767	0.0271	0.4106	0.0342	0.2713	0.2092***	0.1899	0.2170***	0.0465	0.0517	1.0752	0.0454
	(0.4175)	(0.0425)	(0.4936)	(0.1591)	(0.4455)	(0.0303)	(0.3930)	(0.0304)	(0.2110)	(0.0573)	(0.9498)	(0.1072)
1975 Group	0.6887	-0.0331	0.2899	-0.2415*	0.2667	0.0606	0.1704	0.0693	0.0370	-0.0360	0.8927	0.2113*
	(0.4652)	(0.1062)	(0.4570)	(0.1386)	(0.4439)	(0.0741)	(0.3774)	(0.0744)	(0.1896)	(0.0722)	(0.7043)	(0.1117)
1979 Group	0.5357	-0.0415	0.5357	0.0605	0.1000	-0.3332***	0.1000	-0.3667***	0.0000	0.0051	1.1604	-0.4111
	(0.5032)	(0.0811)	(0.5079)	(0.1282)	(0.3025)	(0.1212)	(0.3025)	(0.1230)	(0.0000)	(0.0901)	(0.8247)	(0.2777)
1980 Group	0.2222	0.2927*	0.2000	0.2610	0.0667	-0.0860	0.0667	-0.1214	0.0000	0.0258	0.8740	-0.5252*
	(0.4204)	(0.1765)	(0.4216)	(0.2208)	(0.2523)	(0.0924)	(0.2523)	(0.0913)	(0.0000)	(0.0386)	(0.5480)	(0.3055)
Obs	552	2,079	320	1,553	637	2,199	637	2,199	637	2,199	199	1,300

Notes: For each outcome variable, the mean column reports the mean value before 1974, with the standard deviation in parentheses. The ATT columns report estimated ATTs with standard errors in parentheses clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model. Hourly wages are log-transformed. For wage analysis, individuals without positive wages in any of years from 1998 to 2020 are excluded.

Table 6: Aggregated Group-Time ATTs (Treatment Status Based on Birthplaces)

	HS Attendance		Vocational HS		Any College		4-Year College		Top 50 College		Hourly Wages	
	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT
All Groups	0.7261	-0.0355	0.3376	0.1614*	0.6316	0.0018	0.6316	0.0046	0.0632	-0.0350*	0.9095	-0.0715
	(0.4469)	(0.0285)	(0.4744)	(0.0905)	(0.4832)	(0.0431)	(0.4832)	(0.0430)	(0.2437)	(0.0194)	(0.7640)	(0.0550)
1974 Group	0.7569	-0.0532**	0.3462	0.2363*	0.6333	0.0860***	0.6333	0.0876***	0.0556	-0.0470	0.9473	0.0095
	(0.4304)	(0.0223)	(0.4780)	(0.1230)	(0.4832)	(0.0276)	(0.4832)	(0.0278)	(0.2297)	(0.0323)	(0.8631)	(0.0453)
1975 Group	0.6818	0.0064	0.2927	-0.0163	0.5529	-0.0672**	0.5529	-0.0634*	0.0353	-0.0251	0.8427	-0.1455**
	(0.4693)	(0.0624)	(0.4606)	(0.0463)	(0.5001)	(0.0331)	(0.5001)	(0.0336)	(0.1856)	(0.0173)	(0.6216)	(0.0740)
Obs	943	3,564	487	2,490	1,029	2,445	1,029	2,445	1,029	2,445	330	1,305

Notes: For each outcome variable, the mean column reports the mean value before 1974, with the standard deviation in parentheses. The ATT columns report estimated ATTs with standard errors in parentheses clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model. Hourly wages are log-transformed. For wage analysis, individuals without positive wages in any of years from 1998 to 2020 are excluded.

Table 7: Aggregated Group-Time ATTs (Excluding Never-Treated Group)

	HS Attendance		Vocational HS		Any College		4-Year College		Top 50 College		Hourly Wages	
	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT	Mean	ATT
All Groups	0.7468 (0.4355)	0.0341 (0.0592)	0.3727 (0.4846)	0.2003* (0.1114)	0.5649 (0.4444)	1649** (0.0838)	0.5649 (0.3873)	0.1635** (0.0834)	0.0433 (0.2037)	-0.0031 (0.0201)	1.0148 (0.8777)	0.1537 (0.1199)
1974 Group	0.7767 (0.4175)	0.0346 (0.0696)	0.4106 (0.4936)	0.2464* (0.1294)	0.5736 (0.4455)	0.1878** (0.0935)	0.5736 (0.3930)	0.1860** (0.0932)	0.0465 (0.2110)	-0.0104 (0.0193)	1.0752 (0.9498)	0.1147 (0.1473)
1975 Group	0.6887 (0.4652)	0.0323 (0.1295)	0.2899 (0.4570)	0.0498 (0.1493)	0.5481 (0.4439)	0.0864 (0.0670)	0.5481 (0.3774)	0.0864 (0.0670)	0.0370 (0.1896)	0.0218 (0.0817)	0.8927 (0.7043)	0.2875*** (0.0912)
Obs	413	1,034	258	758	498	1,108	498	1,108	498	1,108	167	683

Notes: For each outcome variable, the mean column reports the mean value before 1974, with the standard deviation in parentheses. The ATT columns report estimated ATTs with standard errors in parentheses clustered at the city level. Least squares regression is used for the outcome model, and inverse probability weighting is used for the treatment model. Hourly wages are log-transformed. For wage analysis, individuals without positive wages in any of years from 1998 to 2020 are excluded.

7 Conclusion

South Korea first introduced the high school leveling policy in its major cities from 1974 to 1980 in an attempt to handle hyper-competition among middle school students toward prestigious high schools, and to reduce educational gaps across high schools. Although previous studies (Nam and Choi, 2004; Kang et al., 2007) examine the policy effects on labor market earnings, given the fact that the leveling policy was rolled-out at different times across cities and that cities likely had heterogeneous policy effects due to pre-existing difference in education environment and policy being updated over time, the recent methodological development suggests that it would benefit from a re-evaluation with a new difference-in-differences estimation method proposed by Callaway et al. (2021).

When it comes to defining the treatment and comparison groups, I address that Nam and Choi (2004) and Kang et al. (2007) use either high school location or high school type, which are outcome variables that were affected by the policy change. Instead, I use the information of city-level residence at age 14 to define the treatment status. In addition, I investigate the policy effect on high school tuition, high school attendance, and the types of high school, as mechanisms through which students' school outcomes and labor market earnings in their later lives could be affected. I find that the high school leveling policy increased public high school tuition by 12.79 to 51.35 percent, while decreasing private high school tuition by 9.74 percent. On top of that, the leveling policy had, on average, negative impacts on high school attendance, getting into a prestigious college, and hourly wages, although they are not statistically significant. I also find that the policy had, on average, positive impacts on vocational high school attendance and getting into any college and 4-year colleges and that the effects on the two college outcomes are statistically significant at the 1% significance level.

To address concerns on potential contamination and endogenous migration response, I conduct robustness checks by restricting the comparison group to the eventually-treated cities and using information of province-level birthplace. The results show that the signs on the policy effects at the aggregated level do not change although there are some changes in the statistical significance. Overall, my analysis suggests that the high school leveling policy in the 1970s improved general college attendance outcomes while not affecting prestigious college entrance outcomes at the city level. However, there were heterogeneous policy effects across cities in that the policy was most beneficial for the 1975 group with the largest hourly wage increase and least beneficial for the 1979 group with the largest drop in high school and college attendance.

While I revisit the effects of the high school leveling policy in the 1970s by using a newly proposed estimation method for the staggered policy adoption and defining the treatment status to minimize potential bias, this paper still has a few limitations due to the nature of small sample size. Since the Callaway et al. (2021) estimation method compares the outcome changes relative to the base period, which is one period ahead of the policy change, it is possible that the small sample in the base period inaccurately computes the propensity score and that the outcome changes in the sample could be different from those in population.

In addition, the contamination issue could be better dealt with if students' middle school grades are incorporated in the analysis. This is because one can directly identify high-achieving students from the information on middle school grades, and they were more likely to go to high schools in the treated cities before the policy change, regardless of their residence at the age of 14. Although the KLIPS collected survey participants' middle school grades through two additional surveys, one in 2006 and the other in 2008, I did not utilize such information because that further reduces the sample size. Despite the limitations, my analysis provides accurate estimates of the policy effects using the most suitable data available.

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