Effects of Ability Mixing in High School on Adulthood Earnings: Quasi-Experimental Evidence from South Korea

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Abstract

We estimate impacts of ability mixing compared to ability grouping in high school education on students' adulthood earnings. To overcome endogeneity and selection problems that plague the previous studies, we exploit a policy experiment in South Korea in the 1970s, which changed the education regime of general high schools from grouping to mixing in major cities. We find that the mixing treatment has a positive but statistically insignificant effect on average adulthood earnings. We also find that while mixing has positive effects on low-ability students' adulthood earnings, it has smaller positive or even negative effects on higher ability students.

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1 Introduction

How ability grouping and mixing in education affect students' educational outcome is hotly debated. Some argue ability grouping benefits both the high ability and the low ability students, because it allows educators to align their resources and instructive styles to the students' academic ability. Others argue that ability grouping puts the low ability students into unfair disadvantage and institutionalizes educational and social inequality. They contend that ability grouping deprives the low ability students of positive peer effects from the high ability students and that the low ability students are often stigmatized and allocated less educational resources than the high ability students (Oakes 1985). Although mixing may hurt the high ability students to some extent, they argue, its overall benefit to the low ability students outweighs the cost incurred to the high ability students.

There have been some theoretical investigations on the issue. Lazear (2001) assumes that one student's disruptive behavior, such as asking questions to which all other students know the answer, impedes learning of all other classmates, and specifies that the educational production function is $Q = \prod_{i=1}^{n} p_i$, where n is the class size and p_i is the probability that the student i behaves at any given time (ability index). Under this specification Lazear (2001) shows that the total educational output is maximized when the students are grouped according to p. He argues, however, that mixing should be preferred if the high p students can transform the low p students into high p students when mixed—abilities are substitutes—or if there is a strong need to reduce educational and income inequality in the society.

Benabou (1996) investigates the short-run and the long-run effects of ability grouping and mixing on economic growth. In his model where all workers are complementary in production, if the degree of complementarity of students' abilities in education production is moderate, mixing tends to slow down growth in the short run yet raise it in the long run, since mixing homogenizes workers over time. However, even in the long run grouping may still be the better regime of the two, if the degree of complementarity in education is so strong that the cost of "levelling down" of mixing in education exceeds the benefit of workers' homogeneity.

The theoretical literature discussed above shows that which regime delivers the better educational or economic outcome depends on the degree of complementarity and substitutability among students' abilities in educational production. Since we have little information on the fundamental parameters of the educational production function, we cannot determine a priori which regime is better. The task of evaluating the two regimes and in its course uncovering the relationship among peers in educational production is left to empirical research.

Most direct evidence we have so far on impacts of ability grouping and mixing in education on individuals is from the studies on effects of tracking in schools on students' academic achievement[1]. The studies exploit either within-school or between-school variations in the students' track status. Within schools, for example, students may belong to the academic track or to the vocational track; across schools, students may attend a school that practices tracking or a school that does not. Studies that estimate the effect of within-school track status on students' academic achievement (e.g., Alexander and McDill 1976, Rosenbaum 1980, Kerckhoff 1986, Gamoran 1987, Vanfossen et al. 1987, Argys et al. 1996) generally find that the students' achievement is positively correlated with being placed in a high ability group and negatively correlated with being placed in a low ability group, even controlling for the students' past academic performance. On the other hand, two recent studies by Betts and Shkolnik (2000) and Figlio and Page (2002) that exploit between-school variations find that tracking has little impact on student's academic achievement at any ability level.

One may doubt, however, whether the estimated relationships indeed capture the *true* effect of ability grouping and mixing, because the student's track status and school type—tracked or untracked—are likely to be endogenous. It is likely that the student's unobserved ability and the parent's unobserved taste for the child's education are correlated with the student's track status or school type. If they are poorly controlled for in estimation, the results may be biased. While most previous studies, especially the earlier ones, do not address the endogeneity problem, several recent studies do so.

Argys et al. (1996), for example, use a two-stage estimation method for self-selection models to deal with the endogeneity of the within-school track status. They assume that the racial makeup of the student body and the location of a school do not determine the student's academic achievement (math score), whereas they determine the track status. The assumption is, however, questionable. Several studies have found that the racial composition does have significant effects on the student's academic outcomes (Hanushek et al. 2004). In addition, the location of the school is likely to be correlated with school funding that may affect directly the student's academic performance. It seems very difficult to find any variable that determines a student's track placement, yet uncorrelated with the student's academic performance.

In order to control for endogeneity of school-level tracking status Figlio and Page (2002) use another set of IVs: the two and three way interactions among state graduation requirements, the number of schools in the county, and the fraction of voters in the county who voted for President Reagan in the 1984 election. They show that their OLS results and the two-stage regression results are similar and argue that the endogeneity of the school type is not likely to bias their results. However, Rees et al. (2000) point out that informal tracking is commonly practiced even in schools that have no formal tracking policy. If the informal tracking practice is common enough, the difference between the tracked and the untracked schools, categorized based on the principal's report as done by both Betts and Shkolnik (2000) and Figlio and Page (2002), is likely to be underestimated.

The 'track debate' discussed above clearly suggests that, to estimate better impacts of ability grouping and mixing on students' achievement, we need to find and exploit an exogenous variation in the regime measured with little error. It is the task that we take on in this paper, studying impacts of South Korea's high school admission policy reform which transformed, during the mid 1970s and the early 1980s, the educational regime in general—non-vocational—high schools in major cities from ability grouping to mixing. In the pre-reform period, general and vocational high schools in Korea were stratified by the students' ability level. The reform 'leveled' general high schools in major cities by allocating incoming students randomly across the schools.

Unlike the previous studies, the outcome we are concerned with in this study is students' adulthood earnings. They should be a better measure than test scores of an individual's human capital, the ultimate educational outcome. In this study we estimate not only the average effect but also the distributional effect of ability grouping and mixing on individuals' adulthood earnings.

In estimating the average effect of mixing, we employ two estimation designs: Before-and-After (BA) and Difference-in-Differences (DD). The BA design is to estimate the effect of mixing with inter-cohort differences in earnings between cohorts educated before and after the mixing treatment. The DD design is to estimate the mixing effect by comparing inter-cohort differences among general high school graduates, who received the mixing treatment, with those among vocational high school graduates, who did not. Our estimation results suggest that the average mixing effect on earnings is positive, but the size of the effect is statistically insignificant. The results of quantile regressions which estimate the distributional effects of mixing on adulthood earnings indicate that mixing has generally positive effects at lower quantiles, but little or even negative effects at higher quantiles. It suggests that lower ability students are the group benefitted most from the mixing treatment.

The balance of the paper is organized as follows. In Section 2, we describe the background and the implementation process of the leveling policy in South Korea and discuss some concomitant changes that should be considered for our study. In Section 3 we discuss our BA and DD designs including identification conditions and assumptions we invoke for this study and explain the estimation methods. We describe the data used for this study in Section 4. In Section 5 our estimation results are presented. Section 6 concludes the paper.

2 Institutional Background

South Koreans go through three stages of formal education before college: elementary school (grades 1 to 6), middle school (grades 7 to 9) and general—i.e., non-vocational—or vocational high school (grades 10 to 12). Typical general high school graduates are expected to take an entrance exam for a college and, if successful, attend one, while vocational high school graduates are expected to find employment upon graduation as entry-level skilled workers.

Before 1974, all general and vocational high schools in South Korea set their own entrance exam questions and admitted new students based on the applicants' exam result. There was a clear and widely recognized hierarchy among the general high schools, primarily based on their seniors' performance in college entrance exams. The strict competition-based admission system sorted the middle school graduates according to their academic ability and kept the ability level of students within a high school homogeneous. During this period a common perception was that the ranking of the high school one graduated from determined the course of one's life. It put students preparing for high school entrance exams and their families under enormous stress. The strict grouping regime was blamed for middle school students' psychological and physical health problems and financial distress of their parents who often hired private tutors for their children to be ahead of the competition. The admission system was also viewed to exacerbate educational and social inequality.

In response to public outcries against the system, the Korean government announced in February 1973 the "leveling policy" to reform the general high school admission practice and to eliminate competition for elite high schools. The reform was, first, to replace the school-level applications and entrance exams with the municipality-level applications and standardized exams, and, second, to allocate applicants who passed the exam randomly among schools in the students' neighborhood. Just one year later, the leveling policy was put into effect in Seoul and Busan, the two largest cities in Korea. It was enforced not only to public schools but to private schools; in South Korea private high schools are little different from public schools in administration or curriculum since they are heavily subsidized by the government. In 1975 the reform was extended to three more cities (Daegu, Inchon and Kwangju), and in 1979, 1980, and 1981 further to sixteen smaller urban municipalities. The leveling policy is still enforced in 23 cities and municipalities where about one half of all general high schools in South Korea are located (Chung 1998).

There are a couple of points in implementation of the leveling policy worth noting. First, the leveling policy was implemented on the entry-year basis. That is, in the first two years of implementation, a high school had mixed and grouped students depending on the grade. However, since there is little interaction between grades in formal high school education process, there is little reason to believe that the effect of the leveling policy on educational outcome is substantially different in the early years of implementation from that in the later years. Second, the leveling policy has been enforced only to general high schools, because it is believed that the competition for elite high schools is fueled by the expectation that a place in one of those schools leads to a place in an elite university, and ultimately to economic and social success[2]. Since vocational high school graduates are not expected to study further in universities, the leveling policy has been deemed unnecessary for them. It is still possible, however, that the leveling policy affects individuals' choices between general and vocation high schools, and changes the underlying student qualities differently across the two types of schools. To control for this, we use instrumental variables (IVs) later in the estimation process using a two-stage estimation method, in which we estimate a school type choice equation in the first stage.

Under the leveling policy, any practice of tracking has been strictly prohibited in general high schools and in lower schools. There is little variation across schools in what the students are taught and all the subjects are taught in mixed classes. Throughout an academic year, students stay in one classroom and with the same classmates. At the beginning of each academic year the students are shuffled and assigned to new classrooms so that each classroom starts with, on average, the similar quality mix of students.

INSERT TABLE 1 HERE.

Implementation of the leveling policy in mid 1970s and early 1980s was accompanied by a couple of notable changes in South Korea's education sector. First, with openings of a large number of new schools, the enrollment in general high schools substantially increased. As shown in Table 1, in 1974 the number of general high schools jumped by 36 percent and the number of general high school freshmen surged by 42 percent. In 1973, 35 percent of all middle school graduates entered a general high school; in 1974, 42 percent did. Unlike general high school enrollment size, vocational high school enrollment size changed little. The expansion of high school education is likely to have widened the overall distribution of student ability in general high schools, most likely extending the lower tail of the distribution. It is also likely to have affected the students' choices between a general and a vocational high school.

Second, in early 1980s South Korea saw an unprecedented expansion of college education. Table 1 shows that the enrollment in four-year colleges has more than doubled between 1978 and 1981. Later cohorts of high school graduates who were educated under the mixing regime had a higher chance of getting into a four-year college than their earlier counterparts educated under the grouping regime. In the next section we discuss how to deal with the potential confounding effects of these changes on the estimates of mixing effect.

3 Empirical Framework

Consider individuals who have attended a general or vocational high school in a city where the educational regime in general high schools changed from ability grouping to mixing in year $\tilde{m}$, while grouping regime in vocational high schools remained unchanged. Suppose that individual i entered a high school between year $\underline{m}$ and year $\bar{m}$ where $\underline{m} < \tilde{m} < \bar{m}$. Let g_i denote the type of high school an individual i attended: $g_i = 1$ for a general high school and $g_i = 0$ for a vocational high school. Define $\tau_i \equiv 1[i\text{'s high school freshman year} \geq \tilde{m}]$, where $1[\cdot]$ is the indicator function. Let d_i denote the mixing regime indicator for individual i which is equal to 1 if the individual was educated under the mixing regime in the high school and 0 otherwise. In the data $d_i = g_i \tau_i$, whereas in theory we may consider a counterfactual that $d_i \neq g_i \tau_i$.

Let $y^{\tilde{d}}_{i\tilde{\tau}t}$ denote *potential* log earnings observed in year $t > \bar{m}$ of individual i with $d_i = \tilde{d}$ and $\tau_i = \tilde{\tau}$. For estimation, we use the following 'parallel-shift' linear model for the potential log earnings (the subscript i is omitted for notational simplicity):

$$y^d_t = \beta_0 + x'_t\beta + \beta_g g + \beta_\tau \tau + \beta_d d + u_t, \tag{1}$$

where x_t is i's characteristics at t and u_t is the unobserved random factor that affects i's earnings such as unobserved ability and economic shock at time t. With this linear setup, the observed log earnings in year t (y_t) is

$$y_t = (1-\tau)y^0_{0t} + \tau\{(1-g)y^0_{1t} + gy^1_{1t}\} = \beta_0 + x'_t\beta + \beta_g g + \beta_\tau \tau + \beta_d g\tau + u_t. \tag{2}$$

In order to estimate the effect of mixing—β_d in equations (1) and (2),—we employ two difference-based study designs: the 'before-and-after' (BA) design based on inter-cohort differences and the 'difference-in-differences' (DD) design based on differences in two inter-cohort differences. Although the assumptions for one design do not necessarily nest those of the other, overall, the assumptions of the DD design are weaker than those of the BA design (see Lee (2005) for details).

In the BA design we estimate the effect of mixing by inter-cohort differences in log earnings between those educated under the grouping regime (cohorts *before* mixing) and those educated under the mixing regime (cohorts *after* mixing)[3]. We use only individuals who attended a general high school, that is, $g = 1$. For this group, the inter-cohort mean difference at time t is

$$\begin{aligned} BA_t &\equiv E(y_t|g=1,\tau=1,x_t) - E(y_t|g=1,\tau=0,x_t) \\ &= E(y^1_{1t}|g=1,\tau=1,x_t) - E(y^0_{0t}|g=1,\tau=0,x_t). \end{aligned} \tag{3}$$

On the condition that the intercohort mean difference would be zero without mixing—i.e., $E(y^0_{1t}|g=1,\tau=1,x_t) = E(y^0_{0t}|g=1,\tau=0,x_t)$—$BA_t$ captures the effect of mixing. Under the linear setup of equations (1) and (2), BA_t is equivalent to the coefficient β_d in the following equation:

$$y_t = (\beta_0 + \beta_g) + x_t'\beta + (\beta_\tau + \beta_d)\tau + u_t. \tag{4}$$

To identify β_d, β_τ should be equal to zero.

The DD design exploits that there are two types of high schools and the mixing treatment was never given to vocational high schools[4]. We define difference-in-differences of log earnings at time t, DD_t, as follows:

$$\begin{aligned} DD_t &\equiv E\left(y_t|g=1,\tau=1,x_t\right) - E\left(y_t|g=1,\tau=0,x_t\right) \\ &\quad - \left\{E\left(y_t|g=0,\tau=1,x_t\right) - E\left(y_t|g=0,\tau=0,x_t\right)\right\} \\ &= E\left(y^1_{1t}|g=1,\tau=1,x_t\right) - E\left(y^0_{0t}|g=1,\tau=0,x_t\right) \\ &\quad - \left\{E\left(y^0_{1t}|g=0,\tau=1,x_t\right) - E\left(y^0_{0t}|g=0,\tau=0,x_t\right)\right\}. \end{aligned} \tag{5}$$

The DD design identifies the mean effect of mixing on earnings for treated individuals, under the condition that without the mixing, the mean earnings would have changed by the equal percentage across general high school graduates and vocational high school graduates. This condition is less strict than the identification condition for BA that without the mixing the mean earnings of general high school graduates should have remained unchanged. In the linear setup, DD design is to estimate the coefficient β_d in equation (2). In the DD design we use observations not only of general high school graduates but also of vocational high school graduates.

For our estimates of β_d in the BA and the DD designs to be consistent, the unobserved characteristics of cohorts should be uncorrelated with the timing of mixing treatment (τ) or the general/vocational school type (g). This may not hold if ability of the cohorts changes with the treatment; if students' geographic mobility pattern changes with the treatment, which in turn affects the cohort characteristics; if some secular changes alter the cohort characteristics over time; or if there is unobserved heterogeneity in general/vocational school type choices. All these four cases are in fact likely to have happened in our data. How we handle the issues are briefly explained below.

First, we address unobserved heterogeneity of school type choices by using IV regressions. IV regressions are employed in estimating equation (2) in the DD design and IV regression results are compared to non-IV counterparts. Second, to control for the effect of secular changes, we estimate the equations using relatively small numbers (window sizes) of contiguous cohorts—six, ten, and fourteen—and including as explanatory variables log time trend and an indicator of labor market condition at the start of the individuals' career. Third, in order to limit the effects of geographic mobility on our estimates, we identify individuals whose mobility pattern was undisturbed by the mixing treatment and use them as our primary sample. This is explained in detail in the next section. Fourth, as for the changes in ability, unfortunately we do not have any clear solution. However, as discussed in the previous section, the expansion of high school education that happened concomitantly with the mixing treatment is likely to lower the overall ability level of high-school educated cohorts, and therefore, at least, we know that the mixing effect is likely to be underestimated.

4 Data

We use a South Korean national household survey titled "Korean Labor and Income Panel Study (KLIPS)." KLIPS is a nationally representative longitudinal survey of Korean households which started in 1998 with 5,000 households and 13,783 individuals over 15 years of age. It is modelled after the National Longitudinal Surveys (NLS) and the Panel Study of Income Dynamics (PSID) and administered annually by the Korea Labor Institute (KLI), a government-sponsored research institute (Korea Labor Institute 1998). The survey collects a wide range of information on individuals such as earnings, family background, and demographic characteristics. In addition, it collects unusually detailed information on individuals' education history. For example, for those who have attended a high school, KLIPS collects information on the name, type, and location of the high school(s) that the respondents have attended, and the years of starting and finishing the high school(s). Those pieces of information allow us to identify the education regime—grouping or mixing—each high school graduate was educated under.

For estimations, we construct a sample from the first five waves of KLIPS—1998 to 2002 survey years—of men who have attended a high school. We exclude women because women's education and earnings during the past three decades have been heavily affected by the society's changing views on women's role, which we cannot adequately control for. For the dependent variable we use log monthly earnings reported in the survey each year. To control for impacts of the nation-wide macroeconomic conditions on earnings, especially those arising from the economic crisis in the late 1990s, we divide individual monthly earnings by the national average earnings each year[5]. Observations whose reported monthly working hours are shorter than 100 hours or longer than 325 hours are dropped from the sample to minimize impacts of outliers.

INSERT TABLE 2 HERE.

In the previous section we discussed that estimates of the mixing effect may be confounded if individuals' choices of high school locations were affected by implementation of the leveling policy. Whether such mobility pattern changes in fact happened is examined in Table 2. The table shows the mobility pattern of men in KLIPS data who entered a general high school from 1966 to 1986. The men are grouped by the region of residence at age 14, the location of the high school, and the year of entrance. The years of entrance are divided into three periods: the pre-mixing period (1966–1973), the first mixing period (1974–1978) and the second mixing period (1979–1986). During the pre-mixing period, all high schools were under the grouping regime; during the first mixing period, general high schools only in the five large cities were under the mixing regime; and during the second mixing period, general high schools both in the five large cities and in the 'mixing municipalities'—the sixteen smaller cities where the leveling policy was implemented during 1979–1981—were under the mixing regime. High schools in the 'grouping municipalities' were never subjected to the mixing treatment.

The geographic mobility pattern shown in Table 2 indicates that the inflow of students into general high schools in the five large cities from the other regions fell considerably after the mixing treatment. Table 2 shows that, in the pre-mixing period, 31 percent of those who attended a general high school in the five large cities were immigrants from other areas. After the leveling policy was put into effect, the percentage of immigrant students fell by more than 10 points. Furthermore, Table 2 shows that the inflow of students from the grouping into the mixing municipalities increased considerably during the second mixing period. These movements might be motivated by attempts to avoid receiving or to receive the treatment. As discussed in the previous section, such movements may cause a bias in estimates, which we have few instruments to control for.

Amid all the changes, however, one group's mobility pattern seems to have changed little across the three periods—that of those who resided at age 14 in one of the five large cities. In every period, about 94 percent of them went on to attend a high school in one of the cities, which have been the absorbing destinations of domestic migrations in South Korea. By exploiting the fact and concentrating our analysis on the subpopulation, we can minimize the potential bias caused by mobility pattern changes. Therefore as the primary sample for the analysis we use the sample of men who resided at age 14 (grade 8) and attended a high school in one of the five large cities where the leveling policy was put into effect in 1974 and 1975.

It should be noted, however, that even if the mobility pattern of individuals in our primary sample changed little, their peer quality may have changed because the mobility pattern of other groups altered—the quality of students coming into the five large cities from other areas may have changed due to the regime change[6]. Our data has no information on the quality of peers an individual had during high school, so we cannot directly control for it in estimations. Although there is no direct evidence, some anecdotal evidence suggests that the quality of students who immigrated into the five large cities declined after the leveling policy was implemented. Under the grouping, many high quality students came to the large cities to study in elite schools; after the mixing, they had no reason to. It suggests that, if peer effects exist, the peer quality change is likely to affect negatively the mixing effect coefficient.

There are additional advantages the primary sample offers. Since the mixing policy was implemented in the five cities with less time for preparation and earlier than the rest of the country, changes in unobserved education quality—e.g., teaching method, quality of teachers, etc.—that may have been caused by the regime shift must have been smaller in those cities than in any other places. The cities also have consistent series of educational statistics we use to control for the overall changes in the school quality that other areas lack.

To control for education quality over time in estimations, we use educational statistics extracted from *Yearbooks of Educational Statistics*, published annually by the Ministry of Education and Human Resource Development. We use a series of variables that indicate the quality of general and vocational high schools aggregated at the city level: the proportion of high school teachers with at least 15 years of education, the proportion of teachers with the first degree license, and the class size (for the national level statistics, see Appendix Table 1). Each year's school quality measures are matched to an individual's data based on the high school type and the year when the individual started the high school.

5 Estimation Results

We first estimate the average effect of ability mixing and grouping on individual monthly earnings by estimating the mean difference in earnings across the two regimes. Then we examine whether grouping and mixing have different impacts on individuals by their standing in the earnings distribution. In this section we present the estimation results using only the primary sample[7].

Estimations are done using three different numbers of cohorts or 'window sizes'—six, ten, and fourteen. We use the symmetric window, that is, the number of cohorts before the treatment is equal to the number of cohorts after it. As discussed in Section 3, making the window size small helps keeping the unobserved characteristics of cohorts comparable before and after the mixing treatment, but reduces the sample size substantially. Thus we use the three different window sizes and compare the results across them[8].

5.1 Average Effect of Mixing and Grouping on Earnings

We estimate the average effect of mixing using five different specifications, or five different sets of covariates x_{it}, which progressively control for more potentially confounding variables. In the baseline specification (S1), x_{it} consists of the individual's age and its higher powers up to the fourth and the high school city dummies. The second specification (S2) extends the baseline specification by adding a dummy variable indicating whether the individual attended a private or a public high school, the log time trend (= log(High school entering year − 1960)), and the city-level measures of high school quality—the proportion of teachers with fifteen or more years of education, the proportion of teachers with the first degree license, and the class size. The log time trend is included to control for potential changes in the student quality over time caused by the expansion of high school education. We use the log time trend because the expansion of high school education, which was explosive at the time of implementation of the leveling policy, has slowed down over the years and it seems to be controlled best with a log time variable. By using log time trend, we also try to avoid the multicollinearity problem with the age variable in case of linear time trend and the loss of the degree of freedom in case of time dummies. It turns out that the functional form of the time trend has little effect on the estimates, especially for the DD design. The results using alternative time trend variables are shown in Appendix Table 4.

The third specification (S3) adds to S2 the father's education, because the expansion of high school education may bring in students with different family background from that of older cohorts. The fourth specification (S4) augments S3 by adding the individual's completed years of education. Controlling for the individual's years of education is particularly important for younger cohorts because of the dramatic expansion of Korea's college education in the late 1970s and the early 1980s. The last specification (S5) adds to S4 the national average monthly earnings of males (broken down by the education level) in South Korea for the year when the individual first entered the labor market[9]. This variable is to control for potential long-term effects of the labor market situation when individuals started their career later on labor market outcomes, suggested by some studies on cohort effects (Chiswick et al. 1997).

In the first three specifications that do not control for the completed education the mixing treatment coefficient may be interpreted as the 'total' effect of mixing on outcomes—the sum of its direct effect and the indirect effect via the individual's completed education. The coefficients in the specifications S4 and S5 show the magnitude of the direct effect, net of the effect of mixing on the educational attainment. In all estimations, we use the random effect (RE) model, as we have multiple observations on earnings (over the survey years 1998-2002) for an individual[10].

In the tables that report the BA and the DD estimation results, we show two test statistics—the usual t-statistics and the bootstrap p-values (reported in square brackets). The bootstrap p-values are suggested by Bertrand et al. to deal with potential biases arising from serial correlations of the treatment. It is the probability for $\left|\frac{\hat{\beta}_b-\hat{\beta}}{se(\hat{\beta}_b)}\right| > \left|\frac{\hat{\beta}}{se(\hat{\beta})}\right|$ where $\hat{\beta}_b$ and $\hat{\beta}$ is the bootstrap-sample and the original-sample coefficient estimate. Each bootstrap observation is the entire time-series of one individual randomly selected with replacements from the original sample. The bootstrap sample size is equal to the original sample size and the number of bootstrap samples is 500.

5.1.1 Before-and-After (BA) Estimates

INSERT TABLE 3 HERE.

Table 3 shows the BA estimation results for earnings using the primary sample. The BA setup uses the sample of general high school graduates only. The key coefficient is that of the mixing dummy τ which shows the average effect of mixing on earnings. Descriptive statistics of the sample for the 14-year-window BA and DD analysis are shown in Appendix Table 2.

The mixing coefficient estimates are positive in Table 3 in all specifications and in all window sizes. In the baseline specification, the estimates range from 0.07 to 0.24, while in the other four specifications they are higher, ranging between 0.22 and 0.36. The estimates in S5 suggest that the student's adulthood earnings would increase, relative to the national average earnings, by about 20 to 25 percent if the education regime shifted from grouping to mixing, controlling for the individual's completed education, aggregate education quality, the father's education, the time trend, and other factors. Most coefficients are, however, imprecisely estimated. According to the usual t-statistics, in only two cases the coefficient is statistically significant at the 5 percent level. There are a couple more cases where the coefficient is marginally significant, but in general we cannot reject the hypothesis that the effect on the mean adulthood earnings of the shift from grouping to mixing in education is zero.

The log time trend coefficients are estimated to be negative, except for the one in S2 with the 6-year window. The magnitude of the coefficient increases as the window size increases, being statistically significant at the 5 percent level in the 14-year window. The negative sign of the coefficients suggests that the overall (unobserved) quality of high school students has declined with the expansion of the general high school education (Table 1). This explains, at least partly, why the size of the estimated mixing effect is substantially smaller in the baseline specification than in the other specifications: it is likely to be underestimated in the baseline specification, because the declining quality of students over time is not controlled for.

The coefficients of the school quality measures are statistically insignificant except for a few marginal cases. To little surprise, the father's education is positively correlated with the son's earnings and the father's education coefficient becomes smaller when the son's own education is controlled for. The coefficients of average earnings index at the labor market entrance are mixed and statistically insignificant. It indicates that the different labor market conditions across the cohorts at the beginning of their careers have little long-term effect on the individuals' earnings.

One may doubt the BA estimation results, because identification of the mixing effect in BA design crucially depends on the assumption $\beta_\tau = 0$ in equation (4). The assumption cannot be tested, and one cannot rule out completely the possibility that $\beta_\tau > 0$. For example, later cohorts may have been benefited more from Korea's rapid economic growth than earlier cohorts, which may not be captured completely by the included covariates. In addition, the BA estimates may be confounded by endogeneity of the school type choice. Using DD design we can relieve such limitations of the BA design. Identification of the mixing effect in DD design does not require β_τ to be zero, but requires it to be only equal across the general and the vocation high school graduates. Endogeneity of the school type choice can be addressed using IVs in DD design.

5.1.2 Difference-in-Differences (DD) Estimates

INSERT TABLE 4 HERE.

Table 4 shows the DD estimation results, along with the IV estimation results. The key coefficient is the coefficient of the interaction term between τ and g which shows the mixing treatment effect. The coefficient of τ alone captures the 'timing' or 'cohort' effect which is assumed to be equal across the general and the vocational high school graduates.

Like the BA estimation results, the mixing treatment effect is estimated to be positive in every case but two. In the baseline specification the estimates range from 0.24 to 0.32 depending on the window size. In other specifications, the non-IV estimates range from 0.02 to 0.22. The non-IV DD estimates in S5 suggest that the shift from grouping to mixing in education increases the student's adulthood earnings by 9 to 22 percent. Most coefficients are, however, statistically insignificant according to the usual t-values and the bootstrap p-values. Comparing the DD estimation results to those in Table 3, we find that, except for the baseline specification, the DD estimates are much smaller than their BA counterparts. It indicates that the BA estimates in S2–S5 may overstate the effect of mixing because the assumption $\beta_\tau = 0$ does not hold.

As discussed before, in DD design we can use IV estimation method to deal with endogeneity of the school type. With multiple instrumental variables, we use the 2SLS method. In the first stage regression of the school type, as instrumental variables we use a dummy variable that indicates whether the father was alive in the year when the individual started the high school, its interaction with τ, and the differences in the school quality measures between the general and the vocational high schools. The first-stage estimation results are shown in Appendix Table 3.

The father's presence at home affects the family's economic situation that should, in turn, affect the individual's school type choice. Those who do not have the father at home are more likely to attend vocational high schools than those who do, because vocational high schools teach their students skills that can be used right away by employers and their graduates usually find jobs upon graduation. General high schools, on the other hand, prepare their students for college entrance exams and equip them with little, if any, occupational skills. The first-stage regression results confirm this expectation for the post-mixing period.

The second requirement of the validity of the father's presence as an IV is that it should be uncorrelated with wages, controlling for other factors. This seems to be an empirical matter, since previous studies show mixed evidence on effects of single parenthood on labor market outcomes (McLanahan and Sandefur 1994, Corak 2001, Lang and Zagorsky 2001). A test for zero correlation between the father's presence and wages is not readily available in our context, because there are interaction terms in the first and second stages, and the person-specific random effects. Nevertheless, we attempted some indirect tests for zero correlation between the father's presence and wages and found no evidence that suggests a significant correlation between them, controlling for other factors[11]. Although our test results do not necessarily guarantee the exogeneity of the father's presence at 14 to adulthood wages, it seems equally difficult to find evidence that our IV is endogenous, given the current data. More detail information on the cause of the father's absence and intermediate outcomes of a student prior to the labor market (such as academic performance near the graduation of high school) would shed light on potential endogeneity of the father's presence at 14. Unfortunately, however, our data do not permit such an analysis. In addition to the father's presence at 14, the quality difference between the two school types may be an important factor considered by individuals who choose between the two, although it is only weakly supported in the first-stage regression.

The 2SLS estimates reported in Table 4 are smaller than their non-IV counterparts except for one case. Whereas the non-IV estimates range from 0.08 to 0.22, the IV estimates range between -0.01 to 0.16. While the non-IV and the IV coefficients are statistically insignificant at any conventional level, the differences in magnitude between those two estimates indicate that the unobserved quality of vocational students may have fallen after the mixing treatment.

As for the coefficient size, one may be surprised by the large magnitude of some estimates. Although it is difficult to pin down the precise reason for it, a couple of explanations seem reasonable. One is that the estimates may be influenced by noise in the earnings observations. This should not be a problem with a large sample, but the sample size of our data is relatively small, as we restrict the sample to men who entered high school during a short span of years before and after the mixing treatment. This problem is likely to be more acute in BA estimates than in DD estimates. It is noticeable that in Appendix Table 4 the estimates using the averaged variables are much smaller than those using the original setup in the BA design, while it does not change the interpretation of the results. The other explanation is that BA and DD non-IV estimates are biased upwards. The estimation results show that the DD-IV estimates are, while qualitatively similar, the smallest in the magnitude in most specifications and windows.

As for other coefficient estimates, the mixing dummy coefficient (β_τ) is of mixed signs depending on specifications and mostly insignificant. The log time trend coefficients are estimated to be negative in all estimations as in BA estimation results. The log time trend coefficient is statistically significant in the 14-year window, but not in the other two windows. There appears to be no significant difference in earnings between the general high school graduates and the vocational high school graduates, everything else equal. The coefficient estimates of g are never statistically significant.

5.2 Distributional Effect of Mixing—Quantile Regression Results

In the previous section we have examined the average effect of grouping and mixing in education on students' adulthood earnings using two research designs. The point estimates of the mixing effect suggest that, controlling for other factors, students educated under the mixing regime earn on average more than those educated under the grouping regime. In few estimation results, however, the effect is statistically significant at any usual level.

In this section we examine whether and how the two education regimes differ in their distributional effects on the students' adulthood earnings. Economic theories and empirical studies on the educational outcome of ability grouping and mixing suggest that effects of each regime should differ by ability level of the students. While the detailed theoretical predictions depend upon the degree of complementarity and substitutability of students' abilities in education production function (Benabou 1996), the consensus seems that the mixing regime is likely to benefit low-ability students and the grouping regime high-ability students. In spite of its intuitive appeal, however, empirical studies have so far failed to confirm or refute convincingly the prediction. Many studies that find tracking has different effects on students' academic outcome by their ability group are questioned because of the potential selection bias problem. Two recent studies by Betts and Shkolnik and Figlio and Page, on the other hand, find little impact of tracking in any ability group.

To examine the distributional effects of mixing on earnings, we employ the quantile regression method (Koenker and Bassett 1978, Buchinsky 1998). Since, everything else equal, an individual's earnings are likely to be correlated positively with his ability, if mixing benefits low-ability students but hurts high-ability students, we expect that mixing has a positive effect on lower quantiles but a negative effect on higher quantiles in the earnings distribution.

We estimate impacts of mixing on adulthood earnings, employing the most extensive specification S5 at five quantiles, 0.10, 0.25, 0.50, 0.75, and 0.90[12]. Endogeneity of the school type choice in the DD setup is dealt with using the two-stage least absolute deviation (2SLAD) estimation method (Amemiya 1982, Powell 1983). Similar to 2SLS, the 2SLAD is a two-stage method. Given the first-stage estimates for the linear probability model of a general high school, the second stage is a quantile regression for a student's outcomes, in which the endogenous dummy variables are replaced by the first-stage predicted probabilities. We employ a bootstrap method to calculate standard errors of the estimators with 100 replications. In the quantile regressions, we treat each earnings observation as if it were an independent realization, due to lack of a proper econometric method for panel LAD models.

INSERT TABLE 5 HERE.

Table 5 shows the quantile regression results. The first panel (A) shows the BA-setup results, the second panel (B) shows the DD-setup results, and the last panel (C) shows the 2SLAD results of the DD setup. Each panel shows two results, one using the 10-year-window and the other using the 14-year-window.

The quantile regression results suggest that mixing has large positive impacts on low-ability students' adulthood earnings. At the lowest 0.10 quantile, the mixing treatment is estimated to increase the earnings by 16 to 45 percent and at the 0.25 quantile by 12 to 33 percent. Two out of the six mixing effect coefficients at each 0.10 and 0.25 quantiles in Table 5 are statistically significant at the 5 percent or the smaller level.

At the next 0.50 quantile mixing is still estimated to increase the earnings, but by much smaller percentage points than at the lower quantiles—by 6 to 13 percent. None of the coefficients is statistically significant at any conventional level. The estimated effect of mixing at the 0.75 quantile is similar to that at the 0.50 quantile. In one estimate, it is negative. At the highest 0.90 quantile, the effect of mixing on earnings is estimated to be little higher than zero and even negative in half of the cases. The estimated effect ranges from -10 percent to 8 percent.

Our quantile regression results suggest that educational regimes do have different effects on students depending on their ability, as the theory predicts. Mixing benefits low-ability students, while grouping favors high-ability students. Positive effects of mixing treatment on students' adulthood earnings are likely to be substantial at lower end in the ability distribution, but they diminish—almost monotonically—as the students' ability level rises, and that mixing may lower high-ability students' adulthood earnings. In terms of statistical significance, we found that the positive effect of mixing on low-ability students receives stronger support than the negative effect of mixing on high-ability students.

Our finding that mixing benefits low-ability students is in line with findings of many existing studies on impacts of tracking on academic achievement. Negative effects of mixing on high-ability students are also found in Argys et al. (1996). Our findings are also consistent with those from studies on impacts of peer ability on students' academic achievement, such as Summers and Wolfe (1977), Henderson et al. (1978), Hoxby (2000) and Hanushek et al. (2003), which find that peer ability has significant positive effects on students' academic achievement. In particular, our results are comparable to non-linear—concave—peer group effects found by Summers and Wolfe (1977) and Henderson et al. (1978).

5.3 Do Occupation Choices Affect Our Results?

South Korea has a very high rate of self-employment in the labor force. According to Blanchflower (2004, Tables 1 and 2), among the OECD countries, South Korea has one of the highest rate of self-employment, along with Greece, Mexico and Turkey. As of 2002, self-employment explains 30.4 percent of the total employment and 26.0 percent of non-agricultural employment in South Korea[13]. Given this high rate of self-employment, it is possible that students may opt for self-employment if their labor market prospects are not promising. It may cause low-ability high school graduates educated under mixing to enter self-employment rather than to become a wage earner, because their labor market prospects are likely to be bleaker than their counterparts in the pre-mixing period due to the substantial expansion in high school education. This may cause upward bias to our estimates of the mixing effect, since the lower tail of potential wage-earners may disappear as a result of the change in occupational choices after the mixing.

INSERT TABLE 6 HERE.

To investigate this possibility, we run a linear probability RE model for the wage-earner status (binary variable) using the same set of explanatory variables used in prior estimations. The comparison group is those who are not wage-earners—that is, those self-employed and unemployed—at the time of the survey. Table 6 summarizes the BA and DD estimation results for becoming a wage-earner.

We find no notable changes in occupational choices due to mixing. Main coefficients show mixed signs depending on the estimation setup and the window size. No coefficient is statistically different from zero at the conventional level of significance. This suggests that our estimates of the mixing effect on earnings are unlikely to be affected by potential changes in occupational choices of students.

6 Concluding Remarks

Existing empirical studies on effects of ability grouping and mixing in education on students' academic outcome exploit within-school or between-school variations in tracking status. However, their results are questioned because of endogeneity and measurement error in students' tracking status. In this paper we try to overcome those problems by using data from South

Korea where an exogenous educational regime shift from grouping to mixing occurred in general high schools during the 1970s. Unlike previous studies that concentrate on students' average academic outcomes, we study average and distributional impacts of the two education regimes on students' adulthood earnings.

Using two study designs based on inter-cohort differences, we estimate that the students' mean adulthood earnings are likely to be higher under mixing than under grouping. Our estimation results using the 10-year window around the year of the regime shift suggest that students' average adulthood earnings are higher by about 15 to 20 percent under mixing than under grouping, controlling for the students' completed years of education, father's education, and municipality-level school quality measures. In most specifications, however, we cannot reject the hypothesis that the difference between the effects of the two regimes is insignificant.

Using quantile regressions we discover that the effects of mixing and grouping differ across the earnings distribution. The positive effect of mixing is large in the lower tail of the earnings distribution (0.10 and 0.25 quantiles); the effect of mixing is still positive but much smaller in the middle part of the distribution (0.50 and 0.75 quantiles); and in the upper tail of the distribution (0.90 quantile), mixing may have negative effects. It suggests that if students of different abilities are mixed, low-ability students are likely to benefit from positive peer influence of high-ability students, but high-ability students may lose out due to negative peer influence from low-ability students. According to our estimation results, positive effects of mixing at the lower tail in the earnings distribution are generally stronger than its negatives effect at the upper tail, which explains why mixing appears to have positive effects on average earnings. The results are consistent with findings of many existing studies on effects of peer group quality on students' academic achievement.

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Notes

[1]Another closely but indirectly related literature is that on peer group effects in education (e.g., Summers and Wolfe 1977, Hanushek et al. 2003). It is mainly concerned with impacts of the 'average' quality of peers such as I.Q. or test scores on students' academic achievement. It does not directly estimate impacts of ability grouping and mixing on students' educational outcome. Another related literature is that on impacts of racial composition in classroom or school on students' achievement (e.g., Hoxby 2000, Angrist and Lang 2004, Hanushek et al. 2004). Although race is strongly correlated with students' academic performance, racial mixing is certainly not equivalent to ability mixing. Their policy objectives are likely to differ and their effects should be evaluated from different perspectives. What we are concerned here is pure ability mixing, keeping everything else, including the racial composition, constant.

[2]It is true that before 1974 graduates from a few elite general high schools dominated in gaining admissions to elite universities. For example, in year 1971, 398 general high schools in the country produced 93,699 graduates in total (Ministry of Education and Human Resource Development, *Yearbook of Education Statistics*, 1971). In that year, one third of all admitted to the Seoul National University, the nation's most prestigious university, were graduates from only four high schools (*Joong-Ang Daily*, 7 February 1972).

[3]Note that our BA design differs from the conventional BA design. In the conventional BA design, outcomes observed before the treatment are compared to those observed after the treatment for the same individuals. In our design, the outcomes of cohorts who came before the treatment are compared to those of cohorts who came after the treatment. The outcomes of both cohorts are observed at the same time long after the treatment.

[4]Another DD method may be conceivable when we consider the fact that the mixing treatment was sequentially implemented to different regions from 1973-74 to 1979-81. For example, we can compare inter-cohort differences in earnings between general high school graduates educated in the first-stage five cities and those educated in the second-stage sixteen municipalities (and/or in municipalities never subject to mixing). We do not adopt this DD method mainly due to the sharp changes in mobility patterns after 1974, which must have led to quality changes in the student body (See Section 4 and Table 2 for details). While we have some IVs available to deal with potential endogeneity of school-type choice, there seem to exist no suitable IVs (at least in our data) that explain the decision about high-school location.

[5]Alternative estimation results using the simple log monthly earnings as the dependent variable are shown in Appendix Table 4. The estimation results are not substantially different.

[6]We thank an anonymous referee for pointing this out to us.

[7]Estimation results using the secondary sample of individuals who resided at age 14 and received the mixing treatment in the sixteen smaller cities during 1979–1981 are available upon request.

[8]Estimation results using other windows sizes are similar and thus omitted.

[9]This information is available from the Korean Statistical Information System Database each year from 1975 to 2003. We use the index of a particular year's average earnings, normalizing the 1975 average to 100. We infer the year of an individual first entering labor market by adding to the year of entering high school six years (three years of high school education plus average three-year duration of compulsory military service) for high school graduates, eight years for two-year college graduates, and ten years for four-year college graduates.

[10]One may argue that the number of observations used for the estimations is inflated because we treat yearly observations for the same individual as separate observations. We estimated the models using averaged variables and found little difference. For the alternative estimates, please see Appendix Table 4.

[11]We attempt two methods to test for zero correlation between the father's presence and wages. The first method is to estimate the specifications (S3) to (S5), including the father's presence and its interaction with τ as extra explanatory variables. The estimates of these two variables are never significantly different from zero both individually and jointly for all the specifications and window sizes. (This finding changes little whether or not the the differences in school quality measures are included.) Although this result should be interpreted with caution—the consistency of the estimated effect of the father's presence depends upon the exogeneity of the type of high school, they are suggestive of zero correlation between the father's presence and wages when a school choice is controlled for. The second method employs averaged variables to avoid the person-specific random effects. Based on the idea of the test for overidentifying restrictions (Stock and Watson 2003, pp.353-4)), we regress 2SLS residuals of the specifications (S3IV) to (S5IV) against all exogenous variables including IVs, and calculate a test statistic equal to $m \times F$ where m is the number of IVs and F is the F-statistic testing the hypothesis that the coefficients of all IVs are jointly equal to zero. The test statistics for all the specifications and window sizes fail to exceed the critical value (7.81) at the 5 percent significance level. This also implies that the father's presence seems to be uncorrelated with wages when a school choice is controlled for.

[12]One may argue that the specification S4 is more appropriate for this purpose since a student's completed education can be affected by his ability level. Conditioning on own education means that one looks at the conditional-on-education quantiles. Noting the risk of mis-interpretation, we chose S5 over S4, because the expansion of college education in early 1980s may inflate the mixing's effects on all quantiles. We believe S5 is a specification that is less vulnerable to potential upward bias. When we employ S4, overall patterns of the mixing's impacts on various quantiles do not vary much compared with those from S5, although some of estimates display a rise and fall. The estimates from S4 are available upon request.

[13]As shown in Appendix Table 2, the proportion of non-wage-earners—those self-employed or unemployed—are 35.2 percent in our data set for the 7-year window DD.

Table 1: Educational Statistics, South Korea: 1965-90

(Unit: thousand, percent)

	Population aged 15-19	4-Year University Matriculants	High School Freshmen in Total		General High School Freshmen		Ratio of General to Vocational Freshmen	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year	Nation	Nation	Nation	Seoul-Busan	Nation	Seoul-Busan	Nation	Seoul-Busan
1965	2,608	22.1 (0.9)	153.9 (5.9)	60.6 (2.3)	91.2	40.6	1.46	2.02
1966	2,698	30.4 (1.1)	152.8 (5.7)	57.9 (2.1)	90.0	38.2	1.44	1.94
1967	2,783	30.0 (1.1)	160.0 (5.7)	60.1 (2.2)	91.8	39.3	1.35	1.89
1968	2,858	34.9 (1.2)	191.3 (6.7)	75.5 (2.6)	104.8	48.2	1.21	1.77
1969	3,017	36.9 (1.2)	200.0 (6.6)	77.5 (2.6)	108.9	49.9	1.19	1.81
1970	3,269	40.6 (1.2)	221.0 (6.8)	83.8 (2.6)	114.2	51.3	1.07	1.58
1971	3,500	43.2 (1.2)	248.7 (7.1)	90.3 (2.6)	125.7	54.6	1.02	1.53
1972	3,723	47.1 (1.3)	277.8 (7.5)	99.1 (2.7)	137.2	59.4	0.98	1.50
1973	3,960	50.1 (1.3)	328.1 (8.3)	119.5 (3.0)	162.2	72.7	0.98	1.55
1974	4,175	54.6 (1.3)	391.9 (9.4)	131.8 (3.2)	230.6	85.7	1.43	1.86
1975	4,334	58.3 (1.3)	429.5 (9.9)	137.7 (3.2)	254.7	89.0	1.46	1.83
1976	4,455	64.2 (1.4)	468.4 (10.5)	144.6 (3.2)	277.2	91.8	1.45	1.74
1977	4,547	71.3 (1.6)	494.9 (10.9)	152.7 (3.4)	287.4	95.4	1.38	1.66
1978	4,604	80.1 (1.7)	525.2 (12.7)	160.3 (3.5)	299.7	99.5	1.33	1.64
1979	4,588	109.9 (2.4)	581.5 (12.7)	170.8 (3.7)	324.1	105.4	1.26	1.61
1980	4,520	134.3 (3.0)	607.0 (13.4)	179.1 (4.0)	342.3	107.4	1.29	1.50
1981	4,412	199.6 (4.5)	675.6 (15.3)	187.5 (4.3)	376.9	113.4	1.26	1.53
1982	4,331	219.4 (5.1)	698.7 (16.1)	194.1 (4.5)	388.0	116.4	1.25	1.50
1983	4,305	233.8 (5.4)	720.2 (16.7)	212.9 (4.9)	407.4	126.6	1.30	1.47
1984	4,339	243.8 (5.6)	758.7 (17.5)	226.5 (5.2)	436.7	138.1	1.36	1.56
1985	4,408	246.6 (5.6)	759.6 (17.2)	234.0 (5.3)	453.0	145.5	1.48	1.64
1986	4,482	243.4 (5.4)	782.6 (17.5)	247.7 (5.5)	464.0	154.7	1.46	1.66
1987	4,550	240.4 (5.3)	809.9 (17.8)	270.7 (5.9)	508.8	174.2	1.69	1.81
1988	4,581	223.3 (4.9)	800.5 (17.5)	267.8 (5.8)	508.8	172.3	1.74	1.80
1989	4,551	235.1 (5.2)	783.7 (17.2)	263.9 (5.8)	501.7	171.9	1.78	1.87
1990	4,442	242.9 (5.5)	764.1 (17.2)	257.0 (5.8)	491.0	167.4	1.80	1.87

Source: National Statistical Office Data Base and *Yearbook of Educational Statistics* (Various Years)
Note: Figures in parnetheses are the proportions (in percentage) in the national population aged 15-19 each year.

Table 2: Mobility Pattern Before and After Mixing: Male General High School Students

Residence Region at 14	Years of Entrance	Region of High School Attendance: Five Large Cities	Mixing Municipalities	Grouping Municipalities	Row Sum
Five Large Cities	1966-73	74(0.69)	1(0.02)	4(0.04)	79(0.33)
		[0.94]	[0.01]	[0.05]	[1.00]
	1974-78	86(0.80)	0(0.00)	4(0.04)	90(0.38)
		[0.96]	[0.00]	[0.04]	[1.00]
	1979-86	181(0.82)	0(0.00)	10(0.06)	191(0.38)
		[0.95]	[0.00]	[0.05]	[1.00]
Mixing Municipalities	1966-73	4(0.04)	27(0.61)	4(0.04)	35(0.15)
		[0.11]	[0.77]	[0.11]	[1.00]
	1974-78	4(0.04)	30(0.75)	3(0.03)	37(0.15)
		[0.11]	[0.81]	[0.08]	[1.00]
	1979-86	7(0.03)	86(0.70)	6(0.04)	99(0.19)
		[0.07]	[0.87]	[0.06]	[1.00]
Grouping Municipalities	1966-73	29(0.27)	16(0.36)	82(0.91)	127(0.53)
		[0.23]	[0.13]	[0.65]	[1.00]
	1974-78	18(0.17)	10(0.25)	85(0.92)	113(0.47)
		[0.16]	[0.09]	[0.75]	[1.00]
	1979-86	32(0.15)	37(0.30)	149(0.90)	218(0.43)
		[0.15]	[0.17]	[0.68]	[1.00]
Column Sum	1966-73	107(1.00)	44(1.00)	90(1.00)	241(1.00)
		[0.44]	[0.18]	[0.37]	[1.00]
	1974-78	108(1.00)	40(1.00)	91(1.00)	240(1.00)
		[0.45]	[0.17]	[0.38]	[1.00]
	1979-86	220(1.00)	123(1.00)	165(1.00)	508(1.00)
		[0.43]	[0.24]	[0.32]	[1.00]

Note: Numbers in the parentheses next to the counts are the column proportions for the years of entrance. Numbers in the square brackets below the column proportions are the row proportions.

Table 3: Before-and-After Estimates: Five Large Cities

	6-year Window					10-year Window				
Specification	(S1)	(S2)	(S3)	(S4)	(S5)	(S1)	(S2)	(S3)	(S4)	(S5)
Mixing	0.240	0.357	0.357	0.308	0.257	0.092	0.320	0.309	0.228	0.216
	(2.15)	(1.58)	(1.59)	(1.58)	(1.24)	(0.91)	(1.73)	(1.62)	(1.40)	(1.30)
	[0.004]	[0.040]	[0.060]	[0.086]	[0.188]	[0.228]	[0.006]	[0.014]	[0.056]	[0.046]
Age	-18.72	-19.39	-20.72	-20.15	-20.53	-3.814	-4.725	-4.772	-5.289	-5.282
	(2.92)	(2.93)	(3.03)	(3.02)	(3.07)	(2.03)	(2.44)	(2.42)	(2.75)	(2.73)
$Age^2/10$	7.253	7.538	8.088	7.812	7.959	1.432	1.744	1.767	1.921	1.919
	(2.85)	(2.87)	(2.98)	(2.95)	(3.00)	(2.10)	(2.49)	(2.48)	(2.76)	(2.74)
$Age^3/100$	-1.232	-1.284	-1.381	-1.326	-1.352	-0.234	-0.281	-0.286	-0.306	-0.305
	(2.77)	(2.81)	(2.92)	(2.88)	(2.93)	(2.16)	(2.55)	(2.54)	(2.78)	(2.76)
$Age^4/1000$	0.077	0.081	0.087	0.083	0.085	0.014	0.017	0.017	0.018	0.018
	(2.70)	(2.73)	(2.86)	(2.80)	(2.85)	(2.23)	(2.60)	(2.60)	(2.81)	(2.79)
Private High School		0.079	0.067	0.133	0.128		0.096	0.088	0.113	0.107
		(0.74)	(0.63)	(1.42)	(1.33)		(1.05)	(0.95)	(1.43)	(1.31)
Log(Year of High		0.048	-0.513	-0.846	-0.657		-0.732	-0.680	-0.390	-0.318
School Entrance)		(0.04)	(0.42)	(0.78)	(0.58)		(1.18)	(1.05)	(0.70)	(0.53)
Teachers with 15+		-0.044	-0.047	-0.051	-0.055		-0.011	-0.011	-0.011	-0.012
Years of Education		(1.37)	(1.42)	(1.75)	(1.85)		(0.92)	(0.95)	(1.12)	(1.17)
Teachers with 1st		0.001	0.017	0.029	0.036		-0.002	-0.005	-0.010	-0.009
Degree License		(0.04)	(0.51)	(0.96)	(1.14)		(0.15)	(0.30)	(0.82)	(0.68)
Class Size		-0.050	-0.099	-0.138	-0.183		-0.034	-0.049	-0.057	-0.068
		(0.47)	(0.93)	(1.47)	(1.75)		(0.42)	(0.59)	(0.80)	(0.94)
Father's Education			0.035	0.024	0.020			0.018	0.007	0.005
			(2.77)	(2.11)	(1.70)			(1.84)	(0.76)	(0.56)
Years of Own				0.074	0.077				0.091	0.091
Education				(4.50)	(4.25)				(6.00)	(5.60)
Avg Earnings Index					-0.673					-0.210
at Market Entrance					(0.77)					(0.35)
Intercept	178.3	190.8	206.3	204.7	211.7	37.42	52.26	53.21	58.30	59.13
	(2.98)	(3.03)	(3.17)	(3.21)	(3.31)	(1.96)	(2.58)	(2.59)	(2.92)	(2.95)
Number of Obs.	228	219	202	202	202	396	374	355	355	355
Number of Persons	66	62	58	58	58	111	102	97	97	97

(Continued to the next page.)

Note : Asymptotic t-statistics are in parentheses. In square brackets are bootstrap p-values (Bertrand et al.(2004)). The high school region dummies are also included in the regressions, but their coefficient estimates are not shown.

Table 3: Before-and-After Estimates: Five Large Cities (Continued)

	14-year Window				
	(S1)	(S2)	(S3)	(S4)	(S5)
Mixing	0.073	0.345	0.332	0.241	0.237
	(0.76)	(2.03)	(1.94)	(1.59)	(1.55)
	[0.358]	[0.004]	[0.010]	[0.018]	[0.034]
Age	-0.260	-1.475	-1.558	-2.409	-2.502
	(0.16)	(0.87)	(0.91)	(1.45)	(1.48)
$Age^2/10$	0.133	0.582	0.619	0.898	0.933
	(0.23)	(0.96)	(1.01)	(1.50)	(1.53)
$Age^3/100$	-0.027	-0.100	-0.107	-0.147	-0.152
	(.29)	(1.04)	(1.1)	(1.56)	(1.58)
$Age^4/1000$	0.002	0.006	0.007	0.009	0.009
	(0.36)	(1.13)	(1.19)	(1.61)	(1.64)
Private High School		0.063	0.070	0.060	0.064
		(0.79)	(0.88)	(0.85)	(0.90)
Log(Year of High		-1.142	-1.124	-0.978	-0.991
School Entrance)		(2.62)	(2.55)	(2.49)	(2.40)
Teachers with 15+		-0.005	-0.005	-0.003	-0.003
Years of Education		(0.55)	(0.50)	(0.40)	(0.38)
Teachers with 1st		0.012	0.011	0.010	0.010
Degree License		(1.35)	(1.30)	(1.31)	(1.29)
Class Size		-0.030	-0.040	-0.062	-0.073
		(0.44)	(0.58)	(1.02)	(1.16)
Father's Education			0.023	0.013	0.011
			(2.66)	(1.60)	(1.43)
Years of Own				0.085	0.082
Education				(6.02)	(5.54)
Avg Earnings Index					0.060
at Market Entrance					(0.15)
Intercept	1.546	18.52	19.38	28.71	30.25
	(0.09)	(1.03)	(1.07)	(1.64)	(1.71)
Number of Obs.	515	483	464	464	464
Number of Persons	146	133	128	128	128

Note : See the previous page.

Table 4: Difference-in-Differences Estimates: Five Large Cities

	6-year Window								10-year Window				
Estimation Method :	RE Method					IV			RE Method				
	(S1)	(S2)	(S3)	(S4)	(S5)	(S3IV)	(S4IV)	(S5IV)	(S1)	(S2)	(S3)	(S4)	(S5)
Mixing	-0.099	0.245	0.128	-0.019	-0.068	0.230	0.120	0.062	-0.172	0.230	0.150	-0.017	-0.061
	(0.70)	(0.95)	(0.50)	(0.09)	(0.29)	(0.91)	(0.53)	(0.26)	(1.48)	(1.26)	(0.78)	(0.10)	(0.35)
	[0.34]	[0.24]	[0.49]	[0.91]	[0.66]	[0.28]	[0.52]	[0.74]	[0.05]	[0.05]	[0.26]	[0.87]	[0.60]
General High-School	-0.075	0.274	0.009	0.099	0.133	-0.641	-0.458	-0.329	0.003	0.332	0.195	0.052	0.037
	(0.59)	(0.88)	(0.03)	(0.37)	(0.48)	(1.16)	(0.93)	(0.65)	(0.03)	(1.42)	(0.81)	(0.24)	(0.17)
	[0.32]	[0.33]	[0.98]	[0.70]	[0.59]	[0.30]	[0.39]	[0.48]	[0.96]	[0.06]	[0.30]	[0.77]	[0.85]
Mixing*General High School	0.318	0.077	0.182	0.210	0.222	0.131	0.109	0.130	0.238	0.038	0.113	0.172	0.202
	(1.81)	(0.32)	(0.76)	(1.00)	(1.05)	(0.56)	(0.53)	(0.62)	(1.68)	(0.20)	(0.58)	(1.00)	(1.15)
	[0.02]	[0.69]	[0.33]	[0.22]	[0.19]	[0.49]	[0.51]	[0.40]	[0.01]	[0.79]	[0.46]	[0.18]	[0.12]
Age	-18.05	-17.11	-17.48	-17.48	-17.38	-14.38	-14.82	-15.35	-2.281	-2.871	-2.842	-3.275	-3.232
	(3.05)	(2.86)	(2.79)	(2.83)	(2.81)	(2.13)	(2.23)	(2.28)	(1.50)	(1.79)	(1.74)	(2.03)	(2.00)
$Age^2/10$	7.047	6.653	6.822	6.787	6.745	5.551	5.697	5.912	0.890	1.077	1.064	1.195	1.179
	(3.00)	(2.80)	(2.75)	(2.77)	(2.76)	(2.06)	(2.16)	(2.21)	(1.62)	(1.87)	(1.81)	(2.07)	(2.04)
$Age^3/100$	-1.208	-1.137	-1.168	-1.157	-1.149	-0.941	-0.962	-1.001	-0.151	-0.177	-0.175	-0.192	-0.189
	(2.96)	(2.74)	(2.70)	(2.72)	(2.70)	(2.00)	(2.09)	(2.15)	(1.75)	(1.97)	(1.90)	(2.12)	(2.09)
$Age^4/1000$	0.077	0.072	0.074	0.073	0.073	0.059	0.060	0.063	0.009	0.011	0.011	0.011	0.011
	(2.90)	(2.68)	(2.65)	(2.66)	(2.64)	(1.93)	(2.02)	(2.08)	(1.87)	(2.06)	(1.99)	(2.18)	(2.15)
Private High School		0.020	0.032	0.119	0.118	0.016	0.101	0.103		0.021	0.029	0.061	0.049
		(0.22)	(0.36)	(1.50)	(1.48)	(0.18)	(1.25)	(1.25)		(0.29)	(0.39)	(0.95)	(0.75)
Log(Year of High School Entrance)		-0.747	-0.575	-0.575	-0.357	-0.894	-0.944	-0.720		-0.793	-0.705	-0.446	-0.198
		(0.87)	(0.68)	(0.78)	(0.47)	(1.07)	(1.28)	(0.92)		(1.90)	(1.65)	(1.17)	(0.45)
Teachers with 15+ Years of Education		-0.023	-0.016	-0.011	-0.011	0.001	0.005	0.003		-0.005	-0.005	-0.005	-0.009
		(0.96)	(0.63)	(0.53)	(0.52)	(0.05)	(0.23)	(0.13)		(0.55)	(0.57)	(0.63)	(0.97)
Teachers with 1st Degree License		0.009	0.006	0.006	0.007	0.009	0.010	0.011		-0.001	-0.002	-0.002	-0.002
		(0.98)	(0.63)	(0.82)	(0.95)	(1.03)	(1.32)	(1.35)		(0.23)	(0.39)	(0.49)	(0.42)
Class Size		-0.089	-0.035	-0.095	-0.119	0.133	0.054	0.008		-0.053	-0.025	-0.023	-0.026
		(1.10)	(0.43)	(1.31)	(1.57)	(0.92)	(0.41)	(0.06)		(0.98)	(0.46)	(0.47)	(0.53)
Father's Education			0.030	0.019	0.016	0.034	0.023	0.020			0.011	0.003	0.001
			(3.01)	(2.10)	(1.70)	(3.31)	(2.42)	(2.01)			(1.45)	(0.37)	(0.18)
Years of Own Education				0.080	0.084		0.077	0.081				0.089	0.091
				(5.23)	(5.23)		(4.94)	(4.95)				(6.62)	(6.50)
Avg Earnings Index at Market Entrance					-0.756			-0.651					-0.538
					(1.21)			(1.02)					(1.16)
Intercept	171.4	171.9	170.1	173.1	174.2	132.1	140.4	148.4	21.59	33.90	31.82	35.17	35.38
	(3.10)	(3.04)	(2.88)	(2.97)	(2.98)	(1.98)	(2.16)	(2.24)	(1.38)	(2.01)	(1.85)	(2.09)	(2.10)
Number of Obs.	366	343	316	316	316	316	316	316	626	582	553	553	553
Number of Persons	105	96	89	89	89	89	89	89	177	161	153	153	153

(Continued to the next page.)

Note : Asymptotic t-statistics are in parentheses. In square brackets are bootstrap p-values (Bertrand et al.(2004)).
The high school region dummies are also included in the regressions, but their coefficient estiamtes are not shown.

Table 4: Difference-in-Differences Estimates: Five Large Cities (Continued)

	10-year Window			14-year Window							
Estimation Method :	IV			RE Method					IV		
	(S3IV)	(S4IV)	(S5IV)	(S1)	(S2)	(S3)	(S4)	(S5)	(S3IV)	(S4IV)	(S5IV)
Mixing	0.196	0.064	0.021	-0.235	0.213	0.156	0.046	0.056	0.230	0.082	0.094
	(0.97)	(0.37)	(0.11)	(2.29)	(1.41)	(0.99)	(0.33)	(0.39)	(1.28)	(0.52)	(0.60)
	[0.20]	[0.59]	[0.86]	[0.01]	[0.03]	[0.13]	[0.62]	[0.62]	[0.08]	[0.42]	[0.43]
General High-School	-0.208	-0.250	-0.182	0.009	0.288	0.162	0.053	0.076	-0.013	0.226	0.246
	(0.40)	(0.55)	(0.40)	(0.09)	(1.52)	(0.83)	(0.30)	(0.42)	(0.02)	(0.51)	(0.56)
	[0.65]	[0.55]	[0.63]	[0.89]	[0.03]	[0.29]	[0.68]	[0.58]	[0.98]	[0.52]	[0.48]
Mixing*General High	0.164	0.143	0.153	0.254	0.021	0.076	0.094	0.086	0.020	-0.004	-0.012
School	(0.88)	(0.88)	(0.93)	(2.11)	(0.13)	(0.45)	(0.61)	(0.56)	(0.12)	(0.03)	(0.08)
	[0.27]	[0.25]	[0.20]	[0.01]	[0.88]	[0.53]	[0.40]	[0.46]	[0.90]	[0.96]	[0.91]
Age	-3.234	-3.797	-3.573	-1.082	-1.645	-1.576	-1.855	-1.911	-1.564	-1.926	-2.026
	(1.69)	(2.01)	(1.94)	(0.98)	(1.45)	(1.38)	(1.65)	(1.69)	(1.31)	(1.69)	(1.75)
$Age^2/10$	1.211	1.388	1.307	0.441	0.647	0.621	0.706	0.726	0.621	0.721	0.756
	(1.75)	(2.04)	(1.97)	(1.11)	(1.58)	(1.50)	(1.73)	(1.77)	(1.47)	(1.76)	(1.82)
$Age^3/100$	-0.199	-0.223	-0.210	-0.078	-0.111	-0.107	-0.118	-0.121	-0.107	-0.118	-0.124
	(1.82)	(2.07)	(2.01)	(1.23)	(1.71)	(1.62)	(1.82)	(1.86)	(1.62)	(1.82)	(1.89)
$Age^4/1000$	0.012	0.013	0.012	0.005	0.007	0.007	0.007	0.007	0.007	0.007	0.007
	(1.88)	(2.11)	(2.05)	(1.35)	(1.84)	(1.74)	(1.91)	(1.95)	(1.75)	(1.88)	(1.96)
Private High School	0.022	0.053	0.043		0.028	0.042	0.051	0.053	0.041	0.054	0.056
	(0.29)	(0.81)	(0.64)		(0.46)	(0.67)	(0.91)	(0.94)	(0.64)	(0.95)	(0.98)
Log(Year of High	-1.059	-0.796	-0.502		-0.736	-0.698	-0.557	-0.575	-0.939	-0.441	-0.462
School Entrance)	(1.72)	(1.50)	(0.88)		(2.66)	(2.50)	(2.19)	(1.97)	(1.49)	(0.83)	(0.86)
Teachers with 15+	-0.001	0.000	-0.003		-0.003	-0.003	-0.003	-0.002	0.002	-0.003	-0.003
Years of Education	(0.05)	(0.04)	(0.33)		(0.44)	(0.41)	(0.37)	(0.34)	(0.15)	(0.30)	(0.29)
Teachers with 1st	0.002	0.002	0.002		0.001	0.001	0.001	0.001	0.005	0.001	0.001
Degree License	(0.27)	(0.32)	(0.28)		(0.37)	(0.33)	(0.39)	(0.40)	(0.65)	(0.16)	(0.17)
Class Size	0.061	0.045	0.025		-0.037	-0.017	-0.024	-0.028	0.016	-0.052	-0.056
	(0.52)	(0.44)	(0.24)		(0.99)	(0.45)	(0.68)	(0.80)	(0.17)	(0.65)	(0.70)
Father's Education	0.014	0.004	0.003			0.013	0.005	0.004	0.016	0.004	0.003
	(1.66)	(0.62)	(0.43)			(1.91)	(0.78)	(0.64)	(1.82)	(0.54)	(0.44)
Years of Own		0.091	0.093				0.084	0.082		0.082	0.080
Education		(6.41)	(6.25)				(6.99)	(6.45)		(6.21)	(5.68)
Avg Earnings Index			-0.510					0.048			0.055
at Market Entrance			(1.09)					(0.17)			(0.20)
Intercept	31.14	36.75	35.99	9.767	19.66	17.59	19.88	20.68	15.53	22.25	23.47
	(1.77)	(2.10)	(2.09)	(0.87)	(1.66)	(1.47)	(1.69)	(1.75)	(1.06)	(1.69)	(1.72)
Number of Obs.	553	553	553	844	781	752	752	752	752	752	752
Number of Persons	153	153	153	239	217	209	209	209	209	209	209

Note : Asymptotic t-statistics are in parentheses. In square brackets are bootstrap p-values (Bertrand et al.(2004)).
The high school region dummies are also included in the regressions, but their coefficient estiamtes are not shown.

Table 5: Quantile Regression Estimates for Earnings: Specification S5

Quantiles :	Average	0.10	0.25	0.50	0.75	0.90	Average	0.10	0.25	0.50	0.75	0.90
Estimation Method :	(A) BA Setup											
	10-year Window						14-year Window					
Mixing	0.216	0.381	0.117	0.109	-0.050	-0.109	0.237	0.446	0.205	0.073	0.066	-0.024
	(1.30)	(1.74)	(0.85)	(0.93)	(0.36)	(0.78)	(1.55)	(2.25)	(1.50)	(0.64)	(0.54)	(0.13)
Estimation Method :	(B) DD Setup											
	10-year Window						14-year Window					
Mixing	-0.061	-0.117	-0.213	-0.066	-0.115	-0.142	0.056	0.184	-0.061	-0.015	-0.011	-0.104
	(0.35)	(0.50)	(1.70)	(0.60)	(0.82)	(0.85)	(0.39)	(0.84)	(0.46)	(0.15)	(0.11)	(0.69)
General High-School	0.037	0.071	0.147	0.067	-0.002	0.029	0.076	0.166	-0.026	0.124	0.104	-0.009
	(0.17)	(0.26)	(0.64)	(0.38)	-(0.01)	(0.14)	(0.42)	(0.62)	(0.11)	(0.70)	(0.76)	(0.04)
Mixing*General High School	0.202	0.416	0.325	0.123	0.178	0.027	0.086	0.161	0.181	0.072	0.071	0.084
	(1.15)	(2.11)	(2.24)	(1.00)	(1.36)	(0.15)	(0.56)	(0.77)	(1.10)	(0.59)	(0.58)	(0.54)
Estimation Method :	(C) DD Setup: 2SLAD											
	10-year Window						14-year Window					
Mixing	0.021	-0.175	-0.209	-0.062	-0.085	-0.18	0.094	-0.190	-0.131	0.030	-0.062	-0.087
	(0.11)	(0.62)	(1.08)	(0.42)	(0.62)	(1.06)	(0.60)	(0.74)	(0.73)	(0.28)	(0.51)	(0.48)
General High-School	-0.182	1.077	0.124	-0.006	0.052	0.368	0.246	1.242	0.344	-0.059	0.236	1.097
	(0.40)	(1.51)	(0.20)	(0.02)	(0.19)	(0.82)	(0.56)	(2.38)	(0.61)	(0.17)	(0.79)	(2.27)
Mixing*General High School	0.153	0.193	0.276	0.128	0.145	0.012	-0.012	0.138	0.147	0.063	0.070	-0.045
	(0.93)	(0.88)	(2.10)	(1.05)	(1.33)	(0.08)	(0.08)	(0.78)	(1.17)	(0.55)	(0.57)	(0.31)

Note : Asymptotic t-statistics are in parentheses. In the panel (C) the bootstrap procedure with 100 replications are used to obtain the t-statistics. The estimates of the other coefficients are suppressed, which are available upon request.

Table 6: Linear Probability Model Estimates for Becoming a Wage Earner

Estimation Design:	BA Setup					DD Setup				
Specifications :	(S1)	(S2)	(S3)	(S4)	(S5)	(S1)	(S2)	(S3)	(S4)	(S5)
	6-year Window (N=349)					6-year Window (N=620)				
Mixing	-0.049	-0.053	-0.022	-0.044	-0.042	-0.006	0.010	0.110	0.099	0.100
	(0.38)	(0.23)	(0.09)	(0.18)	(0.16)	(0.05)	(0.04)	(0.47)	(0.43)	(0.43)
General High-School						0.157	0.187	0.214	0.270	0.253
						(1.37)	(0.63)	(0.68)	(0.87)	(0.80)
Mixing*General High School						0.019	-0.032	-0.066	-0.082	-0.066
						(0.12)	(0.15)	(0.29)	(0.36)	(0.29)
	10-year Window (N=637)					10-year Window (N=1,064)				
Mixing	0.027	-0.164	-0.105	-0.128	-0.111	0.050	-0.088	-0.018	-0.042	-0.019
	(0.24)	(0.92)	(0.57)	(0.69)	(0.58)	(0.44)	(0.57)	(0.11)	(0.26)	(0.11)
General High-School						0.118	0.037	-0.055	-0.057	-0.064
						(1.28)	(0.19)	(0.26)	(0.27)	(0.31)
Mixing*General High School						-0.049	-0.013	-0.019	-0.027	-0.027
						(0.41)	(0.08)	(0.11)	(0.16)	(0.16)
	14-year Window (N=836)					14-year Window (N=1,396)				
Mixing	0.031	-0.075	-0.039	-0.073	-0.066	0.064	0.047	0.090	0.074	0.084
	(0.29)	(0.47)	(0.24)	(0.45)	(0.41)	(0.63)	(0.37)	(0.68)	(0.56)	(0.63)
General High-School						0.084	0.012	-0.074	-0.077	-0.094
						(1.04)	(0.08)	(0.45)	(0.47)	(0.57)
Mixing*General High School						-0.067	-0.046	-0.054	-0.069	-0.065
						(0.66)	(0.33)	(0.37)	(0.48)	(0.45)

Note : Asymptotic t-statistics are in parentheses. The estimates of the other coefficients are suppressed, which are available upon request.

Appendix Table 1: General and Vocational High School Qualities

(Unit: Proportion, Level)

	General High School						Vocational High School					
	Teachers with 15+ Years Education		Teachers with 1st Degree Licence		Class Size		Teachers with 15+ Years Education		Teachers with 1st Degree Licence		Class Size	
Year	Nation	Seoul -Busan	Nation	Seoul -Busan	Nation	Seoul -Busan	Nation	Seoul -Busan	Nation	Seoul -Busan	Nation	Seoul -Busan
1965	0.840[1)]	0.654[1)]	0.133[1)]	0.100[1)]	59.8	60.1	0.840[1)]	0.654[1)]	0.133[1)]	0.100[1)]	53.5	56.1
1966	0.821	0.824	0.153	0.131	59.6	60.4	0.842	0.846	0.184	0.086	52.4	56.9
1967	0.827	0.856	0.180	0.160	60.0	60.2	0.831	0.840	0.225	0.103	52.2	57.3
1968	0.830	0.852	0.221	0.203	59.4	60.2	0.848	0.789	0.246	0.120	53.3	57.5
1969	0.815	0.779	0.284	0.301	59.4	60.3	0.810	0.813	0.262	0.122	54.6	57.5
1970	0.833	0.889	0.277	0.257	60.1	60.7	0.851	0.814	0.266	0.122	56.1	58.6
1971	0.873	0.899	0.298	0.282	60.2	60.4	0.851	0.876	0.281	0.138	56.7	58.8
1972	0.866	0.886	0.307	0.296	60.7	60.7	0.845	0.890	0.290	0.146	57.0	58.4
1973	0.865	0.872	0.299	0.289	60.9	61.0	0.890	0.897	0.296	0.145	57.3	58.3
1974	0.894	0.909	0.305	0.309	60.6	60.6	0.900	0.904	0.214	0.140	57.3	58.1
1975	0.922	0.932	0.340	0.343	59.8	60.3	0.877	0.909	0.228	0.153	57.0	57.4
1976	0.911	0.958	0.366	0.380	59.2	59.9	0.886	0.898	0.242	0.170	56.9	57.0
1977	0.913	0.932	0.404	0.410	59.0	60.0	0.877	0.890	0.262	0.189	57.3	57.8
1978	0.908	0.933	0.434	0.450	59.5	60.4	0.887	0.885	0.296	0.226	58.5	59.2
1979	0.915	0.942	0.461	0.502	59.8	60.5	0.877	0.847	0.308	0.237	59.1	59.9
1980	0.913	0.939	0.487	0.533	59.9	60.5	0.852	0.853	0.335	0.271	58.7	59.4
1981	0.912	0.936	0.510	0.563	59.6	58.8	0.877	0.864	0.345	0.280	58.4	58.9
1982	0.920	0.940	0.527	0.583	59.2	60.2	0.862	0.877	0.343	0.294	57.5	58.6
1983	0.921	0.949	0.532	0.587	59.1	60.3	0.875	0.888	0.344	0.302	56.8	58.5
1984	0.933	0.952	0.547	0.583	58.7	60.3	0.884	0.895	0.341	0.279	56.3	58.4
1985	0.937	0.953	0.551	0.569	58.0	59.5	0.891	0.885	0.339	0.272	55.5	57.8
1986	0.936	0.949	0.557	0.567	57.3	58.7	0.903	0.902	0.337	0.266	54.8	57.0
1987	0.942	0.950	0.563	0.567	56.8	58.6	0.909	0.912	0.330	0.253	54.3	56.8
1988	0.947	0.957	0.563	0.568	56.4	58.4	0.919	0.915	0.321	0.251	54.0	56.7
1989	0.957	0.964	0.570	0.571	55.3	57.6	0.927	0.924	0.319	0.251	53.2	56.0
1990	0.958	0.963	0.565	0.576	53.6	55.6	0.937	0.936	0.313	0.248	51.5	54.1

Source: *Yearbook of Educational Statistics*, Various Years

Note : 1) Figures are the proportions for general and vocational high schools as a whole.

Appendix Table 2: Descriptive Statistics of the Sample: The 14-year-window BA and DD

	General High School				Vocational High School			
Variables	Total	Pre-Mixing	Post-Mixing	Post/Pre Difference	Total	Pre-Mixing	Post-Mixing	Post/Pre Difference
Attendance during Post-mixing Period	0.66				0.57			
	(0.47)				(0.50)			
Log(Earnings)	7.500	7.500	7.500	0.000	7.298	7.392	7.227	-0.165
	(0.466)	(0.504)	(0.445)	(0.045)	(0.440)	(0.439)	(0.428)	(0.048)
Log(Adjusted Earnings)	0.098	0.092	0.101	0.009	-0.106	-0.006	-0.181	-0.176
	(0.465)	(0.507)	(0.443)	(0.045)	(0.443)	(0.445)	(0.427)	(0.049)
Log(Hourly Wages)	9.005	9.015	9.000	-0.015	8.799	8.900	8.723	-0.177
	(0.517)	(0.541)	(0.505)	(0.049)	(0.476)	(0.488)	(0.453)	(0.053)
Log(Adjusted Wages)	-5.304	-5.300	-5.306	-0.007	-5.513	-5.406	-5.594	-0.187
	(0.519)	(0.547)	(0.505)	(0.050)	(0.476)	(0.492)	(0.449)	(0.053)
Monthly Working Hours	225.5	223.0	226.8	3.8	227.5	224.6	229.8	5.1
	(39.7)	(37.9)	(40.6)	(3.6)	(44.1)	(39.3)	(47.4)	(4.7)
Proportion of Wage-Earners	0.634	0.642	0.668	0.025	0.659	0.605	0.657	0.051
	(0.482)	(0.480)	(0.471)	(0.034)	(0.474)	(0.490)	(0.475)	(0.041)
Private High School	0.523	0.494	0.615	0.120	0.553	0.461	0.571	0.110
	(0.500)	(0.501)	(0.487)	(0.047)	(0.498)	(0.500)	(0.496)	(0.053)
Year of High School Entrance	1975.1	1971.0	1977.3	6.3	1974.5	1970.4	1977.5	7.1
	(3.6)	(2.0)	(1.9)	(0.2)	(4.1)	(1.9)	(2.0)	(0.2)
Age	40.2	44.5	38.0	-6.5	41.0	45.3	37.7	-7.5
	(4.4)	(2.9)	(3.3)	(0.3)	(4.8)	(2.5)	(3.5)	(0.3)
Years of Education	15.1	15.0	15.1	0.06	13.3	13.2	13.5	0.27
	(2.6)	(2.6)	(2.6)	(0.23)	(2.0)	(1.9)	(2.0)	(0.20)
Father's Education	9.92	10.11	9.83	-0.28	7.23	7.84	6.77	-1.07
	(4.5)	(4.6)	(4.5)	(0.4)	(4.5)	(4.7)	(4.4)	(0.5)
Father Present at 14	0.94	0.88	0.97	0.09	0.87	0.92	0.84	-0.08
	(0.24)	(0.32)	(0.17)	(0.02)	(0.33)	(0.27)	(0.37)	(0.03)
High School Region								
Seoul	0.54	0.48	0.57	0.09	0.49	0.42	0.55	0.13
	(0.50)	(0.50)	(0.50)	(0.04)	(0.50)	(0.49)	(0.49)	(0.05)
Busan	0.14	0.12	0.15	0.03	0.32	0.43	0.24	-0.19
	(0.34)	(0.33)	(0.35)	(0.02)	(0.47)	(0.50)	(0.43)	(0.05)
Daegu	0.12	0.16	0.09	-0.06	0.09	0.04	0.12	0.08
	(0.32)	(0.37)	(0.29)	(0.03)	(0.28)	(0.20)	(0.33)	(0.03)
Inchon	0.13	0.19	0.09	-0.09	0.10	0.11	0.09	-0.01
	(0.33)	(0.39)	(0.29)	(0.03)	(0.30)	(0.31)	(0.29)	(0.03)
Kwangju	0.08	0.05	0.10	0.05	0.01	0.01	0.00	0.01
	(0.28)	(0.23)	(0.30)	(0.02)	(0.07)	(0.01)	(0.00)	(0.01)
Number of Obs.	522	177	345		396	171	225	
Number of Persons	146	49	97		137	61	76	

Note: Standard deviations or standard errors are in parentheses.

Appendix Table 3: The First-Stage Estimation Results for General High School Choice

Dependent Variable :	General High School		
Window Size :	6-Year	10-Year	14-Year
Mixing	-0.158	-0.183	-0.138
	(2.45)	(2.63)	(2.38)
Father Present at 14	-0.005	-0.045	-0.087
	(0.18)	(1.32)	(2.41)
Mixing*	0.184	0.384	0.312
Father Present at 14	(3.08)	(5.83)	(5.19)
Age	4.616	-2.048	0.714
	(1.84)	(2.11)	(0.91)
$Age^2/10$	-1.863	0.758	-0.196
	(1.89)	(2.19)	(0.69)
$Age^3/100$	0.328	-0.123	0.021
	(1.93)	(2.27)	(0.46)
$Age^4/1000$	-0.021	0.007	-0.001
	(1.97)	(2.36)	(0.23)
Private High School	-0.034	-0.007	0.003
	(1.64)	(0.40)	(0.18)
Log(Year of High	-0.183	-1.138	-1.246
School Entrance)	(1.04)	(7.96)	(10.5)
Teachers with 15+	0.005	0.012	0.019
Years of Education	(0.58)	(3.58)	(6.70)
Teachers with 1st	0.003	0.010	0.012
Degree License	(1.60)	(4.44)	(7.48)
Class Size	0.277	0.229	0.180
	(32.1)	(15.3)	(17.4)
General/Vocational Differences			
Teachers with 15+	2.699	-0.200	-0.534
Years of Education	(6.31)	(0.73)	(2.36)
Teachers with 1st	0.135	0.332	0.654
Degree License	(0.60)	(1.69)	(3.00)
Class Size	-0.060	-0.050	-0.013
	(1.20)	(2.17)	(0.99)
Father's Education	0.008	0.004	0.011
	(3.33)	(2.63)	(6.21)
Years of Own Education	-0.001	0.012	0.017
	(0.39)	(3.29)	(5.06)
Avg Earnings Index	-0.209	-0.125	-0.066
at Market Entrance	(1.57)	(0.95)	(0.72)
Intercept	-57.38	9.220	-18.02
	(2.40)	(0.90)	(2.20)
Number of Obs.	321	562	764
R-Square	0.924	0.866	0.826
F (Instruments Excluded from the 2nd Stage)	22.4	9.3	10.2

Note : Asymptotic t-statistics are in parentheses. The dummy variables of high school regions are also included in the regressions, whose estimates are supressed.

Appendix Table 4: Estimation Results Using Alternative Setups with Specification S5.

Estimation Design:	BA Setup					DD Setup (RE)					DD Setup (IV)				
Setup:	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	6-Yr Window (N=202)					6-Yr Window (N=316)					6-Yr Window (N=316)				
Mixing	0.257	0.259	0.366	0.128	0.311	-0.068	-0.041	0.097	-0.085	0.030	0.062	0.069	0.171	0.044	0.075
	(1.24)	(1.27)	(1.23)	(0.60)	(1.07)	(0.29)	(0.17)	(0.29)	(0.40)	(0.09)	(0.26)	(0.29)	(0.52)	(0.21)	(0.33)
General High-School						0.133	0.146	0.257	0.073	0.244	-0.329	-0.285	-0.319	-0.576	-0.260
						(0.48)	(0.53)	(0.84)	(0.26)	(0.82)	(0.65)	(0.57)	(0.45)	(0.99)	(0.53)
Mixing*General High School						0.222	0.208	0.170	0.229	0.194	0.130	0.125	0.106	0.128	0.124
						(1.05)	(0.98)	(0.77)	(1.19)	(0.90)	(0.62)	(0.59)	(0.48)	(0.68)	(0.61)
	10-Yr Window (N=355)					10-Yr Window (N=553)					10-Yr Window (N=553)				
Mixing	0.216	0.214	0.397	0.098	0.346	-0.061	-0.049	0.130	-0.090	0.116	0.021	0.034	0.221	-0.065	0.061
	(1.30)	(1.29)	(1.36)	(0.62)	(1.19)	(0.35)	(0.28)	(0.43)	(0.54)	(0.38)	(0.11)	(0.19)	(0.73)	(0.38)	(0.34)
General High-School						0.037	0.038	0.106	0.034	0.133	-0.182	-0.275	0.207	0.069	-0.163
						(0.17)	(0.17)	(0.44)	(0.16)	(0.56)	(0.40)	(0.62)	(0.48)	(0.16)	(0.36)
Mixing*General High School						0.202	0.199	0.181	0.197	0.189	0.153	0.165	0.065	0.155	0.156
						(1.15)	(1.13)	(0.96)	(1.17)	(1.01)	(0.93)	(0.99)	(0.37)	(1.00)	(0.96)
	14-Yr Window (N=464)					14-Yr Window (N=752)					14-Yr Window (N=752)				
Mixing	0.237	0.237	0.409	0.181	0.359	0.056	0.054	0.223	0.044	0.222	0.094	0.123	0.289	0.027	0.167
	(1.55)	(1.54)	(1.44)	(1.21)	(1.28)	(0.39)	(0.36)	(0.75)	(0.32)	(0.75)	(0.60)	(0.72)	(0.98)	(0.19)	(1.07)
General High-School						0.076	0.095	0.126	0.066	0.154	0.246	0.022	0.515	0.467	0.239
						(0.42)	(0.53)	(0.66)	(0.38)	(0.81)	(0.56)	(0.05)	(1.21)	(1.12)	(0.54)
Mixing*General High School						0.086	0.084	0.095	0.088	0.091	-0.012	0.002	-0.029	0.027	-0.027
						(0.56)	(0.54)	(0.58)	(0.60)	(0.55)	(0.08)	(0.01)	(0.18)	(0.19)	(0.18)

Note : Asymptotic t-statistics are in parentheses. The estimates of the other coefficients are suppressed, which are available upon request.
(1) The original setup.
(2) Linear time trend is used instead of log time trend.
(3) Entrance year dummies are used instead of log time trend.
(4) Estimations are done using the average values of the variables observed over the years for individuals.
(5) Log monthly earnings instead of (log monthly earnings - log average national monthly earnings) are used as the dependent variable.