

## Labor Market Effects of School Ties: Evidence from Graduates of Leveled High Schools in South Korea

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*Before 1974, general high schools in Korea were stratified by a selective admission system. Starting from 1974, it was replaced by a leveling system in which students were randomly assigned to a school within a district. This paper examines whether the quality of school ties formed by senior graduates of the selective system affects contemporary outcomes of junior graduates randomly assigned to the same school under the leveling system. The quality of school ties does not strongly affect the average junior graduate's outcomes. In contrast, it benefits junior graduates who rank high in the outcome distribution.*

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### I. Introduction

Can anyone expect a benefit of graduating from an educational institution whose alumni networks are of high quality? Do Harvard graduates, for instance, enjoy tangible benefits in the labor market, compared with graduates from a mediocre university? If so, how much of these benefits can be attributed to effects of the alumni networks? Sociology and education literatures have extensively explored effects of school ties and alumni networks on socioeconomic outcomes (Solomon 1975; Collins 1979; Useem and Karabel, 1986; Kingston and Smart, 1990; Lee and Brinton, 1996; Ishida et al., 1997; Buerkle and Guseva, 2002; Kim and Park, 2004;

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Fujimoto, 2005; Kim and Cannella, 2008; Martin, 2009). Economists have just begun to show academic interests in these issues (Manski, 2000; Jackson, 2009, 2010; Blume et al., 2010; Cohen et al., 2008, 2010). Economists' relatively lukewarm interests in school ties are mainly due to their serious concerns about endogeneity and difficulties in drawing causal effects of school ties via empirical analysis. In fact, it is a daunting challenge to empirically establish causality from school ties to an individual's economic and social outcomes. To what extent Harvard graduates' exceptional performance in the labor market can be attributed to the effects of the alumni networks, independent of other confounding factors? How can we separate effects of the alumni networks from different kinds of effects such as impacts of graduate's individual (cognitive and non-cognitive) capabilities, educational support of highly able professors and administrators, the school's favorable educational environment?

A conventional strategy to address such concerns is an OLS method whereby an outcome variable (e.g., earnings, employment in a good job, income, etc.) is regressed against a measure of the quality of school networks of an individual together with a set of personal and school-related characteristics (Lee and Brinton, 1996; Ishida et al., 1997; Buerkle and Guseva, 2002; Fujimoto, 2005; Martin, 2009).<sup>1</sup> However, economists cast serious doubts on methodological assumptions underpinning these studies. It is very difficult, if not impossible, to disentangle impacts of school ties by simply controlling for measured variables in cross-sectional regressions, because this method is likely to omit unobservable and unmeasurable determinants of an individual's economic performance.

In order to remedy analytical pitfalls and to draw a causal estimate of the effects of school ties, an experiment is desired that randomly assigns a quality of school ties to individuals. With this methodological assumption in mind, we believe that the case of South Korea (Korea hereafter) offers a unique opportunity for an analysis of school ties as it allows us to employ random assignment of students to high schools. Korea underwent a drastic change in the school assignment system as the Korean government suddenly introduced the so-called "Leveling Policy" in 1974 and maintained this policy throughout the 1980s (even nowadays in the revised form). Under the Leveling Policy that kicked off in 1974, general (i.e., non-vocational) high school students in Korea were randomly assigned by a lottery to a high school within a residence-based school district. Once assigned, students were not permitted to change high schools within the same district. Those who were allowed to transfer

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<sup>1</sup> Accounting for unusual features (e.g., discreteness, censoring, ordering, etc.) of the outcome variable, a study may use alternative empirical methods such as binary choice models, durations models, limited dependent variable models, etc. Nonetheless, current discussions made for linear OLS regression models are valid for such other empirical methods. For a more detailed evaluation of conventional empirical methods of social capital and social interactions, see Manski (2000) and Durlauf (2002).

to different school districts due to residential relocations were again randomly assigned to a school in the new district.<sup>2</sup> Before the Leveling Policy, however, each general high school admitted only students who passed the entrance exam administered by individual high schools. As a result, there was a clear nation-wide hierarchy of general high schools, primarily based on the graduates' performance in college entrance exams.

We take advantage of this institutional change to draw causal effects of school ties. Specifically, we simply relate contemporary labor market performances of junior graduates who were randomly assigned to a high school to the quality of alumni networks of senior graduates who had entered that school before 1974. We observe contemporary labor market performances of junior graduates from ordinary household data. In order to measure a high school's quality of school ties, we count the total number of board members (executives) in the firms listed in the Korea Stock Exchange. The number of such executives provides a crucial clue to examine which high schools performed well in the pre-leveling regime as well as which schools possess high quality school ties in the current labor market. By relating such a measure of school ties to current labor market performances of junior graduates randomly assigned, we can demonstrate whether junior graduates benefit from school ties of senior graduates, specifically who the beneficiaries are, and how much.

Our analysis reveals that the quality of school ties does not strongly affect the average junior graduate's labor market outcomes. The OLS estimates show that a doubling of the number of senior executive-graduates (e.g., from 20 to 40) increases the average junior graduate's monthly earnings and hourly wages just by 0.7 percent and 1.0 percent, respectively, which are statistically indistinguishable from zero. In addition, the number of senior executive-graduates does not significantly affect a junior graduate's chance of being employed in a regular (as opposed to temporary) job or in a large-sized firm. By contrast, quantile regression estimates suggest that high-quality school ties do benefit junior graduates who rank high in the outcome distribution. A doubling of the number of senior executive-graduates increases the 0.9 quantile of the monthly earnings (hourly wages) distribution by as much as 4.3 (7.7) percent. Robustness checks suggest that such estimates do not seem to be drawn from mere sorting of highly able students to formerly prestigious high schools.

To the best of our knowledge, this paper is the first systematic study on school ties exploiting a quasi-experiment of randomization. In addition to credibly

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<sup>2</sup> For the lottery system of the Leveling Policy and an overview of secondary education in Korea, see Marlow-Ferguson and Lopez (2001, South Korea) and OECD (1998, Chapters 1 and 2). Institutional details relevant to this study will be explained in section 3. The random assignment of students under the Leveling Policy of Korea has been exploited to examine some issues in education research. For instance, Kang et al. (2007) investigate the impact of ability grouping (as opposed to mixing) on adulthood earnings, and Kang (2007) looks into the structure of classroom peer interactions.

measuring causal effects of school ties based on randomization, this study makes new contributions to the existing studies on school ties in several ways. First, there is a recent surge of the analysis of social interactions and social networks in the economics literature.<sup>3</sup> While economists have long been interested in these issues, convincing empirical evidence on causal effects of social networks is relatively scarce. Topa (2001), Munshi (2003), Weinberg et al. (2004), Laschever (2007), Bayer et al. (2008), Hellerstein et al. (2008) and Beaman (2012) are a few recent attempts to offer evidence on how social networks affect an individual's labor market outcomes. Building on empirical studies, this paper exploits random assignment of high school students to better understand the causal impacts of one dimension of social networks, i.e., school ties.

Second, burgeoning researches empirically demonstrate the importance of school ties in economic outcomes. They show the impacts of school ties on investment performances of fund managers (Cohen et al., 2008), recommendation performances of stock analysts (Cohen et al., 2010), compensation levels of high-ranking executives of firms (Hwang and Kim, 2009; Engelberg, Gao and Parsons, 2010; Butler and Gurun, 2012), favorable loan interest rates from banks (Engelberg, Gao and Parsons, 2012). Unlike these studies that shed light on effects of school networks in the financial sector, our study broadens the research scope by analyzing the functioning of school ties in the ordinary labor market.<sup>4</sup>

Third, most of the studies on school ties focus on the networks of post-secondary education institutions (Useem and Karabel, 1986; Lee and Brinton, 1996; Ishida et al., 1997; Chiavacci, 2005; Cohen et al., 2008, 2010). Whether school ties formed by *high school* graduates exert similar impacts calls for further investigations. In particular, the roles of high school alumni networks have been intensely debated in Korea (Kim and Park, 2004; Kim and Cannella, 2008; Kim and Kim, 2008). Several studies point out that school ties—especially high school ties—are a source of cronyism in Korea (Krugman, 1998; Moon and Mo, 2000; Wei and Wu, 2001; Haggard et al., 2003). This study critically examines the validity of such an

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<sup>3</sup> Manski (2000), Jackson (2009, 2010), Blume et al. (2010) and Epplé and Romano (2010) comprehensively review theoretical and empirical literatures.

<sup>4</sup> Important examples that explicitly address endogeneity of social networks in the ordinary labor market are Munshi (2003) and Beaman (2012). Munshi (2003) examines impacts of geographic networks on Mexican migrant workers in the U.S. labor market. By using rainfall as an instrument variable, Munshi demonstrates that Mexican migrants with larger networks are more likely to be employed as well as hired in a higher paying non-agricultural position. To analyze effects of social networks on the labor market outcomes of refugees resettled in the U.S., Beaman (2012) exploits the fact that these refugees are distributed across cities by a resettlement agency, precluding individuals from sorting. She demonstrates that an increase in the number of social network members resettled in the same year or one year prior to a new arrival leads to a deterioration of outcomes, while a greater number of tenured network members improves the probability of employment and raises the hourly wage.

argument by offering credible estimates for the role of high school ties.

Fourth, existing studies on school ties are in general interested in an average effect of school ties, or alternatively an effect of school ties on the average individual. We extend these studies by exploring how the impact of school networks varies by characteristics of individuals. For instance, school networks formed by senior graduates in the top echelon of companies are likely to benefit only junior graduates whose performances are commensurate with their related seniors. School ties are likely to exert little influence on junior graduates whose performance is average or below. In order to address potential heterogeneity of the effect of school ties, this study employs a quantile regression method. We demonstrate that the impact of school ties indeed differs by an individual's location in the distribution of the labor market performance.

The remainder of the paper proceeds as follows. In section II we review theoretical explanations and empirical examinations for roles of school ties; in section III we introduce institutional backgrounds of the Leveling Policy in Korea. The empirical method is explained in section IV; the data are discussed in section V. Estimation results are presented in section VI. The paper concludes in section VII.

## II. Literature Review

Conventional theories, primarily developed to explain personnel connections in the western world, touch upon school networks from various angles.<sup>5</sup> First, one strand of existing explanations is interested in explicating the reasons why school networks are created and preponderant, once created. Drawing on insights from the transaction cost theory (Williamson, 1991), this explanation posits that school networks emerge because they significantly reduce costs between workers and firms (Arrow, 1973; Spence, 1973; Carrington et al., 1996). In addition to formal channels such as job advertisement, job applicants tend to use various informal job referral networks, ranging from school placement offices to alumni networks (Montgomery, 1991; Ioannides and Loury, 2004). High job-searching costs encourage job applicants to utilize school networks as a source of information gathering about newly available jobs.

From a perspective of employers as well, school networks may serve as a useful means to screen job applicants' abilities in the recruiting process (Cornell and Welch, 1996). For example, employee referrals are a useful device for screening job applicants. When they are unable to figure out job applicants' true abilities, employers tend to refer to incumbent workers who are likely to have more reliable information about job applicants (Ishida et al., 1997). In this way, school or alumni

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<sup>5</sup> We use school ties, school networks and alumni networks interchangeably in this paper.

networks allow firms to reduce costs related to screening suitable workers (Brinton and Kariya, 1998). Under the circumstance, workers who possess extraordinary school ties are likely to fare better than workers with inferior networks.

Second, school networks may serve as a mechanism of disciplining workers and securing their loyalty to their firms (Marsden et al., 1990). School networks may reduce social loafing and opportunism within the company, as those behaviors can be quickly revealed to other companies through school networks. This function will ultimately facilitate trust and cooperation among corporate members. Empirical studies show that CEOs tend to recruit board members with the same academic background for this reason, giving rise to 'homophily' in the board of directors (Hillman et al., 1999).

By contrast, effects of school network could be negative on economic outcomes. If a company recruits employees via school ties with the top-level decision makers rather than via their functional expertise, it may increase the risk of adverse selection or reverse discrimination (Shin and Chin 1989). From the firm's perspective, this type of homophily is likely to result in the loss of efficiency, creating cronyism (Krugman, 1998; Wei and Wu, 2001). Although the firm may still enjoy the luxury of such homophily in normal times, this type of homophily is less likely to continue under aggravating business environments or in the highly competitive market situation (Aldrich, 1979).<sup>6</sup>

Despite apparent theoretical possibilities, a majority of existing empirical studies fall short of establishing the causal relationship between school ties and graduates' outcomes, leaving a key question about the effects of school ties unanswered. For example, demonstrating that graduates of top-ranking high schools or universities are highly over-represented in board of directors in companies listed in the Korea Stock Exchange, Kim and Kim (2008) argue that school networks is the factor facilitating such over-representation. To explore the way in which school networks operate, Lee and Brinton (1996) carry out an OLS regression and conclude that graduates of top-tier universities in Korea are more likely to benefit from school networks by way of placement offices, professors, or alumni. Useem and Karabel (1986) and Ishida (1993) make similar arguments for the U.S. and Japanese cases, respectively. Even after close examinations of the highly concentrated composition of board members, however, we are not still assured whether benefits enjoyed by graduates of prestigious schools stem solely from a school's networks independent of

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<sup>6</sup> In a similar vein, firms tend to develop diverse networks to address high external uncertainty. The firm that recruits employees with the same school ties may be less effective in dealing with external uncertainties. Firms with graduates of a variety of schools are more likely to take advantage of diversified networks to find a means to cope with unforeseen external uncertainties. By contrast, the firm with identical yet limited school networks may find itself unable to mobilize diverse networks (Kim and Kim, 2008). This disadvantage may turn out to be fatal at the time of crisis when the firm's survival is at stake.

the graduates' individual ability, or the school's educational advantages.

Understanding difficulties in uncovering causal impacts of school ties, recent studies on school ties in the financial sector employ a fixed effects approach based on panel data (Cohen et al., 2008, 2010; Engelberg et al., 2010, 2012; Butler and Gurun, 2012). These studies attempt to draw the causal effect of school networks based on supposedly random variation of matching in alma mater between fund managers and company executives over time. For example, Butler and Gurun (2012) show that mutual funds whose managers are in the same school network as the firm's CEO are more likely to vote against limiting executive compensation than out-of-network funds; CEOs of firms with higher levels of educationally connected mutual fund ownership are found to have higher levels of compensation than their unconnected counterparts. Engelberg et al. (2010) present that CEOs with the greater number of educationally connected executives and directors in other firms earns more than those with small networks. By the design of the variable 'connectedness', higher connectedness in both studies means graduation from more prestigious schools (See Table 1 of Butler and Gurun (2012) and Figure 1 of Engelberg et al. (2010)). Since they control for firm-fund pair fixed effects (Butler and Gurun, 2012) or firm fixed effects (Engelberg et al., 2010), the identification of the effect of 'connectedness' results from within firm-fund pair variation in educational connectedness between fund managers and company CEOs or within firm variation in educational connectedness of the CEO. However, if there is a common trend in which graduates of a particular school emerge as a new group of power elites at the highest level, studies based on fixed effects are likely to overstate impacts of school networks. For example, if Yale graduates are replaced by Harvard graduates at the highest level of both financial and business sectors upon an inauguration of a new political leadership, a fixed effects approach based on matching in alma mater is likely to yield significant impacts of school ties even in the absence of actual interactions among alumni.

Without solid theoretical backgrounds and properly designed empirical tests, it would be somewhat hasty to conclude that school ties *cause* differences in outcomes in individuals' performance in terms of employment, promotion, or compensation. To overcome various problems of estimation, randomization of the quality of school ties is needed. An examination of the Leveling Policy of Korea offers a unique opportunity to establish causal effects of school ties based on such randomization.

### III. Institutional Backgrounds

To set up the subsequent empirical analysis, we here introduce institutional details of the Leveling Policy that worked in the 1970's and 1980's in Korea. The school system of Korea consists of three stages before college: elementary school

(grades 1 to 6), middle school (grades 7 to 9) and general or vocational high school (grades 10 to 12). Typical general high school graduates are expected to take an entrance exam to a college and, if successful, attend one, while vocational high school graduates are expected to find employment upon graduation as entry-level workers.

A historical turning point of the Korean high school system was 1974. Before 1974, middle school graduates who wanted to advance to high schools were required to take an entrance exam administered by each high school and rank high in that exam. Under this selective admission system, middle school graduates were sorted to general high schools according to their academic ability. A clear nationwide hierarchy thus existed among general high schools, primarily based on their seniors' performance in college entrance exams. A by-product of this admission system was that students were under enormous stress for school advancement and their parents sometimes experienced financial distress to hire private tutors for their children.

In the face of growing public criticism against the system, the Korean government unexpectedly announced in February 1973 the "Leveling Policy." The new system aimed to reform the general high school admission practice by eliminating unduly severe competition for elite high schools. Under the Leveling Policy, the system of school-level applications and entrance exams was replaced by the municipality-level applications and standardized exams that set a minimum requirement for an admission to general high school; applicants who passed this standardized exam would be *randomly* via a lottery assigned to a school in a pre-determined school district adjacent to the students' residential neighborhood.<sup>7</sup> However, practices of an admission to vocational high school would remain unchanged, since the competition was not so severe in that sector.

Starting with students entering general high school in March, 1974, the Leveling Policy was put into effect first in Seoul and Busan—the two largest cities in Korea. In 1975 the policy was extended to another three large cities (Daegu, Incheon and Kwangju). With a brief pause after 1975, it was then further extended to sixteen smaller urban municipalities in 1979, 1980, and 1981.<sup>8</sup> As of 1981, the Leveling Policy was enforced in the majority of urban regions including 23 cities and municipalities where about one half of all general high schools in Korea were

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<sup>7</sup> A regular school district was usually the size of a municipality; there existed a total of 179 high school districts in the nation. For example, the Seoul metropolitan city, in which there were a total of 148 general high schools (66 boys-only, 53 girls-only and 29 co-ed schools) and 112,382 incoming students in 1985, has nine school districts for student allocation (Yearbook of Educational Statistics, 1985). Each district on average contains 16.4 schools to which an average of 10,217 students were randomly assigned. As a result, each school was placed a total of 623 incoming students on average.

<sup>8</sup> These municipalities are Cheongju, Choonchun, Daejon, Jeju, Jeonju, Masan, Suwon (1979), Andong, Cheonan, Iksan, Jinju, Kunsan, Mokpo, Sunghnam, Wonju (1980), and Changwon (1981).



located (Chung, 1998). If a municipality was selected as a leveling region, every general high school including both public and private schools in that region was subject to the Leveling Policy. Private schools were made no exception by the government, because it heavily subsidized them and transformed them into de facto public schools in terms of administration, curriculum and educational environment under the Leveling Policy. If a municipality was not chosen as a leveling region, the old selective system was maintained. Such municipalities were largely located in rural regions.<sup>9</sup>

The rule of the Leveling Policy stipulated that applicants for general high school who passed the required threshold of the exam be assigned *randomly* to a school within a pre-determined school district. Only one school—either private or public—was assigned for each student. Transfer within the same district was not permitted. Students who were not satisfied with the assigned school might attempt to transfer to another school district. However, a new school was also to be randomly chosen in the new district, as long as the district was under the leveling regime.

Under the Leveling Policy, an individual was deemed randomly exposed to a quality of senior graduates who had entered the same high school prior to the leveling regime. Provided that senior graduates form school ties in the current period and that the quality of school ties is determined by that of those senior graduates, school ties are considered randomly treated to an individual in the current labor market who entered general high school under the leveling regime. In the following, we measure the quality of school ties by the number of senior graduates who, having entered a high school prior to the Leveling Policy, became board members in the firms listed in the Korea Stock Exchange as of 2004. Given that the number of these senior executive-graduates is randomly treated to an individual in the contemporary labor market, a difference in labor market performances between graduates of different high schools can be causally attributed to a difference in the quality of school ties.

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<sup>9</sup> For further details of the transition from a competition-based admission system to a leveling system, see Chung (1998), OECD (1998) and Kang et al. (2007). Since the mid-1970s, the Leveling Policy has become the most distinguished feature of the Korean high school system. Although several minor revisions have been made since mid-1990's, the Leveling Policy is still a primary characteristic of the educational scene of Korea. The revisions of mid-1990s do not affect the validity of our analysis, because we focus on students who entered general high school before 1990 in which the Leveling Policy was strictly implemented.

## IV. Empirical Framework

For empirical analysis we consider the following linear model expressed by:

$$y_i = \beta_0 + \ln ST_i \beta_1 + X_i \beta_2 + u_i \quad (1)$$

where  $y_i$  is individual  $i$ 's labor market performance measured by the natural log of monthly earnings or hourly wages, an employment in a regular position or in a large-sized firm,  $\ln ST_i$  is a quality of school ties as measured by the natural log of the number of senior executive-graduates of  $i$ 's high school<sup>10</sup>, and  $X_i$  is a vector of  $i$ 's measured characteristics that include the level of own education (either high school, 2-year college, top-level university, university in Seoul, or university outside Seoul), mother's education level and indicators for marital status, current residence region, employment in the government sector, the year of high school entrance, the school district and the survey year.  $u_i$  is the random error term.

Estimating equation (1) by OLS using ordinary cross-sectional data is not likely to yield a consistent estimator for  $\beta_1$  due to the endogeneity problem. For example, in the absence of the Leveling Policy, high-ability students tend to attend high quality schools in various respects including school ties. To the extent that a common factor such as a student's ability explains both the quality of school ties and the labor market performance of the student, an OLS estimate for  $\beta_1$  reveals nothing but a correlation between  $ST_i$  and  $y_i$ . To establish causality from  $ST_i$  to  $y_i$ , one requires a natural experiment in which a quality of school ties is randomly (or exogenously) assigned to an individual student. The Leveling Policy implemented in Korea from 1974 onward offers such a unique natural experiment.

Provided that  $ST_i$  is exogenously treated to a student under the Leveling Policy, an OLS estimate for  $\beta_1$  shows the average causal effect of  $ST_i$  on  $y_i$ , or alternatively the causal effect of  $ST_i$  on the average  $y_i$ . While an average causal effect is in itself of paramount interest, one may still wonder if the effect is heterogeneous for different individuals. To address such a concern, we run a quantile regression in which differential impacts of  $ST_i$  on  $y_i$  are estimated at different quantiles of the distribution of  $y_i$ . Instead of the sum of squared errors, a quantile regression minimizes the following sum of weighted absolute deviations:

$$\text{Min}_{\Gamma \in \mathbb{R}^K} \left\{ \sum_{i \in \{i: y_i \geq W_i \Gamma\}} \theta \cdot |y_i - W_i \Gamma| + \sum_{i \in \{i: y_i < W_i \Gamma\}} (1 - \theta) \cdot |y_i - W_i \Gamma| \right\} \quad (2)$$

<sup>10</sup> The natural log is taken in order to minimize an impact of outliers. To deal with zeros in the log transformation, a value of 1 is added to every individual's raw number of executive-graduates. When the raw number of executive-graduates is employed instead of the log-transformed value, however, the results are qualitative unaffected. They are in part discussed in section 6.2 and available upon request.

where  $\theta \in (0,1)$ ,  $W_i \equiv (1 \text{ } ST_i X_i)$  and  $\Gamma \equiv (\beta_0 \beta_1 \beta_2')$ . Quantile regression estimates of  $\hat{\beta}_1^\theta$  show the causal impact of  $ST_i$  on the  $\theta$  quantile of the  $y_i$ 's distribution.

## V. Data

### 5.1. Construction of the Analysis Sample

For analysis we merge three separate data sets of Korea. The first data are drawn from an administrative source to construct a measure of the quality of schools ties by the number of senior executive-graduates per school. The second and third data rely on household survey data to build information on junior graduates' high school records and labor market performances.

The first data set is an administrative data base compiled by the Korea Listed Companies Association in 2004. It covers a universe of the companies that are listed in the Korea Stock Exchange (KSE) in 2004.<sup>11</sup> Along with company-related information such as the company's name, value of the assets, total number of employees, the data (KSE-Executive data) contain information on personal attributes of each individual board member (executive) of the companies listed in the KSE. For example, the KSE-Executive data include an executive's year of birth, the names of high school and college attended, the calendar year of graduation, etc. The total number of executives covered by the KSE-Executive data is 6,420. In order to build a measure of the quality of school ties employed in this paper, we restrict the raw sample of the KSE-Executive data to the executives who are expected to have attended high school prior to the leveling regime based on the year of birth.<sup>12</sup>

Specifically, we first choose the executives who attended general high school in either Seoul, Busan, Daegu, Inchon or Kwangju. From them, we next select the executives who were born in 1957 or earlier and graduated from high school, since Korean children enter elementary school in the academic year in which the 7th birthday falls and grade 10 is the first grade of high school. Provided that the Leveling Policy was put into place in Seoul and Busan from 1974, and in Daegu, Inchon and Kwangju from 1975, we can assume that these executives entered

<sup>11</sup> In 2004 securities of a total of 683 companies that include leading companies of Korea such as Samsung Electronics Co., Hyundai Motor Company, LG Electronics Inc., etc. were traded in the KSE. It is thus no exaggeration that firms listed in the KSE represent the population of major companies based in Korea.

<sup>12</sup> In the raw KSE-Executive data, information on an executive's years of high school entrance and graduation is available. Since there is a lot of non-reporting for these variables, however, we use the year of birth to construct the sample of executives who entered high school prior to the Leveling Policy.

general high school under the selective system before 1974. For the executive who attended general high school in sixteen smaller urban municipalities where the Leveling Policy was implemented from 1979, 1980, and 1981, on the other hand, we select those who were born in 1962 or earlier.

Using school information of all the executives who are selected above, we simply count the number of executive-graduates for each high school. We are finally able to identify the number of relevant executives for a total of 347 high schools. In 1973 there were a total of 714 boys-only and co-educational high schools in Korea (Yearbook of Educational Statistics, 1973). Among those identified from the KSE-executive data, a total of 235 high schools have 1 to 10 executives, and the remaining 91 schools have more than 10 executives. If no relevant graduates for a given high school appear in the selected KSE-Executive data, we count the number of executives for that school as zero. Appendix Table 1 lists the names of the high schools that rank within the top 25 in terms of the number of executive-graduates. We use this number of executive-graduates as a measure of the quality of school ties for an analyzed junior graduate.

While the KSE-Executive data are drawn from an administrative source, the second and third data sets are constructed from a household survey in Korea entitled “the Korean Labor and Income Panel Study (KLIPS).” KLIPS is a nationally representative longitudinal study of Korean households, modeled after the National Longitudinal Surveys (NLS) and the Panel Study of Income Dynamics (PSID) of the U.S.<sup>13</sup> It is conducted annually by the Korea Labor Institute (KLI), a government-sponsored research institute (Korea Labor Institute, 1998). The study started in 1998 with 5,000 households and 13,783 individuals aged 15 or older.

KLIPS compiles a wide range of information on individuals, such as earnings, family and education backgrounds, and demographic characteristics. In addition to the completed years of education, it includes unusually detailed information on an individual’s education history in wave 4 (survey year 2001). For example, for those who have attended high school, KLIPS collects information on the name, type, and location of the respondent’s high school(s) as well as the years of entrance and graduation. By combining a respondent’s high school records from the KLIPS with the administrative information as to when and where the Leveling Policy began to apply, we are able to identify the individuals who were subject to the Leveling Policy in high school assignment. The individuals of the analysis are those who entered general high school in 1974 or later in the regions subject to the Leveling Policy.

For the subsequent analysis, we further narrow the sample to men who entered

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<sup>13</sup> This survey has been used elsewhere to study microeconomic issues of Korea (e.g., Cho and Keum, 2004; Lee and Tae, 2005; Kang et al., 2007; Park and Kang, 2008).

leveled general high school between 1974 and 1989 (both inclusive). Women are excluded because school ties are largely defined and functioned as men's social networks in Korea. Men who entered general high school after 1989 are also excluded, since a large age gap between senior executive-graduates and analyzed individuals is likely to weaken the functioning of school ties.

Upon imposing these restrictions, we obtain high school records of a total of 454 individual men. The total number of general high schools attended by these individuals is 181. We merge high school records of the KLIPS respondents with the data on the number of senior executive-graduates of the high school constructed earlier. For each high school, we identify the school district using the information of the school's geographic location. Controlling for school district is crucial in our analysis since randomization of the Leveling Policy was implemented within a school district but not across different districts.

Among a total of 31 school districts identified in the KLIPS data set, we exclude three school districts in the Seoul Metropolitan City—*Kangnam*, *Kangdong* and *Joongbu* Districts—from the subsequent analysis. As can be seen from Appendix Table 1, these districts are the regions where the most selective high schools prior to the Leveling Policy were located.<sup>14</sup> In addition, these districts have been populated by economically well-off and well-educated parents who are eager to send their children to a better high school. In the virtual absence of private high schools in the nation, these parents were likely to exert influence in order to assign their children to a formerly prestigious high school although such attempts were illegal under the Leveling Policy. While such a possibility is merely anecdotal, the KLIPS data contain evidence (shown below) suggesting that random assignment of students to high school was likely to be compromised to a certain degree in these three districts. For other districts, however, we fail to find evidence against randomization as expected from the institutional details on the Leveling Policy.<sup>15</sup>

Our third data set constructs information on a graduate's labor market performances such as monthly earnings, hourly wages, employment in a regular (*vis-à-vis* temporary) job and a large firm from KLIPS waves 6 to 8 (years 2003 to 2005). Since the data on school ties are constructed from the 2004 KSE-Executive data base, we employ the labor market data for the years adjacent to 2004. When the KLIPS data of waves 6 to 8 are merged with individuals' high school information, there are a total of 964 observations for a total of 374 individuals. Since not all the individuals report information on the labor market status and self-employed

<sup>14</sup> For example, Kyunggi, Seoul, Joongdong and Whimoon High School are located in the *Kangnam* District; Posung and Paichai High School in the *Kangdong* District; Kyungbock, Choongang, Kyongdong, Seoul Sadaebugo High School in the *Joongbu* District.

<sup>15</sup> While we focus on the analysis sample excluding the three school districts in Seoul, we report estimation results of the sample including individuals of these three districts in Appendix Table 2. They do not, however, reveal evidence that qualitatively changes the primary results of this paper.

individuals do not have information on earnings, however, the number of observations used for earnings regressions is 572 for a total of 237 individuals.

## 5.2. Descriptive Statistics

Descriptive statistics of the merged sample are reported in Table 1. For a convenient comparison, we further divide the total sample into a sample of the individuals who have zero senior executive-graduates and a sample of those who have a positive number of senior executive-graduates.<sup>16</sup>

[Table 1] Descriptive Statistics

Variables:	Total Sample			No. of Executive -graduates = 0 (N=179)		No. of Executive -graduates > 0 (N=393)	
	N	Mean	S.D.	Mean	S.D.	Mean	S.D.
Monthly earnings (W1,000)	572	2582.9	1120.9	2555.5	1202.7	2595.4	1082.9
Hourly wage (W1,000)	572	12.379	5.968	12.253	5.900	12.436	6.005
Employment in a large firm	474	0.331	0.471	0.356	0.480	0.318	0.467
Employment in a regular job	572	0.948	0.223	0.950	0.219	0.947	0.225
Number of executive-graduates	572	18.67	34.95	0.000	0.000	27.18	39.34
Age	572	37.48	4.125	37.13	3.668	37.64	4.312
Years of education	572	15.17	2.160	14.99	1.990	15.25	2.230
Level of education							
High school	572	0.205	0.404	0.212	0.410	0.201	0.401
2-year college	572	0.166	0.372	0.151	0.359	0.173	0.379
Universities outside Seoul	572	0.281	0.450	0.324	0.469	0.262	0.440
Universities in Seoul	572	0.271	0.445	0.212	0.410	0.298	0.458
Top-level universities	572	0.077	0.267	0.101	0.302	0.066	0.249
Married	572	0.848	0.359	0.821	0.384	0.860	0.347
Employed in a govt sector	572	0.131	0.338	0.061	0.241	0.163	0.370
Mother's education:							
No education	572	0.175	0.380	0.173	0.379	0.176	0.381
Up to primary school	572	0.503	0.500	0.587	0.494	0.466	0.499
Up to middle school	572	0.168	0.374	0.123	0.329	0.188	0.391
Up to high school or above	572	0.154	0.361	0.117	0.323	0.170	0.377
Private school	572	0.664	0.473	0.799	0.402	0.603	0.490
High school quality	572	0.286	0.281	0.224	0.263	0.314	0.284
Year of school entrance	572	1982.5	3.866	1982.7	3.345	1982.4	4.081

<sup>16</sup> From Table 1 comparing mean differences of the variables between the two subdivided samples is not very meaningful, however, since randomization took place within a given school district but not across different districts. Since the comparison in Table 1 shows mean differences of the variables across school districts, it is little surprise that there are statistically significant differences for some variables, e.g., mother's education, between the sample of zero executive-graduates and that of positive executive-graduates. Formal tests of randomization within a school district are given shortly.

As of 2003 to 2005, age of the individuals observed in the total sample ranges between 28 and 48, implying that they entered high school between 1974 and 1989 under the Leveling Policy; the average age of the total sample is 37.5. Their average monthly earnings (converted into the 2005 constant) are KRW 2.6 million (about US\$ 2,528) and the average hourly wage is KRW 12,379 (about US\$ 12.09). About 33.1 percent of individuals are employed in a private large-sized firm with at least 500 employees, while the number of relevant observations (474) is smaller due to non-responses and exclusion of those employed in the government sector. The average number of senior executive-graduates is 18.7, and the proportion of those observations with zero senior executive-graduates is 0.313. The average years of own education is 15.2. 20.5 percent of the sample received education up to high school; 16.6 percent up to 2-year college; 28.1 percent up to universities outside Seoul; 27.1 percent up to universities in Seoul; 7.7 percent up to top-level universities.<sup>17</sup> 84.8 percent of the sample is married, and 13.1 percent is employed in the government sector. The proportion of mothers with no formal education, education up to primary school, middle school, and high school or above is 17.5, 50.3, 16.8, and 15.4 percent, respectively. The mean year of high school entrance of the individuals is 1982.5.

Since an impact of school ties is basically a school effect, an empirical analysis requires a good control for school characteristics to disentangle an impact of school ties from other determinants. We employ two measures of school characteristics other than school ties. The first measure is an indicator for whether an executive's high school is private (as opposed to public). As discussed above, differences in education environment between private and public schools was likely to be small under the Leveling Policy. Nonetheless, there may exist unique features of private schools that affect the school's graduates. Variable 'private' is introduced to capture part of such features. About 66.4 percent of the individuals in the sample attended private high school.

The second measure of school characteristics is the number of a high school's graduates who were admitted to the School of Social Sciences of the Seoul National University (SNU). If a high school offers favorable supports in its classrooms and it is full of quality fellow students, a graduate can show a good performance in the labor market regardless of the quality of school ties. To measure a quality of a school's educational supports and an overall performance of its peers, we count the number of a high school's graduates who entered the SNU's College of Social

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<sup>17</sup> Top-level universities are 8 universities that were generally considered the most prestigious in the 1980s. They include Seoul National University, Korea University, Yonsei, Hanyang, University of Seoul, Sogang, Pusan National University, KyungHee University. Among those not included in top-level universities, universities located within the geographic boundary of Seoul are classified as universities in Seoul; those located outside the boundary of Seoul as universities outside Seoul. On average, universities in Seoul are considered better in student quality than those outside Seoul.

Sciences each year from 1977 to 1994. The College of Social Sciences (the College) is primarily composed of a total of 10 departments (Anthropology, Communication, Economics, International Economics, Geography, International Relations, Political Science, Psychology, Social Welfare, Sociology), while there have been minor changes in member departments over time. Traditionally, the College have been admitting the top 0.5 percent of the population of high school graduates. We construct variable ‘high school quality’ (*HSQ*) by exploiting an administrative database of the SNU and counting the number of students admitted to the College by each unique combination of high school and the year of high school entrance.<sup>18</sup> We match variable *HSQ* with the analyzed individuals by high school and the year of high school entrance. Variable *HSQ* is likely to show educational supports of the school and the overall quality of peers when the analyzed individuals attended high school under the leveling regime. By contrast, our proxy of school ties is designed to measure an influence of senior graduates who were educated under different educational environment and peer assignment.

In order to examine whether *HSQ* contains information on a school effect that is different from school ties, we correlate *HSQ* with the number of executive-graduates per school (*ST*) for the identified 347 schools. The correlation coefficient between *ST* and *HSQ* is 0.582 for year 1974, 0.494 for 1977, 0.411 for 1980, 0.429 for 1983, 0.388 for 1986 and 0.398 for 1989. Although *HSQ* has a statistically significant and positive correlation with *ST*, a large fraction of variation in *HSQ* remains unaccounted for by variation in *ST*. Moreover, the correlation coefficient declines over time. This suggests that potential links of school effects between pre-leveling and post-leveling periods must have become weakened over time as the

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<sup>18</sup> Specifically, variable ‘high school quality’ is constructed as follows. Using the administrative background information on all students admitted to the College for academic years 1970 to 1995, we sort students by high school and the year of high school entrance, and simply count the number of admitted students per high school per year of entrance. Since the 1960s the Department of Education of Korea have been imposing admission quotas for the College, which were subject to change over time. For example, the admission quotas were 205 for academic years 1970 to 1973, 235 for 1974, 250 for 1975 to 1980, 735 for 1981 to 1984, 620 for 1984, etc. To adjust for the changes in admission quotas and stabilize variation in *HSQ* over years, we set up an overlapping three-year window in which admission quotas of the College and the annual numbers of admitted students per high school are separately added. We construct variable *HSQ* by calculating the percentage of the number of a high school’s graduates who entered the College in that window among the three-year total admission quotas of the College. For example, year 1980 value of *HSQ* is the percentage of the number of a high school’s graduates who entered the College in 1982, 1983 and 1984—three years after high school entrance—among the total admission quotas of the College for these three years. If there was no graduate of a high school who entered the College during a three-year window, value 0 is assigned to that high school. We construct this variable for each high school for years 1974 to 1989. When we experiment with different sizes of the window (e.g., four or five years), the primary results of the paper are unaffected. Alternative results are available upon request. We are very grateful to The Center for Social Sciences of the SNU for generously providing us with the administrative data base of student records.



leveling policy was implemented. Overall,  $HSQ$  seems to capture unique aspects of school effects that are not explained by  $ST$ . A school's independent effects other than school ties must have gradually increased as the leveling policy expanded. From Table 1 the average of  $HSQ$  in the analysis sample is about 0.286 percent, while the minimum value is 0 and the maximum value is 2.933 percent.

### 5.3. Tests of Randomization

Before proceeding to primary analysis of the sample, we here examine whether the Leveling Policy really leads to randomization of school ties to individual graduates. In the course we explain why we exclude three suspicious school districts of Seoul from the subsequent analysis.

If assignment of students to high school was truly random under the Leveling Policy, we expect student characteristics that were determined prior to high school assignment to be uncorrelated with school characteristics. Otherwise, randomization under the Leveling Policy would be suspicious and an individual's quality of school ties is not sure to be exogenous. To see if such a (non-)relationship holds for the individuals subject to the Leveling Policy, we run a simple OLS regression in which  $\ln ST$  is related to his mother's and father's years of education, the year of high school entrance, and the school district identifiers. In some specifications we control for the level of an individual's own education, which is determined *after* high school graduation, in order to see whether school characteristics (e.g., class size, teacher quality, school management, etc.) other than school ties formed by senior graduates influence a junior graduate's post-high school education outcomes. If there existed systematic differences in education environment between high schools, current differences in individuals' labor market performances may not be solely due to a difference in the quality of school ties.

Table 2 shows the results of such OLS regressions for the following three samples. Sample A includes individuals of all 31 school districts that are originally identified in the KLIPS-wave 4 data set. Sample B includes only individuals of the three districts in Seoul where random assignment is suspected to have been compromised. Finally, Sample C is Sample A excluding individuals of Sample B.

Regression results for Sample A suggest that random assignment of students to high school must have been in part compromised under the Leveling Policy. According to the estimates in columns (1) to (3), the level of mother's education has a significantly positive association with  $\ln ST$ . Compared with students whose mothers experienced no formal schooling, students whose mothers received education up to high school or above are likely to have attended a high school with a greater number of senior executive-graduates. This raises a suspicion that highly educated mothers (or parents) might have exercised a certain degree of influence to send their children to a formerly prestigious high school under the Leveling Policy.

[Table 2] Tests for Random Assignment to High School

Dep. Variable:	Ln (Number of executive senior graduates)												
	Sample A. All districts (N=454)			Sample B. Three suspicious districts in Seoul (N=80)			Sample C. Districts excluding three districts in Seoul (N=374)			High school quality			Private School Sample C. (13)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
Analysis Sample:													
Explanatory variables													
Mother education (Base: no schooling)													
Primary school	-0.085 (0.216)	-0.129 (0.227)	-0.192 (0.233)	-0.273 (0.685)	-0.425 (0.725)	-0.585 (0.744)	-0.028 (0.227)	-0.035 (0.235)	-0.109 (0.241)	-0.003 (0.041)	-0.007 (0.043)	-0.019 (0.042)	0.087 (0.072)
Middle school	0.277 (0.260)	0.232 (0.278)	0.312 (0.277)	-0.634 (0.752)	-0.870 (0.795)	-0.250 (0.825)	0.324 (0.278)	0.302 (0.291)	0.285 (0.291)	-0.012 (0.046)	-0.018 (0.053)	-0.017 (0.053)	-0.036 (0.091)
High school or above	0.683* (0.279)	0.658* (0.307)	0.663* (0.311)	0.949 (0.633)	0.535 (0.723)	0.687 (0.727)	0.457 (0.310)	0.493 (0.332)	0.452 (0.340)	0.039 (0.050)	0.044 (0.054)	0.044 (0.056)	-0.009 (0.100)
Father education (Base: no schooling)													
Primary school		0.082 (0.346)	0.118 (0.347)		1.107 (0.970)	1.621* (0.972)		-0.126 (0.377)	-0.135 (0.377)		0.040 (0.060)	0.047 (0.061)	-0.018 (0.124)
Middle school		0.011 (0.363)	-0.030 (0.367)		0.683 (0.848)	0.862 (0.796)		-0.040 (0.403)	-0.070 (0.405)		0.090 (0.067)	0.097 (0.069)	-0.036 (0.128)
High school or above		0.138 (0.351)	0.095 (0.358)		1.427* (0.851)	1.568* (0.816)		-0.068 (0.382)	-0.123 (0.387)		0.058 (0.064)	0.058 (0.066)	-0.034 (0.128)
Level of education (Base: high school)													
2-year college			-0.136 (0.247)			-1.289 (1.077)			0.009 (0.246)			-0.015 (0.044)	0.097 (0.068)
Universities outside Seoul			0.137 (0.227)			-0.586 (0.688)			0.257 (0.240)			0.001 (0.035)	0.017 (0.072)
Universities in Seoul			0.102 (0.261)			-0.556 (0.670)			0.281 (0.271)			-0.015 (0.043)	-0.093 (0.084)
Top-level universities			0.106 (0.305)			0.105 (0.721)			0.009 (0.338)			-0.001 (0.054)	0.126 (0.087)
Adj. R-square	0.333	0.343	0.356	0.374	0.398	0.398	0.296	0.309	0.313	0.393	0.394	0.384	0.352
F(all parameters=0)	3.90	2.31	2.12	2.15	1.72	1.49	1.67	1.01	1.15	0.53	0.71	0.53	1.58
P-value	0.009	0.033	0.041	0.103	0.133	0.190	0.174	0.416	0.331	0.663	0.644	0.866	0.112

Notes: Standard errors are reported in parentheses. \* and \*\* indicate that the estimate is significant at the 0.10 and 0.05 levels, respectively. Other explanatory variables, whose estimates are suppressed, include an individual's year of school entrance and school districts.

F-tests reject a hypothesis that there is no impact of parent characteristics and the level of own education on high school assignment.

This finding is in fact somewhat contradictory to a traditional belief that the Leveling Policy was quite strictly implemented between the mid-1970s and early 1990s in Korea. There were few reported cases of open and public complaints against a violation of randomization in the course of the Leveling Policy. Given traditional perception about the Leveling Policy, we suspect that a breach of the randomization principle as found in Table 2 was not likely to be rampant but must have happened in a handful of school districts where some parents possessed a means to influence the randomization procedures. As such suspicious districts, we point to *Kangnam*, *Kangdong* and *Joongbu* Districts of Seoul (the capital city) because many of formerly prestigious high schools were located in these districts and economically well-off and well-educated parents lived there.

As expected, the estimates drawn from Sample B in columns (4) to (6) of Table 2 suggest that parental characteristics show a strong association with  $\ln ST$  in these three districts. Formal tests, however, fail to reject a hypothesis of no association at the 0.1 level of significance probably due to a small sample ( $N=80$ ). Nonetheless, a conservative interpretation would be that the estimates for Sample B offer evidence suggesting that randomization of students must have been somehow compromised in these three districts of Seoul.

In contrast to the results of Sample B, the estimates in columns (7) to (9) of Sample C excluding individuals of the three suspicious districts show no significant association between parental characteristics and the number of executive-graduates. No single characteristic of the parents is found to have a strong association with the number of executive-graduates in Sample C. An individual's level of post-high school education is also uncorrelated with the number of executive-graduates, suggesting that education environment of high school is unlikely to strongly affect an individual's level of completed education. F-tests support a hypothesis that there is no relationship between parent characteristics or the level of own education and high school assignment.

To add to evidence of randomization in Sample C, we run another OLS regressions in which each of an individual's *HSQ* and attendance in private high school is regressed against a set of explanatory variables. If randomization was questionable, both variables are likely to be strongly correlated with parent characteristics or the level of own education. The results in columns (10) to (13), however, suggest evidence against systematic sorting of students across different high schools in school districts of Sample C. There exist no significant (either single or joint) associations between *HSQ* and a propensity to attend private school, on the one hand, and an individual's characteristics, on the other.

The results of Table 2 suggest that the Leveling Policy by and large achieved a fair degree of randomization in school assignment, although there exist counter-

examples in a few school districts in Seoul. Therefore, in the subsequent analysis, we focus on Sample C that includes individuals of general high schools who were subject to strict randomization in school assignment, excluding those who entered high school in *Kangnam*, *Kangdong* or *Joongbu* Districts of Seoul.<sup>19</sup>

## VI. Estimation Results

### 6.1. OLS Results

Table 3 reports OLS estimates for equation (1) with different dependent variables. Columns (1) to (3) use a log of monthly earnings as a dependent variable, columns (4) to (5) a log of hourly wage, column (7) an indicator of employment in a regular (as opposed to temporary) position, and column (8) an indicator of employment in a private large-sized firm. While controlling for the variables identifying school districts in all specifications, we include explanatory variables progressively in columns (1) to (3) and columns (4) to (6) to examine whether the key estimate for  $\ln ST$  changes as a set of control variables varies. If the number of executive-graduates is truly randomized due to the Leveling Policy,  $\hat{\beta}_1$  should not vary much by a set of control variables.

[Table 3] OLS Regression Results

Explanatory variables:	Dependent variables:							
	Ln(Monthly earnings)			Ln(Hourly wage)			Employed in:	
	(1)	(2)	(3)	(4)	(5)	(6)	a regular position	a large firm
Ln(Number of executive-grads)	0.038*	0.013	0.001	0.046**	0.019	0.006	-0.002	-0.008
	(0.020)	(0.020)	(0.017)	(0.023)	(0.023)	(0.020)	(0.008)	(0.021)
Level of edu:								
2-year college			0.180**			0.121	0.174**	0.114
			(0.074)			(0.085)	(0.061)	(0.106)
Universities outside Seoul			0.292**			0.298	0.132**	0.167**
			(0.073)			(0.076)	(0.054)	(0.072)
Universities in Seoul			0.363**			0.346	0.118*	0.277**
			(0.105)			(0.103)	(0.072)	(0.097)
Top-level universities			0.519**			0.499	0.164**	0.426**
			(0.093)			(0.098)	(0.067)	(0.115)

<sup>19</sup> Including those individuals in the analysis, however, does not qualitatively alter the primary results of this paper. The results of the analysis including those individuals are reported in Appendix Table 2.

Married	0.404**	0.353**		0.360	0.308	0.105**	0.191**
	(0.060)	(0.061)		(0.063)	(0.064)	(0.051)	(0.079)
Employed in the gov't sector	-0.035	-0.041		-0.007	-0.016	-0.009	
	(0.052)	(0.049)		(0.061)	(0.060)	(0.056)	
Mother's edu:							
Primary school	0.182**	0.098		0.171	0.092	-0.034	-0.068
	(0.076)	(0.075)		(0.086)	(0.085)	(0.039)	(0.089)
Middle school	0.112	0.029		0.114	0.040	-0.054	-0.185
	(0.090)	(0.097)		(0.102)	(0.103)	(0.051)	(0.104)
High school +	0.190**	0.100		0.199	0.113	-0.041	-0.059
	(0.092)	(0.089)		(0.104)	(0.097)	(0.056)	(0.102)
Private school	-0.099	-0.106*		-0.146	-0.152	-0.016	-0.132*
	(0.062)	(0.059)		(0.069)	(0.065)	(0.032)	(0.075)
High school quality	0.150	0.179*		0.097	0.128	0.041	0.292*
	(0.105)	(0.099)		(0.114)	(0.116)	(0.061)	(0.165)
Adj. $R^2$	0.206	0.381	0.456	0.212	0.340	0.404	0.202
No. of obs.	572	572	572	572	572	572	478

Notes: Standard errors, which are clustered at the high school level, are reported in parentheses.

\* and \*\* indicate that the estimate is significant at the 0.10 and 0.05 levels, respectively.

Other explanatory variables, whose estimates are suppressed, include an individual's year of school entrance, school districts, current residence region and the survey year.

According to column (1), the number of executive-graduates has a positive effect on a junior graduate's monthly earnings. A doubling of the number of senior executive-graduates (from 20 to 40, for instance) on average causes a 3.8 percent increase in monthly earnings. If we employ 0.1 as a cut-off level of significance owing to the small number of observations, such an estimate is statistically significant. When variables for an individual's marital status, employment in the government sector, mother's education level, high school type (private vs. public) and  $HSQ$  are in addition controlled for,  $\hat{\beta}_1$  falls and becomes statistically indistinguishable from zero. If an individual's level of own education is added at last,  $\hat{\beta}_1$  becomes nearly zero, while the estimates for the levels of education relative to high school are significant.

In our analysis, controlling for post-high school education outcomes can be crucial. Although the government attempted to level education environment of different high schools in terms of the quality of student body, class size, teacher quality and other education resources by centralized control, there may have existed differences across schools within the same district that were unintended and difficult to remove. In such a case, average post-high school education outcomes of students may vary across high schools even within the same district, although the results in Table 2 suggest otherwise. Controlling for a junior graduate's post-high school education outcomes is likely to eliminate incidental and potential differences in school environment. If there is a difference in labor market outcomes between two graduates who possess a similar post-high school education outcome but

graduated from different general high schools within a same district, the difference can be attributed to a gap in the quality of school ties.

Ideally, we would prefer two graduates who attended different high schools but have the identical post-high school education outcome, e.g., two individuals attending the same department of the same university. With household survey data, however, it is fairly difficult to find such two individuals. As an alternative, we exploit four dummy variables for the level of an individual's own education to control for post-high school education outcomes. Estimation results in columns (3), (6), (7) and (8) are thus viewed most convincing among those of different specifications. Estimate  $\hat{\beta}_1$  in column (3) implies nearly zero average effects of the quality of school ties. Statistical evidence is weak that the quality of school ties strongly affects a junior graduate's monthly earnings on average.

The estimates for other explanatory variables in column (3) show signs and magnitudes that are conventionally expected. As the level of education goes up from high school to 2-year college to top-level universities, monthly earnings gradually increase. According to an unreported regression, one year of education is associated with about a 6.1 percent increase in earnings. Married men earns about 35.3 percent more than unmarried men; earnings are lower in the government sector; mother's education is significantly associated with an individual's earnings but become insignificant as the level of his own education is controlled for. Graduates who attended public high school enjoy about 10 percent greater monthly earnings than those who attended private school, since schools known to be better in the pre-leveling regime were in general public schools (see Appendix Table 1). *HSQ* is also significantly and positively associated with an individual's earnings. The results for both school characteristics may be viewed as evidence that education environment might have differed across schools in a district even under the leveling regime. It calls for a need to control for a graduate's post-high school education outcomes. A difference in  $\hat{\beta}_1$  between columns (2) and (3) of Table 3, however, is marginal. By comparison, significance of  $\hat{\beta}_1$  in column (1) underscores the importance of controlling for school characteristics in an examination of impacts of school ties.

Estimation results do not substantially change as we use hourly wages as an outcome variable. They are reported in columns (4) to (6). When school characteristics are not controlled for as in column (4), the estimate  $\hat{\beta}_1$  is strongly significant, suggesting that a doubling of the number of senior executive-graduates increases average hourly wages by 4.6 percent. When the full set of explanatory variables including *HSQ* and private school is controlled for in columns (5) and (6), the estimate  $\hat{\beta}_1$  becomes negligible and insignificant, suggesting that the number of senior executive-graduates has no significant average effect on an individual's hourly wages. If we use an indicator of employment in a regular job or a large firm as a dependent variable, average effects of the number of executive

graduates are negligible as well. Overall, empirical evidence is weak that school ties on average strongly affect an individual's labor market performances as measured by monthly earnings, hourly wages and an employment in a regular position or in a large-sized firm.

## 6.2. Quantile Regression Results

Given negligible average impacts of the quality of school ties on individuals, a question arises whether the impact of school ties is heterogeneous across individuals in the sense that they affects some group of individuals but not others. It is likely that school ties of senior graduates may not function uniformly for all junior graduates. They may be important for high-quality junior graduates alone. To be included in high-quality school ties formed by high-performing senior executive-graduates, mere graduation of the same school may not be sufficient; one might have to prove that he has a qualification that is commensurate with the quality of school ties.

To explore differential impacts of school ties, we run quantile regressions for equation (1) using the same sample as in Table 3. Quantile regressions show differential impacts of  $\ln ST_i$  on  $y_i$  at different quantiles of the distribution of  $y_i$ . The quantile estimates are reported in Table 4. The estimates in Panel A (Panel B) are drawn when a log of monthly earnings (hourly wages) is employed as a dependent variable. Column (1) of Table 4 repeats the OLS estimates of columns (3) and (6) of Table 3 for easy comparison. The estimates in columns (2) to (6) show quantile estimates at 0.1, 0.25, 0.5, 0.75 and 0.9 quantiles, respectively.

[Table 4] Quantile Regression Results

Explanatory variables:	Average	Quantiles:				
		0.1	0.25	0.5	0.75	0.9
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Ln(Monthly earnings) as the dependent variable.						
Ln(Number of executive-grads)	0.001 (0.018)	-0.047* (0.025)	-0.022 (0.025)	0.010 (0.019)	0.016 (0.019)	0.022 (0.022)
Level of education						
2-year college	0.180** (0.083)	0.216* (0.113)	0.209* (0.112)	0.100 (0.094)	0.120 (0.094)	0.176** (0.090)
Universities outside Seoul	0.292** (0.073)	0.426** (0.102)	0.423** (0.108)	0.235** (0.092)	0.268** (0.074)	0.279** (0.073)
Universities in Seoul	0.363** (0.105)	0.394** (0.131)	0.404** (0.134)	0.353** (0.116)	0.288** (0.097)	0.380** (0.097)
Top-level universities	0.519** (0.095)	0.720** (0.128)	0.669** (0.122)	0.454** (0.107)	0.398** (0.108)	0.660** (0.130)

Mother's edu:						
Primary school	0.098 (0.073)	0.083 (0.118)	0.141 (0.104)	0.140* (0.076)	0.137* (0.081)	0.174* (0.090)
Middle school	0.029 (0.093)	0.022 (0.139)	0.106 (0.128)	0.004 (0.087)	0.035 (0.109)	0.216* (0.120)
High school +	0.100 (0.083)	0.212* (0.127)	0.106 (0.122)	0.051 (0.088)	0.031 (0.102)	0.124 (0.102)
Private school	-0.106* (0.062)	-0.020 (0.087)	-0.075 (0.084)	-0.036 (0.061)	-0.131** (0.066)	-0.151** (0.069)
High school quality	0.179* (0.104)	0.395* (0.157)	0.265** (0.133)	0.238** (0.116)	0.075 (0.131)	0.285* (0.163)

Panel B. Ln(Hourly wage) as the dependent variable.

Ln(Number of executive-grads)	0.006 (0.021)	-0.036 (0.025)	0.011 (0.024)	0.018 (0.022)	0.011 (0.023)	0.075** (0.28)
Level of education						
2-year college	0.121 (0.092)	0.138 (0.127)	0.097 (0.132)	0.069 (0.108)	0.173* (0.101)	0.063 (0.103)
Universities outside Seoul	0.298 (0.077)	0.369** (0.097)	0.361** (0.107)	0.216** (0.093)	0.316** (0.088)	0.261** (0.090)
Universities in Seoul	0.346 (0.109)	0.364** (0.179)	0.419** (0.141)	0.312** (0.109)	0.416** (0.109)	0.341** (0.108)
Top-level universities	0.499 (0.097)	0.641** (0.142)	0.625** (0.115)	0.506** (0.093)	0.457** (0.105)	0.424** (0.131)
Mother's edu:						
Primary school	0.092 (0.081)	-0.003 (0.116)	0.062 (0.092)	0.111 (0.075)	0.077 (0.088)	0.170 (0.105)
Middle school	0.040 (0.102)	0.053 (0.154)	0.092 (0.122)	0.081 (0.092)	-0.029 (0.110)	0.045 (0.144)
High school +	0.113 (0.095)	0.226* (0.136)	0.120 (0.118)	0.056 (0.091)	0.036 (0.122)	0.075 (0.132)
Private school	-0.152 (0.067)	-0.155* (0.093)	-0.129* (0.079)	-0.129* (0.070)	-0.144* (0.074)	-0.055 (0.083)
High school quality	0.128 (0.122)	0.339* (0.177)	0.199 (0.146)	0.103 (0.121)	0.067 (0.146)	0.220 (0.164)

Notes: Standard errors are reported in parentheses. \* and \*\* indicate that the estimate is significant at the 0.10 and 0.05 levels, respectively. Other explanatory variables, whose estimates are suppressed, include an individual's marital status, employment in the government sector, year of school entrance, school districts, current residence region and the survey year.

The quantile results suggest that impacts of school ties are likely to be heterogeneous across different individuals. As for monthly earnings, the quantile estimates of  $\hat{\beta}_1$  are negative and statistically significant at a 0.1 quantile of the distribution of monthly earnings. This suggests that a low-performing junior graduate are likely to even suffer if he graduated from a high school with a good



quality of school ties. At middle quantiles (0.25, 0.5 and 0.75 quantiles), however, the estimates are statistically insignificant in line with OLS results. At high quantiles (a 0.9 quantile),  $\hat{\beta}_1$  is positive but statistically insignificant. Conditional on the level of own education and other explanatory variables, people who rank high in the monthly earnings distribution are affected far more strongly by the quality of school ties than those ranking in the middle and lower range of the conditional distribution. A doubling of the number of senior executive-graduates increases the 0.9 quantile of monthly earnings by about 2.2 percent.

Similar but more pronounced patterns of heterogeneous impacts of school ties are found when hourly wages are used as a dependent variable. The quantile estimate of  $\hat{\beta}_1$  are negative but statistically insignificant at the 0.1 quantile; the estimates are also insignificant at the 0.25, 0.5 and 0.75 quantiles of the conditional distribution of hourly wages. At the 0.9 quantile, however, the estimate becomes strongly positive and statistically different from zero. A doubling of the number of senior executive-graduates raises the 0.9 quantile of the hourly wage distribution by 7.5 percent. While the quality of school ties seems to have little positive impact on individuals at low and middle quantiles, they are likely to have a strongly positive impact on those at the high end of the earnings and wage distributions.

[Figure 1] Patterns of Quantile Effects

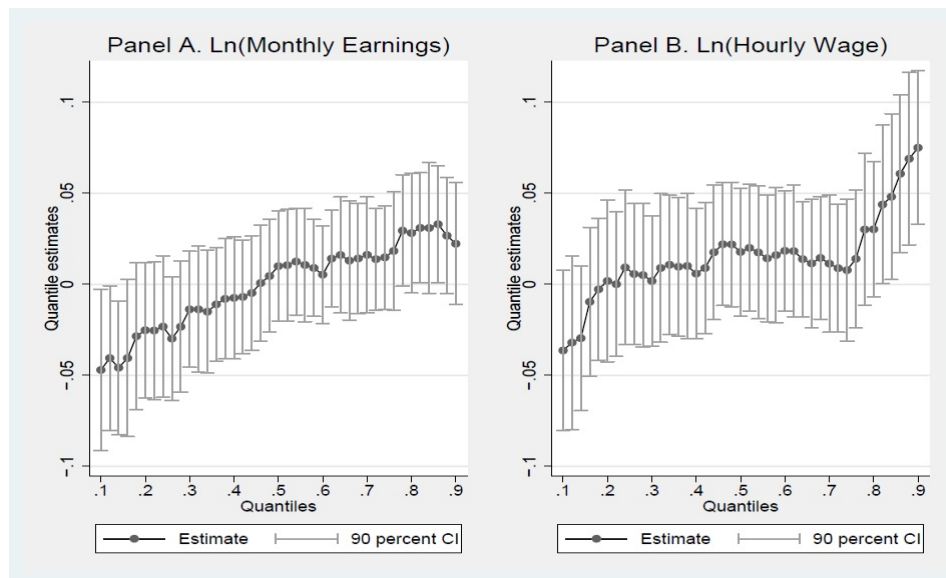
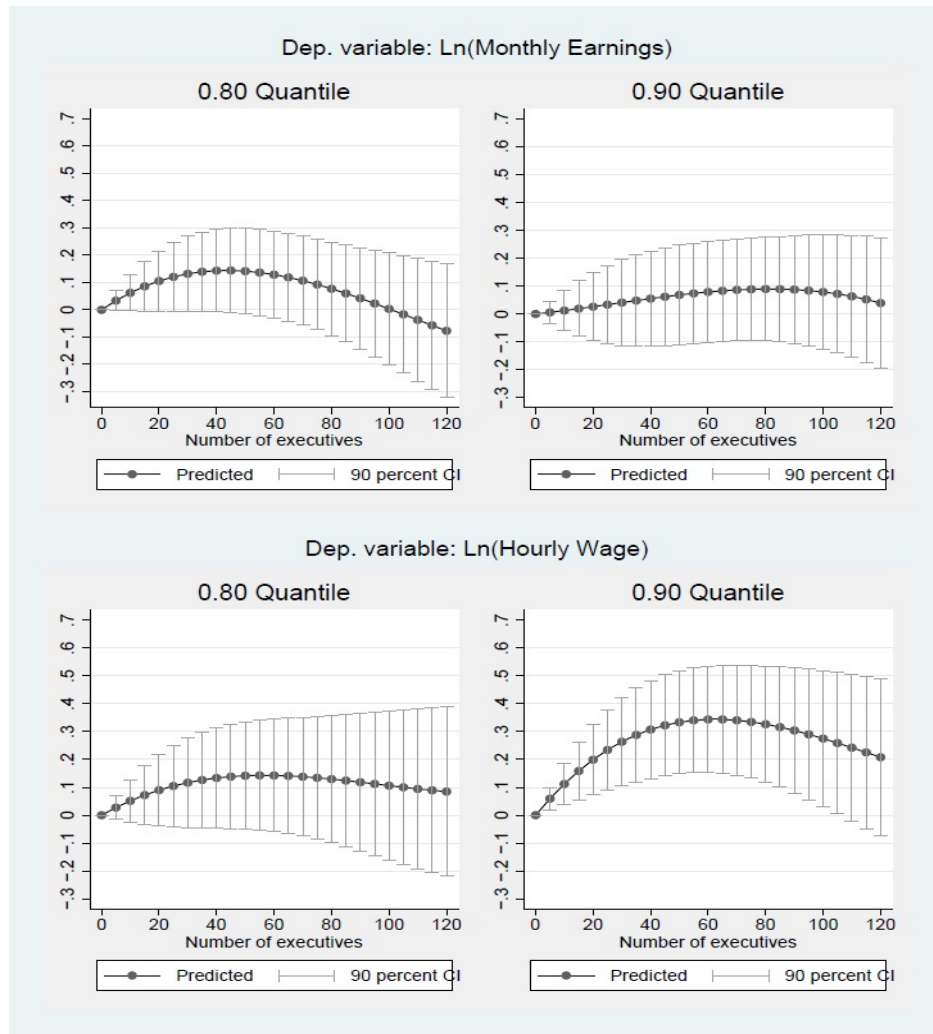


Figure 1 more broadly shows changing patterns of the effect of school ties at various quantiles of the conditional outcome distribution. Panel A (Panel B) shows quantile estimates and their 90 percent confidence intervals (CI) at various

quantiles of the monthly earnings (hourly wage) distribution. As is shown earlier, the impact of school ties seems to be heterogeneous across individuals at different locations of the  $y$ 's distribution; school ties benefit individuals at the high tail (above 0.80 quantile) of the distribution of a labor market performance while exerting no or negative influence on individuals at the low tail and middle range of the distribution.

[Figure 2] Nonlinear Effects of the Number of Executive-graduates



Provided that individuals only above the 0.80 quantile of the performance distribution benefit from school ties, it is worth investigating whether the shape of the impacts at these quantiles is linear over all values of the number of senior

executive-graduates. To examine potential non-linear impacts of school ties, we apply the same quantile regression method to equation (1) except that  $\ln ST$  is replaced by up to cubic terms of the raw number of executive-graduates. Based on the relevant quantile estimates (suppressed for brevity), we plot in Figure 2 the predicted values of the 0.8 and 0.9 quantiles of  $y$  over a range of the number of executive-graduates from 0 to 120. The upper and lower panels are based on quantile estimates for a log of monthly earnings and hourly wage, respectively.

At both 0.8 and 0.9 quantiles, the impacts of school ties on labor market performances seem slightly non-linear and inverted-U shaped. If we set the 0.8 (0.9) quantile of the earnings for individuals with zero senior executive-graduates at zero, the 0.8 (0.9) quantile is maximized when the number of executive-graduates is around 45 (80). As the number of executive graduates exceed this value, the monthly earnings slightly fall. The predicted values are, however, barely significant. When hourly wages are employed as a performance measure, the 0.8 and 0.9 quantiles of the distribution are maximized when the number of senior executive-graduates is around 60 to 75. While the predicted values of the 0.8 quantile are statistically insignificant, those of the 0.9 quantile are significantly different from zero.

### 6.3. Interpretations

In order to get a better picture of the magnitude of the quantile estimates shown above, let us take hypothetical individuals who entered different general high schools under the Leveling Policy in Kwangju City. We select Kwangju because as the largest city in the south-western part of Korea, it has a fairly large population of high school freshmen. Moreover, the entire city was treated as a single school district in high school assignment under the Leveling Policy. During the period of our interest, there were a total of 23 general high schools to which male students (e.g., 13,011 students as of 1987) were to be assigned randomly (*Educational Statistics Data Base*, Korean Educational Development Institute). We take the following six high schools that are different in terms of the number of senior executive-graduates as an example: Jaecil, Kwangju, Chosun-University affiliated High, Salesio, Dongshin, Songwon High School. Table 5 lists each high school by the number of senior executive-graduates. Columns (2) and (3) report the predicted 0.8 and 0.9 quantiles of the conditional monthly earnings distribution for each school, when those for zero executive-graduates are set to zero; columns (4) and (5) show the corresponding figures for the conditional hourly wage distribution.

Compared with a graduate of Songwon High whose monthly earnings are at the 0.8 quantile of the earnings distribution among Songwon High graduates, a graduate of Jaecil High whose earnings are at the same quantile of the distribution among Jaecil High graduates earns about 2.9 percent less. A graduate of Jaecil High

whose earnings is at the 0.9 quantile earns about 6.7 percent more. Both figures are, however, statistically insignificant. The 0.8 and 0.9 quantiles for Kwangju High graduates are about 13.1 and 7.8 percent larger, respectively, than the corresponding quantiles of Songwon High graduates. The 0.8 and 0.9 quantiles for Salesio High graduates are about 7.2 and 1.9 percent greater, respectively.

[Table 5] Impacts of School Ties on High School Graduates in Kwangju

Name of high school	No. of executive- graduates	Ln (Monthly Earnings)		Ln (Hourly Wage)	
		0.8qt	0.9qt	0.8qt	0.9qt
	(1)	(2)	(3)	(4)	(5)
Jaeil High	108	-0.029	0.067	0.097	0.249
Kwangju High	59	0.131	0.078	0.143	0.343
Chosun Univ.-affiliated High	23	0.115	0.031	0.099	0.221
Salesio High	15	0.072	0.019	0.072	0.159
Dongshin High	7	0.046	0.008	0.038	0.081
Songwon High	0	0.000	0.000	0.000	0.000

When hourly wages are used as a measure of comparison, the gaps increase considerably at the 0.9 quantile, while they remain similar at the 0.8 quantile. A graduate of Jaeil High whose wage is at the 0.8 (0.9) quantile of the hourly wage distribution among Jaeil High graduates earns about 9.7 (24.9) percent more than a corresponding graduate of Songwon High. A corresponding graduate of Kwangju High receives about 14.3 (34.3) percent more. Although smaller in magnitude, corresponding individuals of Chosun-University affiliated High, Salesio and Dongshin High at the 0.9 quantile also make more than a reference graduate of Songwon High. Overall, the results of Table 5 suggest that if a man is of high capability and remains high in the outcome distribution, a good quality of school ties is a non-ignorable determinant of his labor market outcomes.

Such findings imply that luck can be an important factor for some high-performing individuals due to randomization of the Leveling Policy and a functioning of school ties in Korea. Senior alumni of alma mater randomly given may be important figures for an individual in the labor market. If an individual possesses a qualification that is commensurate with senior alumni high-performing as board members and executives of firms, he is likely to enter existing school networks of these senior alumni and benefit from them. Otherwise, however, he may not even be considered as a member of the school's networks. Junior graduates who fail to qualify can be found to benefit little from the school ties consisting of high-performing senior graduates. The estimates for the 0.1 quantile suggest that they might even suffer.

Given such results of quantile regressions, a question may arise as to whether luck matters for men performing on the average or poorly in the labor market, because the Leveling Policy was indiscriminately applied not only to high-ability individuals but to mid- and low-ability individuals. Though the findings of this paper do not explicitly answer such a question, they do not rule out a possibility that luck also matters for such groups of individuals. Our current proxy for the quality of school ties simply measures a density of school networks among high-performing individuals such as board members and executives of firms. By design it does not capture the density of the school networks that might exist among men showing mediocre performances in the labor market. If a different type of school ties may indeed exist and function strongly among them, an alternative measure of school ties is likely to reveal the importance of school ties for average individuals. The proxy of the quality of school ties employed in this paper, however, does not properly capture the density of such school networks that exist among these individuals. If one invents an alternative proxy, school ties can also be shown to affect labor market performances of mediocre junior graduates.<sup>20</sup> This paper, however, does not attempt this line of undertaking, relegating the invention of such a measure to future research.

#### 6.4. Robustness Checks

Provided that the quality of school ties as measured in this paper affects only junior graduates who rank high in the labor market performance distribution, a question can still be raised whether such a result is drawn simply by sorting of students with educationally highly motivated parents into formerly prestigious high schools that produce a large number of senior executive-graduates. Given evidence suggesting that highly educated mothers attempted to send their children to a formerly prestigious high school at least in the wealthiest regions of Seoul as shown in Table 2, such a possibility may not be completely ruled out. If similar influences were also exercised by a handful of exceptionally educationally motivated parents in other regions, we expect OLS and quantile results for impacts of school ties to be similar to those observed from our analysis.

In order to address such a concern, we run quantile regressions of  $\ln ST_i$  against parental characteristics using the most comprehensive specification (columns (6) and (9)) of Table 2. If randomization was compromised by some parents who eagerly wanted to send the children to formerly prestigious high schools, the quantile estimates are likely to be statistically significant at higher quantiles of  $ST_i$ . The quantile estimates are reported in Table 6. The left panel shows the estimates

<sup>20</sup> The measure of social networks in Munshi (2003) reflects a network density among such mediocre groups of individuals.

[Table 6] Quantile Regression Tests for Random Assignment to High School

Explanatory variables	Dep. Variable: Ln (Number of executive senior graduates)									
	Sample B. Three suspicious districts in Seoul (N=80)					Sample C. Districts excluding three districts in Seoul (N=374)				
	Quantiles					Quantiles				
	Average (1)	0.5 (2)	0.75 (3)	0.9 (4)	Average (5)	0.5 (6)	0.75 (7)	0.9 (8)		
Mother education (Base: no schooling)										
Primary school	-0.585 (0.744)	-1.166 (1.450)	-0.339 (1.209)	-0.434 (1.264)	-0.109 (0.241)	-0.241 (0.338)	-0.170 (0.347)	-0.058 (0.354)		
Middle school	-0.250 (0.825)	-1.502 (1.642)	-0.616 (1.456)	-0.713 (1.425)	0.285 (0.291)	0.273 (0.447)	0.102 (0.420)	0.037 (0.392)		
High school or above	0.687 (0.727)	0.173 (1.510)	0.339 (1.318)	0.416 (1.263)	0.452 (0.340)	0.161 (0.501)	0.249 (0.554)	0.528 (0.556)		
Father education (Base: no schooling)										
Primary school	1.621* (0.972)	2.197 (2.061)	3.199 (2.019)	3.389* (2.031)	-0.135 (0.377)	0.241 (0.530)	-0.164 (0.633)	0.071 (0.625)		
Middle school	0.862 (0.796)	0.409 (1.883)	2.559 (2.012)	2.806 (1.998)	-0.070 (0.405)	0.193 (0.545)	-0.267 (0.647)	-0.111 (0.621)		
High school or above	1.568* (0.816)	1.268 (1.858)	2.710 (1.766)	2.936* (1.762)	-0.123 (0.387)	0.097 (0.499)	-0.510 (0.649)	-0.177 (0.672)		
Level of education (Base: high school)										
2-year college	-1.289 (1.077)	-1.411 (1.977)	-0.332 (1.958)	-0.774 (2.088)	0.009 (0.246)	0.144 (0.304)	0.022 (0.367)	-0.177 (0.415)		
Universities outside Seoul	-0.586 (0.688)	0.000 (1.414)	0.000 (1.294)	-0.173 (1.379)	0.257 (0.240)	0.258 (0.340)	0.552 (0.418)	-0.008 (0.380)		
Universities in Seoul	-0.556 (0.670)	-0.582 (1.221)	0.127 (1.273)	-0.142 (1.411)	0.281 (0.271)	0.063 (0.351)	0.459 (0.429)	0.053 (0.425)		
Top-level universities	0.105 (0.721)	0.000 (1.458)	0.000 (1.324)	-0.210 (1.437)	0.009 (0.338)	0.371 (0.414)	0.391 (0.426)	0.158 (0.440)		
F-statistic(all parameters=0)	1.49	0.57	0.43	0.52	1.15	0.38	0.53	0.32		
P-value	0.190	0.830	0.925	0.865	0.331	0.954	0.865	0.976		

Notes: Standard errors are reported in parentheses. \* and \*\* indicate that the estimate is significant at the 0.10 and 0.05 levels, respectively. Other explanatory variables, whose estimates are suppressed, include an individual's year of school entrance and school districts.

for Sample B, which consists of individuals who entered high schools in the three suspicious districts in Seoul. Column (1) repeats OLS estimates in column (6) of Table 2; columns (2) to (4) report the quantile estimates at the 0.5, 0.75 and 0.9 quantiles of the number of executive-graduates, respectively. The 0.1 and 0.25 quantile estimates are difficult to identify, since about a third of high schools covered in the current analysis have zero senior executive-graduates as implied in Table 1. The right panel shows the estimates for Sample C, which covers individuals who entered leveled high schools in other school districts in the nation than the three suspicious districts of Seoul. Column (5) repeats OLS estimates in column (9) of Table 2; columns (6) to (8) report the quantile estimates at the 0.5, 0.75 and 0.9 quantiles of the number of executive-graduates, respectively.

The quantile estimates for Sample B reveal a potential breach of the randomization principle in the three suspicious districts in Seoul. While no parental characteristics are strongly associated with the 0.5 and 0.75 quantiles, father's education is significantly associated with the 0.9 quantile of the conditional distribution of the number of executive-graduates at least at the 0.1 level of significance. Provided that the number of observations used in the current quantile regressions is fairly small, the results are somewhat surprising. Hence we suspect that the randomization principle of the Leveling Policy was compromised to a certain extent by highly educated parents especially for formerly prestigious high schools in the three suspicious districts in Seoul.

If our primary results for the impacts of school ties in Sample C are drawn from sorting of students with educationally highly motivated parents, we would expect the estimation results for Sample C to be similar to those of Sample B. In contrast, the quantile estimates for Sample C do not reveal evidence against the randomization principle in other school districts of the nation. The hypothesis that no parental characteristics are associated with the number of senior executive-graduates fails to be rejected for Sample C at the conventional levels of significance. Therefore our quantile results of Table 4 are unlikely to be drawn from mere sorting of students into high schools of quality school ties. Our estimates reveal causal impacts of a quality of school ties on an individual's labor market outcomes.

Provided that our measure of school ties affects those at high portions of the earnings distribution, we expect to see stronger impacts of school ties among college graduates than among high-school graduates. As another robustness check, Table 7 shows OLS and quantile estimates for the effect of school ties subdivided by an individual's education level. As expected, we find stronger impacts of school ties for college graduates than for high-school graduates. While the results for college graduates are similar to those in Table 4, impacts of school ties are uniformly negligible for high-school graduates. The patterns of the impacts of school ties are more pronounced again if hourly wages are used as a dependent variable.

**[Table 7]** Quantile Regression Results by the Education Level

Explanatory variables:	Average	Quantiles:				
		0.1	0.25	0.5	0.75	0.9
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Ln(Monthly earnings) as the dependent variable.						
1. For college graduates ( $N=452$ ):						
Ln(Number of executive-grads)	0.005 (0.020)	-0.035 (0.028)	-0.029 (0.027)	0.018 (0.024)	0.015 (0.021)	0.023 (0.025)
2. For high-school graduates ( $N=120$ ):						
Ln(Number of executive-grads)	-0.052 (0.064)	-0.092 (0.180)	-0.132 (0.176)	0.010 (0.170)	-0.039 (0.153)	-0.033 (0.165)
Panel B. Ln(Hourly wage) as the dependent variables.						
1. For college graduates ( $N=452$ ):						
Ln(Number of executive-grads)	-0.004 (0.023)	-0.073** (0.034)	-0.014 (0.031)	0.017 (0.027)	0.027 (0.026)	0.068** (0.031)
2. For high-school graduates ( $N=120$ ):						
Ln(Number of executive-grads)	-0.037 (0.075)	-0.141 (0.229)	-0.135 (0.204)	-0.063 (0.183)	0.037 (0.163)	0.076 (0.162)

Notes: Standard errors are reported in parentheses. \* and \*\* indicate that the estimate is significant at the 0.10 and 0.05 levels, respectively. The set of control variables are the same as in Table 4. Estimates for other variables are suppressed.

## VII. Concluding Remarks

In this paper, we have investigated causal effects of the quality of school ties on an individual's labor market performances. To circumvent an endogeneity problem that plagues existing studies, we exploit random assignment of high school students under the Leveling Policy of Korea. We demonstrate that the quality of school ties does not strongly affect the average individual's labor market outcomes. The OLS estimates suggest that a doubling of the number of senior executive-graduates (e.g., from 20 to 40) increases the average junior graduate's monthly earnings and hourly wages just by 0.7 percent and 1.0 percent, respectively. These estimates, however, are statistically indistinguishable from zero. The number of senior executive-graduates also fails to significantly affect a junior graduate's chance of being employed in a regular job or in a large-sized firm. In contrast, our quantile estimates suggest that school ties benefit junior graduates who rank high in the



outcome distribution. Robustness checks suggest that our estimates do not seem to be drawn from mere sorting of highly able students to formerly prestigious high schools.

A couple of points arising from the findings of this paper merit further discussion. We demonstrate that junior graduates benefit from school ties formed by senior graduates with extraordinary performances only if they rank high in the outcome distribution. This prompts us to revisit and rethink the effects of school networks in Korea. In particular, the onslaught of the Asian financial crisis reinvigorated numerous criticisms against corporate governance in Korea. At the core of the criticism was that employees including high-ranking board members tend to be recruited based on school ties rather than expertise, ultimately leading to the lack of a “check and balance” system in the Korean corporate governance. Results of this paper in part support such a possibility. If a top-level decision maker of the firm is a senior graduate of the high school that a high-performing junior graduate has attended, the latter is likely to benefit from the former. Nonetheless, whether such a relationship leads to corporate cronyism and actually creates inefficiency is a matter to be explored separately.

Our study also sheds light on the longevity of school networks’ effects. Previous studies argue that the effects of school networks, if any, tend to materialize at the time of recruitment or promotion at low ranks, because employers unable to acquire prior information about employees rely on school networks as an effective screening mechanism at this stage (Ishida et al., 1997). This view posits that the effectiveness of school networks is likely to decline at the later stage as workers are promoted to higher ranks. However, we examined individual workers whose age ranges from 28 to 48 and who are likely to occupy above low- and mid-tier ranks in firms. The findings reveal that these workers still benefit from school networks as long as they maintain good performances. This suggests that the effects of school networks may be enduring even in the longer term than previously thought. Certainly, further research will be needed to firmly establish such a possibility.

**[Appendix Table 1]** List of High Schools by the Number of Senior Executive-graduates

Rank	Number of Executives	Name of high school	School Type	City	School District	Sample selection
1	423	Kyunggi	Public	Seoul	Kangnam	Excluded
2	268	Kyungbock	Public	Seoul	Joongbu	Excluded
3	259	Seoul	Public	Seoul	Kangnam	Excluded
4	204	Busan	Public	Busan		
5	188	Kyeongbuk	Public	Daegu		
6	162	Kyongnam	Public	Busan		
7	156	Daejon	Public	Daejon		
8	138	Jeonju	Public	Jeonbuk		
9	133	Choongang	Private	Seoul	Joongbu	Excluded
10	123	Yongsan	Public	Seoul	Joongbu	Excluded
11	122	Posung	Private	Seoul	Kangdong	Excluded
12	114	Kyongdong	Public	Seoul	Seongbuk	
13	108	Kwangju Jaeil	Public	Kwangju		
14	101	Masan	Public	Kyongnam		
15	97	Daegu Sang	Public	Daegu		
16	96	Jinju	Public	Kyongnam		
17	93	Cheongju	Public	Choongbuk		
18	86	Joongdong	Private	Seoul	Kangnam	Excluded
19	81	Seoul Sadaebugo	Public	Seoul	Joongbu	Excluded
20	75	Kyongbuk Sadaebugo	Public	Daegu		
21	74	Donga	Private	Busan		
22	71	Gyesung	Private	Daegu		
23	70	Paichai	Private	Seoul	Kangdong	Excluded
24	70	Dongrae	Public	Busan		
25	69	Whimoon	Private	Seoul	Kangnam	Excluded

**[Appendix Table 2]** OLS and Quantile Regression Results for All School Districts Including Three Suspicious Districts in Seoul ( $N=693$ )

Explanatory variables:	Average	Quantiles:				
		0.1	0.25	0.5	0.75	0.9
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Ln(Monthly earnings) as the dependent variable.						
Ln(Number of executive-grads)	0.001 (0.016)	-0.034 (0.024)	-0.006 (0.020)	-0.003 (0.016)	0.038** (0.016)	0.038** (0.019)
Level of education						
2-year college	0.187** (0.076)	0.242** (0.106)	0.166 (0.106)	0.051 (0.081)	0.130* (0.075)	0.153* (0.089)
Universities outside Seoul	0.315** (0.066)	0.463** (0.083)	0.408** (0.092)	0.184** (0.074)	0.307** (0.061)	0.290** (0.067)
Universities in Seoul	0.331**	0.434**	0.387**	0.226**	0.283**	0.305**

	(0.086)	(0.108)	(0.100)	(0.084)	(0.070)	(0.084)
Top-level universities	0.510**	0.645**	0.608**	0.395**	0.442**	0.450**
	(0.087)	(0.106)	(0.095)	(0.086)	(0.094)	(0.112)
Mother's edu:						
Primary school	0.099	0.100	0.074	0.173**	0.138**	0.200**
	(0.065)	(0.100)	(0.097)	(0.058)	(0.067)	(0.069)
Middle school	0.096	-0.010	0.155	0.112	0.108	0.244**
	(0.084)	(0.138)	(0.114)	(0.073)	(0.080)	(0.092)
High school +	0.047	0.085	0.085	0.046	0.063	0.124
	(0.074)	(0.114)	(0.101)	(0.072)	(0.084)	(0.079)
Private school	-0.096*	-0.103	-0.053	-0.076	-0.017	-0.113*
	(0.056)	(0.085)	(0.064)	(0.049)	(0.059)	(0.067)
High school quality	0.139**	0.130	0.143	0.162*	0.085	0.055
	(0.071)	(0.110)	(0.092)	(0.090)	(0.096)	(0.119)

Panel B. Ln(Hourly wage) as the dependent variable.

Ln(Number of executive-grads)	0.008	-0.039*	0.002	0.021	0.018	0.050**
	(0.018)	(0.021)	(0.021)	(0.019)	(0.020)	(0.024)
Level of education						
2-year college	0.170**	0.102	0.138	0.114	0.191**	0.098
	(0.086)	(0.137)	(0.112)	(0.089)	(0.086)	(0.102)
Universities outside Seoul	0.358**	0.458**	0.389**	0.286**	0.363**	0.317**
	(0.072)	(0.098)	(0.093)	(0.076)	(0.075)	(0.078)
Universities in Seoul	0.342**	0.508**	0.434**	0.301**	0.386**	0.328**
	(0.094)	(0.119)	(0.112)	(0.096)	(0.096)	(0.102)
Top-level universities	0.558**	0.623**	0.676**	0.483**	0.560**	0.537**
	(0.104)	(0.129)	(0.112)	(0.106)	(0.107)	(0.133)
Mother's edu:						
Primary school	0.075	0.124	0.009	0.120*	0.123	0.116
	(0.073)	(0.112)	(0.087)	(0.071)	(0.080)	(0.097)
Middle school	0.110	0.161	0.104	0.141*	0.081	0.073
	(0.095)	(0.167)	(0.118)	(0.083)	(0.088)	(0.125)
High school +	0.030	0.187	-0.003	0.052	0.043	0.034
	(0.089)	(0.161)	(0.112)	(0.093)	(0.095)	(0.110)
Private school	-0.143**	-0.183**	-0.112*	-0.107*	-0.159**	-0.095
	(0.062)	(0.090)	(0.064)	(0.064)	(0.061)	(0.078)
High school quality	0.084	0.263**	0.096	0.014	0.075	0.293**
	(0.091)	(0.129)	(0.097)	(0.101)	(0.131)	(0.148)

Notes: Standard errors are reported in parentheses. \* and \*\* indicate that the estimate is significant at the 0.10 and 0.05 levels, respectively. Other explanatory variables, whose estimates are suppressed, include an individual's marital status, employment in the government sector, year of school entrance, school districts, current residence region and the survey year.

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