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Does Higher Education Affect Health and Health Behaviors?: Evidence from a Regression Discontinuity Design

WooRam Park, Jisun Baek, and Hyelim Son*

Abstract

This paper examines the causal effect of higher education on health related outcomes. To address the endogeneity in educational attainment, we exploit the educational reform in Korea which has increased the opportunity to engage in college education for affected cohorts. Using the regression discontinuity design, we do not find supportive evidence for health return of higher education. Moreover, we find that higher education has limited causal effects on health behaviors such as smoking and drinking. The limited effect might be a result of universal health care system in Korea which provides health insurance for practically all individuals.

JEL code: I10, I20

Keywords: health; health behaviors; exogenous variation; regression discontinuity; college education

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1 Introduction

The causal effect of education on health is of interest for both economists and policy makers. Reflecting this interest, numerous studies have documented strong correlation between education and health outcomes.[1] However, it is not clear whether this correlation reflects a causal effect. In particular, many unobserved factors such as parents' education level may simultaneously affect individual's education attainment and general health. In addition, reverse causality between health and education may also exist. Thus, studies that do not properly address the endogeneity in education attainment could possibly overstate the effect of education on health. Some papers have recently investigated the causal impact of education on health related outcomes, addressing the endogeneity in education attainment. Unlike previous correlation studies, the result is mixed.[2]

The purpose of this paper is to provide new evidence regarding causal effect of education on health outcomes and behaviors. To address the endogeneity in education attainment, we use a policy experiment; an education reform in Korea. The reform of 1980 exogenously increased the opportunity for college education by increasing the freshmen quota. Combined with the compulsory school entrance law, this created a discrete difference in opportunities for obtaining college education across adjacent birth cohorts.

We exploit this sharp difference in the proportion of the population with college education

[1]For instance, it is well known that an additional year of education is associated with longer life expectancy and fewer chronic diseases (Cutler & Lleras-Muney, 2008, 2010). Other studies indicate that educational attainment is negatively correlated with smoking and excessive drinking which are known to have negative effects on health (Sander, 1995a, 1995b).

[2]For instance, using state-level variation in compulsory education laws in the U.S., Lleras-Muney (2005) finds that an increase in educational attainment lowers mortality, and Oreopoulos (2007) finds a positive effect of secondary education on self-reported health. In contrast, Clark and Royer (2013) exploits two changes in British compulsory schooling laws and find limited evidence on the effect of additional education on mortality and self-reported health. Similarly, Albouy and Lequien (2009) finds limited effect of education on health, using compulsory school reform in France. Moreover, other studies such as Kenkel, Lillard, and Mathios (2006) and de Walque (2007) find that schooling has negative effect on smoking rates whereas C. Park and Kang (2008) find little effect of education on smoking.

across adjacent cohorts and we apply a regression discontinuity design in order to identify the causal effect of higher education. One possible obstacle for identifying the causal effect of higher education on various outcomes using regression discontinuity design is the endogenous sorting of individuals around the cutoff point. However, this is of a less concern in our empirical setting since the education reform was announced in 1980, and we focus on individuals near the college entering age at the time of the reform—cohorts that had been born around 1962. Therefore, it is unlikely that the parents who care about the college educational attainment of the children could have foreseen this event and have planned the birth of their children accordingly.

Using the Korea Welfare Panel Study (KOWEPS) which collects detailed information on health outcomes and health behaviors, we document four findings in this paper. First, we confirm that the cohorts that have been affected by the education reform are more likely to have a college education than the slightly older cohorts. Second, we discover that education has a limited effect on subjective evaluation of poor health and chronic disease incidence. Specifically, despite the discrete increase in the proportion of individuals with a higher education at the birth year cutoff, we do not observe any similar discrete changes in the proportion of individuals who reported their own health as being poor or in the probability of having chronic diseases.

Our next set of results indicates a possible reason as to why higher education has little effect on health outcomes by studying health behavior. We document that higher education has limited impact on reducing behaviors known to have negative effects on individuals' health. To be specific, we do not find any supportive evidence that higher education reduces the probability of currently smoking nor the probability of having ever smoked. In addition, we find that higher education has little negative impact on incidence of alcohol consumption.

Finally, we suggest a possible reason for the limited impact of higher education on both direct health outcomes and health behavior. A priori, higher education may be thought to enhance health outcomes and health behavior through better information and better access to medical institutions. Therefore, we explore this mechanism and test whether the probability of having

health insurance and receiving health checkup is affected by higher education. Consistent with our previous findings, we find that higher education does not increase either measure. We believe that this is mainly due to the universal national health care system in Korea, which implies that health returns to higher education may differ in settings with different institutions.

This paper contributes in several ways to the literature documenting the returns to education. First, we provide causal evidence on the health returns to higher education. Majority of previous works using a regression discontinuity design have focused on impact of secondary education on health (Oreopoulos, 2006). However, as the effect of eduction on health may depend on the education level, it is important to investigate the health return from the higher education level as well. In fact, some recent works do show that returns from college education might differ from the return from secondary education in many aspects (Iranzo & Peri, 2009; Arcidiacono, Bayer, & Hizmo, 2010). Moreover, this paper also adds to the existing literature by providing evidence that the health care system could affect the possible channel of the return of education on health. In particular, the benefit from National Health Insurance Program in Korea, which covers over 95 % of population, does not depend on the amount of contribution to the health insurance. Thus, the universal health care system in Korea is likely to mitigate the inequality in health due to the difference in income that is associated with educational attainment, resulting in limited health return to higher education.

This paper is also related to the group of research exploiting education reform at the national level, which affects adjacent cohorts differentially according to timing of birth. Such studies have adopted regression discontinuity design to compare the outcomes for the cohorts that were affected by the nation-wide reform and those born slightly earlier, thus were less likely to be affected by the reform. For instance, Clark and Royer (2013) uses the two education reforms at the national level in Britain that induced the cohort of 14 year-olds in 1947 and the cohort of 15 year-olds in 1972 to stay in school longer by raising the school leaving age. They exploit this discrete change in educational attainment across adjacent cohorts and apply regression discontinuity using

birth cohort as a forcing variable to identify the causal effect of secondary education on mortality and health behaviors.[3]

The rest of this paper is organized as follows. In Section 2, we discuss detailed information about the reform of the Korean education system we exploit. The overview of the main data, Korea Welfare Panel Study (KOWEPS), and the sample construction are described in Section 3 followed by discussion of empirical strategy and the estimating equation in Section 4. Section 5 includes the main empirical results. We then summarize and conclude the paper in Section 7.

2 Background

Korea provides a unique setting for this type of research because the central government has regulated the admission guidelines for both the private and public colleges. In particular, the government outlined detailed procedures for the admission and factors that should be considered or should not be reflected when granting admissions to students. Moreover, the government controlled access to college education by regulating the number of higher education institutions and the number of freshman students who could be admitted to colleges—the freshman quota.[4] In particular, despite the rising demand for college education, the government adjusted the quota only to a limited extent during the 1970s. Thus, many individuals who wanted to benefit from

[3]Similarly, Malamud and Pop-Eleches (2010) uses Romanian education reform of 1973. Specifically, the reform has discretely increased the general high school attainment for those who were born in 1958. They apply the regression discontinuity design and compare the outcome of the individuals born in or slightly after 1958 to the outcome of those born slightly earlier to identify the effect of general education as opposed to vocational education. Oreopoulos (2006), Albouy and Lequien (2009) and Malamud and Pop-Eleches (2011) also uses regression discontinuity design with a birth cohort as a forcing variable.

[4]Establishing a new college required permission from the government, and the government kept the number of colleges more or less stable during that time of our study. In addition, the freshman quota was strictly administered and was subject to a sizable noncompliance penalty including loss of government funding or subsidy and a reduction in the freshman quota in the following years. These two restrictions rendered the supply of college graduates exogenous to the demand for college education and the returns to a college education.

higher education could not enter a college during the 1970s. The college admission rule and the opportunity to obtain a college education, however, changed radically because of the education reform of July 30th, 1980. This change was unexpected as it was a strategic action by an autocratic military regime to gain popularity by expanding the chance of entering colleges. Specifically, the reform quickly followed the formation of military junta by coup d'ètat in December, 1979 subsequent to President Park Chung-hee's assassination on October 1979.

The education reform consisted of three main components, first and perhaps most importantly, the government discretely increased the freshman quota, by demanding each college to admit more students. This increase benefited a group of individuals who previously had a low proportion of college education attainment due to limited slots for college education.[5] Second, the government abolished the "Bongosa"—a college-specific entrance exam that students were required to pass in order to enter a desired college. Preparing for the college-specific test was costly because students had to prepare for the college-specific exam for each college to which they were applying. Moreover, the scope of examination was often beyond the regular curriculum of the high school and often required private tutoring or cram schooling. The government substituted this exam with a nationally administrated exam "Haklyeoggosa". Unlike the "Bongosa", the newly introduced exam was predominantly based on the regular curriculum of high school, which benefited students who could not afford a tutoring service. Finally, the government prohibited private tutoring often conducted by college students in the metropolitan regions. Because of the fact that private tutoring was expensive, students with affluent background were more likely to take private tutoring. Therefore, by prohibiting such private tutoring, the government was able to effectively increase the chances of higher education for the students in rural regions with disadvantaged background.

[5]Due to this discrete increase in the freshmen quota, the students from more advantaged background did not experience a decrease college education attainment despite the other components of the reform, which have been expected to benefit mainly students from a less privileged background.

Overall, the education reform discretely increased opportunities of attaining college educations, especially for the group that had limited access to a college education. This discrete increase in the opportunity for a college education in 1981, combined with the mandatory school entrance law in Korea, created a large difference in the opportunity to obtain college educations across adjacent cohorts. To be specific, as an individual who was 6 years old as of March was mandated—with few exceptions—to enter an elementary school that year, so most individuals born in 1962 would have entered college in 1981 after completing six years of elementary school, three years of middle school and three years of high school. Thus, they were the first cohort to which the policy was applied to when the freshmen quota was discretely increased by the education reform. Thus, due to the reform, cohorts born in or after 1962 would show a higher proportion of individuals with college education compared to slightly older cohorts. This exogenous discontinuity in the opportunity to obtain college education across otherwise comparable cohorts is our source of identification.

3 Data

We use the Korea Welfare Panel Study (KOWEPS hereafter), collected by the Korea Institute for Health and Social Affairs along with the Department of Social Welfare at the Seoul National University since 2006. The dataset is an annual household panel of about 5200 to 7000 households and includes detailed demographic information for each household member, such as the year of birth, gender, location of residence during youth and education attainment. More importantly, it contains measures of their health status and information on behaviors that could possibly affect health outcomes, such as smoking and drinking. In the main analysis, we pool seven waves—from the first to the seventh—which were collected from 2006 to 2012, hence, the cohorts initially affected by the policy are in their mid to late 40s. Therefore, the data allow us to examine the effects of what higher education has on health outcomes at the time when the individuals are

becoming more vulnerable to health risks and possibly develop chronic diseases.

The reform largely benefited the population who were less likely to afford private tutoring, therefore we limit our sample to the population who has reported that they were not affluent during their youth.[6] Moreover, we exclude the individuals who resided in a metropolitan city in their youth, because the reform mainly benefited students in rural areas and small cities where they had relatively few opportunities to obtain private tutoring. By restricting the sample to the group who were most likely to be affected by the policy, we obtain a strong 'first stage' relationship, that is, a large discrete increase in college education attainment in the birth cohort born in 1962. This discrete variation in college education attainment across adjacent cohorts allows us to apply the regression discontinuity design, which is explained in more detail in the next section. Our strategy is similar to that of Malamud and Pop-Eleches (2011) which examines the effects of the education reform that had a larger impact for the disadvantaged groups in Romania. In one of their specifications, they limit the sample to the more disadvantaged population where the education reform had discretely increased the eligibility to apply for university. Using this discrete increase in college eligibility among this specific group, they apply the regression discontinuity design in order to examine the effect of the increased college eligibility on the actual attendance of university. In a similar sense, we use the discrete increase in college education among the group in which the education reform had a larger impact, to examine the causal effect of education on health.

Table 1 shows the summary statistics. The first three columns contain the number of observations, the mean and the standard deviation of the variables for individuals with less than college education. Columns (5) and (6) describe the mean and the standard deviation for the sample who have received at least some college education. Individuals with at least some college education tend to be younger because the higher education attainment is higher for younger cohorts. In

[6]The data contains information about an individual's economic condition during their youth which is categorized as: very affluent, affluent, average, poor and very poor. We exclude the sample who were either very affluent or affluent during their youth.

particular, average birth year of the individuals with educational attainment less than college is 1961.7, whereas the average birth year is 1966.5 for college educated individuals. In addition, college educated individuals are less likely to be female, which reflects the educational gender gap for the cohorts born in the 1950s, 1960s and early 1970s. Furthermore, individuals with some college education have lower probability of reporting their health status as poor. Furthermore, they are less likely to have some sort of disease such as cancer, myocardial infarction, hypertension, diabetes, chronic renal failure and osteoporosis. The proportion of individuals reporting that they have some kind of disease is 42% for the individuals with no college education experience, whereas it is only 26% for individuals with at least some higher education attainment. However, since the average age is different across the two educational groups, it is hard to attribute these differences to a correlation between health and higher education.

While health measures differ across the two educational groups, the health behaviors—smoking and drinking—exhibits similar patterns. We do not observe a significant decrease in the probability of currently smoking nor in the probability of having ever smoked for individuals with at least some college education. Similar to the pattern on smoking, we do not find a large decrease in the probability of currently drinking for the college educated sample. In particular, we find that the probability of drinking is actually higher for individuals with at least some college attainment. As for the probability of being enrolled in national health insurance program, we observe that most individuals—regardless of their educational attainment—are covered due to the universal health care system. The individuals with higher education are more likely to have health checkup in a given year compared to the individuals who did not attain some higher education. Moreover, individuals with some college education have a better labor market outcome as they have a higher probability of being employed and they are slightly more likely to be living in the urban area and to be married. Finally, parents' educational level that is measured by the probability of having at least middle school education is higher for individuals with at least some college education.

4 Estimating Equation

The basic correlation between education and health in our setting can be examined by estimating the following equation:

$$Y_i = \alpha_0 + \alpha_1 Edu_i + f(birthyear_i) + \mathbf{X}_i'\Delta + \varepsilon_i \tag{1}$$

where Y_i is an outcome of interest such as the probability of having a chronic disease or behaviors that known to affect individual's health—for example, smoking and drinking. Edu_i indicates the variable for the educational attainment of individuals. Specifically, we use a dummy variable which takes value equal to one if an individual's educational attainment is greater or equal to some college. The coefficient of Edu_i, α_1, summarizes the relationship between education and a health-related outcome variable. However, the unobserved characteristics of the individuals, ε_i, is often correlated with the educational attainment of that particular person. Thus, the estimated coefficient of Edu_i is likely to be biased, thus failing to reveal the causal effect of education on health.

We overcome this endogeneity issue by utilizing the education reform which induced a large discrepancy in college education attainment across adjacent cohorts. To be specific, we adopt a regression discontinuity design using birth cohort as a forcing variable (Hahn, Todd, & Van der Klaauw, 2001; Imbens & Lemieux, 2008; Lee & Lemieux, 2010). The idea of the regression discontinuity design is to compare the outcome of the cohorts who were initially affected by the education reform of 1980 to that of the cohorts born slightly earlier and thus were less likely to be affected by the reform. As noted in Section 1, the identification strategy is similar to that of previous studies such as Oreopoulos (2006), Albouy and Lequien (2009), Malamud and Pop-Eleches (2011) and Clark and Royer (2013) which use the nationwide reform that brought different effects across adjacent cohorts. The research design relies on the fact that one cannot completely control the timing of conception or the birth of a child with an anticipation of the future reform.

In particular, among the individuals whose birth year is close to 1962, being born after 1962—and thus being more likely to benefit from the reform—can be seen as a random assignment.[7] This setting naturally leads to the regression discontinuity framework using the year of birth as a forcing variable.

To apply the regression discontinuity framework to KOWEPS data, we first confirm the discrete increase in higher education attainment at the birth year cutoff in our sample. In other words, we show that a certain portion of individuals has responded to the increased chance to engage in college education induced by the reform by actually advancing to college. In particular, we estimate the following regression equation:

$$Edu_i = \delta_0 + \delta_1 Treat_i + f(birthyear_i) + \mathbf{X}_i'\Phi + \varepsilon_i \tag{2}$$

where Edu_i is a dummy variable indicating whether or not individual i has some college education. $Treat_i$ is a variable that takes value 1 if the individual i was born on or after January, 1962 and 0 if he or she was born on or before December 1961. The coefficient of interest, δ_1, estimates the discrete change in receiving college education at the birth year cutoff and ultimately captures the effect of the educational reform on higher education attainment. $\mathbf{X}_i$ is the set of predetermined characteristics such as residence during youth and parents' level of educational, all of which are known to have an effect on the educational attainment. The function of the birth year centered at 1962, $f(birthyear_i)$, captures the underlying relation between birth cohort and education away from the cutoff. In particular, $f(birthyear_i)$ allows the main coefficient, δ_1, to identify the discrete increase in the educational outcome at the birth year cutoff in comparison to the slightly older cohorts which is arguably a good control group. Without $f(birthyear_i)$, the coefficient of interest would only reflect the average difference in the education attainment between younger

[7] Ideally, one would define the treatment group as the individuals who were born on or after March 1st, 1962. Unfortunately, the data does not contain the information about the birth month, nor the exact school year when an individual may have started school.

and older cohorts. The error term is clustered at the birth year in order to avoid complications associated with using a discrete forcing variable (Lee & Card, 2008).

After confirming the discrete increase in higher education attainment at the birth year cutoff, one can attribute the discrete change in the outcome variable as a causal effect of higher education on health. To be specific, we examine the reduced form effect of education on health related outcomes by estimating the following equation:

$$Y_i = \gamma_0 + \gamma_1 Treat_i + f(birthyear_i) + \mathbf{X}_i'\Lambda + \varepsilon_i \tag{3}$$

where Y_i is an outcome of interest such as the probability of having a chronic disease or other behaviors known to have an effect on health—for example, smoking and drinking. As in the first stage regression, the underlying relation between the outcome variable and birth cohort is controlled by $f(birthyear_i)$. In this paper, we specify $f(birthyear_i)$ by a flexible polynomial function. To be specific, we use a quadratic spline—a quadratic function of $birthyear_i$, interacted with $Treat$—which allows the quadratic relation between the outcome variable and birth cohort to differ across the birth year cutoff. In addition, we limit the window of the main analysis to those whose birth years are 15 years before and after the birth year cutoff. Admittedly the choice of the functional form for and the window are arbitrary, thus we provide various robustness checks by changing the functional form for $f(birthyear_i)$ and using alternative windows. The coefficient of $Treat$, γ_1, is our coefficient of interest and describes the causal effect of education on the health-related outcome variables. $\mathbf{X}_i$ includes variables that can affect health outcomes such as the individual's location of current residence and economic activity, and the predetermined characteristics of individuals like the parent's education attainment and their economic status during their youth. As before, the error term ε_i, is clustered at the birth year level. As documented in Lee and Lemieux (2010), this reduced form effect will capture the "intent-to-treat (ITT)" effect of higher education on health. In other words, it examines the effect of the increased chance

to obtain a college education which is induced by the education reform on the outcomes of the individuals' health.[8]

The underlying assumption required for this identification strategy is that the college education attainment and the outcome variables are smooth functions of birth year in the absence of the education reform. If this is the case, we can attribute the discontinuity in the relation between the outcome variable and birth cohort to a causal effect of higher education. This is a relatively mild assumption since the endogenous sorting across birth cohorts—parents caring about the education of their children, postponing the children's births until after 1962—is not a concern in our analysis. It is hard to imagine that any parent could have foreseen the reform in 1980 when planning births in the late 1950s to early 1960s and adjusted the timing of birth in response to the reform. Nonetheless, we show that the predetermined characteristics that could affect both education and health outcomes are smooth at the cutoff (Urquiola & Verhoogen, 2009). In particular, we show that parents' education levels are smooth around the birth year of 1962. Figure 1 presents the proportion of individuals whose father and mother's education levels are greater than primary education and also more than lower secondary education, respectively. There is no discrete change in the relationship between the parents' education level and birth cohorts at the cutoff. Moreover, the proportion of the individuals who have lived in an urban area throughout their childhood does not change discretely at the birth year cutoff. The graphical evidence is consistent with the regression result presented in Table 2 which supports little discrete change of the predetermined variables.

We further examine the possibility of endogenous sorting by plotting the density of the observation across cohorts. In particular, McCrary (2008) suggests that if there is a endogenous sorting across the cutoff, one should observe the heaping of the density after the birth year cutoff.[9] Fig-

[8]Although we focus on the this "intent-to-treat" effect, we also provide a result using the instrumental variable specification in Table 7.

[9]Exploiting this feature, McCrary (2008) suggests a test for endogenous sorting that can possibly undermine the validity of regression discontinuity design. However, we cannot directly apply the test as the running variable in our settings is a discrete variable.

ure 2 describes the density of the running variable—birth year. The density is smooth around the cutoff point, showing little sign of heaping of observation right after the birth year cohort. Thus, the figure provides evidence against the endogenous sorting across birth cohorts.

5 Results

5.1 Impact of the Reform on College Education Attainment

As a first stage, we first confirm the impact of the education reform on higher education attainment. Specifically, we document a discrete increase in the proportion of individuals with at least some college education at the birth year cutoff in Figure 3. The x-axis of the graph denotes birth year centered at 1962 and the y-axis of the graph indicates the proportion of individuals who had received at least some college education. The fitted quadratic trend in birth year for each side of the birth year cutoff is superimposed onto the graph.

This graphic illustration is confirmed by Table 3 which reports the estimates of Equation (2) for various specifications. The first column provides the results with no additional covariates other than the year fixed effects and controls for the gender and age square. The second column additionally controls for the variables that change with time, such as the location of their current residence, marital status and employment. Finally, the third column reports the regression result which further controls the predetermined characteristics of the individuals such as the location of residence during their youth and educational attainment of the parents. The regression results are consistent with Figure 3, which documents that the cohorts who were born in 1962 have a discretely higher proportion of individuals with some college education than the slightly older cohorts. The coefficient estimate of $Treat$ indicates a discrete increase in the proportion of individuals with some college education induced by the education reform, by approximately seven percentage points or 25 percent.

The p-value and the related t-statistics of the estimated coefficient of $Treat$ imply that the

reform is a valid and strong instrument for higher education attainment. In addition, the estimated coefficient is not sensitive to the inclusion of the predetermined variables or additional covariates. This finding implies that the covariates that could affect the individuals' educational attainment do not change systematically with being born on either side of the birth year cutoff. Thus, the result further supports the validity of the regression discontinuity design because being born after the birth year cutoff—thus being treated—can be regarded as a random assignment. Overall, both graphical illustration and regression results show that the exogenous education reform induced an exogenous increase in the college education attainment across otherwise comparable adjacent cohorts.

5.2 Health

In this subsection, we examine the causal impact of higher education on an individuals' health. In particular, we explore whether or not a discrete change in health outcomes is found in the birth cohorts who experienced a discrete increase in the opportunity for higher education due to the educational reform. The two health outcomes we study are subjective measures of health and the likelihood of having chronic diseases.

First, we find limited evidence supporting that college education has a positive causal effect on the subjective—self reported—measures of health. KOWEPS asks individuals the subjective rating of their health which is categorized as follows: very good, good, fair, bad and very bad. We group the two categories—bad and very bad—and examine whether an individual's self-reported health is poor. The simple correlation between higher education attainment and the probability of reporting poor health is negative as described in the panel A of Table 4. However, we find little evidence of a discrete reduction at the birth year cutoff in the fraction of individuals who reported that their health status is poor. Panel A of Figure 4 describes the probability of reporting their health status as poor in birth year. There is a smooth negative relation between the two because members of the younger cohort are less likely to have poor health, but there is no discrete

downward jump at the birth year 1962. The graphical illustration thus provides weak evidence of a return on higher education in terms of self-reported health. Consistent with the graphical evidence, the regression results do not support the positive causal effect of higher education on the self-reported health. Accordingly, Columns (1)-(3) of Panel B in Table 4 report the results for estimating Equation (2) using a dummy variable indicating self reported health being poor as an outcome variable for various specifications. For all specifications, the coefficient of interest is close to zero and statistically insignificant, that is, little discrete change in the proportion of individuals reporting their health as being poor at the birth year cutoff.

Furthermore, we do not find any evidence that suggests that higher education attainment reduces the probability of having chronic diseases such as cancer, hypertension, chronic back pain, chronic kidney disease, diabetes, hyperlipidemia, and etc. In particular, despite the strong negative correlation between having some college education and the probability of having any chronic diseases, which is described in Columns (4)-(6) of Panel A in Table 4, we do not find supportive evidence on the causal effect of higher education on probability of having chronic diseases. Panel B of Figure 4 describes the raw relation between the birth cohort and the probability of having chronic diseases. As expected, a strong negative linear relation exists between the two, reflecting the health beginning to decline with age. However, we do not observe a discrete change in the fraction of individuals with chronic diseases at the birth year cutoff. Despite a large and a discrete increase in the higher education attainment at the birth year cutoff, the fraction of the individuals with chronic conditions does not exhibit a notable discrete decrease at the birth year cutoff. Similar to the graphical evidence, the estimated coefficient of *Treat* in Columns (4)-(6) of Panel B in Table 4 which quantifies the discrete change in the fraction of individuals with diseases at the birth year cutoff is statistically insignificant. Overall, being consistent with some recent literature on the health return for K-12 education (Albouy & Lequien, 2009; Clark & Royer, 2013), we find no supportive evidence for a health return from higher education.

5.3 Health Behaviors

In this section, we try to understand the reason for limited improvement in direct health outcomes despite increased education attainment by studying health behavior. In particular, we focus on smoking and drinking behavior which are associated with potential health risks. Our research reveals a limited causal effect of higher education on the probability of currently smoking or ever smoked. This result is consistent with C. Park and Kang (2008), which documents the limited effect of K-12 education on smoking behaviors in Korea. The OLS results in the first three columns of Panel A in Table 5 exhibit a strong negative correlation between higher educational attainment on the probability of currently smoking for various specifications. However, the reduced form estimates in Panel B, evidently presents little causal effect. Columns (1)-(3) of Panel B in Table 5 summarize the results from estimating Equation (2) with a binary outcome variable that indicates whether the individual is currently smoking. The coefficient of interest is statistically insignificant, which implies little causal effect of education on the probability of currently smoking.

Next, the results on the probability of ever smoked also demonstrates limited impact of college education. As shown in Columns (4)-(6) of Panel B in Table 5, the estimated coefficient indicating the impact of higher education on probability of ever smoked is statistically indistinguishable from zero for all specifications. Panel B of Figure 5 describes the relation regarding the proportion of a cohort who currently smoke or ever smoked. Although there is a kink—a change in the slope of the average probability of smoking—at the birth year cutoff, it is difficult to observe a discrete drop in the proportion of individuals who do not currently smoke or ever smoked at the birth year cutoff.

Finally, we explore the causal effect of higher education on drinking behaviors; namely, whether a person is currently drinking. The last three columns of Panel B in Table 5 summarize the results for the reduced form effect of education on the probability of currently drinking. The results are consistent with Panel C of Figure 5 which exhibits a small discrete change in the probability of currently drinking. In particular, we find that having a college education does not

reduce the probability of currently drinking. This result might be due to the fact that, in Korea, college graduates are more likely to be economically active and drinking is a crucial feature of socializing in work life (C. Park & Kang, 2008).

In consideration of these aspects, the results in this subsection could shed light on the reason for having little health return from higher education. In particular, if higher education attainment does not induce healthier behaviors, one might not expect the causal effect of higher education on health.

6 Robustness Check and Possible Mechanism for the Results

6.1 Robustness Check

In this subsection, we provide a robustness check for the main outcomes by using various functional forms for $f(birthyear_i)$ and alternative samples for the estimation. In particular, Panel A of Table 6 provides the estimates from the alternative samples that were generated based on more narrow and wider windows around the cutoff. The results are similar to the main result when we use 10-year and 20-year windows around the birth year cutoff. Specifically, the estimates do not provide supportive evidence on the positive role of higher education on health measures and health behaviors. Panel B of Table 6 presents the estimates using alternative polynomial functions of the birth year, $f(birthyear_i)$. In particular, regardless of whether we use linear or cubic splines for $f(birthyear_i)$, the estimated coefficients which reflect the causal impact of education on health related outcomes are statistically insignificant.

Panel C of Table 6 shows the estimates based on the alternative birth year cutoffs. Specifically, we estimate the main equation using two proceedings and two following years from January 1st 1962 as an alternative birth year cutoff. Specifically, we examine the equation using pseudo-birth year cutoff by using two following and preceding years of January 1st, 1962 as an alternative birth year cutoff. We find that the results are in line with our main findings that the college education

has a limited positive effect on the health outcomes of individuals.

Finally, Table 7 provides the results from two stage least square estimate(2SLS) using $Treat_i$ as an instrumental variable for higher education attainment. The estimate of 2SLS could be interpreted as a specific type of Local Average Treatment Effect (LATE)—causal effect of higher education for those born in 1962 and for those who had advanced to the college due to the education reform (who would otherwise have not). Consistent with the main result documenting the intent-to-treat (ITT) effect of the reform, the coefficients are all statistically insignificant, thus providing little support for the causal effect of higher education on health.

6.2 Possible Mechanism for the Results

Empirical results in the previous sections suggest that higher education have limited effect on health and health behaviors. In this section, we examine possible reasons behind the limited effect of higher education on health and health behaviors in Korea.

It is hard to attribute the limited impact to the quality of the additional years of college education induced by the reform, because unlike the impact on health outcomes, the education reform has a sizable impact on the amount of earnings for male salary workers.[10]

One possible reason could be Korea's universal health care system which started in 1977. In particular, most Koreans, apart from the very poor are covered by the National Health Insurance Program and are able to receive the health care at a reasonable cost.[11] It is unlikely that receiving a college education would have a large causal impact on the probability of enrolling for the National Health Insurance Program as the majorities of individuals—over 95% in our sample—are

[10]Using KLIPS (Korean Labor & Income Panel Study) data, W. Park and Son (2015) documents a large increase in the wage of salary workers who have induced to take college education due to the education reform. Although the questionnaire is not suited for examining the labor market outcome, we have also estimated the monetary return using KOWEPS data (see Appendix A for the analysis).

[11]The health care for the rest of the population is provided by Medical Aid Program. The program is a public assistance scheme to fulfill the medical needs of the low-income household.

covered by the insurance. Moreover, the universal health care in Korea is egalitarian in the sense that the payment and reimbursement of the medical expenses does not depend on the amount of contribution to the insurance. Thus, it is likely to mitigate the inequality in health outcomes due to the income difference associated with the educational attainment.

Figure 6 illustrates that the probability of being enrolled in the National Health Insurance Program is smooth around the birth year cutoff, displaying that higher education has little causal impact on the likelihood of receiving health care in Korea. The OLS estimate in Table 8 shows a small but positive correlation between obtaining some college education and having health insurance. However, the casual estimate using the regression discontinuity design documents little and statistically insignificant effect of the higher educational attainment on the probability of enrolling for the National Health Insurance. Specifically, the estimates in Columns (1)-(3) of Panel B in Table 8 are statistically insignificant for all specifications.

We also document that the higher education does not causally affect the probability of receiving a health checkup in any given year. In particular, the National Health Insurance provides a free basic health checkup and encourages it to be taken by the insured every other year. In addition to that, many employers subsidize the cost of a health checkup for their employees. Thus, receiving a higher education is less likely to affect the probability of a having regular health checkup, which could detect any potential health problems and provide a chance to promote healthy behavior. Table 6 documents the simple correlation between higher education and having a health checkup in Panel A and the causal effect of higher education on having a health checkup in Panel B. The last three columns of Panel A in Table 6 describe a positive correlation between college education and having a health checkup. Panel B, however, documents a negligible amount of causal effect on higher education on having health checkups. The regression result supports the graphical evidence described in Panel B of Figure 6 which illustrates the average probability of having a health checkup by birth cohorts. In particular, the figure shows little discrete change in the probability for the cohorts born in 1962.

Overall, the result shows that universal health care system in Korea, along with the limited impact of higher education on the probability of receiving a basic health service, could be the possible reason for finding little impact from the education reform on health outcomes.

7 Conclusion and Discussion

In this paper, we examine the causal effects of higher education on individual health. Despite the well-documented positive correlation between education and health, it is not clear whether this correlation reflects the causal effect of education because of the endogeneity in educational attainment. To address the endogeneity, we exploit the education reform in Korea which was motivated by political reasons. The education reform discretely increased the admission openings for college education and abolished costly college-specific entrance exams. This education reform along with the compulsory school entrance law had created a sharp disparity in the opportunity to obtain college education across adjacent cohorts. Using the policy, we apply a regression discontinuity design in order to compare the education and health outcomes across otherwise comparable cohorts. We rely on the mild and reasonable assumption that underlying characteristics that may have affected a higher education attainment does not change discretely across adjacent cohorts.

Using the KOWEPS data, we confirm a discrete increase in the fraction of individuals with some college education and a college degree in the birth cohort initially affected by the reform. However, we find little supportive evidence on the causal effect of higher education on health related outcomes. Despite the discrete increase in higher education attainment at the birth year cutoff, we do not observe a discrete change in the fraction of individuals who had reported their health status as poor or who have chronic diseases at the birth year cutoff. Furthermore, we find no evidence that higher education induces healthier behaviors. The causal link between having a higher education and the probability of currently smoking or having ever smoked is documented as being weak. In addition, we do not find any supportive evidence that having a higher education

reduces the probability of currently drinking. Next, we suggest a possible mechanism for the aforementioned results, based on the institutional setting—health care system—in Korea. We show that college education has little positive impact on having health insurance and also having a regular health checkup, particularly in the Korean setting due to the universal health care system. This implies that the impact of college education may differ in settings without the public health care system or universal health care system.

Overall, the magnitude of the causal link between higher education and health documented in this paper is consistent with the recent literature that questions the health return from K-12 education. In particular, our results suggest using caution when interpreting the relationship between health and higher education attainment as the positive relation might result from the endogeneity in education and/or reverse causality between the two.

Finally, it is important to note that the effect of higher education on health documented in this paper is not the population-average effect. In other words, the casual effect of higher education might be different for the average individuals, since the estimates provided in this paper are based on the individuals who obtained a college education due to the reform in 1980. In particular, the individuals who have entered the college were on the right-tail of ability distribution—only about 15 percent of the individuals attained a college education prior to the reform. Thus, the effect of college education on health could be different for other parts of the ability distribution. However, it is not clear whether or not the effect of higher education on health would be smaller or larger at the lower part of the ability distribution.

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Table 1: Summary Statistics

Variable	edu<=high school graduate			edu>=some college or more		
	Obs (1)	Mean (2)	Std. Dev. (3)	Obs (4)	Mean (5)	Std. Dev. (6)
birth year	23936	1961.681	8.175	4334	1966.486	6.806
female	23936	0.534	0.499	4334	0.359	0.480
health poor	23924	0.134	0.341	4327	0.036	0.186
have disease	23897	0.421	0.494	4330	0.266	0.442
currently smoke	23936	0.271	0.444	4334	0.237	0.425
ever smoke	13151	0.404	0.491	2327	0.440	0.496
currently drink	23776	0.630	0.483	4325	0.705	0.456
national health insurance	20469	0.958	0.200	3712	0.999	0.037
health checkup	23930	0.423	0.494	4333	0.567	0.500
employed	23932	0.654	0.476	4333	0.808	0.394
residence urban	23936	0.894	0.307	4334	0.951	0.215
married	23936	0.836	0.371	4334	0.912	0.283
father's edu>=high school	23936	0.136	0.343	4334	0.360	0.480
mother's edu>=high school	23936	0.051	0.220	4334	0.120	0.326

All summary statistics are based on cohorts whose birth years were 15 years before and after 1962. Obs stands for the number of observations and Std. Dev. stands for the standard deviation. Health poor indicates whether an individual's self reporting health is poor or not. Have disease takes value equal to one if someone reports that he/she has some sort of chronic disease such as cancer, myocardial infarction, hypertension, diabetes, chronic renal failure and osteoporosis etc. Father's edu>=high school and Mother's edu>=high school are dummy variables for parent's educational attainment being at least a middle school graduate.

Table 2: Continuity of the Predetermined Characteristics

Dependent Variable	father's edu > primary school graduate (1)	father's edu > middle school graduate (2)	mother's edu > primary school graduate (3)	mother's edu > middle school graduate (4)	grew up in urban area during childhood (5)
Treat	0.008	-0.012	-0.013	0.019	-0.007
	(0.025)	(0.030)	(0.017)	(0.014)	(0.024)
adj. R-sq	0.111	0.074	0.124	0.042	0.067
Mean of dependent variable	0.262	0.138	0.114	0.037	0.234
N	27667	27667	27712	27712	28265

This table estimates the smoothness of predetermined characteristics of the individuals across the birth year cutoff. All specifications include a quadratic spline of birth year centered on 1962, year fixed effects, controls for sex and age square, and time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. The standard errors are clustered at the birth cohort level. Treat is defined as 1 for individuals born on or after January 1st, 1962 and 0 for individuals born on or before December 31st, 1961

*** statistical significance at the 1% level

** statistical significance at 5% level

* statistical significance at 10% level

Table 3: Effect of the Education Reform on Higher Education Attainment

Dependent variable	edu>=some college		
	(1)	(2)	(3)
Treat	0.077***	0.077***	0.063**
	(0.023)	(0.020)	(0.024)
Quadratic Spline	Y	Y	Y
Additional Control		Y	Y
Predetermined Variables			Y
adj. R-sq	0.084	0.173	0.255
Mean of dependent variable	0.153	0.153	0.153
N	28270	28265	28265

The table reports the estimated effects of the education reform on higher education attainment. The mean of the dependent variable is based on the cohorts whose birth years were 15 years before and after 1962. All specifications include a quadratic spline of the birth year centered on 1962, year fixed effects and controls for sex and age square. Specification (2) additionally controls for time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. Additionally, specification (3) controls for the predetermined variables such as a parent's education attainment and economic status during youth. The standard errors are clustered at the birth cohort level. Treat is defined as 1 for individuals born on or after January 1st, 1962 and 0 for individuals born on or before December 31st, 1961

*** statistical significance at the 1% level

** statistical significance at 5% level

* statistical significance at 10% level

Table 4: Effect of Higher Education on Health

A: OLS						
Dependent variable	health poor			have disease		
	(1)	(2)	(3)	(4)	(5)	(6)
some college or more	-0.067***	-0.038***	-0.023***	-0.066***	-0.052***	-0.038***
	(0.009)	(0.007)	(0.006)	(0.013)	(0.014)	(0.013)
Quadratic Spline	Y	Y	Y	Y	Y	Y
Additional Control		Y	Y		Y	Y
Predetermined Variables			Y			Y
adj. R-sq	0.084	0.142	0.220	0.160	0.179	0.200
Mean of dependent variable	0.147	0.147	0.147	0.410	0.410	0.410
N	28428	28427	28427	28404	28403	28403
B: Reduced Form						
Dependent variable	health poor			have disease		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	-0.018	-0.016	-0.012	0.025	0.028	0.027
	(0.020)	(0.018)	(0.018)	(0.039)	(0.037)	(0.033)
Quadratic Spline	Y	Y	Y	Y	Y	Y
Additional Control		Y	Y		Y	Y
Predetermined Variables			Y			Y
adj. R-sq	0.069	0.118	0.126	0.152	0.164	0.169
Mean of dependent variable	0.147	0.147	0.147	0.411	0.411	0.411
N	28251	28250	28250	28227	28226	28226

The estimates report the correlation between higher education on health in Panel A and the causal effect of higher education on health in Panel B. The mean of dependent variable is based on the cohorts whose birth years were 15 years before and after the year of 1962. Treat is defined as 1 for individuals born on or after January 1st, 1962 and 0 for individuals born on or before December 31st, 1961 For the definition of the dependent variables, please refer to the table notes of Table 1. All specifications include a quadratic spline of the birth year centered on 1962, year fixed effects and controls for sex and age square. Specifications (2) and (5) additionally control for time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. Additionally, specifications (3) and (6) control for the predetermined variables such as parent's education attainment and economic status during youth. The standard errors are clustered at the birth cohort level.

*** statistical significance at the 1% level

** statistical significance at 5% level

* statistical significance at 10% level

Table 5: Effect of Higher Education on Health Behavior

A: OLS									
Dependent variable	currently smoke			ever smoke			currently drink		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
some college or more	-0.141***	-0.112***	-0.116***	-0.105***	-0.085***	-0.087***	-0.027	-0.038**	-0.033*
	(0.015)	(0.015)	(0.015)	(0.013)	(0.014)	(0.014)	(0.017)	(0.018)	(0.019)
Quadratic Spline	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional Control		Y	Y		Y	Y		Y	Y
Predetermined Variables			Y			Y			Y
adj. R-sq	0.324	0.339	0.345	0.597	0.603	0.605	0.166	0.189	0.196
Mean of dependent variable	0.267	0.267	0.267	0.406	0.406	0.406	0.620	0.620	0.620
N	28447	28442	28442	15611	15610	15610	28273	28272	28272

B: Reduced Form									
Dependent variable	currently smoke			ever smoke			currently drink		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat	0.029	0.029	0.028	0.023	0.023	0.024	0.031	0.035*	0.036*
	(0.030)	(0.029)	(0.029)	(0.026)	(0.026)	(0.027)	(0.019)	(0.019)	(0.020)
Quadratic Spline	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional Control		Y	Y		Y	Y		Y	Y
Predetermined Variables			Y			Y			Y
adj. R-sq	0.297	0.322	0.329	0.584	0.593	0.598	0.166	0.187	0.194
Mean of dependent variable	0.267	0.267	0.267	0.405	0.405	0.405	0.620	0.620	0.620
N	28270	28265	28265	15478	15477	15477	28101	28100	28100

Information about having ever smoked is available from the third wave of the KOWEPS. The dependent variables are indicator variables which take value one if the individuals are currently conducting behaviors described by the variable names. The mean of dependent variable is based on the cohorts whose birth years were 15 years before and after the year of 1962. Treat is defined as 1 for individuals born on or after January 1st, 1962 and 0 for individuals born on or before December 31st, 1961 For the definition of the dependent variables, please refer to the table notes on Table 1. All specifications include a quadratic spline of the birth year centered on 1962, year fixed effects and controls for sex and age square. Specifications (2), (5) and (8) additionally control for time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. Additionally, specifications (3), (6) and (9) control for the predetermined variables such as parent's education attainment and economic status during youth. The standard errors are clustered at the birth cohort level.
*** statistical significance at the 1% level
** statistical significance at 5% level
* statistical significance at 10% level

Table 6: Robustness Checks

Dependent Variable	health poor (1)	have disease (2)	current smoke (3)	ever smoke (4)	currently drink (5)
A: Alternative Windows					
10 years	0.014	0.034	0.037	0.023	0.025
	(0.015)	(0.041)	(0.028)	(0.030)	(0.019)
20 years	-0.004	0.017	0.032	0.018	0.014
	(0.013)	(0.030)	(0.026)	(0.024)	(0.022)
B: Alternative Trends					
Linear	0.005	-0.028	0.034	0.006	0.02
	-0.012	-0.023	-0.022	-0.019	-0.017
Cubic	0.023	0.016	0.045	0.032	0.03
	(0.015)	(0.051)	(0.028)	(0.029)	(0.020)
C: Placebo Year Cutoffs					
1960	-0.011	0.025	0.039	0.031	0.025
	-0.016	-0.023	-0.033	-0.033	-0.029
1961	-0.006	0.016	0.038	0.028	0.021
	(0.014)	(0.027)	(0.026)	(0.028)	(0.019)
1963	0.001	0.028	0.018	0.026	0.055**
	(0.020)	(0.044)	(0.036)	(0.036)	(0.023)
1964	-0.005	0.031	0.025	0.059	0.072**
	(0.029)	(0.058)	(0.044)	(0.046)	(0.029)

This table reports the estimates for *Treat* in equation (3) for various sample windows, functional forms of $f(birthyear_i)$ and pseudo birth year cutoffs. For the definition of the dependent variables, please refer to the table notes of Table 1. All specifications include year fixed effects, controls for sex and age square as well as time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. In addition, all specifications for Panels A and B include a quadratic spline of birth year centered on 1962. Panel C also contains a quadratic spline of birth year centered on the corresponding pseudo birth year cutoffs.

*** statistical significance at the 1% level

** statistical significance at 5% level

* statistical significance at 10% level

Table 7: Instrumental Variable Specifications

Dependent Variable	health poor	have disease	current smoke	ever smoke	currently drink
	(1)	(2)	(3)	(4)	(5)
some college or more	-0.206	0.368	0.379	0.293	0.456
	(0.241)	(0.492)	(0.401)	(0.314)	(0.321)
adj. R-sq	0.074	0.066	0.155	0.519	0.036
Mean of dependent variable	0.147	0.411	0.267	0.405	0.620
F-statistics in first stage	14.468	14.200	14.266	12.128	14.365
N	28250	28226	28265	15477	28100

This table reports instrumental variable type estimates for equation (1) using (2) as first stage equation. All specifications include a quadratic spline of the birth year centered on 1962, year fixed effects, controls for sex and age square as well as time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. The standard errors are clustered at the birth cohort level.

*** statistical significance at the 1% level

** statistical significance at 5% level

* statistical significance at 10% level

Table 8: Health Insurance and Health Checkup

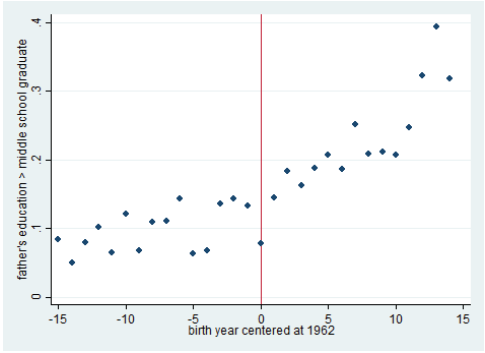
A: OLS						
Dependent variable	national health insurance			health checkup		
	(1)	(2)	(3)	(4)	(5)	(6)
some college or more	0.050***	0.025***	0.016***	0.159***	0.062***	0.062***
	(0.008)	(0.005)	(0.004)	(0.014)	(0.011)	(0.013)
Quadratic Spline	Y	Y	Y	Y	Y	Y
Additional Control		Y	Y		Y	Y
Predetermined Variables			Y			Y
adj. R-sq	0.015	0.156	0.212	0.047	0.140	0.140
Mean of dependent variable	0.945	0.945	0.945	0.415	0.415	0.415
N	24335	24332	24332	28440	28439	28439
B: Reduced Form						
Dependent variable	national health insurance			health checkup		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	-0.006	-0.010	-0.012	0.016	0.017	0.012
	(0.015)	(0.013)	(0.013)	(0.022)	(0.015)	(0.015)
Quadratic Spline	Y	Y	Y	Y	Y	Y
Additional Control		Y	Y		Y	Y
Predetermined Variables			Y			Y
adj. R-sq	0.006	0.128	0.137	0.039	0.152	0.155
Mean of dependent variable	0.944	0.944	0.944	0.415	0.415	0.415
N	24181	24178	24178	28263	28262	28262

The estimations are based on the cohorts whose birth years were 15 years before and after the year of 1962. Treat is defined as 1 for individuals born on or after January 1st, 1962 and 0 for individuals born on or before December 31st, 1961. Dependent variables are indicator variables for being enrolled in the National Health Insurance or receiving a health checkup in any given year. All specifications include a quadratic spline of birth year centered on 1962, year fixed effects and controls for sex and age square. Specifications (2) and (5) additionally control for time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. Additionally, specifications (3) and (6) control for the predetermined variables such as a parent's education attainment and economic status during their youth. The standard errors are clustered at the birth cohort level.

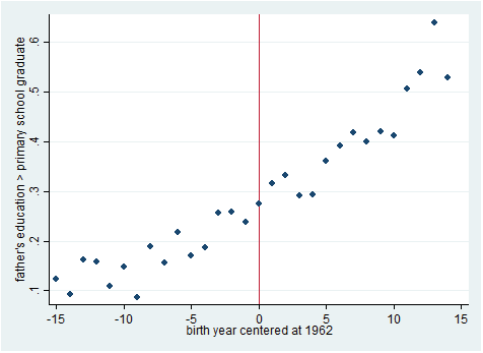
*** statistical significance at the 1% level

** statistical significance at 5% level

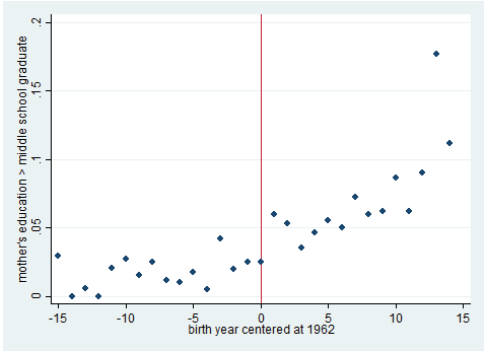
* statistical significance at 10% level



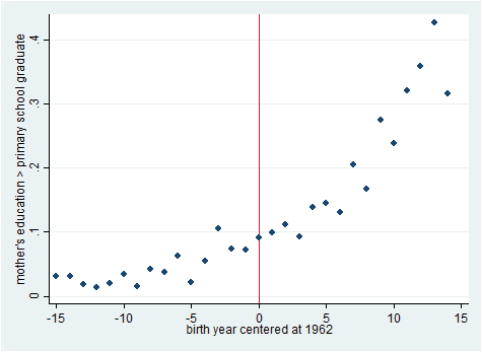
(a) Panel A : father's education> middle school graduate more



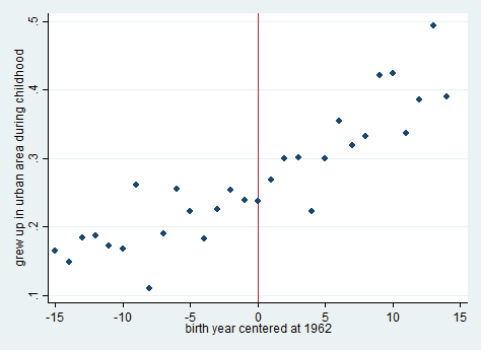
(b) Panel B : father's education> primary school graduate more



(c) Panel C : mother's education> middle school graduate more



(d) Panel D : mother's education> primary school graduate more



(e) Panel E : grew up in urban area during childhood

Figure 1: Trend in Predetermined Characteristics Across Birth Cohorts

Notes: All figure are based on the cohorts whose birth years were 15 years before and after the year of 1962. The solid circles indicate the means of the predetermined characteristics among individuals in each birth year cohort. The red vertical line describes the birth year cutoff—birth year which is equal to 1962.

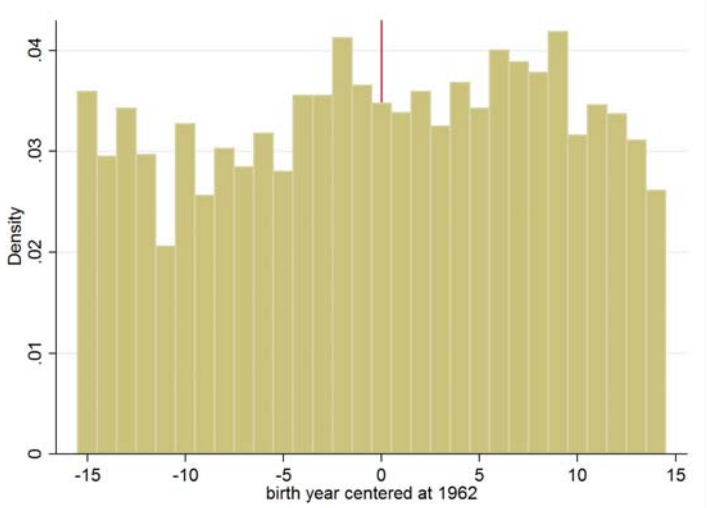


Figure 2: Density of observations across cohorts

Notes: The sample is based on the pooled waves of KOWEPS data. The vertical red line in the graph illustrates the birth year cutoff.

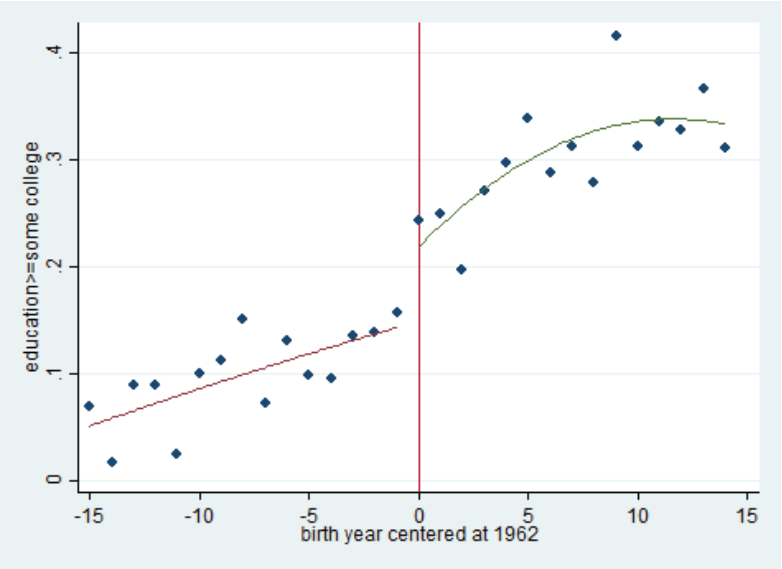
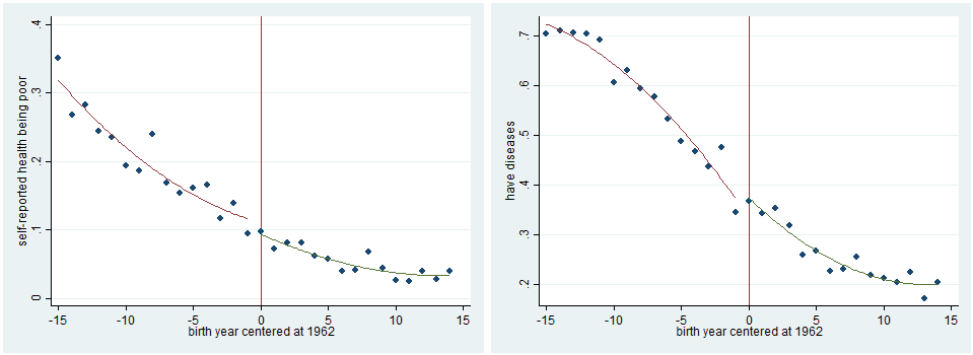


Figure 3: Impact of the Education Reform on Higher Education Attainment

Notes: The figure is based on the cohorts whose birth years were 15 years before and after the year of 1962. The solid circles indicate the average proportion of individuals with at least some college education in each birth year cohort. The solid line in the graph represents the quadratic regression line fitted separately for each side of the birth year cutoff.

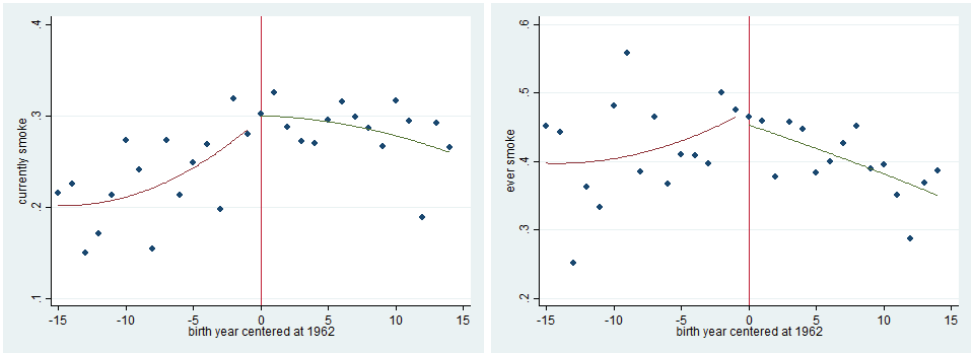


(a) Panel A : self-reported health being poor

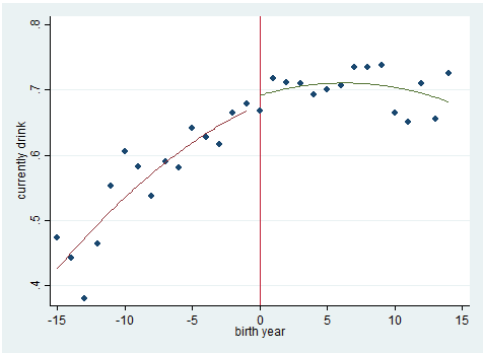
(b) Panel B : have diseases

Figure 4: Effect of Higher Education on Measures of Health

Notes: All panels are based on the cohorts whose birth years were 15 years before and after the year of 1962. The solid circles in Panels A and B indicate the means among individuals in each birth year cohort who reported their health being poor and who have any chronic diseases, respectively. The solid line in the graph represents the quadratic regression line fitted separately for each side of the birth year cutoff.



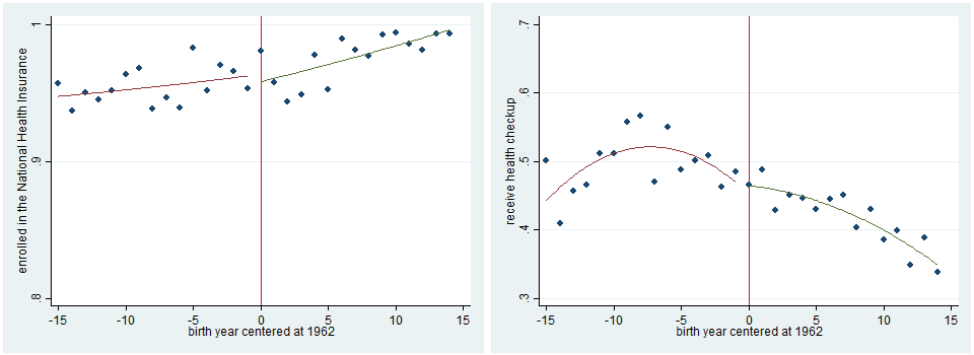
(a) Panel A : currently smoking (b) Panel B : ever smoked



(c) Panel C : currently drink

Figure 5: Impact of Higher Education on Health Behaviors

Notes: All panels are based on the cohorts whose birth years were 15 years before and after the year of 1962. The solid circles indicate the average proportion of the individuals who currently smoke, ever smoked or currently drinks by the birth year. The solid line in the graph represents the quadratic regression fitted separately for each side of the birth year cutoff.



(a) Panel A : enrolled in the national health insurance Program

(b) Panel B : receiving health checkups

Figure 6: Health Insurance and Health Checkup

Notes: All panels are based on the cohorts whose birth years were 15 years before and after 1962. The solid circles in Panels A and B indicate the proportion of the individuals who are enrolled in the National Health Insurance and who received a health checkup in any given year for each birth year cohort, respectively. The solid line in the graph represents the quadratic regression fitted separately for each side of the birth year cutoff.

Appendix A

In this appendix, we examine whether the increase in college education induced by the reform have positive monetary return. Although the questionnaire of KOWEPS is not designed to focus on the labor market outcomes, the survey asks about the annual earnings of the salary workers.[12] We exploit this information and estimate equation (3) using the log of an annual wage income of male salary workers as an outcome variable. Table A.1 summarizes the regression result. The coefficient of *Treat* in Panel B is positive and statistically significant for all specifications, implying a positive effect of educational reform on wage. Thus, the result provides the support on the argument that the magnitude of the effect of higher education on health documented in this paper is not driven by the decrease in the quality of college education as a result of the reform.

[12]The effect of higher education on labor market outcomes, such as wage and the probability of a job loss during economic crises, is well documented in W. Park and Son (2015) using KLIPS (Korean Labor & Income Panel Study) data which is more suited for examining the labor market outcomes at the individual level.

Table A.1: Effect of Higher Education on Personal Income

A: OLS			
Dependent variable	log of annual salary		
	(1)	(2)	(3)
some college or more	0.463***	0.392***	0.380***
	(0.046)	(0.045)	(0.040)
Quadratic Spline	Y	Y	Y
Additional Control		Y	Y
Predetermined Variables			Y
adj. R-sq	0.188	0.375	0.394
N	4712	4712	4712

B: Reduced Form			
Dependent variable	log of annual salary		
	(1)	(2)	(3)
Treat	0.245***	0.222***	0.198**
	(0.082)	(0.071)	(0.075)
Quadratic Spline	Y	Y	Y
Additional Control		Y	Y
Predetermined Variables			Y
adj. R-sq	0.054	0.286	0.319
N	4712	4712	4712

Panels A and B summarize the results from an estimating equation (1) and equation (3) using the log of an annual salary as an outcome variable, respectively. The estimations are based on the male salary workers whose birth years were 15 years before and after the year of 1962. Treat is defined as 1 for individuals born on or after January 1st, 1962 and 0 for individuals born on or before December 31st, 1961. All specifications include a quadratic spline of birth year centered on 1962, year fixed effects and controls for sex and age square. Specification (2) additionally controls for time varying characteristics of individuals such as the location of their current residence, marital status and economic activity. Additionally, specification (3) controls for the predetermined variables such as a parent's education attainment and their economic status during youth. The standard errors are clustered at the birth cohort level.

*** statistical significance at the 1% level

** statistical significance at 5% level

* statistical significance at 10% level