ESRI Discussion Paper Series No.162

The Effect of Age at School Entry on Education and Income

by

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June 2006

Economic and Social Research Institute
Cabinet Office
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[1] This research was conducted as part of a project by the Economic and Social Research Institute, the Cabinet Office of Japan. I thank David Card, Masahiro Kuroda, Tomohiro Machikita, Hisahiro Naito, Satoshi Shimizutani, and Ryo Kambayashi for helpful discussions and comments. I thank Yuko Ueno for helping me gain access to the government micro data. I also thank Donna Maurer for editorial assistance.

Abstract

Children start their schooling at different actual ages because schools accept entering students only once a year. Those born in a late month in a school cohort are said to be handicapped because they are behind their peers in terms of physical and mental development. Primary schools in Japan accept entering students on April 1 and children who turns 6 by that date enter primary schools. Thus, those who are born on April 1 enter primary schools at exactly age 6, while those who are born on April 2 enter primary schools at almost age 7. Thus, for any particular cohort, those who are born in April are more mature than those who are born in March in terms of their physical and mental development. This relative maturity in the same school cohort might act favorably to those who are born in April. This paper reports the effect of the month of birth on educational attainment and labor market outcomes for both sexes, using a large- scale, Japanese labor force survey. Japan is an ideal country for examining the relative age effect because the length of compulsory education does not vary by the birth month. Thus, the variation of educational attainment across birth months is necessarily induced by individual choice, and the systematic difference in individual choice by birth month is most likely to be a product of the relative age effect. The result indicates that April-born children have 0.15 year more education than March-born children, with an average educational attainment of 12.6 years among males. Similar results are obtained for females. This difference in educational attainment between March-born and April-born individuals, however, does not translate into income differences. This is because April-born individuals over-perform in terms of educational attainment, but this over-performance is not rewarded in the labor market. Evidence from the sudden decline of population size due to a cultural superstition is consistent with the relative age effect rather than the absolute age effect.

Contents

1 Introduction

Children start schooling at different actual ages because schools accept entering students only once a year. For example, primary schools in Japan accept entering students on April 1 and children who are age 6 by that day enter primary schools for that year. Thus those who are born in March (including April 1.[1]) enter primary schools at an early age 6, while those who are born in April enter primary schools at almost age 7. Thus those who are born in April have an advantage compared with those born in March in terms of physical and mental development, and this may act favorably for those born in April; this effect is called the relative age effect in the developmental psychology literature.

An almost one-year age difference at age 6, or earlier, considering the entrance into kindergarten or a daycare facility, could have a large impact on students' academic and physical performance, and if this initial difference persists, the birth month could have a lifetime impact on individuals' eventual educational attainment and labor market outcomes. Detecting whether there is a significant, relative age effect is important because if there is such an effect, the educational system could be adjusted so that those who are relatively younger in the same school cohort are not treated unfavorably.

The general public recognizes the existence of a relative age effect, and, in fact, the entrance examinations of some selective primary schools (Keio and

[1]According to Japanese law, people officially age a day before their birthday

Tsukuba, for example) are given for groups of students divided by the candidates' birth month so that those who are born relatively early in the same cohort (*Hayaumare*) are not treated unfavorably. Previous studies indicate that relatively older students in the same cohort achieve higher in schools (Thompson [1971], Allen and Barnsley [1993], Borg and Falzon [1995], Lien et al. [2005] and Datar [2005]), are less likely to be diagnosed as having specific learning disabilities (Martin et al. [2004]), and are less likely to commit suicide (Thompson et al. [1999]). Relative age effects are also found in the field of sports. Dudink [1994] and Helsen et al. [2000] reported that those born early in a cohort are more likely to play in the European professional soccer league.

Although there is an accumulation of research on relative age effects in early-stage school performance, the studies that examine the relative age effect on eventual educational attainment and labor market outcomes are just burgeoning. Exploiting the discontinuity of the school-entering age in the month before and after the cut-off day, Fredriksson and Ockert [2005] examined the effect of the school-entering day on school and labor market performance in Sweden. They found that those who enter primary school at an older age perform better in school and the labor market, based on a sample of 0.8 to 1 million observations. Fertig and Kluve [2005] implemented a similar idea using a German sample, but did not find a significant relation between the month of starting age and school outcomes. However, we should note that their results were based on a sample of less than 2,000 observations.

Identifying the relative age effect on eventual educational attainment and labor market outcomes is important because it has a life-long effect on people's welfare. This study reports the relative age effect on educational attainment and labor market outcomes for both sexes, using a large-scale, Japanese labor force survey. The research design based on Japanese data is notable because the Japanese educational system does not induce a variation of educational attainment by the birth month, as in the US, which was exploited by Angrist and Krueger [1991]. Thus if there is a correlation between the birth month and eventual educational attainment, it can be attributed to the relative age effect.

The analysis results indicate that April-born males in the sample had 0.14 year more education than March-born males, with an average educational attainment of 12.75 years. This difference, however, does not translate into an income difference among males. This is because April-born males over-perform in educational attainment, but this over- performance is not valued in the labor market. Similarly, April-born females have 0.07 year more education than March-born females, with an average education attainment of 12.41 years. Contrary to the results for males, April-born females earn 2 percentage points more than March-born females. However, once the comparison is made for (February+March)-born and (April+May)-born, the difference in income becomes insignificant. These results suggest that the additional amount of education induced by the relative age effect does not increase income, nor does the birth month affect the employment rate for either sex.

I examine the relative importance of the absolute age effect and the relative age effect. The absolute age effect hypothesis claims that those who are born early in a school cohort perform well because they are more ready for schooling in terms of physical and mental development. In contrast, the relative age effect claims that those who are born early in a school cohort do better than their peers because of their relative advantage over their peers. If the hypothesis of the absolute age effect is a valid explanation, postponing the primary school admission date to a later age might enhance students' learning. Thus distinguishing the relative and absolute age effects is important from a policy perspective. To examine the relative importance of the two effects, I exploit the sudden drop in the fertility rate of the 1966 birth cohort due to a Japanese superstition. Evidence based on this natural experiment supports the relative age hypothesis rather than the absolute age hypothesis.

The paper is organized as follows. Section 2 briefly introduces the Japanese educational institution related to this analysis. Section 3 introduces the large-scale, Japanese labor force survey, Section 4 lays out the estimation results, and Section 5 further discusses the results and checks their robustness. The last section concludes.

2 Primer of the Japanese School System

The Japanese school system is similar to that of the US. Compulsory education consists of 6 years of primary school and 3 years of junior high school. After junior high school, students can choose to attend 3 years of high school or 5 years of technical college (*Kosen*). After high school, the choices are 2 years of junior college or 4 years of college. There are graduate schools for advanced degrees afterward. For an illustration, see Figure 1.

The school system is legally defined in the School Education Law (SEL) enacted in 1947. SEL article 22 requires parents to send their children to primary schools once their children will turn age six before the school starting day, which is April 1. Article 23 allows a delay in school entry due to children's illness or underdevelopment, but this exception is rarely applied.[2] According to Japanese law, people become their new age a day before the birth day; thus, children born on April 1 enter primary schools at exactly age 6, while those born on April 2 enter primary schools at almost age 7. So there is about a one-year chronological age difference among students in the first grade. SEL article 39 requires parents to send their children to junior high schools by the end of the school year in which the children turns age 15. Thus 9 years of education is required for all children regardless of their birth month.

[2]In 2004, 2,261 children at the primary school age (ages 6 - 12) did not attend primary schools. At the same time, 7,200,933 children attended primary schools. The percentage of exemption is 0.03 percent.

This requirement for the school-leaving date in Japan contrasts with that of the US, where the school leaving date is defined by age. Those who turn 16 or 17 are allowed to leave school in many states, and those who enter primary school at an older age (measured in months) tend to have fewer years of education because they are allowed to leave school earlier. This variation in the years of education created by the institutional setting is exploited by Angrist and Krueger [1991]. The Japanese institutional setting does not create a variation in the number of years of schooling by birth month. Therefore, if there is the variation in the years of education depending on the birth month in Japan, the variation is induced by individual choice.

Pre-primary school education is quite popular. As of 2004, about 90 percent of children ages 3 to 5 attend either a day-care center (about 39 percent) or kindergarten (about 50 percent).[3] Officially, day-care facilities are non-educational institutions, and there is no clear educational curriculum-based teaching nor class formation based on age cohort. In contrast, kindergartens form classes based on children's ages. Those who are ages 3, 4, and 5 by April 1 are sorted into classes corresponding to their ages. Thus among kindergarten attendees, the relative age effect is in motion.

[3]According to School Basic Statistics (*Gakkou Kihon Tokei* by the Ministry of Education, 1,753,393 pupils attended kindergarten classes for 3, 4, 5 year olds in 2004. The Welfare Administration Record (*Fukushi Gyosei Houkoku Rei*) by the Ministry of Welfare and Labor reported that 1,348,754 children between the ages of 3 and 5 attended day-care centers in 2004, and the population of children between the ages of 3 and 5 was 3,504,000 in 2004, according to the Annual Report of Population Estimates by the Ministry of Internal Affairs and Communications. Based on these figures, 50 percent of children between the ages of 3 and 5 attended kindergarten, and 39 percent of them attended day-care centers.

3 Data

The Employment Status Survey (ESS) 2002 (*Shugyo Kozo Kihon Chosa*) is used in this study. The ESS was conducted on household members ages 15 and older in approximately 440,000 households dwelling in sampled units that cover the complete population.[4] The survey collects information of October 1, 2002 regarding the household members and the labor force status of each individual member. This study utilizes micro data and extracts the information on birth year, birth month, marital status, educational attainment, employment status, and annual income from the main job over the previous year.

The file contains 968,628 individuals, with 459,939 males and 508,689 females. The analysis sample is restricted to those aged 25-60 and out of school to restrict our attention to the completed years of education. Also, the inclusion of those age 60 or below in 2002 assures that people were born in 1942 or later and reached age 6 in 1948 at the earliest. Because Okinawa prefecture was occupied until 1973 by the US and subject to a different school system from mainland Japan, the observations from Okinawa were dropped. This assures that virtually all individuals in the sample were subject to the current educational institutions adopted after World War II. The sample is further restricted to observations with a valid birth year, birth month, mar-

[4]Foreign diplomats, foreign military personnel and their dependents, persons dwelling in camps or ships of the Self Defense Force, and persons serving sentences in correctional institutions are excluded.

ital status, educational background, and employment status. The analysis sample includes 259,756 males and 267,838 females. For junior high school graduates, high school graduates, 9 and 12 years of education are assigned, respectively. Fourteen years of education is assigned for junior college or technical college graduates, and 16 years of education is assigned for 4-year college graduates or graduate school graduates. The survey records the annual earnings in ranges. These ranges are transformed into a continuous variable by using the center value for each range. For the highest range, more than 150 thousand yen, 150 thousand is assigned.

The means of the variables by birth year - birth month appear in the Figures. The number of observations reported in Figure 1 indicates that observations are more likely to be taken from winter months (i.e., between January and March) than summer months (i.e., between June and August). This seasonal pattern of births is observed among elder generations, because farmers tend to deliver their babies during the agricultural off-season. These figures imply that birth month might carry information about parental occupation and other socio-economic background characteristics. In particular, those born in the winter might be more likely to have a farm background.

Figure 3 shows the month-to-month variation in the eventual years of education. Given the possibility that summer- and winter-born individuals have different socioeconomic backgrounds, it is not surprising to find fewer years of education among the winter-born than the summer-born, as reported in Figure 3. Thus, to identify the relative age effect on educational attainment, it is important to focus on the discontinuity of educational attainment between the March-born and the April-born because the discontinuity at this point is arguably created by the cut-off date of primary school admission, which consistently has been April 1 in Japan since 1947. The only sharp gap in educational attainment is found between March and April, as expected. After adjusting for the difference in the eventual years of education across school cohort groups, there is about a 0.14-year gap between the March-born and the April-born.

Figure 4 indicates that there is virtually no annual income gap between March-born and April-born males. In contrast, there is a sharp annual-earnings gap between these two groups. However, once the windows of comparisons are widened to two months before and after the cut-off point, the February+March born and the April+May born do not seem to have a gap in annual income. Figure 5 indicates the employment rate is virtually continuous between March-born and April-born. No gap in the employment rate assures the validity of taking a look at the earnings of the working population because we do not have to worry about sample-selection bias. Figure 5 indicates a fairly continuous marriage rate across birth months.

Because our sample is household-based, we can identify the father's education, given co-residence with father. Figure 6 plots the father's years of education by the birth month of his children as a proxy of socio-economic status. For both males and females, those born in August had fathers with more years of education. This is probably because those who were born in

August were less likely to have a non-farm background. It should, however, be noted that these figures are calculated given co-residence with father and thus issues of sample-selection should be kept in mind.

4 Estimation Results

4.1 The Discontinuity of Variables by Birth Month

Tables 1 and 2 report the formal test of the differences in individual characteristics by birth month. All the specifications include school-cohort dummy variables to capture the time-series changes in years of education, annual income, employment rate, etc. These analyses indicate that the gap in the years of education between March-born and April-born individuals is statistically significant.

The regression results for years of education indicate a discontinuity between the March-born and the April-born, probably because of the relative age effect. It is then interesting to examine at which margin of the years of education the discontinuity between the March-born and the April-born occurs. Identifying the margin of treatment is important to interpret the results on earnings because the local, average treatment effect interpretation of the IV estimator by Imbens and [1994] and Angrist et al. [1996] offers a unified theme for interpreting the results of birth month on education and earnings. The margin of the treatment is identified by running the following regression for s=12, 14, and 16:

$$1(S \geq s) = z\beta + \textit{school cohort dummies} + u, \tag{1}$$

where $1(\cdot)$ is an indicator function and z is a set of birth-month dummy variables. The results of the regression appear in Table 3. The results indicate that being born in April instead of March uniformly increased the probability of finishing high school ($s = 12$), finishing junior college or technical college ($s = 14$), and finishing 4-year college ($s = 16$) by 2 percentage points for males and by 1 percentage point for females.

In contrast to the results for education, there was no statistically significant gap between the March-born and the April-born in terms of annual earnings, the employment rate, or the marriage rate, as reported in Tables 1and 2.

Column (2) of Tables 1 and 2 reports the regression result of the employment status on dummy variables for the birth months, using the male sample. The dependent variable is the dummy variable that indicates being employed and having valid annual earnings information. The result shows that none of the coefficients for the dummy variables is statistically significant. These results suggest that there is no variation in the employment rate induced by differences in birth month. Confirming no systematic selection into employment depending on birth month, we can interpret the regression result of the log annual income on the birth months dummy variables without worrying about sample-selection bias.

The regression result for the log annual income among males is reported in Column (3) of Table 1. The result for males indicates that there is no gap in the log annual earnings between the March-born and the April-born. Regardless of the fact that the April-born had 0.14 year more education than the April-born, the April-born earned as much as the March-born. This implies that having greater educational attainment induced by the relative age effect does not induce an increase of earnings. Those who were born in April did better in terms of educational attainment due to their subtle advantage, but the greater amount of education induced by this advantage was not rewarded in the labor market. It is worth noting that those who were born in August and September earned about two percent more than the April-born. This is probably because those who were born in August or September were likely to have an urban background and higher socio-economic status.

The regression result for the log annual income among females, which is reported in Column (3) of Table 2 contrasts with the result for males: The April-born earned 2 percent more than the March-born. This might suggest a positive return to an additional year of education induced by the relative age effect; however, we should notice that the May-born also earned 2 percent less than April-born. Thus, if the comparison windows are extended for two months, as we will see, there was no significant gap between the February+March-born and the April+May-born.

In term of father's years of education, there was no statistically significant difference between the March- and the April-born among males, as reported in Column (5) of Table 1. However, among females, the March-born had fathers with significantly more education than the April-born, as reported in Column (5) of Table 2. The fact that March-born females had fathers with more education might cause the downward bias in the estimate of the relative age effect. This is because the gap in years of education due to the relative age effect was partly canceled by the better family background of March-born females. To address this potential problem, the regression model with father's educational background as an additional covariate is estimated in a later section.

4.2 The Implied Return to Education

Based on the findings reported so far, it is interesting to discuss the return to additional years of education induced by the relative age effect. To implement this exercise, the analysis sample is basically restricted to the March-born and the April-born because previous results suggested a difference in the socio-economic background between those who were born in winter and summer. The March-born and the April-born presumably shared the same socio-economic background, but the age at school entry was discontinuously different. The continuous socio-economic background between the March-born and the April-born was evidenced by such continuous background variables as marriage rate and father's education, as far as the male sample is concerned. The discontinuity in years of education was arguably created by the cut-off day of school admission. Thus this research is based on a regression discontinuity design.

The concept of the local average treatment effect by Imbens and [1994] and Angrist et al. [1996] offers a unified theme for interpreting the two regressions of education on birth month and annual earnings on birth month. The IV estimate

$$\beta^{LATE} = \frac{E[\ln(inc)|april,\ school\ cohort] - E[\ln(inc)|march,\ school\ cohort]}{E[educ|april,\ school\ cohort] - E[educ|march,\ school\ cohort]} \tag{2}$$

identifies the return to an additional year of education induced by the relative age effect.

To implement this identification, the analysis sample is restricted to the March- and April-born, and a dummy variable that indicates April born, *april*, is created. Using the first-stage regression model:

$$educ = \gamma april + school\ cohort\ dummies + u \tag{3}$$

and the reduced form regression model:

$$\ln(inc) = \delta april + school\ cohort\ dummies + v, \tag{4}$$

we can show that

$$\beta^{LATE} = \frac{\delta}{\gamma}, \tag{5}$$

under the assumptions $E[u|april,\ school\ cohort] = E[v|april,\ school\ cohort] = 0$. The actual estimation of β^{LATE} is made by using the standard IV procedure of regressing $\ln(inc)$ on *educ* and age and city dummies using *april* as an IV for *educ*, but the estimate should not be interpreted as the return to education in the usual sense. Instead, β^{LATE} estimates the return to an additional year of education induced by the relative age effect.

Table 4 reports the estimation results based on the male sample. Table 4 Column (1) reports the return to education estimated by OLS in the usual sense, just for reference. Table 4 Column (2) reports the result of the first-stage regression. It indicates that the April-born had 0.14 year more education than the March-born. Table 4 Column (3) reports the estimation result for the reduced-form equation. As found in the previous section, those who were born in April did not earn more than those who were born in March. The result in Column (4) combines the results of Columns (2) and (3), and the estimated return to an additional year of education induced by the relative age effect is 0.04, but statistically insignificant. The advantage of being born in April is that it makes people stay in school longer, but this additional year of education is not rewarded in the labor market.

Table 5 reports the estimation results based on the female sample. The return to education estimated by OLS is reported in Column (1), just for reference. Column (2) reports the first-stage regression result, which indicates that the April-born had 0.07 year more education than the March-born. The reduced-form estimation result reported in Column (3) indicates that the April-born earned about 2 percent more than the March-born. Combining the results of Columns (2) and (3) together, the LATE estimate reported in Column (4) indicates that an additional year of education due to the relative age effect reaped a 19-percent return in the labor market. The advantage of being born in April is that it makes people stay longer in school, and the effect of this additional year of education is not washed out in the labor market.

Before concluding that there is a positive return to education due to the relative age effect among females, extra attention should be paid to the fact that there was a positive spike in the average annual income of April-born women, as shown in Figure 4. This positive spike could have been created by sampling error due to the relatively small sample size of 23,000 observations of the April-born. If the relative age effect is the true mechanism that increases the annual income of April-born females, it should equally raise the annual income of May-born females, though presumably to a smaller degree. By the same token, if the relative age effect is disadvantageous for the March-born, the same should apply to the February-born. To increase the efficiency of the estimation, the same estimation is repeated, using the sample that consists of those who were born between February and May. Instead of comparing the education and annual income of the March-born and the April-born, the (February + March)-born and the (April + May)-born are now compared. In other words, we extend the sample window for both the control and treatment groups to increase the estimation's efficiency.

The results of the estimation using the February-May-born as a sample appear in Tables 6 and 7. The results for males reported in Table 6 are almost identical to those obtained from the March-April sample reported in Table 4.

This arguably assures the appropriateness of using the February-March born sample to identify the relative age effect. The results for the female sample reported in Table 7 are almost identical to those based on the March-April sample reported in Table 5, except for the annual income regression. The (April+May)-born earned 1 percent more than the (February+March)-born, but this difference is not statistically significant. The implied return to an additional year of education induced by the relative age effect was 10 percent, but this is not statistically significant.

All in all, the analysis suggests that those who were born early in a school cohort had more years of education. In particular, among males, those who were born in April had 0.14 year more education than those who were born in March. The corresponding number for females is 0.07 year. The college-attendance rate was 2 percentage points and 1 percentage point higher for males and females, respectively. These effects are not negligible, considering that the average college attendance rate was 27 percent for males and 9 percent for females in the analysis sample.

Regardless of the significant, relative age effect in terms of education, a robust relative age effect is not found for annual income. This implies that the additional year of education induced by the relative age effect did not lead to a higher income in the labor market. In other words, March-born people earned more than April-born people, given the same amount of education.

5 Discussion

5.1 Birth months and socio-economic background

As we have seen, there used to be a seasonal pattern of birth months during the 1950s and 60s. This seasonal pattern of birth months likely occurred because agricultural households were more likely to have babies in the agricultural off-season, which is winter. Thus the month of birth may be correlated with the family's socio-economic background. Although there is no a priori reason to doubt the difference in socio-economic family background between the March-born and the April-born, it is a useful exercise to examine the relative age effect while controlling for family background.

Father's education is used as a proxy for socio-economic family background. Among the people who lived with their fathers in the same household, fathers' years of education are identified. Categorical dummy variables for the father's years of education are created, and these variables are included in both the education and income equations.

The results of the estimation with the dummy variables for father's years of education appear in Table 8 for males and Table 9 for females. The results for males are almost identical to the estimation results without father's years of education that appeared in Table 4.

The difference in the results for females is notable. After controlling for father's years of education, the April-born had 0.17 year more education. This result contrasts with the result obtained without controlling for father's years of education, which was 0.07, as reported in Table 5. The change in the result comes from the fact that March-born females tended to have fathers with more education, as evidenced in Figure 7. The difference in the coefficients comes from the fact that April-born people had more education than March-born people, regardless of whether they had fathers with shorter years of education.

We should be careful, however, that these results are obtained for individuals who lived with their fathers. The decision of co-residence is presumably endogenous, and this estimation might suffer from sample-selection bias. What we should take from these tables is that the results without father's years of education do not suffer from obvious omitted variable bias due to omitted family background variables.

The other way to examine the differences in socio-economic background that could be correlated with the month of birth is to divide the analysis sample into periods. The seasonality of the birth-month distribution is most notable in the period between 1945 and 1954. This seasonality almost disappeared after 1965. Thus, if there is a correlation between the month of birth and socio-economic status, the correlation should be stronger for the period between 1945 and 1955. If the correlated, socio-economic background creates a gap between the March-born and the April-born in terms of educational and labor market outcomes, we should find a gap for the people who were born during the period between 1945 and 1954 rather than for the people who were born between 1965 and 1974.

The results of the regression applied for the divided sample appear in Tables 10 and 11. For males, the results are fairly stable over the sample period, except that the gap in the years of education between the March-born and the April-born is larger for those who were born between 1945 and 1954.

The results for females reported in Table 11 are also fairly stable over the sample period. Although the size of the coefficients fluctuates, what is important is that we confirm the gap in educational attainment between the March-born and the April-born, even among the birth cohort of 1965-1974 for which the seasonal variation of the birth-month distribution is not present.

5.2 Absolute age effect or relative age effect?

The discussion so far assumes that the gap in educational attainment between the March-born and the April-born is due to the relative age effect. However, previous studies have tended to ask about the optimal school-starting age (Fertig and Kluve [2005] and Datar [2005]). Their implicit presumption is that the effect of age at school entry on educational attainment is due to differences in the degrees of children's physical and mental development. Fredriksson and Ockert [2005] go a step further and ask whether the effect of age at school entry on educational outcomes is due to the absolute age or relative age of a subject within his/her school cohort. They regressed test scores on the absolute and relative measures of age at school entry. They found a large coefficient for absolute age and a small coefficient for relative age and thus dismissed the relative age effect. However, the birth-month distribution within a school cohort does not seem to vary much over time and their test for the relative age effect might not have sufficient power.

To separately identify the absolute and relative age effect, I exploit the sudden drop of birth cohort size in 1966 due to the superstition of the Fire-Horse (*Hinoeuma*) year. The Fire-Horse year comes every 60 years according to the Chinese zodiac calendar. Japanese people share a superstition that women born in this year are hot-tempered and unmarriageable. Due to this superstition, people try to avoid having babies in this specific year and the number of births drops by 25 percent compared with adjacent years.[5]

We can confirm that the number of observations who were born in 1966 is about 25 percent smaller in our analysis sample, as shown in Figure 8. Careful examination of this figure suggests an interesting variation in the number of observations by birth month. The number of observations jumped at the beginning of year 1967 because "the Hinoeuma sanction" was lifted. Due to this huge swing in the population size from one month to the next, there are many people who were born between January and March in the school cohort starting in April 1966. Thus, if the relative age effect is in motion, the gap in the years of education between the April-born and the March-born should be smaller for this school cohort because the March-born

[5] Kurosu [1994] nicely summarizes the Fire-Horse superstition and its effect on fertility, the sex ratio, and the marriage rate. Kaku and Matsumoto [1975] found a similar drop in the fertility rate among Japanese Americans in California and Hawaii. In Korea, women who are born in Horse year that comes every 12 years are stigmatized in the marriage market. Lee [2005] exploited this superstition to estimate the effect of marriage on labor force participation among Korean females.

in this school cohort was not as handicapped as those in other school cohorts. By the same token, those who were born in March 1966 should be the most seriously handicapped because their relative age position in the same school cohort is behind that of other students.

The prediction from the relative age effect hypothesis is tested by running a regression of outcome variables on the April-born dummy and the interaction terms of the 1965 school cohort × April-born dummy and the 1966 school cohort × April-born dummy, using the sample that consists of observations of the March- and April-born. The relative age hypothesis predicts a positive coefficient for the first interaction and a negative coefficient for the second interaction. The sample is restricted to the school cohort of the 1960s.

The results of the regression are reported in Table 12. For males, The April-born had 0.18 year more education than the March-born in the usual years. However, for the 1966 school cohort, the April-born had 0.04 year less education than the March-born, as evidenced by the negative coefficient for the interaction term of 1966 school cohort × April-born dummy variable. Although the coefficient for the interaction term is not statistically significant, the size of coefficient is suggestive for the relative age effect. The school cohort of 1965 does not seem to be different from other school cohorts in the 1960s. The school cohort around 1966 does not seem to be different in terms of its income structure.

For females, the coefficient for the interaction term of the April dummy and the 1965 school cohort dummy is positive and statistically significant. This suggests that those who were born in March 1966 were seriously held back compared with those who were born in April 1965. This evidence, again, is consistent with the relative age effect hypothesis. We should, however, keep in mind that those females who were born in 1966 were stigmatized, and this stigma might have directly affected their educational outcomes.

Overall, the evidence from the Fire Horse (*Hinoeuma*) is consistent with the relative age effect hypothesis as an explanation for why April-born people tend to have more education than March-born people.

6 Conclusion

This paper examined whether those who are older in the same school cohort do better than their younger counterparts in terms of educational attainment and labor market outcomes using a Japanese labor force survey that records birth months. The phenomena that older children in a school cohort take advantage of their physical and mental maturity is called the relative age effect in the developmental psychology literature, and it is widely confirmed in education performance and sports.

This paper exploits the feature of the Japanese school system that defines the school-entering timing by the child's age on March 31. Those who are born in March enter primary schools at the beginning of age 6, while those who are born in April start at the end of age 6 or almost at age 7. Thus those who are born in March are presumably handicapped compared with those who are born in April. Exploiting this feature, we can identify the relative age effect by examining the discontinuity of educational attainment and labor market outcomes between March-born and April-born individuals. Japan is the ideal country for examining the relative age effect because the law requires a uniform 9 years of compulsory education irrespective of birth month. Accordingly, variations in educational attainment across birth months are generated purely by the relative age effect.

The analysis results indicate that April-born males had 0.14 year more education than March-born males, whereas April-born females had 0.07 year more education than March-born females. The initial advantage of April-born over March-born children in primary school persists and eventually develops into a difference in eventual educational attainment. However, this difference in educational attainment does not turn into a difference in annual income. An additional year of education induced by the relative age effect is not rewarded in the labor market. Thus the relative age effect washes out in the labor market.

Two hypotheses are explored to explain the difference in educational attainment between April-born and March-born individuals. One hypothesis is the absolute age effect, which claims that absolute age at school entry determines an individual's performance in school. In contrast, the relative age hypothesis claims that the age at school entry relative to the ages of other students determines school performance. The analysis that exploits the large variation in the distribution of birth months within a school cohort is consistent with the relative age effect hypothesis.

The strong evidence for the relative age effect in terms of the years of education calls for further studies that examine the effect of birth month on educational outcomes. The results so far suggest a need for a policy adjustment that can soften the disadvantage of those who are younger in the same school cohort, particularly at the early stage of primary education. At least, letting teachers know the disadvantage of relatively younger children in a class might lead them to give such children extra attention.

The use of school tracking during the early stage without giving careful consideration to the relative age effect will exacerbate the situation. The Japanese public school system usually starts tracking in high school, but some local governments have initiated tracking during junior high school in the last several years. Extra attention should be paid to the relative age effects at the time of students' admission.

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Table 1: Discontinuity by Birth Month, Males 25-60 in 2002

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Years of Education	Employed	Log(Income)	Married	Father's Education
May	-0.01	-0.00	0.01	-0.00	-0.06
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
June	-0.01	0.00	0.01	0.00	-0.02
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
July	-0.01	0.01	0.01	-0.00	0.00
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
August	0.05	0.00	0.02	-0.00	0.07
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
September	-0.03	0.00	0.02	0.00	0.09
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
October	-0.05	0.00	0.01	0.00	-0.02
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
November	-0.08	0.00	0.01	0.01	0.07
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
December	-0.08	-0.00	0.00	0.00	0.04
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
January	-0.10	-0.00	0.00	0.01	0.05
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
February	-0.12	-0.01	0.01	0.00	0.02
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
March	-0.14	-0.01	-0.01	0.01	0.05
	(0.02)	(0.00)	(0.01)	(0.00)	(0.04)
Constant	11.46	0.90	5.64	0.70	9.85
	(0.04)	(0.01)	(0.02)	(0.01)	(0.20)
Observations	259756	259756	237542	259756	58188
R-squared	0.06	0.17	0.07	0.02	0.09

Note: Heteroskedasticity robust standard errors are in parentheses. All specifications include school cohort dummy variables. Father's education is available given that the father co-resides.

Table 2: Discontinuity by Birth Month, Females 25-60 in 2002

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Years of Education	Employed	Log(Income)	Married	Father's Education
May	0.00	-0.00	-0.02	-0.00	-0.03
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
June	-0.02	-0.00	-0.00	0.00	0.04
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
July	-0.02	-0.00	-0.01	-0.01	0.04
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
August	-0.00	-0.01	-0.00	-0.00	0.11
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
September	-0.00	-0.00	-0.00	-0.01	0.05
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
October	-0.03	0.00	-0.00	0.01	0.02
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
November	-0.04	-0.00	-0.01	0.00	0.04
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
December	-0.07	-0.01	-0.02	0.01	0.09
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
January	-0.03	-0.01	-0.01	0.01	0.11
	(0.02)	(0.00)	(0.01)	(0.00)	(0.06)
February	-0.06	-0.00	-0.02	0.01	0.07
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
March	-0.07	-0.01	-0.02	0.01	0.15
	(0.02)	(0.00)	(0.01)	(0.00)	(0.07)
Constant	10.96	0.81	4.55	0.44	10.82
	(0.03)	(0.01)	(0.02)	(0.01)	(0.47)
Observations	267838	267838	177533	267838	28574
R-squared	0.14	0.11	0.02	0.02	0.06

Note: The same note applies as in Table 1.

Table 3: The effect of birth month on the years of education and annual income.

	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Male	Male	Female	Female	Female
Dep. Var.	1(Educ≥12)	1(Educ≥14)	1(Educ≥16)	1(Educ≥12)	1(Educ≥14)	1(Educ≥16)
May	-0.00	0.00	-0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
June	0.00	-0.00	-0.01	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
July	-0.00	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
August	0.00	0.01	0.01	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
September	-0.01	-0.00	-0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
October	-0.01	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
November	-0.01	-0.01	-0.01	-0.01	-0.01	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
December	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
January	-0.01	-0.02	-0.02	0.00	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
February	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
March	-0.02	-0.02	-0.02	-0.01	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.61	0.17	0.15	0.57	0.10	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Observations	259756	259756	259756	267838	267838	267838
R-squared	0.07	0.04	0.02	0.11	0.09	0.02

Note: Heteroskedasticity robust standard errors are in parentheses. All specifications include school cohort dummy variables.

Table 4: Implied return to education, Male sample, March and April born only

	(1)	(2)	(3)	(4)
Dep. Var.	Log(Income)	Years of Education	Log(Income)	Log(Income)
Est. Method	OLS	OLS	OLS	IV
Education	0.09	-	-	0.04
	(0.00)			(0.05)
April	-	0.14	0.01	-
		(0.02)	(0.01)	
Observations	41956	45893	41956	41956
R-squared	0.16	0.07	0.07	0.13

Note: All specifications include a constant and school cohort dummy variables. Heteroskedasticity robust standard errors are in parentheses.

Table 5: Implied return to education, Female sample, March and April born only

	(1)	(2)	(3)	(4)
Dep. Var.	Log(Income)	Years of Education	Log(Income)	Log(Income)
Est. Method	OLS	OLS	OLS	IV
Education	0.12	-	-	0.19
	(0.00)			(0.12)
April	-	0.07	0.02	-
		(0.02)	(0.01)	
Observations	31711	47746	31711	31711
R-squared	0.07	0.15	0.02	0.05

Note: The same note applies as in Table 4.

Table 6: Implied return to education, Male sample, February-May born only

	(1)	(2)	(3)	(4)
Dep. Var.	Log(Income)	Years of Education	Log(Income)	Log(Income)
Est. Method	OLS	OLS	OLS	IV
Education	0.09	-	-	0.03
	(0.00)			(0.04)
April-May	-	0.12	0.00	-
		(0.02)	(0.00)	
Observations	82006	89770	82006	82006
R-squared	0.16	0.06	0.07	0.12

Note: The same note applies as in Table 4.

Table 7: Implied return to education, Female sample, February-May born only

	(1)	(2)	(3)	(4)
Dep. Var.	Log(Income)	Years of Education	Log(Income)	Log(Income)
Est. Method	OLS	OLS	OLS	IV
Education	0.12	-	-	0.10
	(0.00)			(0.10)
April-May	-	0.07	0.01	-
		(0.01)	(0.01)	
Observations	61446	92726	61446	61446
R-squared	0.07	0.15	0.02	0.07

Note: The same note applies as in Table 4.

Table 8: Controlling Father's Education, Male sample, March and April born only

	(1)	(2)	(3)	(4)
Dep. Var.	Log(Income)	Years of Education	Log(Income)	Log(Income)
Est. Method	OLS	OLS	OLS	IV
Education	0.08	-	-	0.05
	(0.00)			(0.07)
April	-	0.19	0.01	-
		(0.04)	(0.01)	
Observations	9414	10464	9414	9414
R-squared	0.14	0.16	0.09	0.13

Note: All specifications include a constant, school cohort dummy and father's education category dummy variables. Heteroskedasticity robust standard errors are in parentheses.

Table 9: Controlling Father's Education, Female sample, March and April born only

	(1)	(2)	(3)	(4)
Dep. Var.	Log(Income)	Years of Education	Log(Income)	Log(Income)
Est. Method	OLS	OLS	OLS	IV
Education	0.11	-	-	0.19
	(0.01)			(0.12)
April	-	0.17	0.03	-
		(0.04)	(0.02)	
Observations	4105	5093	4105	4105
R-squared	0.09	0.20	0.03	0.05

Note: The same note applies as in Table 8.

Table 10: Stable Results over Time; Male Sample

		(1)	(2)	(3)	(4)
Sample Period		Log(Income)	Years of Education	Log(Income)	Log(Income)
	Estimation Method	OLS	OLS	OLS	IV
1945-1954	Years of education	0.10			0.03
		(0.00)			(0.07)
	April		0.18	0.00	
			(0.04)	(0.01)	
	Observations	13420	14668	13420	13420
	R-squared	0.11	0.02	0.00	
1955-1964	Years of education	0.10			0.05
		(0.00)			(0.11)
	April		0.11	0.00	
			(0.04)	(0.01)	
	Observations	11453	12147	11453	11453
	R-squared	0.12	0.00	0.01	
1965-1974	Years of education	0.07	-	-	0.39
		(0.00)			(0.26)
	April	-	0.10	0.02	-
			(0.04)	(0.01)	
	Observations	10673	11546	10673	10673
	R-squared	0.11	0.00	0.04	

Note: All specifications include a constant, and school cohort dummy variables. Heteroskedasticity robust standard errors are in parentheses.

Table 11: Stable Results over Time; Female Sample

		(1)	(2)	(3)	(4)
Sample Period		Log(Income)	Years of Education	Log(Income)	Log(Income)
	Estimation Method	OLS	OLS	OLS	IV
1945-1954	Years of education	0.10			0.33
		(0.00)			(0.21)
	April		0.07	0.03	
			(0.03)	(0.02)	
	Observations	10523	15224	10523	10523
	R-squared	0.04	0.03	0.00	
1955-1964	Years of education	0.12			0.21
		(0.01)			(0.27)
	April		0.04	0.01	
			(0.03)	(0.02)	
	Observations	9112	12702	9112	9112
	R-squared	0.05	0.01	0.00	0.02
1965-1974	Years of education	0.14			0.31
		(0.01)			(0.24)
	April		0.10	0.02	
			(0.03)	(0.02)	
	Observations	7318	12043	7318	7318
	R-squared	0.10	0.01	0.02	

Note: The same note applies as in Table 10.

Table 12: Evidence for the relative age effect rather than the absolute age effect:

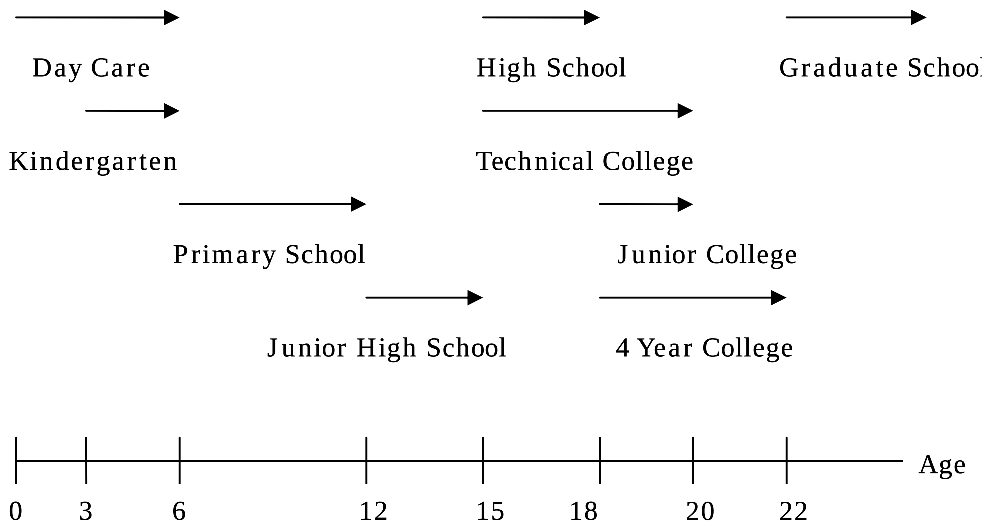
Evidence from the small birth cohort of 1966 due to the Fire-Horse year (*Hinoeuma*).

Sample: Born between 1960 April and 1970 March

	(1)	(2)	(3)	(4)
Sample	Male	Male	Female	Female
Dependent Variables	Years of Education	Log(Income)	Years of Education	Log(Income)
April	0.18	0.02	0.07	0.03
	(0.04)	(0.01)	(0.03)	(0.02)
April *1965 April – 1966 March	-0.02	-0.02	0.32	0.10
	(0.14)	(0.04)	(0.10)	(0.07)
April *1966 April – 1967 March	-0.22	-0.03	-0.04	0.01
	(0.14)	(0.04)	(0.10)	(0.07)
Observations	11386	10640	12064	7707
R-squared	0.00	0.02	0.00	0.01

Note: Due to the superstition based on the Chinese Zodiac, "Hinoeuma," the size of the birth cohort of 1966 is 25 percent smaller than adjacent cohort. Accordingly, there are smaller numbers of January-March born among the 1965 school cohort (1965 April – 1966 March) and larger numbers of January-March born among the 1966 school cohort (1966 April – 1967 March).

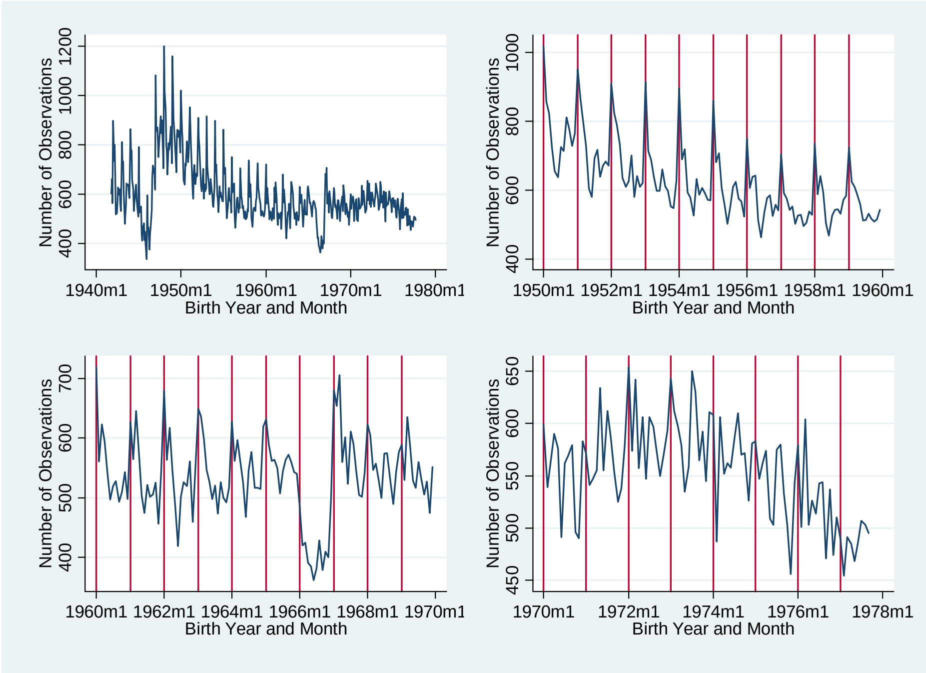
Figure 1: Japanese School System



Note: Primary school and junior high school are compulsory. Major tracking starts from age 15.

Figure 2: Number of Observations

Sample: Male 25-60 in 2002



Sample: Female 25-60 in 2002

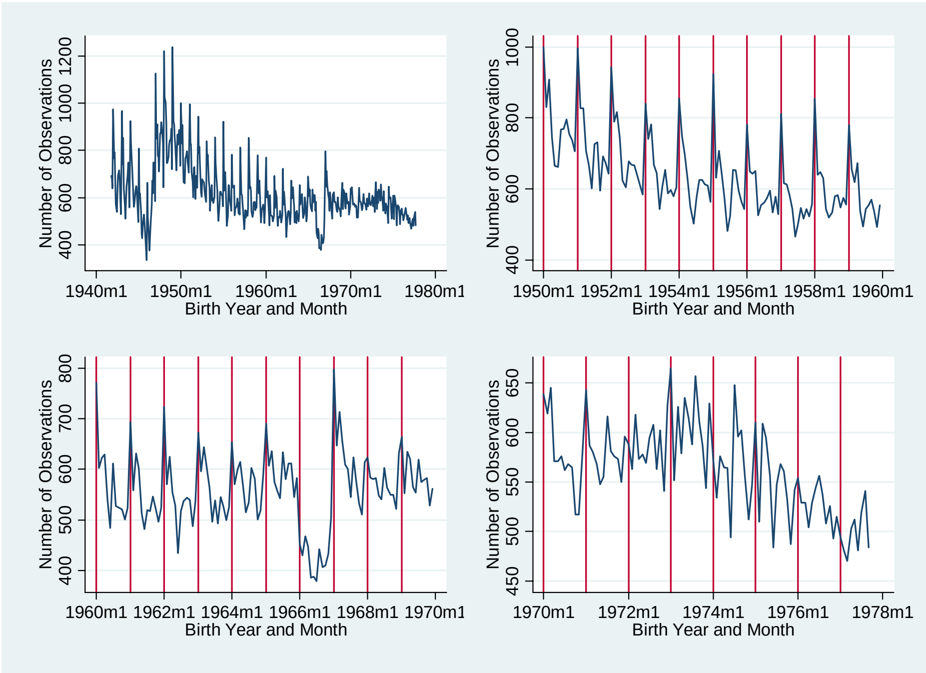
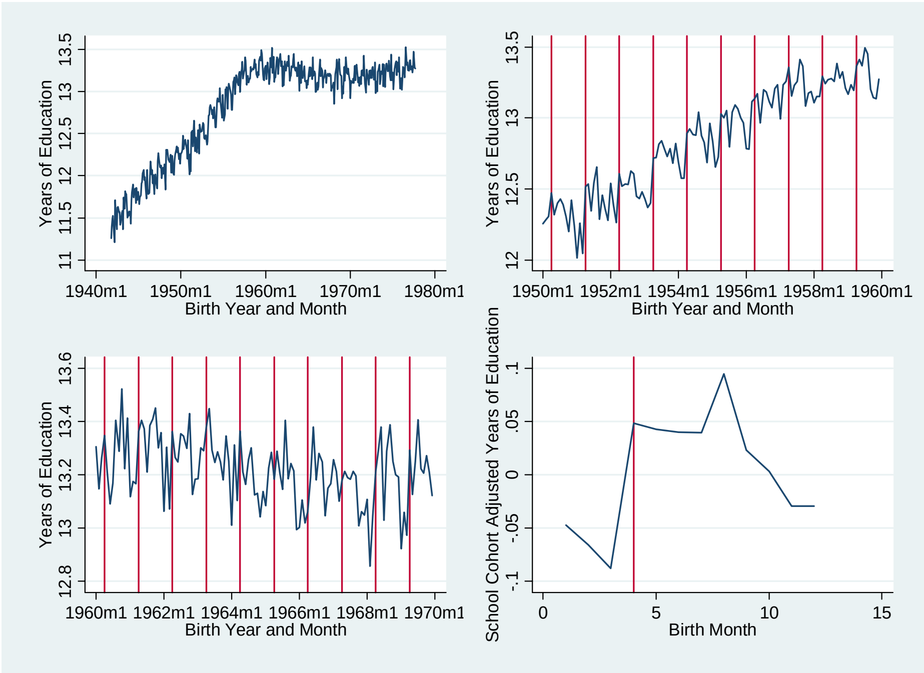


Figure 3: Years of Education

Sample: Male 25-60 in 2002



Sample: Female 25-60 in 2002

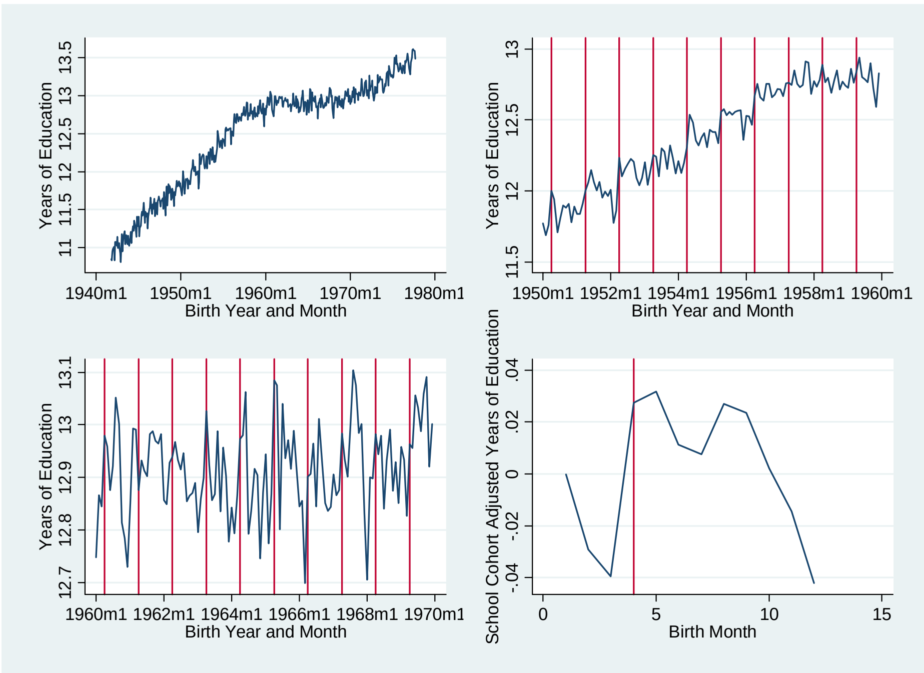
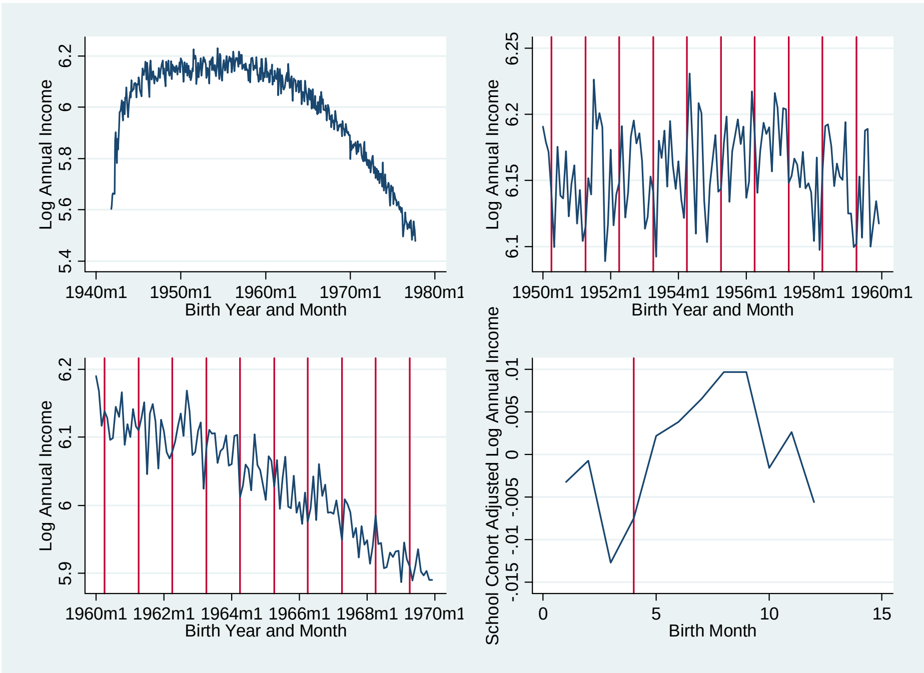


Figure 4: Log Earnings

Sample: Male 25-60 in 2002



Sample: Female 25-60 in 2002

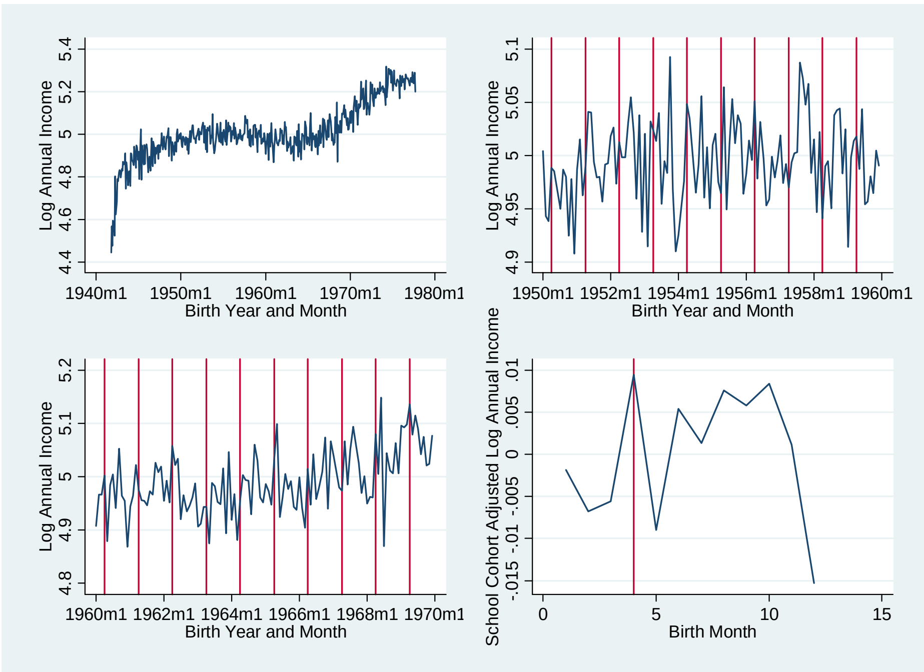
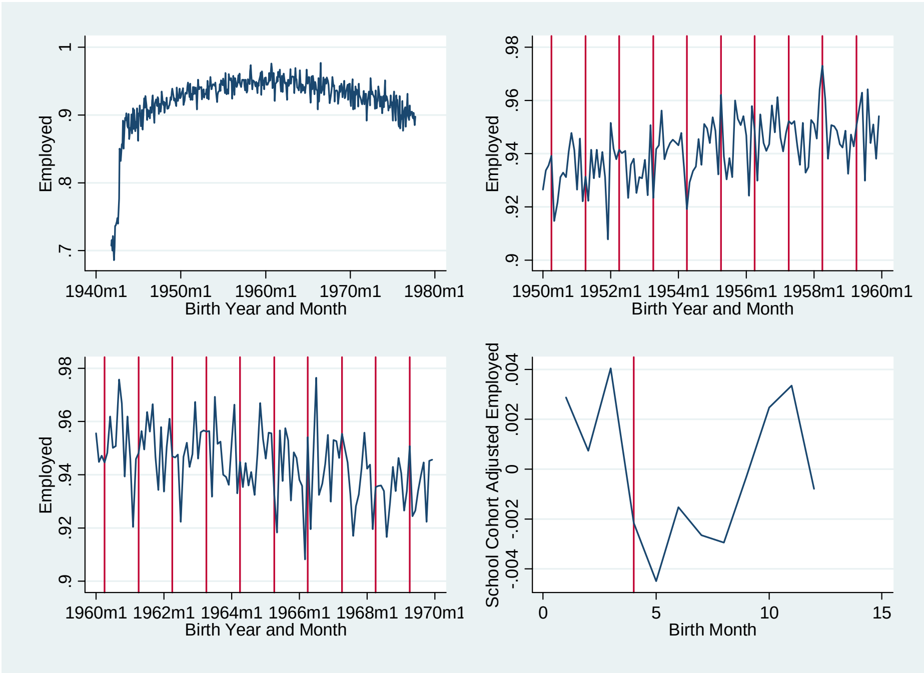


Figure 5: Employment Rate

Sample: Male 25-60 in 2002



Sample: Female 25-60 in 2002

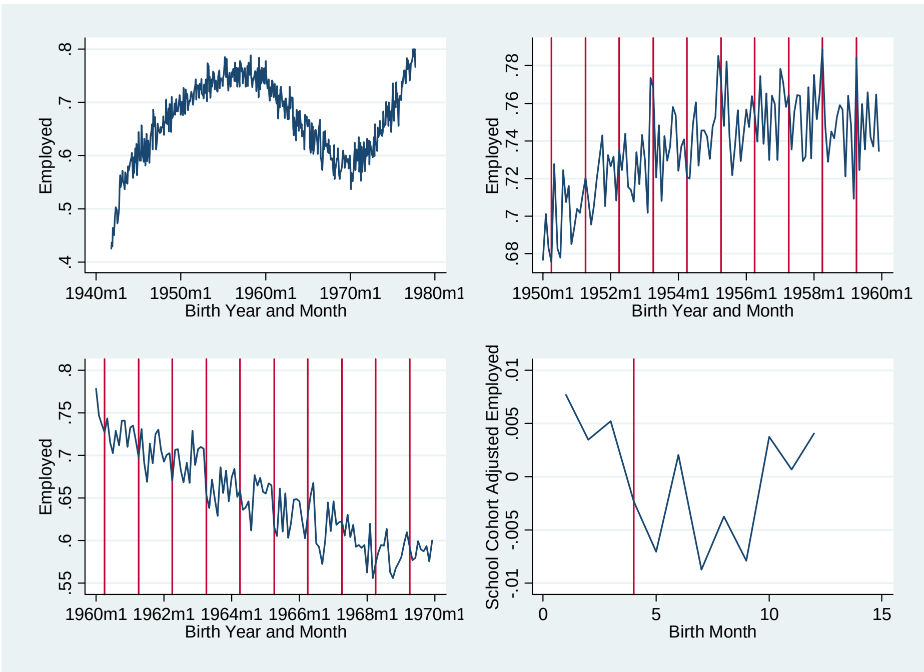
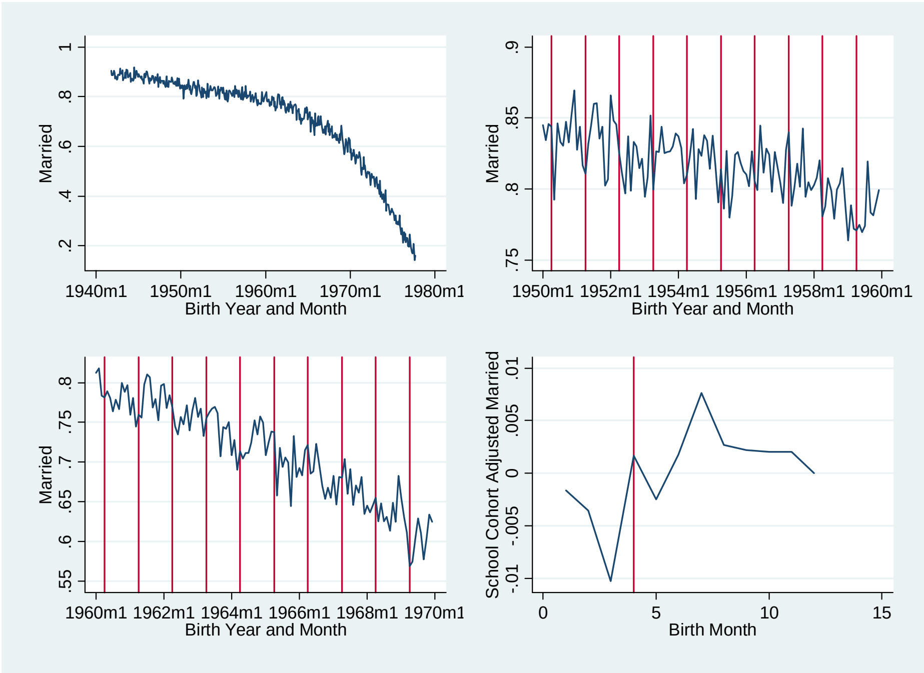


Figure 6: Marriage Rate

Sample: Male 25-60 in 2002



Sample: Female 25-60 in 2002

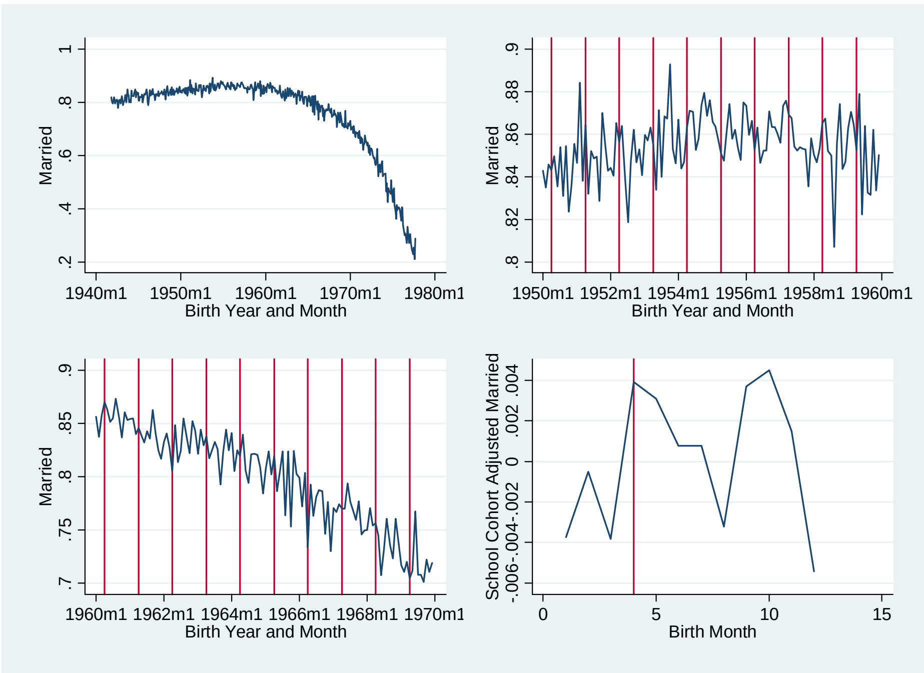
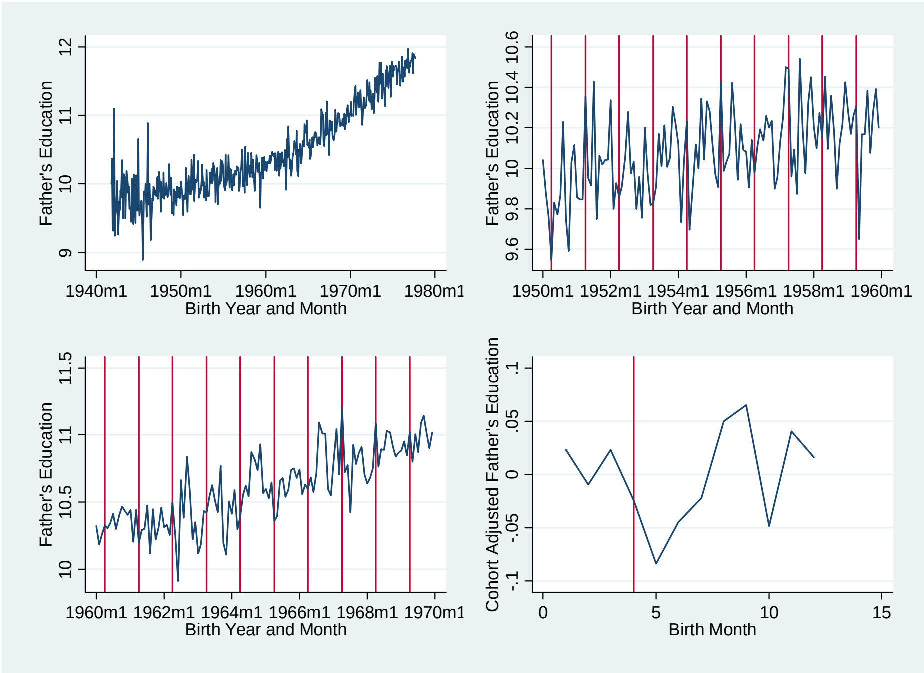


Figure 7: Father's years of education conditional on co residence with own father

Sample: Male 25-60 in 2002



Sample: Female 25-60 in 2002

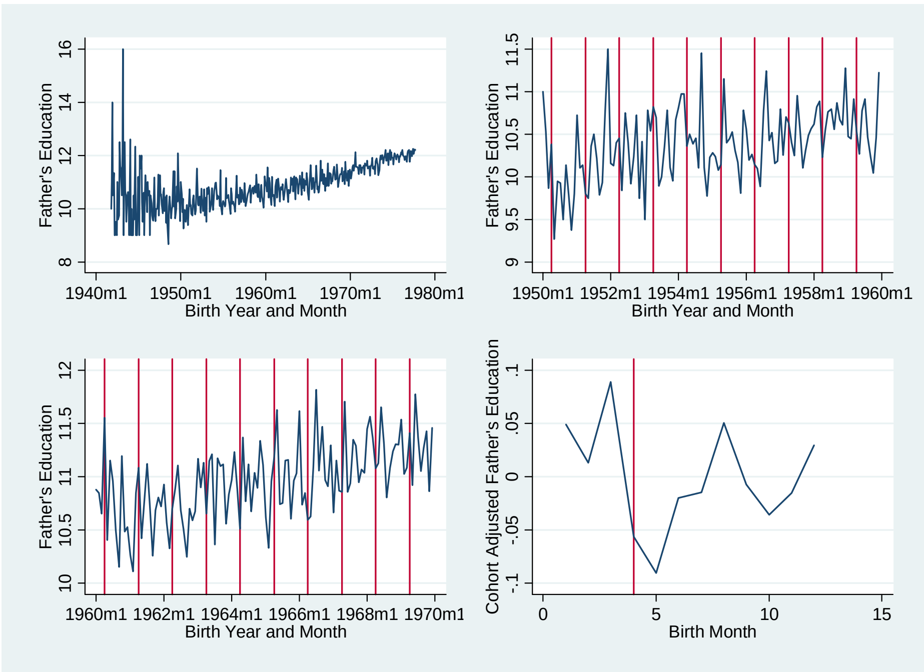
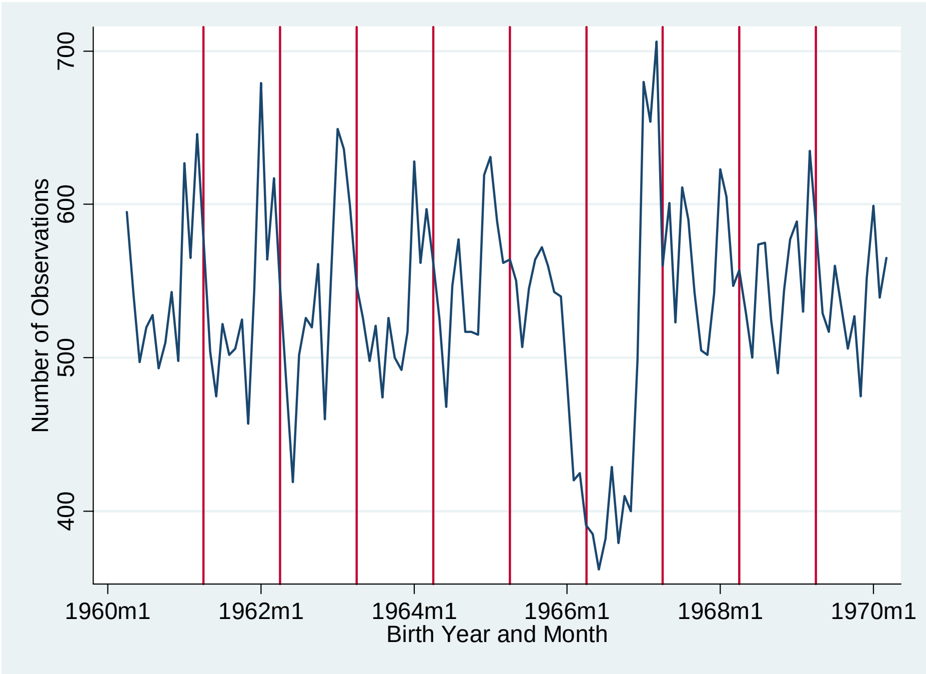


Figure 8: The 1966 fertility rate drop due to the Fire-Horse (*Hinoeuma*) year.

Sample: Male



Sample: Female