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The Effect of Shadow Education Vouchers after the Great East Japan Earthquake: Evidence from regression discontinuity design*

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Abstract

Shadow education vouchers, which beneficiaries flexibly use for any pre-registered institutions such as cram schools and one-to-one tutoring, might be an effective method to support disadvantaged children. In this paper, we estimate empirically the effects of shadow education vouchers provided in the area affected by the Great East Japan Earthquake on mainly cognitive skills by utilizing regression discontinuity design. Our results show a positive impact on academic achievements and study hours during holidays. In addition, the impact is much larger for children living in poverty.

Keywords: Shadow education voucher, Cognitive skill, Child poverty, Regression Discontinuity design, Randomization inference

JEL classification: I24, I26, I38, C21

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1 Introduction

The importance of shadow education, defined as private supplemental education, such as private tutoring and cram schools, has significantly increased in many countries. Japan is a pioneering country of shadow education. According to the statistics from the Japanese Ministry of Education, Culture, Sports, Science and Technology (hereafter, MEXT), the average annual household expenditure on one-to-one tutoring and cram schools has increased gradually in the past two decades, as Figure 1 shows. Particularly for public junior high school and public high school students, this trend is noticeable. The average expenditure for public junior high school students increased, from 181,000 JPY in 1994 to 220,000 JPY in 2016. In addition, during the same period, the average expenditure for public high school students increased, from 99,000 JPY to 117,000 JPY. These increased rates reached approximately 20%. In addition, households' economic circumstances have created a significant inequality in shadow education, and shadow education has come to play an important role in cultivating cognitive and non-cognitive abilities in children. Figure 2 shows the average expenditure on supplementary education, which consists of one-to-one tutoring, cram schools, and so forth, by household annual income. This figure indicates clearly that children living in poverty are inclined not to have adequate opportunities for shadow education. Concerns are growing that the unequal opportunities for shadow education might generate a cycle of poverty. Sano et al. (2016) analyzed the Japan Child Panel Survey data and revealed that household income has a significant impact on the expenditure on private supplemental education.

Therefore, revealing the role and causal effect of shadow education is essential when considering the cycle of poverty and the desirable countermeasures. Most of the previous studies in Japan show that shadow education has an insignificant or negative impact on child achievement. For example, Seiyama and Noguchi (1984) examined the effect of shadow education for junior high school students on the advancement rate to high school. They concluded that the utilization of cram schools and private one-to-one tutoring does not have a significant impact on advancement to high school. Other previous studies drew similar conclusions (e.g.,., Seiyama 1981, Baker et al. 2001, and Konakayama and Matsui 2008).

However, such previous studies showed the correlation between the use of shadow education and student outcomes, including academic abilities and the advancement rates, but they did not examine the causal effects to improve student outcomes by using causal inference methods, such as randomized controlled trials (hereafter, RCT) and quasi-experimental designs (hereafter, QED). Many recent studies based on rigorous causal inference showed significant positive impacts on student achievements. For example, Morris et al. (1990) focused on the Howard Street Tutoring Program, which provides quality, after-school reading instructions to second- and third-grade public school students who have fallen significantly behind their peers in reading. Each student in the Howard Street program receives reading instruction from a volunteer tutor two days a week for an hour each day. Each session ends with the tutor reading aloud to the child for 5–10 minutes.[3] Morris et al. (1990) conducted a randomized evaluation and concluded that the treatment group substantially improved its reading skills relative to the control group. Jacob and Lefgren (2004) used a regression discontinuity design (hereafter, RDD) to evaluate the causal effect of summer school in Chicago in 1996 and grade retention on student achievements. The RDD results show that summer school increased student achievements and grade retention had no negative consequences on students' academic achievements retained during the third grade. Banerjee et al. (2007) claimed that inputs specifically targeted at helping weaker students learn may be effective and evaluated two programs—a remedial education program and a computer-assisted learning program—in urban India using randomized experiments. At least in the short term, both programs had a substantial positive effect on children's academic achievements. Lavy and Schlosser (2005) evaluated the effects of a multiyear program implemented in 1999 in Israel that focused on underperforming high school students to increase the percentage of students with matriculation certificates for remedial education. The results suggested that the remedial

[3] https://www.childtrends.org/programs/howard-street-tutoring-program/

education program improved school matriculation rates by 3–4%.

Grossman and Tierney (1998) measured the effect of the Big Brothers Big Sisters of America program, which seeks to change the lives of children facing adversity for the better, forever. They help children achieve success in school, avoid risky behavior such as getting into fights and trying drugs and alcohol, and improve their self-confidence. The program is designed for children aged between 6 and 18 years and includes a one-to-one mentoring system that enables mentors to monitor children carefully.[4] Using RCT, Grossman and Tierney (1998) measured the causal effect of the program. They divided a group of 959 people into a control group (472 children) and a treatment group (487 children) and observed them for 18 months. They concluded that significant effects existed on drug use (46% less), alcohol use (27% less), and violence (32% less) for the treatment group. Another study conducted by Herrera et al. (2007) also used RCT to examine the impact of the program. They referred to either a 12-month or 24-month individual tutoring intent-to-treat analysis. Consequently, the treatment group had a better evaluation in terms of their grades and homework completeness. In contrast, no significant difference existed between the control group and the treatment group regarding substance and alcohol use, although a significant difference existed in the previous paper.

As described previously, although several studies examined the effects of a shadow education, some issues remain. First, few studies examined the impacts in Japan. As was mentioned previously, shadow education plays an important role in Japan in cultivating children's abilities. Examining its effectiveness not only contributes to related studies but also has policy implications. Second, because more effective shadow education programs may exist, verifying the effectiveness of other shadow education programs that have not been examined is extremely important.

The objective of this paper is to use an RDD to evaluate the effect of a unique shadow education voucher program conducted by a non-profit organization in Japan on several educational outcomes, such as academic achievement, use of cram schools, and study hours.

[4] http://www.bbbs.org/

In addition we focus on the role of non-cognitive ability and economic circumstances. Given that Cunha and Heckman (2008) pointed out that non-cognitive skills promote the formation of cognitive skills, but that cognitive skills do not promote the formation of non-cognitive skills, we examine whether our empirical results reach similar conclusions.

Our empirical results indicate that a shadow education voucher program has a positive impact on academic achievements. In addition, the impact is much stronger for children living in poverty.

This article is organized as follows. Section 2 describes the scheme of the shadow education voucher program that we evaluate. Section 3 describes our empirical strategy and introduces the preliminarily data. Section 4 presents the estimation results and a discussion. Section 5 concludes and proposes subjects for future study.

2 Background: Shadow Education Voucher Program

We evaluate the effect of a unique shadow education voucher program provided in the area affected by the Great East Japan Earthquake that hit on March 11, 2011. The shadow education vouchers introduced by the Chance for Children (hereafter CFC), an independent non-profit organization established after the earthquake. The CFC provides disadvantaged children in devastated areas with quantities[5] of shadow education vouchers to supplement their formal education. The beneficiaries can use the vouchers at every pre-registered private institution that provides shadow education services such as cram schools and one-to-one tutoring. As at the end of March 2015, the number of pre-registered institutions was 116.

In addition to shadow education vouchers, the CFC delivers mentoring support by university student volunteers called "Brother–Sister." Because the beneficiaries of the vouchers were affected by the earthquake, some lost their family members, household income, and motivation for learning. Some children were filled with a sense of deprivation.

[5] These quantities included 150,000 JPY for elementary school students, 200,000 JPY for first- and second-year students in junior high school and high school, and 300,000 JPY for third-year students in junior high school and high school, respectively.

Brothers–Sisters were assigned to the beneficiaries and, once a month primarily by phone, play a role in counseling the children and assisting them with their concerns.

Applicants who hope to receive vouchers must meet the income requirement and the requirements for disaster victims of the Great East Japan Earthquake. Regarding the income requirement, the applicants must satisfy at least one of following two conditions: (i) receive public assistance or (ii) household annual income is less than or equal to the income standard as Table 1 shows. Regarding the requirement for a disaster victim of the earthquake, the applicants must satisfy at least one of following three conditions: (i) own residence was fully or partly destroyed by the earthquake; (ii) had at least one family member die or is still missing because of the earthquake; (iii) at least one family member or the applicant was injured or impaired because of the earthquake, or (iv) at least one family member became unemployed because of the earthquake.

The eligibility index was used to select applicants, which has a maximum value of 300 and consists of the severity of the applicant's household economic conditions (200), how soon the applicant has to take the entrance examination (to high school or university) (60), and the applicant's motivation for learning as measured by a questionnaire (40). The higher the eligibility index value, the more likely that the applicant will receive the voucher. The CFC selected 263 beneficiaries, out of which 127 continued to receive benefits from FY 2013. Out of the 263, 136 were selected as having the highest eligibility index value from 1,807 total applications during FY 2014. Because elementary school student applicants did not answer the questionnaire, their motivation for learning was uniformly scored at 20. All the applicants from households receiving public assistance were selected as beneficiaries regardless of their eligibility index score.

3 Empirical Strategy and Data

3.1 Methodology

As mentioned previously, because the eligibility index is used to select voucher beneficiaries, we exploit an RDD to estimate the causal effect of shadow education vouchers on several educational outcomes. Because the eligibility index was not used to select 127 out of the 263 beneficiaries who continued to receive beneficiaries from FY 2013, we exclude these beneficiaries from our analysis. In addition, applicants receiving public assistance are excluded from the analysis because they were selected as beneficiaries regardless of their eligibility index score. Elementary school student applicants were also excluded because they did not answer the questionnaire that the CFC used to survey the outcome variables, as noted previously. In addition, third graders in high school were excluded from our analysis because we cannot capture the outcome variables of third-grade non-beneficiaries in high schools. As subsequently described, ex-post outcome variables of non-beneficiaries are collected through a survey of reapplicants during the following year. However, because applicants must be current students, last year's third-grade high school students are not included.

Consequently, the number of treated children is 70. To estimate the treatment effect of the voucher program, we must obtain the ex-post outcome variables, such as academic achievements and study hours. Although we can obtain the ex-post outcome variables for beneficiaries through the survey conducted by the CFC at the end of the fiscal year (March 2015), we cannot for non-beneficiaries. However, because approximately one-third of failed applicants reapplied for the voucher program during the following year, we can use the FY 2015 application to obtain their ex-post outcome variables through a questionnaire. Therefore, we use the reapplicants as the control group for our analysis.

Figure 3 shows the distribution of the eligibility index by beneficiaries and non-beneficiaries. The eligibility index is called a running variable in the context of an RDD. The threshold to accept or reject was 262. The applicants whose eligibility index score was greater than or equal to 262 were beneficiaries, and the applicants with a score less than 262 were non-beneficiaries, making 262 the cutoff in the context of an RDD. This variation is exogenous and cannot be manipulated by the applicants around the cutoff. Fortunately, no other clear institutional thresholds exist that could generate confounding discontinuities at 262.

Because our sample is somewhat small, we exploit the local randomization approach proposed by Cattaneo et al. (2017a, 2017b).[6] In this framework, we postulate that treatment assignments are randomized near the cutoff. In other words, the observations closest to the cutoff are viewed as being part of a local randomized experiment. For a small sample, although estimation and inference on the basis of large-sample approximations might be invalid, a local randomization approach that employs a randomization inference is robust. The estimation steps proposed by Cattaneo et al. (2017a, 2017b) are as follows.[7]

1. Set an initial small window near the cutoff.
2. For each of the ex-ante covariates, conduct a test of the null hypothesis of no effect of treatment on the covariates. We employ the Fisherian inference approach, which is valid in any finite sample and use the so-called sharp null hypothesis to conduct statistical tests. The minimum p-values are taken from the k tests.
3. If the minimum p-value obtained in step 2 is larger than some prespecified level (0.15 in this paper), a larger window is chosen, and step 2 is revisited to calculate the minimum p-value. The process is repeated until the minimum p-value is less than 0.15. The selected window is the largest window such that the minimum p-value is equal to or is larger than 0.15.
4. Because a local randomization approach is only plausible in a very small window around the cutoff, we employ the Fisherian inference approach to estimate the treatment effects.

3.2 Data, Descriptive Statistics, and Preliminary Analyses

3.2.1 Data

We utilize surveys conducted by the CFC to estimate the effects of a shadow education on various outcomes. As mentioned previously, the CFC carried out a survey for applicants

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[6] As Cattaneo et al. (2017a, 2017b) pointed out, a global polynomial approach is widely recognized as not delivering desirable point estimates near the boundary.

[7] We utilize the Stata package "rdlocrand" produced by Calonico et al. (2016) to estimate the treatment effects.

at the subscription offering. We use this survey as the baseline. In addition, the CFC conducted a similar survey for beneficiaries (treatment group) at the end of the fiscal year. We utilize this survey as the endline. In contrast, the control group was not surveyed at the endline. However, one-third of the control group reapplied for the voucher in FY 2015 and answered the questionnaire. We use this questionnaire as the endline data of the control group.

We analyze academic achievements, use of cram schools, and study hours as outcome variables.

3.2.2 Academic Achievement

In the surveys, the CFC asked about the self-rated relative academic achievements of the children using a scale of zero to six for the average of all subjects, including mathematics, Japanese, and English. We use these scales as the measurements of academic achievements.

Table 2 to Table 5 show the descriptive statistics of academic achievement for the baseline, endline, and differences by subjects and eligibility index. The means of the differences clearly show discontinuities below and above the cutoff of 262 in this case. For example, for the average of all the subjects, although the mean of the differences is −0.118 just below the cutoff, it is 0.478 just above the cutoff. These results imply that shadow education vouchers have a positive impact on academic achievements.

3.2.3 Use of Cram Schools and Study Hours

Table 6 shows the descriptive statistics of the dummy variable for use of cram schools. The ratios of utilizing cram schools are 54.3% at baseline and 53.9% at endline. The means of the differences somewhat show discontinuity around the cutoff.

Table 7 and Table 8 indicate the descriptive statistics of the study hours on weekdays and holidays, respectively. Surveys conducted by the CFC asked children about study hours and gave them the following choices: (i) not at all, (ii) less than 30 minutes, (iii) 30 minutes or more and less than one hour, (iv) one hour or more and less than two hours, (v) two hours or more and less than three hours, (vi) three hours or more and less than four hours, or (vii) four hours or more. We use the medians of each choice to convert these choices into hours. For example, we convert choice (ii) into 0.25 hours (15 minutes).

The means of study hours on weekdays are 1.568 at baseline and 1.629 at endline, respectively. The means of the differences do not indicate discontinuity around the cutoff. However, the means of the differences in study hours on holidays obviously indicate discontinuity around the cutoff. Although the mean of the difference is 0.051 just below the cutoff, it is 0.533 just above the cutoff. This fact might indicate that recipients of shadow education vouchers increased their study hours on holidays by approximately 30 minutes relative to non-recipients' study hours.

4 Estimation Results

4.1 Window Selection

Before estimating the treatment effects, we must use the window-selection algorithm explained in section 3 to select the desired window. We utilize all the outcome variables at baseline to select the window. Table 9 shows the result of the window selection. The first column provides the window length of each window divided by two. The second column provides the minimum p-value of the balancing test. The third column contains the name of the corresponding variable associated with the p-value in the second column. The fourth and fifth columns show the number of observations to the left and right of the cutoff inside each window.

The largest window for which the second column is equal to or greater than 0.15 is 18.90. Therefore, our window becomes [243.1(=262 − 18.9), 280.9(=262 + 18.9)] and contains 51 observations.

4.2 Academic Achievement

In this section, we show the plots and the estimation results by exploiting an RDD.

Figure 4 to Figure 11 indicate the RD plots for academic achievement at baseline (pre-intervention) and changes in those from baseline to endline by subject. The differences between beneficiaries and non-beneficiaries are not observed at baseline. These facts indicate that the treatment and control groups are quite similar before implementing the voucher program, making these two groups comparable when estimating the causal effects of the program. However, the changes in academic achievement for the average of all the subjects and Japanese from baseline to endline seem quite large. These changes imply that shadow education vouchers might have positive effects on some academic achievements.

Table 10 indicates the RD estimates for changes in academic achievement from baseline to endline. These results confirm the graphical interpretation. Table 10 indicates that shadow education vouchers have a positive impact on academic achievements for the average of all subjects and Japanese. These estimates imply that shadow education vouchers increase academic achievements by an approximate 0.45 standard deviation equivalent (0.442 (=0.707/1.598) for the average of all the subjects and 0.461 (=0.653 / 1.417) for Japanese, respectively). These effect sizes are very close to those in Banerjee et al. (2007), who revealed that remedial education and computer-assisted learning programs in India increased test scores by 0.28 and 0.47 standard deviations, respectively.

Table 12 shows RD estimates for a change in average academic achievement of all subjects after dividing the sample into high and low motivation for learning. We construct an index of motivation for learning using the questionnaires shown in Table 11, which were proposed by Shimoyama et al. (1982, 1983). These questionnaires consisted of 40 items and were classified into eight types on the basis of motivation type, three of which (avoidance of failure, persistence, and values) are inverse indicators. Therefore, the more children meet these categories, the less their motivation for learning becomes. The RD estimates for highly motivated children is statistically insignificant but becomes large (0.504 (=0.806 / 1.598) in standard deviation equivalent). Moreover, it is somewhat larger for children with low motivation. This result might indicate that the effect of shadow education vouchers for highly motivated children is more efficient, and non-cognitive skills such as motivation play an important role in cultivating cognitive skills. This finding is consistent with Chunha and Heckman (2008) and Freyer et al. (2015).

Table 13 shows estimates after dividing the sample into relative poverty and non-relative poverty. Households with incomes less than 50% of the median equivalized income[8] are defined as relative poverty. Half of the median equivalized income is called the relative poverty line. According to the National Livelihood Survey conducted by the Japanese Ministry of Health, Labour, and Welfare, the relative poverty line in Japan was 122 million JPY in 2012 and 2015. We utilize this line to divide the sample. RD estimates indicate that the effect of shadow education vouchers for children in relative poverty is larger than for children in non-relative poverty. Although Becker (1975) theoretically predicted that households facing expensive borrowing costs cannot afford to invest adequately in human capital, our estimation results are consistent with this prediction.

4.3 Use of Cram Schools and Study Hours

Figure 12 to Figure 17 indicate the RD plots for use of cram schools and study hours at baseline (pre-intervention) and the change from baseline to endline. The plots in Figure 13 imply that the rate of utilization of cram schools increases by approximately 20% after providing shadow education vouchers. In addition, Figure 17 indicates that the study hours on holidays increase by approximately 30 minutes. However, the study hours on weekdays are almost stable from baseline to endline.

Table 14 shows the estimates of the changes in usage of cram schools and study hours from baseline to endline. Estimates of usage of cram schools and study hours on weekdays are statistically insignificant. For study hours on holidays, the RD estimate is statistically significant at the 10% level. These results might imply that shadow education vouchers increase the study hours during holidays.

4.4 Examining the Validity of Variable and Sample Selection

4.4.1 Validity of Self-Rated Relative Academic Achievements

In this paper, we utilize self-rated relative scores as proxy variables for academic achievements. We must assess the validity of self-rated scores to draw reliable conclusions

[8] Equivalized income is calculated as the ratio of household income to the square root of household size.

from our estimation results. The CFC conducted tests on recipients who were junior high school students to measure academic achievements in mathematics by using commercially available tests at baseline and endline. We examine the validity of the self-rated score to utilize those tests.

Table 15 presents the estimation results of regressing the deviation value of mathematics on the self-rated relative score. The first column of Table 15 is the result at baseline, and the second column is the result at endline. Both estimates are significantly positive and the R-squared coefficients are 0.469 at baseline and 0.540 at endline, respectively. Therefore, self-rated relative scores are viewed as appropriate measures of academic achievements.

4.4.2 Sample Selection of Control Group

As explained previously, we utilize the reapplicants who failed the 2014 application as our control group. If the reapplicants have systematically different characteristics than one-time applicants, these differences might bias our estimation results.

Figure 18 shows the distribution of the eligibility index by the one-time applicants and the reapplicants and indicates that systematical differences between two groups do not seem to be observed. Table 16 indicates the estimation results from using a probit model to regress a dummy variable of reapplicants on our outcome variables at baseline. The coefficients shown in Table 16 indicate marginal effects. The result implies that the eligibility index is positively related to reapplications. However, the magnitude is not large and, even if the eligibility index increases by 100 points, the probability of reapplication is increased by approximately 7.6% only.

Therefore, the utilization of the reapplicants as our control group does not seem to distort our estimation results.

4.4.3 Sample Selection of Treatment Group

As section 2 explains, our running variable is calculated by summing up three factors. Because of how soon they will take the entrance examination accounts for 60 of the running variable, which has a maximum value of 300 and our cutoff is 262, most of the beneficiaries are third-grade junior high school students.

Table 17, which shows the number of observations by grade and eligibility index, indicates that third-grade junior high school students account for a large portion of beneficiaries. We cannot deny the possibility that this distribution in our sample might distort the estimation results.

However, generally, third graders are inclined to work harder in their studies relative to students in other grades because they will take the entrance examination sooner. Consequently, for third-grade junior high school students, improving the self-rated "relative" scores we use as indexes of academic achievement must be more difficult to do than in other grades. Therefore, our results are more likely to be underestimates than overestimates.

5 Conclusion

This paper utilizes an RDD to estimate empirically the effects of unique shadow education vouchers on cognitive skills, use of cram schools, and study hours. The presented analysis allows us to describe three major findings. First, our empirical results indicated that shadow education vouchers have a statistically significant impact on academic achievement and study hours during holidays. Thus, the shadow education voucher program is an effective method because it is flexible and allows beneficiaries to use any pre-registered institution, such as cram schools and one-to-one tutoring. Our comprehensive estimation results show that shadow education vouchers have a positive impact on academic achievement by increasing the number of courses that beneficiaries take at cram schools or during one-to-one tutoring because the effect of vouchers on the use of cram schools is positive and statistically insignificant. The fact that the use of cram schools was approximately 50% even at baseline also supports this inference. Second, the effect on a child living in poverty is thought to be greater than the effect on a non-poor child, and support measures that focus on poor households could be highly cost-effective. This result might imply that investments in education for disadvantaged children have a high return because poor families cannot adequately afford to invest in education given their financial constraints. Finally, the impacts of shadow education vouchers for highly motivated children might be somewhat greater than for children with low motivation. This result implies that enhancing motivation before providing learning support is crucial to efficiently improving students' cognitive ability. This implication might be consistent with Freyer et al. (2015), who revealed that children with above-median non-cognitive scores before treatment accrued the greatest benefits from treatment. In addition, Chuha and Heckman (2008) revealed that non-cognitive skills promote the formation of cognitive skills but that cognitive skills do not promote the formation of non-cognitive skills. Chuha and Heckman (2008) might also have findings that are consistent with our results.

Our analyses have several limitations. The first limitation concerns the external validity. Our empirical results are for the unusual situation after the Great East Japan Earthquake, and the program we examined is for disadvantaged children. Especially in Japan, estimating the causal effects of policies and practices is rare; yet, further studies are required to examine whether the results in this paper can be generalized. Second, we need to reveal the underlying structure of the program provided by the CFC, which consists of not only shadow education vouchers but also mentoring support from volunteers of university students. Because we estimate only the effect of the entire program, each effect from financial support and mentoring support are yet to be revealed. In addition, future studies need to explore the efficiency of the program. Third, in this paper, we utilize self-rated scores as indicators of academic achievement. We cannot deny that the scores have some biases or noise, and our results need to be reexamined by utilizing more objective indicators. Finally, as previously mentioned, our control group consists of only reapplicants during the following year. In addition, the treatment group mainly consists of third-grade junior high school students. We cannot deny the possibility that these issues with our sample might distort the estimation results.

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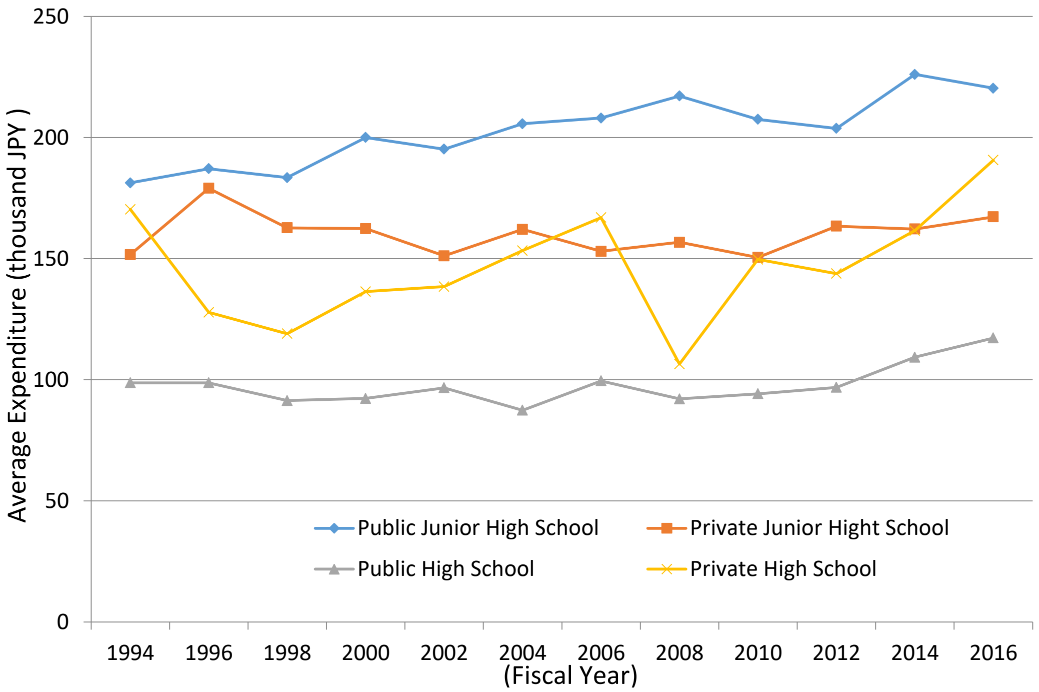
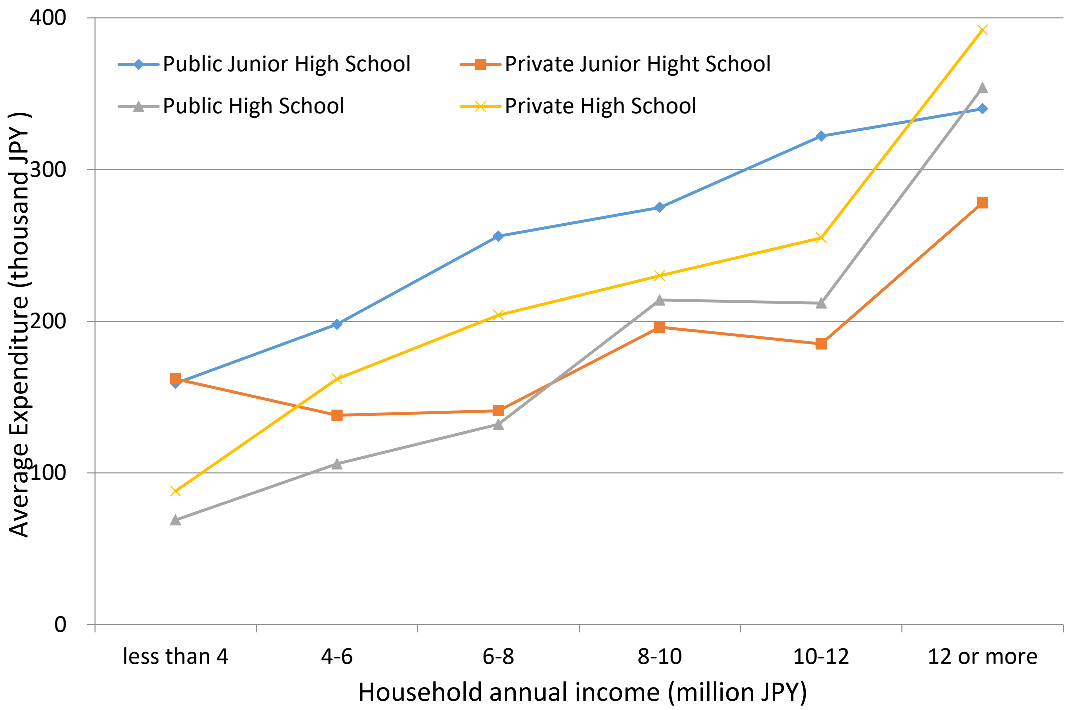
Figure 1 Changes in average expenditure on one-to-one tutoring and cramming school



(Source) Ministry of Education, Culture, Sports, Science and Technology

Figure 2 Average expenditure on supplementary education

by household annual income in 2016



(Source) Ministry of Education, Culture, Sports, Science and Technology

Figure 3 Distribution of the Eligibility Index by Beneficiaries and Non-beneficiaries

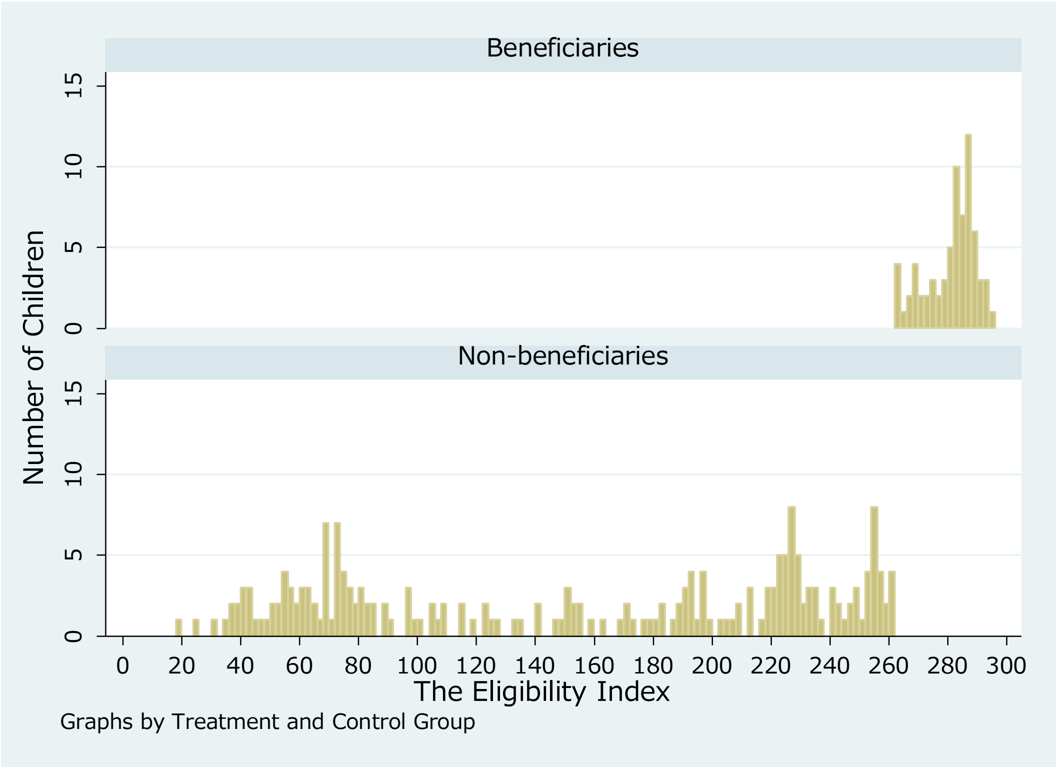


Figure 4 RD Plot of Academic Achievement at Baseline: Average of All Subjects

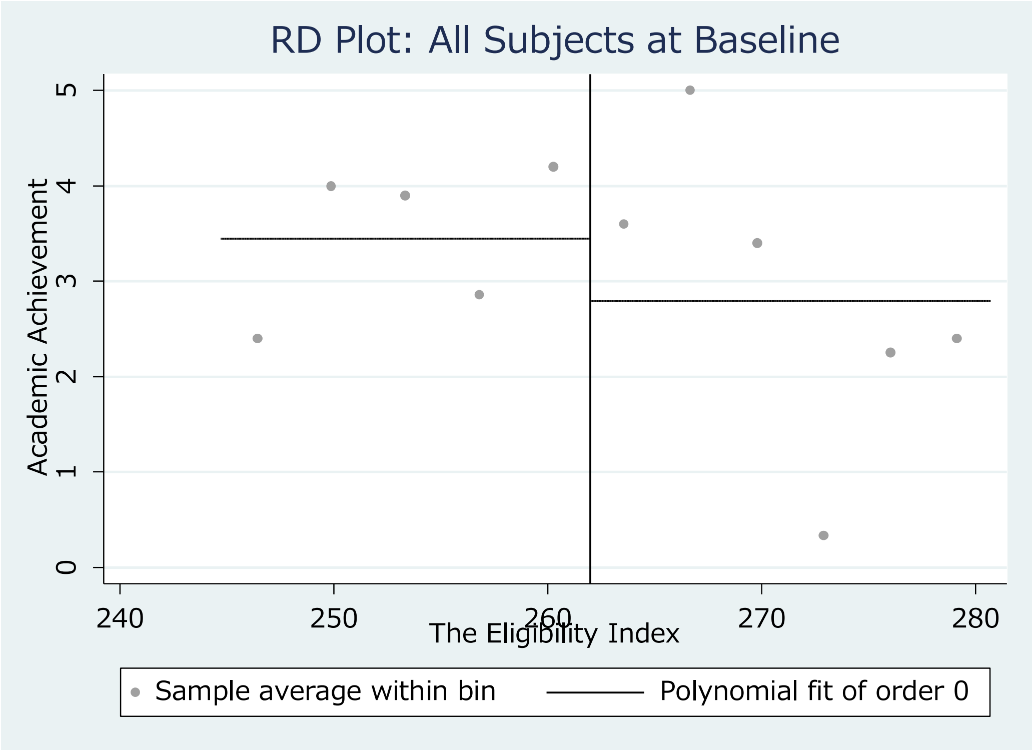


Figure 5 RD Plot of Difference of Academic Achievement: Average of All Subjects

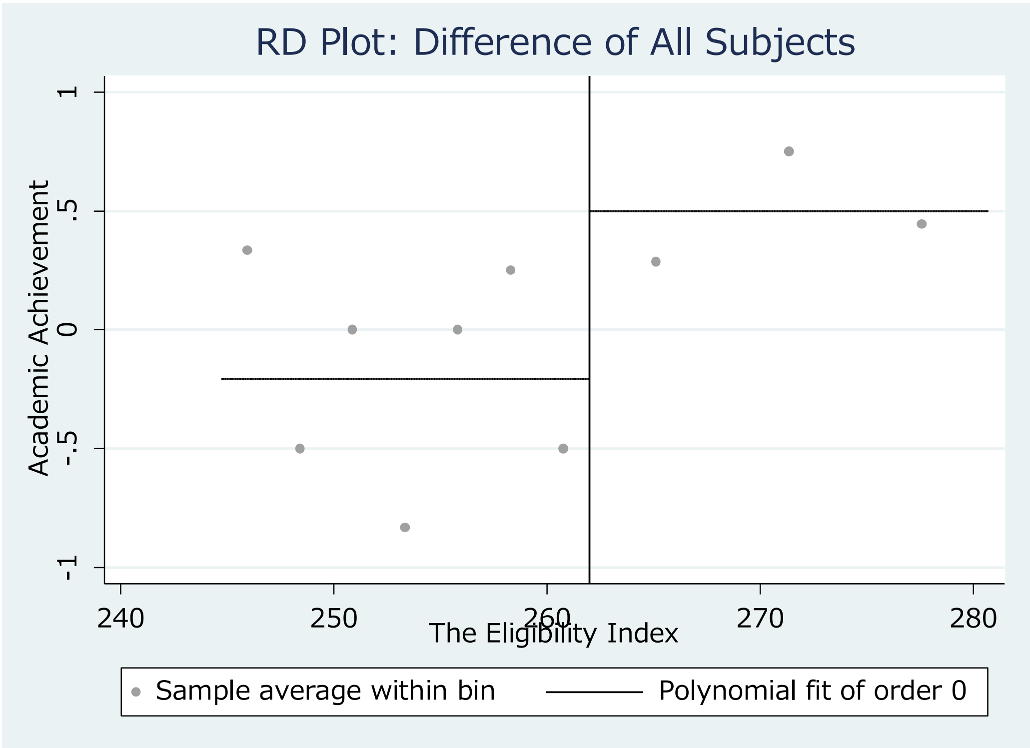


Figure 6 RD Plot of Academic Achievement at Baseline: Mathematics

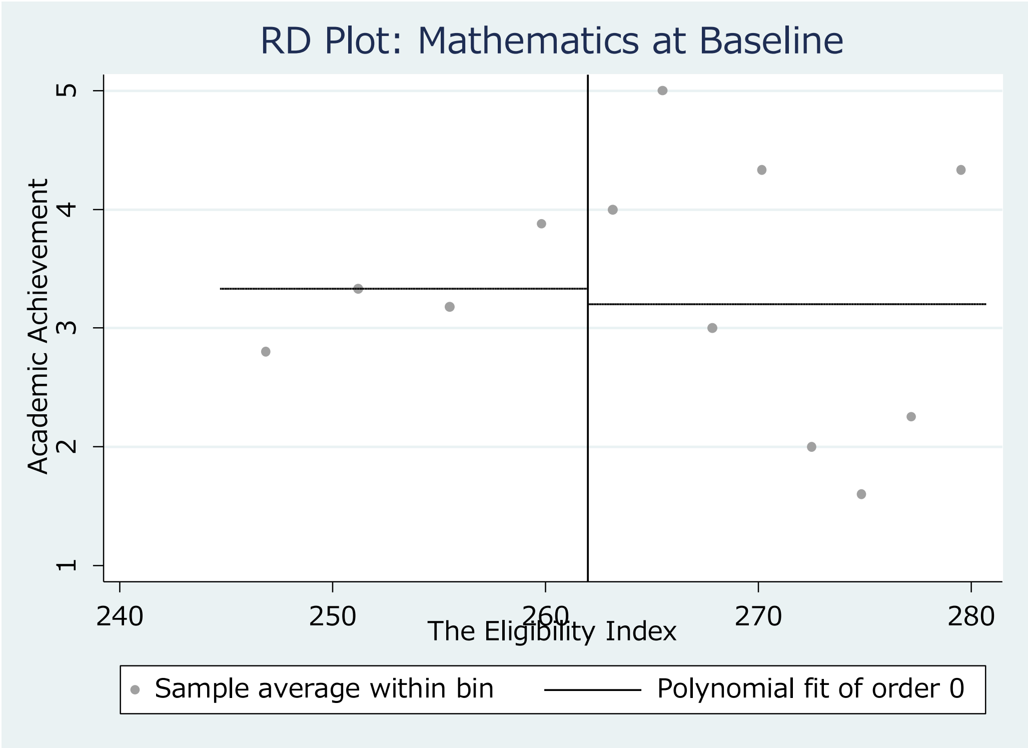


Figure 7 RD Plot of Change in Academic Achievement: Mathematics

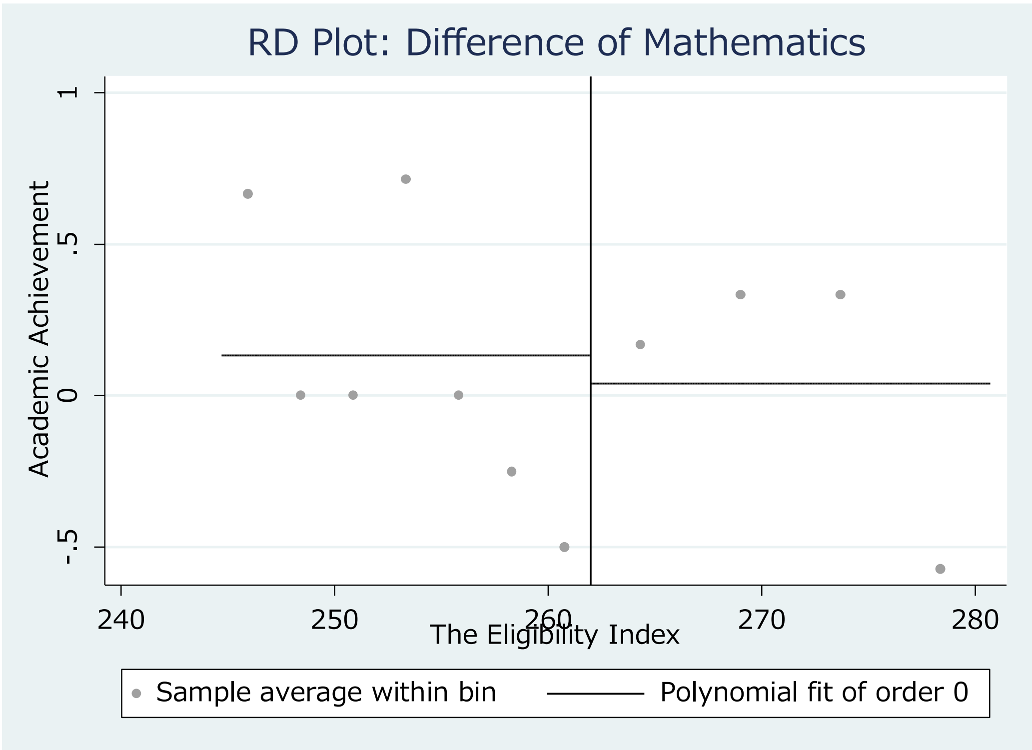


Figure 8 RD Plot of Academic Achievement at Baseline: Japanese

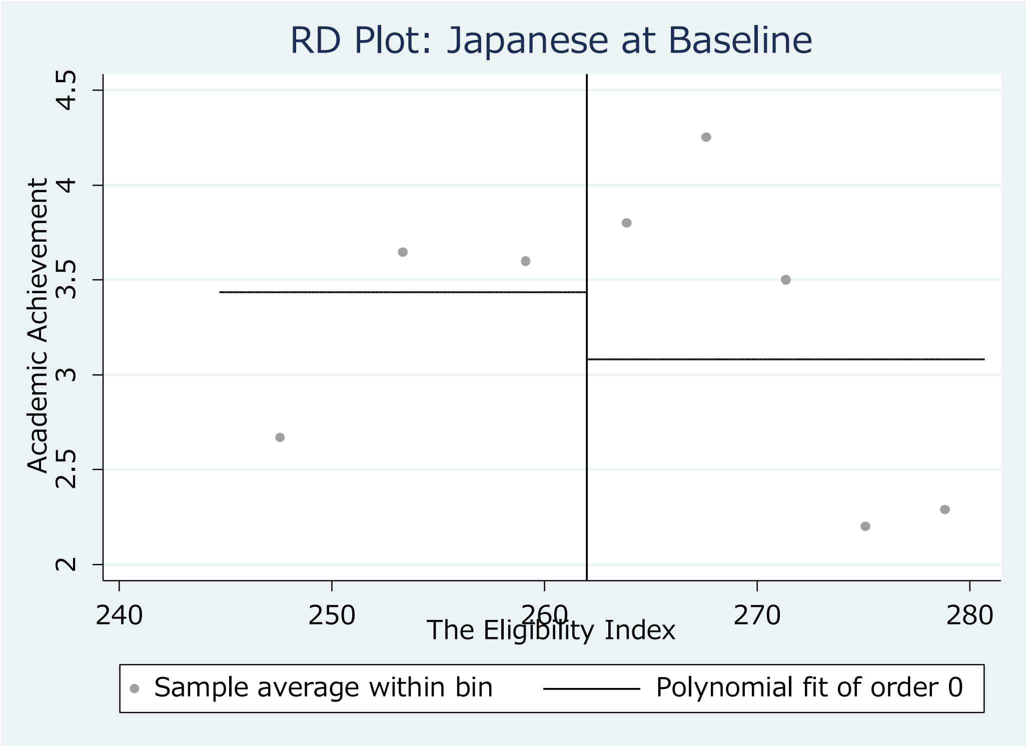


Figure 9 RD Plot of Change in Academic Achievement: Japanese

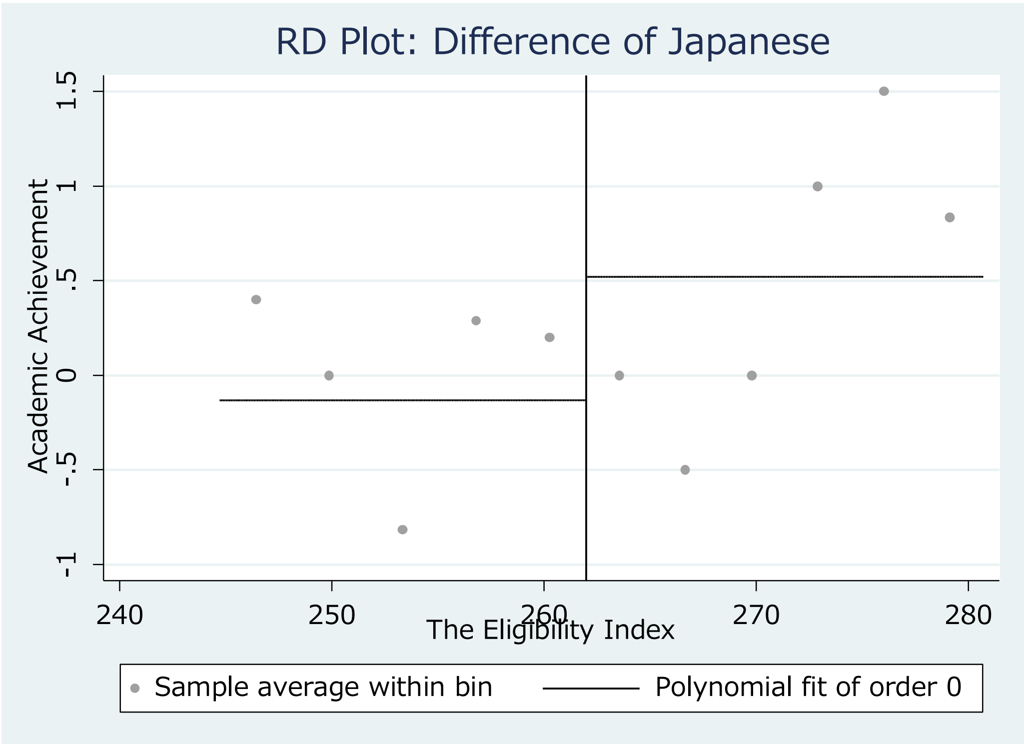


Figure 10 RD Plot of Academic Achievement at Baseline: English

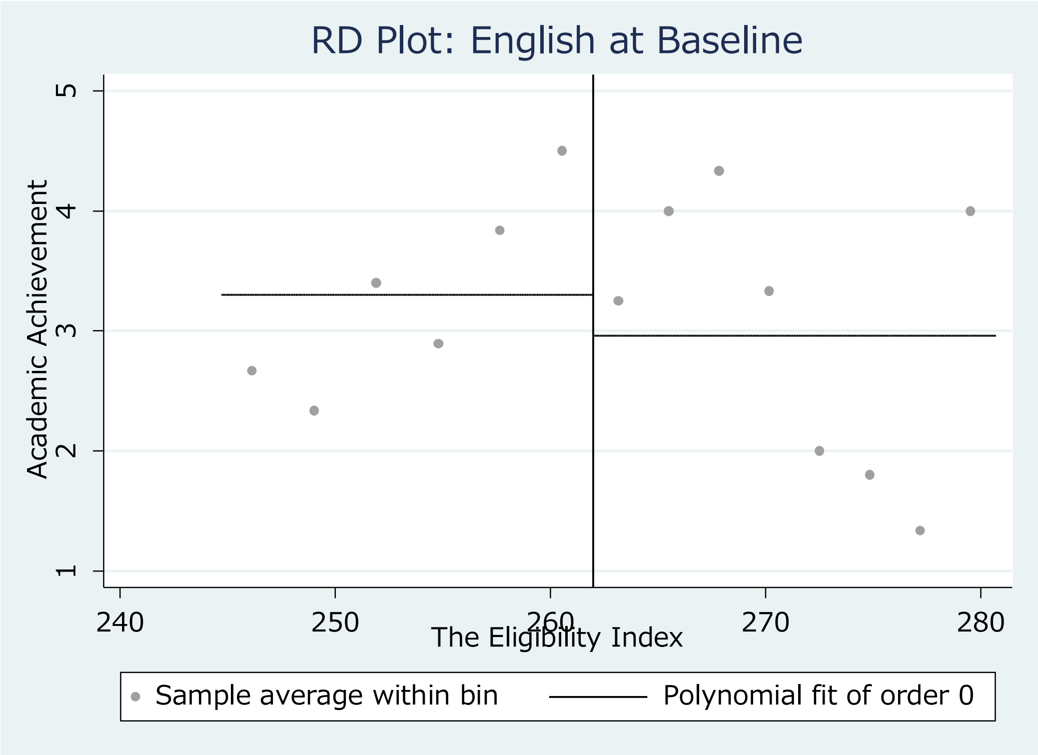


Figure 11 RD Plot of Change in Academic Achievement: English

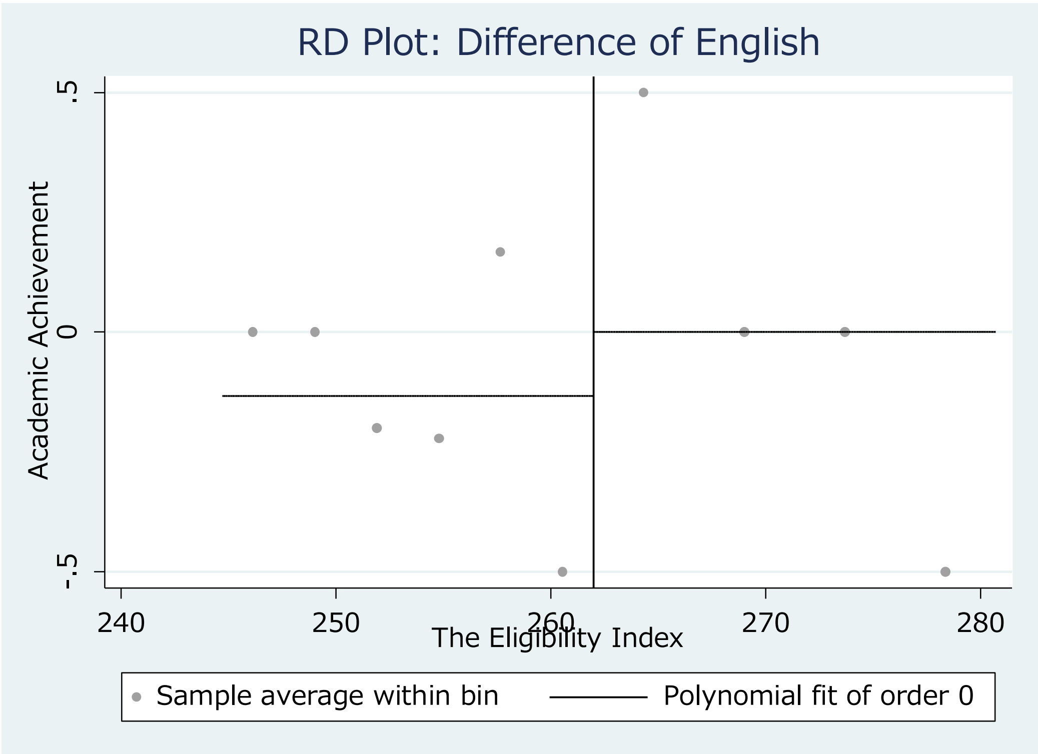


Figure 12 Plot of Use of Cram Schools at Baseline:

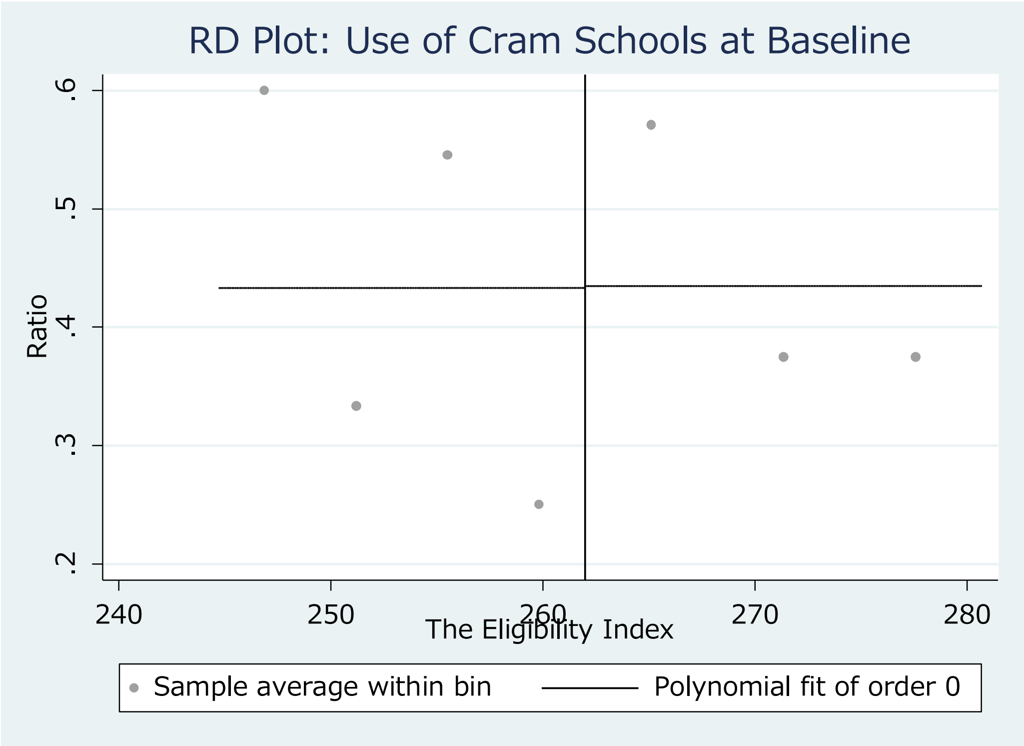


Figure 13 RD Plot of Change in Use of Cram Schools

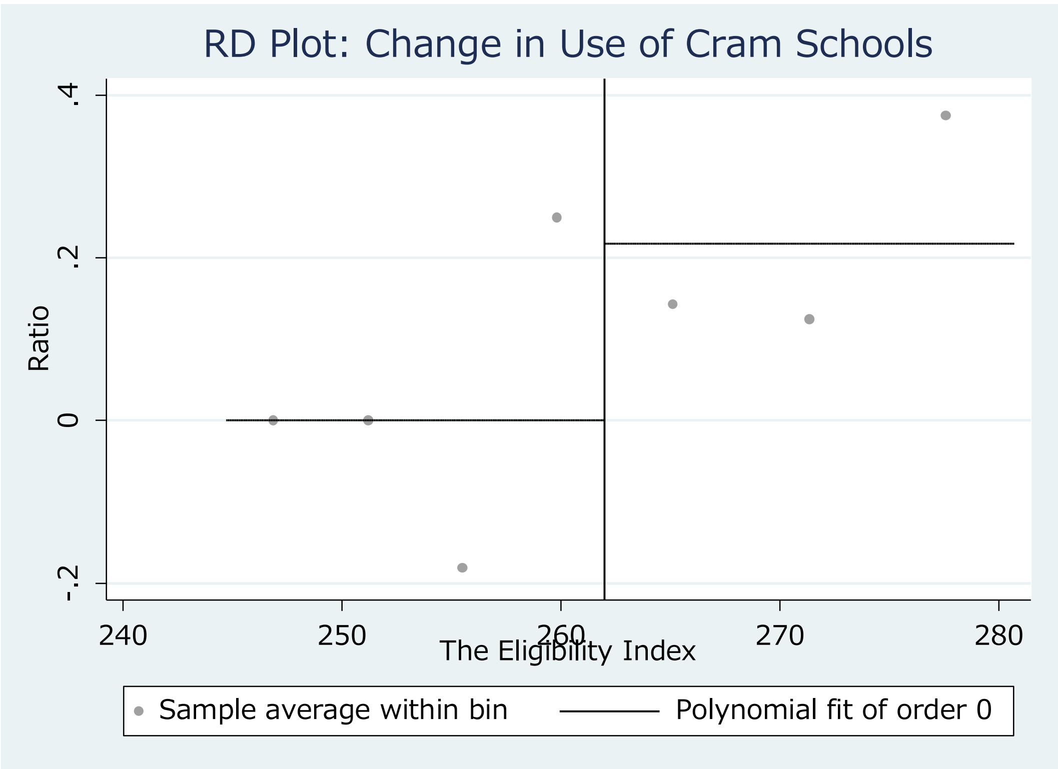


Figure 14 RD Plot of Study Hours on Weekdays at Baseline

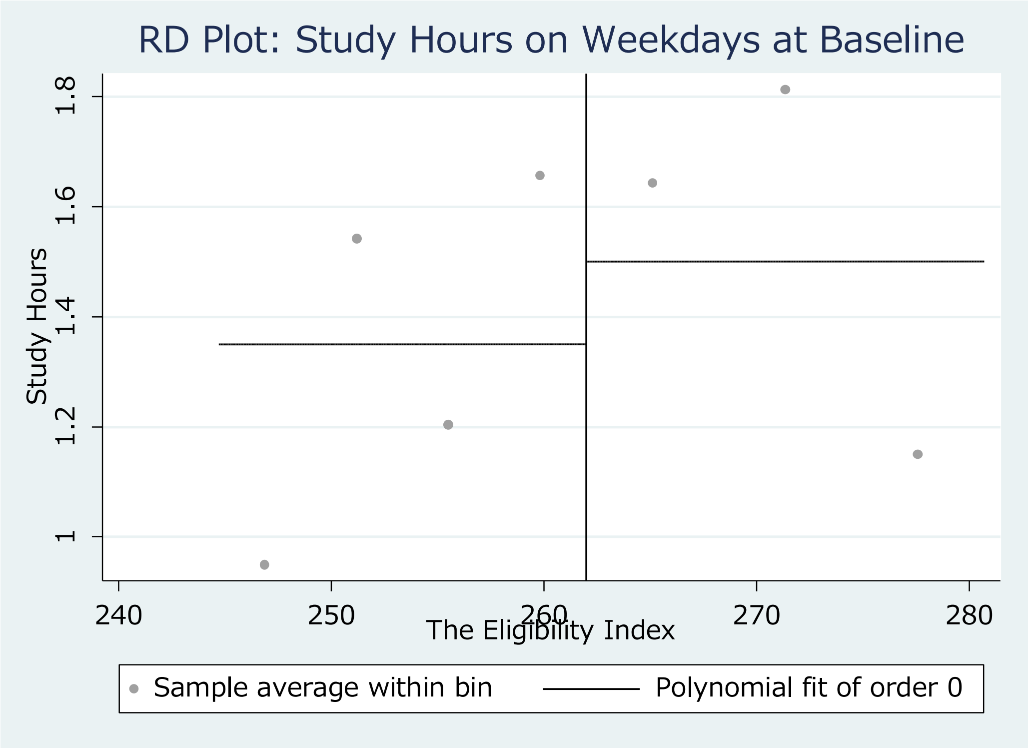


Figure 15 Plot of Change in Study Hours on Weekdays

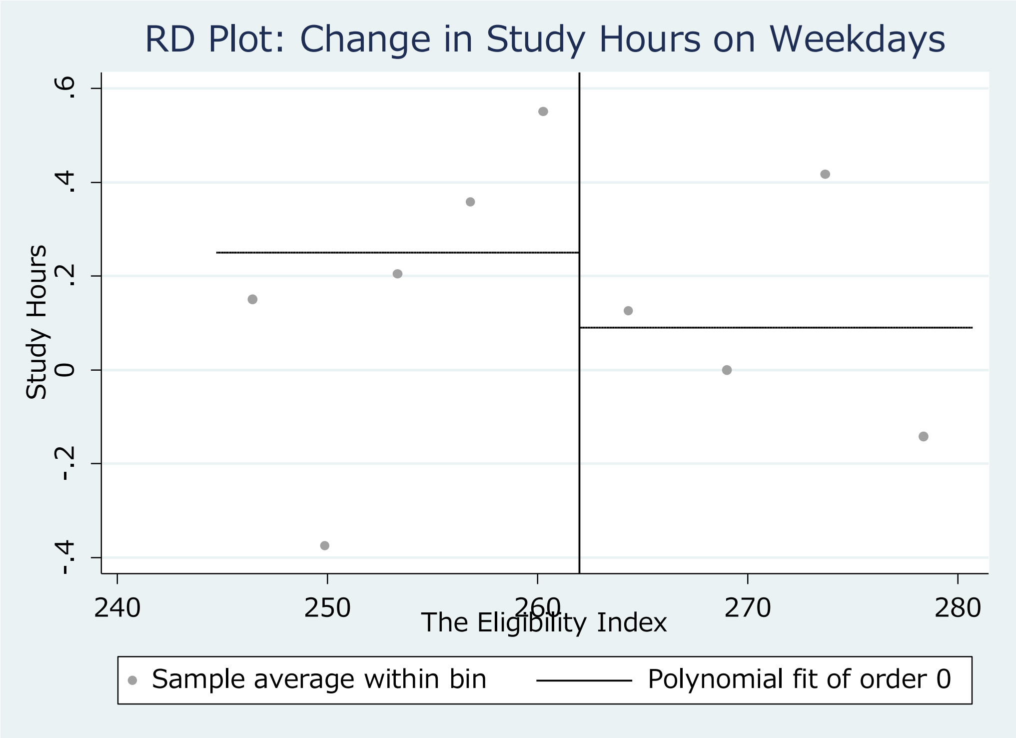


Figure 16 RD Plot of Study Hours on Holidays at Baseline

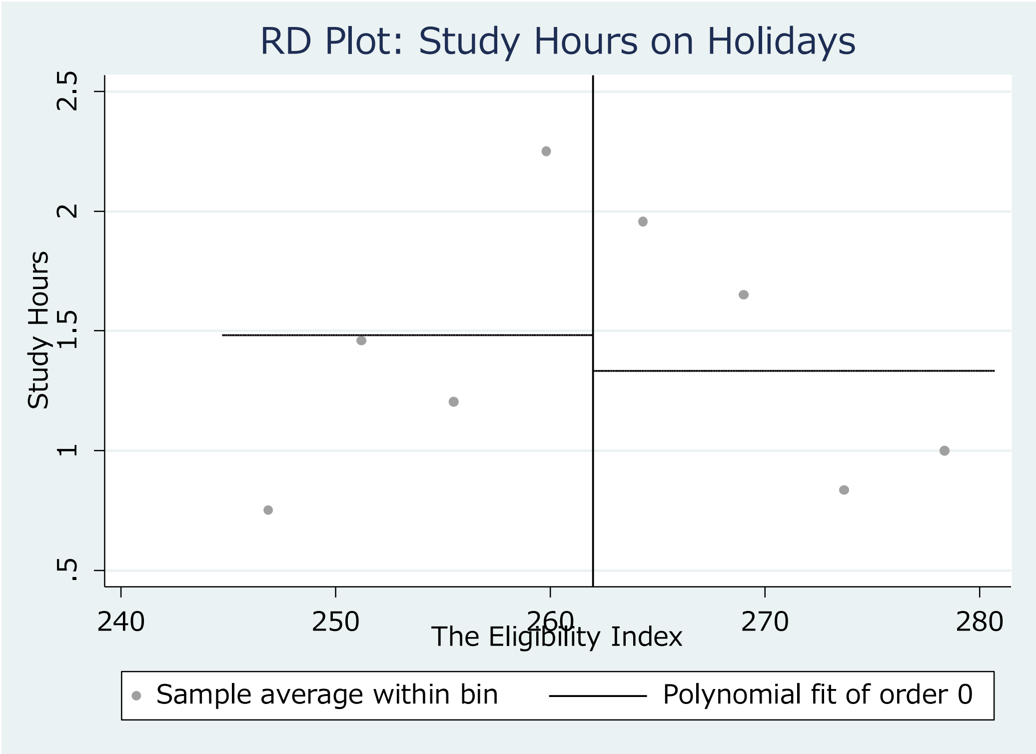


Figure 17 RD Plot of Change in Study Hours on Holidays

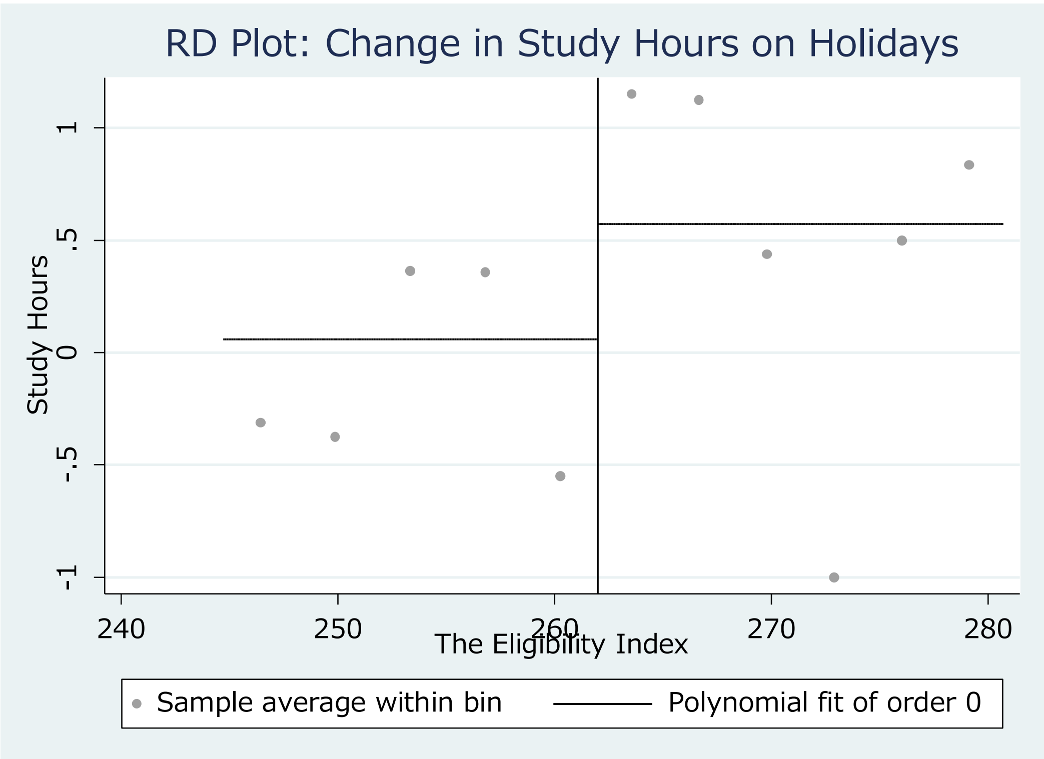


Figure 18 Distribution of the Eligibility Index by One-time Applicants and Re-applicants

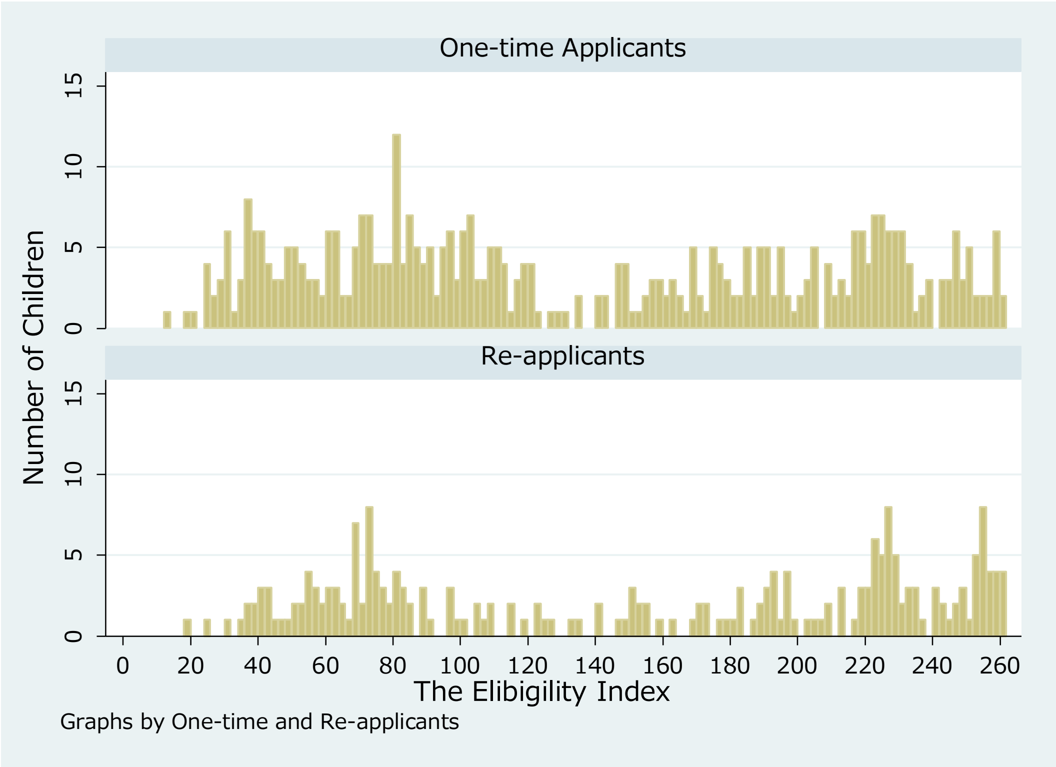


Table 1 Annual Income Standard for Application (JPY)

Size of household	Employment income	Self-employment income
2	4,593,400	2,907,000
3	5,681,400	3,668,600
4	6,630,000	4,386,000
5	7,378,000	4,984,400
6	8,551,000	5,920,080
7	9,273,500	6,496,720
8	9,996,000	7,078,800
9	10,718,500	7,665,440
10	11,441,000	8,256,900

Table 2 Descriptive Statistics of Academic Achievement: Average of All Subjects

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	112	3.250	1.630	3.348	1.609	0.098	1.445
- 200 or less	18	3.222	1.555	3.333	1.414	0.111	1.023
- 220 or less	12	3.083	1.165	2.833	1.850	-0.250	1.357
- 240 or less	35	3.571	1.481	3.457	1.597	-0.114	1.491
- less than 262	34	3.471	1.331	3.353	1.390	-0.118	1.066
- 280 or less	23	2.696	1.869	3.174	1.696	0.478	1.163
- 300 or less	47	3.383	1.751	3.894	1.448	0.511	1.081
Total	281	3.285	1.598	3.416	1.566	0.132	1.312

Table 3 Descriptive Statistics of Academic Achievement: Mathematics

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	112	3.411	1.690	3.250	1.727	-0.161	1.424
- 200 or less	18	3.056	1.589	3.278	1.447	0.222	1.665
- 220 or less	12	2.833	1.528	2.750	2.006	-0.083	1.929
- 240 or less	36	3.861	1.676	3.306	1.802	-0.556	1.629
- less than 262	35	3.343	1.235	3.571	1.420	0.229	1.087
- 280 or less	24	3.083	1.998	3.125	1.752	0.042	1.574
- 300 or less	47	3.745	2.005	4.021	1.567	0.277	1.347
Total	284	3.440	1.717	3.394	1.687	-0.046	1.464

Table 4 Descriptive Statistics of Academic Achievement: Japanese

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	111	3.279	1.422	3.279	1.428	0.000	1.265
- 200 or less	18	3.278	1.227	3.556	1.294	0.278	1.227
- 220 or less	12	3.250	1.288	3.000	1.537	-0.250	1.913
- 240 or less	36	3.500	1.483	3.361	1.313	-0.139	1.437
- less than 262	35	3.486	1.222	3.486	1.337	0.000	1.328
- 280 or less	24	3.042	1.628	3.500	1.668	0.458	1.215
- 300 or less	47	3.383	1.526	3.830	1.185	0.447	1.299
Total	283	3.329	1.417	3.431	1.386	0.102	1.332

Table 5 Descriptive Statistics of Academic Achievement: English

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	111	3.306	1.542	3.306	1.548	0.000	1.421
- 200 or less	18	3.056	1.893	2.889	1.967	-0.167	1.581
- 220 or less	12	2.667	1.826	2.667	1.723	0.000	1.128
- 240 or less	36	3.611	1.626	3.167	1.875	-0.444	1.698
- less than 262	35	3.371	1.497	3.286	1.673	-0.086	1.173
- 280 or less	23	2.826	1.850	2.826	1.749	0.000	1.168
- 300 or less	47	3.383	1.836	3.830	1.551	0.447	1.176
Total	282	3.284	1.659	3.280	1.671	-0.004	1.382

Table 6 Descriptive Statistics of Dummy Variable for Use of Cram Schools

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	112	0.598	0.492	0.536	0.501	-0.063	0.470
- 200 or less	18	0.389	0.502	0.500	0.514	0.111	0.471
- 220 or less	12	0.583	0.515	0.583	0.515	0.000	0.426
- 240 or less	36	0.583	0.500	0.556	0.504	-0.028	0.506
- less than 262	35	0.457	0.505	0.486	0.507	0.029	0.568
- 280 or less	22	0.409	0.503	0.636	0.492	0.227	0.813
- 300 or less	47	0.553	0.503	0.532	0.504	-0.021	0.642
Total	282	0.543	0.499	0.539	0.499	-0.004	0.550

Table 7 Descriptive Statistics of Study Hours on Weekdays

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	118	1.593	0.744	1.530	0.795	-0.064	0.855
- 200 or less	18	1.306	0.755	1.417	0.781	0.111	0.595
- 220 or less	13	1.712	0.776	1.673	0.960	-0.038	0.742
- 240 or less	38	1.684	0.902	1.763	0.953	0.079	0.690
- less than 262	35	1.336	0.653	1.700	0.897	0.364	0.794
- 280 or less	24	1.500	0.894	1.594	0.920	0.094	1.418
- 300 or less	47	1.681	0.810	1.803	0.693	0.122	1.012
Total	293	1.568	0.784	1.629	0.832	0.061	0.899

Table 8 Descriptive Statistics of Study Hours on Holidays

The Eligibility Index	Number of Observations	Baseline		Endline		Difference	
		Mean	SD	Mean	SD	Mean	SD
180 or less	115	1.757	1.051	1.667	1.094	-0.089	0.952
- 200 or less	18	1.694	0.834	1.944	0.860	0.250	0.514
- 220 or less	13	1.577	1.170	1.769	1.321	0.192	0.678
- 240 or less	38	1.737	1.098	1.586	1.182	-0.151	1.110
- less than 262	34	1.588	1.131	1.640	1.192	0.051	1.048
- 280 or less	23	1.283	0.978	1.815	1.398	0.533	1.313
- 300 or less	46	1.609	0.981	2.141	1.158	0.533	1.425
Total	287	1.660	1.042	1.763	1.156	0.103	1.097

Table 9 Result of Window Selection

Window Length/2	Balancing Test p-value	Variable (min p-value)	Num of Observasions left	right
7.70	0.164	Academic Achievment (Jpn)	15	9
13.30	0.250	Academic Achievment (All)	24	16
18.90*	0.261	Academic Achievment (All)	28	23
24.50	0.116	Study Hours on Weekdays	34	47
30.10	0.118	Academic Achievment (All)	40	63
35.70	0.165	Academic Achievment (All)	55	67
41.30	0.328	Study Hours on Holidays	67	67
46.90	0.282	Academic Achievment (All)	72	67
52.50	0.282	Study Hours on Holidays	76	67
58.10	0.338	Study Hours on Holidays	79	67

(Notes) * denotes selected window.

Table 10 RD Estimates for Difference of Academic Achievements

	All	Mathematics	Japanese	English
Treatment Effect	0.707**	-0.093	0.653*	0.133
95% Confidence Interval	[0.076 , 1.296]	[-0.770 , 0.550]	[0.000 , 1.320]	[-0.450 , 0.750]
p-value (Randomization Test)	0.026	0.859	0.071	0.730
Number of Observations	281	284	283	282
Left of Cutoff	211	213	212	212
Right of Cutoff	70	71	71	70
Effect Number of Observations	53	55	55	54
Left of Cutoff	29	30	30	30
Right of Cutoff	24	25	25	24

Table 11 Questionnaires on Motivation for Learning

	Questions	No.	Question Contents	Category
About School	What do you think about Q(i) −Q(viii)?	i	You would feel happy when you are told to do chores by your teacher	4 Pliability
		ii	You would be encouraged to write a diary when you are told by your teacher that it would help improve your essay	
		iii	You can easily start doing chores if you are asked to help by your teacher(s)	
		iv	You would like to read the book which your teacher recommended to you for your Japanese study	
		v	You can easily understand questions on the exams because your teacher is very good at teaching	
		vi	You would be responsible for your duty (work)	3 Responsibility
		vii	You are surely resposible for what you are supposed to do on a group presentation	
		viii	You do not ususally raise your hand in class because you are afraid of being laughed at for your mistakes	6 Avoidance of Failures (Inverse Indicator)
	What do you think about Q(i) −Q(xiv)?	i	You know how well you have done with your exam right after it	5 Self-appraisal
		ii	You check your test score when it has been returned	
		iii	After the exam, you look up the answers to see if you got them correct	
		iv	When you study for the exams, you set a goal for your test score	
		v	The more you think the test is impotant for you, the worse your performance on it will be	6 Avoidance of Failures (Inverse Indicator)
		vi	You either ask your teacher or resolve the questions you could not solve on your maths test until you are satisfied	2 Achievement-oriented
		vii	Once you are stuck in the middle of a test, you tend to fail to get the answers for the questions you could normally solve	6 Avoidance of Failures (Inverse Indicator)
		viii	Even if the qustions seem difficult, you try your best to get the answer(s)	2 Achievement-oriented
		ix	When things you are supposed to do look somewhat difficult, you feel it is even more difficult because you think you cannot do well	6 Avoidance of Failures (Inverse Indicator)
		x	If you think you can solve the maths story problems, you will try your best until you get the answer even if the problems look difficult	2 Achievement-oriented
		xi	You peresistently consdier the difficult questions/problems regarding your Japanese class	
		xii	You underestimate your achievement because you are unwilling to realise that you have done less than you expected	6 Avoidance of Failures (Inverse Indicator)
		xiii	You immdeately start studing even if you do not like it	2 Achievement-oriented
		xiv	You carefully think if your way of learning is good	5 Self-appraisal
About Learning	What do you think about Q(i) −Q(x)?	i	You study at home as well as at school because you want to know a lot of things	1 Autonomous Learning Attitudes
		ii	You think it is not worth studying things you do not want to study	8 Values (Inverse Indicator)
		iii	You think it would be much more fun without studying	
		iv	You study without being told by your family member(s) to do so	1 Autonomous Learning Attitudes
		v	You study the subject you are not good at without being told to do so	
		vi	You think you do not have to study if you can get good scores on your exam(s)	8 Values (Inverse Indicator)
		vii	You set goals and plan well when studying	1 Autonomous Learning Attitudes
		viii	You think it will be better to ask someone who knows than putting too much effort into your studies	8 Values (Inverse Indicator)
		ix	You are usually prepared well for class	1 Autonomous Learning Attitudes
		x	You become skeptical about why you study	8 Values (Inverse Indicator)
	What do you think about Q(i) −Q(viii)?	i	You think you get bored easily	7 Persistence (Inverse Indicator)
		ii	You cannot stop watching your favorite TV programme even when it is about time to study	
		iii	You finish your homework within the day no matter how long it takes	3 Responsibility
		iv	Whenever there is something you have left, you will finish it later	
		v	You tend to get bored with studying	7 Persistence (Inverse Indicator)
		vi	You tend to stop studying when solving difficult questions because you get tired	
		vii	When you study, you stop studying if you find something else that interests you more	
		viii	You always finish your homework even if you are a little sick	3 Responsibility

Table 12 RD Estimates for Difference of Academic Achievements

by High and Low Motivation for Learning: Average of All Subjects

	High	Low
Treatment Effect	0.806	0.621
95% Confidence Interval	[-0.174 , 1.736]	[-0.144 , 1.364]
p-value (Randomization Test)	0.134	0.162
Number of Observations	142	139
Left of Cutoff	101	110
Right of Cutoff	41	29
Effect Number of Observations	25	28
Left of Cutoff	16	13
Right of Cutoff	9	15

(Notes) ***, **, and * denote significance at the 1 %, 5 %, and 10% level respectively.

Table 13 RD Estimates for Difference of Academic Achievements

by Non-relative Poverty and Relative Poverty

	Non-relative Poverty	Relative Poverty
Treatment Effect	0.307	0.750*
95% Confidence Interval	[-0.432 , 1.080]	[-0.133 , 1.600]
p-value (Randomization Test)	0.559	0.088
Number of Observations	169	99
Left of Cutoff	147	57
Right of Cutoff	22	42
Effect Number of Observations	19	32
Left of Cutoff	8	20
Right of Cutoff	11	12

(Notes) ***, **, and * denote significance at the 1 %, 5 %, and 10% level respectively.

Table 14 RD Estimates for Differences of Use of Cram School and Study Hours

	Use of Cram Schools	Study Hours on Weekdays	Study Hours on Holidays
Treatment Effect	0.217	-0.160	0.513*
95% Confidence Interval	[-0.154 , 0.538]	[-0.706 , 0.422]	[-0.095 , 1.095]
p-value (Randomization Test)	0.298	0.592	0.091
Number of Observations	282	293	287
Left of Cutoff	213	222	218
Right of Cutoff	69	71	69
Effect Number of Observations	53	55	53
Left of Cutoff	30	30	29
Right of Cutoff	23	25	24

(Notes) ***, **, and * denote significance at the 1 %, 5 %, and 10% level respectively.

Table 15 Estimation Results: Deviation Value of Mathematics on Self-rated Relative Score

	Deviation Value of Mathematics	
	(1) Baseline	(2) Endline
Self-rated Relative Score at Baseline	8.488***	
	(1.019)	
Self-rated Relative Score at Easeline		11.09***
		(0.970)
Constant	19.14***	14.81***
	(4.069)	(3.932)
Number of Observations	77	91
R-squared	0.469	0.540

(Notes) Robust standard errors in parentheses
***, **, and * denote significance at the 1 %, 5 %, and 10% level respectively.

Table 16 Probit Estimation Results: Re-applicants

		Marginal Effects		
		(1)	(2)	(3)
The Eligibility Index		0.000758***	0.000745***	0.000776***
		(0.000256)	(0.000256)	(0.000259)
Academic Achievements	All		0.0264**	0.0261
			(0.0119)	(0.0255)
	Mathematics			-0.00261
				(0.0172)
	Japanese			0.00642
				(0.0195)
	English			-0.00332
				(0.0172)
Use of Cram Schools				0.0738*
				(0.0405)
Study Hours on Weekdays				-0.0548*
				(0.0311)
Study Hours on Holidays				0.0283
				(0.0226)
Observations		629	629	629
Pseudo R squared		0.0111	0.0171	0.0234

(Notes) Robust standard errors in parentheses.
***, **, and * denote significance at the 1 %, 5 %, and 10% level respectively.

Table 17 Number of Observations by the Eligibility Index

The Eligibility Index	Number of Observations	
	Third Graders of Junior High School	Other Graders
180 or less	13	99
- 200 or less	2	16
- 220 or less	1	11
- 240 or less	2	33
- less than 262	4	30
- 280 or less	21	2
- 300 or less	47	0
Total	90	191