GÖTEBORGS UNIVERSITET

HANDELSHÖGSKOLAN

Methods of Economic Evaluation and Experiments
Spring 2023

Group: 2
Members: Olivia Jonsson, Carlos Pérez

Question 1

a:

See Figures and Graphs in Appendix 1.

Variables used:
- Year of interview.
- Educational attainment.
- Total children ever born.
- Age at first birth.

b:

The relationship between education and childbirth is negative (see Graph 1), meaning higher education results in fewer births, and the respondent is older when they have their first child. However, it is important to note that the marginal change in the total number of children from women that have had secondary education to women with higher education is relatively small (see Graph 2). Also, the age at first birth in the first two cohorts of education is virtually the same, but we see a sudden change in women with secondary or higher education (see Graph 3). Possibly this indicates that sex education and knowledge of contraceptive methods peak at the secondary level of education and that women will be older at the time of their first birth if they decide to dedicate time to a higher level of education. Additionally, there are no significant differences between survey years (see Graph 1); there is a similar trend. Since we have repeated cross-sectional data, we can compare survey years as long as there is a random sampling from the same original population. All surveys are from Uganda and by using a representative probability sample. Therefore, the individuals in the earlier studies can be used as substitutes to compare with those from later studies. However, since the survey uses different individuals annually, we can only say something about population changes over time. (Anthony, Walthery and King-Hele, 2015).

Question 2

a:

We decided to restrict the sample to the survey year 2016 because the surveys have slight differences in the number of individuals and how they categorise the regions. Additionally, the sample size this year was the biggest. In order to compare between years using repeated cross-sectional data, sampling strategies should be the same.

Dependent variable:

v201 total children ever born

We want to estimate the effect of education on childbearing. For this, we used the total number of children ever born. Other variables in the data set connected to childbearing are less relevant and may not represent the total number of pregnancies and births a woman has completed.

Explanatory:

v133 is a good proxy for education, but the negative of using this variable is that it does not indicate how well someone performed in school, i.e. if one graduated or not.

Controls:

Omitted variable bias is a critical threat when running an OLS regression. In order to find a negative causal interpretation of the relationship between education and childbearing, we need to check that we do not have critical confounding variables left in the error term. Otherwise, the result might be biased.

v025 Urban/Rural (binary variable)

We used a binary variable representing whether the individual lives in an urban or rural region. We believe this is an essential control since we know that there are relevant differences in educational and medical accessibility. For example, rural areas tend to have fewer and less equipped schools, and some rural clinics may not be prepared to help women through their pregnancy or sexually educate their youth.

v155 literacy (binary)

While this variable originally had several literacy levels, these levels did not represent a relevant difference in the subject's reading ability. Therefore, we divided these levels and created a binary variable. Reading ability is measured in two categories: the individual can read or can not. Literacy determines the ability of an individual to get information. For example, a young woman cannot read brochures about sexual education or pregnancy prevention if her literacy variable equals 0. This variable could have reverse causality, but it is essential to include it because it could measure how well the individual achieves education.

V191 wealth index

The difference in socio-economic status is critical to determine most of the differences in the population, more so in developing countries. These differences can determine the possibilities for individuals when making decisions. Therefore, we use the wealth index given by the database as a proxy for socio-economic status.

v212 age at first birth

Since the dependent variable is the total number of children ever born and the focus of this study is to reveal the effect that education has on this, then knowing the age at which the subject started having children gives insight into the choices and possibilities that the individual had regarding education.

v010 year of birth

An individual's birth year can indicate what kind of environment they grew up knowing. In addition, it can indicate the individual's values, morals, technology access, and even their country's socio-political landscape. Therefore, it is beneficial to control the dependent variable of childbearing by the age of birth since It is most likely that if the individual made a conscious decision to have children, it was affected by the environment and culture of the time.

b:

The results (see table 7) indicate that an additional year of education decreases the total number of children born by approximately -0.058. Additionally, the coefficient is significant at a 5% significance level. The regression-adjusted R-square, which punishes for the additional controls that we have, has a value of approximately 66, meaning we have high explanatory power in our regression; the regression variables explain much of the variation in

our sample. The effect is what we would expect; however, it is small. All our control variables are significant.

Question 3:

a:

The treatment variable reflects several educational attainment levels; the issue with this treatment is that several subjects still need to complete levels of education. Therefore we decided to create bigger subgroups of educational attainment; the first group would take up all subjects that do not have education or just incomplete primary as the control group, then use complete primary school as treatment. Similarly, in the secondary school treatment, we pair anyone with less than complete secondary as a control and use complete secondary as the treated. We avoid matching incomplete secondary with complete secondary because the extra years of education may bias the actual effect of the treatment.

Characteristics determined before or after the treatment can be used as suitable matching variables. Ideally, these characteristics make the subjects biologically and socio-economically similar so that any exogenous variables that may bias the result area are accounted for. Variables like the wealth index score, age at first intercourse, current age, region, religion, marital status, if they live in an urban or rural environment, and literacy give the model more precision when matching subjects. The options a subject may have at the time of their first pregnancy are highly correlated to their wealth. Age at first intercourse indicates if the subject had a larger probability of getting pregnant at a younger age, and the current age controls for generational bias between the individuals. External factors to the individual that affect their environment are also important; they can affect the subject's access to education or contraceptive methods, and some religions even prohibit contraceptive methods. When looking for covariates that structure the propensity score matching (PSM), it is better to use many variables. This will not affect precision since PSM weights characteristics depending on their relevance. (Stuart, 2010)

b:

The covariates define closeness; combining this will output a distance measurement between the subjects. To select the covariates that define the distance between the individuals, we have to choose the independent variables related to the treatment and outcome; thus, using any

variables that may bias the estimation is obligatory to fulfil the ignorability assumption. The second stage to measure closeness is defining the distance or similarity. This can be defined using four different methods; however, we used propensity score matching; this is the most common method because it gives the least amount of bias. PSM allows us to identify the probability of an individual being treated subject to specific covariates or characteristics. This method assigns the probability of being treated to individuals of both the treated and the control groups and then matches the ones with the same or similar probability.

$$D_{ij} = \left| e_i - e_j \right|$$

If ignorability holds in the set of observable differences, then it also holds for PSM, thus, giving an unbiased estimate of the difference in means (Stuart, 2010).

c:

The most common and reliable method is Nearest Neighbour Matching. This method selects a treated individual k and matches it with the closest control individual j. As we can see from appendix 1, Nearest Neighbour Matching discards a great number of observations, leading to possible reduced power; however, the difference between the total observations and the matched observations is minimal and helps us to give less weight to the individual that cannot be matched and therefore may cause some bias or give results that cannot be generalised. The matching method follows the next equation:

$$\left| p_i - p_j \right| = min\left\{ \left| p_i - p_k \right| \right\}$$

The distance between the subjects has to be equal to the minimum possible distance for matching to occur. The number of neighbours that are included in a set group can be larger than two; however, this could increase the variance of the estimations, but it also increases precision when matching. Since the sample used is within a delimited country and gender, we have minimal variance and thus can use methods to increase precision.(Stuart 2010)

d:

Referring to table 5, we can see that the number of treated subjects remains, meaning that the is no reduced power when using Nearest Neighbour Matching and the number of controlled observations lowers to match the number of treated subjects, It may seem that because the number of observations, in general, is reduced significantly that the matching has less power, however, this reduction is not related to matching power but the number of observations in the category. The covariate balance is greater in the matched case since the variance ratio is reduced in most of the covariates. Also, the standardised means of all covariates are closer to zero in the matched case.

e:

To interpret the effects of the nearest neighbour matching model causally, we have to look at the external and internal validity of the model based on matching and the covariates that were used the same way as Ester Duflo did in her article from 2001 (Duflo, 2001). The first threat to appear against internal validity is compliance with the Conditional Independence Assumption. However, because ignorability holds for all covariates used in this particular model, we can assume that CIA also holds for this set of covariates. Regarding external validity, since the covariates control for most of the characteristics that can tell women apart and can be generalised to other countries, however, region and the cultural context that comes with it may affect Ugandan women in ways that cannot be carried on to other groups of women. Nonetheless, the explanatory power of the region is low enough that it does not represent a major problem for the external validity of the matching method and estimations.

f:

Looking at the covariates balances, the total number of matches and the standardised differences between the characteristics of the individuals, we can conclude that the matching gives us a better estimate of the treatment effect on the treated. Therefore we can make more accurate regressions that better reflect the correlation between a woman's education and the possible number of children she will have. The regression output indicates a decrease of approximately .106 in the number of children the subject will have. When the subjects are exposed to the treatment of primary school, and for the case of treatment of secondary school, the decrease in the total number of children is 0.302. From this, there seems to be an effect on the treatment that correlates with educational attainment and childbearing.

Question 4:

a:

The population sample needs to be big enough in order for the bias sustained by the strong instrument to be small enough. If the instrument is weak, it does not matter how big the sample is; the estimate will still be biased (Murray, 2006).

b:

We include control variables to ensure the instrument is uncorrelated with the error term. That is, the exposure to the reform is not correlated with any other determinants of the total number of children (Stock and Watson 2015, chapter 13). We also want to capture the good variation in the instrument, which is why we include control variables.

Most importantly, we use this control variable to help the IV's local treatment effect approach the average treatment effect of the whole sample. Since we cannot be entirely sure that those affected by the reform are as good as random, it is necessary to use control variables to reduce bias. We have not included v191 wealth index since it is not correlated with our instrument and therefore does not cause bias in our estimate.

c:

The estimated effect is summarised in tables 8 and 9 (see appendix 1). It indicates that the individuals that are affected by the treatment have approximately 1.68 less total number of children ever born. The first stage indicates that if an individual is affected by the reform, they are likely to have approximately 0.388 years of education, so it has a significant impact on education in single years. However, even if you are affected by the treatment, you might not comply, also known as the heterogeneous treatment effect. So the result is only informative about the population of compilers in our sample; that is, we can estimate the local average treatment effect of the treated (Lindskog, 2023).

d:

Using exposure to the reform as an instrument should only be considered if the instrument is relevant and exogenous (Murray, 2006). Starting with the relevance condition, this implies

that the instrument (exposure to the reform, Z_i) is correlated with the variable of interest (education in single years, X_i). In mathematical terms this would look like:

$$corr\ (Z_i,\ X_i)\ \neq\ 0$$

Moreover, the instrument needs to explain the variation in the explanatory variable, in our case, education. It is reasonable to think that exposure to the reform, meaning education will be free for that individual, could explain some variation in education. Uganda has a relatively low GDP per capita, and making it free would create more opportunities for women to partake in primary school education, meaning there would not be zero correlation between the instrument and the variable of interest.

Moving on to the second condition exogeneity, the instrument can not be correlated with the error term:

$$corr\ (Z_i,\ u_i)\ =\ 0$$

If this condition is fulfilled, then the variation that we can find in the educational variable, which is captured by the instrumental variable exposure, is exogenous. Since the reform was enforced in the year 1997, one could argue that it is as if random and, therefore, would not be correlated with the error term. However, since this is an observational study, assuming that the instrument (reform) would be uncorrelated with the error term is unreasonable. Therefore, we include some additional controls.

There is one way to check if the instrument is weak or not, that is if the relevance criteria hold, and that is to look at the first stage F-statistic. The rule of thumb is that the F-statistic should be bigger than 10. In our first stage regression (see table 11), it is equal to 21.22, indicating that we have a strong instrument. The two-stage least squares estimator is unbiased. Our first stage regression:

$$X_i\ =\ \beta_1 Z_i + \beta_2 W_{1i} + \beta_3 W_{2i} + \beta_4 W_{3i} + \beta_5 W_{4i}$$

Question 5:

a:

The running variable is the year of birth. However, it is not possible to estimate a sharp RD because even if the year of birth is the primary determinant of the threshold, other variables and individual characteristics might affect the treatment and make individuals near the threshold have different effect magnitudes. For example, some subjects might have failed some primary years and, consequently, repeated those courses and ended up being affected by the treatment, even though they were not supposed to or had a delayed entrance to school. Like this, several aspects make the RD fuzzy.

b:

See appendix 1 for more information on the RD regression. It is important to first look at underidentification and the weak identification test; this indicates that the instrument and variables used for the fuzzy RD are significant. The zero value of the chi-squared indicates that the IV is relevant. The RD regression needs to satisfy certain assumptions; Independence, the reform causes this complies to this condition since is the main change that happens in those years and since the policy was applied to all schools, subjects that went through the effect should be as good as random; Exclusion, that in this case is satisfied by including control variables, which will isolate the effect of the treatment; Relevance, this condition requires that the first stage of the regression is strong, meaning that there is a strong connection between the threshold and the treatment, lookin at table 13 of Appendix 1, we can observe that the f value of the first stage is greater than 11, to be more precise 25.65, additional to this the p value is zero and the effect that the treatment has on education is of .679, meaning that the treatment has an increasing effect on years of education in the subject, thus the RD is relevant; Monotonicity is covered by the graph on appendix one, where we can see that the effect is equal for those who cross the threshold. However, two main problems arise from this RD. Firstly, adding control variables is the only way to make the RD significant, and secondly, the effect resulting from the treatment is low, and in the reduced form, regression is not significant. This indicates that the policy implemented in 1983 did not

have the expected effect and possibly did not change the overall education landscape in Uganda. (Xiao, 2022)

c:

We need to allow different functional forms on either side of the threshold. Earlier in this question, we assumed a linear trend in our running variable. However, looking at the plot, we can see that discontinuity only holds when one allows for non-linear trends in the reduced form and linear trends in the first stage. For us to be able to find an effect, there needs to be a discontinuity in the first stage. Robustness indicates whether the results are reliable, mainly in fuzzy RDs; robustness can indicate how delicate the results are to changes in the bandwidth or kernel. We look for similar values in closer bandwidths, which means that the results are not volatile and, therefore, significant. As we can see from table 15, the RD estimates are greatly impacted by the bandwidth, even if the change in bandwidth is only one: in the first stage, the result is a coefficient of 0.624, and in the treatment estimate, the coefficient is .111. Additionally, the z scores are greatly reduced when accounting for robustness. Lastly, the way that polynomials change the coefficient estimates can also indicate a lack of robustness and, therefore, reliability. From table 15 through 17 we can see that the coefficients are not similar and are greatly affected by the polynomial level; this means that the results are unreliable and again that the reform did not have a great effect on the levels of education and consequently childbearing.

d:

Increasing bandwidths increases external validity but decreases internal validity. This is because people around the threshold need to be comparable, and increasing the bandwidth increases the likelihood that people are different on either side of the threshold. Tables 18 through 20 show that a change in bandwidth changes the coefficients greatly, and there is not an exact correlation between increasing bandwidth and increasing coefficients. Thus, the results are not reliable.

Question 6:

Firstly we will describe the setting of this canonical difference-in-difference case and later we will go further into how we would find and impact and evaluate its reliability. In this setting it is plausible to assume that Uganda is the only country that implemented the removal of

primary school fees. Kenya does not receive treatment. We would still use repeated cross sectional data for both countries and look at the year before and after treatment, that is 1996 and 1997. The sample would have to be restricted according to what age the respondent was at the time of the interview year. Additionally, discussed earlier in this paper, the sample draws would also need to be random for us to be able to compare outcomes for different time periods.

Moving on to finding the impact, we would have a regression equation looking like this:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 G_i + \beta_3 D_t + u_{it}$$

In the above equation the variable of interest is X_{it}, an interaction term consisting of G_i x D_t. D_t is a binary variable indicating which period we are in, that is before and after treatment. G_i is also a binary variable that shows if an individual is treated or not. The variable t stands for time period (t = 1, 2), i stands for individuals and r indicates the number of individual characteristics that we control for. Therefore, if an individual is treated the interaction term will be active and we can interpret the β_1 as the average treatment effect of the treated.

Moreover, for the suggested regression above to work, we need to make sure the assumption of parallel trends is fulfilled (Roth et. al, 2022). This will result in an unbiased estimator. Parallel trends refers to that if Uganda did not implement the school reform they would have the same trend as Kenya. In mathematical terms that would be:

$$E\left[y_{t=2}^{Uganda}(0) - y_{t=1}^{Uganda}(0)\right] = 0$$

Or:

$$E\left[y_{t=2}^{Uganda}(0) - y_{t=1}^{Uganda}(0)\right] = E\left[y_{t=2}^{Kenya}(0) - y_{t=1}^{Kenya}(0)\right]$$

The goal is to be able to remove the trend in child bearing and only identify the effect of the implementation of the school reform policy. There are some sensitivity checks that can be

done in order to check whether the parallel assumption holds. These include plotting pre treatment trends between treated and control groups in our case between Uganda and Kenya. Placebo tests involve taking all control groups (or pre-treatment period for the treated group) and randomly assign some of them with a placebo treatment status, then run the DID for the placebo treatment group. Moreover, it is also beneficial to think about other shocks that could have happened around the same time as the treatment took place. If there are, the effect we might find might have appeared due to that shock rather than the school reform. One also needs to consider pre-treatment differences between the treatment (Uganda) and the control group (Keynia) that could lead to differential trends in outcomes. Lastly, we need to assess if there is any risk of people from Kenya moving to Uganda in order to receive treatment. If there is a risk, the composition of the treatment and control group might cause problems for the estimation (Stock and Watson 2015, chapter 13).

Question 7:

The aim is to see how the reform (removing primary school fees) might affect years of education and in turn possibly reduce the total number of children. Using data from each of the observed regions and individual birth year, we would be able to estimate the variation of the treatment. The effect of treatment for individuals who are born before 1983 that had either finished or already dropped out of primary school would probably be zero, however there might be some of them who started school late and would be affected by the reform but this should not be a large number of individuals. For children born in 1983 and onward there should be an increasing effect of the school reform for those children due to the fact that for each year after 1983 those children benefit from 2 years of free education and so on until all children have benefitted from the reform their entire school period. However, this continuous treatment variable would be difficult to use since there is no obvious control group to compare these different birth cohorts to, so we will focus on geographical regions.

It is reasonable to assume that depending on your geographical region there are different opportunities for you to attend school. For example, if you live in rural parts of the country with low access to a primary school, the reform would not have much effect, compared to a less rural area where people might be poor but they have access to primary schools, and the effect of the reform would be bigger. Therefore, we would like to use geographical regions to match individuals from the treated group with someone from the control group and that way

identification of average treatment effect would be relaxed and our estimate more accurate. We would be able to test the parallel trends assumption within the age group that was not affected by the treatment. Their educational level should not be different between different regions. When we run the regression the weights would be different depending on which group we compare to whom, and the average of these estimated coefficients would be our difference in difference estimator.

Question 8:

This analysis used data from the Uganda Demographic and Health surveys, conducted in the years 2006, 2011 and 2016. These surveys have a really wide variety of variables for education and childbearing, the relationship between these two is the main focus of this study, thus, selecting correct variables is most important. The first main finding of the research is that there is evidence to say that a significant correlation between the variables exist, however the effect is not big enough to suggest that the amount of education that an individual receives changes the total number of children. Looking at the graphs 2 and 3, we can see that a big effect only exists with women that completed or incurred in higher education. We believe that the Ugandan education system may not be of a quality good enough that It creates big changes between educated and not educated subjects, choosing a Country that has a bigger impact on the educated subject through good schooling programs could better understand the true effect of education on childbearing. This situation might reduce the external validity of the results gathered in this study.

Moving on, we matched the data of different subjects to increase the external validity of the research. This was done taking into account various variables that affect the subjects on an individual level, for example: urban or rural environments, literacy, wealth, marital status, age, region and religion. When matching subjects we can better understand the true effect of just education on childbearing, thus, we can confidently say the the effects are true for Ugandan women in general, however we cannot generalise this effect outside Uganda, since other characteristics should be taking into account, additionally Uganda is not a representative country for the sample of all countries in the world.

Continuing to further results, we found that there was an education reform implemented in Uganda in 1997 where all primary school fees were removed, this reform could affect the

levels of education in women that were born after 1983. This represented an opportunity to better understand the effects of education on childbearing by using the reform as a treatment and the age of birth as the threshold, if the reform was significant then we would see a fuzzy RD that represented a change in the education of the treated. However, since the reform did not address the main issue with this study, that is the quality of education, then the change was not significant and the treated did not differ much from the untreated. Lastly, we applied a theoretical framework to the possibility of using a difference in difference model to estimate the impact of the reform, this using Kenya as a control group, where they didn't get the reform, we assumed that the data was similar.

Regarding the internal validity of the experiment, since the data set is just a random generate subsample of 50% of the main dataset and all regressions and quasi experiments done in this study used a randomised sample of the main experiment, then there is no selection bias that may threaten the results. The instrumental variables that we used when looking at the effect of the reform are valid since their p value is zero, and therefore the instruments are good. Attrition is solved by matching the individual in the control group with individuals in the treatment group that share the same characteristics, so individuals that did not correctly fulfil some parts of the survey are not accounted for, we ended up with 2,308 matches between the subjects. (Stock and Watson, 2015, chapter 5)

In conclusion, this experiment holds great internal validity, we can say that for women surveyed the effect is relevant and negative between years of education and total children, this means that for every extra year studied the subjects will have -.057 children. The external validity is low in this study, we can say that for Ugandan women the effect should be around the same magnitude, however for other countries and women around the globe the magnitude of the effect is uncertain. Causation is a very delicate subject and in this case it is better to not infer it, we can only be sure about the negative correlation that education has on child bearing. It is important to continue studying this relationship, taking into account more variables and bigger datasets.

Appendix 1:
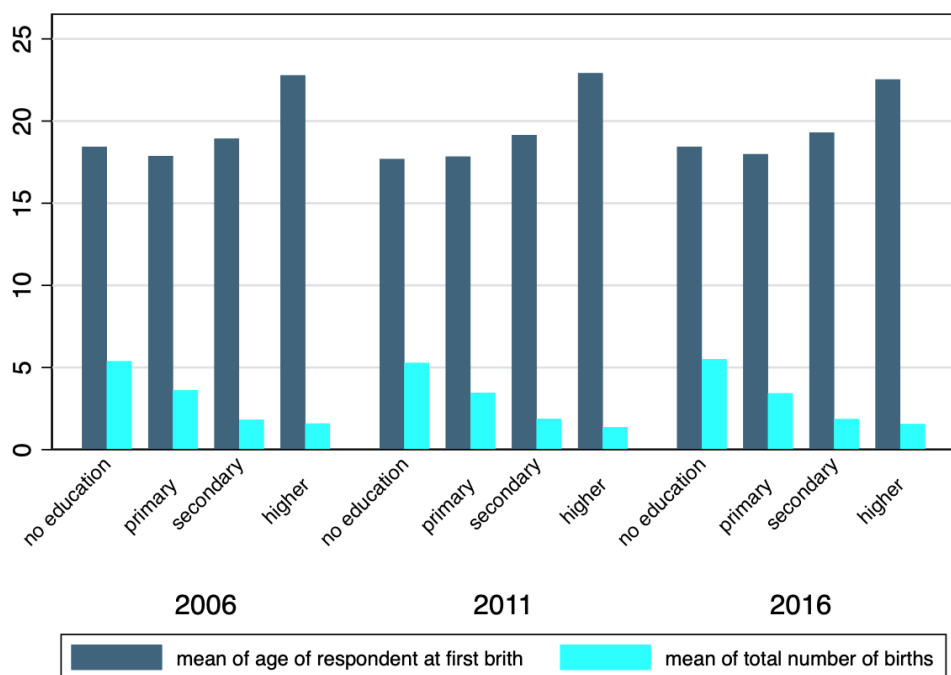
Graph 1

Graph 2

Graph 3

Graph 4

Regression function fit
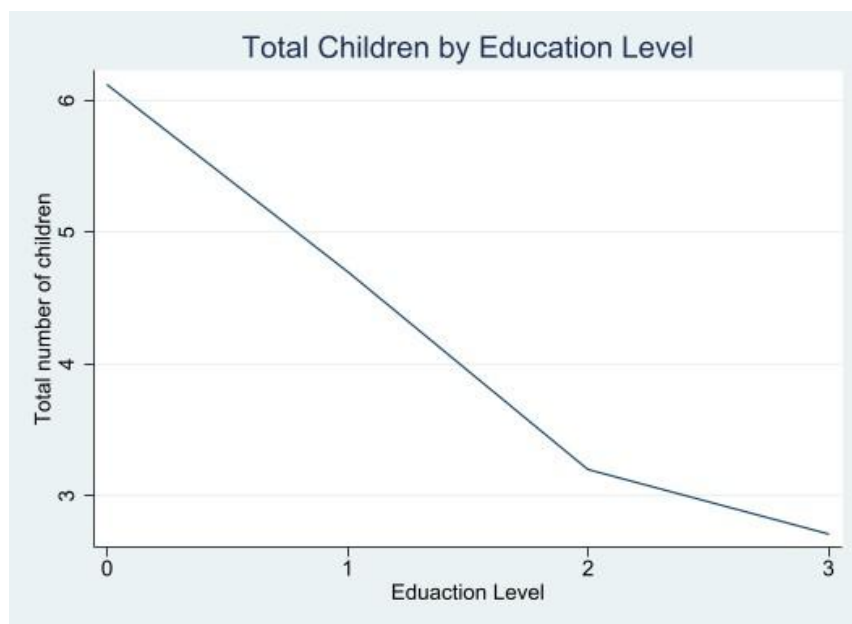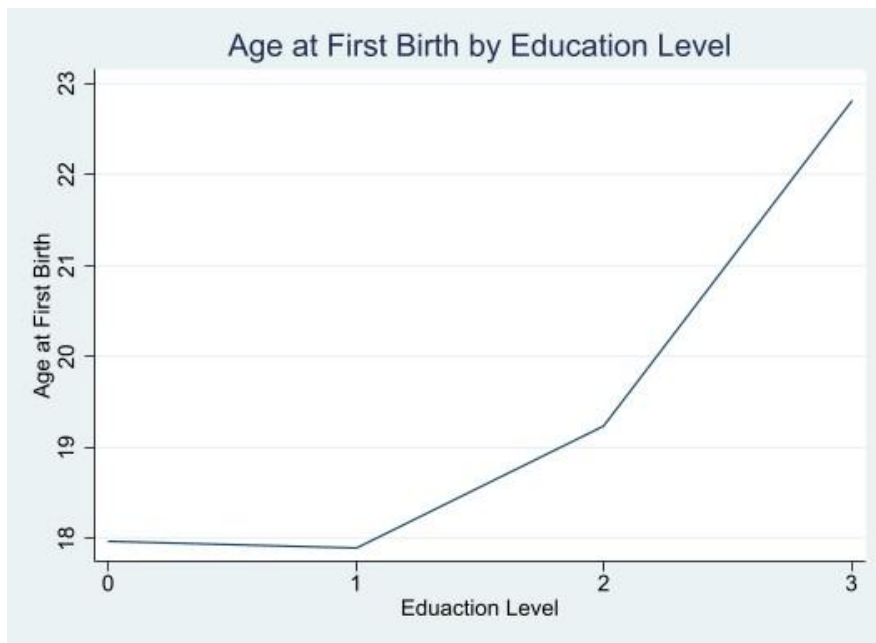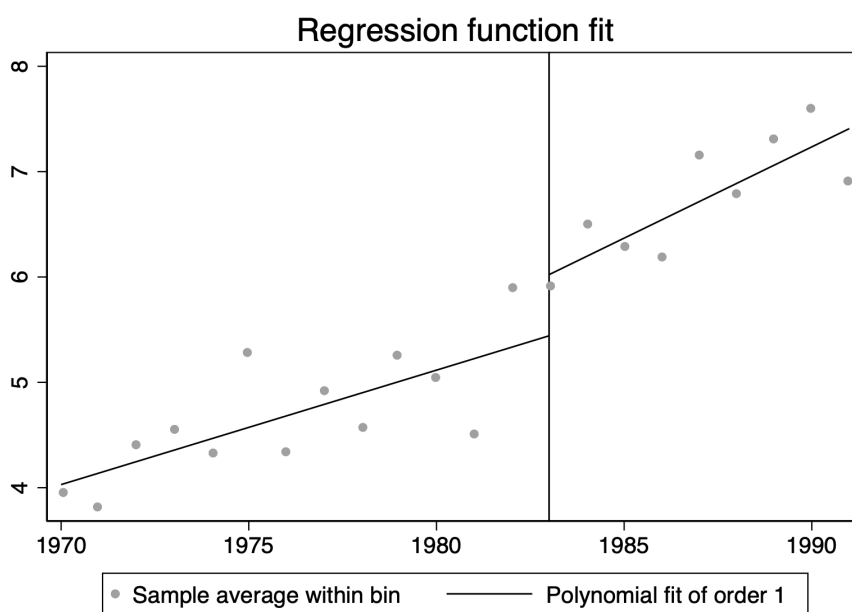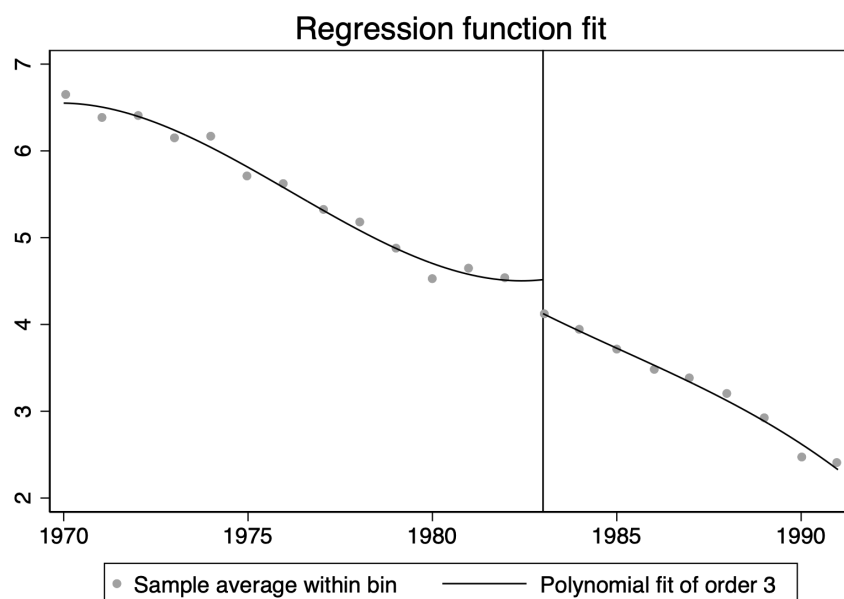
Graph 5

```
. tabstat TotalChildren AgeFirstBirth if Year == 2006, by(HighestEducLevel) stat(mean)

Summary statistics: mean
  by categories of: HighestEducLevel (highest educational level)
```

HighestEducLevel	TotalC~n	AgeFir~h
no education	5.383138	18.44037
primary	3.615199	17.87962
secondary	1.820375	18.92891
higher	1.588957	22.78947
Total	3.612278	18.3023

Table 1

```
. tabstat TotalChildren AgeFirstBirth if Year == 2011, by(HighestEducLevel) stat(mean)

Summary statistics: mean
  by categories of: HighestEducLevel (highest educational level)

HighestEducLevel │  TotalC~n  AgeFir~h
─────────────────┼──────────────────────
     no education │  5.277165  17.68959
         primary │  3.461216  17.84013
       secondary │  1.878607  19.15298
          higher │  1.363985  22.91608
─────────────────┼──────────────────────
           Total │  3.231451  18.30673
```

Table 2

```
. tabstat TotalChildren AgeFirstBirth if Year == 2016, by(HighestEducLevel) stat(mean)

Summary statistics: mean
  by categories of: HighestEducLevel (highest educational level)

HighestEducLevel │  TotalC~n  AgeFir~h
─────────────────┼──────────────────────
     no education │  5.508057  18.44343
         primary │  3.430599  17.98822
       secondary │  1.861004  19.30787
          higher │  1.560584  22.53699
─────────────────┼──────────────────────
           Total │  3.177114    18.585
```

Table 3

```
Treatment-effects estimation              Number of obs     =      6,477
Estimator        : propensity-score matching    Matches: requested =        1
Outcome model    : matching                                  min =        1
Treatment model: logit                                       max =        1
```

v201	Coefficient	AI robust std. err.	z	P>|z|	[95% conf. interval]	
ATET						
primaria						
(1 vs 0)	-.1057192	.1116392	-0.95	0.344	-.324528	.1130895

Table 4

	Raw	Matched
Number of obs =	6,477	2,308
Treated obs =	1,154	1,154
Control obs =	5,323	1,154

Table 5

	Standardized differences		Variance ratio	
	Raw	Matched	Raw	Matched
v191	.4797949	-.0878748	1.257035	.7353867
v012	-.203445	.0498424	.786188	.8671364
v525	.1843975	.0608536	.8796724	1.016475
reg16A2	.2061447	-.0027725	1.933494	.993092
reg16A3	.0680454	-.0123719	1.247546	.9640443
reg16A4	.021471	-.0190934	1.072429	.9428166
reg16A5	-.0511947	-.0353632	.8364204	.8815425
reg16A6	.0266002	-.0292286	1.112453	.8970005
reg16A7	-.0229345	.0345083	.9292041	1.126468
reg16A8	-.2958927	.0446323	.1346844	1.793734
reg16A9	-.0417723	0	.88006	1
reg16A10	-.1427113	.0129507	.5821503	1.060947
reg16A11	-.2621669	.0049493	.3591098	1.026858
reg16A12	.0062449	.0516188	1.021285	1.194491
reg16A13	.0280683	-.0186439	1.092764	.9459536
reg16A14	.1573895	.0112279	1.607604	1.029058
reg16A15	.0377397	-.0145151	1.16059	.9479359
rel16A2	.0722023	-.0091141	1.056078	.9940484
rel16A3	-.0909571	.0408957	.9716299	1.019029
rel16A4	.056763	-.0553104	1.15797	.8808446
rel16A5	.0136579	-.0429379	1.110273	.7453416
cms2	.0403027	.1052892	.9732486	.9370065
cms3	-.0562412	-.0331629	.8884743	.9307658
urban2	-.2732999	-.0472855	1.566322	1.062305
Literacy	1.01338	.0077546	1.00008	.9922362

Table 6

```
. reg v201 v133 v212 v191 v010 Literacy Urban, robust

Linear regression                              Number of obs  =      6,844
                                               F(6, 6837)     =    1431.90
                                               Prob > F       =     0.0000
                                               R-squared      =     0.6601
                                               Root MSE       =     1.5978

                          Robust
      v201 |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-----------+----------------------------------------------------------------
      v133 | -.0579813    .0071419    -8.12   0.000    -.0719817   -.0439808
      v212 | -.2398336    .0068155   -35.19   0.000    -.2531942    -.226473
      v191 | -2.35e-06    2.45e-07    -9.57   0.000    -2.83e-06   -1.87e-06
      v010 |  -.238195    .0029294   -81.31   0.000    -.2439376   -.2324524
  Literacy | -.1216506    .0497626    -2.44   0.015    -.2192009   -.0241004
     Urban | -.2868401    .0523464    -5.48   0.000    -.3894553   -.1842248
     _cons |  481.8699    5.863746    82.18   0.000     470.3751    493.3646

.
. di e(r2_a)
.65976271
```

Table 7

```
. ivregress 2sls v201 v212 v010 Literacy Urban (v133=reform), robust first

First-stage regressions
------------------------

                                               Number of obs  =     13,055
                                               F(  5,  13049) =    3175.08
                                               Prob > F       =     0.0000
                                               R-squared      =     0.5568
                                               Adj R-squared  =     0.5567
                                               Root MSE       =     2.7028

                          Robust
      v133 |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-----------+----------------------------------------------------------------
      v212 |  .2140577    .0088929    24.07   0.000     .1966263     .231489
      v010 |  .0668923    .0042778    15.64   0.000     .0585071    .0752775
  Literacy |  4.810806    .0500573    96.11   0.000     4.712687    4.908926
     Urban |  1.629608    .0658722    24.74   0.000     1.500489    1.758727
    reform |  .3882662    .0842815     4.61   0.000     .2230622    .5534702
     _cons | -133.8769    8.452679   -15.84   0.000    -150.4453   -117.3084
```

Table 8

```
Instrumental variables (2SLS) regression        Number of obs   =      13,055
                                                 Wald chi2(5)    =     2668.49
                                                 Prob > chi2     =      0.0000
                                                 R-squared       =           .
                                                 Root MSE        =       4.717

                          Robust
        v201 |     Coef.   Std. Err.       z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
        v133 | -1.676514    .3807409    -4.40   0.000    -2.422753   -.9302758
        v212 |  .1262906    .0835373     1.51   0.131    -.0374396    .2900207
        v010 | -.0607464    .0320745    -1.89   0.058    -.1236113    .0021184
    Literacy |  7.550071    1.838466     4.11   0.000     3.946743     11.1534
       Urban |  2.043261    .6308387     3.24   0.001     .8068395    3.279682
       _cons |  127.4245     64.0005     1.99   0.046     1.985821    252.8632

Instrumented:   v133
Instruments:    v212 v010 Literacy Urban reform
```

Table 9

```
. reg v133 reform v212 v010 Literacy Urban, robust

Linear regression                                Number of obs   =      13,055
                                                 F(5, 13049)     =     3175.08
                                                 Prob > F        =      0.0000
                                                 R-squared       =      0.5568
                                                 Root MSE        =      2.7028

                          Robust
        v133 |     Coef.   Std. Err.       t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
      reform |  .3882662    .0842815     4.61   0.000     .2230622    .5534702
        v212 |  .2140577    .0088929    24.07   0.000     .1966263     .231489
        v010 |  .0668923    .0042778    15.64   0.000     .0585071    .0752775
    Literacy |  4.810806    .0500573    96.11   0.000     4.712687    4.908926
       Urban |  1.629608    .0658722    24.74   0.000     1.500489    1.758727
       _cons | -133.8769    8.452679   -15.84   0.000    -150.4453   -117.3084
```

Table 10

```
. test reform

 ( 1)  reform = 0

       F(  1, 13049) =    21.22
            Prob > F =     0.0000
```

Table 11

```
                                           Number of obs =      8101
                                           F(  4,  8096) =   1747.56
                                           Prob > F      =    0.0000
Total (centered) SS    =  47693.28058      Centered R2    =    0.4651
Total (uncentered) SS  =       240410      Uncentered R2 =    0.8939
Residual SS            =  25510.8868       Root MSE       =    1.775


        v201 │ Coefficient  Std. err.      z    P>|z|     [95% conf. interval]

        v133 │  -.0236656   .1095945    -0.22   0.829     -.238467    .1911357
        v010 │  -.1940154   .0158036   -12.28   0.000    -.2249899   -.1630408
        v212 │  -.2537582    .031108    -8.16   0.000    -.3147287   -.1927876
        v191 │  -4.89e-06   2.47e-06    -1.98   0.048    -9.73e-06   -5.27e-08
       _cons │   393.936   31.29955    12.59   0.000      332.59     455.282

Underidentification test (Anderson canon. corr. LM statistic):       25.580
                                              Chi-sq(1) P-val =       0.0000

Weak identification test (Cragg-Donald Wald F statistic):            25.646
Stock-Yogo weak ID test critical values: 10% maximal IV size         16.38
                                         15% maximal IV size          8.96
                                         20% maximal IV size          6.66
                                         25% maximal IV size          5.53
Source: Stock-Yogo (2005).  Reproduced by permission.
```

Table 12

```
First-stage regression of v133:

Statistics consistent for homoskedasticity only
Number of obs =                8101
```

v133	Coefficient	Std. err.	t	P>|t|	[95% conf. interval]	
treat	.6799617	.1342699	5.06	0.000	.4167582	.9431652
v010	.0946207	.0109502	8.64	0.000	.0731555	.116086
v212	.2776898	.0104424	26.59	0.000	.25722	.2981595
v191	.0000225	3.50e-07	64.22	0.000	.0000218	.0000232
_cons	-187.6481	21.64555	-8.67	0.000	-230.079	-145.2173

```
F test of excluded instruments:
  F(  1,  8096) =     25.65
  Prob > F      =    0.0000
Sanderson-Windmeijer multivariate F test of excluded instruments:
  F(  1,  8096) =     25.65
  Prob > F      =    0.0000
```

Table 13

```
Reduced-form regression: v201

Statistics consistent for homoskedasticity only
Number of obs =                8101
```

v201	Coefficient	Std. err.	t	P>|t|	[95% conf. interval]	
treat	-.0160917	.0747703	-0.22	0.830	-.1626608	.1304773
v010	-.1962546	.0060978	-32.18	0.000	-.2082079	-.1843013
v212	-.2603299	.005815	-44.77	0.000	-.2717288	-.248931
v191	-5.42e-06	1.95e-07	-27.82	0.000	-5.81e-06	-5.04e-06
_cons	398.3768	12.05367	33.05	0.000	374.7485	422.0051

Table 14

```
. rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(2) h(13 12)
```

Fuzzy RD estimates using local polynomial regression.

Cutoff c = 1983	Left of **c**	Right of **c**		Number of obs	=	17856
				BW type	=	Manual
Number of obs	7351	10505		Kernel	=	Uniform
Eff. Number of obs	5516	7888		VCE method	=	NN
Order est. (p)	2	2				
Order bias (q)	3	3				
BW est. (h)	13.000	12.000				
BW bias (b)	13.000	12.000				
rho (h/b)	1.000	1.000				

First-stage estimates. Outcome: v133. Running variable: v010.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	.62453	.2317	2.6955	0.007	.17041	1.07865
Robust	–	–	0.4201	0.674	-.508844	.786518

Treatment effect estimates. Outcome: v201. Running variable: v010. Treatment Status: v133.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	.11175	.20992	0.5324	0.594	-.29968	.523179
Robust	–	–	1.2774	0.201	-.207551	.98436

Table 15

```
. rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(3) h(13 12)
```

Fuzzy RD estimates using local polynomial regression.

Cutoff c = 1983	Left of **c**	Right of **c**		Number of obs	=	17856
				BW type	=	Manual
Number of obs	7351	10505		Kernel	=	Uniform
Eff. Number of obs	5516	7888		VCE method	=	NN
Order est. (p)	3	3				
Order bias (q)	4	4				
BW est. (h)	13.000	12.000				
BW bias (b)	13.000	12.000				
rho (h/b)	1.000	1.000				

First-stage estimates. Outcome: v133. Running variable: v010.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	.13884	.33046	0.4201	0.674	-.508844	.786518
Robust	–	–	-1.0602	0.289	-1.45383	.433097

Treatment effect estimates. Outcome: v201. Running variable: v010. Treatment Status: v133.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	1.3562	3.8912	0.3485	0.727	-6.27035	8.98279
Robust	–	–	1.4046	0.160	-3.15278	19.1019

Table 16

```
. rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(13 12)

Fuzzy RD estimates using local polynomial regression.
```

Cutoff c = 1983	Left of **c**	Right of **c**			
			Number of obs =		17856
			BW type =		Manual
Number of obs	7351	10505	Kernel =		Uniform
Eff. Number of obs	5516	7888	VCE method =		NN
Order est. (p)	4	4			
Order bias (q)	5	5			
BW est. (h)	13.000	12.000			
BW bias (b)	13.000	12.000			
rho (h/b)	1.000	1.000			

First-stage estimates. Outcome: v133. Running variable: v010.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	-.51037	.48137	-1.0602	0.289	-1.45383	.433097
Robust	–	–	-1.2922	0.196	-2.49648	.512559

Treatment effect estimates. Outcome: v201. Running variable: v010. Treatment Status: v133.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	-.44419	.50965	-0.8716	0.383	-1.44309	.554706
Robust	–	–	-1.3405	0.180	-2.73582	.513438

Table 17

```
. rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(10 11)
```

Fuzzy RD estimates using local polynomial regression.

Cutoff c = 1983	Left of **c**	Right of **c**		Number of obs	=	17856
				BW type	=	Manual
Number of obs	7351	10505		Kernel	=	Uniform
Eff. Number of obs	4492	7327		VCE method	=	NN
Order est. (p)	4	4				
Order bias (q)	5	5				
BW est. (h)	10.000	11.000				
BW bias (b)	10.000	11.000				
rho (h/b)	1.000	1.000				

First-stage estimates. Outcome: v133. Running variable: v010.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	−.90566	.65466	−1.3834	0.167	−2.18877	.377458
Robust	−	−	−2.6739	0.007	−5.64424	−.86964

Treatment effect estimates. Outcome: v201. Running variable: v010. Treatment Status: v133.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	−.61677	.45904	−1.3436	0.179	−1.51647	.282938
Robust	−	−	0.7008	0.483	−1.08766	2.29823

Table 17

26

```
. rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(8 7)
```

Fuzzy RD estimates using local polynomial regression.

Cutoff c = 1983	Left of **c**	Right of **c**		Number of obs	=	17856
				BW type	=	Manual
Number of obs	7351	10505		Kernel	=	Uniform
Eff. Number of obs	3727	4934		VCE method	=	NN
Order est. (p)	4	4				
Order bias (q)	5	5				
BW est. (h)	8.000	7.000				
BW bias (b)	8.000	7.000				
rho (h/b)	1.000	1.000				

First-stage estimates. Outcome: v133. Running variable: v010.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	-3.2251	.93251	-3.4585	0.001	-5.05276	-1.39738
Robust	–	–	-2.6842	0.007	-9.80872	-1.52961

Treatment effect estimates. Outcome: v201. Running variable: v010. Treatment Status: v133.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	-.29334	.1432	-2.0485	0.041	-.574003	-.012671
Robust	–	–	-1.4083	0.159	-1.09425	.179207

Table 18

```
. rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(6 5)
```

Fuzzy RD estimates using local polynomial regression.

Cutoff c = 1983	Left of **c**	Right of **c**		Number of obs	=	17856
				BW type	=	Manual
Number of obs	7351	10505		Kernel	=	Uniform
Eff. Number of obs	2854	3605		VCE method	=	NN
Order est. (p)	4	4				
Order bias (q)	5	5				
BW est. (h)	6.000	5.000				
BW bias (b)	6.000	5.000				
rho (h/b)	1.000	1.000				

First-stage estimates. Outcome: v133. Running variable: v010.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	-7.2926	1.7758	-4.1067	0.000	-10.773	-3.81216
Robust	–	–	-0.8701	0.384	-17.4717	6.7287

Treatment effect estimates. Outcome: v201. Running variable: v010. Treatment Status: v133.

Method	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
Conventional	-.31564	.12127	-2.6027	0.009	-.553334	-.07795
Robust	–	–	-1.9859	0.047	-1.66636	-.010969

Table 19

Appendix 2:

Question 1

```
use "/Users/oliviajonsson/Desktop/assignmentdata.dta", clear
preserve
rename v212 AgeFirstBirth
rename v201 TotalChildren
rename v106 HighestEducLevel
rename v007 Year
graph bar AgeFirstBirth TotalChildren, over(HighestEducLevel) over(Year)
tabstat TotalChildren AgeFirstBirth if Year == 2006, by(HighestEducLevel) stat(mean)
tabstat TotalChildren AgeFirstBirth if Year == 2011, by(HighestEducLevel) stat(mean)
tabstat TotalChildren AgeFirstBirth if Year == 2016, by(HighestEducLevel) stat(mean)
```

Question 2

```
use "/Users/oliviajonsson/Desktop/assignmentdata.dta", clear
preserve

keep if v007 == 2016
codebook v201, tab(100)
codebook v133, tab(100)
codebook v212, tab(100)

drop if v212 ==.

codebook v191, tab (100)
codebook v010, tab (100)
codebook v155, tab (100)

drop if v155==3
drop if v155==4

gen Literacy = 0
replace Literacy = 1 if v155==2
```

codebook v025, tab (100)

gen Urban = 0
replace Urban = 1 if v025==1

pwcorr v201 v133 v212 v191 v010 Literacy Urban, star(0.05)
reg v201 v133 v212 v191 v010 Literacy Urban, robust
di e(r2_a)

Question 3
use "C:\Users\carlo\OneDrive\Documentos\CIDE\Sexto Semestre (Intercambio)\Methods of Economic Evaluation and Experiments\Annika\STATA\assignmentdata.dta"
Preserve

gen Literacy = 0
replace Literacy = 1 if v155==2
tab region_2016, gen(reg16A)
tab religion_2016, gen(rel16A)
tab v502, gen(cms)
tab v025, gen(urban)

gen primaria=cond(v149==2,1,0) if v149<=2
teffects psmatch (v201) (primaria v191 v012 v525 reg16A2 reg16A3 reg16A4 reg16A5 reg16A6 reg16A7 reg16A8 reg16A9 reg16A10 reg16A11 reg16A12 reg16A13 reg16A14 reg16A15 rel16A2 rel16A3 rel16A4 rel16A5 cms2 cms3 urban2 Literacy) if primaria<=1, atet generate(primary)
tebalance density
tebalance summarize

gen secondary=cond(v149==4,3,2,0) if v149<=4
teffects psmatch (v201) (secondary v191 v012 v525 reg16A2 reg16A3 reg16A4 reg16A5 reg16A6 reg16A7 reg16A9 reg16A10 reg16A12 reg16A13 reg16A14 reg16A15 rel16A2 rel16A3 rel16A4 rel16A5 cms2 cms3 urban2 Literacy) if secondary<=3, atet generate(secondary2)

tebalance density

tebalance summarize

Question 4

```
use "/Users/oliviajonsson/Desktop/assignmentdata.dta", clear

preserve

//Exposure to reform

codebook v010, tab(100)

gen reform = 0

replace reform = 1 if v010>=1983

//Controls

codebook v201, tab(100)

codebook v133, tab(100)

codebook v212, tab(100)

drop if v212 ==.

codebook v010, tab(100)

codebook v155, tab (100)

drop if v155==3

drop if v155==4

gen Literacy = 0

replace Literacy = 1 if v155==2

codebook v025, tab (100)

gen Urban = 0

replace Urban = 1 if v025==1

//Checking correlation between varibles contained in the error and the IV

pwcorr reform v010 v191 v212 Literacy Urban, star(0.05)

//Run the IV regression

ivregress 2sls v201 v212 v010 Literacy Urban (v133=reform), robust first

//Check the first stage F stat
```

```
reg v133 reform v212 v010 Literacy Urban, robust
test reform
```

Question 5
```
use "/Users/oliviajonsson/Desktop/assignmentdata.dta", clear
preserve

//Basic fuzzy RD with IV command
codebook v010

//We need to have year of birth as the running variable
//Interaction term year x treat
gen treat= (v010>=1983)
gen v1332 = v133^2
gen interact=v010*treat
ivreg2 v201 v010 v212 v191 (v133=treat) if v010 >=1970 & v010 <=1995 & v012>=25, rf
first

//Check discontinuity in the 1-stage
rdplot v133 v010 if v010 >=1970 & v010 <=1995 & v012>=25, c(1983) p(1) //You need a
Discontinuity in the FS

//Check discontinuity in the outcome
rdplot v201 v010 if v010 >=1970 & v010 <=1995 & v012>=25, c(1983) p(3)

//Check robustness to a non-linear continuous effect of the running variable
rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(2) h(13 12)
rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(3) h(13 12)
rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(13 12)

//Check robustness to a few alternative reasonable bandwidths
rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(10 11)
rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(8 7)
rdrobust v201 v010, kernel(uniform) c(1983) fuzzy(v133) p(4) h(6 5)
```

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