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Can Education Change Risk Preference? Evidence from Indonesia and Mexico

Reene Zhou

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Can Education Change Risk Preference? Evidence from Indonesia and Mexico *

ZIREN ZHOU †

Abstract

To test whether education can change risk preference, I exploit the Indonesian school construction programme and the Mexican education reform in compulsory schooling as two separate natural experiments. Applying the instrumental variable approach, I do not find a causal effect of education on risk preference. The results are consistent in the two different settings, so my findings are externally valid. The results suggest that a change in risk preference may not be the channel via which the impact of education on risk-taking in real life. This paper contributes to the literature on the determinants of social preferences and the outcomes of education.

JEL classification: I25, D90

Keywords: Risk preference, risk aversion, education

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Data sources: The corrected INPRES programme data was obtained from Roodman (2022): https://github.com/droodman/Duflo-2001. The original version of INPRES programme data was provided by Esther Duflo. The individual-level data was obtained from the Indonesian Family Life Survey and Mexican Family Life Survey.

†reneezh980701@gmail.com

1 Introduction

This paper estimates the causal effect of education on risk preference. Risk preference plays an important role in an individual's decision-making behaviours under uncertainty throughout the life cycle, including in domains such as investment and saving, purchase of insurance, occupation choice and marriage (Rosenzweig & Stark, 1989). In developing countries, risk preference is also related to the type of crops planted, adoption of risky high-yielding innovation in agriculture (Duflo et al., 2011; Kremer et al., 2013), migration (Andersen et al., 2014) and utilisation of low-interest loans such as microcredit (Celik et al., 2018). Differences in risk preference lead to different decisions and behaviours and thus inequality in economic outcomes and well-being. Therefore, understanding the determinants of risk preference helps us understand poverty-perpetuating mechanisms. Additionally, understanding risk preference helps policymakers design policies and interventions related to human capital development such as health policies, pension systems and education policies.

Although risk preference is ubiquitous and important, we know limited about the causal processes shaping risk preference. The literature suggests that risk preference is determined by both nature and nurture. Recent studies have empirically examined some factors that might affect risk preference, including natural disasters and violence (Brown et al., 2019; Callen et al., 2014; Voors et al., 2012; Islam et al., 2020). However, limited studies have empirically investigated the impact of education, and the evidence from the literature is mixed (Banks et al., 2019; Jung, 2015; Tawiah, 2022). Therefore, we have not yet reached an agreement on the impact of education on risk preference.

Intuitively, education might influence risk preference in two opposite ways. On the one hand, education could make people less risk-averse, as education encourages individuals to be open to the unknown and thus more willing to challenge themselves through risky activities. This pathway is supported by descriptive studies. For example, farmers with more education are less risk-averse and more likely to adopt technological innovation (Knight et al., 2003) and plant cash crops (Zhao & Yue, 2020). Furthermore, empirical studies also establish causality between education and risk-taking in real life. For example, education leads to an increased probability of participating in the financial market (Black et al., 2018; Cole et al., 2014) and a higher proportion of wealth allocated to financial assets (Shaw, 1996).

On the other hand, education could make people less impulsive or spontaneous and thus less likely to participate in risky activities. This pathway is directly supported by recent empirical evidence (Jung, 2015; Tawiah, 2022). Furthermore, educated individuals are less likely to undertake illegal activities and buy lottery products (Rogers & Webley, 2001), and more likely to purchase insurance and consume preventive health care. The third possible outcome is that education does not change risk preference (Banks et al., 2019). As the evidence on the impact of education is conflicting, we know little about whether education can change risk preference and, if yes, how education affects risk preference.

The problem in terms of testing whether education can change risk preference is that the observable association between risk preference and education might not be causality. The omitted variables that are likely correlated to risk preference would lead to selection bias, such as wealth and family background. Reverse causality could be another concern, as education itself could be considered a risky investment. Innately less risk-averse individuals might be more willing to invest in education. Therefore, to obtain an unbiased causal estimate of the impact of education on risk preference, we need to randomly assign education to individuals or exploit exogenous variations in education.

In this paper, I examine the causal effect of education on risk preference in Indonesia and Mexico. In Indonesia, I first estimate the effect of the introduction of the INPRES school construction programme on education, using the Indonesia Family Life Survey (IFLS) and the administrative data of the INPRES programme (Duflo, 2001; Roodman, 2022). The first-stage difference-in-difference analyses exploit geographic variation in the intensity of the programme and variation in programme exposure based on birth year. They show that school construction programmes improved exposed individuals' years of education and the probability of completing primary school. Applying an instrumental variable (IV) approach, I find no significant effect of education on risk preference.

To provide evidence that the above result is externally valid, I replicate the test in Mexico using the Mexico Family Life Survey (MxFLS). I exploit educational reform in compulsory schooling in 1993 to provide estimates of this policy change. Before 1993, secondary schooling was not compulsory in Mexico, and the reform made secondary schooling compulsory on a national scale. Comparing the risk preference of pre- and post-reform cohorts, I find a consistent result that education has no significant effect on risk preference. Mexico and Indonesia have different cultures, education systems, and general risk attitudes[1]. Therefore, replicating the results in a very different setting suggests that my findings are externally valid.

[1]Figure A2 reports the distribution of risk aversion in two countries. We can see that individuals in Mexico are generally less risk-averse than those in Indonesia.

1.1 Contribution

This paper contributes to several literatures. First, it is closely related to a large number of descriptive studies suggesting that education is negatively correlated with risk aversion (Donkers et al., 2001; Falk et al., 2018; Harrison et al., 2007; Hartog et al., 2002). However, as stated above, a simple ordinary least squares (OLS) estimate of the effect of education on risk preference involves selection bias and reverse causality problems. I contribute by providing a causal estimate of the effect of education on risk preference.

Second, this paper builds on a growing literature on the formation and evolution of risk preference (Hryshko et al., 2011; Falk et al., 2018; Dohmen et al., 2011). Twin studies suggest that genetic component variations can partially explain differences in risk preference (Barnea et al., 2010; Calvet & Sodini, 2014), and risk preference can also be affected by early life environment and culture (Andersen et al., 2013; Armin & Kosse, 2016; Booth & Nolen, 2012). Large empirical papers study the impact of extreme events, such as a loss in investment during the financial crisis (Andersen et al., 2019; Guiso et al., 2013) and exposure to crime and violence (Brown et al., 2019; Callen et al., 2014; Voors et al., 2012) and natural disasters (Islam et al., 2020). These large shocks are too rare to predict and thus could not be manipulated or prevented. In contrast, this paper studies a potential factor, education, which could be manipulated relatively easily by policy and is likely to affect large cohorts.

Third, this paper contributes to the general literature investigating the effect of education on social preferences and economic preferences such as time preferences and competitiveness preferences (Cappelen et al., 2020; Friedman et al., 2016; Jung et al., 2021; Sutter et al., 2020) and directly contributes to that investigating the effect of education on risk attitudes (Banks et al., 2019; Jung, 2015; Tawiah, 2022). Risk preference is usually measured by three methods: self-reported risk attitude[2], hypothetical lotteries[3] and real-stakes gambles or games conducted in experiments[4] (Dohmen et al., 2011). Jung (2015) and Tawiah (2022) applied the IV approach to estimate the causal effect of education on risk attitude, exploiting the school-leaving age reforms in the UK and Germany, respectively. The authors used self-reported risk attitude as a measure of risk preference and found that education makes individuals more risk-averse. However,

[2] For example, Andersen et al. (2008) and Jung (2015) used self-report risk attitudes to measure risk preference.

[3] For example, Brown et al. (2019) and Banks et al. (2019) used hypothetical lotteries to measure risk preference.

[4] For example, Booth & Nolen (2012) used a real-stake gamble to measure the attitudes toward risk and Andersen et al. (2013) conducted games in the field as an experiment to measure the attitudes toward risk.

Banks et al., (2019) conducted real-stake risky choices games in experiments and found that education has no causal effect on risk aversion, exploiting the education reform in the UK. This paper contributes to the literature by estimating the effect of education on risk preference using hypothetical lotteries as a measure of risk preference. Furthermore, all of the above studies are based in high-income countries, and they estimate the effect of one more year of high school education. This paper contributes by estimating the effect of primary and secondary schooling and adding evidence in the developing context.

Additionally, this paper contributes to emerging literature empirically examining the causal effect of education on risk-taking in real life. Cole et al. (2014) and Black et al. (2018) exploited the compulsory schooling reform in the US and Sweden, respectively, as exogenous variations in education and found that schooling increases the probability of participating in the financial market. Cole et al., (2016) found that extra mathematics training in high schools leads to greater financial market participation, exploiting exogenous variation induced by state-level course requirements in the US. Although risky behaviours reflect the risk preference and risk preferences also predict the risky behaviours (Dohmen et al., 2011; Falk et al., 2018), risk-taking in real life is also affected by other factors, such as information asymmetry, saving and investment habits and credit constraints. Black et al. (2018) speculated that a change in risk preference might be a potential pathway via which education influences risk-taking in real life, but this mechanism could not be tested in that study due to the lack of information to measure risk preference. Therefore, this paper contributes to this literature by testing whether risk preference is a pathway in which education affects risk-taking in real life.

Finally, this paper contributes to the mounting literature on the impact of education on individual economic outcomes and wellbeing, particularly those exploiting the INPRES school construction programme in Indonesia (Akresh et al., 2018; Duflo, 2001, 2004; Jung et al., 2021; Mazumder et al., 2019). Most studies used the INPRES administrative data used by Duflo (2001), but Roodman (2022) recently found that data include 10% errors in the intensity of the programme. Furthermore, the old version only includes the total number of schools constructed without detailed information about the number of schools constructed each year during the programme. This paper contributes by using the corrected and detailed data of the INPRES programme to provide a more precise estimate of the impact of the programme.

2 Context

In this section, I briefly describe the background of the natural experiments exploited in Mexico [5].

2.1 The compulsory schooling reform in Mexico

In 1993, the Mexican government introduced a national compulsory schooling reform that made junior secondary schooling compulsory, aimed at improving the national education level.[6] Before the reform, only primary education was compulsory in Mexico. Individuals usually attend primary school at the age of 6 in Mexico. Primary schooling contains six grades and junior secondary education contains three grades, so individuals usually attend secondary school at the age of 12 and complete secondary school at the age of 14. The educational reform in 1993, therefore, raised the length of compulsory schooling from six years to nine years.

Thus, individuals aged below 12 were affected by the reform, while individuals aged above 14 were not exposed to the reform. Individuals aged between 12 and 14 were partially treated. For individuals aged between 12 and 14 (inclusive), if one had not attended secondary school before the reform, they were not affected by the reform as they were too old to attend secondary school. However, if one had already attended secondary school, he/she could drop out of school at any grade without the reform, but was required to complete all the grades in secondary school after the reform. This explains why individuals aged between 12 and 14 were partially treated.

3 Identification Strategy

If education is assigned randomly among individuals, we could simply use OLS to estimate the impact of education on risk preference:

$$R_i = \alpha_0 + \beta S_i + X_i + \varepsilon_i \tag{1}$$

where R indicates the outcome of interest, i.e., risk preference of individual i, measured by the level of risk aversion. A higher value of R denotes a higher level of risk aversion. S is the education of the individual i. X_i is a vector of characteristics. The coefficient of interest is β. $\beta = 0$ refers to education not affecting an individual's risk

[5] The online appendix describes the natural experiment exploited in Indonesia.

[6] Gleditsch et al. (2022) provide more details about the reform.

preference. $\beta > 0$ refers to education making individuals less willing to take risks. $\beta < 0$ refers to education making individuals more willing to take risks.

Estimating equation (1) via OLS will lead to biased estimates of β. This is because the unobservable omitted variables that are likely correlated to risk preference would lead to selection bias, such as genetic component, family background and wealth. Additionally, reverse causality would be another concern, as education itself could be considered a risky investment. Innately less risk-averse individuals might be more willing to invest in education. Therefore, to obtain an unbiased causal estimate of the impact of education, we exploit exogenous variations in education. I use exogenous variations in education induced by the school construction programme in Indonesia and education reform in Mexico. In both countries, my empirical specifications are based on two-stage equations, using the IV approach.

3.1 Indonesia

My first-stage equation in Indonesia is a difference-in-difference strategy which is similar to the strategy used by Duflo (2001). In this strategy, two sources of variations determine an individual's exposure to the programme. First, if an individual was sufficiently young to attend primary school when the programme was introduced, i.e., aged below 7 in 1973, he/she was exposed to the programme and thus belongs to the treatment group. If an individual was too old to attend school, when the programme was introduced, i.e., aged above 12 in 1973, he/she does not benefit from the programme and thus belongs to the control group. Second, the intensity of the programme measured by the number of schools constructed varies across districts. Individuals in districts with a high intensity have a higher exposure than those in districts with a low intensity of the programme. The combination of cohort variation and geographic variation creates a difference-in-difference estimator of the effect of the school construction programme. The first-stage equation is:

$$S_{i,d,t} = c_1 + \sigma(T_i \times Intensity_{t,d}) + X_{i,d,t} + \alpha_d + \gamma_t + \varepsilon_{i,d,t} \tag{2}$$

where i denotes an individual, t denotes the individual's birth year, and d denotes the district of an individual's birth. $S_{i,d,t}$ consists of years of education and an indicator variable for completing primary school. T_i is an indicator variable equal to 1 if an individual was aged below 7 in 1973. $Intensity_{t,d}$ denotes the intensity of the programme, measured by the total number of schools constructed per thousand children by an individual's age of 6 since 1973. α_d captures the district of birth fixed effect and this control for time-invariant unobservable difference across districts. γ_t captures the year of birth fixed effect and this controls for unobservable differences across cohorts.

The term $X_{i,d,t}$ is a vector of controls. The individual-level controls consist of gender, dummy variables whether one lives in an urban area, whether one belongs to the majority religion, whether one belongs to the majority ethnicity and whether one speaks the majority language. To control for the omitted time-varying and district-specific characteristics that correlated with the INPRES programme, I also interact the birth year fixed effect with another ongoing water sanitation programme during the same period, the enrolment rate in the district and the child population before the programme (in 1971) and include these interactions as controls[7]. Finally, I two-way cluster the standard error by birth province and birth year.

There are four main ways in which my estimation differs from that of Duflo (2001). First, I use different data. While Duflo (2001) used the 1995 Intercensal Survey of Indonesia, I use the fourth wave of the Indonesian Family Life Survey (IFLS4) for individual-level information. This is because IFLS4 provides information on risk preference. For INPRES programme data, I used the corrected and detailed version provided by Roodman (2022). According to Roodman (2022), the INPRES data used by Duflo (2001) included around 10% errors in the intensity of the programme. The new version data also collected information about the number of schools constructed each year between 1973 and 1978, while the old version only includes the total number of schools constructed at the district level.

Second, with the detailed version data on the INPRES programme, I also calculate the intensity of the programme for the treatment group in a slightly different way that Duflo (2001) did. Duflo (2001) calculate the intensity of the programme for an individual based on the total number of schools constructed in his/her district within the programme period. I define the intensity of the programme of an individual as the number of schools constructed by his/her age of 6 since 1973, so the intensity of the programme varies across young cohorts based on the birth year. For example, the intensity of an individual who was 6 in 1973 is the number of schools constructed per thousand children in his/her district in 1973; the intensity of an individual who was 6 in 1974 is the number of schools constructed per thousand children in 1973 and 1974 in his/her district; and so on. Individuals who were 6 and younger in 1978 have the same intensity. My way to measure the intensity should be more accurate, as the programme lasted for six years and thus the intensity within the same district should increase each year during the period. With the traditional way to measure the intensity, the intensity only varies at the district level and this might underestimate the impact of the programme.

[7] Controls for district-level follow the controls that were used in Duflo(2001).

Third, the cohorts I include in my samples are slightly different to the cohorts used by Duflo (2001). Duflo (2001) defines individuals aged 2 to 6 in 1974 as young cohorts and individuals aged 12 to 17 in 1974 as old cohorts. I define individuals aged -1 to 6 in 1973[8] as young cohorts and individuals aged 12 to 19 in 1973 as old cohorts. There are two reasons I adjust the sample. First, Duflo (2001) sets the programme started in 1974, while the INPRES data from Roodman (2002) shows that the programme began in 1973[9]. Second, I hope to include more observations, because the individual-level data I used (IFLS) include fewer observations than the one Duflo (2001) used (SUPAS). Additionally, I include both females and males in the sample for my analysis, while Duflo (2001) does not include females[10].

Finally, in addition to the district-level controls used in Duflo (2001), I also include the controls at the individual level. Furthermore, the standard errors were not corrected for auto-correlation within the district in Duflo (2001), while I improve this by two-way clustering the standard errors.

After estimating the effect of the school construction programme on education in the first stage, I then use the predicted education $\widehat{S_{i,d,t}}$ to estimate the effect of education on risk preference. The second stage takes the form:

$$R_{i,d,t} = c_2 + \beta\widehat{S_{i,d,t}} + X_{i,d,t} + \alpha_d + \gamma_t + \varepsilon_{i,d,t} \tag{3}$$

where i denotes an individual, t denotes the individual's birth year, and d denotes the district of an individual's birth. $R_{i,d,t}$ is a measure of the level of risk aversion, ranging from 0 to 4. $R_{i,d,t}$ increases with the level of risk aversion. $R_{i,d,t}$ equal to 0 refers to the least risk-averse, and $R_{i,d,t}$ equal to 4 refers to the most risk-averse. α_d captures the district of birth fixed effect and this control for time-invariant unobservable difference across districts. γ_t captures the year of birth fixed effect and this control for unobservable difference across cohorts. The term $X_{i,d,t}$ denotes the same controls included in the first stage. Now, the coefficient β from the above equation can be interpreted as the causal effect of education on risk preference.

[8]Born between 1967 and 1974

[9]Duflo (2001) stated that the programme started between 1973 and 1974, and she chose to set the beginning in 1973 in the analysis.

[10]Duflo (2001) investigated the effect of education on wages. I think that few females were wage-earners so she did not include females in her analysis.

3.2 Mexico

In Mexico, I use a regression discontinuity design (RDD) approach in my first-stage equation, exploiting the reform to the compulsory schooling law in 1993[11]. As this educational reform is nationwide, there is no geographic variation in the treatment across districts. Therefore, whether an individual was exposed to the reform only depended on his/her birth year. Individuals aged below 12 in 1993 were affected by the reform, while individuals aged above 14 in 1993 were not exposed to the reform. Individuals aged between 12 and 14 (inclusive) were excluded from the main estimation sample, as they were partially exposed to the reform. To sum up, my strategy in Mexico compares the outcomes for individuals aged below 12 in 1993 to the outcomes for individuals aged above 14 in 1993[12].

For the first stage, I estimate:

$$S_i = \alpha + \sigma_1 T_i + \sigma_2 T_i \times (Age93_i - 12) + \sigma_3 (1 - T_i) \times (Age93_i - 12) + X_i + \varepsilon_i \tag{4}$$

where i denotes an individual. S consists of years of education and an indicator variable for completing junior secondary schooling. T is a dummy variable equal to 1 if the age in 1993 is less than 12 and 0 otherwise. $Age93$ denotes the age in 1993. The term X_i is a vector of controls, including gender, parental education, age in the survey[13], an indicator variable whether belongs to the majority ethnicity, municipality fixed effect and birth year fixed effect. Finally, I two-way cluster the standard error by state and the birth year.

I then use the predicted education from the first stage to estimate the second stage:

$$R_i = \alpha + \beta \widehat{S_i} + \beta_2 T_i \times (Age93_i - 12) + \beta_3 (1 - T_i) \times (Age93_i - 12) + X_i + \varepsilon_i \tag{5}$$

where R_i is a measure of the degree of risk aversion, ranging from 0 to 5. $R_{i,d,t}$ equal to 0 refers to the least risk-averse, and $R_{i,d,t}$ equal to 5 refers to the most risk-averse. Now, the coefficient β from the above equation can be interpreted as the causal effect of education on risk preference.

[11] described in Gleditsch et al. (2022)

[12] My strategy in Mexico is similar to the one used in Grépin and Bharadwaj (2015).

[13] The survey in Mexico (MxFLS3) lasted from 2009 to 2013, while the survey in Indonesia (IFLS4) was conducted in 2007. The birth year fixed effect already controls for the age in the survey in Indonesia.

4 Data

I use IFLS and the administrative data of the INPRES programme for Indonesia. I use MxFLS for Mexico. The online appendix describes these data in detail.

Table 1: Indonesia

	Mean	Std.Dev.	Min	Max	Obs
Years of Education	7.90	4.57	0	16	6796
Risk Aversion	2.82	1.40	0	4	6796
Birth of Year	1965.68	6.69	1954	1974	6796
Age	41.32	6.69	33	53	6796
Female (=1)	0.51	0.50	0	1	6796
Main Ethnicity (=1)	6.07	10.46	1	95	6796
Muslim (=1)	0.89	0.31	0	1	6796
Urban (=1)	0.55	0.50	0	1	6796
Wage Earner (=1)	0.51	0.50	0	1	6796
Self-employed (=1)	0.43	0.50	0	1	6796

Note: The sample consists of individuals born between 1954 and 1961 and individuals born between 1867 and 1974 in IFLS4.

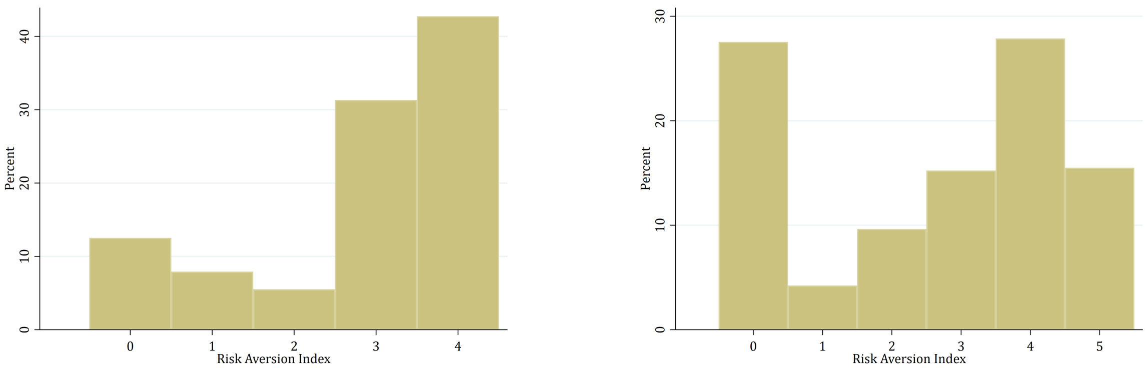
Table 2: Mexico

	Mean	Std.Dev.	Min	Max	Obs
Years of Education	9.46	3.57	0	15	13646
Risk Aversion	2.55	1.85	0	5	13646
Birth of Year	1981.84	7.73	1967	1993	13646
Age	27.30	7.77	15	49	13646
Female (=1)	0.56	0.50	0	1	13646
Main Ethnicity (=1)	0.83	0.38	0	1	13646
Wage Earner (=1)	0.47	0.50	0	1	13646
Self-employed (=1)	0.10	0.29	0	1	13646

Notes: The sample consists of cohorts born in 1967-1993 in MxFLS3.

Figure 1: Distribution of Risk Aversion

(a) Indonesia (b) Mexico



Note: Risk aversion index equals to zero if the individual is lowest risk aversion. Higher values of index denotes higher level of risk aversion. Graph (a) uses the entire Indonesian Family Life Survey (Wave 4) sample. Graph (b) uses the entire Mexican Family Life Survey (Wave 3) sample.

5 Results

5.1 Indonesia

5.1.1 First stage results: Impact of school construction programme on education

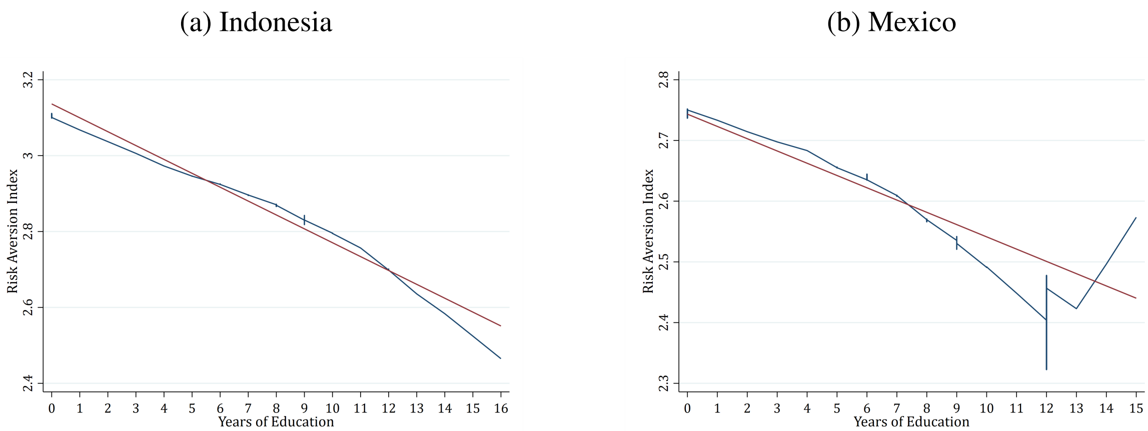
Table 3 summarises the results of the first stage regression equation (2) in Indonesia for the impact of the school construction programme on education: one new school constructed per thousand children leads to an increase in the schooling of 0.3 years, with a 95% confidence interval between 0.08 and 0.54 years; one new school constructed leads to an increased probability of completing primary school by 3.8 percentage points, with a 95% confidence interval between 0.86 and 6.74 percentage points. Individuals who were between 7 to 11 in 1973 were partially treated individuals and were excluded from the sample[14].

5.1.2 Impact of education on risk aversion

I first represent the correlation between years of education and risk aversion. Figure 2 (a) demonstrates a linear relationship and nonparametric locally weighted scatterplot smoothing (LOWESS) plot examining the correlation between years of education and risk aversion in Indonesia. In Table 4, columns (1) and (2) estimate this simple OLS regression of equation (1). Both the simple OLS estimate and the figure show that individuals with more years of education are less risk aversion. However, as stated

[14]Duflo (2001) also drop the partially treated individuals.

Figure 2: Correlation between Risk Aversion and Years of Education



Note: Graphs plot the correlation between risk aversion index and education. The blue lines represent the nonparametric locally weighted scatterplot smoothing (LOWESS) plot. The red lines demonstrate the linear relationship. Graph (a) uses the entire Indonesian Family Life Survey (Wave 4) sample. Graph (b) uses the entire Mexican Family Life Survey (Wave 3) sample.

above, the observed negative correlation cannot be interpreted as causality. Table 4 reports the results from the IV method for the full sample: neither an extra year of education nor completion of primary school has a causal effect on risk preference. Not only do I find no significant effect, but also the confidence intervals around the estimates are tight enough to be informative. The F-statistics of the first stages are both higher than 10, so exposure to the INPRES programme is a strong instrument for years of education and primary school completion.

5.2 Mexico

5.2.1 First stage results: Impact of Mexican reform in compulsory schooling on education

Figure 3 demonstrates the impact of the education reform with the age of the individual in 1993 presented along the x-axis. The two subfigures on the left side represent the impact of the reform on years of schooling. The two subfigures on the right represent the impact of the reform on the probability of completing secondary school. The ages between the two vertical lines in each figures at the age between 12 and 14 are considered partially exposed to the reform. The two subfigures above exclude the partially treated individuals. The individuals on the left-hand side of the vertical line are considered the treatment group, while the individuals on the right side of the vertical line are considered the control group. The discontinuity is easily visible: individuals who were young than 12 in 1993 experienced a greater jump in years of education and

Table 3: First Stage Result - impact of Indonesian INPRES programme on education

VARIABLES	(1) Years of Education	(2) Primary School Completion
$T_i \times Intensity_{t,d}$	0.308**	0.038**
	(0.117)	(0.015)
Observations	6,483	6,483
District FE	YES	YES
Birth of Year FE	YES	YES
Mean of Dependent Variable	7.711	0.722

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table reports the difference-in-difference estimate of the effect of the Indonesian school construction programme INRPES on education, exploiting the interaction between whether an individual is exposed to the programme and the intensity of treatment, measured by the number of schools constructed per thousand children by an individual's age of 6. The sample consists of individuals born between 1954 and 1961 and individuals born between 1867 and 1974 in IFLS4. Following Duflo (2001), all regressions include interactions between the year of birth dummies and the children population in the district (in 1971), school enrollment rate in the district (in 1971) and a water and sanitation programme intensity in the district. All regressions also include the year of birth fixed effects and the district of birth fixed effect. All regressions also include the controls for gender, dummies for majority ethnicity, majority religion (Muslim), urban location and language. Standard errors are two-way clustered at the birth of province and birth of the year. There are 25 provinces and 224 districts.
Sources: Individual-level data are drawn from the Indonesian Family Life Survey (Wave 4). Data on INPRES programme and population density and enrollment rate at the district level come from Roodman (2022), and data on the water and sanitation programme at the district level is provided by Esther Duflo.

probability of completing secondary school relative to those who were older than 14 in 1993. The discontinuity in the probability of completing secondary school is more obvious than that in the years of education.

Table 5 reports the results of the first-stage regressions in Mexico: the compulsory schooling years reform leads to an increase in the schooling of 0.51 years, with a 95% confidence interval between 0.22 and 0.8 years; the compulsory schooling years reform leads to an increased probability of completing secondary school by 9.74 percentage points, with a 95% confidence interval between 5.56 and 13.91 percentage points.

Table 4: Impact of Education on Risk Aversion: Indonesia

VARIABLES	(1) OLS	(2) OLS	(3) IV	(4) IV
Years of Education	-0.027***		0.003	
	(0.006)		(0.103)	
Primary School Completion		-0.111*		0.025
		(0.066)		(0.871)
Observations	6,363	6,363	6,100	6,100
District FE	YES	YES	YES	YES
Birth of Year FE	YES	YES	YES	YES
Mean of Dependent Variable	2.828	2.828	2.816	2.816
First Stage F-Test			10.02	10.80

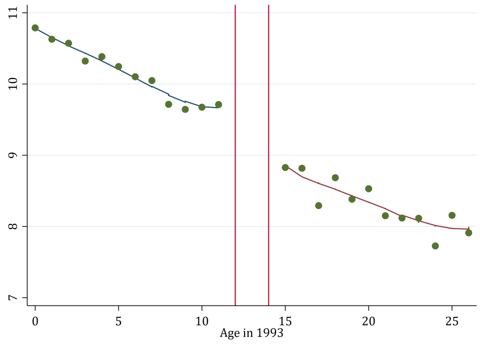
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable is the risk aversion index, ranging from 0 to 4. Higher values of the index denote a higher level of risk aversion. The sample consists of individuals born between 1954 and 1961 and individuals born between 1867 and 1974 in IFLS4. Following Duflo (2001), all regressions include interactions between the year of birth dummies and the children population in the district (in 1971), school enrollment rate in the district (in 1971) and a water and sanitation programme intensity in the district. All regressions also include the year of birth fixed effects and the district of birth fixed effect. All regressions also include the controls for gender, dummies for majority ethnicity, religion (Muslim), urban location and language. Standard errors are two-way clustered at the province of birth and the year of birth. There are 25 provinces and 224 districts.
Sources: Individual-level data are drawn from the Indonesian Family Life Survey (Wave 4). Data on INPRES programme and population density and enrollment rate at the district level come from Roodman (2022), and data on the water and sanitation programme at the district level is provided by Esther Duflo.

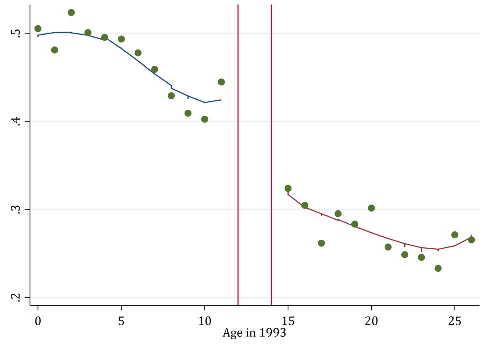
Panel A reports the results for individuals aged 0 to 26 in 1993 and excludes the partially treated individuals. I also use this sample for my main analysis. I further demonstrate the first stage results using other bandwidths as robustness tests in other panels. The effect of reform on the probability of completing secondary school is robust to any bandwidth, while the effect on years of schooling is not significant at a smaller bandwidth. This is consistent with what was mentioned above, i.e., that the jump in the

Figure 3: Education by Age in 1993 in Mexico

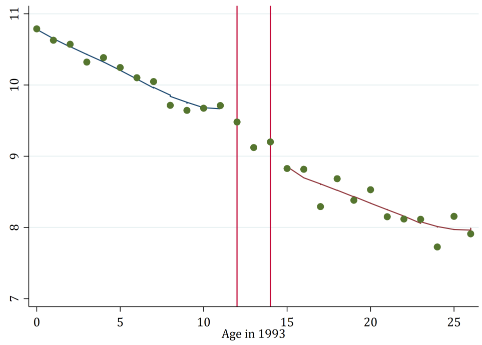
(a) Years of Education



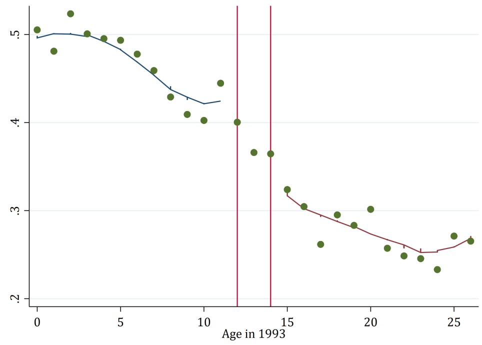
(b) Secondary School Completion



(c) Years of Education



(d) Secondary School Completion



Note: Bottom panel excludes individuals who aged between 12 and 14 in 1993.

probability of completing secondary school is more obvious than that in the years of schooling in Figure 3. I also include the partially treated individuals in panel E. The impact of reform is smaller than that in panel A and this is consistent with what I expected above that the reform should have less impact on individuals aged between 12 and 14 in 1993 than those aged below 12.

5.2.2 Impact of education on risk aversion in Mexico

I also represent the correlation between years of education and risk aversion in Figure 2 (b). Table 6 reports the result from the OLS estimates and IV method for individuals aged 0 to 26 (excluding ages 12 to 14) in 1993. The results show that one extra year of education leads to a decrease in risk aversion by 23.8 percentage points and completion of secondary schooling leads to a decrease in risk aversion by 126 percentage points, but neither of these effects is statistically significant. The F-statistics of the first stages are higher than 10, so the educational reform is a strong instrument for years of education and secondary school completion. In summary, the results of Indonesia and

Table 5: First stage - impact of Mexican compulsory schooling reform on education

	Years of Education	Secondary School Completion
Panel A: Full sample (ages 0 - 26)		
Indicator variable for age less than 12 in 1993	0.510***	0.0974***
	(0.148)	(0.0213)
Observations	12237	12237
Panel B: Bandwidth ages 3 - 23		
Indicator variable for age less than 12 in 1993	0.473**	0.0682**
	(0.157)	(0.0217)
Observations	9043	9043
Panel C: Bandwidth ages 6 - 20		
Indicator variable for age less than 12 in 1993	0.473	0.0794*
	(0.269)	(0.0370)
Observations	5897	5897
Panel D: Bandwidth ages 9 - 17		
Indicator variable for age less than 12 in 1993	0.0197	0.0338***
	(0.173)	(0.00538)
Observations	2805	2805
Panel E: Including ages 12 - 14		
Indicator variable for age less than 12 in 1993	0.272*	0.0547**
	(0.116)	(0.0191)
Observations	13621	13621

Notes: This table reports the estimates of the effect of the Mexican policy change in compulsory schooling (in 1993) on education. The sample consists of individuals who were between 0 and 26 (inclusive) in 1993 (expected where noted), excluding individuals who were between the ages of 12 and 14 (inclusive) in 1993 (except where noted). All regressions include the year of birth fixed effects and municipality of birth fixed effect and the controls for gender, parental education, age at the survey and an indicator variable whether belongs to the majority ethnicity. Clustered standard errors in parentheses. Standard errors are two-way clustered at the state of birth and the year of birth. There are 26 states and 119 municipalities.
Sources: Individual-level data are drawn from the Mexican Family Life Survey (Wave 3).
***p<0.01, **p<0.05, *p<0.1

Mexico are consistent: risk aversion and education are significantly negative correlated, but an extra year of education does not change risk preference. Therefore, I conclude that education has no causal effect on risk preference.

Table 6: Impact of Education on Risk Aversion: Mexico

VARIABLES	(1) OLS	(2) OLS	(3) IV	(4) IV
Years of Education	-0.015***		-0.238	
	(0.006)		(0.202)	
Secondary School Completion		-0.189***		-1.262
		(0.026)		(1.052)
Observations	12,237	12,237	12,237	12,237
District FE	YES	YES	YES	YES
Birth of Year FE	YES	YES	YES	YES
Mean of Dependent Variable	2.564	2.564	2.564	2.564
First Stage F-Test			11.34	11.57

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable is the risk aversion index, ranging from 0 to 5. Higher values of the index denote a higher level of risk aversion. The sample consists of individuals who were between 0 and 26 (inclusive) in 1993 (expected where noted), excluding individuals who were between the ages of 12 and 14 (inclusive) in 1993 (except where noted). All regressions include the year of birth fixed effects and municipality of birth fixed effect and the controls for gender, parental education, age at the survey and an indicator variable whether belongs to the majority ethnicity. Standard errors are two-way clustered at the state of birth and the year of birth. There are 26 states and 119 municipalities.
Sources: Individual-level data are drawn from the Mexican Family Life Survey (Wave 3).

6 Conclusion

To establish whether education can change an individual's risk preference, I exploit the Indonesian INRPES school construction programme and Mexican education reform in compulsory schooling as natural experiments. Applying the instrumental variable approach in two separate experiments, I find that education cannot change risk preference. The results are consistent in two different settings, so my findings are externally valid. My result is consistent with the finding that education does not change decision-making quality and risk preference (Banks et al., 2019), while my result contrasts with findings that education leads to a decrease in risk aversion (Jung, 2015; Tawiah, 2022). This paper contributes to the literature on the determinants of social preference and noncognitive abilities and the literature on the outcomes of education.

The results have several important implications. First, previous studies show that there is a negative correlation between education and risk aversion (Donkers et al., 2001; Falk et al., 2018; Harrison et al., 2007; Hartog et al., 2002), and this correlation also holds in this study. My contribution to these studies is to show that such correlations do not represent causality. Furthermore, such a negative correlation is likely due to reverse causality that innately less risk-averse individuals are more likely to invest in education.

Second, previous studies show that education has a causal effect on risk-taking in real life, such as investment. For example, schooling increases the probability of participating in the financial market (Black et al., 2018; Cole et al., 2014). My contribution to these studies is to empirically test one of the potential mechanisms "preference effects"[15] of such studies. I demonstrate that educated individuals are more likely to invest not because education makes them more willing to take risks. Therefore, a change in risk preference might not be a pathway in which education influences risk-taking in real life. Future studies can empirically test the alternative mechanisms, such as "wealth effects"[16] and "information effects"[17].

Finally, previous literature found that early childhood is an important period to develop social preferences (Andersen et al., 2013; Armin & Kosse, 2016) and that preschool education can change an individual's social preferences in adulthood (Cappelen et al., 2020). I contribute to these studies by investigating the effect of primary and secondary schooling on social preference. My findings suggest that social preferences might develop in early life and might be hard to be changed by future education when

[15]"Preference effects" means that educated individuals invest more because they are more willing to take risks.

[16]"Wealth effect" means that educated individuals invest more because they have more money to invest.

[17]"Information effect" means that educated individuals invest more because they have more inforamtion about how to invest.

individuals grow old. Therefore, any policies or interventions which try to change social preferences should target the early life period.

7 Appendix

Online appendix: https://www.dropbox.com/s/m8dxif68a4zzake/Online%20appendix.pdf?dl=0

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